Essays in Experimental Economics

Inauguraldissertation zur Erlangung
des akademischen Grades eines Doktors
der Wirtschaftswissenschaften der
Universität Mannheim

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Herbstsemester 2016

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Tag der mündlichen Prüfung: 22. November 2016

Eidesstattliche Erklärung

Hiermit erkläre ich, dass ich die vorliegende Dissertation selbstständig angefertigt und die benutzten Hilfsmittel vollständig und deutlich angegeben habe.

Erlangen, 27. September 2016

Timo Hoffmann

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General introduction

Economics is concerned with all kinds of human interactions as well as the role institutions play in these interactions. The research presented in this dissertation is motivated by the idea that challenges and problems that arise in the functioning of economies and societies are driven by human behavior. Studying human behavior is not only helpful in understanding the reasons for the observed behavior, but might provide valuable insights into the origins of these problems. I believe that a better description of human behavior is a necessary first step in finding solutions for many challenges our societies face. More generally, I consider the research of human behavior crucial in order to develop institutions and mechanisms for solving or even preventing these problems and to improve existing organizations.

This dissertation consists of three independent chapters, which analyze different aspects of human behavior. All chapters share the feature that laboratory experiments are used to investigate and describe the behavior of agents. Chapter 1 focuses on a methodological issue of eliciting expectations of players about the likely behavior of their opponent(s) in experiments - the so called beliefs - and investigates the influence of this belief elicitation on observed behavior. Thereby this research touches on the process of belief formation. Chapter 2 analyzes the importance of worker sorting and its interplay with incentive effects for the profitability of rank-order tournaments. More precisely I consider two environments, with and without the possibility of worker sorting, to examine how worker sorting affects the profitability of a tournament for employers. Lastly, chapter 3 investigates the role of private information and available mechanisms on choosing group decision rules. This experiment tests several theoretical predictions made in the economic literature regarding the role of participation constrains for choosing efficient group decision rules. Chapters 1 and 2 are single-authored essays, while chapter 3 is based on joint work with Sander Renes.

Each chapter is followed by an appendix that includes supplementary material such as additional figures, further analyses and provides the instructions for the conducted experiments. The references for all three chapters are collected in one bibliography at the end of this dissertation.

Chapter 1

To understand subjects' decisions it is useful to know more about why a subject makes a certain choice. Especially in the context of strategic interactions, the expectation of a person what others will do - the so called "belief" - can be helpful in understanding the motivation of a subject for choosing a certain action. Beliefs can be used to differentiate between models that predict identical behavior and they provide useful insights in the reasoning of subjects. Specifically if predictions made by models and observed choices diverge, beliefs can be valuable in understanding why model predictions fail. The experiment presented in chapter 1 is concerned with the question whether asking subjects for their beliefs about the expected behavior of their counterpart influences which actions are chosen.

My motivation for this paper is twofold. First, the described advantages of knowing subjects' beliefs when explaining their behavior and the relatively simple possibility of directly eliciting beliefs in laboratory experiments have made belief elicitation frequent in the experimental literature. But beliefs and actions elicited in an experiment can only be used without restrictions if subjects' actions are not changed by the elicitation. In the literature that uses belief elicitation it is mostly assumed that action choices are not altered by a proper elicitation of beliefs. Some studies explicitly consider a potential effect of belief elicitation on game play, however their results are mixed. While a few papers report significant effects on subjects' choices, others claim that choices are not affected by the elicitation. Since the different results might be partially driven by different game properties, I consider it important to analyze whether and how game properties play a role for the effect of belief elicitation on game play. Second, understanding how and specifically in which situations belief elicitation might affect chosen actions can provide useful insights in the decision process of subjects.

Therefore in chapter 1 I investigate whether belief elicitation influences the behavior of subjects in two-person normal-form games. Using games with different properties allows me to analyze whether and how game properties play a role for the effect. In the experiment there are two treatments. In the first treatment subjects' choices and beliefs are elicited simultaneously. Therefore subjects have to think about their beliefs when choosing an action. In the second treatment all choices are made without the elicitation of beliefs. The beliefs are elicited as a surprise after subjects have made all their action choices. Besides enabling me to analyze whether choices are affected by the belief elicitation, this procedure allows me to compare the belief statements between treatments.

Comparing the choices from both treatments, I find that subjects' choices are affected by the belief elicitation for two subgroups of games. In dominance-solvable games subjects are more likely to choose an action that is part of a Nash equilibrium if they state beliefs and actions at the same time. In games that have a symmetric alternative outcome to the Nash equilibrium, belief elicitation decreases the probability of equilibrium play. These results demonstrate that belief elicitation is not always neutral, but can have significant effects on game play. Dependent on game properties the belief elicitation can lead to an increase or decrease of equilibrium play.

In the data there are no differences between the beliefs stated simultaneously with the action choice and the beliefs stated after action choices have already been made. However, there is a clear difference in the number of action choices, which are best responses to own stated beliefs. When beliefs and choices are elicited simultaneously, chosen actions are more often best responses to the stated beliefs compared to a sequential elicitation of actions and beliefs. A possible explanation of this finding is that subjects revise their beliefs when they are asked to make explicit belief statements. The elicitation of beliefs might lead to modified beliefs and these modified beliefs result in a change in the action subjects want to choose. However, subjects can actually "change" their action only if beliefs and actions are elicited simultaneously. In the treatment in which action choices and belief statements are made sequentially subjects cannot change their previously made action choices. Consequently, the previously chosen actions are less often a best response to the stated "modified" beliefs.

The data shows that not only whether an effect exists depends on game properties, but also whether belief elicitation increases or decreases the probability of equilibrium play. Thereby these findings highlight the importance of different game properties for the effect. Furthermore the data indicates that the decision process of subjects does not always include the formation of exact beliefs. The fact that the effect of belief elicitation varies with game properties could suggest that subjects' decision processes are slightly different for different games / environments. I therefore consider the presented study also as a step towards a better understanding of the role of belief formation in the decision process of subjects.

Chapter 2

The importance of worker sorting for the observed output differences between payment schemes is well documented in the economic literature. However, little evidence exists on the role of worker sorting on the profitability of payment schemes for employers. In chapter 2 I analyze the profitability of a rank-order tournament for employers in two environments, which differ in whether self-selection of workers into payment schemes is possible or not.

Rank-order tournaments have the feature that workers' earnings depend on their relative performance and not directly on their output. Consequently, for employers the costs of a tournament are independent of workers' performance - at least when prizes are independent of output. This independence can result in cost savings, if there are high effort provisions of workers despite a relatively cheap tournament. If that is the case, the firm might generate a larger profit with a tournament compared to output-based payment schemes. But such a tournament is usually cheap, because the expected earnings for workers are low. Therefore it is not clear whether a cheap tournament is sufficiently well liked by productive workers, such that they choose the tournament over other payment schemes.

My experimental design exploits the advantage of the laboratory environment to disentangle a potential sorting from the incentive effect. In the first part employers choose a payment scheme among the offered possibilities of a fixed wage, a piece rate and a rank-order tournament. Since workers are exogenously matched with an employer, only an incentive effect is possible. In the second part three employers are grouped together and sorted to the different payment schemes based on their stated preferences. At the same time, six workers are grouped together and state their preferences over the payment schemes. Since each payment scheme has to be offered by exactly one employer, workers can choose between all three payment schemes. Therefore, workers are no longer exogenously matched with an employer, but select their preferred payment scheme. This matching procedure provides an opportunity for worker sorting.

Additionally there are two treatments with different tournament prizes. In the low-prize treatment the tournament is relatively cheap for employers. These low prizes yield low expected incomes, even for productive workers. In the high-prize treatment the prizes are larger and therefore the expected payoff of workers in the tournament, but also the costs for employers, are higher.

My results demonstrate that tournaments provide incentives for workers and therefore lead to high effort provisions. In both tournaments the observed output is similar to the piece-rate performance. Consequently, if worker sorting is not possible - in part one - a low-prize tournament results in higher firm profits than a piece rate. A finding which illustrates the potential cost advantage of rank-order tournaments for firms. However, the tournament in the high-prize treatment, with the larger costs, results in the same average profits as the piece rate. In the second part, in which worker sorting is possible and indeed observed, the cheap low-prize tournament does not yield a larger employer profit than the piece rate. The main reason for this finding is productivity sorting of workers. Productive workers prefer the piece rate if the tournament prize is low. While the high-prize tournament is more attractive to workers, the additional costs for the tournament prizes result in the same profit for employers for the high-prize tournament and the piece rate. Therefore, neither the low nor the high-prize tournament yields a larger employer profit compared to the piece rate if worker sorting is feasible.

The main finding of the paper is therefore that with worker sorting rank-order tournaments are hardly simultaneously attractive for workers and employers. Despite its low costs for employers, a low-prize tournament does not yield larger profits than a piece rate due to worker sorting. A high-prize tournament might be attractive for workers, but it is too costly for employers. This result might be part of the explanation why a tournament is usually not the main contract scheme for workers in labor markets, despite its good incentives.

Chapter 3

In the third chapter the problem of (efficient) mechanism selection in a group decision experiment is analyzed. We study binary comparisons between four decision rules and investigate the role of different outside options and private information on subjects' mechanism choices. By having subjects choose between two available mechanisms, we can test the influence of participation constraints on the selection of mechanisms. Participation constraints are ubiquitous in economic theory. The possibility of "going elsewhere" provides a credible threat for economic agents. Therefore these outside options result in strong bargaining positions and have to be taken into account by the respective counterparts. Despites their importance and their prominent role in economic theory, there is little empirical evidence of the effect of participation constraints on behavior. A major reason for the lack of evidence is that participation constraints generally depend on the choice not made, and thus on unobserved counterfactuals. Without assuming a participation constraint binds strictly, it is nearly impossible to give an estimate of the value attached to the best of the options not taken. To overcome this problem, Sander Renes and I conduct an experiment, which allows us to study the effects of participation constraints on mechanism choices in social choice situations.

Before a group or society can take a decision, whether it is about what restaurant to visit or about the implementation of a reform, the group has to select a decision rule. It would seem that groups should select a rule that maximizes the value of the final decision to the group. In their seminal paper Myerson and Satterthwaite (1983) show that with private information about their preferences over outcomes, there is no efficient decision rule that is unanimously preferred by all group members over the non-implementation status quo. In our experiment we test whether this impossibility theorem can be found in subjects' mechanism choices. Furthermore, we investigate several proposed possibilities in the literature to overcome this impossibility theorem by changing the outside option of subjects.

In our experiment groups of three participants make a collective decision about the implementation of a public project. If implemented, all group members receive a payoff from the project. However, these payoffs are heterogeneous and might be positive or negative for an individual. The decision whether to implement the project or not is taken in a two-stage voting game. In the first stage the group decides between two available mechanisms and in the second stage the selected decision rule is used to determine whether or not the project is implemented. In the experiment we contrast two different environments. In the first part, subjects select a decision rule without being informed about their private payoff from project implementation. However, they know the distribution of possible payoffs and are informed about their private payoff after a decision rule has been chosen, but before they apply the decision rule to determine the project implementation. In the second part subjects are informed about their potential project payoff before selecting a decision rule.

In total there are four group decision rules: (1) an efficient direct revelation mechanism -

called AGV, (2) simply majority voting, (3) a non-implementation status quo mechanism that always prevents project implementation and (4) a random mechanism that implements the project in 50% of cases. Providing subjects with the choice between two of these rules and therefore defining the outside option, allows us to assess the role of participation constraints on mechanism choices. The within-subject variation of the private information about the project payoffs enables us to perform a straight forward test of the impossibility theorem by Myerson and Satterthwaite. We compare the mechanism choices of the same subjects with and without private information about the project payoff.

Our results show that before the revelation of private information, subjects choose the more efficient mechanism, because it yields the largest expected payoff. In the first part of the experiment a large majority of choices is in line with the expected payoffs of the mechanisms. In the second part, when subjects are informed about their valuation for the project, they only prefer the (more) efficient mechanism if their valuation is positive. Otherwise they opt for the non-implementation status quo whenever possible. This result confirms the impossibility theorem of Myerson and Satterthwaite. Furthermore we find evidence that it is easier to get individuals to agree on more efficient mechanisms if the outside option involves risky outcomes.

These results suggest that participation constraints play an important role in the optimal design of institutions. A group that is stuck in an inefficient mechanism might require an outside influence or coercive power to break away from the status quo. Even in the small groups in our experiment private information about the project make the implementation of efficient mechanisms infeasible. The difficulties of negotiating a public project on the scale of a nation would seem close to unsurmountable if unanimity is required. Centralized organizations with an amount of coercive power, like the state or the company, allow participants to bundle individual projects and reforms and take them away from purely decentralized mechanisms like open markets. Our results therefore provide some rational for the existence of such institutions. By forcing group members to participate in individual projects, the group surplus can be increased since not all participation constraints need to be satisfied. In this sense our findings give one reason for the existence of states and their coercive powers, since it does make dealing with participation constraints easier.

Acknowledgments

I foremost would like to thank my supervisor Dirk Engelmann for his continuous support and the excellent guidance over the last six (plus) years. Without his feedback and encouragement this thesis would not have been possible. Furthermore my thesis greatly benefited from the feedback and comments I received in individual talks, many presentations and consecutive discussions with numerous people. While I will acknowledge the feedback of many people in the beginning of each chapter separately, I in particular want to thank Hans Peter Grüner, Christian Koch, Henrik Orzen, Stefan Penczynski, my co-author Sander Renes and Johannes Schneider for all their input and advice.

I gratefully acknowledge the financial support from the Deutsche Forschungsgemeinschaft (SFB 884), which financed the experiments for the third chapter of this thesis.

Moreover, I thank my fellow doctoral students from the Center for Doctoral Studies in Economics (CDSE) for the inspiring atmosphere within the graduate school. Interacting with you on a professional, and sometimes even more on a personal level, was supportive and very enjoyable. Finally, I thank Eva for her continuous support and encouragement. Thank you for always being there for me.

Erlangen, September 27th 2016 *Timo Hoffmann*

Chapter 1

The Effect of Belief Elicitation on Game Play: The Role of Game Properties[1]

1.1 Introduction

Understanding, describing and explaining individual choices is one of the core objectives of economics. A main ingredient for this analysis are models that are formulated to explain and predict behavior. An integral part of most models, especially in strategic situations, are beliefs of subjects about the likely behavior of their opponent(s). In many models the resulting action is the best response to these beliefs. However, a large part of the experimental literature demonstrates that observed choices often are not in line with the equilibrium predictions of various models (for overviews see Kagel et al. (1995) or Camerer (2003)). The source of these differences can be non-equilibrium beliefs or a failure to best respond to beliefs. To differentiate between these explanations subjects' beliefs need to be known. Furthermore, subjects' beliefs can provide useful insights for understanding the reasons for model failures and help to differentiate between competing explanations for observed behavior. But different than action choices beliefs are usually not directly observed. Therefore to know subjects' beliefs they have either to be inferred or elicited.

If beliefs are directly elicited the question whether these elicitations affect subjects' choices arises. Only if subjects' action choices are unchanged by the elicitation of beliefs can the elicited beliefs be used to determine subjects' motives for action choices without restrictions. Additionally, the elicited beliefs can only be directly utilized to explain behavior

[1] I appreciate the comments and the advice received from Dirk Engelmann, Jana Friedrichsen, Werner Güth, Nikos Nikiforakis, Henrik Orzen, Stefan Penczynski, Philipp Schmidt-Dengler, Johannes Schneider, Stefan Trautmann and Roberto Weber. I thank the participants at the Behavioral and Experimental Workshop in Florence, the EEA in Gothenburg, VfS Meeting in Hamburg, GfeW Meeting in Helmstedt, the THEEM Workshop in Kreuzlingen, RES Meeting in Manchester, NERD Workshop in Nuremberg, ENTER Jamboree in Stockholm, the Experimental Conference in Xiamen and the ESA World Meeting in Zurich, as well as seminar participants in Mannheim, Nuremberg and Tilburg for many helpful comments and fruitful discussions. An earlier draft of this paper circulated under the title "The Effect of Belief Elicitation on Game Play".

in similar situations without belief elicitation if subjects' choices are not affected by the elicitation. In this paper I examine whether belief elicitation affects subjects' choices in one-shot two-person normal-form games. The previous literature analyzing possible effects of the elicitation on observed choices obtained mixed results. Therefore the main aim of the paper is to analyze whether the (potential) effects of belief elicitation on game play depend on game properties, since the mixed results might be at least partially explained by different game properties.

Croson (1999, 2000) shows that subjects tend to contribute less in a public good game and defect more often in a prisoner's dilemma if beliefs are elicited.[2] In her study belief elicitation is therefore not neutral, but with belief elicitation subjects' choices are more in line with the theoretical predictions. Costa-Gomes and Weizsäcker (2008) analyze equilibrium play and stated beliefs in normal-form games and examine whether belief elicitation affects game play. They do not find any significant effects on chosen actions.[3] Recent articles about belief elicitation (Schlag et al. 2015; Holt and Smith 2016) conclude that there is a relatively limited number of studies on the effect of belief elicitation on choices and that the reported results are inconclusive.[4] Hence two main facts emerge from the literature: Most importantly belief elicitation does not always affect game play. Additionally, not in all studies with a significant effect belief elicitation drives subjects towards equilibrium choices. Different game properties are one possible reason for the divergent results.

To analyze whether and how belief elicitation affects choices I contrast subjects' choices in 20 two-person normal-form games in two treatments. In a first treatment *(baseline)* subjects state their action choices without being inquired about beliefs. In a second treatment *(belief)* a separate group of subjects plays the same games, but subjects always state their action choices and their beliefs about their opponents behavior simultaneously. By using games that differ in many dimensions I assess whether an effect exists for some, but possibly not all games. Furthermore beliefs of subjects are collected in both treatments. In the belief treatment beliefs are stated together with the action choices, while in the baseline treatment the belief elicitation follows after all action choices have been made, but before subjects receive any feedback. Collecting beliefs of all subjects allows me to analyze whether beliefs

[2] Results in other studies of the public good game are less clear: Wilcox and Feltovich (2000) do not find an effect of belief elicitation on contributions, Gächter and Renner (2010) report increased contributions and a meta-study by Zelmer (2003) documents decreased contributions. However, this meta-study contains only a limited number of experiments with belief elicitation.

[3] There are several notable differences of Costa-Gomes and Weizsäcker (2008) to my analysis. First, in Costa-Gomes and Weizsäcker (2008) the elicitation is always done before or after the action choice. In my design the two tasks are carried out simultaneously. Second, they find no differences on the game level, but also do not analyze the individual behavior across games explicitly as I do in section 1.3.2. Third, my study considers a greater variety of games and their properties.

[4] Nyarko and Schotter (2002) elicit beliefs in a repeated 2x2 game in the context of (belief) learning. They do not find an effect of belief elicitation on game play. Rutström and Wilcox (2009) report that belief elicitation in a repeatedly played asymmetric matching pennies game affects subjects' choices for subjects with a large asymmetry of payoffs and especially in the first rounds. However, these studies are less applicable to mine since they consider repeated interactions, while I concentrate on one-shot games.

are different when elicited together with action choices.

Understanding whether the effect of belief elicitation depends on game properties is also an important step for advancing our knowledge on subjects' decision making processes in different situations. Especially if the effect of belief elicitation is heterogeneous for different subjects or circumstances, we can learn a lot about subjects' reasoning by understanding how an explicit belief statement influences their behavior. If the elicitation increases the awareness for the incentives of the opponent this might hint that subjects lack some strategic sophistication without belief elicitation.

The direct elicitation of beliefs requires less assumptions on the relationship between beliefs and actions than inferring beliefs from subjects' choices. This fact is especially advantageous in one-round games, when beliefs cannot be derived from previous behavior. Consequently eliciting beliefs has been frequent in the experimental literature. It has be used to test model assumptions such as actions being best responses to (own) beliefs (Huck and Weizsäcker 2002; Weizsäcker 2003; Costa-Gomes and Weizsäcker 2008) or predictions of (belief) learning models (Nyarko and Schotter 2002). Belief elicitation has also been applied in the literature about fairness and reciprocity to differentiate between reasons for "social" behavior (Offerman et al. 1996; Dufwenberg and Gneezy 2000; Gächter and Renner 2010), as well as in the literature on behavior in one-shot games (Costa-Gomes et al. 2001; Costa-Gomes and Weizsäcker 2008; Rey-Biel 2009), and in many other areas.[5]

Many papers with belief elicitation simply assume that subjects' behavior (and beliefs) are not altered by these procedures. In Offerman et al. (1996), Haruvy et al. (2007), and Fischbacher and Gächter (2010) for example there are no separate treatments without belief elicitation. Some authors acknowledge a possible effect of the elicitation on stated choices (and beliefs), but argue that the observed behavior is similar to the behavior reported in closely related studies (Haruvy et al. 2007; Fischbacher and Gächter 2010). Therefore they conclude that additional treatments without belief elicitation are unnecessary. But a direct extension of the results found with belief elicitation to situations without belief elicitation is only possible if the behavior is not affected by the elicitation of beliefs. Furthermore, comparing results with belief elicitation to similar studies without the elicitation disregards many possible differences between studies. While studies might be similar, they often differ in the used subject pool, exact wording of instructions and experimental procedures. Therefore claiming that belief elicitation is without effect by simply referring to other experiments with qualitatively identical results, ignores many possible reasons why no difference is found.

A priori it is not obvious that the elicitation is without effect on subjects' decisions. Belief elicitation might influence game play due to two reasons: First, belief elicitation could modify existing beliefs of subjects. The elicitation might have subjects focus more

[5] For additional references regarding papers with belief elicitation see the papers cited in the introduction of Costa-Gomes and Weizsäcker (2008) or Blanco et al. (2010).

on those beliefs and therefore lead to changed action choices. Second, subjects might not build beliefs or not base their action choices on beliefs if these beliefs are not elicited. In this case, contrary to the standard assumption, play is not belief based. For these subjects the elicitation should result in belief formation and consequently in action choices that take these newly formed beliefs into account.[6]

Most game-theoretic concepts such as Nash equilibrium (NE) or even somewhat weaker concepts like rationalizablility (Bernheim 1984; Pearce 1984) assume that players possess (subjective) beliefs, meaning they have an idea in mind what their opponent will do. Often times it is not specified how these beliefs are derived, e.g. through introspection or experience, and without relevance for game play. However, concepts such as introspection or expectations often serve as the driving force behind beliefs. While those and other concepts differ in their requirements on beliefs, it is a common feature that each player has beliefs. Therefore belief elicitation, in theory, should not modify the behavior compared to a situation without elicitation, since adding a simple statement of something that players have anyway neither changes the game nor the optimal behavior.

Theory predicts slightly different results if subjects are paid to state correct beliefs, e.g. by a (proper) scoring rule like the quadratic scoring rule. If the payoff for the belief statement is additional to the game payoff, the incentive structure can change and therefore behavior might be altered. Such a change in incentives is especially likely if subjects are not risk-neutral. My experimental design takes care of this issue by never paying the belief statement and the action choice in the same game. Also the possible payment for the belief statement is in the same range as the potential payoff for the game outcome to ensure that action choices are made with identical incentives in both treatments.[7]

In contrast to the leading hypothesis that belief elicitation does not affect subjects' behavior, the data shows that action choices in the belief treatment differ from the chosen actions in the baseline treatment for several subgroups of games. While the data reveals no general effect of belief elicitation on game play in all games, more chosen actions in the belief treatment are in accordance with Nash equilibrium in dominance-solvable games (60.8% compared to 68.4%). In games with a symmetric alternative to the Nash-equilibrium outcome, fewer action choices are in line with Nash-equilibrium predictions if beliefs are elicited. These results show that the effect of belief elicitation on choices is neither game independent nor does the elicitation always drive subjects towards equilibrium play. While chosen actions are sometimes affected there seems to be no difference in stated beliefs between treatments. It is important to keep in mind that if the elicitation procedure affects

[6] In repeated games belief learning and experience are also relevant, but since this study concentrates on one-shot environments these aspects do not apply.

[7] Blanco et al. (2010) analyze the possibility to use incentivized belief statements as a hedging device. The general message of their paper is that one has to be careful if significant and transparent hedging opportunities arise. See section 1.2.1 for details on the incentives in each treatment and how hedging opportunities are addressed.

beliefs, all stated beliefs are always “modified” beliefs. The beliefs subjects hold when making their action choices in the baseline treatment are not elicited and by definition a direct elicitation of these “original” beliefs is impossible. Therefore only stated beliefs can be compared between treatments. As I will argue in more detail below these results indicate that the effect is triggered by subjects who “think harder” about the decision situation when beliefs are elicited.

The remainder of the paper is structured as follows: Section 1.2 outlines the experimental design and states possible reasons why game play might be affected by belief elicitation. The data is analyzed in section 1.3 by first examining the chosen actions and then stated beliefs, while section 1.4 concludes.

1.2 Experimental design and hypotheses

In this section I first describe the experimental design, the difference between the two treatments and how subjects’ beliefs are elicited. Then I present the used games and discuss their structure before the hypotheses for the experiment are outlined.

1.2.1 Overall experimental design

The experiment consists of two treatments. In both treatments subjects play a series of 20 normal-form games. In the baseline treatment subjects state their choices for all games without stating a belief about the chosen action of the other player. In the belief treatment subjects play the games in the same order, but always state their beliefs and their decision for a game at the same time. Therefore subjects in the belief treatment are asked to indicate their choice and to state their beliefs simultaneously. Beliefs of subjects in the baseline treatment are elicited for all games after all 20 action choices have been made. For the belief statements in the baseline treatment the games are presented to subjects in the same order as for the action choices. While they see the game matrix of the respective game when stating their beliefs subjects in the baseline treatment are not reminded of their own action choices in this game. Also subjects are only asked to state their beliefs and are not given the opportunity to make a (new) action choice.[8]

In both treatments no feedback on the choices of the other subjects is provided until the end of the experiment. Subjects can move from one game to the next at their own speed, but once they submitted an action or belief they cannot return to previous games to alter

[8] This design reduces the probability that the belief statements of subjects in the baseline treatment are chosen in such a way that the previous action choices are rationalized. It therefore allows to analyze whether belief statements differ if they are elicited simultaneously with action choices or after the actions have already been chosen. However, probably not all subjects remember their action choices (correctly), which might affect the number of action choices that are best responses to the stated beliefs (see section 1.3.5 for an analysis of the stated beliefs).

their decisions. In all sessions half of the subjects are randomly assigned the role of "row"-players and the other half are "column"-players. All games are presented to all subjects as row players to prevent any effect of the game representation on action choices or belief statements. For each game subjects are matched with one player of the other type. The matching is completely anonymous and changes after each round. Having all subjects play each game only once, disguising the role change for asymmetric games with the isomorphic transformations and rematching subjects ensures that interactions are one-shot as much as possible. Focusing on one-shot games has the advantage that learning between games is unlikely and that therefore interaction effects between belief elicitation and learning that could affect chosen actions are prevented.[9]

In both treatments each subject's belief over his or her opponent's two (or three) action choices is elicited with a proper scoring rule. Subjects are asked to state how likely they think it is that the other subject chooses an action by stating a probability $p_k \in [0,100]$ for each possible action k.[10] Therefore subjects state a probability distribution over all possible actions of the other player. The following quadratic scoring rule determines the payoff for the belief elicitation: $S_i(p) = \alpha - \beta \sum_{k=1}^{n} (I_k - p_k)^2$. α and β are constants, while the indicator function I_k represents the choice of the other subject. It is set to 100 for the actual choice and to zero for the other action(s). For both treatments $\alpha = 80$, $\beta = 0.004$ and each point is valued at 5 cents. The chosen parametrization limits the payoff for belief statements to 4€ and ensures that the payoffs for the actions are in the same range as the payoffs for the stated beliefs.

Subjects are informed that reporting the expected value of their subjective probability distribution over the other player's actions maximizes their expected payoffs. Stating beliefs truthfully is therefore optimal for risk-neutral subjects.[11] In the baseline treatment subjects are told in the beginning of the experiment that there are two parts. They first enter their choices for all games and only after the completion of this first part they are informed about the content of the second part. They are then given instructions for the second part before they state their beliefs. The belief treatment consists only of one part, since actions and beliefs are stated at the same time.

Paying the action and belief of a subject in the same game might result in hedging opportunities that distort the action choice as demonstrated by Blanco et al. (2010). If only the beliefs or the action choice is paid for a game subjects cannot use their belief statement to insure themselves against unwanted outcomes. My experimental design takes care of

[9] No-feedback learning as in Weber (2003) is possible, but can be controlled for since due to player roles subjects play the asymmetric games in a slightly different order (see section 1.3.1).

[10] Subjects can also enter decimals. All two/three belief statements for a game have to sum up to 100.

[11] There exist scoring rules that are more robust to different risk preferences, e.g. see Holt and Smith (2016) or Schlag et al. (2015). Since these scoring rules are usually more difficult for subjects to understand and the fact that my main research question only relies on the elicitation of the beliefs, but not on the stated beliefs themselves, I consider the quadratic scoring rule to be appropriate. Potential confounds of the used scoring rule for the reported beliefs are discussed in section 1.3.4.

possible hedging opportunities by only paying the belief statement or the chosen action for a given game. A random draw at the end of each session determined six games that were selected for payment for all individuals in this session. In three games subjects are paid the payoff resulting from the action choices and in three different games subjects are paid for their stated beliefs.

The experiment was carried out in the experimental laboratory of the University of Mannheim (mLab) in November / December of 2012 and March of 2013 using zTree (Fischbacher 2007). Six sessions were conducted (three sessions for each treatment) with a total of 122 subjects, mostly undergraduate students of the University of Mannheim, recruited via ORSEE (Greiner 2015). All subjects participated only in one treatment and earned 15.24€ on average.[12] A session lasted always less than 90 minutes.

1.2.2 The games

All games in the experiment are two-person games with two or three actions for each individual, which means the games are of the 2x2, 3x3 or 2x3 form. There are 12 distinct games: 4 symmetric and 8 asymmetric games. Since subjects play every asymmetric game in both roles there are 20 games in total. However, subjects do not play the exact game in both roles, but rather isomorphic transformations.[13] For each asymmetric game a second game is created by transposing the player roles, changing the order of actions and adding or subtracting a constant to all payoffs. Therefore there are eight pairs of equivalent games. This modification leaves all relevant properties of the game (like Nash equilibria and dominance relations) unchanged and results in a total of 16 asymmetric games played by each subject. Because these non-trivial transformations of payoffs and order of actions disguise the equivalence of games, it is unlikely that subjects realize that they play each game in both roles. Additionally two equivalent games are never played in direct sequence. Due to the isomorphic transformations, behavior of row players in asymmetric games can directly be compared to behavior of column players in the equivalent game.

Tables 1.1 and 1.2 list all games and used transformations. To analyze whether game properties play a role in the effect of belief elicitation on action choices the used games vary in multiple dimensions. Besides being either symmetric of asymmetric, important characteristics that differ between games are whether a game is dominance-solvable, if it has a unique or multiple pure-strategy Nash equilibria, whether the Nash equilibrium is Pareto dominated by another outcome and whether or not an alternative outcome to the Nash equilibrium with symmetric payoffs for both subjects exists.[14] Table 1.3 summarizes the main differences

[12] This average contains only five sessions, since in one session a computer crash made it impossible to recover the matching. Subjects in this session received a fixed payment. The crash occurred after all decision and belief statements had been recorded. The datafile containing all decisions and belief statements could be recovered and the data is used in the analyses.

[13] A similar procedure has been used by Costa-Gomes and Weizsäcker (2008).

[14] None of the games has a mixed-strategy Nash equilibrium.

between games that become relevant in the analysis.

Table 1.1: Games 1-12

Game 1

	'T'	'M'	'B'
T	40, 40	20, 30	0, 20
M	30, 20	20, 20	100, 10
B	20, 0	10, 100	100, 100

Game 2

	'T'	'B'
T	75, 75	25, 85
B	85, 25	30, 30

Game 3

	'T'	' M'	'B'
T	10, 10	49, 58	37, 60
M	58, 49	22, 22	16, 36
B	60, 37	36, 16	38, 38

Game 4

	'T'	'M'	'B'
T	26, 26	22, 17	18, 24
M	17, 22	11, 11	33, 21
B	24, 18	21, 33	20, 20

Game 5

	L	R
T	55, 79	84, 52
B	31, 46	72, 93

Game 6

	L	R
T	48, 80	89, 68
B	75, 51	42, 27

Game #6's payoffs are obtained by subtracting four points from Game #5's payoffs.

Game 7

	L	C	R
T	74, 38	78, 71	46, 43
M	96, 12	10, 89	57, 25
B	15, 51	83, 18	69, 62

Game 8

	L	C	R
T	73, 80	20, 85	91, 12
M	45, 48	64, 71	27, 59
B	40, 76	53, 17	14, 98

Game #8's payoffs are obtained by adding two points to Game #7's payoffs.

Game 9

	L	C	R
T	31, 25	23, 12	42, 33
M	36, 36	30, 20	18, 27
B	48, 31	29, 42	23, 16

Game 10

	L	C	R
T	29, 20	18, 25	35, 44
M	22, 32	44, 31	14, 25
B	38, 38	33, 50	27, 33

Game #10's payoffs are obtained by adding two points to Game #9's payoffs.

Game 11

	L	C	R
T	52, 23	36, 27	16, 41
M	61, 31	31, 46	22, 28
B	80, 15	12, 40	34, 53

Game 12

	L	C	R
T	49, 30	24, 18	37, 12
M	11, 76	27, 57	19, 48
B	36, 8	42, 27	23, 32

Game #12's payoffs are obtained by subtracting four points from Game #11's payoffs.

Table 1.2: Games 13-20

Game 13

	L	C	R
T	43, 68	11, 31	23, 31
M	38, 38	44, 25	51, 38
B	31, 12	52, 29	45, 33

Game 14

	L	C	R
T	33, 13	27, 46	31, 54
M	33, 25	40, 53	35, 47
B	70, 45	40, 40	14, 33

Game #14's payoffs are obtained by adding two points to Game #13's payoffs.

Game 15

	L	R
T	74, 62	43, 40
M	25, 12	76, 93
B	59, 37	94, 16

Game 16

	L	C	R
T	10, 23	35, 57	60, 72
B	91, 74	14, 92	38, 41

Game #16's payoffs are obtained by subtracting two points from Game #15's payoffs.

Game 17

	L	C	R
T	67, 46	43, 31	61, 16
B	32, 86	52, 52	89, 62

Game 18

	L	R
T	42, 63	82, 28
M	12, 57	58, 85
B	27, 39	48, 48

Game #18's payoffs are obtained by subtracting four points from Game #17's payoffs.

Game 19

	L	C	R
T	32, 29	26, 18	39, 39
B	19, 28	45, 32	42, 19

Game 20

	L	R
T	30, 21	31, 34
M	21, 44	41, 41
B	34, 47	20, 28

Game #20's payoffs are obtained by adding two points to Game #19's payoffs.

Notes: Tables 1.1 and 1.2 display all 20 games used in the experiment. All even games from game 6 on are isomorphic transformations of the previous game. In those games the payoffs are slightly changed and the order of actions is modified compared to the previous game. In all treatments and sessions the order of games was identical: 7, 11, 2, 8, 12, 19, 1, 6, 20, 5, 10, 13, 17, 9, 18, 14, 4, 16, 3 and 15. This sequence ensures that equivalent games are never played directly after each other. Nash equilibria outcomes are underlined in the payoff tables.

