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DOCTORAL THESIS

Essays in (Experimental) Industrial Organisation:

Online Ratings for Credence Goods, Anchoring Bias in Ratings and Rebate Rules for Crowdfunding

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This thesis is submitted in accordance with the requirements of the University of Liverpool for the degree of *Doctor of Philosophy*.

by

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Dedicated to my grandfather Fevzi.

Abstract

By means of controlled economic experiments, this thesis investigates online interactions such as online ratings and crowdfunding and also potential biases that might arise from them such as the anchoring bias.

Firstly, online ratings and their potential fraud in markets for expert services are analysed theoretically and experimentally. Our findings suggest that ratings significantly reduce fraudulent behaviour by sellers. Better informed buyers are overcharged less and having informed buyers on the market significantly reduces overcharging. But the rating system format does not significantly affect overcharging. In terms of rating patterns, we uncover that informed and uninformed buyers punish fraudulent behaviour with negative ratings whereas non-fraudulent behaviour is rewarded with positive ratings. However, communication through ratings by informed buyers is imperfect. The payoff a buyer receives affects the decision to rate and the rating positively. We further find that being honest pays off as the frequency of a seller being chosen is positively affected by positive ratings and negatively affected by negative ratings.

Secondly, we employ an online experiment to investigate the prevalence of the anchoring bias in online ratings. Our results reveal that high and socially derived anchors lead to significant overrating compared to both control conditions. Surprisingly, we do not observe underrating for low anchors, but indications of overrating as well. Participants place higher trust in socially derived anchors compared to high and low anchors and the trust participants exhibit towards the socially derived anchors is explanatory of the anchoring bias.

Thirdly, we study the efficacy of rebate rules in reward-based crowdfunding, where a funding goal has to be met for a project to be realised. Only investors who provide at least a reservation bid receive a reward from the project. We propose and experimentally test two rebate rules against the customary all-or-nothing rule. In the first rule, we adapt the proportional rebate rule from threshold public good games to our reward-based setting. In the second rule, we introduce the bid-cap rule. Investors pay their bid, which cannot exceed the bid-cap and is determined such that the provision point is exactly met. Overall, the bid-cap rule induces less variance in final payments compared to the proportional rebate rule for fixed bids. We find that both rebate rules increase the project realisation rate compared to the all-or-nothing rule, while we can confirm that the variance of final payments is lower in the bid-cap rule compared to the proportional rebate rule.

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Declaration of Authorship

I, Yigit Oezcelik, declare that this thesis and the work presented in it are my own.

I hereby certify that this thesis has been composed by me and is based on my own work, unless stated otherwise. No other person's work has been used without due acknowledgement in this thesis. All references and verbatim extracts have been quoted, and all sources of information have been specifically acknowledged.

This work was done wholly while in candidature for a research degree at the University of Liverpool.

Where the thesis is based on work done by myself jointly with others, I have made clear exactly what was done by others and what I have contributed myself.

Signed: *Yigit Oezcelik*

Date: 08 August 2022

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Chapter 1

Introduction

Advances in the Internet have provided online marketplaces and online interactions a fertile ground. We make online interactions almost every day from purchasing products, getting advice, rating in-person and online interactions, investing in crowdfunding projects or donating to charity to name a few examples. This thesis experimentally scrutinises such online interactions and also potential biases that might arise from them.

Overall, this thesis consists of five chapters. This first chapter provides a general introduction to the thesis. In the second chapter, we theoretically and experimentally analyse online ratings in markets for expert services. The third chapter takes a more critical stance at consumer-provided online ratings. We employ an online experiment to investigate the prevalence of the anchoring bias in online ratings. The fourth chapter is concerned with reward-based crowdfunding where different rebate rules are compared. The fifth chapter concludes the thesis.

When our car makes strange noises, the washing machine breaks down or we require medical advice, we need to consult an expert. But how can we be sure that experts provided an adequate service and charged the right price? In fact, many markets that are crucial to any economy such as healthcare markets or markets for repair services are plagued by pronounced informational asymmetries between consumers and experts. For example, in OECD countries alone, health care expenditures account for about 9% of GDP (Lorenzoni et al. 2019). A large proportion of these expenditures comes from providing medical treatment, where doctors with informational advantages prescribe treatments to uninformed patients. When seeking help, patients might be uninformed about the most effective treatment in improving their health and the related costs. Furthermore, car repairs carried out in Europe alone are worth 100 billion Euro per annum but a significant share of the repairs being unnecessary (see Kerschbamer & Sutter 2017, Hubbard 1998).

Goods and services provided by better-informed experts to less-informed consumers are so-called credence goods, whose quality is impossible to assess even after consumption (Darby & Karni 1973). Consumers have to rely on the honesty of the experts and usually cannot verify the adequacy of a provided service and charged price. The prevalent informational asymmetries incentivise expert to defraud consumers and results in inefficiencies.

To restore efficiency and undermine fraud several institutional solutions such as liability or verifiability and market mechanisms like competition and reputation-building have been analysed in previous literature (see Dulleck & Kerschbamer 2006, Dulleck et al. 2011). Liability necessitates an efficient quality provision whereas verifiability requires the seller to charge an adequate price for the service or good provided. Both institutions have been shown to be theoretically effective in restoring efficiency on markets for credence goods. Dulleck et al. (2011) and Mimra et al. (2016) investigate the above-mentioned institutions and market mechanisms by means of a large-scale experiment. Liability is identified as a necessary condition for efficiency whereas verifiability has a small but still positive effect on efficiency. Reputation decreases the level of fraud and increases trade volume.

However, in this digital age, it has become very easy for consumers to consult other consumers' reviews on web pages or platforms. Credence goods are also being rated excessively, e.g. on, jameda.de, rateMDs.com, google reviews etc. to name a few examples. In Chapter 2 we investigate whether such voluntary online ratings that are provided by consumers can incentivise expert sellers to provide credence goods of appropriate quality and price. But this necessitates that those ratings are somewhat informative. To allow for ratings to be informative, we explore the possibility that some consumers are better informed than others. We explore whether the effectiveness of the rating system depends on the presence of more informed consumers and whether the format of the rating system affects the decisions of buyers and sellers.

Our findings suggest that ratings significantly reduce fraudulent behaviour by sellers. Better informed buyers are overcharged less and having informed buyers on the market significantly reduces overcharging. But the rating system format does not significantly affect overcharging. Informed and uninformed buyers punish fraudulent behaviour with negative ratings whereas non-fraudulent behaviour is rewarded with positive ratings. However, communication through ratings by informed buyers is imperfect. The payoff a buyer receives affects the decision to rate and the rating positively. We further find that being honest pays off as the frequency of a seller being chosen is positively affected by positive ratings and negatively affected by

negative ratings.
In Chapter 3, we experimentally scrutinise the prevalence of the anchoring bias in online ratings. Decades of research in behavioural economics have shown that human decision-making is imperfect and prone to errors and biases. The most commonly studied bias in this context is the anchoring effect, which was first introduced by Slovic & Lichtenstein (1972) and further elaborated by Tversky & Kahneman (1974). The anchoring bias can be defined as an irrelevant informational cue that might influence behaviour in a way that is inconsistent with rational decision-making (Tversky & Kahneman 1974).

The anchoring bias has been shown to be prevalent in several consequential economic decisions such as credit card minimum repayments (Stewart 2009), real estate evaluations (Northcraft & Neale 1987), strategic interactions (Ivanova-Stenzel & Seres 2021), and stock market estimations (Kaustia et al. 2008) to name a few.

If ratings are affected by the bias, this might hamper their informativeness with detrimental consequences for buyers, online platforms and sellers. We perform an online experiment to assess the effect of the anchoring bias in consumer ratings. Our rating task is a framed variation of the slider task. We diverge from the literature by implementing non-numerical (visual) anchors. We compare three anchoring conditions, with either high, low or socially derived anchors present against two control conditions – one without anchors and an unframed slider task. We find that high and socially derived anchors lead to significant overrating compared to both control conditions. Surprisingly, we do not observe underrating for low anchors, but indications of overrating as well. Participants place higher trust in socially derived anchors compared to high and low anchors and the trust participants exhibit towards the socially derived anchors is explanatory of the anchoring bias.

Crowdfunding is rapidly expanding across the globe. According to Statista (2021) the global reward-based crowdfunding market achieved $12.27 billion in 2021, forecast to double in 2027 with an annual expected growth rate of 11%. This growth has given rise to large-scale platforms such as Kickstarter, Indiegogo or GoFundMe. Agrawal et al. (2014) define crowdfunding as raising capital from many people through an online platform. There are multiple reasons why crowdfunding is utilised. Investors can support people with similar interests, help others and be part of a community. For project developers crowdfunding can be an alternative way to fund their project Furthermore, they can increase the publicity and popularity of their project while keeping the authority on it Crowdfunding can also facilitate long-term customer creation (Gerber & Hui 2013).

On crowdfunding platforms funding supply and demand are matched via a mechanism that is determined by each platform itself. The most commonly used mechanism is the All-or-Nothing mechanism where the developer keeps the investment made funding goal is reached, if not, the money is returned. In reward-based crowdfunding, project backers usually receive non-monetary rewards for their contribution to a project, if their contribution exceeds a pre-set entry fee such as a limited or early version of a product.

In Chapter 4, we study the efficacy of rebate rules in reward-based crowdfunding, where a funding goal has to be met for a project to be realised. Only investors who provide at least a reservation bid receive a reward from the project. We propose and experimentally test two rebate rules against the customary all-or-nothing rule. In the first rule, we adapt the proportional rebate rule from threshold public good games to our reward-based setting. In the second rule, we introduce the bid-cap rule. Investors pay their bid, which cannot exceed the bid-cap and is determined such that the provision point is exactly met. The bid-cap rule induces less variance in final payments compared to the proportional rebate rule for fixed bids. We find in our experiment that both rebate rules increase the project realisation rate compared to the all-or-nothing rule, while we can confirm that the variance of final payments is lower in the bid-cap rule compared to the proportional rebate rule.

Chapter 5 concludes the thesis by providing a general discussion of contributions and outlining future research avenues that builds on the work presented in this thesis.

Chapter 2

Online Ratings for Credence Goods: Experimental Evidence

2.1 Introduction and Literature

Consumers exceedingly rate doctors, financial advisers, and lawyers on different online platforms, see, e.g., jameda.de, google and rateMDs.com. However, medical, law and financial advice are classic examples of credence goods, whose quality is impossible to assess even after consumption (Darby & Karni 1973).

The prevalence of fraud in credence goods markets has been well documented in the literature such as markets for repair services Kerschbamer et al. (2017) and Schneider (2012), healthcare Domenighetti et al. (1993), Gottschalk et al. (2020), taxi rides Balafoutas et al. (2013) and financial advice Anagol et al. (2017). Fraud in credence goods markets can be defined more systematically. A seller may potentially provide insufficient treatment or service to customers. This type of fraud has been coined as undertreatment. Furthermore, a seller can charge for a treatment or service that was not provided, overcharging a customer. Finally, a seller can provide a treatment or service that was not necessary, overtreating a customer.

From a theoretical standpoint, ratings for credence goods may not be informative, given the informational asymmetry between sellers and clients. Empirically, very little is known about the informational content of ratings for credence goods, whether consumers use them when selecting a provider, and most importantly, whether the quality and the price of credence goods are affected by the availability of online ratings.[1]

[1]This is not to be confused with ratings for experience goods, i.e., goods, whose quality can be judged after consuming them (e.g., wine). A large body of literature studies rating systems for experience goods, see, e.g., Hu, Pavlou & Zhang (2009), Nosko & Tadelis (2015),

Important exceptions are Lantzy & Anderson (2020), Kerschbamer et al. (2019), and Bolton et al. (2019) who empirically investigate the role of ratings on markets for expert services or under attributional uncertainty. Empirically comparing medical board sanctions and ratings for doctors taken from RateMDs.com, Lantzy & Anderson (2020) find that ratings can carry signals for doctor suitability. In a natural field experiment, Kerschbamer et al. (2019) scrutinise how information acquisition from specialised webpages and reviews affects the level of sellers' price charges for computer repairs. They find online ratings of repair shops are a good predictor of prices where lower prices are associated with good ratings and higher prices with bad ratings. Bolton et al. (2019) experimentally investigate the endogenous provision of ratings under attributional uncertainty which induces lenient feedback provision.

The first question that we investigate in this chapter is whether voluntary online ratings by consumers can incentivise sellers to provide credence goods of appropriate quality and price. It should be noted that in our analysis of seller behaviour, we mainly focus on undertreatment and overcharging. As we will explain in the next section where we derive our behavioural predictions, overtreatment is irrelevant in our setup as the price for a service or treatment charged by a seller is independent of the treatment applied. Several related experimental studies by Dulleck et al. (2011), Kerschbamer et al. (2017), or Beck et al. (2014), report very low overtreatment rates of around 5%.[2]

Ratings can only incentives sellers to provide goods of appropriate quality and price if these are somewhat informative. To allow for ratings to be informative, we explore the possibility that some consumers are better informed than others. This can be due to their professional background and/or experience, which may partly alleviate the information asymmetry between sellers and consumers and enable consumers to (better) judge the good or service received. In fact, Domenighetti et al. (1993), and more recently Johnson & Rehavi (2016) provide evidence that patients who are physicians themselves are less likely to receive surgical procedures than the general population.

The physician-patient is an example of a consumer, who is better informed than the usual consumer. It could be due to this informational advantage that

Lafky (2014), Bolton et al. (2004), Bolton et al. (2013), Bolton et al. (2018), Bolton et al. (2020), Cui et al. (2020), Li & Xiao (2014) to name a few.

[2]We acknowledge that there is a growing body of field experiments scrutinising overtreatment on credence goods markets such as Balafoutas et al. (2013) for repair service markets, Brosig-Koch et al. (2016); Das et al. (2016); Hennig-Schmidt et al. (2011) for healthcare markets where certain institutional constraints or ethical restrictions may apply. We do not impose any restrictions on sellers' behaviour.

those consumers are not offered unnecessary treatments. If all consumers make their decision to provide a rating dependent on whether they are sufficiently informed, and only the better-informed go ahead and rate, adequate information has the chance to diffuse to all consumers through online ratings. As a result, sellers, who anticipate this information diffusion and wish to stay competitive will provide higher-quality products or services for lower prices. Consequently, ratings may turn out to be a powerful tool for fostering competition among providers of credence goods and for the improvement of product/service quality and consumer welfare.

Hence, our second question is whether the effectiveness of the rating system depends on the presence of more informed consumers who can provide them. Finally, we explore whether the design of the rating system affects the decisions of consumers and providers of credence goods. We explore two different rating systems. In the rating system 'pooled', ratings by better informed and uninformed consumers are presented using the same distribution as this is the case in reality, such that it is impossible to distinguish, whom the rating comes from. In the rating system 'separated', all market participants observe the ratings given by the more informed consumers separately from those provided by the uninformed consumers.

We develop a theory to derive predictions and conduct a controlled laboratory experiment to test them. The laboratory environment allows us to observe who (an informed or an uninformed consumer) provides a rating, who abstains from providing a rating, what experience motivates a consumer to leave a rating, as well as the seller's immediate reaction to ratings. With field data, we would only observe ratings on the aggregate without all this additional information.

In our experiment, we employ a credence goods game à la Dulleck et al. (2011). A consumer faces either a small or a large problem. The seller provides a treatment, which can be high or low. The low treatment fixes the small problem only, while the high treatment fixes both problems. The high treatment is more costly for the seller than the low treatment. As in Mimra et al. (2016), we fix prices by specifying one price for the high treatment and one price for the low treatment. The seller learns which treatment the buyer needs and decides on treatment and price. Consumers do not know which treatment they need. After consumption, consumers learn which profit they made. We modify the game by introducing the possibility for consumers to rate sellers. We compare this setting to a setting where ratings are not available (No Ratings Condition vs. U-Ratings Condition).

As a second step, in addition to the ratings, we introduce a second kind of consumer, who after the interaction with the seller additionally learns which type of treatment they needed and which treatment they got. After this feedback, consumers can leave a positive or a negative rating, or decide to abstain from rating altogether. Depending on the condition, ratings are either presented together or separately for informed (I) and uninformed (U) consumers (U-I-Pooled Condition vs. U-I-Separate Condition). Sellers compete through their reputation, i.e. buyers select a seller to interact with, based on the accumulated ratings of sellers. In Dulleck et al. (2011), sellers compete through "private histories". There, each consumer keeps track of her past experiences with a particular seller but does not observe how this seller treated other consumers.

Mimra et al. (2016) look at competition given private vs. public histories. Private histories are implemented like in Dulleck et al. (2011). With private histories, each consumer has access not only to her private experiences with a seller but also to the experiences of all other consumers with this seller. While public histories are not available in reality (we agree with Dulleck et al. (2011) that consumers can rarely observe how sellers treat other consumers), ratings are. While some ratings will be informative and objective, many will be fraudulent, subjective or based on insufficient knowledge of the quality of the product/service that must be rated. We want to explore, whether ratings for credence goods can be sufficiently informative, to be placed between private histories and public histories in terms of their informational content.

Our findings suggest that ratings significantly reduce overcharging of buyers. Better informed buyers are overcharged less but overall having informed buyers or the rating system format does not significantly affect overcharging. Undertreatment is similar across experimental conditions. Informed and uninformed buyers punish undertreatment with negative ratings whereas honest behaviour is rewarded with positive ratings. However, communication through ratings by informed buyers is imperfect. The payoff a buyer receives affects the decision to rate and the rating positively. The frequency of a seller being chosen is positively affected by positive ratings and negatively affected by negative ratings.

2.2 Behavioural Predictions

This section provides a theoretical analysis of our experiment which draws on Dulleck & Kerschbamer (2006) and Mimra et al. (2016). All proofs can be found in the Appendix. Following figure illustrates the flow of the game.[3]

[3]The first period of the game has the same flow as any other period except for the buyer's choice of seller. In the first round, we decided to match each seller with the same number of buyers to level the playing field and avoid selection effects.

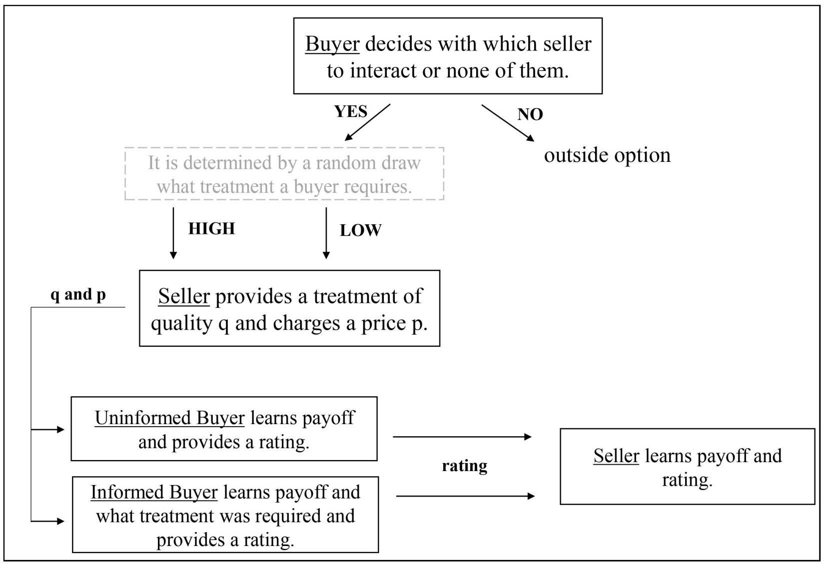

Figure 2.1: Flow of the stage game for periods 2 to T.

Buyers decide if and with which seller to interact. They can be one of two types either requiring a high-cost (q_H) treatment or a low-cost (q_L) treatment, with $q_L < q_H$. The ex-ante probability for a buyer of requiring q_H is h and the probability of requiring is $(1 - h)$. These ex-ante probabilities are common knowledge but the buyers do not know which treatment they require. Sellers are able to identify a buyer's problem and provide treatment of quality q at cost c and charge a price p. Lastly, buyer's can rate the interaction with the seller. When a sufficient treatment is applied, the buyer obtains a value v, where $v > 0$.

Buyers that require a low-cost treatment are treated sufficiently in any case (receiving either q_H or q_L). But if a consumer requires the high-cost treatment q_H then only receiving q_H is sufficient. An insufficient treatment yields a value of $v = 0$. Formally, if q^* is the treatment required, q is the treatment applied and $\mathbb{I}(\cdot)$ is the indicator function, then the buyer's payoff post-treatment equals $v \times \mathbb{I}(q \geq q^*)$. If the price charged is p then the net utility of buyer $b \in B$ is given by

$$U_b(q, p; q^*) = v \times \mathbb{I}(q \geq q^*) - p. \tag{2.1}$$

Seller $s \in S$ applies treatment q, charges price p and get the following net utility

$$U_s(q, p) = p - c(q), \tag{2.2}$$

where $c(q)$ is the cost associated with treatment q, $c(q) \in \{c_H, c_L\}$. Sellers can defraud buyers in two ways. They can undertreat and (or) overcharge buyers. A buyer is undertreated if an insufficient treatment is provided which means that the buyer needed q_H but q_L was provided. Overcharging occurs if low-cost treatment q_L was applied but high-cost treatment price p_H was charged.[4]

We also distinguish two kinds of buyers. Buyers either have Informed (I) status or Uninformed (U) status with the respective sets B^U and B^I, which define the elements of the game they observe. Informed and uninformed buyers observe their payoff after the treatment is received (i.e., they observe whether a sufficient treatment has been applied) and the price charged. The difference is that informed buyers also observe their type and thus know which treatment needs to be applied to reach the desired payoff v. Informed buyers can detect undertreatment and overcharging, whereas uninformed buyers can only infer partial information, see Table 2.1 and Table 2.2[5]. Sellers observe the buyers' information status.

Required	Charged	Value	Undertreament	Overcharging
q_H	p_H	v	-	-
		0	detected	detected
	p_L	v	-	-
		0	detected	-
q_L	p_H	v	-	detected
		0 not possible		
	p_L	v	-	-
		0 not possible		

Table 2.1: Informed buyer's inference from observations q, p, v.

Charged	Value	Undertreament	Overcharging
p_H	v	-	detected with pos.prob. $(**)$
	0	detected	detected
p_L	v	-	-
	0	detected	-

Table 2.2: Uninformed buyer's inference from observations p, v.

[4]Additionally, a seller can overtreat a buyer. One can define overtreatment as q_L required, q_H applied. But overtreatment is irrelevant in our setup where the seller is free to charge p_H or p_L regardless of the treatment applied (the seller does not need to overtreat to charge p_H, she can charge p_H providing the low-cost treatment). (Using the terminology of Dulleck & Kerschbamer (2006), our present setup assumes away verifiability (assumption V).)

[5]Remark $(**)$: When the uninformed buyer is charged p_H and observes v in a one-shot game, there is potential fraud if the buyer required the low-cost treatment in the first place

2.2.1 Perfect Bayesian Equilibrium of the Game

Let us denote $\alpha \in [0,1]$ the share of selfish sellers, and $\beta \in [0,1]$ the share of selfish buyers. We consider several information structures, describing what the buyers observe after every round of the market game. In all cases, the buyers have access to their private history with each seller (Seller IDs are observed). That is, they observe the result of the previous interactions with each of the sellers, from the first round until the most recent round played. Histories are private, which implies that they are not observable to any player other than the buyer and the seller.

We follow Dulleck et al. (2011) and Mimra et al. (2016) in choosing the following model parameters. The seller's costs of providing treatments are $c_L = 2$, $c_H = 6$ and the prices are $p_L = 4$, $p_H = 8$, respectively, for low-cost and high-cost treatment. The price vector $(4, 8)$ is chosen such that a selfish seller is exactly indifferent between offering the high-cost and low-cost treatment at the appropriate prices in a one-shot game.[6] Sellers post the same price vector and the buyers observe it. If a buyer is treated sufficiently $v = 10$, if not $v = 0$. The outside option for both the buyer and the seller $o = 1.6$ [7].

One-shot game:

A one-shot market game—or equivalently, the last round of the repeated game—boils down to a decision problem where the seller maximises utility. A selfish seller maximises his payoff. In contrast, a cooperative seller, who we assume efficiency loving (EL) or inequality averse (IA) following Kerschbamer et al. (2017)[8] maximises a utility function that takes into account not only his own payoff but also the buyer's payoff. Thereby, we maintain Kerschbamer et al.'s assumption that the seller's payoff has a greater effect on his utility than the buyer's payoff. [9] Following tables summarise seller's choices and buyers' inference and payoff in a one-shot game given our parameters of prices and v and costs. [10]

[6]This point is denoted Ω in Kerschbamer et al. (2017).

[7]Hereby, we follow Dulleck et al. (2011) and Mimra et al. (2016).

[8]Selfish, efficiency loving and inequality averse are the only three strong social preference types identified in Kerschbamer et al. (2017). Thus, our theoretical analysis is restricted to these types and does not consider the remaining Spiteful and Inequality Loving social preference types

[9]This is assumption (iii) on p. 402 of Kerschbamer et al. (2017) which is not explicitly modelled into the utility function. 2.4 shows how a cooperative seller's choices affect their payoff.

[10]For our chosen parameters EL and IA sellers behave the same. We acknowledge that in a more general setup, the behaviour of EL and IA sellers can differ. EL sellers treat buyers adequately despite payoff maximisation calling for overtreatment or undertreatment. IA

Buyer		Selfish Seller		
Type	I/U	Action	Description	(Buyer's Payoff, Seller's Payoff)
q_H required		q_L, p_H	Undertreatment & Overcharging	(-8, 6)
	I	Buyer detects Undertreatment & Overcharging		
	U	Buyer detects Undertreatment & Overcharging		
q_L required		q_L, p_H	Overcharging	(2,6)
	I	Buyer detects Overcharging		
	U	Buyer assigns positive probability to Overcharging		

Table 2.3: Selfish Seller's Choice of q and p and buyers' inference and payoffs in a one-shot game.

Buyer		Cooperative Seller		
Type	I/U	Action	Description	(Buyer's Payoff, Seller's Payoff)
q_H required		q_H, p_H	non fraudulent behaviour	(2, 2)
	I	non fraudulent behaviour		
	U	Buyer assigns positive probability to Overcharging		
q_L required		q_L, p_H	Overcharging	(2,6)
	I	Buyer detects Overcharging		
	U	Buyer assigns positive probability to Overcharging		

Table 2.4: Cooperative Seller's Choice of q and p and buyers' inference and payoffs in a one-shot game.

A selfish seller in a one-shot game will provide q_L to both types (which implies undertreatment if the buyer requires q_H) and charge p_H:

$$U_B(q_L, p_H; q_H) = 0 - p_H = -8, U_B(q_L, p_H; q_L) = 10 - p_H = 2,$$
$$U_S(q_L, p_H) = p_H - c_L = 6.$$

An efficiency-loving or inequality-averse seller, as defined in Kerschbamer et al. (2017), in a one-shot game will provide the appropriate treatment for each type, but will charge p_H to both. If the seller charged p_L for treatment q_L, then $(U_B, U_S) = (6, 2)$. Charging p_H for treatment q_L implies $(U_B, U_S) = (2, 6)$. Under the assumption that own payoff has a greater effect on utility than another's payoff (Kerschbamer et al. 2017), the seller prefers the latter situation, regardless of whether they are EL or IA [11]:

$$U_B(q_H, p_H; q_H) = 10 - p_H = 2, U_B(q_L, p_H; q_L) = 10 - p_H = 2,$$
$$U_S(q_H, p_H) = p_H - c_H = 2, U_S(q_L, p_H) = p_H - c_L = 6.$$

sellers dislike inequality and overtreat or undertreat buyers if inequality is reduced despite a reduction in own payoff.

[11] This argument is based on the perfect symmetry of payoffs and does not automatically apply to the case of a seller charging p_H for q_H vs. charging p_H for q_L which results in payoffs of (2,2) vs. (2,6). A strongly inequality-averse seller might prefer the former over the latter.

Perfect Bayesian Equilibrium of the Market Game - Buyer:

Let α_{bst} be the buyer b's belief that seller s is selfish after observing his behaviour in the market game over t rounds. In $t = T$, the buyer chooses to interact with the seller who is least likely to be selfish, i.e., with seller s, where $s \in \arg\min_{\tilde{s}} \alpha_{b\tilde{s}T}$.

Let $\bar{S}$ denote the set of selfish sellers and H_{bs}^t the history of buyer b's observation of seller s up to period t. This always includes the private history and may include publicly observable ratings r, depending on the experimental condition:

$$H^t = \left(H_{bs}^t\right)_{b\in B, s\in S} = \left(a_{bs\tau}, r_{bs\tau}\right)_{b\in B, s\in S, \tau\in\{1,..t\}}$$

where the action of a seller is denoted by a. Buyer b's belief that seller s is selfish, $s \in \bar{S}$, is updated according to the Bayes rule:

$$\alpha_{bst}\left(H_{bs}^t\right) = \Pr\left(s \in \bar{S} | H_{bs}^t\right) = \frac{\Pr\left(s \in \bar{S}, H_{bst}, H_{bs}^{t-1}\right)}{\Pr\left(H_{bs}^t\right)} \tag{2.3}$$

$$= \frac{\Pr\left[H_{bst} | s \in \bar{S}\right] \alpha_{bst-1}\left(H_{bs}^{t-1}\right)}{\Pr\left[H_{bst} | s \in \bar{S}\right] \alpha_{bst-1}\left(H_{bs}^{t-1}\right) + \Pr\left[H_{bst} | s \notin \bar{S}\right]\left(1 - \alpha_{bst-1}\left(H_{bs}^{t-1}\right)\right)}^{12} \tag{2.4}$$

where H_{bst} is the period-t history of seller s observed by buyer b. In the first round, the private history is empty, i.e. $H_{bs}^0 = \emptyset$ for all buyers and sellers, hence, every buyer believes that every seller is selfish with probability $\alpha_{bs0}\left(H_{bs}^0\right) \equiv \alpha$. This is the prior probability of sampling a selfish seller from the population of sellers.

Perfect Bayesian Equilibrium of the Market Game - Seller:

For each buyer-seller interaction in one period, the action set is given by $A = \{q, p\}$ without ratings or $A = \{q, p\} \times \{r^+, r^0, r^-\}$ when ratings are available. Positive ratings are denoted by r^+ and negative ratings by r^-. If a buyer decides not to rate, their rating is denoted by r^0.

The action profile chosen in the interaction between buyer b and seller s in period t is denoted by a_{bst}, with $a_{bst} \in A$. If B_{st} is the set of buyers who choose seller s in period t, then a_{st} denote the action profile in all interactions between s and the buyers in B_{st}. The perfect Bayesian equilibrium (for $\alpha < 1$, $\beta < 1$) is defined by the following Lemma.

[12] The derivation can be found in Appendix 2.6.

Lemma. The action a_{st} of the seller s in period t is part of the equilibrium profile σ^* if and only if it solves the dynamic optimisation program, defined by the Bellman equation[13]

$$V_{st}^* = \max_{a_{st}} \left\{ U_{st} \left(a_{st} \left| B_{st} \right. \right) + \mathbb{E} \left[V_{st+1}^* \left| H^{t-1}, \sigma_{-st}^*, a_{st}, B_{st} \right. \right] \right\}, \qquad (2.5)$$

for all H^{t-1}, B_{st}, where the conditional expectation of the continuation value V_{st+1}^* is taken under the equilibrium assumption, and the terminal condition $\mathbb{E} \left[V_{sT+1}^* \left| H^T \right. \right] = 0$, for any history H^T.[14]

B_{st} denotes the set of buyers that interact with seller s in round t. σ_{-st}^* is the equilibrium strategy profile of all players (buyers and sellers) excluding seller s in period t. The lemma uses the one-shot deviation property that states that it is sufficient to verify that there are no profitable one-shot deviations from equilibrium.

Corollary 1: In equilibrium, a cooperative seller (EL/IA) chooses to treat consumers appropriately and charge p_H in every round of the market game.

This is due to the fact that this action maximises the cooperative seller's utility in a one-shot game and, moreover, leads to a favourable belief update.

In contrast, the selfish seller chooses (q_H, p_H) in the interaction with a buyer b that requires a high-cost treatment if and only if the expected future gain outweighs the loss in the current period. The gain is due to the more favourable belief update by the buyers following their observation of this action, which makes them more likely to choose to interact with the seller s in the future. This directly implies the following corollary.

Corollary 2: In equilibrium, a selfish seller chooses q_L and p_H in the last round of the market game.

As outlined previously, we employ different experimental conditions and depending on the condition, ratings provided by the buyers are either displayed jointly or separately (informed—uninformed) to all consumers. Based on this, we propose the following.

Proposition 1: For any $\alpha < 1, \beta < 1$, $s \in S$, $t \in \{1, ..T\}$, H^{t-1} and B_{st}, if $a_{bst} = c$ for some $b \in B_{st}$ in the perfect Bayesian equilibrium of *U-Ratings* (*U-I-Pooled*), then $a_{bst} = c$ in the perfect Bayesian equilibrium of *U-I-Pooled* (*U-I-Separate*). [15]

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[13] This is a finitely repeated game, hence we assume away discounting.

[14] At the end of the market game, the continuation value is zero for all histories, $V_{sT+1}^* \equiv 0$.

[15] See Appendix 2.6 for the proof.

The proposition says that the seller is at least as likely to choose (q_H, p_H) in the presence of informed consumers and joint ratings as in the absence of informed consumers. Similarly, sellers are at least as likely to choose (q_H, p_H) in the presence of informed consumers and when ratings are separated by information status as in the presence of informed consumers and joint ratings. Other things equal, the selfish seller switches to q_L and p_H faster when the buyer learns slower. Learning is slower in the presence of only uninformed buyers than in the presence of informed and uninformed buyers with joint ratings display. Learning is slower in the presence of informed and uninformed buyers with joint ratings display than in the separate display of ratings. Hence, there is least undertreatment and overcharging of consumers that require a high-cost treatment under separate display of ratings, followed by joint display of ratings when consumers with both informational statuses are present and so on. When the buyer requires the low-cost treatment both "behavioural types" of seller (Selfish and EL/IA) behave in the same way, namely provide q_L and p_H.

This far, we have focused on the case where $\alpha < 1, \beta < 1$. In the following, we will go through three possible cases, where all sellers and all buyers are selfish, all sellers are selfish but some buyers are cooperative, and all buyers are selfish but some sellers are cooperative.

Case 1: All sellers and buyers are selfish ($\alpha = 1, \beta = 1$)

The following holds for all conditions. If ratings are costless, the selfish buyer is indifferent between bad, good or no rating and randomises. Ratings convey no information and are therefore ignored and not used to update any beliefs. The buyers also ignore their previous experience with the (selfish) sellers due to backward induction: any selfish seller's best reply in any round is to maximise their own utility in that round (and randomise if indifferent). The future has no value to the seller because there are no informative ratings to certify good behaviour—no 'promise' of good behaviour is credible under backward induction. Since all selfish sellers behave in the same way, the buyer chooses a seller at random. Since private histories, as well as, ratings do not update the buyers' beliefs, there is no difference across treatments.

Case 2: All sellers are selfish but some buyers are cooperative ($\alpha = 1, \beta < 1$)

When all sellers are selfish and that is common knowledge, ratings do not convey any additional information.

Case 3: All buyers are selfish but some sellers are cooperative ($\alpha < 1$, $\beta = 1$)

Since buyers are selfish, they do not care about transmitting useful information about the sellers to others. Hence, ratings are uninformative, but there is updating conditional on private histories only.

Overall, we can conclude that for ratings to have any impact, it is necessary that at least some buyers and at least some sellers are cooperative.

2.3 Experimental Design

The basis for our stage game is borrowed from Dulleck et al. (2011). A seller observes the type of treatment a buyer needs, which can be low or high, each with probability 0.5. Providing the high treatment costs the seller 6 ECU, and providing the low treatment costs 2 ECU. The seller chooses between four price-treatment combinations, where the price is either low or high, and the treatment is either low or high. The buyer does not know which treatment she needs. When the buyer receives at least the treatment that she needs, her utility is 10 minus the price paid. When the buyer needs the high treatment but receives the low treatment, she is undertreated, and her utility is zero. As in Mimra et al. (2016), we fix prices at 4 for the low treatment and 8 for the high treatment. This ensures that prices do not affect the decision to defraud, as earnings from truth-telling are equal across the high and the low treatments (see Mimra et al. 2016).

We have 3 sellers and 6 buyers per market. Buyers are either uninformed or informed. In conditions *U-No Ratings* and *U-Ratings*, all 6 buyers are uninformed, in conditions *U-I-Pooled* and *U-I-Separate*, half of the buyers are uninformed, and half are informed. Sellers know this and also the information status of the buyer they are interacting with. Uninformed buyers do not know which treatment they need and which treatment they get unless they receive a negative payoff, from which they can infer that they were undertreated. Informed buyers initially do not know which treatment they need. However, after the interaction, i.e., after "consuming" the good, they learn about the treatment they needed, and the treatment they got. Hence, for uninformed buyers, the good is a credence good, while for informed buyers, the good is an experience good.

Sellers receive unique IDs that do not change throughout the experiment. Hence, sellers are always recognisable by buyers. In contrast, buyers do not have IDs and are thus not identifiable for sellers. Therefore, sellers can build up reputations, but buyers cannot. Sellers always know, however, whether they interact with an uninformed or an informed buyer.

The experiment consists of 16 rounds. In the first round, buyers are randomly and exogenously matched to sellers, such that each seller interacts with two buyers. In conditions *U-I-Pooled* and *U-I-Separate*, it is one informed buyer and one uninformed buyer per seller. In all conditions, sellers learn which treatment, high or low, each buyer who is matched with them needs. Then, they pick a price-treatment combination for each buyer. All buyers learn their payoffs. Informed buyers additionally receive feedback about which treatment they needed, which treatment they got, and are reminded of the price they paid. Uninformed buyers neither learn which treatment they needed, nor which treatment they received. Only in the case of a negative payoff can uninformed buyers infer that they got undertreated. After the feedback, the round ends in condition *U-No Ratings*, while in all other conditions, each buyer is given the opportunity to rate their seller. The seller can either leave a positive or a negative rating or refrain from rating. After that, sellers are given feedback about the rating(s) they received (in the context of the treatment needed and provided) and their final payoffs.

In all subsequent rounds (2 to 16), buyers select the seller, they wish to interact with based on their own experience with sellers in condition *U-No Ratings* and additionally on the distribution of accumulated ratings, which we show at the beginning of each round in conditions *U-Ratings*, *U-I-Pooled*, and *U-I-Separate*. For each seller, we display the number of positive ratings, the number of negative ratings, the number of unrated transactions, and the total number of transactions. In condition U-I-Separate, in addition to the distribution, that contains all ratings, participants see the distribution of ratings by the informed buyers only and the distribution of ratings by the uninformed buyers only. Thus, if informed buyers do rate in condition *U-I-Separate*, and if they do so truthfully, then all buyers will know, which sellers are honest at least toward informed buyers. We can compare the ratings by all buyers to the ratings by the informed buyers only and the former and the latter to the actual behaviour of sellers to infer which distribution of ratings is more informative.

We keep track of the personal experience of buyers with sellers to make sure that imperfect recall of buyers does not affect our results. Before choosing a seller, each buyer has access to the following information for all rounds so far: the ID of the sellers she interacted with, the price she paid, the rating she gave, and her payoff. Informed buyers also see which treatment they needed and which treatment they were given. So, in rounds 2 to 16, sellers compete for buyers, based on reputation through individual experience and ratings in *U-Ratings*, *U-I-Pooled*, *U-I-Separate*, and individual experience only in *U-No Ratings*.

In each round, a buyer can choose not to interact, and a seller may not get selected by any buyers. In this case, participants earn 1.6 ECU in this round.[16] If a seller is chosen by more than one buyer, the seller's payoff in a given round is the sum of the payoffs from all her interactions with buyers. At the beginning of the experiment, all participants receive an initial endowment

[16] With our parametrisation of the outside option we follow Dulleck et al. (2011) and Mimra et al. (2016).

of 40 ECU to cover possible losses in individual rounds. At the end of the experiment, the payoffs from all rounds are added up, converted in euros at an exchange rate of $1ECU = 0.14EUR$ and paid in cash together with a show-up fee of 5 EUR. In the experiment, all information is common knowledge to participants.

Table 2.5 summarises our conditions, the maximum number of recommendations, and the number of matching groups per condition.

Condition	*U-No Ratings*	*U-Ratings*	*U-I-Pooled*	*U-I-Separate*
Sellers	3	3	3	3
Buyers	6	6	6	6
Uninformed buyers	6	6	3	3
Informed buyers	0	0	3	3
Ratings	No	Yes	Yes joint display	Yes separate display
Number of matching groups	10	10	10	10
Maximum number of recommendations	960	960	960	960

Table 2.5: Experimental Conditions

The experiment had two pre-announced parts. Instructions for part two were distributed after part one was completed. In part one, participants engaged in the game above. Part two was designed to measure the social preferences of participants. We adapted the dictator game for triples. The dictator was required to pick one of ten possible allocations giving X ECU to the dictator and $11 - XECU$ to each of two other participants with X being an integer between one and ten. We first asked all participants to make a decision in the role of the dictator (strategy method see Selten 1965). After that, the software randomly assigned the role of the dictator to one third of participants. The allocations by all dictators were implemented and were relevant for the payoff of everyone in part two. The exchange rate was the same as in part one. After part two was completed, participants answered a short questionnaire on demographics, after which the experiment ended.

The experiment was programmed with zTree (Fischbacher 2007). Participants were recruited with ORSEE (Greiner 2015). The neutrally framed experimental instructions (see Appendix) were identical for all participants. After a short quiz to check the understanding of the instructions, and after clarifying all questions, the experiment started. Participants made their decisions anonymously and privately. They spend 75 minutes in the laboratory of Technische Universität Berlin and earned 19,80 EUR on average including the show-up fee of 5 EUR. The experiment was approved by the Ethics Board at the University of Liverpool. The following hypotheses in the next section ensue from our theoretical analysis under the latter assumption. First, we spell out our expectations in terms of sellers' behaviour towards buyers.

2.3.1 Hypotheses

In this section, we present our hypotheses that are based on our theoretical analysis. We first focus on the behaviour of sellers.

When comparing experimental conditions, we state that a condition induces more cooperation than another if and only if in any given round, in equilibrium, fewer sellers undertreat and overcharge a buyer who requires q_H of the condition than of the other condition. Hence, we can state the following hypothesis:

Hypothesis 1: *U-Ratings induces at least as much cooperation as No Ratings, U-I-Pooled induces at least as much cooperation as U-Ratings, U-I-Separate induces at least as much cooperation as U-I-Pooled.*

We can also state that:

Hypothesis 2: *In the last round, sellers undertreat and overcharge more.*

Next, we present our hypotheses with regard to buyers' rating behaviour.[17]

Hypothesis 3a: *An informed buyer that requires a high-cost treatment gives a negative rating following the interaction where the seller undertreated and overcharged them.*

Hypothesis 3b: *An informed buyer that requires a low-cost treatment provides no rating if the seller overcharged but provided adequate treatment.*

Hypothesis 3c: *An informed buyer that requires a high-cost treatment gives a positive rating to a seller who has committed no fraud.*

Hypothesis 4: *An uninformed buyer gives the seller a lower rating following undertreatment than following sufficient treatment.*

[17] In the third and fourth hypotheses, we have selected the focal equilibrium where ratings are positively associatively matched with buyers' payoff (i.e., higher payoffs translated into higher ratings).

It is important to note that our equilibrium analysis does not generate predictions off the equilibrium path. In particular, we cannot make a prediction of a buyer's rating in response to any of the following actions: when sellers charge the low price in any round; or sellers who provide high-cost treatment to buyers that require the low-cost treatment in any round.

2.4 Experimental Results

In this section, we present our experimental results. We first focus on seller behaviour, especially the level of undertreatment and level of overcharging[18]. This is followed by the behaviour of buyers.

2.4.1 Undertreatment and Overcharging

Recall that undertreatment occurs when a buyer receives the low-cost treatment, although she needs the high-cost treatment. Overcharging occurs when the low-cost treatment is provided at the high price.

We define the rate of undertreatment as the number of decisions to undertreat divided by the total number of decisions by sellers. Similarly, the rate of overcharging is defined as the number of overcharging decisions divided by the total number of decisions by sellers.[19] The number of decisions by buyers not to interact is low: *U-Ratings* = 60, *U-I-Pooled* = 36, *U-I-Separate* = 59, and *U-No Ratings* = 70 out of 960 maximum possible number of interactions per experimental condition. A large number occurs in the last period of interaction: *U-Ratings* = 27, *U-I-Pooled* = 18, *U-I-Separate* = 17, and *U-No Ratings* = 22 out of maximum 90 possible number of interactions per experimental condition in the last round.

Figures 2.2 and 2.3 below display the levels of undertreatment and overcharging by experimental conditions and buyer status. In all conditions, the level of undertreatment is very low, with fractions being under 5% for the *U-No Ratings*, *U-Ratings* and *U-I-Pooled* conditions and around 5% for the *U-I-Separate* condition.

The fractions of overcharging, however, are much higher. Overcharging is highest in the *U-No Ratings* condition when there are no ratings with a share of above 30% whereas the share is around 20% in the *U-Ratings* and *U-I-Pooled* conditions and 23% in the *U-I-Separate* condition. The fractions of un-

[18] As stated earlier, overtreatment is irrelevant in our setting, the overall fraction of overtreatment is 1.2%

[19] We hence condition on the decision of buyers to interact with sellers.

dertreatment in the *U-I-Pooled* and *U-I-Separate* are similar for informed and uninformed buyers when broken down by buyer status. A different picture emerges for overcharging. The fraction of overcharging differs by information status, as the fraction of overcharged informed buyers is lower than uninformed buyers in both the *U-I-Pooled* and *U-I-Separate* conditions. [20]

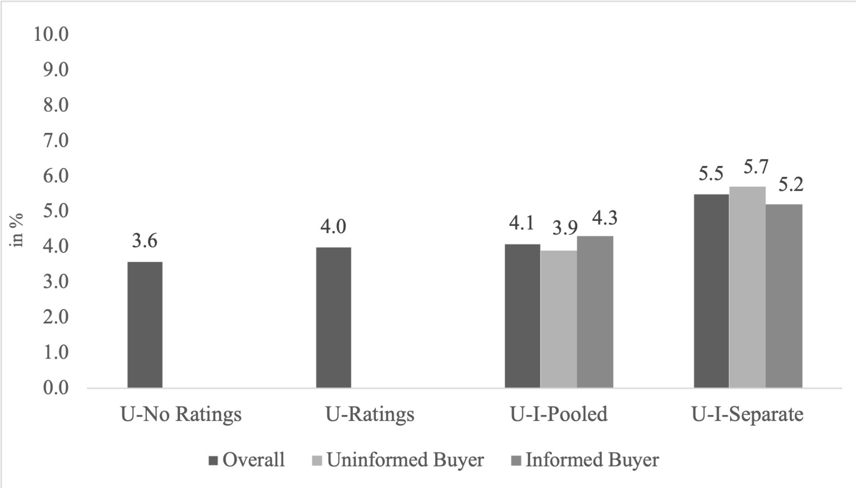


Figure 2.2: Fraction undertreatment by condition and buyer status in %.

