ESSAYS IN EXPERIMENTAL AND BEHAVIORAL ECONOMICS: METHODS AND INTERDISCIPLINARITY

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VORGELEGT VON

MARTIN VOLLMANN

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Contents

Chapter 1

Introduction

Since the 70s, experimental and behavioral economics has exhibited growing attention within the field of economics. This direction is also reflected in a growing list of Noble Prize winners, most prominently Daniel Kahnemann and Vernon Smith and most recently Abhijit Banerjee, Esther Duflo and Michael Kremer. Even if the methodology of experimental economics seems to be nothing new for many natural sciences, its adaption and modification have generated many valuable insights for economics. Therefore, the founding of many competence centers all around the globe, which focus on the application of behavioral insights, does not come as a surprise. [1]

The projects presented in this thesis contribute to a wide range of different behavioral economic issues and, therefore, give an idea of the diversity of this scientific field. Besides answering various research questions, this thesis also aims to provide an impression of the interdisciplinarity and experimental methods that make up this diversity.

The interdisciplinarity is reflected in two artificial field experiments in chapters 4 and 5, which emerged through the collaboration of researchers from psychiatry and clinical psychology. [2] The unique results show which synergy effects can result from the connection between these two fields. However, such a collaboration is often considered an obstacle since different paradigms, methods and statistical approaches lead to the increasing complexity of joint research projects. What these differences are and how they can be overcome is a central element of the discussion in Chapter 7.

Furthermore, the diversity of experimental and behavioral economics is reflected in the methods used concerning the design and execution of experiments. Since these methods reflect the realities of constant technological progress, the multitude of possibilities and the diversity of the approaches in experimental economics can be demonstrated particularly well through them. While, in the beginning, some of today's best-known experiments were carried out in a cumbersome and time-consuming manner using pen and paper (e.g., Güth, Schmittberger, and Schwarze 1982; Hoffman, McCabe, and Smith 1996), computer-aided methods have now almost completely replaced these. Moreover, experiments were spatially linked to laboratories until recently decentralized

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[1] An interactive map of competence centers for behavioral economics can be found at https://insight-austria.ihs.ac.at/weltkarte/.

[2] Harrison and List (2004) have described artificial field experiments as standard laboratory experiments using a non-standard subject pool.

online studies, using platforms such as Amazon Mechanical Turk or Prolific, opened up a new dimension. This recent development from centralized to decentralized experiments is also reflected in this thesis. While the projects in chapters 2, 3, 4 and 5 were executed under laboratory conditions that reflect the current standards, the project in Chapter 6 uses the novel possibilities of online experiments. These new methods for executing experiments try to solve some of the existing problems in conventional laboratory experiments, such as the small number of observations or the lack of demographic diversity. However, with these new methods, old problems are sometimes exchanged for new ones; for example, the remote access of the participants is accompanied by a loss of control for the experimenter. Another problem is the lack of time coordination. While in a conventional laboratory environment it is not a problem to form groups and start an experiment together, this represents a somewhat greater hurdle for online experiments, especially when it comes to interactions between participants. The project in Chapter 6 discusses these concerns and presents potential solutions to overcome these obstacles. Furthermore, the discussion in Chapter 7 tries to raise awareness of the technical challenges that researchers face in the implementation of such studies.

This thesis can be structured into three parts. The first part presents two experimental economics research projects using the "classical" laboratory approach (Chapter 2 and 3). The second part consists of two research projects, which follow an interdisciplinary approach by combining the experimental economic laboratory approach with a non-student patient sample in cooperation with the University Hospital Heidelberg (Chapter 4 and 5). The last part consists of an experimental study, which moves away from the common laboratory setting to a large scale interactive online experiment (Chapter 6). A brief summary of each project will be given in the following paragraph as an overview.

The first research project in Chapter 2 was developed together with Robert J. Schmidt and Christiane Schwieren and published as Schmidt, R., Schwieren, C., & Vollmann, M. (2020). The Value of Verbal Feedback in Allocation Decisions *Journal of Behavioral and Experimental Economics*, 87, 101548. This study was motivated by the question of whether people have a positive willingness-to-pay to avoid or to receive verbal feedback after taking an allocation decision, which affects them and their partner. Since it is often observed that people have preferences for receiving or avoiding information, we wanted to know whether people actually value the reception or respectively the avoidance of verbal-feedback. This study also filled a gap left open by

Langenbach (2016) and Grosskopf and López-Vargas (2014), who studied if receivers of allocation decisions have a positive willingness-to-pay to send feedback. The two main results were, on the one hand, that decision makers that shared their endowment with the recipient equally revealed a positive willingness-to-pay to receive but not to avoid feedback. On the other hand, decision makers who behaved selfishly were willing to pay to receive and avoid feedback. In an exploratory analysis, it could be shown that the main motives behind asking for feedback were curiosity, the desire to receive social approval and giving the recipient the chance to express his/her feelings for feedback acquisition, while shame and fear of negative feedback were the main reasons for avoidance. These findings add to the literature of non-instrumental information and, therefore, challenge standard economic assumptions that information that has no direct payoff relevancy should not be valued in the individual utility function.

The second research project presented in Chapter 3 was co-authored with Stefan Trautmann. The motivation behind this project can best be described by the following scenario. An employee in a company has a history of accomplishments, which she and her supervisor observe; based on their observations, both develop beliefs about the employee's abilities, upon which they have to build their future decisions. The central aspect of this project was, therefore, to investigate how the skill assessment, depending on the individual perspective (Observer or Performer) and the degree of supervision of the Observers (direct and indirect), develops over time and how these beliefs influence performance-related decisions. Two key findings were made. First, if supervision by Observers was direct and exhaustive, they could give a precise prediction of the Performers' actual performance. Since their consecutive decisions mapped to their predictions, this would imply viable decisions by the supervisor if transferred to the work environment. Second, if supervision by Observers was indirect, and they had to rely on the Performer's reports, they could not give a precise prediction of the Performers' actual performance. In this case, Observers systematically reevaluated the received information by assessing lower skill levels when Performers communicated high skill and assessing a higher skill level when Performers communicated low skill. Again, their consecutive decisions mapped to their predictions. Transferred to the work environment, these biased predictions could lead to inferior results in many corporate governance settings, for example, in the context of task allocations and promotions.

The third research project presented in Chapter 4 was co-authored with Haang Jeung, Sabine Herpertz and Christiane Schwieren. It was published as Jeung, H., Vollmann, M., Herpertz, S. C., & Schwieren, C. (2020). Consider others better than yourself: Social decision-making and partner preference in Borderline Personality Disorder. *Journal of Behavior Therapy and Experimental Psychiatry*, 67, 101436. This chapter opens the second part of this thesis using an interdisciplinary approach by working together with physicians from General Psychiatry of the University Hospital Heidelberg. By focusing on a clinical patient sample to address the research question, this project contributes to the growing literature of artificial field experiments. Fundamentally, the integration of a clinical patient sample into experimental economics is very rare but not unique; for example, one of the few studies that used this approach was Kupferberg et al. (2016). The research question of this project was inspired by observations in the clinical practice where patients suffering from Borderline Personality Disorder (BPD) were looking for allies against a third party, even though part of the disorder is a lack of trust in others. The project tried to mimic this by using a non-cooperative three-person coalition formation game with an ultimatum bargaining stage (Okada and Riedl 2005). However, no behavioral differences compared to a healthy control group were found. Overall, patients with BPD showed no differences in fairness perception but demonstrated an alteration in partner preference, indicating a tendency towards unfavorable partner choices. This result is especially interesting because it shows that BPD patients seem to be consciously choosing the wrong partner. This knowledge can, therefore, open new avenues for the treatment of BPD.

The fourth research project presented in Chapter 5 was co-authored with Margarete Mattern, Knut Schnell and Christiane Schwieren and followed a similar interdisciplinary approach. The project focused on the influence of the work context on people suffering from a Major Depressive Disorder (MDD). Since depression is widespread in the work environment, its implications for the economy, businesses and individuals are huge. Therefore, improving the professional circumstances for people will have widespread consequences. This project, therefore, focused on a variable that can, in many cases, be modified—working in a group or individually—. Accordingly, depressed participants were confronted with an artificial work context, where they either worked alone or within a group of healthy participants on a real effort task. Furthermore, the focus lay on two situations to be managed from a company perspective: prevention of dropout from the workforce caused by developing depression and avoiding relapse of reintegrated workers. Thus, two distinct samples i.) individuals at the brink of depression ("Subclinically Depressed") and ii.) with depression under treatment ("Clinically Depressed") were investigated. Contrary to the initial expectations, the results showed that working together in a team caused no negative effects on participants' performance and well-being, which suggests potential positive effects of group work. The results are discussed with a focus on the design of workplaces to both reintegrate clinically depressed employees and prevent subclinically depressed employees from developing major depression.

The fifth and last research project is presented in Chapter 6 and was co-authored with Dietmar Fehr. It is representative of the new methodological developments in experimental economics since it is one of the few large-scale interactive online studies using a sample of the general population of the United States. Besides this innovative character, it also contributes to the growing literature on fairness perception and its consequences on redistributive taxation. The motivation for this project was that people tend to equate success with merit to justify self-interested behavior even when success is obviously due to luck. The experiment was, therefore, designed in such a way that success was effectively determined by the exogenous assignment to the task. The results of the project demonstrated that economically successful participants overweigh the role of effort in their success, perceiving it as more deserved and, as a result, a product of their effort rather than luck. Subsequently, they demand less redistributive taxation.

The last chapter of this thesis, Chapter 7, discusses the subtitle "Methods and Interdisciplinarity" against an implementation-oriented background to raise awareness of practical obstacles and gives some concluding remarks.

Chapter 2

The Value of Verbal Feedback in Allocation Decisions

Abstract. Depending on the context at hand, people's preference for receiving feedback might differ. Especially in allocation decisions that directly concern another individual, feedback from the affected person can have positive or negative value. We study such preferences in a laboratory experiment by eliciting the willingness-to-pay to receive or to avoid verbal feedback from subjects that were previously affected by an allocation decision. We find that most decision makers exhibit a positive willingness-to-pay for having control about whether feedback occurs or not. Specifically, decision makers that shared their endowment with the recipient equally revealed a positive willingness-to-pay for receiving, but not for avoiding feedback. By contrast, among decision makers that behaved selfishly, we identify both: subjects that were willing to pay for receiving and subjects that were willing to pay for avoiding feedback. The stated motivations indicate that curiosity, the desire to receive social approval and giving the recipient the chance to express his/her feelings are the main reasons for feedback acquisition, while shame and fear of negative feedback are the main reasons for avoidance.[3]

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[3] This chapter was co-authored by Robert Schmidt and Christiane Schwieren. Published as Schmidt, R., Schwieren, C., & Vollmann, M. (2020). The Value of Verbal Feedback in Allocation Decisions *Journal of Behavioral and Experimental Economics*, 87, 101548.

2.1 Introduction

It is commonly observed that people have preferences for getting or avoiding feedback in various contexts. For example, people do not want to get to know the true results of a medical test they decided to do (e.g., Lyter et al. 1987). In our study, we investigate, whether such behavior can also be found in allocation decisions, where the salience of a particular decision potentially affects self-image. Therefore, we conducted a simple two-stage experiment to examine how the valuation of verbal feedback depends on a subject´s previous behavior. In the first stage, subjects played a mini-dictator game (MDG) in which the decision maker could choose between a "fair" option and an "unfair" option. In the second stage, the dictator had to decide whether or not he/she prefers to receive feedback from the recipient. We conducted two treatments in order to measure the valuation for reception as well as for avoidance of feedback. In the former, participants had to pay to get feedback whereas in the latter participants had to pay to avoid feedback.

Our study contributes to the more recent literature on preferences for non-instrumental communication and non-instrumental information.[4] For example, it has been shown that individuals tend to acquire costly but non-instrumental information for several reasons. The most common motives are: the satisfaction of curiosity (Loewenstein 1994), the pleasure of knowledge and insight (Karlsson, Loewenstein, and MacCafferty 2004), and reshaping their beliefs in a favorable manner (Karlsson, Loewenstein, and Seppi 2009; Eil and Rao 2011).[5] Likewise, individuals sometimes willfully ignore information about negative consequences of their own actions on others or on the environment (Stoll-Kleemann, Jaeger, and O'Riordan 2001; Norgaard 2006; Dana, Weber, and Kuang 2007; Feiler 2014; Hertwig and Engel 2016).[6]

[4] This literature contrasts the traditional economic view that individuals only care for information if it is instrumental, i.e., if it helps maximizing material payoff. Common examples refer to using information to optimize decision-making (Stigler 1961) or to coordinate actions with others (Crawford and Sobel 1982).

[5] Such results led to the conclusion, that information communication is not only valued for instrumental reasons, but it also directly enters an agent's utility function (Bénabou and Tirole 2006; Golman and Loewenstein 2015; Loewenstein and Molnar 2018). In a recent study, Alós-Ferrer, García-Segarra, and Ritschel (2018) provided evidence that curiosity about one's own performance can trump inequality aversion. Curiosity has also been found to contribute to explaining the endowment effect (van Ven, Zeelenberg, and van Dijk 2005).

[6] Another related strand of literature is about the avoidance of instrumental information. This phenomenon is particularly known from the field of medicine, where patients sometimes choose not to take medical tests or avoid getting to know a test result, even when taking the test is not associated with material cost or when the result would lead to valuable information for future decisions (Lyter et al. 1987; Lerman et al. 1996; Lerman et al. 1999).

Our paper is not the first to examine communication in allocation situations. Xiao and Houser (2007) as well as Ellingsen and Johannesson (2008) found that the expectation of ex-post verbal feedback from the recipient increases generosity in dictator games and concluded that communication influences behavior besides its instrumental content. Similarly, Langenbach (2016) and Bruttel and Stolley (2020) found that pre-play communication increases the dictator´s share allotted to the recipient in subsequent dictator games. The effect of communication on allocation behavior has also been studied in bargaining environments. Xiao and Houser (2005) conducted an ultimatum game that was extended to allow responders to ex-post send a free written message to the proposer in case of acceptance of the allotted share. They found that rejection rates for small offers (20 percent of the pie or less) decreased significantly. The authors concluded that the possibility of displaying disapproval might be a satisfying form of retaliation that substitutes punishment by simply rejecting the offer. As a consequence, economic models started to integrate such preferences by attributing them to concerns of self-image (Bodner and Prelec 2003; Bénabou and Tirole 2006; Grossman and van der Weele 2017; Bénabou, Falk, and Tirole 2018). That is, subjects trade off utility from material gains with potential disutility caused by deterioration of how they evaluate themselves. We contribute to this body of literature by experimentally examining how people manage the acquisition, or respectively avoidance, of information that is likely to affect their self-image.

In particular, we follow Grosskopf and López-Vargas (2014) and Langenbach (2016) with regard to the valuation of verbal feedback. Both examined recipients' preferences for sending messages to an individual who previously affected them in an allocation decision. Grosskopf and López-Vargas (2014) used a power-to-take game and found a positive willingness-to-pay (WTP) for sending messages to the taker afterwards. Langenbach (2016) studied ex-ante and ex-post communication in dictator games and found that recipients exhibit a positive WTP for both types of communication. Based on that, our experiment fills an important gap by studying the opposite direction of those experiments. Specifically, instead of examining the recipients´ preferences to give feedback about the decision makers´ behavior, we shed light on the decision makers´ preferences to receive or to avoid feedback about his/her own behavior.

Following the literature on curiosity and self-image, we hypothesize that dictators who share equally with the responder will only exhibit a WTP for receiving feedback and not for avoiding. On the contrary, for dictators that behave selfishly in the allocation situation, we hypothesize that both acquisition and avoidance of feedback will be observed, as they have to trade off preferences for the satisfaction of curiosity with the desire to avoid the reception of social disapproval.

In accordance with our hypotheses, we find that most decision makers exhibit a positive WTP for having control about receiving or avoiding feedback. Precisely, egalitarian decision makers reveal a positive WTP for receiving, but not for avoiding feedback. By contrast, among selfish decision makers we identify a fraction that pays for getting as well as a fraction that pays for avoiding feedback. Asking subjects about the underlying motives indicates that curiosity, the anticipation of social approval and a preference for giving the recipient the chance to express his/her feelings are the main driving forces for feedback acquisition. By contrast, the fear of receiving social disapproval as well as shame and guilt are the main motivations to avoid receiving feedback from the recipient.

2.2 Experimental Design, Procedures and Hypotheses

2.2.1 Design

At the beginning of the experiment, subjects were randomly assigned to the roles of dictators and recipients and subsequently matched in pairs.[7] The interaction between subjects was performed in two stages. In the first stage, subjects played a MDG in which dictators had to decide how to allocate €10 between themselves and the respective recipient. Each dictator could choose between a "fair" allocation (€5 for the dictator and €5 for the recipient) and an "unfair" allocation (€8 for the dictator and €2 for the recipient).[8] Participants took this decision, knowing that feedback could potentially be distributed. In the second stage of the experiment, the dictator determined whether the recipient was given the possibility to give verbal feedback to him/her or not. Specifically, subjects were informed that feedback would take the form of a verbal message written freely, formulated by the recipient after knowing about the outcome of the MDG, and transmitted subsequently to the dictator. We framed the potential occurrence of feedback

[7] Throughout the whole instructions, the role of the dictators was referred to as "Player A" and the role of the recipients as "Player B".

[8] For the ease of reading this paper, we label subjects choosing the egalitarian option as "fair dictators" and subjects choosing the selfish option as "unfair dictators".

as "a possibility for the recipient to express his/her thoughts and feelings about the dictator's behavior in the previous allocation situation".

We applied two treatments between subjects, *pay to get* and *pay to avoid*, in which we varied the status quo of feedback.[9] In *pay to get*, the default was that dictators would *not receive* a message by the recipient and had to pay to get feedback. Vice versa, in *pay to avoid*, the default was that dictators would *receive* a message and had to pay if they wanted to avoid it. The actual price for getting or avoiding feedback was initially unknown and drawn from a uniform distribution between €0 and €1. To elicit the dictators´ WTP to get, or respectively, to avoid feedback, we used the Becker-DeGroot-Marschak (BDM) method (Becker, DeGroot, and Marschak 1964).[10] For that purpose, all subjects were equipped with an endowment of €1 at the beginning of the experiment. If the stated WTP was at least as high as the actual price, a switch from the status quo was implemented. In this case, the initial endowment was reduced by the price. Otherwise, the status quo remained unchanged and the subject did not incur any costs.

If feedback was enabled, the recipients had three minutes to write a verbal message, which was subsequently displayed on the dictators´ computer screen for another three minutes without the possibility to leave the stage or switch the screen. While recipients entitled to formulate feedback wrote their messages, the remaining recipients, as well as all the dictators, had to copy a small, neutral text. We implemented this for two reasons: First, we did not want to give the recipients the opportunity to express their feelings if the message was not going to be transmitted to the dictator eventually. Second, we wanted to make sure that it is not identifiable who formulates a message by the sound of typing.

To explore emotional changes between stages, we elicited levels of well-being and arousal at three points during the experiment using a 9-point SAM-scale from Bradley and Lang (1994).[11] First, immediately after the experiment had begun, second, after the allocation decisions had been implemented and revealed, and third, after the messages had been written and received. Finally, at the end of the experiment, we asked

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[9] A control treatment where no feedback occurs is thus not necessary, as we are not primarily interested in the allocation behavior per se.

[10] The BDM is an incentive compatible method to measure a subject´s willingness-to-pay.

[11] The SAM-scale is a pictorial, non-verbal measure to elicit different dimensions of emotions. See the appendix for the pictograms we used in the experiment.

the dictators about their motivation for their bidding behavior and simultaneously the recipients stated their beliefs about the underlying motivations of the dictator´s behavior.

2.2.2 Procedures

The experiment was computerized using z-Tree (Fischbacher 2007) and run at the experimental laboratory of Heidelberg University. Participants were recruited using HROOT (Bock, Baetge, and Nicklisch 2014) and we conducted fourteen sessions between June 2016 and November 2017 with a total of 234 subjects. 118 subjects (59 pairs) participated in *pay to get* and 116 (58 pairs) in *pay to avoid*. The average age was 23.26 years. 57% of the subjects were females and 27% of the participants studied in an economic field. A session took about 30 minutes and subjects earned €7.98 on average, including a show-up fee of €2.

2.2.3 Hypotheses

We derived our hypotheses based on the literature on curiosity and on self-image. This literature suggests that subjects exhibit an intrinsic demand to learn about the unknown, *independently* from its content (Laffont 1989; Loewenstein 1994; Golman and Loewenstein 2015). As a result, satisfying curiosity would *always* have a positive effect on the agent´s utility. We thus hypothesize that subjects are curious about the recipients´ reactions and we assume that curiosity is satisfied when feedback is given, and it remains unsatisfied when feedback is not given. Second, a growing body of evidence suggests that individuals like having a positive self-image and dislike factors that threaten its maintenance (Bénabou and Tirole 2006; Grossman and van der Weele 2017; Loewenstein and Molnar 2018). Consequently, they like (dislike) the reception of feedback, given that they expect it to contain social approval (disapproval). In the given setting, we assume that dictators anticipate receiving social approval when they behaved fairly in the allocation situation and that they anticipate receiving social disapproval, when they behaved selfishly. Consequently, agents prefer receiving feedback when they behaved fairly, and they prefer avoiding feedback, when they behaved selfishly. Based on these two strands of literature, we formulate Hypothesis 1, for fair dictators and Hypothesis 2 for unfair dictators.

Hypothesis 1. *Fair dictators exhibit a positive WTP to get, but not to avoid feedback from the recipient.*

Hypothesis 1 reflects the idea that fair dictators exhibit a preference to satisfy their curiosity about the recipients' reactions as well as a preference to receive social approval. We therefore expect that for fair dictators, these preferences translate into a positive WTP to get feedback. For those reasons, we expect no positive WTP to avoid feedback.

Hypothesis 2. *Unfair dictators exhibit a positive WTP to get, and to avoid feedback from the recipient.*

Hypothesis 2 reflects the idea that unfair dictators trade off utility from satisfying their curiosity with the anticipated detrimental effects on self-image. That is, some of them will be more concerned with curiosity than with self-image and will therefore pay to receive feedback from the recipient. As a result, we expect a positive share of unfair dictators to exhibit a WTP to get feedback. On the other hand, there will be some, for which the anticipated effects on self-image are more important than the desire to satisfy their curiosity about the recipient's reaction. We therefore hypothesize that among unfair dictators there will be a fraction of subjects that exhibit a positive WTP to avoid feedback.

2.3 Results

2.3.1 Allocation Behavior

Overall, we had 117 dictators in both treatments. The dictators' choices in the allocation stage do not differ between the two treatments (z=0.455, p=0.649, two-sided Mann-Whitney U test (MWU)). Moreover, we find overall no preference of one over the other allocation option (p=0.460, two-sided binomial test). By contrast, recipients expect the dictator to select the unfair option significantly more often (p<0.001, two-sided binomial test). Conducting regression analyses on allocation behavior, including controls for gender and economics study subject, we find that only dictators studying economics choose the selfish allocation significantly more often (see Appendix, Table A 2.1).

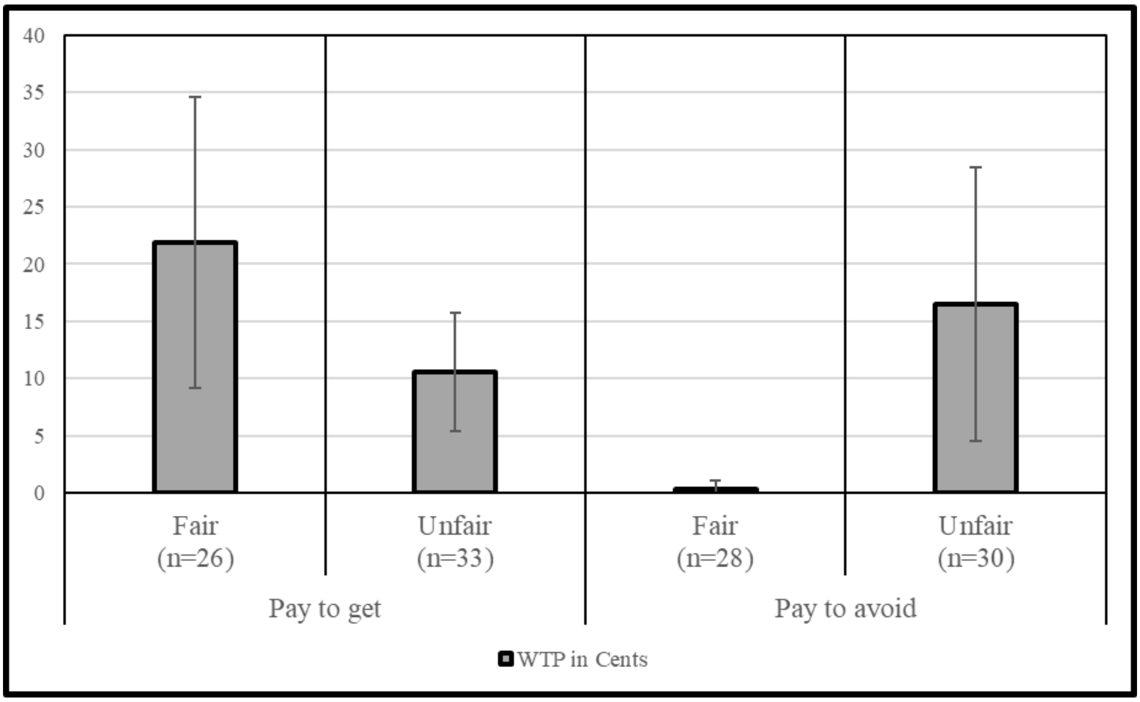
2.3.2 Hypothesis Testing

We find that dictators exhibit a positive WTP for the reception of feedback in *pay to get*, independent of their choice in the allocation stage (Figure 2.1).[12] This constitutes support for Hypotheses 1 and 2. Furthermore, the WTP does not differ between subjects that chose the fair or the unfair option (n=59, z=-1.196, p=0.232, MWU). In *pay to avoid*, we

[12] For all WTP's in Figure 1 we also run one-sided t-tests, which were significant with p<0.01, except for fair dictators in the pay to avoid treatment (p=0.163). Nevertheless, due to the sample size, we focus on the confidence intervals to be a more reliable measurement.

find that unfair dictators exhibit a positive WTP to avoid feedback, while we find no positive WTP to avoid feedback for fair dictators (Figure 2.1). This again supports Hypotheses 1 and 2. Furthermore, we find that unfair dictators exhibit a significantly higher WTP to avoid feedback compared to fair dictators (n=58, z=2.494, p=0.013, MWU). Table 2.1 shows a regression, which supports our findings, while considering all control variables.

Figure 2.1. WTP by Treatment and Decision



Notes: Error bars indicate 95% confidence intervals.

Table 2.1. Linear Regression on Willingness to Pay

	Willingness to Pay	p-values
Pay-to-Get Treatment	-5.415 (6.873)	0.432
Fair Decision	-14.520 (6.440)	**0.026**
Pay-to-get Treatment X Fair Decision	26.404 (8.785)	**0.003**
Well-Being	-0.263 (1.600)	0.870
Arousal	0.780 (1.127)	0.490
Economist	-0.061 (5.029)	0.990
Male	5.431 (4.716)	0.252
Constant	12.313 (13.901)	0.378
Observations	117	

Notes: We report OLS model coefficient estimates with standard errors clustered on the individual level in parentheses. The dependent variable is the willingness-to-pay to either get or avoid feedback depending on the treatment by the dictators. Pay-to-Get Treatment is a dummy variable, which takes the value of 1 for the pay to get treatment and 0 for the pay to avoid treatment. Fair Decision is a dummy variable, which takes the value of 1 if the dictator decided for the fair option and 0 if he decided for the unfair option. We control for the self-reported well-being level, the self-reported arousal level, participants studying economics and gender.

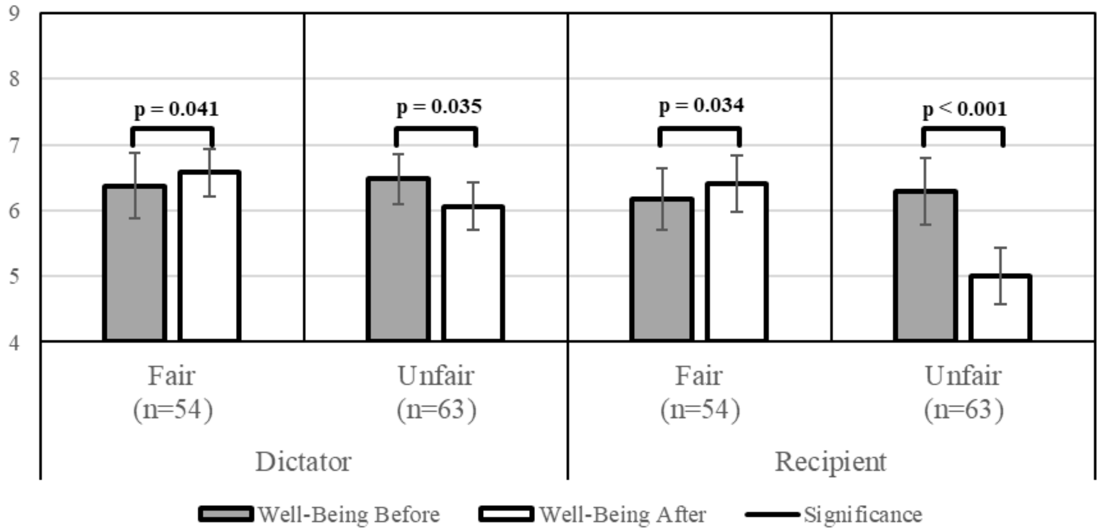
2.3.3 Exploratory Analyses: Well-Being and Arousal

We elicit emotional changes regarding well-being (negative/positive) and arousal (not excited/excited) using a 9-point SAM-scale three times during the experiment. We restrict our analysis to the first two elicitations to infer causal statements, which is, due to multiple paths, only possible to a limited extent afterwards[13]. Figure 2.2 depicts the results for well-being. At the beginning of the experiment, when we elicit well-being and arousal the first time, we find no differences between dictators and recipients (well-being: n=234, z=-0.593, p=0.553; arousal: n=234, z=-0.670, p=0.503, MWU). After the allocation stage is finished and allocations are revealed to recipients, the well-being of fair dictators significantly increases, while it significantly decreases for unfair dictators. Looking at the recipients, we find a significant increase in well-being, when dictators treated them fairly and a significant decrease, when treated unfairly. For the change in

[13] Detailed regression analysis can be found in the online supplement. The analysis showed, except for a huge increase in well-being for fair dictators, which received feedback, only negligible insights.

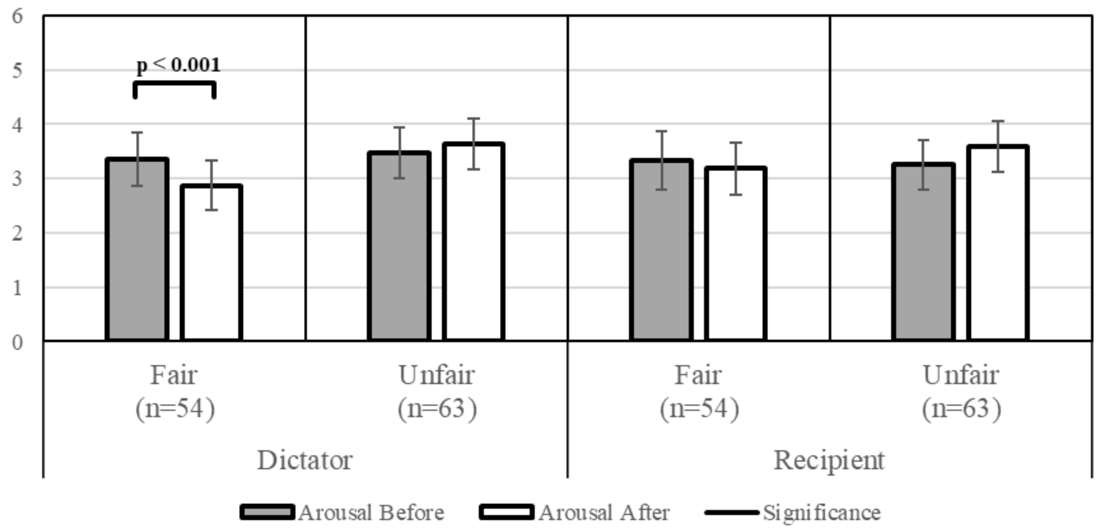
arousal, Figure 2.3 implies a significant decrease for fair dictators and a significant increase for recipients that were treated in an unfair way.

Figure 2.2. Change of Well-Being



Notes: Comparison of self-reported well-being level before and after the allocation decision. Assessment based on the 9-point SAM scale for well-being (negative-positive). Error bars indicate 95% confidence intervals; two-sided Sign test.

Figure 2.3. Change of Arousal



Notes: Comparison of self-reported arousal level before and after the allocation decision. Assessment based on the 9-point SAM scale for arousal (not excited-excited). Error bars indicate 95% confidence intervals; two-sided Sign test.

2.3.4 Exploratory Analyses: Motivation for Bidding

In both treatments, we ask dictators about the motives for their WTP as well as recipients about their beliefs about the dictators´ motives.[14] The main motives stated in *pay to get* by fair dictators are curiosity (63%), the desire to give the recipient the possibility to express his/her feelings (36%) and the expectation to receive positive feedback (36%). The most frequently stated motives by unfair dictators for paying for receiving feedback are the desire to give the recipient the possibility to express his/her feelings (64%) and curiosity (55%). In *pay to avoid* the main motivation stated by unfair dictators are the expectation to receive negative feedback (83%) and feelings of shame or guilt (33%).[15] Furthermore, we see that the beliefs of the recipients correspond to the actual motives stated by dictators.[16]

2.3.5 Exploratory Analyses: Message Content

In total, 58 messages were sent from recipients to dictators.[17] Most messages sent to fair dictators contain positive feedback displaying appreciation for the choice in the allocation stage (85%). In addition, one recipient expresses understanding for the dictator´s behavior, stating that he would have made the same decision (4%). The messages sent to unfair dictators mainly contained negative feedback (66%). However, an appreciable minority states understanding for the dictators' choice (20%).

[14] Two research assistants evaluated the statements independently without knowing about the experiment or the hypotheses. For that purpose, we provided them with a list of motives that are relevant for the derivation of hypotheses and they then checked whether these motives were mentioned by participants. In addition, the evaluators could build new categories, when this was necessary. For the analysis, we apply a conservative approach by considering only those evaluations where both evaluators came to the same conclusion regarding a particular motive. Since we do not put any restrictions on the free text the content can be assigned to multiple categories, and no assignment can be possible. A complete list of all statements and sent messages, as well as the categorization by the student assistants can be found in the Appendix online.

[15] Only one fair dictator exhibited a positive WTP for avoidance, but the stated motivation for that behavior could not be categorized.

[16] Precisely, recipients matched with fair dictators also conjectured that curiosity (36%), the desire to give the recipient the possibility to express his/her feelings (9%) and the expectation to receive positive feedback (55%) are the main motives for feedback. Recipients of unfair feedback from dictators conjectured that the desire to give the recipient the possibility to express his/her feelings (46%) and curiosity (69%) will be the main motives.

[17] 27 messages are sent to fair dictators and 31 to unfair dictators. The content is analyzed the same way as the motivations for the WTP in the previous section. Again, we only report what has been classified identically by the two research assistants.

2.4 Discussion and Conclusion

In summary, our results provide answers to our main research questions and hypotheses: First, both a significant fraction of fair dictators, but also of unfair dictators exhibit a positive willingness-to-pay to enable the recipient to give feedback in an allocation decision. Second, unfair dictators exhibit a willingness-to-pay to avoid feedback from their recipient, while this is not the case for fair dictators. These findings are consistent with our hypotheses.

Looking at the expectations of dictators regarding message content shows that our paradigm worked properly as dictators expected to receive social approval if they chose the fair option and social disapproval if they chose the unfair option. Second, we are confident that the WTP entered by dictators in the different constellations do not result from experimenter demand effects (Zizzo 2010), as 27 out of 28 dictators who chose the fair option entered a WTP of zero for feedback avoidance.

The general demand for receiving feedback corroborates the importance of curiosity. Particularly, as this motive has been mentioned by the majority of fair as well as by the majority of unfair dictators, despite the fact that these two groups strongly differed regarding the expectations about the kind of feedback that they would receive. This is in accordance with literature that defines curiosity as an intrinsic demand to learn about the unknown, *independent* from its content (Laffont 1989; Loewenstein 1994; Golman and Loewenstein 2015). We therefore complement this literature by identifying the importance of curiosity for an understanding of the demand for non-instrumental communication in allocation decisions.

Likewise, the fact that the demand for feedback decreases when dictators behave unfairly in the allocation decision is in accordance with models that assume individuals to trade off self-image with other concerns, e.g., material outcome (Bodner and Prelec 2003; Bénabou and Tirole 2006; Grossman and van der Weele 2017; Bénabou, Falk, and Tirole 2018). We contribute to this literature by providing evidence that this trade-off also holds for non-material outcomes: our results suggest that individuals trade off the negative effect of information that potentially harms self-image with the demand to satisfy curiosity by learning about the recipients' actual reactions.

The exploratory analyses of changes in well-being and arousal shed further light on emotional development between the stages. For example, in fair dictators, we identify a significant increase in well-being, but a decrease in arousal. This could be interpreted as satisfaction for being "nice" to the recipient or behaving in a socially respected manner. For unfair dictators, we find the opposite with respect to well-being. This might be interpreted as a result from feelings of guilt. Looking at the recipients, we find that subjects matched with an unfair dictator perceive a large decrease in well-being. One plausible explanation for this observation could be disappointment about the dictator's decision. Another possible explanation could be anger, which we nevertheless discard, since we do not find a significant increase in arousal.

We are aware that our experiment has some limitations. For example, some parts of the experiment are not incentivized, such as the elicitation of the recipients' guesses or the dictators' beliefs about the content of the messages. Furthermore, participants have been aware about the fact that feedback could potentially be distributed in the second stage. This might have had an effect on the allocation decision in the first stage, which in turn might have influenced the feedback decision in the second stage. As a result, dictators might have decided unfairly less often, compared to the situation where they would not have known about the upcoming feedback stage. This, however, should lead to an *underestimation* of the WTP for the avoidance of feedback, as those dictators whose self-image is especially vulnerable to negative feedback should be most prone to adapt their behavior in case of the presence of feedback. Such individuals would thus depend particularly strong on mechanisms that help them to avoid deterioration of self-image. In addition, the setting is consistent with most field situations as individuals mostly have the possibility to either self-select into environments with a particular feedback structure (i.e., feedback exists or not) or can adapt behavior depending on the structure of feedback that is present in a specific context.

