Essays in Experimental Economics

Dissertation

zur Erlangung des akademischen Grades

Doctor Rerum Politicarum

An der

Fakultät für Wirtschafts- und Sozialwissenschaften

der Ruprecht-Karls-Universität Heidelberg

vorgelegt von

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Heidelberg

September 2018

Acknowledgments

This dissertation has profited, in various ways, from the feedback and support of several people.

First and most importantly, I would like to thank Christoph Vanberg for stimulating discussions, his invaluable feedback and constant guidance. His great insights, ideas and critical feedback led to tremendous improvements of all chapters and he has certainly shaped my views beyond this dissertation.

In addition, I would like to thank Stefan Trautmann who has served as a second referee for this dissertation. Likewise, I would like to thank Guillaume Frechette and Ariel Rubinstein for their encouragement and feedback during my research visit at the NYU Department of Economics.

My dearest thanks go to Johannes Lohse for being an enormously patient and inspiring co-author and for always having an open ear. I thank the faculty and my former colleagues at Heidelberg University for their valuable comments and for creating an atmosphere in which ideas can grow. You will be truly missed. My special thanks go to Freya Schadt who is the glue that holds everything together. Thanks for your warm words, Friday sweets and several birthday cakes.

I am grateful to my friends for their support, love, distraction in the right moments and for a lot of invaluable memories. You have made the time in Heidelberg exceptionally wonderful. I owe special thanks to Vanessa, Heny, Martin, Tom, Laura, Ole, Britta, Franzi, Nina and Gerrit.

Above all, I am indebted to my parents Helga and Max and my three wonderful sisters Charlotte, Paulina and Linda. Thanks for providing a loving home, for your honesty, your endurance and your patience. I would also like to thank Dieter and Gabi for having opened their hearts and home to me. Unfortunately, some important people are not around to see this dissertation being finished. Nevertheless, I thank Ruth, Hedwig, Eberhard and Hugo for being wonderful and loving grandparents.

Most importantly, I want to thank Matthias. Life is just so much better around you.

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Chapter 1

Introduction to the Essays

Experiments have a long tradition in the natural sciences, medicine and psychology. Pioneers such as Isaac Newton believed that experiments could provide important insights for theories. His view about experiments is best summarized in the following quote:

> *"The best and safest method of philosophizing seems to be first to inquire diligently into the properties of things, and establishing those properties by experiments, and then to proceed more slowly to hypotheses for the explanations of them."*
>
> – Isaac Newton

As emphasized in this quote, experiments are an appealing scientific method because they allow researchers to draw causal inferences. More specifically, an experiment typically holds all factors constant and introduces exogenous variation in a single variable, the "treatment". This controlled variation is crucial in order to attribute changes in the outcome of interest to the treatment (Croson and Gächter, 2010).

Experiments in the field of economics are primarily used to test theories. These theories provide abstract descriptions of social and economic phenomena, involving a set of individuals, their information and options of choice as well as the possible outcomes (Croson and Gächter, 2010). Based on behavioral assumptions (such as an assumption about how the individuals evaluate the outcomes), a theory predicts the choices that the individuals should make and the outcomes which would result from these choices. An experiment aimed at testing a theory usually implements its basic features (e.g. the possible choices and the information structure) and exogenously manipulates the factors which, based on the theory, should affect individuals' choices. Thereby, the experiment provides insights as to whether the observed causal effects are consistent or inconsistent with the effects predicted by a theory.

Naturally, experiments are not the only method to test theories. In fact, economic theories have traditionally been tested with data. However, "natural treatments" which would allow researchers to analyze causal relationships are rare outside the lab (Davis and Holt, 1993; Croson and Gächter, 2010). In the absence of naturally occurring exogenous variation, data can be useful in order to establish empirical associations between the variables of interest but it might be difficult to disentangle the exact cause and effect relationship predicted by a theory (Davis and Holt, 1993).

In addition to providing causal tests of theories which might be difficult to obtain with field data, experiments also allow researchers to discriminate between competing theories. Consider the case where two theories predict the very same social phenomenon (e.g. people contribute to public goods) but attribute such behavior to different motivations (e.g. they contribute because they are observed by others and feel obliged to or they are intrinsically motivated to contribute). Despite the fact that such motivations cannot be disentangled in the field, researchers might be able to identify situations in which the two theories make different behavioral predictions (e.g. manipulate the observability of one's contributions) and implement this situation a lab experiment. Thus, in these cases experiments can contribute to our understanding of the motivations underlying certain economic and social phenomena.

A non-obvious contribution of experiments is that they can provide important insights for the design of new theories by establishing empirical patterns through replication (Cassar and Friedman, 2004; Schmidt, 2009). If a replicable pattern is inconsistent with a single or even a collection of theories, new theories are required in order to explain this predictable deviation. Although this is certainly not the main purpose why researchers conduct experiments, several major advancements in Economics can be attributed to such a process. For example, as discussed by Croson and Gächter (2010), prospect theory was developed to explain deviations from expected utility theory. Similarly, theories of social preferences were introduced into economic models as an explanation for other-regarding behavior observed in a broad class of games (Andreoni, 1990; Fehr and Schmidt, 1999).

My above arguments show that there might be good reasons to apply experiments in order to test and distinguish between competing theories, especially in the absence of natural experiments outside the lab. However, experiments often abstract from important features of real-world interactions, such as communication or face-to-face interaction, and study behavior in stylized games. This abstraction implies that the results of an experiment cannot be extrapolated to the field (Levitt and List, 2007), unless "one wants to insist *a priori* that those aspects of behavior under study are perfectly general" (Harrison and List, 2004, p.1009). Hence, any lab experiment should be carefully motivated by an interest in the internal rather than the external validity of a theory. In contrast, an interest in the external validity of a theory might justify the use of experimental methods which impose less rigorous controls, such as field experiments (Harrison and List, 2004). Furthermore, recent developments in econometrics have provided researchers with a large spectrum of quasi-experimental methods, such as instrumental variables, regression discontinuity designs as well as the epidemiological approach. These methods allow researchers to isolate the causal effect of an intervention, even if the population affected by the intervention was not randomly selected (as in classical lab and field experiments). According to Angrist et al. (2013), these methods allow researchers to come as close as possible to the "experiment that could ideally be used to capture the causal effect" (Angrist et al., 2013, p.4), i.e. if a truly exogenous manipulation of the treatment variable were possible in the field. Given the trade-off between internal and external validity, these different experimental methods should be rather seen as complements, providing different perspectives on the same economic question, rather than being treated as substitutes.

Experimental methods have become increasingly popular since the 1960s. With the rise of game theory, social choice and voting theory, economists started to recognize the potential of experiments to causally test and distinguish between different theories (Cassar and Friedman, 2004). Furthermore, given that a single theory could generate multiple (in theory equally likely) equilibria, experiments were employed to study questions of equilibrium selection and coordination. In the late 1960s and early 1970s, several experiments designed as tests of expected utility theory (Slovic and Lichtenstein, 1971; Lichtenstein and Slovic, 1971; Lindman, 1971), oligopoly bargaining theory (Sauermann and Selten, 1960; Siegel and Fouraker, 1960; Friedman, 1963; Malouf and Roth, 1981) and general equilibrium theory (Smith, 1962, 1964) were published in economics and psychology journals. Furthermore, advancements in experimental methods were published in top journals (Chamberlin, 1948; Smith, 1976; Samuelson, 2005; Levitt and List, 2007) which made them available to a broad audience. These developments have facilitated the application of economic experiments to an ever growing range of topics (see Kagel and Roth (2016) for an overview).

This introduction into experimental economics illustrates that experiments are powerful tools in order to causally test economic theories, distinguish between competing theories or establish empirical patterns, especially in the absence of natural treatments and when the research question justifies a high interest in the internal validity of a theory. The four chapters included in this dissertation all apply experimental methods to study fundamental questions about economic behavior. Each chapter is motivated by testing specific economic theories and the results provide important insights for the further development of these theories.

While all chapters apply a common method, they contribute to different fields within economics. Namely, chapters 2 and 3 relate to a recent literature in Behavioral Economics, which analyzes the role of heuristics and biases in social choices. Chapters 4 and 5, in contrast, contribute to the literature on Public Choice, by analyzing how the decision rule affects outcomes and the efficiency of agreements in multilateral bargaining. The remainder of this introduction provides a brief overview of the related literature and outlines the contributions of each chapter. In addition, I provide a brief summary of each chapter.

Behavioral Economics

The objective of Behavioral Economics is to integrate insights from psychology and social sciences into economic models. The aspiration is that more "realistic" assumptions about human behavior generate new insights, allow economists to make better predictions for field behavior and design better policies (Camerer et al., 2011). For this purpose, behavioral economic models relax two assumptions which are at the heart of almost all neoclassical theories: Rationality and self-interest.

Rationality assumes that decision makers integrate all available information, perfectly calculate probabilities and make choices based on a careful assessment of the benefits and costs. Several well-established findings in the Behavioral Economics literature are inconsistent with this notion. Among them are anchoring (Tversky and Kahneman, 1974; Ariely et al., 2003), loss aversion (Tversky and Kahneman, 1991), reference-dependence (Thaler, 1980; Tversky and Kahneman, 1991) and time-inconsistency (Thaler, 1981).

In their seminal research, Amos Tversky and Daniel Kahneman suggest that when decision makers need to solve complex tasks, such as assessing the future value of a dollar or the price they should pay for a house, they rely heavily on heuristics or rules of thumbs (Tversky and Kahneman, 1974, 1981, 1991; Kahneman, 2003). The use of such heuristics leads to predictable biases and choices which are not consistent with rationality.

A separate strand in the Behavioral Economics literature studies the assumption that decision makers maximize their own monetary benefits, independent of how their choices affect the wealth of others. This assumption applies specifically to non-repeated contexts in which the decision maker has no incentive to establish a good reputation. Given that such contexts are rare in the field, researchers have studied this prediction in stylized one-shot games.

The evidence collected from these games is clearly inconsistent with selfish behavior. Namely, most people cooperate with strangers, even if cooperation is a dominated action for a selfish decision maker (Zelmer, 2003). In addition, they pass funds to strangers in trust games, even at the risk of being exploited (Johnson and Mislin, 2011). Furthermore, evidence from dictator games suggest that most people share a significant fraction of their endowment with an anonymous recipient (Engel, 2011).

Several theories have explained this evidence by introducing the concept of social preferences. These theories assume that a decision maker's utility depends on his own as well as on the wealth of others. Two decades of research show that the motivations for social behavior are manifold. Some of the most important concepts in this literature are the following: People dislike inequality in outcomes (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000) and they make choices which increase the social welfare even at own costs (Charness and Rabin, 2002). In addition, they reciprocate fair behavior (Fehr and Gächter, 2000b) and punish selfish behavior of others (Fehr and Gächter, 2000a; Fehr and Fischbacher, 2003). Whenever the link between own actions and outcomes is reduced, they tend to be more selfish (Dana et al., 2007; Fudenberg et al., 2012). Hence, pro-social choices depend on an evaluation of outcomes, the intentions of others as well as the observability of pro-social behavior.

While pro-social choices have traditionally been linked to preferences, a recent literature, inspired by research in psychology, analyzes the role of biases in explaining these choices. This literature studies the emotional and mental processes involved in pro-social choices and attempts to identify how cognitive limits affect these choices. Two of the papers in this dissertation contribute to this recent literature. Chapter 2 (joint with Johannes Lohse) analyzes the claim that pro-social behavior in one-shot games reflects a cooperation heuristic which is highly adapted for repeated interactions outside the lab (Rand et al., 2012). Hence, this literature suggests that a decision maker which decides based on a quick first intuition chooses differently than a decision maker which carefully analyzes the benefits and costs of pro-social choices. Other researchers argue that behavior in these game reflects a single mental process in which decision makers resolve the conflict between selfish and other-regarding motives (Fehr and Camerer, 2007). To distinguish between the two theories, we use a novel experiment with time pressure and time delay manipulations. Our experiment provides clear evidence against the hypothesis that fair behavior in one-shot games can be linked to intuitive biases. Instead, our results suggest that decisions in these games reflect heterogeneous social preferences. This chapter has been accepted for publication and is forthcoming in Experimental Economics.

Chapter 3 analyzes whether donors give more to single identified as compared to groups of statistical recipients. This observation is commonly referred to as the "identifiable victim effect". Researchers have proposed two explanations for the "identifiable victim effect": A bias towards identified as compared to unidentified recipients, based on the assumption that donors experience greater emotional arousal when they can identify the recipient of their donation, as well as a preference for more concentrated divisions of the donation, based on a bias favoring interventions with a greater proportional impact. I design a choice task allowing me to test how both explanations contribute to the "identifiable victim effect". In contrast to previous studies, I control for the information that donors have about recipients in all treatments. The results show that subjects do not donate more simply because they can identify the recipient of their donation. Furthermore, I find that subjects donate more to groups than to single recipients, suggesting that there is no general bias favoring single identified individuals.

In the following, I will provide a more detailed summary of each chapter.

Summary of Chapter 2

The first paper **"Is fairness intuitive? Accounting for subjective utility differences under time pressure"** (*joint with Johannes Lohse*) provides a new test for the hypothesis that "fairness is intuitive" (FII). According to this hypothesis, people are intuitively predisposed to make fair choices, such as choosing to contribute in public good games or sharing their endowment in dictator games. Deliberation can, however, override the impulse to be fair, thus leading to more selfish choices. A large number of papers have tested the FII hypothesis using response time data, i.e. the time it takes before a decision maker enters her choice. Studies which causally test the FII hypothesis, typically compare the choices of subjects who are placed under time pressure, thereby forced to make a quick and intuitive choice, with choices of subjects who are constrained to wait before making a choice. In light of conflicting empirical evidence, we conduct a new test in which we address the concern that response times may be affected by the subjective choice difficulty. We explore how choice difficulty affects decisions under time pressure and time delay and derive conditions under which an increase in fair choices under time pressure can be unambiguously attributed to the FII hypothesis.

We use a simple version of the Drift Diffusion Model (DDM) to show that time pressure increases the frequency of mistakes, especially among decision makers who perceive smaller utility differences between the options of choice. This implies that time pressure can increase the fraction of fair choices relative to time delay, if fair decision makers perceive larger utility differences than selfish decision makers and are less common in the population. In turn, if selfish decision makers perceive larger utility differences than fair decision makers and are less common in the population, time pressure should decrease the fraction of fair choices relative to time delay. The FII hypothesis, on the other hand, predicts that time pressure should always increase the fraction of fair choices. Hence, depending on the type of choice situation, the two theories either predict the same (type I decisions) or opposite effects (type II decisions) which may even cancel each other out. We show that classifying a choice situation into one of these two types is essential in order to correctly interpret the evidence, as the FII hypothesis might be spuriously accepted (if the DDM and the FII hypothesis make the same prediction) or rejected (if the DDM and the FII hypothesis predict opposite effects which may even cancel each other out).

In order to test our considerations, we conduct experiments in which subjects take decisions in two-person binary dictator and prisoner's dilemma games. Across games, we vary the subjective attractiveness of the fair option by increasing the benefits of fair behavior. In particular, our experiment includes choice situations in which we expect that decision makers who prefer the fair option will find it subjectively more, less or as difficult to choose as decision makers who prefer the selfish option such that the DDM and the FII make either consistent or opposite predictions concerning the effect of time pressure. To classify choice situations into one of these two types, we implement an additional treatment in which we observe response time correlations and choice frequencies. Based on a recent paper by Krajbich et al. (2015a), we should find that fair choices are correlated with shorter response times in decision problems in which fair choices are subjectively less difficult than selfish choices and vice versa.

Overall, our analysis provides limited support for the FII hypothesis. In binary dictator and prisoner's dilemma games in which both, the DDM and the FII hypothesis predict that time pressure should increase the fraction of fair choices, we do not observe that time pressure leads to significantly more fair choices as compared to time delay. Similarly, we do not observe that time pressure increases the fraction of fair choices in games, where this increase would constitute unambiguous evidence in favor of the FII hypothesis. In a complementary analysis, we compare the choices of the same decision maker in a given game under time pressure and time delay. Our analysis shows that the observed switching patterns strongly reflect choice difficulty (i.e. indifference). While this pattern is predicted by the DDM, it is inconsistent with the FII hypothesis.

Summary of chapter 3

The third chapter **"Do people prefer donating to identified individuals?"** analyzes behavioral explanations for the observation that people make larger donations to single identified as compared to groups of statistical recipients, commonly referred to as the "identifiable victim effect". Researchers have proposed two explanations for this effect: Identifiability and a preference for more concentrated divisions of the donations.

Papers which study the role of identifiability on donations typically compare donations to a single recipient identified by photo and / or individuating information with donations to an otherwise anonymous recipient. A common finding in these papers is that identified recipients receive larger donations than anonymous recipients. One explanation for this observation is that donors give more to identified recipients in response to the fact that they have better information about them. Another explanation is that donors give more when they can identify the recipient of their donation, for example based on feeling more empathy towards an identified relative to an anonymous recipient. If the latter explanation is true, we would expect that a donor always prefers to observe the recipient of her donation, even if she has the same kind of objective information about all potential recipients. To test for such a *mere effect of identifiability*, I design a new experiment in which I control the amount of information that donors have and exogenously vary whether subjects can observe the recipient of their donation.

For this purpose, I run laboratory experiments in which subjects can make donations to finance school attendance and lunches for children in Uganda. Each subject is matched to a group of three children. A picture of each child is displayed prior to the first donation decision such that subjects have the same kind of objective information about all potential recipients. Subjects take multiple donation decisions. Across these decisions, I vary whether subjects know which child will receive their donation (such that the recipient is identified) or whether they only know that one of the three children will receive the donation (such that the recipient is unidentified).

In a separate treatment, I analyze another common explanation for the "identifiable victim effect". Namely, several researchers have found evidence consistent with a preference for more concentrated divisions of the donation. Such a preference could explain why people prefer to donate to single identified individuals instead of donating to a large scale intervention involving multiple recipients. To analyze how the distribution affects donation decisions, I run a second treatment in which I vary whether a subject's donation is disbursed to a single identified child or whether it is equally disbursed among the three identified children.

Hence, my experiment allows me to analyze how both behavioral explanations contribute to the “identifiable victim effect” in a comparable choice task.

I find that subjects in the first treatment do not give more when they can observe the recipient as compared to cases in which they cannot observe the recipient of their donation. Hence, this evidence is inconsistent with the hypothesis that the mere possibility of being able to identify the recipient increases donations. This result is important for the interpretation of previous studies. Namely, given that identification of the recipient has no effect, the observed differences in donations to vividly identified versus anonymous recipients can be clearly attributed to the fact that donors give more when they have more information about the recipients.

In contrast to previous studies, I find that subjects in the second treatment give less to a single identified recipient than to the group with three identified recipients. Hence, this evidence is not consistent with a preference for a more concentrated distribution of the donation. Despite the fact that I used smaller group sizes as compared to previous studies, I conclude that this result provides evidence that there is no general preference for concentrated distributions which is independent of the concrete details of the choice situation (such as the group size and the endowment).

Public Choice

Public Choice uses the tools of economic theory to study political behavior. The subjects studied include voting behavior, party politics, bureaucracy, constitutions, influence groups as well as collective choice. Models within the Public Choice literature are built on the same behavioral assumptions as neoclassical economic models, i.e. rational and self-interested agents, whose interactions are studied using game theory and decision theory.

A central question within the Public Choice literature is which decision rule should be used in collective bargaining. Early contributions, mostly from the Social Choice Literature, have studied this question from a normative perspective. Several of these theories are built on the assumption that individual preferences can be aggregated to obtain a social “will”, depicted in the form of a social welfare function (Bergson, 1938; Samuelson, 1948). In reaction to this literature, Arrow (1951) showed that no voting system can transform the individual preferences of a group into a complete and transitive social ranking without violating a set of basic normative requirements such as Pareto efficiency and non-dictatorship. His seminal analysis has entered the literature as the “Impossibility Theorem”.

From a normative perspective, Arrow’s Impossibility Theorem proofs that there is no acceptable method to attribute rational preferences to a group of individuals with different interests and that any concept of Social Welfare naturally violates some democratic principles. This has spurred a literature which analyzes the properties of different concepts of Social Welfare and discusses which of the normative criteria defined by Arrow should be fulfilled. An overview of this literature is provided by Sen (1977, 2017).

From a positive perspective, the Impossibility Theorem shows that political decisions may depend strongly on the applied rules. In particular, certain rules may lead to cycles in the political process such that earlier decisions are overruled. This interpretation naturally gives rise to the question how the institutional features and rules affect the outcomes of collective choice (Mueller, 2003). Since the 1970s, several authors have studied this challenging question.

Although real world decision rules can be quite complex, a number of authors have focused on analyzing the properties of different q Majority rules, with q representing the number of individuals which need to consent for a decision to be implemented. Some of the papers in this literature study the effect of decision rules in collective decisions involving a common interest. An example for this kind of situation is jury decision making, where all jurors would want to convict a guilty and acquit an innocent defendant. In this literature, different opinions are seen to reflect the fact that jurors have different information (or at least interpret the objective facts differently). Therefore, the question that this literature poses is which decision rule best aggregates the information of the individual committee members, such that the probability of selecting the "right" outcome is maximized.

The literature on common interest bargaining dates back to Condorcet's "Jury Theorem". In this theorem, Condorcet (1785) argues that a committee is more likely to make the "right" decision under majority rule than each committee member individually. An important assumption in Condorcet's Theorem is that all members vote sincerely. In contrast to this assumption, more recent theories assume that committee members vote strategically, i.e. condition their vote on the event in which they are pivotal (Austen-Smith and Banks, 1996; Feddersen and Pesendorfer, 1996). These theories provide interesting new insights into individual voting behavior which can, for example, provide an explanation for abstention in large scale elections (Feddersen and Pesendorfer, 1996; Robinson and Torvik, 2009; Battaglini et al., 2010). Furthermore, these theories show that majority rule is more efficient in selecting the right outcome than unanimity rule. This theoretical result is interesting given that real world decisions involving common interest, such as jury decisions in the U.S. court system, usually involve unanimity rule. A recent experiment by Goeree and Yariv (2011), however, shows that as soon as the members are allowed to communicate, the predicted efficiency differences between unanimity and majority rule vanish. Hence, this experimental result shows that the theoretically sound differences between different rules might disappear once members can discuss and share their perspective on the interpretation of the facts.

A separate strand, more related to the research presented in this dissertation, studies the role of decision rules in situations with misaligned preferences. Hence, in this literature individuals are seen to have different preferences over the potential outcomes or about whether a joint project should be conducted at all. The key question in this literature is how conflicts are handled under different decision rules. Other equally challenging questions are how the differences in outcomes that individuals could expect under the different rules affect their incentives to engage in joint activities or which decision rule they would preferably apply in situations with misaligned preferences.

In their book “The calculus of consent”, Buchanan and Tullock (1962) provide a seminal analysis of decision rules. They argue that when choosing between different q majority rules, individuals face a tradeoff between “external” and “decision” costs: On the one hand, more inclusive rules which require a larger number of individuals to consent, decrease the probability of being harmed by a decision. These “external” costs are minimized under unanimity rule where each committee member is endowed with veto power, allowing her to protect her interests. On the contrary, the costs of reaching an agreement increase as more individuals are required to consent. Buchanan and Tullock (1962) argue that rational individuals would balance both costs behind the veil of ignorance and thus favor less-than-unanimity rules.

Several recent experimental studies have analyzed this tradeoff by conducting multilateral bargaining experiments. The evidence reported in these studies is consistent with Buchanan and Tullock’s arguments. Namely, outcomes under unanimity rule tend to be more inclusive and involve a more equal division of the surplus as compared to majority rule. Possibly anticipating these effects, people are less likely to make contributions to projects whose proceeds will be distributed by majority instead of unanimity rule (Baranski, 2016). While this might make unanimity rule more desirable than majority rule, groups usually need more time in order to negotiate agreements when unanimity rule is used. This delay results in less efficient agreements. Related research in behavioral economics shows that when individuals face trade-offs between equal and efficient outcomes, they tend to choose efficient outcomes (Charness and Rabin, 2002). If this evaluation extends to collective decisions, unanimity rule might actually be less desirable than majority rule.

The two last chapters of this dissertation contribute to this challenging topic. Previous evidence has compared the outcomes of collective bargaining under majority and unanimity rule in experiments where subjects bargain over an exogenous surplus. A key result in these studies is that outcomes under unanimity rule distribute the funds relatively equal among the committee members, while most collectively agreed outcomes under majority rule constitute minimum winning coalitions, in which only a majority of the group members receives a positive share of the funds. Chapter 4 (joint with Christoph Vanberg) studies whether we continue to observe this difference in outcomes if the surplus to be divided result from joint production. In particular, we study whether subjects indeed form minimum winning coalitions, in which the most productive members are excluded. If so, majority rule might discourage individuals from exerting effort in joint projects whose proceeds will be distributed based on majority rule. In sharp contrast to previous experiments, we find that outcomes under both decision rules constitute convex combinations of the equal and the proportional split. Most notably, we see few minimum winning coalitions being proposed. This suggests that fairness perceptions strongly influence the outcomes in these situations, even in the absence of veto power.

While Chapter 4 focuses on the outcomes under unanimity and majority rule, Chapter 5 (joint with Christoph Vanberg) studies how the decision rule affects the decision costs. A measure of decision costs used in previous multilateral bargaining experiments is delay, i.e. the number of formal proposals which are made before a group reaches an agreement. However, delay in real world legislative bargaining is usually caused by lengthy discussions prior to a formal vote. We therefore study the effect of decision rules in a novel experiment, in which communication itself is costly. Our results show that groups communicate significantly longer under unanimity as compared to majority rule. The difference in communication length is especially large when players are asymmetric to some degree given that communication is required in order to select one among multiple possible outcomes.

In the following, I provide a more detailed summary of Chapters 4 and 5.

Summary of Chapter 4

The third chapter **"Legislative Bargaining with Joint Production: An experimental Study"** (*joint with Christoph Vanberg*) examines bargaining games in which the surplus being divided results from joint production. Such negotiations are likely to be complicated given that group members might disagree about the degree of proportionality that should prevail. The main question we address in this paper, is how such disagreements are handled under different decision rules. Our analysis focuses on the comparison between unanimity and simple majority rule as polar cases of a continuum of q-majority rules being used in practice. The comparison between these two rules is interesting given that unanimity rule allows each party to protect her interests by exerting veto power. Although protection of interests may be deemed as a desirable feature, endowing each party with veto power creates incentives to withhold agreement. In contrast, majority rule allows for the formation of coalitions in which only a majority of individuals is included. The threat of being excluded from such coalitions, therefore, creates incentives to compromise.

Several recent papers have analyzed unanimity bargaining with joint production. In line with the theoretical arguments discussed above, they show that group members use their veto power to enforce proportional outcomes and their subjective fairness perceptions. It is, however, unclear whether the same outcome should prevail under majority rule where groups can form minimum winning coalitions. Several previous studies on majority bargaining over an exogenous surplus show that a majority of games indeed end in such agreements (Diermeier and Morton, 2005; Fréchette et al., 2005a,b; Agranov and Tergiman, 2017; Miller and Vanberg, 2013, 2015). Hence, an important question is whether we continue to observe coalitions being proposed if the surplus is being jointly produced. Especially if members have made different contributions, a crucial question is whether such coalitions include members with higher or those with lower contributions more often.

We experimentally investigate these questions by conducting 3-person Baron and Ferejohn (1989) bargaining games. In this game, individuals take turns in proposing an allocation of the surplus which is voted on. In contrast to previous studies, each subject completes a task prior to bargaining and individually earns points, the sum of which constitutes the bargaining surplus. Across treatments, we vary whether a proposal passes by majority (2 members vote yes) or unanimity rule (all three members vote yes). In addition, we observe several endogenously determined situations in which the group members either contributed the same or different amounts of points to the bargaining surplus. We investigate how the contributions affect proposals, voting behavior and final outcomes under both decision rules.

Our main result is that under both decision rules, outcomes and bargaining behavior are significantly affected by contributions. Most notably, when unanimity rule is used, all proposals and outcomes constitute convex combinations of the three-way equal split and the split that is exactly proportional to relative contributions. This result is consistent with previous evidence from bilateral bargaining and multilateral bargaining with unanimity rule. More surprisingly, we observe a very similar pattern under majority rule. In particular, a large majority of proposals allocates positive shares of the surplus to all group members. This results stands in contrast to previous (comparable) bargaining experiments in which the surplus to be divided is exogenous and where most subjects propose minimum winning coalitions. Our findings further suggest that players who have made relatively smaller contributions tend to make more equal (i.e. less proportional) proposals under both decision rules. This pattern is, however, more pronounced under majority rule. Finally, we observe that majority rule leads to a higher passage rate than unanimity rule, especially when group members have made different contributions to the surplus.

Summary of Chapter 5

In the fourth paper titled **"Legislative bargaining with costly communication"** (*joint with Christoph Vanberg*) we ask whether unanimity rule is associated with more delay than majority rule. This question is important given that decision rules which result in more delay may be considered less efficient. Several previous papers have investigated the link between delay and the decision rule in multilateral bargaining experiments. In these studies, delay is measured as the probability that a proposal fails given that failure causes the bargaining surplus to be discounted. All studies find that unanimity rule is associated with a higher proposal failure rate (Miller and Vanberg, 2013; Agranov and Tergiman, 2014; Miller and Vanberg, 2015; Agranov and Tergiman, 2017). A few recent studies, including the evidence provided in the forth chapter of this dissertation, suggest that the difference in proposal failure rates is especially pronounced if players are heterogeneous to some degree (Miller et al., 2018).

Conflicting evidence is provided by Agranov and Tergiman (2014, 2017) who find that allowing for pre-play communication virtually eliminates delay in the form of proposals failing under both decision rules. While observational data is consistent with their observation, e.g. few formal proposals are voted down in the legislative process, delay typically manifests itself in the length of informal bargaining prior to a formal vote. For this purpose, we re-investigate the link between the length of informal discussions and the decision rule in a new experiment, where communication itself is costly.

To answer our research question, we use a modified version of the Baron and Ferejohn (1989) bargaining game as introduced by Miller et al. (2018). In this game, groups of three subjects bargain over the division of a fixed surplus. Bargaining proceeds over discrete rounds and one of the three group members is selected to make a proposal in each round. Prior to introducing and voting on a formal proposal, players can discuss via a chat window. The proposer decides when to end informal negotiations and make a formal proposal. At this point, the game ends with a probability that depends on the length of informal negotiations. Namely, every two seconds of communication increase the probability of breakdown by one percent. In the event of breakdown, the surplus is lost and each player receives a predetermined disagreement value. Our main treatment variable is the number of group members which are required for a proposal to pass (majority or unanimity). Across games, we additionally vary whether players receive the same or different disagreement values, allowing us to study whether the length of informal negotiations depends on fundamental asymmetries between the players.

Our main findings are the following. When all players have the same disagreement values, the decision rule has virtually no impact on the total communication time. Almost all groups immediately agree on equal splits (two- or three-way) without communicating. In contrast, when players have asymmetric disagreement values, unanimity rule is associated with significantly longer total communication time than majority rule. Our analysis shows that outcomes are significantly more variable in these situations, reflecting disagreement about which outcome to implement. A more detailed analysis of the bargaining structure shows that unanimity rule leads to more communication per round and more proposals failing. Both factors lead to overall longer communication time and higher breakdown frequencies. Our results confirm that unanimity rule is associated with longer communication in order to reach agreement, especially in the absence of focal solutions.

Chapter 2

Is fairness intuitive? Accounting for the role of subjective utility differences under time pressure[1]

Abstract
Evidence from response time studies and time pressure experiments has led several authors to conclude that "fairness is intuitive". In light of conflicting findings we provide theoretical arguments showing under which conditions an increase in "fairness" due to time pressure indeed provides unambiguous evidence in favor of the "fairness is intuitive" hypothesis. Drawing on recent applications of the Drift Diffusion Model (Krajbich et al., 2015a), we demonstrate how the subjective difficulty of making a choice affects decisions under time pressure and time delay, thereby making an unambiguous interpretation of time pressure effects contingent on the choice situation. To explore our theoretical considerations and to retest the "fairness is intuitive" hypothesis, we analyze choices in two-person binary dictator and prisoner's dilemma games under time pressure or time delay. In addition, we manipulate the subjective difficulty of choosing the fair relative to the selfish option. Our main finding is that time pressure does not consistently promote fairness in situations where this would be predicted after accounting for choice difficulty. Hence, our results cast doubt on the hypothesis that "fairness is intuitive".

Keywords: distributional preferences, cooperation, time pressure, response times, cognitive processes, drift diffusion models

JEL Classification: C32, C72, C91, D91, H41

[1]This chapter corresponds to an article published jointly with Johannes Lohse in Experimental Economics, https://doi.org/10.1007/s10683-018-9566-3

2.1 Introduction

Economists are increasingly interested in understanding the cognitive (Alós-Ferrer and Strack, 2014) and emotional (Loewenstein, 2000; Hopfensitz and Reuben, 2009; Drouvelis and Grosskopf, 2016) processes that drive pro-social behavior. One of the central questions within this literature is whether "fairness" is intuitive and automatic or follows from a deliberative weighting of the costs and benefits of making a fair choice. Several authors have approached this question by analyzing response times as a proxy for deliberation (Rubinstein, 2007; Spiliopoulos and Ortmann, 2017). A popular method for understanding the causal impact of deliberation on choices is to place subjects under time pressure or time delay, given that subjects who are constrained to make a fast choice might increase their reliance on intuition compared to subjects who are constrained to wait before making a choice (Wright, 1974; Rand et al., 2012).

Using this method Rand et al. (2012, 2014) find that average contributions in a public good game are higher when subjects are placed under time pressure as compared to subjects who are forced to delay their contribution decision. These results have inspired the "fairness is intuitive" (FII) (Cappelen et al., 2016) hypothesis. According to the FII hypothesis, a decision maker intuitively prefers fairness, i.e. cooperation in a public good or sharing resources in a dictator game.[2] However, this predisposition towards fairness can be overridden by a more deliberative weighting of the costs and benefits, such that deliberation can promote selfishness (Rand et al., 2012).

The FII hypothesis has not been unequivocally confirmed empirically. In contrast to the original results of Rand et al. (2012), Tinghög et al. (2013), Verkoeijen and Bouwmeester (2014), and Bouwmeester et al. (2017) do not find that constraining deliberation by time pressure increases the fraction of cooperative choices in one-shot public good games. Furthermore, Tinghög et al. (2016) find that time pressure does not affect the fraction of fair choices in (modified) dictator games. Finally, findings in Capraro and Cococcioni (2016) and Lohse (2016) suggest that placing subjects under stronger time pressure leads to more selfish choices in public good games. Similarly, Mrkva (2017) finds that time pressure leads to an increase of selfish choices in modified dictator games with high stakes, but not with low stakes.

In light of this mixed evidence, we conduct a new test of the FII hypothesis. In this test, we address a recent concern that factors other than intuition and deliberation also affect response times and thereby distort the identification of intuitive or deliberative

[2] Obviously, the economics literature has come up with various notions of what constitutes a "fair" choice (Rabin, 1993; Engelmann and Strobel, 2004; Fehr and Schmidt, 2006). In section 2.2, we will describe in more detail what we refer to as a "fair" choice in the context of our paper.

choices from fast and slow responses (Recalde et al., 2014; Krajbich et al., 2015a). We explore how the subjective difficulty of choosing between a fair and a selfish option, as one such factor, affects choices under time pressure and time delay. Our theoretical predictions highlight that, without controlling for the effect of choice difficulty in a given situation, observing a positive effect of time pressure is not necessarily evidence in favor of the FII hypothesis; and observing no effect of time pressure is not necessarily evidence against the FII hypothesis. Thereby, we provide one plausible explanation why previous tests of the FII hypothesis might have come to different conclusions.

Our theoretical considerations are based on insights from a recent paper by Krajbich et al. (2015a) who use a Drift Diffusion Model (DDM) to illustrate how choice difficulty may affect response times. The central prediction of the DDM is that more difficult choices, i.e. those in which the utility difference between the fair and the selfish option are small, are associated with longer response times. We build on this insight and explore how the subjective difficulty of making a choice affects a *causal* test of the FII hypothesis. Our analysis is based on the assumption that choices under time pressure may be affected by both, the amount of deliberation involved in the choice and the subjective difficulty of making a choice (Alós-Ferrer, 2016). Hence, the overall effect depends on how time pressure affects choices according to the FII hypothesis *as well as* the DDM.

We use a simple version of the DDM to show that time pressure causes decision makers who perceive smaller utility differences to make more mistakes. Thus, the DDM predicts that time pressure can either increase the fraction of fair choices, if fair decision makers perceive larger utility differences and are less common in the population; or decrease the fraction of fair choices, if selfish decision makers perceive larger utility differences and are less common in the population. The mechanism motivating the FII hypothesis on the other hand predicts that time pressure should always increase the fraction of fair choices in one-shot games. Hence, whenever fair decision makers perceive larger utility differences than selfish decision makers and are less common in the population, the DDM and the FII both predict that time pressure should increase the fraction of fair choices. Observing a positive effect of time pressure in these situations can therefore only provide ambiguous evidence in favor of the FII hypothesis as the same pattern could also be explained by the DDM, while observing no effect is unambiguous evidence against the claim that "fairness is intuitive". On the other hand, whenever selfish decision makers perceive larger utility differences than fair decision makers and are less common in the population, the FII hypothesis and the DDM predict opposite effects of time pressure, which may even cancel each other out. Observing no or even a negative effect of time pressure in these situations is not sufficient to unambiguously reject the claim that "fairness is intuitive", while observing a positive effect provides unambiguous evidence in favor of the FII hypothesis. These arguments illustrate that classifying a choice situation into one of these types is central for the correct interpretation of time pressure effects. Otherwise the FII hypothesis could be spuriously accepted or rejected. The fact that previous studies have not explicitly accounted for subjective utility differences might explain why they have arrived at different conclusions.

To causally test the FII hypothesis, we conduct an experiment in which subjects take decisions under time pressure or time delay in multiple two-person binary dictator and prisoner's dilemma games. Across games, we vary the subjective attractiveness of the fair option by increasing the social benefits of fair behavior. Specifically, our experiment includes choice situations in which we expect that decision makers who prefer the fair option will find it subjectively more, less or as difficult to choose as decision makers who prefer the selfish option such that the DDM and the FII make either consistent or opposite predictions concerning the effect of time pressure. To classify choice situations into one of these two possible types, we use an additional treatment, in which subjects are unconstrained in their response time and in which we observe response time correlations and choice frequencies. According to Krajbich et al. (2015a), we should find that fair choices are correlated with shorter response times in decision problems in which fair choices are subjectively less difficult than selfish choices and vice versa.

Our experiment comprises two further elements: first, it allows for a between- as well as a within-subject test of the FII hypothesis. Within-subject evidence is obtained by letting subjects take the same decision twice in each game, once under time pressure and thereafter under time delay. Second, by comparing evidence from binary dictator and prisoner's dilemma games, we are able to distinguish between fair choices in non-strategic and strategic decisions. Thereby, we investigate whether pro-social behavior follows a common cognitive pattern across different contexts. While several previous tests of the FII hypothesis are based on evidence from strategic decisions in public good or prisoner's dilemma games, non-strategic decisions in simple binary dictator games might allow for a more direct test given that they are unconfounded by strategic uncertainty or misconceptions regarding the game.

Overall, our analysis provides at most limited empirical support for the hypothesis that "fairness is intuitive". In those binary dictator and prisoner's dilemma games, in which our classification suggests that time pressure should increase fairness according to both models, we do not observe such increase across all between-subjects tests. In the same games, there is no consistent within-subjects evidence that subjects who choose the fair option under time pressure are more likely to switch to the selfish option under time delay. In binary dictator games in which an increase of fair behavior under time pressure would constitute unambiguous evidence in favor of the FII hypothesis we do not find that time pressure significantly increases the frequency of fair choices. This evidence holds between- and within-subjects. A complementary analysis shows that switching patterns strongly reflect choice difficulty (subjective indifference), a pattern that is supported by the DDM but not by the FII hypothesis.

The remainder of the paper is organized as follows: section 2.2 contains a detailed description of the DDM and summarizes our predictions. In section 2.3, we explain our experimental design. The results are summarized in Section 2.4. In section 2.5, we conclude with a short discussion of our results.

2.2 Theory and Predictions

The FII hypothesis is based on a dual-process framework in which decisions are jointly determined by a fast and intuitive system I and a more deliberative and rather slow system II (Kahneman, 2003; Frederick, 2005). According to the "Social Heuristics Hypothesis" (Rand et al., 2014), the intuitive system I follows a cooperation heuristic that individuals have developed in repeated everyday interactions. Upon deliberation, the same individuals may realize that there are no strategic incentives to cooperate in atypical one-shot situations implemented in the lab which leads to more defection. Cooperation is the most prominent application of the "Social Heuristics Hypothesis". Its underlying mechanism could, however, apply more broadly to non-strategic choices in the dictator game assuming that sharing resources with other people is also an advantageous long-term strategy because of reciprocity or reputation concerns. We summarize the claim that intuition promotes fairness across different contexts as the FII hypothesis.

The FII hypothesis generates empirically testable predictions concerning the effect of time pressure and time delay on fairness. Since heuristics are seen to operate relatively independently from the details of a choice situation, the FII hypothesis predicts that the same decision maker is more likely to choose the fair option when placed under time pressure than when she makes a deliberative choice. Similarly, when observing choices of different decision makers, subjects who are placed under time pressure should on average choose the fair option more frequently than subjects constrained to wait before making a choice.

However, the observation that time pressure leads the same decision maker to choose the fair option with higher probability or that time pressure increases the fraction of fair choices cannot be unambiguously interpreted as evidence in favor of the FII hypothesis without accounting for choice difficulty. To illustrate how the subjective difficulty of the choice situation could affect choices under time pressure and thereby confound a test of the FII hypothesis, we describe the DDM in more detail.[3]

2.2.1 Time Pressure in the Drift Diffusion Model (DDM)

Assume that a single decision maker faces a binary choice between a "fair" (F) and a "selfish" (S) option. According to the DDM, this decision maker is initially unaware of the utility value she receives from these options. However, she can accumulate stochastic information regarding her preferences in a series of time periods t. In each t, the decision maker observes two new stochastic value signals F_t and S_t which are normally distributed around her true underlying utility values. The difference between the two signals $F_t - S_t$, is added to a subjective state variable X_t^i which, thus, encodes the probability that F yields a higher utility than S (Krajbich et al., 2014; Caplin and Martin, 2015). The accumulation process continues until the subjective state variable crosses a pre-defined upper threshold a, inducing the decision maker to choose F, or the lower threshold b, inducing the decision maker to choose S. The length of the accumulation process, i.e. the number of time periods before the upper or lower threshold is reached, corresponds to the decision maker's response time.

The standard DDM makes two predictions regarding the theoretical distribution of response times and decision errors (e.g., Ratcliff and Rouder, 1998).[4] First, the decision maker's response time depends on the underlying absolute utility difference, $|u^i(F) - u^i(S)|$. If this difference is large, the decision maker is expected to decide faster than if the underlying absolute utility difference is small because she has to sample fewer signals to reach one of the thresholds. Second, given that the final decision is reached by observing a series of noisy signals, the decision maker is more likely to make a mistake (i.e. to choose the option that she does not prefer given her own preferences), the smaller the underlying utility difference between the two options. A small utility difference between the fair and the selfish option implies that the decision maker is more likely to receive signals that contradict her "true" preference. This, in turn,

[3] We will refer to versions of the DDM that have recently been applied to value-based choices and social dilemma situations (Polanía et al., 2014; Krajbich et al., 2014, 2015b). For a more extensive review of the behavioral foundations and the application of DDM in psychology refer to the descriptions in Ratcliff (1988), Ratcliff and Rouder (1998) and a recent summary of this topic aimed at economists by Clithero (2016).

[4] A more detailed description of the DDM as well as proofs and derivations of all predictions are contained in Appendix A.

increases the likelihood of making a mistake by choosing the non-preferred option.

Jointly, these two properties of the DDM generate a third prediction concerning the effect of time pressure and time delay on choices. Time pressure forces decision makers with otherwise longer response times to make a choice before being sufficiently sure about their truly preferred option. Thus, time pressure is equivalent to a reduction in the decision thresholds. This induces decision makers to choose at lower precision because noise has a higher likelihood of influencing their decision. Importantly, the likelihood of making a mistake is larger for decision makers with smaller absolute utility differences because their value signals are less informative.

Aggregating these individual level effects provides predictions for how overall choice frequencies are affected by time pressure. For illustrative purposes, we will distinguish between three situations, labeled *type 0*, *type 1* or *type 2*. Furthermore, we will refer to a decision maker as "selfish" or "fair" depending on which of the two options yields a higher utility value according to her subjective preferences. In situations of *type 0*, the incentives are such that the underlying absolute utility differences are the same for the average selfish and fair decision maker. Thus, fair and selfish decision makers are equally likely to make a mistake under time pressure and time delay. In situations of *type 1*, on the other hand, the absolute utility difference is larger for the average fair than for the average selfish decision maker. Hence, in these situations selfish decision makers are more likely to make a mistake. Finally, in situations of *type 2*, the utility differences are larger for the average selfish than for the average fair decision maker such that fair decision makers are more likely to make a mistake.

Under the simplifying assumption that time pressure exclusively affects decision makers with smaller average utility differences (i.e. weak preferences for one of the options) and that there are no mistakes under time delay, the DDM generates straightforward predictions. In situations of *type 1*, time pressure exclusively causes selfish decision makers to make a mistake such that time pressure inflates the frequency of fair choices relative to a situation without time pressure. For situations of *type 2*, the DDM predicts the reverse effect. Here, fair decision makers should make more mistakes under time pressure, thus reducing the fraction of fair choices under time pressure.

