

Albert, Max; Mertins, Vanessa

Working Paper

Participation and decision making: a three-person power-to-take experiment

MAGKS Joint Discussion Paper Series in Economics, No. 2008,05

Provided in Cooperation with:

Faculty of Business Administration and Economics, University of Marburg

Suggested Citation: Albert, Max; Mertins, Vanessa (2008) : Participation and decision making: a three-person power-to-take experiment, MAGKS Joint Discussion Paper Series in Economics, No. 2008,05, Philipps-University Marburg, Faculty of Business Administration and Economics, Marburg

This Version is available at:

<https://hdl.handle.net/10419/30094>

Standard-Nutzungsbedingungen:

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

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

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

Terms of use:

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

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

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

MAGKS



**Joint Discussion Paper
Series in Economics**

by the Universities of
Aachen · Gießen · Göttingen
Kassel · Marburg · Siegen

ISSN 1867-3678

No. 05-2008

**Max Albert
Vanessa Mertins**

**Participation and Decision Making:
A Three-person Power-to-take Experiment**

Max Albert
Justus Liebig University Giessen
Dept. of Economics
Licher Straße 66, 35394 Giessen
Tel: ++49-641 / 99-22200 Fax: ++49-641 / 99-22209
Email: max.albert@wirtschaft.uni-giessen.de

Vanessa Mertins
Saarland University, Dept of Economics
P.O. Box 151150, 66041 Saarbrücken
Tel: ++49-681/3022170, Fax: ++49-681/3024823
E-mail: v.mertins@mx.uni-saarland.de

This paper can be downloaded from
http://www.uni-marburg.de/fb02/makro/forschung/magkspapers/index_html%28magks%29

Coordination: Bernd Hayo • Philipps University Marburg • Faculty of Business Administration &
Economics • Universitätsstr. 24, D-35032 Marburg
Tel: ++49-6421-2823091, Fax: ++49-6421-2823088 e-mail: hayo@wiwi.uni-marburg.de

Participation and Decision Making: A
Three-person Power-to-take Experiment

Max Albert¹ Vanessa Mertins²

May 6, 2008

¹Dept of Economics, Justus Liebig University Giessen, Licher Str. 66, 35394 Giessen, Germany. E-mail: max.albert@wirtschaft.uni-giessen.de

²Dept of Economics, Saarland University, P.O. Box 151150, 66041 Saarbrücken, Germany. E-mail: v.mertins@mx.uni-saarland.de, Tel: +49-681-3022170, Fax: +49-681-3024823

Abstract

It is often conjectured that participatory decision making may increase acceptance even of unfavorable decisions. The present paper tests this conjecture in a three-person power-to-take game. Two takers decide which fraction of the responder's endowment to transfer to themselves; the responder decides which part of the endowment to destroy. Thus, the responder can punish greedy takers, but only at a cost to herself. We modify the game by letting the responder participate in takers' transfer decision and consider the effect of participation on the destruction rate. We find that participation matters. Responders destroy more if they (1) had no opportunity to participate in the decision making process and (2) are confronted with highly unfavorable outcomes. This participation effect is highly significant for those responders (the majority) who show negative reciprocity (i.e., destroy more when takers are greedier).

JEL Classification: C72, C91, D72

Keywords: fairness, participatory decision making, power-to-take game, procedural fairness, reciprocity

1 Introduction

It is a common experience that people can get seriously annoyed if decisions affecting them are taken ‘over their heads’. Reactions range from negative comments over passive resistance to active resistance. From an economic point of view, the interesting aspect is that it is often the perceived unfairness of the decision procedure, and not so much the decision itself, which triggers these negative reactions. This raises the question of whether a fair procedure, and especially the involvement of the affected parties in the decision process, may increase acceptance of decisions, especially unfavorable decisions.

A minimum requirement of procedural fairness¹ seems to be that, if possible and feasible, the affected parties should be given a *voice*: they are to be heard before the decision is made. This is also a cornerstone of most legal systems. In accordance with the famous legal principle *audiatur et altera pars*, judges are required to give a hearing to both sides in a dispute. In many cases, people want more than just a voice; they want the possibility of *participation* through, for instance, voting or vetoing. Again, legal procedures often contain such stronger representation rights, for instance, the right to reject candidates for a jury. Depending on the decision in question, then, procedural fairness requires representation of the affected parties in the decision making process, where representation rights vary from voice to

¹Economists mostly speak of *procedural fairness*, while psychologists and lawyers seem to prefer *procedural justice*. We consider these expressions to be synonymous.

participation.²

According to the *homo oeconomicus* approach, of course, the design of decision procedures matters to the affected parties only through its effects on outcomes. Specifically, whether someone is granted representation rights or not at an earlier stage of the decision process should, *ceteris paribus*, be irrelevant for his behavior in the face of a given unfavorable decision at a later stage.³ Recent work in behavioral economics and social psychology, however, indicates that this is probably false. People seem to care not only

²We take these distinctions from the psychological literature on procedural fairness. Leventhal (1980) distinguishes between six different procedural rules of fair processes: representativeness, consistency, correctability, bias suppression, accuracy, and ethicality. Representativeness can be subdivided into two aspects: participation and voice. According to Tyler, Boeckmann, Smith and Huo (1997), a process is considered fair if people can participate in the process (participation) and are heard (voice); further criteria which are met by fair decision making processes include adequate time for the process, adequate information, respectful treatment, discussion of and dealing with issues, and lack of bias by authorities. Tyler and Lind (2000: 67-77) add entitlement of authorities. We speak of representation instead of representativeness, with voice (being heard) being the weakest form and participation covering any stronger form of influence. Note that voice is sometimes used in a stronger sense. Anand (2001: 249) defines voice as the extent to which a person has control over a decision. Folger (1977) and Folger, Rosenfield, Grove and Corkran (1979) define voice as the extent to which opinions and preferences of affected parties are considered in the decision-making process.

³The *ceteris paribus* clause covers informational aspects: if the decision to exercise (or not) representation rights reveals private information, this can affect the behavior of a *homo oeconomicus*.

about outcomes but also about the procedure through which an outcome is achieved. They have procedural likes and dislikes, or procedural preferences, which depend on the procedures' perceived fairness or unfairness.⁴

Procedural preferences not only influence choices *between* procedures but, more surprisingly, choices *within* procedures, specifically, the choice to offer resistance. The explanation for this effect seems to be the connection between fairness perceptions and negative reciprocity observed in many experiments.

By negative reciprocity, we mean the adoption of a costly action that harms another person because that person's intentional behavior was perceived to be harmful to oneself.⁵ Of course, whenever people have to share resources, taking something for oneself implies harming the others. However, this does not always trigger negatively reciprocal behavior. Typically, negative reciprocity is caused by the perception of unfairness. In the ultimatum game, for instance, unfair proposals cause responders to reject positive offers (Güth et al. 1982). Falk et al. (2008) have shown that proposers' intentions matter in this respect: it seems that responders wish not (only) to avoid unequal or unfair outcomes but (also) to punish intentionally unfair proposers.

There is evidence that the fairness of the decision procedure itself also influences the degree of negative reciprocity. In a survey of forty independent

⁴On procedural preferences in general, see, e.g., Leventhal 1980, Lind and Tyler (1988), Elster (1989), Tyler et al. (1997), Frey et al. (2004) and Bohnet (2006).

⁵See Cox and Deck (2005). Subsequently, however, we do not distinguish between negative reciprocity as a motivation and negatively reciprocal behavior.

studies, Brockner and Wiesenfeld (1996) show that people are more satisfied even with unfavorable outcomes if these outcomes have been accomplished on a fair basis. Tyler and Lind (2000) found that people are more likely to obey the commands of an authority if they regard the authority to be entitled to their obedience. This holds irrespective of their judgements about the authority's decision. Since entitlement is also an aspect of procedural fairness, this indicates that people may be more likely to accept unfavorable outcomes if the process leading to the outcome was fair (Tyler and Lind 1988, Tyler 1990, Thibaut and Walker 1975).

In a review of the psychological literature, Konovsky (2000) emphasizes the particular importance of procedural fairness in business organizations. Procedural fairness evaluations influence negative employee behaviors such as theft (Greenberg, 1990), employees' job satisfaction and organizational commitment (Lowe and Vodanovich, 1995), organizational change (Tyler and De Cremer, 2005), and turnover intentions (Olkkonen and Lipponen, 2006).