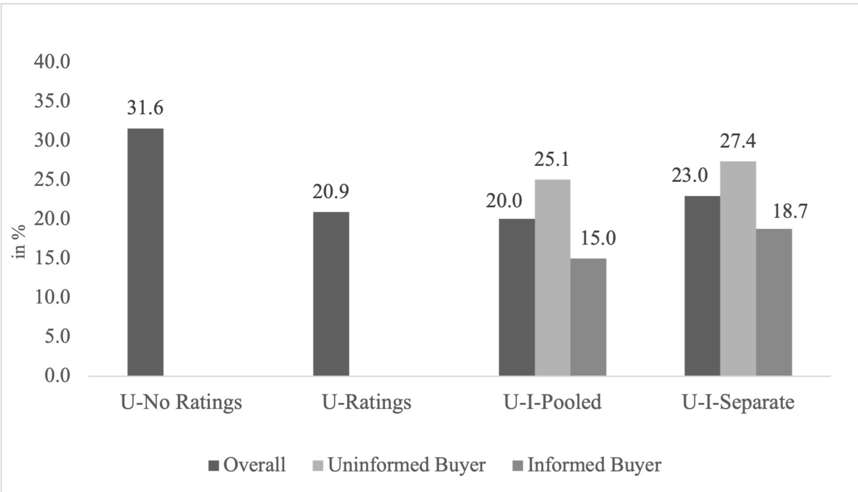


Figure 2.3: Fraction overcharging by condition and buyer status in %.

How the shares of undertreatment and overcharging evolve over time are displayed in Figures 2.4 and 2.5. Most of the time, the share of undertreatment fluctuates around 5% for all conditions. But there is a spike in the last round which is indicative of last-round effects. In terms of overcharging, the share of overcharging is higher in the *U-No Ratings* condition than in the

[20]It must be noted that overall levels of undertreatment and overcharging are quite low, especially when compared to past work of Dulleck et al. (2011) and Mimra et al. (2016). This will be discussed later.

other conditions where ratings are available. In all treatments overcharging increases towards the final rounds of the experiment.

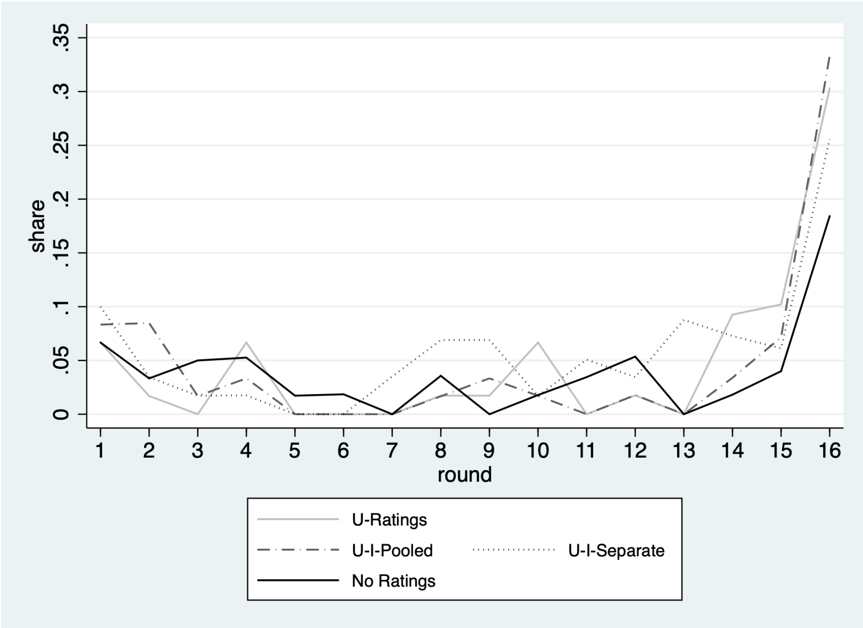


Figure 2.4: Fraction of undertreatment by condition over time.

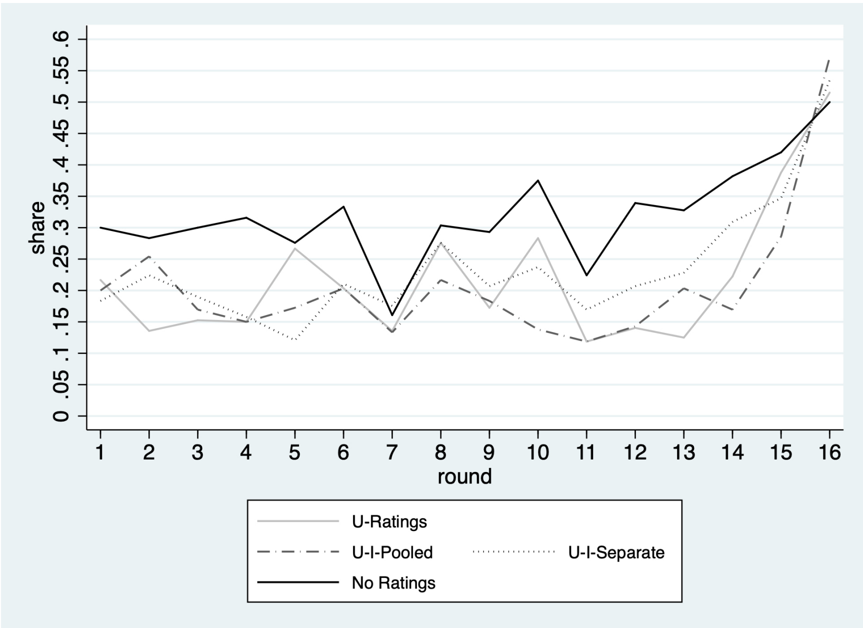


Figure 2.5: Fraction of overcharging by condition over time.

Next, we report results from random effect panel probit regressions as in Dulleck et al. (2011) and Mimra et al. (2016).[21] We scrutinise the impact of ratings in the presence of informed and uninformed buyers and the format of the rating system on undertreatment and overcharging. In our regressions, undertreatment and overcharging are binary variables taking on the value of

[21]Robustness checks can be found in the Appendix.

1 if a consumer was undertreated or overcharged and 0 otherwise. We control for the round an action takes place, the experimental condition and introduce a last round dummy in specification (1). In specification (2) we include the interaction between a buyer's status and the condition. In the third regression which is a random effect probit regression, we include the same explanatory variables as in (2), except for round and the last round dummy as we solely focus on the last round. The regression results for undertreatment are summarised in Table 2.6. Furthermore, post-estimation Wald test results are summarised in table 2.7.

	Dependent Variable: Undertreatment		
	(1)	(2)	(3)
U-Ratings	0.0586	0.0586	0.0380
	(0.1280)	(0.1281)	(0.2983)
U-I-Pooled	0.0319	0.0185	-0.1257
	(0.1280)	(0.1563)	(0.3467)
U-I-Separate	0.1988	0.2138	0.2759
	(0.1230)	(0.1474)	(0.3340)
Informed x U-I-Pooled		0.0265	0.5985
		(0.1776)	(0.3956)
Informed x U-I-Separate		-0.0299	-0.4016
		(0.1631)	(0.3863)
Round	0.0151	0.0015	
	(0.0097)	(0.0097)	
Last Round Dummy	1.2718***	1.2172***	
	(0.1442)	(0.1442)	
Constant	-1.9967***	-1.9970***	-0.0001
	(0.1262)	(0.1263)	(0.2033)
Observations	3615	3615	156
*, ** and *** denote significance at the 10 %, 5 % and 1 % level, respectively.			

Table 2.6: Random-effects panel probit regressions (columns (1) and (2)) and probit regression (column (3)) on undertreatment.

We find that undertreatment is not significantly different across experimental conditions or for buyers with different information levels. As expected, undertreatment is significantly higher in the last period. But there are no significant differences across treatments in the last round.

Next, we will focus on overcharging. Following Table 2.8 displays the regression results for overcharging.

Furthermore, Table 2.9 presents the post-estimation Wald test results for the regressions on overcharging.

We find that overcharging is significantly lower in all experimental conditions when ratings are available. Furthermore, informed buyers are signific-

(1)	(2)	(3)
	Ho: U-Ratings = U-I-Pooled + U-I-Pooled x Informed (0.93)	Ho: U-Ratings = U-I-Pooled + U-I-Pooled x Informed (0.41)
Ho: U-I-Pooled = U-Ratings (p = 0.83)	Ho: U-I-Pooled + U-I-Pooled x Informed = U-I-Separate + U-I-Separate x Informed (0.41)	Ho: U-I-Pooled + U-I-Pooled x Informed = U-I-Separate + U-I-Separate x Informed (0.27)
Ho: U-I-Pooled = U-I-Separate (p = 0.17)	Ho: U-I-Pooled = U-I-Pooled x Informed (0.97)	Ho: U-I-Pooled = U-I-Pooled x Informed (0.76)
	Ho: U-I-Separate = U-I-Separate x Informed (0.37)	Ho: U-I-Separate = U-I-Separate x Informed (0.67)
	Ho: U-I-Pooled = U-I-Separate (0.25)	Ho: U-I-Pooled = U-I-Separate (0.94)
*, ** and *** denote significance at the 10 %, 5 % and 1 % level, respectively.		

Table 2.7: Post-estimation tests for regressions on undertreatment.

	Dependent Variable: Overcharging		
	(1)	(2)	(3)
U-Ratings	-0.3518***	-0.350***	0.0380
	(0.0985)	(0.0951)	(0.2983)
U-I-Pooled	-0.4339***	-0.2390**	-0.1257
	(0.0996)	(0.1164)	(0.3467)
U-I-Separate	-0.2957***	-0.1367	0.2759
	(0.0982)	(0.1156)	(0.3340)
Informed x U-I-Pooled		-0.4030**	0.5985
		(0.1404)	(0.3956)
Informed x U-I-Separate		-0.3236**	-0.4016
		(0.1372)	(0.3863)
Round	0.0157***	0.0144***	
	(0.0057)	(0.0055)	
Last Round Dummy	0.7870***	0.7352***	
	(0.1156)	(0.1123)	
Constant	-0.6745***	-0.6724***	-0.0001
	(0.0821)	(0.0810)	(0.2033)
Observations	3615	3615	156
*, ** and *** denote significance at the 10 %, 5 % and 1 % level, respectively.			

Table 2.8: Random-effects panel probit regressions (columns (1) and (2)) and probit regression (column (3)) on overcharging.

antly less overcharged than uninformed buyers in *U-I-Pooled* when ratings of buyers with both information status are displayed jointly and in *U-I-Separate* when they are displayed separately. Overcharging increases over time and is significantly higher in the last round. But there are no significant differences across treatments in the last round. Our post-estimation Wald tests reveal

(1)	(2)	(3)
	Ho: U-Ratings = U-I-Pooled + U-I-Pooled x Informed (0.02)**	Ho: U-Ratings = U-I-Pooled + U-I-Pooled x Informed (0.22)
Ho: U-I-Pooled = U-Ratings (p = 0.42)	Ho: U-I-Pooled + U-I-Pooled x Informed = U-I-Separate + U-I-Separate x Informed (0.2)	Ho: U-I-Pooled + U-I-Pooled x Informed = U-I-Separate + U-I-Separate x Informed (0.13)
Ho: U-I-Pooled = U-I-Separate (p = 0.17)	Ho: U-I-Pooled = U-I-Pooled x Informed (0.47)	Ho: U-I-Pooled = U-I-Pooled x Informed (0.27)
	Ho: U-I-Separate = U-I-Separate x Informed (0.4)	Ho: U-I-Separate = U-I-Separate x Informed (0.28)
	Ho: U-I-Pooled = U-I-Separate (0.45)	Ho: U-I-Pooled = U-I-Separate (0.30)

*, ** and *** denote significance at the 10 %, 5 % and 1 % level, respectively.

Table 2.9: Post-estimation tests for regressions on overcharging.

that overcharging is significantly lower in the *U-I-Pooled* condition than in *U-Ratings*.

These findings are summarised in **Result 1a**, **Result 1b** and **Result 2**.

Result 1a: *Undertreatment is not significantly different across experimental conditions and buyers' information status.*

Result 1b: *Overcharging is significantly lower when ratings are available and informed buyers are present on the market. Furthermore, informed buyers are overcharged less than uninformed buyers. The separate display of ratings by information status does not reduce overcharging when compared to the joint display of ratings.*

Result 2: *In the last round, there is more undertreatment and overcharging.*

2.4.2 Rating Behaviour

Next, we will focus on rating behaviour of buyers. We will first investigate whether buyers have utilised ratings. Table 2.10 presents the fraction of buyers that decided to provide a rating by buyer status and experimental condition. Overall. the fractions of rating utilisation is similar across experimental conditions. In all experimental conditions, over 75% of buyers decided to provide a rating with the share exceeding 80% for informed buyers. This is not surprising, in the case of informed buyers as they can perfectly communicate how they have been treated through ratings. The rating rate is quite high for uninformed buyers even though their inference is more limited.

Rate	U-Ratings	U-I-Pooled		U-I-Separate	
Buyer Status	Uninformed	Uninformed	Informed	Uninformed	Informed
Yes	80%	76%	81%	76%	86%
No	20%	24%	19%	24%	14%

Table 2.10: Fraction of decision-to-rate by buyer status and experimental condition.

Next, we will focus on ratings provided by buyers to better understand their rating patterns. Following figures 2.6 summarise the overall rating distributions by experimental condition and buyer status.

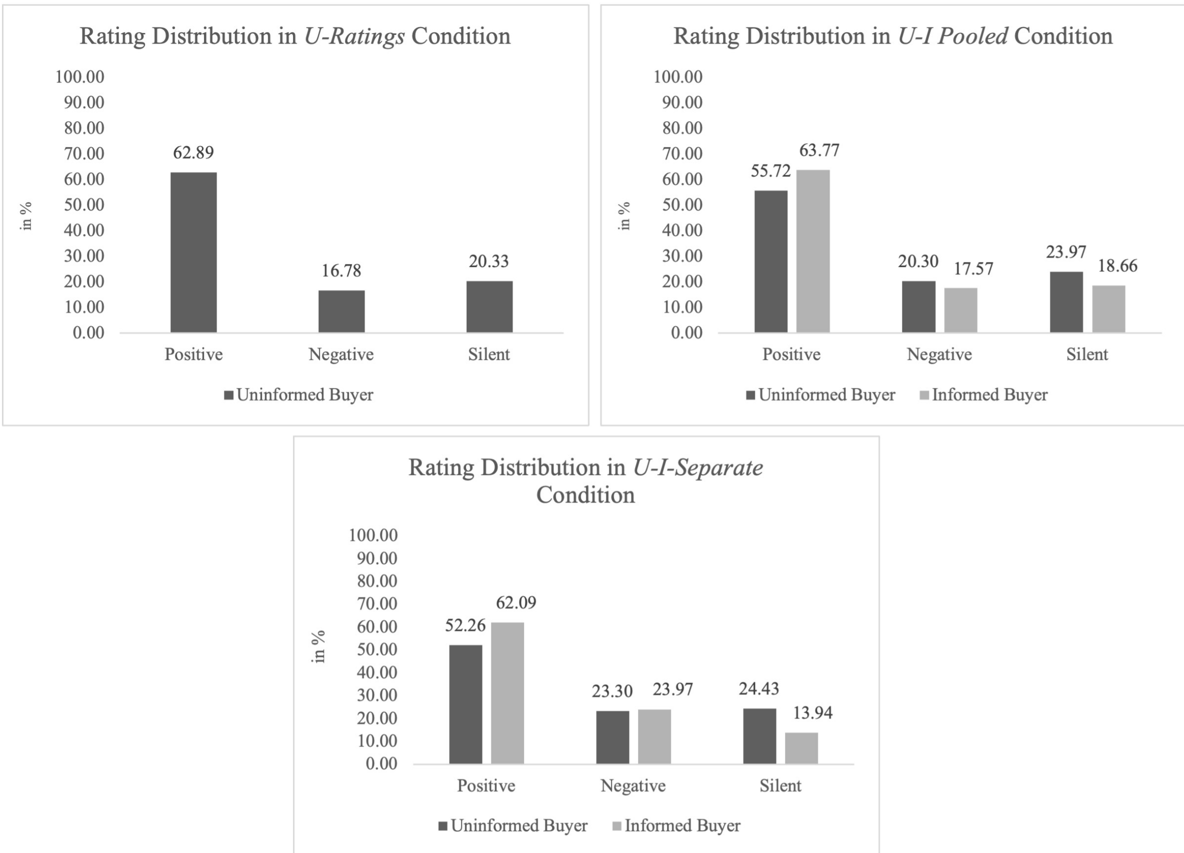


Figure 2.6: Rating distribution in experimental conditions and by information status.

The rating pattern that emerges in all experimental conditions is very similar with a very high fraction of positive ratings. This is not surprising, as the level of overall fraud in our experiment is not very high, especially when compared to previous research findings by Dulleck et al. (2011) and Mimra et al. (2016).

Table 2.11 presents the fraction of ratings and decision-to-rate by payoff for buyers.

Status	Rating	Payoff		
		<0	2	6
Uninformed	Positive	4 out of 74 (5%)	342 out of 607 (56%)	709 out of 722 (98%)
	Negative	70 out 74 (95%)	265 out of 607 (44%)	13 out of 722 (2%)
	Fraction of Raters	74 out of 79 (94%)	607 out of 932 (65%)	722 out of 780 (93%)
Informed	Positive	1 out of 44 (2%)	178 out of 305 (58%)	400 out of 424 (94%)
	Negative	40 out of 41 (98%)	127 out of 305 (42%)	24 out of 242 (6%)
	Fraction of Raters	41 out of 44 (93%)	305 out 413 (74%)	424 out of 463 (92%)

Table 2.11: Fraction of ratings and decision-to-rate by payoff for buyers.

Unsurprisingly, the fraction of consumers that decided to provide a rating in the case of a negative payoff is very high (>92%). In this case, there is no ambiguity about the sellers' decision and buyers can perfectly infer how they have been treated. For the case of a buyer receiving a payoff of 6 ECU, the fraction of ratings is also very high (>90%). A buyer that requires a low-cost treatment that is treated sufficiently and charged adequately can infer non-fraudulent behaviour from a payoff of 6 ECU. [22] Recall that in our behavioural predictions we do not have a behavioural type of seller that provides the low-cost treatment and charges the price of the low-cost treatment, hence in equilibrium buyers never receive a payoff of 6 ECU.

The case of a buyer receiving a payoff of 2 ECU is interesting, as there exists ambiguity for the uninformed buyer. In this case, an uninformed buyer could have been treated and charged adequately if they required a high-cost treatment or they might have been overcharged if they required a low-cost treatment. Even in this ambiguous case, the rating rate for uninformed buyers is quite high (>64%). For the informed buyers there is no ambiguity as they can always infer what treatment they required, how they have been treated and what price they have been charged. The fraction of informed buyers that received a payoff of 2 ECU and decided to rate is 74%. Next, we will focus on the rating pattern.

When buyers receive a negative payoff, they provide a negative rating almost all the time (>94%) irrespective of information status. For a payoff

[22] Hypothetically, it is also possible that a seller provides a high-cost treatment to a high-cost treatment requiring buyer and charges the price for the low-cost treatment but this would induce a loss to the seller.

of 6 ECU, a similar pattern holds, but where a positive rating is provided almost all the time (>93%). This means, honest behaviour is rewarded and undertreatment is punished which is in line with our theoretical analysis in the previous section.

The behaviour of a very small fraction of buyers is not in line with our predictions, e.g. providing a positive rating when the payoff is negative or a negative rating when the payoff is 6 ECU. This might be simply an error or due to a lack of attention. Buyers interpret positive ratings as a good signal, and negative ratings as a bad signal. This is a common finding in the ratings literature (see Lafky 2014, Nosko & Tadelis 2015)

Let us next focus on the ambiguous case of a buyer receiving a payoff of 2 ECU. The following Table 2.12 illustrates the rating distribution for buyers when they receive a payoff of 2 ECU and they have been overcharged or faced no fraud.

	Rating		
Informed	Positive	Negative	No Rating
Overcharging	29%	45%	26%
No fraud	49%	24%	27%
Uninformed	Positive	Negative	No Rating
Overcharging	36%	31%	33%
No fraud	36%	27%	37%

Table 2.12: Rating distribution for a payoff of 2 ECU.

Informed buyers can infer whether they have been treated non-fraudulently or overcharged from a payoff of 2 ECU. If a low-cost treatment is required and the resulting payoff is a 2 ECU, then informed buyers know they have been overcharged. 45% out of those buyers provided a negative rating, 29% a positive rating and 26% decided to remain silent. In contrast, when informed buyers are treated non-fraudulently, the share of positive ratings is 49%, whereas 24% provided a negative rating and 27% did not provide a rating. Informed buyers' ratings do not communicate information perfectly as non-fraudulent behaviour is punished with a negative rating or overcharging is rewarded with a positive rating by a substantial fraction.

For uninformed buyers, as already discussed, there exists ambiguity when the resulting payoff is 2 ECU. 36% of buyers who received a payoff of 2 ECU and required a low-cost treatment provided a positive rating, 31% provided a negative rating and 33% decided to remain silent. When a high-cost treatment was required 36% provided a positive rating, 27% a negative rating and

37% remained silent. The ratings are more evenly distributed for uninformed buyers. Positive ratings and the fraction of no ratings together exceed two-thirds of the overall ratings provided in this ambiguous case. This finding is very similar to Bolton et al. (2019) who report lenient rating behaviour under uncertainty leading to rating inflation and displaying a hesitancy to provide negative feedback.

Following Table 2.13 presents random effects probit regressions on the decision-to-rate (1) and an ordered panel probit regression on the rating provided (2). We use a buyer's payoff, the information status, and the interaction of a payoff 2 dummy as independent variables.

Variables	(1)	(2)
Payoff	0.0944***	0.1667***
	(0.0139)	(0.0078)
Uninformed Dummy	-0.0540	-0.2431*
	(0.1910)	(0.1378)
U-I-Separate	0.1468	-0.0993
	(0.2064)	(0.1538)
U-I-Pooled	0.2539	-0.1042
	(0.2095)	(0.1541)
Constant	1.6349***	
	(0.2342)	
Number of Observations	2880	2880
Number of Participants	180	180
*, ** and *** denote significance at the 10 %, 5 % and 1 % level, respectively.		

Table 2.13: Panel probit regressions with random effects on decision-to-rate (1) and ordered panel probit regression on provided rating (2).

The payoff positively affects the decision to rate and also the rating. When having a closer look at the determinants of ratings a buyer's payoff positively affects ratings. Furthermore, being uninformed reduces the rating provided.

We can summarise our findings for buyers' rating behaviour in Result 3 and Result 4.

Result 3: *Informed buyers punish undertreatment with negative ratings whereas non-fraudulent behaviour is rewarded with positive ratings. However, communication through ratings is not perfect as honest behaviour is punished with a negative rating or overcharging is rewarded with a positive rating by a substantial fraction. The payoff a buyer receives affects the decision to rate and the rating positively.*

Result 4: *Uninformed buyers rate as much as informed buyers. They also punish undertreatment with negative ratings and reward non-fraudulent behaviour with* *positive ratings. When there is ambiguity about a seller's behaviour, the share of positive and no ratings together exceeds two-thirds of the overall ratings provided.*

2.4.3 Determinants of a seller being chosen

Does it pay off for a seller to build a good reputation? The final values of the cumulative number of non-fraudulent interactions and total payoff, after 16 periods reveal a significantly positive relationship $(r(88) = 0.58, p < 0.01)$ [23]. Non-fraudulent sellers earn a higher total payoff. This indicates that ratings are working and are inducing more non-fraudulent behaviour.

After having established that honest behaviour and total payoffs are positively correlated, we will turn our focus next on the determinants of a seller being chosen. Table 2.14 shows the results of panel linear regressions with random effects on the frequency of a seller being chosen in a round based on information presented to the buyers such as the number of positive ratings, negative ratings and silent ratings. As anticipated, in specification (1), the number of positive ratings positively affects the frequency of a seller being chosen and the number of negative ratings has a negative effect.

In specification (2) we focus on the frequency of being chosen by a buyer in a round in the *U-I-Separate* condition. In this condition, ratings provided by informed and uninformed buyers are displayed separately. The overall number of positive ratings has a positive effect on the frequency of being chosen whereas the number of positive ratings provided by informed buyers has no significant effect. The number of negative ratings, silent ratings and number of negative ratings provided by informed buyers have a negative effect on the frequency of being chosen. The number of silent ratings provided by informed buyers has a positive effect on the frequency of being chosen.

When choosing a seller, consumers seem to focus on positive and negative with each affecting the likelihood of a seller being chosen in expected ways: the number of positive ratings positively affects the likelihood of being chosen and negative ratings have a negative effect on the likelihood of a seller being chosen. When ratings are separated by buyers' information status, the joint rating distribution significantly affects the likelihood of a seller being chosen. The number of negative ratings and silent ratings provided by informed buyers significantly affect the likelihood of a seller being chosen. Interestingly the number of positive ratings provided by buyers has no significant effect, there is an asymmetric effect here.

[23]For a scatterplot see figure 2.7 in the Appendix

Variables	(1)	(2)
Number of positive Ratings	0.0772*** (0.0044)	0.0641*** (0.0131)
Number of negative Ratings	-0.1392*** (0.0137)	-0.0812*** (0.0260)
Number of silent consumers	-0.0050 (0.0110)	-0.0624** (0.0281)
Number of positive ratings provided by informed buyers		0.0283 (0.0212)
Number of negative ratings provided by informed buyers		-0.0815** (0.0411)
Number of silent ratings provided by informed buyers		0.1577*** (0.0503)
Constant	1.5892*** (0.0547)	1.5688*** (0.0673)
Number of Observations	1440	480
Number of Participants	90	30
*, ** and *** denote significance at the 10 %, 5 % and 1 % level, respectively.		

Table 2.14: Panel linear regressions with random effects on frequency of being chosen.

<u>**Result 5:**</u> *The effect of ratings on the frequency a seller being chosen are as expected: Positive ratings positively affect the frequency of being chosen whereas the number of negative ratings has a negative effect. The number of negative ratings and silent ratings provided by informed buyers significantly affect the likelihood of a seller being chosen.*

2.4.4 Social Preferences

Finally, we investigate the results of the dictator game for triples to see how non-fraudulent behaviour is associated with the social preference type in the rating treatments. In our theory section, we have outlined the importance of social preferences in order for ratings to be of any relevance. Our results presented in Table 2.15 reveal, almost half (48%) of the sellers and more than half (60%) of the buyers chose the most self-favouring split.

43% of sellers and 34% of buyers tried to implement a nearly equal split. 9% of sellers' and 6% of buyers' choices were indicative of efficiency-loving preferences. The table also displays the mean number of non-fraudulent interactions provided by sellers for each social preference type. The behaviour is as expected, selfish sellers are less truthful when compared to the cooper-

Social Preference type	Sellers	mean number of truthful action (std. dev. in brackets)	Buyers
Selfish (S)	48%	16.81 (12.72)	60%
Inequality-averse (IA)	43%	20.49 (11,94)	34%
Efficiency-loving (EL)	9%	23.38 (15.20)	6%
MWU Test	**S vs. IA**** **(p = 0.046)**	**S vs. EL*** **(p = 0.06)**	IA vs. EL (p = 0.63)

*, **. *** denote significance at 10%, 5% and 1% level, respectively.

Table 2.15: Fractions of social preference types and sellers' corresponding truth-telling behaviour.

ative sellers (IA or EL).

2.5 Conclusions

In this paper, we have theoretically and experimentally investigated whether voluntary online ratings by consumers can incentivise sellers to provide credence goods of appropriate quality and price and reduce overcharging and undertreatment. To allow for ratings to be informative, we explored the possibility that some consumers are better informed than others, which may partly alleviate the information asymmetry between sellers and consumers and enable consumers to (better) judge the good or service received.

We further investigated the effectiveness of the rating system depending on the presence of better-informed consumers and whether the design of the rating system affects the decisions of buyers and sellers. Thereby, we varied how ratings are displayed, jointly or separated by the information status of consumers. We developed a theoretical model to derive behavioural predictions on seller and buyer behaviour and conducted a controlled laboratory experiment to test them.

Our theory and experiment are based on Dulleck et al. (2011) and Mimra et al. (2016). Our main experiment is followed by a dictator game for triples to elicit the social preference type of our participants.

We find that, indeed, ratings significantly reduce overcharging of buyers. Better-informed buyers are overcharged less and having better-informed buyers on the market also significantly reduce overcharging. Separating the ratings by information status of buyers and displaying them separately does not significantly reduce overcharging when compared to the pooled display.

Fractions of undertreatment are similar across experimental conditions. This type of fraud can be detected by all buyers irrespective of information level and rating format. Interestingly, we find much lower levels of undertreatment and overcharging than in previous studies. Mimra et al. (2016) report a fraction of undertreatment of above 30% and overcharging rates above 70% in an experimental condition comparable to our baseline *(U-No Ratings)* condition. We find undertreatment levels around or under between 4% to 6%. In our experimental condition without ratings, the fraction of overcharging is >30%. When ratings are available the share of overcharging is around 20%. Sellers treat buyers differently depending on information status. The fraction of overcharging for informed buyers with <20% is lower than for uninformed buyers which is >25%. Dulleck et al. (2011) report even higher undertreatment and overcharging rates. Their results are not directly comparable as they employed variable prices rather than fixed prices in their experiments. One potential explanation for these differences might be differences in social preferences. Our dictator game with triples revealed that more than half of sellers ($\approx$ 52%) are driven by non-selfish preferences. We have also see that selfish sellers are less truthful when compared to the cooperative sellers (IA or EL). It might be the case, that the fractions of selfish sellers were higher in Mimra et al. (2016) or Dulleck et al. (2011) resulting in much higher fractions of overcharging and undertreatment. Since the authors did not measure social preferences in their papers, this cannot be verified.

In terms of rating behaviour, we find that uninformed buyers rate as much as informed buyers. Buyers of both information statuses punish undertreatment with negative ratings whereas honest behaviour is rewarded with positive ratings which is a common finding in the rating literature (see Lafky 2014, Nosko & Tadelis 2015). However, communication through ratings by informed buyers is not perfect, especially when the focal interpretation of ratings is considered where ratings are positively associatively matched with buyers' payoff (i.e., higher payoffs translated into higher ratings).

Non-fraudulent behaviour is punished with a negative rating or overcharging is rewarded with a positive rating by a substantial fraction of informed buyers. We also found the payoff a buyer receives affects the decision to rate and the rating positively.

A subject for further scrutiny in future could be to incentivise informed buyers to pay more attention when providing ratings in order to improve communication through ratings. It would be interesting to see how this would compare to our findings, especially when the ratings are displayed separately by buyers' information status.

Another interesting observation in terms of rating behaviour is that when uninformed buyers face ambiguity about a seller's behaviour, the share of positive and no ratings together exceeds two-thirds of the ratings provided overall. This finding is indicative of lenient rating behaviour. Bolton et al.

(2019) experimentally investigate the endogenous provision of ratings under attributional uncertainty and obtain a similar finding as ours, where buyers either provide a positive rating or refrain from providing feedback. These findings indicate that there is hesitancy to provide negative feedback. Research on negative rating hesitancy is scarce, hence future research can investigate the determinants of negative rating hesitancy to better understand consumers' rating patterns.

The frequency of a seller being chosen is positively affected by positive ratings and negatively affected by negative ratings, hence building a positive reputation pays off. Furthermore, when ratings are separated by buyer status, the number of negative ratings and the number of no ratings provided by informed buyers significantly affect the likelihood of a seller being chosen.

Broadly, our study contributes to the literature on credence goods by investigating consumer-provided online ratings as a tool to undermine fraudulent behaviour. Furthermore, we add to the literature on ratings, especially when ratings are provided under uncertainty. In fact, even though imperfect, we show that ratings can be a fruitful tool to reduce fraudulent behaviour in credence goods markets.

In our work, we focused on numerical ratings but on rating platforms for credence goods, one can usually provide written textual feedback, that might contain valuable information. A future avenue of research would be to see how textual feedback affects fraud in credence goods markets.

A very common assumption made in the credence goods literature is that a seller learns the type of a buyer for certain and can perfectly determine which service or good is needed. However, in reality, this might not always be the case. In a medical context, doctors might not always be certain about what treatment to apply to a patient or also be uncertain about the success of the treatment applied, especially when the underlying health condition of the patient is complicated or experts differ in their level of expertise. Future work in this area could introduce uncertainty on the seller side where a seller cannot determine what service a consumer needs with certainty. Moreover, heterogeneity in terms of sellers' expertise can be introduced to make the model of credence goods more realistic.

Overall, we can conclude that online ratings are only one promising instrument to reduce fraud in credence goods markets. Reducing fraudulent behaviour and improving outcomes for consumers by investigating other tools and mechanisms will remain a significant topic for future research.

2.6 Appendix

Derivation of $\alpha_{bst}\left(H_{bs}^{t}\right)$

$$\alpha_{bst}\left(H_{bs}^{t}\right) = \tag{2.6}$$

$$\frac{\Pr\left[H_{bst}|s\in\bar{S},H_{bs}^{t-1}\right]\Pr\left[s\in\bar{S},H_{bs}^{t-1}\right]}{\Pr\left[H_{bst}|s\in\bar{S},H_{bs}^{t-1}\right]\Pr\left[s\in\bar{S},H_{bs}^{t-1}\right]+\Pr\left[H_{bst}|s\notin\bar{S},H_{bs}^{t-1}\right]\Pr\left[s\notin\bar{S},H_{bs}^{t-1}\right]} = \tag{2.7}$$

$$\frac{\Pr\left[H_{bst}|s\in\bar{S},H_{bs}^{t-1}\right]\Pr\left[s\in\bar{S}|H_{bs}^{t-1}\right]\Pr\left[H_{bs}^{t-1}\right]}{\Pr\left[H_{bst}|s\in\bar{S},H_{bs}^{t-1}\right]\Pr\left[s\in\bar{S}|H_{bs}^{t-1}\right]\Pr\left[H_{bs}^{t-1}\right]+\Pr\left[H_{bst}|s\notin\bar{S},H_{bs}^{t-1}\right]\Pr\left[s\notin\bar{S}|H_{bs}^{t-1}\right]\Pr\left[H_{bs}^{t-1}\right]} \tag{2.8}$$

$$= \frac{\Pr\left[H_{bst}|s\in\bar{S},H_{bs}^{t-1}\right]\Pr\left[s\in\bar{S}|H_{bs}^{t-1}\right]}{\Pr\left[H_{bst}|s\in\bar{S},H_{bs}^{t-1}\right]\Pr\left[s\in\bar{S}|H_{bs}^{t-1}\right]+\Pr\left[H_{bst}|s\notin\bar{S},H_{bs}^{t-1}\right]\Pr\left[s\notin\bar{S}|H_{bs}^{t-1}\right]} \tag{2.9}$$

$$= \frac{\Pr\left[H_{bst}|s\in\bar{S},H_{bs}^{t-1}\right]\alpha_{bst-1}\left(H_{bs}^{t-1}\right)}{\Pr\left[H_{bst}|s\in\bar{S},H_{bs}^{t-1}\right]\alpha_{bst-1}\left(H_{bs}^{t-1}\right)+\Pr\left[H_{bst}|s\notin\bar{S},H_{bs}^{t-1}\right]\left(1-\alpha_{bst-1}\left(H_{bs}^{t-1}\right)\right)} \tag{2.10}$$

$$= \frac{\Pr\left[H_{bst}|s\in\bar{S}\right]\alpha_{bst-1}\left(H_{bs}^{t-1}\right)}{\Pr\left[H_{bst}|s\in\bar{S}\right]\alpha_{bst-1}\left(H_{bs}^{t-1}\right)+\Pr\left[H_{bst}|s\notin\bar{S}\right]\left(1-\alpha_{bst-1}\left(H_{bs}^{t-1}\right)\right)} \tag{2.11}$$

Proof of Proposition 1

To keep the notation tractable we will denote the cooperative action (when a high-cost treatment requiring buyer receives (q_H, p_H)) with c and the non-cooperative action with d (where a high-cost treatment requiring buyer receives (q_L, p_H)). In expectation conditional on the observed history and ignoring ties we obtain

$$V^*_{st}(B_{st}) = \max_{a_{st}} \left\{ U_{st}(a_{st} | B_{st}) + \mathbb{E}\left[V^*_{st+1}(B_{st+1}) \middle| H^{t-1}, \sigma^*_{-st}, a_{st}, B_{st} \right] \right\} \tag{2.12}$$

Consider the second term in brackets. Expanding this term, we obtain

$$\mathbb{E}\left[V^*_{st+1}(B_{st+1}) \middle| H^{t-1}, a_{st}, B_{st}, \sigma^*_{-st} \right] \tag{2.13}$$

$$= \sum_{B_{st+1} \subseteq B} V^*_{st+1}(B_{st+1}) \prod_{b \in B_{st+1}} \Pr\left[s \in \arg\min_{\tilde{s} \in S} \alpha_{b\tilde{s}t}\left(H^{t-1}, a_{st}, \sigma^*_{-st} \right) \right] \tag{2.14}$$

since the buyers' beliefs and types are independent by construction. Ties are ignored here for brevity of exposition. Accounting for ties does not change the proof.

Recall that

$$\alpha_{bst}\left(H^t_{bs}\right) = \frac{\Pr\left[H_{bst} | s \in \bar{S}\right] \alpha_{bst-1}\left(H^{t-1}_{bs}\right)}{\Pr\left[H_{bst} | s \in \bar{S}\right] \alpha_{bst-1}\left(H^{t-1}_{bs}\right) + \Pr\left[H_{bst} | s \notin \bar{S}\right]\left(1 - \alpha_{bst-1}\left(H^{t-1}_{bs}\right)\right)}, \tag{2.15}$$

which implies that $\alpha_{bst}\left(H^t_{bs}\right)$ increases in $\Pr\left[H_{bst} | s \in \bar{S}\right]$ and decreases in $\Pr\left[H_{bst} | s \notin \bar{S}\right]$.

Holding $\alpha_{bst-1}\left(H^{t-1}_{bs}\right)$ constant (e.g., in the first period where the history is empty) it is more likely to see (q_H, p_H) from a cooperative seller than a selfish seller, because the cooperative seller chooses this action in every round (by Lemma 1): $\Pr\left[c_{bst} | s \in \bar{S}\right] \leq \Pr\left[c_{bst} | s \notin \bar{S}\right] = 1$. (NB $\Pr\left[c_{bsT} | s \in \bar{S}\right] = 0$).

Suppose in equilibrium the selfish seller $s \in S$ starts with the cooperative action in the first period. Then the Bayesian update leads to the same belief α_{bst} about seller s as seller $s' \notin S$, and consequently equal to the prior probability: $\alpha_{bst} = \alpha_{bs't} = \alpha$. This is true also in every subsequent period where the selfish seller chooses (q_H, p_H) for buyer b. It is also irrational for the seller to sacrifice any payoff for a buyer who already believes that they are selfish with probability 1.

After buyer b observes a signal that seller s took a non-cooperative action (in interaction between b and s themselves or through a credible rating), the updated belief is

$$\alpha_{bst}\left(H_{bs}^{t}\right)=\frac{\Pr\left[H_{bst}|s\in\bar{S}\right]\alpha_{bst-1}\left(H_{bs}^{t-1}\right)}{\Pr\left[H_{bst}|s\in\bar{S}\right]\alpha_{bst-1}\left(H_{bs}^{t-1}\right)+0}=1, \tag{2.16}$$

since $\Pr\left[H_{bst}|s\notin\bar{S}\right]=0$ for any action profile H_{bst} that evidences a non-cooperative action by seller s (by Lemma 1). Once $\alpha_{bst}\left(H_{bs}^{t}\right)=1$, buyer b stops interacting with seller s because $-8\Pr\left[q_H\right]+2\Pr\left[q_L\right]<1.6$, the buyer's expected gain from interaction is less than his outside option . Note that the belief does not change even if b receives positive signals about s in the future through ratings given by other buyers (see eq. (2.15)).

For a fixed history H_{bs}^{t-1}(only cooperation from seller s has been observed) let $\Delta_{b'}\alpha_{bst}\left(H_{bs}^{t-1}\right)\equiv\alpha_{bst}\left(H_{bs}^{t-1},d_{b's},c...c\right)-\alpha_{bst}\left(H_{bs}^{t-1},c_{b's},c...c\right)$, where $c...c$ indicates that there is cooperation in all other interactions in period t. A greater $\Delta_{b'}\alpha_{bst}$ implies a greater future cost of acting non-cooperatively, due to the decrease in $\mathbb{E}\left[V_{st+1}^{*}\left(B_{st+1}\right)\left|H^{t-1},a_{st},B_{st},\sigma_{-st}^{*}\right.\right]$ as implied by eqs. 2.14, 2.15.

When there are no ratings, $\Delta_{b'}\alpha_{bst}=0$ for all $b\neq b'$, then there are no information spillovers between buyers. From 2.15 we derive

$$\Delta_{b}\alpha_{bst}\left(H_{bs}^{t-1}\right)\Big|_{\left\{b\in B^{I}:q^{*}(b,t)=q_H\right\}}\geq\Delta_{b}\alpha_{bst}\left(H_{bs}^{t-1}\right)\Big|_{\left\{b\in B^{U}:q^{*}(b,t)=q_H\right\}}\geq 0 \tag{2.17}$$

and

$$\Delta_{b}\alpha_{bst}\left(H_{bs}^{t-1}\right)\Big|_{\left\{b\in B:q^{*}(b,t)=q_L\right\}}=0, \tag{2.18}$$

since both behavioural types of sellers (selfish or IA/EL) choose the same action for a low cost treatment requiring buyer. Equations (2.17) and (2.18) imply the effect of non-cooperative action on updating in period t is strongest for high-cost treatment requiring informed buyers, followed by high-cost treatment requiring uninformed buyers; finally the effect is nil for low-cost treatment requiring buyers, irrespective of informational status. In other words, high-cost treatment requiring informed buyers learn faster than high-cost treatment requiring uninformed buyers; all other buyers do not learn anything in the current period.

Let's consider the case where ratings are available and there are informed and uninformed buyers. When ratings are available, and at least a fraction of buyers' hold other-regarding preferences and are willing to contribute to the public good by communicating their experience through ratings, information spillovers between buyers exist. Giving a rating does not affect the buyer's own payoff, since there is no competition among buyers in this setup, ratings are anonymous and costless by design. Recall that informed buyers (B^I) can distinguish between undertreatment with overcharging and no fraud when they require the high-cost treatment, and detect overcharging when they require the low-cost treatment. Ratings allow them to encode this entire information by using three messages: r^- for undertreatment with overcharging, r^0 for overcharging only, and r^+ for no fraud. Of course, all possible permutations of r^-, r^0 and r^+ form an otherwise equivalent equilibrium. Here we have imposed the 'natural' interpretation of a positive rating as a good signal, a negative rating as a bad signal and no rating as a neutral signal, corresponding to the various degrees of fraud. The fraction $1 - \beta$ of cooperative buyers will communicate this information to the market. Giving a rating is costless, hence even an infinitesimal degree of social preference implies meaningful use of ratings.

Uninformed buyers (B^U) can detect only one type of fraud when they require a high-cost treatment: undertreatment with overcharging (e.g. cooperative buyers' rating is r^-). They assign a positive probability to overcharging and a complementary probability to no fraud (e.g.cooperative buyers' rating is r^0). Alternatively, they attribute rating r^- or mix between r^- and r^0. An uninformed buyer gives the seller a lower rating following undertreatment than following sufficient treatment. It must be noted that we do not have a behavioural type that behaves in a non-fraudulent way.

When ratings are separated by informational status, the information from informed buyers is perfectly communicated, and hence sellers' interaction with informed buyers are effectively public information. There is perfect public detection of fraud, buyers stop interacting with any seller who undertreated and overcharged an informed buyer. In other words, learning is fast, $\alpha_{bst}\left(H^t_{bs}\right) = 1$ whenever $a_{bst} = d$ and $\left\{b \in B^I : q\left(b, t\right) = q_H\right\}$.

When ratings are pooled, informed buyers communicate the information through the ratings in the same way when ratings are separated by informational status, but their communication cannot be separated from the uninformed buyers' communication and hence learning is slower.

When there are only uninformed buyers, who deduce strictly less information from their interactions and therefore the information spillovers are weaker, the learning is slower than when informed buyers are present. All conclusions are made under the ceteris paribus condition, in particular, equal share of cooperative buyers across treatments.

Combining, we obtain

$$\Delta_{b'}\alpha_{bst}^{separated}\left(H_{bs}^{t-1}\right) \geq \Delta_{b'}\alpha_{bst}^{pooled}\left(H_{bs}^{t-1}\right) \tag{2.19}$$

$$\geq \Delta_{b'}\alpha_{bst}^{Only\ Uninformed}\left(H_{bs}^{t-1}\right) \geq \Delta_{b'}\alpha_{bst}^{No\ Ratings}\left(H_{bs}^{t-1}\right), \tag{2.20}$$

which yield the result.

Robustness checks for seller behaviour

	(1)	(2)
U-Ratings	0.0107 (0.08)	0.0107 (0.08)
U-I-Pooled	-0.0488 (-0.35)	-0.0204 (-0.12)
U-I-Separate	0.196 (1.49)	0.208 (1.32)
Round	0.00175 (0.18)	0.00178 (0.18)
Informed x U-I-Pooled		-0.0581 (-0.29)
Informed x U-I-Separate		-0.0245 (-0.14)
Constant	-1.981*** (-14.97)	-1.980*** (-14.98)
Observations	3459	3459

t statistics in parentheses

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.16: Random-effects panel probit regressions on undertreatment excluding last round.

(1)	(2)
Ho: U-I-Pooled = U-Ratings (p = 0.67) Ho: U-I-Pooled = U-I-Separate (p = 0.07*)	Ho: U-Ratings = U-I-Pooled + U-I-Pooled x Informed (0.61) Ho: U-I-Pooled + U-I-Pooled x Informed = U-I-Separate + U-I-Separate x Informed (0.17) Ho: U-I-Pooled = U-I-Pooled x Informed (0.91) Ho: U-I-Separate = U-I-Separate x Informed (0.43) Ho: U-I-Pooled = U-I-Separate (0.22)

$*$, $**$ and $***$ denote significance at the 10%, 5% and 1% level, respectively.

Table 2.17: Post-estimation tests for panel probit and probit regressions on undertreatment without last round.

	(1)	(2)
U-Ratings	-0.375*** (-3.61)	-0.373*** (-3.72)
U-I-Pooled	-0.485*** (-4.58)	-0.251** (-2.05)
U-I-Separate	-0.323*** (-3.10)	-0.164 (-1.34)
Round	0.0161*** (2.80)	0.0161*** (2.80)
Informed x U-I-Pooled		-0.490*** (-3.27)
Informed x U-I-Separate		-0.320** (-2.21)
Constant	-0.661*** (-7.74)	-0.659*** (-7.92)
Observations	3459	3459

t statistics in parentheses
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 2.18: Random-effects panel probit regressions on overcharging excluding last round.

(1)	(2)
Ho: U-I-Pooled = U-Ratings (p = 0.31) Ho: U-I-Pooled = U-I-Separate (p = 0.13)	Ho: U-Ratings = U-I-Pooled + U-I-Pooled x Informed (0.06)* Ho: U-I-Pooled + U-I-Pooled x Informed = U-I-Separate + U-I-Separate x Informed (0.09)* Ho: U-I-Pooled = U-I-Pooled x Informed (0.32) Ho: U-I-Separate = U-I-Separate x Informed (0.51) Ho: U-I-Pooled = U-I-Separate (0.54)
$*$, $**$ and $***$ denote significance at the 10%, 5% and 1% level, respectively.	

