Responsible and Honest Behavior: Essays in Experimental Economics

Inauguraldissertation
zur
Erlangung des Doktorgrades
der
Wirtschafts- und Sozialwissenschaftlichen Fakultät
der
Universität zu Köln

2015

vorgelegt
von

Diplom-Wirtschaftsmathematiker Mark Pigors

aus

Lutherstadt Wittenberg

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Tag der Promotion: 27.08.2015

Danksagung

Ich danke ganz besonders meiner Betreuerin und Koautorin Bettina Rockenbach für die Inspiration, Motivation und Unterstützung während meiner gesamten Promotionszeit.

Weiterer Dank gebührt meinen beiden anderen Koautoren Özgür Gürerk und Thomas Lauer für die hervorragende Zusammenarbeit.

Bernd Irlenbusch danke ich für die Übernahme des Korreferats.

Außerdem danke ich den ehemaligen und aktuellen Kollegen an der Universität Erfurt und an der Universität zu Köln für die angenehme Arbeitsatmosphäre, für die vielen nützlichen Kommentare und für die Hilfe bei der Durchführung der Experimente. Dazu gehören Robert Böhm, Christoph Feldhaus, Karen Heuermann, Mareike Hoffmann, Kerstin Hötte, Fabian Kleine, Florian Kochhan, Juliane Petereit, Christiane Pilz, Andreas Pollak, Balázs Rozsnyói, David Saxler, Anne Schielke, Sebastian Schneiders, Marcin Waligora, Arne Weiß, Christopher Zeppenfeld, Jarid Zimmermann und Nina Zimmermann.

Besonderer Dank gilt meiner gesamten Familie, aber vor allem meiner Frau Katja, die mir stets liebevoll zur Seite stand und einige Entbehrungen auf sich nehmen musste.

Außerdem danke ich der Deutschen Forschungsgemeinschaft (DFG) für die finanzielle Unterstützung bei der Durchführung der Experimente.

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1 Introduction

This thesis consists of four essays about responsible and honest behavior in different economic contexts. Classic economic theory neither considers honesty nor responsibility. The "homo oeconomicus" would be honest if and only if it is monetary benefiting and only has preferences over final monetary outcomes. Procedures itself do not matter.

Recent studies, however, show that subjects are generally sensitive not only to the final payoff allocation, but also to the procedure that produced the allocation (Frey, Benz, & Stutzer, 2004; Frey & Stutzer, 2005; Bolton, Brandts, & Ockenfels, 2005). Subjects treated unfair punish those who are "responsible" (Bartling & Fischbacher, 2012) and responsibility attribution in form of punishment can be reduced by avoiding direct interaction with the person affected (Coffman, 2011; Oexl & Grossman, 2013). Decision makers prefer not to be responsible or not to be perceived as responsible for unfair outcomes even when there is no threat of punishment (Dana, Cain, & Dawes, 2006; Hamman, Loewenstein, & Weber, 2010).

The meaning of the term "responsibility" differs between different environments. From an organizational perspective responsibility can be defined as "power to decide". The first essay in Chapter 2 uses this interpretation of responsibility. Here, we analyze non-strategic delegation as a device to shift responsibility for unfair outcomes.

A more general and ethical perspective might interpret "responsible behavior" as the consideration of all possible consequences of the own actions. Chapters 3 and 4 investigate consumer social responsibility, which is defined as "the conscious and deliberate choice to make certain consumption choices based on personal and moral beliefs", which may show up as "expressed activity in terms of purchasing or non-purchasing behavior" (Devinney, Auger, Eckhardt, & Birtchnell, 2006, p. 32). The research focus is whether consumers take responsibility for the indirect

consequences of their purchasing decisions and, if so, which market conditions promote consumers' responsibility.

While in Chapter 3, the proxy for consumer social responsibility is the wage of a powerless worker, the scope of consumer social responsibility is extended to honest procedures within the firm in Chapter 4. Various studies show that subjects have a preference for honesty and are averse to lying (see, for example, Gneezy, 2005; Lundquist, Ellingsen, Gribbe, & Johannesson, 2009; Gneezy, Rockenbach, & Serra-Garcia, 2013). The propensity to lie, however, depends on the monetary consequences of lying and is highly heterogeneous among subjects (Gibson, Tanner, & Wagner, 2013). While a fraction of subjects is "pure" lying averse and is reluctant to lie at all, many subjects lie the more, the more they can gain from it and the lower the possible negative consequences for others are (see, for example, Erat & Gneezy, 2012; López-Pérez & Spiegelman, 2013; Abeler, Becker, & Falk, 2014). Subjects even punish liars if they are not affected (Ohtsubo, Masuda, Watanabe, & Masuchi, 2010). The research question here is, whether honest or dishonest procedures matter for consumers and whether purchasing decisions may promote honesty in firms.

The economic literature on dishonesty focused on individual behavior so far. In Chapter 5, we investigate dishonest behavior in a teamwork setting where teammates submit self-reports about own productive efforts towards the joint team output. While there are many papers investigating lying, the literature on lying in team context is scarce. The study contributes to the growing experimental literature on lying, especially to unethical behavior in the workplace.

In the following, the four essays are outlined in more detail.

Chapter 2 studies non-strategic delegation as a device to avoid negative images by shifting responsibility to an agent. A principal may decide between an equal distribution and an unfair distribution, where she gains more at the expense of two receivers. Alternatively, the decision right can be delegated to an agent. We observe that a significant share of principals delegates. This indicates a preference for not being (seen as) responsible for unfair outcomes. Purely outcome-based preference models would not predict delegation. We provide a simple behavioral model incorporating social preferences and image concerns in this chapter, which is consistent with the experimental results.

Chapter 3 is joint work with Bettina Rockenbach and investigates the emergence of socially responsible production through consumer decisions. Although consumers regularly state that they would honor socially responsible or environmentally friendly production, even by paying higher prices (Carrigan & Attalla, 2001; Devinney, Auger, & Eckhardt, 2010; Öberseder, Schlegelmilch, & Gruber, 2011), the market for those goods is relatively small. We test whether this is a market failure or whether consumers just prefer to appear as socially responsible rather than really take responsibility through their consumption decisions. In a controlled laboratory experiment we study the repeated interaction of firms and consumers in a market. In every firm a worker produces a homogenous good which has a fixed monetary value to the consumer. The manager of a firm pays the worker and offers the good for a certain price on the market. In every market period the consumer may buy one unit of the good. Here, consumer responsibility means to take the wage into account when making purchasing decisions.

The experimental treatments vary the market conditions in two dimensions – competitiveness and opaqueness of wages. We find that consumers in a monopolistic market are predominantly interested in buying cheap. Consumers with little market power do not care for socially responsible production, irrespective of the information condition. Consequently, socially responsible production reduces a monopolistic supplier's profit and the chances of consumer induced socially responsible production are rather low. With supplier competition, when consumers have market power, socially responsible production positively influences consumers' buying decisions and suppliers offering socially responsible products achieve significantly higher profits, as long as their surcharge is not too high. In this case, already imperfect wage information suffices to achieve significantly higher wages than no wage information. Our results yield valuable insights into the possibilities and limitations of promoting socially responsible production through consumer behavior, and provide evidence for positive effects of competition on fostering social responsibility. The results are in line with Bartling, Weber, and Yao (2015), for example, and opposed to Falk and Szech (2013), who find that competition causes moral behavior to vanish.

A questionnaire study of O'Connor and Meister (2008) asks subjects to rank-order different measures commonly found in corporate social responsibility com-

munication and identifies honesty as the most important attribute of a corporation. Consequently, in Chapter 4 we extend the notion of socially responsible production to honest procedures within a firm. This chapter is also joint work with Bettina Rockenbach. Here, firms in a competitive market may save overall production costs by being dishonest to their workers. Workers have either a high or a low ability for the production process. High ability workers entail low production costs and "deserve" a high wage and vice versa. The worker's ability is observed by the firm, but not by the worker. The firm communicates the ability and the resulting wage to the worker. This communication does not have to be truthful. The research question is whether consumers honor honesty of the firm towards her worker. The experimental results show that increased observability increases producers' honesty as well as wages. If consumers have full information about a firm's honesty it becomes a significant purchasing criterion. In this case, honesty provides a competitive advantage and honest firms make higher profits by selling more units, albeit not at higher prices.

When the notion of social responsibility exceeds the scope of distributional fairness, it is not predicted by outcome-based models of social preferences, for example, inequity aversion as modeled by Fehr and Schmidt (1999). A simple model of consumer choices shows that, apart from allocative preferences, preferences over honest procedures are necessary for consumer responsibility to occur. The results of this chapter are qualitatively consistent with the model proposed.

Chapter 5 is based on Gürerk, Lauer, and Pigors (2015) and analyses dishonest behavior in a teamwork setting. It is very common that firms pay performance based salaries (Boyle, 2001). However, evaluating individual performance accurately is difficult or even impossible in team production. Therefore, performance measures often rely on individuals' self-reports. We experimentally study team production with a supervisor whose task it is to distribute bonuses among teammates. The treatments vary, on the one hand, the information of the supervisor, who either knows the true individual efforts or receives self-reports, which might not be true. On the other hand, the supervisor can freely distribute bonuses or has to follow a fixed ex-ante known distribution scheme. The research interest are the effects of the supervisor's reward power on teammates' self-reported effort information and on overall team performance. The results show that subjects

lie (exaggerate their own efforts) when they provide self-reports. However, they do so less if the supervisor has the power to allocate individual payments at her own discretion. A supervisor with reward power induces more truthful behavior. Consequently, the detrimental effects of exaggerations in self-reports on team performance are less pronounced if the supervisor has reward power.

2 It wasn't me: Responsibility, Image Concerns and Delegation

2.1 Introduction

Delegation is the assignment of a decision right to an agent. The following rationale for delegation is proposed by classical principal-agent theory: an agent might be an expert with more experience, skills and information to solve a special task, or the hiring of a special type of agent could serve as a commitment device (for an overview see, for example, Laffont & Martimort, 2009). In recent years, however, different experimental papers observe delegation in situations where the principal and the agent possess the same information and skills. In these cases, principals may strategically use delegation to avoid possible negative consequences from being responsible for undertaking morally questionable actions.

Bartling and Fischbacher (2012) use a four person dictator game setting with one "fair" and one "unfair" option to examine effects of delegation on perceived responsibility. The dictator is able to delegate the decision right to an agent who has exactly the same payoff function. After the dictator or the agent implemented an outcome, two recipients have the possibility to assign costly punishment points to all three other players. The experiment shows that a dictator can successfully shift the assigned punishment points (the "blame") to the agent, when the latter chooses the unfair option. Delegation is the ex-post profit maximizing strategy. Dictators anticipate this and delegate significantly more often in the treatments where punishment is possible. In addition, the paper provides a formal measure of responsibility of the unfair outcome, which works well in predicting the observed punishment patterns. It is evaluated ex-post and "assigns most responsibility [for

the unfair outcome] to the player whose action had the largest impact on the probability that unfair allocation results" (Bartling & Fischbacher, 2012, p. 74).

Oexl and Grossman (2013) replicate the observation that punishment can be shifted by delegating to an intermediary, even when the intermediary's action set is bounded to unfair options.[1] This challenges the responsibility measure by Bartling and Fischbacher, since a bounded intermediary cannot increase the probability that an unfair outcome occurs.[2] A study by Coffman (2011) also questions the responsibility measure. Here, intermediation reduces third party punishment although the principal's responsibility is not decreased. Punishment is decreased because the use of an intermediary avoids direct interaction with the receiver. Remarkably, even punishers who are aware of the fact that intermediation is used to avoid punishment and that intermediation results in an lower outcome for the receiver punish intermediation less than the comparable direct action.

In the studies mentioned above, delegation (or intermediation) is strategically used to avoid punishment for being seen as responsible for the unfair outcome. The research question of this study is whether the dictator herself prefers not to be responsible for negative outcomes. In the original experiment of Bartling and Fischbacher (2012), 17% of the principals delegate the decision in a control treatment without punishment option for the receivers. Bartling and Fischbacher argue that a principal potentially delegates to avoid "psychological costs" or "cognitive dissonance". Another explanation is, however, that subjects have image concerns and prefer not be perceived as unfair persons (compare, for example, Andreoni & Bernheim, 2009). Principals with such preferences use delegation as device to shift the responsibility for unfair outcomes to the agent.

We experimentally test this hypothesis using the Bartling and Fischbacher design and implement delegation as an "exit-option" (Dana et al., 2006) to rule out other explanations for delegation. The decision between distribution choice and delegation choice is splitted into two steps, so that the principal is not informed about the delegation option when choosing one of the two distributions. After

[1] Oexl and Grossman use the term "intermediation". It is a "weak" delegation – the agent might not get the full decision right (compare Coffman, 2011).

[2] This is in line with the results of Fershtman and Gneezy (2001), who show that the pure presence of an agent (without responsibility) is sufficient to raise the dictators payoff and decrease punishment or rejection rates, respectively.

the principal has chosen distribution 1 or distribution 2, she is asked whether the choice is to be implemented or whether the whole decision should be delegated to the agent. In this manner, delegation cannot serve as a device to avoid decision making in general and the psychological cost of solving the dilemma between self-interest and fairness are already sunk. We provide a simple behavioral model incorporating social preferences and image concerns that are influenced by responsibility attribution. The model predicts no delegation when principals only have preferences over monetary outcomes. With image concerns, however, delegation can be the optimal choice. The results show that still almost one fifth of the principals delegate after they have made a distribution choice. This indicates the existence of image concerns. Moreover, subjects only delegate when they expect the agents to choose the unfair distribution.

2.2 Related literature

The avoidance of responsibility absent any strategic reasons is observed in different experimental settings. Dana et al. (2006) study an exit option in a dictator game. After deciding about the share of money (between 0$ and 10$) they send to the receiver, dictators can choose an exit option in a second stage leading to a (9$, 0$) distribution. By choosing this costly and Pareto-dominated exit option it is ensured that the receiver never gets to know that a dictator game has been played. 28% of the dictators take the exit option. Outcome-based preferences cannot explain such behavior. A purely selfish dictator would not send money leading to the (10$, 0$)-distribution and any dictator with other-regarding preferences would at least prefer (9$, 1$) over (9$, 0$). In another treatment the exit option is almost never chosen when the receivers do not learn that a dictator game is played at all. Here, the exit option is not able to change receivers' beliefs and their attribution of responsibility on the final outcome.

In a related paper Dana, Weber, and Kuang (2007) find evidence that observed fair behavior displays an "illusionary preference for fairness". Their study uses different dictator game settings allowing decision makers to "exploit the moral wiggle room". Dictators can do this by remaining ignorant about the receiver's payoff consequences, although uncovering the receiver's payoff is costless. A dic-

tator avoids full information (and the possibility to implement her most preferred outcome) to hide his intentions and responsibility for the receiver's payoff behind chance.[3]

Erat (2013) reports an experiment about lying and delegation. A sender can send a signal to a receiver or delegate the task to an agent. The final payoff for all players is determined by the receiver's choice. Since the receiver is uninformed about the payoff consequences, the principal sends the potentially false signal and might lie to gain a higher payoff at the expanse of the receiver. Erat finds that principals use agents to avoid lying themselves. The higher the potential harm from lying towards the receiver, the more they delegate.

Hamman et al. (2010) study non-strategic delegation in a repeated dictator game. Dictators can choose whether or not to hire an agent and if so which agent to hire. Even when the agents communicate via cheap-talk how much they are going to give, dictators hire agents giving less than they would give on their own. The results show that dictators do not think they act immorally by just hiring agents and that they do not feel responsible for the final outcome when doing so. Consequently, delegation results in "unfairer" outcomes – the share given to the receiver decreases when an agent decides on the behalf of the dictator.

The experiments show that subjects do not only have preferences over final outcomes, but take the beliefs and perceptions of others into account.[4] Delegation works as an device to avoid a negative self and social image of being an unfair person, or being responsible for an unfair outcome, respectively (Bénabou & Tirole, 2006; Andreoni & Bernheim, 2009).

2.3 Experimental design

Our experiment modifies the experimental game of Bartling and Fischbacher by implementing the delegation choice as an exit option (Dana et al., 2006). The principal's distribution and delegation decision are split into two stages. The

[3]This does not shift responsibility in the sense of Bartling and Fischbacher, since "chance" cannot take responsibility.

[4]In psychological games (Geanakoplos, Pearce, & Stacchetti, 1989) players' payoffs depend on what they think that every player believes.

details of the game are as follows. There is a group of four players: one principal (participant A), one agent (B) and two receivers (C). The principal has to decide how to distribute 20 € . She could choose between two possible distributions (see Table 2.1), an unfair one (called distribution 1) and a fair one (distribution 2).[5] After this choice, the principal can choose whether her decision is implemented or the decision is delegated to the agent. The principal does not know about the delegation option when choosing one of the distributions. Receivers do not make decisions.

Table 2.1: Monetary payoffs for the two distributions

Payment in €	A	B	C	C
Distribution 1	9	9	1	1
Distribution 2	5	5	5	5

In the experiment, the two stage design was implemented in the following way. In the general part of the instructions, subjects were told that either participant A or participant B will decide between the two distributions. In the private instructions, participant A was asked to decide between the two options. At this moment, player A did not know that delegation was possible. In the next stage, participant A could decide whether the former decision should be implemented or whether she wants to delegate the decision to participant B, who is informed about the delegation option. In the latter case her first decision would be neglected and no other player would be informed about the former distribution decision. Simultaneously players B and C were informed that participant A has the choice between the implementation of one of the two allocations and delegating the decision to participant B. B was asked to decide between the two allocations in case of delegation. Supplementally, beliefs about the expected average behavior of the participants of the other types were elicited. Subjects were asked about reasons for and against delegation and had to answer some demographic questions. After the experiment was finished, all players were informed about the final allocation and who made it (delegation or no delegation) and received their payments. See Section 2.7 for an English translation

[5] We prefer the labels "equal" and "unequal" instead of "unfair" and "fair" as in Bartling and Fischbacher.

of the instructions. Instructions carefully prevented any form of deception and decisions were made on a special form to implement the stage by stage design.

We conducted the experiment in pen and paper form at the University of Erfurt in June 2011. There were two sessions in two different undergraduate statistics courses. In total, 133 students of economics and social sciences participated in session 1, where session 2 consisted of 93 students of educational sciences. By the different study programs we can assume that no subject participated in both sessions. The experiment lasted about 30 minutes and took place at the beginning of the particular lecture and had not been announced before the lecture started. The general instructions were distributed and read out loud. Then, types were randomly assigned by the distribution of sealed envelopes with a type depending experiment form. Once everybody finished, the envelopes were collected and groups were randomly matched. After the lecture was finished, everybody privately got an information sheet with the decisions and payments in the matching group.[6] The average payment was 5 €.

2.4 Social preferences and image concerns

In this section we provide a simple model where principals are assumed to have preferences over monetary outcomes and have image concerns that result from an outcome and the responsibility therefor. In the game considered we assume that choosing the equal distribution is "the right thing to do" from a normative perspective and therefore results in a positive image. Choosing the unequal distribution, on the other side, results in a negative image. Not being responsible (in case of delegation) results in no image change.[7] The model is in the spirit of Grossman (2012), Grossman and van der Weele (2013), respectively, and does not argue whether such image concerns are about self- or social image.[8] The image is

[6] There were leftovers of types B and C, where no group matching was possible, due to the random distribution of envelopes. Participant of type B were paid according to their decision and participants of type C were randomly matched to one leftover B and paid accordingly.

[7] Here, one is simply responsible or not. Bartling and Fischbacher (2012) use a more elaborate responsibility measure with more than three states depending on beliefs about average behavior.

[8] The image might be manifested in an observer, who might be the receivers (social-image) or interpreted as a dual-self (self-image), see Grossman and van der Weele (2013).

manifested when the outcome is implemented. For example, choosing distribution 1 in stage 1 will not result in a negative self-image, when one delegates in stage 2. Only the final responsibility for the outcome influences the image. Principals have the following utility function

$$u(x, \tau, \lambda) \quad = M(x, \tau) + \lambda \cdot I(x), \tag{2.1}$$

where $M(x, \tau)$ is the utility from monetary income dependent on action x. The parameter $\tau \in \mathbb{R}_+$ reflects social-preferences. $I(x)$ is the image utility that results from action x. Image concerns are weighted with $\lambda \in \mathbb{R}_+$. The principal chooses $x \in \{1, 2, 3\}$, where 1 represents distribution 1, 2 represents distribution 2 and 3 represents delegation, and believes that the agent will choose option A with probability $0 < b < 1$.[9] The monetary utility in our experimental model is

$$M(x, \tau) \quad = \begin{cases} 9 - \tau & \text{if } x = 1 \\ 5 & \text{if } x = 2 \\ b(9 - \tau) + (1 - b)5 & \text{if } x = 3 \end{cases} \tag{2.2}$$

A principal with type $\tau = 0$ has standard selfish preferences. Any $\tau > 0$ mirrors social preferences like inequity-aversion (Fehr & Schmidt, 1999; Bolton & Ockenfels, 2000) or maximin-preferences (Charness & Rabin, 2002). While there is no disutility from inequity in the equal distribution 2, there is disutility from advantageous inequity in distribution 1 according to the Fehr and Schmidt model.[10] In the Bolton and Ockenfels model the unequal distribution 1 causes disutility from having more than the average of 5. In the Charness and Rabin model τ is the disutility caused by the receivers getting less in distribution 1 than in distribution 2.

The image concerns are as stated above. Choosing the unequal distribution 1 results in a negative image of being an unfair person, so $I(1) = -1$. Choosing the equal distribution 2 results in a positive image of being a fair person, so $I(2) = 1$.

[9] We do not provide an equilibrium analysis where beliefs have to be consistent with actions. The principal has belief about average agent behavior as elicited in the experiment as the the share of agents that choose allocation 1 in case of delegation.

[10] Distribution 1 gives the principal 8 more than both receivers and the same payoff as the agent, so $\tau = \frac{16}{3}\beta$ in the Fehr and Schmidt model.

Delegation does not influence the image, so $I(3) = 0$. Than, the complete utility function is given by

$$u(x, \tau, \lambda) = \begin{cases} 9 - \tau - \lambda & \text{if } x = 1 \\ 5 + \lambda & \text{if } x = 2 \\ b(9 - \tau) + (1 - b)5 & \text{if } x = 3 \end{cases} \tag{2.3}$$

In stage 1 the principal has to decide between distribution 1 and distribution 2. He strictly prefers distribution 1 over distribution 2 if $\tau < 4 - 2\lambda$, strictly prefers distribution 2 over distribution 1 if $\tau > 4 - 2\lambda$ and is indifferent if $\tau = 4 - 2\lambda$. In stage 2 the principal has to decide between delegation and the decision of stage 1. A principal who has chosen distribution 1 in stage 1, will delegate if and only if $\tau > 4 - \frac{\lambda}{1-b}$ and a principal who has chosen distribution 2 will delegate if and only if $\tau < 4 - \frac{\lambda}{b}$.[11] We derive

Proposition 2.1. *A principal without image concerns does not delegate.*

Proof. Suppose no image concerns, so that $\lambda = 0$. Case 1: $\tau < 4$. The principal chooses distribution 1 in stage 1 and prefers to keep distribution 1 over delegation in stage 2. Case 2: $\tau > 4$. The principal chooses distribution 2 in stage 1 and prefers to keep distribution 2 over delegation in stage 2. Case 3: $\tau = 4$. The principal is indifferent between distribution 1 and distribution 2 in stage 1 and prefers to keep either option over delegation in stage 2. □

Proposition 2.2. *A principal, who believes the probability of the agent choosing distribution 1 in case of delegation is lower or equal than one half, does not delegate.*

Proof. Consider a principal who delegates in stage 2, so that $\lambda > 0$ (Proposition 2.1). Case 1: The principal had chosen distribution 1 and delegates in stage 2. It follows $4 - 2\lambda \geq \tau > 4 - \frac{\lambda}{1-b}$. Therefore $b > \frac{1}{2}$. Case 2: The principal had chosen distribution 2 and delegates in stage 2. It follows $4 - 2\lambda \leq \tau < 4 - \frac{\lambda}{b}$. Therefore $b > \frac{1}{2}$. □

Figure 2.1 shows the possible actions of the principals and the according ranges of τ. The model predicts the following: First, purely outcome-based models of social

[11] Thereby, we assume a weak preference to keep the decision of stage 1 to remove indifference in the delegation decision. An interpretation would be risk aversion.

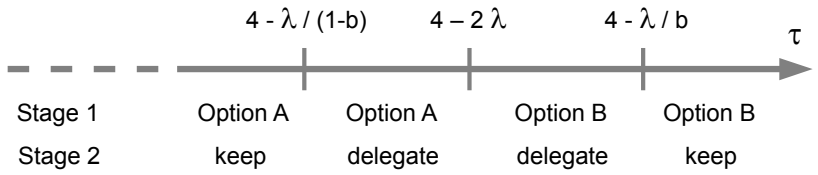


Figure 2.1: Principals actions depending on τ with $\lambda > 0$ and $b > \frac{1}{2}$

preferences do not predict any form of delegation, unless a subject is completely indifferent between the two distributions.[12] Second, the higher the disutility from a negative image or the higher the weight of that image, the higher the likelihood to choose distribution 2 in stage 1 and to delegate in stage 2. In this sense, the model predicts an "illusory preference for fairness" (Dana et al., 2007). A share of principals would like the unequal distribution to be implemented, but does not want to be responsible, so that the distribution 2 is chosen in the first place and delegation when offered.[13] Third, the higher the belief about the the agent choosing the unequal distribution 1, the more likely is delegation. A principal who is willing to implement the unequal distribution 1 in stage 1 also might shift responsibility to the agent, and loose the negative image. The principal lets the agent choose the unequal distribution, when she believes that he will do so. We derive the following hypotheses:

Hypothesis 2.1. *A significant share of principals delegates in stage 2, after choosing distribution 1 or distribution 2 in stage 1.*

Hypothesis 2.2. *Principals delegate if they belief that the probability that the agent will choose the unequal distribution 1 is high ($b > \frac{1}{2}$).*

The model above derives predictions for the behavior of the principals. Agents might behave differently. Some studies show a certain kind of commitment of

[12] This requires very strong social preferences, for example an advantageous inequity parameter $\beta = 0.75$ ($\tau = 4$) in the Fehr and Schmidt (1999) model.

[13] The data of the two punishment-free treatments of Bartling and Fischbacher's experiment support this conjecture, see the more detailed results in the working paper (Bartling & Fischbacher, 2008). Comparing the two treatments, we see that the share of unfair principals, who choose the unequal distribution, is constant independent of whether delegation is possible or not, whereas the share of dictators choosing distribution 2 is halved in benefit of delegation.

Table 2.2: Delegation decisions

	Principals			Agents
	no delegation	delegation	overall	overall
Distribution 1	15	6	21	28
Distribution 2	29	4	33	31
Sum	44	10	54	59

the agents for "their" principals in such relationships (for example, Bartling & Fischbacher, 2012; Hamman et al., 2010). Given Proposition 2.2 and Hypothesis 2.2, the agent has to believe that the principal believes that the agent will choose distribution 1, so that choosing distribution 1 will not result in a negative image for the agent as he is expected to do so.[14] These considerations lead to

Hypothesis 2.3. *The share of agents choosing the unequal distribution is higher than the share of principals choosing the unequal distribution.*

2.5 Results

Table 2.2 shows the distribution and delegation decisions of principals and agents. About 34% (21/54) of the principals choose the unequal distribution 1 in the first stage. Overall, 19% (10/54) of the principals delegate.[15] The principals choosing distribution 1 delegate more often (29% vs 12%), but not significantly (Fisher's exact test, one-sided: p=0.162). Thus, Hypothesis 2.1 cannot be rejected.

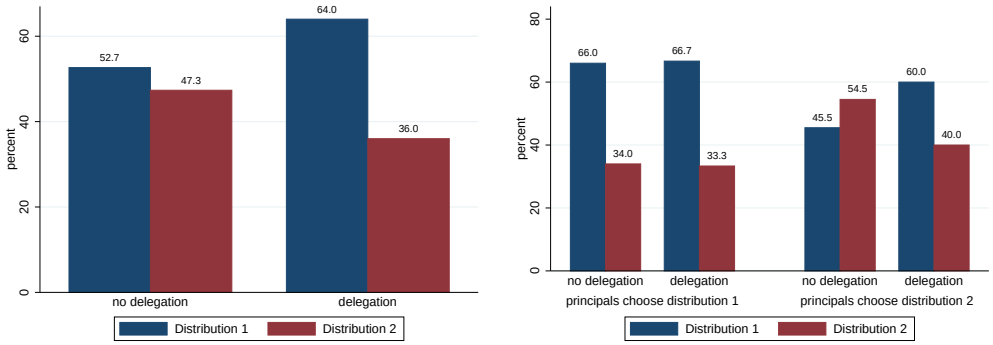
Result 2.1. *A significant share of both types of principals delegates.*

Figure 2.2 displays the principals' beliefs about the agents' behavior depending on the delegation decision.[16] We see that principals who delegate believe that a higher share of agents chooses distribution 1, but the difference is not significant

[14] This is similar to models of guilt aversion (Charness & Dufwenberg, 2006), where choosing distribution 2 would cause feelings of guilt for the agent since the principal expected to get the higher payoff from distribution 1.

[15] This share is significantly higher than a random delegation with 10% probability, (Binomial test, one-sided: p=0.040.)

[16] All beliefs are population averages and were not incentivised.



(a) for the delegation decision

(b) for stage 1 decisions

Figure 2.2: Principals' beliefs about agents choosing distribution 1

(64% vs. 53%, Mann-Whitney U test: p=0.102). In both cases, however, the beliefs are (weakly) significantly higher than the actual share of agents choosing distribution 1, which is 47% (Wilcoxon signed rank test: p=0.096 for no delegation, p=0.028 for delegation). Regarding Proposition 2.2, eight of the ten subjects who delegate belief that more than 50% of agents will choose distribution 1. A more detailed look at the different types of principals reveals some differences. Principals choosing distribution 2 and delegation believe that 60% of the agents choose distribution 1, while the ones who do not delegate believe that only 46% of the agents will do so, whereas there is no difference for the principals who choose the unequal distribution.[17] Overall, Hypothesis 2.2 cannot be rejected.

Result 2.2. *Principals, especially those who delegate, overrate the unfairness of agents.*

28 out of 59 agents (47%) choose distribution 1, while only 21 out of 54 (39%) of the principals do so in stage 1. Nevertheless this difference is not significant (Fisher's exact test, one-sided: p=0.233). There does not seem to be a difference in the behavior of principals and agents (compare Hypothesis 2.3).

Result 2.3. *There is no difference in the distribution choice between principals and agents.*

[17] The differences for principals choosing distribution 2 is not significant, since there are only four observations of delegation.

Finally, it is important to mention that we observe significant differences in behavior between the two sessions. Principals in session 2 (educational science) are much "fairer" than principals in session 1 (economics and social sciences). While 17 of 31 (55%) principals of session 1 choose distribution 1 in stage 1, only 4 of 23 (17%) do so in session 2 (Fisher's exact test, two sided: p=0.010). Interestingly, the behavioral differences do not hold for agents who choose distribution 1 in half of the cases (47% in session 1, 48% in session 2).