Without this simplifying assumption (i.e. assuming that the probability of making a mistake is positive under time pressure and, to a smaller degree, under time delay for all decision makers), the DDM predictions depend on two factors: first, the average strength of preferences and second, the relative frequency of fair and selfish decision makers within the population.[5] The strength of preferences determines the likelihood

[5] We are grateful to an anonymous referee for pointing out this crucial distinction and helping us to refine our model.

Table 2.1: Testing the FII hypothesis

		Predicted effects of Time Pressure		
		Type 0 $p(f) = 0.5$	Type 1 $p(f) < 0.5$	Type 2 $p(f) > 0.5$
Observed effect	↑	unambiguous accept (0a)	ambiguous accept (1a)	unambiguous accept (2a)
	↔ ↓	unambiguous reject (0b)	unambiguous reject (1b)	ambiguous accept (2b)

of committing an error under time pressure and time delay for a given type of decision maker. The population shares, on the other hand, determine the resulting absolute number of mistakes and the aggregate direction of switches. The most simple test case for the FII hypothesis is a situation of *type 0* in which the relative population shares of fair and selfish decision makers are roughly similar. In such a perfectly balanced situation - however rare such situations might be in actual empirical tests - the DDM predicts that time pressure should have no effect on the frequency of fair choices since fair and selfish decision makers are equally likely to make a mistake (under time pressure and time delay) and both groups are of equal size. Consequently, the absolute number of mistakes is perfectly balanced between both groups and there should be no effect of time pressure. The DDM also generates unambiguous predictions when the type of decision maker who has larger utility differences is less common within the population ($< 50\%$). In these cases, the DDM predicts that time pressure increases the fraction of choices which are associated with larger absolute utility differences. For example, if the fair option is preferred by less than 50% of subjects in a situation of *type 1* (where fairness is "easy"), time pressure should increase the fraction of fair choices. This increase is driven by two factors: first, selfish decision makers are more likely to make an error under time pressure and to switch to their preferred choice under time delay as compared to fair decision makers. Second, given that they constitute the larger group, there should be more switches from the fair (under time pressure) to the selfish option (under time delay) than vice versa.

In all other cases, i.e. when the decision makers who have larger utility differences are more common in the population, the predictions of the DDM depend on the relative population shares as well as the unobservable difference in error rates under time pressure and time delay for both types of decision makers.[6]

[6] Appendix 2.1 contains a more formal discussion of the possible results.

2.2.2 Testing the FII hypothesis accounting for DDM predictions

Assuming that choices under time pressure and time delay are affected by the relative use of intuition over deliberation *as well as* the subjective difficulty of making a choice, the arguments above imply that the predictions of the DDM and the FII are congruent in situations of *type 1* as long as the fraction of fair decision makers is smaller than 50%. Hence, observing that time pressure increases the fraction of fair choices in these situations can only provide ambiguous evidence in favor of the FII hypothesis because the same observation could be fully accounted for by the DDM (see Table 2.1, 1a). Instead, if we do not find these predicted patterns, then this constitutes unambiguous evidence against the FII hypothesis (1b).[7]

In contrast, unambiguous evidence in favor of the FII hypothesis can be obtained from situations of *type 2*, as long as the fraction of selfish decision makers is smaller than 50%. Here, the FII hypothesis and the DDM predict opposite time pressure effects which may even cancel each other out. Thus, observing that time pressure does increase the fraction of fair choices would be unambiguous evidence in favor of the FII hypothesis (2a). Not observing any or even a negative effect would not necessarily be inconsistent with the FII hypothesis because the opposite effects of the FII hypothesis and the DDM may actually cancel each other out (2b).

Finally, in situations of *type 0* in which relative population shares are roughly similar, the DDM should have little influence on the direction of time pressure effects as fair and selfish decision makers are equally likely to make mistakes and are present in equal proportions within the population. Thus, observing an increase of fair behavior in such situations would be unambiguous evidence in favor of the FII, while observing no or a negative effect would provide unambiguous evidence against the FII.

Whenever the DDM predictions regarding the direction of time pressure effects are not clear because they depend on unobservable differences in error rates, tests of the FII hypothesis cannot be interpreted unambiguously. Therefore, classifying the choice situation as *type 0*, *type 1* or *type 2* and approximating the population shares of fair decision makers is necessary for correctly interpreting the evidence. Previous tests of the FII hypothesis might, thus, suffer from spuriously accepting the FII hypothesis based on observing an increase of fairness in situations of *type 1* or spuriously rejecting

[7] Note that observing no effect is not necessarily evidence against the DDM in these situations. This is because the true model might be that "selfishness is intuitive". Hence, a negative effect of time pressure attributable to the "selfishness is intuitive" model might be canceled out by a positive effect attributable to the DDM. For this reason, we cannot *jointly* reject the FII hypothesis *and* the DDM.

it based on observing no effect or a decrease of fairness in situations of *type 2*.

2.3 Experimental Design

In our experiment, we collect decisions from four binary dictator (see Table 2.2) and four prisoner's dilemma games (see Table 2.3). In each game, subjects are asked to choose between a "fair" and a "selfish" option (labeled option "A" or "B" on the decision screen). In line with the FII hypothesis (Rand et al., 2014), we label a choice as "fair" if it implies sharing resources with another individual at own costs. According to this definition the equal allocation is the "fair" choice in the binary dictator (BD) games and cooperation is the "fair" choice in the prisoner's dilemma (PD) games. Across the four BD and PD games, we increased the social benefits of choosing the fair option from VERY LOW to HIGH. For example, in the VERY LOW binary dictator game, choosing the fair (equal) option increases the recipient's payoff by 10 cents for every Euro that the dictator gives up relative to the selfish (unequal) option. In HIGH, the recipient receives 2.25 for every Euro that the dictator gives up.[8]

If subjective utility differences reflect the costs and benefits of choosing the fair option (Andreoni and Miller, 2002), we expect that fair decision makers should perceive smaller utility differences in the VERY LOW games than selfish decision makers. In these games, the benefits of choosing the fair option are relatively small since the decision maker needs to sacrifice a high amount of her own payoff to increase the payoff of the other participant by only a small amount. Hence, these games potentially resemble a type 2 choice situation that would allow for an unambiguous test of the FII hypothesis. By the same logic we expect that the HIGH games resemble a type 1 choice situation in which fair decision makers perceive larger utility differences than selfish decision makers. Here, decision makers need to give up only a small amount in order to increase the payoff of the other participant by a high amount.

Despite these considerations, it is hard to predict a priori if choosing the fair option will be subjectively more or less difficult than choosing the selfish option in a given game. Furthermore, a correct interpretation of the evidence also requires a measure of whether the fair or the selfish option is preferred by a majority of decision makers. To gain empirical insights into the subjective difficulty of choosing the fair and the selfish option as well as the respective population shares, we conducted additional sessions in which subjects could decide without being constrained in their response times. Based

[8] Labeling the equal outcome as fair in the binary dictator game also aligns our FII predictions with recent findings in Capraro et al. (2017) who show that equal outcomes are preferred by intuitive decision makers whereas deliberation allows for a variety of motives to affect decisions.

Table 2.2: Binary dictator games used in the experiment

VERY LOW		LOW		MEDIUM		HIGH	
Unequal	11, 0	Unequal	9, 0	Unequal	10, 1	Unequal	15, 2
Equal	1, 1	Equal	3, 3	Equal	6, 6	Equal	11, 11

Table 2.3: Prisoner's dilemma games used in the experiment

VERY LOW			LOW			MEDIUM			HIGH		
	C	D		C	D		C	D		C	D
C	3.10, 3.10	1, 5.10	C	4, 4	1, 6	C	6, 6	1, 8	C	8, 8	1, 10
D	5.10, 1	2, 2	D	6, 1	2, 2	D	8, 1	2, 2	D	10, 1	2, 2

on the previous finding that response times reflect the relative difficulty of the choice situation (Krajbich et al., 2015a), we use these additional observations to classify games as type 0, 1 or 2.

We used the following procedures in our experiment: Part 1 of the experiment consisted of two successive blocks. In block 1, subjects made decisions in the four prisoner's dilemma games displayed in Table 2.3 in randomized order. After each prisoner's dilemma game, subjects made choices in unrelated filler games (see Appendix 2.2). Once subjects had completed block 1 and a short questionnaire, we elicit choices in the exact same four prisoner's dilemma and filler games again in block 2. The games were presented in the same order in block 1 and 2 for each subject.[9]

Part 2 of the experiment also consisted of two successive blocks. In block 1, subjects made choices in the four binary dictator games displayed in Table 2.2 in randomized order. Choices were elicited using the strategy vector method, i.e. both subjects in a pair made a choice before the computer randomly assigned them to the roles of dictator or recipient. After each binary dictator game, subjects took choices in three filler games (see Appendix 2.2). Once subjects had completed block 1 and another short questionnaire, they made choices in the same four binary dictator and filler games again in block 2.

For each binary choice, subjects were randomly re-matched in pairs and no feedback on their partner's choice was given until the very end of the experiment. At the end of the experiment, one of the games was randomly drawn and subjects were paid according to their own and their partner's choice.

To analyze the effect of time pressure on the fraction of fair choices, we randomly assigned subjects to one of four (between-subjects) conditions, in which we implemented

[9] We randomized the order in which the prisoner's dilemma games were displayed across sessions. The filler games were presented in the same order in all sessions. Subjects were not informed that they would make the same choices in both blocks.

Table 2.4: EXPERIMENTAL DESIGN

		TIME PRESSURE (TP)	STRONG TIME PRESSURE (STP)	TIME DELAY (TD)	UNCON-STRAINED (U)
PART 1	BLOCK 1 4 PDs + 4 Filler Games	≤ 12 seconds	≤ 8 seconds	> 12 seconds	no constraint
	BLOCK 2 4 PDs + 4 Filler Games	> 12 seconds	> 12 seconds	> 12 seconds	no constraint
PART 2	BLOCK 1 4 BDs +12 Filler Games	≤ 6 seconds	≤ 4 seconds	> 6 seconds	no constraint
	BLOCK 2 4 BDs + 12 Filler Games	> 6 seconds	> 6 seconds	> 6 seconds	no constraint

Notes: This table summarizes the Experimental Design. Each cell displays the response time limit which subjects faced during their choice. We abbreviate prisoner's dilemma as "PD", and binary dictator game as "BD".

different response time constraints: in the two Time Pressure conditions, TP and STP, subjects were constrained to choose under time pressure in block 1 and forced to wait before making a choice in block 2. In the Time Delay (TD) condition, subjects were forced to delay their decision in both, block 1 and block 2. In the Unconstrained condition subjects did not face an exogenous time limit in either block.

In the Time Pressure (TP) condition, the time limit was 12 seconds for all prisoner's dilemma games and 6 seconds for all binary dictator games. These time limits correspond to the first quartile of the response time distribution of the *first* choice in the Unconstrained condition.[10] Given that subjects usually get faster over time and that it is unclear how much time is required to induce intuitive decisions[11], we implemented a stricter time limit of 8 seconds in the PD games which was reduced to 4 seconds in

[10] To our knowledge there is no common method according to which time pressure was defined in previous studies. For instance, subjects in Rand et al. (2012) were constrained to decide within 10 seconds which corresponds to the median response time in their response time correlation study. Buckert et al. (2017) define time pressure as 2/3 of the median response time in a Cournot game. Our analysis of response times in the Unconstrained treatment revealed that the response time distribution of the 25% fastest decision makers was independent of the order in which the games were presented. Thus, the time limit in our study avoids heterogeneous effects across different order conditions.

[11] For instance, Myrseth and Wollbrant (2017) argue that any time limit above 4 seconds could allow decision makers to engage in some level of deliberation. Spiliopoulos and Ortmann (2017) report that mean response times fall by up to 30 percent when subjects face the exact same game multiple times.

the BD games in the Strong Time Pressure (STP) condition. These time limits correspond to the first quartile of the response time distribution for the *last* decision in the Unconstrained condition. The time delay limit was 12 seconds for the PD games and 8 seconds for the BD games in both the TP and the STP condition, so that there is a small gap in the STP condition. The payoffs were displayed graphically as stacked and colored bars in all games (see Appendix 2.2) in order to make them easily accessible and comparable, even under time pressure.

To ensure compliance with our treatment, we forced subjects to delay their decision by displaying the choice buttons only after 12 seconds (6 seconds) had passed. Since compliance with time pressure cannot be enforced in the same way, we instead chose to incentivize compliance by informing subjects that they would lose their show-up fee of 3 Euro if they violated the time constraint in the decision chosen for payment.[12] A counter, displaying seconds spent, was included on each decision screen in both the Time Delay and the Time Pressure conditions.

At the end of part 1, we elicited subjects' beliefs regarding the choices of other participants which allows us to test whether time pressure and time delay affected beliefs differently.[13] Subjects were paid an additional Euro for a correct guess. In addition, we asked subjects to provide a subjective assessment regarding which of the two options they perceive as the fairer option for the very first BD and PD games they encountered in each block. This assessment can be used to identify if the equal (cooperative) option is indeed perceived as "fair" by a majority of our subjects.[14]

2.4 Results

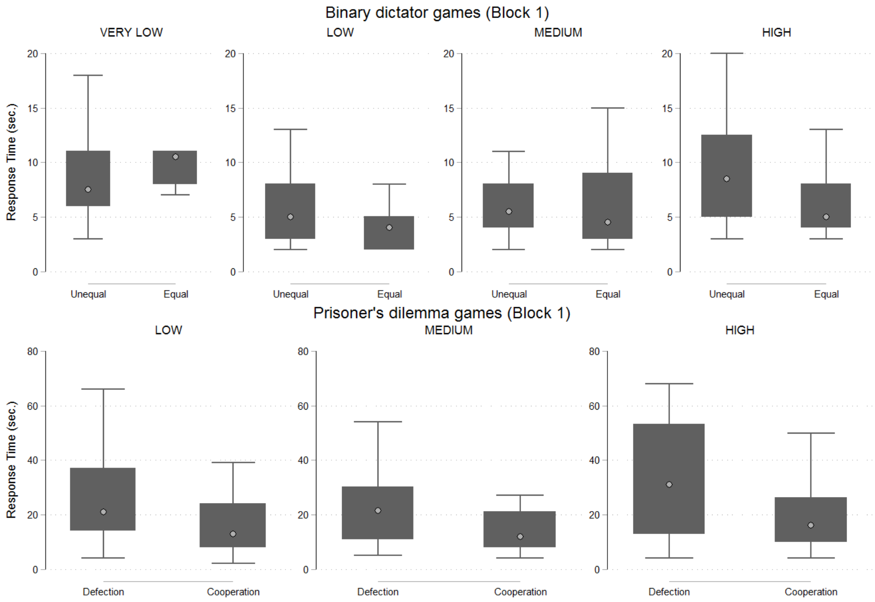
The experiment was conducted at the University of Heidelberg AWI Lab. In total, 238 undergraduate and graduate students of all disciplines were recruited to participate in the experiment (62 in Unconstrained, 74 in Time Delay, 72 in Time Pressure and 30 in Strong Time Pressure) via HROOT (Bock et al., 2014). We restricted our recruitment

[12]One important limitation of previous studies has been that a large fraction of subjects violate the time constraints set by the experimenter which potentially reduces their explanatory power (Tinghög et al., 2013; Bouwmeester et al., 2017). In contrast, we observed few violations of the time limit: Averaged over all decisions and treatments, the time pressure conditions were violated in 2.5 percent of the BD and 1.7 percent of the PD games. There is no significant difference in violations between the TP and STP condition.

[13] In the Time Pressure treatment, subjects were constrained to indicate their belief within 12 seconds. In the Time Delay treatment, subjects could indicate their belief only after 12 seconds had passed.

[14] Despite being unincentivized and thus noisy, this survey approach can provide some insights into the modal fairness perceptions of subjects (Faravelli, 2007; Cubitt et al., 2011; Reuben and Riedl, 2013).

Figure 2.1: Response times in prisoner's dilemma and binary dictator games (Unconstrained condition)



to subjects who had not participated in more than four experiments (and no experiment involving social dilemma or distribution tasks). The experiment was programmed in z-Tree (Fischbacher, 2007). Subjects received all instructions (reproduced in Appendix 2.3) on the screen and questions were answered privately. At the end of the experiment, subjects were paid in private. The average earnings were 12 Euro, including a 3 Euro show-up fee. In the following, we will report the results of the Unconstrained condition before analyzing the results of the Time Pressure and Time Delay conditions.

2.4.1 Unconstrained condition

The purpose of the Unconstrained condition was to identify situations in which the fair choice is faster or slower than the selfish choice and to approximate the frequency of fair and selfish choices. This information can be used to classify the different games according to the theoretical considerations outlined in section 2.2.

In Figure 2.1 (top panel) we compare the distribution of response times between choices of the equal ("fair") and the unequal ("selfish") option in the BD games. The frequency of "fair" choices rises significantly from 9.7% in the VERY LOW game to 43.5% in the LOW, 51.6% in the MEDIUM, and to 61.3% in the HIGH game (Pairwise Sign Test,

$p < 0.01$).[15] In line with the results in Krajbich et al. (2015a), we observe that the correlation between choices of the equal option and response times reverses as the benefits of the fair option increase: in the VERY LOW game, the median response time of subjects who chose the equal option is larger than the median response time of subjects who chose the selfish option (Rank-sum test, $p < 0.1$). Hence we classify this game as type 2. In the LOW and HIGH games, the median response times of subjects who chose the equal option are smaller than the response times of subjects who chose the selfish option (Rank-sum test, $p < 0.1$) which is why we classify these games as type 1. There is no significant difference in response times for the MEDIUM game (Rank-sum test, $p = 0.64$) which thus constitutes a type 0 game.

We use observed choice frequencies to determine if the DDM makes unambiguous predictions concerning the effect of time pressure in the different games. For the only type 2 situation (VERY LOW), the share of selfish decision makers is much larger than 50% such that the DDM makes ambiguous predictions regarding the expected effect of time pressure. For the two type 1 situations, the DDM makes unambiguous predictions for the LOW game (< 50% fair choices) but not for the HIGH game in which a majority of subjects chose the fair option. For the MEDIUM game, the DDM unambiguously predicts that time pressure should not have any effect on the fraction of fair choices given that subjects choose the fair and the selfish option at roughly equal rates (Binomial test, $p = 0.9$). Thus, solely the LOW and the MEDIUM game allow for unbiased tests of the FII hypothesis.

The distribution of response times for the three prisoner's dilemma games are displayed in the bottom panel of Figure 2.1.[16] Most importantly, the fraction of cooperators increases with the benefits: only 34% of subjects chose to cooperate in LOW, while this frequency rises to 55% in the MEDIUM and to 58% in the HIGH game. Looking at response times, we find that the median response time of subjects who chose to cooperate is significantly smaller than the median response time of those subjects who chose to defect in each of the three games (Rank-sum test, $p < 0.05$). Thus, the Unconstrained condition only includes PDs of type 1. Looking at the fraction of fair choices, the DDM only makes an unambiguous prediction in the LOW game since the share of cooperators is smaller than 50% in this game. For the MEDIUM and HIGH games, on the other hand, the DDM predictions are not unambiguous and hence they cannot provide unambiguous evidence in favor of or against the FII hypothesis. To

[15] In the Unconstrained condition the games were separated by additional distribution tasks, which were replaced by different filler games in the subsequent Time Pressure and Time Delay conditions. A full analysis of all 12 BD games is available upon request.

[16] We later added the VERY LOW game in the subsequent Time Pressure and Time Delay sessions because each of these games represent a type 1 situation.

also analyze the effect of time pressure in a game that is more likely to be of type 2, we added an additional prisoner's dilemma game (VERY LOW) in the Time Pressure and Time Delay conditions in which we further reduced the benefits of cooperation.[17]

2.4.2 Constrained response time in the binary dictator games

We begin our discussion of the constrained decision time treatments with a series of manipulation checks. Most importantly, time pressure significantly speeds-up decisions across all BD games from an average of 13.34 seconds (CI: 11.58, 15.11) in the TD to 3.22 seconds (CI: 3.02, 3.44) in the TP and 2.16 seconds (CI: 1.97, 2.36) in the STP condition.[18] In addition, average decision times are significantly smaller in the STP as compared to the TP condition (Rank-sum test, $p \leq 0.001$). Game-wise comparisons furthermore indicate that the effect of time pressure is similar across different games and significantly reduces response times in all four decisions (Rank-sum test, $p \leq 0.001$).[19] Finally, subjects in the TP and STP conditions, who take their first decision under time pressure and their second decision under time delay, are significantly faster (Sign-rank test, $p \leq 0.001$) when taking their first decision as compared to their second decision (TP: 9.66 seconds [CI: 9.04, 10.30]; STP: 9.51 seconds [CI: 7.90, 11.13]). Overall, these comparisons indicate that time pressure successfully induced faster decision making among subjects.

In a second manipulation check, we analyze whether subjects indeed perceive the equal option as the fair outcome in all four binary dictator games. For this purpose we analyze subjective (unincentivized) fairness statements elicited at the end of the experiment. We find that in all games, a large majority of subjects perceive the equal option as the fairest outcome (81% in VERY LOW, 97% in LOW, 100% in MEDIUM, 100% in HIGH). We also find that subjective fairness assessments do not differ across the three conditions. This indicates that labeling the equal option as "fair" is strongly in line with the fairness perceptions of our subjects, in particular for the LOW and MEDIUM games that are of most interest for testing the FII hypothesis.

[17] Since we do not observe response time correlations or choice frequencies for this game, all time pressure results can only be interpreted under the assumption that it indeed represents a type 2 situation in which there is a majority of fair decision makers. The second assumption is unlikely to hold, given that in the LOW game already only 34% of subjects cooperate despite stronger incentives to cooperate.

[18] Note, that the mean decision time in the TD condition is significantly higher than 6 seconds and only a minority of decisions (13.8 percent) is made within the range of 6-7 seconds. This indicates that there are only few subjects in the TD condition, who have already completed their decision process when reaching the delay cutoff.

[19] More detailed statistics on response time distributions for each game are available in Appendix 2.4

Table 2.5: Between-subject comparison of the average rate of fair choices in the binary dictator games

	Time Delay		Time Pressure		p-Value Fisher's	Strong Time Pressure		p-Value Fisher's
	N	mean	N	mean	exact	N	mean	exact
VERY LOW	74	0.11	72	0.11	1	30	0.13	0.74
LOW	74	0.32	72	0.44	0.17	30	0.57	0.03
MEDIUM	74	0.53	72	0.60	0.41	30	0.63	0.39
HIGH	74	0.64	72	0.74	0.22	30	0.63	1

Notes: The mean rate of fair choices displayed is calculated over all orders. We report the p-Values of a two-sided Fisher's exact test, comparing the fraction of fair choices in the TD condition with the TP (column 6) and the STP condition (column 9).

We now turn to analyzing the effect of time pressure on the fraction of fair choices in the BD games. Averaged over all decisions, we do not find evidence that subjects in the time pressure conditions chose the equal "fair" option significantly more often than subjects who took their decision under time delay (40% in TD vs. 47% in TP vs. 49% in STP, Rank-sum test, $p > 0.1$). The main results of our between-subjects test of the FII hypothesis are summarized in Table 2.5 in which we report the mean fraction of equal "fair" choices in each of the four games separately.

In the LOW game, the equal "fair" option is not chosen significantly more often when comparing the TD and TP conditions (Two-sided Fisher's exact test, $p = 0.17$). We obtain a different result, if we restrict our sample to those subjects who took their very first choice in this game (Two-sided Fisher's exact test, $p = 0.05$). When time pressure is stronger, we do find that the equal allocation is chosen significantly more often in the STP compared to the TD condition (Two-sided Fisher's exact test, $p = 0.028$), yet this significance vanishes when we restrict our analysis to first choices only (Two-sided Fisher's exact test, $p = 0.24$). Based on these results we can neither accept nor reject the hypothesis that "fairness is intuitive". In both treatments we observe that time pressure increases the fraction of fair choices either in the full sample or when restricting our analysis to first choices. Given that in the LOW game both the FII hypothesis and the DDM predict that time pressure should increase the fraction of fair choices, the observed increases in fair choices can, however, at most provide ambiguous evidence in favor of the FII hypothesis.

According to the DDM, time pressure should have no effect on the fraction of fair choices in the MEDIUM game. Hence an increase in fairness would be unambiguous evidence in favor of the FII. The fraction of subjects who select the equal option in the TP and STP conditions is indeed slightly higher under time pressure as compared to the TD condition but this increase is not significant (Two-sided Fisher's exact test; TP: $p = 0.41$, STP: $p = 0.39$). Restricting our comparison to first choices only does not alter this result (Two-sided Fisher's exact test; TP: $p = 0.76$, STP: $p = 0.10$). These results constitute unambiguous evidence against the FII hypothesis.

In the VERY LOW and HIGH game we find no evidence that time pressure affects the frequency of fair choices (Two-sided Fisher's exact test, $p > 0.1$). Since for these games it is unclear if the DDM predicts effects that are in line or orthogonal to the FII, we cannot unambiguously reject the FII based on these observations.

Result 1 (Between-Subject evidence in BDs) *We find evidence that time pressure increases the fraction of fair choices in games, in which this increase can be accounted for by both the DDM and the FII hypothesis (LOW game). In contrast, we do not find that time pressure significantly increases the fraction of fair choices in games, in which such increase can only be accounted for by the FII hypothesis (MEDIUM game).*

Our design also allows assessing within-subjects evidence by comparing a subject's initial choice under time pressure and her second choice (in the same game) under time delay. The two games that can provide unambiguous evidence in favor of or against the FII hypothesis are the LOW and the MEDIUM game. Given that the likelihood to switch from one to the other option may also be due to the fact that subjects gain more experience with the task between their first and their second decision, we compare the switching rates in the Time Pressure conditions to the switching rates in the Time Delay condition, where subjects take both decisions under time delay. To analyze switching behavior, we computed a variable that takes a value of 1 if a subject switched from the fair (first choice) to the selfish option (second choice), and a value of -1 if a subject switched from the selfish to the fair option (see Table 2.6).

Table 2.6: Within-subject comparison of switching behavior in the binary dictator games

	Time Delay			Time Pressure			Rank-sum p-Value	Strong Time Pressure			Rank-sum p-Value
	N	mean	Sign-rank	N	mean	Sign-rank		N	mean	Sign-rank	
	(within-subjects)			*(within-subjects)*			*(across)*	*(within-subjects)*			*(across)*
VLOW	74	0.04	0.18	72	0.03	0.41	0.78	30	0.1	0.08	0.32
LOW	74	0.07	0.19	72	0.15	0.02	0.27	30	0.27	0.01	0.05
MEDIUM	74	0.03	0.59	72	-0.03	0.64	0.48	30	0	1	0.80
HIGH	74	-0.01	0.74	72	0.11	0.03	0.05	30	-0.03	0.71	0.81

Notes: In this table, we report the direction of switches for each of the four binary dictator games. The switching variable takes a value of 1 if the subject switched from choosing the equal option in block 1 to choosing the unequal option in block 2. The decision in block 1 is taken under time pressure in the TP and STP condition and under time delay in the TD condition. The decision in block 2 is taken under time delay in all three conditions. Columns 4,7 and 11 report the p-Value of a Wilcoxon Sign Rank Test, performed on subject's switching behavior within one condition. In addition, we report the p-Value of a Rank-sum test, which compares switching behavior across the TD and TP (column 8) or STP (column 12) conditions.

First, looking at the switching rates in the two time pressure conditions, we do find that subjects are more likely to switch from choosing the fair option under time pressure to choosing the selfish option under time delay than vice versa in the LOW but not in the MEDIUM game. The former finding is consistent with the DDM and the FII hypothesis while the latter finding is inconsistent with the FII hypothesis. To control for a potential time trend in the probability to behave fairly which is not caused by our treatment, we compare the switching rate in the LOW and MEDIUM games in the two time pressure conditions to the Time Delay condition. The results of the Rank-sum test are reported in Table 2.6. Our analysis shows that in the LOW game, subjects in the STP condition were indeed more likely to switch from the equal to the unequal option compared to subjects in the TD condition. However, given that the DDM and the FII hypothesis both support this prediction, the evidence can only provide ambiguous evidence in favor of the FII hypothesis. In contrast, there is no statistical significant difference in switching patterns for subjects in the TP and TD conditions. This result constitutes unambiguous evidence against the FII hypothesis. For the MEDIUM game we find no evidence that there is significantly more switching behavior under time pressure than under time delay. This is direct evidence against the FII hypothesis.[20]

The interpretation of the previous results rests on the assumption that we indeed classified the games correctly. As a robustness check, we employ a complementary within-subject test that does not depend on the classification of the games but instead exploits the fact that we observe choices in four different games per subject. Based on these four choices we infer in which game a subject should be closest to her individual indifference point.[21] Let C_i=(x_1, x_2, x_3, x_4) describe the set of choices that a subject makes in the first four games such that $C_0 = (F, F, F, F)$ describes a subject who chooses the fair option in all four games and $C_2 = (S, S, F, F)$ describes a subject who chooses the selfish option only in the VERY LOW and LOW game. A C_0 subject is closest to her indifference point in the VERY LOW game since in this game choosing the fair option is more costly than in any of the other games. A C_2 subject is closest to her indifference point in the LOW or MEDIUM game since she switches from the selfish to the fair option between these two games. Overall, there are five different choice patterns that allow to approximate the location of the indifference point and 84.66%

[20] We do not find evidence that there are differences in switching behavior for the VERY LOW game, while there is significantly more switching for the HIGH game in the TP but not in the STP condition. Since both of these game can only provide ambiguous evidence in favor or against the FII hypothesis, we are not discussing these results in more detail.

[21] This method rests on the assumption that subjects have transitive preferences over own-other allocations that are only violated by mistake (Andreoni and Miller, 2002).

of subjects can be classified according to these patterns.[22] This classification can shed light on the role of subjective choice difficulty in the following way: the DDM predicts that subjects are more likely to make a mistake in games in which they are closer to indifference. Thus, when comparing a subject's first choices with her second choices in the same four games, she should be more likely to switch options in games closer to her indifference point. The FII, on the other hand, predicts that subjects should display a similar rate of switching for all games in which they have initially selected the fair option. Furthermore, there should be few to no switches in games in which subjects have initially selected the selfish option. These two predictions can be easily illustrated using an exemplary subject: assume a subject has chosen C_1=(S,F,F,F) in the initial four games and is classified accordingly. The FII hypothesis predicts that when comparing the subject's first to her second choices in the same games, she should switch to the selfish option with the same probability in the LOW, MEDIUM and HIGH games. Conversely, according to the DDM, the highest frequency of switches should occur in the VERY LOW or LOW game – as the exemplary subject is closest to her indifference point in these games – while there should be fewer switches in the MEDIUM and HIGH games. Figure 2.2 displays the propensity to switch in each game for each choice pattern.[23]

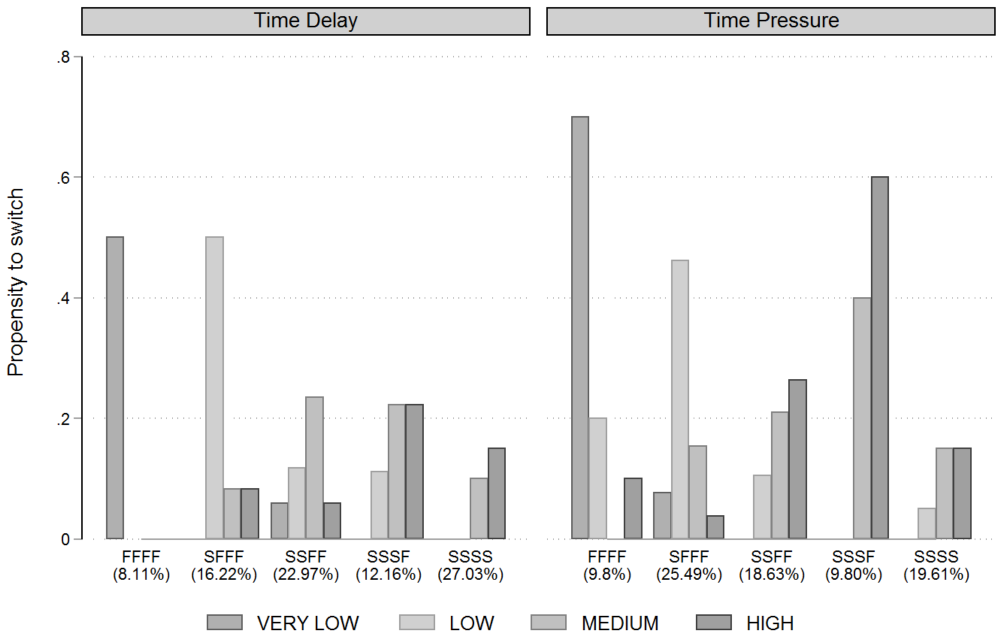
For almost all classifications, switching patterns are more closely in line with predictions of the DDM than with predictions of the FII hypothesis. With the exception of the *SSFF* pattern, we observe that subjects are more likely to switch in games which are closer to their indifference point. In contrast, we find little evidence that subjects are switching at similar rates in all games in which they have chosen the fair option under time pressure. This is most evident for the *FFFF* pattern, where a majority of switches occur in the first (VERY LOW) game even under time pressure. In contrast to the predictions of the FII hypothesis, there is also substantive evidence for switching from the selfish to the fair option (most pronounced for the *SSSF* and *SSSS* choice patterns). Given that most of these switches occur for games that are close to individual indifference points, this pattern is closely in line with the predictions of the DDM.

Result 2 (Within-Subject evidence in the BDs) *We do find evidence that subjects*

[22] The five choice patterns are (F,F,F,F), (S,F,F,F), (S,S,F,F), (S,S,S,F) and (S,S,S,S). For any other pattern (e.g. (S,F,S,F)) there is no clear indication in which decision a subject might have made an error that violates transitivity and hence where the indifference point for this subject might be located. Note that a consistent pattern does not necessarily imply that subjects have not made any error since a (S,F,F,F) subject could have made an error in either the VERY LOW game implying that her actual preferences are (F,F,F,F) or in the LOW game implying that her actual preferences are (S,S,F,F) or could have made more than one error.

[23] Due to the smaller group size for some classifications in the STP condition, we pooled data from both time pressure conditions for this analysis.

Figure 2.2: Conditional switching probabilities in the BDs



Notes: This figure displays the propensity to switch between first and second choice in a given game across five different classifications of consistent decision making. The left panel displays switching behavior when first choices have been made under time delay and the right panel displays switching behavior when first choices have been made under time pressure. The percentages indicate how common a classification is within the population.

are more likely to switch from the fair to the selfish option in games, in which this prediction is supported by the FII and the DDM (LOW); but not in games, in which this prediction is only supported by the FII hypothesis (MEDIUM). The latter provides unambiguous evidence against the FII hypothesis.

Table 2.7: Random effects probit regression for the effect of time pressure in the binary dictator games

	(1)		(2)		(3)	
TIME PRESSURE (TP)	0.350	(0.01)	0.00634	(0.43)	0.249	(1.33)
STRONG TIME PRESSURE (STP)	0.463	(1.30)	0.115	(0.20)	0.261	(0.33)
LOW	1.986****	(7.11)	1.632****	(4.28)	1.707****	(4.20)
MEDIUM	2.574****	(7.18)	2.480****	(6.26)	2.524****	(6.22)
HIGH	2.773****	(9.40)	2.627****	(6.28)	2.698****	(5.93)
SCREEN2	-0.345*	(-1.80)	-0.382*	(-1.95)	-0.259	(-0.87)
SCREEN3	0.251	(1.41)	0.259	(1.44)	0.622**	(2.02)
SCREEN4	0.176	(0.71)	0.197	(0.77)	0.239	(0.63)
TP * LOW			0.541	(1.12)	0.392	(0.69)
TP * MEDIUM			0.242	(0.49)	0.176	(0.33)
TP * HIGH			0.489	(0.88)	0.436	(0.77)
STP * LOW			1.010*	(1.78)	1.273	(1.60)
STP * MEDIUM			0.291	(0.49)	0.567	(0.83)
STP * HIGH			-0.0806	(-0.13)	-0.0736	(-0.11)
TP * SCREEN2					-0.0606	(-0.14)
TP * SCREEN3					-0.621	(-0.51)
TP * SCREEN4					-0.0298	(-0.05)
STP * SCREEN2					-0.602	(-1.04)
STP * SCREEN3					-0.598	(-1.26)
STP * SCREEN4					0.0114	(0.02)
CONSTANT	-2.396****	(-7.25)	-2.231****	(-5.85)	-2.410****	(-5.57)
Observations	704		704		704	
Subjects	176		176		176	
Prob > Chi^2	0.0000		0.0000		0.0000	

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Notes: The dependent variable takes a value of 1 if the equal option was chosen and 0 otherwise. The variables Time Pressure and Strong Time Pressure equal 1 if the subject was assigned to the corresponding condition and 0 otherwise. Low, Medium and High are dummy variables for the games with which we confronted subjects in block 1 and effects are reported relative to the Very Low game which is the omitted category. The screen variables capture potential order effects by indicating whether a choice was presented on the second, third or last screen. The effects are reported relative to the decision on the first screen.

A potential concern, given the observed decline in average fair behavior across decisions, is that subjects' choices as well as their response time may be influenced by the order in which the different games were presented. This concern should already be limited by the fact that we presented the games in a randomized order. In addition, we did not find any evidence in favor of the FII hypothesis using only the first decision taken by each subject. We additionally address this concern using a set of probit regression models. Each of these models takes individual choices in the four decisions of block 1 as a dependent variable. Importantly, the dependent variable encodes the order in which the choices were taken. That is, if a subject entered the LOW game on the first screen, it is coded as choice 1. We report the results of three different specifications in Table 2.7. In specification (1), we find no evidence that time pressure increases the frequency of equal choices, when controlling for order effects and the benefits of choosing the fair option. In specification (2) we add interaction terms between the treatment dummy and the benefits of choosing the fair option. Again, we find no evidence that time pressure significantly affects equal choices in any of the four games. Moreover, all interaction terms for the standard time pressure (TD) treatment are insignificant. This is further evidence that even in those games where both the DDM and the FII would predict an increase of fairness under time pressure there is no such effect. For stronger time pressure (STP), there is weakly significant evidence that time pressure increases the frequency of fair choices in the LOW game, but not in the MEDIUM game. The former finding is, however, predicted by both the DDM and the FII hypothesis. Finally, in specification (3) we add interaction terms between the TP and the SCREEN variables. These interactions terms would be significant if the order in which the games were presented would moderate the treatment effect - which we do not observe. As suspected, we do observe that the screen variables are significant in all three specifications. Thus, if a game was presented on a specific decision screen, the likelihood that a subject would choose the equal option was increased or decreased depending on the specification.

2.4.3 Constrained response time in the prisoner's dilemma games

As for the BD games, we find that time pressure significantly speeds up choices in the PD games.[24] Furthermore subjects' individual fairness assessments, which we elicited at the end of the experiment, are strongly consistent with our label: a large majority of the subjects perceives the cooperative option as the fairest outcome in all four prisoner's dilemma games (VLOW 94%, LOW 96%, MEDIUM 88%, HIGH 97%), independent

[24] All response time statistics are available in Appendix 2.4.

of the treatment condition.

Based on the analysis of response times and choice frequencies in the Unconstrained condition, the only PD game that can shed light on the FII hypothesis is the LOW game. The fraction of cooperative “fair” choices in the LOW game increases from 41% in the TD condition to 44% in the TP and 50% in the STP condition. This increase is not statistically significant though (Two-sided Fisher’s exact test; TD vs. TP: $p = 0.74$, TD vs. STP: $p = 0.39$). When we restrict our analysis to choices on the first decision screen (TD: 69% vs. TP: 60% vs. STP: 50%) we again find no evidence that time pressure increases the fraction of fair behavior but rather observe a slight decrease (Two-sided Fisher’s exact test; TD vs. TP: $p = 0.73$, TD vs. STP: $p = 0.43$). Given that in the LOW game the FII and the DDM both predict that time pressure should increase the fraction of fair choices, this observation is unambiguous evidence against the hypothesis that "fairness is intuitive" and the findings in Rand et al. (2012).

One potential concern is that our time pressure manipulation could have affected beliefs. If subjects were more optimistic about average contributions of others in the Time Delay compared to the Time Pressure or Strong Time Pressure condition, we might have observed no evidence in favor of the FII hypothesis for this reason. To address this concern, we compare the stated beliefs. We find that in the LOW game, average beliefs did not differ between the two conditions (Rank-sum test; TD vs. TP: $p = 0.63$, TD vs. STP: $p = 1$).

In a second step, we analyze the within-subject effect of time pressure on choices in the LOW prisoner’s dilemma game. For this purpose, we compute switching probabilities by comparing a subject’s first choice with her second choice in the same game. If fairness was indeed intuitive, we would expect that more subjects initially choose to cooperate under time pressure and switch to defection under time delay. Note that the DDM makes the same prediction. Thus, if we do not observe the expected switching pattern, this would constitute unambiguous evidence against the FII hypothesis.

The first thing to note is that subjects in the TP and STP conditions are indeed more likely to switch from cooperation under time pressure to defection under time delay (Sign Rank Test; TP $p = 0.07$, STP $p = 0.01$). Subjects in the TD condition are also more likely to switch from cooperation to defection, but this difference is not significant (Sign Rank Test; $p = 0.2$). When we compare switching behavior in each of the two Time Pressure to switching behavior in the Time Delay condition, we do not find that subjects in either of the two time pressure conditions were more likely to switch from cooperation to defection as compared to subjects in the Time Delay condition (Rank-sum test; TD vs. TP: $p = 0.62$, TD vs. STP: $p = 0.12$). Hence, instead of reflecting a reassessment of an initial intuitive decision, the decline of cooperative choices in the

Time Pressure conditions might simply reflect the well-known fact that subjects tend to become more selfish in repeated decisions even without receiving feedback (Ledyard, 1994). Therefore, our within-subject evidence in in this game does not support the FII hypothesis.

Like in the BD games, a complementary analysis of conditional switching patterns at the individual level shows that most switches occur in games in which subjects are close to their indifference point. These observations support the idea that switching behavior under time pressure reflects choice difficulty instead of a reassessment of an intuitive fair choice.[25]

Result 3 (Between- and within-subject evidence in the PDs) *In the Prisoner's dilemma games, we do not find evidence that time pressure increases the fraction of fair choices even when both the FII as well as the DDM would support this prediction (LOW game). Within-subjects, we find that subjects in both Time Pressure conditions are as likely to revise an initially fair choice as subjects in the Time Delay condition. Both results are inconsistent with the FII hypothesis.*

2.5 Conclusion and Discussion

In this paper we propose and conduct a new test of the FII hypothesis (Rand et al., 2012; Cappelen et al., 2016). Our test takes into account that a causal test of this hypothesis, using time pressure and time delay manipulations, needs to account for the subjective difficulty of making a choice. We use a simple version of the Drift Diffusion Model (DDM) to show that time pressure can increase or decrease the frequency of fair choices, depending on whether decision makers who prefer the fair option perceive smaller or larger utility differences than decision makers who prefer the selfish option and depending on the distribution of preference types within the population. Hence, these predicted effects may either be aligned with those of the FII hypothesis or affect choices under time pressure in the opposite direction. In our experiment, we then analyze the effect of time pressure in choice situations in which both the DDM and the FII hypothesis predict that time pressure should increase the fraction of fair choices. In neither of the BD or PD games classified accordingly, we find that time pressure consistently increases the fraction of fair choices. On the other hand, we do not find that time pressure increases the fraction of fair choices in games, in which this increase is only predicted by the FII hypothesis, thus rendering unambiguous evidence

[25] The full analysis of the remaining games and switching patterns can be found in Appendix 2.4.

against the FII hypothesis. Our empirical test therefore provides little support for the hypothesis that "fairness is intuitive" in a general way. This result holds between- and within-subjects. A complementary analysis further demonstrates that switching patterns strongly reflect choice difficulty (subjective indifference), a pattern that is supported by the DDM but not by the FII hypothesis.

On the one hand our rejection of the FII hypothesis is in line with a number of recent papers (Fiedler et al., 2013; Tinghög et al., 2013; Martinsson et al., 2014; Duffy and Smith, 2014; Verkoeijen and Bouwmeester, 2014; Achtziger et al., 2015; Kocher et al., 2016; Lohse, 2016; Capraro and Cococcioni, 2016; Tinghög et al., 2016) and a large scale replication project (Bouwmeester et al., 2017) which also suggest that in some instances behaving fairly might not be intuitive and might even require additional deliberation or stronger self-control. On the other hand, our results are surprising at least to the degree that they contradict a significant number of previous studies which tend to find that time pressure or other forms of inducing intuitive decision making lead to more cooperative or fair choices. For instance, a recent meta-study finds that relying on intuition relative to deliberation increases the average rate of cooperation by 6.1 percentage points in one-shot games (Rand et al., 2016). Similarly, several current theories on the link between intuition and pro-social behavior are based on the idea that deliberation can never increase cooperation (Dreber et al., 2014; Rand et al., 2014; Bear and Rand, 2016)[26]. The observation that some experiments have found an increase of fairness under time pressure while other experiments report no effect or even a reduction of fairness could well be in line with our theoretical considerations because none of the previous experiments has explicitly accounted for subjective utility differences. Hence, it is conceivable that some experiments have looked at choice situations where the DDM and the FII predict effects of time pressure which go in the same direction while other experiments have looked at choice situations in which the DDM and the FII hypothesis make opposite predictions. The most obvious reason for such differences is the choice of the experimental task or its parameters. But even in experiments that analyze the same game (e.g., a public good game with MPCR 0.5) subject pools might differ (e.g., students vs. non-students) and it is possible that subjects with different individual attributes or cultural backgrounds might attach different subjective valuations to options in the same task, thereby leading to unobserved heterogeneity in terms of the perceived choice difficulty as well as the share of fair decision makers. Given that both of these factors determine if the DDM predicts an increase or decrease of fair behavior under time pressure, these experiments might come to different conclusions

[26] For a discussion of the last paper see Myrseth and Wollbrant (2016) and Jagau and van Veelen (2017).

concerning the FII hypothesis.