The special importance of participation is emphasized by Frey et al. (2004), who measure procedural utility by individuals' reported subjective well-being or happiness, arguing that individuals gain procedural utility, in addition to outcome utility, through actual participation or even the mere possibility of participation. The same outcome may be evaluated differently, depending on whether it is a market outcome or the result of voting, bargaining, or command. In particular, judgements of procedural fairness, for instance, whether participation is allowed or denied, may have an impact on

the acceptance of outcomes.

Despite the significance ascribed to procedural fairness in the literature, there are few experimental studies where participants' decisions have monetary consequences. An important exception is Bolton et al. (2005), who show that allocations resulting from unbiased random procedures, which are usually viewed as fair, are more readily accepted than the same allocations when chosen by another person. Grimalda et al. (2007) address a question similar to ours. In a three-person ultimatum game⁶, they vary the degree of participation: in the non-participation treatment (our term), a decision maker is selected randomly; in the participation treatment, every player makes a proposal and one of the proposals is selected randomly. As the authors emphasize, these two treatments are strategically equivalent. Perhaps not surprisingly, then, they find only weak evidence that participation makes a difference: after players have gained some experience, proposers seem to demand less, and responders seem to concede less, in the participation treatment, which might be due to an entitlement effect.

As far as we know, the experiment reported in this paper is the first incentivized experiment varying the degree of strategically relevant participation.

We modify Bosman and van Winden's (2002) power-to-take game, using a setup with one responder and two takers. In the first stage of this game, the takers decide which fraction of the responder's endowment to transfer to

⁶With, it seems, monetary payoffs; however, Grimalda et al. (2007) do not mention the exact values.

themselves (the take rate). In the second stage, the responder decides which part of the endowment to destroy (the destruction rate). Thus, the responder can punish greedy takers, but only at a cost to herself. In comparison to the ultimatum game, the responder can vary the degree of rejection by destroying only a part of her endowment. The game approximates social environments characterized by appropriation, for instance, taxation, common agency, or monopolistic selling (Bosman and van Winden, 2002).

We further modify the game by letting the responder participate in the takers' decision and consider the effect on her choice of the destruction rate. To study the impact of the degree of participation, we consider four different versions of the first-stage group decision making process. In all cases, the take rate is determined as the weighted average of three simultaneous proposals, two of which are made by the takers. The third proposal is made either by the responder (participation treatment) or by a computerized dummy making a random choice (no-participation treatment). While takers' proposals have always equal weights, the weight of the responder's or dummy's proposal is either equal to the weight of a taker's proposal (low-influence treatment) or twice as high (high-influence treatment).

In contrast to previous studies based on the power-to-take game, we use Selten's (1967) strategy method for responders' decisions on destruction rates, thus asking responders to choose destruction rates for all possible take rates. This allows us to observe the extent of negative reciprocity shown by a participant, that is, the extent to which participants choose higher de-

struction rates as a response to higher take rates.⁷

It is well-known from many experiments that not all participants show reciprocal behavior. Instead, participants fall into different categories called player types. The following types have been observed in many experiments.⁸

Type 0 (*homo oeconomicus*): This type shows no reciprocity, behaving instead just like the *homo oeconomicus* model predicts.

Type 1 (unconditional cooperator): This type also shows no reciprocity but behaves cooperatively even in the face of non-cooperative behavior.

Type 2 (*homo reciprocans*): Also known as conditional cooperator. This type shows reciprocity to various degrees.⁹

Type 3 (erratic): This type shows erratic behavior.

Given that the occurrence of these types is a well-known phenomenon, an analysis of average behavior differences between treatments is clearly in-

⁷It is plausible that the strategy method weakens the influence of emotions on decision making since participants consider their reactions to hypothetical, and not actual, choices of other players (“cold”, in contrast to “hot”, settings, see Brandts and Charness 2000). Evidence on the effect of applying the strategy method is mixed. Brandts and Charness (2000) and Fischbacher and Gächter (2006) do not find a difference between hot and cold settings, whereas Brosig et al. (2003) do.

⁸See, e.g., Fischbacher et al. (2001), Goeree et al. (2002), Kurzban and Houser (2005), Burlando and Guala (2005), Fischbacher and Gächter (2006), and Albert et al. (2007). Although the discussion of agents’ heterogeneity has a long history in economics, experimental evidence on the existence of different player types is still very imperfect.

⁹See, e.g., Falk and Fischbacher (2006) for theories of reciprocity. Dohmen et al. (2006) explore the prevalence of reciprocity in the population by analyzing survey data.

sufficient. A difference in averages may be due to changes in the frequency of different types, or due to changes of the behavior of the types. From the literature reviewed above, no hypothesis concerning the frequency of different types in different treatments emerges. Our hypothesis is that responder types are given exogenously and that, therefore, any observed differences in frequency between treatments are due to chance. This seems to be the usual hypothesis in the literature; it can, moreover, be tested.

This leaves changes in the behavior of types. By definition, type 0 and type 1 behaviors, which in our experiment need not be different, cannot be affected by our treatments.¹⁰ With respect to the erratic type, any influence is possible, but the literature contains no hypotheses, again more or less by definition, since susceptibility to systematic influences means that behavior is only partially erratic.

In the case of type 2, *homo reciprocans*, which usually is the most frequent type, a clear hypothesis emerges. If, as conjectured, participation is an aspect of fairness, negative reciprocity should be less pronounced in the participation treatments. Whether a higher influence also increases the perceived fairness of the decision procedure is an open question.

The paper proceeds as follows. Section 2 explains the experimental de-

¹⁰Actually, this is not quite correct. Types 0 and 1 both do not engage in negative reciprocity. However, type 0 is indifferent between all destruction rates when faced with a take rate of 1. Strictly speaking, a treatment effect on indifferent choices is not ruled out by the *homo oeconomicus* model.

sign and procedures. Section 3 presents the experimental results. Section 4 concludes.¹¹

2 The experiment

2.1 Experimental design

Participants play a three-person two-stage one-shot game. In each game, there are two takers, also called player 1 and 2, and one responder, also called player 3. The responder has an endowment e . Takers have no endowments. In the first stage, a take rate $t \in [0, 1]$ is determined in a simultaneous move. In the second stage, the responder chooses a destruction rate $d \in [0, 1]$ in response to the take rate determined in stage 1. The experimental payoff to each taker $j = 1, 2$ is $\pi_j = 0.5t(1 - d)e$ as takers' total payoff is equally split among them. The responder's experimental payoff is $\pi_3 = (1 - t)(1 - d)e$. Thus, the responder can punish the takers by destroying more or less of her endowment. As long as $t < 1$, punishment is costly.

To study the impact of participation and strength of influence, we consider four different versions of the first-stage group decision making process. In all cases, the take rate is determined as the weighted average of three simultaneous proposals, two of which are made by the takers. The third proposal is made either by the responder (participation treatment) or by a computerized

¹¹Translations of the instructions and a post-experimental questionnaire are available from the authors upon request.

Table 1: Treatment Groups

		Participation	
		yes	no
Influence	low	PartLow	NoPartLow
	high	PartHigh	NoPartHigh

dummy making a random choice (no-participation treatment). We implemented a dummy in order to make the participation and no-participation treatments comparable.

Let t_1 and t_2 be the proposals of taker 1 and taker 2 respectively. Let t_3 be the proposal of the responder (in the participation treatment) or the dummy (in the no-participation treatment). The take rate is then determined as $t = \frac{t_1+t_2+wt_3}{2+w}$. The weight $w/(2+w)$ of the responder's or the dummy's proposal is either $\frac{1}{3}$ for $w = 1$ (low-influence treatment) or $\frac{1}{2}$ for $w = 2$ (high-influence treatment). This results in $2 \times 2 = 4$ treatment groups with the mnemonic names PartLow, PartHigh, NoPartLow and NoPartHigh (see table 1).