Table 1.3: Game properties

Overview of selected game properties for all games

Property/Game #	1	2	3	4	5/6	7/8	9/10	11/12	13/14	15/16	17/18	19/20
unique NE		X	X	X	X	X	X	X		X	X	X
NE Pareto dominated	E	X	X		X	X				X		
dominance-solvable	X	X	X		X	X			X	X	X	
symmetric alternative	W	X	X	X			X		W			X

Notes: Game numbers correspond to game numbers in tables 1.1 and 1.2. E for NE Pareto dominated means that the other outcome is also a NE, W for symmetric alternative means that the symmetric alternative(s) are weakly dominated by the NE.

1.2.3 Hypotheses

If belief statements and action choices are properly incentivized the payments for the stated beliefs should not affect subjects' action choices due to hedging. But there exit two other channels through which subjects' choices might be affected by eliciting beliefs. First, beliefs are usually elicited based on the idea that subjective beliefs are relevant for subjects' action choices. If this assumption is true than chosen actions of subjects might change if beliefs are altered by the elicitation. Existing beliefs could be altered due to the elicitation, because subjects modify their otherwise coarse beliefs if they are asked to state exact numbers for their beliefs or subjects' understanding of the decision situation is increased by the belief statements. Second, subjects who do not form beliefs if they are not asked to state them or subjects' whose action choices are not belief based might condition their action choices more heavily on their beliefs if they are required to state them. In both cases subjects' chosen actions in the same decision situation could differ dependent on whether they are asked to state beliefs or not.

If subjects hold beliefs, even when they are not elicited, and play is belief-based, being asked to put exact numbers on the expected behavior of the other player could motivate subjects to think more about the game in general and about the choice of their opponent in particular. This increased effort could lead to a modification of the "original" beliefs (the beliefs a player has without being explicitly asked to state them) though the increased involvement with the game at hand and thus lead to a deeper understanding of the decision situation. This "thinking-harder" hypothesis predicts subjects behave "more like a game theorist" (Croson 2000) compared to situations without belief elicitation. According to this hypothesis belief elicitation should result in an increased frequency of action choices that are part of a Nash equilibrium and therefore I should observe more equilibrium play in the belief treatment compared to the baseline treatment.

The differences between the treatments should be larger for games in which beliefs are more likely to be affected by an increased sophistication of subjects. More specifically, in dominance-solvable games stating own beliefs about the action choice distribution of the opponent can lead to the discovery of a dominated action of the other player. When a player observes that an action of the other player is dominated, he or she should place a zero (or very small) probability on the likelihood that this action is played. In dominance-solvable games such a belief often results in an own action that should not be chosen, since it is now "iteratively" dominated. The action is only dominated if an action of the other player is assumed not to be chosen. In such a case the modification of beliefs due to the discovery of an iteratively dominated action can influence the own action choice. Therefore the difference in chosen actions between the treatments should be larger for dominance-solvable games.

If play is not belief based subjects employ other mechanisms or (simple) heuristics to determine which action to choose. A subject might always pick the action that yields him or her the highest possible payoff, select the action which ensures him or her the highest payoff for sure (max-min choice) or even choose an action randomly. These strategies do not require any expectations about the behavior of the other player. While this behavior might be unlikely in situations in which subjects possess a lot of experience, it is more likely in the studied context of one-shot games in which the question whether subjects hold meaningful beliefs at all arises.[15]

For subjects who do not to base their action choices on beliefs, the elicitation could induce some beliefs. As subjects are required to state beliefs and rewarded for correct predictions, it is likely that they actually think about the other player's most probable action and thereby form meaningful beliefs. Given that they spend effort in deriving their beliefs and in the belief treatment they have not made a final action choice yet, they might consider these beliefs to select their action. Therefore for those subjects the elicitation could lead to a change in behavior. Furthermore, for subjects who have beliefs, but do not take these beliefs into account for their action choice, the elicitation could encourage them to actually consider these beliefs in their action choice. If that is the case, also for these subjects the chosen actions might be different with belief elicitation. Consequently the choice distribution could be changed compared to a situation without belief elicitation for both types of players. Additionally, if subjects best respond to their beliefs, it is more likely that a player selects an action which is part of a Nash equilibrium, since a Nash equilibrium exactly defines mutual best responses. While this effect should be present in all games, it will most likely be more pronounced in games in which the formation of beliefs has a decisive effect on players' decisions. Therefore, similar to the prediction stated above, a stronger effect on game play is expected for dominance-solvable games.

Furthermore, if eliciting subjects' beliefs actually influences game play the effect has likely different implications for very sophisticated subjects and subjects who are less sophisticated. In the extreme case the action choices of a fully rational subject who always plays according to Nash equilibrium predictions even without belief elicitation should not

[15] Although learning between multiple one-shot games could take place, since no feedback about the choices of the other players is given until the end of the experiment, only "no-feedback learning" is possible, which I consider not very likely.

be affected. Such a player is already assuming that his or her opponent plays the equilibrium action and therefore his or her beliefs should not change due to the elicitation. On the other side of the spectrum are subjects without any beliefs, e.g. a player that chooses the action which can yield him or her the highest possible payoff or a player who does not consider the incentives of his or her opponent. For these subjects the belief statements might help to organize their thoughts in a meaningful manner and therefore increase the probability of equilibrium play. If that is the case models of game play that consider various levels of subjects' sophistication should result in different distributions of subjects' types between the treatments. Especially, I consider several player types as defined by the level-k model (Stahl and Wilson 1994). I expect a larger share of subjects being classified as relatively sophisticated, e.g. showing a higher level of strategic reasoning, in the belief treatment than in the baseline treatment.

But belief elicitation is also an additional task subjects have to perform. Since subjects have to understand how to state their beliefs and how they are rewarded for it, belief elicitation could lead to more confused subjects and result in more random action choices. Therefore belief elicitation induces a different choice due to confusion. If that is the case, I would not consider it as an effect of the elicitation, but rather the consequence of poor instructions. Since all decisions in this experiment are made without any time pressure I consider it unlikely that the additional task distracted subjects. Nevertheless, if it was the case, it should result in less equilibrium play, since there is no reason why a confused subject should play as predicted by a theory that has strong demands on subjects' rationality and strategic sophistication. Therefore distraction works in the opposite direction as predicted by the two hypotheses above and would weaken a potential effect. Also if the belief elicitation is rather a burden than a supportive tool for less sophisticated subjects, it is not clear that the distributions of different "sophistication types" differs between the treatments.

1.3 Results

1.3.1 Preliminaries

The experimental design with row and column players playing isomorphic games was chosen assuming that this variation does not affect subjects' behavior. While for both types all games are equivalent, the order of games differs between row and column players. If the transformation and the different order of games is without consequences for the chosen actions, I can pool the data across player roles. To test whether the transformations or the order of games affects subjects' action choices I contrast row players' choices in asymmetric games with the action choices of column players in the transformed game (and vice versa). For symmetric games no test is necessary, since all players see the games as row players. Such a test compares categorical data between independent samples, therefore I conduct

Fisher's Exact Probability Tests (FEPT) for count data.[16]

Comparing subjects' aggregate actions separately on each game in each treatment yields only two significantly different distributions between types, which is in line with the expected number of rejections for 40 comparisons.[17] For the tests in the next sections the data is therefore pooled across player roles. To analyze whether the chosen actions are different from random play I perform χ^2 goodness of fit tests separately for each game. Testing the chosen actions yields 32 significant differences from the uniform distribution (out of 40).[18] The frequent deviations from random play in both treatments clearly demonstrates that subjects' choices are not random. These results yield that player roles are without effect on game play and that actions are not picked randomly.

Before discussing the effect of the belief elicitation on the data I provide the frequencies for all action choices in all games and treatments. Table 1.4 contains the percentage of actions chosen for all 20 games separately for the two treatments and player roles.

There are two main take aways from the numbers reported in table 1.4. First, there seems to be some heterogeneity among subjects' choices. In very few games an action is chosen by more than 75% of subjects. Second, while subjects' choices are heterogeneous the choice frequencies already indicate that subjects' behavior is not random. In many games one action is only chosen by a small fraction of subjects. While subjects seem to have a common understanding of which actions are less desirable, they do not agree on which action is the "best" in most cases.

1.3.2 Belief elicitation and the role of game properties

The research about the likelihood of Nash-equilibrium play has shown that various game properties and the decision environment affect the predictive power of Nash equilibrium, e.g. see Rey-Biel (2009) for normal-form games. It is likely that the effect of belief elicitation on game play also depends on game properties. Given the mixed results reported in the literature whether belief elicitation affects the likelihood of action choices that are part of a NE, it is especially interesting to analyze the role of game properties jointly with a possible effect of the belief elicitation. In order to exploit the various game properties of the games used in

[16] There exists a literature regarding the best tests for contingency tables showing that unconditional tests like Boschloo's test have greater power than conditional tests like the FEPT at least in 2x2 tables. However, the power advantage vanishes quickly with sample size for larger tables like 2x3 tables, e.g. see Mehta and Hilton (1993). Since most of the 20 games studied in this paper are not of the 2x2 form, the additional computational difficulties for using unconditional tests are large compared to the potential gain in power. Therefore FEPT are used for the following analysis.

[17] All test in this paper are two-sided unless explicitly stated otherwise. In game 11 in the baseline treatment (p-value < 0.01) and game 8 in the belief treatment (p-value $= 0.02$) the differences between the player roles are significant. All other p-values are above the 5%-level and are usually much higher.

[18] For 5 games in the baseline treatment, games 2, 6, 16, 19 and 20, and 3 games the belief treatment, games 6, 19 and 20, there are no significant differences from random play. In the pooled data only the choices in games 6, 16 and 19 are not different from random play.

Table 1.4: Percentages of chosen actions by game, treatment and player role

	Chosen actions baseline treatment						Chosen actions belief treatment					
	Row players			Column players			Row players			Column players		
Game	T	M	B	L	C	R	T	M	B	L	C	R
1	5	33	62	–	–	–	3	44	53	–	–	–
2	45	–	55	–	–	–	35	–	65	–	–	–
3	5	5	90	–	–	–	5	6	89	–	–	–
4	57	7	37	–	–	–	56	15	29	–	–	–
5	63	–	37	40	–	60	77	–	23	52	–	48
6	43	–	57	70	–	30	32	–	68	94	–	6
7	63	3	33	3	70	27	74	10	16	0	87	13
8	77	23	0	70	20	10	58	39	3	65	32	3
9	10	30	60	60	13	27	10	6	84	71	23	6
10	13	23	63	13	57	30	3	29	68	10	65	26
11	3	50	47	7	27	67	6	42	52	3	26	71
12	73	3	23	73	10	17	71	0	29	68	29	3
13	30	50	20	50	3	47	19	61	19	32	0	68
14	0	50	50	20	67	13	3	48	48	6	77	16
15	10	20	70	60	–	40	26	19	55	61	–	39
16	33	–	67	23	60	17	68	–	32	10	61	29
17	63	–	37	77	10	13	67	–	32	87	10	3
18	77	7	17	63	–	37	94	6	0	87	–	13
19	53	–	47	27	23	50	58	–	42	42	26	32
20	30	43	27	47	–	53	39	42	19	35	–	65

Notes: A - indicates that there was no action for this player role in the given game. Games 1-4 are symmetric games, therefore there are no column players, but all action choices are reported in the columns for row players. Due to player roles and the changed order of actions between equivalent games the stated percentages cannot directly be compared across player roles even for isomorphic transformations. All entries are in %.

the experiment each decision (action choice) of an individual is analyzed separately. The variable of interest is whether the chosen action is part of a pure-strategy NE or not. Since each subject made 20 decisions there are in total 2440 observations (122 subjects) available for the analysis.

The main properties considered are whether a game is dominance-solvable, whether a game is symmetric, whether a symmetric alternative to the NE exists and whether the NE is Pareto dominated by another outcome. I run a probit model on dummies for these four game properties: dominance solvability (*dominance solvable*), symmetry (*symmetric*), whether the NE is Pareto dominated by another outcome (*NE dominated*) and whether a symmetric alternative to the NE exists (*symmetric alternative*). Also a treatment dummy for decisions made in the belief treatment is used (*belief treatment*). Additionally all game property dummies are interacted with the treatment dummy to control for possible interaction effects: dominance-solvable games and the treatment (*dominance solvable & belief treatment*), symmetric games and the treatment (*symmetric alternative & belief treatment*), NE dominated and treatment (*NE dominated & belief treatment*) and symmetric alternative and belief treatment (*symmetric alternative & belief treatment*). Since in this analysis all decisions of an individual are considered separately, it is very likely that the residuals are not homogeneous,

Table 1.5: Probit estimation of NE

	Probit	
dep. variable: NE	Marginal Effect	(Std. Err.)
belief treatment	0.04	(0.05)
Game properties		
dominance solvable	0.44***	(0.04)
symmetric	0.36***	(0.04)
NE dominated	−0.43***	(0.04)
symmetric alternative	−0.07	(0.04)
Interaction effects		
dominance solvable & belief treatment	0.14**	(0.06)
symmetric & belief treatment	0.08	(0.08)
NE dominated & belief treatment	−0.09	(0.07)
symmetric alternative & belief treatment	−0.14**	(0.06)
Observations	2440	
Pseudo R^2	0.137	
Predicted probability	0.562	

Notes: Probit estimates. Marginal effects (evaluated at the mean of independent variables); * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All standard errors are clustered at the subject level.

as the errors of the same subject are most likely correlated. Therefore the shown regression includes standard errors that are clustered at the subject level.

The fist result from the regression is that there is no significant treatment effect on the likelihood of equilibrium play across all games. The coefficient on the treatment dummy (*belief treatment*) is not significantly different from zero. Subjects who are asked to simultaneously state their beliefs and action choice are not more likely to select an action that is part of a pure-strategy NE than subjects who only choose their action.

Result 1. *Belief elicitation does not generally increase the probability of equilibrium play.*

Result 1 does not necessarily indicate that there is no effect of belief elicitation on game play, since in the analysis all games are considered jointly. But the finding is evidence that suggests that if there is an effect on game play, it is not homogeneous for all games and all subjects. The regression coefficients on the four considered game properties show that game properties affect the likelihood of equilibrium play. The probability of NE play is significantly increased in games which are dominance-solvable or symmetric. The coefficients for both dummy variables (*dominance solvable* and *symmetric*) are significantly positive (p-values < 0.01). Furthermore NE play is less likely if the NE is Pareto dominated by another available outcome (*NE dominated*). Whether a symmetric alternative exists does not affect the probability of equilibrium play (*symmetric alternative* is not significantly different from zero).

Result 2. *Game properties have a significant influence on the chosen actions.*
(i) More actions in accordance with NE are played if the game is dominance-solvable or symmetric,
(ii) and less NE actions are chosen if the NE is dominated by another outcome.

The hypotheses outlined above predict a stronger effect of belief elicitation on chosen actions for games that are dominance-solvable. This hypothesis is supported by the significantly positive coefficient on the interaction between the dominance-solvable games and the treatment (*dominance solvable & belief treatment*, p-value < 0.05). While the size of the interaction effect is modest, compared to the effect of dominance solvability alone, it is important to note that this effect is basically "on top". Subjects in the belief treatment are more likely to choose an action that is part of a pure-strategy NE in dominance-solvable games than subjects in the baseline treatment.

The data reveals an opposite effect for games with a symmetric alternative. The coefficient for the interaction effect between the symmetric alternative and the treatment (*symmetric alternative & belief treatment*) is negative and significantly different from zero (p-value < 0.05). Therefore in the belief treatment NE play is less likely in games with a symmetric alternative. The two interactions between the belief treatment and the symmetric games or games with a dominated NE are not significant. Belief elicitation does not affect the probability of equilibrium play for these games.[19]

Result 3. *Belief elicitation affects the chosen actions. If beliefs are elicited together with actions*
(i) more actions in accordance with NE are chosen in dominance-solvable games,
(ii) and less NE actions are played if a symmetric alternative to the NE exists.

Overall the analysis of the individual game properties results in two main findings. First, there is treatment effect on the likelihood of NE action play due to belief elicitation in two subgroups of games. In dominance-solvable games belief elicitation increases equilibrium play, while the probability is reduced by the elicitation in games with a symmetric alternative. Second, the results of the role of game properties on the likelihood of NE play, independent of the treatment effect, are in line with other findings in the literature.[20]

[19] The results are qualitatively unchanged if a logit model or a linear regression is used. In table A.2 in the appendix the results of a linear probability model and a logit estimation with marginal effects are reported.

[20] Costa-Gomes et al. (2001) report higher rates of equilibrium compliance for games that are dominance-solvable, especially for games that are solvable in 1 or 2 rounds of iterated pure-strategy dominance. Rey-Biel (2009) reports higher percentages of equilibrium play for constant sum normal-from games compared with variable sum games (78% vs 68%).

1.3.3 Treatment effects on subjects' choices

In this section I first analyze whether belief elicitation affects games play by considering subjects' choices in each game separately. Then I analyze whether subjects are differently affected by the elicitation by comparing subject specific measures on the probability of equilibrium play over games and then compare the distribution of this measure between treatments. Since many experiments have shown that Nash equilibrium predictions and subjects' decisions do not always coincide, I consider some alternative models that have been applied to explain subjects' choices in similar games. Based on their choices I classify subjects into different types and examine whether there is a difference in the distributions of these types between the treatments.

In order to test whether there exists a treatment effect regarding the chosen actions in the individual games, I perform FEPT to compare the chosen actions between treatments. Testing all 20 individual games for differences between the treatments, I find a significant difference only in two games.[21] Therefore the data from the individual games does not imply a treatment effect. While the difference is not significant in most games, it is interesting to note that in seven out of the ten games that have a unique NE and are dominance-solvable, the share of NE actions selected is higher in the belief treatment. This observation is in line with result 3(i), while the insignificant differences for most games supports result 1.

The data of the individual games does not reveal a lot of information about what is happening between the treatments. Therefore to identify whether the elicitation of beliefs has an influence on the decision of subjects, I calculate for each subject the total number of actions chosen in accordance with NE predictions. The measure "# NE actions chosen" counts how often an individual picks an action that belongs to a pure-strategy NE of the game. Overall, subjects in the baseline treatment play an action in accordance with NE in 54.6% of all cases. This percentage is in line with previous results on normal-form games.[22] The share of equilibrium actions chosen is 58% in the belief treatment. According to a Mann-Whitney-U (MWU) test this difference is not statistically significant (p-value = 0.14) and therefore confirms result 1 from above.

However the hypotheses outlined above predict a stronger effect for games that are dominance-solvable and the probit regression also yields an effect for these games. If only dominance-solvable games are analyzed the picture changes and the effect size increases. Figure 1.1 presents for each individual how often she or he selected an action in accordance with a NE for all 13 dominance-solvable games. The right histogram, which shows the distribution for the belief treatment, seems to be shifted to the right compared to the left, which

[21] The difference is significant in games 5 and 10. The result is unchanged if not all actions are compared, but only whether an action that is part of a pure-strategy NE or not.

[22] Of course the percentage highly depends on the games played. Rey-Biel (2009) for example finds higher rates of equilibrium play in arguably "easier" games, while Costa-Gomes and Weizsäcker (2008) find lower rates.

presents the distribution in the baseline treatment. While the percentage of equilibrium actions played increases in both treatments (to 61% for the baseline and to over 68% for the belief treatment), the difference between the two treatments is much larger and significant at the 1%-level (MWU test, p-value < 0.01).[23] Analyzing subjects' choices between treatments for dominance-solvable games therefore confirms result 3(i).

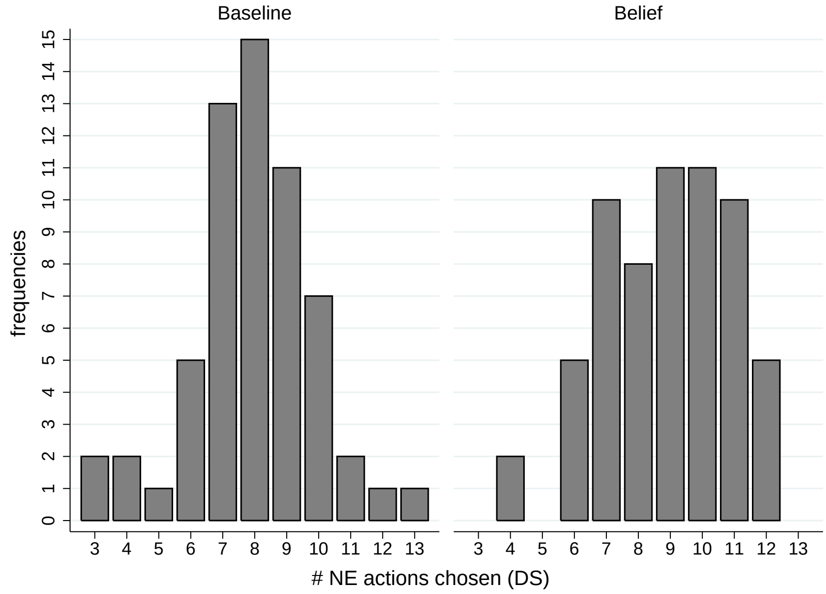

Figure 1.1: Histogram of NE actions for both treatments

The thinking-harder hypothesis implies that belief elicitation guides subjects to invest more effort in thinking about the decision situation and therefore they better understand the situation. This deeper understanding should result in more action choices in line with game theoretic predictions. In dominance-solvable games subjects should detect dominant or dominated actions of the other player more often. As a consequence subjects will act based on this knowledge and therefore will less often pick actions that are iteratively dominated, meaning actions that are only dominated if one eliminates all dominated actions of the other player from consideration.

Surprisingly this effect cannot be found. Similar to the measure "NE actions chosen" above, I sum up the number of games in which a subject plays an action that is not iteratively dominated and compare these aggregates between treatments. There is no difference between the treatments. In the baseline treatment subjects play an iteratively dominated action in about 4.4 games, while in the belief treatment subjects select such an action in about 4.2 games (MWU test, p-value = 0.63). Therefore the chosen actions do not indicate that subjects are more likely to detect a dominated action of the other player and respond accordingly

[23] Further restricting the set to games which are strictly dominance-solvable, meaning only including games that are dominance-solvable by eliminating actions that are strictly dominated, yields similar results (MWU test, p-value = 0.01). Dropping the observations from the crashed belief session yields the same qualitative results.

in the belief treatment.

Result 4. *Iteratively dominated actions are equally like to be played in both treatments.*

While result 3(i) shows that equilibrium play increases in dominance-solvable games, there is no differences in avoiding iteratively dominated actions between treatments. Playing an iteratively dominated action means that either a subject does not realize that his or her action is iteratively dominated or does not believe that the opponent realizes the domination of one of his or her actions. In both cases a subject might play an action that is iteratively dominated, but in the second case the subject is aware of the domination. There is a weak indication that belief elicitation increases the understanding of the game for subjects which would support the interpretation that subjects are more likely to be aware of the domination if their beliefs are elicited. In the baseline treatment subjects play an action that is dominated (not iteratively dominated) on average in 2 games. In the belief treatment subjects play such an action in 1.4 games. While the difference is not significant (MWU test, p-value = 0.12), this observation could hint that the "thinking harder" due to the belief elicitation helps subjects to detect an own dominated action. In that case the "thinking harder" would take place at a "lower level" than expected.

The analysis so far focused on NE play and how belief elicitation and game properties influence the likelihood of NE play. Since many experiments have shown that Nash equilibrium predictions and subjects' decision do not always coincide, various alternative models have been applied to explain the choices made by subjects. A very prominent one is the level-k model of cognitive reasoning developed by Stahl and Wilson (1994) in which player types differ in their strategic thinking ability.

Subjects with a low level of sophistication are assumed to have no beliefs about the behavior of their opponent(s). While different versions of how these subjects make their decisions exist, one frequently used assumption is that they choose their action randomly from all available options. The version of the *L0* model I use therefore predicts that a player choses all available actions with equal probability. A player who plays a *L1* strategy best responds to the predicted choice of a *L0* player and a *L2* player best responds to the play of a *L1* player. Higher levels of play, like *L3*, *L4* etc., are defined accordingly. Since previous studies have found that only very few subjects choose strategies according to the predictions of *L4* or higher types, I consider the strategies *L1* to *L3*. The other models of game play considered are *D1*, *D2*, *Maxmin* and *Efficiency*. The *D1* model predicts that a player best responds to a subject choosing all available undominated actions with equal probability. *D2* players best respond to the predicted choice of a *D1* player. A *Maxmin* player selects the action with the highest minimum payoff, while an *Efficiency (Ef)* type picks the action that maximizes the (potential) sum of both players' payoff. An *efficiency* player therefore chooses the action, which can lead to the cell with the highest sum of payoffs for both players.

Table 1.6 states how many of the chosen actions are in accordance with the predictions

Table 1.6: Percentage of actions matched by models' predictions

Treatment	Games	NE	L1	L2	L3	D1	D2	Ef	Maxmin
Baseline	not DS	0.43	0.57	0.48	0.43	0.53	0.41	0.47	0.51
	DS	0.61	0.60	0.59	0.50	0.56	0.52	0.41	0.58
	All	0.55	0.59	0.55	0.47	0.55	0.48	0.43	0.56
Belief	not DS	0.39	0.58	0.50	0.40	0.55	0.44	0.50	0.57
	DS	0.68	0.60	0.65	0.56	0.61	0.59	0.33	0.62
	All	0.58	0.59	0.60	0.51	0.59	0.54	0.39	0.60
Average	**All**	**0.56**	**0.59**	**0.58**	**0.49**	**0.57**	**0.51**	**0.41**	**0.58**

of the various models. The table lists the percentages split for the treatments baseline and belief, and further contains the results only for dominance-solvable games (DS) and games that are not dominance-solvable (not DS). The last row displays the percentages pooled for both treatments and all games. The table illustrates again that more actions in accordance with Nash equilibrium have been chosen in dominance-solvable games. For all splits the percentage of NE actions is higher for DS games compared to not dominance-solvable games and, as discussed in section 1.3.2 above, the effect is larger in the belief treatment. Considering all choices in all treatments, *L1*, *L2* and *Maxmin* are slightly better than the *NE* predictions, but the differences are not large. The worst performing model is the *Efficiency* model, which does extremely poor for dominance-solvable games especially in the belief treatment.[24]

While between the treatments the number of choices correctly predicted by the *L1* model are roughly similar (always within one %-point), the differences for models with higher strategic thinking requirements (*L2*, *L3*, *D1* and *D2*) are larger. More subjects in the belief treatment make choices in accordance with these models compared to the subjects in the baseline treatment (an increase of 4 to 6%-points). Similar to the difference in NE actions chosen, the difference between the treatments mainly stems from dominance-solvable games. Subjects' choices in the belief treatment are more in line with model predictions of higher cognitive reasoning levels.

The paragraphs above consider each choice of a subject separately. In the remainder of this section subjects are classified according to their individual choices in all 20 games jointly. "X-type players" are subjects for which model "X" makes the most correct predictions. *Clear cases* are subjects for which one model predicts more choices correctly than any other considered model. *Ties* are stated for subjects for which exactly two models predict the most chosen actions correctly. In this case a subject is proportionally allocated to these two models. Therefore, if three or more models predict equally many choices of a subject correctly, and no other model makes more correct predictions, the subject is still classified as *not identified*. Table A.1 in the appendix states the results for this classification.

The classification leads to about a quarter of all subjects not being sorted into one of the eight categories. For the eight models considered here there is one notable difference

[24] For the 20 games (6 with two and 14 with three actions) the probability for predicting the correct choice is about 38.1% for purely random predictions. Therefore the 39% of the *Efficiency* model are really weak.

between treatments. When all games are considered the biggest treatment difference is the portion of subjects who are classified as *Efficiency* types. Many more subjects are classified as *Efficiency* types in the baseline compared to the belief treatment (15% vs. 4%). There is no big difference in any other category.

Next subjects are classified based on their choices in dominance-solvable games only, since it was demonstrated that subjects' choices differ between treatments in these 13 games. Therefore by restricting the classification to these games, it becomes more likely that the simple sorting mechanism applied here (the modal number of correct predictions) can identify a difference between the treatments.

Table 1.7: Classification of subjects by model predictions - only dominance-solvable games

	Clear cases		Ties		Clear cases and ties	
	Baseline	Belief	Baseline	Belief	Baseline	Belief
Model	(1)	(2)	(3)	(4)	(5)	(6)
NE	13.33	29.03	3.33	1.61	16.66	30.64
L1	13.33	14.52	5.00	1.61	18.33	16.13
L2	11.67	12.90	5.00	1.61	16.67	14.51
L3	0.00	0.00	0.83	0.00	0.83	0.00
D1	1.67	3.23	4.17	1.61	5.84	4.84
D2	0.00	0.00	0.00	0.81	0.00	0.81
Ef	15.00	4.84	2.50	0.81	17.50	5.65
Maxmin	15.00	14.52	2.50	1.61	17.50	16.13
not identified	30.00	20.97	6.67	11.29	6.67	11.29

Notes: The first and second column only state unique predictions. The ties, which are reported in the third and fourth column, are ties between exactly two models. In case of a tie the subject is classified as 50% for each model in the last two columns. All entries are in %.

The result of this analysis is presented in table 1.7. Columns one and two state the clear cases for all models, while the ties between exactly two models are listed in columns three and four. The last two columns list the total percentage of subjects for each model split for the two treatments, including clear cases and ties. Regarding the procedure to allocate subjects in case of a tie refer to the description above.

The table shows that many more subjects are classified as "Nash" players in the belief treatment - almost twice as many - compared to the baseline treatment. The 30% of subjects classified as "Nash" players are the largest group within the belief treatment. About twice as many subjects are in this group compared to the next best performing model. Given that in the belief treatment more choices are in line with the prediction of Nash equilibrium in dominance-solvable games the difference between the treatments is not surprising. The only other notable difference is for the *Efficiency* model. While 17.5% of subjects in the baseline treatment are classified as *Efficiency* types, less than 6% are in this category for the belief treatment. The percentages in the other categories are similar in both treatments. The number of subjects who cannot be classified is much higher for the belief treatment once ties between two models are considered. For some reason there are many ties between exactly two models in the baseline treatment, but many more ties between three or more models in the belief treatment.

There are two major findings: First, belief elicitation affects game play for dominance-solvable games and leads to more equilibrium play. Consequently more subjects are classified as *Nash equilibrium players* in the belief than in the baseline treatment. Second, at least for the games considered here, no other *player type* of cognitive reasoning outperforms the Nash predictions. However, one has to keep in mind that a cognitive reasoning model consists of more than a single type. This analysis indicates that the treatment effect of belief elicitation influences all subjects and not in particular subjects with a low level of cognitive reasoning. The results regarding the type distribution in both treatments support the explanation that belief elicitation leads to a better understanding of the situation at hand, but does not result in a huge improvement of the strategic sophistication of subjects.

When discussing the classification considering all choices as well as the classification based on the dominance-solvable games, it is important to keep in mind that the experiment was not designed to generate differences between these models. Different than in studies that focus on cognitive reasoning models, the games are not selected to produce differences between the model predictions. Out of the 20 games, the NE and the *L1* model therefore predict different actions to be chosen only in nine games. Naturally the number of games for which the predictions are different to NE are even lower for models of higher cognitive reasoning like the *L2* or the *D2* model. There are four games in which the predictions for the chosen action are different between the *L2* and the NE model and three games for the predictions made by the *D2* and the NE model. Therefore I consider it surprising that there are the reported differences between the models' success in predicting choices.

In sum the results presented so far can be interpreted in favor of the "thinking-harder" hypothesis. The fact that belief elicitation affects game play mainly in dominance-solvable games and that the effect results in more equilibrium play is in line with the idea that subjects who state their beliefs gain an increased understanding of the game. Spending more effort in understanding the game can also explain result 3(ii). If subjects get more familiar with the game, they might play an equilibrium action more often, but also they seem to get aware of the existence of symmetric alternatives and consequently choose this alternative more often. Since the action that results in the symmetric payoff profile is not the equilibrium action choice, the probability of equilibrium play in these games is reduced with belief elicitation. Therefore these results are consistent with subjects inspecting the game more carefully when they are asked to state their beliefs and thereby become more likely to play an equilibrium action or to discover and choose a symmetric alternative. Furthermore, as indicated by the type distributions stated in table 1.7, belief elicitation does not affect less sophisticated subjects stronger than rather sophisticates subjects. While the share of NE types increases this increase is not driven by subjects with a low level of sophistication.

1.3.4 Descriptive statistics of stated beliefs

While subjects state their beliefs in both treatments, before beliefs can be compared a first test concerns the question, whether subjects really state their true beliefs. The major concern is that the quadratic scoring rule is only incentive compatible for risk-neutral subjects. Risk-averse subjects might state beliefs that are biased towards a uniform distribution to secure themselves a sure payoff for the elicitation. Subjects who state a uniform belief over all feasible actions of their opponent either do not have any clue what the other player is going to do, think the other player evenly randomizes over all available actions or use a uniform belief statement to receive a sure payoff. Figure 1.2 shows how often a subject stated non-uniform beliefs. Uniform beliefs are defined as stating no single belief above 51% if the other player has two actions to choose from or not stating a single belief above 34% if the other player has three actions to choose from. Figure 1.2 demonstrates that subjects usually state non-uniform beliefs. I consider this observation as evidence that the problem of subjects stating uniform beliefs in order to generate a sure payoff is relatively limited.[25] Most uniform belief statements should be from subjects who are simply unsure about the likely action of their opponent.[26]

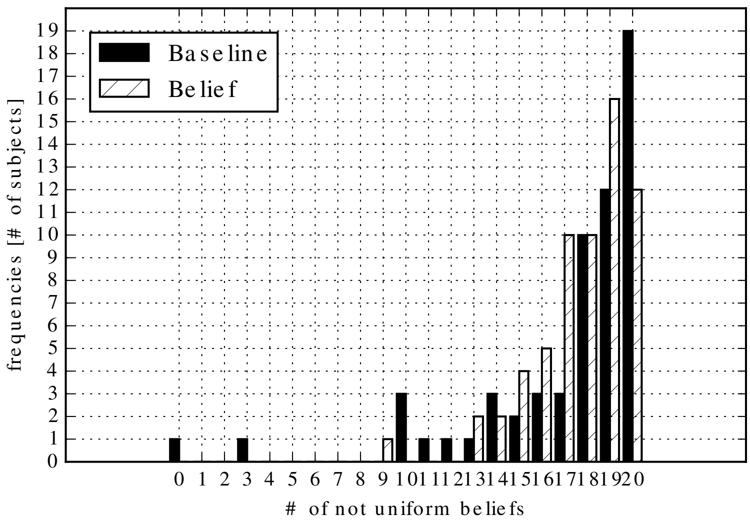

Figure 1.2: Histogram of not uniformly stated beliefs per subject

Beliefs are generally continuous and the elicitation allowed for beliefs to be stated on

[25] Two subjects stated uniform beliefs in 17 or more games. These two subjects only needed 53 and 173 seconds to state all of their beliefs. The mean for the other 58 subjects was 482 seconds, with a minimum of 198 seconds. I consider it likely that these subjects did state uniform beliefs to secure themselves a sure payoff from the elicitation.

