

The Effects of Procedures on Decision Making

– Experimental Evidence –

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Chapter 1

Introduction

There is substantial empirical evidence and increasing recognition that not only outcomes, but also the procedures leading to them, can affect people's utility from, and their reactions to, those decisions. Indeed, procedures have been shown to matter in a broad range of areas and settings. For instance, in organizational contexts, it has been shown that procedures influence negative employee behavior, e.g. theft (*Greenberg, 1990*), as well as employees' job satisfaction and organizational commitment (*Lowe and Vodanovich, 1995*), organizational change (*Tyler and De Cremer, 2005*), turnover intentions (*Olkkonen and Lipponen, 2006*) or mentoring relationships (*Richard et al., 2002*).

Procedural aspects within polity and society have also been shown to be important. For example, government policies which aim at overcoming individuals' resistance to controversial projects (e.g., the construction of power plants) tend to be most successful if people feel that the process honors their concerns and therefore perceive the procedure as fair (*Oberholzer-Gee et al., 1995*). Research on tax compliance and tax evasion has also shown that the way taxpayers are treated, e.g. with respect and dignity, significantly influences their willingness to pay taxes (*Feld and Frey, 2002, 2007*). Besides, people's willingness to engage in cooperative actions within groups, organizations and societies has also been shown to significantly depend on procedural judgments (*Tyler and Blader, 2000*).

Another important area where procedures are of prime importance is law. Various studies found that people react adversely to unfair legal procedures, irrespective of the objective judgment made by the court. People rather obey a decision if they regard the authority that made that decision as legitimate and entitled to be obeyed, irrespective of their own judgment about the decision (*Tyler and Lind, 2000; Tyler and Mitchell, 1994*). Thus, the effectiveness of legal authorities seems to depend upon citizens' procedural

fairness judgments.

However, there is still a large gap between numerous studies by non-economists (e.g., psychologists, sociologists, political scientists, and legal scholars) and the fact that economists began to investigate the role of procedures only recently. Some rare attempts to account for individuals' procedural concerns include recent work on procedural utility, notably done by *Frey* and colleagues (see, e.g., *Frey et al.*, 2004; *Frey* and *Stutzer*, 2001).

Besides, procedural aspects have not only been demonstrated to matter in various settings but also in a wide range of methodologies, including panel surveys, questionnaire studies, and psychometric work. However, research on procedures rarely made use of economic experiments. Only a few procedural aspects have been studied so far, the most notable ones are the roles of appointment procedures (e.g., *Brandts et al.*, 2006; *Grimalda et al.*, 2008a), intentions (e.g., *Charness*, 2004; *Offerman*, 2002; *Falk et al.*, 2008), and procedural fairness (e.g., *Bolton et al.*, 2005; *Grimalda et al.*, 2008b).

I would argue that, due to clear evidence for procedures influencing human decision-making, economists can not remain silent about procedural aspects of strategic interactions any longer: not only outcomes shape human behavior, the way in which decisions are taken do as well. There is an important research agenda to be developed. Behavioral economics may serve as an important link. Its aim is to integrate the insights of cognitive and social psychologists, as well as those of experimental economists, with neoclassical economic theory. This dissertation intends to contribute to it by using an experimental approach. My objective is to analyze possible effects of (a) procedural fairness judgements, (b) procedural satisfaction, and (c) participation opportunities. These three topics are closely related to each other. Most of the previous research about procedural effects is concerned with procedural fairness to which I intend to contribute. In addition, I introduce the notion of procedural satisfaction, as it not only implies procedural fairness, but also goes beyond it: individuals may be satisfied with the use of a particular procedure because they judge it to be fair for example. This concept is inspired by *Dietrich's* (2005) claim for 'Procedural Autonomy'. Rather than looking for the 'right' procedure from a procedural, epistemic, or different perspective, he suggests that a group should decide how to aggregate judgments. In short, if the group wants a monarchy it will get a monarchy, if it wants a particular type of democracy it will get this type of democracy, etc. When asking individuals which procedure is right to use, I aim at eliciting procedural preferences without being interested in identifying people's underlying motivation. The third topic of interest is concerned with participation which is also closely related to procedural fairness. According to the vast body of literature about procedural fairness, a fair decision-making

procedure requires one or more criteria to be met. Among other rules of fair processes, *Leventhal* (1980) mentions representativeness, i.e., representation of the affected parties in the decision making process where representation rights may vary from voice to participation. A minimum requirement of procedural fairness 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. In many cases, people want more than just a voice, they want the possibility of participation through, for instance, voting or vetoing. *Tyler et al.* (1997) consider a process to be fair if people can participate in the process and are heard.

In particular, I conducted (or took part in conducting) three economic experiments to test whether:

1. individuals rather accept an outcome resulting from a fair procedure than from an unfair one;
2. individuals rather accept an outcome resulting from a preferred procedure than from a non-preferred one;
3. individuals rather claim a smaller share of a group endowment if they have been appointed by the group to be proposers than if the group has dismissed the procedure used to lead to the appointment;
4. individuals rather accept an outcome if they have the opportunity to participate in the decision-making process concerning the distribution of a group endowment.

The remainder of this dissertation is structured as follows: it is divided into two parts (theoretical foundations and experimental evidence), and consists of five main chapters (chapters 2 to 6) and a conclusion (chapter 7). Each main chapter is based on a separate work, being composed of an introduction, a main body, and a conclusion. Chapters 1 to 4, as well as chapter 7, were written by myself; chapters 5 and 6 are based on co-authored work.

Chapter 2 contains a literature survey on procedural preferences. I review findings from the social sciences that have a potential application for human decision-making, present a classification, and discuss the most prominent theories in the field. As the predestined method to study these procedural effects in the realm of economics seems to be the experimental method, I discuss these previous findings in detail. Finally, I present recent efforts to integrate individuals' procedural concerns into economic models. This chapter is based on *Mertins* (2008b).

Chapter 3 discusses two methodological issues related to the conducted experiments. In the first part, I survey methods of inducing players to reveal their preferences and review experimental findings that shed light on the question whether individuals' decisions depend on the elicitation method. In the second part, I discuss various methods to classify people according to player types and consider whether this approach may assist to a better understanding of behavior in experimental games.

Then, the three experiments are presented with a separate chapter being dedicated to each of them. Chapter 4 builds upon *Mertins* (2008a), whereas chapter 5 is based on *Albert et al.* (2008). Both studies should be seen in connection with each other as they are part of a greater research program.¹ In chapter 4, I present an experimental game with one proposer who has distributive power over a pie and four responders who can invest in resisting the proposer's demand. I allowed each group of five participants to express their individual preferences concerning two feasible appointment procedures for choosing a proposer among them. I then told them the procedure preferred by the group's majority and the procedure actually used, which either was the procedure preferred by the majority or not. Besides, I elicited participants' fairness evaluations regarding both procedures and studied whether responders' resistance to various demands were affected by their procedural fairness judgments and their procedural satisfaction. Thus, the experiment is dedicated to answer research questions one and two.

A similar experimental design is used in chapter 5 which is based on *Albert et al.* (2008). We used the same threshold public good game with one proposer and four responders and elicited individuals' procedural preferences. Then, we studied whether participants' behavior differs with respect to procedural satisfaction. The experimental design and procedures differ in two main aspects. First, in the modified design, procedural preference elicitation and treatment allocation takes place before roles are allocated and pay-off relevant decisions are made.² Thus, the original design has the ability to an-

¹Before conducting both experiments, I conducted a pilot, the results of which can be found in *Mertins* (2005). *Mertins* (2005) and *Mertins* (2008a) differ mainly in the applied payment scheme: The first study applies the rather seldom used binary lottery mechanism with high and low prizes being gifts, whereas the second one uses a standard payoff scheme. With this modification, we intended to comply with the common publication requirements (see, e.g., *Guala*, 2005). Besides, there are two follow-up papers which are to be finished in the near future: *Albert et al.* (2009b) analyze exactly the same experimental game as *Albert et al.* (2008) within a culturally different subject pool consisting of German students. *Albert et al.* (2009a) discuss the significantly different findings between Bulgarian and German students' behavior alongside a cross-cultural line of reasoning.

²Consult figures 4.1 and 5.1 to realize the differences between the sequences of events in both experiments.

alyze responders' behavior depending on procedural judgments, whereas the modified design is additionally able to capture proposers' behavior. Thus, by using the modified design, we can also analyze responders' behavior and compare it with previous findings (e.g. by *Brandts et al., 2006*). Moreover, the responders' decision situation differs in the information given. In both settings, responders can be responsive to proposers' claims. However, in the original design, proposers decide on a claim without knowing whether the group has supported their appointment or not. This information may affect proposers' decisions. In turn, responders may evaluate proposers' claims differently, depending on the information available to proposers. Second, the subject pool does not consist of (mostly) German students and visitors of an University's Open Day, but of students from a Bulgarian University. People from transitional countries are expected to show stronger reactions to procedural dissatisfaction (see e.g., *Tontrup and Gaissmaier, 2008*). The experimental results shed light on question two and three. Note that due to the modifications in the experimental design and procedures, it is rather difficult to compare findings from both studies.

Chapter 6 aims at providing answers to research question four, i.e., does participatory decision making increase acceptance of (unfavorable) decisions? The experimental game consists of 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, responders can punish greedy takers, but only at a cost to themselves. Participation is implemented by letting the responder participate in takers' transfer decision. The study considers the effect of participation on the destruction rate. Chapter 7 summarizes the main results and concludes.

Part I

Theoretical foundations

Chapter 2

Procedural preferences: A survey

2.1 Introduction

Experimental economics is among the fastest growing areas of economic research. The field has evolved over the past decades both in terms of the methods it employs and the increasing range of economic phenomena it addresses. A topic currently attracting growing attention is the issue of procedures, i.e. the way that results in a certain outcome. Indeed, any decision in human interactions is inherently associated with a procedure. It is impossible to take a decision without deciding first on *how* to take it (*Sebald, 2007a*).

Judgments about a procedure might entail questions such as the following (*Deutsch, 1975*): Who gets to divide the money? Why? How were these things decided? To what degree did each party take part in the decision-making? Various procedural aspects that affect human decision-making have been identified. All of them show that people seem to care not only about outcomes themselves but also about how they emerged in the first place. With their pioneering research, *Thibaut* and *Walker* (1975) established the procedural fairness hypothesis¹: Disputants are more satisfied with and willing to voluntarily accept decisions that are the result of a fair procedure. A large literature found procedural fairness judgments having a distinct influence upon the acceptance of decisions and obedience toward rules, policies, and laws, as well as upon people's willingness to engage in cooperative actions

¹Following *Greenberg* (1990), procedural fairness is concerned with the perceived fairness of the process or system by which distribution decisions are made and implemented. Economists mostly speak of *procedural fairness*, while psychologists and lawyers seem to prefer the term *procedural justice*. We consider these expressions to be synonyms (see also *Van den Bos* and *Lind*, 2002).

within groups, organizations and societies.

While experimental economists only recently started to address procedural questions, this literature has grown enormously in other social sciences. Indeed, procedures have been shown to matter in a broad range of areas and settings (for an overview, see *Frey and Stutzer* (2001) and *Frey et al.* (2004)).²

In the realm of economics, procedures are of particular importance for analyzing the behavior of consumers and workers. In the case of consumers it has been shown in a survey-based study by *Maxwell* (2002) that the knowledge of how a price is determined has a significant effect on how the price is being perceived. In organizational contexts, there exists a large literature on distinct concerns for procedural fairness (see *Cohen-Charash and Spector* (2001) and *Konovsky* (2000) for reviews). For example, procedural fairness evaluations influence negative employee behavior, e.g. theft (*Greenberg*, 1990), as well as employees' job satisfaction and organizational commitment (*Lowe and Vodanovich*, 1995), organizational change (*Tyler and De Cremer*, 2005), turnover intentions (*Olkkonen and Lipponen*, 2006), mentoring relationships (*Richard et al.*, 2002), and may serve as indicator for trust (*Lengfeld and Krause*, 2006). Whereas fair procedures generally trigger positive responses, the opposite may be true as well. The implementation of participation in the decision-making process (usually assumed to increase procedural fairness) has been shown to result in negative effects: when decision makers fail to respond to inputs, perceptions of unfairness may be higher than if inputs had not been solicited at all (*Greenberg and Folger*, 1983).

Procedural aspects within polity and society have also been shown to be important. It is, for example, a well-known fact that people expect a fair political process (like having the right to participate) within democratic institutions. For example, government policies which aim on overcoming individuals' resistance to controversial projects (e.g. the construction of power plants) tend to be most successful if people feel that the process honors their concerns and is therefore perceived as fair (*Oberholzer-Gee et al.*, 1995). Research on tax compliance and tax evasion has also shown that the way taxpayers are treated, e.g. with respect and dignity, significantly influences their willingness to pay taxes (*Feld and Frey*, 2002, 2007). Last but not least, people's willingness to engage in cooperative actions within groups, organizations and societies has been shown to significantly depend on procedural fairness judgments (*Tyler and Blader*, 2000).

²Procedural aspects have not only been demonstrated to matter in various settings, but also in a wide range of methodologies (including panel surveys, questionnaire studies, and psychometric work) and cultures (see *MacCoun* (2005) for a survey).

Another important area where procedures are of prime importance is law. Various studies found that people react adversely to unfair legal procedures, irrespective of the objective judgment made by the court. People rather obey a decision if they regard the authority that made the decision as legitimate and entitled to be obeyed, irrespective of their own judgment about the decision (*Tyler and Lind, 2000; Tyler and Mitchell, 1994*). Indeed, people seem to link their fairness evaluation of a procedure with the legitimacy of authorities and to make their subsequent compliance behavior dependent thereon (see *Tyler et al. (1997)* for an overview). Thus, the effectiveness of legal authorities depends upon citizens' procedural fairness judgments.

We review some of these aspects in more detail below. This survey article is intended to give the reader an idea of the large gap between the existing evidence on procedural concerns provided mostly by psychologists and the neglect thereof by standard economic theory. Behavioral economics may serve as an important link. It aims on integrating insights derived by cognitive and social psychologists as well as experimental economists with neoclassical economic theory. We argue that experimental economics, a method of empirical investigation in a highly controlled environment, should increase its efforts to identify procedural effects and thus help enrich the standard economic model. In order to highlight the need for rethinking the standard economic consequentialist approach we review findings from the social sciences that have a potential application for human decision-making, present a classification, and discuss the most prominent theories in the field (section 2.2). As the predestined method to study these procedural effects in the realm of economics seems to be the experimental method, we discuss previous findings in section 2.3. Section 2.4 briefly presents recent efforts to integrate individuals' procedural concerns into economic models, and section 2.5 offers a conclusion.

2.2 Procedures in social sciences

There is substantial empirical evidence and increasing recognition that not only outcomes but also procedures leading to them can affect people's utility from and their reactions to those decisions. However, there is still a large gap between a vast number of empirical, experimental, and theoretical studies by non-economists and the fact that economists begun to investigate the role of procedures only recently. Whereas many social scientists, from psychologists and sociologists to political scientists and legal scholars, early started to raise procedural issues, economists have remained silent for a long time. Today, there is increasing recognition that rethinking the standard economic

consequentialist approach is necessary. *Sen* (1995, 1997), for example, has repeatedly argued that economic models should combine preferences for outcomes with those for processes.

In this chapter, we focus on the basic findings in the social sciences, thereby paying special attention to fundamental psychological research on procedural effects. Among any procedural aspects, procedural fairness judgments are especially emphasized, and thus we focus on them, as well. This chapter has three aims. The first is to identify and classify potentially relevant procedural characteristics. The second aim is to consider the reasons why procedures matter. The third is to review the effects of procedural considerations on people's utility and on their behavior.

2.2.1 Relevant procedural characteristics: What matters?

Procedural fairness can be seen to be the most thoroughly researched aspect among procedural effects. A large literature provides answers to what constitutes a fair procedure.³ *Leventhal* (1980) distinguishes between six different procedural characteristics of fair processes: representativeness, consistency, correctability, bias suppression, accuracy, and ethicality. Each characteristic may be of a higher or lower value. Representation rights, for example, may vary from voice to participation, i.e. from the opportunity to be heard before the decision is made to direct involvement in bringing about a decision.⁴

In their seminal paper, *Bies* and *Moag* (1986) emphasize the decision maker's role and add the concept of interactional fairness.⁵ They state that while obviously people are clearly concerned about the fairness of outcomes and formal procedures, they are also concerned about the interpersonal treatment they receive during the process. The authors identify four criteria for interactional fairness: respect, propriety, truthfulness and justification. These criteria have also been shown to explain perceptions of process-related

³See, e.g., the seminal works of *Thibaut* and *Walker* (1975) or *Lind* and *Tyler* (1988) where different procedural characteristics have been discussed at some length.

⁴Note that in the literature, voice is sometimes used in a less narrow sense in that it is a mere synonym to representativeness. *Anand* (2001) defines voice as the extent to which a person has control over a decision. *Folger* (1977) defines voice as the extent to which opinions and preferences of affected parties are considered in the decision-making process.

⁵Whereas some studies highlight distinctions between procedural and interactional fairness (e.g., *Bies*, 2001; *Cohen-Charash* and *Spector*, 2001), others (e.g., *Greenberg*, 1990; *Lind* and *Tyler*, 1988; *Tyler* and *Bies*, 1990) argue that interactional fairness should be subsumed under the rubric of a broader conceptualization of procedural fairness, an approach which we will follow throughout.

fairness partially (e.g., *Colquitt et al., 2001*). *Dolan et al. (2007)* systematically evaluate the literature on procedural fairness⁶ and identify six broad procedural characteristic categories: voice, neutrality, consistency, accuracy, reversibility and transparency. This short list of definitions should give the reader an understanding of the complexity of what may constitute a fair procedure.

Yet, despite these comprehensive classifications, the general understanding of the importance of various procedural characteristics and their interactional effects remains limited. The characteristics are described in a variety of ways making it thus difficult to compare findings across studies. Besides, only few studies provide data on more than one of the procedural characteristics and none provides information on trade-offs (see *Dolan et al., 2007*). Others (e.g., *Sondak and Tyler, 2007*) collect questionnaire data on various characteristics, but combine them into an overall index. Empirical evidence on the relative importance of different procedural characteristics as well as its interactive effects has been identified as a future challenge (e.g., *Leventhal, 1980; Skarlicki and Folger, 1997*) but is still limited. One exception is a meta-analysis by *Colquitt et al. (2001)*. They find that voice explains 26% of the variance in perceptions of procedural fairness. When controlling for voice, the Leventhal criteria mentioned above explain an additional 21% of the variance in procedural fairness perceptions. Further research in this direction would help to identify those procedural characteristics which matter most for procedural fairness judgments. Hereby deduced results would be an asset for advising decision makers within organizations and society on how to shape institutions to bring about desired reactions (e.g. acceptance of decisions or obedience towards rules). *Sondak and Tyler (2007)* note that procedural design has successfully been implemented in the realm of dispute resolution institutions. As studies have shown that people evaluate procedural elements associated with mediation to be fairer than those associated with arbitration or formal trials (*Tyler, 1987, 1988, 1990, 1997*), the legal system has increasingly adopted mediation processes. This change resulted in increased satisfaction with the legal system and greater willingness of disputants to defer to third party dispute resolution decisions.

Thus, separating various procedural characteristics can be of value. Research also suggests, though, that these characteristics interact to affect individuals' fairness perceptions and subsequent behaviors. In the following, we will focus on procedures per se and discuss of what importance they are.

⁶*Dolan et al. (2007)* use keyword searches of electronic databases and hand searches of specific journals and papers by selected authors. They identify a total of 107 articles and books from across a range of decision-making contexts based on a systematic literature review.

2.2.2 Why do people care about procedures?

It has been shown that procedures with their various characteristics play an important role in human decision-making. Possible reasons *why* procedures matter can be grouped into two rough categories (e.g., *Konow, 2003*): (1) procedures as means for fair/favorable outcomes (instrumental, consequentialist or direct reason), and (2) procedures as aim independent of therewith achieved outcomes (non-instrumental, expressive, proceduralist or indirect reason).⁷

Reason (1) states that people care about procedures because they affect outcomes. In their classic work on procedural fairness, *John Thibaut* and *Laurens Walker* (1975) still subscribe to this view, arguing that procedures matter because they permit people to feel that they can help mold outcomes. For example, people are more likely to appraise a procedure as being fair if it gives them control over the decision-making process. Thus, process control is not seen to matter as an end in itself but as a means to an end: a way of improving one's prospects given the inevitability of not having full decision control. Hence, they suggest that people's desire to express their opinion is directly linked to their view that these arguments influence the decision. Later psychological research, however, has incorporated non-instrumental reasons due to increasing evidence pointing in other motivational directions (cf. the following paragraph). On the other hand, neo-classic economic theory (especially traditional welfare economics) still values procedures only to the extent to which they promote utility-leading outcomes as standard models are based on utilitarianism, which requires that every choice is judged only by the consequent states of affairs.

Reason (2) is based on the idea that people may also attribute an intrinsic value to the process itself. Political scientists (e.g., *Lane, 1988*) early started to argue that the democratic process per se provides utility to citizens. A proceduralist explanation why procedures are of prime importance has been offered by *Lind* and *Tyler* (1988) and *Tyler* (1990). They propose an identity-based group-value model in which they suggest that procedures are valued because they communicate status and inclusion in groups. By expressing their own view, people are informed of their connections with group members and authorities. In this sense, procedures may lead to positive feelings and are associated with the perceived quality of social relationships between individuals and decision-makers. *Tyler* and *Lind* (1992) have ex-

⁷An exception is the categorization by *Dolan et al. (2007)* which distinguishes between three potential reasons: (a) procedures affect outcomes, (b) procedures affect any other factors than outcomes, and (c) procedures are valued in their own right. However, *Dolan et al. (2007)* cannot clearly explain the differences between reason (b) and (c).

tended this line of thinking to the study of authority relations. They argue that people view group authorities as representatives of the group, and are therefore sensitive to how those authorities exercise their authority. Using fair procedures to exercise authority both communicates that people are respected by the group, and it also suggests that the group is worth identifying with and being involved in. Besides, it has been shown that fair procedures tend to evoke feelings of loyalty to one's group and legitimize the authority of leaders (*Tyler and Belliveau, 1995*). Related research reviewed above is not restricted to perceived procedural fairness, but many other intrinsic benefits of a procedure have been identified, among them the utility gained by facing and meeting challenges, expressing oneself, using one's talents, and reporting experiences. Note that procedures may also lower utility, for instance by being cognitively taxing, or by forcing one into making a decision (e.g., *Lane, 1988*).

Whereas a growing literature in the social sciences, especially in psychology, political science and sociology helps to shift the focus to non-instrumental issues of procedures, only few economists early admitted that people care about procedures for non-instrumental reasons (e.g., *Frey and Stutzer, 2001; Hahn, 1982; Ng, 1988; Sen, 1995, 1997*). However, as we will reveal in the following chapters, economists have begun to investigate non-instrumental roles procedures play in human decision-making.

We can summarize that the distinction between instrumental and non-instrumental reasons has transformed the way we think about the meanings and implications of procedures. Procedures *do* matter not only because of their effects on outcomes but those on other factors as well. In a next step, we will discuss these non-instrumental effects in more detail.

2.2.3 The effects of procedures: How do they matter?

Remarkable advancements have been reached in integrating processes and outcomes by studying their interactive effects, primarily procedural fairness effects (e.g., *Folger, 1984; Frey and Stutzer, 2001; Greenberg, 1986; Lind and Tyler, 1988; Sweeney and McFarlin, 1993*). But when comparing procedural and outcome fairness, what matters most? Meta-analyzes by *Cohen-Charash and Spector (2001, 2002)* and *Colquitt et al. (2001)* suggest that fairness judgments have independent effects on a variety of attitudinal and behavioral outcomes. *MacCoun (2005)* notes that in some studies, procedural fairness has better predicted attitudes toward one's supervisors and organization, whereas distributive fairness has better predicted pay satisfaction and job satisfaction. However, as he points out, direct horse race comparisons of predictor strength between procedural and distributive fairness are problematic

due to its multiplicative, interactive effects.

The psychology of justice research (most notably done by *Tom Tyler*, *Allan Lind*, and colleagues) early raised the question why people care about and react to their evaluations of the fairness or unfairness of procedures. A particular effect has attracted remarkable attention: the *fair process effect* (also referred to as the *fair outcome effect*, depending on the individual viewpoint).

The fair process effect, describing the finding that people are more likely to accept decisions when they feel that they are made via fair procedures (*Folger*, 1987), is said to be extremely robust. There is enormous support for this effect (*Brockner* and *Wiesenfeld*, 1996; *Van den Bos* et al., 1999).⁸ It is of particular importance for negative outcomes as a fair process enhances the perceived fairness of those unfavorable outcomes and thus results in higher satisfaction with negative outcomes. The effect is of great practical relevance because authorities' effectiveness depends heavily on the voluntary acceptance of rules and decisions by group members (*Sondak* and *Tyler*, 2007). For example, legal authorities seek voluntary compliance with law, political officials seek voluntary payment of taxes, and managers seek acceptance of workplace rules. *Lind* et al. (1993), for instance, found that real-life litigants who judge the arbitration process as fair are much more likely to accept the court's decision, irrespective of the actual instrumental outcome. Further, *Tyler* and *Lind* (2000) show 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 judgments about the authority's decision.

The form of the process-outcome-interaction can be expressed in two different ways. From one viewpoint, process fairness mitigates the effects of outcomes, such that when process fairness is low, outcomes exert stronger effects on overall fairness perceptions. When process fairness is high, however, outcomes exert less impact on fairness perceptions. From another perspective, outcomes moderate the effects of process fairness such that when outcomes are negative, process fairness has stronger effects on fairness perceptions. When outcomes are favorable, however, process fairness has less impact on fairness perceptions.

Brockner and *Wiesenfeld* (1996) reviewed 45 studies examining the interaction between procedural fairness and outcome perceptions. The results

⁸However, empirical and experimental economic evidence on the fair process effect is still rare. An exception is the study by *Bischoff* et al. (2008) who show by analyzing representative survey data that procedural fairness plays an important role for the acceptance of a given income distribution. Recent findings from the field of experimental economics are discussed below.

revealed strong interaction effects in 43 samples which suggest that both effects exist. Thus, processes and outcomes have substitutable effects since people perceive overall fairness as existent as long as either the process or the outcome is fair. However, *Lind and Tyler* (1988) show that there is considerably more support for the mitigating effect of fair procedures on negative outcomes (fair process effect) than for the mitigating effect of fair outcomes on unfair procedures (fair outcome effect).

Of course, procedures are not similarly important to each individual; for example, *Gonzales and Tyler* (2008) note that procedural fairness is more important to those who believe themselves to be socially excluded, peripheral, or marginalized than to those perceiving themselves as socially included, central, and integrated. Besides, procedural fairness is found to be especially important when issues of identity and relationship are salient (*Tyler and DeCremer*, 2005). *Brockner et al.* (1995), for instance, show that the perceived favorability of outcomes depends on how they are framed. They find that when procedural fairness was high, decision frame had no effect on layoff survivors' reactions. When it was low, survivors reacted more favorably in the positive than in the negative frame.

Two distinct theories explaining the interaction between procedures and outcomes have been proposed: referent cognitions theory (e.g., *Folger*, 1984, 1987) and fairness heuristic theory (*Lind*, 1995, 2001; *Van den Bos et al.*, 1997). For a review on both theories, see, e.g., *MacCoun* (2005).

In contrast to the overwhelming majority of studies that validate the robustness of the fair process effect, some studies find under certain conditions the opposite to be true (see, e.g., *Cohen*, 1985; *Folger*, 1977; *Kulik and Clark*, 1993; *Lind and Tyler*, 1988): fair procedures can also result in less satisfaction with negative outcomes than unfair procedures. *Folger* (1977) show that outcomes that improved after an opportunity for "voice" are perceived as less fair than the same outcome without voice. Voice was operationalized here as the opportunity to express to an allocator one's own perception of just deserts. *Folger* referred later to this apparent contradiction to the usual finding of a positive relationship as *frustration effect*. *Cohen* (1985) suggests that the effect occurs likely in situations where allocators receive whatever amount they do not pay to recipients. He explains this with the recipients' impression procedures including an opportunity for voice could rather serve as an insincere attempt to give the allocation the semblance of fairness than be a real attempt to solicit views. In that case the recipient will not experience any enhancement of procedural fairness from the voice procedure and thus is expected to view the outcome as dissatisfying. *Lind and Tyler* (1988) add that the frustration effect only occurs in settings where the characteristics that give the procedure a procedural fairness advantage are relatively

weak.

Procedures not only provoke outcomes or influence subsequent behavior in response to these outcomes, but may also have a value on their own. This value has been subsumed under the notion of procedural utility: the use of the preferred procedure may directly yield utility. For example, if people prefer the rule one-person-one-vote, the use of it yields immediate utility (Ng, 1988). The idea that procedural utility can explain deviations from standard interpretations of rationality is not new. In other social sciences than economics, procedural utility has a long tradition. It refers to the concept of Aristotelian eudaimonic well-being. However, the notion of procedural utility has received recent attention. It has been empirically shown (notably by Bruno Frey and colleagues) that people have preferences over procedures and that utility is provided if their preferences regarding the process as such are satisfied. For example, econometric analyses by Frey et al. (2004) suggest that people derive utility from mere participation in the political process. Their approach is based on self-reported and hence subjective well-being, happiness or satisfaction with life. Happiness measures outcome in the sense that a higher level is preferable to a lower level. They compare data on subjective well-being of Swiss citizens and non-nationals. Since only citizens have direct democratic rights and thus the opportunity to participate, while non-nationals are excluded, their data suggests that two-thirds of the gain in well-being is based on the possibility to be able to participate in the direct democratic process. Frey et al. (2004) add that individuals gain procedural utility in addition to outcome utility not only through actual participation but even through 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. They suggest that people are concerned about how they are treated by institutions and procedures because this has an impact on their identity and how positively (negatively) they feel about themselves.

Last but not least, decision-making procedures also convey important information about individuals' relationships within a group and with its authorities. In their seminal work, Lind and Tyler (1988) argue that people are concerned about issues of identity and status. They suggest that people use the fairness of procedures to learn about their status and to evaluate the degree to which they want to define their identity according to the groups to which they belong (group-value model of procedural justice).⁹

⁹In their 1992 publication, Tyler and Lind present their relational model of authority which extends the earlier model more generally beyond decision procedures to public support of authorities and rules more generally.

2.3 Procedures in economic experiments

Until recently, there was a noticeable lack of evidence in economic research on the behavioral effects of procedures which was partly due to some kind of willful ignorance, partly to the difficulty of disentangling the effects of outcome and process considerations. We argue that with the rise of laboratory experiments, a powerful method that is able to clearly differentiate between both aspects has been established. All studies discussed here refer to the general issue of whether people's motivation and subsequent behavior is affected by the process by which an allocation is reached in the presence of real incentives. There are many different procedural aspects that may have some influence; a few of them have been studied so far, the most notable ones are reviewed below, namely the roles of appointment procedures, intentions, and procedural fairness.

2.3.1 The role of appointment procedures

Frequently, decisions are made by decision makers, i.e. individuals or groups who decide on issues whose outcomes affect others. In these cases, decision makers have to be appointed by the preceding use of a role assignment procedure. Typically, roles in bargaining experiments (e.g. proposers and responders in ultimatum game settings) are assigned via random procedures. In these cases, the strength of property rights seems to be accompanied by a fairness norm suggesting that all players have (more or less) the same right to a share, resulting in a high probability for 50:50 allocations. This normative right has been shown to be altered by the introduction of real effort (e.g. cracking walnuts or solving a general knowledge quiz) to obtain the pie (see, e.g., *Frey and Bohnet, 1995; Ruffle, 1998*). There is substantial experimental evidence on this "entitlement effect". It says, in short, the larger an individual's input into obtaining a pie, the fairer it is for them to keep a large share (*Fahr and Irlenbusch, 2000; Frey and Bohnet, 1995*).

Hoffman and Spitzer (1985) and *Hoffman et al. (1994)*, for example, show that proposers offer less and responders accept more unequal offers whenever the role of the proposer was earned (e.g. by scoring high on a general knowledge quiz) rather than randomly assigned. Further evidence is provided by *Cherry et al. (2002)* who conduct a dictator game with earned surplus and high anonymity and observe that 95% of the dictators keep the whole amount at stake.

In a three-player ultimatum game, *Grimalda et al. (2008a)* implement a particular procedural characteristic by varying the degree of participation: in the non-participation treatment, a proposer is randomly selected ex ante,

and she becomes the only person to propose a division of the pie. In the participation treatment, all of the three group members, individually and simultaneously, make a proposal. One proposal among the three is then selected by a random draw, and the responders decide whether to accept or reject by majority vote. The authors emphasize that these two treatments are strategically equivalent in the sense that the expected payoffs are *ceteris paribus* the same. 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. The authors conjecture that the effect of a higher number of institutions allowing more participation in the decision-making process also brings about more “socially responsible” behavior in the players and more equality in payoff distribution. They note that at the same time, however, this effect may also result in more conflict, as individuals are less prone to accept unequal offers from the proposer.

Backes-Gellner et al. (2008) recently criticized that many experimental games were not appropriate to represent real relationships (e.g. in the realm of employment). *Brandts et al.* (2006) meet these concerns by introducing a role allocation beyond chance or effort, i.e. they test whether a selection procedure on the basis of information about personal characteristics may not only have an allocative impact, but may also cause other effects. They find that the very fact that people are selected on the basis of information about their personal characteristics results in lower demands compared to situations in which people are randomly determined. They provide statistical evidence for two different effects. First, knowingly selected allocators keep less for themselves than randomly selected ones (“I-want-YOU effect”). This effect has weakly been confirmed in round 1, but not in the following rounds. Second, selected players reward the selecting player more generously than a third party involved (“gratitude effect”) with weak statistical significance in the first round but not in the following rounds.

Mertins (2008a) and *Albert et al.* (2008) study another aspect of appointment procedures. They test whether individuals’ procedural satisfaction, and their procedural fairness judgments on appointment procedures affect people’s behavior. *Albert et al.* (2008) focus on both, responders’ and proposers’ behavior. They show that proposers claim significantly less if they were chosen by majority vote (which implies the satisfaction of the responders’ group preferences regarding the proposer’s appointment procedure) thus confirming the result by *Brandts et al.* (2006) within a different framework. Furthermore, *Mertins* (2008a) and *Albert et al.* (2008) show that the behavior of responders also depends on procedural satisfaction. Responders’ demands vary depending on whether proposers obtain their roles with or without the

responders' support: procedural satisfaction results in stronger resistance against various outcomes. People rather seem to accept decisions made by proposers which they have not supported (individually or by majority vote). As discussed before, this counterintuitive finding may be explained by the "frustration effect" (see, e.g., *Folger*, 1977; *Lind* and *Tyler*, 1988). Besides, the judgment whether a procedure is seen to be the fairer one out of two did not prove to significantly effect different reactions to outcomes.

2.3.2 The role of intentions

Numerous findings in experimental economics suggest that individuals deviate from the standard economic model in having strong social preferences (e.g. *DellaVigna*, forthcoming). That is, people care about fairness, equity, and reciprocity (for an overview, see *Camerer* (2003) or *Kagel* and *Roth* (1995)). However, there is disagreement within the scientific community whether outcome preferences are sufficient to predict the reciprocal actions observed, or whether it is necessary to account for *intentions* to measure reciprocal response. People might care not only about outcomes but also about intentions underlying distributional decisions.

Two different procedural settings are feasible to study the effects of intentions. First, responders receive information about which alternatives were available to proposers. The alternatives not chosen may yield information about the intention or attitude of the decision-maker, which in turn may trigger reciprocal behavior. Second, the same outcome may be chosen by either a human decision-maker or a random procedure. The former is assumed to act with consciousness, thus may be perceived as intentional while the latter is assumed not to be perceived as intentional.

Experimental evidence on the role of intentions is mixed. For example, *Charness* (2004) and *Offerman* (2002) find little or no evidence that the attribution of fairness intentions matters in the domain of positively reciprocal behavior, i.e. reward. *Blount* (1995) and *Offerman* (2002) find weak evidence that it matters in the domain of negatively reciprocal behavior, i.e. punishment. *Bolton* et al. (1998) report on an experiment that allows studying both positive and negative reciprocal action in a single framework. In the reward treatment, results can fully be explained by outcome (instead of procedural) considerations. For the punishment treatment, there is some evidence that intentions might play a role. However, the difference in behavior is not statistically significant. *Charness* and *Levine* (2007) find strong evidence that intentions matter for both punishment and reward situations. We will review some of these findings in more detail below.

