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Essays on Behavioral Labor Economics

experimental evidence from Germany and Ghana

Author:
Kerstin Grosch

Supervisor:
Marcela Ibañez

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Erstgutachterin:	Prof. Marcela Ibañez, Ph.D.
Zweitgutachterin:	Prof. Dr. Claudia Keser
Drittprüfer:	Prof. Dr. Oliver Mußhoff

Declaration of Authorship

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How selfish soever man may be supposed, there are evidently some principles in his nature, which interest him in the fortunes of others, and render their happiness necessary to him, though he derives nothing from it, except the pleasure of seeing it.

Adam Smith

The natural distribution is neither just nor unjust; nor is it unjust that persons are born into society at some particular position. These are simply natural facts. What is just and unjust is the way that institutions deal with these facts.

John Rawls

Money does not buy you happiness, but lack of money certainly buys you misery.

Daniel Kahnemann

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List of Co-Authors

- **Fischer**, Sabine, M. Sc.; Ph.D. candidate at *University of Goettingen, Germany*,
  Chapter 5
- **Ibañez**, Marcela, Ph.D.; Professor of Behavioral Development Economics at *University of Goettingen, Germany*,
  Chapter 2
- **Rau**, Dr. Holger A.; Assistant Professor of Experimental Economics at *University of Goettingen, Germany*,
  Chapter 3 & 4
- **Viceisza**, Angelino, Ph.D.; Assistant Professor of Economics at *Spelman College Atlanta, U.S.*,
  Chapter 2

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Chapter 1

General Introduction

1.1 Introductory words

The dissertation presents a collection of essays that deal with behavioral aspects on labor markets and focus especially on social interactions. Each Chapter from 2 to 5 represents one separate project/essay. In this general introduction, I will embed the four essays in the broader frame of behavioral labor economics and elucidate the relevance for agricultural markets.

1.2 International market integration

The efficient design of institutions in labor markets is key for economic growth. Employees in companies and independent entrepreneurs create added value and contribute substantially to a country's welfare. In recent times, global agri-food systems have undergone a rapid transformation towards higher-value products, higher quality and a higher degree of international and vertical integration (Reardon et al., 2009). Especially companies in developing countries can largely benefit when they integrate into international value chains, e.g., exporting goods to western countries or attracting foreign direct investment. Modern supply channels hold potentials to gain higher margins and regions may profit from increasing labor demand (Reardon et al., 2009). However, developing countries in particular suffer from weak institutions that impede successful participation in emerging supply chains (Gómez et al., 2011, Holzapfel and Wollni, 2014). Weak institutions may aggravate contract conclusion since investors shy away from contracts that inhere too much uncertainty. Also, once contracts are concluded, contract compliance may be challenged when legal systems are not functioning properly. In Section 1.5, I will elaborate on this aspect.

Another challenge that companies or smallholder farmers face, who integrate into international markets, is the exposure to more intense competition. To prevail in the global market, permanently refining organization's efficiency is crucial. Since agriculture is still highly labor intense in Sub-Saharan Africa, it is important to increase particularly agricultural labor productivity (Dorward, 2013). Various studies point out that the share of employment is about three times larger than the share in value added coming from agriculture (Kuznets et al., 1971, Gollin et al., 2002, 2014, Cai and Pandey, 2015). The factor is even larger for developing countries. This implies that the value-added per worker is lower in the agricultural than in the non-agricultural sector. Even

when taking into account the particularities of the agricultural sector such as the number of hours worked and the education level, the "agricultural productivity gap" remains (Gollin et al., 2014). This emphasizes the importance of enhancing productivity especially in the agricultural sector. Productivity can be increased by using (pay) incentives or building up social capital. In the following, I describe the two instruments in more detail.

1.3 Social capital

Social capital can be described as "features of an organization such as networks, norms and social trust that facilitate coordination and cooperation for mutual benefit" (Putnam, 1995, p.67). At the workplace, social relations among co-workers can be critical for an organization's success (Beal et al., 2003). A friendly and cooperative work environment can reduce transaction costs as workers are more willing to support each other, share information and cooperate in joint projects (Brief and Motowidlo, 1986). In addition, good interpersonal relations facilitate communication and enhance employee commitment (Leana and Van Buren, 1999, Adler and Kwon, 2002, Bright et al., 2006).
As much as prosocial behavior is beneficial to companies as harmful can be antisocial behavior at the workplace. Antisocial behavior describes actions which bring harm to an organization, its employees, or its stakeholders (Griffin and Lopez, 2005). Consequences are substantial costs due to, e.g., fraud and theft (Coffin, 2003).
In standard economic theory, workers on labor markets are treated as Homo Oeconomicus, who are self-interested profit-maximizing actors not caring about other people's welfare. Some attention has been paid to the idea that people may not solely be acting in this self-oriented way but may also be altruistic, that is they selflessness care about the well-being of non-akin others (Trivers, 1971). But only with the addition of reciprocity and the inclusion of fairness norms that theory becomes more accurate (e.g. Rabin, 1993, Berg et al., 1995). Altruism and social behavior is a complex matter, meaning that the occurrence is context-dependent. For instance, the potential length of relation and other persons' actions, either cooperative or not, can be crucial for the motivation to engage in voluntary cooperative behavior (Keser and Van Winden, 2000). If somebody is nice to you, fairness norms dictate to respond in a nice manner (positive reciprocity). However, if somebody acts nastily towards you, fairness norms also allow to behave nastily accordingly (negative reciprocity).
Examples for reciprocity, positive or negative, in the organizational context are manifold. In a work relation between employer and employee for instance, the employer can send signals to the employee: higher wages paid are rewarded by higher effort levels

(Akerlof, 1982, Prendergast, 1999) whereas wage cuts can lead to reductions in effort exerted (Kube et al., 2013). Negative reciprocity can be observed, e.g., when employers increase to monitor employees' work. Workers feel not trusted any longer and consequently punish the employer by withdrawing effort (Falk and Kosfeld, 2006, Sliwka, 2007). Negative reciprocity may also arise when there is not one single employer to blame for the work environment but when workers are fed up with the institution and the payment procedures. This may lead to indirect reciprocity: Workers feel badly or unfairly treated and may pass these negative feelings on to their co-workers and reduce pro-social actions or increase anti-social actions. This topic will be subject of Chapter 2 and Chapter 3, which I elaborate on in the subsequent sections.
As described, it has been shown that humans are not solely driven by profit-maximizing motives. Individuals reflect other people's behavior and are affected by the organizational setting, and can be prosocial or antisocial accordingly. This social behavior can be crucial for an organization's success. Therefore, assessing social capital should not be neglected when examining company's efficiency and labor market outcomes.
In the next section, I explain the other instrument to increase labor productivity: incentives.

1.4 Incentives

In standard economic theory, it is argued that wages are paid by companies depending on demand and supply of workers. The wage paid clears the market resulting in zero unemployment. The problem with this simple story is that managers have no exact information about employer's productivity levels and workers might want to exert as little effort as possible. Because of this lack of information, wages are not in line with the market-clearing wage by nature. Moreover, wages my be evaluated by workers and effort levels adjusted to the perceived fairness of the wage. The fair wage-effort hypothesis introduced by Akerlof and Yellen (1990) constitutes that workers will withdraw effort if they do not perceive their wage level as fair. Experimental evidence corroborates this theory: Fehr et al. (1993) finds in an experimental study that employers offer a higher wage than the market-clearing wage to evoke that employees deliver high-quality work. However, that leads to involuntary unemployment on the labor market.
This shows, that incentives, which are "interventions to influence behavior by altering economic costs or benefits of some targeted activity" (Bowles and Polania-Reyes, 2012, p.368) can be used to increase individual productivity by using incentives for performance. They can be effective and spur effort exerted by workers (e.g. Prendergast, 1999,

Lazear, 2000a, Shearer, 2004). Extra money might be perceived as a gift and increases worker's motivation (Akerlof, 1982). This idea of a positive effort effect as a response to higher wages has been shown in the lab (e.g. Hannan et al., 2002, Charness, 2004). Similarly, negative reciprocity to perceived unfair wages have been shown to reduce effort exerted (e.g. Fehr and Gächter, 2000, Fehr et al., 2009). However, field experiments support positive reciprocity only weakly (Gneezy and List, 2006, Cohn et al., 2009, Kube et al., 2012). There seem to be other determinants that play a role: When paying more, intrinsic motivation might be crowded out (Frey and Oberholzer-Gee, 1997), a reference payoff might bound effort at a certain level (Camerer et al., 1997) or workers are inequality averse and concerned about coworkers payoff (Bandiera et al., 2010). Moreover, it has been found that the currency of the gift matters. Field experimental research suggests that non-monetary incentives such as awards or gifts can be similarly effective compared to monetary incentives (Kosfeld and Neckermann, 2011, Kube et al., 2012, Ashraf et al., 2014).

This summary of research emphasizes that incentives can have the intended effect. However, there seem to be other relevant factors altering behavior that have not been captured and fully analyzed in the lab yet. One relevant factor may be crowding out of initial motivation. For instance, it has been shown that the use of incentives can crowd out intrinsic motivation (see for a review Bowles and Polania-Reyes, 2012). Putting a market price on desired behavior can therefore have opposite effects than intended (e.g. Frey and Jegen, 2001, Tirole, 2006). Depending on the context, an explanation can be that people want to be admired for a good deed that they do voluntarily. As soon as that good deed is imprinted with a monetary incentive, people cannot brush up their self-image with this deed any longer.

We contribute to these studies and test potential negative side effects from incentives on another domain: (anti-)social behavior. Pervasively used in companies are bonus payments to increase labor productivity. At the same time, workplace relations matter for the organization's efficiency. Therefore, it is interesting to examine potential side effects of pay incentives on social capital. We expected that the (wrong) use of incentives can be perceived as unfair and can be evaluated in relation to coworkers. For instance, if coworkers are treated fairly, that is, they receive the opportunity to obtain a bonus but others not, those who are treated unfairly may become resentful and may feel the urge to retaliate. In Chapter 2 and Chapter 3, we examine how different pay regimes, discriminatory and competitive, affect (anti-)social behavior at the workplace. In the following, I briefly summarize the motivation, experimental design and contributions to the literature of these two essays.

1.4.1 Competitive incentives (Chapter 2)

In Chapter 2, we investigate how pay regimes affect prosocial attitudes at the workplace. Commonly used is competitive remuneration, i.e., extra payment can be earned exclusively by workers that have prevailed in the competition for bonuses. Both theoretical and empirical literature suggest that competitive payment schemes can increase effort and productivity (e.g. Lazear and Rosen, 1981, Erev et al., 1993, Bandiera et al., 2011). Yet, it is not clear if such type of incentives could negatively affect the ex-post quality of co-workers relationships. In this essay, we address this question and investigate the effects that competitive payment schemes have on pro-social attitudes between co-workers after they have been exposed to competition.

Our main hypothesis is that competitive payment schemes generate a negative effect on the quality of co-workers relationships. For example, competition generates a feeling of rivalry among competitors. Workers might perceive others as opponents and adopt a more individualistic behavior in such an environment (Dechenaux et al., 2015). We test various channels that could play a role for becoming less prosocial (or reciprocal) such as rival feelings, inequality aversion and perceived unfairness.

To test for the effect of competition on prosociality, we conducted an experiment with workers from a banana-producing agribusiness in Ghana. The experiment comprises three stages. In the first stage, we measure baseline prosociality by a public goods game (PGG) and social value orientation (SVO) game (Murphy et al., 2011). In the second stage, participants solve a real-effort/output task that takes the form of individually assembling ballpoint pens. In this stage, we implement a between subject design and each subject is randomly allocated to either a competitive or a non-competitive payment scheme. In the competitive scheme, the participant who assembles most pens correctly earns a high payment and in the non-competitive payment scheme, every participant who correctly assembles more than a given number pens wins a high payment. The second dimension that we vary in our design is the difference between the winner's and loser's payoffs (dispersion in payments) being either high or low. In the high dispersion conditions, the winner's payoff is 3 times the loser's while in the low dispersion it is 1.5 times. Finally, in the third stage, we measure the ex-post effect of different payments on prosociality. Hence, subjects repeat the PGG and the SVO. In the analysis, we compare prosociality (PGG or SVO) in the third stage relative to the first stage, across the payment schemes and dispersion. Since all treatments have a high-income earner and a low-income earner, we can further compare the effect of the payment scheme of each group of participants across payments and dispersion levels.

We find that when much is at stake, i.e., the dispersion between the winner's and loser's payoffs is high, competition crowds out prosociality which confirms prior lab findings.

This effect seems to be mainly driven by (1) those who win the competition, (2) those who are more inequality averse, (3) those who usually work in teams at the company and (4) those not aware of the bonus system in place. However, when there is less at stake, we find that competition does not affect prosociality compared with the threshold payment.
With this study we contribute to the literature of competitive incentives and side effects on prosociality. Buser and Dreber (2015) have demonstrated in an amazon-mturk study that competitive remuneration reduces prosocial behavior. Our experimental design allows to control for relative income effects. A novel finding is therefore that the effect of winning a competition is driving (partly) the effect independent of potential income effects. Furthermore, we show that inequality aversion reduces prosociality under a competitive payment scheme. Furthermore, our study is a field study showing that work-related determinants such as exposure to teamwork influence cooperation heterogeneously.

1.4.2 Discriminatory incentives (Chapter 3)

Another characteristic of incentives is that they are often (deliberately or not) discriminatory. Such obscure pay regimes are pervasive as monitoring of effort and ability is imperfect (Berger et al. 2013). Thus, when pay regimes are not transparent, workers may doubt the fairness of the process. Especially discriminated workers, i.e., workers that are excluded from a competition for bonuses, might become resentful and reduce the organization's social capital by becoming more antisocial. This essay investigates if discriminatory pay regimes lead to more pronounced antisocial behavior among coworkers compared to non-discriminatory pay regimes. Especially "unjustifiably-paid" workers, i.e., workers with a high (perceived) performance who receive no compensation, may feel frustrated. Finally, we examine if a discriminatory pay regime lowers prosocial actions towards co-workers (Buser and Dreber, 2015, Grosch et al., 2017).
To investigate the link of discriminatory pay regimes and antisocial behavior, we conduct a real-effort experiment. Here, we vary two determinants of pay regimes: discrimination and justification of payments by performance. In our *Discrimination* treatment, half of the workforce is randomly selected and promoted and participate in a tournament for bonuses (high-income workers) whereas the other half receives no payment (low-income workers). In another treatment *Competition*, all payments are justified and there is no discrimination. All subjects participate in a tournament, are ranked by performance and receive payments according to their performance (best 50% receive bonuses and worse 50% receive zero payment). Afterwards, antisocial behavior is

measured by a Joy-of-Destruction game where participants can destroy canteen vouchers. To measure spillover-effects on cooperative behavior, we implement a prisoner's dilemma game at the end of the experiment.
The data show that low-income workers destroy significantly more vouchers than high-income workers. A driver to destroy could be individual aversion to inequality. However, results show that inequality aversion influences decision-making in the competitive but not in the discriminatory pay regime. Destruction behavior in the discriminatory pay regime is driven by workers who receive payments that are not justified by performance. When all payments are justified, that is in our competition treatment, the difference vanishes. By using a treatment with random payments, we show that unjustifiably-paid workers destroy less when they had equal opportunities to receive a high payment, i.e., when they were not discriminated by the pay regime.
Our findings contribute to managerial economics and the design of fair procedures in pay regimes to mitigate worker frustration. A large strand of literature emphasizes that workers' intrinsic motivation is sensitive to the inappropriate use of financial incentives (e.g. Gneezy and Rustichini, 2000, Benabou and Tirole, 2003). We draw on this and show that employees do not only respond negatively to the "wrong" use of financial incentives per se. Instead, we emphasize the importance of the appropriate design of pay institutions and highlight that intransparent pay regimes may cause antisocial behavior. Due to the fact that antisocial behavior often raises high costs, these insights may help to achieve higher workplace efficiency.

1.5 Honesty and Compliance

Another important aspect in work relationships is compliant and honest behavior. In business relationships compliance with contracts is key for fruitful long-term relationships. In Africa, the majority of agricultural produce comes from smallholder farmers. They often face difficulties to flourish because of underlying financial constraints – they cannot afford to purchase fertilizer, buy new equipment or hire staff (Reardon et al., 2009). Therefore, resource-based contracts, i.e., a party invests money or provides inputs in a farm and the farmer sells a certain amount of (high-quality) produce in return to the investor, can increase revenues for smallholder farmers. However, only when smallholder farmers comply to contracts they may benefit from cooperations in the long run. Compliance with an agreed upon contract can be seen as a first step towards trustworthy rapports, which is a vital determinant for economic growth (Algan and Cahuc, 2010). However, especially in many developing countries building up trustworthy business relationships is impeded due to non-functioning legal systems

and, on average, lower trust levels in societies with a certain history in slave trade and civil wars (Nunn and Wantchekon, 2011). As well as that, many African countries are ethnically diverse which can further curb trustworthy and compliant behavior (Glaeser et al., 2000). Therefore, it is particularly interesting to examine determinants hampering compliant behavior in the African country context. In our paper presented in Chapter 5, we are interested in examining the influence of overconfidence in one's own performance on compliant behavior. In Section 1.5.2 more details will be given on that study. Related to compliant behavior is honest behavior since honest behavior can be interpreted as being compliant to a social norm. If people get the opportunity, they are dishonest to gain a certain benefit (Mazar et al., 2008). However, not all individuals are dishonest and a substantial share of people are commonly found honest and forgo economic rents. In the majority of studies in the field of honest behavior, individual-level motives as an important determinant of honest behavior are neglected so far. We can contribute with our study to a better understanding why some people may be and other people may be not acting honestly. In the following section we describe the study, which will be presented in Chapter 4, in more detail.

1.5.1 Gender differences and the role of social preferences (Chapter 4)

Being compliant or honest in a particular situation might have payoff consequences. For instance, if we imagine a situation where worker A receives unmeritedly credit for successfully settling a project, s/he can decide to be honest and tell that her colleague B deserves the acknowledgments. This decision may have payoff consequences if settling the project is rewarded by a bonus. If worker A tells the truth, s/he forgoes the bonus. If worker A lies, s/he receives the bonus but at the same time threatens the norm to be honest. Therefore, for the decision to tell the truth or not, it might matter how concerned somebody is about another person's payoff.

In this chapter, we experimentally analyze the role of social preferences and gender on honest behavior. We focus on a situation where dishonest behavior pays off at the cost of somebody else. In such situations, rational people are dishonest to the full extent to maximize their monetary payoff irrespective of the negative externality. However, honesty is a social norm and people face psychological costs when violating this norm since it might induce guilt (Battigalli and Dufwenberg, 2007) or a disadvantageous change in self-perception (Bénabou and Tirole, 2011). Studies in the domain of the norm honesty confirm that psychological costs affect behavior. In experiments, psychological costs

can be varied and behavior differs according to priming (Cappelen et al., 2013, Cohn et al., 2015), moral balancing (Ploner and Regner, 2013, Clot et al., 2014) and contextual cues that allow for self-delusion/self-justification (Mazar et al., 2008, Shalvi et al., 2011, Jiang, 2013, Kajackaite and Gneezy, 2017). However, in the majority of these studies individual-level motives are neglected as an important determinant of honest behavior. In other words, if the context is varied and honest behavior is compared between different contexts, it is not clear which characteristics or preferences make people respond differently.

In this study, we fill this gap and examine how the individual-level motive of social preferences affects honest behavior. Social preferences can reflect preferences for payoff distributions. In turn, engaging in dishonest behavior affects payoffs. As a consequence, social preferences could explain why some people behave dishonestly and some people act honestly in the same situation. People with more pronounced social preferences, i.e., people who are relatively more prosocial, might face higher psychological costs when their immoral act has negative payoff consequences for another person's payoff. Thus, more pronounced social preferences might translate into behaving more honestly to avoid a negative effect on somebody else's payoff.

To test the link between social preferences and dishonest behavior, we run a laboratory experiment. First, we elicit subjects' social value orientation (SVO) with the measure of Murphy et al. (2011). This measure reflects people's magnitude of concern for other people's payoffs. Here, subjects make decisions on payoff distributions for themselves and another person. Based on these decisions, an individual SVO angle can be calculated for each subject. A lower angle reflects a higher value put on one's own monetary payoff, whereas a relatively high angle reflects concern for another person's payoff. Subsequent to the elictiation of SVO, we implement a die rolling game to elicit dishonest behavior (Fischbacher and Föllmi-Heusi, 2013). Subjects are asked to roll a die 10 times and report the number after each cast. Reporting higher numbers is rewarded by higher monetary gains. Dishonest behavior in this game can be called a selfish black lie (Erat and Gneezy, 2012). The reason is that dishonesty guarantees gains for the liar and bears costs for the experimenter. Using the mean of the 10 die rolls allows us to investigate individual-level dishonest behavior. When we refer to dishonest behavior in our study, we mean that although people are supposed to tell the truth they misreport the die roll. Regarding behavioral predictions, we expected that with an increasing SVO angle, fewer misreporting will occur.

Our results show that individual social preferences matter for explaining differences in honest behavior. With an increasing SVO angle, subjects are more honest. In other words, prosocial subjects behave more honestly than individualistic subjects. Besides

this novel result we confirm previous findings of gender differences, i.e., women are more honest than men. Focusing on gender and SVO reveals that indeed women's SVO is more pronounced than men's. In a mediation analysis, we find that the gender effect can be explained by a difference in SVO between men and women.
Transferring our results to the workplace example from the beginning, it suggests that women may behave more honestly and miss chances for promotions or rewards because they have more pronounced social preferences compared to men. Rand et al. (2016) demonstrated that women show certain "female traits" such as social preferences to a higher extent when they are particularly aware of their gender role. This implies that in settings where gender roles are less salient, women may behave similarly to men. That is an interesting finding that may induce ideas for the design of institutions, such as providing anonymity in certain situations, to avoid "gender-role driven" behavior.

1.5.2 Overconfidence (Chapter 5)

The design of institutions can also be crucial in another context: compliance with contracts. This is especially important for developing countries. One reason is that many companies (established or in the set-up phase) depend on principal's investments to build up their business, to enhance produce or services and to integrate into international value chains. However, in developing countries functioning legal systems are often absent, corrupt or too slow to be usable (Dixit, 2003). This emphasizes the importance of contract compliance and the understanding of its drivers.
In Chapter 5, we present field experimental evidence of contract breach in a principal-agent setting conducted in Ghana. Our main focus is on an agent's individual-level motive to break a contract, i.e., biased expectations about his/her performance. In behavioral economics, there is extensive evidence that people's judgment under uncertainty is often misguided, i.e., people often do a poor job in assessing their own performance in a certain task (e.g. Moore and Healy, 2008). Often, people are overconfident and overestimate their real performance.
Cognitive limitations may have crucial consequences for principal-agent relations. It is likely that overconfidence in combination with payoff expectations may rationalize contract breach. Think of an agent with biased expectations of output and payoff. It is likely that s/he overestimates her performance and consequently makes a generous offer to a principal. The principal invests in the agent's production, the production gains in value and then the agent realizes that her expectations do not come true. In this scenario the agent's real output is lower than expected. Therefore, the agent would

receive a "real payoff", which is below her expectations, if s/he complied to the contract. In such a situation, agents might strive to meet their payoff expectations (Camerer et al., 1997). Breaching the contract allows agents to eradicate the divergence between payoff expectations and the status quo. Based on this reasoning, our main hypothesis is that overconfidence fosters contract breach. The channel that we want to test is reference-dependent preferences. More precisely, payoff expectations resulting from performance misjudgment may serve as a reference point. This means, after materialization of the contract, agents compare the status quo (real payoff from the contract) with their expected payoff. Furthermore, we want to examine possible moderators that may bolster reference-dependent behavior. It has been shown that people may evaluate payoffs as gains and losses rather than as a detached state of wealth (Kahneman and Tversky, 1979). In the moment of realizing that expectations do not materialize, agents may sense a loss. Along the lines of the model on reference-dependent preferences from Koeszegi and Rabin (2006), sensed losses may resonate the more intensely with individual aversion towards losses. To counteract the relatively high utility loss, overconfident agents might engage in contract breaching to a higher extent with increasing loss aversion. Furthermore, breaching might be facilitated if the environment is non-deterministic, i.e., production shocks can occur, compared to a deterministic environment. Production shocks are prevalent in many businesses. For example, in the farming context, weather shocks (positive or negative) can occur and affect production outcomes. In such an environment, agents are enabled to attribute outcome/performance to external factors according to their ex-ante beliefs (Grossman and Owens, 2012). Own accountability from failing to meet the performance goal can be (partly) shifted to the occurrence of a shock. This way, agents can utilize the shock to legitimate contract breach while keeping up their self-image.

We investigate our hypotheses in a lab-in-the-field experiment with students in Ghana. We use a multi-stage investment game where an agent and a principal conclude a contract with an inherent hold-up problem. We designed the experiment in a way that targets the investigation of payoff expectations as reference points. Our focus is on the formation of payoff expectations, the alleged reference point. Payoff expectations vary by individual misjudgment of performance/ output. In the final stage of the experiment, agents can decide to comply or not to comply. Payoff expectations are made salient at this stage as we want to provoke that agents compare the expected payoff with the status quo.

We establish the following results: With increasing overconfidence, sellers breach contracts to a higher extent. We can also confirm that individual loss aversion moderates the effect of sensed losses on breaching contracts. Interestingly, these results only hold

in a non-deterministic environment. In order to identify whether the effect from overestimation is causal, we manipulate expectations by an experimental treatment, where performance/output guesses are raised by an anchor (Tversky and Kahneman, 1975) to ensure that reported effects are causal.
A novelty of our study is that we show how a common heuristic such as overconfidence (e.g. Barber and Odean, 2001) may lead to non-compliant behavior in contract settings by forming inflated payoff expectations. In our set-up, the reference point forms endogenously based on one's own performance misjudgment. Furthermore, in contrast to the majority of studies that focus on ex-ante behavioral change under reference points (e.g. Camerer et al., 1997, Rabin, 2000, Abeler et al., 2011), we focus on an ex-post impact of unmet expectations. Moreover, this study contributes to the scarce empirical and experimental literature on reference-dependent preferences.

1.6 Research method: Economic lab experiments

In this dissertation, all essays presented make use of the research method of economic lab experiments. This has several reasons. Most experimentalists would probably argue that the key advantage of laboratory experiments is that the experimenter can control circumstantial conditions. By varying a single factor, lab experiments are a tool to cleanly test for causal relationships. Identifying cause-effect-relationships can be tricky when using empirical methods particularly in labor markets since contextual variables differ across sectors, companies or working groups. For example, workers self-select into certain jobs and workers differ in their individual characteristics. Moreover, there are differences in the microcosms of relations between co-workers or between hierarchy levels within a firm. Therefore, although there are rich data sets available, accounting for the variation of contextual variables might be very difficult if not impossible (Charness and Kuhn, 2011).
Another advantage of lab experiments is the cost-and time efficient way to elicit individual heterogeneity in (social) preferences and behavior. Moreover, different levels of (social) preferences can be captured in the lab. In contrast, collecting reliable data in survey studies can be very challenging in terms of elicitation method, time and costs. Controlling for individual-level (social) preferences is important for all studies presented in this dissertation. In the study "Competition and prosociality", we study how social preferences are affected by different payment schemes. Here, social preferences are center stage and the main outcome variable of interest. In the studies presented

in Chapters 3 and 4, we use measures of inequality aversion and distributional preferences for examining mechanisms of behavior. We also elicited a selection of (social) preferences in the study presented in Chapter 5 to control for varying preferences in the analysis. As well as that, using lab experiments allowed us to measure loss aversion at an individual level. We used this measure to dig deeper into the underlying motives and test theoretical evidence on reference-dependent preferences.

The majority of laboratory experiments in economics and psychology are conducted with participants from WEIRD (Western, educated, industrialized, rich and democratic) societies. Experimental findings indicate considerable behavioral differences among societies in diverse domains such as analytic reasoning, fairness or cooperation (Henrich et al., 2010, Jones, 2010). Depending on the research question external validity can be increased by using a more natural subject pool. For this reason, I conducted two studies in Ghana – one with workers from a large agri-food company and one with students in urban areas. For the other two studies, it did not seem necessary to study the research questions in the field and hence we conducted those at the University of Goettingen.

In the upcoming Chapters 2 to 5, the four experimental studies are presented in full length. Chapter 6 overviews the results and concludes.

Chapter 2

Competition and prosociality: A field experiment in Ghana

This chapter is joint work with Marcela Ibañez and Angelino Viceisza. I was mainly responsible for literature research, field work in Ghana, data analysis and writing up the manuscript. Marcela Ibañez has been involved intensely in developing the experimental design, analyzing the data and polishing the manuscript. Angelino Viceisza improved the study by critically commenting and giving input at all stages of the study. Moreover, he contributed substantially to the final writing of the paper.

2.1 Introduction

The success of an organization critically depends on social relations among co-workers (Beal et al., 2003). Good interpersonal relations are associated with more willingness of people within an organization to help each other, share information and cooperate in joint projects (Brief and Motowidlo, 1986). In addition, good interpersonal relations facilitate communication, enhance employee commitment, foster individual learning, strengthen relationships and result in improved organizational performance (Leana and Van Buren, 1999, Adler and Kwon, 2002, Bright et al., 2006). Employees report higher job satisfaction, organizational commitment, and less absenteeism when they perceive that co-workers are friendly (Rhoades and Eisenberger, 2002). Hence, managers should try to create and maintain a more friendly working environment.

Despite the importance of workplace relations, managers are also under extreme pressure to increase individual productivity and thus, turn to incentives for boosting performance. Relative payment schemes, where workers compete for a bonus, are a very common instrument used in the workplace. Both theoretical and empirical literatures suggest that competitive payment schemes that are based on relative performance can increase effort and productivity relative to fixed payments or piece rate (e.g. Lazear and Rosen, 1981, Erev et al., 1993, van Dijk et al., 2001, Irlenbusch and Ruchala, 2008, Bandiera et al., 2011). Yet, it is not clear if such competitive incentives could negatively affect the ex-post quality of coworker relationships. In this paper, we assess how competitive versus individual payments obtained from a *non-collaborative* task impact cooperation and prosocial attitudes *after* such a task. Our setting can thus be thought of as a work environment in which individuals work independently and the best performing individual receives a bonus. We consider the effect that this payment has on interactions among workers afterwards.

Our main hypothesis is that competitive payment schemes generate a negative effect on the quality of coworker relationships. There are various channels that might explain this effect. First, competition generates a feeling of rivalry among competitors. Confrontations in the workplace might cause workers to see each other as opponents and thus, adopt more individualistic behavior (e.g. Drago and Garvey, 1998, Brandts et al., 2009, Dechenaux et al., 2015). Second, there are always winners and losers in a competition. This generates inequality in endowments and status. Empirical evidence suggests that those two forms of heterogeneity are associated with lower levels of prosociality and lower incentives to enter into competition (e.g. Chan et al., 1999, Cherry et al., 2005, Buckley and Croson, 2006). Moreover, it has been shown that people with a pronounced aversion to inequality are less likely to enter a competition (e.g. Bartling et al., 2009). Compelled to work under a competitive payment scheme, these

workers might become unsatisfied with the workplace environment and ultimately reduce their prosociality. Lastly, competitive payments can be regarded as unfair. This in turn may decrease incentives to act prosocially after individuals have been exposed to a competition (e.g. Akerlof and Yellen, 1990). Especially workers usually exposed to teamwork might perceive an individual bonus scheme as unfair. This might lead to frustration and less prosocial actions.

To test the effect of competition on prosociality, we conducted a lab-in-the-field experiment (artefactual field experiment in the terminology of Harrison and List, 2004) with workers from a banana-producing agribusiness in Ghana. The field context is particularly relevant for this question for two reasons. First, due to low productivity at the time the experiments were being designed, the firm was considering introducing a competitive bonus system with relative incentives in order to boost effort provision. Second, teamwork is important for the performance of the firm. In fact, about 50% of the tasks (e.g. bunchcare, harvesting, and quality control in packaging) rely on teams.[1] So, to test the potential effectiveness of a competitive bonus system and evaluate the potential side effects on (un)cooperative behavior among team members, the firm permitted us to do an experiment with a sample of their workers.

In our experimental design, we randomly and confidentially match two participants for the duration of a three-stage experiment.[2] In the first stage, we measure baseline prosociality via a one-shot public goods game (PGG) and a social value orientation (SVO) game (à la Murphy et al., 2011). To eliminate income or reputational effects, feedback on the outcomes of the first stage is not provided until after the third stage. In the second stage, participants complete a real-effort/output task in which they individually assemble ballpoint pens. We chose an individual task in order to make the link between effort and payment salient to subjects. Finally, in the third stage, we measure prosociality again by means of the PGG and the SVO. In the second stage, we implement a between-subjects design where each subject is randomly allocated to either a competitive, a threshold, or a random payment scheme. In the competitive scheme, the participant who assembles most pens correctly earns a high payment; in the threshold scheme, every participant who correctly assembles more than a given number pens earns a high payment; and in the random scheme, the high earner is determined by chance. The second dimension that we vary in our design is the difference between the winner's and loser's payoffs (dispersion in payments). In the high-dispersion condition, the winner's payoff is 3 times the loser's while in the low-dispersion condition, it is 1.5 times. This gives rise to a 3×2 design. In the analysis, we first compare effort and

[1] Of course, the whole production line could also be considered one large team as it is an integrated production system where failure in one setting affects another.

[2] This is comparable to van Dijk et al. (2002).

then, changes in prosociality (PGG or SVO) between the first and the third stage across payment schemes and dispersion levels.

Since all treatments have a high-income earner, which we refer to as "winner", and a low-income earner, which we refer to as "loser", we can further compare the effect of the payment scheme on each group (winners and losers) and also across dispersion levels. Moreover, the comparison of behavior in the competitive and the random payment schemes relative to the threshold payment scheme allows us to disentangle the effect of winning against another personfrom the pure effect receiving a relatively higher or lower payment by luck. To test for additional, potential mechanisms, we also elicit preferences for risk, competition, and inequality aversion prior to the experiment.

We find the following. When much is at stake, i.e., the dispersion between the winner's and loser's payoffs is high, competition crowds out prosociality. This is in line with prior findings (e.g. Buser and Dreber, 2015). The effect seems to be mainly driven by those who (1) win the competition (comparable to Schurr and Ritov, 2016, Gee et al., 2016), (2) are more inequality averse, (3) usually work in teams, and (4) are unaware of the existing bonus system. However, when there is less at stake, competition does not affect prosociality. Random payment also crowds out prosociality when the dispersion of payment is high and has no effect when dispersion of payments is low. Compared with competition, the crowding out effect of random payments is lower in at least one dimension (SVO). This suggests that competition does have a particular effect on prosociality that is distinguishable from only high and low payments by luck. Overall, our findings suggest that the impact of competitive schemes (such as relative pay for performance) on workplace cooperation is likely to be context-specific. So, managers should keep the nuances of incentive systems in mind as they consider implementing such schemes in the workplace (as alluded to by for example Holmström, 2017).

Theoretical and lab-experimental evidence suggest that individuals respond to the incentives induced by competition. For example, in order to win a competition people might (1) sabotage competitors (e.g. Lazear, 1989, Harbring and Irlenbusch, 2011), (2) behave more dishonestly (e.g. Gill et al., 2013, Kilduff et al., 2016), (3) be less trusting/trustworthy (e.g. Dirks and Ferrin, 2003, Keck and Karelaia, 2012), and (4) cooperate and coordinate more (e.g. Bornstein and Erev, 1994, Bornstein et al., 2002). Apart from these incentive effects, some papers have examined whether exposure to competition or awareness of it could crowd out ex-post prosociality by increasing dishonesty, destructive behavior and individualism, or by affecting moral judgment (e.g. Chen, 2011, Buser and Dreber, 2015, Schurr and Ritov, 2016, Jauernig et al., 2016).[3] Related,

[3]This has also led to a tangential research agenda exploring the 'thin line' between competition and prosociality (e.g. Savikhin and Sheremeta, 2013, Milkman et al., 2014), as tends to be the case in most naturally-occurring workplace environments.

another line of literature argues that the potential downside effects of relative pay for performance are likely to depend on organizational transparency (e.g. Ockenfels et al., 2015, Breza et al., 2016, Cullen and Pakzad-Hurson, 2017).

Our main novelties relative to previous literature are (1) experimental variation of the strength of competition by having high- and low-dispersion conditions; (2) the field context, which allows us to assess how relevant pre-characteristics or conditions "outside the lab" impact responses to our treatments; (3) the use of a threshold payment scheme, which enables us to keep the distribution of payments (mean and variance) similar between competitive and non-competitive payments; and (4) the pre- and post-elicitation of measures of prosociality, which enable us to control for baseline differences that may confound the treatment estimates. We thus seek to understand the underlying mechanisms that previous literature has not.

The remainder of the paper proceeds as follows. Section 2.2 discusses the field context and study design. Section 2.3 presents the main findings. Finally, Section 2.4 concludes with some discussion and potential policy implications.

2.2 Study design

2.2.1 Field context

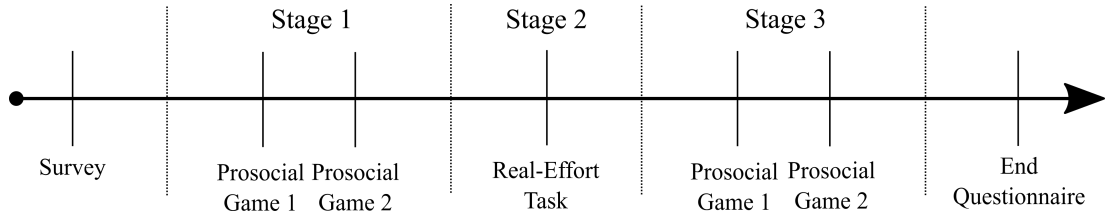
We recruit workers from a major banana-producing agribusiness in Ghana. The firm is fair-trade certified and exports all of its produce, which constitutes 95% of Ghana's national production of bananas to Europe. Its workforce comprises approximately 1815 men and 230 women, all of whom are employed full-time. Most of the employees complete basic jobs such as bunchcare, harvesting, packaging and quality control.

Banana production is divided into eight sectors. All sectors have the same structure: a plantation with a cableway system moving the banana bunches to one of eight packing houses. The majority of employees are specialized in a specific job and work in a specific sector. Sectors 1-7 employ 200 to 250 people every day from Monday to Friday. About 45 people are employed on sector 8, where organic bananas are cultivated. The remaining workers are not attached to a specific sector and get assigned based on need every morning. Apart from being assigned to a sector, workers also specialize in a certain type of job such as caring for and harvesting of banana bunches, cutting leafs off the banana trees, and packaging bananas for transport. Workers in several of these jobs – bunchcare, harvesting, and quality control of packaging – report that they regularly work in teams.

In order to foster higher productivity, the firm established a rather complex bonus system that rewards employees when a target production level is reached. Approximately one third of the workers report being unaware of how the existing bonus system works. At the time these experiments were being designed, the firm was considering revising its existing bonus (i.e., relative/competitive payment) scheme. Therefore, the managers gave our research team permission to carry out the experiments with their workers. The results of the study were also presented to them.

2.2.2 Experiments and surveys

FIGURE 2.1: Study session



A study session comprised a pre-survey, an experiment with three stages (the crux of the session), and a post-questionnaire as shown in 2.1.

At the beginning of the experiment, we randomly and confidentially matched two participants (i and j). Groups remained fixed throughout the experiment. Instructions were presented stage by stage. In the first stage, we elicited the baseline level of prosociality. In the second stage, participants engaged in an individual real-effort task under one of six treatments with either an individual or a relative payment scheme (more below). In the third stage, we elicited participants' ex-post level of prosociality using the same measures as in the first stage. We thus assess the change in prosociality from the first to the third stage as a result of being exposed to an individual versus a relative payment scheme. Below we explain the procedures used in each of the stages.

Stage 1: Baseline level of prosociality.

Prosociality was measured through two games: a one-shot public goods game (PGG) and a social value orientation (SVO) game, the order of which was randomized. In the PGG (Figure A.1), subjects received an endowment of GHS 10 (represented by 10 paper coins during the task) and had to decide how much to invest in an individual or a joint account (represented by two envelopes). The return on investment in the private account was 1 while the marginal per-capita return from the joint account was 0.7. After making a decision, each subject i was asked to guess the amount the other

person j contributed to the group account. Correct guesses earned GHS 2; guesses that deviated by one unit earned GHS 1; and guesses that deviated by two units earned GHS 0.5.

The SVO game (Figure A.2) is based on Murphy et al. (2011).[4] Due to time constraints, we used the reduced version in which subjects compare six distinct money allocations for themselves (i) and their partners (j). To calculate the so-called SVO angle, the preferred amounts across the six decision sets are summed up for i and j respectively and then, the inverse tangent of the proportion of the sums is used to determine the angle. The higher the angle, the more altruistic/prosocial a person is. This game was played via the strategy method and accordingly, the role of payoff-allocator (dictator) was randomly assigned to one of the participants (more when discussing information revelation further below).

Stage 2: Real-effort task and differential payment schemes

Subjects completed a real-effort (RE) task in which we exogenously varied the incentives for performance. The task entailed assembling ballpoint pens for eight minutes (Figure 2.2). Each participant received components for up to 65 ballpoint pens. This task was chosen since it is a task that can be completed regardless of education level and it is easy to to assess quality: A properly functioning (high-quality) pen was one that was able to eject/retract; anything else was of low quality. For purposes of payment, only properly functioning pens were counted.

Stage 3: Ex-post level of prosociality

In the last stage, prosociality was measured again using the same procedures as in Stage 1. The only difference was that the decision sets for the SVO game were presented in a different order to mitigate mere mimicking/repetition of the decisions made previously.

Procedures

As mentioned previously, while participants knew that the experiment had different tasks, instructions were presented stage by stage. Information revelation occurred as follows. Subjects were informed that either Stage 1 or Stage 3 and only one of the prosociality games (either PGG or SVO) would be selected at random for payment. If the SVO was selected, the role of dictator and one of the six decisions would also be selected at random (given the strategy method was used). Feedback on these stages

[4]The original amounts were divided by 12.5 such that incentives were similar across SVO and PGG.

FIGURE 2.2: Instruction phase

(in particular Stage 1) was given only after subjects completed the post-questionnaire such that changes in prosociality were unlikely to be due to endowment, learning, or reputation effects. Participants did receive feedback immediately after the RE task and these earnings were paid with certainty (unlike those for Stages 1 and 3). The latter was done to enhance the salience of the main treatments, i.e., exposure to the payment schemes.

To further investigate the drivers of changes in prosocial behavior and complement the findings from our experiment treatments, we also had subjects complete a pre-survey. This included questions on (1) basic socioeconomic characteristics, (2) work-related measures such as job satisfaction, and (3) behavioral measures such as social preferences (including inequality aversion), risk and time preferences (à la Charness and Viceisza, 2015), competitive preferences (à la Gneezy et al., 2009), Schwartz-values (à la Schwartz, 1992), and self-esteem. Inequality aversion and competitive preferences were elicited in an incentivized way. As these measures were elicited before the experiment, they can be argued to be exogenous to treatment. We thus use them to further explore the drivers of behavioral change.

We also obtained limited administrative data (e.g. job type and sector) from the firm to validate/complement (some of) the work-related measures in the pre-survey.

2.2.3 Treatments

We implemented a 3×2 between-subjects design with three different payment schemes (threshold, competitive, and random) and two different dispersion levels between winners and losers (high and low). Subject-pairs were randomly assigned to one of the resulting six treatments (Table 2.1). In the threshold scheme (T), any participant who assembled 40 or more pens correctly (the median output observed during pilot sessions of the competition, high-dispersion treatment) received a high payment while those who did not received a low payment. We refer to participants who received the *high payment* as winners and those who received the *low payment* as losers (not to be confused with high- and low-dispersion treatments below). In the competitive scheme (C), payments were based on relative performance. The subject (in the pair) who assembled most pens correctly won/earned the high payment. Finally, in the random scheme (R), the winner was determined at random. In the high-dispersion treatments (H), the winner and loser received 15 and 5 respectively and in the low-dispersion treatments (L), they received 12 and 8 respectively.

TABLE 2.1: Experimental treatments

	Competition (C)	Threshold (T)	Random (R)
High (H)	most pens earns 15 other earns 5	$\geq$ 40 pens earns 15 $<$ 40 earns 5	randomly earns 15 other earns 5
Low (L)	most pens earns 12 other earns 8	$\geq$ 40 pens earns 12 $<$ 40 earns 8	randomly earns 12 other earns 8

Our experimental design ensures that, in this stage, winners and losers in the threshold treatments have the same monetary payoffs as winners and losers in the competitive and random treatments. Therefore, conditional on being a winner or loser, differences in behavior across treatments cannot be driven by differential payoffs. Moreover, this design should give rise to similar distributions of payments (mean and variance) across treatments. While we were mainly interested in the differential effect of competitive versus threshold payments on changes in prosociality, we included the random payment scheme in order to isolate the potential effect of being confronted with other participants from the effect that would result from receiving a differential payment.[5] If we find that prosociality decreases more in the competition than in the random payment, we can attribute this change to being exposed to relative payments. If the effect is of similar magnitude, then we can conclude that it is not competition that affects prosociality but rather the inequality that it generates.

[5]Like treatments have also been compared to "murky" bonus schemes (e.g. Buser and Dreber, 2015).

2.2.4 Recruitment and sample

The firm provided a listing of its employees. This list included employee names and identification numbers, sector numbers, and the type of job. A sample of employees was randomly selected and assigned to experimental sessions. However, there was imperfect compliance in terms of actual attendance. Employees had to be released by their sector supervisors, some of whom were less cooperative. In addition, due to the nature of the tasks, packing-house employees tended to be available during the morning. So, while relatively substantial compared to the population of employees, our sample is not necessarily representative of all sectors and job types across the firm.