At this point it is also important to stress that our paper does not attempt to directly replicate previous test of the FII hypothesis or pinpoint any other moderating factor (e.g. confusion, experience, social value orientation, default options) that might also affect the direction of a time pressure effect. Rather, we aim at pointing out that it is unclear whether previous tests provide ambiguous or unambiguous evidence in favor of or against the FII hypothesis, since they do not account for subjective utility differences. Therefore our test differs from these previous tests of the same hypothesis along several dimensions that are motivated by our theoretical considerations: in our test subjects were confronted with several one-shot choice situations instead of only one, the specifics of each choice situation were only revealed on the decision screen and not on a preceding instruction screen[27], stakes were considerably higher than in many of the previous internet experiments, we used a graphical interface to visualize the payoffs of the different choice options and the compliance with the response time manipulations was more strongly enforced and consequently substantially higher. We believe that each of these design changes was well motivated and necessary in order to provide an unbiased test of the FII hypothesis. Furthermore, none of these changes should make it less likely to find evidence in favor of the FII hypothesis in an obvious way if it was generally valid as suggested by the mechanism motivating the social heuristics hypothesis (Rand et al., 2014).

Overall our results suggest that the link between intuition and fairness is more complicated and nuanced than previously thought. A closer inspection of further moderating factors might provide useful insights into the conditions or individual attributes that influence the link between intuition and fairness. Several recent contributions have already provided first insights into the role of confusion (Recalde et al., 2014; Stromland et al., 2016; Goeschl and Lohse, 2016), gender (Rand et al., 2016; Tinghög et al., 2016), culture (Nishi et al., 2017), stake size (Mrkva, 2017) and social-value-orientation (Chen and Fischbacher, 2015; Mischkowski and Glöckner, 2016).

[27] This is in line with Fiedler et al. (2013) and Capraro and Cococcioni (2016) but differs from (Rand et al., 2012). We, however, believe that giving subjects a possibility to fully deliberate on a task before entering the decision screen will affect the chances of isolating intuitive tendencies via time pressure.

Appendix 2.A

2.A.1 Model and Predictions

In this Appendix, we will describe the Drift Diffusion Model (DDM), introduced in section 2.2, in more detail. This description will derive the following three predictions from the DDM: First, the higher the (absolute) subjective utility difference between the options of choice, the lower a decision maker's expected response time. Second, a decision maker is more likely to make a mistake (i.e. to choose the option that yields a lower subjective utility) the smaller the utility difference between the two options of choice. Third, a decision maker is more likely to make a mistake under time pressure the smaller the utility difference between the two options of choice.

The first two predictions are common in the DDM literature and have previously been used to show that the *correlation* between response times and "fair" behavior can reflect subjective utility differences (Krajbich et al., 2014, 2015a,b). The third prediction is novel, at least in the context of the literature on fairness and time pressure, and follows immediately from the second prediction. A set of plausible parameter assumptions furthermore allows us to infer how this individual level prediction affects the aggregate share of fair choices under time pressure and time delay.

For the purpose of illustration, we will refer to a basic version of the DDM. This basic version can be summarized as follows: A decision maker accumulates stochastic information about her preferences for a "fair" (henceforth: F) and a "selfish" (henceforth: S) option over a series of time periods t. We denote the decision maker's true underlying utility value for the fair and the selfish option by $u^i(F) = u_F$ and $u^i(S) = u_S$. Thus, the true underlying utility difference between the fair and the selfish option is $V = u_F - u_S$. In each period t, decision makers observe noisy value signals $F_t \sim \mathcal{N}(u_F, \sigma_F^2)$ and $S_t \sim \mathcal{N}(u_S, \sigma_S^2)$ which are centered around the true means of the underlying value function and which are independently and identically distributed (i.i.d.).

In line with the existing literature (Krajbich et al., 2015a), we will assume that $\sigma_F^2 = \sigma_S^2$, i.e. the distribution functions from which the signals are drawn only differ in their respective means. After observing a pair of signals, the decision maker computes the value difference between the two signals, i.e. $V_t = F_t - S_t$. The stochastic evidence observed until period t is accumulated in a subjective state variable X_t. The accumulation process stops as soon as the state variable X_t crosses an upper threshold a, inducing the decision maker to choose F, or a lower threshold b, inducing the decision maker to choose S. We will follow the convention and assume that the two decision thresholds a and b are equidistant from 0 so that $b = -a$. The evolution of the subjective state variable X_t before hitting either of the decision thresholds in discrete time can be written as:

$$X_t = X_{t-1} + (u_F - u_S) + \epsilon_t = X_{t-1} + V + \epsilon_t \tag{2.1}$$

where $\epsilon_t \sim \mathcal{N}(0, \sigma^2)$ captures the noise in the process with

$$X_t \sim \mathcal{N}(tV, t\sigma^2) \tag{2.2}$$

This simple variant of the DDM can also be modeled in continuous time as a Brownian motion with drift (Ratcliff and Rouder, 1998; Smith, 2000; Bogacz et al., 2006) for which expressions have been derived for the probability of choosing option F for $V > 0$ and the mean response time given that $V \neq 0$ (Palmer et al., 2005; Clithero, 2016)[28]. The probability that a decision maker who prefers the fair option ($V > 0$) actually chooses this option can then be written as:

$$P_F^F = \frac{1}{1 + e^{\frac{-2Va}{\sigma^2}}} \tag{2.3}$$

As can be easily verified by letting $V \to \infty$ and $V \to 0$, $P_F^F \in [0.5; 1[$.

From expression (2.3), the probability of choosing the selfish option given that $V > 0$ (i.e. the probability that a fair decision maker chooses the selfish option by mistake) directly follows as

$$P_S^F = 1 - P_F^F \tag{2.4}$$

so that $P_S^F \in]0; 0.5]$.

The expected number of periods (which is commonly referred to as "response time" in the economics and psychology literature) until one of the thresholds a or b is reached for $V \neq 0$ can furthermore be written as[29]:

$$E[t] = \frac{a}{V} \tanh\left(\frac{aV}{\sigma^2}\right) \tag{2.5}$$

By symmetry, equations (2.3) and (2.4) can be expressed equivalently for the probability that a selfish decision maker with $V < 0$ chooses the fair or the selfish option.

[28] An additional assumption is that there is no initial bias in favor of one of the two options s.t. $X_0 = 0$.

[29] This expression makes use of the hyperbolic tangent function $\tanh(z) = \frac{e^z + e^{-z}}{e^z - e^{-z}}$

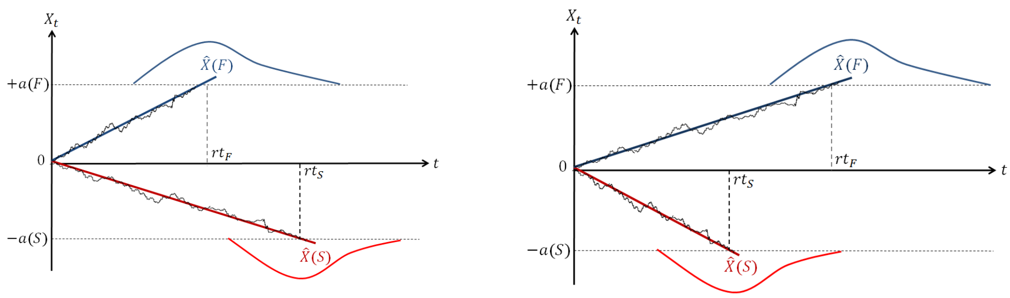
Prediction I

Prediction I states that the expected response time decreases as the absolute utility difference between the fair and the selfish option (i.e. $|V|$) increases. Since the first derivative of equation (2.5) w.r.t V is strictly negative for $V > 0$, the above statement follows immediately. Assuming symmetrical thresholds and no initial bias, the same is true for $V < 0$.

Figure 2.3 illustrates the relationship between expected response times and $|V|$ and its implications for inferring choice difficulty from response times. We denote the utility difference of fair decision makers as $V_F > 0$, and the utility difference for selfish decision makers will be denoted $V_S < 0$. A direct implication of Prediction I is that fair choices are relatively faster if $V_F > |V_S|$ (see Figure 2.3a). Similarly, fair choices are expected to be relatively slower, if $V_F < |V_S|$ (see Figure 2.3b). A direct corollary of this relationship is that arbitrary correlations between fair choices and response times can be created by varying the relative attractiveness of the fair option (Krajbich et al., 2015a).

Figure 2.3: An illustration of two exemplary processes

(a) Fair choices are faster (b) Selfish choices are faster



Notes: This Figure displays two exemplary Drift Diffusion Processes. $\hat{X}(F)$ ($\hat{X}(S)$) represents the expected evolution of the subjective state variable for the average decision maker with $V > 0$ ($V < 0$) option. Expected response times are labeled as rt_F and rt_S. The actual evolution of the subjective state variable is subject to noise, as characterized by the black lines that fluctuate around $\hat{X}(F)$ and $\hat{X}(S)$. The expected response time distribution of fair choices are displayed above a, the corresponding distribution for selfish choices is displayed below $-a$.

Prediction II

Prediction II states that a decision maker is less likely to make a mistake (i.e. to choose the option that yields the lower subjective utility value) as $|V|$ increases. We will demonstrate this by showing that a decision maker is more likely to choose her truly preferred option as $|V|$ increases. For $V > 0$, this follows immediately from equations (2.3) and (2.4). The first derivative of statement (2.3) w.r.t. V is given by:

$$\frac{\partial P_F^F}{\partial V} = \frac{2ae^{\frac{-2aV}{\sigma^2}}}{\sigma^2(1+e^{\frac{-2aV}{\sigma^2}})^2} > 0 \tag{2.6}$$

Hence, the probability of choosing the truly preferred fair option increases in V for $V > 0$. As a corollary, using statement (2.4), P_S^F decreases in V. By symmetry, an equivalent result can be derived for $V < 0$. The intuition behind this prediction is that noise in the decision process will have a larger impact on the value of X_t if $|V|$ is small.

Prediction III

Prediction III states that time pressure will reduce the probability of a decision maker to choose her truly preferred option. We will follow the convention in the psychological literature and model time pressure as leading to a collapse of the decision thresholds a and b to a lower absolute level (Bogacz et al., 2006; Milosavljevic et al., 2010; Hawkins et al., 2015).[30] Intuitively, a lower decision threshold implies that decision makers will choose at lower precision because noise will gain a higher weight in the decision process resulting in a higher likelihood that the wrong threshold is crossed. In the following, we will assume that a decision maker's threshold is A if he is constrained to wait (i.e. $t > t_L$) before making a choice, and $a < A$ if he is constrained to make a fast choice (i.e. $t < t_L$). Hence, A can be re-written as $A = z \cdot a$ with $z > 1$.[31]

Taking the first derivative of equation (2.3) w.r.t a shows that the probability of choosing the correct option for a given $V > 0$ decreases as a decreases. Using equation (2.4), this implies that higher time pressure causes a decision maker to make more mistakes.

[30] This assumption allows to base predictions on equation (2.3). An alternative way to model time pressure would be to analyze the distribution of X_t at different points in time using equation (2.2) or to analyze the distribution of first barrier passage times from which equations (2.3) and (2.5) are derived. The first approach would ignore the presence of decision boundaries. The second approach would rely on an analytical expression for the first passage times with two boundaries $P(T_a \leq t_l) = 1 - (e^{\frac{aV}{\sigma^2}} K_T^{\infty}(a) - e^{\frac{aV}{\sigma^2}} K_T^{\infty}(b))$ as derived e.g. in Smith (2000) or Hieber and Scherer (2012). However, this expression contains the infinite sum ($K_T^N(k) := \frac{\sigma^2\pi}{(a-b)^2} \sum_{n=1}^{N} \frac{(n(-1)^{(n+1)})(e^{(-T(\frac{V^2}{2\sigma^2}+\frac{\sigma^2 n^2\pi^2}{2(a-b)^2})}) sin(\frac{n\pi k}{a-b})}{\frac{V^2}{2\sigma^2}+\frac{\sigma^2 n^2 \pi^2}{2(a-b)^2}}$) for which no closed form solution for the derivative w.r.t V can be found without relying on approximation (Voss et al., 2004; Bogacz et al., 2006).

[31] This approach implicitly assumes that there are sufficient incentives so that all subjects will decide within the time limit, instead of explicitly modeling the choices of subjects who have not crossed the (now lower) decision threshold at t_L. One could e.g. assume that these undecided subjects decide randomly or simply by the sign of the state variable (Bogacz et al., 2006). Both approaches are not opposed to, but would rather strengthen Prediction III as the likelihood of being undecided rises as $|V|$ falls.

$$\frac{\partial(1-P_F^F)}{\partial a} = -\frac{2ae^{\frac{-2aV}{\sigma^2}}}{\sigma^2(1+e^{\frac{-2aV}{\sigma^2}})^2} < 0 \tag{2.7}$$

Hence, a direct corollary of Predictions II and III is that time pressure will lead to a higher frequency of mistakes among decision makers with smaller $|V|$. To see this intuitively, compare two decision makers and assume that $V_F > |V_S|$. In this case, selfish decision makers are more likely to cross the wrong decision threshold compared to fair decision makers when they have to take a decision under time pressure.

Aggregate choice frequencies

We will use the results of Predictions II and III to show how aggregate choice frequencies respond to time pressure and time delay. Let α be the fraction of decision makers who prefer the fair option ($V > 0$), and $1-\alpha$ be the fraction of decision makers who prefer the selfish option ($V < 0$). Furthermore, let $P_k^m(V,a)$ be a function that describes the probability of a decision maker of type $k \in \{F,S\}$ to choose an option $m \in \{F,S\}$ using equations (2.3) and (2.4). As described above, we assume that time pressure leads to a collapse of the decision thresholds, i.e. the decision threshold is A under time delay and $a < A$ under time pressure such that $A = z \cdot a$ with $z > 1$. We will look at a case where $V_F{>}|V_S|$. We write $V_F{>}l \cdot |V_s|$ with $l > 1$.

We write for the probability of choosing the fair option under time pressure

$$p_F(a) = \alpha \cdot (P_F^F(V_F,a)) + (1-\alpha) \cdot (P_F^S(V_S,a)) \tag{2.8}$$

Similarly, the probability of observing a fair choice under time delay is

$$p_F(A) = \alpha \cdot (P_F^F(V_F,A)) + (1-\alpha) \cdot (P_F^S(V_S,A)) \tag{2.9}$$

Using equations (2.8) and (2.9), we can derive the condition under which the fraction of fair choices is higher when the decision threshold is a as compared to A:

$$p_F(a) \geq p_F(A)$$

$$\alpha \cdot (P_F^F(V_F,a)) + (1-\alpha) \cdot (1-P_S^S(V_S,a)) \geq \alpha \cdot (P_F^F(V_F,A)) + (1-\alpha) \cdot (1-P_S^S(V_S,A))$$

This equation can be re-written as

$$(1-\alpha)\cdot(P_S^S(V_S,A)-P_S^S(V_S,a))\geq\alpha\cdot(P_F^F(V_F,A)-P_F^F(V_F,a)) \tag{2.10}$$

We want to show that this conditions holds if $\alpha < 0.5$ (i.e. if the type with the stronger preference is less common in the population). For $\alpha \leq 0.5$ it suffices to show that

$$P_S^S(V_S,A)-P_S^S(V_S,a)>P_F^F(V_F,A)-P_F^F(V_F,a) \tag{2.11}$$

because $(1-\alpha)\geq\alpha$ and hence if (2.11) holds, (2.10) holds as well.

We re-write equation (2.11) by plugging in (2.3). To simplify, we will set $a = 1$ such that $A = z$. In addition, we use $V_F = l \cdot V_S$ with $l > 1$ and define the signal-to-noise ratio as $y = \frac{V}{\sigma^2} > 1$. Thereby, we can re-write equation (2.11) as

$$\frac{1}{1+e^{-2yz}}-\frac{1}{1+e^{-2y}}>\frac{1}{1+e^{-2lyz}}-\frac{1}{1+e^{-2ly}} \tag{2.12}$$

This equation states that time pressure leads to a higher frequency of fair choices, if the difference in error rates under time pressure versus time delay is larger for selfish than for fair decision makers.

To further simplify, we will define the following function:

$$g(\lambda,z)=\frac{1}{1+e^{-2\lambda z}}-\frac{1}{1+e^{-2\lambda}} \tag{2.13}$$

Note that the L.H.S of equation (2.12) is $g(y,z)$ and the R.H.S. is $g(ly,z)$. Thus, to show that equation (2.12) holds, we show that $g(\lambda,z)$ is a strictly decreasing function in λ for some reasonable parameter assumptions.

$$\frac{\partial g(\lambda,z)}{\partial\lambda}=2z\frac{1}{(1+e^{-2\lambda z})^2}e^{-2\lambda z}-2\frac{1}{(1+e^{-2\lambda})^2}e^{-2\lambda}<0 \tag{2.14}$$

This equation can be re-written as:

$$z\frac{(1+e^{-2\lambda})^2}{(1+e^{-2\lambda z})^2}e^{-2\lambda(z-1)}<1 \tag{2.15}$$

which holds if $\lambda > 1$ and $z > 1$.[32]

[32] More precisely, there exists a λ_0 for which this equation always holds if $\lambda > \lambda_0$ which is a function of z. The bigger z (i.e. the stronger the effects of time pressure) the smaller λ_0 becomes. If we drop the assumption that $a = 1$ this parameter restriction translates to $a \cdot \lambda > 1$. In other words, we have to assume that either the signal to noise ratio y is sufficiently strong or that the decision threshold a is sufficiently large which ensures that decisions are not fully governed by noise. Since we treat the DDM as a model of decision making, it seems reasonable to assume that the average decision maker receives value signals which are strong enough. In addition, our empirical design ensures that

This result leads to four predictions depending on the population share α and l.

1. For $\alpha = 0.5$ and $l = 1$ it is easy to verify that the L.H.S. and R.H.S. of equation (2.10) are exactly equal. This indicates that we would not expect a change in the frequency of fair choices due to the DDM - a case that we have described as a *type 0* ("perfectly balanced") situation in the main text of the paper.

2. For $\alpha \leq 0.5$ and $l > 1$, we have shown above that equation (2.12) is fulfilled as long as the signal to noise ratio $y > 1$ or the decision threshold a is sufficiently large (or a combination of both). Here, time pressure should increase the fraction of fair choices relative to time delay. We refer to this as a situation of *type 1*.

3. For $\alpha > 0.5$ and $0 < l < 1$, we can derive a reverse statement of equation (2.12). Here, the DDM predicts that time pressure decreases the fraction of fair choices. We refer to this as a *type 2* situation in the paper.

4. For $\alpha > 0.5$ and $l > 1$, it does not suffice to show that equation (2.12) is fulfilled. As can be seen from equation (2.10), whether the prediction holds depends on the absolute difference in error probabilities. Hence, without knowing the mistake probabilities under a and A, the DDM does not make clear predictions in these situations. By symmetry, a reverse statement can bee derived for the case where $\alpha < 0.5$ and $0 < l < 1$.

2.A.2 Experimental Details

Visual Presentation of Games

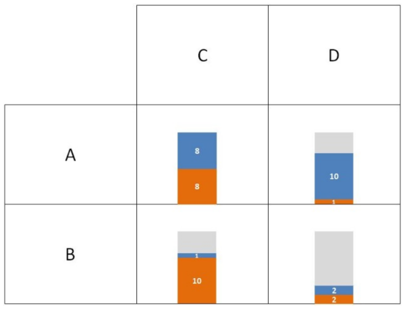
The payoffs associated with each binary choice were displayed graphically as stacked and colored bars in all games (see Figure 2.4). The subject's own payoff corresponded to the orange and the other participant's payoff to the blue bar in all binary choice situations. Subjects received detailed instructions on how to read the bars and we confirmed their understanding in four control questions before they could start with the actual decision tasks. Furthermore, the examples used for the control questions did not relate to prisoner's dilemma games but displayed arbitrary payoffs to prevent potential priming effects. We believe that this way of displaying the payoffs has two advantages. First, it ensures that participants with different types of preferences can identify and implement their preferred choice with equal difficulty. Second, it makes the

time pressure is not extreme, allowing individuals to make non-random decisions. When both z and y become large, note that $g(y, z) \sim g(ly, z)$ so that the difference is mainly reflecting differences in population shares.

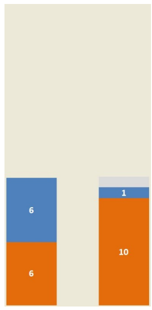
payoffs easily accessible and comparable across the choice options, even under response time constraints.

Figure 2.4: PRESENTATION OF GAMES IN THE EXPERIMENT

(a) Prisoner's dilemma games



(b) Binary dictator games

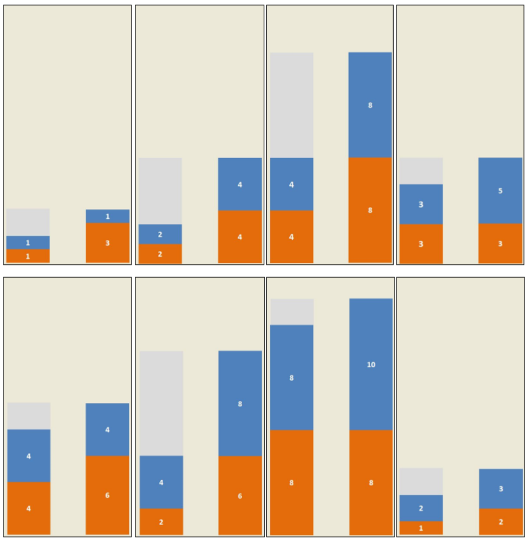


Filler Games

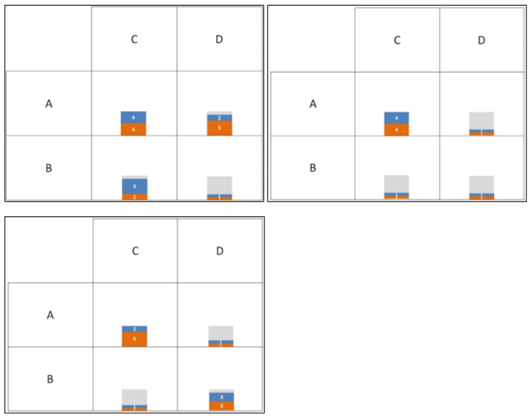
In Figure 2.5, we display the filler games that subjects faced during the experiment.

Figure 2.5: Filler Games

(a) Binary dictator games (Part 2)



(b) Prisoner's dilemma games



2.A.3 Instructions

Instructions were presented on the screens in German language. A translated version of the original instructions is presented below. The original instructions are available upon request.

Instructions for part 1 of the experiment

You will now start with the first part of the experiment.

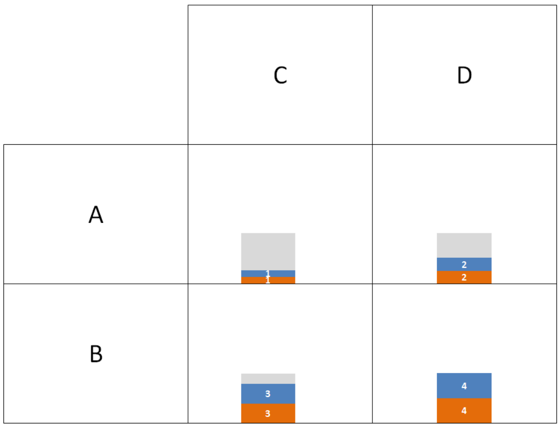
This part of the experiment consists of **10 rounds**.

In each round, you will interact with one other randomly chosen participant. No participant is going to be informed with whom he or she has interacted during the experiment.

Procedure within each round

In each round, both participants **simultaneously** choose one of two options: You decide between option “A“ and “B“, the other participant decides between option “C“ and “D“. Hence, you decide between options A and B without knowing which option has been chosen by the other participant.

Your payoff and the payoff of the other participant depend on the decisions of both participants. At the beginning of each round (so before you and the other participant have made a choice), both participants will see a table in which the four different payoffs are displayed.



Each of the four possible payoffs is depicted as a bar chart. The bars consist of several coloured parts.

Your own payoff corresponds to the orange part of the bar. The number within the orange part of the bar indicates the exact Euro amount that you will going to receive in that case.
The payoff of the other participant corresponds to the blue part of the bar. The number within the blue part of the bar indicates the exact Euro amount that the other participant is going to receive in that case.

The height of the orange and the blue part corresponds to the sum of payoffs for both participants. The grey part of the bar indicates the payoff difference to the maximum achievable sum in this round.

Examples:

Example 1:

You have chosen option A, the other participant has chosen option C. This results in the following payoffs: You receive 1 Euro and the other participant receives 1 Euro.

Example 2:

You have chosen option B, the other participant has chosen option D. This results in the following payoffs: You receive 4 Euro and the other participant receives 4 Euro.

Please note

The actual payoff table is going to **look different** in the experiment. Also, the payoffs will differ in each round.

End of a round

The round is over as soon as both participant have taken a decision. You **will not be informed** about the choices of the other participant.

Your payoff

At the end of the experiment the computer will randomly **choose one round from this or the other part of the experiment. You receive the amount which results from your own and the decision of the other participant.** Hence, each decision in this part of the experiment can influence your final payoff at the end of the experiment

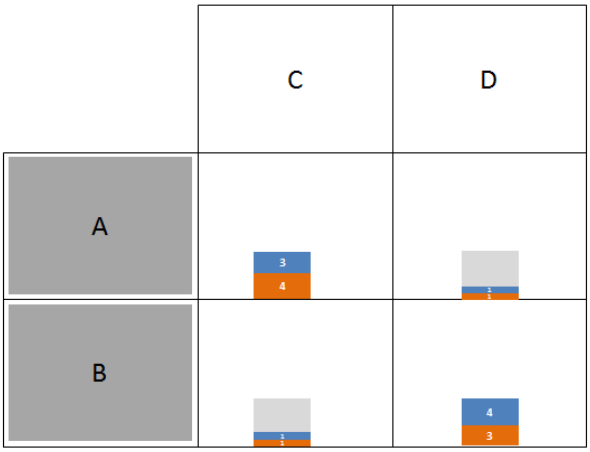
You have received all instructions for the first part of the experiment now. Press “continue“ to learn more about how each choice will be displayed on your computer and to test your comprehension on an example.

Screen:

Example

In this part you can test your comprehension using the payoff table displayed below. Your choices in this part will not influence your final payoff.

Please look at the payoffs displayed in the table:



Question 1:

Suppose you choose option A and the other participant chooses option C.

Your payoff in Euro: ____________________
Payoff of the other participant in Euro: ___________

Question 2:

Suppose, you choose option B and the other participant chooses option C.

Your payoff in Euro: ____________________ Euro (1)
Payoff of the other participant in Euro: ___________ Euro (1)

Press “continue“ to find out whether you answered correctly.

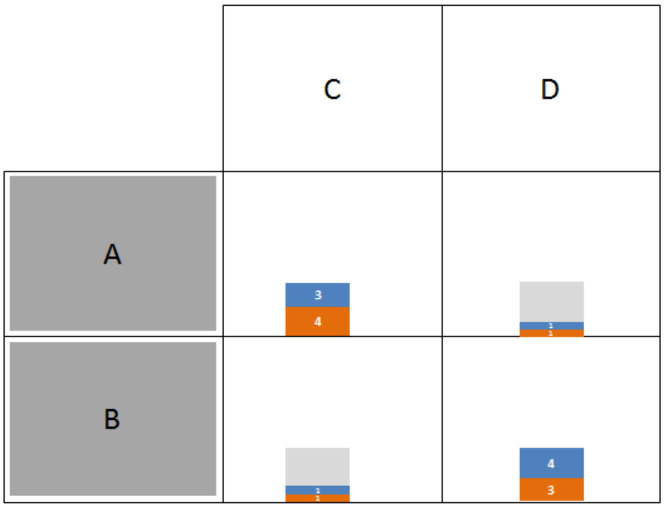
Screen:

Feedback (correct):

You have answered both questions correctly. You can start with round 1 now.

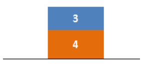
Feedback (wrong):

Unfortunately, you did not answer all questions correctly. Please take a look at the payoffs displayed in the table once more:



<u>Question 1 (wrong):</u>

In question 1 you were asked: What payoff would you and the other participant receive in the case that you would chose option A and the other participant would choose option C. The payoffs are as follows:



In this case you would receive 4 Euro (orange part of the bar). The other participant would receive 3 Euro (blue part of the bar).

Please make sure that you have understood all instructions. If you need further help please contact the experimenter.

<u>Question 2 (wrong):</u>

In question 2 you were asked: What payoff would you and the other participant receive in the case that you would chose option B and the other participant would choose option C. The payoffs are as follows:

In this case you would receive a payoff of 1 Euro (orange part of the bar). The other participant would receive a payoff of 1 Euro (blue part of the bar).

Please make sure that you have understood all instructions correctly. If you still have problems, please contact the experimenter.

Screen:

[Time Pressure]

In the following 5 rounds you should decide quickly.

Please select option A or B in **less than 12 seconds** in every round.

The remaining decision time is displayed above the payoff table.

If your decision takes longer than 12 seconds in one round and if this round is chosen for payoff, you will not receive your show-up fee of 3 Euro.

[Time Delay]

In the following 5 rounds you should wait before making a decision.

In each round you should wait **at least 12** seconds before you decide between options A und B.

Only after 12 seconds have passed, the grey choice buttons labelled "A" and "B" will appear on the screen.

You don't have to decide precisely after 12 seconds. You **can think as long as you want**.

Press "continue" to start with the first round.

Screen:

[Time Pressure]

In the following 5 rounds, there is a **time limit** for your decision.

Suppose, one round from this part of the experiment is chosen. **You receive your show up fee of 3 Euro only if you …**

… took a decision in more than 12 seconds
… took a decision in less than 12 seconds
… took a decision in exactly 12 seconds
… took a decision in less than 20, but more than 12 seconds

… in the randomly chosen round.

Please select the correct answer. On the next screen you will be informed whether your answer was correct.

[Time Delay]

In the following 5 rounds there is a **time limit for your decision.**

In each round you should…

… take a decision in less than 12 seconds.
… wait at least 12 seconds before to take a decision.
… take a decision in exactly 12 seconds
… take a decision in at least 8, but less than <TimePressure|1> seconds to take a decision.

Screen:

Feedback Correct:

You answered the question correctly and can now start with round 1.

Feedback Wrong:

Unfortunately, you haven't answered the question correctly. Please take a look at the following advice again.

[Time Pressure]

In the following 5 rounds you should decide quickly.

Please select option A or B in **less than 12 seconds** in every round.

The remaining decision time is displayed above the payoff table.

If your decision takes longer than 12 seconds in one round and if this round is chosen for payoff, you will not receive your show-up fee of 3 Euro.

[Time Delay]

In the following 5 rounds you should wait before making a decision.

In each round you should wait **at least 12** seconds before you decide between options A and B.

Only after 12 seconds have passed, the grey choice buttons labelled "A" and "B" will appear on the screen.

You don't have to decide precisely after 12 seconds. You **can think as long as you want**.

Press „continue" in order to start with the first round.

2.A.4 Additional Results

Results for Prisoner's Dilemma Games

Table 2.8: Between-subject comparison of the average rate of fair choices in the prisoner's dilemma games

	Time Delay		Time Pressure		p-Value Fisher's	Strong Time Pressure		p-Value Fisher's
	N	mean	N	mean	exact	N	mean	exact
VERY LOW	74	0.30	72	0.44	0.09	30	0.57	0.01
LOW	74	0.41	72	0.44	0.74	30	0.50	0.39
MEDIUM	74	0.43	72	0.53	0.32	30	0.60	0.14
HIGH	74	0.64	72	0.56	0.40	30	0.47	0.13

Notes: The mean rate of fair choices displayed is calculated over all orders. We report the result of a two-sided Fisher's exact test comparing the fraction of fair choices in the BD and the TP and STP conditions.

Table 2.9: Within-subject comparison of switching behavior in the prisoner's dilemma games

	Time Delay			Time Pressure			Rank-sum	St. Time Pressure			Rank-sum
	N	mean	Sign-rank	*N*	mean	Sign-rank	p-Value	*N*	mean	Sign-rank	p-Value
	(within-subjects)			*(within-subjects)*			*(across)*	*(within-subjects)*			*(across)*
VLOW	74	0.11	0.05	72	0.28	0.01	0.04	30	0.33	0.01	0.03
LOW	74	0.08	0.20	72	0.13	0.07	0.62	30	0.27	0.01	0.12
MEDIUM	74	0.04	0.53	72	0.31	0.01	0.01	30	0.27	0.02	0.07
HIGH	74	0.19	0.01	72	0.08	0.32	0.38	30	0.07	0.32	0.23

Notes: In this table we report the direction of switches for each of the four prisoner's dilemma games. The switching variable takes a value of 1 if the subject switched from cooperation in block 1 to defection in block 2. The decision in block 1 is taken under time pressure in the TP and STP condition and under time delay in the TD condition. The decision in block 2 is taken under time delay in all three conditions. Columns 4 and 7 report the p-Value of a Wilcoxon Sign Rank Test, performed on subject's switching behavior within one condition. In addition, we report the p-Value of a Rank-sum test which compares switching behavior across the TD and TP (column 8) and the STP condition (column 12).

Figure 2.6: CONDITIONAL SWITCHING PROBABILITIES IN THE PRISONER'S DILEMMA GAMES

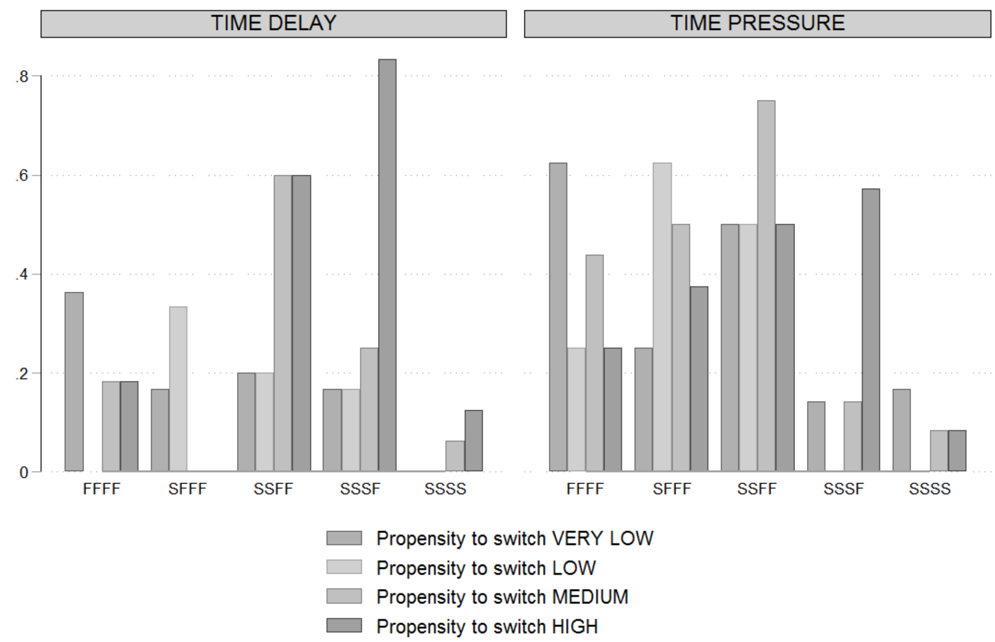


Table 2.10: Random effects probit regression for the effect of time pressure in the prisoner's dilemma games

	(1)		(2)		(3)	
TIME PRESSURE (TP)	0.180	(0.95)	0.546*	(1.87)	0.417	(1.15)
STRONG TIME PRESSURE (STP)	0.316	(1.35)	0.922**	(2.47)	0.763	(1.53)
LOW	0.0397	(0.19)	0.394	(1.44)	0.730**	(2.23)
MEDIUM	0.350*	(1.79)	0.568**	(2.16)	0.938***	(3.13)
HIGH	0.576****	(3.45)	1.171****	(4.68)	1.181****	(4.55)
SCREEN2	-0.528***	(-2.64)	-0.573***	(-2.79)	-1.252****	(-4.56)
SCREEN3	-0.668****	(-3.89)	-0.709****	(-4.06)	-0.981****	(-3.83)
SCREEN4	-0.777****	(-4.31)	-0.728****	(-3.99)	-0.786***	(-2.96)
TP * LOW			-0.421	(-1.21)	-0.778*	(-1.74)
TP * MEDIUM			-0.219	(-0.66)	-0.640	(-1.49)
TP * HIGH			-0.780**	(-2.22)	-0.810**	(-2.25)
STP * LOW		-0.662	(-1.24)	-1.485**	(-2.37)	
STP * MEDIUM			-0.294	(-0.72)	-1.577**	(-2.52)
STP * HIGH			-1.470***	(-2.90)	-1.175**	(-2.10)
TP * SCREEN2					0.865**	(2.05)
TP * SCREEN3					0.295	(0.82)
TP * SCREEN4					0.196	(0.51)
STP * SCREEN2					1.915***	(2.93)
STP * SCREEN3					0.845	(1.48)
STP * SCREEN4					-0.252	(-0.45)
CONSTANT	0.0532	(0.27)	-0.239	(-1.03)	-0.175	(-0.67)
Observations	704		704		704	
Subjects	176		176		176	
Prob > Chi^2	0.0000		0.0000		0.0000	

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Response Time Statistics

Figure 2.7: Response times in the binary dictator games

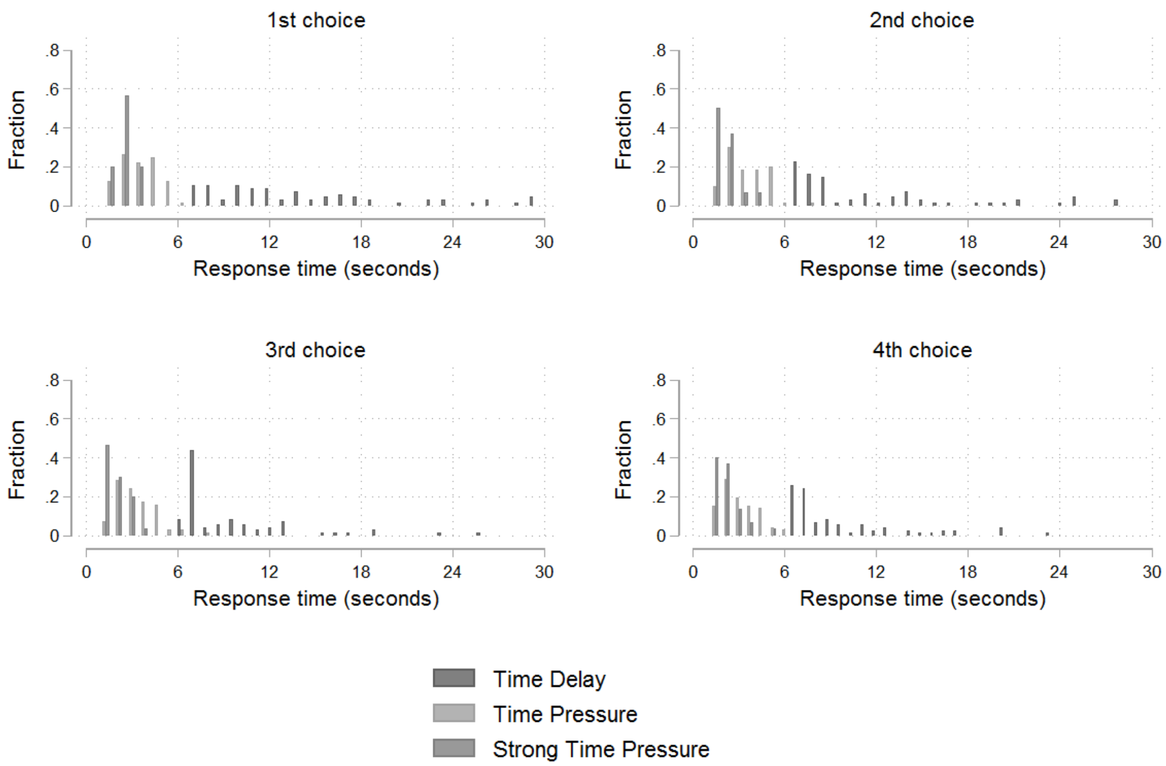


Figure 2.8: Response times in the prisoner's dilemma games

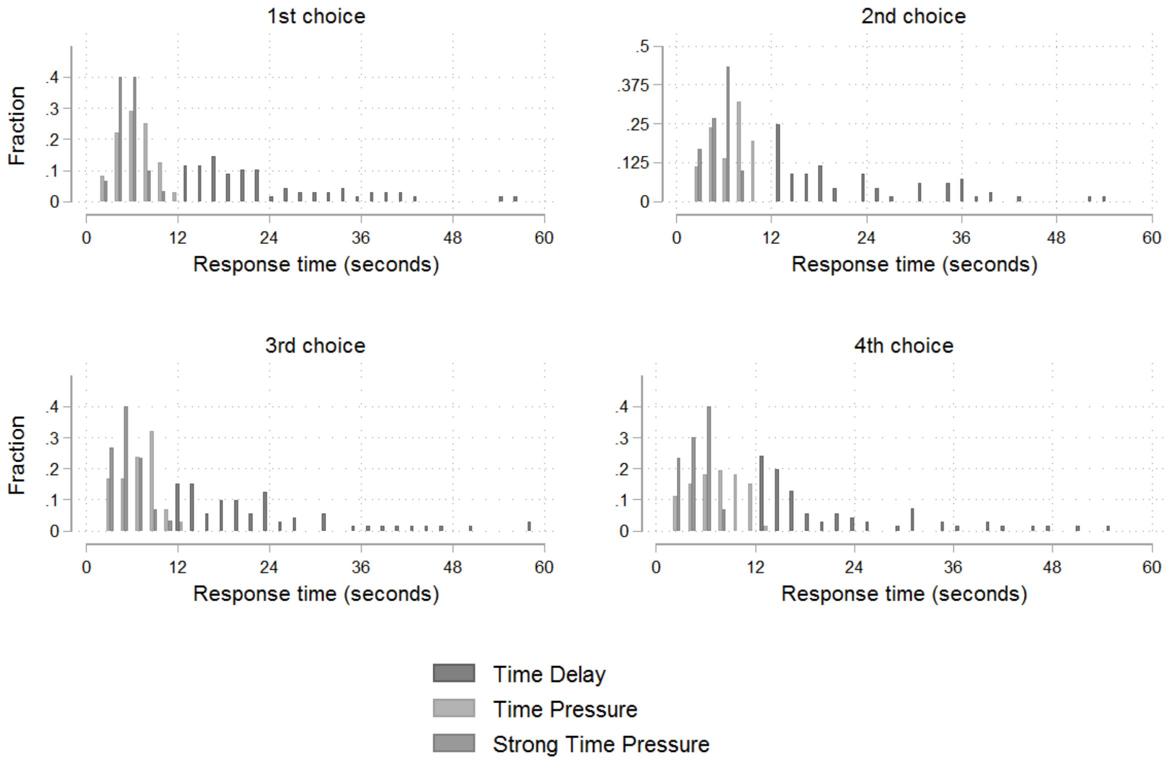


Table 2.11: Average and maximum response times across all Binary dictator Games

	Time Delay		Time Pressure		St.Time Pressure	
	Mean (Max.) Block 1	Mean (Max.) Block 2	Mean (Max.) Block 1	Mean (Max.) Block 2	Mean (Max.) Block 1	Mean (Max.) Block 2
VLOW	17.37 (103.54)	9.86 (30.68)	3.48 (5.98)	11.61 (30.62)	2.51 (3.79)	10.84 (31.11)
LOW	14.72 (64.96)	9.13 (25.20)	3.46 (8.00)	10.32 (36.26)	2.11 (4.42)	10.52 (46.87)
MEDIUM	11.48 (98.36)	7.85 (106.89)	3.21 (7.84)	8.54 (17.40)	1.88 (3.82)	8.76 (27.09)
HIGH	9.79 (23.76)	8.54 (38.98)	2.98 (6.07)	8.18 (29.85)	2.14 (0.32)	7.90 (13.74)

Notes: This table shows response time averages for the different BD games across blocks I and II and treatment conditions.

Table 2.12: Average and maximum response times across all Prisoner's dilemma Games

	Time Delay		Time Pressure		St.Time Pressure	
	Mean (Max.) Block 1	Mean (Max.) Block 2	Mean (Max.) Block 1	Mean (Max.) Block 2	Mean (Max.) Block 1	Mean (Max.) Block 2
VLOW	28.85 (127.85)	23.25 (87.78)	6.25 (12.46)	26.51 (84.89)	5.31 (8.86)	23.27 (78.98)
LOW	27.57 (108.15)	21.95 (98.54)	6.51 (9.92)	21.08 (62.95)	5.06 (7.58)	18.21 (46.98)
MEDIUM	25.03 (99.21)	25.96 (204.10)	6.75 (12.75)	22.05 (50.87)	4.98 (9.93)	20.68 (75.76)
HIGH	26.14 (243.61)	20.61 (152.516)	7.19 (13.18)	23.66 (63.92)	4.73 (7.79)	22.90 (77.47)

Notes: This table shows response time averages for the different PD games across blocks I and II and treatment conditions.

Chapter 3

Do people prefer donating to identifiable victims?

Abstract
Several papers have found that people tend to make larger donations to single identified individuals than to groups of statistical recipients. This is often referred to as the "identifiable victim effect". One explanation which has been proposed for this finding is that people give more if they can identify the recipient of their donation. An alternative explanation is that people prefer concentrated over equal distributions of their donation. To distinguish between these alternative hypotheses, I run two experimental treatments in which each subject is matched to a group of children whose photos are presented to subjects. In the first treatment, subjects can only donate to one of the children. For some decisions, subjects know which child is chosen (i.e. the recipient is identified), in other situations they just know that one of the three children will be selected such that the recipient is unidentified. In the second treatment, the donation is either disbursed to a single child or equally divided among all three children. I find that subjects do not donate more to identified versus unidentified recipients in this setting. Furthermore, I find that groups of children receive higher donations than single children.