Table 2.19: Post-estimation tests for panel probit and probit regressions on overcharging without last round.

Scatterplot of total payoff and cumulative number of Non-fraudulent behaviour

Figure 2.7: Scatterplot of total payoff and cumulative number of Non-fraudulent (honest) behaviour.

Translated Instructions

[Expressions in square brackets were not visible to participants.]

[All Experimental Conditions:]

Welcome to this experiment and thank your for your participation!

These instructions are identical for each participant. In this experiment you will earn money which depends on your decisions but also on the decisions of other participants. Hence, it is very important that you read these instructions carefully. In case you have any questions, please raise your hand and an experimenter will come by to answer your questions.

Please do not communicate with other participants during the experiment. Please switch off your mobile phones now.

You will make your decisions in front of a computer. All decisions are made anonymously. You will never learn the identity of other participants and they will never learn your identity. To make the instructions as simple and comprehensible as possible, we will use male pronouns. The money you have earned in the experiment will be paid to you privately in cash at the end of the experiment. You will also receive 5 Euros for your participation.

The experiment today consists of two parts. In the beginning of each part you will receive detailed instructions. The two parts are independent from each other, which means that your decisions from the first part will not affect the second part of the experiment.

Plan for today:

1. Read instructions for Part 1
2. Answer comprehension questions
3. Part 1 of experiment
4. Read instructions for Part 2
5. Part 2 of experiment
6. Respond to Questionnaire
7. Receive Payment

[No Ratings Condition and U-Ratings Condition:]

Part 1:

Rounds, Roles, Groups
This experiment consists of 16 periods. There are two roles: Participant A and Participant B. In the beginning of the experiment you will learn which role was randomly assigned to you. You will keep this role until the end of the experiment.

You will be assigned to a group that consists of nine participants: three participants with role A and 6 participants with role B. Each participant with role A will receive a unique number that will identify him. There are Participant A1, Participant A2 and Participant A 3. The number assigned to the participants A will remain the same throughout the experiment, i.e. the umber 1 always represents the same same Participant A. The Participants B will not receive a number and won't be identifiable. In each round, each Participant A will interact with a number a number of Participants B that can vary between zero and six. Each Participant B interacts with at most one Participant A in a round.

Round 1 and Rounds 2 to 16
In round 1 two Participants B are randomly assigned to each Participant A which means each Participant A interacts with two Participants B. From round 2 on, each Participant B can choose whether and if yes, with which Participant A to interact.

Required Service for Participants B
In every round each Participant B either requires a small or a large service. These services are provided by Participant A. Whether a Participant B requires a small or large service will be determined randomly and independently for each Participant B anew in every round by the computer. Randomly means that that in each round a a (virtual) coin is tossed for each Participant B. If the coin shows heads then a Participant B requires a large service and if the coin shows tails the Participant B requires a small service. Participants B are not informed about which service they require.

Decisions of Participants A
Before a Participant A makes a decision in a round, he learns

1. how many Participants B want to to interact with him
2. the required service for each Participant B that wants to interact with him.

If more than one Participant B would like to interact with the same Participant A then these Participants B will be displayed in a random order to the the Participant A in every round. This helps that Participants A cannot distinguish and identify the Participant B they are interacting with. The only thing a Participant A knows about a Participant B is which service they require in the respective round.

Then a Participant A chooses a Price-Service Combination for each Participant B that has chosen to interact with him. All monetary units are denoted in Experimental Currency Units (ECU). There are following possible Price-Service Combinations:

1. Price 4 ECU, small service
2. Price 8 ECU, small service
3. Price 4 ECU, large service
4. Price 8 ECU, large service

If a Participant A chooses a small service then costs of 2 ECU arise for him. If a Participant A chooses a large service then costs of 6 ECU arise for him.

Payoffs
The Interaction Payoffs of a Participant A is the payoff resulting from the interaction with a Participant B. The Interaction Payoff is the difference between the required price and the costs that arise for the provided service:

$$Interaction\ Payoff = Price - Costs\ of\ chosen\ service.$$

If a Participant A is chosen by multiple Participants B then the Payoff for the round is the sum of his Interaction payoffs. It is also possible that a Participant A is not chosen by any Participant B. In this case, the Payoff for the round equals 1.60 ECU. The Interaction Payoff of a Participant B depends on what service was required and what service was received.

If the Participant B requires a small service, then he always earns

$$10\ ECU - Price.$$

If the Participant B requires a large service and receives the large service then he earns

$$10\ ECU - Price.$$

If the Participant B requires a large service and receives the small service then he earns

$$0\ ECU - Price.$$

The Payoff for the round of Participant B equals his Interaction Payoff. If a Participant B chooses not to interact with any Participant A, then he receives a Payment for the round of 1.60 ECU.

The table below provides an overview of Interaction Payoffs (In ECU) for Participants A and Participants B for all possible combinations of required service, chosen service and chosen Price (given that a Participant B wants to interact):

	B earns		
A chooses	B requires small service	B requires large service	A earns
Price 4, small service	10 - 4 = 6	0 - 4 = -4	4 - 2 = 2
Price 8, small service	10 - 8 = 2	0 - 8 = -8	8 - 2 = 6
Price 4, large service	10 - 4 = 6	10 - 4 = 6	4 - 6 = -2
Price 8, large service	10 - 8 = 2	10 - 8 = 2	8 - 6 = 2

As an example we will focus on the first row where Participant A chooses a price of 4 ECU and a small service. This Price-Service combination generates a payoff of $4 - 2 = 2\ ECU$ for Participant A (see last columns). How much Participant B earns depends on what service is required. If Participant B requires a small service, then he earns $10 - 4 = 6\ ECU$. If Participant B requires a large services, then he earns a payoff of $0 - 4 = -4\ ECU$.
In the beginning of the experiment each participant receives an endowment of 40 ECU. Potential losses can be covered with this in individual periods. In the end of the experiment the Payoffs received in every round and the endowment are added and paid out in cash.

Feedback for Participants B
In the end of each round, Participants B are informed about how much they have earned in the respective round. Participants B will neither be informed about which service they required nor which service they received. But in some cases, Participants B can infer which service they needed and have received:

1. A negative interaction payoff tells the Participants B that they needed the large service but received the small service.

2. From an interaction payoff of 2 ECU or 6 ECU a Participant B cannot infer which service he needed and which service he received. In this case, the Participant B either needed the small service and received a small service, or needed a large service and received a large service or required a small service but received the large service.

[U-Ratings Condition:]

Decisions of Participants B
After a Participant B was informed about his Interaction payoff, he can provide a rating for the interaction with the chosen Participant A. Participant B can choose to provide following ratings 'satisfied' , 'unsatisfied' and 'no rating'.

Feedback for Participants A
In the of every period, a Participant A is informed about his payoff in the respective round. Furthermore, he observes how each of his decision was rated.

Rounds 2 to 16
In the beginning of a round, from period 2 on, the ratings of each three Parti-

cipant A are displayed to every Participant A and Participant B. This means Participants A and B observe following information for each Participant A:

1. total number of 'satisfied' ratings up until current round
2. total number of 'unsatisfied' ratings up until current round
3. total number of 'no ratings' up until current round
4. total number of interactions up until current period

In addition, a table is displayed to all Participants B containing all their own decisions made, up to the current period. The table reminds Participants B with which Participant A they have interacted in which round, which price they paid, it further reminds them of their interaction payoffs and the ratings they have provided in each round.
Based on these information a Participant B can choose a Participant A with whom he wants to interact. A Participant B can also choose to refrain from any interaction. In this case, the round ends for the Participant B.

Overview of a Round from Round 2 on

1. All participants observe the ratings of the three Participants A. In addition to this the Participants B are reminded with which Participant A they have personally interacted this far.
2. Participants B decide whether and if yes with which Participant A to interact. (If a Participant B decides not to interact the round ends for this Participant. Also if a Participant A was not chosen by any Participant B then the round ends for him as well.)
3. Participants A are informed about what service each Participant B that decided to interact with them require.
4. Participants A select a Price-Service combination for each Participant B that decided to interact with them .
5. Participants B receive feedback.
6. Participants B can rate the interaction with the Participant A they have chosen in the beginning.
7. Participants A receive feedback. They see for every Participant B that decided to interact with them what service they needed, what service they have provided and the obtained ratings. They also learn how much they earned in the respective period.

In the first period, the first two steps are omitted as each Participant A is randomly matched with two Participants B.

[No-Ratings Condition:]

Feedback for Participants A
In the of every period, a Participant A is informed about his payoff in the respective round.

Decisions of Participants B

Rounds 2 to 16
In the beginning of a round, from period 2 on, a table is displayed to all Participants B containing all their own decisions made, up to the current period. The table reminds Participants B with which Participant A they have interacted in which round, which price they paid and it further reminds them of their interaction payoffs in each round.
Based on these information a Participant B can choose a Participant A with whom he wants to interact. A Participant B can also choose to refrain from any interaction. In this case, the round ends for the Participant B.

Overview of a Round from Round 2 on

1. Participants B are reminded with which Participant A they have personally interacted this far.

2. Participants B decide whether and if yes with which Participant A to interact. (If a Participant B decides not to interact the round ends for this Participant. Also if a Participant A was not chosen by any Participant B then the round ends for him as well.)

3. Participants A are informed about what service each Participant B that decided to interact with them require.

4. Participants A select a Price-Service combination for each Participant B that decided to interact with them .

5. Participants B receive feedback.

6. Participants A receive feedback.

In the first period, the first two steps are omitted as each Participant A is randomly matched with two Participants B.

[U-I-Pooled Condition and U-I- Separate Condition:]

Part 1:

Rounds, Roles, Groups
This experiment consists of 16 periods. There are three roles: Participant A, Participant B and Participant C. In the beginning of the experiment you will learn which role was randomly assigned to you. You will keep this role until the end of the experiment.

You will be assigned to a group that consists of nine participants: three participants with role A and 6 participants with role B. Each participant with role

A will receive a unique number that will identify him. There are Participant A1, Participant A2 and Participant A 3. The number assigned to the participants A will remain the same throughout the experiment, i.e. the umber 1 always represents the same same Participant A. The Participants B and Participants C will not receive a number and won't be identifiable. In each round, each Participant A will interact with a number a number of Participants B that can vary between zero and three and with a number of Participants C that can also vary between zero and three. Each Participant B and also each Participant C interacts with at most one Participant A in a round.

Round 1 and Rounds 2 to 16
In round 1 one Participant B and one Participant C are randomly assigned to each Participant A which means each Participant A interacts with one Participant B and one Participant C. From round 2 on, each Participant B can choose whether and if yes, with which Participant A to interact. From round 2 on, each Participant C can also choose whether and if yes, with which Participant A to interact.

Required Service for Participants B and Participants C
In every round each Participant B and Participant C either require a small or a large service. These services are provided by Participant A. Whether a Participant B rand a Participant C require a small or large service will be determined randomly and independently for each Participant B and Participant C anew in every round by the computer. Randomly means that that in each round a a (virtual) coin is tossed for each Participant B and Participant C. If the coin shows heads then a Participant B and a Participant C require a large service and if the coin shows tails the Participant B and Participant C require a small service. Participants B and Participants C are not informed about which service they require.

Decisions of Participants A
Before a Participant A makes a decision in a round, he learns

1. how many Participants B and Participants C want to to interact with him
2. the required service for each Participant B and Participant C that wants to interact with him.

If more than one Participant B would like to interact with the same Participant A then these Participants B will be displayed in a random order to the the Participant A in every round. This helps that Participants A cannot distinguish and identify the Participant B they are interacting with. The only thing a Participant A knows about a Participant B is which service they require in the respective round. The same holds for Participants C.
Then a Participant A chooses a Price-Service Combination for each Participant B and Participant C that has chosen to interact with him. All monetary units are denoted in Experimental Currency Units (ECU). There are following possible Price-Service Combinations:

1. Price 4 ECU, small service

2. Price 8 ECU, small service
3. Price 4 ECU, large service
4. Price 8 ECU, large service

If a Participant A chooses a small service then costs of 2 ECU arise for him. If a Participant A chooses a large service then costs of 6 ECU arise for him.
Payoffs
The Interaction Payoffs of a Participant A is the payoff resulting from the interaction with a Participant B or a Participant C. The Interaction Payoff is the difference between the required price and the costs that arise for the provided service:

$$Interaction\ Payoff = Price - Costs\ of\ chosen\ service.$$

If a Participant A is chosen by multiple Participants B or Participants C then the Payoff for the round is the sum of his Interaction payoffs. It is also possible that a Participant A is not chosen by any Participant B or Participant C. In this case, the Payoff for the round equals 1.60 ECU. The Interaction Payoff of a Participant B or Participant C depends on what service was required and what service was received.

If the Participant B or Participant C requires a small service, then he always earns

$$10\ ECU - Price.$$

If the Participant B or Participant C requires a large service and receives the large service then he earns

$$10\ ECU - Price.$$

If the Participant B or Participant C requires a large service and receives the small service then he earns

$$0\ ECU - Price.$$

The Payoff for the round of Participant B or Participant C quals his Interaction Payoff. If a Participant B or Participant C chooses not interact with any Participant A, then he receives a Payment for the round of 1.60 ECU.

The table below provides an overview of Interaction Payoffs (In ECU) for Participants A , Participants B and Participants C for all possible combinations of required service, chosen service and chosen Price (given that a Participant B or Participant C wants to interact).
As an example we will focus on the first row where Participant A chooses a price of 4 ECU and a small service. This Price-Service combination generates a payoff of $4 - 2 = 2\ ECU$ for Participant A (see last column). How much Participant B or C earn depends on what service is required. If Participant B (or C) requires a small service, then he earns $10 - 4 = 6\ ECU$. If Participant B(or C) requires a large services, then he earns a payoff of $0 - 4 = -4\ ECU$.

	B or C earns		
A chooses	B or C requires small service	B or C requires large service	A earns
Price 4, small service	10 - 4 = 6	0 - 4 = -4	4 - 2 = 2
Price 8, small service	10 - 8 = 2	0 - 8 = -8	8 - 2 = 6
Price 4, large service	10 - 4 = 6	10 - 4 = 6	4 - 6 = -2
Price 8, large service	10 - 8 = 2	10 - 8 = 2	8 - 6 = 2

In the beginning of the experiment each participant receives an endowment of 40 ECU. Potential losses can be covered with this in individual periods. In the end of the experiment the Payoffs received in every round and the endowment are added and paid out in cash.

Feedback for Participants B and C
In the end of each round, Participants B and C are informed about how much they have earned in the respective round. Participants C will also be informed about what service they required and what service they have received. Participants B will neither be informed about which service they required nor which service they received. But in some cases, Participants B can infer which service they needed and have received:

1. A negative interaction payoff tells the Participants B that they needed the large service but received the small service.

2. From an interaction payoff of 2 ECU or 6 ECU a Participant B cannot infer which service he needed and which service he received. In this case, the Participant B either needed the small service and received a small service, or needed a large service and received a large service or required a small service but received the large service.

Decisions of Participants B and C
After a Participant B was informed about his Interaction payoff and a Participant C has been informed about his interaction payoff, required and received service, he can provide a rating for the interaction with the chosen Participant A. ParticipantS B and C can choose to provide following ratings 'satisfied' , 'unsatisfied' and 'no rating'.

Feedback for Participants A
In the of every period, a Participant A is informed about his payoff in the respective round. Furthermore, he observes how each of his decision was rated.

[U-I Pooled Condition:]

Rounds 2 to 16
In the beginning of a round, from period 2 on, the ratings of each three Participant A are displayed to every Participant A, Participant B and Participant C. This means Participants A, B and C observe following information for each Participant A:

1. total number of ’satisfied’ ratings up until current round
2. total number of ’unsatisfied’ ratings up until current round
3. total number of ’no ratings’ up until current round
4. total number of interactions up until current period

In addition, a table is displayed to all Participants B and C containing all their own decisions made, up to the current period. The table reminds Participants B and c with which Participant A they have interacted in which round, which price they paid, it further reminds them of their interaction payoffs and the ratings they have provided in each round. Participants C are also reminded of which service they required and what service they have received.
Based on these information a Participant B or C can choose a Participant A with whom he wants to interact. A Participant B or C can also choose to refrain from any interaction. In this case, the round ends for the Participant B.

[U-I Separate Condition:]

Rounds 2 to 16
In the beginning of a round, from period 2 on, the ratings of each three Participant A are displayed to every Participant A, Participant B and Participant C. This means Participants A, B and C observe following information for each Participant A:

1. total number of ’satisfied’ ratings up until current round
2. total number of ’unsatisfied’ ratings up until current round
3. total number of ’no ratings’ up until current round
4. total number of interactions up until current period

These information are summarised in three tables. The first table contains the total ratings provided by Participants B and C combined up until the current round. The second table displays the total ratings provided by Participants B up until the current round. The third table displays the total ratings provided by Participants C up until the current round.

In addition to these, a table is displayed to all Participants B and C containing all their own decisions made, up to the current period. The table reminds Participants B and c with which Participant A they have interacted in which round, which price they paid, it further reminds them of their interaction payoffs and the ratings they have provided in each round. Participants C are also reminded of which service they required and what service they have received.
Based on these information a Participant B or C can choose a Participant A with whom he wants to interact. A Participant B or C can also choose to refrain from any interaction. In this case, the round ends for the Participant B.

[U-I-Pooled Condition and U-I- Separate Condition:]

Overview of a Round from Round 2 on

1. All participants observe the ratings of the three Participants A. In addition to this the Participants B and Participants C are reminded with which Participant A they have personally interacted this far.

2. Participants B and C decide whether and if yes with which Participant A to interact. (If a Participant B or C decides not to interact the round ends for this Participant. Also if a Participant A was not chosen by any Participant B then the round ends for him as well.)

3. Participants A are informed about what service each Participant B and C that decided to interact with them require.

4. Participants A select a Price-Service combination for each Participant B and C that decided to interact with them .

5. Participants B and C receive feedback.

6. Participants B and C can rate the interaction with the Participant A they have chosen in the beginning.

7. Participants A receive feedback. They see for every Participant B and C that decided to interact with them what service they needed, what service they have provided and the obtained ratings. They also learn how much they earned in the respective period.

In the first period, the first two steps are omitted as each Participant A is randomly matched with one Participant B and Participant C.

[All Experimental Conditions:]

Feedback in the End of the Experiment
In the end of the experiment each participant is informed about how much they have earned in the experiment.

The exchange rate is $1\ ECU = 0.14\ EUR$**.**

Comprehension Questions and Next steps
First you will be asked to answer comprehension questions to guarantee that you have understood the experiment and the instructions. After all your (potential) questions have been answered the first part of the experiment will start. You will receive the instructions for the second part after the first part of the experiment is over.

Part 2:

Each Participant receives one of the three roles at random: Participant X, Participant Y or Participant Z. A Participant Y and Participant X are randomly assigned to a Participant X. A Participant X, a Participant Y and a Participant

Z form a group. Each Participant X makes one decision only. A Participant X decides how 11 ECU should be split in the group. Participants Y and Participants Z always have to accept the decision of Participant X. Following splits are possible:

Split	X receives	Y receives	Z receives
1	1 ECU	10 ECU	10 ECU
2	2 ECU	9 ECU	9 ECU
3	3 ECU	8 ECU	8 ECU
4	4 ECU	7 ECU	7 ECU
5	5 ECU	6 ECU	6 ECU
6	6 ECU	5 ECU	5 ECU
7	7 ECU	4 ECU	4 ECU
8	8 ECU	3 ECU	3 ECU
9	9 ECU	2 ECU	2 ECU
10	10 ECU	1 ECU	1 ECU

More generally speaking, Participant X decides what amount $k\ ECU)$ to keep for himself. Which means that Participant Y and Z each receives $(11-k)\ ECU$. When making the decision nobody will be informed about their role. Everybody needs to make the decision as there is a possibility of being assigned the role of Participant X. after every participant has made their decision, you will learn which role was randomly assigned to you and how much you have earned in the second part of the experiment. This concludes the second part of the experiment.

The exchange rate is $1\ ECU = 0.14\ EUR$**.**

Control Questions

Please select the wrong statement:

Question 1:

a. The experiment consists of 16 rounds.

b. There are two roles: Participant A and Participant B. [OR: There are three roles: Participant A, Participant B and Participant C.] (in U-I-Pooled und U-I-Separate)

c. In each round, the number of Participants B a Participant A can interact with varies between zero to six. [OR: In each round, the number of Participants B and C, a Participant A can interact with varies between zero to three each.] (in U-I-Pooled und U-I-Separate)

d. A Participant B interacts at most with only one Participant A. [OR: A Participant B interacts at most with only one Participant A. A Participant C interacts at most with only one Participant A.] (in U-I-Pooled und U-I-Separate)

e. In your group, there are 10 participants in total.

Question 2:

a. The required service is determined anew, independently and at random in every period for every Participant B. [OR: The required service is determined anew, independently and at random in every period for every Participant B and Participant C.] (in U-I-Pooled und U-I-Separate)

b. Participant A is informed about the service each participants needs that chose to interact with him.

c. Participant A has to choose the same Price-Service Combination for each participant that wants to interact with him.

d. Participants A can always be identified

Question 3:

a. If a participants requires a small quality, then Participant A has to provide the small quality. If a participants requires a large quality, then Participant A has to provide the large quality.

b. Participants B cannot be identified.

c. Participants C are always informed about whether they required a small or large service in the end of every period. [OR Participants B are never informed about which service they require.] (in U-Ratings und U-No Ratings)

d. From a negative interaction payoff Participants B can infer that they needed the large service but required the small service.

e. From a Payoff of 2 ECU, Participants B cannot infer what services they needed and received.

Question 4:

Assume Participant B requires the small service and he has to pay a price of 8 ECU. What is the resulting interaction payoff? (10-8=2 ECU)

Question 5:

Assume Participant B requires the large service and he received a small service but has to pay a price of 8 ECU. What is the resulting interaction payoff? (8-8=0 ECU)

Question 6:

Assume Participant A is interacting with two Participants B. For the first Participant B, Participant A provides the small services and charges the price of the small service and for the second a large service is provided and the price of the large service is charged. What is the resulting payoff for Participant A? (2+2=4 ECU)

Question 7:

a. Participants B can rate the Interaction with a Participant A. [OR: Participants B and C can rate the Interaction with a Participant A.](in U-I-Pooled und U-I-Separate)

b.Participant A learns how himself and the other Participants A have been rated.

c. One can always distinguish whether a rating has been provided by a Participant B or Participant C. (in U-I-Pooled) [OR One cannot distinguish whether a rating has been provided by a Participant B or Participant C.] (in U-I-Separate) [OR The table presented in the beginning of each round from round two on contains Information about all positive and negative ratings each Participant has received up until the current period.] (in U-Ratings)

d. If a Participant C provides a rating, he know which service he required, which service he has received and what price he has been charged. [OR No Option d] (in U-Ratings) [OR No Question 7] (in U-No Ratings)

Chapter 3

Non-numerical and Social Anchoring in Consumer-generated Ratings

3.1 Introduction

Trust among economic agents is crucial for online markets to function. A common way to establish and sustain trust in markets is to facilitate reputation building, e.g. through voluntary online feedback. Nowadays online ratings are ubiquitous and have become an important part of our everyday lives. When making a purchase, deciding where to eat or even which doctor to visit, many of us consult ratings. Ratings have been shown to significantly affect economic interactions and we mainly use them in order to make better-informed decisions.

Chevalier & Mayzlin (2006) and Dellarocas et al. (2007) show that ratings can significantly affect the behaviour of buyers and product success in general. Dellarocas (2003) analyses reputation building through a rating system and finds that rating systems can increase market efficiency, and foster co-operative outcomes and trust between buyers and sellers. Comparable results are obtained by Keser (2002), Danilov & Sliwka (2017), Bohnet & Huck (2004) and Bolton et al. (2004). Chen & Xie (2008) suggest that online reviews created by users can work as "sales assistants" to help novice consumers identify the products that best match their preferences. The prevalence and importance of ratings in economic interactions raise the question about how accurate and hence helpful consumer-generated ratings really are.

Decades of research in behavioural economics have shown that human decision-making is imperfect and prone to errors and biases. This paper investigates behavioural biases that might arise. A commonly studied bias is the anchoring effect, which was first proposed by Slovic & Lichtenstein (1972) and further elaborated by Tversky & Kahneman (1974). Anchoring can be defined as an irrelevant informational cue that might influence behaviour in a way that is inconsistent with rational decision-making (Tversky & Kahneman 1974). If ratings are affected by the anchoring bias, this might hamper their informativeness with detrimental consequences for buyers, sellers and online market platforms that bring buyers and sellers together. If not accurate, ratings can impair consumers' decision-making and result in erroneous decisions. Inaccurate ratings might also be harmful to firms; consumers might reward low-quality providing firms and punish more efficient firms with detrimental effects on overall welfare. Online market platforms are also interested in accurate ratings due to reputation concerns. Hence, it is imperative to understand whether ratings are anchored or not. Potential sources for anchoring could be invoked by the design of the rating system, ratings by other people and ratings for different products.

Using economic incentives and repeated decisions, this study experimentally scrutinises the prevalence and persistence of the anchoring bias in online ratings by isolating the post-purchase and consumption rating decision. To closely resemble the online rating provision, we implement an online experiment, where participants make rating decisions. The anchor is presented as a suggested rating, which participants can either implement or change. We present participants either with no anchor, a high anchor (upper bound of rating scale), a low anchor (lower bound of rating scale) or a social anchor (average rating of prior, independent, round). We report significant asymmetric anchoring effects with respect to high and low anchors. Participants provide significantly higher ratings when they are presented with a high anchor but the low anchor has no significant effect. Furthermore, the endogenously derived social anchor significantly affects ratings and is perceived as more relevant. Overall, our findings cast doubt on the informativeness of ratings and can help to design less error-prone rating platforms that avoid anchoring. Our study mainly contributes to two strands of literature. Firstly, we further the knowledge of the existence of anchoring effects in rating settings by providing controlled experimental evidence. Secondly, to our knowledge, we are the first to show the impact of non-numerical anchors on economic decisions.

The remainder of this Chapter is organised as follows. The next section includes a brief review of the anchoring literature followed by the experimental design, behavioural hypotheses and experimental results. Finally, we conclude the paper with a discussion.

3.2 Literature Review

The anchoring bias has been shown to be pervasive and robust in many situations (Furnham & Boo 2011). Klein et al. (2014) obtain similar results and are able to replicate anchoring effects across 36 different samples. The anchoring effect has been shown to be prevalent in consequential economic decisions such as credit card minimum repayments (Stewart 2009), purchase quantity (Wansink et al. 1998), negotiations (Schweinsberg et al. 2012), reservation prices (Krishna et al. 2006), real estate evaluations (Northcraft & Neale 1987), strategic interactions (Ivanova-Stenzel & Seres 2021), and stock market estimations (Kaustia et al. 2008).[1] However, some researchers question the ubiquitous prevalence and robustness of anchoring effects due to the lack of real economic conditions such as monetary incentives and learning effects through feedback. List & Millimet (2008) and Loomes et al. (2003) argue that economic incentives combined with learning effects filter irrationality and thus reduce biases. Several experimental willingness-to-pay/accept studies report weak anchoring effects and provide evidence for a lack of robustness (Alevy et al. 2015, Fudenberg et al. 2012, Maniadis et al. 2014). However, the evidence on pervasiveness and robustness of anchoring effects in willingness-to-pay/accept studies is mixed since there are also studies reporting robust anchoring effects (Ariely et al. 2003, Yoon et al. 2019). Yoon & Fong (2019) show that anchoring effects can persist and can result in lasting changes in valuation judgments.

All these studies use exogenous anchors in autonomous and individual decision-making environments. However, many economic decisions take place in social settings. Besides incorporating monetary incentives and learning through feedback, it is important to consider the social context when scrutinising anchoring effects. Observing other individuals' behaviour and the information they are sharing can enhance learning effects. In fact, there is some evidence of social anchoring. Phillips & Menkhaus (2010) investigate the effect of an endogenously derived anchor on willingness to pay and accept decisions in an auction environment. Hereby, the average price of the previous round serves as an anchor. The authors find evidence of significant anchoring effects. Using an estimation task with market conditions such as economic incentives and possible learning through feedback, Meub & Proeger (2015) present robust social anchoring effects. The average estimation of the previ-

[1]Four classes of explanation have been offered: i.) underadjustment (Tversky & Kahneman 1974, Epley & Gilovich 2001), ii.) numerical priming (Jacowitz & Kahneman 1995) , iii.) confirmatory hypothesis testing (Chapman & Johnson 1999) and iv.) scale distortion theory (Frederick & Mochon 2012).

ous round serves as the anchor. The anchoring bias increases with cognitive load and there are only weak learning effects. They compare the socially derived anchor to a classical anchor and show that the social context increases the anchoring bias. de Wilde et al. (2018) obtain comparable results.

Ratings are provided in social environments. Social information about how other people have rated a certain service or product is available and this information, given accurate, can improve decision quality. But if affected by the anchoring bias, ratings can be detrimental for consumers, sellers and online market platforms.

Research on rating provision has uncovered a j-shaped distribution of ratings (Hu, Zhang & Pavlou 2009). This translates into some low ratings and a lot of high ratings with almost nothing in between these two extremes. Consumers usually rate to 'brag and moan' (Lafky 2014). They reward or punish sellers depending on whether expectations are met or not and rate leniently under uncertainty (Nosko & Tadelis 2015, Zervas et al. 2017, Bolton et al. 2019). The stark polarisation and upward compression could lead to anchoring effects. Assuming ratings are strongly skewed towards the positive extreme, this might induce lenient rating behaviour. After observing the positive skewness, an individual who would have provided a medium rating might leave more favourable feedback, which does not reflect the true rating. Besides positive skewness, average ratings, which are a prominent feature on many rating platforms, can also serve as an anchor.

The majority of research that investigates the anchoring bias, includes studies on social anchors, which heavily rely on abstract, numerical anchors. However, diverging from the standard literature, in this paper, we rely on non-numerical, visual anchors. We do this for mainly two reasons: Firstly, in a rating environment, a lot of information is not only displayed numerically but also visually via coloured stars or bars. For instance, on Amazon or Uber a continuous five-star rating system is used. The user has to colour the number of stars that they want to provide. Seeing the 5 stars from the beginning might already serve as a high anchor and create a sense of largeness. In fact, LeBoeuf & Shafir (2006) report non-numerical anchoring effects, using various stimuli such as length, weight or volume. Oppenheimer et al. (2008) provide experimental evidence for non-numerical and cross-modal anchoring effects. The authors explain the effect via magnitude priming which is the creation of a perception of largeness (or smallness). However, these studies lack monetary incentives and the possibility for learning which our study includes. Secondly, using a non-numerical anchor increases the validity of our results and remedies a loss of control arising due to the experiments being conducted online. Using a numerical anchor in an online experimental setup would have made our results less reliable since the data is noisier. An advantage of conducting our study online is that ratings are usually provided online in real life. This makes our experimental setup less abstract and more familiar for the participants and increases the external validity of our findings.

Recent studies have shown that rating behaviour is biased and potentially prone to the anchoring bias. Consumers' provided ratings can be affected by previously posted reviews. Moe & Trusov (2011) empirically identify bandwagon behaviour in ratings. Moreover, Coker (2012) provides evidence of asymmetrical affective perseverance when forming attitudes. Consumers overshoot their judgments when positive information is replaced with negative information but not vice versa. This means positive reviews anchor positive attitudes, even if negative reviews follow later and consumers know previous information was erroneous. Evidence on behaviour that is indicative of the anchoring bias is not only provided for product ratings but also for performance ratings. Thorsteinson et al. (2008) used both field and laboratory studies to scrutinise anchoring effects in performance judgements and found asymmetrical anchoring effects where the low anchors have a weaker effect than higher anchors. Berger & Daumann (2021) report anchoring effects for judging basketball players.

We contribute to the literature by studying the prevalence of the anchoring bias in rating provision under market conditions.

3.3 Experimental Design and Hypotheses

3.3.1 Experimental Design

We report an experiment, using a between-subject design with five experimental conditions: No Anchor, High Anchor, Low Anchor, Social Anchor and Slider Task. We use an individual decision-making task where participants provide quality ratings in the four anchor conditions. Participants are presented with a slider which represents the overall range of quality. On the slider, a quality interval is displayed. The task consists of rating the quality that is contained in the quality interval by moving the slider for twelve rounds without feedback between rounds. There are no numerical values displayed on the slider or quality interval. The true quality and hence, its exact position in the quality interval is unknown to the participants. But participants know that the true quality is always contained in the interval, with each value within the interval having equal probability of being randomly drawn as the quality. This implements an element of uncertainty. We do this since there is always an element of uncertainty about the exact quality of a good even after consuming it. One can roughly judge and assign a rough estimate of whether the quality of the consumed good met expectations or not but is hard to pin down the quality to an exact number. In the Slider Task condition, we employ a standard slider task with a neutral framing, meaning that the task is not framed as a rating task. In the slider task participants are supposed to move the sliders' handle to an invisible target value which is always positioned in the middle of the interval. The closer the participants are to the true quality in the rating task or the target value in the slider task, the higher is the payoff. The participants know this. Thus, the slider task ensures that participants are able to accurately utilise the sliders' handle. Furthermore, we can identify participants who are not affected by the framing in the rating conditions but behave similarly to those in the slider task condition.

We utilise pixels (px) to measure the provided ratings and to determine payoffs.[2] Hereby, one px equals one Experimental Currency Unit (ECU). This in turn means, that each minuscule movement of the slider is payoff relevant for the participants. As participants never learn the true quality, the rational strategy is to choose the expected quality value which is the middle of the quality interval to minimise deviation from the true quality. In other words, the rational rating is $r^* = \frac{q^H - q^L}{2}$ where q^H is the upper bound of the quality range and q^L is the lower bound of the quality range.[3]

The displayed quality intervals were randomly drawn in the first session and kept the same across all other sessions to ensure comparability of experimental conditions. To ensure irrelevance of anchor values the maximum and minimum of the slider were excluded from the draw. These values were used as anchor values in the High Anchor and Low Anchor conditions.

In the High and Low Anchor conditions, the participants can either choose the presented anchor as their rating or choose to adjust the rating. The presented anchor is the maximum of the slider in the High Anchor condition and the minimum in the Low Anchor condition. If participants choose to adjust the rating they have to reinitialise their rating by clicking anywhere on the slider. These two anchors present irrelevant informational cues as the quality interval never contains the minimum or maximum of the slider. Differently from the High Anchor and Low Anchor conditions, the anchor is derived endogenously in the Social Anchor condition and not provided exogenously. The participants are divided into groups of five that remain the same throughout the experiment. At the beginning of every round, starting from round two on, the group average rating from the previous period serves as the anchor and

[2]The CSS unit px is usually understood as the smallest unit of measurement in CSS applications.

[3]See Appendix for the formal derivation of the rational rating.

is shown to the participants. The participants can either select the socially derived anchor or can adjust their rating. For instance, if the participants are in round two, they see the quality interval and the average rating across all group members from round one as the anchor.

The socially derived anchor does not contain any additional information value as the quality interval is independently drawn in each round. However, in some instances, the social anchor is contained in the quality interval. We will discuss the implications of this further below in the results section. In the No Anchor condition, there is no anchor present in any form. Participants always have to initialise the slider by clicking on it and can readjust their rating by dragging the sliders' handle. The Slider Task condition is similar to the No Anchor condition, but under a different framing, as explained before.

In addition to the rating task, respectively slider task, we collected a number of controls. We ran a standard cognitive reflection test (CRT). Additionally, we ran a questionnaire to get an indication of statistical aptitude that consists of three questions. In the first two questions, we asked participants to state whether they have any prior knowledge of statistics and whether they know what an expected value is. In the third question, participants were asked to work out the expected value for a dice roll problem. In the anchoring conditions, we additionally elicited the perceived relevance of the presented anchor by using a 7-point Likert scale. The participants were asked to rate perceived helpfulness, perceived informativeness and cue use from 'I do not agree at all' to 'I fully agree'.Before the experiment started we provided the participants with detailed instructions and control questions to ensure comprehension of the task.[4]

Experiments were programmed using oTree (Chen et al. 2016) and conducted virtually in April, May and October 2021 using the ORSEE database of the joint lab of WZB Berlin And TU Berlin (Greiner 2004). On average participants earned 14 EUR, including a show-up fee of 5 EUR, (where 100 ECU = 1 EUR). Sessions lasted for 30 minutes. In the end, one round was selected at random to determine the final payoff. In total 246 participants took part in the experiment over five sessions per experimental condition.

3.3.2 Hypotheses

In our experimental setup, the anchors are designed to be useless and contain no relevant informational value. This means, if participants are not anchored, their rational choice is to select the middle of the quality interval as the rating

[4]See Appendix for the full translated and original instructions, as well as the post-experimental questionnaire.

in order to minimise deviations from the true quality and maximise payoffs. But as the anchoring effect has been shown to be prevalent in many economic environments, we expect there to be anchoring effects. In fact, we hypothesise that ratings will be biased towards the thresholds in the respective anchor conditions.

Hypothesis 1a: *Ratings will be biased upwards in the High Anchor condition.*

Hypothesis 1b: *Ratings will be biased downwards in the Low Anchor condition.*

Participants make the same rating decision for twelve periods. By this we can investigate the persistence of anchoring effects, as participants might be less prone to the anchoring bias in the later rounds of the experiment. Therefore, we include round effects and interactions.

In the Social Anchor condition, the anchor is derived in a social context by averaging the previously provided ratings within a group. Hence, depending on the ratings of the previous round this might give rise to both high and low socially derived anchors. Similar to before, we expect both, high and low social anchors to bias ratings.

Hypothesis 2: *Ratings will be biased towards the group anchor in the Social Anchor condition.*

As it has been reported, such a social context can increase the perceived relevance and informativeness of the anchor de Wilde et al. (2018). We expect that the anchor will be perceived as more trustworthy in the Social Anchor condition compared to the other anchor conditions. Furthermore, we expect that the anchoring bias is more pronounced for those individuals who place greater trust in the socially derived anchor.

Hypothesis 3: *The social anchor will be perceived as more relevant than high and low anchors. The anchoring bias will be more pronounced when the anchor is perceived as more relevant.*

3.4 Experimental Results

In the following, we explore our hypotheses at hand of the experimental results. We mainly refer to the effects of experimental conditions and control variables on the normalised rating $(r_{it} - r_t^*)/(q_H - q_L)$ or the absolute rating

r_{it}. We drop those observations where participants directly chose the presented anchor. Such decisions may come down to one of two reasons. First, participants may have defaulted to the presented anchor. Second, participants may have made this decision by mistake, as it was irreversible. We drop 122 such observations[5], which leaves us with a total of 2,830 rating observations by 246 participants who each give twelve ratings in consecutive rounds.[6] If not stated otherwise we employ random-effects regressions clustered on the subject level.

Experimental condition	Mean	Std. Dev.	Min	Max
No Anchor	0.027	0.207	-0.667	0.829
High Anchor	0.058	0.233	-0.873	1.994
Low Anchor	0.049	0.248	-1.317	1.526
Social Anchor	0.069	0.300	-1.053	1.609
Slider Task	0.001	0.015	-0.052	0.070

Table 3.1: Summary statistics of normalised ratings.

In Table 3.1 we show the summary statistics of the normalised ratings across all experimental conditions. In Figure 3.4 in the Appendix, we show an according kernel density estimation of the normalised rating. Taken together we observe that the Social Anchor condition diverts ratings from the rational expectation the most, whereas the High and Low Anchor conditions divert somewhat compared to the No Anchor condition. All anchors seem to inflate ratings, with the highest impact of the high and social anchors.

In Figure 3.1 we show the ratings over rounds for all experimental conditions normalised around the rational prediction.[7]

In Table 3.2 we show results on whether anchors in any experimental condition have a direct impact on the normalised rating compared to the No Anchor condition and compared to the Slider Task condition. In specifications (1) and (2) we compare the anchoring conditions to the No Anchor control. In the latter, we include round effects and interactions. The only significant impact that carries through is the High Anchor condition. The significant effect of the Social Anchor condition in (1) is mediated when including round

[5]Six in the High Anchor condition, 14 in the Low Anchor condition and 48 in the Social Anchor condition. Six out of the 48 in the Social Anchor condition were outside the quality range and 42 were inside the quality range. Our results remain qualitatively similar when not dropping any of these observations.

[6]There is one clear outlier in the Slider Task, where a participant gave a rating that was 297px from the target, where all other ratings are within 21px of the target. Except for the summary statistics we keep this observation, though all results are robust to dropping it.

[7]In Figure 3.5 in the Appendix we show the development of ratings over rounds and the boundaries of the quality range.

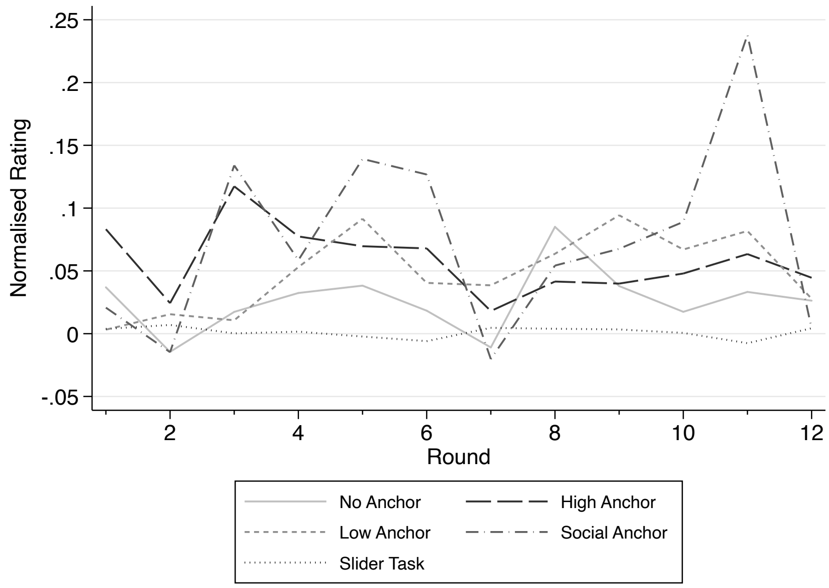


Figure 3.1: Average normalised ratings over rounds by experimental condition.

	Dependent variable: Normalised rating $(r_{it} - r_t^*)/(q_H - q_L)$			
	(1)	(2)	(3)	(4)
High Anchor	0.035**	0.067***	0.058***	0.077***
	(0.017)	(0.026)	(0.014)	(0.020)
Low Anchor	0.023	0.002	0.046***	0.013
	(0.015)	(0.028)	(0.010)	(0.023)
Social Anchor	0.043**	0.036	0.066***	0.047*
	(0.021)	(0.031)	(0.017)	(0.027)
No Anchor			0.023**	0.011
			(0.011)	(0.016)
Round		0.002		-0.000
		(0.002)		(0.000)
High Anchor × Round		-0.005		-0.003
		(0.003)		(0.002)
Low Anchor × Round		0.003		0.005*
		(0.004)		(0.003)
Social Anchor × Round		0.001		0.003
		(0.004)		(0.004)
No Anchor × Round				0.002
				(0.002)
Constant	0.033***	0.023	0.008	0.010
	(0.013)	(0.017)	(0.007)	(0.007)
Base Category	No Anchor	No Anchor	Slider Task	Slider Task
Observations	2242	2242	2829	2829
Number of subjects	197	197	246	246

Standard errors in parantheses. Estimation random-effects regression clustered on subject-level. Controls are the CRT and statistical aptitude scores. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 3.2: Treatment effects on normalised rating.

effects and interactions in (2).[8]

Specifications (3) and (4) mirror the approach from before relative to the Slider Task condition. We observe significant overrating in all experimental

[8] We explore the role of socially derived anchors in more detail later.

conditions compared to the Slider Task, when no controlling for round effects and interactions. When controlling for round effects and interactions we find that the significant overrating in the High Anchor condition carries through, whereas the overrating in the Low Anchor condition seems to build up over rounds. However, we cannot infer a clear indication of overrating in the No Anchor condition, while the effect in the Social Anchor condition is marginally significant.

Result 1: *The High Anchor condition has a positive effect on ratings. The Low Anchor condition has no negative effect on ratings.*

In the following, we explore the role of the Social Anchor on rating behaviour. There are multiple things to note here. The socially derived anchor may take varying roles between a high and a low anchor. Expectations on the effect of the social anchor naturally differ between these situations. We proceed in two steps. First, we check whether the socially derived anchor is predictive of the observed rating when controlling for the rational expectation. Second, we check whether the bias differs between high and low social anchors. We classify social anchors according into three: No anchor, High Social Anchor ($\bar{r}_{t-1} > r_t^*$) and Low Social Anchor ($\bar{r}_{t-1} < r_t^*$). The following model explains rating r_{it} of participant i at time t in terms of the rational expectation r_t^* and the anchoring deviation $r_t^* - \bar{r}_{t-1}$ at time t:

$$r_{it} = \alpha r_t^* + \beta(r_t^* - \bar{r}_{t-1}) + \epsilon_{it}. \tag{3.1}$$

For a fully rational participant, we would expect $\alpha = 1$ and $\beta = 0$. That is, the rating is fully described by the rational expectation. An anchoring deviation, i.e. a biased rating towards the anchor is present when $\beta < 0$.

First, we estimate this model for all observations to identify the general anchoring deviation in the Social Anchor condition. Subsequently, we divide our observation pool according to our distinction into those where a high social anchor ($\bar{r}_{t-1} > r_t^*$) and those where a low social anchor ($\bar{r}_{t-1} < r_t^*$) is present.

We show the regression results of the aforementioned model in Table 3.3. We find that α is slightly larger than, one. This indicates a general tendency towards rating over the rational rating. In specification (1) we can confirm the anchoring deviation by a significant $\beta < 0$. By splitting the observations into low and high social anchors we observe that this effect is still present in (3) where we restrict our attention to high social anchors, but not in (2) where we restrict to low social anchors. We conclude that high social anchors distort

	Dependent variable: Rating r_{it}		
	(1)	(2)	(3)
Rational rating r_t^*	1.028***	1.006***	1.027***
	(0.00890)	(0.0150)	(0.0198)
Deviation $(r_t^* - \bar{r}_{t-1})$	-0.0782***	-0.0170	-0.0903**
	(0.0172)	(0.0430)	(0.0337)
Considered observations	All	$\bar{r}_{t-1} < r_t^*$	$\bar{r}_{t-1} > r_t^*$
Observations	462	193	269
Number of subjects	50	50	50

Standard errors in parantheses. Estimation by OLS regression. ** and *** denote significance at the 5% and 1% level, respectively.

Table 3.3: OLS regression of equation (3.1).

ratings upwards, whereas low social anchors induce participants to give close to rational ratings.