We see three avenues for future research. First, it might be useful to take a closer look at the relative importance of the motives underlying the decision to receive or avoid feedback. Although our experiments identified the importance of each of the examined motives, our data does not allow to draw precise conclusions about the relative importance of the competing motives. Further research on how people trade off motives such as curiosity and avoiding social disapproval could help understanding social interactions to a higher degree. Second, it might be interesting to shed further light on the desire to give the recipient the possibility for expression, a desire that has been mentioned both by fair and unfair dictators, i.e., independently from the expected content of the message. Third, given the results from Xiao and Houser (2007) and Ellingsen and Johannesson (2008) on the effects of costless post-decision messages on dictator giving, it might be interesting to study whether the effects of feedback on the dictator's decision change when dictators take a costly decision about the feedback option.

Appendix 2

Table A 2.1. Dictators' WTP by Allocation Choice

		Fair Dictators (n=54)	**Unfair Dictators (n=63)**	Test for difference in mean/median
Pay to Get (n=59)	Observations	26	33	
	WTP	15/26	15/33	p = 0.435, Fisher's exact test
	Share (WTP > 0)	58%	45%	
	Avg. WTP in €	0.22 (0.32)	0.11 (0.14)	z = -1.196, p = 0.232, MWU
	Avg. WTP in € (conditional on WTP>0)	0.38 (0.33)	0.23 (0.13)	z = -0.875, p = 0.382, MWU
Pay to Avoid (n=58)	Observations	28	30	
	WTP	1/28	8/30	**p = 0.026**, Fisher's exact test
	Share (WTP > 0)	4%	27%	
	Avg. WTP	0.00 (0.02)	0.17 (0.32)	z = 2.494, **p = 0.013**, MWU
	Avg. WTP in € (conditional on WTP > 0)	0.10 (n.a.)	0.62 (0.32)	z = 1.556, p = 0.120, MWU

Notes: All reported tests are two-sided. Standard error in parenthesis.

Table A 2.2. Dictators' WTP by Treatment

		Pay to Get (n=59)	**Pay to Avoid (n=58)**	Test for difference in mean/median
Fair Dictators (n=54)	Observations	26	28	
	WTP	15/26	1/28	**p < 0.001**, Fisher's exact test
	Share (WTP > 0)	58%	4%	
	Avg. WTP in €	0.22 (0.32)	0.00 (0.02)	z = -4.290, p < 0.001, MWU
	Avg. WTP in € (conditional on WTP>0)	0.38 (0.33)	0.10 (n.a.)	z = -0.654, p = 0.513, MWU
Unfair Dictators (n=63)	Observations	33	30	
	WTP	15/33	8/30	**p < 0.001**, Fisher's exact test
	Share (WTP > 0)	45%	27%	
	Avg. WTP	0.11 (0.14)	0.17 (0.32)	z = -0.766, p = 0.444, MWU
	Avg. WTP in € (conditional on WTP > 0)	0.23 (0.13)	0.62 (0.32)	z = 2.919, **p = 0.004**, MWU

Notes: All reported tests are two-sided. Standard error in parenthesis.

Table A 2.3. Recipients' Beliefs About Dictators WTP by Treatment

		Pay to Get (n=59)	**Pay to Avoid (n=58)**	Test for difference in mean/median
Expected Fair Dictator Behavior (n=54)	Observations	21	20	
	WTP	11/21	15/20	p = 0.197 Fisher's exact test
	Share (WTP > 0)	52%	75%	
	Avg. WTP in €	0.18 (0.26)	0.32 (0.31)	z = 1.812, p = 0.070, MWU
	Avg. WTP in € (conditional on WTP>0)	0.34 (0.28)	0.42 (0.28)	z = 1.053, p = 0.292, MWU
Expected Unfair Dictator Behavior (n=63)	Observations	38	38	
	WTP	22/38	17/38	p = 0.359 Fisher's exact test
	Share (WTP > 0)	66%	45%	
	Avg. WTP	0.20 (0.26)	0.21 (0.30)	z = -0.348, p = 0.728, MWU
	Avg. WTP in € (conditional on WTP > 0)	0.35 (0.25)	0.47 (0.27)	z = 1.818, p = 0.069, MWU

Notes: All reported tests are two-sided. Standard error in parenthesis.

Table A 2.4. Recipients' Beliefs About Dictators WTP by Allocation Choice

		Fair Dictators (n=54)	**Unfair Dictators (n=63)**	Test for difference in mean/median
Pay to Get (n=59)	Observations	21	38	
	WTP	11/21	22/38	p = 0.786 Fisher's exact test
	Share (WTP > 0)	52%	66%	
	Avg. WTP in €	0.18 (0.26)	0.20 (0.26)	z = 0.448, p = 0.654, MWU
	Avg. WTP in € (conditional on WTP>0)	0.34 (0.28)	0.35 (0.25)	z = 0.193, p = 0.847, MWU
Pay to Avoid (n=58)	Observations	20	38	
	WTP	15/20	17/38	p = 0.051 Fisher's exact test
	Share (WTP > 0)	75%	45%	
	Avg. WTP	0.32 (0.31)	0.21 (0.30)	z = -1.754, p = 0.080, MWU
	Avg. WTP in € (conditional on WTP > 0)	0.42 (0.28)	0.47 (0.27)	z = 0.498, p = 0.619, MWU

Notes: All reported tests are two-sided. Standard error in parenthesis.

Table A 2.5. Probit Regression on Fair Decision

	Fair Decision	p-values
Pay-to-Get Treatment	-0.143	0.555
	(0.241)	
Well-Being	-0.043	0.606
	(0.084)	
Arousal	-0.028	0.675
	(0.067)	
Economist	-0.627	**0.031**
	(0.291)	
Male	-0.423	0.081
	(0.242)	
Constant	0.689	0.302
	(0.668)	
Observations	117	

Notes: We report probit model coefficient estimates with standard errors clustered on the individual level in parentheses. The dependent variable is the decision for the fair outcome by the dictators. We control for the two different treatments, participants studying economics and gender.

Instructions and Raw Data

The German version and an English translation of the instructions can be found online at https://doi.org/10.11588/data/R1CRC4. The repository also includes the raw data, and also a replication code to generate the analysis and tables presented in this paper.

Chapter 3

Performance Prediction and Performance-Based Decision

Abstract. In many situations, self-perception and perception by others diverge. Such differences can play an important role in corporate governance settings, for example, in the context of task allocation and promotions. An employee and her supervisor will both develop beliefs about the employee's skills, which will, in turn, inform their beliefs about the employee's future performance. In this study, we ran two laboratory experiments to study how predictions about one's own performance and the performance of others develop over time and how they influence consecutive performance-based decisions. Our findings indicate that participants are generally able to give correct predictions about their own performance and the performance of others. However, our findings suggest that participants who receive performance reports without directly observing each other's performance, revalue them in a systematic way. For the performance-based decision, we find that participants favor tasks based on their own performance compared to performance-matched lotteries, while they are indifferent when the tasks are based on someone else's performance.[18]

[18] This chapter was co-authored by Stefan Trautmann.

3.1 Introduction

An essential part of every job is the constant evaluation of one's performance to guide work-related decisions.[19] However, for employees, the evaluation of their performance by colleagues and especially by superiors plays another major role. The quality of the evaluation depends on various factors, such as the complexity of the activity to be assessed or the individual judgment ability (Zell and Krizan 2014). Another aspect is the observability of the performance. While an employee can usually observe her work directly, this is not always the case for her supervisor, who sometimes has to rely on the self-assessment of her employee and has to form an opinion about the performance of her employee on this basis.

Taking these factors into account, this study intends to answer the following research questions: First, do people performing a task (*Performers*) and those observing them (*Observers*) develop congruent performance predictions about the Performer's skill, when the tasks and the successes or failures of the Performer are directly observable? This question corresponds to a situation in which a supervisor permanently looks over her employee's shoulder and can observe the situation as a whole.

Second, since the previous scenario does not normally correspond to day-to-day business activities, we additionally want to answer how the Observer's skill assessment develops when relying on the Performer's self-assessment. This is comparable to a situation in which both parties know the task, but the successful or unsuccessful completion of this task cannot be assessed directly or can only be assessed with delay by the supervisor, reflecting a classical *principle-agent problem*. Note that we keep the interests of Performers and Observers aligned and, therefore, do not create an incentive for false reports.

Third, we want to investigate how these assessments form the basis for upcoming performance-based decisions; for example, a manager has to base decisions about promotions, transfers or the assignment of high-profile projects on these assessments. At the same time, an employee has to decide whether she feels up to these new challenges.

[19] In our study, performance is determined by the participant's skill. Thus, these terms have been used interchangeably. We are aware of the fact that performance is very often associated with effort. However, by design, maximum effort is ensured by the incentive structure of the experiment and should, therefore, reflect the actual skill/ability of the participant. In addition, the performance translates directly into outcome.

A misjudgment can lead to negative consequences for a company's economic success and the personal development of the employee.

In summary, we want to answer questions concerning how Performers' and Observers' performance predictions develop over time depending on direct and indirect observability and whether Performers and Observers draw the appropriate conclusions based on their assessments.

To answer these questions, we run two experiments, where we mimic a supervisor-employee (Observer-Performer) relationship. In the first stage of the experiments, the Performer answers a series of incentivized quiz questions while she is being observed by the Observer. During this stage, the Performer and Observer constantly give predictions about the Performer's skill to correctly answer the next question. Furthermore, we vary the directness of the Observers observations. In the first experiment, the Observer monitors the Performer directly (*Direct Skill Assessment*), while in the second experiment, the Observer has to rely on the Performer's self-reports (*Indirect Skill Assessment*). In the second stage, we imitate a situation where both the Observer and the Performer have to make a decision based on their previously developed performance predictions. This decision is comparable with either investing in a risky financial asset (Chance) or in the employee's skill (Skill). Based on the decision, we can trace to what degree Observers' and Performers' stated performance predictions map to their behavior.

We do not find convergence of performance predictions between Observers and Performers in Experiment 1 (Direct Skill Assessment). While Observers' performance predictions are congruent with the Performers' actual performance, Performers underreport their actual performance. In contrast, Performers decide more often in favor of the option that depends on their actual performance (Skill). We discuss this observation against the background of the modesty and competence literature. In Experiment 2 (Indirect Skill Assessment), we also do not find convergence of performance predictions for Observers and Performers. However, in contrast to Experiment 1, we observe that Performers' performance predictions are now congruent with their actual performance. Furthermore, our results show that Observers do not follow the self-reports one-to-one but rather revalue them depending on the level of the transmitted performance predictions. If the self-reported performance predictions by the Performers are high, the Observers state lower performance predictions and if the self-reported performance predictions by the Performers are low, the Observers stated higher performance predictions. This revaluation is also reflected in the Observers' decisions. Transferred to the work environment, this finding would lead to unfavorable decisions, such as promoting someone who is not suited for a position or, on the other hand, omitting a skilled worker from a project to which she could make valuable contributions.

The remainder of the paper is organized as follows. Section 3.2 presents Experiment 1 and introduces the general experimental design, followed by the results and discussion. Section 3.3 presents Experiment 2, states the differences to the general experimental design and otherwise follows the structure of the previous section. Section 3.4 provides a conclusion.

3.2 Experiment 1: Direct Skill Assessment

3.2.1 Experimental Design

Our design was partly based on Experiment 1 of (Heath and Tversky 1991). At the beginning, participants were randomly divided into pairs of two. Within the groups, one group member received the role of Performer and the other the role of Observer. Overall, the experiment consisted of two parts, the structure of the second part being identical to the first part, only the roles within the group were changed. Each part consisted of two stages: the Quiz Stage and the Decision Stage.

In the Quiz Stage, the Performer had to answer a series of 15 quiz questions. The Performers had one minute to answer each question by selecting one of four possible answers, each represented by a button (Figure 3.1). At the same time, the Observer saw the question including the correct answer, to avoid the feeling that they knew the correct answer all along (*hindsight bias*), which could affect their upcoming decisions (Figure 3.2). Following the Performer's reply, both received immediate feedback about the correctness of her answer.

Figure 3.1. Sample Question (Performer)

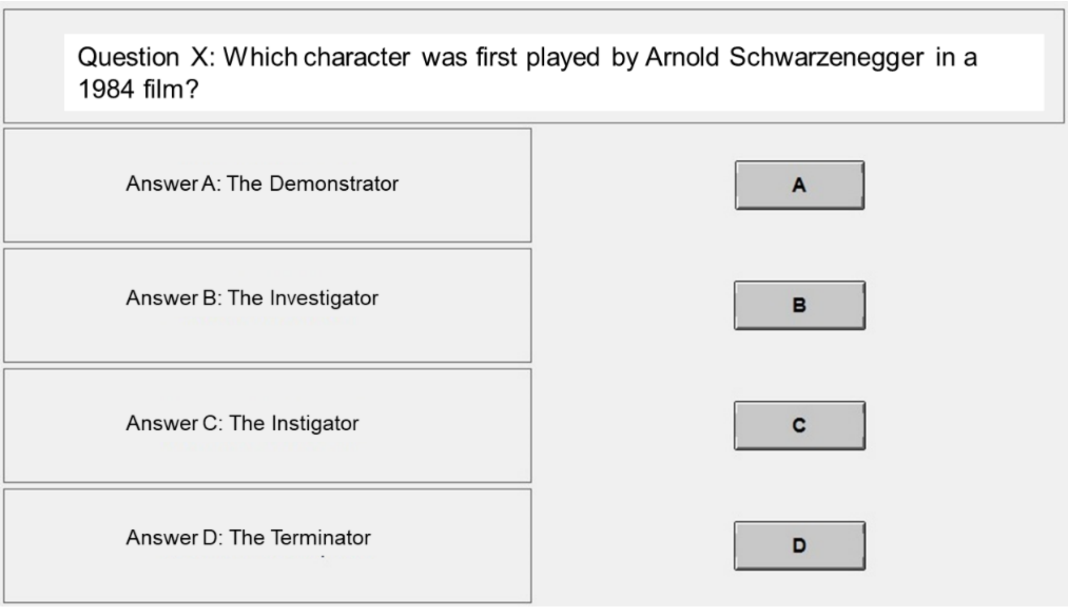
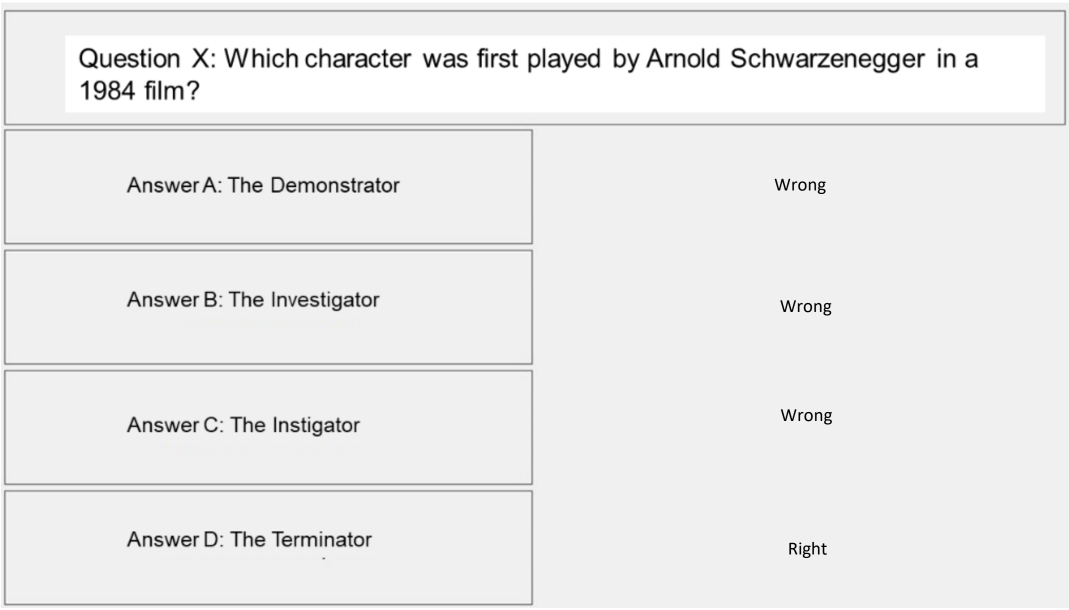


Figure 3.2. Sample Question (Observer)



The quiz questions and answers came from a collection of questions from the German version of the TV-Show “Who Wants to Be a Millionaire” (Strerath-Boly 2002). In total, we used 446 different questions. We varied between two question categories and two different difficulty levels. The categories were trivia and general knowledge. Trivia covered questions about TV-series, music, alcoholic beverages, commercials, sport and similar domains. General knowledge covered questions about history, geography, economics, art and similar domains. The question categories were announced to the participants before each part and were switched between the two parts to avoid the participants drawing strong inferences from part one to part two. The difficulty levels were hard and easy, to get an exogenous variation in the number of correctly answered quiz questions. For the hard difficulty level, the 15 questions consisted of 10 hard, 4 middle and 1 easy question. For the easy difficulty level, the 15 questions consisted of 4 hard, 6 middle and 5 easy questions. This combination of questions aimed to prevent people from getting all answers correct in the easy difficulty level and getting all answers incorrect in the hard difficulty level. We based the categorization of the questions as easy, middle and hard on the systematic of the "Who Wants to Be a Millionaire" questions.[20] The difficulty level stayed the same during the two parts. The participants were not informed about the different difficulty levels. Using this setup, we confronted participants with a diverse set of questions, which helped us to approximate the ever-changing demand of typical work environments.

To investigate the development of the predictions over the Performers' skills, the Performers and Observers stated their performance predictions 16 times (before each question and after the last quiz question). The performance predictions ("likelihood of answering the next question correctly") were indicated on a continuous scale from 0 (absolute unlikely) to 100 (absolute likely) and not shared between the participants. To avoid any bias, the scale did not show a pre-selected default. Predictions were not incentivized.

In the Decision Stage, the participants had to decide between two alternatives: Alternative 1 (Chance) was a skill-matched lottery, where the probability of winning was endogenously matched to the Performers' true relative performance in the Quiz Stage. For example, 9 correct answers out of 15 questions yielded a wining probability of 60%. Participants observed the winning probabilities as given and were not informed about the relationship between the number of correctly answered questions and the Performer's probability of winning the lottery. In Alternative 2 (Skill), the prize depended on the Performers' answers to a 16th quiz question. A correct answer yielded the prize with certainty, while an incorrect answer yielded zero. The participants were informed that the 16th quiz question was drawn from a pool of questions with the same category and level of difficulty as the 15 previous questions. Since the winning probability matched the

[20] In the show "Who Wants to Be a Millionaire" the contestants have to answer 15 questions with increasing difficulty levels. Therefore, we labeled the question 2-6 as easy, 7-10 as middle and 11-15 as hard. We neglected the first question since these questions do not, in the broadest sense, test knowledge but rather logical reasoning.

observed performance, the expected payoff of the two alternatives was theoretically the same. The participants decided between the two alternatives by allocating a probability weight using a slider. By default, the probability weight was set to 50% for each alternative. The probability weight for either alternative was a maximum of 90% and a minimum of 10%. This operation was done to ensure that even when the Performer put the maximum probability on the chance-based alternative, she still had an incentive to answer the 16th question in the skill-based alternative correctly.

3.2.1.1 Incentives

The payoff was determined in the following way: First, the payoff relevant part was randomly selected. Second, one of the two stages was randomly chosen to be paid out. If the Quiz Stage was selected, Performers received €1 for each correct answer (max. €15 if all questions were answered correctly), and Observers received a fixed amount of €7.50. If the Decision Stage was selected, the payoff was determined by the selected alternative based on the assigned probability weight. Depending on the probability weight, either Alternative 1 (Chance) or Alternative 2 (Skill) was selected individually for the Performer and the Observer. If Alternative 1 was selected, and the lottery turned out positive, the participant received €7.50 and €0 otherwise. If Alternative 2 was selected, and the Performer's answer was correct, the participant received a payoff of €7.50 and €0 otherwise. This setup ensured that all stages were properly incentivized without giving the participants the possibility to hedge.

3.2.1.2 Procedural Details

A total of 310 subjects participated in Experiment 1, which was programmed using zTree (Fischbacher 2007). The experiment was conducted in the experimental laboratories of Heidelberg University (AWI Lab) and the University of Mannheim (mLab). Participants were mainly undergraduate students invited with the recruitment software Hroot (Bock, Baetge, and Nicklisch 2014) in Heidelberg and ORSEE (Greiner 2015) in Mannheim. The number of available questions was sufficient to create eight unique question sets (see Appendix Table A 3.1.). Since we conducted the study in two different laboratories, we were able to run 16 sessions while avoiding any potential spillover effects between subjects (i.e., sharing of correct answers with fellow students between sessions). The final payoffs consisted of a show-up fee of €3 and a variable payoff (cf. section 3.2.1.1). On average, the participants earned €8.35, ranging from €3 to €17. Each session lasted approximately 60 minutes.

3.2.2 Hypotheses

In line with our research questions, Experiment 1 has been designed to answer three hypotheses. First, we wanted to find out if Performers and Observers would have converging performance predictions about the Performers' skills when both receive identical (direct) signals. Since the task was unknown to both parties, the updating process should be unbiased, and identical (direct) signals should lead to a convergence of performance predictions over time.

Hypothesis 1: *Performers' and Observers' performance predictions converge over time.*

The next hypothesis concerns the quality of the performance predictions, meaning how close the last performance prediction is to the actual underlying performance. We thereby approximate the actual performance with the percentage of correct answers in the quiz stage. Since the received signals reflect the actual performance, the performance prediction at the end of the quiz stage should be equal to the actual performance if participants follow the received signals.

Hypothesis 2: *Performers and Observers both correctly predict Performers' actual performance.*

Finally, we want to answer if participants make the "right" decision based on their last stated performance predictions. Since the winning probability in the skill-matched lottery (Alternative 1 (Chance)) reflects the Performers' actual performance, a participant who predicted the Performers' actual performance correctly should be indifferent towards the skill-matched lottery and answering another question (Alternative 2 (Skill)). Following from this, a participant who over-predicts the Performers' actual performance should put a lower probability weight on Alternative 1 (Chance) compared to Alternative 2 (Skill). Otherwise, a participant who under-predicts the Performers' actual performance should put a higher probability weight on Alternative 1 (Chance) compared to Alternative 2 (Skill).

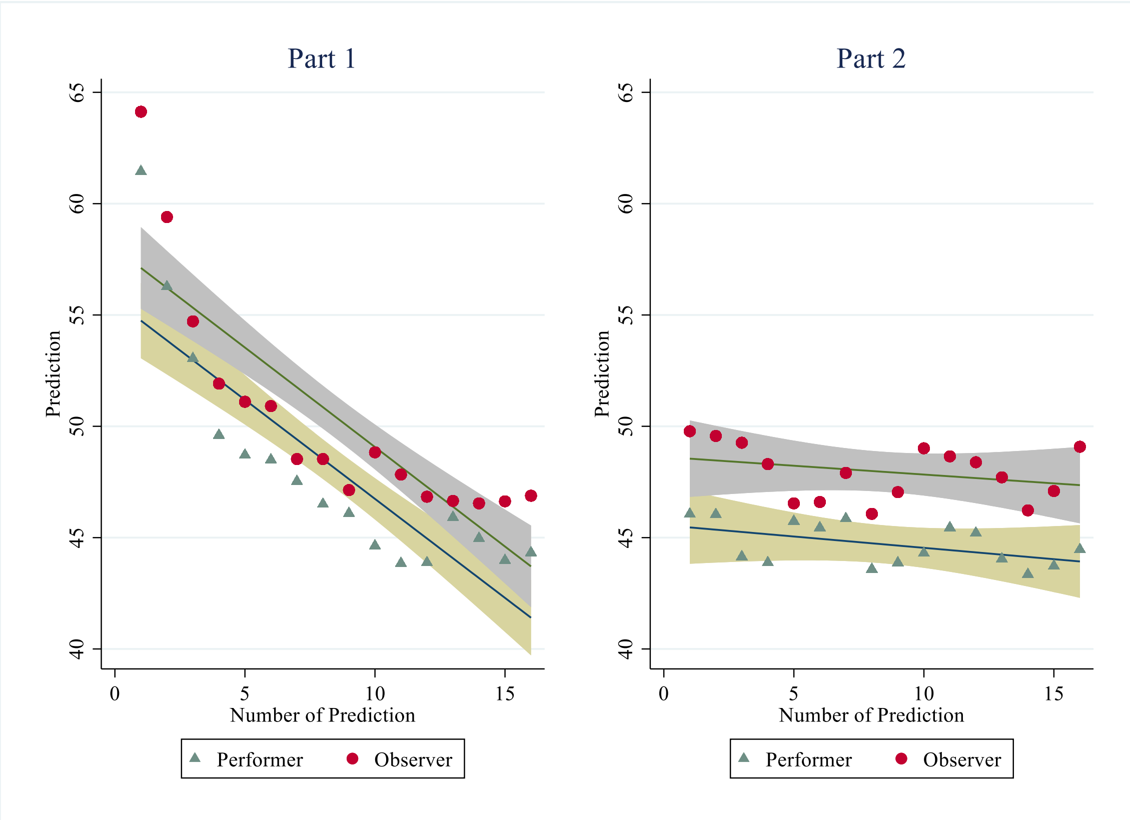
Hypothesis 3: *Participants that correctly predict the Performers' actual performance are equally likely to choose either Alternative 1 (Chance) or Alternative 2 (Skill). Participants that over-(under-) predict the Performers' actual performance put a higher (lower) weight on Alternative 2 (Skill).*

3.2.3 Results

3.2.3.1 Performance Prediction

Figure 3.3 shows the development of the Observers' and Performers' predictions over the course of the experiment.[21] Here, two observations can be made: First, one can see a parallel trend between the performance predictions rather than a convergence and, second, one can see that Performers constantly make lower performance predictions than Observers.

Figure 3.3. Experiment 1: Development of Predictions by Part



Notes*:* 95%-confidence interval around fitted values.

The panel regression in Table 3.1 confirms this result. Performers stated lower performance predictions compared to Observers (Table 3.1). Therefore, Hypothesis 1 cannot be supported. The performance prediction increased with the *Proportion of Correct Answers*. The Proportion of Correct Answers was dynamically calculated in each round and was given as the quotient of correct answers to number of questions ranging from 0 to 1. Furthermore, one can observe that the performance predictions decreased

[21] The exogenous manipulation to get a higher variation in the number of correct answered quiz questions was successful, see Appendix Table A 3.2.

with the number of questions (Table 3.1), which indicates a dynamic learning process overall.

Table 3.1. Panel Regression of Predictions Experiment 1

	Prediction	
	(a)	(b)
Performer	-2.769***	-2.769***
	(1.009)	(1.008)
Proportion of Correct Answers	38.245***	38.206***
	(1.822)	(1.825)
# of Questions	-0.206***	-0.394***
	(0.051)	(0.061)
Constant	33.912***	31.588***
	(1.386)	(1.552)
Observations	9,300	9,300
Controls	No	Yes

Notes: We report GLS coefficient performance predictions with standard errors clustered on the individual level in parentheses using a random effects model of over 32 question rounds. The dependent variable is the individual performance prediction of the probability of answering the next question correctly in percentage terms [0, 100]. Controls include dummy variables for Part 2, male, participants having an economics related study field and whether the data was collected in Heidelberg or in Mannheim. *, **, *** indicates a significant difference from zero; at the 10%, 5%,1% level.

Result 1: *Instead of convergence we found a parallel trend between Performers' and Observers' performance predictions. Performers' performance predictions were constantly below the Observers' performance predictions.*

3.2.3.2 Accuracy

We defined the *Actual Performance* as the percentage of correctly answered questions in the Quiz Stage. Comparing the Actual Performance with the *Last Prediction* (i.e., the 16^{th} performance prediction), we found the following. While the Performers' Last Prediction was significantly lower ($p<0.01$) compared to their Actual Performance, we found no difference between the Observers' Last Prediction and the Performers' Actual Performance ($p=0.777$). Hypothesis 2 can be supported for the Observers, but not for the Performers.

Result 2: *Observers' Last Prediction was not different to Performers' Actual Performance. Performers' Last Prediction was lower compared to their Actual Performance.*

3.2.3.3 Decision

The decision in the Decision Stage was reflected by the variable *Share-on-Skill*, which was defined as the selected percentage share from the interval 10 – 90%, where higher percentages indicated a higher weighting on Alternative 2 (Skill) and less on Alternative 1 (Chance). Observably, the Performers put significantly more weight on Alternative 2 (Skill) compared to Alternative 1 (Chance) ($p<0.001$), while the Observers showed no tendency for either direction ($p = 0.708$). The difference between Performer and Observer decisions was significant ($p<0.001$).

To see if the level of performance would have an influence on the Share-on-Skill, we divided the observations into low performance, i.e., the Performer answered between 0-5 questions correctly, intermediate performance, i.e., the Performer answered 6-10 questions correctly and high performance, i.e., the Performer answered between 11-15 questions correct (Table 3.2). What was found was that Performers put a significantly higher weight on the skill-based alternative when performing at an intermediate ($p<0.01$) or high level ($p<0.05$), but not during low performance ($p=0.450$). For the Observers, we did not find an effect of different performance levels for the Share-on-Skill variable.

To see if Performers and Observers' performance predictions map to choices, we compared the difference between their Last Prediction and the Performers' Actual Performance (*Δ-Prediction-Performance*) with their Share-on-Skill. Suppose a participant's Δ-Prediction-Performance is equal to zero. In that case, one can expect indifference between the two alternatives, since the winning probability in Alternative 1 (Chance) would be equal to the Actual Performance and, therefore, equal to the winning probability in Alternative 2 (Skill). Thus, any Share-on-Skill would be reasonable. If a participant's Δ-Prediction-Performance is negative, the participant underpredicts the Actual Performance and Alternative 1 (Chance) would yield a higher expected value compared to Alternative 2 (Skill); therefore, it would be reasonable to put a lower Share-on-Skill. On the other hand, if a participant's Δ-Prediction-Performance is positive, the participant overpredicts the Actual Performance, and Alternative 2 (Skill) would yield a higher expected value compared to Alternative 1 (Chance); consequently, it would be reasonable to put a higher Share-on-Skill.

We found Observers' behaviors to be consistent with this definition independent of the level of the Performers' Actual Performance (Table 3.2). In contrast, only the Performers with a low performance level consistently acted according to our previous definition, while the Performers with an intermediate and high-performance level acted inconsistently by underpredicting their performance but putting a significantly higher Share-on-Skill. As a result, hypothesis 3 can be supported for the Observers, but not for the Performers.

Result 3: *Observers correctly predicted the Performers' Actual Performance and were equally likely to choose either Alternative 1 (Chance) or Alternative 2 (Skill). Performers underpredicted their Actual Performance, but put a higher Share-on-Skill on Alternative 2 (Skill).*

Table 3.2. Performer and Observer Share-on-Skill and Δ-Prediction-Performance in % Depending on Level of Actual Performance

	Low Performance (0-5) (N=80)	Intermediate Performance (6-10) (N=171)	High Performance (11-15) (N=59)
Performer			
Share-on-Skill	52.21 (26.06)	58.04$^{###}$ (22.20)	59.12$^{##}$ (29.42)
Δ-Prediction-Performance	-0.76 (18.54)	-3.65** (19.70)	-8.80***(16.25)
Observer			
Share-on-Skill	49.49 (27.58)	52.13 (25.26)	47.46 (26.89)
Δ-Prediction-Performance	-0.03 (20.65)	-1.30 (17.74)	2.27 (16.01)

Notes: Numbers in brackets for the performance levels indicate number of correct answers. Share-on-Skill describes the selected percentage share from the interval 10 – 90%, where higher percentages indicate a higher weighting on Alternative 2 (Skill) and less on Alternative 1 (Chance). Δ-Prediction-Performance indicates the difference between the last performance prediction and the actual performance. #, ##, ### indicates a significant difference between putting a weight of 50% on each alternative; at the 10%, 5%,1% level, two-sided t-test; *, **, *** indicates a significant difference from zero; at the 10%, 5%,1% level, two-sided t-test.

Running a regression analysis on Share-on-Skill confirmed these results, and we found that Performers put a higher weight on Alternative 2 (Skill) (Table 3.3, (a)-(c)). Furthermore, the Share-on-Skill increased with Δ-Prediction-Performance (Table 3.3, (c)-(e)). Ultimately, we found an increasing effect of the Performers having a high and intermediate performance on the Share-on-Skill, but only for the Performers (Table 3.3 (d)).

Table 3.3. Regression Analysis of Share-on-Skill in %

	Share-on-Skill				
				Performer	Observer
	(a)	(b)	(c)	(d)	(e)
Performer	6.184***	6.184***	7.211***		
	(1.887)	(1.856)	(1.775)		
Δ-Prediction-			0.286***	0.270***	0.312***
Performance			(0.048)	(0.066)	(0.073)
High			3.997	8.879*	-1.590
Performance			(3.523)	(4.786)	(4.628)
Intermediate			4.901**	6.645**	3.510
Performance			(2.449)	(3.251)	(3.628)
Constant	50.558***	58.240***	55.685***	58.464***	59.831***
	(1.488)	(3.423)	(3.659)	(5.534)	(5.377)
Observations	620	620	620	310	310
Controls	No	Yes	Yes	Yes	Yes

Notes: We report GLS coefficient Share-on-Skill with standard errors clustered on the individual level in parentheses. The dependent variable is Share-on-Skill, which is the selected percentage share from the decision interval from 10 – 90%. Higher percentages indicate a higher weighting on Alternative 2 (Skill) and less on Alternative 1 (Chance). An excluded category for performance is Low Performance. Controls include dummy variables for Part 2, male, participants having an economics related study field and whether the data was collected in Heidelberg or in Mannheim. *, **, *** indicates a significant difference from zero; at the 10%, 5%,1% level.

3.2.4 Discussion

We did not find convergence for the Performer and Observer performance predictions, but rather a parallel trend. Observers' predictions were very close to the actual performance of the Performers, while the Performers' predictions were systematically below their actual performance. Tedeschi (1986) provided a possible explanation for this observation. He described a discrepancy between what participants *actually* believe and what they communicate when they are under observation. Since the Observers were able to predict the actual performance quite well, the communicated performance predictions of the Performers could be interpreted as modesty. Modesty is commonly understood as "the quality of not talking about or not trying to make people notice your abilities and achievements" (Cambridge English Dictionary 2020).

We see the desire to let oneself appear in a positive light as the decisive motive for the modesty in reporting (Schneider 1969; Stires and Jones 1969; Baumeister and Jones

1978). This argument finds additional support if we look at the results differentiated by performance levels. The discrepancy between actual performance and self-assessment increased with increasing performance. This behavior follows mechanically from the desire for modesty, since a high-performing Performer naturally has to discount his performance more than a mediocre-performing Performer in order to communicate the same level of modesty. This scenario also leads to the observation that Performers with low performance did not have to “correct” their assessment in order to appear humble and, therefore, their prediction corresponded to their actual performance. This outcome also explains the apparently irrational behavior in the Decision Stage, in which Performers preferred the skill-based task to the skill-matched lottery, although—based on their communicated prediction—the skill-matched lottery had a higher expected value. However, if we assume modesty, Performers would be able to correctly predict their own skill but would not report it properly.

If we now compare the decisions of Observer and Performer, we see that Performers placed a significantly higher weight on the skill-based alternative in comparison with the Observers. This observation is in line with previous results (Cohen and Hansel 1959; Howell 1971; Heath and Tversky 1991), which have shown that people have a preference for making results dependent on their own skill. In particular, the behavior of the Performers is in accordance with the Competence Hypothesis by Heath and Tversky (1991), which states that “people prefer betting on their own judgment over an equiprobable chance event when they consider themselves knowledgeable” (Heath and Tversky 1991, p. 5). We indeed found a higher weight on skill for better performing Performers.

Our interpretation rests on the assumption that the Performers could correctly predict their own performance, but deliberately stated it differently. An alternative explanation could be the lack of incentivization, which prevented Performers from correctly stating their performance. To study this, we adjusted the incentive structure for Performers in Experiment 2.

3.3 Experiment 2: Indirect Skill Assessment

3.3.1 Experimental Design

Experiment 2 aimed to assess the processing of indirect information by the Observers and examine whether the Performers would state better predictions about their Actual

Performance when the performance predictions were incentivized. We kept everything identical to Experiment 1 except for the following two features. First, the Observers did not directly observe if the Performer answered the question correctly. Instead, the Observers received the Performers' predictions of their performance after they stated their own predictions.[22] Second, to ensure that the Performer stated her performance prediction in the Quiz Stage truthfully, only the Observer could assign a probability weight to the two alternatives in the Decision Stage, while the realization affected both participants equally. Therefore, if the Performer wanted the Observer to make a well-informed decision, it was in her own interest to give a correct performance prediction because incorrect performance predictions became potentially costly.

3.3.1.1 Procedural Details

We had 248 subjects participating in Experiment 2, which was programmed using zTree (Fischbacher 2007). The experiment was conducted in the experimental laboratories of Heidelberg University (AWI Lab) and the University of Mannheim (mLab). Participants were mainly undergraduate students invited with the recruitment software Hroot (Bock, Baetge, and Nicklisch 2014) in Heidelberg and ORSEE (Greiner 2015) in Mannheim. We used the same questions as in Experiment 1, where the number of questions was sufficient to create eight unique question sets (see Appendix Table A 3.1). Again, we conducted the study in two different laboratories so we could run 16 sessions. To avoid potential spillover effects from Experiment 1, we ensured that participants did not participate in the previous experiment and that a sufficient length of time, of more than half a year, was between the two experiments. The final payoffs consisted of a show-up fee of €3 and a variable payoff (cf. section 3.2.1.1). On average, the participants earned €9.35, ranging from €3 to €18. Each session lasted approximately 60 minutes.

3.3.2 Hypotheses

Building up on our first experiment, Experiment 2 has been designed to answer three hypotheses. First, we wanted to test if Observers' performance predictions would match

[22] To avoid any kind of communication (e.g., the Performer could indicate a performance prediction of 100% if she answered the question correctly and a performance prediction of 0% if she answered the question incorrectly), each performance prediction was transmitted with a probability of 50%. If the performance prediction was not transmitted, they received a corresponding message ("No transmission of Performer's prediction"). Thus, Performers had an incentive to submit a precise series of performance predictions to prevent a biased random sample. The exception was the 16th performance prediction, which was always transmitted to ensure that all Observers had at least the last performance prediction upon which to base their decision.

the transmitted Performers' performance predictions or diverge from them in a systematic way. Since incorrectly transmitted performance predictions were potentially costly for Performers, they had an incentive to truthfully communicate the signal. Therefore, Observers should completely base their performance predictions on the Performers' reports since they received no other signals about the Performers' performance.