Keywords: charitable giving, identifiability, social preferences

JEL Classification: C44, C91, D91

3.1 Introduction

Numerous psychological and economic experiments have shown that people tend to be far more generous when they can identify the recipient instead of being presented with an abstract large scale problem. A popular example is that of Jessica McClure, a little girl who fell into a well in Texas in 1987. Her ordeal was closely followed by the media until her rescue 2 days after she had fallen into the well. Americans responded with enormous sympathy and the McClure family received more than $700,000 donations (Jenni and Loewenstein, 1997). Despite the impressive amount of sympathy and helping behavior, Cryder et al. (2013) note that this outburst of generosity occurred at a time where UNICEF estimated that millions of unidentified children would die from causes for which relatively cheap treatments are available.

The greater emotional response towards identified as compared to statistical recipients has entered the literature as the "identifiable victim effect" (Schelling, 1968). According to a recent article in The New Yorker (2013)[1], academic research on the "identifiable victim effect" has inspired the design of fund-raising campaigns. For example, the charity Benevolent allows donors to sponsor specific individuals in reaching a predetermined goal, such as buying a piece of land in Uganda or renewing a professional license. Other charities, such as World Vision, allow donors to support a single identified child with regular donations. Does the possibility to observe the recipient increase donations? This is what I study in this paper.

Several papers have studied the role of identifiability on donations by conducting experiments. These studies typically compare donations to recipients identified by photo, name and age and otherwise anonymous recipients, for which no individuating information is provided (Jenni and Loewenstein, 1997; Small and Loewenstein, 2003; Kogut and Ritov, 2005a,b; Genevsky et al., 2013; Small et al., 2007). All of these studies find that donors give more to identified as compared to anonymous recipients.

An interesting open question arising from this literature is whether donors increase their donation in response to the available information (Cryder et al., 2013), or whether they value identifiability *per se*. Some scholars explicitly support the latter view. For example, Kogut and Ritov (2005a,b) argue that identifiability increases the likelihood that donors adopt the perspective of the recipient. Related studies find that adopting the perspective of another person in need increases empathy and altruistic motivations to help (Batson, 1987; Batson et al., 2016). Hence, according to this view, there could

[1] New help for the poor: Cash grants though a website (2013), https://www.newyorker.com/business/currency/new-help-for-the-poor-cash-grants-through-a-web-site

be a difference between individuating information (such as a recipient's name, age and photo) and the mere fact of *knowing who* receives one's donation. As a result, we would expect that a donor who observes a group of individuals, all of which are presented by the same kind of individuating information (such as photos), would still donate more if she knows *which* of the presented individuals receives her donation instead of knowing that *one* of the individuals will receive the donation.

In addition to identifiability, some scholars argue that the "identifiable victim effect" could result from a preference for concentrated over dispersed donations. Papers which study this second hypothesis typically compare donations to single identified recipients and groups of fully identified recipients. An important feature of these experiments is that donations to the group are either equally disbursed among the recipients or framed as contributions to a public good. A common finding is that single identified recipients receive larger donations than groups of fully identified recipients (Kogut and Ritov, 2005a,b). These results suggest that even in the absence of informational differences between single recipients and groups, donors prefer concentrated distributions to a single recipient over equal distributions to a group of recipients. Scholars have pointed out that this preference could results from the fact that concentrated distributions generate a higher perceived impact (Baron, 1997; Jenni and Loewenstein, 1997).

The purpose of this paper is to provide controlled laboratory evidence on how both explanations, identifiability as well as the distribution of the donation, contribute to the "identifiable victim effect". I will analyze the two explanations in a comparable choice task in which I control the amount of information. Therefore, differences in donations cannot reflect the fact that donors give more as a result of having better information about recipients.

For this purpose, I run experiments in which student subjects can make donations to finance school attendance and lunches for children in Uganda. Each subject is matched to a group of three children. Photos of all three children are presented to the subject prior to her first donation decision such that she has the same kind of individuating information about all three children.

In the *Identified versus Unidentified (IvU) Recipient Treatment*, subjects take two types of donation decisions (one of which is randomly selected for payment): In the first type, subjects can donate to one of the children which has been randomly selected as the recipient before the donation decision. Hence, in this type of choice situation, the recipient is *identified*. In the second type of decision, one of the three children is randomly selected as the recipient only after subjects have entered their donation. Subjects are not informed which child has been chosen. Hence, I will say that the recipient is *unidentified* in this situation, given that subjects do not know *which* of the three children has been selected as the recipient. By comparing donations across these two kinds of decisions, I can isolate the mere effect of identifiability on donations.

If subjects indeed donate more to identified as compared to unidentified recipients in this treatment, this is clear evidence in favor of the hypothesis that identifiability has an added value independent of individuating information. In contrast, not observing this effect implies that identifiability *per se* does not have an effect on donations. If so, the differences in donations to identified and anonymous recipients, observed in previous studies, could be better explained by the donor's response to information.

I run a second treatment, in which I vary whether the donation is disbursed to a single child or equally shared among the three children. In this *Single Identified Recipient versus Multiple Identified Recipients (SvM) Treatment*, I again confront subjects with two kinds of decisions: The first kind of decision involves a *single identified* recipient, i.e. subjects can donate to a child selected as the recipient prior to their donation decision. Hence, this decision is exactly the same as in the IvU treatment. In the second kind of situation, every amount donated will be equally divided among the three children. Hence, in this decision subjects can make a donation to *multiple identified* recipients.

If subjects indeed donate more to a single identified recipient as compared to a group of identified recipients, this constitutes evidence in favor of the claim that donors prefer concentrated over dispersed distributions of their donation, observed in previous studies by Kogut and Ritov (2005a,b). Not observing this pattern, in contrast, is inconsistent with a preference for more concentrated distributions.

Finally, by comparing the results across the IvU and the SvM treatment, I can explore the joint role of identifiability as well as the distribution of the donation in explaining a bias towards identified single recipients. In particular, I can assess whether both factors contribute to the "identifiable victim effect" (if subjects donate more to identified versus unidentified and to single recipients versus groups) or whether a single factor explains this phenomenon as soon as we control for the information available to the donor.

An important question is whether donors choose to observe the recipient of their donation or actively avoid exposure to single identified recipients. Several studies have found that people tend to avoid situations in which they feel especially compelled to give (DellaVigna et al., 2012; Andreoni et al., 2017). These papers, however, only vary the solicitation method while the characteristics of the charity program are naturally held constant. In contrast to this explanation, donors might actively seek charities which reveal the identity of the donor if identifiability increases the perceived impact or warm glow of giving.

I explore this question by matching subjects in both treatments with a new group of three children from Uganda after they have completed the treatments described above. Before subjects can enter a donation, they are asked to choose between two modes of donating. In the IvU treatment, subjects can decide whether they want to donate to an identified recipient, randomly chosen prior to their donation decision, or whether one of the children should be selected without them being informed of the recipient's identity. Respectively, subjects in the SvM treatment can choose whether their donation should be disbursed to a single child (randomly selected before their donation decision) or whether their donation should be equally shared among the three children.

The results are as follows: In the IvU treatment, I do not find that subjects make higher donations to identified as compared to unidentified recipients. This result holds within-subjects (when comparing the choices of the same subjects in the two kinds of donation decisions) as well as between-subjects (when comparing the first choices in the two kinds of decisions across sessions). Therefore, my findings suggest that identifiability *per se* has no effect on donations. Hence, the differences in donations to identified and anonymous recipients, observed in previous studies, can be solely attributed to the fact that donors give more, the more vivid details they have about the recipient.

In the SvM treatment, I find that subjects donate more when their donation is equally shared among the three identified recipients, instead of being allocated to a single identified recipient. As for the other treatment, I can confirm this result within- as well as between-subjects. These results are inconsistent with previous studies. A potential explanation (discussed in more detail in the conclusion) is that single identified recipients only receive higher donations if the group is large enough and / or if subjects have a relatively small endowment. I conclude that the results of this paper might be taken to suggest that there is no general bias towards identified single recipients which is independent of the choice details.

Finally, my results suggest that a majority of subjects choose the mode in which they previously donated more in both treatments. Hence, I do not observe that subjects "avoid the ask". Nevertheless, I observe that subjects are less likely to choose donating to single identified recipients in both treatments, even if they previously donated the same amount to an identified single recipient and an unidentified recipient (in the IvU treatment) or to a group involving three identified recipients (in the SvM treatment).

The rest of the paper is organized as follows: Section 3.2 provides an overview of the related literature. The experimental design and the hypothesis are summarized in Section 3.3. Section 3.4 presents the results. I conclude and discuss the results in section 3.5.

3.2 Related Literature

The present paper contributes to the literature analyzing how being able to identify the recipient of one's donation affects charitable giving. Several studies in this literature compare donations to single identified and otherwise anonymous recipients. In these studies, recipients are either identified by vivid and individuating information (Jenni and Loewenstein, 1997) or by a photo that is displayed to the donor (Kogut and Ritov, 2005a,b; Genevsky et al., 2013).[2] A common finding in this literature is that identified recipients receive larger donations than anonymous recipients.

Several authors have linked the "identifiable victim effect" to the fact that subjects experience greater emotional arousal and empathy for identified as compared to anonymous recipients (Jenni and Loewenstein, 1997; Kogut and Ritov, 2005a; Slovic, 2010). This greater emotional arousal may reflect that donors experience more sympathy and distress the more vivid information they have about the recipient (Genevsky et al., 2013). Consistent with this view, Jenni and Loewenstein (1997) and Kogut and Ritov (2005a) find that donations increase in the amount of details provided about the recipient. Furthermore, Cryder et al. (2013) report evidence that donors give more to interventions (not involving an identified victim) the more vivid the information about the intervention. Hence, these studies provide evidence that information tends to increase generosity.

Some scholars, however, argue that at least a part of the greater emotional arousal can be attributed to identifiability (Kogut and Ritov, 2005a,b). According to this view, a subject is more likely to adopt the view of a recipient if the recipient is identified. Related studies find that subjects which project themselves into another person in need, i.e. by trying to imagine how that person feels, experience greater empathy and, consequently, are more willing to help that person (Batson, 1987; Batson et al., 2016). This link between empathy and altruism is referred to as the "empathy-altruism theory" (Batson, 1987). Similarly, a study by Redelmeier and Tversky (1990) finds that doctors recommend more treatments when primed to think about a patient as a single individual instead of being part of a group of patients with similar symptoms. A recent study by Dickert et al. (2009) suggest a different channel, namely that single individuals attract more attention and evoke greater sympathy, both of which might

[2] Small and Loewenstein (2003) find that subjects share greater amounts of their endowment in a dictator game if the recipient is already determined when the dictator chooses an allocation instead of being randomly selected from a pool of recipients after the dictator has made a choice. Thus, their paper explores determinedness as a source of vivid information. In a later paper, Cryder and Loewenstein (2012) attribute their findings to increased feelings of responsibility in cases where the dictator's decision determines the recipient's payoff.

increase the donor's generosity.

These results suggest that identifiability has an effect which can be separated from the effect of vivid and individuating information. For example, imagine that a donor observes a group of recipients, all of which are presented to the donor by a photo. Naturally, a donor might be willing to donate more to a randomly drawn member in this group (in which each member is presented by a photo) as compared to a randomly drawn member from a second group for which she has no individuating information. This increase in donations is attributable to the effect of information. Nevertheless, she might give even more if she can observe *which* individual receives her donation as compared to knowing that *one* of the presented individuals will receive the donation. This increase reflects the *mere value* of identifiability, i.e. being able to *observe* the recipient. A possible explanation for this effect, as formulated by the empathy-altruism hypotheses, is that a donor might feel more empathy for an identified recipient, given that it is easier to adopt the view of a *concrete* individual as opposed to adopting the view of a *potential* recipient.

A few recent studies suggest an alternative explanation for the "identifiable victim effect", namely that donors prefer a more concentrated distribution of their donation. For example, Kogut and Ritov (2005a,b) run experiments in which subjects are matched to a single child, identified by a photo, or a group of eight children, all of which are identified by a photo. Both studies find that a single identified recipient receives more donations than a group of fully identified children. An important feature of these experiments is that every dollar donated to the group is used to finance a public good which will be provided to all children in the group. Hence, these studies suggest a separate channel for the "identifiable victim effect", namely that donors prefer to donate to a single recipient instead of disbursing their donation among multiple recipients. Related research suggests that donors judge concentrated interventions (with a smaller number of recipients) as generating a higher impact (Baron, 1997; Jenni and Loewenstein, 1997). Thus, the larger amount donated to single identified individuals could be explained by the fact that people perceive a dollar in the pocket of one individual as generating a higher impact compared to disbursing the dollar to a potentially large group.

This paper also contributes to the "avoiding the ask" literature. Several papers show that people tend to avoid situations in which they feel especially compelled to give (Dana et al., 2006; Broberg et al., 2007; Dana et al., 2007; Andreoni and Rao, 2011; DellaVigna et al., 2012; Andreoni et al., 2017). Hence, donors might avoid being exposed to identified recipients and instead choose programs which do not reveal the recipient's identity. On the other hand, identifiability could increase the perceived impact or warm glow of donating, such that people might actively choose programs in which they can identify the recipient.

Furthermore, this paper contributes to a broader literature, examining the role of identifiability in strategic and non-strategic interactions. Most of these papers highlight the role of identifiability in providing information. Mobius and Rosenblat (2006) show that subjects rated as more attractive receive higher wages in an experimental labor market. Brosig et al. (2003) find that identification of one's partner does not increase cooperation relative to a standard four person public good game. However, groups reach almost full cooperation when they are allowed to communicate via a video chat function. Although all of these papers attribute the observed effects to the information contained in a photo, being able to observe one's partner could have an effect that goes beyond the informational value that the decision maker extracts from the photo.

3.3 Experimental Design and Hypotheses

The experiment started with a questionnaire and subjects were promised a fixed payment of 15 EUR for answering the questions.[3] After completing the questionnaire, subjects were informed that they could donate a share or their entire payment to Abaana e.V., a German charity supporting school children in Uganda.[4] Subjects were informed that all donations collected in the experiment would be used to finance school attendance as well as school lunches for individual children. Each subject took five donation decisions, one of which was implemented for each subject individually. Subjects were paid 15 EUR minus their donation in the randomly chosen situation (if any).

For the first four donation decisions, I matched each subject to a group of three children. The photos of all three children were displayed on the subject's computer screen prior to the first donation decision. Subjects were informed that they could make a donation to one or several of the displayed children.

To investigate the impact of identifiability as well as the distribution of benefits on donations, I assigned subjects to one of two treatment conditions:

In the *Identified versus Unidentified Recipient (IvU)* treatment, I present each subject with two kinds of decision situations. In the first situation, one of the three children was selected as the recipient before subjects made their choice. A photo of the child was displayed on the decision screen where subjects could enter their donation. Subjects

[3] The questionnaire contained questions from the German Socioeconomic Panel (SOEP) as well as the Big Five Personality questionnaire. On average, it took subjects 8 minutes to answer all questions.

[4] Subjects received information about the charity's purpose on their decision screen and could browse through a brochure issued by Abaana e.V.

were informed that the child displayed on the screen would receive the entire amount donated. Thus, I will say that the recipient is *identified* in this choice situation. In the second situation, one of the three children was selected as the recipient only after subjects entered their donation. On the decision screen, where subjects could enter their donation, they could see the pictures of all three children. Subjects were informed that one of the displayed children would receive the entire donation but that they would not be informed which child had been selected. Hence, I will say that the recipient is *unidentified* in this situation given that the donor does not know which of the three displayed children will receive her donation.

In order to assess whether subjects would donate the same amounts to all three children in their group, each child was presented as identified recipient once (in randomized order). Across sessions, I varied whether the situation involving an unidentified recipient was presented as the first or the second decision. Therefore, I can compare the effect of identifiability within-subjects (by comparing a subject's choice across the two kinds of situations) and between-subjects (by comparing the choices of subjects whose first choice involved an identified versus an unidentified recipient).

According to the existing evidence, discussed in the previous section, we would expect that subjects experience more empathy towards an identified recipient as compared to an unidentified recipient, given that it is easier to adopt the position of a concrete as opposed to a potential recipient. If we indeed observe this pattern, this is unambiguous evidence in favor of the hypothesis that the mere possibility of being able to identify the recipient increases donations. Instead, not observing this pattern, implies that identifiability does not have an effect on donations as soon as differences in information about identified and unidentified recipients are eliminated.

Hypothesis 1. *In the IvU treatment, subjects donate more to an identified than to an unidentified recipient.*

To explore the role of the distribution of donations, I conduct a second treatment. In this *Single Identified Recipient versus Multiple Identified Recipients (SvM)* treatment, subjects were confronted with two kinds of choice situations: The first situation was the same as in the IvU treatment. Namely, subjects could donate to a child selected as the recipient before their donation decision. Hence, there is a *single identified* recipient in this situation. In the second situation, the photos of all children were displayed on the decision screen. Subjects were informed that their donation would be equally divided among all three children. Hence, this situation involved a donation to a group of *multiple identified* recipients.

To assess whether subjects want to donate the same amount to all three children, each child was presented single identified recipient once (in randomized order). Across sessions, I varied whether the first choice involved a single recipient or a group of three recipients. Therefore, I am able to analyze the effect of the distribution on donations within-subjects (by comparing a subject's donations across the two choice situations) and between-subjects (by comparing donations of subjects whose first choice involved a single recipient versus a group of recipients).

Based on the existing evidence discussed above, we would expect subjects to believe that their donation will generate a higher impact if it is disbursed to a single recipient instead of being equally shared among a group of three recipients. Hence, we would expect that subjects donate more to a single identified recipient than to a group of three identified recipients. If we find this effect, then this is evidence in favor of the hypothesis that donors prefer more concentrated divisions of their donations. Instead, not observing this effect would be inconsistent with such a preference.

Hypothesis 2. *In the SvM treatment, subjects donate more to a single identified recipient than to a group with multiple identified recipients.*

Finally, to analyze whether subjects avoid situations in which they feel compelled to give more, I implement a fifth choice. Prior to their decision, subjects were matched to a new group of three children.[5] The pictures of all three children were displayed on the computer screens. In the IvU treatment, subjects could then choose whether they wanted to donate to an identified or an unidentified recipient. In the SvM treatment, subjects could choose whether they wanted to donate to a single child or to the group of three children. If a subject selected the first option, a photo of the child selected as the recipient was displayed on the decision screen where subjects could enter their donation.[6] Subjects who chose the second option saw the pictures of all three children on the decision screen.

In line with the literature on "avoiding the ask", we would expect that subjects in the IvU treatment are less likely to select an identified recipient instead of an unidentifed recipient. Similarly, subjects in the SvM treatment should be less likely to select a single identified recipient instead of selecting a group with three identified recipients.

Hypothesis 3. *In the IvU treatment, subjects less often choose to make a donation to identified as compared to unidentified recipients, thereby avoiding to make higher donations.*

[5] This group had not been matched to any other participant in the previous donation decisions.

[6] It was emphasized to the participants that they would not be able to choose the recipient of their donation.

Hypothesis 4. *In the SvM treatment, subjects less often choose to make a donation to single as compared to groups of recipients, thereby avoiding to make higher donations.*

To ensure that choices and payments are anonymous, each subject secretly drew a card with a four-digit participant code at the beginning of the experiment. Subjects had to enter the code in order to start the experiment and were instructed to keep the card until the end of the experiment. All choices and the final payment were recorded under this participant code. The experimenter could not attribute choices and payments to a specific person. At the end of the experiment, the experimenter placed the final payments (15 EUR - donation in the randomly chosen situation) in envelopes numbered with the corresponding participant code and left the room. At this point, one randomly selected subject was instructed to call the other participants to the front desk and supervise that the code on the card drawn at the beginning of the experiment matched the envelope collected by the participant. The payment procedure was explained in detail before subjects made the donation decisions.

An important concern is that subjects might prefer some of the children they are matched to. Thus, they might donate more if their preferred child is presented as the single identified recipient in both treatments. We might falsely attribute this behavior to signal a preference for identified as compared to unidentified (in the IvU treatment) or as a preference for a single as compared to multiple recipients (in the SvM treatment). I tried to limit this concern by forming the groups such that the three children had the same gender and approximately the same age. In addition, I matched the children in terms of their overall appearance, such as their clothes and the background against which the photo was taken. Furthermore, to ensure that the fifth decision is comparable to a subject's previous donation decisions, subjects were assigned two very similar groups (see Figure 3.1 as an example). Finally, to ensure that choices are comparable across treatments, I used the same groups of children in every session.

Another concern is that subjects might want to behave consistent across the different choice situations. Suppose that a subject would want to donate more to an identified as compared to an unidentified child. However, this type of behavior might appear inconsistent if subjects face both decisions sequentially. Hence, a preference to make consistent choices could eliminate a possible within-subject treatment effect. To limit the extent of such a consistency bias, I did not inform subjects how many donation decisions they would take. In addition, I can conduct between-subject tests by comparing the first choices of subjects across sessions. Given that subjects did not know which choices they would take, the between-subject evidence should not be affected by

Figure 3.1: Example of groups assigned to one subject

(a) First group

(b) Second group

a preference to make consistent choices.

Finally, subjects might want to avoid being exposed to photos of needy children, thus rapidly entering a donation in order to leave the decision screen. To prevent subjects from avoiding the photo, subjects could enter and confirm their choice only after 15 seconds had passed.[7]

3.4 Results

The experiments were conducted in June 2018 at the University of Heidelberg Lab. In total, I recruited 125 students from all disciplines via Hroot (Bock et al., 2014). The experiment was programmed in z-Tree (Fischbacher, 2007) and subjects received all instructions (reproduced in Appendix 3.2) on their decision screen. I conducted 4 sessions for each treatment, involving a total of 62 subjects in the IvU and 63 in the SvM treatment.[8]

[7] Total decision times were also recorded but not used in the empirical analysis.

[8] Each session was planned for 16 subjects. Due to no-shows, one of the sessions in the IvU treatment involved only 14 subjects, and one session in the SvM treatment involved only 15 subjects.

3.4.1 Identified versus unidentified recipient

I will start with a discussion of the within-subject effect of identification, observed in the IvU treatment. In this treatment, subjects took 4 choices, 3 of which involved an identified recipient and one choice involving an unidentified recipient. To analyze the effect of identifiability on donations, I compare a subject's average donation to the three identified children and the same subject's donation to an unidentified child. On average, the mean donation to the three identified children was 3.83 EUR and the unidentified child received an average donation of 3.84 EUR. Using a sign test, I find that there is no statistical difference between a subject's average donation to the three identified recipients and her donation to an unidentified recipient ($N = 62$, $p = 0.32$). This finding is inconsistent with Hypothesis 1.

Given that donating to an unidentified recipient is essentially a lottery in which the computer chooses the recipient, participants may want to give less if the donation is randomly allocated than if they know that it will be allocated to their "favorite" child. Moreover, if they are risk-averse, donations to an unidentified child should be less than their average donation to the three identified children. In order to look more closely at within-sample differences, I will distinguish between subjects who donated the same and those who donated different amounts to the three identified recipients in the following.

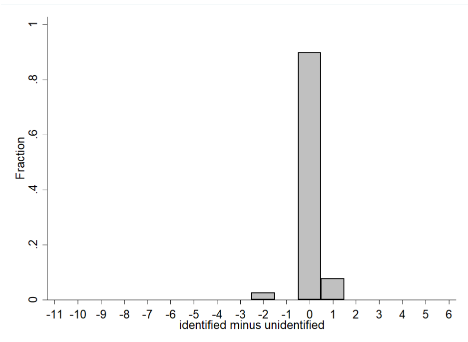
Overall, 63% of subjects donated the same amount to all three identified children. Panel (a) in Figure 3.2 depicts the difference in donations to the three identified and the unidentified recipient for these subjects. If the mere possibility of being able to identify the recipient increases donations, this difference should be positive on average, reflecting a tendency to give more when the recipient is identified. Clearly, this is not the case given that almost all subjects donated the same amount to identified and unidentified recipients (Sign Test, $N = 39$, $p = 1$). Only 5% of subjects exhibit behavior which is consistent with the identified victim effect and donate more to each identified recipient than to the unidentified recipient. As above, this evidence is inconsistent with Hypothesis 1.

In total, 37% of subjects did not donate the same amount to all three children. I run random effects Tobit regressions confirming that differences in donations to the three children are not explained by order effects[9] nor is it the case that some children

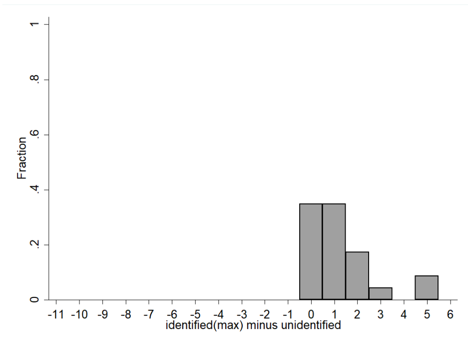
[9] To check for order effects, I run random effects Tobit regressions with sum donated as the dependent and dummies controlling for whether an identified recipient was presented first or second. The regression results show that donations to children presented as identified recipients last do not differ from donations to children presented as identifiable recipients first (Marginal effect $\beta_1 = -0.11$, $\sigma_1 = 0.27$, $p = 0.67$) or second (Marginal effect $\beta_2 = 0.22$, $\sigma_2 = 0.27$, $p = 0.41$).

Figure 3.2: Difference in Donations to identified versus unidentified recipients (Within-subjects)

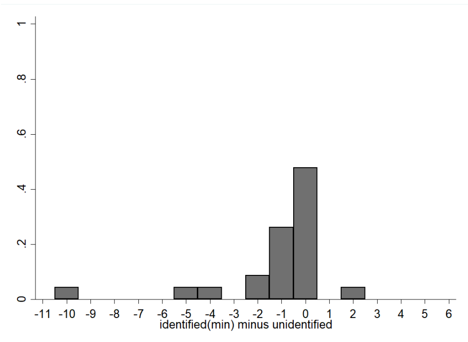
(a) Same donations ($N = 39$)



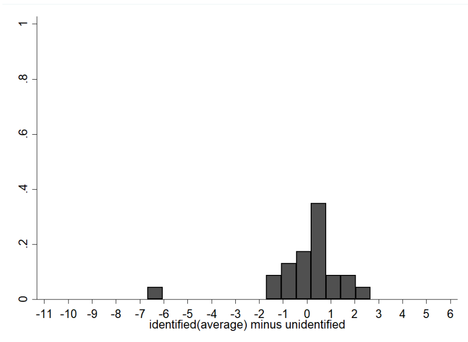
(b) Different donations - Max ($N = 23$)



(c) Different donations - Min ($N = 23$)



(d) Different donations - Mean ($N = 23$)



Note: This figure displays the difference in a subject's donation to an identified versus an unidentified recipient. Panel (a) displays this difference for all subjects which chose to make the same donations to the three identified recipients. Panels (b)-(d) display information for subjects which did not make the same donation to the three identified recipients. Therefore, these panels display the difference between a subject's maximum (b), minimum (c) and average donation (d) to an identified minus her donation to an unidentified recipient.

receive higher donations by all subjects they are matched to.[10] These results suggests that differences in donations to the three children are explained by some unobserved preference which varies at the individual level.

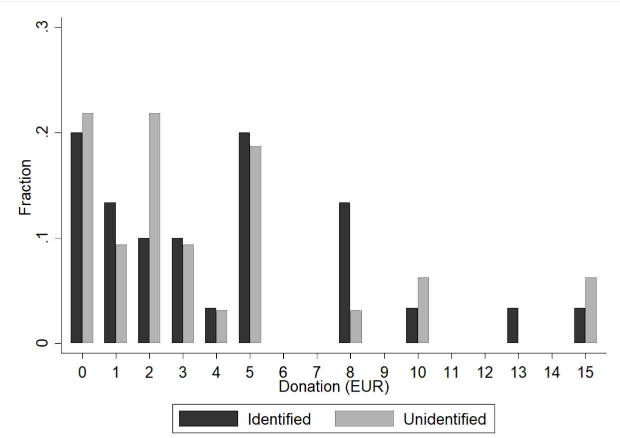
The evidence for these subjects is depicted in panels (b) to (d) where I plot the difference between a subject's maximum, minimum and average donation to the three identified recipients and her donation to the unidentified recipient. As can be inferred from panel (b), the maximum donation to an identified recipient is either the same or higher than the amount donated to an unidentified recipient for all subjects. Using a Sign test, I confirm that this pattern is statistically significant ($N = 23$, $p < 0.001$). Although this might be interpreted as evidence in favor of Hypothesis 1, the same observation is consistent with a preference to donate to one's "favorite" child. Hence, the observed difference cannot be unambiguously attributed to identifiability. Furthermore, the effect is, reversed in panel (c) where I plot the difference in a subject's minimum donation to an identified recipient and her donation to an unidentified recipient. Here, almost all subjects donate either the same or less to an identified versus an unidentified recipient, the pattern again being statistically significant (Sign Test, $N = 23$, $p = 0.01$). Given that this pattern is consistent with the assumption that subjects want to donate more to the lottery than to their least "favorite" child, we cannot reject Hypothesis 1 based on this evidence. In particular, a positive effect of identifiability may be overridden by a preference to give less to one's least "favorite" child in these cases. What can, however, be said is that only few subjects exhibit behavior which could lead us to conclude that the latter (negative) effect is smaller than a positive effect attributable to identifiability. Namely, only 5% of subjects donated a higher minimum amount to an identified as compared to an unidentified recipient. Finally, panel (d) plots the difference between average donations to the three identified versus the (one) donation to an unidentified recipient. Clearly, this difference is distributed almost symmetrically around zero and I do not find any statistically significant difference (Sign test, $N = 23$, $p = 0.56$).

To summarize, the evidence reported in panels (b) to (d) is consistent with the assumption that subjects donate more to their preferred identified recipient than to an unidentified recipient (see panel (b)), and that they donate less to their least preferred recipient than to an unidentified recipient (see panel (c)).

In conclusion, the within-subject results cast clear doubt on the hypothesis that people give more to identified than to unidentified recipients.

[10] I run a separate set of random effects Tobit regressions the sum donated as the dependent and dummies for each child as independent variables. None of the child dummies is significant at the 5% level.

Figure 3.3: Donations to identified versus unidentified children (Between-subjects)



Note: This figure displays the distribution of donations among subjects whose first choice in the experiment involved an identified recipient (black bars) versus the distribution of donations among subjects whose first choice involved an unidentified recipient (gray bars).

Result 1a. *When comparing the donation decisions of each subject, I do not find that subjects donate more to an identified than to an unidentified recipient (inconsistent with Hypothesis 1).*

In what follows, I provide evidence on the between-subject effect of identifiability on donations. Thereby, I can address whether the lack of observing evidence in favor of Hypothesis 1 is explained by a preference to make consistent choices. Assume that subjects did want to donate higher amounts to the three identified recipients than to the one unidentified recipient. This behavior might, however, appear inconsistent when subjects face both decisions sequentially. Hence, a preference to make consistent choices in this setting could eliminate a possible within-subject treatment effect.

To address this concern, Figure 3.3 plots the distribution of donations among subjects whose first choice involved an identified versus subjects whose first choice involved an unidentified recipient. As can be seen, there are some noticeable differences in the two distributions. Namely, identified recipients are more likely to receive a donation of 8 EUR than unidentified ones. In turn, more subjects chose to donate 2 EUR to an unidentified as compared to an identified recipient. However, using a Ranksum test, I do not find any significant differences in the donations to identified versus unidentified recipients ($N = 62$, $p = 0.61$). Thus, in line with the within-subject evidence reported above, the between-subject results cast doubt on the notion that people donate more if they can identify the recipient of their donation.

Result 1b. *When comparing the first donation choices of subjects across sessions, subjects whose first choice involved an identified recipient do not donate more than subjects whose first choice involved an unidentified recipient (inconsistent with Hypothesis 1).*

3.4.2 Single versus multiple recipients

In what follows, I will describe the results of the SvM treatment, starting with the within-subject evidence. Subjects took 4 donation decisions, 3 of which involved a single and identified recipient and 1 choice in which the donation was equally divided among all three identified children. I start by comparing a subject's average donations to the three single children and the donation to the group. Averaged over all subjects, the mean donation to the three single recipients was 3.30 EUR and 4.30 EUR to the group of recipients. Using a Sign test, I find that the average donation to the three single recipients was indeed significantly smaller than the donation to the group of three recipients ($N = 63$, $p = 0.001$). Averaged over all subjects, I find that the donation to the group exceeds the average donation to the three identified recipients by 60%. This evidence is inconsistent with Hypothesis 2.

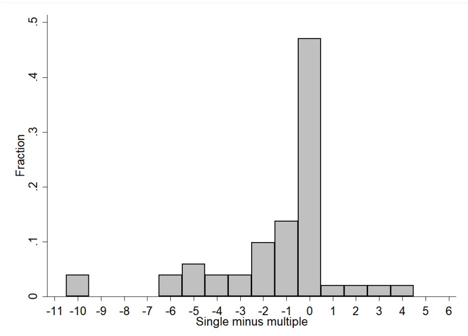
As for the IvU treatment, the interpretation of results crucially depends on whether subjects are indifferent between the three children they are matched to. Namely, a subject might donate more if she knows that her donation will be disbursed to her "favorite" child instead of being equally divided among the three children (with her "favorite" child receiving only a third of the total amount donated). Hence, I will distinguish between subjects who donated the same and those who donated different amounts to the single recipients in the subsequent analysis.

Overall, 81% of subjects chose to make the same donation to all three single and identified recipients.[11] Panel (a) in Figure 3.4 displays the difference in donations to each single recipient and the donation to the group of three children (each of which will receive a third of the total amount donated). Contrary to Hypothesis 2, almost half of the subjects donate the same (total) amount to a single child and the group of three children and 42% of the subjects donate *higher* amounts to the group. Using a Sign Test, I can reject the hypothesis that subjects donate the same amount to a single recipient and a group with three recipients (Sign Test, $N = 51$, $p < 0.001$). Hence, the evidence in panel (a) is inconsistent with the hypothesis that donors give more to

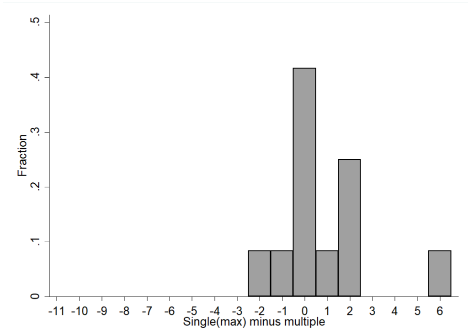
[11] This percentage is significantly higher than in the IvU treatment (Ranksum, $N = 125$, $p = 0.02$). A possible explanation for this difference is that enforcing an equal division of the funds decreases the likelihood of donating different amounts to subsequent identified recipients. Given that the option to donate to the group was presented either as the first or the second decision, I cannot test this explanation.

Figure 3.4: Difference in donations to Single Identified versus Multiple Identified recipients (Within-subjects)

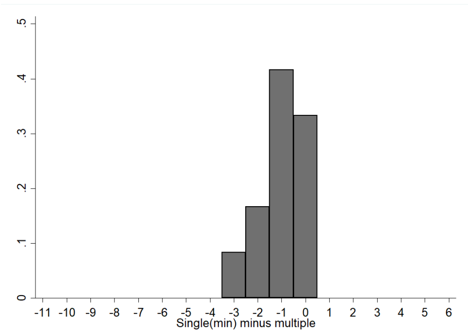
(a) Same donations ($N = 51$)



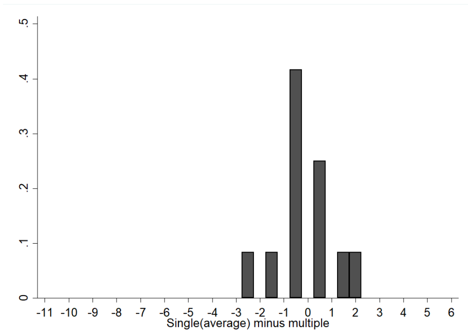
(b) Different donations - Max ($N = 12$)



(c) Different donations - Min ($N = 12$)



(d) Different donations - Mean ($N = 12$)



Note: This figure displays the difference in a subject's donation to a single (identified) recipient versus a group of three (identified) recipients. Panel (a) displays this difference for all subjects which chose to make the same donations to all three single recipients. Panels (b)-(d) display information for subjects which did not make the same donation to all three single recipients. Therefore, these panels display the difference between a subject's maximum (b), minimum (c) and average donation (d) to a single recipient minus her donation to the group of three recipients.

a single identified recipient as compared to a group with three identified recipients. Indeed, the opposite seems to be true.

The remaining 19% of subjects chose to donate different amounts to the three single recipients. As in the IvU treatment, I run random effects Tobit regressions allowing me to asses whether these differences are explained by order effects (i.e. identified recipients presented first receive higher donations than identified recipients presented last) or by the fact that certain children received higher donations than others. The regressions results confirm that differences in donations cannot be attributed to the latter explanation. However, I find that children presented as single recipients first receive significantly larger donations than those presented last.[12] A subsequent analysis, however, revealed that excluding a subject's first choice does not change the size or significance of any treatment effect (see Appendix 3.1).

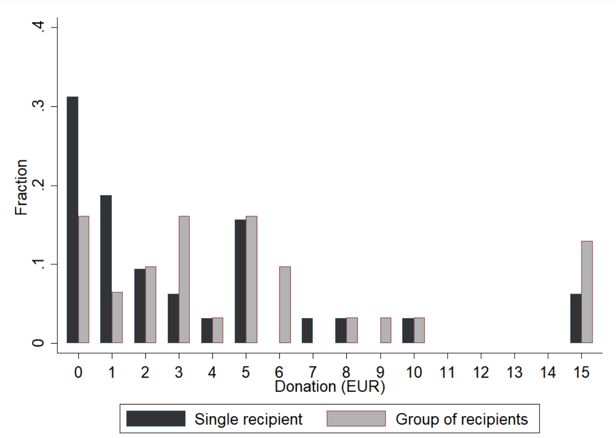
Panels (b) to (d) summarize the evidence by plotting the difference in a subject's maximum, minimum and average donation to the three single recipients minus her donation to the group of recipients. Panel (b) depicts the difference in a subject's maximum donation to a single recipient and her donation to the group of three recipients (among which the donation will be equally shared). As can be seen there, 42% of subjects donate the same amount to a single recipient and a group of three recipients. Another 42% of subjects donate strictly more to a single child. Based on this evidence, I cannot reject the hypothesis that subjects donate the same amount to single recipients and the group (Sign Test, $N = 12$, $p = 0.45$). This result is surprising given that we would expect that subjects prefer donating to a single child, even more so to their "favorite" child. Hence, this result cannot be consistent with Hypothesis 2. The data pattern is much clearer in panel (c), where a majority of subjects donated less to at least one single recipient than to the group. This difference is strongly statistically significant (Sign Test, $N = 12$, $p = 0.01$). However, given that this pattern could also be explained by the fact that subjects may want to donate more to the group than to their least "favorite" child, we cannot reject Hypothesis 2 based on this evidence. As depicted in panel (d), there are no differences in average donations to single recipients and groups (Sign Test, $N = 12$, $p = 0.77$).

In conclusion, the results do not confirm the hypothesis that single recipients receive larger donations than groups of recipients.

Result 2a. *When comparing the donation decisions of each subject, I do not find that*

[12] To check for order effects, I use the donation amount as dependent variable and include dummies controlling for whether a single recipient was presented first or second. The results show that children presented as single recipient first receive significantly larger donations (Marginal effect, $\beta_1 = 1.25$, $\sigma_1 = 0.32$, $p = 0.01$) but children presented second do not receive more ($\beta_2 = 0.78$, $\sigma_2 = 0.32$, $p = 0.25$) as compared to the child presented last.

Figure 3.5: Donations to single versus multiple recipients (Between-subjects)



Note: This figure displays the distribution of donations among subjects whose first choice in the experiment involved a single (identified) recipient (black bars) versus subjects whose first choice involved a donation to the group of three (identified) children (gray bars).

subjects donate more to a single identified recipient than to a group of three identified recipients (inconsistent with Hypothesis 2). Instead, I find that groups receive significantly higher donations than single recipients.

Finally, Figure 3.5 summarizes the between-subject evidence by plotting the distribution of donations among subjects whose first choice involved a single recipient versus subjects whose first choice involved a group of three recipients. In this figure, the distribution of donations to groups appears to be shifted to the right. Most notably, groups are less likely to receive no donations than single recipients while at the same time being more likely to receive the highest possible donation of 15 EUR. Using a Ranksum test, I find that there is a significant difference in donations received by a single recipient and those received by the group of three recipients ($N = 63$, $p = 0.05$). Hence, the between-subject evidence does not confirm the hypothesis that single recipients receive higher donations than groups of recipients either.

Result 2b. *When comparing the subjects' first donation decision across sessions, I do not find that subjects donate more to a single identified recipient than to a group with three identified recipients (inconsistent with Hypothesis 2).*

3.4.3 Preferred mode of donating

In this section, I will report the results from the fifth donation decision. For this decision, subjects were matched to a new group of three children and asked to choose

Table 3.1: Donation Mode chosen in decision 5 (IvU Treatment)

Decisions 1-4 Donations	Number of observations	Decision 5 Chosen mode	
		Identified	Unidentified
Identified (avg) > Unidentified	16 (23%)	3 (19%)	13 (81%)
Identified (avg) = Unidentified	36 (58%)	11 (31%)	25 (69%)
Identified (avg) < Unidentified	10 (16%)	1 (10%)	9 (90%)
	62	15	47

the mode in which they would like to donate. This allows me to assess whether subjects avoid modes of donating in which they might feel compelled to give more.

First and most notably, a majority of subjects in the IvU treatment (47 out of 62 subjects) chose to donate to an unidentified recipient. Using a proportions test, I can reject the hypothesis that both modes were chosen with the same probability (Proportions Test, 76 versus 24%, $N = 62$, $p = 0.15$). This pattern is inconsistent with the first part of Hypothesis 3. What is surprising about this result is that although most subjects donated the same amount to identified and unidentified recipients (and might, thus, be indifferent) in the previous part of the experiment, there seems to be a preference for unidentified recipients.

To analyze individual choices in more detail, Table 3.1 depicts the chosen mode of donating in decision 5 (columns) by the difference in average donations to the three identified recipients and the unidentified recipient observed in decisions 1-4 (rows). Hence, if subjects "avoid the ask" we should see that they are more likely to select the mode in which they previously donated less.[13]

Three patterns are immediately visible: First, a majority of subjects which previously donated the same average amount to the three identified recipients and the one identified recipient, chose to donate to an unidentified recipient in decision 5. If these subjects were indifferent, we would instead expect that each option is chosen with 50% probability. Using a Proportions Test, I can reject the hypothesis that both options are chosen with the same probability (31 versus 50%, $N = 36$, $p = 0.02$). Hence, there

[13] It should be pointed out that a subject's actual donation in decision 5 (after she has selected a mode) is strongly correlated with her previous donation in the respective mode: A Tobit regression with the donation amount in decision 5 as the dependent and the previous donation in the same mode as dependent variable finds that the previous donation is strongly predictive of a subject's donation in decision 5 ($\beta = 0.93$, $p < 0.0001$). In a separate regression, I also include the selected mode as a separate independent variable. I do not find that this changes the explanatory power of previous donations on donations in decision 5 ($\beta = 0.93$, $p < 0.0001$).

Table 3.2: Donation Mode chosen in decision 5 (SvM treatment)

Decisions 1-4 Donations	Number of observations	Decision 5 Chosen mode	
		Single Recipient	Group of Recipients
Single (avg) > Group	9 (14%)	3 (33%)	6 (66%)
Single (avg) = Group	24 (38%)	0 (0%)	24 (100%)
Single (avg) < Group	30 (48%)	4 (13%)	26 (87%)
	63	7	56

appears to be a preference for unidentified recipients even among subjects which previously made the same donations. Second, among subjects which previously donated higher average amounts to the three identified recipients than to the unidentified recipient, only 19% of subjects chose an identified recipient. This implies that a majority of these subjects "avoid the ask". However, this might simply reflect a general preference for an unidentified recipient, as displayed in row 2 of the table.[14] In contrast, subjects which previously donated more to an unidentified recipient do not "avoid the ask". As displayed in the table, 90% of these subjects choose to donate to an unidentified recipient in decision 5.

In conclusion, the data pattern observed is not consistent with the hypothesis that subjects "avoid the ask". However, the results reveal a strong preference for unidentified recipients, even among subjects which previously donated the same amounts to unidentified and identified recipients.

Result 3. *I do find that subjects are less likely to choose to donate to an identified as compared to an unidentified recipient. However, most subjects which choose to donate to an identified recipient previously donated the same or an even higher amount to an unidentified as compared to an identified recipient. Hence, there is no evidence that subjects "avoid the ask" (inconsistent with Hypothesis 3).*

The corresponding evidence for the SvM treatment is summarized in Table 3.2. First and most notably, I observe that a large majority of subjects (56 out of 63) chose to donate to the group instead of making a donation to a single recipient. Using a proportions test, I can reject the hypothesis that both modes were chosen with the same probability (89 versus 11%, $N = 63$, $p < 0.0001$). Given the above finding that

[14] Using a proportions Test, I cannot reject that the fraction of subjects which chose an unidentified recipient is the same in rows 1 and 2 (Proportions test, 81 versus 69%, $N = 16$, $p = 0.37$).

subjects donated more to a group as compared to a single recipient, this choice pattern is inconsistent with Hypothesis 4.