2.6 Conclusion

We use a two stage dictator game with delegation opportunity to study the avoidance of negative images through the shifting of responsibility. We find that one fifth of the principals delegates the decision right after making the distribution choice. The results indicate a preference for not being (seen as) responsible for unfair outcomes – subjects have preferences over images. A simple behavioral model incorporating social preferences and image concerns is consistent with our observations, while purely outcome-based preference models would predict no delegation at all. Moreover, as the model predicts, subjects delegate when they expect that the agents will choose the unequal distribution. Finally, the results also show "an illusionary preferences for fairness".

2.7 Appendix

In this section we present the instructions of the experiment. The original instructions were in German and the ones listed below are translations into English.

General Instructions for Participants

We cordially welcome you to this economic experiment and we thank you for your participation.

It is very important that you read the following instructions carefully. In this experiment, you can earn money - depending on your decisions and the decisions of the other participants. If you have any questions, please direct them to us. During the experiment, **it is not allowed to talk to the other participants**. Non-compliance with this rule will result in **exclusion from the experiment and from all payments**. In the experiment, we will not speak of Euros but of points. Your entire income will thus first be calculated in points. The total number of points scored during the experiment will be converted into Euros in the end, with

$$1 \text{ Point} = 1 \text{ Euro}.$$

To conduct the experiment, you will receive an experimental sheet on which you will give all answers in written form. After the experiment and the lecture, you will receive an answer sheet with the results of your group from us and the number of points you earned in the experiment will be paid to you in cash.

The Experiment

In the experiment, **you and three other persons are randomly assigned to a group**. You do not learn the identity of these three persons, neither during the experiment, nor afterwards. The three persons assigned to you do not learn anything about your identity, either. In this experiment, there are three types of participants: One participant A and one participant B, as well as two participants C. The assignment of types is also **random**. In this experiment, either participant A or participant B decides, **how 20 points are divided between the four participants**. In the course of the experiment, you will learn who of the two participants decides.

Both participants C cannot make a decision in this experiment.

Participant A or B has to decide between two possible allocations:

- Distribution 1: Participant A and participant B each receive 9 points and both participants C receive 1 point, respectively.
- Distribution 2: Participant A, participant B and both participants C receive 5 points each.

The following table again gives you an overview over the two distributions, between participant A or participant B has to decide.

	Points of participant A	Points of participant B	Points of one participant C	Points of th other participant C
Distribution 1	9	9	1	1
Distribution 2	5	5	5	5

Important Notes

- Only use pens, fineliners or the like (no pencils) on the experimental sheet.

- Think carefully before you decide and make an entry. Corrections – of any kind – are not possible and cannot be considered.
- Moving backwards through the pages or opening the sheet ahead of time is not allowed.

Non-compliance with these rules will lead to exclusion from the experiment and from all payments.

If you have any remaining questions, please ask one of the experimenters now.

Question Form Type A

[**Stage 1**]

You are participant A

You take the decision. Please choose now between allocation 1 and allocation 2.

	A	B	C1	C2	Your choice
Distribution 1:	9	9	1	1	❐
Distribution 2:	5	5	5	5	❐

[**Stage 2**]

You can now choose if the allocation decision you just made shall be implemented or if you want to delegate the decision to participant B. Participant B knows that you have the opportunity to delegate to him/her. He/she decides just like you between allocation 1 and allocation 2 in case you want to delegate. Both participants C also know that you have the opportunity to delegate.

You now have the following alternatives:

Maintain the decision	Delegate the decision
...the allocation chosen in the last step will be implemented. ...participant B and participants C will learn that you have decided and will learn the final allocation.	...all participants will learn that you delegated to participant B. No participant will learn your previous choice. ...all participants will learn that participant B has decided and will learn the final allocation.
Maintain the decision: ❐	Delegate the decision: ❐

[**Stage 3**]

What do you think

How large is the share of participants B who decide for allocation 1 or allocation 2, respectively, in case it is delegated to him/her?

Please enter your expectations. Please consider that the percentages have to add up to 100.

	A	B	C1	C2	Share in percent
Distribution 1:	9	9	1	1	_____
Distribution 2:	5	5	5	5	_____

Which arguments speak in favor of delegation?

Which arguments speak against delegation?

Question Form Type B

[**Stage 1**]

You are participant B

Participant A takes the decision initially. Participant A can either decide between allocation 1 and allocation 2 him-/herself or he/she can delegate the decision to you. If participant A does not delegate, his/her decision determines the payoffs. If participant A delegates, then your decision determines the payoffs.

Please choose now either allocation 1 or allocation 2 in case participant A delegates to you.

	A	B	C1	C2	Your choice
Distribution 1:	9	9	1	1	❐
Distribution 2:	5	5	5	5	❐

[**Stage 2**]

What do you think

How large is the share of participants A who decide for allocation 1, allocation 2, or delegation, respectively?

Please enter your expectations. Please consider that the percentages have to add up to 100.

	A	B	C1	C2	Share in percent
Distribution 1:	9	9	1	1	_____
Distribution 2:	5	5	5	5	_____

[**Stage 3**]

What do you think

From the point of view of participant A, which arguments speak in favor of delegation?

Which arguments speak against delegation?

Question Form Type C

[**Stage 1**]

You are a participant C

Participant A takes the decision initially. Participant A can either decide between allocation 1 and allocation 2 him-/herself or he/she can delegate the decision to participant B. If participant A **does not delegate** the decision between allocation 1 and 2, participant A takes the decision. If participant A **delegates** the decision between allocation 1 and 2, participant B takes the decision.

Participant B chooses between allocation 1 and 2. He/she knows that his/her decision will only be implemented in case of a delegation.

Which behavior do you expect from participant A and participant B? Please enter your answers on the next page.

[**Stage 2**]

What do you think

How large is the share of participants A who decide for allocation 1, allocation 2, or delegation, respectively?

Please enter your expectations. Please consider that the percentages have to add up to 100.

	A	B	C1	C2	Share in percent
Distribution 1:	9	9	1	1	_____
Distribution 2:	5	5	5	5	_____

How large is the share of participants B who decide for allocation 1 or allocation 2, respectively, in case it is delegated to him/her?

Please enter your expectations. Please consider that the percentages have to add up to 100.

	A	B	C1	C2	Share in percent
Distribution 1:	9	9	1	1	_____
Distribution 2:	5	5	5	5	_____

[**Stage 3**]

What do you think

From the point of view of participant A, which arguments speak in favor of delegation?

Which arguments speak against delegation?

Questionaire all Types

Questionaire

Finally, please answer the following three questions.

Age: _____

Sex: female ❒ male ❒

Did you ever participate in an economic experiment yet?

Yes ❒ No ❒

3 Consumer Social Responsibility

3.1 Introduction

Socially responsible (SR[1]) production is not a new topic. Poor working conditions and insufficient wages were already debated during times of industrial revolution more than a century ago. The new facet in today's discussion is that increased globalization and highly decentralized production processes have added more complexity to the problem. Recently, insufficient SR in production was prominently discussed when Apple Inc. was accused of the poor working conditions in the factories of Foxconn leading to workers' suicides[2], or when fires in garment factories in Pakistan and Bangladesh which produced for western companies killed hundreds of workers[3]. "Fair" prices for coffee, cocoa and bananas or the usage of child-labor in production (for example, rugs, soccer balls or cocoa, see Burke, 2012) are recurrent discussions in recent decades. In reaction, the public as well as politicians call for stronger regulations on the producer side.

But what about the other market side? Do consumers care about SR production and if so do they try to influence the social dimension of production through their consumption decisions? Although stated preferences in questionnaires and hypothetical consumption decisions indicate that a substantial fraction of consumers would honor socially responsible or environmental friendly production, even by paying higher prices (Carrigan & Attalla, 2001; Devinney et al., 2010; Öberseder et al., 2011), the market for those goods is relatively small. Is this a market failure or do consumers not put their money where their mouth is? Understanding

[1]Depending on context, we use SR both for "socially responsible" and "social responsibility".
[2]Light and death. A series of deaths expose a big computer-maker to unaccustomed scrutiny, Economist 2010.
[3]A "distinctly South Asian" tragedy, Economist 2012.

the causes of this discrepancy is essential for efficiently using consumers' social responsibility in production.

The literature offers four predominant explanations for the differences between stated preferences and actual consumption decisions. One explanation lies in the *opaqueness of the production process*: Consumers do not know whether the mark-up for SR products actually reaches the intended addressees or whether the money dissipates on the way (Balineau & Dufeu, 2010; Enste, Knelsen, & Neumann, 2012). Eckhardt, Belk, and Devinney (2010) conduct in-depth interviews across eight countries and offer three other (not mutually exclusive) explanations: *Economic rationalization* (consumers want to get the most value for their money, regardless of their ethical beliefs), *Institutional dependency* (consumers believe that institutions such as the government are responsible to ethically regulate what products can be sold), and finally *Developmental realism* (consumers believe that some unethical behaviors on the part of corporations must exist in order for macro level economic development to occur).

Although this already helps shedding light on the discrepancy between stated preferences and actual consumption behavior, the impact of each explanation and their possible interactions remain unclear. Consequently, Eckhardt et al. (2010) state that their study "reinforces the need for non-survey-based research to understand nuanced consumer reactions and behaviors in ethical consumerism" (p. 427). Our study aims at filling this gap by examining small economies with both production and consumption decisions involving real monetary consequences under controlled conditions. Our setup allows for investigating the effects of various market characteristics on SR consumption decisions and thus for disentangling the proposed causes and their interactions. The novelty of our approach lies in analyzing SR consumption in a variety of relevant market settings by focusing on consumers as well as producers.

In our laboratory experiment, consumers and firms interact in a market. Firms offer a good with a fixed induced monetary value to the consumers. Next to the good's price, the only other attribute is the wage, which is paid to the firm's worker for producing the good. The wage is our proxy for SR. The experimental treatments vary this basic design in two (orthogonal) dimensions. One dimension varies the opaqueness of the SR in the production process, i.e., the availability

and precision of the wage information to the consumer. In this dimension we have five different treatment variations. In the *Baseline* treatment consumers have no information on the worker's wage and are not able to acquire this info. In the *Full Info* treatment the wage is fully transparent. Moreover, we conduct three treatments with an endogenous information transmission. In treatment *Choice* ex ante uninformed consumers choose whether they acquire information on the wage or whether they remain ignorant. In treatment *Label* firms have the possibility to acquire a label which assures that the firm follows minimal standards in SR production, i.e., a minimum wage. Hence, in this treatment imperfect wage information can be transmitted by the firms. Finally, in treatment *Face*, workers can signal wage information to the consumers by showing their satisfaction with the wage to the consumers. This condition mirrors cases in which workers use mass media to call attention to their poor working conditions, as happened at Amazon or Foxconn, or stitch SOS notes in clothes, as reported to has happened at Primark.

Our second treatment dimension studies how the competitiveness of the market influences consumers' SR behavior. We compare all five information treatments described above for a monopolistic supplier (*No Competition*) and for suppliers in Bertrand duopoly (*Competition*). By studying SR consumption decisions in the 10 treatment conditions (5 information $\times$ 2 competitiveness conditions), we are able to draw an elaborate picture on the determinants of consumer social responsibility. In the laboratory experiment we cannot tackle the explanation of *developmental realism*. But we ask whether consumers indeed always go for the lowest price or whether they – and if so under which conditions – take the SR in the production into account (*economic realization*). Additionally we ask whether consumers take responsibility for the kind of products on the market by banning products not produced in a socially responsible way, thus tackling the question of *institutional dependency*. Moreover, we can determine how the *opaqueness* of the information on SR and the way in which this information is transmitted influences consumption decisions and in which way these factors interact with the competitiveness of the market. By doing so, we provide the first study systematically varying conditions for consumer social responsibility.

Our findings are striking. In the absence of competition consumers are predominantly interested in buying cheap and do not care for SR production, irrespective of the information on SR. Consequently, SR production significantly reduces the profit of monopolistic suppliers. This seems to indicate that with a monopolistic supplier the chances of consumer induced SR production are rather low. When suppliers compete, however, we find that consumers take SR in production as a decision criterion and go for the SR-produced good whenever the price premium for SR is not too high. Interestingly, not only full wage information, but also imperfect wage information (in treatment *Face*) or the possibility of acquiring wage information (in treatment *Choice*) suffices to achieve significantly higher wages than without any wage information. Accordingly, in competitive settings suppliers with higher levels of SR even achieve significantly higher profits, as long their price is not higher. Our results show a positive effect of competition on SR, but at the same time demonstrate that a regulatory focus on the producers is necessary to increase the overall level of SR in production.

3.2 Related literature

To overcome the problem of non-incentivized questionnaire studies, several field experiments on consumption decisions study consumers' preferences for labeled products manipulating the supply side. Arnot, Boxall, and Cash (2006), for example, manipulate the prices of fair-trade and conventional coffee in a university campus cafeteria and study the consumption behavior. Fair-trade-coffee demand in a U.S. grocery store is analyzed by Hainmueller, Hiscox, and Sequeira (2014). Hiscox, Broukhim, Litwin, and Woloski (2011), as well as Hiscox and Smyth (2011), analyze the effects of labels certifying fair working conditions on the sales of polo shirts on eBay, respectively on candles, towels, and dolls in a store in New York. These studies conclude that there is a fraction of consumers with rather low price sensitivity which is willing to pay for SR produced goods, while another fraction of consumers is very price sensitive. Auger, Devinney, Louviere, and Burke (2008, 2010) experimentally examine how consumers value specific social attributes on different products. The former study measures the price premium consumers are willing to pay for ethical features on products (shoes and soap). The willingness

to pay such a premium is much higher when there is no dilemma between the ethical and a "normal" product feature. The latter, a multi-country comparative study, shows that social attributes are more important in developed countries than in emerging ones. Tagbata and Lucie (2008) measure consumers' willingness to pay for organic and fair trade products using the Becker-DeGroot-Marschak mechanism (1964) with real consumption consequences. They show that for a specific cluster of consumers labels increase consumers' willingness to pay. The advantage of these studies is that decisions with real monetary consequences close the attitude-behavior gap between questionnaires and real markets, but they only analyze one market side.

Laboratory experiments studying SR production in competitive markets address this issue. Rode, Hogarth, and Le Menestrel (2008) experimentally analyze tripolistic markets with ethical differentiation. While two firms set prices for a homogeneous good, the third producer bears an extra cost. For every unit sold the cost difference is donated to an NGO fighting child labor. They find that many consumers are willing to pay higher prices to buy the product with the extra cost. Remarkably, the price premium is higher than the extra cost. Though the experiment has the advantage of modeling both market sides, producers cannot compete in social responsibility. Its degree is experimenter imposed and randomly attached to one of the three producers. This problem is partially solved by Etilé and Teyssier (2012) as well as Feicht, Grimm, and Seebauer (2014) by endogenizing the level of the donation to an NGO. The treatments vary the credibility of signals on donations. Results show that SR behavior demands credible signals and offering SR products does not increase firms' profits. Also in these two experiments, however, SR is only indirectly attached to the production process by donations to a third party, not involved in production.

In the experiments mentioned so far, donations are made per unit sold. When consumers "punish" socially less-responsible producers by refusing to buy their products, they automatically reduce the social benefits. This may induce consumers to refuse from punishing non-SR producers. Similar effects are reported in Danz, Engelmann, and Kübler (2012). They study the effect of minimal wage standards on consumers' SR. In their experimental duopoly market, a consumer buys up to ten units of a good. The two producers pay a piece wage to their

assigned worker for producing an otherwise identical good. Consumers often just split their demand equally between both firms in order to support both workers even when prices and wages differ.

Bartling et al. (2015) analyze SR in competitive markets where each producer can offer one unit of two possible products: a "normal" one and one with lower production cost that reduces the payoff of a third person. The visibility of the goods' impact on the third party is varied between treatments. SR is measured as the share of products without negative impact. In their experiment producers offer the costly normal goods and consumers accept a price premium for these goods. Although increased firm competition (8 instead of 6 firms) lowers prices, SR behavior is not affected. Also in this experiment the negative externality towards the third party only occurs when the good is traded. Bartling et al. interpret this as a negative externality that arises by consumption or as "production on demand". It is very likely, however, that consumers of soccer balls or shirts do not consider the situation in this way. The good is already produced and by refusing to buy the offered good, a consumer may at best affect future working conditions.

With our design, we aim at closing important gaps in the literature on SR production by combining the following characteristics: First, we study both market sides (consumers and producers) in an experimental market with real monetary consequences. Second, the production process is transparent and there is no uncertainty as to who is affected by SR production. Third, by affecting another participant the SR is directly connected to the production process, independent of whether or not the good is actually traded. This implies that workers cannot be used as a "hostage", as they receive their wage irrespective of the good being sold. Consumers can only honor or punish the managers with their purchase decisions.

3.3 The market model

In our experiment we study a simple market environment. Firms offer a good on the market, and consumers may purchase at most one unit of the good. Each firm has one manager and one worker. Each manager determines the wage $w \in \{0, 1, \ldots, 30\}$ of her worker and the price $p \in \{0, 1, \ldots, 30\}$ of the good. Managers can neither condition the wage on sales nor can they price-discriminate between

consumers. The worker receives the wage and produces the units of the good at zero costs. Consumers' valuation of the good is 30. Thus, trade generates a surplus of 30 and payoffs are:

$$\Pi_{Manager} = -w + p \cdot \text{number of units sold} \tag{3.1}$$

$$\Pi_{Worker} = +w \tag{3.2}$$

$$\Pi_{Consumer} = \begin{cases} 30 - p & \text{if consumer buys one unit of the good} \\ 0 & \text{if consumer does not buy the good} \end{cases} \tag{3.3}$$

The wage paid to the worker will be our proxy for SR in production. To serve our research focus we vary the competition environment as well as the information on the SR of production.

3.3.1 The competition environments

We study two competition environments. The *non-competitive market* is a bilateral monopoly, consisting of one firm and one consumer. The *competitive market* is a Bertrand duopoly with two firms and two consumers, where each firm may serve both consumers. First, both firms decide simultaneously (on wage and price) and then consumers decide simultaneously on whether, and if so at which firm to buy. Focusing on these two conditions seems appealing both from a theoretical and an applied perspective. Under standard preferences these two environments yield extreme predictions: While in monopoly the firms receive the entire gains from trade, the consumers have the market power in duopoly. In practice, some of the goods in the focus of SR production (like trendy smart phones or fashionable sneakers) seem to be produced in monopoly-like situations while other products (like coffee or basic shirts) seem to be produced in Bertrand-like markets. Moreover, the consequences resulting from "punishing" a non SR-producer are different for the consumer. While in monopoly the consumer can only punish by not buying at all resulting in zero payoff, in duopoly the consumer may just buy at the other firm.

3.3.2 The information on the SR of production

In addition to varying the competitiveness of the market environment, we vary the observability of the social responsibility of production (i.e., the worker's wage) in the following way:

In treatment *Baseline* (*No Info*) the worker's wage is private information of the manager and the worker of the firm. Neither the consumer nor the other firm (in duopoly) are or can be informed on the wage.

In *Full Info* the consumers are fully informed about the wage(s) when making the buying decision. In the other conditions information is subject to choice and in two conditions it is even imperfect.

In *Choice*, the consumer is a priori not informed about the wage(s), but may acquire this information at no cost. This condition mirrors the situation of uninformed consumers who may surf the web for details about the firms' SR policies or may decide to remain ignorant. Firms are not informed whether or not consumers acquire information.

In *Label* the firm decides whether or not to acquire a label (in duopoly both firms decide simultaneously) prior to setting the wage. A firm acquiring a label agrees to pay an exogenously defined minimum wage of $w_{\min}$ to the worker. The consumers receive the information whether or not a firm has a label together with the firm's price for the good. Thus the label signals that the firm is following a minimum requirement, but does not reveal the exact wage of the worker. This condition mirrors the case of SR labels. To reflect the costs of certification, acquiring a label incurs costs of c_{label} for the firm.[4]

Finally, in treatment *Face* each worker communicates her satisfaction with her wage on a five-point scale by sending a face to the consumer(s) (see Figure 3.1). The consumers receive this information together with the price for the good. This condition mirrors cases in which workers may send imperfect signals, for example, on their poor working conditions. Firms are also informed about the workers' signals.

[4] In the experiment the minimum wage was set to 4 and the cost for acquiring a label was set to 1. It was determined such that the total cost of 5 for a label firm was (slightly) higher than the average voluntary wage payment observed in the no competition baseline treatment (which is 4.7).

Figure 3.1: Scale of worker satisfaction

3.4 Experimental implementation

The orthogonal variation of the two competition and the five information conditions establishes our ten experimental treatments. To account for learning effects, the stage-game market is repeated for 30 periods. Every market consists of fixed groups of three (no competition) or six (competition) subjects. Roles and firm composition remain identical throughout the entire experiment. In the duopoly market, firms are distinguished by assigned letters A and B and consumers by assigned letters X and Y. Table 3.1 provides an overview over the number of independent observations and the number of subjects in each treatment. We strived for ten independent observations in each treatment. Due to no-shows we collected only nine in some treatments. For manager competition in Label, we decided to double the number of independent observations to allow for a sufficient number of observations for the endogenously occurring choices for having or not having a label. In total 492 subjects took part in the experiment.

At the end of the experiment subjects answered a questionnaire concerning their attitude towards SR.[5] The main body of the questionnaire uses questions of the Eurobarometer 47.0 (Melich, 2000). We complemented those questions with specific questions concerning our experimental setup. The different sessions of the experiment were conducted between November 2012 and February 2014 at the Cologne Laboratory for Economic Research (CLER). Interaction was computerized using the software z-Tree (Fischbacher, 2007). Participants were recruited with ORSEE (Greiner, 2004). At the beginning of the experiment written instructions[6] were distributed and read aloud. Sessions lasted between 75 and 105 minutes. Subjects received an initial endowment of 40 points and additional 5 points at the beginning

[5] See Section 3.8.3 for the questions asked and the statistical analysis of the answers.
[6] English translations can be found in Section 3.8.4.

Table 3.1: Number of independent observations (and subjects) in each treatment

	Baseline	**Full Info**	**Label**	**Choice**	**Face**
No Competition	9 (27)	9 (27)	10 (30)	10 (30)	10 (30)
Competition	9 (54)	9 (54)	20 (120)	10 (60)	10 (60)

of every round.[7] After the experiment all points were converted into € and paid in cash with an exchange rate of 40 points for 1€ and an additional show-up fee of 2.50 €. Average total earnings are 13.29€ in the monopoly (with a minimum of 6.03€ and a maximum of 22.23€) and 14.61€ in the duopoly sessions (with a minimum of 3.53€ and a maximum of 28.25€).

3.5 Theoretical considerations on CSR in our experimental game

3.5.1 Stage game equilibria under selfish preferences

Without competition, the stage game is strategically equivalent to an ultimatum game with a bystander. By setting wage and price, the manger makes a proposal (w, p) how to divide the gains of trade (i.e., 30) between the three players. When the consumer accepts (buys), the 30 points are divided as follows: the manager gets $p - w$, the worker gets w and the consumer gets $30 - p$. When the consumer rejects, the manager receives $-w$, the worker w and the consumer receives 0. Under common knowledge of selfish preferences, a profit maximizing consumer just considers p, since the wage w does not influence her profit. The consumer accepts any (w, p) with $p < 30$ and is indifferent between accepting and rejecting $p = 30$. The manager's best response to the consumer's behavior is to charge the highest price that is accepted by the consumer and to pay a wage of zero. Thus, we derive two subgame perfect Nash equilibria in pure strategies: 1. the manager proposes $(w = 0, p = 30)$ and the consumer accepts all manager proposals; 2. the

[7] The initial endowment and the roundly endowment could cover potential losses which are possible for subjects in the roles of managers (from not trading or trading at prices lower than costs). It never happened that a subject had a negative account in one point of time.

manager proposes ($w = 0, p = 30 - \varepsilon$) and the consumer accepts all offers with $p = 30 - \varepsilon$, where $\varepsilon > 0$ is the smallest money unit (which is 1 in our experimental setting). Hence, the manager has the market power to receive (almost) the entire rent.

Under competition each firm may serve both consumers. When prices differ, selfish consumers buy the cheaper good. If both firms set the same price, assume that consumers randomly choose where to buy. Again, selfish consumers do not care for wages, so that selfish managers pay zero wages. Then there is a subgame perfect Nash equilibrium with $p = w = 0$ for both firms and consumers that always buy. This results in zero profits for managers and workers and the entire market surplus going to consumers. Since our design only allows for integer prices, there are two additional subgame perfect equilibria, one where both firms set ($w = 0, p = 1$) and another where both firms set ($w = 0, p = 2$).[8]

Hypothesis 3.1 (Selfish preferences)**.**
No competition: *With selfish preferences the manager pays zero wage, sets the maximal price and the consumer buys. The manager earns the entire market rent while the worker and the consumer make zero profits.* Competition: *Under selfish preferences managers pay zero wages, set the minimal price and the consumers buy the cheaper good. The consumers earn all the market rent while managers and workers make zero profits.*

In the experiment we repeat the stage game for 30 periods. Under common knowledge of selfish preferences this does not change the results. Backward induction predicts equilibrium behavior in every period.

Related experiments that also use a market framing (Bartling et al., 2015; Rode et al., 2008; Etilé & Teyssier, 2012; Feicht et al., 2014) have shown that subjects' allocation behavior is not as extreme as expected under common knowledge of selfish preferences and that deviations may be explained by social preferences. The next section will discuss the predictions of prominent social preference theories for our setups.

[8] $p = 1$ occurs in an equilibrium, since any deviation (decreasing the price to zero or increasing the price) would reduce the manager's profit from one to zero. If both managers charge a price of $p = 2$ they receive expected profits of 2. A price increase would lead to zero profits and a decrease to $p = 1$ would keep the deviating manager a profit of two.

3.5.2 Stage game equilibria under non-selfish preferences

Devinney et al. (2006) understand consumer social responsibility as "one component of the complex consumer decision-making process" and define it as "the conscious and deliberate choice to make certain consumption choices based on personal and moral beliefs" (p. 32). According to their definition, it may show up as the "expressed activity in terms of purchasing or non-purchasing behavior" (p. 32). In our simple experimental model, we strongly reduced the complexity of the consumer decision-making process. In case of monopoly, the consumer's purchasing decision determines the payoff allocation between the three players. For manager determined (w, p), the consumer decides between $(w, p - w, 30 - p)$ (in case of buying) and $(w, -w, 0)$ (in case of not buying) as payoffs to the worker, the manager, and the consumer, respectively.[9] This decision situation is close to the Güth and van Damme (1998) ultimatum game with a bystander, however, with the difference that in their game a rejection leads to zero payoffs for all three players. Güth and van Damme observe very low bystander payoffs, low rejection rates and no single rejection that can be attributed to a low bystander share.

(Bolton & Ockenfels, 1998) show that the low bystander payoffs in Güth and van Damme are in line with the inequality aversion model ERC, introduced in Bolton and Ockenfels (2000). This model assumes that individuals are motivated by their absolute and their relative monetary payoff within the group. Applied to our situation, the ERC model would predict the worker's wage not to be relevant for the consumer's purchasing decision, since solely the price determines the consumer's absolute as well as relative payoff. The wage just distributes payoff between manager and worker, but changes neither the absolute nor the relative payoff of the consumer.

Another prominent model of allocative preferences is the inequity aversion model by Fehr and Schmidt (1999). In this model, a player receives utility from her monetary payoff and disutility from advantageous as well as disadvantageous payoff differences to each of the other players. In particular, this means that the consumer compares her payoff also to the worker and may gain disutility from too

[9] Notice, that the worker's wage is not influenced by the purchasing decision. This is meant to reflect the fact that the good is already produced and an immediate benefit to the worker cannot be the reason for acceptance or rejection (as, for example, in Bartling et al., 2015).

large deviations between the two payoffs. In Section 3.8.2, we calculate the stage game equilibria depending on different levels of inequity aversion. We show, that for low inequity averse consumers, Fehr-Schmidt equilibria still specify a wage of zero. However, if consumers are highly inequity averse, positive wages occur in equilibrium, even when the manger is selfish.

Consumer social responsibility may not only come as distributional preferences between the participants. The consumer's "personal and moral beliefs" may also address more general aspects, like an aversion to having certain members of society with very low payoffs or a concern for the overall payoff of all participants. Charness and Rabin (2002) include these concerns in their model of quasi-maximin preferences, which describes that a participant is motivated by the own monetary payoff, but also by the lowest payoff in the group and the sum of all payoffs. When facing a given proposal (w, p), a consumer would never reject, since this would result in a personal payoff of zero, a minimum payoff of $-w$ and a joint payoff of zero. Thus, rejecting would yield an overall negative value, while accepting a given proposal would lead to a positive utility. Hence, a consumer with quasi-maximin preferences would never reject an offer, irrespective of the wage.

With competition, the strategic situation gains complexity. Nonetheless, consumers with inequality aversion as modelled in ERC (Bolton & Ockenfels, 2000) would only be concerned about the price and not the workers' wage, as the wage does not influence the consumer's relative payoff standing. Consumers with Fehr-Schmidt preferences in contrast take the wage into account. They are concerned about inequity not only to the workers, but to all market participants, including the managers and the other consumer. In duopoly, inequity averse consumers predominantly buy at the cheaper firm, but it may also occur that they buy at the more expensive firm (see Section 3.8.2 for a more detailed discussion). However, in that case the motivation is not only the worker's wage, but also inequity concerning the manager of the more expensive firm, who would make a loss of $-w$ if both consumers buy at the cheaper firm. Similarly, consumers with quasi-maximin preferences (Charness & Rabin, 2002) may accept the more expensive offer, driven by a concern for the manager of the more expensive firm and not by a concern for the worker. A manager's loss of $-w$, which is the minimal payoff among all

participants in case both consumers buy at the other firm, strongly reduces the consumer's utility.

Hypothesis 3.2 (Non-selfish preferences)**.**
No competition: *Without competition, models of other-regarding preferences leave only little room for consumers rejecting offers due to too low wages. While the models of Bolton and Ockenfels (2000) and Charness and Rabin (2002) exclude refusals to buy due to too low wages, consumers with Fehr and Schmidt (1999) preferences may forego "extreme" allocations, in particular those with a too high price that do not come with a high wage, only if they are highly inequity averse.*
Competition: *With competition, the situation changes in the Fehr and Schmidt (1999) and Charness and Rabin (2002) models. Consumers may indeed buy the more expensive good, however, they are not only motivated by concerns for the workers, but also for the managers.*

The considerations leading to hypotheses 3.1 and 3.2 have shown that in our stage game we cannot expect consumer social responsibility in the sense of banning products with low wages on large grounds. In the experiment, we repeat the stage game for 30 periods. SR consumers, who aim at positively influencing the wage in the longer run, may accept instantaneous losses in payoff or utility to achieve their long-term goal. In the repeated setting, managers may learn the demanded level of SR and consumers may signal their preferences through their consumption behavior. Obviously, this signaling and learning process may depend on the precision of the provided wage information, discussed in the next section.

3.5.3 Information treatment effects

In the analyses so far, we have focused on full wage information. An important aspect of our study is that the treatments vary the wage information, inspired by market settings in which CSR is an issue. In this section, we discuss how these variations in information may influence behavior.