Building upon early findings by psychologists on the non-instrumental

value of procedures, *Blount* (1995) was among the first who designed an economic experiment to analyze the effects of intentions.¹⁰ She compares reactions to decisions either made by humans (perceived as intentional as humans can think about their actions and control them) or non-humans (random or natural occurrences are typically not perceived as intentional due to the absence of consciousness). She compares responder rejections in a standard ultimatum game with (a) rejections in a treatment in which an outside party, receiving no payoff for the game, proposed the allocation and (b) rejections in a treatment in which the proposal is said to result from a random number generator with equal probabilities. She finds rejection rates to be lowest in the random treatment, but still significantly positive.

Charness (2004) compares second movers' behavior in reaction to first movers' decisions with first movers being either human participants having an interest in the outcome (standard), having no interest (third party) or being a random machine. By analyzing a gift exchange game he finds that second mover contributions are slightly higher in the third party and random treatments than in the standard game. But positive correlation between first mover and second mover contributions is found in all three treatments, which is a contradiction to the intentions hypothesis: if intentions matter, there should be no correlation for random and third party treatments.

Kagel et al. (1996) study an ultimatum game in which treatments vary according to pie size and information thereon. Incomplete information treatments consist of second movers knowing both, but first movers knowing only their endowment, making it more difficult to attribute unfair intentions to the first mover. If intentions play a role, rejection rates are expected to be lower in the incomplete information treatments than in the complete ones. Indeed, *Kagel* et al. (1996) report evidence supporting the intentions hypothesis, but at the same time they also find the opposite to be true for another comparison of treatments. These observations suggest alternative interpretations including strategic considerations.

Falk et al. (2008) show that proposers' intentions matter for responders' behavior: it seems that responders wish not (only) to avoid unequal or unfair outcomes but (also) to punish intentionally unfair proposers. The authors present three experiments which suggest that many of the observed punishments are actually triggered as a response to unkindness, not as an attempt to reduce inequality.

Charness and *Levine* (2007) analyze workers' reactions to pay decisions

¹⁰Note that *Blount's* studies follow the methodological requirements of economic experiments with one exception: one treatment involves deception in subjects believe that other participants (and not the experimenter) made the proposals.

by firms following different wage-setting procedures. Each randomly paired group consisted of 2 players, one being the firm, the other representing the worker. Firms, endowed by \$12, had the choice between paying a high (\$8) or a low wage (\$4) to the worker. Then, a coin determined whether the economic conditions were good (bad) resulting in a worker's wage increase (decrease) by \$2. After having experienced the firms' decisions and the coin flips, workers chose an effort level: low effort cost \$1 and reduced the firm's payoff by \$4, medium effort cost workers nothing and did not change the firms' initial payoff, and high effort cost also \$1, but increased the firms' payoff by \$4. Thus, the same wage of \$6 could be determined by two different procedures: high wage (good intention) coupled with bad condition and low wage (bad intention) coupled with good condition. The authors find that workers who end up receiving medium wages respond much more positively when this is due to the firm offering a high wage but bad luck lowering the worker's pay than when the opposite holds: the firm offering a low wage and good luck raising the pay. Thus, participants' effort levels (i.e. rates of punishment and rewards) react strongly to intentions and more modestly to distributional outcomes.

Betrayal aversion is in line with recent theoretical models and empirical evidence that people care about how outcomes come to be and about others' intentions. *Bohnet* and *Zeckhauser* (2004) study this concept by examining whether the decision to trust a stranger in a one-shot interaction is equivalent to taking a risky bet, or if a trust decision entails an additional risk premium to balance the costs of trust betrayal. They state the hypothesis that it is fundamentally different to trust another person than to rely on a random device that offers the same outcomes: people are averse to being betrayed. The authors compare a binary-choice trust game with a structurally identical, binary-choice risky dictator game with good or bad outcomes and elicit individuals' minimum acceptable probabilities of getting the good outcome such that they would prefer the pure chance for the sure payoff. Supporting their hypothesis, first movers state higher minimum acceptable probabilities in the trust game than in situations where nature determines the outcome. *Bohnet et al.* (2008) build upon this experimental design in testing whether betrayal aversion can be found outside the United States. In their cross-country study among Brazil, China, Oman, Switzerland, Turkey and the United States, they found people to be betrayal averse in each of these nations. The effect has been found to be most pronounced in Oman.

Although being psychological research, we discuss the work by *Fukuno* and *Ohbuchi* (2003) here as their research is strongly related to the questions raised. Their study addresses both the role of appointment procedures and that of intentions at the same time. Participants playing an ultima-

tum game received one of three offers: unfavorable and unequal, equal, or favorable but unequal. These offers were determined by either the other participant or by a computerized lottery, thus manipulating the arbitrariness of the role assignment procedure. The authors find that outcome acceptance is determined by distributive and procedural fairness judgments, and that these types of fairness are influenced by different situational characteristics, such as intentionality, the size, and the equality of the offer. Participants perceive the intentional small offer as more unfair than the unintentional small offer, while they perceive the same offers as unfair in the distributive sense, regardless of intentionality. They rather reject the intentional than the unintentional small offer. Besides, people perceive the arbitrary procedure of the role assignment as highly unfair, whereas the difference of arbitrariness in role assignment procedures has no significant impact on their reactions to the offer.

2.3.3 The role of procedural fairness

Given that procedural fairness is an integrative part of any of the issues discussed above, we address the procedural fairness effect in a separate chapter. As pointed out before, the fair process effect is one of the most frequently replicated findings in social psychology (*Van den Bos et al.*, 1999), and is said to be one of the most important discoveries in justice research (*Van den Bos et al.*, 1998). Surprisingly, there is still extremely little experimental evidence for cases where the participants' decisions have monetary consequences. The first experimental economists explicitly tackling the question of procedural fairness are *Bolton et al.* (2005) who show that allocations resulting from fair procedures (implemented by unbiased random procedures), are more acceptable than the same unfair outcome chosen by a third-party. *Bolton et al.* (2005) investigate within ultimatum games whether the allocation bias of a random fair procedure influences the ex post acceptability of the outcome of the procedure. Their main result is that settings with fair procedures leading to unequal outcomes and settings with equal outcomes seem to be equivalently treated by responders. That is, procedural fairness – even when leading to unequal outcomes – is indeed a substitute for outcome equality.

A related study is provided by *Grimalda et al.* (2008b), who study fairness regarding the allocation of initial opportunities by varying the probability (opportunity) that a player becomes the proposer in an ultimatum game (i.e., 0%, 1%, 20% and 50% opportunity). That is, whereas *Bolton et al.* (2005) analyze whether the allocation bias of a random fair procedure influences the ex post outcome acceptability, *Grimalda et al.* (2008b) focus on fairness prior to the unfolding of the interaction. In particular, the latter

hypothesize that opportunity has a symbolic value (e.g. some kind of procedural utility) to participants. That is, responders will accept more unequal outcomes, as the procedure becomes relatively more unbiased and hence more procedurally fair. Since proposers are expected to anticipate these reactions, they will increase their demands on average. In other words, the fairer the procedure, the higher the inequality in outcomes. The authors find that a 1% probability of becoming a proposer leads to significantly lower offers and higher acceptance rates compared to the case where participants have no such a chance. By raising this probability further, the observed effect continues but is no longer significant with respect to the 1% treatment. The authors draw the conclusion that people in this setting are motivated solely by the symbolic aspect of opportunity, rather than by the actual fairness in the allocation of opportunities.

Albert and *Mertins* (2008) study the influence of more or less participation in the decision-making process, thus testing the procedural fairness hypothesis that more participation (higher fairness) increases acceptance of unfavorable decisions. Their work 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. Hence, the responder can punish greedy takers, but only at her own expense. The authors modify the game by letting the responder participate in the takers' transfer decision and consider the effect of participation on the destruction rate. They conclude that participation matters. Responders destroy more if they (1) have 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 more greedy).

By focusing on procedural fairness norms the study by *Dittrich* and *Tontrup* (2008) aims on facilitating the analysis of institutional processes like administrative procedures. They recently started investigating the existence and impact of several factors related to procedural fairness norms. In their experimental setting, a decision-maker earns an entitlement to some objectively determined payoff in a real effort task. His actual payoff is, however, determined by a neutral third party, which decides based on incomplete information. Rational, risk-averse decision-makers should accept the sure offer, but many participants filed an objection against it and subsequently faced a gamble with expectations equal to the payoff determined by the neutral third party. The authors show that higher transparency of the decision process of the third party reduces objections by 40 percentage points. Their future research is intended to elicit the willingness to pay for filing an objection and

for increasing the transparency of the third party's decision process. Besides, institutional factors will be manipulated to gain insights into the sensitivity of procedural fairness norms.

Tontrup and Gaissmaier (2008) analyze the effects of perceived legitimacy of procedures on people's willingness to cooperate. In their experimental public good game, they allow participants to vote on a set of rules vs. imposing the same institutions exogenously. They provide statistical evidence for their hypothesis that average contributions to the public good are higher in the voting than in the control condition (85.2% vs. 58.5%). That is, participation in the procedure is sufficient to increase cooperation rates. The authors show that the size of the effect does not depend on the set of rules participants actually decided for nor on whether subjects actually received the institution they personally voted for. The authors assume the perceived legitimacy of procedures to be an important determinant of people's willingness to cooperate with one another in social dilemmas. To provide evidence for this explanation, the authors replicate the experiment in China, since China is perceived as a country where the democratic majority rule is not seen to be particularly legitimate. As hypothesized and in sharp contrast to the results in Germany, they find no increased contributions in the voting groups of the Chinese sessions.

As mentioned before, procedural fairness is the best analyzed aspect of procedural concerns; procedural favorability has mostly been treated interchangeable as both concepts overlap to a great extent. The same is true for outcome fairness and outcome favorability. There is empirical evidence that strong correlations between individuals' perceptions of outcome fairness and outcome favorability (e.g., *Greenberg, 1994; Tyler and Caine, 1981*) or even no differences at all exist (*Brockner and Wiesenfeld, 1996*). More recent research suggests, however, that fairness and favorability judgments do not always show the same effect (*Van den Bos et al., 1997, 1998*). To our knowledge, *Mertins (2008a)* and *Albert et al. (2008)* are the first papers by experimental economists which explicitly differentiate between the effects of procedural satisfaction and procedural fairness judgments. *Mertins (2008a)* finds that resistance against any feasible claim is higher if the proposer's appointment procedure is judged to be the fairer one (in comparison to another procedure available). However, the differences in the willingness to offer resistance were not statistically significant. On the other hand, resistance differs significantly depending on whether people's procedural preferences are satisfied or not. Surprisingly, the author cannot provide evidence for the robust fair process effect, but for the rather exceptional frustration effect (*Folger, 1977*). That is, people rather seem to accept decisions (i.e. offer less resistance)

made by proposers which they have not supported (individually or by majority vote). This observation may be explained by people asserting a claim on their behalf (i.e. demand fair treatment like a generous offer) when voting for a procedure and thus for a proposer.

2.4 Procedures in economic theory

Due to the recent popularity of the field of behavioral and experimental economics, an impressive amount of interesting results has been produced. Concurrently, *Bergh* (2008) argues the supply of theoretical explanatory frameworks remained rather limited. This is particular true for the research on procedures. Whereas increasing experimental evidence indicates that procedures matter, their impact on human decision-making still awaits a proper theoretical foundation as both, traditional economic theory and even most models of social preferences, are based on a consequentialist view (see *Sobel* (2005) for a survey).

One class of models of social preferences assumes that individuals maximize their utility according to well-defined preferences, but permit preferences to depend on the payoffs of others. Let us, for example, consider the theory of inequity aversion by *Fehr* and *Schmidt* (1999) and the ERC model by *Bolton* and *Ockenfels* (2000). These models of distributional concerns assume that people's utility depends only on outcomes and is independent from any procedures preceding the outcome. Indeed, economic models are expected to be simplified to focus the attention on the important mechanisms of behavior. However, in case of a theory of fairness, reduction to distributional fairness and thus neglect of an enormous amount of evidence on the behavioral importance of procedural fairness seems not to be adequate (see *Bergh* (2008) for a critical discussion).

There is another class of models of social preferences (see e.g., *Dufwenberg* and *Kirchsteiger*, 2004; *Rabin*, 1993) which incorporates procedural aspects in the form of perceived intentions by assuming them to trigger corresponding responses (positive or negative reciprocity). *Falk* and *Fischbacher* (2006) expand this approach by presenting a formal theory of reciprocity which takes into account both that people evaluate the kindness of an action by its consequences and also by the underlying motivation, i.e. intention. The theory explains the relevant stylized facts of a wide range of experimental games. Among them are the ultimatum game, the gift-exchange game, a reduced best-shot game, the dictator game, the Prisoner's Dilemma, public good games, and the investment game. Furthermore, the theory explains why subjects behave differently in treatments where they experience the actions

of real persons compared to treatments where they face actions caused by a random device.

However, both classes of theories cannot explain all observed behavior. Thus, it has repeatedly been argued that fairness models that go further by combining both intentionality and distributional concerns are needed (see e.g., *Bereby-Meyer and Niederle, 2005*). Besides, some recent experimental findings focusing on procedural effects can neither be explained by distributional nor by reciprocity models, resulting in the appearance of completely new theories. *Grimalda et al. (2008b)*, for example, observe a discontinuous jump from no opportunity to 1%-opportunity. They propose a combination of inequality aversion models with Nozick's symbolic utility. The authors note that this is not the attempt to introduce a new social utility model. However, their approach demonstrates the increasing effort to explain observed behavior associated with procedural effects. Another attempt to explain data that are neither outcome-based nor explainable by reciprocity models is the process model by *Trautmann (2007)*. He proposes a model of individual preferences for process fairness that complements the Fehr-Schmidt model for outcome fairness. The introduction of process fairness raises issues of dynamic consistency of fairness preferences. The author discusses theoretical and policy implications of inconsistency in a dynamic decision context. He provides applications to welfare improvements and illustrates the integration of the process model in economic theory. *Konow (1996, 2000)* proposes a positive theory of economic fairness giving a fundamental importance to procedures and context. It is based on the accountability principle: A person's fair allocation varies in proportion to the relevant variables she can influence, but not according to those she cannot reasonably influence.

In the context of expected utility theory and in search of a model for the utility of gambling, *Le Menestrel (2001)* introduces a first model of procedural utility by treating it as a qualitative argument outside the utility function. It allows distinguishing the game of payoffs presented to the individuals from the psychological game that is perceived and played. *Le Menestrel (2006)* introduces a game-theoretic model of rationality that combines procedural utility over actions with consequential utility over payoffs. Thus, empirically observed cooperative behavior can be rationally explained by a procedural utility for cooperation. The conventional interpretation of the Prisoners' Dilemma is that individuals should defect, whatever the payoff differentials between cooperation and defection. In the proposed model, mutual cooperation can emerge as the unique Ideal Nash Equilibrium when procedural utility for cooperation is sufficiently strong. Moreover, a given game of consequences may be played differently by different individuals with different procedural utility. The results of the model allow predictions about the dependence of

rational behavior upon individuals and the social context. Similar work has been done by *Sebald* (2007a), who provides a game-theoretic framework that integrates procedural concerns into economic analysis.¹¹

The experiment by *Bolton et al.* (2005) is accomplished by an extension of the ERC model to address the observed findings by meshing procedural fairness norms with allocation fairness norms. They argue the key insight is that changing the feasible outcome space, or determining the allocation by randomization, creates alternative fairness norms. The authors embed these competing norms into the ERC model by refining the reference point. They note that refining the reference points in social utility models indeed appears to be a promising research path. But since the model is extended post hoc, the experimental data cannot be considered a test. Besides, the authors view their theoretical attempt as a sketch because they want to demonstrate that a relative payoff model with a more refined reference point can, in principle, capture the observed phenomena.

A similar approach is employed by *Krawczyk* (2007), who also provides an extension of the ERC model, allowing for indirect modeling of reciprocal behavior. In presenting a new model aimed at predicting behavior in games involving a randomized allocation procedure, he builds upon the central procedural fairness hypothesis (see the preceding discussion). The model is designed to capture the relative importance and interaction between procedural and distributive fairness. The author drops the consequentialist perspective by explicitly incorporating a new term into the utility function, which captures the way in which outcomes are generated. By doing so, procedural considerations including procedural fairness can be accounted for. By applying the model to ten experimental games, the author shows that the model predicts well. However, he suggests further verification of the model and possible extensions.

Bolton et al. (2005) intend to open the way to a more careful consideration of how allocation and procedural fairness interact, and to point to a role for competing fairness norms in understanding procedural fairness. They argue that different situations might systematically evoke different fairness norms, so that a practical taxonomy can be developed by identifying natural classes of games to which they apply. At the same time, they admit that even the most sophisticated models do not capture the heterogeneity of individuals' perceptions of what is fair in games. Additionally, these models cannot be general enough to capture different fairness norms that might emerge in other, possibly more complex games.

¹¹*Sebald* (2007b) provides an application of the general framework.

2.5 Conclusion

Indeed, clear evidence for procedures influencing human decision-making exists. This survey article shows that, on the one hand, social scientists other than economists have provided a vast number of empirical, experimental, and theoretical studies validating the conjecture that not only outcomes shape human behavior but also the way in which decisions are taken. On the other hand, procedures have been neglected by economists for a long time. Recently, the economic research increased its attempts to identify procedural effects and started to build theories based on these observations.

We argue that behavioral economics may serve as an important link because it integrates insights by psychologists as well as experimental economists with neoclassical economic theory. Future economic research should make use of the experimental method, as it is able to systematically investigate economic behavior under controlled and replicable laboratory conditions. It has been argued that many experimental games are not appropriate to represent real relationships, e.g. real employment relationships (*Backes-Gellner et al., 2008*). Their survey shows that only recently, laboratory experiments have begun to meet these concerns by introducing additional factors like competition, social interaction or real effort. We perceive the introduction of procedural aspects as part of the same line of reasoning and argue that its implementation is already overdue.

By discussing some recent experimental findings focusing on procedural effects, we provide evidence for a promising new research agenda to be developed and to demonstrate at the same time the shortcomings of existing models. Neither traditional models nor those based on distributional or reciprocal fairness explain the observed behavior. We agree with *Grimalda et al. (2008b)* who argue that recent experimental results demand an altogether different conceptual approach or at least some generalization of previous economic models. But if different games systematically trigger different fairness perceptions, a general model explaining the facts without addressing the heterogeneity of fairness norms is hard to imagine. Further, *Bolton et al. (2005)* argue if there is no such thing as a universal fairness or reciprocity norm to guide social behavior independent of the game, any model with a simple statement of those norms is bound to be incomplete. However, this does not mean that such models are useless. A large literature has shown that we can go a surprisingly long way with very simple models of fairness in some important classes of games. For more challenging games, systematic variations of fairness norms could be incorporated in a way that allows subsequent empirical tests and theoretical refinements.

The increasing awareness for procedures is a step in the right direction

though there is still a lot to do. As long as procedures are excluded from an experimental design (e.g. by assuming a pie to appear like manna from heaven) simplistic models of human behavior might explain observations. However, when applying a more realistic approach by introducing particular procedures, we find strong evidence from various fields and methods that procedures do matter.

Chapter 3

Experimental methodology: Selected topics

3.1 Introduction

The purpose of this survey article is to discuss two important conceptual issues in experimental economics: preference elicitation methods and player types.¹ These topics are of particular relevance for experimental economics, such as the studies by *Mertins* (2008a), *Albert* and *Mertins* (2008) and *Albert* et al. (2008). Studies such as those mentioned ask participants to indicate their complete strategy to a specific game, which has been explained to them. Thus, the players state their contingent responses for each information set in the game. This method is the subject of some controversy. Some researchers stress its exceptional advantages, while others do not use it for fear that it may alter the actions of the players relative to other methods.² It is this necessity to discuss the relevant literature in greater detail.

Furthermore, as the studies mentioned above focus on social preferences, one may argue that these types of preferences are relevant only to particular types of individuals. Recently, economists began to classify people according to several criteria. *Albert* and *Mertins* (2008), for example, distinguishes between four types of responders, which differ in their behavior, and notes that non-reciprocal responder types are not influenced by treatment variation. If these non-reciprocal responders are frequent enough, treatment effects on re-

¹Fundamental summaries of experimental methodology can be found in *Bergstrom* and *Miller* (1997), *Camerer* (2003), *Davis* and *Holt* (1993), *Friedman* and *Cassar* (2004), *Friedman* and *Sunder* (1994), *Guala* (2005), *Kagel* and *Roth* (1995), and *Plott* and *Smith* (2008).

²When presenting work among economists not familiar with this method, some have noted that they felt like this method seemed to be an “experiment trick.”

ciprocal types do not show up in the aggregate. Thus, averages may mask substantial heterogeneity in individual behavior.

The present chapter is divided into two parts. The next part (section 3.2), presents two alternative preference elicitation procedures and examines whether behavior patterns are independent of the method used. The experimental evidence is mixed, with some studies finding that the method matters while others find that it does not matter. In particular, some experimental findings raise the possibility that people play games differently depending on the elicitation method. The advantages and disadvantages of both methods are discussed. To conclude this section, I apply these findings to experimental work I have conducted or participated in. In the last part (section 3.3), I examine recent attempts to classify player behavior into types and consider whether this is a promising approach for future research.

3.2 Preference elicitation methods

Experimental economics is among the fastest growing areas of economic research. It has evolved over the past decades, with increases in both the range of economic phenomena it addresses and in the methods it employs. However, there are some fundamental methodological concerns, such as concerns over different methods of preference elicitation. Various experimental studies indicate that human behavior may depend, sometimes dramatically, on how a problem is presented.

Early research in cognitive psychology revealed that subtle differences in elicitation can have a profound effect on stated preferences (e.g., *Kahneman and Tversky, 1979; Tversky and Kahneman, 1986*). *Brandts and Charness (2000)* state that there might be a natural concern that behavior depends on specific features of the experimental procedures that do not correspond to any issues of intrinsic motivation. Clearly, it is crucial that the experimental design does not bias the choices made by subjects. In this section I examine whether responses to more or less unfavorable actions depend on the elicitation procedure.

In order to elicit participants' preferences in an experimental setting, researchers usually apply one of two procedures. The first one is called the *strategy method* (*Selten, 1967*): subjects are asked to indicate their complete strategy. In other words, the second player decides on a contingent action for each and every possible move of the first player. Thus, subjects state contingent responses at each information set. One of the responses will correspond to the effective action(s) and will determine the individual experimental payoff. The method has generally been used in simultaneous-move

games, but its use is not restricted to them. There is a large field of application within almost any experimental game. It has been used, for instance, in an ultimatum game (*Mitzkewitz and Nagel, 1993*), an oligopoly experiment (*Selten et al., 1997*), a moonlighting game (*Falk et al., 2008*), a public good game (*Fischbacher et al., 2001*), a common pool resource game (*Keser and Gardner, 1999*) and a prisoner’s dilemma game (*Axelrod, 1984*). *Charness and Rabin (2002)* provide data from an array of 29 different simple games using the strategy method.

The application of the strategy method is rather simple. Consider, for example, a standard public goods experiment. Subjects are asked to indicate for each (average) contribution level of other group members how much they themselves want to contribute to the public good before finding out their actual choices (*Fischbacher et al., 2001*). Applying the strategy method to the power-to-take game³ means asking responders to indicate their destruction rate for any feasible take rate (*Andreoni et al., 2003; Albert and Mertins, 2008*). The strategy method can be changed slightly with the minimum acceptable payoff procedure: responders are asked to state the minimum payoff that they were willing to accept (see, for example, *Knez and Camerer (1995)*). By doing so, researchers implicitly assume that strategies are monotone. However, recent studies show that substantial fractions of responders not only reject low, but also high offers and would only accept equal splits. This kind of non-monotone rejection behavior is reported, for instance, by *Hennig-Schmidt et al. (2008)* and *Güth et al. (2003)*.

The other method employed by researchers is the *sequential elicitation method*: second movers respond to first players’ actual observed actions. Thus, subjects spontaneously respond during the course of the play. This method is usually used when a game is played in the ordinary manner as the first mover is clear. By letting the same participants play the game repeatedly, experimenters also might gain insights about individuals’ contingent strategies.

3.2.1 Does the elicitation procedure affect behavior?

Brandts and Charness (2000) explicitly test for differences in observed behavior between the two methods in sequential prisoner’s dilemma and chicken games. They let participants play the games both as first and second movers using either the strategy or sequential method. Each participant is subject to only one treatment of the game. The authors aimed at the simplest possible

³*Andreoni et al. (2003, 2005)* refer to the power-to-take game as the “convex ultimatum game”, *Rabin (1997)* calls it “squishy game”.

context by applying a 2 x 2 game matrix. In the sequential treatment, second movers have to respond to actual decisions made by their paired first movers: the row player first chooses a row. After being informed of row's move, the column player chooses a column. In the strategy treatment, the row player chooses a row and simultaneously, the column player chooses a column for each of row players' feasible choices. From a game-theoretic point of view, it should not matter whether the games were played with either method. The payoff-maximizing column player strictly prefers one row choice over the other in both games.

Brandts and *Charness* (2000) focus on choices that give people a lower financial reward and compare these rates among treatments. They find that behavior does not differ significantly: none of the differences in frequencies between treatments is significant using a test of the equality of proportions ($p \geq .26$, two-tailed). Besides, they analyze first movers' behavior across treatments as those participants might anticipate any effects caused by the used procedure that could have an effect on their choices. However, this is not the case. To conclude, the experimental evidence provided by *Brandts* and *Charness* (2000) suggests that preferences do not depend on the elicitation method: the strategy method seems not to affect sequential responses.

A related study was provided by *Schotter* et al. (1994) who study framing or representation effects. They confront their participants with either the normal form (presented as matrix with simultaneous moves) or the extensive form (presented as tree with sequential moves) of simple games. Although all games generate the same equilibrium outcome, data suggests that the strategy chosen depends significantly on the presentation of the decision problem. They observe significant differences in player behavior between a normal form representation using the strategy method and an extensive form representation using the sequential method. *Brandts* and *Charness* (2000) argue that the combined presentation and elicitation procedure effect may evoke these significant differences. Indeed, one of the findings by *Schotter* et al. (1994) is that player behavior is independent of the preference elicitation method when holding the form in which the game is presented constant.

By contrast, *Güth* et al. (2001) report modest evidence on games in which behavior is not robust to elicitation methods. In their experimental test, subjects play a slightly altered version of the mini ultimatum game (*Bolton* and *Zwick*, 1995) in which only two offers, a fair and an unfair one, are feasible. *Güth* et al. (2001) replace exactly equal splits by nearly equal splits. If behavior is robust, these minor payoff changes would not matter. Indeed, this is true when the strategy method is employed. However, when subjects play the game sequentially significant differences are observed. Any other design failed to reveal the behavioral relevance of exactly equal splits. Thus, their

experimental data suggests that elicitation methods can significantly affect behavior, at least in some games. *Brosig et al. (2003)* also find significant differences between the sequential and strategy method. They asked subjects to play one out of two one-shot sequential games presented in a 2 x 2 matrix format which were similar to the trust game with one exception: if subjects have chosen the inefficient outside option (no trust), second movers could punish them. The authors test not only for differences between the elicitation procedures but between different costs of punishment (high versus low). They find that the use of the strategy method influences behavior in certain environments. Second movers punish unfriendly first movers' behavior only when the strategy method is used and when confronted with low costs. Furthermore, first movers' behavior differs depending on whether participants are in the sequential or strategy treatment.

In contrast, *Cason and Mui (1998)* find no considerable significant difference between the ordinary and the strategic play of the sequential dictator game. In the standard treatment of the game, participants make two dictator decisions and receive information regarding another subject's dictator allocation before making their second decision. In the strategy method treatment, participants receive no information between their two decisions. After having submitted the first dictator allocation, subjects fill out a form specifying a complete strategy contingent on all possible first dictator decisions selected by the subject with whom they were randomly paired.

The authors compared first dictator choices as well as the second dictator choice in the standard treatment with the realized second dictator choice in the strategy method treatment. They did not observe behavioral differences in distributions, means or the fraction of individuals' maximizing payoffs. They did note a minor difference between both treatments: 27 of 40 participants change their decisions in the strategy method treatment, while only 16 participants change their decisions in the standard treatment (Fisher's exact test, $p = .03$, two-tailed).

Besides direct experimental tests of whether there is a difference in player behavior between games played with the sequential method and those played with the strategy method, some papers examining other topics are relevant. For example, *Weber et al. (2004)* show that the timing of unobserved moves can affect decisions. *Rapoport (1997)* finds that two different extensive forms corresponding to the same normal form yield different decisions. However, as *Brandts and Charness (2000)* note, his games are of fairly high complexity. Thus, they argue that the strategy method can be seen as a valid technique.

Blount and Bazerman (1996) demonstrate an inconsistency in how people weight fairness concerns depending upon how preferences for outcomes are elicited. They compare responder behavior in the ultimatum game under

two different elicitation procedures: choice and minimum acceptable offer. In the choice treatment, responders are asked to respond in advance to each possible proposal by stating whether they accept or reject it. In the minimum acceptable payoff treatment, responders are asked to state the minimum payoff that they are willing to accept. In doing so, participants determine how much deviation from the equal split they are willing to tolerate. This frame is expected to result in an evaluative process that focuses greater attention on comparative, rather than absolute, payoff concerns. *Blount and Bazerman* (1996) find that subjects place significantly more weight on fairness considerations in the minimum acceptable offer treatments and accepted more unequal offers under the choice treatment.

3.2.2 Differences between preference elicitation procedures

Researchers using the strategy method do not believe that the use of this elicitation method is an important factor in the results observed (*Charness and Rabin*, 2002). Indeed, some experimental findings support this belief (*Brandts and Charness*, 2000; *Cason and Mui*, 1998; *Oxoby and McLeish*, 2004). Besides, neither standard game-theoretic nor extended models based on reciprocity (*Dufwenberg and Kirchsteiger*, 2004; *Falk and Fischbacher*, 2006; *Rabin*, 1993) or inequality aversion (*Bolton and Ockenfels*, 2000; *Fehr and Schmidt*, 1999) predict any impact of the elicitation procedure on behavior.

However, there are plausible reasons why the strategy method induces different behavior than a direct-response method. These are discussed in the next section. There is also experimental evidence suggesting some differences in the behavior of players based on which methods is employed (*Brosig et al.*, 2003; *Croson*, 2000; *Güth et al.*, 2001; *Shafir and Tversky*, 1992). It should be noted that the preference elicitation procedures differ in at least three points:

1. available information
2. number of decisions
3. order of play

First, under the strategy method, participants are asked to base their decisions on hypothetical actions that might be taken. That is, they have information about any feasible first move, but not about the actual move

taken. In contrast, the sequential elicitation method allows players to observe the action taken before deciding how to react. Participants definitely know the first move, but they do not necessarily know all of the feasible ones. Thus, the procedures differ with respect to the information that players have available to base their decisions upon. Second, both environments vary according to the number of decisions. In the strategy method, people are required to consider decisions for each of the subgames, not just for those subgames which arise during the course of play. That is, depending on the structure of the game in question, participants have to make not only one, but several or even many decisions. Third, the order of play is different and thus the timing of the decisions differs. When considering the temporal structure of either method, we find that under the strategy method, decisions are presented simultaneously and thus are temporarily combined. In contrast, the sequential elicitation method usually requires players to make only one decision at a time. None of these differences has an effect upon the resulting payoff, and thus, game-theoretic predictions do not differ depending on the preference elicitation method.

However, a broad body of evidence suggests that any of these methodological differences can affect decisions. For instance, *Bazerman et al. (1999)* provide evidence that the number of decisions may have an effect on the decisions themselves, and *Weber et al. (2004)* show that the temporal ordering of decisions is of great importance. Why do participants take different actions under the different procedures? The literature contains a number of suggested explanations. Whereas experimental environments using the strategy method have been referred to as “cold”, “hot” environments are given by situations in which only effective actions are recorded, i.e. participants have to respond to the decisions made by their partners. From a standard game-theoretic point of view, results should be the same whatever decision procedure. However, it seems intuitive that people show stronger emotional responses to real actions than to hypothetical ones. Especially in games where individuals can sacrifice part of their own monetary payoff in order to reward (i.e. increase the other person’s payoff) or punish someone (i.e. decrease the other player’s payoff), one might expect different actions depending on the elicitation method.⁴ This argument is closely connected with the psychological phenomenon that individuals attach too much weight to salient or vivid events and too little weight to non-salient events (*Cason and Mui, 1998*).

Furthermore, there is evidence that simply knowing one player moves first can affect behavior, even when the first mover’s action is unobservable.

⁴See *Loewenstein and Schkade (1999)* for a discussion of the “hot-cold empathy gap”.

Building upon it, *Weber et al.* (2004) conduct an experiment that avoids, unlike many previous studies, confounding timing and response modes by either confronting participants with specific offers or eliciting an minimum acceptable offer (MAO). Their findings suggest that timing matters even when players can not observe each other's moves. They suggest that similar findings by other researchers may be explained by focusing on the timing of decisions. For example, *Blount and Bazerman* (1996) find differences in players' MAOs when players were asked to provide the MAOs directly compared to when the players were provided with a list of possible offers from which to select. *Weber et al.* (2004) explain the observation of substantially higher MAOs in the direct treatment compared to the list method by noting that the list method creates a proposer-first timing cue that the direct method does not create.

Bazerman et al. (1999) review a broad body of evidence demonstrating that the temporal structure of elicitation, i.e., whether the options are sequentially or simultaneously presented, determines preferences. The authors discuss several alternative cognitive explanations, including the *norm theory* (*Kahneman and Miller*, 1986; *Ritov and Kahneman*, 1997), the *want/should proposition* (*Bazerman et al.*, 1998) and the *evaluability hypothesis* (*Hsee et al.*, 1998). The norm theory notes that people tend to use comparisons to make decisions and must work harder to evaluate a single option than to evaluate several options presented together. In fact, when faced with a single item to evaluate, the player evokes a set of available, internal references for comparison and evaluates the item in the context of these references. However, when individuals are presented with more than one item to evaluate, the alternatives themselves provide the comparison set of evaluation. If there are differences in category membership across the alternatives, reconciling these category differences will dominate assessment.

The want/should proposition presumes a tension between what individuals want to do versus what they think they should do. If only one item is presented, a counterbalancing alternative is lacking and there is a tendency to do what we want instead of what we should. However, if confronted with more than one option, we lean towards the most justifiable option. An alternative explanation is provided with the evaluability hypothesis. It suggests that when two options involve trading off between a hard-to-evaluate attribute and an easy-to-evaluate attribute, the hard-to-evaluate attribute will have less impact when items are sequentially evaluated due to a systematic bias: in the presence of ambiguous or confusing information, a decision maker will always overweight familiar information. It can be thought of as an informational effect: When more information about the value of an attribute is available in simultaneous evaluation, individuals weight that information

more heavily. *Bazerman et al. (1999)* evaluate whether these three theoretical explanations predict their results. They find that each explanation has one or more limitations and that none of them is able to describe all their data. Thus, the authors develop a framework through which the different approaches are integrated.

Brandts and Charness (2000) provide a possible explanation for the observed lack of differences between the hot and the cold treatment. They hypothesize that their results may be due to two psychological factors operating in different directions and thus reducing the overall effect: First, reactions toward unfavorable first moves might be stronger than hypothetical decisions. Second, it is also easier to hypothetically sacrifice money to punish or reward someone than to actually pay a certain amount of money with certainty.

3.2.3 Strengths and weaknesses of the strategy method

The strategy method as a procedure to generate information on players' preferences has both strengths and weaknesses. *Roth (1995)* notes that its advantages are clear: experimenters acquire data on all information sets of the game for a relatively low cost. *Brosig et al. (2003)* add that the strategy method is often seen as a cost saving substitute to those comparatively more expensive experimental designs where the on-the-spot reactions of individuals can be observed. In order to gain the same number of data by either the strategy or the sequential elicitation method, there is the choice between hiring more participants or conducting various repetitions. Repetitions, however, complicate matters by introducing intertemporal considerations on strategy choices. In these cases, the use of the strategy method avoids a further source of errors. Furthermore, as *Roth (1995)* states, observing participants' entire strategies helps to gain insights into their motivation. *Hennig-Schmidt et al. (2008)* note that for some research agendas it is essential to apply the strategy method as it provides the information needed. *Fischbacher et al. (2001)*, for instance, elicit a contribution schedule (i.e. a vector of contribution) for each player in a standard linear public goods game. If participants choose a contribution schedule that is increasing in the average contribution of others, the authors argue that this can be taken as an unambiguous measure of participants' willingness to be conditionally cooperative. The method even allows them to classify people's behavior according to different "types" of players (see Section).