In the actual game, we restrict proposals to three possibilities, $t_j \in \mathcal{T} = \{\frac{1}{3}, \frac{2}{3}, 1\}$ for $j = 1, 2, 3$, and destruction rates to 101 possibilities, $d \in \mathcal{D} = \{\frac{0}{100}, \frac{1}{100}, \dots, \frac{100}{100}\}$. The possible group take rates in the low-influence treatments were therefore restricted to the set $\mathcal{T}^{\text{Low}} = \{\frac{3}{9}, \frac{4}{9}, \frac{5}{9}, \frac{6}{9}, \frac{7}{9}, \frac{8}{9}, 1\}$. In the high-influence treatments, possible group take rates were restricted to the

set $\mathcal{T}^{\text{High}} = \{\frac{4}{12}, \frac{5}{12}, \frac{6}{12}, \frac{7}{12}, \frac{8}{12}, \frac{9}{12}, \frac{10}{12}, \frac{11}{12}, 1\}$. Note that $\mathcal{T}^{\text{Low}} \cap \mathcal{T}^{\text{High}} = \mathcal{T}$. For $t = \frac{2}{3}$, the group's endowment would be equally distributed among group members, irrespective of d . Table 2 shows the possible take rates t .

Table 2: Possible group take rates t

Low treatments: $t = \frac{t_1+t_2+t_3}{3}$

$\{t_1, t_2\} =$

	$\{\frac{1}{3}, \frac{1}{3}\}$	$\{\frac{1}{3}, \frac{2}{3}\}$	$\{\frac{1}{3}, 1\}$	$\{\frac{2}{3}, \frac{2}{3}\}$	$\{\frac{2}{3}, 1\}$	$\{1, 1\}$
$t_3 = \frac{1}{3}$	$\frac{3}{9}$	$\frac{4}{9}$	$\frac{5}{9}$	$\frac{5}{9}$	$\frac{6}{9}$	$\frac{7}{9}$
$\frac{2}{3}$	$\frac{4}{9}$	$\frac{5}{9}$	$\frac{6}{9}$	$\frac{6}{9}$	$\frac{7}{9}$	$\frac{8}{9}$
1	$\frac{5}{9}$	$\frac{6}{9}$	$\frac{7}{9}$	$\frac{7}{9}$	$\frac{8}{9}$	1

$\mathcal{T}^{\text{Low}} = \{\frac{3}{9}, \frac{4}{9}, \frac{5}{9}, \frac{6}{9}, \frac{7}{9}, \frac{8}{9}, 1\}$

High treatments: $t = \frac{t_1+t_2+2t_3}{4}$

$\{t_1, t_2\} =$

	$\{\frac{1}{3}, \frac{1}{3}\}$	$\{\frac{1}{3}, \frac{2}{3}\}$	$\{\frac{1}{3}, 1\}$	$\{\frac{2}{3}, \frac{2}{3}\}$	$\{\frac{2}{3}, 1\}$	$\{1, 1\}$
$t_3 = \frac{1}{3}$	$\frac{4}{12}$	$\frac{5}{12}$	$\frac{6}{12}$	$\frac{6}{12}$	$\frac{7}{12}$	$\frac{8}{12}$
$\frac{2}{3}$	$\frac{6}{12}$	$\frac{7}{12}$	$\frac{8}{12}$	$\frac{8}{12}$	$\frac{9}{12}$	$\frac{10}{12}$
1	$\frac{8}{12}$	$\frac{9}{12}$	$\frac{10}{12}$	$\frac{10}{12}$	$\frac{11}{12}$	1

$\mathcal{T}^{\text{High}} = \{\frac{4}{12}, \frac{5}{12}, \frac{6}{12}, \frac{7}{12}, \frac{8}{12}, \frac{9}{12}, \frac{10}{12}, \frac{11}{12}, 1\}$

2.2 Equilibria under maximization of expected experimental payoffs

Assuming rationality and maximization of own expected experimental payoffs, we can derive the subgame perfect equilibria of the game.¹²

Responders' behavior can be analyzed independently of the details of the treatments. Responders should always propose the lowest take rate possible, i.e., $t_3 = \frac{1}{3}$. The optimal destruction rate of the responder in stage 2 depends on the take rate determined in stage 1. We describe their strategies by an n -tuple of destruction rates $d := (d_1, d_2, \dots, d_n) \in \mathcal{D}^n$, where d_j is the response to group take rate t_j , $t_1 < t_2 < \dots < t_n$, and where the number n of possible take rates and their values depend on the treatment. Maximizing one's experimental payoff as a responder means to destroy nothing if this is costly (if $t < 1$), and to destroy any fraction of the pie if this is costless (if $t = 1$). We denote these strategies by $d_x := (0, 0, \dots, 0, x)$ with arbitrary $x \in \mathcal{D}$.

Let us consider treatment PartLow first. Given that $t < 1$ because of $t_3 = \frac{1}{3}$, the responder's choice among the strategies d_x never matters. Moreover, given that $t < 1$, the takers have no reason to play anything but $t_1 = t_2 = 1$. Hence, the subgame perfect equilibria are given by the strategy profiles $(t_1, t_2, t_3, d) = (1, 1, \frac{1}{3}, d_x)$, $x \in \mathcal{D}$. The same reasoning, result, and notation apply to treatment PartHigh, with the difference that the set of possible

¹²On the implied assumption of risk neutrality, see the discussion in n. 15 below.

take rates changes. Equilibria in the Part treatments are efficient because the expected destruction rate is zero.

In the NoPart treatments, $t_1 = t_2 = 1$ results in a probability of $\frac{1}{3}$ for the event $t = 1$ (through $t_3 = 1$). We first consider NoPartLow. If $t_1 = t_2 = 1$, three take rates are possible and occur with probability $\frac{1}{3}$: $t \in \{\frac{7}{9}, \frac{8}{9}, 1\}$. Since the responder plays d_x in the second stage, the expected payoff of takers is

$$\pi_1 = \pi_2 = \frac{15}{54}e + \frac{1-x}{6}e.$$

Destruction can be avoided if one of the takers chooses a take rate $t_j = \frac{2}{3}$, which prevents $t = 1$.¹³ Again, three take rates are possible and occur with probability $\frac{1}{3}$: $t \in \{\frac{6}{9}, \frac{7}{9}, \frac{8}{9}\}$. The expected payoff of takers is

$$\pi_1 = \pi_2 = \frac{21}{54}e.$$

Note that taker payoffs depend on the group take rate, not on the individual take rate, and are therefore identical for both takers.

The strategy profiles $(t_1, t_2, d) = (1, 1, d_x)$ on the one hand and $(t_1, t_2, d) = (\frac{2}{3}, 1, d_x)$ or $(t_1, t_2, d) = (1, \frac{2}{3}, d_x)$ on the other hand lead to the same expected payoffs for the takers iff $x = \frac{1}{3}$ (which cannot occur since $x \notin \mathcal{D}$). If $x < \frac{1}{3}$, the takers' expected payoffs are higher for $t_1 = t_2 = 1$; if $x > \frac{1}{3}$, their payoffs are higher if one of them chooses a take rate of $\frac{2}{3}$.

Hence, if we restrict considerations to pure strategies, we find two sets of subgame perfect Nash equilibria. The first set is $(t_1, t_2, d) = (1, 1, d_x)$,

¹³In the absence of communication or repetition, of course, takers could coordinate on asymmetric strategy choices only by chance.

$x \in [0, \frac{1}{3}] \cap \mathcal{D}$. All these strategy profiles have an expected destruction rate of $\frac{x}{3}$ and are, therefore, inefficient if $x > 0$. The second set is $(t_1, t_2, d) \in \{(1, \frac{2}{3}, d_x), (\frac{2}{3}, 1, d_x)\}$, $x \in [\frac{1}{3}, 1] \cap \mathcal{D}$. These profiles have a zero expected destruction rate and are, therefore, efficient.

Analogous considerations apply to NoPartHigh. If $t_1 = t_2 = 1$, three take rates are possible and occur with probability $\frac{1}{3}$: $t \in \{\frac{8}{12}, \frac{10}{12}, 1\}$. Since the responder plays d_x in the second stage, the expected payoff of takers is

$$\pi_1 = \pi_2 = \frac{1}{4}e + \frac{1-x}{6}e.$$

If, however, one of the takers chooses a take rate $t_j = \frac{2}{3}$, the three equiprobable take rates are $t \in \{\frac{7}{12}, \frac{9}{12}, \frac{11}{12}\}$. The expected payoff of takers, then, is

$$\pi_1 = \pi_2 = \frac{9}{24}e.$$

If $x = \frac{1}{4} \in \mathcal{D}$, takers' expected payoffs are the same in both cases. Hence, we find the following two sets of equilibria: $(t_1, t_2, d) = (1, 1, d_x)$, $x \in [0, \frac{1}{4}] \cap \mathcal{D}$ and $(t_1, t_2, d) \in \{(1, \frac{2}{3}, d_x), (\frac{2}{3}, 1, d_x)\}$, $x \in [\frac{1}{4}, 1] \cap \mathcal{D}$. Equilibria in the first set are inefficient if $x > 0$. Equilibria in the second set are efficient.