In the Baseline treatment the price of the good is the only available characteristic. Managers know that consumers cannot condition their purchase on wages and thus do not condition their wage decision on potential consumer preferences.

Consequently, we expect wages to be rather low. Hence, any observed positive wages in Baseline should be attributed to inequity aversion, altruism or warm glow (Andreoni, 1989) of managers rather than SR concerns of consumers.

In all other treatments, more (potentially implicit) information is available. Consumers intending to include SR information in their consumption decision should value any piece of information they can achieve about the worker's wage. In Full Information, SR consumers may not only base their consumption decision on the price but also on the worker's wage. Anticipating this, managers will pay higher wages to workers than in Baseline.

In the *Choice* condition, a consumer may acquire the wage information at no cost. A consumer who includes workers' wage info into her consumption decision acquires this information. However, it has to be expected that not all consumers choose to acquire information. Recent experimental findings show that deliberate ignorance occurs in different environments (for example, Conrads & Irlenbusch, 2013; Dana et al., 2007; Grossman & van der Weele, 2013) and suggests that consumers might prefer to not learn the wages. Managers anticipating this pay higher wages than in *Baseline*, however, consumers' possibility of remaining ignorant may lead to lower wages than in *Full Info*.

In *Label* a manager who is intrinsically motivated to pay a wage of at least 4 may do so without acquiring a label. However, the label provides the opportunity of credibly signaling SR production. The signal is particularly strong, as the exogenously determined level "certifies" a sufficient SR level and thus removes any uncertainty of what is an appropriate wage level, which may be present in *Full Info*. If managers expect SR consumers to include the fact whether or not the firm has a label into the consumption decision, they will acquire a label to increase their expected revenue if the fraction of SR consumers is sufficiently high. Thus in *Label* we expect wages to be higher than in *Baseline* and not lower than in *Full Info*.

In *Face* workers send signals about their satisfaction with the wage. Consumers know that these signals do not have to correlate with the actual wage and it may be that workers (mis-)use the signal to achieve an "excessive" wage. Experimental findings show however, that subjects can be quite trustworthy even in situations where they can manipulate such information (for example, Gneezy, 2005; Cai &

Wang, 2006). Yet, since there is no objective scale to convert wage into satisfaction level and vice versa, the satisfaction level might be a weaker SR indicator than the wage in *Full Info*. Managers anticipate this and pay wages lower than in *Full Info*.

Hypothesis 3.3 (Information treatment effects)**.** *Wages are highest in Full Info and lowest in Baseline. Wages in Label are higher than in Baseline and not lower than in Full Info. Wages in Choice and Face are lower than in Full Info.*

3.6 Results

This section presents the results of our experiment. In Section 3.6.1 we analyze the treatment effects on wages, our proxy for SR production. We study consumers' buying decisions in Section 3.6.2 to ask whether consumer behavior makes SR production profitable for firms in Section 3.6.3. In Section 3.6.4, we connect subjects' behavior in the experiment to their willingness to pay for SR products stated in the questionnaire. In what follows all comparisons between treatments use the Mann-Whitney u-test (MWT) and all comparisons within treatments use the Wilcoxon signed-rank test (WSR) on the basis of the independent observations (see Table 3.1), both two-sided. Table 3.2 provides an overview over the aggregated market outcomes.

3.6.1 Treatment effects on SR production

On average managers pay positive wages in each information treatment of both competition conditions. Although in the no-competition treatments the average wage in Baseline (4.70) is lower than the average wages in Full Info (7.39), Choice (5.55) and Face (6.96), they are not statistically different in non-parametric tests (see Table 3.11 in in Section 3.8.1). Only in Label (2.39) the average wage is significantly lower than in Baseline and any other treatment.[10] How can we explain

[10] In *Label*, there is an extra cost whenever the manager buys the label, which makes *Label* different to all other treatments. Including the cost of buying the label, does not change results. The average wage cost (i.e., wage plus label acquisition cost) of 2.81 in Label is still significantly lower than the average wages of the other treatments.

Table 3.2: Aggregated market outcomes

No Competition	**Baseline**	**Full Info**	**Choice**	**Label**	**Face**
Wage paid	4.70 (1.02)	7.39 (1.49)	5.55 (0.60)	2.39 (0.66)	6.96 (1.05)
Wage in accepted offers	4.69 (1.11)	7.25 (1.47)	5.52 (0.65)	2.38 (0.68)	6.70 (1.10)
Wage in rejected offers	6.59 (1.66)	8.94 (2.07)	5.95 (0.67)	2.34 (0.62)	8.03 (0.92)
Price offered	19.21 (0.61)	19.27 (1.36)	19.29 (1.03)	18.37 (0.82)	19.39 (0.78)
Price in accepted offers	18.71 (0.69)	18.88 (1.40)	18.62 (1.12)	17.86 (0.88)	18.89 (0.79)
Price in rejected offers	23.46 (1.07)	22.02 (1.09)	20.92 (0.77)	20.49 (0.99)	21.73 (0.81)
Units sold per firm	0.89 (0.02)	0.82 (0.05)	0.74 (0.04)	0.79 (0.03)	0.81 (0.04)
Payoff Manager	11.96 (1.58)	7.96 (1.41)	8.38 (1.44)	11.37 (1.33)	8.31 (1.03)
Payoff Consumer	10.00 (0.61)	9.31 (1.43)	8.37 (0.88)	9.42 (0.60)	9.03 (0.78)
Competition	**Baseline**	**Full Info**	**Choice**	**Label**	**Face**
Wage paid	2.86 (0.38)	5.34 (0.92)	5.46 (0.59)	3.29 (0.46)	6.06 (0.71)
Wage in accepted offers	2.71 (0.43)	5.48 (0.99)	5.65 (0.62)	3.41 (0.50)	6.13 (0.71)
Wage in rejected offers	3.06 (0.40)	5.18 (0.83)	5.16 (0.60)	3.12 (0.43)	5.94 (0.71)
Price offered	11.12 (1.20)	12.41 (1.32)	9.89 (1.37)	11.04 (0.74)	8.08 (0.99)
Price in accepted offers	9.45 (1.23)	11.12 (1.63)	9.28 (1.37)	9.95 (0.71)	7.27 (0.94)
Price in rejected offers	13.02 (1.19)	14.49 (0.93)	11.04 (1.47)	12.72 (0.86)	9.36 (1.02)
Units sold per firm	0.97 (0.02)	0.99 (0.01)	0.99 (0.01)	0.99 (0.01)	1.00 (0.00)
Payoff Manager	6.13 (1.27)	5.56 (1.27)	3.66 (1.22)	5.79 (0.54)	1.27 (0.82)
Payoff Consumer	20.02 (1.31)	18.82 (1.61)	20.68 (1.36)	19.95 (0.74)	22.62 (0.94)

Notes: The table reports averages and standard errors (in parentheses) based on independent observations (see Table 5.1). Rejected offers are offers where no consumer buys and accepted offers are those where at least one consumer buys. A more detailed overview over prices and payoffs is provided in Figure 3.5 and the Tables 3.10 and 3.12-3.14 in Section 3.8.1.

this difference in the Label treatment? Mangers choose the label in 42.0% of the cases. The possibility of acquiring a label seems to strongly separate managers into two groups: Managers who acquire a label pay a wage of exactly 4 in 68.3% of the cases (average wage of 4.57), while managers without a label pay a wage of 0 in 78.7% of the cases (average wage of 0.81) (see Figure 3.3 in Section 3.8.1). These wages are significantly different (p=0.036). Thus, the label seems to provide an anchor, signaling the "appropriate" wage and seems to crowd out any voluntary payment exceeding this benchmark. Managers not acquiring a label seem to feel "licensed" to pay nothing at all. The observed effect demonstrates a potential detrimental effect of minimum wages.

Figure 3.2 shows the development of the average wages over time and demonstrates that the wages in the no competition treatments (except for *Choice*) slightly

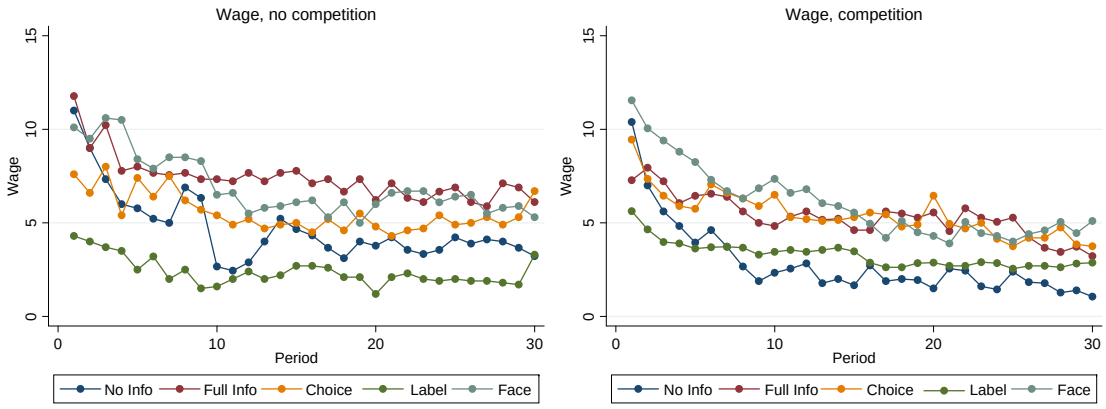


Figure 3.2: Average wages per treatment

decrease over time (left panel).[11] We also find a negative time trend in average wages under competition (right panel).[12] To account for these time trends, we will control for "period" in the later analyses.

With competition wages vary across treatments: The average wages are highest in *Face* (6.06), *Choice* (5.46) and *Full Info* (5.34), significantly higher than in *Baseline* (2.86) and *Label* (3.29) (see Table 3.11 in Section 3.8.1).[13] In *Label* we observe the same effect as in the no-competition setting. Managers choose a label in 57.6% of the cases. After acquiring the label, managers pay a wage of 4 in 82.8% of the cases (average wage of 4.56) and without a label managers pay a wage of 0 in 62.7% of the cases (average wage of 1.55, see Figure 3.4 in Section 3.8.1). Wages without a label are significantly lower than with a label (p=0.001).

The positive wage effect observed in Face is remarkable (compare Hypothesis 3.3). Although vulnerable for manipulations, the stated satisfaction level highly correlates to the wage (no competition: Spearman's rho = 0.642, p<0.001; competition: Spearman's rho = 0.673, p<0.001). It seems that the direct communication

[11] Random-effects GLS regression of wage on period with robust standard errors clustered by manager id, including a coefficient for period: *Baseline*: -0.143 (p=0.008), *Full Info*: -0.104 (p=0.012), *Choice*: -0.067 (p=0.260), *Label*: -0.047 (p=0.036), *Face*: -0.142 (p=0.054).

[12] Random-effects GLS regression of wage on period with robust standard errors clustered by manager id, coefficient for period: *Baseline*: -0.166 (p=0.000), *Full Info*: -0.105 (p=0.021), *Choice*: -0.116 (p=0.000), *Label*: -0.065 (p=0.001), *Face*: -0.192 (p=0.000).

[13] Again, adding the costs of acquiring a label does not change the results. The average wage costs of 3.86 in *Label* is also not significantly different from the average wage in *Baseline*, but significantly lower than the average wages of the other treatments.

Table 3.3: Treatment effects on wage

Competition	-1.848^{*}
	(1.048)
Full Info	2.689
	(1.709)
Choice	0.850
	(1.124)
Label	-2.314^{**}
	(1.150)
Face	2.256
	(1.388)
Competition × Full Info	−0.209
	(1.871)
Competition × Choice	1.753
	(1.283)
Competition × Label	2.745^{**}
	(1.296)
Competition × Face	0.948
	(1.590)
Period	-0.112^{***}
	(0.013)
Constant	6.445^{***}
	(0.991)
n	4920
Wald chi^2	60.06
R^2	0.1294

Notes: Random-effects GLS regression. Robust standard errors in parentheses, clustered by manager id: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Dependent variable: Wage.

between workers and consumers reduces the social distance (Hoffman, McCabe, & Smith, 1996; Bohnet & Frey, 1999; Rankin, 2006; Charness & Gneezy, 2008) and triggers social concerns of the consumers for the workers (van Dijk & van Winden, 1997; Malmendier & Schmidt, 2012), which results in high wages for the workers. Moreover, explicitly showing the satisfaction may reduce the uncertainty of whether or not the wage is acceptable for the worker and thus reduces consumers' "moral wiggle room" (Dana et al., 2007).

Wages in the competition settings are not significantly different from the respective no-competition setting (see Table 3.11 in Section 3.8.1). In Table 3.3, we present a random-effects GLS regression using factorial interactions between the competition and information conditions. The coefficient "Competition" shows that competition has a weakly significant negative effect on wages in Baseline (not

identified in the non-parametric tests in 3.11 in Section 3.8.1). The next four coefficients capture the effects of the four information treatments in no competition. Here, we find a significantly lower wage in *Label*. The interaction terms capture the competition effect in the different treatments compared to the competition effect *Baseline*.[14] While there is no significant effect in *Full Info*, *Choice* and *Face*, there is a significantly positive effect for *Label*. The wage in *Label* is higher under competition than under no competition.

Result 3.1. *Without competition there are no information treatment effects on wages, except the negative effect in Label. However, with competition information significantly increases wages: In Full Info, Choice, and Face wages are significantly higher wages than in Baseline.*

3.6.2 Consumers' decision to buy

The no-competition condition

Without competition, consumers have only two choices: accept the monopolist's offer (buy) and reject (do not buy). Consumers buy in roughly 80% of the cases (see Units sold per firm in Table 3.2). When do consumers refuse to buy? In each of the information treatments, the prices of the accepted offers are significantly lower than the prices of the rejected offers (see Table 3.2).[15] Except for Baseline, consumers may base their consumption decision not only on the price but also on the wage information they have. The regression presented in Table 3.4 tests how the different attributes of a good affect the consumers' propensity to buy. We use a conditional fixed-effects logistic regression to meet the panel structure of the data. The dependent variable is Buy, which equals 1 when the consumer accepts a monopolist's offer and 0 otherwise. Consumers may reject an offer to signal their dissatisfaction, for example, with the price being too high. Since the value of such a signal might be lower in later periods, we add the variable period to control for time effects.

[14] That means the average absolute wage decrease between competition and no-competition in the information treatments is compared to the average absolute wage decrease between competition and no-competition in *Baseline*.

[15] WSR test on the difference between prices of accepted and rejected offers: *Baseline* p=0.012, *Full Info* p=0.018, *Choice* p=0.017, *Label* p=0.005, *Face* p=0.005.

Table 3.4: Consumers' propensity to buy, no competition

	Baseline	**Full Info**	**Choice**	**Label**	**Face**
Price	−0.640*	−0.553***	−0.414***	−0.270***	−1.390***
	(0.348)	(0.135)	(0.159)	(0.062)	(0.348)
Wage		0.208	0.141		
		(0.139)	(0.227)		
Label				1.303	
				(0.827)	
Satisfaction					0.354**
					(0.178)
Period	0.022	0.040	−0.003	0.030	0.017
	(0.033)	(0.044)	(0.057)	(0.039)	(0.043)
n	240	210	300	300	300
log pseudol	-47.60	-69.71	-120.84	-109.46	-64.94
Wald chi^2	4.35	91.92	13.66	22.82	38.22

Notes: Conditional fixed-effects logistic regression. Robust standard errors in parentheses, clustered by consumer id: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Dependent variable: Wage. Dependent variable: Buy. In *Choice*, we include all cases, independent of whether or not the consumer revealed the wage to keep it comparable to *Full Info* where we cannot control whether or not the consumer actually looked at the wage.

The regression shows the strong negative effect of the price on the consumers' propensity to buy. Strikingly, in *Full Info*, when consumers are perfectly informed about the worker's wage, the paid wage does not influence their purchasing decision. Also in *Choice* the paid wage does not influence the consumers' buying decision. The lacking influence of the label choice in Label is also reflected in the non-parametric analyses. In the *Label* treatment there is no difference in the label choices for accepted and rejected offers (44.3% accepted vs. 38.2% rejected, WSR: p=0.797) and no difference in the average acceptance rate between offers with label and offers without label (69.9% with label vs. 61.5% without label, WSR: p=0.779). Only in treatment *Face* the stated satisfaction does influence consumers, although there is no significant difference in satisfaction between accepted and rejected offers (satisfaction in accepted offers = 2.89, satisfaction in rejected offers = 2.53, WSR: p=0.285).

Result 3.2. *Without competition, we observe positive wages, in contrast to the predictions by purely selfish preferences (compare Hypothesis 3.1). However, the positive wages cannot be attributed to consumer behavior. As suggested by ERC (compare Hypothesis 3.2), consumers predominantly care for the price. There are no indications, except for treatment* Face*, that SR (wage or wage indicators) affects*

consumers' decisions. Rejected offers are due to high prices and not due to low wages.

The competition condition

In duopoly consumers may or may not buy, and when they buy, they can choose between two potentially different offers. In contrast to the monopoly, it almost never happens that consumers do not buy at all: they buy in about 99% of the cases (see Table 3.2). As in the cases of no competition, in each of the information treatments the prices of the accepted offers are considerably and significantly lower than the prices of the rejected offers (see Table 3.2).[16] To investigate whether and if so how consumers trade off their own payoff against the worker's wage in their purchasing decisions we report an alternative-specific conditional logit model (McFadden, 1974) for the cases in which consumers buy (see Table 3.5). The choice model reflects the specific situation that consumers accept one of two potentially different offers. We specify a case as a single decision of a single consumer in a period. Each case consists of two single observations (alternatives) which are the two firms' offers. Alternative-specific variables are the attributes of an offer, i.e., price and SR information. The dependent variable is *Buy*, which equals one for the accepted offer. As a case-specific variable, we include *Period*, which is not significant.[17]

The regressions in Table 3.5 show that in *Baseline* as well as in all information conditions the price has a highly significant negative influence. In all information treatments the paid wage (in *Full Info* and *Choice*[18]) or the imperfect information on the wage (in *Label* and *Face*) has a significant positive influence on consumer choices. However, in *Full Info* and *Choice* the negative effect of the price is by far stronger than the positive effect of the wage. In *Label*, having a label has a highly significant positive effect on consumers' propensity to accept an offer. In *Face*

[16] WSR test on the difference between prices of accepted and rejected offers: *Baseline* p=0.008, *Full Info* p=0.021, *Choice* p=0.0005, *Label* p<0.001, *Face* p=0.005.

[17] *Period* and the constant term are insignificant in all treatments. This means that there is no propensity to prefer the first alternative (firm A) to the second (firm B) in general or over time.

[18] In *Choice* consumers disclose at least one wage in 95.8% of the cases and disclose both wages in 95.0% of the cases.

Table 3.5: Consumers' buying decision, competition

	Baseline	**Full Info**	**Choice**	**Label**	**Face**
Price	−1.707***	−0.751***	−0.605***	−0.488**	−0.541***
	(0.529)	(0.229)	(0.129)	(0.200)	(0.129)
Wage		0.298***	0.395***		
		(0.093)	(0.066)		
Label				1.376***	
				(0.524)	
Satisfaction					0.584***
					(0.129)
n	1044	1070	1132	2368	1198
cases	522	535	566	1184	599
log pseudol	-139.50	-261.92	-298.13	-589.91	-302.13
Wald chi^2	15.96	13.57	59.63	10.85	38.64

Notes: Alternative-specific conditional logit model (McFadden's choice model) for the cases in which consumers buy. Robust standard errors in parentheses, clustered by consumer id: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Dependent variable: *Buy*. Case-specific variables (not reported): Period. In *Choice* only cases where consumer is fully informed, because only in these cases the consumer can compare both offers.

workers' satisfaction influences consumers' choice positively. Since price, label and satisfaction are on different scales, they are not directly comparable.

Table 3.6 indicates how subjects trade off the price against worker's wage when the prices of the two firms differ. In all treatments consumers predominantly buy at the firm with the lower price. These firms pay (an about 4) lower wage. Consumers rarely buy at the firm with the higher price. But if they do, they predominantly buy at the firm that pays the higher wage, and the wage difference is about 2.5. If both firms set the same price, the majority of consumers buy at the firm with the higher wage. Thus, it seems that consumers follow a lexicographic decision rule: The first priority is to buy cheap, but if prices do not differ too much, the worker's wage guides the decision.

Result 3.3. *With competition, wages are positive (in contrast to Hypothesis 3.1). The price has a significant negative influence and the wage information has a significant positive influence on consumers' buying decisions. Consumers buy the more expensive good if it has a higher level of SR and the mark-up is not too high. This behavior is in contrast to the predictions by ERC (Bolton & Ockenfels, 2000), but not with the predictions by Fehr and Schmidt (1999) and Charness and Rabin (2002), compare Hypothesis 3.2.*

Table 3.6: Consumer choices on competition markets with different prices

	Buying at low price firm (LF)			**Buying at high price firm (HF)**		
		LF higher SR	**Avg. price diff**		**HF higher SR**	**Avg. price diff**
Baseline	96.8%		4.06	3.2%		2.07
Full Info	82.8%	28.6%	4.82	17.2%	94.2%	2.43
Choice	77.0%	31.6%	3.40	23.0%	92.9%	2.89
Label	82.7%	16.0%	4.38	17.3%	70.5%	3.04
Face	72.2%	30.6%	3.39	27.8%	78.7%	1.99

Notes: The table shows consumer choices on markets with different prices: how often do they buy at the cheap or expensive firm, how often is this firm better in terms of social responsibility and the price difference to the other firm.

3.6.3 Is socially responsible production profitable?

The study of both market sides, consumers and producers, allows for explicitly focusing on the drawbacks of consumer decisions on producers and analyzing whether consumer behavior makes SR production profitable. First, consider the monopoly case. The manager's profit is $\Pi_{Manager} = -w + p \cdot sales$. The regression presented in Table 3.4 already showed that consumers' purchasing decisions (*sales*) are – except for treatment *Face* – not influenced by the wage paid. Thus, the results presented in Table 3.7 come as no surprise. The regression's dependent variable is manager's *sales* with the manager's strategic parameters *price*, *wage*, and *label* acquisition as independent variables. Since increasing the wage does not increase the probability of selling the good, the wage is just a production cost for the manager and therefore decreases profits, except for *Face*. Although, in *Face*, the stated satisfaction has a positive effect on consumers' propensity to buy, it does not increase manager's profit. A regression shows that the higher wage costs to increase satisfaction are not covered by the increased propensity to sell.[19]

For duopoly, Table 3.5 has shown that SR positively influences consumers' purchase decisions in all information conditions. The question is whether this suffices to make SR production profitable for managers. Since the consumers in the competition condition almost always buy and select one of the two offers, not the absolute level of prices and SR but the differences between the two firms do de-

[19] Fixed-effects (within) regression of manager profit with price, wage and period as independent variables: price (-1.430***), wage (-0.937**), period (0.198***) and constant (39.496***).

Table 3.7: Manager's sales, no competition

	Full Info	**Choice**	**Label**	**Face**
Price	−0.553***	−0.414***	−0.280***	−1.367***
	(0.135)	(0.159)	(0.085)	(0.340)
Wage	0.208	0.141		0.061**
	(0.139)	(0.227)		(0.026)
Label			1.201	
			(2.380)	
Wage with label			0.106	
			(0.178)	
Wage without label			0.073	
			(0.357)	
Period	0.040	−0.003	0.033	0.034
	(0.044)	(0.057)	(0.042)	(0.041)
n	210	300	300	300
log pseudol	-69.71	-120.84	-109.46	-65.54
Wald chi^2	91.92	13.66	181.71	58.11

Notes: Conditional fixed-effects logistic regression. Robust standard errors in parentheses, clustered by consumer id: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Dependent variable: *Sales*.

termine consumers' choices. Nine possible constellations of product differentiation on the duopoly market are possible: A firm may be lower, equal or higher in price than the competitor and a firm may be lower, equal or higher in SR than the competitor. Table 3.15 in Section 3.8.1 shows the average sales and the resulting payoff for the information treatments in duopoly.

In Table 3.8, we estimate the effects on the manager's payoff using a linear panel regression where the categories of price and SR differentiation are interacted as independent variables. The base category is the case of two homogeneous goods: both mangers offer the same price and the same level of SR. All other categories represent a deviation from that case. The first line shows that offering a product with the same level of SR than the competitor, but at a lower price significantly increases the manager's profit in all treatments. Not surprisingly, a higher price at the same level of SR as the competitor decreases the manager's profit (second line). The last three categories represent a positive product differentiation in SR. A manager can significantly increase her payoff by offering goods with a higher level of SR at a lower or equal price (compared to two identical goods). Under *Full Info*, a higher level of SR offered at a higher price significantly reduces profits. In the other treatments, when SR information is more indirect, the effect on manager

Table 3.8: Manager profit for all cases of price and SR differences, competition

	Full Info	**Choice**	**Label**	**Face**
Equal SR × Lower Price	10.507***	7.258*	7.445***	4.687***
	(2.201)	(3.638)	(1.005)	(1.432)
Equal SR × Higher Price	−9.747***	−8.156**	−8.860***	−4.527**
	(1.977)	(3.384)	(1.100)	(1.695)
Lower SR × Equal Price	−6.841***	−3.761	−3.738***	−2.738**
	(2.112)	(2.867)	(1.285)	(1.163)
Lower SR × Lower Price	4.614**	4.085	5.039***	3.377*
	(1.837)	(3.042)	(1.440)	(1.649)
Lower SR × Higher Price	−7.742**	−5.415*	−8.711***	−8.242***
	(2.973)	(3.082)	(1.194)	(1.436)
Higher SR × Equal Price	7.712***	5.401*	6.889***	4.434***
	(2.453)	(2.821)	(1.479)	(1.481)
Higher SR × Lower Price	8.211***	6.948**	9.828***	8.560***
	(2.215)	(3.300)	(1.663)	(2.153)
Higher SR × Higher Price	−5.968***	−4.124	−2.300	−1.387
	(2.032)	(3.008)	(1.570)	(1.745)
Period	−0.157*	−0.071	−0.097**	−0.035
	(0.084)	(0.055)	(0.047)	(0.055)
Constant	8.132***	4.530	7.17***	1.280
	(1.653)	(2.660)	(1.102)	(1.247)
n	540	600	1200	600
F Ratio	10.02	13.30	25.81	12.66
R^2	0.3391	0.2577	0.3654	0.2509

Notes: Fixed-effects (within) regression. Robust standard errors in parentheses, clustered by manager id: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Dependent variable: manager payoff. The regression uses two categorical variables indicating Price and SR differences: SR is lower (equal, higher) if a manager pays a lower (equal, higher) wage than the competitor in *Full Info* and *Choice*. SR is lower (equal, higher) if a manager has lower (equal, higher) satisfied worker than the competitor in *Face*. In *Label*, the SR is higher (lower) if a manager has (no) Label but the competitor does not (does) have one. The SR is equal if both firms have or do not have a label.

profit is insignificant. Thus, consumer behavior not only results in higher sales for the firm with the higher level of SR, it may also result in higher profits.

Result 3.4. *Monopolists lose profit by offering goods with increased SR, while this is not true under supplier competition. In all treatments, duopolists with the higher level of SR have significantly higher profits as long the price is not higher. If the price is higher, a higher level of SR is only detrimental under Full Info.*

3.6.4 Stated preference and actual behavior

After the experiment, subjects answered a questionnaire with various questions on SR behavior. The questionnaire and the statistical analyses of the answers are provided in detail in Section 3.8.3. The questionnaire allows us to map the stated preferences with the actual actions in the experiment on an individual level. Specifically, we may ask whether those who state that they value SR in the questionnaire actually act in such a way in the experiment. The questionnaire contains the two questions "*Would you be willing to pay an additional price for convenience goods that have been produced demonstrably under better social conditions than competing products?*" and "*Would you be willing to pay an additional price for convenience goods that are demonstrable more climate-friendly / more ecologically than competing products?*". Subjects answer each question on the following 5-point scale: 0 (I would not accept a premium.), 1 (I would accept a premium of up to 10%), 2 (... up to 20%), 3 (... up to 30%), 4 (... more than 30%). The answers to the two questions are highly correlated (Spearman's rho = 0.6742, p<0.001, $n = 492$). We take the sum of both answers as a subject's overall willingness to pay for SR (variable WTP with $0 \leq WTP \leq 8$). The regression presented in Table 3.9 considers all instances in the experiment in which a consumer may choose between two products that differ in the SR level in the duopoly markets. In *Full Info* this means that they were produced under different wages, in *Label* it means that one product is produced by a labeled firm, while the other is not, etc. The dependent variable is the share of SR-products bought, i.e. the number of cases where a consumer buys the product with the higher level of SR divided by the number of cases where the two offers differ in the SR level.

The regression shows a weakly significant positive effect of the subject's stated WTP on subject's likelihood to buy the product that was produced more socially responsible. This means that subjects' questionnaire responses are consistent with their experimental behavior: subjects who state to value SR in the questionnaire are actually more likely to act in an SR way as a participant in the experiment.

Table 3.9: Purchase of and willingness to pay for SR products

Dependent variable: Rate of buying the product with the higher level of SR	
WTP	0.034*
	(0.019)
Full Info	0.500***
	(0.067)
n	98
R^2	0.041

Notes: OLS regression. Robust standard errors in parentheses: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Dependent variable: The rate of buying the product with the higher level of SR (if products differ). All consumers of *Full Info*, *Choice*, *Label* and *Face* in competition.

3.7 Conclusion

This chapter analyzes the emergence of social responsibility (SR) in production through consumer behavior. In small laboratory economies, our experimental treatments vary the availability and opaqueness of the information on SR in the production process and the competitiveness on the supplier side of the market. We find that absent competition consumers are predominantly interested in buying cheap and do not care for the SR in production, irrespective of the information on SR. Consequently, socially responsible production significantly reduces the profit of monopolistic supplier. However, when suppliers compete, consumers take SR in production as a decision criterion, whenever the price premium for SR is not too high. Accordingly, in competitive settings SR production is no longer detrimental for the producer.

Two aspects are particularly noteworthy. The first being the strong effect of the treatment *Face*. Here workers directly communicate with the consumers by showing their satisfaction with their wage. Although this is an imperfect information transmission that is highly vulnerable to strategic manipulations it turns out that this direct communication is highly effective by directly addressing consumers' responsibility (compare van Dijk & van Winden, 1997; Malmendier & Schmidt, 2012). Our second noteworthy finding is the profit enhancing effect of SR production under competition. When consumers face a monopolistic supplier, the costs of banning goods with low SR in production is very high. By refusing to buy, they forego the gains from trade and consumers as well as managers are left with zero or negative payoffs. If however an alternative product exists, consumers can ban

non-SR products at lower personal costs. By purchasing the SR product, they may lose some fraction, but not their entire profit. In the experiment, consumers use this power. With competition, SR production significantly increases consumers' propensity to buy. In our experiment, this not only results in an increase in sales. A firm can significantly increase its profit if it offers a product with a higher level of SR than the competitor, as long the price premium is not too high. Thus, SR can be used as a profit enhancing means in product differentiation. In this aspect, our results demonstrate a positive effect of competition on fostering consumer social responsibility. This is in line with Bartling et al. (2015) and demonstrates that market interaction does not lead to more immoral behavior per se (Falk & Szech, 2013). Yet, at the same time it becomes clear that a regulatory focus on the producers seems necessary to increase the overall level of SR in production.