Hypothesis 4: *If incorrect performance predictions are potentially costly, Observers' performance predictions match the transmitted Performers' performance predictions.*

Second, we wanted to test if Performers would give better performance predictions about their own performance when giving incorrect performance predictions is potentially costly compared to Experiment 1.

Hypothesis 5: *If incorrect performance predictions are potentially costly, Performers' performance predictions are more accurate than in Experiment 1.*

Third, like in our first experiment, we wanted to test whether Observers' performance predictions would map to their choices. Since the winning probability in the skill-matched lottery (Alternative 1 (Chance)) reflected the Performers' actual performance, an Observer who predicted the Performers' actual performance correctly should be indifferent between Alternative 1 (Chance) and Alternative 2 (Skill). Following from this, an Observer who over-predicted the Performers' actual performance should put a lower probability weight on Alternative 1 (Chance) compared to Alternative 2 (Skill). Otherwise, an Observer who under-predicted the Performers' actual performance should put a higher probability weight on Alternative 1 (Chance) compared to Alternative 2 (Skill).

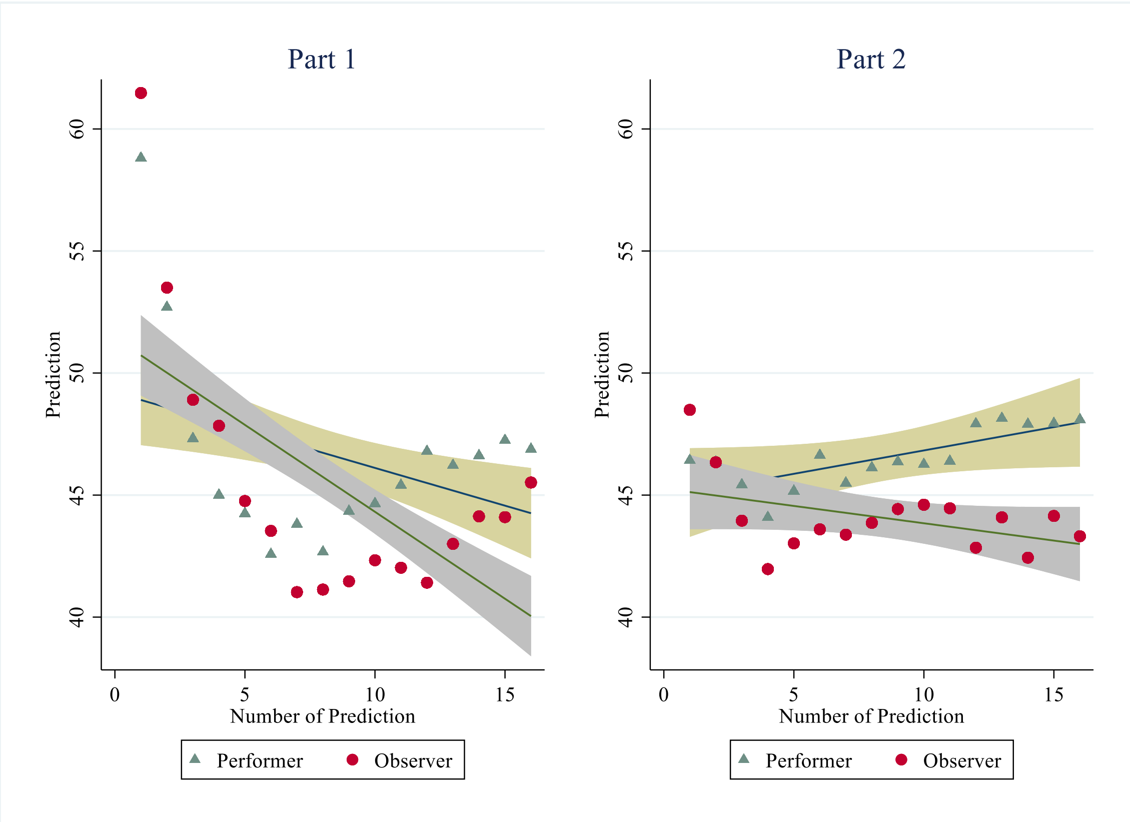
Hypothesis 6: *If incorrect performance predictions are potentially costly, Observers that correctly predict the Performers' actual performance are equally likely to choose either Alternative 1 (Chance) or Alternative 2 (Skill). Observers that over-(under-) predict the Performers' actual performance put a higher (lower) weight on Alternative 2 (Skill).*

3.3.3 Results

3.3.3.1 Performance Prediction

Figure 3.4 does not show a converging trend between Observers and Performers in their performance predictions.[23] Performers, for the most part, stated higher performance predictions than Observers.

Figure 3.4. Experiment 2 Development of Predictions by Part



Notes: 95%-confidence interval around fitted values.

In the panel regression, we see that the Performers' performance predictions were above the performance predictions of the Observers (Table 3.4). Again, the Prediction increased with the Proportion of Correct Answers and decreased with the number of Questions (Table 3.4). Consquently, hypothesis 4 cannot be supported.

[23] The exogenous manipulation to get a higher variation in the number of correct answered quiz questions was successful, see Appendix Table A 3.2.

Table 3.4. Panel Regression of Predictions Experiment 2

	Prediction	
	(a)	(b)
Performer	2.119**	2.119**
	(1.079)	(1.079)
Proportion of Correct Answers	21.406***	21.422***
	(2.136)	(2.150)
# of Questions	-0.073	-0.177***
	(0.055)	(0.065)
Constant	35.347***	36.051***
	(1.492)	(2.050)
Observations	7,440	7,440
Controls	No	Yes

Notes: We report GLS coefficient performance predictions with standard errors clustered on the individual level in parentheses using a random effects model over 32 question rounds. The dependent variable is the individual estimation of the probability of answering the next question correct. Controls include dummy variables for Part 2, male, participants having an economics related study field and whether the data was collected in Heidelberg or in Mannheim. *, **, *** indicates a significant difference from zero; at the 10%, 5%,1% level.

Result 4: *Observers gave lower performance predictions compared to the Performers.*

3.3.3.2 Accuracy

Comparing the Actual Performance with the Last Prediction (Δ-Prediction-Performance), we found the following. While the Observers' Last Prediction is significantly lower ($p<0.001$) compared to the Performers' Actual Performance, we find no difference between the Performers' Last Prediction and their Actual Performance ($p=0.225$) in contrast to Experiment 1. Therefore, hypothesis 5 can be supported.

Result 5: *Performers' Last Prediction was congruent with their Actual Performance. Observers' Last Prediction was lower than Performers' Actual Performance.*

3.3.3.3 Decision

Looking at the decision (Share-on-Skill) in the Decision Stage, we found that Observers put significantly less probability weight on Alternative 1 (Skill) compared to Alternative 2 (Chance) ($p<0.1$). Looking at Δ-Prediction-Performance and Share-on-Skill depending on the performance level (Table 3.5), we observed that Performers stated a lower performance prediction when they were high performing. Looking at the Δ-Prediction-Performance, we found that Observers overpredicted the Actual Performance of the

Performers when their actual performance level was low and underpredicted it when Performers had an intermediate or high-performance level. This result also maps onto the Observers' decisions and, therefore, supports Hypothesis 6.

Table 3.5. Δ-Prediction-Performance and Share-on-Skill in % Depending on Level of Actual Performance

	Low Performance (0-5) (N=61)	Intermediate Performance (6-10) (N=128)	High Performance (11-15) (N=59)
Performer			
Δ-Prediction-Performance	5.73* (23.90)	-2.13 (18.28)	-7.93***(18.07)
Observer			
Δ-Prediction-Performance	11.42***(21.40)	-6.85*** (18.37)	-16.47***(20.30)
Share-on-Skill	59.18## (27.65)	47.08 (29.04)	33.78### (27.31)

Notes: Numbers in brackets for the performance levels indicate number of correct answers. Δ-Prediction-Performance indicates the difference between the last performance prediction and the actual performance. Share-on-Skill describes the selected percentage share from the interval 10 – 90%, where higher percentages indicate a higher weighting on Alternative 2: quiz question and less on Alternative 1: skill-matched lottery. #, ##, ### indicates a significant difference between putting a weight of 50% on each alternative; at the 10%, 5%,1% level, two-sided t-test; *, **, *** indicates a significant difference from zero; at the 10%, 5%,1% level, two-sided t-test.

Having run a regression analysis, we found that the weight on the skill-based alternative increased with Δ-Prediction-Performance (Table 3.6). Conversely, being grouped with a high performing Performer led to a decrease in weight for the skill-based alternative (Table 3.6).

Table 3.6. Regression Analysis of Share on Skill in % (Observers Only)

	Share-on-Skill	
	(a)	(b)
Δ-Prediction-Performance	0.511***	0.498***
	(0.093)	(0.090)
High Performance	-11.146**	-12.258**
	(5.610)	(5.675)
Intermediate Performance	-2.765	-3.254
	(4.634)	(4.567)
Constant	53.343***	51.798***
	(3.649)	(6.813)
Observations	248	248
Controls	No	Yes

Notes: We report GLS coefficient performance predictions with standard errors clustered on the individual level in parentheses. The dependent variable is the selected percentage share from the decision interval from 10 – 90%. Higher percentages indicate a higher weighting on Alternative 2: quiz question and less on Alternative 1: skill-matched lottery. The variable Confidence is a binary variable and becomes one if the last performance prediction > actual performance. In each regression, we control for participants having an economics related study field and whether the data was collected in Heidelberg or in Mannheim. *, **, *** indicates a significant difference from zero; at the 10%, 5%,1% level.

Result 6: *If Obsevers underpredicted the Performers' Actual Performance, they put less Share-on-Skill on Alternative 2 (Skill). If Observers overpredicted the Performers' Actual Performance they put more Share-on-Skill on Alternative 2 (Skill).*

3.3.4 Discussion

We find that the Performers correctly assess their own performance. However, if we consider the different performance levels, we find a similar pattern for the high performing Performers as in Experiment 1, where Performers strongly underpredict their actual performance. Since we only observe this for the high performing Performers, the underestimation might be deliberate and not due to a lack of ability to assess their own performance. This observation is especially interesting since the incentive structure encourages correct performance predictions. Therefore, the modesty hypothesis from Experiment 1 can only be upheld if Performers value the appearance of modesty higher than the consequences of falsely stated performance predictions. An alternative explanation could be that Performers might want to avoid the Observers developing expectations that they may not be able to meet. This behavior is very often described as impostor syndrome, where high achieving people doubt their achievements (Clance and Imes 1978) and, therefore, underreport their skills.[24]

Furthermore, we find that Observers reevaluate the indirect performance information depending on the communicated performance level. If the Performer transmits high performance predictions, the Observer discounts this assessment, which means that they expect the actual performance of the Performer to be lower. On the other hand, if the Performer transmits low performance predictions, the Observer surcharges this assessment, which means that they expect the actual performance of the Performer to be higher. Similar results were also found by Moore and Healy (2008) and Mobius et al. (2014). Mobius et al. (2014, p. 2) described this behavior as conservatism since participants "interpret signals as less informative than they are." Further reasons for this behavior could also be a lack of trust in the willingness and ability of the Performer to communicate a correct self-assessment. Statistical considerations can also (possibly unconsciously) influence the differing assessments. Since the revaluation is strongest for high and low performance levels, Observers might see extreme values as more unlikely and expect Performers to be more average than they claim.

Looking at the Decision Stage, we observe that the performance predictions map to the Observers' decisions. Nevertheless, regarding the reevaluation, this outcome would imply uneconomical decisions in the actual work context. The consequences of this could be, for example, that an employee is assigned to an unsuitable project or, on the other hand, not expected to take on a project for which they are the most qualified.

3.4 Conclusion

The central aspect of this study is the development of skill predictions depending on the individual perspective (Observer or Performer). Varying the degree of supervision of the Observers, we report two key findings. First, if supervision by Observers is direct and exhaustive, they can give a precise prediction of the Performers' actual performance. Considering that their decisions match their predictions, this finding would transferred to the work environment imply viable decisions by the supervisor. Second, if supervision by Observers is indirect and they have to rely on the Performers' reports, they cannot give a precise prediction of the Performers' actual performance, since they systematically

[24] While Clance and Imes (1978) focused on high-achieving women, Langford and Clance (1993) demonstrated that impostor syndrome is equally prevalent independent of gender.

reevaluate them. Again, their decisions mapped to their predictions; however, since the predictions were not equivalent to the Performers' actual performance, the decisions could lead to inferior results in a work environment. While discounting a communicated high skill level can be seen as a precaution, it is not straightforward why a supervisor would expect a higher skill level from an employee who reports having a low skill level. A possible explanation could be that supervisors use some kind of "regression to the mean"-heuristic, where they relativize employees' assessments that appear to them unrealistic or extreme. For Performers, we find in both experiments a tendency to underreport their actual performance when it was high. Since this biased reporting decreases with the Performers' performance, it increases the difficulty for supervisors to distinguish between those who state a lower performance because they are less able and those who are very able but try to disguise it. In an actual work environment, this self-assessment might be less problematic for minor and repeated tasks where supervisors can regularly observe and control the outcomes. However, for important and non-repeatable decisions, it is essential to assess an employee's skill as accurately as possible.

The ability to correctly assess oneself and others and to draw the right conclusions about them is an essential part of any corporate activity. Our study demonstrates that the ability to assess oneself correctly is not synonymous with the correct communication or interpretation of this assessment. We postulate from this divergence a loss of efficiency through suboptimal decisions. Based on this, we consider it worthwhile to investigate this connection even more closely in future studies.

Appendix 3

Table A 3.1. Structure of Conducted Sessions

Number of Sessions	Part 1	Part 2	Difficulty Level
2xHD + 2xMA	Trivia	General Knowledge	Hard
2xHD + 2xMA	Trivia	General Knowledge	Easy
2xHD + 2xMA	General Knowledge	Trivia	Hard
2xHD + 2xMA	General Knowledge	Trivia	Easy

Notes: Sessions were conducted in Heidelberg (HD) and Mannheim (MA). In each session the game was repeated with switched roles. Difficulty levels Hard and Easy stayed the same during each session. Question categories Trivia or General Knowledge switched between Part 1 and Part 2.

Table A 3.2. Manipulation Check

		Hard	***Easy***
Experiment 1	All	6.24 (N=150)	8.18 (N=160) ***
(N=310)	Trivia	6.75 (N=75)	8.01 (N=80) ***
	General Knowledge	5.73 (N=75) ###	8.35 (N=80) ***
Experiment 2	All	6.64 (N=120)	8.03 (N=128) ***
(N=248)	Trivia	7.03 (N=60)	8.11 (N=64) **
	General Knowledge	6.25 (N=60)	7.95 (N=64) ***

Notes: Entries are number of correct answers with a maximum of 15. *, **, *** indicates a significant difference between difficulty levels; #, ##, ### indicates a significant difference between Trivia and General Knowledge; at the 10%, 5%,1% level, two-sided t-test.

Chapter 4

Consider Others Better than Yourself: Social Decision-Making and Partner Preference in Borderline Personality Disorder

Abstract. Patients with Borderline Personality Disorder (BPD) suffer from interpersonal difficulties. They have been shown to be distrustful and yet involved in abusive relationships. In this study, we examined whether the perception of fairness and partner preference are altered by BPD. We employed a coalition formation game in which a participant could choose whether to interact in dyads or triads, resulting in exclusion or inclusion of a third potential interaction partner. Furthermore, triads received a higher endowment, such that dyads were not only unfair to one partner but also economically inefficient, as the participant reduced the overall amount of money available for distribution. Subsequently, we compared how participants predicted another person's game strategy (inclusive, exclusive or mixed) and rated its fairness, and which partner the participant would select. Our study demonstrates no differences in fairness perception but an alteration in partner preference of patients with BPD, which might contribute to unfavorable partner choices and impairments of interpersonal functioning in BPD.[25]

[25] This chapter was co-authored by Haang Jeung, Sabine C. Herpertz and Chistiane Schwieren. Published as Jeung, H., Vollmann, M., Herpertz, S. C., & Schwieren, C. (2020). Consider others better than yourself: Social decision-making and partner preference in Borderline Personality Disorder. *Journal of Behavior Therapy and Experimental Psychiatry*, 67, 101436.

4.1 Introduction

Borderline Personality Disorder (BPD) is characterized by severe and persistent impairment in interpersonal functioning (Zanarini et al. 2016). Compared to healthy controls, patients with BPD report less social support, more conflicts and less integration in their social networks (Jeung and Herpertz 2014; Beeney et al. 2018). Some of the interpersonal difficulties experienced by patients with BPD might result from the choices patients with BPD make in relationships. For instance, they prefer few but close and tense relationships (Stepp et al. 2009). With regard to partner preference, female patients with BPD tend to engage in romantic relationships with men who, in particular, have antisocial personality disorder (Bouchard et al. 2009),which is a risk for intimate partner abuse in itself (Ross and Babcock 2009). However, there are only a few experimental studies that examine the interpersonal choices made by patients with BPD.

In previous economic-exchange studies, patients with BPD have appeared to act mostly "rationally" in their own self-interest and independently from social signals (Jeung, Schwieren, and Herpertz 2016). Their motives for non-cooperation have been seen as the result of a lower trust in others (Unoka et al. 2009), lower trustworthiness (King-Casas et al. 2008) and negative reciprocity in the sense of a "tit for tat" type of response (King-Casas et al. 2008; Saunders, Goodwin, and Rogers 2015). However, trust and reciprocity are not the only social preferences that shape social decision-making. In one-shot encounters, strategic behavior concerning reciprocity should not matter (Falk and Fischbacher 2006). Nevertheless, fairness motives significantly affect human behavior independent of the strategic situation (Fehr and Schmidt 1999). Fehr and Schmidt described "inequity averse" individuals as making decisions so as to minimize inequity in outcomes. Previously, we have proposed that individuals with BPD show less inequity aversion than healthy individuals when it comes to accepting unfair offers from others (Jeung, Schwieren, and Herpertz 2016).

Interestingly, there are conflicting possibilities for interpreting a patient's sense of fairness. On the one hand, patients with BPD engage in altruistic punishment, i.e., they punish fairness violations of others even at their own cost, just as well as healthy controls (Wischniewski and Brüne 2013). On the other hand, they accept unfair offers by others more frequently than healthy controls (Polgár et al. 2014). This appears to be contradictory at first sight, but it can be explained by the unequal treatment of another person or oneself. Most of them having experienced abuse and neglect in childhood;

patients with BPD have been described as having strong emotional reactions, mainly anger, with the urge to defend the rights of others when they observe injustice (Cousineau and Young 1997). Three previous studies examined how injustice and unfairness towards others and themselves may affect social interactions in BPD (De Panfilis 2017; Lis 2017; Unoka 2017). Patients with BPD did not only report higher justice sensitivity as compared with healthy controls but were also more willing to behave in solidary in a lottery game (Lis 2017). At the same time, patients with BPD might perceive unfair treatment to the detriment of themselves as deserved as they expect a more negative outcome and anticipate fewer positive emotions in the case of a positive outcome for themselves. Similarly, patients with BPD rejected higher rates of fair offers and reported more anger and less happiness than healthy controls after fair offers in a modified ultimatum game (De Panfilis 2017). Moreover, 50% of patients with BPD rejected the total endowment offered by a proposer, whereas only 8% of healthy controls did so (Unoka 2017). In parallel, patients with BPD expected unfair treatment by others as they have repeatedly shown a bias towards the perception of exclusion, independent of their factual participation, and a higher intensity of negative emotions after exclusion in several Cyberball paradigms (Staebler et al. 2011; Renneberg et al. 2012). Yet, interaction is bilateral in nature. Up to now, it has not been studied whether patients with BPD make fair offers to their interaction partners once they are in control of inclusion and exclusion without future reciprocity concerns.

4.2 Overview and Hypotheses

It has been described before that individuals with BPD do not cooperate with others because they distrust them. In this study, we want to examine fairness in BPD independent of reciprocity. Therefore, we apply an economic-exchange game that has been previously introduced to study social exclusion by non-cooperative coalition formation in non-psychiatric subjects (Okada and Riedl 2005). More precisely, subjects in the game can choose whether to interact in dyads or triads. The dyads are characterized by excluding a third potential interaction partner and depriving this person of the possibility to earn money from the game. Furthermore, triads get a higher amount of money as an endowment, such that dyads are unfair to one partner and economically inefficient, as the subjects reduce the overall amount of money available for distribution.[26]

Hypothesis 1: *Given their difficulties with interpersonal trust and preference for a few close relationships, we hypothesize that patients with BPD, in contrast to healthy controls, prefer dyads over triads, and thus, the social exclusion of the third person.*

Hypothesis 2: *At the same time, due to their higher justice sensitivity towards others, patients with BPD offer their interaction partner a fair or even higher split of the endowment in contrast to healthy controls.*

Hypothesis 3: *As the literature indicates no judgment differences between the two groups, patients with BPD and healthy controls rate triads as fairer than dyads without differences.*

Hypothesis 4: *When it comes to partner preferences, however, due to the tendency to engage in unfair, abusive relationships, patients with BPD, but not healthy controls, choose an interaction partner with a preference for dyads.*

4.3 Materials and Methods

4.3.1 Participants and Recruitment

Twenty-six women with borderline personality disorder (BPD) and twenty-nine female healthy controls (HCs) matched for age (18–40 years) and educational background took part in the study. Demographic and clinical characteristics are shown in Table 4.1. Patients with BPD were outpatients of the Department of General Psychiatry, University Hospital Heidelberg, and healthy controls were recruited from the community through online advertisement and flyers. BPD exclusion criteria comprised neurological diseases, history of head trauma, current alcohol or drug dependence, acute and chronic psychotic disorders, bipolar disorders, a history of illicit drug use in the previous two months, alcohol dependence or abuse for the last two months as well as any medical condition that may affect central nervous system functioning. Only HCs without any lifetime psychiatric disorder, including BPD, and who were not taking any psychotropic medications, were enrolled.

[26] When talking about inefficiency, we refer to it in a monetary sense, reflecting forgone resources Okada and Riedl (2005). In our case, less money is distributable in the dyad compared to the triad.

Table 4.1. Demographic and Clinical Characteristics of Participants and Interaction Partners

a. Demographic and clinical characteristics of proposers

	HCs (*n* = 29)		BPD (*n* = 26)				
	M	SD	M	SD	*t*	*p*	
Age (in years)	23.3	4.6	24.6	3.2	−1.2	.2	n.s.
Education (in years)	11.1	1.1	11.2	1.1	−0.2	.9	n.s.
BSI GSI	.2	.2	1.8	.7	−11	0	**
BSL-23 Score	.2	.2	1.9	.9	−9.1	0	**
Currently employed	*n*	%	*n*	%	c^2	*p*	
	26	90	12	46	12.2	0	**
In a relationship	18	62	13	50	.81	.4	n.s.
Children	1	3.4	2	7.7	.48	.5	n.s.

b. Demographic and clinical characteristics of responders

	StudA (*n* = 60)		StudB (*n* = 60)				
	M	SD	M	SD	*t*	*p*	
Age (in years)	22.8	2.9	22.7	2.7	.2	.8	n.s.
Education (in years)	12	.4	11.9	0.5	.59	.6	n.s.
BSI GSI	.6	.7	.7	.6	-.4	.7	n.s.
BSL-23 Score	.6	.7	.7	.7	-.7	.5	n.s.
Currently employed	*n*	%	*n*	%	c^2	*p*	
	40	67	46	77	1.48	.2	n.s.
In a relationship	30	50	32	53	.13	.7	n.s.
Children	0	0	0	0			n.s.

Notes: For group comparison between individuals with borderline personality disorder (BPD) and healthy controls (HCs), and between students paired with individuals with BPD (StudA) and students paired with HCs (StudB), t-tests and $\chi 2$-tests with a level of significance of $p < 0.05$ were conducted for demographic and clinical characteristics, BSI = brief symptom inventory; BSL-23 = borderline symptom list; GSI = global severity index; n.s. = not significant. * = significant at $p < 0.05$ ** = significant at $p < 0.01$.

Further, we employed 120 co-players who came from our standard pool of student subjects. The experiment was organized and student subjects were recruited with the software hroot (Bock, Baetge, and Nicklisch 2014), an online recruitment system for economic experiments.

4.3.2 Clinical Assessment

All patients and healthy controls underwent clinical assessment with the Structured Clinical Interview for DSM-IV Axis I Disorders (SCID-I) (First, Spitzer, Gibbon, & Williams, 1995) and International Personality Disorders Examination, BPD section

(IPDE-BPD) (Loranger, Janca, and Sartorius 1997). All interviews were conducted by the principal investigator who is a trained senior psychiatrist. For all participants and student subjects, we collected commonly used measures of symptom severity, namely the Brief Symptom Inventory (BSI (Derogatis 1993)) and Borderline Symptom List (BSL-23 (Bohus et al. 2009)), in order to characterize our sample.

4.3.3 Procedure

After a complete description of the study to the patients and controls, written informed consent was obtained. The study was approved by the local Ethics Committee of the University of Heidelberg.

On the day of the experiment, we invited three focal participants (either patients with BPD or healthy controls) and six student subjects who were not aware that this study also involved patients. Additionally, we invited one extra student subject in case that one participant did not show up. If the focal participant did not show up, this student subject would participate instead, but her data was not included in the final analysis. A total of nine participants was required for each session to fulfill the randomization conditions. All participants and student subjects were asked to report their medication as prescribed. The majority of the patients with BPD disclosed taking psychotropic medication (69.3%). All participants and student subjects received a show-up fee of €4.00 plus earnings from the experiment, which is described below.

4.3.4 Tasks

The experiment was conducted in the laboratory of the Alfred Weber Institute of Economics, University of Heidelberg, and it consisted of two tasks.

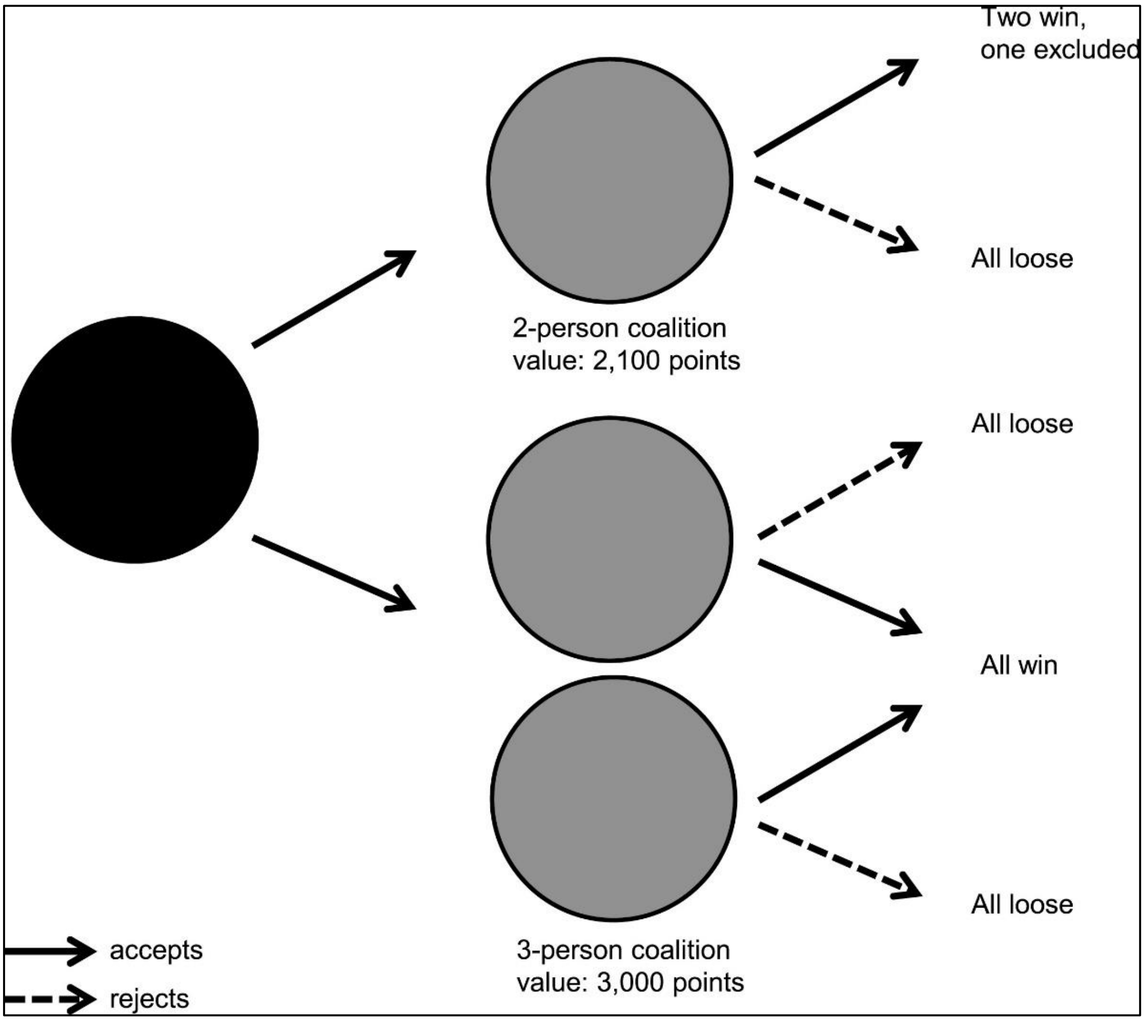
4.3.4.1 Task 1: Coalition Formation

As a laboratory task, we adapted a non-cooperative three-person coalition formation game with an ultimatum bargaining stage (Okada and Riedl 2005), which was conducted through the Zurich Toolbox for Ready-made Economic Experiments (z-Tree) (Fischbacher 2007), version 3.4.7. Each game consisted of three rounds. In order to prevent strategic and reciprocal behavior, one participant and two students were randomly assigned to a group of three for each round. The randomization was done in a way that a player was never matched twice with the same partner. The three players involved were called proposer, responder 1, and responder 2. Patients and healthy controls were always proposers, and students were always responders. Furthermore, the players did not know the identities of their fellow players.

The sequence of the play was the following (see also Figure 4.1): A proposer could choose between forming a dyad (two-person coalition) and excluding one of the responders or forming a triad (three-person coalition) and including all players. [27] A dyad had a value, i.e., an endowment, of 2100 points to be split into intervals of ten between the members of the dyad. A triad had a value of 3000 points to be divided between all three of them. Deciding on the dyad, therefore, would result in a monetary efficiency loss of 30% (or 900 points) compared to the triad. The proposer then had to divide the coalition value between herself and the chosen responder(s). If she chose the triad, she could choose how to split the money between herself and the two responders, such that both responders received the same number of points, and she could keep the rest. If she selected a dyad, she would propose one split only with the chosen responder and keep the rest, while the third person would not get any points. If a responder was chosen as a member of either the dyad or the triad, she could decide whether to accept or reject the proposal. If the chosen responder(s) accepted the proposal, all players would receive their shares; otherwise, nobody earned anything. If a potential responder was not chosen, she could not influence the outcome and had a zero payoff from that part of the game. The exchange rate from points to Euro was 500 points = €1.

[27] If the proposer chose the dyad, one random responder was excluded from the coalition.

Figure 4.1. Coalition Formation Game



Notes: In each round, proposers (either individuals with borderline personality disorder or healthy controls) were randomly assigned to two interaction partners (students). Proposers could choose to form a dyad (two-person coalition) or triad (three-person coalition) with one or both interaction partners. The dyad had a coalition value of 2100 points (endowment); the triad had a coalition value of 3000 points (endowment). Interaction partners who were excluded from the dyad were observers and could not participate in this round as a responder. Proposers could offer responders a split of the endowment. If (all) responders accepted the offer, the points were divided amongst proposers and responders as proposed. If (one of the) responders rejected the offer, no one received any points.

4.3.4.2 Task 2: Judgment, Fairness Ratings and Partner Preferences

In the second task, all participants watched the possible game histories of three separate proposer types in the same game that they had played before. It was explicitly explained that they should review the three distinct strategies carefully, one by one. After seeing the coalition decision, the participant had to indicate on a scale from 0 to 100% how high she estimated the chance that the proposer she was observing would choose a triad in the next round. Each game consisted of nine rounds; hence, the number of guesses was eight for each proposer type. In one condition ("inclusive strategy"), the proposer always chose triads, and in another condition ("exclusive strategy"), the proposer always chose dyads.

Additionally, there was a third condition in which the proposer randomly chose triads or dyads ("mixed strategy"). The strategies were presented in a randomized order.

After watching all game histories, participants were asked to rate the fairness of each proposer type on a scale from 0 (= not at all) to 4 (= very much). Finally, they had to select the proposer type they would prefer to play with in a game. To prevent participants from hedging (Blanco et al. 2010), their guesses and choices did not have any payoff consequences.[28]

4.3.5 Statistical Analyses

The data were analyzed with SPSS (Version 24; SPSS Inc., Cary, NS). For all analyses, five percent was chosen as the level of statistical significance. Categorical data were analyzed with nonparametric statistics ($\chi 2$ tests). All other comparisons were conducted with t-tests for independent samples and one-way analyses of variance (ANOVAs).

4.4 Results

4.4.1 Participants' Characteristics

Means and standard deviations for descriptive statistics and all self-report measures are presented in Table 4.1a for patients with BPD and HCs and in Table 4.1b for student subjects, respectively. Initially, 30 individuals with ASPD and 30 HCs were enrolled in the study. Five participants dropped out of the study not showing up (four BPD and one HC). Their participation was replaced by student subjects whose data were not analyzed further.

4.4.2 Hypothesis 1: Frequency of Coalition Decision

First, we checked whether patients with BPD preferred dyads over triads in comparison to healthy controls. About 30% of participants chose dyads over triads. Opposed to our hypothesis, there was no difference between patients with BPD (in total 30.8%) and healthy controls (in total 28.7%) in either round (all p's>0.05). See Table 4.2a for details.

[28] If guesses were incentivized, they would become part of the payoff relevant action space, which would give the participants the possibility to use their stated beliefs to offset the risk of adverse outcomes in the rest of the experiment (Blanco et al., 2010, p. 413).

Table 4.2. Coalition Decisions

a. Frequency of coalition decisions

		HC ($n = 29$)		BPD ($n = 26$)			
		n	**%**	***n***	**%**	***c²***	***p***
Dyads in round 1		6	20.7	7	26.9	.30	.59
Dyads in round 2		11	37.9	9	34.6	.07	.8
Dyads in round 3		8	27.6	8	30.8	.07	.8
Overall dyads		25	28.7	24	30.8	.08	.8

b. Bargaining behavior in dividing coalition values

		HC ($n = 29$)		**BPD (n = 26)**			
		M offer	**SD**	**Mean offer**	**SD**	**T**	**p**
Round 1	Dyads	46.1	8.6	57.1	18.9	−1,31	0.216
	Triads	31.0	11.2	31.5	8.2	−0,18	0.857
Round 2	Dyads	46.1	10.8	47.4	4.4	−0,35	0.73
	Triads	32.2	6.1	32.2	3.9	−0,01	0.996
Round 3	Dyads	40.2	13.6	46.2	5.3	−1,16	0.267
	Triads	27.7	7.9	32.1	3.9	−2,18	0.036
		Rejections	**%**	**Rejections**	**%**	***c²***	***p***
Round 1	Dyads	0	0.0	0	0.0	–	–
	Triads	4	17.4	2	10.5	0.4	0.53
Round 2	Dyads	0	0.0	0	0.0	–	–
	Triads	1	5.6	2	11.8	0.43	0.51
Round 3	Dyads	0	0.0	1	12.5	1.07	0.30
	Triads	4	21.1	0	0.0	4.25	0.04
		M win	**SD**	**Mean win**	**SD**	***t***	***p***
Round 1	Dyads	53.9	8.6	42.9	18.9	1.31	0.22
	Triads	25.5	16.6	29.0	13.6	−0.75	0.46
Round 2	Dyads	53.9	10.8	52.6	4.4	0.35	0.73
	Triads	32.0	12.4	30.6	13.4	0.32	0.75
Round 3	Dyads	59.8	13.6	47.0	19.5	1.56	0.14
	Triads	31.0	19.7	35.8	7.8	−0.94	0.35

Notes: For group comparison between individuals with borderline personality disorder (BPD) and healthy controls (HCs), t- and χ^2-tests with level of significant of $p<0.05$ were conducted for coalition decisions.

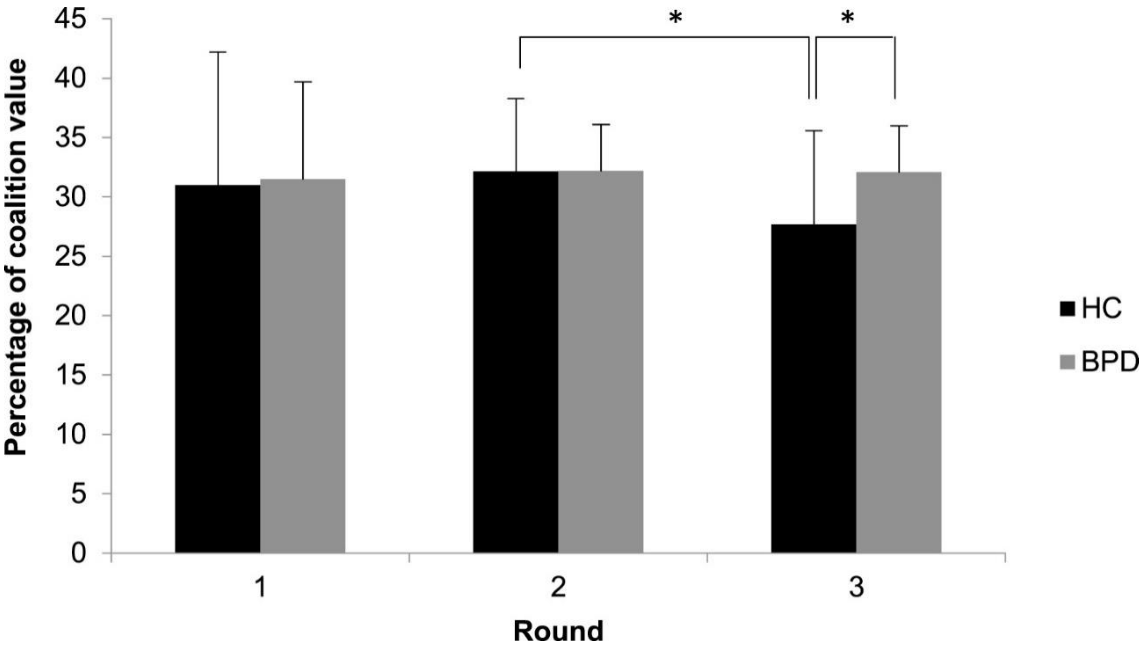
4.4.3 Hypothesis 2: Bargaining Behavior in Dividing Coalition Values

Next, we assessed whether patients with BPD and healthy controls offered their co-players fair splits of the endowment. On average, participants offered about 47% of the dyad value and about 30% of the triad value to each responder. See Table 4.2b for details.