On the other hand, *Roth (1995)* points out that the possible effects of physical timing in the course of the game are removed as subjects are required to simultaneously make all choices at the same time. For the case of

ultimatum games played by the strategy method, it will not be possible to observe all effects that are due to the responder making her decision after the proposer has made his decision. Besides, since both players know the timing structure of the game, this could also have an effect on the proposer. The author notes that this might be true for purely behavioral reasons not captured in formal game-theoretic models of idealized rationality, but also for reasons rejected in formal game-theoretic models. The notion of subgame perfect equilibria, for example, is lost in transition from the extensive to the strategic form of the game.

Another possible concern with the strategy method is that subjects might be less focussed when submitting entire strategies instead of concentrating on those information sets that arise in the course of the game. This might lower the incentive to think carefully about every possible element of the strategy which in turn may lead to more variance in choices in strategy method treatments (*Cason and Mui, 1998; Walker and Smith, 1993*). Note that *Selten (1967)* himself views the strategy method as a more general procedure that attempts to increase the transparency of the environment.

In addition, the strategy method can be criticized on the behavioral grounds that its hypothetical character may reduce the seriousness with which players play; the strategy method may appear too abstract to simulate a natural setting (*Brandts and Charness, 2000*). *Albert et al. (2008)* argue that the abstract character of the strategy method would be an advantage if it leads to more thoroughly considered decisions - it makes it more plausible that the results are relevant to important decisions that are not taken at the spur of the moment.

The choice of the elicitation method seems to depend to a great extent on the research question. If the examination of emotions is part of the focus of the research, then the strategy method would not be the best choice. However, its use seems to be appropriate if researchers are interested in decisions formed in the absence of strong emotions.

3.2.4 The strategy method within our own experimental work

The experimental work I have conducted or participated in has been realized with the strategy method; a sequential treatment was not utilized. Given our reliance on the strategy method, should we expect our findings to be influenced by our selection of it? Obviously, both preference elicitation procedures differ and each has its own strengths and weaknesses. Common sense suggests that different procedures might result in different behaviors.

Changing from ordinary play of the game to having participants simultaneously make all potential decisions at the same time may amount to a meaningful change in the game itself. However, as this is a change that leaves the game-theoretic predictions unchanged, an unambiguous conclusion cannot be drawn. The experimental findings reviewed in this work suggest that the actual influence of the elicitation method seems to depend on the structure of the game. For some games, it makes a (modest) difference, for others, it does not make a difference. The games analyzed here are rather simple and strategic interdependence is nonexistent, so we should not expect any behavioral differences. However, as only one decision is payoff-relevant, there is a reduced incentive to think carefully about each decision.⁵ This effect might be important in the games analyzed here. However, as we observe significant differences between treatments, the effect should not be of prime importance. Besides, we find that data are in line with previous research using the sequential elicitation method. In addition, *Roth* (1995) argues that the strategy method forces individuals to think about each information set in a different way than if they could primarily concentrate on the information sets that arise in the course of the game. As we want individuals to make thoroughly considered decisions that are not taken at the spur of the moment, that argues in favor of the strategy method; the abstract nature of the method may induce players to more fully consider all possibilities. *Brosig et al.* (2003), among others, report that second movers are more willing to bear the costs of punishment in a hot environment. Thus we should expect responders' willingness to resist to be higher if our games would have been played in a sequential manner, i.e. without using the strategy method. In addition to these reasons to select the strategy method, we decided to use the strategy method because we intended to obtain data on the whole strategy space and could thus gain valuable insights on people's motivation. That is, we asked participants to make decisions not only about resistance levels (*Mertins*, 2008a; *Albert et al.*, 2008) or destruction rates (*Albert and Mertins*, 2008) as reaction to the actual offer, but also to any feasible offer. In case of the study on the behavioral effects of participation opportunities (*Albert and Mertins*, 2008), the method even allows us to give evidence on the behavior of different player types. This is the topic of the following section.

⁵In the threshold public good games, responders are asked to make 6 contingent decisions. In the power-to-take game, responders decide on 6 or 9 decisions, respectively.

3.3 Player types

Contrary to the standard assumption of a universal homo oeconomicus, a substantial number of people exhibit social preferences. That is, they are not solely motivated by material self-interest but also care about the material payoffs of relevant reference agents (*Fehr and Fischbacher, 2002*). As the standard assumption that people are exclusively motivated by their material self-interest does not hold up, how should we model player behavior? I argue that focusing on individual differences among participants may help to better understand and model behavior in experimental games. The study of individuals' heterogeneity is a promising approach for future research for several reasons. First, various theoretical approaches, inspired by numerous robust empirical findings, model social preferences by focusing on relative payoffs. These approaches include preferences for reciprocity (*Charness and Rabin, 2002; Falk and Fischbacher, 2006; Rabin, 1993*), inequity aversion (*Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000*), pure altruism (*Andreoni, 1989*) and envious preferences (*Kirchsteiger, 1994*).

Such models of social preferences predict different behavior than models focusing solely on the individual's payoff. One may argue that any model can be justified if it accurately predicts behavior. However, a model may be more accurate with one group of individuals than with another group of individuals. By assessing the predictive power of the *Fehr/Schmidt (1999)* model of inequity version, *Blanco et al. (2007)* show that the model performs well at the aggregate level, but less well at the individual level. Thus, classifying individuals according to their behavior seems to be legitimate. Furthermore, to better understand individual behavior, recent approaches analyze whether individual differences in personality dimensions and demographic variables are correlated with player types. *Kurzban and Houser (2001)*, for instance, find that player types are correlated with the personality dimensions self-monitoring, self esteem, neuroticism and conscientiousness. It has also been shown that different types differ in their ability to signal the own type and to recognize the type of others (*Brosig, 2002*).

Second, different player types influence the dynamics of play in group environments and thus alter a group's cooperative outcome.⁶ Consider for example the well-established phenomenon of declining contributions in public good experiments. Cooperative decay has been shown to depend on a group's composition (see e.g., *Burlando and Guala, 2005; Houser and Kurzban, 2003; Kurzban and Houser, 2001*). By identifying different player types, one might

⁶The overwhelming majority of analyzed experimental studies mentions this argument as motivation for their conducted research.

better predict outcomes and even may influence the outcomes. Recent work on “assortative regrouping” (*Fischbacher* and *Gächter*, 2008) uses the typing approach to form groups exogenously (e.g., *Burlando* and *Guala*, 2005; *Dannenberg* et al., 2007; *Gächter* and *Thöni*, 2005; *Gunnthorsdottir* et al., 2007; *Page* et al., 2005). *Dannenberg* et al. (2007), for example, show that groups consisting of two fair players contribute more to the public good than groups with two egoistic players or mixed groups. However, it turns out that information about the co-player’s type is crucial: As long as fair players are not informed on the fact that their co-player is fair too, they act like egoistic players. By forming groups exogenously according to player types, *Burlando* and *Guala* (2005) have shown that it is possible to raise the overall level of contribution to a public good. They note that homogenous group formation can be seen to have a similar function to cooperation-sustaining mechanisms such as the punishment of free-riders. Thus, forming groups by player type may also have some practical benefit, for example, in a management context, by preventing free-riding in working groups. Similarly, the presence of free-riders has been shown to put the group in a spiral toward lower and lower levels of contribution with the decay happens faster the more free-rider types are in the group (e.g., *Fischbacher* and *Gächter*, 2008; *Kurzban* and *Houser*, 2001). A similar line of reasoning has been provided in the context of agent-based simulations. Numerous studies suggest that results are highly sensitive to the fraction of player types and the frequency with which they interact (e.g., *Halpin*, 1999; *Lomborg*, 1996).

The third reason that focusing on heterogeneity is a promising approach is that the study of averages might mask substantial differences in individual behavior.⁷ Assume a standard one-shot public goods experiment. Purely selfish individuals will contribute zero regardless of others’ contributions. Treatment variations that lead to high contributions by others will not influence their homo-oeconomicus behavior. Consider a treatment variable that changes only the behavior of reciprocity-minded players. If this type is not frequent enough, treatment effects will not show up in the aggregate. However, when focusing solely on reciprocal types, one may identify important influencing factors on their behavior. This approach has been used by *Albert* und *Mertins* (2008) to examine the effects of participation in the decision-making process.

Thus, I argue that it seems to be appropriate to take a further look at people’s heterogeneity. I subscribe to the view of *Andreoni* and *Miller*

⁷To my surprise, this important argument in the realm of experimental economics has been raised rather seldom, exceptions include *Albert* and *Mertins* (2008) and *Kurzban* and *Houser* (2001).

(2002) who postulate that a model that predicts well in the aggregate is not necessarily useful for explaining individual behavior. They suggest that capturing the variety of individual choices and then aggregating them would help to better understand people's behavior.

Kurzban and *Houser* (2001) point to the similarity of the concept of a "type" in both fields of research, economics and psychology: the concept includes ways in which individuals systematically differ from one another. However, they also identify differences in the use of the concept in these different fields. Whereas the game-theoretic concept captures differences in agents' information, belief, and preferences in a particular game-theoretic environment, the psychological concept stresses the important ways in which people differ in their behavior in consistent ways in social settings. *Houser* (2003) defines a person's type by the decision rule (i.e. the way a person's actions depend on personal information) they use. In this respect, decision rules form the link between a person's situation and decisions, and thus, it seems to be natural to define a person's type by the decision rule an individual uses.

Although the discussion of agents' heterogeneity has a long history in psychology (e.g., *Johnson* and *Nohrem-Hebeisem*, 1979; *Kelley* and *Stahelski*, 1970; *Messick* and *McClintock*, 1968) and is increasingly significant in the realm of economics (e.g., *Heckman*, 2001a, 2001b), experimental evidence on the existence of different player types and studies of its implications and relevance, is still very imperfect. In particular, there is neither a well-established methodology, nor even a "common practice" broadly shared by practitioners. There is only some experimental work explicitly accounting for participants' heterogeneity that is reviewed below. What is lacking is a standard approach for classifying people into player types. In this survey article, I review different methods, e.g. type classification based on observed behavior, pre-experimental tests or multi-method approaches. Whereas there is wide variety in the methods used to classify players, there appears to be a convergence concerning the specific types identified and the frequency with which each occurs. This work is intended to clarify the current state of research in the field and to integrate our own experimental work on player types into the broader context.

3.3.1 Classification schemes

I agree with *Kurzban* and *Houser* (2001) who note that there seems to be agreement that players use strategies that differ in systematic and discernable ways but do not find consensus on the correct classification scheme. Early studies such as *Suleiman* and *Rapoport* (1992) use rather broad schemes,

dividing participants in three groups of players: those who are concerned with equity, those who try to maximize their utility, and those whose play defied easy categorization.⁸ Later studies propose a division into four categories.⁹

However, some consistent features of these disparate classification schemes can be identified.¹⁰ Most studies divide their participants in a category consisting of players who behave as the standard economic theory predicts. This type is called *Homo Oeconomicus* (Albert and Mertins, 2008) or (*Strong*) *Free-rider* (Burlando and Guala, 2005; Kurzban and Houser, 2001, 2005), depending on the game in question. This type shows no reciprocity, behaving instead just like the *homo oeconomicus* model predicts. Albert and Mertins (2008) analyze behavior in a power-to-take experiment and find that 25% of responders behaving like payoff-maximizers, whereas Andreoni et al. (2005) observe in a similar game that only 12% appear motivated to maximize their own payoffs.¹¹ Kurzban and Houser (2001) note that about 28% of participants belong to this type, Kurzban and Houser (2005) classify 20% as Free-riders, and Burlando and Guala (2005) observe a fraction of about 32%.

The next category consists of *Unconditional Cooperators* (Albert and Mertins, 2008) or simply (*Strong*) *Cooperators* (Burlando and Guala, 2005; Kurzban and Houser, 2001, 2005). This type also shows no reciprocity but behaves cooperatively even in the face of non-cooperative behavior. The number of people belonging to this type seems to be similar with observed fractions ranging from 13% (Kurzban and Houser, 2005) or 18% (Burlando and Guala, 2005) to 25% (Kurzban and Houser, 2001).

The third category contains players who reciprocate, called *Reciprocators* (Burlando and Guala, 2005; Kurzban and Houser, 2005), *Homo Reciprocans* (Albert and Mertins, 2008), or *Conditional Cooperators* (Fischbacher et al., 2001). This type responds to others' behavior by using a conditional strategy and shows reciprocity to various degrees. By studying reciprocation in a one-shot public goods experiment, Fischbacher et al. (2001) count those players to this group who are willing to contribute more to a public good the more others contribute. They note that the underlying behavior can be

⁸Weimann (1994) also suggests that there are three types of players (Cooperative Types, Strong Free-riders and Weak Free-rider), but do not account for noisy behavior as a type on its own.

⁹For instance, Fischbacher et al. (2001) suggest that players can be classified as Conditional Cooperators, Free-riders, Hump-shaped Types, and Others.

¹⁰As Kurzban and Houser (2005) mention, it is interesting to note that social psychologists and economists have postulated similar classification schemes.

¹¹Note that Albert and Mertins (2008) do not differentiate between this and the following type.

considered as either a motivation of its own or as a consequence of preferences related to fairness, such as altruism, warm-glow, inequity aversion, or reciprocity. Reciprocators constitute a large portion of the experimental population (Albert and Mertins, 2008; Burlando and Guala, 2005; Fischbacher et al., 2001). Observed fractions start from 29% (Kurzman and Houser, 2001) and 35% (Burlando and Guala, 2005) and raise up to 51.7% (Albert and Mertins, 2008) or even 63% in one study (Kurzman and Houser, 2005).

A residual group of *Noisy* players (Burlando and Guala, 2005) complements the list of standard types.¹² These participants do not fall clearly into one category (ambiguous or borderline cases) and thus their data are not easily interpretable. The fraction of participants classified as noisy players seems to depend heavily on the aggregation scheme (in the case of cumulative methods) or on the definition of remaining classification groups. Accordingly, Fischbacher et al. (2001) classify less than 7% of participants as noisy whereas Burlando and Guala (2005) and Kurzman and Houser (2001) found 15% and 18%, respectively. Albert and Mertins (2008), who use a rather narrow definition for remaining types, find a fraction of 23.3%. In his early attempt to separate the hypotheses of kindness and confusion as potential explanations for cooperative behavior in public good experiments, Andreoni (1995) shows that about half of all cooperative behavior is the result of errors or confusion.

A further category used in several studies consists of *Hump-shaped* behavior (Fischbacher et al., 2001) or *Triangle Cooperators* (Kurzman and Houser, 2001). These are participants who are (on average) close to Conditional Cooperators up to a certain contribution level by other players. Beyond this level, they steadily reduce their contributions. Kurzman and Houser (2001) observe a fraction of about 3% and thus, they disregard this type in their further research (e.g. in Kurzman and Houser, 2005). Against the background of other studies classifying these behavioral patterns as noisy (e.g., Albert and Mertins, 2008), the observed results of Fischbacher et al. (2001), who find that 14% fall into this category, appear noteworthy. However, when added to the small fraction of players classified as noisy, 7%, their results are indeed similar to the findings of others.

3.3.2 Classification methods

As Burlando and Guala (2005) point out, it is rather difficult to identify and classify subjects according to types. Several techniques have been used in

¹²Whereas some authors define a category called *Erratic Type* (Albert and Mertins, 2008) or *Other Patterns* (Fischbacher et al., 2001), the usual approach seems to be to arrange these participants as an out-of-classification group.

the literature and these are discussed below. Some categorization attempts are more behavioral and others more psychological in nature. However, any classification approach consists of two steps: First, identification of behavioral patterns or attitudes and second, designation to particular pre-defined types. I identified three methods used in the field of experimental economics which I will discuss in greater detail: classification based on observed behavior, classification based on value orientation and classification employing multiple approaches.¹³

3.3.2.1 Classification based on observed behavior

The standard approach for player type classification is based on observed behavior. However, classification methods differ in their concrete realizations in several points which are reviewed in detail below:

- preference elicitation method (sequential elicitation vs. strategy method)
- preference description (utility function vs. best reply)
- timing of classification (pre-experimental vs. simultaneous typing)
- type designation

Classification schemes may either use individual data obtained by repetition of simple measurements (sequential method; see e.g. *Burlando and Guala* (2005) or *Burlando and Webley* (1999)) or strategy profiles achieved through the use of the strategy method (*Albert and Mertins*, 2008; *Fischbacher and Gächter*, 2008; *Fischbacher et al.*, 2001).¹⁴ Basically, any appropriately designed game (e.g. public good game, ultimatum games, dictator games) may be suitable to measure individual preferences. However, as *Kurzban and Houser* (2005) note, public good games provide a natural environment because the results of previous experiments are consistent with the conjecture that types exist and affect cooperative outcomes. Thus, the overwhelming majority of studies employs public good experiments.

¹³Some experimental studies use questionnaire answers referring to the understanding of the game's rules, expectations on others' behavior, individual objectives or the chosen strategy itself to support their type classification approach (see, e.g., *Burlando and Guala*, 2005; *Fischbacher and Gächter*, 2008). Both, *Kurzban and Houser* (2005) as well as *Fischbacher and Gächter* (2008) asked individuals to complete a 10-question quiz that had to be answered correctly before participants could proceed. However, as I am not aware of any study basing its classification solely on questionnaires, I do not consider questionnaires as a classification method on its own.

¹⁴For a discussion of preference elicitation methods see section 3.2.

By allowing to revise their decisions repeatedly, *Kurzban* and *Houser* (2001) propose a particular method of sequential preference elicitation to observe participants' contribution decisions to a public good contingent on the information about others' average contributions. They implement a circular public goods game. At the beginning, players take an initial decision on how many tokens they want to transfer to their individual private accounts and to the single group account.¹⁵ Then, one player at a time is informed of the aggregate contribution in the group account and is given the option to change the own contribution. Play continues as each player in turn is given the same opportunity. The game ends at a pre-determined random point that is unknown by players. Payoffs are given by the final allocation of tokens to the group and private accounts. This procedure allows the researcher to observe how the individual contribution decisions depend on the average contribution to the group account.

However, whereas the authors themselves highlight the straightforwardness of their approach, it builds upon granting players the option of changing their decision after having observed others' decision. Thus, the instructions explicitly advice participants of making the own decision contingent on others' decisions. This may cause people to decide in a different way than when having received the information about others' behavior in a different setting. In particular, an experimenter demand effect, describing a phenomenon of experimental testing in which participants attempt to behave they feel the experimenter wants, cannot be ruled out. In the same line of reasoning, *Fischbacher* and *Gächter* (2008) argue that the approach comprises strategic incentives and repeated game effects.

Classification methods differ also in the applied description of preferences. Whereas some methods aim on eliciting or estimating individuals' utility functions (*Andreoni* et al., 2005; *Blanco* et al., 2007; *Brosig* et al., 2005), most approaches are satisfied with knowing individuals' best replies conditional on others' decisions. In the latter case, these responses are interpreted as particular preferences. *Fischbacher* and *Gächter* (2008), for instance, interpret individuals' willingness to voluntarily contribute to a public good as a participant's cooperation preference; *Albert* and *Mertins* (2008) explain people's willingness to shrink the pie in a power-to-take game as a preference for negative reciprocity. *Blanco* et al. (2007) use the utility function approach to answer a particular research question, i.e. whether the inequality aversion model by *Fehr* and *Schmidt* (1999) has predictive power

¹⁵As tokens in the private account yield a higher monetary return to the participant than tokens in the group account, but tokens in the group account improve the groups aggregate outcome, the public good game in general is considered a clean way to investigate how people trade off their own interests against those of a group.

not only at the aggregate but also at the individual level. By employing the strategy method, *Blanco et al. (2007)* obtain (near) point estimates of the parameters for aversion to advantageous (β_i) and disadvantageous inequality (α_i) for each individual i . The parameter α_i can be derived from the minimum acceptable offers in the ultimatum game. It is defined as the offer that makes responder i indifferent between accepting and rejecting the offer. The parameter β_i is given by the egalitarian allocation, such that the dictator is indifferent in a modified dictator game between keeping the entire endowment and the egalitarian outcome.¹⁶

Classification methods based on behavior also differ in the key question whether behavior is observed before or during the actual experiment. *Dannenberg et al. (2007)* and *Burlando and Guala (2005)*, for instance, follow the pre-experimental typing approach by using a two-steps procedure. In the first step, preferences are measured. Subsequently, participants are classified according to types. In the second step, the same participants interact in a controlled environment under the games' rules for which hypotheses regarding the individual behavior have been derived in advance (*Dannenberg et al., 2007*). By contrast, type classification and hypotheses testing can be simultaneous (see, e.g., *Albert and Mertins, 2008*; *Kurzban and Houser, 2001*).¹⁷

Last but not least, classification methods differ in their applied type designation procedure. A systematic categorization of how individual behavior is assigned to types seems not to be achievable due to the evident heterogeneity in type assignment approaches. Thus, I am not able to discuss all of these procedures as nearly each experimental study uses a different approach. Some methods are extremely simple. *Dannenberg et al. (2007)*, for example, define types according to the individual value of β_i , the parameter for aversion to advantageous inequality. Participants with $\beta_i \in [0.3; 1[$ are defined as egoistic (very little inequity averse) whereas participants with $\beta_i \in [0; 0.3]$ are typed as fair (highly inequity averse).¹⁸

Albert and Mertins (2008) provide a technical characterization of players' strategies and classification into pre-defined types. They define types according to responder behavior, which they describe by an n -tupel 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, they consider the

¹⁶*Dannenberg et al. (2007)* use nearly the same method.

¹⁷The following section provides a discussion on underlying assumptions and consequences of the two basic approaches.

¹⁸No participant has $\beta_i = 0.3$ and the authors do not clarify whether $\beta_i = 0.3$ is counted among the egoistic or fair types.

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)

The authors assume that types play only certain strategies. Type 0 (Homo Oeconomicus) maximizes its experimental payoff, which implies d_0 or d_+ . Type 1 (Unconditional Cooperator) is always cooperative and therefore plays d_0 . Type 2 (Homo Reciprocans) shows (negative) reciprocity, that is, plays d_+ or d_{++} . Type 3 (Erratic Type) 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.

Hence, the authors tentatively classify types on account of their behavior. They classify users of d_0 and d_+ as type 0/1, users of d_{++} as type 2, and users of d_- as type 3. They then validate this classification by looking at some relevant statistics and find that types differ not only in their strategy choices but differ also significantly in their reactions to take rates. This is not implied by the type definition and suggests that types are really different.

A similar approach is used in the seminal work by *Fischbacher et al.* (2001).¹⁹ Participants indicate how much they want to contribute to the public good for each average contribution level of other group members. Thus, by using the strategy method, the authors elicit the contribution schedules of each individual. The category of Conditional Cooperators include those schedules that are increasing and either (weakly) monotonic or have at least a highly significant and positive Spearman rank correlation coefficient between own and other's contributions. Free-riders have contribution schedules that contain zero at all entries, and Hump-shaped contributions are those which are on average close to perfect conditional cooperation for contribution levels of up to 10 tokens of the other group members. Beyond this level, contributions are decreasing. *Fischbacher and Gächter* (2008) use the same preference elicitation mechanism but assign types in a different way, i.e., they assign types by evaluating the slope of a linear regression of the schedule and the mean contribution in the schedule. Free-riders have a slope and a mean contribution of zero, Perfect Conditional Cooperators have a slope of one and

¹⁹The same procedure is used by *Fischbacher and Gächter* (2008). *Ockenfels* (1999) suggests a similar design independently of *Fischbacher et al.* (2001).

a mean contribution of 10 (= half of the endowment).²⁰ Any schedule is depicted in a scatter plot as dot. The abscissa shows the slope of the schedules, whereas the ordinate shows the average contribution in the schedule.

Another approach used in several experimental studies (e.g., *Kurzban and Houser, 2001*) is the procedure suggested by *El-Gamal and Grether (1995)*. It requires one to specify in advance a set of candidate decision rules. A statistical procedure chooses the best subset of these rules and each participant is subsequently assigned to one of the rules in the subset. I exemplify the application by the work of *Kurzban and Houser (2001)*. They assume that each participant is one of four possible types (Free-riders, Cooperators, Conditional Cooperators, and Triangle Cooperators).²¹ They label a contribution as consistent with a particular type if it is in accordance with a pre-determined move.²² Note that some contributions may be consistent with more than one type or may not be consistent with any type. In a last step, the authors assign to each participant the type that is consistent with the greatest number of their moves.²³ If less than one-third of a participant's moves are consistent with one of the types, the player is dropped from the sample. More than 15% of participants are thus eliminated. Players may be classified by only one-third of their moves, ignoring two-thirds of their actions. Thus, their approach can be criticized on the ground that it accounts only for participants' predominant behavior, but that this may still comprise the minority of the players' actions. They find that 60% of players' moves are consistent with their assigned types, thus disregarding a considerable amount of decisions. In particular, the authors observe that Free-riders occasionally contribute relatively high amounts to the public good, whereas Strong Cooperators sometimes contribute little. *Kurzban and Houser (2001)* themselves note that their approach might be too simplistic to capture more complex behavioral regularities.

Kurzban and Houser (2005) expand their own approach by implementing a statistical procedure that uses an individual's linear conditional-contribution profile (LCP) to classify each participant as one of three types.²⁴ A LCP is defined as the outcome of an ordinary least-squares regression of the indi-

²⁰For any other type, the authors do not provide clear specifications.

²¹*Houser et al. (2004)* provide a similar type classification algorithm that does not require one to specify the types of interest a priori.

²²The authors apply rather broad categories. For example, they define a contribution as consistent with the Free-rider type, if it is less than 20% of the endowment. To be consistent with the Conditional Cooperator type requires the contribution to be within 5 tokens (= 10% of the endowment) of the groups' average contribution.

²³The authors note that *El-Gamal and Grether (1995)* provide a rigorous statistical foundation for this approach to type players.

²⁴In this study, the authors do not account for Triangle Cooperators any more.

vidual's contribution decisions on the mean contribution that the participant observes immediately before making a contribution decision. Hence, any contribution strategy reflects two parameters: the intercept and the slope of an individual's LCP. The intercept measures how willing a subject is to cooperate even when the counterparts contribute little to the public good. The slope provides a measure of an individual's responsiveness, both in direction and magnitude, to other's contributions.

Participants are classified according to the individual estimated LCP as follows: If the LCP lies everywhere below 25, the person is typed as Free-rider. This means that the participant is expected to contribute less than half of the endowment at all levels of other's contributions. A Cooperator is defined as having a LCP at or above 25. Hence, this type is expected to contribute more than half of the endowment to the public good regardless others' contributions. The third type, the Reciprocator, is defined for LCPs with positive slopes lying above and below the 50%-line. The Reciprocators' contributions are expected to increase with the group average increasing. Participants with any other LCP are not classified as one of these three types.²⁵

The authors themselves note that there are more sophisticated (and cumbersome) procedures than the one they use. However, they stress an important advantage of the applied approach: It provides a simple, fast, and, as they say, accurate method for inference about individual differences. In order to demonstrate that contribution decisions differ substantially among these types, *Kurzban* and *Houser* (2005) conduct a three-sample Medians test showing that the median per-round contributions for three different average contributions of others are statistically significant with $p < .001$. Furthermore, they validate their type classifications by demonstrating that types predict well the decisions individuals' make in games played afterward (see the discussion on type stability below). In their view, their algorithm is a formal extension of the visual-inspection approach by *Fischbacher* et al. (2001).

3.3.2.2 Classification based on value orientation

Classification may also be based on an independent measure of value orientation, i.e. the weight an individual attaches to her or his own welfare relative to the welfare of others. As *Liebrand* (1984) states, it is assumed in the definition and measurement of social motivation that each motivation has its own utility function as determined by the linear combination of own and other's

²⁵*Kurzban* and *Houser* (2005) observe less than 4% of all participants to have any other LCP.

outcomes. *Griesinger* and *Livingston* (1973) devised a geometric taxonomy defining various social motives by using this procedure which is extensively used in social psychological (see, e.g., *Liebrand* 1984) and, to a lesser extent, in economic research (see, e.g., *Brosig*, 2002; *Levati et al.*, 2007; *Offerman et al.*, 1996). The decomposed game technique can be employed to test individuals for cooperative behavior. To test for different other-regarding behavior, another classification method must be used.

Individual value orientations are assessed by the so-called “decomposed game technique”: Each participant is asked to make several binary choices over combinations of own and other monetary payoffs. All pairs of outcomes lie equally spaced on a circle, centered at the origin (0,0) and with a radius of 15 units of a particular currency. Participants are asked to make choices between two adjacent outcomes on the circle. The horizontal axis measures the amount of money allocated to oneself (x) and the vertical axis measures the amount of money offered to the other (y). A motivational vector in the circle-space is a measure of an individual’s preferences regarding own and other individuals’ payoffs. It is calculated by adding all the participant’s responses. This vector is then mapped back onto the circle and is used to characterize the individual into one of five categories, from most social to least: Altruistic, Cooperative, Individualistic, Egoistic and Competitive.²⁶ Social psychologists have developed this standard classification scheme on the basis of the observed motivational vectors. More specifically, motivational vectors between degree -112.5 and -67.5 are classified as aggressive, between -67.5 and -22.5 as competitive, between -22.5 and 22.5 as individualistic, between 22.5 and 67.5 as cooperative, and between 67.5 and 112.5 as altruistic.²⁷

3.3.2.3 The multi-method classification approach

Another method is the multi-method approach which builds its categorization upon various methods as done for example by *Burlando* and *Guala* (2005). The authors triangulate between various methods: They use four sources of evidence and the aggregate classification comes from weighted single classification results. There are various reasons for a multi-method approach.

²⁶ *Offerman et al.* (1996) concede that this classification is ad hoc, but is in their view very natural.

²⁷ *Carpenter* (2005), among others, uses 4 categories: Motivational vectors that fall in the range of 112.5 to 67.5 degrees are classified as altruistic, between 67.5 and 22.5 as cooperative, between 22.5 and -22.5 as egoistic, and between -22.5 and -67.5 as competitive. *Liebrand* (1984) argues for distinguishing between four classes of social motivations, that is: altruism, cooperation, individualism and competition.

First, a single method may to be considered as insufficient to adequately classify types due to its inherent shortcomings. Accordingly, the authors state that a multi-method approach is better than the one that would be provided by each method applied in isolation. For the case of questionnaires, *Burlando and Guala (2005)* note that this method is not highly valued in experimental economics. Second, different tasks may tap on different aspects of a complex psychological construct that a single approach might overlook. Last but not least, the authors stress that their experimental data would clearly support their approach. Participants were classified according to the multi-method approach discussed before. Then, people were redivided in homogenous groups and played the game again. As behavior differs significantly between both sessions, the categories cannot be considered to be totally arbitrarily or ill-defined. Besides, the authors observe a high degree of convergence between the various classification methods. Note that this finding also speaks in favor of any of the methods used. Thus, a multi-method approach seems not to be mandatory. *Burlando and Guala (2005)* recognize that their approach can be criticized for its reliance on intuitive rather than theoretically-founded measures. Indeed, their assignment of weights to the four classification methods seems to be ad-hoc: they put 40% weight to the observed data, and only 20% to each of the three other measurements. Besides, only in cases where the different procedures do not converge at all or ties occurred, a player was assigned to a “noisy” group. Thus, it was sufficient to reach a 50% threshold in order to be assigned to one of the types.

The crucial factor for *Brosig (2002)* to use different classification procedures is the objective to test for different forms of other-regarding behavior. She identifies participants’ cooperative behavior by employing the decomposed game technique and tests for individuals’ fairness by using a dictator game. Type classification according to the degree of cooperative behavior follows the standard classification procedure developed for this technique. Depending on individuals’ dictator game giving, people are labeled “self-centered” (responder receives less than one-third of the endowment) or “beneficent” (responder receives one-third or more of the endowment).

The use of a multi-method approach in order to classify individuals points to the still preliminary character of player type classification. To date, there is no generally used method, no common practice yet.

3.3.3 Consistency and stability of individual behavior

When typing individuals according to their observed behavior, we are not obliged to make any assumption about consistency and stability of individual behavior. Following *Kurzban and Houser (2005)*, I subscribe to the view that

the notion of type must be understood with respect to a particular class of decisions building upon specific institutions. Experimental evidence suggests that even minor changes (e.g., whether cooperation is more or less costly) may change individual behavior significantly (see, e.g., *Isaac and Duncan, 2000*). Accordingly, *Kurzban and Houser (2005)* postulate the need for additional research to determine the conditions under which individuals separate into types, and which institutions have the effect of homogenizing play.

Some studies, however, use pre-experimental tests²⁸ to classify player types (e.g., *Burlando and Guala, 2005; Dannenberg et al., 2007; Fischbacher and Gächter, 2008*). When doing so, the questions occur whether individuals behave consistently within and across different classes of games and whether individuals behavior is stable over time. *Brosig et al. (2007)* note that standard economic theory as well as any theory of other-regarding behavior assumes that individual preferences do not vary across games and over time. *Dannenberg et al. (2007)* merely assume that preferences are stable at least within a short time period.

Fischbacher and Gächter (2008) measure people's cooperation preferences in a public good game and observe the same participants in a sequence of ten one-shot games. Then, they analyze whether elicited preferences have any predictive power in explaining actual contributions. They provide clear evidence that observed behavior significantly depends on measured preferences: average cooperation levels are highly correlated (Spearman's $\rho = .40$, $p = .0000$). Besides, they provide experimental evidence that elicited preferences are not affected by the order of the used elicitation procedure.

Kurzban and Houser (2001) point to the observed homogeneity in behavior for a given player across different groups. They find that type differences are consistent in the sense that players typed as Strong Free-riders tend to pursue this strategy in different groups.

Within and across game consistency is investigated by *Blanco et al. (2006)* and *Brosig et al. (2007)*. *Brosig et al. (2007)* confront participants in a within-subject design with different variants of modified dictator games and the prisoner's dilemma (three classes of games). They test whether participants showing either purely self-interested behavior or other-regarding behavior according to the theories by *Charness and Rabin (2002)*, *Fehr and Schmitt (1999)* or *Andreoni and Miller (2002)* have consistent preferences. Besides, *Brosig et al. (2007)* test whether preferences are stable over time by repeating

²⁸Note that I use the term "pre-experimental test" for any two-step approach observing the same individuals twice, once in order to observe actual behavior and once to assign types. Thus, I do not differentiate between pre- and post-experimental tests. *Fischbacher and Gächter (2008)*, for instance, control for possible order effects by applying a random matching protocol.

the experiment three times with the same participants within three months. With regard to within game consistency, the authors find that all measures of consistency explain a significant fraction of observed individual behavior, with consistency increasing during the course of the game. This increase is due to the fact that, over time, more and more subjects make consistently selfish decisions. The strongest test for the consistency of individual behavior is given with the concept of consistency across games, i.e. the comparison of decisions made by a particular individual in different strategic situations. *Brosig et al.* (2007) report a rather low proportion of consistent behavior across games compared to the proportion of within-game consistency. In the third wave, selfishness clearly dominates behavior and about 60% of all decisions are consistent across games.

To test for stability of individual behavior, the authors observe whether individuals make always the same decision in the same game. They find that people change their behavior tremendously, but these changes have only one direction, which is common to all participants. They show purely self-interested behavior. *Brosig et al.* (2007) could not find any other-regarding behavior which is stable over time. Stable behavior can be observed only by those, who behave strictly selfish. Accordingly, in those classes of games, which reveal a high number of selfish moves, a high frequency of stable behavior can be found (up to 60% on average over all three waves). The authors offer two plausible explanations. First, individuals may learn to be selfish, or second, individuals' obligation to help others is fulfilled by helping once.

Blanco et al. (2006) add to the analysis of within and across game consistency within a similar framework using four different experimental games. They differ from *Brosig et al.* (2007) in that they solely focus on inequality aversion as modeled by *Fehr and Schmidt* (1999). Their data exhibit some degree of consistency and correlation between decisions: about one third of participants behave consistently in comparable games. The partial failure of the model poses some questions. Are preferences inconsistent, indeed or is the model simply not able to include the multiplicity of relevant behavioral forces? The authors note that altruism, for example, is not consistent across games if a participant behaves altruistically in one game but not in another. Another explanation is that there is not such a simple motivation as altruism. Instead, there are several motives. As further explanation for observed inconsistent behavior, the authors mention that participants may make different choices in the same game if the situation has changed in other respects.

By letting participants play some out-of-sample games with randomly reassigned partners, *Kurzban and Houser* (2005) also test for preference stability. They find that an individual's type is stable over time: Free-riders

continued to contribute less on average than their counterparts, Cooperators more, and Reciprocators about the same. Using a three-sample Medians test, they find that the median contributions of the three types are statistically significant at $p < .001$.