3.8 Appendix

3.8.1 Figures and Tables

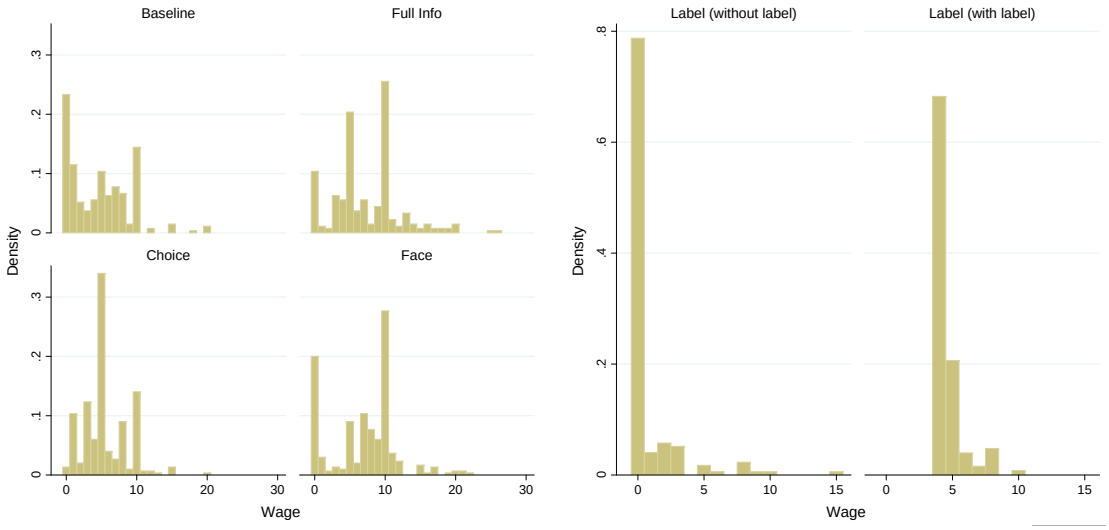


Figure 3.3: Histogram of wages, No competition

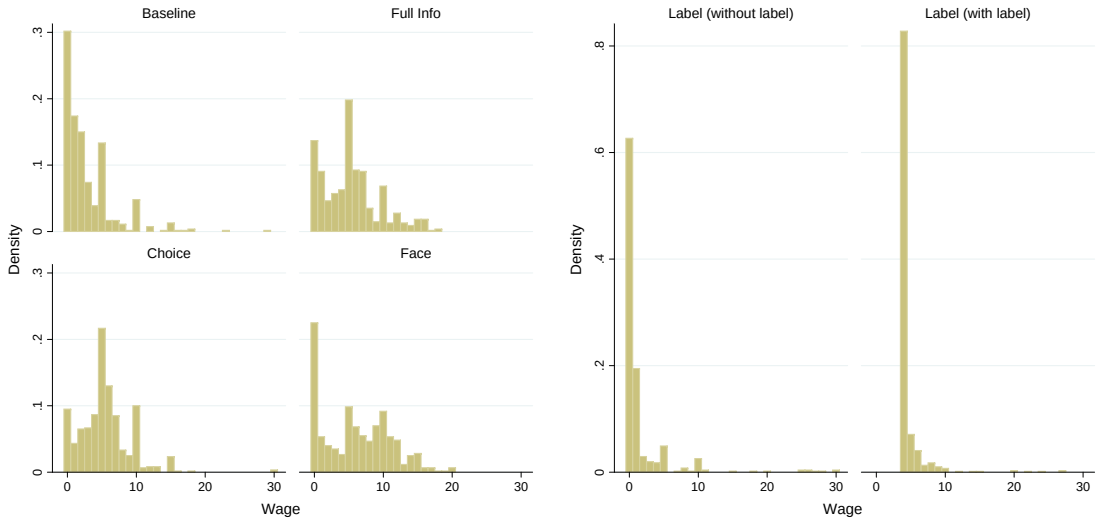


Figure 3.4: Histogram of wages, Competition

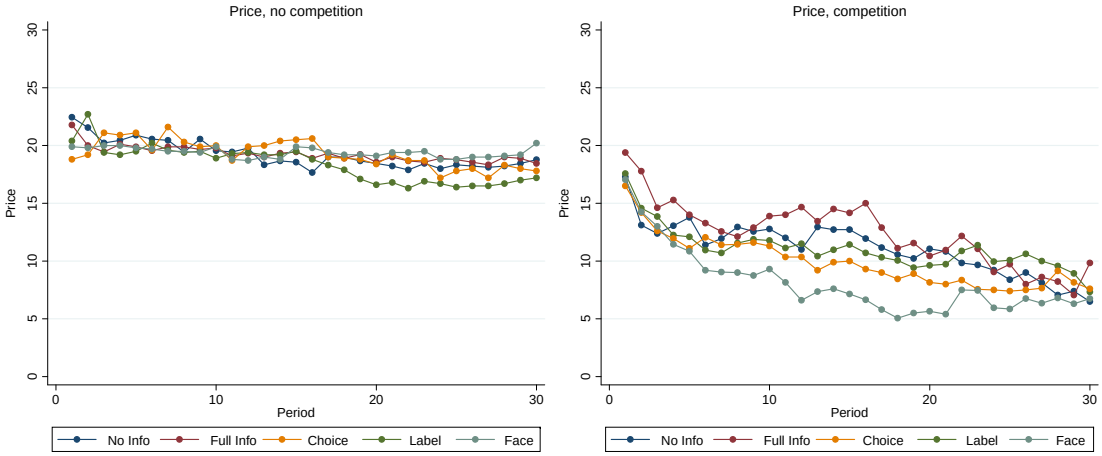


Figure 3.5: Prices per treatment

Figure 3.5 shows the development of average prices over time. In each of the five no-competition treatments the average price is slightly below 20, with no statistical difference between the treatments (see Table 3.10 below). Prices start above 20 and slightly decrease below 20 in period 30. A regression of prices on period finds a negative time trend for *Baseline*, *Choice* and *Label*.[20] In each of the five competition treatments the average prices are around 10 and significantly lower than in the corresponding no-competition treatment (see Table 3.10). With competition the average charged price in Face is significantly lower than in *Full Info* and *Label*. There is no statistical difference between the treatments in any other pairwise comparison (see Table 3.10). Prices under competition start slightly below 20, quickly decrease and end up below 10 in all treatments. Using a regression confirms the time trend.[21]

[20] Random-effects GLS regression of price on period with robust standard errors clustered by manager id, coefficient for period: *Baseline*: -0.114 (p=0.055), *Full Info*: -0.064 (p=0.298), *Choice*: -0.102 (p=0.088), *Label*: -0.159 (p=0.000), *Face*: -0.021 (p=0.385).

[21] Random-effects GLS regression of price on period with robust standard errors clustered by manager id, coefficient for period: *Baseline*: -0.235 (p=0.000), *Full Info*: -0.280 (p=0.000), *Choice*: -0.225 (p=0.000), *Label*: -0.165 (p=0.000), *Face*: -0.248 (p= .000).

Table 3.10: Average prices

	No competition	Competition	Comparison
Baseline	19.211 (0.612)	11.119 (1.195)	0.0005
Full Info	19.274 (1.363)	12.407 (1.320)	0.0031
Choice	18.370 (0.825)	11.041 (0.744)	0.0009
Label	19.287 (1.027)	9.887 (1.372)	0.0000
Face	19.393 (0.783)	8.083 (0.986)	0.0002
Treatment comparison	**No Competition**	**Competition**	
Base vs. Full	0.2692	0.0092	
Base vs. Choice	0.8065	0.0043	
Base vs. Label	0.0411	0.5555	
Base vs. Face	0.2055	0.0033	
Full vs. Choice	0.2203	0.5954	
Full vs. Label	0.0025	0.0083	
Full vs. Face	0.9673	0.2530	
Choice vs. Label	0.0113	0.0056	
Choice vs. Face	0.2568	0.4272	
Label vs. Face	0.0025	0.0024	

Notes: Averages on group means, standard errors on group means in parentheses. Equality-tests using MWU.

Table 3.11: Average wages

	No competition	Competition	Comparison
Baseline	4.704 (1.020)	2.856 (0.384)	0.1711
Full Info	7.393 (1.489)	5.335 (0.915)	0.1999
Choice	5.553 (0.604)	5.458 (0.588)	0.9097
Label	2.390 (0.655)	3.287 (0.458)	0.5093
Face	6.960 (1.047)	6.060 (0.705)	0.4497
Treatment comparison	**No Competition**	**Competition**	
Base vs. Full	0.2692	0.0092	
Base vs. Choice	0.8065	0.0043	
Base vs. Label	0.0411	0.5555	
Base vs. Face	0.2055	0.0033	
Full vs. Choice	0.2203	0.5954	
Full vs. Label	0.0025	0.0083	
Full vs. Face	0.9673	0.2530	
Choice vs. Label	0.0113	0.0056	
Choice vs. Face	0.2568	0.4272	
Label vs. Face	0.0025	0.0024	

Notes: Averages on group means, standard errors on group means in parentheses. Equality-tests using MWU.

Table 3.12: Average manager payoff

	No competition	**Competition**	**Comparison**
Baseline	11.963 (1.577)	6.126 (1.266)	0.0092
Full Info	7.963 (1.412)	5.565 (1.266)	0.1223
Choice	11.373 (1.330)	5.787 (0.545)	0.0191
Label	8.380 (1.444)	3.663 (1.220)	0.0008
Face	8.307 (1.033)	1.272 (0.823)	0.0009
Treatment comparison	**No Competition**	**Competition**	
Base vs. Full	0.1023	0.8253	
Base vs. Choice	0.1207	0.1025	
Base vs. Label	0.8703	0.6886	
Base vs. Face	0.2207	0.0055	
Full vs. Choice	0.7439	0.2885	
Full vs. Label	0.0864	0.7061	
Full vs. Face	0.8703	0.0143	
Choice vs. Label	0.1123	0.0822	
Choice vs. Face	0.7054	0.1509	
Label vs. Face	0.1736	0.0005	

Notes: Averages on group means, standard errors on group means in parentheses. Equality-tests using MWU.

Table 3.13: Average consumer payoff

	No competition	**Competition**	**Comparison**
Baseline	10.000 (0.609)	20.019 (1.310)	0.0003
Full Info	9.311 (1.434)	18.822 (1.608)	0.0009
Choice	9.417 (0.598)	19.950 (0.741)	0.0002
Label	8.367 (0.884)	20.678 (1.355)	0.0000
Face	9.033 (0.775)	22.618 (0.941)	0.0002
Treatment comparison	**No Competition**	**Competition**	
Base vs. Full	0.6587	0.6587	
Base vs. Choice	0.1651	0.6831	
Base vs. Label	0.7750	0.9249	
Base vs. Face	0.4142	0.1651	
Full vs. Choice	0.7440	0.3475	
Full vs. Label	0.6532	0.3704	
Full vs. Face	0.9025	0.1208	
Choice vs. Label	0.3643	0.4032	
Choice vs. Face	0.6775	0.4497	
Label vs. Face	0.6775	0.0430	

Notes: Averages on group means, standard errors on group means in parentheses. Equality-tests using MWU.

Table 3.14: Payoff comparisons

No competition	**M vs. W**	**M vs. C**	**W vs. C**
Baseline	0.0176	0.3424	0.0090
Full Info	0.1229	0.6350	0.4764
Choice	0.1141	0.5751	0.0218
Label	0.0051	0.2839	0.0051
Face	0.2411	0.3074	0.1688
Competition	**M vs. W**	**M vs. C**	**W vs. C**
Baseline	0.0382	0.0109	0.0077
Full Info	0.6784	0.0077	0.0077
Choice	0.2026	0.0069	0.0051
Label	0.0017	0.0001	0.0001
Face	0.0166	0.0051	0.0051

Notes: Equality-tests of payoffs between types in all treatments and conditions using WSR.

Table 3.15: Managers' sales and payoff, competition

		Full Info		
		Lower Price	**Equal Price**	**Higher Price**
Lower SR	Sales	1.52 (0.12)	0.39 (0.13)	0.00 (0.00)
	Payoff	11.30 (2.33)	0.13 (1.47)	−3.76 (1.29)
Equal SR	Sales	1.86 (0.08)	0.97 (0.03)	0.12 (0.08)
	Payoff	16.23 (4.10)	5.60 (2.09)	−4.27 (1.25)
Higher SR	Sales	1.98 (0.02)	1.59 (0.13)	0.47 (0.13)
	Payoff	15.23 (3.81)	12.01 (3.43)	−1.19 (1.26)
		Choice		
		Lower Price	**Equal Price**	**Higher Price**
Lower SR	Sales	1.38 (0.09)	0.22 (0.08)	0.09 (0.07)
	Payoff	7.71 (2.10)	−1.80 (0.80)	−3.16 (0.93)
Equal SR	Sales	1.93 (0.07)	0.92 (0.08)	0.07 (0.07)
	Payoff	11.19 (2.72)	3.03 (2.00)	−6.00 (0.76)
Higher SR	Sales	1.91 (0.07)	1.78 (0.08)	0.62 (0.09)
	Payoff	12.40 (2.76)	10.41 (2.90)	−0.96 (0.90)
		Label		
		Lower Price	**Equal Price**	**Higher Price**
Lower SR	Sales	1.31 (0.13)	0.19 (0.10)	0.00 (0.00)
	Payoff	11.20 (1.41)	2.37 (1.65)	−1.90 (0.73)
Equal SR	Sales	1.82 (0.06)	0.98 (0.02)	0.13 (0.06)
	Payoff	13.08 (1.08)	5.47 (0.69)	−2.91 (0.87)
Higher SR	Sales	2.00 (0.00)	1.79 (0.10)	0.69 (0.13)
	Payoff	12.76 (2.44)	13.58 (2.87)	2.34 (1.80)
		Face		
		Lower Price	**Equal Price**	**Higher Price**
Lower SR	Sales	1.11 (0.19)	0.23 (0.11)	0.02 (0.02)
	Payoff	4.83 (1.74)	−2.49 (1.19)	−6.65 (0.84)
Equal SR	Sales	1.77 (0.12)	1.00 (0.00)	0.23 (0.12)
	Payoff	5.61 (1.36)	0.34 (0.97)	−3.40 (1.22)
Higher SR	Sales	1.98 (0.02)	1.77 (0.11)	0.89 (0.19)
	Payoff	7.86 (2.06)	4.61 (2.07)	−1.77 (2.28)

Notes: Averages on group means, standard errors on group means in parentheses. Managers' sales and payoff depending on offering a lower, equal or higher price and on SR. SR is lower (equal, higher) if a manager pays a lower (equal, higher) wage than the competitor in *Full Info* and *Choice*. SR is lower (equal, higher) if a manager's worker has lower (equal, higher) satisfaction level than the competitor's worker in *Face*. In Label, the SR is higher if a manager has a label but the competitor does not and vice versa. The SR is equal if both firms have or do not have a label.

3.8.2 Predictions assuming inequity aversion

Consider individuals with Fehr and Schmidt (1999) preferences and an information condition as in *Full Info*. A consumer in the no competition condition has the following utility from buying ($x = 1$) or not buying ($x = 0$) a given offer (w, p):

$$\begin{aligned} u_C^{buy}(w,p) &= (30-p) - \frac{\alpha}{2} \cdot (\max\{0, 2p-w-30\} + \max\{0, w+p-30\}) \\ &\quad -\frac{\beta}{2} \cdot (\max\{0, 2p-w-30\} + \max\{0, w+p-30\}) \\ u_C^{not\ buy}(w,p) &= 0 - \frac{\alpha}{2}w - \frac{\beta}{2}w \end{aligned}$$

In both equations the first term corresponds to the monetary payoff, while the other two terms resemble the disutility from inequity. The α-term captures the disutility from disadvantageous payoff comparisons, i.e. the cases where other player(s) have a higher payoff. The β-term captures the disutility from advantageous payoff comparisons, i.e. the cases where other player(s) have a lower payoff. The assumption $\alpha \geq \beta$ expresses that disadvantageous inequity looms larger than advantageous inequity. For a given allocation, a consumer chooses to buy or not to buy (accept or reject), which provides the highest utility. For simplicity we assume that the consumer accepts if and only if $u_{buy}(w,p) \geq u_{not\ buy}(w,p)$. Using the argument of backward induction the manager will propose the allocation that provides her with the highest utility, where

$$\begin{aligned} u_M^{buy}(w,p) &= (p-w) - \frac{\alpha}{2} \cdot (\max\{0, 2w-p\} + \max\{0, 30+w-2p\}) \\ &\quad -\frac{\beta}{2} \cdot (\max\{0, p-2w\} + \max\{0, 2p-30-w\}) \\ u_M^{not\ buy}(w,p) &= -\left(\frac{3\alpha}{2}+1\right)w \end{aligned}$$

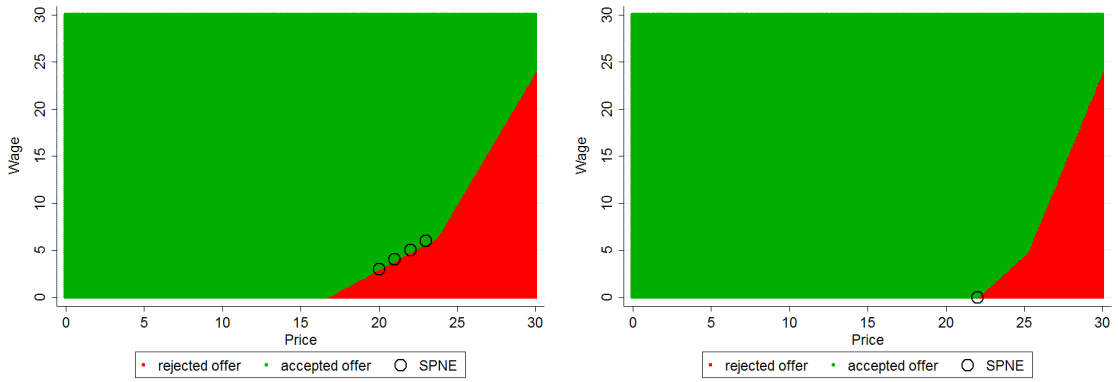
In order to calculate the subgame perfect Nash equilibria (SPNE) for individuals with Fehr-Schmidt preferences in the stage game of no competition in *Full Info*, we have to specify assumptions on the parameters α and β. We consider the case of selfish individuals ($\alpha = 0, \beta = 0$), which only care for their monetary payoff and three different levels of inequity aversion:[22] low ($\alpha = 1, \beta = 0.25$), medium ($\alpha = 4, \beta = 0.6$), and high ($\alpha = 4, \beta = 1$). By varying these four "types" for the consumer and the manager, we receive 16 different cases. Table 3.16 displays the Fehr-Schmidt equilibria of these cases and Figure 3.6 graphically

[22] Comparable parameters have been assumed by Fehr and Schmidt (1999). The "high"-type represents an extreme kind of inequity aversion. For experimental results on the distribution of α and β (see, for example, Blanco, Engelmann, & Normann, 2011).

shows the outcomes for two of those cases. These examples show that inequity avers consumers may refuse to buy when a high price does not come with a positive wage (red areas in the lower right corners of Figure 3.6). In equilibrium, this induces even selfish managers to pay positive wages if consumers are highly inequity avers (see right panel of Figure 3.6).

Table 3.16: Subgame Perfect Nash Equilibrium outcomes for (price,wage) with different levels of inequity aversion (IA), no competition

	Selfish Consumer $\alpha = 0, \beta = 0$	Low IA Consumer $\alpha = 1, \beta = 0.25$	Medium IA Consumer $\alpha = 4, \beta = 0.6$	High IA Consumer $\alpha = 4, \beta = 1$
Selfish Manager $\alpha = 0, \beta = 0$	(30,0)	(22,0)	(17,0), (18,1),(19,2), (20,3),(21,4), (22,5), (23,6)	(20,3), (21,4), (22,5), (23,6)
Low IA Manager $\alpha = 1, \beta = 0.25$	(30,0)	(22,0)	(17,0), (18,1), (19,2), (20,3), (21,4), (22,5), (23,6)	(20,3), (21,4), (22,5), (23,6)
Medium IA Manager $\alpha = 4, \beta = 0.6$	(30,0)	(22,0)	(17,0), (18,1), (19,2), (20,3), (21,4), (22,5), (23,6)	(20,3), (21,4), (22,5), (23,6)
High IA Manager $\alpha = 4, \beta = 1$	(20,10)	(20,10)	(20,10)	(20,10)



(a) medium IA manager, low IA consumer

(b) selfish manager, high IA consumer

Figure 3.6: Subgame Perfect Nash Equilibrium outcomes for different levels of inequity aversion, no competition

The competition analysis is far too complex to provide a closed form solution. To get an intuition about how inequity aversion affects consumer choices in duopoly, we calculate the best responses of a consumer facing the two offers (w_A, p_A) and (w_B, p_B). Note, that the best response of a consumer depends also the choice of the other consumer. To simplify matters we make the following assumptions: the other consumer has selfish preferences and always buys at the cheaper firm. In case of identical prices, she buys at firm B. The inequity averse consumer prefers to buy at firm A over firm B over not to buy in case of indifference. Table 3.17 provides the average choices of the inequity averse consumer depending on the differences in price and wages between the two firms. The table shows that an inequity avers consumer mostly buys at the firm with the lower price (about 90 percent in all cases) and that the likelihood to buy at the lower price firm increase with the firm's wage. It is however possible that the inequity avers consumer buys at the firm with the higher price to reduce inequity.

Table 3.17: Likelihood to buy for an inequity averse consumer, competition

		Low IA Consumer $\alpha = 1, \beta = 0.25$		
		Lower Price A	Equal Price	Higher Price A
Lower Wage A	Buy A	0.932	0.900	0.019
	Buy B	0.063	0.028	0.971
	Buy Not	0.005	0.072	0.010
Equal Wage	Buy A	0.952	0.949	0.040
	Buy B	0.040	0.000	0.952
	Buy Not	0.008	0.051	0.008
Higher Wage A	Buy A	0.971	0.971	0.063
	Buy B	0.019	0.000	0.932
	Buy Not	0.010	0.029	0.005
		Medium IA Consumer $\alpha = 1, \beta = 0.6$		
		Lower Price A	Equal Price	Higher Price A
Lower Wage A	Buy A	0.887	0.865	0.030
	Buy B	0.101	0.027	0.948
	Buy Not	0.012	0.108	0.022
Equal Wage	Buy A	0.918	0.922	0.064
	Buy B	0.064	0.000	0.918
	Buy Not	0.018	0.078	0.018
Higher Wage A	Buy A	0.948	0.953	0.101
	Buy B	0.030	0.000	0.887
	Buy Not	0.022	0.047	0.012
		Low IA Consumer $\alpha = 4, \beta = 1$		
		Lower Price A	Equal Price	Higher Price A
Lower Wage A	Buy A	0.873	0.870	0.035
	Buy B	0.117	0.027	0.943
	Buy Not	0.010	0.104	0.022
Equal Wage	Buy A	0.909	0.928	0.075
	Buy B	0.073	0.000	0.908
	Buy Not	0.018	0.072	0.018
Higher Wage A	Buy A	0.943	0.961	0.119
	Buy B	0.035	0.000	0.871
	Buy Not	0.022	0.039	0.010

Notes: Averages choices of an inequity averse consumer for all (price, wage) combinations.

3.8.3 Questionnaire Results

The following demographic variables were retrieved: age, sex, semester, experience in experiments (dummy) and experience in market experiments (dummy), expe-

rience (number of participations in experiments). The following questions were asked:

What role did the wage of a worker play in the purchase decision?
1: no ... 7: a very big

	Baseline	Full Info	Label	Choice	Face
No competition	2.000	3.333	2.567	3.333	2.567
Competition	1.741	3.333	3.342	4.500	3.833

The role of the wage is significantly lower in *Baseline* in than in the other treatments in No Competition (MWT: *Baseline* vs. *Full* (p=0.004), vs. *Choice* (p=0.003), vs. *Label* (p=0.044), vs. *Face* (p=0.012)). There are no significant differences in other pairwise comparisons. In Competition, *Baseline* values are also significantly lower (MWT on subject level: *Baseline* vs. *Full* (p=0.000), vs. *Choice* (p=0.000), vs. *Label* (p=0.000), vs. *Face* (p=0.000)) and *Choice* values significantly higher (MWT on subject level: *Choice* vs. *Full* (p=0.001), vs. *Label* (p=0.000), vs. *Face* (p=0.042)). There are no significant differences in other pairwise comparisons. Between competitions conditions we find significant differences for *Choice* (MWT on subject level: p=0.007), *Label* (MWT on subject level: p=0.029) and *Face* (MWT on subject level: p=0.027).

What do you think? Which role (manager or consumer) in this experiment had more "market power"? The market power lay with...
1: the manager only ... 7: the consumer only

	Baseline	Full Info	Label	Choice	Face
No competition	4.074	4.556	4.533	4.133	4.733
Competition	5.185	5.444	5.200	5.392	5.650

There is no statistical difference between the treatments within competition conditions (Chi-squared test, no competition: p=0.690, competition: p=0.461), but between competition conditions (Chi-squared test, treatments pooled, p=0.000).

By manufacture, transport, use and disposal of a product greenhouse gases are released. Would it affect your purchase decision when products were labeled with a corresponding value of the amount of greenhouse gases?
1: Yes, a labeling of the climate impact would affect my purchasing behavior.
0: No, a labeling of the climate impact would not affect my purchasing behavior.
-1: I do not know

There is no statistical difference between the treatments and competition conditions (Chi-squared test, no competition: p=0.146, competition: p=0.202, between competition conditions: p=0.674), so results are pooled:

Do not know	Yes, a labeling of the climate impact would affect my purchasing behavior.	No, a labeling of the climate impact would not affect my purchasing behavior.
20%	11%	70%

The following questions are modified versions from the Eurobarometer 47.0 (Melich, 2000) about fair trade consumption (Question Q66 etc., pp. 419).

Some products from developing countries have a label (e.g., Fair trade, Organic certification, GOTS, RugMark). Such labeled products guarantee that during their manufacture certain social and sometimes environmental criteria are met. Do you know these or similar labels? If so, have you already purchased products that were labeled?

0: No, not known.
1: Yes, known, but not purchased.
2: Yes, known and already purchased.
3: Yes, known and regularly purchased.

There is no statistical difference between the treatments and competition conditions (Chi-squared test, no competition: p=0.688, competition: p=0.623, between competition conditions: p=0.960), so results are pooled:

No, not known	Yes, known, but not purchased	Yes, known and already purchased	Yes, known and regularly purchased
6%	18%	62%	15%

Do you, in general, feel sufficiently informed about the production conditions when making a purchasing decision?

0: No ... 1: Yes

There is no statistical difference between the treatments and competition conditions (Chi-squared test, no competition: p=0.498, competition: p=0.386, between competition conditions: p=0.433), so results are pooled:

No	Yes
86%	14%

Would you be willing to pay a premium for convenience goods that have a comparable performance but have been demonstrably produced under better social conditions than competing products?

0: I would not accept a premium.
1: I would accept a premium of up to 10%
2: I would accept a premium of up to 20%
3: I would accept a premium of up to 30%
4: I would accept a premium of more than 30%

There is no statistical difference between the treatments (Chi-squared test, no competition p=0.692, competition: p=0.422, between competition conditions: p=0.883), so results are pooled:

I would not accept a premium	I would accept a premium of up to 10%	I would accept a premium of up to 20%	I would accept a premium of up to 30%	I would accept a premium of more than 30%
8%	42%	35%	11%	4%

Would you be willing to pay premium for convenience goods that have a comparable performance but that are demonstrably produced more climate-friendly/ more ecologically than competing products?

0: I would not accept a premium.
1: I would accept a premium of up to 10%
2: I would accept a premium of up to 20%
3: I would accept a premium of up to 30%
4: I would accept a premium of more than 30%

There is no statistical difference between the treatments (Chi-squared test, no competition p=0.504, competition: p=0.314, between competition conditions: p=0.384), so results are pooled:

I would not accept a premium	I would accept a premium of up to 10%	I would accept a premium of up to 20%	I would accept a premium of up to 30%	I would accept a premium of more than 30%
12%	49%	29%	7%	3%

3.8.4 Instructions

In this section we present the instructions for the treatments No competition - *Choice* and Competition - *Label*. They serve as examples as all instructions are formulated in a similar way. The instructions of the other treatments may be obtained from the authors upon request. The original instructions were in German and the ones listed below are translations into English.

Instructions for treatment No competition *Choice*

Instructions for the experiment

General information We welcome you to this economic experiment. It is very important that you read the following explanations carefully. If you have any questions, please direct them to us. In this experiment, you can earn money depending on your own decisions and the decisions of the other participants.

During the experiment, you are not allowed to talk with other participants of the experiment. Non-compliance with this rule results in exclusion from the experiment and all payments. All decisions are taken anonymously, i.e., none of the other participants learns the identity of the participant who has taken a specific decision. Payment is anonymous as well, i.e., no participant learns the payments of the other participants. During the experiment, your entire income is calculated in points. In the end of the experiment, the total number of points that you earned during the experiment is converted into Euro, where

$$40 \text{ points} = 1 \text{ Euro}.$$

At the end of today's experiment, you receive the number of points earned during the experiment plus 2.50 € show-up fee. In addition, at the beginning of the experiment, you receive an initial endowment of 40 points. On the following pages, we will explain to you the detailed procedure of the experiment.

Information about the experiment

Course of the experiment

- The experiment consists of 30 rounds, and each round has the same structure.
- You are part of a group with 3 members. During the entire experiment, you exclusively interact with the members of your group. The composition of the group remains the same across all rounds.

Company, manager, worker, and consumer

- There is a **company** and a **consumer**.
- The company consists of a **manager** and a **worker**.
- Which role you are assigned to is randomly determined at the beginning of the experiment, and remains unchanged during the entire course of the experiment. Please note that your role allows for no conclusions about your identity.

Good, price, value, and trade

- In the company, the worker produces a units of a **good**.
- The manager of a company sets:
  - the **wage** of the workers of the company (integer **between 0 and 30 points**), and
  - the **price** at which the company offers the good (integer **between 0 and 30 points**).
- The consumer can buy **up to one unit** of the good, and decides whether he buys the good or not. The good has a **value of 30 points** for the consumer.
- If the consumer buys the good, he pays the price set by the manager of the company. By a purchase, the consumer receives 30 points minus the price paid. A consumer who does not buy receives 0 points.
- Whether the company sells a unit of the good depends on the purchasing decision of the consumer. Therefore, the company can sell none or one unit.

Income in a round Each participant receives an **endowment** of **5 points per round**. The remaining income depends on decisions in the following way:

- Income of participants in one round

Manager:	endowment - wage of the worker + price × number of units sold
Worker:	endowment + wage
Consumer:	endowment + number of units purchased × (30 - price of the good)

Course of the experiment Before the start of the first round, you are informed about your role. All rounds take place according to the following scheme:

- **Step 1: Actions of the worker and the manager**
  - The worker produces the unit of the good.
  - The manager sets the wage of the worker, and the price of the good.
- **Step 2: Actions of the consumer**
  - The consumer gets informed about the price of the good.
  - The consumer decides whether he wants to get informed about the wage of the worker.
  - The consumer decides whether to buy a unit of the good.
- **Step 3: Information**
  - The worker gets informed about his wage, and the price of the good.
  - The manager and the worker get informed about the purchasing decision of the consumer.
  - Each participant learns his round income.