Behavior in dyads: In dyads, there were no differences in mean offer, rejection rate, and mean proposer earnings within rounds and between patients with BPD and healthy controls (all p's>0.05). All offers but one were accepted when dyads were formed.

Behavior in triads: The bargaining behavior in dividing coalition values in triads is shown in Figure 4.2. While patients with BPD and healthy controls did not differ in mean offer, rejection rate and mean proposer earnings during rounds 1 to 2 (all p's>0.05), healthy controls offered significantly lower amounts (HC 27.7% vs. BPD 32.1%, $t_{30.20}$=−2.28, p=0.03) in round 3 (the final round) than patients with BPD.

Figure 4.2. Mean Relative Offer in Triads

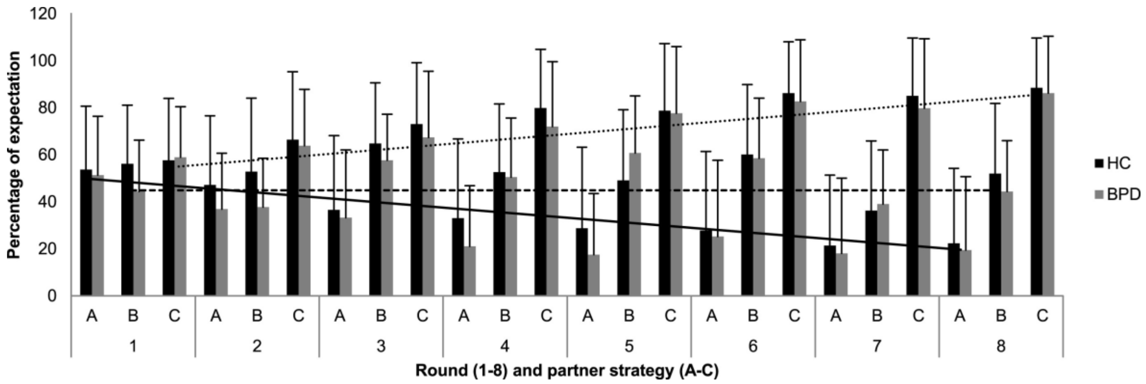


Notes: Comparison of offers to responders in percentage of endowment (coalition value: 3000 points) between individuals with borderline personality disorder (BPD, n = 26) and healthy controls (HCs, n = 29) in three rounds. Both responders received the same number of points. Error bars indicate standard deviation. * = significant at p<0.05, ** = significant at p<0.01.

4.4.4 Hypothesis 3: Judgment and Fairness Ratings

Judgment: We verified whether the groups expected the recurrence of a partner strategy similarly and found a non-significant group by strategy by round interaction, indicating that, in accordance with our hypothesis, the two groups did not differ in judgment. There were statistically significant main effects of strategy ($F_{2,89.36}$=80.93, p<0.01, η²=3175.775) and round ($F_{7,268.64}$=3.98, p=0.01, η²=547.244). Over all the groups, the participants rated the likelihood of a triad being chosen to be 30.8% by the exclusive strategy, 75.2% by the inclusive strategy and 51% by the mixed strategy. There was a statistically significant two-way interaction of strategy and round ($F_{14,456.98}$=15.56, p=0.01, η²=753.74). While participants rated a decreasing likelihood of a triad to be chosen by the exclusive strategy (from 52.5% in round 1–20.9% in round 8), they rated an increasing likelihood for the inclusive strategy (from 58.3% in round 1–87.4% in round 8) and a fluctuating likelihood for the mixed strategy (around 50% during all rounds). Figure 4.3 illustrates how participants anticipated the corresponding proposer type to select triads.

Figure 4.3. Partner Strategy for Triads in %



Notes: Comparison of partner strategy expectation for triads between individuals with borderline personality disorder (BPD, n=26) and healthy controls (HCs, n=29) as a function of partner strategies (A = choosing dyads in each round, B = randomly choosing either dyads or triads, C = choosing triads in each round) in eight rounds. Error bars indicate standard deviation. There was a statistically significant two-way interaction of strategy and round (p=0.01). While the likelihood of the partner strategy expectation for a triad decreased during rounds while the partner displayed the exclusive strategy (solid line), it increased while the partner displayed the inclusive strategy (dotted line), and it fluctuated by about 50% while the partner displayed the mixed strategy (dashed line).

Fairness ratings: With regard to fairness ratings, the group by strategy interaction was also not statistically significant. Overall, the main effect of strategy was statistically significant ($F_{2,106}$=39.72, p<0.01, η²=1.27). On a scale from 0 (= not at all) to 4 (= very much), patients with BPD and healthy controls rated inclusive strategies (M=3.4, SD=0.1) to be fairer than mixed strategies (M=2.4, SD=0.1) and mixed strategies to be fairer than exclusive strategies (M=1.4, SD=0.2, all p's<0.01).

4.4.5 Hypothesis 4: Partner Preference

There was a statistically significant difference in partner preference between patients with BPD and healthy controls ($c^2{}_{2,n=55}$=8.55, p<0.05). Table 4.3 presents the details. While the majority of healthy controls preferred partners with an inclusive strategy (triads; 86.2%), only half of patients with BPD (53.8%) opted for partners with an inclusive strategy. While the minority of healthy controls (3.4%) selected partners pursuing a mixed strategy, one-third of patients with BPD (30.8%) did. Partners with an exclusive strategy (dyads) were chosen by one-tenth of healthy controls (10.3%) and patients with BPD (15.4%).

Table 4.3. Preference for Partner Strategies

	HCs	**BPD**	**Total**
Dyads	3	4	7
Mixed	1	8	9
Triads	25	14	39
Total	29	26	55

Notes: BPD = borderline personality disorder; HCs = healthy controls.

4.5 Discussion

In this study, we wanted to examine whether patients with BPD act fairly in cases where no reciprocity concerns are relevant. Against our expectations, patients with BPD chose inclusion as often as healthy controls and, thereby, form economically efficient and fair coalitions, i.e., they chose triads more often than dyads and offer their responders amounts of monetary units (MUs) similar to the amount offered by healthy controls. The observation that perception and execution of fairness seem to be equal in patients with BPD and healthy controls was also made when patients with BPD a) predicted and b) rated their partner's (un)fairness, as did healthy controls. Notably, despite their equal judgment, nearly half of the patients with BPD would rather choose an unfair interaction partner (i.e., someone who pursues either the two-person or mixed strategy), whereas the majority of healthy controls would choose a fair interaction partner. In other words, patients with BPD tend to rush headlong into disaster with their eyes wide open.

Unfavorable choices were also made in an ultimatum game in which BPD responders accepted low offers at significantly higher rates than healthy responders

(Polgár et al. 2014). This could be, in accordance with the *homo economicus*,[29] interpreted as a strategy for maximal monetary gain on the one hand (Jeung, Schwieren, and Herpertz 2016) and submissive acceptance of any (mal)treatment on the other hand. Matching the latter interpretation are observations that patients with BPD reject fair and generous offers of others (De Panfilis 2017; Unoka 2017).

The unfavorable partner preference of patients with BPD might be grounded in their negative and instable self-image due to childhood experiences of abuse or neglect (Zanarini et al. 1997). These often traumatic childhood experiences and the frustration of basic childhood needs (e.g., secure attachment) have been linked to impairments in mentalizing (the ability to understand their own and others' mental states), which impact relationships negatively (Fonagy and Bateman 2008).

In prior studies, mentalizing abilities in patients with BPD have been predominantly tested by questionnaires, stories and emotional facial stimuli (for a review, see Jeung and Herpertz 2014). These behavioral studies, despite some inconsistencies, demonstrated that patients with BPD have lower mentalizing abilities in complex tasks. For instance, they had difficulties in recognizing the intentions of others in video clips (Preißler et al. 2010). Interestingly, our results suggest that patients with BPD prefer unfavorable partners even if and when they accurately perceive the intentions underlying others' behaviors.

Another result of negative childhood experiences is the interference with the normative developmental process of integrating disparate mental representation of the self, relationships to others and the world (Kernberg 1975); hence the development of early maladaptive schemas (self-defeating emotional, cognitive and behavioral patterns) (Young, Klosko, and Weishaar 2003). A cluster of schemas and coping styles is called a schema mode. In healthy individuals, schema modes are mild, flexible mind states. Conversely, for individuals with a personality disorder, schema modes are severe, rigid mind states. Patients with BPD are particularly characterized by a disorder-specific schema mode model (Young, Klosko, and Weishaar 2003; Arntz, Klokman, and Sieswerda 2005) of which the punitive parent mode, i.e., self-hatred, shame, self-devaluation and self-punishment, explains the behavior of submissive acceptance of maltreatment by patients in our experiment. Indeed, patients with BPD have been found

[29] "Homo economicus" is used in economics as a benchmark model of theoretical ideal of the consistently rational and self-interested agent.

to evaluate themselves even more negatively after negative feedback, whereas healthy controls did not change their self-evaluation (Korn et al. 2016).

Similarly, disagreement with others' opinions provoked shame in patients with BPD but not in healthy controls (Jeung et al. 2018). However, in order to prevent participants from hedging monetary income (Blanco et al. 2010), our study design did not include payoff consequences in the second task and, thus, does not allow for straight-forward conclusions concerning whether patients with BPD accept unfair offers. As a result, the transfer from our findings of partner preference to actual unfavorable partner choice in BPD is preliminary.

In the original set-up, the coalition formation game was developed to study inefficiency and social exclusion in multilateral bargaining (Okada and Riedl 2005). About one-third of responders were excluded from bargaining and earned nothing. Similarly, we found only 30 percent of the BPD and healthy proposers opted for dyads in our experiment. In accordance, it has been previously shown that patients with BPD engaged as much as HCs in altruistic punishment (Wischniewski and Brüne 2013) and, exceedingly, showed solidarity with their unlucky co-players (Lis 2017), which might be indicators of other-orientated empathic concerns when fairness is violated. In line with this, individuals with clinically relevant BPD features show a higher victim sensitivity than individuals without clinically relevant BPD features (Lis et al. 2018).

Our study employed three one-shot encounters in which each game round can be considered a first and only interaction with an unfamiliar partner. In one-shot encounters, strategic behavior with the intention of reciprocity should not matter (Falk and Fischbacher 2006). In contrast, multiple encounters require trust and reciprocity, which patients with BPD seem to lack (Jeung, Schwieren, and Herpertz 2016). In the one-shot encounters, there were no differences in bargaining behaviors between patients with BPD, patients with major depressive disorder and healthy controls in trust and punishment games (Preuss et al. 2016). In our multi-round encounter study, patients with BPD behaved even fairer than healthy controls in the end round because, unlike healthy controls, they did not reduce their offer. The behavior of the healthy proposers was in line with the behavior of healthy volunteers in previous economic research studies where so-called "free riding" is often observed to increase towards the end in finitely repeated games, resulting in a large drop in the average level of investment in the final round (Andreoni 1988).

In view of the literature on the sensitivity of patients with BPD to unfairness, we have to see this persistent fair behavior as a manifestation of social norms which patients with BPD follow in an inflexible manner. This trait has been described before as “fierce determination for justice to prevail in all circumstances” (Bateman and Krawitz 2013).

4.5.1 Strengths and Limitations

This is the first study to assess equal and efficient coalition formation as well as judgment, fairness perception and partner preference in patients with BPD. To increase the ecological validity of our procedure, participants engaged with human interaction partners. Nevertheless, we would like to address several limitations of our study.

Since there are growing concerns regarding the replication of laboratory findings in clinical psychological research (Tackett et al. 2017), our sample size of 26 patients with BPD and 29 healthy control participants might have been too small. As mentioned in the introduction, three preliminary studies (De Panfilis 2017; Unoka 2017) with similar sample sizes have been carried out before, which fit into the framework of this study. However, further replication with current and similar paradigms is needed before strong conclusions can be drawn.

Secondly, like many studies on BPD, we only tested female patients with BPD. The majority of whom—typical of a naturalistic sample—patients had psychiatric comorbidities and took psychotropic medication (Tomko et al. 2014). Comparing the scores of the BSL-23 for disorder-specific symptoms with those of the validation study (Bohus et al. 2009), we found comparable scores.

Furthermore, we did not include a patient control group, so we cannot draw conclusions specific for BPD. However, we matched our healthy controls on demographic variables. Additionally, our patients with BPD showed significantly higher values in the average total score (Global Severity Index) on the BSI, which reflected a moderate level of psychiatric symptomatology and was similar to scores obtained in other studies of BPD (Linehan et al. 2002; Rizvi et al. 2011). Finally, the interaction partners of the proposers were all female students who did not undergo clinical assessment. Still, this should not be a problem since they are not our focus of interest.

4.6 Conclusion

Our study provides preliminary evidence that the judgment of patients with BPD of what they deserve from others seems to be altered, whereas the perception and execution of fairness are unaffected. The former finding mirrors the previously described unfavorable partner choice of patients with BPD, presumably due to a deeply negative self-image and low self-esteem. While clinical descriptions focus on dysfunctional relationships of patients with BPD with their significant others, recent economic-exchange games point to impairments in interaction also with unfamiliar partners. In the long term, research based on the current findings may help to stop the circle of negative self-image and low self-esteem that appears to underlie particularly rigid moral behavior to their own disadvantage. For instance, one interventional approach could be to foster self-compassion (Feliu-Soler et al. 2017) as part of therapeutic interventions.

Chapter 5

Performance and Mood of Depressed Workers and Coworkers in Different Work Contexts

Abstract. Depression in the workplace is a significant factor for reduced personal well-being and productivity. Consequently, this has negative effects on the economic success of the companies in which depressed people are employed. In addition, the economy has to deal with the significant burden of this illness on the health system. In this paper, we investigated how different working contexts —working in a group or individually— influenced depressed individuals towards higher or lower well-being and productivity. We examined this using a laboratory experiment. In this setting, we were also able to analyze how, in turn, a depressive individual impacted the productivity and affective situation of their workgroup, reflecting the company perspective. The experimental design mimicked the very basic processes of a workplace. We used two distinct samples: subclinically and clinically depressed, both working together with healthy controls. As expected, we found generally lower performance in the clinically depressed sample, but in the subclinically depressed sample, we only found this in the individual work context. In contrast to our expectations, the performance of subclinically depressed individuals working in groups with healthy controls was even higher than that of healthy controls in homogenously healthy groups. The performance of the entire group with a depressed member was lower for the sample with clinically manifested depression, while the performance of groups with a subclinically depressed participant was significantly higher than the performance of homogeneously non-depressed control groups. We discuss our results with a focus on the design of workplaces to both re-integrate clinically depressed employees and prevent subclinically depressed employees from developing major depression.[30]

[30] This chapter was co-authored by Christiane Schwieren, Margarete Mattern and Knut Schnell.

5.1 Introduction

Depression in the workplace is a major factor for the reduction of personal well-being and productivity. Unipolar depressive disorders are widespread (Bromet et al. 2011) and as Mathers and Loncar (2006) pointed out, they will most likely become the second leading cause of Disability-Adjusted Life Years (DALYs) by 2030 behind HIV/AIDS. One reason for this is the failure to support and re-integrate individuals with depression into the workforce, which has a negative effect on the course of the disorder (Kim and Knesebeck 2016; Zülke et al. 2018). From the management perspective within a company, depression means a significant reduction of performance and presence in the workplace (Kessler and Frank 1997; Druss, Rosenheck, and Sledge 2000; Verow and Hargreaves 2000; Stewart et al. 2003; Lerner and Henke 2008; Plaisier et al. 2010), which can cause a loss of knowledge holders who are pivotal for the success of entire projects (Whooley et al. 2002). In the larger scope, depression derived disability is also a problem for national economies by means of expenses for treatment costs and sickness benefits. Sobocki et al. (2006) estimated that depression resulted in a total annual cost of 118 billion Euro in 2004 for Europe.

To avoid the perceivable consequences of depressive disorders, companies should be able to take measures to i.) prevent the incidence of depressive disorders in their employees and ii.) avoid relapse of depression in workers after treatment. To do so, it is essential to know how the work environment can be designed to reduce stress, positively influence the mood and productivity of employees with a risk of depression and, similarly, prevent those with prevalent milder forms of depression from dropping out of the workforce. To address these questions, we apply an experiment to test how the *interpersonal context* of the workplace—working in a group or individually—impacts the mood and productivity of depressed and non-depressed workers and the teams they work in.

Current research suggests that the typical performance reductions found in people suffering from depression are caused by an interaction of reduced cognitive resources (e.g., Rock et al. 2014; Ahern and Semkovska 2017) and altered motivational schemas with indifference or a shift from a behavioral approach towards avoidance (Radke et al. 2014; Yang et al. 2014; Struijs et al. 2017). The associated cognitive schema has been described by Beck's cognitive model of depression (Beck 1967).[31] Following this model and the current findings, excessive work-related demands, perceived reduction in performance and resulting negative feedback should increase the aversive perception of the work context, reinforce depressive assumptions and result in the increased severity of the depressive syndrome. Thus, depressed individuals should be more open to react to the negative feedback of group members in comparison to positive feedback, which initiates a vicious cycle.

It has been demonstrated that performance in depressed and non-depressed workers generally depends on job characteristics like occupational status and psychosocial working conditions (Böckerman and Ilmakunnas 2008; Marklund, Bolin, and Essen 2008). Plaisier et al. (2012) found that high job support, high job control, fewer work hours, being self-employed and having a high-skilled job positively affected psychopathology, absenteeism and work performance. Another factor that can influence individual well-being and productivity is the *interpersonal context* of the workplace. According to the mentioned models of depression, interaction with other employees can either be a source of resilience or a stress factor causing further mood dysregulation and subsequent loss of productivity. Therefore, the core question of this research concerns whether or not working in groups pushes depressed employees further into a negative mood state and how the productivity of the whole group is affected by the depressed team member. From the perspective of employers, understanding the effects of such interactions could help prevent the initial emergence of major depression and relapse during reintegration attempts and the resulting enduring loss of the workforce.

We are the first to systematically study in a controlled experimental context how the mood and productivity of a depressed individual are influenced by the working context in a group and how, in turn, a depressive individual affects the productivity and affective situation of the group. Moreover, we compare this scenario to the same task done individually to be able to give recommendations to managers on how to optimize the interpersonal work context for depressed employees. In addition, we focus on two situations to be managed from a company perspective: prevention of dropout from the

[31] Here depressive cognition is characterized by a negative view of oneself, the people around and the future. A contemporary update of this model by Beck (2008) included three elements of dysfunctional cognition: biased attention, biased processing and biased memory. All these biases are supposed to create a feedback loop which supports the initiation or sustainment of an episode of depression (Disner et al. 2011).

workforce caused by developing depression and avoiding the relapse of reintegrated workers. Thus, we look at two distinct samples i.) individuals on the brink of depression ("Subclinically Depressed") and ii.) with depression under treatment ("Clinically Depressed"), both working together with healthy controls. As in real life working groups, the group members were not informed if one of their colleagues was suffering from depression.

The structure of the remaining paper is as follows. Section 2 presents the experimental design. Section 3 contains the hypotheses and section 4 presents the results divided by the two samples. The last section discusses the results and contains concluding remarks.

5.2 Experimental Design

5.2.1 Setup

To understand how the interpersonal context—groupwork ("Group") and individual work ("Single")—influences the performance and emotional state of individuals on the brink of depression ("Subclinically Depressed") and with depression under treatment ("Clinically Depressed"), we used two distinct samples. On the one hand, we used a standard student sample ("Subclincial Sample"), which underwent an on-site self-classification based on the Beck Depression Inventory-II (Beck, Steer, and Brown 1996). Depending on the result, participants were categorized as "Subclinically Depressed" or "Healthy Control." On the other hand, we invited a sample consisting of clinically diagnosed major depressive disorder (MDD) patients that were close to being reintegrated into the workforce and a matched healthy control group ("Clinical Sample").

In the "Group" setting, each group consisted either of four "Healthy Controls" or of three "Healthy Controls" and one "Subclinically/Clinically Depressed" participant. In the "Single" setting, participants worked individually on the same task. Table 5.1 summarizes all four experimental treatments. This design fulfills the purpose of isolating the direct effect of depression on performance from the effect of performing in a group setting, including social evaluation by peers. The categorization of depressed or healthy (i.e., not depressed) was not disclosed to the participants to avoid the potential effects on behavior.

Table 5.1. Experimental Treatments

	Group	**Single**
Subclinically/Clinically Depressed	3 Healthy Controls 1 Depressed Participant	1 Depressed Participant
Healthy	4 Healthy Controls	1 Healthy Participant

5.2.2 Participant Recruitment and Depression Assessment

Participants in the “Subclinical Sample” were recruited from the subject pool of the experimental laboratory of Heidelberg University (AWI-Lab). The experiment was organized and the sample recruited with the software hroot (Bock, Baetge, and Nicklisch 2014). The participants underwent an on-site self-classification with the Beck Depression Inventory-II (BDI-II) (Beck, Steer, and Brown 1996). Based on this, participants were categorized as “Subclinically Depressed” or “Healthy Control.” The cutoff was chosen based on the standard norms where scores >13 indicate at least a mild depressive syndrome. Even if the BDI-II was not designed for diagnostic purposes, its classification accuracy has been shown in multiple studies (Steer et al. 1999; Lasa et al. 2000; Marton et al. 2017), and it is regularly used to categorize student samples (e.g., Arens et al. 2018).

Participants in the “Clinical Sample” were recruited in two different ways. “Clinically Depressed” participants were recruited from the Clinic of General Psychiatry of the Heidelberg University Hospital or the Asklepios Medical Center Göttingen and participated in agreement with their attending physician. Thereby, major depressive disorder (MDD) diagnosed by expert raters according to ICD 10 criteria (F32.1, F32.2, F33.1 or F33.2) had to be the main diagnosis. Exclusion criteria comprised a set of other mental disorders.[32] Other psychiatric comorbidities did not constitute exclusion criteria. Furthermore, participants had to fulfill the following inclusion criteria. They had to be between 20 - 60 years old and worked at least part-time no longer than twelve months before the inpatient or day-clinic treatment. Furthermore, they had to be close to being released from a stationary treatment or a day-clinic treatment.

[32] The set of mental disorders were comprised of organic mental disorders, addictions (except nicotine), schizophrenic disorders, bipolar disorders, post-traumatic stress disorders, personality disorders, attention deficit syndromes, autism disorders and impairment of intelligence.

The "Healthy Control" participants were recruited by announcements on the internet and at different locations within the city of Heidelberg. To match these participants with the treatment group, they had to meet the same inclusion criteria (age, professional status, gender) except that they never suffered from a psychiatric illness. To ensure that the potential participants met these conditions, a student assistant conducted a short telephone screening. The screening questions were based on the short form of the Camberwell Assessment of Need (CAN) (Phelan et al. 1995), from which those were chosen that could detect all mental disorders defined as exclusion criteria.

During the recruitment and the conduction of the experiment, all requirements to ensure medical confidentiality were met and documented. The local Ethics Committee of Heidelberg University approved the study (AZ Schwi 2018/1-2).

5.2.3 Procedure Group Setting

In the "Group" setting, the procedures differed slightly between the two samples ("Subclinical" and "Clinical"). For the "Subclinical Sample," the participants arrived at the laboratory and were randomly placed according to the standard procedure of the AWI-Lab. The group matching was made by an algorithm based on the BDI-II Scores of the participants that were assessed computer-based at the beginning of each session. For the "Clinical Sample," all participants assembled in front of the Clinic of General Psychiatry of the Heidelberg University Hospital (respectively Asklepios Clinic Göttingen). Afterward, they were collectively guided to the experimental laboratory, where they received an identification (ID) number, which ensured that the questionnaires from the pre-screening could be connected to the entries in the group task on the computer. For the experiments in Göttingen, we used a mobile laboratory consisting of four laptops and cubicles, which matched in size and appearance with the AWI-Lab to ensure comparability. Clinically depressed participants received a special ID number, which made it possible to identify them later in the data set. The distribution of the numbers and the numbers themselves were designed in a way that complete anonymity was ensured. After arrival at the laboratory, participants could choose a computer, and the experimenter started the experiment. Since the participants in the "Clinical Sample" were not registered in the participant pool of the AWI-Lab, written informed consent was obtained before the experiment started.

The experiment itself was divided into four parts. In the first part, the participants either answered the BDI ("Subclinical Sample") or entered their ID number ("Clinical

Sample"). Thereafter, the instructions were displayed on the screen. The instructions were the same for all participants and provided them with complete information for the upcoming task.

In the second part, the participants got to know the task by playing two practice periods that allowed participants to become acquainted with the setup (cf. Brüggen and Strobel 2007). The results of this had no impact on their payoff, and they were not analyzed.

Afterward, 12 payment periods followed, which were identical to the practice periods, only differing in the time limit. The time to solve the task in the practice periods was shorter to avoid a strong learning effect.

Finally, all participants had to fill out a demographic questionnaire. When the participants had finished the computer tasks and filled out the questionnaire, they received their payment and left the laboratory. In addition, participants in the "Clinical Sample" filled out several psychological and psychiatric questionnaires for clinical assessment, including the Holt and Laury risk elicitation task (Holt and Laury 2002), on a pen and paper basis.[33]

The task itself was structured in the following way. Participants had to solve simple calculus problems, i.e., adding or subtracting numbers by one or two digits. These mathematical tasks are commonly used in experimental research to evoke real effort without strong demands on intellectual abilities (Sutter and Weck-Hannemann 2003; Eriksson, Poulsen, and Villeval 2009; Dohmen and Falk 2011). Each period was started by a five second countdown followed by a 60 second time limit to answer as many calculus problems as possible. After the end of the 60 seconds, the screen immediately changed, and no further entries were possible. On the next screen, the participants were informed of several aspects of performance. In the "Group" setting, they learned i.) the total number of correct answers by the group, ii.) their own number of correct answers, iii.) how many correct answers the other group members had individually achieved, iv.) their own profit and v.) the group profit. The group profit increased based on the number of correct calculations by all group members following a step function. Each correct calculation added 5 cents to the group profit with a 50 cent bonus for each tenth correct

[33] A complete list of the questionnaires used and the Holt and Laury task can be found in the Online Appendix. Three "Clinically Depressed" and four "Healthy Controls" did not fill out their questionnaires.

calculation.[34] The total group profit was equally shared among all group members (own profit = group profit/four). On the next screen the participants had to express how satisfied they were with their own performance (very dissatisfied - very satisfied), how they felt (unpleasant - pleasant) and how high their feeling of arousal was (not aroused - aroused), based on a 9-point SAM (Self-Assessment Manikin) scale (Bradley and Lang 1994). On the next screen, the participants received a detailed list about the performance of all group members and were asked to evaluate them on the same 9-point scale as they evaluated their own performance (very dissatisfied - very satisfied). Then, on the next screen, the participants received feedback on their evaluation from the other group members and again were asked to state their feelings (unpleasant - pleasant) and their feeling of arousal (not aroused - aroused). On the last screen, they were informed about their current total profit.

The duration of the experiment was about 45 minutes. The participants in the "Clinical Sample" filled out the additional questionnaires directly after the experiment. They also had the chance to hand in the questionnaires later, since clinical depression also affects the ability to concentrate and potentially the stress level after such an experiment. Therefore, it would have been irresponsible to keep them in the laboratory for extended periods of time. We allowed both the "Clinically Depressed" and the "Healthy Control" to complete the questionnaires later so that the anonymity of the depressed participants was kept. Nevertheless, all "Healthy Control" participants filled out the questionnaires directly after the experiment. The computer program used for the experiment was the Zurich Toolbox for Readymade Economic Experiments (z-Tree) (Fischbacher 2007), which is broadly used by experimental economists.

5.2.4 Procedure Single Setting

In the "Single" setting, the participants did not interact with other participants and, therefore, received no information or feedback from others. Otherwise, the setting followed the same procedure as mentioned above. The individual measurements of the "Clinically Depressed" group did not take place in the experimental laboratory but in a mobile laboratory consisting of a laptop and a cubicle, which matched in size and appearance with the one in the experimental laboratory.

[34] This additional bonus is meant to reduce the decline in motivation towards later rounds.

5.2.5 Demographic and Clinical Characteristics

5.2.5.1 Subclinical Sample

In total, 339 students participated in our study; 89 had a BDI-II Score above 13 and were categorized as depressed. All demographics and clinical characteristics can be seen in Table 5.2. The sample sizes in the different settings can be seen in Table 5.3.

Table 5.2. Demographic and Clinical Characteristics of Subclinical Sample

	Subclinically Depressed	***Healthy Control***	
N	89	250	
Age (years)	22.71 (3.95)	23.29 (3.94)	n.s.
Women (%)	67.46	50.00	***
Economist (%)	27.19	27.20	n.s.
BDI-II Score	19.08 (5.01)	6.16 (3.87)	***

Notes: Age: age in years; Women: percentage of women; Economist: percentage of participants having an economics related study field; BDI-II Score: Beck Depression Inventory Score. Two-sided t-test. Standard deviation reported in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5.3. Number of Observations in the Subclinical Sample by Treatments

	Group	**Single**
Subclinically Depressed	31	58
Healthy	30	37

5.2.5.2 Clinical Sample

In total, we recruited 132 participants; 24 were patients with a treated MDD (“Clinically Depressed”). We only recruited female patients to avoid having to control for gender effects in the group composition of the smaller “Clinical Sample.” The prevalence of MDD is much higher among women (Jacobi et al. 2004), and it would have been difficult to balance the composition of workgroups with regard to the sex of the patients included. All demographics and clinical characteristics can be seen in Table 5.4. The sample sizes in the different settings can be seen in Table 5.5.

Table 5.4. Demographic and Clinical Characteristics of Clinical Sample

	Clinically Depressed	***Healthy Control***	
N	24	108	
Age (years)	42.00 (9.67)	35.07 (10.65)	***
High Education (%)	66.66	82.69	*
BDI-II Score	25.05 (10.13)	4.69 (5.19)	***

Notes: Age: age in years; High Education: percentage of people with at least a high school diploma ("Abitur"); BDI-II Score: Beck Depression Inventory Score. Two-sided t-test. Standard deviation reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 5.5. Number of Observations in the Clinical Sample by Treatments

	Group	**Single**
Clinically Depressed	11	13
Healthy	14	19

5.3 Hypotheses

Building on the empirical and theoretical foundations, we derived the following hypotheses. First of all, it can be expected that the overall performance of depressed participants is lower than that of healthy participants (Goldberg and Steury 2001) caused by cognitive deficits and a lack of energy and motivation. The lower performance of depressed individuals will lead to negative feedback by the remaining group members, starting a vicious cycle. In our experiment, that process should lead to lower and decreasing performance, lower satisfaction with one's own performance, lower well-being and higher negative arousal for the depressed participants over time.

Hypothesis 1: *The Group Context negatively influences the performance of depressed participants, as compared to the Individual Context and healthy controls.*

Hypothesis 2: *The Group Context leads to a decrease in performance over time for depressed participants, stronger than the Individual Context and stronger than for healthy controls.*[35]

Hypothesis 3: *The Group Context negatively influences satisfaction in depressed individuals, as compared to the Individual Context and healthy controls.*

Hypothesis 4: *The Group Context negatively influences emotional states in terms of the arousal and well-being of depressed individuals, as compared to the Individual Context and healthy controls.*

5.4 Results

5.4.1 Subclinical Sample

5.4.1.1 Performance

As expected, we found that the average performance of the "Subclinically Depressed" was significantly lower compared to the "Healthy Control" ($p < .001$, one-sided t-test). Comparing the performance between "Group" and "Single" treatment, we found a positive effect of the "Group" treatment on the individual performance for both types (Table 5.6).

Comparing the performance between the "Subclinically Depressed" and "Healthy Control" we found no difference between them in the "Group" treatment but observed significantly lower performance of the "Subclinically Depressed" compared to the "Healthy Control" in the "Single" treatment (Table 5.6).

Table 5.6. Individual Performance by Treatment

Correct Calculations	Group (G)	Single (S)	Diff. G – S
Subclinically Depressed (D)	13.65 (3.85)	12.67 (3.99)	0.98***
Healthy Control (H)	13.71 (4.13)	13.30 (3.53)	0.41**
Diff. D – H	-0.06	-0.64***	

Notes: Average sum of correct calculations in the incentivized periods compared between treatments and types. Two-sided t-test results concern between-group differences. Standard deviation reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

[35] We also expected a decrease in performance for healthy controls, as over the rounds, motivation might decline.

We ran a panel regression to see how the performance of the participants developed over time. We found that the performance of the "Subclinically Depressed" was increasing and not decreasing over time (Table 5.7, (1) – (4)). This increase was even higher in the "Group" treatment (Table 5.7, (3)-(4)). The performance of the "Healthy Controls" was also increasing over time (Table 5.7, (1)-(2) & (5)-(6)). Furthermore, we found that the performance of the "Healthy Controls" that were grouped with the "Subclinically Depressed" participants was even higher than for those grouped with other "Healthy Controls," displaying an additional increase over time (Table 5.7, (5)-(6)).

Table 5.7. Panel Regression on Correct Calculations in the Subclinical Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Subclinically Depressed		Healthy Control	
Dep. Variable			Correct Calculations			
Group Treatment	-0.0134 (0.602)	0.154 (0.607)	0.150 (0.834)	0.104 (0.843)	0.178 (0.611)	0.377 (0.616)
Period	0.124*** (0.0275)	0.124*** (0.0275)	0.0706** (0.0287)	0.0706** (0.0287)	0.124*** (0.0275)	0.124*** (0.0275)
Group Treatment x Period	-0.0133 (0.0304)	-0.0133 (0.0304)	0.0976* (0.0530)	0.0976* (0.0531)	-0.0358 (0.0329)	-0.0358 (0.0329)
Sub. Depressed	-0.183 (0.734)	0.270 (0.763)				
Sub. Depressed x Group Treatment	-1.065 (1.042)	-1.388 (1.050)				
Sub. Depressed x Period	-0.0533 (0.0396)	-0.0533 (0.0396)				
Sub. Depressed x Group Treatment x Period	0.111* (0.0609)	0.111* (0.0609)				
Healthy Control w/ Sub. Depressed	1.229** (0.493)	1.284*** (0.476)			0.791 (0.518)	0.865* (0.503)
Healthy Control w/ Sub. Depressed x Period					0.0515** (0.0257)	0.0515** (0.0257)
Constant	12.25*** (0.501)	12.44*** (1.304)	12.07*** (0.539)	11.42*** (2.631)	12.25*** (0.501)	12.84*** (1.427)
Observations	4,068	4,068	1,068	1,068	3,000	3,000
Controls	No	Yes	No	Yes	No	Yes
Number of Subjects	339	339	89	89	250	250

Notes: We report GLS coefficients with standard errors clustered on the individual level in parentheses using a random effects model over 12 periods. The dependent variable is the sum of correct calculations. Controls include dummy variables for male, age and participants having an economics related study field. *** p<0.01, ** p<0.05, * p<0.1

Looking at the Group Performance (i.e., the sum of the individual performances within a group), we found that the "Subclinically Depressed Group" had a significantly higher performance compared to the "Healthy Control Group" (p<0.001, one-sided t-test). This result was caused by the significantly higher performance of the "Subclinically Depressed" and "Healthy Controls" in this setting as compared to the performance of the "Healthy Controls" playing only with other "Healthy Control" ($p<0.05$ and $p<0.01$, two-sided t-test, Appendix Figure A 5.1).

5.4.1.2 Satisfaction

"Subclinically Depressed" indicated, on average, a significantly lower level of satisfaction with their own performance compared to "Healthy Controls" ($p<0.001$, one-sided t-test). In addition, we found a positive effect of the "Group" compared to the "Single" treatment on satisfaction for both types (Table 5.8).

Table 5.8. Individual Satisfaction by Treatment

Satisfaction	Group (G)	Single (S)	Diff. G – S
Subclinically Depressed (D)	5.33 (2.12)	4.74 (1.95)	0.59***
Healthy Control (H)	6.25 (2.06)	5.99 (1.84)	0.26**
Diff. D – H	-0.93***	-1.25***	

Notes: Average sum of satisfaction compared between treatment and types using a 9-point Likert Scale from 1 – very unsatisfied to 9 – very satisfied. Two-sided t-test results concern between-group differences. Standard deviation reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

5.4.1.3 Affective States

On average, "Subclinically Depressed" indicated a significantly lower level of well-being compared to "Healthy Controls" ($p<0.001$, one-sided t-test). The "Subclinically Depressed" in the "Group" treatment indicated a significantly higher level of well-being than those in the "Single" treatment (Table 5.9). As expected, we found that the "Subclinically Depressed" stated significantly lower levels of well-being compared to "Healthy Controls" in both treatments (Table 5.9).

Table 5.9. Well-Being Before Feedback by Treatment

Well-Being	Group (G)	Single (S)	Diff. G – S
Subclinically Depressed (D)	5.06 (1.84)	4.57 (1.77)	0.48***
Healthy Control (H)	6.23 (1.87)	6.12 (1.55)	0.11
Diff. D – H	-1.17***	-1.51***	

Notes: Average sum of well-being before feedback compared between treatment and types using a 9-point Likert Scale from 1 – unpleasant to 9 – pleasant. Two-sided t-test results concern between-group differences. Standard deviation reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

The "Subclinically Depressed" indicated a significantly higher average level of arousal compared to "Healthy Controls" (p<0.001, one-sided t-test). We also confirmed that the "Subclinically Depressed" stated significantly higher levels of arousal compared to "Healthy Controls" in the "Group" treatment but not in the "Single" treatment (Table 5.10). Comparing the arousal between the "Subclinically Depressed" and "Healthy Controls," we found higher arousal in the "Group" treatment but no difference in arousal between the "Subclinically Depressed" and "Healthy Controls" in the "Single" treatment (Table 5.10).

Table 5.10. Arousal Before Feedback by Treatment

Arousal	Group (G)	Single (S)	Diff. G – S
Subclinically Depressed (D)	5.47 (1.98)	5.10 (2.16)	0.36***
Healthy Control (H)	4.73 (2.10)	5.10 (2.15)	- 0.37***
Diff. D – H	0.65***	0.00	

Notes: Average sum of well-being before feedback compared between treatment and types using a 9-point Likert Scale from 1 – unexcited to 9 – excited. Two-sided t-test results concern between-group differences. Standard deviation reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

5.4.2 Clinical Sample

5.4.2.1 Performance

Again, as expected, we found that on average the performance of the "Clinically Depressed" was significantly lower compared to the performance of "Healthy Controls" (p < 0.001, one-sided t-test). Comparing the performance between "Group" and "Single" treatments for the "Clinically Depressed" and "Healthy Controls," we found no effect on the individual performance for any of the types (Table 5.11). Comparing the performance between the "Clinically Depressed" and "Healthy Controls" we found a lower performance of the “Clinically Depressed” compared to “Healthy Controls” for both treatments (Table 5.11).

