Social identity and social preferences
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In this thesis, I focus on the connection between social identity and social preferences. Will a friend of a decision maker be more likely to win a prize, even when her competitor bribes? Is a person with a high social status more generous or selfish than her low-status counterpart? Will employees work harder once they know they are better off than their peers? The answers to these questions are crucial for economists to form a firm understanding of behaviours in social contexts, and consequently, bridge the gap between social motives and economic actions. Since social identity is multidimensional, I focus on one dimension of identity in each chapter. To derive clear causal relationship between identity and behaviour, I use laboratory experiments in chapters 2 and chapter 3. To expand external validity, I adopt nationally representative survey data in chapter 4 and an online survey experiment in chapter 5. The thesis renders interesting findings: in-group favouritism vanishes once bribes are involved (chapter 2), high-status players are less self-fish and more efficiency-concerned when they feel entitled to the status (chapter 3), reciprocal behaviours can be generalized to workplace with a representative sample (chapter 4), and wage comparison with similar workers seems ineffective with an online experiment (chapter 5).

Jin Di Zheng, an experimental economist whose research interest relies in questions related to social identity, identifies herself as a lifetime explorer and adventurist.
Social Identity and Social Preferences: An Empirical Exploration
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Social Identity and Social Preferences:
An Empirical Exploration

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给我的外公江诗萃
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It is a far, far better thing that I do, than I have ever done; it is a far, far better rest that I go to than I have ever known.

Charles Dickens

Six years back, I flew from a gigantic city in the world – Beijing, the capital of China – to Europe and I landed on another capital, Amsterdam, thinking naively that it must be congested with traffic as well. One year after, I perfectly adapted myself to the Dutch life style to commute between my apartment and workplace on a bike with my height, through crowds of Asian tourists in front of Rijksmuseum, in flocks of young and happy people in Vondelpark, besides teams of colourful boats in twilight along the Amstel river, and in pouring rain or crazily strong wind or both for most of the time.

Two years’ study at the Tinbergen Institute (TI) was definitely a dark and tough life experience that made me to think that I was in the wrong place and that I chose the wrong track. The fact that I might not be good at rigorous, meticulous, and abstract theoretical thinking has been torturing me throughout the whole M.Phil studies. Nevertheless the pain has been eased by the five-star company from the best cohort in TI history: my favourite Dutch buddies Rugter and Ronald, the one and only Aussie David, the Canadian break-dancer Travers, the German squad Uwe and Elisa, the Italian gentleman Luka, drama-queen Swapnil, happy Ilke, and my beloved Chinese family Shawny (肖晓), Yang (刘洋), Zhiling (王芷苓), Zhaokun (张兆琨), and Xingchen (朱醒辰).
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Working with Robert and Arthur is an amazing experience. Though they are not working on the same project with me, I feel the same passion and creativity from both of them. When it comes to a new research idea, I can totally imagine Robert talking about a plausible theory with his eyebrows flying and arms stretching, and Arthur writing a five-page long document proving what his theory would predict. Memories blur but images remain. I could see those afternoons in the ninth-floor office in Rotterdam where I was brainstorming with Robert; there were drawings of our experimental design on the whiteboard, together with his kids’ next to them; and outside the rain was pouring over Erasmusbrug. I could also see the days when CREED’s offices were located temporally in the P building where the walls were paper-thin. Gönül was arguing with Arthur about a technical detail in our joint project for half an hour and I was staring at the traces of rain on the window. In those moments, there was only one voice in my brain: “What a crazy scene! What a lovely scene!”

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Jin Di Zheng

郑金笛

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Chapter 1

Introduction

Many economic activities are fundamentally influenced by individuals’ identities – a person’s sense of self – and many phenomena in economic interactions cannot be well explained by current economic theories (Akerlof and Kranton 2000). For instance, Asian-Americans females perform better in a math task when they have been primed with their Asian identity than when they were primed with their identities of being females (Shih et al. 1999). The authors argue that by priming one of the girls’ multidimensional social identities, the powerful stereotypes which are associated with each identity are triggered accordingly, i.e., Asians possess excellent quantitative skills and women do not; therefore, researchers are able to change their performance by simple priming.

Incorporating identity into standard economic analysis provides a compelling approach to understanding important phenomena in our society such as discrimination, group conflicts, and social preferences. Will a friend of a decision maker be more likely to win a prize, even when her competitor bribes? Is a person with high social status more generous or selfish than her low-status counterpart? Will employees work harder once they know they are better off than their peers? The answers to these questions are crucial for economists to form a firm understanding of behaviours in social contexts; and consequently, bridge the gap between social motives and economic actions.

In this thesis, I focus on the connection between social identity and social preferences.
Since social identity is multidimensional, I focus on one dimension of identity in each chapter. In chapter 2, I study in-group and out-group membership. In chapter 3, I investigate social status. In chapter 4 and 5, I focus on comparable employees at work. To derive clear causal relationship between identity and behaviour, I use laboratory experiments in chapters 2 and chapter 3. To expand external validity, I adopt nationally representative survey data in chapter 4 and an online survey experiment in chapter 5.

In chapter 2, I incorporate group identity into a bribery setting and explore the role of social distance in bribery. Social distance describes the perceived closeness between individuals in groups. There are three stages in the experiment. We start with manipulating social distance, using the minimum group paradigm. Then we conduct tasks to reinforce the manipulation, such that subjects from the same group (in-group) have a shorter social distance than that with an out-group person. In the third stage, two subjects perform a real effort task and submit their results to a judge. The judge decides who is the winner to receive a ten-euro award. The performers can bribe the judge.

The experimental evidence suggests that both bribes and in-group favouritism bias judge’s decision-making. The results suggest that if bribes are unavailable, having the same group-membership as the judge significantly increases one’s chances of winning; however, if bribes are allowed, sharing the same group membership has no such effect. The findings in this experiment set a first step to understand how corruption interacts with social identity. For instance, when will a closer relationship work or not work in corruption? Can anti-corruption policies such as staff-rotating – to create greater social distances – can effectively deter corrupted behaviour?

In chapter 3, I investigate the causal relationship between social status and pro-social behaviour. Using a laboratory experiment, the results show that a high status person is more generous and efficiency-concerned. However, this only occurs when her status is considered to be earned. Social status, in this study, is defined as a ranking in a group that is publicly recognised by its members. In a novel one-shot, two-player, two-stage game,
a high-status subject is paired with a low-status subject to determine joint production. Player 1 gives a non-binding advice $a$ to player 2, suggesting an effort level for player 2. Player 2 learns about $a$ and determines the actual effort for both players. Total effort is constant and it is costly to exert effort. Deviation from the advice causes a loss in the team payoff.

Results show that pro-social behaviour is status-specific: 1) High-status players 2 are less selfish than their low-status counterparts. 2) When status is earned, there are even fewer selfish players 2 with high status; moreover, they deviate less from the advice such that less efficiency is lost. These significant differences disappear, however, if the status is assigned randomly.

Chapter 4 empirically investigates the relations of reciprocity with effort and job satisfaction, using a representative survey (German Socio-Economic Panel) from Germany. I construct a reference wage and measures for reciprocity for each worker. Depending on how much more or less a worker earns than the reference, I investigate how effort and job satisfaction react to reciprocal tendencies. The results show that reciprocity is associated with effort, and that this relation is strengthened by the interaction between reciprocity and relative income, compared to similar peers. Specifically, I find that, for workers who earn more than their peers in the economy, the unpaid overtime hours increase with relative income, and the more so, the stronger the workers’ positive reciprocity is. There is no such interaction for negative reciprocity. Job satisfaction is related to reciprocity, but insensitive to the interaction between reciprocity and relative income.

The final chapter follows up on the idea in chapter 4. In an artificial field experiment, we use an online platform to test the effect of more detailed wage information on the employees’ satisfaction and expected effort. This experiment was registered at the American Economic Association Social Science Registry for Randomized Controlled Trials (www.socialscienceregistry.org/trials/289).

We implement a survey experiment on a nationwide website (www.loonwijzer.nl) in
the Netherlands. After gathering personal characteristics from the survey participants, we elicit respondents’ beliefs on their own position in the wage distribution of peers. Next, we provide the ‘true’ position in the relevant wage distribution (using historical data) to half of the respondents, who are randomly chosen by the website. We compare job satisfaction, wage satisfaction, the intention to work hard, the intention to work overtime without payments, and the intention to stay with the current job in the treatment group, to those employees in a control group without information about the actual relative income. The results show that the treatment effect on satisfaction of providing additional information is marginal and insignificant. In light of the failure to affect wage satisfaction, we decide not to continue examining heterogeneous treatment effects with respect to reciprocity or when employers dismiss underperforming employees. We provide a few conjectures for why we fail to get significant treatment effects.
Chapter 2

Friend or Foe? Social Distance in Bribery

2.1 Introduction

People are often asked to make a judgement about others. Such judgements can be informal like assessing which hairdresser to go to or formal such as deciding on whom to hire for a job. Some judgment calls may be affected by monetary factors such as bribes (Abbink et al. 2002, Gneezy et al. 2015).