Total income

Your total income is the sum of the incomes of all rounds plus the initial endowment of 40 points.

Good luck!

Instructions for treatment Competition *Label*

Instructions for the experiment

General information We welcome you to this economic experiment. It is very important that you read the following explanations carefully. If you have any questions, please direct them to us. In this experiment, you can earn money depending on your own decisions and the decisions of the other participants.

During the experiment, you are not allowed to talk with other participants of the experiment. Non-compliance with this rule results in exclusion from the experiment and all payments. All decisions are taken anonymously, i.e., none of the other participants learns the identity of the participant who has taken a specific decision. Payment is anonymous as well, i.e., no participant learns the payments of the other participants. During the experiment, your entire income is calculated in points. In the end of the experiment, the total number of points that you earned during the experiment is converted into Euro, where

$$40 \text{ points} = 1 \text{ Euro}.$$

At the end of today's experiment, you receive the number of points earned during the experiment plus 2.50 € show-up fee. In addition, at the beginning of the experiment, you receive an initial endowment of 40 points. On the following pages, we will explain to you the detailed procedure of the experiment.

Information about the experiment

Course of the experiment

- The experiment consists of 30 rounds, and each round has the same structure.
- You are part of a group with 6 members. During the entire experiment, you exclusively interact with the members of your group. The composition of the group remains the same across all rounds.

Companies, managers, workers, and consumers

- There are two companies (A and B) and two consumers (X and Y).
- Each of the two companies consists of a manager and a worker. At the beginning of the experiment, it is randomly determined which manager and which worker form company A, and which manager and which worker form company B. This assignment remains unchanged over the entire course of the experiment.
- Which role you are assigned to is randomly determined at the beginning of the experiment, and remains unchanged during the entire course of the experiment. Please note that your role allows for no conclusions about your identity.

Good, price, value, and trade

- In both companies, the worker produces several units of an identical good.
- The manager of a company sets:
  - the wage of the workers of the company (integer between 0 and 30 points), and
  - the price at which the company offers the good (integer between 0 and 30 points).
- Every consumer can buy up to one unit of the good, and decides whether he buys the good from company A, from company B, or whether he does not buy the good at all. The good has a value of 30 points for each of the two consumers.

- If a consumer buys the good from company A, he pays the price set by the manager of company A. If a consumer buys from company B, he pays the price set by the manager of company B. By a purchase, a consumer receives 30 points minus the price paid. A consumer who does not buy receives 0 points.
- How many units a company sells depends on the purchasing decisions of the consumers. Therefore, a company can sell none, one or two units. Certificate
- The managers can buy a certificate for their company. A company with a certificate must pay its worker a wage of at least 4 points. If the company has acquired a certificate, both the consumers and the other company receive the information: "A wage of at least 4 points is paid". The costs for the certificate are 1 point.
- If the company does not buy a certificate, the manager can set the wage for the worker of his company freely between 0 and 30 points. In both cases, neither the consumers nor the other company receive information about the actual wage.

Income in a round Each participant receives an endowment of 5 points per round. The remaining income depends on decisions in the following way:

- Income of participants in one round

Manager:	endowment - wage of the worker + price × number of units sold -1 (if certificate was purchased)
Worker:	endowment + wage
Consumer:	endowment + number of units purchased × (30 - price of the good)

Course of the experiment Before the start of the first round, you are informed about your role (manager A, worker A, manager B, worker B, consumer X or consumer Y). All rounds take place according to the following scheme:

- Step 1: Actions of the workers and managers
  - The workers produce the units of the good.
  - The managers decide whether to buy a certificate.
  - The managers set the wage of the worker of their company, and the price of the good.
- Step 2: Actions of the consumers
  - The consumers get informed about the price of the good of company A, and the price of the good of company B.
  - They receive the information: "A wage of at least 4 points is paid " if the respective manager has bought the certificate, and they receive no information about the wage otherwise.
  - The consumers decide whether and from which company to buy a unit of the good.
- Step 3: Information
  - Every worker gets informed about his own wage and the prices set by the two managers.
  - Both managers get informed about the price of the other company.
  - Both workers and both managers receive the information about the wage of the other company: A wage of at least 4 points is paid " if the manager of the other company has bought the certificate, and they receive no information about the wage of the other company otherwise.

- Both managers, both workers and both consumers get informed about the purchasing decisions of both consumers.
- Each participant learns his round income.

Total income

Your total income is the sum of the incomes of all rounds plus the initial endowment of 40 points.

Good luck!

4 The Competitive Advantage of Honesty

4.1 Introduction

Consumer social responsibility is "the conscious and deliberate choice to make certain consumption choices based on personal and moral beliefs", which may show up as "expressed activity in terms of purchasing or non-purchasing behavior" (Devinney et al., 2006, p. 32). The impact of consumer social responsibility on total consumption decisions and its drawbacks to producers is a widely debated issue in consumer research (for an overview, see, for example, Smith, 2007). In recent years, these questions have also been addressed in controlled experimental frameworks. In the experiments of Rode et al. (2008), Etilé and Teyssier (2012), and Feicht et al. (2014) consumers are willing to pay (slightly) higher prices to buy a product which implies a donation to an NGO. Bartling et al. (2015) find that consumers accept a price premium for goods that do not harm a third person. In chapter 3 we study the effect of consumer social responsibility in different competition conditions. We find no indications for consumer social responsibility when the supplier is a monopolist. In competitive markets, however, when consumers can choose between different offers, consumers' purchasing behavior may make social responsible production profitable for suppliers. These experiments focus on payoff allocations as a proxy for social responsibility. Although distributional consequences are an important aspect, it is for sure not the only one influencing consumers' "moral beliefs". Companies' compliance, for example, to human rights, animal rights, environmental protection, political rights, tax laws, and workers' rights are important factors in the recent discussions of corporate social responsibility and are triggers for boycott calls of "ethical consumers". O'Connor and

Meister (2008) ask subjects to rank-order different measures commonly found in corporate social responsibility communication and identify honesty as the most important attribute of a corporation.[1] In this chapter, we follow up on this and examine the role of a corporation's honesty on consumer's purchasing behavior in an experimental setup where we disentangle honesty and distributional concerns.

We study the effects of a corporation's honesty (towards her employees) on consumers' purchasing decisions. In an experimental market, firms offer a good with a fixed induced monetary value to the consumers. The firms determine the product's price and may save overall production costs by being dishonest to their workers. Workers have either a high or a low ability for the production process. High (low) ability workers entail low (high) production costs and "deserve" a high (low) wage. The worker's ability is observed by the firm, but not by the worker. The firm communicates the ability to the worker and the resulting wage (high wage for high ability and low wage for low ability worker). This communication does not have to be truthful. A dishonest firm may lie by communicating a low ability to her high ability worker and thus benefit from the low production cost of the high ability worker, but only pay the low wage, appropriate for a low ability worker. Do consumers honor honesty of the firm towards her worker? Specifically, would consumers pay a higher price to buy at an honest firm? And if so, what are the conditions that make honesty a competitive advantage in a competitive market?

We address these questions in an experimental Bertrand duopoly market with two firms and two consumers. The experimental treatments vary the consumer's information on the managers' honesty towards their workers and allow us to test for consumers' preferences for within-firm honesty. In the *Baseline* treatment consumers have no information on the workers ability and the paid wage and thus are not able to make any inference about firms' honesty. In the second treatment, *Wage Info*, consumers learn the wage of the firms' workers, but are not informed about the workers' abilities. Thus, consumers are not able to verify whether the wage paid to the worker is actually the wage he deserves according to his ability.

[1] "I think a corporation should be honest" ranked prior to "produces quality products and services", "treat employees fairly", "give back to the community through philanthropic activities" and "conduct business in an environmentally friendly manner".

In the final treatment, *Full Info*, the consumers have full transparency. Consumers see the wages and whether or not the firms are honest towards their workers.

We find that absent any wage and honesty information (*Baseline*), honesty is very low, resulting in low wages for workers. When consumers see wages (Wage Info), honesty as well as wages increase and the wage level is a significant determinant in consumers' purchasing decisions. If consumers are additionally informed about firms' honesty (*Full Info*), consumers on top of the wage level also take firms' honesty as a significant criterion in their purchasing decisions. As a result, honesty generates a competitive advantage for firms. Honest firms make higher profits by selling more units, albeit not at higher prices. We complement the experimental findings by testing a simple model for consumers' purchasing decisions, which incorporates social preferences as well as moral preferences for honest transactions. Our data show that apart from allocative preferences, preferences for buying at an honest firm play a crucial role in consumers' purchasing decisions.

4.2 The market model

In our experiment, we study a bilateral Bertrand duopoly with two firms (A and B) and two consumers (X and Y). Firms offer a good on the market, consumers may purchase at most one unit of the good, and each firm may potentially serve both consumers. Each consumer has a fixed valuation v for the good ($v = 30$). Both firms consist of one manager and one worker each. The manager determines the price p for the units of the good her firm offers ($p \in \{0, 1, \ldots, v\}$) and determines the wage w of the worker (see below). The worker produces the firm's units of the good. Before the market starts, a "state of the world" $S \in \left\{\underline{S}, \overline{S}\right\}$ is randomly drawn, both realizations equally likely. You may think of the state S as a proxy for the production environment of the firm. A firm's worker does not know the exact production environment, i.e. the realization of S, but provides a guess $\hat{S}$ for S. The accuracy of the worker's guess is a proxy for the worker's ability and determines the production costs c he entails for his firm. If the worker guessed right ($\hat{S} = S$), the worker is a "high ability worker" who entails low production costs c_1 ($c_1 = 0$) for his firm. Otherwise, if $\hat{S} \neq S$, the worker entails high production costs $c_0 > c_1$ ($c_0 = 6$) for his firm. The firm's manager has complete information

on the production environment and the worker's guess, i.e. knows both S and $\hat{S}$. The manager informs her worker whether he guessed right or wrong, i.e. sends information $I \in \{\text{guessed right}, \text{guessed wrong}\}$. This information does not have to be true: it can be *honest*, i.e. ($\hat{S} = S$ and $I =$ guessed right) and ($\hat{S} \neq S$ and $I =$ guessed wrong), or *dishonest*. The information sent perfectly determines the worker's wage. If a manager informs her worker that he guessed right, she has to pay a high wage w_1 ($w_1 = 7$) to the worker. If a manager informs her worker that the guess was wrong, she has to pay a low wage $w_0 < w_1$ ($w_0 = 3$) to the worker. The manager has no further discretion to change this exogenously fixed wages for high and low ability workers. Hence, payoffs are:

$$\Pi_{Consumer} = \begin{cases} v - p = 30 - p & \text{if consumer buys one unit of the good} \\ 0 & \text{if consumer does not buy} \end{cases} \tag{4.1}$$

$$\Pi_{Worker} = w = \begin{cases} w_0 = 3 & \text{if manager informs that guess is wrong} \\ w_1 = 7 & \text{if manager informs that guess is right} \end{cases} \tag{4.2}$$

$$\Pi_{Manager} = p \cdot \text{number of units sold} - (c + w), \quad \text{with} \tag{4.3}$$

$$c + w = \begin{cases} c_1 + w_1 = 7 & \text{if guess is right and manager is honest} \\ c_1 + w_0 = 3 & \text{if guess is right and manager is dishonest} \\ c_0 + w_0 = 9 & \text{if guess is wrong and manager is honest} \\ c_0 + w_1 = 13 & \text{if guess is wrong and manager is dishonest} \end{cases} \tag{4.4}$$

Obviously, the manager profits from having a high ability worker (a worker who guessed right). She has the lowest overall costs $c_1 + w_0 = 3$, if her worker guessed right and she is dishonest by informing her worker that he guessed wrong.

4.3 A model of consumer choice

Our research is motivated by the question of whether consumers' purchasing decisions in a competitive market environment are influenced by honest behavior within the firm they may buy from. To think of the question in a more structural way, we propose a simple model of consumer choice. When making the purchasing decision, a consumer in the duopoly market faces the market condition $m = (S, g_A, i_A, p_A, g_B, i_B, p_B)$, with the realization S of the state of the world,

the guesses $g_A, g_B \in \left\{\underline{S}, \overline{S}\right\}$ of the workers of firms A and B, the information i_A, $i_B \in \{\text{guessed right}, \text{guessed wrong}\}$ sent by managers of firms A and B and finally the prices $p_A, p_B \in \{0, 1, \ldots, 30\}$ asked by the firms A and B. The production costs $c_A, c_B \in \{0, 6\}$, the wages $w_A, w_B \in \{3, 7\}$ and the mangers' honesty can be derived from S, g_A, g_B, i_A and i_B. Each consumer maximizes her utility $u(m, x)$ by choosing an action $x \in \{0, A, B\}$, where 0 represents the decision not to buy, and A and B represent buying at firm A and B, respectively.[2] With respect to our research focus, we allow the consumer's utility function u to depend on the monetary consequences of action x as well as on the honesty of the firm the consumer is buying from. The utility function has the form:

$$u(m, x) \quad = \quad M(m, x) + P(m, x), \tag{4.5}$$

where the function $M(m, x)$ captures the monetary consequences of the consumer's decision. If the consumer just cares for her own monetary payoff, $M(m, x) = \Pi_{Consumer}$ (see equation (4.1)). More generally, the consumer may have social preferences and also cares for the monetary payoffs of the interaction partners. We propose a utility function based on Fehr and Schmidt (1999) preferences with inequity aversion towards the firm they are purchasing from.[3] Hence,

$$M(m, x) = \begin{cases} 0 & \text{if } x = 0 \\ \begin{array}{l}(30 - p_A) \\ \quad -\frac{1}{2}\alpha\left((2p_A - w_A - c_A - 30)^+ + (w_A + p_A - 30)^+\right) \\ \quad -\frac{1}{2}\beta\left((30 + w_A + c_A - 2p_A)^+ + (30 - w_A - p_A)^+\right)\end{array} & \text{if } x = A \\ \begin{array}{l}(30 - p_B) \\ \quad -\frac{1}{2}\alpha\left((2p_B - w_B - c_B - 30)^+ + (w_B + p_B - 30)^+\right) \\ \quad -\frac{1}{2}\beta\left((30 + w_B + c_B - 2p_B)^+ + (30 - w_B - p_B)^+\right)\end{array} & \text{if } x = B \end{cases} \tag{4.6}$$

[2] For simplicity, we assume that the consumer's utility does not depend on the actions of the other consumer. We do not propose an equilibrium model, but a model of consumer choice.

[3] We assume that a consumer does not care for all agents on the market, but only for those she directly or indirectly interacts with. Thus, we ignore payoff comparisons to market participants not involved the consumers' trade. See, for example, van Dijk and van Winden (1997), Malmendier and Schmidt (2012) for models with different utility weights for agents from a reference group.

If the consumer does not buy ($x = 0$), she only cares about her own monetary income that is zero. If the consumer buys, she cares about the income distribution within the firm. The α-term captures the disutility from disadvantageous payoff comparisons, i.e. the cases where other players have a higher payoff. The β-term captures the disutility from advantageous payoff comparisons, i.e. the cases where other players have a lower payoff. The assumption $\alpha \geq \beta$ expresses that disadvantageous inequity looms larger than advantageous inequity.

Importantly, the function $M(m, x)$ just incorporates the monetary payoffs, independent of how these allocations came about. In our model of consumer choice, consumers may care for the process that led to the payoff allocation and potentially experience disutility when buying from a firm in which the manager lied to the worker. This disutility is captured in the function $P(m, x)$. To acknowledge that "lying costs" have found to be proportional to the "size of the lie" (Gneezy, 2005) we assume that the disutility of buying at a dishonest firm is proportional to the consequences of the lie. In our market, managers may tell a "black lie" by paying a low wage to a worker who guessed right. Or, managers may tell an (altruistic) "white lie" by paying a high wage to a worker who guessed wrong (for a categorization of lies see Erat & Gneezy, 2012). In both cases, we assume that the disutility from buying at a firm that lied to the worker is proportional to resulting wage difference $|w_1 - w_0| = 4$, however, we allow that black lies are weighted differently than white lies. Thus, in case of buying from a firm with a black lie, the consumer achieves a disutility of $-\lambda_b \cdot 4$, and when buying from a firm with a white lie, the consumer achieves a disutility of $-\lambda_w \cdot 4$, with $\lambda_b \geq \lambda_w \geq 0$:

$$P(m, x) = \begin{cases} 0 & \text{if buying from an honest manager} \\ -\lambda_b \cdot 4 & \text{if buying from a dishonest manager telling a black lie} \\ -\lambda_w \cdot 4 & \text{if buying from a dishonest manager telling a white lie} \end{cases} \tag{4.7}$$

The predictions of our model on consumer choice depend on the parameters α and β weighting the disutilities in the Fehr and Schmidt model and the parameters λ_b, λ_w, weighting the disutility from buying at a firm with a dishonest manager. In Figures 4.1 - 4.4 in Section 4.7.1, we visualize consumer choice for different exemplary parameter constellations. Figure 4.1 features ($\alpha = 0, \beta = 0, \lambda_b = \lambda_w =$

0), i.e. the case of a purely selfish consumer who just cares for the own payoff and is neither inequity averse nor averse to buying from a firm whose manager lied. As expected, this consumer always buys the cheaper good.

Figure 4.2 shows the choices of a selfish consumer who experiences disutility from buying from a dishonest firm ($\alpha = 0, \beta = 0, \lambda_b = 0.5, \lambda_w = 0.1$).[4] In this case the disutility of dishonesty is just an add-up to the price. Consider for example the figure in the last row and third column of Figure 4.2. It displays consumer decisions when both workers guessed right, but firm A is honest and firm B is dishonest. The upwards-shift of the diagonal (compared to the same situation in Figure 4.1) shows that the consumer buys at the honest firm A, as long the firm's mark-up is small. Only if the mark-up is too high, the firm buys at the dishonest firm B.

Figure 4.3 displays the consumer decisions for an inequity averse consumer who experiences no disutility from buying at a firm with a dishonest manager ($\alpha = 1.5, \beta = 0.5, \lambda_b = \lambda_w = 0$).[5] The figure shows that consumers do not buy if both prices are relative high, since the disutility of disadvantageous inequality is higher than the own monetary payoff. Otherwise, if both firms have the same production costs (either both workers guessed right or both workers guessed wrong), the consumer buys at the firm which offers the lower price (see the figures with the rows 1-2 and columns 1-2, or rows 3-4 and columns 3-4). Thus, in these cases, and under the ranges expected in a Bertrand duopoly, the inequity averse consumer does not behave different from the selfish consumer. When firms differ in their production costs, i.e. when one firm has a worker who guessed right, while the other firm has a worker who guessed wrong, the consumer prefers to buy at the firm with the lower production costs (i.e. the one where the worker guessed right), if the price is not too high (lower than 17 and the price difference lower than 6). Importantly, note that the consumer's decision is not affected by honesty, but just by inequity concerns. It is easy to see that whenever $p < 16.5$, the consumer has the highest payoff of all three transaction partners and thus experiences disutil-

[4] Since there are no obvious parameter choices for λ_b and λ_w, we exemplarily present $\lambda_b = 0.5$, $\lambda_w = 0.1$. We did the calculations for a wide range of parameter choices; however, the results do not change qualitatively.

[5] As inequity aversion parameters, we chose $\alpha = 1.5$, $\beta = 0.5$, which is well in the range of experimentally observed values (for example, Blanco et al., 2011).

ity from advantageous inequity vis-à-vis the worker and the manager. Honesty only distributes income between the manager and the worker, but keeps the total disutility from advantageous inequity constant and thus has no effect on the consumer's decision.

Finally, Figure 4.4 displays the decisions of a consumer, who is both inequity averse and averse to buying at a firm with a dishonest manager ($\alpha = 1.5, \beta = 0.5, \lambda_b = 0.5, \lambda_w = 0.1$). To see how consumers' dishonesty aversion affects choices, focus on the interesting cases that the two firms only differ with respect to the manager's honesty. Consider, for example, the graph in the last row and the third column of Figure 4.4. This resembles the situation that both firms have workers that guessed right, while the two firms differ with respect to their honesty. Firm A honestly informs her worker and pays the high wage, while firm B lies to her worker and pays the low wage. The graph shows a strong asymmetry in the consumer's purchasing behavior. In most cases, the consumer accepts a price premium between 5 and 10 for the honest firm A. As already seen in Figure 4.3, if both firms ask for very high prices, the consumer refrains from buying at all, due to her inequity aversion. A different pattern in asymmetry can be seen in other figures off diagonal, as for example, when both managers pay the low wage, but one firm by honestly informing a worker with a wrong guess and the other firm by dishonestly informing a worker with a right guess (first row & third column and third row & first column). The figures show that consumers who are averse to buying from dishonest firms, buy at the honest firm as long as the mark-up is not too high, where the honest firm bears higher production costs, which lead to higher inequity.

An important prediction following from our simple model of consumer choices is that under selfish preferences as well as under Fehr and Schmidt preferences, but absent any aversion from buying at a dishonest firm, dishonest firms are not expected to sell fewer units. However, we expect honest firms to sell more units, if consumers are averse from buying at dishonest firms (as long as $\lambda_b > 0$).[6] To test for consumers' aversion from buying at dishonest firms, we conduct an experimental study with treatments allowing us to disentangle the different effects.

[6] As shown in Section 4.7.2, this prediction also holds if we assume inequality aversion in the spirit of Bolton and Ockenfels (2000).

4.4 Experimental treatments, hypotheses, and implementation

4.4.1 Experimental treatments

In our main treatment *Full Info*, consumers are fully informed about the state S, the workers' guesses and the managers' information to their workers. Thus, consumers know the payoffs of all market participants and can derive the honesty of the managers. Note, that we avoided calling mangers' decisions "honest" or "dishonest", to avoid loaded language and potential experimenter demand effects. Yet, consumers could infer a manger's honesty by comparing the state S, the worker's guess and the manager's information. To investigate the effects of honesty on consumers' choices, we additionally conducted two control treatments varying consumers' information necessary to infer the wages and the managers' honesty. In *Baseline* (*No Info*), consumers just see the prices the two firms ask for. The workers' guesses and the mangers' information to the workers are private information of the firms. Thus, consumers are neither able to infer the wages nor the managers' honesty. In treatment *Wage Info*, consumers are partially informed. For each firm they see the manager's information to the worker and thus the wage paid. However, since the workers' guesses are not revealed, consumers cannot (directly) infer on managers' honesty and thus whether the firm bears high or low production costs.[7] Table 4.1 summarizes the information conditions in the different treatments.[8]

[7] Although no direct inference on honesty is possible, the incentive structure of the game may allow some indirect inference. Since paying the high wage is costly for the manager, consumers may infer that those managers are likely to be honest. A low wage, on the other hand, may result from a dishonest manager or an honest manager with a worker who guessed wrong

[8] Please note that in our setting a treatment, where consumers are informed about honesty, but not about the wages is not possible. As mentioned above we avoided the terms "honest" and "dishonest" in the experiment and consumers could infer honesty by comparing S, the guess and the information. Consequently, the information necessary to detect honesty (S, guess, and information) automatically reveals the wage.

Table 4.1: Information conditions in the experimental treatments

	Consumers receive information on ...			Consumers may thus infer ...	
	S	managers' information	workers' guesses	workers' wages	managers' honesty
Baseline	yes	no	no	no	no
Wage Info	yes	yes	no	yes	no
Full Info	yes	yes	yes	yes	yes

4.4.2 Hypotheses

In *Baseline*, consumers can neither infer the worker's wage nor infer the manager's honesty. Thus, managers have strong incentives to lie to reduce their wage costs. In *Wage Info*, honesty cannot directly be detected, while in *Full Info*, honesty is perfectly observable. In the light of the recent literature, we should not expect all managers to lie to their workers, even if consumers cannot detect it. Various studies show that subjects are averse to lying (Gneezy, 2005; Lundquist et al., 2009; Gneezy et al., 2013) and that the propensity to lie depends on the monetary consequences of lying and is highly heterogeneous among subjects (Gibson et al., 2013). Some subjects are even found to be "pure" lying averse, independent of the monetary benefits from lying (Erat & Gneezy, 2012; López-Pérez & Spiegelman, 2013; Abeler et al., 2014). This leads to:

Hypothesis 4.1. *Not all managers lie when the worker guessed right, independent of the treatment.*

The treatments may influence mangers' honesty. The better consumers can infer mangers' honesty, the more honest managers can be assumed to be. Two effects pointing in the same direction nurture this hypothesis. First, managers having image concerns may be the more reluctant to lie, the more transparent lying becomes (see, for example, Andreoni & Bernheim, 2009; Ariely, Bracha, & Meier, 2009; Bénabou & Tirole, 2006). Second, managers that deem it possible that consumers may react to honesty, may increase honesty, the better honesty can be detected (see below). This leads to:

Hypothesis 4.2. *Honesty increases in the information consumers receive (Full Info > Wage Info > Baseline).*

There is experimental evidence that subjects are sensitive not only to the final payoff allocation, but also to the procedure that produced the allocation (Frey et al., 2004; Frey & Stutzer, 2005; Bolton et al., 2005). Ohtsubo et al. (2010) show that "dishonesty provokes costly third-party punishment".[9] Thus, we may expect that consumers not only take the final payoff distribution into account, but also whether or not the allocation was achieved by dishonest actions. Even if the dishonesty is vis-à-vis another participant, consumers may prefer not to buy from a dishonest manager, to avoid participating in immoral behavior and to keep a positive (self-) image while consuming (Bénabou & Tirole, 2006, 2011). These considerations are reflected in positive λ_b, λ_w in our simple model of consumer choice in Section 4.3 and lead to the following:

Hypothesis 4.3. *Consumers with honesty concerns prefer to buy from an honest manager, as long as the price is not too high. As a result, honest managers may sell significantly more units than dishonest mangers (compare Section 4.3).*

The effects described in hypotheses 4.2 and 4.3 impact on the wages paid. Since increased honesty rates, in particular concerning the black lie (informing a worker who guessed right that he guessed wrong), lead to increased wages, it follows:

Hypothesis 4.4. *Wages increase in the information consumers receive (Full Info > Wage Info > Baseline).*

4.4.3 Experimental implementation

To account for learning effects, the stage-game market was repeated for 30 periods with fixed roles and partner matching. During the experiment, assigned letters A and B distinguished firms and consumers were assigned letters X and Y. We framed the market by using the words "manager", "worker", "consumer", "wage", "buying", etc. But, as already mentioned in the treatment descriptions, we avoided

[9] The authors study trust games with uninvolved observers, who can costly punish the trustee. The lower the trustee's transfer, the higher is the punishment he receives. For all non-maximal transfers, the punishment is higher when the trustee had signaled to send the maximal transfer than when sending no signal.

terms like honesty or lying.[10] The state was called "state of the period" and was the same for all groups in the same session.

We conducted six sessions of the experiment in November 2014 at the Cologne Laboratory for Economic Research (CLER). We recruited subjects using ORSEE (Greiner, 2004). Thirty subjects participated in every session, leading to 180 participants overall and ten independent observations (markets of six subjects) for every treatment. At the beginning of the experiment, written instructions[11] were distributed and read aloud. The experimental interaction was computerized using the software z-Tree (Fischbacher, 2007). Sessions lasted between 95 and 120 minutes. Subjects received an initial endowment of 100 points and additional 5 points at the beginning of every period.[12] After the experiment all points were converted into Euro and paid in cash with an exchange rate of 1€ for 30 points plus an additional show-up fee of 2.50€. Average total earnings are 19.86€.

4.5 Results

We present the results of our experiment in this section. First, we analyze the treatment differences regarding the managers' honesty (Section 4.5.1), before we present a deeper analysis of consumers' decisions (Section 4.5.2). In Section 4.5.3, we finally ask whether consumers' behavior results in higher payoffs for honest managers. In the following, all comparisons between treatments use the two-sided Mann-Whitney U test (MWU) and all comparisons within treatments use the two-sided Wilcoxon signed-rank test (WSR), unless stated otherwise. Table 4.2 provides an overview on the aggregated market outcomes.

[10] The managers were instructed to send information to the worker, whether the guess was right or wrong and that this signal does not have to be true.

[11] English translations can be found in Section 4.7.3.

[12] The initial endowment and the roundly endowment could cover potential losses which are possible for subjects in the roles of managers (from not trading or trading at prices lower than costs). It never happened that a subject had a negative account in one point of time.

Table 4.2: Aggregated market outcomes

	Baseline	**Wage Info**	**Full Info**
Wage paid	3.65 (0.19)	4.40 (0.22)	4.60 (0.18)
Price offered	12.22 (2.14)	10.42 (1.04)	11.46 (1.21)
Worker is right	0.52 (0.02)	0.51 (0.02)	0.53 (0.02)
Manager signals "right"	0.16 (0.05)	0.35 (0.06)	0.40 (0.06)
Manager's honesty (overall)	0.63 (0.06)	0.79 (0.05)	0.81 (0.05)
Manager's honesty (if worker is wrong)	0.99 (0.01)	0.95 (0.03)	0.93 (0.02)
Manager's honesty (if worker is right)	0.31 (0.10)	0.65 (0.10)	0.70 (0.09)
Units sold per firm	0.99 (0.01)	1.00 (0.00)	0.99 (0.00)
Payoff Manager	4.26 (1.96)	1.63 (0.94)	2.91 (1.07)
Payoff Consumer	18.97 (2.06)	21.02 (1.12)	19.42 (1.04)

Notes: The table reports averages and standard errors (in parentheses) based on independent observations.

4.5.1 Treatment effects on wages and honesty

Since guessing right is pure chance, we should expect that workers guess correctly in about 50% of the cases. As row 3 in Table 4.2 shows, this is indeed the case.[13] Thus, in half of the cases workers "deserve" a low wage of 3, while in the other half they "deserve" a high wage of 7, resulting in an average wage of 5. As shown in the first row of Table 4.2, wages are lower than 5 and increase with the amount of information provided in the treatment. The average wage is lowest in Baseline (3.65), significantly lower than in Wage Info (4.40, p=0.034) and Full Info (4.60, p=0.007). There is no statistical difference between the wages in Wage Info and Full Info (p=0.325). Thus, we partly confirm Hypothesis 4.4.

As shown in the sixth row of Table 4.2, the wage effects are not driven by the cases in which the worker guessed wrong. In these cases, managers almost always inform honestly and pay the low wage.[14] Thus, we hardly observe any white lies. The main treatment difference is in the black lies, where honesty is "expensive" for the manager. If the worker guessed right, the managers honestly pay the high wage in only 31% of the cases in Baseline. This is significantly lower than in Wage

[13] Workers guesses are statistically not different from chance, two-sided binomial probability test: *Baseline* p=0.307, *Wage Info* p=0.713, *Full Info* p=0.153.

[14] Differences between honesty if worker guessed wrong: *Baseline* vs. *Wage Info* p=0.478, *Baseline* vs. *Full Info* p=0.075, *Wage Info* vs. *Full Info* p=0.518.