Brosig (2002) tackles a related question in evaluating the relationship between two kinds of other-regarding behavior (cooperation and fairness) measured by two different classification procedures. First, she finds that the order in which the two classification procedures are administered exert a significant influence on the allocation decisions in the dictator game. Participants who played the dictator game first give significantly more to the anonymous responders than participants who first filled in the questionnaires for the decomposed games. Second, the two kinds of other-regarding behavior seems to be unrelated to each other. The Spearman rank-order correlation coefficients, relating the vectors in the decomposed games to the dictator allocations, are not significant. Thus, the decisions in both classification methods reveal different behavioral characteristics. Accordingly, *Kurzban* and *Houser* (2005) suggest not to map social value orientations onto types derived from behavior in two-player, two-option games as n-player and two-player interactions differ significantly. They refer to the mixed evidence found in the psychological literature: Whereas *Liebrand* (1984) provides evidence that an individual's social value orientation can predict behavior in resource dilemmas, it does not so in public good games (*Parks*, 1994). To conclude, individual-level comparisons have rarely been conducted and thus, little is known to date about how individuals play across and within games and over time.

3.3.4 Type classification within the own experimental work

Albert and *Mertins* (2008) test the conjecture that participatory decision making may increase acceptance even of unfavorable decisions using an experimental 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. First, we analyze people's behavior in the aggregate and 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. However, we do not find significant treatment effects. As averages

may mask substantial heterogeneity in individual behavior, we account for this fact by classifying responders according to the strategy chosen. Similar to *Fischbacher et al. (2001)*, we apply the strategy method (1967) to directly elicit individuals' preferences. According to the chosen destruction rates depending on the claimed transfers, we distinguish between four types: the Homo Oeconomicus (type 0), the Unconditional Cooperator (type 1), the Homo Reciprocans (type 2), and the Erratic Type (type 3).²⁹

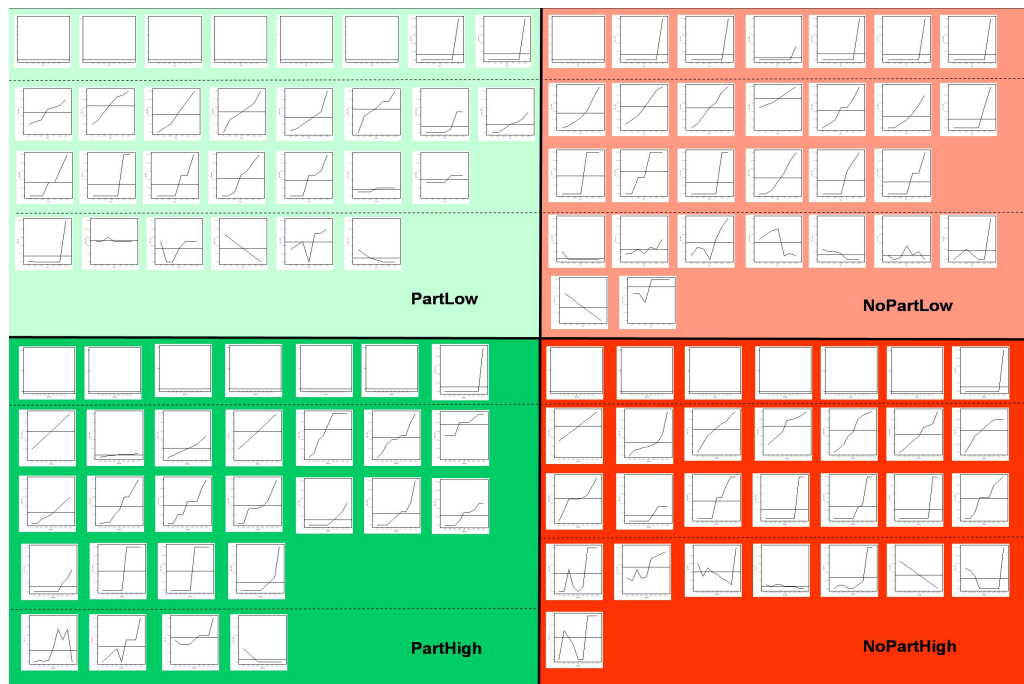


Figure 3.1: Responder types

In figure 3.1, the destruction schedules of all 116 responders are displayed, sorted by treatment and type.³⁰ Note that the meaning of Homo Reciprocans in our design differs clearly from other classifications. As we capture destruction rates depending on claims, the schedules do not reflect conditional cooperation but rather conditional punishment and thus some kind of negative instead of positive reciprocity. Thus, our schedules and those by *Fischbacher et al. (2001)*, similar at first sight, display a different relationship. Nevertheless, the fraction of Reciprocators (irrespective of either being motivated by positive or negative reciprocity) seems to be exceedingly robust.

²⁹We do not differentiate between type 0 and 1, as in our design, their strategy can only differ with respect to the case where destruction is costless.

³⁰Appendix A.1 contains four separate figures, one for each treatment.

Whereas we find 51.7% to be classified as Homo Reciprocans, *Fischbacher et al. (2001)* report exactly 50%. Note that our classification scheme, in contrast to *Fischbacher et al. (2001)* among others, is rather strict: Contribution schedules that show an increasing trend but display at least one slight negative deviation from the trend are assigned to the erratic type.

With respect to our research question, we can restrict the hypothesis of participation affecting the outcome acceptance to type 2 as non-reciprocal responder types are not influenced by treatment variation. By definition, type 0 and type 1 behaviors cannot be affected by the treatments. With respect to the erratic type, any influence is possible, but the literature contains no hypotheses. Thus, we use the type classification approach to identify those individuals who might be affected by participation opportunities. For the Homo Reciprocans a clear hypothesis emerges: If, as conjectured, participation is an aspect of fairness, negative reciprocity should be less pronounced in the participation treatments. Indeed, we find that the participation effect is highly significant for those responders who show negative reciprocity.

Note that our work differs from any of the discussed studies in that we use the type classification approach to identify those reciprocity-minded individuals who are potentially affected by treatment variation. We then focus solely on this particular type and compare their behavior within different treatments. By contrast, other studies identify various types to explain group outcomes by the groups' actual compositions (e.g., *Burlando and Guala, 2005*; *Fischbacher et al., 2001*) or to evaluate different motivations for cooperative behavior (e.g., *Levati et al., 2007*) without implementing different treatments. Our approach can be criticized on the ground that we did not assess *a priori* preferences. Whereas we do not expect preferences to be influenced by treatments, we cannot completely eliminate the possibility that people's chosen strategies are affected by the assigned treatment. For instance, a player having the opportunity to participate in the decision making process (treatment part) may behave like the homo oeconomicus model predicts, whereas the same individual might have shown some kind of reciprocal behavior in the no-part treatment. Implementing an *a priori* type classification method, accounting for negative reciprocity, before assigning participants to treatments would rule out this possibility.

3.4 Conclusion

The literature on type classification reviewed above is primarily concerned with the identification and analysis of individuals' cooperation preference and its implications for group outcomes. Accordingly, the existence of dif-

ferent behavioral types has been confirmed by experiments using threshold public good games. This class of games is commonly used to investigate how individuals trade off their own interests against those of a group. Thus, the study by *Albert and Mertins (2008)* differs from what has been done before in several aspects. By analyzing a power-to-take game, it focuses on negative reciprocity and categorizes individual behavior according to that dimension. It aims on identifying those individuals that are concerned with reciprocity and thus are likely to respond to procedural variations. In doing so, the study applies the strategy method to classify players into types. As discussed before, this method is extensively used and seems to be an appropriate tool to elicit preferences and to classify behavior. It proves to be a standard procedure and a legitimate tool in many contexts.

To conclude, this survey illustrates the recent increasing emphasis on the importance of individual differences in understanding and modeling behavior in experimental games. I argue that future work should develop more sophisticated type classification methods to extend the player type approach. I follow *Andreoni et al. (2005)* who claim that accounting for people's heterogeneity is key to a successful account of bargaining behavior. I discussed attempts that provide several interesting findings. However, many open questions remain. Experimental research on player types is far from having provided a common practice for researchers to follow. In addition, more detailed findings on individuals' heterogeneity could perhaps put renewed emphasis on theories and experiments on bargaining behavior. Furthermore, scientific progress in the identification of different player types may inspire new research on institutions that perform well in the presence of heterogeneous agents.

Part II

Experimental evidence

Chapter 4

Experiment 1: Procedural judgements

4.1 Introduction

One persistent finding in experimental economics is that not only outcomes but also procedures leading to them matter (e.g. *Albert and Mertins, 2008; Blount, 1995; Bolton et al., 2005; Brandts et al., 2006; Charness and Levine, 2007; Falk et al., 2008; Fukuno and Ohbuchi, 2003*).¹ In particular, procedures seem to play an eminent role in workplace relations: behavioral reactions to promotion decisions, bonus allocations or dismissals have been shown to significantly depend on the selection procedure. Thus, it seems to be a factual necessity – for organizations or any other institution – to respect the procedural preferences within a group. In particular, one may argue that a procedure needs to have at least some support within a group to ensure that people submit information and respect outcomes (*Dietrich, 2005; Dolan et al., 2007*). The latter aspect is the focus of our study.

Consider a group of employees which chooses a person among them to be the “team leader”. With the selection, the team leader is given some de-

¹Only recently, economists begun to investigate the role of procedures. In contrast, there is a broad consensus among other social scientists that people react differently to identical outcomes depending on the way in which decisions were taken, see e.g. *Grimes (2006), Konovsky (2000), Thibaut and Walker (1975), Tyler (1990), Tyler and Lind (2000)*. Economists just started to theoretically address the issue of procedural concerns. Recent work includes *Bolton et al. (2005), Krawczyk (2007) and Trautmann (2007)*. *Sebald (2007)* provides an game-theoretic framework that integrates procedural concerns into economic analysis. Empirical work on procedures include *Bischoff et al. (2008)* who show by analyzing representative survey data that procedural fairness plays an important role for the acceptance of a given income distribution.

gree of distributive power. Henceforth, she makes allocation decisions whose outcomes affect all group members.² Do team members judge allocations solely according to outcomes or do they consider the procedure that led to the selection of the team leader and thus in turn led to the allocation chosen by the team leader?³

Our research is closely related to the experimental study by *Brandts et al.* (2006). They tackle the question whether or not a selection procedure is able to change individuals' goodwill towards others in general and/or towards the selectors in particular. The authors presume that people may have a tendency to avoid the guilt that results from disappointing the expectations of others. Their first hypothesis posits that selected allocators are more generous towards others than random allocators ("I-want-YOU effect"). Their second hypothesis states that the selected party will favor the selecting party more than the third party involved ("gratitude effect"). There is some statistical evidence for both effects.⁴ *Albert et al.* (2008) confirmed this finding within a different experimental framework. They show that proposers claim significantly less if responders' group preferences regarding proposer's appointment procedure were satisfied than if they were violated ($p = .046$, Mann-Whitney test, one-sided). Both studies provide evidence that the selected party's (i.e. proposers') motivation is affected by selection. We pick up the topic by expanding their research question on the selecting party's (i.e. responders') behavior.

There exists by now a broad consensus that property rights induce fairness norms and are thereby crucial for any kind of outcome (*Frey and Bohnet*, 1995; *Sudgen*, 1986). Proposers claim a share and responders accept or reject a proposed share according to their individual fairness norm. Typically, roles in bargaining experiments are assigned by a random procedure. In these cases, the strength of property rights seems to be accompanied by a fairness norm suggesting that all players have (more or less) the same right to a share, resulting in a high probability for 50:50 allocations. This normative right was shown to be altered by the introduction of effort to obtain the pie. Numerous studies provide evidence on this "entitlement effect". *Hoffman* and

²Another example are employees who choose among them their representatives and thus allow them to lobby. Beyond labor market relations, one might think of any democratic system in which citizens elect the government.

³Note that in many situations, people do not choose a procedure leading to an outcome but choose an allocator by an appointment procedure and thus give them some degree of distributive power.

⁴By using a Mann-Whitney test, the "I-want-YOU effect" is confirmed in the first round ($p = .043$), but not in the following rounds ($p = .089$ and $p = .48$). The same is true for the "gratitude effect" (round 1: $p = .042$, Wilcoxon signed-ranks test)

Spitzer (1985) and *Hoffman* et al. (1994), for example, show that when the role of the proposer was earned (e.g. by scoring high on a general knowledge quiz) rather than being randomly assigned proposers offer less and responders accept more unequal offers.

We argue that besides role allocation by chance or effort, there is a further variable deserving recognition: role assignment by a procedure judged right to use or fair. A growing literature in the social sciences indicates that people seem to care not only about outcomes themselves but also about how they emerged in the first place (for a review, see *Mertins* (2008b)). We test whether individuals' procedural judgments affect their behavior. Responders' demands may vary depending on whether proposers obtained their roles with or without responders' support.

Based on a simple experimental game, we shed light on these questions. One proposer faces four responders who can offer resistance against allocation proposals. Resistance is modeled as a threshold public good.⁵ The public good "resistance" is provided if and only if the sum of voluntary contributions is greater than or equal to a specific threshold. In this case, the proposer gets nothing. We implement an experimental setting with two feasible appointment procedures (AP). These AP's appoint the role of the proposer and thus distributive power. We elicit participants preferences and fairness evaluations over both procedures. Then, we assign either the AP which is favored by a majority or not. Hence, the individual and group procedural preferences of responders may either be satisfied or violated. We consider different procedural aspects discussed in the literature: We investigate whether AP's themselves, fairness evaluations and procedural satisfaction have an effect on responders' willingness to offer resistance against various outcomes. By measuring people's resistance levels, we can identify their willingness to accept various outcomes.

First, we refer to the appointment procedures themselves. A number of studies provide evidence which allocative mechanisms are considered "acceptable" and which not. *Stutzer* and *Frey* (2005) quote some examples where random mechanisms were chosen because they were considered to be an adequate tool, e.g. the selection of persons to form a jury in criminal and civil cases, or random mechanisms to select politicians and public officials in classical democracy in Athens. The authors propose the usage of random selection as a principle in international organizations in order to give every citizen an equal chance of being chosen. Surveys, however, show that a considerable number does not agree with the use of random mechanisms.

⁵The study of threshold public good games may be said to begin with the work of *Marwell* and *Ames* (1979) and *Palfrey* and *Rosenthal* (1984).

Frey and *Pommerehne* (1993) report that raising prices to ration demand is considered to be fair by less voters than allocation by a “first come, first served” rule or allocation by an authority. Accordingly, *Frey* and *Oberholzer-Gee* (1996) report on people’s unwillingness to accept any solutions where to site environmental hazards that include monetary values. *Pommerehne* et al. (1997) rank people’s procedural preferences (in descending order): expert judgment, layperson judgment, allocation by lottery and an auction mechanism. We put two alternative mechanisms to the vote: (AP 1) allocation by an authority (the experimenter), and (AP 2) election. In the following, we try to assess which mechanism is favored and whether the allocated AP’s themselves influence responders willingness to resist.

Second, we investigate the possible impact of procedural fairness evaluations. A critical finding in this line of research is the *fair process effect*: Outcomes from fair procedures are rather accepted than outcomes from unfair procedures, even if the outcomes themselves are viewed as unfair (*Brockner* and *Wiesenfeld*, 1996; *Greenberg* and *Folger*, 1983; *Thibaut* and *Walker*, 1975; *Tyler*, 1990; *Tyler* and *Lind*, 2000). Whereas the fair process effect is said to be extremely robust and one of the most frequently replicated findings in social psychology, there is still little experimental evidence with decisions having monetary consequences (for a survey, see *Mertins* (2008b)). An exception is *Bolton* et al. (2005) who show in an economic experiment that an unfair outcome chosen by a fair (i.e. unbiased) lottery is more acceptable than the same unfair outcome chosen by a third-party. We will test whether participants self-reported procedural fairness judgments on both AP’s affect their willingness to offer resistance against various outcomes against the background of monetary consequences.

Third, we consider participants’ satisfaction with an applied appointment procedure. Procedural fairness judgments and procedural satisfaction judgments have mostly been treated interchangeable as both concepts overlap to a great extent. However, there is empirical evidence that the two constructs do not always show the same effect (*Van den Bos* et al., 1998). Our experimental setting is able to differentiate between effects resulting from either procedural fairness or procedural satisfaction judgments. That is, we test whether perceived procedural fairness and/or procedural satisfaction positively affects how people react to outcomes. To our knowledge, there is no experiment that has explicitly examined this issue before. It has been argued, e.g. by *Ng* (1988), and empirically shown (notably by *Bruno Frey* and colleagues) that the use of the preferred procedure directly yields utility. The latter propose the term “procedural utility” to denote people’s utility provided if their preferences about the process as such are satisfied. *Frey* et al. (2004) measure utility by individuals’ reported subjective well-being or

happiness. *Frey and Stutzer (2002)* show that individuals obtain procedural utility, i.e., by having the mere opportunity to participate in the political decision-making process. Furthermore, it has been said that the way in which decisions are made can affect people’s reactions to those decisions (*Dolan et al., 2007*). In particular, it has been argued that a procedure needs to have at least some support within a group to ensure that people respect outcomes (*Dietrich, 2005*) or support even apparently unfavorable decision (*Dolan et al., 2007*). This consideration is related to the literature on perceived legitimacy as the primary explanation for compliance. *Tyler and Lind (2000)* state the so-called “legitimacy and deference hypothesis”: People obey a decision if they regard the authority, who made the decision, as entitled to be obeyed, irrespective of their own judgment about the decision. We intend to examine the assumed causality: Do people rather comply with a decision (i.e. invest less in the public good resistance) if they support the underlying procedure? We state the formal hypotheses in the following section.

The remainder of this chapter is organized as follows. Section 4.2 describes the experimental design and procedures, and states the behavioral hypotheses we intend to test. Section 4.3 presents experimental results. Section 4.4 provides some discussion and concludes.

4.2 The experiment

4.2.1 The basic game

Consider the following game between five players, one proposer and four responders. Each responder has an initial endowment of $\frac{p}{4}$, where $p > 0$ (in chips) is the total experimental payoff at stake in the game. The proposer has no endowment. The proposer tells responders the tax, that is, the number $x \in [0, \frac{p}{4}]$ of chips she would like to get from each of them. If her proposal goes through, she gets $4x$ while each responder keeps $\frac{p}{4} - x$. However, each responder i can pay $y_i \in [0, \frac{p}{4}]$ on a group account. Responders’ decisions are simultaneous. Payments to the group account are lost to the group. If the amount on the group account reaches a threshold t with $p > t > \frac{p}{4}$, that is, if $\sum_i y_i \geq t$, the proposal is rejected, and each responder keeps $\frac{p}{4} - y_i$ while the proposer gets nothing. Note that no single responder can achieve rejection. If, however, the group account stays below the threshold t , each responder has to pay up to x to the proposer, depending on how much of his endowment is left, and the proposer gets up to $4x$, depending on whether responders can pay her in full or not. Thus, each responder pays $z_i := \min\{\frac{p}{4} - y_i, x\} \geq 0$ and keeps $\frac{p}{4} - y_i - z_i \geq 0$, while the proposer gets $\sum_i z_i \geq 0$.

Resistance is modeled as a threshold public good. The public good is provided if and only if the sum of voluntary contributions is greater than or equal to the cost of the public good t with t being the threshold. The value of a provided public good is x , which equals the proposed tax and is the same for all responders. As the public good is fixed-sized, contributions exceeding the threshold do not affect the level of provision. The applied decision rule is not simply a binary one as players can choose to contribute any integer $y_i \in [0, \frac{p}{4}]$. This reflects the idea that resistance in real world is seldom an all-or-nothing decision. People typically have more responses than either to accept or reject a decision or a claim, e.g. they can join a protest as instigators or mere supporters. Thus, different grades of participation, corresponding with different costs, are conceivable.

In the experiment, the group endowment was $p = 100$ chips, and the threshold was $t = \frac{p}{4} + 1 = 26$. The tax proposal was restricted to multiples of five chips plus zero: $x \in \{0, 5, 10, 15, 20, 25\}$. Responders could choose any contribution $y_i \in \{0, 0.5, 1, 1.5, 2, \dots, 24.5, 25\}$.

For the theoretical analysis, we consider only subgame perfect equilibria in pure strategies assuming that players maximize their own experimental payoffs. Under these assumptions, a subgame perfect equilibrium is a strategy profile $(x^*, y_1(x), y_2(x), y_3(x), y_4(x))$ with the following properties. First, $y_i(x) \leq x$, that is, no responders invests more in resistance than the tax proposal x , because x is the return of successful resistance. Second, $y_i(x) = 0$ for $x \leq 6.5$ because then the maximum collective investments are $4x < 26$, that is, resistance cannot be successful. Third, $\sum_i y_i(x) \in \{0, 26\}$ if $x \geq 6.5$, that is, whenever x is high enough for successful resistance, responders either coordinate on no resistance or on exactly the right amount of resistance because otherwise at least one responder could profit by reducing his investment unilaterally. Thus, for proposals $x \geq 6.5$, the game becomes a coordination game: there are many subgame perfect equilibria in pure strategies which compete against each other.

Given the responder strategies, the best response of the proposer is $x^* := \max\{x: \sum_i y_i(x) = 0\}$, that is, to select the highest value of x for which there is no resistance. Since $x \in \{0, 5, 10, 15, 20, 25\}$, it follows that $x^* = 5$. Note that $x = 5$ means equal division of the 100 chips among all five players if there is no resistance.

4.2.2 Experimental design and procedures

The game is explored by applying *Selten's* (1967) strategy method⁶ for three reasons: First, we intend to acquire a maximum of data. Second, we aim at eliciting contingent decisions that cover the strategy space. Third, we expect that highly unfavorable claims could trigger strong emotional responses in a hot (i.e. emotional) environment, which we intend to avoid. The applied cool frame even strengthens the results offered.

Furthermore, the experimental design has the feature of asking subjects to decide in both roles (as proposer and as responder) before the participant's final role is determined. The purpose is not to connect behavior in both roles but to allow responders to have an idea of proposer's decision situation. However, we will solely focus on responders' behavior.

A total of 100 participants took part in the non-computerized experiment conducted at Saarland University.⁷ The experiment took about 35 minutes and participants earned 4.47 Euro on average.

As indicated in figure 4.1, the experiment consisted of a pre-experimental questionnaire (appendix B.1), three decision stages (appendix B.2) and a post-experimental questionnaire (appendices B.3 and B.4).⁸ Before running the (one-shot) experiment, each participant was asked to fill out a sign-up sheet serving as pre-experimental questionnaire⁹ Thus, participants provided basic demographic information and indicated their average time spent in voluntary work. As part of our experimental design, we had to generate information about participants' age and social commitment. Their answers were used in a subsequent stage to allow voting for a specific group member (the oldest one or the one which volunteers most). Then, all participants were assembled in a large room and randomly assigned to a group of $n = 5$.

In stage one, all participants were asked to decide in the role of the proposer on how to allocate the group pie of $p = 100$ chips with the monetary value of 1 chip being 0.25 Euro. In the beginning, each responder i was given

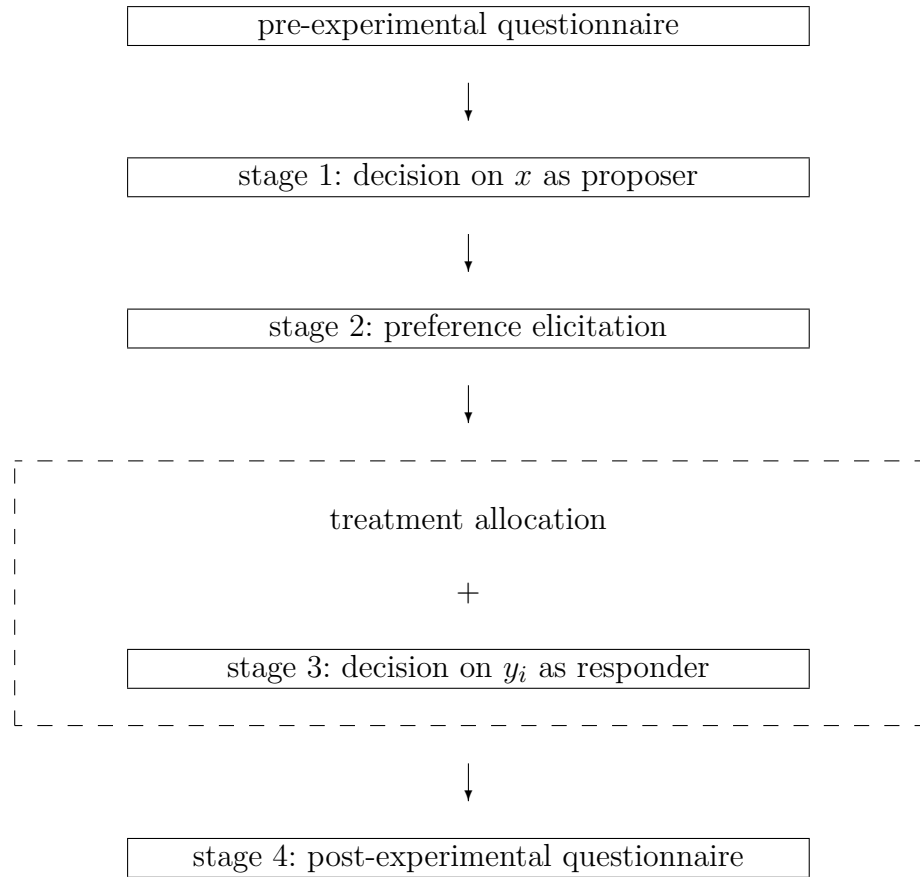
⁶By applying the strategy method, subjects are asked to submit entire strategies by stating what they do conditional on each of the proposers' choices prior to observing the proposers' actual choice.

⁷Session 1-3 with 55 subjects was conducted on 2nd July 2005 and session 4 and 5 with 45 subjects on 12th July 2005. In the former case, participants were visitors of the university's Open Day (most of them were prospective students), and in the latter case, participants were students from various fields. An Analysis of Variance (ANOVA) revealed that no significant differences in the behavior of students and non-students were observable ($p \geq .168$) and thus, data are pooled. We had 51% female participants. The average age was 23.2 with age ranging from 17 to 53 years.

⁸Original (German) instructions are available from the author upon request.

⁹The sign-up sheet is given in appendix B.1.

Figure 4.1: Sequence of events



an initial endowment of 25 chips (6.25 Euro), whereas the proposer had nothing. The proposer proposed a tax $x \in \{0, 5, 10, 15, 20, 25\}$ to be paid by each responder.

In decision stage 2, participants were asked to consider two appointment procedures (AP) for the group's proposer:

- AP 1 (experimenter despotism): The experimenter appoints a proposer.
- AP 2 (majority vote): Group members appoint a proposer by majority vote.

AP 1 was in fact a random mechanism. We drew a lot to determine the proposer; participants, however, did not know this. AP 2 was more difficult to implement since anonymity had to be respected. We asked participants to vote for a criterion to determine the proposer. They had the choice between

age (the oldest group member) and high engagement in student and voluntary activities (the one who spent most time per week on such activities). The relevant information was available from the pre-experimental questionnaire.

First, participants casted their vote for AP 1 or AP 2. Abstention was impossible. As each group consisted of five members, there was necessarily a majority—large (5:0), medium (4:1) or narrow (3:2)—in favor of one AP. In a next step, participants gave reasons for their choice. Moreover, independently of their own preferences with respect to the AP, participants had to vote for one criterion, high age or high engagement, so that, in case of AP 2, a proposer could be chosen. Again, abstention was impossible. Furthermore, participants stated their opinion on the importance of the APs using a seven-point scale ranging from 1 (very important) to 7 (not important at all). Besides, participants ranked both APs on a seven point scale from 1 (very unfair) to 7 (very fair). When stage 2 was completed, we counted the votes and completed instructions for stage 3.

Instructions for decision stage 3 contained information about the results from stage 2. We told participants the preferences of their group, that is, the AP preferred by the majority and the size of the majority. We then told them whether the preference of the majority was respected (treatment M^+) or not (treatment M^-), and which AP, 1 or 2, had accordingly been used to appoint the proposer. For instance, a group may have voted 4:1 in favor of AP 2; participants were given this information together with the information that the will of the majority did not prevail and that, instead, AP 1 was used to appoint the proposer. Another group may have voted 3:2 in favor of AP 2; participants were given this information together with the information that the will of the majority prevailed and that we had determined the proposer according to the criterion preferred by the group. Half of the groups (50 participants) were randomly assigned to treatment M^+ , the other half to treatment M^- . We applied a between-subject design where participants were not aware of the different treatments.

In stage 3, all participants were asked to decide as responders whether or not offering resistance against any feasible allocation. Participants chose their contributions $y_i \in \{0, 0.5, 1, 1.5, \dots, 25\}$ to the responders' group account for each $x \in \{0, 5, 10, 15, 20, 25\}$ the proposer might have chosen (strategy method). The instructions emphasized that these contingent contribution choices were binding and that one would be carried out depending on the actually proposed tax.

When all subjects have finished all three decision stages, completed decision forms were taken to another room to determine the roles according to the rules of the game and to calculate payments. While the calculations lasted, all subjects filled in a post-experimental questionnaire intended to

check their understanding of the game.

4.2.3 Hypotheses

Each responder finds himself in one of four possible situations (see table 4.1 below): Individual AP preferences may be satisfied (situation I^+) or not (situation I^-), and group AP preferences –that is, the preference of the majority– may be satisfied (situation M^+) or not (situation M^-). These situations were determined partly by our treatment (M^+ versus M^-) and partly by the responder’s preference in combination with our treatment (I^+ versus I^-).

Table 4.1: Responder situations

		individual preference	
		satisfied	not satisfied
group preference	satisfied	I^+M^+	I^-M^+
	not satisfied	I^+M^-	I^-M^-

Game-theoretic predictions of responders’ behavior on the basis of egoistic or purely distributional social preferences do not differ between the four situations. Besides, neither the appointment procedure itself nor fairness assessments of it should affect responders’ behavior. Thus, we state the following null hypothesis:

H_0 . Responders’ behavior is the same under any AP, any fairness evaluation of the applied AP and any treatment situation for any claim and claims altogether.

The possibility that participants have procedural preferences, however, motivates different hypotheses. Specifically, we investigate the following behavioral hypotheses, which are suggested by the literature on procedural preferences discussed in the introduction and apply to any tax $x > 0$.

First, the appointment procedure itself may have an impact on responders’ willingness to offer resistance against proposers’ claims:

H_1 . Responders’ behavior differs between AP 1 and AP 2.

Second, previous findings on procedural fairness suggest that individual procedural fairness evaluations play an important role in responders’ willingness to accept or reject an offer by the selected party. In particular, the

procedural fairness hypothesis states that outcomes from fair procedures are rather accepted than outcomes from unfair procedures, even if the outcomes itself are viewed as unfair. This yields the following directed hypothesis:

H₂. Responders offer less resistance if the procedure through which the proposer has been assigned distributive power is seen to be fairer (or at least as fair) as the other AP available (c.p.).

Third, we try to assess the impact of procedural satisfaction, an issues which has not attracted much attention – at least in the realm of experimental economics – yet. When arguing for procedural autonomy (i.e. group decisions should be taken on the ground of procedural judgments within the group), justification may be based on normative reasons as well as on the pragmatic argument that people would otherwise not accept outcomes. In the long-term, it has been argued, this could even generate major instability within the organization. The satisfaction of procedural preferences can possibly reduce negative reciprocity. Individuals may think it is right that their individual or group preferences are satisfied, and may react negatively if this is not the case. According to the legitimacy and deference hypothesis, responders are expected to obey a decision if they regard the proposer as entitled to be obeyed. Entitlement is assumed to be given if responders' procedural preferences are satisfied. These considerations result in the following hypotheses:

H_{3a} Responders offer less resistance if their individual preferences regarding the AP are satisfied than if they are not satisfied (c.p.).

H_{3b} Responders offer less resistance if their group preferences regarding the AP are satisfied than if they are not satisfied (c.p.). Moreover, the effect becomes stronger the larger the majority setting the group preference.

H_{3c} Responders offer least resistance if their individual and group preferences regarding the AP are satisfied (c.p.). Responders offer most resistance if they are both not satisfied (c.p.). Responders offer medium levels of resistance if either their individual or group preferences are satisfied (c.p.).

4.3 Experimental results

4.3.1 Procedural preferences

On the question whether AP 1 or AP 2 should be applied in their group, 64% voted for AP 1 (experimenter despotism) and 36% for AP 2 (majority vote). In case that AP 2 was applied, 42% advocated “age” as key criterion and 58% “engagement”.

According to treatment variation rules, 50 participants (10 groups with 5 members each) get exactly the rule which the majority wants (M^+), and other 50 not (M^-). AP 1 is thus applied in 45 cases, and AP 2 is applied in 55 cases. The result of this assignment is that 51 participants decide under I^+ , 49 participants decide under I^- . The frequency of each responder situation is given in table 4.2 below.¹⁰

Table 4.2: Participants n in each responder situation

	I^+M^+	I^+M^-	I^-M^+	I^-M^-
n	35	18	15	32

Only 8% of all participants indicate that proposers’ appointment procedure is of little or no importance (6 or 7 on a seven point scale). Importance attach 46% of all subjects (1 or 2). On a seven point scale with 7 meaning very fair, participants perceive AP 1 as fairer (median 5) than AP 2 (median 4). By defining the AP which received a higher fairness evaluation as “fair AP” and the other AP as “unfair AP”, we find that 55 participants are assigned a fair AP (45 an unfair AP). All together, 87% choose those rule that they judge to be the fairer one (or the one which was seen as fair as the other). This means that only 13% choose exactly the other rule (mostly for selfish reasons according to their self-reported motivation). Answer frequencies to rule importance and rule fairness are given in table 4.3.

4.3.2 Allocated appointment procedures

We test for potential differences in resistance levels depending on whether AP 1 or AP 2 is applied. Interestingly, we find no significant differences

¹⁰As group size is equal under $M^{+/-}$ and nearly equal under $I^{+/-}$, we will analyze these data by using ANOVA/ANCOVA. For the analysis of responder situations, where groups are extremely unequal, we refer to non-parametric test statistics like Mann-Whitney or Kruskal-Wallis.

Table 4.3: Answer frequencies (in %) to decision stage 1 questions

points	rule importance (1=very important) (7=very unimportant)	fairness AP 1 (1=very unfair) (7=very fair)	fairness AP 2 (1=very unfair) (7=very fair)
1	14	2	3
2	32	12	25
3	21	7	17
4	20	19	18
5	5	14	9
6	5	26	15
7	3	20	13

between mean resistance levels under both appointment procedures (see table 4.4). Thus, participants do not behave differently depending on the allocated appointment procedure for any $x \geq 5$ and for x altogether¹¹.

Regularity 1. For any given value of x and for x altogether, contributions to the provision of the public good resistance do not differ significantly with respect to the allocated appointment procedure.

Table 4.4: Responders' mean contributions to resistance for AP 1 vs. AP 2

x	AP 1	AP 2	difference ^a	p^b
5	.989	1.518	-0.529	.341
10	3.444	3.764	-0.320	.666
15	5.822	5.127	0.695	.247
20	7.067	6.427	0.640	.402
25	7.633	7.318	0.315	.728
all	4.991	4.831	0.160	.672

^afirst column minus second column

^bsingle-factor ANOVA, two-sided

¹¹Note that mean resistance levels labeled with *all* include any decision taken for $x \in \{5, 10, 15, 20, 25\}$.

4.3.3 Procedural fairness

We test whether individual procedural fairness evaluations influence responders' willingness to accept or reject a tax. According to the procedural fairness hypothesis, we expect responders to offer less resistance if the procedure through which the proposer has been assigned distributive power is seen to be fairer one.¹²

Indeed, we find that responders offer less resistance if the AP they deem fairer is used. This is true for any tax and overall tax claims. However, from the analysis of variance output (see table 4.5, we retain the overall null hypothesis of equal population means in group "fair AP" and "not fair AP").

Regularity 2. For any given value of x and for x altogether, contributions to the provision of the public good resistance do not differ significantly with respect to the individual fairness judgment on the allocated appointment procedure. However, responders offer less resistance against any tax x and for x altogether if a fair procedure has been allocated to them.

Table 4.5: Responders' mean contributions to resistance according to procedural fairness evaluations

x	fair AP	not fair AP	difference ^a	p^b
5	.982	1.644	-0.662	.117
10	3.573	3.678	-0.105	.444
15	5.355	5.544	-0.189	.376
20	6.473	7.011	-0.538	.240
25	7.245	7.722	-0.477	.300
all	4.725	5.120	-0.395	.149

^afirst column minus second column

^bsingle-factor ANOVA, one-sided

4.3.4 Individual preference satisfaction

According to hypothesis H3a, we expect responders to offer less resistance if their individual procedural preferences are satisfied. However, we find

¹²We define "fair AP" as the procedure which is deemed fairer (or at least as fair) as the other AP available according to questionnaire answers on a seven-point scale on AP fairness. The other procedure is called "not fair AP" and it describes the procedure which was seen to be less fair (lower scale value) than the other one.

that the opposite is true for any x : mean contributions y to resistance are lower in situations I^- as compared with I^+ (see columns 2 and 3 of table 4.6 below). Thus we should reject H3a. However, to detect *any* regularity underlying responders' behavior, we test the alternative hypothesis whether mean resistance levels are significantly higher for I^+ than for I^- using an one-factor ANOVA. Resulting p -values are given in column 4 of table 4.6.