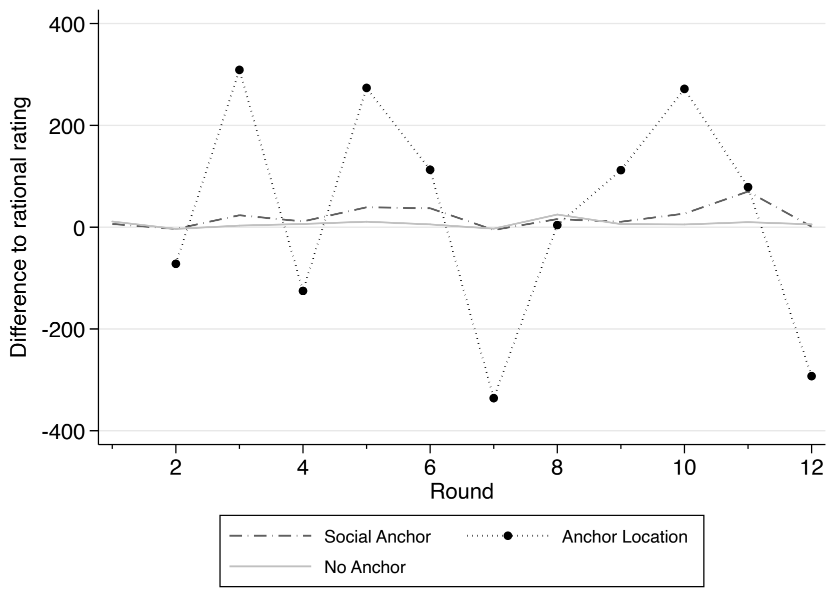


Figure 3.2: Relation of social anchor location and mean ratings.

We depict the main intuition of the described relation between the social anchor and the normalised ratings in Figure 3.2. The observed normalised ratings follow the path of the social anchors when they are high. The upwards trends do not faint when subsequent anchors fall. Only low social anchors pull ratings down close to the level of the rational predictions. We have to note that we have only a few instances of two subsequent high social anchors and no such cases for low anchors.

To compare the impact of these findings we will classify rounds according to the type of social anchors that are present. We will “mark” those rounds, where we observe only high anchors, such that for all observations $\bar{r}_{t-1} >$

Round	1	2	3	4	5	6
SA classification	No	Low	High	Low	High	High
SA norm. rating	6.200	-3.521	23.319	11.190	38.959	37.147
NA norm. rating	11.040	-3.581	3.020	6.188	10.714	5.367
Δ norm. rating	-4.840	0.060	20.299	5.002	28.245	31.780
Round	7	8	9	10	11	12
SA classification	Low	Mid	High	High	High	Low
SA norm. rating	-5.580	15.783	10.542	26.617	69.967	0.878
NA norm. rating	-3.020	24.755	5.918	5.204	9.771	5.469
Δ norm. rating	-2.560	-8.972	4.624	21.413	60.196	-4.591

Table 3.4: Classification of Social Anchoring (SA) and calculation of difference between mean normalised ratings in Social Anchoring condition and No Anchoring (NA) condition.

r_t^*, as 'High'. Likewise, we depict those as 'Low' where for all observations $\bar{r}_{t-1} < r_t^*$. If neither condition applies we will depict a round as 'Mid'. In Table 3.5 we show that by this we classify rounds 3, 5, 6, 9, 10 and 11 as 'High', rounds 2, 4, 7 and 12 as 'Low' and round 8 as 'Mid'[9], while round 1 has no classification, as there is no anchor present. Further, we calculate the difference of normalised ratings between the Social Anchor and No Anchor conditions as a first indication of the extent of the anchor deviations.

We run regressions where we use an indicator for the "marked rounds" (with a high social anchor present) as an explanatory variable and find that participants significantly overrate in exactly these marked rounds compared to the No Anchor condition. The results are shown in Table 3.5. In specification (1) we show that there is a direct impact of the social anchor in the marked rounds, compared to the unmarked rounds, captured by the significant interaction effect. There is neither a direct effect for the social anchor nor for the marked rounds. To ensure that there are no other impacts in these specifically chosen rounds, we show in specification (2) that there is no significant interaction in the Low and High Anchor conditions in the marked rounds. We conclude that our depiction captures the impact of the social anchor considerably well and that the social anchors do have an effect on ratings and can confirm our Hypothesis 1b.

Result 2: *A high social anchor distorts ratings upwards , while a low social anchor has no effect.*

Next, we explore the role of trust among the anchoring conditions. We de-

[9]Only the 8th round is depicted as Mid, with both $\bar{r}_{t-1} > r_t^*$ and $\bar{r}_{t-1} < r_t^*$ for some observations, respectively. Out of 50 observations, 27 were dropped in the 8th round since participants chose rating = group anchor.

	Dependent variable: Normalised rating $(r_{it} - r_t^*)/(q_H - q_L)$			
	(1)	(2)	(3)	(4)
Marked Rounds	0.001	0.001	-0.006***	-0.006***
	(0.018)	(0.017)	(0.001)	(0.001)
Social Anchor	-0.012	-0.012	0.007	0.008
	(0.025)	(0.025)	(0.022)	(0.022)
High Anchor		0.027		0.046***
		(0.019)		(0.015)
Low Anchor		0.009		0.028*
		(0.019)		(0.016)
No Anchor				0.020*
				(0.011)
Social Anchor × Marked Rounds	0.110***	0.110***	0.117***	0.118***
	(0.035)	(0.035)	(0.030)	(0.031)
High Anchor × Marked Rounds		0.016		0.024
		(0.028)		(0.022)
Low Anchor × Marked Rounds		0.029		0.037
		(0.032)		(0.026)
No Anchor × Marked Rounds				0.007
				(0.017)
Constant	0.038**	0.033***	0.015	0.011*
	(0.015)	(0.013)	(0.009)	(0.007)
Base Category	No Anchor	No Anchor	Slider Task	Slider Task
Observations	1097	2242	1099	2829
Number of subjects	99	197	99	246

Standard errors in parantheses. Estimation by random-effects regression clustered on subject level. Marked rounds are rounds 3, 5, 6, 9, 10 and 11. Controls are the CRT and statistical aptitude scores. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 3.5: Impact of anchors in "marked rounds".

rive a trust score which is normalised and bounded by 0 and 1. The trust score is the fraction of the sum of numerical responses provided to the three trust-related questions divided by the maximum trust value of 18. The maximum value is 18 since a 7-point Likert scale was used for each question. In Figure 3.3 we show kernel density estimations of the trust score. There is a clear indication of higher and more diversified trust scores in the Social Anchoring condition. Both in the High and Low Anchoring conditions, participants more often opted for the lowest possible scores, with 18/29 in the High Anchor condition and 16/31 in the Low Anchor condition, compared to 6/30 in the Social Anchor condition.

Trust is significantly higher in Social Anchoring ($M = .374$, $SD = .043$) compared to the High Anchor condition ($M = .192$, $SD = .048$), $t(97) = 2.853, p = .003$ and compared to the Low Anchor condition ($M = .253$, $SD = .051$), $t(97) = 1.842, p = .034$, based on one-sided t-tests.[10] There is no significant difference between the High and Low Anchor conditions based on a two-sided t-test, $t(96) = 0.878, p = .382$.

Next, we want to determine whether trust in the social anchors drives

[10]We employ one-sided t-tests as we hypothesised more trust in the Social Anchor condition. Two-sided t-tests are significant as well. All results also hold when using Wilcoxon rank-sum tests.

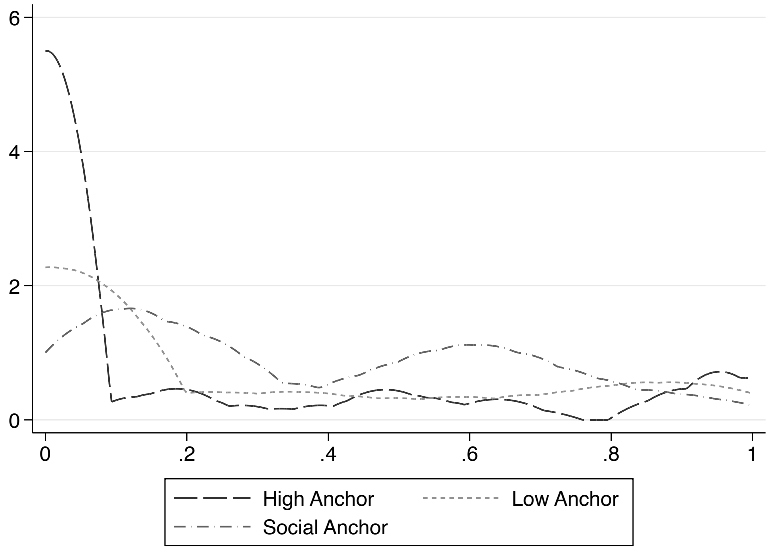


Figure 3.3: Kernel density estimation of trust score for anchoring conditions.

the anchor deviation. We extend the model in (3.1) by an interaction of the normalised individual trust score s_i with the anchoring deviation $(r_t^* - \bar{r}_{t-1})$:

$$r_{it} = \alpha r_t^* + \beta(r_t^* - \bar{r}_{t-1}) + \gamma s_i(r_t^* - \bar{r}_{t-1}) + \epsilon_{it}. \tag{3.2}$$

Similar to before we would expect $\alpha = 1$, $\beta = 0$ and $\gamma = 0$ when participants act rationally. If participants are biased by the group anchor irrespective of trust we would expect $\beta < 0$ and $\gamma = 0$. However, if this bias is amplified by trust we expect $\beta + \gamma < 0$.

In Table 3.6 we show the results of our estimation. Introducing the trust score s_i reveals, that we cannot further sustain that $\beta < 0$, while we find that $\beta + \gamma < 0$. That is, the priorly depicted anchoring deviation is predominantly driven by those who place high trust in the signal.

Result 3: *The anchor is perceived as more relevant in the Social Anchor condition compared to the High Anchor and Low Anchor conditions. Trust is explanatory for the anchoring deviation in the Social Anchor condition.*

3.5 Discussion and Conclusion

In this paper, we have presented an online experiment that studies the prevalence of anchoring effects in a rating environment. We isolated the post-purchase and post-consumption provision of ratings. As ratings are ubiquitous and have become an essential metric guiding our everyday decisions, from what products to purchase or what doctors to consult, it is important

	Dependent variable: Rating r_{it}		
	(1)	(2)	(3)
Rational rating r_t^*	1.028***	1.006***	1.027***
	(0.00887)	(0.0150)	(0.0200)
Deviation $(r_t^* - \bar{r}_{t-1})$	-0.0374	0.00292	-0.0323
	(0.0261)	(0.0535)	(0.0438)
Deviation $\times$ trust $(r_t^* - \bar{r}_{t-1})s_i$	-0.111*	-0.0553	-0.156
	(0.0609)	(0.0652)	(0.100)
F-statistic $(\beta + \gamma = 0)$	11.46***	0.98	5.95**
Prob. > F	0.0014	0.3273	0.0184
Considered observations	All	$\bar{r}_{t-1} < r_t^*$	$\bar{r}_{t-1} > r_t^*$
Observations	462	193	269
Number of subjects	50	50	50

Standard errors in parantheses. Estimation by OLS regression. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 3.6: OLS regression of equation (3.2).

to understand whether ratings are anchored by visual cues, suggested ratings, or irrelevant ratings. Anchored ratings can hamper the informativeness of ratings and can send wrong signals and diffuse inaccurate information about product quality and service quality resulting in erroneous decisions. This might result in potential welfare losses for consumers, as they purchase products they do not need, pay too much or buy products of inferior quality due to inflated ratings. Also, firms and online market platforms can suffer reputation damages.

Our online experiment focuses on non-numerical and social anchoring under market conditions such as economic incentives and repeated decision-making. The study features three different anchors, which are low, high and socially derived anchors. We uncovered significant anchoring effects. For all anchoring conditions, we observe rating inflation with the high and social anchors having the highest impact. The effect of the high anchor is significantly persistent throughout. But the anchoring effect is asymmetric as for the low anchor there is no significant anchoring effect. The socially derived anchor takes on varying roles between a high, a low and a mid-anchor and is indeed directly predictive of the observed ratings. Our overall findings do not support the notion that market conditions act as a filter for heuristics and biases as we observe anchoring effects despite economic incentives. We observe anchoring effects in a rather simple environment, that are not driven by numbers, only through visual changes. If anchoring effects are prevalent in non-numerical settings such as ours, we could expect similar anchoring effects in many real-world rating systems, especially those implemented in online markets and platforms.

When ratings provided in each anchoring condition are compared to a slider task which is almost identical to the rational benchmark, we observe significant overrating in all anchoring conditions. This is consistent with other studies that scrutinise rating behaviour under uncertainty. Clients tend to provide lenient ratings, which in turn can exacerbate the upward compression of ratings. This is also a more general pattern in rating behaviour that has been uncovered previously, with usually a large fraction of positive feedback with very few negative ratings. This pattern is also mirrored by asymmetric anchoring effects. Whereas it is possible to inflate the ratings, low anchors have no significant effects on ratings. Our study indicates that irrelevant informational cues that work as anchors can contribute to the positive skewness of ratings.

Our results show how easy it is to bias and inflate ratings, as they are prone to high anchors. Many online market platforms rely on truthful ratings as part of their marketing strategy to create and promote an accurately reflected reputation. But our results highlight that market platforms should not solely rely on ratings but also include other factors, such as the number of explicit complaints and returns. At first glance, rating inflation seems to be favourable for firms. However, upward compression of ratings makes it harder for firms to set themselves apart from their competitors through positive ratings. It also makes it more difficult to distinguish between good-quality and bad-quality offering firms. Furthermore, anchored ratings make it more difficult to use clients' ratings as feedback to assess the quality of their product.

Like de Wilde et al. (2018) we find that the social anchor is perceived as more relevant even though it does not contain any informational value in our experiment. There are two potential explanations for this. Firstly, as each participant contributes into the social anchor, this might create a notion of overconfidence where each participant overweighs their own rating which in turn increases perceived importance and trust in the social anchor. Secondly, the social anchor might create an illusion of the wisdom of crowds inducing more trust in the endogenously derived anchor. Which of these two explanations hold, is subject to future research.

Overall, our study mainly contributes to two strands of literature. Firstly, we contribute to the literature on ratings and platform design. by furthering the knowledge of the existence of anchoring effects in rating settings by providing controlled experimental evidence. Our study can help to design less error-prone rating platforms where anchoring is avoided. We also add to the literature on non-standard anchors by focusing on social and non-numerical anchors. The anchoring literature strongly focuses on numerical anchoring and in our paper, we focused on the opposite case of purely non-numerical anchoring. However, many rating environments in the real-world is are a hybrid of both, e.g., a 5-star rating system. A promising future avenue for research could be to focus on non-numerical but countable environments.

3.6 Appendix

Derivation of optimal rating r^*

A rational decision maker wants to maximize the expected utility $E(U) = A - E(|r-q|)$, where A is the maximum attainable utility, when the rating r coincides with the uniformly distributed quality $q \sim \mathcal{U}(q_L, q_H)$, where q_L is the lower bound of the quality interval and q_H is the upper bound of the quality interval. Given the linear payment rule the absolute difference between the rating and quality $|r-q|$ is subtracted from A. The decision problem is

$$\max_r E(A - |r-q|).$$

We consider three cases *i)* $r < q_L$, *ii)* $r > q_H$ and *iii)* $r \in [q_L, q_H]$. In case *i)* it is immediate that any $r < q_L$ is strictly dominated by setting $r' = q_L$, as the utility is larger by $q_L - r$ for any possible q. Similarly in case *ii)* it is immediate, that any $r > q_H$ is strictly dominated by $r'' = q_H$. The optimal rating r^* must therefore be within the interval $[q_L, q_H]$. We rewrite the expected utility accordingly:

$$E(A - |r-q|) = A - \frac{1}{q_H - q_L}\int_{q_L}^{r} r - \tilde{q}\, d\tilde{q} - \frac{1}{q_H - q_L}\int_{r}^{q_H} \tilde{q} - r\, d\tilde{q},$$

where solving the integrals yields

$$E(A - |r-q|) = A - \frac{1}{q_H - q_L}(r^2 - rq_L - rq_H + \frac{q_L^2 + q_H^2}{2}).$$

We solve the first-order condition with respect to r

$$\frac{\partial E(A - |r-q|)}{\partial r} = -\frac{2r - q_L - q_H}{q_H - q_L} \stackrel{!}{=} 0 \iff r^* = \frac{q_H + q_L}{2}.$$

The second order condition verifies that r^* is indeed a local maximiser

$$\frac{\partial^2 E(A - |r - q|)}{\partial r^2} = -2 < 0.$$

Additional figures

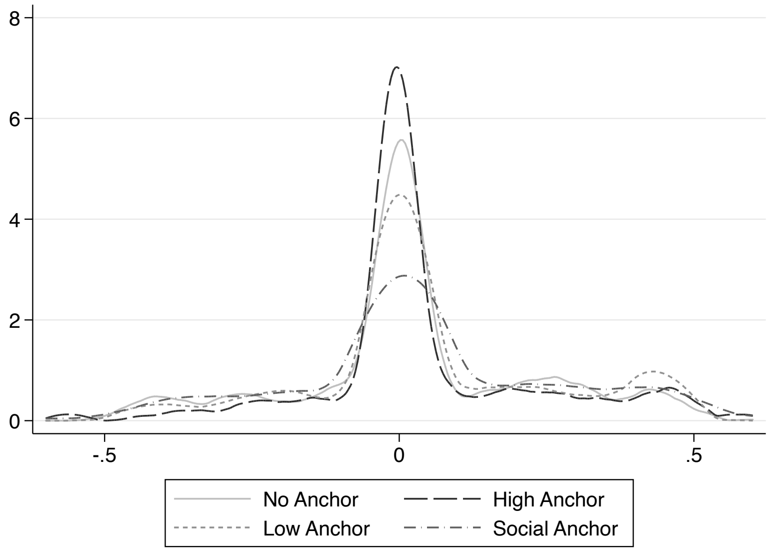


Figure 3.4: Kernel density estimation of normalised rating for experimental conditions

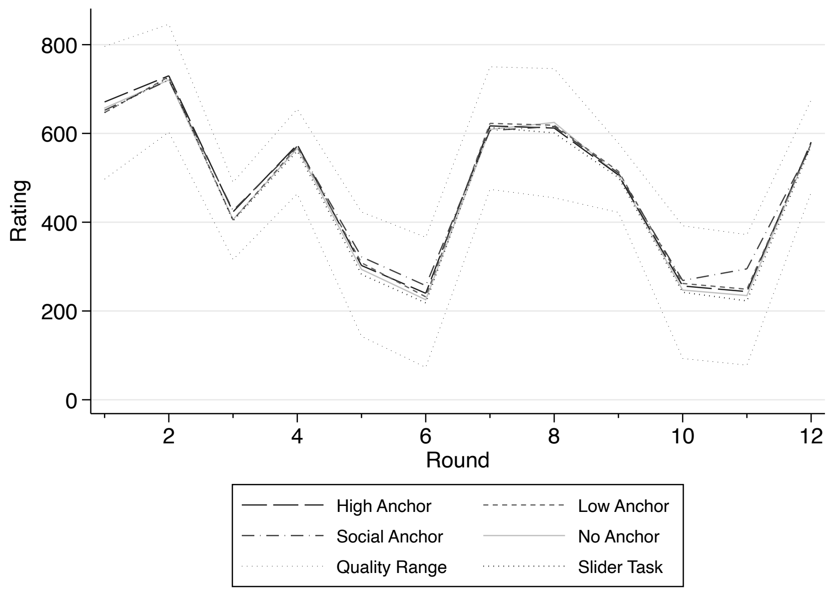


Figure 3.5: Ratings over rounds by experimental condition.

Post-experimental questionnaire

Cognitive reflection test

1. A bat and a ball cost 1.10 euros. The bat costs one euro more than the ball. How many euro cents does the ball cost? [ ]
2. 5 machines need 5 minutes to produce 5 products. How many minutes does it take 100 machines to produce 100 products? [ ]
3. The water lilies in a pond double in size every day. If after 48 days the lake is completely covered with water lilies, how many days did it take until it was half covered? [ ]

Statistical aptitude questions

1. Do you have prior knowledge of statistics? [Yes / No]
2. Do you know what an expected value is? [Yes / No]
3. Consider a fair 6-sided die. If the number rolled is a 5 or 6, you win 6 euros. If the number rolled is 4 or less, you win 3 euros. Please determine the expected profit for rolling the die once. [ ]

Perceived relevance of anchors (Only in anchoring conditions)

1. The displayed rating in each round helped me with my evaluation.

   [I do not agree ○ ○ ○ ○ ○ ○ ○ I agree completely]

2. The displayed rating in each round was informative for me.

   [I do not agree ○ ○ ○ ○ ○ ○ ○ I agree completely]

3. I based my decision on the displayed rating and this influenced my decision.

   [I do not agree ○ ○ ○ ○ ○ ○ ○ I agree completely]

Translated Instructions and Review questions

[Text in brackets was not observed by participants. Presented sliders were interactive in the digital instructions. For consecutive pictures of sliders only the first was visible to subjects, the remaining are exemplary of the interaction.]

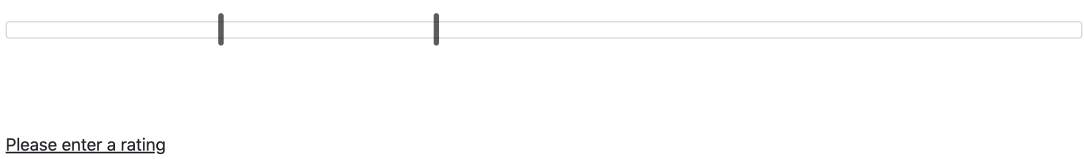
Thank you for your participation in today's experiment. Please do not communicate with other participants during the experiment. Throughout the entire duration of the experiment, please only use the experiment programme that will be displayed to you and please do not use any other programmes or applications on your computer. You can earn money in this experiment. The exact amount depends on your decisions and the other participants decisions. If you have any questions during the experiment, please use the chat function on Zoom to contact one of the experimenters.

In this experiment you will make simple decisions on your computer. All decisions will remain anonymous. This means, that you will never learn the identity of the other participants and none of the other participants will learn your identity. All monetary values will be displayed in Experimental Currency Units [ECU].

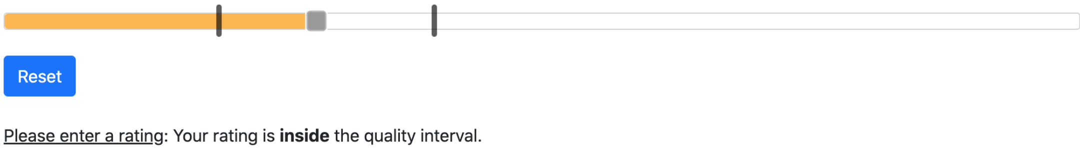
[No Anchor condition]

Your Task:
In every round of this experiment, you will be presented with a quality interval which is located on a bar. The bar represents the entire quality range. The true quality is always contained in this interval and each point within the range is equally likely. The quality is increasing from left to right along the bar.

Your task consists of rating the quality you obtain. You can provide a rating with the help of the handle on the bar. You can initialise your rating by clicking on the bar. Then you can move the handle on the bar to the left or right and bring it to the desired position. Below is an example of how to operate the handle.



[Examples of interaction]



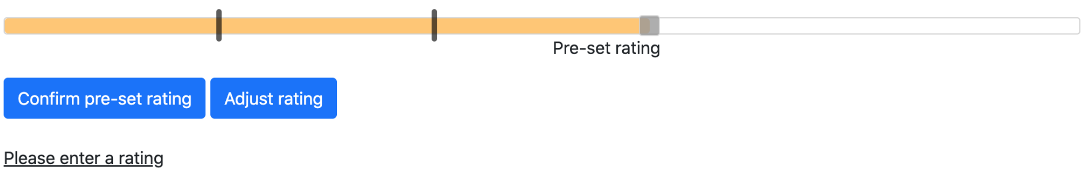
[End examples]

[High Anchor and Low Anchor conditions]

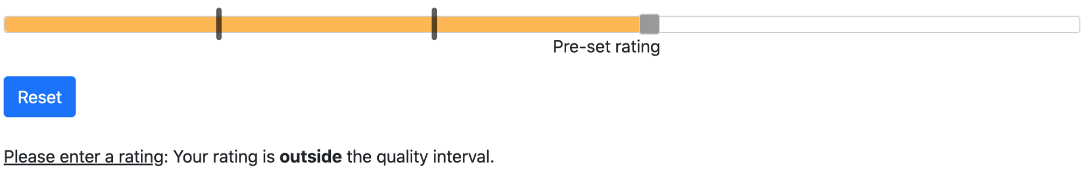
Your Task:

In every round of this experiment, you will be presented with a quality interval which is located on a bar. The bar represents the entire quality range. The true quality is always contained in this interval and each point within the range is equally likely. The quality is increasing from left to right along the bar.

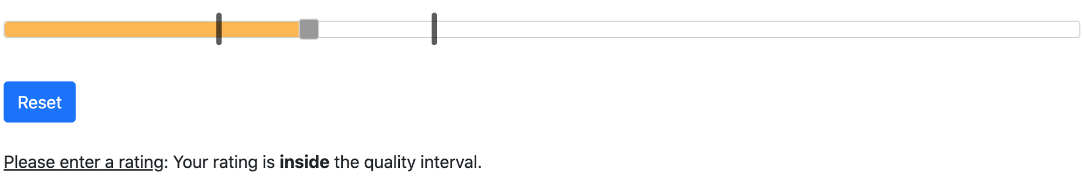
Your task consists of rating the quality you obtain. You can provide a rating with the help of the handle on the bar. First, you can choose whether you want to choose a pre-set rating or adjust the rating. If you choose the pre-set rating in a round, you cannot revoke it. If you want to adjust the rating, you must initialise your rating by clicking on the bar. Then you can move the handle on the bar to the left or right and bring it to the desired position. Below is an example of how to operate the handle.



[Example of interaction after 'confirm pre-set rating']



[Examples of interaction after 'adjust rating']

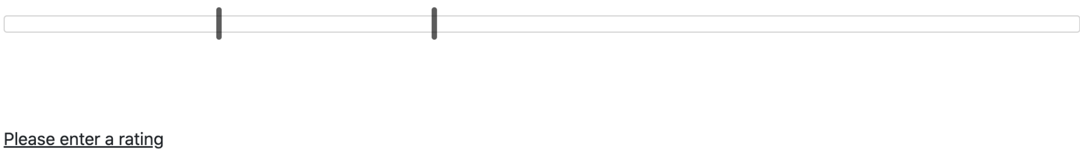


[End examples]

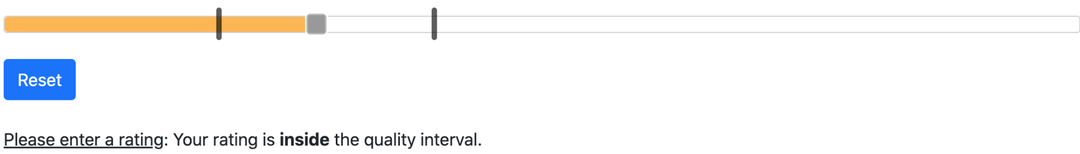
[Social Anchor condition]

Your Task:
In this experiment, you will be assigned to a group consisting of you and four other participants. This group remains the same and does not change throughout the twelve rounds of the experiment. You and all other party members are shown a quality interval on a bar each round. The true quality lies in this interval, with each value in the interval being equally likely. The quality is increasing from left to right along the bar. The quality interval and true quality are the same for all group members.

Your task consists of rating the quality you obtain. You can provide a rating with the help of the handle on the bar. You can initialise your rating by clicking on the bar. Then you can move the handle on the bar to the left or right and bring it to the desired position. Below is an example of how to operate the handle in the first round.

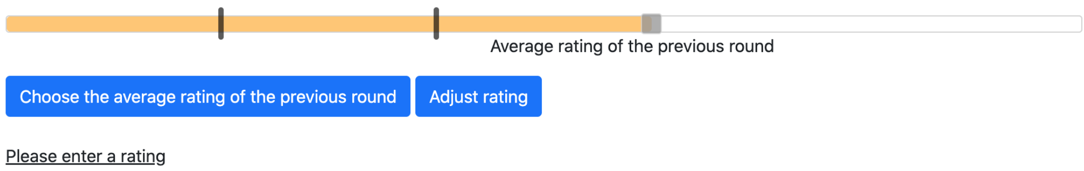


[Example of interaction]

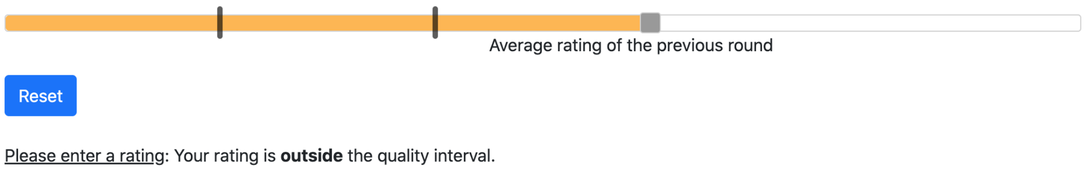


[End example]

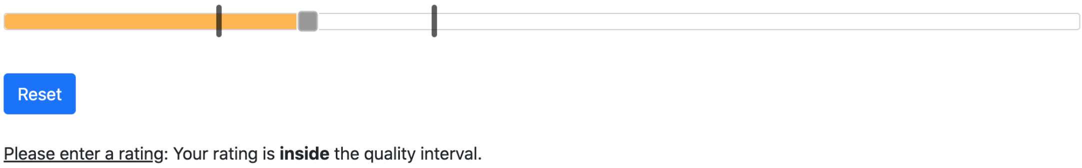
Beginning with the second round, the average rating of your group (including your rating) from the previous round will be displayed next to the quality interval. First, you must decide to either choose the average rating of the previous round or to adjust the rating. If you choose the average rating of the previous round in a round, you cannot revoke it. If you want to adjust the rating, you must initialise your rating by clicking on the bar. Then you can move the handle on the bar to the left or right and bring it to the desired position. Below is an example of how to operate the handle from the second round onwards.



[Example of interaction after 'Choose average rating of the previous round']



[Examples of interaction after 'Adjust rating']

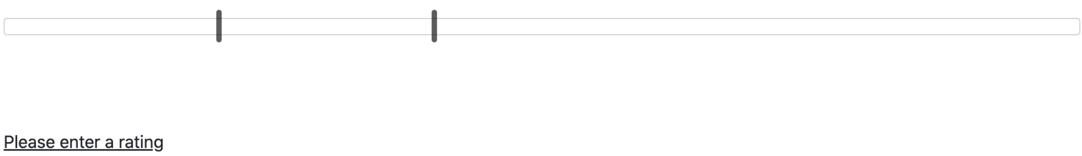


[End examples]

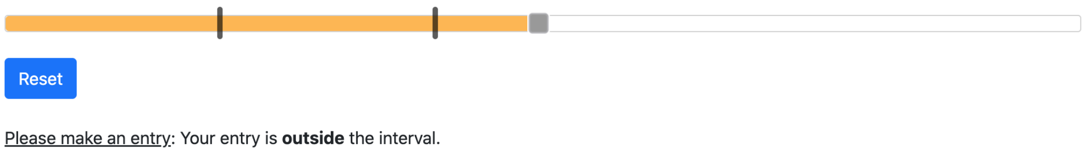
[Slider Task condition]

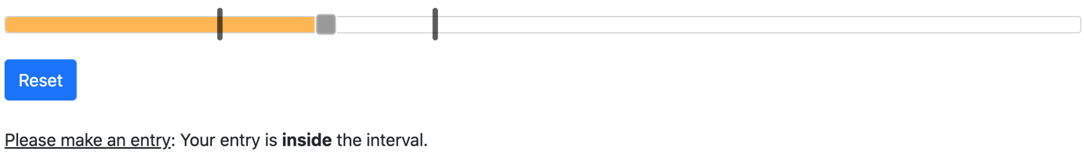
Your Task: In every round of this experiment, you will be presented with an interval which is located on a bar. The target value is exactly in the middle of the interval.

Your task consists of hitting this target value as closely as possible. You can make an entry with the help of the handle on the bar. You can initialise your entry by clicking on the bar. Then you can move the handle on the bar to the left or right and bring it to the desired position. Below is an example of how to operate the handle.



[Examples of interaction]





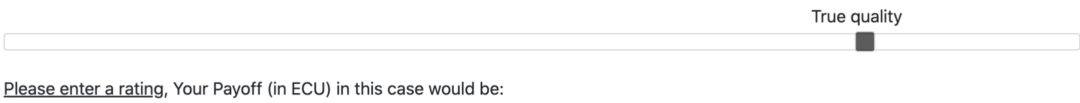
[End examples]

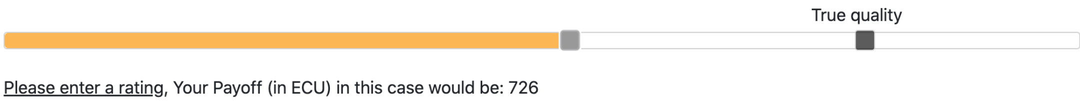
[End conditions]

[No Anchor, High Anchor, Low Anchor and Social Anchor conditions]

Your Payoff:

There are twelve rounds in total. In each round, the quality interval, and thus the true quality, is independent of the previous round. After completing the twelve rounds, a round will be chosen at random, which will determine your payoff. The amount of money in this round depends on how close you get to true quality, i.e. the closer your rating is to the true quality, the higher are your earnings in the round. Your earnings break down as follows: one thousand ECU less the deviation from the true quality. Depending on the true quality and your rating, your payoff will range from zero ECU to one thousand ECU. In the following, you find an example of your payoff depending on your rating and the true quality.

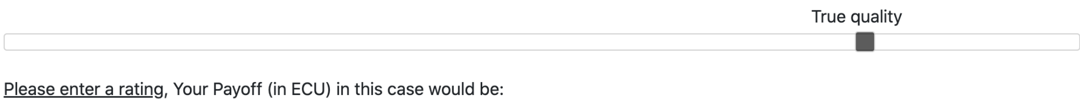


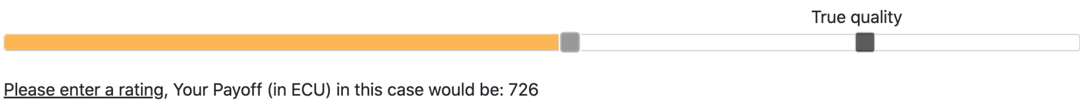


[Slider Task condition]

Your Payoff:

There are twelve rounds in total. In each round you see a new interval. After completing the twelve rounds, a round will be chosen at random, which will determine your payoff. The amount of money in this round depends on how close you get to target value, i.e. the closer your entry is to the target value, the higher are your earnings in the round. Your earnings break down as follows: one thousand ECU less the deviation from the target value. Depending on the target value and your entry, your payoff will range from zero ECU to one thousand ECU. In the following, you find an example of your payoff depending on your entry and the target value.





[End conditions]

The ECU collected during the experiment will be paid out in euros after the experiment. One hundred ECU equals one euro. For taking part in today's experiment you will also receive a participation fee of two euros. Your earnings from this experiment will be paid to you via PayPal no later than the day after the experiment.

[No Anchor, High Anchor, Low Anchor and Social Anchor conditions]
Review Questions:
1. Which of the following statements is true?

   a.) The true quality is always contained in the quality interval within the two black bars.

   b.) The true quality is not always contained in the quality interval within the two black bars.

2. Which of the following statements is true?

   a.) The further the rating is from the true quality, the higher is my payoff.

   b.) The closer the rating is to the true quality, the higher is my payoff.

3. Which of the following statements is true?

   a.) The quality intervals and the true quality are independent between rounds.

   b.) The quality intervals and the true quality are interdependent between rounds

[Slider Task condition]
Review Questions:
1. Which of the following statements is true?

   a.) The target value is always contained in the quality interval within the two black bars.

   b.) The target value is not always contained in the quality interval within the two black bars.

2. Which of the following statements is true?

   a.) The further the entry is from the target value, the higher is my payoff.

   b.) The closer the entry is to the target value, the higher is my payoff.

3. Which of the following statements is true?

   a.) The target value is always exactly in the middle of the interval.

   b.) The target value takes on a random value within the interval.

[End experimental conditions]

Chapter 4

Rebate Rules in Reward-based Crowdfunding: Introducing the Bid-cap Rule

4.1 Introduction

Cowdfunding is quickly expanding worldwide. According to (Statista 2021), the global reward-based crowdfunding market achieved \$12.27 billion in 2021, forecast to double in 2027 with an annual expected growth rate of 11%.

Crowdfunding is defined as raising capital from many people through an online platform (Agrawal et al. 2014). There exist several reasons for project creators but also investors, commonly referred to as project backers, to use crowdfunding. Project creators that have traditionally relied on other sources like banks or venture capitalists can raise funds needed directly from a wide base of backers to realise projects. Crowdfunding also provides project creators with limited access to traditional financing sources with a new channel to raise money and pursue their projects. Furthermore, crowdfunding can increase the popularity of a project and can stimulate long-term customer creation (Gerber & Hui 2013). On the backer side, investors can be part of a community, support similarly interested people or get compensation (Deb et al. 2019).

The rapid expansion of crowdfunding in many countries has given rise to multiple large-scale crowdfunding platforms such as Kickstarter, GoFundMe and Indiegogo among others. Multiple empirical studies looked and crowdfunding dynamics on these platforms. Agrawal et al. (2018) shows that funding is highly skewed on these platforms, e.g. on Kickstarter 10% of projects accounted for 63% of funds when empirical study was conducted. Investment propensity increases with capital accumulation and may lead to herding behaviour Agrawal et al. (2015). This pattern is particularly strong towards the end of a project. It slows down in the middle of the funding period due to the bystander effect which creates a perception that project funds will be raised regardless Kuppuswamy & Bayus (2018). Crosetto & Regner (2018) conclude that the success of a project only partially depends on the path. Pledges that were made early anticipate project success but lack of them does not necessarily manifest into failure.

On these platforms funding, supply and demand are matched via a mechanism. The most commonly used crowdfunding mechanisms are the all-or-nothing and the keep-it-all models. Under the all-or-nothing model project, developers get all pledges if the funding goal is reached, while all pledges are paid back otherwise. In contrast under the keep-it-all model, the project developers get all the pledges that have been accumulated during the funding time independent of whether the funding goal has been reached or not. Comparing these two model it is commonly understood that all-or-nothing is superior to the keep-it-all in attracting higher contributions and project success (see Coats et al. 2009, Cumming et al. 2020, Wash & Solomon 2014).

Similar to the conventional financial markets not all demand for funding can be satisfied on crowdfunding platforms and around 60% of projects on Kickstarter fail to reach the self-set funding goal as of August 2022.[1] While most failed projects are far away from reaching the funding goal there is a sizeable share of projects that just miss the funding goal. For these projects, a small increase in pledges would mean project success. Hence, project developers (as well as crowdfunding platforms) want to find ways to either reach new backers or increase the pledges of existing backers if the number of backers is exhausted. There have been several studies focusing on ways of increasing the number of backers and the coordination between backers, e.g. by encouraging early contributions (Ansink et al. 2017, Solomon et al. 2015) as initial backing of a project influences its success (Colombo et al. 2015), the dissemination of positive opinions (Comeig et al. 2020, Courtney et al. 2017, Petitjean 2018), highlighting specific projects (Corazzini et al. 2015) and the timing of promotions (Li & Wang 2019). However, the case in which a fixed number of backers can be induced to increase their pledges has not been thoroughly studied in the crowdfunding literature. In this case raising the funds to reach the funding goal can be viewed as a residual threshold public good game among all backers who are investing. A potential way to increase contributions and project success rate identified in the threshold public good lit-

[1] See `https://www.kickstarter.com/help/stats?ref=global-footer` for more information

erature is rebating contributions that exceed the provision point back to contributors (see Marks & Croson 1998, Rondeau et al. 1999, Spencer et al. 2009).

In this chapter, we focus on reward-based crowdfunding, where project backers receive direct non-monetary rewards for their contribution to a project, if their contribution exceeds pre-set entry fee.[2] To check whether rebating excess funds also increase pledges and the chance for project success in reward-based crowdfunding we adapt the proportional rebate rule to a reward-based crowdfunding framework. Additionally, we develop a new rebate scheme which we call the bid-cap rule. The bid-cap rule sets an ex-post limit on bids such that the funding goal is exactly reached. Pledges above this limit are reduced to the cap. The bid-cap rule induces less variance in final payments compared to the proportional rebate rule for fixed bids. We then experimentally test whether the bid-cap and proportional rebate rules actually induce higher pledges and a greater success rate compared to the customary all-or-nothing rebate model.[3] With our experiment we show that both rebate rules largely increase contributions and the project realisation rate compared to the all-or-nothing model, while we can confirm that the variance of final payments is lower under the bid-cap rule compared to the proportional rebate rule. We contribute to the crowdfunding literature and to the general literature concerning threshold public goods by introducing the new bid-cap rebate rule and by adapting the proportional rebate rule to a reward-based crowdfunding framework.

The next section provides a review of the relevant literature followed by a theoretical comparison of the bid-cap rule to the proportional rebate rule and the all-or-nothing model in Section 4.3. Section 4.4 outlines the experimental design and hypotheses. Section 4.5 presents the experimental results. The chapter is concluded with a discussion, which includes implications for crowdfunding platforms, project creators and investors.

4.2 Literature Review

In recent decades, several mechanisms have been developed and investigated to mitigate free riding in the private provision of public goodsto mitigate free riding in the private provision of public goods. The provision point mechanism was introduced into the literature by Palfrey & Rosenthal (1984) and

[2]Such rewards can take on various forms such as early access to a product, a limited version of a product or some forms of individualisation, like signed or otherwise customised products.

[3]As a baseline we only consider the all-or-nothing model since nearly all crowdfunding platforms offering reward-based crowdfunding use this model while the keep-it-all model is mainly used in other crowdfunding contexts.

Bagnoli & Lipman (1989).

Bagnoli & Lipman (1989) illustrate that in a threshold public goods game with simultaneous voluntary contributions there exists an efficient equilibrium when a full refund is offered in case of total contributions not reaching the threshold level. Coats et al. (2009) compare simultaneous and sequential contributions when a full refund is issued to the contributors in case the provision point is not reached. They find that introducing a refund increases efficiency when contributions are made simultaneously. For the sequential mechanism, public goods are funded more frequently and efficiently without a refund.

Smith (1980) originally proposed a proportional rebate rule in public good auctions. Marks & Croson (1998) and Rondeau et al. (1999) introduced this rebate rule in provision point mechanisms, by rebating excess contributions proportionally back to contributors when the provision point was not met. Marks & Croson (1998) compare contributions in the presence and absence of proportional rebates and an extended benefit provision rule. They find that under rebate rules similar contributions are obtained as under no rebate rules and that contributions were the highest with extended benefits. Rondeau et al. (1999) show that a provision point mechanism with refunds and rebates can be empirically demand-revealing. Besides the proportional rebate rule, Spencer et al. (2009) consider five alternative rebate rules including variations of lottery-like winner-take-all rules and random rebate rules. They find that for all rules total contributions equal total benefits or exceed them.

In more recent laboratory experiments, Cason & Zubrickas (2017) and Cason & Zubrickas (2019) find that contributors respond to incentives induced by refund bonuses in line with predictions and that refund bonuses can increase the rate of project realisation substantially. Cason et al. (2021) scrutinise the dynamics of funding by focusing on refund bonuses that are only rewarded to early contributors in case of fundraising failure. They find that offering refund bonuses to early contributors only works as well as offering the bonus to every contributor. However, since refund bonuses are granted upon project failure it remains to be clarified who would pay for them. Project owners may not have the funds, otherwise, they might not use crowdfunding as a financing tool in the first place.

4.3 The Game

Consider N individuals, where each individual $i \in \{1, \ldots, N\}$ has an endowment of E_i. Each individual can decide on a bid $b_i \in [0, E_i]$ that they pledge towards the completion of a project. If the total investment by all individuals $\sum b_i$ weakly exceeds an endogenously determined provision point PP the project is realised.[4] If the sum of total investment is short of PP each individual gets back their investment b_i and pays 0. In order to capture the nature of reward-based crowdfunding an individual receives their valuation v_i from the realised project if and only if they invest more than a reservation price of r. Reservation prices are commonly observed on big crowdfunding platforms such as Kickstarter, Indiegogo and StartNext, where project creators post a minimal price which individuals have to pledge to receive the good but are generally allowed to pledge more or less than this reservation price. Hence, if the project is realised but an individual i has invested $b_i < r$ they will not receive their valuation from the project, while all individuals with $b_i \geq r$ will receive their valuation v_i from the project. Hence, bids below r are collected and are considered donations towards the completion of the project. In the following, we will refer to individuals contributing at least r as investors, where the number of investors is $n \leq N$ and individuals with bids $b_i \in [0, r)$ as donors, where the number of donors is $N - n$.[5]

We assume that $\sum v_i > PP$ such that the good is socially desirable as its total benefits exceed the provision cost. Further, we will focus on the cases where $PP > N \cdot r$. When $PP \leq N \cdot r$ all mechanisms trivially succeed, hence the assumption is not crucial for our results.[6] In this chapter, however, we want our projects to have a public good character (and focus on the more interesting cases). This is why we are only focusing on cases where $PP > N \cdot r$.

In the case where the total contribution exceeds the provision point the excess investment may be reallocated back to the investors or may be kept in the project yielding no additional benefit to individuals. The latter corresponds to the all-or-nothing model. We further consider the case where the excess contribution is rebated to investors proportional to their investment exceeding the reservation price. Additionally, we consider a rebate scheme we refer to as bid-cap. Details for these rebate rules as well as the individual payoff function according to these rules are explained below.

All-or-nothing

Since excess investments above the PP are not rebated to investors the indi-

[4]Throughout $\sum$ always implies $\sum_{i=1}^{N}$ if not stated otherwise.

[5]For ease of notation we include individuals who pledge 0, i.e. non-contributors, in the donors. More generally we include donors for completion.

[6]The assumption assumption $PP > N \cdot r$ means that when everybody contributes the reservation price r, this is not enough to cover the project costs. In reality, other cases are of course possible such as $PP \leq N \cdot r$.

vidual payoff π_i under the all-or-nothing model is given by:

$$\pi_i = \begin{cases} E_i - b_i + v_i & \text{if} \quad \sum b_i \geq PP \text{ and } b_i \geq r \\ E_i - b_i & \text{if} \quad \sum b_i \geq PP \text{ and } b_i < r \\ E_i & \text{if} \quad \sum b_i < PP \end{cases}. \tag{4.1}$$

The marginal penalty associated with over-contribution is[7]

$$\frac{\partial \pi_i}{\partial b_i} = -1, \tag{4.2}$$

meaning that every dollar invested above the provision point is reducing individual i's payoff by exactly this dollar.

Proportional rebate

In contrast to Rondeau et al. (1999), Marks & Croson (1998) and Spencer et al. (2009), who chose to rebate proportional to the total bid, we choose to rebate excess investment proportional to the difference of bid and reservation price $e_i := \max\{0, b_i - r\}$. This prevents situations in which investors pay less than the reservation price after receiving the rebate.[8] Thus, the individual payoff π_i is:

$$\pi_i = \begin{cases} E_i - b_i + v_i + \frac{e_i}{\sum e_i}\left(\sum b_i - PP\right) & \text{if} \quad \sum b_i \geq PP \text{ and } b_i \geq r \\ E_i - b_i & \text{if} \quad \sum b_i \geq PP \text{ and } b_i < r \\ E_i & \text{if} \quad \sum b_i < PP \end{cases}. \tag{4.3}$$

The marginal penalty associated with over-contribution is

$$\frac{\partial \pi_i}{\partial b_i} = \begin{cases} -1 + \frac{\left(\sum b_i - PP\right)\left(\sum e_i - e_i\right) + \left(e_i \sum e_i\right)}{\left(\sum e_i\right)^2} & \text{if} \quad b_i \geq r \\ -1 & \text{if} \quad b_i < r \end{cases}. \tag{4.4}$$

Note that the marginal penalty associated with over-contribution under the proportional rebate rule is in absolute terms weakly smaller than under the all-or-nothing model. This is the case since, given that the good is provided, $\sum b_i - PP \geq 0$, $\sum e_i - e_i \geq 0$ and $e_i \sum e_i > 0$ so that the second term in the first case must be positive or zero. In Appendix 4.7.1 we further show that the second term in the first case is smaller than one such that the penalty of marginally increasing the bid is strictly negative.