Info (65%, p=0.049) and Full Info (70%, p=0.023). There is no difference between Wage Info and Full Info (p=0.734). Thus, we partly confirm Hypothesis 4.2.[15]

Honesty rates differ between managers (see Figure 4.5 in the Section 4.7.1). In all three treatments, there are managers who are always honest if the worker guessed right and managers who are always dishonest in that case. In *Baseline* when honesty cannot be observed by consumers, about 15% of all mangers are always honest. Although it looks as if the more information consumers receive, the lower is the fraction of managers that always lie and the higher is the fraction of managers that are always honest, these effects are not significant.[16] Thus, we see lying aversion, as formulated in Hypothesis 4.1.

A logistic panel regression on managers' honesty in Table 4.3 summarizes the effects from the non-parametric analysis. Independent variables are the interaction of the treatment condition and the worker's guess. The base category is a wrong guess in Baseline, where the manager is honest with about 99% probability (see Table 4.2). The first two coefficients show the decrease in the likelihood of the manager's honesty if the worker guessed wrong in the two treatments, here also weakly significant for *Wage Info*. It might be that managers assume that paying a high wage is a competitive advantage when consumers can observe it, so that managers lie in this case to pay a high wage. However, as will be shown in Table 4.6, a white lie does not increase consumers' likelihood to buy. The next three coefficients show a highly significant decrease in the likelihood of honesty if the worker guesses right in all three treatments. But, this effect is significantly stronger in *Baseline*, while the coefficients between *Wage Info* and *Full Info* are not statistically different.[17]

Result 4.1. *In all treatments, there is some degree of honesty and a fraction of mangers that never lies (compare Hypothesis 4.1). Information significantly*

[15] Of course, our design does not allow to strictly separate pure lying aversion, since every signal is connected to the wage and therefore has a distributional implication.

[16] The share of managers who never tells a black lie is 15% in *Baseline*, 30% in *Wage Info* and 35% in *Full Info*. There is no statistical difference between treatments (Fisher's exact test 2-sided, "always honest" vs. "not always honest" if worker guesses right): *Baseline* vs. *Wage Info* p=0.451, *Baseline* vs. *Full Info* p=0.273, *Wage Info* vs. *Full Info* p=1.000.

[17] Wald test: Worker guesses right × Baseline=Worker guesses right × Wage Info, p=0.0015; Worker guesses right × Baseline=Worker guesses right × Full Info, p=0.0001; Worker guesses right × Wage Info=Worker guesses right × Full Info, p=0.4665.

Table 4.3: Managers' likelihood to be honest

Worker guesses wrong × Wage Info	-1.660^{*}
	(0.892)
Worker guesses wrong × Full Info	-1.765^{**}
	(0.891)
Worker guesses right × Baseline	-6.689^{***}
	(0.635)
Worker guesses right × Wage Info	-4.535^{***}
	(0.856)
Worker guesses right × Full Info	-4.062^{***}
	(0.859)
Period	-0.017^{*}
	(0.009)
Constant	5.771^{***}
	(0.740)
n	1800
groups	60
log l	-572.87
chi^2	250.26

Notes: Random-effects logistic regression, standard errors in parentheses, clustered by manager id: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Dependent variable: Honest.

increases the overall honesty compared to Baseline (compare Hypothesis 4.2). Due to the increased honesty, wages are higher in the information treatments, compared to the Baseline (compare Hypothesis 4.4). Interestingly, there are no differences between the two information conditions regarding honesty or wages. The partial information on honesty that may be inferred in Wage Info seems sufficient to increase wages and honesty over Baseline.

4.5.2 Consumer choices

The two consumers may or may not buy, and when they buy, they can choose between two potentially different offers. Table 4.4 shows the difference between offers that are accepted (at least one consumer buys) and offers that are rejected (no consumer buys). In all treatments, prices of accepted offers are considerably and significantly lower than the prices of rejected offers. While there is no difference between wages of accepted and rejected offers in *Baseline* and *Full Info*, the wages of accepted offers are weakly significantly higher in *Wage Info*. In *Full Info*, the average honesty is significantly higher for accepted than for rejected offers.

Table 4.4: Average managers' offers dependent on sells

Treatment	Variable	Accepted (sold 1 or 2)	Rejected (sold nothing)	Differences (WSR)
Baseline	Price	10.92 (2.07)	14.02 (2.25)	p=0.005 ($n = 10$)
	Wage	3.59 (0.20)	3.74 (0.22)	p=0.285 ($n = 10$)
	Honesty	0.60 (0.06)	0.67 (0.06)	p=0.028 ($n = 10$)
Wage Info	Price	8.89 (1.11)	12.71 (0.99)	p=0.005 ($n = 10$)
	Wage	4.69 (0.29)	3.94 (0.20)	p=0.075 ($n = 10$)
	Honesty	0.79 (0.06)	0.79 (0.05)	p=0.959 ($n = 10$)
Full Info	Price	10.42 (1.08)	13.14 (1.44)	p=0.005 ($n = 10$)
	Wage	4.68 (0.19)	4.38 (0.29)	p=0.333 ($n = 10$)
	Honesty	0.85 (0.04)	0.72 (0.07)	p=0.032 ($n = 10$)

Notes: The table reports averages and standard errors (in parentheses) based on independent observations.

Table 4.5 provides information on market constellations that deserve special attention. In *Baseline*, we focus on the cases in which the two firms ask for different prices. In those cases, consumers buy the cheaper good in 96% of the cases. In *Wage Info*, we focus on situations where firms pay different wages. Although one firm pays the low and the other firm pays the high wage, the prices asked are almost identical. Yet, the firm paying the high wage sells more units and has a higher profit. In *Full Info*, we focus on two interesting cases, which provide first impressions on whether consumers reward honesty. We already discussed these cases in the interpretation of Figure 4.3. The first case features the situation that both workers guessed right; yet one manger honestly pays the high wage, while the other manager is dishonest and pays the low wage. The table shows that the honest manager, who has to bear the higher wage costs, asks for a considerably higher price (9.29) than the dishonest manger (6.71). Nonetheless, the honest manager sells considerably more units (1.46) than the dishonest manager (0.54), sufficient to make a considerably higher profit (5.00) than the dishonest manager (1.50). The second case features the situation that both managers pay the low wage. While one manager is honest and pays the low wage to a worker that guessed wrong, the other manger dishonestly pays the low wage to a worker who guessed right. Both managers ask for very similar prices (11.16 and 12.21, respectively), despite their different total production costs (9 and 3, respectively). The table shows that the honest manager sells considerably more units (1.42) than the dishonest

Table 4.5: Consumer choices for special market constellations

Baseline	**Price**	**Sells**	**Market share**	**Payoff**
low price vs.	12.12 (0.44)	1.90 (0.02)	96 %	16.55 (0.87)
high price	15.66 (0.48)	0.08 (0.02)	4 %	−5.52 (0.35)
Wage Info	**Price**	**Sells**	**Market share**	**Payoff**
high wage vs.	11.53 (0.52)	1.27 (0.08)	64 %	5.83 (1.14)
low wage firm	11.51 (0.59)	0.73 (0.08)	36 %	−1.32 (0.91)
FullInfo	**Price**	**Sells**	**Market share**	**Payoff**
right guess, honesty vs.	9.29 (1.08)	1.46 (0.12)	73 %	5.00 (1.60)
right guess, dishonesty	6.71 (0.84)	0.54 (0.12)	27 %	1.50 (1.40)
wrong guess, honesty	11.16 (0.80)	1.42 (0.11)	74 %	6.05 (1.65)
right guess, dishonesty vs.	12.21 (1.23)	0.51 (0.10)	26 %	1.30 (1.07)

Notes: The table reports averages and standard errors (in parentheses) based on individual observations.

manager (0.51), sufficient to make a considerably higher profit (6.05) than the dishonest manager (1.30). Both examples already indicate that consumers reward manager's honesty.

The statistical analysis of this observation is provided in Table 4.6. Here, we report an alternative-specific conditional logit model (McFadden, 1974). The choice model specifies for every single decision of a consumer in a period (called case) the available alternatives for the consumer, represented by the two potentially different offers.[18] Alternative-specific variables are the attributes of an offer, i.e., price and the information about wage and honesty. The dependent variable is Buy, which equals one for the accepted offer. As a case-specific variable, we include period, which is not significant and not reported.

In all treatments, the price has a highly significantly negative influence. In *Wage Info* the wage has a significant positive influence on consumer choices. The high wage may be interpreted as a strong signal for honesty. In *Full Info*, we compare the four different constellations of manager's honesty and worker's guess. The base category is a wrong guess and a manager that honestly pays the low wage. The third row of Table 4.6 shows that consumers' propensity to buy is not increased if a manager dishonestly pays the higher wage. Hence, consumers' propensity to

[18] We exclude the theoretical third alternative, not buy, form the analysis since it is almost never chosen.

Table 4.6: Consumers' likelihood to buy

	Baseline	**Wage Info**	**Full Info**
Price	−1.664**	−0.964***	−0.949***
	(0.765)	(0.260)	(0.201)
High wage		1.624***	
		(0.436)	
Wrong guess & dishonesty			−0.012
			(0.706)
Right guess & dishonesty			−2.313***
			(0.573)
Right guess & honesty			0.845***
			(0.262)
n	1190	1200	1190
cases	595	600	595
log pseudol	-197.05	-226.86	-270.47
Wald chi^2	5.35	23.34	73.33

Notes: Alternative-specific conditional logit model (McFadden's choice model) for the cases in which consumers buy. Robust standard errors in parentheses, clustered by consumer id: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Dependent variable: Buy. High wage is a binary variable that is one (w=7), when the manager sends the signal that the worker guessed right and it is zero (w=3), when the manager send the signal that the guess is wrong. Case-specific variables (not reported, not significant): Period.

buy is not increased by telling a white lie. But, consumers strongly react to black lies. Consumers' propensity to buy is significantly decreased if managers lie to pay the low wage (compare line 4) and significantly increased if they honestly pay the high wage (compare line 5). Notice that this strong reaction to lying is the more remarkable as we deliberately did not use morally loaded wordings as "lying" or "dishonesty" in the experiment. Consumers received information on the true state S, the worker's guess and the manager's information, and by comparing this information, they could detect manager's dishonesty. It may well be that the effect gets even stronger when using a morally loaded wording.

Result 4.2. *In Full Info when managers' honesty is fully transparent, consumers reward honest and punish dishonest managers in case the worker "deserves" the high wage (compare Hypothesis 4.3). In Wage Info when only wage information is provided, the high wage significantly increases the consumers' likelihood to buy.*

4.5.3 Profitability of being honest

This section sheds light on the question whether the observed consumer behavior makes honesty profitable for managers. Table 4.7 analyzes the cases where honesty is expensive, i.e. paying a high wage to a worker who guessed right. The table shows that in none of the treatments, an honest manger asks for a higher price than a dishonest manager does. In *Baseline*, honesty significantly reduces the manager's profit. When there is only wage information (*Wage Info*), being honest neither reduces nor increases the payoff compared to a dishonest manager. Under *Full Info*, however, honest managers not only sell significantly more units, they also make significantly higher profits.[19] The fact that under *Full Info* honest managers sell significantly more units than dishonest managers, yields support for the assumption of an aversion of being involved in a dishonest trade (i.e. $\lambda_b > 0$ in equation 4.7).

The analyses of the previous subsections showed that wages as well as honesty levels are not significantly different between the two information treatments *Wage Info* and *Full Info*. We explained this by the strong honesty signal a high wage sends in *Wage Info*. Indeed, the payoffs of honest mangers in *Wage Info* and *Full Info* are not significantly different (p=0.821). The difference lies in the dishonest managers: in *Full Info* dishonest managers receive a significantly lower payoff than honest ones, while this is not the case in *Wage Info*. The explanation may be as follows. In *Full Info*, a dishonest manager is unambiguously identified and "punished" by dishonesty averse consumers. In *Wage Info*, however, a low wage is not a clear signal of dishonesty. A low wage may be the appropriate wage for the worker who guessed wrong or resulting from a lie to the worker who guessed right. It seems that this ambiguity is sufficient to preclude dishonest mangers from being punished by dishonesty averse consumers (in dubio pro reo). This observation is in line with experimental findings that punishment is reduced if norm-violators cannot be identified unambiguously (Feess, Schramm, & Wohlschlegel, 2014) and that subjects with a social value orientation underestimate norm violation of others (Irlenbusch & Ter Meer, 2013).

[19] Compared to Table 4.6, in Table 4.7 we compare only honest versus dishonest behavior in all cases (independent of the competitor's choice). See Table 4.8 in Section 4.7.1 for the average sales and the resulting payoffs regarding different competition conditions.

Table 4.7: Average managers' profit when workers guessed right

Treatment	Variable	Low wage (lying)	High wage (honesty)	Differences (WSR)
Baseline	Price offered	12.22 (2.24)	13.76 (2.10)	p=0.169 ($n = 10$)
	Units sold	1.12 (0.06)	0.70 (0.12)	p=0.028 ($n = 10$)
	Profit	9.81 (2.69)	1.70 (1.90)	p=0.005 ($n = 10$)
Wage Info	Price offered	9.86 (1.44)	10.81 (1.04)	p=0.594 ($n = 9$)
	Units sold	0.84 (0.14)	1.17 (0.08)	p=0.441 ($n = 9$)
	Profit	3.34 (1.62)	4.15 (2.04)	p=0.767 ($n = 9$)
Full Info	Price offered	10.27 (1.87)	11.41 (1.36)	p=0.594 ($n = 9$)
	Units sold	0.67 (0.14)	1.14 (0.08)	p=0.021 ($n = 9$)
	Profit	2.11 (1.10)	4.31 (0.94)	p=0.066 ($n = 9$)

Notes: The table reports averages and standard errors (in parentheses) based on independent observations when the worker guesses right. There is a high variance of honest behavior if the worker guesses right. In *Wage Info* and *Full Info* there is one group each where both managers are always honest, so there are only 9 independent observations.

Result 4.3. *Honesty is highly unprofitable when it is not observed by consumers (Baseline). In Wage Info, the profits of honest and dishonest managers are not different. When consumers have Full Info on managers' honesty, however, being honest is significantly more profitable for suppliers than being dishonest.*

4.6 Conclusion

In this chapter, we experimentally study the influence of producers' honesty on consumers' purchasing decisions. We find that when consumers have full transparency on producers' honesty, being honest provides a competitive advantage. Honest firms make higher profits by selling more units, albeit not at higher prices. The synopsis of the treatments shows that this result only holds when producers' honesty is fully transparent. When the market provides strong signals for honesty, but leaves consumers with some uncertainty (as in *Wage Info*), being honest is not more profitable than being dishonest, but also not less profitable. When honesty information is completely absent, honesty is detrimental for the firm's profit (as in the case of *Baseline*). The competitive advantage of honesty in *Full Info* is consistent with the assumption of a disutility from being involved in a dishonest transaction that is independent of any social preferences concerning the final payoff allocation, as assumed in our simple model of consumer choice.

Our findings extend the literature on consumer social responsibility by showing that a firm's honesty – apart from allocative preferences – is an important decision criterion in consumer choice. Yet, they show that in order to exploit the competitive advantage of honesty, the firm (or any party interested in promoting honesty) has to strive for full transparency on the honesty of the market participants.

4.7 Appendix

4.7.1 Figures and Tables

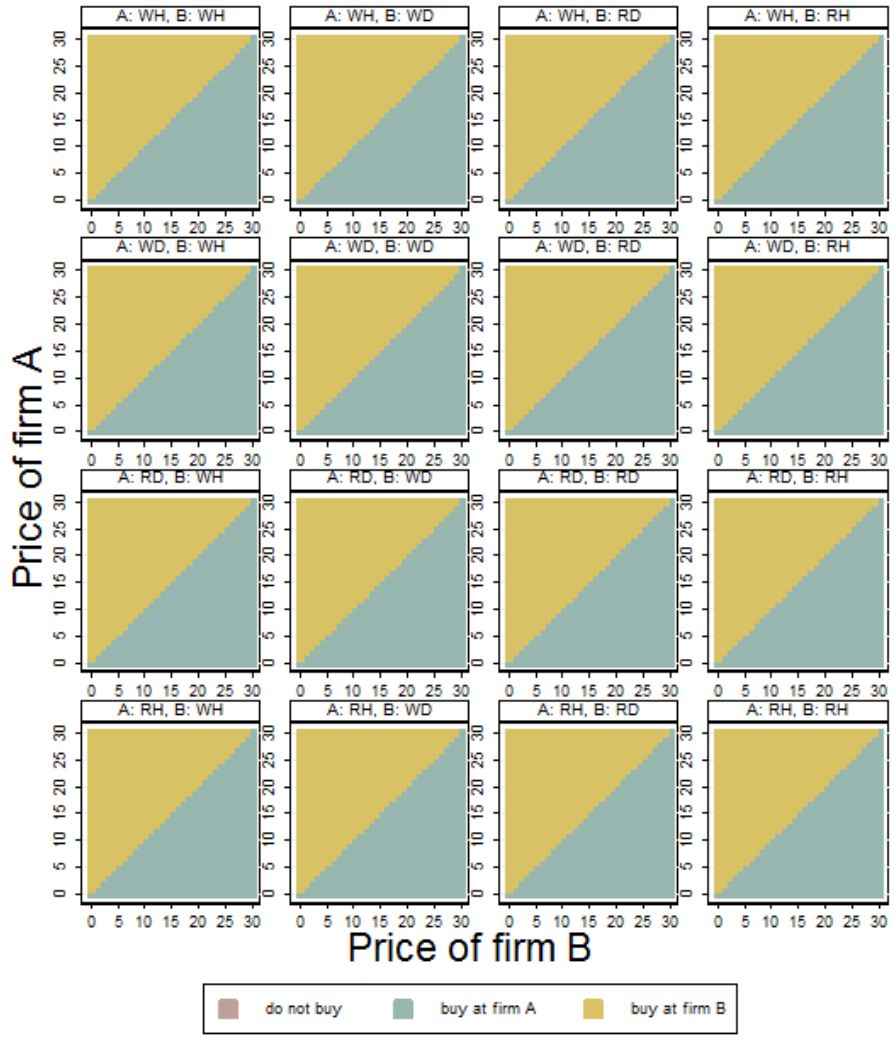


Figure 4.1: Consumption decisions for $(\alpha = 0, \beta = 0, \lambda_b = \lambda_w = 0)$

Figure 4.1 displays the consumer decisions for a consumer with utility function (4.5) and selfish preferences $(\alpha = 0, \beta = 0, \lambda_b = \lambda_w = 0)$. Every square represents all price combinations of a specific market situation, for example, in first row, fourth column: "A: RH, B: WH" means that the worker of firm A guessed right (R) and manager A honestly (H) paid the high wage, and in firm B the worker

guessed wrong (W) and the manager honestly (H) paid the low wage. The figure shows that the consumers do simply buy at the low price firm.

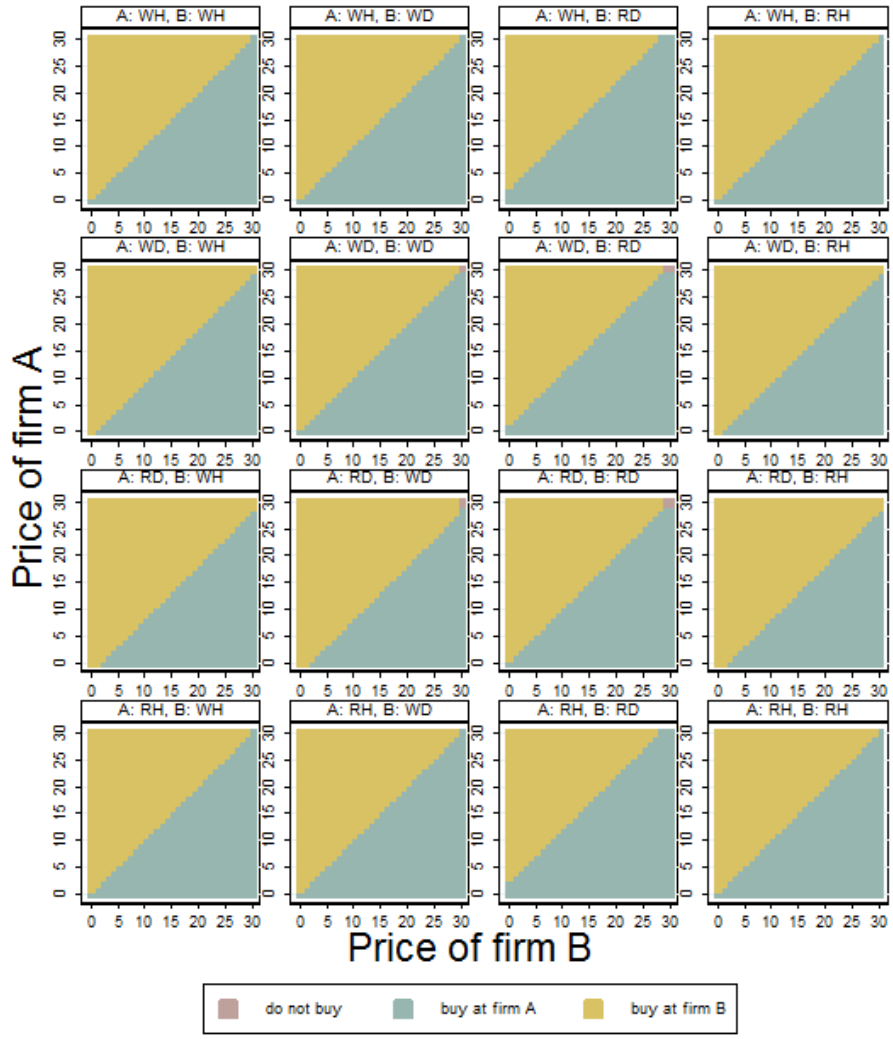


Figure 4.2: Consumption decisions for $(\alpha = 0, \beta = 0, \lambda_b = 0.5, \lambda_w = 0.1)$

Figure 4.2 displays the consumer decisions for a consumer with utility function (4.5), who is interested in his own monetary income and is dishonesty averse ($\alpha = 0, \beta = 0, \lambda_b = 0.5, \lambda_w = 0.1$). Dishonesty makes the product more "expensive" for the consumer and has the same effect as a price increase by $4 \cdot \lambda_b$ in case of a black lie and $4 \cdot \lambda_w$ in case of a white lie.

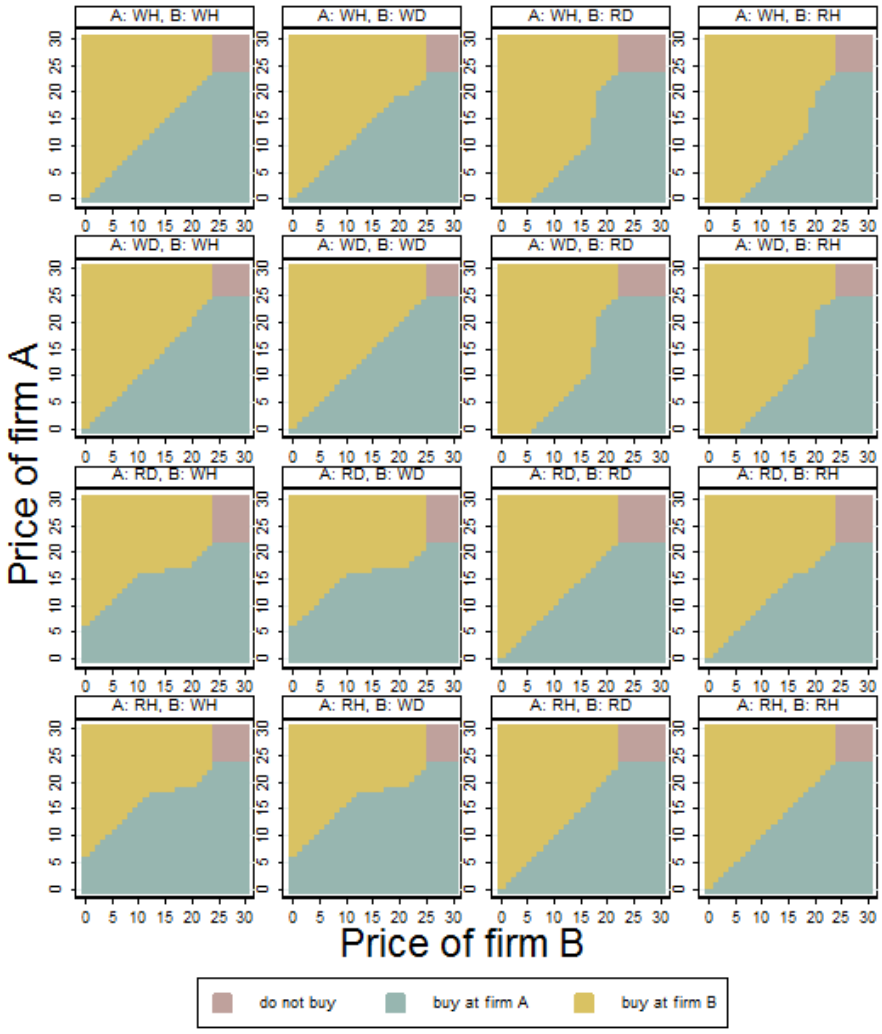


Figure 4.3: Consumption decisions for $(\alpha = 1.5, \beta = 0.5, \lambda_b = \lambda_w = 0)$

Figure 4.3 displays the consumer decisions for a consumer with utility function (4.5), assuming very high inequity aversion and no disutility from buying at a firm with a dishonest manager $(\alpha = 1.5, \beta = 0.5, \lambda_b = \lambda_w = 0)$.[20] The figure shows that consumers do not buy if both prices are relative high, since the disutility of disadvantageous inequality is higher than the own monetary payoff. The consumer buys at the firm which offers the lower price, if both firms have the same production costs (both workers guessed right or both workers guessed wrong). Note, in these cases with price ranges expected in a Bertrand duopoly, the inequity averse

[20] For experimental results on the distribution of α and β see, for example, Fehr and Schmidt (1999); Blanco et al. (2011).

consumer does not behave different from the selfish consumer. In case of different production costs, the consumer prefers there firm were the worker guessed right, if the prices are not too high (lower than or equal to 16.5) and price differences not higher than 6. In these cases the consumer has the highest payoff and inequity is reduced in firms with lower production costs. Note, again the consumer does not care for honesty. When the consumer has the highest payoff, "honesty" only distributes income between manager and worker, but does not influence the total disutility from inequity

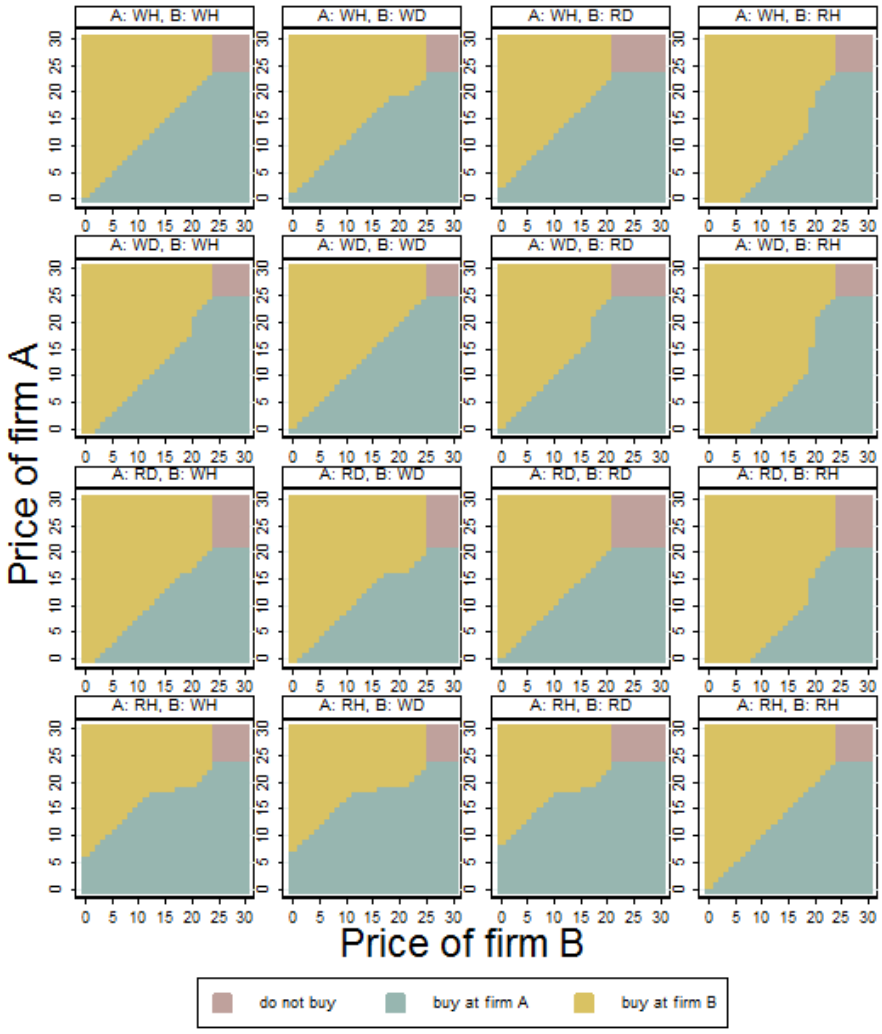


Figure 4.4: Consumption decisions for $(\alpha = 1.5, \beta = 0.5, \lambda_b = 0.5, \lambda_w = 0.1)$

Figure 4.4 displays the consumer decisions for a consumer with utility function (4.5), assuming inequity aversion as well as disutility from buying at a firm with

a dishonest manager ($\alpha = 1.5, \beta = 0.5, \lambda_b = 0.5, \lambda_w = 0.1$). The figure shows that the consumer does not buy when both prices are very high and buying would result in highly unequal payoffs between consumer, manager and worker. If two offers are similar regarding wage and honesty, consumers choose the cheaper offer. But, consumers prefer honest firms over dishonest firms, see for example fourth row, third column. The honest (high wage) firm A even sells at considerably higher prices than the dishonest firm B.