Table 3 lists all subgame perfect pure strategy equilibria with (expected) take rates, (expected) destruction rates and (expected) payoffs.

Table 3: Subgame perfect pure strategy equilibria for all treatment groups

PartLow	Equilibrium	$(t_1, t_2, t_3, d) = (1, 1, \frac{1}{3}, d_x), x \in \mathcal{D}$
	Group take rate	$\frac{7}{9}$
	Expected destruction rate	0
	Payoffs	$\pi_1 = \pi_2 = \frac{7}{18}e, \pi_3 = \frac{4}{18}e$
PartHigh	Equilibrium	$(t_1, t_2, t_3, d) = (1, 1, \frac{1}{3}, d_x), x \in \mathcal{D}$
	Group take rate	$\frac{6}{9}$
	Expected destruction rate	0
	Payoffs	$\pi_1 = \pi_2 = \pi_3 = \frac{1}{3}e$
NoPartLow	Equilibrium	$(t_1, t_2, d) = (1, 1, d_x), x \in [0, \frac{1}{3}] \cap \mathcal{D}$
	Group take rate	$\frac{8}{9}$
	Expected destruction rate	$\frac{x}{3}$
	Payoffs	$\pi_1 = \pi_2 = \frac{24-8x}{54}e, \pi_3 = \frac{3-x}{27}e$
	Equilibrium	$(t_1, t_2, d) \in (1, \frac{2}{3}, d_x), (\frac{2}{3}, 1, d_x), x \in [\frac{1}{3}, 1] \cap \mathcal{D}$
	Group take rate	$\frac{7}{9}$
	Expected destruction rate	0
	Payoffs	$\pi_1 = \pi_2 = \frac{7}{18}e, \pi_3 = \frac{2}{9}e$
NoPartHigh	Equilibrium	$(t_1, t_2, d) = (1, 1, d_x), x \in [0, \frac{1}{4}] \cap \mathcal{D}$
	Group take rate	$\frac{10}{12}$
	Expected destruction rate	$\frac{x}{3}$
	Payoffs	$\pi_1 = \pi_2 = \frac{15-5x}{36}e, \pi_3 = \frac{3-x}{18}e$
	Equilibrium	$(t_1, t_2, d) \in (1, \frac{2}{3}, d_x), (\frac{2}{3}, 1, d_x), x \in [\frac{1}{4}, 1] \cap \mathcal{D}$
	Group take rate	$\frac{3}{4}$
	Expected destruction rate	0
	Payoffs	$\pi_1 = \pi_2 = \frac{3}{8}e, \pi_3 = \frac{2}{8}e$

2.3 Experimental procedures

In each experimental session, the following procedure was used. By randomly assigning a seat number, each participant was assigned a role (taker or responder) and was matched with two other participants whose identities were never revealed. The instructions were read, followed by some role-independent exercises intended to check participants' understanding of the procedures. Participants then learned about their role and played the game once. We framed the game as neutral as possible, avoiding any suggestive terms.

In stage 1, takers and responder or dummy chose take rates $t_j \in \mathcal{T}$. Additionally, we asked responders to indicate their preferred group take rate $t^{pref} \in \mathcal{T}$. In stage 2, responders learnt about the take rate chosen by their dummy (no-participation treatments) or were reminded of their own choice (participation treatments). Thus, responders in all treatments received formally the same information. However, as responders did not learn about the take rates chosen by the takers, they did not know the group's actual take rate t yet. Responders then had to choose their destruction rate d for each feasible group take rate t (strategy method).¹⁴ At the end of stage

¹⁴Note that responders were asked to make a decision on the destruction rate for any t . As responders knew at that time their own or the dummy's take rate choice, they might have eliminated take rates no longer feasible. For instance, if a responder/dummy had chosen $t_3 = 1$, it follows that $t \geq \frac{5}{9}$ in the low treatments and $t \geq \frac{2}{3}$ in the high treatments. Thus, choices on $d(t)$ for small take rates were hypothetical. For the following analysis,

2, each participant learned about the take rate chosen by the group, the corresponding destruction rate, and the resulting individual payoff. Thereafter, all participants filled in a post-experimental questionnaire concerning preferences, expectations and social background.

Overall, 348 undergraduates from the introductory microeconomics course at Saarland University participated in the experiment. Their fields of study were business administration (81.3%), economic education science (10.6%), business information management (3.2%), and other fields (4.8%). We had 54.6% male and 45.4% female participants. None of the participants had participated in an economic research experiment before.

Participants formed 116 groups of three players. Thus, we collected data from 232 takers and 116 responders. Each participant was assigned randomly to one of the four treatments and each treatment was applied to 87 participants (between-subject design). The experiment was computerized using z-Tree (Fischbacher 2007). We conducted twelve sessions, all on the same day (3 sessions of 12 participants each, 1 session of 24 participants, 8 sessions of 36 participants each).

Participants were paid in chips according to the decisions made. Every earned chip was exchanged for a lottery ticket. Lottery tickets determined participants' chance of winning the lottery prize of 500 Euro (binary lottery

we ignore this fact because, first, we focus on treatment effects which are not touched by these considerations, and second, main effects appear for high take rates and these are feasible for most responders.

mechanism). The lottery mechanism was carefully explained to the participants. In order to avoid tournament-type rewards, the number of tickets was fixed at 9 per group. In order to guarantee that one participant actually got the prize, remaining tickets of group 1 were randomly distributed among the members of group 2, remaining tickets of group 2 to group 3, and so on, with the remaining tickets of the last group going to the participants of the first group.¹⁵

As a show-up fee, every participant received some extra points for the compulsory introductory microeconomics exam. The experiment took about 25 minutes; thus, average earnings per hour were about 3.45 Euro. The average earnings in chips were 1.75 for takers and 1.63 for responders (see also table 4 for average earnings per treatment).

¹⁵For participants who maximize the expected utility of monetary payoffs, the binary lottery mechanism implies risk neutrality. See Roth and Malouf (1979), Berg et al. (1986). For a critical discussion of the mechanism, see Selten et al. (1999), for an experimental test, see, e.g., Prasnikar (1998). However, risk considerations do not play an important role in our context. We used the lottery mechanism mainly because we assumed that the chance of winning a large prize would make participation in the experiment more attractive. For the same reason, we wanted to make sure that the prize would actually go to some participant. This required the redistribution of “destroyed” tickets as explained in the text. Thus, the final number of lottery tickets a participant received resulted from two sources: earnings within the group, which provided the incentive to consider seriously how to decide; and a possible windfall profit from unearned tickets from other groups, which was unrelated to decision making within the participant’s own group.

3 Results

Table 4 shows means, standard deviations (sd), and medians of take rate proposals of takers (t_1 , t_2) and of responders (t_3), as well as of group take rates preferred by responders (t^{pref}), and of actual group take rates (t) for each of the four treatments and overall treatments. Furthermore, average earnings in chips for takers and responders are given. The last columns of tables 5 and 6 (on pp. 26 and 27) provide a summary of destruction rate data.

For all statistical tests, data from different treatments are independent as we applied a between-subject design. Take rates and destruction rates are measured on an interval scale. While we restrict choices to a small number of values, we might conjecture that hypothetical unrestricted choices would come from a continuous distribution; thus, we might treat the data as grouped data from a continuous distribution. Nevertheless, we cannot use normality assumptions since take and destruction rates are restricted to the unit interval; thus, approximating normality would only be possible with a very small variance.¹⁶ For these reasons, we use only nonparametric tests.

¹⁶We have tested and rejected normality in all relevant cases with the help of the one-sample Kolmogorov-Smirnov test.