Table 4.6: Responders' mean contributions to resistance for I^+ vs. I^- depending on tax proposal x

x	I^+	I^-	difference ^a	p^b
5	1.333	1.224	0.109	.4222
10	4.108	3.112	0.996	.087* ^c
15	5.471	5.408	0.063	.459
20	7.176	6.235	0.941	.107
25	8.147	6.745	1.402	.059*
all	5.247	4.545	0.702	.035**

^afirst column minus second column

^bone-factor ANOVA, one-sided

^c*significant at 10%, **significant at 5%, ***significant at 1%

Regularity 3. For any given value of x , responders offer more resistance if their individual procedural preferences are satisfied. This is the opposite of what we have expected according to H3a. The difference is significant for $x = 10$ ($p = .087^*$) and $x = 25$ ($p = .059^*$).

In order to further investigate the correlation between the satisfaction of procedural preferences and resistance, we conduct an one-factor analysis of covariance (ANCOVA) overall tax claims x . Our variable of interest is a measure of how much resistance responders offer for all tax claims together under I^+ and I^- . The ANCOVA tests whether the factor "individual procedural preference satisfaction" has an effect on the outcome variable resistance after removing the variance for the covariate "allocated AP (1 or 2)" accounts. Mean resistance for I^- is 4.545, and for I^+ , it is 5.247. We find that the difference is significant at $p = .035^{**}$ (one-factor ANCOVA, one-sided)¹³.

Regularity 4. Taken over all tax claims x together, ANCOVA reveals that responders offer significantly more resistance ($p =$

¹³Note that the covariate itself is not significant ($p = .456$).

.035**, one-sided) if their individual procedural preferences are satisfied.

4.3.5 Group preference satisfaction

According to H3b, tax proposals should be more readily accepted, that is, mean contributions y to resistance are expected to be lower in situations M^+ as compared with M^- . Columns 2 and 3 of table 4.7 below show observed values of y depending on tax proposals x for M^+ and M^- , respectively.

Table 4.7: Responders' mean contributions to resistance for M^+ vs. M^- depending on tax proposal x

x	M^+	M^-	difference ^a	p^b
5	1.438	1.080	0.358	.235
10	4.090	3.150	0.940	.100*
15	5.900	4.980	0.920	.061*
20	7.100	6.330	0.770	.156
25	7.950	6.970	0.980	.138
all	5.304	4.502	0.802	.013**

^afirst column minus second column

^bone-factor ANOVA, one-sided

Obviously, we find that H3b is to be rejected due to the fact that for any x the effect is reversed from what we have expected: contributions to resistance are higher for M^+ than for M^- . Thus we tested whether the difference is significant using an one-factor ANOVA (one-sided). For $x = 10$ and $x = 15$, the difference is significant with $p = .100^*$, and $p = .061^*$ respectively. To sum up, there is evidence that procedural satisfaction within the group has an effect on outcome acceptance and a fifth result is:

Regularity 5. For any given value of x , responders offer more resistance if their group procedural preferences are satisfied. This is the opposite of what we have expected according to H3b. The difference is significant for $x = 10$ ($p = .100^*$) and $x = 15$ ($p = .061^*$).

By applying an ANCOVA test with allocated rule to be the covariate and without distinguishing between tax claims, we find that responders under M^+ invest 5.304 on average in resistance, whereas responders under M^-

offer 4.502 mean resistance. The difference is highly significant at $p = .013^{**}$ (one-factor ANCOVA, one-sided).¹⁴

Regularity 6. Taken over all tax claims x together, ANCOVA reveals that responders offer significantly more resistance ($p = .013^{**}$, one-sided) if their group procedural preferences are satisfied.

4.3.6 Responder situations

Since we found a significant difference in average responder behavior depending on whether individual preferences are satisfied (I^+) or not (I^-) and whether group preferences are satisfied (M^+) or not (M^-), there might be an interaction effect as well. We therefore compare behavior between the four different responder situations I^+M^+ , I^+M^- , I^-M^+ and I^-M^- . The frequency of each responder situation is given in row 2 in table 4.8.

Table 4.8 shows average contributions to resistance for each of the four situations. For any claim $x \geq 5$, resistance is highest either for situation I^+M^+ or for situation I^-M^+ . This is a further indication that it is most important whether group preferences are satisfied. The lowest average resistance levels correspond either to situation I^-M^- (for any x but $x = 15$) or to situation I^+M^- (for $x = 15$). Thus, hypothesis H3c can clearly be rejected. Indeed, the causality is reversed from what we have expected. The differences between the four situations, however, are not significant using a Kruskal-Wallis test for any x and all x together.

Regularity 7. For any x (but $x = 15$), resistance is lowest if neither individual nor group preferences are satisfied. For any x (but $x \in \{5, 15\}$), resistance is highest if both, individual and group preferences, are satisfied. These results are the opposite of those posited in H3c. The differences between the situations, however, are not significant for any x ($p \geq .399$) and x altogether ($p = .125$).

4.4 Conclusion

Any decision in human interaction is inherently associated with a procedure. It is impossible to take a decision without deciding first on *how* to take it (*Sebold*, 2007a). Besides, there is substantial evidence that not only outcomes

¹⁴The covariate “allocated rule” is not significant with $p = .227$.

Table 4.8: Average contributions to resistance situation by situation depending on tax proposal x

x	I^+M^+ (35) ^b	I^+M^- (18)	I^-M^+ (15)	I^-M^- (32)	p^a
5	1.371	1.306	1.733 ^c	<i>0.953</i> ^d	.604
10	4.214	3.833	3.800	<i>2.766</i>	.399
15	5.700	<i>4.889</i>	6.367	5.031	.430
20	7.243	7.056	6.767	<i>5.922</i>	.799
25	8.343	8.056	7.033	<i>6.359</i>	.749
all	5.374	5.028	5.140	4.206	.125

^aKruskal-Wallis test, two-sided

^bnumbers in brackets: participants in each situation

^cbold numbers: row maxima

^ditalic numbers: row minima

but also procedures leading to them can affect people's utility from and their reactions to those decisions. However, there is a large gap between a vast number of empirical, experimental, and theoretical studies by non-economists and the fact that there is hardly any economic research on procedures. In particular, experimental economists remained surprisingly silent about procedural aspects of strategic interactions. The present study is intended to contribute to this issue by analyzing the effects of various procedures and procedural judgments.

Our experimental data suggest that the effects of procedural fairness judgments are not as strong as assumed. Indeed, people offer less resistance if a fair procedure is used, but the effect is not significant. By contrast, satisfaction of procedural preferences seem to outrank procedural fairness: The willingness to resist various outcomes differs significantly according to the satisfaction/nonsatisfaction of individual and group procedural preferences.

Following the common conjecture that the implementation of favored procedural characteristics increases the chances that individuals will support even apparently unfavorable decisions (e.g. *Dolan et al., 2007*), we expected procedural satisfaction to lower resistance. To our surprise, the causality is reversed: Procedural satisfaction results in stronger resistance against various outcomes. Experimental data show that both individual and group procedural preference satisfaction increase significantly resistance against various outcomes and taken over all outcomes together. Thus, responders' demands vary depending on whether proposers obtained their roles with or without responders' support: People rather seem to accept decisions made by proposers

they have not supported.

In an organizational context, subordinates are usually assumed to comply with leaders' directives because they come to believe that "what is" is "what ought to be" in terms of leader behavior (*Walker and Zelditch, 1993*). However, we may explain the observed behavior by people's belief in their leader. By stating that a procedure should be used, people argue for that appointment procedure. If exactly this procedure is applied, people have indirectly countenanced and maintained the authority. At the same time, responders seem to assert a claim on their behalf: They insist on a fair treatment, or to be precise, on moderate demands. By choosing a decision-maker, some kind of agreement (following Hobbes's Social Contract) seems to become effective. Both the sovereign and the citizens have reason to honor their responsibilities under the terms of the contract. If the sovereign breaks his agreement (e.g. by claiming a disproportional large share), the citizenry might offer resistance, join a protest or try to bring down the sovereign.

Some psychological studies refer to a similar finding, known as *frustration effect* (see, e.g. *Folger, 1977; Cohen, 1985; Lind and Tyler, 1988; Kulik and Clark, 1993*): Fair procedures can not only trigger positive reactions but can also result in less satisfaction with negative outcomes than unfair procedures. This apparent contradiction of the usual finding of a positive relationship was observed under certain conditions. The present study provides evidence that not only procedural fairness but also procedural satisfaction can trigger strong resistance reactions against unfavorable outcomes.

Our experimental setting was intended to differentiate between procedural fairness and procedural satisfaction judgments. Indeed, the two concepts of procedural fairness and procedural satisfaction are closely connected: Procedural fairness judgments frequently involve procedural satisfaction since people usually prefer a procedure they deem fair. However, procedural satisfaction is beyond procedural fairness: Fairness may motivate preferences, but need not to. Many other motives are conceivable but rather elusive.

One key contribution of this study is to reveal the basic phenomenon of procedural dissatisfaction causing compliance. The finding provides further impetus to study procedural preferences and its consequences on human decision-making. To conclude, experimental data discussed here suggest that procedural concerns should not be excluded if we want to avoid overlooking an important source of utility. Further research on the effects of individuals' procedural judgments is necessary.

Chapter 5

Experiment 2: Procedural satisfaction

5.1 Introduction

This chapter reports the results of an experiment on resistance against centralized decisions in small groups. Common sense as well as experimental economics suggests that the willingness to offer resistance depends not only on the decision itself but also on how it comes about, that is, on the decision procedure. There are many different aspects that may have an influence: What were the alternatives? Who made the decision? According to which rules? Who made the rules? And how was the decision maker appointed?

Some of these aspects have been studied experimentally. For instance, the alternatives not chosen may yield information about the intention or attitude of the decision maker, which in turn may trigger (negatively) reciprocal behavior (*Falk et al.*, 2008). The rules governing decision making involve, among others, the issue of participation, which also influences reciprocal behavior (*Albert and Mertins*, 2008).

Subsequently, we are concerned with appointment procedures. We implement a game involving five players, a proposer and four responders, and a pie that must be divided among the group.¹ Each responder receives a quarter of the pie as an initial endowment; the proposer gets nothing. The proposer then claims a share of the pie in the form of equal payments, subsequently called the tax, to be made by each responder. If there is no resistance, the proposer receives the sum of the tax payment while the responders keep their endowments minus the tax. However, after learning the tax proposal, responders can simultaneously invest in resistance. These investments reduce their

¹This game and the basic design of the experiment are due to *Mertins* (2005, 2008a).

endowments and are lost for the group. If responders' investments taken together exceed a threshold, resistance is successful: responders keep their residual endowments, while the proposer gets nothing. If resistance fails, each responder pays the tax, as far as his residual endowment allows, to the proposer and keeps the rest.

Thus, among responders, successful group resistance is a threshold public good (see *Marwell and Ames (1979)*, *Palfrey and Rosenthal (1984)*, and, for an overview, *Ledyard (1995)*). This public good is provided (or not) simultaneously through voluntary contributions.

In our experiment, we were concerned with the procedure for appointing the proposer and its effects on responders' resistance. In this context, three issues are important: social preferences, procedural preferences, and the problem of coordination.

By social preferences, we mean that group members care about the distribution of the pie beyond what they get themselves. A distribution may, in the eyes of group members, be unfair or too unequal, and this perception can trigger resistance, as a special case of negative reciprocity. Effects of social preferences on resistance are subsequently called "distribution effects". These effects have for a long time been in the center of interest among experimental economists. In our setting, they can most easily be observed when we compare reactions of the same responders to alternative tax proposals, which requires the use of *Selten's (1967)* strategy method.² While distribution effects are not the main focus of this experimental study, they are nonetheless important because we expect an interaction between social and procedural preferences. Specifically, we expect that higher taxes will lead to more resistance, as has been observed in similar settings.³ The effects of

²The use of the strategy method could influence results because the method may force participants to think about each information set in a different way than if they could primarily concentrate on the information sets that arise in the course of the game (*Roth, 1995*). For instance, highly emotional responses may be suppressed: it is plausible that people will have stronger emotional reactions to a real action than to a hypothetical one ("hot" versus "cold" environments). Laboratory findings are mixed. *Brandts and Charness (2000)*, for example, find no significant difference in behavior between the hot and the cold treatments. By contrast, *Güth et al. (2001)* report evidence on games in which behavior is not robust to elicitation methods. In our view, it would be an advantage if the strategy method led to more thoroughly considered decisions: it makes it more plausible that our results are relevant to important decisions that are not taken at the spur of the moment.

³The basic regularities from a large body of experiments suggest that a considerable number of individuals show reciprocity, especially negative reciprocity. These results led to reciprocity-based models (*Rabin, 1993; Bolton and Ockenfels, 2000; Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006*) in which players form a judgement about whether another player has treated them fairly, and respond negatively to unfair actions and positively to fair actions. See *Camerer (2003, chs. 2.8, 9.1)* for an overview.

procedural preferences, then, can modify this relationship.

By procedural preferences, we mean, following *Ng* (1988), that group members care about aspects of the appointment procedure other than the instrumental value of the procedure in bringing about a desired distribution.⁴ Dissatisfaction with these aspects of a procedure may again trigger resistance. We speak of “procedure effects”. Procedure effects can be observed by comparing responder strategies in different treatments which differ with respect to these aspects (between-subjects design). Specifically, we expect dissatisfaction with the appointment procedure to raise resistance, at least for sufficiently high taxes.

In the game described above, there are two channels by which distribution or procedure effects can come about. First, social or procedural preferences may lead responders to offer more or less resistance. This may be quite independent of expectations concerning the behavior of the other responders. For instance, responders may wish to punish a high-taxing proposer by destroying part of the pie. Second, the expectation that other responders are going to offer resistance can make resistance rational even for an egoist who does not care about distribution or procedure. For instance, in the face of a high tax, a responder may reckon that other responders will offer sufficient resistance so that his own contribution tips the balance. There can, of course, be paradoxical effects; for instance, responders may reduce their own contribution to resistance in the face of high taxes because they expect the others to invest enough for resistance to be successful. Speculation of responders about the social or procedural preferences of other responders (or about other responders’ speculations) can modify these expectation effects.

In our experimental setting, we seek for procedure effects and for expectation effects with respect to procedural preferences. Such expectation effects require that group members use information about procedural preferences of other group members when deciding whether to offer resistance. For instance, a responder may expect a higher level of resistance if the procedure actually used was rejected by a majority. We call this a second-order procedure effect. Observing second-order procedure effects requires the provision of information about other players’ attitudes towards the procedure actually used.

Of course, procedure effects and expectation effects may go in the same direction. In this case, our experimental design does not allow us to distin-

⁴A growing literature argues for the importance of procedural preferences; see, e.g., *Elster* (1989), *Tyler* (1990), *Sen* (1997), *Tyler and Lind* (2000), *Frey et al.* (2004), *Bolton et al.* (2005), *Brandts et al.* (2006), *Trautmann* (2007), *Krawczyk* (2007), *Charness and Levine* (2007). For a systematic review of the theoretical literature on procedural preferences, see *Dolan et al.* (2007). Chapter provides an extensive survey on this literature.

guish these effects. Discrimination between the two effects is only possible when they go in different directions.

Subsequently, we speak of individual procedural preferences and group procedural preferences.⁵ We say that a procedural preference is satisfied if the preferred procedure is used. Our hypothesis coincides with the common sense: we expect satisfaction of procedural preferences to reduce group resistance.

Instead of confronting all participants with an exogenous assignment of participants to roles, we allowed each group of five participants to express their individual preferences concerning different procedures for choosing a proposer among them. We then told them the procedure preferred by the group and the procedure actually used. This led to different constellations concerning the satisfaction of individual and group preferences. By comparing responders' investments in resistance in the different treatment groups, we were able to test our hypotheses concerning the influence of social and procedural preferences on resistance and to provide first results concerning the role of the satisfaction of procedural preferences.

This chapter is organized as follows. Section 5.2 describes the experimental design and procedures and states theoretical predictions. Section 5.3 presents experimental results. Section 5.4 concludes.⁶

5.2 The experiment

5.2.1 The basic game

Consider the following game between five players, one proposer and four responders. Each responder has an initial endowment of $\frac{p}{4}$, where $p > 0$ (in chips) is the total experimental payoff at stake in the game. The proposer has no endowment. The proposer tells responders the tax, that is, the number $x \in [0, \frac{p}{4}]$ of chips she would like to get from each of them. If her proposal goes through, she gets $4x$ while each responder keeps $\frac{p}{4} - x$. However, each responder i can pay $y_i \in [0, \frac{p}{4}]$ on a group account. Responders' decisions are simultaneous. Payments to the group account are lost to the group. If the amount on the group account reaches a threshold t with $p > t > \frac{p}{4}$, that is, if $\sum_i y_i \geq t$, the proposal is rejected, and each responder keeps $\frac{p}{4} - y_i$ while the

⁵We consider only two alternatives; hence, problems of preference aggregation will not occur.

⁶The appendices consist of translations of the instructions (appendix C.1), the post-experimental questionnaire (appendix C.2) and answers to the questionnaire (appendix C.3).

proposer gets nothing. Note that no single responder can achieve rejection. If, however, the group account stays below the threshold t , each responder has to pay up to x to the proposer, depending on how much of his endowment is left, and the proposer gets up to $4x$, depending on whether responders can pay her in full or not. Thus, each responder pays $z_i := \min\{\frac{p}{4} - y_i, x\} \geq 0$ and keeps $\frac{p}{4} - y_i - z_i \geq 0$, while the proposer gets $\sum_i z_i \geq 0$.

In the experiment, the group endowment was $p = 100$ chips, and the threshold was $t = \frac{p}{4} + 1 = 26$. The tax proposal was restricted to multiples of five chips plus zero: $x \in \{0, 5, 10, 15, 20, 25\}$. Responders could choose any contribution $y_i \in \{0, 0.5, 1, 1.5, \dots, 24.5, 25\}$.⁷

For the theoretical analysis, we consider only subgame perfect equilibria in pure strategies assuming that players maximize their own experimental payoffs. Under these assumptions, a subgame perfect equilibrium is a strategy profile $(x^*, y_1(x), y_2(x), y_3(x), y_4(x))$ with the following properties. First, $y_i(x) \leq x$, that is, no responder invests more in resistance than the tax proposal x , because x is the return of successful resistance. Second, $y_i(x) = 0$ for $x < 6.5$ because then the maximum collective investments are $4x < 26$, that is, resistance cannot be successful. Third, $\sum_i y_i(x) \in \{0, 26\}$ if $x \geq 6.5$, that is, whenever x is high enough for successful resistance, responders either coordinate on no resistance or on exactly the right amount of resistance because otherwise at least one responder could profit by reducing his investment unilaterally. Given the responder strategies, the best response of the proposer is $x^* := \max\{x: \sum_i y_i(x) = 0\}$, that is, to select the highest value of x for which there is no resistance. Since $x \in \{0, 5, 10, 15, 20, 25\}$, it follows that $x^* \geq 5$. Note that $x = 5$ means equal division of the 100 chips among all five players if there is no resistance.

This analysis shows that, for proposals $x > 5$, the game becomes a coordination game. There are many subgame perfect equilibria in pure strategies. Only some of them are symmetric in responder strategies. Symmetric equilibria require all four responders to choose the same function $y(x)$ satisfying $y(0) = y(5) = 0$ and $y(x) \in \{0, 6.5\}$ for $x = 10, 15, 20, 25$.

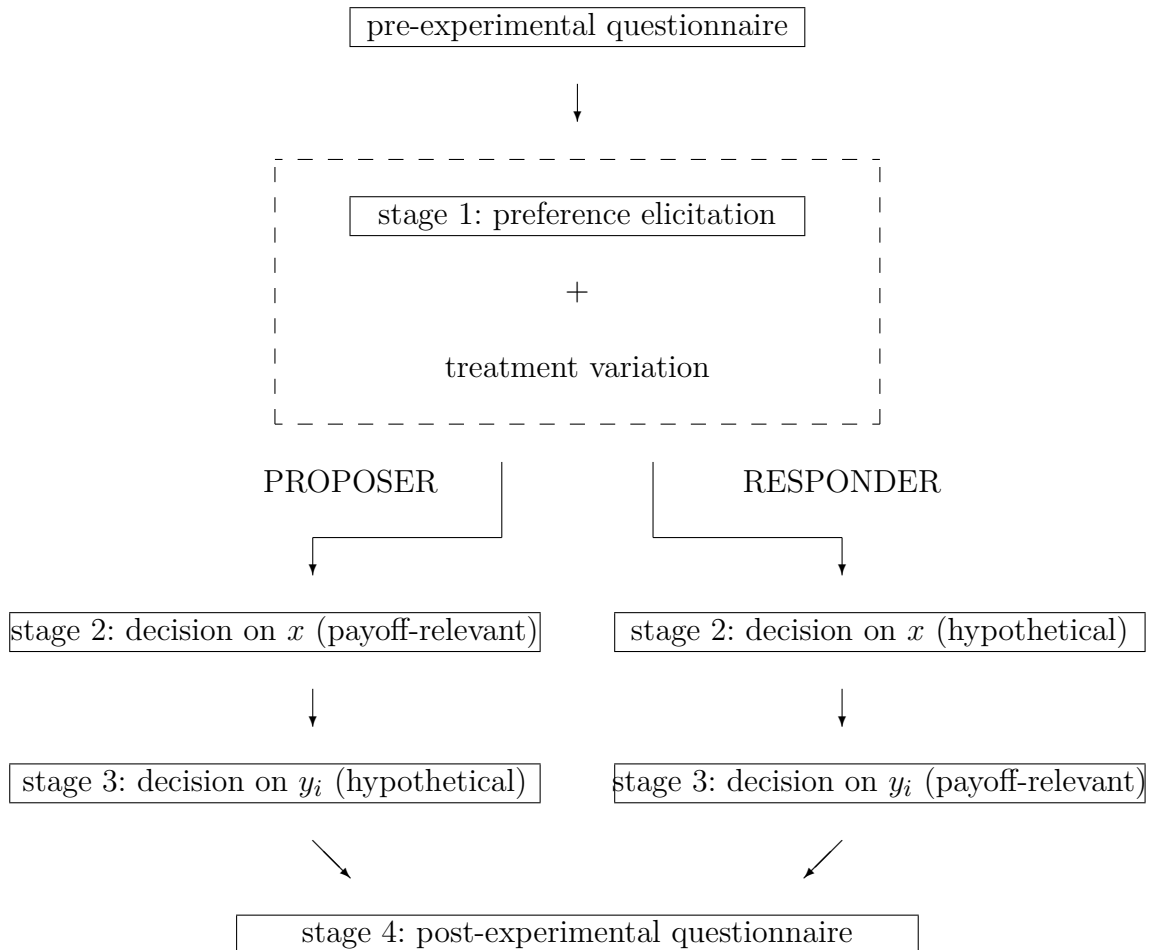
Thus, two types of equilibria compete against each other: First, the free-rider equilibrium where no responder contributes anything to resistance no matter how high the tax. Second, the threshold equilibria where responders successfully coordinate on resistance for at least one tax proposal.

⁷We allowed for half chips in order to make possible a symmetric equilibrium with resistance, where each responder sets $y_i = 6.5$. Indeed, 6.5 has been chosen relatively frequent, e.g., by 16.3% of all responders confronted with $x = 10$.

5.2.2 Experimental design

As indicated in figure 5.1 below, each session consisted of a pre-experimental questionnaire inquiring for basic demographic information and the average time spent in voluntary work, three decision stages (see appendix C.1), and a post-experimental questionnaire (see appendices C.2 and C.3) where we tested whether participants had understood the game. After answering the pre-experimental questionnaire, participants were randomly assigned to groups of five participants. However, nobody received information about the composition of his own group or other groups.

Figure 5.1: Sequence of events



In decision stage one, participants were asked to consider two appoint-

ment procedures (AP) for the group’s proposer:⁸

- AP 1 (experimenter despotism): The experimenter appoints a proposer.
- AP 2 (majority vote): Group members appoint a proposer by majority vote.

Appointment procedure 1 was in fact a random mechanism. We drew a lot to determine the proposer; participants, however, did not know this. Appointment procedure 2 was more difficult to implement since anonymity had to be respected. We asked participants to vote for a criterion to determine the proposer. They had the choice between age (the oldest group member) and high engagement in student and voluntary activities (the one who spent most time per week on such activities).⁹ The relevant information was available from the pre-experimental questionnaire.

First, participants stated their opinion on the importance of the appointment procedure using a seven-point scale ranging from 1 (very important) to 7 (very unimportant). Second, participants ranked both APs on a seven point scale from 1 (very unfair) to 7 (very fair).

Then, participants cast their vote for AP 1 or AP 2. Abstention was impossible. As each group consists of five members, there was necessarily a majority—large (5:0), medium (4:1) or narrow (3:2)—in favor of one AP. Moreover, independently of their own preferences with respect to the AP, participants had to vote for one criterion, high age or high engagement, so that, in case of AP 2, a proposer could be chosen. Again, abstention was impossible. Both APs were favored by a considerable number of participants. We were, however, not interested in these procedural preferences as such. Rather, we were interested in whether behavior differed depending on whether procedural preferences were satisfied or not. When stage one was completed, we counted the votes and determined proposers as described before.

Instructions for decision stage two contained information about the results from stage one. We told participants the preferences of their group, that is, the AP preferred by the majority and the size of the majority. We then told them whether the preference of the majority was respected (treatment M^+) or not (treatment M^-), and which AP, 1 or 2, had accordingly been

⁸At this stage, they were already informed about the structure of the game.

⁹“Student and voluntary activities” include for instance voluntary activities and (paid and non-paid) internships and jobs at private or state organizations. In the Bulgarian cultural context we use this criterion to distinguish between participants as “active” or “passive”. People who are considered to be active have a higher social esteem (cf. *Tilkidjiev*, 2002).

used to appoint the proposer. For instance, a group may have voted 4:1 in favor of AP 1; participants were given this information together with the information that the will of the majority did not prevail and that, instead, AP 2 was used to appoint the proposer. Another group may have voted 3:2 in favor of AP 1; participants were given this information together with, say, the information that the will of the majority prevailed and that we had determined the proposer according to the criterion preferred by the group.

Half of the groups (50 participants) were randomly assigned to treatment M^+ , the other half to treatment M^- . We applied a between-subject design where participants were not aware of the different treatments.

Participants were also informed whether they were a proposer or a responder.¹⁰ Accordingly, participants received different instructions for decision stages two and three.

In decision stage two, proposers were asked to allocate the group pie of $p = 100$ chips, with the monetary value of 1 chip being 0.25 Lev. Initially, each responder i had 25 chips (6.25 Leva), whereas the proposer had nothing. The proposer had to propose a tax $x \in \{0, 5, 10, 15, 20, 25\}$ of chips to be paid to him by each responder. In decision stage three, the proposers were asked how they would behave in case they were responders (hypothetical decision).

Responders were asked in decision stage two which tax x they would claim in case they had been appointed as proposers (hypothetical decision). In stage three, they had to choose their contribution $y \in \{0, 0.5, 1, 1.5, \dots, 24.5, 25\}$ to the responders' group account for each $x \in \{0, 5, 10, 15, 20, 25\}$ the proposer might have chosen (strategy method). The instructions emphasized that these contingent contribution choices were binding and that one would be carried out depending on the actually proposed tax.

Apart from the fact that we made clear that hypothetical decisions (stage 2 decisions of responders and stage 3 decisions of proposers) were not payoff-relevant, instructions for proposers and responders were the same.¹¹

Participants were one hundred undergraduate students from Sofia University's faculty of economics and business administration with no previous experience in economic experiments. The non-computerized experiment took place on October 26, 2006 and consisted of four sessions. 61 percent of participants were female. Age ranged from 18 to 27 years, with an average of 20.23 years. According to their own statement, participants on average spend 4.41 hours per week in voluntary work, varying from 0 to 17 hours.

¹⁰This is the main modification compared to the original design by *Mertins* (2005, 2008a), where participants took both decisions without knowing their payoff-relevant role.

¹¹All participants were confronted with equal decision tasks independent of their role for mostly two reasons: we wanted participants to put themselves in the role of the opponent to understand the other's task and the game as a whole.

The experiment took about 45 minutes and participants earned 5.9 Leva on average, including 1 Lev show-up fee (that is, 7.85 Leva per hour).¹²

5.2.3 Hypotheses

Each responder finds himself in one of four possible situations (see table 5.1 below): individual AP preferences may be satisfied (situation I^+) or not (situation I^-), and group AP preferences—that is, the preference of the majority—may be satisfied (situation M^+) or not (situation M^-). These situations were determined partly by our treatment (M^+ versus M^-) and partly by the responder’s preference in combination with our treatment (I^+ versus I^-). We were mainly interested in the question of whether these situations had an influence on resistance.

Table 5.1: Responder situations

		individual preference	
		satisfied	not satisfied
group preference	satisfied	I^+M^+	I^-M^+
	not satisfied	I^+M^-	I^-M^-

Game-theoretic predictions of responder behavior on the basis of egoistic or social preferences do not differ between the four situations. Neither the appointment procedure itself nor participants’ preference satisfaction should affect responders’ behavior. The possibility that participants have procedural preferences, however, suggests different hypotheses. Specifically, we investigate the following behavioral hypotheses, which are suggested by the literature on procedural preferences discussed in the introduction:

- 1) *Ceteris paribus*, responders contribute more to resistance if their individual preferences regarding the appointment procedure are not satisfied than if they are satisfied. That is, for any tax proposal $x > 0$ and corresponding contribution y : y in situations I^- is higher than y in situations I^+ .
- 2a) Responders contribute more to resistance if group preferences regarding the appointment procedure are not satisfied than if they are satisfied.

¹²One Lev is 0.51 Euro. A research assistant at Sofia University earns between 1.5 (undergraduate) and 4 Leva (graduate) per hour; a shop assistant in Sofia earns about 2 or 3 Leva per hour.

That is, for any tax proposal $x > 0$ and corresponding contribution y : y in situations M^- is higher than y in situations M^+ . Moreover, the effect becomes stronger the larger the majority setting the group preference.

- 2b) Responders contribute less to resistance if group preferences regarding the appointment procedure are not satisfied than if they are satisfied. That is, for any tax proposal $x > 0$ and corresponding contribution y : y in situations M^+ is higher than y in situations M^- . Moreover, the effect becomes stronger the larger the majority setting the group preference.

A potential explanation for behavior according to hypothesis 1 is that satisfaction of individual procedural preferences reduces negative reciprocity. The same explanation could be proposed for behavior according to hypothesis 2a: individuals may think it right, *ceteris paribus*, that group preferences should be satisfied—they may have democratic preferences, so to speak—, and may react negatively when this is not the case.

Hypotheses 2a and 2b are alternatives. Both hypotheses can potentially be explained by an expectation effect: even when individuals may not care about group preferences, they may use the information that group preferences were satisfied or not satisfied to form expectations about the contributions of others. Such an expectation effect—which, in this case, is a second-order procedure effect since it is triggered by information concerning procedural preferences of other players—can go in either direction: it may raise contributions or lower contributions, depending on whether one expects one's own contribution to be necessary or superfluous to make resistance successful. Hypothesis 2b is just the simplest hypothesis that contradicts 2a and can conceivably be explained by an expectations effect.

For simplicity, we label hypothesis 2a “democratic preferences” and hypothesis 2b “expectation effect”, even though the expectation effect may also offer an explanation of 2a.

In both cases, 2a and 2b, and independently of the explanation in terms of democratic preferences or expectation effects, it is plausible that the effect becomes stronger with the strength of the majority.

Our final hypothesis deals with proposers' behavior. In their experimental study, *Brandts et al. (2006)* addressed the issue whether a selection procedure is able to change individuals' goodwill towards others in general and/or towards the selectors in particular. They presume that people may have a tendency to prefer avoiding the guilt that results from disappointing the expectations of others. Their first hypothesis posits that selected allocators are

more generous towards others than random allocators (“I-want-YOU effect”). Their second hypothesis states that the selected party will favor the selecting party more than the third party involved (“gratitude effect”). There is weak statistical evidence for both effects.¹³

We will test the I-want-YOU hypothesis within a different experimental setting by comparing the claims of allocators either selected by majority vote (M^+) or declined by the majority of responders (M^-). According to the findings by *Brandts et al. (2006)*, we expect that selected proposers are more generous towards responders (i.e. claim less) than non-selected proposers. Thus, we state the following hypothesis:

- 3) *Ceteris paribus*, proposers claim less if responders have been chosen by group majority vote. That is, tax proposal x is higher in situations M^- than in situations M^+ .

5.3 Experimental results

5.3.1 Aggregate results

In this subsection, we look at aggregate results, without distinguishing between the four responder situations.

5.3.1.1 Procedural preferences

On the question whether appointment procedure 1 or appointment procedure 2 should be applied in their group, 73% voted for AP 2 (majority vote) and 27% for AP 1 (experimenter despotism). In case that AP 2 were applied, 30% advocated ‘age’ as key criterion and 70% ‘engagement’. Answer frequencies to rule importance and rule fairness are given in table 5.2. Most participants (56%) attach importance to the proposers’ appointment procedure (1 or 2 on a seven point scale) and only 6% think that the procedure is of little or no importance (6 or 7); the median answer is 2. On a seven point scale with 1 meaning very unfair, participants perceived AP 2 as fairer (median 6) than AP 1 (median 4).

5.3.1.2 Tax proposals

In stage two, proposers were asked to propose the tax $x \in \{0, 5, 10, 15, 20, 25\}$ to be paid by each responder. This was their main decision and critical for

¹³By using a Mann-Whitney test, the “I-want-YOU effect” has been confirmed in round 1 ($p = .043$), but not in the following rounds ($p = .089$ and $p = .48$). The same is true for the “gratitude effect” (round 1: $p = .042$, Wilcoxon signed-rank test).

Table 5.2: Answer frequencies (in %) to decision stage 1 questions

points	rule importance (1=very important) (7=very unimportant)	fairness AP 1 (1=very unfair) (7=very fair)	fairness AP 2 (1=very unfair) (7=very fair)
1	20	11	3
2	36	15	6
3	16	19	8
4	12	19	15
5	10	11	13
6	1	10	27
7	5	15	28

their monetary payoff. An equal split of the pie ($x = 5$) was proposed by 35% of proposers and another 35% decided in favor of $x = 10$. A tax of $x = 15$ was proposed by 25% and the whole pie was claimed by only 5% of all proposers. No responder has chosen a tax $x \in \{0, 20\}$. The average tax choice of proposers is 10.25 (sd 5.250).¹⁴

5.3.1.3 Contributions to resistance

In stage three, responders were asked, for any feasible tax x , which amount y they would contribute to resistance. They could choose any contribution $y \in \{0, 0.5, 1, 1.5, \dots, 24.5, 25\}$. Average contributions are given in table 5.3. Not surprisingly, mean resistance levels increased with the tax. Whereas responders facing a tax of zero contributed only 1.35 chips on average to resistance, average contributions for $x = 25$ were 8.46 chips (about $\frac{1}{3}$ of the individual endowment). For tax proposals $x \leq 10$, the average contribution was below 6.5 and therefore, on average, insufficient for a responder group to reach the threshold of 26. For tax proposals $x \geq 15$, on the other hand,

¹⁴Responders were assigned the same task but their decisions were just hypothetical and had no monetary impact. We observed the following relative frequencies: $x = 0$ has been chosen by 2.50%, $x = 5$ by 57.50%, $x = 10$ by 25.00%, $x = 15$ by 6.30%, $x = 20$ by 2.50% and $x = 25$ by 6.30% of all responders. The mean tax proposed by responders was 8.38 which is slightly lower than proposers' mean. Relative frequencies of tax proposals do not differ significantly at $p = .01$ using a binomial test suggesting that responders took their task seriously and tried to put themselves in the role of responders.

average contributions were above 6.5 (see table 5.3 below).¹⁵

Table 5.3: Mean resistance y for each tax proposal x

x	0	5	10	15	20	25
y (mean)	1.350	1.594	5.263	6.594	7.494	8.456

5.3.2 Procedure effects

5.3.2.1 Frequency of responder situations

As already explained, ten groups, each with four responders, were assigned to the treatment M^+ where the appointment procedure preferred by the majority is used. The other ten groups were assigned to the treatment M^- where the appointment procedure rejected by the majority is used. Appointment procedure 1 was applied in eight groups, and AP 2 in twelve groups. The result of this assignment was that 39 of eighty responders decided under the individually preferred AP (I^+) and 41 under the non-preferred AP (I^-). The frequency of each responder situation is given in table 5.4 below.