[26] Just to demonstrate the distribution of individual beliefs, figure A.1 in the appendix shows the number of stated beliefs for a single action that are higher than 70 percent. Pooling the data of both treatments, since the distributions appear to be very similar, yields an average of about seven games with a stated belief of 70% or more for a single action per subject.

a 0.1%-point gird. However, no subject stated a belief that is not on the 1%-grid and many statements are on the 10% grid. To ease a systematic analysis of the stated beliefs I categorize all belief statements. If a subject stated a belief for a game in which the opponent has three actions to choose from I classify his or her belief into one of four possible categories. Category one contains all beliefs statements with at least a weight of 50% on the first action. In the second category are all beliefs of 50% or more on the second action and similarly the third category contains all beliefs with a weight of at least 50% on the third action. If an individual stated beliefs that do not place a weight of at least 50% on an action his or her beliefs are categorized in the fourth category. For games in which the opponent has two actions the cutoff is changed to 70% and there are three instead of four categories.

I first test for each game whether stated beliefs are different from randomly chosen beliefs by performing χ^2 goodness of fit tests for both treatments separately. This test examines whether the frequency of the belief categories differs from a uniform distribution over the four (three) categories. These tests reject random beliefs for 19 games in both treatments. Only for game #2, in both treatments, the stated beliefs are indistinguishable from random guesses. Comparing the categorized beliefs separately for each treatment, game and player role with a FEPT (5% significance level) yields only two rejections of equivalence in 40 comparisons.[27] These results let me confidently conclude that the stated beliefs are not the result of random answers and that belief statements do not differ with respect to player roles. In the following analysis I therefore consider the stated beliefs as subjects' true beliefs and pool the stated beliefs over player roles.

Subjects' beliefs in the baseline treatment could be systematically different from those stated in the belief treatment if some (very sophisticated) subjects expect other subjects to select different actions if they are asked to state beliefs compared to the case when they are not asked. In part two of the baseline treatment they should state own beliefs that are as close as possible to the choice distribution in part one. Therefore these subjects might state beliefs that do not resemble the pattern of action choices they would expect if the other subjects would be asked to state actions and beliefs simultaneously, as in the belief treatment. While I consider this highly unlikely, a difference in the stated beliefs between the treatments could indicate such a behavior.

A difference in the belief statements between treatments could also be the result of subjects in the baseline treatment using their belief statements as rationalizations for their own behavior in part one. Then these subjects would not really state what they expect the other player to do, but rather state beliefs that make their previous action choice a best response. Since action choices are significantly different between treatments for some subgroups of games the beliefs in the baseline treatment should differ from the ones stated in the belief treatment if subjects try to rationalize their previous choices.

[27] For games 8 and 14 in the belief treatment the stated beliefs are not significantly different from random beliefs.

Result 5. *There is no difference in the stated beliefs between the two treatments.*

Pooling the data across player roles within each treatment and conducting FEPT separately on each game yields no significant difference between the treatments for any game (p-values > 0.05). These observations mitigate concerns that subjects respond differently to the question about their beliefs in the two treatments. Subjects seem to provide beliefs that correspond to their expectations about the other players' behavior. Whatever drives the effect described in section 1.3.2 above, it appears to be triggered by the explicit belief elicitation.

1.3.5 Relationship between actions and stated beliefs

The previous section revealed the equivalence of the stated beliefs in both treatments. In this section the assumption that play is belief based is tested by analyzing whether the chosen actions are best responses to the stated beliefs. For this test I assume that subjects are maximizing their expected payoff. This assumption implies that subjects only care about their own monetary payoff and it excludes any other motives for play like social preferences. Social preferences are important in many settings, and I take some social motives into account when analyzing the role of game properties in section 1.3.2 (symmetric alternative) and in the classification of subjects (efficiency type), but for the calculation of best responses all subjects are assumed to be only self-interested.

Calculating the percentage of best responses over both treatments for all subjects yields a best response rate of 55.3%. This percentage is in line with the results of Costa-Gomes and Weizsäcker (2008) who report best response rates around 50% and it is slightly lower than the results of Rey-Biel (2009). He finds best response rates around 67%, but given that his games are arguably easier than the ones used in my experiment the difference is expected.[28]

If belief elicitation leads to the formation of beliefs or to harder thinking, it is less likely that action in the baseline treatment are best responses. When "thinking harder" is triggered by the explicit elicitation of beliefs and this process leads to refined beliefs, then a likely consequence is that subjects place more weight on their beliefs when the action choice is made. Consequently the probability of playing best response should increase. Given that the beliefs in the baseline treatment are not stated until all action choices are made and that the probability of NE play increases in the belief treatment, a difference in the number of best responses could arise. To test for such an effect I calculate the number of best responses per subject separate for both treatments. The histogram in figure 1.3 indicates a difference between the treatments.

On average subjects in the baseline treatment best respond to their own stated beliefs

[28] I consider his games easier, since he has a higher share of dominance-solvable games (8 of his 10 games and 13 of the 20 used games in my experiment are dominance-solvable) and half of the subjects only play constant-sum games. Additionally constant-sum games are more likely to lead to best responses, since social preferences typically do not change the predictions in those games.

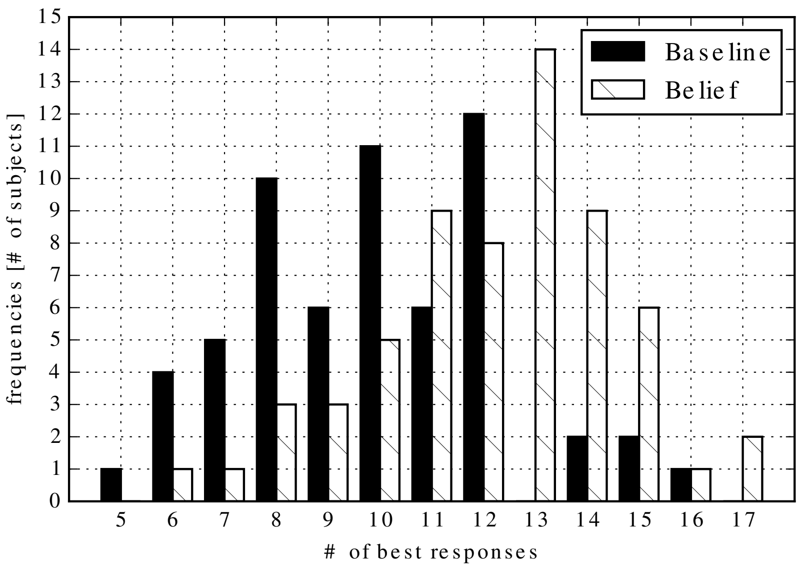

Figure 1.3: Histogram of best responses per subject to stated beliefs

9.9 times, while subjects in the belief treatment do so 12.2 times (out of 20 possible). This difference in means is significant (MWU, p-value < 0.01). The shift of the best response distribution seems to affect all subjects in the same way, which is consistent with an identical influence of the effect on subjects with a low and subjects with a high number of equilibrium action choices.

Result 6. *Chosen actions in the belief treatment are significantly more often best responses to stated beliefs.*

The cumulative distribution function of the number of best responses in the belief treatment actually first order stochastically dominances the one of the baseline treatment. This finding relates to the similar distributions of types in both treatments from above and suggests a homogeneous effect on subjects independent of subjects' strategic sophistication. Result 6 is also consistent with subjects "thinking harder" about the game, when they are asked to state their beliefs. Thinking about the likely action of their opponent increases the probability that they realize an aspect of the game that they missed when they made their action choice, e.g. an own dominated action. When they then state their beliefs, they state the beliefs they have after thinking about the game again. By stating these modified beliefs subjects act in the expectation that their opponents have been more sophisticated than themselves in the action choice. Such a behavior could explain why there is a small treatment difference in the chosen actions and a difference in the number action choices being best responses to own stated beliefs between the treatments, despite the fact that belief statements are not different between treatments.

Of course it is also possible that the elicitation increases the relevance subjects attribute to their beliefs when picking their action. If that is the case, then the beliefs are unchanged by the elicitation and the different action choices are the result of subjects taking their beliefs more into account when beliefs are elicited. Such an effect would be closer to an experimenter demand effect by shifting the focus of subjects. Part of the differences in best responses between the treatments is also likely to be driven by the fact that subjects in the baseline treatment are not reminded about their previous action choice in the game when they state beliefs. This procedure prevents unwanted ex-post rationalizations by subjects in the baseline treatment, but for subjects who falsely remember choosing another action or subjects who were unsure which action to choose the procedure likely results in fewer best responses compared to a simultaneous elicitation of actions and beliefs. While the procedure is likely to increase the difference in best responses, I believe that the clear difference between the treatments is unlikely to be caused by it.

1.4 Discussion and conclusion

The results of this paper show that belief elicitation can have a significant effect on the chosen actions of individuals. The overall effects are rather weak and concentrated on specific subgroups of games. These findings are good news for experimentalists that elicit beliefs, but usually do not want that the elicitation affects subjects' choices. But the data also shows that asking for the beliefs of subjects together with their action choice increases equilibrium play in dominance-solvable games and reduces it for games with a symmetric alternative. These results demonstrate that game properties play an important role for the presence and the size of the effect. A possible channel for this finding is that subjects understand the decision situation better when they state beliefs.

A better understanding of the game means that a subject becomes aware of an aspect of the game that he or she did not realize without stating beliefs. Depending on the game increased understanding results in choices closer to the game theoretic predictions, in dominance-solvable games, or in less equilibrium play if a symmetric alternative to the equilibrium exists. These results support the so-called "thinking-harder" hypothesis, which claims that belief elicitation raises awareness of subjects for the situation or has subjects increase their effort to understand the game. While the experiment is not able to definitely distinguish between the "thinking-harder" hypothesis and the claim that belief elicitation leads to the formation of beliefs, some support for the hypothesis that the reported difference in actions for dominance-solvable games is not driven by subjects whose play is not belief based without the elicitation of beliefs comes from the analysis of the stated beliefs.

Analyzing the belief statements shows that beliefs are not different between treatments, but there is a clear difference in the number of best responses to own beliefs. The fact that in the belief treatment subjects respond more frequently optimally to their stated beliefs is not surprising and fits the story of "thinking harder". Investing more effort in understanding the game and especially the likely behavior of the opponent leads to more best responses. The setup of the experiment guides subjects to revise their beliefs in part two of the baseline treatment. Since they cannot change their action choices (made in part one), the chosen actions are less often best responses to the stated beliefs compared to the belief treatment. However, I believe that the difference in best responses found in this experiment is more likely close to an upper bound of the effect. Since subjects in the baseline treatment are not explicitly reminded of their previous action choice in the given game some part of the reduced number of best responses in the baseline treatment might be driven by subjects who simply do not remember their action choice(s).

Without specifying a complete model of game complexity, it seems as if belief elicitation is likely to have an effect in games that are "easy", meaning in games that are dominance-solvable and symmetric. The fact that the treatment effect can only be found in the subgroup of dominance-solvable and in the opposite direction for games with a symmetric alternative, hints that there is an effect of belief elicitation, but that it is too small to show up in more complicated games. The "harder thinking" subject therefore seems to be less of a "more strategic" thinking subject, but rather a subject who spends more effort in understanding the actual game. "Thinking harder" takes place on a lower level than expected and therefore does not always lead subjects to behave "more like a game theorist". This interpretation of the "thinking-harder" hypothesis is also supported by the finding that belief elicitation seems to affect subjects with different levels of strategic sophistication equally.

Given the mixed results other researchers report on the effect of belief elicitation, the "complexity" of the games might be a logical explanation. Assuming the "thinking-harder" hypothesis is true, it is very likely that belief elicitation does not play a major role in very easy games (subjects understand the game anyhow) nor in very complicated games (in which the effect is not strong enough). The fact that the treatment effect was significant for dominance-solvable games clearly points into this direction. In additional studies it would be interesting to test whether further game properties, besides dominance solvability and symmetric alternatives, also play a role and if the effect can be extended to other situations besides normal-form games. Also the question how to define game complexity and how it relates to the effect of belief elicitation is worth being investigated further.

Appendix A

Appendix Chapter 1

A.1 Appendix

A.1.1 Classification

In the table below subjects are classified based on their decisions in all 20 games. For details on the classification and an interpretation of the results see section 1.3.3.

Table A.1: Percentage of subjects classified by model predictions

	Clear cases		Ties		Clear cases and ties	
Model	Baseline	Belief	Baseline	Belief	Baseline	Belief
NE	10.00	9.68	2.50	1.61	12.50	11.29
L1	16.67	19.35	5.00	0.81	21.67	20.16
L2	15.00	11.29	3.33	3.23	18.33	14.52
L3	0.00	0.00	0.00	0.00	0.00	0.00
D1	5.00	8.06	1.67	0.81	6.67	8.87
D2	0.00	3.23	0.83	0.00	0.83	3.23
Ef	13.33	3.23	2.50	0.00	15.83	3.23
Maxmin	16.67	20.97	2.50	3.23	19.17	24.20
not identified	23.33	24.19	5.00	14.51	5.00	14.51

A.1.2 OLS and logit regression

This section contains additional estimation results for the analysis of section 1.3.3 on the influence of different game properties on the likelihood of equilibrium play.

Table A.2: OLS and logit estimation of NE

	(1) OLS		(2) Logit	
dep. variable: NE	Coeff. / Marg. Eff.	(Std. Err.)	Coeff. / Marg. Eff.	(Std. Err.)
belief treatment	0.03	(0.05)	0.05	(0.06)
Game properties				
dominance solvable	0.42***	(0.04)	0.44***	(0.04)
symmetric	0.41***	(0.05)	0.36***	(0.04)
NE dominated	−0.43***	(0.04)	−0.45***	(0.04)
symmetric alternative	−0.07*	(0.04)	−0.08*	(0.05)
Interaction effects				
dominance solvable & belief treatment	0.09*	(0.05)	0.16**	(0.07)
symmetric & belief treatment	0.05	(0.07)	0.10	(0.08)
NE dominated & belief treatment	−0.03	(0.05)	−0.12	(0.08)
symmetric alternative & belief treatment	−0.11**	(0.05)	−0.16**	(0.07)
Observations	2440		2440	
R^2	0.176			
Pseudo R^2			0.139	
Predicted probability	0.563		0.563	

Notes: Logit estimates: Marginal effects (evaluated at the mean of independent variables); * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All standard errors are clustered at the subject level.

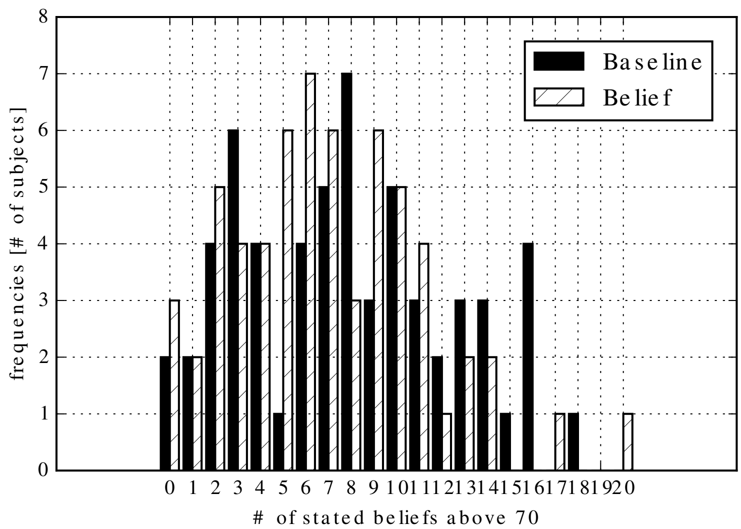

Figure A.1: Histogram of stated beliefs above 70%

A.1.3 Non-uniform beliefs

The histogram shows the number of individual belief statements above 70% per subject. It illustrates that most subjects state beliefs that are far away from a uniform distribution.

A.1.4 Instructions

Below are the English translations of the original German instructions for the belief treatment (available upon request). The instructions for the baseline treatment are almost identical, but split in two parts (games and belief statements). The emphases are also in the original instructions. The first part (WELCOME) was given to the participants at the start, while the second part (Detailed instructions) was handed out right before the start of the actual experiment.

WELCOME!

Thank you for participating in this experiment. The amount of money you earn depends on your choices and the choices of the other participants. The entire experiment will last about **90 minutes** and you receive your payment directly after the experiment in cash.

It is very important that you remain silent and do not talk to other participants during the entire experiment. In case you have any questions regarding the procedures of the experiment, please raise your hand. An experimenter will come to you to clarify the question directly with you. Thank you for your cooperation.

During the experiment you will be matched with other participants. The other participant will always be called "person B". In each round you will be matched with a new person. This means you will never be matched with the same person in two successive rounds. Neither during nor after the experiment you will learn with whom who you were matched in a round. You and person B will simultaneously see a decision situation. Your decision and the decision of person B together yield the result. This result determines how many points you and person B receive and therefore your payoff.

We now provide you with a detailed description of the decision situation. If you have any remaining questions after reading the instructions, please raise your hand. We will come to your seat and answer any open questions.

Detailed instructions for the decision situation

In this experiment we show you multiple decision situations. An example for such a decision situation can be seen in the table below. Please take a good look at the table and read the instructions below.

	Person B: §	Person B: #	Person B: +
You: &	40 10	40 40	10 10
You: %	50 20	40 40	0 20
You: =	20 30	20 50	40 10

In the actual decisions you will see a similar table on your computer screen (but with different numbers and possibly less cells). You are asked to make a decision, which means that you choose between the given alternatives (here: **&**, **%** and **=**). Person B decides simultaneously between their alternatives (here: **§**, **#** and **+**). You always choose between rows, while person B always chooses between columns of the table. The cell that results from these decisions states the points you and person B receive. Your potential payoffs are given in the bottom left of a cell, while the potential payoffs for person B are given in the upper right of each cell. Consider for example the following possibilities:

- You choose & and person B selects §, **you** receive 10 and person B receives 40 points.
- You choose % and person B selects §, **you** receive 20 and person B receives 50 points.
- You choose = and person B selects #, **you** receive 50 and person B receives 20 points.

You encounter multiple of those decision situations in the experiment. At the end of the experiment three decisions are randomly chosen and you receive the sum of points, which you earned in the three selected decisions. All earned points for these three decisions add up to your point total. **At the end of the experiment you receive 5 cents for each point.** This means 20 points are equivalent to 1€.

Furthermore we ask you about your expectation, which alternative person B will choose. This means for each table we ask you to think about the likely behavior of person B. You can interpret the question as follows: If 100 persons would make this decision, how often would each of the given alternatives be chosen?

Of course only one person will actually make the decision and not 100 different persons. But you can interpret the question about the behavior as how likely it is that a person B chooses each of the possible alternatives (here: **§**, **#** and **+**).

For example, if you are certain that person B will select alternative **§** in a decision situation and never alternatives **#** or **+**, then you would answer the question by filling in the numbers 100, 0 and 0 in the fields of the respective alternatives **§**, **#** and **+**.

If you instead believe that person B will not choose alternative **§** very often, but alternative **#** more often and action **+** most frequently, then you could fill in the numbers 20, 30 and 50 for the respective alternatives. If you assume that all alternatives are equally likely to be chosen you can enter the numbers 33, 34 and 33 for the respective fields.

Please note: The sum of all expectations has to sum up to 100.

Your payment depends on your chosen alternative, the chosen alternative of person B and which expectations you have stated about the behavior of person B.

At the end of the experiment three additional decision situations are randomly selected. You receive the sum of points you have earned for your expectations in these three situations. All points you earn in these decision situations are added to your point total.

For each decision situation only the result **OR** your expectations are paid. This means you are paid for six decision situations. In three decisions you are paid the result and in three different decisions for your expectations. You never receive points for both tasks for the same decision.

How many points you receive four your expectations depends on how correct your expectations are and therefore on the decision of person B. You receive more points if the difference between your expectations and the actual decision of person B is small. The table below shows you for the examples given above which payoff you might receive.

The first table lists four examples for stated expectations about the behavior of person B. For example in row 1 the following expectations are listed: You believe that person B will choose alternative § and not alternatives # or +.

	Person B: §	Person B: #	Person B: +
Example 1:	100	0	0
Example 2:	20	30	50
Example 3:	33	34	33
Example 4:	80	20	0

The next table states the possible payments for your stated expectations dependent on whether person B chooses alternative §, # or +. The first column lists your payoff if person B chooses alternative §, the second column lists your payoff if person B chooses alternative # and the third column lists you payoff if person B chooses alternative +.

	Person B: §	Person B: #	Person B: +
Payoff in example 1:	80	0	0
Payoff in example 2:	41	49	65
Payoff in example 3:	53	54	53
Payoff in example 4:	77	29	13

If you state expectations as given in row 1 and person B chooses alternative §, than you receive 80 points. If person B selects a different action, # or +, then you receive 0 points.

If you state expectations as given in row 3 and person B chooses alternative §, than you receive 53 points. If person B chooses alternative # you receive 54 points and if person B chooses alternative + you receive 53 points.

The table states for all four examples from above all possible payments depending on the decision of person B. **Each point is worth 5 cents**, independently if you receive it for your decision or your expectation. You can receive at most 80 points and you receive at least 0 points for an expectation. In the example this means when person B selects alternative § and you are paid for your expectation you receive 80 points (= 4 Euro) in the first example, 41 points (= 2.1 Euro) in the second and 53 points (= 2.7 Euros) in the third example. Your expected payment is maximized if you state your true expectations.

Please note: The numbers state above are only examples. These examples do NOT provide any suggestions for the decision situations.

Please make sure that you understood the table and that you know how you receive points for your stated expectations. Raise your hand if you have any questions. If you think that you understood these instructions and you know how you are paid in the decision situations, please click with the mouse on the OK button. We will then ask you some understanding questions to a different decision situation. After you have answered these questions correctly you can start with the experiment.

Chapter 2

Profitability of Tournaments with Worker Sorting: An Experiment[1]

2.1 Introduction

Offering a contract to potential employees serves two main purposes for employers. First, the incentive scheme included in the contract should attract the right workers. Second, once a worker has agreed to a contract, the incentive scheme should align the interests of the employer and the employee. In labor market relationships aligning the interests of employers and employees generally means incentivizing workers to produce a large output. Several papers have documented the importance of worker sorting for the observed output differences between payment schemes (Lazear 2000; Dohmen and Falk 2011; Leuven et al. 2011). However, there exists little evidence on the effect of worker sorting on the profitability of payment schemes for employers. Especially not much is known about the effect of worker sorting on profits when workers can choose between multiple variable-payment schemes.

In this paper I analyze the profitability of a rank-order tournament for employers when self-selection of workers in different payment schemes is possible in a laboratory experiment. The following questions are addressed: Can a rank-order tournament be more profitable than a piece rate? In particular I analyze the role of tournament prizes and the possibility of worker sorting for the profit of employers. Furthermore, how does the sorting decision of workers depend on their personal characteristics, such as risk aversion, productivity, gender or self-assessment of their productivity?

The real-effort experiment contrasts two different environments. In the first part, the

[1] I appreciate the comments and the advice received from Dirk Engelmann, Henrik Orzen, Stefan Penczynski and Gerhard Riener. I thank participants of the Verein für Socialpolitik meeting 2015 (Münster), the London Experimental Workshop 2015 (London), the European meeting of ESA 2014 (Prague), the IMEBESS meeting 2015 (Toulouse) and GAMES 2016 (Maastricht), as well as seminar participants in Mannheim and Nuremberg for many helpful comments. A predecessor of this paper was circulated under the title *"Performance Pay, Sorting and Employers' Choice: Are Tournaments an Attractive Payment Scheme?"*. All remaining errors are my own.

no-competition stage, employers - called managers - do not compete for workers. They are randomly assigned two workers and choose how these workers are paid by selecting one of the three available payment schemes: fixed wage, piece rate or rank-order tournament. Workers cannot influence which payment scheme they receive. Consequently, in the no-competition stage only the incentive effect of a payment scheme and its costs are relevant for its profitability. In the second part, the competition stage, three managers compete for six workers. Managers and workers state their preference ordering over all three payment schemes. The stated preferences of managers are used to assign each manager a payment scheme with the restriction that each payment scheme has to be offered. This restriction ensures that workers have the choice between all three payment schemes. Workers' preferences are then considered to sort two workers to a manager and therefore to a payment scheme. The matching procedure of workers to payment schemes gives priority to more productive workers to resemble advantages of these workers in the job search. As a result, the competition stage allows for workers' self-selection and thereby introduces competition among employers for productive workers. In the competition stage the profitability of a payment scheme depends on the incentives it provides to workers, its costs and on workers' sorting behavior.

The theoretical literature (Lazear and Rosen 1981; Green and Stokey 1983; Nalebuff and Stiglitz 1983) as well as experimental studies (Bull et al. 1987; Bartling et al. 2009; Eriksson et al. 2009; Balafoutas et al. 2012) have demonstrated that rank-order tournaments provide incentives for workers to exert effort. Therefore tournaments should be a profitable payment scheme for employers. However, in practice tournaments as payment schemes are uncommon. While they are used in the form of promotion tournaments and to award bonuses, they are rarely used as main payment scheme for employees. A main feature of rank-order tournaments is the dependence of workers' payments on their rank and not directly on their output. Therefore if prizes are independent of workers' output, tournament costs do not relate to workers' performance. On the one hand this independence can increase employers' profits, if the tournament results in large worker output while being relatively cheap. In such a case an employer might generate larger profits with a tournament than with output-based payment schemes, like a piece rate. But on the other hand such a tournament might be unattractive for (productive) workers, since the tournament will usually be cheap if prizes are low and therefore provide low expected earnings for workers. Additionally tournaments create uncertainty for workers about their wage, since their payment is neither a priori given nor fully determined by their own output. This wage uncertainty could make even tournaments with relatively large expected wages unattractive for risk-averse workers. Consequently when worker sorting is feasible it is unclear how profitable such a tournament is for the employer. My experiment addresses this issue. The comparison of the profitability of the tournaments in the no competition and the competition stage allows me to explicitly analyze the role of worker sorting.

Productivity differences have been shown to be important for worker sorting and therefore for performance differences between payment schemes. Lazear (2000) documents in his field experiment that the self-selection into a piece rate accounts for a large share of the observed performance increase compared to a fixed wage payment. Leuven et al. (2011) demonstrate in a field setting with different tournaments that an observed increase in performance, attributed to an incentive effect of larger tournament prizes, can mainly be explained by the sorting of subjects. To be able to account for workers' individual productivity in the sorting decision in the experiment first subjects' productivity in the real-effort task is measured. Dohmen and Falk (2011) show that for the sorting between a fixed wage and a variable wage alternative (either a piece rate, a tournament or a rent-sharing contract) not only workers' productivity matters. Variable-payment schemes additionally attract individuals with certain characteristics, such as higher willingness to take risks. Therefore in my experiment also workers' relative ranking beliefs and their risk preferences are elicited and I collect additional individual characteristics that might be relevant for the sorting behavior.

While many studies show the relevance of worker sorting for output differences workers usually do not choose between variable-payment schemes, but either between a fixed wage and a variable-payment scheme (Lazear 2000; Dohmen and Falk 2011) or between different tournaments (Leuven et al. 2011). Therefore my study differs from these as well as most other papers on worker sorting, since I explore the sorting behavior of subjects between multiple variable-payment schemes. Bartling et al. (2009), Eriksson et al. (2009), and Balafoutas et al. (2012) are concerned with the endogenous choice of subjects between a tournament and a piece rate. They demonstrate that productivity sorting is the main driver for subjects' decisions. But these studies do not address the effect of the sorting for the profitability of the different payment schemes. Also in these and in most other papers regarding worker sorting, all subjects are workers and their payment scheme choices are independent of other subjects' choices. Therefore there does not exist a competition for (productive) workers. In my experiment the number of workers in each payment scheme is limited. This design creates competition among employers for the most productive workers and allows to study the effect of worker sorting on the profitability of payment schemes in a competitive environment.

Furthermore, the absence of employers who benefit from the performance of workers might limit the effect of social preferences, especially in fixed-wage contracts. In particular in small firms with only a few employees social preferences might be an important factor for the performance of workers and consequently for the profitability of different payment schemes. That social preference affect the optimal incentive contract is shown by a large (experimental) literature that analyzes the effects of different fixed-wage offers on workers' performance. Fehr et al. (1993) were the first to demonstrate experimentally that large wage "gifts" of employers, in the form of unconditional offers, are indeed on average rewarded with higher effort choices by workers.[2] Therefore it pays for employers in these "gift-exchange" experiments to offer above minimum wages.[3] In my study social preferences might affect the profitability of the payment schemes also in an additional way. In the gift-exchange game employers do not select a contract type, instead they choose the fixed wage. Therefore employers control a parameter of the contract, the wage, but do not select the contract itself.[4] Having managers choose the payment schemes instead of exogenously assigning them in the no-competition stage allows for social preferences of workers to affect their performance. It is possible that the employers' choice of a specific payment scheme has an effect on the effort provision of workers, similar to the role of reciprocity in setting a profit-maximizing fixed wage. In the competition stage however, the employer might have to offer a payment scheme that is not her most favored payment scheme. By comparing workers' performance in the same payment scheme between these stages I can check whether this difference matters for their performance or not.

The results obtained illustrate the importance of worker sorting for the profitability of payment schemes. The tournament payment scheme provides incentives to workers. Their performances in both tournaments and the piece rate are high and do not significantly differ between these variable-payment schemes. Therefore in the no-competition stage - without worker sorting - a low-prize tournament results in larger manager profits than a piece rate due to the low tournament costs for employers. But a high-prize tournament yields the same manager profits as the piece rate. However, for productive workers a tournament is only an attractive payment scheme with sufficiently large prizes. As a consequence, in the competition stage the cost advantage of the low-prize tournament cannot outweigh that for productive workers this tournament is not attractive. Due to workers' self-sorting behavior the low-prize tournament does not yield larger manager profits than the piece rate. While the high-prize tournament is attractive for productive workers, the potential advantage from worker sorting is off-set by the high costs and as a result also a high-prize tournament does not result in larger manager profits than a piece rate in the competition stage. These findings illustrate that with worker sorting rank-order tournaments are hardly simultaneously attractive for workers and employers.

The paper is organized as follows. The next section describes the experiment, treatments and outlines the hypotheses. Section 2.3 presents the results. I first discuss managers' payment-scheme choices, then workers' performances and then compare the resulting prof-

[2] The idea was described theoretically before by Akerlof (1982) and Akerlof and Yellen (1990).

[3] This finding has been replicated in many different environments in the lab and in the field. As a starting point see Fehr et al. (1997, 1998), Fehr and Falk (1999), and Gneezy and List (2006).

[4] Similarly in the modified gift-exchange game of Falk and Kosfeld (2006) employers can restrict the minimum effort level of workers from below. In their setting employers clearly choose what to offer, but the choice is between different parameterizations of the same contract type. In Anderhub et al. (2002), a test of the principal agent model, subjects choose between different parameterizations of the same contract type, but not between different contracts. Lindner et al. (2013) experimentally investigate employers' and employees' choices between two equivalent tournaments. However, there is no choice between different payment schemes.

its before analyzing the sorting of workers. Lastly the fixed-wage contracts are analyzed. Section 2.4 discusses the implications of the results and concludes.

2.2 Experimental design

To address the outlined questions individual characteristics of subjects, such as productivity and risk preferences are needed. Especially data on productivity and preferences about payment schemes are typically not elicited in labor markets and personal characteristics, like risk preferences, are hard to measure. To collect this information and to exogenously change incentive structures without any disturbances, I conduct a laboratory study. While the laboratory provides an ideal environment to control possible outside influences, not all aspects of labor markets can be modeled. However, I believe that experiments provide a valuable tool in the study of incentives, self-selection and especially sorting, since sorting and incentive effects can be disentangled.

2.2.1 The real-effort task

The real-effort task subjects perform is to count ones in a ninety digits table of zeros and ones - see figure 2.1.[5] The probability of a one is randomly determined (between 0.3 and 0.7) and constant within a table, but varies between tables. The randomization prevents subjects from guessing the answer.

The task is easy to understand, but for most subjects it should not be enjoyable. It therefore resembles features of typical work tasks, which can be solved by workers, but are not enjoyable in itself. Furthermore, the number of solved tables is a natural productivity measure. The obtained data documents some heterogeneity in workers' productivity, which is a prerequisite for productivity sorting.

```
0100110001
0101000000
1101010011
1000100011
0001000101
1101110111
1111000010
0100000001
0100010011
```

Figure 2.1: Example of task

[5] A similar task is used by Abeler et al. (2011).

2.2.2 Structure of the experiment

The experiment consists of 10 steps, which are shown in table 2.1.[6] The productivity of subjects and their effort and rank beliefs are elicited in steps 1 to 4. For these steps all subjects are in the role of workers. Starting with step 5, the no-competition stage, subjects are randomly selected to be managers - one third - or workers - two thirds. All subjects remain in their roles from step 5 onwards. While in the no-competition stage managers choose a payment scheme, workers have no influence which payment scheme they receive. In step 7, the competition stage, the sorting decision of workers takes place. Workers state their preferences over the three available payment schemes and these preferences are used to sort workers into the different payment schemes. Steps 6 and 8-10 are additional questionnaires in which subjects' effort and rank beliefs as well as their risk preferences are elicited.

Table 2.1: Design of the experiment

Productivity measures and questionnaire I				**No competition**	Question-naire II	**Competition**	Question-naire III
Step 1	Step 2	Step 3	Step 4	**Step 5**	Step 6	**Step 7**	Steps 8-10
solve one table (unpaid)	solve one table (paid)	solve tables for 5 min (piece rate)	elicit effort & rank beliefs I	**managers select payment scheme:** **(a) fixed wage** **(b) piece rate** **(c) tournament** **working time 10 minutes**	elicit effort & rank beliefs II	**workers' sorting decisions:** **(a) fixed wage** **(b) piece rate** **(c) tournament** **working time 10 minutes**	elicit effort & rank beliefs III risk preferences demographics

Subjects are informed about the approximate duration of the experiment and existence of multiple steps, but only receive detailed on screen instructions in the beginning of each step.[7] Each session consists of 18 subjects, divided into two groups of nine. From step 5 onwards three subjects in each group become managers, while the remaining six subjects become workers.

Measuring subjects' productivity: steps 1 - 3

To learn the task subjects solve one table without incentives in step 1. In step 2 they solve a second table with payment. From a budget of 150 points two points are subtracted for

[6] Table B.1 in the appendix provides more details regarding the 10 steps.
[7] The appendix includes a translation of the instructions (see section B.2).

each second until the correct answer is given. For a wrong answer 20 points are subtracted and subjects can enter a new answer, but they cannot make losses. In step 2 all tables have a probability of 40% for a one in each digit. The time needed to solve the first and second table are productivity indicators I and II respectively. The main measure of productivity used in the following analysis comes from step 3. Subjects are asked to solve as many tables as possible within 5 minutes and are paid with a piece rate (8 points per correct answer and a penalty of 5 points for wrong answers). Productivity indicator III is the number of correctly solved tables.

Questionnaires and belief elicitation: steps 4, 6 and 8-10

I elicit subjects' beliefs and their subjective impressions about their stress level at several points during the experiment. Once after the productivity measurements - step 4 - and again after the no competition - step 6 - and the competition stage - step 8. To elicit their beliefs about their relative performance I ask subjects "How many of the other workers solved fewer tables then you?" (in a group of nine/six) and I ask managers "How many tables did your workers solve in this stage?". Workers' beliefs are rewarded if they predict their rank correctly, +/- 1. Managers receive a payment if they correctly estimate the number of solved tables, +/- 3 tables.