[7] Following the literature we define over-contribution as $\sum b_i > PP$.

[8] Often the reservation price is set at marginal cost whereby a company would inquire a loss on investors paying less than r.

Bid-cap

Consider ordered bids by all individuals $(b_1, \ldots, b_N)$ with $b_1 \leq b_2 \leq \ldots \leq b_N$. In the case where total investment exceeds the provision point we find a cut-off bid $\bar{b} > r$[9] such that $\sum_{i=1}^{k} b_i + (N-k)\bar{b} = PP$, where $\bar{b} \in [b_k, b_{k+1})$ is the highest payment any investor has to make and k is the number of investors, who pay their full bid.[10] Hence, $N-k$ investors pay the cut-off bid $\bar{b}$. The individual payoff π_i is given as

$$\pi_i = \begin{cases} E_i - b_i + v_i + (b_i - \bar{b}) & \text{if} \quad \sum b_i \geq PP \text{ and } b_i \geq \bar{b} \\ E_i - b_i + v_i & \text{if} \quad \sum b_i \geq PP \text{ and } b_i \in [r, \bar{b}) \\ E_i - b_i & \text{if} \quad \sum b_i \geq PP \text{ and } b_i < r \\ E_i & \text{if} \quad \sum b_i < PP \end{cases} . \tag{4.5}$$

The marginal penalty associated with over-contribution is

$$\frac{\partial \pi_i}{\partial b_i} = \begin{cases} 0 & \text{if} \quad b_i \geq \bar{b} \\ -1 & \text{if} \quad b_i < \bar{b} \end{cases} . \tag{4.6}$$

In Appendix 4.7.2 we show that a solution $(k, \bar{b})$ to this must always exist and in Appendix 4.7.3 we further show that this solution is unique we further show that for any sequence of bids $(k, \bar{b})$ is uniquely determined.

Note that as under the proportional rebate rule the marginal penalty of over-contribution is again weakly smaller in absolute terms than under the all-or-nothing model. The marginal penalty of over-contribution under the bid-cap rule is weakly smaller in absolute terms than under the proportional rebate rule if $b_i \geq \bar{b}$ and absolutely greater if $b_i < \bar{b}$.

Comparison of rebate rules

Since individuals' bids are likely to be influenced by the respective rebate rule we must limit the comparison of the proportional rebate rule and the bid-cap rule to the assumption that they do not induce different bidding behaviour. That is, we consider a fixed sequence of ordered bids $(b_1, \ldots, b_N)$ with $\sum b_i > PP$ and compare who wins (loses) under the bid-cap rule compared to the proportional rebate rule. In the proportional rebate rule every individual who pledges more than the reservation price, r gets a rebate. In contrast, in the bid-cap rule only individuals $i \in \{k+1, \ldots, N\}$, i.e. individuals who pledged

[9]Note, that the condition $\bar{b} > r$ must be necessarily fulfilled since we only consider cases where $PP > N \cdot r$.

[10]For the special case of $k = 0$ we have $r \leq \bar{b} < b_1 \leq \ldots \leq b_N$ where everything that follows holds as well.

more than $\bar{b}$, get a rebate. Since the total amount of rebates cannot change, it follows that individuals with high (low) bids must be better (worse) off under the bid-cap rule compared to the proportional rebate rule.

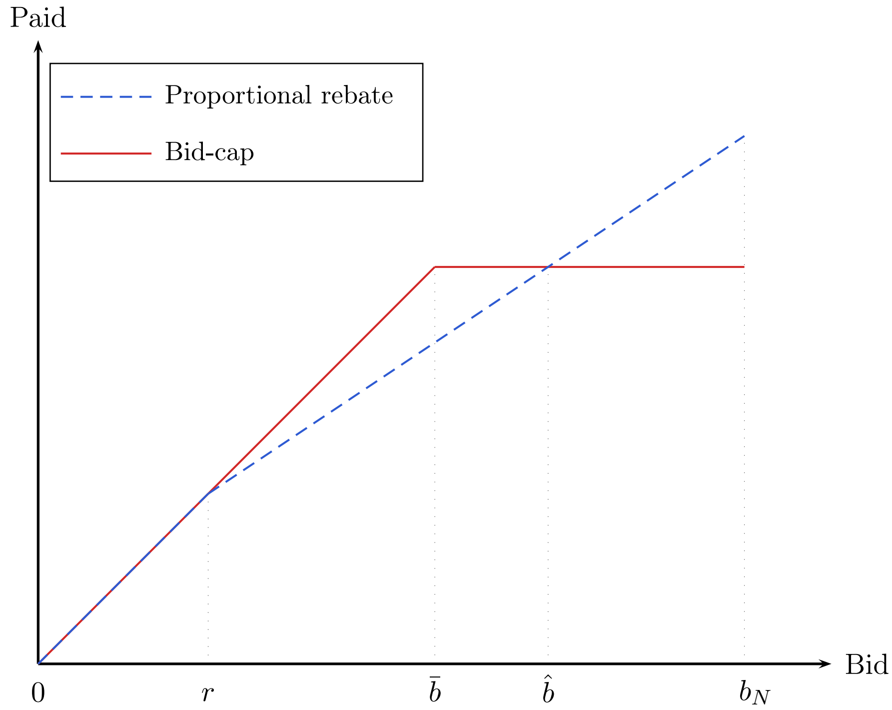


Figure 4.1: An example of payments by bid under rebate rules.

In fact, we find that the relation of payments is as shown in Figure 4.1, where all individuals below a cut-off bid $\hat{b} \in (\bar{b}, b_N)$ are better off under the proportional rebate rule and all individuals with bids above the cut-off are better off under the bid-cap rule. Individuals bidding close to (or equal to) $\bar{b}$ are worst off under the bid-cap rule compared to the proportional rebate rule. We show these properties formally in 4.7.4. A direct consequence of these findings is that the variance of final payments is lower under the bid-cap rule compared to the proportional rebate rule for any given sequence of bids that in sum exceed the provision point.

4.4 The Experiment

4.4.1 Experimental Design

Our experiment consists of three experimental conditions: all-or-nothing, bid-cap and proportional rebate. We chose the parameters in the experiment such that the number of players, aggregate benefits to costs ratio and share of aggregate endowment necessary for project realisation are in line with past work on threshold public goods (Croson & Marks 2000). Overall, we recruited 60 participants, 20 participants for each experimental condition. Each condition consists of two parts and is structured as follows. Participants were randomly allocated to a computer upon entry to the lab. First, participants read the instructions for the first part and were able to ask questions to ensure comprehension. The instructions for the first part stated that the participants play the game described in Section 4.3 once. Further, it is mentioned that the experiment includes a second part but not what task will be done in the second part. The second part's task was revealed in separate instructions that were provided after the first part has elapsed. Since participants did not know that they were playing the same game in the second part again, we interpret the first round as behaviour in a one shot game.

The participants are assigned to groups comprising $N = 10$ participants that remain the same throughout the experiment. Each participant is endowed with $E_i = 65$ experimental currency units (ECU) and can invest out of this endowment towards a project. The project will only be realised if the provision point of $PP = 300$ ECU is reached. We follow Spencer et al. (2009) by referring to the provision point as "investment costs" in the instructions. In contrast to Spencer et al. (2009) we inform participants about the provision point ex-ante since in crowdfunding the provision goal is most commonly featured in the projects' descriptions. If a participant invests at least the reservation price of $r = 15$ ECU then this participant becomes an investor and can obtain a payout from project realisation. Investments below 15 ECU are just seen as a donation and do not entitle a participant for the payout. If the total investments made are below the provision point of 300 ECU then each participant is refunded their invested amount. If the project is successfully funded, then each investor receives their valuation v_i of 45 ECU. This means each investor has the same project valuation. If the total investments exceed the provision point of 300 ECU then the excess amount is rebated to the investors in the bid-cap rule and proportional rebate rule conditions according to the rebate rule applied in each experimental condition. In the all-or-nothing model condition, the excess amount is not rebated.

After the first part has ended, each participant receives instructions for the repeated version of the game. The second part is almost identical to the first part with two major changes. Now the game is repeated for 10 periods and the value that an investor ascribes to the project is heterogeneous. The valuation is a whole number that is drawn from a uniform distribution with support $[30, 60]$. Thereby, the minimum aggregate project value for a group is 300 ECU which covers the investment costs. The actual realised minimum aggregate project value was 409 ECU. The final payoff for the second part equals the payoff obtained in a randomly drawn round where each round is equally likely to be drawn.

We would like to illustrate the workings of the bid-cap rule in more detail through an example as it is a novel rebate rule. Consider N=10 with following bids

Player	A	B	C	D	E	F	G	H	I	J
Investment	0	7	14	21	28	35	42	49	56	63

Based on these investments D, E, F, G, H, I, and J are Investors whereas A, B and C are not. Consequently, the investments of A, B and C are merely considered donations. After subtracting the donations of A, B and C $0+7+14 = 21$ *ECU* from the investment costs, only 279 *ECU* are needed in order to realise the project.

Now it is checked whether 279 *ECU* can be covered if all investors make the lowest investment of 21 *ECU*. As $21 \cdot 7 = 147 < 279$, this does not cover the costs. D pays 21 *ECU* and it is checked whether 279 *ECU* can be covered if all other investors E, F, G, H, I, and J each make the second-lowest investment of 28 *ECU*. As $6 \cdot 28 + 21 = 189 < 279$ the costs are not covered. This process continues until Investor **I** is reached. In this case, all investors pay their invested amounts and I and J pay 56 *ECU* each. This results in a total investment of $21 + 28 + 35 + 42 + 49 + 2 \cdot 56 = 287$ ECU which covers the investment cost minus the donations. Therefore, J pays 56 *ECU* instead of 63 *ECU*.
These investments generate excess investments of 8 *ECU* (287-279=8 *ECU*). The 8 *ECU* are distributed equally between I and J, so I and J receive additional excess payments of 4 *ECU*.

In the end, all investors pay their invested amounts except for I and J, who pay less than their suggested investments as their entire investment is not needed to cover the investment costs. J pays 56 *ECU* (instead of 63 *ECU*). Additionally, I and J are being rebated 4 *ECU* each. The **paid amount** of J is 63 *ECU* minus 7 *ECU*, as J's investment is reduced to I's investment, minus 4 *ECU*. as J receives a rebate of 4 *ECU* out of the excess investments.

The following table summarises the suggested investments and the paid amounts for all investors.

Player	A	B	C	D	E	F	G	H	I	J
Investment	0	7	14	21	28	35	42	49	56	63
Paid amount	0	7	14	21	28	35	42	49	52	52

The experiment was conducted in June 2022 and was programmed with zTree (Fischbacher 2007). Participants were recruited with ORSEE (Greiner 2015). The neutrally framed experimental instructions (see Appendix) were identical for all participants. Sessions lasted around 30 minutes. Participants earned 10.89 EUR on average including a show-up fee of 5 EUR.

4.4.2 Hypotheses

All three rules underlying the experimental conditions share the same efficient Nash equilibria, which are any combinations of individually rational bids ($b_i \leq v_i$) that exactly sum up to the provision point. Nonetheless, the proportional rebate rule and the bid-cap rule better the outcome of some (or all) individuals in the off-path cases, where the sum of bids strictly exceeds the provision point. Due to this, higher bids come with lower risk in the rebate rule conditions, as also indicated by the lower marginal damage for all bids above the reservation price ($b_i > r$) under the proportional rebate rule, as shown in (4.4) and for all bids above the bid-cap ($b_i > \bar{b}$) under the bid-cap rule as shown in (4.6). This leads to our first hypothesis.

Hypothesis 1: *The bids will be higher in the rebate rule conditions compared to the all-or-nothing model condition.*

In consequence of Hypothesis 1, we expect that the bids are sufficiently increased in the rebate rule conditions compared to the all-or-nothing model condition to positively affect the probability of a project realisation, yielding our second hypothesis.

Hypothesis 2: *The project realisation rates will be higher in the rebate rule conditions compared to the all-or-nothing model condition.*

The mean payments are $PP/N = 30$ by design in both rebate rule conditions. Hence, we can only expect differences in the distributions of final payments. Given that the bid-cap rule only reduces high bids ($b_i > \bar{b}$), but does not impact low bids ($b_i \leq \bar{b}$), while the proportional rebate rule reduces all bids above the reservation price ($b_i > r$) and assuming that bidding behaviour is not too dissimilar between the rebate rule conditions we arrive at our third hypothesis.

Hypothesis 3: *The variance of payments will be smaller under the bid-cap rule compared to the proportional rebate rule.*

This Hypothesis requires that bidding behaviour is not too different between the two rebate rule conditions. A possible behavioural conjecture would be that the bid-cap rule both induces higher bids among contributing individuals, i.e. individuals who try to guarantee the realisation of the project and lower bids among free-riding individuals, i.e. individuals who try to maximise their own payoff with little regard for the realisation of the project. In this case, the differing bidding behaviour could even lead to the contrary result of more variance in payments under the bid-cap rule compared to the proportional rebate rule or to a null result.

4.5 Results

In Table 4.1 we summarise descriptive statistics by experimental condition. Notably, the bids are on average larger than the efficient symmetric equilibrium prediction of 30 under both rebate rules, while they are below 30 under the all-or-nothing model. By design, the mean payments are exactly 30 under both rebate rules. Under the all-or-nothing model, less than 10% of the projects were funded, while 72% (55%) of the projects were funded under the proportional rebate rule (bid-cap rule). Lastly, we find that under the proportional rebate rule participants pledge more than their valuation about twice as often as under the other two rules. In contrast to the all-or-nothing model and the bid-cap rule, we find that there is a notable amount of maximum bids of 65 under the proportional rebate rule.

	AoN	Proportional	Bid-cap
Means:			
Bid b_i	26.07[a]	32.07	30.41
	(11.80)	(13.45)	(11.67)
Demand revelation b_i/v_i	0.57[b]	0.71[b]	0.67[b]
	(0.24)	(0.27)	(0.24)
Payment when project funded	33.15	30	30
	(15.63)	(10.36)	(8.94)
Shares:			
Projects funded	0.09	0.72	0.55
Bids over valuation $b_i > v_i$	0.04	0.10	0.05
Bids below reservation price $b_i < r$	0.04	0.04	0.02

[a]Significantly different from equilibrium prediction of 30, see Table 4.3 in the Appendix.
[b]Bids are significantly different from valuation, see Table 4.4 in the Appendix.

Table 4.1: Descriptive statistics of Part 1 and Part 2 pooled by experimental condition with standard deviations in brackets.

In Figure 4.2 we show a mapping from bids to payments in both rebate rule conditions. We also add fitted predictions based on our observations. For the fit of the proportional rebate rule condition, we ran a regression of payments on bids for bids $b_i \in [15, 65]$ with suppressed constant term to determine the slope, while payments are equal to the bids for $b_i < 15$ by the design of the rebate rules. For the fit of the bid-cap rule condition, we calculated the mean $\bar{b}$ for funded projects. This constant is the payment for every bid $b_i \geq \bar{b}$, while payments are equal to the bids for $b_i < \bar{b}$ by the design of the rule.

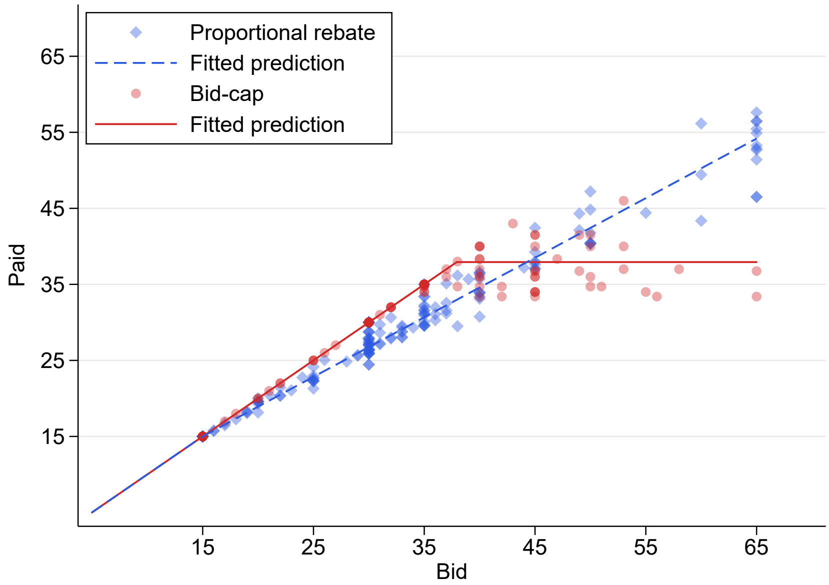


Figure 4.2: Bid to paid mappings of funded projects.

In Table 4.3 in the Appendix, we normalise the bids to the equilibrium prediction by subtracting 30 from each bid and run regressions on the constant remainder. We find that participants pledge significantly less than in equilibrium under the all-or-nothing model in both parts of our experiment. We do not find a significant effect in either direction in both of our rebate rule conditions. Based on Spearman's rank correlation coefficients we find that bids are correlated with the drawn valuations in all experimental conditions; all-or-nothing: Spearman's rho $= 0.43$, $p < 0.001$; proportional rebate: Spearman's rho $= 0.48$, $p < 0.001$; bid-cap: Spearman's rho $= 0.48$, $p < 0.001$. As already indicated by demand revelations below 1 and despite these correlations, we find that participants bid significantly below their valuation in all experimental conditions and both parts of the experiment, as seen in Table 4.4 in the Appendix. There we run the same regressions as before, albeit normalising the bids at the individual valuation instead of the mean equilibrium bids. This result stands in contrast to the findings of Rondeau et al. (1999) and

Spencer et al. (2009), who find demand revelation close to one, i.e. that bids were equal to valuations, in pure public good settings. A potential reason for this difference is that Rondeau et al. (1999) and Spencer et al. (2009) did not provide information on the exact provision point but only on the distribution, it is drawn from.

Next, we test for treatment effects on bids and the projects' realisation rates. We show the results in Table 4.2. For both parts of our experiment, we find that bids are significantly larger under both rebate rules compared to the all-or-nothing model, while there is no significant difference between the two rebate rules. Similarly, projects were significantly more likely to be funded under a rebate rule, while again there is no significant difference between rebate rule conditions in the project realisation rate.[11] Given these results, we can confirm our first and second Hypotheses. These findings stand in contrast to Marks & Croson (1998), who found no difference in contributions and project realisation rates between a no rebate and a proportional rebate rule condition in a threshold public good setting. The most notable difference between their and our experiment is that they provided feedback between rounds, introducing reputation effects and punishment opportunities.

	Part 1 (One round)	Part 2 (Ten rounds)	
	$b_i \in [0, 65]$	$b_i \in [0, 65]$	Funded $\in \{0, 1\}$
Proportional rebate	5.10**	6.08***	0.65***
	(2.347)	(2.280)	(0.142)
Bid-cap	6.25*	4.14*	0.45**
	(3.162)	(2.280)	(0.212)
Constant	23.80***	26.31***	0.10
	(2.028)	(1.546)	(0.079)
Level of observations	Subject	Subject	Group
Number of observations	60	600	60
Postestimation Wald tests to compare proportional rebate and bid-cap conditions:			
H_0: Proportional rebate = bid-cap	$p = 0.67$	$p = 0.41$	$p = 0.38$

Standard errors in parentheses. Estimation by OLS regression with robust standard errors for Part 1 and estimation by random-effects regression with clustering on level of observations for Part 2. The baseline category is All-or-nothing in all specifications. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 4.2: Analysis of treatment effects on bids and the successful realisation of projects.

Lastly, we compare the bidding patterns and final payments for funded projects between the two rebate rule conditions. In Figure 4.3 in the Appendix, we show the cumulative distribution and a kernel density estimation of the bids by experimental condition and in Figure 4.4 in the Appendix,

[11] In part one 0/2 projects were funded under the all-or-nothing model, while 1/2 projects were funded under each rebate rule, respectively. Including part one in the estimation does not change the results.

we show the cumulative distribution of bids and payments for funded projects.[12] Using Kolmogorov-Smirnov tests we find that there is no significant difference in distributions of bids of funded projects between rebate rule conditions, $D(280) = 0.0958$, $p = 0.56$, while there is a significant difference in distributions of payments of funded projects between rebate rule conditions, $D(280) = 0.30$, $p < 0.01$. Similarly, using variance-comparison tests we find that there is no significant difference in the standard deviation of bids of funded projects, $F(159, 119) = 1.10$, $p = 0.60$ (two-sided F-Test), while the standard deviation of payments of funded projects is significantly lower under the bid-cap rule compared to the proportional rebate rule, $F(159, 119) = 1.34$, $p < 0.05$ (one-sided F-test)[13]. We find that for sufficiently similar bidding behaviour, the bid-cap rule leads to narrower distributed payments than under the proportional rebate rule. Therefore, we can confirm our third Hypothesis.

4.6 Conclusion

In this study, we derive two rebate rules for reward-based crowdfunding and compare their theoretical properties to each other and to the widely applied all-or-nothing model. Both rebate rules achieve efficient outcomes whenever the sum of bids weakly exceeds the provision point, which has to be met for project realisation while the all-or-nothing model yields efficient outcomes only if the provision point is exactly met.

Applying all three rules in a laboratory experiment we conclude that both rebate rules greatly increase the bids and project realisation rate compared to the all-or-nothing model. In line with its theoretical properties and since bidding behaviour is similar under both rebate rules we observe that the bid-cap rule induces less variance in the payments compared to the proportional rebate rule. In the bid-cap rule high (low) bidders pay less (more) than under the proportional rebate rule.

Since projects are realised more often if excess investment is rebated it seems advisable for crowdfunding platforms to offer some kind of rebate rule. However, we cannot give clear guidance on which rebate rule to implement. While we observed (insignificantly) more project realisations under the proportional rebate rule, we also observed more occurrences of bids over valuation. A potential reason might be that participants misunderstood the proportional rebate rule and erroneously tried to wager on high rebates, even

[12] In Figure 4.5 in the Appendix we show according kernel density estimations of bids and payments for funded projects.

[13] Since our Hypothesis is directed, we use a one-sided test. In the corresponding two-sided F-test the p-value is 0.09.

though this could not increase gains and might in fact even lead to losses. Hence, the proportional rebate rule might be preferred by project creators but not by the crowdfunding platform and investors since it might induce over-investment. On the other hand, the bid-cap rule might be preferred by investors who are concerned about fairness in terms of final payments, since final payments exhibit less variance under the bid-cap rule.

A caveat for these implications is that on crowdfunding platforms project creators can endogenously determine the provision point and reservation price. The creators might increase the provision point when they offer rebate rules as they cannot keep the excess investments. If this is the case the positive effect of rebate rules might be diminished.

We focus on cases where the provision point cannot be met when all individuals pledge the reservation price, yielding a residual public good game. For future research one could extend the present study by introducing uncertainty in the number of individuals who participate in the crowdfunding game, such that it is unclear whether a residual public good game arises or not. Uncertainty in the number of individuals is equivalent to an uncertain provision point as in Rondeau et al. (1999) and Spencer et al. (2009). It would be interesting to test whether the bid-cap rule extends to this situation similarly to the proportional rebate rule in that demand revelation increases. Also, in line with most crowdfunding applications, the rules could be extended to allow for different tiers of rewards. Lastly, it would be interesting to apply rebate rules in field experiments, using actual crowdfunding services.

4.7 Appendix

4.7.1 Proof of negative marginal penalty of over-contribution

For the marginal damage to be negative it remains to show that the denominator is bigger than the numerator since thereby the second term in the first case will be strictly smaller one i.e.

$$\left(\sum e_i\right)^2 > \left(\sum b_i - PP\right)\left(\sum e_i - e_i\right) + e_i\left(\sum e_i\right). \tag{4.7}$$

Rearranging yields:

$$\sum e_i > \sum b_i - PP. \tag{4.8}$$

To see this inequality holds true under the assumptions made and the good is funded consider that there are $n \in (0, N]$ investors and $N - n$ donors[14]. In

[14]Note, that due to the assumption $N \cdot r < PP$, there needs to be at least one investor if the project is realised.

the following, we refer to the set of investors as $I = \{\text{all } i \text{ such that } b_i > r\}$ and use this to express $\sum e_i$ in terms of bids b_i:

$$\sum e_i = \sum_{i \in I} (b_i - r). \tag{4.9}$$

Plugging this into (4.8) and rearranging yields

$$PP > \sum_{i \notin I} b_i + n \cdot r. \tag{4.10}$$

Since people are considered investors iff $b_i \geq r$ and donors iff $b_i < r$ the RHS is bounded from above by $N \cdot r$. Since we consider the case where $N \cdot r < PP$, the inequality is satisfied.[15]

4.7.2 Proof that a solution for the bid-cap rule must exist

The bid-cap rule determines a solution of the form $(k, \bar{b})$ for the following equation:

$$PP = \sum_{i=1}^{k} b_i + (N-k)\bar{b}, \tag{4.11}$$

where $b_k \leq \bar{b} < b_{k+1}$ and $k \in \{0, \ldots, N-1\}$. We arrive there by starting with $\sum b_i > PP$ and introducing the slack variable $S > 0$ to turn the inequality into an equality:

$$\sum b_i - S = PP. \tag{4.12}$$

We can set (4.12) equal to (4.11):

$$\sum b_i - S = \sum_{i=1}^{k} b_i + (N-k)\bar{b}. \tag{4.13}$$

We substitute $S = \sum_{i=k+1}^{N} s_i$:

$$\sum_{i=k+1}^{N} b_i - \sum_{i=k+1}^{N} s_i = (N-k)\bar{b} \iff \sum_{i=k+1}^{N} (b_i - s_i) = (N-k)\bar{b} = \sum_{i=k+1}^{N} \bar{b}. \tag{4.14}$$

[15] If $N \cdot r > PP$ this result must not necessarily hold. When $\sum_{i \in I} r + \sum_{i \notin I} b_i > PP$ the marginal damage will be positive and individuals would choose infinitely large bids.

We can represent (4.14) with the definitions of S and $\bar{b}$ as a system of equations and when $b_i - s_i = \bar{b}$ for all $i \in [k+1, N]$ holds, we get:

$$\begin{aligned} \sum_{i=k+1}^{N} s_i &= S \\ b_{k+1} - s_{k+1} &= \bar{b} \\ &\dots \\ b_N - s_N &= \bar{b} \\ b_{k+1} &> \bar{b} \\ b_k &\leq \bar{b} \end{aligned} \tag{4.15}$$

We continue to show that a solution to this system of equations must exist. Note that we only consider cases where $\sum b_i > PP$. Consider the upper interval limit $k = N-1$. The system of equations reduces to $b_N - S = \bar{b}$ and $\bar{b} \geq b_{N-1}$. The highest contributor gets the full rebate S. In the upper interval limit, the bid of individual N is needed for the project to be completed. Hence, $0 < S \leq b_N - b_{N-1}$ which implies that the inequalities above are satisfied. Now consider the lower interval limit $k = 0$. Everyone gets a positive rebate and pays exactly $\bar{b}$. This is a solution as $r \leq \bar{b} < b_1$ and $S > \sum(b_i - b_1)$. We generalise this observation to note that for any $S > 0$ we can find a k to solve the system of equations:

$$\exists k \text{ such that } \sum_{i=k+1}^{N} (b_i - b_{k+1}) < S \leq \sum_{i=k+1}^{N} (b_i - b_k) \text{ and } b_k \leq \bar{b} < b_{k+1}. \tag{4.16}$$

As this only requires $\sum_{i=k+1}^{N}(b_i - b_k) > \sum_{i=k+1}^{N}(b_i - b_{k+1})\ \forall k$, which holds as $\sum_{i=k+1}^{N}(b_i - b_k) = \sum_{i=k+1}^{N} b_i - (N-k) \cdot b_k > \sum_{i=k+1}^{N} b_i - (N-k)b_{k+1} = \sum_{i=k+1}^{N}(b_i - b_{k+1})$ since $0 \leq k \leq N-1$ and $b_{k+1} > b_k$ by definition. Now we express S in terms of $\bar{b}$ which is

$$S = \sum_{i=k+1}^{N} (b_i - \bar{b}). \tag{4.17}$$

and notice that this does not violate (4.16), as $b_k \leq \bar{b} < b_{k+1}$. As plugging (4.17) back into (4.12) yields (4.11), a solution of the proposed form always exists as long as we have $\sum b_i > PP$.

4.7.3 Proof that the solution in 4.7.2 is unique

We conduct our proof by contradiction. Consider a solution to (4.11) that we call $(k, \bar{b})$ following 4.7.2.

First suppose $(k', \bar{b}')$ with $k' < k$ and $b_{k'} \leq \bar{b}' < b_k$, is also a solution to (4.11). The last inequality follows as we consider a situation in which decision maker k does not cap out her bid in contrast to $(k, \bar{b})$. However, $(k', \bar{b}')$ can not be a solution to (4.11) as $\sum_{i=1}^{k'} b_i + (N - k')\bar{b}' < \sum_{i=1}^{k} b_i + (N - k)\bar{b}' < \sum_{i=1}^{k} b_i + (N - k)b_k \leq \sum_{i=1}^{k} b_i + (N - k)\bar{b}$.

Now assume $(k', \bar{b}')$ with $k' > k$ and $\bar{b} < b_{k+1} \leq \bar{b}'$ is a solution to (4.11). The inequalities follow since as $k' > k$ we must have at least $k' \geq k + 1$, while $\bar{b} \leq b_k < b_{k+1}$ when under $(k, \bar{b})$ only consumers up to k cap out their bids. Again, $(k', \bar{b}')$ can not solve (4.11) since

$$\sum_{i=1}^{k'} b_i + (N - k')\bar{b}' > \sum_{i=1}^{k} b_i + (N - k)\bar{b}' \geq \tag{4.18}$$

$$\sum_{i=1}^{k} b_i + (N - k)b_{k+1} > \sum_{i=1}^{k} b_i + (N - k)\bar{b}. \tag{4.19}$$

There are also following other cases:

- $k' < k, \bar{b}' > \bar{b}$
  Consider a sequence of ordered bids $(b_1, \ldots, b_N)$. When we have $k' < k$, then it has to be $b'_k < b_k$ as bids are ordered. This in turn means that it has to be $\bar{b}' < \bar{b}$ which contradicts $\bar{b}' > \bar{b}$.

- $k' > k, \bar{b}' < \bar{b}$
  Consider a sequence of ordered bids $(b_1, \ldots, b_N)$. When we have $k' > k$, then it has to be $b_k < b'_k$ as bids are ordered. This in turn means that it has to be $\bar{b}' < \bar{b}$ which contradicts $\bar{b}' < \bar{b}$.

- $k' = k, \bar{b}' > \bar{b}$ or $k' = k, \bar{b}' < \bar{b}$
  Consider a sequence of ordered bids $(b_1, \ldots, b_N)$. When we have $k' = k$, then it has to be $b_k = b'_k$ as bids are ordered. This in turn means that it has to be $\bar{b}' = \bar{b}$ which contradicts both $\bar{b}' > \bar{b}$ and $\bar{b}' < \bar{b}$.

4.7.4 Proof of payment relation of bid-cap and proportional rebate

Consider a sequence of ordered bids $(b_1, \ldots, b_N)$ with $\sum b_i > PP$ where we w.l.o.g. assume that $b_1 > r$. The sequence of final payments for all N individuals under proportional rebate is given by:

$$\left(b_1 - \frac{e_1 \cdot (\sum b_i - PP)}{\sum e_i}, \ldots, b_N - \frac{e_N \cdot (\sum b_i - PP)}{\sum e_i} \right), \tag{4.20}$$

where $e_i \cdot (\sum b_i - PP) / \sum e_i$ are the individual rebates, which are weakly increasing, just like the final payments. Similarly, we denote the rebates and payments for all N individuals in the bid-cap rule:

$$\begin{array}{lccccccc} \text{Rebate:} & (0, & \ldots, & 0, & b_{k+1} - \bar{b}, & \ldots, & b_N - \bar{b}), \\ \text{Payment:} & (b_1, & \ldots, & b_k, & \bar{b} \quad , & \ldots, & \bar{b} \quad). \end{array} \tag{4.21}$$

All individuals from 1 to k would be increasingly better off under the proportional rebate rule, since they receive no rebate under the bid-cap rule, while rebates under proportional rebate are increasing proportionally with bids. The difference in payments made is maximised when $b_k = \bar{b}$. Since under both rules the total final payment is equal to PP, the individuals from $k+1$ to N would receive the same total rebates under the bid-cap rule as all individuals from 1 to N would receive under the proportional rebate rule. Moreover, these $N-k$ individuals each pay the fixed bid $\bar{b}$. Hence, the sum of payments by individuals from $k+1$ to N must be larger under the proportional rebate rule compared to the bid-cap rule.
For a sequence of ordered bids $(b_1, \ldots, b_N)$ with $\sum b_i > PP$ where we w.l.o.g. assume that $b_1 > r$, the total final payment equals PP. This means that the total rebates R under both rebate rules are the same.

$$R_{bidcap} = \sum_{i=1}^{k} 0 + \sum_{i=k+1}^{N} (b_i - \bar{b}) = \sum_{i=1}^{N} \frac{e_i \cdot (\sum b_i - PP)}{\sum e_i} = R_{prop} \tag{4.22}$$

The total rebates for the proportional rule R_{prop} can be decomposed into

$$\sum_{i=1}^{k} \frac{e_i \cdot (\sum b_i - PP)}{\sum e_i} + \sum_{i=k+1}^{N} \frac{e_i \cdot (\sum b_i - PP)}{\sum e_i}. \tag{4.23}$$

This in turn indicates that

$$\sum_{i=k+1}^{N} \frac{e_i \cdot (\sum b_i - PP)}{\sum e_i} < R_{bidcap}, \tag{4.24}$$

hence after a cut-off bid $\hat{b}$, with $\hat{b} \in (b_k, b_N)$, payments made are higher under the proportional rebate rule.

Since $b_k \leq \bar{b}$ we must have $\hat{b} \in (b_k, b_N)$. Therefore, there exists a $\hat{k} \in [k, N]$ from which onward investors are better off under the bid-cap rule.

4.7.5 Additional regressions

	Part 1 (One round)			Part 2 (Ten rounds)		
	$b_i - 30$	$b_i - 30$	$b_i - 30$	$b_i - 30$	$b_i - 30$	$b_i - 30$
Constant	-6.20***	-1.10	0.05	-3.70**	2.39	0.45
	(2.028)	(1.181)	(2.425)	(1.570)	(1.703)	(1.703)
Condition	All-or-nothing	Proportional rebate	Bid-cap	All-or-nothing	Proportional rebate	Bid-cap
Observations	20	20	20	200	200	200

Standard errors in parentheses. Estimation by OLS regression with robust standard errors for Part 1 and estimation by random-effects regression with clustering on subject level for Part 2. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 4.3: Analysis of bids compared to equilibrium prediction within experimental conditions.

	Part 1 (One round)			Part 2 (Ten rounds)		
	$b_i - v_i$	$b_i - v_i$	$b_i - v_i$	$b_i - v_i$	$b_i - v_i$	$b_i - v_i$
Constant	-21.20***	-16.10***	-14.95***	-18.89***	-12.80***	-14.74***
	(2.028)	(1.181)	(2.425)	(1.624)	(1.474)	(1.632)
Condition	All-or-nothing	Proportional rebate	Bid-cap	All-or-nothing	Proportional rebate	Bid-cap
Observations	20	20	20	200	200	200

Standard errors in parentheses. Estimation by OLS regression with robust standard errors for Part 1 and estimation by random-effects regression with clustering on subject level for Part 2. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 4.4: Analysis of bids compared to valuation within experimental conditions.

4.7.6 Additional figures

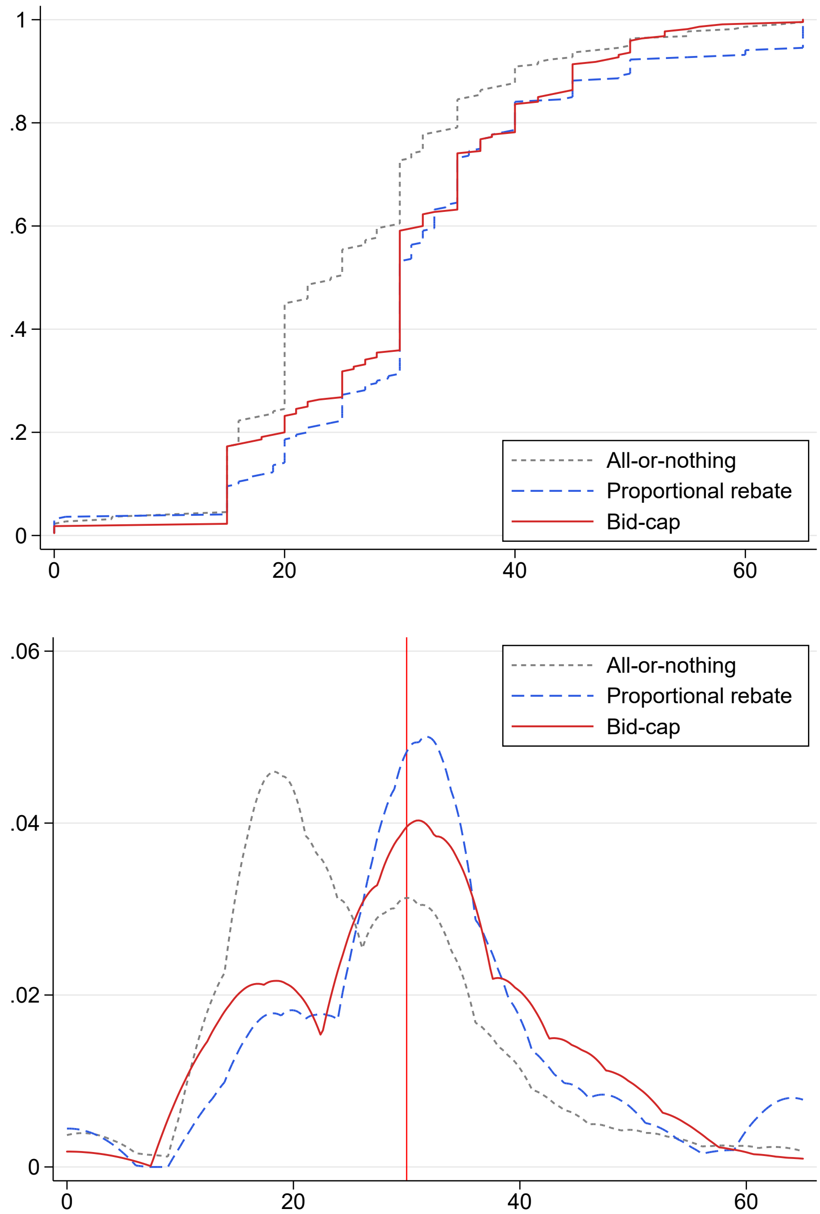


Figure 4.3: Cumulative distribution of bids (top) and kernel density estimation of bids (bottom) by experimental condition Part 1 and 2 pooled.

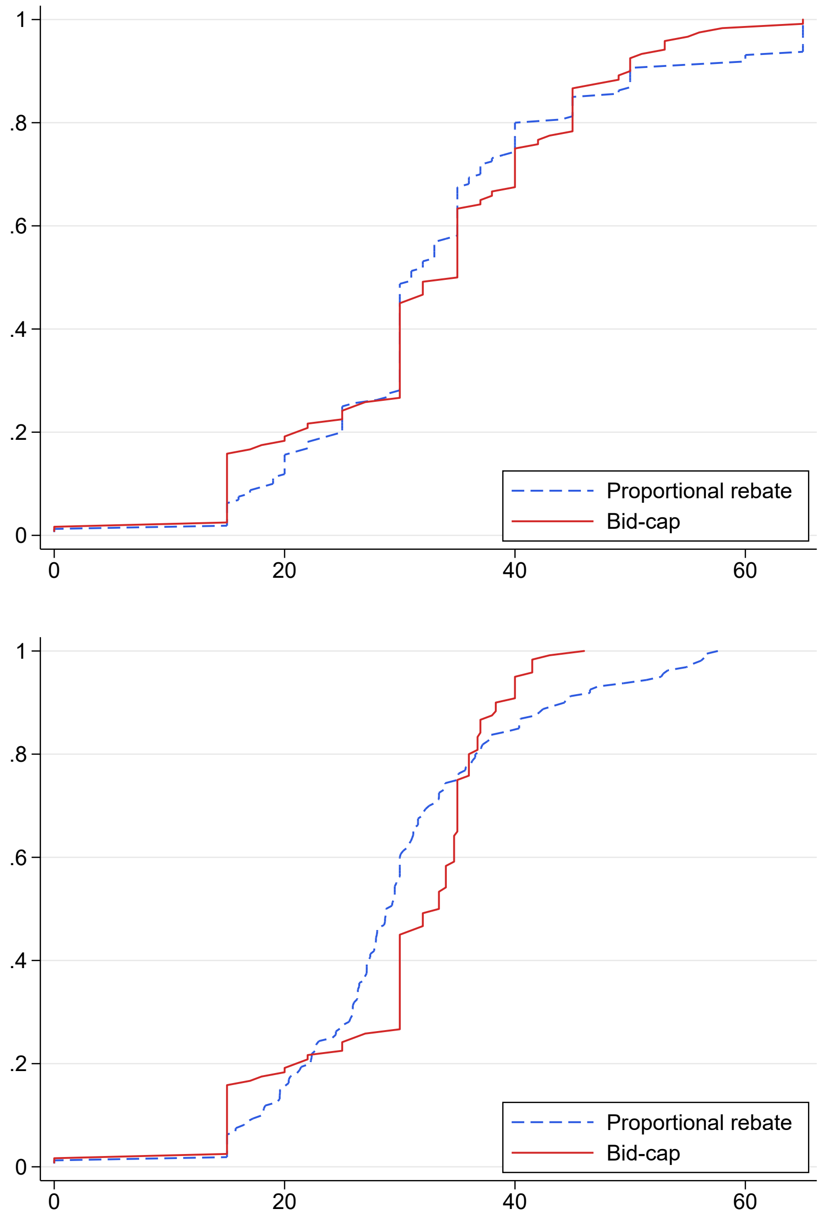


Figure 4.4: Cumulative distribution of bids (top) and payments (bottom) of funded projects by experimental condition (only rebate rule conditions) Part 1 and 2 pooled.

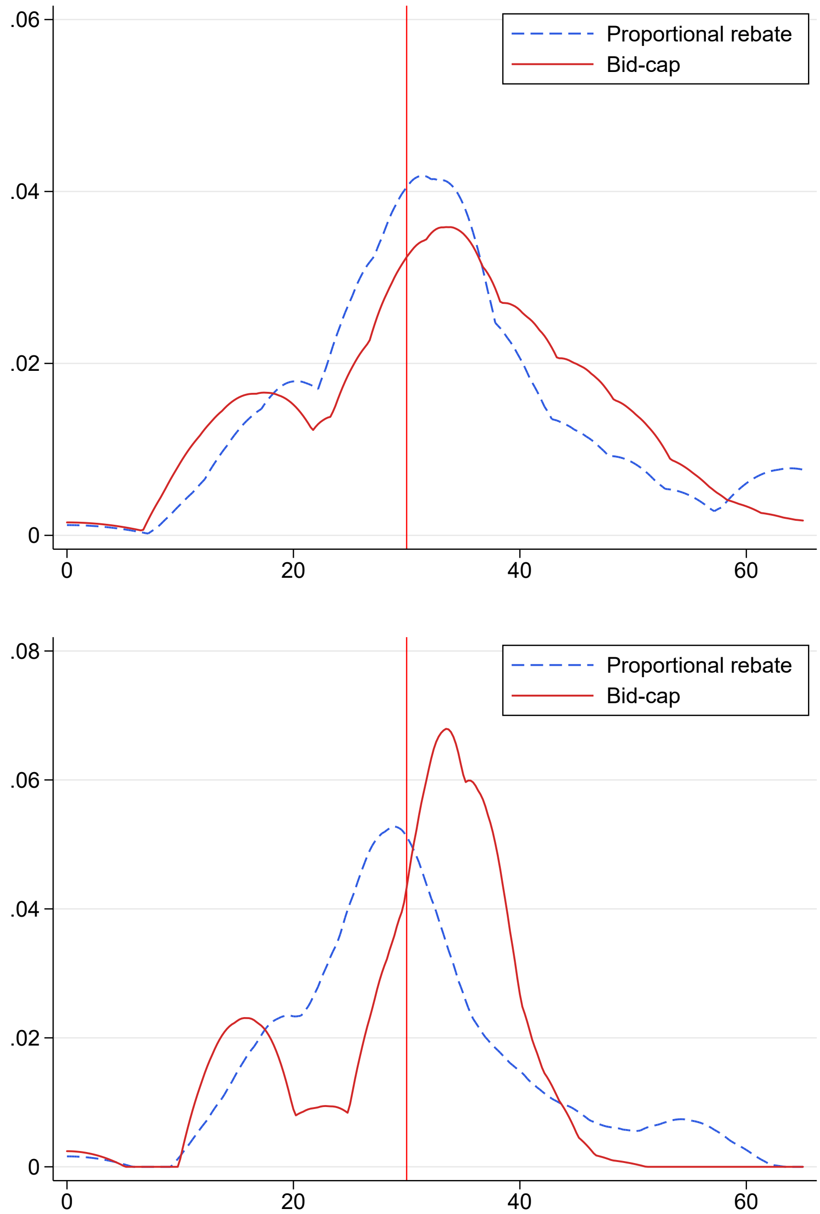


Figure 4.5: Kernel density of bids (top) and payments (bottom) of funded projects by experimental condition (only rebate rule conditions).

Translated instructions

[Expressions in square brackets were not visible to participants]

Instructions [All experimental conditions]

Welcome to this experiment and thank you for your participation! This experiment begins now. Please read these instructions carefully. The instructions are identical for each participant. If you have any questions, please raise your hand and an experimenter will come by to you to answer your questions. If the question that you have asked should be relevant for everybody, then we will repeat the questions for all and provide a response.

Please do not communicate with other participants during the experiment and please turn off your mobile phones now. This is an experiment on decision-making. You can earn money in this experiment which depends on your decisions and the decisions of the other participants. The amount you earn will be paid out to you in cash after the experiment.

In beginning, you will be randomly assigned to a group consisting of you and **nine** other participants. This group remains the same and does not change throughout the experiment. You will make your decisions privately and **not** learn who the other group members are.

This experiment consists of **two** parts. You can find the instructions for the first part below. You will receive instructions for part **two** after the first part is over.

All monetary values in this experiment are denominated in Experimental Currency Units (*ECU*). Your total earning is the sum of your payoff you earned in part 1 and in part 2 which will be exchanged at a rate of 10 *ECU* = 0,40 Euro at the end of the experiment. In addition, independent of your earnings in part 1 and 2, you receive 5 Euros for your participation.