Non-monetary factors might be at least as important, however. For example, judgment calls might be affected by social ties that are irrelevant for the issue being judged (Charness and Gneezy 2008, Chen and Li 2009, Fiedler et al. 2011). The mere fact of having a close relationship with someone might bias one’s judgement in that person’s favour, irrespective of objective measurements of her acts. In academia, for instance, coming from the same hometown as the members of a selection committee increases candidates’ chances (Fisman

This chapter is based on the work of Zheng et al. (2017a). Financial support of the Research Priority Area Behavioral Economics of the University of Amsterdam is gratefully acknowledged. We are grateful to Aaron Kamm and Max Hoyer for comments on programming.
et al. 2017). In sports, there is empirical evidence showing that soccer referees tend to make more beneficial calls to players who are from the same country as they are (Pope and Pope 2015). In investment, investors exhibit home country bias and are reluctant to diversify their equity portfolio across nations (French and Poterba 1991). In the labor market, “it’s whom you know that counts” (Xie 2017) and for some types of jobs a majority of job seekers find their jobs through personal contacts (Granovetter 1995).

Similar observations can be made with respect to politics and business. In China, *guanxi*—a well-known Chinese term referring to interpersonal connections— is important in the business world for “finding solution to a business rather than personal problem by using ‘personal’ connections” (Fan 2002). When the business to government relationship relies too much on *guanxi*, rent-seeking behaviour is likely to occur, as are nepotism, pork-barrel politics, and corruption, all of which may damage the economy in various ways (Bardhan 1997). In Italy, for example, politically-connected firms enjoy a positive revenue premium from domestic sales (Cingano and Pinotti 2013).

In 2014, the biggest political scandal in China was the case of Zhou Yongkang, a former Minister of Public Security and a former member of the Politburo Standing Committee (PSC, China’s highest decision-making body). He was convicted by the intermediate court of bribery, abuse of power and the intentional disclosure of state secrets in June, 2015. He received a life sentence for directly accepting more than 1 million dollars in bribes, whereas his family was said to have extracted more than 20 times more than Zhou had received himself. The interesting part of Zhou’s story is that all names appearing in his net of corruption are characterised by kinship, nepotism, and peers: they are either relatives, previous co-workers, or alumni of the same university.¹ It seems that Zhou’s corruption (i.e., biased judgment calls) was strongly aimed at people with whom he shared a close relationship.

The relationship between a close social distance and biased decisions when bribery is

involved is an understudied phenomenon, however. We address this issue in this chapter. More specifically, this chapter addresses a two-fold research question. First, when person A is asked to judge person B, does a closer relationship between A and B result in a judgement that deviates more from the ‘objective’ judgement than were the relationship more distant? Second, how do bribes and social relationships interact in affecting such a judgement?

Consider the following scenario: a judge has to decide who is a winner of a competition between a pair of participants. The winner will be awarded with a prize and the loser gets nothing. During the process, the participants cannot observe each other’s performance; meanwhile, it is common knowledge that one participant has been the judge’s friend for quite a while but the other participant performs better. Who would be declared the winner, the better performer or the friend? Would this change if the competitors could send monetary bribes to the judge?

Our aim is to study the interaction between bribery and social distance in a laboratory experiment. To induce variations in social distance in the laboratory, we apply an extended version of the minimum group paradigm (Tajfel 1970, Tajfel et al. 1971, Turner et al. 1979, Tajfel and Turner 1986, Chen and Li 2009). This creates two ‘social groups’. Two individuals within the same group (‘in-group’) are induced to have smaller social distance than two from distinct groups (‘out-group’). We then introduce the possibility of corruption by using the ‘Bribery game’. Subjects form triads consisting of one judge and two performers. The performers first each perform a real-effort task. Then, the judge nominates a winner out of the two performers. In some treatments, the performers have an opportunity to bribe the judge in an attempt to affect her decision. We distinguish between three situations with respect to the social distances between the three triad members: the judge has an out-group relationship with both performers (‘Both out-group’ treatment); she has an in-group relationship with one performer and an out-group relationship with the other (‘One in-group’ treatment); or all members have in-group
relationships (‘Both in-group’ treatment).

The laboratory provides a natural environment to address our research questions. It has various benefits compared to observational field data. Since we are interested in the effect of social distance on decision making, it is important to tease out noise surrounding social distance that may systematically affect decisions. For instance, people who exhibit self-regarding preferences might be more likely to respond to bribes than those who have a preference towards fairness. If the extent of social preferences were correlated with group membership, this could bias the results obtained from observational field data. The minimum group paradigm in the laboratory ensures that the differences between members of two groups as small as possible other than the distinct membership itself, because it creates groups based on criteria that are deemed unrelated to the decisions at hand. Moreover, collecting data about corruption and bribes in the field has practical limitations. After all, not many people will likely to be happy to report having given or accepted bribes. Finally, laboratory control allows us to make inferences about causal relations that are not easily attainable possible in the field. For example, we can isolate the effect of bribes by comparing two cases that are identical except for the possibility of bribing.

To address our research questions about the effects of (the interaction between) social distance and bribery, we compare the effects of social distance on decision making when bribery is not available to what happens if bribes can be offered. Social identity theory (Turner et al. 1979) predicts what will happen without bribes. Multiple studies on group identity have shown an in-group favouritism in cooperation with even the “minimum group”. For our experiment, this straightforwardly yields the prediction that an in-group member is more likely to be selected the winner when competing with an out-group member (from the perspective of the judge). In a seminal paper, Chen and Li (2009) show that when matched with an in-group member, participants are more prosocial. More specifically, they are more altruistic, less inclined to punish misbehaviour, and more
efficiency-concerned. Goette et al. (2012) extend the analysis of Chen and Li (2009) by considering naturally formed group in the Swiss Army. They find that soldiers cooperate more in a prisoner’s dilemma game when they are matched with in-group members.\(^2\)

Another strand of literature on social ties is also relevant to our study. When the strength of social ties is defined as “the extent to which they (two individuals) care about each other’s well-being” (Van Dijk and Van Winden 1997), Bosman and Van Winden (2002) study naturally-occurring social ties by allowing friends to jointly sign up for a laboratory experiment. The platform used is the power-to-take game, where a proposer is grouped with two recipients. The proposer can confiscate some (or all) of the recipients’ endowments; in response, a recipient can destroy her own endowment, leaving nothing for the proposer to take. The authors distinguish between treatments where the recipients are friends and where they are strangers to each other. The results show that friends punish the proposer more often by destroying their endowment and are also more likely to coordinate on this punishment (Reuben and Van Winden 2008). Other studies in experimental economics show that a reduced social distance increases the likelihood of risk-sharing (Hayashi et al. 1996, Fafchamps and Lund 2003, Angelucci et al. 2016), increases reciprocity in a “Lost Wallet” game (Charness et al. 2007), and cultivates trust in trust games (Fiedler et al. 2011). Meanwhile, it decreases rejections of unfair offers in the ultimatum game (Kim et al. 2013). However, the role of social distance in bribery and corruption remains under-investigated.\(^3\)

For predicting the interaction between social distance and bribery, two competing theories play a role. On the one hand, social identity (or social ties) theories predict that a judge will prefer to nominate an in-group performer. A counteracting force derives from

\(^2\)Goette et al. (2012) conclude, however, with a caveat that groups with real social interactions behave profoundly different from minimal groups in some cases. For instance, deception by an in-group soldier is punished more harshly than that by a ‘minimal in-group’ soldier.

\(^3\)An exception is a recent study by Rong et al. (2016), who find in a committee decision making process that sharing monetary incentives improves truth-telling in the absence of social identity, while holding a different social identity reduces truth-telling behaviour compared to situations with shared identities or no identities.
reciprocity theory as discussed in Fehr and Gächter (2000), for instance. We are interested in environments (also implemented in our experimental design) where a judge will keep all bribes, irrespective of whether she responds to a bribe in the manner expected by the briber. In this case, the judge has no direct monetary incentive to adapt her decision in response to a bribe, which is received before the decision is made. However, one may also view a bribe as a ‘gift’, which creates an environment where gift exchange might take place (Fehr et al. 1993). In this perspective, a reciprocal judge might reward the higher briber by awarding her the prize. In situations where the performer with the lower performance offers the higher bribe, bribes may thus affect the decision. If one of the two performers shares the same social identity with the judge, this ‘bribe effect’ might interact positively or negatively with the effects of social identity.

Our experimental results show that social distance plays a role in the judge’s decision to award the prize. We find no effect, however, of a closer social distance on the decision to bribe, nor on the amount bribed. More than 60% of the performers bribe, irrespective of their social ties with the judge. Bribes seem to matter for the judge: a better performer only loses the prize if she bribes less than her competitor. Secondly, sharing the same group membership, that is, having a shorter social distance does impact judges’ decisions. In-group members have a higher chance to be winners when bribery is not possible. When bribery is made available, in-group membership crowds out both merit (nomination based on performance) and reciprocity (nomination based on bribes). We show that judges value both performance and bribes as long as the social distance between two performers are the same; as soon as there is an in-group performer competing with an out-group performer, the importance of performance and bribes decreases.