Table 5.11. Individual Performance by Treatment

Correct Calculations	Group (G)	Single (S)	Diff. G – S
Clinically Depressed (P)	8.65 (3.45)	8.81 (3.26)	-0.16
Healthy Control (HC)	11.23(3.91)	10.93 (2.92)	0.31
Diff. P – HC	-2.58***	-2.11***	

Notes: Average sum of correct calculations in the payment periods compared between treatments and types. Two-sided t-test results concern between-group differences. Standard deviation reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

We ran a panel regression to see how the performance of the participants developed over time. We found that the performance of the “Clinically Depressed” was lower compared to the “Healthy Controls” but increased over time in contrast to our expectations, but in line with the findings in the “Subclinical Sample” (Table 5.12).

Table 5.12. Panel Regression on Correct Calculations in the Clinical Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Clinically Depressed		Healthy Control	
Dep. Variable			Correct Calculations			
Group Treatment	0.405	0.166	0.313	1.195	0.431	0.161
	(0.731)	(0.646)	(1.336)	(1.321)	(0.743)	(0.661)
Period	0.173***	0.173***	0.183***	0.196***	0.173***	0.173***
	(0.0431)	(0.0432)	(0.0275)	(0.0283)	(0.0431)	(0.0432)
Group Treatment x Period	-0.0330	-0.0343	-0.0732	-0.0849	-0.0369	-0.0369
	(0.0467)	(0.0468)	(0.0520)	(0.0565)	(0.0497)	(0.0497)
Clin. Depressed	-2.179**	-1.866***				
	(0.986)	(0.715)				
Clin. Depressed x Group Treatment	-0.413	0.868				
	(1.512)	(1.437)				
Clin. Depressed x Period	0.0104	0.0236				
	(0.0509)	(0.0512)				
Clin. Depressed x Group Treatment x Period	-0.0402	-0.0506				
	(0.0691)	(0.0722)				
Healthy Control w/ Clin. Depressed	0.321	0.123			0.251	0.114
	(0.719)	(0.752)			(0.788)	(0.821)
Healthy Control w/ Clin. Depressed x Period					0.0107	0.00785
					(0.0343)	(0.0343)
Constant	9.802***	7.825***	7.624***	1.112	9.802***	8.937***
	(0.518)	(2.094)	(0.856)	(2.646)	(0.518)	(2.389)
Observations	1,584	1,500	288	252	1,296	1,248
Controls	No	Yes	No	Yes	No	Yes
Number of Subjects	132	125	24	21	108	104

Notes: We report GLS coefficients with standard errors clustered on the individual level in parentheses using a random effects model over 12 periods. The dependent variable is the sum of correct calculations. Controls include dummy variables for education and age. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Looking at the Group Performance, we found that, on average, the “Clinically Depressed Group” had a significantly lower performance compared to the “Healthy Group” ($p<0.1$, one-sided t-test).

5.4.2.2 Satisfaction

As expected, the “Clinically Depressed” indicated, on average, a significantly lower level of satisfaction compared to the “Healthy Controls” ($p<0.001$, one-sided t-test). In contrast to our hypothesis, but in line with the findings in the subclinical sample, we found a positive effect of the “Group” compared to the “Single” treatment on satisfaction for both types (Table 5.13).

Table 5.13. Individual Satisfaction by Treatment

Satisfaction	Group (G)	Single (S)	Diff. G – S
Clinically Depressed (P)	5.68 (2.45)	5.02 (2.55)	0.66**
Healthy Control (HC)	6.36 (2.07)	5.76 (1.97)	0.60***
Diff. P – HC	-0.68***	-0.74***	

Notes: Average sum of satisfaction compared between treatment and types using a 9-point Likert Scale from 1 – very unsatisfied to 9 – very satisfied. Two-sided t-test results concern between-group differences. Standard deviation reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

5.4.2.3 Affective States

"Clinically Depressed" indicated on average a significantly lower level of well-being compared to "Healthy Controls" (p<0.001, one-sided t-test). We found no differences in well-being for the "Clinically Depressed" between the treatments and a positive effect of the group treatment on the "Healthy Controls" (Table 5.14). We found that the "Clinically Depressed" stated significantly lower levels of well-being compared to the "Healthy Control" in both treatments (Table 5.14)).

Table 5.14. Well-Being Before Feedback

Well-Being	Group (G)	Single (S)	Diff. G – S
Clinically Depressed (P)	5.29 (2.31)	5.45 (2.59)	-0.16
Healthy Control (HC)	6.23 (1.95)	5.99 (1.82)	0.24*
Diff. P – HC	-0.94***	-0.54**	

Notes: Average sum of well-being before feedback compared between treatment and types using a 9-point Likert Scale from 1 – unpleasant to 9 – pleasant. Two-sided t-test results concern between-group differences. Standard deviation reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

"Clinically Depressed" indicated on average a significantly higher level of arousal compared to the "Healthy Controls" (p<0.001, one-sided t-test). As hypothesized, the "Clinically Depressed" in the "Group" treatment indicated a significantly higher level of arousal than those in the "Single" treatment (Table 5.15). We found that the "Clinically Depressed" stated significantly higher levels of arousal compared to the "Healthy Control" in both treatments (Table 5.15).

Table 5.15. Arousal Before Feedback by Treatment

Arousal	Group (G)	Single (S)	Diff. G – S
Clinically Depressed (P)	6.44 (1.97)	5.40 (2.01)	1.04***
Healthy Control (HC)	4.92 (2.25)	4.90 (2.02)	0.02
Diff. P – H	1.52***	0.50**	

Notes: Average sum of well-being before feedback compared between treatment and types using a 9-point Likert Scale from 1 – unexcited to 9 – excited. Two-sided t-test results concern between-group differences. Standard deviation reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

5.5 Summary and Discussion

The aim of this project was to use a controlled laboratory setting to test how the *interpersonal context* of the workplace—working in a group or individually—impacts the mood and productivity of depressed and non-depressed workers and the teams they work in. In two studies, healthy and depressed individuals solved real-effort tasks, either in a group or in an individual working context. Groups were comprised of either only healthy subjects or healthy subjects and one depressed participant. The two studies differed in the sample used: i.) a random student population divided into a subclinically depressed and healthy subgroup and ii.) a clinical sample of depressed individuals at the end of an inpatient or day-clinic treatment and matched to healthy controls. These two samples were chosen to understand both the influence of the work context of those on the brink of depression and on those returning to the workplace after a depressive episode. Based on the assumption that the cognitive performance of depressed participants is lower than that of healthy individuals, we hypothesized that the context would have specific effects on performance and mood in the different settings over the time of the experiment. We expected the group context to have negative effects on the performance, satisfaction and emotional states of the depressed participants and potentially their group members.

The results show the expected lower performance of depressed participants compared to healthy participants within each of the samples. However, in contrast to our expectations, the performance of subclinically depressed individuals working in groups with healthy controls is even higher than the performance of healthy controls in homogenously healthy control groups. In addition, a positive performance effect of working in a group is found in the student sample for both healthy and subclinically depressed individuals. We observe no decrease in performance of groups with subclinically depressed participants over time.

Thus, we neither confirm our *first hypothesis* that being in a group context negatively affects performance of depressed participants in general, nor our *second hypothesis*, which predicted a decrease in performance over time.

The difference in the performance effects of depression in the two samples can most likely be attributed to the difference in the severity of depression. Students have a significantly lower depression severity (i.e., average BDI scores of 19.08 (5.01) among students vs 25.05 (10.13) in the clinically depressed group, $p<0.001$ one-sided t-test), which might account for the preserved ability to increase their performance in the group context. Such a link between depression severity and cognitive performance is supported by various studies (Rock et al. 2014; Ahern and Semkovska 2017). Clinically depressed individuals show reduced cognitive performance at baseline but not an impaired training effect or exhaustion during the task. In contrast, the performance increase of the subclinically depressed students is still even high enough to let their whole group surpass the performance of groups without a depressed participant.

Another factor explaining the difference in performance between the student sample and the clinically depressed sample is the higher overall performance level and the lower average age in the student sample. Both factors could provide higher cognitive baseline resources as a resilience factor to compensate for cognitive impairments associated with depression. Thus, we confirm through our experiments that depression impairs cognitive resources, which is reflected in performance reduction in an individual work context. The group context can compensate for this impairment if i.) depression severity is not too high and ii.) baseline performance/cognitive resources provide resilience to compensate for the task.

Our *third hypothesis* claimed that the group context negatively influences satisfaction in depressed individuals; this, however, is not confirmed by our experiments. On the contrary, the overall work satisfaction is generally higher in groups for both samples, the student sample and the clinical sample.

Concerning the *fourth hypothesis*, we find mixed results. While the group context had a positive effect on the well-being of the subclinically depressed participants and no effect on the well-being of the clinically depressed participants, we could confirm a negative effect of being in a group on arousal in both samples. The functional model of depression by Holtzheimer and Mayberg (2011) has characterized the development of major depression as a failure of homeostatic interaction with the social environment. It is suggested that "depression is better defined as the tendency to enter into, and inability to disengage from, a negative mood state rather than the mood state per se" (Holtzheimer and Mayberg 2011, p.1). In this concept, depression is defined by a deficit of homeostatic self-regulation when facing stressful life events with increased proneness to enter and stay in negative mood states. It seems that the group context could at least partially alleviate this.

The positive effects of a group context which we find in the student sample, underlines the idea that these may be restricted to less depressive and younger individuals with greater resources to compensate for the cognitive effects of depression. The assumption that not only depression severity but also age is associated with reduced motivational and affective flexibility is supported by recent empirical data (e.g., Wrzus et al. 2015; Bruine de Bruin et al. 2018).

While, to the best of our knowledge, this study is the first to systematically compare the effects of different kinds of work organization on (sub)clinically depressed workers and their teams, it also has some limitations. First, the sample of individuals suffering from a diagnosed major depressive disorder is relatively small and consists of only women, whereas the sample of subclinically depressed participants is comprised of male and female students, i.e., the two samples differ in sex, age and education level. However, for practical reasons, as the population of clinically depressed participants available for research is small and diagnosed major depression is more frequent among women, it was not feasible to match the clinical sample with the student sample in size and demographics. Further studies, including patient recruitment in multiple centers, are necessary to figure out how generalizable the results from the small all-female patient samples are to the general population of the clinically depressed.

Another question that our experiment cannot solve is whether the positive effects of being in a group are sustainable for both sides. Our results reflect only a relatively short interaction, in which the healthy controls partially overcompensate for the reduced performance of the depressed team members; this might be a strategy that healthy team members cannot and do not want to sustain over larger periods. Thus, it remains to be studied whether these positive effects of group work on both the well-being and performance of depressed group members are sustainable over the long run. The outcomes clearly depend on the severity of depression, but—in our setting—all depressed group members profit from or at least do not worsen in well-being and performance in a group setting.

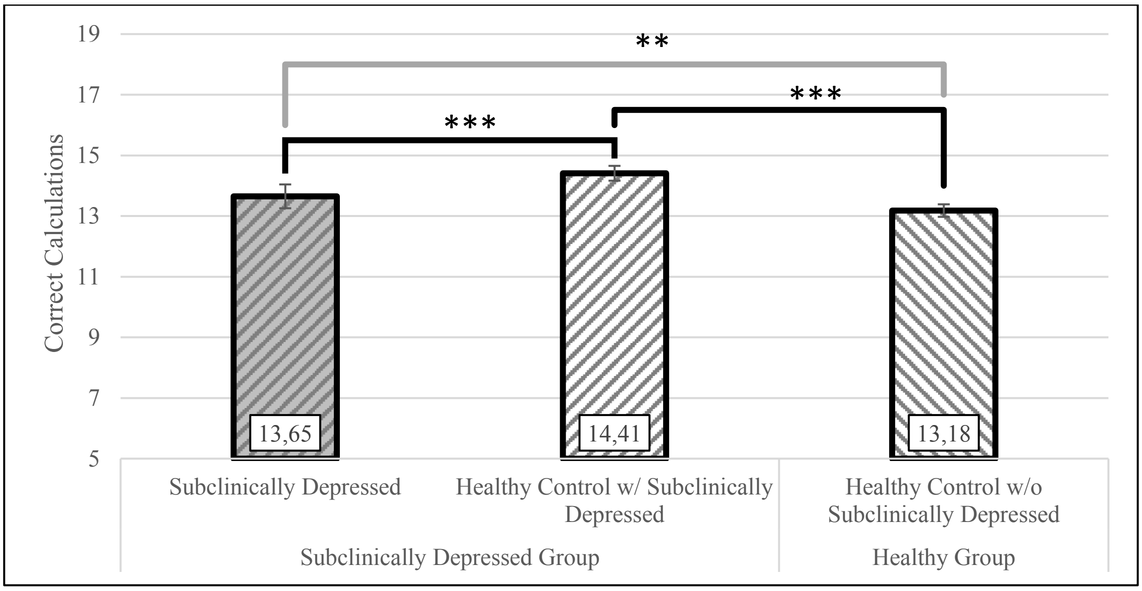
Notably, enabling work in the group context is not only valuable advice in an attempt to preserve the resilience of a companies' workforce but also from the individual employees' perspective. Working in a group also preserves individual well-being. Thus, both from the perspective of the employer and that of the employee, it is important to make sure that people at risk for or recovering from depression are integrated into work teams to preserve their ability to work and to avoid relapse.

While some questions need to be addressed by future research, we can already recommend that mental healthcare and management strategies integrate a deliberate choice of work context allowing for integration into teams of healthy workers to improve rehabilitation.

Appendix 5

Appendix 5.1 Subclinical Sample

Figure A 5.1. Individual Performance by Treatment and Type



Notes: Average sum of correct calculations compared between treatment and types. Group w/ Subclinically Depressed: groups with a subclinically depressed individual; w/o Subclinically Depressed: groups without a subclinically depressed individual. Two-sided t-test results concern between-group differences. *** p<0.01, ** p<0.05, * p<0.1

Table A 5.1. Panel Regression on Satisfaction in the Subclinical Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Subclinically Depressed		Healthy Control	
Dep. Variable			Satisfaction			
Group Treatment	0.354	0.361	0.439	0.420	0.357	0.356
	(0.323)	(0.324)	(0.454)	(0.449)	(0.329)	(0.329)
Period	0.0253	0.0253	-0.0271	-0.0271	0.0253	0.0253
	(0.0264)	(0.0264)	(0.0195)	(0.0196)	(0.0264)	(0.0264)
Group Treatment x	-0.0142	-0.0142	0.0178	0.0178	-0.0144	-0.0144
Period	(0.0278)	(0.0278)	(0.0376)	(0.0377)	(0.0291)	(0.0291)
Sub. Depressed	-0.809**	-0.771**				
	(0.378)	(0.383)				
Sub. Depressed x	0.0197	0.0152				
Group Treatment	(0.555)	(0.556)				
Sub. Depressed x	-0.0524	-0.0524				
Period	(0.0328)	(0.0328)				
Sub. Depressed x	0.0320	0.0320				
Group Treatment x Period	(0.0466)	(0.0467)				
Healthy Control	0.0653	0.0598			0.0602	0.0452
w/ Sub. Depressed	(0.211)	(0.211)			(0.243)	(0.242)
Healthy Control					0.000607	0.000607
w/Sub. Depressed x Period					(0.0175)	(0.0175)
Constant	5.776***	5.512***	4.967***	5.308***	5.776***	5.319***
	(0.279)	(0.501)	(0.256)	(0.900)	(0.279)	(0.600)
Observations	4,068	4,068	1,068	1,068	3,000	3,000
Controls	No	Yes	No	Yes	No	Yes
Number of Subjects	339	339	89	89	250	250

Notes: We report GLS coefficients with standard errors clustered on the individual level in parentheses using a random effects model over 12 periods. The dependent variable is the level of satisfaction. Controls include dummy variables for education and age. *** p<0.01, ** p<0.05, * p<0.1

Table A 5.2. Panel Regression on Well-Being in the Subclinical Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Subclinically Depressed		Healthy Control	
Dep. Variable			Well-Being			
Group Treatment	0.255	0.264	0.285	0.260	0.263	0.263
	(0.306)	(0.307)	(0.388)	(0.371)	(0.310)	(0.311)
Period	0.0265	0.0265	-0.0401**	-0.0401**	0.0265	0.0265
	(0.0250)	(0.0250)	(0.0183)	(0.0184)	(0.0250)	(0.0250)
Group Treatment x	-0.0194	-0.0194	0.0233	0.0233	-0.0205	-0.0205
Period	(0.0263)	(0.0263)	(0.0340)	(0.0341)	(0.0273)	(0.0273)
Sub. Depressed	-0.983***	-0.933***				
	(0.344)	(0.349)				
Sub. Depressed x	-0.0135	-0.0223				
Group Treatment	(0.493)	(0.490)				
Sub. Depressed x	-0.0665**	-0.0665**				
Period	(0.0309)	(0.0309)				
Group Treatment x	0.0427	0.0427				
Sub. Depressed x Period	(0.0428)	(0.0429)				
Healthy Control	0.0443	0.0385			0.0240	0.00730
w/ Sub. Depressed	(0.212)	(0.212)			(0.235)	(0.236)
Healthy Control					0.00238	0.00238
w/ Sub. Depressed x Period					(0.0164)	(0.0164)
Constant	5.897***	5.585***	4.914***	5.278***	5.897***	5.379***
	(0.261)	(0.506)	(0.226)	(0.863)	(0.261)	(0.635)
Observations	4,068	4,068	1,068	1,068	3,000	3,000
Controls	No	Yes	No	Yes	No	Yes
Number of Subjects	339	339	89	89	250	250

Notes: We report GLS coefficients with standard errors clustered on the individual level in parentheses using a random effects model over 12 periods. The dependent variable is the level of well-being. Controls include dummy variables for education and age. *** p<0.01, ** p<0.05, * p<0.1

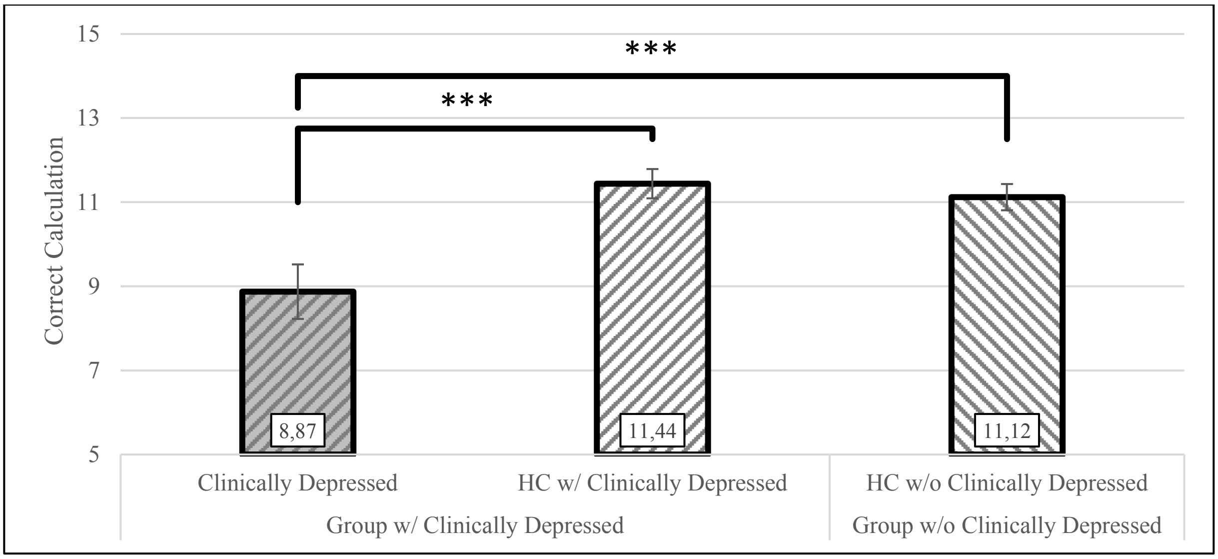
Table A 5.3. Panel Regression on Arousal in the Subclinical Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Subclinically Depressed		Healthy Control	
Dep. Variable			Arousal			
Group Treatment	-0.119	-0.0939	0.599	0.571	-0.142	-0.123
	(0.405)	(0.408)	(0.505)	(0.506)	(0.410)	(0.415)
Period	0.0161	0.0161	0.0260	0.0260	0.0161	0.0161
	(0.0195)	(0.0195)	(0.0216)	(0.0216)	(0.0195)	(0.0195)
Group Treatment x	-0.0455**	-0.0455**	-0.0281	-0.0281	-0.0428*	-0.0428*
Period	(0.0216)	(0.0216)	(0.0428)	(0.0429)	(0.0231)	(0.0231)
Sub. Depressed	-0.0785	-0.0316				
	(0.455)	(0.463)				
Sub. Depressed x	0.395	0.339				
Group Treatment	(0.644)	(0.652)				
Sub. Depressed x	0.00992	0.00992				
Period	(0.0290)	(0.0290)				
Sub. Depressed x	0.0174	0.0174				
Group Treatment x Period	(0.0478)	(0.0478)				
Healthy Control	0.323	0.339			0.375	0.371
w/ Sub. Depressed	(0.261)	(0.264)			(0.287)	(0.288)
Healthy Control					-0.00618	-0.00618
w/ Sub. Depressed x Period					(0.0192)	(0.0192)
Constant	4.963***	5.262***	4.884***	6.726***	4.963***	4.703***
	(0.354)	(0.729)	(0.287)	(1.326)	(0.355)	(0.684)
Observations	4,068	4,068	1,068	1,068	3,000	3,000
Controls	No	Yes	No	Yes	No	Yes
Number of Subjects	339	339	89	89	250	250

Notes: We report GLS coefficients with standard errors clustered on the individual level in parentheses using a random effects model over 12 periods. The dependent variable is the level of arousal. Controls include dummy variables for education and age. *** p<0.01, ** p<0.05, * p<0.1

Appendix 5.2 Clinical Sample

Figure A 5.2. Individual Performance by Treatment and Type



Notes: Average sum of correct calculations compared between treatment and types. Group w/ Clinically Depressed: groups with a clinically depressed individual; w/o Clinically Depressed: groups without a clinically depressed individual. Two-sided t-test results concern between-group differences. *** p<0.01, ** p<0.05, * p<0.1

Table A 5.4. Panel Regression on Satisfaction in the Clinical Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Clinically Depressed		Healthy Control	
Dep. Variable			Satisfaction			
Group Treatment	0.288	0.212	0.693	1.225	0.247	0.209
	(0.524)	(0.529)	(0.901)	(0.971)	(0.529)	(0.531)
Period	0.0210	0.0210	0.00726	0.0124	0.0210	0.0210
	(0.0383)	(0.0383)	(0.0260)	(0.0306)	(0.0383)	(0.0383)
Group Treatment x Period	0.0216	0.0255	-0.00472	0.00579	0.0280	0.0280
	(0.0415)	(0.0417)	(0.0530)	(0.0572)	(0.0439)	(0.0439)
Clin. Depressed	-0.655	-0.718				
	(0.782)	(0.719)				
Clin. Depressed x Group Treatment	-0.0420	0.278				
	(1.027)	(1.062)				
Clin. Depressed x Period	-0.0137	-0.00858				
	(0.0460)	(0.0485)				
Clin. Depressed x Group Treatment x Period	-0.0263	-0.0198				
	(0.0665)	(0.0696)				
Healthy Control w/ Clin. Depressed	0.447	0.465			0.560*	0.563
	(0.288)	(0.293)			(0.339)	(0.368)
Healthy Control w/ Clin. Depressed x Period					-0.0173	-0.00730
					(0.0314)	(0.0332)
Constant	5.627***	5.168***	4.972***	3.066	5.627***	5.723***
	(0.475)	(1.239)	(0.634)	(4.056)	(0.475)	(1.199)
Observations	1,584	1,500	288	252	1,296	1,248
Controls	No	Yes	No	Yes	No	Yes
Number of Subjects	132	125	24	21	108	104

Notes: We report GLS coefficients with standard errors clustered on the individual level in parentheses using a random effects model over 12 periods. The dependent variable is the level of satisfaction. Controls include dummy variables for education and age. *** p<0.01, ** p<0.05, * p<0.1

Table A 5.5. Panel Regression on Well-Being in the Clinical Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Clinically Depressed		Healthy Control	
Dep. Variable			Well-Being			
Group Treatment	0.0942	0.0511	-0.131	0.623	0.0699	0.0703
	(0.470)	(0.480)	(0.924)	(0.999)	(0.470)	(0.473)
Period	0.0118	0.0118	-0.0355	-0.0420	0.0118	0.0118
	(0.0229)	(0.0229)	(0.0231)	(0.0270)	(0.0229)	(0.0230)
Group Treatment x	0.00952	0.0132	-0.00455	0.000350	0.0133	0.0133
Period	(0.0264)	(0.0266)	(0.0455)	(0.0511)	(0.0286)	(0.0286)
Clin. Depressed	-0.235	-0.703				
	(0.782)	(0.768)				
Clin. Depressed x	-0.449	0.0875				
Group Treatment	(1.022)	(1.082)				
Clin. Depressed x	-0.0473	-0.0537				
Period	(0.0322)	(0.0349)				
Clin. Depressed x	-0.0141	-0.0128				
Group Treatment x Period	(0.0518)	(0.0564)				
Healthy Control	0.224	0.208			0.290	0.252
w/ Clin. Depressed	(0.325)	(0.331)			(0.334)	(0.351)
Healthy Control					-0.0101	-0.000321
w/ Clin. Depressed x Period					(0.0265)	(0.0275)
Constant	5.915***	5.582***	5.679***	4.711	5.915***	5.873***
	(0.411)	(1.375)	(0.679)	(4.562)	(0.411)	(1.362)
Observations	1,584	1,500	288	252	1,296	1,248
Controls	No	Yes	No	Yes	No	Yes
Number of Subjects	132	125	24	21	108	104

Notes: We report GLS coefficients with standard errors clustered on the individual level in parentheses using a random effects model over 12 periods. The dependent variable is the level of well-being. Controls include dummy variables for education and age. *** p<0.01, ** p<0.05, * p<0.1

Table A 5.6. Panel Regression Arousal in the Clinical Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Clinically Depressed		Healhty Cotnrol	
Dep. Variable			Arousal			
Group Treatment	0.228	0.114	0.575	-0.185	0.169	0.0554
	(0.571)	(0.576)	(0.896)	(0.906)	(0.571)	(0.577)
Period	-0.00626	-0.00626	0.0137	0.0162	-0.00626	-0.00626
	(0.0408)	(0.0409)	(0.0485)	(0.0577)	(0.0409)	(0.0409)
Group Treatment x Period	-0.00675	-0.00596	0.0708	0.0824	0.00232	0.00232
	(0.0443)	(0.0446)	(0.0606)	(0.0688)	(0.0470)	(0.0471)
Clin. Depressed	0.371	0.906				
	(0.791)	(0.794)				
Clin. Depressed x Group Treatment	0.809	0.243				
	(1.065)	(1.104)				
Clin. Depressed x Period	0.0200	0.0225				
	(0.0626)	(0.0695)				
Clin. Depressed x Group Treatment x Period	0.0776	0.0884				
	(0.0741)	(0.0804)				
Healthy Control w/ Clin. Depressed	-0.462	-0.364			-0.303	-0.245
	(0.428)	(0.445)			(0.440)	(0.451)
Healthy Control w/ Clin. Depressed x Period					-0.0245	-0.0243
					(0.0334)	(0.0349)
Constant	4.944***	6.212***	5.315***	8.762**	4.944***	5.683***
	(0.501)	(1.571)	(0.626)	(3.866)	(0.501)	(1.674)
Observations	1,584	1,500	288	252	1,296	1,248
Controls	No	Yes	No	Yes	No	Yes
Number of Subjects	132	125	24	21	108	104

Notes: We report GLS coefficients with standard errors clustered on the individual level in parentheses using a random effects model over 12 periods. The dependent variable is the level of arousal. Controls include dummy variables for education and age. *** p<0.01, ** p<0.05, * p<0.1

Chapter 6

Misperceiving Economic Success: Experimental Evidence on Meritocratic Beliefs and Inequality Acceptance

Abstract. Most people tend to equate success with merit, a tendency that is particularly pronounced among conservatives. However, in practice it is exceedingly difficult to discern the relative impact of luck and effort to economic success. Based on a large-scale online study that samples the general US population, we investigate whether individuals misperceive the importance of luck for success, and how this mediates their meritocratic beliefs and acceptance of inequality. We randomly assign participants in pairs to compete in an easy or hard work assignment. The tasks are structured such that working on the easy work assignment almost certainly results in better performance and economic success. We show that economically successful participants overweight the role of effort in their success, perceiving high income as more deserved than unsuccessful participants. Subsequently, they demand less redistributive taxation, and they also show little interest in receiving information about the true determinants of their success. These general findings hold true regardless of political orientation. Successful liberals are as meritocratic as conservatives are, sharing the same beliefs in deservingness and preferences for low redistributive taxes.[36]

[36] This chapter was co-authored by Dietmar Fehr.

6.1 Introduction

People tend to accept more inequality if it reflects hard work, effort, and performance (Fong 2001; Alesina and La Ferrara 2005; Cappelen et al. 2007; Almås, Cappelen, and Tungodden 2020). This widely held meritocratic fairness ideal may explain variation in income inequality and redistributive policies across countries (Alesina and Glaeser 2004; Alesina and Angeletos 2005), and it is at the core of the "American Dream," i.e., the notion that success can be attained by all who work sufficiently hard. Against this backdrop, it is not surprising that many tend to equate success with merit (Frank 2016; Gauriot and Page 2019; Mijs 2019). However, it is difficult, if not impossible, to discern the relative contributions made by luck and effort to economic success. As a result, individuals may conclude in many cases that merit is the source of their success, when in fact luck has played a crucial role.

This paper provides evidence on such misperceptions and their consequences for redistributive tax preferences using a large-scale interactive online study with a sample of the general population of the United States. We are particularly interested in how economic success shapes an individual's perception of merit and how these perceptions affect their preferences for redistributive taxation. Given that ideological dispositions on fairness views and inequality often differ between liberal and conservative voters and appear as critical inputs for government tax policy policies (e.g., Alesina and Glaeser 2004; Congdon, Kling, and Mullainathan 2009), we also examine how meritocratic beliefs and redistributive preferences differ in relation to political orientation.

Political affiliation is a strong indicator of how people perceive and navigate political and economic issues (Campbell 1960; Bartels 2002). Indeed, liberals and conservatives generally adhere to divergent explanations of the underlying causes of economic success: in public opinion polls, liberals consistently emphasize the role of luck in economic success, while conservatives typically support the view that success is the result of hard work, which makes any resulting inequality morally fair (Dunn 2018; Pew Research Center 2019).[37] Yet a persistent concern is that these opinions do not necessarily reflect what people think and do when they are forced to appraise their own

[37] There is a handful of studies providing evidence of a correlation between political orientation and the role of luck and effort in economic success (see, for example, Gromet, Hartson, and Sherman 2015; Karadja, Mollerstrom, and Seim 2017; Fehr, Muller, and Preuss 2020).

success. Do liberals still believe in luck when they are successful, and are conservatives still proponents of meritocracy when they are unsuccessful?

A number of obstacles complicate the credible identification of a causal relationship between economic success and perceptions surrounding the role of merit and inequality acceptance. First, identification is complicated by the difficulty to characterize the determinants of economic success in observational data. It is typically hard to identify and to quantify the relative impact of luck and effort ex-post, let alone to study the associated beliefs. Second, it is difficult to gather data on individuals' beliefs before and after they achieve economic success, and if it is possible, any observed variation in beliefs is likely endogenous with respect to economic success and behavior. Third, a correlation between political orientation and meritocratic beliefs may indicate that causality runs in both directions with political orientation informing such beliefs, and vice versa.

We overcome these identification challenges by designing a work assignment that gives us control over the details of the task while also allowing us to introduce the necessary exogenous variation in economic success. The work assignment is a simple code-entry task, for which we recruited a large sample of workers from an online labor market platform. The code-entry task requires no prior knowledge or specific skills such that performance should depend almost entirely on exerted effort. We randomly match workers into pairs and pay them by their relative performance resulting in highly unequal incomes within pairs, i.e., the worker with the higher score receives a high bonus, whereas the worker with the lower score receives no bonus payment. To create the necessary random variation in economic success, we leverage the relative performance payment scheme that depends on exerted effort and randomly assign workers to either an *easy* or a *hard* version of the task, without disclosing this assignment to the workers. The two versions of the task are calibrated such that working on the *easy task* results with near certainty in a higher score than working on the *hard task*. Thus, while everyone has to exert effort to have a chance of success, some have a larger exogenous advantage than others – as is often the case in socioeconomic reality (e.g., Chetty et al. 2020). Furthermore, given that success is largely predetermined by one's random assignment to the *hard* or *easy task* and that participants are uncertain about task difficulty, we can identify its impact on meritocratic beliefs, and support for redistributive taxes.

After participants complete the work assignment, but before they learn about their success or failure, we elicit their beliefs about task difficulty, their relative performance, and the extent to which they deserve the bonus payment. After revealing the bonus payment (i.e., economic success), we measure these beliefs again. Gathering data on these perceptions both before and after disclosing the bonus payment allows us to account for heterogeneous prior beliefs and to precisely measure whether success changes beliefs. This is important because it accounts for heterogeneity in behavior. In a next step, we investigate how meritocratic beliefs shape support for redistributive taxes. Specifically, we elicit participants' preferences for a redistributive tax scheme and their willingness to pay for information about task difficulty and performance. In addition, we gather information on a broad range of socio-economic characteristics from participants, including political orientation and party affiliation, before the start of the work assignment.

The experiment generates two main findings. First, we observe that economically successful participants assign excess weight to the role of effort, leading to a strong polarization in attitudes. That is, we document a strong treatment effect on meritocratic beliefs. Economic success leads to a 14 percentage point higher belief that receiving the bonus payment is deserved. Similarly, successful participants are 16 percentage points more likely than unsuccessful participants to think that success in the work assignment depends on effort. Although it is very salient that success is random in our setting, participants predominantly attribute their success to hard work.

Economic success in our setting also conditions preferences for redistributive taxes. Specifically, successful participants tend to prefer a lower tax rate and thus less redistribution than unsuccessful participants. The difference in preferred tax rates is about 40 percentage points, equivalent to a three-times lower tax revenue. The difference in tax rates preferred by successful and unsuccessful participants can be fully explained by their prior beliefs regarding merit. That is, participants with a higher prior belief that they deserve the bonus payment demand less redistribution (i.e., a lower tax rate) when they are successful, but more redistribution if they are unsuccessful.

Consistent with the relationship between perceptions of personal merit and preferred tax rates, we document that a significant share of participants is highly willing to remain in the dark about the relative importance of merit for their success. About 50 percent of participants are unwilling to forego even 1 cent to obtain information regarding task difficulty, the main determinant of economic success. Moreover, the willingness to pay for this piece of information is significantly lower for successful than for unsuccessful participants, indicating that individuals are more than willing to maintain false perceptions about the causes of their success, misperceptions that justify greater inequality.

Second, the findings bring empirical evidence to the divisive political debate regarding fairness views and economic issues. In particular, we cast doubt on the broadly held notion that liberals are less likely to equate success with merit than conservatives. In fact, when liberals are economically successful, they advocate meritocracy just as frequently as conservatives, despite the overwhelming role played by luck in our setting. In other words, meritocratic beliefs and behavior do not differ by political orientation: when they are successful, liberals and conservatives both identify merit as the cause of success, and they both prefer lower redistributive taxes. Moreover, liberals assign as little importance to learning about the role of luck in their success as conservatives, they are less likely to revise their tax preferences, and if they revise them, the magnitude of change is smaller when compared to that of conservatives.

The findings of our paper contribute to several strands in the literature. Most importantly, we add to the voluminous literature on fairness preferences and fairness views. An important and consistent finding that has emerged in observational studies (Fong 2001; Alesina and Angeletos 2005; Karadja, Mollerstrom, and Seim 2017) and laboratory studies alike (Konow 2000; Cappelen et al. 2013; Cappelen et al. 2017) is that people tend to accept greater inequality if it is the result of effort rather than luck.[38] While the importance of the source of inequality is well documented, empirical evidence on inequality acceptance when individuals are uncertain or have limited information about the source of inequality is scarce (but see, for example, Cappelen et al. 2017; Cappelen, De Haan, and Tungodden 2020). Unlike most of these papers, however, we present causal evidence on how economic success impacts meritocratic beliefs when individuals are able to ascribe their success to their own actions. The selfish behavior that we observe is consistent with self-serving fairness norms described in the prior literature (Babcock et al. 1995; Engelmann and Strobel 2004; Croson and Konow 2009; Konow 2009; Cappelen et al. 2013; Durante, Putterman, and van der Weele 2014; Deffains, Espinosa, and Thöni 2016). We advance this literature by showing that participants display little interest in

[38] There is also evidence that rich people accept more inequality if they experienced upward mobility compared to rich people who inherited their wealth Cohn et al. (2019).

correcting biased views about merit. We also show that this lack of interest applies to liberals and conservatives alike.

Our paper belongs to a growing literature in economics that documents political polarization on a host of social and economic issues. Recent studies show that this polarization is not confined to political attitudes or fairness views alone (e.g., Gromet, Hartson, and Sherman 2015; Cappelen et al. 2020), but also applies to perceptions of factual reality, including inequality (Kuziemko et al. 2015), relative income (Cruces, Perez-Truglia, and Tetaz 2013; Karadja, Mollerstrom, and Seim 2017; Fehr, Mollerstrom, and Perez-Truglia 2019), social mobility (Alesina, Stantcheva, and Teso 2018; Fehr, Muller, and Preuss 2020), and immigration (Alesina, Miano, and Stantcheva 2020; Grigorieff, Roth, and Ubfal 2020). Other studies suggest that liberals tend to be less accepting of inequality (Fisman, Jakiela, and Kariv 2017; Cappelen, Haaland, and Tungodden 2019; Almås, Cappelen, and Tungodden 2020). While we find that liberals are more open to redistributive taxation, and are thus less accepting of inequality, we find no difference in how liberals and conservatives react to economic success – that is, liberals display the same meritocratic beliefs and behavior as conservatives.