Part 1 [All-or-nothing condition]

Your task in the first Part:

You and your other group members each are given 65 *ECU* to start. You and each of your group members can invest any amount out of the given **starting capital** into a project which will only be realised if the total **investment costs** of 300 *ECU* are reached. Your payoff in the first part depends on whether you are an investor of the project and whether your group realises the project or not.

To be considered an **investor** of the project, you need to make a minimum investment of **at least** 15 *ECU*. This also holds for the other group members. Investments below 15 *ECU* are just seen as a **donation** and do not entitle a participant for a payout. In case the investment costs are reached and the project is realised, group members that have invested less than 15 *ECU* do not receive a payout from the project. They just receive the remaining amount of the starting capital that has not been invested.

If the total group investments are below the investment costs of 300 *ECU*, the investment put forward by yourself and your group members are returned. If your groups total investment meets the investment costs of 300 *ECU*, each **Investor** receives a **payout** of 45 *ECU*, and their remaining amount of the starting capital that was not invested into the project. The following example illustrates this rule in more detail:

Example

Investor	A	B	C	D	E	F	G	H	I	J
Investment	0	7	14	21	28	35	42	49	56	63

Based on these investments D, E, F, G, H, I, J are investors whereas A, B and C are <u>not</u>. Consequently, the investments of A,B and C are merely considered as donations. After subtracting the donations of A,B and C $0 + 7 + 14 = 21$ *ECU* from the investment costs, only 279 *ECU* are needed in order to realised the project. The remaining investments are enough to cover 279 *ECU*, as $28 + 35 + 42 + 49 + 56 + 63 = 294$. This results in additional excess investments of 15 *ECU* which remain in the project.

Summary of potential earnings

- If the investment cost are <u>not</u> reached, you receive:

  $Earnings = Starting\ Capital$

- If the investment costs are covered but you invested less than the minimum required to become an investor, your investment is considered a donation:

  $Earnings = Starting\ Capital - Donation$

- If the investment costs are covered and you invested at least the minimum required to become an investor, your paid amount is determined by:

  $Earnings = Starting\ Capital + Payout - Investments$

If you have questions with regard to **Part 1**, please raise your hand and an experimenter will come by and answer your question.

Part 1 [Proportional rebate rule condition]

Your task in the first Part:

You and your other group members each are given 65 *ECU* to start. You and each of your group members can invest any amount out of the given **starting capital** into a project which will only be realised if the total **Investment costs** of 300 *ECU* are reached. Your payoff in the first part depends on whether you are an investor of the project and whether your group realises the project or not.

To be considered an **Investor** of the project, you need to make a minimum investment of **at least** 15 *ECU*. This also holds for the other group members. Investments below 15 *ECU* are just seen as a **donation** and do not entitle a participant for a payout. In case the investment costs are reached and the project is realised, group members that have invested less than 15 *ECU* do not receive a payout from the project. They just receive the remaining amount of the starting capital that has not been invested.

If the total group investments are below the investment costs of 300 *ECU*, the investment put forward by yourself and your group members are returned. If your groups total investment meets the investment costs of 300 *ECU*, each **Investor** receives a **payout** of 45 *ECU*, and their remaining amount of the starting capital that was not invested into the project. If your group invests more than the required investment costs, then each investor receives a share of the excess investments. The **rebate** of the excess investments is made according to following rule:

Firstly, it is determined for each investor how much more than the minimum investment of 15 *ECU* each of them has invested. The difference between the investment and minimum investment is called **contribution**. The share out of the excess investments each investor gets, is directly proportional to each investors share of the sum of contributions. For instance, if an investor is responsible for half of the total contributions then this investor receives half of the excess investments. We refer to the part of the investment actually used to realise the project – i.e. what an investor ultimately pays for the realisation of the project – as the **paid amount**. The following example illustrates this rule in more detail.

Example

Investor	A	B	C	D	E	F	G	H	I	J
Investment	0	7	14	21	28	35	42	49	56	63

Based on these investments D, E, F, G, H, I, J are Investors whereas A, B and C are not. Consequently, the investments of A,B and C are merely considered as donations. After subtracting the donations of A,B and C $0 + 7 + 14 = 21$ *ECU* from the investment costs, only 279 *ECU* are needed in order to realise the project. The remaining investments are enough to cover 279 *ECU*, as $28 + 35 + 42 + 49 + 56 + 63 = 294$. So $294 - 279 = 15$ *ECU* will be contributed as excess investments, which will be returned proportionally to the **investors' contributions**. Below you will find the calculation of the contributions and rebates of all investors:

The minimum investment in order to become investor is 15 *ECU* and D invests 21 *ECU*. D's contribution is then $21 - 15 = 6$ *ECU*. E's contribution is 28-15=13 *ECU*, F's contribution is 35-15=20 *ECU*, G's contribution is 42-15=27 *ECU*, H's contribution is 49-15=34 *ECU*,I's is 56-15=41 *ECU* und J's is 63-15=48 *ECU*. The sum of all contributions is then 6+13+20+27+34+41+48=189 *ECU*.

D's contribution is 6 ECU and the sum of all contributions is 189 ECU. So D's share of the contributions is 6/189. This portion of the contributions is multiplied by the excess investment of 15 ECU to determine the rebate.

Consequently, D receives a rebate of $\frac{6}{189} \cdot 15 = 0.48$ *ECU*. Equivalently, E receives a rebate of $\frac{13}{189} \cdot 15 = 1.13$ *ECU*, F a rebate of $\frac{20}{189} \cdot 15 = 1.59$ *ECU*, G a rebate of $\frac{27}{189} \cdot 15 = 3.14$ *ECU*, H a rebate of $\frac{34}{189} \cdot 15 = 2.7$ *ECU*, I a rebate of $\frac{41}{189} \cdot 15 = 3.25$ *ECU* and J a rebate of $\frac{48}{189} \cdot 15 = 3.81$ *ECU*.

In the table below you can see the investments and the paid amounts made by all investors.

Investor	A	B	C	D	E	F	G	H	I	J
Investment	0	7	14	21	28	35	42	49	56	63
Paid amount	0	7	14	20.52	26.97	33.41	39.86	46.30	52.75	59.19

Summary of potential earnings

- If the investment cost are not reached, you receive:

  $Earnings = Starting\ Capital$

- If the investment costs are covered but you invested less than the minimum required to become an investor, your investment is considered a donation:

  $Earnings = Starting\ Capital - Donation$

- If the investment costs are covered and you invested at least the minimum required to become an investor, your paid amount is determined by the rule above and you receive:

  $Earnings = Starting\ Capital + Payout - Paid\ amount$

If you have questions with regard to **Part 1**, please raise your hand and an experimenter will come by and answer your question.

Part 1 [Bid-cap rule condition]

Your task in the first Part:

You and your other group members each are given 65 *ECU* to start. You and each of your group members can invest any amount out of the given **starting capital** into a project which will only be realised if the total **Investment costs** of 300 *ECU* are reached. Your payoff in the first part depends on whether you are an investor of the project and whether your group realises the project or not.

To be considered an **Investor** of the project, you need to make a minimum investment of **at least** 15 *ECU*. This also holds for the other group members. Investments below 15 *ECU* are just seen as a **donation** and do not entitle a participant for a payout. In case the investment costs are reached and the project is realised, group members that have invested less than 15 *ECU* do not receive a payout from the project. They just receive the remaining amount of the starting capital that has not been invested.

If the total group investments are below the investment costs of 300 *ECU*, the investment put forward by yourself and your group members are returned. If your groups total investment meets the investment costs of 300 *ECU*, each **Investor** receives an **payout** of 45 *ECU*, and their remaining amount of the starting capital that was not invested into the project. If your group invests more than the required investment costs, then each investor receives a share of the excess investments. The **rebate** of the excess investments is made according to following rule:

Firstly, the donations of non-investors are subtracted from the investment costs. Then it is checked whether the investment costs subtracted with the donations would be reached if each investor contributes the lowest investment amount that has been invested. If yes, then each investor pays the lowest investment amount and the excess investments are distributed equally between investors.

If this is not the case, then the investor(s) who made the lowest investment, pay the lowest amount and then it is checked again whether the investment cost minus the donations are reached, if all other investors pay the second-highest investment. If yes, then the investor(s) with the lowest investment contribute their lowest investment and all other investors invest the second-highest investment. The excess investments are distributed equally between investors that paid the second-highest investment.

This process continues until the he investment costs subtracted with the donations are reached. The Investors pay at most their own investment with the potential of paying less if the entire investment is not needed to cover the investment costs. The actual share of the investments that are utilised to finance the project we call **paid amount**. The following example illustrates this rule in more detail:

Example

Investor	A	B	C	D	E	F	G	H	I	J
Investment	0	7	14	21	28	35	42	49	56	63

Based on these investments D, E, F, G, H, I, J are Investors whereas A, B and C are not. Consequently, the investments of A,B and C are merely considered as donations. After subtracting the donations of A,B and C $0 + 7 + 14 = 21$ *ECU* from the investment costs, only 279 *ECU* are needed in order to realised the project.

Now it is checked whether 279 *ECU* can be covered if all investors make the lowest investment of 21 *ECU* . As $21 \cdot 7 = 147 < 279$, this does not cover the costs. D pays 21 *ECU* and it is checked whether 279 *ECU* can be covered if all other investors E ,F ,G ,H, I, J each make the second-lowest investment of 28 *ECU*. As $6 \cdot 28 + 21 = 189 < 279$ the costs are not covered. In the next iteration, D pays 21 *ECU* und E pays 28 *ECU* and it is checked whether 279 *ECU* can be covered if all other investors each make the third-lowest investment of 35 *ECU* which results in $21 + 28 + 5 \cdot 35 = 224 < 279$.
This process continues until Investor **I** is reached. In this case, all investors pay their invested amounts and I and J pay 56 *ECU* each. This results in a total investment of $21 + 28 + 35 + 42 + 49 + 2 \cdot 56 = 287$ ECU which covers the investment cost minus the donations. Therefore, J pays 56 *ECU* instead of 63 *ECU*.
These investments generate excess investments of 8 *ECU* (287-279=8 *ECU*). The 8 *ECU* are distributed equally between I and J , so I and J receive additional excess payments of 4 *ECU*.

In the end, all investors pay their invested amounts except for I and J, who pay less than their suggest investments as their entire investment is not needed to cover the investment costs. J pays 56 *ECU* (instead of 63 *ECU*). Additionally, I and J are being rebated 4 *ECU* each. The textbfpaid amount of J is 63 *ECU* minus 7 *ECU*, as J's investment is reduced to I's investment, minus 4 *ECU*. as J receives a rebate of 4 *ECU* out of the excess investments.

The following table summaries the suggested investments and the paid amounts for all investors.

Investor	A	B	C	D	E	F	G	H	I	J
Investment	0	7	14	21	28	35	42	49	56	63
Paid amount	0	7	14	21	28	35	42	49	52	52

Summary of potential earnings

- If the investment cost are not reached, you receive:

  $Earnings = Starting\ Capital$

- If the investment costs are covered but you invested less than the minimum required to become an investor, your investment is considered a donation:

  $Earnings = Starting\ Capital - Donation$

- If the investment costs are covered and you invested at least the minimum required to become an investor, your paid amount is determined by the rule above and you receive:

  $Earnings = Starting\ Capital + Payout - Paid\ Amount$

If you have questions with regard to **Part 1**, please raise your hand and an experimenter will come by and answer your question.

Part 2 [All experimental conditions]

The second part of the experiment begins now. In this part you will be repeating the task from **Part 1** <u>**ten**</u> times. In each round you will receive a **starting capital** of 65 *ECU*. The **payout** each investor can get (in *ECU*) will be determined independently for each individual in the beginning of every round through a random draw from the interval $[30, 60]$. Each number in the interval is equally likely to be drawn. The total payout in your group is the same as in **Part 1** of the experiment. You will neither get feedback about the investments that your group members made in previous rounds nor whether the investment costs were reached.

In this part overall, your group members, the investment costs, your starting capital, the minimum investment in order to be become an investor and the rule concerning excess investments are the same. In the beginning of each round you will learn your randomly drawn payout as investor.

Your potential earnings in each round are determined the exact same way as in the first part of the experiment. You receive your investment back in case the project is not successfully realised. Your earning equals the start capital minus your investment if the project is realised but you invested less than 15 *ECU*. If the investment costs are covered and you invested <u>at least</u> 15 *ECU*, then you receive your randomly drawn payout in addition to the starting capital, minus your paid amount, which is determined following the same rule as in part 1. Your overall payoff from **Part 2** equals the payoff you have received in a randomly drawn round. Hereby, each round is equally likely to be drawn. Your earning in this part will be exchanged at a rate of 10 *ECU* = 0,40 Euro.

In case you have any questions with regard to **Part 2** please raise your hand and an experimenter will come by to you to answer your questions. If the question that you have asked should be relevant for everybody, then we will repeat the questions for all and provide a response.

Part 2 [All-or-nothing condition]

The second part of the experiment begins now. In this part you will be repeating the task from **Part 1** **ten** times. In each round you will receive a **starting capital** of 65 *ECU*. The **payout** each investor can get (in *ECU*) will be determined independently for each individual in the beginning of every round through a random draw from the interval $[30, 60]$. Each number in the interval is equally likely to be drawn. The total payout in your group is the same as in **Part 1** of the experiment. You will neither get feedback about the investments that your group members made in previous rounds nor whether the investment costs were reached.

In this part overall, your group members, the investment costs, your starting capital and the minimum investment in order to be become an investor are the same. Excess investments still remain in the project. In the beginning of each round you will learn your randomly drawn payout as investor.

Your earnings in each round is determined the exact same way as in the first part of the experiment. You receive your investment back in case the project is not successfully realised. Your earning equals the start capital minus your investment if the project is realised but you invested less than 15 *ECU*. If the investment costs are covered and you invested at least 15 *ECU*, then you receive your randomly drawn payout in addition to the starting capital and minus your investment. Your overall payoff from **Part 2** equals the payoff you have received in a randomly drawn round. Hereby, each round is equally likely to be drawn. Your earning in this part will be exchanged at a rate of 10 *ECU* = 0,40 Euro.

In case you have questions with regard to **Part 2** please raise your hand and an experimenter will come by to answer your questions. If the question that you have asked should be relevant for everybody, then we will repeat the questions for all and provide a response.

Part 2 [Proportional rebate rule condition]

The second part of the experiment begins now. In this part you will be repeating the task from **Part 1** **ten** times. In each round you will receive a **starting capital** of 65 *ECU*. The **payout** each investor can get (in *ECU*) will be determined independently for each individual in the beginning of every round through a random draw from the interval $[30, 60]$. Each number in the interval is equally likely to be drawn. The total payout in your group is the same as in **Part 1** of the experiment. You will neither get feedback about the investments that your group members made in previous rounds nor whether the investment costs were reached.

In this part overall, your group members, the investment costs, your starting capital, the minimum investment in order to be become an investor and the rebate rule of excess investments are the same. In the beginning of each round you will learn your randomly drawn payout as investor.

Your earnings in each round is determined the exact same way as in the first part of the experiment. You receive your investment back in case the project is not successfully realised. Your earning equals the start capital minus your investment if the project is realised but you invested less than 15 *ECU*. If the investment costs are covered and you invested at least 15 *ECU*, then you receive your randomly drawn payout in addition to the starting capital, minus your investment, and potential additional repayment from excess investments based on above rule. Your overall payoff from **Part 2** equals the payoff you have received in a randomly drawn round. Hereby, each round is equally likely to be drawn. Your earning in this part will be exchanged at a rate of 10 *ECU* = 0,40 Euro.

In case you have any questions with regard to **Part 2** please raise your hand and an experimenter will come by to you to answer your questions. If the question that you have asked should be relevant for everybody, then we will repeat the questions for all and provide a response.

Chapter 5

Conclusion and Further Research

This thesis has experimentally investigated interactions that take place in online environments such as the provision of ratings and investing in crowdfunding and also potential biases that might arise from them. The overarching purpose has been to contribute new insights into the experimental literature on such online interactions. More specifically, we aimed to advance our understanding of rating provisions under uncertainty such as on markets for expert services. We also took a more critical stance on consumer-provided online ratings and scrutinised potential biases that accompany them. Lastly, we analysed investment behaviour of consumers in reward-based crowdfunding under different rebate rules.

After providing a general introduction to the thesis, we have theoretically and experimentally investigated whether voluntary online ratings by consumers can incentivise sellers to provide credence goods of appropriate quality and price in Chapter 2. We further investigated the effectiveness of the rating system depending on whether informed consumers are present and whether the design of the rating system affects the decisions of buyers and sellers. We first developed a theoretical model to derive behavioural predictions and then conducted a controlled laboratory experiment to test our hypotheses. Our work in this chapter is based on Dulleck et al. (2011) and Mimra et al. (2016).

We find that ratings significantly reduce overcharging of buyers. Thereby, consumers that are better informed are overcharged less than uninformed consumers. Furthermore, having better-informed buyers on the market has also been shown to significantly reduce overcharging. When changing the rating format and displaying ratings separately by information status of buyers does not significantly reduce overcharging when compared to the joint display of ratings.

We found similar fractions of undertreatment across experimental conditions which do not differ significantly. This type of fraud can be detected by all buyers irrespective of information level and rating format. Interestingly, we find much lower levels of undertreatment and overcharging than previous studies such as Mimra et al. (2016) or Dulleck et al. (2011). We have provided one potential explanation for these differences. The differences in results might arise due to differences in social preferences. We found that in our experiment more than half of sellers are driven by non-selfish preferences. It might well be the case, that the fraction of selfish sellers was higher in previous studies resulting in much higher fractions of overcharging and undertreatment than in our study.

In terms of rating behaviour, we reported that uninformed buyers rate as much as informed buyers. When the buyers exactly know how they have been treated the rating pattern of informed and uninformed buyers is similar. Buyers of both information statuses punish undertreatment with negative ratings whereas honest behaviour is rewarded with positive ratings. Our findings support previous research on rating behaviour (see Lafky 2014, Nosko & Tadelis 2015). However, we also found that communication through ratings by informed buyers is imperfect, (especially when the focal interpretation of ratings is considered where ratings are positively associatively matched with buyers' payoff (i.e., higher payoffs translated into higher ratings)). This is surprising as informed buyers can perfectly detect the behaviour of sellers. A substantial fraction of informed buyers punish non-fraudulent behaviour with a negative rating or reward overcharging with a positive rating.

Another interesting observation we made in terms of rating behaviour is that when uninformed buyers face ambiguity about a seller's behaviour, the share of positive and no ratings together exceeds two-thirds of the ratings provided. This finding is indicative of lenient rating behaviour and is in line with past work on rating under uncertainty. For instance, Bolton et al. (2019) experimentally analyse ratings provision under attributional uncertainty and get similar findings to us. Buyers either provide positive ratings or refrain from providing feedback under attributional uncertainty. These findings indicate that there is hesitancy to provide negative feedback.

We also presented implications of ratings on the supply side. The frequency of a seller being chosen is positively affected by positive ratings and negatively affected by negative ratings, showing a positive reputation-building pays off. Furthermore, when ratings are displayed separately by buyer status, the number of negative ratings and the number of no ratings provided by informed buyers significantly affect the likelihood of a seller being chosen.

Broadly, our study in chapter 2 contributes to the literature on credence goods by investigating consumer-provided online ratings as a tool to undermine fraudulent behaviour. Furthermore, we add to the literature on ratings, especially when ratings are provided under uncertainty.

We also have identified fruitful avenues for future research based on our findings. In our study, we have reported that communication through ratings by informed buyers is imperfect. A subject for further scrutiny could be to incentivise informed buyers to pay more attention when providing ratings in order to improve information communicated through ratings. It would be very interesting to see how incentivising informed buyers would compare to our findings, especially when the ratings are displayed separately by buyers' information status.

Furthermore, a common finding that has been reported in our work but also identified by previous research is the hesitancy to provide negative feedback, especially in uncertain environments. But research on negative rating hesitancy is scarce, hence future research can investigate the determinants of negative rating hesitancy by means of carefully designed lab experiments to better understand consumers' rating patterns.

In our work, we focused on numerical ratings but on rating platforms in general and for credence goods, one can provide textual feedback by leaving a comment. Written feedback might contain valuable informational signals. A future avenue of research could be to investigate how textual feedback affects fraud on credence goods markets through lab experiments or data gathered through platforms. Then textual analysis through machine learning techniques can be performed on the gathered data.

A commonly made assumption in the credence goods literature is that a seller always learns the type of a buyer with certainty and can perfectly treat the buyer's problem. However, in reality, this perfect inference might be more limited. For instance, in a medical context, doctors might not always be certain about what treatment to apply to a patient or also be uncertain about the success of the treatment applied, especially when the underlying health condition of the patient is complicated or experts differ in their level of expertise. Future work in this area could introduce uncertainty on the seller's side, where an expert cannot determine the problem of a consumer with certainty. Moreover, heterogeneity in terms of sellers' expertise can be introduced to make the model of credence goods more realistic.

In fact, even though imperfect, we have shown in this chapter that ratings can be a fruitful tool to reduce fraudulent behaviour in credence goods markets. This is important as it illustrates that it can pay off for consumers to check ratings before seeking advice from an expert. On the supply side, this also implies that experts should be wary of ratings and keep an eye on them to build a good reputation as this increases the likelihood of being visited by a buyer and payoffs.

Overall, we can conclude that online ratings are only one potential instrument to reduce fraud in credence goods markets. Reducing fraudulent behaviour and improving outcomes for consumers by investigating other tools and mechanisms, will remain a significant topic for future research.

Next, we proceeded with chapter 3, where we presented an online experiment that studies the prevalence of anchoring effects in a rating environment. Thereby, different to previous work on anchoring, our online experiment focussed on non-numerical and social anchoring under market conditions such as economic incentives and repeated decision-making. Our study featured a high, low and socially derived anchor.

We uncovered significant anchoring effects. For all anchoring conditions, we observed inflation of ratings with the high and social anchors having the highest impact. We further found that the effect of the high anchor is significantly persistent throughout. Interestingly, we uncovered asymmetric anchoring effects as we did not observe significant anchoring effects for the low anchor. In our study, the socially derived anchor took on varying roles between a high, a low and a mid-anchor and has been found to be directly predictive of observed ratings.

We have also compared the provided ratings in each anchoring condition to a slider task which is almost identical to the rational benchmark. Our comparison uncovered significant overrating in all anchoring conditions. This is consistent with other studies that scrutinise rating behaviour under uncertainty like Bolton et al. (2019) or our study in 2. As discussed in the previous chapter, consumers tend to provide lenient ratings under uncertainty and tend to be hesitant in providing negative feedback, which in turn can exacerbate the upward compression of ratings.

This pattern is also mirrored by asymmetric anchoring effects: Whereas it is possible to compress the ratings upwards, low anchors have no significant effects on ratings. More generally, our study indicates that irrelevant informational cues that work as anchors can contribute to the positive skewness of ratings, hampering their informativeness.

Like de Wilde et al. (2018), our work has revealed that the social anchor is perceived as more relevant when compared to the other anchors despite lacking any informational value. We have considered two potential explanations for this. Firstly, as each participant contributes to the social anchor, this might create a notion of overconfidence. This might induce each participant to overweigh their own rating which in turn increases perceived importance and trust in the social anchor. Secondly, the social anchor might create an illusion of the wisdom of crowds inducing more trust in the endogenously derived anchor. A potential avenue for future research is to uncover which of these two explanations holds.

We have discussed the implications this has on consumers, sellers and market platforms. Our results highlight how easy it is to bias and inflate ratings which might lead to consumers making erroneous decisions. Many online market platforms rely on truthful ratings as part of their marketing strategy to create and promote an accurately reflected reputation. However, our findings underline that market platforms should not solely rely on ratings but also include other factors to promote their reputation.

At first glance, rating inflation seems to be favourable for sellers. However, upward compression of ratings can make it harder for firms to set themselves apart from competitors by means of positive ratings. It also makes it more difficult to distinguish between good-quality and bad-quality offering firms. Furthermore, anchored ratings can make it more difficult for firms to use clients' ratings as feedback and assess the overall quality of their products.

Our overall findings do not support the notion that market conditions act as a filter for heuristics and biases as we observe anchoring effects despite economic incentives. More generally, we observed anchoring effects in a rather simple environment, that is not driven by numbers but only through visual changes. If anchoring effects are present in non-numerical settings such as ours, it is highly likely that these effects will be amplified when numerical values are present. Furthermore, in real-world settings, these anchoring effects will only be exacerbated, as they constitute more complex environments.

This study predominantly contributes to two strands of literature. Firstly, we contribute to the literature on rating provision and platform design by furthering the knowledge of the existence of anchoring effects in rating settings through controlled experimental evidence. Our study can help to design less error-prone rating platforms where anchoring is avoided. Furthermore, we also add to the growing body of literature on non-standard anchors by focusing on social and non-numerical anchors. The anchoring literature predominantly focuses on numerical anchoring and in our research, we focused on the opposite case of purely non-numerical anchors. However, many rating environments in the real-world is are a hybrid of both, e.g., a 5-star rating system. A promising future avenue for research could be to focus on non-numerical but countable environments to see if the anchoring bias is prevalent in these environments.

In Chapter 4 we turned our focus to another form of online interaction and scrutinised investment behaviour in reward-based crowdfunding. We derived two rebate rules for reward-based crowdfunding and compared their theoretical properties to each other, and to the widely applied all-or-nothing rule. We found that both rebate rules achieve efficient outcomes whenever the sum of bids weakly exceeds the provision point, which has to be met for a project to be realised, whereas the all-or-nothing rule yields efficient outcomes only if the provision point is exactly met.

Our experimental results revealed that both rebate rules greatly increase bids and the project realisation rate, especially when compared to the all-or-nothing rule. Bidding behaviour is similar under both rebate rules. We have further observed that the bid-cap rule induced less variance in the payments compared to the proportional rebate rule.

We have also discussed the implications of the rebate rules on investors and crowdfunding platforms. Under the bid-cap rule, high bidders pay less than the proportional rebate rule. Whereas the opposite holds for low bidders. Since the presence of a rebate rule increases project realisation it seems advisable for to platform to implement some kind of rebate rule. However, we cannot give clear guidance on which rebate rule to implement.

However, we have also noted a caveat for these implications is that on crowdfunding platforms project creators can endogenously determine the provision point and reservation price. As creators cannot keep the excess investments, this might incentivise them to increase the provision point. If this is the case the positive effect of rebate rules might be diminished.

Our work in this chapter contributes to the literature on threshold public goods by introducing the bid-cap rule to this stream and comparing it to the all-or-nothing rule and proportional rebate rule. We also more generally advance the crowdfunding literature by scrutinising potential rules that can help to increase project realisation rates on crowdfunding platforms. and thus is relevant for crowdfunding practitioners.

Overall, in this chapter, we have focused on cases where the provision point cannot be met when all individuals just pledge the reservation price, yielding a residual public good game. But, a fruitful future avenue of research could be to extend the present study by incorporating uncertainty in the number of individuals who participate in the crowdfunding game, so it is unclear whether a residual public good game arises or not.

Rondeau et al. (1999) and Spencer et al. (2009) pointed out that uncertainty in the number of individuals is equivalent to an uncertain provision point. Hence, It would be interesting to see whether the bid-cap rule that we have introduced in this chapter would extend to this situation similar to the proportional rebate rule in that demand revelation increases.

Moreover, in line with most crowdfunding applications, the presented rules could be extended to allow for different tiers of rewards.

Lastly, it would be interesting to apply rebate rules in field experiments and not only in a laboratory setting, by using actual crowdfunding services.

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Appendix A

Original Instructions in German

A.1 Original Instructions - Chapter 2

[Expressions in square brackets were not visible to participants.]

[No Ratings Condition:]

Herzlich Willkommen und vielen Dank für Ihre Teilnahme!

Diese Instruktionen sind für alle Teilnehmer identisch.Sie werden in diesem Experiment Geld verdienen, das abhängig ist von Ihren Entscheidungen und denen der anderen Teilnehmer. Es ist daher sehr wichtig, dass Sie diese Instruktionen sorgfältig durchlesen. Sollten Sie eine Frage haben, heben Sie bitte Ihre Hand. Wir werden dann zu Ihnen kommen und Ihre Frage beantworten. Bitte beachten Sie, dass es während des gesamten Experiments nicht gestattet ist, mit anderen Teilnehmern zu sprechen. Bitte schalten Sie jetzt auch Ihre Mobiltelefone aus.Sie treffen Ihre Entscheidungen am Computer.

Alle Entscheidungen bleiben anonym. Das heißt, Sie erfahren die Identität anderer Teilnehmer nicht, und kein Teilnehmer erfährt Ihre Identität. Der sprachlichen Einfachheit halber verwenden wir nur männliche Bezeichnungen.hr Verdienst in diesem Experiment wird Ihnen am Ende des Experiments privat und in bar ausbezahlt. Für Ihr pünktliches Erscheinen bekommen Sie eine Teilnahmeprämie in Höhe von 5 Euro. Das Experiment besteht aus 2 Teilen. Zu Beginn jeden Teils erhalten Sie detaillierte Instruktionen. Die Teile sind unabhängig voneinander, das heißt, Ihre Entscheidungen in Teil 1 haben keine Auswirkungen auf Teil 2.

<u>Der Ablauf heute:</u>

1. Instruktionen für Teil 1 lesen
2. Kontrollfragen beantworten
3. Experiment Teil 1
4. Instruktionen für Teil 2 lesen
5. Experiment Teil 2
6. Fragebogen
7. Auszahlung und Ende

Teil 1:

Perioden, Rollen, Gruppen
Dieses Experiment besteht aus 16 Perioden. Es gibt zwei Rollen: Teilnehmer A und Teilnehmer B. Zu Beginn des Experiments erfahren Sie, welche Rolle Ihnen per Zufall zugeordnet wurde. Diese Rolle werden Sie bis zum Ende des Experiments beibehalten.

In Ihrer Gruppe sind insgesamt neun Teilnehmer: drei Teilnehmer A und sechs Teilnehmer B. Jedem Teilnehmer A wird eine Nummer zugewiesen, die ihn im Verlauf des Experiments eindeutig identifiziert. Es gibt Teilnehmer A1, Teilnehmer A2 und Teilnehmer A3. Die Nummern der Teilnehmer A sind fest, d.h. Nummer 1 repräsentiert immer denselben Teilnehmer A. Die Teilnehmer B werden keine Nummern bekommen. Teilnehmer B werden nicht identifizierbar sein. In jeder Periode interagiert jeder Teilnehmer A mit einer Anzahl von Teilnehmern B, die zwischen null und sechs variieren kann. Jeder Teilnehmer B interagiert mit maximal einem Teilnehmer A.
Periode 1 und Perioden 2 bis 16
In Periode 1 werden jedem Teilnehmer A zunächst zufällig zwei Teilnehmer B zugeordnet. Ab Periode 2 entscheidet jeder Teilnehmer B, ob er in dieser Periode interagieren möchte und falls ja, mit welchem Teilnehmer A.

Benötigte Leistung für Teilnehmer B
In jeder Periode benötigt jeder Teilnehmer B entweder eine große oder eine kleine Leistung. Diese Leistungen werden vom Teilnehmer A bereitgestellt. Ob der Teilnehmer B eine große oder eine kleine Leistung benötigt, wird für jeden Teilnehmer B in jeder Periode neu und unabhängig per Zufall vom Computer bestimmt. Per Zufall bedeutet, dass in jeder Periode für jeden Teilnehmer B eine (virtuelle) Münze geworfen wird. „Kopf“ bedeutet, dass Teilnehmer B eine große Leistung benötigt, „Zahl“ bedeutet, dass Teilnehmer B eine kleine Leistung benötigt. Teilnehmer B werden nicht darüber informiert, welche Leistung sie benötigen.

Entscheidungen von Teilnehmer A
Bevor Teilnehmer A in jeder Periode seine Entscheidung trifft, erfährt er

1. wie viele Teilnehmer B mit ihm interagieren möchten
2. die benötigte Leistung von jedem dieser Teilnehmer

Sollten mehrere Teilnehmer B mit einem und demselben Teilnehmer A interagieren wollen, werden diese Teilnehmer B in jeder Periode in einer zufälligen Reihenfolge dem Teilnehmer A angezeigt. Damit ist es unmöglich für Teilnehmer A, die Teilnehmer B auseinander zu halten. Das einzige, was Teilnehmer A wissen wird, ist, welche Leistung jeder Teilnehmer B in dieser Periode benötigt.

Dann wählt Teilnehmer A für jeden Teilnehmer B eine Kombination aus einem Preis und einer Leistung aus. Es gibt folgende Preis-Leistungs-Kombinationen:

1. Preis 4 ECU, kleine Leistung

2. Preis 8 ECU, kleine Leistung
3. Preis 4 ECU, große Leistung
4. Preis 8 ECU, große Leistung

Wählt Teilnehmer A die kleine Leistung, so hat er Kosten von 2 ECU. Wählt Teilnehmer A die große Leistung, so hat er Kosten von 6 ECU.

Auszahlungen
Die Interaktionsauszahlung von Teilnehmer A ist seine Auszahlung aus der Interaktion mit einem Teilnehmer B. Die Interaktionsauszahlung ist der Unterschied zwischen dem verlangten Preis und den Kosten für die gewählte Leistung:

$$Interaktionsauszahlung\ A = Preis - Kosten\ fuer\ die\ gewaehlte\ Leistung.$$

Wird Teilnehmer A von mehreren Teilnehmern B gewählt, so ist die Periodenauszahlung von Teilnehmer A die Summe seiner Interaktionsauszahlungen. Es ist möglich, dass ein Teilnehmer A von keinem Teilnehmer B ausgewählt wird. In diesem Fall ist die Periodenauszahlung von diesem Teilnehmer A gleich 1,60 ECU. Die Interaktionsauszahlung von Teilnehmer B hängt davon ab, welche Leistung er benötigt und welche Leistung er bekommen hat.

Benötigt Teilnehmer B die kleine Leistung, dann verdient er immer

$$10\ ECU - Preis.$$

Benötigt Teilnehmer B die große Leistung und bekommt die große Leistung, so verdient er

$$10\ ECU - Preis.$$

Benötigt Teilnehmer B die große Leistung, bekommt aber die kleine Leistung, so verdient er

$$0\ ECU - Preis.$$

Die Periodenauszahlung von Teilnehmer B ist gleich seiner Interaktionsauszahlung.

Wenn ein Teilnehmer B nicht interagieren möchte, bekommt er 1,60 ECU als Periodenauszahlung.

Hier ist ein Überblick über die Interaktionsauszahlungen (in ECU) von Teilnehmer A und Teilnehmer B für alle möglichen Kombinationen aus benötigter Leistung, gewählter Leistung und gewähltem Preis (sofern Teilnehmer B interagieren möchte):
Als Beispiel betrachten wir die Zeile in der obigen Tabelle, in der Teilnehmer A den Preis 4 und die kleine Leistung wählt. Dann verdient Teilnehmer A immer $4 - 2 = 2\ ECU$ (letzte Spalte). Wie viel Teilnehmer B verdient, hängt davon ab, welche Leistung Teilnehmer B benötigt. Wenn Teilnehmer B die

	B verdient		
A wählt	B benötigt die kleine Leistung	B benötigt die große Leistung	A verdient
Preis 4, kleine Leistung	10 - 4 = 6	0 - 4 = -4	4 - 2 = 2
Preis 8, kleine Leistung	10 - 8 = 2	0 - 8 = -8	8 - 2 = 6
Preis 4, große Leistung	10 - 4 = 6	10 - 4 = 6	4 - 6 = -2
Preis 8, große Leistung	10 - 8 = 2	10 - 8 = 2	8 - 6 = 2

kleine Leistung benötigt, verdient er $10 - 4 = 6\ ECU$. Wenn Teilnehmer B die große Leistung benötigt, verdient er $0 - 4 = -4\ ECU$.
Zu Beginn des Experiments erhält jeder Teilnehmer eine Anfangsausstattung von 40 ECU. Mit dieser Anfangsausstattung können mögliche Verluste in einzelnen Perioden ausgleichen werden.

Am Ende des Experiments werden alle Periodenauszahlungen und die Anfangsaustattung aufsummiert und in bar ausgezahlt.

Feedback für Teilnehmer B
Am Ende jeder Periode werden alle Teilnehmer B darüber informiert, wie viel sie in dieser Periode verdient haben. Teilnehmer B werden nicht darüber informiert, welche Leistung sie benötigt haben und welche Leistung sie bekommen haben. Allerdings werden Teilnehmer B in manchen Fällen erkennen können, welche Leistung sie benötigt und bekommen haben:

1. Eine negative Interaktionsauszahlung sagt Teilnehmer B, dass er die große Leistung benötigt und die kleine Leistung bekommen hat.
2. Aus einer Interaktionsauszahlung von 2 ECU oder 6 ECU kann Teilnehmer B nicht schließen, welche Leistung er benötigt und welche er bekommen hat. Teilnehmer B hat entweder die kleine Leistung benötigt und bekommen, oder die große Leistung benötigt und bekommen, oder die kleine Leistung benötigt und die große Leistung bekommen.

Feedback für Teilnehmer A
Am Ende jeder Periode wird jeder Teilnehmer A darüber informiert, wie viel er verdient hat.

Entscheidungen von Teilnehmern B

Perioden 2 bis 16
Zu Beginn von jeder der Perioden 2 bis 16 sieht jeder Teilnehmer B eine Tabelle mit seinen eigenen bisherigen Interaktionen. Die Tabelle erinnert Teilnehmer B daran, mit welchem Teilnehmer A er in welcher Periode interagiert hat, welchen Preis er bezahlt hat und welche Interaktionsauszahlung er erhalten hat.

Anhand dieser Informationen wählt Teilnehmer B den Teilnehmer A, mit dem er interagieren möchte. Teilnehmer B kann aber auch entscheiden, in dieser Periode nicht zu interagieren. Somit endet die Periode für ihn.
Ablauf einer Periode ab Periode 2 im Überblick

1. Teilnehmer B werden daran erinnert, mit welchen Teilnehmern A sie persönlich bisher interagiert haben.

2. Teilnehmer B entscheiden, ob sie interagieren möchten und falls ja, mit welchem Teilnehmer A. (Wenn Teilnehmer B nicht interagieren möchte, dann endet die Periode für ihn. Wenn ein Teilnehmer A von keinem Teilnehmer B ausgewählt wurde, dann endet die Periode für diesen Teilnehmer A.)

3. Jeder Teilnehmer A wird über die benötigte Leistung von den Teilnehmern B informiert, die mit ihm interagieren möchten.

4. Teilnehmer A wählen eine Preis-Leistungs-Kombination für jeden einzelnen Teilnehmer B, mit dem sie interagieren.

5. Teilnehmer B bekommen Feedback.

6. Teilnehmer A bekommen Feedback.

Schritt 1 und 2 entfallen in Periode 1, da dort jedem Teilnehmer A zwei Teilnehmer B per Zufall zugewiesen werden.

Feedback am Ende des Experiments
In the end of the experiment each participant is informed about how much they have earned in the experiment.

Der Wechselkurs ist $1\ ECU = 0.14\ EUR$**.**

Kontrollfragen und weiteres Vorgehen
Zunächst werden Sie gebeten, einige Kontrollfragen zu beantworten, um sicherzustellen, dass Sie die Instruktionen und das Experiment verstanden haben. Nachdem all Ihre eventuellen Fragen von den Experimentatoren beantwortet wurden, wird das Experiment beginnen. Die Instruktionen für Teil 2 werden ausgeteilt, nachdem Teil 1 abgeschlossen ist.

[U Rating Condition:]

Herzlich Willkommen und vielen Dank für Ihre Teilnahme!

Diese Instruktionen sind für alle Teilnehmer identisch.Sie werden in diesem Experiment Geld verdienen, das abhängig ist von Ihren Entscheidungen und denen der anderen Teilnehmer. Es ist daher sehr wichtig, dass Sie diese Instruktionen sorgfältig durchlesen. Sollten Sie eine Frage haben, heben Sie bitte Ihre Hand. Wir werden dann zu Ihnen kommen und Ihre Frage beantworten. Bitte beachten Sie, dass es während des gesamten Experiments nicht gestattet ist, mit anderen Teilnehmern zu sprechen. Bitte schalten Sie jetzt auch Ihre Mobiltelefone aus.Sie treffen Ihre Entscheidungen am Computer.

Alle Entscheidungen bleiben anonym. Das heißt, Sie erfahren die Identität anderer Teilnehmer nicht, und kein Teilnehmer erfährt Ihre Identität. Der sprachlichen Einfachheit halber verwenden wir nur männliche Bezeichnungen.hr Verdienst in diesem Experiment wird Ihnen am Ende des Experiments privat und in bar ausbezahlt. Für Ihr pünktliches Erscheinen bekommen Sie eine Teilnahmeprämie in Höhe von 5 Euro. Das Experiment besteht aus 2 Teilen. Zu Beginn jeden Teils erhalten Sie detaillierte Instruktionen. Die Teile sind unabhängig voneinander, das heißt, Ihre Entscheidungen in Teil 1 haben keine Auswirkungen auf Teil 2.

Der Ablauf heute:

1. Instruktionen für Teil 1 lesen
2. Kontrollfragen beantworten
3. Experiment Teil 1
4. Instruktionen für Teil 2 lesen
5. Experiment Teil 2
6. Fragebogen
7. Auszahlung und Ende

Teil 1:

Perioden, Rollen, Gruppen
Dieses Experiment besteht aus 16 Perioden. Es gibt zwei Rollen: Teilnehmer A und Teilnehmer B. Zu Beginn des Experiments erfahren Sie, welche Rolle Ihnen per Zufall zugeordnet wurde. Diese Rolle werden Sie bis zum Ende des Experiments beibehalten.

In Ihrer Gruppe sind insgesamt neun Teilnehmer: drei Teilnehmer A und sechs Teilnehmer B. Jedem Teilnehmer A wird eine Nummer zugewiesen, die ihn im Verlauf des Experiments eindeutig identifiziert. Es gibt Teilnehmer A1, Teilnehmer A2 und Teilnehmer A3. Die Nummern der Teilnehmer A sind fest, d.h. Nummer 1 repräsentiert immer denselben Teilnehmer A. Die Teilnehmer B werden keine Nummern bekommen. Teilnehmer B werden nicht identifizierbar sein. In jeder Periode interagiert jeder Teilnehmer A mit einer Anzahl von Teilnehmern B, die zwischen null und sechs variieren kann. Jeder Teilnehmer B interagiert mit maximal einem Teilnehmer A.
Periode 1 und Perioden 2 bis 16
In Periode 1 werden jedem Teilnehmer A zunächst zufällig zwei Teilnehmer B zugeordnet. Ab Periode 2 entscheidet jeder Teilnehmer B, ob er in dieser Periode interagieren möchte und falls ja, mit welchem Teilnehmer A.

Benötigte Leistung für Teilnehmer B
In jeder Periode benötigt jeder Teilnehmer B entweder eine große oder eine kleine Leistung. Diese Leistungen werden vom Teilnehmer A bereitgestellt. Ob der Teilnehmer B eine große oder eine kleine Leistung benötigt, wird für jeden Teilnehmer B in jeder Periode neu und unabhängig per Zufall vom Computer bestimmt. Per Zufall bedeutet, dass in jeder Periode für jeden Teilnehmer B eine (virtuelle) Münze geworfen wird. „Kopf“ bedeutet, dass Teilnehmer B eine große Leistung benötigt, „Zahl“ bedeutet, dass Teilnehmer B eine kleine Leistung benötigt. Teilnehmer B werden nicht darüber informiert, welche Leistung sie benötigen.

Entscheidungen von Teilnehmer A
Bevor Teilnehmer A in jeder Periode seine Entscheidung trifft, erfährt er

1. wie viele Teilnehmer B mit ihm interagieren möchten

2. die benötigte Leistung von jedem dieser Teilnehmer

Sollten mehrere Teilnehmer B mit einem und demselben Teilnehmer A interagieren wollen, werden diese Teilnehmer B in jeder Periode in einer zufälligen Reihenfolge dem Teilnehmer A angezeigt. Damit ist es unmöglich für Teilnehmer A, die Teilnehmer B auseinander zu halten. Das einzige, was Teilnehmer A wissen wird, ist, welche Leistung jeder Teilnehmer B in dieser Periode benötigt.

Dann wählt Teilnehmer A für jeden Teilnehmer B eine Kombination aus einem Preis und einer Leistung aus. Es gibt folgende Preis-Leistungs-Kombinationen:

1. Preis 4 ECU, kleine Leistung

2. Preis 8 ECU, kleine Leistung
3. Preis 4 ECU, große Leistung
4. Preis 8 ECU, große Leistung

Wählt Teilnehmer A die kleine Leistung, so hat er Kosten von 2 ECU. Wählt Teilnehmer A die große Leistung, so hat er Kosten von 6 ECU.

Auszahlungen
Die Interaktionsauszahlung von Teilnehmer A ist seine Auszahlung aus der Interaktion mit einem Teilnehmer B. Die Interaktionsauszahlung ist der Unterschied zwischen dem verlangten Preis und den Kosten für die gewählte Leistung:

$$Interaktionsauszahlung\ A = Preis - Kosten\ fuer\ die\ gewaehlte\ Leistung.$$

Wird Teilnehmer A von mehreren Teilnehmern B gewählt, so ist die Periodenauszahlung von Teilnehmer A die Summe seiner Interaktionsauszahlungen. Es ist möglich, dass ein Teilnehmer A von keinem Teilnehmer B ausgewählt wird. In diesem Fall ist die Periodenauszahlung von diesem Teilnehmer A gleich 1,60 ECU. Die Interaktionsauszahlung von Teilnehmer B hängt davon ab, welche Leistung er benötigt und welche Leistung er bekommen hat.

Benötigt Teilnehmer B die kleine Leistung, dann verdient er immer

$$10\ ECU - Preis.$$

Benötigt Teilnehmer B die große Leistung und bekommt die große Leistung, so verdient er

$$10\ ECU - Preis.$$

Benötigt Teilnehmer B die große Leistung, bekommt aber die kleine Leistung, so verdient er

$$0\ ECU - Preis.$$

Die Periodenauszahlung von Teilnehmer B ist gleich seiner Interaktionsauszahlung.

Wenn ein Teilnehmer B nicht interagieren möchte, bekommt er 1,60 ECU als Periodenauszahlung.

Hier ist ein Überblick über die Interaktionsauszahlungen (in ECU) von Teilnehmer A und Teilnehmer B für alle möglichen Kombinationen aus benötigter Leistung, gewählter Leistung und gewähltem Preis (sofern Teilnehmer B interagieren möchte):
Als Beispiel betrachten wir die Zeile in der obigen Tabelle, in der Teilnehmer A den Preis 4 und die kleine Leistung wählt. Dann verdient Teilnehmer A immer $4 - 2 = 2\ ECU$ (letzte Spalte). Wie viel Teilnehmer B verdient, hängt davon ab, welche Leistung Teilnehmer B benötigt. Wenn Teilnehmer B die

	B verdient		
A wählt	B benötigt die kleine Leistung	B benötigt die große Leistung	A verdient
Preis 4, kleine Leistung	10 - 4 = 6	0 - 4 = -4	4 - 2 = 2
Preis 8, kleine Leistung	10 - 8 = 2	0 - 8 = -8	8 - 2 = 6
Preis 4, große Leistung	10 - 4 = 6	10 - 4 = 6	4 - 6 = -2
Preis 8, große Leistung	10 - 8 = 2	10 - 8 = 2	8 - 6 = 2

kleine Leistung benötigt, verdient er $10 - 4 = 6\ ECU$. Wenn Teilnehmer B die große Leistung benötigt, verdient er $0 - 4 = -4\ ECU$.
Zu Beginn des Experiments erhält jeder Teilnehmer eine Anfangsausstattung von 40 ECU. Mit dieser Anfangsausstattung können mögliche Verluste in einzelnen Perioden ausgleichen werden.