This chapter contributes to the literature in the following two ways. First, we contribute to the methodology of creating minimum groups, which allows us to systematically vary the social distance between subjects. The previous literature has proposed various ways to study social distance. For example, some studies vary social distance by changing
the language or procedures (Hoffman et al. 1996), by conducting the same experiment in the classroom and on the Internet with students from different countries (Charness et al. 2007), by revealing the family name of the players (Charness and Gneezy 2008), or by participating in a common task and through communication to form group identities (Robalo et al. 2017). Our approach differs from such previous studies, by introducing paintings by children under the age of four. This approach benefits from minimising the differences in unobservable characteristics between subjects from two different groups. To be consistent with the minimum group paradigm in Turner et al. (1979) or Chen and Li (2009), group identities are created based on trivial criteria. However, instead of using masterpieces of Klee and Kandinsky whose works have been exhibited around the world and been extensively studied by social identity theory for decades, our choice of unskillful paintings seems to make the selection criterion even more trivial. In this way, we ensure that no homegrown norms can exist concerning the paintings that one ought to prefer. Using toddler’s paintings, there is less danger of spurious correlations between individuals’ sensitivities to paintings and the behaviour we are studying here.

Second, our experiment contributes to studies on bribery by applying an arguable more realistic setting. To the best of our knowledge, our work is the first to study the interaction between in-group favouritism and bribery in a laboratory experiment. Though bribery has often been studied by experimental economists (Abbink et al. 2002, Abbink 2004, Abbink and Hennig-Schmidt 2006, Abbink et al. 2016, Banerjee 2016, Barr and Serra 2009, Gneezy et al. 2015), no prior research addresses how it interacts with the social distance between the briber and the bribee. Moreover, previous experiments have typically involved abstract decision-making environments. We believe that the experiments used in this chapter increase the external validity of this type of studies by using a real effort task where performance can be objectively measured and performers can attempt to influence judgements by bribes. We believe that the findings in our experiment will help in forming a better understanding of the effects of anti-corruption policies like staff rotation, which
target at enlarging social distances among government officials.

The remainder of this chapter is structured as follows: Section 2 describes our experimental design. Section 3 introduces the data and results. Section 4 discusses the robustness of our results in the experiment, and section 5 concludes.

2.2 Experimental design

The experiment took place in December 2014 and February 2015 at the CREED laboratory of the University of Amsterdam. The experiment was programmed in PHP/MySQL. In total, we recruited 336 (14 sessions and exactly 24 subjects per session) participants from the CREED subject pool. On average each session lasted for approximately one hour and the average earnings were 21 euro, including a fixed 7 euro show-up fee.

There are three parts in the experiment. We give instructions separately at the beginning of each part. In the first two parts, we generate groups where the social distance between out-group members is larger than between in-group members. In the third part, we conduct a bribery game in which a triad is formed consisting of one judge and two performers. The judge selects one of the two performers as the a winner of a contest. There are three possible ways to form the triads based on their social distances *vis-à-vis* each other, as determined in the previous parts of the experiment.

We apply a full-factorial $3 \times 2$, between-subject design and apply a real-effort task. On one treatment dimension we distinguish between three triad compositions: the judge may come from a different group than both performers (who therefore share out-group identity; ‘both out-group’), the judge and one performer share group identity while the other performer is from the other group (‘one in-group’), and the judge and two performers can all be from the same identity group (‘both in-group’). In the second treatment dimension we separate the cases where performers can bribe the judge from cases where they cannot.

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4Experimental instructions and survey questions can be found in Appendix A.
2.2.1 Minimum group

Upon arrival at the laboratory, each of the 24 subjects first individually reviews three pairs of paintings. They are asked to indicate their appreciation for paintings by dividing 100 appreciation units between the two paintings in each pair. More units indicate a higher relative appreciation. An example of this task is shown in Figure 2.1. Subjects use a slider to adjust their relative preference for the two paintings. The slider keeps the sum equal to 100. The default position for the slider is at the middle of the bar (at 50-50), indicating indifference between the two paintings in a pair. This task is not incentivised. Once subjects have finished, they are separated into two groups based on their preferences for paintings on the left or right. One group has a higher score for paintings on the left and the other group has a higher score for paintings on the right. All subjects need to change to a new seat. One group moves to new seats in the original laboratory, the other to seats in an adjoining laboratory. Participants choose between pairs of paintings made by children under the age of four without knowing the painters of the paintings.\(^5\)

2.2.2 Social distance manipulation

After subjects have been reallocated across the two laboratories, they read the instructions for the second part of the experiment. The purpose of the second part is to create a closer relationship among subjects in the same laboratory and to enlarge the social distance between subjects from different laboratories. The procedures used follow those introduced in Robalo et al. (2017). There are three tasks: first, subjects choose a slogan for their own laboratory. They do so by chatting via a chat box which is only available to subjects in the same laboratory, see Figure 2.2; they then vote for a slogan to be selected from three candidates. The group decision is made via majority rule. There is no time constraint for the chat. Subjects are informed that the chosen slogan will be shown on their computer monitors in the remaining parts of the experiment.

\(^5\)For more details about the paintings in the first part, please see Appendix B.
Figure 2.1: Minimum group: painting pair 1

**Please choose the scores for Pair 1:**

![Painting Pair 1](image)

- **Score (left) is:** 28
- **Score (right) is:** 72

**Notes:** Subjects can move the grey slider beneath a pair of paintings. The sum of appreciation units to any pairs of paintings in this part of the experiment is 100. The default position of the slider distributes 50 - 50 units to the pair.
Figure 2.2: Group chat

Welcome, A

(1:29 PM) R: my precious
(1:29 PM) W: I think "my precious, my!" can represent young genius and gifted artist all at once
(1:29 PM) B: exactly, so I'll go for my precious
(1:29 PM) D: agree
(1:29 PM) S: Ok my precious
(1:29 PM) D: so most of u agree my precious
(1:29 PM) R: yeah
(1:29 PM) H: yes
(1:30 PM) B: yes
(1:30 PM) F: yes
(1:30 PM) W: yes
(1:30 PM) I: And gifted artist has nothing special about it... my precious, my is different than the others
(1:30 PM) F: I agree with I
(1:30 PM) I: yes
(1:30 PM) R: perfect

Three options for your lab are:

- Young Genius!
- My precious, my!
- Gifted Artist!

Notes: The conversations is a screenshot from session 16 in the experiment. Subjects are randomly assigned capital letters as their personal IDs. The three options for the laboratory slogan are listed on the same page beneath the chat box. Subjects can see their conversations in the same laboratory only and they can exit the chat whenever they wish.

The second task in this part 2 is a tournament between the two laboratories. This aims at further increasing the sense of group identity within the laboratory. Each subject individually reviews five pairs of paintings and is asked to determine the sources of the
paintings in each pair, see Figure 2.3 for an example. They are told that each painting may have been made by a child or by a professional artist. There are four possible answers for each pair (both by children, left by child under the age of 15, right by professional, etc.). For each correct answer by an individual, the accumulated score of the laboratory as a whole increases by one. The laboratory with the higher final score receives 24 points as a prize to be divided equally amongst the twelve subjects in that laboratory.

Figure 2.3: Group tournament

**Pair 1**

Please choose the sources for Pair 1.

- They are both painted by children.
- They are both by professional painters.
- The one on the left is by a professional painter, the one on the right is by a child.
- The one on the left is by a child, the one on the right is by a professional painter.

**Notes:** Screenshot of the first pair of paintings in the group tournament. The painting on the left is by an 11-year-old from Minsk, Belarus. The painting on the right is by Sam Gilliam “coffee thyme”. The correct answer is then that the left is painted by a child and the right by a professional painter.

The final task in part 2 is an other-other dictator allocation task. Each subject is

---

6 The paintings used in this part are shown in Appendix C.
7 Points are the currency used in the experiment. Points earned were converted to euros at the end of the experiment at a rate of 1:1.
asked to allocate two points (in increments of 0.1) between a random participant in the own laboratory (excluding herself) and a random participant in the other laboratory. For example, if 12 subjects are assigned ID 1 to 12 and the other 12 subjects from another group are assigned ID from 13 to 24, the person with ID 1 is matched with person 2 who is in-group and with person 13 who is from another group; person 2 is matched with person 3 who is in-group and with person 14 who is out-group; ...; and person 12 is matched with person 1 in the same group and with person 24 who is out-group. Since the IDs are determined by table numbers, subjects are randomly assigned to recipients in the other-other allocation task. This gives us a measure for a bias towards the in-group relative to the out-group. To avoid possible effects on part 3 of the experiment, the results for the tournament and other-other allocation are revealed only at the end of the experiment. All decisions are paid, including the group tournament, other-other allocation, and the payoffs in the bribery game that is introduced in the next section.

**Triad composition in the bribery game**

To inform them about the composition of the triads, subjects are told from which laboratory the judge and the other performer are. There are three possible compositions, where we label the name of the treatment groups according to the performers’ relationship to the judge. We summarise these in Table 2.1. Suppose the laboratory the judge comes from is always laboratory A, then any performer from laboratory A is an in-group member to the judge. Any performer from a different laboratory B, is an out-group member to the judge. The three triad compositions in our experiment are then:

1) Both out-group treatment: the judge comes from one laboratory, say, laboratory A, both performers from the other laboratory B; ergo, the judge has an out-group relationship to both performers.

2) One in-group treatment: the judge and one of the performers are from laboratory A, the other performer is from laboratory B; ergo, the judge has an in-group relationship
with one performer and an out-group relationship with the other.

3) Both in-group treatment: all three players are from laboratory A. Ergo, the judge has an in-group relationship with both performers.