In total, we conducted 51 sessions, one in the morning and one in the afternoon on Mondays through Fridays, over the course of five weeks. The sessions were announced as "workshops" and supervisors were informed of selected employees a week in advance in order to release them at a given time. Table 5.1 shows the number of sessions, individuals, winners, and losers across treatment conditions. Sessions lasted approximately three hours and paid 26.31 Ghanaian cedi/GHS (USD 7), relative to a daily wage equivalent of GHS 18. A total of 619 individuals (589 of whom were men) showed up.

TABLE 2.2: Number of observations

Treatment	Sessions	Individuals	Winner		Loser	
CH	8	94	48	(0.51)*	46	(0.49)*
CL	9	107	50	(0.47)*	57	(0.53)*
TH	10	117	66	(0.56)	51	(0.44)
TL	10	105	69	(0.66)	36	(0.34)
RH	7	93	46	(0.49)	47	(0.51)
RL	7	103	54	(0.52)	49	(0.48)
Total	51	619	333	(0.54)	286	(0.46)

*If the number of subjects in a session was uneven, the "extra" subject was randomly assigned to an existing group.

For purposes of internal validity, we run balancing tests across a wide range of pre-characteristics as well as baseline levels of the outcome variables, PGG and SVO. Table 2.3 contains a selected set of variables, in particular those that are significantly different at the 5% level and below. As expected, subjects appear to be significantly different based on some firm/work-related variables such as length of employment, bonus awareness, and the number of other subjects they have "close" relationships with. In addition, subjects appear to be different on age, education, and preferences for risk and competition. Finally, subjects contribute differently to the PGG at baseline across treatments. In the following section, we discuss how our estimation strategy deals with this unbalancedness.

TABLE 2.3: Internal validity balancing tests

Variables	N_i	All	CH	CL	TH	TL	RH	RL	p-value
Demographics									
Age	618	31.26	31.94	30.80	32.92	32.13	29.88	29.62	0.03**
Female	617	0.05	0.10	0.05	0.05	0.07	0.01	0.00	0.02**
Years of schooling	586	9.98	10.07	10.21	9.19	9.80	10.47	10.29	0.00***
Ethnicity[a]	617	2.66	2.67	2.70	2.74	2.51	2.62	2.65	0.71
Marital Status[b]	610	1.52	1.56	1.54	1.46	1.52	1.48	1.53	0.85
HH size	603	5.20	5.91	4.82	5.56	4.92	4.74	5.23	0.06
Poverty[c]	601	1.34	1.40	1.49	1.49	1.32	1.26	1.31	0.40
Behavioral									
Trust[d]	619	0.89	0.95	0.87	0.92	0.89	0.86	0.85	0.22
Fairness[e]	618	0.39	0.34	0.38	0.37	0.40	0.39	0.46	0.67
Risk seeking[f]	606	3.88	3.41	3.70	3.37	3.86	4.59	4.43	0.00**
Inequality averse[g]	619	2.51	2.70	2.35	2.75	2.44	2.37	2.42	0.18
Time[h]	605	202.20	191.68	186.75	157.57	192.07	184.13	300.70	0.26
Competition[i]	619	0.62	0.46	0.70	0.52	0.62	0.71	0.73	0.00**
Schwartz values[j]									
Benevolence	617	4.53	4.39	4.61	4.50	4.51	4.66	4.48	0.07
Conformity	618	4.57	4.54	4.64	4.60	4.55	4.53	4.51	0.67
Collectivism	619	0.73	0.62	0.73	0.71	0.78	0.75	0.76	0.15
Firm-related									
Months employed	611	42.60	41.18	45.74	49.41	45.17	42.75	30.20	0.00***
Monthly wage	610	374.19	377.49	390.38	373.99	369.43	365.85	366.87	0.18
Aware of bonus	619	0.69	0.46	0.72	0.62	0.71	0.77	0.82	0.00***
Works in team	614	0.45	0.55	0.45	0.51	0.38	0.46	0.36	0.06
Job satisfaction[k]	615	4.50	4.33	4.60	4.51	4.38	4.64	4.56	0.00***
Job type	606	4.79	4.61	4.92	4.56	4.89	5.39	4.44	0.16
Sector	602	4.64	4.89	4.49	4.26	4.97	4.96	4.41	0.08
Close relations[l]	615	1.18	1.16	1.14	1.53	1.24	0.58	1.30	0.00***
Outcomes									
PGG (stage 1)	619	0.49	0.47	0.46	0.50	0.53	0.45	0.52	0.04**
SVO (stage 1)	619	21.82	20.95	22.78	22.57	22.76	18.67	22.62	0.09
PGG (stage 3)	619	0.47	0.44	0.47	0.51	0.47	0.44	0.48	0.13
SVO (stage 3)	619	22.23	19.80	23.01	23.04	22.16	20.84	24.02	0.16

*** $p < 0.01$, ** $p < 0.05$. The last column is obtained by running a one-way ANOVA test. These values are also robust to running a seemingly unrelated regression model for continuous variables and a χ^2-test for categorical variables. The tests for baseline equivalence of outcomes (PGG and SVO) are additionally robust to a Wilcoxon rank-sum test.

Variable definitions (see questionnaires for additional detail): [a] 1=Akan, 2=Ewe, 3=Ga/Dangbe, 4=Krobo, 5=Hausa; [b] 1=married, 2=single, 3=separated, 4=divorced, 5=widowed; [c] number of adults per bedroom in the home; [d] 0=most people can be trusted, 1=need to be very careful trusting; [e] 0=most people take advantage, 1=most people try to be fair; [f] number of seeds out of 10 chosen that are risky; [g] based on payoff equalization or not (aka Fehr allocation activity); [h] average GHS needed in one month to sacrifice 100 GHS tomorrow; [i] based on choice to be paid relative to someone else (compete) in a marble activity; [j] based on Schwartz (1992); [k] 1=terrible, 2=unhappy, 3=mixed, 4=mostly satisfied, 5=pleased; [l] number of people known during experiment session.

Overall, the average participant is 31 years old, lives in a household with 5 persons (including children), has been employed by the firm for 43 months, and has a close relationship with 1 other person in the session.

2.2.5 Empirical strategy

Given we collected measures of prosociality in Stages 1 and 3 (i.e., at baseline/pre-treatment and follow-up/post-treatment) and there is evidence of some baseline imbalance, we estimate our treatment effects according to the following specification:

$$\Delta Y_i = \beta_0 + \beta_C C_i + \beta_R R_i + \beta_{Y_0} Y_{i0} + \beta_Z Z_i + \epsilon_i, \tag{2.1}$$

where ΔY_i is the difference in prosociality between Stages 1 and 3 at the individual level i; C_i and R_i are dummies for individual-level exposure to treatment, competition and random respectively. So, threshold (T_i) is taken as the control. Y_{i0} is the initial level of prosociality in Stage 1; Z_i is a set of covariates comprising the unbalanced characteristics in Table 2.3; and ϵ_i is an error term. We run these specifications for both PGG and SVO for the pooled sample as well as separately for the low- and high-dispersion subsamples.

To further tease apart mechanisms, we expand Equation 2.1 by adding interactions between the treatment dummies (C_i and R_i) and covariates of interest X_i. Among these covariates are (1) whether or not the subject is a winner (i.e., earned 15 or 12 depending on whether s/he is in the high- or low-dispersion condition); (2) typical behavioral measures such as risk and inequality aversion; (3) preferences for competition (see for example Brandts et al., 2009, Gneezy et al., 2009); and potentially relevant administrative/external variables such as (4) whether or not the subject engages in teamwork (i.e., a more prosocial context) in her/his usual job and (5) whether or not the subject is aware of the bonus the firm currently has in place.

We thus run the following specification:

$$\Delta Y_i = \beta_0 + \beta_C C_i + \beta_R R_i + \beta_X X_i + \beta_{CX} C_i X_i + \beta_{RX} R_i X_i + \beta_{Y_0} Y_{i0} + \beta_Z Z_i + \epsilon_i, \tag{2.2}$$

where all is as defined previously.

2.2.6 Hypotheses

In line with prior literature (e.g. Buser and Dreber, 2015), we expect the coefficient β_C to be negative indicating a larger decrease in prosociality in the competitive relative to the non-competitive (threshold) payment scheme. We also expect the decrease in

prosociality to be more pronounced in the high-dispersion treatments (where winners earn 15 and losers earn 5) than in the low-dispersion treatments (where they earn 12 and 8 respectively). This would be consistent with Lazear (1989), although some of the mechanisms are likely to be different, given sabotage is not possible in our context. In lieu of a theoretical framework, we elaborate on the potential mechanisms for these hypothesized effects in the context of existing literature.

Erkal et al. (2011) and Schurr and Ritov (2016) find that winners of a competition tend to behave in a less prosocial way than losers. Schurr and Ritov (2016) in particular demonstrate that merely remembering the moment of winning a competition is sufficient to increase cheating behavior. The implication for our context is that winning in a competition, thus alters prosocial behavior. So, winners might keep more money for themselves (in PGG or SVO) in Stage 3 if they think that their "superior" (winner) status entitles them to do so. Consistent with this finding, we would expect winners in the competitive payment scheme to decrease prosociality more in Stage 3 than winners in the non-competitive (threshold) payment scheme, i.e., $\beta_C < 0$.

The coefficient β_R further allows us to pin this down. In the random treatment the outcome is determined by sheer luck. So, a comparison of the effect of random and competitive treatments enables us to assess if income inequality alone explains the decrease in prosociality. If that is the case, we expect to observe a similar magnitude of the effect between competition and random treatments, i.e., β_C would be of a similar magnitude as β_R. However, if competition generates a feeling of rivalry and confrontation, we expect that the crowding out effect of prosociality would be larger in the competition than in the random treatment, so β_C would be larger than β_R. In summary, a significant effect for β_C but not for β_R would be more solid evidence that changes in prosociality are due to competition.

Evoked feelings of rivalry from competition (relative to the threshold treatment) could also lead to a decrease in prosocial behavior. For example, Kilduff et al. (2016) find that increased rivalry is related to "competitors" being more (1) concerned with their status and (2) performance-oriented. Similar mechanisms could be at play here.

Finally, perceived unfairness of the competitive payment scheme could also affect prosocial behavior (e.g Akerlof and Yellen, 1990). Beliefs about unfairness could lead to frustration and anger, which in turn discourage worker effort and demotivate them to behave prosocially. Subjects might perceive a competitive payment scheme as unfair (relative to the threshold payment), since there is an exclusive bonus at play that ultimately only one worker in the dyad will benefit from directly. This perception might be particularly pronounced for those who are (1) more inequality averse (e.g. Bartling

et al., 2009); (2) less used to incentive schemes as part of their day-to-day work environment (as proxied by not being aware of the firm's existing bonus system or not being used to working in teams); and (3) more inclined to compete (as proxied by our measure of preferences for competition).

2.3 Results

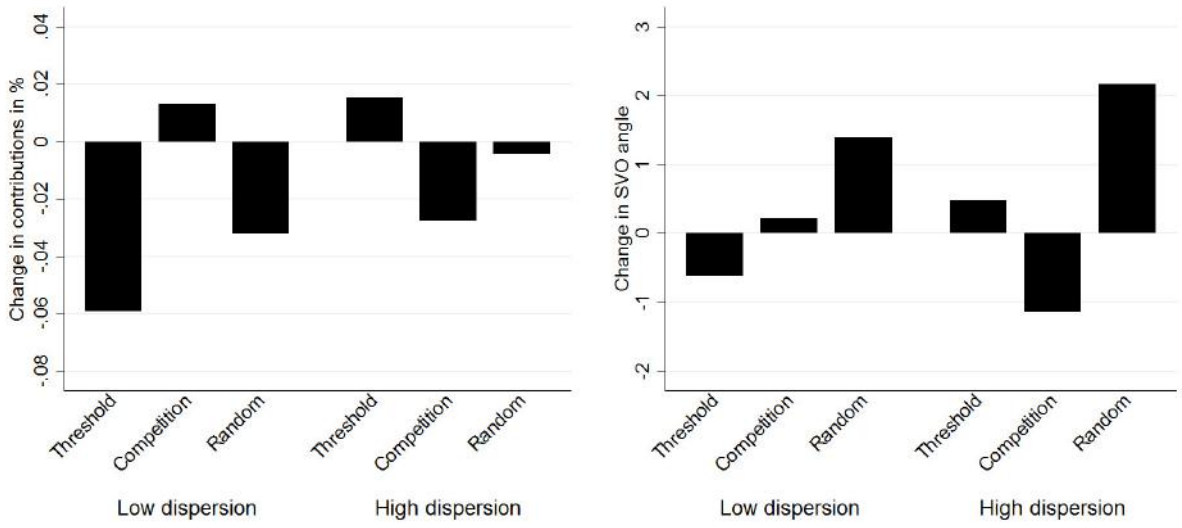
2.3.1 Descriptives

To get a sense of potential unconditional treatment effects, we start with some graphs. Figure 2.3 looks at the difference in PGG contributions and SVO angle between Stages 1 and 3 across threshold (T), competition (C), and random (R); low (L) and high (H) dispersion. Two aspects are striking:

1. The bars for T and C typically point in opposite directions. Evidence is somewhat mixed for C versus R.

2. Dispersion seems to matter. The bars for H point in the expected direction, specifically the contributions (and angle) in C *decreased* between Stages 1 and 3 while they *increased* in T. However, the bars for L show the opposite.

FIGURE 2.3: Changes in PGG contributions (left) and SVO angle (right) across treatments

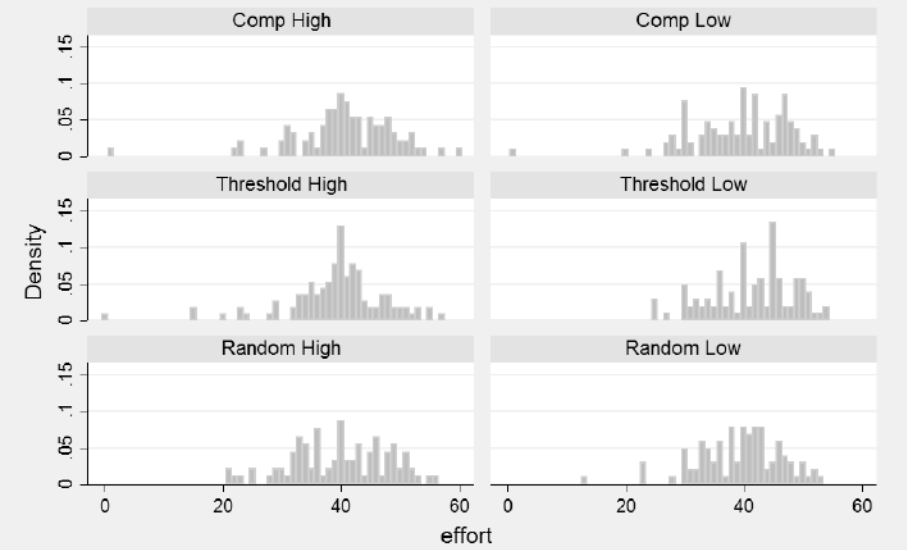


Collectively, these findings suggest that competition led to a greater decrease in prosociality across Stages 1 and 3 relative to the threshold treatments, but only when the dispersion between the winner's and loser's payoffs is high (i.e., when there is

much at stake). When the dispersion between the winner's and loser's payoffs is low, this effect is reversed. Indeed, statistical tests confirm these findings for PGG (t-test, p-value < 0.1).

Figure 2.4 shows the distribution of the number of ballpoint pens assembled across treatments. On average, subjects completed about 40 pens. Both, on average and over the whole distribution, there are limited statistically significant impacts across treatments. So, effort appears to be unaffected by the type of payment scheme, regardless of dispersion. Perhaps this is not so surprising when comparing T and C as both treatments create an incentive for higher performance. However, the finding that effort in R (the random treatment) is similar to that in T and C is more striking, given the outcome is determined by sheer luck. That said, this could be because subjects have exerted effort to attend the session or feel observed by the experimenters (and indirectly, the firm) and thus have the need to "do something" while sitting in the session.

FIGURE 2.4: Distribution of effort across treatments



As stated in Section 2.2.4, there are some baseline imbalances across treatments. So, the claims made in this section should be taken with caution. Next, we present conditional effects according to the specifications in Section 2.2.5. As stated in Section 2.2.4, there are some baseline imbalances across treatments. So, the claims made in this section should be taken with caution. Next, we present conditional effects according to the specifications in Section 2.2.5.

2.3.2 Treatment effects

Table 2.4 presents the estimates of the treatment effects according to the specification in Equation 2.1. Panel A presents the impacts on changes in PGG and Panel B presents the

impacts on changes in SVO. For the sake of brevity, the table does not explicitly report the coefficients for the Z covariates; however, the table footnote lists the covariates that are included when applicable. Results are available from the authors upon request.

In columns (1) and (2), we pool observations across high and low dispersion for the three payment schemes: competition, random, and threshold. The constant term is positive and significant, indicating that in the threshold treatment (the omitted category) there is an increase in prosociality from Stage 1 to Stage 3. The negative and significant effect of the baseline level of prosociality (PGG and SVO) indicates that in Stage 3 the dispersion in prosociality decreases. In other words, individuals with initially low levels of prosociality cooperate more in the third stage than in the first stage, while individuals with initially high levels of prosociality cooperate relatively less. Looking at the pooled data, competition seems to have no effect on prosociality. To investigate whether these effects may be heterogeneous with respect to high and low dispersion, we run separate regressions (columns 3-6).

Once we disaggregate by high and low dispersion, a different picture emerges. Under high dispersion (columns 3 and 4) there seems to be an increase in prosociality in the threshold treatment from the first to the third stage. Focusing on column (4), which controls for the full spectrum of baseline imbalances, PGG contributions are 16.8% higher post-treatment (although not statistically significant) and the SVO angle increases by 10.99 points post-treatment. However, PGG contributions increase by 5.6% less in the competition than in the threshold between Stages 1 and 3. Similarly, the SVO angle increases by 4.2 degrees (about 38%) less under competition than under threshold. Both of these effects are significant at the 5% level. Prosociality also increases less under the random treatment compared with the threshold treatment. Yet, while the magnitude of the decrease is not significantly different between random and competition for PGG contributions (3.25% lower increase in random), the SVO angle decreases more under competition than under the random treatment (0.151 points). These results suggest that competition has a larger crowding-out effect on prosociality than random payments. This finding is thus consistent with the hypothesis that competition indeed can erode prosociality (à la Lazear, 1989, Holmström, 2017).

Under low dispersion (columns 5 and 6), prosociality also increases in the threshold treatment from the first to the third stage. However, contrary to the case of high dispersion, there are no differential changes in prosociality, be it PGG or SVO, across the competitive and threshold or random and threshold treatments (once that we control for socioeconomic characteristics). There is also no significant difference in change in PGG or SVO between the competition and the random treatment. While we expected that competition under low dispersion would have less of an impact on prosociality

TABLE 2.4: Treatment effects on change in PGG and SVO (pooled, high, low dispersion)

	(1) Pooled	(2) Pooled	(3) High	(4) High	(5) Low	(6) Low
Panel A: Δ PGG contributions						
Competition	-0.0197	-0.0143	-0.0768**	-0.0561**	0.0372	0.0530
	(0.0231)	(0.0222)	(0.0276)	(0.0216)	(0.0296)	(0.0375)
Random	-0.0200	-0.00226	-0.0534**	-0.0325	0.0180	0.0522
	(0.0208)	(0.0227)	(0.0208)	(0.0193)	(0.0311)	(0.0421)
Baseline PGG (Y_{i0})	-0.444***	-0.455***	-0.422***	-0.427***	-0.457***	-0.473***
	(0.0453)	(0.0459)	(0.0665)	(0.0705)	(0.0629)	(0.0591)
Constant	0.214***	0.184**	0.234***	0.168	0.185***	-0.129
	(0.0266)	(0.0766)	(0.0394)	(0.108)	(0.0328)	(0.113)
R-squared	0.217	0.242	0.226	0.308	0.233	0.314
Panel B: Δ SVO angle						
Competition	-1.046	-1.018	-3.586**	-4.199**	1.328	1.074
	(1.102)	(1.110)	(1.409)	(1.854)	(1.540)	(2.062)
Random	1.504	0.948	-0.151	0.490	3.150*	2.869
	(1.003)	(1.235)	(1.189)	(1.201)	(1.598)	(2.281)
Baseline SVO (Y_{i0})	-0.661***	-0.651***	-0.655***	-0.657***	-0.679***	-0.635***
	(0.0499)	(0.0507)	(0.0687)	(0.0741)	(0.0741)	(0.0736)
Constant	14.86***	12.87**	15.44***	10.99**	14.45***	22.82**
	(1.368)	(5.123)	(1.787)	(5.048)	(2.155)	(8.972)
R-squared	0.324	0.347	0.302	0.341	0.357	0.405
Observations	538	538	262	262	276	276
Covariates[a]	NO	YES	NO	YES	NO	YES

[+] Robust standard errors clustered at the session level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
[a] Covariates: age, female, risk, inequality averse, poverty, competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session.

relative to high dispersion, we did not expect this effect to be statistically insignificant. These results thus suggest that competition may not always lead to a decrease in prosociality. It depends on the context; notably, how well/badly off the competition leaves winners relative to losers.

2.3.3 Mechanisms

To investigate potential mechanisms beyond those that are feasible using only our experimental variations, we first run the specification in Equation 2.2. Table 2.5 summarizes the effects for changes in prosociality across treatments for individuals of different characteristics X under high dispersion.[6] The first, third and fifth column present the effects for changes in PGG contribution and the second, fourth and sixth column presents the effects for changes in the SVO angle for comparisons between the competition and the threshold treatment (columns 1 and 2), between the random and the threshold treatment (columns 3 and 4) and the comparison between these two former comparisons (columns 5 and 6). The results for low dispersion are included in Table A.1 in Appendix A.

We find the following under high dispersion:

1. *Winners:* Those who win the competition are less prosocial after having been exposed to the competition than those who win in the threshold treatment. This effect is only significant for changes in PGG contributions. This is consistent with Erkal et al. (2011) who find that winners are more likely to behave in a selfish manner. However, in contrast to Erkal et al. (2011), this does not seem to be due to selection of less prosocial types into the winner position as this specification controls for various individual and social preferences as discussed previously. So, we think this is more likely due to winner-subjects feeling more entitled and thus, believing they deserved their payments more than had they been in the threshold treatment. High earners in the random payment treatment, however, do not behave less cooperatively compared with high earners in the threshold treatment. This suggests that indeed competition has a particular effect on winners that is not only due to higher income. This also relates to Gee et al. (2016) who find that when income is earned through performance, individuals use income differences as a heuristic to infer relative deservingness.

2. *Inequality aversion:* Based on an easy distribution task from Fehr et al. (2008), we classify individuals as inequality averse when they preferred the equal distribution over the unequal distribution in all three questions and *not* inequality averse otherwise. We find that those who are inequality averse decrease prosociality (PGG) in competition compared with threshold. The effect for SVO is also negative, but not statistically significant. This is consistent with findings from Cherry et al. (2005) who find that contribution levels are significantly lower when

[6] The effects reported in this table are equivalent to $(\beta_C + \beta_{CX}) + (\beta_0 + \beta_X)$ in Equation 2.2 (aka contrasts, e.g., in Stata).

TABLE 2.5: Heterogeneous effects on change in PGG and SVO (high dispersion; C vs. T)[a]

	(1) Δ PGG	(2) Δ SVO
Income effect		
loser	-0.0239	-4.3625
	(0.0562)	(3.3742)
winner	-0.1309***	-4.0296
	(0.0403)	(2.8920)
Behavioral variables		
not inequality averse	-0.0335	-3.7620
	(0.0322)	(3.2476)
inequality averse	-0.1214*	-4.6301
	(0.0682)	(3.4913)
risk seeking	0.0028	0.0439
	(0.0087)	(0.5227)
dislikes competition	-0.0871	-3.3085
	(0.0543)	(3.3379)
likes competition	-0.0676	-5.0836
	(0.0441)	(3.3031)
Work-related variables		
does not work in teams	-0.0411	-4.5677
	(0.0373)	(3.1905)
works in teams	-0.1138*	-3.8243
	(0.0617)	(3.2144)
is not aware of bonus	-0.1081**	-6.4739**
	(0.0489)	(3.0383)
is aware of bonus	-0.0466	-0.9182
	(0.0455)	(3.3706)
R-squared	0.3476	0.3710
Observations	262	262
Covariates[b]	YES	YES

[+] Robust standard errors clustered at the session level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

[a] This table presents contrasts across C and T, i.e., $(\beta_C + \beta_{CX}) - (\beta_0 + \beta_X)$ as discussed in Section 2.3.3. Contrasts across R and T are not shown.

[b] Covariates: PGG or SVO (stage 1), age, female, risk, preference for competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session.

TABLE 2.6: Effects of payment on fairness, rivalry, attachment (high dispersion)[a]

	(1) fairness	(2) rivalry	(3) attachment
Competition (C)	-0.287**	0.219	0.0799
	(0.134)	(0.168)	(0.438)
Random (R)	-0.395**	0.0391	-0.104
	(0.193)	(0.208)	(0.447)
Winner	0.0369	0.0433	-0.0360
	(0.0386)	(0.107)	(0.294)
$C\times$ Winner	0.177*	-0.0637	-0.271
	(0.0924)	(0.136)	(0.374)
Inequality averse	-0.000741	0.198*	0.651**
	(0.0560)	(0.114)	(0.277)
$C\times$ Inequality averse	0.0876	-0.257*	-0.614*
	(0.0983)	(0.154)	(0.368)
Works in teams	-0.00554	-0.0201	-0.228
	(0.0348)	(0.0987)	(0.213)
$C\times$ Works in teams	-0.113	0.106	-0.0330
	(0.105)	(0.142)	(0.340)
Is aware of bonus	0.0184	-0.109	-0.613**
	(0.0387)	(0.102)	(0.260)
$C\times$ Is aware of bonus	-0.0970	0.0636	-0.216
	(0.114)	(0.150)	(0.379)
Baseline PGG (Y_{i0})	-0.295***	-0.0228	0.123
	(0.113)	(0.123)	(0.297)
Constant	1.415***	-1.124***	2.395***
	(0.219)	(0.277)	(0.736)
R-squared	0.364	0.359	0.280
Observations	262	262	261
Covariates[b]	YES	YES	YES

[+] Robust standard errors clustered at the session level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

[a] This table only presents effects that were previously significant, particularly for interactions with the competitive treatment (C). Interactions with the random treatment dummy (R) are omitted.

[b] Covariates: age, female, risk, preference for competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session.

groups have heterogeneous (rather than homogeneous) endowments. In the random payment treatment, we do not observe differences in contributions to PGG or SVO between inequality averse individuals compared with the threshold treatment. Here again, competition appears to trigger a change in behavior compared with the random treatment.

3. *Risk seeking:* We find no significant effect of risk preferences on changes in prosociality between competition and threshold or for random and threshold, for neither PGG nor SVO. This is different from for example Teyssier (2012) who finds that risk aversion is significantly and negatively correlated with contributions of first movers; although we recognize that our impacts are identified on changes in prosociality and not contribution levels.

4. *Preferences for competition:* Competitive preferences are measured in the ex-ante survey with a simple marble game in three stages à la Niederle and Vesterlund (2007). We find that preferences for competition do not explain differences in PGG or SVO across treatments.

5. *Working in teams:* We find that participants who are used to working in teams, reduce prosociality in competition relative to threshold. As before, the effect is only significant for changes in PGG. This finding could imply that the erosion of prosociality may be exacerbated when competition is induced between members of same team rather than between teams. No such effects are observed in the random treatment. Future work should explore whether inter-team competition has a differential effect on in- versus out-group members.

6. *Bonus awareness:* Participants who are unaware of the firm's existing bonus also decrease prosociality in the competition compared to the threshold payment and in the random compared with the threshold treatment. The magnitude of this effect is higher for the competition than for the random treatment. This suggests that lack of prior exposure to related schemes can increase the negative impacts of newly implemented relative-performance schemes.

Finally, under low dispersion, none of the covariates significantly predict differential behavior across competition and threshold. That said, losers do have a greater (positive) point estimate (albeit statistically insignificant) than winners, possibly suggesting an effect à la Schurr and Ritov (2016).

2.4 Conclusion

In this study, we conduct a lab-in-the-field experiment with workers from an agribusiness in Ghana to test whether competitive (relative to individual) payment schemes crowd out prosociality in subsequent tasks. We thus partially revisit a question that has been addressed by Buser and Dreber (2015), in a context where the findings have the potential to immediately inform workplace and development policy. We particularly seek to understand underlying mechanisms by first experimentally varying the strength of the competition through the dispersion between the winner's and loser's payoffs and two interacting treatment variation with survey covariates as well as external, work-related variables.

When there is much at stake, i.e., when the dispersion between the winner's and loser's payoffs is high, we confirm prior findings: Competition crowds out prosociality. This effect seems to be mainly driven by (1) those who win the competition, (2) those who are inequality averse, (3) those who usually work in teams and (4) those who are unaware of an already existing bonus scheme at the firm. However, when there is less at stake, we find quite the opposite: Competition does not affect prosociality. Closer analysis indicates that prosociality is also eroded under random payments. Yet, the erosion of prosociality occurs in more dimensions (i.e., SVO) under competition than under random payment. Our results only partly replicate the findings by Buser and Dreber (2015) but are in line with Schurr and Ritov (2016) who find that subject's (dis)honesty is impacted by exposure to competitive environments. Furthermore, Drago and Garvey (1998) demonstrate that strong promotion incentives can crowd out helping behavior among coworkers. Overall, our findings suggest that the impact of competitive schemes (such as bonuses and merit pay based on relative performance) on cooperation in the workplace is likely to be context-specific. So, managers should keep the complexities of incentive systems in mind as they consider implementing them in the workplace (as alluded to by for example Lazear, 1989, Holmström, 2017). Since we report results from a stylized experiment, we need to be careful with proclaiming definite work-policy advice. For example, in companies coworkers interact frequently and repetitively. Several times over the course of a year, they may get feedback on their performance in form of a bonus pay. In the short-term, it has been shown that workers who once won the competition will be motivated to work in the subsequent working phase (Bandiera et al., 2013). Over a longer term, the bonus incentive may wear off and workers may reduce effort exerted. Furthermore, the workers who rarely receive a bonus may become dissatisfied with their job which can have negative consequences for the organization's efficiency in terms of less organizational support (Miceli and Mulvey, 2000) or crowding out of intrinsic motivation (Bowles and Polanía-Reyes,

2012). Moreover, incentives may not work as intended when social ties among coworkers are considered. The effect from an incentive pay on social interaction may also collude with the strength of bonds among coworkers (Ashraf and Bandiera, 2017).
Our findings leave some avenues for future research. First, our setting could be extended to include collaborative tasks where strategic complementarities are important. Second, the effect of different threshold levels on effort and subsequently prosociality could be explored. Third, it would be interesting to look at environments in which individual performance is not perfectly observable and could lead to perceived discrimination (e.g. Grosch and Rau, 2017a). Fourth, in lieu of a measure of cooperation and prosociality, future work could look at the impact of different payment incentives on subsequent effort provision/productivity tasks. Fifth, in light of the literature on gender differences in competition (e.g. Gneezy et al., 2003, 2009) and uncertainty aversion (e.g. Croson and Gneezy, 2009), future research could explore the differential impact of competitive payments on cooperation across women and men.

Chapter 3

Do discriminatory pay regimes unleash antisocial behavior?

This chapter is joint work with Holger A. Rau. We jointly developed the research idea, conducted the experiment and analyzed the data. I did the first write-up of the manuscript including introduction, experimental design, hypotheses, results, mechanisms and conclusion. We jointly polished the manuscript.

3.1 Introduction

Antisocial behavior at the workplace brings harm to an organization, its employees, or its stakeholders (Griffin and Lopez, 2005). The occurrence of antisocial actions is not rare (Charness et al., 2013) and may lead to substantial efficiency losses. Empirical evidence corroborates this and reports that US firms lose about $50 billion each year because of white collar crime, i.e., fraud and theft (Coffin, 2003).[1] This emphasizes the importance for management to design workplace environments which counteract worker resentment. Importantly, dissatisfaction at work is not only affected by management styles and personal interactions with colleagues. There is evidence that institutional factors such as pay regimes and pay transparency may crucially reduce worker satisfaction (French et al., 2000, Miceli and Mulvey, 2000, Card et al., 2012).

In the organizational economics literature, it is argued that antisocial behavior at the workplace can be unleashed if there is a trigger, a so-called *frustrator* (Giacalone and Greenberg, 1997). Such *frustrators* can lead to work dissatisfaction and increase workers' engagement in antisocial activities. Examples of these triggers are controversies with superiors (Geddes and Baron, 1997), co-workers (Skarlicki and Folger, 2004), or perceived unfairness in an organization (Neuman, 2004).[2]
A widely used pay regime are bonuses as they generate incentives to put forth effort (Lazear, 2000b). Importantly, the allocation procedure of bonuses is often not fair. It is possible that managers do not grant all employees similar and fair chances to get promoted. For example, Price (2012) experimentally shows that male managers discriminate against female employees in a context where performance information is available.[3] Furthermore, discrimination can also be unintentional and can arise from institutional procedures (French et al., 2000). That is, when performance is not impeccably observable since work processes are complex and the monitoring of effort and ability is imperfect (Berger et al., 2013). UK data show that more than one-third of financial professionals believe that bonuses given to top earners are unjustified and cause resentment in the office (CIMA, 2016).[4] We argue that discriminatory pay regimes which deprive workers of the opportunity to receive a bonus, might serve as a *frustrator*. In

[1] Similarly, Disselkamp (2004) reports that German firms bear costs of 5€0 billion because of inner dismissals, conflicts in the workplace, and high drop-out rates reflected in the number of staff on sick leave.

[2] Empirical evidence also shows that wage cuts damage work morale (Kube et al., 2013).

[3] An overview of experiments on discriminatory actions is given by Anderson et al. (2006). Riach and Rich (2002) summarize field-experimental evidence of discrimination against non-whites and women in markets.

[4] The importance of procedural fairness is also emphasized in experiments. The studies analyze the redistribution decisions of stakeholders or spectators and show that they are affected by the processes of how incomes are allocated (Konow, 2000, Cappelen et al., 2007, 2013, Akbaş et al., 2016).

particular, if (perceived) high performance is not rewarded, workers might become resentful which could ultimately trigger antisocial behavior.
Our study investigates whether discriminatory pay regimes lead to more pronounced antisocial behavior among co-workers compared to non-discriminatory pay regimes. "Unjustifiably-paid" workers, i.e., workers with a high (perceived) performance who receive no compensation, may feel particularly frustrated. Finally, we examine whether a discriminatory pay regime lowers prosocial actions toward co-workers (Buser and Dreber, 2015, Grosch et al., 2017).

To investigate the link between discriminatory pay regimes and antisocial behavior, we conduct a real-effort experiment. We vary the procedures of pay regimes and subsequently measure workers' engagement in antisocial actions. In the *Discrimination* treatment, half the participants are randomly selected and receive a zero payment. The remaining half are promoted and compete for bonuses. Here, relative performance within the group of promoted workers determines payments, i.e., the 50% best-performing subjects receive €15, whereas the 50% least-performing participants from this group receive €5. By contrast, in the treatment *Competition* all payments are justified by performance and there is no discrimination – *all* workers participate in a competition for bonuses. The competitive pay regime is characterized by the transparency of payments in accordance with subjects' relative performance. That is, a worker's performance is directly revealed by the payment she receives.[5] After subjects receive information on their payments, we measure antisocial behavior in a "joy-of-destruction (JoD)" game (Abbink and Sadrieh, 2009, Abbink and Herrmann, 2011). Workers who received no payment are paired with a worker from the paid group. Both subjects receive six canteen vouchers and simultaneously decide how many canteen vouchers of their paired player they want to destroy. After the JoD game, we implement a sequential prisoner's dilemma game to test whether a discriminatory pay regime dampens prosocial behavior.
JoD experiments find that people enjoy harming others, even though this action does not increase own monetary benefits (Abbink and Sadrieh, 2009, Abbink and Herrmann, 2011). Moreover, inequality-averse subjects engage in money burning to equalize incomes (Zizzo and Oswald, 2001, Zizzo, 2003, Fehr, 2016). Fehr (2016) finds that subjects burn money to retaliate against sabotage behavior. We extend that research, considering whether the source of income inequality (i.e., discriminatory compensation vs. equal chances), the transparency of remuneration, and unjustified payments, affect the level of antisocial behavior. In our set-up, inequality in payments is kept constant

[5]Workers who rank in the first quartile receive €15, workers who rank in the second quartile receive 5 €, and the 50% worst-ranked workers earn nothing.

among the worker groups across all treatments. We collect baseline measures on inequality aversion (Blanco et al., 2011) before the crux of the experiment to examine the explanatory power of individual preferences.

As expected, we find that low-income workers destroy a larger fraction of vouchers than high-income workers. Antisocial behavior is more pronounced in the *Discrimination* treatment compared to non-discriminatory pay regimes. Inequality aversion only has explanatory power for non-discriminatory pay regimes and cannot explain the large destruction level under a discriminatory pay regime. We find that the treatment effect is entirely driven by workers who receive an unjustified payment. We disentangle the effect of discrimination from receiving an unjustified payment in a treatment called *Random*. It turns out that workers engage in particularly antisocial behavior only when a discriminatory payment procedure leads to unjustified payments. In addition, discrimination crowds out prosocial behavior, as workers are less cooperative in the *Discrimination* treatment compared to the non-discriminatory pay regimes. Our findings may inhere interesting implications for organizational economics and the choice of appropriate pay regimes. Hence, we test for external validity and examine how different Big-5 personality traits (Costa and McCrae, 1989) are linked to our measure of antisocial behavior.[6] Our data suggest that observed behavior in the JoD game is a valid proxy for antisocial behavior at work, i.e., "neuroticism" and "agreeableness" can partly predict destructive behavior in our JoD game. This is in line with empirical data (Jones et al., 2011, Miller and Lynam, 2001) which show that workers with high scores in this trait are more likely to engage in antisocial actions.

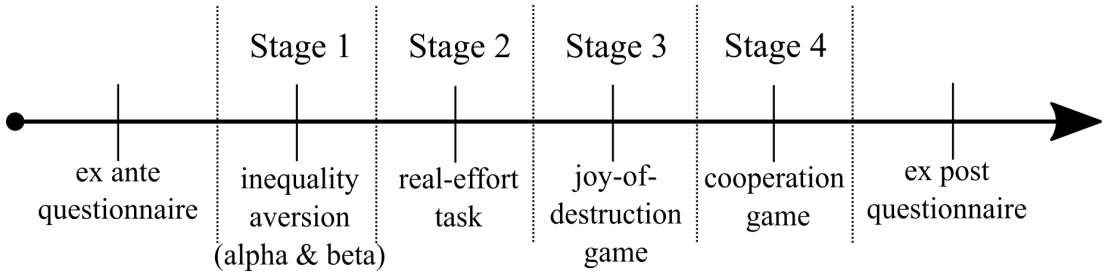
3.2 Experimental Design

3.2.1 Experimental Framework

The experiment consists of a short pre-survey, the main part with four stages, and an ex-post questionnaire. The sequence of actions is illustrated in Figure 2.1. After a brief questionnaire, we elicit the inequality-aversion parameters of the Fehr and Schmidt (1999) model with the method introduced by Blanco et al. (2011) at stage 1. The idea is that inequality aversion can partly explain antisocial behavior. The subsequent stages and the ex-post survey are summarized in the following. Full instructions can be found in Appendix B.

[6]This is motivated by empirical studies, which demonstrate that "neuroticism" and "agreeableness" are important predictors of antisocial behavior (for meta-analyses see Jones et al., 2011, Miller and Lynam, 2001). For instance, the trait "neuroticism" measures emotional instability and the level of experiencing anger and anxiety.

FIGURE 3.1: Experimental sequence



Stage 1: Inequality Aversion (α and β)

To measure inequality aversion, we apply the method of Blanco et al. (2011) and implement two different games. The first one is to measure β, aversion to advantageous inequality, and the second one serves to elicit α, aversion to disadvantaged inequality (see Fehr and Schmidt, 1999).

For measuring β a modified dictator game is implemented. The dictator is confronted with 21 pairs of competing payoff distributions. One option, in which the dictator receives 20 tokens and the recipient 0 (20,0), is kept constant. The other payoff distribution holds equal payoffs starting from (1,1), (2,2) etc. rising to (20,20) for dictator and recipient. Dictators have to decide for a switching point at which they prefer equal outcomes to being ahead. Participants' β varies between -0.025 and 1 and increases with the level of aversion of being ahead. After decisions are made, one of the 21 decisions is randomly drawn for payment and one participant of a dyad is randomly selected for the role of the dictator who determines payoffs.

To measure the parameter α that captures the acceptance of disadvantageous inequality, we ask the responder in an ultimatum game for the minimum acceptable offer. First, participants in the role of the proposer are given 20 tokens and make an integer offer of x tokens to the responder, keeping 20 tokens - x tokens to themselves. Responders have to indicate which minimum first-mover offer they would accept. The lowest amount that can be accepted is 0 tokens for herself and 20 tokens for the other participant. This amount determines an individual's parameter α ranging from 0 to 4.5. The higher the parameter, the more a person dislikes disadvantageous inequality. For calculating payments, we randomly draw one of the 21 possible payment allocations and randomly select one participant in each dyad for the role of the proposer and respondent respectively. If the minimum acceptance level exceeds the offer, both earn nothing. Otherwise, the randomly drawn payment allocation is implemented.

In the two inequality games, we apply an exchange rate of 1 token = €0.15. Across the games at stage 1, we use stranger matching and strategy method. Participants are always informed about exchange rates from experimental currency to real currency at

the beginning of each game.

Stage 2: Real-Effort Task

In this stage, subjects work on a real-effort task in which we exogenously vary the pay regime. The eight-minute task involves individually counting zeros in 5 x 9 matrices consisting of random numbers of zeros and ones. After completion of the task half the participants will be assigned to group A and the remaining half will be assigned to group B. Participants A receive 15€ or 5€, whereas participants B receive a zero payment. The payment allocation results in three different payment groups which is kept constant across treatments: half the workforce receive zero (type B), 25% earn 5€ (type A5), and 25% earn 15€ (type A15).
The pay regime, i.e., how participants are assigned to the groups, is our treatment variable. Participants are informed about the allocation before they work on the real-effort task. Information about the group assignment and payment is done by distributing envelopes with money (if any) and a card with a written 'A' or 'B' enclosed.
In three treatments we vary two determinants of pay regimes in the real-effort task: *(i) discrimination* to receive a bonus and *(ii) justification of all payments* by performance. "Discrimination" can be defined as treating a particular group of people differently, e.g., withdrawing such opportunities as the participation in a competition for bonuses. We define a pay regime where the pay procedures guarantee that better-performing workers will receive at least as much of a payoff as an equivalent-performing worker, as a pay regime where "all payments are justified." These two channels are switched on and off in the different treatments. A brief overview can be found in Table 3.1. We elaborate on the design details of the *Discrimination* treatment and the two non-discriminatory treatments in the following paragraphs.

TABLE 3.1: Summary of treatments

Treatment	non-discriminatory	all payments justified
Discrimination	x	x
Competition	✓	✓
Random	✓	x

In *Discrimination*, we create a pay regime with neither equal opportunities nor justified payments for all workers. Participants are randomly assigned to either the role of a type-A or a type-B worker. Here, the performance of type-B workers is ignored when determining the payoffs and therefore they are therefore discriminated against. In contrast, type-A workers are ranked within group A based on their total number of correctly solved matrices. The workers ranked in the upper half of the distribution

receive €15 and those ranked in the lower half receive €5.
In contrast, in the *Competition* treatment, all participants are treated equally and all payments are justified. In this case, a performance ranking among *all* participants is executed. Participants who rank in the first quartile of the distribution earn €15, participants who rank in the second quartile of the distribution earn €5. They are assigned to group A. Workers who rank in either the third or fourth quartile receive nothing and are assigned to group B.
To control for the impact of receiving an unjustified payment under a non-discriminatory pay regime, we run another treatment called *Random*. Here, the assignment to groups A and B is imposed randomly, as is the ranking within group A. Consequently, workers are paid independently of their performance. As all workers still have an equal chance of receiving a bonus, the pay regime is not characterized to be discriminatory. However, not *all payments are justified*, as the random pay mechanism ignores workers' individual performance. The treatment comparison of unjustifiably-paid workers between *Random* and *Discrimination* enables us to disentangle the effects of discrimination from unjustified payments. Before subjects work in the real-effort task, they are informed about the pay procedures. After the real-effort task, we hand out envelopes with some information on group affiliation, A or B, and we enclose the respective banknotes (if any) they earned in this stage.

Stage 3: JoD game

We modify the JoD game by Abbink and Sadrieh (2009). At the beginning, each participant is virtually endowed with six canteen vouchers.[7] A participant B is matched with a participant A. Type Bs are informed about the exact payment (5€ or 15€) of the matched partner in the previous stage. Type As know that they are matched with a type B. Every participant (type A and B) then simultaneously decide how many vouchers (between 0 and 6) they want to destroy from the matched participant.[8] Decisions are entered on a computer screen and destruction is free of cost. Subjects know that a random parameter, which destroys vouchers with a 50% probability, is implemented. In this case, the computer randomly destroys 0–6 vouchers (subjects know that all levels are equally likely). In the other 50% of the cases the participant's decision determines

[7]Students at the university hold a student identity card. This ID card is used to pay for meals at the university's canteen and can be topped up with credit. With one of our vouchers participants could top up their credit by 1 €.

[8]Subjects know that destroyed vouchers would become useless for both subjects. To explain the mechanism, we applied the wording "you can remove vouchers."

the number of vouchers destroyed. The implementation of the random component reduces moral costs since mean actions can be hidden under the guise of a possible random event (Abbink and Herrmann, 2011). In real life, in many cases antisocial actions such as stealing from or bullying co-workers cannot be traced back to one particular person. Only at the end of the experimental session do we inform participants about the number of devalued vouchers.