In steps 4, 6 and 8 I collect data on subjects' impressions "How hard did you work in the just completed stage?", "How difficult was the task?" and "How stressed did you feel?" on seven point Likert scales (No stress at all - Very stressful). In step 9 I measure subjects' risk preferences using a Holt and Laury (2002) lottery choice. In step 10 subjects state how fair they perceive the different payment schemes and fill in some demographic and employment information. All questionnaires are conducted before subjects receive feedback on their payments for the respective step. Payments for the belief elicitation are revealed at the end of the experiment.

No competition and competition stage: steps 5 and 7

In the no-competition stage (step 5) managers select their preferred payment scheme and thereby determine how the two workers in their firm are paid. While managers choose one of the three available payment schemes, workers are aware of the different payment schemes, but cannot influence to which manager and therefore payment scheme they are assigned. The three possible payment schemes are a tournament, a piece rate and a fixed wage.

In the tournament subject i competes with the other worker in the firm. Whoever solves more tables wins the tournament and receives prize T^w, while the other worker receives the lower prize T^l. Both workers receive a payoff to limit the downside risk for workers, a feature which is realistically present in labor markets. Prizes are independent of output and the only determinant for the tournament winner is the comparison between the two workers.

Ties are broken randomly. With x_i being the number of tables solved by a worker his income in a tournament is given by

$$w_i^T = \begin{cases} T^w, & \text{if } x_i > x_j \text{ and } i \neq j; \\ T^w \text{ with 0.5 probability and } T^l \text{with 0.5 probability}, & \text{if } x_i = x_j; \\ T^l, & \text{otherwise.} \end{cases} \quad (2.1)$$

Prizes vary by treatment: In the high-prize treatment the winner prize is $T^w = 450$ points and the loser receives $T^l = 50$ points. In the low-prize treatment these prizes reduce to $T^w = 400$ and $T^l = 25$ points. The 50 point difference between the winner prizes might not appear large, but it is substantial. For workers the potential winning prize is 12.5% larger in the high-prize treatment. Furthermore, for a worker these 50 points correspond to more than six additional solved tables with the piece rate (8 points per table). Given a mean of 29 tables solved with a piece rate in the no-competition stage, a difference of six tables is not small. In both treatments the winner prize is large, while the loser prize is relatively small. This design feature has two advantages: First, the high winner's prize incentivizes workers in a tournament to perform well, since there is a large prize to win. Second, the low loser prize limits the costs of the tournament.

In the piece rate a worker receives 8 points for a solved table, therefore his wage is given by:

$$w_i^{PR} = 8 * x_i. \quad (2.2)$$

A manager that selects a fixed wage also sets the wage w_i^F. The boundaries for the fixed wage are the tournament prizes. Therefore the fixed wage can be between 50 and 450 points in the high-prize treatment and between 25 and 400 points in the low-prize treatment.[8] A worker i receives this wage even if he does not solve a table. Both workers in a firm receive the same wage. The income of a worker with a fixed wage is

$$w_i^F = \text{fixed wage}. \quad (2.3)$$

In all payment schemes workers' wages are paid out of managers' earnings. A manager earns

$$w_i^M = \begin{cases} 14 * (x_i + x_j) - 2 * w^F + W, & \text{if she chooses a fixed wage;} \\ 14 * (x_i + x_j) - 8 * (x_i + x_j) + W, & \text{if she chooses a piece rate;} \\ 14 * (x_i + x_j) - T^w - T^l + W, & \text{if she chooses a tournament.} \end{cases} \quad (2.4)$$

[8] Managers typically stayed away from these boundaries, e.g. the upper limit was never chosen. Therefore the differences in the bounds between treatments should not have a large effect.

Each table her workers complete yields a manager 14 points, independently of the payment scheme. Managers can also solve tables to earn additional money, but their income mainly depends on their workers' performance.[9] A manager receives W depending on her own performance x_M:

$$W = \begin{cases} 0, & \text{if } x_M < 10; \\ 50, & \text{if } 20 > x_M \geq 10; \\ 100, & \text{if } 40 > x_M \geq 20; \\ 150, & \text{if } x_M \geq 40. \end{cases} \tag{2.5}$$

To ensure that participants do not work on the task out of boredom, they could answer trivia questions instead. Subjects could go back and forth between the task and the quiz. All participants are first informed about the payment schemes and the quiz. Then a random draw determines their role - worker or manager. Finally managers choose a payment scheme and in case of a fixed payment select the fixed wage.[10] Workers are informed about their payment scheme and then the 10 minute working time starts.

This no-competition stage yields managers' preferences for payments schemes without worker self-selection. But this stage does not deliver any information about workers' preferences. These preferences are elicited in the competition stage (step 7). In this stage worker sorting is feasible. At the beginning of the stage three managers and six workers are grouped together - subjects keep their respective roles. Managers first enter the fixed wage they would pay in case of a fixed-wage contract and then state their preference ordering of the three payment schemes. This procedure ensures that each manager could rank the payment schemes using the fixed wage she would like to pay. All subjects are informed that in a group each payment scheme has to be offered by exactly one manager.

The payment scheme a manager offers is determined with the following procedure: First, managers' first choices are considered. If a payment scheme is ranked first by only one manager, she receives this payment scheme. If two or more managers rank the same payment scheme first the tie is broken randomly. Next, the second choices of managers who did not receive their first choice are considered. Again any ties are broken randomly, such that if multiple managers made identical first and second choices one receives her third choice. This procedure ensures that managers had an incentive to state their preferred ordering over all three contracts.

While the procedure incentivizes managers to rank their preferred payment schemes higher, it can be optimal not to rank the most preferred payment scheme first. If a manager

[9] The option for managers to solve tables makes it harder for subjects in the lab to identify managers. It also provides some insurance against losses for managers.

[10] Managers are not allowed to choose the level of the piece rate or tournament prizes, to ensure a better comparability between groups and sessions as well as between treatments. Not being able to select the piece rate should make the piece rate less attractive for managers.

believes a particular payment scheme, e.g. the piece rate, to be popular among all managers and if she wants to avoid another payment scheme, assume the fixed wage, then it could be optimal to rank the tournament first and the piece rate second, to avoid the fixed wage.

However stating such a ranking is only preferred to revealing the true preferences under several conditions: First, a manager needs a smaller "preference difference" between her first and second option than between her second and third option. Second, she needs to believe that the other managers have the same preference order and third she must believe that other managers would not act strategically. Only if all three conditions are fulfilled ranking the second most preferred payment scheme first makes sense. Therefore in the results section the first choices of managers are used assuming that the revealed preferences represent true preferences.[11]

After managers are allocated to the contracts, the six workers in the group see the payment schemes (including the offered fixed wage) and state their preferences over these contracts. Their stated preferences are used to match exactly two workers to each manager (and therefore payment scheme). Workers are ranked within their group based on their performance in step 3 from 1st to 6th. Choices of better ranked subjects are fulfilled first, such that all workers with a rank of three or worse only get their first choice if less than two higher ranked workers select this payment scheme. Due to this procedure more productive subjects are more likely to receive their preferred payment scheme, which reflects the advantages of more productive workers in the job search. The mechanism therefore allows for productivity sorting of workers. The procedure is common knowledge and is equivalent to workers choosing their preferred payment scheme in the order of their ranking, just that choices are elicited using a strategy method. Subjects state a belief about their ranking in step 4 and therefore probably thought about their relative ranking. However, they did not receive feedback about their true ranking.[12]

2.2.3 Treatments and procedures

The two main treatments only differ in the tournament prizes. In the low-prize treatment the tournament winner receives 400 points and the loser earns 25 points. Essential this is a tournament with a prize of 375 points and a sure payoff of 25 points. In the high-prize treatment the prizes are 450 and 50 points respectively. Each subject faces the same tournament in the no competition and the competition stage. For a manager the cost of a

[11] In case preferences were not stated truthfully, I consider assumption three (not all managers are believed to behave strategically) to be the likeliest not to be fulfilled. If the first two conditions are fulfilled the likeliest bias is that the preferred payment scheme is stated as second choice with positive probability. Given the actual choices such a bias should result in the piece rate being ranked second, even if it is the most preferred payment scheme.

[12] Workers in a tournament receive some information about the other worker within the same firm by learning whether they won the tournament or not in the no-competition stage. Managers only know the total number of tables their workers solved.

tournament is the sum of prizes: 425 points in the low prize and 500 points in the high-prize treatment.

Additionally a control treatment with high tournament prizes was conducted, but the requirement that each manager has to offer a different payment scheme in the competition stage was lifted. Therefore all managers offered their preferred payment scheme. Since there are no incentives for stating any order for the not chosen payment schemes in this treatment only the first choice of managers are reported. The matching procedure for workers was identical to that of the other treatments. Since the treatment is identical to the high-prize treatment until step 6, the data from these treatments is analyzed jointly for steps 1-6. The results for the competition stage of the control treatment are not discussed or reported in this paper. Table 2.2 provides an overview over the treatments and the relevant payment scheme parameters.

Table 2.2: Treatment differences and payment scheme parameters

Treatment	Payment schemes parameters (no competition and competition stage)				Workers' choice set (competition stage)
	Tournament		Piece rate	Fixed wage	
High prize	winner prize T^w:	450 points	8 points	$w_i \in [50, 450]$	all payment
	loser prize T^l:	50 points	per table		schemes are offered
Low prize	winner prize T^w:	400 points	8 points	$w_i \in [25, 400]$	all payment
	loser prize T^l:	25 points	per table		schemes are offered
Control	winner prize T^w:	450 points	8 points	$w_i \in [50, 450]$	dependent on
	loser prize T^l:	50 points	per table		managers' choices

Notes: For the analysis of the no-competition stage the results of the high prize and the control treatment are pooled ("HIGH"). No results of the competition stage in the control treatment are reported in this paper.

The experiment was computerized (using zTree, Fischbacher (2007) and recruitment was done with ORSEE, Greiner (2015)), instructions were provided on screen and all interactions were anonymous. Any additional aids to count the ones in the table, like calculators etc., were not allowed.[13] The sessions were conducted at the mLab of the University of Mannheim (two high-prize sessions, two low-prize sessions and the three sessions of the control treatment) and in the AWI-laboratory of the University of Heidelberg (three sessions each in the low prize and high-prize treatment). Overall 234 subjects - 113 males and 121 females, mostly undergraduate students - participated. Each session consisted of 18 subjects - two groups of three managers and six workers. One point was exchanged for 1.3 Cents. On average subjects received 14.9€, including a 2€ show-up fee.

[13] Some subjects used their finger, the mouse courser or a pen to count. This behavior was not stopped by the experimenter.

2.2.4 Hypotheses

In order to derive some hypotheses for the behavior of subjects in the no competition and the competition stage a rough model is outlined below. Let x_i be the output of a worker i, meaning the number of tables he solves during a working stage. The output is given by the production function $x_i = g(\theta_i, e_i) + \varepsilon_i$ of the worker, where ε_i is an error term which is assumed to have a zero mean. This term captures aspects like "luck", e.g. getting an "easy" task, mood effects, etc. Workers are heterogeneous in their ability θ_i, which is assumed to be continuously distributed on some interval $[\underline{\theta}, \bar{\theta}]$. e_i is the effort exerted by the worker. Furthermore, $g_\theta(\theta, e) > 0$, $g_e(\theta, e) > 0$ and $g_{e\theta}(\theta, e) \geq 0$, which means higher ability and more effort yield a larger output and the marginal product of effort increases with ability. Assume the worker has a utility function that is separable in the wage w_i he receives and the effort costs $c(e_i)$: $u(w_i, e_i) = w_i - c(e_i)$. His utility depends positively on the wage $(u_w > 0)$ and negatively on effort $(u_e < 0)$, because exerting effort is painful for the worker $(c_e > 0)$.

How many tables x_i a worker solves therefore depends on the payment scheme, his ability (θ_i) and his exerted effort (e_i). With a fixed wage $\frac{\partial w^F}{\partial e} = 0$ and since $c_e > 0$, the expected utility of a worker is maximized with the lowest possible effort level $\underline{e}$. Independently of the paid wage w_i^F and their ability, all workers with a fixed wage will provide no effort and therefore not solve a table $x_i^F = 0$.[14]

A worker who is paid with a piece rate receives 8 points per table. As long as the marginal benefits from providing some effort outweigh his effort costs $\left(8g_e(\theta_i, \underline{e}) > \frac{\partial c(\underline{e})}{\partial e}\right)$ a worker will provide some effort, which means he will solve at least one table. The optimal effort level e^*_{PR} for an individual worker is given by $8g_e(\theta_i, e) = \frac{\partial c(e)}{\partial e}$. Given $g_{e\theta} \geq 0$ it is ensured that workers with higher ability exert more effort. Without specifying $g(\theta, e)$, $c(e)$ and a distribution of ability $F(\theta)$ no exact predictions regarding the effort levels can be derived. However, for all θ, it follows that $x_i^{PR} \geq x_i^F = 0$.

In a tournament the optimal effort of a risk-neutral worker also depends on his belief of the expected productivity of the other worker. As a result the optimal effort is a function of his productivity, the effort costs, the tournament prize and the probability of winning the tournament (which depends on the expected productivity of the other worker in the tournament).[15] Assuming that higher effort provisions increase the probability of winning the tournament, with $\frac{\partial E(w^T)}{\partial e} > 0$ it follows that $x_i^T > 0$ if $\frac{\partial E(w^T)}{\partial e} > \frac{\partial c(e)}{\partial e}$. Again, for all feasible θ it follows that $x_i^T \geq x_i^F = 0$.

Workers have no influence on their payment scheme in the no-competition stage, since they are randomly assigned to managers, who select the payment scheme. Given that a

[14] While there is no reason to provide effort with a fixed wage (according to standard theory), the results in the gift-exchange literature show that workers might positively respond to higher fixed wages. I analyze the effects of different fixed wages on workers' performance in section 2.3.7.

[15] While the optimal effort will most likely be a mixed strategy over some range of feasible efforts, since increasing his own effort (e_i) increases his output (x_i) and given the tie breaking rule zero effort provision is usually not an equilibrium.

worker maximizes his expected utility in a fixed-wage contract by not solving a single table for any feasible fixed wage, the manager makes negative profits with any positive wage. With a piece rate and a tournament at least workers with a sufficiently high θ should solve some tables. Since with a piece rate a manager makes a profit if at least one table is solved, a manager's expected profit from this contract should be positive. A manager should not offer a fixed-wage contract in the no-competition stage, because she can offer a piece rate, which yields a positive expected profit, instead. The same arguments are true for the competition stage. Therefore in the competition stage no manager should rank the fixed wage first.

Hypothesis 1. *No manager offers / wants to offer a fixed-wage contract in any stage. If a worker receives a fixed-wage contract he provides no effort.*

Workers are assumed to provide no effort with a fixed wage, but potentially provide some effort with the piece rate. Since managers make positive profits with a piece-rate contract if at least one table is solved, fixed-wage contracts should result in lower profits than a piece rate.

Hypothesis 2. *Manager profits with a fixed wage are smaller than with a piece rate.*

Whether a manager earns more with a tournament than with a fixed wage or the piece rate depends on the output of her workers. Assuming that managers rule out the fixed wage in the no-competition stage, since it does not result in positive profits, the question is whether a tournament or a piece rate yields a larger profit. Both contracts provide incentives for workers to exert effort, since with both payment schemes the expected payoff for a worker is larger with more solved tables. Whether a tournament is profitable for a manager also depends on the tournament costs, which differ between treatments. For the high-prize tournament workers need to solve 36 tables to ensure the manager a positive profit (tournament costs: 500 points, profit for each table: 14 points). In the low-prize treatment workers need to solve 31 tables for a zero profit of the manager. Similarly, workers have to solve more than 62 tables in the high-prize and more than 53 tables in the low-prize treatment for a manager to earn a larger profit with the tournament than with the piece rate. Using the performance of workers in the no-competition stage, I can analyze whether the cost difference in the tournaments affects the relative profitability of the tournament with respect to the piece rate.

In the competition stage workers rank all three payment schemes. Which payment scheme yields a worker a higher expected utility depends on his ability, the offered fixed wage and his belief about the productivity of the other worker in the tournament. A worker prefers the piece rate over the fixed-wage contract if his output exceeds the following threshold: $\hat{x} = \frac{1}{8}\left(w^F - c(\underline{e}) + c(e^*_{PR})\right)$. With common effort cost $c(e)$ workers with a higher ability θ need to provide less effort to reach this threshold. For a given wage all workers with a sufficiently high ability prefer the piece rate over the fixed wage. The threshold $\hat{x}$ increases with w^F.

Whether a worker prefers a tournament over the piece rate additionally depends on his belief about the productivity of the other worker $\left(\theta_j\right)$ in the tournament. A worker's expected utility from the tournament is $\left(T^w - T^l\right) \Pr\left\{g_i(\theta_i, e_T^*) + \varepsilon_i > g_j(\theta_j, e_T^*) + \varepsilon_j\right\} + T^l - c(e_T^*)$.[16]

This expected utility needs to exceed the utility from the piece rate $8\left(g(\theta_i, e_{PR}^*) + \varepsilon_i\right) - c(e_{PR}^*)$ for a worker to choose the tournament over the piece rate. While in this framework the decision between a piece rate and the fixed-wage contract boils down to productivity sorting, the choice between the tournament and the piece rate is likely to depend on the productivity of a worker (especially if the random component ε of the output has variance close to zero), but a worker's productivity is not the only determinant for the decision. However, it is possible that the probability of winning the tournament is increasing in the own ability θ_i. In this case there might exists a productivity threshold which determines whether a worker prefers the piece rate or the tournament. But since the expected utility of the tournament also depends on a worker's belief about the productivity of the other worker it is not clear whether such a productivity threshold exists.[17]

In the competition stage workers rank all three contracts. Because the fixed wage is only preferred over the piece rate by workers with a low productivity, workers' average productivity in the fixed wage should be lower than in the piece rate.

Hypothesis 3. *In the competition stage, the average productivity of workers preferring a fixed-wage contract is lower than the productivity of those workers who rank the piece rate first.*

With the outlined assumptions no prediction regarding workers' choice between the tournament and the fixed wage or the piece rate can be made. Whether the tournament is chosen by productive workers or not is therefore an open question that can be answered empirically by analyzing workers' payment scheme choices in the competition stage. I address this question as well as the role of the tournament prizes, workers' risk preferences and other individual characteristics for the choice between the payment schemes in the results section.

Managers also rank the payment schemes in the competition stage. Which payment scheme is preferred by a manager mainly depends on her belief about the expected output produced by the workers under the different payment schemes. Since in the competition stage productivity sorting of workers into the piece rate is possible and expected, I consider it likely that most managers will prefer the piece rate over the fixed wage. Whether managers prefer a tournament over the piece rate or the fixed wage and how the tournament prizes and

[16] $g_i(\cdot)$ and $g_j(\cdot)$ denote the output of workers i and j, given the individual ability and optimal effort choices.

[17] A worker chooses the tournament over the fixed wage if his expected utility of entering the tournament exceeds the utility from choosing the fixed wage $w^F - c(\underline{e})$. As for the comparison with the piece rate a worker's ability θ_i is likely a main determinant in this decision. However, since for the expected utility from the tournament also the expected probability of winning the tournament matters, the decision between the fixed wage and the tournament also depends on the belief of a worker about the productivity of the other worker in the tournament. Therefore there does not necessarily exist a productivity cut-off for the decision between the fixed wage and the tournament.

therefore the two treatments affect this decision is an open question I will consider in the results section.

Furthermore, this experiment addresses the question whether a tournament is more profitable in the absence of worker sorting. In the no-competition stage workers cannot avoid the payment scheme chosen by their manager. Therefore it is possible that incentives provided by a tournament result in relatively large manager profits and lead to larger manager profits than a piece-rate contract. However, whether that is the case cannot be answered definitely without further distributional assumptions. But even if a tournament yields larger manager profits than a piece rate in the no-competition stage, worker sorting in the competition stage could either reinforce or destroy the advantage of the tournament. On the one hand, the advantage of the tournament might increase with worker sorting if very productive workers prefer the tournament over the piece rate. On the other hand, the tournament profits might be lower than the profits with a piece rate, because productive workers choose the piece rate. The effect of worker sorting on the profitability of the tournament and the piece rate is one of the main research questions in this paper. It is addressed in section 2.3.3. Also whether a higher tournament prize leads to increasing profits with worker sorting can be tested by comparing manager profits with a tournament between the two treatments. In the following section I describe the results of the experiment.

2.3 Results

In section 2.3.1 I focus on the payment scheme choices of managers. Section 2.3.2 addresses the performance of workers with different payment schemes and in section 2.3.3 the profitability of the payment schemes is analyzed. Which payment schemes workers choose and how individual characteristics correlate with workers' choices is analyzed in sections 2.3.4 and 2.3.5. Section 2.3.6 highlights and shortly summarizes the main finding before section 2.3.7 examines the effect of different fixed wages on workers' performance.

2.3.1 Managers' choices

The payment scheme choices of mangers in both stages and all treatments are shown in table 2.3. In the no-competition stage of the high-prize treatment 8 managers chose a fixed-wage contract, 37 the piece rate and 3 selected the tournament. In the low-prize treatment all managers selected a variable-payment scheme: 25 choose the piece rate and 5 selected the tournament. Therefore the low-prize tournament was selected by 17%, while the high-prize tournament was chosen by 6% of managers. This difference is not significant (comparing the distributions of the variable-payment schemes in the two treatments using Fishers Exact Probability Test (FEPT), p-value = 0.28).[18] In all treatments the piece rate was the most

[18] All tests in this paper are two-sided unless indicated differently.

preferred payment scheme.

Table 2.3: Managers' payment scheme choices in the no competition and competition stage

Payment scheme	No-competition stage				Competition stage			
	HIGH		Low prize		High prize		Low prize	
fixed wage	8	(17%)	0	(0%)	3	(10%)	1	(2%)
piece rate	37	(77%)	25	(83%)	23	(77%)	24	(80%)
tournament	3	(6%)	5	(17%)	4	(13%)	5	(17%)
Total	48	(100%)	30	(100%)	30	(100%)	30	(100%)

Notes: In the no-competition stage the results of the high prize and the control treatment are pooled ("HIGH"). For the competition stage the first choices of managers are shown.

The piece rate is also the most preferred payment scheme in the competition stage. As the right part of table 2.3 shows, in both treatments more than three out of four managers stated the piece rate as their first choice. 4 managers in the high prize and 5 in the low-prize treatment wanted to offer a tournament, while even fewer managers preferred the fixed wage (3 in the high prize and 1 in the low-prize treatment). These two distributions are not significantly different between the treatments (FEPT p-value = 0.80).[19] The few choices of a fixed-wage contract are in line with the first part of hypothesis 1 which states that managers prefer variable-payment schemes to the fixed wage. In general the data shows that the piece rate is the most preferred contract in both treatments and independently whether worker sorting is feasible or not. Furthermore, the tournament costs seem not to have a big influence on managers' payment scheme choices.[20]

2.3.2 Workers' performances - the incentive effect

Workers random allocation to managers and therefore payment schemes in the no-competition stage allows me to analyze the incentive effect of the payment schemes. Figure 2.2 below displays the mean number of solved tables by workers for the different payment schemes. The left panel shows the averages for the no-competition stage and the right panel for the competition stage. There is no entry for the fixed wage in the no-competition stage in the low-prize treatment, since no manager selected a fixed-wage contract.

With a variable-payment scheme (piece rate and tournament) workers solved about 30 tables in both treatments. Workers with a fixed-wage contract in the high-prize treatments solved 12.4 tables. Using the number of tables solved by workers in each payment scheme

[19] Managers never had an incentive to rank one of their two most preferred payment schemes third. Managers' third choices support the finding of strong preferences for the piece rate: The piece rate was never ranked third, while the tournament was ranked third by 35 and the fixed wage by 25 managers (pooled over treatments).

[20] A multivariate regression of managers' choices with individual characteristics of managers (such as risk preferences) as regressors yields no significant results. Neither risk preference nor productivity beliefs have a significant influence on managers' preferred payment schemes.

to conduct Mann-Whitney U tests (MWU) yields that significantly more tables were solved with a variable-payment scheme (high prize-treatment: fixed wage vs. piece rate, p-value < 0.01 and fixed wage vs. tournament, p-value < 0.01).[21] The low average performance of workers with a fixed wage suggests that the average effort supplied was low, which confirms part two of hypothesis 1.

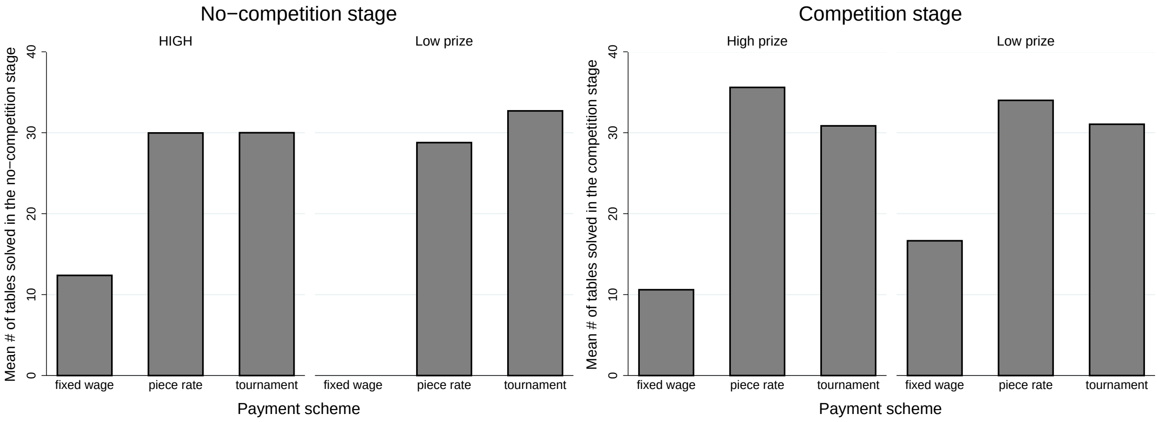

Figure 2.2: Mean number of correctly solved tables by workers with different payment schemes

The data also shows that workers' performance in the variable-payment schemes is not significantly different between payment schemes at any conventional level: piece rate vs. tournament in the high-prize treatment (MWU test, p-value = 0.78) and piece rate vs. tournament in the low-prize treatment (MWU test, p-value = 0.20). There is also no significant difference for each payment scheme between the treatments: piece rate in high prize vs. low-prize treatment (MWU test, p-value = 0.48) and high-prize tournament vs. low-prize tournament (MWU test, p-value = 0.83). These results indicate that the variable-payment schemes provide incentives for workers to exert effort. Furthermore, the equal output in both tournaments implies that the different in tournament prizes did not have a big effect on workers' effort. However, given the low number of observations in the tournaments this result might not be very robust.

While workers' average performance in the variable-payments schemes is not different, the differences in the variances appear large. For the no-competition stage however, only the variance for the fixed wage performance is significantly larger than the variance of the tournament in the high-prize treatment (F-test H_0 : same variance: p-value = 0.09).[22] I consider it plausible that the low number of observations in the fixed wage and tournament in the no-competition stage plays a big role for these results. Given the low number of

[21] "HIGH" pools the results of the high-prize treatment and the control treatment for the analysis of the no-competition stage.

[22] For the other comparisons the same test yields no significant difference: high-prize treatment, fixed wage vs. piece rate (p-value = 0.26) and piece rate vs. tournament (p-value = 0.18) and for the low-prize treatment: piece rate vs. tournament (p-value = 0.78).

subjects in these payment schemes even few outliers have a big influence on the observed results.

In the competition stage any differences in the performance of workers with different payment schemes cannot be completely attributed to an incentive effect, since the allocation of workers to payment schemes is most likely not random. While the results regarding worker sorting are discussed in detail in section 2.3.4, it is useful to note here that the two main results from the no-competition stage seem to be sustained also with worker sorting. First, workers' average performance is lower with a fixed-wage contract than in a variable-payment contract. Workers solve more tables with a variable-payment scheme in the high-prize treatment, fixed wage vs. piece rate (MWU test, p-value < 0.01) and fixed wage vs. tournament (MWU test, p-value < 0.01), and in the low-prize treatment, fixed wage vs. piece rate (MWU test, p-value < 0.01) and fixed wage vs. tournament (MWU test, p-value < 0.01). Second, there is also no significant difference in workers' performance for the variable-payment schemes between treatments: piece rate in high vs. low-prize treatment (MWU test, p-value = 0.41) and high-prize tournament vs. low-prize tournament (MWU test, p-value = 0.68).[23] These results confirm the findings from the no-competition stage and since with the applied matching procedure the number of workers in each payment scheme is identical in the competition stage (n=40 for all payment schemes) the caveat of the low number of observations for the fixed wage and tournament contracts in the no-competition stage does not apply. The heterogeneity in workers' performances in the different payment schemes in the competition stage is discussed in the section about worker self-selection.

2.3.3 Managers' profits and payoff-maximizing contracts

To compare managers' profits between payment schemes I compute the profit per contract. Figure 2.3 displays the mean of manager profits generated by workers, but excludes any earnings managers made due to their own performance or from their questionnaire answers (profit = # of solved tables * 14 points - wage payment). The left panel shows the results for the no-competition stage and the right panel shows the results for the competition stage.

In the no-competition stage of the high-prize treatment managers' profits with a variable-payment scheme are significantly larger than with a fixed-wage contract: fixed wage vs. piece rate (MWU test, p-value < 0.01) and fixed wage vs. tournament (MWU test, p-value = 0.04). Offering a fixed wage was therefore not the profit-maximizing choice of managers. This result is the consequence of the low average worker performance with a fixed wage reported above. Comparing profits from the variable-payment schemes in the high-prize treatment yields no significant difference: piece rate vs. tournament (MWU test, p-value = 0.78). The high-prize tournament and the piece rate are therefore on average equally prof-

[23] A comparison of the fixed-wage contracts between the treatments seems not useful, since the paid wages differ. For an analysis of the effect of fixed wages on workers' performances see section 2.3.7.

itable for managers. While there is no significant difference in the profits, with a tournament managers might make negative profits, which is impossible with a piece rate. Given that the high-prize tournament does not result in a larger profit, for a risk-averse manager the piece rate might be more attractive. Furthermore, while the differences in the variances of managers' profits for the high-prize tournament and the piece rate are not significantly different (F-test, p-value = 0.41), with the very low number of observations for the tournament this test result should be taken with caution.[24]

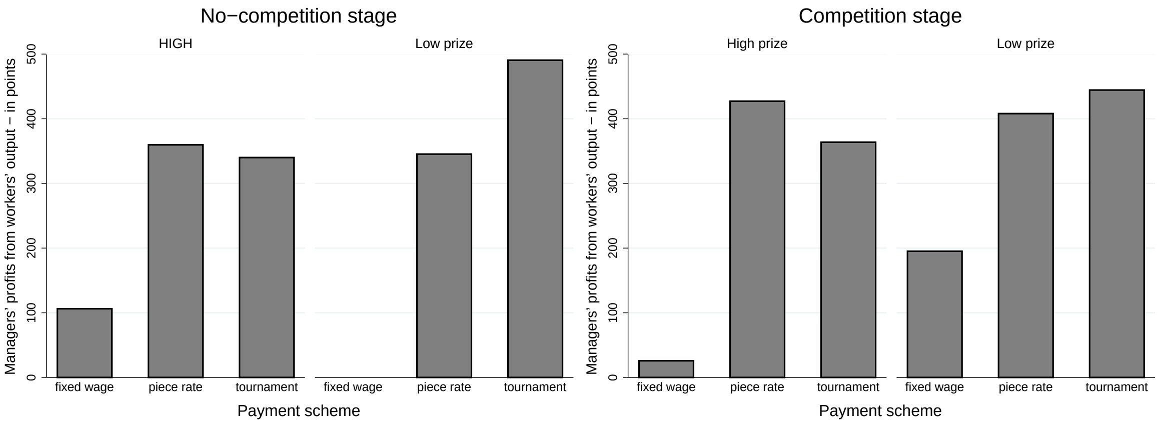

Figure 2.3: Mean profit of managers from workers' output with different payment schemes

In the low-prize treatment the average profit was larger with the tournament than with the piece rate (MWU test, p-value = 0.02). The possible profit increase for managers who selected the low-prize tournament is sizable. The piece rate yielded a mean profit of 345 points while the low-prize tournament resulted in a mean profit of 490 points - a difference of 145 points and an increase of more than 40%. This increase is driven by the lower tournament prizes, which reduce managers' costs. The variance in profits is not significantly different between the piece rate and the low-prize tournament (F-test, p-value = 0.17). The caveat of few observations for the tournament mentioned above for the variance test of the workers' performance also applies here. Additionally, by design the number of observations for managers' profits is only half the number of observations for workers.

While the low-prize tournament yields a larger manager profit in the no-competition stage, the difference between these payment schemes in the competition stage is not significant: piece rate vs. low-prize tournament (MWU test, p-value = 0.33). Comparing the profits from the piece rate and the high-prize tournament also results in a non-significant difference: piece rate vs. high-prize tournament (MWU test, p-value = 0.20). Furthermore, managers' profits for the same variable-payment scheme are not different across treatments in the competition stage: piece rate in high prize vs. low-prize treatment (MWU test, p-value = 0.94) and high-prize tournament vs. low-prize tournament (MWU test, p-value = 0.17).

[24] Give that the variance in tournament profits in the data is 7644 compared to 4586 for the piece rate, I consider it likely that with more observations the difference would not remain insignificant.

Managers earn on average the same profits with the piece rate and with a tournament in both treatments. For both tournaments the profit variances are larger than for the piece rate: piece rate vs. high-prize tournament (F-test, p-value = 0.03) and piece rate vs. low-prize tournament (F-test, p-value < 0.01).

Like in the no-competition stage the fixed-wage contract was on average not very profitable in the competition stage. In both treatments the fixed-wage contract resulted in the lowest manager profits.[25] The right part of figure 2.3 also suggests a difference in the profitability of fixed-wage contracts between the treatments. But since the paid wages can differ substantially between contracts and therefore also between treatments, a more detailed analysis is required before such a statement can be made. I analyze how the offered fixed wages affect workers' performance and address potential treatment differences in detail in section 2.3.7.

There are two main takeaways for the profitability of the payment schemes. First, managers' profits are larger with a variable-payment scheme in both treatments and stages. These findings are in line with hypothesis 2 and additionally the data shows that a tournament results in larger average profits than a fixed-wage contract. Second, only in the no-competition stage and with low prizes a tournament is more profitable than the piece rate. Therefore the rank-order tournament outperforms the piece-rate contract in this experiment if no worker sorting is feasible. However, the next section shows that the low-prize tournament loses its advantage in the competition stage due to the productivity sorting of workers.

