

# ESSAYS ON BIDDING BEHAVIOR IN AUCTIONS

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# Chapter 1

## Introduction

This thesis presents a collection of studies that use laboratory experiments to investigate how changes in the rules of an auction or its environment can affect its outcome. The auctions studied vary from a simple auction-like setting, where the optimization problem is an individual-choice task, to a complex market with sequential multi-unit auctions and opportunities for resale. The experimental results indicate limitations of existing theories or provide helpful insights for the market designer.

In the ancient times auctions were used for selling wives and slaves, mine concessions, war booty, and various kinds of commodities (Klemperer 2004). More recently they have become an omnipresent trading instrument used in C2C transactions (usually via online auction platforms), private and public procurement of goods and services, as well as regulated markets, such as those for electricity and for emission permits (Klemperer 1999; Krishna 2002). This surge in the application of auctions in a variety of context has a number of explanations, including the rapid development of information technologies, advances in the fields of game theory and experimental economics, as well as the willingness of politicians to adopt auctions instead of subjective competitive hearings and inherently inefficient lotteries that were previously used to regulate the allocation of goods and services (Kittsteiner and Ockenfels 2006; Milgrom 2004).

However, given the well established theoretical, empirical, and anecdotal evidence about the way even subtle changes in the auction context or auction

rules can affect its outcome<sup>1</sup>, the spread of auctions has created not only opportunities but challenges, as well. While information technologies have decreased the costs of conducting and participating in an auction substantially (Greiner et al. 2012), by resolving the necessity for physical presence of the bidders in online auctions, they have created a new strategic environment, where the number of competitors as well as their characteristics are surrounded by uncertainty (Chen, Katuscak and Ozdenoren 2007). Furthermore, the proliferation of auctions in private procurement, where, among many other complexities, the buyer can have long-standing relationships with (one of) the suppliers (Arozamena and Weinschelbaum 2009; Walker 1999), has increased the importance of understanding how such relationships can be accounted for in the rules of the auction. Finally, as regulators started adopting auctions to allocate licenses and to organize new markets, such as those for emission permits, the high complexity of the market environment has made it necessary to supplement insights from economic analysis with computational and experimental analysis (Roth 2002).

Some of the questions that arise through the spread of auctions to these new environments are: How does uncertainty (in online auctions) affect behavior? Is one particular auction design more suitable than another for allowing the buyer to account for her existing relationships with (one of) the suppliers? Would theoretically equivalent auctions interact identically with a subsequent market?

This thesis uses theoretical and experimental tools to address these questions. Theoretical analysis helps to develop the intuition behind different design choices and isolate particular effects (Kittsteiner and Ockenfels 2006). However, reality is often too complex, so that a tractable formal analysis is possible only under certain assumptions and simplifications of the real world. Furthermore, formal analysis alone may be incapable of providing the practitioners with clear recommendations (see, for example, the analysis in Ockenfels (2009) concerning the optimal frequency of auctions in the European Emission

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<sup>1</sup>For an overview of theoretical research, see Klemperer (1999) and Krishna (2002), for example. For an overview of empirical research on the effect of auction rules in online auctions, see Ockenfels, Reiley and Sadrieh (2006) and Greiner, Ockenfels and Sadrieh (2012). For an overview of experimental research, see Kagel (1995) and Kagel and Levin (2011). For selected examples of anecdotal evidence, see Klemperer (2002) and Milgrom (2004).

Trading System). Hence, insights from empirical and experimental approaches are often needed to validate the theoretical predictions in more complex situations that are closer to reality. While empirical analysis with field data is helpful, it is not always possible (before a market has been created, for example) or feasible (due to unclear causal relationships)(Kittsteiner and Ockenfels 2006; Lusk and Shogren 2007). Laboratory experiments allow the researcher to study causal effects in a highly controlled environment with real (boundedly rational) agents taking the decisions. Laboratory experiments have, thus, become an important complement to theoretical and empirical analysis with field data, especially with regard to investigating the applicability and limits of existing theories and testing key features of new market mechanism before they come into existence in an environment that closely resembles the field environment (Kittsteiner and Ockenfels 2006; Roth 1995).<sup>2</sup>

The experimental studies presented here complement the existing theoretical and empirical literature that addresses the questions mentioned above. The first study (Chapter 2) investigates what impact uncertainty about one's competitiveness has on bidding behavior. The second one (Chapter 3) compares how two mechanisms for favoritism affect the auction outcome. And the third one (Chapter 4) studies the effects of the frequency of auctions for the allocation of emission permits as well as free allocation on the efficiency of an emission trading market, which is designed to closely mirror the properties of the European Emission Trading System. The first two experiments reveal limitations of existing theories, while the last one provides helpful insights about the way theoretically equivalent mechanisms perform in an environment with resale opportunities.

The payoff-maximizing bid in a first-price sealed-bid auction optimizes the trade-off between the surplus achieved upon winning and the probability to win. The probability to win for any given bid depends on a number of characteristics of one's competitors, which are often not known. In online auctions, for example, where bidders are geographically dispersed and anonymous, ambi-

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<sup>2</sup>For example, one of the contributions of the early literature on experimental auctions reviewed in Kagel (1995) was rejecting the well-known revenue equivalence theorem, while Holt, Shobe, Burtraw, Palmer and Goeree (2007) tested a variety of designs for the auctioning of emission permits within the Regional Greenhouse Gas Initiative in the US before one was implemented.

guity may surround the number of rivals as well as their valuations. Similarly, in laboratory auctions, bidders who interact for the first time will be uncertain about the bidding strategies of their opponents.

Chapter 2, therefore, provides theoretical and experimental analysis of bidding behavior in a simple, auction-like environment with and without ambiguity. In particular, participants are required to submit bids for a fictitious good. Their bid competes against computerized bids, the exact distribution of which is either known or not known. The optimization problem is, thus, independent of common and consistent beliefs required in the majority of the auction literature and makes it possible to eliminate other sources of ambiguity, such as uncertainty about the bidding strategies of one's competitors. We observe that bids in the ambiguous environment are significantly lower than those in the environment without ambiguity, thus, rejecting the predictions of standard ambiguity theories. By adapting Goeree, Holt and Palfrey (2002)'s analysis of the effect of nonlinear probability weighting on bidding behavior to account for ambiguity preferences according to Klibanoff, Marinacci and Mukerji (2005)'s model of smooth ambiguity, we demonstrate that a combination of pessimistic beliefs and nonlinear probability weighting can organize the empirical findings.

While the majority of the auction literature assumes that the seller and the bidders have no prior relationships, this is often not the case. A pre-auction agreement between the seller and one of the bidders may exist and affect the rules of the auction. Such agreements usually put the bidder in a more favorable position relative to her competitors and can therefore serve to extract rents from the non-preferred bidders (Choi 2009) and as protection of established relationships (Walker 1999). The experimental study presented in Chapter 3, thus, investigates how favoritism in auctions can be used to increase the rents the seller and the preferred bidder extract from the other bidders. Two forms of favoritism are compared to a standard first-price auction. The first one, among practitioners also known as "last call", gives the preferred bidder the right of first refusal. Hence, she can win the auction by matching the highest bid of the other bidders. The second one optimizes the seller and the preferred bidder's expected surplus by awarding the good to the other bidders only if their bids exceed an optimal reserve price. Both mechanisms are formally studied in Burguet and Perry (2009) and predicted to increase the

seller and the preferred bidder's joint surplus. However, the optimal mechanism requires that the seller learns the preferred bidder's valuations. She does so via an incentive compatible elicitation mechanism, which turns out to lack robustness to boundedly rational behavior.

The experimental results support part of the theoretical predictions – the auction revenue and the payoff of the other bidders deteriorate as a result of favoritism. However, the results also show some limitations of Burguet and Perry (2009)'s formal analysis, which requires risk-neutral bidders and truth-telling under the the optimal mechanism. In particular, only the mechanism with the right of first refusal increases the joint surplus of the seller and the preferred bidder and the auction revenue deteriorates more under the optimal mechanism than under the auction with the right of first refusal. It is shown that the results can be organized by accounting for risk aversion and for boundedly rational behavior.

Finally, Chapter 4 presents the results of an economic experiment that studies behavior in repeated multi-unit auctions with a resale market. Whereas the first two studies are concerned with relatively simple environments, where the complexity of the real world is highly reduced, this one attempts to closely mirror the design of an existing market – the European Emission Trading System. By considering the majority of design aspects of this particular emissions trading market, it investigates the effect of auctions and free allocation on the efficiency of the environmental instrument. The theoretical predictions are based on a multi-stage competitive equilibrium model. The free allocation mirrors the average allocation in one of the auction treatments, thus allowing us to investigate the direct effect of handing out permits for free rather than against a payment.

The theoretical analysis does not predict any differences in the final allocation and the development of markets with different allocation mechanisms. We find only partial support for the theoretical predictions. The allocative efficiency before trade is not significantly different between markets with more and markets with less frequent auctioning. Furthermore, the method of allocation – for free or against in payment – does not seem to influence the allocative efficiency after trade. However, the method of allocation has significant effects on the ability of the emission trading market to induce cost-efficient emission

reduction. Less frequent auctioning leads to higher allocative efficiency after trade, lower price variability, and lower compliance costs than more frequent auctioning. Free allocation leads to even lower price variability and compliance costs. However, it transfers wealth from the regulator to the regulated firms and its practical design bears a number of contentious issues, such as how closures and new entries should be dealt with, for example (Neuhoff, Martinez and Sato 2006). Hence, when choosing the method of allocation, the regulator needs to take these factors into account.

The content of Chapter 2 and Chapter 4 was created as a result of joint projects. The study on ambiguity was conducted in collaboration with Vitali Gretschko and Axel Ockenfels. Under the guidance of Axel Ockenfels, I developed the research idea, the motivation for the study, and the experimental design. I also gathered the experimental data and conducted the statistical analysis. Vitali Gretschko provided the formal analysis. Axel Ockenfels contributed to the design of the instructions, in particular to the wording and choice of information in the ambiguity treatment. He also guided me with very helpful suggestions about the emphasis in the review of related literature and the focus of the statistical analysis. The study on allocation mechanisms in emissions trading markets was conducted as a joint project with Veronika Grimm. The research idea arose during my graduate studies and became more specific during my first year as a post-graduate, also thanks to the close collaboration with Veronika Grimm. She contributed to the study with very helpful suggestions regarding its structure, its motivation and the literature review, as well as the organization of the statistical analysis. She also developed most of the formal analysis.

# Chapter 2

## Nonexpected Utility and Bidding Behavior in First-Price Auctions With(out) Ambiguity

*The content of the following chapter was produced in collaboration with Vitali Gretschko and Axel Ockenfels.*

### 2.1 Introduction

Deciding on a bid in an auction is a complex process, often driven by uncertainty and ambiguity. A bidder's assessment about her competitors' willingness-to-pay may be very subjective and the distribution of bids that she faces ambiguous. In a laboratory experiment, we study how ambiguity about competing bids affects bidding behavior in a simple auction-like environment, where ambiguity is operationalized in the spirit of Ellsberg (1961) as unmeasurable uncertainty or missing information.<sup>1</sup> We find that bids in the ambiguous environment are significantly different from those in the non-ambiguous environment and show that these differences are not driven by aversion to ambiguity – as common wisdom may suggest – but by an interaction effect between ambiguity tastes and beliefs, on the one hand, and nonlinear

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<sup>1</sup>Note that one strand of the early literature operationalizes ambiguity as “uncertainty with second order probabilities”, where probabilities are drawn from a set of known distribution functions with known probabilities of realization. For discussion of the terminology see Camerer and Weber (1992).

probability weighting, on the other.

Firms that participate in auctions spend considerable amount of time and money to determine the optimal bid. In order to place a bid in a first-price auction, more than just one's own valuation for the object has to be taken into account. Beliefs about the distribution of bids of the competing bidders are required for the optimization process. If the same group of bidders competes repeatedly for similar objects, those beliefs can become accurate. However, in many contexts bidders do not have accurate beliefs about their competitors. For example, Chen et al. (2007) point out that in online auctions, where there is no physical presence and the bidders can be geographically dispersed, the number of bidders as well as their valuations are often unknown. Furthermore, in a setting with heterogeneous, boundedly rational competitors interacting for the first time, even when a bidder is informed about the distribution of her competitors' valuations, the subjective beliefs and risk preferences behind their bidding strategies may be ambiguous.

To investigate how ambiguity affects behavior, we run a laboratory experiment, which has a simple and parsimonious setup. Each bidder decides on her bid in an individual-choice task framed as a first-price auction. She knows that she is facing three symmetric computerized competitors, that the bids of these competitors are uniformly distributed, and that the lowest possible bid is 0. In the first treatment she also knows the highest possible bid. In the second treatment she has no information about the highest possible bid.

We find that bids are significantly lower if the highest possible computerized bid is ambiguous. We demonstrate that expected utility theory and established theories of ambiguity aversion are incapable of explaining this result since they do not predict any differences between the treatments in the particular experimental setup.<sup>2</sup> The intuition behind this result is that in a symmetric first-price auction the optimal bid in an increasing equilibrium is conditional on the fact that the bidder has the highest valuation in the auction. Competing bids that are associated with valuations higher than the valuation of the bidder do not affect the optimal bid. Hence, a variation of the highest possible bid should not have an effect as long as it is consistent with symmetry. Given that

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<sup>2</sup>We assume that the highest possible computer bid is sufficiently high to avoid discussion of corner solutions.



ambiguity theories make a statement about how subjective beliefs regarding ambiguous parameters, i.e., the highest possible bid, are formed and weighted, they also do not predict any treatment differences in our experimental setup.<sup>3</sup>

To explain the results, we incorporate smooth ambiguity preferences in the sense of Klibanoff et al. (2005) in a model of non-expected utility. While the highest possible computerized bid does not affect the optimal bid of an expected utility maximizer, it does affect the winning probability for any given bid and thereby the way a non-expected utility maximizer accounts for this probability in her optimization problem. Under the assumption of nonlinear probability weighting as axiomized by Prelec (1998), this translates into into relatively higher bids if the winning probability is believed to be rather high, and into relatively lower bids if the winning probability is believed to be rather low. We demonstrate that such combination of nonlinear probability weighting with ambiguity aversion and pessimistic beliefs can organize the experimental results.

Previous studies on the impact of ambiguity on the auction outcome have focused on unknown distributions within known bounds of a given support (Bose and Daripa 2009; Chen et al. 2007; Lo 1998; Salo and Weber 1995). Under these conditions, ambiguity aversion in the sense of Gilboa and Schmeidler (1989) is predicted to lead to higher bids. Experimental studies compare bidding behavior in (potentially) asymmetric auctions, where all bids are submitted by human competitors. Güth, Selten and Ivanova-Stenzel (2003) do not find any significant differences between bidding against a competitor whose valuation stems from a known distribution as opposed to an unknown distribution. In conclusion, they question the role of beliefs in the bid submission process. Chen et al. (2007) find that bids are significantly lower under the ambiguous environment, which, given the prediction of their equilibrium model, would suggest ambiguity-loving preferences. However, they point out that such behavior may also be the result of ambiguity aversion and myopic beliefs and recommend further explicit studies. Our experimental design allows

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<sup>3</sup>Note that theories of bounded rationality, such as the impulse balance theory (Neugebauer and Selten 2006; Ockenfels and Selten 2005; Selten 2004) and theories of regret (Engelbrecht-Wiggans and Katok 2007; Filiz-Ozbay and Ozbay 2007), are also incapable of explaining the differences in behavior we observe. They are based on the assumption that deviations from the optimal risk-neutral bid arise as a result of opportunity costs, which do not depend on ambiguity about the highest possible competing bid.

us to address this issue by studying behavior in a situation where the best reply depends only on first-order beliefs. Additionally, our data suggests that contrary to Güth et al.’s conclusion, beliefs about the distribution of opponents’ bids and with it beliefs about winning probabilities affect the decision-making process.<sup>4</sup>

This study contributes to the existing theoretical and empirical literature on ambiguity by providing formal analysis of the joint effect of ambiguity and nonlinear probability weighting on bidding behavior and empirical support for this joint effect. Furthermore, the experimental design, where the auction is presented as an individual-choice task, allows us, firstly, to exclude any uncontrolled ambiguity, which may arise endogenously from competing against human bidders, and, secondly, to resolve any potentially contradicting predictions between equilibrium and myopic behavior.

The rest of the paper is organized as follows. In Section 2.2 we describe the experimental design and procedure. In Section 2.3, we develop the theoretical predictions based on a model of non-expected utility that combines Klibanoff et al.’s model of smooth ambiguity and Prelec (1998)’s axiomization of non-expected probability weighting. In the subsequent Section 2.4, we present the results of our experiment, and the last section concludes.

## 2.2 Experimental Design and Procedure

We study the effect of ambiguity on bidding behavior in a simple individual-choice experiment, in which one human bidder competes for an indivisible good against three computer-simulated bids. Valuations of all competitors are uniformly distributed on  $[0, 100]$ .<sup>5</sup> In the treatment without ambiguity, in the following referred to as **INFO**, participants are informed about the exact distributions of the computerized bids and their own valuations. In the treatment with ambiguity, in the following referred to as **NOINFO**, participants are informed only about the lower bound and the uniformity of the respective

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<sup>4</sup>This result is indirectly related to findings in Armantier and Treich (2009) that overbidding in first-price auctions can be partially explained by incorrect beliefs about the probabilities to win for any given bid.

<sup>5</sup>Valuations are decimal numbers with up to two digits after the decimal point. For tractability all theoretical analysis assumes continuous distribution. This inconsistency between the formal analysis and the practical implementation is a common but unavoidable problem in the literature on experimental auctions.

distributions. The individual choice is framed as a first-price sealed-bid auction, in which the highest bid wins and the winner receives a payoff equal to the difference between her valuation and her bid. In case of a tie, the winner is randomly determined. The computer-simulated bids correspond to the optimal bid in a risk-neutral setting, which equals three fourths of the valuation.

In September 2010 and January 2011 we ran a total of four sessions – two for each treatment. No subject participated in more than one session. The sessions were held in the Cologne Laboratory for Economic Research. Participants were students at the University of Cologne with various backgrounds. They were recruited via ORSEE (Greiner 2004) and did not have any prior experience with experimental first-price auctions. The experiment was programmed and conducted with the software z-Tree (Fischbacher 2007). Upon entering the lab, participants were randomly assigned to a computer terminal. They received written instructions and were encouraged to ask questions in case of doubt. Questions were privately answered. The individual-choice task was repeated over 20 rounds, with one auction per round and new independent draws for all values. After each auction, participants were informed about whether or not they received the good, about the auction price, and about their own payoff. At the end of the experiment, participants responded to a questionnaire on demographical data and on their experience with laboratory experiments and auctions in general.<sup>6</sup> They could also briefly comment on their bidding strategy. After all participants had finished answering the questions, they were privately paid the sum of their earnings over the 20 rounds plus a show-up fee of EUR 2.5. A total of 64 students participated in the INFO and 62 in the NOINFO treatment. Each session took about 45 minutes to complete (including the payment stage). The average payoff was EUR 8.53 (SD = 5.04), without any significant differences between the two treatments.

We employed computerized competitors to avoid a setting, in which even with full information about the valuations of one’s opponents, bidders may face

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<sup>6</sup>We do not find any significant demographical differences between the subject pools with one exception – significantly more participants in NOINFO reported to have taken a class in game theory in comparison to INFO ( $p = 0.001$  for a two-sided t-test). However, as Table 2.3 in the appendix shows, no effect of this variable on the final payoff is found in any Ordinary Least Squares regression model that controls for the average valuation over the 20 rounds (and any number of additional control variables, such as gender, field of studies, etc). Hence, we can safely attribute all differences in bidding behavior reported below to the treatment effect.

ambiguity about their opponents' bidding strategies. Hence, employing computerized competitors allows us to control the exact level of ambiguity and investigate its effect in a noise-free environment not influenced by interpersonal interaction, where factors such as spite (Morgan, Stiglitz and Reis 2003) and inequality aversion (Ockenfels and Selten 2005), for example, could affect behavior. Furthermore, facing non-human competitors simplifies the decision problem from an equilibrium decision to a payoff-maximizing individual-choice task and resolves the requirement for consistency between actions and probabilistic beliefs of all interacting parties. Experience from previous experiments on bidding against computerized opponents in auction(-like) environments suggests that the use of computerized instead of human competitors does not affect the qualitative results (Dorsey and Razzolini 2003; Engelbrecht-Wiggans and Katok 2009; Harrison 1989; Neugebauer and Selten 2006; Walker, Smith and Cox 1987).

## 2.3 Theoretical Analysis

In this section we demonstrate how the experimental design allows us to investigate the role of nonlinear probability weighting in auctions with ambiguity. In particular, we show that neither expected utility, nor ambiguity aversion, nor ambiguity tastes alone predict any treatment effects, while nonlinear probability weighting together with ambiguity preferences do so. On the basis of empirical evidence concerning the general form of the probability weighting function and the typical tastes and beliefs under ambiguity, we derive predictions about the expected between-treatment differences.

Consider a setting in which a bidder decides on an optimal bid in a first-price sealed-bid auction. Her valuation for the auctioned object is  $x$  and she is either informed that the bid of each of her  $N$  competitors is uniformly distributed on  $[0, 1]$  or that the bid of each of her competitors is uniformly distributed on  $[0, \bar{b}]$  without any further information on  $\bar{b}$ . Let  $u$  denote the von Neumann-Morgenstern utility function of the bidder and  $\mu$  her subjective probability distribution of  $\bar{b}$ .

## Expected Utility

First, suppose the bidder is an expected-utility maximizer. If the bidder knows that the bids of her competitors are uniformly distributed on  $[0, 1]$ , her optimal bid is the solution to

$$\max_b b^N u(x - b). \quad (2.1)$$

Suppose  $u$  and  $x$  are such that (2.1) has a unique interior solution.<sup>7</sup> The first-order condition of this optimization problem is

$$Nb^{N-1}u(x - b) - b^N u'(x - b) = 0. \quad (2.2)$$

If, on the other hand, the bidder is only informed that the bids of her competitors are uniformly distributed on  $[0, \bar{b}]$  without any information on  $\bar{b}$ , her optimal bid given her subjective probability distribution solves the following optimization problem:

$$\max_b \int_{-\infty}^{\infty} \frac{b^N}{\bar{b}^N} u(x - b) d\mu(\bar{b}). \quad (2.3)$$

Suppose a unique interior solution exists. The first-order condition of this optimization problem is

$$\begin{aligned} & \int_{-\infty}^{\infty} \frac{Nb^{N-1}}{\bar{b}^N} u(x - b) - \frac{b^N}{\bar{b}^N} u'(x - b) d\mu(\bar{b}) = 0 \\ \Leftrightarrow & \int_{-\infty}^{\infty} Nb^{N-1} u(x - b) - b^N u'(x - b) d\mu(\bar{b}) = 0 \\ \Leftrightarrow & Nb^{N-1} u(x - b) - b^N u'(x - b) = 0. \end{aligned}$$

Thus, the optimal bid does not depend on  $\bar{b}$  – it is the same in both cases and ambiguity does not affect the bidding behavior of an expected utility maximizer.

## Ambiguity Aversion

Expected utility theory cannot explain behavior in many situations (see Machina 2008, for a brief review of related literature). Given that the treatment vari-

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<sup>7</sup>If, for example,  $\bar{b}$  is sufficiently low, a corner solution also exists, such that  $b = \bar{b}$ .

ation concerns ambiguity about the distribution of competing bids, it is reasonable to expect attitudes towards ambiguity to affect the bidding behavior. However, in the following we show that a departure from expected utility that accounts for ambiguity tastes and beliefs in the individual-choice task does not change the optimal bid. More formally, consider the smooth model of decision making under ambiguity that was introduced by Klibanoff et al. (2005). The optimal bid then solves the following optimization problem:

$$\max_b \int_{-\infty}^{\infty} \phi \left( \frac{b^N}{\bar{b}^N} u(x - b) \right) d\mu(\bar{b}). \quad (2.4)$$

Herein  $\phi$  is a strictly increasing mapping from  $\mathbb{R}_+$  to  $\mathbb{R}_+$ . The curvature of  $\phi$  corresponds to the subject's attitude towards ambiguity.<sup>8</sup> The first-order condition can be written as

$$\begin{aligned} & \int_{-\infty}^{\infty} \phi' \left( \frac{b^N}{\bar{b}^N} u(x - b) \right) \left( \frac{Nb^{N-1}}{\bar{b}^N} u(x - b) - \frac{b^N}{\bar{b}^N} u'(x - b) \right) d\mu(\bar{b}) = 0 \\ \Leftrightarrow & \int_{-\infty}^{\infty} \phi' \left( \frac{b^N}{\bar{b}^N} u(x - b) \right) d\mu(\bar{b}) (Nb^{N-1}u(x - b) - b^N u'(x - b)) = 0. \end{aligned} \quad (2.5)$$

As  $\phi$  is an increasing function, (2.5) is true if and only if (2.2) is true for any belief function  $d\mu(\bar{b})$ . It follows that ambiguity aversion does not affect the optimal bid. This result is fairly intuitive as we have shown that the optimal bid is independent of the subjective belief about  $\bar{b}$ . Attitudes towards ambiguity are reflected in attitudes towards different distributions of  $\bar{b}$ . Hence, neither ambiguity tastes, nor beliefs about  $\bar{b}$  are predicted to have an impact on the bidding decision.<sup>9</sup>

## Nonlinear Probability Weighting

Another departure from expected utility theory concerns the way different objectively known probabilities affect the decision making process. It is well established in the psychological literature that subjects tend to put decision

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<sup>8</sup>Ambiguity-averse (loving) attitude corresponds to a concave (convex)  $\phi$ , neutrality towards ambiguity is given by a linear  $\phi$  as modeled above.

<sup>9</sup>The same logic applies to other models of ambiguity aversion. In particular, it can be shown that it holds in the widely applied model of Gilboa and Schmeidler (1989) for any finite beliefs about  $\bar{b}$ . However, the analysis above is based on Klibanoff et al.'s model due to its differentiability and tractability.

weights on probabilities when taking decisions under uncertainty. They act as if the probabilities over outcomes were transformed with some nonlinear weighting function. This kind of nonlinear probability weighting has been brought forward as an explanation for overbidding in first-price auctions by Goeree, Holt and Palfrey (2002) and Armantier and Treich (2009). In what follows, we adapt the model used by Goeree et al. to account for ambiguity tastes and beliefs and show that, in contrast to a model based on ambiguity aversion alone, a model that incorporates subjective probability weighting yields an optimal bid which is different in the ambiguous setting.

If the bidder knows that the bids of each of her competitors are uniformly distributed on  $[0, 1]$  and weights the probabilities of winning in a nonlinear manner, the optimal bid solves

$$\max_b w(b^N)u(x - b). \quad (2.6)$$

Herein  $w(\cdot)$  denotes the probability weighting function. The first-order condition for this problem can be written as

$$\begin{aligned} Nb^{N-1}w'(b^N)u(x - b) - w(b^N)u'(x - b) &= 0 \\ \Leftrightarrow Nb^{N-1}u(x - b) - \frac{w(b^N)}{w'(b^N)}u'(x - b) &= 0. \end{aligned} \quad (2.7)$$

In case the distribution of the bids of the other bidders is ambiguous, the optimal bid solves<sup>10</sup>

$$\max_b \int_{-\infty}^{\infty} \phi \left( w \left( \frac{b^N}{\bar{b}^N} \right) u(x - b) \right) d\mu(\bar{b}). \quad (2.8)$$

In this case the first-order condition is

$$\begin{aligned} \int_{-\infty}^{\infty} \phi' \left( w \left( \frac{b^N}{\bar{b}^N} \right) u(x - b) \right) \\ \left( \frac{Nb^{N-1}}{\bar{b}^N} w' \left( \frac{b^N}{\bar{b}^N} \right) u(x - b) - w \left( \frac{b^N}{\bar{b}^N} \right) u'(x - b) \right) d\mu(\bar{b}) = 0, \end{aligned}$$

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<sup>10</sup>Herein  $\phi$  is defined as in the previous section.

which is equivalent to

$$\int_{-\infty}^{\infty} \phi' \left( w \left( \frac{b^N}{\bar{b}^N} \right) u(x-b) \right) \left( N b^{N-1} u(x-b) - \frac{w \left( \frac{b^N}{\bar{b}^N} \right) \bar{b}^N}{w' \left( \frac{b^N}{\bar{b}^N} \right)} u'(x-b) \right) d\mu(\bar{b}) = 0. \quad (2.9)$$

Substituting the solution to (2.6) in (2.9) suggests that whether or not the optimal bid with and without ambiguity is the same depends on  $\phi$ ,  $w$  and  $\mu$ . Hence, unless  $w$  is linear, bids in both situations will not coincide.