Table 4: Take rates: overview

Variable		Treatment				overall
		PartLow	PartHigh	NoPartLow	NoPartHigh	
t_1, t_2	mean	0.805	0.822	0.764	0.822	0.803
	(sd)	(0.188)	(0.209)	(0.234)	(0.200)	(0.208)
	median	0.667	0.100	0.667	0.100	0.667
t_3	mean	0.506	0.425	0.644	0.655	0.557
	(sd)	(0.211)	(0.152)	(0.266)	(0.289)	(0.252)
	median	0.333	0.333	0.667	0.667	0.500
t^{pref}	mean	0.483	0.414	0.414	0.402	0.428
	(sd)	(0.191)	(0.145)	(0.145)	(0.137)	(0.157)
	median	0.333	0.333	0.333	0.333	0.333
t	mean	0.728	0.632	0.747	0.750	0.714
	(sd)	(0.125)	(0.103)	(0.142)	(0.151)	(0.139)
	median	0.777	0.583	0.778	0.750	0.667
mean earnings						
takers		1.76	1.66	1.62	1.97	1.75
responders		1.55	2.14	1.52	1.31	1.63

3.1 Take rates

Although take rates are not immediately relevant to our purposes, we take a closer look at them in order to exclude the possibility of freak results that would shed doubt on the validity of our experimental procedures.

Takers' choices. Averaging over all treatments, the mean take rate proposal of takers is 0.803. Remember that in our design, we have 2 takers and 1 responder forming a group. Thus, a take rate of about 80% means that each taker claims on average about 40% of responders' endowment e , leaving 20% to the responder. This take rate is completely in line with the findings of Bosman et al. (2006), who report for a game with three takers and three responders that takers claim on average 81% of the whole pie.¹⁷ Exactly the same proportion has been found by Reuben and van Winden (2008), who analyze a game with one taker and two responders, and nearly the same by Sutter et al. (2003), who report data of a two-person game and found a mean take rate of 78% of the whole pie. This number seems to be astonishingly robust throughout different group sizes and group compositions despite the fact that the overall take rate must be divided by the number of takers to

¹⁷In a standard power-to-take game, both sides, takers and responders, have an initial endowment, with responders' endowment being at stake. Thus, the reported mean take rate is not directly comparable to our result. Conversion based on the particular whole pie is necessary.

obtain the share claimed by the individual.¹⁸

The lowest take rate of $\frac{1}{3}$ was chosen by only 7.3% of the takers, whereas 44.4% decided in favor of $\frac{2}{3}$, and a narrow majority of 48.3% chose 1, which is the most obvious equilibrium strategy. However, in the NoPart treatments, a subgame perfect equilibrium can also be reached by one taker choosing 1 and the other taker choosing $\frac{2}{3}$. This suggests that we might observe a higher proportion of takers choosing $\frac{2}{3}$ in the NoPart treatments than in Part treatments. Actually, this is not the case: 43.1% of NoPart takers chose $\frac{2}{3}$ (47.4%: 1; 9.5%: $\frac{1}{3}$) whereas 45.7% of Part takers chose $\frac{2}{3}$ (49.1%: 1; 5.2%: $\frac{1}{3}$).

In order to evaluate whether takers take the applied decision rule into account when choosing a take rate, we compare takers' take rate proposals of the four treatments. For both high influence treatments, take rates are on average 0.822. For PartLow, the mean take rate is 0.805; for NoPartLow, it is 0.764. The Kruskal-Wallis test finds no evidence that the take rates from the four treatments come from different distributions ($p = .491$). A pairwise comparison using the two-sided Wilcoxon-Mann-Whitney test finds also no significant differences between takers' take rate choices (all six p -values above .186).

Low treatments allow for equal influence of all decision makers (2 takers and 1 responder/dummy), whereas High treatments privilege the respon-

¹⁸This robustness is even more surprising when taking into account that we restricted take rate proposals to three possibilities, $t_j \in \{\frac{1}{3}, \frac{2}{3}, 1\}$.

der/dummy. As this fact might cause takers to worry about the level of the resulting take rate, we tested the directional hypothesis that take rates of takers are higher in High than in Low treatments. We find a weakly significant difference between the pooled data of Low and High treatments using a one-sided Wilcoxon-Mann-Whitney test ($p = .085$). This effect seems to result mainly from a weakly significant difference between the NoPart treatments (NoPartLow vs NoPartHigh: $p = .102$, PartLow vs PartHigh: $p = .251$).

Take rate proposals of responders. Take rates chosen by responders (PartLow and PartHigh) and generated with the help of pseudo-random numbers (NoPartLow and NoPartHigh) are labeled as variable t_3 . The mean take rate for PartLow was 0.506, for PartHigh only 0.425. The average take rate of all responders was 0.466. Dummy mean take rates were 0.644 (NoPartLow) and 0.655 (NoPartHigh). The overall average was 0.557. The Kruskal-Wallis test shows a highly significant difference between all four groups ($p = .001$), thus providing evidence that not all samples came from the same distribution. This is not surprising since we used a uniform distribution for dummy take rate proposals in the NoPart-treatments; it just shows that responder proposals were not uniformly distributed.

By comparing PartLow with NoPartLow, we can also show that responders' choices are significantly different from dummies' take rate proposals (two-sided Wilcoxon-Mann-Whitney test, $p = .002$). The same is true for High treatments ($p = .043$).

As responders in the PartLow treatment might try to compensate their low level of influence (although they do not know that another treatment exists), the mean take rate for PartLow is expected to be higher than the mean take rate for PartHigh. In fact, average take rate proposals in the PartLow treatment are significantly higher (one-sided Wilcoxon-Mann-Whitney test, $p = .068$).

Responders' preferred group take rates. Most responders (72.4%) reported that they preferred a group take rate of $\frac{1}{3}$, which is the lowest possible take rate and maximizes their own experimental payoff. Almost all the others (26.7%) stated that their preferred group take rate was $\frac{2}{3}$, which leads to an equal distribution of the pie. Only one participant (0.9%) stated a preferred group take rate of 1. The average preferred take rate is 0.428, the median is $\frac{1}{3}$. PartLow responders prefer the highest take rate on average (0.483), whereas NoPartHigh responders show the lowest mean (0.402). The average preferred group take rates of the other two treatments are slightly higher (both average take rates are 0.414).

The median is $\frac{1}{3}$ for all treatments; not surprisingly, then, we cannot reject the null hypothesis that the median is the same in all four treatments (Kruskal-Wallis test, $p = .254$). A pairwise comparison of average preferred take rates in the four treatments shows no significant difference except for PartLow versus NoPartHigh (two-sided Wilcoxon-Mann-Whitney test, $p = .082$).

3.2 Destruction rates: aggregate results

The mean destruction rate in response to the actual group take rates was 0.294. This is slightly higher than the value of 0.208 observed by Bosman et al. (2006) and Hennig-Schmidt and Geng (2005). Of 116 responders, 81.9% used the possibility of destroying part of the pie. The average destruction rate is 0.289 over all choices. If the take rate is smaller than 1, destroying part of the pie is costly; nevertheless, 71.6% of responders destroy even in this case, with an average destruction rate of 0.234. This is in accordance with the common observation that people are prepared to punish even if punishment is costly to themselves.

However, 30.2% of the responders destroy part of the pie even if the take rate is at its minimum of $\frac{1}{3}$, although the average destruction rate of 0.095 is lowest in this case. This is in accordance with the observation that rejections in restricted ultimatum games occur even when the proposer cannot make a better offer (Falk et al. 2008). Even when the take rate is $\frac{2}{3}$, which implies equal shares of the pie for all and is seen to be the fair take rate by an overwhelming majority of 78.4% of all participants ($t = \frac{1}{3}$: 17.2%; $t = 1$: 4.3%), the average destruction rate is 0.216.

Nevertheless, cost considerations seem to be important for punishment. The average destruction rate for take rates over $\frac{2}{3}$ but below 1 is 0.384; at a take rate of 1, where punishment is costless, the destruction rate jumps to 0.662. Obviously, average destruction rates increase with increasing group

take rates.

Table 5 highlights proportions of responders classified by their destruction rate choice for any take rate. Average destruction rates for various take rates are reported in the last column of table 6.