<table>
<thead>
<tr>
<th></th>
<th>Both out-group</th>
<th>One in-group</th>
<th>Both in-group</th>
</tr>
</thead>
<tbody>
<tr>
<td>Judge</td>
<td>A</td>
<td>A</td>
<td>A</td>
</tr>
<tr>
<td>Performer 1</td>
<td>B</td>
<td>A</td>
<td>A</td>
</tr>
<tr>
<td>Performer 2</td>
<td>B</td>
<td>B</td>
<td>A</td>
</tr>
</tbody>
</table>

*Notes: A and B refer to laboratory A and laboratory B. We define laboratory A as the judge’s laboratory.*

Combining this with the bribe/no-bribe treatment dimension, Table 3.2 illustrates all treatment conditions and lists the number of subjects in each of the six treatment groups.

<table>
<thead>
<tr>
<th></th>
<th>Both out-group</th>
<th>One in-group</th>
<th>Both in-group</th>
</tr>
</thead>
<tbody>
<tr>
<td>Without bribe</td>
<td>48</td>
<td>48</td>
<td>48</td>
</tr>
<tr>
<td>With bribe</td>
<td>60</td>
<td>68</td>
<td>60</td>
</tr>
</tbody>
</table>

### 2.2.3 The bribery game

The third part of the experiment is a bribery game. This has the same structure as in Gneezy et al. (2015). Subjects are divided into triads, consisting of one “judge” and two “performers”. Performers first conduct a real effort task taken from Weber and Schram (2016), the results of which are sent to the judge. Then, the judge selects one winner from the two performers. Each performer receives 5 points at the beginning of the real-effort task. The winner receives an additional 10 points of reward. The judge receives a fixed payoff of 10 points.
Task

Performers in this part are asked to find the sum of the largest numbers in two $10 \times 10$ matrices. They are shown two $10 \times 10$ matrices, in which each cell contains a two-digit number between 01 and 99. The task is to find the highest numbers in each of the two matrices and add them up. After entering a number, another set of randomly chosen matrices appear on the screen, independently of whether the previous answer is correct. The two performers are given eight minutes to solve as many of these matrix puzzles as possible. A running digital clock reminds performers of the time. When time is up, a page pops up showing the number of correct answers for themselves (but not for the other performer).\(^8\)

After performers have finished the matrix puzzle and seen their (own) performance in the task, we reveal the group identity of everyone in the triad. In the bribe treatments, performers are also informed about the possibility to send money (points) to the judge. Then the performer sends the result (and bribes, in the bribe treatments) to the judge.\(^9\) The judge sees the results (and bribes) from both performers and selects a winner. Each performer remains unaware of the performance and bribes of the other performer.

Performers may send any bribe between 0 and 10 points.\(^{10}\) The judge is aware of the group relationships with both performers when she is selecting a winner. It is common knowledge that the bribes are non-refundable, there is no punishment to bribers, and it is up to the judge to select a winner. In summary the judge selects a winner knowing the number of correct answers by the two performers, the bribes from both performers, and her relationship with both performers.

---

\(^8\)After having finished the instructions for this part and before starting the task, all subjects have to correctly answer a set of questions testing their understanding of the task, the duties related to being the judge or a performer, the payoff scheme, and the time constraint. Before assigning roles to judges and performers, all subjects go through a trial session of the matrix puzzle to familiarise themselves with the task.

\(^9\)We design the experiment in such a way that performers have to truthfully report their results to the judge. No lies are possible.

\(^{10}\)The show up fee for each subject is 7 euro so that (with the 5 point starting capital) a briber still ends up with 2 euro even if she bribes 10 euro and has no other earnings.
Figure 2.4: Matrix task

Notes: A screenshot of the matrix task. Performers are given 8 minutes to solve as many matrices as possible. They need to find the largest number in each matrix and sum them up. The program allows them to see the amount of correct and incorrect answers. In this example, the correct answer should be 186 (= 91 + 95).
Performers' bribing decisions are made only once. To collect more observations from judges, we ask judges to make two rounds of decisions, however. After selecting a winner from the first pair of performers, we ask the judges to again select a winner from a different pair of performers. These are randomly chosen and with the same triad composition (in terms of group identities) as in the first round. Judges are informed about the second round of decisions only after they have made the decisions for the first round. Subjects, performers and judges, are informed that the final payment is determined by the decisions in one randomly chosen round.

Finally, before we reveal any results from the experiment, we elicit (incentivised) beliefs of the performers about other performers’ ability to solve the matrix game. Performers guess the distribution of the correct number of answers in their session, i.e., how many performers manage to solve only 1 matrix, how many solve 2 matrices, and etc. The closer their guesses are to the real distribution, the higher their payoff there will be. They have 5 experimental points as an initial endowment, and we subtract 0.25 points for every unit deviation (in number of correct matrices) between their answer and the correct answer.\textsuperscript{11} We also collect information on socio-economic characteristics as well as the subjective attachment towards in-group and out-group members.

In summary, the earnings are determined as follows. Aside from a 7 euro show-up fee, a performer’s earning is composed of five parts: the award from the painting tournament if her laboratory wins, earnings from the other-other allocation task, 5 points of fixed payoff as a performer in the bribery game minus the amount of bribe she offers to the judge, the award of being selected the winner in the bribery game (0 if she loses), and earnings from the post-experimental belief elicitation. A judge’s earning is also composed of four parts on top of the show-up fee, namely, the award in the painting tournament if her laboratory wins, money from the other-other allocation task, 10 points of fixed payoff as a judge in the bribery game, and bribes from both performers. In treatments without

\textsuperscript{11}To know more about this task, see question 6 in the Exit Survey in Appendix A.
bribes performers are not given an opportunity to send money to the judge and judges only receive the fixed wage.

2.3 Results

2.3.1 Creating social distance in the laboratory

Our manipulation of social distance works in the way we intended. Figure 2.5 presents two measures of social distance. First, recall that each subject is asked to divide money between a random in-group member and a random out-group member in other-other allocation task. The observed allocations are shown on the left panel of Figure 2.5. The euros that subjects allocate are presented on a scale of 0 to 20 in the graph. Results for a subjective measure of closeness collected in the post-experimental questionnaire (measured on a ten-point scale) are shown on the right panel in Figure 2.5.

The results show that subjects distribute more money and feel more attached to an in-group member than to an out-group member (N = 332, in both cases p < 0.01, Mann-Whitney henceforth MW). These two measures are correlated (Pearson’s correlation coefficient is 0.188, p < 0.01). The left panel in Figure 2.5 shows that the median amount of money allocated to a random in-group member is three times more than that distributed to a random out-group member. Moreover, the median feeling of closeness towards an in-group member is 5 out of 10, while it is merely 2 (out of 10) towards an out-group member, according to the right panel. Since the dictator allocation measure and the closeness measure are significantly correlated and because of the data problems reported in footnote 12, we will use the subjective measure in our subsequent analyses.

\footnote{Due to technical problem, the allocation data of the first eight sessions were lost. The figure therefore plots observations collected in six sessions (144 observations).}
Notes: The left panel shows the results of the ‘other-other allocation’ task. Subjects are asked to distribute 2 euro (in increments of 10 cents, 20-unit scale) to a random in-group and a random out-group member. The right panel illustrates the subjective feelings (10-unit scale) of closeness towards in-group members and out-group members as expressed in the post-experimental questionnaire. The upper line and bottom on each graph is the maximum and minimum value; the upper line in each box represents the 75th percentile of the value and the bottom line in the box shows the 25th percentile; while the middle line in each box is the median.

2.3.2 Descriptive statistics

Table 3.3 reports summary statistics for all treatment groups. There are 224 performers, 128 in the Bribery treatment and 96 in the No-Bribery treatment. There are 108 judges, 60 in the Bribery treatment and 48 in the No-Bribery treatment. There are no statistically significant differences in the distribution of personal characteristics such as gender and majoring in economics between any pair of treatments (all $p > 0.1$, MW) except that subjects in the ‘both out-group’ with bribes and ‘one in-group’ with bribes treatments are slightly older.\footnote{4 judges in the Bribery treatment are dropped due to a programming error in the first session. Performers’ decisions in this session are not affected.} \footnote{Average age in ‘both out-group’ with bribes vs. average age in ‘both in-group’ with bribes: $p = 0.060$; ‘both out-group’ with bribes vs. ‘one in-group’ with bribes: $p = 0.060$; ‘both out-group’ with bribes vs. ‘both out-group’ without bribes: $p = 0.027$; ‘both out-group’ with bribe vs. ‘both in-group’ without bribes: $p = 0.064$; ‘one in-group’ with bribes vs. ‘one in-group’ without bribes: $p = 0.024$; ‘one in-group’ with bribes vs. ‘both in-group’ without bribes: $p = 0.074$; and ‘both out-group’ without bribes vs. ‘one in-group’ without bribes: $p = 0.088$, all MW.}
### Table 2.3: Descriptive statistics