By making some additional assumptions about the preferences under ambiguity and the form of  $w$  based on well established empirical evidence, we can derive more precise predictions about the expected differences in bidding behavior. Firstly, in line with the general literature on behavior under ambiguity, we assume ambiguity-averse bidders (see Camerer and Weber 1992). Secondly, following the flavor of Gilboa and Schmeidler (1989)'s maximin model, where the decision maker optimizes the worst possible outcome, we assume that under ambiguity beliefs about  $\bar{b}$  are pessimistic.<sup>11</sup> This general assumption is supported by the observation that when tastes and beliefs are explicitly accounted for, pessimistic beliefs are required in addition to ambiguity aversion to explain the high ambiguity premium observed in insurance and investment markets (Chateauneuf, Eichberger and Grant 2007).<sup>12</sup> Thirdly, we assume that the probability weighting function is shaped like an inverted "S", concave for low probabilities and convex for high probabilities.<sup>13</sup> Probability weighting functions with such shape overweight low probabilities and underweight high probabilities. The following two-parameter functional form generates a family of functions that includes all inverted "S-shaped" functions:

$$w(p) = \exp(-\beta(-\ln(p))^\alpha), \quad (2.10)$$

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<sup>11</sup>With increasing ambiguity, Gilboa and Schmeidler's model predicts deterioration of the payoff via the worst possible (imaginable) case regardless of the parameter measuring ambiguity attitudes. This can be interpreted as pessimistic beliefs.

<sup>12</sup>A somewhat less direct support is found in Heath and Tversky (1991), where a negative relationship is observed between subjective confidence about one's ability to estimate unknown probabilities and the reluctance to make ambiguous bets. An ambiguously-averse bidder should care little about the level of ambiguity unless higher ambiguity is associated with more pessimistic beliefs.

<sup>13</sup>In what follows, we will use the functional form axiomized by Prelec (1998). He also shows that this functional form fits most of the empirical evidence on nonlinear probability weighting.



where  $\alpha$  and  $\beta$  are positive parameters that determine the shape of the weighting function. We can now state the following result.

**Lemma 2.1** *Let  $\alpha \neq 1$ ,  $b_I := \arg \max_b w(b^N)u(x - b)$  and*

$$v(b, \bar{b}) := Nb^{N-1}u(x - b) - \frac{w\left(\frac{b^N}{\bar{b}^N}\right)\bar{b}^N}{w'\left(\frac{b^N}{\bar{b}^N}\right)}u'(x - b). \quad (2.11)$$

*The following holds true. If*

$$\int_1^\infty \phi'\left(w\left(\frac{b^N}{\bar{b}^N}\right)u(x - b)\right)v(b_I, \bar{b})d\mu(\bar{b}) > \int_0^1 \phi'\left(w\left(\frac{b^N}{\bar{b}^N}\right)u(x - b)\right)v(b_I, \bar{b})d\mu(\bar{b}), \quad (2.12)$$

*the optimal bid when the bidder faces ambiguity is lower than without ambiguity, i.e.*

$$b_I > b_A = \arg \max_b \int_{-\infty}^\infty \phi\left(w\left(\frac{b^N}{\bar{b}^N}\right)u(x - b)\right)d\mu(\bar{b}).$$

**Proof** The proof is relegated to the appendix. ■

The interpretation of Lemma 2.1 is straightforward: the larger  $\bar{b}$ , the lower the chance for the bidder to win ex-ante. When small probabilities are overweighted, the trade-off between a subjectively increased probability to win and rents received upon winning is maximized at a lower objective probability. Hence, if a bidder believes that the probability to face a rather high  $\bar{b}$  is large, this overweighting causes her to bid less in the presence of ambiguity. Ambiguity preferences enter this reasoning through expression (2.12). First, if the bidder is ambiguity-averse,  $\phi$  is concave and hence  $\phi'$  is decreasing. It follows that  $\phi'\left(w\left(\frac{b^N}{\bar{b}^N}\right)u(x - b)\right)$  is increasing in  $\bar{b}$ . Therefore, she overweights larger realizations of  $\bar{b}$  and (2.12) is more likely to hold true. Second, if the bidder has pessimistic beliefs,  $\mu(\bar{b})$  places more mass on less advantageous realizations of  $\bar{b}$  and (2.12) is more likely to hold true. Note that regardless of the ambiguity tastes, pessimistic beliefs about  $\bar{b}$  are required if (2.12) is to be fulfilled.

Given the empirically motivated assumptions above, Lemma 2.1 yields the following prediction about the bidding behavior in the two experimental treatments.

**Hypothesis 2.1** *The optimal bid when the bidder faces ambiguity (NOINFO) is lower than without ambiguity (INFO).*

## 2.4 Experimental Results

In this section, we report the results for the treatments INFO and NOINFO described above with upper bound of the uniform distribution equal to 100. To circumvent potential robustness problems, which may arise through the sensitivity of the relative bids to the height of the respective valuation, in the following, we report analysis of median values for each participant. Furthermore, in the questionnaire participants in the INFO treatment frequently report that for valuations above  $3/4$  of the support they played 'safe' by bidding (at least) the maximum bid of the computerized opponents. To account for potential bias in the data such behavior might cause, we provide separate analysis for bids on valuations from the total support and on valuations, for which no 'safe play' is available.<sup>14</sup>

Table 2.1 gives an overview of the median relative bids at the beginning, at the end, and throughout the whole experiment. The round number refers to the respective independent repetition of the individual-choice task (e.g. independent auction). Bids in the initial rounds are especially interesting, since they were submitted before subjects in NOINFO could gather any (or sufficient) information about the upper bound of the support of the computer bids.

In line with results from standard experiments on bidding in first-price auctions, with the exception of the last 5 rounds in the NOINFO treatment, all of the medians reported in Table 2.1 are significantly different from the risk-neutral best reply ( $p < 0.05$  and lower for two-sided t-tests). Also in line with experimental results in Güth et al. (2003) and Chen et al. (2007), relative

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<sup>14</sup>Two of the participants in the INFO treatment systematically submitted bids above their valuations despite the explicit note in the instructions that such bids may lead to losses, which they will have to pay for. When their bids are excluded from the data on suspicion of malicious behavior, the results remain qualitatively the same.

TABLE 2.1: MEDIAN RELATIVE BIDS

Valuations	All Rounds		Round 1		Round 1-5		Round 16-20		N <sup>a</sup>
	$\leq .75\bar{b}$	All	$\leq .75\bar{b}$	All	$\leq .75\bar{b}$	All	$\leq .75\bar{b}$	All	
INFO	0.871 (0.091)	0.853 (0.068)	0.815 (0.197)	0.813 (0.187)	0.852 (0.125)	0.833 (0.012)	0.886 (0.181)	0.866 (0.084)	64
NOINFO	0.818 (0.098)	0.818 (0.087)	0.734 (0.178)	0.747 (0.200)	0.794 (0.112)	0.794 (0.109)	0.843 (0.159)	0.831 (0.096)	62
p-values <sup>b</sup>	0.001	0.001	0.068	0.004	0.006	0.005	0.168	0.008	

Legend: <sup>a</sup> Nr. of independent observations for all valuations; <sup>b</sup> two-sided t-test for between-treatment differences.  
 Note: Standard deviation across subjects in parenthesis.

bids in the NOINFO treatment are significantly lower than relative bids in the INFO treatment.<sup>15</sup> Especially in the first and in the first five rounds, when respectively no and little information about the upper bound of the rivals' support is available, more overbidding (in absolute terms) is observed in the INFO treatment. The differences are robust to the separate analysis for valuations within the distribution of the computer bids, for which 'safe play' is not possible.

**Result 2.1** *Participants' bids are significantly lower in the NOINFO than in the INFO treatment.*

This result rejects the predictions of the expected utility theory and the smooth ambiguity model in Section 2.3. To investigate the relative role of ambiguity tastes and beliefs, let us consider the effect of repetition on bidding behavior. Table 2.2, which presents the results of random effects estimates of the bidding function clustered on the subject level, supports the results about the effects of repetition presented in Table 2.1. In line with standard theoretical predictions, the intercept is not significantly different from zero. The slope of the bidding function is significantly different from 0.75 ( $p < 0.001$ ). In the first five rounds, there is strong and significant negative effect of NOINFO on the intercept. An effect of NOINFO on the slope of the bidding function is present only in the model with valuations within the distribution of the computer bids, where it is relatively small and only marginally significant. Hence, between-treatment differences seem to be driven mainly by different intercepts rather than different slopes of the bidding function. Furthermore,

<sup>15</sup>Differences in Güth et al. are not significant but qualitatively in the same direction as those reported here.

between-treatment differences on the intercept remain marginally significant in the next five rounds and then disappear, suggesting that bidding in the treatments becomes more alike. The regression estimates, therefore, support the observation in Table 2.1 that, especially in the initial rounds, bids in NOINFO are significantly lower than those in INFO.

TABLE 2.2: THE EFFECT OF REPETITION ON SUBMITTED BIDS

Variable	Valuations $\leq .75*\bar{b}$			All valuations		
	Coef.	Std. Err.	p-value	Coef.	Std. Err.	p-value
Valuation	0.868	0.012	0.000	0.806	0.009	0.000
NOINFO	-2.133	0.845	0.012	-2.922	0.948	0.002
Valuation*NOINFO	-0.030	0.016	0.063	-0.006	0.014	0.674
Rounds 6-10	0.100	0.547	0.854	0.573	0.627	0.361
Rounds 11-15	0.020	0.663	0.976	0.561	0.645	0.385
Rounds 16-20	0.971	0.919	0.291	1.174	0.863	0.174
Rounds 6-10*NOINFO	1.910	0.984	0.052	1.899	1.039	0.068
Rounds 11-15*NOINFO	1.406	0.953	0.140	1.565	1.025	0.127
Rounds 16-20*NOINFO	0.819	1.128	0.468	0.823	1.153	0.475
Constant	-0.550	0.710	0.438	1.098	0.771	0.154
Nr. of observations	1885			2520		
Nr. clusters	126			126		
<i>Random Effects</i>						
R <sup>2</sup> within	0.920			0.927		
R <sup>2</sup> between	0.594			0.701		
R <sup>2</sup> overall	0.899			0.912		

Note: Robust random effects estimates clustered on the subject level.

The experimental results in Table 2.1 and Table 2.2 suggest that bids in the NOINFO treatment increase more rapidly than those in the INFO treatment. Given that individual ambiguity tastes – i.e.,  $\phi$ 's form – remain constant over time, (2.12) suggests that the different effects of repetition on the bidding function can be ascribed to changes in the beliefs about  $\bar{b}$ . Since  $\bar{b}$  is constant over all 20 rounds, any feedback on winning or losing (to a given computerized bid) allows the participants to update their beliefs about it. The more accurate these beliefs, the less differences between the treatments. The lower bids in NOINFO, therefore, indicate that without any information about the upper bound of the bids of their competitors, subjects tend to form pessimistic beliefs – i.e., they place more mass on larger upper bounds. This increases the share of bids, for which the winning probabilities are overweighted, leading to lower subjectively optimal bids. Interestingly, in the very first round, when no information about the upper bound of support of the computer bids is

available, there is significant underbidding in the NOINFO treatment (t-test  $p < 0.05$ ). This suggests even stronger overestimation of disadvantageous  $\bar{b}$ 's.

Note that our interpretation of the lower bids in the ambiguous environment differs from the one in Chen et al. (2007), where they are attributed to ambiguity-loving attitudes. However, as the authors note, ambiguity-loving in the strategic environment of an auction can have different interpretation from ambiguity attitudes in individual-choice experiments.

## 2.5 Conclusion

This study contributes to an increasing experimental practice to study behavior in auctions in a highly controlled environment, which resolves issues of equilibrium play and interpersonal preferences and allows us to focus on the effect of a small environmental change on bidding behavior. In this we demonstrate that ambiguity-averse bidders who form pessimistic beliefs about their competition may in response decrease their bids.

Moreover, our findings add evidence to the importance of accounting for nonlinear probability weighting when analyzing data from experimental auctions. To our knowledge, only few studies on bidding behavior in first-price auctions have explicitly controlled for participants' beliefs about their probabilities to win with a certain bid (see Armantier and Treich 2009; Kirchkamp and Reiß 2011). Given that Armantier and Treich (2009) show that accounting for nonlinear probability weighting can lead to different interpretation of the experimental results, we hope that it will receive more attention in the future when theories of bidding behavior are tested in the laboratory.

## 2.6 Appendix

### 2.6.1 Proof of Lemma 2.1

**Proof** Observe first that

$$\frac{w\left(\frac{b^N}{\bar{b}^N}\right)\bar{b}^N}{w'\left(\frac{b^N}{\bar{b}^N}\right)} = \frac{-\ln\left(\frac{b^N}{\bar{b}^N}\right)^{1-\beta}b^N}{\alpha\beta}.$$

It follows that  $v(b, \bar{b})$  is single-crossing from above in  $b$  and hence  $\int_{-\infty}^{\infty} \phi'\left(w\left(\frac{b^N}{\bar{b}^N}\right)u(x-b)\right)v(b, \bar{b})d\mu(\bar{b})$  is single-crossing from above for all  $b > 0$ . Moreover,

$$\frac{-\ln\left(\frac{b^N}{\bar{b}^N}\right)^{1-\beta}b^N}{\alpha\beta}$$

is increasing in  $\bar{b}$  and hence  $v(b, \bar{b})$  is decreasing in  $\bar{b}$ . Together with  $v(b_I, 1) = 0$  it follows that

$$\begin{aligned} & \int_1^{\infty} \phi'\left(w\left(\frac{b^N}{\bar{b}^N}\right)u(x-b)\right)v(b_I, \bar{b})d\mu(\bar{b}) \\ & > \int_0^1 \phi'\left(w\left(\frac{b^N}{\bar{b}^N}\right)u(x-b)\right)v(b_I, \bar{b})d\mu(\bar{b}) \\ & \Rightarrow \int_{-\infty}^{\infty} \phi'\left(w\left(\frac{b^N}{\bar{b}^N}\right)u(x-b)\right)v(b_I, \bar{b})d\mu(\bar{b}) < 0. \end{aligned}$$

As  $\int_{-\infty}^{\infty} \phi'\left(w\left(\frac{b^N}{\bar{b}^N}\right)u(x-b)\right)v(b, \bar{b})d\mu(\bar{b})$  is single-crossing from above, the optimal  $b_A$  must be smaller than  $b_I$ . This completes the proof.  $\blacksquare$

## 2.6.2 Additional Results

TABLE 2.3: OLS ESTIMATES OF DETERMINANTS OF THE FINAL PAYOFF

Variable	(I)	(II)	(III)
NOINFO	0.397	0.388	1.056
Class in Game Theory	10.344*	4.34	4.688
NOINFO*Class in Game Theory	-10.139	-3.673	-4.775
Avg. Valuation		0.424***	0.482***
NOINFO*Avg. Valuation		-0.003	-0.066
Additional Controls <sup>a</sup>			X
Intercept	8.242***	-13.134**	-16.304***
N	126	126	126
F	1.43	12.4	5.28
R <sup>2</sup>	0.034	0.341	0.418

*Legend:* \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ ; <sup>a</sup> Additional controls were gender, student at the Faculty of Social and Economic Studies, self-reported experience with economic experiments, and self-reported experience with auctions.

## 2.6.3 Instructions

*In the following the instructions for the INFO treatment are provided. It is indicated where they differ for the NOINFO treatment.* Welcome and thank

you for participating in this experiment! Please read these instructions carefully. We kindly ask you to refrain from talking to the other participants or communicating with them in any other way. Please raise your hand if you have any questions. The experimenter will then come to you and answer them. All participants have received the same instructions.

### General Information

In this experiment you will be able to earn money depending on your decisions. During the experiment your profits will be calculated in Experimental Currency Units (ECU). 9 ECUs are equivalent to 1 Euro. At the end of the experiment your profits will be converted into Euros and paid out to you in

addition to a 2.50 Euro show-up fee. Your decisions as well as your profits will be treated confidentially: no other participant will be informed about them.

### **Game Structure**

In this experiment you will be able to purchase a fictitious good in each of 20 auctions. With you three programmed robots submit bids, so that there are four bidders (you and the three bidding robots) in every auction. If in an auction you submit the highest bid, you will win the fictitious good and pay the price you offered in return. If you do not submit the highest bid, you won't receive or pay anything.

### **What is the fictive good's value?**

The exact value of the fictitious good varies between auctions and bidders. At the beginning of each auction, before you make your bid, you are informed about your valuation, i.e., the amount in ECU you will receive in case you win the auction. Your valuation is determined by chance and will be a number with two decimal places, between 0.00 ECU and 100.00 ECU [NOINFO: and a maximum number of ECU's you do not know]. Every number is equally likely. Before each new auction, your valuation will be determined by chance.

### **How high is my profit?**

In case you have made the highest bid in the auction, you win the fictitious good. Your profit is your valuation of the fictitious good in ECU minus your bid. (If you win the auction with a bid which is higher than your valuation, you will suffer losses. Possible losses will be set off against the 2.50 Euro show-up fee. At the end of the experiment you will have to pay for any losses which exceed your show-up fee.) If your bid is lower than the highest bid of the three bidding robots in the auction, you do not receive any payoff for this auction. If two or more bids are equal, the winner of the auction is determined by chance. Your profits over all 20 auctions are cumulated at the end of the experiment, converted according to the exchange rate above, and paid to you in addition to the show-up fee.



### **How do the three bidding robots bid?**

In each auction there are four bidders – you and the three robots. The robots are programmed to make a bid between 0.00 ECU and 75.00 ECU [NOINFO: and a maximum number of ECU's you do not know]. Every number is equally likely. Before each new auction, the robots' bids will be determined according to this random principle, independently from one another and from your valuation.

### **What feedback do I receive after the auction?**

After each auction you will be informed whether or not you have won the auction, the auction price (i.e., the highest bid) and your profit in this auction.

### **Final remarks**

All auctions are relevant for your final payoff. Please raise your hand if you have any questions.

# Chapter 3

## Favoritism in Auctions with Risk-Averse Bidders

### 3.1 Introduction

In many situations a seller and an uninformed buyer, hereafter referred to as “preferred”, can increase their joint surplus by making a mutually beneficial pre-auction agreement that determines the rules of a subsequent auction, in which the buyer competes with other buyers. The agreement will be such that the rules of the subsequent auction will more accurately reflect the seller and the preferred bidder’s joint opportunity costs from selling the good to one of the other buyers, thus allowing them to avoid jointly unprofitable trades and to extract more rents from the other buyers (Hua 2007). Burguet and Perry (2009, hereafter BP09) study formally two such agreements of auctions with favoritism. The first one allows the preferred bidder to match the highest rivals’ bid after the final bidding round is over – a so called “last call” or “right of first refusal”.<sup>1</sup> This competitive advantage increases the preferred bidder’s ex ante surplus at the expense of the regular bidders and the seller. However, the benefit she obtains exceeds the seller’s losses in revenue, thus leading to higher joint surplus. The second agreement maximizes the expected joint surplus via an optimal mechanism, such that the preferred bidder reports her valuation to the seller who then awards the good via an auction with an optimal reserve

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<sup>1</sup>Note that the term “right of first refusal” has a number of definitions and applications. For some examples, see Walker (1999). Here, it will be used in the described auction context.

price.

In a laboratory experiment I investigate the robustness of the predictions of BP09's formal analysis. Two bidders – one preferred and one regular – whose independent valuations stem from a common uniform distribution, compete for an indivisible good. Prior to the auction and before valuations have been privately learned, the preferred bidder has the opportunity to induce favoritism by making a lump-sum payment to the seller. The regular bidder observes whether or not the payment is made, but does not know its amount. If the preferred bidder does not make the payment, both bidders compete in a standard first-price, sealed-bid auction. Otherwise, depending on the treatment, one of the following auctions with favoritism is implemented. In the first treatment, the regular bidder submits her bid and the preferred bidder has the right to match it. In the second treatment, the preferred bidder reports her valuation to the seller, who then awards the good to the regular bidder only if the latter can match a reserve price that optimizes the seller's and the preferred bidder's joint surplus.

The experimental results support BP09's conjecture that both forms of favoritism tend to increase the joint surplus of the seller and the preferred bidder. They also suggest that although favoritism decreases the auction revenue, it can benefit the seller when through the pre-auction agreement she can extract (part of) the preferred bidder's additional surplus. However, differences between the two forms of favoritism are observed that are not predicted by BP09's theoretical analysis with risk-neutral bidders. In particular, the auction with the optimal reserve price does not extract more rents from the regular bidder than the auction with the right of first refusal. Furthermore, favoritism with the optimal mechanism has a stronger negative effect on the auction revenue than favoritism with the right of first refusal. Thus, although the preferred bidder's payoff is higher in the first type of favoritism, the joint surplus is not maximized under the optimal mechanism. Furthermore, relative to the standard first price auction, the auction with the right of first refusal does not bring about additional allocative inefficiencies. I demonstrate that accounting for risk aversion and bounded rationality can organize these deviations from the risk-neutral predictions.

One of the motivations for awarding a potential bidder the right of first

refusal is the necessity to protect her ex ante investments. The right of first refusal is, therefore, often given to commercial tenants to increase their incentives to upgrade the property (Bikhchandani, Lippman and Ryan 2005), or to investors in the form of pre-acquisition agreements (Hua 2007). Another possible field of application could be in industries, where it is common practice to conduct research and development of prototypes for new products in close cooperation with one of the potential suppliers, but to award contracts for serial production via a competitive process. In the research and development stage, the outcome of a research project – its success as well as the properties of the final procurement contract – is unknown. However, if the research partner is promised the right of first refusal in the subsequent competitive process, she might have greater incentives to invest in the research project, making its success more likely.

This study is closely related to the literature on favoritism and corruption with an exogenously determined preferred bidder. In both strands of the literature, prior to the auction one of the bidders is given the right to match the highest rival bid and this is common knowledge. Under corruption, the preferred bidder acquires her status in a pre-auction stage, where she bribes a corrupt intermediary agent running the auction. Under favoritism, the preferred bidder receives the right of first refusal for free or against a payment to the seller (Burguet and Perry 2009; Choi 2009; Hua 2007). In both cases, the right of first refusal extracts additional rents from the non-preferred bidders (Arozamena and Weinschelbaum 2009; Burguet and Perry 2009; Choi 2009). Whether or not the seller benefits from granting the right of first refusal, depends on her ability to collect these additional rents and on the way the right of first refusal affects her revenue. In the context of corruption, only the intermediary agent can benefit from the additional rents. In the context of favoritism, collecting the rents is usually not explicitly analyzed, with the exception of Hua (2007) discussed below.

The effect of the right of first refusal on the auction revenue depends on the distribution of the bidders' valuations. If bidders are ex ante asymmetric, then Burguet and Perry (2007) and Lee (2008) demonstrate that granting the right of first refusal to the weaker bidder can sometimes improve the auction revenue. However, if bidders are ex ante symmetric and the distribution of

their valuations is regular, i.e., a bidder's virtual valuation is increasing in her true valuation, Arozamena and Weinschelbaum (2009) and Burguet and Perry (2009) show that the auction revenue deteriorates as a result of granting the right of first refusal to one of the bidders. Hence, the seller has incentives to grant the right of first refusal only if the rents she collects in the pre-auction stage outweigh her losses in revenue.<sup>2</sup>

Regarding the optimality of the auction rules, Arozamena and Weinschelbaum (2006) derive the properties of any mechanism that in a setting with independent private values would maximize the joint surplus of the seller and the preferred bidder. They demonstrate that no auction with the right of first refusal fulfills these properties. Hua (2007) derives the optimal direct revelation mechanism in a setting with one non-preferred bidder and a preferred bidder who has financial constraints in the pre-auction stage. If the preferred bidder is not financially restricted, Hua's analysis applies to the two bidder case which is experimentally analyzed here.

The experimental literature on favoritism and corruption in auctions with an exogenously determined preferred bidder is rather scarce. To the best of my knowledge, an experiment by Grosskopf and Roth (2009) on the effect of the right of first refusal in (reversed) negotiations is the only experimental study that remotely touches upon this topic. This paper fills this gap. It also provides an instrument for determining the division of additional surplus between the seller and the preferred bidder. Furthermore, it investigates the robustness of the theoretical predictions to boundedly rational behavior. In particular, the optimal mechanism in BP09 requires truthful value revelation by the preferred bidder. However, there is well established experimental evidence that not all incentive compatible mechanisms are capable of inducing truthful value revelation in practice. For example, bidding behavior in the strategically equivalent English and sealed-bid, second-price auctions usually differs, so that only the English auction provides a robust mechanism for eliciting bidders' valuations (see Kagel 1995, for a review of related literature). Given that in private procurement firms often interact repeatedly, it is rea-

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<sup>2</sup>There is also vast and less related literature on corruption in auctions, where the preferred bidder is endogenously determined – i.e., after valuations have been privately announced, bidders compete for the right of first refusal. The interested reader is referred to Lengwiler and Wolfstetter (2010) and the references therein.

sonable to believe that suppliers, who wish to keep their negotiation power in future interactions, would not be willing to disclose their true costs regardless of the short-term incentive compatibility of any elicitation mechanism. The laboratory experiment, therefore, challenges the robustness of the optimization mechanism proposed by BP09 to (behaviorally driven) deviations from equilibrium play.

The remainder of this paper is organized as follows. Section 3.2 reviews BP09’s formal analysis. Section 3.3 describes the experimental design and the subsequent Section 3.4 summarizes the theoretical predictions given the particular experimental framework. The experimental results are presented in Section 3.5. Section 3.6 attempts to explain the deviations between predicted and observed behavior. Section 3.7 concludes.

## 3.2 Theoretical Framework

BP09 analyze the effect of both mechanisms for favoritism studied here for  $N$  risk-neutral suppliers competing for a contract in a reversed procurement auction. In the following, I introduce the notation and review BP09’s main results in the context of a standard (selling) auction. Furthermore, using insights from the standard auction literature, I derive the explicit payoff functions of all parties under the optimal mechanism described below. Deviating from BP09, I assume that the alternative to any form of favoritism is a standard first-price sealed-bid auction.<sup>3</sup>

$N$  bidders – one preferred ( $P$ ) and  $N - 1$  regular, non-preferred ( $R_j$ ,  $j \in \{1, 2, \dots, N - 1\}$ ) – bid for an indivisible good. Valuations are independently drawn from a common distribution  $F(\cdot)$ , with continuous density  $f(\cdot)$  and support  $[\underline{v}, \bar{v}]$ .  $F(x)/f(x)$  is assumed to be an increasing function, s.t. the inverse hazard rate function  $(1 - F(x))/f(x)$  is decreasing and bounded.<sup>4</sup> The risk-neutral seller ( $S$ ) does not have any outside value for the good and her

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<sup>3</sup>In BP09 the optimal mechanism that maximizes the coalition’s surplus is compared to an optimal auction that maximizes the payoff of the auctioneer alone. This deviation of the experimental setting increases the comparability between the treatments without affecting the general implications.

<sup>4</sup>For the analysis of the joint surplus, BP09 make an additional assumption about  $(1 - F(x))/f(x)$ . However, a more general version of their result is found in Proposition 3 in Arozamena and Weinschelbaum (2009), where BP09’s result holds for any  $F(x)/f(x)$ . Hence, in the following, no additional assumptions are required.

costs are normalized to zero. The following two games are studied.

### Game 1: Favoritism with the Right of First Refusal

$F(\cdot)$  is common knowledge.

- Stage 1 S and P negotiate a lump-sum payment from P to S and the rules of the auction in Stage 3.
- Stage 2 Valuations are privately announced. The auction rules in Stage 3 are publicly announced. The amount of the lump-sum payment, if there is any, remains private knowledge of S and P.
- Stage 3 (a) If S and P have reached an agreement in Stage 1, P has the right of first refusal – i.e., the regular bidders submit their bids simultaneously, P is informed about the highest price submitted by them and offered to buy the good at this price. If P rejects the offer, the regular bidder with the highest bid wins and pays her bid.  
 (b) If S and P have not reached an agreement in Stage 1, all bidders compete in a standard first-price auction.

### Game 2: Favoritism with an Optimal Mechanism

$F(\cdot)$  is common knowledge.

- Stage 1 S and P negotiate a lump-sum payment from P to S and the rules of the auction in Stage 3.
- Stage 2.1 Valuations are privately announced. The auction rules in Stage 3 are publicly announced. The amount of the lump-sum payment, if there is any, remains private knowledge of S and P.
- Stage 2.2 (a) If S and P have reached an agreement in Stage 1, P reports her valuation  $\hat{x}$  to the seller in exchange for a transfer  $T(\hat{x})$ .  
 (b) If S and P have not reached an agreement in Stage 1, the game proceeds to Stage 3 (b).
- Stage 3 (a) If S and P have reached an agreement in Stage 1, the regular bidders compete for the good in a first-price sealed-bid auction with a commonly known reserve price  $r(\hat{x})$  that optimizes the joint surplus of S and P. If none of the regular bidders' bids is higher than or equal to the reserve price, P receives the good and pays her reported valuation  $\hat{x}$ .  
 (b) If S and P have not reached an agreement in Stage 1, all bidders compete in a standard first-price auction.

Denote with  $\Pi_i^t(x)$  the expected payoff of agent  $i \in \{P, R_j, S\}$  with valuation  $x \in F(\cdot)$  under condition  $t$ , where  $t \in \{FPA, ROFR, OM\}$  indicates the

auction rules in Stage 3. FPA indicates that bidders compete in a standard first-price auction. ROFR indicates that an agreement has been reached in the first game and P has the right of first refusal. OM indicates that an agreement has been reached in the second game and the auction has a reserve price that optimizes the joint surplus of S and P. Let  $E\Pi_i^t(\cdot)$  denote the respective ex ante expected payoff before valuations are privately announced and  $\beta(x)^t$  the respective bidding strategy of a (regular) bidder with valuation  $x$  in an increasing, symmetric equilibrium. Both games can be solved through backward induction.