Table 5: Responders' destruction rate choices (in %) for different take rates

	$d = 0$	$0 < d \leq 0.1$	$0.1 < d \leq 0.2$	$0.2 < d \leq 0.3$	$0.3 < d \leq 0.4$	$0.4 < d \leq 0.5$	$0.5 < d \leq 0.6$	$0.6 < d \leq 0.7$	$0.7 < d \leq 0.8$	$0.8 < d \leq 0.9$	$0.9 < d < 1$	$d = 1$
$t = \frac{3}{9}$	69.8	6.9	6.9	4.3	1.7	6.0	0.9	3.4	0	0	0	0
$\frac{3}{9} < t < \frac{6}{9}$	44.8	14.7	10.3	7.8	11.2	6.9	3.4	0.9	0	0	0	0
$t = \frac{6}{9}$	48.3	2.6	7.8	5.2	12.9	11.2	5.2	2.6	3.4	0	0	0.9
$\frac{6}{9} < t < 1$	30.2	2.6	9.5	4.3	6.0	10.3	6.9	12.1	7.8	5.2	0	5.2
$t = 1$	25.0	1.7	0.9	0	1.7	8.6	0.9	0	3.4	0.9	1.7	55.2
$\frac{3}{9} \leq t \leq 1$	43.6	5.7	7.1	4.3	6.7	8.6	3.5	3.8	2.9	1.2	0.3	12.3

Let us now tackle the related question whether there is a correlation between the levels of destruction and take rates. Hennig-Schmidt and Geng (2005) found that the null hypothesis of independence can be rejected at $p = .05$, indicating a positive correlation (Spearman's $\rho = .57$). In our data, the correlation is substantially smaller but more significant (Spearman's $\rho = .23$, $p = .006$).

Table 6: Destruction rates: overview

		treatment				overall
		PartLow	PartHigh	NoPartLow	NoPartHigh	
d_t with $t = \frac{3}{9}$	average	0.109	0.059	0.118	0.093	0.095
	(sd)	(0.186)	(0.143)	(0.204)	(0.193)	(0.182)
d_t with $\frac{3}{9} < t < \frac{6}{9}$	average	0.136	0.119	0.150	0.179	0.146
	(sd)	(0.186)	(0.164)	(0.200)	(0.187)	(0.184)
d_t with $t = \frac{6}{9}$	average	0.218	0.210	0.219	0.218	0.216
	(sd)	(0.235)	(0.256)	(0.275)	(0.264)	(0.255)
d_t with $\frac{6}{9} < t < 1$	average	0.343	0.406	0.384	0.404	0.384
	(sd)	(0.295)	(0.352)	(0.355)	(0.335)	(0.333)
d_t with $t = 1$	average	0.597	0.610	0.749	0.690	0.662
	(sd)	(0.433)	(0.446)	(0.411)	(0.452)	(0.435)
d_t with $\frac{3}{9} \leq t \leq 1$	average	0.269	0.273	0.308	0.305	0.289
d_t with $\frac{3}{9} \leq t < 1$	average	0.214	0.231	0.234	0.257	0.234

3.3 Destruction rates: treatment effects

A first analysis of destruction rate data shows almost no evidence in favor of our hypothesis that participation diminishes negative reciprocity.

Table 6 shows destruction rate choices for different take rates and for all treatment groups. Possible take rates differ between the High and Low treatments. In order to allow for a comparison, data are therefore grouped into five categories: $d(t = \frac{3}{9})$, $d(\frac{3}{9} < t < \frac{6}{9})$, $d(t = \frac{6}{9})$, $d(\frac{6}{9} < t < 1)$, $d(t = 1)$. We now consider the effects of participation and strength of influence on destruction rates. Since we hypothesized directional effects of participation and strength of influence on destruction rates, we apply one-sided tests throughout.

A visual inspection suggests that the distribution of destruction rates in each treatment group is similar in shape. We used the Kruskal-Wallis test to compare the four groups of data, finding that, for each take rate category, destruction rates do not differ significantly between the treatment groups.¹⁹

The picture does not change much when we look at pairwise comparisons of the four groups. First, we test whether observations on $d(t)$ in the PartLow treatment are smaller than in the NoPartLow treatment. Comparing average destruction rates of both treatments, we find that, for all take rates $t \in \mathcal{T}^{\text{Low}} \cup \mathcal{T}^{\text{High}}$ common to both treatments, $d(t)$ is higher in NoPartLow than in PartLow. However, according to the one-sided Wilcoxon-Mann-Whitney, these differences are not significant: all p -values are clearly higher than .05

¹⁹The smallest significance level $p = .181$ is found for $d(t = 1)$.

except for $d(t = 1)$, where $p = 0.054$.²⁰

When we look at the differences between PartHigh and NoPartHigh, we get similar results. Average destruction rates are higher in NoPartHigh than in PartHigh for any feasible take rate t (with the exception of $d(t = \frac{3}{4})$: mean destruction is 0.361 for PartHigh and 0.277 for NoPartHigh). According to the one-sided Wilcoxon-Mann-Whitney test, none of the differences is significant.

There is also no significant effect of the strength of influence. When comparing PartLow and PartHigh or, less interestingly, NoPartLow and NoPartHigh, the smallest p -value is $p = .78$, associated with $d(t = \frac{1}{3})$ in PartLow and PartHigh. There is even no evidence that strength of influence has any directional effect: for some t , destruction rates are higher in the Low treatments; for other t , destruction rates are lower in the Low treatments.

Since destruction rates from High and Low treatments do not differ in medians (Wilcoxon-Mann-Whitney test) nor in the distribution of values (Kolmogorov-Smirnov test), we have also pooled the data in two groups, Part (PartLow and PartHigh) and NoPart (NoPartLow and NoPartHigh). Again, we found mean destruction rates to be higher in NoPart than in Part, which we expected. Whereas the difference is positive for any $d(t)$, it ranges from

²⁰The test uses ranks and assumes that the scores come from a continuous distribution, where the probability of ties is zero. With discrete data, ties may occur, which happened in our case. In such cases, each of the tied observations is given the ranks they would have had if no ties had occurred. For our data, the proportion of ties is quite large and occurred between observations involving both groups. We applied the correction for ties.

very small (0.0074 for $d(t = \frac{6}{9})$) to relative large (0.1167 for $d(t = 1)$), with the other values in the range from 0.0189 to 0.0364. However, none of the differences is significant according to the one-sided Wilcoxon-Mann-Whitney test: the smallest p -value is 0.55 for $d(t = 1)$. There is also no significant difference when we analyze destruction rates for any feasible group take rate t instead of destruction rate categories.

3.4 Destruction rates: responder types

We found that average destruction rates increase with increasing group take rates but do not differ significantly among the treatments. Thus, we find negative reciprocity on average but no treatment effects. However, averages may mask substantial heterogeneity in individual behavior. Non-reciprocal responder types are not influenced by our treatments. If these types are frequent enough, treatment effects on reciprocal types may not show up in the aggregate.

In accordance with other experimental studies, we distinguish four types of responders: the *homo oeconomicus* (type 0), the unconditional cooperator (type 1), the *homo reciprocans* (type 2), and the erratic type (type 3). This classification of responder behavior is not ad hoc. It is known from other experiments that types 0 and 1 occur, although their behavior is untypical for average human behavior in many situations. Type 2 behavior covers the kind of negative reciprocity we observe, on average, in basic power-to-take games.

Type 3 is the most problematic type. It is known from many experiments that there are almost always participants who are confused or not motivated to decide carefully. However, we cannot exclude the possibility that the group of type 3 responders contains participants who have understood the instructions and decided carefully but just had aims we failed to understand.

These types differ in terms of their responder behavior, which we describe, as before, by an n -tuple of destruction rates $(d_1, d_2, \dots, d_n) \in \mathcal{D}^n$, where d_j is the type's response to take rate t_j , $t_1 < t_2 < \dots < t_n$, and where the number n of possible take rates and their values depend on the treatment. Specifically, we consider the following strategies:

- $d_0 := (0, \dots, 0)$ (never destroy)
- $d_+ := (0, 0, \dots, 0, x)$ with $x > 0$, $x \in \mathcal{D}$ (destroy iff this is costless)
- $d_{++} := (d_1, d_2, \dots, d_n)$ with $d_1 \leq d_2 \leq \dots \leq d_n$ and $d_{n-1} > 0$ (nondecreasing, destroy in spite of costs)
- $d_- := (d_1, d_2, \dots, d_n)$ with $d_{j+1} < d_j$ for at least one j (nonmonotonic)

According to the type definitions, types play only certain strategies. Type 0 maximizes its experimental payoff, which implies d_0 or d_+ . Type 1 is always cooperative and therefore plays d_0 . Type 2 shows (negative) reciprocity, that is, plays d_+ or d_{++} . Type 3 may play any strategy; however, if there is a strong random influence on the behavior of type 3, it will most likely play d_- , because this is the largest class of strategies.