Stage 4: Cooperative behavior

We use a sequential-move prisoner's dilemma (Blanco et al., 2014) to measure cooperation. All participants (independent of their type) are matched in dyads and receive no information about the matched partner. The first mover makes a binary decision and chooses to cooperate or to defect. Similarly, the second mover responds with either cooperation or defection. If both defect, both players receive a payoff of 10 Tokens. If both cooperate, they receive 14 Tokens each. If the first mover cooperates and the second mover defects, the first mover earns 17 Tokens and the second mover earns 7 Tokens (for a game-tree illustration see experimental instructions in Appendix B). In this game we apply the strategy method. To determine the payments, one participant in each dyad is randomly selected into the role of the first mover and the other participant is selected into the role of the second mover. We apply an exchange rate of 1 Token=€0.20.

Questionnaires

At the very beginning of the session, participants fill out a short pre-survey in which we collect baseline measures on subjects' mood and risk preferences.[9] After the experimental session, participants are asked about their fairness perceptions of the pay regime among other questions about the experiment. Additionally, we capture personality traits using the BIG-5 query (Costa and McCrae, 1989) and conduct post-experimental questionnaires.[10] Finally, socio-demographic features such as age and study program are recorded.

3.2.2 Experimental procedures

We collected the experimental data from June to August 2016. In total, 252 students from the University of Göttingen took part in 13 sessions. In each session, we had

[9] For this purpose, we ask subjects to classify them based on their risk preferences on a scale between 1 (not prepared to take risks) and 10 (fully prepared to take risks) (Dohmen et al., 2012).

[10] We collect data on a measure for the acceptance of hierarchies, called "social dominance orientation" (Pratto et al., 1994).

16 to 24 participants. The experiment was programmed and conducted in z-Tree (Fischbacher, 2007). Subjects from various fields of studies were recruited with ORSEE (Greiner, 2015). The sessions lasted approximately 90 minutes and subjects earned €17 (ca. $16) on average.

3.2.3 Hypotheses

In this section, we derive our hypotheses. The real-effort framework is characterized by bonus payments which lead to income inequality between (high-income) type-A and (low-income) type-B workers. Hence, type-B workers, who are matched with type-A workers, may suffer a utility loss because they particularly dislike disadvantageous inequality (e.g., Fehr and Schmidt, 1999, Card et al., 2012). Experiments studying antisocial behavior report that subjects burn money to equalize incomes (e.g., Fehr, 2016, Zizzo, 2003, 2004). Thus, inequality-averse type-B workers may try to level the playing field with to type-A workers by burning vouchers. It follows that the amount of destroyed vouchers depends on type B's level of aversion toward disadvantageous inequality.

Hypothesis 1:
(a) Type-B workers destroy significantly more vouchers than type-A workers.
(b) The destruction level depends on the degree of type Bs' aversion toward disadvantageous inequality.

Giacalone and Greenberg (1997) argue that workers engage in antisocial behavior if they are dissatisfied. Worker resentment may be induced by perceived injustice in the workplace (Neuman, 2004). We vary an institutional aspect, i.e., discrimination of the pay regime, that potentially serves as a *frustrator*. Based on relative deprivation theory (Davis, 1959), workers might compare their payoff in their group relative to the payoff of another group and may feel deprived if they do not earn what they feel would have been procedurally fair. In the *Discrimination* treatment, type-B workers are deprived of the right to participate in a competition for bonuses (group A). They might therefore be particularly frustrated when comparing their payoff with the payoff of type-A workers. Laboratory evidence is provided by Bracha et al. (2015) who show in a real-effort experiment that remuneration differences affect subjects' work supply. The study finds that when subjects are aware of pay differences, the disadvantaged group reduces effort provision. In contrast to our *Discrimination* treatment, pay differences are justified

by performance differences in *Competition*. Hence, we expect that the difference in destruction levels between type Bs and type As is more pronounced in *Discrimination* than in *Competition*.
Generally, being paid unjustifiably may increase frustration and hence antisocial behavior. Under discrimination we expect that type-B workers who had a relatively high performance and expected a promotion may feel entitled to take part in the competition for bonuses (Gurr, 1970, Crosby, 1976, Clark and Oswald, 1996) and they therefore become more dissatisfied and antisocial than justifiably-paid workers. Moreover, as there is no competition at all in *Random*, unjustifiably-paid workers cannot feel deprived of the right to take part in a competition. Therefore, frustration should be lower and unjustifiably-paid workers become more antisocial under discrimination than under a random pay regime. Based on the reasoning above, we deduce the following three hypotheses.

Hypothesis 2:
(a) The difference in destruction levels between type B and type A is more pronounced in Discrimination compared to Competition and Random.
(b) Unjustifiably-paid workers destroy more than justifiably-paid workers.
(c) Unjustifiably-paid workers under Discrimination destroy more than unjustifiably-paid workers under the random treatment.

Beliefs about the sentiments of co-workers might differ depending on the pay regime. The discrimination of workers can lead to resentment within an organization (CIMA, 2016). Such an begrudged atmosphere might affect the willingness to cooperate with co-workers. Based on that, we deduce our third hypothesis.

Hypothesis 3:
Workers are less cooperative under Discrimination compared to workers under Competition or Random.

3.3 Results

This section first reports the main results of the JoD game. Afterwards, we scrutinize potential mechanisms. Finally, we examine whether discrimination also spills over to less cooperative behavior. When applying non-parametric tests we always report two-sided $p - values$ throughout.

3.3.1 Main results

Overall, the results show that, on average, type B destroys more vouchers (1.33) than type A15 (0.87) and type A5 (0.65).[11] The same pattern is supported by the data on the destruction frequency, i.e., type B destroys more often (37%) than type A15 (30%) or A5 (24%). In what follows, we merge the data on the destruction levels of A5 and A15 types as they do not significantly differ between each other in any of the treatments.[12]

FIGURE 3.2: Destruction levels and frequencies of type A and type B

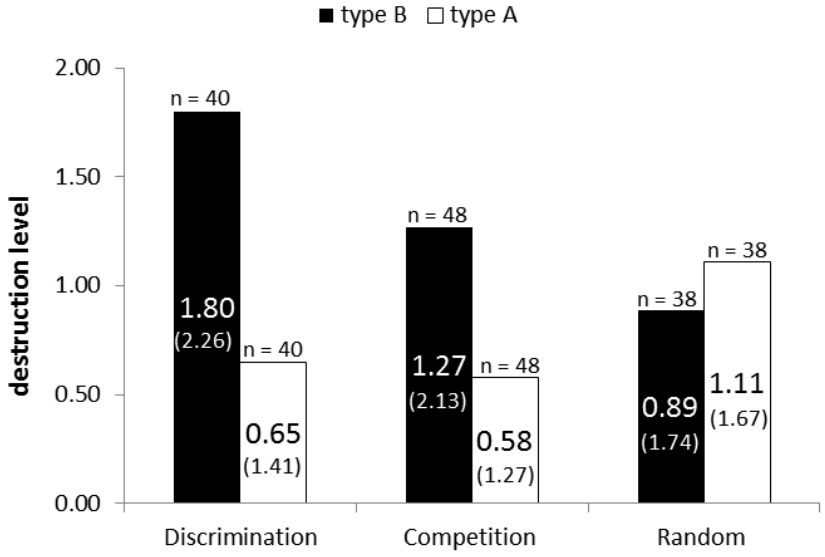


Figure 3.2 displays the average level of destroyed vouchers in the treatments and standard deviations in parentheses from type-A and type-B workers. We find that type-B workers destroy significantly more vouchers (1.33) than all type-A workers (0.76) (Mann-Whitney test, $p = 0.050$) confirming Hypothesis 1a. A closer look reveals that this difference is especially pronounced in *Discrimination*, where type-B workers destroy significantly more vouchers (1.80) than type-A workers (0.65) (Mann-Whitney test, $p = 0.009$). In contrast, in the *Competition* treatment no significant differences can be observed between the destruction levels of type Bs (1.27) and type As (0.58) (Mann-Whitney test, $p = 0.198$). Similarly, in *Random* the destruction level of type Bs (0.89) is insignificantly lower than the destruction level of type As (1.11) (Mann-Whitney test,

[11] We provide an overview of destruction levels, destruction frequency, performance, and observation numbers in Table B.1 in Appendix B.

[12] Data on destruction levels: all treatments, Mann-Whitney test, $p > 0.457$; data on destruction frequency: all treatments χ^2-tests, $p > 0.500$.

$p = 0.463$).[13] This confirms Hypothesis 2a, i.e., the difference between the antisocial behavior of type-B and type-A workers is more pronounced in *Discrimination* compared to non-discriminatory pay regimes.
Regarding the performance in the real-effort task, we find no difference between *Discrimination* (30.03) and *Competition* (30.65). This shows that the treatment manipulation between *Discrimination* and *Competition* did not affect workers' performance. In *Competition*, type assignment is dependent on performance by design and therefore type-B workers perform significantly worse than type-A workers (Mann-Whitney test, $p < 0.001$). In the *Discrimination* treatment, however, due to the random assignment to groups A and B, type-B workers have a similar performance to type-A workers (Mann-Whitney test, $p = 0.751$). Therefore, we claim that part of the workforce is indeed discriminated against in the *Discrimination* treatment. Not surprisingly, subjects' performance is lowest (24.43) in the *Random* treatment, where subjects' remuneration is independent of their performance.
To summarize, the results show that a discriminatory pay regime leads to more pronounced antisocial behavior between type-B and type-A workers than non-discriminatory pay regimes. This is in line with our expectation that discrimination at the workplace serves as a *frustrator* and ultimately evokes resentment and antisocial behavior.

Result 1:
(a) Type-B workers destroy significantly more vouchers than type-A workers under a discriminatory pay regime.
(b) In non-discriminatory pay regimes, such as Competition *and* Random, *there is no difference in antisocial behavior between type-A and type-B workers.*

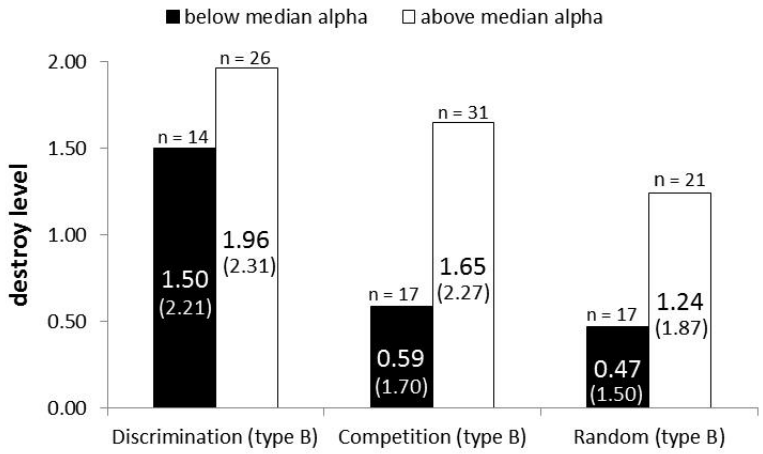
In the following sections, we will analyze the mechanisms of destructive behavior. In particular, we will examine the impact of inequality aversion and test Hypothesis 1b. Furthermore, we will test hypotheses 2b and 2c. That is, we analyze the effect of receiving an unjustified payment. That way, we can further disentangle the effect of discrimination, per se, from the effects of resulting unjustified payments. We conclude this section by linking personality traits to antisocial behavior.

[13] These results are robust when focusing on destroy frequencies. In *Discrimination* type-B workers destroy significantly more often (50%) than type-A workers (23%) ($\chi^2(1) = 6.545;\ p = 0.011$). No differences can be observed between the destruction frequencies of type As (32%) and Bs (23%) in *Competition* ($\chi^2(1) = 0.844,\ p = 0.358$) and *Random* (type A: 29%; type B: 37%) ($\chi 2(1) = 0.002,\ p = 0.963$).

3.3.2 Mechanisms: Inequality aversion

Inequality aversion between workers' incomes may be a potential source of why workers destroy vouchers. With respect to total incomes type Bs are always behind type As. Thus, a reduction of the income inequality is possible, if Bs burn more vouchers than their counterparts.[14] Figure 3.3 depicts type Bs' destruction levels conditioned on their aversion to disadvantageous inequality (α) (standard deviations in parentheses). The diagram conditions subjects on the median alpha of the whole data set (0.93). We distinguish between type-B subjects with an above/below median alpha.

FIGURE 3.3: Destruction levels of type Bs conditioned on aversion to disadvantageous inequality



On average, type-B subjects with an above-median alpha destroy more than workers with a below-median alpha. In the treatments *Competition* and *Random*, Spearman's rank correlation coefficients find significant positive correlations between workers' aversion to disadvantageous inequality and destruction levels (*Competition:* $\rho = 0.281$, $p = 0.053$; *Random:* $\rho = 0.389$, $p = 0.016$). In *Competition*, highly inequality-averse subjects destroy significantly more vouchers (1.65) than subjects with a low inequality aversion (0.59) (Mann-Whitney test, $p = 0.050$). Similarly, in *Random* workers characterized by high alphas clearly remove more vouchers (1.24) than workers with low alphas (0.47) (Mann-Whitney test, $p = 0.054$). This correlation does not exist in *Discrimination*, where destruction levels of high-alpha (1.96) and low-alpha subjects (1.50) do not significantly differ (Mann-Whitney test, $p = 0.543$). We therefore only find support for Hypothesis 1b in the non-discriminatory treatments but not in *Discrimination*. Thus,

[14] Note that if Bs are matched to A5s they could equalize incomes by destroying all of their vouchers. However, equalizing incomes also requires that A5s do not destroy more than one voucher from a type B. It is never possible for type-B workers to catch up to A15-workers.

antisocial behavior is obviously not (solely) triggered by inequality aversion when discrimination is at hand.[15]

Result 2:
(a) Workers' inequality aversion determines destruction levels in non-discriminatory pay regimes.
(b) Inequality aversion does not predict destruction levels under a discriminatory pay regime.

Since inequality aversion cannot explain the differences in destruction levels in *Discrimination*, we focus on an alternative explanation for type-B workers' antisocial behavior: unjustified payments.

3.3.3 Mechanisms: Unjustified payments

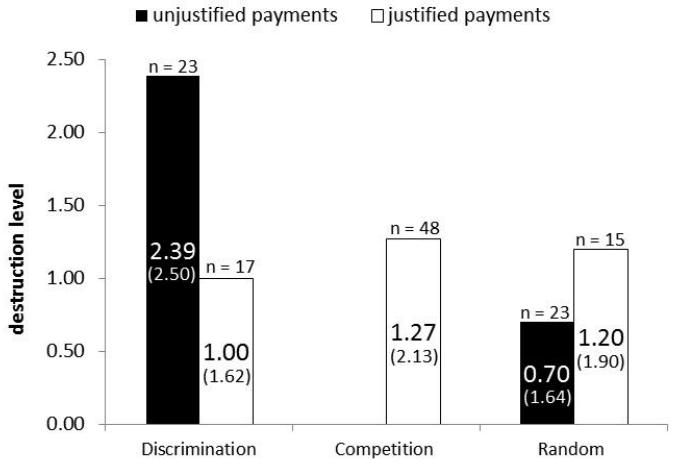
In this section we analyze the situations when workers received payments that are not justified by performance. These cases may occur in the *Discrimination* and the *Random* treatment. The idea of a potential mechanism is that particularly high-performing B types (who are aware of this) become frustrated when informed about receiving a zero payment (Hypothesis 2b). When comparing unjustifiably-paid workers from these two treatments, it is interesting to test whether workers feel more frustrated and destroy more under a discriminatory vs. a non-discriminatory pay regime (Hypothesis 2c).
To identify unjustifiably-paid workers, we focus on their productivity and classify them based on their precision in the real-effort task, i.e., the share of correct answers. Workers receive no feedback on their absolute performance. Therefore, we use the precision in the task as a proxy for (perceived) ability, assuming that workers who rarely make mistakes can assess their performance well. As a consequence of the simple task, high-ability workers with a high precision should be aware of their high performance. Indeed, we find a significant positive correlation between workers' precision and their belief about belonging to the 50% best subjects in their session (Pearson's correlation coefficient, $\rho = 0.132$, $p = 0.036$).[16] Therefore, conditioning subjects on their work precision serves as a valid proxy of "perceived high performance." Type-B workers with a high work precision are classified as unjustifiably-paid, whereas type-B workers with a low work precision are classified as justifiably-paid. For the precision measure, we use an indicator variable which takes on a value 1 if type-B workers have a precision which

[15] Focusing on beta we find that this measure proxies prosociality. That is, subjects with higher beta destroy significantly less in *Discrimination* ($\rho = -0.316$, $p = 0.047$) and *Competition* ($\rho = -0.250$, $p = 0.086$) but not in *Random* ($\rho = 0.076$, $p = 0.651$).

[16] We asked all subjects about their belief of their relative performance directly following their participation in the real-effort task. At this stage they had not yet been told of the payment sum they would receive. The belief elicitation was incentivized, i.e., subjects had to guess in a ranking which quartile their performance belonged to. Subjects received €0.50 if they correctly guessed their quartile.

is above the median of correctly solved puzzles (83.90%).[17]
Figure 3.4 displays the destruction levels of type-B subjects with unjustified/justified payments based on our classification (standard deviations in parentheses).

FIGURE 3.4: Destruction levels under "unjustified payments" and "justified payments"



A noticeable finding is that the destruction level is highest (2.39) for unjustified workers in *Discrimination*. These subjects destroy significantly more than type Bs with a justified payment in *Competition* (1.27) (Mann-Whitney test, $p = 0.045$). The data reveal a highly significant difference when comparing the destruction levels of unjustifiably-paid workers in *Discrimination* to the destruction levels of all (justifiably-paid) subjects in *Competition* (0.93) (Mann-Whitney test, $p = 0.007$). When focusing on unjustifiably-paid workers in the *Random* treatment, we find that the destruction levels are significantly lower (0.70) than in *Discrimination* (Mann-Whitney test, $p = 0.051$).

No treatment effect can be observed in the destruction levels of justifiably-paid subjects between *Discrimination* and *Random* (Mann-Whitney test, $p = 0.898$). As unjustifiably-paid subjects destroy less in *Random* than in *Competition*, we conclude that unjustified payments do not frustrate workers per se. Instead, workers become antisocial when unjustified payments result from discriminatory pay regimes. This confirms Hypothesis 2c.[18]

Result 3:
(a) Unjustifiably-paid workers behave more antisocially than justifiably-paid workers.

[17]The calibration is based on subjects' precision in our control treatments *Competition* and *Random*.
[18]One might think that this difference occurs as workers in *Random* generally performed worse. However, we find in *Random* that type-B workers with a performance of at least 30 destroy less (1.00) than justifiably-paid workers in *Competition*.

(b) Unjustifiably-paid workers destroy more vouchers under a discriminatory pay regime compared to a non-discriminatory pay regime.

3.3.4 Regression analyses

To better understand the functional interplay of discrimination, unjustified payments, and individual inequality aversion, we now conduct ordered probit regressions on the destruction levels of type Bs (Table 3.2). The dependent variable of our models corresponds to the number of destroyed vouchers (0-6) by type Bs. The regressions include treatment dummies (*Discrimination*, *Random*), which are positive for the corresponding treatment. The omitted treatment is the *Competition* treatment where all payments are justified. All regressions control for subjects' aversion to disadvantageous inequality (*Alpha*) and advantageous inequality (*Beta*). Model (2) analyzes the effect of the matched type A on B's decision to destroy, i.e., *Matched A15* is a dummy which is positive (zero) for a type A15 (A5). The model also adds interactions between *Matched 15* and the treatment dummies. In Model (3) we incorporate dummies which analyze the effect of receiving unjustified payments in *Discrimination* (*Unj. Discrimination*) and *Random* (*Unj. Random*). We refer to our classification applied in section 3.3.3, i.e., the dummies are positive (zero) for an above-median (below-median) precision in the task. The regression also controls for the interaction effects of unjustified payments when matched with a certain type A. Model (4) analyzes interactions between the inequality-aversion parameters, the treatment dummies, and the unjustified dummies. Due to space limitations we report these interactions in Appendix B. None of these interactions are significant.

Models (1) and (2) show that type-B workers who received an unjustified payment in *Discrimination*, destroy significantly higher levels than all type-B workers in *Competition*. In contrast, *Random* is never significant, i.e., no treatment effects can be found between the destruction behavior of low-income workers in *Random* and *Competition*. Model (2) finds that the treatment effect between *Discrimination* and *Competition* is robust when controlling for the income of matched type As. Generally, type Bs destroy moderately more vouchers from type-A workers who earned €15. However, the interactions of *Matched A15* with *Discrimination* and *Random* are both insignificant. Thus, only type Bs in *Competition* destroy more from A15s.[19] In Model (3) we additionally control for *Unj. Discrimination*, which is significant and positive. A conspicuous finding is that the treatment dummy *Discrimination* becomes insignificant at the same time.

[19]This is emphasized by a Pearson's rank correlation coefficient between the destruction level and *Matched A15* which is only significant in *Competition* but not in *Discrimination* and *Random*. Competition: $\rho = 0.267$; $p = 0.067$; Discrimination: $\rho = 0.112$; $p = 0.490$; Random: $\rho = 0.184$; $p = 0.268$.

TABLE 3.2: Treatment effects on destruction behavior of type B

	destruction level of type-B workers				
	(1)	(2)	(3)	(4)	(5)
Discrimination	0.435*	0.771**	0.192	-0.201	-0.229
	(0.259)	(0.379)	(0.463)	(0.817)	(0.847)
Random	-0.082	0.121	-0.281	-0.721	-0.802
	(0.280)	(0.383)	(0.437)	(0.827)	(0.843)
Unj. Discrimination			0.973**	1.749**	1.786**
			(0.493)	(0.850)	(0.837)
Unj. Random			0.782	0.189	0.175
			(0.513)	(0.824)	(0.844)
Inequality Aversion					
Alpha	0.184***	0.176***	0.183***	0.175	0.162
	(0.063)	(0.063)	(0.065)	(0.108)	(0.106)
Beta	-0.798**	-0.794**	-0.986**	-1.366	-1.385
	(0.358)	(0.363)	(0.386)	(0.873)	(0.863)
Matching with type A					
Matched A15		0.649*	0.675*	0.707*	0.705*
		(0.386)	(0.397)	(0.408)	(0.404)
Matched A15 × Discrimination		-0.588	-0.299	-0.309	-0.271
		(0.520)	(0.633)	(0.646)	(0.650)
Matched A15 × Random		-0.348	1.039	1.003	0.175
		(0.560)	(0.658)	(0.704)	(0.844)
Matched A15 × Unj. Discrimination			-0.453	-0.354	-0.399
			(0.702)	(0.757)	(0.752)
Matched A15 × Unj. Random			-2.411***	0.189	-1.990
			(0.881)	(0.842)	(0.880)
Pseudo R^2	0.053	0.064	0.103	0.115	0.117
Obs.	126	126	126	126	126
Inequality-Aversion Interactions	NO	NO	NO	YES	YES
Covariates	NO	NO	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Hence, the treatment effect reported in models (1) and (2) is obviously driven by type-B workers who received an unjustified payment in *Discrimination*. This confirms our previous results of section 3.3.3. Turning to the interactions with type As, we find that the significant effect of unjustifiably-paid type Bs is independent of the matching type, which we conclude from the insignificant interaction *Matched A15* $\times$ *Unj. Discrimination*. By contrast, *Matched A15* $\times$ *Unj. Random* is negative and highly significant. Thus, unjustifiably-paid type-B workers in *Random*, who are matched with A15, destroy significantly less than all other type-B workers.

Focusing on inequality aversion, we generally find in models (1)–(3) that type Bs, who are averse to disadvantageous inequality, destroy more as *Alpha* is positive and significant. At the same time, workers who are averse toward advantageous inequality destroy less, i.e., *Beta* is negative and significant. Importantly, this pattern vanishes in Model (4) where we interact the inequality-aversion parameters with the treatment dummies and the dummies of subjects who received unjustified payments. The treatment effect between unjustifiably-paid workers in *Discrimination* and *Competition* is robust, that is, positive and significant. Model (5) highlights that the latter finding also holds when controlling for covariates such as *female*, *age*, and *economics students*. None of these covariates is significant. This confirms the results we observed in section 3.3.2. In *Discrimination*, workers' antisocial behavior is not motivated by inequality aversion. Instead, unjustified payments matter.

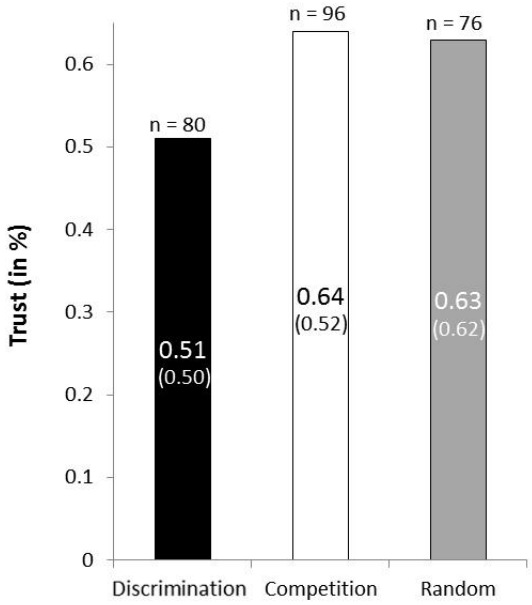
3.3.5 Effects on cooperation

Our main results demonstrate that discrimination leads to more pronounced differences in antisocial behavior compared to *Competition* or *Random*. In this section, we will test Hypothesis 3 and assess whether discrimination affects cooperative behavior as well. Cooperative behavior is measured by using a sequential prisoner's dilemma game which we played after subjects made their decisions in the JoD. These results might therefore be interpreted as long-term consequences on cooperative behavior and the work climate in general. Figure 3.5 reports first-mover cooperation levels (standard deviations in parentheses).

It can be seen that the average cooperation rate is not significantly different in the non-discriminatory pay regimes, i.e., 64% of subjects cooperate in *Competition* and 63% of subjects cooperate in *Random* ($\chi 2(1) = 0.003,\ p = 0.959$). In contrast, in *Discrimination* workers show a lower degree of cooperation (51%). The difference is weakly significant ($\chi 2(1) = 3.330,\ p = 0.068$). No treatment differences can be found for second-mover cooperative behavior.[20]

[20] In *Discrimination* 65% of the second-movers cooperate, 66% in *Competition*, and 68% in *Random*.

FIGURE 3.5: Share of cooperating subjects (in %) across treatments



Result 4:
After experiencing a discriminatory pay regime, workers show lower cooperation levels compared to non-discriminatory pay regimes.

3.3.6 Post-experimental questionnaire

After the experimental session, we conducted an ex-post questionnaire where subjects had to rate their fairness perception of the payment procedure. Moreover, we asked subjects for their motivations to destroy.

Fairness perception

To elicit fairness perceptions of payment regimes we asked participants: "How fair did you perceive your payment from stage 2 as being in the experiment (counting task)? Decide on a scale from 1 to 10, where 1 signifies very unfair and 10 represents very fair. You can grade your answer with the values in-between." We find that type-A workers report a higher degree of perceived fairness (*Discrimination:* 7.13; *Competition:* 7.54) as compared to type-B workers (*Discrimination:* 3.45; *Competition:* 4.96) in all treatments where workers' performance could basically matter. The Mann-Whitney test of the reported levels between type As and Bs is highly significant ($p < 0.001$). No treatment differences can be found between the reported levels of type As ($p = 0.393$). Turning to type-B workers, however, we find that subjects in the *Discrimination* treatment report

a significantly lower perceived fairness than in *Competition* (Mann-Whitney test, $p = 0.002$). Hence, the reported perceptions indicate that type-B workers in *Discrimination* might experience more *frustration*, which is in line with our previous findings that low-income workers become more antisocial under a discriminatory pay regime compared to a competitive pay regime.

Stated reasons to destroy

All participants were asked about their reasons for destroying or not destroying. We adjusted the selection of answers for the two different actions. The reason the payment was perceived as unfair was majorly stated by 36% of type-B workers under *Discrimination*. In the competitive environment, the majority of participant Bs (38%) justified their decision by referring to the existence of the random destroy parameter as an excuse. Hence, those subjects argued that even if they had not destroyed vouchers, it could anyway have happened that the computer destroyed vouchers from the matched partner. This behavior may be interpreted as pure "joy of destruction" where frustration plays a less important role.

In all treatments, 30% of As and Bs explained not having destroyed vouchers because they were satisfied with their performance.
To learn more on the external validity of our findings, we now investigate whether destroying vouchers in the JoD game may be a proxy for antisocial behavior in the field.

3.3.7 Personality traits and destructive behavior

Heterogeneity in personality traits can be an explanation for antisocial behavior. We proxy different dimensions of personality with a reduced version of the widely used measure called Big-5 or NEO Five Factor Inventory (Costa and McCrae, 1989). Meta-analyses from empirical studies have demonstrated that the traits conscientiousness, agreeableness, and neuroticism can be linked to antisocial behavior at the workplace (Jones et al., 2011, Miller and Lynam, 2001). Based on these studies, we briefly describe the traits and the predicted effect of antisocial behavior in the following paragraph.
Persons that score high in conscientiousness have increased control over themselves and act planned rather than spontaneously (Caspi et al., 2005). Our hypothesis is therefore that the more conscientious people are the less they will destroy. We predict a similar effect for the trait agreeableness, as it reflects kindness, empathy, and trusting behavior. We expect a negative relationship for the trait neuroticism since a high score suggests emotional instability and a tendency to experience anger and anxiety.
Table 3.3 presents ordered probit regressions on the impact of Big-5 personality traits

TABLE 3.3: The effect of personality traits on antisocial behavior

	destruction level of type-B workers			
	(1)	(2)	(3)	(4)
BIG-5 neuroticism	0.026**	0.030**		
	(0.013)	(0.013)		
BIG-5 agreeableness			-0.046**	-0.055***
			(0.018)	(0.019)
BIG-5 conscientiousness			-0.014	-0.012
			(0.017)	(0.017)
Random	-0.224	-0.236	-0.291	-0.327
	(0.280)	(0.278)	(0.279)	(0.283)
Discrimination	0.424*	0.480*	0.436	0.493*
	(0.253)	(0.265)	(0.278)	(0.288)
Pseudo R^2	0.017	0.048	0.036	0.048
Observations	126	126	126	126
Covariates	NO	YES	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

on destructive behavior for type-B workers. The dependent variable is the amount of vouchers destroyed of the matched partner. Spearman's rank correlation coefficient tests reveal that the trait *neuroticism* is correlated with *agreeableness* ($\rho = -0.271$, $p < 0.001$). Therefore, we run separate regressions for these traits. We include treatment dummies in all regressions and control for age, gender, and economics student in model specifications (2) and (4).

Regressions (1) and (2) confirm that higher emotional stability (lower score in *neuroticism*) leads to more destructive behavior. Higher ranks in the traits agreeableness lead to significantly less destruction in the JoD game. Also, higher ranks in conscientiousness lead to less antisocial behavior in our set-up, but not significantly so. All personality effects are in line with findings from empirical studies. This suggests that the destruction behavior in our experimental set-up may indeed be linked to similar channels like antisocial behavior in companies (Jones et al., 2011, Miller and Lynam, 2001).

Result 4:
The Big-5 personality traits of neuroticism and agreeableness predict destructive behavior in the JoD game.

3.4 Conclusion

In this paper, we analyze the impact of discrimination at the workplace on employee frustration and ultimately on employees' engagement in antisocial behavior. Generally, we find that a substantial fraction of workers engage in antisocial behavior, which is in line with other experiments in this area (e.g., Abbink and Sadrieh, 2009, Abbink and Herrmann, 2011, Charness et al., 2013, Fehr, 2016). Our novel result is that bonus schemes may lead to more pronounced antisocial behavior when they have discriminatory effects on workers. Particularly, discriminated workers who receive unjustified payments relieve their resentment in destructive behavior that hurts better-off co-workers. Only the payment procedure itself triggers more antisocial behavior. Hence, the findings highlight the importance of the appropriate design of institutional factors such as pay regimes to maintain worker satisfaction.

A closer look at our data reveals that in non-discriminatory pay regimes, antisocial behavior can be partly predicted by individual social preferences in the form of inequality aversion (Zizzo, 2003). More precisely, low-income workers who are highly inequality averse destroy more vouchers from high-income workers in a setting where discrimination is absent. In a regime with discrimination, however, inequality aversion is not the driving factor. Here, unjustifiably-paid workers, that is, workers who received a lower payment than deserved by performance, become particularly fed up with the pay regime and act more antisocially.

We are aware that we are reporting the findings of a stylized laboratory experiment. However, since the pay procedure is exogenously varied by the experimenter and antisocial behavior is harmful for the co-worker, we can exclude that the motive of retaliating intentional discriminatory actions by a superior explains the behavior. In our *Discrimination* treatment, it is a random process which selects employees who participate in the promotion mechanism. Although one may argue that this procedure is somewhat artificial, we believe that in reality it nevertheless resembles many promotion mechanisms. An important reason is that promotion decisions are often the outcome of institutions in the form of promotion boards where many persons are involved in the decision-making. Thus, it is likely that employees are not able to trace back the source of decisions that come into effect. Hence, employees may be frustrated at the outcome of this institutional promotion mechanism, but it is hardly possible to retaliate against a (responsible) superior. Moreover, to strengthen the external validity of antisocial behavior, we successfully linked personality traits, i.e., the Big-5 personality traits, in our analysis with subjects' destructive actions. Taken together with the insights from empirical studies on the impact of personality traits on antisocial behavior (Hershcovis et al., 2007, Jones et al., 2011, Miller and Lynam, 2001), we demonstrate that destructive

behavior in our experiment may be similar to antisocial behavior in real life.
Our findings contribute to managerial economics and the design of fair procedures in pay regimes to mitigate worker frustration. A large strand of the literature emphasizes that workers' intrinsic motivation is sensitive to the inappropriate use of financial incentives (e.g., Gneezy and Rustichini, 2000, Benabou and Tirole, 2003, Bowles and Polania-Reyes, 2012, Gneezy and Rey-Biel, 2014). We draw on this and show that employees do not only respond negatively to the "wrong" use of financial incentives per se. Instead, we emphasize the importance of the appropriate design of pay institutions and highlight that discriminatory pay regimes may cause antisocial behavior. Due to the fact that antisocial behavior often raises high costs, these insights may help to achieve higher workplace efficiency. For instance, our study may add interesting new insights for the design and conduct of job promotion mechanisms. Our results particularly emphasize the importance of transparency and equality of opportunities when bonuses are granted to maintain worker satisfaction.

Chapter 4

Gender differences in honesty: The role of social value orientation

This chapter is joint work with Holger Rau. We jointly conducted the experiment and wrote down the manuscript. Holger Rau has been mainly responsible for the analysis whereas I primarily worked out the other sections such as the introduction, the literature review, the hypotheses, the experimental design and the discussion.

4.1 Introduction

Honesty plays a key role in many spheres of life, such as purchasing tickets on buses, returning excessive change in restaurants or accurately making a tax declaration. The enforcement of honesty is imperfect and sometimes the norm is violated. In the notation of Erat and Gneezy (2012), it can be differentiated between three different situations: Altruistic white lies, where dishonesty increases the payoff of somebody else but not one's own payoff; Pareto white lies where dishonesty pays off for all parties; and selfish black lies where dishonesty benefits the liar, but comes at a cost for another party. In this paper, we focus on selfish black lies. In such situations, rational people are dishonest to the full extent to maximize their monetary payoff irrespective of the negative externality. However, honesty is a social norm and people face psychological costs when violating this norm since it might induce guilt (Battigalli and Dufwenberg, 2007) or a disadvantageous change in self-perception (Bénabou and Tirole, 2011). Studies in the domain of the norm honesty confirm that psychological costs affect behavior. Psychological costs can be varied in experiments and behavior differs according to priming (Cappelen et al., 2013, Cohn et al., 2015), moral balancing (Ploner and Regner, 2013, Clot et al., 2014) and contextual cues that allow for self-delusion/self-justification (Mazar et al., 2008, Shalvi et al., 2011, Jiang, 2013, Kajackaite and Gneezy, 2017). However, in the majority of these studies individual-level motives are neglected as an important determinant of honest behavior. In other words, if the context is varied and honest behavior is compared between different contexts, it is not clear which characteristics or preferences make people respond differently.

In this study, we fill this gap and examine how the individual-level motive of social preferences affects honest behavior. Social preferences can reflect preferences for payoff distributions. In turn, engaging in dishonest behavior affects payoffs. As a consequence, social preferences could explain why some people behave dishonestly and some people act honestly in the same situation. People with more pronounced social preferences, i.e., people who are relatively more prosocial, might face higher psychological costs when their immoral act has negative payoff consequences for another person's payoff. Thus, more pronounced social preferences might translate into behaving more honestly to avoid a negative effect on somebody else's payoff.

To test the link between social preferences and dishonest behavior, we run a laboratory experiment. First, we elicit subjects' social value orientation (SVO) with the measure of Murphy et al. (2011). This measure reflects people's magnitude of concern for other people's payoffs. Here, subjects make decisions on payoff distributions for themselves and another person. Based on these decisions, an individual SVO angle can be calculated for each subject. A lower angle reflects a higher value put on one's own monetary

payoff, whereas a relatively high angle reflects concern for another person's payoff. Subsequent to the elictiation of SVO we use a die rolling game to elicit dishonest behavior (Fischbacher and Föllmi-Heusi, 2013). Subjects are asked to roll a die 10 times and report the number after each cast. Reporting higher numbers is rewarded with higher monetary gains. Dishonest behavior in this game can be called a selfish black lie (Erat and Gneezy, 2012). The reason is that dishonesty guarantees gains for the liar and bears costs for the experimenter. Using the mean of the 10 die rolls allows us to investigate individual-level dishonest behavior. When we refer to dishonest behavior in our study, we mean that although people are supposed to tell the truth they misreport the die roll. Regarding behavioral predictions, we expect that with an increasing SVO angle, less misreporting will occur.

Our experimental design allows us to draw new insights for existing gender differences in honest behavior and their origins. Overall, women have been found to be more honest than men when it comes to selfish black lies (see for meta-analyses Abeler et al., 2016, Capraro, 2017). Thus, in our set-up we expect that, on average, men will report higher die rolls than women. An additional contribution of this paper is to explain this commonly found gender effect in honesty by gender differences in social preferences.

According to Rosenbaum et al. (2014), "[..] it seems that the experimental agenda regarding honesty could greatly benefit from having a more precise understanding of underlying individual-level motives and how they aggregate to prosocial norms, which would provide a better understanding of how dishonest behavior may be impeded" (p.194). We aim to fill this gap and our results show that individual social preferences matter for explaining differences in honest behavior. With an increasing SVO angle, subjects are more honest. In other words, prosocial subjects behave more honestly than individualistic subjects. Besides this novel result we confirm previous findings of gender differences, i.e., women are more honest than men. Focusing on gender and SVO reveals that women's SVO is indeed more pronounced than men's. In a mediation analysis we find that the gender effect can be explained by a difference in the SVO between men and women. As some studies found significant effects from individual risk tolerance, age and having an economics background on honest behavior (Childs, 2012, Friesen and Gangadharan, 2012, Conrads et al., 2013, Kajackaite and Gneezy, 2017), we control for these variables and find that our results are robust.

In the next section we will give an overview of the literature. In section 4.3, we will derive our hypotheses. The experimental design will be explained in section 4.4. Results are presented in section 4.5. Section 4.6 concludes with a discussion.

4.2 Literature review

In recent years, there has been a fast-growing experimental research interest in economics and psychology that study factors affecting dis/honest behavior. In the following, we will give an overview of related topics to emphasize the novelty of our paper. For a more exhaustive literature review see Rosenbaum et al. (2014) and for meta-analyses Abeler et al. (2016) and Capraro (2017).

Overall, the strength of norms vary on a societal level. Honest behavior differs across countries, which has been attributed to economic growth rates and culture (Hugh-Jones, 2016) as well as political systems (Ariely et al., 2015). There is only scarce literature on individual-level characteristics and factors influencing honesty. Experimental evidence indicates that individual risk aversion might impede dishonest behavior (Friesen and Gangadharan, 2012, Kajackaite and Gneezy, 2017) since risk-averse individuals might be afraid of getting caught cheating and are therefore more reluctant to behave dishonestly. People with an economics background have been found to be more prone to acting dishonestly (Friesen and Gangadharan, 2012, López-Pérez and Spiegelman, 2012). Moreover, individual hormonal balance of testosterone and cortisol foster dishonest actions (Lee et al., 2015). Shalvi and De Dreu (2014) demonstrate that higher levels of the hormone oxytocin causes increased dishonesty to benefit members of an in-group.

In contrast to the scarce literature on individual-level motives, there is vast research on contextual variables and their effects on honest behavior. People have non-consequential preferences meaning that the same monetary benefits are treated differently depending on the situation (Capraro, 2017). For example, there is evidence that people are more honest in personal relationships (DePaulo et al., 1996, DePaulo and Kashy, 1998, Chakravarty et al., 2011) and that people are also more willing to be dishonest to benefit other people they feel close to (Cadsby et al., 2016). Perceived social closeness can be decreased, in a competitive set-up where behaving dishonestly yields an advantage over somebody else. Here, the amount of money at stake fosters dishonesty (Conrads et al., 2014, Faravelli et al., 2015).

Apart from the absolute amount of money, the payoff consequences for oneself relative to another party are an important determinant of the situation. As depicted in the introduction, it can be differentiated between selfish black lies, Pareto white lies, and altruistic white lies. Overall, Erat and Gneezy (2012) have shown that people are most willing to engage in Pareto white lies and least willing in altruistic white lies. Focusing on gender effects the evidence is mixed. Erat and Gneezy (2012) find that women are more dishonest than men in telling an altruistic white lie. However, Biziou-van Pol et al. (2015) find the exact opposite. A recent meta-analysis from Capraro (2017)

reveals that, overall, men are more willing to be dishonest than women in an altruistic-white-lie situation. For Pareto white lies the experimental evidence suggests only weak correlation, implying that women are more honest than men (Capraro, 2017). In contrast, several studies find that women are more honest than men in selfish-black-lie situations (Dreber and Johannesson, 2008, Friesen and Gangadharan, 2012, Houser et al., 2012, Fosgaard et al., 2013, Cohn and Maréchal, 2015, Kajackaite and Gneezy, 2017) and few studies that strive to replicate these gender effects have not been successful (Childs, 2012, Gylfason et al., 2013).[1] Abeler et al. (2016) do not differentiate between different types of lies but find that, in general, women are more honest than men. Capraro (2017) confirms that women are more honest than men in a meta-analysis of selfish black lies.[2] It is not clear yet why women behave differently compared to men. Kajackaite and Gneezy (2017) find the common gender effect in the standard die rolling game from Fischbacher and Föllmi-Heusi (2013). They also implemented a modified version called the "mind game" (Jiang, 2013) in which the gender effect vanishes. In this game, the psychological costs of lying can be seen as reduced since there is room for self-delusion and participants can be very certain of not getting caught lying. The result suggests that women might exhibit higher costs of being dishonest due to fears of being caught. Erat and Gneezy (2012) further point out that "it appears as if women have a higher cost of lying, but at the same time are more sensitive to another person's payoffs" (p.724).
There is only scarce literature on how social preferences affect dishonest behavior. Cappelen et al. (2013) find that people who pass on a higher share in a dictator game are more likely to communicate an honest message to a counterpart in a Pareto-white-lie situation. In a study from Biziou-van Pol et al. (2015) the order of games is reversed and they measure the effects from dishonesty on cooperation in a prisoner's dilemma game and altruism in a dictator game. Their results are in line with Cappelen et al. (2013), i.e., Biziou-van Pol et al. (2015) find that altruism and cooperation are positively correlated with honest behavior when focusing on Pareto white lies. However, in an altruistic-white-lie situation they find the opposite, that is, altruism and cooperation are negatively correlated with honest behavior. A meta-analysis from Balliet et al. (2009) points out that prosocial subjects are more sensitive to social norms and evaluate social-dilemma situations in terms of morality. By contrast, individualistic subjects assess such dilemmas in terms of strength and power (Van Lange and Kuhlman, 1994,

[1] There are many more studies with similar experimental designs that do not report any gender effects. In this study, we will give one explanation as to why the gender effect has sometimes been found but not in all cases.

[2] Groups seem to make similar decisions in terms of violating the norm of honesty than individuals (Muehlheusser et al., 2015). Even the gender effect has been replicated in groups, i.e., female groups lie less than male or mixed gender groups which has been shown in the lab (Muehlheusser et al., 2015) and in the field (Azar et al., 2013).

De Cremer and Van Lange, 2001). If we assume that men and women differ in their SVO, this might explain honest behavior. To our knowledge, we are the first to investigate the link of SVO on honesty as well as bridging gaps in the understanding of gender differences in honest behavior.

4.3 Hypotheses

A situation where people have the opportunity to earn extra payoffs by being dishonest at the expense of the experimenter (selfish black lie) generates an ethical dilemma. The decision to behave dishonestly is a rational calculation in which the individual weighs the personal payoff against the expected costs and is dishonest if the net payoff is positive (Okeke and Godlonton, 2014). Since a dishonest act has implications for both one's own payoff and somebody else's payoff, we expect that the SVO angle as a general measure for distributional money preferences may predict dishonest behavior. People who are more concerned about their own payoff might derive higher utility from a higher own payoff (Erat and Gneezy, 2012). Therefore, people with relatively low SVO angles benefit to a greater extent from being dishonest since their utility increase from a larger payoff is relatively high. In contrast, people with relatively high SVO angles place high valuation in other peoples' payoff. They might face larger costs when considering whether to be dishonest since it negatively affects somebody else's payoff. Based on this argumentation, we deduce our first hypothesis.

Hypothesis 1:
Dishonest reporting decreases with an increasing SVO angle.