<table>
<thead>
<tr>
<th></th>
<th>Bribe Treatment</th>
<th>No Bribe Treatment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Both out-group</td>
<td>One in-group</td>
</tr>
<tr>
<td><strong>Task performance</strong></td>
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<td></td>
</tr>
<tr>
<td>Median</td>
<td>7.5</td>
<td>8</td>
</tr>
<tr>
<td>Mean</td>
<td>7.45</td>
<td>7.92</td>
</tr>
<tr>
<td>(2.18)</td>
<td>(2.66)</td>
<td>(2.01)</td>
</tr>
<tr>
<td><strong>Bribes</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% that bribes</td>
<td>82.50%</td>
<td>66.67%</td>
</tr>
<tr>
<td>Median (if bribe)</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>Mean (if bribe)</td>
<td>3.45</td>
<td>3.38</td>
</tr>
<tr>
<td>(1.94)</td>
<td>(1.43)</td>
<td>(1.89)</td>
</tr>
<tr>
<td><strong>Demographics</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age</td>
<td>22.62</td>
<td>22.54</td>
</tr>
<tr>
<td>(2.74)</td>
<td>(2.61)</td>
<td>(2.21)</td>
</tr>
<tr>
<td>% Female</td>
<td>60.00%</td>
<td>60.00%</td>
</tr>
<tr>
<td>(0.49)</td>
<td>(0.60)</td>
<td>(0.50)</td>
</tr>
<tr>
<td>% Majoring in economics$^\text{note1}$</td>
<td>71.67%</td>
<td>73.53%</td>
</tr>
<tr>
<td>(0.45)</td>
<td>(0.44)</td>
<td>(0.46)</td>
</tr>
<tr>
<td># Performers</td>
<td>40</td>
<td>48</td>
</tr>
<tr>
<td># Judges</td>
<td>20</td>
<td>20$^\text{note2}$</td>
</tr>
<tr>
<td># Subjects</td>
<td>60</td>
<td>68</td>
</tr>
</tbody>
</table>

**Notes:** Task performance refers to the number of matrix pairs correctly solved. Treatments are defined in section 2.2.1. Standard deviations are between parentheses. We find no statistically significant difference in task performance nor bribing behaviour across treatments (all pairwise comparisons, $p > 0.1$, MW). Other characteristics except age are balanced in all treatments (see footnote 13).

$^\text{note1}$: Studying at the faculty of economics and business is defined as majoring in economics. Other majors include psychology, law, humanities, and medicine.

$^\text{note2}$: Data for four judges were lost due to a programming error in the first session.

### Randomisation

A first thing to note from Table 3.3 is that task performance does not vary significantly across treatments. The average number of correct matrix summations is between 7 and 8. We find no statistically significant differences in performance across treatment groups, which suggests that our randomisation of subjects across treatments was successful (recall that subjects did not know of any treatment characteristics when performing the task).
Bribe rate

A second thing to note in Table 3.3 is that when given the opportunity, many subjects send a bribe, but the average amount bribed (conditional on sending a bribe) varies little across treatments. More specifically, more than 66% of the performers send money to judges even though bribes are non-refundable. In ‘both out-group’ and ‘both in-group, the bribe rate goes up to 82.5%, but this is not statistically different from 66% in the ‘one in-group’ treatment (for both comparisons, $p > 0.1$, MW).

Figure 2.6: Bribes across treatments

![Box plot showing amount bribed (conditional on bribing) across treatments]

Notes: We plot the maximum/minimum and median value of bribes by performers, conditional on the decision to bribe. The vertical axis is the amount bribed in experimental points (with 1:1 exchange rate to euro).

The bribe rate in our experiment should not come as a surprise. We can compare our rates with those observed in a number of recent studies. Subjects in Abbink et al. (2002) bribe at a rate of 51-76%, depending on the treatment. Subjects in Abbink (2004) bribe at a rate of 34 - 51% while those in Abbink and Hennig-Schmidt (2006) show rates of 48 -
51%. The bribe rate is 63 - 87% in Barr and Serra (2009). Finally, in Gneezy et al. (2015) where the structure and the judge-performer set up is similar to ours, the data show that even if the task is objective and the judge only keeps the winner’s bribe, there is still an offer rate of 64%.

On average bribers - conditional on the decision to bribe - give more than 3 euro to the judge (see Table 3.3 and Figure 2.6). We observe no statistically significant differences across treatments (‘both out-group’ vs. ‘one in-group’: $p = 0.772$; ‘one in-group’ vs ‘both in-group’: $p = 0.294$; ‘both out-group’ vs. ‘both in-group’: $p = 0.433$, MW).

### 2.3.3 Better performers are treated unfairly

Is the selection of winners affected by bribes? We find that the answer is yes; bribes affect judges’ decisions in all treatments.

A judge’s decision cannot be explained by greed, because she keeps bribes from both performers by design. Her selection of a winner, however, could be influenced by reciprocity. If the worse performer bribes with a higher amount than the better performer, then even if the judge takes both bribes, she might choose the worse performer to reciprocate the generosity of a higher bribe.

Do we observe this mechanism in our experiment? To what extent have judges’ decisions been affected by bribes? A natural way to investigate such questions is by looking at the better performers in each treatment, and checking whether or not they have been selected as winners.

To investigate this, Figure 2.7 plots, by treatment, cases where the better performer was selected and those where she was not. The figure distinguishes between the extent to which the performance of the better performer exceeds that of the worse performer, and the difference in the amounts of bribes offered. The performance difference is always non-negative, by definition; but the bribe difference can be negative, zero, or positive because the better performer can bribe more than, the same as, or less than the worse
performer.

Figure 2.7 shows that in all treatments with bribes better performers lose the game only when they bribe less than the worse performer: all hollow markers appear to the left of \( x = 0 \). Considering only the cases where the better performer gave a lower bribe, she was not selected in 8 out of 16 cases (50%), 5 out of 10 cases (50%), and 7 out of 12 cases (58.33%) in ‘both out-group’, ‘one in-group’, and ‘both in-group’ treatments, respectively. Two-sample proportion tests show that the fractions of such better performers are not significantly different across treatment (both out-group vs. one in-group: \( p = 1.00 \); one in-group vs. both in-group: \( p = 0.70 \); and both out-group vs. both in-group: \( p = 0.66 \)).

### 2.3.4 Treatment effects

Now we turn to the main research question: how does social distance affect the decisions by performers and judges? Do in-group performers bribe more? Does judge evaluate an in-group performer and an out-group performer differently, given the same performance and bribes?

To answer these questions, we start with the performers’ behaviour and investigate how social distance influences the bribe rate and amount. Then we turn to the judges to study, first, how an in-group relationship affects the selection of winners when no bribery is allowed; and second, when bribing is possible, how judges select winners, given performance, bribes, and their social distance to both performers.

In short, we find that social distance works differently for performers and judges. A closer social distance does not affect bribes. However – in the absence of bribes – an in-group relationship to the judge does increase a performer’s chances of winning when the other performer is in the out-group. When bribes are allowed, judges value both performance and bribes of the two performers, when the latter have equal social distance to the judge. The effects of performance and bribes are less clear when an in-group performer is matched with an out-group performer, however. Here, the number of correct
Figure 2.7: Distribution of Performance and Bribe of the Better Performer

Notes: The vertical axis shows the difference in correct solutions between the better performer and the worse performer. The horizontal axis depicts the difference in the amount of the bribe between the same pair of performers. Solid markers represent the cases where the better performer was nominated as the winner and hollow markers indicate the cases where she lost. The size of the markers is proportional to the frequency of cases.
answers does not affect the in-group performer’s chances of winning.

The effects of social distance on performers’ bribing behaviour

First of all, we find no treatment effects for performers. Consider first the correlations between bribing and the treatment dummy variables as presented in Table 2.4. The first two columns show that the decision to bribe and the amount bribed are uncorrelated with the treatments. More specifically, all six correlation coefficients between the propensity to bribe or the amount bribed on the one hand and the social identity treatment dummies on the other hand are statistically insignificant and below 0.09. The decision to bribe is, however, significantly positively correlated with one’s performance in the matrix task and the beliefs about the other’s ability in the same task. Majoring in economics is marginally significantly correlated with a higher bribing rate and lower amount bribed, and the more one allocates to an in-group member in the other-other allocation task, the lower is the bribe rate. Otherwise, the correlation coefficients are low and statistically insignificant.

To further explain bribing, we use regression analysis. We regress the amount bribed ($Bribe_i$, $bribe_i = 0$ if one does not bribe) on performance ($P_i$), a dummy indicating that the performer is from the same group as the judge (‘in-group’, $I_i$), treatment group dummies ($BothOut_i$ refers to Both out-group treatment and $OneIn_i$ means One in-group treatment), the interactions between performance and treatments, constant term $\eta$, and (as a control) individual characteristics (vector $Z_i$).\footnote{The reason we add the dummy on in-group membership to the treatment dummies is that it allows us to distinguish in the One in-group treatment between the performer that shares group identity with the judge and the one that does not.} We present the results of an OLS model (eq. 4.1).\footnote{A Tobit regression allowing the dependent variable to be censored at lower limit 0 and upper limit 10 gives qualitatively and quantitatively very similar results. See Appendix D.} The results are shown in Table 2.5. We choose the Both in-group treatment as the baseline in which all players are from the same group.
**Econ**

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**Allocation**

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**Close**

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</tr>
<tr>
<td>0.1</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>0.2</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
</tbody>
</table>

**Performance**

<table>
<thead>
<tr>
<th></th>
<th>0.0</th>
<th>0.1</th>
<th>0.2</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>0.1</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>0.2</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
</tbody>
</table>

**Bride (a)**

<table>
<thead>
<tr>
<th></th>
<th>0.0</th>
<th>0.1</th>
<th>0.2</th>
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<tr>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>0.1</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>0.2</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
</tbody>
</table>