A closer look at the individual choice patterns, displayed in Table 3.2, reveals that subjects were more likely to select donating to the group, independent of their previous donation decisions. In particular, all subjects which previously donated the same average amount to the three single recipients and the group, chose to donate to a group in decision 5. A large majority of the remaining subjects also chose to donate to the group. Using a proportions test, I cannot reject the hypothesis that subjects which previously made higher average donations to the three identified recipients than to the group (see row 1), chose both modes with equal probability (33 versus 66%, $N = 9$, $p = 0.30$). Nevertheless, this pattern could be explained by a general preference for the group as compared to the single recipient (see row 2). Perhaps surprisingly, subjects which previously donated more to the group than to the three single recipients (on average) are also significantly more often choose to donate to the group (87 versus 13%, $N = 30$, $p < 0.001$). Hence, as for the other treatment, I do not find evidence that subjects “avoid the ask”.

Result 4. *I find that subjects are less likely to choose to donate to a single identified recipient than to a group of three identified recipients, independent of whether they previously donated more, less or the same average amount to the three single recipients and the group. Hence, there is no evidence that subjects “avoid the ask” (inconsistent with Hypothesis 4).*

3.5 Conclusion

This paper studies behavioral explanations for the “identifiable victim effect”, i.e. the observation that people tend to be more generous to single identified recipients as compared to groups of statistical recipients. In particular, I analyze two common explanations for this phenomenon: identifiability and a preference for concentrated distributions. According to the first hypothesis, donors give more if they can identify the recipient of their donation. The second hypotheses states that donors prefer concentrated donations, disbursed to a single recipient, instead of equal distributions of their donation among a potentially large group of recipients.

I compare the two explanations in a unified choice experiment. Each subject is assigned to a group of three school children from Uganda to which she can make a donation. Prior to the first donation decision, all three children are presented by a photo. Thereby, I can control the amount of information that donors have over the potential recipients.

In the first treatment, subjects can only donate to one of the children. Across choice situations, I vary whether subjects know which child receives their donation (such that the recipient is identified) or whether they only know that *one* of the three children will receive the donation (such that the recipient is unidentified). In contrast to the hypothesis that identifiability should increase donations, I do not find that subjects give more to identified as compared to unidentified recipients in this setting.

In a second treatment, subjects either make a donation to a single identified child or to all three identified children. In the latter case, the amount donated is equally shared among the three (identified) recipients, thus allowing me to analyze how the distribution of the donation affects giving. I find that subjects donate more to the group than to a single recipient. Hence, this result is inconsistent with a preference for concentrated donations, as observed in previous studies.

In addition, I analyze whether subjects tend to avoid single and identified recipients when given the choice between different modes of donating. To answer this question, subjects are matched to a new group of children (whose photos are displayed on the decision screen prior to the decision) and asked to choose between two modes. Subjects in the first treatment can either donate to an identified child or make a donation without knowing which child receives the donation. In the second treatment, subjects can either donate to a single child or make a donation to the group, with the donation being equally divided among the three children in the group. I find that only a minority of subjects chose to donate to a single and identified child in both treatments. Nevertheless, I do not find that subjects "avoid the ask", i.e. strategically choose modes in which they previously donated less.

Given the difference of my own to previous findings, I find it important to point out and discuss possible explanations. Concerning the role of identifiability, my design clearly departs from previous papers which compare donations to vividly identified and otherwise anonymous recipients. Such differences could reflect that donors give more the more vivid information they have and / or that they give more to identified than to unidentified recipients. The design presented in this paper clearly eliminates the first explanation. Hence, all differences in donations to identified and unidentified recipients observed in this paper can only be attributed to identifiability. Given that I do not observe any effect of identifiability on donations, my results provide an interpretation for previous studies. Namely, the differences observed can be solely and unambiguously attributed to the donor's response to vivid information and not to the fact that donors can identify the recipient.

It is not entirely obvious why this study finds a tendency to donate more to groups instead of single children, given that several papers have found the exact opposite effect

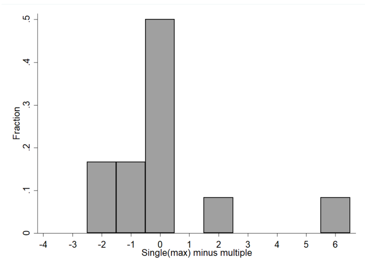
(Kogut and Ritov, 2005a,b). One difference to these studies is that I use a much smaller group size (3 instead of 8 children). Clearly, this makes donating to the group more attractive in this as compared to the previous studies. Furthermore, subjects received a much higher endowment (15 instead of 3 EUR). In combination, these differences imply that subjects could potentially make the same donation to a single child and the group of three children (e.g. by donating 2 EUR to a single and 6 EUR to a group of children), thereby eliminating differences in the perceived impact (which few subjects, however, do). Despite these differences, the results obtained here suggest that there is no general tendency to donate more to single recipients than to groups, which is independent of the choice situation. Hence, it might be possible to create arbitrary biases by varying the group size as well as well as the endowment. Interestingly, the finding that groups receive higher donations than single recipients is also in line with results from dictator games in which all participants remain anonymous. For example, Engel (2011) finds that dictators keep smaller amounts as the number of recipients increases. Hence, this raises the question how identifiability interacts with group size, in order to explain such differences.

Appendix 3.A

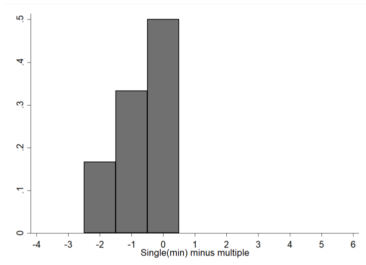
3.A.1 Additional Results

Figure 3.6: Donation to Single Identified recipient versus Multiple identified recipients (Within-subjects) - excluding first choice

(a) Maximum ($N = 12$)



(b) Minimum ($N = 12$)



(c) Average ($N = 12$)

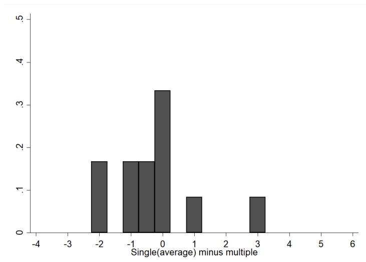
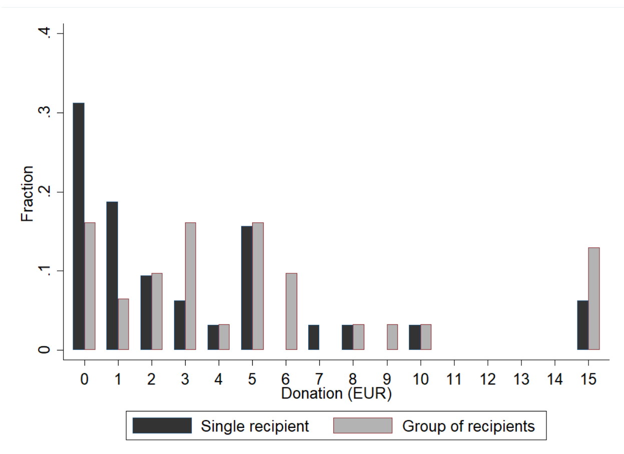


Figure 3.7: Donations to single identified recipients versus multiple identified recipients (Between-subjects) - excluding first choice



3.A.2 Instructions

Instructions
Identifiable Treatment

Your participant code

You have drawn a card at the beginning of this experiment. On this card, you will find a four-digit participant code.

You are the only person who knows this code. The experimenter does not know which participant has drawn which code such that your choices in this experiment are completely anonymous.

Please keep this card until the end of the experiment.

Please enter your code to start the experiment.

Questionnaire

We would like to ask you to answer a couple of questions. Your answers will be used for scientific purposes only and do not influence the course of this experiment.

Please read all instructions carefully and take as much time for answering the questions, as you need. It is very important for the scientific analysis that you answer all questions as precise as possible.

Your payment

You will be paid 15 EUR for completing the questionnaire.

Every participant can proceed through this experiment in their own speed. However, you will receive your payment once every participant has finished the experiment.

You have answered all questions.

You have answered all questions and earned 15 EUR.

Donation

You have the chance to donate a share or your entire earnings of 15 EUR.

After thorough research, we have chosen the organization Abaana e.V. as the recipient of your donation. Abaana e.V. supports approximately 800 children in Nyamirima Village Uganda. Uganda is one of the poorest countries in the world and to visit one of the few public schools, the children from Nyamirima Village have to walk several hours. Classes with up to 120 children are common in public schools such that there is little opportunity to attend the needs of individual children. In addition, families often cannot afford the costs for school uniforms and books such that few children in Nyamirima village have the opportunity to go to school.

The donations collected in this experiment will be used to enable children to attend the local school (financed through donations) and allow them to take part in the school lunches (with two meals per day).

All of Abaana's employees are volunteers, such that every EURO donated will be spent on the children in Nyamirima Village.

More information about Abaana is contained in the brochure next to your computer screen.

Several decisions

You will make several donation decisions. Only one of these decisions will be selected for payment. In every decision, you can donate between 0 and 15 Euro to one or several children in Nyamirima Village.

If you chose to make a donation in the decision selected for payment, the donation will be deducted from your earnings and transferred to Abaana e.V. and the rest will be paid out in cash at the end of the experiment.

The AWI Lab Team guarantees that your donation will be transferred to Abaana e.V. If you want to verify that your donation has been transferred, please write an email to info@abaana.com and indicate your participant code in the mail.

Payment

Your payment is anonymous. Neither the experimenter nor any other participants will be informed how much you donated. To guarantee anonymity the payment is organized according to the following rules:

At the end of the experiment, the experimenter only sees which amount has to be paid to which participant code. The corresponding payments are placed in envelopes, marked with the respective participant code and sealed. The experimenter places all envelopes on the table in the entry area and leaves the room.

At this point, the computer randomly selects one of the participants. This participant calls the other participants one after another (according to their cubicle number) to the front desk and asks them to show the card with the participant code drawn at the beginning of the experiment. He or she then supervises that the other participants collect only the envelope marked with the exact same code.

Decision fields and buttons

In every decision, the fields and buttons will be displayed after 15 seconds have passed. Take as much time for your decision as you need.

You can only donate integer amounts (0,1,2,…,15).

Summary

Here is a summary of all details for the following decisions:

1. You will take several decisions. At the end of the experiment, one of the decisions will be randomly selected for each participant (the chosen decision might, thus, be different for different participants) and you will be paid according to your choices in this situation.
2. In every decision, you can donate an amount between 0 and 15 EUR. If you chose to make a donation in the decision selected for payment, your donation will be deducted from your earnings and transferred to Abaana e.V.
3. Your payment is anonymous. Neither the experimenter nor any other participant will learn how much you chose to donate.
4. You can enter and confirm your choices only after 15 seconds have passed. You can only donate integer amounts.

Please click on the “Start” button if you have read all instructions. You will not have the opportunity to read the instructions again as soon as you click this button.

Every participant has been matched to a group of three children from Nyamirima village. On the screen, you can see the photos of the three children in the group you have been matched to.

In the following decisions, you will have the opportunity to donate to one of the children in this group.

For this experiment, we chose a large group of children from Nyamirima Village. It is, thus, possible but rather unlikely that another participant in this session has the possibility to make a donation to the same child as you.

First decision

Below you see the picture of one of the three children. This child has been randomly selected and will receive your entire donation.

<Photo>

Your donation: _____________

(Please enter an amount between 0 and 15 EUR)

Second decision

Below you see the photos of all three children. The computer will select one of these three children. The child selected by the computer will receive your entire donation. You will, however, not be informed during or after the experiment which of the three children has been selected.

<Photo>

Your donation:

(Please enter an amount between 0 and 15 EUR)

Chapter 4

Legislative Bargaining with Joint Production: An Experimental Study[1]

Abstract
We conduct 3-person bargaining experiments in which the surplus being divided is produced by completing a prior task. Using a Baron-Ferejohn framework, we investigate how differences in contributions to production affect bargaining under different decision rules. Under unanimity rule, all proposals and agreements constitute convex combinations of the equal and proportional splits. Contrary to our predictions, this pattern largely persists under majority rule. In sharp contrast to prior experiments in which an exogenous surplus is divided, few subjects attempt to build minimum winning coalitions when the surplus is jointly produced.

Keywords: bargaining, subjective claims, Baron and Ferejohn bargaining game, distributional preferences, proportionality, fairness, experiments

JEL Classification: C72, C78, C91, D33, D63

[1] This chapter has been jointly written with Christoph Vanberg.

4.1 Introduction

Whenever groups of individuals collaborate in productive activities, decisions must be made about how to distribute gains resulting from joint production. Unless the division is contractually specified ex ante, it must instead be negotiated ex post. For example, governments need to distribute the tax budget across different departments and private companies need to decide how to allocate revenues across different divisions. Such negotiations are likely to be especially complicated when different group members have made different 'contributions' to the prior productive activity, inducing disagreement about the degree of 'proportionality' that should prevail.[2] How are such disagreements handled under different decision rules? This is what we want to investigate in this paper.

A number of authors have experimentally shown that joint production can lead to the establishment of 'subjective claims' to a resulting surplus, and investigated how such claims affect bargaining behavior. In these experiments, groups of two or more subjects 'produce' a joint surplus by completing a real effort task such as answering trivia questions. Subsequently, subjects bargain over how to distribute that surplus. In a bilateral context, Gächter and Riedl (2005) and Karagözoğlu and Riedl (2014) find that subjects *expect* distributions to reflect relative contributions (e.g. the number of correct answers given), and also judge such proportionality as *fair*. Further, they show that bargaining outcomes reflect these considerations. Gantner et al. (2016) extend the analysis to a three-player context, comparing the impact of contributions under three different bargaining procedures, all of which require unanimous consent to reach agreement. They also find that fairness judgments reflect individual contributions, but to a lesser extent than suggested by a strict norm of proportionality.

To our knowledge, this is the first paper to experimentally investigate *majority rule* bargaining with joint production. All prior experiments on bargaining with joint production have looked at either bilateral situations or at multilateral situations with unanimity rule. There are many interesting situations, however, where distributive decisions are made using majority rule. Examples include labor-management negotiations, coalition formation, bargaining over distributive politics, and budget negotiations in national or international organizations.

As an example, consider budget allocation decisions within the European Union. Here, representatives from different member states bargain over how to allocate resources,

[2] Such disagreements are likely to be especially pronounced in contexts where relative contributions are difficult to assess, or where they are perceived to result from 'luck' as opposed to 'effort' (Hoffman and Spitzer, 1985; Konow, 2003; Fischbacher et al., 2009; Almås et al., 2010; Becker, 2013).

both across different budget categories (e.g. agriculture, regional development, etc.) and within categories, to projects located in specific member states. Although many expenditures serve to create shared benefits for all member states (e.g. defense, administration), there is some truth to the common perception that the process ultimately boils down to the splitting of a cake between the separate member states. Likewise, a widely held view is that some member states are entitled to a larger slice of that cake than others, because they have made larger contributions in the form of membership fees.[3]

There are good reasons to believe that bargaining behavior and outcomes under majority rule are different from those observed under unanimity rule. Under unanimity rule, each player holds veto power which can be used to defend one's claim. This is fundamentally different under majority rule, where players can form minimum winning coalitions and exclude certain group members from the allocation. Prior experiments on majority rule bargaining over an exogenous surplus have consistently shown that most games end with such agreements. Hence, an important question is whether we continue to observe such outcomes when all players hold claims to the surplus. If so, an interesting question is which player is more likely to be included in a coalition - the one who has a larger or a smaller claim?

In this paper, we experimentally investigate how claims based on contributions to production affect bargaining behavior under both unanimity and majority rule. In our experiment, groups of 3 subjects bargain over a surplus which they have previously produced by separately engaging in an individual real effort task. The bargaining procedure is a finite horizon version of the Baron and Ferejohn (1989) game (henceforth BF game). Our main treatment variable (exogenously manipulated) is the number of votes required to pass a proposal (majority vs. unanimity rule). In addition, we observe a number of different (endogenously determined) situations in terms of the relative contributions the group members have made, depending on their individual performance in the real effort task. We investigate and compare how the resulting claims affect proposals, voting behavior, passage rates, and final outcomes under each rule.

Our main findings are the following. Under both rules, proposals and voting behavior are significantly affected by claims. In treatments with unanimity rule, virtually all

[3] For example, in the recent 'Brexit' referendum, Britain's rising net contributions, calculated as the fees contributed to the EU minus received transfers, was one of the most contentious issues. Not only EU critics but also the popular media discussed this issue as an argument against UK's continued membership. Net contributions were also a central topic during Scotland's first independence referendum in 2014 which would have enabled Scotland to become and independent member of the EU. Prior to the referendum, the government examined Scotland's potential role within the EU and critically pointed out that Scotland was likely to become a net contributor.

proposals and outcomes constitute convex combinations of the three-way equal split and the split that is exactly proportional to relative contributions. This result is consistent with prior evidence discussed in the next section. More surprisingly, we observe a very similar pattern under majority rule. In particular, the vast majority of proposals allocate positive shares to all participants. This result stands in stark contrast to comparable experiments on BF bargaining in which subjects divide 'manna from heaven' and most subjects propose minimum winning coalitions.

Under both decision rules, we find that players who have made relatively smaller contributions tend to make more equal (i.e. less proportional) proposals. This pattern is more pronounced under majority rule. In combination with the fact that players with lower claims are more likely to support more equal proposals, this leads to more equal outcomes under majority rule when a majority (i.e. two players) have made relatively small contributions. Finally, we find that majority rule leads to a higher passage rate than unanimity rule, especially when group members have made different contributions to the surplus.

The rest of the paper is organized as follows. Section 4.2 discusses related literature. Section 4.3 presents our experimental design. Section 4.4 summarizes our hypotheses. Results are presented in section 4.5. Section 4.6 concludes. Further analyses and experimental instructions are provided in Appendix 4.

4.2 Related Literature

Our paper contributes to a recent literature analyzing how claims, resulting from joint production, affect behavior and outcomes in experimental bargaining games. For a review on bargaining games with joint production see Karagözoğlu (2012). Most closely related are three recent studies which examine the role of claims in bilateral (Gächter and Riedl, 2005; Karagözoğlu and Riedl, 2014) and multilateral bargaining (Gantner et al., 2016). In these experiments, subjects earn endowments by answering a series of quiz questions. These endowments are then combined to form a common surplus. Subsequently, either two (Gächter and Riedl, 2005; Karagözoğlu and Riedl, 2014) or three (Gantner et al., 2016) subjects bargain over the distribution of the surplus using unanimity rule. A common finding in all three papers is that subjects who have made higher contributions are offered more compared to subjects with lower contributions. Further evidence suggests that individuals derive 'subjective claims' which reflect their relative contributions to the jointly produced surplus. According to Schlicht (1998), claims (or 'entitlements') are "rights, as perceived by the individual (...) that go along with a motivational disposition to defend them" (Schlicht, 1998, p.24). Moreover, he defines obligations as the counterpart of claims, i.e. people will feel obliged to comply with what they perceive as another person's right. Hence, claims appear to capture what a person *expects* to receive as well as her subjective fairness view.

In sum, several prior studies have found evidence that claims have a significant impact on bargaining under unanimity rule, i.e. when all group members must consent to the final agreement. In contrast, there is to our knowledge no experimental evidence on the effects of claims under majority rule. The key difference is that a majority coalition (in our case 2 players) can, in principle, ignore the claims of a minority player, as his consent to the allocation is not required. If no player can enforce his own claim by vetoing a potential agreement, do claims become meaningless?

Of obvious relevance to this point are several studies looking at two-person dictator games with a jointly produced surplus. Cappelen et al. (2007) conduct an experiment in which subjects contribute endowments earned in a prior investment stage. Importantly, endowments are a combination of the sum a subject decided to invest in one of two projects and a randomly determined high or low interest rate paid for each dollar invested. Both subjects in a pair decide how to allocate the joint surplus and one (randomly chosen) decision is implemented. Subjects are repeatedly matched and thus take decisions in different distributional situations which allows the authors to classify subjects into types. They find that a majority of subjects can be classified as 'liberal egalitarian' or 'libertarian' types and thus take the investment made by the other subject into account when choosing an allocation. Almås et al. (2010) conduct dictator games with children in grades 5 to 13 where the surplus is the result of a real effort task. They find that as children get older, their offers more strongly reflect the contributions of their partners. In a recent meta study on dictator game behavior, Engel (2011) finds that dictators tend to give less if they have earned the endowment or take less from the receiver if she has earned the endowment. Overall, these experiments provide evidence that dictators tend to 'respect' a recipient's claim, at least to some extent, even though the recipient has no veto power. Applied to our own context, this suggests that subjects may be reluctant to form minimum winning coalitions under majority rule, and instead allocate positive shares to all players.

The previous findings from unanimity bargaining and the dictator game appear to be compatible with the idea that behavior is motivated by fairness concerns which take claims into account. Thus, the literature examining 'fairness' of outcomes in situations with joint production is also informative for this paper. For example, Selten (1988) discusses the role of the so-called 'equity principle' for understanding behavior in allocation tasks and bargaining games. He defines a 'proportional equity rule' as follows: "The proportional equity rule can be thought of as a modification of the equal division principle. Whereas the equal division principle prescribes the same reward for every person, the proportional equity rule prescribes the same reward for every unit of achievement." Among others, he discusses reward allocation experiments conducted by Mikula (1972) and Mikula and Uray (1973). In these experiments, subjects first engage in a task and subsequently one subject is asked to allocate a sum of money. As summarized by Selten (1988), subjects tend to divide equally if performance in the task was equal. If performance was however unequal, there was a tendency towards more proportional distributions. Konow (2003) reviews a very large collection of empirical studies (mostly experiments and vignette surveys) to assess the degree to which different conceptions of 'justice' are descriptive of how people commonly make impartial fairness judgments. He proposes "a multi-criterion theory of justice' (...) in which three justice principles are interpreted, weighted, and applied in a manner that depends on context." (Konow, 2003, p. 1235) These principles are *equity*, *efficiency*, and *need*. In discussing evidence on the 'equity principle', he cites extensive experimental and survey evidence showing that subjects consider it fair to distribute resources in a way that is proportional to all variables under a person's control, such as work effort. In the multilateral bargaining game discussed above, Gantner et al. (2016) find that impartial fairness assessments, elicited from independent and unaffected participants, are a convex combination of proportionality and equality, giving rise to pluralism of fairness norms which might guide individual behavior in these situations.

An important finding is that such fairness perceptions can be self-servingly biased. For example, Gantner et al. (2016) find that low contributors are more likely than high contributors to judge an egalitarian division of the surplus as fair. Further evidence comes from an experiment by Konow (2000), in which all subjects perform the same real effort task (prepare a given amount of letters) but earn different piece rates. The funds of both subjects are then pooled and either the subject with the higher piece rate or an uninvolved third person decides how to allocate the funds among the two subjects. The results of the experiment indicate that partial subjects are more likely to deviate from the accountability principle than impartial subjects, indicating a self-serving bias. In summary, these findings suggest that (at least a majority of) people judge proportionality as fair, and that the degree of proportionality they favor might be self-servingly biased. We conjecture that such judgments are likely to affect bargaining behavior under majority rule.

Finally, we add to a vast experimental literature on the Baron and Ferejohn bargaining game (McKelvey, 1991; Fréchette et al., 2003, 2005a,b,c; Diermeier and Morton, 2005; Agranov and Tergiman, 2014; Miller and Vanberg, 2013, 2015). The central findings of that literature can be briefly summarized as follows. First, most proposers form minimum winning coalitions (MWCs) under majority rule, especially after gaining some experience with the game. Second, the most commonly observed proposals and agreements implement equal splits (either overall or within a MWC). Third, unanimity rule leads to more delay as compared to majority rule.[4] To our knowledge, we are the first to report on a Baron-Ferejohn experiment involving the division of a previously produced surplus. Baranski (2016) studies a majoritarian BF game in which the surplus to be allocated is the result of voluntary contributions. His main interest is how allowing subjects to bargain over the distribution affects incentives to contribute. Our context differs from this in several respects. First, performance in the real effort task is not a strategic choice given that players are not informed about the decision rule when they earn their contributions. Second, differences in performance result at least in part from luck, such that there is likely more disagreement about the distribution of the surplus. These design choices reflect the fact that we are interested in the influence of claims (as exogenous parameters) on bargaining behavior.

4.3 Experimental Design

The experiment consists of of two stages, a 'production' stage followed by a 'bargaining' stage. In the production stage, subjects individually earn 'points' by answering a series of trivia questions organized into 12 'blocks'. Each block consisted of 2 multiple choice questions on different topics (i.e. geography, history, arts, science). On each block, subjects could earn either zero, one, or three points, depending on whether neither, one, or both questions were answered correctly. Each block contained one 'easy' question that we expected most subjects to answer correctly, and a second question that varied in difficulty. After completing the production stage, each subject thus had 'produced' a list of 12 separate scores, each either 0,1, or 3 points.

After all subjects had completed the production stage, they proceeded to the bargaining stage. This consisted of 12 separate rounds. In each round, subjects were matched into groups of three. Each group was then assigned a surplus equal to 5 EUR times the sum of three randomly and independently chosen scores, one from each of the lists that they had previously produced. Thus, the scores contributed by the members of a group would usually come from different 'quiz blocks'. The sampling of scores was

[4] Recent findings by Agranov and Tergiman (2014, 2017) suggest that free communication (chatting) between the group members leads to more unequal agreements under majority rule and to more equal allocations under unanimity rule. In addition, communication virtually eliminates delay under both rules.

done with replacement, so that it was possible for a given subject to have the same quiz block selected multiple times over the course of the experiment. Each subject was informed about the quiz block selected for her and about the number of points she had earned. In addition, they were informed about the number of points contributed by the other players, as well as each group member's percentage share of all contributed points. Subjects were *not* informed about the quiz block selected for the other two group members.

These design features were chosen with three goals in mind. First, the presence of an easy question in every quiz block was meant to ensure that all subjects would have a positive claim, at least in most games. Second, the more difficult questions should lead to heterogeneity in claims, as some but not all subjects will score 3 points on the quiz block chosen for them. Third, differences in difficulty between blocks implies that individual contributions constitute a noisy signal of relative performance. That is, subjects could not be sure whether differences in the number of points contributed were due to good performance (answering difficult questions) or luck (having an easy quiz block chosen).[5]

The bargaining game itself followed a finite horizon Baron-Ferejohn framework. That is, bargaining proceeded over a finite number of discrete rounds. Within each round, the sequence of events was as follows. First, all subjects were asked to propose a division of the surplus. Next, all subjects voted either 'yes' or 'no' on each of the three proposals made in their group. Once the votes had been cast, *one* of the three proposals was randomly selected and the votes were counted.[6] Depending on the treatment, the proposal passed if either a majority (two) or all three subjects voted 'yes'. In that case, the game ended. Otherwise, the surplus shrank by 20% and bargaining proceeded to a new round. If the surplus fell below 2 EUR (i.e. after 8 rounds of bargaining), the game was terminated and all group members earned 0 EUR.[7] At the end of the experiment, one of the 12 bargaining games was randomly chosen and subjects were paid according to the corresponding outcome.

The experiment was conducted at the AWI Lab at the University of Heidelberg, Germany, in June 2016 and January 2017. In total, 198 students, from various disciplines,

[5] Note that the element of 'luck' is indeed present because a given subject's quiz scores for different games are drawn *with replacement*. Therefore some subjects will be luckier than others even if they perform equally well, and even if we aggregate across all games played.

[6] In the standard formulation of the BF game, the proposer is selected at the beginning of the round and only one proposal is made. Our procedure allows us to observe three times as many proposals and votes. Although this does not alter the SSPE predictions, it may impact real behavior if subjects react to the additional information provided. However, any such effects are of course present in all our treatment conditions.

[7] This feature of our design implies that ours is a finite horizon BF game.

Table 4.1: Symmetric equilibrium proposals

	Proposer share	Responder share
Majority rule	73%	27% (to one)
Unanimity rule	46%	27% (to both)

participated (108 in the June and 90 in the January sessions). We conducted twelve sessions, six for each treatment (majority and unanimity rule). Each session involved 18 subjects, divided into three matching groups of six participants.[8] Due to no-shows, we conducted three sessions with 12 subjects. Hence, in total we have 33 matching groups (17 for majority and 16 for unanimity rule). Upon entering the laboratory, subjects were randomly assigned to isolated computer terminals. Paper instructions (reproduced in the Appendix) were handed out and questions were answered in private. The experiment was programmed in z-Tree (Fischbacher, 2007). Sessions took approximately 70 minutes, and average earnings amounted to 13 EUR (highest: 23.5 EUR, lowest: 4 EUR) including a 4 EUR show-up fee.

4.4 Benchmark predictions and hypotheses

While the BF bargaining game admits multiple subgame perfect equilibria, the prior literature has typically focused on symmetric and stationary equilibria, which are (essentially) unique. For the finite horizon version, the relevant equilibrium concept is Symmetric Markov Perfect Equilibrium (SMPE). See Norman (2002) for a detailed analysis. As established there, the unique SMPE has three interesting properties which can be tested empirically. The first is that proposers attempt to form minimum winning coalitions in which only the number of individuals required to vote yes receive positive offers. Second, these 'coalition partners' are offered exactly their continuation value, i.e. the amount that they expect to receive if the current proposal were to fail. This implies an unequal distribution of the surplus, favoring the proposer. Third, the first proposal passes without delay. All three of these predictions are independent of the decision rule being employed. The predicted outcomes for our version of the game ($n = 3$ players and discount factor $\delta = 0.8$) are presented in Table 4.1.

Naturally, these SMPE predictions are unaffected by the prior production phase con-

[8] Admittedly, these are small matching groups. However, we believe that repeated game effects within the matching groups are unlikely. First, subjects were not told about the size of the matching group. In the instructions, they were only informed that they would be re-matched at the beginning of each round. Second, the identifying labels on the decision screens changed randomly between games. The advantage of implementing small matching groups is that we obtain 3 independent observations for each session.

ducted in our experiment. By definition, they are based on the assumption that all players employ the same strategy, effectively ignoring any differences in the relative contributions they have made to the surplus. Under unanimity rule, the SMPE corresponds to the only subgame perfect equilibrium. The fact that players can selectively build coalitions under majority rule, leads to multiple and asymmetric equilibria. Hence, in these cases players could use the relative contributions to coordinate on asymmetric and / or non-stationary equilibria of the game (see Norman, 2002). For this reason, it is especially interesting to study how claims affect behavior under majority rule.

In addition to the SMPE predictions, we formulate a number of additional hypotheses which are based on the idea that players are motivated by material self-interest as well as notions of fairness, which take claims into account (Konow, 2000, 2003). Players are assumed to be heterogeneous in how much weight they place on either of these two motives. As outlined in Section 4.2, prior evidence on unanimity rule bargaining appears to support this idea, and demonstrates that such preferences have a systematic impact on behavior and outcomes. We separately formulate our additional hypotheses for situations with *symmetric claims* (i.e. all group members have made the same contribution) and situations with *heterogeneous claims* (i.e. the group members have made different contributions).

Symmetric Claims Situations with symmetric claims are those where all three group members have contributed either 1 point (5 EUR) or 3 points (15 EUR) to the surplus. Various theories of fairness, such as summarized by Konow (2003) suggest that the unique 'fair' outcome in this situation is an equal split. This should motivate 'fair-minded' players to propose the equal split, and to vote for it (and against other proposals). Anticipating this behavior, even purely self-interested players should do the same under unanimity rule, knowing that anything else is likely to only increase delay.[9] Thus, under *unanimity rule*, we hypothesize that subjects will propose and agree on the equal split.

Hypothesis 1. *In symmetric situations with unanimity rule, most proposers suggest three-way equal splits. Group members more often vote 'yes' on such proposals than on unequal splits. Therefore, equal splits pass with higher probability.*

The predictions implied for *majority rule* are less straightforward. Since proposers can build minimum winning coalitions, 'selfish' (or less 'fair-minded') players may attempt

[9] That is, if at least one of the players in a given group is 'fair-minded' in the way outlined, no unequal division can pass under unanimity rule.

to do so, hoping that the included player will vote 'yes', either because he is also selfish, or because the larger share that he can be given (e.g. 50% instead of 33%) is enough to outweigh his fairness concerns. Thus, depending on (beliefs about) the distribution of types in a population, 'selfish' proposers will build minimum winning coalitions, and perhaps make relatively generous offers to their partners within such coalitions. This could result in a mix of three-way and two-way equal splits being proposed. When voting, fair-minded players should be more likely to support 'grand' proposals that are equal splits, and all players should be more likely, ceteris paribus, to support proposals that allocate larger shares to them. In sum, it appears difficult to predict which allocations will be proposed under majority rule. Relative to unanimity rule, however, we can expect minimum winning coalitions to be more common. We therefore formulate the following hypothesis to be compared against the results obtained.

Hypothesis 2. *In symmetric situation with majority rule, proposers attempt to build minimum winning coalitions. These coalitions are more likely to pass the larger the share offered to the coalition partner.*

Asymmetric claims Our second set of hypotheses is formulated for situations in which the group members have made different contributions, leading to heterogeneous claims. Given that high contributors *expect* to receive higher shares, and indeed people regard this as *fair* (Gächter and Riedl, 2005; Gantner et al., 2016), it is difficult for proposers to ignore claims under unanimity rule, as doing so is likely to result in failure of their proposal. Thus, players with larger contributions should receive higher offers. This prediction is in line with the existing evidence on the effect of heterogeneous claims under unanimity rule (Gächter and Riedl, 2005; Karagözoğlu and Riedl, 2014; Gantner et al., 2016).

Hypothesis 3. *In asymmetric situations with unanimity rule, shares offered are increasing in relative points contributed.*

In the presence of a self-serving bias, proposals should be more proportional the larger a player's contribution, as material self-interest and fairness concerns are aligned in these cases. Similarly, when voting, players with higher contributions should more often vote 'yes' the more proportional a proposal than individuals with lower contributions.

Hypothesis 4. *In asymmetric situations with unanimity rule, individuals with larger contributions more often suggest, and are more likely to vote 'yes' on the proportional split than members with smaller contributions.*

When claims are asymmetric, individuals are likely to differ in how much proportionality they perceive as 'fair', thus causing heterogeneity in fairness views. This, in turn, may lead to more delay in negotiations in asymmetric as compared to symmetric situations. In line with this prediction, Karagözoğlu and Riedl (2014) find that the bargaining duration significantly increases in treatments where subjects derive heterogeneous claims based on performance feedback relative to treatments in which no performance feedback is provided.

Hypothesis 5. *Under unanimity rule, delay occurs more frequently when players have asymmetric claims than when claims are symmetric.*

One reason why claims are likely to influence bargaining outcomes under unanimity rule is that all players have *veto power* which can be used to enforce claims as well as fairness perceptions. As was already discussed, this situation is fundamentally altered when *majority rule* is used. A player seeking to maximize his payoff may propose a minimum winning coalition excluding one responder. When responder claims differ, it is even conceivable that the proposer would systematically discriminate against the player with the larger claim, as she might be perceived as more 'expensive'. This hypothesis may fail if players' fairness conceptions cause them to be reluctant to exclude others from the winning coalition. As mentioned above, evidence from dictator games with prior production indicate that many subjects are indeed reluctant to exclude others in situations where they could do so. Note, however, that the frequency of minimum winning coalitions in (standard) Baron-Ferejohn experiments is significantly larger than the frequency of zero offers in standard dictator games. That is, subjects in multilateral bargaining games appear to be more willing to allocate nothing to one player. Therefore we tentatively conjecture that this willingness to exclude a player from payment will persist in our setting, even when the surplus is jointly produced. These considerations lead us to formulate the following hypothesis.

Hypothesis 6. *In asymmetric situations with majority rule, proposers attempt to build minimum winning coalition.*

Should this hypothesis prove to be true, an interesting follow-up question is which responder is more likely to be included in a minimum winning coalition. When responder 'claims' differ, two competing considerations may play a role. On the one hand, the responder with the larger claim may appear more deserving, and thus fairness concerns may dictate that she be included in the coalition. On the other hand, it appears likely that the responder with the smaller claim will be 'cheaper' - i.e. more likey to vote 'yes' for a given share being offered. Thus, proposers may strategically exclude the player with the larger claim. Which of these considerations prevails more often is an empirical question. We will organize our analysis around the following hypothesis.

Hypothesis 7. *When the responders' contributions differ, proposers who build minimum winning coalitions are more likely to include responders with smaller contributions.*

As under unanimity rule, heterogeneous claims are likely to cause more disagreement in subjective fairness ideals which will lead to more delay in negotiations as compared to situations with homogeneous claims.

Hypothesis 8. *Under majority rule, delay occurs more frequently when players have asymmetric claims.*

Majority versus Unanimity rule All hypotheses formulated thus far concern the effects of claims *within* each of our treatments (majority and unanimity rule). Finally, we formulate two hypotheses regarding differences between the two treatments. First, claims should affect proposals (and final outcomes) more strongly under unanimity than under majority rule. Under unanimity rule, the existence of veto power implies that claims and fairness perceptions can be enforced. Under majority rule, in contrast, subjects can trade off fairness against higher shares for themselves which might cause less fair-minded players to propose minimum winning coalitions and even relatively fair-mindeded individuals might propose less proportional and more equal divisions of the surplus. Thus, under majority rule proposals and final outcomes should shift away from the proportional split.

Hypothesis 9. *Proposals and final outcomes under majority rule are less proportional than under unanimity rule whenever the proposer has made a smaller contribution.*

The final hypothesis concerns the length of the bargaining process under both decision rules. Given that under majority rule less members need to consent, majority rule should lead to faster agreement than unanimity rule. This effect should be particularly pronounced in situations with heterogeneous claims as group members are more likely to hold conflicting fairness views. The final hypothesis is also in line with previous research conducted on the BF bargaining game. For example, Miller and Vanberg (2013, 2015) and Miller et al. (2018) find that delay occurs more frequently under unanimity rule.

Hypothesis 10. *Delay occurs more frequently under unanimity than under majority rule, especially in situations involving heterogeneous claims.*

4.5 Results

As indicated above, we purposefully designed the quiz blocks such that most subjects should earn at least one point, and some would earn three points. We did this because we want to focus on situations where all group members have made positive contributions, but the size of these contributions may differ. Table 4.2 summarizes the frequency with which we observed various constellations of points within the bargaining groups that were formed in both treatments. By focusing on situations where all contributions are positive, we lose approximately 25% of the data. We analyze these excluded cases in Appendix 4. Also, we have relatively few observations where all subjects contributed either one point or three points. Since the *relative* contributions are the same in these situations, we will pool these data in the subsequent analysis.

Table 4.2: CONSTELLATIONS OF POINTS CONTRIBUTED

		Number of games	
Contributions	Surplus	Unanimity rule	Majority rule
(1,1,1)	15 EUR	20	30
(3,3,3)	45 EUR	47	39
(1,1,3)	25 EUR	87	117
(1,3,3)	35 EUR	140	116
not all positive	various	90	106
Total		408	384

As is typically done in the literature on Baron-Ferejohn bargaining, most of our empirical analysis will focus on the first round of bargaining. Given our method of having all subjects make a proposal, we observe three proposals per game. In situations where relative contributions differ, we will distinguish cases according to whether the proposer has made a relatively large or small contribution.[10] With this in mind, Table 4.3 presents the number of proposals we observed in each of five possible situations. Here and later, the first coordinate of the contribution vector (in bold) denotes the relative contribution of the *proposer*. When responder contributions differ, they are ordered such that the smaller contributor is listed first (i.e. the second coordinate). When responder contributions are the same, they are ordered alphabetically according

[10] Recall that, by design, individual contributions can take on only two values, 1 and 3.

to the letter i.d. ('A', 'B', or 'C') that players were randomly assigned at the start of the game.

Table 4.3: Situations observed (first round)

Percentage Contributions†	Number of proposals Majority rule	Unanimity rule
(**33**,33,33)	207	201
(**20**,20,60)	234	174
(**60**,20,20)	117	87
(**14**,43,43)	116	140
(**43**,14,43)	232	280
Total	906	882

† The first coordinate is the proposer's percentage contribution.

4.5.1 Symmetric claims

We begin by discussing the situations where all subjects have contributed the same number of points (either 1 or 3). Figure 4.1 displays the distribution of proposals within a simplex. In this and the following figures, the simplex is defined such that the shares allocated to responders 1 and 2 are measured along the horizontal and vertical axes, respectively. As mentioned above, responders are ordered alphabetically according to the letter i.d. they were assigned on the decision screen. The south-west corner would correspond to a proposal where the proposer demands the entire pie, and the right and top corners represent points where everything is allocated to responder 1 and responder 2, respectively. For orientation, a number of focal points are highlighted. Equal splits (both two- and three way) are marked in blue. The proportional split (reflecting claims) is marked in red. (In the symmetric case, the proportional split is identical to the three-way equal split.) The size of the bubbles reflect the relative frequency of the corresponding proposals, and the pie charts within the bubbles display the fraction of proposals that pass (in green) and fail (in red). Finally, each (sub)figure contains information about the three most frequently observed proposals. For example, the most frequently observed proposal under unanimity rule is an equal split.[11] It accounts for 88% of all offers, and it passes 95% of the time.

[11] Although the figure displays these as $(34, 33, 33)$, these may include some proposals that were actually $(33, 33, 33)$. The simplex is constructed such that the first coordinate is 100 minus the other two, i.e. we are assuming that all proposals sum to 100.

Figure 4.1: Proposals and passage rates, equal claims

(a) Unanimity rule (N=201) (b) Majority rule (N=207)

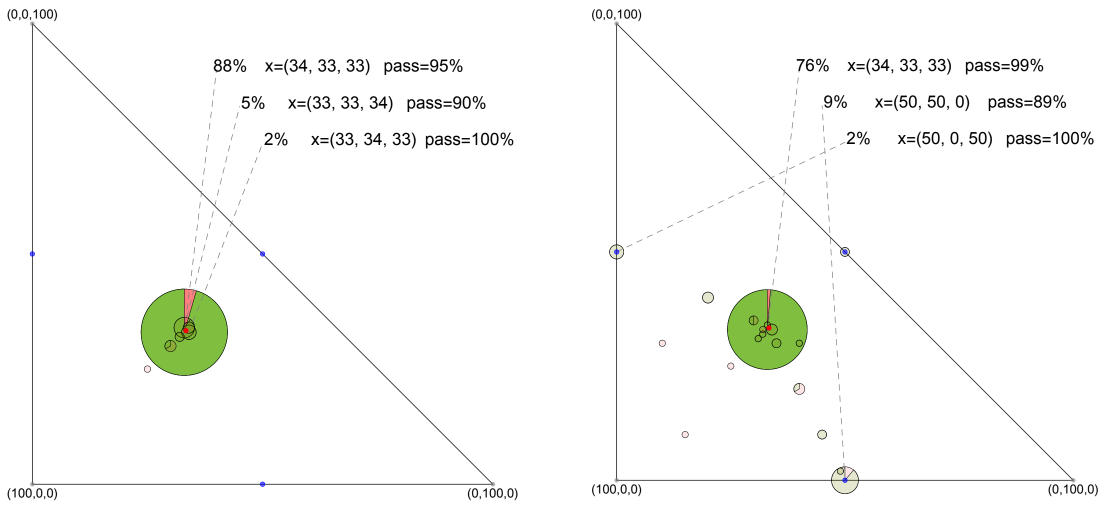
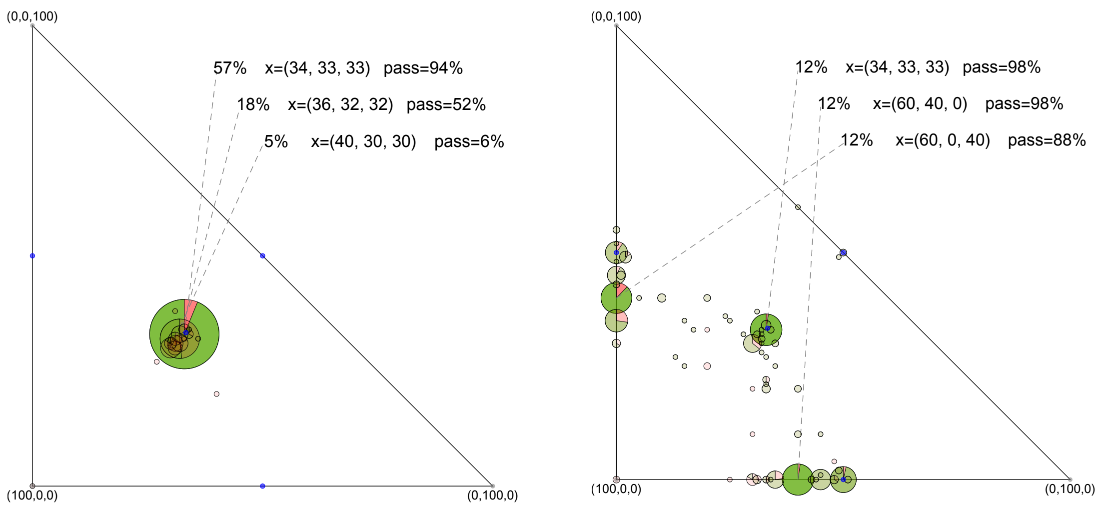


Figure 4.2: Proposals and passage rates, no claims†

(a) Unanimity rule (N=312) (b) Majority rule (N=351)



† These data are taken from a previous experiment (Miller and Vanberg 2013)

As can be easily recognized by inspecting Figure 4.1, behavior in the symmetric situation is quite similar under both rules. In particular, the vast majority of proposals are either equal splits or very close to equal splits, and these proposals almost always pass. Overall, 94% and 95% of proposals pass under unanimity and majority rule, respectively (see Table 4.4 below). Under majority rule, we observe only few minimum winning coalitions being proposed and all of them suggest the two-way equal split.