According to gender theories in psychology, women tend to be more collectively-oriented (Karau and Williams, 1993), more altruistic (Piliavin and Unger, 1985), and are expected to possess higher levels of communal qualities (Bakan, 1966) than men. Women often conform to these normative gender expectations as they might otherwise be sanctioned and disliked (Heilman and Okimoto, 2007, Rudman and Phelan, 2008). Many economic experiments confirm these findings and demonstrate that women commonly cooperate more often in social dilemmas (Cadsby and Maynes, 1998, Ortmann and Tichy, 1999, Van den Assem et al., 2012), and may offer more in dictator games (Eckel and Grossman, 1998, Andreoni and Vesterlund, 2001, Croson and Gneezy, 2009, Rand et al., 2016). Some papers find mixed evidence. Croson and Gneezy (2009) argue that experiments often differ in their design which may cause these results, as men's

decisions are less context-specific than women's (Gilligan, 1982).[3] Building on the psychological and experimental economic evidence that women tend to cooperate more and behave more altruistically, we expect that, on average, they hold higher SVO angles than men. Apart from that, we expect women to behave more honestly. As pointed out earlier, women are found to be more honest in selfish-black-lie situations (Abeler et al., 2016, Capraro, 2017). We formulate Hypothesis 2.

Hypothesis 2:
(a) On average, women have a higher SVO angle than men.
(b) On average, women are more honest than men.

Building on hypotheses 1 and 2, we expect that individual SVO angles can (at least partly) explain why women are more honest than men. When women hold on average higher SVO angles than men, and higher SVO angles translate into more honest behavior, the SVO angle might explain the predominantly observed gender effect. Therefore, we formulate Hypothesis 3.

Hypothesis 3:
The individual SVO angle mediates the effect of gender on dishonest behavior.

There is evidence that women are less risk tolerant than men (Charness and Gneezy, 2012). People with a lower risk tolerance might be more afraid of getting caught being dishonest (Friesen and Gangadharan, 2012, Kajackaite and Gneezy, 2017). To control for this potential effect we will include risk tolerance in our analysis. In addition, age has been found to increase dishonest behavior for men (Friesen and Gangadharan, 2012) and a professional economics background might increase dishonest behavior for women (Friesen and Gangadharan, 2012). It is possible that economists might more readily maximize their individual payoff. We will therefore include age and enrollment in an econ program as additional covariates to check if our results are robust.

[3] Rand et al. (2016) add to this and demonstrate that women have internalized their gender role and are more altruistic when acting intuitively. In a situation where women decide deliberately, salience of the female gender role increases altruism. This may cause inconsistent findings as experimental settings differ in design and cultural context.

4.4 Experimental design

The experimental design for this study comprises two stages. In the first stage, we elicit subjects' individual Social Value Orientation (SVO). In the second stage, we apply a modified version of the die rolling game introduced by Fischbacher and Föllmi-Heusi (2013) to elicit dishonest behavior at an individual level. In the die rolling game, behavior affects payoff distributions between oneself and the experimenter. This simple game rules out strategic concerns that might lead to heterogeneous effects.[4] The elicitation of SVO at the very beginning of the experiment allows us to make inferences from general distributional preferences on the domain of honesty where distributional preferences could also play a role. As this experiment took less than 10 minutes we embedded it in another experiment (Fischbacher and Föllmi-Heusi, 2013).[5]

To elicit SVO we use the measure by Murphy et al. (2011). Here, participants are matched in dyads. There are two roles – an active decision-maker and a passive player who has to accept the decision of the other person. Participants are confronted with six different decision sets each consisting of nine different money distributions. In each situation subjects have to choose the preferred money allocation for themselves and their matched partner. For payoff decisions, we present the original amounts used in Murphy et al. (2011). The exchange rate is 1 point = €0.03. At the end, one player was randomly assigned the role of the active decision-maker and one out of the six decision sets is randomly selected for payment. The other player is passive and has to accept the allocation. An SVO angle can be calculated for each person by evaluating the participant's decisions in the active role. The angle allows a classification into four groups of altruistic, prosocial, individualistic, and competitive types. The types differ in the magnitude of concern they have for other people's payoff. For example, individualistic types mainly maximize their own payoffs whereas prosocial types maximize the sum of earnings for themselves and the matched participant. Murphy et al. (2011) advises preferably using the continuous measure of the SVO angle. We follow this recommendation in our analysis but for illustrative reasons we refer to the categorical classification as well.

[4]Strategic concerns can play a role in the so-called "sender-receiver games" often used to trace dishonesty (e.g., Gneezy, 2005, Cappelen et al., 2013). Here, person A has an information advantage and can decide to forward some honest advice or dishonest advice to person B. Person B does not need to follow the advice. The action of person B, however, has payoff consequences for both. Person A might therefore form beliefs about person B's response to the advice. These individual strategic considerations could have confounding effects which is why we opted for a game that excludes this determinant.

[5]The experiment encompasses three treatments and focuses on the role of distributive justice on antisocial behavior (Grosch and Rau, 2017b). The data of the die rolling game do not significantly differ between the treatments (for all pairwise comparisons we find for two-sample Kolmogorov-Smirnov tests that $p > 0.6$).

In the die rolling game, subjects are asked to roll a die 10 times and report the number afterwards. Reporting higher numbers is rewarded and participants receive the die roll times €0.2, e.g., rolling a three yields €0.6. The only exception is the number six where subjects earn zero. Note that, Fischbacher and Föllmi-Heusi (2013) demonstrate that dishonest behavior is not affected by the level of stakes.[6] Only one of the 10 die rolls is randomly drawn for payment. To eschew that participants become too wary about the die rolling game they are told that they can receive an extra payoff for completing a questionnaire before reading the instructions of the die rolling game.
The experiment was conducted at the University of Goettingen and programmed using z-Tree (Fischbacher, 2007).[7] Subjects from various fields were recruited with ORSEE (Greiner, 2004). We ran 14 sessions with 268 subjects (129 male and 139 female subjects). Subjects' average payment in the main experiment was €12.65 (they earned €1.54 in the SVO-elicitation task and €0.63 in the die rolling game). Participants were told that they would earn at least €5 but that there would not be an additional show-up fee.

4.5 Results

In this section we present our results on the relation of subjects' SVO and honesty. In a next step, we will focus on gender differences in honesty and SVO. We close the results section with a mediation analysis. We always report two-sided $p - values$ when applying non-parametric tests.

4.5.1 The influence of SVO on dishonest behavior

In the analysis, we first focus on the general influence of subjects' SVO on dishonest behavior. Figure 1 illustrates the distribution of reported profit levels (left panel). Profit levels range from 0 to 5. The lowest level (€0) is paid when subjects report a 6 whereas profit levels 1–5 increase for a die roll of 1 (€0.2) to 5 (€1). The correlation between the mean reported profit levels and SVO angles is illustrated in the right panel. The reported mean is calculated at an individual level based on the 10 observations of die rolls we obtained per participant.

If we assume fair dice and honest behavior, the reported profit levels should follow a uniform distribution. The predicted average profit level reported, i.e., the average of

[6] Moreover, there is evidence that low-stake sizes do not impact the replication of standard results in ultimatum, dictator, trust, public-good games (Amir et al., 2012, Kocher et al., 2008), and social preferences (Müller and Rau, 2016).

[7] The z-Tree code, the resulting data, and the analysis scripts can be permanently downloaded at https://www.uni-goettingen.de/de/485779.html

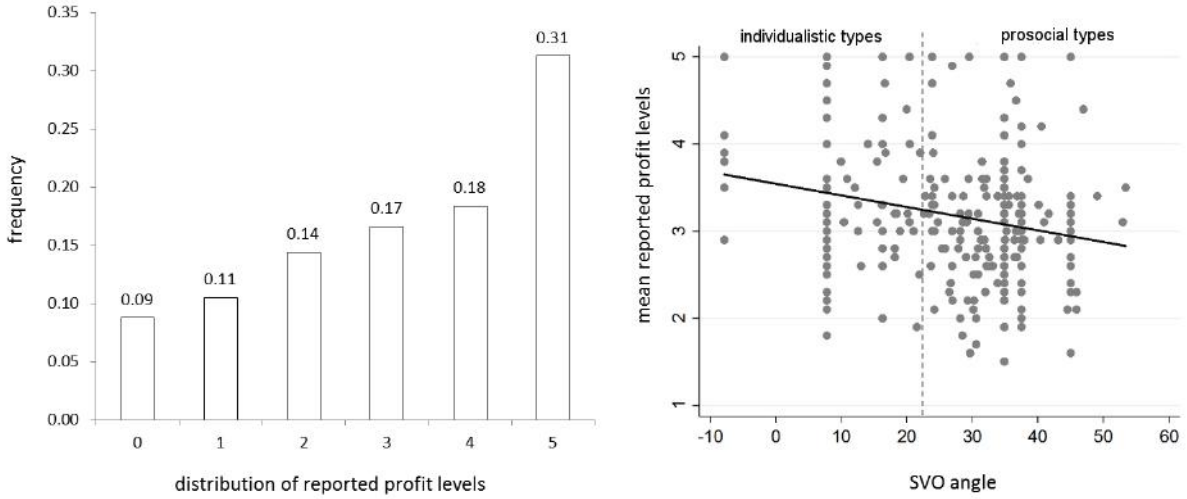


FIGURE 4.1: Reported profit levels (left panel) and the influence of SVO (right panel).

the 10 reported outcomes, should be 2.5. On average, however, subjects report a profit level of 3.19.[8] The mean of 3.19 is clearly higher than the predicted mean of 2.5. Moreover, the left panel of Figure 4.1 shows that the observed distribution is not uniform (one sample Kolmogorov-Smirnov test, $p < 0.001$). Hence, we find evidence of dishonest behavior.

We now focus on the influence of SVO on dishonest behavior. The scatter plot (right panel) illustrates a highly significant negative correlation between SVO angle and reported profit levels (Spearman's rank correlation coefficient, $\rho = -0.198,\ p = 0.001$).[9] This result emphasizes that subjects' SVO affects their dishonest behavior in the die rolling game. That is, individualistic subjects characterized by a low angular degree of SVO report higher profit levels, whereas prosocial subjects report relatively lower profit levels. We therefore find support for Hypothesis 1.

Result 1

SVO significantly affects dishonest behavior: with an increasing SVO angle subjects report lower profit levels.

In the next section, we focus on gender differences in SVO and dishonesty to test Hypothesis 2.

[8]The result is similar to previous findings of Conrads et al. (2013) who played a die rolling game with higher stakes. In their paper they find that subjects report an average profit level of 3.31 in the individual treatment. Thus, our setting replicates common data on die rolling games although the stake size is lower.

[9]The result is also supported by a significant Pearson's correlation coefficient ($\rho = -0.229,\ p < 0.001$).

4.5.2 Gender differences in dishonest behavior and SVO

In this section we focus on gender differences in dishonest behavior and SVO. Figure 2 depicts the frequency of reported profit levels (left panel) and compares the CDFs of the SVO angles (right panel). Both illustrations are conditioned on gender.

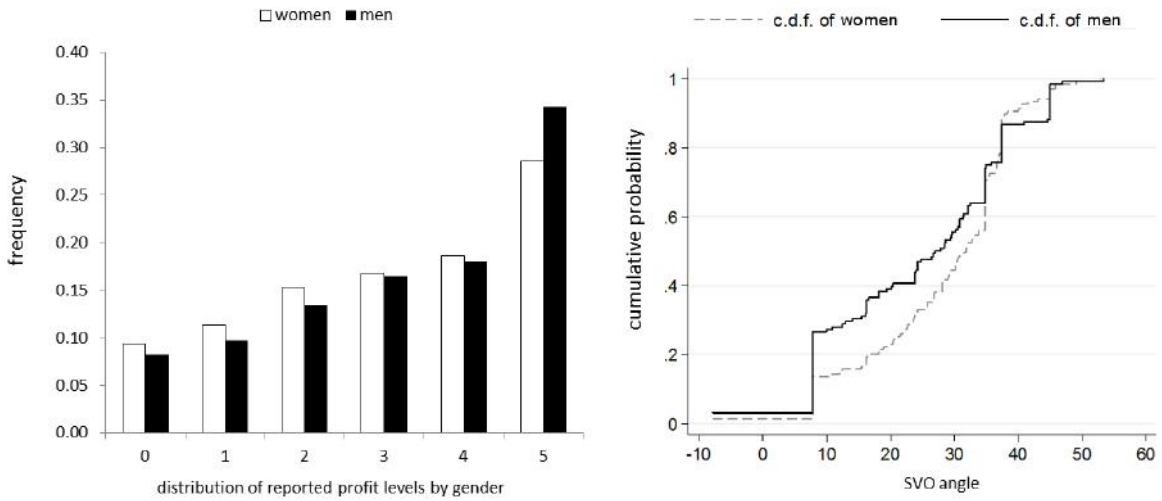


FIGURE 4.2: Reported profit levels conditioned on gender (left panel) and CDFs on SVO angles of women and men (right panel).

The bar chart demonstrates that the distribution of reported payoffs is right-shifted for both gender. More precisely, one sample Kolmogorov-Smirnov tests reject the hypothesis that the distributions are uniform (both gender: $p < 0.001$). Thus, both gender apparently behave dishonestly. However, men report a significantly higher average profit level (3.29) than women (3.10) (Mann-Whitney test, $p = 0.035$). Men's distribution is clearly more right-shifted and significantly differs from women's distribution. This is confirmed by a two sample Kolmogorov-Smirnov test on average reported profit levels ($p = 0.047$). Hence, we support the gender differences predominantly found in the literature (Abeler et al., 2016, Capraro, 2017).

At the same time, the cumulative distribution function (right panel) shows that the distribution of SVO angles significantly differs between women and men (two sample Kolmogorov-Smirnov test, $p = 0.025$). More precisely, we find that women have a higher mean SVO angle (28.10) than men (24.63) (Mann-Whitney test, $p = 0.065$). Put together, the gender effects in mean reported profit levels and the significant difference between women's and men's distributions in SVO support Hypothesis 2.

Result 2

(a) Male subjects behave more dishonestly than female subjects.

(b) The distributions of women's and men's SVO angles are significantly different.

So far, the data have shown that a significant negative correlation exists between SVO angles and reported profit levels. At the same time, we find gender differences in dishonest behavior and subjects' SVO angles. It will be interesting to analyze whether gender differences in dishonest behavior are mediated by gender differences in SVO. Before we conduct a mediation analysis in the next section, we summarize our findings in Table 4.1.

	women (n = 139)	men (n = 128)	aggregate data (n = 267)
mean reported profit level	3.10 (0.71)	3.29 (0.89)	3.19 (0.77)
SVO angle	28.10 (11.79)	24.63 (14.35)	26.44 (13.17)
risk tolerance	5.14 (1.92)	5.60 (2.16)	5.36 (2.04)
age	24.40 (4.89)	25.11(4.16)	24.74 (4.56)
econ student (in %)	0.41 (0.49)	0.47 (0.50)	0.44 (0.50)

TABLE 4.1: Summary statistics. Standard deviations in parentheses.

The table also reports subjects' sociodemographics. In this respect, *risk tolerance* corresponds to the mean of subjects' self-reported risk tolerance.[10] *Age* represents subjects' average age in years, and *econ student* reports the share of subjects studying economics or business economics. Overall, the average data suggests that women are less risk tolerant than men (Mann-Whitney test, $p = 0.057$) which confirms common gender differences in risk-taking (Charness and Gneezy, 2012). Female and male participants are almost the same age. It can be seen that 44% of the sample are econ students.

4.5.3 Mediation analysis on the influence of SVO

In this section, we test whether the gender effect is mediated by different levels of SVO between men and women. For this purpose, we make use of a mediation analysis by using Structural Equation Modeling (SEM).[11] We used the command "sem" with the STATA 13 software package. First, we only include the gender dummy and the SVO angle. Second, we run an SEM where we take into account factors which have been found to influence dishonesty. In addition to the gender dummy we include subjects' *risk tolerance* and subjects' *age*. We also include a dummy called *econ student* which is positive for students studying economics or business economics. Figure 4.3 presents the results of the SEM in a path model. Panel A is computed with an OLS regression. The panel illustrates the effect of female subjects compared to male subjects on mean reported profit levels. Panel B is computed with the SEM and focuses on the direct and

[10] We elicited risk in a questionnaire before the main experiment by asking: "Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?" Participants could answer on a scale from 1 (risk-averse) to 10 (risk-seeking) (Dohmen et al., 2012).

[11] We thank the editor, Susann Fiedler, for raising this point.

indirect effects of female subjects' on mean reported profit levels. See Table C.1 in the appendix for a detailed illustration of regression results.

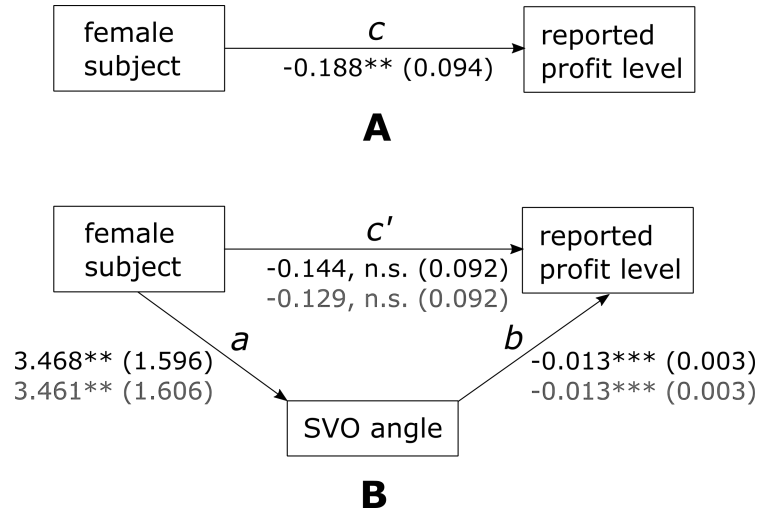


FIGURE 4.3: Schematic diagram of mediation analysis results.
Reported path values are unstandardized regression coefficients with standard errors in parentheses. Data in grey color represent results when including covariates (age, risk tolerance, and econ student) in the model. Significance levels are: **: $p < 0.05$; ***: $p < 0.01$; n.s.: non-significant.

It turns out that the coefficient of *female subject* is negative and significant when ignoring a potential mediator (Panel A). That is, women, on average, report profits which are 0.19 profit levels lower than the reported profit levels of men. If we include the possible mediator (*SVO angle*) and the control variables in the path model, we find that this effect becomes smaller and non-significant (Panel B).[12] None of the control variables are significant. We find that the proportion of variance in *reported profit level* is 0.548 and the standard error is 0.047. It can be seen that *female subjects* have a significant positive effect on the level of *SVO angle*. More precisely, on average, women have a 3.5 degree higher *SVO angle* than men. At the same time, it turns out that the *SVO angle* negatively affects the mean reported profit level. The influence is highly significant and it can be seen that each (positive) angular degree of the SVO scale reduces the reported profit level by 0.013. That is, only the subjects' SVO has a significant influence on dishonest behavior in the form of mean reported profit levels. A conspicuous finding is that the indirect path from *female subject* through *SVO angle* to *reported profit level* is significant. In this case, we find that the coefficient of *female subject* is negative (-0.045) and significant at the 10-percent level ($p = 0.062$). Hence, we find that the gender effect on dishonest behavior is mediated by the subjects' SVO angle.

[12]We observe similar results if we exclude the control variables.

Result 3
(a) Subjects' SVO is a significant predictor of mean reported profit levels.
(b) Subjects' SVO mediates the gender difference in dishonest behavior.

4.6 Discussion and conclusion

In our simple experiment we analyzed how individual SVO relates to honest behavior in a situation where dishonesty pays off but comes at a cost for somebody else (selfish black lie). First, we demonstrated that people with relatively high SVO angles, which reflect higher concern about somebody else's payoff, are more honest than people with a relatively low SVO angle. Second, we confirmed a predominant gender finding in honest behavior which is that women are more honest than men (Abeler et al., 2016, Capraro, 2017). At the same time, on average, women have higher SVO angles than men, i.e., they care more about another person's payoff than men do. In a mediation analysis, we showed that individual SVO angles explain the gender effect.
Our results make two contributions to the existing literature. We can isolate the effect of individual social preferences on honest behavior for selfish black lies. Moreover, we can link the commonly found gender difference in honesty (Abeler et al., 2016, Capraro, 2017) to gender differences in social preferences. In the following, we will embed these findings into the existing literature and highlight the novelty of our results. We will also discuss limitations and suggest extensions of our study for future research.
Only few studies investigate individual-level motives such as social preferences for being honest. Our study especially relates to the papers from Cappelen et al. (2013) and Biziou-van Pol et al. (2015). People have been shown to behave more honestly with increasing altruism in a sender-receiver game where strategic considerations might play a role (Cappelen et al., 2013). Biziou-van Pol et al. (2015) find that engaging in altruistic white lies leads to higher cooperation and higher altruism levels than being honest in the same situation. This could imply that people with high pronounced social preferences have lower psychological costs for being dishonest. However, Biziou-van Pol et al. (2015) find reversed effects from being dishonest in a Pareto-white-lie situation. The interpretation of these opposing results is not straightforward. It could be that people with rather selfish attitudes behave dishonestly in Pareto-white-lie-situations where they can increase their own monetary payoff but not in an altruistic-white-lie situation where dishonesty benefits only another person. In contrast to the existing literature, we use the SVO angle rather than dictator or prisoner's dilemma games as an instrument to elicit distributional preferences. Furthermore, we reverse the order and measure general preferences first and then honest behavior. Moreover, we focus on

selfish-black-lie situations and not on Pareto or altruistic white lies.
This paper also contributes to the explanation of gender differences in honesty by considering differences in the social preferences of men and women. Looking at the regression analysis it seems at first glance that the gender difference is fully mediated by individual SVO angles. We acknowledge that statistical power issues might account for completely non-significant gender effects when controlling for SVO. Furthermore, the translation of social preferences into honest behavior might be context-dependent. Across different contexts women and men might be affected differently (Gilligan, 1982) and might face different levels of the psychological costs of being dishonest. For example, our study is characterized by a high degree of anonymity since data was generated in an experimental laboratory. This may affect the magnitude of gender differences in SVO and honest behavior. In an anonymous set-up the salience of gender roles is attenuated. In a setting where gender roles are made salient women might conform to normative gender expectancies because they fear being sanctioned and disliked otherwise (Heilman and Okimoto, 2007, Rudman and Phelan, 2008). In regard to altruism, Rand et al. (2016) demonstrate in a meta-analysis of lab experiments that women behave more altruistically after they have been made aware of their gender role, whereas men do not change in altruism level. The awareness of the gender role can affect behavior, for instance, in negotiations at the workplace. Female negotiators are punished when claiming high shares as this violates gender roles (Bowles et al., 2007). When women anticipate this, it is possible that they "don't ask" for too much in negotiations (Babcock and Laschever, 2009). If we consider these insights when interpreting our anonymous experimental findings, it is likely that the lab setting underestimates the gender difference in SVO. However, it is not quite clear in which way more pronounced social preferences would affect the level of honest behavior. This might be highly context-dependent. For example, in settings with a strong probability of getting caught red-handed being dishonest, individual risk preferences might overwrite social preferences and might be more predictive for dis/honest behavior. Another contextual variable that could influence behavior is the absolute amount of money at stake. Moreover, people might face varying psychological costs to violate the norm honesty depending on the strength of the norm in society (Ariely et al., 2015, Hugh-Jones, 2016) and the level of the externality inflicted on somebody else's payoff.
We conclude that social preferences correlate with honest behavior in a situation where being honest inheres little opportunity costs. Although differences in SVO can explain differences in honest behavior in our setting, it might be less informative in situations with, for instance, higher levels of risk involved. It is especially relevant to learn more

about individual preferences which may correlate with dishonest behavior. In this regard, experimental economics is a powerful discipline as one of its evident strengths is the elicitation of subjects' preferences and the consequences of their choices. Several studies successfully applied simple experiments such as the die rolling game, to predict dishonest behavior in the field, such as corrupt behavior (Hanna and Wang, 2013), school misconduct (Cohn and Maréchal, 2015), refrain from reporting overpayments (Potters and Stoop, 2016) and the free riding of public transport (Dai et al., 2017). Therefore, it might be promising to further scrutinize the interplay of individual (social) preferences, gender, and payoff consequences in future experimental lab studies.

Chapter 5

Contract compliance under biased expectations

This chapter is joint work with Sabine Fischer. We were equally involved at all stages of the project: the development of the idea, literature review, programming of the study, field work in Ghana, the analysis of data and writing up the paper.

5.1 Introduction

Encouraging contract compliance can be challenging especially when legal enforcement mechanisms do not function properly (Chemin, 2012). Particularly, in developing countries legal systems are often absent, corrupt or too slow to be usable (Dixit, 2003). Therefore, contract conclusion is a risky endeavor in terms of post-contractual opportunistic behavior although contracts hold potentially high returns on investment (Klein et al., 1978). The uncertainty may shy away potential investors from building business relationships. Consequently, countries do not achieve their full potential since contract repudiation is one major problem why poor countries fail to catch up (Keefer and Knack, 1997). Positively expressed, contracts can be an important tool to stimulate growth (Reardon et al., 2009). North (1990) asserts "the inability of societies to develop effective, low-cost enforcement of contracts is the most important source of both historical stagnation and contemporary underdevelopment". Therefore, it is important to better understand the drivers of contract compliance in contexts where enforcement mechanisms do not function properly.

In behavioral economics, there is extensive evidence that people's judgment under uncertainty is often misguided, i.e., people often do a poor job in processing probabilities (Tversky and Kahneman, 1975), and people fail in assessing their performance and are overconfident about their performance (e.g. Moore and Healy, 2008). Moore and Healy (2008) provide an excellent survey on recent work on overconfidence and introduce the following three categories: relative overconfidence (thinking that you are better than others), overestimation (thinking that you are better than you actually are), and overprecision (thinking that your beliefs are more accurate than they actually are). Our paper's focus is on overestimation (absolute overconfidence).

Crucial consequences might occur for compliant behavior if contract conditions result from misjudged performance since overconfident agents may form unrealistically high payoff expectations. Payoff expectations can influence economic decision-making. Camerer et al. (1997) found evidence that New York taxi drivers only provide services until they have reached their payoff expectations for that day. Even if it is a rainy day and circumstances promise higher benefits, taxi drivers will stop working once they reached their payoff expectations. Here, it has been show that payoff expectations can serve as a reference point. That is, people strive to earn the amount of money that they expected to earn.

In this study, our main hypothesis is that individual overestimation of performance fosters contract breach. The channel that we want to test is reference-dependent preferences. More precisely, payoff expectations resulting from performance misjudgment may serve as a reference point. This means, after materialization of the contract, agents

compare the status quo (real payoff from the contract) with their expected payoff. If agents overestimate their performance, the expected payoff exceeds the real payoff. Similar to Camerer et al. (1997), agents may strive to meet their expectations. Breaching the contract allows agents to eradicate the divergence between payoff expectations and the status quo. Hence, we expect that the more agents overestimate their performance/output, the more they engage in breaching actions.

Furthermore, we want to examine possible moderators that may bolster reference-dependent behavior. It has been shown that people may evaluate payoffs as gains and losses rather than as a detached state of wealth (Kahneman and Tversky, 1979). In the moment of realizing that expectations do not materialize, agents might sense a loss. Along the lines of the model on reference-dependent preferences from Koeszegi and Rabin (2006), sensed losses may resonate more intensely the more pronounced individual loss aversion is. To counteract the relatively high utility loss when overconfident and highly loss averse, agents might engage in contract breaching to a higher extent. Another channel that we want to test are the influence of potential production shocks. Production shocks are prevalent in many businesses. For example, in the farming context, weather shocks (positive or negative) can occur and affect production outcomes. In such an environment, agents are able to attribute outcome/performance to external factors according to their ex-ante beliefs (Grossman and Owens, 2012). Own accountability from failing to meet the performance goal can be (partly) shifted to the occurrence of a shock. This way, agents can utilize the shock to legitimate contract breach while keeping up their self-image. Therefore, breaching might be facilitated if the environment is non-deterministic, i.e., production shocks can occur, compared to a deterministic environment.

We investigate our hypotheses with a lab-in-the-field experiment with students in Ghana. Since our research questions are particularly interesting for developing economies, we consider our subject pool from Ghana as a major strength of this study.[1] We use a multi-stage investment game where an agent and a principal conclude a contract with an inherent hold-up problem. We designed the experiment in a way that targets the investigation if payoff expectations are used as reference points in contract situations. Here, our focus is on the formation of payoff expectations, the alleged reference point. Payoff expectations vary by individual misjudgment of performance/output. In the final stage of the experiment, agents can decide to comply or not to comply. Payoff expectations are made salient at this stage as we want to provoke that agents compare

[1]The majority of laboratory experiments in economics and psychology are conducted with participants from WEIRD (Western, educated, industrialized, rich and democratic societies. Experimental findings indicate considerable behavioral differences among societies in diverse domains such as analytic reasoning, fairness or cooperation (Henrich et al., 2010, Jones, 2010).

the expected payoff with the status quo. The design is described in detail in Section 3. We establish the following results: With increasing overestimation sellers breach contracts to a higher extent. We can also confirm that individual loss aversion moderates the effect of sensed losses on breaching contracts. Interestingly, these results only hold within a non-deterministic environment. In order to identify whether the effect from overestimation is causal, we manipulate expectations by an experimental treatment, where performance/output guesses are raised by an anchor (Tversky and Kahneman, 1975). We find that the effect on contract breach remains significant once overestimation is instrumented by this treatment implying that the effect is causal.[2] Apart from our main experimental results, we find further convincing evidence for reference-dependent behavior. Data analysis from an ex-post questionnaire reveals that agents with similar final payoffs are less satisfied if payoff expectations were not met initially. Moreover, overconfident agents state more often that they felt disappointed when informed about their real payoff compared to non-overconfident agents.

5.2 Related literature

Our study builds on the idea that people have reference-dependent preferences. Previous studies have predominantly focused on ex-ante behavioral change due to reference points. For instant, it has been shown that costumers adjust their consumption behavior (Huang and Liu, 2017) and gambler their lottery choices (Kahneman and Tversky, 1979) to avoid anticipated losses. Abeler et al. (2011) demonstrate experimentally the impact of reference points on effort provision. By exogenously varying expectations of earnings, workers adjust effort levels to avoid anticipated losses. An empirical study from Camerer et al. (1997) find that cab drivers in New York set a daily income target and use it as a reference point. Once this target is reached, they stop working and spurn the opportunity of potentially high-income days.
In the contract literature, there are few studies that focus on reference points and contract compliance. Hart and Moore (2008) demonstrated theoretically and Fehr et al. (2011) showed experimentally that agents breach less under a rigid contract, i.e., a contract where the price for the agent's output is fixed ex ante, than under a flexible contract, i.e., the price is not fixed ex ante but can vary within a certain range. Under flexible contracts, agents engage in costly punishment if the principal pays a lower price than expected. These studies demonstrate that people alter their behavior and costly punish when their reference payoff is not realized.

[2] This approach is comparable to (Hill et al., 2012), who examine if observing peers alters the decision to reciprocate in a trust game. By using a treatment, they instrument their explanatory variable of interest and justify therewith that their reported effect is indeed causal.

The empirical evidence on reference points is hard to reconcile with standard economic assumptions. However, the findings are consistent with the idea that people hold reference-dependent preferences. Here, utility is not only dependent on a factual payoff per se but also on a reference payoff to which the factual payoff is compared to (e.g. Bell, 1985, Loomes and Sugden, 1986, Gul, 1991, Hart and Moore, 2008, Koeszegi and Rabin, 2006). That means, an individual derives less utility in a situation where the real payoff falls short of expectations compared to a situation in which payoff expectations are met. Generally, there are few studies testing reference-dependent behavior experimentally or empirically since it is a challenging endeavor to clearly identify reference-dependent preferences.

Beside the literature on reference-dependent preferences, we draw and contribute to the literature on performance misjudgment and especially overestimation/overconfidence. There are several reasons why people fail to correctly assess their own performance under uncertainty (e.g. Moore and Healy, 2008). A large stream of literature focuses on cognitive mechanisms such as the use of judgmental heuristics and faulty integration of relevant information in the assessment process (e.g. Tversky and Kahneman, 1975, Fischhoff et al., 1978). Another reason might be that people are incompetent in a task and too incompetent to even realize their incompetency (Kruger and Dunning, 1999, Ehrlinger et al., 2016). Such miscalibration of own abilities can affect economic outcomes. The majority of literature examines effects of overconfidence for financial markets and managerial decision making (e.g. Barber and Odean, 2001, Scheinkman and Xiong, 2003). It has been shown that overconfidence can lead to deadweight losses in markets. Since overconfident agents are prone to overestimate future market shares, they excessively enter these markets (Camerer and Lovallo, 1999). Overconfidence can also affect contract outcomes. Overconfident job-seekers may underestimate risks involved in certain contracts, misjudge effort that has to be exerted, and the wage that will be gained. This way, contract and incentive designs are adjusted on the employer's side, and sorting decisions and effort provision are affected on the employee's side by overconfidence (De la Rosa, 2011, Sautmann et al., 2011, Larkin and Leider, 2012, Santos-Pinto, 2012, Hoffman and Burks, 2017).

We can contribute to the above literature on overconfidence and reference-dependent behavior. We test how overconfidence affects compliant behavior in contract situations. The mechanism we examine is via payoff expectations that result from performance overestimation and may be used as a reference point. Reference points in our set-up emerge endogenously as a result of (mis-)judgment of own performance/output, which is the major novelty of our study.

5.3 Study design

5.3.1 Experimental design and survey

A study session comprises a short pre-survey, an experiment with two parts and an ex-post questionnaire.[3] In the experiment's first part, we elicit (social) preferences, such as social value orientation, risk preferences, inequality aversion and loss aversion, which will be used to check if our main variable of interest (overestimation) is independent of individual characteristics. Moreover, we will use the measure on loss aversion to test Hypothesis II. Afterwards, subjects exert a "trial phase" of a real-effort task, where all participants can practice the task. Details about the experiment's first part can be found in D.2 in the Appendix.

In part two, participants engage in an investment game in either the role of a seller, that is a person who produces goods, or a buyer, that is a person who invests in a seller's production to enhance the goods' value. Sellers exert the real-effort task (produce goods) again and assess their performance afterwards. Based on their guess, sellers make an offer about a certain number of goods to sell to the buyer. If the buyer accepts the contract, a contract is concluded. At this time, sellers are informed about their expected payoff from the contract – the alleged reference point. Next, sellers, who concluded a contract, are informed about their real payoff if they honored the contract (status quo). Here, status quo and expected payoff are juxtaposed to push sellers to compare these two payoffs. Sellers can then decide to comply, to breach or even to "over-fulfill" the contract. This way, sellers can offset the divergence between status quo and expected payoff.

Complete instructions can be found in Appendix D. The experiment was programmed on tablets with the software "otree" (Chen et al., 2016). Sessions lasted approximately 120 minutes and students were paid in total about GHS 44 ($11), including a show-up fee of GHS 20. Payments varied between GHS 22 and GHS 90. In the following paragraph, we will elaborate on the experiment's main part.

Experiment's main part (investment game)

At the beginning, subjects are matched in dyads and are randomly assigned either to the role of a seller or to the role of a buyer. In the following, we describe the different steps in the contract phase. The steps 2 to 5 are illustrated in Figure 5.1.

1. Real-effort task and performance guess: Sellers are confronted with 25 puzzles with

[3] As well as that, we played a modified version of the die rolling game from Fischbacher and Föllmi-Heusi (2013). Since we played it at the very end of the experimental session, it cannot have influences any of the experimental data. For brevity, we will therefore not explain the game's details.

four possible answers each. We explain to subjects that correctly solved tasks correspond to produced goods. Each correctly solved puzzle is worth GHS 0.20. We decided for a rather difficult task, as it has been demonstrated that such difficult tasks generate higher overestimation (Larrick et al., 2007). Time was limited to 15 seconds per task and the first answer was always logged in as a default.[4] Afterwards, sellers are asked to estimate their performance and earn an additional GHS 5 if the guess turns out to be exactly correct.

2. Contract offer: Sellers can decide whether they want to offer the buyer a contract and if so how *many* goods they want to offer. The maximum of goods that can be offered is limited to their performance guess. We inform participants about the values of goods: Sellers earn GHS 1 for each good sellers sell to the buyer. Goods can also be sold to an alternative market for a unit price of GHS 2.5. However, only if the buyer accepts the seller's offer, these prices come into effect. Buyers receive an endowment of GHS 4, which they can decide to invest in the seller's production to enhance the value of produce. If buyers decide not to invest, the value of the seller's production cannot be "enhanced" and goods are only worth GHS 0.20 at the alternative market. If the seller decides not to offer any contract, buyers keep their endowment of GHS 4 and the seller earns GHS 0.20 per good. We provide sellers with a calculation table to facilitate payoff calculations and to ensure that sellers make reasonable decisions.

In real life for example, contract farming can manifest such a price structure. Since the buyer invests in the seller's production, buyers may pay lower prices to redeem the investment compared to prices offered at the spot market. Therefore, farmers can be tempted to sell to the alternative market. Other than that, farmers might be present-biased and value buyer's money they have to wait for less than instantly obtainable money.

3. Contract acceptance: Buyers are informed about sellers' performance in the real-effort task in the "trial phase". They can decide to accept or to reject the seller's offer. If the buyer accepts the contract, GHS 4 are invested in the seller's production. The buyer receives GHS 1 for each good the seller sells to the buyer (y_{sold}). Payoffs for sellers are as described in step 2. If the buyer rejects the offer, the buyer keeps the endowment. In this case, buyer and seller are informed about their payoffs and the game ends.

The buyer's payoff (π_B) is as follows:

[4] We decided for a default answer as there might be strategically acting participants not working at all on the task, who are then perfectly able to assess their performance otherwise.

$$\pi_B(y_{sold}) = \begin{cases} GHS\,1 * y_{sold} - GHS\,4, & \text{if contract is concluded} \\ GHS\,4, & \text{otherwise} \end{cases}$$

4. Information about total output and occurrence of a production shock: After contract conclusion, sellers and buyers are informed about the total output produced. The total output equals the real performance in the real-effort task if no shock affected "the production". A shock affects the production with a 50% chance and output is either topped up by 2 goods or reduced by 2 goods (each case is equally likely). The two parties learn if a shock has occurred or not and the resulting total output is displayed. In the shock condition, subjects are not informed about the real performance but only about the resulting total output.

5. Selling goods: To remind parties on the agreement made, the contract is summarized and displayed. Then, sellers can decide on the amount of goods they ultimately want to sell to the buyer (y_{sold}). Here, the seller is provided (again) with a calculation table while this time the payoff expectation and the real payoff if the contract will be honored is saliently displayed at the top of the screen. This way, we make sellers to pay attention to their payoff expectations and stress the difference to the status quo. The seller then decides how many goods of their total output (y_{real}) to sell to the buyer and how many goods to sell to the alternative market. The seller's payoff (π_S) can be described as:

$$\pi_S(y_{real}, y_{sold}) = \begin{cases} GHS\,2.5 * (y_{real} - y_{sold}) + GHS\,1 * y_{sold}, & \text{if contract is concluded} \\ GHS\,0.20 * y_{real}, & \text{otherwise} \end{cases}$$

In our set-up, sellers break the contract by selling less goods to the buyer than offered. Breaching the contract and selling more goods to the alternative market is always monetary beneficial for the seller. Similar moral-hazard situations exist in real-life. For example, in a contract-farming situation: Once a principal has invested in the production, farmers reduce their effort exerted in the production or only sell low quality produce. In the experiment, this step is the crucial step where our outcome variable (*breach (absolute)*) is generated. We observe which sellers comply to the contract (sell the amount offered to the buyer), breach the contract (sell fewer goods than offered) or even over-fulfill the contract (sell more goods than offered).

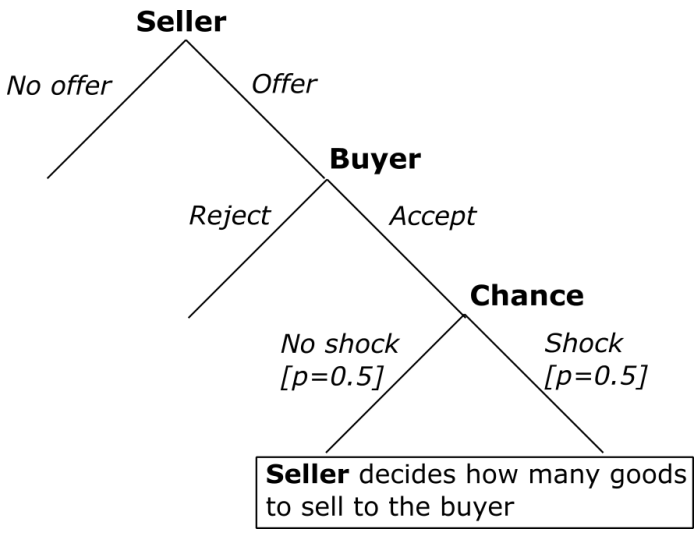


FIGURE 5.1: Sequence of actions in the modified investment game

General procedures and ex-post survey

Before starting a session, we ensured to have an even number of participants in the session to guarantee one-to-one interactions in the investment game. Subjects are fully informed about the entire course of action before they took actual decisions. We explained the game twice, one time with text on time mainly with pictures, and used control questions to make sure that participants understood the sequence of actions.
The ex-post questionnaire started off with questions about the experiment such as payoff satisfaction and seller's feelings when learning about the real payoff from the contract. This survey-data we will use to scrutinize the reference-point hypothesis. We also collected data on experience in experiments and on how many other persons they knew in the room. At the very end, we asked about socio-demographic background such as education and income.

Manipulation of overestimation

We use an experimental treatment to exogenously manipulate performance guesses to increase payoff expectations. For that purpose, we use a simple yes-no question serving as an anchor which is "Do you think you have produced more than 20 goods?". Recall that sellers worked on 25 puzzles in total. Less than 1% of participants actually reached a total performance of over 20 goods. Therefore, the anchor set is extremely high and effectively not reachable by the majority of participants. However, participants use this initial number of 20 to make their subsequent performance guess and

start iterating downwards until they reach their final guess (Tversky and Kahneman, 1975). The number of 20 goods might sound arbitrary but we chose the number for at least two reasons. First, we wanted to implement a substantially high anchor to raise performance guesses and second we wanted to avoid that participants contemplate about the question.

Discussion of design decisions

Common in real life is that an investor makes an offer to a producer. Then, the seller can agree and starts producing. In our set-up, sellers (and not buyers) offer the contract. With this procedure, we want to assure a certain level of agents' commitment and counteract potential non-compliance with contracts because agents might perceive the contract as unfair.[5] It would have been a major challenge to capture fairness perceptions within the investment game, if it would not have been impossible. Therefore, we decided to turn the more intuitive order of offer and acceptance of buyer and seller around and let the seller make the offer. For the same reason we allowed for self-selection into contracts. That is, sellers could decide to offer a contract and buyers could decide to accept the contract. Not allowing for voluntary agreements and assigning predefined contracts could have leaded to a situation in which sellers breach primarily because they do not feel committed to the contract and perceive the contract as unfair. Allowing subjects to choose their preferred contract (if any) is common practice when studying behavior after contract conclusion (e.g. Fehr et al., 2011, Bartling and Schmidt, 2015).

5.3.2 Recruitment and sample

The sample consists of students recruited at the two largest cities in Ghana – the capital Accra and the university town Kumasi. Our study was publicly announced without any reference to the content of the study and students could register in advance for a particular session. To attract participants, we announced a show-up fee of GHS 20 and used non-monetary incentives such as certificates for participation and a free drink for each participant.
Overall, the majority of participants are male (about 70 % men and 30 % women). On

[5]Let us imagine that the buyer makes the offer. In this case, sellers might accept the offer mainly because the buyer's investment enhances the value of sellers' goods and holds substantial benefits in any case. However, the seller might perceive the buyer's offer as unfair and because of that behaves non-compliantly. In such a set-up, the perceived unfairness effect may superimpose an effect from unmet payoff expectations.

average, participants are 21 years old and state to have GHS 200 a month for free disposal (corresponds to about US$ 40). Most of the students participated in a lab experiment for the first time.
The data was collected in November and December 2016. In total, we conducted 52 sessions over the course of five weeks and at the aforementioned two locations. The sessions comprised between 16 to 24 participants each. We implemented four different treatments. Treatments and shock conditions were randomized within sessions. We analyze data only from two treatments in this paper – from the control treatment explained in Section 5.3.1 and the treatment with the anchor.[6] In these two treatments 478 participants took part, that is 239 sellers and 239 buyers. We received an ethical approval for this study by the ethical board of Social Sciences of the University of Goettingen.

5.3.3 Hypotheses

In this section, we will derive our main hypotheses on reference points and contract compliant behavior. Before that, we will briefly discuss expected compliant behavior under the assumption that sellers are solely profit-maximizers and under the assumption that sellers have other-regarding preferences.
According to standard economic theory, profit-maximizing sellers will breach the contract (once accepted by the buyer) to the full extent. In other words, profit-maximizers sell their total output to the alternative market independent of any promises made to the buyer ex ante. However, behavioral economics has shown that people are concerned with other-regarding preferences and do reward pro-social actions by reciprocating (Berg et al., 1995, Fehr et al., 1998, 2002)[7], which has also been demonstrated for agents in contract situations (Ellingsen and Johannesson, 2004, Fehr et al., 2011, Hackett, 1994). Based upon the discussion above, we will expect that a substantial share of agents will comply to contracts.

In this study, we are primarily interested if agents orientate on their payoff expectations

[6] In addition to the anchor and control Treatment we implemented two other treatments in which we gave performance feedback; an exact and a noisy feedback. As we use a between-subjects design and randomized treatments within sessions, we do not have any concerns to analyze data from different treatments separately.