Table 5.4: Participants n in each responder situation

	I^+M^+	I^+M^-	I^-M^+	I^-M^-
n	31	8	9	32

5.3.2.2 Procedures

We tested for potential differences in resistance levels depending on whether AP 1 or AP 2 is applied. Interestingly, we found no significant differences between resistance levels under both appointment procedures (see table 5.5). Thus, the appointment procedure itself seems to be not very important for explaining the level of resistance.

¹⁵Proposers were asked how they would behave in case they were responders (hypothetical decision). Their contributions were also strictly increasing in x , with means ranging from .90 for $x = 0$ to 8.125 for $x = 25$. Their hypothetical choices did not differ significantly from responders' payoff-relevant decisions (Mann-Whitney test, two-sided, $p \geq .429$).

Table 5.5: Responders' mean contributions to resistance for AP 1 vs. AP 2 depending on tax proposal x

x	AP 1	AP 2	difference ^a	p^b
0	1.406	1.313	0.093	.258
5	2.016	1.313	0.703	.300
10	5.578	5.052	0.526	.333
15	6.828	6.438	0.390	.137
20	7.438	7.531	0.907	.700
25	8.078	8.708	-0.630	.796

^afirst column minus second column

^bMann-Whitney test, two-sided

5.3.2.3 Individual preferences

We tested our first hypothesis that tax proposals are more readily accepted, that is, mean contributions y to resistance are lower, in situations I^+ as compared with I^- . Columns 2 and 3 of table 5.6 below show observed values of y depending on tax proposals x for I^+ and I^- , respectively.

Table 5.6: Responders' mean contributions to resistance for I^+ vs. I^- depending on tax proposal x

x	I^+	I^-	difference ^a	p^b
0	1.462	1.244	0.218	.159
5	1.538	1.646	-0.108	.501
10	4.628	5.866	-1.238	.084
15	5.859	7.293	-1.434** ^c	.007
20	6.346	8.585	-2.239**	.002
25	7.103	9.744	-2.641**	.002

^afirst column minus second column

^bMann-Whitney test, one-sided

^c*significant at 5%, **significant at 1%, ***significant at 0.1%

For $x = 0$, mean resistance is higher for I^+ than for I^- , but the difference is not significant. For any tax proposal $x \geq 5$, mean resistance is, in line with our hypothesis, higher for I^- than for I^+ . For $x \leq 5$, the difference is not significant ($p = .501$); however, p values fall as x rises, reaching strong significance for $x > 10$. Thus, a first result is:

For high tax proposals ($x \in 15, 20, 25$) and taken over all situations, responders contributed significantly less to resistance if their individual procedural preferences were satisfied. For low tax proposals ($x \in 5, 10$), there is no significant effect.

5.3.2.4 Majority preferences

In order to test whether hypothesis 2a or 2b holds, we compare mean contributions y to resistance in situations M^+ and in M^- . Columns 2 and 3 of table 5.7 below show observed values of y depending on tax proposals x for M^+ and M^- , respectively.

Table 5.7: Responders' mean contributions to resistance for M^+ vs. M^- depending on tax proposal x

x	M^+	M^-	difference ^a	p^b
0	1.413	1.288	0.125	.651
5	1.438	1.750	0.312	.572
10	5.500	5.025	0.475	.698
15	6.825	6.636	0.189	.898
20	7.838	7.150	0.688	.618
25	8.863	8.050	0.813	.532

^afirst column minus second column

^bMann-Whitney test, two-sided

There is no evidence that procedural satisfaction within the group has any effect on its own. For $x = 5$, contributions to resistance are higher for M^- ; for any other x , contribution rates are lower for M^- ; however, all differences in resistance are not significant. Thus, a second result is:

Taken over all situations, responders do not behave significantly different depending on whether group preferences are satisfied (M^+) or not (M^-).

5.3.3 Responder situations

While it makes no significant difference to average responder behavior whether group preferences are satisfied (M^+) or not (M^-), there might still be an interaction effect with the situation concerning individual preferences (I^+ or I^-). We therefore compare behavior between the four different responder situations I^+M^+ , I^+M^- , I^-M^+ and I^-M^- (see table 5.1 above).

Table 5.8 shows average contributions to resistance for each of the four situations. For any claim $x > 5$, resistance is highest for situation I^-M^+ , followed up by situation I^-M^- . This is a further indication that it is most important whether individual preferences are satisfied. The differences between the four situations are significant for high tax proposals ($x \geq 15$).

Table 5.8: Average contributions to resistance situation by situation depending on tax proposal x

x	I^+M^+ (31) ^b	I^+M^- (8)	I^-M^+ (9)	I^-M^- (32)	p^a
0	1.823 ^c	.063	.000	1.594	.240
5	1.726	.813	.444	1.984	.731
10	4.839	3.813	7.778	5.328	.211
15	6.129	4.813	9.222	6.750	.034 ^d
20	7.000	3.813	10.722	7.984	.001 ^{***}
25	7.806	4.375	12.500	8.969	.000 ^{***}

^aKruskal-Wallis test, two-sided

^bnumbers in brackets: participants in each situation

^cbold numbers: row maxima

^d*significant at 5%, **significant at 1%, ***significant at 0.1%

As a next step, we considered pairwise comparisons between situations. Table 5.9 below shows differences in average contributions to resistance. For low tax proposals ($x \leq 5$), there are no significant differences. For medium proposals ($x \in \{10, 15\}$), there are three significant differences: between situations I^+M^+ and I^-M^+ (for both x -values) as well as between situations I^+M^- and I^-M^+ (for $x = 15$). For high proposals ($x \geq 20$), most differences are significant.

Table 5.9: Differences in average contributions to resistance depending on tax proposal x in pairwise comparisons of responder situations

x	1	2	3	4	5	6
	I^+M^+ vs. I^+M^-	I^+M^+ vs. I^-M^+	I^+M^+ vs. I^-M^-	I^+M^- vs. I^-M^+	I^+M^- vs. I^-M^-	I^-M^+ vs. I^-M^-
0	1.760 ^a (.239) ^c	1.823 ^b (.126)	0.229 (.448)	.063 (.471)	-1.531 (.566)	-1.594 (.309)
5	0.913 (.887)	1.282 (.328)	-0.258 (.760)	0.369 (.598)	-1.171 (.709)	-1.540 (.260)
10	1.026 (.600)	-2.939 ^{*d} (.034)	-0.489 (.632)	-3.965 (.093)	-1.515 (.397)	2.450 (.189)
15	1.316 (.311)	-3.093 [*] (.014)	-0.621 (.134)	-4.409 [*] (.015)	-1.937 (.135)	2.472 (.205)
20	3.187 ^{**} (.006)	-3.722 ^{**} (.006)	-0.984 (.128)	-6.909 ^{**} (.003)	-4.171 ^{**} (.005)	2.738 (.069)
25	3.431 ^{**} (.004)	-4.694 ^{**} (.005)	-1.163 (.144)	-8.125 ^{***} (.001)	-4.594 ^{**} (.005)	3.531 (.055)

^amean first situation minus mean second situation

^bnumbers in boldface: highest absolute difference in the respective row

^cMann-Whitney test with Bonferroni-Holm correction for multiple test procedures, two-sided

^d*significant at 5%, **significant at 1%, ***significant at 0.1%

For easier reference, we have numbered the pairwise comparisons from 1 to 6. We discuss each comparison separately under its number. In interpreting our results, we make use of the idea of the expectation effect (hypothesis 2b).

1. Observation: When individual preferences are satisfied and tax proposals are high ($x \geq 20$), resistance is significantly higher when the group preference is also satisfied. Interpretation: This observation is consistent with hypothesis 2b (expectation effect) but not with hypothesis 2a (democratic preferences).
2. Observation: When group preferences are satisfied and tax proposals are moderately high ($x \geq 10$), resistance is significantly higher when the individual preference is not satisfied. Interpretation: This effect is in line with hypothesis 1. In this comparison, hypotheses 2a or 2b should play no role.
3. Observation: In a comparison of a situation where individual and group preferences are satisfied with a situation where both kinds of preferences are not satisfied, there are no significant differences in average resistance for any tax proposals. Interpretation: This observation contradicts the conjunction of hypotheses 1 and 2a, because according to these hypotheses, we should observe an especially strong effect in this comparison. A conjunction of hypothesis 1 and hypothesis 2b, on the other hand, allows for a cancelation of effects.
4. Observation: In a comparison of a situation where group preferences are satisfied and individual preferences are not satisfied with a situation where this is just the other way round, we find that for high tax proposals ($x \geq 15$), resistance is significantly higher (indeed much higher) when individual preferences are not satisfied. Interpretation: Effects derived from hypothesis 2b should strengthen the effects expected according to hypothesis 1. Effects derived from hypothesis 2a should have the opposite effect. Hence, the observation speaks in favor of hypothesis 2b.
5. Observation: When group preferences are not satisfied and tax proposals are high ($x \geq 20$), resistance is significantly higher when the individual preference is not satisfied. Interpretation: This effect is in line with hypothesis 1. In this comparison, hypotheses 2a or 2b should play no role.
6. Observation: When individual preferences are not satisfied, there are no significant differences in resistance depending on whether group prefer-

ences are satisfied or not. Interpretation: This is in line with hypothesis 1. Hypotheses 2a or 2b both predict a difference. The observed algebraic signs for high tax proposals favor hypothesis 2a, but since the differences are not significant, the evidence is inconclusive.

Thus, our third result is:

Pairwise comparisons between the four responder situations confirm hypothesis 1 (cet. par., less resistance when individual preferences are satisfied than when they are not) and speak against hypothesis 2a (democratic preferences) and for hypothesis 2b (expectation effect).

5.3.4 Strength of majority

We next turn to the relationship between contributions to the public good resistance and strength of majority. The strength of majority is the extent to which voting results are more or less clear cut. As each group consists of five members, there is necessarily an appointment procedure (AP) which is preferred by a majority. This majority might be a large (5:0), medium (4:1) or narrow (3:2) majority. Table 5.10 below describes the six possible situations with the number of participants in each.

Table 5.10: Strength of majority

treatment	narrow (2:3 and 3:2)	medium (4:1 and 1:4)	large (5:0 and 0:5)
M^+	low M^+ (12) ^a	medium M^+ (20)	high M^+ (8)
M^-	low M^- (16)	medium M^- (16)	high M^- (8)

^anumbers in brackets: participants in each situation

Table 5.11 below shows the average contributions to resistance depending on the tax proposal for all six situations. The Kruskal-Wallis test shows that resistance differs significantly between the three M^- situations for high claims ($x \in \{20, 25\}$): Resistance is highest in case of a large majority (high M^-), and lowest in case of a narrow majority (low M^-). The differences are significant at $p = .034$ ($x = 25$) and $p = .028$ ($x = 20$), respectively. This result is more in line with hypothesis 2a (democratic preferences) than with

Table 5.11: Average contributions to resistance depending on tax proposal x and strength of majority

x	low M^+	low M^-	medium M^+	medium M^-	high M^+	high M^-
0	.833	1.031	2.300^a	1.875	0.063	0.625
5	.667	1.094	2.225	1.750	0.625	3.063
10	6.875	4.344	5.025	5.375	4.625	5.688
15	7.917	5.531	6.425	6.688	6.188	7.375
20	8.583	6.094	7.675	6.781	7.125	10.000
25	10.875	7.156	8.175	7.750	7.563	10.438

^anumbers in boldface: highest absolute value in the respective row

hypothesis 2b (expectation effect): Resistance increases if the group preference, which is not satisfied, is more clear cut. For the three M^+ situations, the strength of majority does not result in any significant differences.

In table 5.12, we compare average contributions in M^+ and M^- situations for a given strength of majority. We find significant differences only for the case of a large majority: in this case, resistance is lower for any positive tax proposal when the group preference is satisfied (M^+); these differences are significant for $x = 5$ and highly significant for $x \in \{20, 25\}$.

Again, this effect is more in line with hypothesis 2a (democratic preferences) than with hypothesis 2b (expectation effect): in the case of large majorities, satisfaction of group preferences reduces resistance on average.

The results concerning the effects of the strength of majorities, then, are weak but point in the same direction. Our fourth result is:

The few significant effects of the strength of majority speak for hypothesis 2a (democratic preferences) and against hypothesis 2b (expectation effect).

5.3.5 I-want-YOU effect

Our final observation is concerned with proposers' behavior and thus hypothesis 3. Proposers were asked to choose a tax $x \in \{0, 5, 10, 15, 20, 25\}$. If an I-want-YOU effect exists, proposers in the M^+ treatment situation are expected to claim less than proposers in the M^- treatment situation. Indeed, M^+ proposers claim an average tax of 9, whereas M^- proposers claim 11.5 on average. A Mann-Whitney test shows that the difference is significant ($p = .046$, one-sided), thus providing statistical evidence for hypothesis 3. This finding is completely in line with *Brandts et al.* (2006). It underlines

Table 5.12: Differences in average contributions to resistance depending on tax proposal x and strength of majority

x	low M^+ vs. low M^-	medium M^+ vs. medium M^-	high M^+ vs. high M^-
0	-0.198 ^a (.382) ^c	0.425 (.231)	-0.562 ^b (1.000)
5	-0.427 (.665)	0.475 (.655)	-2.438 (.055)
10	2.531 (.221)	-0.350 (.931)	-1.063 (.485)
15	2.386 (.175)	-0.263 (.438)	-1.187 (.145)
20	2.489 (.122)	0.894 (.640)	-2.875 ^{**d} (.007)
25	3.719 [*] (.050)	0.425 (.595)	-2.875 [*] (.022)

^amean first group minus mean second group

^bnumbers in boldface: highest absolute difference in the respective row

^c p value acc. to Mann-Whitney test, two-sided

^d*significant at 5%, **significant at 1%, ***significant at 0.1%

the robustness of our experimental design and can be seen as further evidence for the importance of procedural judgements.

5.4 Conclusion

In this experimental study, we were concerned with the effects of procedural preferences on resistance against centralized decisions in small groups. The procedure in question was the appointment procedure (AP) for a decision maker. We distinguished individual and group preferences concerning the AP. Preferences concerning the AP differed considerably. The AP itself, although important in the eyes of most participants, did not explain the observed level of resistance. In contrast, the satisfaction of individual preferences significantly lowered resistance against unfavorable decisions. This is a remarkable finding as it highlights the importance of procedural preferences in human decision making. This finding goes far beyond procedural fairness considerations as it seems not to be important which procedure is used or whether the applied procedure is deemed fair, but whether individual

procedural preferences are satisfied.

In many situations, and also in our experiment, successful resistance requires sufficiently high investments by the group, meaning that successful resistance is a threshold public good. Information about group preferences, then, may lead to successful coordination on resistance. Such coordination may come about as a side effect of democratic preferences: Independently from their own procedural preferences, individuals may dislike it when the preferences of a majority are ignored and react with increased resistance. Alternatively, coordination may result from expectations concerning the behavior of others. In the latter case, almost anything is possible.

Our results show no clear-cut effects of information about group preferences as a coordination device. Resistance is neither significantly higher nor significantly lower when group preferences are satisfied as compared with the situation where they are not satisfied. When we consider the interaction between satisfaction of individual preferences and satisfaction of group preferences, we find some evidence against the idea of democratic preferences. When we consider the effect of the strength of the majority, the evidence is weak but points in the other direction. A clarification may be achieved within the present experimental design by asking responders about their expectations concerning the contributions of the other responders in their group. This is a topic for further research.

Last but not least, we provided further evidence that an “I-want-YOU effect” exists. In particular, we showed that proposers who have been appointed by a majority are more generous towards responders than proposers who have been appointed against the majority’s will. That is, procedure judgements are able to change proposers’ goodwill towards responders. To conclude, when neglecting the procedure by which a decision maker has been appointed, a relevant behavioral issue has been left aside.

Chapter 6

Experiment 3: Participatory decision making

6.1 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 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' in-

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 et al. (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 et al. (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*.

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, e.g., Cox and Deck, 2005. Subsequently, however, we do not distinguish between negative reciprocity as a motivation and negatively reciprocal behavior.

tentions 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 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.* (2008a) 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

⁶With, it seems, monetary payoffs; however, *Grimalda et al.* (2008a) do not mention the exact values.

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 here 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 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 destruction rates as a response to higher take rates.⁷

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* (2008) do not find a difference between hot and cold settings, whereas *Brosig* et al. (2003) do.

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 insufficient. 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.

This chapter proceeds as follows. Section 6.2 explains the experimental design and procedures. Section 6.3 presents the experimental results. Section

⁸See, e.g., *Fischbacher et al. (2001)*, *Goeree et al. (2002)*, *Kurzban and Houser (2005)*, *Burlando and Guala (2005)*, *Fischbacher and Gächter (2008)*, 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.

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.

6.4 concludes.¹¹

6.2 The experiment

6.2.1 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 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 6.1).

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

¹¹Translations of the instructions, the post-experimental questionnaire and questionnaire answer frequencies can be found in appendices D.1 to D.3.

Table 6.1: Treatment groups

		Participation	
		yes	no
Influence	low	PartLow	NoPartLow
	high	PartHigh	NoPartHigh

$t = \frac{2}{3}$, the group's endowment would be equally distributed among group members, irrespective of d .

Table 6.2 shows the possible take rates t .

Table 6.2: Possible group take rates t

Low treatments: $t = \frac{t_1+t_2+t_3}{3}$						
$\{t_1, t_2\} =$						
	$\{\frac{1}{3}, \frac{1}{3}\}$	$\{\frac{1}{3}, \frac{2}{3}\}$	$\{\frac{1}{3}, 1\}$	$\{\frac{2}{3}, \frac{2}{3}\}$	$\{\frac{2}{3}, 1\}$	$\{1, 1\}$
$t_3 =$	$\frac{3}{9}$	$\frac{4}{9}$	$\frac{5}{9}$	$\frac{5}{9}$	$\frac{6}{9}$	$\frac{7}{9}$
	$\frac{4}{9}$	$\frac{5}{9}$	$\frac{6}{9}$	$\frac{6}{9}$	$\frac{7}{9}$	$\frac{8}{9}$
	$\frac{5}{9}$	$\frac{6}{9}$	$\frac{7}{9}$	$\frac{7}{9}$	$\frac{8}{9}$	$\frac{9}{9}$
	$\frac{5}{9}$	$\frac{6}{9}$	$\frac{7}{9}$	$\frac{7}{9}$	$\frac{8}{9}$	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: $t = \frac{t_1+t_2+2t_3}{4}$						
$\{t_1, t_2\} =$						
	$\{\frac{1}{3}, \frac{1}{3}\}$	$\{\frac{1}{3}, \frac{2}{3}\}$	$\{\frac{1}{3}, 1\}$	$\{\frac{2}{3}, \frac{2}{3}\}$	$\{\frac{2}{3}, 1\}$	$\{1, 1\}$
$t_3 =$	$\frac{4}{12}$	$\frac{5}{12}$	$\frac{6}{12}$	$\frac{6}{12}$	$\frac{7}{12}$	$\frac{8}{12}$
	$\frac{6}{12}$	$\frac{7}{12}$	$\frac{8}{12}$	$\frac{8}{12}$	$\frac{9}{12}$	$\frac{10}{12}$
	$\frac{12}{12}$	$\frac{12}{12}$	$\frac{12}{12}$	$\frac{12}{12}$	$\frac{12}{12}$	$\frac{12}{12}$
	$\frac{8}{12}$	$\frac{9}{12}$	$\frac{10}{12}$	$\frac{10}{12}$	$\frac{11}{12}$	1
	$\mathcal{T}^{\text{High}} = \{\frac{4}{12}, \frac{5}{12}, \frac{6}{12}, \frac{7}{12}, \frac{8}{12}, \frac{9}{12}, \frac{10}{12}, \frac{11}{12}, 1\}$					

6.2.2 Equilibria under maximization of expected pay-offs

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

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

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 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 $\frac{2}{3}$.

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

Hence, if we restrict considerations to pure strategies, we find two sets of subgame perfect Nash equilibria. The first set is $(t_1, t_2, d) = (1, 1, d_x)$, $x \in [0, \frac{1}{3}] \cap \mathcal{D}$. 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 6.3 lists all subgame perfect pure strategy equilibria with (expected) take rates, (expected) destruction rates and (expected) payoffs.

6.2.3 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 (see appendix D.1) 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

Table 6.3: Subgame perfect pure strategy equilibria for 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	\emptyset
	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	$\frac{7}{9}$
	Expected destruction rate	\emptyset
	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	$\frac{2}{3}$
	Expected destruction rate	$\frac{2}{3}$
	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	\emptyset
	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	$\frac{4}{10}$
	Expected destruction rate	$\frac{4}{12}$
	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	$\frac{3}{9}$
	Expected destruction rate	\emptyset
	Payoffs	$\pi_1 = \pi_2 = \frac{3}{8}e, \pi_3 = \frac{2}{8}e$

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 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 (see appendices D.2 and D.3) 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%),

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, 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.

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 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 6.4 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.

6.3 Experimental results

Table 6.4 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 6.5 and 6.6 (on pp. 122 and 123) provide a summary of destruction rate data.

Table 6.4: 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	

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.

6.3.1 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.

6.3.1.1 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 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%: $\frac{1}{3}$).

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

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.

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

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 responder/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: $p = .251$).

6.3.1.2 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,

$p = .068$).

6.3.1.3 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, $p = .082$).

6.3.2 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 6.5 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 6.6.

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 6.5: 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

6.3.3 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 6.6 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

Table 6.6: Destruction rates: Overview

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

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 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

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

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.

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 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.o

6.3.4 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.

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 6.7 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, $p < .001$). 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

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

Table 6.7: Responder types
Treatment

	PartLow	PartHigh	NoPartLow	NoPartHigh	Total
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%)

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 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 6.8 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 ($t < \frac{9}{12}$), we observe no clear-cut effect concerning mean destruction rates between both treatments. However, for $t \geq \frac{9}{12}$, mean destruction rates of responders in the NoPart treat-

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.

ments 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 $t = \frac{8}{9}$ ($p = .026$), $t = \frac{8}{9}$ ($p = .044$), and $t = 1$ ($p = .021$). The last column of table 6.8 provides the corresponding p -values (one-sided) for any possible group take rate t .

Table 6.8: Destruction rates of type-2 responders

Take rate	Treatment						p^{23}
	Part			NoPart			
	n	mean	sd	n	mean	sd	
$t = \frac{3}{3}$	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
$t = \frac{4}{3}$	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
$t = \frac{7}{3}$	15	0.182	0.184	13	0.165	0.189	.379
$t = \frac{9}{12}$	18	0.222	0.244	14	0.283	0.240	.261
$t = \frac{10}{3}$	33	0.314	0.247	27	0.319	0.266	.485
$t = \frac{11}{12}$	18	0.478	0.332	14	0.468	0.285	.500
$t = \frac{12}{3}$	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
$t = \frac{8}{12}$	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

Thus, we find that participation matters for type 2 responders. For low and medium group takes rates ($t \leq \frac{10}{12}$), the destruction rates do not differ significantly between Part and NoPart treatments ($p \geq .151$). However, for high group take rates ($t > \frac{10}{12}$), 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.

6.4 Conclusion

This chapter 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.

Chapter 7

Conclusion

There is substantial empirical evidence and increasing recognition that not only outcomes but also procedures leading to these outcomes affect people's utility from and their reactions to those decisions. However, as discussed in chapter 2, economists only recently started to focus on procedures in contrast to a huge number of studies provided by non-economists (e.g., psychologists, sociologists, political scientists, and legal scholars). In this dissertation, I have presented experimental evidence in favor of the existence of procedural effects. In particular, the economic experiments which I have conducted (or which I have taken part in conducting) were intended to answer four main research questions as stated in chapter 1. To sum up, the experiments provide the following evidence: first, individual procedural fairness judgements are not of prime importance. In line with previous research from various fields, individuals offer less resistance to unfavorable decisions if these decisions resulted from a fair procedure rather than from a procedure judged to be less fair. This is consistent with the *fair process effect*, describing the finding that people are more likely to accept outcomes if they feel that they are made via fair procedures. Although the effect is said to be exceedingly robust, the observed behavioral differences according to people's fairness evaluations turned out not to be statistically significant in experiment 1 (chapter 4).

Second, experimental data from experiment 1 and 2 suggest that *procedural satisfaction* is relevant for decision making. The studies introduced the notion of procedural satisfaction to account for possible behavioral effects resulting from the mere fact that people's preferences concerning procedures are either fulfilled or not. The experimental design is able to capture two kinds of procedural satisfaction, i.e., by individuals and by the group. Analogous to the fair process effect, we hypothesized that individuals rather accept an outcome which results from a preferred than from a non-preferred procedure. Indeed, experiment 2 (chapter 5) confirms the hypothesis for individual

procedural satisfaction. Responders offered less resistance to any outcome if their individual procedural preferences were satisfied. The effect is significant for high tax claims. In contrast, experiment 1 (chapter 4) revealed the opposite: responders whose individual procedural preferences were satisfied offered significantly *more* resistance than those whose procedural preferences were violated. As both experiments differ in a strategic feature, i.e., proposers either know or do not know how they have been appointed, responders seem to evaluate proposers' claims differently depending on the information being available to proposers.

With regard to group procedural satisfaction, data from both experiments suggests a more unified picture. For any claim, responders offered more resistance if their group procedural preferences are satisfied. However, the differences are only significant in experiment 1. The direction of the effect is reversed from what we expected. Two possible reasons come to the fore: the *frustration effect* and the *expectation effect*. The expectation effect describes the fact that people form expectations about the contributions of others and adjust their contribution decisions accordingly. Such an expectation effect can go in either direction: it may raise contributions or lower contributions, depending on whether one expects one's own contribution to be necessary or superfluous to make resistance successful. Further research would be necessary to explain the findings.

Previous research has shown that fair procedures generally trigger positive responses, but the opposite may be true as well. Some studies found a so-called frustration effect to occur under certain conditions: fair procedures can result in less satisfaction with negative outcomes than unfair procedures. It has been argued that the effect only occurs in settings where the characteristics that give the procedure a procedural fairness advantage are relatively weak. In chapter 2, I discuss the effect and its underlying reasons in greater detail. I argue that these findings from the procedural fairness literature can be transferred to the broader notion of procedural satisfaction. Again, further research in this direction is encouraged.

Third, I investigated whether individuals claim a smaller share of a group endowment if they have been appointed proposers based on a procedure which was either accepted by the group or rejected. Experiment 2 reveals that an *I-want-You effect* exists: proposers whose group procedural preferences were satisfied claimed less than proposers whose group procedural preferences were not satisfied. This finding is in line with previous findings and thus underlines the robustness of the experimental design. Besides, it shows that procedural judgements are of great importance for both responders and proposers.

Fourth, this dissertation was intended to answer the question whether

individuals rather accept an outcome if they have the opportunity to participate in the decision making process regarding the distribution of a group endowment. Procedural fairness can be characterized by various fairness criteria, amongst others participation in the decision making process. Experiment 3 (chapter 6) shows that behavior depends on the possibility to participate. Responders destroyed more if they had no opportunity to participate in the decision making process and were confronted with highly unfavorable outcomes. This participation effect is highly significant for those responders (the majority) who show negative reciprocity (i.e. they destroyed more when takers are greedier).

I believe that these results contribute to a deeper understanding of the behavioral foundations of procedural effects in decision-making. The findings suggest that future research should account for people's procedural judgments in order to contribute to the inclusion of any behaviorally relevant factor.