2.3.4 Sorting of workers

After learning the fixed wage offered in the competition stage workers ranked managers' offers knowing that a higher rank for a contract increased the probability of receiving this contract. The mechanism creates an incentive for workers to rank the payment schemes according to their preferences. Since priority is given to more productive workers, very productive workers are basically guaranteed to receive their first choice. Consequently, they have no incentive for stating their true preferences for their second and third choice. But they can base their behavior only on their believed ranking, since they do not receive any feedback on their relative performance before the end of the experiment.[26]

Furthermore, an extensive literature in psychology and economics documents that most people are overconfident about their own performance and have too optimistic beliefs regarding their own relative ranking. Camerer and Lovallo (1999) were among the first to demonstrate that overconfidence can result in above equilibrium entry rates into competi-

[25] For the high-prize treatment: fixed wage vs. piece rate (MWU test, p-value < 0.01) and fixed wage vs. tournament MWU test, p-value < 0.01). For the low-prize treatment: fixed wage vs. piece rate (MWU test, p-value = 0.08) and fixed wage vs. tournament (MWU test, p-value = 0.03).

[26] The only workers who receive limited feedback on their relative ranking are workers who are paid with a tournament in the no-competition stage. By learning whether they won the tournament these workers know whether they solved at least as many tables as one other worker in their group or not.

tions, especially if the number of entrants should be low in equilibrium. As a consequence of excess entry profits in competitions can be below equilibrium predictions.[27] If subjects in my experiment are overoptimistic regarding their performance they might overestimate the probability of receiving their first contract choice. If that is the case their second and third contract choices might not correspond to their true preferences. Also workers might misjudge their probability of winning the tournament and enter the tournament too often. Since the following analysis focuses on workers' first choices and the incentives for stating the most preferred contract first are not biased by overconfidence, I assume that workers' first choices are their most preferred contracts. Whether workers in the experiment are overconfident regarding their own performance and how their beliefs affect their stated preferences for payment schemes is addressed later in this section.

In the left panel of table 2.4 workers' first choices split for both treatments are shown. In both treatments the piece rate was ranked first by at least 50% of workers. The tournament was chosen by more workers in the high-prize treatment (18 in high prize vs. 9 in low-prize treatment), but the distributions of workers' first choices for the two treatments are not significantly different (FEPT, p-value = 0.15). The fixed-wage contract was selected by 20% (high prize) respectively 27% of workers (low-prize treatment). The percentages should not be directly compared, since the offered wages were different.

For the piece rate the wage of a worker only depends on the number of solved tables and in the tournament his wage depends on whether he outperformed the other worker in the same firm. A worker's ability to solve tables is therefore the most important determinant for his expected wage. Consequently it is likely that workers with different productivities prefer different contracts. As a first indicator whether workers' choices are reflective of self-sorting by productivity consider the right panel of table 2.4. Using productivity indicator III - the number of tables solved with a piece rate in step 3 - workers' average productivity is reported split for their preferred payment schemes.[28]

In the high-prize treatment workers selecting a fixed wage have the lowest productivity: workers' productivity with first choice fixed wage vs. piece rate (MWU test, p-value < 0.01) and fixed wage vs. tournament (MWU test, p-value = 0.07). The difference for workers with a preference for the piece rate or the tournament is not significantly different in either treatment: high prize (MWU test, p-value = 0.23) and low-prize treatment (MWU test, p-value = 0.13). In the low-prize treatment workers choosing the fixed wage are significantly less productive than those who selected the tournament (MWU test, p-value = 0.04), but

[27] For a summary of studies regarding endogenous entry into competitions see Camerer (2003). Examples of experimental studies that find excess entry, at least under some conditions, are Cason et al. (2010) and Morgan et al. (2012).

[28] There is a high degree of correlation between all three productivity indicators and therefore most results are unchanged if productivity indicator I or II is used instead. Table B.2 in the appendix lists the Spearman rank correlations between the three indicators and between the indicators and workers' performance in the two working stages.

Table 2.4: First choices of workers and their average productivity given their first choices

	Workers' first choices				Mean productivity (# of tables solved in step 3)		
	Treatment				Treatment		
Payment scheme	High prize		Low prize		High prize	Low prize	Pooled
fixed wage	12	(20%)	16	(27%)	11.9	12.1	12.0
piece rate	30	(50%)	35	(58%)	14.8	13.9	14.3
tournament	18	(30%)	9	(15%)	14.1	15.4	14.5
Total	60	(100%)	60	(100%)	14.0	13.7	13.8

not compared to those who wanted the piece rate (MWU test, p-value = 0.11). Pooling the data for both treatments results in significant differences between workers choosing a fixed wage and those selecting a variable-payment scheme: fixed wage vs. piece rate (MWU test, p-value < 0.01) and fixed wage vs. tournament (MWU test, p-value < 0.01). There is no significant difference between workers choosing a variable-payment scheme: piece rate vs. tournament (MWU test, p-value = 0.95).[29] These findings support hypothesis 3: The comparisons of workers' productivity deliver clear indications for productivity sorting of workers. More productive workers avoid the fixed wage and choose the piece rate or the tournament. However, there is no clear pattern of productivity sorting of these more productive workers between the two variable-payment schemes in either treatment.

In table 2.4 only workers' average productivity is reported. To analyze the productivity sorting for different productivity levels, figure 2.4 shows for all payment schemes the fraction of subjects who preferred a payment scheme by productivity clusters. Dependent on productivity indicator III each worker is in one of these four clusters: less than 10, 11 to 14, 15 to 18, or more than 18 tables solved.[30] The figure shows that more productive subjects in both treatments are less likely to choose a fixed wage (upper left graph): While in the least productive first group 75% of workers in the high-prize treatment (44% in the low-prize treatment) selected a fixed wage, the proportion declines to 21% in the second group (28% in the low-prize treatment), to 15% for the third group (20% in the low-prize treatment) and in the highest productivity group no worker chose a fixed wage (17% in the low-prize treatment).

The data shows an opposite pattern for selecting a variable-payment scheme (lower right graph): In the high-prize treatment 25% of workers in the first group, 79% in the second, 85% in the third and 100% in the fourth group state the piece rate or a tournament as their

[29] Comparing the differences in productivity between treatments for the same payment scheme yields an insignificant difference for fixed wage (MWU test, p-value = 0.98), the piece rate (MWU test, p-value = 0.15) and the tournament (MWU test, p-value = 0.20).

[30] Pooling the data over the two treatments: 11% of workers are in the first group (< 10 tables), 44% in the second group (11-14 tables), 33% in the third group (15-18 tables) and 12% in the most productive fourth group (> 18 tables). The distributions of workers using these clusters are not different between the two treatments (FEPT, p-value = 0.52).

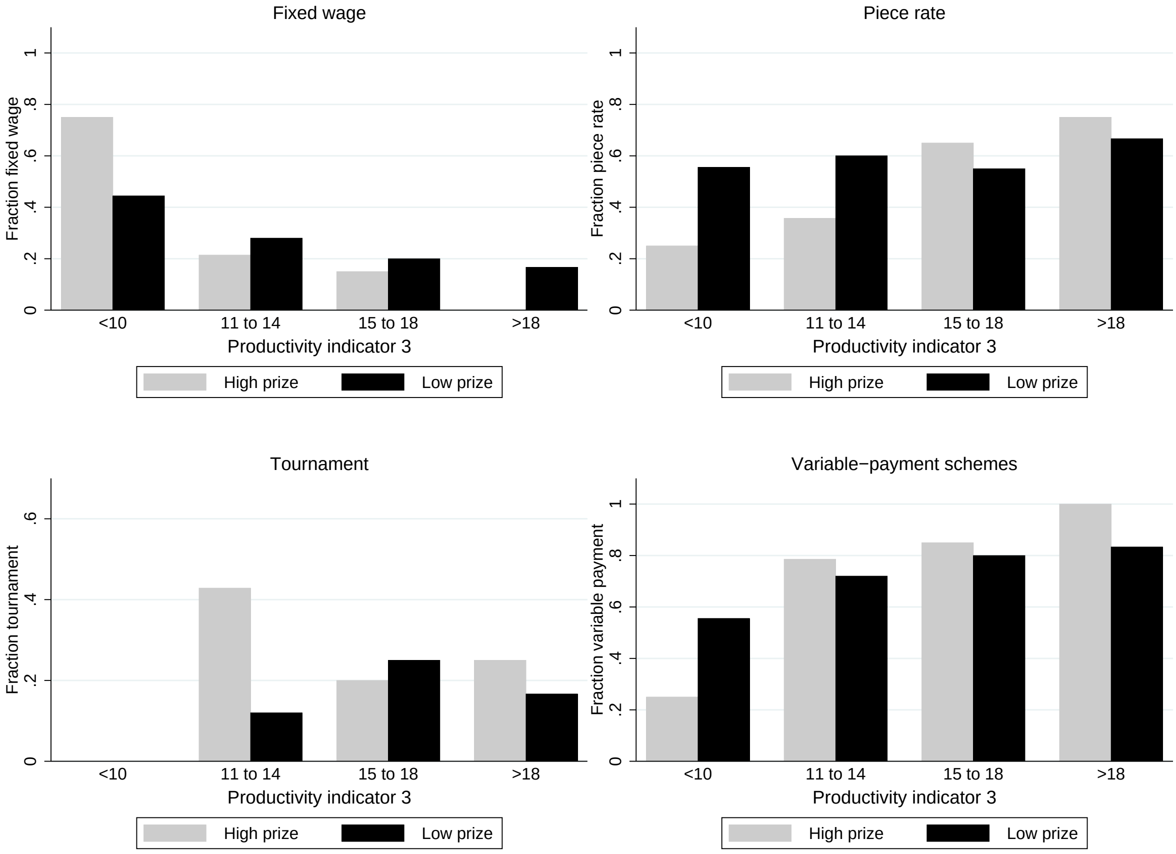

Figure 2.4: Fraction of workers opting for the different payment schemes by productivity

preferred payment scheme. In the low-prize treatment the proportion of workers who want a variable-payment scheme is 56% in the first, 72% in the second, 80% in the third and 83% in the fourth group. The data is less clear when looking at the two variable-payment schemes separately. While the proportion of workers preferring the piece rate (upper right graph) increases over the four groups in the high-prize treatment, the proportion is relatively stable for the low-prize treatment. The data for the tournament (lower left graph) does not show a clear connection between the four productivity groups for either treatment. But interestingly no worker of the lowest productivity group (< 10 tables) preferred the tournament.

Overall figure 2.4 presents clear evidence of productivity sorting in the data. The more productive a worker is the likelier it is that he chooses one of the variable-payment schemes. Also this more detailed analysis of productivity sorting does not reveal many insights regarding workers' sorting between the piece rate and the tournaments. It seems as if workers with a very low productivity to not want to enter the tournament, however given that a worker does not belong to the very unproductive workers (at least 10 tables solved) his productivity does not influence the probability of entering the tournament. Interestingly, the data indicates that even a larger tournament prize does not influence this sorting pattern.

Dohmen and Falk (2011) find a similar productivity sorting of workers. They further describe a significant influence of other individual characteristics, e.g. the willingness to take risks and gender, on the sorting. In order to determine the influence of worker characteristics in the sorting decision I use two probit regressions with worker characteristics and contract parameters. The results are shown in table 2.5, which reports marginal effects at the means for all continuous regressors and marginal effects for a change of dummy variables from 0 to 1, with all other variables being set to their respective means. In column 1 the dependent variable is 1 if a worker's first choice was a variable-payment scheme and in column 2 the dependent variable is 1 if the worker selected a tournament. While the regression reported in column 1 considers all workers,[31] in the regression in column 2 only workers who selected a variable-payment scheme (piece rate or tournament) are taken into account.

The following explanatory variables for workers' choices are included in the regressions: *Productivity* is the number of tables solved during step 3 (productivity indicator III), *1 if male* is a dummy which is one for males, *Relative self-assessment* is the belief of a worker how many workers solved less tables then himself in step 3 (higher numbers mean better relative believed ranking), *1 if rank = 1 or 2* is a dummy that is one if the worker was one of the two most productive workers in his group and *Risk attitude* is a measure of risk aversion (number of safe lottery choices in step 9; more risk-averse subjects make more safe choices). Furthermore, *Fixed wage* is the offered fixed wage (rescaled by dividing the wage in points by 10 to increase the visibility of a fixed wage increase) and *Low-prize treatment* is a dummy variable which is one for the low-prize treatment.[32] The regression results in column 1 illustrate again that a worker's productivity matters for his payment scheme choice. The coefficient for *Productivity* is positive and significant at the 1%-level. Its value implies that solving one additional table in step 3 increases the probability to select a variable-payment scheme in the competition stage by 6%-points. The significantly negative coefficient on the level of the fixed wage shows that a variable-payment scheme is less attractive with a larger fixed wage alternative. The fixed-wage coefficient implies that a 10 point larger fixed wage reduces the likelihood of a worker to choose a variable-payment scheme by 2%-points.

The coefficients for the gender, the belief about the own relative ranking, being one of the most productive workers or the risk attitude of a worker are all not significantly different from zero. The same is true for the level of the tournament prizes, since the dummy for the treatment is not significantly different from zero. This result implies that the tournament prizes do not affect the decision of workers to choose a variable-payment scheme.

Column 2 of table 2.5 shows the influence of the explanatory variables for the choice between the piece rate and the tournament. The productivity has no significant effect for the choice between these payment schemes. Furthermore, while the subjective belief about the own productivity does not matter for the expected payoff in a piece rate, this belief affects

[31] In total 120 workers, 60 in each treatment, participated in the experiment. Four observations from subjects with multiple switching points in the lottery are deleted, since it is not clear how to interpret their lottery choices.

[32] The standard errors are not clustered at the session or group level, since the only difference between the groups was the fixed wage, which is included in the regression as an explanatory variable.

Table 2.5: Probit estimations of workers' first choice

	(1)		(2)	
Dependent variable:	1 if variable payment		1 if tournament	
	Marg. Eff.	(SE)	Marg. Eff.	(SE)
Ind. characteristics				
Productivity	0.06***	(0.02)	0.01	(0.02)
1 if male (d)	−0.03	(0.07)	0.17*	(0.10)
Relative self-assessment	−0.02	(0.02)	0.06	(0.04)
1 if rank = 1 or 2 (d)	−0.07	(0.12)	−0.14	(0.13)
Risk attitude	0.02	(0.02)	0.01	(0.03)
Contract parameters				
Fixed wage	−0.02***	(0.00)	0.01**	(0.01)
Low-prize treatment (d)	−0.04	(0.07)	−0.12	(0.10)
Observations	116		88	
Predicted pro.	0.868		0.293	
Pseudo R^2	0.343		0.126	

Notes: Probit estimates. Marginal effects (evaluated at the mean of independent variables); * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Subjects with multiple switching points in the risk elicitation are excluded. (d) indicates a discrete change for dummy variables from 0 to 1. The larger the value of the self-assessment variable is, the more productive a subjects thinks he is relative to others. The dummy variable *1 if rank = 1 or 2* means that the subject is one of the two most productive workers of the group in step 3. In column 2 only workers who selected a variable-payment scheme (piece rate or tournament) are considered.

the expected payoff in a tournament. I would therefore assume that a worker's belief about his relative ranking affects his decision to choose a tournament. But the coefficient on the relative self-assessment shows that subjects with a more positive self-assessment are not more likely to choose a tournament. Also risk preferences do not have a significant effect. The significantly positive gender dummy (10% significance level) however implies that male workers are 17%-points more likely to choose the tournament compared to otherwise equal female workers. This effect is consistent with the findings of Niederle and Vesterlund (2007) that females are less likely to enter competitive environments.

The insignificant treatment dummy shows that larger tournament prizes do not affect the choice between the piece rate and the tournament. Also an interaction effect between the treatment and the individual productivity of a worker, to control for heterogeneous effects of the treatment for workers with different productivity levels, does not result in a significant effect. The same is true for replacing the relative self-assessment variable with a dummy for overconfident workers, meaning workers who overestimated their own relative ranking, or very overconfident workers, overestimating their own rank by two or more ranks (regressions are not shown). While there is no significant effect of the tournament prizes, the coefficient on the level of the fixed wage is significantly positive. The coefficient implies that a 10 point larger fixed wage increases the likelihood of choosing the tournament by 1%-point. Given that the level of the fixed wage should not influence the trade-off between the piece rate and the tournament, this effect is surprising to me.

To summarize the analyses regarding workers' sorting there are three main results: First, in both treatments most workers prefer the piece rate. Second, there is self-sorting of workers based on their individual productivity. A more productive worker is more likely to prefer a variable-payment scheme. Third, while more productive workers choose variable-payment schemes, there is no clear productivity sorting in the tournament or the piece rate. Productivity differences do not explain which of the two variable-payment schemes a worker prefers. However, this lack of productivity sorting into the tournaments leads to the previous finding that neither tournament yields larger profits than the piece rate with worker sorting. While the low-prize tournament is cheap it is not attractive enough for workers. Since not enough productive workers want this payment scheme even the low costs are not sufficient to generate a larger manager profit than the piece rate. The high-prize tournament is also not attractive enough for very productive workers. Therefore managers do not earn more with the high-prize treatment than with the piece rate. Whether the individual worker choices were payoff maximizing is analyzed in the next section.

2.3.5 Workers' payoff-maximizing choices

While workers' average earnings can be compared between payment schemes, the more interesting question is whether an individual worker selected the payoff-maximizing payment scheme.[33] To determine the payoff-maximizing choice of a worker his ability has to be taken into account.[34] The following analysis compares workers' payoffs with a piece rate in the competition stage and the hypothetical payoffs of these workers in a tournament. To calculate the hypothetical tournament payoff I make the following assumptions: First, a worker's performance in the piece rate is assumed to be his performance in a tournament.[35] Second, the probability of winning the tournament (p_{win}) is determined by a worker's rank among all other workers in the tournament. His winning probability is one if he solved more tables than all other workers and it is 50% if half of the other workers solved more tables than him.[36] A

[33] Comparing workers' average earnings between payment schemes yields a significant difference for the fixed wage vs. piece rate (MWU test, p-value < 0.01) and fixed wage vs. tournament (t-test, p-value = 0.03) in the high-prize treatment. In the low-prize treatment only the comparison of the fixed wage vs. piece rate (MWU test, p-value < 0.01) is significant, while the difference between a fixed wage and the tournament is not significant (t-test, p-value = 0.13). The difference in earnings between the piece rate and the tournament is not significant in either treatment: high prize, t-test, p-value = 0.47 and low-prize treatment, t-test, p-value = 0.21. T-tests for unequal variances are used for the comparisons including a tournament, since rank based tests seem inappropriate for comparing workers' earnings if 50% of the tournament workers earned much more (tournament winners) and 50% earned much less (tournament losers) than most workers with a piece rate.

[34] Payoff maximization does not necessarily correspond to utility maximization, since in the analysis e.g. effort costs or social preferences are not considered.

[35] Given that workers' performances in the piece rate and the tournaments are not significantly different in the no-competition stage I consider this assumption reasonable.

[36] Workers' performances in both tournaments are pooled to get a finer grid on the winning probability. I consider this pooling to be justified, since the data shows no significant differences between workers' performances in both tournaments.

worker's hypothetical payoff in a tournament is $p_{\text{win}} * T^w + (1 - p_{\text{win}}) * T^l$, with T^w and T^l being the respective tournament prizes.

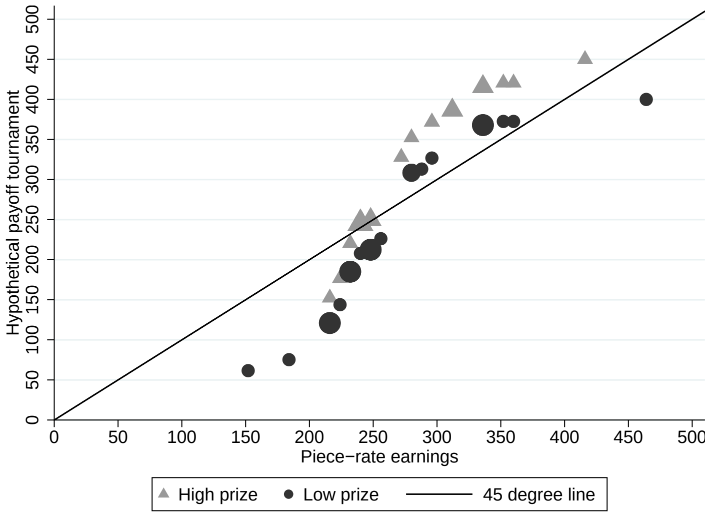

Figure 2.5: Hypothetical tournament payoff vs. payoff with piece rate for workers paid with a piece rate in the competition stage

For each worker paid with the piece rate figure 2.5 displays the piece-rate earnings on the x-axis ("Piece-rate earnings") and the calculated hypothetical tournament payoffs on the y-axis. Triangles in the graph represent workers in the high-prize treatment and dots subjects in the low-prize treatment. Larger symbols indicate multiple workers, since the calculation treats workers with the same number of solved tables identically. The upward sloping line is the 45-degree line. Workers who are below this line earned more with the piece rate than their hypothetical tournament payoffs. Subjects who are above the 45-degree line would have earned more in the tournament.

Workers who solved few tables are in the bottom left of the graph. These workers earned more with the piece rate. Most of these subjects are in the low-prize treatment. Workers who are in the middle to upper right of the graph could have earned more in the tournament. The difference in the tournament prizes between the treatments can be seen in the vertical difference of workers in this group. Many subjects are above the line, but while workers in the low-prize treatment (dots) are only marginally above the 45-degree line, workers in the high-prize treatment (triangles) are usually much further above the line. Their "lost earnings" in the piece rate compared to the tournament are larger.

This effect of the different tournament prizes is illustrated in the left part of table 2.6. For all 20 workers in each treatment who were paid with the piece rate the table shows whether their payoff in the piece rate was larger or smaller than their hypothetical payoff from the tournament. In the high-prize treatment for 18 workers the hypothetical tournament payoff is larger than the piece rate earnings, while this is only the case for 9 subjects in the low-prize treatment (FEPT p-value < 0.01). With low tournament prizes the hypothetical tournament earnings are not large enough to result in an improvement over the piece rate for most subjects. Workers in the high-prize treatment often could have increased their expected earnings in the tournament.

Table 2.6: Payoff-maximizing payment schemes for workers paid with a variable-payment scheme

Larger (hypothetical) earnings with	Workers with piece rate Treatment				Workers with a tournament Treatment			
	High prize		Low prize		High prize		Low prize	
tournament	18	(90%)	9	(45%)	12	(60%)	9	(45%)
piece rate	2	(10%)	11	(55%)	8	(40%)	11	(55%)
Total	20	(100%)	20	(100%)	20	(100%)	20	(100%)

Notes: All 80 workers who were paid with a variable-payment scheme in the competition stage are included. Tournament earnings are hypothetical earnings, calculated as described above. Piece-rate earnings are the actual payoff for workers being paid with a piece rate and hypothetical earnings for workers paid with a tournament.

The results of the identical analysis for workers in a tournament is displayed in figure 2.6 and in the right part of table 2.6. The graph includes all 40 workers paid with a tournament in the competition stage. The y-axis shows their hypothetical tournament payoffs - calculated as described above - and the x-axis displays the hypothetical piece-rate earnings - 8 points times the number of solved table for each subject. The line is the 45-degree line. Filled symbols (triangles for the high prize and dots for the low-prize treatment) indicate that the subject won the tournament, while hollow symbols represent tournament losers.

As for the workers who were paid with the piece rate, there are two groups: Workers above the 45-degree line had a higher hypothetical payoff in the tournament and workers below the line had a higher hypothetical payoff with the piece rate. The data shows that for about half of the workers the tournament was the payoff-maximizing payment scheme. Furthermore, there is no visible difference between the treatments. While in figure 2.5 above many workers in the high-prize treatment were above the line, in figure 2.6 for both treatments there are roughly as many symbols above the 45-degree line as below. The right part of table 2.6 shows that for 8 workers in the high prize and 11 workers in the low-prize treatment, the piece rate would have resulted in a higher hypothetical payoff. These distributions are not significantly different (FEPT, p-value = 0.53).

The analysis so far showed that especially workers paid with a piece rate in the high-prize treatment could have increased their hypothetical payoff by being paid with a tournament. However, to answer the question whether workers chose their payoff-maximizing payment scheme also their choices have to be considered. Of the 20 workers being paid with a piece rate in the high-prize treatment 17 stated the piece rate as their first choice. For 15 of these 17 workers the hypothetical tournament payoff was larger than the payoff with the piece rate. In the low-prize treatment also 17 of the 20 workers paid with the piece rate

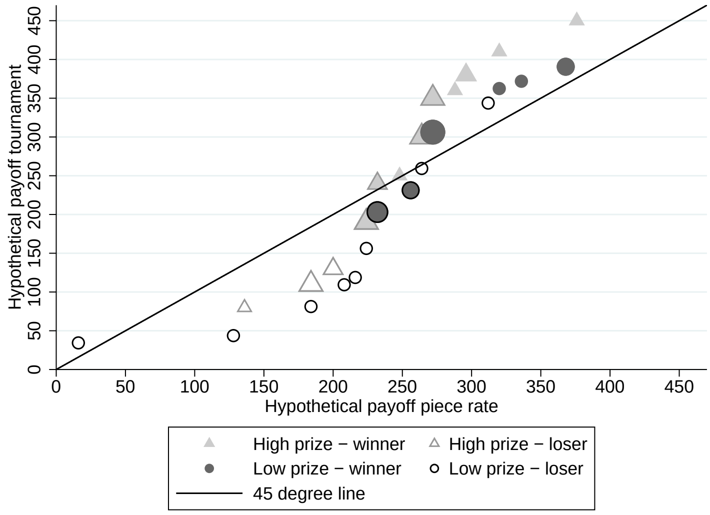

Figure 2.6: Hypothetical tournament payoff vs. hypothetical payoff with piece rate for workers paid with a tournament in the competition stage

wanted the piece rate. Of these 17 workers only 6 had a larger hypothetical tournament payoff. These distributions are significantly different (FEPT, p-value < 0.01). Of the workers being paid with the tournament only 13 in the high prize and 8 in the low-prize treatment stated the tournament as their preferred payment scheme. For 9 of these workers in the high prize and 4 workers in the low-prize treatment the tournament was the payoff-maximizing payment scheme. These distributions are not significantly different between the two treatments (FEPT, p-value = 0.65).

These findings show that the difference in tournament prizes affects workers' hypothetical payoffs. For productive risk-neutral workers the high-prize tournament is attractive. In fact the majority of workers selecting a piece rate in the high-prize treatment would have increased their hypothetical payoff by being in the tournament. In the low-prize treatment fewer workers would have benefited from the tournament. There is no such treatment differences for workers selecting a tournament. Additionally, as figure 2.5 shows, the (potential) advantage in hypothetical payoffs of being in the tournament are larger for workers in the high-prize treatment. While these observations suggest that many workers in the high-prize treatment did not select the payoff-maximizing contract by choosing the piece rate, the analysis does not take other aspects of the different payment schemes into account, e.g. riskiness or "fairness".

2.3.6 Main result

The results of the previous sections demonstrate the incentives tournaments can provide for workers. As shown in section 2.3.2 workers' performances in both tournaments are on par with the output in the piece rate. Therefore the inexpensive low-prize tournament in the no-competition stage generates a larger manager profit than the piece rate. This finding demonstrates the potential cost advantage of a fixed-prize tournament compared to a piece rate.

With worker sorting in the competition stage the low-prize tournament does not attract enough productive workers and therefore the cost advantage of the tournament does not translate into larger manager profits as in the environment without sorting. Even though for many workers the high-prize tournament provides the potential for larger earnings (see the results in the previous section), there is no sufficient self-selection of productive workers into the tournament to outweigh the cost increase for managers. Therefore neither tournament outperforms the piece rate with worker sorting. This finding is a main result of the study. With the given parameters, it is never more profitable for managers to offer a tournament instead of a piece rate if worker sorting is feasible.

2.3.7 Fixed wages and gift-exchange

Whether an employer that pays higher (fixed) wages to his or her employees is rewarded with higher effort is an extensively researched topic in economics. Within this experiment it can be tested whether gift-exchange takes place by analyzing the fixed wages and effort provisions in both stages. There are a few other papers that study the gift-exchange phenomena with employers choosing between different contracts. Probably the closest paper to the setup discussed here is Fehr et al. (2007). In their setting employers decide between trust and incentive contracts. In their study they find that gift-exchange takes place, but it is not as efficient as in studies in which only a fixed-wage contract is available.

In table 2.7 the results of an OLS regressions of workers' performance with a fixed wage in the competition stage are presented. While generally the analysis could be done for the no-competition stage as well, the low number of fixed-wage contracts in this stage does not allow for a meaningful analysis. Therefore in table 2.7 only the results for the competition stage are reported. The dependent variable is the number of tables solved. The following explanatory variables are used in the regression: *Fixed wage* is the wage a worker received (in points, dividend by 10 to illustrate the effect of a meaningful wage increase), *1 if male* is a dummy variable that is one for males, *Productivity* is the number of tables solved in step 3 (productivity indicator III), *First choice fixed wage* is a dummy which is one for workers whose first choice was the fixed-wage contract and *Low-prize treatment* is a dummy variable that is one for the low-prize treatment.

If higher wages are perceived as a gift higher fixed-wage payments should result in higher effort and therefore in more tables solved. This gift-exchange hypothesis is confirmed by the significant positive coefficient on *Fixed wage*. The coefficient implies that a 10 point higher wage leads to about one more table solved. Since a manager receives 14 points for each table offering a higher wage was profitable.[37]

[37] A scatter plot with all offered wages in both stages, the corresponding output of workers and the regression line is shown in figure B.1 in the appendix.

Table 2.7: Determinants of workers' output in fixed-wage contracts

Dependent variable: # of tables solved	Competition stage	
	Coefficient	(SE)
Fixed wage	0.95***	(0.18)
1 if male	6.46*	(2.90)
Productivity	−0.22	(0.69)
First choice fixed wage (d)	−10.36**	(3.47)
Low-prize treatment (d)	8.18**	(2.93)
Constant	1.12	(9.02)
Observations	40	
R^2	0.420	

The *First choice fixed wage* coefficient describes that workers who self-selected into a fixed wage solved fewer tables than workers who wanted a different payment scheme. The inclusion of *Productivity* ensures that the effect is not due to productivity sorting or differences in effort costs. The negative coefficient on *First choice fixed wage* might demonstrate a selection based on the "unwillingness" to work. The coefficient is relatively large, with about 10 tables less solved by workers who chose a fixed wage.[38]

The weakly significant positive coefficient of the gender dummy indicates that males generated more output than females. Furthermore, the significant and positive coefficient on the treatment dummy implies a stronger effect in the low-prize treatment. One possible explanation for this treatment difference could be that the same wage was perceived as "fairer" by workers if the alternative tournament prizes were lower. To analyze whether a certain fixed wage was perceived fairer in the low-prize treatment the fairness rankings elicited in step 10 can be used. In table 2.8 the average fairness rankings (from 1 - 7) for the question "How fair do you consider a fixed payment of ... points?" of workers being paid with a fixed wage in the competition stage are listed. While workers rated a higher wage as more "fair", the differences in the fairness ranking for the same level of the fixed wage between treatments are small. Workers in the two treatments considered the fixed wages as about equally fair and therefore the positive treatment coefficient is not explained by a different "fairness perception" in the low-prize treatment.

The raw data for both stages reveals that the positive *Low-prize treatment* coefficient is driven by a few workers in the low-prize treatment, who received a high wage (250 points and more) and solved many tables (30 and more). Excluding these observations results in an insignificant *Low-prize treatment* coefficient. While there is no reason to exclude these observations, this procedures hints that the positive coefficient is driven by these observations.

[38] To ensure that this result is not due to a selection that is driven by the difference in offered wages the regression was also done with an interaction term between *First choice fixed wage* and *Fixed wage*. This regression yields similar results, which indicates that the significantly negative coefficient of *First choice fixed wage* is not entirely driven by sorting (regression results are not reported).

Table 2.8: Average stated fairness

	Treatment	
Payment scheme	High prize	Low prize
fixed wage:		
100 points	2.45	2.65
200 points	4.90	4.15
300 points	5.15	5.35
tournament prize	5.20	5.70
tournament	2.50	2.45
piece rate	5.20	5.50

Notes: "Fairness" is stated on a 1-7 Likert scale. Only workers paid with a fixed wage in the competition stage are included.

Therefore a possible reason of the reported treatment difference might be higher wage offers in the low-prize treatment and not a different perception of the fixed wages.

2.4 Discussion and conclusion

Rank-order tournaments are a frequently studied payment scheme in the theoretical, but also experimental economic literature. In this paper the profitability of a tournament as payment scheme with the possibility of worker sorting is analyzed. The data illustrates the incentive effect of tournaments for workers' performance, but also that employers only benefit from this effect if workers are not able to sort themselves into different contracts. In the environment with worker sorting a tournament does not generate larger firm profits than a piece rate. While a tournament can be an attractive payment scheme for productive workers if the prizes are sufficiently large, for a manager a tournament with large prizes does not yield greater profits than a piece-rate contract. Due to productivity sorting also a low-prize tournament does not yield larger profits than a piece rate. This finding is likely part of the explanation why a tournament is usually not the main contract scheme for many workers in labor markets, despite its good incentives.

These results could explain why tournaments are usually used as "add-ones" in (implicit) contracts - e.g. a promotion tournament or competition for a bonus - but not as the main payment scheme. When other incentive inducing payment schemes are available - the piece rate in the presented experiment - managers have no advantage by offering a tournament. While managers in the presented setting do not earn significantly less with a tournament compared to a piece rate, this result could be conditional on the used matching procedure. In the experiment each manager receives a sufficient number of workers with all payment schemes. In real world labor markets positions with an unattractive payment scheme might be unfilled or it might take some time until vacancies are filled.

Workers' payment scheme choices show a clear sorting pattern. More productive workers prefer variable-payment schemes, confirming previous findings. But the individual productivity does not explain the sorting between the two variable-payment schemes piece rate and tournament. The only personal characteristic with a significant effect on the choice between the piece rate and a tournament is the gender. Females are less likely to select a tournament, but they are not less likely to select a variable-payment scheme in general.

However, the experiment does not replicate the results regarding the importance of risk preferences for workers' choices reported in previous studies. Typically variable-payment schemes and especially a tournament are considered as risky for workers, since the exact wage payment is unknown before the conclusion of the task. In the presented experiment there are two factors which might reduce the importance of risk preferences: First the uncertainty in the piece rate is relatively low. Subjects had already worked for more than 15 minutes on the task when they stated their preferences over payment schemes. Therefore they probably had a good idea how many tables they could solve. Second, risk preferences were elicited with lottery choices and not with a more general statement "How willing are you to take risks in general?" as in Dohmen and Falk (2011). They argue that both risk measures - lottery choices and general statement - are highly correlated, but use the questionnaire answers for their analysis. While it is possible that the lottery task did not accurately measure subjects' risk attitudes regarding employment choices, I view the non-significance of the elicited risk preferences for the payment scheme choices as surprising.

Equally unexpected is the irrelevance of workers' relative self-assessment of their rank for the decision whether or not to enter the tournament. Even though there is no significant effect of workers' self-assessment for the payment scheme choice, many subjects in the experiment have overoptimistic beliefs. 30% of workers (36 subjects) overestimate their own rank in step 3 and 18% (21 subjects) overestimate their rank by at least two ranks.

This study shows that tournaments might not be used very often simply because they are either unattractive for workers or too expensive for firms. These effects should be true for most tournaments designs, but I cannot exclude the possibility that there exist tournaments that are not more expensive for managers and simultaneously more attractive than a piece rate for workers. While the cost argument makes it unlikely that larger tournament prizes are a possibility, a smaller prize spread between the winner and the loser prize could be such a design. Furthermore, a tournament might be more attractive for workers if the other available payment schemes are not as well liked as the piece rate in this study. Alternatively the level of the piece rate could be smaller, such that very productive workers prefer a relatively cheap tournament to the piece rate.