**Bride (d)**

<table>
<thead>
<tr>
<th></th>
<th>0.0</th>
<th>0.1</th>
<th>0.2</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>0.1</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>0.2</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Table 2: Correlations</th>
<th>Br</th>
<th>Bride (a)</th>
<th>Bride</th>
<th>Bride (d)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Made</td>
<td>0.0</td>
<td>0.1</td>
<td>0.2</td>
<td>0.0</td>
</tr>
<tr>
<td>Chance</td>
<td>0.0</td>
<td>0.1</td>
<td>0.2</td>
<td>0.0</td>
</tr>
<tr>
<td>Other</td>
<td>0.0</td>
<td>0.1</td>
<td>0.2</td>
<td>0.0</td>
</tr>
<tr>
<td>Age</td>
<td>0.0</td>
<td>0.1</td>
<td>0.2</td>
<td>0.0</td>
</tr>
<tr>
<td>Performance</td>
<td>0.0</td>
<td>0.1</td>
<td>0.2</td>
<td>0.0</td>
</tr>
</tbody>
</table>

**Note:** There are two versions regarding bringing a dummy for the decision to bridge or not and the amount brought conditional on decision. The decision is a dummy variable equal to 1 (0) of the subject bringing.
\[
Bribe_i = \eta + \alpha_1 \text{BothOut}_i + \alpha_2 \text{OneIn}_i + \beta_1 P_i + \beta_2 I_i + \eta_1 \text{BothOut}_i \times P_i + \eta_2 \text{OneIn}_i \times P_i + \delta_1 Z'_i + \varepsilon_i
\]

(2.1)

In column (1) we present the results when regressing only on treatment and performance variables. We find no significant coefficients. We add the interactions between treatment dummies and the performance variable in column (2). Again the results show no significant effects. In column (3), we control for a set of background variables, including the beliefs about others’ ability to solve the matrix task and the expressed closeness towards in-group members. The absence of significant treatment effects remains in column (3) where only the subjective closeness towards in-group members is marginally significantly related to higher bribes \((p < 0.1)\). We conclude that bribes offered by the performers are not affected by the group identity relations between the performers and the judge.

One might ask whether the decision whether or not to bribe (as opposed to the amount bribed), is affected by social distance. Our analysis below shows that this is not the case. We report Probit regression results for performers’ decision to bribe. The independent variables are the same as in model (1) without the constant term \(\eta\). Here, the dependent variable is the decision whether or not to bribe. Marginal effects are reported.

Again, we find insignificant treatment effects for the decision to bribe except for a marginally significant (negative) coefficient for ‘one in-group treatment’ after controlling for a series of variables in Table 2.6.
Table 2.5: OLS regression for amount bribed

<table>
<thead>
<tr>
<th></th>
<th>Model: OLS</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td></td>
</tr>
<tr>
<td>Both out-group treatment</td>
<td>0.347</td>
<td>0.371</td>
<td>0.408</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.494)</td>
<td>(0.519)</td>
<td>(0.528)</td>
<td></td>
</tr>
<tr>
<td>One in-group treatment</td>
<td>-0.451</td>
<td>-0.433</td>
<td>-0.330</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.509)</td>
<td>(0.528)</td>
<td>(0.498)</td>
<td></td>
</tr>
<tr>
<td>Performance</td>
<td>0.216</td>
<td>0.135</td>
<td>0.169</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.185)</td>
<td>(0.436)</td>
<td>(0.474)</td>
<td></td>
</tr>
<tr>
<td>In-group membership</td>
<td>0.310</td>
<td>0.309</td>
<td>0.0626</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.547)</td>
<td>(0.556)</td>
<td>(0.572)</td>
<td></td>
</tr>
<tr>
<td>Performance (own correct matrices)</td>
<td>0.136</td>
<td>0.190</td>
<td></td>
<td></td>
</tr>
<tr>
<td>× Both out-group treatment</td>
<td>(0.523)</td>
<td>(0.601)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Performance</td>
<td>0.088</td>
<td>0.161</td>
<td></td>
<td></td>
</tr>
<tr>
<td>× One in-group treatment</td>
<td>(0.514)</td>
<td>(0.536)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Beliefs of other performers’ ability</td>
<td>-0.001</td>
<td></td>
<td></td>
<td>(0.002)</td>
</tr>
<tr>
<td>Closeness towards in-group members</td>
<td>0.147*</td>
<td></td>
<td></td>
<td>(0.078)</td>
</tr>
<tr>
<td>Being female</td>
<td>0.280</td>
<td></td>
<td></td>
<td>(0.420)</td>
</tr>
<tr>
<td>Age</td>
<td>-0.010</td>
<td></td>
<td></td>
<td>(0.086)</td>
</tr>
<tr>
<td>Major in economics</td>
<td>-0.314</td>
<td></td>
<td></td>
<td>(0.527)</td>
</tr>
<tr>
<td>Session fixed effects</td>
<td>7</td>
<td>7</td>
<td>3</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>2.525***</td>
<td>2.506***</td>
<td>2.478</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.339)</td>
<td>(0.351)</td>
<td>(2.316)</td>
<td></td>
</tr>
<tr>
<td>R-squared</td>
<td>0.027</td>
<td>0.028</td>
<td>0.089</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>128</td>
<td>128</td>
<td>128</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Dependent variable is the amount performers send to the judge as a bribe. The value ranges from 0 to 10. The variable “closeness towards in-group members” comes from the questionnaire where subjects are asked to give an evaluation of attachment feelings towards in-group members and out-group members on a scale from 0 to 10. Robust standard errors between parentheses allow for cluster at the triad level. * p < 0.1, **p < 0.05, *** p < 0.01.
Table 2.6: Bribe decision for performers

<table>
<thead>
<tr>
<th>Y = Decision to bribe</th>
<th>Model: Probit</th>
<th>M.E.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Both out-group treatment</td>
<td>0.029</td>
<td>0.028</td>
</tr>
<tr>
<td></td>
<td>(0.087)</td>
<td>(0.091)</td>
</tr>
<tr>
<td>One in-group treatment</td>
<td>-0.158</td>
<td>-0.159</td>
</tr>
<tr>
<td></td>
<td>(0.098)</td>
<td>(0.101)</td>
</tr>
<tr>
<td>Performance</td>
<td>-0.002</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.040)</td>
<td>(0.083)</td>
</tr>
<tr>
<td>In-group membership</td>
<td>0.070</td>
<td>0.068</td>
</tr>
<tr>
<td></td>
<td>(0.121)</td>
<td>(0.117)</td>
</tr>
<tr>
<td>Performance</td>
<td>-0.084</td>
<td>-0.099</td>
</tr>
<tr>
<td>× Both out-group treatment</td>
<td>(0.102)</td>
<td>(0.106)</td>
</tr>
<tr>
<td>Performance</td>
<td>0.023</td>
<td>0.024</td>
</tr>
<tr>
<td>× One in-group treatment</td>
<td>(0.103)</td>
<td>(0.100)</td>
</tr>
<tr>
<td>Beliefs of other performers’ ability</td>
<td>-0.000</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Closeness towards in-group members</td>
<td>0.012</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Being female</td>
<td>0.098</td>
<td>(0.072)</td>
</tr>
<tr>
<td>Age</td>
<td>-0.014</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Major in economics</td>
<td>0.100</td>
<td>(0.090)</td>
</tr>
<tr>
<td>Session fixed effect</td>
<td>7</td>
<td>7</td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td>-68.929</td>
<td>-68.309</td>
</tr>
<tr>
<td>Observations</td>
<td>128</td>
<td>128</td>
</tr>
</tbody>
</table>

*Notes: The dependent variable is a dummy variable equal to 1 if the performer bribed and 0 otherwise. Marginal effects are reported. Robust standard errors between parentheses allow for cluster at the triad level. * p < 0.1, **p < 0.05, *** p < 0.01.

The effects of social distance on judges’ decisions

We next turn to the treatment effects for judges. We will report two major findings. First, in-group favouritism exists when bribes are not allowed. Sharing the same group membership with the judge increases the odds of being chosen as the winner when competing with an out-group performer. Second, when bribery is available, in-group membership makes
performance and bribes less important in the comparison to an out-group performer.

To capture how judges choose the winner from two candidate performers under varying constellations of group membership, we adopt a conjoint analysis (Green and Srinivasan 1978, Hair et al. 2010).\(^ {17} \) This allows us to investigate how judges value performers based on different attributes of the performers themselves, including performance, bribes, and especially, the relative social distance to the judge. Since each judge in our experiment is asked to rate winners in two rounds, we have four observations for each judge: the winner in the first round, the loser in the first round; the winner in the second round, and the loser in the second round. Therefore we have 432 (related) observations for the 108 judges in our experiment.

**Rank-Ordered Logit Model specification**

We choose a Rank-Ordered Logit Model (ROLM) for our analysis. The reason to choose this model is that the choice of winners always takes place in triads where a winner is matched with a loser. A linear probability model with the chance of being a winner as the dependent variable and observable characteristics of the performers as the main independent variables, for instance, is not suitable for our study. This is because the error terms and the dependent variable (being a winner or not) are not independently distributed: a winner in a pair of performers always comes with a loser chosen by the same judge. ROLM analyses the preferences of a judge in a specific triad by looking at how she combines attributes of alternative performers into overall evaluations of the attractiveness of these alternatives.\(^ {18} \)

We thus transform the winner-selection process into a preference problem for judges: whom to prefer given the two performers’ performances, bribes, and possibly in-group

\(^ {17} \)Originating in mathematical psychology, conjoint analysis has been developed and applied in many social sciences, including personnel economics, operations research, and marketing (Green and Srinivasan 1990). A nice example using data obtained from a ‘vignette method’ is De Wolf (2000) in a study on employer’s preference for job candidates.