Table 7: Responder types

	Treatment				Total
	PartLow	PartHigh	NoPartLow	NoPartHigh	
Type 0/1	8 (6.9%)	7 (6.0%)	7 (6.0%)	7 (6.0%)	29 (25.0%)
Type 2	15 (12.9%)	18 (15.5%)	13 (11.2%)	14 (12.1%)	60 (51.7%)
Type 3	6 (5.2%)	4 (3.4%)	9 (7.8%)	8 (6.9%)	27 (23.3%)

Hence, we can tentatively classify types on account of their behavior. We classify users of d_0 and d_+ as type 0/1, users of d_- as type 3, and users of d_{++} as type 2. We then validate this classification by looking at some relevant statistics.

On the basis of this classification, table 7 lists the observed frequency of the three responder types by treatments and over all treatments. Overall, type 2 responders constitute the majority (slightly above 50%), while 25% of the responders are of type 0/1 and the rest (not quite 25%) is of type 3. The distribution of types over treatment groups does not differ significantly from a uniform distribution (chi-square test, $p = .781$). Thus, types seem to be exogenous, which supports our type classification.

Destruction decisions also differ substantially among these types. Median destruction rates of type 0/1, type 2, and type 3 players are significantly

different for any possible group take rate smaller than 1 ($p < .001$, three-sample Median test).²¹ Thus, types differ not only in their strategy choices but also in their reactions to each group take rate below 1. This is not implied by the type definition and suggests that our types are really different. A pairwise comparison of destruction choices of type 0/1 with type 2 as well as with type 3 yields the expected results: for any take rate $t < 1$, type 0/1 responders destroy significantly less (Wilcoxon-Mann-Whitney test, one-sided, $p < .004$). Even more interesting results provides a comparison between type 2 and type 3 responders. Type 2 players destroy less than type 3 responders on average when confronted with $t \leq \frac{5}{9}$ and more as reaction to $t > \frac{5}{9}$. For some small and some high take rates, destruction decisions are significantly different, for others (especially medium take rates), they are not (Wilcoxon-Mann-Whitney test, two-sided), but there seems to be no meaningful pattern.

Moreover, types differ with regard to the correlation between take rate and destruction rate. As already explained, we found a significantly positive correlation in the aggregate. For type 0/1 responders, we find, trivially, a positive and significant correlation (Spearman's $\rho = .312$, $p = .050$ one-sided), whereas for type 3 responders, we find a negative, non-significant correlation (Spearman's $\rho = -.175$, $p = .384$ two-sided). For type 2 responders, the correlation between take and destruction rate is positive by definition of the

²¹For $t = 1$, the Median test could not be performed because there were no cases above the median.

type, large, and highly significant (Spearman's $\rho = .697$, $p < .001$ one-sided).

The hypothesis that participatory decision making reduces negative reciprocity concerns only the reciprocal type 2; thus, we focus on type 2 responders, who also form a clear majority. Table 8 provides the following data for type 2 responder behavior in Part and NoPart treatments: number of type 2 responders (n)²², mean destruction rate and standard deviation (sd).

For low and medium take rates ($t \leq \frac{9}{12}$), we observe no clear-cut effect concerning mean destruction rates between both treatments. However, for $t > \frac{9}{12}$, mean destruction rates of responders in the NoPart treatments clearly exceed destruction rates in the Part treatments. This is in line with the hypothesis that responders allowed to participate in the decision-making process destroy smaller shares of the pie than NoPart-responders. The Wilcoxon-Mann-Whitney test (one-sided) shows that mean destruction rates of Part-responders are significantly lower than of NoPart-responders for $t = \frac{8}{9}$ ($p = .026$), $t = \frac{11}{12}$ ($p = .044$), and $t = 1$ ($p = .021$). The last column of table 8 provides the corresponding p -values (one-sided) for any possible group take rate t .

Thus, we find that participation matters for type 2 responders. For low and medium group takes rates ($t \leq \frac{9}{12}$), the destruction rates do not differ significantly between Part and NoPart treatments ($p \geq .151$).

²²Number of responders differ within the Part and NoPart treatments because some group take rates are only possible in either the High or Low treatments while others are possible in both.

Table 8: Destruction rates of type 2 responders

Take rate	Treatment						<i>p</i>
	Part			NoPart			
	<i>n</i>	mean	sd	<i>n</i>	mean	sd	
$t = \frac{3}{9}$	33	0.054	0.117	27	0.042	0.118	.151
$t = \frac{5}{12}$	18	0.077	0.143	14	0.120	0.134	.155
$t = \frac{4}{9}$	15	0.125	0.149	13	0.077	0.155	.152
$t = \frac{6}{12}$	18	0.156	0.181	14	0.207	0.197	.241
$t = \frac{5}{9}$	15	0.182	0.184	13	0.165	0.189	.379
$t = \frac{7}{12}$	18	0.222	0.244	14	0.283	0.240	.261
$t = \frac{6}{9}$	33	0.314	0.247	27	0.319	0.266	.485
$t = \frac{9}{12}$	18	0.478	0.332	14	0.468	0.285	.500
$t = \frac{7}{9}$	15	0.452	0.238	13	0.532	0.311	.266
$t = \frac{10}{12}$	18	0.534	0.311	14	0.567	0.315	.380
$t = \frac{8}{9}$	15	0.626	0.212	13	0.784	0.184	.026
$t = \frac{11}{12}$	18	0.657	0.300	14	0.835	0.202	.044
$t = 1$	33	0.824	0.273	27	0.960	0.137	.021

However, for high group take rates ($t > \frac{9}{12}$), destruction rates in Part are significantly lower than destruction rates in NoPart ($p \leq .044$). Pooling data for $t = \frac{8}{9}$ and $t = \frac{11}{12}$ (which both result from the same action profile $(t_1, t_2, t_3) = (1, 1, \frac{2}{3})$ and differ only in the weight of t_3 due to treatment variation) yields a highly significant difference ($p = .0005$).

We do not find, however, that there is a significant difference in type 2 responder behavior between PartLow and PartHigh. The smallest p -value is $p = .259$, associated with $d(t = \frac{1}{3})$ (Wilcoxon-Mann-Whitney test, one-sided). Nevertheless, strength of influence seems to have a directional effect: for any t being feasible in both treatments (i.e. $t \in \{\frac{1}{3}, \frac{2}{3}, 1\}$), destruction rates are lower in PartHigh.

4 Conclusion

This paper reports data from a laboratory experiment on procedural aspects of decision making. The literature shows that an individual's willingness to accept unfavorable decisions, without resorting to negatively reciprocal behavior like punishment or revenge, may depend on the perceived fairness of the decision procedure. We have focused on a specific aspect of procedural fairness, namely, participation. In our experiment, participation does indeed increase acceptance of unfavorable decisions for the majority of participants who show reciprocal behavior. Participatory decision making had no effects

in the case of moderately unfavorable decisions, but there were significant effects in the case of highly unfavorable decisions.

Acknowledgements

We are grateful to Henrik Egbert, Stefan Klössner, Hannes Koppel, Jürgen Meckl, Ernesto Reuben, Carsten Schmidt, Birgit E. Will, Frans van Winden and participants at the *International Meeting of the Society for Social Choice and Welfare* 2006 in Istanbul, the *Econometric Society European Meeting* 2007 in Budapest, and the *Brown Bag Seminar* 2007 in Giessen. Financial support by the *Vereinigung der Freunde der Universität des Saarlandes* is gratefully acknowledged.