[7] In trust games (and our set-up is comparable to a trust game) people violate the tenets of neoclassical economic analysis pervasively. The first mover in the trust game trusts responders at substantial rates, even if a favorable response is very uncertain and identities are not known to the responder or experimenter. Similar to the sellers are the responders in trust games: They often turn out to be trustworthy, returning money to the first mover when they have no compelling economic reason to do so (Berg et al., 1995, Croson and Buchan, 1999, Johnson and Mislin, 2011). Besides that, Vanberg (2008) has demonstrated that people have a preference to hold promises, which is comparable to commitments to contracts, in a modified trust game.

and are even willing to breach a contract to reach initial expectations. Camerer et al. (1997) have shown that people work until they have reached their payoff expectations. Mapping that result into utility theory, peopl maximize their utility by working until they meet their payoff expectations. They derive less utility when they stop working before they reached their initial goal. Similarly, evidence from psychology suggests that the marginal utility increase below a target is strictly higher than the marginal utility increase above a target (Heath et al., 1999). In contrast to the taxi-driver situation from Camerer et al. (1997), balancing off the utility loss cannot be resolved by exerting more effort in our contract setting but only by breaching the contract. When sellers learn about their real payoff from the contract (status quo), they experience an utility loss ($-\Delta U$) depending on the divergence between expected payoff and status quo:

$$-\Delta U = [Q * 1 + (Y_{s,exp} - Q) * 2.5] - [Q * 1 + (Y_{s,real} - Q) * 2.5],$$

where Q is the number of goods offered to the buyer, $Y_{s,exp}$ the estimated performance/output and $Y_{s,real}$ the real performance/output. The first square bracket corresponds to seller's expected payoff whereas the second square bracket corresponds to the status quo. Simplification of the previous equation results in $-\Delta U = Y_{s,exp} - Y_{s,real}$. The larger seller's overestimation ($Y_{s,exp} - Y_{s,real}$), the larger will be the utility loss ($-\Delta U$).[8] The utility loss can be counteracted by selling less goods to the buyer than offered since the alternative market holds prices that are 2.5 times higher than when selling goods to the buyer. Based on this argumentation we derive our first hypothesis:

Hypothesis I: *With increasing overestimation sellers will breach contracts to a higher extent.*

In our contract setting, there is uncertainty about the future status quo when concluding the contract. This is a major difference to the study from Camerer et al. (1997) where uncertainty does play an inferior role. That said, sellers might not only try to reach payoff expectations in contract situations. Sellers who overestimate sense a loss since they compare expected payoff with the status quo. Koeszegi and Rabin (2006) explain in their model of reference-dependent preferences that losses loom larger with an increasing aversion to losses, i.e., with increasing loss aversion (λ). Transferring that to our setting, when sellers are informed about the status quo they experience an absolute loss which is their individual overestimation ($Y_{s,exp} - (Y_{s,real}$). This loss is sensed to a larger extent with increasing aversion towards losses (λ). We formalize that in the following equation:

[8] The utility equation is obviously very simplified and only focuses on the main mechanism we want to test. We are aware that we ignore other important determinants such as other-regarding preferences. However, we will control for other-regarding preferences in all regressions.

$$-\Delta U = \lambda(Y_{s,exp} - Y_{s,real})$$

With increasing overestimation and individual loss aversion the utility loss becomes larger. To make up for the utility loss, sellers can engage in contract breach and raise their utility by increasing their monetary gains. Therefore, our next hypothesis is the following.

Hypothesis II: *With increasing individual loss aversion, sellers engage in more contract breach conditional on overestimation.*

In our set-up, opportunistic behavior pays off but people might still want to keep up their positive self-image and may try to delude themselves for the profitable action (Mazar et al., 2008). A production shock gives the seller the possibility to avoid negative self-signals from failing to meet expectations and to avoid updating their self-concept (Sackeim and Gur, 1979, Dana et al., 2007). Therefore, we expect that sellers in the shock condition will breach the contract to a higher extent than sellers in the no-shock condition. Similarly, contract breach might be amplified under the shock for sellers with a high utility loss from overestimation and individual loss aversion. We deduce the following hypotheses.

Hypothesis III: *a) Sellers in the shock condition will breach the contract to a higher extent than sellers in the no-shock condition. b) The effect of sensed losses on contract breach is stronger in the shock condition than in the no-shock condition.*

5.4 Data and results

5.4.1 Descriptive statistics

In Table 5.1, we summarize observations and differentiate between under-/correct estimators and overestimators. Participants are categorized as under-/correct estimator either if their performance guess is below the real performance or if their performance guess was exactly correct. Sellers categorized as overestimators have a guess that exceeds their real performance.
The majority of sellers, 67.20%, overestimate their performance in the real-effort task whereas 32.80% of sellers correctly assess or underestimate their performance.

In total 186 contracts are concluded from which 143 contracts are fulfillable, i.e., the real performance/output was higher than the offer. Observation numbers in different

TABLE 5.1: Estimation types and compliant behavior

	under-/correct estimator		overestimator		Total	
compliance	32	(52.46%)	38	(30.40%)	70	(37.63%)
breach[1]	15	(24.59%)	86	(68.80%)	101	(54.30%)
over-fulfillment	14	(22.95%)	1	(0.80%)	15	(8.06%)
total	61	(32.80%)	125	(67.20%)	186	(100.00%)
breach[2]	14	(23.33%)	44	(53.01%)	58	(40.56%)

[1]whole sample; [2]only fulfillable contracts

stages and treatments can be looked up in Table D.1 and in Table D.2 in the Appendix. Generally, we see that close to 38% of sellers comply to their contracts whereas the majority breaches the contract and a small fraction over-fulfills the contract.[9] Strikingly, sellers who overestimate breach the contract significantly more often than under-/correct estimators ($\chi^2(1) = 19.759,\ p < 0.001$).[10] To avoid skewing results, we will focus only on the sample of "fulfillable" contracts from now on. In total, our analysis is based on 132 observations.[11]

We will now briefly describe means and results of non-parametric tests from outcome variables of the investment game. The average performance/output in the real-effort task was 12.43 and does not significantly differ between under-/correct estimators (12.63) and overestimators (12.89). Sellers who overestimate are off the mark by on average 4.13 goods. The absolute breach rate, which we define as the difference of offered goods and sold goods, among overconfident agents is on average 2.20 whereas under-/correct estimators shortchange on average 0.23 goods. The difference in breaching among the two estimation types is statistically significant (Mann-Whitney test, $p < 0.001$).

Result I: *Overall, we find that a share of 38% of sellers complied to contracts. Sellers who overestimate their performance breach contracts more often and to a higher extent than under-/correct estimators.*

[9]Sellers who underestimate their performance have a higher income than expected. By comparing their real payoff with the expected payoff, these sellers might feel elated (Bell, 1985). The unexpected gain might feel like windfall money and make people more willing to share this income (Carlsson et al., 2013, Kameda et al., 2002).

[10]Focusing only on fulfillable contracts, sellers who overestimate still breach significantly more often than under-/correct estimators (χ^2-test, $p = 0.011$).

[11]Due to power outages in the field we have missing values for certain covariates. In order to keep the sample constant we had to remove in total 14 observations of the 228 in which contracts were offered (resulting in 214 offers, 172 accepted contracts and 132 fulfillable contracts).

In the following, we will check if individual overestimation is independent of individual characteristics and preferences which could potentially distort results. In Table 5.2, we present results of OLS regressions from different characteristics on overestimation; standard errors in parentheses. Only one regressor was included at a time. The

TABLE 5.2: Test of independence for overestimation

	age	income	alpha	loss aversion	risk seeking	SVO angle
Overestimation	-0.0141	5.635	0.00927	-0.0136	0.0574	0.0367
	(0.0399)	(4.024)	(0.0109)	(0.0416)	(0.0373)	(0.330)
Constant	21.46	245.8	0.234	3.857	3.211	26.44
	(0.177)	(17.88)	(0.0486)	(0.185)	(0.166)	(1.466)
Observations	132	132	132	132	132	132

Standard errors in parentheses

variable *Overestimation* is a continuous measure and calculated by the difference between performance guess and real performance. None of the coefficients is significant implying that different levels of overestimation are independent of fairness preferences such as Social Value Orientation (*SVO angle*) or inequality aversion (*alpha*). Literature suggests that men might be more overconfident than women (e.g. Barber and Odean, 2001). We do not find a gender difference in overestimation. Moreover, students enrolled in an econ program are not different from students enrolled in other programs in respect to performance overestimation (both Mann-Whitney test, $p > 0.4384$).
In the next section, we test our hypotheses from Section 5.3.3 with regression analyses.

5.4.2 Estimation results

Does overestimation affect contract breach?

First, we will resume graphically the effect from overestimation on contract breach. In Figure 5.2 sellers who underestimate their performance range left from *0* on the x-axis. In contrast, sellers who overestimate their performance range right from *0* on the x-axis. Sellers with a guess spot-on can be found on the y-axis. The upward trend of the fitted line suggests that with rising overestimation contract breach increases, which is in line with Hypothesis I. In the following, we will prove if estimation results confirm this first impression.

The underlying OLS Model we use is of the following form:

$$Y_i = \beta_0 + \beta_1 X_i + \beta_3 S_i + \beta_2 P_i + \beta_4 C_i + \epsilon_i,$$

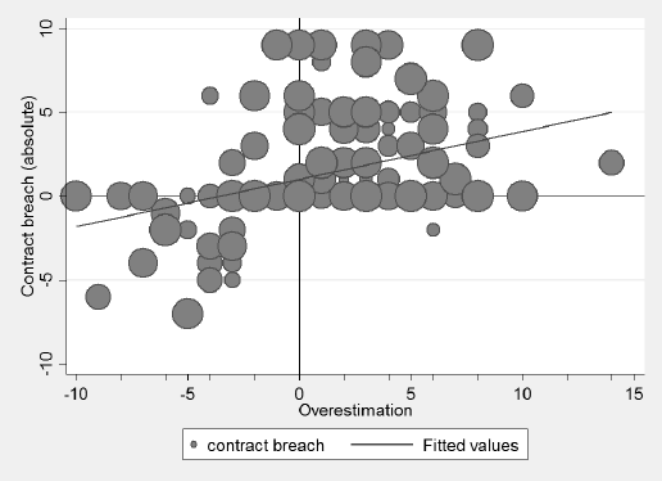


FIGURE 5.2: Effect of overestimation on contract breach (N=132)

where Y_i is the level of contract breach and measured by the difference of sellers' offered and sold goods (*contract breach (absolute)*). Note, that Y_i is zero when sellers comply, becomes positive when sellers shortchange and negative when sellers overfulfill the contract. X_i represents the level of overestimation and is measured by the difference between performance guess and real performance (*overestimation*). S_i is an indicator variable which is positive if a shock affected output and 0 otherwise (*shock*); P_i is sellers' performance (*performance*)[12]; C_i represents a vector of covariates at the individual level and ϵ_i is the error term.

In the IV approach, we use the anchor treatment as an instrumental variable to estimate overestimation. In Table 5.3, we present results based on OLS regressions in Models (1), (3) and (5) and we used an IV approach in the presented Models (2) and (4). Results from all models demonstrate that with rising overestimation contract breach significantly increases. This is in line with Hypothesis I. The IV regressions (Model (2) and (4)) reveal that the reported effects from overestimation are causal.[13] Results from the first stage regression demonstrate highly significant effects from the anchor treatment on overestimation and can be looked up in Table D.3 in the Appendix.[14]

[12]We control for performance in all our regressions since sellers with very high performances cannot overestimate themselves as strongly as sellers with very low performances.

[13]The instrument has an F-statistic close to 10 indicating that the instrument is strong (Staiger and Stock, 1997). As well, the Wu-Hausman test (tested for non-robust standard errors) reveals that we can reject the hypothesis that the variable *overestimation* is endogenous.

[14]On average, sellers guesses are 2.2 units higher under the anchor treatment compared to the no-anchor treatment. This difference is significant (Kolmogorov-Smirnov test $p = 0.012$). The anchor did not alter the distribution of overestimation. To prove that, we reduce individual overestimation under the anchor by the mean of 2.201. When comparing the adjusted anchor distribution with the no-anchor distribution of overestimation, there is no significant difference (Kolmogorov-Smirnov test $p = 0.255$). This way, we showed that the anchor increases guesses to a similar extent at all different performance levels.

Without including covariates (Models (1) and (2)), *performance* has a significant positive

TABLE 5.3: Effect of overestimation on contract breach (OLS and IV results)

	contract breach (absolute)			
	(1) (OLS)	(2) (IV)	(3) (OLS)	(4) (IV)
Overconfidence	0.270***	0.492**	0.259***	0.476***
	(0.048)	(0.199)	(0.048)	(0.174)
Shock	0.007	-0.073	-0.009	-0.109
	(0.384)	(0.406)	(0.392)	(0.410)
Performance	0.066	0.100*	0.033	0.082
	(0.050)	(0.058)	(0.059)	(0.068)
Inverse mills ratio	2.421***	2.347***	2.428***	2.325***
	(0.246)	(0.261)	(0.261)	(0.279)
Constant	0.187	-0.466	1.647	0.463
	(0.739)	(0.880)	(2.648)	(3.301)
Observations	132	132	132	132
Adjusted R^2	0.512	0.420	0.518	0.431
Covariates[a]	No	No	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[a]Covariates: SVO angle, alpha, risk seeking, loss aversion, dummy for female, dummy for experiment's location (Kumasi or Accra), income per month (in real life), age in years, dummy for enrollment in an econ program

effect. A reason can be that with rising performance/output, opportunity costs of compliance with the contract increase. Therefore, sellers with a high performance/output might be more prone to breach the contract to secure a high payoff from the lucrative alternative market. Covariates are included in Models (3) to (5). In general, none of the included covariates are significant at a 5%-level. In these models, the effects from overestimation remain similar. However, the effect of *performance* vanishes in the OLS-regressions. Since there could be sellers planning to breach the contract when making an exorbitant offer, we check in Model (5) if our results of overestimation are robust when including the number of offered goods. Although the coefficient of overestimation loses in magnitude, it remains highly significant. The highly significant effect from *offered goods* suggests that some sellers offer a high number of goods to successfully conclude a contract but probably plan to breach the contract in advance.

The presented results are in accordance with the assumption of reference-dependent

preferences since sellers mitigate the divergence between payoff expectations, the alleged reference point, and the status quo. We formulate our finding from the first regressions in the following *Result II*.

Result II: *With increasing overestimation sellers breach the contract to a higher extent.*

Is the effect of subjective losses on contract breach conditional on loss aversion?

We test Hypothesis II in the following. We expect that sellers with a more pronounced individual loss aversion sense a loss from falling short of the payoff expectations more severely. To counteract this sensed loss, we hypothesize that sellers with a pronounced loss aversion will breach to a higher extent to increase monetary gains than sellers with a less pronounced loss aversion. To test this link, we include an interaction term between loss aversion (λ_i) and overestimation.

$$Y_i = \beta_0 + \beta_1 X_i + \beta_2 \lambda_i + \beta_3 \lambda_i X_i + \beta_6 P_i + \beta_7 S_i + \beta_8 C_i + \epsilon_i,$$

Results for this model specification are presented in Table 5.4. When including the interaction term, overestimation becomes insignificant (Model (1)). However, the interaction effect with loss aversion is significant at a 10%-level. This result indicates that indeed sellers with a relatively high loss aversion experience falling short from the reference outcome more severely.

Result III: *With increasing individual loss aversion sellers breach the contract to a higher extent conditional on overestimation.*

Does the occurrence of shocks facilitate contract breach?

We investigate how the occurrence of a shock affected contract breach (Hypothesis III) now. In general, when focusing at the frequency of breaching, 43.42% sellers breach the contract in the shock condition whereas only 35.71% of sellers engage in breaching in the no-shock condition. However, this difference is not significant ($\chi^2(1) = 0.806,\ p = 0.668$). Moreover, the dummy of the shock condition has neither been significant in any of the models presented in Table 5.3 nor in the first model in Table 5.4. Therefore, we reject *Hypothesis IIIa*.
In the following, we test *Hypothesis IIIb* and examine if the effect from loss aversion is more pronounced when a shock occurred compared to a situation without a production shock. For this purpose, we will rerun the specification presented in the precedent

TABLE 5.4: Effect of loss aversion on contract breach (pooled, shock and no shock condition)

	contract breach (absolute)				
	(1) (pooled)	(2) (shock)	(3) (shock)	(4) (no shock)	(5) (no shock)
Overestimation	0.124	-0.0525	-0.111	0.540**	0.502**
	(0.109)	(0.121)	(0.122)	(0.248)	(0.243)
Loss aversion	-0.104	-0.0305	-0.0157	-0.263	-0.313
	(0.124)	(0.179)	(0.156)	(0.212)	(0.201)
Overestimation × loss aversion	0.0487*	0.106***	0.0895**	-0.0394	-0.0565
	(0.0291)	(0.0347)	(0.0370)	(0.0497)	(0.0525)
Performance	0.110	0.105	-0.134	0.126	0.0142
	(0.0847)	(0.123)	(0.123)	(0.143)	(0.185)
Shock	0.217				
	(0.549)				
Offered goods			0.409***		0.326
			(0.0955)		(0.278)
Constant	-0.203	-3.327	-2.661	5.414	3.643
	(3.328)	(5.068)	(5.152)	(4.960)	(5.081)
Observations	132	76	76	56	56
R-squared	0.262	0.321	0.417	0.340	0.364

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
In all regressions, we control for the following covariates: SVO angle, alpha, risk seeking, dummy for female, dummy for experiment's location (Kumasi or Accra), income per month (in real life), age in years, dummy for enrollment in an econ program

section but run the regressions separately for the two shock conditions.

The results are presented in Table 5.4. Model (2) reveals that the effect from loss aversion is amplified under a production shock. The coefficient gains in magnitude and in significance compared to results in the pooled sample (Model (1)). When additionally controlling for offered goods, the effect remains robust (Model (3)). Interestingly, when no production shock affects output, sellers breach more with increasing overestimation but there is no effect from individual loss aversion (Model (4)). This result remains significant when controlling for offered goods (Model (5)). None of the covariates is significant at a 5%-level.

Recall that the shock can be negative or positive and sellers are not informed about the direction of the shock. Still, the shock can extend the moral wiggle room for those who overestimate their performance (Dana et al., 2007). However, results indicate that only sellers with a high loss aversion who fall short of their reference payoff make use of the moral wiggle room. The fact that the effect from loss aversion is only significant

in non-deterministic environments might have several reasons for instance might these types have a strong preference to hold a promise if accountability is assured.

Result IV: *The occurrence of shocks does not increase the level of contract breach in general. The effect of overestimation on contract breach is conditional on loss aversion and only holds in non-deterministic environments.*

5.4.3 Evidence for reference-dependent behavior

In this section, we will discuss evidence for expectation-based reference-dependent preferences in our experiment. For this purpose, we will first recall the results presented in the previous section. Moreover, data from the ex-post questionnaire on feelings when learning about the status quo and sellers' satisfaction with the payoff from the contract support the idea of reference-dependent behavior. All test results presented are based on data from the reduced sample used in the previous section which excludes non-fulfillable contracts.

Estimation results

We find an effect of overestimation on contract breach even when we control for offers made. Surely, we cannot disentangle this effect impeccably from strategic behavior, meaning that sellers might have anticipated breaching the contract when making an (exorbitant) offer. There are several compelling reasons that this might not be the case: offered goods only affect compliant behavior significantly in non-deterministic environments. Moreover, if offers and performance were too far-off, buyers would not have accepted the contract in the first place. Additionally, we have shown that the effect of overestimation on contract breach is conditional on loss aversion and particularly pronounced in non-deterministic environments. This result clearly demonstrates that a deviation from payoff expectations, the alleged reference point, is perceived as a loss. The perception of a loss can only eventuate if sellers indeed compared the status quo with the reference payoff.

Disappointment

In the ex-post questionnaire, we asked sellers how they felt after they learned how much they produced and preset the following four answers "I felt disappointed", "I felt joyful", "I was surprised" and "something else". We would expect that overestimators would feel more disappointed than under/correct estimators. And this is exactly what we find. From 76 overestimators, 39.47% stated that they are disappointed,

28.95% were surprised and 14.47% joyful. In contrast, only 4 subjects (7.14%) of the under/correct estimators felt disappointed, 41.07% surprised and 33.93% joyful. Overall, about 17% stated that they felt "something else" than our preset anwers. The distribution of stated feelings is significantly different between the two estimation types ($\chi^2(1) = 19.855,\ p < 0.001$). Disappointment is an emotion and it has been shown that emotions can directly affect decision-making. Sellers feeling disappointed might want to discharge this negative emotion by taking what Loewenstein and Lerner (2003) call an "emotion-relieving action". In our set-up, sellers might counteract disappointment from unmet payoff expectations by breaching the contract. Breaching the contract gives them the opportunity to make up for the experienced subjective loss. We indeed find that disappointed sellers shortchange on average 1.77 goods more compared to non-disappointed sellers. This difference is statistically significant using a Mann-Whitney test ($p = 0.007$).[15]

Income dissatisfaction

If sellers compared their real payoff with the alleged reference point, they might be less satisfied if they had failed to reach it. In other words, the higher the experienced subjective loss, the less satisfied might sellers be with their actual payoff.

In the following, we test the effect from overestimation on payoff satisfaction stated in the ex-post questionnaire of the experimental session. We asked sellers "How satisfied are you with your final payoff from the production phase (part 3)?" They could grade their answer on a scale from 1 to 10 whereas 1 meant very low satisfaction and 10 very high satisfaction. Results are presented in Table 5.5. The variable *payoff* entails the final payoff from the investment game. In both Models, we control for sex, location, income in real life, age and enrollment in an econ program in both regressions. Generally, econ students are significantly less satisfied with their payoff from the investment game. The other covariates, that are not presented in the table, are not significant.
To examine if overestimation reduces payoff satisfaction for a given payoff level, we include an interaction term of overestimation and payoff in Model (1). Interestingly, the payoff itself does not significantly increase payoff satisfaction. Here we see that overestimation increases payoff satisfaction overall. However, the interaction effect is negative and highly significant. This suggests that indeed overestimators are less satisfied

[15]To test if disappointed sellers breach more, we created a dummy variable that takes on the value 1 if sellers stated that they were disappointed and takes on the value zero if seller stated "joyful", "surprised" or "something else". We tested if the means of absolute breach rates are different between the two groups with a Mann-Whitney test.

TABLE 5.5: Seller's payoff satisfaction

	Income satisfaction	
	(1)	(2)
Overestimation	0.533**	0.515**
	(0.207)	(0.203)
Payoff	0.0560	0.0364
	(0.0349)	(0.0379)
Share offered	-0.253	-0.616
	(0.941)	(0.982)
Overestimation × *Payoff*	-0.0298***	-0.0304***
	(0.00958)	(0.00972)
Performance	0.0204	0.0490
	(0.0731)	(0.0782)
Shock	0.359	0.369
	(0.309)	(0.310)
Breach (absolute)		0.0407
		(0.0434)
Constant	5.346***	5.547***
	(2.018)	(1.998)
Observations	132	132
R-squared	0.176	0.182

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
In all regressions, we control for female, location, income (in real life), age, econ-dummy

with their payoff.[16] It might be that breaching the contract affect income satisfaction. To test this link, we include the variable *breach (absolute)* in Model (2). However, the effects reported above are robust and *breach (absolute)* has no significant effect.

5.5 Conclusion

In contract situations, overestimating one's own performance can lead to inflated payoff expectations. If overconfident agents use payoff expectations as a reference point, they might sense a loss as soon as they realize that their performance falls short of this reference payoff. As a consequence, agents may try to balance off these losses by breaching contracts and selling their goods to a more profitable alternative market. In

[16]We can rule out that this effect originates from general differences in the distribution of payoffs between the estimation types. The distributions of payoffs from the investment game do not significantly differ between under-/correct estimators and overestimators (pairwise comparison with a Kolmogorov-Smirnov test $p = 0.408$).

this study, we find evidence for this linkage. Results from our lab-in-the-field experiment show that the more agents overestimate the more they breach contracts. By manipulating overestimation in a treatment with a performance anchor, we can show that the reported effect is causal. Moreover, there are several indications that this effect can be explained by reference-dependent preferences. These indicators reveal that agents compared their status quo with the alleged reference point – their payoff expectations. First, we find evidence that higher levels of loss aversion stimulate contract breach for overconfident agents. This implies that loss-averse agents who fail to meet their performance goal compare their status quo with their payoff expectations. Second, our post-experimental questionnaire reveals that agents with high payoff expectations are more often disappointed in the moment of learning about their status quo than agents with rather low expectations. Third, we find that agents become less satisfied with their payoff from the contract with rising overestimation.

A novelty of our study is that we show how a common heuristic such as overconfidence (e.g. Barber and Odean, 2001) may lead to non-compliant behavior in contract settings by forming inflated payoff expectations. In our set-up, the reference point forms endogenously based on one's own performance misjudgment. Furthermore, in contrast to the majority of studies that focus on ex-ante behavioral change under reference points (e.g. Camerer et al., 1997, Rabin, 2000, Abeler et al., 2011), we focus on an ex-post impact of unmet expectations. Moreover, this study contributes to the scarce empirical and experimental literature on reference-dependent preferences. Particularly, we investigated the theoretical idea of Koeszegi and Rabin (2006) that overestimated expectations may lead to a utility loss. According with this theory, we find that individual loss aversion increases contract breach when the status quo falls short of the reference payoff.

There are obviously different reasons why and when people respond to reference payoffs. Payoff expectations might simply serve as a goal that agents want to achieve (Camerer et al., 1997, Oettinger, 1999, Fehr and Goette, 2007). In contract situations, agents' feelings of entitlement to a reference payoff has been found to play a role: If principals "withdraw" part of the reference payoff, agents feel betrayed and punish principals by breaching the contract (Fehr et al., 2011). Another reason for responding to a reference payoff may be negative emotions such as disappointment (e.g. Bell, 1985, Loomes and Sugden, 1986) or dissatisfaction with the status quo. Breaching the contract might serve as an "emotion-relieving action" (Loewenstein and Lerner, 2003) that can counteract the source of disappointment. Our results support that emotions such as disappointment and dissatisfaction play a role. The discussed motives for responding to reference payoffs may be intertwined. Depending on the context of the situation and depending on individual characteristics/preferences some motives may play a more

prominent role than others.

Apart from the results on payoff expectations in general, we find that the occurrence of production shocks alters behavior of specific types of agents. More precisely, overconfident agents who are loss averse are more likely to breach a contract under a production shock compared to a situation where no shock hit the production output. The shock might serve as a convenient justification to breach the contract since here sellers can shift accountability for the failure in performance judgment to the random event. Accordingly, Blanton et al. (2001) found that people are concerned about their self-image and want to avoid cognitive dissonance. Therefore the cognitive bias from overconfidence might be self-sustaining via a confirmation bias. Real life is full of non-predictable events: The farmer might complain about the weather, the manager of a factory about soaring supplier prices and the head of a service center about unreliable staff. The examples illustrate that it is hard to imagine a situation where there is no opportunity to delude oneself and shift accountability of own failure to another event or persons.

We are careful in making strong inferences for policy makers since we used a lab experiment and external validity may be constrained. Still, our results suggest that supporting people to judge own abilities correctly, especially those who are highly loss averse, can reduce contract breach. Even though overconfident agents gain experiences in certain tasks, it has been shown that overconfidence reduces very slowly over time if at all (Hoffman and Burks, 2017). In a different study, we will examine to what extent feedback mechanisms can enhance performance judgment (Fischer and Grosch, 2017). Furthermore, policies should make sure to not raise unrealistic expectations. Such policies may backfire in fostering opportunistic behavior if agents fail to meet these payoff expectations. To curb the temptation of self-delusion in non-deterministic environments, bookkeeping and documenting meaningful events at everyday work could support contract compliance.

Loss aversion plays a major role for effort provision. It has been shown that agents with a high loss aversion exert fewer effort once they have reached their reference payoff as from this point on marginal utility increase is lower for high loss-averse people than for low loss-averse people (Kahneman and Tversky, 2000, Fehr and Goette, 2007). In turn, when loss-averse agents fear to fall short of a reference payoff, they will exert more effort to avoid a sensed loss (Abeler et al., 2011). Since overconfidence triggers high reference points, overconfidence may spur higher effort levels – especially for loss-averse agents. We cannot test this potential positive effect of overconfidence with our data set and leave this question open for future research.

Chapter 6

Conclusion

This dissertation focuses on (social) preferences and how they influence behavior in different work settings. We have shown that pay institutions, such as competitive or discriminatory pay, can have negative effects on social capital in an organization. Paying workers competitively, i.e., announcing bonuses for best workers in a company, may lead to more pronounced perceived rivalry among workers resulting in less prosocial behavior. Apart from competitive bonus schemes, it is not uncommon that pay regimes in companies may discriminate groups of workers. Managers might not deliberately discriminate these workers but jobs within a company are complex and therefore it may be difficult to monitor work and pay all workers objectively. We demonstrated that workers, who are discriminated by the pay regime, become resentful which triggers antisocial behavior towards colleagues who are better-off.
In another experiment, we illuminated why, on average, women have been found more honest than men. This question is of importance in many labor-market contexts since honesty and compliance are the basis for trustful business relationships. We demonstrated that social preferences can (partly) predict dishonest behavior in a context where being dishonest benefits oneself but harms somebody else. We also show that women behave more prosocial than men. As a consequence, our data can explain the commonly found gender effect in honesty by differing social preferences of men and women. In the final study of this thesis, we focus on compliance behavior in contracting situations. In this paper, we show that individual overconfidence affects compliance of signatories. Overconfidence in individual performance leads to exaggerated payoff expectations when contracts are concluded before actual production. We find evidence that these payoff expectations serve as a reference point and agents are willing to breach contracts to reach these self-set targets.
These insights may be valuable for agri-food companies and smallholder farmers especially when they try to integrate into international value chains to increase margins (e.g. Reardon et al., 2009). To integrate successfully, the first step is to build up a good reputation and trustful business relationships. Our data shows that overconfidence may hamper contract compliance. Especially when foreign investors promise high outcomes and benefits, producers may become disappointed when their initial expectations are not be met. Side-selling produce or using other marketing channels can hold higher prices in the short term. Since smallholder farmers are often poor and make rather myopic decisions (Haushofer and Fehr, 2014), they may breach contracts and forgo the benefits of long-term business relationships in order to reduce the divergence between payoff expectations and status quo from the contract. A conspicuous finding in our lab-in-the-field experiment is that agents characterized by a high degree of loss aversion are more prone to contract breach. We find that contract breach is frequent of these

types particularly in non-deterministic environments. This confirms our reasoning that agents search for an excuse to justify their immoral action of breaching the contract. Especially loss-averse agents may feel the urge to breach but only if they can shift accountability to an outside event.

In agriculture, there are weather shocks that affect the quantity of agricultural produce. There may be rain and drought at different times of the year – from sowing to harvesting. It may not always be clear if the weather shocks were mainly harmful or rather advantageous during that period for the quantity and quality of harvest. Yet, our data suggest that independent of the real weather effect, farmers, who conclude contracts before the harvest, may argue that the weather was rather harmful to justify non-compliant actions. Our results cannot exclude, however, that overconfidence has also positive effects on, e.g., exerted work put into crops. It is possible that (performance) goals in contracts stimulate farmers' work motivation. In our experiment, the sequence of actions is reversed and, moreover, the time sequence is short. In real life, farmers may first conclude a contract and afterwards start to produce and as well, more time will pass by between the different events in the contract cycle. They can adjust their effort level depending on different contextual variables. If they do not reach their payoff expectations, they may be disappointed and unsatisfied. Unlike in our experiment, however, these emotions can cool down in real life and agents may decide more prudently. That means, there may be less contract breach if emotions are one major factor driving contract breach when payoff expectations are not met.

When farmers or agri-food companies decide to reach out for other sales markets and, e.g., tap into the export market, they face a higher degree of competition in the market. That is, there is more pricing pressure and exporters need to boast high productivity to prevail in the global market. This requires that incentives are correctly set: improve worker's motivation and mitigate potential costs from negative side effects. The thesis demonstrates that incentives can have detrimental side effects on social capital at the workplace. Depending on the type of work, social capital can be a crucial factor for the organization's success. It follows that managers should be careful in implementing competitive incentives since workers may reflect from how they are paid on their coworkers relations. Similarly, our data implies that managers should also make sure that pay procedures are non-discriminatory. Otherwise, workers may become frustrated and more antisocial when they are paid on unfair grounds. Managers should therefore be careful when implementing pay incentives and weigh up the benefits from higher work motivation with the potential harm for social capital within the organization.

In real life, job seekers have a choice between different jobs at the market. Also, already

hired by a company, workers have the chance to resign and search for another vacancy. It might be that people, who dislike certain payment schemes, do not choose to work for an employer who applies that payment scheme in the first place. However, the workers particularly disliking a competitive or discriminatory pay regime may be the ones responding most notably to the incentives in the lab. Therefore, we might overestimate the effect from incentives on social capital. However, in developing countries, the situation may be a bit different. Many workers, especially in rural areas, are not mobile for several reasons such as lack of money, obligations within the family or they are bound to land they own. Furthermore, labor demand is relatively low, which binds workers to their employer once they found a job (ILO, 2016). Thus, these workers are at the company's mercy and cannot freely choose under which incentive scheme they want to work. Randomizing workers into different payment schemes may therefore be relatively accurate in estimating relative effects in regions where workers are bound to work for a certain company.

Behavioral economics is a well-suited research method to examine the kind of questions we have posed. It is a powerful discipline to investigate how individuals behave under different institutions since experiments can help to understand underlying mechanisms of behavior, i.e., which types of people respond in which way to different institutional environments. This can allow to deduce more concrete predictions for planned policy interventions. Moreover, the lab allows to capture a variety of behavior which is difficult or cost-intense to measure with other research methods. However, critics say that data from lab experiments lack external validity. Experimental studies with a field component demonstrate that lab measures are externally valid in different realms. For example, cheating data from the lab have been shown to predict cheating behavior in real life (Hanna and Wang, 2013, Potters and Stoop, 2016, Dai et al., 2017). Social preferences and loss aversion measured in the lab have been successfully linked to effort provision in the field (Carpenter and Seki, 2011, Fehr and Goette, 2007). Karlan (2005) proved that trustworthy people in the lab are more likely to repay their loans. However, pure lab studies might sometimes lead to wrong conclusions. For example, it has been shown that students in a lab respond to higher wages with higher effort levels (e.g. Hannan et al., 2002, Charness, 2004). However, field experiments support positive reciprocity only weakly (Gneezy and List, 2006, Cohn et al., 2009, Kube et al., 2012).

To further extend our research and to scrutinize external validity, combined studies of lab and field measures could shed light on other important topics regarding the effectiveness of institutions. For instant, unequal labor opportunities for women still persist and can be an important aspect for fostering economic growth (ILO, 2017). Gender gaps

are created by discriminating women in terms of payment, hiring procedures or promotions. Our data has shown that discrimination can inhere detrimental side effects on social capital. As well, we found some evidence that women internalize their gender role and act accordingly. Pay institutions and internalized gender roles may interrelate meaning that certain payment procedures may provoke women to behave stereotypically and may partly explain gender gaps at the workplace. Therefore, an interesting topic for further research could be to investigate how institutions can counteract internalized gender-specific behavior.

In this dissertation, we touched important research realms of how institutions can support processes to keep up compliance to contracts and to improve productivity. As well, we shed light on commonly found gender differences for being honest. In this regard, the behavioral results may add valuable insights for the design and improvement of institutions. I hope that some of the research presented in this dissertation inspires other researchers and will spark off future interesting research projects.

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Appendix A

Appendix of Chapter 2

A.1 Tables

TABLE A.1: Heterogeneous effects on change in PGG and SVO (low dispersion; C vs. T)[a]

	(1) Δ PGG	(2) Δ SVO
Income effect		
loser	0.0609	0.6386
	(0.0861)	(4.1840)
winner	-0.0311	0.2876
	(0.0799)	(3.0165)
Behavioral variables		
not inequality averse	-0.0056	-0.7307
	(0.0737)	(3.4142)
inequality averse	0.0353	1.6570
	(0.0983)	(4.3247)
risk seeking	0.0137	0.4230
	(0.0144)	(0.3580)
dislikes competition	0.0228	-0.7872
	(0.0845)	(3.6898)
likes competition	0.0069	1.7135
	(0.0848)	(4.0513)
Work-related variables		
does not work in teams	-0.0052	-0.1767
	(0.0723)	(4.0470)
works in teams	-0.0350	1.1029
	(0.0949)	(3.8855)
is not aware of bonus	-0.0258	2.1395
	(0.0951)	(5.1746)
is aware of bonus	0.0040	-1.2133
	(0.0808)	(3.2864)
R-squared	0.3633	0.4152
Observations	276	276
Covariates[b]	YES	YES

[+] Robust standard errors clustered at the session level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
[a] This table presents contrasts across C and T, i.e. $(\beta_C + \beta_{CX}) - (\beta_0 + \beta_X)$ as discussed in Section 2.3.3. Contrasts across R and T are not shown.
[b] Covariates: PGG or SVO (stage 1), age, female, risk, preference for competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session.

A.2 Experimental Instructions

Information for the reader of the experiment's instructions:

- The experimental instructions were read out loud by a proctor in the English language.
- Reminders for the proctor are marked by square brackets and italics. This information is not read aloud but was only meant as a reminder for the proctor.
- Posters were used for illustration which can be found in Appendix A3.
- The "Ballpoint-Activity" is the stage where we implemented treatments. Remember that we have a high and a low dispersion treatments. In the instructions, numbers for the low dispersion are in brackets. To avoid that the proctor reads wrong numbers out loud, we had separate printed-out instructions for each treatment (in total six) and blacked numbers that did not belong to the respective treatment.
- Participants received answering sheets and a pen to answer control question and for actual decision making.

Introduction

Hello. We would like to welcome you to today's workshop. This workshop is part of a study from the University of Goettingen. Kerstin Grosch is part of a study program called Global Food. The objective of that program is to understand how globalization affects the living conditions of local communities and how local communities adapt to deal with the challenges of globalization. Today's workshop aims at investigating those questions.
Before we get started, I would like you to stand-up and come to the front. Please bring all your personal belongings with you as you might sit at a different place.
Now we will give you a participation number. This number will identify you during the whole workshop. On the tables, you will find the numbers 1 to 15. Please draw a number from the bag and sit down at the table with the corresponding number.
The workshop will be given in the English language. However, we have individual question rounds where you can ask questions to our assistants in your local language.

What is this workshop about? During the workshop you will have the opportunity to earn money. The money that you earn during the workshop will be paid in cash at the end of the activity. How much you earn will depend on your decisions and luck - so please listen carefully to the instructions.

How is your payment calculated? You will receive 2 Cedi for participating in the workshop. You will get this payment no matter how you take your decisions in the activities. *[Show at the Poster TW]* The workshop is divided into five activities. During each activity you can earn Cedi. Activity three will be paid for sure as we will explain below. For the other four activities we will only select ONE decision that will be paid at the end. To decide which activity will be the one to be paid we will draw one of these four cards. *[Show the cards numbered 1 to 5 – except 3].* After the workshop, you will be asked some questions by our assistants and do some individual activities.
Before we start, let me explain you something else. This study is designed in a way that anonymity of participant's decisions and cash earnings can be assured. To preserve this anonymity, we ask you that from this point on, you do not talk or try to communicate with other participants and that everybody takes precautions to maintain the confidentiality of their materials. Please do NOT talk to other participants or use your mobile phone during the workshop. This said, if at any point you have questions, please do not hesitate to raise your hand and ask us.
It is very important that if you have any question, please ask us. We will answer your questions individually. Please wait until one of the assistants comes to you before you ask your question out loud. We ask you for your understanding regarding this rule.

Before we begin, please verify that you have the following items on your desk:
1. Sheets (held together by a paperclip) turned upside down
2. Six bowls with the individual parts of a ballpoint pen
3. A pen

Please do not turn around the sheets until we say so. You will only turn around ONE sheet at a certain time. Don't worry; we make sure that everybody gets the information WHEN to turn around the next sheet. Your earnings will not only depend on your own decisions but also on the decisions of somebody else in this room.
[Show at the bottom part of poster TW] You will be paired with another participant. We will call this other person participant B.
During all five activities in the workshop the pairings will remain fixed and you will always interact with that exact same person. *[Example: point at one participant and say that this participant is participant no. 2. He could be the one paired with participant no. 8.]* Then those two participants will play all five activities together.)
During or after the workshop, you will not get to know who participant B is and participant B will also not learn who you are. However, you will do all five activities with him or her.
We really want to make sure that everybody understands all activities. Therefore, before you make your real decision in each activity, we do trivia quizzes and ask you

control questions to check for your understanding. Please, try to answer those as well as you can.
Now, we will start with the first activity.

1 Investment Activity (PGG)

[Poster I1] You and participant B are paired together. Each of you receives 10 Cedi for your individual account. You have to decide how many Cedi you want to transfer to a joint account. You can put all, some or none of your Cedi into the joint account. The distribution of Cedi between your individual account and the joint account can be freely chosen. However, you can only invest in 1 Cedi units. So you can invest 0, 1 Cedi, 2 Cedi, etc. up to 10 Cedi.
Your income from the individual account: *[Poster IA1]* Each Cedi you do not invest in the joint account, you can keep for yourself. Nobody except you earns Cedi from your individual account.
Your income from the joint account: The total amount of money in the joint account is the money you invested plus the money participant B invested. Each of you receives 0.7 times of the money in the joint account back. You and participant B receive therefore the **same** outcome from the joint account. You will **benefit** from the money that participant B put into the joint account. For each joint member, the income from the joint account will be determined as follows:

Income from the joint account = Money in the joint account x 0.7
Your total income: Your total income consists of the income from your individual account plus the income from the joint account. Let's see some examples on how the payments are calculated.
[Poster I1; 1^{st} *example]* In this first example somebody invested 2 Cedi in the joint account and participant B invested 4 Cedi. That means that this person keeps 8 Cedi in his individual account. Each Cedi in the individual account gives 1 Cedi back. Hence, the payment in the individual account is 8 Cedi. In total there are 6 Cedi in the joint account, 2 Cedi that this person invested plus 4 Cedi that participant B invested. Each Cedi invested in the joint account gives him/her and participant B 0.7 Cedi back. So the income from the joint account is 6 times 0.7 that is 4.2 Cedi for each person in the joint account.
The total payment is the sum of the income in the individual account and the income from the joint account. In total this person would receive 4.2 Cedi from the joint account plus 8 Cedi from the individual account summing up to a total earning of 12.2 Cedi.
[Poster I2] In order to facilitate the calculations, we prepared this table. The numbers in

bold in the *rows* indicate your possible investments in the joint account. The numbers in bold in the *columns* indicate the investment of participant B. Coming back to our example, if somebody invested 2 Cedi and participant B invested 4 Cedi in the joint account, the payment is 12.2 Cedi: That is the amount you are earning when you invest 4 and participant B 2. In the table you can look up what your outcome will be depending on your decision and participant B's decision. The outcome in the table corresponds with the outcome we calculated before.

Example 2

Let's have a look at another example. How much would participant B receive? *[Poster I1; 2^{nd} example]* In this example you invested 4 Cedi in the joint account and kept 6 Cedi in the individual account. Participant B invested 2 Cedi in the joint account. The total investment in the joint account is therefore 6 Cedi: 4 that this person invested plus 2 that the other person invested. Each Cedi in the individual account gives 0.7 Cedi back to each person. Hence the earning from the joint account is 6 Cedi times 0.7 that is 4.2 Cedi. The total earnings are composed of the earnings from the individual account and the earnings from the joint account, so in total this person receives 6 from the individual account plus 4.2 from the joint account. In total this person earns 10.2 Cedi.

[Poster I2] Let's take a look at the table now. This person invested 4 Cedi in the joint account and participant B invested 2 Cedi *[show numbers with the hands]* The income for this participant sums up to 10.2 Cedi.

Let's compare the earnings of the two participants. The participant who invested 2 Cedi in the joint account earns 12.2 Cedi when participant B invested 4 Cedi. The participant who invested 4 Cedi earns 10.2 Cedi when participant B invested 2 Cedi.

Let us take a look at the last example on this poster *[show at poster I1; 3^{rd} example]*. There, one person invests 4 Cedi and participant B also invests 4 Cedi. In the joint account are therefore 8 Cedi that get multiplied by 0.7. The outcome you receive from the joint money is therefore 8 Cedi times 0.7 which is 5.6. As well as that you kept 6 Cedi in your individual account. When we sum up the outcome from the joint account 5.6 and the outcome from the individual account 6, we get your total outcome for that situation which would be 5.6 plus 6 equals 11.6. We do not need to calculate that – we can also quickly look the outcome up in the table *[show at 4/4 at the Poster I2]*.

Let's take a look at another example: How much would participants in a match receive if both invest nothing *[show at the table 0/0]* in the joint account? When both you and participant B invest nothing, both you and participant B keep all your money in your individual account and keep your 10 Cedi. When both invest all their money, they both end up with 14 Cedi. *[Show at the table with outcomes for "10/10"]* Therefore, as both invest more in the joint account, the earnings increase for both players. If one person

invests nothing in the joint account at all and participant B puts all his money in the joint account *[Show at the table 0/10]*, participant A will benefit from his contribution to the joint account and the earning for participant A would be 17 Cedi. *[Show at the table 10/0]* If participant 1 contributes all his money to the joint account and participant B contributes nothing at all – then participant A earns only 7.0 from the joint account and nothing is left in his individual account as he invested all his money in the joint account.