\(^ {18} \)ROLM differs from the regular logistic regression in that the data are grouped and the likelihood is the product of a series of conditional likelihoods.
memberships. Suppose judges in our experiment have a random utility framework as in Manski (1977) defined in equation 2.2:

\[ U_{ij} = V_{ij} + \varepsilon_{ij}, \]  

(2.2)

where the random utilities for judge \( i \) are a set of latent variables \( U_{i1}, ..., U_{iJ} \), \( i = 1, ..., N \) indexes judges and \( j = 1, ..., J \) indexes performers in the judge’s triad so that \( J = 2 \). The utilities consist of \( V_{ij} \), a deterministic component which is determined by observed performer characteristics; and \( \varepsilon_{ij} \), a random component of the utility, which is usually assumed to have an independent type-I extreme value distribution (Luce 2005). Let \( Y_{ij} = 1 \) denote that judge \( i \) prefers performer \( j \) over performer \( -j \) in her triad. Because we assume that \( \varepsilon \) follows a type-I extreme value distribution, this leads to the probability that performer \( j \) is most preferred by judge \( i \)

\[
Pr[Y_{ij} = 1; \beta] = Pr[U_{ij} > U_{i-j}] = \frac{\exp(V_{ij})}{\sum_{k=1}^{2} \exp(V_{ik})}
\]  

(2.3)

where \( \beta \) is a parameter vector specific to the judge \( i \).

To proceed, we assume a linear function for the latent variable \( V_{j} \) (for simplicity we drop the subscript \( i \) for the judge). Then the value of performer \( j \) is assumed to be given by a linear combination of observable characteristics:

\[
V_{j} = \alpha + \beta_1 Perform_{j} + \beta_2 InGroup_{j} + \beta_3 Perform_{j} \times InGroup_{j} + \beta_4 Bribe_{j} + \beta_5 Bribe_{j} \times InGroup_{j} + \beta_6 Z'_{j} + \varepsilon_{j},
\]  

(2.4)

where \( V_{j} \) is a latent variable indicating the value assigned to performer \( j \) by the
judge. This value depends on the following characteristics. \( \text{Perform}_j \) is the number of correct matrices solved in the competition, \( \text{Bribe}_j \) is the amount bribed (only used bribe treatments), in-group membership is a dummy variable \( \text{InGroup}_j \) (when \( \text{InGroup}_j = 1 \) if she shares group identity with the judge while her competitor does not and 0 otherwise), the interaction between performance and in-group membership \( \text{Perform}_j \times \text{InGroup}_j \) and the interaction between amount bribed and in-group membership \( \text{Bribe}_j \times \text{InGroup}_j \) (in treatments with bribes). Since performers in ‘Both In-group’ and ‘Both Out-group’ treatments have the same social distance to the judge – either they are both in-group or they are both out-group – there is no difference between the two performers in terms of the distance to the judge. Moreover, personal characteristics of a judge are stable across her valuation of the performers so they do not enter the estimation equation.

In our experimental setting, the judge is able to rank all performers in such an order that the ranking of performer \( j \) (winner) is better than performer \( -j \) (loser), thus \( \text{Pr}[U_j > U_{-j}] \). Substituting equation 4.3 into equation 2.5, the probability that performer \( j \) is preferred to performer \( -j \) by the judge is as simple as

\[
\text{Pr}[Y_{ij} = 1; \beta] = \frac{1}{1 + \exp(V_{i-j} - V_{ij})} = \frac{\exp(V_{ij})}{\exp(V_{ij}) + \exp(V_{i-j})}. \tag{2.5}
\]

Equation 2.5 is the model we seek to fit.\(^{19}\)

We estimate the combination of model 2.5 and 4.3 separately for treatments with and without bribes. In Table 2.7 we report regression coefficients, in columns (1) for the no bribe treatments and in column (2) for the bribe treatments.

We follow Long and Freese (2006) to interpret the coefficients. The coefficients reflect the direction and weights that the judge puts on the attributes when valuing two performers: a positive coefficient indicates that the judge values a higher score on the

\(^{19}\text{ROLM has the property of having no intercept term in } V_{ij}. \text{ Assume } V_j = a + x_j \beta, \text{ where } V \text{ is the latent variable reflecting the value assigned to performer } j \text{ and on the right hand side the characteristics evaluated by the judge } i. \text{ Then equation 2.5 becomes } \text{Pr}[Y_j = 1; \beta] = \frac{e^{a + x_j \beta}}{e^{a + x_j \beta} + e^{a + x_{-j} \beta}} = \frac{e^{x_j \beta}}{1 + e^{x_{-j} \beta}}. \text{ In other words, the intercept plays no role in the conditional probability of performer } j \text{ being selected as a winner. This is why we do not include an intercept in our estimation.}
attribute concerned; the larger the coefficient is, the higher is the weight that the judge puts on the attribute. $\beta_1$ is the effect of performance of a performer who comes from a treatment where two performers share the same group membership, and $\beta_4$ measures the importance of bribes by that performer. The marginal effect of being the judge’s ONLY in-group performer on performance is reflected by $\beta_1 + \beta_3$, and that on bribes is reflected by $\beta_4 + \beta_5$.

Table 2.7: Treatment effects for judges: Chance of winning

<table>
<thead>
<tr>
<th>Y = Winner or not</th>
<th>No bribe treatments (1)</th>
<th>Bribe treatments (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Model: ROLM</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Performance</td>
<td>2.516***</td>
<td>1.700***</td>
</tr>
<tr>
<td></td>
<td>(0.907)</td>
<td>(0.454)</td>
</tr>
<tr>
<td>Bribe</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>2.002***</td>
<td>(0.550)</td>
</tr>
<tr>
<td>Being in-group</td>
<td>2.338***</td>
<td>0.294</td>
</tr>
<tr>
<td></td>
<td>(0.617)</td>
<td>(0.531)</td>
</tr>
<tr>
<td>Performance</td>
<td>1.756**</td>
<td>-0.526</td>
</tr>
<tr>
<td>$\times$ Being in-group</td>
<td>(0.812)</td>
<td>(0.579)</td>
</tr>
<tr>
<td>Bribe</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\times$ Being in-group</td>
<td></td>
<td>(0.365)</td>
</tr>
<tr>
<td>Wald-test p-value</td>
<td>0.031</td>
<td>0.077</td>
</tr>
<tr>
<td>(for interaction terms)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td>-23.706</td>
<td>-41.979</td>
</tr>
<tr>
<td>N</td>
<td>192</td>
<td>240</td>
</tr>
</tbody>
</table>

**Notes:** The unit of observation is the single performer. The dependent variable is whether or not the judge selects the performer to be the winner. Performance and bribe are both standardised. “Being In-group” is a dummy variable which equals 1 if the performer comes from the judge’s in-group and competes with an out-group member. Rank-ordered logit regression coefficients are reported. Robust standard errors between parentheses allow for clustering at the level of a judge. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The results in column (1) in Table 2.7 show that in the no bribe treatments both performance and in-group membership contribute positively to the chance of being selected as the winner. Overall in treatments without bribes, the better a performer’s performance, the more likely she becomes the winner. A standard deviation increase in performance (in
triads where performers have the same social distance with the judge) in the matrix task increases the odds of the chance of winning by 1238.3% ($= e^{2.516}$), holding other variables constant.\footnote{The regression coefficient of “Performance” is 2.516, thus the odds of better performance is increased by $e^{2.516}$. The same calculations apply to other odds.} All else being equal, having the same group membership as the judge – and thus the other performer having a different group membership as the judge – increases the odds of being chosen to be the winner by 1036.5% ($= e^{2.338}$). The marginal effect of a standard deviation increase in performance on the odds of being a winner is 578.92% ($= e^{1.756}$) higher for an in-group member than for the out-group member in the same triad.

We report the regression coefficients for the bribe treatments in column (2) in Table 2.7. We notice that performance and bribes (still) have significant impacts on the chance of being chosen as winner. Specifically, a standard deviation increase in performance (of performers in triads where they have the same group membership as the judge) increases the odds of being a winner by 547.2% ($= e^{1.7}$) and a standard deviation increase in bribes (of those performers) increases the odds of being a winner by 740.4% ($= e^{2.002}$), holding other variables constant. The inclusion of in-group membership, however, makes the importance of performance for the in-group performer (in triads where her group membership is the same as the judge and the other performer is not) insignificantly different from zero: the effect of the in-group performer’s performance is $\beta_1 + \beta_3 = 0$ ($\chi^2(1) = 2.45, p = 0.118, \chi^2$-test). More bribes still increases an in-group performer’s chance of winning: $\beta_4 + \beta_5 > 0$ ($\chi^2(1) = 8.93, p = 0.003, \chi^2$-test). Though neither interaction term (between in-group membership and performance and between in-group membership and bribes) has a significant effect in isolation, they jointly changed the way judges evaluate performance and bribes ($p = 0.077, \chi^2(2) = 5.12, \chi^2$-test).
Round effects

One might conjecture that the judges' decisions in two rounds might differ, for instance, because a judge might want to compensate for an 'immoral' choice of choosing a high briber in the first round by switching to a better performer in the second round. We report the ROLM analysis of the first round alone in Table 2.8. The regression structure is identical to model 4.3 and we find highly consistent results with that in the previous analysis for both rounds. For instance, in column (1) where only the treatment without bribes is analysed, all attributes appear important for the judge when selecting a winner. Being in-group to the judge increases the odds of becoming the winner by 546.6%, keeping other variables constant.