Am Ende des Experiments werden alle Periodenauszahlungen und die Anfangsaustattung aufsummiert und in bar ausgezahlt.

Feedback für Teilnehmer B
Am Ende jeder Periode werden alle Teilnehmer B darüber informiert, wie viel sie in dieser Periode verdient haben. Teilnehmer B werden nicht darüber informiert, welche Leistung sie benötigt haben und welche Leistung sie bekommen haben. Allerdings werden Teilnehmer B in manchen Fällen erkennen können, welche Leistung sie benötigt und bekommen haben:

1. Eine negative Interaktionsauszahlung sagt Teilnehmer B, dass er die große Leistung benötigt und die kleine Leistung bekommen hat.
2. Aus einer Interaktionsauszahlung von 2 ECU oder 6 ECU kann Teilnehmer B nicht schließen, welche Leistung er benötigt und welche er bekommen hat. Teilnehmer B hat entweder die kleine Leistung benötigt und bekommen, oder die große Leistung benötigt und bekommen, oder die kleine Leistung benötigt und die große Leistung bekommen.

Entscheidungen von Teilnehmern B
Nachdem Teilnehmer B seine Interaktionszahlung erfahren hat, kann Teilnehmer B die Interaktion mit dem Teilnehmer A bewerten. Dabei wählt Teilnehmer B zwischen „zufrieden“, „unzufrieden“ und „keine Bewertung abgeben“.

Feedback für Teilnehmer A
Am Ende jeder Periode wird jeder Teilnehmer A darüber informiert, für welche seiner Entscheidungen er welche Bewertung in der aktuellen Periode bekommen hat und wie viel er verdient hat.

Perioden 2 bis 16
Zu Beginn einer jeden Periode ab Periode 2 werden allen Teilnehmern A und allen Teilnehmern B alle bisherigen Bewertungen der drei Teilnehmer A angezeigt. D.h., Teilnehmer A und Teilnehmer B sehen für jeden Teilnehmer A:

1. die Anzahl aller bisher gesammelten Bewertungen mit „zufrieden“

2. die Anzahl aller bisher gesammelten Bewertungen mit „unzufrieden“

3. die Anzahl der fehlenden Bewertungen, also die Anzahl „keine Bewertung“

4. die Anzahl aller bisherigen Interaktionen

Außerdem sieht jeder Teilnehmer B eine Tabelle mit seinen eigenen bisherigen Interaktionen. Die Tabelle erinnert Teilnehmer B daran, mit welchem Teilnehmer A er in welcher Periode interagiert hat, welchen Preis er bezahlt hat, welche Interaktionsauszahlung er erhalten hat und welche Bewertung er gegeben hat.
Anhand dieser Informationen wählt Teilnehmer B den Teilnehmer A, mit dem er interagieren möchten. Teilnehmer B kann aber auch entscheiden, in dieser Periode nicht zu interagieren. Somit endet die Periode für ihn.

Ablauf einer Periode ab Periode 2 im Überblick

1. Alle Teilnehmer sehen die Bewertungen von Teilnehmern A. Zusätzlich werden Teilnehmer B daran erinnert, mit welchen Teilnehmern A sie persönlich bisher interagiert haben.

2. Teilnehmer B entscheiden, ob sie interagieren möchten und falls ja, mit welchem Teilnehmer A. (Wenn Teilnehmer B nicht interagieren möchte, dann endet die Periode für ihn. Wenn ein Teilnehmer A von keinem Teilnehmer B ausgewählt wurde, dann endet die Periode für diesen Teilnehmer A.)

3. Jeder Teilnehmer A wird über die benötigte Leistung von den Teilnehmern B informiert, die mit ihm interagieren möchten.

4. Teilnehmer A wählen eine Preis-Leistungs-Kombination für jeden einzelnen Teilnehmer B, mit dem sie interagieren.

5. Teilnehmer B bekommen Feedback.

6. Teilnehmer B können die Interaktion mit Teilnehmer A bewerten, sofern sie mit einem Teilnehmer A interagiert haben.

7. Teilnehmer A sehen für jeden Teilnehmer B, mit dem sie interagiert haben, welche Leistung er benötigt hat, welche Preis-Leistungs-Kombination für ihn gewählt wurde und welche Bewertung er gegeben hat. Darüber hinaus erfahren Teilnehmer A, wie viel sie in dieser Periode verdient haben.

Schritt 1 und 2 entfallen in Periode 1, da dort jedem Teilnehmer A zwei Teilnehmer B per Zufall zugewiesen werden.

Feedback am Ende des Experiments
In the end of the experiment each participant is informed about how much they have earned in the experiment.

Der Wechselkurs ist $1\ ECU = 0.14\ EUR$**.**

Kontrollfragen und weiteres Vorgehen

Zunächst werden Sie gebeten, einige Kontrollfragen zu beantworten, um sicherzustellen, dass Sie die Instruktionen und das Experiment verstanden haben. Nachdem all Ihre eventuellen Fragen von den Experimentatoren beantwortet wurden, wird das Experiment beginnen. Die Instruktionen für Teil 2 werden ausgeteilt, nachdem Teil 1 abgeschlossen ist.

[U-I-Pooled Condition:]

Herzlich Willkommen und vielen Dank für Ihre Teilnahme!

Diese Instruktionen sind für alle Teilnehmer identisch.Sie werden in diesem Experiment Geld verdienen, das abhängig ist von Ihren Entscheidungen und denen der anderen Teilnehmer. Es ist daher sehr wichtig, dass Sie diese Instruktionen sorgfältig durchlesen. Sollten Sie eine Frage haben, heben Sie bitte Ihre Hand. Wir werden dann zu Ihnen kommen und Ihre Frage beantworten. Bitte beachten Sie, dass es während des gesamten Experiments nicht gestattet ist, mit anderen Teilnehmern zu sprechen. Bitte schalten Sie jetzt auch Ihre Mobiltelefone aus.Sie treffen Ihre Entscheidungen am Computer.

Alle Entscheidungen bleiben anonym. Das heißt, Sie erfahren die Identität anderer Teilnehmer nicht, und kein Teilnehmer erfährt Ihre Identität. Der sprachlichen Einfachheit halber verwenden wir nur männliche Bezeichnungen.hr Verdienst in diesem Experiment wird Ihnen am Ende des Experiments privat und in bar ausbezahlt. Für Ihr pünktliches Erscheinen bekommen Sie eine Teilnahmeprämie in Höhe von 5 Euro. Das Experiment besteht aus 2 Teilen. Zu Beginn jeden Teils erhalten Sie detaillierte Instruktionen. Die Teile sind unabhängig voneinander, das heißt, Ihre Entscheidungen in Teil 1 haben keine Auswirkungen auf Teil 2.

Der Ablauf heute:

1. Instruktionen für Teil 1 lesen
2. Kontrollfragen beantworten
3. Experiment Teil 1
4. Instruktionen für Teil 2 lesen
5. Experiment Teil 2
6. Fragebogen
7. Auszahlung und Ende

Teil 1:

Perioden, Rollen, Gruppen
Dieses Experiment besteht aus 16 Perioden. Es gibt drei Rollen: Teilnehmer A, Teilnehmer B und Teilnehmer C. Zu Beginn des Experiments erfahren Sie, welche Rolle Ihnen per Zufall zugeordnet wurde. Diese Rolle werden Sie bis zum Ende des Experiments beibehalten.

In Ihrer Gruppe sind insgesamt neun Teilnehmer: drei Teilnehmer A, drei Teilnehmer B und drei Teilnehmer C. Jedem Teilnehmer A wird eine Nummer zugewiesen, die ihn im Verlauf des Experiments eindeutig identifiziert. Es gibt Teilnehmer A1, Teilnehmer A2 und Teilnehmer A3. Die Nummern der Teilnehmer A sind fest, d.h. Nummer 1 repräsentiert immer denselben Teilnehmer A. Die Teilnehmer B und die Teilnehmer C werden keine Nummern bekommen. Teilnehmer B und Teilnehmer C werden nicht identifizierbar sein. In jeder Periode interagiert jeder Teilnehmer A mit einer Anzahl von Teilnehmern B, die zwischen null und drei variieren kann und mit einer Anzahl von Teilnehmern C, die zwischen null und drei variieren kann. Jeder Teilnehmer B interagiert mit maximal einem Teilnehmer A. Auch jeder Teilnehmer C interagiert mit maximal einem Teilnehmer A.

Periode 1 und Perioden 2 bis 16
In Periode 1 werden jedem Teilnehmer A zunächst zufällig ein Teilnehmer B und ein Teilnehmer C zugeordnet. Ab Periode 2 entscheidet jeder Teilnehmer B, ob er in dieser Periode interagieren möchte und falls ja, mit welchem Teilnehmer A. Ab Periode 2 entscheidet auch jeder Teilnehmer C, ob er in dieser Periode interagieren möchte und falls ja, mit welchem Teilnehmer A.

Benötigte Leistung für Teilnehmer B und Teilnehmer C
In jeder Periode benötigen jeder Teilnehmer B und jeder Teilnehmer C entweder eine große oder eine kleine Leistung. Diese Leistungen werden vom Teilnehmer A bereitgestellt. Ob der Teilnehmer B und der Teilnehmer C eine große oder eine kleine Leistung benötigen, wird für jeden Teilnehmer B und für jeden Teilnehmer C in jeder Periode neu und unabhängig per Zufall vom Computer bestimmt. Per Zufall bedeutet, dass in jeder Periode für jeden Teilnehmer B und für jeden Teilnehmer C jeweils eine (virtuelle) Münze geworfen wird. „Kopf" bedeutet, dass Teilnehmer B oder Teilnehmer C eine große Leistung benötigt, „Zahl" bedeutet, dass Teilnehmer B oder Teilnehmer C eine kleine Leistung benötigt. Teilnehmer B und Teilnehmer C werden nicht darüber informiert, welche Leistung sie benötigen.

Entscheidungen von Teilnehmer A
Bevor Teilnehmer A in jeder Periode seine Entscheidung trifft, erfährt er

1. wie viele Teilnehmer B und wie viele Teilnehmer C mit ihm interagieren möchten

2. die benötigte Leistung von jedem dieser Teilnehmer

Sollten mehrere Teilnehmer B mit einem und demselben Teilnehmer A interagieren wollen, werden diese Teilnehmer B in jeder Periode in einer zufäl-

ligen Reihenfolge dem Teilnehmer A angezeigt. Damit ist es unmöglich für Teilnehmer A, die Teilnehmer B auseinander zu halten. Das einzige, was Teilnehmer A wissen wird, ist, welche Leistung jeder Teilnehmer B in dieser Periode benötigt. Das Gleiche gilt für Teilnehmer C.
Dann wählt Teilnehmer A für jeden Teilnehmer B und für jeden Teilnehmer C eine Kombination aus einem Preis und einer Leistung aus. Es gibt folgende Preis-Leistungs-Kombinationen:

1. Preis 4 ECU, kleine Leistung
2. Preis 8 ECU, kleine Leistung
3. Preis 4 ECU, große Leistung
4. Preis 8 ECU, große Leistung

Wählt Teilnehmer A die kleine Leistung, so hat er Kosten von 2 ECU. Wählt Teilnehmer A die große Leistung, so hat er Kosten von 6 ECU.
Auszahlungen
Die Interaktionsauszahlung von Teilnehmer A ist seine Auszahlung aus der Interaktion mit einem Teilnehmer B oder einem Teilnehmer C. Die Interaktionsauszahlung ist der Unterschied zwischen dem verlangten Preis und den Kosten für die gewählte Leistung:

$$Interaction\ Payoff = Price - Costs\ of\ chosen\ service.$$

Wird Teilnehmer A von mehreren Teilnehmern B bzw. Teilnehmern C gewählt, so ist die Periodenauszahlung von Teilnehmer A die Summe seiner Interaktionsauszahlungen. Es ist möglich, dass ein Teilnehmer A von keinem Teilnehmer B und keinem Teilnehmer C ausgewählt wird. In diesem Fall ist die Periodenauszahlung von diesem Teilnehmer A gleich $1,60 ECU$. Die Interaktionsauszahlung von Teilnehmer B bzw. Teilnehmer C hängt davon ab, welche Leistung er benötigt und welche Leistung er bekommen hat.

Benötigt Teilnehmer B bzw. Teilnehmer C die kleine Leistung, dann verdient er immer

$$10\ ECU - Preis.$$

Benötigt Teilnehmer B bzw. Teilnehmer C die große Leistung und bekommt die große Leistung, so verdient er

$$10\ ECU - Preis.$$

Benötigt Teilnehmer B bzw. Teilnehmer C die große Leistung, bekommt aber die kleine Leistung, so verdient er

$$0\ ECU - Preis.$$

Die Periodenauszahlung von Teilnehmer B bzw. Teilnehmer C ist gleich seiner Interaktionsauszahlung.

Wenn ein Teilnehmer B bzw. Teilnehmer C nicht interagieren möchte, bekommt er 1,60 ECU als Periodenauszahlung.

Hier ist ein Überblick über die Interaktionsauszahlungen (in ECU) von Teilnehmer A , Teilnehmer B und Teilnehmer C für alle möglichen Kombinationen aus benötigter Leistung, gewählter Leistung und gewähltem Preis (sofern Teilnehmer B bzw. Teilnehmer C interagieren möchte):

	B bzw. C verdient		
A wählt	B bzw. C benötigt die kleine Leistung	B bzw. C benötigt die große Leistung	A verdient
Preis 4, kleine Leistung	10 - 4 = 6	0 - 4 = -4	4 - 2 = 2
Preis 8, kleine Leistung	10 - 8 = 2	0 - 8 = -8	8 - 2 = 6
Preis 4, große Leistung	10 - 4 = 6	10 - 4 = 6	4 - 6 = -2
Preis 8, große Leistung	10 - 8 = 2	10 - 8 = 2	8 - 6 = 2

Als Beispiel betrachten wir die Zeile in der obigen Tabelle, in der Teilnehmer A den Preis 4 und die kleine Leistung wählt. Dann verdient Teilnehmer A immer $4 - 2 = 2$ ECU (letzte Spalte). Wie viel Teilnehmer B bzw. C verdient, hängt davon ab, welche Leistung Teilnehmer B bzw. C benötigt. Wenn Teilnehmer B bzw. C die kleine Leistung benötigt, verdient er $10 - 4 = 6$ ECU. Wenn Teilnehmer B bzw. C die große Leistung benötigt, verdient er $0 - 4 = -4$ ECU.
Zu Beginn des Experiments erhält jeder Teilnehmer eine Anfangsausstattung von 40 ECU. Mit dieser Anfangsausstattung können mögliche Verluste in einzelnen Perioden ausgleichen werden.

Am Ende des Experiments werden alle Periodenauszahlungen und die Anfangsaustattung aufsummiert und in bar ausgezahlt.

Feedback für Teilnehmer B und Teilnehmer C
Am Ende jeder Periode werden alle Teilnehmer B und alle Teilnehmer C darüber informiert, wie viel sie in dieser Periode verdient haben. Teilnehmer C werden außerdem immer darüber informiert, welche Leistung sie benötigt haben und welche Leistung sie bekommen haben. Teilnehmer B werden nicht darüber informiert, welche Leistung sie benötigt haben und welche Leistung sie bekommen haben. Allerdings werden Teilnehmer B in manchen Fällen erkennen können, welche Leistung sie benötigt und bekommen haben:

1. Eine negative Interaktionsauszahlung sagt Teilnehmer B, dass er die große Leistung benötigt und die kleine Leistung bekommen hat.

2. Aus einer Interaktionsauszahlung von 2 ECU oder 6 ECU kann Teilnehmer B nicht schließen, welche Leistung er benötigt und welche er bekommen hat. Teilnehmer B hat entweder die kleine Leistung benötigt und bekommen, oder die große Leistung benötigt und bekommen, oder die kleine Leistung benötigt und die große Leistung bekommen.

Entscheidungen von Teilnehmern B und Teilnehmern C
Nachdem Teilnehmer B seine Interaktionszahlung erfahren hat und nachdem Teilnehmer C seine Interaktionszahlung, seine benötigte Leistung und seine erhaltene Leistung erfahren hat, können Teilnehmer B und Teilnehmer C die Interaktion mit dem Teilnehmer A bewerten. Dabei wählen Teilnehmer B und Teilnehmer C zwischen „zufrieden“, „unzufrieden“ und „keine Bewertung abgeben“.

Feedback für Teilnehmer A
Am Ende jeder Periode wird jeder Teilnehmer A darüber informiert, für welche seiner Entscheidungen er welche Bewertung in der aktuellen Periode bekommen hat und wie viel er verdient hat.

Zu Beginn einer jeden Periode ab Periode 2 werden allen Teilnehmern A, allen Teilnehmern B und allen Teilnehmer C alle bisherigen Bewertungen der drei Teilnehmer A angezeigt. D.h., Teilnehmer A, Teilnehmer B und Teilnehmer C sehen für jeden Teilnehmer A:

1. die Anzahl aller bisher gesammelten Bewertungen mit „zufrieden“
2. die Anzahl aller bisher gesammelten Bewertungen mit „unzufrieden“
3. die Anzahl der fehlenden Bewertungen, also die Anzahl „keine Bewertung“
4. die Anzahl aller bisherigen Interaktionen

Außerdem sehen jeder Teilnehmer B und jeder Teilnehmer C eine Tabelle mit ihren eigenen bisherigen Interaktionen. Die Tabelle erinnert Teilnehmer B bzw. Teilnehmer C daran, mit welchem Teilnehmer A er in welcher Periode interagiert hat, welchen Preis er bezahlt hat, welche Interaktionsauszahlung er erhalten hat und welche Bewertung er gegeben hat. Teilnehmer C wird zusätzlich daran erinnert, welche Leistung er in der jeweiligen Periode benötigt und welche er bekommen hat.

Anhand dieser Informationen wählen Teilnehmer B und Teilnehmer C den Teilnehmer A, mit dem sie interagieren möchten. Teilnehmer B bzw. Teilnehmer C kann aber auch entscheiden, in dieser Periode nicht zu interagieren. Somit endet die Periode für ihn.

Ablauf einer Periode ab Periode 2 im Überblick

1. Alle Teilnehmer sehen die Bewertungen von Teilnehmern A. Zusätzlich werden Teilnehmer B und Teilnehmer C daran erinnert, mit welchen Teilnehmern A sie persönlich bisher interagiert haben.
2. Teilnehmer B und Teilnehmer C entscheiden, ob sie interagieren möchten und falls ja, mit welchem Teilnehmer A. (Wenn Teilnehmer B bzw. Teilnehmer C nicht interagieren möchte, dann endet die Periode für ihn. Wenn ein Teilnehmer A von keinem Teilnehmer B und von keinem Teilnehmer C ausgewählt wurde, dann endet die Periode für diesen Teilnehmer A.)

3. Jeder Teilnehmer A wird über die benötigte Leistung von den Teilnehmern B und den Teilnehmern C informiert, die mit ihm interagieren möchten.

4. Teilnehmer A wählen eine Preis-Leistungs-Kombination für jeden einzelnen Teilnehmer B und Teilnehmer C, mit dem sie interagieren

5. Teilnehmer B und Teilnehmer C bekommen Feedback.

6. Teilnehmer B und Teilnehmer C können die Interaktion mit Teilnehmer A bewerten, sofern sie mit einem Teilnehmer A interagiert haben.

7. Teilnehmer A sehen für jeden Teilnehmer B und für jeden Teilnehmer C, mit dem sie interagiert haben, welche Leistung er benötigt hat, welche Preis-Leistungs-Kombination für ihn gewählt wurde und welche Bewertung er gegeben hat. Darüber hinaus erfahren Teilnehmer A, wie viel sie in dieser Periode verdient haben.

Schritt 1 und 2 entfallen in Periode 1, da dort jedem Teilnehmer A ein Teilnehmer B und ein Teilnehmer C per Zufall zugewiesen werden.

Feedback am Ende des Experiments
In the end of the experiment each participant is informed about how much they have earned in the experiment.

Der Wechselkurs ist $1\ ECU = 0.14\ EUR$**.**

Kontrollfragen und weiteres Vorgehen
Zunächst werden Sie gebeten, einige Kontrollfragen zu beantworten, um sicherzustellen, dass Sie die Instruktionen und das Experiment verstanden haben. Nachdem all Ihre eventuellen Fragen von den Experimentatoren beantwortet wurden, wird das Experiment beginnen. Die Instruktionen für Teil 2 werden ausgeteilt, nachdem Teil 1 abgeschlossen ist.

[U-I-Separate Condition:]

Herzlich Willkommen und vielen Dank für Ihre Teilnahme!

Diese Instruktionen sind für alle Teilnehmer identisch.Sie werden in diesem Experiment Geld verdienen, das abhängig ist von Ihren Entscheidungen und denen der anderen Teilnehmer. Es ist daher sehr wichtig, dass Sie diese Instruktionen sorgfältig durchlesen. Sollten Sie eine Frage haben, heben Sie bitte Ihre Hand. Wir werden dann zu Ihnen kommen und Ihre Frage beantworten. Bitte beachten Sie, dass es während des gesamten Experiments nicht gestattet ist, mit anderen Teilnehmern zu sprechen. Bitte schalten Sie jetzt auch Ihre Mobiltelefone aus.Sie treffen Ihre Entscheidungen am Computer.

Alle Entscheidungen bleiben anonym. Das heißt, Sie erfahren die Identität anderer Teilnehmer nicht, und kein Teilnehmer erfährt Ihre Identität. Der sprachlichen Einfachheit halber verwenden wir nur männliche Bezeichnungen.hr Verdienst in diesem Experiment wird Ihnen am Ende des Experiments privat und in bar ausbezahlt. Für Ihr pünktliches Erscheinen bekommen Sie eine Teilnahmeprämie in Höhe von 5 Euro. Das Experiment besteht aus 2 Teilen. Zu Beginn jeden Teils erhalten Sie detaillierte Instruktionen. Die Teile sind unabhängig voneinander, das heißt, Ihre Entscheidungen in Teil 1 haben keine Auswirkungen auf Teil 2.

Der Ablauf heute:

1. Instruktionen für Teil 1 lesen
2. Kontrollfragen beantworten
3. Experiment Teil 1
4. Instruktionen für Teil 2 lesen
5. Experiment Teil 2
6. Fragebogen
7. Auszahlung und Ende

Teil 1:

Perioden, Rollen, Gruppen
Dieses Experiment besteht aus 16 Perioden. Es gibt drei Rollen: Teilnehmer A, Teilnehmer B und Teilnehmer C. Zu Beginn des Experiments erfahren Sie, welche Rolle Ihnen per Zufall zugeordnet wurde. Diese Rolle werden Sie bis zum Ende des Experiments beibehalten.

In Ihrer Gruppe sind insgesamt neun Teilnehmer: drei Teilnehmer A, drei Teilnehmer B und drei Teilnehmer C. Jedem Teilnehmer A wird eine Nummer zugewiesen, die ihn im Verlauf des Experiments eindeutig identifiziert. Es gibt Teilnehmer A1, Teilnehmer A2 und Teilnehmer A3. Die Nummern der Teilnehmer A sind fest, d.h. Nummer 1 repräsentiert immer denselben Teilnehmer A. Die Teilnehmer B und die Teilnehmer C werden keine Nummern bekommen. Teilnehmer B und Teilnehmer C werden nicht identifizierbar sein. In jeder Periode interagiert jeder Teilnehmer A mit einer Anzahl von Teilnehmern B, die zwischen null und drei variieren kann und mit einer Anzahl von Teilnehmern C, die zwischen null und drei variieren kann. Jeder Teilnehmer B interagiert mit maximal einem Teilnehmer A. Auch jeder Teilnehmer C interagiert mit maximal einem Teilnehmer A.

Periode 1 und Perioden 2 bis 16
In Periode 1 werden jedem Teilnehmer A zunächst zufällig ein Teilnehmer B und ein Teilnehmer C zugeordnet. Ab Periode 2 entscheidet jeder Teilnehmer B, ob er in dieser Periode interagieren möchte und falls ja, mit welchem Teilnehmer A. Ab Periode 2 entscheidet auch jeder Teilnehmer C, ob er in dieser Periode interagieren möchte und falls ja, mit welchem Teilnehmer A.

Benötigte Leistung für Teilnehmer B und Teilnehmer C
In jeder Periode benötigen jeder Teilnehmer B und jeder Teilnehmer C entweder eine große oder eine kleine Leistung. Diese Leistungen werden vom Teilnehmer A bereitgestellt. Ob der Teilnehmer B und der Teilnehmer C eine große oder eine kleine Leistung benötigen, wird für jeden Teilnehmer B und für jeden Teilnehmer C in jeder Periode neu und unabhängig per Zufall vom Computer bestimmt. Per Zufall bedeutet, dass in jeder Periode für jeden Teilnehmer B und für jeden Teilnehmer C jeweils eine (virtuelle) Münze geworfen wird. „Kopf" bedeutet, dass Teilnehmer B oder Teilnehmer C eine große Leistung benötigt, „Zahl" bedeutet, dass Teilnehmer B oder Teilnehmer C eine kleine Leistung benötigt. Teilnehmer B und Teilnehmer C werden nicht darüber informiert, welche Leistung sie benötigen.

Entscheidungen von Teilnehmer A
Bevor Teilnehmer A in jeder Periode seine Entscheidung trifft, erfährt er

1. wie viele Teilnehmer B und wie viele Teilnehmer C mit ihm interagieren möchten

2. die benötigte Leistung von jedem dieser Teilnehmer

Sollten mehrere Teilnehmer B mit einem und demselben Teilnehmer A interagieren wollen, werden diese Teilnehmer B in jeder Periode in einer zufäl-

ligen Reihenfolge dem Teilnehmer A angezeigt. Damit ist es unmöglich für Teilnehmer A, die Teilnehmer B auseinander zu halten. Das einzige, was Teilnehmer A wissen wird, ist, welche Leistung jeder Teilnehmer B in dieser Periode benötigt. Das Gleiche gilt für Teilnehmer C.
Dann wählt Teilnehmer A für jeden Teilnehmer B und für jeden Teilnehmer C eine Kombination aus einem Preis und einer Leistung aus. Es gibt folgende Preis-Leistungs-Kombinationen:

1. Preis 4 ECU, kleine Leistung
2. Preis 8 ECU, kleine Leistung
3. Preis 4 ECU, große Leistung
4. Preis 8 ECU, große Leistung

Wählt Teilnehmer A die kleine Leistung, so hat er Kosten von 2 ECU. Wählt Teilnehmer A die große Leistung, so hat er Kosten von 6 ECU.
Auszahlungen
Die Interaktionsauszahlung von Teilnehmer A ist seine Auszahlung aus der Interaktion mit einem Teilnehmer B oder einem Teilnehmer C. Die Interaktionsauszahlung ist der Unterschied zwischen dem verlangten Preis und den Kosten für die gewählte Leistung:

$$Interaction\ Payoff = Price - Costs\ of\ chosen\ service.$$

Wird Teilnehmer A von mehreren Teilnehmern B bzw. Teilnehmern C gewählt, so ist die Periodenauszahlung von Teilnehmer A die Summe seiner Interaktionsauszahlungen. Es ist möglich, dass ein Teilnehmer A von keinem Teilnehmer B und keinem Teilnehmer C ausgewählt wird. In diesem Fall ist die Periodenauszahlung von diesem Teilnehmer A gleich $1,60 ECU$. Die Interaktionsauszahlung von Teilnehmer B bzw. Teilnehmer C hängt davon ab, welche Leistung er benötigt und welche Leistung er bekommen hat.

Benötigt Teilnehmer B bzw. Teilnehmer C die kleine Leistung, dann verdient er immer

$$10\ ECU - Preis.$$

Benötigt Teilnehmer B bzw. Teilnehmer C die große Leistung und bekommt die große Leistung, so verdient er

$$10\ ECU - Preis.$$

Benötigt Teilnehmer B bzw. Teilnehmer C die große Leistung, bekommt aber die kleine Leistung, so verdient er

$$0\ ECU - Preis.$$

Die Periodenauszahlung von Teilnehmer B bzw. Teilnehmer C ist gleich seiner Interaktionsauszahlung.

Wenn ein Teilnehmer B bzw. Teilnehmer C nicht interagieren möchte, bekommt er 1,60 ECU als Periodenauszahlung.

Hier ist ein Überblick über die Interaktionsauszahlungen (in ECU) von Teilnehmer A , Teilnehmer B und Teilnehmer C für alle möglichen Kombinationen aus benötigter Leistung, gewählter Leistung und gewähltem Preis (sofern Teilnehmer B bzw. Teilnehmer C interagieren möchte):

	B bzw. C verdient		
A wählt	B bzw. C benötigt die kleine Leistung	B bzw. C benötigt die große Leistung	A verdient
Preis 4, kleine Leistung	10 - 4 = 6	0 - 4 = -4	4 - 2 = 2
Preis 8, kleine Leistung	10 - 8 = 2	0 - 8 = -8	8 - 2 = 6
Preis 4, große Leistung	10 - 4 = 6	10 - 4 = 6	4 - 6 = -2
Preis 8, große Leistung	10 - 8 = 2	10 - 8 = 2	8 - 6 = 2

Als Beispiel betrachten wir die Zeile in der obigen Tabelle, in der Teilnehmer A den Preis 4 und die kleine Leistung wählt. Dann verdient Teilnehmer A immer $4 - 2 = 2\ ECU$ (letzte Spalte). Wie viel Teilnehmer B bzw. C verdient, hängt davon ab, welche Leistung Teilnehmer B bzw. C benötigt. Wenn Teilnehmer B bzw. C die kleine Leistung benötigt, verdient er $10 - 4 = 6\ ECU$. Wenn Teilnehmer B bzw. C die große Leistung benötigt, verdient er $0 - 4 = -4\ ECU$.
Zu Beginn des Experiments erhält jeder Teilnehmer eine Anfangsausstattung von 40 ECU. Mit dieser Anfangsausstattung können mögliche Verluste in einzelnen Perioden ausgleichen werden.

Am Ende des Experiments werden alle Periodenauszahlungen und die Anfangsaustattung aufsummiert und in bar ausgezahlt.

Feedback für Teilnehmer B und Teilnehmer C
Am Ende jeder Periode werden alle Teilnehmer B und alle Teilnehmer C darüber informiert, wie viel sie in dieser Periode verdient haben. Teilnehmer C werden außerdem immer darüber informiert, welche Leistung sie benötigt haben und welche Leistung sie bekommen haben. Teilnehmer B werden nicht darüber informiert, welche Leistung sie benötigt haben und welche Leistung sie bekommen haben. Allerdings werden Teilnehmer B in manchen Fällen erkennen können, welche Leistung sie benötigt und bekommen haben:

1. Eine negative Interaktionsauszahlung sagt Teilnehmer B, dass er die große Leistung benötigt und die kleine Leistung bekommen hat.

2. Aus einer Interaktionsauszahlung von 2 ECU oder 6 ECU kann Teilnehmer B nicht schließen, welche Leistung er benötigt und welche er bekommen hat. Teilnehmer B hat entweder die kleine Leistung benötigt und bekommen, oder die große Leistung benötigt und bekommen, oder die kleine Leistung benötigt und die große Leistung bekommen.

Entscheidungen von Teilnehmern B und Teilnehmern C
Nachdem Teilnehmer B seine Interaktionszahlung erfahren hat und nachdem Teilnehmer C seine Interaktionszahlung, seine benötigte Leistung und seine erhaltene Leistung erfahren hat, können Teilnehmer B und Teilnehmer C die Interaktion mit dem Teilnehmer A bewerten. Dabei wählen Teilnehmer B und Teilnehmer C zwischen „zufrieden“, „unzufrieden“ und „keine Bewertung abgeben“.

Feedback für Teilnehmer A
Am Ende jeder Periode wird jeder Teilnehmer A darüber informiert, für welche seiner Entscheidungen er welche Bewertung in der aktuellen Periode bekommen hat und wie viel er verdient hat.

Perioden 2 bis 16
Zu Beginn einer jeden Periode ab Periode 2 werden allen Teilnehmern A, allen Teilnehmern B und allen Teilnehmer C alle bisherigen Bewertungen der drei Teilnehmer A angezeigt. D.h., Teilnehmer A, Teilnehmer B und Teilnehmer C sehen für jeden Teilnehmer A:

1. die Anzahl aller bisher gesammelten Bewertungen mit „zufrieden“
2. die Anzahl aller bisher gesammelten Bewertungen mit „unzufrieden“
3. die Anzahl der fehlenden Bewertungen, also die Anzahl „keine Bewertung“
4. die Anzahl aller bisherigen Interaktionen

Diese Informationen sind in drei Tabellen zusammengefasst. Die erste Tabelle enthällt alle bisherigen Bewertungen, die Teilnehmer B und Teilnehmer C gegeben haben. Die zweite Tabelle enthält nur die Bewertungen, die Teilnehmer B gegeben haben. Die dritte Tabelle enthält nur die Bewertungen, die Teilnehmer C gegeben haben.

Außerdem sehen jeder Teilnehmer B und jeder Teilnehmer C eine Tabelle mit ihren eigenen bisherigen Interaktionen. Die Tabelle erinnert Teilnehmer B bzw. Teilnehmer C daran, mit welchem Teilnehmer A er in welcher Periode interagiert hat, welchen Preis er bezahlt hat, welche Interaktionsauszahlung er erhalten hat und welche Bewertung er gegeben hat. Teilnehmer C wird zusätzlich daran erinnert, welche Leistung er in der jeweiligen Periode benötigt und welche er bekommen hat.

Anhand dieser Informationen wählen Teilnehmer B und Teilnehmer C den Teilnehmer A, mit dem sie interagieren möchten. Teilnehmer B bzw. Teilnehmer C kann aber auch entscheiden, in dieser Periode nicht zu interagieren. Somit endet die Periode für ihn.

Ablauf einer Periode ab Periode 2 im Überblick

1. Alle Teilnehmer sehen die Bewertungen von Teilnehmern A. Zusätzlich werden Teilnehmer B und Teilnehmer C daran erinnert, mit welchen Teilnehmern A sie persönlich bisher interagiert haben.

2. Teilnehmer B und Teilnehmer C entscheiden, ob sie interagieren möchten und falls ja, mit welchem Teilnehmer A. (Wenn Teilnehmer B bzw. Teilnehmer C nicht interagieren möchte, dann endet die Periode für ihn. Wenn ein Teilnehmer A von keinem Teilnehmer B und von keinem Teilnehmer C ausgewählt wurde, dann endet die Periode für diesen Teilnehmer A.)

3. Jeder Teilnehmer A wird über die benötigte Leistung von den Teilnehmern B und den Teilnehmern C informiert, die mit ihm interagieren möchten.

4. Teilnehmer A wählen eine Preis-Leistungs-Kombination für jeden einzelnen Teilnehmer B und Teilnehmer C, mit dem sie interagieren

5. Teilnehmer B und Teilnehmer C bekommen Feedback.

6. Teilnehmer B und Teilnehmer C können die Interaktion mit Teilnehmer A bewerten, sofern sie mit einem Teilnehmer A interagiert haben.

7. Teilnehmer A sehen für jeden Teilnehmer B und für jeden Teilnehmer C, mit dem sie interagiert haben, welche Leistung er benötigt hat, welche Preis-Leistungs-Kombination für ihn gewählt wurde und welche Bewertung er gegeben hat. Darüber hinaus erfahren Teilnehmer A, wie viel sie in dieser Periode verdient haben.

Schritt 1 und 2 entfallen in Periode 1, da dort jedem Teilnehmer A ein Teilnehmer B und ein Teilnehmer C per Zufall zugewiesen werden.

Feedback am Ende des Experiments
In the end of the experiment each participant is informed about how much they have earned in the experiment.

Der Wechselkurs ist $1\ ECU = 0.14\ EUR$**.**

Kontrollfragen und weiteres Vorgehen
Zunächst werden Sie gebeten, einige Kontrollfragen zu beantworten, um sicherzustellen, dass Sie die Instruktionen und das Experiment verstanden haben. Nachdem all Ihre eventuellen Fragen von den Experimentatoren beantwortet wurden, wird das Experiment beginnen. Die Instruktionen für Teil 2 werden ausgeteilt, nachdem Teil 1 abgeschlossen ist.

[All experimental conditions]

Teil 2:

Jedem Teilnehmer wird per Zufall eine von drei Rollen zugewiesen: Teilnehmer X, Teilnehmer Y oder Teilnehmer Z. Jedem Teilnehmer X werden ein Teilnehmer Y und ein Teilnehmer Z zugewiesen. Ein Teilnehmer X, ein Teilnehmer Y und ein Teilnehmer Z bilden zusammen eine Gruppe.

Teilnehmer X trifft eine einzige Entscheidung über die Aufteilung von 11 ECU für seine Gruppe. Die Teilnehmer Y und Z müssen die Entscheidung von Teilnehmer X akzeptieren.

Es gibt folgende Aufteilungen:

Aufteilung	X bekommt	Y bekommt	Z bekommt
1	1 ECU	10 ECU	10 ECU
2	2 ECU	9 ECU	9 ECU
3	3 ECU	8 ECU	8 ECU
4	4 ECU	7 ECU	7 ECU
5	5 ECU	6 ECU	6 ECU
6	6 ECU	5 ECU	5 ECU
7	7 ECU	4 ECU	4 ECU
8	8 ECU	3 ECU	3 ECU
9	9 ECU	2 ECU	2 ECU
10	10 ECU	1 ECU	1 ECU

Teilnehmer X trifft also die Entscheidung, $k\ ECU)$ für sich zu beanspruchen, wodurch Teilnehmer Y und Teilnehmer Z jeweils $((11 - k)\ ECU$ erhalten. Zuerst wird niemand über seine Rolle informiert. Alle müssen eine Entscheidung treffen für den Fall, dass Ihnen die Rolle X zugeordnet wird. Nachdem alle eine Entscheidung getroffen haben, werden alle darüber informiert, welche Rolle ihnen zugeordnet wurde und welche Auszahlung die Teilnehmer X, Y und Z jeweils verdient haben. Danach endet Teil 2.

Der Wechselkurs ist $1\ ECU = 0.14\ EUR$**.**

Kontrollfragen

Finden Sie die falsche Aussage:

Frage 1:

a. Das Experiment besteht aus 16 Perioden.

b. Es gibt zwei Rollen: Teilnehmer A und Teilnehmer B. [ODER: Es gibt drei Rollen: Teilnehmer A, Teilnehmer B und Teilnehmer C.] (in U-I-Pooled und U-I-Separate)

c. In jeder Periode interagiert Teilnehmer A mit einer Anzahl von Teilnehmern B, die zwischen null und sechs variieren kann. [ODER: In jeder Periode interagiert Teilnehmer A mit einer Anzahl von Teilnehmern B, die zwischen null und drei variieren kann und mit einer Anzahl von Teilnehmern C, die zwischen null und drei variieren kann.] (in U-I-Pooled und U-I-Separate)

d. Teilnehmer B interagiert mit maximal einem Teilnehmer A. [ODER: Teilnehmer B interagiert mit maximal einem Teilnehmer A. Teilnehmer C interagiert mit maximal einem Teilnehmer A.] (in U-I-Pooled und U-I-Separate)

e. In Ihrer Gruppe sind insgesamt 10 Teilnehmer.

Frage 2:

a. Die benötigte Leistung wird in jeder Periode neu, unabhängig und per Zufall für jeden Teilnehmer B bestimmt. [ODER: Die benötigte Leistung wird in jeder Periode neu, unabhängig und per Zufall für jeden Teilnehmer B und für jeden Teilnehmer C bestimmt.] (in U-I-Pooled und U-I-Separate)

b. Teilnehmer A wird über die benötigte Leistung aller Teilnehmer informiert, die mit ihm interagieren möchten.

c. Teilnehmer A muss für alle Teilnehmer, die mit ihm interagieren möchten, dieselbe Preis-Leistungs-Kombination wählen.

d. Teilnehmer A sind immer identifizierbar.

Frage 3:

a. Wenn die kleine Leistung benötigt wird, muss Teilnehmer A immer die kleine Leistung wählen. Wenn die große Leistung benötigt wird, muss Teilnehmer A immer die große Leistung wählen.

b. Teilnehmer B sind nie identifizierbar.

c. Teilnehmer C werden am Ende der Periode darüber informiert, ob sie die große oder die kleine Leistung benötigt haben. [ODER Teilnehmer B werden nie darüber informiert, welche Leistung sie benötigt haben.] (in U-Ratings und U-No Ratings)

d. Aus einer negativen Interaktionsauszahlung können Teilnehmer B immer schließen, dass sie die große Leistung benötigt und die kleine Leistung erhalten haben.

e. Aus einer Interaktionsauszahlung von 2 ECU können Teilnehmer B nicht schließen, welche Leistung sie benötigt und welche sie erhalten haben.

Frage 4:

Angenommen Teilnehmer B benötigt die kleine Leistung und muss einen Preis von 8 ECU zahlen. Wie hoch ist seine Interaktionsauszahlung? (10-8=2 ECU)

Frage 5:

Angenommen Teilnehmer B benötigt die große Leistung, bekommt aber die kleine Leistung und muss einen Preis von 8 ECU zahlen. Wie hoch ist seine Periodenauszahlung? (8-8=0 ECU)

Frage 6:

Angenommen Teilnehmer A interagiert mit zwei Teilnehmern B. Teilnehmer A wählt für den ersten den Preis 4 und die kleine Leistung und für den zweiten den Preis 8 und die große Leistung. Wie hoch ist die Periodenauszahlung von Teilnehmer A? (2+2=4 ECU)

Frage 7:

a. Teilnehmer B können den Teilnehmer A bewerten, mit dem sie interagiert haben. [ODER: Teilnehmer B und Teilnehmer C können den Teilnehmer A bewerten, mit dem sie interagiert haben.](in U-I-Pooled und U-I-Separate)

b. Teilnehmer A sieht, wie er bisher bewertet wurde und wie die anderen Teilnehmer A bisher bewertet wurden.

c. Man kann immer unterscheiden, ob die Bewertungen von den Teilnehmern B oder von den Teilnehmern C kommen. (in U-I-Pooled) [ODER Man kann nie unterscheiden, ob die Bewertungen von den Teilnehmern B oder von den Teilnehmern C kommen.] (in U-I-Separate) [ODER Die Tabelle mit den Bewertungen enthält für jeden Teilnehmer A die Anzahl aller bisher gesammelten Bewertungen mit „zufrieden“ und die Anzahl aller bisher gesammelten Bewertungen mit „unzufrieden“.] (in U-Ratings)

d. Wenn Teilnehmer C eine Bewertung gibt, kennt er die Leistung, die er benötigt hat, die Leistung, die er erhalten hat und den Preis, den er bezahlt hat. [ODER Entferne Option d] (in U-Ratings) [ODER Entferne Frage 7] (in U-No Ratings)

A.2 Original Instructions - Chapter 3

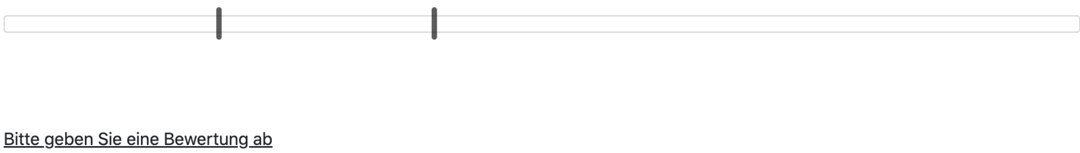
[Text in brackets was not observed by subjects. Presented sliders were interactive in the digital instructions. For examples of the interactions see translated instructions.]

Vielen Dank für Ihre Teilnahme am heutigen Experiment. Während des Experimentes ist es Ihnen nicht erlaubt, mit anderen teilnehmenden Personen zu kommunizieren. Bitte benutzen Sie nur die für das Experiment vorgesehenen Programme und Funktionen und benutzen Sie während des Experimentes keine weiteren Anwendungen auf Ihrem Computer. Außerdem können Sie mit den Aktionen, die Sie während des Experiments durchführen, Geld verdienen. Der genaue Betrag, den Sie erhalten, wird während des Experimentes festgelegt und hängt von Ihren Entscheidungen und den Entscheidungen anderer ab. Wenn Sie während des Experiments Fragen haben, melden Sie sich bitte über die Chatfunktion bei Zoom und warten Sie, bis die/der Experimentator:in sich bei Ihnen meldet.

In diesem Experiment werden Sie einfache Entscheidungen am Computer treffen. Alle Entscheidungen bleiben anonym. Das heißt, Sie erfahren die Identität der anderen Teilnehmer nicht und kein Teilnehmer erfährt Ihre Identität. Sämtliche Geldangaben innerhalb des Experiments werden in ECU (Experimental Currency Unit) angegeben.

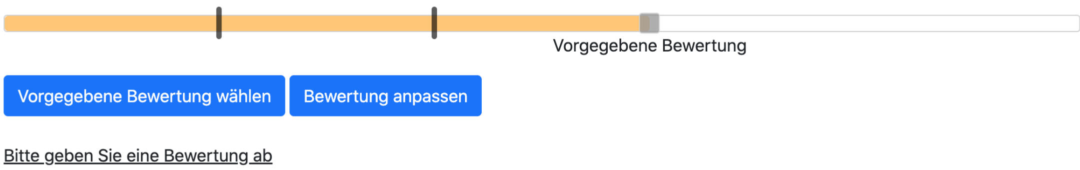
[No Anchor condition]
Ihre Aufgabe:
In diesem Experiment wird Ihnen jede Runde ein Qualitätsintervall angezeigt, welches auf einem Balken liegt. Die wahre Qualität liegt in diesem Intervall, wobei jeder Wert im Intervall gleichwahrscheinlich ist. Die Qualität steigt auf dem Balken von links nach rechts an.

Ihre Aufgabe besteht darin, diese Qualität, die Sie erhalten, zu bewerten. Die Bewertung nehmen Sie mit Hilfe eines Reglers auf dem Balken vor. Durch einen Mausklick auf den Balken initialisieren Sie ihre Bewertung. Sie können anschließend den Regler weiter bewegen. Im Folgenden finden Sie ein Beispiel zur Bedienung des Reglers.



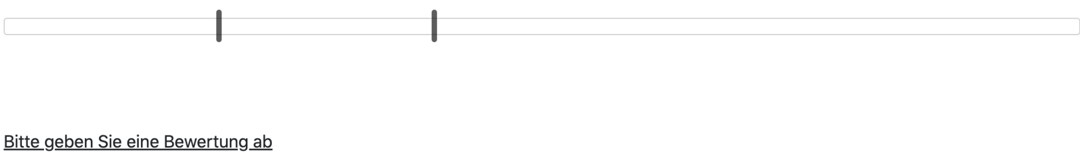
[High Anchor and Low Anchor conditions]
Ihre Aufgabe:
In diesem Experiment wird Ihnen jede Runde ein Qualitätsintervall angezeigt, welches auf einem Balken liegt. Die wahre Qualität liegt in diesem Intervall, wobei jeder Wert im Intervall gleichwahrscheinlich ist. Die Qualität steigt auf dem Balken von links nach rechts an.