We are now to do a little trivia quiz! Please, I ask two people of you to come to the front. Any volunteers? Please raise your hand! *[If there are no volunteers, then just call out two random numbers from 1 to 12. Place them in a way in the front, so they can see the poster with the payoff table]*. Welcome to today's first trivia quiz. I will ask you questions one after another. I will call you participant 1, and you participant 2 *[look at the respective persons]*. I will start with a first question for participant 1:

Participant 1, what is your payoff if you chose to invest 1 Cedi and participant 2 chose to invest 8 Cedi in the joint account? *[Answer: 15.3 Cedi. If the right answer is given, say "well done" and if no answer or a wrong answer is given, ask the audience for help]*.
Participant 2, what is your payoff if you chose to invest 5 Cedi and participant 1 invests 4 Cedi? *[Answer: 11.3 Cedi. If the right answer is given, say "well done" and if no answer or a wrong answer is given, ask the audience for help]*.
Participant 1, what is your payoff if you chose to invest 3 Cedi and participant 2 invests 10 Cedi? *[Answer: 16.1 Cedi. If the right answer is given, say "well done" and if no answer or a wrong answer is given, ask the audience for help]*.
Participant 2, what is your payoff if you chose to invest 8 Cedi and participant 1 invests 2 Cedi? *[Answer: 9 Cedi. If the right answer is given, say "well done" and if no answer or a wrong answer is given, ask the audience for help]*.
Thank you very much, participant 1 and participant 2, for being candidates in our trivia quiz.

Before we continue, we would like to verify that we had been sufficiently clear with the explanation of the task. Therefore, we will ask you some questions. **Please turn around the page from your sheet package** *[Take your "show package" and show how to do that]. [Show the sheet control questions and point at the respective positions you are talking about]*. We ask you here what your outcome would be if someone called participant A chose a certain amount as a contribution in the joint account and participant B chose a certain contribution for the joint account. We ask you to write down the outcome for participant A in the gaps on the right for those different situations. Similar to the examples we showed you, you can look up your outcome in the table that you find at the bottom of your sheet. *[show that in front of you on the "show answering sheet"]* Put

your finger on the row that shows your investment, for instant 2 Cedi, and the other finger in the column that shows your partners contribution, for instant 4 Cedi. Now look at the cell where the row and the column cross and you can read the outcome for this combination. *[Show again on the respective position on the control sheet]* This outcome is the outcome you write down in the gap right next to each question on your sheet.

Control Questions for 'Investment Activity'

Control questions

How many Cedis would participant A earn if:

.... participant A invested 2,0 and participant B 7,0 GHC_____

.... participant A invested 2,0 and participant B 2,0 GHC_____

.... participant A invested 0 and participant B 0 GHC_____

.... participant A invested 5,0 and participant B 0 GHC_____

.... participant A invested 9,0 and participant B 3,0 GHC_____

		Participant B										
		0,0	**1,0**	**2,0**	**3,0**	**4,0**	**5,0**	**6,0**	**7,0**	**8,0**	**9,0**	**10,0**
Participant A	**0,0**	10,0	10,7	11,4	12,1	12,8	13,5	14,2	14,9	15,6	16,3	17,0
	1,0	9,7	10,4	11,1	11,8	12,5	13,2	13,9	14,6	15,3	16,0	16,7
	2,0	9,4	10,1	10,8	11,5	12,2	12,9	13,6	14,3	15,0	15,7	16,4
	3,0	9,1	9,8	10,5	11,2	11,9	12,6	13,3	14,0	14,7	15,4	16,1
	4,0	8,8	9,5	10,2	10,9	11,6	12,3	13,0	13,7	14,4	15,1	15,8
	5,0	8,5	9,2	9,9	10,6	11,3	12,0	12,7	13,4	14,1	14,8	15,5
	6,0	8,2	8,9	9,6	10,3	11,0	11,7	12,4	13,1	13,8	14,5	15,2
	7,0	7,9	8,6	9,3	10,0	10,7	11,4	12,1	12,8	13,5	14,2	14,9
	8,0	7,6	8,3	9,0	9,7	10,4	11,1	11,8	12,5	13,2	13,9	14,6
	9,0	7,3	8,0	8,7	9,4	10,1	10,8	11,5	12,2	12,9	13,6	14,3
	10,0	7,0	7,7	8,4	9,1	9,8	10,5	11,2	11,9	12,6	13,3	14,0

Please start now with answering the questions. Please turn around your sheets as soon as you have answered all questions. Our assistants will then collect the sheets.
Has everybody answered the questions? We will then now collect the sheets. *[Assistants collect sheets]*. Now, the assistants will check for your understanding. If we see, that many of you have not completely understood this activity, we will explain it to you again. If there are only some that have problems, we will explain it to you individually.

Now we are ready to start. Nor you or participant B will receive any information on any investment decisions until the very end of the workshop. We will walk around now and distribute the envelopes while you turn around the next sheet. There you see the outcome table again that should help you making your decision. *[Wait a little until everybody has turned around the sheet]* Please take now the white envelope and take out the coins copied on paper. Count if you have 10 coins *[wait until everybody has done so]*. Remember, use the table to decide how much you want to put in the joint account. The money you want to invest in the joint account, you put in the brown envelope. The left-over coins, you keep in your individual account. Those coins you please put in the white envelope. Make sure that at the end no coin is left on your table.
Does anybody have questions? If yes, please raise your hand now and an assistant will come individually to you!
Now, all questions are answered and we are ready to start. Please distribute the coins now in the respective envelopes.
(After ca. 2 minutes): Has everybody made their decisions? Please raise your hand if you need some additional time. *[If somebody does so, wait for another minute – then collect envelopes.]*
Now your task is to guess how much participant B invested in the joint account. If your guess is right, you earn another 2 Cedi. If your guess deviates 1 unit from the actual investment of participant B you receive 1 Cedi. If your guess deviates 2 units from participant B's investment, you receive 0.5 Cedi.
[Show the respective sheet and point at the row where to make the cross] Please indicate on the vertical axis with a cross how much you expect participant B will invest in the joint account.
Does anybody have questions? If yes, please raise your hand now and an assistant will come individually to you!
Please take your decision now!
Please turn around your sheet when you finished this activity.
We will now collect the envelopes and the answering sheets.

1 Money Allocation Activity (SVO)

In this activity there are two roles: role A and role B. The person in role A has to actively decide how to allocate Cedi between participant B and himself/herself. The person in role B has to accept the preferred allocation by the participant in role A.
You will all actively decide on your preferred allocations (this would be the role A) – however for which role you will be paid will be determined at the end of the workshop

Answering sheet for expected contribution of participant B in "investment Activity"

Expected Investment

YOU \ Participant B	0,0	1,0	2,0	3,0	4,0	5,0	6,0	7,0	8,0	9,0	10,0
0,0	10,0	10,7	11,4	12,1	12,8	13,5	14,2	14,9	15,6	16,3	17,0
1,0	9,7	10,4	11,1	11,8	12,5	13,2	13,9	14,6	15,3	16,0	16,7
2,0	9,4	10,1	10,8	11,5	12,2	12,9	13,6	14,3	15,0	15,7	16,4
3,0	9,1	9,8	10,5	11,2	11,9	12,6	13,3	14,0	14,7	15,4	16,1
4,0	8,8	9,5	10,2	10,9	11,6	12,3	13,0	13,7	14,4	15,1	15,8
5,0	8,5	9,2	9,9	10,6	11,3	12,0	12,7	13,4	14,1	14,8	15,5
6,0	8,2	8,9	9,6	10,3	11,0	11,7	12,4	13,1	13,8	14,5	15,2
7,0	7,9	8,6	9,3	10,0	10,7	11,4	12,1	12,8	13,5	14,2	14,9
8,0	7,6	8,3	9,0	9,7	10,4	11,1	11,8	12,5	13,2	13,9	14,6
9,0	7,3	8,0	8,7	9,4	10,1	10,8	11,5	12,2	12,9	13,6	14,3
10,0	7,0	7,7	8,4	9,1	9,8	10,5	11,2	11,9	12,6	13,3	14,0

by luck.

You will be presented a series of decisions like this one *[Poster MA; point at* 1^{st} *decision set]*. Each of the middle lines determines how much you and participant B receive. Looking at the first middle line, the person in role A receives 7.5 Cedi and participant B receives 15 Cedi. As we move to the right, the payment for the person in the role A increases to 8.1, 8.9 up to 12.8. The payment of participant B decreases from 15 to 12.8 at the same time.

The task of the person in role A is to decide on the preferred allocation by marking the midline with a cross on the preferred distribution *[show at* 2^{nd} *illustration on poster MA]*. The marked distribution will determine the payments of the person in Role A and person B in Role B. Only ONE distribution can be marked in each question. In our example on this poster the person marked the midline for the allocation where he gets 11.4 Cedi whereas participant B receives 13.4.

After you marked your preferred allocation with the cross, we write in the upper gap to the right of the question the amount that you would receive if you are selected in role A (which is 11.4) and the amount that participant B will receive (which is 13.4).

What should you avoid? *[Point at the poster with the crossed question]* Please do not make more than one cross. You can only decide on ONE money allocation that is determined by the mid line that connects the two boxes with the corresponding outcomes. If you decided for that ONE allocation, please write the CORRESPONDING outcomes in the

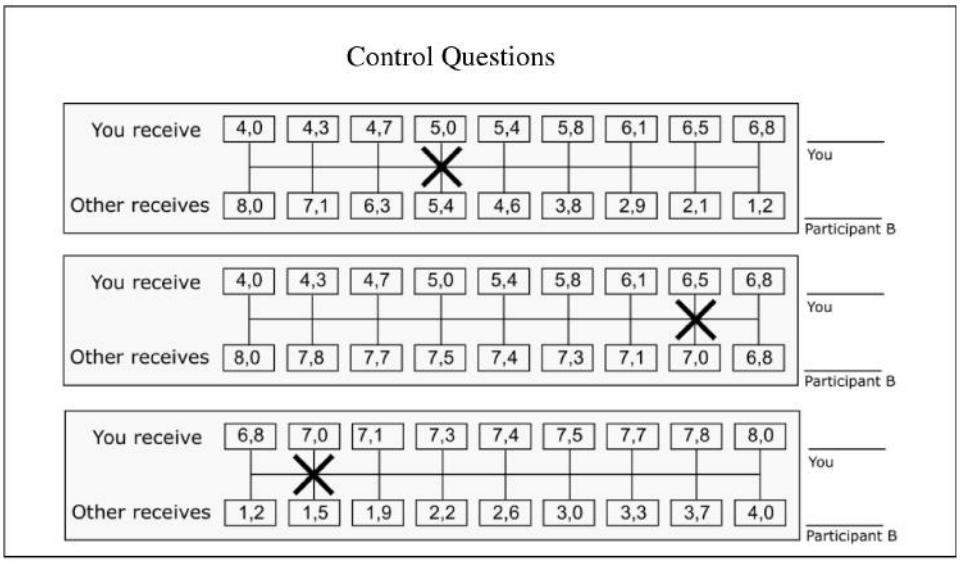
right gaps. Do not make allocations up that are not an option in this activity. You *cannot* mark the boxes on the top and bottom, but only the point in the middle line.

We will now do a little trivia quiz! Please, I ask two people of you to come to the front. Any volunteers? Please raise your hand! *[If there are no volunteers, then just call out two random numbers from 1 to 12. Place them in a way in the front, so they can see the poster with the decision set]*. Welcome to today's first trivia quiz. I will ask you questions one after another. I will call you participant 1, which means that you are in role A, and you participant 2, which means that you take up the passive role B *[look at the respective persons]*. I will start with a first question for participant 1:
Participant 1, take a look at the first question on the poster. Choose one allocation and point with your finger on the respective middle line. In that allocation you chose: How much would you earn?
Participant 2, in that chosen allocation from participant A, how much would you get? *[if no answer or a wrong answer is given, ask the audience for help]*.
Participant 1, take another look at the first question on the poster. Choose one allocation and point with your finger on the respective middle line. In that allocation you chose: How much would you earn?
Participant 2, in that chosen allocation from participant A, how much would you get? *[if no answer or a wrong answer is given, ask the audience for help]*.
Thank you very much, participant 1 and participant 2, for being candidates in our trivia quiz.
Before we continue, we would like to verify that we had been sufficiently clear with the explanation of the task. Therefore, we will ask you some questions.
[Point at the bottom of the poster]: If a participant who is selected in role A, has taken the following decision *[made the cross at a certain position]*, how much would he receive?
How much would participant B receive?

Please turn around the next sheet from your sheet package for answering the control questions *[wait for some seconds until everybody has done so]*. Have a look at what are the preferred allocations of that person in that example you can see in front of you. That person marked his preferred allocation with a cross at the midline. What are the outcomes for *that* person and participant B? Please write down the missing outcomes to the right of each of the three questions for the person and participant B.

Do you have any questions? Please raise your hand! Do you have any other questions? Start now with answering the control questions! *[After a while ask everybody if they have finished. If not they should raise their hands. Then control sheets get collected.]*
We will now collect your answering sheets. Our assistants will quickly check if everybody in the room has understood the task. If there are some people that are still a little

Control Questions for "Allocation Activity"



unsecure with the task, they will be approached by one of our assistants. *[Look to the assistants and see if there is a person that needs further explanation – if so, wait until assistant approached that person and explained the task again to him/her]*.
In this activity there are not right or wrong answers. That is all about your personal preferences.
In this activity you will make six decisions. If this activity is chosen for payment, we will randomly select one question for payment at the end of the workshop. Remember that only one activity will be selected for payment.
After you have made your decision, write the resulting distribution of Cedi if you were selected in role A in the gaps on the right. As you can see, your choices influence both the amount of Cedi you receive as well as the amount participant B receives.
Please turn around the next sheet now. *[Wait some seconds until everybody has done so.]*
Remember how to approach the task *[point again at the poster]*: Look at each of the six different questions carefully. Decide for your preferred allocation and make your cross at the respective midline. Then fill out the gaps to the right and write down the respective outcomes for yourself and participant B.

Ballpoint-Activity - Comp

In this activity, your task is to put together ballpoint pens. On your table, you find 6 boxes with the different parts of a pen. *[Take the different parts and show each of the parts to the participants]*. To assemble the pen, you need a spring, the refill, the upper brass tube

and the lower brass tube. First, put the spring on top of the refill. Then insert the refill with the spring into the upper brass tube. The last step is to set the lower brass tube on top and turn it until it is fixed on the lower part. Now we see that the pen is working – that means that if we push the button the ballpoint pen is retractable.
You will have eight minutes for accomplishing this task. Afterwards, your ballpoint pens will be collected and we will count how many of your assembled ballpoint pens work. Only pens that are functioning *[pressing the push button either exposes or retracts the ballpoint]* will be counted. Please do not tell any other one in the room how many ballpoint pens you managed to assemble.
Your earnings in this activity depend on the performance of participant B. The person in the team who puts more ballpoint pens together will earn 15 Cedi (CL 12 Cedi). The person who puts together less ballpoint pens than participant B will earn 5 Cedi (CL 8 Cedi). In case of a draw, we will randomly assign a "winner". You will receive your earnings depending on your performance in any case. Only ballpoint pens that work will be counted. The average number of ballpoint pens that people had put in this task is 40. After you accomplished that task, you will get an envelope with your payoff for this task enclosed.

Are there any questions? Please raise your hand if so.

We will now do a little trivia quiz! Please, I ask two people of you to come to the front. Any volunteers? Please raise your hand! *[If there are no volunteers, then just call out two random numbers from 1 to 12.]*
Welcome to today's trivia quiz. I will ask you now questions one after another. I will call you participant 1 and you participant 2 *[look at the respective persons]*. I will start with a first question for participant 1:
Participant 1, how much would you earn if you assembled 20 ballpoint pens and participant 2 32 ballpoint pens? *[Answer is 5 or 8 respectively. If no answer or a wrong answer is given, ask the audience for help]*. How much would participant B earn? 12 / 8
Participant 2, how much would you earn if you assembled 41 ballpoint pens and participant 2 40 ballpoint pens? *[Answer is 15 or 12 respectively. If no answer or a wrong answer is given, ask the audience for help]*. How much would participant B earn? 12 / 8
Participant 1, how much would you earn if you assembled 63 ballpoint pens and participant 2 51 ballpoint pens? *[Answer is 15 or 12 respectively. If no answer or a wrong answer is given, ask the audience for help]*. How much would participant B earn?
Participant 2, how much would you earn if you assembled 18 ballpoint pens and participant 2 55 ballpoint pens? *[Answer is 5 or 8 respectively. If no answer or a wrong answer is given, ask the audience for help]*. How much would participant 2 earn?
Thank you very much, participant 1 and participant 2, for being candidates in our trivia

quiz.

Before we continue, we would like to verify that we had been sufficiently clear with the explanation of the task. Therefore we will ask you some questions on the respective outcomes depending on your own performance and the performance of participant B. For that, please turn around the next sheet. *[Wait some seconds until everybody has done so].* We ask you to write down your respective outcome on the right gap for each of the three questions. Remember: The participant that assembled more ballpoint pens in this task will earn 15 Cedi (CL 12 Cedi) whereas the participant that assembles less earns 5 Cedi (CL 8 Cedi). Please start answering the questions now. *[Wait until everybody has done so and collect the answering sheets.]*

If you have questions, please raise your hand and we will come to help you.

Control Questions for "Ballpoint Pen Activity"

Control Questions (Ballpoint)	
1. How many Cedi does participant A earn if:	
Participant A put together 32 ballpoint pens and participant B 37?	_____
2. How many Cedi does participant A earn if:	
Participant A put together 45 ballpoint pens and participant B 53?	_____
3. How many Cedi does participant A earn if:	
Participant A put together 28 ballpoint pens and participant B 67?	_____

Our assistants will now collect the answering sheets. They check if you got the answers right. No problem if you did not get the answers right in the first place! The assistants will approach participants that they feel need some further explanation.

Before we start with the task, you can try out how to assemble ONE ballpoint pen. You will not receive any payment for the ballpoint pen that you put together in this practice round.

We start now with the practice round. Please start now with practicing with only ONE ballpoint pen!

Please check whether your ballpoint pen is working. Now, please disassemble the ballpoint pen you just assembled in the practice round. *[Wait a bit until everybody has done so].*

We are now ready to start with this activity. I will start the stopwatch. As soon as the time runs out I say stop. Please stop working then immediately and raise both hands *[show how to raise the hands].* Afterwards our assistants will bring you bags with your participant number in which you can put the assembled ballpoint pens.

Are there any questions? Please raise your hand and we will come to help you.
After our countdown, you will have eight minutes to work on the task. We tell you when the time has run out. Please stop working when we say "Time is over".
Please start after counting: 3, 2, 1, start
... (subjects work on the task)
"There is 1 minute left"/"There are 15 seconds left"
..."3, 2, 1... stop and raise your hands! Please do not work any longer. We collect and count the assembled ballpoint pens now."
[Go around, put ballpoint pens in the respective bags, bring them to the front, count pens with the back facing the participants.]
We ask you now for some patience. The assistants will now count your assembled ballpoint pens. You will receive your payoff for this task in a white envelope then.
I will distribute some drinks now to shorten the time of waiting. Please, do not talk to any other participant and stay at your seat.
[DO NOT CONTINUE THE ACTIVITY UNLESS FEEDBACK WAS GIVEN]
[Feedback is given: envelopes with money in it will be distributed]
Please look inside the envelope but do not let anybody in this room know how much money is in there. This outcome you get for your performance in the ballpoint pen activity. Leave the envelope on your table and take the money out as soon as we have finished ALL activities.

Ballpoint-Activity - Threshold

In this activity, your task is to put together ballpoint pens. On your table, you find 6 boxes with the different parts of a pen. *[Take the different parts and show each of the parts to the participants]*. To assemble the pen, you need a spring, the refill, the upper brass tube and the lower brass tube. First, put the spring on top of the refill. Then insert the refill with the spring into the upper brass tube. The last step is to set the lower brass tube on top and turn it until it is fixed on the lower part. Now we see that the pen is working – that means that if we push the button the ballpoint pen is retractable.
You will have eight minutes for accomplishing the task. Afterwards, your ballpoint pens will be collected and we will count how many of your assembled ballpoint pens work. Only pens that are functioning (pressing the push button either exposes or retracts the ballpoint) will be counted. Please do not tell any other one in the room how many ballpoint pens you managed to assemble.
What can you earn in this task? You can earn 15 Cedi (TL 12 Cedi) if you put together equal or more ballpoint pens than the average person does. If you put less ballpoint

pens together than the average person then you earn 5 Cedi (TL 8 Cedi). You will receive your earnings depending on your performance in any case. Only ballpoint pens that work will be counted. The average number of ballpoint pens that people had put in this task is 40.

Are there any questions? Please raise your hand if so. We will now do a little trivia quiz! Please, I ask two people of you to come to the front. Any volunteers? Please raise your hand! *[If there are no volunteers, then just call out two random numbers from 1 to 12.]* Welcome to today's trivia quiz. I will ask you now questions one after another. I will call you participant 1 and you participant 2 *[look at the respective persons]*. I will start with a first question for participant 1:
Participant 1, how much would you earn if you assembled 20 ballpoint pens and participant 2 32 ballpoint pens? *[Answer is 5 or 8. If no answer or a wrong answer is given, ask the audience for help]*. How much would participant 2 earn? 5 / 8
Participant 2, how much would you earn if you assembled 41 ballpoint pens and participant 1 40 ballpoint pens? *[Answer is 12 or 15. If no answer or a wrong answer is given, ask the audience for help]*. How much would participant 2 earn? 12 / 15
Participant 1, how much would you earn if you assembled 63 ballpoint pens and participant 2 51 ballpoint pens? *[Answer is 12 or 15. If no answer or a wrong answer is given, ask the audience for help]*. How much would participant 2 earn? 12 / 15
Participant 2, how much would you earn if you assembled 18 ballpoint pens and participant 1 55 ballpoint pens? *[Answer is 5 or 8. If no answer or a wrong answer is given, ask the audience for help]*. How much would participant 1 earn? 12 / 15

Thank you very much, participant 1 and participant 2, for being candidates in our trivia quiz.

Before we continue, we would like to verify that we had been sufficiently clear with the explanation of the task. Therefore we will ask you some questions on the respective outcomes depending on your own performance and the performance of participant B. For that, please turn around the next sheet. *[Wait some seconds until everybody has done so]*. We ask you to write down your respective outcome on the right gap for each of the three questions. Remember: The person that assembled more or equal than 40 ballpoint pens in this task will earn 15 Cedi (TL 12 Cedi) whereas the participant that assembles less than 40 ballpoints will earn 5 Cedi (TL 8 Cedi). Please start answering the questions now. *[Wait until everybody has done so and collect the answering sheets.]*

Before we start with the task, you can try out how to assemble ONE ballpoint pen. You will not receive any payment on the ballpoint pen that you put together in this practice round.

We start now with the practice round. Please start now with practicing with only ONE ballpoint pen!
Please check whether your ballpoint pen is working. Now, please disassemble the ballpoint pen you just assembled in the practice round. *[Wait a bit until everybody has done so.]*
We are now ready to start with this activity. I will start the stopwatch. As soon as the time runs out I say stop. Please stop working then immediately and raise both hands *[show how to raise the hands]*.
Are there any questions? Please raise your hand and we will come to help you.
After our countdown, you will have eight minutes to work on the task. We tell you when the time has run out. Please stop working when we say "Time is over".
Please start after counting: 3, 2, 1, start
(subjects work on the task)
..."There is 1 minute left"/"There are 15 seconds left"
..."3, 2, 1... stop and raise your hands! Please do not work any longer. We collect and count the assembled ballpoint pens now."
[Go around, put ballpoint pens in the respective bags, bring them to the front, count pens with the back facing the participants.]
We ask you now for some patience. The assistants will now count your assembled ballpoint pens. Then, we will see if you managed to assemble more or less than 40. You will receive your payoff for this task in a white envelope then.
I will distribute some drinks now to shorten the time of waiting. Please, do not talk to any other participant and stay at your seat.
[DO NOT CONTINUE THE ACTIVITY UNLESS FEEDBACK WAS GIVEN]
[Feedback is given: envelopes with money in it will be distributed]
Please look inside the envelope but do not let anybody in this room know how much money is in there. This outcome you get for your performance in the ballpoint pen activity. Leave the envelope on your table and take the money out as we have finished all activities.

Ballpoint-Activity - Random

In this activity, your task is to put together ballpoint pens. On your table, you find 6 boxes with the different parts of a pen. *[Take the different parts and show each of the parts to the participants]*. To assemble the pen, you need a spring, the refill, the upper brass tube and the lower brass tube. First, put the spring on top of the refill. Then insert the refill with the spring into the upper brass tube. The last step is to set the lower brass tube on top and turn it until it is fixed on the lower part. Now we see that the pen is working –

that means that if we push the button the ballpoint pen is retractable.
You will have eight minutes for accomplishing the task. Afterwards, your ballpoint pens will be collected and we will count how many of your assembled ballpoint pens work. Only pens that are functioning (pressing the push button either exposes or retracts the ballpoint) will be counted. Please do not tell any other one in the room how many ballpoint pens you managed to ensemble.
Your earnings in this activity depend on participant B. One person in the team will earn 15 Cedi (RL 12 Cedi) whereas the other person will earn 5 Cedi (RL 8 Cedi). We will randomly decide who will receive 15 Cedi (RL 12 Cedi) or 5 Cedi (RL 8 Cedi). You will receive your earnings in any case. Only ballpoint pens that work will be counted. The average number of ballpoint pens that people had put in this task is 40. After you accomplished that task, you will get an envelope with your payoff for this task enclosed.

Are there any questions? Please raise your hand if so.

We will now do a little trivia quiz! Please, I ask two people of you to come to the front. Any volunteers? Please raise your hand! *[If there are no volunteers, then just call out two random numbers from 1 to 12.]*
Welcome to today's trivia quiz. I will ask you now questions one after another. I will call you participant 1 and you participant 2 *[look at the respective persons]*. I will start with a first question for participant 1:
Participant 1, how much would you earn if you assembled 20 ballpoint pens and participant 2 32 ballpoint pens? *[Answer: 5 or 15 (8 or 12). If no answer or a wrong answer is given, ask the audience for help]*. How much would participant 2 earn? 5 or 15 (8 or 12).
Participant 2, how much would you earn if you assembled 41 ballpoint pens and participant 1 40 ballpoint pens? *[Answer is 5 or 15 (8 or 12). If no answer or a wrong answer is given, ask the audience for help]*. How much would participant 2 earn? 5 or 15 (8 or 12).
Participant 1, how much would you earn if you assembled 63 ballpoint pens and participant 2 51 ballpoint pens? *[Answer is 5 or 15 (8 or 12). If no answer or a wrong answer is given, ask the audience for help]*. How much would participant 2 earn? 5 or 15 (8 or 12).
Participant 2, how much would you earn if you assembled 18 ballpoint pens and participant 1 55 ballpoint pens? *[Answer is 5 or 15 (8 or 12). If no answer or a wrong answer is given, ask the audience for help]*. How much would participant 1 earn? 5 or 15 (8 or 12).

Thank you very much, participant 1 and participant 2, for being candidates in our trivia quiz.

Before we continue, we would like to verify that we had been sufficiently clear with the explanation of the task. Therefore we will ask you some questions on the respective outcomes depending on your own performance and the performance of participant B.

For that, please turn around the next sheet. *[Wait some seconds until everybody has done so]*. We ask you to write down your respective outcome on the right gap for each of the three questions. Remember: We will randomly assign either you or participant a payoff of 15 Cedi or 5 Cedi (RL 12 Cedi or 8 Cedi). Please start answering the questions now.
[Wait until everybody has done so and collect the answering sheets.]
Before we start with the task, you can try out how to assemble ONE ballpoint pen. You will not receive any payment on the ballpoint pen that you put together in this practice round.
We start now with the practice round. Please start now with practicing with only ONE ballpoint pen!
Please check whether your ballpoint pen is working. Now, please disassemble the ballpoint pen you just assembled in the practice round. *[Wait a bit until everybody has done so.]*
We are now ready to start with this activity. I will start the stopwatch. As soon as the time runs out I say stop. Please stop working then immediately and raise both hands show how to raise the hands.
Are there any questions? Please raise your hand and we will come to help you.
After our countdown, you will have eight minutes to work on the task. We tell you when the time has run out. Please stop working when we say "Time is over".
Please start after counting: 3, 2, 1, start
...(subjects work on the task)
"There is 1 minute left"/"There are 15 seconds left"
..."3, 2, 1... stop and raise your hands! Please do not work any longer. We collect and count the assembled ballpoint pens now."
[Go around, put ballpoint pens in the respective bags, bring them to the front and count pens with the back facing the participants.]
We ask you now for some patience. The assistants will now count your assembled ballpoint pens. You will receive your payoff for this task in a white envelope.
I will distribute some drinks now to shorten the time of waiting. Please, do not talk to any other participant and stay at your seat.
[DO NOT CONTINUE THE ACTIVITY UNLESS FEEDBACK WAS GIVEN]
[Feedback is given: envelopes with money in it will be distributed]
Please look inside the envelope but do not let anybody in this room know how much money is in there. This outcome you get for your performance in the ballpoint pen activity. Leave the envelope on your table and take the money out as we have finished all activities.

2 Investment Activity (PGG)

[Poster I1] We will now do the investment activity again. You have been already part of the investment activity earlier on. We will quickly remind you what this activity was about: You received 10 Cedi. This money you have in your individual account. *[Poster I1]* You have to decide how much you want to keep and how much you want to invest in the joint account with participant B. Each Cedi kept in your individual account, will give you one Cedi back. Only you receive Cedi from the individual account. Each Cedi that you contribute to the joint account will be summed up with the contribution of participant B and multiplied by 0.7. Both you and participant B receive the same number of Cedi from the joint account independently on how much each of you two invests.

Do you have any questions? Please raise your hand!

You can now turn around the next sheet which you use as an orientation for your outcome only. Our assistants will now walk around and give you your envelopes back. Remember, you will get the white one which represents your individual account and you will get the brown envelope representing the joint account. *[Wait until everybody got their envelopes back]*. Please take now the white envelope and take out the coins. Count if you have 10 coins. Then, please put in the white envelope the amount of money you want to keep in your individual account and in the brown envelope the amount of money you want to invest in the joint account. Make your decision now.
Now your task is to guess how much participant B invested in the joint account. If your guess is right, you earn another 2 Cedi. If your guess deviates 0.5 units from the actual investment of participant B you receive 1 Cedi. If your guess deviates 10 units from participant B's investment, you receive 0.5 Cedi.
[Show the respective sheet and point at the row where to make the cross] Please indicate on the vertical axis with a cross how much you expect participant B will invest in the joint account.
Please turn around your sheet when you finished this activity.
We now collect your answering sheets.

2 Money Allocation Activity (SVO)

In this activity we repeat the money allocation activity. We will shortly summarize this activity for you. *[Show poster MA]* In this activity one person is selected to take up role A. This person has to decide on the preferred allocation of income between himself/herself and participant B. Participants in role B have to accept any Cedi allocation chosen by the participant in role A.

[Point at the respective places on the poster] Remember the approach for this task: First, look carefully at the question and decide on your preferred allocation. Mark the midline for that particular allocation. Then write down the respective outcome on the gaps to the right of each decision set.
You will be making six decisions and you can only make ONE mark for each question. If this activity is chosen for payment at the end, we will randomly select one for payment among the six questions.

Do you have any questions? Please raise your hand!

Now, we would like to start. Please turn around the last sheet for today. *[Wait some seconds.]* Please mark the midline for the distribution you prefer for each question and write down the corresponding outcomes on the right of each question. Please turn around your sheet when you finished this activity.

Selection of activity for payment: This was the last activity for this workshop. Now we will select which activity will be paid. Please select one of these four cards. *[Show the cards with the numbers 1 to 4]* The activity selected for payment is the activity No. X (If the selected activity is the allocation activity) In this activity, we have two roles. Kerstin will select who takes up the role A and who takes up the role B. *[Select one card for each pair.]* In this activity you took six decisions. Now we will select which of the decisions is paid *[select one of six cards]*. The selected decision for payment is the decision X. In this decision, the allocation decision was to distribute ... *[Show the respective answering sheet]*

Material: We kindly ask you to leave all ballpoint pens here. We plan to run more workshops the upcoming days and are reliant on all the material. Please, also take out the money your earned in activity 3 from the white envelope and leave the envelope on the table. Thank you for your understanding.

Confidentiality: We ask you not to talk to your colleagues or to anybody else about the content of today's workshop. As well as that, please keep the information that you can earn money in this workshop confidential. Otherwise, you take away the nice surprise from other potentially invited participants to this workshop. Furthermore, we ask you not to talk even with the other people in the room about your earnings. How much you will earn depends on your decisions but also on luck. If you talk to other participant's about your earning it does not say anything about how well you made your decisions.

Questionnaire: While we estimate the payments, we kindly ask you to help us filling out a questionnaire. Our assistants will approach you one by one. Those who finished filling out the questionnaire, please stay around for some time until Kerstin did the calculations for your payments. Please bring the questionnaire with you. Those who are

not yet ready, please wait until one of our enumerators approaches you. The ones that are not filling out questionnaires are free to walk around in the room. Everybody is free to take one package of biscuits and one soft drink.

End

(payment is given and receipt is signed.)

A.3 Posters

Introductory Poster ("TW")

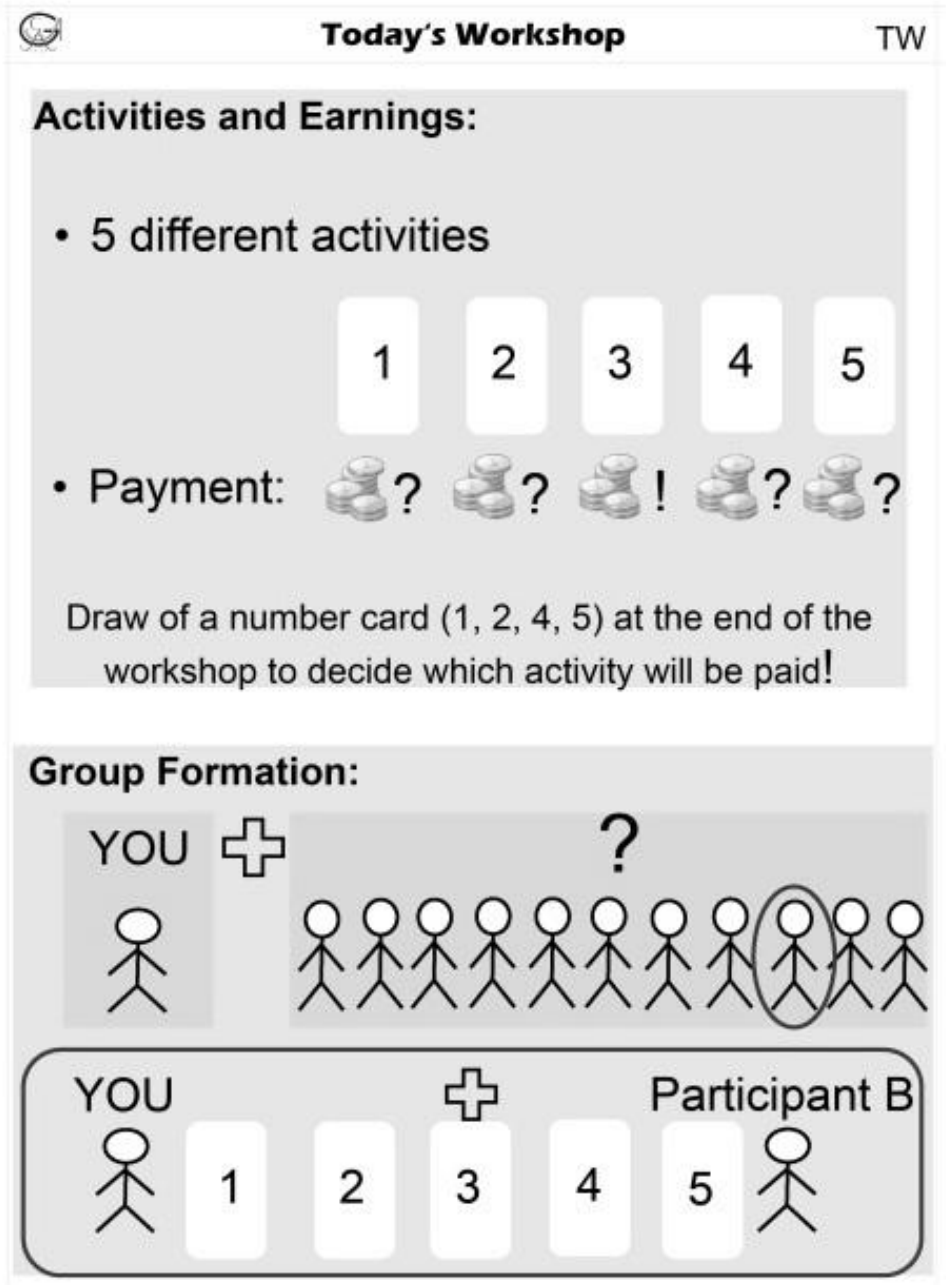
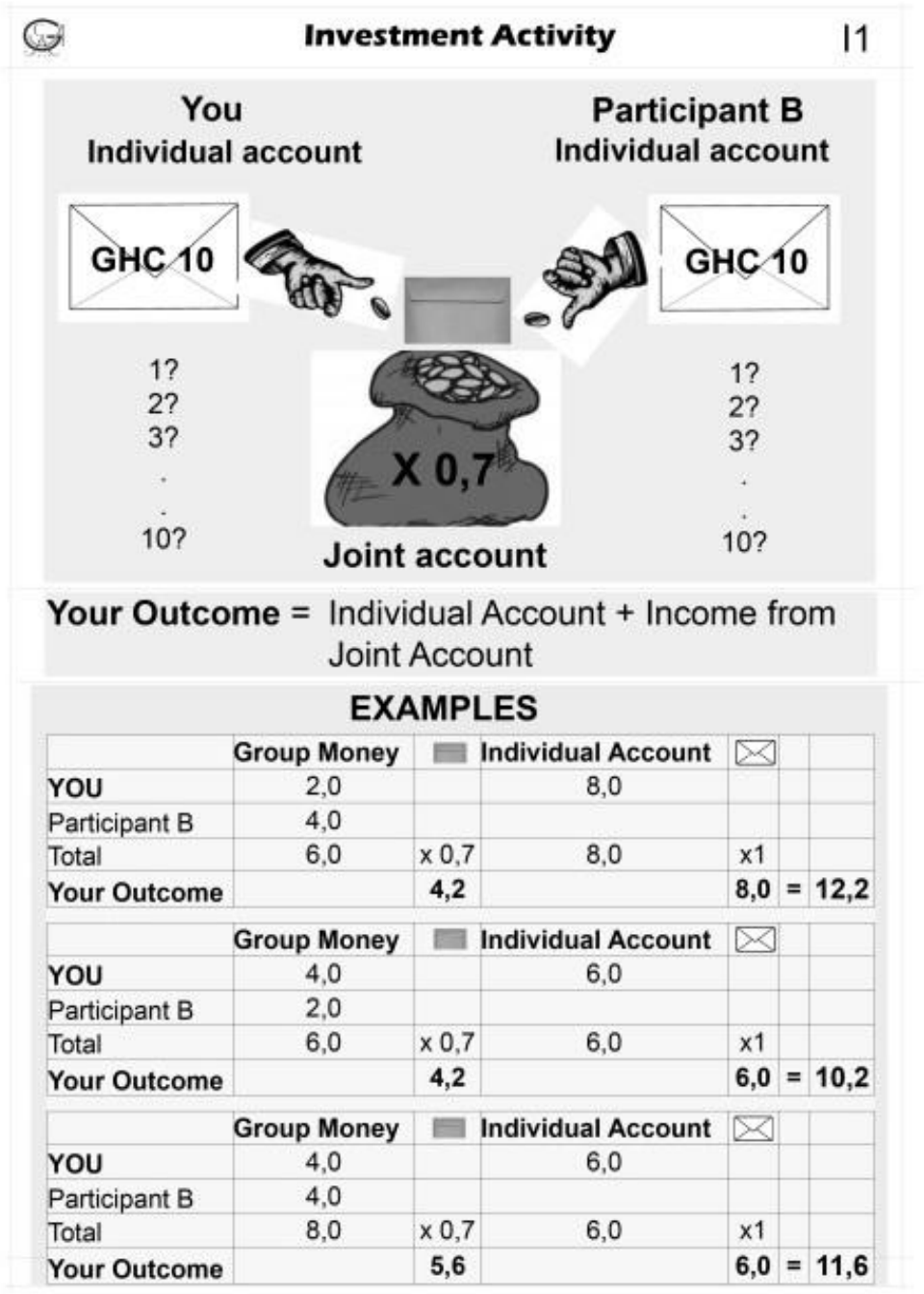


FIGURE A.1: Poster for 'Investment Activity' ("IA")

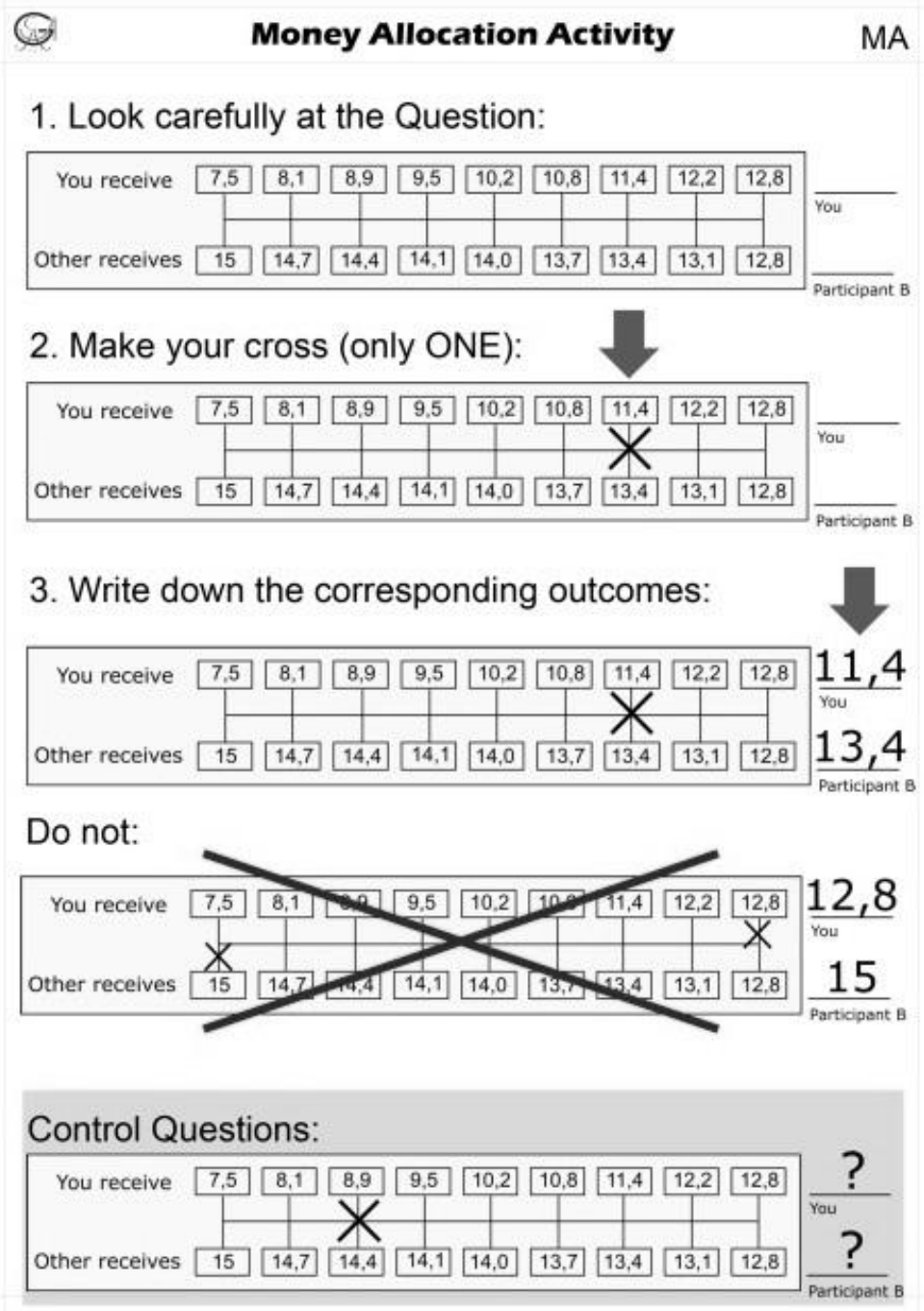


Poster Payoff Matrix for 'Investment Activity' ("I2")

Investment Activity I2

		Participant B										
		0,0	**1,0**	**2,0**	**3,0**	**4,0**	**5,0**	**6,0**	**7,0**	**8,0**	**9,0**	**10,0**
YOU	**0**	10,0	10,7	11,4	12,1	12,8	13,5	14,2	14,9	15,6	16,3	17,0
	1,0	9,7	10,4	11,1	11,8	12,5	13,2	13,9	14,6	15,3	16,0	16,7
	2,0	9,4	10,1	10,8	11,5	12,2	12,9	13,6	14,3	15,0	15,7	16,4
	3,0	9,1	9,8	10,5	11,2	11,9	12,6	13,3	14,0	14,7	15,4	16,1
	4,0	8,8	9,5	10,2	10,9	11,6	12,3	13,0	13,7	14,4	15,1	15,8
	5,0	8,5	9,2	9,9	10,6	11,3	12,0	12,7	13,4	14,1	14,8	15,5
	6,0	8,2	8,9	9,6	10,3	11,0	11,7	12,4	13,1	13,8	14,5	15,2
	7,0	7,9	8,6	9,3	10,0	10,7	11,4	12,1	12,8	13,5	14,2	14,9
	8,0	7,6	8,3	9,0	9,7	10,4	11,1	11,8	12,5	13,2	13,9	14,6
	9,0	7,3	8,0	8,7	9,4	10,1	10,8	11,5	12,2	12,9	13,6	14,3
	10,0	7,0	7,7	8,4	9,1	9,8	10,5	11,2	11,9	12,6	13,3	14,0

FIGURE A.2: Poster Money Allocation Activity ("MA")



A.4 Questionnaire

Information for the reader of the experiment's questionnaire:

- We had eight enumerators who interviewed participants one by one.
- Questions in Section "A. Workshop" are about the experimental session. These few questions were asked after the experimental session. All other questions (and the little experiments for eliciting competitive, inequality and risk preferences) were asked before the experiment began.