**Proposition 3.1** *In Stage 3, all bidders maximize their expected payoff given their private and common knowledge. For  $N > 2$  and a continuously differentiable density function  $f(x)$ , an increasing symmetric equilibrium exists in every subgame, s.t.:*

(i) *the increasing bidding strategy of all bidders under FPA is given by*

$$\beta^{FPA}(x) = \frac{1}{F(x)^{N-1}} \int_{\underline{v}}^x z(N-1)F(z)^{N-2}f(z) dz; \quad (3.1)$$

(ii) *under ROFR, P accepts the offer to buy the good at the highest price offered by the regular bidders if her valuation is higher than this price, and reject it otherwise, while the increasing bidding strategy of the regular bidders is such that, given a boundary condition  $\beta^{ROFR}(\underline{v}) = \underline{v}$  and  $N > 2$ , it solves*

$$(\beta^{ROFR})'(x) = \frac{H(\beta^{ROFR}(x))(N-2)[x - \beta^{ROFR}(x)]}{H(x)[H(\beta^{ROFR}(x)) - (x - \beta^{ROFR}(x))]}, \quad (3.2)$$

where  $H(x) = F(x)/f(x)$ ;

(iii) *the increasing bidding strategy of the regular bidders under OM, when the commonly known reserve price is  $r$ , equals*

$$\beta^{OM}(x) = r \frac{F(r)^{N-2}}{F(x)^{N-2}} + \frac{1}{F(x)^{N-2}} \int_r^x z(N-2)F(z)^{N-3}f(z) dz \quad (3.3)$$

for  $x > r$  and 0 otherwise.

**Proof** (i) is a standard result in the auction literature found in Krishna (2002, Ch. 2.3), for example. (ii) follows from the fact that P will accept the offer if her valuation is higher than the highest bid of the regular



bidders, and will reject it otherwise.<sup>5</sup> Hence, a regular bidder with valuation  $x$  maximizes the expected payoff function  $\Pi_{R_j}(\beta^{ROFR}(x), x) = [x - \beta^{ROFR}(x)]F(\beta^{ROFR}(x))F(x)^{N-2}$ . The condition in (iii) is equivalent to the one in (5) in BP09. (iii) is a standard result in the auction literature found in Krishna (2002, Ch. 2.5), for example. ■

Note that for  $N = 2$  the optimization problem for the regular bidders under ROFR is dominance solvable. A solution is provided in Section 3.4.

**Proposition 3.2** *Under OM, given the regular bidders' bidding strategy and provided that the hazard rate function  $f(x)/(1 - F(x))$  is monotonically increasing, the optimal mechanism is such that, when  $P$  reports a valuation  $\hat{x}$ ,*

(i) *the reserve price in Stage 3 solves*

$$r - \frac{1 - F(r)}{f(r)} = \hat{x}, \quad (3.4)$$

(ii) *while in Stage 2.2,  $S$  elicits  $P$ 's valuation by paying her an incentive compatible transfer  $T(\hat{x}) = \int_{\underline{v}}^{\hat{x}} F(r(z))^{N-1} dz$ .*

**Proof** Under OM,  $S$  and  $P$  have agreed to form a coalition and sell the good via a mechanism which maximizes their joint surplus. Hence, they act as one agent, whose valuation for the good equals the surplus of trade between the two parties, i.e.,  $P$ 's valuation.<sup>6</sup> The proof of (i) is found in Krishna (2002, Ch. 2.5) or Myerson (1981). The proof of (ii) is equivalent to the proof of Proposition 5 in BP09. ■

Note that  $r(x)$  is independent of the number of regular bidders. Furthermore, for regular distributions, i.e. when  $x - (1 - F(x))/f(x)$  is an increasing function,  $r(x)$  is also an increasing function. Also note that the transfer is not conditional on  $P$  actually winning the good.<sup>7</sup> Due to the incentive compatibility of the elicitation mechanism, it follows that  $\hat{x} = x$  and  $r(\hat{x}) = r(x)$ .

<sup>5</sup>Note that  $P$  has no incentives to submit a positive bid, since any bid above the highest bid of the regular bidders would increase the price she has to pay.

<sup>6</sup>Remember that  $S$  does not have any value for the good.

<sup>7</sup>It has been suggested to make the elicitation mechanism such that the transfer  $P$  receives is conditional on her winning the good. However, it can be shown that such a transfer is not incentive compatible when there are two bidders and  $P$  is risk-averse. Since risk aversion is found to organize the experimental results better than risk neutrality, such a transfer would not be part of a feasible mechanism.

**Proposition 3.3** *The respective ex ante expected payoff of a regular bidder  $R_j$ ,  $j \in \{1, 2, \dots, N - 1\}$  in Stage 1, depending on the auction rules in Stage 3, is given by*

(i)

$$E\Pi_{R_j}^{FPA}(\cdot) = \int_{\underline{v}}^{\bar{v}} (1 - F(z))F(z)^{N-1} dz, \quad (3.5)$$

(ii)

$$E\Pi_{R_j}^{ROFR}(\cdot) = \int_{\underline{v}}^{\bar{v}} (1 - F(z))F(z)^{N-2}F(\beta^{ROFR}(z)) dz, \quad (3.6)$$

and

(iii)

$$E\Pi_{R_j}^{OM}(\cdot) = \int_{r(\underline{v})}^{\bar{v}} f(x)F(x)^{N-2} \left( xF(r^{-1}(x)) - \int_{\underline{v}}^{r^{-1}(x)} \beta^{OM}(x, r(z))f(z) dz \right) dx. \quad (3.7)$$

where  $r(x)$  is characterized by equation (3.4).

**Proof** The payoffs in (i) and (ii) are equivalent to (2) and (8) in BP09, respectively. The proof of (iii) is relegated to the appendix. ■

**Proposition 3.4** *The respective ex ante expected payoff of the preferred bidder in Stage 1, depending on the auction rules in Stage 3 and barring any lump-sum payments to S, is given by*

(i)

$$E\Pi_P^{FPA}(\cdot) = \int_{\underline{v}}^{\bar{v}} (1 - F(z))F(z)^{N-1} dz, \quad (3.8)$$

(ii)

$$E\Pi_P^{ROFR}(\cdot) = \int_{\underline{v}}^{\beta^{ROFR}(\bar{v})} (1 - F(z))F[(\beta^{ROFR})^{-1}(z)]^{N-1} dz + \int_{\beta^{ROFR}(\bar{v})}^{\bar{v}} (1 - F(z)) dz, \quad (3.9)$$

and

(iii)

$$E\Pi_P^{OM}(\cdot) = \int_{\underline{v}}^{\bar{v}} f(x) \int_{\underline{v}}^x F(r(z))^{N-1} dz dx = \int_{\underline{v}}^{\bar{v}} (1 - F(z))F(r(z))^{N-1} dz. \quad (3.10)$$

**Proof** (i) is equivalent to (i) in Proposition 3.3. (ii) is equivalent to (9) in BP09 and (iii) follows from the fact the P's ex ante expected payoff is determined by the expected value of the elicitation transfer for all possible realizations of her valuation. ■

**Proposition 3.5** *The respective ex ante expected revenue of the seller in Stage 1, depending on the auction rules in Stage 3 and barring any lump-sum payments from P, is given by*

(i)

$$E\Pi_S^{FPA}(\cdot) = N \int_{\underline{v}}^{\bar{v}} (1 - F(z))z(N - 1)F(z)^{N-2}f(z) dz, \quad (3.11)$$

(ii)

$$E\Pi_S^{ROFR}(\cdot) = (N - 1) \int_{\underline{v}}^{\bar{v}} f(x)\beta^{ROFR}(x)F(x)^{N-2} dx, \quad (3.12)$$

and

(iii)

$$\begin{aligned} \Pi_S^{OM}(\cdot) = & \int_{\underline{v}}^{\bar{v}} f(x) \left[ (N - 1) \left( r(x)(1 - F(r(x)))F(r(x))^{N-2} \right. \right. \\ & \left. \left. + \int_{r(x)}^{\bar{v}} z(1 - F(z))(N - 2)F(z)^{N-3}f(z) dz \right) \right. \\ & \left. + F(r(x))^{N-1}x - \int_{\underline{v}}^x F(r(z))^{N-1} dz \right] dx. \end{aligned} \quad (3.13)$$

**Proof** (i) is a standard result for first-price sealed-bid auctions with  $N$  bidders (see Krishna 2002, Ch. 2.4, for example). (ii) is equivalent to (11) in BP09. And (iii) uses the results for the expected auction revenue in first-price auctions with reserve prices (see Krishna 2002, Ch. 2.5), while accounting for the randomness of  $r(x)$  and for the elicitation transfer  $T(x)$ . ■

Remember that in a standard first-price auction, in an increasing, symmetric equilibrium, the expected payoff of any bidder  $i \in \{R_j, P\}$  with valuation  $x$  is given by<sup>8</sup>

$$\Pi_i^{FPA}(x) = F(x)^{N-1}(x - \beta^{FPA}(x)). \quad (3.14)$$

Under OM, the expected payoff of a regular bidder with valuation  $x$  before the reserve price  $r(z)$  is made common knowledge, can be expressed as

$$\Pi_{R_j}^{OM}(x, r(z)) = F(x)^{N-2}F(r^{-1}(x))(x - E[\beta^{OM}(x, r(z))|x > r(z)]). \quad (3.15)$$

Note that  $r(x) > x$ , i.e.  $r^{-1}(x) < x$ , and  $\beta^{FPA}(x) < \beta^{OM}(x, r)$  for any  $x \in F(\cdot) \setminus \{\bar{v}\}$  and  $r > \underline{v}$ . Using the results from the propositions above and from (3.14) and (3.15), it follows that  $E\Pi_P^{ROFR} > E\Pi_i^{FPA} > E\Pi_{R_j}^{ROFR}$  and  $\Pi_P^{OM}(\cdot) > \Pi_i^{FPA}(\cdot) > \Pi_C^{OM}(\cdot)$ , where  $i \in \{P, R_j\}$ . It is obvious that  $E\Pi_S^{ROFR} < E\Pi_S^{FPA}$ . The comparison of the auction revenue under FPA and OM is not that straightforward, but in section 3.4 it is shown to deteriorate under OM when  $F(x) \sim U[\underline{v}, \bar{v}]$ . Hence, based on the outcome of the auction alone, both forms of favoritism redistribute rents away from the seller and the regular bidders to the preferred bidder. Arozamena and Weinschelbaum (2009) demonstrate that ROFR increases the joint surplus of the seller and the preferred bidder. Given its construction, OM also increases the joint surplus. Hence, the seller would have incentives to run an auction with any of the two forms of favoritism only if through the lump-sum payment in the negotiation Stage 1 she manages to extract a part of this surplus that is sufficient to outweigh her losses in revenue. BP09 note that, as long as the preferred bidder does not face any budget or financial constraints, any redistribution of additional surplus in the negotiation stage is individually rational. Hence, provided that there are no financial constraints, the height of the lump-sum payment in Stage 1, and with it the seller's incentives to run an auction with favoritism depend on the bargaining power of the seller and the preferred bidder. Details are discussed in Section 3.4.

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<sup>8</sup>See Krishna (2002, Ch. 2), for example.

### 3.3 Experimental Design and Procedure

In a laboratory experiment, I study the effect of favoritism in a first-price sealed-bid auction with two bidders competing for a nondivisible good. One of the bidders is exogenously determined to be preferred. Prior to the auction and before the bidders learn their valuations, the preferred bidder can influence the rules of the subsequent competition by submitting a lump-sum payment to the seller. Her willingness-to-pay to avoid competing in a standard first-price auction is elicited via the Becker-DeGroot-Marschak-method (Becker, DeGroot and Marschak 1964), where the actual amount of the lump-sum payment is randomly determined. If the reported willingness-to-pay is higher than the random amount, the preferred bidder pays the random amount and both bidders compete in an auction with favoritism. Otherwise, both bidders compete in a standard first-price sealed-bid auction. Both bidders know that one of them is preferred and the manner in which they will compete as soon as it has been determined. Only the preferred bidder knows her willingness-to-pay and the randomly determined lump-sum payment for avoiding the first-price auction.

In the first treatment, in the following referred to as **T-ROFR**, in the auction with favoritism the non-preferred bidder submits her bid and the preferred bidder is allowed to match it. If she can do that, she wins the auction and pays the bid of the non-preferred bidder. Otherwise, the non-preferred bidder wins and pays her own bid. In the other treatment, in the following referred to as **T-OM**, in the auction with favoritism the preferred bidder reports a valuation to the auctioneer in exchange for an incentive compatible transfer as defined in Section 3.2. The non-preferred bidder is then offered to purchase the good at a (reserve) price that depends on the reported valuation. If she accepts that price, she wins the auction and pays the reserve price. If she does not accept it, the preferred bidder wins the auction and pays her reported valuation.<sup>9</sup>

Figures 3.1 and 3.2, which were also included in the instructions, depict the possible course of one round in the T-ROFR and T-OM treatment, respectively. In T-OM participants were also presented with a table that informed

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<sup>9</sup>Note that when there are only two bidders, the selling mechanism with favoritism is no longer an auction in its true sense but a take-it-or-leave-it mechanism. However, this deviation does not affect the theoretical predictions. Therefore, the labels are chosen for consistency with the general literature.

them about the transfer to the preferred bidder and the reservation price for the competing bidder resulting from different valuations reported by the preferred bidder. The experiment was neutrally framed – the auctions with favoritism were referred to as “*sequential play*” and the first-price auctions as “*simultaneous play*”.

The valuations of both bidders were random numbers with up to one digit after the decimal point, i.i.d. on  $[0, 100]$ .<sup>10</sup> They were randomly drawn for each round. Both bidders were privately informed about their own valuation after the game type – sequential or simultaneous – had been determined. The random lump-sum payment for the auction with favoritism was framed as a price for sequential play and was drawn from a commonly known uniform distribution with support  $[-10, 25]$ . The negative values in the support allow me to investigate whether some participants might actually request money in order to play sequentially.<sup>11</sup> Also, the average expected lump-sum payment was somewhat lower than the lump-sum payment, which would make a seller indifferent between a standard first-price auction and the respective auction with favoritism when bidders are risk-neutral, but it ensured that a sufficient number of observations of auctions with favoritism could be gathered.

Upon entering the lab, participants were randomly assigned to a computer terminal. They received written instructions and were encouraged to ask questions in case of doubt. Questions were privately answered. The role of the preferred bidder was randomly assigned and kept throughout the experiment. In each round participants were randomly matched to a participant with a different role.<sup>12</sup> At the end of each round, feedback on the auction price and

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<sup>10</sup>The distribution of the valuations is thus not truly continuous but this is a common and unavoidable problem in the majority of literature on experimental auctions.

<sup>11</sup>Rational players do not have any incentives to report negative willingness-to-pay. However, humans are known to care for their relative payoffs in comparison to others and to dislike large positive as well as large negative discrepancies (see Bolton and Ockenfels 2000; Fehr and Schmidt 1999, 2006, and the references therein). Hence, if participants have extreme fairness preferences, they might be unwilling to compete sequentially, as sequential play increases disproportionately the expected payoff of the preferred bidder relative to the one of the regular bidder. In the 1,800 individual decisions per treatment, negative willingness-to-pay is observed only rarely. In T-ROFR it is driven mainly by three participants and observed in 6.7% of the cases. In T-OM it is driven by one participant and observed in 0.5% of the cases.

<sup>12</sup>In fact, participants were separated into groups of three preferred and three non-preferred bidders. In each round they were randomly re-matched to a bidder with a different role within the respective group. The instructions provided information only about the fact

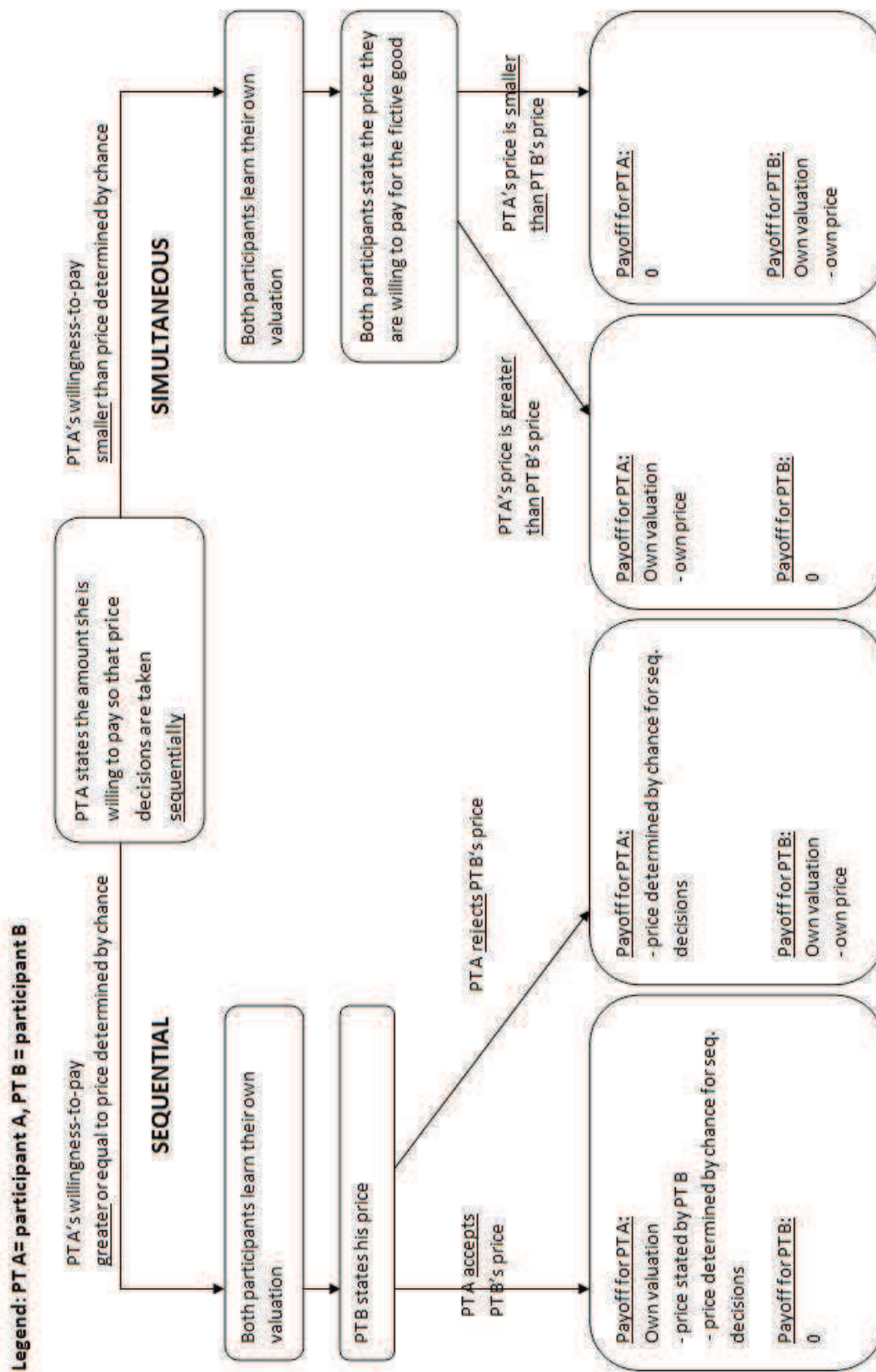


FIGURE 3.1: THE COURSE OF ONE ROUND IN T-ROFR

that in every round one's partner was randomly determined.

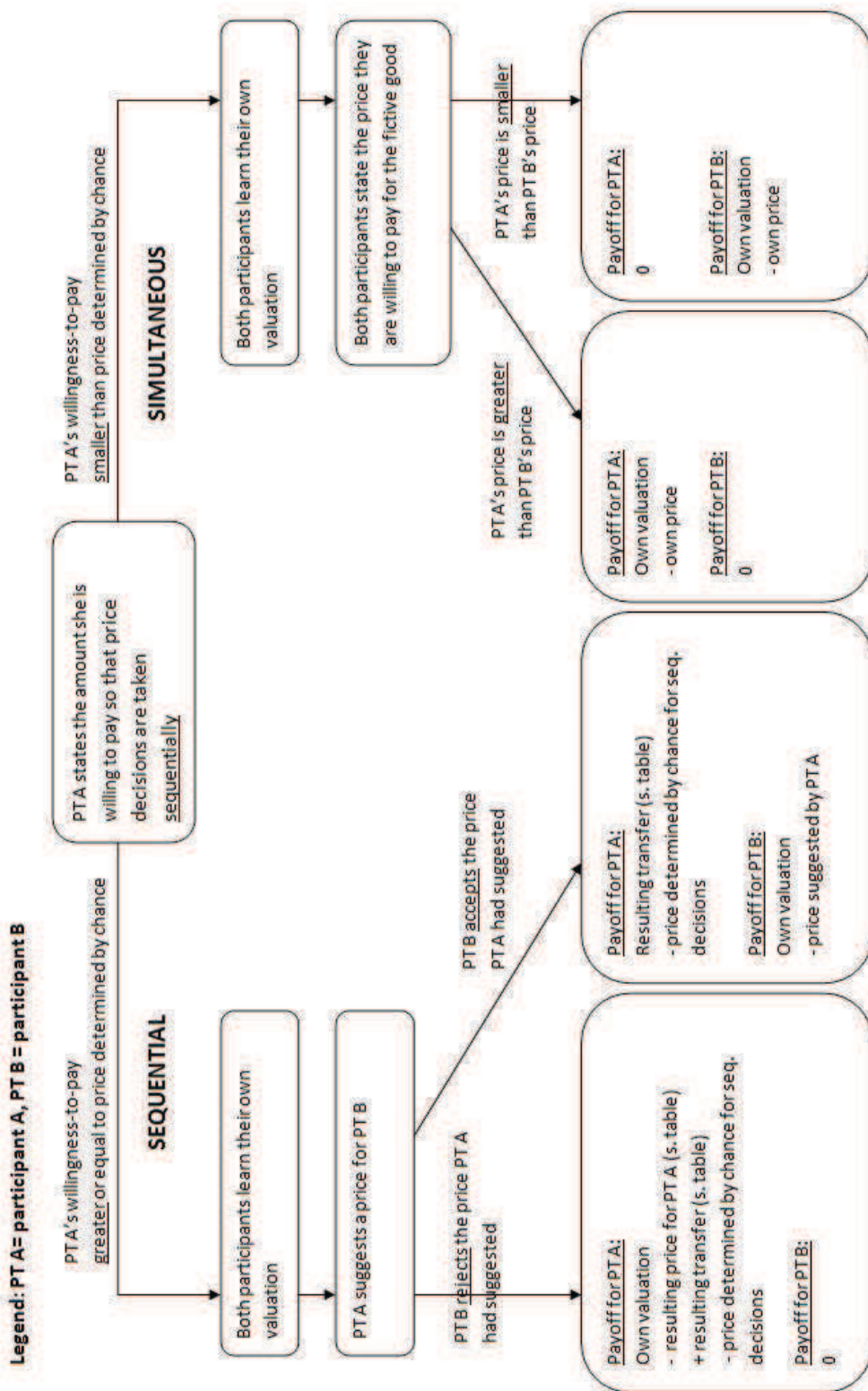


FIGURE 3.2: THE COURSE OF ONE ROUND IN T-OM



one's own payoff was provided in the first-price auctions and on one's price and one's own payoff in the auctions with favoritism. The final payoff was cumulated over a random half of the 60 rounds determined by the throw of a dice. At the beginning of each half, participants received new initial endowments. Instructions are available in the appendix.

In total, 120 students at the University of Cologne with prior experience in economic experiments participated in four sessions – two for each treatment – conducted in November 2011 in the Cologne Laboratory for Economic Experiments. Participants were recruited via ORSEE (Greiner 2004) and the experiment was programmed and conducted in zTree (Fischbacher 2007). One session lasted 1.5 - 2 hours. The average earnings were 17.17 EUR in T-ROFR and 20.73 EUR in T-OM, including a 2.50 EUR show-up fee.

### 3.4 Parametrization and Theoretical Predictions

Table 3.1 gives an overview of the theoretical values for the ex ante expected payoffs and the allocative efficiency when there are two bidders, whose valuations stem from a continuous uniform distribution with support  $[\underline{v}, \bar{v}] = [0, 100]$ . Note that the calculation of the expected payoffs disregards any lump-sum payments in the negotiation stage.

With some abuse of notation let  $\beta(\cdot)$  denote the bidding function in the respective condition. The results, barring predictions about  $\beta^{ROFR}(x)$  and the efficiency of allocation, follow directly from the analysis in Section 3.2 when  $N = 2$  and  $F \sim U[\underline{v}, \bar{v}]$  is assumed. From Proposition 1 in Arozamena and Weinschelbaum (2009), which states that the optimal bidding strategy of the regular bidders does not change in response to the existence of a preferred bidder as long as  $F(x)/f(x)$  is a linear function (and for  $F(x) \sim U(\cdot)$  this is obviously the case), it follows that

$$\beta^{ROFR}(x) = \beta^{FPA}(x) = (x + \underline{v})/2 = (x - \underline{v})/2 + \underline{v}.$$

The intuition behind this result is that in both cases the optimal bid is conditional on the (regular) bidder having the highest valuation. She then submits

a bid equal to the expected second highest valuation, which is the same in both cases.

The derivation of the predictions for the allocative efficiency is relatively straightforward. Let  $\theta^t$ ,  $t \in \{FPA, ROFR, OM\}$ , denote the expected efficiency under the respective condition and  $\pi(\Omega)$  the probability of an event  $\Omega$  occurring. Under the symmetric equilibrium of the FPA the bidder with the highest valuation wins, i.e.,  $\theta^{FPA} = 1$ . ROFR and OM lead to inefficient allocation whenever the preferred bidder wins despite having the lower valuation.

When the preferred bidder's valuation equals  $x_P$ , the probability that she non-deservingly wins and ROFR leads to inefficient allocation is given by

$$\pi(\text{ROFR inefficient}, x_P) = \begin{cases} \pi(x_P < z) & \text{for } x_P \geq \beta^{ROFR}(\bar{v}) \\ \pi(\beta^{ROFR}(z) < x_P < z) & \text{for } x_P < \beta^{ROFR}(\bar{v}) \end{cases}$$

where  $Z$  is the regular bidder's random valuation and  $\beta^{ROFR}(\bar{v}) = (\bar{v} + \underline{v})/2$  is her maximum possible bid, above which the preferred bidder always wins. Accounting for the uniform distribution gives  $\pi(x_P \geq \beta^{ROFR}(\bar{v})) = \pi(x_P < \beta^{ROFR}(\bar{v})) = 0.5$  and the following expected allocative efficiency:<sup>13</sup>

$$\begin{aligned} \theta^{ROFR} &= 1 - \pi(\text{ROFR inefficient}, x_P) \\ &= 1 - \left( 0.5 \int_{\underline{v}}^{\bar{v}} F(z) f(z) dz + 0.5 \int_{\underline{v}}^{\bar{v}} (F(z) - F(\beta^{ROFR}(z))) f(z) dz \right) \\ &= 1 - \left( 0.5 \frac{1}{2} F(z)^2 \Big|_{\underline{v}}^{\bar{v}} + 0.5 \frac{1}{4} F(z)^2 \Big|_{\underline{v}}^{\bar{v}} \right) \\ &= 1 - \frac{3}{8} = 0.625. \end{aligned} \tag{3.16}$$

Now assume that the regular bidder's valuation is  $x_R$  and P's valuation is the random variable  $Z$ . The probability that the preferred bidder non-deservingly wins and OM leads to an inefficient allocation is given by

$$\pi(\text{OM inefficient}, x_R) = \begin{cases} \pi(z < x_R) & \text{for } x_R < r(\underline{v}) \\ \pi(z < x_R < r(z)) & \text{for } x_R \geq r(\underline{v}) \end{cases}$$

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<sup>13</sup>The calculations in (3.16) use that  $F(\beta^{ROFR}(z)) = \frac{(z-\underline{v})/2+\underline{v}-\underline{v}}{\bar{v}-\underline{v}} = F(z)/2$ .