Another aspect that cannot be addressed with the current design is the role of repetition. Workers make the sorting decision exactly once. Despite the fact that they have accumulated quite some experience in the task when deciding between the available payment schemes, they have relatively little information regarding the productivity of other workers and their relative ranking. On the one hand, having subjects perform a real-effort task has the advantage of good measurements of their productivity and the data clearly demonstrates that productivity differences between workers are an important factor for the observed sorting behavior. On the other hand, the real-effort tasks creates a natural limit on the duration of the experiment and therefore leaves the question whether the sorting behavior would change when workers make the sorting decision several times unanswered.

This paper is a first attempt in understanding the effects of competition for workers on the profitability of labor-market contracts. The separation of the incentive and sorting effect demonstrates the importance of worker sorting for firms' profits. The explicit choice between different incentive schemes influences which payment schemes are attractive for productive workers. In further research it will be interesting to investigate the importance of sorting for the profitability of tournaments more thoroughly in especially with regard to different payment schemes and more variation in tournament prizes and designs.

Appendix B

Appendix Chapter 2

B.1 Appendix

Table B.1: Overview of the experiment

Step 1	Step 2	Step 3	Step 4	Step 5	Step 6	Step 7	Step 8	Step 9	Step 10
First table	Skill elicitation	Effort elicitation	Question-naire I	No competition	Question-naire II	Competition	Question-naire III	Risk preferences	Question-naire IV
solve one table as fast as possible (unpaid)	solve one table as fast as possible (paid)	work for 5 min solve as many tables as possible piece rate of 8 pts per table	state amount of stress [1-7] state amount of effort [1-7] How many subjects performed worse? [0-8]	subjects randomly become managers or workers managers choose payment scheme 10 min working stage	state amount of stress [1-7] state amount of effort [1-7] How many subjects performed worse? [0-5]	managers rank payment schemes workers state preferences over payment schemes 10 min working stage	state amount of stress [1-7] state amount of effort [1-7] How many subjects performed worse? [0-5]	10 binary lottery choices	fairness ranking of payment schemes rate real-effort task and experiment social demographics and employment status

B.1.1 Productivity indicators

In table B.2 below the Spearman rank correlations for all three productivity indicators, as well as the Spearman rank correlations between the three measures and the number of tables a worker solved in steps five (no-competition stage) and seven (competition stage) if the worker was paid with a variable-payment scheme are listed.

For productivity indicator I there are 225 observations (9 subjects did not solve the table correctly in the given time). For productivity indicators II and III there are 234 observations. In the table pooled data for all treatments is presented. There are 140 observations used for the tables solved in step five (no-competition stage), 110 observations for the number of tables solved in step seven (competition stage) and 101 for the correlation of the performances in steps five and seven. The difference to the 234 total subjects comes from workers with a fixed wage and managers. Both types of subjects are excluded for these two correlations. Since many workers do not provide a lot of effort if they are paid with a fixed wage, these observations are not included. Remember: Productivity indicator I is the time needed to solve the first table, productivity indicator II is the payoff received for solving the 2nd table and productivity indicator III is the number of tables solved in step 3. All correlations have the expected sign and are significantly different from zero.

Table B.2: Spearman rank correlations among productivity indicators and workers' outputs

	Productivity indicator I	Productivity indicator II	Productivity indicator III	Tab. solved step 5	Tab. solved step 7
Productivity ind. I	1.0000				
Productivity ind. II	-0.1736	1.0000			
Productivity ind. III	-0.1845	0.4335	1.0000		
Tables solved step 5	-0.3117	0.5474	0.7406	1.0000	
Tables solved step 7	-0.3442	0.5259	0.5589	0.7688	1.0000

Notes: All correlations are significantly different from zero (p-values < 0.01).

B.1.2 Scatter plot of fixed wages and workers' output

Figure B.1 shows the offered fixed wages in the no-competition and competition stage and the resulting performance by workers. Dots represent observations from the low-prize treatment and triangles are observations from the high-prize treatment. There are no observations from fixed-wage contracts in the low-prize treatment in the no-competition stage. Furthermore, the OLS regression lines (see section 2.3.7 above) are shown. The regression line for the competition stage illustrates the significant effect of higher wages on workers' performance. The shown line for the no-competition stage comes from a similar regression that is not reported in the paper due to the low number of observations. The coefficient on the fixed

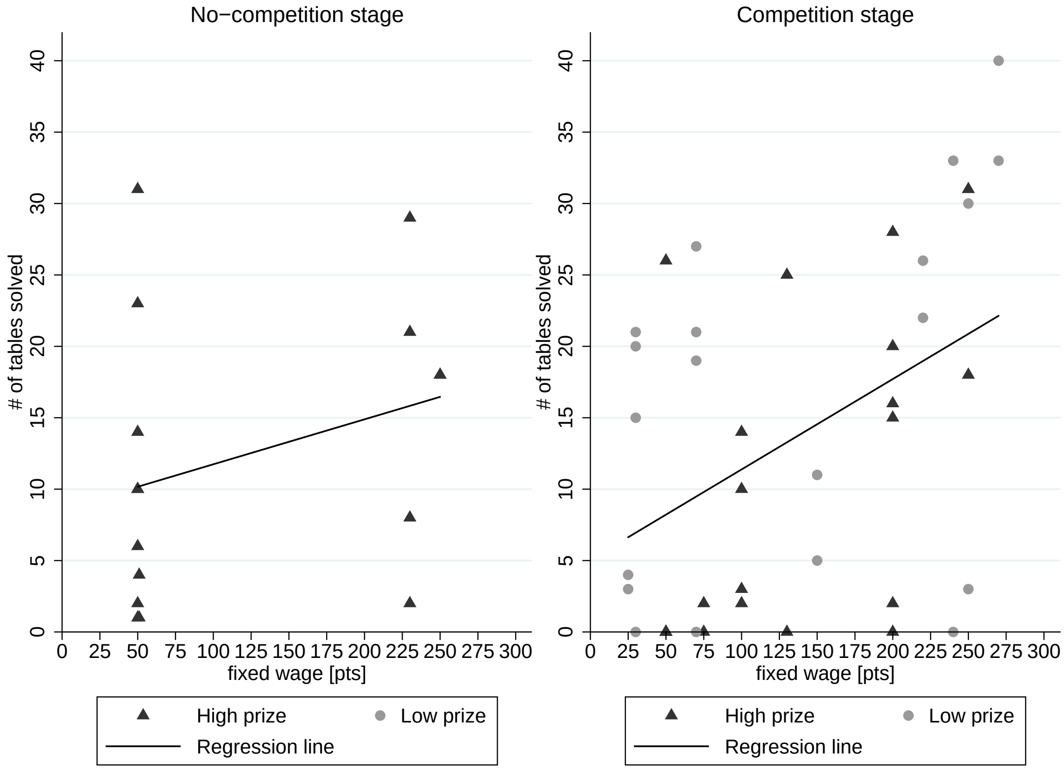

Figure B.1: Scatterplot of fixed wages and workers' performances

wage is not significantly different from zero for the no-competition stage.

B.2 Appendix II - Instructions

Below is the translation of the German instructions for the high-prize treatment. The originals are available from the author upon request. Instructions for the low-prize treatment only differ in the tournament prizes and bounds for the feasible fixed wages. Instructions were provided on screen before each step. Since the questionnaires in steps 6 and 8 were almost identical for workers and managers, only the question that differs for managers is included in the translation for steps 6 and 8. All shown emphasizes are in the original instructions.

Welcome

WELCOME!

Thank you for taking part in this experiment. The experiment lasts approximately 2 hours and you will receive your reward directly at the end of the experiment.

The experiment consists of multiple parts and at the start of each section you will see the instructions on your screen. Each screen with instructions will be marked by the header **Instructions**. If you still have questions after reading the instructions, please raise your hand. An experimenter will come to you to answer your question in private.

In the different parts of the experiment you can earn points. At the end of the experiment all points are converted to Euros and result in your payment. **10 points** are always **13 Cents**.

Please remain quite during the experiment and do not talk to other participants. During the experiment you will have to solve multiple exercises. You have to solve these tasks without any aids. Because of this, please store all your cell phones, calculators, paper, pens, books and so one in your bag.

Please click on CONTINUE.

Step 1

Instruction Part I

It is your task to count how often the digit **one** appears in the table below. The table consists of ones and zeros. You can see an example in the lower left. This table has **38** ones.

You have a total of 75 seconds. Please solve the task as quickly as possible.

Here you can enter your answer and confirm your entry.

Please click on CONTINUE. Your time starts as soon as all participants have clicked on CONTINUE and when you see the table on your screen.

Step 2

Instruction Part II

In this part you have to solve an identical task to the part before.

Different than before you will be paid for the correct solution of the task. Your payment is larger, the quicker you solve the task correctly. You have a total of 75 seconds. The quicker you enter the correct number, the larger your payment. In the beginning you have 150 points. Each second that elapses until you enter the correct answer results in a deduction of 2 points from your payment.

If you take 20 seconds to solve the task you receive 110 points for this part.

If you enter a wrong number, 20 points will be deducted from the remaining points and you can enter a new number. You will only receive a payment if you enter the correct number within 75 seconds. Under no circumstances your payment can be negative.

Please click on CONTINUE. Your time starts as soon as all participants have clicked on CONTINUE and when you see the table on your screen.

Step 3

Instruction Part III

Your task in part three of the study is it to solve as many tables as possible. You will again see a table with ones and zeros, like in the previous parts. However, now the task is not to solve one table, but to solve as many tables as possible. A new table will only appear if you have solved the previous one correctly.

You have a total of 5 minutes to count in as many tables as possible the number of ones. After you have entered the correct answer for a table, a new table appears. If your answer is wrong, you will see a message and you can enter a new answer.

For each correct answer you receive 8 points. Each wrong answer results in a deduction of five points from your payment.

If you solve three tables correctly and enter one wrong answer you receive 19 points for this part of the study. The remaining time is shown in the upper right corner.

Please click on CONTINUE now. The next part starts as soon as all participants have clicked on CONTINUE.

Step 4

The working time of five minutes is over.

Please state as how exhausting you experienced the task in the previous part: Very easy - Very hard

Please state how much effort you exerted for the task in the previous stage: Not at all - As much as possible

In total there are 18 participants in this session in two groups. You therefore belong to a group of 9 participants. Please estimate how many participants in this group solved **fewer** tables than you in the previous part?

Your payment for this task is larger, the better your estimate is. If you estimate is correct you receive **75.0** points. If your estimate does not over or undershoot by more than one person you receive **37.5** points. If your estimate over or undershoots by more than one person you do not receive a payment for this task.

0 - No one, no participant solved fewer tables than me.

8 - I solved the most tables, all other eight participants solved fewer tables than me.

Step 5

Instruction Part IV

The instructions for part four are somewhat longer. Therefore you can go back and forth with the Page 1 and Page 2 buttons, which are at the bottom of the screen, below the screen with the instructions.

With the start of the next part there are two different roles in the experiment. Each participant is from now on either an employee or a manager. Despite the fact that employees and managers can be female as well as males, the instructions will call all managers female managers *(Note: there is a gender distinction in German)* while all employees are male employees.

For the next part of the experiment three participants will form a group. Each group acts in the following part as one firm. In each firm there is **one** manager and **two** employees. Also the selection who will be a manager and who will be an employee is randomly determined by the computer.

The task of the employees is, like in the previous parts, to count the number of ones in tables. For this participants have **10** minutes. Which means the working time is exactly twice the time of the previous part.

How you will be paid for this task depends on the decision of the manager which payment scheme to choose. The payment of the manager, however, depends mainly on the number of correctly solved tables by the employees (more on this on page 2 of the instructions).

Below the tasks and the choices of managers and employees are described. Please read these instructions carefully and raise your hand in case you have some remaining questions after reading the instructions.

Managers have three different payment schemes to choose from:

Fixed wage	Variable payment	Competition
Each employee gets a fixed wage. This wage is independent of the number of tables solved by the employee. Managers chooses the level of the fixed wage. It must be at least 50 points and can maximally be 450 points. Each of the two employees within a firm receives the same fixed wage.	Each employee receives for each correctly solved table a payment. Each correct answer yields an employee 8 points. This means the payment of the employee only depends on the number of tables this employee solved correctly.	At the end of the working period the employee, who solved more tables correctly, receives a wage of 450 points. Hereby it is not relevant how many tables he solved. It is only important whether he solved more or less tables than the second employee in his firm. The employee who solved fewer tables receives a wage of 50 points. If both employees solve the same number of tables a lottery draw determines who receives which wage. The payment of a employee therefore only depends on whether he solved more or less tables than the 2. employee.

Instruction Part IV Page 2

The payment of managers consists of two different components.

1) Each manager receives for each correctly solved table by her employees **14** points. However, she has to pay the employees with this revenue. This means from her earnings the wage payments to the employees are deducted. Here the manager has the choice between the three payment schemes described on the previous page. The manager makes her choice and workers are told the payment scheme before the start of the working period.

2) A manager receives a payment dependent on the number of tables she solves correctly.

If the manager solves more than 10, but less than 20 tables, she receives 50 points. Does she solve more than 20 but less than 40 tables, she receives 100 points and if she solves more than 40 tables she receives 150 points. This payment is added to her earnings.

The payment of a manager therefore depends on the number of tables solved by her workers as well as on the number of tables she solves herself.

Example

Assume that the manager solved 14 table herself and her employees solved 20 and 10 tables. The manager therefore receives a payment of 50 points + (20+10)*14 = 470 points.

From this payment the wages of the employees are deducted. This means, dependent on the payment scheme, which the manager selected at the start of this part, the following wage payments are deducted from her payment:

With the variable payment (20+10)*8 points, so a total of 240 points are deducted. With a fixed wage the manager has to pay the fixed wage and with a competition she has to pay a total of 500 points.

Quiz

In this part of the experiment, both, managers and employees, have the possibility to do something else besides counting ones in tables.

Below the table there will be two buttons. With the button “Quiz” a participant can switch to a window in which quiz questions are asked. With the button “Experiment” the screen switches back to the table.

During the time a participant is on the quiz screen he or she cannot solve any tables. The answering of quiz questions is not paid. Each participant can go back and forth between the experiment and the quiz during the entire working time.

When you have understood these instructions and do not have any further questions, please click on CONTINUE.

Step 6 - Questionnaire for mangers

First two questions are identical to step 4.

How many tables, do you think, have been solved by your employees?

Your payment for your estimate is larger, the better your estimate is. If your estimate is correct you receive **37.5** points. If your estimate is not wrong by more than plus or minus three tables, you receive **18.8** points. If your estimate is wrong by more than three tables, you do not receive a payment for this task.

(Workers again state how many workers they believe solved less tables than they themselves. [0-5])

Step 7

Instruction Part V

This part 5 is very similar to the previous one. Again managers can choose between the three payment schemes for their employees (competition, fixed wage or variable payment). Also the payment of the managers and the working time stay the same. However, different to before, the employees are **not** tied to one manager anymore.

In this part of the experiment **three** managers and **six** employees are grouped together. Each manager will offer a **different** payment scheme, which means that each payment scheme will be used by one firm. First managers choose the level of the fixed wage, which they are willing to pay, if their firm wants / has to pay a fixed wage. Then each of the managers states which payment scheme she wants the most, which one she likes second best and which one she likes the least.

If two (or more) managers prefer the same contract (choose the same contract as their "first choice") a random draw determines who can offer this contract. After this the second choice of the remaining managers are considered and it is tried to fulfill these choices. If again the managers agree in their second choice one manager has to offer her third choice. Hereby the previously chosen fixed wage of the manager who offers a fixed wage is chosen.

After the statements of the managers (and possibly the random draw) decided, which manager offers which contract, the employees choose between the different contracts. Each employee states which of the three payment scheme he prefers most, which one would be his second choice and which one he likes the least.

Different than the statements of the managers **not a random draw** decides if multiple employees prefer the same payment system, but the statements of an employee who solved **more tables in part 3** are fulfilled fist. (This was the part in which each participant was asked to solve as many tables as possible within five minutes.) According to the choices of the employees each manager again receives two employees. Each firm therefore again consists of one manager and two employees.

Summary

Managers again receive 14 points for each table that was correctly solved by one of her employees. From this payment they have to pay the wage of their employees.

The same three payment schemes as in the part before are possible: Variable payment, competition or a fixed wage. The details of the payment schemes of the three methods also have not changed, e.g. the variable payment is still 8 points per table.

The split in employees and managers remains unchanged. If you have been an employee before, you are also an employee in this part. Like in the part before there is again a quiz for all participants. The participation in the quiz is not paid for any participant.

Example

Each manager now states which fixed wage she would pay. Then each manager states the order in which she would select the different payment schemes.

After these statements have been used to determine which manager offers which contract, each employee sees the choices of all three managers on his screen. Each payment scheme has a number 1-3. The employee then states the order in which they would select these three payment methods.

The number of the method you prefer the most you please state in the field “1st Choice”, the number of the method you want as your second choice you please state in the field “2nd Choice” and in the field “3rd choice” you state the method you like the least.

Please click on CONTINUE, once you have understood these instructions.

Selection of fixed wage - managers

Your role in this study is unchanged. This means you are still a manager.

Please select now which fixed wage you pay to your employees in case you want / have to offer a fixed wage in this part.

Reminder: Different than before employees are not directly linked to a manager. This means workers see your wage offer, as well as the contracts of the other managers. Each manager offers a different contract, this means that if you offer a fixed wage, the other two managers offer a competition and a variable payment.

Please select the level of the fixed wage for your workers [in points]:

Reminder: You receive 14 points for each table that is correctly solved by one of your employees. If you select a fixed wage, each employee receives the same wage, independent of the number of tables he solved. In the variable payment from the 14 points 8 points go to the employee. In the competition the employee who solved more tables receives 450 points and the employee who solved fewer tables receives 50 points.

Selection of managers

Possibility 1: **Fixed wage** of **X** points

Possibility 2: **Variable payment**

Possibility 3: **Competition**

1st choice, 2nd choice, 3rd choice

Reminder: You receive 14 points for each table that is correctly solved by one of your employees. If you select a fixed wage, each employee receives the same wage, independent of the number of tables he solves. In the variable payment from the 14 points 8 points go to the employee. In the competition the employee who solved more tables receives 450 points and the employee who solved fewer tables receives 50 points.

Worker selection screen

The three managers of your group offer the following contracts:

Offer 1: Fixed wage Offered wage: X points

Offer 2: Competition

Offer 3: Variable payment

Please select your favorite order of the contracts. The higher you rank an offer, the likelier it is that you will receive this offer.

Enter the number of the offer you like best in the first field, the number of the second best offer in the second field and the number of the contract you like least in the third field.

Step 8 - Questionnaire

Identical to Step 6 above.

Step 9 - Questionnaire

Questionnaire page 1 of 3

In the questions on this page you play for real money. Below you see 10 bets we offer you. Please decide in each row whether you want bet A (left column) or bet B (right column).

Please indicate now in each row, whether you want the left or the right bet.

Once you have finished the questionnaire, we will play out one of the 10 bets. This means a random draw will determine which row we will play. In the selected row, we will play the bet you have chosen.

Example

e.g. in the first row you have the choice between bet A, which pays 2 Euros with 10% probability and with 90% 1.60 Euro. You can also choose bet B, which pays with 10% probability 3.85 Euros and with 90% 0.10 Euro.

Bet A		Bet B
10% chance of winning 2 Euro and 90% of 1.60 Euro.	Bet A / B	10% chance of winning 3.85 Euro and 90% of 0.10 Euro.
20% chance of winning 2 Euro and 80% of 1.60 Euro.	Bet A / B	20% chance of winning 3.85 Euro and 80% of 0.10 Euro.
30% chance of winning 2 Euro and 70% of 1.60 Euro.	Bet A / B	30% chance of winning 3.85 Euro and 70% of 0.10 Euro.
40% chance of winning 2 Euro and 60% of 1.60 Euro.	Bet A / B	40% chance of winning 3.85 Euro and 60% of 0.10 Euro.
50% chance of winning 2 Euro and 50% of 1.60 Euro.	Bet A / B	50% chance of winning 3.85 Euro and 50% of 0.10 Euro.
60% chance of winning 2 Euro and 40% of 1.60 Euro.	Bet A / B	60% chance of winning 3.85 Euro and 40% of 0.10 Euro.
70% chance of winning 2 Euro and 30% of 1.60 Euro.	Bet A / B	70% chance of winning 3.85 Euro and 30% of 0.10 Euro.
80% chance of winning 2 Euro and 20% of 1.60 Euro.	Bet A / B	80% chance of winning 3.85 Euro and 20% of 0.10 Euro.
90% chance of winning 2 Euro and 10% of 1.60 Euro.	Bet A / B	90% chance of winning 3.85 Euro and 10% of 0.10 Euro.
100% chance of winning 2 Euro and 0% of 1.60 Euro.	Bet A / B	100% chance of winning 3.85 Euro and 0% of 0.10 Euro.

We add the money you earn to your total income of the study.

Questionnaire page 2 of 3

How well did you like the task in this experiment? Not at all - - - - - - - Very good

As how difficult did you perceive the task in this experiment? Very easy - - Very difficult

How well did you like the entire experiment? Not at all - - - - - - - Very good

How fair do you consider the following payment schemes for a worker in parts 4 and 5?

Fixed payment of 100 points Very unfair - - - - - - - Very fair

Fixed payment of 200 points Very unfair - - - - - - - Very fair

Fixed payment of 300 points Very unfair - - - - - - - Very fair

Fixed payment of 450 points Very unfair - - - - - - - Very fair

Variable payment of 8 points per table Very unfair - - - - - - - Very fair

Competition with 450 points for the best worker and 50 for the other
Very unfair - - - - - - - Very fair

Questionnaire page 3 of 3

Please state your Abitur grade: If your highest school diploma is not a German Abitur e.g. Matura, A-levels, ... please state a **0** in the field.

Please state your final high school math grade: If your final math grade was not in the German 15 point system please state a **0**.

Please state your final high school German grade: If your final German grade was not in the German 15 point system please state a **0**.

Please state the year of your highest high school certificate:

Please state your gender: female - - male

Are you currently enrolled in a University / University of Applied Science? Yes - No

Are you working at a University / University of Applied Science?
(e.g. as teaching or research assistant, doctoral student or employee) Yes - No

Do you have a job outside of the university? [e.g. employee, teacher, servant, ...] Yes - No

Chapter 3

Flip a coin or vote: An Experiment on Choosing Group Decision Rules[1]

with Sander Renes

3.1 Introduction

Before a group or society can take a decision, whether it is about what restaurant to visit, or whether to implement a social security reform, the group has to select a decision rule to aggregate individual preferences. To maximize the value of the final decision to the group, its members should agree on a mechanism that maps the vector of individual preferences into an efficient decision. However, in a seminal paper Myerson and Satterthwaite (1983) show that if individuals are privately informed about their preferences over outcomes, there is no efficient decision rule that is unanimously preferred over the non-implementation status quo. If a participant can force non-implementation, she[2] should do so whenever she learns that the (proposed) decision rule can lead to outcomes she likes less than the status quo. Because of the existence of an outside option (the status quo), some individuals are better off not participating in the efficient mechanism at all. In this paper we address the problem of (efficient) mechanism selection in a group decision experiment. We study binary comparisons between four decision rules and investigate the role of different outside options and private information on subjects' mechanism choices.

In a collective decision about the implementation of an indivisible public project, an optimal mechanism results in implementation if the sum of individual valuations is larger than zero. While such mechanisms exist and groups can in theory replace any existing mecha-

[1]We appreciate the comments and the advice received from Dirk Engelmann, Hans Peter Grüner, Eckhard Janeba, Henrik Orzen, the participants at the ESA conference in Heidelberg, the SEET meeting in Malta, the seventh Bavarian Micro Day in Passau and seminar participants in Mannheim, Nuremberg and Rotterdam. Financial support from the German research foundation (DFG), SFB 884 is gratefully acknowledged. All remaining errors are our own.

[2] For better readability only female pronouns are used.

nism by a more efficient one, other mechanisms are often selected in practice. The theoretical literature has identified private information and outside options as key aspects of the mechanism choice situation that determine whether an efficient mechanism can be implemented. Comparing Myerson and Satterthwaite (1983), Schmitz (2002), Segal and Whinston (2011), and Grüner and Koriyama (2012) shows that, if individuals can better serve their interest in an inefficient alternative than in the efficient mechanism, they have no reason to select the efficient mechanism. Additionally, the Myerson-Satterthwaite theorem illustrates that any individual with private information about her project payoff should use this information to determine her preferences over the mechanisms. Individuals that dislike the public project often have a better chance of blocking the project in an inefficient mechanism, and therefore are worse off under the efficient mechanism.

In this paper we study the influence of both of these aspects on individuals' mechanism choices. We conduct an experiment with a two-stage voting procedure in a collective decision making situation. In the first stage (mechanism choice), individuals select their preferred decision rule from two available mechanisms. In the second stage, each three-person group applies the chosen mechanism to determine the group decision on the provision of an indivisible public good (implementation decision). By varying the distribution of private valuations for the public good between treatments and the available mechanisms between rounds, we investigate the role of the outside option on the mechanism choice. Furthermore, we vary the moment at which the private information about the individual project payoffs is revealed within treatment. This design enables us to test the relationship between preferences and the revelation of private information within subject. Understanding how the revelation of private information alters choices, can help us to shed light on problems that occur in many choice institutions. Since in many real-life situations decision rules can be altered in the interim stage where individuals have private information, we consider this aspect particularly important.

In our experiment the mechanism selection is done via a random dictator procedure. We ask subjects to declare their preferred mechanism out of 2 options. After each subject has selected her preferred mechanism in the first stage, one group member is randomly chosen in stage two and her preferred mechanism is implemented as the group decision rule. While this random dictator rule is neither an optimal nor a realistic procedure, it elicits the willingness to participate in a mechanism without any distortions. It ensures incentive compatibility, while it allows us to present the required choices in a randomized order to minimize presentation or order effects. The four group decision rules we use are: the Simple Majority (SM) mechanism, a first-best optimal direct revelation game known as the Arrow, d'Aspremont Gérard-Varet (AGV) mechanism,[3] the Non-implementation Status Quo (NSQ) and the flip of a fair coin, or Random implementation (RAND). Since this two-stage voting

[3] This mechanism is also known as the expected externality mechanism. It was originally proposed in Arrow (1979) and d'Aspremont and Gérard-Varet (1979).

procedure is a game of imperfect information, there are several measures of efficiency that could be relevant. We measure efficiency through realized values, and thus efficiency refers to ex-post classical efficiency.

With the chosen design we can conduct a strong within-subject test of the Myerson-Satterthwaite theorem by comparing subjects' mechanism choices in an ex ante and an ad-interim stages. Ex ante, an uninformed individual should prefer the more efficient mechanism. Ad interim, if she knows she has a negative payoff from project implementation, the same individual should prevent the use of any mechanism that allows for project implementation, and thus prefer NSQ. By changing the two available mechanisms between rounds, we can test the predictions of Schmitz (2002), Segal and Whinston (2011), and Grüner and Koriyama (2012) that participation constraints are less binding if the outside option includes risky outcomes. The variation of the potential valuations for the public good over treatments allows us to demonstrate that the observed change in preferences is driven by changes in expected payments.

Our results demonstrate that without private payoff information individuals select efficient mechanisms. Subjects choose the more efficient mechanism because it yields the largest expected payoff ex ante. Ad interim, when they are informed about their valuation for the project, they only prefer the (more) efficient mechanisms if they have a positive private valuation, otherwise they opt for the non-implementation status quo whenever possible, exactly as predicted by Myerson and Satterthwaite (1983). Interestingly, we find indications that the differences in expected payoffs from the mechanisms *as they are played in the lab*, strongly influence the choices made. These results illustrate the importance of thinking about the decision rule to use in a group, or committee, before this group faces any actual decisions. Deciding upon a decision rule after an issue comes up is very inefficient. This effect is, for instance, sometimes clearly observed in hiring committees at universities and research institutes. Committees that fail to agree upon a decision rule before meeting the candidates, will likely have different subgroups within the committee that favor different candidates. In the ensuing discussion, the subgroups can propose the criteria that favor their preferred candidate. With different preferred candidates this will lead to a discussion about what set of criteria to use, and a costly delay of the decision or even a failure to hire a suitable candidate is likely.

Our results from the ad-interim stage suggest that it can be feasible to implement an efficient mechanism as long as the outside option yields uncertain results. With a risky outside option, individuals who know they do not want to implement the public project have no guaranteed way of blocking the implementation. In the absence of a secure non-implementation option, these subjects prefer the (more) efficient mechanism because it allows them to influence outcomes. Since both subjects with positive and subjects with negative valuations of the public project prefer to have influence over the outcome, agreements on efficient mechanisms are possible. These results support the predictions of Schmitz (2002), Segal and

Whinston (2011), and Grüner and Koriyama (2012).

With a safe, non-implementation status quo, our three-person groups are already severely hindered in implementing an efficient mechanism. Public projects and reforms usually require all involved individuals to cooperate, pay part of the price (through taxes for instance), permit use of their resources or even the reorganization (or removal) of their property rights. In a completely voluntary setting the kind of inertia caused by a non-implementation status quo in our experiment, would make public projects virtually impossible to negotiate. Simultaneously, virtually all decision bodies require a proposal be brought up for consideration before they can change the status quo, such that this type of inertia can occur virtually everywhere.

In many situations the mechanism choice takes place when agents are already informed about their preferences over outcomes and the outside option is fixed by the existing situation. If the existing situation does not involve an efficient mechanism the group might need coercive power to force individual members to cooperate. As a consequence, our results provide a rationale for the role of coercive power in group decisions. By forcing group members to participate in individual projects, the group surplus can be increased since individual projects no longer need to satisfy all participation constraints. If an entity has coercive power, the participation constraints of individuals become less relevant. Furthermore, our results also provide evidence that specifying decision procedures in constituting documents is a good idea. By setting a standard procedure and demanding that these procedures can only be changed through a considerably more demanding procedure, the drafters of the constitution ensure that the mechanisms are de facto established in an ex-ante stage.

The rest of the paper is organized as follows: section 3.2 summarizes the previous research on group mechanism choices and participation constraints and section 3.3 outlines the experimental design and the three treatments. Section 3.4 states the theoretical predictions, while section 3.5 describes the results on all predictions and discusses further findings. Finally, section 3.6 concludes.

3.2 Related literature

Our setup is most closely related to the literature discussing the choice of voting rules or constitutions. The discussion on constitutional choice features two (connected) ideas: stability and inertia. Since these constitutions contain many meta-rules, changing them often is undesirable. In a seminal contribution Barbera and Jackson (2004) show that a constitution that specifies the same simple majority rule to implement policy and to change the constitution, can be hijacked by self-interested majorities. To ensure a stable constitution, it has to adhere to a property these authors call self-stability. Under the rules set-out by the constitution, the society should not decide to change the constitution. Requiring a qualified majority to change the constitution is an easy way to create more self-stability. A similar idea is proposed in an explicitly dynamic setting by Acemoglu et al. (2012). Given some constraints that guarantee sufficient patience and prevent Condorcet-like voting cycles, these authors show that the stability of the constitution crucially depends on the set of other dynamically stable constitutions. As long as no winning coalition can be found that prefers another stable constitution, the state is stable. In this sense the set of stable coalitions form long-term outside options that should not be preferable to the agents. These results do not imply, however that the self-stable states are efficient (especially in the short-run). In fact, the authors argue that the inefficiencies in self-stable states could cause several of the seemingly costly problems observed in many systems.

Inertia is the central concept in a second strand of research, the social choice literature. This, mostly theoretical, literature is riddled with impossibility theorems that show it is not possible to design a social choice rule, or an implementing mechanism that combines some set of desirable properties in every imaginable circumstance. The most famous impossibility result by Arrow (1950) states that non-dictatorship, Pareto efficiency and independence of irrelevant alternatives cannot be obtained by any social choice function for all potential preference profiles. In similar vein, Myerson and Satterthwaite (1983) show that even in a setting with only two players and independent valuations, an efficient, interim incentive compatible and budget neutral mechanism for trade does not exists, as long as players can guarantee themselves a sufficiently large payoff when not trading.[4] The result that individual rationality, incentive compatibility and budget balance are incompatible for a N-player public good setting, of which our experiment is a special case, was proven by Mailath and Postlewaite (1990). Similar results are obtained by Güth and Hellwig (1986, 1987) for the private supply of a public good. When the mechanism to decide on production levels is selected through a veto rule (i.e. unanimous acceptation), efficient production cannot be reached unless a subsidy is provided. These impossibility results illustrate how a certain non-implementation status quo can stifle any chance of (efficient) mechanism change.

Several papers have derived possibility results that illustrate the dependence of impossibility results on the assumptions on the status quo that occurs if the mechanism is rejected. Cramton et al. (1987) show with a status quo of a more or less equal distribution of the good (ownerships rights in their setting), it is possible to design an auction like procedure that is both ad interim incentive compatible and ex-post efficient, without requiring subsidies. Schmitz (2002) shows that decisions on public good provisions can be made through an efficiently designed mechanism for some particular status quo settings. In many cases a status quo, either an interim allocation or a probability of implementation between 0 and 1, can be found that allows an efficient implementation and does not violate the incentive compatibility constraints at the interim stage. In case the valuation of the public good is identically

[4] An older, less general result can be found in Chatterjee (1982), while a more general statement can be found in a.o. Segal and Whinston (2014). The interpretation in this paper is mostly due to Cramton et al. (1987).

and independently distributed, such a status quo can always be found. This implies that both in the bargaining game of Myerson and Satterthwaite (1983) and in a public good provision problem, a status quo exists that will incentivize individuals to accept an efficient mechanism ad interim. Segal and Whinston (2011) make a similar point by demonstrating how background risk, or a status quo that is not quite as secure as the no-trade outcome, can increase the willingness of individuals to accept mechanism changes. Most importantly for our paper, their proposition 1 states that individuals are willing to accept a more efficient mechanism, if it has the same equilibrium distribution over allocations as the status quo, or default mechanism. Grüner and Koriyama (2012) illustrate that it is even possible for groups to shift from a (simple) majority voting system to the AGV mechanism, without violating interim participation constraints. This result is quite remarkable, since majority systems are much more efficient than a fixed or random status quo. The efficiency gains of moving to the AGV are therefore limited. However, for some settings and distributions the gains are large enough to compensate individuals for the potential loss in information rents.

Two closely related experimental papers study the effect of social preferences on mechanism choice. Bierbrauer et al. (2014) identify the theoretically optimal mechanism assuming social preferences of the players exist. Their experiment shows that choices for a small, but significant number of subjects, are better explained by including other-regarding preferences. They also illustrate that if enough of such subjects are present, the optimal mechanism with strictly self-interested rational players is no longer optimal to a social planner. The article most closely related to ours is Engelmann and Grüner (2013). In their experiments, groups of five subjects select their preferred mechanism out of 5 potential voting mechanisms. The voting mechanisms differ in the amount of positive votes required for implementation of a common project. Individuals differ in their utility from project implementation (utility is equal to zero if it is not implemented). A completely self-interested and rational subject should always opt for the voting rule that requires only one (all five) vote(s) for implementation, if she has a positive (negative) valuation of implementation, while voting positively (negatively) in the implementation decision. However, the authors find that subjects often choose mechanisms that require two, three or four positive votes for implementation. These deviations could be explained by efficiency or pro-social concerns in the mechanism choice stage. The authors note that this implies possible efficiency gains in decision making by letting groups vote on voting rules, before they vote on issues. If individuals indeed decide upon mechanisms with more efficiency/equality related criteria then they use for implementation decisions, participation constraints are less binding.