Appendix A

Appendix to Chapter 3

A.1 Figures: Responder types

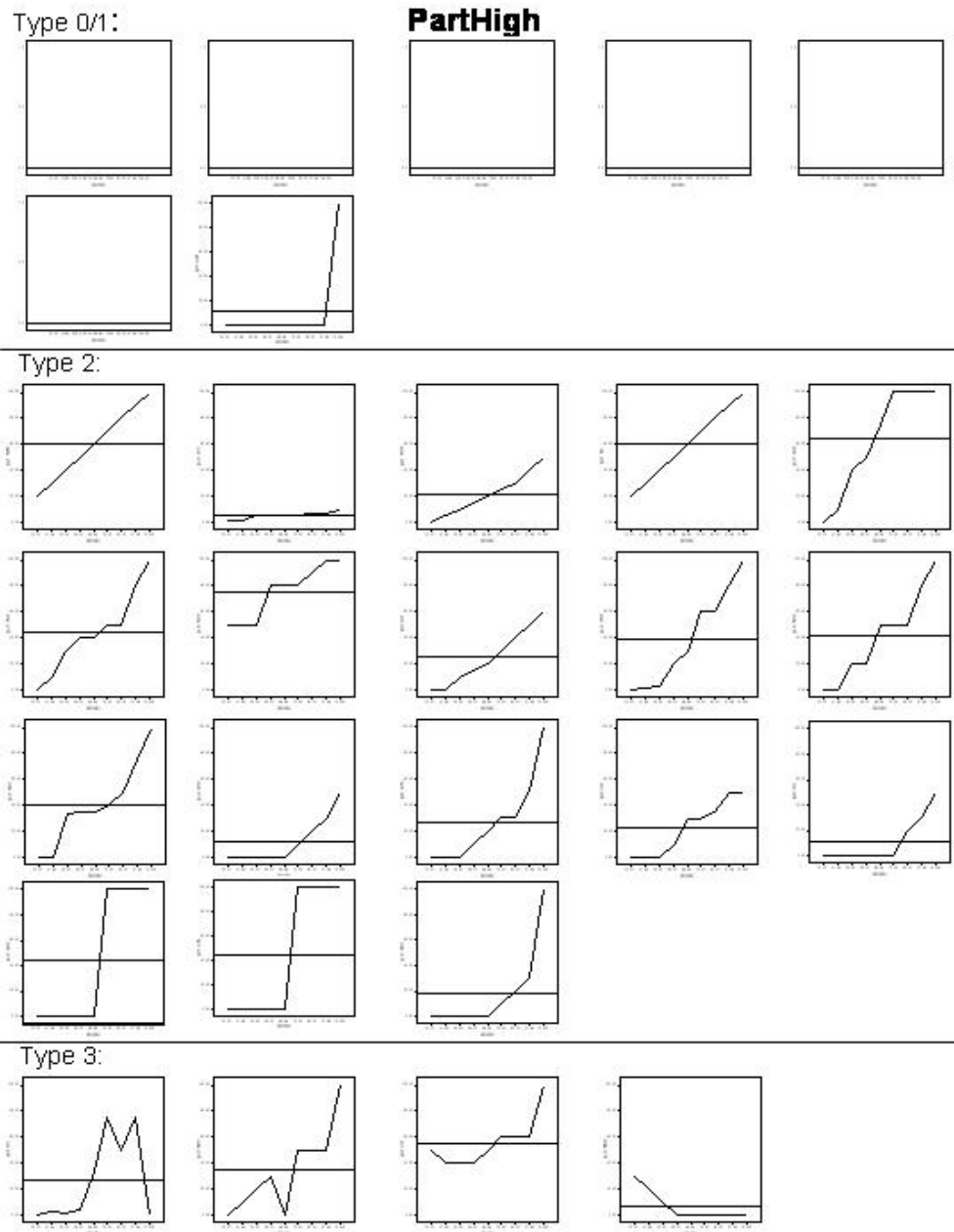


Figure A.1: Responder types in treatment PartHigh

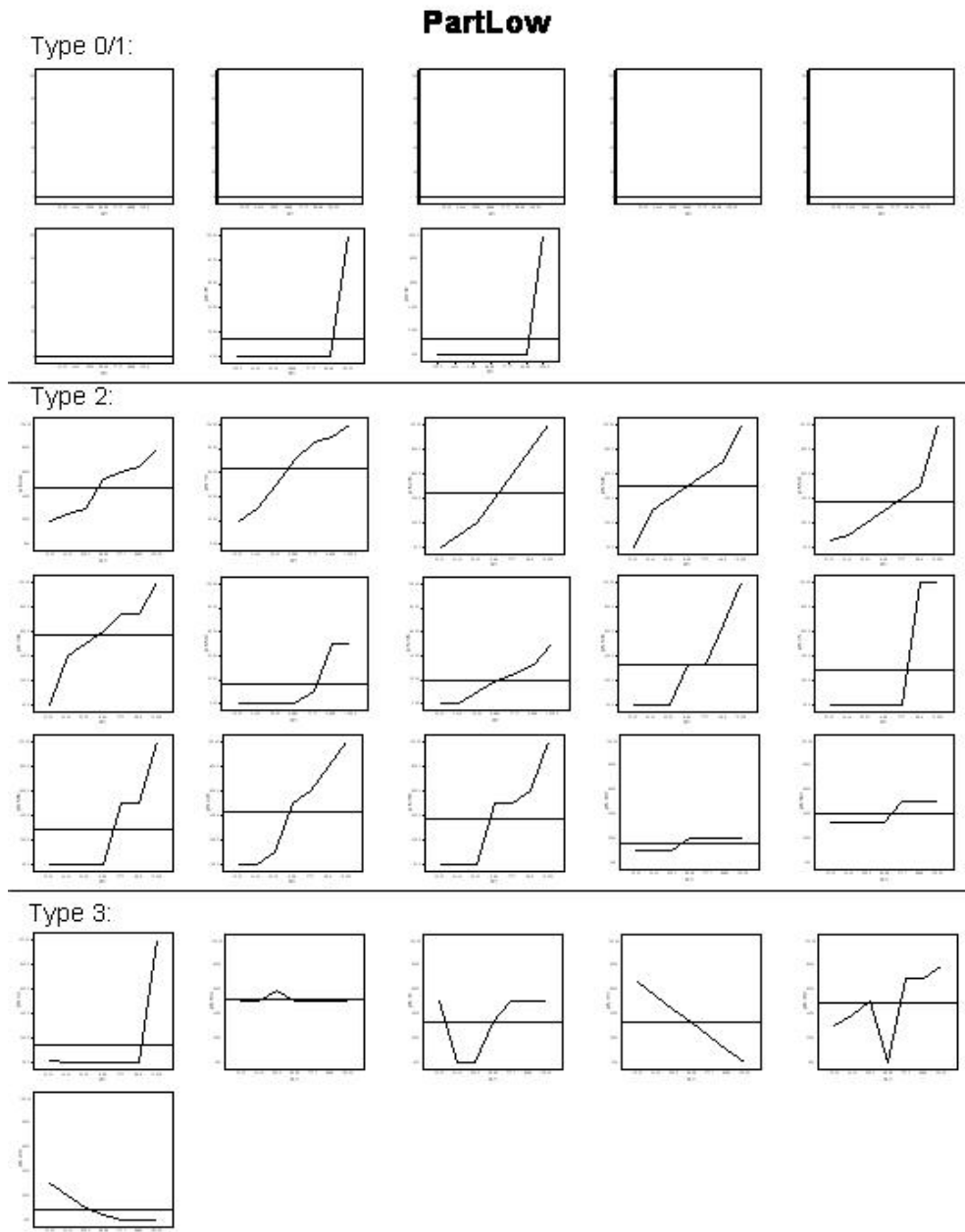


Figure A.2: Responder types in treatment PartLow

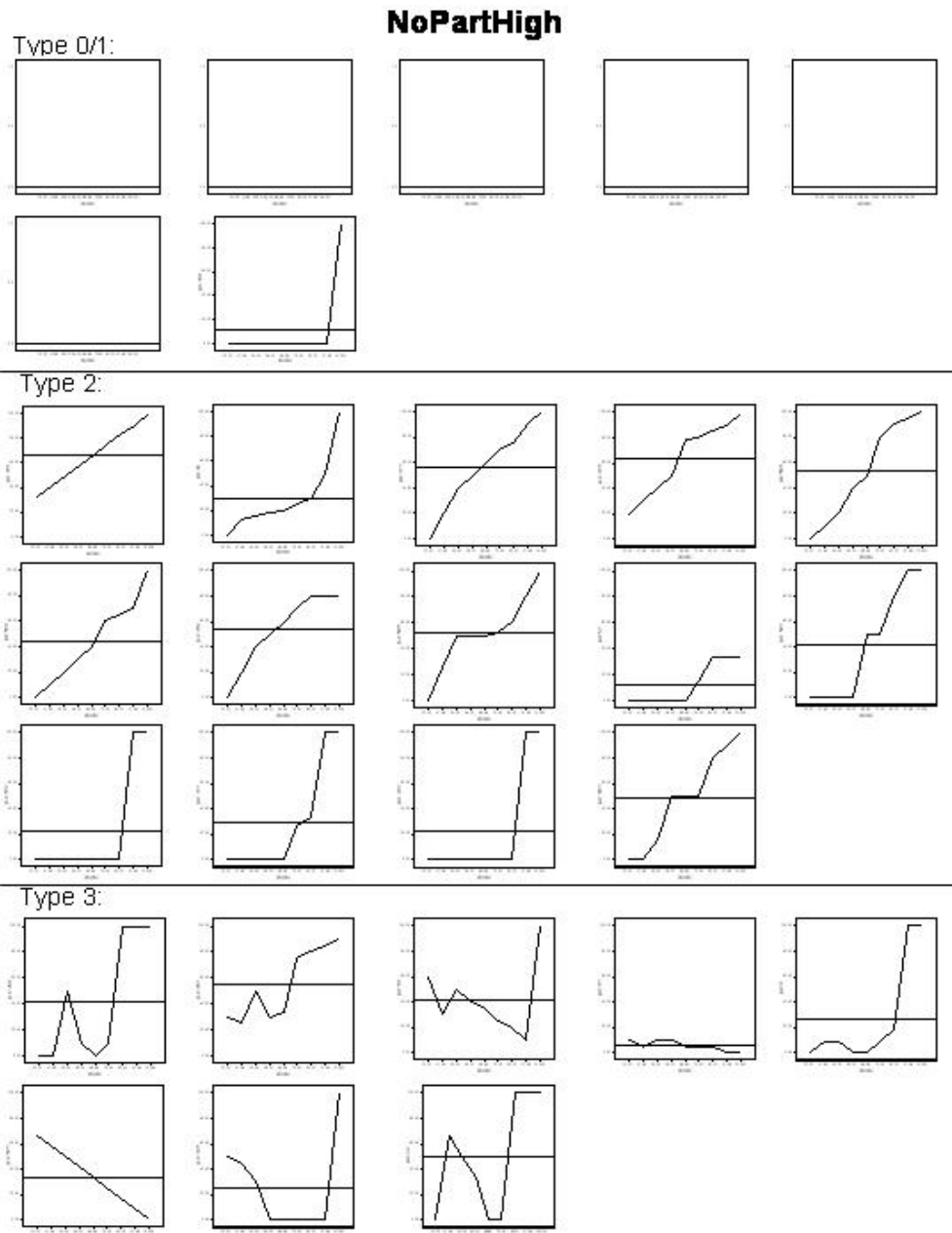


Figure A.3: Responder types in treatment NoPartHigh

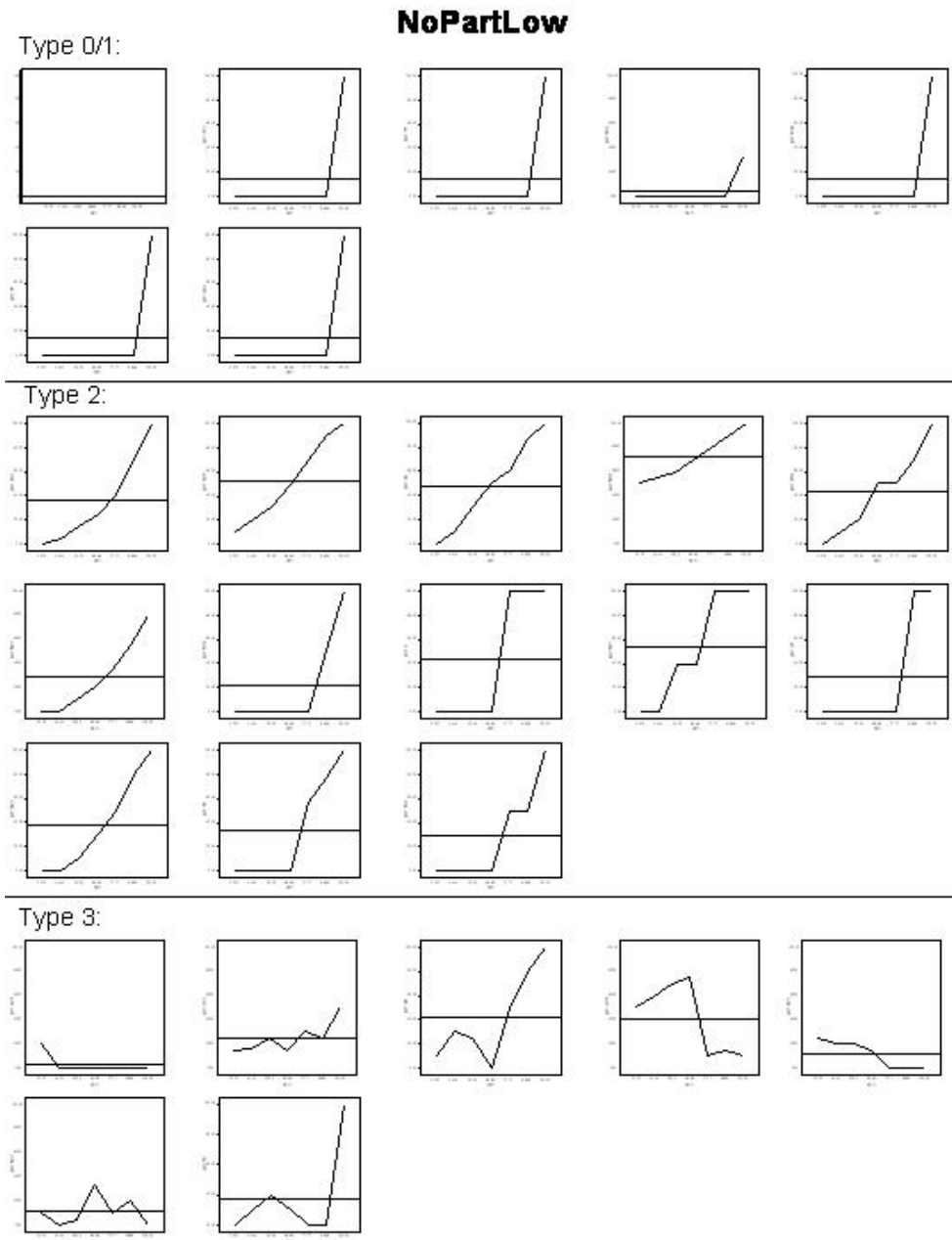


Figure A.4: Responder types in treatment NoPartLow

Appendix B

Appendices to Chapter 4

B.1 Sign-up sheet

The original sign-up sheets were in German. Each participant filled in this sheet before the experiment was conducted.

1. Participant number: _____

2. Gender:

male

female

3. Age: _____

4. Approximately how many hours per week do you volunteer?

less than 1 hour/week

about 1 hour/week

about 2 hours/week

about 3 hours/week

about 4 hours/week

about 5 hours/week

about 6 hours/week

about 7 hours/week

about 8 hours/week

more than 8 hours/week

5. Do you participate in this experiment for the first time? [*applied in session 1 to 3 only*]
- yes no
6. Have you participated in the experiment at the University's Open Day 2005? [*applied in session 4 and 5 only*]
- yes no
7. Are you studying? [*applied in session 4 and 5 only*]
- yes, field of study: _____ no

B.2 Written instructions

The original instructions were in German. They will be sent on request. This appendix reprints a translation of them.

Instructions

Welcome to our experiment!

The experiment lasts about 30 minutes and has four stages. During the first three stages, you will be asked to make decisions. You will get sheets with precise instructions. **Your payment depends on your decisions and the decisions of the other participants.** In stage four, we will ask you to fill in a questionnaire.

Please do not communicate with any other participant from now on. If you have any questions, please raise your hand. We will assist and help you. You are allowed to ask questions at any time. Please do not ask questions in public. Please ask only us, do not ask other participants. Thank you!

Please read the explanations for any decision stage carefully. Then make your decision and enter it in the appropriate sheet. A stage is completed when all decisions are made. Please do not turn back the page.

You will be assigned to a group of **5 people** (you and four other participants). During the experiment, you will play in the same group. Each of your teammates will be handed out exactly the same instructions. We will keep group membership, decisions and payment in confidence.

In our experiment, you can earn **chips**. **At the end of the experiment your earnings in chips will be converted to Euros at the rate of 4 chips = 1 Euro. That is, each earned chip equals 0.25 Euro.** The more chips you earn, the higher your payment will be.

Decision sheet (stage 1)

There are 4 *chip owners* in your group. Each chip owner has 25 chips. The fifth member of your group has no chips at the beginning. The group member without chips will be called *participant X*. No one in your group knows, who participant X is. It might be that you are participant X. Your first task is to imagine to be participant X. As participant X, you have no chips, while each chip owner has 25 chips. It is now up to you to propose, how many chips each chip owner is supposed to yield to you:

0 chips, 5 chips, 10 chips, 15 chips, 20 chips, or 25 chips.

Each participant in your group has been asked to make such a transfer proposal; only one group member is participant X. Later in the course of the experiment, it will be decided who participant X is. Besides, later during the course of the experiment, it will be decided whether the transfer proposal of participant X will be realized.

Example: You decide in favor of a transfer of 10 chips. If you are participant X and your proposal has been realized, you will get 10 chips from each chip owner. These are 40 chips in sum for you. As each chip owner has 25 chips in the beginning and now has to give over 10 chips to you, 15 chips remain for each chip owner. The result of your proposal would be: 40 chips for you in the role of participant X, 15 chips for each chip owner.

Please decide now in favor of a transfer. Mark with a cross the chosen transfer in the last column! Mark with only one tick. If you want to cancel an entry, cross it clearly out and tick another box.

Transfer:	Each chip owner keeps:	As participant X, you get:	Mark <u>one</u> entry:
0 chips	25 chips	0 chips	
5 chips	20 chips	20 chips	
10 chips	15 chips	40 chips	
15 chips	10 chips	60 chips	
20 chips	5 chips	80 chips	
25 chips	0 chips	100 chips	

Stage 1 is completed when you made your decision.

Decision sheet (stage 2)

In stage 1, **each member of your group** made a proposal in the role of participant X, how many chips the other group members should transfer to him/her. However, only **one** member of your group can really be participant X. **One of the following rules** will decide who participant X will be.

Rule 1: The experimenter decides someone to be participant X without being aware of your proposals.

Rule 2: Your group is asked to vote for one of two options in deciding someone to be participant X: the **oldest group member or the one who volunteers his/her time most frequently**. **If more than one group member comes into question (e.g. because two persons are of the same age), participant X will be drawn.**

It will not be announced at any time who participant X is.

A) Please indicate your opinion about both rules.

1. In your opinion, which rule should be used: rule 1 (the experimenter decides) or rule 2 (vote of the group)?

rule 1 rule 2

2. Please give reasons for your decision:

3. How do you rate the importance of the appointment procedure on a scale from very important to completely unimportant?

very important completely unimportant

4. How do you rate rule 1 (the experimenter decides) on a scale from very unfair to completely fair?

very unfair completely fair

5. How do you rate rule 2 (vote of the group) on a scale from very unfair to completely fair?

very unfair completely fair

B) Please vote now: Who should be participant X, if rule 2 is used?
If you do not tick any box, you cast a ballot for the first possibility.

- the oldest participant
 the participant who volunteers his/her time most frequently.

Stage 2 is completed when you have answered all questions.

Comments on stage 3

In stage 2, you and any other group member have indicated whether **rule 1** (the experimenter decides) or **rule 2** (vote of the group over the criteria *age* and *engagement*) should be used. The result of the vote is as follows:

_____ group member(s) voted for rule 1 and
 _____ group member(s) voted for rule 2.

Thus, the majority voted for rule _____.

In your group, the majority / minority (*to be deleted as applicable*) decides which rule will be used. Thus:

Participant X of your group will be assigned by rule _____.

Participant X is yet to be determined, but you do not know, who it is and which transfer participant X claims. In case you are **not** participant X but a chip owner, you have to make another decision: Do you pay voluntarily the transfer to participant X or do you resist? Because you do not know, which transfer participant X has claimed, you have to decide **for each possible proposal** of participant X. In case you are participant X, your decision in stage 3 is irrelevant.

As a chip owner, you are now endowed with 25 chips. Participant X claims a transfer from you and from the other chip owners: either 0, 5, 10, 15, 20, or 25 chips. **If you do not want to pay the transfer**, you may pay any number of chips out of your 25 chips into a **common account**. Half chips can be payed, to. All chip owners are asked to do the same. **In case the chip owners pay at least 26 chips in sum into the common account, the transfer to participant X need not to be paid.**

Thus there are two alternatives:

1. **There are paid 26 chips or more in sum into the common account:** Then nobody has to transfer anything to participant X. Thus participant X receives 0 chips. The chip owners can keep all chips, which they have not paid into the common account. The chips paid into the common account are lost.
2. **There are paid less than 26 chips in sum into the common account:** The chips paid into the common account are lost. All chip owners have to pay the transfer; if they have not enough chips left, they will pay as

much as they can. Participant X receives the transfer he/she has asked for or somewhat less, if a chip owner has not got enough chips left to pay the whole transfer.

Three examples: Participant X asks each chip owner for a transfer of 10 chips. You do not want to pay the transfer.

1. You pay 5.5 chips into the common account. The other three chip owners pay 22.5 chips in sum. Thus, there are 28 chips in the common account. Therefore, the transfer need not to be paid. You will receive 19.5 chips: 25 chips minus 5.5 chips (your transfer into the common account) equals 19.5 chips. Participant X receives 0 chips.

2. You pay 6 chips into the common account. The other three chip owners pay 13.5 chips in sum. Thus, there are 19.5 chips in the common account. Therefore, the transfer must be paid. You will receive 9 chips: 25 chips minus 6 chips (your transfer into the common account) minus 10 chips (transfer) equals 9 chips. Participant X receives 40 chips in sum: 10 from each chip owner.

3. You pay 20 chips into the common account. The other three chip owners pay 3.5 chips in sum. Thus, there are 23.5 chips in the common account. Therefore, the transfer must be paid. You will receive 0 chips: 25 chips minus 20 Chips (your transfer into the common account) equals 5 chips; you have to transfer these 5 chips to participant X. Participant X receives 35 chips in sum: only 5 chips from you, because you cannot pay more, and 10 chips from each other chip owner.

You will receive 0.25 Euro cash for each chip you have earned in stage 1 to 3.

Decision sheet (stage 3)

For your group applies:

Participant X will be assigned by rule _____.
--

As a reminder: rule 1 says that the experimenter determines participant X. Rule 2 says that the group votes.

In case you are a chip owner and not participant X please specify, how many chips you want to pay into the common account: 0 chips, 0.5 chips, 1 chip, 1.5 chips, 2 chips, 2.5 chips, 3 chips, ...24 chips, 24.5 chips, or all 25 chips. You are asked to make a decision for *each* possible transfer proposal of participant X.

1. Assume that participant X claims a transfer of 25 chips. In this case, how many of your 25 chips do you want to pay into the common account?
I would like to pay _____ chips.
2. Assume that participant X claims a transfer of 20 chips. In this case, how many of your 25 chips do you want to pay into the common account?
I would like to pay _____ chips.
3. Assume that participant X claims a transfer of 15 chips. In this case, how many of your 25 chips do you want to pay into the common account?
I would like to pay _____ chips.
4. Assume that participant X claims a transfer of 10 chips. In this case, how many of your 25 chips do you want to pay into the common account?
I would like to pay _____ chips.
5. Assume that participant X claims a transfer of 5 chips. In this case, how many of your 25 chips do you want to pay into the common account?
I would like to pay _____ chips.
6. Assume that participant X claims a transfer of 0 chips. In this case, how many of your 25 chips do you want to pay into the common account?
I would like to pay _____ chips.

Stage 3 is completed when you have made all six decisions.

B.3 Post-experimental questionnaire

Questionnaire (stage 4)

Finally, we would like to ask you to fill in a short questionnaire.

1. The comments to stage 1 (decision as participant X) were clear and comprehensible.

I agree I do not agree

2. The comments to stage 2 (determination of participant X) were clear and comprehensible.

I agree I do not agree

3. The comments to stage 3 (decisions as chip owner) were clear and comprehensible.

I agree I do not agree

4. I tried to earn as many chips and thus as many Euro as possible.

I agree I do not agree

5. I thought twice about each decision.

I agree I do not agree

6. Your group has been assigned one out of two possible rules for determination of participant X. Subsequently, you have decided on your individual resistance for each possible transfer proposal of participant X. How do you rate the influence of the assigned rule on your individual resistance decisions?

strong influence no influence at all

7. Each group member has voted which rule to determine participant X is right. The result of the vote has been communicated to you. How do you rate the influence of the group opinion on your individual resistance decisions?

strong influence no influence at all

8. What is a fair transfer proposal of participant X?

A fair transfer proposal of participant X is _____ chips.

9. Remember your decision in the role of a chip owner in response to a transfer proposal of 15 chips. In your opinion, should your group offer resistance?

no

yes

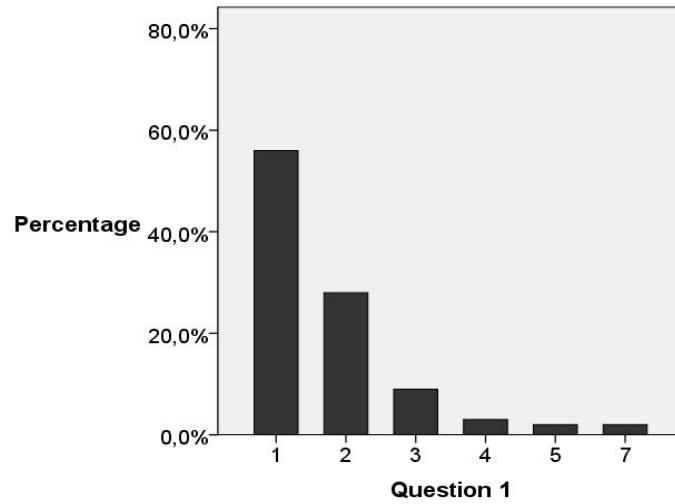
10. Let us assume that you would know the other three chip owners have paid exactly 19.5 chips into the common account in response to a transfer proposal of 15 chips. What would you do?

In that case, I would pay _____ chips into the common account.

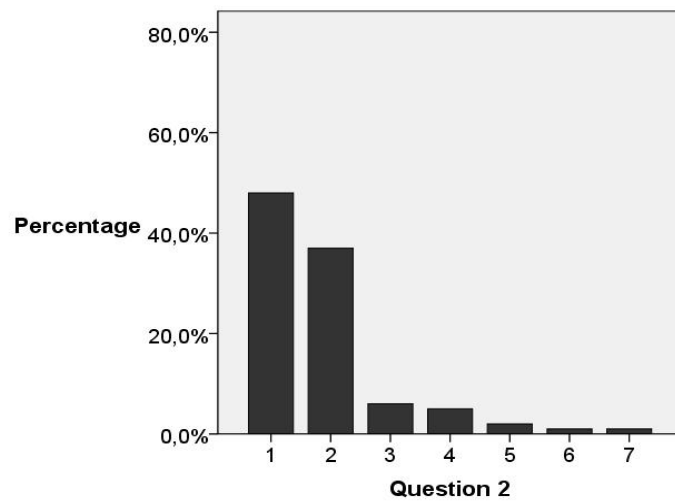
Thank you very much for participating!

B.4 Post-experimental questionnaire: Answers

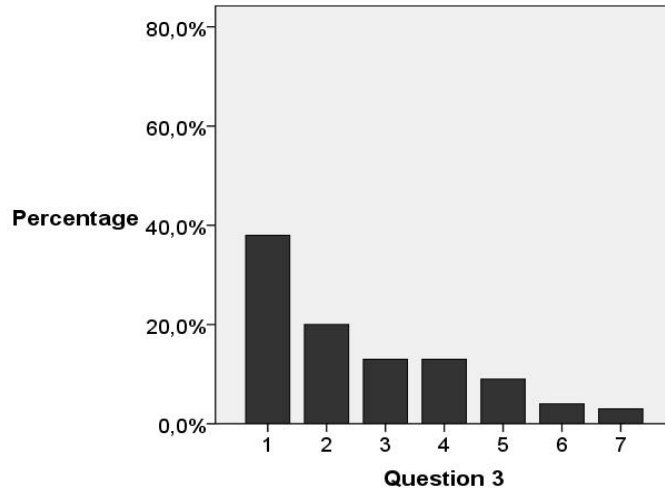
1. The comments to stage 1 (decision as participant X) were clear and comprehensible. (1: “I agree” to 7: “I do not agree”)



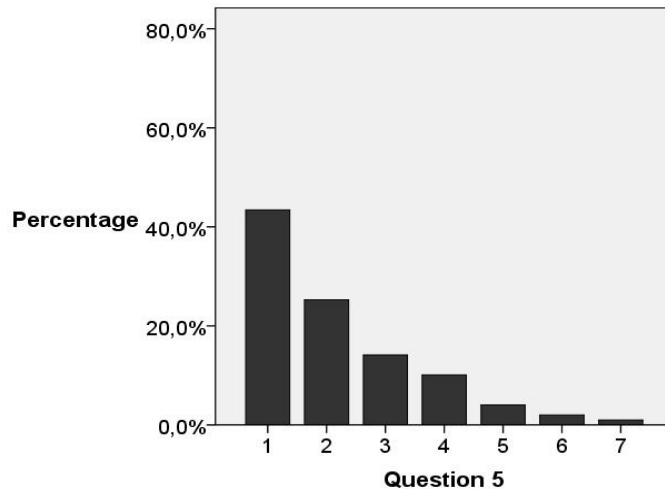
2. The comments to stage 2 (determination of participant X) were clear and comprehensible. (1: “I agree” to 7: “I do not agree”)



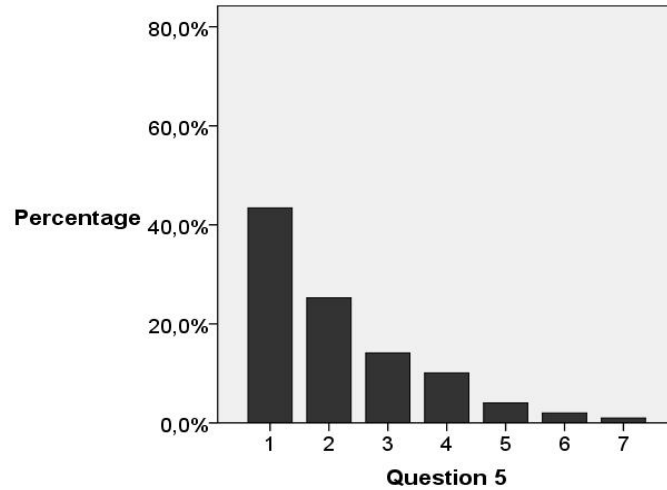
3. The comments to stage 3 (decisions as chip owner) were clear and comprehensible. (1: “I agree” to 7: “I do not agree”)



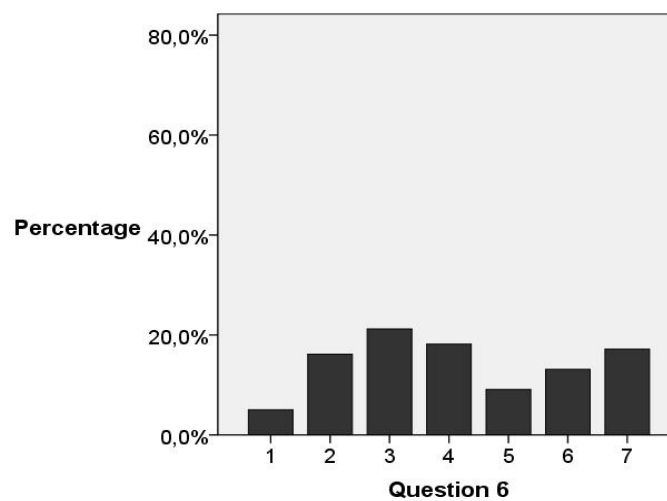
4. I tried to earn as many chips and thus as many Euro as possible. (1: “I agree” to 7: “I do not agree”)



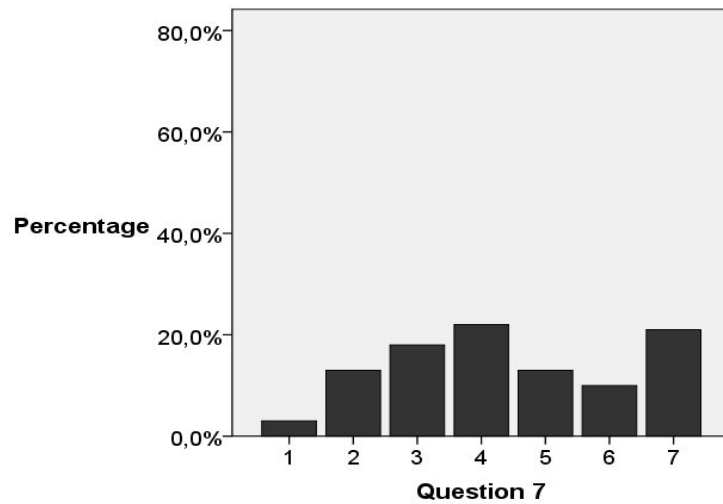
5. I thought twice about each decision. (1: “I agree” to 7: “I do not agree”)



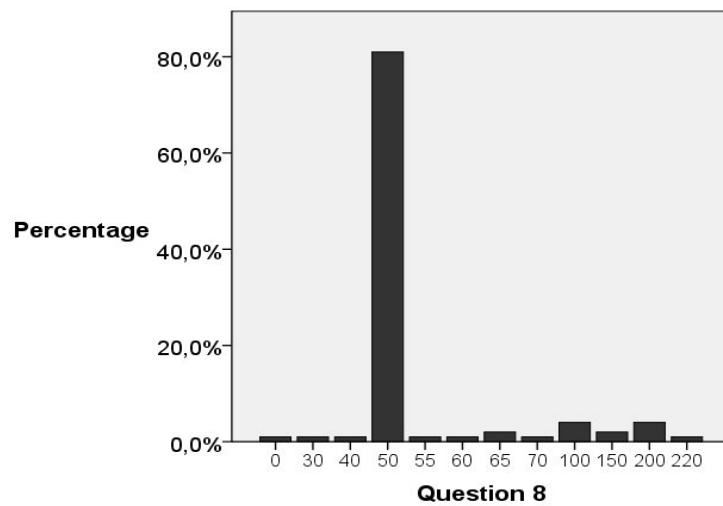
6. Your group has been assigned one out of two possible rules for determination of participant X. Subsequently, you have decided on your individual resistance for each possible transfer proposal of participant X. How do you rate the influence of the assigned rule on your individual resistance decisions? (1: “strong influence” to 7: “no influence at all”)



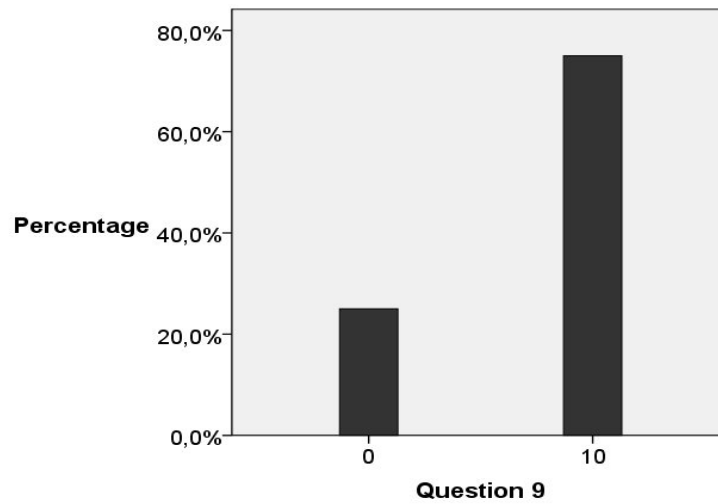
7. Each group member has voted which rule to determine participant X is right. The result of the vote has been communicated to you. How do you rate the influence of the group opinion on your individual resistance decisions? (1: “strong influence” to 7: “no influence at all”)



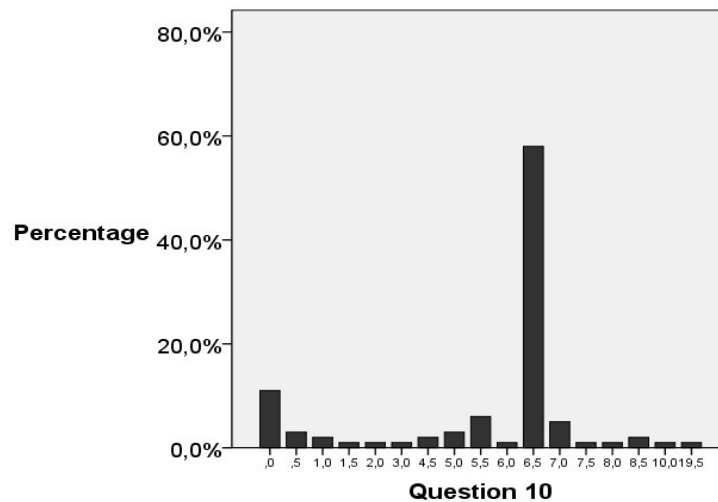
8. What is a fair transfer proposal of participant X?



9. Remember your decision in the role of a chip owner in response to a transfer proposal of 15 chips. In your opinion, should your group offer resistance?



10. Let us assume that you would know the other three chip owners have paid exactly 19.5 chips into the common account in response to a transfer proposal of 15 chips. What would you do?



Appendix C

Appendices to Chapter 5

C.1 Written instructions

The original instructions are in Bulgarian. This appendix reprints a translation of majority treatment instructions for proposers. The original instructions in Bulgarian or a German version will be sent on request.

Instructions

Welcome to our experiment! This experiment lasts about 45 minutes.

Please do not communicate with any other participant from now on. If you have any questions, please raise your hand. We will answer them. You can raise questions throughout the experiment. Please do not ask questions in public. Ask only the experimenter, do not ask other participants. Thank you!

The experiment has four stages. You can earn money depending on your decisions and on the decisions of the other members in your group. In stage four, we will ask you to fill in a short questionnaire. You have been randomly assigned to a group of 5 people (you and four other participants). You are paired with these persons throughout the experiment. Nobody, however, will be informed about who is paired with whom or who earns how much. This information we will keep in confidence.

In our experiment, you can earn **chips**. At the end of the experiment your earnings in chips will be converted to Leva at the rate of 4 chips = 1 Lev, i.e. each earned chip equals 0.25 Lev.

Please read all sheets carefully. Then take your decisions and fill in the sheets accordingly. A stage is completed if all decisions are filled in. Please do not scroll back.

Decision sheet (stage 1)

There are 4 *chip owners* in your group and 100 chips in total. Each chip owner gets 25 chips. The fifth member of your group has 0 chips at the beginning. This group member without chips is called *participant X*.

During the experiment participant X proposes how many chips each chip owner is supposed to yield to him. Thereafter, chip owners have to decide whether or not to offer resistance against the proposal of participant X. The attempt to offer resistance costs chips. If no resistance is offered or resistance was unsuccessful, participant X obtains the number of chips claimed (as long as they have not been spent for resistance). If resistance is successful, participant X receives nothing.

However, only one group member is actually participant X. One of the following two rules determines who will be participant X.

Rule 1: The experimenter chooses someone to be participant X.

Rule 2: The group is asked to vote for one of two options in deciding someone to be participant X: either the oldest group member or the one who spends most time in student and volunteer activities. If more than one group member has to be considered (for instance, two persons are of the same age) participant X will be chosen by lot.

A) Please indicate your opinion about both rules.

1. How do you rate the importance of the rule for choosing participant X on a scale from very important to very unimportant?

very important very unimportant

2. How do you rate rule 1 (the experimenter decides) on a scale from very unfair to extremely fair?

very unfair very fair

3. How do you rate rule 2 (group voting) on a scale from very unfair to extremely fair?

very unfair very fair

B) Please vote now: If you do not tick any box, your vote counts for the first one.

1. In your opinion, which rule should be used: rule 1 (the experimenter decides) or rule 2 (vote of the group)?

- rule 1
- rule 2
- 2. If rule 2 is applied, who should be participant X?
 - the oldest participant
 - the participant who spends most time for student and volunteer activities

Stage 1 is completed when you have answered all questions.

<h2 style="margin: 0;">Decision sheet (stage 2)</h2>
--

In stage 1, each group member has voted for a rule to be applied for choosing participant X. The voting result is as follows:

_____ votes for rule 1.

_____ votes for rule 2.

Thus, the majority voted for rule _____

The majority decides which rule will be used.

Thus: Rule _____ applies.

Based on this rule you have been assigned the role of participant X.

No participant of the experiment will never know that you are participant X. As participant X, you have 0 chips, while each chip owner has 25 chips. It is now up to you to propose how many chips each chip owner is supposed to yield to you:

0 chips, 5 chips, 10 chips, 15 chips, 20 chips or 25 chips.

In stage 3 of the experiment, chip owners can decide whether to offer resistance against your proposal or not. The attempt to offer resistance costs chips. If no resistance is offered or resistance was unsuccessful, you as participant X receive the number of chips you had proposed (as long as the chips have not been spent for resistance). If resistance is successful, you receive nothing.

Example: You propose a transfer of 10 chips. If your proposal is implemented, you will get 10 chips from each chip owner, that is, 40 chips in sum. As each chip owner has 25 chips at the beginning and is then asked to yield 10 chips to you, 15 chips are left for the chip owner. The result would be: 40 chips for you as participant X and 15 chips for each chip owner.

Please decide now. Mark with a cross the chosen transfer in the last column! Tick only one transfer. In case you tick more than one or none transfer, the lowest transfer is counted. If you want to cancel an entry, cross it clearly out and tick another entry.

Transfer:	Each chip owner keeps:	You will get:	Your proposal
0 chips	25 chips	0 chips	
5 chips	20 chips	20 chips	
10 chips	15 chips	40 chips	
15 chips	10 chips	60 chips	
20 chips	5 chips	80 chips	
25 chips	0 chips	100 chips	

Stage 2 is completed when you have chosen a transfer.

Comments on stage 3

You as participant X have decided in stage 2 how many chips you claim from each participant. Now the four other participants take their decisions. However, we would like to ask you to imagine that you are in the situation of a chip owner. Now we would like to ask you how you would decide in such a situation. Your decision now has no consequences for you payoff.

The chip owners have to decide either to pay the chips which you have demanded as player X or to show resistance. Since the chip owners do not know how much you have demanded they have to decide for each possible claim.

Please imagine now that you are in the role of a chips owner. As a chip owner, you are endowed with 25 chips. Participant X asks for a transfer from you and from the other chip owners: either 0, 5, 10, 15, 20 or 25 chips. If you do not want to pay the transfer, you may pay any number of chips out of your 25 chips into the common account. All chip owners are asked to do the same. In case the chip owners pay at least 26 chips in sum into the common account, the transfer to participant X need not to be paid.

Thus there are two alternatives:

1. **There are paid 26 chips or more in sum into the common account:** Then nobody has to transfer anything to participant X. Thus participant X receives 0 chips. The chip owners can keep all chips, which they have not paid into the common account. The chips paid into the common account are lost.
2. **There are paid less than 26 chips in sum into the common account:** The chips paid into the common account are lost. All chip owners have to pay the transfer; if they have not enough chips left, they will pay as much as they can. Participant X receives the transfer he has asked for or somewhat less, if a chip owner has not got enough chips left to pay the whole transfer.

Three examples:

1. Participant X asks each chip owner for a transfer of 10 chips. You do not want to pay the transfer. You pay 5.5 chips into the common account. The other three chip owners pay 22.5 chips in sum. Thus, there are 28 chips in the common account. Therefore, the transfer need not to be paid. You will receive 19.5 chips: 25 chips minus 5.5 chips (your transfer into the common account) equals 19.5 chips. Participant X receives 0 chips.
2. Participant X asks each chip owner for a transfer of 10 chips. You do not want to pay the transfer. You pay 6 chips into the common account. The other three chip owners pay 13.5 chips in sum. Thus, there are 19.5 chips in the common account. Therefore, the transfer must be paid. You will receive 9 chips: 25 chips minus 6 chips (your transfer into the common account) minus 10 chips (transfer to participant X) equals 9 chips. Participant X receives 40 chips in sum: 10 from each chip owner.
3. Participant X asks each chip owner for a transfer of 10 chips. You do not want to pay the transfer. You pay 20 chips into the common account. The other three chip owners pay 3.5 chips in sum. Thus, there are 23.5 chips in the common account. Therefore, the transfer must be paid. You will receive 0 chips: 25 chips minus 20 Chips (your transfer into the common account) equals 5 chips; you have to transfer these 5 chips to participant X. Participant X receives 35 chips in sum: only 5 chips from you, because you cannot pay more, and 10 from each other chip owner.