TABLE 3.1: THEORETICALLY PREDICTED VALUES

Variable	First-Price Auction	Right of First Refusal	Coalition
$E\Pi_R(\cdot)$	$\int_{\underline{v}}^{\bar{v}}(1 - F(x))F(x) dx = \frac{1}{6}(\bar{v} - \underline{v}) = 16.67$	$\int_{\underline{v}}^{\bar{v}}(1 - F(x))F(\beta(x)) dx = \frac{1}{12}(\bar{v} - \underline{v}) = 8.33$	$\int_{r(\underline{v})}^{\bar{v}}f(x)\Upsilon(x) dx = \frac{1}{24}(\bar{v} - \underline{v}) = 4.17$
$E\Pi_P(\cdot)$	$\int_{\underline{v}}^{\bar{v}}(1 - F(x))F(x) dx = \frac{1}{6}(\bar{v} - \underline{v}) = 16.67$	$\Phi(x) + \int_{\beta(\bar{v})}^{\bar{v}}(1 - F(x)) dx = \frac{7}{24}(\bar{v} - \underline{v}) = 29.17$	$\int_{\underline{v}}^{\bar{v}}(1 - F(x))F(r(x)) dx = \frac{1}{3}(\bar{v} - \underline{v}) = 33.33$
$E\Pi_S(\cdot)$	$2 \int_{\underline{v}}^{\bar{v}}f(x)(1 - F(x))x dx = \frac{1}{3}(\bar{v} - \underline{v}) + \underline{v} = 33.33$	$\int_{\underline{v}}^{\bar{v}}f(x)\beta(x) dx = \frac{1}{4}(\bar{v} - \underline{v}) + \underline{v} = 25.00$	$\int_{\underline{v}}^{\bar{v}}f(x)\Psi(x) dx = \frac{1}{4}(\bar{v} - \underline{v}) + \underline{v} = 25.00$
$\beta(x)$	$0.5x$	$0.5x$	$r$ for $x > r$ and $0$ else <sup>d</sup>
All. Efficiency	1.0	0.625	0.625

Legend: <sup>a</sup>  $\Upsilon(x) = (x \cdot F(r^{-1}(x)) - \int_{\underline{v}}^{r^{-1}(x)} r(z)f(z) dz)$ ; <sup>b</sup>  $\Phi(x) = \int_{\underline{v}}^{\beta(\bar{v})}(1 - F(x))F(\beta^{-1}(x)) dx$ ; <sup>c</sup>  $\Psi(x) = r(x)[1 - F(r(x))] + xF(r(x)) - T(x)$ ; <sup>d</sup>  $r$  denotes the realized reserve price in Stage 3 of OM.

Accounting for the uniform distribution gives  $r(\underline{v}) = (\bar{v} + \underline{v})/2$ , so that  $\pi(x_R < r(\underline{v})) = \pi(x_R \geq r(\underline{v})) = 0.5$ . The resulting ex ante allocative efficiency then equals<sup>14</sup>

$$\begin{aligned}
\theta^{OM} &= 1 - \pi(\text{OM inefficient}, x_R) \\
&= 1 - \left( 0.5 \int_{\underline{v}}^{\bar{v}} (1 - F(z))f(z) dz + 0.5 \int_{\underline{v}}^{\bar{v}} (F(r(z)) - F(z))f(z) dz \right) \\
&= 1 - \left( 0.5F(z) \Big|_{\underline{v}}^{\bar{v}} - 0.5\frac{1}{2}F(z)^2 \Big|_{\underline{v}}^{\bar{v}} + 0.5\frac{1}{2}F(z) \Big|_{\underline{v}}^{\bar{v}} - 0.5\frac{1}{4}F(z)^2 \Big|_{\underline{v}}^{\bar{v}} \right) \\
&= 1 - \frac{3}{8} = 0.625. \tag{3.17}
\end{aligned}$$

Note that when valuations are uniformly distributed, both forms of favoritism lead to the same expected allocative efficiency. The optimal mechanism is thus predicted to allow the coalition to maximize the rents extracted from the non-preferred bidder, without causing additional deadweight losses.

Table 3.1 and the above analysis give the following predictions. Note that these predictions do not consider any lump-sum payments in the negotiation stage, which are discussed below.

**Hypothesis 3.1 (Payoff Regular Bidder)** *The expected payoff of the regular bidder is lowest under OM, followed by ROFR, and then FPA.*

**Hypothesis 3.2 (Payoff Preferred Bidder)** *The expected payoff of the preferred bidder is highest under OM, followed by ROFR, and then FPA.*

**Hypothesis 3.3 (Auction Revenue)** *The expected auction revenue (elicitation payment under OM considered) is lower under any of the auctions with favoritism than under the standard first-price auction. It is not significantly different between OM and ROFR.*

**Hypothesis 3.4 (Joint Payoff)** *The expected joint payoff of the preferred bidder and the seller is highest under OM, followed by ROFR, and then FPA.*

**Hypothesis 3.5 (Allocative Efficiency)** *The expected allocative efficiency is lower under any of the auctions with favoritism than under the standard first-price auction. It is not significantly different between OM and ROFR.*

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<sup>14</sup>The calculations in (3.17) use that  $F(r(z)) = F((z + \bar{v})/2) = (z - \underline{v})/2 + (\bar{v} - \underline{v})/2 = 1/2 + F(z)/2$ .

The intuition behind these predictions is the following. In the FPA, a bidder wins the auction if and only if she has the highest valuation. Under any mechanism with favoritism, the regular bidder can lose the auction despite having the higher valuation. This leads to lower efficiency in the auctions with favoritism (Hypothesis 3.5) and to redistribution of rents away from the regular bidder to the preferred bidder. Hypothesis 3.1 and Hypothesis 3.4 follow from the fact that OM is designed to maximize this rent extraction. Favoritism affects the expected payoffs of the seller and the preferred bidder in opposite directions. As the preferred bidder receives the good with higher probability, her ex ante expected payoff is higher in the auctions with favoritism. It is highest under OM where she receives the total additional surplus extracted from the competing bidder as informational rents (Hypothesis 3.2). Favoritism reduces the expected auction revenue. Under FPA it equals the bid corresponding to the highest valuation. Under ROFR it equals the bid of the regular bidder, whose bid is not affected by the existence of favoritism and who may not have the highest valuation. Hence, the expected auction revenue is lower under ROFR than under FPA. Since all of the additional rents under OM are used to elicit the preferred bidder's valuation, the auction revenue is the same under both forms of favoritism (Hypothesis 3.3).

The above analysis of the expected payoffs disregards any lump-sum payments by the preferred bidder in Stage 1. In line with the general literature on favoritism, it finds that when the seller's payoff is determined by the auction revenue alone, the seller does not have any incentives to run an auction with favoritism. However, if the seller can extract (part of) the preferred bidder's additional surplus arising from favoritism, her incentives to grant any form of favoritism depend on the total payoff she receives from running the auction.

The realized distribution of surplus in the experiment is an artefact of the experimental design, thus making a direct comparison between the seller's payoff under the first-price auction and her payoff under any of the auctions with favoritism not very insightful. As noted above, provided that the preferred bidder does not face any financial constraints, the distribution in practice will depend on the bargaining power of the negotiating parties. In the following, it is assumed that in Stage 1 the seller has the bargaining power. Given the symmetric information structure in Stage 1, she makes a take-it-or-leave-it offer

to the preferred bidder that extracts the latter’s maximum willingness-to-pay for the auction with favoritism, who then accepts this offer. The preferred bidder’s maximum willingness-to-pay will be such that she becomes indifferent between paying this amount and receiving the expected payoff under an auction with favoritism on the one hand, and not paying this amount and receiving the expected payoff under the first-price auction on the other. The following prediction about the maximum lump-sum payment the preferred bidder would accept in the negotiation stage is a corollary of Hypothesis 3.2.

**Hypothesis 3.6 (Maximum Willingness-To-Pay)** *The maximum willingness-to-pay by a preferred bidder to avoid the standard first-price auction is higher in T-OM than in T-ROFR.*

As the seller’s payoff equals the sum of her auction revenue and the rents she extracts from the preferred bidder in the negotiation stage, the following prediction about the maximum achievable payoff a seller who has the bargaining power and extracts the preferred bidder’s maximum willingness-to-pay in Stage 1 is a corollary of Hypotheses 3.3 and 3.6.

**Hypothesis 3.7 (Achievable Payoff Seller)** *If the seller can extract the preferred bidder’s total willingness-to-pay for avoiding the first-price auction, her payoff is highest under OM, followed by ROFR, and then FPA.*

## 3.5 Experimental Results

This section presents the experimental results. The experimental design allows for between-subject comparison of T-ROFR and T-OM and within-subject comparison of any of the forms of favoritism and the FPA. With ten independent observations per treatment, I run exact non-parametric, two-sided tests. The Wilcoxon Signed-Rank test (WSR) compares the distributions of observed with predicted values and paired data within one treatment. The Mann-Whitney U-test (MWU) is used for between-treatment comparisons.

TABLE 3.2: AVERAGE PAYOFFS AND ALLOCATIVE EFFICIENCY (PER ROUND)

	T-ROFR			T-OM			
	FPA	p-within <sup>a</sup>	ROFR	p-between <sup>b</sup>	OM	p-within <sup>a</sup>	FPA
<b>Payoff Regular Bidder</b>	8.19 (2.52)	0.002	5.54 (1.36)	0.853	5.29 (1.10)	0.002	9.63 (4.41)
<b>Payoff Preferred Bidder<sup>c</sup></b>	10.81 (1.70)	0.002	22.77 (2.83)	0.001	31.88 (2.71)	0.002	7.49 (3.09)
<b>Auction Revenue<sup>d</sup></b>	47.33 (4.31)	0.002	36.85 (4.38)	0.001	25.4 (0.87)	0.002	46.96 (5.28)
<b>Achievable Payoff Seller<sup>e</sup></b>	47.40 (5.33)	0.010	54.05 (5.24)	0.002	46.55 (2.29)	0.992	47.08 (5.33)
<b>Joint Payoff</b>	58.21 (4.42)	0.105	59.63 (2.91)	0.063	57.28 (2.51)	0.084	54.57 (6.20)
<b>Allocative Efficiency</b>	0.88 (0.06)	0.625	0.87 (0.05)	0.001	0.75 (0.04)	0.002	0.88 (0.08)

*Note:* Standard deviation across independent observations in parenthesis.

*Legend:* <sup>a</sup> two-sided Wilcoxon Signed-Rank test; <sup>b</sup> two-sided Mann-Whitney-U test; <sup>c</sup> lump-sum payment for auction with favoritism excluded; <sup>d</sup> revenue from auction price, accounting for the transfer payment under OM and excluding the lump-sum payment for auction with favoritism; <sup>e</sup> full extraction of willingness-to-pay with cap at 25 assumed.

Table 3.2 gives an overview of the descriptive statistics over all 60 rounds, together with within- and between-treatment comparisons for all hypotheses except for Hypothesis 3.6. Except for the joint payoff in T-OM and the auction revenue under OM, all empirical values reported in Table 3.2 are significantly different from the theoretically predicted ones in Table 3.1 ( $p_{WSR} < 0.05$ ).<sup>15</sup>

In line with Hypothesis 3.2, the preferred bidder benefits from both forms of favoritism. She does significantly more so in T-OM than in T-ROFR, where the price she pays under the auction with favoritism is determined by risk-averse regular bidders, who overbid substantially (see Section 3.6).

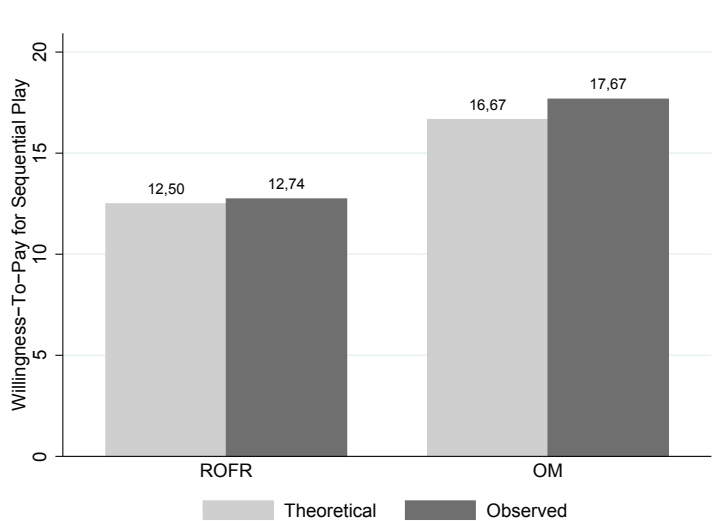


FIGURE 3.3: AVERAGE WILLINGNESS-TO-PAY FOR AN AUCTION WITH FAVORITISM (NO OVERBIDDERS)

Figure 3.3 depicts the average willingness-to-pay reported by the preferred bidders in both treatments.<sup>16</sup> In line with Hypothesis 3.6 and the empirically observed differences between the preferred bidder's payoff in auctions with and without favoritism, subjects in T-OM report significantly higher

<sup>15</sup>For the variables reported in Table 3.2, significant differences between the first 30 and the last 30 rounds are observed only for the T-OM treatment where the payoff of the preferred bidder under OM increases significantly from 30.31 ECU (SD=3.20) to 33.23 ECU (SD=2.28) and the respective joint surplus increases from 55.80 ECU (SD=3.61) to 58.65 ECU (SD=2.33). As shown in the appendix, these differences are the result of more truthful valuation reports after restart.

<sup>16</sup>Figure 3.3 excludes data with values higher than 25 ECU, since neither the probability to compete in an auction with favoritism nor the possible lump-sum payment are affected by such announcements. Including this data and capping the willingness-to-pay at 25 ECU gives average willingness-to-pay of 14.03 ECU (SD=4.88) in T-ROFR and 19.08 ECU (SD=3.21) in T-OM. Hence, the qualitative results remain the same.



willingness-to-pay to avoid the first-price auctions than their counterparts in T-ROFR ( $p_{MWU} = 0.043$ ). Both values are not significantly different from the theoretical maximum willingness-to-pay of a risk-neutral preferred bidder, which equals 12.5 in T-ROFR and 16.66 in T-OM ( $p_{WSR}^{ROFR} = 0.084$  and  $p_{WSR}^{OM} = 0.375$ ).

The rest of the theoretical predictions enjoy only partial support. Possible reasons for the observed deviations are discussed in Section 3.6. While the payoff of the regular bidder is significantly lower under both forms of favoritism in comparison to her payoff under FPA, OM does not lead to higher rent extraction than ROFR as was predicted in Hypothesis 3.1. Furthermore, the experimental results do not support the prediction that the joint surplus of the preferred bidder and the seller is increased by both forms of favoritism and maximized under OM (Hypothesis 3.4). The average joint surplus under OM is marginally higher than the respective joint surplus under FPA. However, it is also marginally lower than the average joint surplus under ROFR. This provides additional support for the observation that OM is incapable of optimizing rent extraction from the regular bidder.

When the seller does not receive any lump-sum payment from the preferred bidder in the negotiation stage, then her payoff is determined only by the auction revenue. In line with Hypothesis 3.3 and with the general literature on favoritism (see McAdams and Schwarz 2007, for example), the direct effect of this unconditional favoritism on the seller's payoff is negative. However, contrary to the theoretical predictions, the negative effect is higher in T-OM than in T-ROFR. Furthermore, assuming that in the negotiation stage the seller has the bargaining power and makes a take-it-or-leave-it offer to the preferred bidder, such that the lump-sum payment required in exchange for the auction for favoritism extracts all of the preferred bidder's willingness-to-pay, the seller's payoff remains higher under ROFR than under OM. This result shows that under OM the rents extracted from the regular bidder are insufficient to outweigh the losses in revenue. Hence, contrary to Hypothesis 3.7, favoritism can benefit the seller only if it is in the form of a ROFR, but not a OM.

Finally, the experimental results suggest that, contrary to Hypothesis 3.5, favoritism does not necessarily impair the allocative efficiency of the auction.

This is due to the first-price auction not being perfectly efficient. Hence, a seller would prefer favoritism with ROFR over favoritism with OM out of both – payoff-maximizing and allocative efficiency concerns.

### 3.6 Discussion

This section discusses two complementary explanations for the disparities between the theoretical predictions and the experimental results. In fact, the theoretical analysis in Section 3.2 is based on a couple of assumptions, which are not fulfilled in the experimental laboratory. It is demonstrated that the experimental results can be organized, firstly, by abandoning the assumption of risk-neutral bidders and, secondly, by allowing the preferred bidders to be boundedly rational and not reveal their true valuation under OM. In particular, heterogenous risk-aversion organizes the ranking of the observed auction revenue and the allocative efficiency, while the preferred bidder’s deviations from the optimal reporting strategy under OM explain the lack of additional rent extraction and the relatively low joint surplus under OM.

Risk-neutral (regular) bidders should submit bids equal to half of their valuations under FPA (and ROFR). Table 3.3 presents fixed effects estimates for the bidding functions of the preferred bidder (I) and the regular bidder (II) under FPA, and the regular bidder under FPA and ROFR (III). The estimates are clustered on the observation level, with robust error terms and controls for the effect of repetition. The baseline is the first-round bidding function under FPA. Treatment effects are accounted for through full interaction effects.<sup>17</sup> In line with the theoretical predictions, the intercepts over all models are not significantly different from 0 and the presence of favoritism does not affect the bid of the regular bidder. However, contrary to the theoretical predictions and in line with the experimental literature on first-price auctions, the slope of the bidding function is significantly different from 0.5 across all models.

It is well established that risk aversion is one of the reasons for such over-bidding.<sup>18</sup> As shown in the appendix section, risk aversion alters behavior

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<sup>17</sup>Estimating the same models with random effects shows that there is no significant treatment effect on the intercept.

<sup>18</sup>There is a large body of literature discussing the relative importance of risk aversion in explaining bidding behavior in first-price auctions. For an excellent if somewhat dated overview, see Kagel (1995). Recent developments have provided a number of alternative

TABLE 3.3: FIXED EFFECTS ESTIMATES FOR THE BIDDING FUNCTIONS UNDER FPA AND ROFR

Variable/Model	(I)	(II)	(III)
Valuation	0.633***	0.712***	0.706***
T-OM*Valuation	0.116**	-0.039	
ROFR			0.028
ROFR *Valuation			0.027
Round	0.031	0.039	0.036
T-OM*Round	-0.088*	-0.111*	
ROFR*Round			-0.001
Constant	-0.918	0.819	0.008
N	879	879	1800
Nr. of clusters	20	20	10
R <sup>2</sup> between	0.703	0.524	0.269
R <sup>2</sup> within	0.895	0.844	0.854
R <sup>2</sup> overall	0.845	0.791	0.805

*Legend:* \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ ; (I) preferred bidder under FPA; (II) regular bidder under FPA; (III) regular bidder under FPA and ROFR.

under FPA and ROFR, but does not change the predictions of the OM model. In particular, when bidders are risk-averse, the optimal bids under FPA and ROFR increase, whereas a risk-averse preferred bidder should continue to report her true valuation under OM (see below). As a result, the probability that under ROFR a preferred bidder wins the auction despite having the lower valuation decreases, while the price she pays increases. This implies that risk aversion reinforces the predictions in Hypothesis 3.2, namely that the payoff of the preferred bidder is highest under OM, followed by ROFR, and then by FPA, and decreases the negative effect of favoritism on the allocative efficiency under ROFR, but not under OM.

**Corollary 3.1** *When bidders are risk-averse, the allocative efficiency under OM is lower than the allocative efficiency under ROFR.*

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explanations for the frequently observed overbidding (see Andreoni, Che and Kim 2007; Armantier and Treich 2009; Engelbrecht-Wiggans and Katok 2007; Filiz-Ozbay and Ozbay 2007, for examples of alternative behavioral explanations, such as spite, regret, and non-linear probability weighting). However, they usually complement risk aversion and cannot eliminate it as an explanatory variable.

From the perspective of the seller, risk aversion leads to a higher winning bid under FPA and a higher regular bidder's bid under ROFR. Behavior and revenue remain the same under OM. As a result, the expected auction revenue is no longer equal under both forms of favoritism.

**Corollary 3.2** *When bidders are risk-averse, the expected auction revenue is lowest under OM, followed by ROFR, and then by FPA.*

The predictions of both corollaries are in line with the experimental results in Table 3.2. Given that risk aversion affects the preferred bidder's payoff and the auction revenue in an opposite manner, its impact on the achievable payoff of the seller and the joint surplus of the seller and the preferred bidder is ambiguous.

Note that the corollaries do not require any assumptions about the individual levels of risk aversion. If heterogeneity of risk preferences is accounted for, then the bidding functions under FPA are no longer symmetric. Thus, the bidder with the lower valuation can win the auction with a positive probability, suggesting that the expected allocative efficiency of the FPA is no longer equal to 1. Depending on the extent to which heterogeneous risk preferences cause the allocative efficiency under FPA to decline, ROFR does not necessarily harm the allocative efficiency as predicted in Hypothesis 3.5.<sup>19</sup>

So far we could establish that accounting for risk aversion organizes the experimental results concerning the auction revenue and the allocative efficiency. However, it is still puzzling that OM does not maximize rent extraction from the regular bidder and the preferred bidder's and the seller's joint surplus. Figure 3.4, which depicts the preferred bidder's reported valuation in relation to their true one, throws some light on the reasons behind the inability of OM to maximize the coalition's surplus.

The predictions of the optimal mechanism assume that the preferred bidder truthfully reports her valuation in Stage 2.2 of the second game. In the appendix, it is demonstrated that the elicitation mechanism is such that even a

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<sup>19</sup>For example, assume that the preferred bidder is risk-neutral and has the higher valuation. Then allocation under ROFR would be efficient for any level of risk-aversion of the regular bidder. However, when the regular bidder has a sufficiently high valuation (which is lower than the one of the preferred bidder) and is sufficiently risk-averse, then her bid in the FPA would be higher than the preferred bidder's bid in the FPA, leading to inefficient allocation.

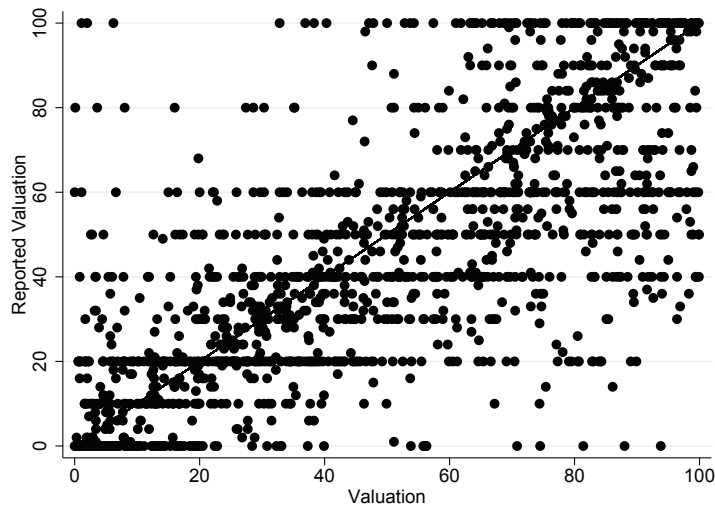


FIGURE 3.4: REPORTED VALUATIONS BY THE PREFERRED BIDDER IN T-OM

risk-averse preferred bidder should report her true valuation. The frequent and large deviations from the 45 degree line indicate that she only rarely reports a valuation which is near her true value. The deviation of the reported from the true valuation does not exceed 1 ECU in only 8.2% of the cases. Moreover, the absolute deviation exceeds 5 ECU in more than 70% of the cases. The reported valuation is on average 4.87 ECU (SD=4.45) lower than the true valuation, which suggests that there is a general tendency to underreport.<sup>20</sup> As Table 3.4 and 3.5 in the appendix show, misreporting does not pay off and therefore decreases over time. Nonetheless, in the last ten rounds, around 65% of preferred bidders still misreport by more than 5 ECU. Hence, although subjects were provided with a table that allowed them to identify truthful value revelation as an optimal strategy, the elicitation mechanism was incapable of inducing truth-telling. Given that the optimization properties of OM seem to be highly dependent on the (in)ability of the elicitation mechanism to induce truthful report revelation by boundedly rational agents, its feasibility cannot be supported.

Given that lack of truthful value revelation is one of the drivers behind

<sup>20</sup>Note that the results may be driven by a preference for round numbers, which is a phenomenon commonly reported in the psychological and financial markets literature (see ?, for example). In fact, while about 10.5% of the true valuations can be rounded to a number which is a multiple of 10, 66.9% of the reported valuations are numbers which are a multiple of 10.

OM's relatively bad performance, it may also be useful to analyze how ROFR compares to an optimal mechanism where there are no informational asymmetries between the seller and the preferred bidder in Stage 2. This would be the case, for example, if the seller and the preferred bidder were members of a corporate enterprise that needs to make a make-or-buy decision. When the experimental results for ROFR are compared to the theoretical predictions for OM, the qualitative results remain the same – under ROFR the payoff of the preferred bidder is significantly lower than 33.33 ECU ( $p_{WSR} = 0.001$ ), the auction revenue is significantly higher than 25.00 ECU ( $p_{WSR} = 0.001$ ), and the joint surplus is significantly higher than 58.33 ECU ( $p_{WSR} = 0.023$ ). This result seems to be driven by the fact, that risk aversion on the side of the regular bidder affects the auction revenue under ROFR but not under OM. Note that this result may be an artefact of the experimental design, which due to lack of competition among the regular bidders, does not allow us to investigate the effect of risk aversion on their bidding strategies under OM.

### 3.7 Conclusion

This study provides empirical evidence that favoritism can increase the joint surplus of a seller and a preferred bidder. Provided that the seller can collect a sufficient part of the additional surplus, she benefits from granting the right of first refusal but not from the auction with an optimal reserve price. This study also sheds light on the feasibility of the implicit assumption that the seller knows the preferred bidder's valuation, which is present in some of the literature on favoritism and corruption in auctions with a share-based division of surplus among the coalition partners. In particular, Burguet and Perry (2009) provide a mechanism for the elicitation of the preferred bidder's valuation that I show to be incentive compatible for risk-averse bidders as well. However, in the experiment the reported valuations deviate from the true ones. The difference between the two decreases with repetition but remains significant. As noted in the introduction, the willingness of private companies to reveal their true costs when they interact repeatedly with a procurer is also questionable. Hence, more research is needed into alternative mechanisms to form coalitions, which do not require symmetric information of the coalition partners or whose optimization properties are more robust to deviations from

equilibrium play.

The experimental results also suggest that even when there are no information asymmetries between the seller and the preferred bidder, an auction with the right of first refusal tends to outperform an auction with an optimal reserve price. However, this result may be driven by the particular experimental setting, where there is only one regular bidder. As a result, the seller and the preferred bidder can extract risk rents only in the auction with the right of first refusal but not in the auction with the optimal reserve price, where the regular bidder's acceptance of the take-it-or-leave-it offer is not affected by her risk preferences. Competition among the regular bidders may lead to different results.

It is important to note that the experimental results rely on a couple of critical assumptions. Firstly, in line with the general experimental literature, the preferred bidder's willingness-to-pay for the respective auction with favoritism is elicited via the Becker-DeGroot-Marschak-method (Becker et al. 1964). However, a common problem with this instrument is that it is not incentive compatible for decisions made under uncertainty (Horowitz 2006; Karni and Safra 1996). As the preferred bidder's true benefit from favoritism is a random variable, the empirical data for the potential benefits for the seller may therefore be unreliable.

Finally, the analysis assumes that the existence of publicly known favoritism does not discourage the non-preferred bidders from participating in the auction. However, if bidders incur entry costs for preparing their bids, for example, then the common knowledge of favoritism could lead to lower participation. This in turn decreases the rents that can be extracted from the non-preferred bidders (Walker 1999), so that they no longer outweigh the losses in auction revenue. Future research should attempt to provide more insights on the effect of favoritism in auctions with costly participation.

## 3.8 Appendix

### 3.8.1 Proof of Proposition 3.3, (iii)

**Proof** In Stage 3 the reserve price  $r$  is commonly known. Following Krishna (2002, Ch. 2.5), in a symmetric equilibrium with  $N - 1$  regular bidder, the increasing bid function equals

$$\beta^{OM}(x) = r \frac{F(r)^{N-2}}{F(x)^{N-2}} + \frac{1}{F(x)^{N-2}} \int_r^x z(N-2)F(z)^{N-3}f(z) dz \quad (3.18)$$

for  $x > r$  and 0 otherwise. In the symmetric, increasing equilibrium, the expected payoff of a regular bidder  $R_j$ ,  $j = \{1, 2, \dots, N - 1\}$ , with valuation  $x$  is thus  $\Pi_{R_j}^{OM}(x, r) = F(x)^{N-2}(x - \beta^{OM}(x, r))$ .