While this behavior was to be expected under unanimity rule (see Hypothesis 1), it is somewhat surprising under majority rule. As mentioned, previous experiments on the BF game without claims have found that most proposers build minimum winning coalitions (MWCs), excluding one responder from payment. As an example, consider Figure 4.2, which presents the distribution of proposals in a prior BF experiment without claims (Miller and Vanberg, 2013). Our results suggest that the willingness to completely exclude one player from payment is substantially reduced when the surplus being distributed has been jointly produced. Comparing our own and results reported in Miller and Vanberg (2013), we find that the fraction of MWCs is significantly lower in our sample (Chi-squared test, 11% vs. 66%, $p < 0.01$, $N_1 = 207$ and $N_2 = 354$). Thus, we can reject Hypothesis 2.[12]

One reason why individuals might propose a three-way equal split more often than a MWC is that MWCs may be less likely to pass. Although we have only few relevant observations, we find that the passage rate in MWCs is smaller than in grand coalitions (85% versus 96%). To test for significance, we compare the fraction of passed proposals in grand and minimum winning coalitions for each matching group. We do not find that the difference in passage rates is statistically significant (Wilcoxon matched-pairs signed-ranks test, $p = 0.53$, $N = 13$).[13] To the extent that subjects could have anticipated or learned this over time, the fact that few MWCs are proposed suggests that individuals indeed regard it as fair to respect other subjects' claims.

To analyze how the location of a proposal affects voting behavior, we run a Random-effects probit regression using the voting decision as dependent variable.[14] The independent variables are the Euclidean distance to the equal (proportional) split and the period. Under both decision rules, we find that the probability to vote 'yes' decreases significantly as the distance to the equal split increases (Average marginal effect; Unanimity rule $\beta = -0.02, p < 0.01$; Majority rule $\beta = -0.01, p < 0.01$). Hence, deviations

[12] It should be noted that the frequency of MWCs increases over time. If we focus only on the final 4 periods, it is 17%. This is still substantially smaller than what is observed in periods 9-12 of Miller and Vanberg (2013) (79%, $p < 0.01$, $N_1 = 48$ and $N_2 = 96$)

[13] We observe MWCs being proposed in 13 of 17 matching groups in the majority rule treatment.

[14] Each individual votes on the proposals of both other group members in every game. We use panel methods assuming that voting decisions are uncorrelated with individual characteristics.

from the equal split result in higher disapproval.[15]

Result 1. *In symmetric situations, the vast majority of proposers suggest a three-way equal split under both decision rules. Under majority rule, only a small number of proposers attempt to build a minimum winning coalition. Those that do always propose a two-way equal split. Under both decision rules, proposals are more often rejected, the larger the distance to the equal split. (Consistent with Hypothesis 1, inconsistent with Hypothesis 2.)*

4.5.2 Asymmetric claims, unanimity rule

Next we look at situations in which the group members have contributed different amounts to the surplus. We begin by considering behavior under unanimity rule.

Figure 4.3 displays the distribution of proposals and corresponding passage rates in the $c = (20, 20, 60)$ situation. The left panel depicts cases in which the proposer has contributed 20%, the right panel those in which his contribution is 60%.

Three patterns are immediately visible. First, virtually all proposals are located on a line connecting the *proportional* (marked in red) to the *three-way equal* split (blue). Second, the distribution of proposals shifts away from the equal split and towards the proportional split when the proposer's own contribution is relatively larger (right panel). In these cases, the proposer suggests the proportional split almost twice as often (57% vs. 30%). Finally, the proportional split passes less often when the proposer has made a comparatively large contribution (68% vs. 85%) but this difference is only marginally significant (Wilcoxon matched-pairs signed-ranks test, $N = 30$, $p = 0.1$).

The corresponding distributions for the $c = (14, 43, 43)$ situation are depicted in Figure 4.4. Again, the left and right panels depict the cases where the proposer's contribution is relatively small (i.e. 14%) or large (43%). In the second asymmetric situation, we observe the exact same pattern as in the previously discussed $c = (60, 20, 20)$ situation.

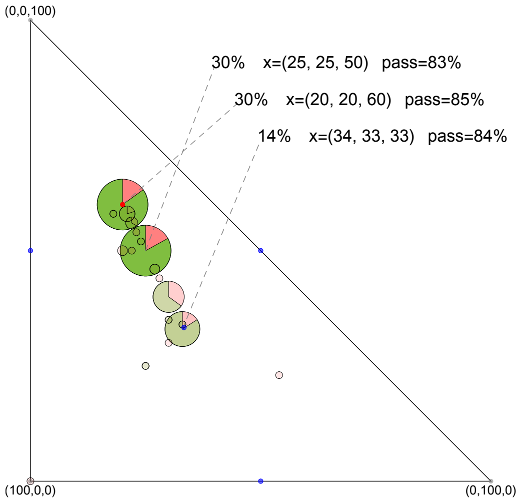
Given that virtually all proposals in both asymmetric situations are somewhere in between the equal and proportional splits, it follows immediately that offers are affected by claims. Table 4.4 summarizes the average offers made in all situations and in both treatments. Focusing on the middle column for now, we can see that the ordinal ranking of offers received matches that of the claims in all situations. This pattern is consistent with Hypothesis 3.

Result 2. *In asymmetric situations with unanimity rule, shares offered are increasing*

[15] Given that all MWC proposals suggest a two-way equal split, we cannot test the second part of Hypothesis 2.

Figure 4.3: Proposals and passage rates, $c = (20, 20, 60)$, unanimity rule

(a) $(20, 20, 60)$ (N=174)



(b) $(60, 20, 20)$ (N=87)

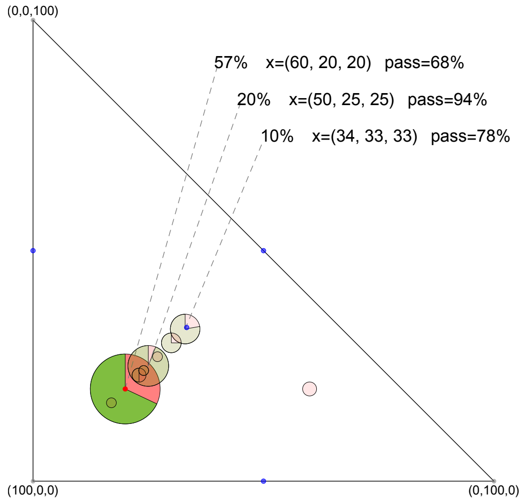
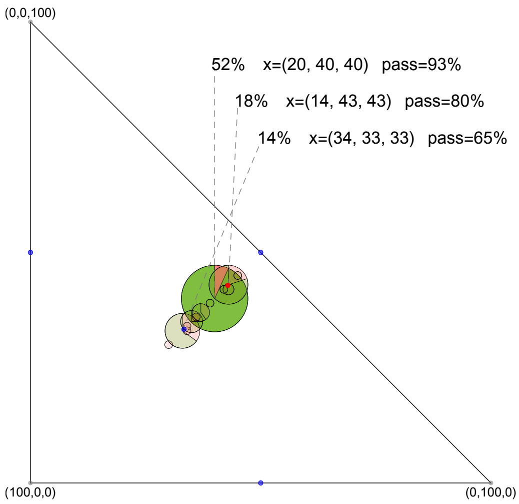


Figure 4.4: Proposals and passage rates, $c = (14, 43, 43)$, unanimity rule

(a) $(14, 43, 43)$ (N=140)



(b) $(43, 14, 43)$ (N=280)

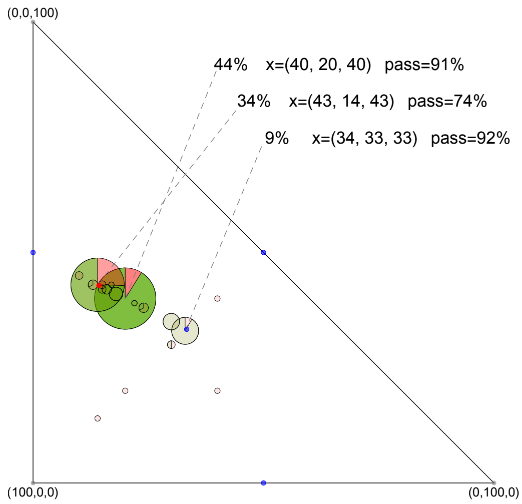


Table 4.4: Average proposed shares†

Percentage Contributions (c_0, c_1, c_2)	*Unanimity Rule* Average Offers (y_0, y_1, y_2)	*Majority Rule* Average Offers (y_0, y_1, y_2)
(**33**, 33, 33)	(33, 33, 33)	(36, 34, 30)
(**20**, 20, 60)	(26, 25, 48)	(31, 29, 40)
(**60**, 20, 20)	(53, 24, 23)	(55, 25, 20)
(**14**, 43, 43)	(22, 39, 39)	(28, 39, 33)
(**43**, 14, 43)	(40, 19, 40)	(43, 16, 41)

† When responder contributions are the same, they are the same, they are ordered according to the letter i.d. assigned to them in the experiment.

in relative points contributed. (Consistent with Hypothesis 3.)

In order to assess the statistical significance of these patterns, we take advantage of the fact that almost all proposals are located along the line connecting the proportional to the three-way equal split. This allows us to reduce the data to a single dimension, as follows. For each proposal y_i, we identify its *scalar projection* onto the line described by the equation

$$y_i = (1 - a_i) \cdot \text{equal split} + a_i \cdot \text{proportional split}$$

The corresponding value of a_i characterizes the point on the line which is closest to the proposal, i.e. whose connecting vector is orthogonal to the line. Thus, $a_i = 0$ corresponds to the equal, and $a_i = 1$ to the proportional split. After we identify the a_i for each proposal, we can look at the distribution of the a_i as well as its effect on voting and passage rates.

Figure 4.5 displays the distribution of a_i values in the $c = (20, 20, 60)$ situation. As above, the left and right panels show the situation where the proposer's own contribution is 20% and 60%, respectively. Within each bar, the lighter region represents the fraction of proposals that passed. Comparing the right to the left panel, we see that the distribution appears to be shifted to the right, with nearly twice as much weight on the proportional split (located at $a_i = 1$) when the proposer's own contribution is large. Using paired matching group averages as our unit of observation, we find that this difference is statistically significant (Wilcoxon matched-pairs signed-ranks test, $p < 0.01$, $N = 16$).

The corresponding distribution of a_i values for the $c = (14, 43, 43)$ situation are displayed in Figure 4.6. Again, we see that the distribution shifts to the right, i.e. towards

Figure 4.5: Distribution of a_i values, $c = (20, 20, 60)$, unanimity rule

(a) $(20, 20, 60)$ (N=174) (b) $(60, 20, 20)$ (N=87)

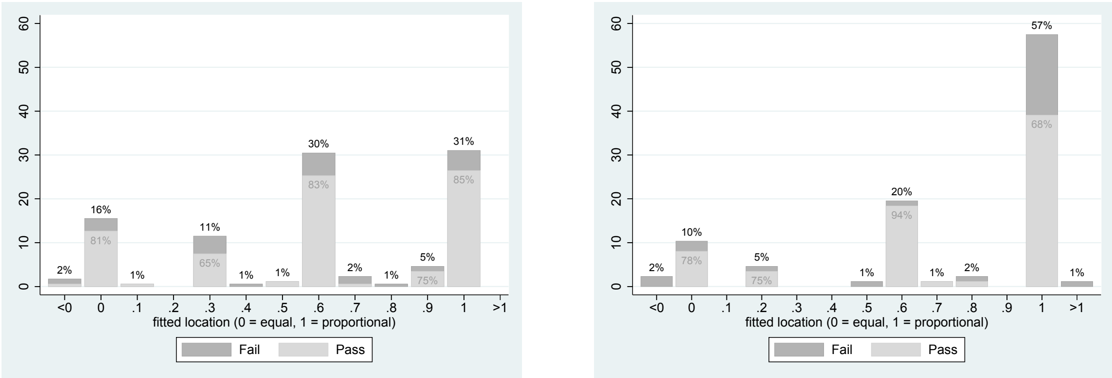
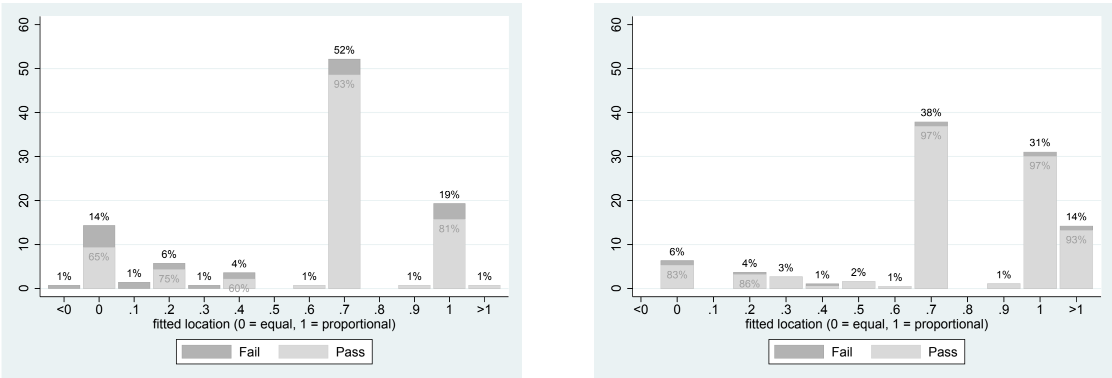


Figure 4.6: Distribution of a_i values, $c = (14, 43, 43)$, unanimity rule

(a) $(14, 43, 43)$ (N=140) (b) $(43, 14, 43)$ (N=280)



the proportional split, when the proposer has made a relatively large contribution (right panel). To test for significance, we compare the average values of a_i in all matching groups and find a significant difference (Wilcoxon matched-pairs signed-ranks test, $p < 0.01$, $N = 16$). Hence, in both asymmetric situations we find that proposals are more proportional if the proposer himself has made a relatively large contribution. This supports the first part of Hypothesis 4.

To assess the effect of proposal location on voting behavior, we run Random-effects probit regressions. Results for unanimity rule are summarized in the top part of Table 4.5. In each regression, the dependent variable is the voting decision, coded as $v_i = 1$ if a subject votes 'yes' and $v_i = 0$ otherwise. The independent variables are a_i and the period. For the $(20, 20, 60)$ situation, we find that the coefficient on a_i is positive and significant for responder 2 but insignificant for responder 1. That is, the subject with the larger claim is significantly more likely to vote yes if the proposal is closer to the proportional split. We observe a similar pattern in the $(43, 14, 43)$ situation.

Namely, the coefficient on a_i is positive and significant for responder 2 but negative and significant for responder 1. Hence, in this situation the individual with the larger claim is more likely to vote yes if the proposal is closer to the proportional split while the opposite is true for the individual with the smaller claim. We also find that the coefficient of a_i is positive in the $(14, 43, 43)$ situation where both responders have made a relatively large contribution. In contrast, we find no significant opposite effect of a_i on voting in the $(60, 20, 20)$ situation, where both responders have made a relatively small contribution. In summary, our results indicate that responders with relatively large contributions vote 'yes' more often the more proportional a proposal. On the other hand, we find only partial evidence that individuals with lower contributions less often vote 'yes', as suspected in the second part of Hypothesis 4.

Result 3. *In asymmetric situations and under unanimity rule, individuals who have made relatively large contributions make proposals that are closer to the proportional split than do individuals who have made relatively small contributions. Responders with large contributions are more likely to vote 'yes' on proposals closer to the proportional split. (Partially consistent with Hypothesis 4.)*

Table 4.5: Effect of proposal location on responder votes

		(20, 20, 60)	**(60, 20, 20)**	**(14, 43, 43)**	**(43, 14, 43)**
Unanimity rule	Responder 1	-.03	-.09	.21 ***	-.48 ***
	Responder 2	.24 ***			.07 *
	# of obs	(174)	(174)	(280)	(280)
	# of ids	(74;60)	(74)	(85)	(67;85)
Majority rule	Responder 1	-.14***	-.48***	.34***	-.70 ***
	Responder 2	.434 ***			.19 ***
	# of obs	(234)	(234)	(232)	(232)
	# of ids	(90;67)	(90)	(86)	(57;86)

Notes: The table reports average marginal effects of proposal location. ($a_i = 0$ and $a_i = 1$ correspond to equal and proportional splits.) The coefficient can *roughly* be interpreted as the effect of proposing the proportional rather than the equal split. (However, it is not evaluated at the equal split.)

Turning to rates of passage, it is apparent that proposals fail more often in the asymmetric situation (Figures 4.3 and 4.4) than in the symmetric situation (Figure 4.1, left panel). Table 4.6 presents information on the overall passage rates in each of the situations observed. Pooling all asymmetric situations, the overall rate of passage under unanimity rule is 79%, as compared to 94% in the symmetric situation. By comparing average passage rates within each matching group, we find that this difference is signif-

Table 4.6: Passage rate by situation (all first round proposals)

	(33,33,33)	(20,20,60)	(60,20,20)	(14,43,43)	(43,14,43)	Total
Unanimity	94%	78%	71%	82%	81%	83%
	189/201	136/174	62/87	115/140	228/280	730/882
Majority	95%	93%	84%	76%	95%	90%
	196/207	217/234	98/117	88/116	220/232	819/906
Rank Sum p†	0.95	0.01	0.67	0.89	<0.01	< 0.01

† Rank sum tests are based on fraction passed within each matching group (16 and 17 observations for unanimity and majority rule, respectively).

icant (Wilcoxon signed-ranks test, $p = 0.01$, $N = 16$). This supports our Hypothesis 5.

Result 4. *Under unanimity rule, the passage rate is larger in situations where claims are symmetric as compared to situations in which claims are asymmetric. (Consistent with Hypothesis 5.)*

4.5.3 Asymmetric claims, majority rule

Now we turn to the majority rule treatment, and continue to look at situations where subjects have heterogeneous claims. Figures 4.7 and 4.8 display the distribution of proposals and the corresponding passage rates in detail. A salient pattern in these figures is that proposals are concentrated in three distinct areas. As in the unanimity rule treatment, the vast majority is located along a line connecting the three-way equal to the proportional split. In addition, a small number of proposals are located along either the horizontal or vertical axis, corresponding to minimum winning coalitions with responder 1 or responder 2, respectively.

Looking only at the grand coalitions in the $c = (20, 20, 60)$ and the $c = (14, 43, 43)$ situations, we observe that the distribution of proposals shifts towards the proportional split when the proposer's contribution is relatively larger (right panels). In these cases the proposer suggests the proportional split three times as often in the $c = (20, 20, 60)$ (12% vs. 39%), and almost twice as often in the $c = (14, 43, 43)$ situation (18% vs. 34%). Although we observe few minimum winning coalitions ((20,20,60) 16%, (60,20,20) 19%, (14,43,43) 9%, (43,14,43) 18%), the distribution of offers within these coalitions seems to reflect claims. That is, a two-way equal split is proposed if both coalition partners have made the same contribution, whereas partners with higher (lower) contributions are offered more (less) than the two-way equal split. For example, in the (20,20,60) the average offers to responder 1 and 2 are 50 and 62%, respectively.

Figure 4.7: Proposals and passage rates, $c = (20, 20, 60)$, majority rule

(a) $(20, 20, 60)$ (N=86) (b) $(60, 20, 20)$ (N=43)

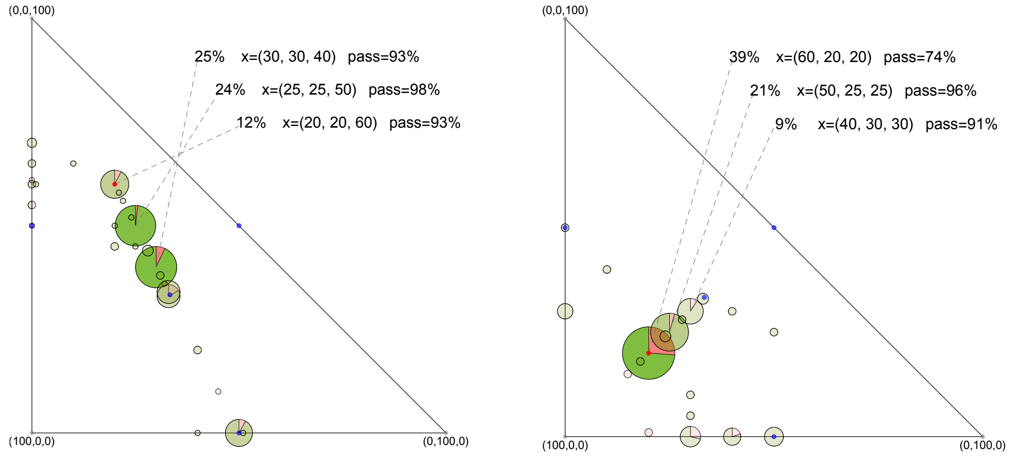
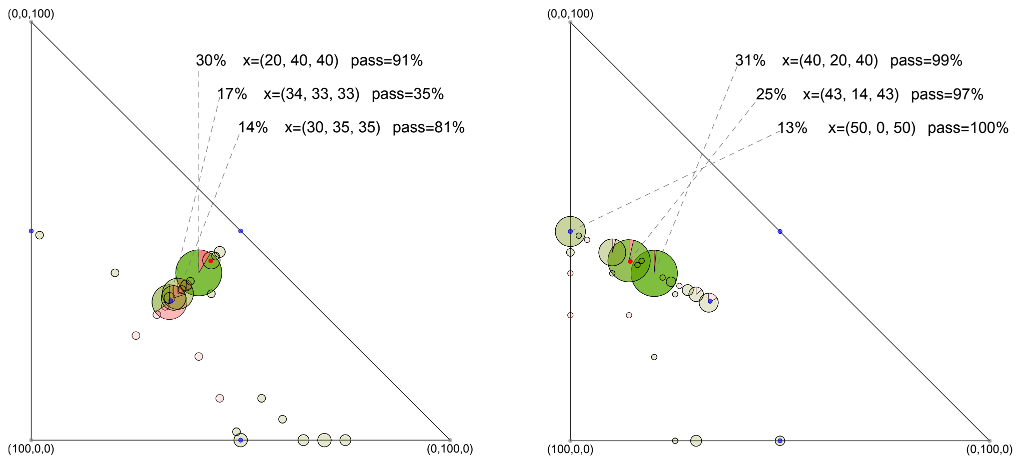


Figure 4.8: Proposals and passage rates, $c = (14, 43, 43)$, majority rule

(a) $(14, 43, 43)$ (N=86) (b) $(43, 14, 43)$ (N=43)



In the (43,14,43) situation average offers within MWCs are 37% to responder 1 and 50% to responder 2.

To study the composition and frequency of MWCs in more detail, we split proposals into three categories according to whether they are closest to one of the axes or the line connecting the equal and the proportional splits (extending out beyond those points).[16] Thus, by this definition, a proposal that allocates a very small share to one responder would be classified as a 'fitted' minimum winning coalition. Note that this measure will classify *more* proposals as MWCs than a more 'strict' definition would. The percentage of proposals that are thereby categorized as 'fitted' MWCs and 'fitted' grand coalitions is summarized in Table 4.7. The left and right parts of the table provide information on all periods and on the last 4 periods, respectively.

Table 4.7: PROPOSED COALITION COMPOSITION, MAJORITY RULE

	All periods				Periods 9-12			
	MWC with		Grand		MWC with		Grand	
Situation	resp. 1	resp. 2	coalition	N	resp. 1	resp. 2	coalition	N
(33,33,33)	10%	3%	87%	207	15%	6%	79%	48
(20,20,60)	12%	5%	83%	234	18%	3%	79%	94
(60,20,20)	18%	5%	77%	117	26%	6%	68%	47
(14,43,43)	12%	1%	87%	116	24%	0%	76%	38
(43,14,43)	3%	15%	82%	232	5%	25%	70%	76
Total	10%	7%	83%	906	16%	10%	74%	303

Notes: 'Situations' are defined such that the first coordinate is the proposer, the second and third are responder percentage contributions.

In every situation, we find that the vast majority of proposers (83%) build grand rather than minimum winning coalitions (MWCs). Although the fraction of MWCs increases somewhat over time, it remains low even in the last four experimental periods (26%). This evidence is inconsistent with Hypothesis 6. Comparing the fraction of MWCs across situations, we find that they are more frequent in asymmetric (18%) than in symmetric situations (13%). This difference persists, although smaller in size, in the last 4 periods (21% vs. 27%) and is significant (Wilcoxon matched-pairs signed-ranks test, $p = 0.06$, $N = 10$). Turning to the composition of MWCs, we do not find evidence that proposers systemically exclude members with higher claims as conjectured in

[16] For this purpose, we compute the scalar projection onto the line connecting the three-way equal and the proportional split. Thereafter, we calculate the Euclidean distance (ϵ) of the vector connecting a proposal to this line. In addition, we measure the distance to the horizontal and the vertical axes which are x_2 and x_1 respectively. By comparing the length of the three vectors, we are able to identify a proposal as 'fitted grand coalition' (i.e. $\epsilon < x_1$ and $\epsilon < x_2$) etc.

Hypothesis 7. In the (20,20,60) situation, proposers are indeed more likely to include responder 1 who has contributed a smaller share (Wilcoxon matched-pairs signed-ranks test, $p = 0.08$, $N = 8$). However, in the (43,14,43) situation, proposers are more likely to include responder 2 who has made a larger contribution (Wilcoxon matched-pairs signed-ranks test, $p < 0.01$, $N = 10$). This is despite the fact that responder 2 is offered higher shares when included in a MWC than responder 1 (see above). Hence, when responders have different claims, it appears that the proposer is more likely to include the responder who has contributed the same share as the proposer. Thus, we do not find that the responder with the higher claim is systematically excluded. This evidence stands in contrast to our Hypothesis 7.[17]

Result 5. *In asymmetric situations with majority rule, the vast majority of proposers attempt to build grand coalitions. Those who do build minimum winning coalitions are more likely to include the responder who has made the same contribution as themselves. (Inconsistent with Hypotheses 6 and 7.)*

Focusing only on the 'fitted' grand coalitions, Figures 4.9 and 4.10 provide histograms of the a_i values (calculated as above - see Subsection 4.5.2). Among the 'fitted' grand coalitions, we observe the same pattern as in the unanimity rule treatment. Namely, in both figures, the distribution of proposals seems to be shifted to the right, i.e. towards the proportional split, when the proposer has made a relatively large contribution (right panels). Using matching group averages of a_i as unit of observation, we find that the average values of a_i are indeed significantly larger when the proposer has made a relatively large contribution in both situations (Wilcoxon matched-pairs signed-ranks test; (20,20,60), $p < 0.01$, $N = 17$; (14,43, 43), $p < 0.01$, $N = 16$).

Result 6. *In asymmetric situations with majority rule, proposers with larger contributions are more likely to suggest the proportional split.*

Turning to voting behavior, we explore how the location of a proposal affects the decision to vote 'yes'. We do so separately for grand and minimum winning coalitions, starting with the latter. As would be expected, the most important determinant of voting on MWC proposals is whether a subject is included in the proposed coalition. If not, virtually all subjects (96%) vote 'no'. In contrast, those included vote 'yes' in

[17] In addition, we find that whenever responders have the same claims, proposers are more likely to include responder 1. Remember that we ordered responders according to the letter i.d. they received on the decision screen. That is, if the proposer's i.d. is 'A', responder 1 corresponds to the individual displayed as 'B' on the decision screen. If the proposer's i.d. was instead 'B' responder 1 corresponds to the individual displayed as 'A' on the decision screen. Hence, in both of these cases responder 1 is the person displayed below the proposer on the decision screen which might have affected the likelihood of receiving a positive offer.

92% of all cases. To test how the location of a proposal affects the decision to vote 'yes' within a MWC, we run a Random-effects probit regression[18], with the voting decision as dependent and the period as well as the share being offered as the independent variables. Our tests reveal that coalition members are more likely to vote 'yes' the higher the share they are offered (Average marginal effect, $\beta = 0.01$, $p = 0.04$).

In a second step, we explore voting behavior within the 'fitted' grand coalitions that we observe in the majority rule treatment. For this purpose, we again run a set of Random-effects probit models, using the voting decision as dependent and the period as well as a_i as independent variables. The bottom half of Table 4.5 reports the average marginal effects of a_i on the decision to vote yes. In the $(20, 20, 60)$ and the $(43, 14, 43)$ situations, the coefficient on a_i is negative (and significant) for responder 1 and positive (and significant) for responder 2. Consistent with this pattern, we find that the coefficient on a_i is negative (and significant) in the $(60, 20, 20)$ and positive (and significant) in the $(14, 43, 43)$ situation. Hence, our findings indicate that individuals with relatively large claims are more likely to vote yes if a proposal is closer to the proportional split while the opposite holds for individuals with smaller claims.

Result 7. *In asymmetric situations with majority rule, responders with larger contributions more often vote 'yes' the more proportional a proposal suggested in a grand coalition. Responders included in a MWC more often vote 'yes' the larger the share they are being offered.*

In a last step we explore passage rates. As displayed in Table 4.6, we observe that 89% of the proposals pass in the asymmetric situations. This is significantly smaller than the passage rate in symmetric situations which amounts to 95% (Wilcoxon matched-pairs signed-ranks test, $p = 0.01$, $N = 17$) which supports our Hypothesis 8.[19]

Result 8. *Under majority rule, the passage rate is larger in situations where claims are symmetric as compared to situations in which claims are asymmetric. (Consistent with Hypothesis 8.)*

[18] Each subject votes on the proposals of the other two group members. We use the voting decisions of each individual as panel variable assuming that voting decisions are independent of individual characteristics.

[19] We also do not find that the passage rate is larger in grand than in minimum winning coalitions (Wilcoxon matched-pairs signed-ranks test, $p = 0.18$, $N = 12$) although this result is based on few observations. If subjects were able to anticipate or learn this over time, the fact that we observe few MWCs suggests that individuals prefer to form grand coalitions. We are unable to test this conjecture given that we did not elicit beliefs over passage rates.

4.5.4 Majority versus Unanimity rule

So far, we have separately discussed outcomes under both rules. In contrast to our hypotheses, we find a remarkable number of similar patterns. First, average shares offered increase in relative points contributed under both decision rules (see Table 4.4). Hence, offers reflect claims even under majority rule. Second, we find that offers under both rules are concentrated on a line connecting the three-way equal and proportional splits, moving closer to the proportional split if the proposer has made a relatively larger contribution. Third, individuals with relatively large contributions are more likely to vote 'yes' the closer a proposal to the proportional split. In this section, we analyze how the decision rule itself affects offers as well as passage rates and explore differences in these common patterns.

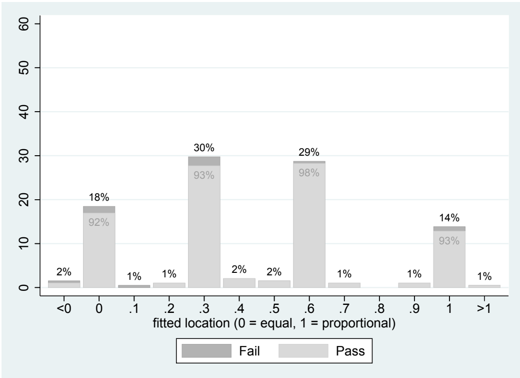
We start by comparing the distribution of grand coalition offers (i.e. distribution of a_i) between treatments. The corresponding distributions for the $(20, 20, 60)$ situation are displayed in the left panels of Figures 4.5 and 4.9. It appears that the distribution is shifted to the right (i.e. towards the proportional split) under unanimity as compared to majority rule. In particular, we observe almost twice as many proportional proposals under unanimity rule (31% vs. 14%). This is also the case in the $(14, 43, 43)$ situation, depicted in the left panels of Figures 4.6 and 4.10. Here, the fraction of proportional proposals is 19% under unanimity and only 5% under majority rule. By comparing the average values of a_i across matching groups, we find that proposals are indeed significantly closer to the proportional split under unanimity rule in both situations (Wilcoxon rank-sum test; $(20, 20, 60)$, $p = 0.02$; $(14, 43, 43)$, $p = 0.01$; $N = 33$). In contrast, we do not find that the decision rule has a significant effect in the $(43, 14, 43)$ (Wilcoxon rank-sum test, $p = 0.26$, $N = 32$) and the $(60, 20, 20)$ situations, i.e. when the proposer has made a relatively large contribution (Wilcoxon rank-sum test, $p = 0.8$, $N = 33$). These findings lend partial support for our Hypothesis 9.

Result 9. *Proposals under majority rule are less proportional (and more equal) as compared to unanimity rule in situations where the proposer's contribution is relatively small. In contrast, the degree of proportionality does not differ significantly when the proposer has made a relatively large contribution. (Partially consistent with Hypothesis 9.)*

As stated in Hypothesis 10, we are also interested in how the decision rule affects the incidence of delay. Given that delay is costly in our setting, this allows us to comment on the efficiency of agreements reached under both decision rules. Table 4.6 above summarizes the passage rates under both decision rules for each situation observed in our experiment. Averaged over all situations (including the symmetric ones), we find

Figure 4.9: Distribution of a_i values in 'fitted' grand coalitions, $c = (20, 20, 60)$, majority rule

(a) $(20, 20, 60)$ (N=195)



(b) $(60, 20, 20)$ (N=90)

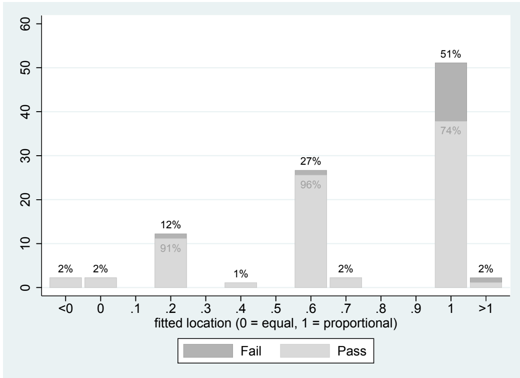
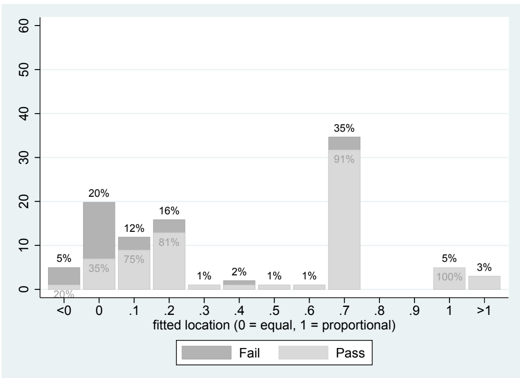
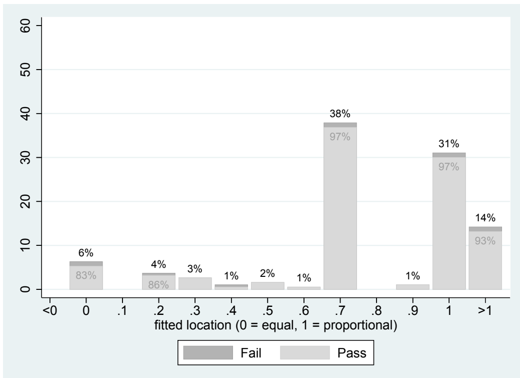


Figure 4.10: Distribution of a_i values in 'fitted' grand coalitions, $c = (14, 43, 43)$, majority rule

(a) $(14, 43, 43)$ (N=101)



(b) $(43, 14, 43)$ (N=190)



that the passage rate is significantly higher under majority than under unanimity rule (83% vs. 90%, Wilcoxon rank-sum test, $p < 0.01$, $N = 33$). This difference in passage rates is slightly higher in the asymmetric situations (78% vs. 89%, Wilcoxon rank-sum test, $p < 0.01$, $N = 33$). However, when comparing the passage rates in each situation separately, we find no significant differences in the $(60, 20, 20)$ situation, nor in the $(14, 43, 43)$ situation. Hence, we only find partial support for our Hypothesis 10.

Result 10. *On average, the passage rate is significantly higher under majority as compared to unanimity rule, especially when considering asymmetric situations only. However, when comparing the passage rates under unanimity and majority rule for each situation separately, we do not find significant differences in the* $(60, 20, 20)$ *and the* $(14, 43, 43)$ *situations. (Partially consistent with Hypothesis 10.)*

4.5.5 Final Outcomes

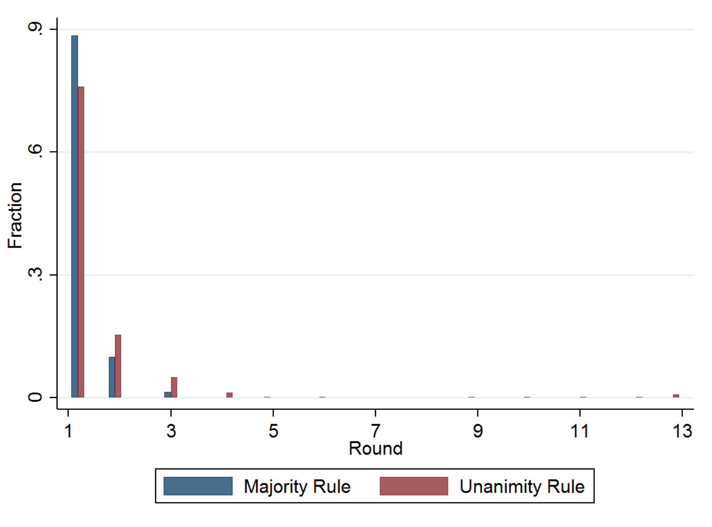
So far, our analysis has focused on the first proposals within each game. In this section, we will instead analyze final outcomes. As a first step, we want to assess how the decision rule affects the length of the bargaining process, i.e. how many rounds of bargaining were necessary before a given group reached agreement. Figure 4.11 plots the distribution of bargaining rounds in the majority and the unanimity rule treatment. Although many groups reach an immediate agreement under both rules (89% under majority and 82% under unanimity rule), we observe significantly more groups which continue to bargain over several rounds in the unanimity rule treatment (Wilcoxon rank-sum test, $p < 0.01$, $N = 33$). Hence, this is additional support for the hypothesis that unanimity rule leads to more delay as compared to majority rule.

To study final outcomes, we restrict our analysis to the first randomly selected proposal which passes.[20] In situations where the group members have made different contributions, we will not distinguish between the points contributed by the proposer and the two responders, but instead simply study the share of the surplus received by each group member.

Given the large share of proposals which pass immediately, we would expect that the final outcomes resemble initial proposals, analyzed in detail in the last sections. Figures 4.12 to 4.14 depict the distribution of final outcomes in all three situations. (In each Figure, players are ordered according to the size of their contribution, from low to high.) The left panels depict the distribution of final outcomes under unanimity rule, the right

[20] The number of observations that we observe for each constellation of points can be inferred from Table 4.2. Only one of the groups in the $(1, 1, 3)$ situation did not reach an agreement in the unanimity treatment. As in the previous sections, we will focus on relative contributions and, thus, pool the cases in which all group members have either contributed one or three points.

Figure 4.11: Rounds before reaching agreement



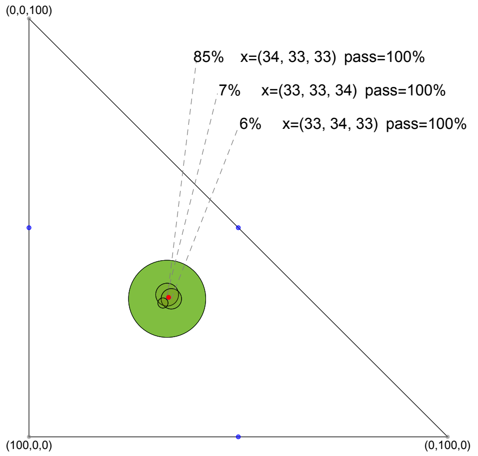
panels those under majority rule. Indeed, we observe the exact same patterns as in the previous sections: First, final bargaining outcomes are quite similar under both decision rules. Most notably, we continue to observe few minimum winning coalitions being formed under majority rule. Second, almost all grand coalitions are located on a line connecting the equal and the proportional splits. However, comparing the left and right panels of Figures 4.13 and 4.14, we see that outcomes move away from the proportional split under majority rule. For example, in the $(14, 43, 43)$ situation, the fraction of proportional outcomes falls from 49% to 34% when moving from unanimity to majority rule. Using scalar projections (see above), we find that outcomes are indeed significantly less proportional under majority rule in the (20,20,60) situation (Ranksum test, $p = 0.09$, $N = 33$) but not in the (14,43,43) situation (Ranksum test, $p = 0.14$). Hence, outcomes are less proportional under majority rule when a majority of individuals have made relatively small contributions (i.e. in the (20,20,60) situation) but not if a majority of individuals have made relatively large contributions (i.e. in the (14,43,43) situation).

Result 11. *The final outcomes in grand coalitions are less proportional under majority as compared to unanimity rule if at least two group members have contributed less than 33% to the surplus. Otherwise, we do not find any difference between the final outcomes in the majority and unanimity rule treatments.*

As noted above, we observe few MWCs among the final outcomes. Using the same classification of proposals as above, 17% of the final outcomes can be classified as fitted minimum winning coalitions, while 82% are grand coalitions. Table 4.8 depicts the relative frequency with which we observe MWCs for each pair of group members

Figure 4.12: Final outcomes in $c = (33, 33, 33)$

(a) Unanimity rule (N=67)



(b) Majority rule (N=69)

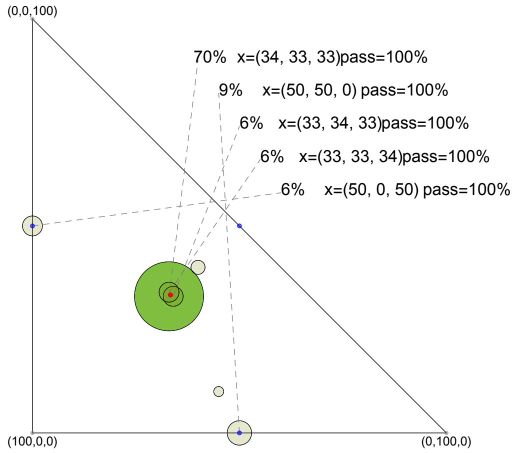
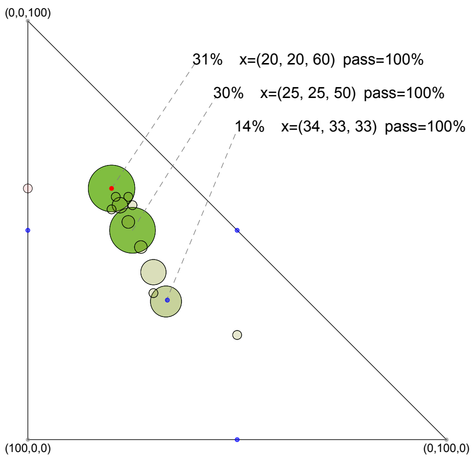


Figure 4.13: Final outcomes in $c = (20, 20, 60)$

(a) Unanimity rule (N=87)



(b) Majority rule (N=117)

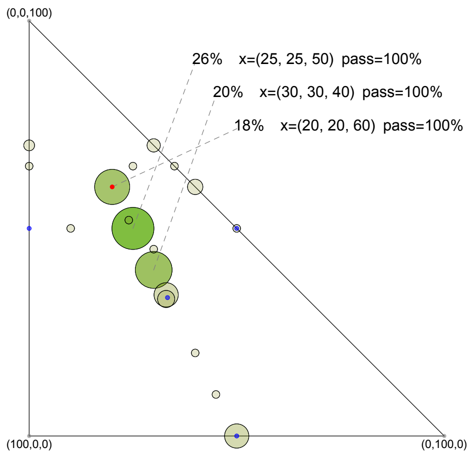
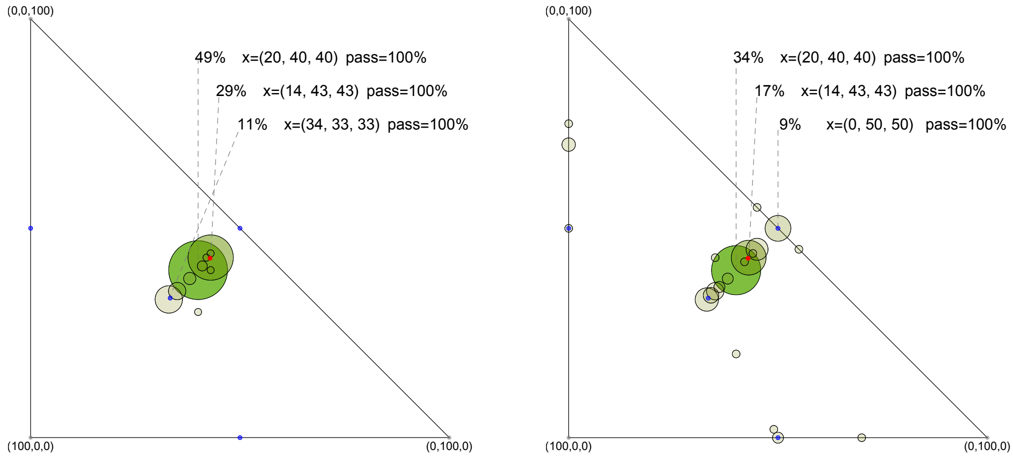


Figure 4.14: Final outcomes in $c = (14, 43, 43)$

(a) Unanimity rule (N=140) (b) Majority rule (N=116)



in all periods (left) and the last 4 periods (right). As in our previous analysis, we do not find evidence that group members with higher contributions are systematically excluded from MWCs. For example, in the (20,20,60) situation 20% of final outcomes suggest a MWC. Of these, 11% include the group member who has contributed 60% to the surplus.