Finally, we contribute to a rapidly growing strand of economic research that relies on online platforms such as MTurk, Prolific, Dynata, Luc.id, and YouGov. The vast majority of these studies use such platforms to implement surveys and survey experiments (e.g., Kuziemko et al. 2015; Weinzierl 2017; Grigorieff, Roth, and Ubfal 2020) or decision tasks and one-shot experiments (e.g., Bordalo et al. 2016; De Quidt, Haushofer, and Roth 2018; DellaVigna and Pope 2018b, 2018a; Enke and Graeber 2019; Exley and Kessler 2019; Gagnon, Bosmans, and Riedl 2020). Our study combine these elements and demonstrates the feasibility of conducting large-scale interactive experiments using an online platform (see also Arechar, Gächter, and Molleman 2018; Molleman et al. 2019). We discuss the implementation of the experiment in Section 6.2 below, and also provide practical advice on conducting successful interactive online experiments.

6.2 Experimental Design

Our study, which combines a survey and incentivized decision tasks, consists of four parts: a socio-demographic questionnaire, a work assignment, a redistribution task, and an information acquisition task. Screenshots of the survey and all tasks are available in the Appendix. We pre-registered the design and a pre-analysis plan in the AEA RCT Registry (AEARCTR-0004455).

Setup: In the first part, we introduce participants to the general details of the study and ask for their consent. We then elicit some basic socio-demographic information and personality traits. More details and a complete list of all covariates can be found in Appendix A 6.1. In the second part, participants work on a real effort task for 3 minutes. The task consists of retyping a series of randomly generated sequences of upper- and lower-case letters. There are two task types: An *easy task* consisting of five-letter sequences and a *hard task* consisting of 15-letter sequences. We informed participants that there are two task types and that they would be randomly assigned to one of the two (treatment assignment). While participants know that the *easy* task involves shorter sequences and the *hard* task involves longer sequences, they are not told the exact number of letters in each task type, thus engendering uncertainty about their task assignment. We intentionally designed the tasks to ensure divergence between participant scores based on task assignment, rather than participant skill or effort. Specifically, due to the length of the sequences, participants in the *hard task* will retype fewer sequences than participants assigned to the *easy task* (see Section 6.4.1 for more details).

Participants are paid according to their performance. That is, we randomly match a participant working on the *easy task* with a participant working on the *hard task* and compare their scores. The participant with the higher score receives a bonus payment of $2 and the participant with the lower score receives $0. Note that the matching protocol is public knowledge, i.e., participants are uncertain about the difficulty of their task, but know their matching partner is doing the other task (whether *hard* or *easy*).

Before we reveal the outcome of the performance comparison (i.e., the bonus payment), we ask participants: (1) to estimate the likelihood that they worked through the *hard task* ("Prior Belief, Task Difficulty"), (2) how much they think they deserve the $2 -bonus payment ("Prior Belief, Deserving Bonus"), and (3) to estimate how many of 100 participants performing the same task achieve a lower score ("Prior Belief, Relative Performance"). After revealing the bonus payment, we ask the same questions again ("Posterior Beliefs"). Additionally, we ask participants to assess the extent to which they think the bonus payment depends on luck or effort ("Belief Effort Determines Success"). Building on evidence suggesting that complex incentivation rules do not outperform introspection (e.g., Trautmann and van de Kuilen 2015; Charness, Gneezy, and Rasoscha

2020; Danz, Vesterlund, and Wilson 2020), we do not remunerate the elicitation of these beliefs in order to avoid complicating the tasks and to keep the study within a reasonable time frame.

In the third part, both participants in the matched pair have to decide about a redistributive tax rate, in which the tax revenue is equally distributed between the pair. This implies in our setting that the successful participant pays half of the tax revenue as tax while the unsuccessful participant receives half of the tax revenue. Using an interactive slider, participants can indicate a tax rate ("Tax Rate") between 0% and 100% and immediately see how the tax rate will affect their income and that of the other person. We randomly select one of the two proposed tax rates and apply the choice to the matched pair at the end of the study.[39]

In the fourth part, we offer participants an opportunity to buy information about task difficulty and the task performance of the other participant. We elicit their willingness to pay ("WTP") for this information with a simple price list. In this price list, we present participants with eight scenarios in which they have to decide between seeing the information or receiving extra money, with amounts ranging from $0.01 to $0.50. For instance, in Scenario 1 they have to choose between seeing information and receiving $0.01, and in Scenario 8 they have to choose between seeing information and receiving $0.50. To incentivize participants, we randomly pick one of the eight scenarios for each participant and implement their choice in this scenario. That is, a participant will either receive the information immediately after the price-list decision or receive the extra money at the end of the survey. In a last step, all participants who have received the information and a random subset of the remaining participants (50%) have the opportunity to revise their tax rate ("Revised Tax Rate"). Note that we only implement the revised tax rate if the first tax proposal from that participant was initially chosen for implementation. Finally, participants receive a detailed overview about the composition of their final payout.

Implementation: We used the open source software oTree (Chen, Schonger, and Wickens 2016) to program and run the study. We recruited and paid participants via

[39] Note that this procedure elicits participants' true preferences for redistributive taxation given that participants are consequentialists and care about final outcomes. This assumption seems reasonable in our setting as merit considerations typically overlay ex-ante fairness concerns (Cappelen et al. 2013; Durante, Putterman, and van der Weele 2014; Cappelen et al. 2017).

Amazon Mechanical Turk (MTurk). This platform offers access to a quite diverse population (e.g., Berinsky, Huber, and Lenz 2011; Buhrmester, Kwang, and Gosling 2011; Arechar, Kraft-Todd, and Rand 2017) and mounting evidence suggests that the findings of studies run on MTurk are robust to results using other subject populations, such as student, convenience, and nationally representative samples (e.g., Horton, Rand, and Zeckhauser 2011; Arechar, Gächter, and Molleman 2018; Coppock and McClellan 2019; Snowberg and Yariv 2020). However, some researchers have noted that data quality has recently declined, in particular due to automated responses (bots) and inattention (Ahler, Roush, and Sood 2020; Chmielewski and Kucker 2020). To address these concerns, we took several precautionary measures. First, we limited participation to MTurkers based in the US with more than 1000 performed Human Intelligence Tasks (HITs) and an acceptance rate of at least 98%. Second, we used a simplified CAPTCHA (adding two numbers) to screen for bots, i.e., only participants that correctly answered this question could access our survey. Third, the letter sequences in the work assignment were in non-machine-readable format, providing another layer of protection against bots.

We also took great care to address other practical challenges associated with running experiments on an online platform such as MTurk. First, MTurkers often multitask and work simultaneously on several HITs. To minimize inattention due to switching between HITs, we requested in the beginning that participants should exclusively work on our HIT, and stated that they have a total of 20 minutes to complete the HIT, that there are timeouts on each question, and that any payment is conditional on completing the HIT within the time limit. The timeouts are set such that participants have sufficient time to thoughtfully answer our questions, yet they must remain attentive. Moreover, we paid a relatively high flat payment of $0.75 and promised substantial additional payments. On average, participants earned about $1.90, which is substantially above the US minimum wage considering our usual HIT duration of 12 minutes.

Second, since participants typically do not arrive simultaneously, we designed the experiment such that the survey and the work assignment can be completed independently. There was, however, one important exception. To determine the bonus payment, it is necessary to compare two participants' performances in the real-effort task. For this reason, every participant entered a virtual waiting room before the announcement of the bonus payment. If a suitable matching partner was already waiting, participants were immediately matched, and each could independently work through the rest of the survey. If there was no matching partner available, participants had to wait for a minimum of three minutes. As soon as a suitable matching partner arrived in the waiting room, they were matched. Participants had the possibility to end the survey after three minutes (if no suitable matching partner had arrived), in which case they only received the base payment. Alternatively, they could continue waiting until they were matched (but they ran the risk of exceeding the HIT time limit, in which case they received no payment).

Finally, we aimed to minimize the risk of participants dropping out before completing the survey. Despite numerous possibilities for dropping out voluntarily or involuntarily (e.g., if no matching partner is available), internal validity is only threatened by dropouts after the announcement of the bonus payment (which depends on the random task assignment). As long as such dropouts are random across the treatment, our treatment estimates remain unbiased (as it is the case, as shown below). However, we also took some steps to minimize this risk ex-ante. We informed participants that they would not receive any payment *and* no HIT approval if they dropped out due to a time out. Evidence suggests that these are sensible requirements, as MTurkers are sensitive to rejections (a low approval rate prevents them from participating in HITs that require a high approval rate; see Hara et al. (2018).

Attrition and sample characteristics: The overall attrition rate was about 9 percent, which is comparatively low for this type of study.[40] In total, 2,026 participants started the work assignment and 1,845 participants finished all tasks.[41] Importantly, attrition was random across the treatment assignment (10 percent in the *hard* and 8 percent in the *easy* task, t-test, p=0.25). The low level of attrition illustrates the effectiveness of the implemented measures to minimize dropouts and suggests that the treatment assignment did not cause participants to quit our HIT. A regression of an indicator for dropouts on the treatment indicator shows no difference in the likelihood of attrition between the *easy*

[40] For example, Kuziemko et al. (2015) report an attrition rate of 15 percent in survey experiment and Arechar, Gächter, and Molleman (2018) report an attrition rate of 18 percent in an interactive online experiment.

[41] A total of 2,535 MTurkers accepted our HIT. Of those, 383 failed on the simple CAPTCHA, which served as a first robot control, and 105 did not finish the demographics survey. Our work assignment served as a second robot control as we displayed the tasks in non-machine-readable format and 21 MTurkers dropped out after the survey but before the work assignment resulting in our final sample of 2,026.

and *hard* treatment (see Appendix Table A6.1).[42] Moreover, comparing socio-demographic characteristics (including political views) of dropouts and non-dropouts reveals no differences (see Appendix Table A6.2). Across 30 tests, there is no single t-statistic above 1.96. Therefore, attrition is unlikely to affect our results.

In our final sample, we dropped 20 participants, because they ended up with the same score and the bonus was split equally within pairs. This leaves us with 1,825 observations. In Appendix Table A6.3, we show that the participants do not differ along a large set of observables in the two tasks. A joint test for all observables being equal to zero reveals an F-statistic of 1.09 (p=0.35). Moreover, comparing our MTurk sample with data from the US census reveals remarkable similarities along a large set of observables. Our sample closely matches the US population in terms of age, gender, marital status, household size and income, and geographic location, but white and educated people are overrepresented (see Appendix Table A6.4).

6.3 Empirical Strategy

Our treatment involves the random assignment of participants to the *easy* and *hard task*. Participants know at the outset that they will be assigned to one of the two tasks with equal probability and that they will be randomly matched to a participant completing the other task. Importantly, they do not learn and cannot infer the difficulty of the task from the task itself. We calibrated the difficulty of the two tasks such that the participant assigned to the *easy task* can easily outperform his or her counterpart assigned to the *hard task*. Consequently, economic success (i.e., receiving the $2 bonus payment) should coincide with the random assignment to the *easy task*. This allows us to identify the causal effect of economic success on meritocratic beliefs and behavior.

In practice, treatment compliance was, however, not perfect. About 6 percent of participants assigned to the *hard task* had a better performance than their matched counterparts in the *easy task* (for details, see Section 6.4.1). To deal with this non-compliance, we use the treatment assignment (*easy* or *hard task*) to estimate *intention-to-treat (ITT)* effects. The general regression framework thus takes the following form:

$$Y_i = \beta_0 + \beta_1 EasyTask_i + \gamma \boldsymbol{X} + \varepsilon_i \quad (1)$$

[42] The coefficient for the treatment indicator is -0.015 (s.e. 0.013). The same is true if we run the same regression but only consider dropouts after participants learned about the bonus assignment (coefficient -0.013, s.e. 0.009).

where Y_i is one of our outcome variables (i.e., our belief measures and the tax rate), $EasyTask_i$ indicates if a participant was randomly assigned to the *easy task*, $\boldsymbol{X}$ is a set of standard controls (including gender, age, marital status, education level, ethnicity, employment status, and household income), and ε_i is an individual-specific error term. In some specifications, we consider participants' political views by including its interaction with the treatment. For this purpose, we asked participants about their political orientation ranging from "strongly liberal" to "strongly conservative" (on a 6-point scale) and classify them as liberal if they indicate that they are "strongly liberal", "moderately liberal" or "slightly liberal."[43] We run OLS regressions, use robust standard errors, and estimate (1) with and without controls.

Because non-compliance is low, we report ITT estimates throughout the paper, and relegate and discuss the IV estimates (effects of the treatment on the treated) to Appendix A 6.5. These estimates are similar in magnitude to the ITT estimates. Therefore, we interpret our results reported below as the effect of the bonus assignment or economic success. We pre-specified the analysis in our pre-analysis plan (AEARCTR-0004455) and we follow this plan if not stated otherwise.

6.4 Results

Our aim is to explore whether economic success affects how people think about the role of merit and whether it affects inequality acceptance (i.e., participants' attitudes toward redistributive taxation). We present three sets of results. First, we document participants' perception about merit in the work assignment and examine how these perceptions change with the exogenous bonus assignment Second, we examine how perceptions of merit affect redistributive choices. Third, we are interested in participants' willingness to learn about the underlying determinant of their success.

6.4.1 Work Assignment and Prior Beliefs

We start by looking at participants' performance in the two tasks. Table 6.1 provides an overview. It is apparent that, on average, participants in the *easy task* coded substantially more sequences of letters compared to participants in the *hard task* (35 vs. 10). However, as indicated above, the scores in the two tasks overlap to some extent. That is, the 90th percentile in the *hard task* is 17, while the 10th percentile in the *easy task* is 16. This

[43] We also asked participants about their party affiliation (Republican, Democrat, other). Our results do not change if we use this information or a combination of both questions in our analysis.

overlap results in a non-compliance to the treatment assignment in about 6 percent of cases, because the bonus is paid to a participant completing the *hard task*, instead of the participant performing the *easy task*.

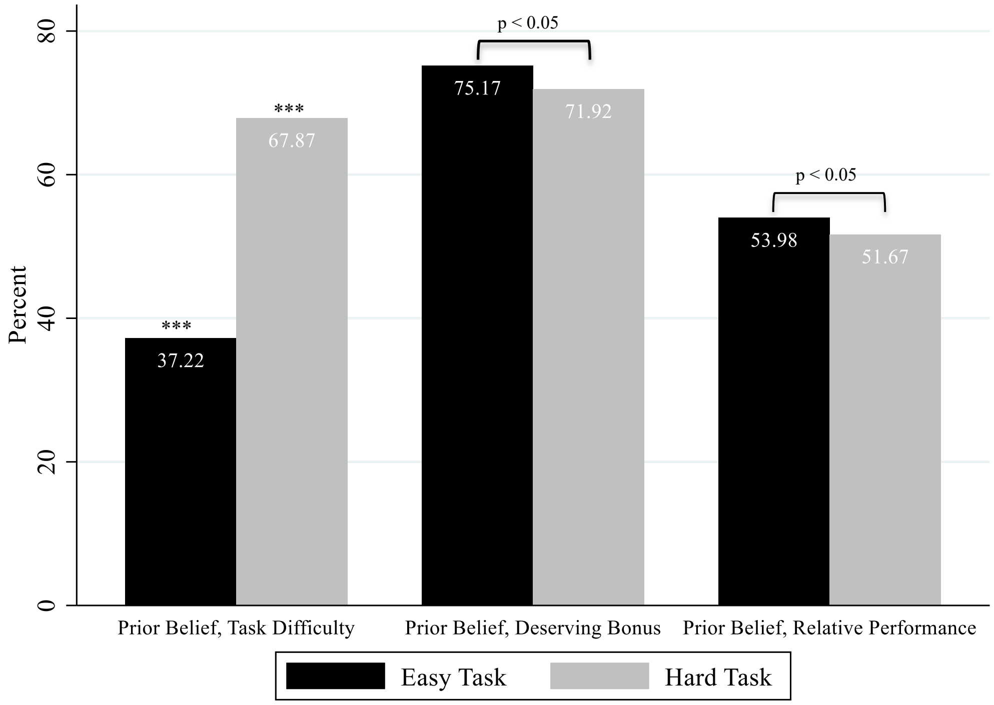
Table 6.1. Comparison of Exogenous Task Difficulty (Treatment)

Difficulty	**Mean**	**Standard Deviation**	P_{10}	P_{50}	P_{90}
Hard	10.25	5.45	4	10	17
Easy	34.86	15.47	16	33	56

Notes: Mean, standard deviation and percentile of correct letter sequences by treatment.

Figure 6.1 shows participants' beliefs regarding task difficulty, their deservingness of the bonus, and their relative performance prior to the announcement of the bonus payment. As shown in the figure, actually performing the task was a weak signal of task difficulty, as intended. Nevertheless, participants had some notion of their task assignment: 67.9 percent of participants in the *hard task* thought they had been assigned to the *hard task*, which is significantly above 50 percent ($p<0.001$, two-sided t-test). Similarly, 62.8 percent of participants in the *easy task* thought they had been assigned to the *easy task*. Again, this is significantly different from chance ($p<0.001$, two-sided t-test).

Figure 6.1. Prior Beliefs by Treatment



Notes: The Figure shows prior beliefs about task difficulty, deservingness, and relative performance that we elicited before revealing the bonus assignment. All beliefs are measured on a scale from 0 – 100: “Prior Belief, Task Difficulty”: likelihood of performing in the *hard task* in %; “Prior Belief, Deserving Bonus”: deserving the $2-bonus payment in %; “Prior, Belief Relative Performance”: perceived number of participants performing the same task with a lower score. *** indicates significant difference from 50% at the 1% level, two-sided t-test. P-values based on t-tests.

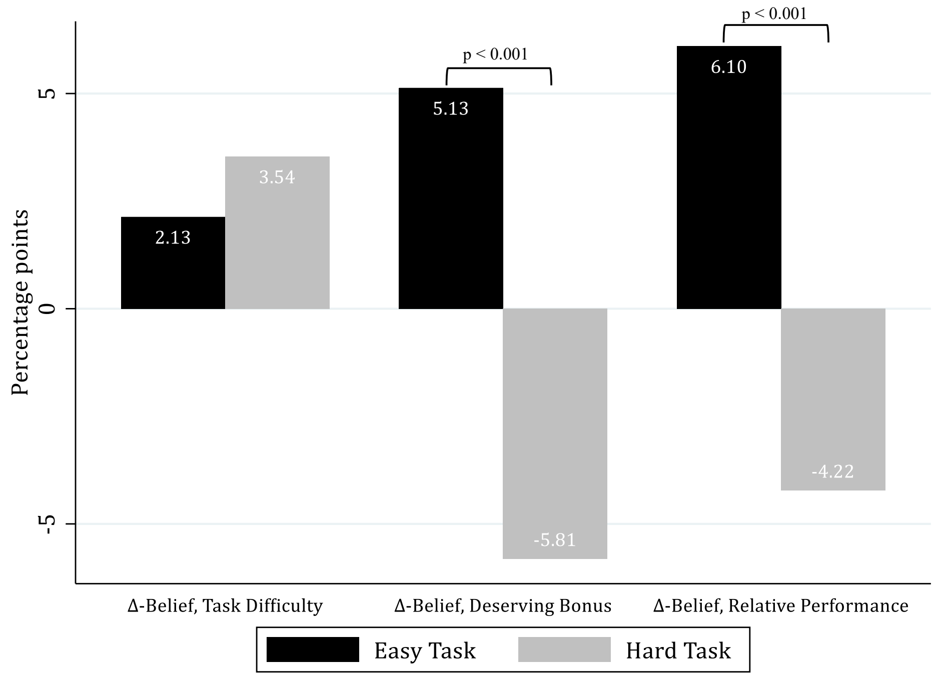
At the same time, we observe that participants in the *easy task* find themselves as more deserving of the $2 bonus compared to participants in the *hard task* (75.2 percent vs. 71.9 percent, $p<0.05$, two-sided t-test). This is notable, as it suggests that performance (i.e., coding a larger number of sequences) creates a perception that one worked *hard* and thus deserves a bonus. Indeed, performance and perceptions of deservingness are strongly correlated (each point increase in performance increases beliefs in deservingness by approximately 0.28 percentage points; see Figure 6.1). In line with this finding, we observe that coding more sequences, on average, is related to the impression that one ranks higher in the performance distribution. Specifically, participants in the *easy task* thought they outperformed 54 percent of other participants completing the same task, whereas participants in the *hard task* thought they were better than 52 percent of those completing the *hard task*. Although this difference is small, it is statistically significant ($p<0.05$, two-sided t-test). Interestingly, political views are not related to beliefs about deservingness and performance. That is, these beliefs do not differ between liberals and conservatives.

6.4.2 Effects on Posterior Beliefs

Figure 6.2 displays the difference between posterior and prior beliefs and thus illustrates how economic success (i.e., bonus assignment) changes beliefs. Notably, the bonus announcement does not change perceptions of task difficulty. However, we observe that bonus announcement results in significant changes in perceived deservingness and relative performance. We see that economic success increases perceived merit by 5 percentage points, while at the same time, failure decreases perceived merit by almost 6 percentage points. This further increases the wedge in merit perceptions between successful and unsuccessful participants. Economic success results in a 14 percentage point higher belief that receiving the bonus payment is deserved.

Similarly, success increases belief in relative performance but decreases it for those who are left empty-handed. Participants in the *easy task* think their performance is better than 60 percent of others, while participants in the *hard task* think their performance is only better than 47 percent of others. This suggests that being successful also triggers overconfidence. Indeed, if we compare how participants' posterior beliefs about relative performance compare to their true rank in the performance distribution of all participants completing the same task, we see a higher share of overconfident participants in the *easy task* than in the *hard task* (0.59 vs. 0.46; t-test, $p<0.01$). This is not the case before the bonus announcement, i.e., if we compare prior beliefs about relative performance to the true rank. In this case, the share of overconfident participants in the *easy task* is nearly the same as in the *hard task* (0.52 vs. 0.50; t-test, $p<0.37$).

Figure 6.2. Treatment Effect on Beliefs



Notes: The Figure shows the difference between posterior and prior beliefs about task difficulty, deservingness, and performance in the two conditions. All beliefs are measured on a scale from 0 – 100: "Δ-Belief, Task Difficulty": likelihood of performing in the hard task in %; "Δ-Belief, Deserving Bonus": deserving the $2-bonus payment in %; "Δ-Belief, Relative Performance": perceived number of participants performing the same task with a lower score.

Table 6.2 presents rigorous statistical evidence on how economic success impacts these perceptions. We regress the difference between posterior and prior beliefs on a treatment indicator, participants' political beliefs, and its interaction with the treatment indicator. To compare the results from this exercise with the observed patterns in the raw data we include a specification without political beliefs and covariates. There are several things to note. First, it is apparent that the regressions confirm the results presented above. Receiving the bonus has no effect on the perceived task difficulty, while it increases participants' perceptions that they deserve the bonus and that they performed better than others. Second, one can see in columns 3, 6, and 9 that controlling for participants demographic and economic status (such as gender, age, education, income, household size, ethnicity, employment status, marital status, and geographic indicators) does not meaningfully affect the estimated treatment effects. Third, political views are largely unrelated to changes in beliefs. In particular, we observe equally strong feelings of deserving the bonus among liberals and conservatives, and they do not differ in their perceptions of task difficulty. Overall, our treatment resulted in strong effects on meritocratic beliefs. Most notably, there is a sizable impact on perceptions about deservingness that is independent of political views.

Table 6.2. Regression: Change in Beliefs (Posterior – Prior)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Variable	Δ - Belief Task Difficulty			Δ-Belief Deserving Bonus			Δ - Belief Relative Performance			Effort Determines Success		
Easy task	-1.404	-0.703	-0.745	10.943***/#	11.418***/#	11.150***/#	10.324***/#	8.571***/#	8.404***/#	16.213***/#	16.358***/#	16.465***/#
	(0.934)	(1.374)	(1.373)	(0.882)	(1.478)	(1.472)	(0.713)	(1.167)	(1.173)	(1.355)	(2.123)	(2.126)
Liberal		-0.269	0.001		0.619	0.221		-0.898	-0.897		-3.905*	-4.014*
		(1.349)	(1.370)		(1.496)	(1.493)		(1.069)	(1.082)		(2.184)	(2.216)
Liberal		-1.130	-1.007		-0.776	-0.620		2.844*	2.995**		-0.185	0.0813
x *Easy task*		(1.859)	(1.867)		(1.840)	(1.841)		(1.474)	(1.485)		(2.752)	(2.751)
Constant	3.538***	3.703***	-12.65*	-5.811***	-6.190***	-8.671	-4.223***	-3.674***	-1.402	54.054***	56.439***	40.207***
	(0.683)	(0.979)	(7.446)	(0.710)	(1.221)	(7.064)	(0.530)	(0.809)	(5.827)	(1.072)	(1.691)	(10.422)
Observations	1,825	1,825	1,822	1,825	1,825	1,822	1,825	1,825	1,822	1,825	1,825	1,822
Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
R-squared	0.001	0.002	0.010	0.078	0.078	0.084	0.103	0.105	0.112	0.073	0.077	0.091

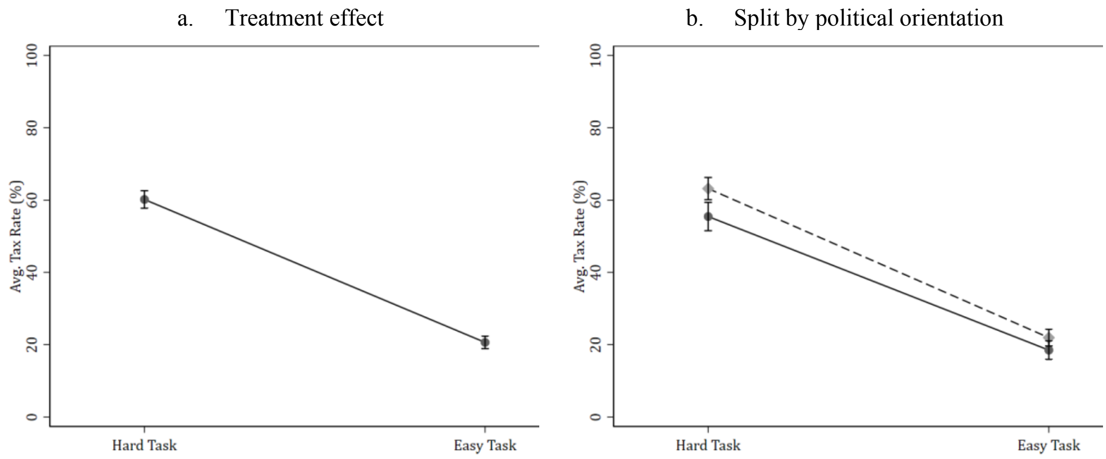
Notes: OLS-Regression with robust standard errors in parentheses. "Δ" is the difference between posterior and prior beliefs. Beliefs are elicited before the bonus assignment (prior) and after the bonus assignment (posterior). All beliefs are measured on a scale from 0 – 100: "Prior Belief, Task Difficulty": likelihood of performing in the *hard task* in %; "Prior Belief, Deserving Bonus": deserving the $2-bonus payment in %; "Prior Belief, Relative Performance": perceived number of participants performing the same task with a lower score; "Effort Determines Success": likelihood that the $2-bonus payment depends on her exerted effort in %. "*Easy task*" is an indicator for random assignment to the *easy task*. "Liberal" is an indicator for respondents who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and a set of indicator variables for white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). *** $p<0.01$, ** $p<0.05$, * $p<0.1$; # indicates significance , when using the adaptive linear step-up procedure by (Benjamini, Krieger, and Yekutieli 2006) that controls for a false discovery rate at q=0.05 for the treatment variable *"Easy task"*

6.4.3 Behavioral Measure: Redistributive Taxes

We now address how misperceptions of economic success translate into tax preferences. Panel a. of Figure 6.3 shows a strong divergence of tax rates across the two conditions: the average tax rate in the *easy task* is about 20.6 percent and in the *hard task* about 60.2 percent. Despite this divergence, it is apparent that fairness considerations matter. That is, tax rates are far from the extremes of no and full redistribution. In Table 6.3, we present regressions showing how success and failure shape redistributive tax-rate decisions. The first column confirms that the proposed tax rate is about 40 percentage points lower if participants received the $2 bonus. This effect is substantial and corresponds to a 3-times lower tax revenue. Including covariates does not change the estimate (column 2).

Next, we examine the relationship between tax-rate decisions and political views using pre-treatment information on participants' self-assessment in the political left-right spectrum. Panel b. of Figure 6.3 illustrates that economic circumstances affect redistributive preferences irrespective of political views: conservatives *and* liberals prefer high taxes if they are unsuccessful whereas they both choose low taxes if they are successful. However, it is also true that liberals propose, on average, higher tax rates than conservatives. Specifically, the difference in tax rates is about 8 percentage points in the *hard task* (t-test, $p<0.01$), while it is about 3 percentage points in the *easy task* (t-test, $p<0.06$). While this finding echoes correlational evidence that liberal voters are more favorable toward taxation (Wahlund 1992; Reed 2006; Hardisty, Johnson, and Weber 2010), the differences are small, particularly among those who are successful.

Figure 6.3. Tax Rate by Treatment and Political Orientation



Notes: The Figure shows the average tax rate across different conditions. Panel a. displays average tax rates across treatments (*hard task* and *easy task*) and panel b. shows the average tax rates across condition split by political orientation. Conservatives (solid black line) and liberals (dashed light-gray line). Error bars denote 95% confidence interval.

Table 6.3. Regression: Tax Rate and Political Views

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable			Tax Rate			
Easy task	-39.543***	-39.445***	-39.588***	-39.486***	-36.959***	-36.730***
	(1.519)	(1.528)	(1.513)	(1.523)	(2.381)	(2.409)
Liberal			5.586***	5.787***	7.729***	8.010***
			(1.540)	(1.608)	(2.535)	(2.561)
Liberal x *Easy task*					-4.276	-4.476
					(3.082)	(3.108)
Constant	60.165***	72.095***	56.753***	67.010***	55.445***	66.226***
	(1.237)	(11.360)	(1.560)	(11.43)	(1.994)	(11.454)
Observations	1,825	1,822	1,825	1,822	1,825	1,822
Controls	No	Yes	No	Yes	No	Yes
R-squared	0.272	0.277	0.277	0.282	0.278	0.283

Notes: OLS-Regression with robust standard errors in parentheses. “Tax Rate” is the redistribution rate of the $2-bonus payment in percent (0-100). “*Easy task*” is an indicator for respondents randomly assigned to the *easy task*. “Liberal” is an indicator for respondents who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions. *** p<0.01, ** p<0.05, * p<0.1

Following our pre-analysis plan, we corroborate these findings using a similar regression specification as above. In Table 6.3, column 3, we observe that, on average, liberals demand more redistribution, and thus set a higher tax rate than conservatives. Interacting treatment status with political views, we find a negative and statistically insignificant effect, which corresponds to roughly half of the difference between liberals and conservatives in the *hard task*. That is, while liberals tend to set higher tax rates than conservatives, the difference in the *easy task* is substantially smaller than in the *hard* *task*. Again, adding covariates does not change the coefficient estimates (columns 4 and 6).

6.4.4 Impact of Beliefs on Redistributive Taxes

Differences in tax preferences between liberals and conservatives are often associated with differences in beliefs about the role of effort in economic success. Liberals tend to assign luck a greater role in economic success than effort, while conservatives believe that effort dominates (Gromet, Hartson, and Sherman 2015; Karadja, Mollerstrom, and Seim 2017; Fehr, Muller, and Preuss 2020). Indeed, when asking participants whether they think economic success is the result of luck or effort, liberals are less likely to believe the bonus payment is the result of effort (see Table 6.2 columns 11 and 12).[44] This finding accords with liberals' "locus of control": that is, liberals are more likely to believe life outcomes are the result of fate or luck, and therefore beyond one's control (see Appendix Table A6.8). However, the correlation between locus of control (LoC) and political orientation is not strong, and we find that LoC itself has no impact on tax rate preferences. In the Appendix

Table A6.9, we regress the tax rate on our treatment, LoC, and the interaction of the two and find no measurable effect of LoC on tax rate preferences.

To shed light on the factors underlying tax-rate decisions, we examine how they relate to beliefs. We are particularly interested in the heterogeneity with respect to prior beliefs about the task. All beliefs (except beliefs that effort determines success) were elicited *before* the bonus announcement and thus reflect heterogeneity in beliefs that are unaffected by the bonus announcement. We include these perceptions about the work assignment one-by-one in the regressions and additionally control for a full set of covariates. Table 6.4 presents the results and reproduces, for comparison, the treatment effect on taxation in columns 1–2. In line with the previous literature, we find that a stronger belief that effort determines success reduces tax rates in both conditions (column 3). That is, participants are less willing to redistribute if they more strongly believe that the bonus is the result of hard work.

[44] The regression also reveals that in both tasks, participants believe that effort is more important than luck for success. However, the results in Table 1, column 10 highlight a strong disparity: successful participants believe to a much greater extent than unsuccessful participants that receiving the bonus is attributable to effort (16-percentage-point difference).

Examining heterogeneous effects offers a more nuanced picture of possible mechanisms, even though we observe in all specifications that beliefs are related to the tax-rate decision. We first note that beliefs about task difficulty are positively related to taxes in the case of failure, while they are negatively related when successful (column 4). That is, in both treatments we see that participants who are more certain about task difficulty react more strongly by demanding more (*hard task*) and less taxes (*easy task*), respectively. There is a similar pattern for relative performance beliefs (column 5). Believing in stronger performance is associated with demanding a larger share of the pie, i.e., beliefs are positively related to taxes for economically unsuccessful participants and negatively related to taxes for the successful. Importantly, in both cases we observe a large and significant treatment effect.

In contrast to these observations, the treatment effect is no longer significant when we include beliefs about deservingness. The regressions in column 6 reveal that a higher belief in deserving the bonus payment is associated with a higher tax rate for unsuccessful participants, but not for successful participants. More precisely, a 1 percentage point higher belief in deserving the bonus payment is associated with a 0.23 percentage point higher tax rate for unsuccessful participants, but a 0.44 percentage point lower tax rate for successful participants. Given the effect size of the interaction term, the joint effect with prior beliefs is negative and significant as well (Wald test, $p<0.01$). This suggests that the treatment effect is mediated by the belief that success is an indicator of deservingness.

Table 6.4. Regression: Tax Rate and Beliefs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Variable	Tax Rate						
Easy task	-39.543***	-39.445***	-44.599***	-27.695***	-16.049***	-6.828	3.196
	(1.519)	(1.528)	(3.765)	(3.792)	(3.751)	(4.388)	(5.302)
Prior Belief, Task Difficulty				0.176***			0.123**/#
				(0.048)			(0.049)
Easy task x Prior Belief, Task Difficulty				-0.171***			-0.091
				(0.057)			(0.059)
Prior Belief, Relative Performance					0.256***		0.126**
					(0.051)		(0.064)
Easy task x Prior Belief, Relative Performance					-0.444***		-0.226***/#
					(0.065)		(0.079)
Prior Belief, Deserving Bonus						0.228***	0.143***/#
						(0.042)	(0.053)
Easy task x Prior Belief, Deserving Bonus						-0.444***	-0.319***/#
						(0.056)	(0.069)
Effort Determines Success			-0.219***				
			(0.039)				
Easy task x Effort Determines Success			0.125**				
			(0.055)				
Constant	60.165***	72.095***	81.631***	60.496***	57.002***	51.022***	42.71***
	(1.237)	(11.360)	(11.537)	(11.690)	(11.618)	(11.614)	(11.91)
Observations	1,825	1,822	1,822	1,822	1,822	1,822	1,822
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.272	0.277	0.296	0.284	0.303	0.297	0.310

Notes: OLS-Regression with robust standard errors in parentheses. “Tax Rate” is measured in percent (0-100 percent). “Easy task” is an indicator for random assignment to the easy task. Prior beliefs elicited before the bonus assignment and measured on a scale from 0 – 100: “Prior Belief, Task Difficulty”: likelihood of performing in the hard task in %; “Prior Belief, Deserving Bonus”: deserving the \$2-bonus payment in %; “Prior Belief, Relative Performance”: perceived number of participants performing the same task with a lower score; “Effort Determines Success”: likelihood that the \$2-bonus payment depends on her exerted effort in %. Controls include sex, age, household size, log income and a set of indicator variables for white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). *** p<0.01, ** p<0.05, * p<0.1; # indicates significance , when using the adaptive linear step-up procedure by (Benjamini, Krieger, and Yekutieli 2006) that controls for a false discovery rate at q=0.05 in column (6).

6.4.5 Willingness to Correct Beliefs

Thus far, we have shown that receiving the bonus caused a shift in perceived deservingness of the bonus and in beliefs about the role of effort for success. This shift in beliefs explains the substantial disparity in the willingness to redistribute, with successful participants proposing a lower tax rate than unsuccessful participants. Recall that we randomly assigned participants to the *easy* and *hard task* and that they only learned whether they received the $2 bonus or not, but neither received information on which task they completed, nor the score of their opponent. This uncertainty in relation to task difficulty and performance allows participants to maintain distorted and self-serving beliefs about whether they deserve the bonus.

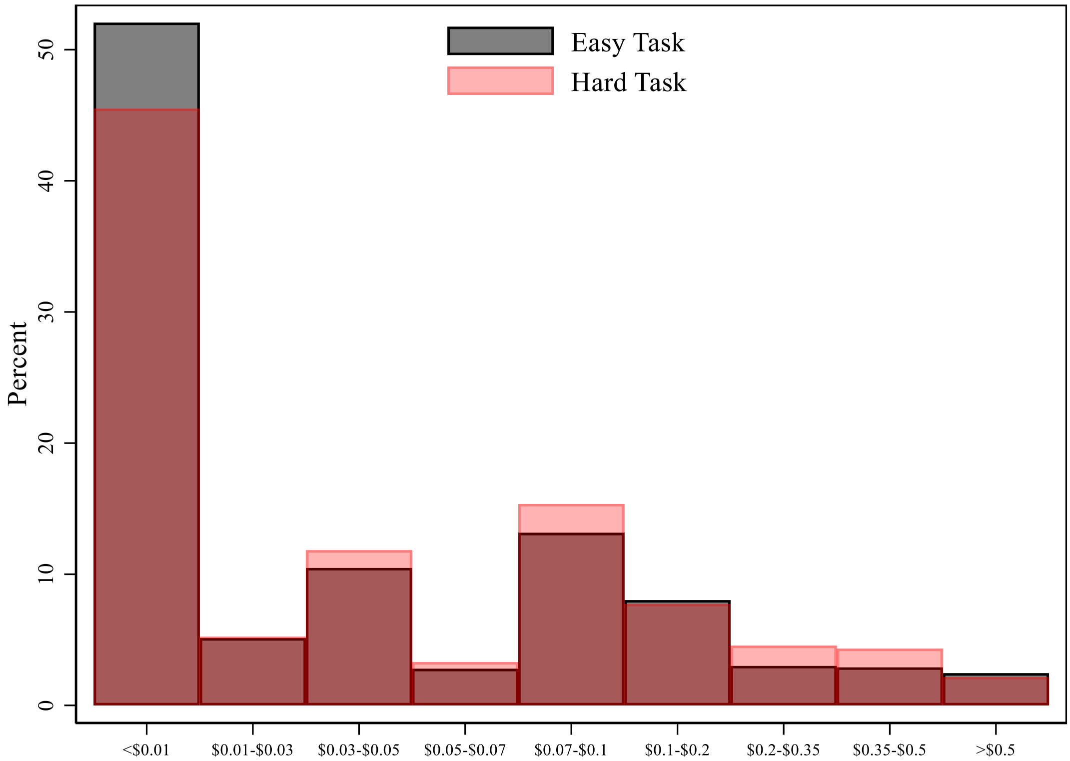
In a next step, we therefore examine whether participants are willing to pay for information that would allow them to update their beliefs about task difficulty and thus to verify their perceptions about the role of luck in success. We elicited participants' willingness to pay (WTP) with the help of an incentivized price list in the last part of the survey. That is, participants had to choose between receiving an additional sum (which varied between 1, 3, 5, 7, 10, 20, 35, and 50 cents) or information about the difficulty of the completed task and the score of their opponent.