Before Stage 3,  $r$  is a function of P's random valuation, giving an expected payoff of a regular bidder with valuation  $x$  equal to

$$\begin{aligned} \Pi_{R_j}^{OM}(x, r(z)) &= F(x)^{N-2}(x - E[\beta^{OM}(x, r(z)) | x > r(z)])\pi(x > r(z)) \\ &= F(x)^{N-2} \left( xF(r^{-1}(x)) - \int_{\underline{v}}^{r^{-1}(x)} \beta^{OM}(x, r(z))f(z) dz \right). \end{aligned} \quad (3.19)$$

Thus, in Stage 1, her ex ante expected payoff is given by

$$\begin{aligned} E\Pi_{R_j}^{OM}(\cdot) &= \int_{r(\underline{v})}^{\bar{v}} f(x)F(x)^{N-2} \left( xF(r^{-1}(x)) \right. \\ &\quad \left. - \int_{\underline{v}}^{r^{-1}(x)} \beta^{OM}(x, r(z))f(z) dz \right) dx. \end{aligned} \quad (3.20) \quad \blacksquare$$

### 3.8.2 Bidding Functions and Allocative Efficiency with Risk-Aversion

In the following, the bidding functions under FPA and ROFR are formally derived. In line with Cox, Roberson and Smith (1982) and the majority of the auction literature, a utility function in the form  $U_i(y) = y_i^\alpha$  is assumed, where  $y \in \mathbb{R}_+$ ,  $\alpha_i \in (0, 1]$ , and  $1 - \alpha_i$  is the measure of Arrow-Pratt constant



relative risk aversion of player  $i$ .<sup>21</sup> For tractability, assume that bidders share the same risk preferences. Valuations are i.i.d. on  $F(x) \sim U[\underline{v}, \bar{v}]$ . Cox et al. show that under FPA the increasing equilibrium bid function is given by  $\beta^{FPA}(x, \alpha) = (x - \underline{v})/(1 + \alpha) + \underline{v}$ .<sup>22</sup>

Under ROFR the regular bidder submits a bid  $b^{ROFR}$ , which maximizes her expected utility  $U_R^{ROFR}(x, b^{ROFR}, \alpha) = F(b^{ROFR})(x - b^{ROFR})^\alpha$ . With some abuse of notation, the first-order condition equals

$$\partial U / \partial b = f(b)(x - b)^\alpha - \alpha F(b)(x - b)^\alpha.$$

This first-order condition is fulfilled when

$$f(b)(x - b) - \alpha F(b) = 0 \Leftrightarrow x - b = \alpha(b - \underline{v}) \Leftrightarrow x + \alpha \underline{v} = b(\alpha + 1).$$

Rearranging this term gives  $\beta^{ROFR}(x, \alpha) = (x - \underline{v})/(1 + \alpha) + \underline{v}$ , which is identical to the equilibrium bid function in the standard first-price auction. Hence, the bidding behavior of a risk-averse regular bidder is also not affected by the presence of right of first refusal, when  $F(x)$  is a uniform distribution. Furthermore, for any  $\alpha \in (0, 1)$  and  $x \in F(x) \setminus \{\underline{v}\}$  the equilibrium bidding function with risk aversion is greater than the one without.

As noted in Section 3.6, as long as both bidders share the same risk preferences, the allocative efficiency of the first-price auction is not affected by risk aversion. In the following, the allocative efficiency of the ROFR is derived, where  $\alpha$  denotes the constant relative risk aversion of the regular bidder. Note that  $F(\beta^{ROFR}(x)) = \frac{(x - \underline{v})/(1 + \alpha) + \underline{v} - \underline{v}}{\bar{v} - \underline{v}} = F(x)/(1 + \alpha)$ . Hence, when  $\pi(\Omega)$  denotes the probability that an event  $\Omega$  occurs and  $P$ 's valuation equals  $x_P$ , then  $\pi(x_P > \beta^{ROFR}(\bar{v})) = \alpha/(1 + \alpha)$  and  $\pi(x_P \leq \beta^{ROFR}(\bar{v})) = 1/(1 + \alpha)$ .

Analogous to equation (3.16), when the regular bidder has risk preferences such that  $\alpha \in (0, 1)$ , the expected allocative efficiency under ROFR is given

<sup>21</sup>Cox et al. (1982) note that due to  $-yU_i''(y)/U_i'(y) = 1 - \alpha_i$ , the interpretation of  $1 - \alpha$  as Arrow-Pratt's constant relative risk aversion is valid only when the utility function is defined on income, but not on terminal wealth.

<sup>22</sup>Note that the solution in Cox et al. (1982) assumes that the bidding function does not exceed some  $\bar{b}$ . Krishna (2002), for example, shows that the equilibrium bidding strategy with two bidders with identical risk preferences, whose values are drawn from  $F(x)$ , is the same as the equilibrium bidding strategy with two risk-neutral bidders, whose values are drawn from a distribution  $F_\alpha(x) = F(x)^{1/\alpha}$ . Hence, when  $\bar{b} = \beta^{ROFR}(\bar{v}, \alpha)$  is assumed, no further analysis for bids above  $\bar{b}$  is required.

by

$$\begin{aligned}\theta^{ROFR}(\alpha) &= 1 - \left[ \frac{\alpha}{1+\alpha} \int_{\underline{v}}^{\bar{v}} F(z)f(z) dz + \frac{1}{1+\alpha} \int_{\underline{v}}^{\bar{v}} (F(z) - F(\beta^{ROFR}(z)))f(z) dz \right] \\ &= \frac{(\alpha+1)^2 + 1}{2(1+\alpha)^2} = \frac{1}{2} + \frac{1}{2(1+\alpha)^2}.\end{aligned}\tag{3.21}$$

It is obvious that the allocative efficiency under ROFR is higher for any  $\alpha < 1$  than for  $\alpha = 1$ . Expression (3.21) also demonstrates that for an infinitely risk-averse regular bidder, ROFR leads to perfect allocative efficiency.

### 3.8.3 Misreporting under OM

Matthews (1983) demonstrates then when the regular bidder is risk-averse,  $r(x)$  no longer optimizes the expected joint surplus of the seller and the preferred bidder (regardless of the preferred bidder's risk preferences).<sup>23</sup> However, if the elicitation mechanism is such that it fulfills all requirements of Lemma 2 in Maskin and Riley (1984), its incentive compatibility remains unaffected by the preferred bidder's risk preferences.

**Proposition 3.6** *The elicitation mechanism under OM with allocation rule  $F(r(x))^{N-1}$  and transfer  $T(x) = \int_{\underline{v}}^x F(r(z))^{N-1} dz$ , where  $r(x)$  is the reserve price defined in Section 3.2, fulfills the conditions in Lemma 2 in Maskin and Riley (1984).*

**Proof** Firstly, the utility function is such that  $U(y)$  is continuously differentiable,  $U'(y) > 0$ ,  $U''(y) < 0$ , and  $\partial^2 U(y_1, y_2) / \partial y_1 \partial y_2 \leq 0$  for every  $\alpha \in (0, 1)$ . Furthermore, the probability to win  $F(r(x))^{N-1}$  and the transfer  $T(x)$  are non-decreasing in  $x$ . Denote with  $V(x, x) = \max_{\hat{x}} V(\hat{x}, x)$  the maximized expected utility, where

$$V(\hat{x}, x) = F(r(\hat{x}))^{N-1} U(x + T(\hat{x}) - \hat{x}) + (1 - F(r(\hat{x}))^{N-1}) U(T(\hat{x})).$$

Since  $V(x, x)$  is an increasing function, Lemma 2 in Maskin and Riley (1984) postulates that a mechanism is incentive compatible when it fulfills the follow-

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<sup>23</sup>In fact, the joint surplus could be increased, if instead of a reserve price with sure outcome, the regular bidder is offered a lottery which extracts additional rents for risk avoidance.

ing condition:

$$\frac{d}{dx}V(x, x) = \frac{\partial}{\partial \hat{x}}V(x, \hat{x}). \quad (3.22)$$

As  $V(x, \hat{x}) = F(r(x))^{N-1}U(\hat{x} + T(x) - x) + (1 - F(r(x))^{N-1})U(T(x))$ , the term on the right-hand side equals

$$\begin{aligned} \frac{\partial V(x, \hat{x})}{\partial \hat{x}} \Big|_{\hat{x}=x} &= F(r(x))^{N-1} \alpha (\hat{x} + T(x) - x)^{\alpha-1} \\ &= F(r(x))^{N-1} \alpha T(x)^{\alpha-1}, \end{aligned} \quad (3.23)$$

while the term on the left-hand side equals

$$\frac{d}{dx}V(x, x) = \frac{dT(x)^\alpha}{dx} = \alpha T(x)^{\alpha-1} F(r(x))^{N-1}. \quad (3.24)$$

From (3.23) and (3.24) it is obvious that the condition in (3.22) is fulfilled. ■

The intuition behind this result is as follows. Due to the properties of  $T(x)$  and  $F(r(x))$ , a risk-neutral preferred bidder does not have incentives to report valuations lower than her true one. However, for any  $\hat{x} > x$  she incurs costs that in expectation are higher than the benefit of the additional transfer. Hence,  $\hat{x} = x$  maximizes the trade-off between the secure payment  $T(x)$  and the expected costs (or benefits) from  $x - \hat{x}$ . A risk-averse preferred bidder has a higher utility from a secure payment  $T(x)$  than a lottery with the same expected payment. Hence, she would have even stronger incentives to avoid the risk of not winning when  $x > \hat{x}$  or winning, but incurring costs when  $x < \hat{x}$ . Thus, risk aversion reinforces the pull towards  $\hat{x} = x$ .

Despite the theoretical incentive compatibility of the elicitation mechanism, there is significant misreporting with absolute deviation between the reported and true valuation equal to 17.562 ECU (SD=3.521) in the first 30 rounds and 14.275 ECU (SD=3.303) in the second 30 rounds. Thus, although with repetition deviations from equilibrium decrease, they do not disappear. Tables 3.4 and 3.5 support the analysis in Section 3.6.

TABLE 3.4: FIXED EFFECTS ESTIMATES OF THE EFFECT OF MISREPORTING ON THE PREFERRED BIDDER'S PAYOFF

Variable/Model	(I)	(II)	(III)
reported - true valuation	-0.162**	-0.153**	-0.159**
Valuation	0.742***	0.705***	0.707***
Winner		9.222***	9.196***
Round			-0.026
Constant	-8.482***	-13.377***	-12.541***
N	1482	1482	1482
Nr. of clusters <sup>a</sup>	10	10	10
R <sup>2</sup> between	0.566	0.565	0.569
R <sup>2</sup> within	0.729	0.754	0.754
R <sup>2</sup> overall	0.726	0.75	0.751

Legend: \* p<0.05; \*\* p<0.01; \*\*\* p<0.001; <sup>a</sup> clustered on the observation level, with robust error terms.

TABLE 3.5: FIXED EFFECTS ESTIMATES OF THE EFFECT OF REPETITION ON MISREPORTING

Variable/Model	(I) <sup>a</sup>	(II) <sup>a</sup>	(III) <sup>a</sup>	(IV) <sup>a</sup>
Round	-0.124*	-0.419**	-0.135**	-0.446**
Round squared		0.005**		0.005**
Valuation			0.128**	0.128**
Constant	19.760***	22.828***	13.721***	16.920***
N	1482	1482	1482	1482
Nr. of clusters <sup>b</sup>	10	10	10	10
R <sup>2</sup> between	0.007	0.004	0.001	0.001
R <sup>2</sup> within	0.024	0.033	0.096	0.106
R <sup>2</sup> overall	0.018	0.0251	0.0745	0.0825

Legend: \* p<0.05; \*\* p<0.01; \*\*\* p<0.001; ; <sup>a</sup> dependent variable is |reported valuation - true valuation|; <sup>b</sup> clustered on the observation level, with robust error terms.

### 3.8.4 Instructions

*In the following the instructions for the experiment are provided. A [Treatment] indicates which sections were present only in the instructions of the respective treatment.*

Welcome and thank you for participating in this experiment! Please read these instructions thoroughly. We kindly ask you to refrain from talking to or in any other way communicating with other participants. We also ask you to turn off your mobile phones. Please raise your hand if you have any questions. The experimenter will then come to you and answer your questions. All participants have received the same instructions.

### **General Information**

You will be able to earn money during this experiment. Your respective amount depends on your own decisions but also on decisions of other participants. During the experiment your current payoff is calculated in ECU (Experimental Currency Units). 30 ECU equals 1 Euro. At the end of the experiment your profits will be converted into Euro according to this exchange rate and paid out to you in cash plus a 2.50 Euro show-up fee.

This experiment is made up of two identical parts of 30 rounds respectively. You will receive a starting credit of 100 ECU at the beginning of each part. This amount can vary as a result of your behavior over the 30 rounds. Whether part 1 or part 2 is relevant for your final payoff, will be determined by chance at the end of the experiment.

In each round two persons interact with each other. We will denote one person as “participant A” and the other as “participant B”. You will be assigned your role, participant A or participant B, by chance at the beginning of the experiment. You will keep the role you have been assigned over the entire experiment. In each round, you will be randomly matched to another participant. The matching of participants will remain strictly anonymous.

### **How can I earn money?**

In each of the 30 rounds of one part of the experiment, you will be able to acquire a fictitious good. If in one round you acquire the fictitious good, the experimenter will credit you with ECU at the end of this round. The respective amount of ECU depends on your “valuation” of the fictitious good. For your final payoff for the respective round other costs or credits that may arise will be deducted or respectively added to this valuation (see below). The valuation will vary between participants and rounds. The valuation for both participants will be determined by chance and will be a number with one decimal place,

between 0.0 ECU and 100.0 ECU. Each number will be equally likely. The valuation of the other participant will be determined according to the same rules as yours, it will be independent from your valuation, and usually it will differ from yours. (At no time point of the experiment will you be informed about the valuation of the other participant.)

### **The course of a round?**

The chart on the next page illustrates the possible course of one round. The following text outlines the individual steps in detail.

[Figure 3.1 and 3.2 were respectively displayed.]

### **Explanations to the chart**

The price decisions of the two matched participants determine who purchases the fictitious good. Participant A can influence whether the price decision is taken *simultaneously* or *sequentially* (see chart on the previous page). Both participants learn their respective valuation as soon as the course of the round has been determined.

If price decisions are taken *simultaneously*, both participants submit their price for the fictitious good at the same time without receiving any further information. The participant, who submit the higher price, receives the fictitious good and pays the *price he submitted*. The other participant does not receive the good and pays nothing. In case of a tie, who receives the fictitious good is determined by chance.

[ROFR] If price decisions are taken *sequentially*, participant B submits his price first. Participant A can then acquire the good at the *price submitted by participant B*. If he turns this price down, then participant B receives the good and pays *the price he submitted*.

[OM] If price decisions are taken *sequentially*, participant A specifies a price between 50.0 and 100.0 ECU. Participant B can acquire the fictitious good *at this price*. If he turns this price down, then participant A receives the fictitious good and pays *a price, which depends on the price he submitted, but differs from it*. Additionally, participant A is credited a fixed transfer that also depends on the price specified for participant B, regardless of who receives the good. The table at the end of these instructions presents the prices and the transfers for participant A for some alternative price specifications for participant B. Participant A can use an automatic calculator to learn the exact amounts for

other price specifications before he commits to a certain price.

### **How does participant A decide on the course of a round?**

At the beginning of each round, before the participants have received any information about their valuation, participant A states the maximum amount he is willing to pay in order for price decisions to be taken *sequentially* (and *not simultaneously*). The actual price he has to pay is determined by chance. The price is a number with one decimal place between -10.0 ECU and 25.0 ECU. (Note: A negative price corresponds to a credit.) Each number is equally likely. If the amount stated by participant A is at least as high as the price determined by chance, then price decisions are taken *sequentially* during this round, and at the end of the round participant A *pays the price determined by chance*. Otherwise, price decisions are taken *simultaneously*, and participant A has *no extra costs* at the end of this round. Please note, that participant A does not pay the price he stated, but the price determined by chance. It is, therefore, optimal for him to consider what sequential price decision making (instead of simultaneous price decision making) is worth to him and state this amount as his maximum willingness-to-pay.

### **How high is the payoff of participant A at the end of each round?**

If price decisions are taken *simultaneously* and participant A receives the fictitious good, he is credited his valuation minus the price he submitted. If price decisions are taken *simultaneously* and participant A does not receive the fictitious good, then his payoff is zero.

[ROFR] If price decisions are taken *sequentially*, then participant A always has to pay the price for sequential price decisions determined by chance. If he receives the fictitious good, then he is credited his valuation minus the price submitted by participant B. If he does not receive the fictitious good, he does not get any credits.

[OM] If price decisions are taken *sequentially*, then participant A always has to pay the price for sequential price decisions determined by chance. Additionally, he is credited the fixed transfer from the experimenter, which depends on the price specified for participant B. If he receives the fictitious good, then he is also credited his valuation minus the price that results from his price specifications for participant B. If he does not receive the fictive good, he does not get any additional credits.

### **How high is the payoff of participant B at the end of each round?**

[ROFR] If participant B receives the fictitious good, then he is credited his valuation minus the price he submitted. If he does not receive the fictitious good, his payoff equals zero. His payoff at the end of a round, thus, does not depend on whether price decisions were taken *simultaneously* or *sequentially*.

[OM] If participant B receives the fictitious good and price decisions were taken *simultaneously*, then he is credited his valuation minus the price he stated. If participant B receives the fictitious good and price decisions were taken *sequentially*, then he is credited his valuation minus the price specified by participant A. If he does not receive the fictitious good, his payoff equals zero, regardless of whether price decisions were taken *simultaneously* or *sequentially*.

### **What happens if the payoff is negative in one round?**

Losses incurred in some rounds will be set off against gains in other rounds. If the dice determines that a part of the experiment in which you have accumulated losses is the part relevant for your final payoff, then these losses will be set off against the 2.50 Euro show-up fee.

### **What information do I receive?**

You will be informed about your own valuation once it has been determined, and whether price decisions are taken simultaneously or sequentially and before you have to make a price decision. Additionally, participant A is immediately informed about the price for sequential price decisions determined by chance.

After *simultaneous* price decisions, you will receive the following information at the end of each round: whether you have received the fictitious good, what was the highest price, and how high is your payoff for this round.

After *sequential* price decisions, you will receive the following information at the end of each round: the price determined by the other participant, whether you have received the fictitious good, and how high is your payoff for this round.

### **Final remarks**

In total, you will play for 60 rounds. At the end of the experiment, one participant will toss a dice and thereby determine which part of the experiment is relevant for your final payoff (uneven number = first part, even number =



second part). All rounds are relevant for your final payoff until the die has been tossed at the end of the experiment. Please raise your hand if you have any questions.

TABLE 3.6: PRICE AND TRANSFER FOR PARTICIPANT A (PT A)  
 DEPENDING ON THE PRICE SPECIFIED FOR PARTICIPANT B (PT B)

Price for PT B	Price for PT A	Transfer for PT A	Price for PT B	Price for PT A	Transfer for PT A	Price for PT B	Price for PT A	Transfer for PT A
50.0	0.0	0.0	67.0	34.0	19.9	84.0	68.0	45.6
50.5	1.0	0.5	67.5	35.0	20.6	84.5	69.0	46.4
51.0	2.0	1.0	68.0	36.0	21.2	85.0	70.0	47.3
51.5	3.0	1.5	68.5	37.0	21.9	85.5	71.0	48.1
52.0	4.0	2.0	69.0	38.0	22.6	86.0	72.0	49.0
52.5	5.0	2.6	69.5	39.0	23.3	86.5	73.0	49.8
53.0	6.0	3.1	70.0	40.0	24.0	87.0	74.0	50.7
53.5	7.0	3.6	70.5	41.0	24.7	87.5	75.0	51.6
54.0	8.0	4.2	71.0	42.0	25.4	88.0	76.0	52.4
54.5	9.0	4.7	71.5	43.0	26.1	88.5	77.0	53.3
55.0	10.0	5.3	72.0	44.0	26.8	89.0	78.0	54.2
55.5	11.0	5.8	72.5	45.0	27.6	89.5	79.0	55.1
56.0	12.0	6.4	73.0	46.0	28.3	90.0	80.0	56.0
56.5	13.0	6.9	73.5	47.0	29.0	90.5	81.0	56.9
57.0	14.0	7.5	74.0	48.0	29.8	91.0	82.0	57.8
57.5	15.0	8.1	74.5	49.0	30.5	91.5	83.0	58.7
58.0	16.0	8.6	75.0	50.0	31.3	92.0	84.0	59.6
58.5	17.0	9.2	75.5	51.0	32.0	92.5	85.0	60.6
59.0	18.0	9.8	76.0	52.0	32.8	93.0	86.0	61.5
59.5	19.0	10.4	76.5	53.0	33.5	93.5	87.0	62.4
60.0	20.0	11.0	77.0	54.0	34.3	94.0	88.0	63.4
60.5	21.0	11.6	77.5	55.0	35.1	94.5	89.0	64.3
61.0	22.0	12.2	78.0	56.0	35.8	95.0	90.0	65.3
61.5	23.0	12.8	78.5	57.0	36.6	95.5	91.0	66.2
62.0	24.0	13.4	79.0	58.0	37.4	96.0	92.0	67.2
62.5	25.0	14.1	79.5	59.0	38.2	96.5	93.0	68.1
63.0	26.0	14.7	80.0	60.0	39.0	97.0	94.0	69.1
63.5	27.0	15.3	80.5	61.0	39.8	97.5	95.0	70.1
64.0	28.0	16.0	81.0	62.0	40.6	98.0	96.0	71.0
64.5	29.0	16.6	81.5	63.0	41.4	98.5	97.0	72.0
65.0	30.0	17.3	82.0	64.0	42.2	99.0	98.0	73.0
65.5	31.0	17.9	82.5	65.0	43.1	99.5	99.0	74.0
66.0	32.0	18.6	83.0	66.0	43.9	100.0	100.0	75.0
66.5	33.0	19.2	83.5	67.0	44.7			

# Chapter 4

## The Effect of Different Allocation Mechanisms in Emissions Trading Markets

*The content of the following chapter was produced in collaboration with Veronika Grimm.*

### 4.1 Introduction

As part of their strategy to curb greenhouse gas emissions governments are increasingly adopting various forms of emissions trading. The European Union Greenhouse Gas Emission Trading System (EU ETS), the Regional Greenhouse Gas Initiative (RGGI) in the U.S., and the Carbon Pollution Reduction Schemes (CPRS) in Australia are just a few examples of its large-scale implementation. The reason behind emissions trading's increasing popularity is the prevalent belief among economists that it is a highly efficient mechanism to control greenhouse gases.<sup>1</sup>

The efficiency of a particular emissions trading market, however, depends on the details of its market design. One of the most controversial issues is the initial allocation mechanism for allowances. While in theory tradable emission permits allow the regulated industry to efficiently reduce its emissions regardless of the initial allocation (Montgomery 1972), in practice different allocation

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<sup>1</sup>While the regulator may have other goals, such as revenue maximization or transparency of the environmental instrument, we believe efficiency to be the most important one.

rules may lead to different outcomes. Two allocation mechanisms are usually discussed — free allocation (called grandfathering) and auctioning. Free allocation is preferred by the industry, which makes it more politically feasible. Auctioning is favored by the majority of economists due to its transparency and alleged efficiency (see Betz, Seifert, Cramton and Kerr 2010; Cramton and Kerr 2002; Hepburn, Grubb, Neuhoff, Matthes and Tse 2006; Holt et al. 2007; Ockenfels 2009), and is increasingly being used in the context of emissions trading<sup>2</sup>, firing the discussion about a proper auction mechanism.

Although in recent years different auction formats have been used prior to emission trading in RGGI and EU ETS it is hardly feasible to directly compare the success of auctions and grandfathering using field data.<sup>3</sup> Auctions typically cover only a part of the allocated permits and coexist with grandfathering. Still, most previous and current recommendations for auction design are based on theoretical argumentation without any systematic empirical evidence from the field or controlled experiments.<sup>4</sup> We contribute to filling this gap by running an experiment which investigates market performance under two commonly recommended auction designs (Neuhoff, Matthes, Betz, Dröge, Johnston, Kudelko, Löschel, Monjon, Mohr, Sato and Suwala 2008) and compare them to a special kind of free allocation procedure. In particular, we investigate the effects of more and less frequent auctioning on the efficiency of a secondary market for emission permits. To control for any effects caused by the auction as a method of allocation we also run a treatment with grandfathered permits, which is identical to the treatment with less frequent auctioning in all respects except for the free allocation of permits.

We find that the frequency of auctioning affects the ability of the market to deliver cost-efficient joint compliance within the system. We attribute this observation to an interaction effect between the auction and the secondary

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<sup>2</sup>At least 70% (and increasing) of the emission permits in the RGGI have already been auctioned. A large share of the permits in the CPRS and EU ETS (up to 70% in 2020) will be auctioned.

<sup>3</sup>Most of the auctions were single-round, sealed-bid, uniform-price formats, however, they differed in terms of their frequency and the supply-demand ratio. Auctions in RGGI take place on quarterly basis and have been lately characterized by supply surplus, (RRGI 2012), while auctions in EU ETS are without ex ante fixed and commonly know dates and with significant shortage of supply (Co2-Handel.de 2012a; CO2-Handel.de 2012b).

<sup>4</sup>An exception is the design of the RGGI auctions, where Holt et al. (2007) had run a series of experiments within the design process.

market and to stronger overbidding in the smaller but more frequent auctions. In addition to inducing more efficient prices, less frequent auctioning leads to more efficient allocation after trade on the secondary markets. As a result, total compliance cost tends to be lower if permits are auctioned less frequently. When studying the method of allocation – for free or against a payment – we find that it does not affect the efficiency of final allocation of permits (after trade on secondary market). However, it influences the temporal development of market prices as well as banking and borrowing behavior. On the one hand, auctioning leads to earlier abatement activities, on the other hand, it increases total compliance cost. Under all allocation rules we studied, the static efficiency of the emissions trading system is hampered by naive abatement and trade decisions. As a result, neither market managed to deliver nearly perfect allocation despite the use of double auction as trading institution.

Let us briefly review the related literature. The experimental and theoretical literature provides abundant evidence for the effect of different design aspects on the efficiency of an emission permit market.<sup>5</sup> However, we are aware of only few experiments that study the effect of different initial allocation rules on the functioning of a subsequent trading market. Previous work that also analyzes a secondary market includes experimental comparisons of auctioning and grandfathering by Benz and Ehrhart (2007) and Goeree, Holt, Palmer, Shobe and Burtraw (2010), as well as a comparison of different types of auctions by Holt et al. (2007). Our work differs from those studies with respect to the design of the grandfathering treatment, the use of increasing (instead of constant) marginal abatement costs functions, and the trading institution employed. In an attempt to closely mirror a real trade exchange, we use continuous double auction, which is known for its high efficiency in the lab<sup>6</sup> as opposed to the dynamic uniform double auction and the single-round, limit-order, call market used in Benz and Ehrhart (2007) and Goeree et al. (2010), respectively.

Holt et al. (2007) compare several auction types and find that sealed-bid and

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<sup>5</sup>For review of experiments on banking, liability rules, and regulatory enforcement, see Cason (2010). For the role of transaction costs, see Stavins (1995) and Cason and Gangadharan (2003). For the role of the trading institution, see Cason and Gangadharan (1998) and Muller, Mestelman, Spraggon and Godby (2002).

<sup>6</sup>For examples of highly efficient continuous double auction in the context of emission trading see Ledyard and Szakaly-Moore (1994) and Sturm (2008).

ascending-clock auctions perform equally well. They recommend frequent auctioning for reasons of planing security and liquidity constraints of generators, as well as competition considerations. However, they do not provide experimental results that support this conjecture. Benz and Ehrhart (2007) and Goeree et al. (2010) compare auctioning and grandfathering and find that, at least for relatively stringent emission caps, markets with auctioned permits tend to outperform markets with grandfathered permits. However, in both experiments the initial allocation before trade is endogenous in the auction treatments and (arbitrarily) exogenous in the grandfathering treatments. Hence, the observed differences in the final permit allocation can be due to both, the initial allocation<sup>7</sup> as well as the allocation method. The specifics of our allocation design allow us to disentangle these two factors.

The rest of the paper is structured as follows: In Section 4.2 we give detailed description of our experimental design and procedure. In Section 4.3 we present a simple theoretical model and derive some hypotheses. The experimental results are reported and discussed in Section 4.4, Section 4.5 concludes.

## 4.2 Experimental Design and Procedure

### General Settings

We conducted a computerized trading experiment<sup>8</sup> which was designed to closely mirror the market settings of the EU ETS. Banking and quasi-borrowing were allowed (see below for details). For reasons of simplicity any interaction with a downstream market was disregarded.<sup>9</sup>

Similarly to the second phase of EU ETS, one emission permit market consisted of four compliance periods. Each compliance period had the following stages: allocation stage(s), in which permits were distributed among the market participants according to the treatment rule; four trade and production stages, in which participants could abate emissions and trade permits; a check of compliance, where compliance with the emission target for the preceding

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<sup>7</sup>See Stavins (1995) for a theoretical and Cason and Gangadharan (2003) for an empirical example of when the initial, pre-trade allocation affects post-trade allocation.

<sup>8</sup>The experiment was programmed and conducted in z-Tree (Fischbacher 2007).

<sup>9</sup>For studies of the effect of different allocation rules on the downstream market see Goeree et al. (2010) and Wråke, Myers, Mandell, Holt and Burtraw (2008).

trade and production stages was controlled and non-compliance was punished. The exact sequence of stages in the benchmark treatment is depicted in Figure 4.1.