Table 4.8: Coalition composition, final agreements (majority rule)

	MWC			Grand	Fitted MWC			Fitted Grand	
Situation	1&2	1&3	2&3	coalition	1&2	1&3	2&3	coalition	N
(33,33,33)	9%	6%	0%	86%	10%	6%	0%	84%	69
(20,20,60)	9%	3%	8%	81%	9%	3%	8%	80%	117
(14,43,43)	3%	4%	11%	82%	3%	4%	11%	81%	116
Total	6%	4%	7%	82%	7%	4%	7%	81%	302

4.6 Conclusion

We experimentally investigate how claims, derived from relative contributions to a commonly produced surplus, affect bargaining behavior and outcomes under two decision rules, namely unanimity and majority rule. Under unanimity rule, each group member possesses veto power which may be used to defend one's claim. Hence, while unanimity rule might result in fair (in the sense of proportionality) outcomes, endowing each party with veto power could cause severe delay. Majority rule, on the other hand, enables a minimum winning coalition to ignore the claims of a minority member. While this may reduce the degree of proportionality reflected in final outcomes and, consequently, be deemed unfair, requiring fewer group members to consent might allow groups to reach an agreement more quickly.

We study how claims affect fairness and efficiency in a laboratory experiment in which groups of three subjects first jointly produce a surplus and then bargain over the distribution of the surplus. Bargaining takes place in a finite horizon Baron and Ferejohn framework. Across treatments, we vary whether two or all three group members have to agree on a proposed division of the surplus. In line with previous evidence, we find that claims affect proposals and final outcomes under unanimity rule. Specifically, offers received increase in relative points contributed. A closer inspection reveals that virtually all proposals are located between the equal and the proportional split. In addition, we find that proposals are closer to the proportional split if the proposer has made a relatively large contribution, and hence benefits from receiving the proportional instead of the three-way equal share. Studying voting behavior, we find that individuals with higher claims are also more likely to vote yes the closer the proposal to the proportional split.

Turning to majority rule, we detect many similar patterns. In contrast to previous experiments without claims, we find that a majority of proposers suggests a grand instead of a minimum winning coalition and that average offers reflect the ranking of contributions. This is despite the fact that minimum winning coalitions are as likely to pass as grand coalitions. Although we observe few minimum winning coalitions, proposers are more likely to include group members who have made the same contributions. This behavior might result from the fact that there is a clear norm to share the benefits equally with partners who have contributed the same amount, while it is more difficult to assess how much needs to be offered to individuals with higher or lower contributions. Within grand coalitions, proposals are closer to the proportional split if the proposer has made a relatively large contribution. Thus, under both decision rules we find that proposers attempt to implement the proportional split more often if they have made a relatively large contribution. Conversely, they attempt to distribute the surplus more equally whenever they have made a relatively small contribution. In these latter cases, we find that proposals as well as final outcomes outcomes are closer to the equal split under majority as compared to unanimity rule. In terms of efficiency, we find that majority rule leads to a higher passage rate, especially in situations in which individuals have made different contributions.

While we do find that the decision rule affects proposer behavior, final outcomes as well as the incidence of delay, these differences are not as large as one might have expected based on previous Baron and Ferejohn experiments without claims. In these papers, differences in offers under unanimity and majority rule are mostly driven by the fact that proposers form minimum winning coalitions under majority rule. Our results suggest that the willingness to do so is substantially reduced when all individuals have contributed to the surplus via a real effort task. This result is likely to reflect fairness perceptions, i.e. proposers deliberately choose to respect claims because they regard this as fair.

Our paper shows that the differences between the two decision rules are instead more subtle in the presence of claims. In particular, we do observe that individuals strategically propose and approve less proportional distributions whenever this is to their own advantage and whenever the decision rule leaves them more discretion to ignore the claims of other group members (as under majority rule). This results in less proportional outcomes, whenever a majority of group members has contributed relatively little. Given that individuals seem to balance their offers between two prevalent fairness norms, proportionality and equality, this behavior may be indicative of a self serving bias in fairness norms. That is, in a given situation, individuals opportunistically choose the fairness norm which suits their own interests most (Messick and Sentis, 1983; Cappelen et al., 2007). Although the consequences for high contributors are not as drastic as, for example, being excluded from a coalition, this behavior certainly shows that individuals are willing to ignore the claims to the benefit of more equality within the group.

These (latter) findings may also be relevant for real world instances of bargaining with claims, such as budget allocation within the EU. Several recent reforms of the EU decision rules appear to be motivated by settling the conflict between redistribution from richer to poorer member states and preserving proportionality at the same time. While redistribution from poorer to richer member states is an explicit goal of the EU, richer member states provide most of the budget and also represent a majority of the population. Hence, preserving proportionality might be an important goal in order to secure support from the voters in these countries and to preserve the EU's legitimacy. Several recent voting reforms have indeed shifted voting rights from newer and poorer member states to older and richer member states. Research in political science suggests that this voting reform has led to more proportional outcomes which come at the cost of less equal outcomes. For example, with the 2004 enlargement the EU moved from the traditionally employed unanimity rule to a system with qualified majority rule and country voting weights, allocated roughly approximate to population. It has been shown that members with higher voting weights were in fact able to secure higher shares of structural and agricultural funds (Aksoy, 2010). The latest reform implemented a system of double majority, according to which a proposal passes if it is approved by 55% of the member states who represent at least 65% of the population. Effectively, this reform has been found to redistribute voting weights from newer towards older EU15 members, especially to Germany (Leech and Aziz, 2013). Although our experiment is not directly applicable to the complex institutional setting of the EU, we believe that it captures some relevant facts on how decision rules affect the distribution of benefits and may, thus, be informative for the public discourse about optimal decision rules.

Appendix 4.A

4.A.1 Analysis of excluded cases

In this section, we provide an analysis of all cases in which at least one of the responders has contributed 0 points to the surplus. We excluded these cases because they are relatively rare and do not occur in every matching group, leaving us with few independent observations to test for differences between and within treatments. Table 4.9 summarizes the frequency with which we observed the various constellations of points. Given that the relative contributions are the same in the first and the second as well as in the third and forth situation, we will pool these data in the subsequent analysis.

In situations, where relative contributions differ, we will distinguish whether the proposer has made a small, an intermediate or a large contribution. Given that all subjects in each group make a proposal, we observe three proposals for each game. Table 4.10 presents the number of proposals we observe in each of the 7 possible situations. The first coordinate of the contribution vector denotes the relative points of the proposer. When responder contributions differ, they are ordered such that the responder with the smaller contribution is listed first. When the responder contributions are the same, they are ordered alphabetically, according to the letter i.d. that players were assigned at the beginning of the game.

Table 4.9: Constellation of points contributed (excluded cases)

		Number of games	
Contributions	Surplus	Majority rule	Unanimity rule
(0,0,1)	5 EUR	12	3
(0,0,3)	15 EUR	8	5
(0,1,1)	10 EUR	24	10
(0,3,3)	30 EUR	15	29
(0,1,3)	20 EUR	43	42
Total		102	89

We begin by discussing outcomes under unanimity rule. Figures 4.15 to 4.17 display the distribution of proposals under unanimity rule. In all figures, the left panels display the cases in which the proposer has contributed nothing, while the right panels display cases in which the proposer has made a positive contribution.

In the $c = (100, 0, 0)$ and the $c = (0, 50, 50)$ situation we observe that a large majority of proposals is located on a line connecting the equal and the proportional splits. Proposals which are not located on this line are almost always rejected. In the $c =$

Table 4.10: Situations observed (Excluded cases)

Percentage Contributions	Number of proposals Majority rule	Unanimity rule
(0,0,100)	40	16
(100,0,0)	20	8
(0,50,50)	39	39
(50,0,50)	78	78
(0,25,75)	43	42
(25,0,75)	43	42
(75,0,25)	43	42
Total	306	267

Figure 4.15: Proposals and passage rates, $c = (0, 0, 100)$, Unanimity rule

(a) $(0, 0, 100)$ (N=16) (b) $(100, 0, 0)$ (N=8)

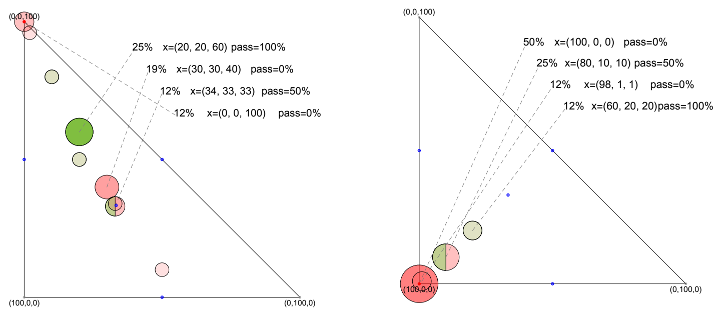


Figure 4.16: Proposals and passage rates, $c = (0, 50, 50)$, Unanimity rule

(a) $(0, 50, 50)$ (N=39) (b) $(50, 0, 50)$ (N=78)

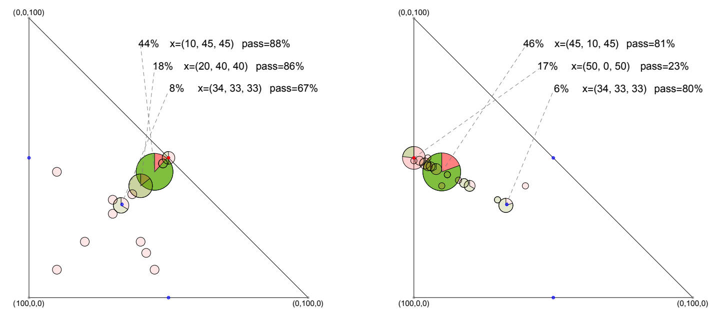
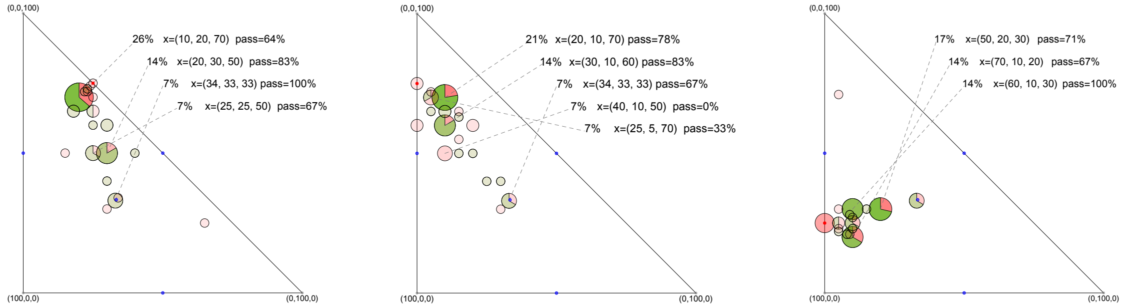


Figure 4.17: Proposals and passage rates, $c = (0, 25, 75)$, Unanimity rule

(a) $(0, 25, 75)$ (N=42) (b) $(25, 0, 75)$ (N=42) (c) $(75, 0, 25)$ (N=42)



$(0, 25, 75)$ situation, proposals are *concentrated around* the line connecting the equal and the proportional splits. The fact that proposals are farther away from the line might be explained by the fact that all subjects have contributed different amounts, making it more complicated to target points on the line. As in our main analysis, we find that the distribution of proposals appears to be closer to the proportional split whenever the proposer has contributed a positive share (right panels) as compared to having contributed nothing. We do, however, observe a very small passage rates among proportional splits. Most notably, the proportional split is always rejected in the $(0, 0, 100)$ situation. Given that almost all proposals are located on (or close to) the line connecting the equal to the proportional split, we reduce the data to a single dimension by identifying the scalar projection onto the line for each proposal (see section 4.2). Hence, each proposal is characterized by a value a_i. As in the previous Results section, $a_i = 0$ corresponds to the equal and $a_i = 1$ to the proportional split. We use the average values of a_i within each matching group to test whether proposals are closer to the proportional split whenever the proposer has made a relatively large contribution. Only in the $c = (0, 50, 50)$ situation we find that proposals are indeed closer to the proportional split when the proposer's contribution is 50% compared to cases in which the proposer has contributed nothing (Wilcoxon matched-pairs signed-ranks test, $p = 0.02$, $N = 12$). In all other cases, we do not find any significant differences ($(0, 0, 100)$ versus (100,0,0): $p = 0.12$, $N = 6$; $(0, 25, 75)$ versus $(75, 0, 25)$: $p = 0.66$, $N = 13$).[21]

Now, we turn to discussing proposals under majority rule. The relevant distributions of the proposals are displayed in Figures 4.18 to 4.20. It is apparent that proposals are concentrated in three areas: As in the unanimity rule treatment, the vast majority of proposals is located on a line connecting the equal and the proportional split. In

[21] Given that we do not observe any constellation of points in all 16 matching groups, the number of observations that we use for our tests ranges from 6 to 13.

Figure 4.18: Proposals and passage rates, $c = (0, 0, 100)$, Majority rule

(a) $(0, 0, 100)$ (N=40) (b) $(100, 0, 0)$ (N=20)

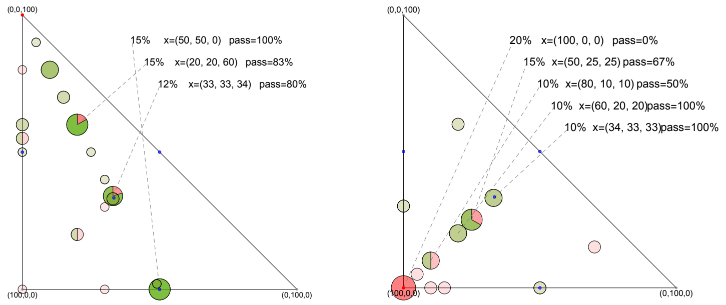


Figure 4.19: Proposals and passage rates, $c = (0, 50, 50)$, Majority rule

(a) $(0, 50, 50)$ (N=39) (b) $(50, 0, 50)$ (N=78)

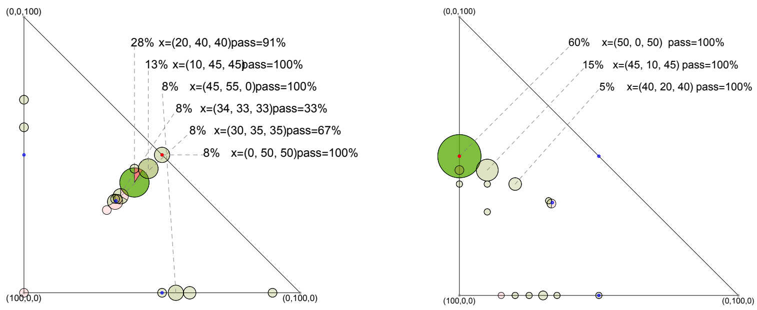
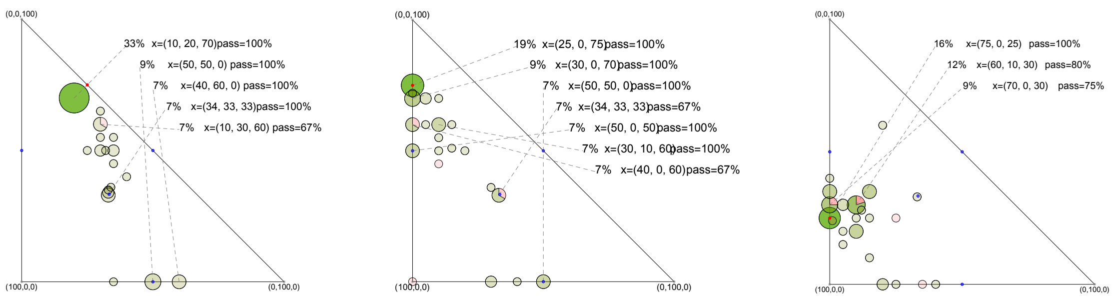


Figure 4.20: Proposals and passage rates, $c = (0, 25, 75)$, Majority rule

(a) $(0, 25, 75)$ (N=43) (b) $(25, 0, 75)$ (N=43) (c) $(75, 0, 25)$ (N=43)



addition, many proposals are located along either the horizontal or the vertical line, corresponding to minimum winning coalitions with responder 1 or responder 2, respectively. We begin our discussion of results by looking at the size of coalitions under majority rule. Table 4.11 summarizes the share of proposals which can be classified as minimum winning coalitions, allocating 0 to at least one other group members, and grand coalitions. To differentiate between attempted minimum winning coalitions and proportional splits, we report the fraction of proportional proposals separately. The left and right parts of the table provide information on all periods and on the last 4 periods, respectively. Most notably, we find that proposers are more likely to build grand coalitions instead of minimum winning coalitions in all situations (24% vs. 52%). This difference is, however, much smaller in the last 4 rounds (37% vs. 39%), i.e. after subjects have gained some experience.

Interestingly, we observe that 23% of the proposers suggest the proportional split. This fraction is especially high in situations where the proposer has made a positive contribution. The high fraction of proportional proposals is indeed interesting given that we observe few minimum winning coalitions being proposed in this and our main analysis presented in section 4.5. Thus, our findings suggest that proposers may be more willing to offer nothing to some group members if such proposals can be justified by proportionality.

In order to study the composition of minimum winning coalitions, we computed the inclusion frequencies for each responder. In contrast to our results in section 4.5.3, we do not find that proposer are more likely to include the individual who has made the same contribution in the first 4 situations. For example, in the $(50, 0, 50)$ situation, proposers are more likely to include responder 1 instead of responder 2 who has contributed the same share of points as the proposer. In the last three situations, where all group members have made different contributions, we find that proposers are more likely to include responder 2 (who has contributed a positive amount in all cases) if the proposer has made a positive contribution himself. Instead, if the proposer has contributed nothing, responder 2 is never included.

Table 4.11: Coalition Composition (Excluded cases)

	all periods					periods 9-12				
	MWC with		Grand	Propor-		MWC with		Grand	Propor-	
Situation	resp. 1	resp. 2	coalition	tional	N	resp. 1	resp. 2	coalition	tional	N
(0,0,100)†	18%	15%	65%	0%	40	31%	19%	44%	0	16
(100,0,0)	15%	5%	60%	20%	20	13%	13%	50 %	25%	8
(0,50,50)†	18%	5%	67%	8%	39	44%	11%	44%	0 %	9
(50,0,50)	9%	4%	27%	60%	78	28%	0 %	6%	67%	18
(0,25,75)	19%	0%	81%	0%	43	29%	0%	71 %	0 %	17
(25,0,75)†	14%	26%	40%	19%	43	24%	24%	24%	30%	17
(75,0,25)	12%	19%	53%	16%	43	24%	6%	47%	24%	17
Total	14%	10%	52%	23%	306	27%	10%	39%	23%	102

† In each of these three situations, we observe one proposals in which the proposer suggests 100% for himself. Given that this is neither a minimum winning coalition with responder 1 or responder 2 nor a grand coalition, we can classify less than 100% of the proposals in these three situations.

In order to study the distribution of proposals in more detail, we turn to Figures 4.18 to 4.20. Looking only at the grand coalitions, it appears that the distribution of proposals is closer to the proportional split if the proposer has made a positive contribution (right panels). As noted above, in these cases the proposer also suggests the proportional split more often. However, this attempt to distribute the surplus more proportionally, leads to a high rejection rate whenever two individuals have contributed less than the equal split ((75,0,25) and (100,0,0)). In order to test whether the distribution is significantly closer to the proportional split whenever the proposer has made a positive contribution (right panels), we first compute the number of 'fitted grand coalitions' (see section 4.5.3) and then compare the average values of a_i within matching groups. Only in the $c = (0, 50, 50)$ situation, we find that the average values of a_i are indeed larger when the proposer has made a positive as compared to no contribution (Wilcoxon matched-pairs signed-ranks test, $p = 0.03$, $N = 12$). Hence, in these cases proposals are indeed significantly closer to the proportional split if the proposer has made a positive contribution. In the other two situations, we do not find any difference (Wilcoxon matched-pairs signed-ranks test; (0,0,100) vs. (100,0,0), $p = 0.12$, $N = 6$; (0,0,100) vs. (75,0,25), $p = 0.66$, $N = 13$). Our tests are, however, based on a very small sample because we did not observe each situation in all 17 matching groups.

Turning to the passage rate, Table 4.12 summarizes the passage rate in each of the situations. First and most notably, the passage rate is smaller compared to the situations in which all group members have made a positive contribution (83% under unanimity, 90% under majority rule, see discussion in section 4.5.5). Second, the passage rate is significantly smaller under unanimity as compared to majority rule in all situations. Hence, compared to the situations discussed in section 4.5.5, we find a more pronounced difference between the passage rates under majority and unanimity rule.

Table 4.12: Passage rate by situations (excluded cases)

	(0,0,100)	(100,0,0)	(0,50,50)	(50,0,50)	(0,25,75)	(25,0,75)	(75,0,25)	Total
Unanimity	44%	25%	64%	58%	64%	55%	62%	59%
rule	7/16	2/8	25/39	45/78	27/42	23/42	26/42	155/267
Majority	80%	50%	85%	95%	98%	91%	88%	88%
rule	32/40	10/20	33/39	74/78	42/43	39/43	38/43	268/306
Rank-sum p	0.01	0.89	$<$0.01	$<$0.01	0.03	$<$0.01	0.02	$<$0.01
N †	6/8	6/8	12/13	12/13	14/13	14/13	14/13	14/16

† The Wilcoxon rank-sum test test is based on the passage rate within each matching group. Given that we do not observe all situations in every matching group, we report the number of observations in the Unanimity / Majority treatment in this row

4.A.2 Instructions

-- Page 1 --

Dear Participant,

Thank you for attending this experiment. Before we describe today's experiment in more detail, we would like to inform you about some general rules:

General rules:

- This experiment lasts for approximately 70 minutes. During this time, you should not leave your seat.
- Please turn off your mobile phone and store it in your pocket or bag. There should not be anything on your table. (A beverage is of course allowed.)
- Please be quiet during this experiment and do not talk to other participants.
- If you have any questions, please raise your hand and wait for the experimenter to attend you at your seat.
- For your participation, you will receive a four Euro show-up fee. However, you can earn more money in this experiment. How much money you can earn depends on your own as well as the choices of other participants.

What happens at the end of this experiment?

Once all participants have finished this experiment, the experimenter will call the participants to the front desk one after another. You will then receive your payment.

-- Page 2 --

Description of the experiment

This experiment has **two parts**.

Part 1 consists of **12 quiz blocks**. In each quiz block you have to answer two questions. For each question, 4 possible answers are given. Only one of these answers is correct.

In each quiz block you can earn between 0 and 3 points by selecting the correct answers: You earn 1 point if you are able to answer one question correctly. If you answer both questions correctly, you earn 3 points. However, if you answer none of the two questions correctly, you will earn 0 points. You will not be informed how many points you collected in any of the quiz blocks.

Please note: **All participants have to answer the exact same questions.**

Part 2 of this experiment also consists of 12 rounds. At the beginning of each round, groups of 3 participants will be randomly formed. For each round, the computer **randomly selects one of the 12 quiz blocks from part 1 for each participant in the group**. The points that the group member have collected in the randomly chosen quiz blocks will be added**. For each point collected, the group receives five Euro.** The group's task is to bargain over the distribution of this surplus.

You will receive more instructions for part 2 after you and all other participants have completed part 1.

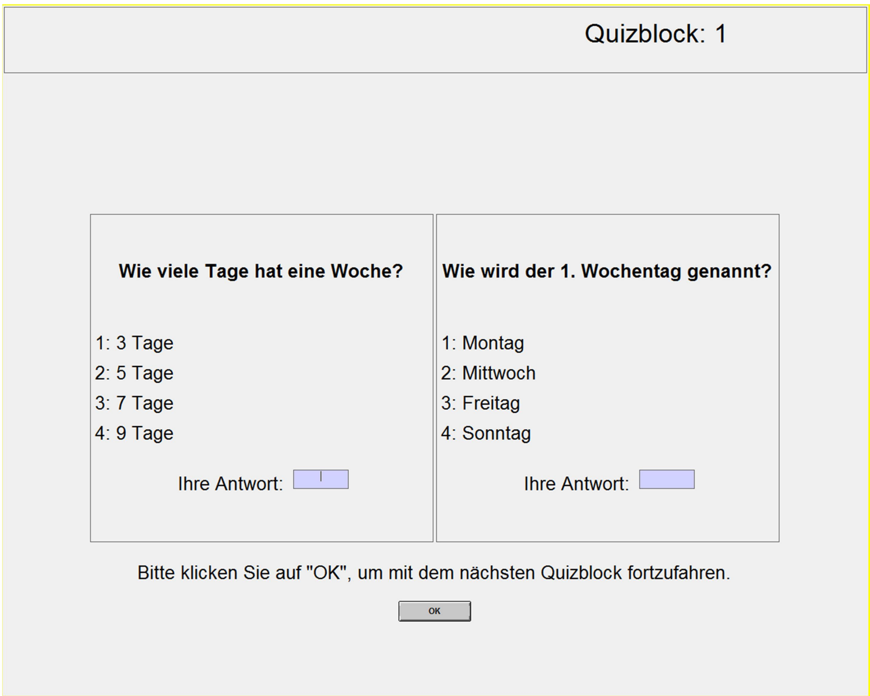
Your payment at the end of the experiment

Once all groups have finished part 2, the computer randomly selects one of the 12 rounds in part 2 of this experiment. All participants receive the amount agreed upon in this randomly selected round.

--- **Page 3** --

Examples for part 1:

Here is an example of what you will see in each of the 12 quiz blocks (in German):



- Displayed on the top right of the screen are the quiz block number.
- The first question is displayed in the left; the second question is displayed in the right box.
- The 4 possible answers are displayed below each question and numbered from 1 to 4.
- Please type the number of the correct answer into the field labeled “Your answer”. For example, if you think that answer number 1 is correct, type “1” into the field.
- As soon as you have typed an answer into both fields, please click on the “OK” button. You will then move to the next quiz block.

--- **Page 4** --

Details for the 2nd part of this experiment

Part 2 of this experiment consists of **12 rounds**. At the beginning of each round, **groups of three participants will be randomly formed**. Thus, you will interact with different participants in each round. No participant will know with whom he or she has been grouped during the experiment.

At the beginning of every round, each participant in a group will be assigned an ID (“A”, “B” or “C”). These IDs remain fixed throughout the round.

In every round, the computer **randomly selects one of the 12 quiz blocks for each participant**. Then, each participant will be informed which quiz block has been drawn for him / her personally and he /she will see how many points he / she has collected in the randomly selected quiz block. You will also be informed about the number of points collected by the other two group members. However, you will not be informed about the quiz block that was selected for the other two participants.

All points collected in the randomly chosen quiz blocks are then added. **The group receives 5 Euro for each point collected by its members.** For example, if all three participants have collected 3 points, the group receives 15 Euro.

The group’s task is to bargain how to divide the surplus which the group has received among the members of the group.

Decisions are made by majority rule, using the following procedure:

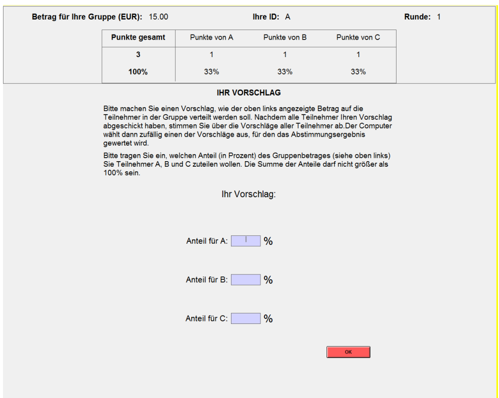
First, every participant makes a **proposal** as to how much each group member should receive (expressed in percent of the surplus). Next, all group members **vote “yes” or “no”** on the proposal of each group member. Finally, **one of the proposals** is randomly chosen and votes are counted. If **at least two group members voted “yes**” on the randomly chosen proposal, it **passes** and the round ends. If less than two group members voted “yes”, the proposal is **rejected** and bargaining continues. In this case, the available surplus **shrinks** by 20 percent (e.g. from 15 to 12 Euro). Then, all participants make a proposal and vote on the proposals of all group members. If the randomly chosen proposal is rejected again, the surplus shrinks by 20 percent once more (e.g. from 12 to 8.60 Euro), etc.

The round ends as soon as at least two group members vote “yes” on the randomly chosen proposal. In addition, a round ends if the available surplus **shrinks below 2 Euro**. In this case, all group members receive 0 Euro.

-- Page 5 ---

Examples for part 2:

Here is an example of what you will see on the proposal screen (in German):



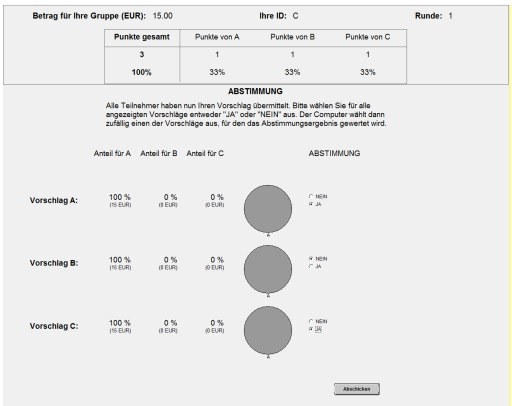
- Displayed on the top are the current period, your id and the available surplus (in Euro).

- The table displays how many points the group has collected in total. In addition, the table reports each group member's contribution in points and his/her share of contributed points in percent. (The displayed shares are rounded.)
- Below, you will find three boxes into which you must type your proposal. You must type the share of the pie (%) you wish to allocate to "A" (upper box), the share of the pie (%) you wish to allocate to "B" (middle box), and the share of the pie (%) you wish to allocate to "C" (lower box). You can allocate at most 100 percent.

After all three participants in the group have submitted a proposal, you will move to the voting screen.

-- Page 6 --

Here is an example of what you will see on the voting screen (in German). In this Example, we assume that all group members propose to give 100 percent off the surplus to participant "A".

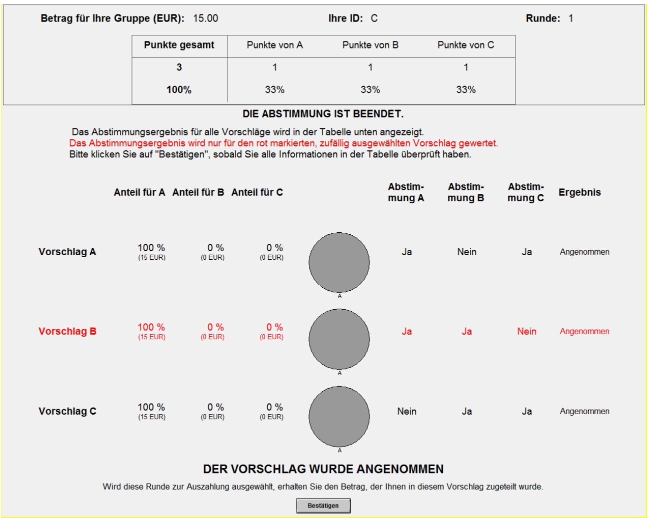


- The top part of the screen contains the same information as the previous proposal screen.
- Below, you will see each of the submitted proposals displayed both numerically (percent share and exact amount in Euro) and graphically (as pie chart).
- To the right of each proposal, you will find the buttons used to vote on the proposals.
- After selecting yes or no for each proposal, click submit to cast your votes.

As soon as all group members have cast their votes, you will move to the Results screen.

-- Page 7 --

Here is an example of what you will see on the Results screen (in German):



- The proposals are displayed on the left side of the screen.
- On the right side, you can see whether the other participants voted “yes” or “no” on a proposal. At the very right, you will be informed whether the proposal has passed or whether it has been rejected.
- The votes will only count for the randomly selected proposal, marked in red.

Chapter 5

Legislative bargaining with costly communication [1]

Abstract
We investigate the effects of voting rules on delay in a multilateral bargaining experiment with costly communication. Our design is based on a variant of the Baron-Ferejohn framework. Communication takes place after a proposer is selected and before a proposal is made. In contrast to prior experiments involving communication, it is directly associated with costs in our setup. Specifically, every second of communication increases the probability that the game is terminated before a proposal can be made. In case of 'breakdown', each player receives an exogenously fixed disagreement value. These values sum up to less than the size of the available surplus, implying that delay due to communication is costly. We vary the decision rule (majority versus unanimity) as well as the distribution of disagreement values (symmetric or asymmetric). We find that unanimity rule leads to longer communication and a higher frequency of breakdown in asymmetric, but not in symmetric situations.

Keywords: bargaining, communication, Baron and Ferejohn bargaining game, distributional preferences, proportionality, fairness, experiments

JEL Classification: C72, C78, C91, D33, D63

[1] This chapter was jointly written with Christoph Vanberg.

5.1 Introduction

One of the most basic problems in Public Choice Theory is the choice of a decision rule to be used by a committee. Going back at least to Buchanan and Tullock (1962), several authors have investigated the relative merits of alternative q-majority rules, with simple majority rule and unanimity rule as polar cases. The central trade-off identified in this line of research is that unanimity rule has the advantage that any decision taken must constitute a compromise that is acceptable to all parties, i.e. a Pareto improvement over the status quo. The most important disadvantage of unanimity rule is that it may be associated with greater costs of decision making, most notably in the form of *delay*.

A number of authors have used observational data to investigate whether unanimity rule is indeed associated with greater delay in decision making. For example, in the context of the European Union, Schulz and König (2000) and König (2007) measure the time lag between the initiation of a legislative proposal through the commission and the council's decision on a final proposal. They find that issues which require unanimous consent are associated with a larger time lag than those for which majority rule is used. A causal interpretation of these findings can, however, be questioned given that majority and unanimity rule are applied to substantively different issues.

For this reason, it is interesting to look at experiments, in which the issue under consideration can be held constant. Several studies have investigated the link between decision rules and delay in multilateral bargaining (Miller and Vanberg, 2013; Agranov and Tergiman, 2014; Miller and Vanberg, 2015; Agranov and Tergiman, 2017). These studies implement variants of the Baron Ferejohn 'divide-the-dollar' game. In this game, individuals take turns in proposing allocations of a given surplus. Depending on the decision rule, a proposal passes if either a majority or all group members vote 'yes'. If a proposal fails, the available surplus is discounted and a new round of bargaining begins.

The discounting of payoffs in the Baron-Ferejon model reflects the assumption that a significant amount of time passes before a new proposal can be made. Thus, the main measure of *delay* in this model is the probability with which proposals fail. According to standard equilibrium predictions (discussed in more detail below), this should not occur under either decision rule. In contrast to these predictions, both Miller and Vanberg (2013) and Miller and Vanberg (2015) find that first round proposals fail with positive probability, and significantly more often under under unanimity as compared to majority rule. These experimental findings constitute causal evidence supporting the hypothesized link between unanimity rule and delay in decision making.

Further experimental evidence consistent with this idea is provided by Merkel and Vanberg (2017) and Miller et al. (2018), who investigate variants of the BF game which introduce asymmetries between players. Merkel and Vanberg (2017) conduct experiments in which subjects bargain over a surplus that is jointly produced. In situations where all subjects have contributed equally in production, they find no difference in passage rates under unanimity and majority rule. When contributions differ, passage rates under unanimity rule drop significantly, while those under majority rule remain at the same level. In Miller et al. (2018), failure of a proposal results, with some probability, in a 'breakdown' of negotiations. If and when breakdown occurs, each player receives an exogenously given payoff, which may differ between players. Consistent with the prior studies, they find that unanimity rule is associated with lower rates of passage. In addition, this difference is especially pronounced in the presence of asymmetries in the form of heterogeneous disagreement values. These findings suggest that unanimity rule is associated with greater delay, especially if there are fundamental asymmetries between players.

Conflicting evidence is provided by Agranov and Tergiman (2014) and Agranov and Tergiman (2017). These authors conduct Baron and Ferejohn experiments in which subjects are allowed to verbally communicate prior to making proposals. They find that allowing for communication virtually eliminates delay in the form of proposals failing, under both decision rules. Since verbal communication between bargaining partners is a realistic feature of group decision making in most real-world applications, this evidence raises the question whether unanimity and majority rule are actually equally efficient under realistic circumstances.

Indeed, observational data suggests that very few proposals that are formally voted on in legislatures ever fail. For example, statistics collected by govtrack.us show that, out of the 11,224 bills and resolutions that were introduced to the 115th Congress and referred to a committee, only 712 were put to a vote, of which only 9 failed.[2] Going back to the 93rd Congress (1973), the percentage of formal votes resulting in failure has consistently been below 1%.

These data demonstrate that the formal vote on a proposal is only the final step in an often lengthy process of largely *informal bargaining* between legislators. In reality, therefore, delay manifests itself in the length of these negotiations rather than in failure of formal proposals. Naturally, such delay will normally be associated with costs.[3]

[2] see https://www.govtrack.us/congress/bills/statistics

[3] As an example, consider the ongoing negotiations between EU member states regarding the handling of refugees. While these negotiations take place, the 'refugee crisis' continues, and the possible benefits of reform are delayed. Similarly, the longer the negotiation takes, the larger the risk that individual member states will take alternative measures, i.e. the opportunity to reach a mutually

Therefore, the observation that unanimity and majority rule lead to equal passage rates on formal proposals does not imply that they are equally efficient. The relevant question is whether unanimity rule leads to longer 'informal' negotiations leading up to a formal vote. For these reasons, we conduct experiments in which communication *itself* is associated with costs.

Our experimental design is based on the modified BF game introduced by Miller et al. (2018). We chose this game because it introduces asymmetries in the form of heterogeneous breakdown values. This allows us to investigate whether the decision rule has different effects in symmetric vs. asymmetric situations, as observed in prior experiments without communication. As explained above, the main feature of this game as originally formulated is that proposal failure leads, with some probability, to a breakdown of negotiations. In order to implement a version in which communication itself is associated with costs, we make the probability of breakdown depend upon the length of communication. Substantively, the idea is that lengthy 'informal' negotiations are associated with a risk that the opportunity to undertake a collective action will pass.

In our experiment, groups of three subjects bargain over the division of a fixed surplus. The bargaining process is divided into discrete 'rounds', and one subject is randomly assigned the role of 'proposer' in a given round. Prior to formally introducing and voting on a proposal, subjects can exchange messages via chat windows. At any point in time, the proposer can terminate this 'informal' negotiation in order to make a formal proposal. However, after the proposer has closed the chat, the game is terminated with a probability that depends on the length of time spent communicating. Specifically, every two seconds of communication increase the probability of breakdown by one percent. In this case, the surplus is lost and players are paid predetermined disagreement values.

Our main findings are the following. When all players have the same disagreement value, the decision rule has no impact on the total time spent communicating, aggregated over all 'rounds' of each game. Virtually all groups quickly agree on equal splits (either two- or three-way). A more detailed analysis reveals that the average time spent communicating is slightly shorter under unanimity as compared to majority rule. However, a larger number of proposals fail, such that there is no difference in the realized frequency of breakdown in the symmetric situations.

When disagreement values differ, unanimity rule is associated with significantly longer total communication time. Here, the more detailed analysis shows that the time spent communication within a given round is longer, and in addition more proposals fail

agreeable compromise may pass.

under unanimity rule. Both of these phenomena contribute to longer aggregate communication times as well as a higher frequency of breakdown. Thus, unanimity rule does appear to be associated with more inefficient delay in the asymmetric situations.

The rest of the paper is organized as follows. Section 5.2 describes the existing literature in more detail. Section 5.3 provides an overview of our experimental design and the procedures. Results are presented in Section 5.4. The last section concludes and discusses our results. Instructions and additional empirical analysis are presented in Appendix 5.

5.2 Related Literature

Several studies have analyzed the effect of decision rules on the length of negotiations in the Baron and Ferejohn (1989) bargaining game. In this stylized bargaining game, a committee of size n bargains over the division of a fixed surplus in a sequence of rounds. At the beginning of each round, one member is randomly chosen to propose an allocation and the committee members can vote 'yes' or 'no'. Depending on the decision rule, a proposal passes if it is approved by a majority or all members of the committee. In this case, the proposal is implemented and the game ends. Otherwise, the available surplus is discounted by a factor of $\delta < 1$ and a new round of bargaining begins. In Miller et al. (2018), failure of a proposal leads to a breakdown of negotiations with probability .2. In this case, players receive exogenously fixed disagreement values. The standard measure of costly delay in all of these experiments is the fraction of first round proposals that fail.

Table 5.1 summarizes the first round passage rates observed in these experiments. Miller and Vanberg (2013) conduct 3-player games with a discount factor of $\delta = .9$. Miller and Vanberg (2015) have groups of 3 and 7, with a discount factor of $\delta = .5$. Both studies find similar passage rates, with significantly more proposals failing under unanimity rule. As explained above, Merkel and Vanberg (2017) have groups of 3 players who have previously produced the surplus. Their discount factor is $\delta = .8$. They find significantly more delay under unanimity rule in situations where players have contributed different amounts to the surplus, but not when players contributed the same amounts. In the experiment with breakdown values, Miller et al. (2018) find lower passage rates under unanimity rule, especially if players have different (exogenous) disagreement values. In sum, these findings support the notion that unanimity rule is associated with greater delay, especially if there are fundamental asymmetries between players.

Table 5.1: First round passage rates

	Unanimity	Majority
No Communication		
Miller and Vanberg (2013)		
groups of 3, $\delta = .9$	70%	87%
Miller and Vanberg (2015)		
groups of 3, $\delta = .5$	74%	88%
groups of 7, $\delta = .5$	67%	75%
Merkel and Vanberg (2017)		
same contributions	94%	95%
different contributions	79%	94%
Miller et al. (2018)		
homogeneous disagreement values	46%	77%
heterogeneous disagreement values	35%	78%
With Communication		
Agranov and Tergiman (2014, 2017)		
without communication	57%	81%
with communication	93%	89%

In two recent experiments, Agranov and Tergiman (2014) and Agranov and Tergiman (2017) conduct Baron and Ferejohn bargaining games with surplus discounting. Across treatments, they exogeneously manipulate whether players are able to discuss prior to making formal proposals. They find that introducing communication virtually eliminates differences in passage rates. As indicated above, a possible interpretation of these findings is that allowing for communication causes participants to engage in informal bargaining prior to making proposals. Since informal bargaining is costless in these experiments, players can continue chatting until they verbally agree on a proposal which they are reasonably confident will pass. This allows groups to avoid efficiency losses resulting from proposals being rejected. With this in mind, it is perhaps not too surprising that the decision rules are not associated with differences in efficiency when communication is *free*. It is, however, important to remark that the two papers by Agranov and Tergiman (2014) and Agranov and Tergiman (2017) were interested in how communication affects proposals, and in particular proposer power.

Given the prior evidence reviewed above, an important unanswered question is how the decision rule affects the length of informal negotiations pior to voting. This question has been studied in jury experiments, where groups of subjects act as jurors on a fictitious case. Foss (1981) finds that groups discuss on average twice as long under unanimity as compared to qualified majority rule. Note, however, that jury experiments represent situations of common interest, in which all jurors would want to convict a defendant that is guilty, whereas Baron and Ferejohn bargaining experiments represent situations with misaligned preferences.

5.3 Experimental Design

Our experimental design is based on a modified version of the Baron and Ferejohn (BF) bargaining game introduced by Miller et al. (2018). This game works as follows: As in the standard BF game, bargaining proceeds over a potentially infinite number of discrete 'rounds'. At the beginning of each round, one player is randomly chosen to propose a division of the surplus, which is immediately voted on by all group members. Depending on the decision rule, a proposal passes if either all three or a majority of two group members vote 'yes'. In this case, the game ends and the points were distributed according to the proposal. If a proposal fails, the game continues to another round with an exogenously given probability δ. With probability $(1-\delta)$, the game is terminated. In the latter case, each player is paid an exogenously specified 'disagreement value'. We chose this version of the game because it allows us to easily introduce asymmetries between the players, in the form of different disagreement values.

We introduce communication by allowing subjects to communicate using a chat window. As in the experiments of Agranov and Tergiman (2014, 2017), communication takes place after a subject has been identified as proposer, and prior to making a formal proposal. Subjects could exchange both 'public' messages visible to all group members and 'private' messages visible only to specific players. Communication continued until the proposer decided to terminate the chat and move to a proposal screen.

To introduce the notion that communication is costly, the time spent on the chat screen determined the likelihood that the game would terminate immediately after the chat. Specifically, every two seconds spent on the chat screen increased the breakdown probability by one percent. Throughout the chatting phase, the current breakdown probability was displayed to subjects as a counter above the chat window. Once the proposer ended the chat, a random number between 1 and 100 was drawn for each group. If the randomly drawn number was smaller than (or equal to) the breakdown probability, the game was terminated immediately and players received their disagreement values (see next paragraph). Otherwise, the proposer entered a proposal which was then immediately voted on. In order to prevent proposers from forcing a certain breakdown of the game, we imposed a 60 second maximum communication time.

In the event of breakdown, each group member received a pre-determined number of points (his disagreement value). These points added up to 60 points in every period (making agreement efficient) but their distribution was varied. Specifically, we implemented three different constellations of disagreement values, each of which was repeated in four consecutive periods. The first distribution was symmetric and assigned each player 20 points. These situations are perhaps most comparable to standard BF games without disagreement values. The other two situations were asymmetric. One situation assigned 60 points to one group member (and 0 to both others), the second assigned 40, 20 and 0 points to the three group members. In the two asymmetric situations, disagreement values were randomly assigned and remained fixed at the individual level for all four periods. To control for order effects, the sequence of situations varied between sessions. At the end of the experiment, one of the 12 periods was randomly selected and subjects were paid 0.35 EUR for each point they had received.

The experiment was conducted at the KD2 Lab in Karlsruhe, Germany in September 2017. In total, 210 students from various disciplines participated in the experiment. We conducted ten sessions, five for each treatment (unanimity and majority rule). Each session involved 21 subjects. We used a pre-determined matching scheme which ensured that any pair of subjects would meet at most twice. Upon entering the lab, subjects were randomly assigned to visually isolated computer terminals, paper instructions (reproduced in the Appendix) were handed out and questions were answered in private. Sessions took approximately 60 minutes, and average earnings amounted to 16.30 EUR (highest 29.50 EUR, lowest 5 EUR), including a 5 EUR show-up fee.