3.3 Experimental design

We first describe the game subjects participated in and explain the mechanisms used. We then outline the treatments and describe the procedures of the experiment. The only difference between treatments is the set of potential private valuations for the public project. The underlying procedures, game and all other details of the experiment, e.g. number of rounds, group size, available mechanisms, etc. are identical across all treatments.

3.3.1 The game

Subjects interact in groups of three and each group faces the question whether or not to implement an indivisible public project. Non-implementation results in a zero payoff for all subjects. If the project is implemented each player receives a project payoff equal to her valuation. The private valuations are drawn independently from a known uniform distribution on a given set of four values that depend on the treatment. The distribution and its support are common knowledge and remain the same within a session.

Each of the 18 experimental rounds proceeds in two steps. First, subjects select a mechanism to make the group decision. Second, the group decides about the implementation of the public project through the chosen mechanism. In all treatments the same four mechanisms are used and in each round subjects chose between two of them. The mechanisms we consider are:

Mechanism I AGV mechanism

All group members report a valuation for the implementation of the project. They can only report valuations that are present in the type space. If the sum of reported valuations is larger than zero the project is implemented, otherwise the project is not implemented. Independent of project implementation, subjects pay or receive a transfer that depends on the vector of reported valuations.

Mechanism II Voting - Simple Majority (SM)

All group members vote for or against the project (no abstention). If two or more group members vote for implementation the project is implemented, otherwise the project is not implemented.

Mechanism III Non-implementation Status Quo (NSQ)

The public project is not implemented.

Mechanism IV Random implementation (RAND)

Whether the public project is implemented depends on the flip of a fair coin. If the result of the coin toss is heads, the project is implemented otherwise the project is not implemented. Therefore this mechanism has a 50% probability of implementation independent of subjects' valuations.

At the beginning of a round subjects are informed about the two available mechanisms. They cannot influence which mechanisms are available in a round, and the order of the comparisons is randomly altered between sessions. Each subject privately selects one of the two given mechanisms. After mechanism choices have been recorded, the computer randomly picks one group member as the dictator and the mechanism chosen by this random dictator is executed. All group members are informed of the selected mechanism, but they do not learn whose choice was selected nor what mechanism the other two subjects selected.

If the AGV or SM mechanism is selected, all group members state a valuation for the project (AGV) or vote on the implementation of the project (SM). If the NSQ or RAND mechanism is selected no further action by the subjects is required. The computer determines whether the project is implemented through the selected mechanism and payoffs are realized accordingly. The project payoff is equal to the private valuations if the project is implemented, otherwise the project payoffs are 0. In the AGV subjects additionally pay or receive transfers that depend on the reported valuations but not on project implementation.

The experiment proceeds in two parts. In the first part, the first twelve rounds, subjects learn their private valuation for the public project ad interim that is, after choosing their preferred mechanism but before the mechanism is played. In part two, the last six rounds, subjects are informed about their private valuation for the project at the start of each round and therefore are aware of their valuation when choosing a mechanism. Subjects are never informed about valuations of other subjects. Our subjects face all six possible binary mechanism choices twice in the ex-ante condition (rounds 1-12), before going to the ad-interim rounds (rounds 13-18). Since subjects make the mechanism choice both in an ex ante and an ad-interim setting, we can compare mechanism choices in both settings within subjects.

By design, the choices in the ex-ante rounds are not influenced by previous experiences in the ad-interim rounds. Since we consider the expected value calculations to be more demanding in the ex-ante rounds than in the ad-interim rounds, we chose a design that delivers the cleanest decisions in the ex-ante rounds as our main treatment. Because we did not observe any signs of consistency concerns or order effects in the choices made by our subjects, we did not conduct sessions with a reversed order.

The design is in many aspects similar to the two-step voting procedure studied by Engelmann and Grüner (2013), but there are three important differences. First, in our study subjects choose between two mechanisms rather than five. This clearly identifies the outside option. Second, we have four very different mechanisms, rather than five mechanisms from the class of simple voting rules. This allows us to make the same comparisons studied in the theoretical papers. We describe the mechanisms used and the reasons for selecting these four mechanism in the next section. Third, Engelmann and Grüner (2013) did not look at the effects of private information on the behavior of subjects. As we show in this paper, the presence of private information fundamentally changes the choices made by our subjects.

3.3.2 The four mechanisms

The four mechanisms are chosen because of their theoretical implications and relevance for group decision making. The AGV mechanism, or expected externality mechanism, is the theoretically optimal mechanism for decisions about indivisible public projects, like reforms. It is incentive compatible, ex-post budget balanced and induces efficient implementation. It was first suggested by Arrow (1979) and d'Aspremont and Gérard-Varet (1979) who also give a formal proof of its properties. The AGV is a direct revelation game in which all individuals send a message from the type space (they can behave like other types but not invent new types). Based on the reports the surplus generated by the project is calculated and the project is implemented if and only if the reported surplus is positive. If individuals report truthfully, this leads to efficient project implementation. To ensure truthful reports, the mechanism calls for transfers equal to the expected externality an individual generates for the others with her reported valuation.[5] This forces individuals to take the expected surplus generated for the other players into account, and makes all individuals residual claimants of a value equal to their expected societal surplus (their own surplus, plus the expected surplus generate for others). Consequently, they should send the message resulting in the highest expected social surplus. Since the mechanism leads to efficient implementation decisions if all subjects report truthfully, this induces truthful reporting of all types. Because it combines incentive compatibility with efficiency and budget balance, the AGV provides the theoretical benchmark to compare other mechanisms to. If it is impossible to switch from a given mechanism to the most efficient mechanism, the AGV, a switch to any other (less efficient) mechanism is unlikely.

The SM mechanism is chosen for two reasons. First, it is a common mechanism used in committee and small group decision making and therefore provides a natural benchmark for the empirical performance of the AGV. Second, the comparison between AGV and SM is the focus of the possibility theorem in Grüner and Koriyama (2012), such that we need it to reproduce the theoretical situation. The other two mechanisms, NSQ and RAND, are chosen to reproduce the comparisons studied in Myerson and Satterthwaite (1983), Schmitz (2002), and Segal and Whinston (2011).

3.3.3 Treatments

In all treatments a uniform distribution over a type space with four possible valuations for the public project is used. We have one treatment with a symmetric and two treatments with skewed distributions. The two skewed treatments differ from the symmetric treatment in the valuation of a single type. The type spaces and distributions used are shown in table 3.1

Table 3.1: Distribution of valuations for public project by treatment

Treatment	Valuations			
symmetric	-3	-1	1	3
right skewed (+7)	-3	-1	1	7
left skewed (-7)	-7	-1	1	3
probability	25%	25%	25%	25%

Notes: Probabilities are the same in all treatments.

below.

Subjects draw a new private valuation for the project in each round and only participate in one treatment. The distribution of private valuations determines the expected payoff for the four mechanisms. In section 3.4 we provide the expected payoffs for all mechanisms in all three treatments and the tested theoretical predictions.

3.3.4 Procedures

The computerized experiments (zTree, Fischbacher (2007)) were conducted in the mLab of the University of Mannheim. Subjects were mostly undergraduate students from the University of Mannheim (recruitment through ORSEE, Greiner (2015)). Each session consisted of 18 rounds with random rematching among subjects.[6] All interactions were anonymous and subjects did not know who they were matched with in any round. To prevent income effects only one randomly selected round was paid in addition to a show up fee of 9€. Each round was equally likely to be chosen for payment and the selected round was identical for all subjects within a session. We conducted 9 sessions with 9 to 24 subjects, resulting in 150 participants (45 in the symmetric, 42 in the right skewed, 45 in the left-skewed treatment and 18 in a robustness check session we describe in section 3.5.5). 85 (57%) subjects were male and the average age of participants was 23 years.

The 18 rounds were split into three six-round blocks: two blocks of ex-ante rounds, rounds 1-12, followed by one block of ad-interim rounds, rounds 13-18. Subjects were aware of the existence of rounds 13-18 at the beginning of the experiment, but were only informed about the difference - the revelation of private valuations before the mechanism choice in the ad-interim rounds - after round 12. In all treatments, subjects made each of the six possible binary mechanism choices once in each block, yielding three choices for each comparison. The order of the pairwise comparisons was randomized within each block and between sessions. Additionally for each binary choice the order of the two mechanisms on the screens of the subjects was randomized between the three blocks. In the next section we state theoretical predictions for all treatments.

[5] The translated instructions for the symmetric treatment, which include a table of the possible transfers, are in section C.1.3 in the appendix.

[6] In sessions with 18 or more participants, subjects were split in two matching groups and could only meet subjects within their own matching group.

3.4 Theoretical predictions

To derive the theoretical predictions for our setting, we assume risk-neutrality and rational behavior in the second stage. Under these assumptions, the AGV is always efficient, and the predictions 2, 4 and 5 depend on this efficiency. For these results to hold without further qualification, the AGV should be ex-post efficient in all distributions, both in the ex ante and ad-interim rounds:

Prediction 1. *The AGV is the most efficient of the four mechanisms.*

In the ex-ante rounds a rational, risk-neutral agent should consider the Bayes-Nash equilibrium of each mechanism and select the mechanism with the highest expected payoff. The payoff-maximizing mechanism depends on the possible private valuations (and their probability distribution, which is common in all treatments) and therefore on the treatment.

Table 3.2 below displays the preference ordering of mechanisms in the ex-ante rounds for each treatment.[7] Because the AGV is the only efficient mechanism, it yields the largest expected payoff in all treatments. In the symmetric treatment a risk-neutral subject should prefer the SM mechanism over NSQ and RAND. For the comparisons between mechanisms with the same expected payoff, e.g. NSQ and RAND in the symmetric treatment, no prediction can be made for risk-neutral agents. However, a small amount of risk aversion would imply a strict preference for NSQ.

Table 3.2: Predicted mechanism choices (ex ante)

Treatment	Ordering of mechanisms						
symmetric	AGV	≻	SM	≻	NSQ	∼	RAND
right skewed (+7)	AGV	≻	SM	≻	RAND	≻	NSQ
left skewed (-7)	AGV	≻	SM	≻	NSQ	≻	RAND

Notes: ≻ and ∼ indicate the preferences ordering of the four mechanisms for a risk-neutral subject. The ordering of mechanisms corresponds to their expected payoffs given the respective treatment.

The relative advantage of the AGV over the SM, measured in the gain in expected payoff, is much larger in the two skewed treatments than in the symmetric treatment. In the symmetric treatment the AGV yields a 6% higher expected payoff than the next best mechanism (SM). This difference is 16% in the right-skewed treatment and it is 81% in the left-skewed treatment.[8]

[7] The calculations for the AGV and for the SM mechanism assume truthful valuation reports (AGV) and sincere voting (SM), both in accordance with their respective Bayes-Nash equilibria. All calculations are in the appendix.

[8] In the symmetric treatment the ex ante expected payoff from the AGV mechanism is 0.53125€, while the SM has an expected payoff of 0.5€, NSQ and RAND both yield an expected payoff of 0. In the right-skewed treatment the expected payoffs are 1.452125€ for the AGV, 1.25€ for SM, 1€ for RAND and 0€ for the NSQ mechanism. In the left-skewed treatment the expected value for AGV and SM is still positive,

By definition, ex ante all subjects are equal. Therefore it follows that the payoff-maximizing mechanism for each subject is also maximizing the expected group surplus. Without private information, payoff maximization should induce subjects to choose the most efficient mechanism, in which case the AGV and SM mechanisms should be preferred over NSQ and RAND. While the AGV should be preferred over SM if the Bayes-Nash equilibrium is played, if the equilibrium is not played the preferred mechanism can depend on the realized efficiency of the two mechanisms.

Prediction 2. *Without private information, all individuals prefer the AGV and the SM over the NSQ and the RAND mechanism.*

In the ad-interim rounds subjects should consider the expected value of each mechanisms given their valuation. Therefore, an individual with a negative valuation of the public project should choose the mechanism with the lowest implementation probability (given the strategies played in the next stage). From this observation the Myerson-Satterthwaite impossibility theorem directly follows. In our setting the NSQ has a zero probability of implementation, it therefore dominates all other options for individuals with a negative project valuation.

Prediction 3. *With private information, individuals with a negative valuation prefer the NSQ over all other mechanisms.*

Table 3.3 shows the order of the expected payoffs in the ad-interim rounds per treatment and valuation, again assuming the Bayes-Nash equilibrium is played.

Table 3.3: Predicted mechanism choices (ad interim)

Treatment	Valuation	Ordering of mechanisms						
symmetric	3	AGV	$\succ$	SM	$\succ$	RAND	$\succ$	NSQ
	1	AGV	$\sim$	SM	$\succ$	RAND	$\succ$	NSQ
	−1	NSQ	$\succ$	SM	$\sim$	AGV	$\succ$	RAND
	−3	NSQ	$\succ$	AGV	$\succ$	SM	$\succ$	RAND
right skewed	7	AGV	$\succ$	SM	$\succ$	RAND	$\succ$	NSQ
	1	AGV	$\succ$	SM	$\succ$	RAND	$\succ$	NSQ
	−1	NSQ	$\succ$	SM	$\succ$	AGV	$\succ$	RAND
	−3	NSQ	$\succ$	SM	$\succ$	AGV	$\succ$	RAND
left skewed	3	SM	$\succ$	AGV	$\succ$	RAND	$\succ$	NSQ
	1	SM	$\succ$	AGV	$\succ$	RAND	$\succ$	NSQ
	−1	NSQ	$\succ$	AGV	$\succ$	SM	$\succ$	RAND
	−7	NSQ	$\succ$	AGV	$\succ$	SM	$\succ$	RAND

Notes: $\succ$ and $\sim$ indicate the preferences ordering of the four mechanisms for a risk-neutral subject. The ordering of mechanisms corresponds to their expected payoffs given the respective treatment and valuation.

0.453125€ (AGV) and 0.25€ (SM), while the expected payoff for the NSQ mechanism remains at 0 and actually is negative, -1€, for RAND.

Schmitz (2002) and Segal and Whinston (2011) show that by replacing the safe outside option with riskier ones, the impossibility problem of prediction 3 can be overcome. In our experiment this translates to the prediction that the AGV should be preferred over RAND, even with private information. Similarly, since with a three-person group and the chosen distributions the SM mechanism is much more efficient than the RAND mechanism, all subjects should choose the SM over the RAND mechanism.

Prediction 4. *With private information*

(i) all individuals prefer the AGV over the RAND mechanism and

(ii) all individuals prefer the SM over the RAND mechanism.

Grüner and Koriyama (2012) demonstrate that individuals can prefer the AGV over the SM, even with a negative valuation, as long as some conditions are met. Mainly because we have odd numbered groups, these conditions do not always hold in our setting. However, their results translate to the following qualified prediction:

Prediction 5. *In the symmetric treatment*

(i) subjects with a private valuation of -3 or +3 strictly prefer the AGV over the SM mechanism,

(ii) subjects with a private valuation of -1 or +1 are indifferent between the AGV and the SM.

In the skewed treatments

(iii) subjects with a private valuation of -3 or -1 (right-skewed treatment) and 3 or 1 (left-skewed treatment), strictly prefer the SM over the AGV,

(iv) while all other subjects prefer the AGV over the SM mechanism.

Additionally we test whether subjects prefer the theoretical optimal AGV mechanism over other, better known mechanisms. If subjects' choices in the ex-ante rounds are sensitive to expected payoff differences, the AGV mechanism should be chosen more often in the skewed treatments than in the symmetric treatment. Furthermore, the AGV transfers in the right-skewed treatment are usually paid by subjects reporting extremely high valuations. This "taxing the winner" property could be seen as fair by subjects, since an individual benefiting strongly from project implementation has to compensate other group members. In the left-skewed treatment this "tax" is levied from the loser(s). If such fairness concerns play a role in mechanism selection, the AGV should be more desirable in the right skewed than the left-skewed treatment. However, we do not believe that the mechanism choices in the ad-interim rounds will be affected, since the known private valuation for the project should make the own payoff consequences of the mechanism choice more focal and therefore fairness concerns might be less relevant.

Prediction 6. *The AGV mechanism is chosen more often* *(i)* *in the left and right-skewed treatment than in the symmetric treatment due to efficiency differences,*

(ii) *in the right-skewed treatment than in the left-skewed treatment, since subjects prefer "winners" rather than "losers" of the project implementation to pay transfers.*

(iii) *Both effects are more pronounced in the ex-ante rounds then in the ad-interim rounds.*

3.5 Results

We present the results in the same order as the predictions, starting with the realized efficiency of the AGV and the SM mechanisms before analyzing subjects' ex-ante choices in more detail. Next we present our findings on the Myerson-Satterthwaite impossibility theorem and then discuss the ad-interim mechanism choices before concluding with an analysis of subjects' behavior in stage two of the AGV (value reports) and the SM mechanism (voting).

3.5.1 Realized surplus

Whether the AGV is actually more efficient than the other mechanisms depends on subjects' behavior and especially on the question whether they truthfully report their type (AGV) and vote sincerely (SM mechanism). Theoretically the AGV is incentive compatible, such that truthful reporting should result in equilibrium. However, we cannot assume perfectly rational expected-value maximization. If subjects misreport their valuation it is not clear whether the AGV actually generates the largest surplus.

We do not use the actual surplus generated in the lab as our measure of efficiency, since this measure is strongly influenced by the realization of private valuations as well as the mechanism choices by the random dictator. Instead, we use the observed distribution of reports/votes made by subjects with a specific type in a treatment, as the behavioral strategy for that type in that treatment. We calculate project implementation probabilities for all permutations of the type vector given these strategies. The displayed surplus (in €) is the expected value of the group surplus in the mechanisms given these strategies and the probability that a particular permutation of the type vector occurs. It is therefore the surplus that would have realized if all possible combinations of private valuations occurred with their expected probabilities and all individuals with the same type used the observed reporting/voting strategy.

Table 3.4 below shows the Bayes-Nash equilibrium surplus and the realized group surplus for the AGV or SM mechanisms in the ex-ante rounds in all treatments.[9] The theoretical surplus of each mechanism is reported in columns 2 (AGV) and 5 (SM), the realized surplus

[9] We concentrate on the ex-ante results, because we have more observations in these rounds than in the ad-interim rounds, which makes the results more reliable. Although they are noisier, results for the ad-interim rounds are qualitatively similar.

in columns 3 and 6, and columns 4 and 7 show the absolute (and relative) surplus loss compared to the theoretically benchmark.

Table 3.4: Theoretical and realized group surplus with AGV and SM (ex ante)

	AGV Group surplus			SM Group surplus		
Treatment	theoretical	realized	lost (%)	theoretical	realized	lost (%)
symmetric	1.59	1.18	0.41 (26%)	1.50	1.34	0.16 (11%)
right skewed (+7)	4.36	3.84	0.51 (12%)	3.75	3.68	0.07 (2%)
left skewed (-7)	1.36	0.93	0.43 (32%)	0.75	0.66	0.09 (13%)

The results in table 3.4 clearly show that the AGV generates a higher expected surplus than the SM mechanism in theory. However, the table also illustrates that neither mechanism reaches its full theoretical efficiency level. In the symmetric treatment the surplus loss of the AGV is so large that the efficiency ordering is reversed. The AGV mechanism only realizes an expected group surplus of 1.18, while SM reaches a surplus of 1.34. In the two skewed treatments the AGV is more efficient than the SM mechanism, but the advantage is smaller than predicted.

These results only partially support prediction 1. Both the AGV and the SM do not perform as well as predicted. The efficiency loss in the AGV is larger than in SM in all three treatments. The AGV is, ex ante, the most efficient mechanism only in the two skewed treatments. In the symmetric treatment the SM mechanism is theoretically very close to optimal, and it slightly outperforms the AGV in terms of efficiency in this setting. While we discuss the reasons for the lower surplus of the AGV mechanism in detail at the end of the results section, it is important to note that the valuation reports suggest that the loss is unlikely to be caused by a small number of confused subjects. Something more systematic seems to be going on.

3.5.2 Ex-ante choices

In this section we analyze subjects' mechanism choices in the ex-ante rounds 1-12. We concentrate on the results of the symmetric treatment. Since most results are not qualitatively different among treatments, we only discuss comparisons between mechanisms in the skewed treatments that are particularly interesting.

The results of all six binary comparisons in the symmetric treatment are shown by the histograms in figure 3.1. In five cases there is a clear majority for one mechanism: AGV and SM are clearly preferred to the NSQ and the RAND mechanism and SM is generally chosen over AGV.[10] There is no clear preference in the choice between the NSQ and RAND mechanism. Subjects are almost evenly split between these mechanisms and a binomial test

[10] Binomial tests confirm a significant difference from a 50:50 split for all comparisons (p-values < 0.01).

does not reject a 50:50 split (p-value 0.46). This indifference is not surprising given the identical expected payoff from both mechanisms. The split between RAND and NSQ seems to indicate risk neutrality of our subjects.

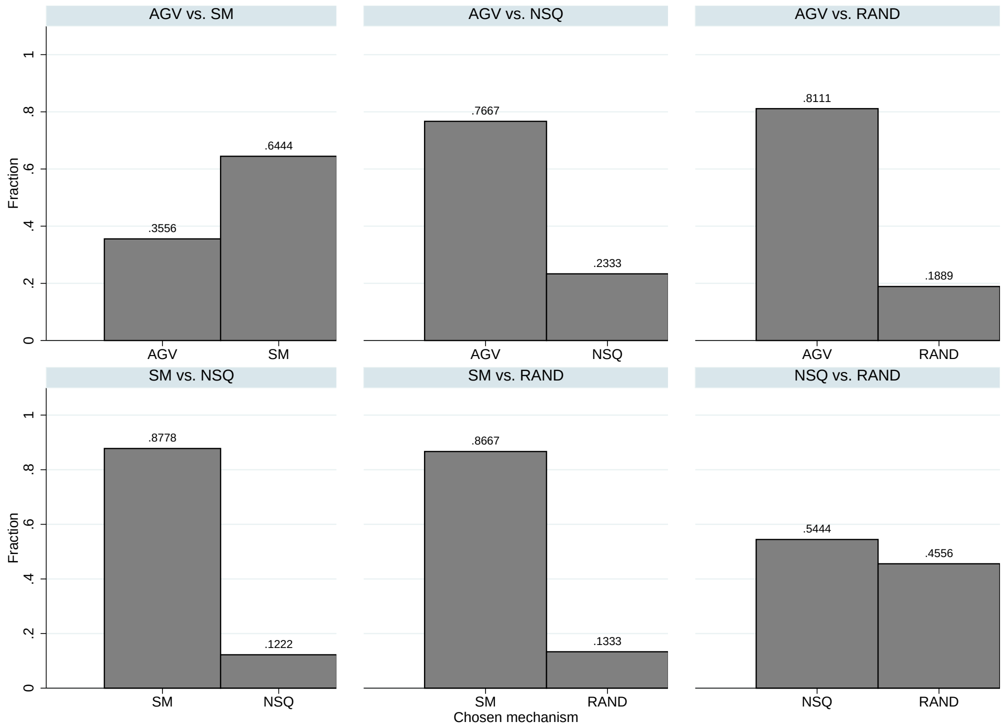

Figure 3.1: Ex-ante binary choices (symmetric treatment)

If we compare the predictions stated in table 3.2 with the results in figure 3.1, we see only one deviation. Only in the choice between AGV and SM the majority of subjects does not prefer the mechanism with the larger expected payoff in theory. However, as we have shown above, within the symmetric treatment the SM mechanism generates more surplus then the AGV mechanism. Therefore, a majority of subjects prefers the mechanism with the largest expected payoff in all six comparisons.[11]

Table 3.5 below shows for all comparisons and treatments the mechanism selected by a majority of subjects in the ex-ante rounds. The prediction that subjects select the most efficient mechanism corresponds to completely unanimous choices in every comparison. While this is not the case, in many comparisons one mechanism is preferred by a large majority. Although the predictions concentrate on individual choices, we present the median choice in this table. Since we are dealing with binary choices and about 80% of the mechanism rankings obtained from individual binary comparisons within a block of 6 rounds satisfy strict transitivity, this aggregation is consistent with the preferences of our 'average' subject.

The modal stated preference goes in the predicted direction for all, but two compar-

[11] Which of the two mechanisms is listed first seems to be without effect. We vary the order of comparisons between the two rounds of comparisons and between sessions, but there are no signs of order effects in any direction. The detailed results for all comparisons in the ex-ante rounds are in the appendix in table C.2.

isons. In the symmetric and the right-skewed treatments the AGV is not preferred to the SM mechanism.

Table 3.5: Mechanisms chosen by a majority of subjects in the ex-ante rounds

Treatment	AGV vs. SM	AGV vs. NSQ	AGV vs. RAND	SM vs. NSQ	SM vs. RAND	NSQ vs. RAND
symmetric	SM**	AGV	AGV	SM	SM	NSQ ∽ RAND
right skewed (+7)	SM ∽ AGV*	AGV	AGV	SM	SM	RAND
left skewed (-7)	AGV	AGV	AGV	SM	SM	NSQ

Notes: The mechanism in each cell was chosen by the majority of subjects in the respective treatment. All results are aggregated over both comparisons (rounds 1-6 and 7-12). The number of observations for the three treatments are: 90 (symmetric), 84 (right skewed) and 90 (left skewed). Binomial tests reject a 50:50 split at the 1%-level for all but two comparisons: NSQ vs. RAND in the symmetric treatment and AGV vs. SM in the right-skewed treatment. A * indicates that the majority choice is not in line with the theoretical efficiency prediction, ** indicates that the choice is in line with realized but not with theoretical efficiency.

We provide the results for the comparison between the AGV and the SM in all treatments in figure 3.2 below. In the symmetric (64%) and right-skewed treatment (58%) a majority chose the SM over the AGV mechanism. This ordering flips around for the left-skewed treatment, in which 70% prefer the AGV. This increase is in line with the increase of the relative advantage of the AGV over the SM mechanism.[12]

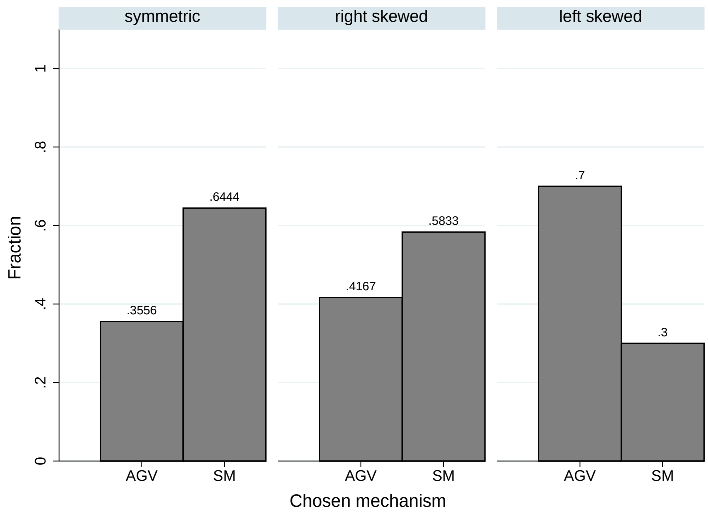

Figure 3.2: Ex-ante choices between AGV and SM mechanism (by treatment)

Comparing subjects' choices with the realized surplus, shows that a majority chooses the mechanism with the highest realized surplus in all comparisons, except in the right-skewed treatment for the comparison between AGV and SM. Therefore, we can conclude that the average mechanism choice of subjects is almost perfectly in line with the ordering predicted by realized efficiency. Since the theoretical predictions about efficiency has almost the same order as the realized efficiency, subjects generally prefer the theoretically most

[12] More subjects prefer the AGV over the SM mechanism in the left-skewed treatment than in the two other treatments (Mann-Whitney-U (MWU) tests, p-values < 0.01) while the difference is not significant between the symmetric and the right-skewed treatment (MWU test, p-value 0.41). For the symmetric and left-skewed treatments a binomial test rejects a 50:50 split (p-values < 0.01). The 50:50 split cannot be rejected in the right-skewed treatment (p-value 0.16).

efficient mechanism in the ex-ante rounds, confirming prediction 2. Note that this means we can already reject all parts of prediction 6. Our subjects do not appear to prefer taxing winners over taxing losers, not even in the ex-ante rounds. Choices seem to follow realized expected value in the lab, rather than any form of other-regarding preferences.

3.5.3 Impossibility results

The Myerson-Satterthwaite theorem predicts that no (efficient) mechanism is unanimously preferred over the non-implementation status quo. Figure 3.3 shows all choices made between NSQ and the other mechanisms in the symmetric treatment.[13] In the top (bottom) row the revealed preferences for the ex-ante (ad-interim) comparisons are shown. For each decision the figure first shows the choices for subjects with a positive and then for those with a negative valuation. Since subjects do not know their valuation when making the mechanism choice in the ex-ante rounds (top row), the choices of the subjects with positive and negative valuations are statistically almost indistinguishable.[14]

The expected choice reversal can be seen by comparing the graphs in each column. The change in choices is obvious in all three comparisons: The AGV and SM are preferred over the NSQ in the ex-ante rounds (top, columns one to four) and the RAND and NSQ mechanism are about equally likely to be chosen by all subjects (top, columns five and six), these choices reverse for virtually all subjects with a negative valuation in the ad-interim rounds. In our experiment subjects with a negative valuation prefer the NSQ over the other mechanism (bottom, columns two, four and six).[15] These results confirm prediction 3: many individuals would prefer not to participate in the efficient group choice mechanism, making unanimous agreement virtually impossible.

The effect of private information can be seen very clearly in the comparison between the RAND and NSQ mechanism (columns five and six). With a symmetric value distribution both mechanisms have a zero expected payoff ex ante and the choices of subjects seem to indicate this "indifference". With private information, however, subjects' revealed preferences are almost perfectly correlated with valuations: NSQ is preferred by subjects with a negative valuation and RAND by those with a positive valuation. Even complete randomness is acceptable, as long as it increases own income (at least in the lab). Unlike the behavior observed by Engelmann and Grüner (2013), in our setting social or efficiency concerns seem

[13] Because we did not find significant differences between the first and second block of ex-ante mechanism choices, we pool these choices in the analysis. Results for the other treatments are very similar.

[14] Of the 9 comparisons (three per treatment) only in the right-skewed treatment for the choice between the NSQ and RAND we find a statistically different mean choice between subjects with a positive and subjects with a negative valuation (p-value of 0.045, MWU test). Since subjects were unaware of their valuation when they made the choice, this difference has to be random. All other comparisons do not yield a significant difference (p-values > 0.10, MWU).

[15] MWU tests show that significantly more subjects with a negative valuation chose the NSQ mechanism in all three comparisons in the ad interim than in the ex-ante rounds (p-values < 0.01).

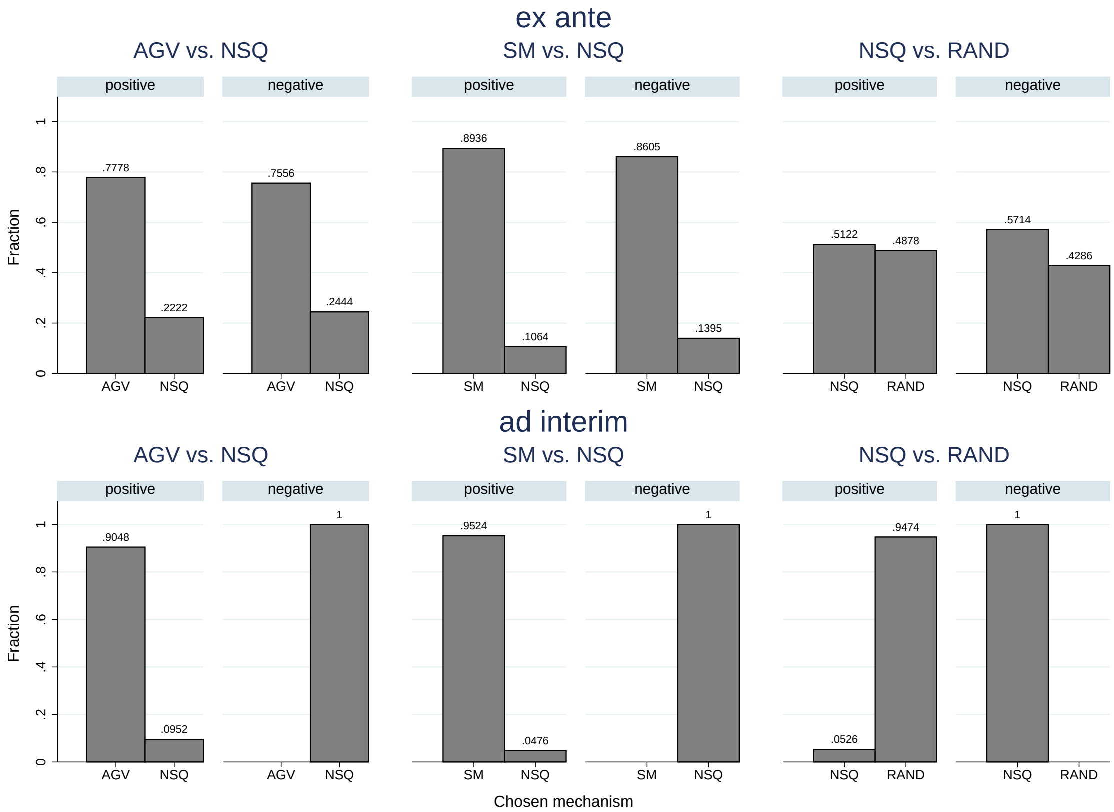

Figure 3.3: Mechanism choice by positive/negative private valuation (symmetric treatment)

not to play a role for subjects' mechanism choices. Our subjects appear almost perfectly rational and narrowly self-interested with respect to their mechanism choice.

3.5.4 Ad-interim choices

We now turn to the results for the other ad-interim comparisons. Prediction 4 states that all subjects should prefer the AGV and the SM over the RAND mechanism, regardless of their valuation. Our results are qualitatively equivalent for the binary comparisons of the AGV vs. the RAND mechanism and the SM vs. the RAND mechanism. In the interest of space we only report the former in figure 3.4.[16] It shows that at the aggregate level the AGV is clearly preferred over the RAND mechanism. As was predicted by Schmitz (2002) and Segal and Whinston (2011), the Myerson-Satterthwaite impossibility theorem can be overcome if the outside option is a risky rather than a save status quo.[17]

The claim in prediction 4 is actually even stronger than a preference for the AGV over RAND on the aggregate level, since it predicts a preference for the AGV by all types. This stronger prediction is also confirmed by our data, with one minor exception. In the left-skewed treatment the AGV and RAND mechanism are equally often preferred by individuals with type +3: exactly 50% chose the AGV. In all other treatments and for all other valuations,

[16] We show the results of all ad-interim choices separately for treatments and private valuations in table C.3 in the appendix.