References

- Albert, M., W. Güth, E. Kirchler, and B. Maciejovsky (2007).** “Are We Nice(r) to Nice(r) People? An Experimental Analysis.” *Experimental Economics*. 10, 53–69.
- Anand, P. (2001).** “Procedural Fairness in Economic and Social Choice: Evidence from a Survey of Voters”. *Journal of Economic Psychology*. 22, 247–270.
- Berg, J., L. Daley, J. Dickhaut, and J. O’Brien (1986).** “Controlling Preferences for Lotteries on Units of Experimental Exchange.” *The Quarterly Journal of Economics*. 2, 282–306.
- Bohnet, I. (2006).** “How Institutions Affect Behavior: Insights From Economics and Psychology.” In D. De Cremer et al. (eds.), *Social Psychology and Economics*. London: Lawrence Erlbaum, 213–238.
- Bolton, G.E., J. Brandts, and A. Ockenfels (2005).** “Fair Procedures. Evidence from Games Involving Lotteries.” *Economic Journal*. 115, 1054–1076.
- Bosman, R., H. Hennig-Schmidt, and F. van Winden (2006).** “Exploring Group Decision Making in a Power-to-Take Experiment.” *Experimental Economics*. 9, 35–51.
- Bosman, R. and F. van Winden (2002).** “Emotional Hazard in a Power-to-Take Experiment.” *Economic Journal*. 112, 147–169.
- Brandts, J. and G. Charness (2000).** “Hot vs. cold: Sequential Responses and Preference Stability in Experimental Games.” *Experimental Economics*. 2, 227–238.
- Brockner, J. and B.M. Wiesenfeld (1996).** “An Integrating Framework for Explaining Reactions to Decisions. Interactive Effects of Outcomes and Procedures.” *Psychological Bulletin*. 120, 189–208.
- Brosig, J., J. Weimann, and C.-L. Yang (2003).** “The Hot versus Cold Effect in a Simple Bargaining Experiment.” *Experimental Economics*. 6, 75–90.
- Burlando, R. and F. Guala (2005).** “Heterogeneous Agents in Public Goods Experiments.” *Experimental Economics*. 8, 35–54.
- Cox, J. and C. Deck (2005).** “On the Nature of Reciprocal Motives.” *Economic Inquiry*. 43, 623–635.
- Dohmen, T., A. Falk, D. Huffman, and U. Sunde (2006).** “Homo Reciprocans: Survey Evidence on Prevalence, Behavior and Success.” IZA Discussion Papers 2205. Institute for the Study of Labor (IZA), Bonn.

- Elster, J.** (1989). *Solomonic Judgements*. Cambridge: Cambridge University Press.
- Falk, A., E. Fehr, and U. Fischbacher** (2008). “Testing Theories of Fairness - Intentions Matter.” *Games and Economic Behavior*. 62, 287–303.
- Falk, A. and U. Fischbacher** (2006). “A theory of reciprocity.” *Games and Economic Behavior* 54, 293-315.
- Fischbacher, U.** (2007). “Z-Tree: Zurich Toolbox for Ready-made Economic Experiments.” *Experimental Economics*. 10, 171–178.
- Fischbacher, U. and S. Gächter** (2006). “Heterogenous Social Preferences and the Dynamics of Free Riding in Public Goods.” CeDEx Discussion Paper No. 2006-01. University of Nottingham.
- Fischbacher, U., S. Gächter, and E. Fehr** (2001). “Are People Conditionally Cooperative? Evidence from a Public Goods Experiment.” *Economics Letters*. 71, 397–404.
- Folger, R.** (1977). “Distributive and Procedural Justice: Combined Impact of ‘Voice’ and Improvement of Experienced Inequity.” *Journal of Personality and Social Psychology*. 35, 108–119.
- Folger, R., D. Rosenfield, J. Grove, and L. Corkran** (1979). “Effects of Voice and Peer Opinions on Responses to Inequity.” *Journal of Personality and Social Psychology*. 37, 2253–2261.
- Frey, B.S., M. Benz, and A. Stutzer** (2004). “Introducing Procedural Utility: Not Only What, But Also How Matters.” *Journal of Institutional and Theoretical Economics*. 160, 377–401.
- Goeree, J., C. Holt, and S. Laury** (2002). “Private Costs and Public Benefits: Unrevealing the Effects of Altruism and Noisy Behavior.” *Journal of Public Economics*. 83, 255–276.
- Greenberg, J.** (1990). “Employee Theft as a Reaction to Underpayment Inequity: The Hidden Cost of Pay Cuts.” *Journal of Applied Psychology*. 75, 561–568.
- Grimalda, G., A. Kar, and E. Proto** (2007). “On the Value of Participation: Endogenous Emergence of Social Norms in a Three-Player Ultimatum Game.” Munich Personal RePEc Archive (MPRA), Paper No. 1620.
- Güth, W., R. Schmittberger, and B. Schwarze** (1982). “An Experimental Analysis of Ultimatum Bargaining.” *Journal of Economic Behavior and Organization*. 3, 367–388.
- Hennig-Schmidt, H. and H. Geng** (2005). “Über Emotionen und Klugheit in impliziten Verhandlungen - Eine experimentelle Untersuchung anhand des Power-to-take Spiels.” In A. Scherzberg et al. (eds.), *Kluges Entscheiden*. Tübingen: Mohr Siebeck, 91–107.

- Konovsky, M.A.** (2000). "Understanding Procedural Justice and its Impact on Business Organizations." *Journal of Management*. 26, 489–511.
- Kurzban, R.** and D. Houser (2005). "Experiments Investigating Cooperative Types in Humans: A Complement to Evolutionary Theory and Simulations." *Proceedings of the National Academy of Sciences*. 102, 1803–1807.
- Leventhal, G.S.** (1980). "What Should Be Done With Equity Theory? New Approaches to the Study of Fairness in Social Relationships." In K.J. Gergen et al.(eds.), *Social Exchange: Advances in Theory and Research*. New York: Plenum Press, 27–53.
- Lind, E.A.** and T.R. Tyler (1988). *The Social Psychology of Procedural Justice*. New York: Plenum Press.
- Lowe, R.H.** and S.J. Vodanovich (1995). "A Field Study of Distributive and Procedural Justice as Predictors of Satisfaction and Organizational Commitment." *Journal of Business and Psychology*. 10, 99–114.
- Olkkonen, J.** and J. Lipponen (2006). "Relationships between Organizational Justice, Identification with Organization and Work Unit, and Group Related Outcomes." *Organizational Behavior and Human Decision Processes*. 100, 202–215.
- Prasnikar, V.** (1998). "How Well Does Utility Maximization Approximate Subjects' Behavior? An Experimental Study." Working Paper. University of Pittsburgh, Department of Economics.
- Reuben, E.** and F. van Winden (2008). "Social Ties and Coordination on Negative Reciprocity: The Role of Affect." *Journal of Public Economics*. 92, 34–53.
- Roth, A.E.** and M.W.K. Malouf (1979). "Game-theoretic Models and the Role of Bargaining." *Psychological Review*. 86, 574–594.
- Selten, R.** (1967). "Die Strategiemethode zur Erforschung des eingeschränkt rationalen Verhaltens im Rahmen eines Oligopolexperiments." In H. Sauermann (ed.), *Beiträge zur experimentellen Wirtschaftsforschung*. Tübingen: J.C.B. Mohr (Paul Siebeck), 136–168.
- Selten, R., A. Sadrieh,** and K. Abbink (1999). "Money Does Not Induce Risk Neutral Behavior, but Binary Lotteries Do Even Worse." *Theory and Decision*. 46, 211–249.
- Sutter, M., R. Bosman, M. Kocher,** and F. van Winden (2003). "Experimental Evidence of the Importance of Gender Pairing in Bargaining." *Discussion Papers on Strategic Interaction* 27. Max Planck Institute of Economics, Strategic Interaction Group, Jena.
- Thibaut, J.** and L. Walker (1975). *Procedural Justice: A Psychological Analysis*. Hillsdale, NJ: Lawrence Erlbaum Associates.

- Tyler, T.R.** (1990). *Why People Obey the Law*. New Haven: Yale University Press.
- Tyler, T.R., R.J. Boeckmann, H.J. Smith, and Y.J. Huo** (1997). *Social Justice in a Diverse Society*. Boulder: Westview Press.
- Tyler, T.R. and D. De Cremer** (2005). "Process-based Leadership: Fair Procedures and Reactions to Organizational Change." *Leadership Quarterly*. 16, 529–545.
- Tyler, T.R. and E.A. Lind** (2000). "Procedural Justice." In J. Sanders and V.L. Hamilton (eds.), *Handbook of Justice Research in Law*. New York: Kluwer Academic/Plenum Publishers, 65–92.