Questionnaire

Dear Respondent! My name is _____. This study has been organized by the University of Göttingen. Your cooperation in answering these questions is very much appreciated. There are no "right" or "wrong" answers to *any* of these questions and you will not be judged in any way based on your responses. Please answer all questions as accurately and truthfully as possible. The survey will take no more than 40 minutes. We assure you that your individual responses will not be disclosed to anyone. **Your responses will be treated as completely confidential and will be used for research purposes only!**

INTERVIEW PROTOCOL

1. Name of Enumerator		2. *SNo.*/PNo.	/	*3. Employee No.*	

A. WORKSHOP. Let me ask you some questions regarding the workshop.

Question	**Answers**
A1. How could you make most money in the investment activity?	1 Investment of GHC 0 2 Investment of GHC 10 3 Investment of an amount between GHC0 and GHC 10 4 It depends on participant B's decision

Questions	**Not at all**	**Not very**	**Somehow**	**Quite**	**Very**
A2. How interesting were the activities for you?	☹	😕	😐	🙂	☺
A3. How hard have you tried to do the activities to the best of your ability?	☹	😕	😐	🙂	☺

We will now ask you two questions about the activity where you assembled ballpoint pens:

Items	**Not at all**	**Small extent**	**Somehow**	**Very much**	**Large Extent**
A4. To what extent does your outcome reflect the effort you have put into your work?	☹	😕	😐	🙂	☺
A5. To what extent is your outcome justified, given your performance?	☹	😕	😐	🙂	☺
A6. Please rate now how closely attached you felt to participant B throughout the experiment?	☹	😕	😐	🙂	☺
A7. How rival have you felt towards participants B?	☹	😕	😐	🙂	☺

A8. How many people do you know of the other participants who sat in this room? ☐

A9. Of those people you know: How many would you say you have a close relationship with? ☐

Before, we continue with the other questions, we will now do some activities again (see extra sheet). You will be only p for one of the next two activities.

A10 !!! FEHR-ALLOCATION-ACTIVITY: You will now take some decisions on how to allocate money between yourself and a matched partner in this room. Who the matched partner is, will Kerstin randomly decide by drawing a number card with a participant number. You will be either selected in role A or role B like in the allocation activity during the workshop.
We ask you in total three questions. You have to decide between two different allocation options in each question. You need to decide which of the two options you prefer. What the *partner* receives, we will put in this mint envelope [show envelope]. What *you* receive, we will put in this white envelope. You will be paid for sure for this task. Mind that your decision affects also the outcome of somebody else in this room! The money will be handed out to you at the very end of the workshop.

A11 !!! MARBLE ACTIVITY: I will place one marble on the ground. With a rope, I will measure a certain distance. Your task is to stand 1meter and hit the marble lying on the ground with another marble. We will count how many times you hit the marble. We will play three rounds. You have 10 shots for each round. At the very end of the workshop you will be paid for this activity as we explain below. Before we continue, please try five times to get a feeling for that activity!

A12: The Grain Farmer – on extra sheet!

SECTION B: WORKER INFORMATION

B1. Surname of Worker: _____ B2. Name(s) _____

B3. Gender: 1 Male 2 Female B4. Is English your mother-ton tongue? 1 Yes 2 No

B5. Age (in years)		years
B6. How many people live in your household?		No. of People including Children
B7. How many are children below 15 years?		No. of Children
B8. In how many houses are all the household members accomodated?		No. of Houses
B8. How many bedrooms does your house(s) have to accommodate all persons in the household?		Bedrooms (amount)

B9. What is the monthly income in your household? (Please give your best estimate!)
1 Less than GHC160 2 Between GHC160-299 3 Between GHC 300-499 4 Between GHC 500-999 5 More than GHC 1000

B10. Are you, in your household, the person who contributes most to the household income? 1 Yes; 2 No 3 As much as others (50 + 50)

B11. Marital Status 1 Married 2 Single 3 Separated 4 Divorced 5 Widowed

B12. What is your main ethnicity? 1 Akan 2 Ewe 3 Ga/Dangbe 4 Krobo 5 Hausa 6 Other (specify) _____

B13. What was the last school level you attended? 0 None 1 Primary School; 2 JSS/JHS Middle School 3 SHS -High School 4 HND / Diploma (Go to B14) 5 Graduate / Post-graduate – University (Go to B14)

B14. At this level, how many years have you approved? _____ Years

SECTION BII. PREFERENCES

We will now ask you some questions about your opinions and views:

Item	Never willing	Rarely willing	Sometimes willing	Often willing	Extremely willing
B14. How do you see yourself: Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?	☹	😟	😐	🙂	☺
	Never patient	**Rarely patient**	**Sometimes patient**	**Often patient**	**Extremely Patient**
B15. Are you generally an impatient person, or someone who always shows great patience?	☹	😟	😐	🙂	☺

B16. Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair? 1 Most people take advantage 2 Most people try to be fair

B17. Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people? 1 Most people can be trusted 2 Need to be very careful

B18. Imagine that you received a gift of money. You can choose between getting the gift tomorrow or in one month. If you wait one month, you will receive *more* money. Would you prefer to get:

	Tomorrow	In one month
a- GHC100 tomorrow or GHC110 in one month?	_____	_____
b- GHC100 tomorrow or GHC120 in one month?	_____	_____
c- GHC100 tomorrow or GHC130 in one month?	_____	_____

d- GHC100 tomorrow or GHC140 in one month?	|_____|	|_____|

B19. Given you had the option to get a gift tomorrow of GHC100 – how much money would somebody has to offer you that you would be willing to wait for one month for that gift? |__|__|__|

C. OPINION

We will now read out statements. Please tell us how much you agree with those statements from strongly disagree to strongly agree.

Items	Strongly disagree	Slightly disagree	Somehow agree	Mostly agree	Strongly agree
C1. Many of the unhappy things in people's lives are partly due to bad luck.	☹	🙁	😐	🙂	☺
C2. Becoming a success is a matter of hard work, luck has little or nothing to do with it.	☹	🙁	😐	🙂	☺
C3. Many times we might just as well decide what to do by flipping a coin.	☹	🙁	😐	🙂	☺
C4. I feel that I am a person of worth, at least on an equal plane with others.	☹	🙁	😐	🙂	☺
C5. I am able to do things as well as most other people.	☹	🙁	😐	🙂	☺
C6. On the whole, I am satisfied with myself.	☹	🙁	😐	🙂	☺

D. PERSONALITY

Here we briefly describe some other guy. Please listen to each description and think about how much that person is or is not like you.

Items	Not at all like me	Not like me	Somewhat like me	Like me	Very much like me
D1. It's very important to him to help the people around him. He wants to care for their well-being.	☹	🙁	😐	🙂	☺
D2. It's important to him to show his abilities. He wants people to admire what he does.	☹	🙁	😐	🙂	☺
D3. He believes that people should do what they're told. He thinks people should follow rules at all times, even when no-one is watching.	☹	🙁	😐	🙂	☺
D4. It is important to him that he does his job better than others.	☹	🙁	😐	🙂	☺
D5. He relies on himself most of the time; he rarely relies on others.	☹	🙁	😐	🙂	☺
D6. If a co-worker gets a prize, he would feel proud.	☹	🙁	😐	🙂	☺
D7. The well-being of his co-workers is important to him.	☹	🙁	😐	🙂	☺

E. WAGE EMPLOYMENT

E1. When did you start your employment?		Month/ Year
E2. How many hours do you work per day on average?		Hours
E3. What is your salary?		GH₵ per month
E4. If salary is paid per crate/ box, how many do you fill per day?		Number (if the salary is not paid per crate/box, make a cross)
E5. Is overtime paid extra?		1 = Yes / 2 = I don't work extra hours / 3 = The company does not pay overtime / 4 = No ***>>> E6***
E6a. **If yes**, how much?		GH₵ per hour
E6. Do you get extra payments based on the overall performance of the company?		1 = Yes / 2 = No
E7. Do you get extra payments based on the overall performance of a group (for instant targets)?		1 = Yes / 2 = No
E7a. If you get a production/productivity bonus – how much		GH₵ per month (if no "Productivity Bonus" is received, make

do you get on average per month?		a cross)
E7b. If you get a bonus for high quality work – how much do you get on average per month?		GH₵ per month (if no "Quality Bonus" is received, make a cross)
E8a. On average, what is the amount you get usually as an attendance bonus?		GH₵
E9. Everything included, what do you earn within a month on average?		GH₵
E10. Which of the following alternatives would best describe your skills in your own work?		1 I need further training to cope well with my duties 2 My duties correspond well with my present skills 3 I have the skills to cope more demanding duties
E11. Do you feel bullied/ discriminated at work?		1 Yes 2 No (Go to F1)
E2a. **If yes**, why?		1 Threats of physical violence; 2= Bullying/ harassment 3 Discrimination linked to ethnic background; 4 Other, please specify_____

F. JOB SATISFACTION

Statement	**Terrible**	**Unhappy**	**Mixed**	**Mostly satisfied**	**Pleased**
F1. How satisfied are you with the working conditions at Golden Exotics?	☹	😕	😐	🙂	☺
F2. How do you feel about your job?	☹	😕	😐	🙂	☺
F3. How do you feel about the people you work with – your co-workers?	☹	😕	😐	🙂	☺

G. ORGANIZATIONAL IDENTIFICATION / COMMITMENT

Please rate the following statements from strongly disagree to strongly agree…

Items	**Strongly disagree**	**Slightly disagree**	**Somehow agree**	**Slightly agree**	**Strongly agree**
G1. When someone criticizes [CN], it feels like a personal insult	☹	😕	😐	🙂	☺
G2. In general, I view [CN] problems as my problems	☹	😕	😐	🙂	☺
G3. I am willing to put in a great deal of effort beyond that normally expected in order to help [CN] to be successful.	☹	😕	😐	🙂	☺
G4. There are often breakdowns in communication here*	☹	😕	😐	🙂	☺
G5. People are suspicious of other departments /working groups*	☹	😕	😐	🙂	☺
G6. There is very little conflict between departments/working groups here	☹	😕	😐	🙂	☺
G7. Does your job involve doing all or part of your work in a team?	Yes	No			
[If yes, ask the following, if not jump to ENDING]					
G8. I feel strong ties with my working group	☹	😕	😐	🙂	☺
G9. The supervisors at [CN] do not foster competition between workers*	☹	😕	😐	🙂	☺
G10. Suggestions and contributions of team members are respected.	☹	😕	😐	🙂	☺
G11. There were many personal conflicts in our team.*	☹	😕	😐	🙂	☺
G12. Every team member feels responsible for maintaining and protecting the team.	☹	😕	😐	🙂	☺

ENDING: Thank you very much for answering the questions. Please take the questionnaire and go to Kerstin to receive your payment for today. If there is a queue, please keep some distance to her desk and wait until she is free.

MARBLE_ACTIVITY/FEHR-ALLOC

A10: FEHR-ALLOCATION; PARTICIPANT: |_____|

The different questions:

Round	Option 1 YOU / Partner	Option 2 YOU / Partner	Decision (Option1 or 2)	*Information for Kerstin only*
ENV	0,50 / 0,50	0,50 / 1,00	|_____|	3
PRO	0,50 / 0,50	0,50 / 0,00	|_____|	2
SHA	0,50 / 0,50	1,00 / 0,50	|_____|	1

A11: MARBLE-ACTIVITY

- **Round 1:** In this round, you will be matched with someone else in this room. Kerstin will draw any other participant number afterwards to determine your matched partner. Depending on who of you two will hit the marble more often, will get GHC2,5 whereas the other one receives GHC0,5. Start now with your ten throws!
- **Round 2:** In this round, you will get for each hit GHC 0,25! Start now with your 10 throws!
- **Round 3:** In this round, you can decide on how you want to be paid:

  Option 1: Get paid GHC 0,25 for each marble hit

  Option 2: Become matched with somebody else and you will receive GHC2,5 if your performance is better than the performance of your matched partner. Otherwise you will receive GHC0,5.

  For which option do you decide? OPTION |___|

  [Let participant do the activity and report the performance and decision on report-table**]**

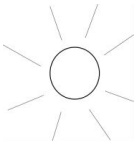
Round	Performance	*Information for Kerstin only*
1		COMPETITION
2		PIECE-RATE
3		SELF-SELECTION

MARBLE_ACTIVITY/FEHR-ALLOC

A12: The Grain-Farmer

We are on the Golden Exotics banana plantation right now. There is good weather and bad weather for bananas. Which of the two pictures stands for good weather and which stands for bad weather?
Imagine that you are a grain farmer and you are given 10 kilos of seeds for free for the coming season. You can choose between two different types of seeds to take. You can either take the seeds of Abdu or of Bara. It is important that you realize you can take as many kilos of Abdu and Bara seeds as you want as long as at the end you are taking 10 kilos in total—no more and no less. On your answer sheet you will see a place for you to write how many Abdu seeds and Bara seeds you will take.

- **Explanation of the differences of Abdu and Bara seed:** *Abdu seed is of higher quality than Bara seed but is more vulnerable to the weather.That is, when there is good weather the Abdu seed produces a harvest that sells for GHC 300. When the weather is bad the harvest is so bad that it cannot be sold, eaten, or fed to the animals. On the other hand, the Bara seed does not respond to the weather and always gives GHC 100 per kilo.*
- Go through the different columns for the ABDU seed and explain the different quantities of Abdu seed affect one*s income from the harvest given GOOD weather (afterwards bad weather)!
- Go through the different columns for the BARA seed and explain the different quantities of Abdu seed affect one*s income from the harvest given GOOD weather (afterwards bad weather)!
- Ask control questions: If the weather is good and one had 5 kilos of bara seeds, how many Abdu seeds does one have? How much money does one make from these Bara seeds? … from these Abdu seeds? In total? (repeat those type of questions until you feel confident about understanding)
- Tell about weather: That it is unknown if there is bad weather or good weather. The chance of good weather is 50% and the chance of getting bad weather is 50%.

										
Your Decision	Kilos of Seed Abdu	Kilos of Seed Bara		Yield Abdu	Yield Bara	Total Yield		Yield Abdu	Yield Bara	Total Yield
	0	10		0	1000	1000		0	1000	1000
	1	9		300	900	1200		0	900	900
	2	8		600	800	1400		0	800	800
	3	7		900	700	1600		0	700	700
	4	6		1200	600	1800		0	600	600
	5	5		1500	500	2000		0	500	500
	6	4		1800	400	2200		0	400	400
	7	3		2100	300	2400		0	300	300
	8	2		2400	200	2600		0	200	200
	9	1		2700	100	2800		0	100	100
	10	0		3000	0	3000		0	0	0

Appendix B

Appendix of Chapter 3

B.1 Tables

TABLE B.1: Summary statistics on destructive behavior and performance in the treatments

	Discrimination	Competition	Random	All data
Number of observations				
Type B	40	48	38	126
Type A5	20	24	19	63
Type A15	20	24	19	63
All data	80	96	76	252
destruction level				
Type B	1.80	1.27	0.89	1.33
Type A5	0.70	0.42	0.89	0.65
Type A15	0.60	0.75	1.32	0.87
All data	1.23	0.93	1.00	1.04
destruction frequency				
Type B	0.50	0.31	0.29	0.37
Type A5	0.20	0.21	0.32	0.24
Type A15	0.25	0.25	0.42	0.30
All data	0.36	0.27	0.33	0.32
performance (RE task)				
Type B	30.65	24.60	23.58	26.21
Type A (A5 & A15)	29.43	36.69	25.29	28.58
Type A5	23.85	32.96	25.53	27.83
Type A15	35.00	40.42	25.05	34.06
All data	30.03	30.65	24.43	28.58

TABLE B.2: Inequality-aversion interactions for Table 3.2

	destruction level of type-B workers				
	(1)	(2)	(3)	(4)	(5)
Inequality Aversion					
Alpha	0.184***	0.176***	0.183***	0.175	0.162
	(0.063)	(0.063)	(0.065)	(0.108)	(0.106)
Beta	-0.798**	-0.794**	-0.986**	-1.366	-1.385
	(0.358)	(0.363)	(0.386)	(0.873)	(0.863)
Alpha x Unj. Base				-0.163	-0.152
				(0.242)	(0.242)
Beta x Unj. Base				-1.018	-1.068
				(1.199)	(1.203)
Alpha x Unj. Random				-0.127	-0.0998
				(0.204)	(0.203)
Beta x Unj. Random				1.243	1.012
				(1.136)	(1.182)
Pseudo R^2	0.053	0.064	0.103	0.115	0.117
Obs.	126	126	126	126	126
Covariates	NO	NO	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

B.2 Experimental instructions

Information for the reader of the experiment's instructions:

- The experiment was conducted in the German language at the University of Goettingen. This is a translation of the original text.
- To ease readability/understanding, words or explanations are added. This is marked by square brackets.
- We conducted the experiment on computers with z-tree. The instructions were displayed on the screen, except for the real-effort task. For the real-effort task, we handed out printed instructions.

Introduction

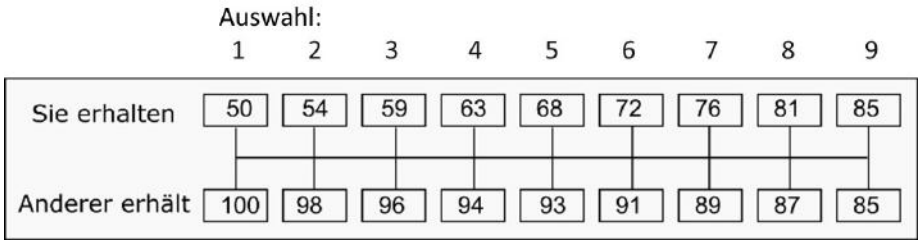
Welcome to the experiment. We are glad that you made time for participating today. Participants earn at least €5 today. Depending on your decisions and the decisions of the other participants, you may earn additional money during the experiment. We guarantee your anonymity during and after the experiment. Please do not talk to other participants and switch off your mobile phone. The experiment contains a small questionnaire, the experiment with four phases and a concluding questionnaire. You will get instructions at the beginning of each phase. Please look at your display now and answer the following questions.

Phase 1

Phase 1a [Social Value Orientation]

We will now start with the instructions for phase 1. It contains three parts: 1a, 1b and 1c. Please press "ok" and we will start with the instructions for phase 1a. Here, the computer will randomly match you with another participant. Then, you and the matched participant will simultaneously face multiple decisions. The identity of you and the matched partner will not be revealed during or after the experiment. Decisions will be made in Tokens with an exchange rate of 1 Token = 0.02 Euro. You will face six different decision sets. The decision sets hold different payoffs for you and the matched participant. Below you can see a possible example. Your personal payoff is displayed in the upper row "Sie erhalten" ("You receive"). The payoff of the matched participant is displayed in the lower row "Anderer erhaelt" ("Other receives"). You can choose

between nine different allocations of Token between you and the other participant. In each of the six situations, you have to choose one of the nine possibilities.
We describe two examples based on the decision set below: If you chose "2", you would receive 54 Token. The matched participant would receive 98 Token.
If you chose "6", you would get 72 Token. The matched participant would get 91 Token.



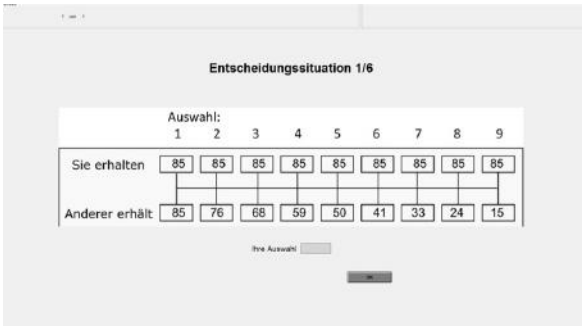
Role of A and B: The person in role A decides actively about money allocations. The person in role B is passive and has to accept A's decision. Each participant will decide in role A. At the end of the experiment, the computer will randomly assign you role A or role B (equally for the matched participant). If you get assigned to role A, your active decision determines the payoffs and the matched participant has to accept the allocation. Similarly, if you get assigned to role B, the matched participant is active (and you are passive) and his/her decision determines the payoffs.

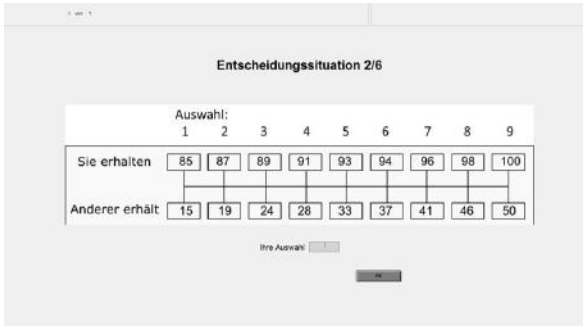
Payoff: At the end of this phase the computer randomly draws one of the six decision sets for payment. Moreover, the computer randomly assigns role A and role B within the dyads. At the end for payment, Tokens will be converted to Euro. We will inform you about the chosen decision set and which role (A or B) was assigned to you at the very end of the experiment. Furthermore, you will learn about the resulting payoff of phase 1a. Please raise your hand if you have any questions. If that is the case, we will come to your cabin to answer your question in private. After all participants finished reading and all questions are answered, we will start with decision-making in phase 1a.

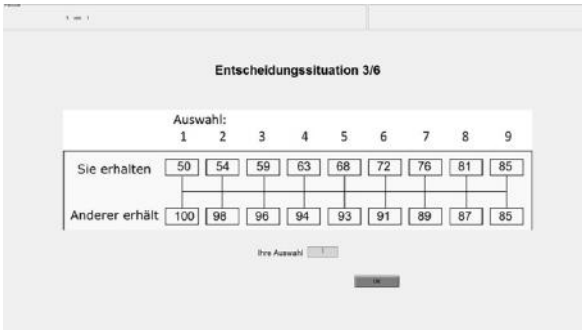
Phase 1b [Inequality aversion (beta)]

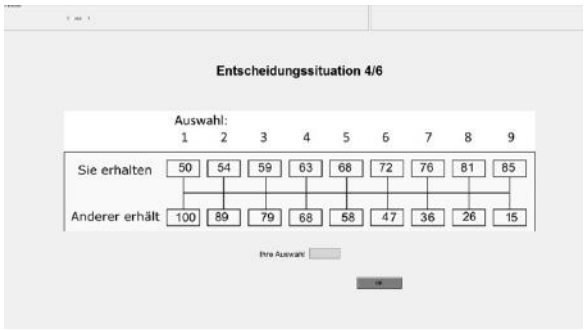
We will now start with the instructions for phase 1b. In this phase the computer will randomly match you with another participant. Afterwards you and the matched participant will simultaneously face multiple decisions. The identity of the matched partner will not be revealed during or after the experiment. Your decisions will be made in Token. The exchange rate is 1 Token = 0.15 Euro. In this phase the person in role A will choose between two possible distributions of Token between him-/herself and another

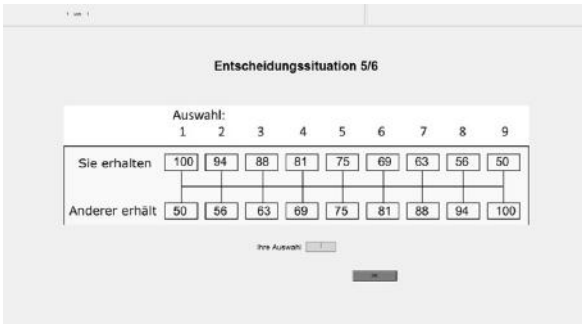
Decision-making

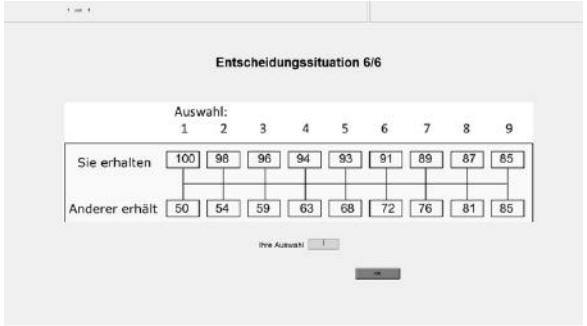












[Translation: Sie erhalten = You receive; Anderer erhält = Other person receives; Ihre Auswahl = Your choice]

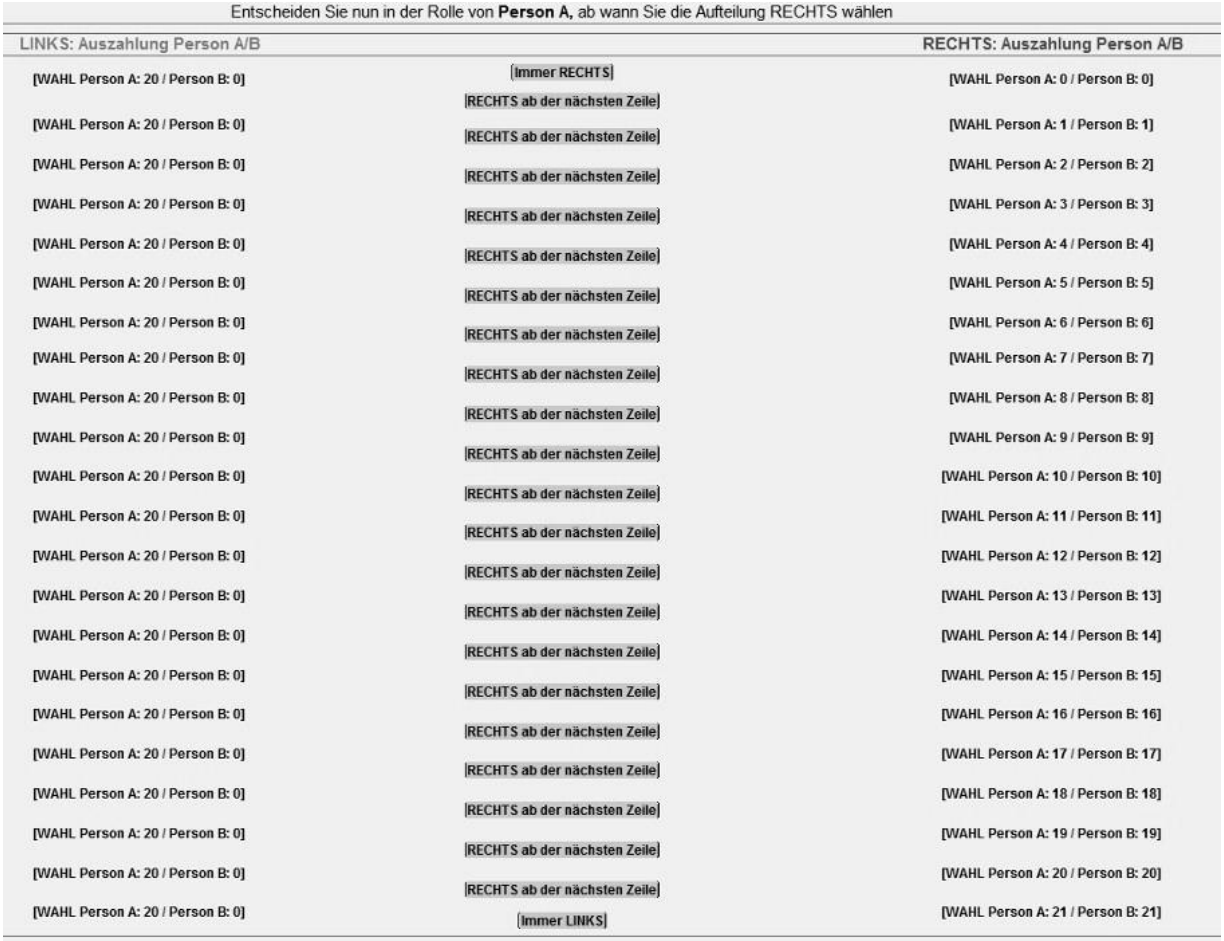
person in role B. There are 22 decision situations. Please read the following sections carefully before making your decision.
The decision sets will be displayed in a table. Below you can see an example which is an excerpt from the table. You will decide in role A. For the problem displayed below the choice of LINKS [Translation: links = left] results in an outcome of 20 Token for yourself. The choice of RECHTS [Translation: rechts = right] results in an equal outcome for you and participant in role B of 5 Token each.]

LINKS: Auszahlung Person A/B	RECHTS: Auszahlung Person A/B
[WAHL Person A: 20 / Person B: 0]	[WAHL Person A: 5 / Peson B: 5]

The decision problems will be displayed in a table encompassing 22 decision sets. Below you can see a screen shot of the table.
One single decision is relevant for all 22 decision situations: As you can see below the left distribution is always the same for each of the 22 situations. You earn 20 Token and person B receives 0 Token. The distributions on the right are always distributions with equal payment of you and person B. By clicking on "RECHTS AB DER NÄCHSTEN ZEILE RECHTS" [Translation: rechts ab der nächsten Zeile = right from the next row on) you can decide from which distribution onwards you would prefer to choose distributions on the right. This means you decide for LINKS in all previous lines. The decision from LINKS to RECHTS automatically influences all 22 decision situations.] After clicking the button the lines will be colored accordingly: All decisions you chose LINKS will be marked green and all decisions you chose RECHTS will be marked blue.

Roles A and B: The person in role A has to decide between two possible distributions of Token between him-/herself and person B. The person in role A can decide actively, whereas the person in role B is passive and has to accept the decision of A. Each participant decides in role A. At the end of the experiment the computer will randomly assign the roles A and B within the dyads. If you get assigned to role A, your decisions will determine the outcome. The matched participant will be in role B in this case and has to accept your chosen distribution. If you are assigned to role B, the matched participant acts as person A and his/her decision will be implemented. You have to accept this decision in this case.

Payoff: The computer will randomly draw one of the 22 decision sets at the end of the phase. The drawn decision set is the one that will be relevant for your payoff in this phase. Furthermore, the computer will select if your own decision (role A) or the matched participant's decision will determine the payoffs. For payment, Tokens will be



Entscheiden Sie nun in der Rolle von **Person A**, ab wann Sie die Aufteilung RECHTS wählen

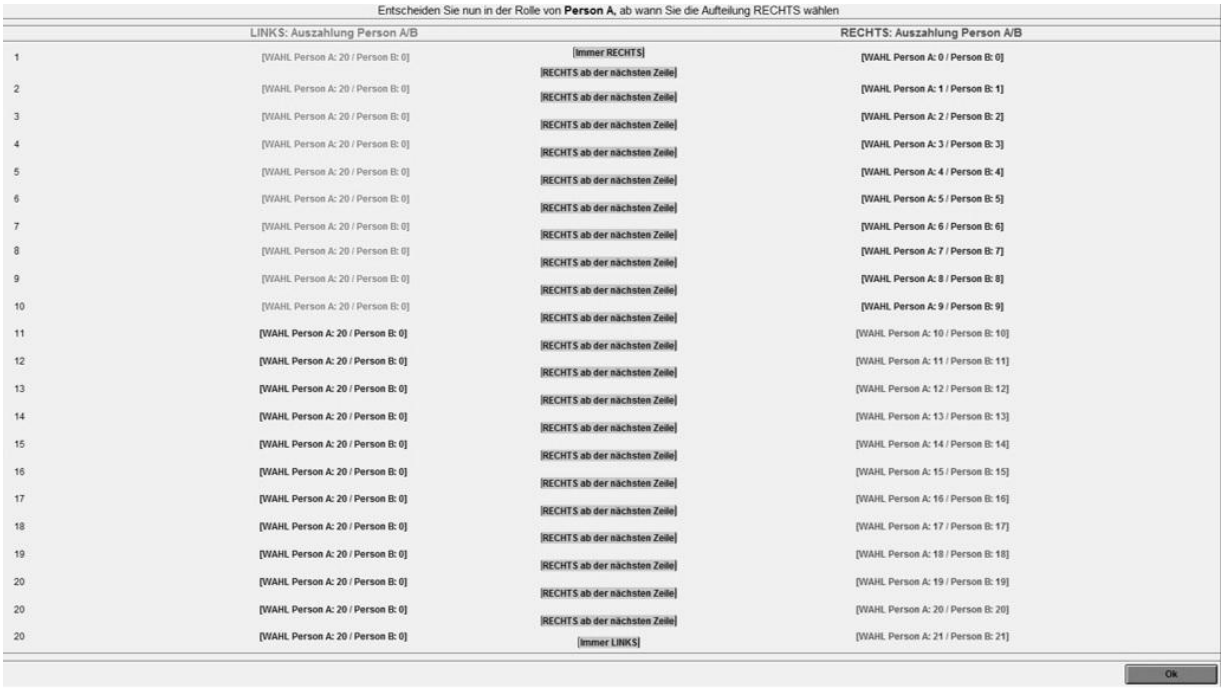
LINKS: Auszahlung Person A/B		RECHTS: Auszahlung Person A/B
[WAHL Person A: 20 / Person B: 0]	[Immer RECHTS]	[WAHL Person A: 0 / Person B: 0]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 1 / Person B: 1]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 2 / Person B: 2]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 3 / Person B: 3]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 4 / Person B: 4]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 5 / Person B: 5]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 6 / Person B: 6]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 7 / Person B: 7]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 8 / Person B: 8]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 9 / Person B: 9]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 10 / Person B: 10]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 11 / Person B: 11]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 12 / Person B: 12]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 13 / Person B: 13]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 14 / Person B: 14]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 15 / Person B: 15]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 16 / Person B: 16]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 17 / Person B: 17]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 18 / Person B: 18]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 19 / Person B: 19]
[WAHL Person A: 20 / Person B: 0]	[RECHTS ab der nächsten Zeile]	[WAHL Person A: 20 / Person B: 20]
[WAHL Person A: 20 / Person B: 0]	[Immer LINKS]	[WAHL Person A: 21 / Person B: 21]

converted to Euro. We will inform you about the chosen decision situation at the end of the experiment. In addition, we will inform you if you were randomly assigned to role A or role B. Furthermore you will learn the resulting payoff for phase 1b. Please raise your hand if you have any questions. If that is the case, we will come to your cabin to answer your question in private. After all participants finished reading and all questions are answered we will start with phase 1c.

Phase 1c [Inequality Aversion (alpha)]

We will now start with the instructions for phase 1c. In this phase the computer will randomly assign a partner to you. Afterwards you and the matched participant will simultaneously face multiple decisions. The identity of the matched participant will not be revealed during or after the experiment. Your decisions will be made in Token. The exchange rate is 1 Token = 0.15 Euro.
In this phase all participants decide first as person A and afterwards as person B. Person A has to choose one of 20 possible distributions of Token between him-/herself and

Decision-making



[Translation: Links: Auszahlung Person A/B = left: payoff person A/B; Rechts: Auszahlung Person A/B = right: payoff person A/B; immer rechts = always right; RECHTS ab der nächsten Zeile = right from the next row on forwards]

the assigned partner. Person B can either accept or reject the distribution person A will choose. If person B accepts the proposed distribution, this distribution will determine the payoffs. If person B rejected the distribution, both persons receive 0 Token. The computer will randomly select the two participants in a dyad into the roles of person A and B.

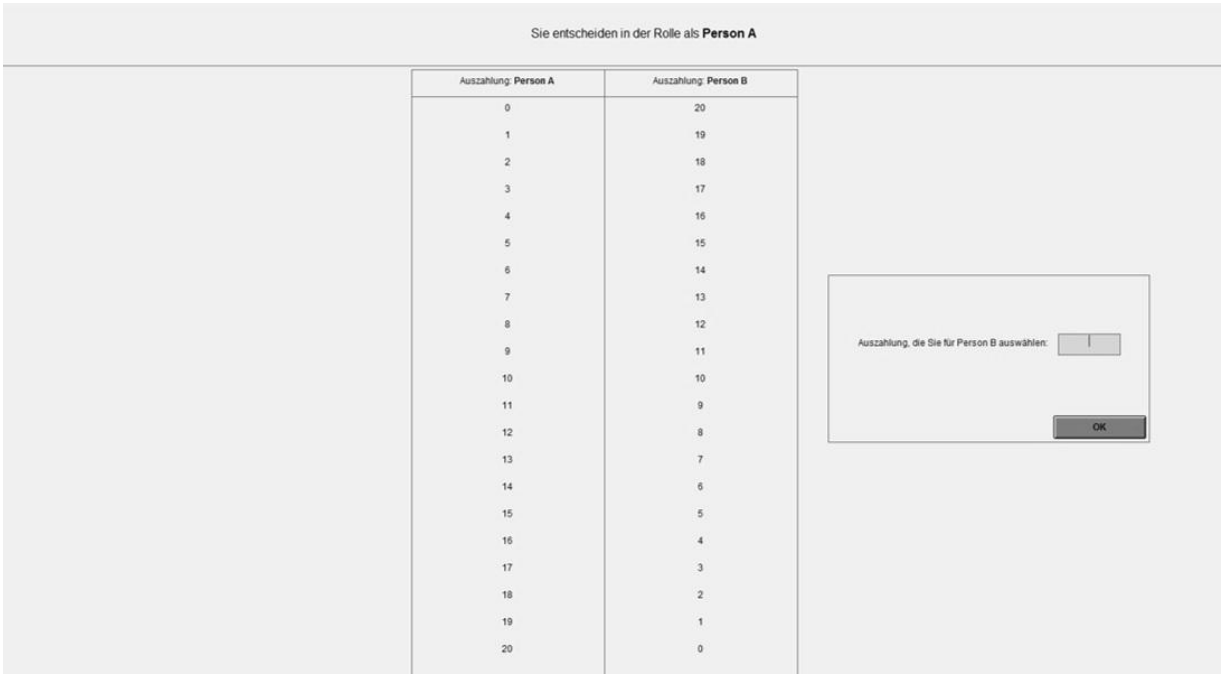
Please read all of the following sections carefully before making your decision. First you decide in the role of person A which share of 20 Token you want to offer person B. Afterwards you have to decide as person B which of the 20 possible distributions you accept and which money distributions you would reject. The decision problems will be displayed in a table. On the next screen we will show you the table. You will see 21 decision sets and you will decide from which distribution onwards you would accept the proposal of person A. After clicking the distributions will be colored accordingly.

One decision is relevant for all 21 situations: You can see below that on the left side all possible distributions of the 20 Token are displayed. If you decide to click on "Annahme ab der nächsten Zeile" [Translation: Annahme ab der nächsten Zeile = accepted distribution from next row onwards] from a particular distribution, this means you are willing to accept all subsequent distributions between person A and yourself. The

accepted distributions will be marked green. All previous distributions (where you would get less Token) will be refused. Those are marked blue.

The computer decides randomly if you will be assigned to role A or role B. If you will be in role A you will determine the payoffs if person B accepted your proposed distribution. However, if person B rejected your proposed distribution you and person B get 0 Token. If you will be in role B, you are passive and the other person in role A will determine the payoffs - however, only if you accepted that particular distribution. If that money distribution was rejected according to your choice both you and person B get 0 Token. We will inform you about your role, the decision and your payoff in phase 1c. Please show up if you have any questions. If that is the case, we will come to your cabin to answer your questions. After all participants finished reading and all questions are answered we will start with phase 1c.

Decision-making



[Translation: Sie entscheiden in der Rolle als Person A = You decide in the role of person A; Auszahlung Person A = payoff person A; Auszahlung, die Sie für Person B auswählen = Payoff, you choose for person B]

Phase 2 [Real-effort task]

[Comment: The instructions for phase 2 were handed out on paper]

[Treatment Baseline] In this phase you will participate in a task. Your display will show matrices with the numbers 0 and 1. Below you can see an example:

Your task is to count the zeros in each of those matrices. Please enter your result in the box below "How many zeros are in the matrix?". Afterwards a dialogue box saying "Please start now with the next matrix" will be displayed. Press "o". A new matrix will pop up. Your old result will still be in the result box. Please, simply delete it. You will have 8 minutes for this task.

Payoff of the task: After finishing the task, the computer will randomly select 12 participants (of 24). Those will be called participants A and rewarded according to the number of correctly counted tables. The other 8 participants are called participants B below and do not get a reward for this task. Participants A will be ranked according to their performance. The more tasks will be solved correctly the higher the ranking. The amount of payment for participants A is linked to the ranking: Ranks 1-6 receive €15 cash each and ranks 7-12 receive €5 cash each. The payment of the task will take place

Decision-making

[Translation: Entscheiden Sie nun in der Rolle von Person B ab welcher Zeile Sie annehmen würden = Decide now in the role of person B from which row on you would accept the distribution; Annahme: Auszahlungen Person A/B = acceptance: payoff person A/B; Ablehnung: Auszahlung Person A/B = rejection: payoff person A/B; Annahme ab der nächsten Zeile = acceptance from next row on; immer annehmen = always accept; immer ablehnen = always reject; Beider Personen erhalten = both persons receive]

right after completion. All 24 participants will be handed in an envelope. If you are a participant A, the envelope will contain money.

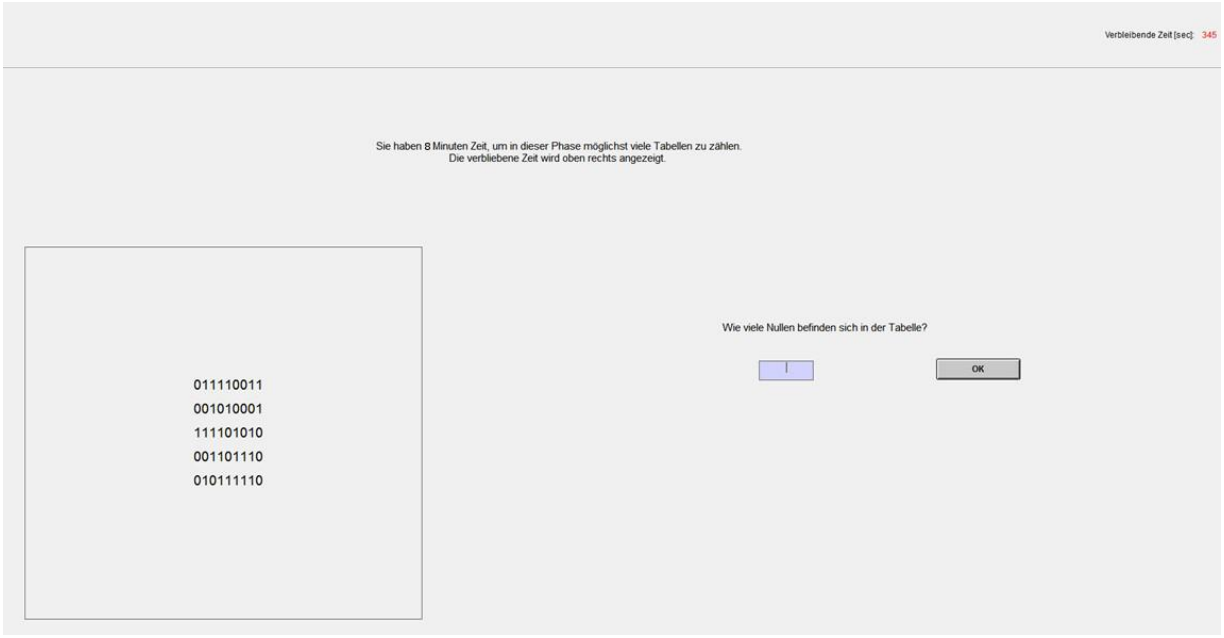
- You receive €15 if your performance is ranked 1-6. Furthermore you get a card with a note that you are a participant A.
- You receive €5 if your performance is ranked 7-12. Furthermore you get a card with a note that you are a participant A.

If the computer randomly draws you as a participant B your envelope will not contain money, but a note that you are participant B.

[Treatment: Competition]

In this phase you will participate in a task. Your display will show matrices with the numbers 0 and 1. Below you can see an example:

(Translation: Sie haben 8 Minuten Zeit, um in dieser Phase möglichst viele Tabellen zu



[Translation: Sie haben 8 Minuten Zeit, um in dieser Phase möglichst viele Tabellen zu zählen. Die verbleibende Zeit wird oben rechts angezeigt. = You have 8 minutes in this phase to count matrices. The time that is left, is displayed on the top left of the screen.; Wie viele Nullen befinden sich in der Tabelle? = How many zeros are in the matrice?]

zählen. Die verbleibende Zeit wird oben rechts angezeigt. = You have 8 minutes in this phase to count matrices. The time that is left, is displayed on the top left of the screen.; Wie viele Nullen befinden sich in der Tabelle? = How many zeros are in the matrice?) Your task is to count the zeros in each of those matrices. Please enter your result in the box below "How many zeros are in the matrix?". Afterwards a dialogue box saying "Please start now with the next matrix" will be displayed. Press "ok". A new matrix will pop up. Your old result will still be in the result box. Please, simply delete it. You will have 8 minutes for this task.

Payoff of the task: After finishing the task the computer will calculate the ranking of all participants. The more tasks are solved correctly the higher the ranking. The participants on the first 12 ranks will be called participants and rewarded according to their ranking. The other 12 participants are called participants B.
The amount of payment for participants A is linked to the ranking: Ranks 1-6 receive €15 cash each and ranks 7-12 receive €5 cash each. The payment of the task will take place right after finishing. All 24 participants will get an envelope:
If you are a participant A, the envelope will contain money.

- You receive €15 if your performance is ranked 1-6. Furthermore you get a card with a note that you are a participant A.

- You receive €5 if your performance is ranked 7-12. Furthermore you get a card with a note that you are a participant A.

If the computer randomly draws you as a participant B your envelope will not contain money, but a note that you are participant B.

[Treatment: Random] In this phase you will participate in a task. Your display will show matrices with the numbers 0 and 1. Below you can see an example: [Picture comes here in original instructions. As examples were similar across treatments and to economize see for the example in treatment Baseline above]
Your task is to count the zeros in each of those matrices. Please enter your result in the box below "How many zeros are in the matrix?". Afterwards a dialogue box saying "Please start now with the next matri" will be displayed. Press "ok". A new matrix will pop up. Your old result will still be in the result box. Please, simply delete it. You will have **8 minutes** for this task.
Payoff of the task: After finishing the task the computer will randomly select 12 participants (of 24). Those will be called Participants A below and rewarded *independently* of the number of tasks solved correctly. The other 12 participants are called Participants B and do not get a reward for this task.
To get the payoff for the participants A the computer creates a random ranking, which is *independent* of the number of tasks solved correctly. The amount of payment for participants A is linked to the ranking: Ranks 1-6 receive €15 cash each and ranks 7-12 receive €5 cash each. The payment of the task will take place right after finishing. Each of the 24 participants will get an envelope: If the computer selected you to be one of the participants A, the envelope will contain money.