Figure 6.3 shows the distribution of participants' WTP with consistent answers, separated by task.[45] It is apparent that in both tasks a significant share of the participants are not interested in the information and always opt for the money (46 percent in the *hard task* and 52 percent in the *easy task*) and that WTP is lower in the *easy task*. At the same time, there is a sizable share of participants who are interested in learning about task difficulty. In Table 6.5, we use interval regressions to provide statistical support for these observations. Column 1 reveals that the average WTP in the *hard task* is about 7.4 cents, and about 1 cent lower in the *easy task*, a 14 percent lower WTP. Adding controls in column 2 leaves the coefficient of the treatment variable nearly unchanged. Moreover, we see that political views play no role in willingness to obtain information: liberals and conservatives display a similar willingness to pay. These findings suggest that

[45] As is typically the case with this procedure, a few participants displayed inconsistent behavior by switching multiple times between buying information and keeping the offered amount of money. The share of inconsistent participants is 3 percent, which is at the lower end of the range observed in other papers using a similar procedure. For example, Fehr, Mollerstrom, and Perez-Truglia (2019) and Fuster, Perez-Truglia, and Zafar (2018) report 5 percent inconsistent choices, whereas Cullen and Perez-Truglia (2018) report 15 percent. Note that the low rate of inconsistent answers also speaks to the attentiveness of participants.

participants are more likely to prefer remaining ignorant when they are successful, possibly to maintain their meritocratic beliefs, and this applies to liberals and conservatives in equal degree.

Figure 6.3. Willingness-to-Pay for Information on Task Difficulty



Notes: The figure shows the distribution of respondents' willingness to pay (WTP) for information about the task difficulty (using all participants with consistent answers: *N=1,776*). The grey bars indicate the WTP in the easy task and the overlaying rose bars the WTP in the hard task. An amount smaller than $0.01 indicates that the participant always preferred money over information and vice versa for an amount larger than $0.50.

Table 6.5. Regression: Willingness to Pay for Information

	(1)	(2)	(3)	(4)
Dep. Variable		WTP		
Easy task	-0.991*	-1.109**	-0.990*	-1.109**
	(0.535)	(0.531)	(0.534)	(0.531)
Liberal			-0.635	-0.213
			(0.565)	(0.579)
Constant	7.367***	-0.892	7.760***	-0.697
	(0.403)	(3.832)	(0.558)	(3.816)
Observations	1,776	1,773	1,776	1,773
Controls	No	Yes	No	Yes

Notes: Interval-Regression, robust standard errors in parentheses. The sample includes only participants with consistent answers, i.e. we dropped 49 participants who switched multiple times between a monetary amount and receiving information. "WTP" is the willingness to pay for receiving information about the task difficulty and the score of the other participant. The variable is categorized in 9 intervals [0¢,1¢]; [1¢,3¢]; [3¢,5¢]; [5¢,7¢]; [7¢,10¢]; [10¢,20¢]; [20¢,35¢]; [35¢,50¢]; [50¢,inf). "*Easy task*" is an indicator for respondents randomly assigned to the *easy task* (treatment). "Liberal" is an indicator for respondents who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). *** p<0.01, ** p<0.05, * p<0.1

Next, we examine whether obtaining information about task difficulty and the opponents' score leads to revised tax-rate preferences. All participants who received the information (approx. 25 percent) and a random subset of the remaining participants (approx. 50 percent) had the possibility to revise their tax decision. This results in a sample of *N=1,130*. In a slight deviation from our pre-analysis plan, we look here at the likelihood of participants changing the tax rate *and* the magnitude of change. In all regression specifications, we control for WTP as participants with a higher WTP have a higher probability of receiving the information. In other words, receiving information is only random after conditioning on WTP. Table 6.6 displays the results. Conditional on WTP, receiving information increases the likelihood of revising the tax rate by 27 percent. However, once we control for treatment status and political views (including a full set of interactions) the coefficient estimate becomes substantially smaller and insignificant. Instead, we see that the likelihood of revising the tax rate is lower for liberals (columns 3 and 4). Columns 5–8 present the effects on the magnitude of change. Again, we see that receiving information leads to larger changes in the tax rate than not receiving information. Controlling for treatment status and political views indicates that changes are smaller in the *easy task* and for liberals irrespective of treatment status, while the coefficient on received information is less precisely estimated.

Together, these results suggest that participants in the *easy task* want to maintain their meritocratic beliefs to justify their tax decision, and this tendency is particularly pronounced among liberals.

Table 6.6. Regression: Revising Tax Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable	Revising Tax Rate=1				Change in Tax Rate			
Received info	0.078**	0.081**	0.009	0.024	3.787***	3.944***	4.495	4.782
	(0.036)	(0.036)	(0.072)	(0.072)	(1.231)	(1.253)	(3.376)	(3.374)
Easy task			-0.093	-0.078			-4.617**	-4.522**
			(0.06)	(0.06)			(2.087)	(2.042)
Liberal			-0.092*	-0.103*			-4.017**	-4.455**
			(0.055)	(0.057)			(1.954)	(2.003)
Easy task x Liberal			0.051	0.042			2.935	3.211
			(0.075)	(0.074)			(2.376)	(2.326)
Easy task x Received info			0.026	0.001			-1.216	-2.085
			(0.098)	(0.097)			(3.941)	(3.932)
Liberal x Received info			0.084	0.076			-1.417	-1.225
			(0.086)	(0.086)			(3.599)	(3.600)
Easy task x Liberal x Received info			0.003	0.026			2.567	3.178
			(0.123)	(0.122)			(4.525)	(4.519)
WTP	0.313**	0.278*	0.309**	0.271*	4.032	1.858	2.799	0.716
	(0.152)	(0.149)	(0.153)	(0.150)	(4.829)	(4.984)	(4.772)	(4.889)
Constant	0.285***	0.609***	0.374***	0.676***	4.647***	2.170	8.635***	5.833
	(0.018)	(0.221)	(0.045)	(0.226)	(0.534)	(6.809)	(1.690)	(6.953)
Observations	1,096	1,094	1,096	1,094	1,096	1,094	1,096	1,094
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.020	0.047	0.027	0.054	0.019	0.035	0.037	0.054

Notes: OLS-Regression, robust standard errors in parentheses. The sample includes all participants who had the opportunity to revise their initial tax decision, i.e. these are all participants who received information about the task difficulty and a random subset of participants who did not receive this information. “Revising Tax Rate=1” is an indicator for revising the initially chosen tax rate and “Change in Tax Rate” is the absolute difference between initial and revised tax rate. “Received info” is an indicator for participants who received information about the task difficulty and the performance of the other participant. “*Easy task*” is an indicator for participants randomly assigned to the *easy task* (treatment) and “Liberal” is an indicator for participants who self-identified as strongly liberal, moderately liberal and slightly liberal. “WTP” is the willingness to pay for receiving information about the task difficulty and the score of the other participant. The variable is categorized in 9 intervals [0¢,1¢]; [1¢,3¢]; [3¢,5¢]; [5¢,7¢]; [7¢,10¢]; [10¢,20¢]; [20¢,35¢]; [35¢,50¢]; [50¢,inf). Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). *** p<0.01, ** p<0.05, * p<0.1

6.4.6 Exploratory Analysis: Impact of Correcting Misperceptions on Behavior

Given the variation in beliefs about task difficulty, the impact of information disclosure may differ substantially across participants. For example, a participant who is relatively certain about having worked on the *hard task* will not be too surprised to learn that she was in fact assigned to the *hard task*, thus making her less likely revise her tax-rate decision. To capture this effect and to account for the fact that a subset of participants received no information and therefore could not update their beliefs, we estimate the following regression model:

$$Y_i = \beta_1 \cdot \left(100 - b_i^{posterior}\right) \cdot R_i + \beta_2 \cdot \left(100 - b_i^{posterior}\right) + WTP_i + \gamma \boldsymbol{X} + \varepsilon_i$$

where Y_i is an indicator for revising the tax rate (or not), or the absolute value of the change in the tax rate. $b_i^{posterior}$ is the posterior belief about task difficulty and R_i is a binary variable, indicating whether a participant received information or not. The parameter of interest is β_1, which shows the causal effect (conditional on WTP) of receiving information on task difficulty, i.e., the effect of learning that the likelihood of being in the *hard*/*easy task* is 1 percentage point higher than previously thought. The variable $\left(100 - b_i^{posterior}\right)$ controls for non-random variation in misperceptions about the task difficulty, which ensures that β_1 is identified by random variation in receiving information about task difficulty. This analysis is exploratory, as we did not specify it in our pre-analysis plan.

In Table 6.7, column 1, we see that the information shock has no effect on the likelihood of changing the tax rate. The coefficient is close to zero and precisely estimated. Controlling for treatment status (column 3) reveals that participants in the *easy task* are less likely to revise the tax rate, which is in line with the estimates in Table 6.6. This negative effect on taxes is only present among conservatives (column 5), but not among liberals (column 4) when controlling for the news shock (β_1). In contrast to these results, the information shock has a significant and positive effect on the size of the tax revisions. Learning that the task difficulty is 10 percentage point higher than previously thought results in a 5 percentage point larger change in tax rate (column 6). This is sizable given that the average bias is about 33 percentage points. Adding covariates in column 7 and controlling for treatment status in column 8 leaves the coefficient estimate for information unchanged. If we differentiate between political views, we see that liberals drive this effect. They react strongly to the information shock (column 9), while conservatives do not react at all (column 10). To summarize, the information shock has no influence on the decision to revise the tax, but if participants revise their tax rate, changes are larger for liberals who experienced a larger information shock.

Table 6.7. Regression: Misperception about Task Difficulty and Revising Tax Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Variable			Revising Tax Rate=1					Change in Tax Rate		
	All	All	All	Liberal	Conservative	All	All	All	Liberal	Conservative
Misperception x	0.001	0.000	0.000	0.001	-0.000	0.048*	0.048*	0.046*	0.081**	0.004
Received Info	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.026)	(0.026)	(0.026)	(0.032)	(0.045)
Misperception	0.002***	0.002***	0.002***	0.001*	0.002***	0.003	0.001	0.011	-0.017	0.044
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.017)	(0.017)	(0.016)	(0.016)	(0.033)
Easy task			-0.084***	-0.055	-0.112**			-3.160***	-1.329	-5.279***
			(0.029)	(0.036)	(0.047)			(0.896)	(0.985)	(1.750)
WTP	0.380***	0.361***	0.343**	0.502***	0.130	7.981*	6.432	5.765	4.831	6.998
	(0.141)	(0.138)	(0.137)	(0.189)	(0.204)	(4.466)	(4.515)	(4.449)	(5.070)	(7.773)
Constant	0.253***	0.567***	0.595***	0.360	0.790**	5.164***	4.169	5.239	-5.223	16.93
	(0.022)	(0.218)	(0.218)	(0.278)	(0.368)	(0.709)	(6.839)	(6.850)	(7.475)	(13.40)
Observations	1,130	1,128	1,128	694	434	1,130	1,128	1,128	694	434
Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
R-squared	0.028	0.051	0.059	0.066	0.083	0.014	0.029	0.039	0.051	0.058

Notes: OLS-Regression, robust standard errors in parentheses. The sample includes all participants who had the opportunity to revise their initial tax decision, i.e. these are all participants who received information about the task difficulty and a random subset of participants who did not receive this information. "Revising Tax Rate=1" is an indicator for revising the initially chosen tax rate and "Change in Tax Rate" is the absolute difference between initial and revised tax rate. "Misperception" indicates the difference between the actual task difficulty and the posterior belief about task difficulty in %. "Received Info" is an indicator for participants who received information about the task difficulty and the performance of the other participant. "*Easy task*" is an indicator for participants randomly assigned to the *easy task* (treatment). "WTP" is the willingness to pay for receiving information about the task difficulty and the score of the other participant. The variable is categorized in 9 intervals [0¢,1¢]; [1¢,3¢]; [3¢,5¢]; [5¢,7¢]; [7¢,10¢]; [10¢,20¢]; [20¢,35¢]; [35¢,50¢]; [50¢,inf). Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

6.5 Discussion

We conducted a large-scale online experiment to investigate how "randomness" in economic success affects meritocratic beliefs and redistributive preferences when participants have an opportunity to "mentally" justify their success by attributing it to their own effort. Our results demonstrate that experiencing economic success or failure leads to a significant divergence in meritocratic beliefs and inequality acceptance. Successful participants believe they are more deserving of the bonus and demand substantially lower tax rates than unsuccessful participants.

Participants are well aware of the random assignment to one of two tasks that differ in difficulty. Therefore, it is very salient for matched participants that one of them has an easier path to success. Meritocratic principles would call for redistribution in such a situation, as circumstances are beyond one's control (e.g., Cappelen et al. 2007). At the same time, however, participants have an incentive to reap the full material benefits of their success. This conflict between self-interest and fairness principles may result in cognitive dissonance (Festinger 1957). To reduce this tension, people may follow different strategies: one may reduce self-interested behavior, or, alternatively, engage in self-deceptive behavior by manipulating their own beliefs (Konow 2000).

The latter strategy appears to be visible in our data, as participants adapt their beliefs to reconcile their wish for maximizing outcomes with the wish for a fair outcome. This is evident based on the share of successful participants who believe they deserve the bonus, which increases substantially in the *easy task* after the bonus announcement. Moreover, it is in accordance with their belief that effort determines success. Consequently, to resolve this cognitive dissonance, participants try to uphold their beliefs in a self-serving manner (Loewenstein et al. 1993). This may also explain why participants in the *easy task* have a lower willingness-to-pay for information about task difficulty and score of the other participant. Köszegi (2006) refers to this the "self-image protection motive," which impels individuals to avoid information that might distort existing beliefs. That participants have a fairly good sense of the difficulty of the task they performed is indicative of the strength of this motive.

There is widespread support for meritocratic principles in modern societies. Indeed, few would disagree that people should be able to climb the ladder of success and reap its associated rewards, if they only work hard enough. Against the backdrop of rising inequality, it is therefore unsurprising that academics, policymakers and voters have repeatedly called for greater equality of opportunity to achieve this ideal. Nevertheless, in most countries, reality diverges sharply from the meritocratic ideal. Social mobility within the United States, for example, is among the lowest across developed countries, in no small part due to inequality of opportunity (Corak 2006; Chetty et al. 2014; Chetty et al. 2017). These unequal opportunities are particularly pronounced in the college admission process. The most selective colleges in the US, which also offer the best earning prospects, predominantly enroll students from affluent families. Indeed, the share of students at elite colleges coming from families in the top 1% of the income distribution is higher than the share from the bottom 50% (Chetty et al. 2020). Given the strong correlation between college affiliation and income, some individuals clearly have a much easier route to success than others. Our setting seeks to replicate this uneven playing field. Although the conditions of unequal opportunity in our setting are arguably more salient than in many real-world settings, our results nevertheless suggest that success is typically viewed as a reward for ability and effort, and not as the result of luck. Consequently, people may cling to the belief that going from rags to riches is possible given enough effort, allowing meritocratic beliefs to prevail despite structurally predetermined unfair outcomes.

This tendency to uphold meritocratic beliefs also illustrates a potentially dark side of meritocracy. According to our data, successful participants self-servingly opt for lower tax rates because they feel entitled to their high income. Their success may, however, also distort their perception of others' meritocratic credentials. The psychological literature suggests that people are more likely remember the obstacles they faced than the advantages they had (e.g., Davidai and Gilovich 2016). This asymmetry may induce people to attribute others' failure to a lack of effort and perseverance, and this tendency may be particularly pronounced in successful people who have managed to overcome the hurdles they faced. In this way, our results suggest that attribution of success solely to personal merit may be an important impediment to encouraging greater fairness and equality in socioeconomic outcomes.

Appendix 6

A 6.1. List of Covariates

- Gender (Male / Female / Other / I prefer not to say)
- Age (in years)
- Marital status (Single / Married)
- Education (Not completed high school/ High school/ Some college/ 2-year college degree/ 4-year college degree/ Masters degree/ Doctoral degree/ Professional degree (JD, MD))
- Ethnicity (White/European-American / Black/African-American / Asian/Asian-American/Pacific Islander / Hispanic/Latino / Other)
- Number of household members
- Political beliefs (Strongly liberal / Moderately liberal / Slightly liberal / Slightly conservative / Moderately conservative / Strongly conservative)
- Political party identification (Democratic Party/ Republican Party/ Other)
- US residence (Yes / No)
- Home state (list of US states)
- Employment status (Full-time employee / Part-time employee / Self-employed or small business owner / Unemployed and looking for work / Student / Not in labor force)
- Household income ($0 - $9,999 / $10,000 - $14,999 / $15,000 - $19,999 / $20,000 - $29,999 / $30,000 - $39,999 / $40,000 - $49,999 / $50,000 - $74,999 / $75,000 - $99,999 / $100,000 - $124,999 / $125,000 - $149,999 / $150,000 - $199,999 / $200,000 and more)

A 6.2. Locus-of-Control Module

A person's locus of control describes the degree to which they feel to have control over the outcomes in their life. We elicit locus of control (LoC) with a 7-item battery (Cobb-Clark and Schurer 2013), and summarize the responses in a single measure that ranges between seven (full control over life, i.e. internal LoC) and 49 (no control over life, i.e. external LoC).

a. "I have little control over the things that happen to me."
b. "There is really no way I can solve some of the problems I have."
c. "There is little I can do to change many of the important things in my life."
d. "I often feel helpless in dealing with the problems of life."
e. "Sometimes I feel that I'm being pushed around in life."
f. "What happens to me in the future mostly depends on me."
g. "I can do just about anything I really set my mind to do."
(7-point scale; Disagree strongly – Agree strongly)

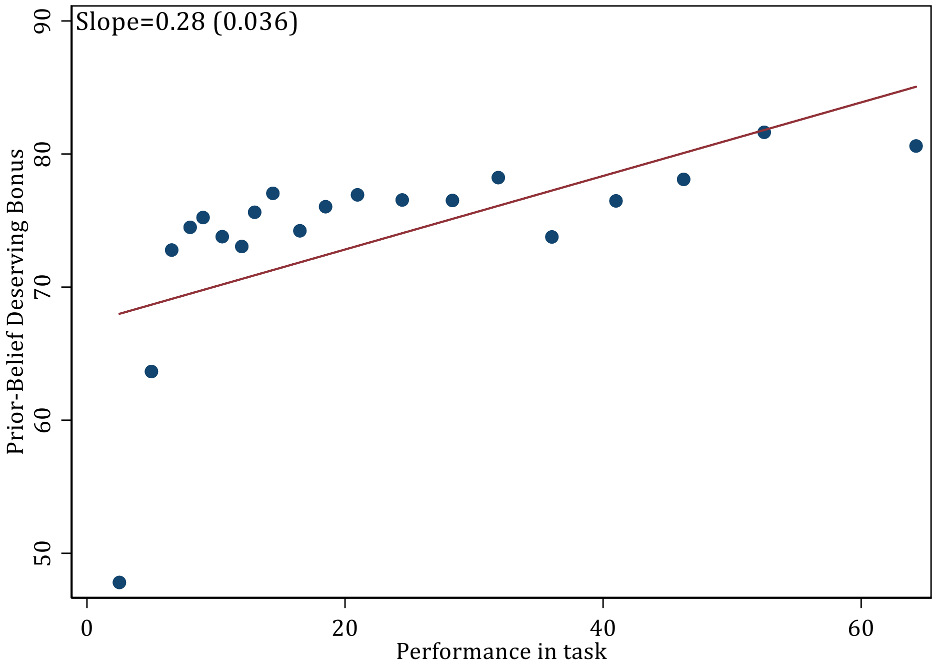
Calculating the combined locus of control index (L-o-C-Index) by summing responses to the five external items (a - e), subtracting the sum of responses to the two internal items (f - g) and adding 16. Specifically,

$$L - o - C - Index_i = \sum_{j=a}^{e} ELOC_{i,j} - \sum_{j=f}^{g} ILOC_{i,j} + 16$$

This index is therefore increasing in external control tendencies and is bounded between 7 (internal) and 49 (external).

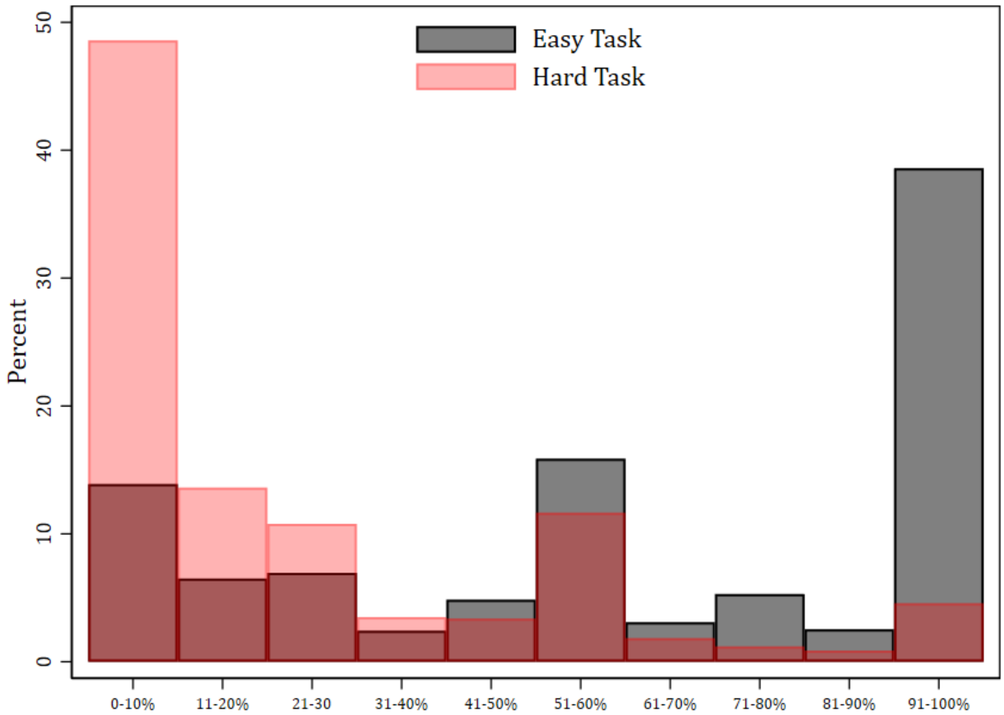
A 6.3. Additional Figures

Figure A6.1. Relationship between Task Performance and Deservingness of Bonus



Notes: Binned scatterplot showing the relationship between task performance and perceived deservingness of the bonus (Prior-Belief Deserving Bonus). Estimate based on whole sample (*N=1,825*).

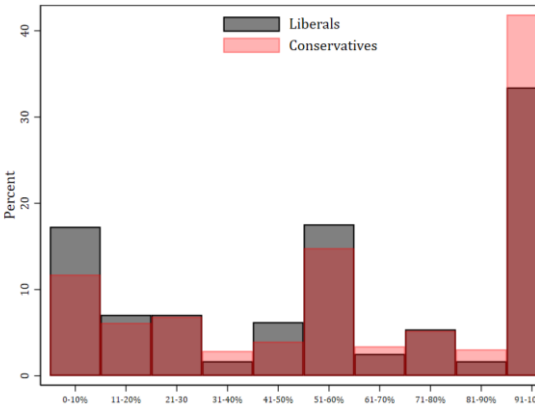
Figure A 6.2. Distribution of Tax Rates in Easy and Hard Task



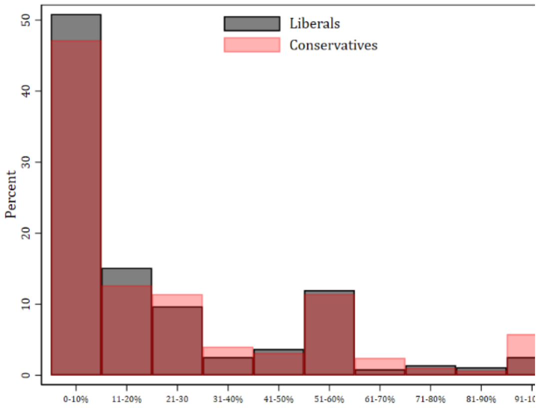
Notes: Histograms showing the distribution of tax decision in steps of 10% separated by *Easy* and *Hard* Task (*N=1,825*).

Figure A 6.3. Distribution of Tax Rates by Political Orientation

a. Hard Task



b. Easy Task



Notes: Histograms showing the distribution of tax decision in steps of 10% separated by Political Orientation. The left panel shows the distribution for the *Hard* Task and the right panel for the *Easy* Task.

A 6.4. Additional Tables

Table A6.1. Regression: Dropout on Easy Task

Dep. Variable	(1) Dropout	(2) Dropout
Easy task	-0.016	-0.013
	(0.013)	(0.009)
Constant	-0.014***	0.044***
	(0.010)	(0.007)
Observations	2,027	1,987
Controls	No	No
R-squared	0.001	0.001

Notes: (1) OLS-Regression with robust standard errors in parentheses. "*Easy task*" is an indicator for respondents randomly assigned to the *easy task* (treatment). (2) is the same regression but only considers dropouts after participants learned about the bonus assignment. *** p<0.01, ** p<0.05, * p<0.1

Table A6.2. Balance between No-Dropouts and Dropouts

	No-Dropouts (n=1825)		***Dropouts* (n = 202)**		
Variables	**Mean**	**S.D.**	**Mean**	**S.D.**	p-value
L-o-C-Index	20.95	9.16	21.88	9.94	0.17
Age (in years)	39.17	12.41	37.65	11.66	0.09
Female (in %)	52.44	49.95	45.05	49.88	0.05
White (in %)	76.66	42.31	73.27	44.37	0.28
Married (in %)	45.21	49.78	41.58	49.41	0.33
People in Household	2.66	1.42	2.70	1.37	0.72
Full-Time Employed (in %)	61.37	48.70	67.82	46.83	0.07
Part-Time Employed (in %)	11.34	31.72	11.39	31.84	0.99
Self-Employed (in %)	11.12	31.45	8.91	28.56	0.34
Not-in-Labor-Force (in %)	9.75	29.67	5.94	23.70	0.08
Income (in $)	64,784	42,589	62,203	40,993	0.41
Strongly Liberal (in %)	18.14	38.54	15.84	36.60	0.71
Moderately Liberal (in %)	22.30	41.64	24.75	43.26	0.43
Slightly Liberal (in %)	21.04	40.77	21.29	41.04	0.94
Slightly Conservative (in %)	20.27	40.22	19.31	39.57	0.75
Moderately Conservative (in %)	12.66	33.26	13.86	34.64	0.63
Strongly Conservative (in %)	5.59	22.98	4.95	21.75	0.38
Democrats (in %)	52.88	49.93	54.46	49.92	0.67
Republicans (in %)	28.27	45.05	25.74	43.83	0.45
No/ Other Political Party (in %)	18.85	39.12	19.80	39.95	0.74
Northeast Region (in %)	19.04	39.28	21.78	41.38	0.35
South Region (in %)	38.36	48.64	37.62	48.56	0.84
Midwest Region (in %)	20.75	40.56	18.81	39.18	0.52
West Region (in %)	21.84	41.33	21.78	41.38	0.98
Only High school Degree (in %)	8.98	28.61	7.43	26.28	0.46
Only Some College (in %)	24.27	42.89	21.29	41.04	0.35
2-Year College Degree (in %)	12.22	32.76	12.38	33.01	0.95
4-Year College Degree (in %)	38.36	48.64	37.38	45.05	0.06
Master Degree (in %)	12.22	32.76	11.39	31.84	0.73
Doctoral/ Professional Degree (in %)	3.67	18.81	1.98	13.97	0.22

Notes: The L-o-C-Index is a measure for locus of control (for details see main text or Appendix). The last column presents p-values from separate OLS regressions of the form $y_i = \beta_0 + \beta_0 * covariate + \varepsilon_i$, where y_i is a treatment indicator. The F-statistic from a joint significance test of all covariates is 0.83 (p-value=0.727).

Table A6.3. Summary Statistics and Balance Between *Easy* and *Hard task*

	All (n=1825)		***Hard task* (n = 907)**		***Easy task* (n = 918)**		
Variables	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	p-value
L-o-C-Index	20.95	9.16	21.16	9.12	20.74	9.20	0.34
Age (in years)	39.17	12.41	39.30	12.40	39.04	12.42	0.66
Female (in %)	52.44	49.95	53.14	49.91	51.74	50.00	0.55
White (in %)	76.66	42.31	75.74	42.88	77.56	41.74	0.36
Married (in %)	45.21	49.78	46.97	49.94	43.46	49.60	0.13
People in Household	2.66	1.42	2.63	1.37	2.69	1.48	0.33
Full-Time Employed (in %)	61.37	48.70	61.63	48.66	61.11	48.78	0.82
Part-Time Employed (in %)	11.34	31.72	10.80	31.06	11.87	32.37	0.47
Self-Employed (in %)	11.12	31.45	12.57	33.17	9.69	29.61	0.05
Not-in-Labor-Force (in %)	9.75	29.67	9.59	29.46	9.91	29.90	0.82
Income (in $)	64,784	42,589	64,388	41,709	65,089	43,241	0.73
Strongly Liberal (in %)	18.14	38.54	17.64	38.14	18.63	38.95	0.58
Moderately Liberal (in %)	22.30	41.64	21.50	41.10	23.09	42.17	0.41
Slightly Liberal (in %)	21.04	40.77	21.94	41.41	20.15	40.14	0.35
Slightly Conservative (in %)	20.27	40.22	19.96	39.99	20.59	40.46	0.74
Moderately Conservative (in %)	12.66	33.26	12.90	33.54	12.42	33.00	0.76
Strongly Conservative (in %)	5.59	22.98	6.06	23.88	5.12	22.05	0.38
Democrats (in %)	52.88	49.93	52.70	49.95	53.05	49.93	0.88
Republicans (in %)	28.27	45.05	28.34	45.09	28.21	45.03	0.95
No/ Other Political Party (in %)	18.85	39.12	18.96	39.22	18.74	39.04	0.90
Northeast Region (in %)	19.04	39.28	20.40	40.32	17.65	38.14	0.13
South Region (in %)	38.36	48.64	38.04	48.57	38.56	48.70	0.82
Midwest Region (in %)	20.75	40.56	20.18	40.15	21.24	40.92	0.57
West Region (in %)	21.84	41.33	21.28	40.95	22.33	41.67	0.59
Only High school Degree (in %)	8.98	28.61	9.59	28.61	8.39	27.74	0.37
Only Some College (in %)	24.27	42.89	23.70	42.55	24.84	43.23	0.57
2-Year College Degree (in %)	12.22	32.76	12.90	33.53	11.55	31.98	0.38
4-Year College Degree (in %)	38.36	48.64	37.38	48.41	39.32	48.87	0.39
Master Degree (in %)	12.22	32.76	12.23	32.79	12.20	32.75	0.98
Doctoral/ Professional Degree (in %)	3.67	18.81	4.19	20.04	3.16	17.50	0.24

Notes: The L-o-C-Index is a measure for locus of control (for details see main text or Appendix). The last column presents p-values from separate OLS regressions of the form $y_i = \beta_0 + \beta_0 * covariate + \varepsilon_i$, where y_i is a treatment indicator. The F-statistic from a joint significance test of all covariates is 1.09 (p-value =0.348).

Table A6.4. Comparison Between Selected Experiment Demographics and U.S. Population

Variables	Experiment	U.S. Population
Median Age (in years)	36.0	38.2
Female (in %)	52.4	50.8
White (in %)	76.7	60.4
Married (in %)	45.21	49.78
People in Household	2.66	2.52
Median Household Income (in $)	62,500	61,937
Bachelor's degree or higher (in %)	68.7	32.6
Northeast Region (in %)	19.0	17.1
Midwest Region (in %)	20.8	20.8
West Region (in %)	21.8	23.9
South Region (in %)	38.4	38.4

Notes: The U.S. Population data was taken from the U.S. Census Bureau: Median age (2018)[46], Female (2019)[47], White (not Hispanic or Latino)(2018)[48], Married (2018)[49], People in Household (2019)[50], Median Household Income (2018)[51] , Bachelor's degree or higher (25 years age or over)(2018)[52], Region (Northeast, Midwest, West, South)(2019)[53].

[46] https://data.census.gov/cedsci/table?q=female&tid=ACSST1Y2018.S0101&vintage=2018&hidePreview=true(03.04.2020)

[47] https://www.census.gov/quickfacts/fact/table/US/LFE046218 (03.04.2020)

[48] https://www.census.gov/quickfacts/fact/table/US/LFE046218 (03.04.2020)

[49] https://data.census.gov/cedsci/table?q=S1201%3A%20MARITAL%20STATUS&tid=ACSST1Y2018.S1201&vintage=2018&hidePreview=true (03.04.2020)

[50] https://www.statista.com/statistics/183648/average-size-of-households-in-the-us/ (03.04.2020)

[51] https://data.census.gov/cedsci/table?q=median%20income&tid=ACSST1Y2018.S1903&t=Income%20%28Households,%20Families,%20Individuals%29&hidePreview=true&vintage=2018 (03.04.2020)

[52] https://data.census.gov/cedsci/table?q=education&tid=ACSST1Y2018.S1501&t=Education&vintage=2018&hidePreview=true (03.04.2020)

[53] https://www.census.gov/popclock/data_tables.php?component=growth (03.04.2020)

A 6.5. IV-Estimates: Effect of the treatment on the treated

We identify the causal impact of economic success on meritocratic beliefs and redistributive taxes through the random assignment of participants to the *easy* and *hard task*. Recall that we calibrated the two tasks such that completing the *easy task* results in a better performance than completing the *hard task*. Consequently, economic success should coincide with the random task assignment.

Because treatment compliance was imperfect, we reported the *intention-to-treat (ITT)* effects in the paper. In the following we present the effects of treatment on treated (i.e. the effect of receiving the bonus – economic success – on meritocratic beliefs and redistributive taxes) by using our random assignment to the two tasks as an instrument. In specifications that include an interaction term between economic success and political view, we also instrument the interaction term with the interaction between task assignment and political view. Non-compliance was about 6 percent and the magnitude of the ITT estimates reported in the paper is similar to the IV estimates presented here.

Table A6.5. IV-Regression: Change in Beliefs (Posterior - Prior)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Variable	Δ - Belief Task Difficulty			Δ-Belief Deserving Bonus			Δ - Belief Relative Performance			Effort Determines Success		
Economic Success	-1.579	-0.788	-0.832	12.31***/#	12.80***/#	12.47***/#	11.61***/#	9.610***/#	9.394***/#	18.23***/#	18.34***/#	18.42***/#
	(1.049)	(1.539)	(1.533)	(0.983)	(1.648)	(1.637)	(0.793)	(1.277)	(1.282)	(1.509)	(2.369)	(2.364)
Liberal		-0.195	0.0623		0.652	0.297		-1.096	-1.057		-3.918*	-3.965*
		(1.435)	(1.451)		(1.585)	(1.577)		(1.127)	(1.140)		(2.294)	(2.315)
Liberal x Economic Success		-1.277	-1.147		-0.813	-0.570		3.250**	3.485**		-0.120	0.285
		(2.086)	(2.090)		(2.049)	(2.045)		(1.630)	(1.640)		(3.067)	(3.054)
Constant	3.629***	3.747***	-12.76*	-6.517***	-6.915***	-8.479	-4.888***	-4.219***	-0.923	53.01***	55.40***	40.59***
	(0.727)	(1.042)	(7.400)	(0.753)	(1.295)	(6.987)	(0.559)	(0.855)	(5.728)	(1.127)	(1.774)	(10.37)
F-statistic first stage	6891.64	7645.23	7645.23	6891.64	7645.23	7645.23	6891.64	7645.23	7645.23	6891.64	7645.23	7645.23
Observations	1,825	1,825	1,822	1,825	1,825	1,822	1,825	1,825	1,822	1,825	1,825	1,822
Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
R-squared	0.002	0.003	0.011	0.095	0.095	0.100	0.122	0.120	0.128	0.089	0.093	0.107

Notes: Two-stage least squares (2SLS) regression with robust standard errors in parentheses. “Economic Success” is an indicator for the bonus payment and is instrumented by “Easy task” an indicator for respondents being randomly assigned to the easy task (treatment). “Δ” is the difference between the posterior and the prior belief. The beliefs are elicited before the bonus assignment (prior) and after the bonus assignment (posterior). All beliefs are measured on a scale from 0 – 100: “Prior Belief, Task Difficulty”: likelihood of performing in the hard task in %; “Prior Belief, Deserving Bonus”: deserving the $2-bonus payment in %; “Prior Belief, Relative Performance”: perceived number of participants performing the same task with a lower score; “Effort Determines Success”: likelihood that the $2-bonus payment depends on her exerted effort in %. “Liberal” is an indicator for respondents who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). In columns (1), (4), (7) and (10) we report the F-statistic of “Economic Success” instrumented by “Easy task”; in columns (2), (3), (5), (6) , (8), (9) , (11) and (12) we report the F-Statistics of “ Liberal x Economic Success ” instrumented by “Liberal x Easy task” *** p<0.01, ** p<0.05, * p<0.1 # indicates significance , when using the adaptive linear step-up procedure by (Benjamini, Krieger, and Yekutieli 2006) that controls for a false discovery rate at q=0.05 for the treatment variable “Easy task”.