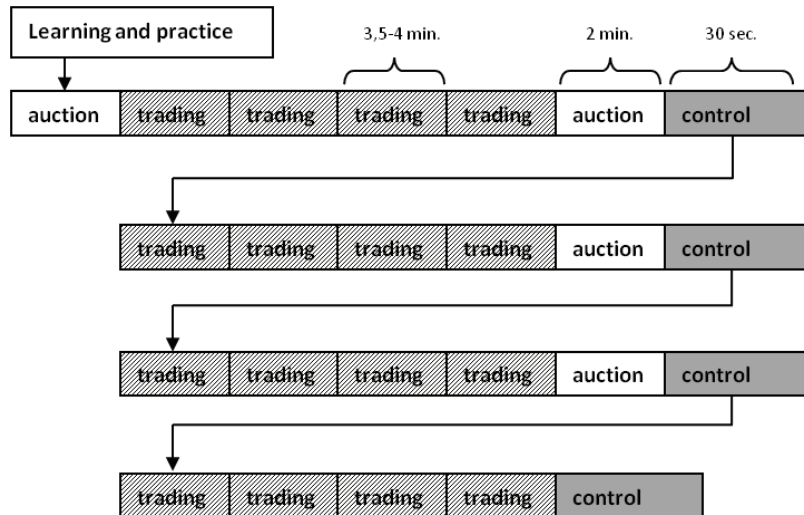


FIGURE 4.1: SEQUENCE OF STAGES IN THE BENCHMARK TREATMENT

A compliance period always started with an allocation stage. In the benchmark treatment with single auctioning, **A1**, and in the treatment with free allocation (or, grandfathering), **G**, the allocation stage was followed by a sequence of four consecutive trade and production stages. In the treatment with frequent auctioning, **A4**, an allocation stage preceded each of the four trade and production stages. For each trade and production stage participants received an emission target, which was privately disclosed at the beginning of a respective stage. Participants could meet these targets through trade of permits and through abatement. Individual abatement decisions remained private knowledge. Trading transactions were common knowledge (see below for details). Compliance for any of the stages was not required before the compliance check stage. Participants paid a fine only for missing permits at the time of the compliance check.<sup>10</sup> Banking within and across compliance periods was allowed. The amount of banked and missing permits remained private knowledge. Similarly to EU ETS, we simulated quasi-borrowing by allowing for an overlap between the compliance periods, such that (part of the) permits

<sup>10</sup>The height of the fine was equal to 2.6 times the the value of the ex ante efficient permit price. Similarly to EU ETS, this fine did not release noncompliant participants from the obligation to provide missing permits in the following compliance period.

from one compliance period could be used to meet the emission target in the preceding one.

A special characteristic of our experimental design is the information structure. The emission target of every participant was a random discrete number independently drawn from a commonly known uniform distribution. This number varied across participants and production stages. At the beginning of each trade and production stage participants were privately informed about their own respective emission target. One's future emission targets as well as the emission target's of one's opponents were only known in expectation.<sup>11</sup>

### **Allocation Stage**

In A1 the emission permits for one compliance period were sold in a single auction. In A4 emission permits were sold in four identical auctions. In G, similarly to the benchmark treatment, emission permits were given to participants in one single round of free allocation per compliance period. The overall amount of emission permits allocated for each compliance period remained constant throughout the experiment and was publicly known. The quasi-borrowing design of EU ETS was mimicked by setting the compliance check only after allocation of all permits (in A1 and G) or one quarter of the permits (in A4) for the subsequent compliance period had taken place.

In the auction treatments we used single-round, multi-unit, sealed-bid, uniform-price auctions.<sup>12</sup> Participants were required to submit demand schedules for a given set of prices. Individual demand schedules were aggregated to form a demand function, via which the auction clearing price was calculated. Bids were served according to the individual demand functions resulting from the demand schedules. After the auction all participants were informed about the auction clearing price and the number of permits they had won. One's bid schedule and wins remained private knowledge.

In the free allocation treatment permits were distributed among traders in a way to closely mirror the average pre-trade efficiency of allocation in A1 as

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<sup>11</sup>We introduced this additional complexity in order to capture the uncertainty most regulated firms face about their permit demand at the time of allocation. For instance, in the energy sector variations in the availability of renewable energy also leads to uncertain level of emissions at the time of permit allocation.

<sup>12</sup>The majority of auctions in the EU ETS and all of the RGGI auctions had this general format.



observed in the last compliance period.<sup>13</sup> Participants learned the number of permits, which they were going to receive for free each compliance period at the beginning of the experiment. This amount did not change throughout the experiment.

### Trade and Production Stage

After being privately informed about her emission target in the respective stage, each participant could meet this target by using permits received in the allocation stage, using permits bought in the secondary market, and conducting individual abatement. Individual abatement incurred costs, which were on the margin equal to the abated amount.<sup>14</sup> In addition to a printed table in the instructions with the marginal and total costs for any possible abatement level, participants were informed about the respective costs by an automatic calculator before they could commit to an abatement decision.

The trading environment in the trade and production stage was a computerized, continuous double auction without any bid-ask spread-reduction rules. Within a randomly drawn duration of a trading period<sup>15</sup>, participants could submit bids to buy and offers to sell or accept (part of) other traders' bids and offers. Bids and offers were price-quantity bundles on the demand and supply side of the market. The list of open bids and offers and the price of the most recent transaction were public information.

### Framing and Procedure

The emission trading market was framed as a market for a single input factor. In particular, participants were told that they are firms who have to serve an exogenously given amount of delivery commitment for a final product, which represented the emission target (in the following also called "*external demand*"). They could do so by using up already available units of the input

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<sup>13</sup>Note that in G permits are not grandfathered in the sense that allocation is based on past or expected emissions. However, this method of allocation allows us to disentangle the effects of the efficiency of pre-trade allocation and the allocation method. Additional details are available on request.

<sup>14</sup>For simplicity, it was possible to abate all emissions of one trade and production stage.

<sup>15</sup>The duration was randomly drawn from an interval of [3.5,4.0] minutes. Uncertainty about the exact duration of the trade and production stage creates incentives for early trading. The uniform distribution of the possible duration was common knowledge.

factor, e.g., emission permits, or by producing (part of) the necessary input factor, e.g., abatement of emissions.<sup>16</sup>

Participants were students of Business and Economics or another Economics related field at the University of Cologne. They were recruited via ORSEE (Greiner 2004). Upon entering the lab 32 participants were randomly assigned to one of two trading groups with 16 traders each.<sup>17</sup> The composition of the groups did not change throughout the experiment.

Before the beginning of the experiment participants received written instructions, which they read in private. Also in private, they answered questions testing their understanding of the instructions and had the possibility to try out a simulation of the trading environment with the computer as trading partner. The whole experiment took 2.5-3 hours to complete, from which 1-1.25 were reserved for the learning stage. We gathered 5 independent observations per treatment, with a total of 240 participants – 80 per treatment. Participants earned on average 22.86 € (SD=7.13) with substantial differences across the treatments.

### 4.3 Theoretical Predictions

In the appendix we develop a theoretical model that motivates the following hypotheses. For simplicity (and partly diverging from our experimental setting) we assume a competitive permit market and no transaction costs for participating in the auction or the secondary market.

**Hypothesis 4.1 (Compliance)** *All firms are compliant.*

**Hypothesis 4.2 (Abatement)** *In each trading and production stage all participants choose the same abatement level. The abatement level depends positively on the level of aggregate realized demand, which is reflected in the permit price. Abatement in earlier periods equals expected abatement in later*

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<sup>16</sup>It has been argued that a non-neutral framing would promote higher abatement levels due to potential environmental concerns of the participants. However, lab and field experiments by Ostertag, Schleich, Ehrhart, Goebes, Müller, Seifert and Küpfer (2010) for the trade of land urbanization rights indicate that if there are any differences between practitioners and students and neutral and non-neutral framing, then they are only quantitative and insignificant. Besides, as reported below, we observe too high abatement levels despite the neutral framing.

<sup>17</sup>One out of three sessions per treatment was conducted with one trading group, thus making random assignment to one of two groups impossible.

periods, given the information all participants already have on the level of external demand (via the permit price).

**Hypothesis 4.3 (Permit Price)** *The permit price does not depend on the allocation procedure. The permit price depends positively on the level of realized aggregate demand. The permit price in earlier periods equals the expected permit price in later periods.*

**Hypothesis 4.4 (Permit Demand)** *Individual total permit demand (over all periods) is proportional to total individual realized external demand (over all periods).*

**Hypothesis 4.5 (Auction)** *The willingness-to-pay in the auction equals the ex ante expected permit price, the auction price is weakly lower than this price. The auction frequency does not affect the pre-trade allocation of permits.*

**Hypothesis 4.6 (Banking and Borrowing)** *In the case of two successive compliance periods banking (borrowing) occurs if observed external demand was lower (higher) than expected in the first compliance period.*

**Hypothesis 4.7 (Allocation)** *The allocation procedure does not affect the final efficiency of allocation.*

## 4.4 Experimental Results

In this section we first analyze the static efficiency with respect to the total compliance costs caused by individual abatement and purchase of permits at the observed prices. Then, we take a closer look at the efficiency of allocation of permits and efficiency gains induced by the secondary market. Finally, we discuss dynamic inefficiencies induced by positive levels of banking, borrowing, and non-compliance. We compare our benchmark treatment with any of the other two treatments using aggregate data on the group-level as one observation, unless otherwise stated. Table 4.1 presents an overview of the observed average values for each treatment and the way they relate to the theoretically efficient values and the benchmark treatment.<sup>18</sup> The theoretically efficient values were calculated according to our model presented in the appendix.

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<sup>18</sup>The reported prices are a weighted average of the transaction prices over all trading and production stages. The reported average standard deviation is over all transactions in a

TABLE 4.1: OVERVIEW OF THE MAIN VARIABLES

Treatment	A4		A1		G	Th. Prediction
Payoffs (in EUR)	19.90 (6.21)	<**	23.54 (6.25)	<*	25.15 (7.84)	-
Abatement level	17.48 <sup>†</sup> (13.41)	≈	17.95 <sup>†</sup> (12.10)	>*	16.40 (12.23)	16.69
Price level	21.98 <sup>†</sup> (8.07)	>**	17.61 (5.03)	<*	18.36 (2.92)	16.69
Pre-trade all. efficiency	0.42 <sup>†</sup> (0.06)	≈	0.44 (0.08)	-	0.43 (0.00)	- <sup>a</sup>
Post-trade all. efficiency	0.64 <sup>†</sup> (0.09)	<*	0.73 <sup>†</sup> (0.03)	≈	0.71 <sup>†</sup> (0.05)	100

*Legend:* <sup>†</sup>  $p < 0.1$  for two-sided WSR-test for  $\neq$  efficient level; \* $p < 0.05$ , \*\* $p < 0.01$ , and \*\*\* $p < 0.001$  for one-sided MWU-test for between-treatment differences. <sup>a</sup> no theoretical prediction due to the existence of multiple equilibria. *Note:* Standard deviation across independent groups in parenthesis.

Due to the low number of independent observations, we use the following exact non-parametric tests: One-Sample Wilcoxon Signed-Rank test (WSR) for comparison between predicted and observed values and for paired data within one treatment; and Mann-Whitney U-test (MWU) for between-treatment comparisons.

### Abatement Levels

In the auction treatments the observed average abatement level is significantly different from the cost-efficient level. A cross-treatment comparison shows that the average abatement level tends to differ between A1 and G, but not between the auction treatments. In the following we investigate the determinants of the individual abatement level as predicted by our theoretical model (Hypothesis 4.2). In particular, no effect of the individually observed external demand and a positive effect of the observed permit price are expected.

Figure 4.2 depicts the average and individual abatement levels in selected representative trading groups from all treatments. At first glance average abatement correlates strongly with average price.<sup>19</sup> However, the average lev-

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treatment without accounting for any weighting. Multivariate Hadi's outliers were excluded from the calculation.

<sup>19</sup>Correlation coefficients per session vary between 0.61 and 0.89 and they are significant at  $p < 0.01$  and lower. Average abatement and price levels per compliance period are significantly different only in the first two periods in A4 and in the last two in A1 ( $p_{WSR} = 0.063$ ), when permit prices are inefficiently high or low, respectively.

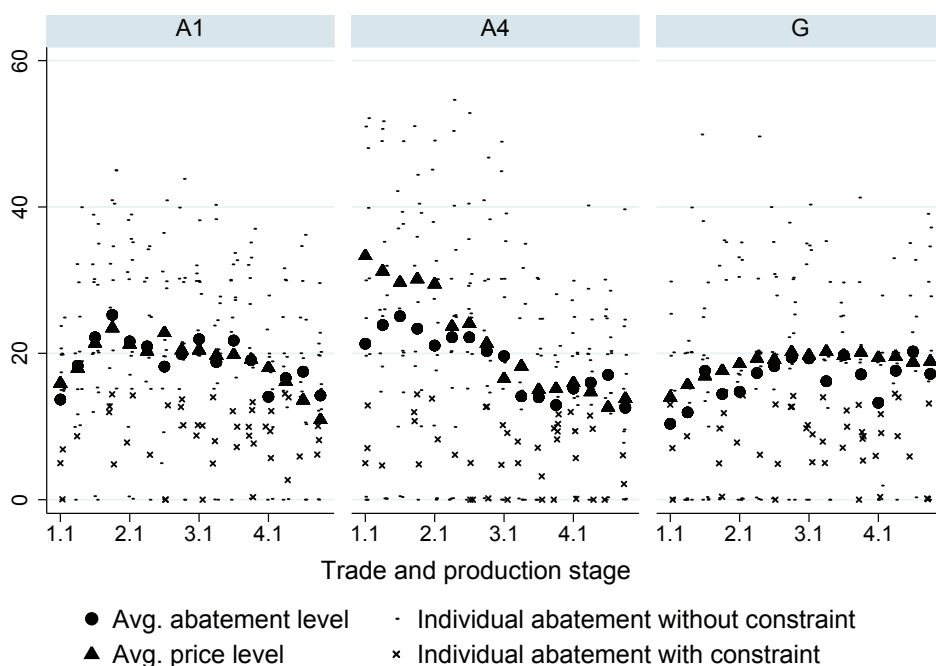


FIGURE 4.2: AVERAGE AND INDIVIDUAL ABATEMENT LEVELS AND PERMIT PRICES IN SELECTED TRADING GROUPS

els result from a combination of irrationally low and irrationally high individual levels, which differ greatly from the average price.<sup>20</sup> Table 4.2 presents the results of a clustered Ordinary Least Squares Model (OLS), which was estimated for each treatment separately and shows that individual decisions do not depend on the average observed market price. The regression equation is as follows:

$$\begin{aligned}
 abatement_{git} = & b_0 + b_1 avgprice_{gt} + b_2 demand_{git} + b_3 suffpermits_{git} \\
 & + b_4 W_{gi} + b_5 Z_g + b_6 T_{gt} + \varepsilon_{git}
 \end{aligned}$$

The indices  $g$ ,  $i$ , and  $t$  denote the respective trading group, participant, and trade and production stage.  $abatement_{git}$  is the observed abatement level,  $avgprice_{gt}$  is the observed average price at the secondary market,  $demand_{git}$  is the external demand participant  $i$  observed in trade and production stage  $t$  (linear and squared),  $suffpermits_{git}$  is a dummy equal to 1 if in  $t - 1$  the

<sup>20</sup>Exogenous restriction on abatement could occur if the realized external demand, which was uniformly distributed on  $[5, 55]$  and serves as an upper constraint for the periodical abatement level, was lower than the observed permit price.

participant possessed enough permits to cover her total unabated emissions until  $t$ ,  $W_{gi}$  and  $Z_g$  are vectors of participant and group specific characteristics respectively,  $T_{it}$  is a vector with dummy variables for the different trading stages, and  $\varepsilon_{git}$  is the error component.<sup>21</sup> Through robust regression coefficients we mitigate any effects of heteroskedasticity.<sup>22</sup> The estimates are clustered on the participant level.<sup>23</sup>

Column (OLS) shows the estimates for the complete regression model specified above with data from the whole experiment. The columns (OLS (1)) and (OLS (4)) display the results of the regression model with data from the first and the last compliance period, respectively. Column (IV) shows the estimates for an instrumental variable model which corrects for the exogeneity assumption about the average permit price.<sup>24</sup> The last two columns present the results of the separate IV-regression for the first and the last compliance period, respectively. As instrument for the price we use the auction clearing price in the preceding auction which would have occurred had participant  $i$  not taken part in it. First-stage F-values suggest that, except for the last compliance period in A1, the instrument used has good predictive power. To compare the coefficients between the models within and across treatments, we run OLS models with pooled data and complete interaction effects for the comparison treatment.

Contrary to Hypothesis 4.2, the level of individual external demand has a positive and highly significant effect on the level of abatement in all treatments, whereas the effect of the average observed permit price is present only in A1, where it decreases with learning. Also with learning the effect of individual external demand decreases significantly in A1 but increases in A4 and G. As a result, cross-treatment differences remain significant but change their direction over time. These estimates contradict the observations in Figure 4.2 but support the idea that although on average abatement and price levels are

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<sup>21</sup>We control whether the session of the trading group was conducted before or after noon and for the level of self-reported experience in laboratory experiments, trading markets, and auctions.

<sup>22</sup>White-test for heteroskedasticity is rejected for all treatments at  $p = 0.001$ .

<sup>23</sup>It has been suggested to cluster the estimates on the trading group level. However, due to the low number of independent observations, the estimates then become unreliable. The group dummies above should control for some of the within-group interdependencies.

<sup>24</sup>The estimates correct for the assumption that 16 participants per market are sufficient to ensure that individual abatement does not influence total permit demand.

TABLE 4.2: DETERMINANTS OF THE ABATEMENT LEVEL

a. Treatment A1<sup>a</sup>

Variable	OLS	OLS (1)	OLS (4)	(IV)	IV (1)	IV (4)
Avg. price	0.530***	0.285	0.768*	0.551*	0.401	14.746
Ext. dem.	0.876***	1.078***	0.359*	0.876***	1.077***	0.298
Ext. dem. sq.	-0.008***	-0.011***	-0.002	-0.008***	-0.011***	-0.001
Suff. permits	-2.427**	-4.464**	-2.298	-2.424**	-4.435**	-3.437
Constant	-4.952	1.024	-0.369	-8.977**	-1.531	-161.351
N	1280	320	320	1280	320	320
First stage F <sub>(1,79)</sub>				392.68	770.91	0.04
R <sup>2</sup>	0.377	0.363	0.27	0.377	0.363	-5.69

b. Treatment A4<sup>a</sup>

Variable	OLS	OLS (1)	OLS (4)	(IV)	IV (1)	IV (4)
Avg. price	0.188	0.313	0.220	0.290	0.332	-0.066
Ext. dem.	0.689***	0.558*	0.901***	0.691***	0.558**	0.898***
Ext. dem. sq.	-0.005**	-0.001	-0.012***	-0.005**	-0.001	-0.012***
Suff. permits	-3.660***	-5.428**	-2.815	-3.584***	-5.411**	-2.930*
Constant	3.328	-0.899	3.174	1.031	3.273	4.976
N	1280	320	320	1280	320	320
First stage F <sub>(1,79)</sub>				1801.39	1116.67	425.32
R <sup>2</sup>	0.369	0.385	0.223	0.368	0.385	0.218

b. Treatment G<sup>a</sup>

Variable	OLS	OLS (1)	OLS (4)
Avg. price	0.352	0.807	0.454
Ext. dem.	0.811***	0.276	1.006***
Ext. dem. sq.	-0.008***	0.001	-0.012***
Suff. permits	-1.156	-4.952**	-0.325
Constant	-10.365	-3.389	-7.337
N	1280	320	320
R <sup>2</sup>	0.271	0.324	0.234

Legend: \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ ; <sup>a</sup> Nr. of clusters = 80. Note: Estimates clustered on the subject level.

correlated, most participants fail to perceive the role of the market price as signal for the scarcity of permits in the market. Hence, participants tend to wrongly base their abatement decision on the individual demand observation instead of on the observed market price.

Notably, a strong and unpredicted effect is observed for the possession of sufficient permits to cover total unabated emissions. This effect is, however, in line with a suggestion in Gagelmann (2008) that in order to be less dependent on the market risk-averse buyers (sellers) of permits are expected to have higher (lower) levels of abatement than the efficient one.

**Result 4.1 (Abatement)** *Firms choose different abatement levels. Generally abatement in earlier periods does not equal expected abatement in later periods, given the information all firms already have on the level of external demand. Abatement levels depend significantly on the level of individual external demand and on the amount of permits at hand after the allocation stage. If permits are auctioned only once at the beginning of a compliance period (treatment A1), abatement also depends on the current permit price level.*

## Permit Prices

Table 4.3 present the average permit prices per compliance period and Figure 4.3 displays the average permit prices per trade and production stage. In the figure the beginning of a new compliance period is indicated by its respective number and the number of its first trade and production stage. The figure contains auction clearing prices (in the auction treatments), as well as permit prices in the secondary market.<sup>25</sup>

The observed average prices over all compliance periods are not significantly different from the efficient one with the exception of A4. Hence, for A1 and G we cannot reject the relationship between observed prices and realized aggregate external demand proposed in Hypothesis 4.3. Contrary to Hypothesis 4.3 between-treatment comparison shows that the average price levels differ significantly between the treatments and seem to follow different patterns depending

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<sup>25</sup>The lack of any extreme price tendencies towards the end of the experiment in all treatments suggests that the clearing rule with a virtual aftermarket deals effectively with end-game effects.



TABLE 4.3: AVERAGE PERMIT PRICES ON THE SECONDARY MARKET

Compl. period	A4		A1		G	Efficient price
1	29.953 <sup>†</sup> (5.429)	>**	20.001 (2.857)	≈	17.043 (1.005)	15.80
2	25.349 <sup>†</sup> (2.117)	>**	20.033 <sup>†</sup> (0.872)	≈	19.643 <sup>†</sup> (1.638)	16.38
3	18.986 (4.952)	≈	17.948 (3.257)	≈	19.062 (2.324)	16.57
4	12.947 <sup>†</sup> (2.143)	≈	12.650 <sup>†</sup> (3.921)	<*	17.776 (3.208)	18.01
Total	21.947 <sup>†</sup> (2.596)	>**	17.605 (1.812)	<*	18.361 (1.828)	16.69

*Legend:* <sup>†</sup>  $p < 0.1$  for two-sided WSR-test for  $\neq$  efficient level;  $*p < 0.05$ ,  $**p < 0.01$ , and  $***p < 0.001$  for one-sided MWU-test for between-treatment differences. *Note:* Values per compliance period, standard deviation across independent trading groups in parenthesis.

on the allocation procedure.

In the auction treatments prices in the first half of the experiment are inefficiently high and decrease to an inefficiently low level in the last compliance period. In the grandfathering treatment no such trend can be recognized. In total, when stability of prices is measured in the standard deviation of the permit price within each trading group, prices tend to be most stable in G, followed by A1 and then by A4. Hence, the allocation method obviously affects price development even in treatments with similar allocation frequency but different allocation rules.

The relationship between auction clearing prices and subsequent permit prices in the secondary market, on the one hand, and between auction clearing prices and expected permit prices, on the other, is a potential explanation for the observed price development. Due to arbitrage opportunities we would not expect significant differences between prices in one auction and the trade and production stage which follows it. This conjecture is confirmed for A1 and for most of the stages in A4. However, higher than efficient auction prices affect the initial prices in the secondary market.<sup>26</sup> As a result in the last compliance

<sup>26</sup>The willingness-to-pay expressed by the auction clearing price is compared to the expected (efficient) price, where the reference in A1 is the average efficient price in the respective compliance period and in A4 the average price in the subsequent trade and production stage. Contrary to Hypothesis 4.5 the willingness-to-pay in the middle two auctions in A1

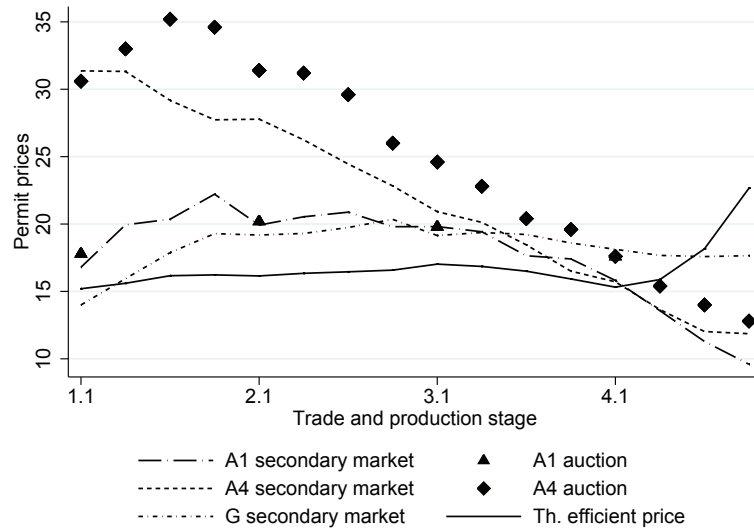


FIGURE 4.3: AVERAGE PRICES PER TRADING PERIOD

period permit prices fall below the expected permit price.

Let us briefly comment on the effects the observed price dynamics have on abatement, banking, and borrowing decisions. We observe that high initial auction clearing prices in A1 and (even more so) in A4 lead to high prices in the secondary market. These in turn lead to over-abatement and inefficient banking, which increase permit supply relative to demand in later periods. In treatment G, on the contrary, initial prices and abatement levels are inefficiently low and participants tend to borrow permits from the subsequent periods. This leads to a relative shortage of permits in later trading periods and to increased market clearing prices.

**Result 4.2 (Permit Price)** *The average permit price and, in particular, its temporal development depend on the allocation procedure. In the auction treatments prices start off inefficiently high and decrease over time. In the grandfathering treatment the opposite relationship is observed. It cannot be rejected that the permit price depends positively on the level of realized ag-*

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and in the majority of auctions in A4 is significantly higher than the ex ante efficient and expected permit price. It is beyond the scope of this paper to analyze the reasons behind the high auction prices. However, these results may be related to two common observations in laboratory auctions. Firstly, overbidding in sealed bid auctions is one of the most robust experimental results. Secondly, while it is frequently reported in sealed bid second price auctions, it disappears in the strategically equivalent English auction, where the price paid is more salient (see Kagel 1995, for a review of related literature). This may explain part of the difference between the auction prices and those in the secondary market.

gregate demand. Only with frequent auctioning it is often different from the expected permit price in later periods.

**Result 4.3 (Auction)** *In the initial trading periods (in A4 in particular), the willingness-to-pay in the auction does not equal the ex ante expected permit price. The auction clearing price is generally higher than the expected permit price.*

### Efficiency of Permit Allocation

We analyze two types of allocation efficiency after the fourth compliance stage.<sup>27</sup> The *pre-trade allocation efficiency* is the efficiency of allocation as a result of the allocation method alone. It assumes that no secondary market exists to amend for inefficiencies of the allocation rule and disregards any changes in the efficiency brought about by the secondary market. The *post-trade allocation efficiency* corrects for such assumptions.

The reference efficient level of emission permits in the fourth compliance period is calculated for each participant separately. It takes into account the value of one's external demand and assumes that through a Walrasian process the market price reflects the total realization of individual external demands. Participants are considered to be a post-trade (pre-trade) buyer if they hold fewer than the efficient level of permits at the time of the fourth compliance check, where changes through trade are (not) considered. Similarly to Cason and Gangadharan (2003), the efficiency of allocation at any time point is defined as the overall amount of permits held by all current buyers in a market as a share of the efficient amount of permits these same buyers should possess.

The pre-trade allocation efficiency in G was exogenously set to mimic the pre-trade allocation efficiency in the benchmark treatment.<sup>28</sup> Hence, we analyze it only for the auction treatments. Figure 4.4 depicts the overall pre-trade and post-trade efficiency of permit allocation across treatments, Table 4.1 gives the descriptive statistics. Although the secondary market leads to a significant increase of efficiency, it remains significantly lower than 100% in all treatments.

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<sup>27</sup>Due to trade opportunities in the last compliance stage, no unique banking equilibrium exists, which prescribes the number of permits each participant should hold at the end of the previous compliance stages.

<sup>28</sup>The insignificant difference between A1 and G was unavoidable due to design constraints.

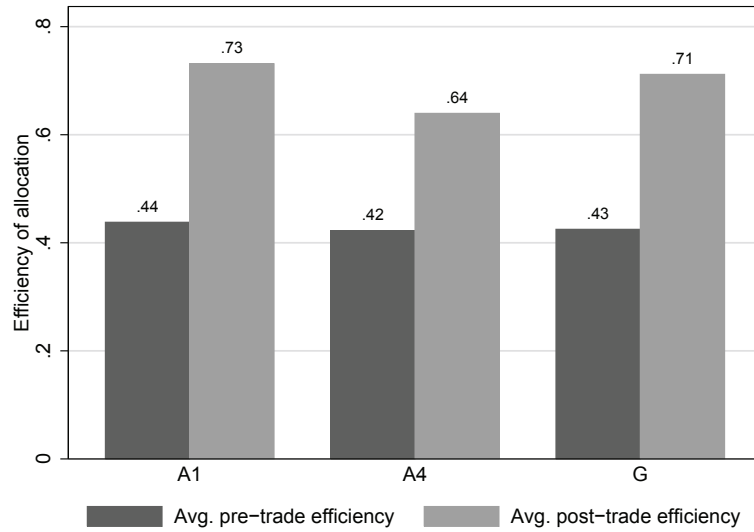


FIGURE 4.4: OVERALL PRE-TRADE AND POST-TRADE ALLOCATION EFFICIENCY

A between-treatment comparison cannot reject the null Hypothesis of equal pre-trade efficiency of allocation in the auction treatments. The post-trade efficiency in the last compliance period in A1 tends to be higher than in A4 but it is not significantly different from the one in G.