5.4 Hypotheses

Miller et al. (2018) derive benchmark equilibrium predictions for the modified BF game with breakdown values. As is typical in the literature on the BF game, they focus on Stationary Subgame Perfect Equilibria (SSPE). Their equilibrium predictions depend on the exogenously given continuation probability δ, which in our experiment is endogenously determined. We therefore briefly summarize qualitative predictions which are consistent with all values of $\delta \in [0, 1)$.

In an SSPE, each player demands a minimum 'price' in order to approve a proposal. This price is increasing in disagreement values (strictly under unanimity rule and weakly under majority rule). That is, players with larger disagreement values need to be offered at least as much for a 'yes' vote as players with smaller disagreement values (and strictly more under unanimity rule). Given these prices, proposers form minimum winning coalitions (MWC), purchasing only the cheapest votes required for a proposal to pass. Hence, under unanimity rule players with larger disagreement values receive larger shares and achieve higher expected payoffs in equilibrium. When majority rule is used, more 'expensive' players can be excluded from winning coalitions. This implies that players with larger disagreement values are less often included and ex-ante expected payoffs are either non-monotone or even decreasing in disagreement values.[4] Given that all members of a MWC are offered enough to vote 'yes', the first proposal passes, independently of the decision rule being used.

Miller et al. (2018) find support for some, but not all of these qualitative predictions. Their most important findings for the purpose of our own analysis can be roughly summarized as follows. Under unanimity rule, the vast majority of proposals and agreements constitute convex combinations of the three-way equal split and the predicted equilibrium division. Since the two end-points of this spectrum are farther apart in asymmetric situations, those situations are associated with a greater variability in proposals and final outcomes. This variability, in turn, leads to differences in delay: in the symmetric situation, most groups quickly agree on a three-way equal split. In the asymmetric situations, in contrast, initial proposals are significantly more likely to fail. Under majority rule, Miller et al. (2018) find that most subjects attempt to build minimum winning coalitions, especially after gaining some experience. In asymmetric situations, the responder with the larger disagreement values is less often included. The overall variability of outcomes does not differ between situations, and there are no differences in delay. Finally (and as a consequence of these patterns), there is significantly more delay - *in the sense of first round proposals failing* - under unanimity rule than under majority rule, especially in asymmetric situations.

Based on these prior results, we formulate the following hypotheses regarding final agreements and delay in our experimental context. Under unanimity rule, we expect greater variability in final outcomes in asymmetric situations. Specifically, we expect most groups to agree on a three-way equal split in the (20,20,20) situation. In the asymmetric situations, we expect players with larger disagreement values to secure larger shares, with some *variability* in terms of distance from the equal split. Under majority rule, we hypothesize that final agreements will constitute minimum winning coalitions, and that responders with larger disagreement values are less often included.

Hypothesis 1. *Under unanimity rule, final agreements will exhibit greater variability in the asymmetric situations (0,20,40) and (0,0,60) than in the symmetric situation (20,20,20).*

[4] If a player with a larger disagreement value has the *same* 'price' as one with a smaller disagreement value, this implies that he is less often included, and as a consequence achieves a lower expected payoff.

Hypothesis 2. *Under majority rule, final agreements constitute minimum winning coalitions. When responder disagreement values differ, the one with the smaller disagreement value is more often included.*

Finally, we turn to our main hypotheses, concerning the extent of inefficient delay. Our context differs from Miller et al. (2018) in that inefficient delay manifests itself in the *length of communication* rather than the failure of proposals. Although a variety of measures seem reasonable (some of which are discussed below), our main hypotheses will be stated in terms of the *total* length of communication prior to the passage of a proposal (i.e. possibly over multiple rounds of bargaining).[5]

First, consider the symmetric situation (20,20,20). Given the conjectured lack of variability in final outcomes under unanimity rule, we expect that proposers will anticipate that only a three-way equal split is likely to pass, even without communicating with the responders. Thus we conjecture that unanimity rule will not be associated with greater delay in the symmetric situation.[6]

Hypothesis 3. *When players have symmetric disagreement values, the total length of communication and the frequency of breakdown do not differ between majority and unanimity rule.*

For the two asymmetric situations, we hypothesize that the anticipated variability in final outcomes will translate into greater delay under unanimity rule. That is, the variability in outcomes makes it more likely that players will disagree about the allocation to be implemented. Given that each player has veto power, proposers have an incentive to communicate and settle any such disagreement prior to making a proposal, while responders can withhold agreement in order to force concessions. The situation is fundamentally different under majority rule, as proposers can exclude responders who insist on a particular outcome. The threat of being excluded creates incentives to compromise. Second, differences in disagreement values may help proposers to break indifference with respect to coalition formation. For example, anticipating that players with larger disagreement values will be weakly more expensive, proposers have an incentive to communicate with the cheaper responder only and offer him a place in the coalition. Thus, given the incentive to compromise and the focality of splits with cheaper responders, we would less communication under majority rule in both asymmetric situations.

[5] In cases where breakdown occurs, total communication time prior to agreement is unobserved. These observations are censored. We will return to this issue when presenting our results.

[6] Indeed, a plausible alternative conjecture would be that proposers communicate longer under *majority rule* in the symmetric situation, as there is no obvious 'focal' outcome in that condition.

Table 5.2: Number of observations

	All Periods			Periods 5-12		
	Majority	Unanimity	Sessions per Treatment	Majority	Unanimity	Sessions per Treatment
(20,20,20)	140	140	5	84	84	3
(0,20,40)	140	140	5	112	112	4
(0,0,60)	140	140	5	84	84	3

Hypothesis 4. *When players have asymmetric disagreement values, the total length of communication and the frequency of breakdown are greater under unanimity than under majority rule.*

5.5 Results

In total, we observe behavior from 140 games for each constellation of disagreement values and in each treatment. The order in which the three constellations were implemented was varied between sessions. Several of our results are based on data from periods 5 to 12, and most of our tests of significance will use session level averages as units of observation. With this in mind, Table 5.2 summarizes, for each constellation of disagreement values, the number of games observed and the number of sessions in which those games were played, both overall and in later periods.[7]

5.5.1 Final agreements

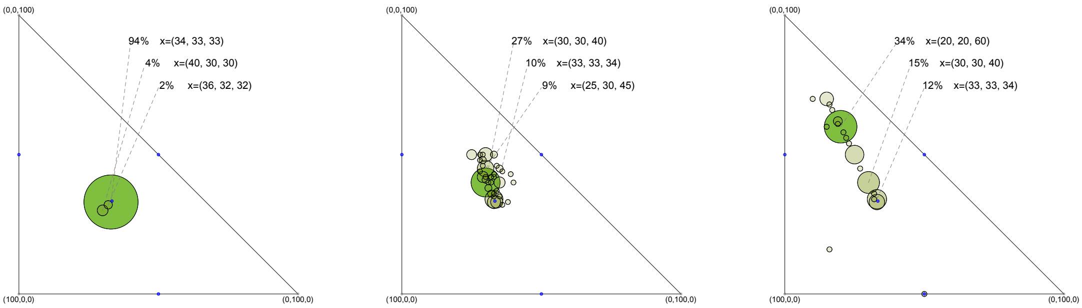
Our main objective is to investigate how the decision rule affects the extent of delay and the frequency of breakdown. However, our main hypotheses regarding delay (Hypotheses 3 and 4) are based on the conjecture that final agreements are likely to vary more in asymmetric situations, implying a greater potential for disagreement and delay. We therefore begin our analysis by presenting information on the type of agreements reached, and testing Hypotheses 1 and 2.

Figure 5.1 depicts, within a series of simplexes, the distribution of final agreements in the unanimity rule treatment. Each simplex represents one of the three constellations of

[7] Across sessions, we varied the order in which the three constellations of disagreement values were presented (see Table 5.5 in Appendix 5). Due to a large number of no-shows, we were only able to run 5 of the 6 possible orders. For each of the 5 orders used in this experiment, we conducted one session in the unanimity and the majority rule treatments.

Figure 5.1: Outcomes under Unanimity Rule (periods 5-12)

(a) $(20, 20, 20)$, $N = 84$ (b) $(0, 20, 40)$, $N = 112$ (c) $(0, 0, 60)$, $N = 84$



disagreement values, (20,20,20), (0,20,40), and (0,0,60). The simplexes are constructed such that players are ordered from the smallest to the largest disagreement value, and the shares allocated to the second and third players are measured on the horizontal and vertical axis, respectively. (The share allocated to the first player is the remainder.) When two players have the same disagreement value, they are ordered according to the share received, with larger shares first. The size of the bubbles reflect how often a specific outcome was observed. For orientation, equal splits (either three- or two-way) are marked in blue. Each (sub-)figure also displays information about the three most common outcomes. We depict outcomes observed in periods 5 to 12, i.e. after subjects have gained some experience with the game.

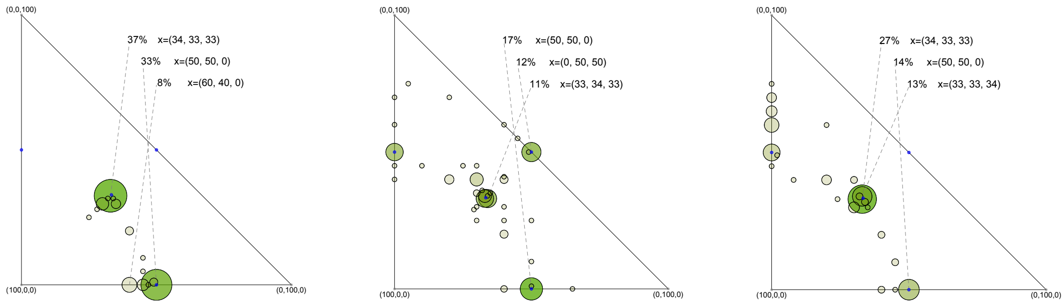
As can be recognized by inspecting Figure 5.1, the variation in outcomes is substantially smaller in the symmetric (left figure) as compared to the asymmetric (middle and right figure) situations. In the symmetric situation, 94% of outcomes are three-way equal splits. This supports our conjecture that the equal split constitutes a clear focal outcome under unanimity rule. In contrast, the variability of outcomes in the two asymmetric situations suggests that there is no clear or focal outcome. Hence, these situations are likely to be associated with greater disagreement concerning which allocation to implement. Our main hypothesis is that this disagreement will lead to delay in bargaining, i.e. to more and / or longer rounds of communication prior to agreement.

Result 1. *Under unanimity rule, final agreements exhibit greater variability in the asymmetric situations (0,20,40) and (0,0,60) than in the symmetric situation (20,20,20) (consistent with Hypothesis 1).*

Another pattern visible in the middle and right panels of Figure 5.1 is that 100% of allocations in the (0,20,40) and 96% in the (0,0,60) situation, are located in the region

Figure 5.2: Outcomes under Majority Rule (periods 5-12)

(a) $(20, 20, 20)$, $N = 84$ (b) $(0, 20, 40)$, $N = 112$ (c) $(0, 0, 60)$, $N = 84$



where shares are increasing in disagreement values.[8] Pooling data from periods 5 to 12, we find that in the (0,20,40) situation, players receive on average 28, 31 and 40 percent of the surplus (Wilcoxon matched-pairs signed-ranks test, pairwise comparison, $N = 8$, 28 versus 31 $p = 0.05$, 31 vs. 40 $p = 0.05$). In the (0,0,60) situation, players with $r = 0$ and $r = 60$ receive 24 and 50 percent on average (Wilcoxon matched-pairs signed-ranks test, $N = 6$, $p = 0.1$). This evidence is consistent with the qualitative equilibrium predictions discussed above.

Figure 5.2 depicts final outcomes observed in the majority rule treatments. Two patterns are immediately visible: First, there is substantial variation in outcomes, especially in the two asymmetric situations (middle and right figures). Second, in contrast to the SSPE predictions, a majority of outcomes in the asymmetric situations are grand coalitions in which all three players receive positive shares (38% in the (20,20,20), 53% in the (0,20,40) and 58% in the (0,0,60) situation). This contradicts the first part of Hypothesis 2, i.e. the prediction that most outcomes would constitute minimum winning coalitions.

To study whether players with larger disagreement values are excluded more often, we provide information on the type of coalitions we observe among final outcomes. Table 5.3 displays the fraction of MWCs depending on the proposer's own (in bold) and the responders' disagreement values. When responder contributions differ, proposers are more likely to include the responder with the smaller disagreement value. For example, in the (0,0,60) situation, 28% of proposals suggest an MWC with responder 1, while only 9% suggest a MWC with responder 2. An exception is the (40,0,20) situation, where coalitions are more likely to include responder 2 instead of responder

[8] This is the region above the 45 degree line and above a line connecting the top left corner to the midpoint of the horizontal axis. A somewhat surprising result is that 10% and 12% of games in the (0,20,40) and (0,0,60) situations end with agreement on the three-way equal split, implying that one player accepts less than her disagreement value.

Table 5.3: Majority rule outcomes (periods 5-12)

	Grand coalitions		MWC	
	Non equal	equal	With resp. 1	With resp. 2
(**20**,20,20)	23%	15%	33%	28%
(**0**,20,40)	36%	15%	30%	19%
(**20**,0,40)	25%	38%	28%	9%
(**40**,0,20)	35%	10%	23%	32%
(**0**,0,60)	35%	33%	23%	9%
(**60**,0,0)	32%	15%	24%	29%
ALL	30%	19%	28%	22%

1. In order to assess the statistical significance of these patterns, we compare the average inclusion frequencies of responder 1 and 2 within each session. The results of a Wilcoxon matched-pairs signed-ranks test suggest that none of the observed differences are statistically significant.[9] Note however, that this result is based on few observations.

Result 2. *Under majority rule, most outcomes in the symmetric situation suggest minimum winning coalitions, while most outcomes in the asymmetric situations suggests grand coalitions. Those proposers who suggest minimum winning coalitions are on average more likely to include the responder with the smaller disagreement value although this pattern is not significant (inconsistent with Hypothesis 2).*

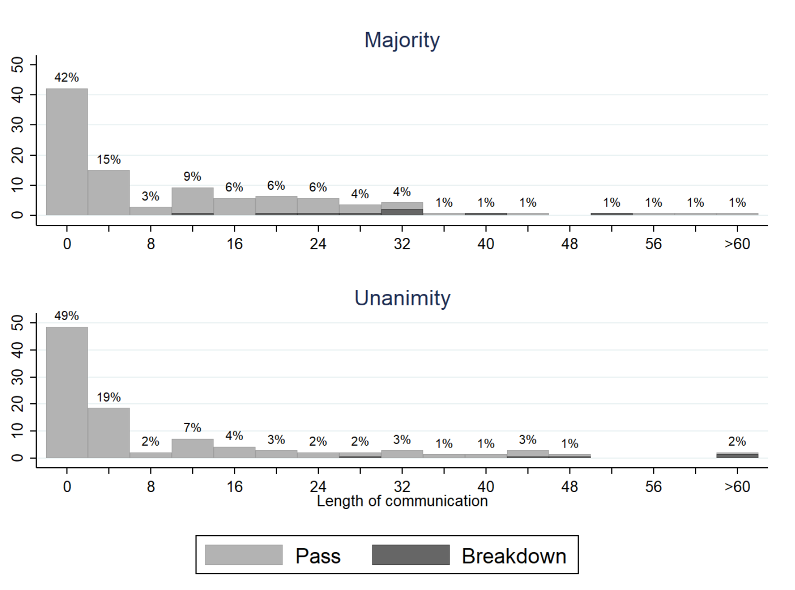
5.5.2 Length of communication

Having established some basic patterns concerning final agreements, we now turn to our main research question: (How) does the decision rule affect the length of negotiations and the probability of breakdown? Before proceeding, we should highlight the fact that the data collected is quite rich, which is why we will focus on a few relatively simple measures of delay.

Four features of our data need to be stressed: First, we observe some groups which reach immediate agreement (and communicate at most once). Other groups bargain and communicate over multiple rounds before eventually reaching an agreement. Still others bargain for shorter or longer periods of time before experiencing breakdown. Second, some groups communicate relatively much in one round (e.g. 30 seconds) while other groups communicate *as long* over the course of *multiple* rounds. Third, whenever bargaining involves multiple rounds, the proposer role is randomly re-assigned in each round. In the asymmetric situations, this implies that players with different

[9] Wilcoxon matched- pairs signed-ranks test using observations from periods 5 to 12 ($N = 6$): (0,20,40) $p = 0.35$, (20,0,40) $p = 0.13$ (40,0,20) $p = 0.27$, (0,0,60) $p = 0.17$

Figure 5.3: Communication length in the (20,20,20) situation



disagreement values control the length of communication. And lastly, in cases where bargaining ends with breakdown, we do not observe at which time an agreement *would* have occurred if the game had continued, i.e. breakdown censors our data.

Due to all of these reasons, our data is relatively complex and multiple factors need to be addressed in order to answer our main research question. In what follows, we begin by focusing on the *total* communication time, aggregated over all rounds prior to either agreement or breakdown. In a second step, we discuss how this measure might be refined in light of the complications mentioned.

Figure 5.3 displays the distribution of total communication length in the symmetric situation, separately for unanimity (lower panel) and majority rule (upper panel). As can be easily recognized, the two distributions are very similar. Approximately half of the groups communicate for less than 4 seconds, and few groups communicate for more than 32 seconds under both decision rules. As a result, we observe few instances of breakdown under both, majority and unanimity rule. In order to test for significance, we compute the average length of communication within each session and compare them across the two decision rule treatments. Using a Ranksum test, we do not find any significant differences in game length ($N = 10$, $p = 0.92$). We obtain the same results, if we compare the average length of communication in periods 5-12, in which subjects have already gained some experience with the game (Ranksum test, $N = 6$, $p = 0.51$). These results are consistent with our Hypothesis 3.

Result 3. *In the symmetric situation, unanimity rule does not lead to longer communication than majority rule.*

Figures 5.4 and 5.5 display the distribution of communication length in the two asymmetric situations. As before, the upper panel corresponds to majority and the lower panel to unanimity rule. In both figures, the distribution of communication length is shifted to the right under unanimity as compared to majority rule. Most notably, we see fewer groups which communicate at most 4 seconds and more groups which discuss at least 116 seconds under unanimity as compared to majority rule.[10] We assess the significance of these differences by comparing the average communication length within each session across the two treatments. The results of our Ranksum test reveal that unanimity rule indeed leads to significantly longer communication in the (0,0,60) ($N = 10$, $p < 0.01$) and the (0,20,40) situation ($N = 10$, $p < 0.01$). Although smaller, this effect persists if we drop the first 4 periods of bargaining (Ranksum test, (0,20,40): $N = 6$, $p = 0.05$; (0,0,60): $N = 8$, $p = 0.02$). These results are consistent with Hypothesis 4.

Result 4. *In both asymmetric situations, unanimity rule leads to longer communication than majority rule.*

As highlighted above, an analysis that focuses on total communication length abstracts from the fact that groups can bargain and communicate over multiple rounds. In what follows, we investigate whether the pattern of communication over multiple rounds differs between the decision rules. For this purpose, Figure 5.6 depicts the average length of communication in a given round (measured on the left axis) observed in each situation. In addition, every figure also depicts the cumulative passage rate (measured on the right axis) up to a given bargaining round, i.e. the percentage of groups which reached an agreement by a given round. In all three situations, the cumulative passage rate under majority rule is always larger than under unanimity rule. This difference is especially large in the two asymmetric situations. Thus, as commonly observed in the literature on BF bargaining, groups need more rounds of bargaining before reaching an agreement when unanimity rule is used. In addition, the figures reveal that the average communication length per round is longer under unanimity as compared to majority rule in the two asymmetric but not in the symmetric situation. Thus, in the two asymmetric situations, groups bargain over more rounds and discuss, on average, more

[10] Given that several groups bargained over as much as 60 seconds, we investigated whether groups indeed communicated or whether the proposer simply let the clock run down to 60 seconds (not sending or replying to any messages sent) in an attempt to maximize the breakdown probability. Such hold out strategies may be especially important when the proposer expects to receive a share smaller than his disagreement value in the process of negotiating. We find that proposers exhibit such hold out behavior in only 3% of all rounds observed in our experiment. The results of a probit regression (reported in Table 5.6 in Appendix 5) reveal that hold out occurs more often under unanimity as compared to majority rule. This result is surprising, given that proposers are endowed with veto power under unanimity rule. Hence, one might expect hold-out to occur more frequently under majority rule.

Figure 5.4: Communication length in the (0,20,40) situation

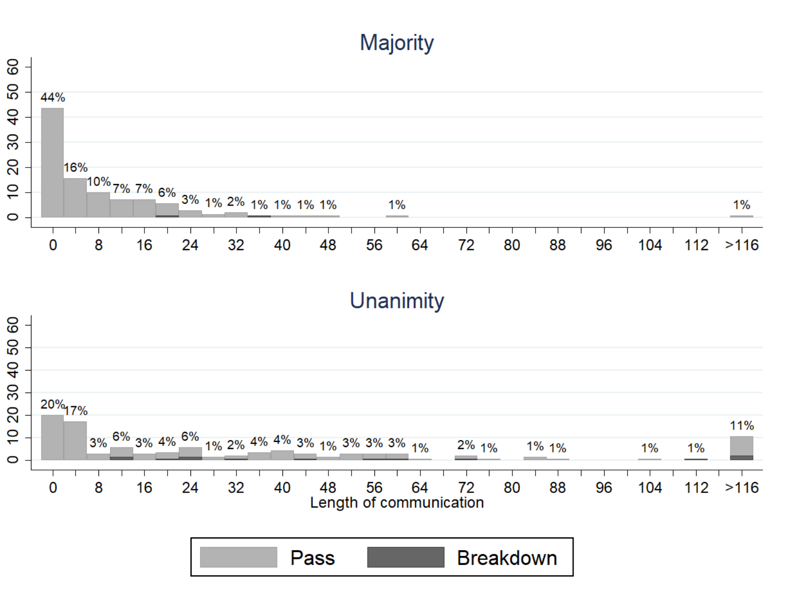


Figure 5.5: Communication length in the (0,0,60) situation

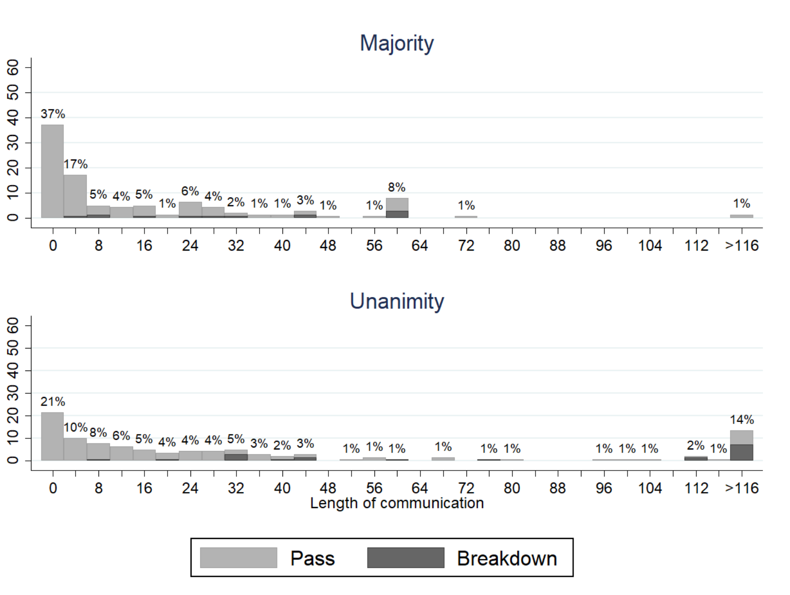
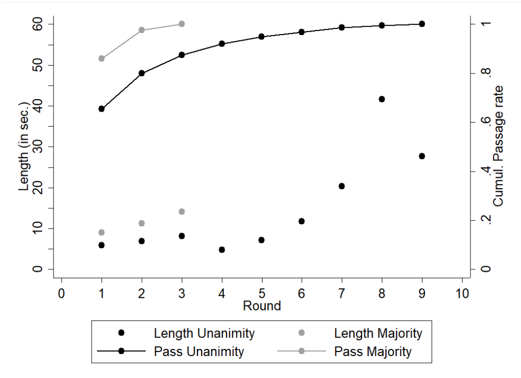
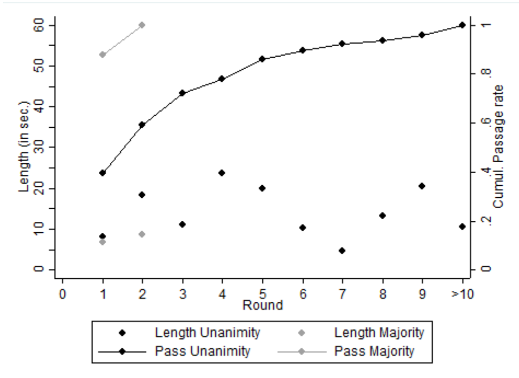


Figure 5.6: Length and Aggregate Passage rates

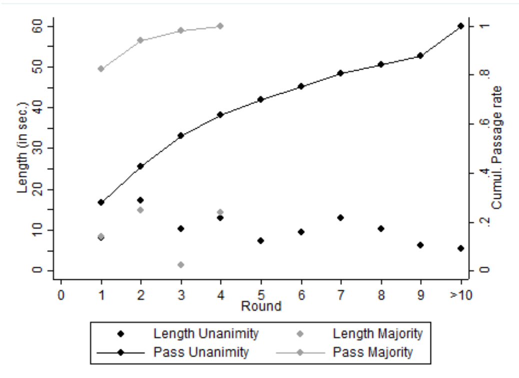
(a) (20,20,20) situation



(b) (0,20,40) situation



(c) (0,0,60) situation



under unanimity as compared to majority rule. Both factors explain our above finding that unanimity rule leads to longer overall negotiations in these situations. In the symmetric situations, in contrast, majority rule is associated with fewer rounds which, however, involve longer discussions as compared to majority rule, thus eliminating any differences in terms of overall negotiation time.

We briefly analyzed the chat protocols in order to verify that subjects indeed used communication to bargain informally. A large number of conversations can indeed be classified as informal negotiations in which the proposer suggests an allocation (i.e. "70-15-14?", "33 for everybody", "50-50 for us") and responders answer either 'yes' or make a counter proposal. Several messages also contain the word "fairness", usually in combination with a proposal to split the surplus equally. Few messages appear to be unrelated to the actual game ("Can anybody tell a joke?", "What's your problem?"). In addition, responders frequently remind the proposer to close the chat window ("Be smart and terminate the chat!"). Hence, our brief analysis indicates that communication is used to bargain informally prior to making a formal proposal.

Table 5.4: Observed and implied probability of breakdown (in percent)

	Observed breakdown		Implied breakdown	
	Majority	Unanimity	Majority	Unanimity
(20,20,20)	6.4	3.4	5.4	5.1
(0,20,40)	4.5	10	4.4	17
(0,0,60)	9.2	16	8	17.4
All situations	5.7	10	6.0	13.2

5.5.3 Breakdown

Next, we investigate how the length of communication translates into breakdown. In our setting, the probability of breakdown within a given round is linearly increasing in the length of communication. Thus, based on the results discussed in the previous section, we would expect that unanimity rule is associated with more breakdown and less efficient outcomes in the two asymmetric situations.

To start, we simply present the observed frequency of breakdown (Columns 2 and 3 in Table 5.4). As can be seen, breakdown occurred more frequently under unanimity rule in asymmetric, but not in symmetric situations. Using Ranksum tests, we do not find a significant difference in breakdown probabilities in the symmetric situations ($N = 6$, $p = 0.33$; dropping first 4 periods $N = 6$ $p = 0.82$). However, in the asymmetric situations, we find that there is significantly more breakdown under unanimity as compared to majority rule (Ranksum using all periods $p = 0.03$; dropping first 4 periods $N = 6$ $p = 0.11$).

Result 5. *For symmetric situations, we find no difference in the frequency of breakdown. For asymmetric situations, we find significantly more breakdown under unanimity rule (consistent with hypotheses 3 and 4.)*

A drawback of looking only at the observed frequency of breakdown is that this depends in part on chance, i.e. on the random numbers drawn at the end of each round. Therefore another way to analyze the data is to compute the implied breakdown probability given a group's communication profile, i.e. the number of rounds and the length of communication per round until an agreement is reached. For this purpose we calculate the implied breakdown probability for a game lasting T periods as

$$p_B(t_1, ..., t_T) = 1 - \prod_{j=1}^{T}(1 - t_j/200)$$

where t_j is the length of communication in round j, and T is the time of agreement *or breakdown.*[11] Columns 4 and 5 in Table 5.4 display the average implied breakdown probability in our experiment for each decision rule and situation. At first glance, it is apparent that the effects based on implied and actual breakdown are the same: First and most notably, unanimity rule is associated with a higher probability of breakdown in the two asymmetric situations. Using a Ranksum test comparing implied breakdown probabilities, we find that this effect is strongly significant ((0,20,40): $N = 10, p < 0.01$; (0,0,60): $N = 10$, $p < 0.01$) and persists if we constrain our analysis to outcomes in periods 5 to 12 ((0,20,40): $N = 8$, $p = 0.02$; (0,0,60): $N = 6$, $p = 0.06$). In the symmetric situation where people communicate little and reach fast agreements under both rules, we do not find any significant difference in the implied breakdown probability (Ranksum test all periods $N = 10$, $p = 0.98$, periods 5-12 $N = 6$, $p = 0.51$).

As indicated above, our estimates of the implied breakdown probability are censored in all cases where breakdown occurs, since in the absence of breakdown additional rounds of communication would have occurred. Thus, the true breakdown probability should be larger than our measure of the implied breakdown probability, especially for unanimity rule. In Appendix 5, we use standard techniques of survival analysis to address this issue, confirming our results.

Result 6. *In asymmetric situations, the implied probability of breakdown is significantly higher under unanimity as compared to majority rule. In contrast, the implied breakdown probability in the symmetric situations does not differ significantly between unanimity and majority rule.*

5.6 Conclusion

This paper contributes to a growing experimental literature on the effects of voting rules in group decision making. In the context of multilateral bargaining, prior authors have found that unanimity rule is associated with significantly lower rates of passage as compared to majority rule. These findings support the notion that the use of unanimity rule may be associated with efficiency losses due to delay.

Conflicting evidence was provided by Agranov and Tergiman (2014, 2017). When subjects were given the ability to communicate at no cost prior to making proposals, passage rates increase dramatically and outcomes approached full efficiency under both decision rules. However, these efficiency gains are driven by the fact that communication itself was *costless* in those experiments. Thus, although Agranov and Tergiman's experiments reveal valuable information about the types of agreements reached under

[11] When breakdown occurs, p_B constitutes a *censored* measure of the breakdown probability.

communication, they are not necessarily suited to compare the efficiency of alternative decision rules. (And indeed, that is not the authors' intent.) We therefore revisit the question of efficiency by conducting multilateral bargaining experiments with *costly* communication.

Our experimental design is based on a modified version of the Baron-Ferejohn game. The costs of communication are introduced in the form of a probability of breakdown which is increasing in the amount of time spent communicating. In case of breakdown, players are paid disagreement values. These values were varied such that we observe behavior in symmetric and asymmetric situations. Our main finding is that unanimity rule is associated with significantly longer communication and more frequent breakdown in asymmetric situations.

We interpret these findings as demonstrating that majority rule may be preferred if delay in decision making is associated with costs. This may include situations in which committee members value the status quo differently or have different outside options, thus raising the question as to how much heterogeneity should be reflected in final outcomes. Our results may, thus, be applicable to real world contexts such as decision making in the EU, where despite recent reforms unanimity rule continues to be applied in situations of high conflict (such as the inclusion of new member states as well as changes in the EU treaty). The choice which decision rule to use in these instances may be subject to an efficiency-fairness tradeoff: While each member might favor unanimity over majority rule in order to protect her interests, majority rule may be preferred to unanimity rule in terms of the efficiency of outcomes. Hence, arguments in favor of the continued use of unanimity rule need to emphasize fairness over efficiency concerns. Whether this adequately represents citizen preferences is unclear. For example, related experimental research suggests that individuals are more concerned about efficiency than about reducing inequality in outcomes (Charness and Rabin, 2002; Engelmann and Strobel, 2004). Hence, an investigation into the relative importance of fairness and efficiency of outcomes may provide important insights allowing us to choose decision rules which maximize the utility of the citizens.

Appendix 5.A

5.A.1 Additional Results

Table 5.5: Disagreement value constellations used in the experiment

Order condition	Games 1-4	Games 5-8	Games 9-12
1	(20,20,20)	(0,20,40)	(0,0,60)
2	(20,20,20)	(0,0,60)	(40,0,20)
3	(40,0,20)	(20,20,20)	(0,0,60)
4	(60,0,0)	(20,20,20)	(0,20,40)
5	(60,0,0)	(0,20,40)	(20,20,20)

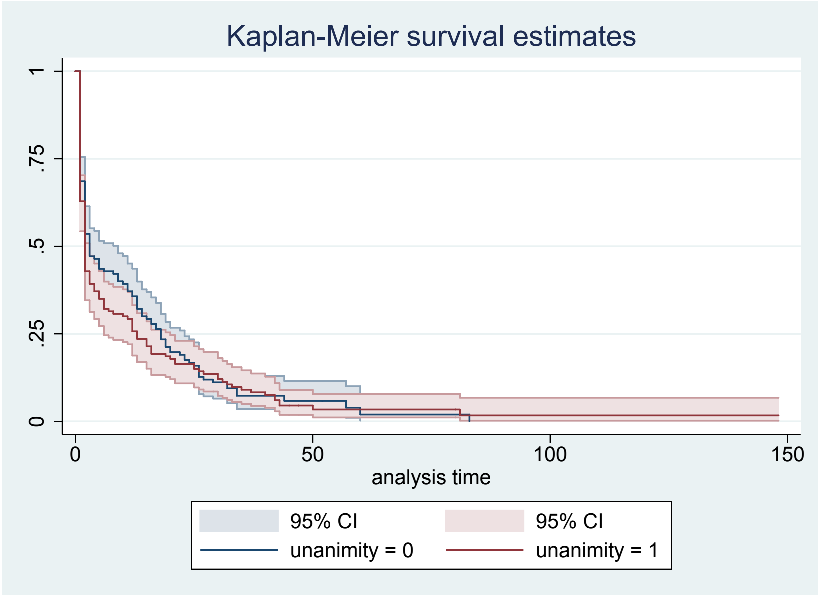
Table 5.6: Probit Regression on Hold-Out

Probability to Hold-Out	(1)
Unanimity	0.64***
	(0.17)
Player 3 proposes	0.34
	(0.21)
Round	-0.04
	(0.04)
N	1,578
Prob> F	0.00

5.A.2 Survival Analysis

One of the complicating features of our data is that some games end with breakdown, so that we do not observe the time at which agreement *would have* occurred. By simply looking at the total length of communication without distinguishing games that end in agreement or breakdown, the previous analysis treats cases of breakdown as if agreement had occurred at that point in time. This leads us to *underestimate* the time until agreement. One method of addressing this problem is to employ survival analysis in order to estimate the time to agreement.

Figure 5.7: Kaplan-Meier estimated survival function: $r = (20, 20, 20)$



Length of communication

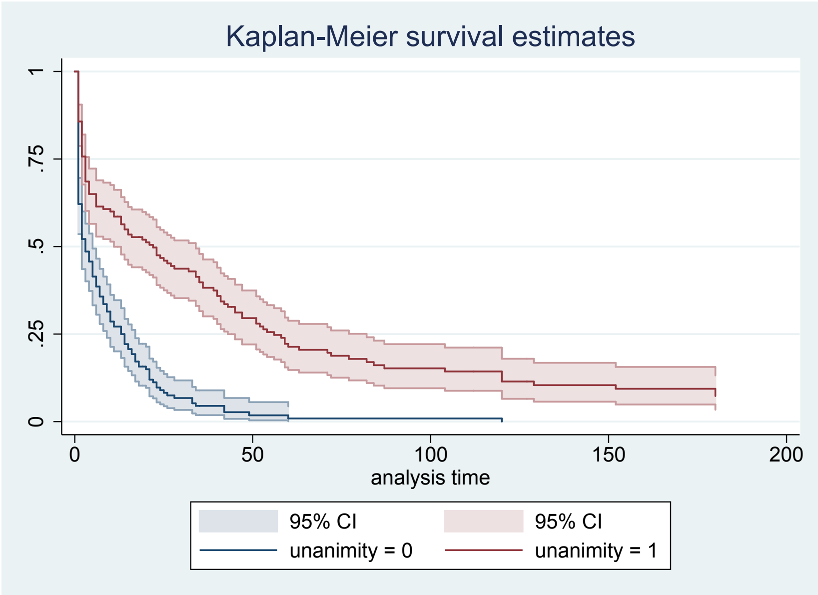
One way to estimate the distribution of time to agreement is to construct a Kaplan-Meier survival function.[12] Figure 5.7 displays the estimated survival functions for unanimity and majority rule in the symmetric situations, along with 95% confidence intervals. As is apparent from these figures, the two survival functions are very similar. Both a log-rank and a Wilcoxon test comparing these survival functions confirm that they are not significantly different ($p = 0.41$ and $p = 0.15$, respectively).

Figures 5.8 and 5.9 displays the estimated survival functions for the asymmetric situations. Especially for the $r = (0, 20, 40)$ situation, we see that unanimity rule is associated with longer estimated times to agreement. Under majority rule, roughly 75% of games are estimated to last less than 13 seconds, whereas under unanimity rule, 75% of games last less than 56 seconds (see Table 5.7). Both log-rank and Wilcoxon tests confirm that the survival functions differ significantly in the asymmetric situations ($p < 0.001$, both tests and both comparisons).

Table 5.7 displays summary statistics based on these estimated survival functions. According to these estimates, the average time to agreement under unanimity rule is 57 seconds in the (0,20,40) situation and 76 seconds in the (0,0,60) situation, as

[12] Basically, this method calculates, for each point in time, the proportion of games that end with agreement *conditional* on not yet having ended in breakdown. See Cleves et al. (2008) for an accessible introduction to survival analysis.

Figure 5.8: Kaplan-Meier estimated survival function: $r = (0, 20, 40)$



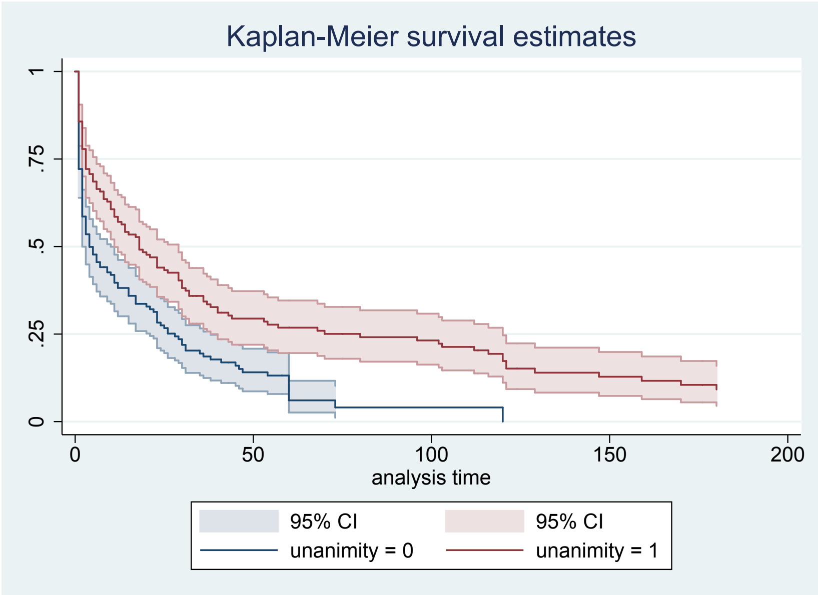
compared to 10 and 20 seconds, respectively, under majority rule.

A second method to compare the time to agreement is to estimate a Cox proportional hazard model. This approach assumes that the likelihood with which agreement will occur at a given moment in a treatment is equal to a baseline hazard multiplied by a constant, called the *hazard ratio*, which is a function of control variables including a treatment dummy. A hazard ratio larger than one would imply that the treatment is associated with a larger probability of agreement (and therefore less delay) as compared to the baseline, and vice versa for a hazard ratio below one. Table 5.8 displays the result of this estimation using the majority rule treatment as the baseline condition. All

Table 5.7: Estimated communication time until agreement (in seconds)

		games	mean	p25	p50	p75
All games	Majority	420	13.88	1	3	18
	Unanimity	420	49.70	2	11	40
Symmetric	Majority	140	12.33	1	3	18
	Unanimity	140	11.94	1	2	13
(0,20,40)	Majority	140	9.55	1	3	13
	Unanimity	140	57.11	3	22	56
(0,0,60)	Majority	140	19.62	1	4	28
	Unanimity	140	75.95	3	18	80

Figure 5.9: Kaplan-Meier estimated survival function: $r = (0, 0, 60)$



estimations are based on the pooled data from both asymmetric situations. Columns 2 and 3 add controls for the period of the experiment and (in addition) a dummy for the $r = (0, 0, 60)$ situation, respectively. All regressions involve standard errors clustered at the session level. Three patterns are visible. First, unanimity rule is associated with a hazard ratio slightly below 0.5, implying that the probability of reaching agreement at any given time is approximately half as large under unanimity rule as compared to majority rule. Second, the period variable is associated with a hazard ratio of approximately 1.05, implying that the probability of agreement rises by roughly 5% from one period to the next, i.e. as subjects gain experience. Finally, the $r = (0, 0, 60)$ dummy exhibits a hazard ratio of approximately .8, implying that agreement is 20% less likely in that situation, leading to more delay than in the $r = (0, 20, 40)$ situation. Each of these effects is significant at the 1% level or more.

Table 5.8: Cox proportional hazard models (asymmetric games)

	(1)	(2)	(3)
Unanimity	0.487***	0.471***	0.461***
	(0.0525)	(0.0312)	(0.0305)
Period		1.057***	1.055***
		(0.0126)	(0.0107)
$r = (0, 0, 60)$			0.801**
			(0.0658)
Observations	560	560	560

Exponentiated coefficients; Standard errors in parentheses
std. err. adjusted for 10 session clusters
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

5.A.3 Instructions

Description of the experiment

Rounds and Groups

This experiment consists of 12 rounds. At the beginning of each round, you will be **randomly matched in groups of three participants**. This means you will be interacting with two, randomly selected participants in this room. All participants remain anonymous throughout the experiment. At the beginning of each round, every group member receives an ID ("A", "B" or "C") which he or she keeps until the end of the current round. You will be **interacting with different participants in every round**. This means, you will interact in the same group only once.

Description of a round

In each round, the group has the possibility to divide a surplus of **100 points** among its members. (One point has a value of 35 EUR cents.) An agreement is reached if a majority of individuals (i.e. 2 out of three group members) consent on a division of the surplus. This happens according to the following rules:

Proposal and Voting: One of the three participants in a group is randomly selected to make a **proposal**. (Every participant has the same probability of being selected.) This participant can make a proposal how much of the 100 points he wants to allocate to members "A", "B" and "C". Once the proposal is submitted, all group members (including the proposer himself) **vote** either "Yes" or "No". If a majority of the group members (at least two of the three group members) vote "yes", the proposal **passes** and the round ends. If this round is selected for payment, each group member is paid according to the proposal. If a majority of the group members rejects the proposal, it counts as **rejected**. In this case, one of the three participants in the group is randomly selected to make a proposal (it *may* be the same group member). **This process is repeated until a proposal passes *or* until bargaining is terminated (see below).**

Communication via Chat: Once a proposer is selected and before he or she submits a proposal, the group can **communicate** via a chat window. The chat window is open for at most 60 seconds. However, the proposer can decide to **close the chat window** at any time.

Possibility of termination: Once the proposer has closed the chat window, there is a chance that the round is terminated. In this case, he or she cannot propose how to allocate the 100 points. Instead, each group member will receive a predetermined amount of default points. Each member is informed about the default points that she as well as both other participants in the group receive in case of termination at the beginning of each round.

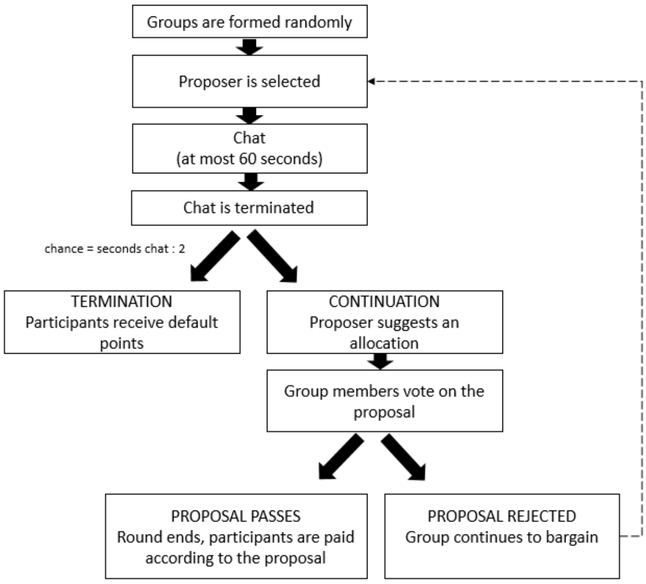
Probability of termination: The probability that the round is terminated depends on how long a group decided to communicate via the chat. For every 2 seconds of chatting, the probability of termination rises by 1 percent. For example, if the group chats 40 seconds, the probability that the round is terminated amounts to 40 : 2 = 20 percent. If the group chats the maximum allowed time of 60 seconds, the probability of termination amounts to 30 percent.

Your payment at the end of this experiment

At the end of this experiment, the computer will randomly draw one of the 12 rounds. For each point that you received in the randomly chosen round, you will be paid 35 EUR cents. In addition, you receive a 5 EUR show up fee for participating in this experiment.

Summary of a round:

This picture summarizes the sequence of each round.



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