Table A6.6. IV-Regression: Tax Rate and Beliefs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Variable				Tax Rate			
Economic Success	-44.47***	-44.41***	-29.32***	-7.670	-17.06***	5.173	-50.05***
	(1.652)	(1.657)	(4.098)	(5.202)	(4.279)	(6.059)	(4.185)
Prior Belief, Task Difficulty			0.217***			0.152***/#	
			(0.0489)			(0.0490)	
Economic Success x Prior Belief, Task Difficulty			-0.219***			-0.121*	
			(0.0647)			(0.0643)	
Prior Belief, Deserving Bonus				0.273***		0.147***/#	
				(0.0425)		(0.0543)	
Economic Success x Prior Belief, Deserving bonus				-0.499***		-0.334***/#	
				(0.0653)		(0.0797)	
Prior Belief, Relative Performance					0.345***	0.205***/#	
					(0.0508)	(0.0655)	
Economic Success x Prior Belief, Relative Performance					-0.521***	-0.294***/#	
					(0.0717)	(0.0878)	
Effort Determines Success							-0.174***
							(0.0400)
Economic Success x Effort Determines Success							0.125**
							(0.0605)
Constant	62.71***	70.99***	56.47***	49.03***	52.99***	38.19***	79.06***
	(1.268)	(11.49)	(11.70)	(11.52)	(11.54)	(11.68)	(11.65)
F-statistic first stage	6891.64	6891.64	3418.65	7027.15	6461.62	3418.65	7580.40
Observations	1,825	1,822	1,822	1,822	1,822	1,822	1,822
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.318	0.323	0.334	0.357	0.355	0.371	0.335

Notes: Two-stage least squares (2SLS) regression with robust standard errors in parentheses. "Economic Success" is an indicator for the bonus payment and is instrumented by "Easy task" an indicator for respondents being randomly assigned to the easy task (treatment). "Δ" is the difference between the posterior and the prior belief. The beliefs are elicited before the bonus assignment (prior) and after the bonus assignment (posterior). All beliefs are measured on a scale from 0 – 100: "Prior Belief, Task Difficulty": likelihood of performing in the hard task in %; "Prior Belief, Deserving Bonus": deserving the $2-bonus payment in %; "Prior Belief, Relative Performance": perceived number of participants performing the same task with a lower score; "Effort Determines Success": likelihood that the $2-bonus payment depends on her exerted effort in %. "Liberal" is an indicator for respondents who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). In columns (1), (4), (7) and (10) we report the F-statistic of "Economic Success" instrumented by "Easy task"; in columns (2), (3), (5), (6) , (8), (9) , (11) and (12) we report the F-Statistics of " Liberal x Economic Success " instrumented by "Liberal x Easy task" *** p<0.01, ** p<0.05, * p<0.1; # indicates significance , when using the adaptive linear step-up procedure by (Benjamini, Krieger, and Yekutieli 2006) that controls for a false discovery rate at q=0.05 for the treatment variable in column (6)

Table A6.7. IV-Regression: Tax Rate and Political Views

	(1)	(2)	(3)	(4)
Dep. Variable	Tax Rate			
Liberal	5.540***	5.425***	8.064***	8.169***
	(1.490)	(1.543)	(2.597)	(2.612)
Economic Success	-44.52***	-44.45***	-41.44***	-41.08***
	(1.645)	(1.651)	(2.581)	(2.604)
Liberal x Economic Success			-5.016	-5.492
			(3.347)	(3.366)
Constant	59.33***	66.23***	57.79***	64.97***
	(1.570)	(11.52)	(2.042)	(11.54)
F-statistic first stage	6891.64	6891.64	7645.23	7645.23
Observations	1,825	1,822	1,825	1,822
Controls	No	Yes	No	Yes
R-squared	0.323	0.328	0.323	0.328

Notes: Two-stage least squares (2SLS) regression with robust standard errors in parentheses. "Tax Rate" is the redistribution rate of the $2-bonus payment in percent (0-100 percent). "Liberal" is an indicator for respondents who self-identified as strongly liberal, moderately liberal and slightly liberal. "Economic Success" is an indicator for the bonus payment and is instrumented by "*Easy task*" an indicator for respondents being randomly assigned to the *easy task* (treatment). Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). In columns (1) and (2) we report the F-statistic of "Economic Success" instrumented by "*Easy task";* in columns (3) and (4) we report the F-Statistics of " Liberal x Economic Success " instrumented by "Liberal x *Easy task*". *** p<0.01, ** p<0.05, * p<0.1

A 6.6. Locus-of-Control: Estimates

Table A6.8. Regression: Locus of Control Index on Liberal

	(1)	(2)
Dep. Variables	L-o-C-Index	
Liberal	1.308***	1.084**
	(0.438)	(0.443)
Constant	20.145***	49.977***
	(0.341)	(3.370)
Observations	1,825	1,822
Controls	No	Yes
R-squared	0.005	0.073

Notes: OLS-Regression with robust standard errors in parentheses. “L-o-C-Index” is bounded between 7 (internal) and 49 (external). “Liberal” is an indicator for respondents who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). *** p<0.01, ** p<0.05, * p<0.1

Table A6.9. Regression: Tax Rate and Locus-of-Control Index

	(1)	(2)	(3)	(4)
Dep. Variable	Tax Rate			
L-o-C-Index	0.060	0.061	-0.035	-0.033
	(0.084)	(0.087)	(0.139)	(0.141)
Easy task	-39.518***	-39.416***	-43.455***	-43.330***
	(1.521)	(1.531)	(3.884)	(3.888)
L-o-C-Index x *Easy task*			0.188	0.187
			(0.170)	(0.170)
Constant	58.889***	68.990***	60.906***	70.322***
	(2.202)	(12.242)	(3.244)	(12.350)
Observations	1,825	1,822	1,825	1,822
Controls	No	Yes	No	Yes
R-squared	0.277	0.272	0.273	0.277

Notes: OLS-Regression with robust standard errors in parentheses. “Tax Rate” is the redistribution rate of the $2-bonus payment in percent (0-100 percent). “L-o-C-Index” is bounded between 7 (internal) and 49 (external). “*Easy task*” is an indicator for respondents randomly assigned to the *easy task* (treatment). Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions. *** p<0.01, ** p<0.05, * p<0.1

Table A6.10. Regression: Change in Beliefs (Posterior – Prior) and Locus-of-Control Index

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable	Δ - Belief Task Difficulty		Δ-Belief Deserving Bonus		Δ - Belief Relative Performance		Effort Determines Success	
L-o-C-Index	-0.046	-0.032	0.086	0.076	-0.122*	-0.121*	-0.289**	-0.260**
	(0.075)	(0.076)	(0.078)	(0.079)	(0.067)	(0.0694)	(0.121)	(0.123)
Easy task	-0.457	-0.813	10.261***	10.152***	6.661***	6.701***	16.866***	16.681***
	(2.311)	(2.320)	(2.198)	(2.179)	(1.838)	(1.839)	(3.489)	(3.492)
L-o-C-Index x *Easy task*	-0.047 (0.105)	-0.028 (0.104)	0.0346 (0.098)	0.0316 (0.098)	0.174** (0.084)	0.169** (0.085)	-0.037 (0.154)	-0.015 (0.155)
Constant	4.515***	-10.782	-7.622***	-13.112*	-1.651	1.632	60.165***	50.286***
	(1.644)	(7.905)	(1.789)	(7.695)	(1.435)	(6.657)	(2.799)	(11.016)
Observations	1,825	1,822	1,825	1,822	1,825	1,822	1,825	1,822
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.002	0.010	0.080	0.086	0.106	0.113	0.082	0.093

Notes: OLS-Regression with robust standard errors in parentheses. "Δ" is the difference between posterior and prior beliefs. The beliefs are elicited before the bonus assignment (prior) and after the bonus assignment (posterior). All beliefs are measured on a scale from 0 – 100: "Belief Task Difficulty": likelihood of performing in the *hard task* in %; "Belief Deserving Bonus": deserving the $2-bonus payment in %; "Belief Relative Performance": perceived number of participants performing the same task with a lower score; "Effort Determines Success": likelihood that the $2-bonus payment depends on her exerted effort in %. "*Easy task*" is an indicator for random assignment to the *easy task*. "Liberal" is an indicator for respondents who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and a set of indicator variables for white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). *** p<0.01, ** p<0.05, * p<0.1

Table A6.11. Regression: Willingness to Pay and Locus-of-Control Index

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable	WTP		Revised Tax Rate			
L-o-C-Index	0.022	0.045	-0.103	-0.063	-0.176	-0.114
	(0.042)	(0.043)	(0.176)	(0.178)	(0.242)	(0.248)
Easy task	-0.801	-0.916	-49.037***	-48.991***	-51.08***	-50.594***
	(1.281)	(1.268)	(4.924)	(4.949)	(6.468)	(6.524)
L-o-C-Index	-0.009	-0.008	0.381*	0.382*	0.493*	0.476
x *Easy task*	(0.057)	(0.056)	(0.219)	(0.220)	(0.294)	(0.296)
Receive Info					2.321	3.365
					(8.112)	(8.123)
L-o-C- Index					0.115	0.0560
x Receive Info					(0.352)	(0.354)
Receive Info					6.227	5.291
x *Easy task*					(10.02)	(10.02)
L-o-C-Index x Receive					-0.266	-0.234
Info x *Easy task*					(0.443)	(0.442)
Constant	6.911***	-3.034	65.819***	59.377***	65.135***	60.008***
	(0.971)	(4.173)	(4.039)	(16.721)	(5.416)	(17.260)
Observations	1,776	1,773	1,130	1,128	1,130	1,128
Controls	No	Yes	No	Yes	No	Yes
R-squared			0.284	0.295	0.289	0.299

Notes: (1) - (2) is an Interval-Regression and (3) - (6) an OLS-Regression, robust standard errors in parentheses. “WTP” is the willingness to pay for seeing information about the task difficulty and score of the other participant or receiving extra money. The variable is categorized in 9 intervals [0¢,1¢]; [1¢,3¢]; [3¢,5¢]; [5¢,7¢]; [7¢,10¢]; [10¢,20¢]; [20¢,35¢]; [35¢,50¢]; [50¢,inf). We dropped 49 participants with multiple switching points, since they could not be assigned to a category. “Revised Tax Rate” is the redistribution rate of the $2-bonus payment in percent (0-100 percent) after participants decide to receive or not receive additional information about their assigned treatment. All participants who received the information and half of the participants who did not receive the additional information could revise their previous tax rate. “L-o-C-Index” is bounded between 7 (internal) and 49 (external). “*Easy task*” is an indicator for respondents randomly assigned to the *easy task* (treatment). “Receive Info” indicates a dummy variable for having received information about the task difficulty and the performance of the other participant. Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). *** p<0.01, ** p<0.05, * p<0.1

A 6.7. Screenshots of Survey and Experimental Tasks

Bot Control-Question

Before we start, please answer the following question. Note that we are only able to approve submissions that answered this question correctly.
All other submissions will be rejected. Please indicate the sum of two plus seven in the box below.
You can proceed if your entry is correct.

Next

End of Experiment (if Bot Control-Question wrong)

End of Experiment

You did not correctly answer the control question and can therefore not proceed.

General Instructions

General Instructions

You will now take part in an academic research project from Heidelberg University. Your responses and decisions in this study help us to contribute to our knowledge as a society.

It is very important for the success of our research that you **answer honestly** and **read the questions very carefully** before answering. Anytime you don't know an answer, just give your best guess. It is also very important for the success of our research project that you **complete the entire study**, once you have started. This study should take (on average) less than 12 minutes to complete.

Your participation in this study is entirely voluntary and you will remain anonymous throughout the study. Results may include summary data, but you will never be identified. By continuing, you consent to the publication of study results.

For completing this study, you will receive a **fixed payment of $0.75**. You also have the chance to **earn additional payments** during the study, depending on your decisions and the decision of a random device. Any additional payments will distributed as a bonus payment within three days upon **completion of the study**. If you have any question regarding this study, you may contact socialsciencesurvey2019@gmail.com.

Next

Locus-of-Control Questionnaire

Questionnaire

The following statements apply to different attitudes towards life and the future. To what degree do you personally agree with the following statements.

	Disagree strongly	Disagree moderately	Disagree a little	Neither agree nor Disagree	Agree a little	Agree moderately	Agree strongly
I have little control over the things that happen to me.	○	○	○	○	○	○	○
There is really no way I can solve some of the problems I have.	○	○	○	○	○	○	○
There is little I can do to change many of the important things in my life.	○	○	○	○	○	○	○
I often feel helpless in dealing with the problems of life.	○	○	○	○	○	○	○
Sometimes I feel that I'm being pushed around in life.	○	○	○	○	○	○	○
What happens to me in the future mostly depends on me.	○	○	○	○	○	○	○
I can do just about anything I really set my mind to do.	○	○	○	○	○	○	○

Next

Demographic Questionnaire

Questionnaire

Please select your gender.

- Male
- Female
- Other
- I prefer not to say.

Please enter your age.

Please indicate your marital status.

- Single
- Married

How many persons live in your household (including you)?

What is the highest level of education you have completed?

- Not completed high school
- High school
- Some college
- 2-year college degree
- 4-year college degree
- Masters degree
- Doctoral degree
- Professional degree (JD, MD)

What is your current employment status?

- Full-time employee
- Part-time employee
- Self-employed or small business owner
- Unemployed and looking for work
- Student
- Not in labor force (for example: retired, full-time parent)

What was your TOTAL household income, before taxes, last year (2018)?

- $0 - $9,999
- $10,000 - $14,999
- $15,000 - $19,999
- $20,000 - $29,999
- $30,000 - $39,999
- $40,000 - $49,999
- $50,000 - $74,999
- $75,000 - $99,999
- $100,000 - $124,999
- $125,000 - $149,999
- $150,000 - $199,999
- $200,000 and more

What is your ethnicity?

- White/European-American
- Black/African-American
- Asian/Asian-American/Pacific Islander
- Hispanic/Latino
- Other

On a continuum from liberal to conservative, how would you describe your political beliefs?

- Strongly liberal
- Moderately liberal
- Slightly liberal
- Slightly conservative
- Moderately conservative
- Strongly conservative

Which of the following political parties do you identify with most?

- Democratic Party
- Republican Party
- Other

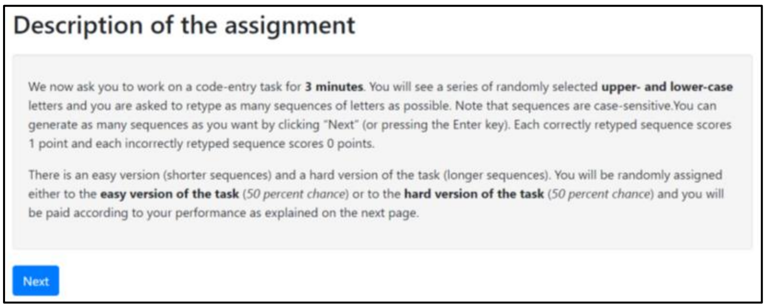
Do you live in the United States?

- Yes
- No

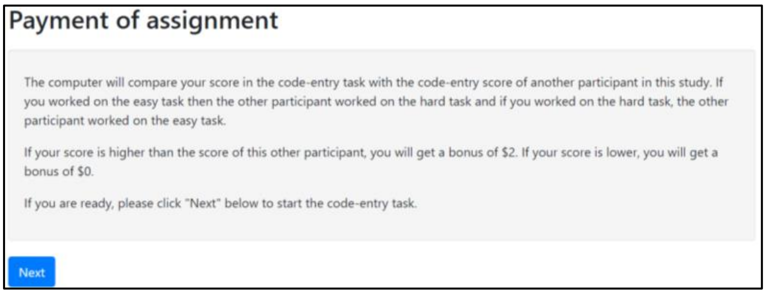
In which state do you live?

Next

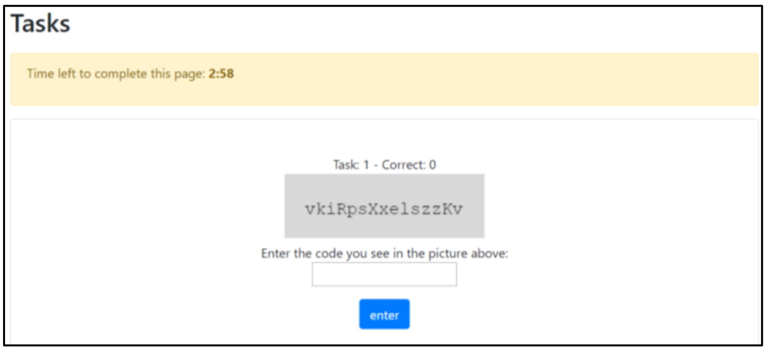
Description Real Effort Task



Description Experiment Payment



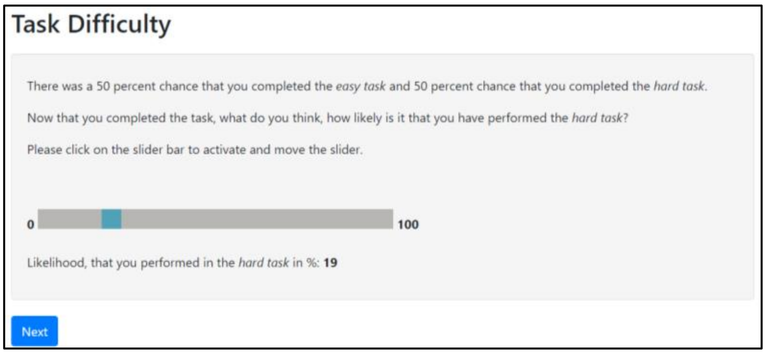
Hard Real Effort Task



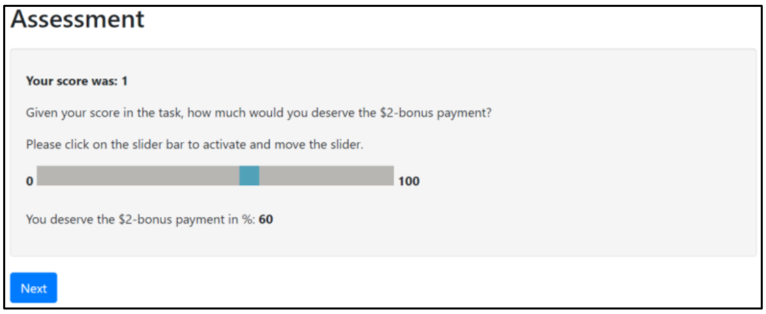
Easy Real Effort Task

Information Real Effort Task Finished

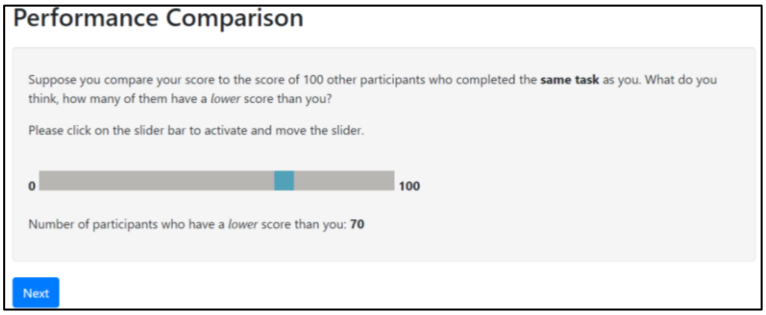
Prior-Belief about Task Difficulty



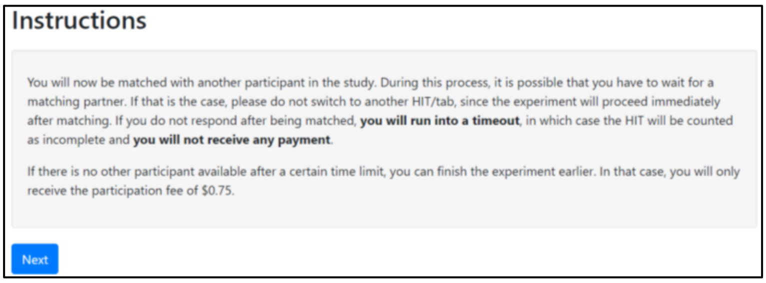
Prior- Belief about Deserving the Bonus



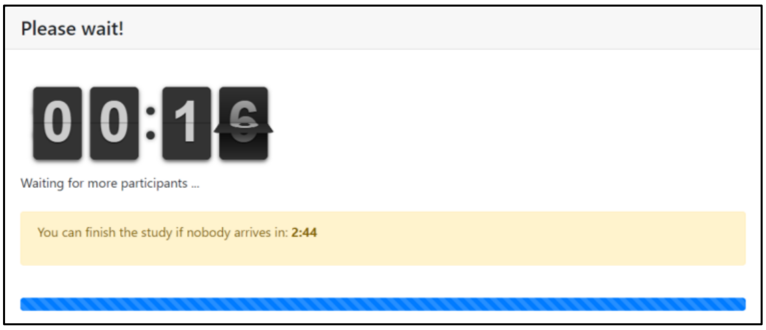
Prior-Belief about Relative Performance



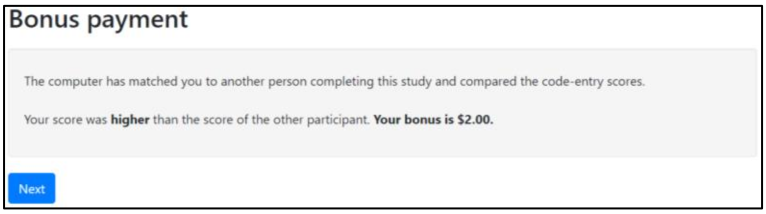
Instructions about Matching Mechanism



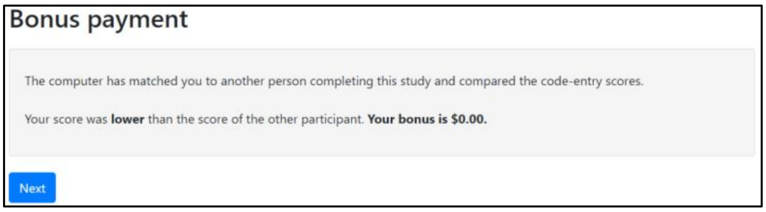
Waiting Room



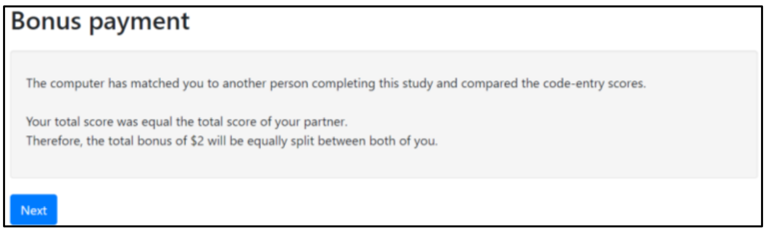
Information about Bonus Assignment (Bonus)



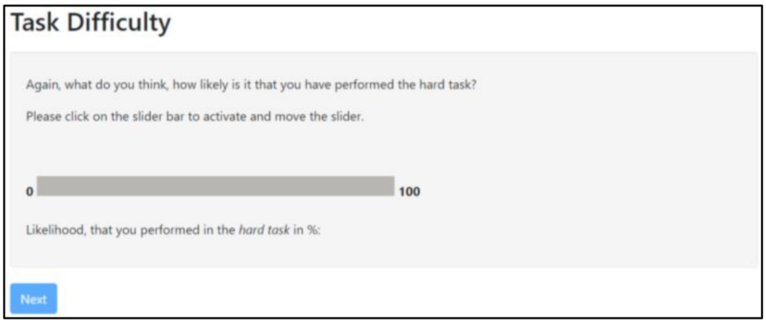
Information about Bonus Assignment (No Bonus)



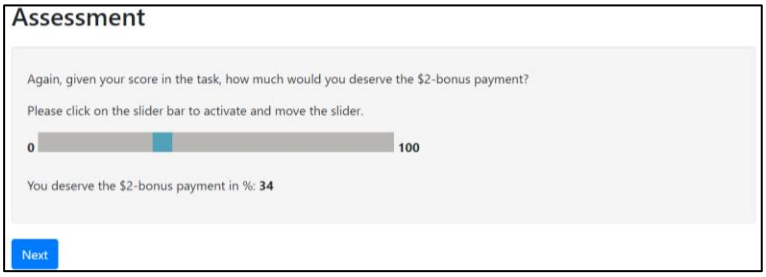
Information about Bonus Assignment (Bonus shared if equal performance)



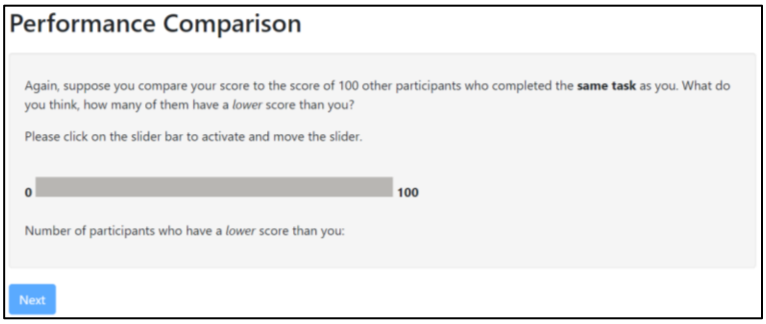
Posterior-Belief about Task Difficulty



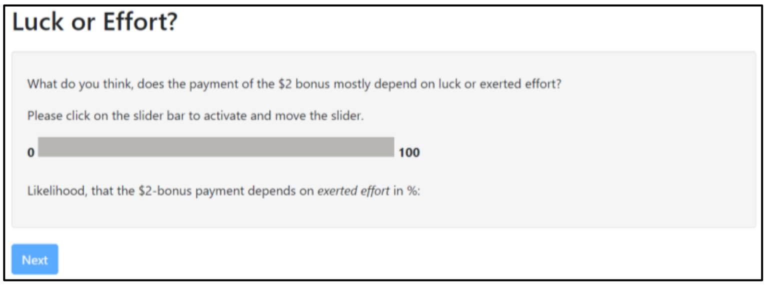
Posterior-Belief about Deserving the Bonus



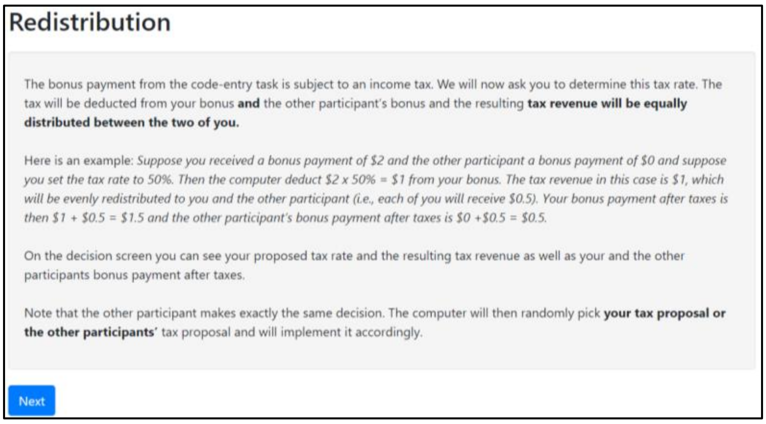
Posterior-Belief about Relative Performance



Belief about Bonus Depending on Effort



Information about Redistribution Mechanism



Redistribution

The bonus payment from the code-entry task is subject to an income tax. We will now ask you to determine this tax rate. The tax will be deducted from your bonus **and** the other participant's bonus and the resulting **tax revenue will be equally distributed between the two of you.**

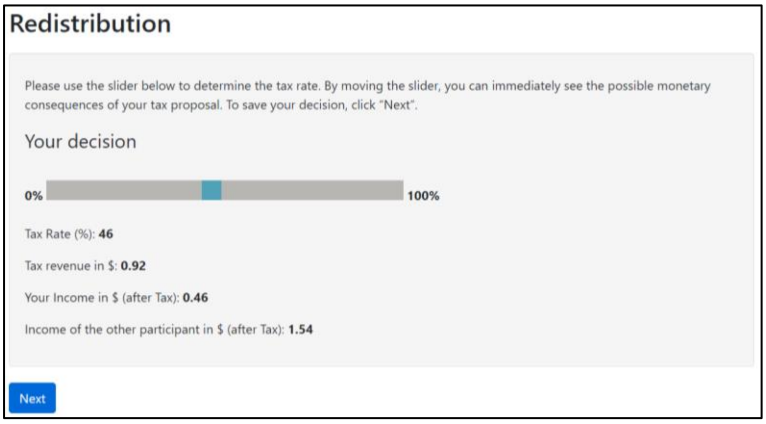
Here is an example: *Suppose you received a bonus payment of $2 and the other participant a bonus payment of $0 and suppose you set the tax rate to 50%. Then the computer deduct $2 x 50% = $1 from your bonus. The tax revenue in this case is $1, which will be evenly redistributed to you and the other participant (i.e., each of you will receive $0.5). Your bonus payment after taxes is then $1 + $0.5 = $1.5 and the other participant's bonus payment after taxes is $0 +$0.5 = $0.5.*

On the decision screen you can see your proposed tax rate and the resulting tax revenue as well as your and the other participants bonus payment after taxes.

Note that the other participant makes exactly the same decision. The computer will then randomly pick **your tax proposal or the other participants'** tax proposal and will implement it accordingly.

Next

Redistribution First Time



Redistribution

Please use the slider below to determine the tax rate. By moving the slider, you can immediately see the possible monetary consequences of your tax proposal. To save your decision, click "Next".

Your decision

0% **100%**

Tax Rate (%): **46**

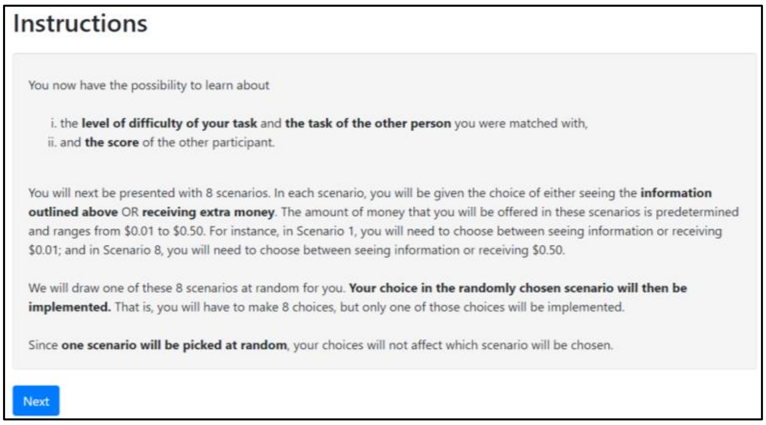
Tax revenue in $: **0.92**

Your Income in $ (after Tax): **0.46**

Income of the other participant in $ (after Tax): **1.54**

Next

Information about Price List to Receive additional Information about Partner



Instructions

You now have the possibility to learn about

i. the **level of difficulty of your task** and **the task of the other person** you were matched with,
ii. and **the score** of the other participant.

You will next be presented with 8 scenarios. In each scenario, you will be given the choice of either seeing the **information outlined above** OR **receiving extra money**. The amount of money that you will be offered in these scenarios is predetermined and ranges from $0.01 to $0.50. For instance, in Scenario 1, you will need to choose between seeing information or receiving $0.01; and in Scenario 8, you will need to choose between seeing information or receiving $0.50.

We will draw one of these 8 scenarios at random for you. **Your choice in the randomly chosen scenario will then be implemented.** That is, you will have to make 8 choices, but only one of those choices will be implemented.

Since **one scenario will be picked at random**, your choices will not affect which scenario will be chosen.

Next

Price List to Receive additional Information about Partner

Scenarios

You will now be asked to make a decision for each of the **8 scenarios**.
Note: One of the 8 scenarios is randomly chosen for you, and your choice in this scenario will be implemented. If you choose the information, you will see it on the next page. Instead, if you choose the money, you will receive the money on top of your other earnings.

Scenario 1:
Would you like to see information about your relative performance OR receive $0.01?
◉ see Information ○ receive $ 0.01

Scenario 2:
Would you like to see information about your relative performance OR receive $0.03?
◉ see Information ○ receive $ 0.03

Scenario 3:
Would you like to see information about your relative performance OR receive $0.05?
◉ see Information ○ receive $ 0.05

Scenario 4:
Would you like to see information about your relative performance OR receive $0.07?
◉ see Information ○ receive $ 0.07

Scenario 5:
Would you like to see information about your relative performance OR receive $0.10?
○ see Information ◉ receive $ 0.10

Scenario 6:
Would you like to see information about your relative performance OR receive $0.20?
○ see Information ◉ receive $ 0.20

Scenario 7:
Would you like to see information about your relative performance OR receive $0.35?
○ see Information ◉ receive $ 0.35

Scenario 8:
Would you like to see information about your relative performance OR receive $0.50?
○ see Information ◉ receive $ 0.50

Next

Result of Price List Decisions (see Information)

Result

Time left to complete this page: **0:04**

Scenario 2 was picked at random for you.

You had chosen to receive information about the assignment.

i. you completed the **hard code-entry task** (i.e., retyping sequences of **15** upper- and lower-case letters) and the other person completed the **easy code-entry task** (i.e., retyping sequences of **5** upper- and lower-case letters),
ii. and the score of the other participant in the *easy code-entry task* was **2**. (Your score was: **0**)

Next

Result of Price List Decisions (receive Money)

Result

Scenario 5 was picked at random for you.

You had chosen to receive $0.10.

Next

Redistribution Second Time

Redistribution

You now have a non-recurring chance to revise your proposal.

Your earlier proposal was 83%.

If you do not want to revise your earlier proposal, move the slider to 83%, if you want to revise your earlier proposal move the slider to a different position.

Your decision

0% **100%**

Tax Rate (%): **67**

Tax revenue in $: **1.34**

Your Income in $ (after Tax): **1.33**

Income of the other participant in $ (after Tax): **0.67**

Next

Payment Summary

Summary

You have finished the study. Thank you very much for your participation.

Your payment:

Fixed payment for study completion: $0.75.

Additional payments:

Assignment:
The computer has chosen your tax proposal for implementation.
The tax rate is 67%.
Your bonus payment after taxes is $1.33.

Scenarios:
You received $0.10 because you opted for the money instead of seeing information on the task difficulty.

Total payment:

Your total payment is $2.18.

Note that you will receive the fixed payment and the additional payments as a bonus payment within three days.

Please click "Finish" to end the study

Finish

Information if Participants run into Timeout

Unfortunately, you did not finish the HIT in time. Therefore this HIT is incomplete and you will not receive any payment.

If you have any question regarding this study, you may contact socialsciencesurvey2019@gmail.com.

Please click "Finish" to end the study.

Finish

Chapter 7

Discussion and Conclusion

While the results of the individual research projects and their scientific contributions have already been comprehensively discussed in the individual chapters, this chapter addresses the interdisciplinary approach and the methods used. The focus is, thereby, on the challenges of applying them in practice and providing meaningful advice.

The interdisciplinary element is the first to be discussed. Within this thesis, this has been reflected in the linking of experimental economics and clinical psychology. Even if both disciplines share some overlap, there are still large paradigmatic differences, for example, in approaching a research question. Cooperation is, therefore, only possible if both sides are willing to compromise. However, this becomes especially difficult when one side has specific non-negotiable terms. For experimental economists, the no-deception rule is an essential component, but this is uncommon in many areas of psychology and is often seen as an exaggerated measure.[54] On the other hand, in clinical and non-clinical psychology, permission from an ethics committee is mandatory. If these obstacles can be overcome, both disciplines can greatly benefit from each other. For instance, behavioral and experimental economics can offer closed theories, like the concept of utility functions to predict behavior, while psychology can contribute its cognitive theories that describe the unobservable processes behind behavior. Accordingly, it can be concluded that the willingness to compromise can lead to cooperation between the disciplines and consequently generate synergistic effects, allowing for research questions to be addressed that the disciplines could not answer individually.

A more general skill that can be developed through interdisciplinary cooperation is the ability to approach other disciplines without prejudice. What sounds relatively trivial is quickly put to the test in practice, as one often perceives one's research area in a biased manner by considering one's approach and methods as superior to others. Comparing the projects in chapters 4 and 5 with each other, it is noticeable that the experimental setups are very similar, but the writing styles, statistical conventions and approaches to the research questions vary noticeably. The authors of both studies consisted of economists and psychiatrists in equal parts. Still, the project in Chapter 4 was written in the form more customary for clinical psychology, while the project in Chapter 5 was mainly based

[54] It has to be mentioned that within economics, this no-deception rule is also controversial discussed (see Bonetti 1998; Hey 1998). However, this discussion will not be part of this chapter, since the majority of experimental economics treat the no-deception rule as mandatory.

on the convention's customary for experimental economics. Of course, it can be discussed which of the two approaches is better, but it is precisely this discussion, which can ensure the maintenance of openness towards new ideas, which forms the basis for scientific progress.

Against a methodological background, this thesis is able to show which new possibilities open up through online experiments. However, new methods also pose new challenges for researchers. In the early years of experimental economics research, the questions that could be answered were often restricted by technical limits, but these have almost been dissolved with the advancements in the field of computer technology.

The introduction of the zTree software in 1998, which made it possible to carry out interactive experiments within a closed network, was certainly a milestone. Requiring only little programming knowledge and with a steep learning curve, the software became the standard in many experimental laboratories around the world. However, the increasing demands of the experimenters in terms of performance and complexity resulted in the fact that this standard was no longer sufficient.

The outcome of this situation was the development of various new software solutions such as Lioness Lab in 2015 and oTree in 2016. These browser-based applications made the researchers independent of stationary laboratories and enabled the decentralized execution of experiments. In Chapter 6, various problems relating to proper scientific practice with these programs have already been discussed. Still, apart from that, many of these programs place high demands on the technical skills of researchers. For example, using oTree requires knowledge of Python, JavaScript, HTML and CSS. Although developers are constantly working on new and user-friendly applications, they run behind the needs of researchers. This matter is hardly surprising since new research questions are continually coming into focus, which place new challenges on technical solutions.

Another big part of online experiments is connecting different interfaces. First, finished programs have to be migrated to a server that the researchers themselves may have to set up; second, they have to be connected to a recruitment platform such as mTurk or Prolific. Furthermore, extended knowledge, e.g., of Amazon Web Services (AWS), can be necessary, e.g., to initiate participant payouts due to technical problems.

Ultimately, technical progress has opened many new possibilities for experiments that are far from exhausted. While, for example, virtual reality studies (e.g.,Gürerk et al. 2019) may seem exotic at the moment, this may change in the near future. Again, the downside is that an expansion of possibilities goes hand-in-hand with a disproportionate increase in complexity with regards to implementation. A stronger institutionalization within the individual laboratories or across larger research associations appears to be reasonable, since knowledge of the technical implementations would not only be centralized but the transfer and further training could also be more efficiently organized. Of course, the downsides are huge investment costs in the beginning and a potential loss of individual independence. Nevertheless, the growth in scientific knowledge, will certainly overcome these drawbacks in the long run.

In summary, this thesis provides a contribution through its individual projects, gives an impression of the current methodological possibilities within experimental economic research and shows in, a comparative way, what contributions interdisciplinary cooperation can make.

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