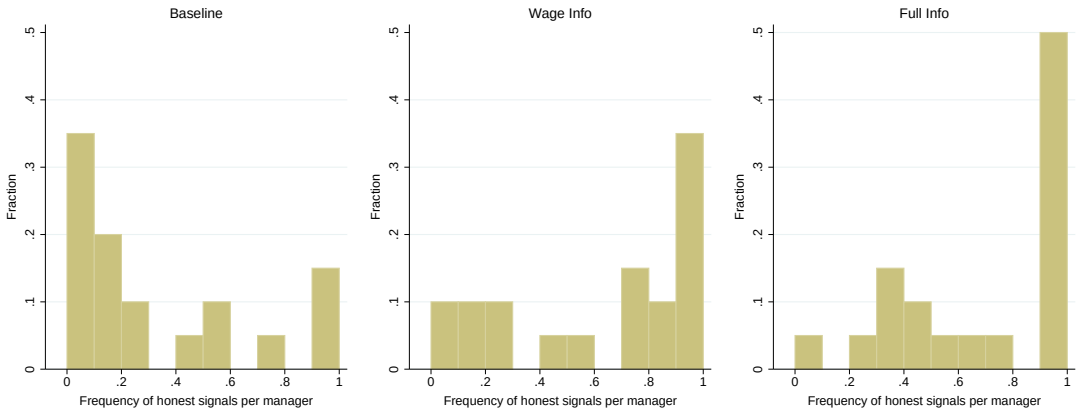


Figure 4.5: Distribution of managers' honesty if worker guesses right

Table 4.8: Managers' sales and payoffs

		Wage Info		
		Lower Price	Equal Price	Higher Price
Lower Wage	Sales	1.40 (0.14)	0.43 (0.16)	0.04 (0.04)
	Payoff	5.41 (2.13)	−4.07 (1.04)	−7.29 (0.75)
Equal Wage	Sales	1.93 (0.03)	1.00 (0.10)	0.07 (0.03)
	Payoff	10.31 (1.51)	1.34 (1.11)	−6.76 (0.62)
Higher Wage	Sales	1.96 (0.04)	1.57 (0.16)	0.60 (0.14)
	Payoff	10.73 (2.51)	7.30 (2.45)	−2.28 (1.72)
		Full Info Info		
		Lower Price	Equal Price	Higher Price
Lower Wage	Sales	1.27 (0.15)	0.64 (0.14)	0.00 (0.00)
	Payoff	5.78 (1.79)	−1.61 (1.77)	−9.00 (0.00)
Equal Wage	Sales	1.59 (0.10)	1.00 (0.13)	0.37 (0.10)
	Payoff	7.95 (1.60)	2.25 (1.42)	−3.87 (1.05)
Higher Wage	Sales	2.00 (0.00)	1.36 (0.14)	0.73 (0.15)
	Payoff	10.60 (3.04)	5.66 (2.20)	−0.69 (2.27)
		Full Info Info		
		Lower Price	Equal Price	Higher Price
Lower Honesty	Sales	1.12 (0.15)	0.38 (0.18)	0.01 (0.01)
	Payoff	3.02 (1.66)	−1.19 (0.90)	−8.34 (2.22)
Equal Honesty	Sales	1.74 (0.08)	1.00 (0.08)	0.25 (0.08)
	Payoff	10.40 (1.57)	2.54 (1.37)	−5.89 (0.88)
Higher Honesty	Sales	1.94 (0.04)	1.63 (0.18)	0.87 (0.15)
	Payoff	8.57 (2.45)	4.19 (3.04)	1.35 (2.18)

Notes: The table reports averages and standard errors (in parentheses) based on independent observations. Managers' sales and payoff depending on offering a lower, equal or higher price and on the differences in wage and honesty.

4.7.2 The consumer's choice model with ERC preferences

In Section 4.3, we assume that consumers with social preferences have preferences of the Fehr and Schmidt type. In the following we show that we achieve qualitatively similar results, if we instead assume that consumers have ERC-preferences (Bolton & Ockenfels, 2000) towards the firm the consumer purchases from.[21] For formulating the ERC-preferences, we assume a utility function of the functional form as proposed in Bolton and Ockenfels (2000). Assume a consumer's utility from the monetary allocation to be as follows:

$$M(m,x) = \begin{cases} 0 & \text{if } x = 0 \\ (30 - p_A) - \frac{1}{3}\gamma \left(\frac{30-p_A}{30-c_A}\right)^2 & \text{if } x = A \\ (30 - p_B) - \frac{1}{3}\gamma \left(\frac{30-p_B}{30-c_B}\right)^2 & \text{if } x = B \end{cases}$$

If the consumer does not buy ($x = 0$), she only cares about her own monetary income that is zero. If the consumer buys at firm A, $(30-p_A)$ is the own monetary payoff from buying at firm A and the term $(30 - c_A)$ is the total monetary payoff from buying at firm A. The γ-term expresses the disutility from comparing the consumer's relative monetary payoff to the equal split. Exemplarily, we choose $\gamma = 30$, as this is the highest possible total payoff (if the worker guessed right).

Figure 4.6 visualizes consumer decisions for consumers with ERC-preferences without any disutility from buying at a dishonest firm. If both firms have the same production costs, the total monetary output from buying is independent of where the consumer buys and thus the consumer prefers the cheaper offer. Only if both firms offer the maximal price, the consumer prefers not to buy to avoid the disutility from inequity. If both firms have different production costs, the consumer prefers the low cost firm and accepts a price premium if both prices are very low to reduce disutility from inequity (similar to Fehr and Schmidt preferences). The honest or dishonest behavior of managers does not influence consumers' decisions since this does not change the consumers' payoff shares.

Figure 4.7 shows consumer decisions for an ERC consumer with a disutility from buying at a dishonest firm. If the consumer additionally has preferences for buying at an honest firm, there are two differences. First, the "area of rejection" at high prices is increased due to the disutility from buying at dishonest firms. Second, consumers accept a price premium for honest firms. See, for example, row 4-column 3, the consumer prefers the offer of (honest) firm A even if the price is a little higher than the price of (dishonest) firm B.

[21] For simplicity, we assume again that a consumer does not care for all agents on the market, but for those she interacts with.

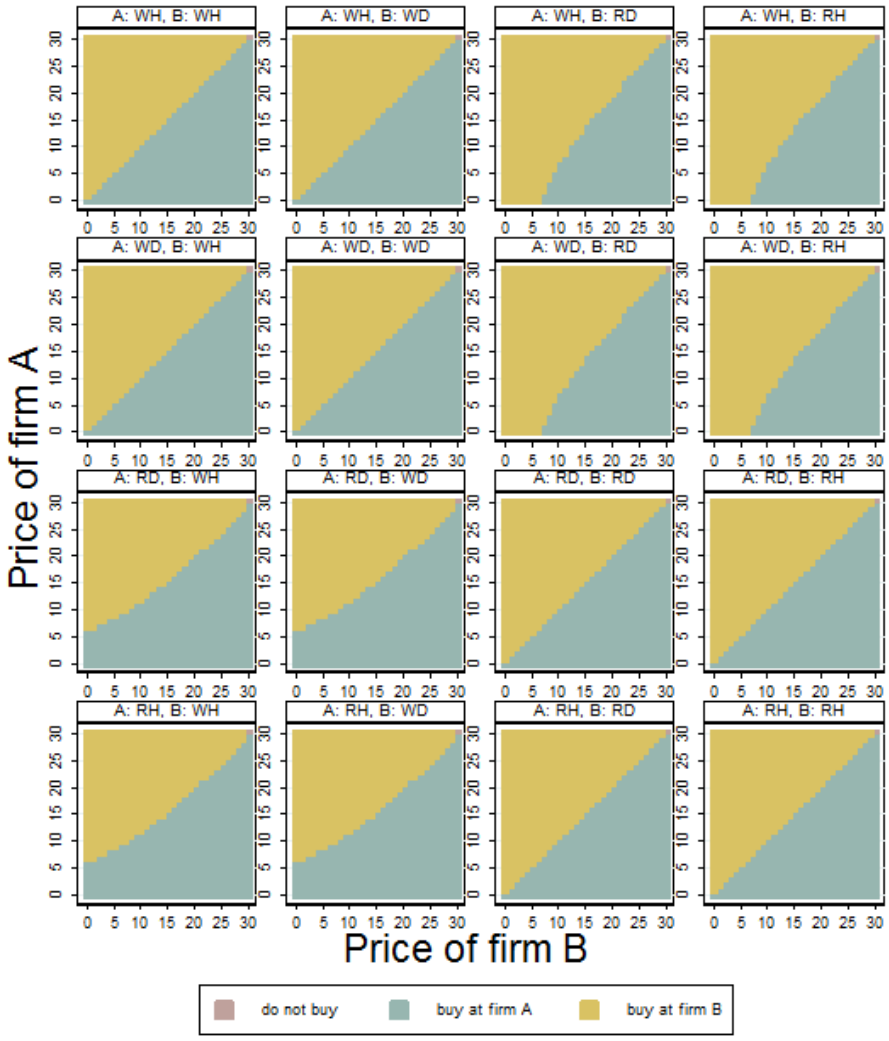


Figure 4.6: Consumption decisions for $(\gamma = 30, \lambda_b = \lambda_w = 0)$

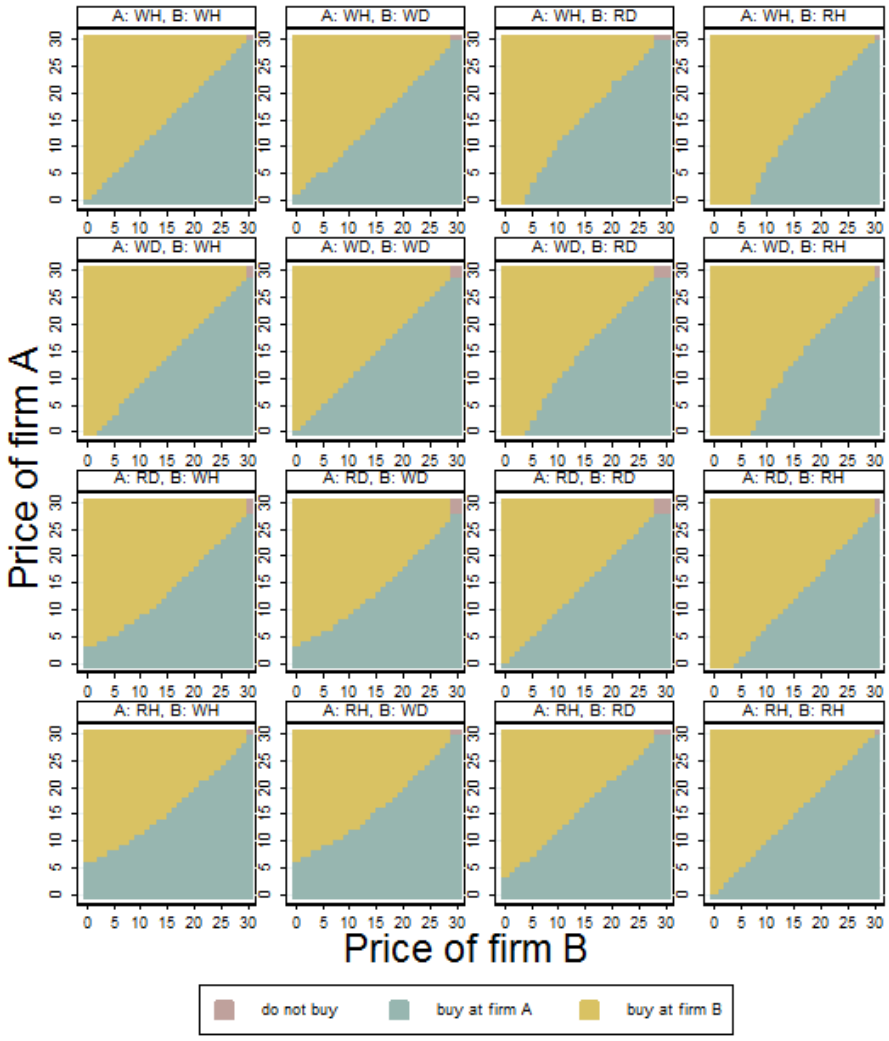


Figure 4.7: Consumption decisions for $(\gamma = 30, \beta = 0.5, \lambda_b = 0.5, \lambda_w = 0.1)$

4.7.3 Instructions

Here we present the instructions for the treatment Full Info. It serves as examples as all instructions are formulated in a similar way. The only treatment differences are the information conditions (given by bullet 1 in "Step 4: Actions of the Consumers"). The original instructions were in German and the one listed below is a translation into English.

Instructions for the experiment

General information We welcome you to this economic experiment. It is very important that you read the following explanations carefully. If you have any questions, please direct them to us.

In this experiment, you can earn money depending on your own decisions and the decisions of the other participants.

During the experiment, you are not allowed to talk with other participants of the experiment. Non-compliance with this rule results in exclusion from the experiment and all payments. All decisions are taken anonymously, i.e., none of the other participants learns the identity of the participant who has taken a specific decision. Payment is anonymous as well, i.e., no participant learns the payments of the other participants.

During the experiment, your entire income is calculated in points. In the end of the experiment, the total number of points that you earned during the experiment is converted into Euro, where

$$40 \text{ points} = 1 \text{ Euro}.$$

At the end of today's experiment, you receive the number of points earned during the experiment plus 2.50 € show-up fee in cash. In addition, at the beginning of the experiment, you receive an initial endowment of 100 points. On the following pages, we will explain to you the detailed procedure of the experiment.

Information about the experiment

Course of the experiment

- The experiment consists of 30 rounds, and each round has the same structure.
- You are part of a group with 6 members. During the entire experiment, you exclusively interact with the members of your group. The composition of the group remains the same across all rounds.

Companies, managers, workers, and consumers

- There are two companies (A and B) and two consumers (X and Y).
- Each of the two companies consists of a manager and a worker. At the beginning of the experiment, it is randomly determined which manager and which worker form company A, and which manager and which worker form company B. This assignment remains unchanged over the entire course of the experiment.
- Which role you are assigned to is randomly determined at the beginning of the experiment, and remains unchanged during the entire course of the experiment. Please note that your role allows for no conclusions about your identity.

Production

- In both companies the worker produces several units of an identical **good**.
- Each round is randomly assigned a **state**: "Red" or "Blue" with a probability of 50% each. At the beginning of each round, the company's worker guesses the current state. If the worker's guess is correct, the company has **no production costs**. If the worker's guess is wrong, the company has **production costs of 6 points**. The workers do not learn the true state of the round.

Production Costs

	State "Red"	State "Blue"
Worker guesses "Red"	0	6
Worker guesses "Blue"	6	0

- A company's manager learns the true state of the round. The manager informs the worker if his guess was correct or wrong. This information can be, but it does not have to be true. If the manager informs the worker that his **guess was correct**, he will pay him a **wage of 7 points**. If the manager informs the worker that his **guess was wrong**, he will pay him a **wage of 3 points**.

Wage

Manager informs the worker that his guess was correct	7
Manager informs the worker that his guess was wrong	3

- A company's manager determines the **price** at which the company offers the good (integers **between 0 and 30 points**).

Purchase

- Each consumer can buy **up to one unit** of the good and decides whether he buys the goods from company A, from company B or whether he does not buy the good. The good has a **value of 30 points** to each of two consumers.
- If a consumer buys the good from company A, he will pay the price set by the manager of company A. If a consumer buys at company B, he will pay the price set by the manager of company B. By a purchase, a consumer receives 30 points less the price paid. A consumer who does not buy a good receives 0 points.

Consumer Profit

Consumer buys a unit of the good	30 - price
Consumer does not buy a unit	0

- The number of goods a company sells depends on the purchasing decisions of the two consumers. Therefore, a company can sell none, one or two units.

Income in each round Each participant receives a **round endowment** of **5 points** in each round. The rest of the income depends on the decisions as follows:

- The number of goods a company sells depends on the purchasing decisions of the two consumers. Therefore, a company can sell none, one or two units.

Income of the participants in a round

Manager:
Income = Round endowment
+Price of the good × Number of units sold
-Productions costs $\begin{cases} 0 & \text{if the worker guessed correctly} \\ 6 & \text{if the worker guessed wrong} \end{cases}$
-Wage $\begin{cases} 7 & \text{if the manager informs the worker that he guessed correctly} \\ 3 & \text{if the manager informs the worker that he guessed wrong} \end{cases}$

Worker:
Income = Round endowment
+Wage $\begin{cases} 7 & \text{if the manager informs the worker that he guessed correctly} \\ 3 & \text{if the manager informs the worker that he guessed wrong} \end{cases}$

Consumer:
Income = Round endowment
+Consumer Profit $\begin{cases} 30 - \mathit{Price} & \text{if the consumer buys} \\ 0 & \text{if the consumer does not buy} \end{cases}$

Course of the experiment Before the start of the first round, you are informed about your role (manager A, worker A, manager B, worker B, consumer X or consumer Y). All rounds take place according to the following scheme:

- Step 0: State of the round
  - The state of the round is randomly drawn. With a probability of 50%, it is "Red" and with a probability of 50%, it is "Blue".
- Step 1: Actions of the workers
  - The workers guess the state of the round.
  - The workers produce the units of the good.
- Step 2: Actions of the workers and the managers
  - The managers learn the state of the round and the guess of their worker.
  - The managers inform the workers whether their guess was correct or not (this information can be, but it does not have to be true) and about the resulting wage.
  - The managers determine the price of the good.
- Step 3: Actions of the consumers
  - The consumers learn the state of the round, the workers' guesses as well as the information sent by the managers to the workers whether their guesses were correct or not and the resulting wages.
  - The consumers learn the price of the good of company A and the price of the good of company B.
  - The consumers decide whether and at which company they buy a unit of the good.
- Step 4: Information
  - The workers receive the information from their managers whether their guess was correct or not as well as the resulting wage.
  - Both managers learn the guess of the respective other worker and receive the information sent by the other manager to the worker whether the guess was correct or not.

Total income

Your total income is the sum of the incomes of all rounds plus the initial endowment of 100 points.

Good luck!

5 Reward Power and Lying in Self-Reports

5.1 Introduction

Teamwork is characterized by the use of individual inputs to build a joint output. Since it involves a tension between individual and collective interest, teamwork is prone to the same crucial problem as a social dilemma: potential cooperation breakdown due to free riding of some teammates on others' contributions or efforts (for a comprehensive review of social dilemmas see van Lange, Joireman, Parks, & van Dijk, 2013). Performance-contingent rewards offer a possible solution to overcome shirking in teams (Lazear, 1995). Indeed, organizations often pay their employees performance-based salaries, as many of Fortune 500 companies (Boyle, 2001). Abundant evidence shows that rewards like performance pay have a positive effect on employees' effort if they are closely related to individual performance (for an overview, see, for example, Milkovich & Newman, 2008).

In most firms, it is the supervisors' task to link individual effort and payment. Evaluating individual performance accurately, however, is often difficult or even impossible. Consider, for instance, a supervisor, who has to evaluate the members of a creative team. In such teams, many ideas and solutions arise from sessions of brainstorming and it is difficult to judge who contributed what amount of effort and what the corresponding value is. One possible way to gather information about individual efforts is to ask the teammates for their self-assessments. Indeed, self-reports are frequently used in performance evaluations complementary to other sources, for example in multi-source or 360-degree feedback processes (Campbell & Lee, 1988; Chen & Kemp, 2012).

The empirical evidence from the organizational literature is inconclusive about the effectiveness of self-reports (for a review see Dunning, Heath, & Suls, 2004) and poses the question of whether self-reports really do add any value to an effective performance evaluation. Furthermore, it is not clear as to how self-reports ultimately affect a team's performance (Shore & Tashchian, 2002). Previous questionnaire-studies from the organizational literature show that teammates tend to overestimate their own efforts or even deliberately lie about their actual efforts (Campbell & Lee, 1988). Recent experimental studies show that a substantial amount of people does indeed lie if self-reports have monetary consequences and cannot be verified (Fischbacher & Föllmi-Heusi, 2013). On the other hand, many people show a lying aversion that varies to different extents (see, for example, Erat & Gneezy, 2012; Gibson et al., 2013; Gneezy et al., 2013; Hoffmann, Lauer, & Rockenbach, 2013). A recent representative survey confirms the existence of lying costs in the population (Abeler et al., 2014). The stylized facts from the empirical studies mentioned above is that lying behavior is heterogeneous and context dependent.

In this study, we systematically investigate the effectiveness of self-reports in team production. Our main research question is to what extent the existence and the reward allocation power of a supervisor induces teammates to state more truthful self-reports, compared to a situation in which the supervisor lacks such reward power. To answer our research question, we consider two different mechanisms, which vary with respect to supervisor's reward power. The first mechanism forces the supervisor to allocate performance pay according to an exogenously given rule, which is publicly known ex-ante. One could consider this mechanism as a primitive form of a forced distribution system (FDS). The former CEO of General Electric Jack Welch popularized the use of the FDS that contain rigid instructions how to allocate rewards, for example, "fire the least productive 10% of your personnel". The empirical findings on the effectiveness of such systems, however, are mixed (Stewart, Gruys, & Storm, 2010). While FDS can provide powerful incentives for high performers, they may unsecure middle and low performers and damage their working morale. Another possible detrimental effect of an FDS is that it may render the team supervisor ineffective as a "lame-duck" who has only to follow a rule created by a higher-level authority ("firm policy"). When the reward scheme

is "written in stone" this may lead low performers to cheat heavily in order to avoid an unfavorable classification.

We hypothesize that giving the supervisor (strong) reward power might increase her effectiveness in obtaining more truthful self-reports. In order to investigate this, we consider a second mechanism that enables the supervisor to allocate rewards at his or her own discretion without sending any signals in advance. In this case, it is not straightforward how payment is linked to effort. The uncertainty about the monetary consequences of self-reports reduces the incentives to lie and may lead to more truthful self-reports. Thus, the detrimental effects of self-reports may be less problematic in a setting with a supervisor provided with reward power compared to a setting in which rewards allocation is hard-wired. With the following four steps, we systematically investigate our main research question. (i) To what extent do teammates exaggerate their efforts when the allocation of performance pay is exogenous (forced distribution)? (ii) Is teammates' reporting behavior different when a supervisor with reward power endogenously decides on performance pay compared to a situation when the allocation rule is exogenous (and the supervisor is only an observer)? (iii) Are supervisors able to allocate incentive-compatible payments when receiving self-reports as effectively as when they are able to monitor true individual efforts? (iv) Finally, what is the ultimate effect of self-reports on team performance?

We find: (i) Teammates systematically exaggerate their efforts when reporting. (ii) They exaggerate less when the supervisor has reward power than when the payments are allocated exogenously. (iii) The supervisor's rewarding behavior does not change, irrespective of whether the supervisor observes true individual efforts or self-reports. (iv) The exaggerations in self-reports have detrimental effects on team performance. These effects are less pronounced if the supervisor has reward power and allocates performance pays endogenously than when the payments are distributed exogenously.

In this study, we utilize a controlled laboratory setting to investigate our research question. Laboratory experiments allow the unveiling of casual relationships between the variables of interest. Since previous non-laboratory studies lacked the possibility to observe true efforts, they could not quantify the frequency and magnitude of the exaggerations in self-reported effort information. To understand the

effects and the value of self-reports, however, it is important to know the direction and amount of deviances from true (actual) efforts. We designed this experimental study to detect truthfulness in self-reports in a quantifiable way. To our best knowledge, this is the first experimental study concerning the effects of self-reports on team performance. Additionally, our study contributes to the growing experimental literature on lying and other unethical behavior in the workplace. While there are many papers investigating lying, the literature on lying in team context is scarce. Hence, this study contributes to the lying literature showing lying in a team setting. Finally, the findings of this study are also relevant for the leadership literature.

5.2 A simple model of team production

We consider a work team pretty much in the spirit of Alchian and Demsetz (1972), consisting of three productive teammates and a supervisor. To capture the tension between individual and collective interest in a work team, we utilize a slightly modified public goods game. Each teammate i is endowed with E_T tokens and decides how many tokens to contribute to the team project, i.e., his or her effort choice e_i (with $0 \leq e_i \leq E_T$). The tokens not contributed, $E_T - e_i$, go into the own private account of teammate i. Each token contributed to the team project is multiplied by the productivity factor M, with $\frac{M}{3} < 1 < M$. The team output, R, is the sum of the efforts $W = \sum_{i=1}^{3} e_i$, multiplied by the productivity factor M, i.e., $R = M \cdot W$. To conduct a committed observation and action, the supervisor receives a compensation that partly depends on the team output. The team output is allocated among the three teammates and the supervisor as follows: R is divided into three parts: αR, βR and γR, with $\alpha + \beta + \gamma = 1$. The first part, αR, describes a variable team payment that depends on the overall team performance, and is equally distributed to the teammates. Hence, independent of the individual effort, each teammate receives $\frac{\alpha}{3}R$. One could interpret αR as the non-separable part of team output, which the supervisor cannot attribute to a single teammate. One could interpret αR also as a group-based reward honoring the group's performance as a whole. Firms utilize such rewards frequently (see, for example, Zenger & Marshall, 2000). The second part, βR, represents the individual

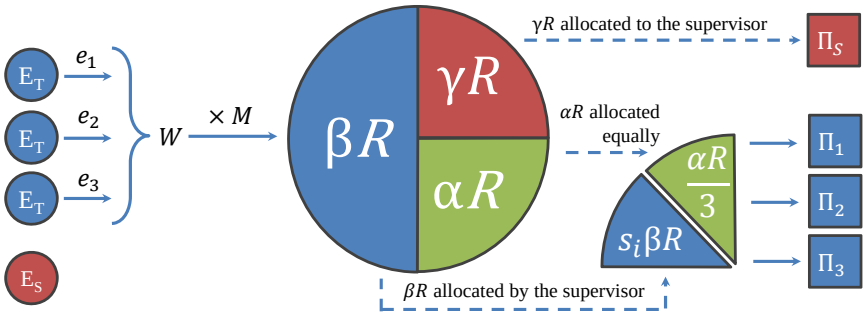


Figure 5.1: Basic model of efforts and payoff in our teamwork setting

performance-contingent pay, the individual reward. Each teammate receives a share s_i of βR, with $\sum_{i=1}^{3} s_i = 1$. The last part, γR, is used to compensate the supervisor. Additionally, the supervisor receives a fixed payment of E_S tokens. Since the compensation of the supervisor depends partly on the team output, the supervisor has an interest in the team output being as large as possible. Figure 5.1 visualizes our model. Equations (5.1) and (5.2) state the payoffs for a teammate and the supervisor respectively:

$$\Pi_i = E_T - e_i + \frac{1}{3}\alpha R + s_i \beta R, \quad i = 1, 2, 3 \tag{5.1}$$

$$\Pi_S = E_S + \gamma R. \tag{5.2}$$

In our design, we set the parameters as follows: $M = 2$, $\alpha = \gamma = \frac{1}{4}$, $\beta = \frac{1}{2}$, $E_T = 20$ and $E_S = 10$.[1] With these parameters, we can re-formulate the payoffs as follows:

$$\Pi_i = 20 - e_i + \frac{1}{6}\sum_{j=1}^{3} e_j + s_i \sum_{j=1}^{3} e_j, \quad i = 1, 2, 3 \tag{5.3}$$

$$\Pi_S = 10 + \frac{1}{2}\sum_{j=1}^{3} e_j. \tag{5.4}$$

[1] We choose these parameter values for two reasons. First, they allow us to keep the marginal per capita return (MPCR) close to the value of most previous experiments on public good provision. In our setup, the MPCR amounts to $(\alpha+\beta)\cdot\frac{M}{3} = \frac{1}{2}$. Second, our set of parameters eases the task of the supervisor. Since the endogenously distributed part of the team-output equals the sum of efforts, the appropriate (incentive-compatible) proportional share is equal to each teammate's effort.

From (5.4), it is easy to see that the supervisor's payoff is maximized if all teammates contribute their full endowment to the team project, i.e., if each teammate exerts full effort. The supervisor can induce maximum efforts by distributing the variable part, βR, of the team output proportional[2] to the teammates' relative efforts, i.e., by choosing the shares such that $s_i = \frac{e_i}{W}$. In this case, a teammate's payoff is

$$\Pi_i = 20 + \frac{1}{6}\sum_{j=1}^{3} e_j. \tag{5.5}$$

As can be seen in 5.5, full effort is a strictly dominant strategy for every teammate. Hence, assuming full information, there is a Nash equilibrium (of the stage game), where the supervisor should allocate a performance pay share to each teammate proportional to his or her effort rate (compared to the team's total effort). A supervisor who simply distributes rewards equally among the teammates, $s_i = \frac{1}{3}$, would change each teammate's profit in (5.3) to $\Pi_i = 20 - e_i + \frac{1}{2}\sum_{j=1}^{3} e_j$. This change transforms the strategic situation in a public good game with a marginal per capita rate of $\frac{1}{2}$ and a unique Nash equilibrium in which all teammates contribute zero. In public good experiments, however, subjects do contribute positive amounts (Chaudhuri, 2011). Thus, under equal distribution of βR, we expect effort levels comparable to previous public good experiments. The positive externality of contributions on the supervisor's payoff in our setting, however, might have an effect on teammates' efforts. Engel and Rockenbach (2011), for instance, find that in a public good experiment with a non-contributing bystander, efforts were lower than in the standard setting without a bystander.

5.3 Experimental design and procedures

The experiment consists of 20 rounds, in which subjects play the game explained above. Before the first round, subjects are randomly allocated into groups of four.

[2]The proportional distribution scheme is not the only scheme that incentivizes the teammates to high effort levels, for example, "the winner takes it all" is another incentive-compatible payment scheme. Yet we consider the proportional allocation to be the most obvious one under the given parameters. The results in section 5.5 show that the great majority of supervisors stick to the proportional distribution rule.

In each group, one subject is randomly assigned as the supervisor and three subjects as teammates.[3] The subjects' roles as well as the group matching remain unchanged during the experiment. Each group constitutes an independent observation.

5.3.1 Treatments

In order to investigate our research questions, we apply a 2×2 full fractional design. First, we vary the information that the supervisor receives. The supervisor is informed about either the true individual efforts or the teammates' self-reports. Second, we manipulate how the performance pay βR is distributed. In the exogenous treatments FORCED and FORCED-SR, rewards are distributed exogenously and proportionally to the actual or reported amounts. In the endogenous treatments FREE and FREE-SR, the supervisor allocates βR at her own discretion. This means, in the FORCED treatments, the supervisor is an observer with no distributive power, and is paid according to the supervisor's payoff function (compare Equation (5.4)). The performance pay shares s_i are determined proportional; either according to the self-reports or to the actual efforts. In the treatment we name FORCED, the supervisor observes the true individual efforts, e_i, so the proportional distribution mechanism implies: $s_i = \frac{e_i}{W}$. In the treatment FORCED-SR, the s_i, are determined according to the self-reports, i.e., $s_i = \frac{r_i}{\sum_{i=1}^{3} r_i}$, with r being the self-report of player i and $\sum_{i=1}^{3} r_i$ being the sum of the reported efforts. In the FREE treatments, the supervisor discretionally decides about s_i. In the treatment we name FREE, before deciding, the supervisor is informed about the true efforts, while in the treatment FREE-SR the supervisor receives the self-reports. Table 5.1 displays our 2×2 experimental design.

In addition to the four treatments introduced above, we conduct a "baseline" treatment named EQUAL, which uses a simple equal split as a FDS, i.e., each teammate's performance pay share equals $s_i = \frac{1}{3}$. This transforms the game into a standard public-goods game, with the supervisor as bystander, since αR as well as

[3] We are aware of the relevance of legitimating the supervisor by implementing some sort of selection process. However, considering different forms of appointment would have added another dimension to our design. Therefore, we use a random selection process, from which we expect the smallest impact on actual efforts and allocation decisions.

Table 5.1: Treatment design

Distribution mechanism	**Supervisor's information based on** Self-reports	Actual efforts
Free allocation: Supervisor has reward power	FREE-SR	FREE
Forced distribution: Supervisor is only an observer	FORCED-SR	FORCED

βR is allocated equally to the teammates. The EQUAL treatment is relevant as it describes an alternative allocation when the supervising authority cannot observe individual efforts. In EQUAL, instead of relying on self-reported information, the teammates are paid equally independent of their (relative) contribution.