Decision sheet (stage 3)

As a reminder: In stage 1 each group member has decided for a rule in order to determine participant X. **The rule which received the majority of votes was applied to select participant X.** Please indicate how many chips you would like to transfer to the common account. You can pay any sum between 0 and 25 chips and you can also pay half chips. You are asked to make a decision for each possible transfer proposal of participant X.

1. Assume that participant X asks for a transfer of 25 chips. In this case, how many chips of your endowment of 25 chips do you want to pay into the common account?

I would like to pay _____ chips.

2. Assume that participant X asks for a transfer of 20 chips. In this case, how many chips of your endowment of 25 chips do you want to pay into the common account?

I would like to pay _____ chips.

3. Assume that participant X asks for a transfer of 15 chips. In this case, how many chips of your endowment of 25 chips do you want to pay into the common account?

I would like to pay _____ chips.

4. Assume that participant X asks for a transfer of 10 chips. In this case, how many chips of your endowment of 25 chips do you want to pay into the common account?

I would like to pay _____ chips.

5. Assume that participant X asks for a transfer of 5 chips. In this case, how many chips of your endowment of 25 chips do you want to pay into the common account?

I would like to pay _____ chips.

6. Assume that participant X asks for a transfer of 0 chips. In this case, how many chips of your endowment of 25 chips do you want to pay into the common account?

I would like to pay _____ chips.

Stage 3 is completed when you have reached all six decisions.

C.2 Post-experimental questionnaire

Questionnaire (stage 4)

Code No.:

At the end of the experiment, we would like to ask you to fill in a short questionnaire.

1. The comments to stage 1 (determination of participant X) were clear and comprehensible.

I agree I do not agree

2. The comments to stage 2 (decision in the role of participant X) were clear and comprehensible.

I agree I do not agree

3. The comments to stage 3 (decisions in the role of a chip owner) were clear and comprehensible.

I agree I do not agree

4. I tried to earn as many chips and thus as many Lewa as possible.

I agree I do not agree

5. I thought twice about each decision.

I agree I do not agree

6. According to rule 1, the experimenter decides who becomes participant X. Do you prefer the experimenter to be a professor from your department or an unknown third person or don't you care?

preferably a professor

preferably an unknown third person

I don't care

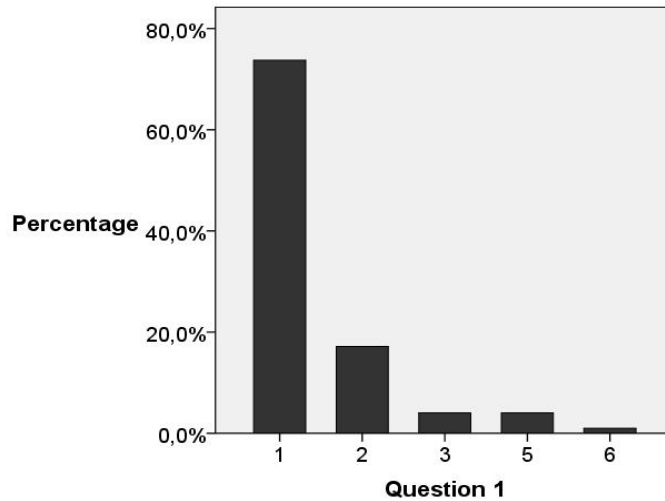
7. To which study group (German, English, French) do you belong to?

Your group: _____

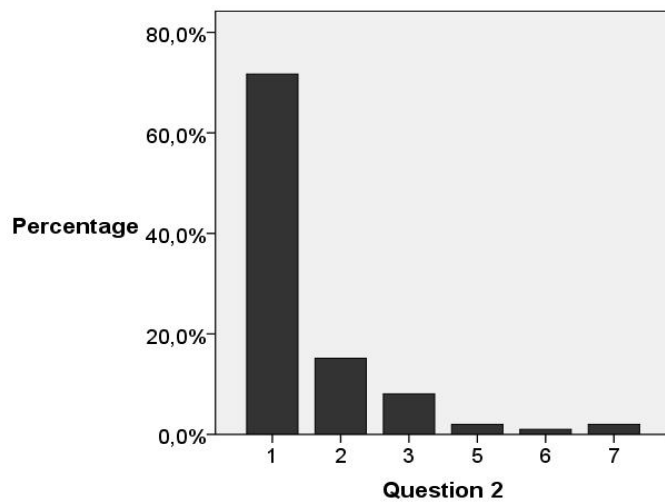
8. Imagine, you take part in the experiment twice. You know in the first round that the other 4 group members belong to the same study group

C.3 Post-experimental questionnaire: Answers

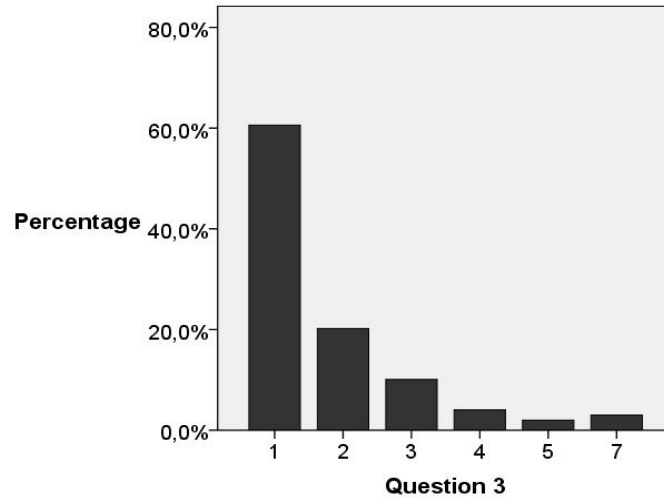
1. The comments to stage 1 (determination of participant X) were clear and comprehensible. (1: “I agree” to 7: “I do not agree”)



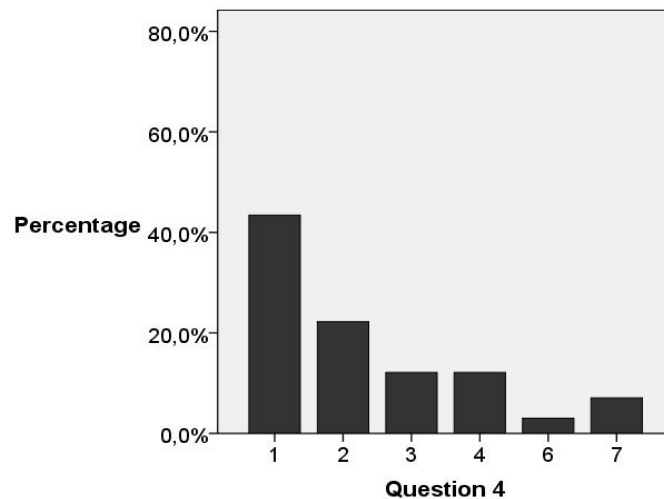
2. The comments to stage 2 (decision in the role of participant X) were clear and comprehensible. (1: “I agree” to 7: “I do not agree”)



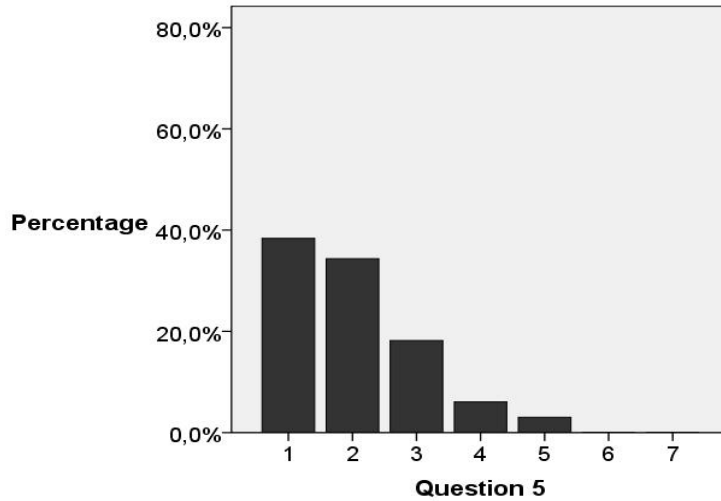
3. The comments to stage 3 (decisions in the role of a chip owner) were clear and comprehensible. (1: “I agree” to 7: “I do not agree”)



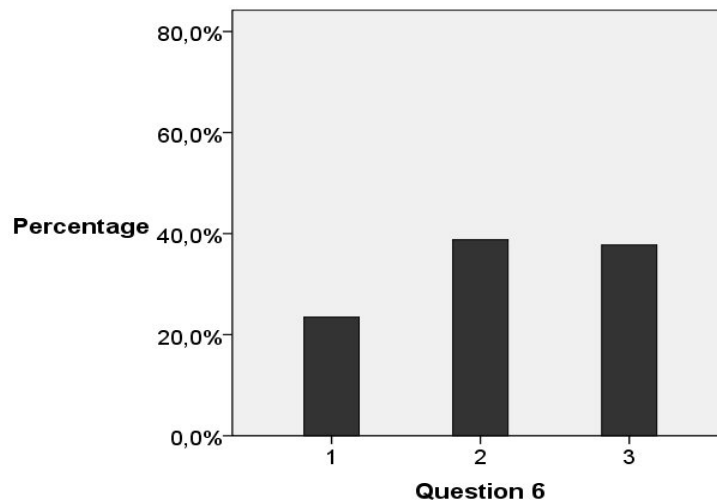
4. I tried to earn as many chips and thus as many Lewa as possible. (1: “I agree” to 7: “I do not agree”)



5. I thought twice about each decision. (1: “I agree” to 7: “I do not agree”)

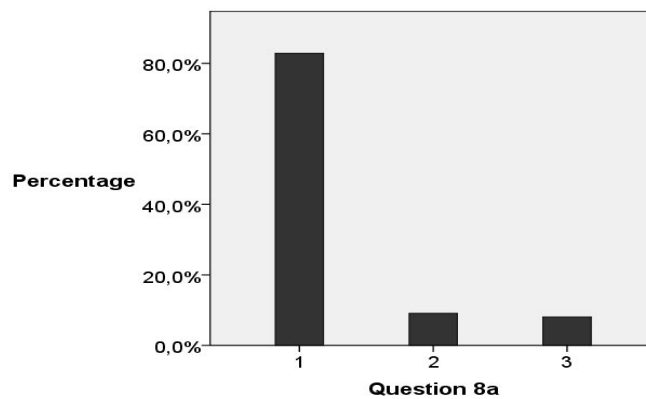


6. According to rule 1, the experimenter decides who becomes participant X. Do you prefer the experimenter to be a professor from your department or an unknown third person or don't you care? (1: “professor”; 2: “unknown third person”; 3: “I don't care”)

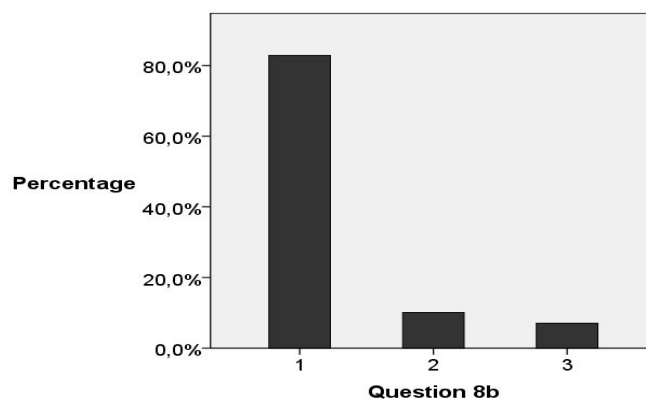


8. Imagine, you take part in the experiment twice. You know in the first round that the other 4 group members belong to the same study group (English, French, German) as you do (homogeneous group composition). In the second round the study groups are mixed up (heterogeneous group composition). Would you make exactly the same decisions in both rounds or would you make different decisions? (1: “I make the same decisions”; 2: “I claim a larger part in a homogeneous group”; 3: “I claim a smaller part in a homogeneous group”)

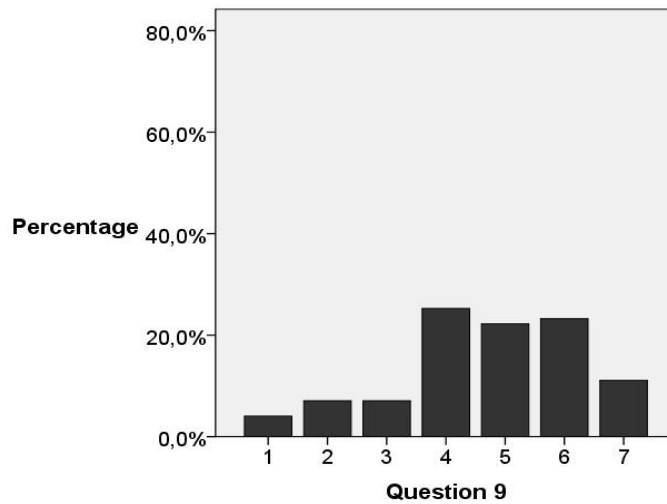
As participant X:



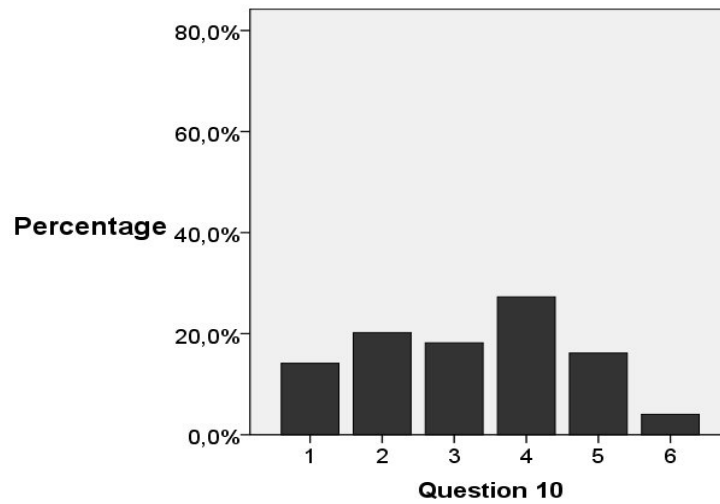
As a chip owner:



9. In your opinion, how strong is the influence of participant X's legitimacy on the resistance level? (1: "very weak" to 7: "very strong")



10. How do you rate your religiousness on a six-point scale? (1: "not religious" to 6: "very religious")



Appendix D

Appendices to Chapter 6

D.1 Written instructions

These are the written instructions, translated from the German original, for the PartLow treatment. The instructions for the other three treatments were very similar and are available upon request. Formatting (e.g., use of bold face) follows the original, with the exception of spacing. Screenshots of the computerized experiment, z-tree files and raw data are also available from the authors upon request.

Instructions

Welcome! This experiment lasts about 20 minutes and consists of three stages. In stage 1 and 2, you will be asked to make decisions. For this, you will receive precise instructions. In stage 3, you are asked to fill in a short questionnaire. All answers are treated as confidential. Your chances to win the prize of 500 Euro depends only on your decisions and the decisions of your group members in the first two stages. Your chances do not depend on your questionnaire answers. **Please do not communicate with any other participant from now on.** If you have any questions, please raise your hand. An experimenter will come to you and answer your questions individually. **At every moment in the experiment, you can ask the experimenters questions. Do not ask anyone else and make sure you ask quietly.** Thank you!

Please read the instructions carefully. The experiment does not start until each participant has completely read and understood these instructions. You have to type your decisions into the corresponding field on the screen. Once you have entered and confirmed your decisions, a stage is completed. Note that all decisions are final and cannot be changed at a later date.

In this experiment, you have been randomly assigned to a **group of three players**. No participant will find out, who his/her teammates are and which decisions they made. We will treat group membership and decisions as confidential. **Each group consists of two A participants and one B participant**. By drawing a number at the reception desk, you were assigned either role A or role B. You will keep this role throughout the experiment. At a later date, you will be informed about your role.

In our experiment, you can earn **chips** by making decisions. **Your chances to win the prize of 500 Euro depends on your decisions and the decisions of your teammates. Every whole earned chips will be converted into a lottery ticket**. Example: You earn 3.7 chips and thus receive 3 lottery tickets. At the drawing, taking place during lecture time next week, the number of lottery tickets will be fixed at 1044. Thus, the probability of winning the prize of 500 Euro is $1/1044$ for each earned chip. This probability does not depend on the number of lottery tickets other participants earn. **Remember: The more chips you earn, the higher your chance to win the prize of 500 Euro**. In addition, remaining lottery tickets are distributed among all participants by a fair random mechanism. In addition to and independent of individual decisions, each participant who participates in the experiment according to the rules, receives 5 credit points for the written examination in *Introductory Microeconomics*.

Stage one (group claim)

There are 2 participants A and 1 participant B in your group. **At the beginning, the B participant has an endowment of 9 chips and each A participant has 0 chips.** In stage 1, the group chooses a percentage. **This percentage determines how many of the 9 chips participant B has to transfer after stage two to both participants A (one half each).**

Example: Suppose that the group decides that 33.33% of the endowment of 9 chips of participant B (that is 3 chips) will be transferred to both participants A. Each A receives 1.5 chips and B keeps 6 chips.

The voting procedure is as follows:

Both participants A and participant B propose a percentage. This percentage determines how much of B's endowment of 9 chips after stage two will be transferred to the participants A. The following percentages are available: 33.33%, 66.66% and 100%. Each group member makes a proposal by ticking the appropriate number.

The decision rule is a *compromise* of all three proposals: All proposals are added and divided by three. Thus, the average take rate is the group decision. To conclude, the voting rule is:

$$[(A's \text{ proposal}) + (A's \text{ proposal}) + (B's \text{ proposal})] : 3$$

Note that participant B is involved in bringing about a decision on how many chips to transfer from participant B to both participants A.

Example: One participant A chooses 100%, the other participant A chooses 33.33%. B chooses 100%. The average proposal is $(100\% + 33.33\% + 100\%) : 3 = 77.77\%$ and this is the group decision on their take rate.

Stage two (destruction of endowment)

In stage one, the group has decided on participant's B transfer to both participants A. This decision has been made by participants A and participant B.

In stage two, only participant B makes a decision.

Now, participant B can react to the group's decision in stage one by deciding which percentage of his/her endowment will be de-

stroyed. The percentage chosen must be an integer in the interval $[0, 100]$.

The transfer from participant B to each participant A will be based on the residual endowment of participant B that is left after stage two.

At that time, participant B does not know yet which percentage has been chosen by the group in stage one. Participant B is only informed of the percentage he/she has proposed, but not of the percentages each participant A has chosen. Thus, in stage two, B has to decide on the destruction of his/her endowment for any feasible resulting percentage.

After stage two, the experiment is completed. In stage three, you will be asked several questions. Your answers on this questions do not influence your chances to win the prize.

Payoffs

After stage two, all participants will be informed of the number of lottery tickets they have earned during the experiment. They will also be informed of the number of lottery tickets earned by their teammates. Additionally, each participant may receive further lottery tickets by a random procedure. No participant is able to influence the allocation of additional lottery tickets.

Example of how to calculate your number of lottery tickets:

Remember that participant B has an endowment of 9 chips. Each A participant has 0 chips. Suppose that in **stage one a percentage of 66.66% has been chosen by the group**. This means that participant B keeps 3 chips and transfers 6 chips (66.66% of 9 chips) to the A participants. Thus, the payoff of each participant A equals 3 chips ($6 : 2 = 3$).

Remember that in **stage two**, participant B can destroy any percentage of his/her initial endowment of 9 chips. Destroyed chips are not available for allocation within the group any more.

Suppose that participant B decides to **destroy 0% of his/her endowment**. In this case, the whole endowment of 9 chips remains. Participant B's transfer to both participants A is 6 chips (66.66% of 9 chips). Each participant A receives 3 chips and participant B keeps also 3 chips (participant B's endowment of 9 chips minus the transfer of 6 chips). Thus, each participant of this group receives 3 lottery tickets.

Now suppose that, in this example, participant B decides to **destroy 50% of his/her endowment** as reaction to a group claim of 66.66%. Thus, only 4.5 chips would remain as the half of 9 chips have been destroyed. In this case, participant B's transfer to both participants A is again 66.66%, but 66.66% of 4.5 chips, thus 3 chips. Each participant A receives 1.5 chips and participant B keeps 1.5 chips. As the exchange rate between chips and lottery tickets is 1:1 and only whole numbers of chips are converted to lottery tickets, each group member receives 1 lottery ticket.

If you have any questions now, please raise your hand and we will come to you. If you do not have questions any more, please wait until an experimenter announces the start of the experiment and click then on "start experiment".

Thank you for participating!

16. How do you rate a transfer of 66.66% (stage 1)?
very fair very unfair
17. How do you rate a transfer of 100% (stage 1)?
very fair very unfair
18. Decision in stage 1 (fraction of B's endowment to be handed out to both A's): which claim is the fairest?
 33.33% 66.66% 100%
19. Decision in stage 2 (fraction of B's endowment that B destroys): do you consider the possibility of participants B to destroy part of their own endowment reasonable?
 yes no
20. What might be the motivation of participants B to destroy part of their own endowment? _____
21. What do you think: which fraction of their endowment do participants B destroy on average in reaction to a claim of 66.66%? _____
22. Do you think that participants B destroy a different fraction depending on whether they proposed a transfer or not?
 yes no
23. Do you think that participants B who proposed a transfer destroy a larger, a smaller or the same fraction in reaction to a low transfer in comparison to participants B who did not propose a transfer?
 larger fraction smaller fraction same fraction
24. Do you think that participants B who proposed a transfer destroy a larger, a smaller or the same fraction in reaction to a medium transfer in comparison to participants B who did not propose a transfer?
 larger fraction smaller fraction same fraction
25. Do you think that participants B who proposed a transfer destroy a larger, a smaller or the same fraction in reaction to a high transfer in comparison to participants B who did not propose a transfer?

larger fraction
fraction

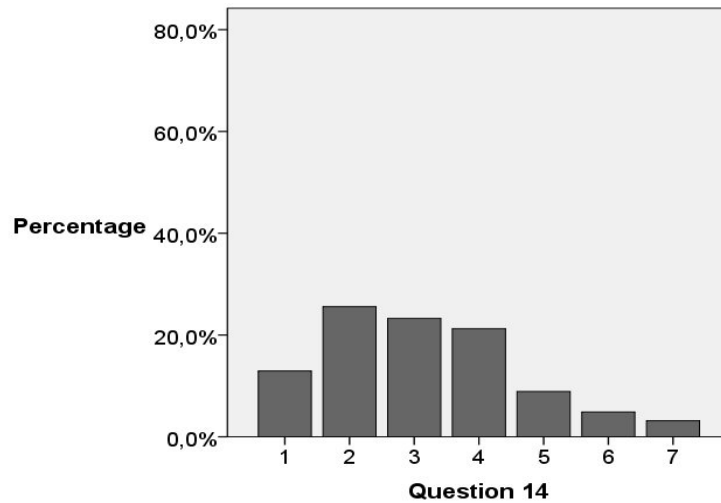
smaller fraction

same

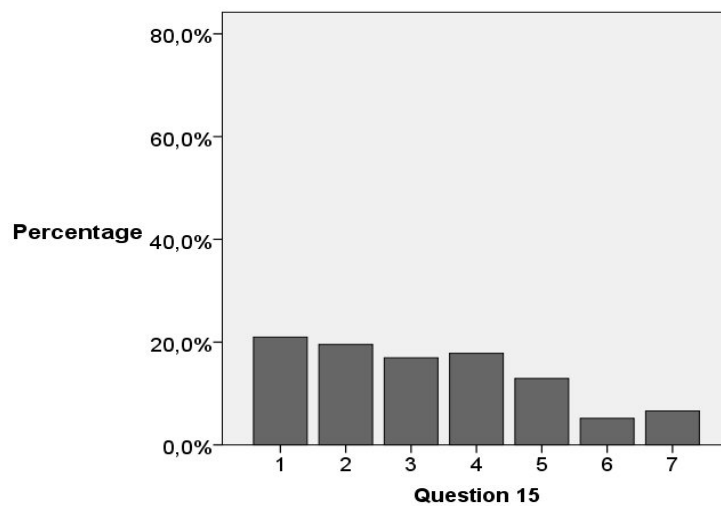
Thank you very much for taking part in the experiment. You can leave now.

D.3 Post-experimental questionnaire: Answers

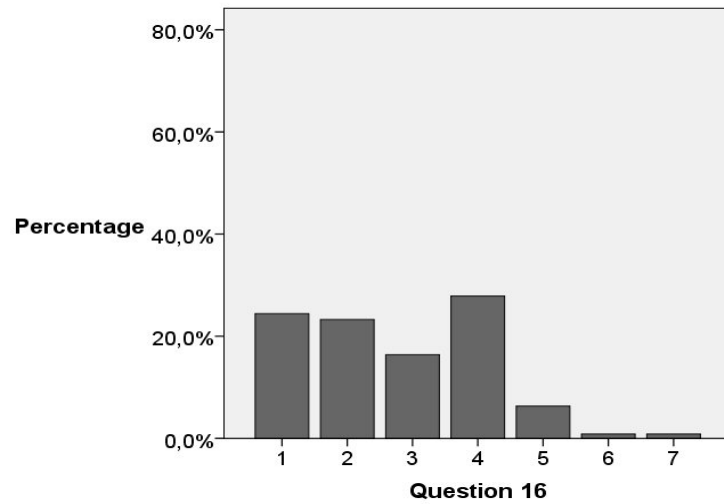
14. How do you rate the voting procedure? (1: “very fair” to 7: “very unfair”)



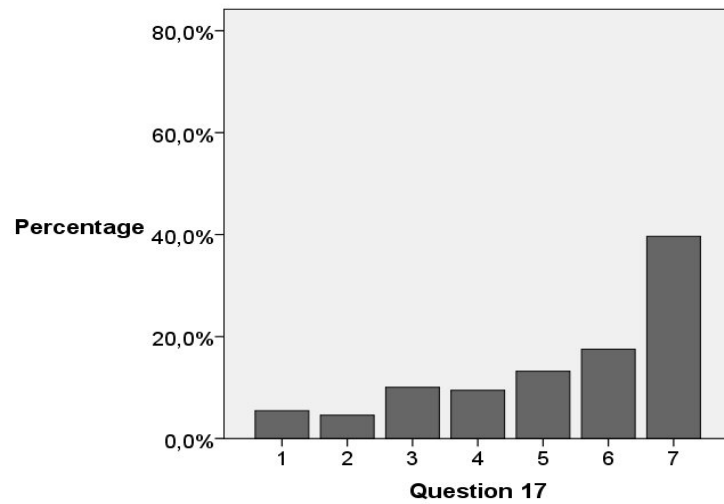
15. How do you rate a transfer of 33.33% (stage 1)? (1: “very fair” to 7: “very unfair”)



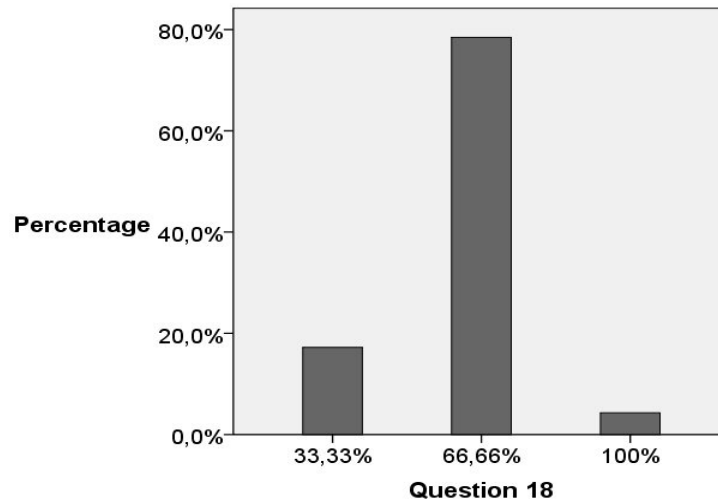
16. How do you rate a transfer of 66.66% (stage 1)? (1: “very fair” to 7: “very unfair”)



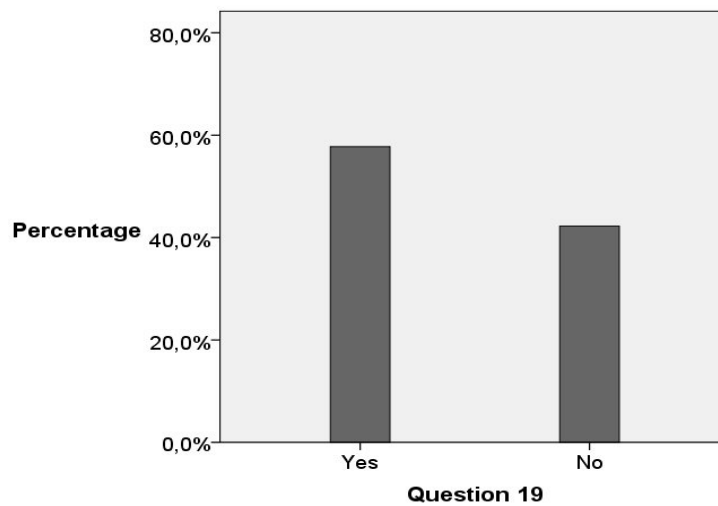
17. How do you rate a transfer of 100% (stage 1)? (1: “very fair” to 7: “very unfair”)



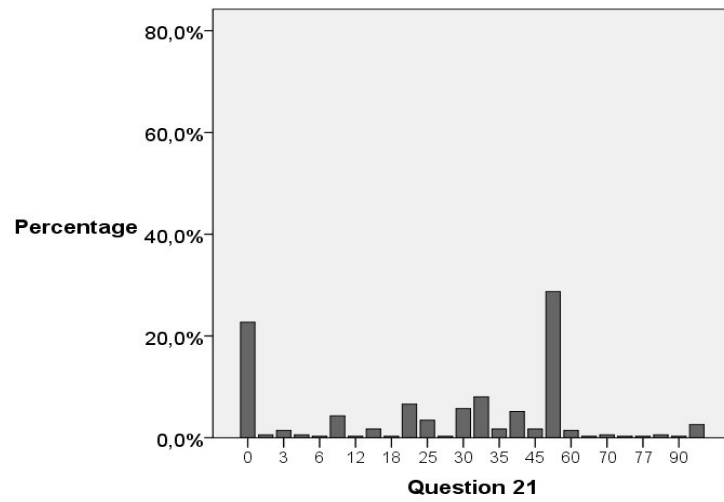
18. Decision in stage 1 (fraction of B's endowment to be handed out to both A's): which claim is the fairest one?



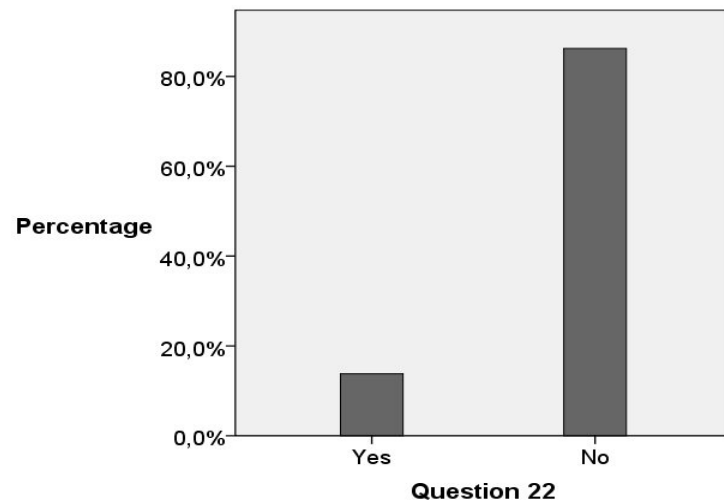
19. Decision in stage 2 (fraction of B's endowment that B destroys): do you consider the possibility of participants B to destroy part of their own endowment reasonable?



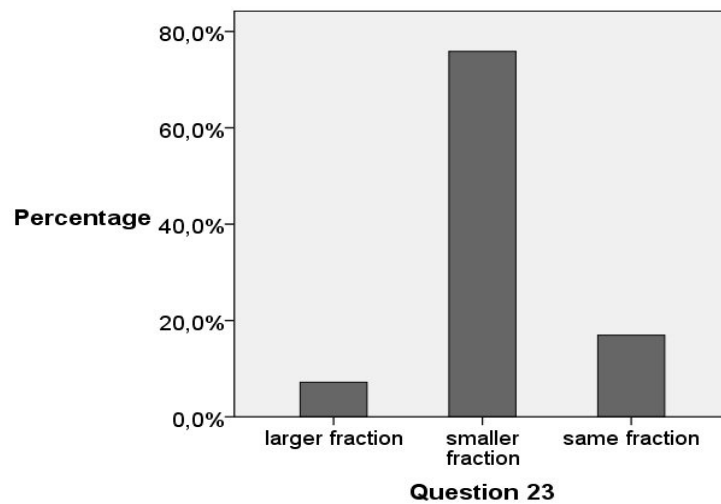
21. What do you think: which fraction of their endowment do participants B destroy on average in reaction to a claim of 66.66%?



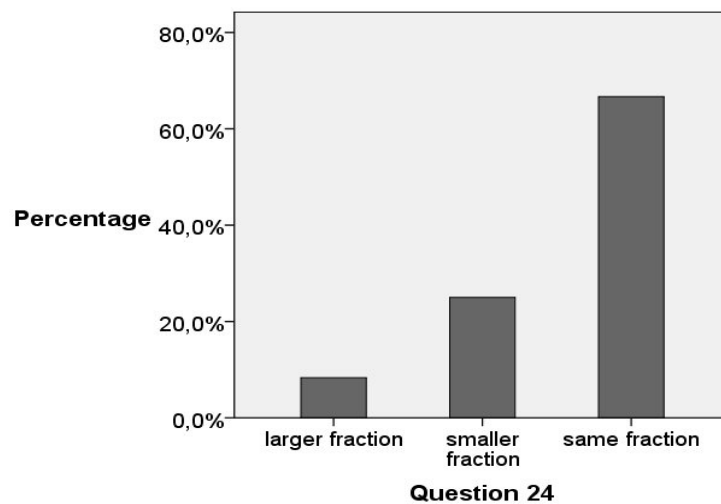
22. Do you think that participants B destroy a different fraction depending on whether they proposed a transfer or not?



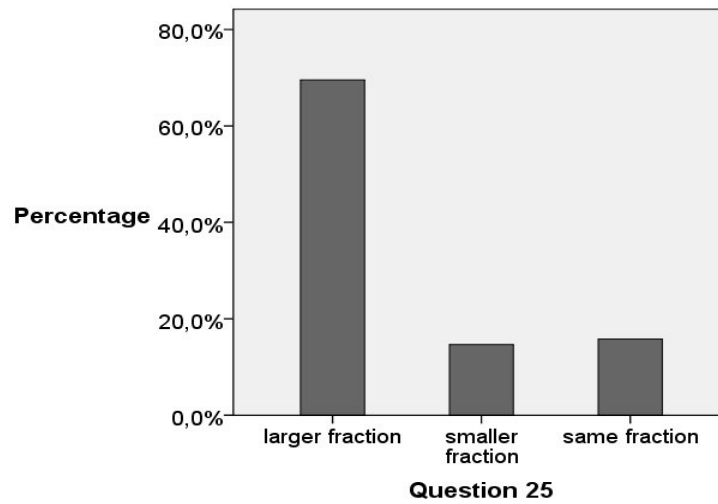
23. Do you think that participants B who proposed a transfer destroy a larger, a smaller or the same fraction in reaction to a low transfer in comparison to participants B who did not propose a transfer?



24. Do you think that participants B who proposed a transfer destroy a larger, a smaller or the same fraction in reaction to a medium transfer in comparison to participants B who did not propose a transfer?



25. Do you think that participants B who proposed a transfer destroy a larger, a smaller or the same fraction in reaction to a high transfer in comparison to participants B who did not propose a transfer?



Appendix E

Eidesstattliche Erklärung

Ich erkläre an Eides statt:

Ich habe die vorgelegte Dissertation selbständig, nur mit den Hilfen angefertigt, die ich in der Dissertation angegeben habe und ohne unerlaubte fremde Hilfe. Alle Textstellen, die wörtlich oder sinngemäss aus veröffentlichten oder nicht veröffentlichten Schriften entnommen sind, und alle Angaben, die auf mündlichen Auskünften beruhen, sind als solche kenntlich gemacht. Bei den von mir durchgeführten und in der Dissertation erwähnten Untersuchungen habe ich die Grundsätze guter wissenschaftlicher Praxis, wie sie in der Satzung der Justus-Liebig-Universität Gießen zur Sicherung guter wissenschaftlicher Praxis niedergelegt sind, eingehalten.

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