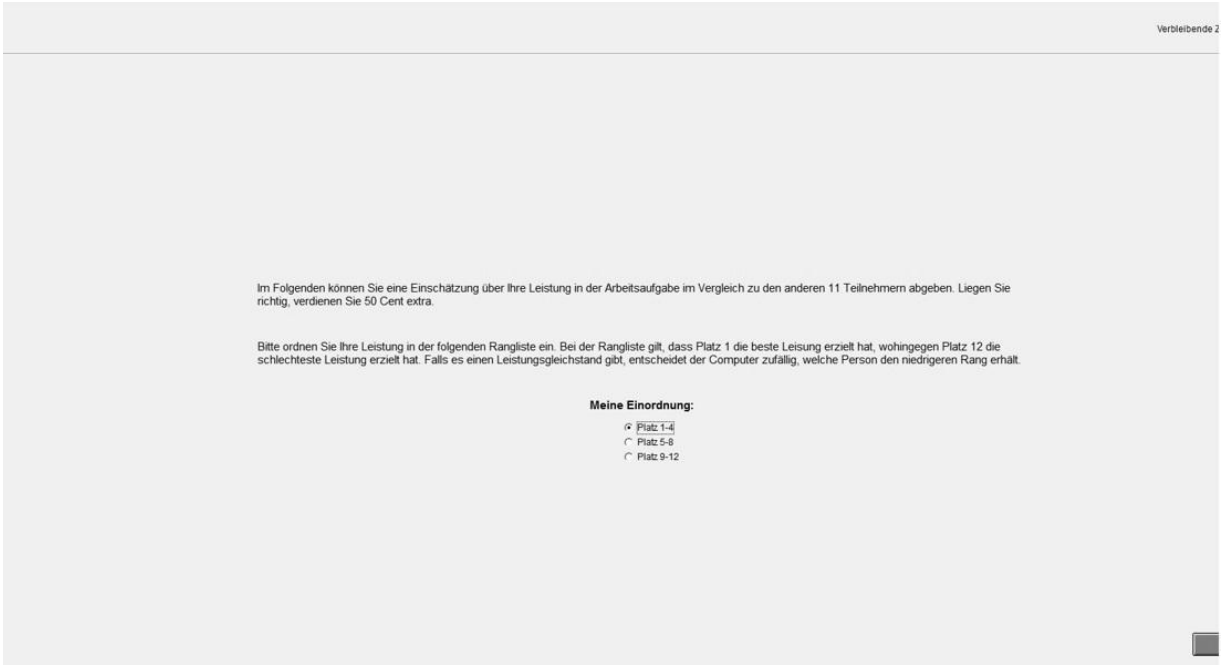
- You receive €15 if your randomly assigned performance is ranked 1-6. Furthermore you get a card with a note that you are a participant A.
- You receive €5 if your randomly assigned performance is ranked 7-12. Furthermore you get a card with a note that you are a participant A.

If the computer randomly draws you as a participant B your envelope will not contain money, but a note that you are participant B.

..8 minutes pass by...

Do you think, you will become a participant A? Yes:_, No:_

[Oral information that envelopes will be distributed]



[Translation: In the following, you can give an assumption about your own performance compared to the other 11 participants. If you hit it, you receive €0.50 in addition.]

Phase 3 [Joy of destruction game]

Open your envelope now. Empty the envelope and put it aside. You learn now, if you have been assigned to group A or group B. Information regarding the assigned group, can be found on a laminated card, enclosed in the envelope. If you are a participant A, your ranking can be found on the card's back. Additionally, participant As will find their payment enclosed in the envelope. If you have screened the content of your envelope, press "ok" and we continue to the next phase.

- In phase 3, all participants A and B receive 6 vouchers for €1 for the canteen.
- The computer randomly assigns a participant A to a participant B. Complete anonymity is ensured during the experiment as well as after the experiment.
- Both participants decide at the same time, how many vouchers they want to remove from the other participant. Removed vouchers are invalid and will not be received by the participant who removed them. The following amounts of vouchers can be removed: 0, 1, 2, 3, 4, 5, 6.
- Participants B will be informed who much (€5 or €15) the matched participant earned in the counting task.

- Simultaneously the computer decides how many vouchers are removed from the participant assigned to you, as well. Number of vouchers is thereby selected randomly. Following amounts of vouchers can be removed: 0, 1, 2, 3, 4, 5, 6. They all have the same probability to be selected.
- After you have confirmed your decision, with a probability of 50% either your decision or the computer's decision will become relevant.
- At the end of the experiment, will only learn about the remaining amount of vouchers that they will receive. Thereby, we do not let you know, if the vouchers have been removed by the other participant or from the computer.

Please press "OK" when you are done reading.

Phase 4 [Trust game]

We start with phase 4 now. In this phase, you are randomly assigned to another participant. The participants are called Person 1 and Person 2 during this phase. Your decisions will be made in Token. Thereby, the following exchange rate applies: 1 Token = 0.20 Euro

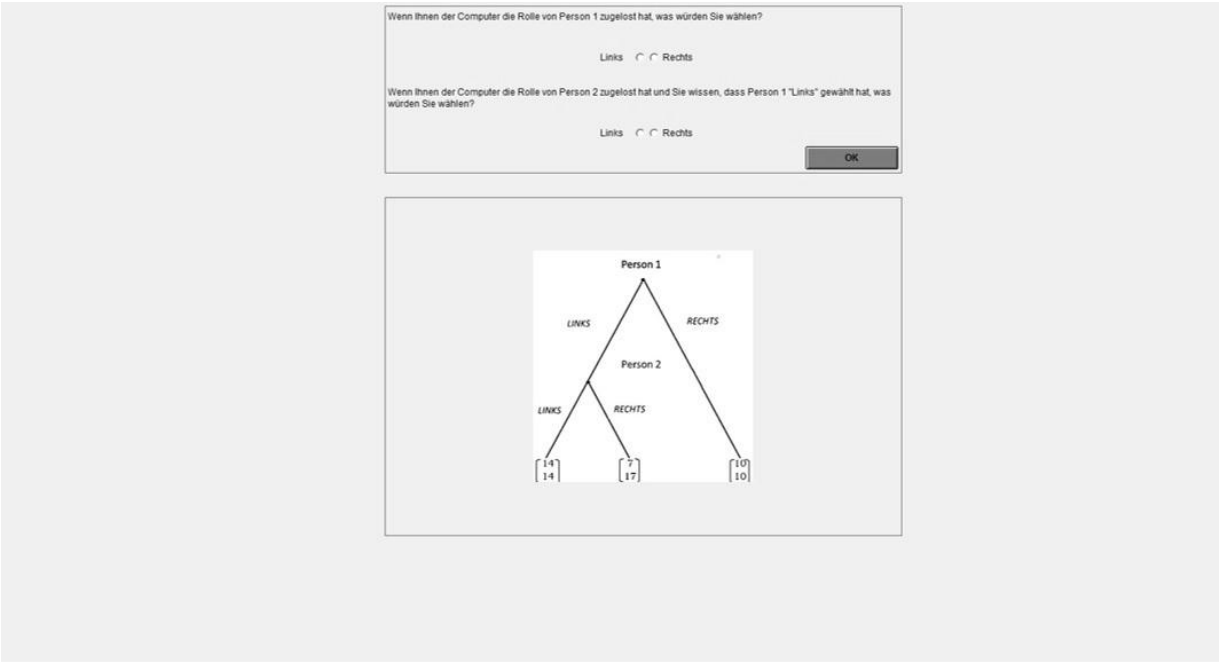
Your decision: In this phase, you have to decide in both roles (Person 1 and Person 2). Deciding as Person 1, you can choose either LINKS or RECHTS (see image below). In case you choose LINKS, Person 2 has to choose between LINKS and RECHTS, too. In case you choose RECHTS, the game directly ends and Person 2 does not have to choose.

Image: The upper number presents the payment in Token for person 1. The lower number presents the payment in Token for person 2.
In case both persons choose LINKS, both persons receive 14 Token.
If person 1 chooses LINKS and person 2 chooses RECHTS, person 1 receives 7 Token and person 2 17 Token.
If person 1 chooses RECHTS, person 2 cannot choose and both persons receive 10 Token.
You will make your decision in the role of person 1 and person 2. In the last case, you have to decide if you choose LINKS or RECHTS, assuming that person 1 chose LINKS.

Payment: At the end of this stage, the computer will randomly match you with another participant from this room. Furthermore, the computer randomly assigns the two roles to the two persons in the dyad. You will only learn your role at the end but not with whom you were matched. After the matching and assignment of roles, the respective decisions of the two participants will be implemented. Hence, your payment might depend on the decision made by the participant that is matched with you, too.

Please show up if you have any questions. We will turn to you in private to answer any questions. After all participants finished reading, and in case there are no further questions, we will start with phase 4.

Decision-making



[Translation: If the computer assigned the role of person 1 to you, would you choose LEFT or RIGHT? If the computer assigned the role of person 2 to you and you know that person 1 had chosen LEFT, would you choose LEFT or RIGHT?]

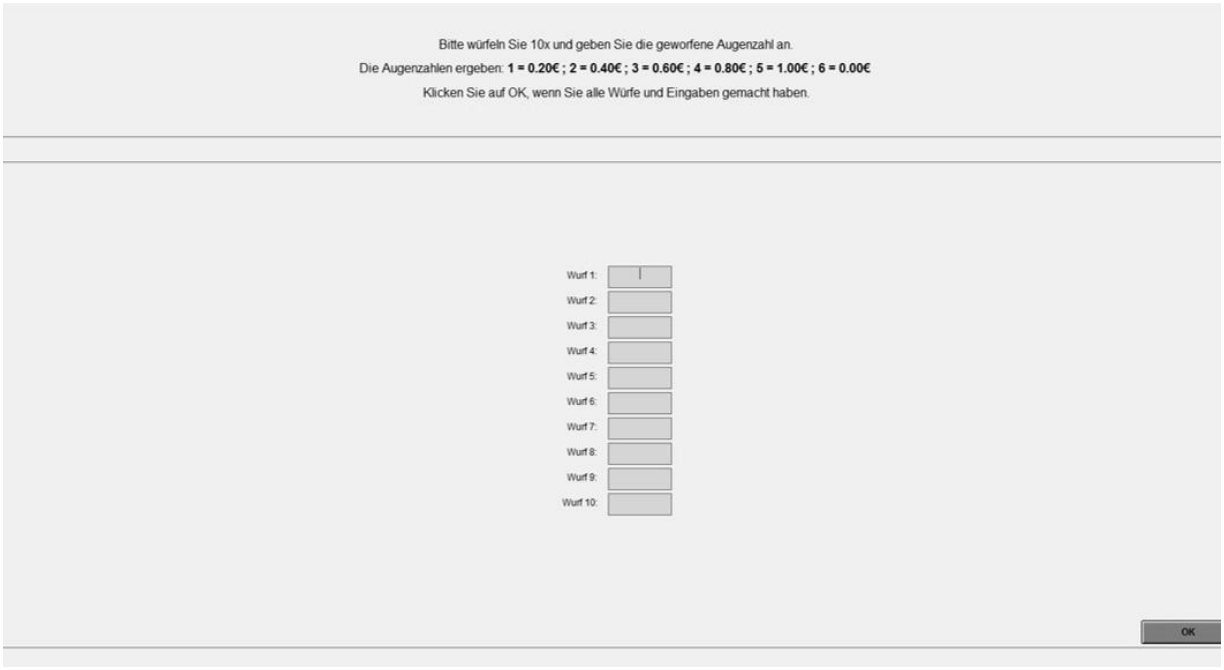
Before the questionnaire [Cheating Game]

In the following, we ask you to fill out a questionnaire. You will get an additional payment for this task.

- The payment will be determined by die rolls.
- Please roll the die 10 times and type in the number on the die (and input screen will appear on the next screen)
- At the end of the experiment, one of the die rolls will be randomly selected. This number will determine your additional payment for filling out the questionnaire.
- The numbers on the die will lead to the following payments: 1= €0.20, 2= €0.40, 3= €0.60, 4= €0.80, 5= €1.00, 6= €0.00

Please press "ok" when you finished reading. We will hand out die and cups now.

Decision-making



[Translation: Please roll the die 10 times and report the rolled number on the die. You earn for a rolled 1 = €0,20, ... Click "ok" if you will have made all entries.]

[Translation: Please roll the die 10 times and report the rolled number on the die. You earn for a rolled 1 = €0,20, ... Click "ok" if you will have made all entries.]

Appendix C

Appendix of Chapter 4

C.1 Tables

	rep. p. level OLS (1)	*rep. p. level* SEM (2)	*SVO angle* SEM (3)	*rep. p. level* SEM (4)	*SVO angle* SEM (5)
female	-0.188**	-0.125	3.334**	-0.144	3.468***
	(0.094)	(0.093)	(1.603)	(0.092)	(1.596)
SVO angle		-0.013***		-0.013***	
		(0.004)		(0.003)	
risk tolerance		0.017	-0.319		
		(0.022)	(0.391)		
age		0.013	0.231		
		(0.010)	(0.175)		
econ student		0.033	-2.912		
		(0.122)	(2.122)		
constant	3.290***	3.169***	21.188***	3.169***	24.634***
	(0.068)	(0.293)	(4.949)	(0.296)	(0.108)
var (rep. p. level)		0.549		0.554	
		(0.047)		(0.048)	
obs.	267	267	267	267	267
R^2	0.015	–	–	–	–
Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1					

TABLE C.1: Mediation analysis with Structural Estimation Models (SEM).
We use the following abbreviations: *rep. p. level* (= reported profit level), *var (rep. p. level)* (= variance (reported profit level)).

C.2 Experimental instructions

Information for the reader of the experiment's instructions:

- This experiment was conducted as part of a larger experiment which has been presented in Chapter 3. Therefore, experiment's instructions can be found in Appendix B.2. Only data from the Social Value Orientation (Phase 1a) and the cheating game, which can be found at the very end of the experimental instructions in Appendix B.2, were used for this paper.

Appendix D

Appendix of Chapter 5

D.1 Tables

TABLE D.1: Observations in contracting stages and treatments

	under-/correct estimator		overestimator		Total	
Contracting						
offers (seller)	72	(31.58%)	156	(68.42%)	228	(100%)
acceptances (buyer)	32	(32.80%)	125	(67.20%)	186	(100%)
fulfillable contracts	60	(41.96%)	83	(58.04%)	143	(100%)
Treatments						
control	53	(43.80%)	68	(56.20%)	121	(50.63%)
anchor	23	(19.49%)	95	(80.51%)	118	(49.37%)
total sellers	76	(31.80%)	163	(68.20%)	239	(100%)

TABLE D.2: Observation numbers in treatments and conditions (only fulfillable contracts)

	control		anchor		Total	
shock	68	(43.80%)	64	(54.24%)	132	(55.23%)
no shock	53	(56.20%)	54	(45.76%)	107	(44.77%)
total	121	(100%)	118	(100%)	239	(100%)

TABLE D.3: 1st stage results for Table 5.3

	Overestimation	
	(2)	(4)
Anchor	2.150***	2.286***
	(0.726)	(0.732)
Shock	0.472	0.573
	(0.740)	(0.732)
Performance	-0.110	-0.170*
	(0.093)	(0.100)
Constant	1.473	3.663
	(1.391)	(6.215)
Observations	132	132
R^2	0.074	0.150
Covariates	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
None of the covariates included in Model (4) are significant expect risk seeking is positively correlated with overestimation ($p < 0.10$)

D.2 Experimental design

Individual overestimation and preferences

Baseline overestimation

Participants exert a similar designed real-effort task as in step 1 of the investment game described in Section 5.3.1. Puzzles are not exactly the same but of a similar style. For each correctly given answer participants earn GHS 0.20. Then, participants assessed their performance in the real-effort task and receive GHS 5 if their guess was exactly correct.

Social value orientation: We apply the method of Murphy et al. (2011) and the reduced set of six decision sets to measure Social Value Orientation (henceforth SVO). Participants are matched in dyads and decide for their most preferred money distribution out of a set of nine. The original amounts from Murphy et al. (2011) were kept for decision-making and we introduce an exchange rate of 1 unit = GHS 0.03. At the end, one decision set is selected for payment and one player is randomly assigned the role of the active decision-maker. An angle, the SVO angle, can be calculated from the decisions made. An increasing SVO angle expresses peoples' rising magnitude of concern for another person's payoff.

Inequality aversion (alpha): We apply a simplified version of the method of Blanco et al. (2011) to measure envy. Participants are matched in dyads and are first in the role of a proposer and then in the role of a responder . The proposer is endowed with GHS 8 and makes an offer of GHS x to the responder, keeping GHS 8 - GHS x to herself. Then, the responder can accept or reject the offer. In the case of rejection both players earn zero. The parameter $alpha$ captures the level of envy and is estimated by the minimum amount a responder is willing to accept in an ultimatum game. After decision-making, the two roles in the dyad are randomly assigned.

Risk preferences: The method from Eckel and Grossman (2002) serves as a simple way to measure risk preferences. Here, participants are presented six different gambles and are asked to choose their preferred one. Each of the gambles involve a 50% chance of receiving a low payoff and a 50% chance of receiving a high payoff. There is one non-risky gamble with a certain payoff of GHS 2.5 in both states. For the subsequent 4 lotteries, the expected payoff increases with the risk involved in the lotteries, measured by the standard deviation. The last lottery increases only in riskiness but not in expected payoff.

Loss aversion: Loss aversion is measured by a method following Gächter et al. (2007). Participants face seven different gambles with a good state and a bad state; each coming into effect with a 50% chance.[1] The good outcome is always GHS 9 whereas the bad outcome increases from a loss of GHS 1.5 to a loss of GHS 10.5 in gamble 7. For each gamble, participants have to decide if they want to take the risk and play the lottery or not. Only one lottery is randomly chosen for payment. Rejection of a lottery gives a payoff of GHS 0.[2] One lottery was randomly chosen for payment.

We apply stranger matching when measuring SVO and inequality aversion. As well as that, these games are played as strategy method. The preferences for risks and losses are individually played. Information about decisions made and payoffs earned are only given at the very end of the experimental session.

D.3 Experimental Instructions

[1] Note the original game consists of only 6 gambles. We add a first gamble with a potential loss of GHS 1.5 to increase variance in choices.

[2] As the gambles become riskier with each gamble, it is irrational to switch once accepting a gamble to rejecting a riskier gamble. Therefore, we restricted choice-making and participants could only switch one time from acceptance to rejection.

Information for the reader of the experiment's instructions:

- The study was conducted with tablets. To make their decisions, participants just touched the respective alternative/choice. In some cases, e.g., the calculation tables the computer filled out automatically some spaces based on precedent entries by the participant, which are marked by "...". Blank spaces where subjects could enter a number are indicated by these rectangles ☐
- To ease readability, sometimes words or explanations are added. This is marked by square brackets.
- For each different game, we asked control questions to deepen the understanding of the games. Since experimental instructions become very long, I decided not to include control questions in the thesis. I marked the omission by "[...] Control questions [...]"
- Also, the in total 50 puzzles of the real-effort task are left out for economy of space in this dissertation.

Welcome to today's session!

We would like to welcome you to today's session! We start when all participants are in the room. If all participants are seated, you will get a little introduction and further information on today's session.

Two short questions before we start...
1. Generally speaking, would you say that most people can be trusted or that your need to be very careful in dealing with people? Please answer on a scale from 1 to 10 whereas 1 means "need to be very careful" and 10 means "most people can be trusted". Please choose one number between 1 and 10. You may grade your answers with values in between.
2. Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair? Please answer on a scale from 1 to 10 whereas 1 means "people would try to take advantage" and 10 means "people would try to be fair". Please choose one number between 1 and 10. You may grade your answer with values in between.

Section 1

Section 1 contains 4 subsections: 1A, 1B, 1C and 1D. We start with subsection 1A.

Section 1A: [Social Value Orientation]

In this section, you will be paired with one other participant in the room. You will not learn who this other participant is and the participant you are matched with will not learn who you are. There are two roles in this activity: role A (in the following person A) and role B (in the following person B). Person A has to actively decide how to allocate coins between person B and himself/herself. Person B has to accept the preferred allocation by person A. All persons will actively decide in the role of person A - however, for which role (A or B) you will be paid will be randomly chosen by the computer. On the following page, we will explain the activity in further detail.

You will be presented a series of decision like this one:

Each of the middle lines determines how much person A and person B receives. Looking at the first middle line, person A (that is you if you are selected into this role) receives 50 coins and person B (that is the person you are matched with) receives 100 coins. As we move to the right, the payment for the person in role A increases to 54, 59 up to 85. The payment of participant B decreases from 100 to 15 at the same time. The task of the person in role A is to decide on the preferred allocation by choosing one of nine money allocations. The chosen distribution will determine the payments of the person in role A and the person in role B. Only one distribution can be marked in each question. On the next slide, we will give you an example of how such a decision can look like.

A person in role A decided for the coin-allocation you see here:

A participant in role A decided for allocation 5. If this decision set is chosen for payment, person A would receive 68 coins. The person he/she is matched with, would receive 58 coins. On the next page, we show you a second example.

A person in role A decided for the coin-allocation you see here:

A participant in role A decided for allocation 3. If this decision set is chosen for payment, person A would receive 89 coins. The person he/she is matched with, would receive 24 coins. On the next page, we will explain how the payoff will be calculated.

Payment: You will make decisions for six different decision sets. You make your decisions with "coins" whereas 1 coin = GHC 0.03. The computer will randomly decide for which role you are paid. The computer will randomly pick one of the decision sets for payment. You will only learn which decision set was drawn, for which role you are paid and how much you earned at the very end of today's workshop. We have finished the instructions for this subsection. Now, we move on to the control questions.

[...] Control questions [...]

You finished section 1A. We will now start with section 1B. We wait until all other participants have finished this subsection.

Section 1B: Inequality Aversion

We start now with the instructions for subsection 1B. In 1B, the computer will randomly pair you with one other participant in the room. You will not learn who this other participant is and the participant you are matched with will not learn who you are. In this section, the situation is as follows: Person 1 is matched with person 2. Person 1 will receive GHC 8 and can decide how much of the GHC 8, he/she wants to pass on to person 2. Person 2 knows that person 1 has been called to make this decision, and may either accept the distribution chosen by person 1, or reject it. In case that person 2 accepts person 1's proposed distribution, that distribution will be implemented. If person 2 rejects the offer, both receive nothing. You will have to make decisions as if you were person 1 and also as if you were person 2. In the latter case, you will have to decide when you would accept or reject the distribution by person 1.

Payment: The computer will randomly chose if you will be paid for the role of person 1 or the role of person 2. If the computer picks you to take up the role of person 1, there are two cases:

1) Person 2 accepts your offer. Then your decision will be implemented: You earn GHC 8 minus the amount you pass on. (And Person 2 receives the amount you pass on).

2) Person 2 does not accept your offer. Then your decision will not be implemented. You and Person 2 both earn GHC 0.

If the computer picks you to take up the role of person 2, you will earn the payoff that person 1 (the participant in the role of Person 1 you are paired with) chose for person 2 but only if you had accepted that particular offer. Otherwise, you both earn nothing. You will only learn at the very end of today's session how much you earned in this activity. We will give you an example on decisions of a person 1 and a person 2 and the resulting payoffs on the following pages.

Person 1 is asked: How much of the GHC 8 would you pass on to person 2? It could be any amount between 0 and 8 but only full amounts (0, 1, 2, 3, 4, 5, 6, 7 or 8). Decision of person 1: GHC 3

Person 2 is asked at the same time: Person 1 received GHC 8. What is the minimum amount Person 2 has to offer you that you accepted the offer? Decision of person 2: GHC 2

Payment: Person 1 would pass on GHC 3. Person 2 only claims GHC 2 to accept the offer. Therefore this distribution will be implemented. Person 1 earns GHC 5 (GHC 8 – GHC 3 = GHC 5). Person 2 earns GHC 3. We will give you one other example on the

next page.

Person 1 is asked: How much of the GHC 8 would you pass on to person 2? It could be any amount between 0 and 8 but only full amounts (0, 1, 2, 3, 4, 5, 6, 7 or 8). Decision of person 1: GHC 5
Person 2 is asked at the same time: Person 1 received GHC 8. What is the minimum amount Person 2 has to offer you that you accepted the offer? Decision of person 2: GHC 7
Payment: Person 1 would pass on GHC 5. However, person 2 claims GHC 7 to accept the offer. Therefore this distribution will not be implemented. Person 1 earns GHC 0. Person 2 earns GHC 0.

[...] Control questions [...]

You finished reading the instructions and answered the control questions. You are now ready to make your final decisions. You will first make your decision in the role of person 1. Afterwards, you will make your decision in the role of person 2.

You are now making your decision for the role of person 1: How much of the GHC 8 would you pass on to person 2? It could be any amount between 0 and 8 but only full amounts (0, 1, 2, 3, 4, 5, 6, 7 or 8).
Your decision GHC: ☐

You now make your decision for the role of person 2: What is the minimum amount Person 2 has to offer you that you accepted the offer?
Your decision GHC: ☐

You finished section 1B. We will now start with section 1C. We wait until all other participants have finished this subsection.

Section 1C: Risk Preferences

In this subsection, we will offer you six different lotteries. These lotteries can have two outcomes: a high outcome (state A) or a low outcome (state B). It is randomly decided by a coin flip by the computer, which state will be realized. (Heads and tails are equally likely). You have to select one of the six lotteries. That should be the one that you prefer most. On the next page, we give you an example how the lotteries will look like.

Example I: In the table you see six different lotteries. Each row defines one lottery. There are two states A and B. If a participant decides for the first lottery, then he/she receives GHC 2.5 for sure because the payoff is GHC 2.5 for heads and tails.
Example II: If a participant decides for lottery 4, the coin flip will be relevant for the outcome: If the coin shows heads, the participant earns GHC 1.4. If the coin shows tails, the participant earns GHC 4.6.

Lottery	State A (coin shows heads)	State B (coin shows tails)	Select one Lottery
1	GHC 2.5	GHC 2.5	o
2	GHC 2.1	GHC 3.2	o
3	GHC 1.8	GHC 3.9	o
4	GHC 1.4	GHC 4.6	o
5	GHC 1.1	GHC 5.4	o
6	GHC 0.2	GHC 6.3	o

Payment: In this activity, you will only make one decision. The computer will randomly pick which state (A or B) will be realized in the lottery you picked. You will be paid for this decision for sure. You will only learn how much you earned at the very end of todays workshop. We have now finished the instructions for this subsection and will now check for your understanding with three control questions.

[...] Control questions [...]

As announced, you have six lotteries to choose from! Please choose now the lottery that you would like to play most of all six lotteries: ☐

You finished section 1C. We will now start with section 1D. We wait until all other participants have finished this subsection.

Section 1D: Loss Aversion

In the following activity, you can again decide on if to play a lottery. However, this time you can choose more than one lottery that you would like to play. If you accept the lottery, you can either win or lose money. Whether you win or lose is decided randomly by a coin flip. If you reject the lottery, you earn 0. Please press next to see how such a lottery would look like.

Lottery	Accept	Reject
If the coin turns up heads, then you lose GHC 2; if the coin turns up tails, you win GHC 6	○	○

Above you see an example of a lottery. Left is described what you can lose if the coin turns up heads and what you can gain if the coin turns up tails. Right you can

decide for acceptance or rejection of the lottery. In the example above, the participant has to decide if he/she wants to accept the lottery of winning GHC 6 if the coin turns up tails and lose GHC 2 if the coin turns up heads. If the participant decides to accept the lottery, the computer will toss a coin. If tails is up, the participant wins GHC 6. However, if the computer coin turns up heads, the earnings from the participant are reduced by GHC 2. If the participant decides to reject the lottery, the participant will earn 0.

Payment: You will make decisions for six different lotteries. The computer will randomly pick one of the lotteries for payment. If you accept a lottery, there will be a random "coin toss" by the computer. This coin toss is decisive for your earnings - if you gain something or lose something - in the lottery. You will only learn which lottery was drawn and how much you have earned or lost at the very end of today's workshop. If you lose money, it will be deduced from your total payoff in today's session. Of course, you will never end up with less than GHC 20 (your participation fee). Please note that the lotteries will be ordered in the final decision. The amount that can be lost will increase while the amount that can be won is the same in all lotteries. Therefore, you can only switch once. We have finished the instructions for this subsection. We now move on to the control questions.

[...] Control questions [...]

Please make your decision for each of the following lotteries now. Please note that the lotteries will be ordered in the final decision. The amount that can be lost will increase while the amount that can be won is the same in all lotteries. Therefore, you can only switch once.

Lottery

1. If the coin turns up heads, then you lose GHC 1.5; if the coin turns up tails, you win GHC 9 **accept** **reject** ☐

2. If the coin turns up heads, then you lose GHC 3; if the coin turns up tails, you win GHC 9 **accept** **reject** ☐

3. If the coin turns up heads, then you lose GHC 4.5; if the coin turns up tails, you win GHC 9 **accept** **reject** ☐

4. If the coin turns up heads, then you lose GHC 6; if the coin turns up tails, you win GHC 9 **accept** **reject** ☐

5. If the coin turns up heads, then you lose GHC 7.5; if the coin turns up tails, you win GHC 9 **accept** **reject** ☐

6. If the coin turns up heads, then you lose GHC 9; if the coin turns up tails, you win GHC 9 **accept** **reject** ☐

7. If the coin turns up heads, then you lose GHC 10.5; if the coin turns up tails, you win

GHC 9 **accept** **reject** ☐

You finished section 1! We will now start with section 2. Please press the next button. Please press Next. We wait until all other participants have finished this subsection. Please be patient.

Section 2

All other participants have finished Section 1. Now we are ready to start with Section 2. Before we start, please answer the following question: Are you male or female?

Section 2 consists of three different parts. The three parts are associated with each other. In Part 1 you are going to work on a production task in which you will solve puzzles. In Part 2 you will assess your own performance and the performance of others. In Part 3 you will perform the task with similar puzzles again. We will explain each part separately in detail one after another.

Section 2 Part 1

We start now with the instructions for Section 2 Part 1. In this part you will work on a production task, that is producing goods by solving puzzles. For each puzzle that will be solved correctly one good will be produced. You will be confronted with 25 puzzles in form of multiple choice questions. For each puzzle you will have 15 seconds time to choose the right answer. After 15 seconds the next question will appear automatically. It will not be possible to go back to previous questions.

As a default always the first answer in every question is logged in automatically. If you do not make any entry the computer will keep answer 1. Please note that it is completely random which number belongs to the correct answer. Not choosing actively the right answer and always leaving the default answer logged in does not increase your chances of having correct answers. On the next page, we give you an example of how such a puzzle could look like.

In this task you will have to choose the symbol that fits into the pattern. Example (1):

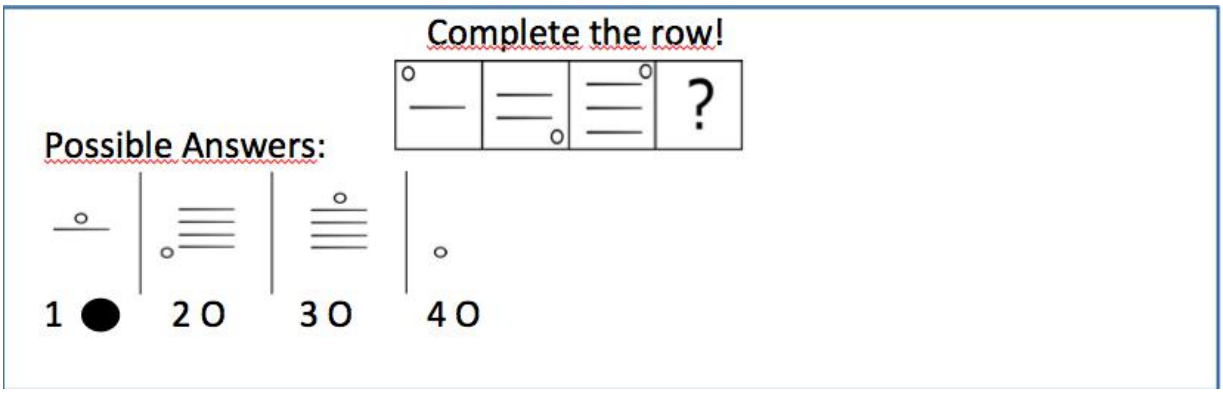
There is only one correct answer for each question. Note that by default the first answer is logged in. Is that the right answer? Think about the right answer and check if you are right on the next page.

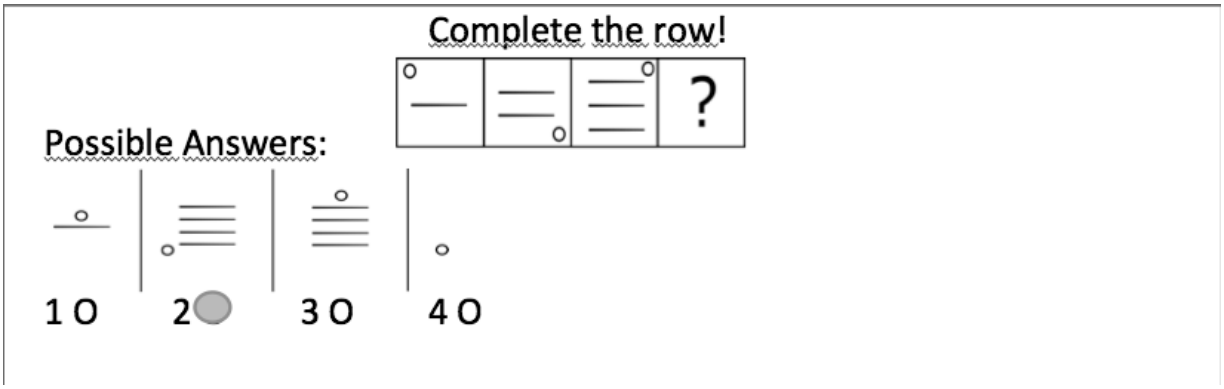
In this task you will have to choose the symbol that fits into the pattern. Example (2):

The correct answer is answer number 2.

You will earn GHC 0.20 for each correct answer. We have finished the instructions for Part 1. Now, we move on to the control questions.

[...] Control questions [...]





You finished the control questions! Now you are ready to produce goods!

Section 2 Part 1: Task 1

Complete the row!

[...] 25 Tasks [...]

You finished Section 2 Part 1! We will now start with Part 2.

Section 2 Part 2

We start now with the instructions for Part 2. In this part we will ask you questions about how you evaluate your own performance and also the performance of other people in the production task. Payment: For each correct guess you can earn 5 Ghana Cedi. You will be informed at the end of the session about how much you earned in this part.

Estimate how many goods you have produced in the production task in Part 1! Recall that you worked on 25 tasks in total. If your estimation is correct you will earn GHC 5. What do you think, how many goods did you produce in Part 1?

I think I produced ☐ **goods.**

Please give us your estimation on how many goods have been produced by men on average.cIf your estimation is correct you will earn GHC 5. What do you think, how many goods have been produced on average by other men in the room (excluding your

own performance).

I think other men produced on average ____ goods.

Now we would like you to estimate how many goods have been produced by other participants in the room. Please give us your estimation on how many goods have been produced by the other female participants on average. If your estimation is correct you will earn GHC 5. What do you think, how many goods have been produced on average by other women in the room (excluding your own performance).

I think other women produced on average ____ goods.

We have finished Part 2 of Section 2.

Section 2 Part 3

We start now with the instructions of Part 3. Instructions for this part are a little bit longer than for the other activities. We explain the procedure therefore first only in words. Afterwards you will see illustrated instructions again. So do not worry if you do not get everything the first time. In this section, you will be paired with one other participant in the room. You will not learn who this other participant is and the participant you are matched with will not learn who you are.

The two persons who are matched with each other will be assigned different roles. One person will be selected to be a buyer and the other person will be selected to be a seller. The buyer receives an endowment of GHC 4. It will be random whether you will have the role of the buyer or the role of the seller.

The buyer and the seller will be able to earn money by trading with each other.The seller produces low value goods. Buyer and seller can decide to virtually sign a contract under which the buyer makes an investment that increases the value of the seller's goods and the seller delivers high value goods to the buyer. The steps of trading are explained in detail on the following pages:

Step 1: Production of goods (1)

The seller gets the opportunity to produce goods by answering 25 questions that are very similar to the ones in Part 1. For every correct answer a low value good will be generated. That means that if all 25 questions will be answered correctly, a maximum of 25 goods can be produced by the seller. The value of one low value good produced is GHC 0.20.

Step 1: Production of goods (2)

It is possible that a shock occurs and affects the production of the seller. If the shock occurs, the production of the seller can be affected positively or negatively. That means

that some more goods are generated or some less goods are generated by chance, independently from the performance of the seller.

Step 2: Decision of Seller
The seller can decide to offer a contract to the buyer on selling a certain quantity of goods to the buyer or choose to sell the goods to an alternative market for 0.20 GHC. The seller does not know yet the number of goods that he or she produced but will be asked to give an estimation about his production similar as in Part 2. If the seller does not offer a contract then the buyer keeps his endowment of GHC 4. If the seller decides to offer a contract she/he will choose the number of goods that she/he is willing to sell to the buyer. Only when the seller decides to offer a contract to the buyer, we continue with step 3.

Step 3: Decision of Buyer
The buyer can decide whether to accept or to reject the contract. If the buyer accepts the contract the endowment of GHC 4 will be invested in the seller's production. The investment of the buyer increases the value of the seller's goods. The goods are transformed by the buyer's investment to high value goods. If the buyer rejects the offer, the buyer keeps his endowment and the seller sells each good to the alternative market for 0.20 GHC.

Step 4: Selling goods (1)
After the contract has been made, the seller and the buyer will be informed about the amount of goods produced. Seller and Buyer also get the information on whether or not a random shock could have affected the production. In case there was a shock neither the seller nor the buyer will be informed about whether the shock was positive or negative.

Step 4: Selling goods (2)
The seller has two different opportunities for selling high value goods. High value goods can be sold to the buyer for GHC 1 per good. High value goods can also be sold to the alternative market for GHC 2.5 per good. The seller is supposed to sell the number of goods to the buyer, that has been agreed upon in the contract. The surplus in produced goods are not supposed to be sold to the buyer and can be sold to the alternative market.

Step 4: Selling goods (3)
The contract is not enforceable: The seller has the final decision on how many goods to

sell to the buyer. The buyer earns GHC 1 for each good that the seller sells to him. The buyer does not earn anything for goods that are sold to the alternative market.

Please remember that in Part 1, you all engaged in a similar production task already. Please note that the buyers will be informed about the performance of the sellers during the test questions in Part 1.

We have finished the instructions for Part 3. Now, we move on to the control questions.

[...] Control questions [...]

You finished the control questions. You are now randomly matched with another person and randomly assigned by the computer to either the role of the buyer or the role of the seller.

Seller (before working on re-task)
You have been randomly selected to be a seller. You will stay a seller for the entire second part of this section.

[Re-task (all treatments)]
You can now start to produce goods. Be reminded that the task consists of 25 puzzles and you will have 15 seconds for each puzzle. For every correct answer one low value good with a value of GHC 0.20 will be produced.

[...] 25 Tasks [...]

[Buyer (before working on re-task)]
You have been randomly selected to be a Buyer. You will stay a Buyer for the entire second part of this section.

Before you start to produce goods we would like to inform you about how many goods you produced in Part 1.

Your performance in Part 1 was **... goods .**

The seller is now producing goods. In the meantime you have two different options, while you are waiting. You can
1. Do the task of Part 1 with similar puzzles again for yourself. You will get 1 candy if your performance is above 3 goods. OR
2.Do the task of Part 1 with similar puzzles again in a competition against another randomly selected player. You will compete against the performance of a randomly selected player in Part 1. If you win the competition you will earn 4 candys.
You will learn about how many candies you earned only at the very end of today's

session.

Please decide what you want to do while the seller is producing goods. (The first option is logged in automatically. If you want to switch, please press the button for option 2.)

I want to do the task again for myself.

I want to do the task again and compete against another participant.

[...] 25 Tasks [...]

[Seller in anchor treatment (after re-task)]

Time's up! Do you think you gave more than 20 correct answers?

Yes ☐ **No** ☐

What do you think, how many correct answers did you give? I think my performance was ☐

correct answers.

If your guess is correct you will earn GHC 5. How sure are you that your guess is correct? The more you move the slider to the right the more sure you are. The more you move the slider to the left the less sure you are.

Not Sure - - - - | - - - - Sure

Does the position of the slider represent how sure you are with your estimation?

Yes ☐

If not, please adjust the ruler, so that it fits you.

You can now decide to offer a contract to the buyer or not to offer any contract and sell your goods for GHC 0.20 per piece to the alternative market. If you offer a contract: You will make a decision on the amount of produced goods you want to sell to the buyer for a price of GHC 1. If the buyer accepts the contract by making an investment the value of your goods will increase. According to the contract terms you will be supposed to sell the number of goods to the buyer that you have agreed upon.

You stated that you estimate that you produced ... goods. The buyer will only be informed about the amount of goods that you produced in the trial round in Part 1.Please remember that the value of your goods will only increase if the buyer agrees to make an investment. Therefore, think carefully about your offer.

Calculate Profits

With this tool you can calculate the expected profit from the contract depending on your expected performance and your offer. You can change your entry as often as you want. You can also see your profit and the buyers profit.

My expected performance is ... goods

I offer ☐ goods to the buyer.
Your income from selling to the buyer: GHC ...
Your income from selling to outside option: GHC ...
Your total income: GHC ...
The buyers' total income: GHC ...

Your expected performance is ... goods. **Will you offer the buyer a contract?**
Yes ☐ **No** ☐

Your expected performance is ... goods. **How many goods do you want to offer?**
☐

Waiting for other participant.

Offer accepted
The buyer decided to accept your contract and made the investment. Your goods are now transformed to high value goods.

No Shock Treatment

Your production has **not** been affected by a shock.

You produced **... goods**.

The buyer has been informed about the number of goods that you produced and that your production has not been affected by a random shock. The buyer also knows your performance in Part 1.

Contract Overview
Your expected performance was **... goods .** You produced **... goods.** According to the contract you are supposed to sell **... goods** to the buyer. According to your expected performance and the contract that you signed your expected payoff was **... GHC** . If you sticked to the contract you and the buyer would earn the payoffs written below:
Please note that if your number of produced goods is not enough in order to fulfill the contract this overview assumes that you sell all produced goods to the buyer and nothing to the alternative market.
Your income from selling to the buyer: GHC ...
Your income from selling to outside option: GHC ...
Your total income: GHC ...
The buyers' total income: GHC ...

Your expected performance was **... goods .** You produced **... goods.** According to the contract you are supposed to sell **... goods** to the buyer. According to your expected

performance and the contract that you signed your expected payoff was **... GHC**.
Please make now your final decision on how much you want to sell to the buyer. You can see your profit and the buyer's profit depending on your decision. You can try out scenarios in the upper part of the page. You make your final decision at the bottom of the page.
Please note that you can't sell more than the maximum amount of produced goods.
I sell ☐ goods to the buyer.
Your income from selling to the buyer: GHC ...
Your income from selling to outside option: GHC ...
Your total income: GHC ...
The buyers' total income: GHC ...
How much do you want to sell to the buyer?
☐

You sold **... goods** to the buyer. Your income from the contract is GHC...

Shock Treatment

Your production has been affected by a shock.

The shock could have been positve or negative. That means that the number of goods you produced was either increased by two or decreased by two. You're final output is **... goods.**

The buyer has been informed about the number of goods that you produced and that your production has been affected by a random shock. The buyer also knows your performance in Part 1.

Contract Overview
Your expected performance was **... goods .** You produced **... goods.** According to the contract you are supposed to sell **... goods** to the buyer. According to your expected performance and the contract that you signed your expected payoff was **... GHC** . If you sticked to the contract you and the buyer would earn the payoffs written below:
Please note that if your number of produced goods is not enough in order to fulfill the contract this overview assumes that you sell all produced goods to the buyer and nothing to the alternative market.
Your income from selling to the buyer: GHC ...
Your income from selling to outside option: GHC ...
Your total income: GHC ...
The buyers' total income: GHC ...

Your expected performance was **... goods .** You produced **... goods.** According to the

contract you are supposed to sell **... goods** to the buyer. According to your expected performance and the contract that you signed your expected payoff was **... GHC**.
Please make now your final decision on how much you want to sell to the buyer. You can see your profit and the buyer's profit depending on your decision. You can try out scenarios in the upper part of the page. You make your final decision on the bottom of the page.
Please note that you can't sell more than the maximum amount of produced goods.
I sell ☐ goods to the buyer.
Your income from selling to the buyer: GHC ...
Your income from selling to outside option: GHC ...
Your total income: GHC ...
The buyers' total income: GHC ...
How much do you want to sell to the buyer?
...
You sold **... goods** to the buyer. Your income from the contract is GHC...
You finished section 2! Before you proceed to the questionnaire, we want to give you an extra payment for answering the questionnaire. For that, we do a short activity with dice. There will be two questions and one of these questions will be randomly chosen for payment at the end of today's session. We will now distribute dice and cups.

Buyer (after re-task)
The seller produced in the trial round in Part 1 **... goods.** Now you have been offered **... goods. Do you want to accept the contract?**
Yes ☐ **No** ☐
Waiting for the other participant.

[No Shock Treatment]

The seller's production has not been affected by a shock.

The Seller's output is **... goods.**

[Shock Treatment]

The seller's production has been affected by a shock.

The shock could have been positive or negative. That means that the number of goods the seller produced was either increased by two or decreased by two. The seller's final output is **... goods.**

The seller sold **... goods** to you. Your income from the contract is GHC...