Ihre Aufgabe besteht darin, diese Qualität, die Sie erhalten, zu bewerten. Die Bewertung nehmen Sie mit Hilfe eines Reglers auf dem Balken vor. Zunächst können Sie wählen, ob sie eine vorgegebene Bewertung wählen oder die Bewertung anpassen möchten. Sofern Sie in einer Runde die vorgegebene Bewertung wählen, können Sie diese nicht widerrufen. Sofern Sie die Bewertung anpassen möchten, müssen Sie durch einen Mausklick auf den Balken Ihre Bewertung initialisieren. Sie können anschließend den Regler weiter bewegen. Im Folgenden finden Sie ein Beispiel zur Bedienung des Reglers.

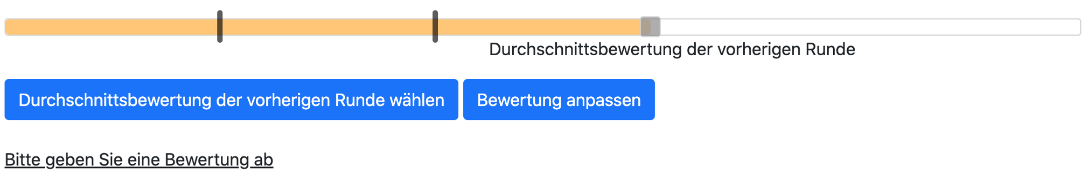


[Social Anchor condition]
Ihre Aufgabe:
In diesem Experiment werden sie einer Gruppe zugeordnet, welche aus Ihnen und vier weiteren Teilnehmern besteht. Diese Gruppe bleibt während der zwölf Runden des Experiments gleich und ändert sich nicht. Ihnen und allen anderen Gruppenmitgliedern wird jede Runde ein Qualitätsintervall auf einem Balken angezeigt. Die wahre Qualität liegt in diesem Intervall, wobei jeder Wert im Intervall gleichwahrscheinlich ist. Die Qualität steigt auf dem Balken von links nach rechts an. Das Qualitätsintervall und die wahre Qualität sind für alle Gruppenmitglieder identisch.

Ihre Aufgabe besteht darin, diese Qualität, die Sie erhalten, zu bewerten. Die Bewertung nehmen Sie mit Hilfe eines Reglers auf dem Balken vor. Durch einen Mausklick auf den Balken initialisieren Sie Ihre Bewertung. Sie können anschließend den Regler weiter bewegen. Im Folgenden finden Sie ein Beispiel zur Bedienung des Reglers in der ersten Runde.

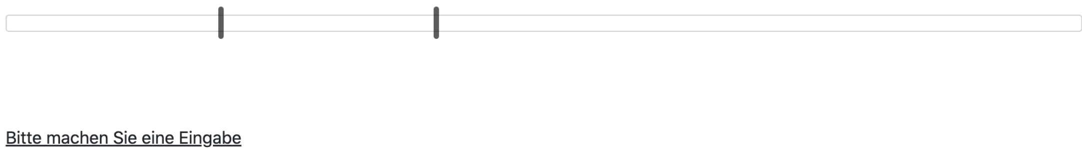


Ab der zweiten Runde wird Ihnen neben dem Qualitätsintervall die Durchschnittsbewertung Ihrer Gruppe (inklusive Ihrer Bewertung) aus der vorherigen Runde angezeigt. Sie können zunächst wählen, ob sie die Durchschnittsbewertung der vorherigen Runde wählen oder die Bewertung anpassen möchten. Sofern Sie in einer Runde die Durchschnittsbewertung der vorherigen Runde wählen, können Sie diese nicht widerrufen. Sofern Sie die Bewertung anpassen möchten, müssen Sie durch einen Mausklick auf den Balken Ihre Bewertung initialisieren. Sie können anschließend den Regler weiter bewegen. Im Folgenden finden Sie ein Beispiel zur Bedienung des Reglers ab der zweiten Runde.



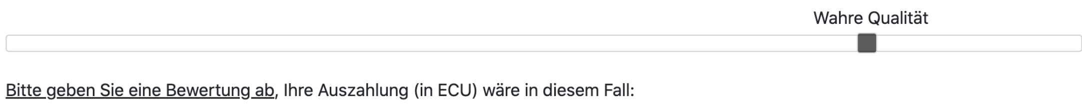
[Slider Task condition]
Ihre Aufgabe: In diesem Experiment wird Ihnen jede Runde ein Intervall angezeigt, welches auf einem Balken liegt. Genau in der Mitte innerhalb des Intervalls ist der Zielwert.

Ihre Aufgabe besteht darin, diesen Zielwert mit einer Eingabe auf dem Balken so genau wie möglich zu treffen. Die Eingabe nehmen Sie mit Hilfe eines Reglers auf dem Balken vor. Durch einen Mausklick auf den Balken initialisieren Sie ihre Eingabe. Sie können anschließend den Regler weiter bewegen. Im Folgenden finden Sie ein Beispiel zur Bedienung des Reglers.



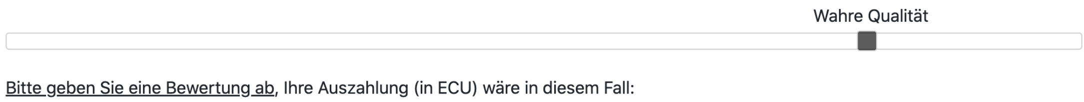
[End experimental conditions]

[No Anchor, High Anchor, Low Anchor and Social Anchor conditions]
Ihre Auszahlung:
Insgesamt gibt es zwölf Runden. In jeder Runde ist das Qualitätsintervall und damit die wahre Qualität unabhängig von der vorherigen Runde. Nach Abschluss der zwölf Runden wird zufällig eine Runde gewählt, welche Ihre Auszahlung bestimmt. Der Geldbetrag dieser Runde hängt davon ab wie nahe Sie an die wahre Qualität kommen, d.h. je näher Ihre Bewertung an der wahren Qualität ist, desto höher ist Ihr Verdienst in der Runde. Ihr Verdienst setzt sich wie folgt zusammen: eintausend ECU abzüglich der Abweichung von der wahren Qualität. Abhängig von der wahren Qualität und Ihrer Bewertung, liegt Ihre Auszahlung zwischen null ECU und eintausend ECU. Im Folgenden wird Ihnen Ihre Auszahlung in Abhängigkeit ihrer Bewertung und der wahren Qualität an einem Beispiel verdeutlicht.



[Slider Task condition]
Ihre Auszahlung:
Insgesamt gibt es zwölf Runden. In jeder Runde sehen Sie ein neues Intervall. Nach Abschluss der zwölf Runden wird zufällig eine Runde gewählt, welche Ihre Auszahlung bestimmt. Der Geldbetrag dieser Runde hängt davon ab wie nahe Sie mit Ihrer Eingabe an den Zielwert kommen, d.h. je näher Ihre Eingabe am Zielwert ist, desto höher ist Ihr Verdienst in der Runde. Ihr

Verdienst setzt sich wie folgt zusammen: eintausend ECU abzüglich der Abweichung vom Zielwert. Abhängig vom Zielwert und Ihrer Eingabe, liegt Ihre Auszahlung zwischen null ECU und eintausend ECU. Im Folgenden wird Ihnen Ihre Auszahlung in Abhängigkeit ihrer Eingabe und dem Zielwert an einem Beispiel verdeutlicht.



[End experimental conditions]

Die während des Experiments gesammelten ECU werden werden im Anschluss an das Experiment in Euro ausgezahlt. Dabei entsprechen zweihundert ECU = einem Euro. Für die Teilnahme am heutigen Experiment erhalten Sie zusätzlich eine Teilnahmevergütung von drei Euro. Ihr Verdienst in diesem Experiment wird Ihnen spätestens am Folgetag des Experiments über PayPal ausgezahlt.

[No Anchor, High Anchor, Low Anchor and Social Anchor conditions]
Kontrollfragen:
1. Welche der folgenden Aussagen trifft zu?

a.) Die wahre Qualität liegt immer im Qualitätsintervall zwischen den beiden schwarzen Balken.

b.) Die wahre Qualität liegt nicht immer im Qualitätsintervall zwischen den beiden schwarzen Balken.

2. Welche der folgenden Aussagen trifft zu?

a.) Je weiter entfernt die Bewertung von der wahren Qualität ist, desto größer ist mein Verdienst.

b.) Je näher die Bewertung an der wahren Qualität ist, desto größer ist mein Verdienst.

3. Welche der folgenden Aussagen trifft zu?

a.) Die Qualitätsintervalle und die wahre Qualität sind unabhängig voneinander zwischen den Runden.

b.) Die Qualitätsintervalle und die wahre Qualität sind abhängig voneinander zwischen den Runden.

[Slider Task condition]
Review Questions:
1. Welche der folgenden Aussagen trifft zu?

a.) Der Zielwert liegt immer im Intervall zwischen den beiden schwarzen Balken.

b.) Der Zielwert liegt nicht immer im Intervall zwischen den beiden schwarzen Balken.

2. Welche der folgenden Aussagen trifft zu?

a.) Je weiter entfernt die Eingabe von dem Zielwert ist, desto größer ist mein Verdienst.

b.) Je näher die Eingabe am Zielwert ist, desto größer ist mein Verdienst.

3. Welche der folgenden Aussagen trifft zu?

a.) Der Zielwert liegt immer genau in der Mitte des Intervalls.

b.) Der Zielwert nimmt einen zufälligen Wert innerhalb des Intervalls an.

[End experimental conditions]

A.3 Original Instructions - Chapter 4

Instruktionen - Bid-Cap Rule

Herzlich Willkommen und vielen Dank für Ihre Teilnahme! Das Experiment beginnt nun. Bitte lesen Sie die Instruktionen aufmerksam durch. Diese Instruktionen sind für alle Teilnehmer:innen identisch. Melden Sie sich wenn Sie eine Frage haben. Jemand wird dann zu Ihnen kommen und Ihre Frage beantworten. Sollte Ihre Frage für alle Teilnehmer:innen relevant sein, werden wir sie laut wiederholen und für alle beantworten.

Bitte beachten Sie, dass es während des gesamten Experiments nicht gestattet ist, mit anderen Teilnehmer:innen zu sprechen. Bitte schalten Sie jetzt auch Ihre Mobiltelefone aus. Dies ist ein Experiment über Entscheidungsverhalten. Abhängig von Ihren Entscheidungen und den Entscheidungen anderer Teilnehmer:innen verdienen Sie Geld, welches Sie im Anschluss an das Experiment ausgezahlt bekommen.

Zu Beginn werden Sie zufällig mit **neun** anderen Teilnehmer:innen einer Gruppe zugeteilt. Mit Ihren Gruppenmitgliedern werden Sie das **gesamte** Experiment interagieren. Sie erfahren **nicht**, wer die anderen Teilnehmer:innen Ihrer Gruppe sind.

Dieses Experiment besteht aus **zwei** Teilen. Die Instruktionen für den **ersten** Teil finden Sie nachfolgend. Die Instruktionen für den **zweiten** Teil erhalten Sie, sobald der zweite Teil des Experiments beginnt.

Alle Geldangaben innerhalb des Experiments werden in Experimental Currency Units (*ECU*) angegeben. Ihre Bezahlung ist die Summe Ihrer Verdienste aus Teil 1 und Teil 2 und wird im Anschluss an das Experiment umgerechnet zum Kurs 10 *ECU* = 0,40 Euro. Zusätzlich erhalten Sie unabhängig von der Bezahlung aus Teil 1 und Teil 2 eine Teilnahmevergütung von 5 Euro.

Teil 1

Ihre Aufgabe im ersten Teil:

Sie und jedes Ihrer Gruppenmitglieder erhalten ein **Startkapital** in Höhe von 65 *ECU*. Von diesem Startkapital können Sie und jedes Gruppenmitglied einen beliebigen Teil in ein Projekt investieren, welches nur realisiert wird, wenn insgesamt die **Investitionskosten** von 300 *ECU* erreicht werden. Ihr Verdienst in diesem Teil hängt davon ab, ob Sie Investor des Projekts sind und ob Ihre Gruppe das Projekt realisiert oder nicht.

Um als **Investor** des Projekts zu gelten, müssen Sie **mindestens** eine **Einstiegsinvestition** von 15 *ECU* tätigen, dies gilt ebenso für alle anderen Gruppenmitglieder. Investitionen unterhalb dieses Betrags werden als **Unterstützung** aufgenommen, berechtigen aber nicht dazu, eine Auszahlung aus dem Projekt zu fordern. Gruppenmitglieder, die weniger als 15 *ECU* investiert haben, erhalten keine Auszahlung aus dem Projekt und erhalten somit bei Erreichen der Investitionskosten nur den verbleibenden Teil des Startkapitals, welchen Sie nicht investiert haben.

Sollte Ihre Gruppe zusammen weniger als die Investitionskosten von 300 *ECU* investieren, erhält jedes Gruppenmitglied seine Investition zurück. Sollte Ihre Gruppe zusammen mindestens die Investitionskosten von 300 *ECU* investieren, erhält jeder **Investor** eine **Auszahlung** in Höhe von 45 *ECU*, sowie den verbleibenden Teil des Startkapitals, welchen dieser nicht investiert hat. Sollte Ihre Gruppe insgesamt mehr als die geforderten Investitionskosten investieren, erhalten die Investoren potenziell noch einen Anteil der überschüssigen Investitionen zurück. Die **Rückzahlung** der überschüssigen Investitionen erfolgt dabei nach der folgenden Regel:

Als Erstes werden die Unterstützungen der Nicht-Investoren von den Investitionskosten abgezogen. Es wird nun überprüft, ob die Investitionskosten abzüglich der Unterstützungen erreicht werden, wenn alle Investoren die niedrigste Investition zahlen würden. Ist dies der Fall, zahlen alle Investoren die niedrigste Investition und der überschüssige Betrag wird gleichmäßig an alle Investoren zurückgezahlt.

Ist dies nicht der Fall, so zahlen die Investoren, die die niedrigste Investition getätigt haben die niedrigste Investition und es wird überprüft, ob die Investitionskosten abzüglich der Unterstützungen erreicht werden, wenn alle anderen Investoren die zweitniedrigste Investition zahlen würden. Ist dies der Fall zahlen diejenigen Investoren, die die niedrigste Investition getätigt haben, die niedrigste Investition und alle anderen Investoren die zweitniedrigste Investition. Der überschüssige Betrag wird dann gleichmäßig an alle Investoren zurückgezahlt, die die zweitniedrigste Investition getätigt haben.

Dies wird so lange wiederholt, bis die Investitionskosten abzüglich der Unterstüzungen erreicht werden. Die Investoren zahlen also maximal ihre Investition und potenziell weniger als diese wenn ihre volle Investition nicht benötigt wird um die Investitionskosten zu decken. Den tatsächlich zur Projektfinanzierung eingesetzten Teil der Investition bezeichnen wir als **geleistete Zahlung**. Das nachfolgende Beispiel verdeutlicht diese Regel noch einmal.

Beispiel

Spieler	A	B	C	D	E	F	G	H	I	J
Investition	0	7	14	21	28	35	42	49	56	63

Basierend auf diesen Investitionen sind D, E, F, G, H, I und J Investoren, da ihre Investition jeweils größer als die Einstiegsinvestition von 15 *ECU* ist. A, B und C sind keine Investoren – folglich werden deren Investitionen lediglich als Unterstützungen aufgenommen. Abzüglich der Unterstützungen von $0 + 7 + 14 = 21$ *ECU* werden also noch 279 *ECU* benötigt, um das Projekt zu realisieren.

Es wird nun geprüft, ob dieser Betrag von 279 *ECU* erreicht wird, wenn alle Investoren die niedrigste Investition von 21 *ECU* zahlen würden. Jedoch sind $21 \cdot 7 = 147 < 279$. Somit zahlt D 21 *ECU*, und es wird geprüft, ob 279 *ECU* erreicht werden, wenn D 21 *ECU* und die übrigen Investoren E ,F ,G ,H, I und J jeweils 28 *ECU* zahlen. Jedoch ist $21 + 6 \cdot 28 = 189 < 279$ und somit zahlt D 21 *ECU* und E 28 *ECU*. Es wird nun überprüft ob 279 *ECU* erreicht werden, wenn D 21 *ECU*, E 28 *ECU* und die restlichen Investoren jeweils 35 *ECU* zahlen. Diese Investitionen ergeben dann $21 + 28 + 5 \cdot 35 = 224 < 279$.

Dies geht solange weiter bis Investor **I** erreicht wird. Alle Investoren bis zu I zahlen ihre Investition und I und J zahlen jeweils 56 *ECU*. Das ergibt insgesamt Investitionen von $21 + 28 + 35 + 42 + 49 + 2 \cdot 56 = 287$ ECU was die Investitionskosten abzüglich der Unterstützungen deckt. Deshalb zahlt J nicht die investierten 63 *ECU* sondern 56 *ECU*.

Zusätzlich führen diese getätigten Investitionen zu überschüssigen Investitionen von 287-279=8 *ECU*. Diese 8 *ECU* werden nun gleichmäßig auf die Investoren I und J verteilt, sodass I und J jeweils eine Rückzahlung von 4 *ECU* erhalten.

Am Ende zahlen also bis auf I und J alle Investoren ihre volle Investition. Die vollen Investitionen von I und J werden nicht benötigt um die Investitionskosten zu decken. J zahlt anstatt 63 *ECU* nur die Investition von I in Höhe von 56 *ECU*. Zusätzlich erhalten I und J als am meisten zahlende Investoren eine Rückzahlung von 4 *ECU*. Die geleistete Zahlung von J wäre in diesem Fall also J's Investition von 63 *ECU* minus 7 *ECU*, da J's Investition auf die Investition von I reduziert wird, minus 4 *ECU*, da I und J eine Rückzahlung aufgrund der überschüssigen Investitionen erhalten.

In der nachfolgenden Tabelle sehen Sie die Investitionen und die geleisteten Zahlungen aller Investoren.

Spieler	A	B	C	D	E	F	G	H	I	J
Investition	0	7	14	21	28	35	42	49	56	63
geleistete Investition	0	7	14	21	28	35	42	49	52	52

Zusammenfassung Ihrer möglichen Verdienste

- Sollten die Investitionskosten nicht erreicht werden erhalten Sie:

  $Verdienst = Startkapital$

- Sollten die Investitionskosten erreicht werden und Sie haben weniger als die Einstiegsinvestition getätigt, wird Ihre Investition als Unterstützung aufgenommen und Sie erhalten:

$Verdienst = Startkapital - Unterstützung$

- Sollten die Investitionskosten erreicht werden und Sie haben mindestens die Einstiegsinvestition getätigt, bestimmt sich Ihre geleistete Investition nach oben stehender Regel und Sie erhalten:

  $Verdienst = Startkapital + Auszahlung - geleistete\ Zahlung$

Sollten Sie Fragen zu **Teil 1** haben, heben Sie nun die Hand und wir werden zu Ihnen kommen und Ihre Frage beantworten.

Teil 2

Es beginnt nun der zweite Teil des Experiments. In diesem Teil wiederholen Sie die Aufgabe aus **Teil 1** **zehn** weitere Male. Dabei erhalten Sie in jeder Runde das Startkapital von 65 *ECU*. Jedoch wird die mögliche **Auszahlung** als Investor (in *ECU*) für jedes Gruppenmitglied zu Beginn jeder Runde **zufällig** aus dem Intervall $[30, 60]$ gezogen. Dabei ist jede Zahl innerhalb des Intervalls gleich wahrscheinlich und jedes Gruppenmitglied erhält seine Zahl individuell und unabhängig von den Zahlen der anderen Gruppenmitglieder. Sie erfahren **nicht** welche Investitionen Ihre Gruppenmitglieder in den vorhergegangenen Runden getätigt haben und ob die Investionskosten erreicht wurden.

In diesem Teil sind also Ihre Gruppenmitglieder, die Investitionskosten, Ihr Startkapital, die Einstiegsinvestition, um als Investor zu gelten, sowie die Regel bezüglich der Rückzahlung von überschüssigen Investitionen gleich. Zu Beginn jeder Runde erfahren Sie Ihre zufällig gezogene Auszahlung als Investor.

Ihr möglicher Verdienst in jeder Runde wird genauso wie im ersten Teil bestimmt. Sollte das Projekt nicht realisiert werden, erhalten Sie Ihre Investition zurück und Ihr Verdienst entspricht Ihrem Startkapital. Wenn das Projekt realisiert wird, Sie aber weniger als 15 *ECU* investiert haben, wird Ihre Investition als Unterstützung aufgenommen und Ihr Verdienst entspricht Ihrem Startkapital abzüglich dieser Unterstützung. Sollten Sie mindestens 15 ECU investiert haben und das Projekt wird realisiert, entspricht Ihr Verdienst Ihrem Startkapital, zuzüglich der zufällig gezogenen Auszahlung und abzüglich Ihrer geleisteten Zahlung, die sich nach der selben Regel wie in Teil 1 bestimmt.

Ihre Bezahlung für den zweiten Teil ist dann Ihr Verdienst einer zufällig ausgewählten Runde, wobei jede Runde mit gleicher Wahrscheinlichkeit ausgewählt werden kann. Dieser Verdienst wird weiterhin zum Kurs 10 *ECU* = 0,40 Euro in Euro umgerechnet. Im Anschluss an die zehnte Runde erfahren Sie, welche Runde für Sie als auszahlungsrelevant gezogen wurde, sowie Ihre Verdienste aus beiden Teilen des Experiments.

Sollten Sie eine Frage zu **Teil 2** haben, heben Sie jetzt bitte Ihre Hand. Jemand wird dann zu Ihnen kommen und Ihre Frage beantworten. Sollte Ihre Frage für alle Teilnehmer:innen relevant sein, werden wir sie laut wiederholen und für alle beantworten.

Instruktionen - Proportional Rebate

Herzlich Willkommen und vielen Dank für Ihre Teilnahme! Das Experiment beginnt nun. Bitte lesen Sie die Instruktionen aufmerksam durch. Diese Instruktionen sind für alle Teilnehmer:innen identisch. Melden Sie sich wenn Sie eine Frage haben. Jemand wird dann zu Ihnen kommen und Ihre Frage beantworten. Sollte Ihre Frage für alle Teilnehmer:innen relevant sein, werden wir sie laut wiederholen und für alle beantworten.

Bitte beachten Sie, dass es während des gesamten Experiments nicht gestattet ist, mit anderen Teilnehmer:innen zu sprechen. Bitte schalten Sie jetzt auch Ihre Mobiltelefone aus. Dies ist ein Experiment über Entscheidungsverhalten. Abhängig von Ihren Entscheidungen und den Entscheidungen anderer Teilnehmer:innen verdienen Sie Geld, welches Sie im Anschluss an des Experiment ausgezahlt bekommen.

Zu Beginn des Experiments werden Sie zufällig mit **neun** anderen Teilnehmer:innen einer Gruppe zugeteilt. Mit Ihren Gruppenmitgliedern werden Sie das **gesamte** Experiment interagieren. Sie erfahren dabei **nicht**, wer die anderen Teilnehmer:innen Ihrer Gruppe sind.

Dieses Experiment besteht aus **zwei** Teilen. Die Instruktionen für den **ersten** Teil finden Sie nachfolgend. Die Instruktionen für den **zweiten** Teil erhalten Sie, sobald der zweite Teil des Experiments beginnt.

Alle Geldangaben innerhalb des Experiments werden in Experimental Currency Units (*ECU*) angegeben. Ihre Bezahlung ist die Summe Ihrer Verdienste aus Teil 1 und Teil 2 und wird im Anschluss an das Experiment umgerechnet zum Kurs 10 *ECU* = 0,40 Euro. Zusätzlich erhalten Sie unabhängig von der Bezahlung aus Teil 1 und Teil 2 eine Teilnahmevergütung von 5 Euro.

Teil 1

Ihre Aufgabe im ersten Teil:

Sie und jedes ihrer Gruppenmitglieder erhalten ein **Startkapital** in Höhe von 65 *ECU*. Von diesem Startkapital können Sie und jedes Gruppenmitglied einen beliebigen Teil in ein Projekt investieren, welches nur realisiert wird, wenn insgesamt die **Investitionskosten** von 300 *ECU* erreicht werden. Ihr Verdienst in diesem Teil hängt davon ab, ob Sie Investor des Projekts sind und ob Ihre Gruppe das Projekt realisiert oder nicht.

Um als **Investor** des Projekts zu gelten, müssen Sie **mindestens** eine **Einstiegsinvestition** von 15 *ECU* tätigen, dies gilt ebenso für alle anderen Gruppenmitglieder. Investitionen unterhalb dieses Betrags werden als **Unterstützung** aufgenommen, berechtigen aber nicht dazu, eine Auszahlung aus dem Projekt zu fordern. Gruppenmitglieder, die weniger als 15 *ECU* investiert haben, erhalten keine Auszahlung aus dem Projekt und erhalten somit bei Erreichen der Investitionskosten nur den verbleibenden Teil des Startkapitals, welchen Sie nicht investiert haben.

Sollte Ihre Gruppe zusammen weniger als die Investitionskosten von 300 *ECU* investieren, erhält jedes Gruppenmitglied seine Investition zurück. Sollte Ihre Gruppe zusammen mindestens die Investitionskosten von 300 *ECU* investieren, erhält jeder **Investor** eine **Auszahlung** in Höhe von 45 *ECU*, sowie den verbleibenden Teil des Startkapitals, welchen dieser nicht investiert hat. Sollte Ihre Gruppe insgesamt mehr als die geforderten Investitionskosten investieren, erhalten die Investoren potenziell noch einen Anteil der überschüssigen Investitionen zurück. Die **Rückzahlung** der überschüssigen Investitionen erfolgt dabei nach der folgenden Regel:

Als erstes wird für jeden Investor bestimmt wie viel mehr als die Einstiegsinvestition von 15 *ECU* getätigt wurde. Die Differenz aus Investition und Einstiegsinvestition wird im Folgenden **Beitrag** genannt. Der Anteil, den jeder Investor aus den überschüssigen Investitionen zurückerhält, ist direkt proportional zu seinem Anteil an der Summe aller Beiträge. Ist ein Investor beispielsweise für die Hälfte aller Beiträge verantwortlich, erhält dieser Investor die Hälfte der überschüssigen Investitionen als Rückzahlung. Den tatsächlich zur Projektrealisierung eingesetzten Teil der Investition - also das was ein Investor schlussendlich für die Projektrealisierung aufbringt - bezeichnen wir als **geleistete Investition**. Das nachfolgende Beispiel verdeutlicht diese Regel noch einmal.

Beispiel

Spieler	A	B	C	D	E	F	G	H	I	J
Investition	0	7	14	21	28	35	42	49	56	63

Basierend auf diesen Investitionen sind D, E, F, G, H, I und J Investoren, da ihre Investition jeweils größer als die Einstiegsinvestition von 15 *ECU* ist. A, B und C sind <u>keine</u> Investoren – folglich werden deren Investitionen lediglich als Unterstützungen aufgenommen. Abzüglich der Unterstützungen von $0 + 7 + 14 = 21$ *ECU* werden noch 279 *ECU* benötigt, um das Projekt zu realisieren. Dies wird mit den restlichen Investitionen erreicht, da $28 + 35 + 42 + 49 + 56 + 63 = 294$. Es werden also 294-279=15 *ECU* als überschüssige Investitionen beigetragen, die proportional zu den **Beiträgen** der **Investoren** zurückgezahlt werden. Nachfolgend finden Sie die Berechnung der Beiträge und Rückzahlungen aller Investoren:

D investiert 21 *ECU* und die Einstiegsinvestition ist 15 *ECU*. Deshalb ist D's Beitrag $21 - 15 = 6$ *ECU*. E's Beitrag ist 28-15=13 *ECU*, F's Beitrag ist 35-15=20 *ECU*, G's Beitrag ist 42-15=27 *ECU*, H's Beitrag ist 49-15=34 *ECU*, I's Beitrag ist 56-15=41 *ECU* und J's Beitrag ist 63-15=48 *ECU*. Die Summe aller Beiträge ist 6+13+20+27+34+41+48=189 *ECU*.

D's Beitrag ist 6 *ECU* und die Summe aller Beiträge ist 189 *ECU*. D's Anteil an den Beitragen ist also 6/189. Dieser Anteil an den Beiträgen wird mit den überschüssigen Investitionen in Höhe von 15 *ECU* multipliziert um die Rückzahlung zu bestimmen.

Folglich erhält D eine Rückzahlung von $\frac{6}{189} \cdot 15 = 0.48$ *ECU*. Äquivalent erhält E eine Rückzahlung von $\frac{13}{189} \cdot 15 = 1.13$ *ECU*, F eine Rückzahlung von $\frac{20}{189} \cdot 15 = 1.59$ *ECU*, G eine Rückzahlung von $\frac{27}{189} \cdot 15 = 3.14$ *ECU*, H eine Rückzahlung von $\frac{34}{189} \cdot 15 = 2.7$ *ECU*, I eine Rückzahlung von $\frac{41}{189} \cdot 15 = 3.25$ *ECU* und J eine Rückzahlung von $\frac{48}{189} \cdot 15 = 3.81$ *ECU*.

In der nachfolgenden Tabelle sehen Sie die Investitionen und die geleisteten Investitionen aller Investoren.

Spieler	A	B	C	D	E	F	G	H	I	J
Investition	0	7	14	21	28	35	42	49	56	63
geleistete Investition	0	7	14	20.52	26.97	33.41	39.86	46.30	52.75	59.19

Zusammenfassung Ihrer möglichen Verdienste

- Sollten die Investitionskosten <u>nicht</u> erreicht werden erhalten Sie:

  $Verdienst = Startkapital$

- Sollten die Investitionskosten erreicht werden und Sie haben <u>weniger</u> als die Einstiegsinvestition getätigt, wird Ihre Investition als Unterstützung aufgenommen und Sie erhalten:

  $Verdienst = Startkapital - Unterstützung$

- Sollten die Investitionskosten erreicht werden und Sie haben <u>mindestens</u> die Einstiegsinvestition getätigt, bestimmt sich Ihre geleistete Investition nach oben stehender Regel und Sie erhalten:

  $Verdienst = Startkapital + Auszahlung - geleistete\ Investition$

Sollten Sie Fragen zu **Teil 1** haben, heben Sie nun die Hand und wir werden zu Ihnen kommen und Ihre Frage beantworten.

Teil 2

Es beginnt nun der zweite Teil des Experiments. In diesem Teil wiederholen Sie die Aufgabe aus **Teil 1 zehn** weitere Male. Dabei erhalten Sie in jeder Runde das Startkapital von 65 *ECU*. Jedoch wird die mögliche **Auszahlung** als Investor (in *ECU*) für jedes Gruppenmitglied zu Beginn jeder Runde **zufällig** aus dem Intervall $[30, 60]$ gezogen. Dabei ist jede Zahl innerhalb des Intervalls gleich wahrscheinlich und jedes Gruppenmitglied erhält seine Zahl individuell und unabhängig von den Zahlen der anderen Gruppenmitglieder. Sie erfahren **nicht** welche Investitionen Ihre Gruppenmitglieder in den vorhergegangenen Runden getätigt haben und ob die Investionskosten erreicht wurden.

In diesem Teil sind also Ihre Gruppenmitglieder, die Investitionskosten, Ihr Startkapital, die Einstiegsinvestition, um als Investor zu gelten, sowie die Regel bezüglich der Rückzahlung von überschüssigen Investitionen gleich. Zu Beginn jeder Runde erfahren Sie Ihre zufällig gezogene Auszahlung als Investor.

Ihr möglicher Verdienst in jeder Runde wird genauso wie im ersten Teil bestimmt. Sollte das Projekt nicht realisiert werden, erhalten Sie Ihre Investition zurück und Ihr Verdienst entspricht Ihrem Startkapital. Wenn das Projekt realisiert wird, Sie aber weniger als 15 *ECU* investiert haben, wird Ihre Investition als Unterstützung aufgenommen und Ihr Verdienst entspricht Ihrem Startkapital abzüglich dieser Unterstützung. Sollten Sie mindestens 15 ECU investiert haben und das Projekt wird realisiert, entspricht Ihr Verdienst Ihrem Startkapital, zuzüglich der zufällig gezogenen Auszahlung und abzüglich Ihrer geleisteten Investition, die sich nach der selben Regel wie in Teil 1 bestimmt.

Ihre Bezahlung für den zweiten Teil ist dann Ihr Verdienst einer zufällig ausgewählten Runde, wobei jede Runde mit gleicher Wahrscheinlichkeit ausgewählt werden kann. Dieser Verdienst wird weiterhin zum Kurs 10 *ECU* = 0,40 Euro in Euro umgerechnet. Im Anschluss an die zehnte Runde erfahren Sie, welche Runde für Sie als auszahlungsrelevant gezogen wurde, sowie Ihre Verdienste aus beiden Teilen des Experiments.

Sollten Sie eine Frage zu **Teil 2** haben, heben Sie jetzt bitte Ihre Hand. Jemand wird dann zu Ihnen kommen und Ihre Frage beantworten. Sollte Ihre Frage für alle Teilnehmer:innen relevant sein, werden wir sie laut wiederholen und für alle beantworten.

Instruktionen - All-or-Nothing-Rule

Herzlich Willkommen und vielen Dank für Ihre Teilnahme! Das Experiment beginnt nun. Bitte lesen Sie die Instruktionen aufmerksam durch. Diese Instruktionen sind für alle Teilnehmer:innen identisch. Melden Sie sich wenn Sie eine Frage haben. Jemand wird dann zu Ihnen kommen und Ihre Frage beantworten. Sollte Ihre Frage für alle Teilnehmer:innen relevant sein, werden wir sie laut wiederholen und für alle beantworten.

Bitte beachten Sie, dass es während des gesamten Experiments nicht gestattet ist, mit anderen Teilnehmer:innen zu sprechen. Bitte schalten Sie jetzt auch Ihre Mobiltelefone aus. Dies ist ein Experiment über Entscheidungsverhalten. Abhängig von Ihren Entscheidungen und den Entscheidungen anderer Teilnehmer:innen verdienen Sie Geld, welches Sie im Anschluss an des Experiment ausgezahlt bekommen.

Zu Beginn des Experiments werden Sie zufällig mit **neun** anderen Teilnehmer:innen einer Gruppe zugeteilt. Mit Ihren Gruppenmitgliedern werden Sie das **gesamte** Experiment interagieren. Sie erfahren dabei **nicht**, wer die anderen Teilnehmer:innen Ihrer Gruppe sind.

Dieses Experiment besteht aus **zwei** Teilen. Die Instruktionen für den **ersten** Teil finden Sie nachfolgend. Die Instruktionen für den **zweiten** Teil erhalten Sie, sobald der zweite Teil des Experiments beginnt.

Alle Geldangaben innerhalb des Experiments werden in Experimental Currency Units (*ECU*) angegeben. Ihre Bezahlung ist die Summe Ihrer Verdienste aus Teil 1 und Teil 2 und wird im Anschluss an das Experiment umgerechnet zum Kurs 10 *ECU* = 0,40 Euro. Zusätzlich erhalten Sie unabhängig von der Bezahlung aus Teil 1 und Teil 2 eine Teilnahmevergütung von 5 Euro.

Teil 1

Ihre Aufgabe im ersten Teil:

Sie und jedes ihrer Gruppenmitglieder erhalten ein **Startkapital** in Höhe von 65 *ECU*. Von diesem Startkapital können Sie und jedes Gruppenmitglied einen beliebigen Teil in ein Projekt investieren, welches nur realisiert wird, wenn insgesamt die **Investitionskosten** von 300 *ECU* erreicht werden. Ihr Verdienst in diesem Teil hängt davon ab, ob Sie Investor des Projekts sind und ob Ihre Gruppe das Projekt realisiert oder nicht.

Um als **Investor** des Projekts zu gelten, müssen Sie **mindestens** eine **Einstiegsinvestition** von 15 *ECU* tätigen, dies gilt ebenso für alle anderen Gruppenmitglieder. Investitionen unterhalb dieses Betrags werden als **Unterstützung** aufgenommen, berechtigen aber nicht dazu, eine Auszahlung aus dem Projekt zu fordern. Gruppenmitglieder, die weniger als 15 *ECU* investiert haben, erhalten keine Auszahlung aus dem Projekt und erhalten somit bei Erreichen der Investitionskosten nur den verbleibenden Teil des Startkapitals, welchen Sie nicht investiert haben.

Sollte Ihre Gruppe zusammen weniger als die Investitionskosten von 300 *ECU* investieren, erhält jedes Gruppenmitglied seine Investition zurück. Sollte Ihre Gruppe zusammen mindestens die Investitionskosten von 300 *ECU* investieren, erhält jeder **Investor** eine **Auszahlung** in Höhe von 45 *ECU*, sowie den verbleibenden Teil des Startkapitals, welchen dieser nicht investiert hat. Sollte Ihre Gruppe insgesamt mehr als die geforderten Investitionskosten investieren, verbleiben die überschüssigen Investitionen im Projekt. Das nachfolgende Beispiel verdeutlicht diese Regel noch einmal.

Beispiel

Spieler	A	B	C	D	E	F	G	H	I	J
Investition	0	7	14	21	28	35	42	49	56	63

Basierend auf diesen Investitionen sind D, E, F, G, H, I und J Investoren, da ihre Investition jeweils größer als die Einstiegsinvestition von 15 *ECU* ist. A, B und C sind keine Investoren – folglich werden deren Investitionen lediglich als Unterstützungen aufgenommen. Abzüglich der Unterstützungen von $0 + 7 + 14 = 21$ *ECU* werden also noch 279 *ECU* benötigt, um das Projekt zu realisieren.
Dies wird mit den restlichen Investitionen erreicht, da $28 + 35 + 42 + 49 + 56 + 63 = 294$. Zusätzlich werden *15 ECU* als überschüssige Investitionen beigetragen, die in dem Projekt verbleiben.

Zusammenfassung Ihrer möglichen Verdienste

- Sollten die Investitionskosten nicht erreicht werden erhalten Sie:

  $Verdienst = Startkapital$

- Sollten die Investitionskosten erreicht werden und Sie haben weniger als die Einstiegsinvestition getätigt, wird Ihre Investition als Unterstützung aufgenommen und Sie erhalten:

  $Verdienst = Startkapital - Unterstützung$

- Sollten die Investitionskosten erreicht werden und Sie haben mindestens die Einstiegsinvestition getätigt erhalten Sie:

  $Verdienst = Startkapital + Auszahlung - Investition$

Sollten Sie Fragen zu **Teil 1** haben, heben Sie nun die Hand und wir werden zu Ihnen kommen und Ihre Frage beantworten.

Teil 2

Es beginnt nun der zweite Teil des Experiments. In diesem Teil wiederholen Sie die Aufgabe aus **Teil 1** **zehn** weitere Male. Dabei erhalten Sie in jeder Runde das Startkapital von 65 *ECU*. Jedoch wird die mögliche **Auszahlung** als Investor (in *ECU*) für jedes Gruppenmitglied zu Beginn jeder Runde **zufällig** aus dem Intervall $[30, 60]$ gezogen. Dabei ist jede Zahl innerhalb des Intervalls gleich wahrscheinlich und jedes Gruppenmitglied erhält seine Zahl individuell und unabhängig von den Zahlen der anderen Gruppenmitglieder. Sie erfahren **nicht** welche Investitionen Ihre Gruppenmitglieder in den vorhergegangenen Runden getätigt haben und ob die Investionskosten erreicht wurden.

In diesem Teil sind also Ihre Gruppenmitglieder, die Investitionskosten, Ihr Startkapital und die Einstiegsinvestition, um als Investor zu gelten gleich. Überschüssige Investitionen verbleiben weiterhin im Projekt. Zu Beginn jeder Runde erfahren Sie Ihre zufällig gezogene Auszahlung als Investor.

Ihr möglicher Verdienst in jeder Runde wird genauso wie im ersten Teil bestimmt. Sollte das Projekt nicht realisiert werden, erhalten Sie Ihre Investition zurück und Ihr Verdienst entspricht Ihrem Startkapital. Wenn das Projekt realisiert wird, Sie aber weniger als 15 *ECU* investiert haben, wird Ihre Investition als Unterstützung aufgenommen und Ihr Verdienst entspricht Ihrem Startkapital abzüglich dieser Unterstützung. Sollten Sie mindestens 15 ECU investiert haben und das Projekt wird realisiert, entspricht Ihr Verdienst Ihrem Startkapital, zuzüglich der zufällig gezogenen Auszahlung und abzüglich Ihrer Investition.

Ihre Bezahlung für den zweiten Teil ist dann Ihr Verdienst einer zufällig ausgewählten Runde, wobei jede Runde mit gleicher Wahrscheinlichkeit ausgewählt werden kann. Dieser Verdienst wird weiterhin zum Kurs 10 *ECU* = 0,40 Euro in Euro umgerechnet. Im Anschluss an die zehnte Runde erfahren Sie, welche Runde für Sie als auszahlungsrelevant gezogen wurde, sowie Ihre Verdienste aus beiden Teilen des Experiments.

Sollten Sie eine Frage zu **Teil 2** haben, heben Sie jetzt bitte Ihre Hand. Jemand wird dann zu Ihnen kommen und Ihre Frage beantworten. Sollte Ihre Frage für alle Teilnehmer:innen relevant sein, werden wir sie laut wiederholen und für alle beantworten.

University of Liverpool Management School
PhD Thesis – PhD Structured as Papers

AUTHORSHIP DECLARATION – JOINT AUTHORED PAPERS - APPENDIX B

1. CANDIDATE

Name of the Candidate	Student number
Yigit Oezcelik	201355893
Thesis Title	
Essays in (Experimental) Industrial Organisation: Online Ratings for Credence Goods, Anchoring Bias in Ratings and Rebate Rules for Crowdfunding	

2. FORMAT OF THE THESIS

Is the candidate intending to structure their thesis as papers?	**Yes**	If Yes, please complete Section 3 (sole authored paper) OR 4 (joint paper) **If No, you do not need to complete this form**

3. PAPER INCLUDED IN THE THESIS – JOINT AUTHORED PAPER

Title of the paper	Has this paper been published, presented at a conference or under review with a journal	If Yes, please complete the boxes below. If No, go to section 4
Online Ratings for Credence Goods: Experimental Evidence	No	
If the paper has already been published please refer to the University guidelines on presentation of publications within a PGR Thesis - *https://www.liverpool.ac.uk/media/livacuk/tqsd/code-of-practice-on-assessment/annex-7.2-PGR-CoP.pdf*		
If the paper is under review with a journal, give details of the journal, including submission dates and the review stage N/A		
If the paper is presented at a conference, give details of the conference N/A		

4. DESCRIPTION OF ALL AUTHOR CONTRIBUTIONS (including the PhD candidate)

Name and affiliation of author	Contribution(s) (for example, conception of the project, design of methodology, data collection, analysis, drafting the manuscript, revising it critically for important intellectual content, etc.)
Yigit Oezcelik, University of Liverpool, UK	Conception of the project, Literature Overview, Design of methodology, Ethics Approval, Programming of Experiment, Theoretical Model, Data Collection, Data Analysis, Drafting of the manuscript
Dr Vera Angelova, Technical University of Berlin, Germany	Conception of the project, Design of methodology, Data Collection, Data Analysis, Drafting, Critical Revision of manuscript
Dr Olga Gorelkina, University of Liverpool, UK	Conception of the project, Design of methodology, Theoretical Model, Drafting, Critical Revision of manuscript

5. AUTHOR DECLARATIONS (including the PhD candidate)

I agree to be named as one of the authors of this work, and confirm:

i. *that the description in Section 4 of my contribution(s) to this publication is accurate,*

ii. *that there are no other authors in this paper,*

iii. *that I give consent to the incorporation of this paper/publication in the candidate's PhD thesis submitted to the University of Liverpool*

Name of author	Signature*	Date
Yigit Oezcelik	Oezcelik	21.07.2022
Dr Vera Angelova	Vera Angelova	21.07.2022
Dr Olga Gorelkina	Gorelkina	21.07.2022

6. OTHER CONTRIBUTOR DECLARATION

I agree to be named as a non-author contributor to this work.

Name and affiliation of contributor	Contribution	Signature* and date
N/A		
N/A		

This consent form (Appendix B) or the sole author consent form (Appendix A) for each paper must be completed and kept by the PhD candidate once the paper is finalised. If the paper is to be included as part of the thesis, a copy of this form must be included in the PhD thesis with each publication.

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3. PAPER INCLUDED IN THE THESIS – JOINT AUTHORED PAPER

Title of the paper	Has this paper been published, presented at a conference or under review with a journal	If Yes, please complete the boxes below. If No, go to section 4
Non-Numerical and Social Anchoring in Consumer-generated Ratings	Yes	
If the paper has already been published please refer to the University guidelines on presentation of publications within a PGR Thesis - *https://www.liverpool.ac.uk/media/livacuk/tqsd/code-of-practice-on-assessment/annex-7.2-PGR-CoP.pdf*		
If the paper is under review with a journal, give details of the journal, including submission dates and the review stage N/A		
If the paper is presented at a conference, give details of the conference Economic Science Association Conference 2020 (World Meeting)		

4. DESCRIPTION OF ALL AUTHOR CONTRIBUTIONS (including the PhD candidate)

Name and affiliation of author	Contribution(s) (for example, conception of the project, design of methodology, data collection, analysis, drafting the manuscript, revising it critically for important intellectual content, etc.)
Yigit Oezcelik, University of Liverpool, UK	Conception of the project idea, Literature Overview, Design of methodology, Ethics Approval, Programming of Experiment, Theory, Data Collection, Data Analysis, Drafting of the manuscript
Michel Tolksdorf, Technical University of Berlin, Germany	Literature Overview, Design of methodology, Programming of Experiment, Theory, Data Collection, Data Analysis, Drafting of the manuscript

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Name of author	Signature*	Date
Yigit Oezcelik	*Oezcelik*	21.07.2022
Michel Tolksdorf		29.07.2022

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Title of the paper	Has this paper been published, presented at a conference or under review with a journal	If Yes, please complete the boxes below. If No, go to section 4
Rebate Rule in Reward-Based Crowdfunding: Introducing the Bid-Cap Rule	No	
If the paper has already been published please refer to the University guidelines on presentation of publications within a PGR Thesis - *https://www.liverpool.ac.uk/media/livacuk/tqsd/code-of-practice-on-assessment/annex-7.2-PGR-CoP.pdf*		
If the paper is under review with a journal, give details of the journal, including submission dates and the review stage N/A		
If the paper is presented at a conference, give details of the conference N/A		

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Yigit Oezcelik, University of Liverpool, UK	Literature Overview, Design of methodology, Application for Ethics Approval, Programming of Experiment, Theoretical Model, Data Collection, Data Analysis, Drafting of the manuscript, Critical Revising
Michel Tolksdorf, Technical University of Berlin, Germany	Conception of the project idea, Literature Overview, Design of methodology, Programming of Experiment, Theoretical Model, Data Collection, Data Analysis, Drafting of the manuscript
Fabian Gerstmeier, Technical University of Berlin, Germany	Literature Overview, Design of methodology, Programming of Experiment, Theoretical Model, Data Collection, Data Analysis, Drafting of the manuscript

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Name of author	Signature*	Date
Yigit Oezcelik	*Oezcelik*	21.07.2022
Michel Tolksdorf		29.07.2022
Fabian Gerstmeier		29.07.2022

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