Efficient post-trade allocation requires that individual total permit demand is proportionate to total individual external demand (see Hypothesis 4.4). Figure 4.5 shows the relationship between the total number of permits a participant has purchased on any of the available markets and the total external demand she faced over the whole experiment. The relationship is significantly positive in A1 and G with robust coefficients of the estimated regression models for the last trading period, which were clustered on the trading group level, of 0.951 and 1.090 respectively. The slope of the regression in A4 is 0.345 and not significantly different from zero. Given the abatement cost structure, participants should have abated about half the expected emissions. The coefficients in the auction treatments are not significantly different from this benchmark. The coefficient in G is significantly different ( $p = 0.012$ ). No significant between-treatment differences could be detected.

**Result 4.4 (Auction)** *The frequency of auctioning does not affect the pre-trade allocation of permits. Neither infrequent, nor frequent auctioning delivers efficient permit allocation with regard to expected emission goals.*

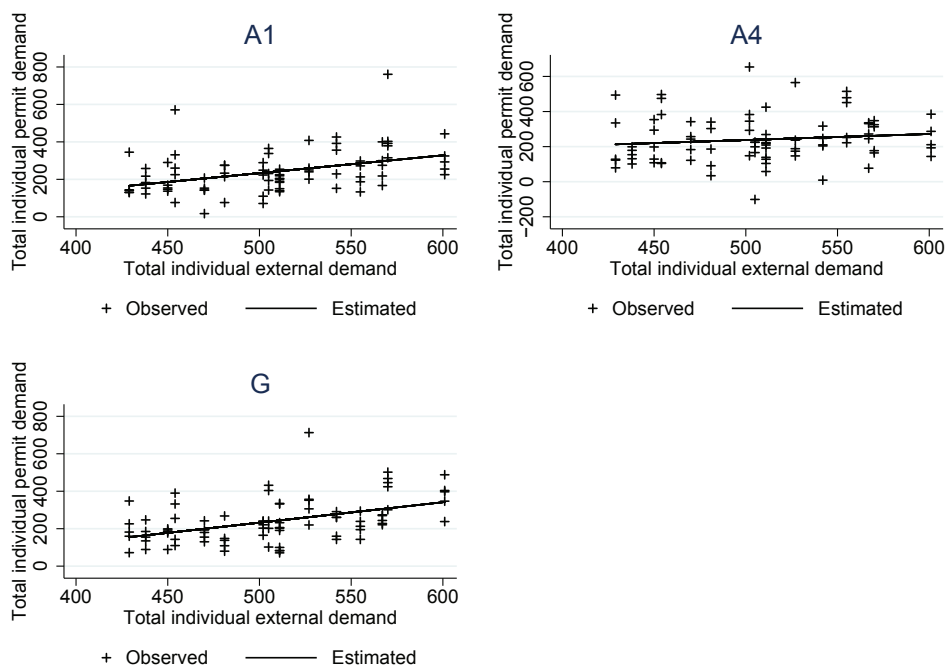


FIGURE 4.5: RELATIONSHIP BETWEEN TOTAL PERMIT DEMAND AND TOTAL EXTERNAL DEMAND

**Result 4.5 (Permit Demand)** *In the treatments with a single allocation stage per compliance period individual permit demand (over all periods) is proportional to total realized external demand (over all periods).*

**Result 4.6 (Allocation)** *The frequency of allocation affects the final allocative efficiency, whereas the allocation method – for free or against a payment – does not have a significant impact as long as the initial distribution of permits does not have a significant impact as long as the initial distribution of permits is similar. In all treatments, the secondary market fails to deliver near-perfect efficiency.*

### Inefficient Trading Patterns at the Secondary Market

The average number of transactions per participant (selling and purchasing together) is 42.81 (SD=32.88) in A1, 47.09 (SD=32.62) in A4, and 68.94 (SD=45.18) in G, respectively. For the auction treatments we do not find a significant difference, which is surprising since in A4 the secondary market should be needed less for correcting inefficiencies resulting from lack of information. We do find significant differences between the benchmark and

the grandfathering treatment, suggesting that participants are more willing to correct an initial allocation that they did not determine themselves.

TABLE 4.4: TRADE INEFFICIENCIES BY PLAYER TYPE

	Pre-trade role	% Post-trade buyers	Nett sold amount	Efficient sold amount	N
<b>A1</b>	Seller	0.41	195.45 ( 212.28 )	238.61 ( 185.28 )	29
	Buyer	0.69	-111.14 ( 81.48 )	-135.68 ( 73.01 )	51
<b>A4</b>	Seller	0.47	174.80 ( 230.31 )	233.26 ( 213.48 )	30
	Buyer	0.68	-104.88 ( 117.15 )	-139.96 ( 82.68 )	50
<b>G</b>	Seller	0.49	149.69 ( 164.66 )	176.21 ( 116.35 )	35
	Buyer	0.64	-116.42 ( 115.49 )	-137.05 ( 57.22 )	45

*Note:* Standard deviation within one treatment in parenthesis.

To investigate the reasons for inefficient post-trade allocations despite high trading activity, we analyze the direction of permit exchange presented in Table 4.4. There are two sources of inefficiency: First, when traders transact although their trade is inefficient. Second, when profitable trades are not executed. The second row gives an overview of the share of pre-trade sellers (buyers) who sold too many (bought too few) permits given their external demand and as a consequence became (remained) a post-trade buyer.<sup>29</sup> Such role switching is the result of the first inefficiency type. The third and the fourth row list the average number of permits which were traded in the right direction and the respective amount which should have been traded in order to achieve perfect efficiency. The observed differences are the result of the second inefficiency type. Hence, markets in all treatments fail to reach full efficiency due to both – too much and too little trade.

## Banking and Borrowing

As already discussed in the previous section, we observe a tendency to over-bank permits. This is also in line with previous experiments on emissions

<sup>29</sup>The share of pre-trade buyers who became post-trade sellers is one minus the reported share.

trading (Gangadharan, Farrell and Croson 2005). Table 4.5 gives an overview of the average number of banked permits per participant for each compliance period.

TABLE 4.5: AVERAGE BANKED PERMITS

Compliance period	A4 mean		A1 mean		G mean	Efficient level
1	6.563 <sup>†</sup> (40.651)	≈	4.850 <sup>†</sup> (91.214)	> <sup>*</sup>	-16.025 (62.442)	-14.741
2	29.588 <sup>†</sup> (42.663)	≈	24.588 <sup>†</sup> (102.665)	> <sup>*</sup>	-2.813 (63.512)	-12.648
3	41.963 <sup>†</sup> (73.353)	≈	48.938 <sup>†</sup> (98.426)	> <sup>**</sup>	12.250 <sup>†</sup> (67.109)	-3.710
4	30.013 <sup>†</sup> (102.343)	≈	38.050 <sup>†</sup> (92.522)	> <sup>*</sup>	11.738 (53.069)	0

*Legend:* <sup>†</sup>  $p < 0.1$  for two-sided WSR-test for  $\neq$  efficient level; <sup>\*</sup>  $p < 0.05$ , <sup>\*\*</sup>  $p < 0.01$ , and <sup>\*\*\*</sup>  $p < 0.001$  for one-sided MWU-test for between-treatment differences. *Note:* Values per compliance period, within-treatment standard deviation in parenthesis.

Given the realized external demand, on average participants should have been borrowing permits in all compliance periods. However, we observe banking in the auction treatments, which is significantly different from the efficient borrowing level in all compliance periods. Banking in G is significantly different from the efficient level only in the third compliance period. As noted above, deviations of the market price from the efficient one reflect inefficiencies in banking and borrowing behavior. Similar price dynamics in the auction treatments are the result of similar banking and borrowing behavior. For details on the between-treatment comparison see Table 4.5.<sup>30</sup>

**Result 4.7 (Banking and Borrowing)** *When permits are auctioned, a significant amount of permits is banked, although the observed external demand should induce borrowing. When permits are grandfathered, initial borrowing is followed by later banking.*

<sup>30</sup>One reason for inefficiently high banking in the auction treatments may be a sunk-cost fallacy following the development of prices in these treatments. Having paid an (inefficiently) high price for a permit, participants may fail to recognize that if prices are expected to fall in the future, it is better to sell permits at a small loss in the current trade and production stage, rather than at a big loss in later stages. The sunk-cost fallacy may also explain the second type of trade inefficiencies discussed above.

## Non-Compliance

Contrary to Hypothesis 4.1, we observe frequent cases of non-compliance. The descriptive statistics for the level and share of non-compliance in each period is presented in Table 4.6. The first row in the table shows the average number of missing permits, given that a participant was non-compliant. The middle row shows the share of these missing permits from the total emissions in a BAU-case. The last row contains the average share of non-compliant participants from the total of 16 in one trading group.

TABLE 4.6: NON-COMPLIANT PERMITS AND PARTICIPANTS

	Compliance period				Total
	1	2	3	4	
<b>A1</b>					
Avg. amount of NC permits	-60.125 <sup>†</sup> (41.684)	-45.800 (53.844)	-62.500 (27.577)	-50.625 (52.679)	-53.913 <sup>†</sup> (45.135)
Avg. share of NC permits	0.044 <sup>†</sup>	0.011	0.004	0.010	0.017 <sup>†</sup>
Avg. Share of NC participants	0.100 <sup>†</sup>	0.063	0.025	0.100	0.072 <sup>†</sup>
<b>A4</b>					
Avg. amount of NC permits	-39.222 <sup>†</sup> (35.031)	-	-50.333 (40.857)	-72.750 <sup>†</sup> (71.749)	-54.300 <sup>†</sup> (53.315)
Avg. share of NC permits	0.032 <sup>†</sup>	-	0.005	0.014 <sup>†</sup>	0.017 <sup>†</sup>
Avg. Share of NC participants	0.113 <sup>†</sup>	-	0.038	0.100	0.084 <sup>†</sup>
<b>G</b>					
Avg. amount of NC permits	-48.000 <sup>†</sup> (31.102)	-56.500 (40.510)	-43.250 (24.226)	-45.692 (41.728)	-48.056 <sup>†</sup> (35.076)
Avg. share of NC permits	0.057 <sup>†</sup>	0.016	0.006	0.015	0.024 <sup>†</sup>
Avg. Share of NC participants	0.016 <sup>†</sup>	0.075	0.050	0.163	0.076 <sup>†</sup>

*Legend:* <sup>†</sup> $p < 0.1$  for two-sided WSR-test for  $\neq$  efficient level. *Note:* Within-treatment standard deviation in parenthesis.

In all treatments significantly positive non-compliance is observed, with non-compliance rates being highest at the beginning and at the end of the experiment. Possible reasons are therefore lack of understanding of the compliance check mechanism (despite detailed instructions and a learning stage) and shortage of funds, respectively.<sup>31</sup>

**Result 4.8 (Compliance)** *On average noncompliance is significantly positive in all treatments.*

<sup>31</sup>It was hard but not impossible to become bankrupt despite the constant flow of funds from the fixed revenue income from the downstream product.



## 4.5 Conclusion

Based on the results of an experiment, designed to closely mirror the institutional settings of the EU ETS, we show that the method of allocation may affect the efficiency of the emissions trading system. We demonstrate that paying or not paying for emission permits does not affect the efficiency of permit allocation after trade as long as the initial allocation of permits before trade is fairly similar. It does affect, however, the average abatement level as well as the dynamic efficiency of the instrument by inducing different price variability and different distribution of abatement activities over time. Auctioning seems to promote early rather than late abatement in comparison to free allocation with the same distribution of permits among firms. However, auctioning also leads to higher total compliance costs of the system and lower price stability, which although not considered in our experiment, may have an effect on the long-term incentives to invest in carbon-low technology. Furthermore, due to overbidding in the auctions additional compliance costs arise, which can be avoided through free allocation.

When comparing less to more frequent auctioning, we demonstrate that there is a trade-off between the comfort of steady injection of liquidity, on the one hand, and paying different prices for identical goods, on the other. The first is usually an argument in favor of frequent auctioning brought forward by regulated firms (and some theorists, see Hepburn et al. (2006) for an example). The latter is typical in multiple auctions for identical goods (see Ashenfelter 1989, for an early report on this phenomenon) and is supported by our results.<sup>32</sup> Having different prices for the same object hinders regulated firms in making reliable investment plans. It also prevents the market from sending correct price signals about the rentability of short-term abatement, thus compromising the overall efficiency of the emissions trading system. In addition, frequent auctioning affects the ability of the system to minimize joint compliance costs by inducing lower efficiency of allocation after trade. Hence, if the regulator chooses to allocate permits via auctioning, the experimental results would recommend large and less frequent auctions.

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<sup>32</sup>Differences of about 0.8 EUR were also observed in the Dutch and the Lithuanian auctions of the EU ETS, which took place only a month apart (Co2-Handel.de 2012a; CO2-Handel.de 2012b).

In sum, we demonstrate that the method of permit allocation affects the efficiency of the environmental instrument on multiple levels. When choosing a method of allocation, the regulator should consider its impact on the secondary market, price development, distribution of abatement decisions in time, the ability of the market to provide efficient permit allocation, as well as the banking and borrowing behavior resulting from all these factors.

## 4.6 Appendix

### 4.6.1 Derivation of the Theoretical Predictions

#### One Compliance Period

Consider the following game, which reproduces the stages of one *compliance period*.

In the first stage (e.g. the *allocation stage*) a regulator introduces an emission cap  $X$  and distributes emission permits according to a certain allocation rule (either grandfathering, or auctions, or a mixture of both) among  $n$  ex ante identical firms. We denote by  $g_i$  the number of permits firm  $i$  obtains for free and by  $a_i$  the number of permits  $i$  acquires in an auction at auction price  $p^a$ .

The second stage consists of  $T$  substages (e.g. *trade and production stages*). In each of these substages the firms face exogenous demand  $Y_{it}$ ,  $i = 1, \dots, n$ ,  $t = 1, \dots, T$ , for a downstream product. The exogenous demand is ex ante uncertain, e.g.  $Y_{it}$  are independent random variables with distribution  $F(\cdot)$  and density  $f$ . Realized exogenous demand  $y_{it}$  is privately observed by each firm. We assume that the production of one unit of the downstream product implies emission of one unit of emissions if a standard generation technology is used. Production costs with the standard technology are normalized to 0. Emissions can be avoided by using "green" technology. Costs of abatement  $C(r_{it})$  are increasing and convex ( $C'(r_{it}) > 0$  and  $C''(r_{it}) \geq 0$ ), where  $r_{it}$  is the number of units that is produced emission-free by firm  $i = 1, \dots, n$  in substage  $t = 1, \dots, T$ . Any permits received in the first stage through grandfathering or auctioning can be traded with the other firms in a common market. We denote the price at the permit market in substage  $t$  by  $p_t^e$ . As "dirty" production is cost-free, firms are assumed to serve the entire external demand and receive an exogenously fixed price  $p^y$  per unit of the downstream product.

In the third stage (e.g. *compliance check stage*) the regulator penalizes any firm which in this stage does not possess enough permits to cover its emissions from the second stage. The height of the fine depends on the per-unit penalty  $f^e$  and the number of missing permits  $N_i$ .

We solve the game by backwards induction. Let  $d_{it}$  denote the permit demand of firm  $i$  in substage  $t$ . The profit function of each firm  $i = 1, \dots, n$  in

the last substage before compliance check is given by

$$\begin{aligned}\pi_{iT} &= p^y \sum_{t=1}^T y_{it} - \sum_{t=1}^T C(r_{it}) - \sum_{t=1}^{T-1} p_t^e d_{it} - p^a a_i \\ &- p_T^e \left( \sum_{t=1}^T y_{it} - \sum_{t=1}^T r_{it} - (a_i + g_i) - N_i \right) - N_i f^e.\end{aligned}$$

When the fine for noncompliance is greater than the permit price, the first derivative of the profit function with respect to  $N_i$  is always negative ( $\frac{\partial \pi_i}{\partial N_i}(r_i, N_i) = p_T^e - f^e < 0$  for  $f^e > p_T^e$ ). Hence, a profit maximizing firm is always compliant. In the following we assume that the fine is high enough (as it is the case in the experimental design), set  $N_i = 0$  and disregard  $N_i$  in our further analysis (see Hypothesis 4.1).

In the last substage firm  $i$  takes its demand for permits, abatement levels and permit prices from the previous substages as given and maximizes its profit by choosing the optimal  $r_{iT}$ . Differentiation with respect to  $r_{iT}$  yields

$$\frac{\partial \pi_{iT}}{\partial r_{iT}} = -C'(r_{iT}) + p_T^e = 0 \quad \Leftrightarrow \quad p_T^e = C'(r_{iT}).$$

Firms have identical cost structures and therefore choose the same abatement levels in the last trading and production stage (see Hypothesis 4.2). Since firms choose to be compliant, if the fine is high enough (see above), abatement must be chosen such that emissions meet the cap:

$$\sum_{i=1}^n \sum_{t=1}^T (y_{it} - r_{it}) = X \tag{4.1}$$

Since firms have identical costs structures, in the last substage they all choose the same abatement level, which is equal to the permit price  $p_T^e$  in the last substage. It therefore holds that

$$r_{iT}(p_T^e) = \frac{\sum_{i=1}^n \left( \sum_{t=1}^T y_{it} - \sum_{t=1}^{T-1} r_{it} \right) - X}{n} \tag{4.2}$$

and

$$p_T^e = C' \left( \frac{\sum_{i=1}^n \left( \sum_{t=1}^T y_{it} - \sum_{t=1}^{T-1} r_{it} \right) - X}{n} \right) \quad (4.3)$$

Thus, the permit price in the last substage depends positively on the level of total realized external demand over all substages regardless of the allocation mechanism (see Hypothesis 4.3).

Now let us consider substage  $T - 1$ . Since compliance is only controlled for in the third stage, permits in  $T - 1$  and  $T$  are perfect substitutes. Thus, in any equilibrium it must hold that  $p_{T-1}^e = E[p_T^e]$ . But then firms will choose to abate as much as they expect to abate in the last substage (see Hypothesis 4.2), i.e.

$$\begin{aligned} r_{i(T-1)} &= E[r_{iT}] = \frac{\sum_{i=1}^n \left( \sum_{t=1}^{T-1} y_{it} + E[Y_T] - \sum_{t=1}^{T-1} r_{it} \right) - X}{n} \\ &= \frac{\sum_{i=1}^n \sum_{t=1}^{T-1} y_{it} - \sum_{i=1}^n \sum_{t=1}^{T-2} r_{it}}{n} + E[Y_T] - r_{i(T-1)} - \frac{X}{n} \end{aligned}$$

The last equality holds because firms always choose equal abatement levels, independently of their privately observed exogenous demand. Thus, in equilibrium it holds that

$$r_{i(T-1)} = \frac{1}{2} \left( \frac{\sum_{i=1}^n \sum_{t=1}^{T-1} y_{it}}{n} + E[Y_T] - \frac{\sum_{i=1}^n \sum_{t=1}^{T-2} r_{it}}{n} - \frac{X}{n} \right)$$

and

$$p_{i(T-1)}^e = E[p_T^e] = C' \left( \frac{1}{2} \left( \frac{\sum_{i=1}^n \sum_{t=1}^{T-1} y_{it}}{n} + E[Y_T] - \frac{\sum_{i=1}^n \sum_{t=1}^{T-2} r_{it}}{n} - \frac{X}{n} \right) \right)$$

Since  $E[Y_t] = E[Y]$  for all  $t = 1, \dots, T$ , by way of induction it is easy to

show for any substage  $k$  the optimal abatement level is

$$r_{ik}^* = \frac{1}{T - k + 1} \left( \frac{\sum_{i=1}^n \sum_{t=1}^k y_{it}}{n} + (T - k)E[Y] - \frac{\sum_{i=1}^n \sum_{t=1}^{k-1} r_{it}^*}{n} - \frac{X}{n} \right) \quad (4.4)$$

where  $r_{it}^* = r_t^*$  for all  $i = 1, \dots, n$  denotes the *efficient* abatement level in all preceding stages. Note that in stage  $k$  the participants have already observed the first  $k$  realizations of their external demand. In a competitive and frictionless trading market aggregate external demand is "revealed" in the market clearing price at the spot market. The efficient abatement level implicitly determines the efficient price level:

$$p_k^{e*} = C'(r_{ik}^*)$$

The results so far are independent of the allocation mechanism. Now, let us finally look at the allocation stage prior to the  $T$  trade and production stages. Ex ante, i.e. before firms have privately observed any of their external demand, the expected abatement level (and implicitly the expected permit price) is the same for all substages  $t = 1, \dots, T$ , namely

$$E[r_t] = \frac{1}{T} \left( E \left[ \sum_{t=1}^T Y_t \right] - \frac{X}{n} \right).$$

Hence, each firm's ex ante expected profit is given by:

$$\begin{aligned} \pi_{i(1)} &= E \left[ \sum_{t=1}^T Y_t \right] p^y - TC \left( \frac{1}{T} \left( E \left[ \sum_{t=1}^T Y_t \right] - \frac{X}{n} \right) \right) \\ &\quad - C' \left( \frac{1}{T} \left( E \left[ \sum_{t=1}^T Y_t \right] - \frac{X}{n} \right) \right) \left( \frac{X}{n} - a_i - g_i \right) - p^a a_i \end{aligned}$$

Differentiation with respect to  $a_i$  yield that each firm bids at most

$$p^a = C' \left( \frac{1}{T} \left( E \left[ \sum_{t=1}^T Y_t \right] - \frac{X}{n} \right) \right)$$

on  $E \left[ \sum_{t=1}^T Y_t \right] - g_i$  units in the auction (see Hypothesis 4.5).

### Multiple Compliance Periods

One complete game from above represents one compliance period. Assume that the game is repeated  $m$  times and that borrowing and banking between the compliance periods is allowed. As long as no extreme shocks in the supply of permits or exogenous demand for the downstream product, efficient borrowing is possible. Hence, permits in the  $mT$  trading and production stages over all compliance periods become perfect substitutes. The efficient permit price in substage  $k = 1, \dots, mT$  is then given by  $p_k^{e*}(r_t^*) = C'(r_t^*)$ , where

$$r_t^* = \frac{1}{mT - k + 1} \left( \frac{\sum_{i=1}^n \sum_{t=1}^k y_{it}}{n} + (mT - k)E[Y] - \frac{\sum_{i=1}^n \sum_{t=1}^{k-1} r_{it}^*}{n} - \frac{mX}{n} \right)$$

The optimal abatement level  $r_{ik}^*$ , allows us to calculate the optimal individual permit demand of participant  $i$  in substage  $k$ :

$$d_{ik}^* = y_{ik} - r_{ik}^*,$$

and, of course, average permit demand over all stages. Note that due to the compliance check regulations and banking and borrowing freedom in the model,  $d_{ik}$  is not necessarily equal to the amount of permits a participant acquires in stage  $k$ . Thus, participant  $i$  must possess  $\sum_{k=1}^{mT} d_{ik}$  permits in the end, but it is not uniquely determined in which periods they should be acquired. Therefore, a unique individual banking or borrowing amount from one compliance period to the next does not exist. We call the total amount to be jointly banked or borrowed by all participants between two compliance periods  $BB_\tau$ , where  $\tau = 1, \dots, m$ <sup>33</sup>:

$$BB_\tau^* = \sum_{i=1}^n \sum_{k \in \tau} d_{ik} - X.$$

Note that if there is an auction before each trade and production stage, the game becomes equivalent to the basic game with one substage and  $mT$

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<sup>33</sup>Given the randomly drawn values for the external demand used in the experiment, we find that all values of the optimal banking and borrowing are feasible in any of the treatments.

repetitions. The banking and borrowing conditions remain unaffected. Hence, the results from above are independent of the number of auctions as long as the total number of permits is equal in both allocation mechanisms.

## 4.6.2 Instructions

*In the following the instructions for the experiment are provided. A [Treatment] indicates which sections were present only in the instructions of the respective treatment.* Thank you for taking part in our trading experiment. Please read

these instructions carefully. Before with proceed with the experiment, we will check your understanding by asking a few questions. Simultaneously, you will also have the opportunity to become acquainted with the experimental environment.

In this experiment you can earn money whereby the sum of your earnings depends on your decisions and the decisions of others. Your earnings will be calculated in virtual money-units “GE”. After the game is over, their Euro value will be calculated at an exchange rate of 450 virtual units for one Euro and you will receive this value together with a show-up-fee of 2.50 Euros.

### Your situation

You and 15 other participants in the experiment will be able to trade with each other in a market. At no time will you be told who the other 15 participants are. Throughout the whole experiment you will be trading with the same 15 participants. Thus, the composition of your group will not change. Every participant was given these instructions.

[A1, A4] You will receive an initial endowment of 5500 GE. You will then have the task of serving the demand for a good. The level of the demand will be a random number between 5 and 55. Every number has the same probability. The demand you have to serve is independent of the demand the other participants have to serve.

[G] You will receive an initial endowment in GE. You will be told the amount of GE when the experiment begins. You will then have the task of serving the demand for a good. The level of the demand will be a random number between



5 and 55. Every number has the same probability. The demand you have to serve is independent of the demand the other participants have to serve.

At the beginning you do not know the level of demand. It changes every trading-period and will be disclosed to you in the information-box in the top left hand corner of your screen (see figure at the end of these instructions). For each unit of demand you are required to serve, you will be paid a fixed price of 25 GE. This means that as soon as you are informed about the level of the demand, you will receive - as an advance payment - 25 GE for every unit of the good demanded in this trading period. You receive this advance payment regardless of whether you have enough units of the good to serve the demand or not.

There are altogether 16 trading periods. After every four trading-periods, we will check if you have provided a sufficient amount of the good to serve the demand in these four trading-periods. Should you not be able to serve the demand, you will have to pay a fee for every missing unit of the good and you will also have to retroactively provide any missing units. Details will be explained later in the text.

[A1, A4] You have the following options to fulfill your demand-serving obligations:

1. You can produce units of the good by yourself;
2. You can trade units of the good in a market;
3. You can purchase units of the good in auctions organized by the experimenter.

[G] You have the following options to fulfill your demand-serving obligations:

1. You can produce units of the good by yourself;
2. You can trade units of the good in a market;
3. You will be allocated free units of the good by the experimenter.

These options can be combined to satisfy the given demand. Details will be explained later in the text.

## Self-Production

When a trading period is running, you can see the production-box below the information-box (see screenshot at the end of the instructions). In this box, you can determine the number of units you want to provide through self-production. You can produce only once per trading-period. The amount of your self-production cannot exceed the demand level in the respective trading-period.

Self-production induces costs. These costs amount to 1 GE for the first produced unit, 2 GE for the second produced unit, 3 GE for the third produced unit, and increase by 1 GE for every additional unit you produce by yourself. The sum of the production costs over all units gives the total production costs. The following table illustrates how the production costs are calculated.

Amount of self-production	Additional costs caused by last produced unit	Total production costs for this amount
1	1	1
2	2	3
3	3	6
4	4	10
...	...	...
n	n	$\sum_{i=1}^n i$

If you click on the button “Calculate”, we will calculate the total costs of production for you. In addition to that, at the end of these instructions we have provided you with a table which gives an overview of the costs per additional unit and the total costs.

Please keep in mind that you should make your production decision before the respective trading-period is over.

## Trading rules

Together with self-production you can serve some of the demand through trading in a market. In the market you can:

- make bids for buying an amount of the good (“bids”),
- make offers for selling an amount of the good (“asks”),
- or delete an already stated bid/ask.

[A1, A4] Every trading period takes at least 3 minutes and 30 seconds and at most 4 minutes. The exact time will be determined by chance. Every time spread between 3 minutes 30 seconds and 4 minutes can occur with the same probability. When a trading period is over, any open bid/ask will expire.

[G] Both free allocated and self-produced units can be traded. Every trading period takes at least 3 minutes and 30 seconds and at most 4 minutes. The exact time will be determined by chance. Every time spread between 3 minutes 30 seconds and 4 minutes can occur with the same probability. When a trading period is over, any open bid/ask will expire.

By posting bids and asks you can trade with the other participants in your market. Bids can only be posted if the bid's value doesn't exceed your disposable capital. Your disposable capital is calculated by subtracting the value of your open bids in the market from your total capital. Asks can only be posted, if you possess at least as many units as you want to sell and these units are not reserved for other currently open asks of yours.

You can see your "disposable capital" (German: "verfügbares Vermögen") and your "disposable goods" (German: "verfügbare Güter") at the top left corner of the trading period's screen. At the screen's bottom you see your currently open bids/asks. (see screenshot at the end of the instructions)

#### *Posting an ask*

An example for an ask is "I offer 4 units for 48 GE per unit". The ask's price and amount must be in whole numbers and higher than or equal to 1.

Your ask will be traded instantly, only if it is the ask with the lowest price and if this price is equal to or lower than the price of the highest bid in the bids' queue. Otherwise it will be listed in the asks' queue. Open asks are listed on the right, upper half of the screen. (see screenshot at the end of the instructions)

#### *Posting a bid*

An example for a bid is "I would like to buy 8 units and I offer 12 GE per unit". The bid's price and amount must be in whole numbers and greater than or equal to 1.

Your bid will be traded instantly, only if it is the bid with the highest price and if this price is equal to or higher than the lowest ask in the asks' queue. Otherwise it will be listed in the bids' queue.