5.3.2 Timing of a period

A period starts with the elicitation of subjects' beliefs about the other teammates' effort decisions. After the belief elicitation, each teammate chooses his or her own effort.[4] Then, teammates are informed about the actual (true) efforts of fellow teammates. Following that, except in EQUAL, each teammate states his or her own preferred reward distribution, i.e., each teammate suggests a distribution of the partial team output βR. In the FREE treatments, teammates then provide self-reports about their own effort. Following this, depending on the treatment, the supervisor is either informed about the individual self-reports or the actual efforts. In the two FREE treatments, teammates additionally state their beliefs about the supervisor's distribution decision. In all treatments, the supervisor is informed about the true value of the team's total effort. After receiving the information, in the FREE treatments, the supervisor decides about the individual shares, s_i, while in the exogenous FORCED treatments, the shares are allocated exogenously as described above.[5] At the end of each period, each teammate receives feedback

[4] Subjects had to choose their effort level, as well as the self-report, from the set $\{0, 5, 10, 15, 20\}$. This restricted set simplifies the task for the supervisor and makes exaggerations meaningful. The belief elicitation was not incentivised.

[5] To simplify the allocation decision, the supervisor enters the individual payoff, $s_i \alpha R$, directly instead of calculating s_i for each teammate.

Table 5.2: Timing of a period

Stage	Teammates	Supervisor
1	input: beliefs about teammates' efforts	
2	decision: own effort	
3	feedback: individual efforts	feedback: total effort
4	input: preferred allocation of βR	
5	decision: self-report (SR-treatments)	feedback: efforts or self-reports (dep. on treatment)
6	input: belief about supervisor's allocation (FREE-treatments)	allocation: s_i (dep. on treatment proportional, FORCED, or by supervisor, FREE)
7	feedback: payoffs	

about his or her own payoff in the current period. Table 5.2 summarizes the timing of a period.

5.3.3 Procedures

The experiment was conducted in September 2012 and February 2013 in the Cologne Laboratory for Economic Research (CLER). In total, 156 students from different disciplines participated. We collected eight independent observations (matching groups) per treatment for the treatments FORCED, FORCED-SR, FREE, and FREE-SR, and seven for EQUAL[6]. On their arrival, we informed subjects about the experimental procedure.[7] The experiment was programmed with z-Tree (Fischbacher, 2007), and the subjects were recruited via ORSEE (Greiner, 2004). Each experimental session lasted about 90 minutes, and subjects earned about 18 € on average.

[6]Since some subjects did not show up, we had to run this session with seven groups.

[7]See subsection 5.7.2 for a translation of the instructions. Original instructions are written in German.

5.4 Predictions

Do teammates exaggerate in self-reports?

The accuracy of self-reports in teamwork situations is subject to at least two problems (Campbell & Lee, 1988). First, individuals may not be able to evaluate and report their own efforts objectively and reliably (overestimation). Second, they may deliberately misreport their efforts. In our study, we rule out by design the existence of (unintended) overestimation, since an individual is always able to evaluate his or her own effort perfectly. Hence, in our setting, if a teammate misreports, he or she must do so deliberately. To answer the question of whether teammates do indeed lie about actual efforts, we utilize our treatment, Forced-SR, in which there is a strong incentive to do so. As explained before, in Forced-SR, the supervisor has to pay the teammates proportionally according to self-reports. This forced proportional distribution maximizes the incentives to lie. In other words, for a teammate, it is strictly dominant to report maximum effort. On the other hand, for each teammate, it is rational to set one's actual effort to zero. In light of previous experimental studies, one should expect that the actual efforts will be higher than zero (Ledyard, 1995) and people will not lie to full extent (see, for example, Fischbacher & Föllmi-Heusi, 2013). Given that, it is unlikely to observe extreme decisions of zero contributions and maximum reports. Hence, we state our first hypothesis as follows:

Hypothesis 5.1. *In* Forced-SR *and* Free-SR*, the average level of self-reports are higher than the actual efforts.*

Do teammates lie to a lesser extent if the supervisor has reward power?

Next, we ask whether teammates' reporting behavior might be different when the supervisor is free to decide about the performance pay shares. To investigate this question, we contrast the reporting behavior from the Forced-SR treatment explained above with the behavior from the Free-SR treatment in which the supervisor allocates the rewards at her own discretion after receiving self-reports. Should we expect a different behavior in Free-SR how teammates' choose their reports and actual effort levels, compared to Forced-SR? The answer depends

on teammates' expectations of the supervisor's reward distribution in FREE-SR. If teammates believe that the supervisor in FREE-SR will deviate from the proportional distribution, for example, if she decides to choose an egalitarian distribution, then the teammates no longer have an incentive to report maximum effort. In this case, in fact, teammates are indifferent between reporting the maximum or any other amount. If, however, teammates expect the supervisor to stick to the proportional allocation, then they have an incentive to report maximum efforts – exactly as in the FORCED-SR treatment.

Indeed, the supervisor in the FREE-SR treatment has no reason to apply any other distribution than the proportional one, since all other distributions would decrease her expected payoff compared to the proportional distribution. Why exactly should the supervisor choose a proportional distribution of the performance pay βR? Assume that the supervisor observes that the sum of the reported efforts is higher than the actual total contribution. Since there is no possibility to identify the exact amount of individual deviations in reports from the actual efforts, the supervisor is not able to discriminate between the teammates who lied and those who stated their true effort. By design, the supervisor is also not able to sanction the team as a whole by withholding payments, because the supervisor must allocate the entire amount of βR. An egalitarian allocation of βR is also not feasible, since the equal allocation would transform the game into a public goods game. In public goods games, it is very likely that the team output will be low or decrease over time, thus lowering the supervisor's payoff. Hence, for the supervisor, with a very high probability, deviating from the proportional distribution would lead to worse outcomes than if she applied the proportional allocation. Hence, the teammates in FREE-SR may correctly anticipate the proportional distribution and exaggerate their own efforts maximally.

Hypothesis 5.2. *We expect no differences, either in the average level of reported efforts or in the average level of actual efforts between the treatments* FORCED-SR *and* FREE-SR*. Thus, we do not expect a different level of lying (exaggeration) in* FREE-SR *and in* FORCED-SR*.*

Recent experimental studies show that some people have a lying aversion that varies to different extents as laboratory studies (Vanberg, 2008; Gneezy et al., 2013;

Conrads, Irlenbusch, Rilke, Schielke, & Walkowitz, 2014) as well as a study with a representative sample suggests (Abeler et al., 2014). These "lying costs" could be more salient in the FREE-SR treatment, i.e., when the supervisor deliberately decides on rewards than in the FORCED-SR treatment in which the supervisor does not make such a decision. Hence, subjects might have a stronger reluctance to lie to a supervisor with reward power as in FREE-SR, than to a "lame-duck", observing-only supervisor, as in FORCED-SR. Thus, we formulate an alternative hypothesis:

Hypothesis 5.3. *Teammates exaggerate less in* FREE-SR *than in* FORCED-SR.

Are supervisors able to allocate incentive compatible payments when receiving self-reports as effective as when they are informed about the true individual efforts?

As explained above, to maximize her own payoff, the supervisor should allocate the payments proportional to individual efforts. To conduct an efficient allocation, the supervisor must know the true individual efforts. In the FREE treatment, we provide the supervisor with true information about the individual efforts. Hence, in FREE, the supervisor is able to motivate the teammates to contribute by setting $s_i \beta R = e_i$.

In the FREE-SR treatment, the supervisor receives individual self-reports as feedback. If teammates exaggerate their own efforts, the supervisor is faced with uncertainty. For two reasons, this uncertainty might render her reward allocation ineffective. First, if teammates exaggerate their efforts, then the proportional allocation of payments is not incentive-compatible. Second, since the supervisor is not able to monitor the actual efforts, she cannot condition the payments on efforts in any other incentive compatible way. Hence, with self-reports, we expect the supervisor will be less effective in distributing incentive compatible performance pay than with true effort information. This ineffectiveness might lead to lower actual efforts in FREE-SR than in FREE, in which the supervisor is able to monitor true efforts.

Hypothesis 5.4. *When receiving self-reports, supervisors are less effective in using incentive-compatible rewards than when they receive true effort information.*

What is the net effect of self-reports and supervisor's reward power on team performance?

We expect the highest team performance in the FORCED treatment and the lowest in the FORCED-SR. If the supervisor in the FREE treatment is able to allocate the payments proportional to the individual contributions, the team performance can be expected to be as high as in the FORCED. Otherwise, if the supervisor choses a non-incentive compatible distribution, then the teammates do no longer have an incentive to contribute their full endowment. This may lead to a lower team performance in the FREE treatment. In contrast to that, the supervisor's ability to depart from the proportional distribution may have positive effects in the FREE-SR treatment as compared to the FORCED-SR treatment. This positive effect is twofold. First, the level of reported efforts should be lower, since maximum reports are no longer automatically rewarded. Second, this could encourage honest teammates to contribute, because they are less likely to be exploited by exaggerating teammates. For given beliefs about others' contributions, the FREE-SR treatment may reduce the perceived risk of exploitation, since the supervisor is able to detect extreme exaggerations and may distribute βR accordingly. At the same time, those teammates who exaggerate their effort take the risk of being identified as a liar. This is all the more important, since the supervisor in the FREE-SR could remember previous exaggerations, in contrast to the supervisor in the FORCED-SR, who does not have this possibility.

5.5 Results and Discussion

Subjects exaggerate self-reports when under forced distribution of rewards

We start our analysis by looking at the reporting behavior in the FORCED-SR. Remember, the proportional allocation of performance pay according to self-reports in the FORCED-SR treatment provides a strong incentive to exaggerate. As the left panel of Figure 5.2 shows, teammates in FORCED-SR do indeed exaggerate

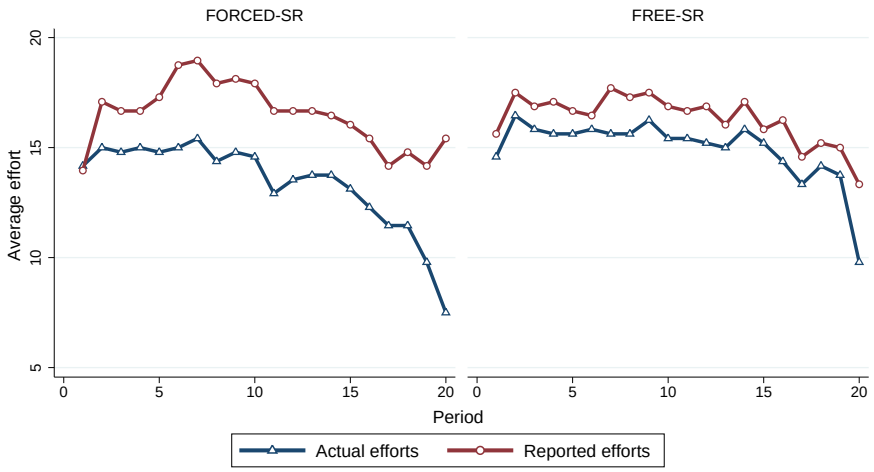


Figure 5.2: Actual and reported efforts

their efforts when reporting.[8] Actual efforts amount on average to 13.4, while self-reports amount to 16.5. The difference between the reported and the actual efforts is of considerable magnitude (3.1) and statistically significant (Pitman-Fisher permutation test[9], one-sided, p=0.008). These numbers provide clear support for our Hypothesis 5.1.

Result 5.1. *If performance payments are allocated exogenously according to the self-reported effort information, then self-reported efforts are significantly higher than the actual efforts.*

Subjects exaggerate self-reports less when rewards are allocated endogenously by the supervisor

Do teammates report differently when the supervisor discretionarily decides on the allocation of performance pay, as in FREE-SR? We see in the right panel of

[8] There is no case, in which a teammate exaggerated her effort when the actual sum of efforts in the group was zero, i.e., there is no case, in which a supervisor was able to detect an individual exaggeration for sure.

[9] We decided to use the Fisher-Pitman test instead of the Wilcoxon rank sum test. While both tests are permutation tests, the Wilcoxon test is based on ranks and therefore ignores a substantial part of the information in the sample data. The Pitman-Fisher permutation test (FPP test) uses the more powerful approach based on the original sample values without transformation (compare Kaiser, 2007).

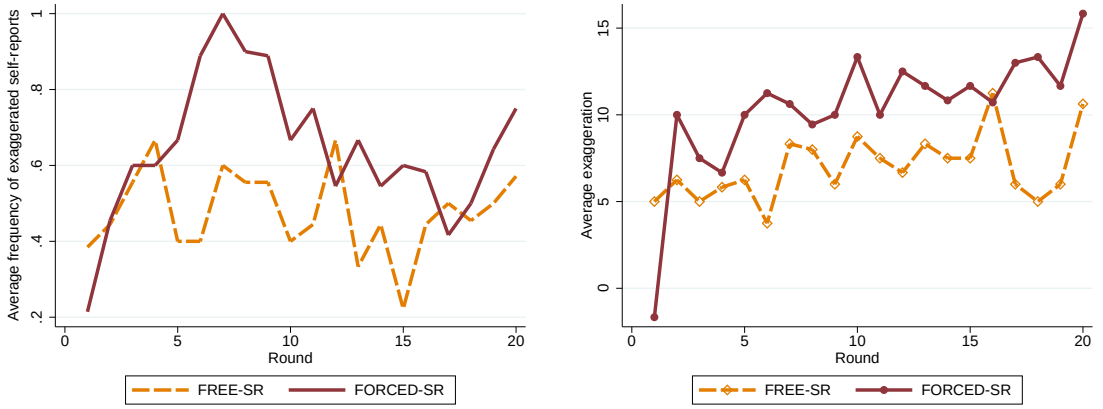
Figure 5.2, the pattern of reporting behavior is similar to the FORCED-SR, as predicted in our Hypothesis 5.2. As in FORCED-SR, in the FREE-SR treatment, self-reported efforts (16.3) are significantly higher than the actual efforts (14.9, FPP test, one-sided, p=0.008). The magnitude of the exaggeration in FREE-SR, however, is clearly smaller than in FORCED-SR (1.4 versus 3.1). Teammates, on average, exaggerate to a lesser extent if the supervisor has reward power than when she is just an observer. However, based on non-parametrical tests, neither the averages of reports nor the means of efforts, nor the average extent (amount) of exaggerations differ significantly between the FORCED-SR and the FREE-SR treatments.

Result 5.2. *The overall pattern of reporting behavior, the average level of actual efforts as well as of the reports are not statistically different between the* FREE-SR *and the* FORCED-SR *(FPP test, two-sided, p=0.625 for actual efforts and p=0.936 for reported efforts).*

Nevertheless, a closer look at the exaggerations uncovers some substantial differences in reporting behavior between the FREE-SR and the FORCED-SR. Panel (a) of Figure 5.3 presents the frequency of exaggerations as the ratio of the actual untruthful self-reports to the number of cases with exaggeration possibilities, i.e., to all cases in which actual efforts are below 20. There are 218 such possible exaggeration cases in FORCED-SR and 197 in FREE-SR. On average, teammates exaggerate more often in FORCED-SR than in FREE-SR (62.8% versus 47.7%). This difference is significant according to Fisher's exact test (p = 0.002).[10] Moreover, if we consider only those teammates who lied and not those who reported truthfully, then we see that teammates in FORCED-SR inflate their efforts considerably more than the teammates in FREE-SR. Panel (b) of Figure 5.3 presents the average amount of exaggeration for the cases in which the reported effort is strictly higher than the actual effort.[11] The average deviation in these cases amounts to 13.2

[10] We are aware of the fact that Fisher's exact test does not consider clustering within groups. However, since we are primarily interested in the overall difference between the two treatments, we decided to use this test.

[11] We exclude the cases, in which subjects reported lower amounts than their actual efforts. There are two such cases in FORCED-SR and 12 in FREE-SR. If we include these cases, the frequency of untruthful self-reports increases to 63.8% in FORCED-SR, and to 53.8% in FREE-SR.



(a) Frequency of exaggeration

(b) Amount of exaggeration

Figure 5.3: Frequency and amount of exaggeration in FREE-SR and FORCED-SR

in FORCED-SR, whereas it is 8.2 in FREE-SR. The difference is weakly significant according to the FPP test (p=0.076). A random Tobit regression reveals a highly significant negative effect of the supervisor with reward power on teammates' extent of exaggeration (see Table 5.4 in subsection 5.7.1 of the appendix). The coefficient of the "Supervisor with reward power" is -3.370, the robust standard error is 1.184 and the p-value is 0.005. This fact confirms the difference in reporting behavior between the FORCED-SR and FREE-SR.

Result 5.3. *In* FORCED-SR*, teammates exaggerate more often and stronger than in* FREE-SR.

Taken Result 5.2 and Result 5.3 together, we find some substantial support for the Hypothesis 5.3 that teammates lie to a lesser extent in FREE-SR than in FORCED-SR.

Supervisors are able to allocate incentive compatible payments when receiving self-reports as effectively as when they are informed about actual individual efforts

We conducted the treatment FREE, in which the supervisor is informed about teammates' true efforts, to investigate whether supervisors are able to allocate

performance pay in an incentive-compatible[12] way. Indeed, in FREE, in 97.7% of the cases supervisors allocate incentive compatible rewards. In the FREE-SR treatment, the respective number is 95.5% with respect to reports (89.7% with respect to actual efforts). The difference between both treatments is statistically not significant.

Result 5.4. *Supervisors in* FREE-SR *receiving self-reports are able to apply an incentive compatible reward allocation as effectively as supervisors in* FREE *who are informed about actual efforts.*

This result is not surprising, considering that we used a design that made it easy for the supervisor to pay incentive compatible rewards. By doubling the contributions and distributing one half of the result as the variable payoff, the incentive compatible share is always equal to each teammates' contribution.

The effect of self-reports on team performance

How do self-reports ultimately affect team performance? The answer depends on which setting we compare. We could compare self-report treatments to an ideal setting of perfect monitoring and perfectly incentivizing performance pay allocations. In the FORCED treatment, we set up such an ideal setting. As Figure 5.4 shows, compared to the FORCED treatment, both self-report treatments have detrimental effects on team performance. The clear difference in average efforts between FORCED (19.3 on average) and FORCED-SR is statistically significant (13.4, FPP test, one-sided p=0.033). The other extreme situation we set up is that of a standard public goods setting in the EQUAL treatment. Compared to EQUAL, in which average effort amounts to 11.1, both self-report treatments perform considerably better, although the differences are not significant according to FPP test (p=0.236 for FORCED-SR and p=0.117 for FREE-SR).

[12] We measure a supervisor's performance as the deviation from the proportional distribution benchmark. As mentioned above, the proportional distribution is not the only incentive-compatible allocation rule. However, the results from the FREE treatment show that almost all chosen distributions were proportional. Formally, the supervisors' "quality" is defined by the normed geometric distance from the perfect proportional allocation $1-\frac{1}{\sqrt{2}}\sum_{i=1}^{3}(s_i-s_i^*)^2$ where s_i^* is member$i$'s share in the proportional distribution based on contributions $\left(s_i^*=\frac{e_i}{W}\right)$ or on reported contributions in FREE-SR.

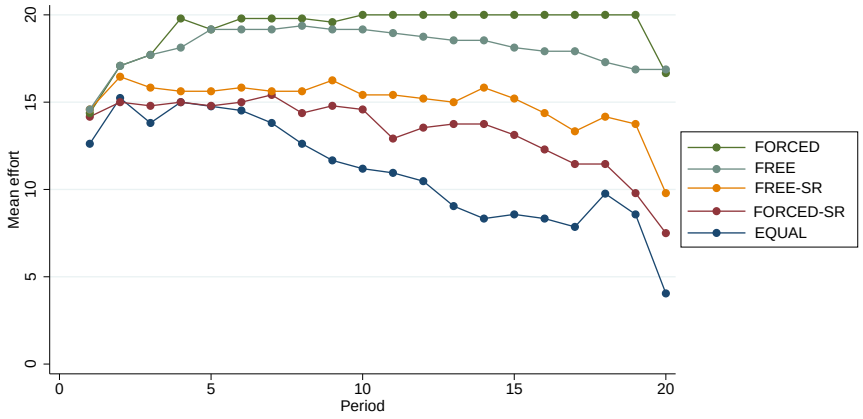


Figure 5.4: Average team performance

Result 5.5. *Compared to an ideal situation in* Forced*, self-reports have detrimental effects.*

The comparison of average efforts in Free and Free-SR shows that there is no significant detrimental effect of self-reports when the supervisor has reward power. Although the average efforts tend to be smaller in Free-SR (14.9) than in Free (18.1), this difference is statistically not significant (FPP test, one-sided p=0.107).

Result 5.6. *Self-reports have no significant detrimental effect on team performance if the supervisor has distributive power.*

The positive effect of having a supervisor with distributive power under self-reports becomes obvious when we focus on the average distances between Forced and Forced-SR, and between Free and Free-SR, respectively. Whereas self-reports reduce the average efforts by about 5.94 tokens if the distribution of performance pay is exogenous (Forced minus Forced-SR), the negative effect of self-reports is only 3.18 tokens between both endogenous treatments (Free minus Free-SR).

Result 5.7. *The detrimental effects of self-reports on team performance are smaller when the supervisor has distributive power than when the supervisor is forced to follow exogenous rule to allocate performance pay (compare the tobit regression in appendix 5.7.1).*

Regression analysis

In a set of random effects panel regressions (see Table 5.3), we can confirm all of the results that we have presented so far. We assume that the following model(s) explains the variance in teammates' efforts. The first explanatory variable that we add to all three variants of the model is a subject's belief about the teammates' average effort in the next period (*Belief about others' average efforts*). Similar to Fischbacher and Gächter (2010) we assume that subjects update this belief depending on the received feedback in a first step, before they use it to determine their next period's effort. The second explanatory variable is a measure for *supervisor performance* that we construct as "1 - the average absolute distance" between the chosen share (reward) for the teammate (by the supervisor) and the reward the teammate would receive if the shares would be allocated perfectly proportional to the actual efforts. Since the teammates learn the supervisor's allocation decision only at the end of each round, we include the first lag $(t-1)$ of the supervisor performance measure. The dummy variable *Equal distribution* is 1 in the Equal treatment in which all teammates receive the same performance pay share irrespective of their relative effort. The second dummy *Self-report* is set to 1 in the Free-SR and the Forced-SR. The dummy *Supervisor with reward power* is 1 for the treatments in which the supervisor has reward power (Free and Free-SR). Finally, we include an interaction term between the self-report dummy and the supervisor with reward power dummy that is set to one only in Free-SR. The results of the random effects panel regressions with robust standard errors clustered on groups are presented in Table 5.3.

The *belief about others' average effort* has a positive effect on a teammate's own effort in each of the three variants of the model, i.e., the higher a teammate's expectation of others' average effort, the more she is willing to contribute. The regression also shows a large positive effect of *Supervisor performance in* $t-1$ on efforts. The overall effect of supervisor's ability to match the perfect proportional share is in line with the results obtained with non-parametrical tests for the comparison between the Free and the Forced treatment.

The negative coefficient for the dummy *Equal distribution* (see model I in Table 5.3) confirms the positive effect of performance based payments in the exogenous

Table 5.3: Effects on effort

Dependent variable: Effort in t	I	II	III
Belief about others' average efforts	1.066***	1.057***	1.051***
	(0.054)	(0.059)	(0.064)
Supervisor performance in $t-1$	3.179***	2.639**	2.937**
	(0.845)	(1.256)	(1.400)
Equal distribution	−2.121**		
	(1.056)		
Self-reports		−1.771**	−2.429**
		(0.708)	(1.079)
Supervisor with reward power			−0.375
			(0.303)
Supervisor with reward power×Self-reports			1.306
			(1.294)
Constant	1.907***	3.413**	3.373**
	(0.707)	(1.607)	(1.461)
n	2223	1824	1824
Wald chi^2	832.90	712.22	832.89
R^2	0.672	0.691	0.693

Notes: Random-effects GLS regression. Robust standard errors in parentheses, clustered by matching group: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Dependent variable: Effort.

(Forced, Forced-SR) and endogenous treatments (Free, Free-SR) compared to the Equal treatment. The dummies for Self-reports confirm the results of the nonparametric tests (see model II in Table 5.3). If the payment is mainly based on self-reports as in the two self-report treatments, the teammate's effort decreases by about 1.771. The coefficient for the interaction between the supervisor with reward power and self-reports (*Supervisor with reward power* × *Self-reports*) shows the positive effect that a supervisor with distributive power has on efforts, i.e., the detrimental effects of self-reports are much less pronounced if there is an supervisor with reward power (see model III in Table 5.3).

5.6 Conclusion

Our most important result is that the detrimental effects of self-reports are less pronounced when a supervisor has distributive power to allocate performance pay than under an exogenous forced distribution system (FDS) where the supervisor has limited power. Teammates exaggerate self-reports strongly under a FDS when

the allocation mechanism of performance pay is exogenous and known ex-ante. If the supervisor possesses the power to allocate rewards at her own discretion and ex-post, self-reports are less exaggerated. Interestingly, the uncertainty stemming from the fact that the supervisors ex-post decide on performance pay shares at their own discretion seem to increase efforts and decrease the amount of exaggerations.

Translating the main result explained above as a practical implication for reality would mean the following: In situations where a supervisor must rely (at least to some extent) on self-reported effort information, it might be better for the management not to announce hard-wired performance pay ex-ante. Management should rather, keep the possibility open of determining/adjusting performance pay until observed the joint outcome and the (possibly exaggerated) self-reports. The results point to a possible "cost of transparency" when a performance pay scheme is hard-wired and challenge the suitability and effectiveness of FDS in cases similar to our setting.

We also experimentally complement the finding from the organizational behavior literature that one has to be cautious when implementing and evaluating self-reports in performance appraisals. Compared to an ideal but rather unrealistic situation of full information, the ultimate effect of self-reports on team performance is negative. The transferability of this study's results to reality, however, might be limited by the fact that we have rather a strong test for self-reporting. In reality, the detection of false self-reports might be easier. Hence, our findings may overestimate the (detrimental) effects of self-reports on team performance in reality. Future research could test different possibilities for detecting false self-reports.

Evidence from tournament experiments shows that team members invest in activities that harm other teammates in order to improve their own relative standing (Harbring & Irlenbusch, 2011). Future research may also investigate the reporting behavior in a setting where teammates may not only self-report their own efforts but are also able to transfer (possibly false) information about their fellows' efforts.

5.7 Appendix

5.7.1 Tables

Table 5.4: Tobit regression of exaggeration

Dependent variable: exaggeration in t	
Treatment (0=FORCED-SR | 1=FREE-SR)	-3.365^{***}
	(1.306)
Period	0.048
	(0.050)
Own contribution in t	-0.533^{***}
	(0.061)
Constant	14.714^{***}
	(1.262)
n	231
Wald chi^2	108.39

Notes: Random-effects tobit regression. Robust standard errors in parentheses, clustered by subject id: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Dependent variable: exaggeration.

5.7.2 Instructions

Here we present the instructions for the treatment FREE-SR as example. The original instructions were in German and the one listed below is a translation into English.

General information

We welcome you to this economic experiment. It is very important that you read the following explanations carefully. If you have any questions, please address them to us. In this experiment, you can earn money depending on your own and other participants' decisions.

During the experiment, you are not allowed to talk to other participants. Non-compliance with this rule will result in your being excluded from the experiment and from all payments. All decisions will be taken anonymously, i.e., none of the other participants will learn the identity of the participant who has taken a specific decision. Payment is anonymous as well, i.e., no participant knows the payments of other participants.

During the experiment, your entire income is calculated in points. At the end of the experiment, the total number of points earned during the experiment are converted to Euros, where

35 points = 1 Euro.

At the end of today's experiment, you will receive the number of points earned during the experiment plus a 2,50 € show-up fee.

On the following pages, the detailed procedure of the experiment is explained.

Course of the experiment

- The experiment will consist of 20 rounds, each round having the same structure.
- You will be part of a group of 4 members. During the whole experiment, you will be exclusively interacting with the members of your group. The composition of the group will remain unchanged across all rounds of the experiment.

Contributors and observer Your group will consist of **three contributors** and **one observer**. Whether you will be assigned to the role of one of the contributors or the observer will be randomly determined and communicated to you before the first round begins. Moreover, a letter (A, B, or C) will be randomly assigned to each contributor. This letter will be visible to the other contributors and to the observer, and will remain unchanged throughout the whole experiment. At the beginning of each round, **each contributor** will receive an **endowment** of **20 points**. The observer will receive an **endowment** of **10 points**.

Stage 1: Decisions of the contributors

- Each **contributor** has to decide how many of the 20 points she wants to **contribute** to the **group account**. Points which are not contributed to the group account will be kept by the contributor. Possible contributions are: $[0, 5, 10, 15, \textbf{or} 20]$ **points**.
- Each point contributed will be **multiplied by two**, and the **sum** of the doubled points is the **group result**. (sum of contributed points x 2 = group result)
- **All contributors** will be **informed** about the **individual decisions** and the **group result**.
- **The observer** will only be **informed** about the level of the **group result**.
- **Each contributor will decide** what information about the level of her contribution is **transmitted** to the **observer**.

Distribution of the group result - see figure below

- The group result is divided into three parts:
  - **Part I**, $\frac{1}{4}$ of the group result, is distributed equally among the **3 contributors**.
  - The **distribution** of **part II**, $\frac{1}{2}$ of the group result, is carried out by the **observer**.
  - **Part III**, $\frac{1}{4}$ of the group result, is **given to the observer**.

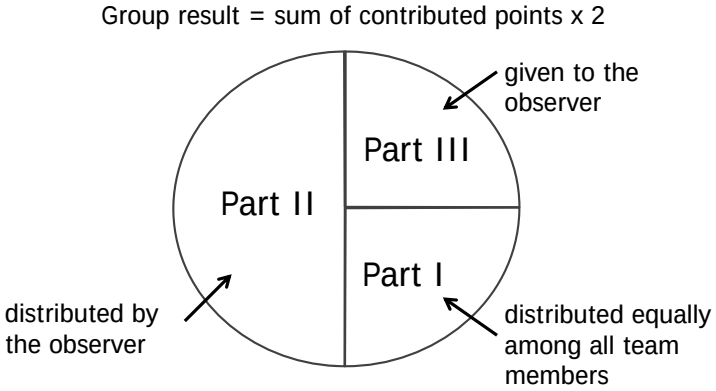


Figure: Distribution of the group result

Stage 2: Decisions of the observer

- The observer **cannot contribute** her round endowment to the group account.
- The observer has to **completely** distribute **part II among the contributors**. The observer cannot keep any points for herself.
- **At her own discretion**, the observer assigns a share of part II to each contributor.

Calculation of the round incomes of the contributors

- The round income of each contributor consists of three parts:
  - **Share of the round endowment** which has not been contributed,
  - **Share of part I** which has been distributed equally among all contributors,
  - **Share of part II** which the observer has assigned to the contributor.

Round income of the contributor =
+ 20 - contribution + part I / 3 + assigned share of part II

Calculation of the round income of the observer

- The round income of the observer consists of two parts:
  - **Round endowment** and
  - **part III**

Round income of the observer =
10 + part III

Stage 3: End of the round

- At the end of each round, all contributors will be informed about the decision of the observer and about their round incomes.
- The observer will be informed about her round income.

Total income Your total income will be the sum of the incomes from all rounds.

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