[17] Binomial tests reject an equal distribution in all treatments (p-values < 0.01).

the AGV receives at least 60% of all votes and in most cases it is chosen by a larger margin.[18] Our results therefore clearly confirm prediction 4.

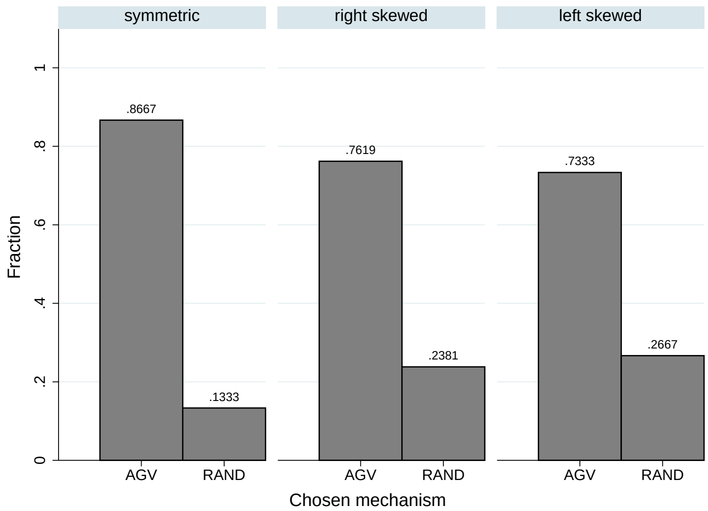

Figure 3.4: Ad-interim choices between AGV and RAND mechanism (by treatment)

The revealed preferences of subjects for the ad-interim choice between the AGV and the SM mechanism are shown in figure 3.5 per treatment and type. Although the statements made in prediction 5 are the most sensitive to the small number of observations in some cells, the comparative statics are largely borne out by the data. In the symmetric treatment, preference for the AGV is more pronounced for the types -3 and 3 then for the types -1 and 1. Similarly, the preference for the AGV seems to increase with the valuation in the right-skewed treatment, and decreases with valuation in the left-skewed treatment. The only exception being the -3 type in the right-skewed treatment.

While the AGV mechanism is preferred by all subjects with the most extreme private valuations (+7 and -7), subjects with a valuation of +/-1 are almost evenly split between the mechanisms. The clear preference for the AGV of subjects with an extreme valuation is not only in line with the prediction, it is also an indication that subjects understood that in the AGV mechanism an extreme valuation report is equivalent to certain implementation (+7), respectively a veto against implementation (-7). Since subjects in the skewed treatments like the AGV mechanism less than predicted, prediction 5 is not fully confirmed.

The largest deviations from the prediction seems to stem from the fact that the AGV is not always as efficient in the lab as predicted by theory. This was caused by subjects' second stage reporting (AGV) and voting (SM) strategies, which are analyzed in the next section.

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[18] While the results appear clearly in the appropriate graphs, formal tests cannot confirm the results at the common significance levels for the different private valuations, since the low number of cases (8-15 per valuation and treatment) results in relatively high p-values even if 60% or more selected the AGV mechanism.

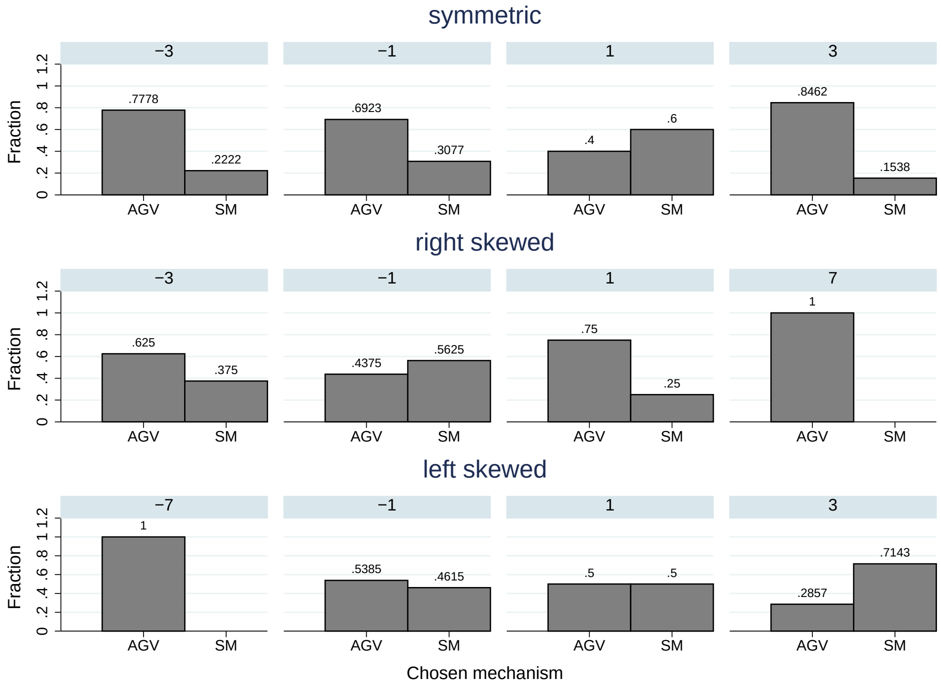


Figure 3.5: Ad-interim choices between AGV and SM mechanism (by treatment and valuation)

3.5.5 Voting and reporting behavior

In section 3.5.1 we showed that both the SM and AGV mechanism perform below theoretical expectations. In this section we use the individual reports (AGV) and votes (SM) to analyze the reasons for this efficiency loss.

In the AGV truthful reporting forms a Bayes-Nash equilibrium. To make sure that our subjects were aware of this, our subjects were told that if the other subjects report truthfully, it maximizes their expected payoff to report their true valuation as well. However, there is no guarantee that subjects understand and act in accordance with those statements, let alone that they believe others do. Table 3.6 shows three tables, one for each treatment, with the reported valuations as a function of private valuations for the ex-ante rounds in which the AGV mechanism was used.

If all subjects reported their true valuation all entries should be on the main diagonal of the tables. However, as all the off-diagonal elements show, many subjects misreport. We consider two types of false reports separately. Over- or under-reporting is defined as sending a report that is more (or less) extreme than the subjects' true valuation but has the same sign. This kind of reporting can be caused by the desire to ensure (non-)implementation or avoid paying transfers. Misreporting the sign of the valuation, e.g. reporting +1 with a valuation of -1, is of a different caliber. There is no reason to misreport the sign of the valuation if a subject is maximizing her expected payoff. A subject with a negative valuation should not want the project to be implemented. By reporting a positive valuation she increases the probability of implementation, which can never be optimal. Remember that no implementation results

Table 3.6: AGV reports (ex ante)

(a) symmetric treatment

true valuations	reported valuations 3	1	−1	−3	Total
3	41	7	0	0	48
1	16	28	1	0	45
−1	1	3	28	16	48
−3	4	6	7	25	42
Total	62	44	36	41	183

(b) right-skewed treatment

true valuations	reported valuations 7	1	−1	−3	Total
7	43	1	0	0	44
1	13	21	1	0	45
−1	3	8	11	24	46
−3	6	5	5	37	53
Total	65	43	17	61	183

(c) left-skewed treatment

true valuations	reported valuations 3	1	−1	−7	Total
3	35	10	1	0	46
1	23	36	0	0	59
−1	1	5	35	14	55
−7	4	3	2	53	62
Total	63	54	38	67	222

in a zero payoff from the project for all group members, independently of the valuation. The same argument, with reversed signs, holds for positive valuations. Therefore, while over- or under-reporting can be rationalized (at least to some extent) by small mistakes, misreporting the sign of the valuation cannot.

Table 3.6b shows that the reports that involve an incorrect sign in the right-skewed treatment are concentrated on subjects with a negative valuation. Only one of the false reports of subjects with a positive valuation misreports the sign. In striking contrast, 22 (43%) of subjects' reports with a negative valuation include an incorrect sign. This pattern is not limited to the right-skewed treatment, see tables 3.6a and 3.6c, or the ex-ante rounds (not reported). This pattern is not caused by a few individuals. In all three treatments about 30% of reports differ from true valuations and 25% of subjects incorrectly report the sign of their valuation at least once. The 30% misreported values are spread over a large share of the subject population, such that we believe that confusion is not the main reason for the reports.

These averages are also quite stable over rounds. E.g. conditioning on the first 6, first 12 or last 6 periods (ad-interim choices), yields similar percentages of misreports. Learning either does not happen, or again, the underlying behavior is not driven by confusion. However, there does appear to be a significant difference between subjects who select the AGV and those who favor the alternative mechanism. The first group is statistically less likely to misreport the sign of their valuation (Chi-Square test, p-value < 0.01, using AGV reports for all ex-ante rounds). However, sign misreporting takes place in both groups.

Given these results, we ran an additional session that eliminates most reasons for misreporting as a robustness check. Again, the only difference between this treatment and the others is the type space. Private valuations were drawn from the set {-3 €, -2 €, -1 €, 7 €}.

These valuations result in identical transfers and implementation probabilities for all negative reports. Therefore under- or over-reporting has no effect on payoffs. Furthermore, all valuations had a unique absolute value, decreasing the probabilities of accidentally selecting -1 rather than +1 and vice versa. The AGV reports in the ex-ante rounds of this session are shown in table 3.7.

While we still see reports with an incorrect sign, these are less frequent than could be expected if the subjects played a similar strategy as in the other treatments. Some of the misreported signs are likely to have been mistakes. In this session all reports of a negative value have the same effect on the implementation probability of the project and result in identical transfers. The fraction of over-reporting subjects with a negative valuation seems to indicate subjects understood this. Given these observations and the fact that in all sessions private valuations are random, change from round to round, and the experimental screens showed the sign of the valuation (both the + and the - sign), we have no explanation for the pattern we find.

Table 3.7: AGV reports (ex ante) in the robustness session

true	reported valuations				
valuations	7	−1	−2	−3	Total
7	19	1	1	0	21
−1	1	9	4	11	25
−2	1	0	8	8	17
−3	0	0	0	15	15
Total	21	10	13	34	78

Notes: As in the other treatments the probability of each value is 25%.

In order to approximate the loss in expected group surplus that is caused by the two different types of false reports we repeated the calculations of table 3.4 after excluding over- and under-reporting, and misreported signs respectively. Table 3.8 shows both the original (columns 4-5) and the adjusted results. Comparing the adjusted efficiency without misreported signs (columns 6-7) with the adjusted efficiency without under- and over-reporting (columns 8-9) shows that most efficiency is lost through the falsely reported signs. Depending on the treatment between 11% (right-skewed treatment) and 23% (symmetric treatment) of the theoretical group surplus is lost due to valuation reports with an incorrect sign.[19]

Unlike the reports in the AGV mechanism, the voting behavior of subjects is very close to theoretical predictions and almost perfectly rational. For all treatments and private valuations, subjects vote according to their valuations in 89% to 100% of the rounds. There is no

[19] The sum of the surplus from both types of false reports does not sum up to the difference between the theoretical and realized group surplus, since both types of misreports can occur together and thus interact in the realization of actual efficiency. The total surplus loss due to the misreporting can thus be higher or lower than sum of the separate calculations.

Table 3.8: Group surplus loss in the AGV under different types of false reports (ex ante)

Treatment	All reports correct theoretical	All reports realized	All reports lost (%)	Effect of over- / under-reports adjusted	Effect of over- / under-reports lost (%)	Effect of sign misreports adjusted	Effect of sign misreports lost (%)
symmetric	1.59	1.18	0.41 (26%)	1.46	0.13 (8%)	1.22	0.37 (23%)
right skewed (+7)	4.36	3.84	0.51 (12%)	4.12	0.24 (6%)	3.88	0.48 (11%)
left skewed (-7)	1.36	0.93	0.43 (32%)	1.12	0.24 (18%)	1.08	0.28 (21%)

Notes: The columns for *Effect of over- / under-reports* remove all reports with a false sign from the behavioral strategy of the subjects. Similarly, in the columns *Effect of sign misreports* all reports that over- or under-report the true value are not considered. The *lost* columns state the absolute (relative) group surplus loss with regard to the theoretical group surplus under truthful reporting.

pattern of incorrect votes in relation to the sign of the valuation. Subjects are about equally unlikely to vote against their private valuations for positive and negative valuations.

The different rates of rational reporting/voting drive the relatively small realized efficiency advantage of the AGV over the SM mechanism. Especially the incorrectly reported signs result in large efficiency losses of the AGV. The higher percentage of misreports in the AGV compared to the non-sincere votes in the SM mechanism can be partially explained by larger familiarity of subjects with the SM. However, the systematic difference in the reporting behavior of individuals with positive and negative types is unlikely to be explained by mistakes alone. Up to this point we have no explanation for this difference.

3.6 Conclusion

This paper presents the results of a first experimental study on the effects of private information and outside options on mechanism selections in a group decision experiment. Our results on the ex-ante preferences in all treatments demonstrate that subjects are aware of the efficiency differences between the mechanisms. In almost all cases a clear majority of subjects selects the mechanism that is more efficient in the lab. Not too surprisingly, if the difference in efficiency between two mechanisms is small, results are less clear.

The behavior in the ad-interim rounds also largely confirms related theoretical predictions. As the Myerson-Satterthwaite impossibility theorem predicts, the same subjects who prefer the efficient AGV mechanism ex ante, suddenly opt for the complete inertia of the non-implementation status quo after learning their private valuation. Similarly, most subjects prefer the AGV over flipping a coin (RAND) even after learning their private valuation, as predicted by Schmitz (2002) and Segal and Whinston (2011). Our data is less clear about the predictions made by Grüner and Koriyama (2012) regarding the choice between AGV and SM. Although the overall pattern seems to conform to the theoretical predictions, efficiency differences between these mechanisms are small and clear majorities for either AGV or SM often do not exists.

These results highlight the importance of participation constraints in the design of institutions. In many situations it is impossible to set an efficient decision rule ex ante. Also it might not be possible to establish an efficient mechanism through a decentralized and completely voluntary procedure ad interim. This combined impossibility touches upon one of the most fundamental questions in mechanism design, political economy, and more generally political philosophy. Since participation constraints already create problems in the small groups in our experiment, the difficulties of negotiating a public project on the scale of a nation would seem close to unsurmountable if unanimity (or completely voluntary participation by all parties) is required. A group that is stuck in an inefficient mechanism might require an outside influence or coercive power to break away from the status quo. Centralized organizations with an amount of coercive power, like the state or the company, are able to force participation and thus avoid these problems. In doing so, these organizations allow participants to bundle individual projects and reforms and take them away from purely decentralized mechanisms like open markets. Often the gains in efficiency from extra investment in common projects are large enough to compensate participants for their involvement in projects that are not individually rational to them. In this sense, our findings give one reason for the existence of states. Although a centralized state might not be as efficient in dealing with incentive constraints as the market, it does make dealing with the participation constraints on individual projects a lot easier.

Because our subjects play all mechanisms, we can compare the relative efficiency of the AGV and SM mechanism on the same group of subjects. The SM mechanism is almost as efficient in the lab as theoretical calculations with rational self-interested agents predict. The AGV is perfectly efficient in theory, but loses a lot of its efficiency in practice due to false reports. In our experimental results we find a puzzling pattern in the reporting strategy used by subjects in the AGV. While both subjects with positive and negative valuations sometimes over- or under-report their valuation, only subjects with a negative valuation systematically misreport the sign of their valuation. These valuation reports with an incorrect sign account for most of the efficiency loss of the AGV in our experiment. Interestingly, this pattern is present in all treatments and does not seem to be driven by the behavior of a few individuals. Subjects in our experiment gather some experience in the AGV, but not too much. Depending on the random allocation of private valuations, a subject might never experience the real advantage of the AGV over the much more common SM mechanism. In order to have a "fair" comparison, it might be necessary to provide subjects with more opportunities to learn how the AGV actually works and to demonstrate why the mechanism is more efficient. Given that we do not familiarize our subjects with the AGV in this manner, it is actually quite remarkable how often the AGV is chosen. Still our findings indicate that there is room for further research in the area of efficient mechanism implementation.

Our setup allows us to vary individual participation constraints and to apply strong within-subject tests of theoretical predictions. The crispness of the results obtained is a clear indication of the strength of this methodological setup. We believe the method by which participation constraints are implemented, measured and varied in this experiment could be fruitfully applied to experimentally investigate other questions surrounding participation constraints.

Appendix C

Appendix Chapter 3

C.1 Appendix

C.1.1 Derivation of predictions

Predictions 1 and 2

Note that all mechanisms generate as much surplus as is generated by the common investment, as the rest of the (experimental) budget is ex-post balanced. From the four mechanisms, the AGV mechanism is the only mechanism that implements (in Bayes-Nash equilibrium) the project if and only if the generated surplus is larger than 0. The other mechanisms all have an efficiency loss from wrong implementation, or wrong non-implementations and therefore are less efficient in expectation. These differences in efficiency imply prediction 1 and the preference of individuals without private information for the AGV over NSQ and RAND mechanism in prediction 2. The SM mechanism implements if and only if at least two people vote in favor. If we assume that individuals vote in favor if they have a positive valuation and against if it they have a negative valuation, we can see that when the loss of efficiency in implementation occurs. In the symmetric treatment this happens in two cases (type vectors {-1,-1,3} and {1,1,-3}), both of which cost 1€ and occur with a probability of 0.046875, such that the expected loss of the SM mechanism relative to the efficient outcome is 0.09€, or 5.88% of the maximum efficiency.

In the right-skewed treatment with the +7 value there are four cases of inefficient implementation, type vectors {-3,-3,7}, {-3,-1,7}, {-3,1,1} and {-1,-1,7}, occurring with probabilities 0.046875, 0.09375, 0.046875 and 0.046875 respectively. The expected loss is 0.61€, or 13.98% of maximum efficiency. In the left-skewed treatment with the -7 value there are four cases of inefficient implementation, type vectors {1,1,-7}, {3,1,-7}, {3,-1-,1} and {3,3,-7}, occurring with probabilities 0.046875, 0.09375, 0.046875 and 0.046875 respectively. The expected loss is also 0.61€, but this is 44.82% of maximum efficiency in this setting, since the maximum efficiency delivers a much lower surplus.

The RAND mechanism has a zero expected surplus for the symmetric treatment, a -1 expected surplus in the left-skewed treatment (-7), and a +1 expected surplus in the right-skewed treatment (+7). The loss of efficiency of the NSQ is a 100% always. Since the efficiency loss in the SM mechanism is always lower than the loss in the NSQ or RAND mechanism, this proves prediction 2.

Prediction 3

With known private values v_i, individuals can calculate their expected utility as a function of mechanism Γ:

$$E(U) = v_i * \Pr(\text{Y=1} \mid \Gamma = \text{M}).$$

With $Y = 1$ denoting implementation and $M \in \{\text{NSQ}, \ \text{RAND}, \ \text{AGV}, \ \text{SM}\}$. With a negative private value, v_i, the best choice is the one with lowest probability of implementation. Since Pr(Y=1 | Γ = NSQ) = 0, the NSQ (weakly) dominates all other mechanisms for these individuals.

Predictions 4 and 5

For the AGV assume that individuals report truthfully in the second stage when playing AGV, and similarly for SM vote in favor in case of positive valuation and against otherwise. Each individual should then choose the mechanism that maximizes her expected payoff, which for SM is as before:

$$E(U) = v_i * \Pr(\text{Y=1} \mid \Gamma = \text{M}).$$

In the AGV the expected payoff is additionally influenced by the expected transfer each individual has to pay. Since the individuals possess private information, this can be either positive or negative. It is straightforward, albeit somewhat tedious, to calculate the expected utility of each type for each of the three mechanisms in all treatments. The results are displayed in table C.1 below.

Table C.1: Expected utility by type and mechanism

	Mechanism				
		AGV		RAND	SM
type	symmetric	right skewed	left skewed	all treatments	
−7			−0.60417	−3.5	−1.75
−3	−0.6875	−1.16667		−1.5	−0.75
−1	−0.25	−0.27083	−0.14583	−0.5	−0.25
1	0.75	0.854167	0.760417	0.5	0.75
3	2.328125		1.885417	1.5	2.25
7		6.401042		3.5	5.25

Like it was shown more generally by Segal and Whinston (2014), no single type prefers to flip a coin over playing the AGV (or SM in this case). For the predictions of Grüner and Koriyama (2012) we have a slightly more qualified result. In the skewed treatments the types -3 and 3 prefer the SM mechanism while the other types {-7, -1, 1, 7} prefer the AGV mechanism. In the symmetric treatment the types -1 and 1 are indifferent, while the types -3 and 3 prefer AGV.

C.1.2 Further results - All choices

In table C.2 the results for all binary comparisons in the ex-ante rounds (round 1: 1-6, round 2: 7-12) are shown. The mechanism stated in each cell is the mechanism chosen by a majority of subjects for the binary comparison in this column. E.g. the 69% in the row *'symmetric treatment, round 1'* in the third column *(AGV vs. SM)* mean that 69% of subjects chose the SM over the AGV mechanism (consequently 31% chose the AGV mechanism) in the first comparison of these mechanisms.

Table C.2: Percentage of subjects who chose each mechanism in the ex-ante rounds

Treatment	# of subjects	Binary choice AGV vs. SM	AGV vs. NSQ	AGV vs. RAND	SM vs. NSQ	SM vs. RAND	NSQ vs. RAND
symmetric							
round 1	45	SM (69%)	AGV (78%)	AGV (76%)	SM (89%)	SM (89%)	RAND (53%)
round 2	45	SM (60%)	AGV (76%)	AGV (87%)	SM (87%)	SM (84%)	NSQ (62%)
right skewed (+7)							
round 1	42	SM (55%)	AGV (81%)	AGV (79%)	SM (90%)	SM (88%)	RAND (74%)
round 2	42	SM (62%)	AGV (83%)	AGV (90%)	SM (90%)	SM (88%)	RAND (69%)
left skewed (-7)							
round 1	45	AGV (69%)	AGV (78%)	AGV (82%)	SM (73%)	SM (93%)	NSQ (60%)
round 2	45	AGV (71%)	AGV (73%)	AGV (82%)	SM (60%)	SM (93%)	NSQ (69%)

Notes: The mechanism named in each cell was chosen by the majority of subjects (percentage). Each subject made a choice in each round, therefore the number of subjects for the three treatments are: 45 (symmetric), 42 (right skewed) and 45 (left skewed).

In table C.3 the mechanism that was chosen by the majority of subjects for each binary comparison in the ad-interim round of all treatments is listed. The table reports the proportions of subjects for each valuation, e.g. the cell in the row *'symmetric, 3'* and second column *(AGV vs. SM)* states that 11 of 13 subjects with a valuation of +3 chose the AGV mechanism over the SM mechanism (consequently 2 of 13 subjects selected the SM mechanism).

Table C.3: Proportion of subjects who chose each mechanism in the ad-interim rounds

Treatment / Valuation	Binary choice AGV vs. SM		AGV vs. NSQ		AGV vs. RAND		SM vs. NSQ		SM vs. RAND		NSQ vs. RAND	
symmetric												
3	AGV	(11/13)	AGV	(10/11)	AGV	(7/8)	SM	(10/11)	SM	(11/12)	RAND	(10/10)
1	SM	(6/10)	AGV	(9/10)	AGV	(9/11)	SM	(10/10)	SM	(9/12)	RAND	(8/9)
−1	AGV	(9/13)	NSQ	(5/5)	AGV	(12/14)	NSQ	(14/14)	SM	(10/11)	NSQ	(11/11)
−3	AGV	(7/9)	NSQ	(19/19)	AGV	(11/12)	NSQ	(10/10)	SM	(8/10)	NSQ	(15/15)
right skewed (+7)												
7	AGV	(6/6)	AGV	(10/12)	AGV	(9/10)	SM	(14/14)	SM	(5/7)	RAND	(10/11)
1	AGV	(9/12)	AGV	(10/11)	AGV	(12/15)	SM	(11/11)	SM	(13/14)	RAND	(10/12)
−1	SM	(9/16)	NSQ	(9/10)	AGV	(5/7)	NSQ	(8/9)	SM	(11/13)	NSQ	(11/12)
−3	AGV	(5/8)	NSQ	(8/9)	AGV	(6/10)	NSQ	(8/8)	SM	(6/8)	NSQ	(7/7)
left skewed (-7)												
3	SM	(10/14)	AGV	(7/8)	AGV	(5/10)	SM	(16/16)	SM	(6/9)	RAND	(14/14)
1	AGV	(5/10)	AGV	(11/11)	AGV	(9/12)	SM	(17/17)	SM	(9/11)	RAND	(8/9)
−1	AGV	(7/13)	NSQ	(6/9)	AGV	(7/10)	NSQ	(5/5)	SM	(9/13)	NSQ	(6/8)
−7	AGV	(8/8)	NSQ	(17/17)	AGV	(12/13)	NSQ	(7/7)	SM	(11/12)	NSQ	(14/14)

Notes: The mechanism named in each cell was chosen by the majority of subjects with the specified valuation (number of subjects who chose the stated mechanism/total number of subjects with given valuation). Each subject makes each binary choice one time with a randomly drawn valuation. For each treatment the sum of choices of all four valuations within a binary comparison is the number of subjects: 45 in symmetric, 42 in right skewed and 45 in left-skewed treatment.

C.1.3 Translated instructions

This is the translation of the original instructions used for treatment one (symmetric distribution). The instructions for other treatments only differ with respect to the described distribution and therefore the used examples and tables. All emphasizes are in the original. The original instructions for all treatments are available from the authors upon request.

Instructions

Thank you for taking part in this experiment. The amount of money you can earn in this experiment depends on your choices and the choices of the other participants. It is therefore important that you understand the instructions. Please do not communicate with the other participants during the experiment. If you have any questions after reading the instructions, please raise your hand. We will then clarify your question.

All the information you provide will be treated anonymously.

You will begin the experiment with a starting budget of 9€. This amount can be increased or decreased depending on all participants' choices in one of the 18 rounds of this experiment. In each round each participant receives a payment. This payment can be zero, positive or negative. At the end of the 18 rounds, one round will be randomly determined for payment. The payment of the selected round will be added to or subtracted from your starting budget. The sum of your starting budget and the payment of the selected round yields your final payoff. In each round you should act as if the round was selected for payment. You will receive your final payoff in cash at the end of the experiment. The payments are chosen in such a way that you cannot make losses under any circumstances. Each participant can earn between 5.75€ and 12.25€. Your payment will be treated anonymously.

The entire experiment is organized in two phases. Phase I consists of rounds 1-12 and phase II of rounds 13-18. You will now receive information about phase I. We will explain any changes in phase II after round 12, but before the start of round 13 (the start of phase II).

Thank you for participating.

STRUCTURE OF THE EXPERIMENT

In each round of the experiment you will be part of a group with 3 members (you and two randomly selected other participants). Each group has the possibility to conduct a project, called project A. If you do not conduct the project each group member receives a payoff of 0€ for this round. If your group conducts project A, then each group member receives his or her private valuation for the project as payment for this round. The private valuation of project A can be different for each member of your group. If your group decides not to conduct project A, all group members receive a payoff of zero. The valuation for project A is newly determined each round and each participant receives a new private valuation in each round. Groups are newly formed in each of the 12 rounds.

The experiment is computer based. Therefore individual participants cannot identify the other group members. You will not know which other participants are in your group in which round, neither during nor after the experiment.

One round consists of two parts. In the first part each group choses a decision rule which is used to determine whether project A is implemented or not. In the second part your group uses the selected rule to determine whether project A is implemented or not. You will be informed about your private valuation for project A **after** part one of a round. We will now describe the two different parts of each round as well as the possible decision rules in detail.

PART ONE

In part one you have the choice between two different decision rules, which will be used in part two to determine whether project A is implemented or not. The two available rules change from round to round. **Each of the three group members suggests one of the two available rules for part two of this round. The computer randomly picks one of these suggestions as group rule. This decision rule determines how in part two the question whether project A is implemented or not is resolved.** The different rules are explained below. In part one you do not know whose rule suggestions will be the group decision rule. Your suggestion can be selected, but also the suggestion of another group member. Each group member has the same chance in each round for his or her suggestion to be selected. Non selected suggestions will not be made known to the other group members. Please note that the decision rule is important, because dependent on the decision rule the implementation of project A is easier or more difficult.

PART TWO

In part two the selected decision rule is used to determine whether project A is implemented or not. The group decision arises directly from the decisions of all group members in part two. The decision is announced and each participant is informed about his or her payment in this round.

VALUATIONS

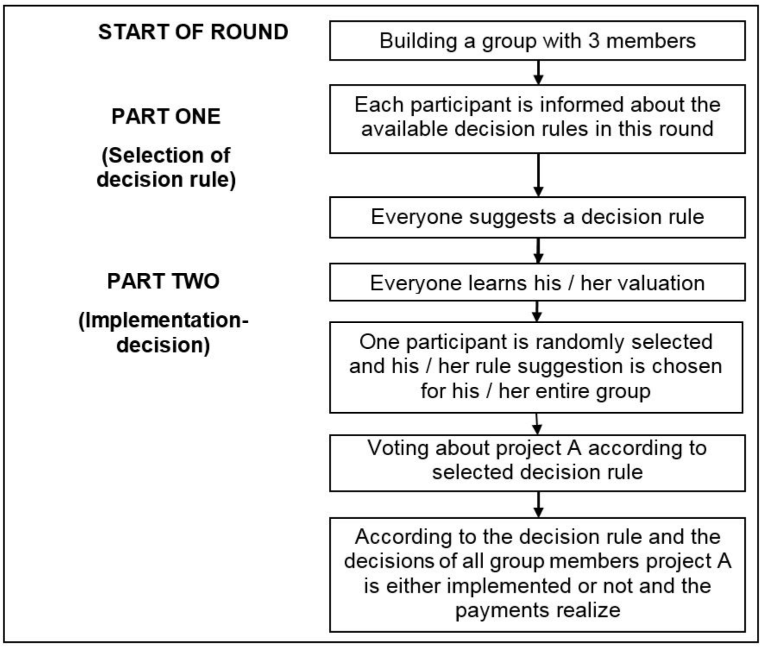
In case project A is implemented all group members receive a payment dependent on their project valuations. This means, if your valuation for project A is positive, you benefit from the implementation of project A, and when your valuation for project A is negative, then you have to pay if the project is implemented. **Your valuation for project A is randomly given to you in each round anew. You learn your valuation after part one.** Therefore you do not know your valuation when you decide between the different decision rules in part one, but you know your valuation in part two, when you decide about the implementation of project A according to the selected decision rule.

Please note that you will know your exact valuation for the project, but not the valuations of the other group members. The valuation of each group member can be -3€, -1€, +1€ or +3€. All values are equally likely. The values are independently distributed, such that your valuation in one round does not allow any conclusions for the valuation of other members in your group. Furthermore your valuations are independent between rounds. Therefore your valuation in one round does not depend on previous or future valuations.

Example: Assume your valuation in round 1 is -1€ and +3€ in round 2. If your group decides to implement project A in both rounds, then your payment (not necessarily your final profit) in these rounds is your valuation. If round 1 would be randomly selected for payment, then your final profit in the experiment would be 8€ (=9€ - 1€). If round 2 would be selected your final profit would be 12€ (= 9€+3€).

If your group does not implement project A, each group member receives 0€ for this round, meaning in this round you neither gain nor lose anything, independently of your valuation for project A. Therefore if such a round is selected for payment, your final profit is your starting budget of 9€.

Here is the structure of the experiment in a short overview:



POSSIBLE DECISION RULES

In part one each group member has the choice between two decision rules. The rules are identical for all group members in each round. The following four decision rules (I.-IV.) are possible:

Rule I. Whether project A is implemented or not depends on the stated valuations of all group members. With this decision rule each group member states his or her valuation for the project in part two of the round. **If the sum of all stated valuations is larger than 0, then project A is implemented. If the sum is smaller, the project is not implemented.** Each participant has to state a possible valuation (-3€, -1€, +1€ or +3€). He can state his true valuation, but also any other possible valuation. The calculation of the sum only depends on the three **stated valuations**. The true valuations are not taken into account.

With this decision rule there are transfer payments between the group members additionally to the payments from an implementation of project A. The transfer payments depend on the stated valuation and the stated valuations of the other group members. You can see which transfers you receive / pay dependent on the stated valuations in table 1 below. Please note: A transfer payment is independent of your true valuation and the implementation of project A. You can also receive or pay a transfer if project A is **not** implemented. Transfer payments **only** exist in this decision rule.

Transfers are chosen in such a way that your expected payoff is maximized if you state your true valuation and also the other group members state their true valuation. The table states the transfers for all possible situations. The first column contains your statement and the respective columns to the right list the transfers dependent on the statements of the other group members.

	Stated valuations of the other group members:									
Your statement:	**3, 3**	**1, 3 or 3, 1**	**-1, 3 or 3, -1**	**-1, 1 or 1, -1**	**-1, -1**	**3, -3 or -3, 3**	**1, -3 or -3, 1**	**-1, -3 or -3, -1**	**1, 1**	**-3, -3**
3	0	-0.125	-0.125	-0.25	-0.25	0	-0.125	-0.125	-0.25	0
1	0.25	0.125	0.125	0	0	0.25	0.125	0.125	0	0.25
-1	0.25	0.125	0.125	0	0	0.25	0.125	0.125	0	0.25
-3	0	-0.125	-0.125	-0.25	-0.25	0	-0.125	-0.125	-0.25	0

Table 1

Example 1: Assume you state a valuation of -1€. If the other two group members state valuations of -1€ and 3€, then you receive a transfer of 0.125€.

Example 2: Assume you state a valuation of 1€. If the other two group members state valuations of -3€ and 3€, then you receive a transfer of 0.25€.

Example 3: Assume you state a valuation of -3€. If the other two group members state valuations of -1€ and 3€, then you receive a transfer of -0.125€. Therefore you have to pay 0.125€.

Example 4: Assume you state a valuation of 3€. If the other two group members state valuations of -3€ and -3€, then you receive a transfer of 0.

Please note that transfers payments are always made, independent of whether project A is implemented or not. You receive / pay a transfer **on top** of the payments from project A.

Rule II. At least two group members have to vote for the implementation of project A. In part two all group members vote either for or against the implementation of project A. At least 2 group members have to vote for the implementation, otherwise project A is not implemented **(simple majority)**.

Rule III. Project A is never implemented. Group members do not make any further statements in part two. There is no voting and no valuations are stated.

Rule IV. The decision for or against implementation of project A depends on the result of a coin flip. There is no voting. If the coin flip results in HEADS, the project is implemented. If the result is TAILS, the project is **not** implemented. Both results, HEADS and TAILS, are equally likely. Therefore with rule IV. project A is implemented in 50% of all cases and not implemented in the other 50%.

Please note that in decision rules I and II each participant has to state a valuation / vote. It is not possible to abstain.

We now ask you to answer several understanding questions regarding the various decision rules and your possible payments. Please answer these questions on the computer screen. After all participants have answered the seven understanding questions all participants will take part in four practice rounds. In each round you will apply one of the four possible decision rules (I.-IV.). In these rounds there is no choice between two rules, but the rule is predetermined.

In these four rounds you are not in a group with two other participants. The computer simulates the decisions of your group members. The computer randomly choses between all available actions. E.g. with rule II the computer will vote "YES − implement project A" in 50% of all cases and "NO − do not implement project A" in the other 50%.

These four rounds do **not** count towards your final profit. They are just meant to familiarize you with the four possible decision rules. After all participants have completed these four rounds the actual experiment starts.

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