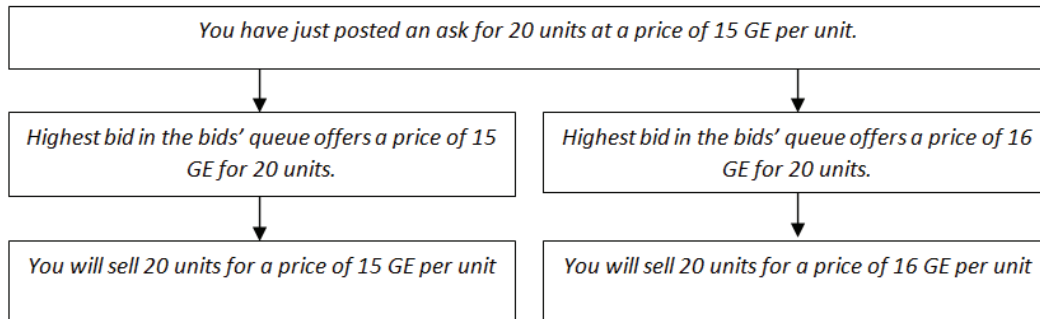
Open bids are listed on the right, lower half of the screen. (see figure at the end of production)

**Attention:** In the case of instant trading, every ask you post will be matched with precisely one bid (the highest). By the same token, every bid you post will be matched with precisely one ask (the lowest).

#### *Transaction price*

The price will be determined in the following manner:

- If the prices of the matched bid and ask are equal, they will be traded at this price.
- If prices differ, trade will take place at the price of the older post. Thus, if the bid you just posted is matched with an older ask with a lower price, the transaction will be carried out at the ask's price. If the ask you just posted is matched with an older bid with a higher price, the trading transaction will be carried out at the bid's price.



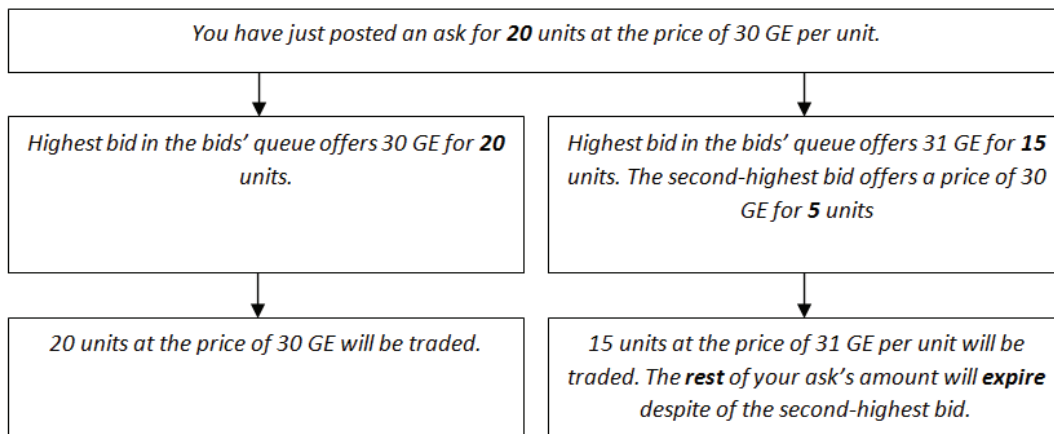
#### EXAMPLE FOR THE TRANSACTION PRICE

#### *Transaction amount*

As in the case of instant trading every bid/ask will be matched with precisely one older ask/bid, the amount of goods to be transacted is determined in the following manner:

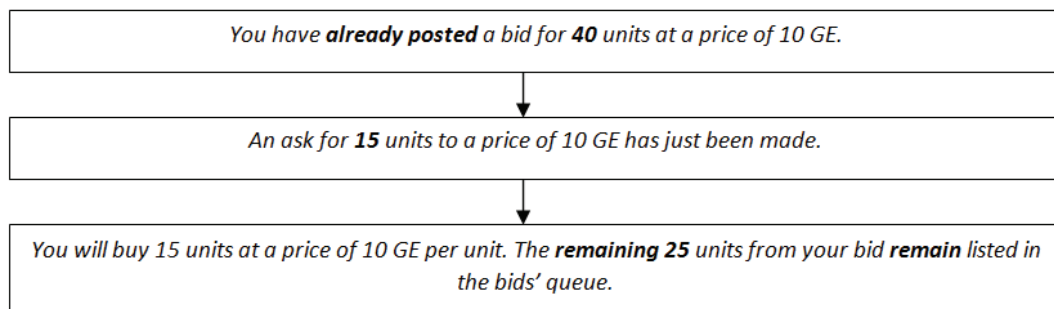
- If the bid or the ask you just posted has a lower or an equal amount to the ask or bid it is matched with, the complete amount of your bid/ask will be traded.

- If the bid or the ask you just posted has a higher amount than the ask or bid it is matched with, only the smaller amount amount of the older ask or bid will be traded. This means that your bid or ask will be only partly transacted. All remaining units from your offer will expire and will not be served by other asks or bids in the queue. If you want to trade more units, you will have to post two sequential offers.



EXAMPLE FOR TRANSACTION AMOUNT OF AN INSTANT OFFER

If your ask is already listed in the asks' queue, though, and a bid, which was posted later demands a smaller amount than the amount your ask offers, the non-transacted part of your ask remains listed. By the same token, the rest of your bid remains listed if it was already listed in the bids' queue and an ask, which was posted later, offers a smaller amount than your bid demands.



EXAMPLE FOR TRANSACTION AMOUNT OF A STANDING OFFER

### *Deleting bids and asks*

In the left, lower half of the screen you see a list of your posted bids and asks. You can delete these at any time. Keep in mind that you cannot alter any posted bids or asks – you can only delete them. Bids or asks cannot be deleted after they have been accepted by other traders.

### *Rankings of bids and asks*

Posted bids are listed in the bids' queue. Posted asks are listed in the asks' queue. Both lists are sorted by price and time of the posting. When two or more bids/asks with the same price are listed in a queue, they are ranked by time, the older one coming first.

In the middle you can see the price of the last transaction. If there has not been any trading yet, you will see an “–”. A bid/ask remains in its queue until it is deleted or accepted by another trader and, as a consequence, trade takes place.

### *Self-trade*

It is allowed to trade with yourself. This does not change the most recent price being displayed and is not listed as a trade either. Trading with oneself can be regarded as withdrawing part of your own offer.

## **[A1, A4] Auction rules**

### **How to make a bid in the auction**

[A1] In addition to self-production and trading, you can serve parts of the demand by purchasing units of the good in auctions organized by the experimenter. These auctions are scheduled before the control period (see picture at the end of the instructions). Furthermore, there will be an auction at the beginning of the experiment, before the first trading period starts. There will not be any auctions after the last trading period, before the last control period at the end of the experiment, though. Overall, there will be 4 auctions. In each auction 960 units of the good will be sold.

[A4] In addition to self-production and trading, you can serve parts of the demand by purchasing units of the good in auctions organized by the experimenter. These auctions are scheduled after each trading period (see picture at the end of the instructions). Furthermore, there will be an auction at the

beginning of the experiment, before the first trading period starts. There will not be any auctions after the last trading period, before the last control period at the end of the experiment, though. Overall, there will be 16 auctions. In each auction 240 units of the good will be sold.

You can submit bids in the auction as price-quantity combinations. We will specify 11 prices for which you will have to state the number of units you are willing to buy at this particular price. The stated amount for a certain price cannot be higher than the amount for a lower price. Furthermore, only whole numbers – starting from “0” – will be accepted as valid numbers for the amount you wish to buy at a certain price. By clicking on “Check” you can see if your set of auction bids is consistent with these rules without making a binding bid schedule.

You will be given 2 minutes to state your set of requested amounts as a binding bid schedule. Your auction bids will remain unknown for the other participants, so that the time you make the bid will not influence in any way your chances to win units in the auction. Only your final auction bids, which you transmit through clicking on “Send” will be taken into account.

### **Calculation of the clearing price and serving the auction bids**

After the auction on the basis of your auction bids your demand function will be estimated. For every price the sum of the requested amounts by all participants will be calculated. Then, every price-amount-combination formed in the described way will be connected linearly. This gives the aggregated demand function. As the figure below shows, it is decreasing.

For every price the total, aggregated requested amount by all participants will be calculated. The price, for which the aggregated demand matches the amount supplied by the experimenter, is the auction’s clearing price. Please, keep in mind that this price can be between those specified by us for you to bid on. On the basis of individual auction bid it will be calculated how many units of the good every participant would want to buy at the clearing price. This amount will be credited to the winners. They will pay the clearing price for every unit.

Should the aggregated demand over all participants at the lowest price of “0” be lower than the supply amount in the auction, surplus units would be distributed among participants in the group proportionately to their bids at

the price “0”. Should the aggregate demand at the highest price of “50” exceed the supply amount in the auction, all bids at the highest price would be served only partially.

After the end of the auction you will be informed about the auction clearing price and the amount you bought.

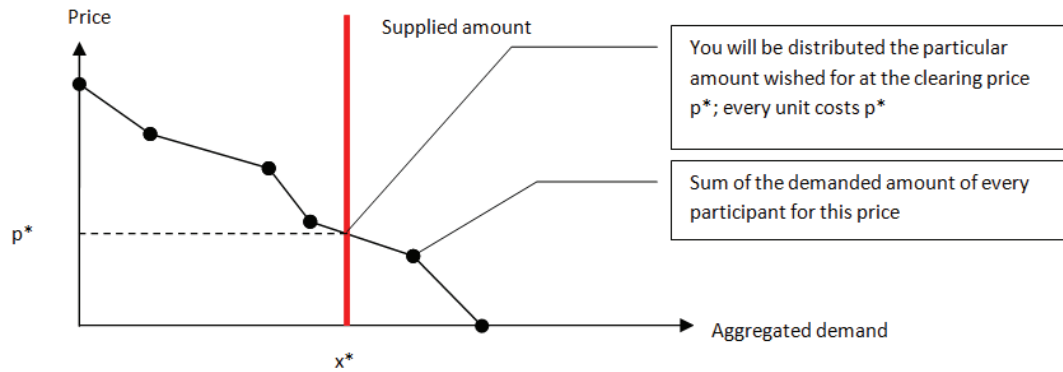


FIGURE 4.6: CALCULATING THE AUCTION CLEARING PRICE

### [G] Free allocation

In addition to self-production and trading, you can serve some of the demand with those units the experimenter will hand you for free. These allocations are scheduled before the control period (see picture at the end of the instructions). Furthermore, there will be an allocation at the beginning of the experiment, before the first trading period starts. There will not be an allocation after the last trading period, before the last control period at the end of the experiment, though. Overall, there will be 4 free allocations.

In each allocation 960 units of the good will be distributed amongst the participants in your group. Each participant may receive a different amount of free units. Each time there is a free allocation three participants will receive 14 units. Three other participants will receive 20 units. Four other participants will receive 42 units. Three other participants will receive 90 units. The remaining three participants will receive 140 units ( $3 \cdot 14 + 3 \cdot 20 + 4 \cdot 42 + 3 \cdot 90 + 3 \cdot 140 = 960$ ).

Before the experiment starts, the amount of free units for each participant will be randomly chosen according to the values from above. The resulting amount set for each participant at the beginning will not change during the



experiment. Thus, the participant will receive this amount at each of the free allocations.

You will be told the amount you receive along with your initial endowment when the experiment starts.

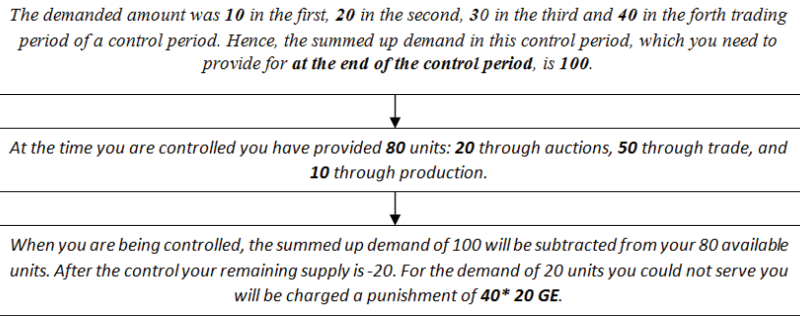
### **Control and punishment**

[A1, A4] Four times throughout the experiment – after every fourth trading period – we will check whether you can serve the demand from these periods through self-production and purchases. With this aim, the amount of goods you provided through self-production and purchases will be compared to the last 4 trading periods' summed up demand. The summed up demand from the last 4 trading periods will be subtracted from the amount of goods you have provided up to this point. If the difference is smaller than “0”, this would mean that you are not able to serve the demand. In this case you will be charged 40 GE for every missing unit. This punishment will not release you from the obligation to retroactively provide any missing units. Thus, in case of punishment, any missing amounts will be transferred into the next period.

[G] Four times throughout the experiment – after every fourth trading period – we will check whether you can serve the demand from these periods through self-production, purchases and free allocations. With this aim, the amount of goods you provided through self-production and purchases will be compared to the last 4 trading periods' summed up demand. The summed up demand from the last 4 trading periods will be subtracted from the amount of goods you have provided up to this point. If the difference is smaller than “0”, this would mean that you are not able to serve the demand. In this case you will be charged 40 GE for every missing unit. This punishment will not release you from the obligation to retroactively provide any missing units. Thus, in case of punishment any missing amounts will be transferred into the next period.

Hence, you do not have to serve every trading period's demand by the end of the respective trading period, but only the summed up demand of 4 trading periods within the respective control period.

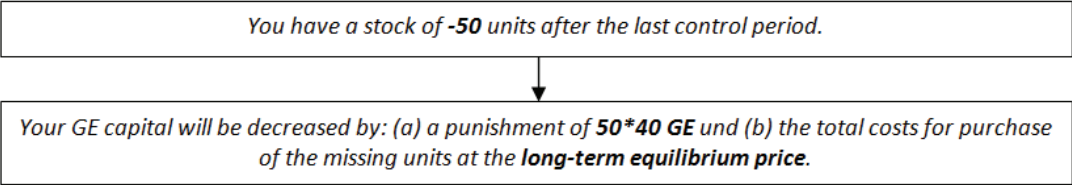
Because the experiment ends after the last control period, you will not be able to retroactively provide any missing units. These will be sold to you at the long-term equilibrium price. As a result, your GE capital will be decreased by



EXAMPLE FOR THE CONTROL PROCEDURE

the value of the missing units. By the same token, any surplus units after the end of the experiment will be bought from you at the long-term equilibrium price leading to an increase of your GE capital by the value of the surplus units.

The long-term equilibrium price is the price resulting from the best possible combination of all participants' purchase and self-production of units of the good. This price is a theoretical value and cannot be influenced by any of the participants.



EXAMPLE FOR CREDITS AT THE END OF THE EXPERIMENT

[A1] Experimental procedure

The experimental procedure is identical for all participants from your group. It is depicted in the figure below. Details on the different stages were explained above.

There are four control periods. Every control period consists of 4 trading periods. Before each control there is an auction. There is no auction before the last control. Furthermore, an auction precedes the first trading period at the beginning of the experiment. Thus, altogether there are 4x4=16 trading periods and 4 auctions. In every auction 960 units of the good are auctioned

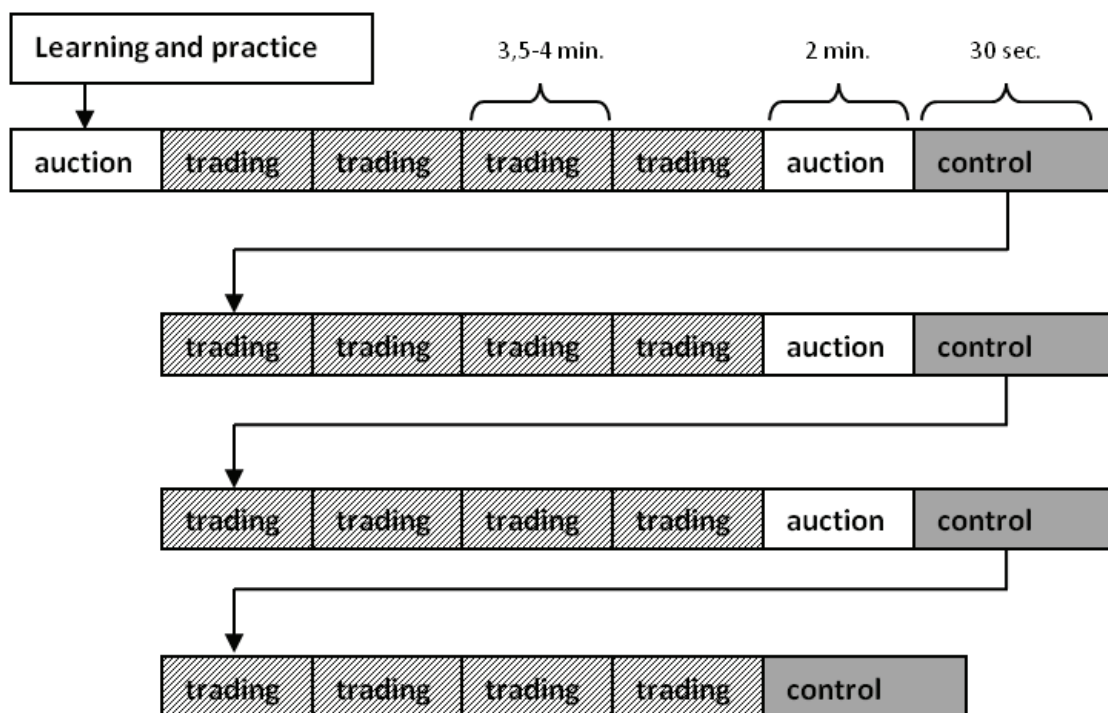


FIGURE 4.7: EXPERIMENTAL PROCEDURE [A1]

off. After each auction you receive the units you have won and your capital of GE is charged accordingly.

At the beginning of every trading period you will be informed about the demand you have to serve in this trading period. Simultaneously, you will receive an advance payment for this demand regardless of your ability to serve it. Please note that at the beginning of every new trading period, open bids or asks from the previous trading period expire.

Every four trading periods are followed by a control stage, which checks whether you have met the sum of the demand of the 4 preceding trading periods. In case of missing units you are punished. After the last control stage the missing units will be sold to you and any surplus units will be bought from you by the experimenter automatically.

#### [A4] Experimental procedure

The experimental procedure is identical for all participants from your group. It is depicted in the figure below. Details on the different stages were explained above.

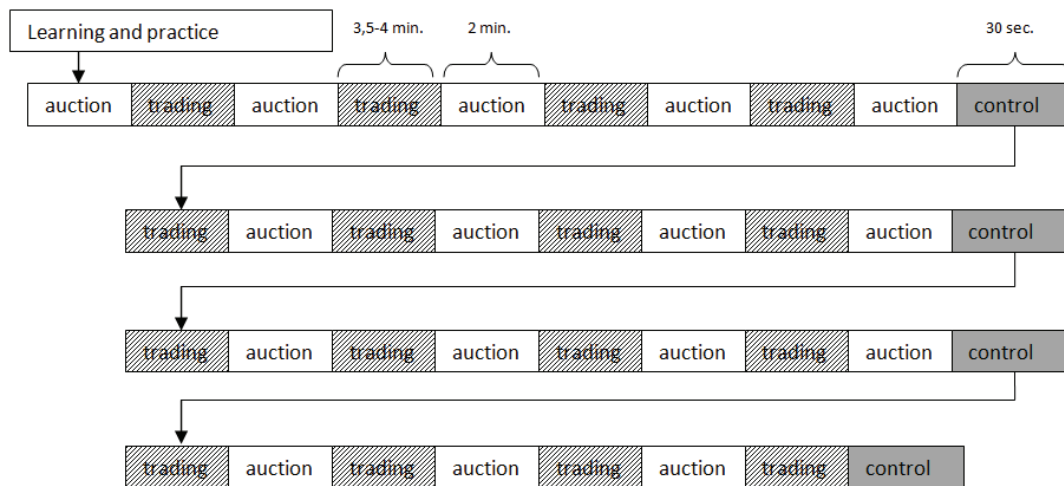


FIGURE 4.8: EXPERIMENTAL PROCEDURE [A4]

There are four control periods. Every control period consists of 4 trading periods. After each trading period there is an auction. There is no auction before the last control. Furthermore, an auction precedes the first trading period at the beginning of the experiment. Thus, altogether there are  $4 \times 4 = 16$  trading periods and 16 auctions. In every auction 240 units of the good are auctioned off. After each auction you receive the units you have won and your capital of GE is charged accordingly.

At the beginning of every trading period you will be informed about the demand you have to serve in this trading period. Simultaneously, you will receive an advance payment for this demand regardless of your ability to serve it. Please note that at the beginning of every new trading period, open bids or asks from the previous trading period expire.

Every four trading periods are followed by a control stage, which checks whether you have met the sum of the demand of the 4 preceding trading periods. In case of missing units you are punished. After the last control stage the missing units will be sold to you and any surplus units will be bought from you by the experimenter automatically.

### [G] Experimental procedure

The experimental procedure is identical for all participants from your group. It is depicted in the figure below. Details on the different stages were explained

above.

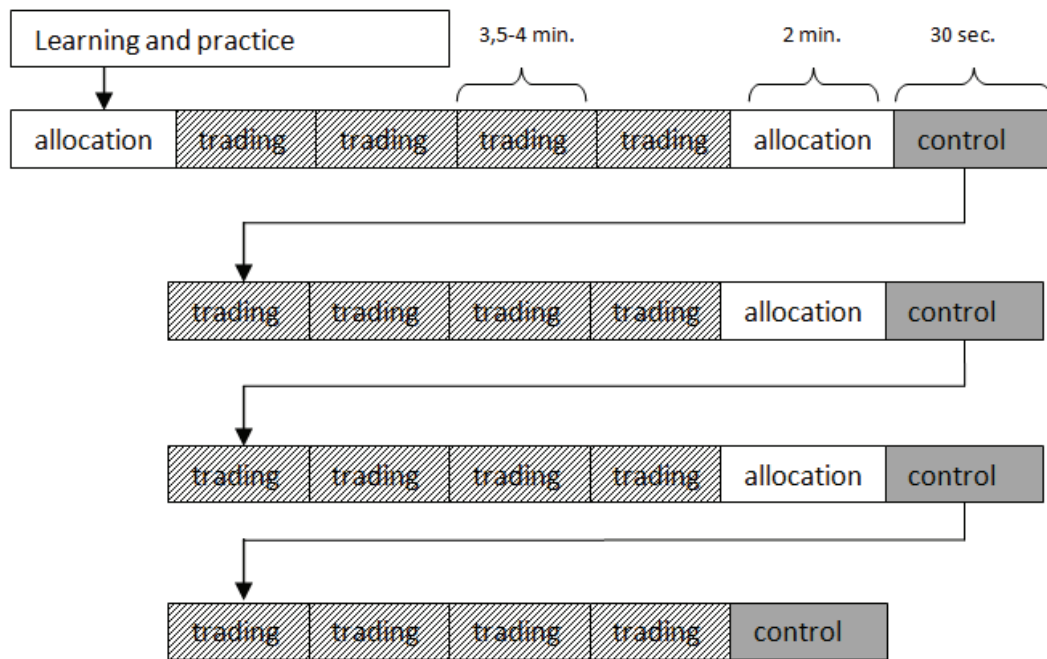


FIGURE 4.9: EXPERIMENTAL PROCEDURE [G]

There are four control periods. Every control period consists of 4 trading periods. There is an allocation before each control. There is no allocation before the last control. Furthermore, an allocation precedes the first trading period at the beginning of the experiment. Thus, altogether there are  $4 \times 4 = 16$  trading periods and 4 allocations. In every allocation 960 units of the good are distributed amongst the participants in your group.

At the beginning of every trading period you will be informed about the demand you have to serve in this trading period. Simultaneously, you will receive an advance payment for this demand regardless of your ability to serve it. Please note that at the beginning of every new trading period, open bids or asks from the previous trading period expire.

Every four trading periods are followed by a control stage, which checks whether you have met the sum of the demand of the 4 preceding trading periods. In case of missing units you are punished. After the last control stage the missing units will be sold to you and any surplus units will be bought from you by the experimenter automatically.

Handelsperiode: 1 Kontrollperiode: 1	Ihr derzeitiges Vermögen ist: 5000 Ihr derzeitiger Güterbestand ist: 240	Verbleibende Zeit [Sek]: 170
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**Information-box**

<b>Ihr Vermögen:</b>	5600
J. davon reserviert	110
<b>Verfügbares Vermögen:</b>	5490
<b>Ihr Güterbestand:</b>	240
J. davon reserviert	30
<b>Verfügbarer Güterbestand:</b>	210
<b>Nachfragemenge in dieser Handelsperiode:</b>	20
Aufsummierte Nachfragemenge in dieser Kontrollperiode	20
<b>Ihr Güterüberschuss über die aufsummierte Nachfragemenge:</b>	220
<b>Bereits produziert:</b>	Produktion ist möglich

**production-box**

**Eigene Produktion:**

Zu produzierende Menge:  **Produktion**

Zusätzliche Kosten durch die Produktion der letzten Einheit: 0 **Berechnen**

Gesamtkosten für die Produktion dieser Menge: 0

**posting bids/ asks**

**Neue Handelsgebote abgeben:**

Preis pro Stück:  **Verkauf**

Menge:  **Kauf**

**Open bids/ asks**

**Übersicht der offenen eigenen Gebote**

Nr.	Typ	Preis	Menge
1		20	10
2	Verkauf	15	8
3	Verkauf	25	12
4	Kauf	3	10
5	Kauf	2	15
6	Kauf	10	5

Nummer:  **Rückzahlen**

**open asks in the market**

**Verkaufsangebote (absteigend)**

Preis pro Stück	Menge
25	12
20	10
15	8

**open bids in the market**

**Kaufangebote (absteigend)**

Preis pro Stück	Menge
10	5
3	10
2	15

Letzter Preis: 0 **Gib zu Kontrolle/Bestellung**

FIGURE 4.10: SCREENSHOT FROM THE TRADING AND PRODUCTION STAGE

Amount of production	Additional costs caused by last produced unit	Total costs of production for this amount
1	1	1
2	2	3
3	3	6
4	4	10
5	5	15
6	6	21
7	7	28
8	8	36
9	9	45
10	10	55
11	11	66
12	12	78
13	13	91
14	14	105
15	15	120
16	16	136
17	17	153
18	18	171
19	19	190
20	20	210
21	21	231
22	22	253
23	23	276
24	24	300
25	25	325
26	26	351
27	27	378
28	28	406
29	29	435
30	30	465

Amount of production	Additional costs caused by last produced unit	Total costs of production for this amount
31	31	496
32	32	528
33	33	561
34	34	595
35	35	630
36	36	666
37	37	703
38	38	741
39	39	780
40	40	820
41	41	861
42	42	903
43	43	946
44	44	990
45	45	1035
46	46	1081
47	47	1128
48	48	1176
49	49	1225
50	50	1275
51	51	1326
52	52	1378
53	53	1431
54	54	1485
55	55	1540

# Chapter 5

## Conclusion

This work presents the results of three experimental studies that investigate how changes in the auction environment or the auction rules can affect the auction outcome. All studies find that, if theorists want to predict the outcome of an auction correctly, they need to account for a number of behavioral anomalies.

In particular, Chapter 2 studies behavior in auctions with and without ambiguity and shows that without accounting for nonlinear probability weighting, standard theories are incapable of explaining the experimental results. Nonlinear probability weighting is a widely established behavioral phenomenon (Prelec 1998), which has received little attention in the context of auctions. Armantier and Treich (2009) are one of the few exceptions – they show that when nonlinear probability weighting is controlled for, the effect of risk aversion on overbidding decreases substantially. Chapter 2 provides another example of the importance of nonlinear probability weighting in the context of auctions, thus supporting the notion that future research on bidding behavior in auctions should attempt to account for this behavioral phenomenon.

Chapter 3 investigates experimentally the predictions of two models of auctions with favoritism studied in Burguet and Perry (2009). It also shows that unless risk aversion and bounded rationality are accounted for, the experimental results only partially support the theoretical predictions. More importantly, the alleged optimality of one of the mechanisms with favoritism is shown to be highly contingent on the favored bidder behaving rationally. Hence, the practical applicability of the optimal mechanism is contested and future re-



search should provide optimal mechanisms more robust to boundedly rational behavior.

Finally, the experimental study presented in Chapter 4 shows that while one big and several smaller auctions for multiple homogenous goods lead to similar allocation after the auctions, the prices paid differ substantially and so does the development of an aftermarket. Although the causes for this observation are not explicitly discussed in Chapter 4, the “declining price anomaly” we observe in the sequential auctions for homogenous goods is a commonly reported phenomenon (see Ashenfelter 1989, for example). Risk aversion has been identified as one of its possible causes (Mezzetti 2011). Given that prior to the introduction of the US Greenhouse Gas Initiative and the European Emission Trading Scheme, most of the analysis regarding the auction frequency was only theoretical, future research should also attempt to enrich the analysis of multi-unit auctions to account for boundedly rational behavior.

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# Erklärung

## nach §6 der Promotionsordnung vom 16. Januar 2008

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