Table 2.8: Treatment effects for judges in the first round: Chance of winning

<table>
<thead>
<tr>
<th>Y = Winner or not (First round)</th>
<th>No bribe treatments (1)</th>
<th>Bribe treatments (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Model: ROLM</td>
<td>Performance</td>
<td>Bribe</td>
</tr>
<tr>
<td></td>
<td>2.166**</td>
<td>1.976***</td>
</tr>
<tr>
<td></td>
<td>(1.092)</td>
<td>(0.644)</td>
</tr>
<tr>
<td></td>
<td>Bribe</td>
<td></td>
</tr>
<tr>
<td></td>
<td>1.860***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.678)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Being in-group</td>
<td></td>
</tr>
<tr>
<td></td>
<td>2.215***</td>
<td>0.0647</td>
</tr>
<tr>
<td></td>
<td>(0.854)</td>
<td>(0.895)</td>
</tr>
<tr>
<td></td>
<td>Performance × Being in-group</td>
<td>-2.155**</td>
</tr>
<tr>
<td></td>
<td>1.867*</td>
<td>(0.968)</td>
</tr>
<tr>
<td></td>
<td>(1.088)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Bribe × Being in-group</td>
<td>-0.456</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.826)</td>
</tr>
<tr>
<td>Wald-test for interaction term(s)</td>
<td>0.086</td>
<td>0.053</td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td>-13.385</td>
<td>-20.313</td>
</tr>
<tr>
<td>N</td>
<td>96</td>
<td>120</td>
</tr>
</tbody>
</table>

Notes: The dependent variable is whether or not the judge selects the performer to be the winner in the first round. Robust standard errors between parentheses allow for clustering at the level of a judge.
* p < 0.1, **p < 0.05, *** p < 0.01.

We find a stronger ‘crowding out’ effect of the in-group membership in the treatments with bribes. In column (2), where bribes are allowed, the negative coefficient for ‘Performance × Being in-group’ shows that an in-group membership significantly decreases
the importance of better performance of that performer, holding other variables constant. The effect of performance for an in-group performer $\beta_1 + \beta_3$ decreases to 0 ($\chi^2(1) = 0.03$, $p = 0.87$, $\chi^2$-test). What is different from the finding in Table 2.7 is that we also find the effect of an in-group performer’s bribes to be zero in the first round: $\beta_4 + \beta_5 = 0$ ($\chi^2(1) = 2.33$, $p = 0.127$, $\chi^2$-test), compared to a positive effect when we combine both rounds. A Wald-test for the interaction terms confirms that adding the interaction term to the regression significantly decreases the valuations of performance and bribes of an in-group performer ($p = 0.053$, $\chi^2(2) = 5.75$, $\chi^2$-test).

### 2.4 Concluding remarks

We use a laboratory experiment to examine the interaction between social distance and bribery. After generating group identity, triads of players enter a bribery game where one judge selects one of two performers as the winner of a prize. There are three possible scenarios in terms of the social distance between the judge and the performers. This allows us to draw conclusions about the causal relationship between social distance and the decisions made in the bribery game. We find that in-group membership matters in an unexpected way, depending on whether or not bribery is allowed. When bribes are unavailable, judges exhibit a strong in-group favouritism. When the judge can be bribed, the importance of performance and bribes is diluted by an in-group membership; especially in the first round, performance and bribes of an in-group performer are not relevant to her chances of winning.

The experiment designed here aims to set a first step towards a better understanding of how corruption interacts with social identity. Various extensions to our setup could provide interesting further insights. First, one could extend the environment to allow for repeated interaction between performers and the judge. In the world outside the laboratory, interactions are often not of a one-shot nature. Repetition would introduce a role of reputation (e.g., a reputation of being open to bribes). Secondly, to deter
corruption, policies typically include a penalty or other type of punishment. It would be interesting to allow for such punishment in future extensions of this research. Thirdly, by design, we create a very corrupted environment because the judges are ‘forced’ to take both bribes. This design choice was made to create an environment where bribes have somehow become a ‘way of life’ and socially acceptable. Future work could extend on this and allow for rejection of bribes. Though such extensions certainly provide interesting venues for future research, we believe that the experiments discussed here provide valuable first insights into the interaction between social distance and corruption.
Appendix A: Experimental Instructions

Instruction in Part One

Welcome to our experiment!

This is an experiment in decision-making. The amount of money you earn will depend upon the decisions you make and on the decisions other people make. This experiment has 3 parts and in total there are 24 participants.

Now you have already got 7 Euro for showing up here. Your total earnings will be the sum of your payoffs and the show-up fee. In this experiment, we use experimental points (1 euro = 1 points). At the end of the experiment you will be paid IN CASH. Everyone will be paid in private and you are under no obligation to tell others how much you earn.

You will receive separate instructions for the three parts before each part begins. Please read all instructions carefully and do NOT communicate with each other during the experiment. If you have a question, feel free to raise your hand, and an experimenter will come to help you.

[Subjects go to instruction on next page.]

For this experiment, we have randomly assigned you a personal ID (in capital). Yours is – (the personal ID). Please remember your ID because you will use it later. Also please enter below a three digit number between 99 and 1000 (for example: 123) as your personal password to relog-in your page.

In this part you view three pairs of paintings so that we can know your preference for paintings. You grade paintings with a slider (see below). The total scores of two paintings in one pair will add up to 100. The more you prefer a painting, the higher score you give. For example, if you strongly prefer a painting and dislike the other one, you can give 100 to the former and 0 to the latter.

Now you can practice with the slider below. The default score is 50 for each painting. If you prefer the painting on the left, simply move the slider to the left and you will see the corresponding score. If you prefer the painting on the right, move the slider to the right. You can always change your preferences before submitting your results.

[Subjects see the slider and they can practice with the example slider.]

Click on the slider, drag it, and see how the scores change! If it does not work, raise your hand!

There are no right or wrong answers in this part. Also, you will not earn money in this part.
After you decide your preference, click on “confirm” to submit your choice. You will not come back to the previous page.

Create your personal password: — (a three-digit number between 99 and 1000.)

[Subjects go to next page.]

**Instruction Question in Part One**

Before the experiment starts, we will ask you some questions to check your understanding about the first part. You can return to the instructions by clicking at the bottom of the page.

Will you get paid in part one?
- No.
- Yes.
- It depends.

When you are viewing two paintings, what does a score of “0” for a painting mean on a slider?
- I really don’t like this painting.
- I really like this painting.
- I have no idea about this painting.

How often can you go back from the current paintings to change your scoring for the previous pair?
- Never.
- Once.
- As often as you like.

[Three pairs of paintings are reviewed.]

**Result Part One**

Your score for the paintings on the left is —, and your score for paintings on the right is —.

Your rating is among the 12 people who have the highest rankings for paintings on the right ([left for the other half of subjects]) in today’s experiment.

Thus We will now split the 24 participants in todays experiment in two laboratories. The split will be based on your taste for paintings. You and the other 11 with the highest preferences for paintings on the right will stay in this laboratory. You will move to another computer station, however. Please pick up you belongings and find a desk in this
laboratory with a red marker and sit down. Then log into the computer by entering the ID that was assigned to you and the 3-digit password you chose. Further instructions will follow when everyone has logged in.

[Subjects move to the other laboratory or different seats in the same lab. They will re-login with their ID and password.]
Instruction in Part Two

Now you have selected yourself into this lab with people who have similar tastes for paintings. You have three tasks to do in this part. You will read the instructions at the beginning of each of the three tasks.

Now the first task is:

Choose a slogan for your own lab

We have prepared three candidate slogans for your laboratory. Before you choose, you will be given a chance to discuss the options with the other laboratory members via a chat box. Before this is started, you will need to answer a few questions to check whether you have understood these instructions.

The chat box will be opened on the next page. You will be able to chat with the others in this laboratory, but not with the other laboratory. The other laboratory will not be able to see your chat.

In this part you are going to choose a slogan for your lab from three options. You log in with your personal ID.

You can leave the chat by choosing “exit chat”. You will then be asked to vote for a slogan. The slogan chosen by the majority will be the one used by your laboratory.

[Group chat box is open and they can talk with their group members about the slogan by typing. There is no time limit.]

Choose one from the three candidates for your lab: [an example from one lab]
- Young Genius!
- My precious, my!
- Gifted Artist!

[After that, subjects go to the second task.]

Lab Tournament

In this part, your laboratory will participate in a tournament with the other laboratory. Everyone will individually be shown five pairs of paintings. Each painting was made either by a young child or by a professional artist. You will be asked to choose for each of the two paintings which is the case.

All paintings were randomly selected from a pool of 15 paintings by young children and 15 by professional painters. For each pair, there are four possible answers: both paintings are by children; the one on the left is by a child and the one on the right by
a professional; left is by a professional and right by a child; or both are by professional painters.

For each correct answer, you will receive one point, so you can earn a maximum of five points. The points scored by everyone in your laboratory will be added up. The laboratory with most points in total will receive 24 points, to be split equally. This means that if your team scores more than the other lab, you will receive 2 points as a prize. If the other team scores best, you will receive nothing for this task.

The winning lab of this tournament will be announced in the end of the experiment.

[Five pairs of paintings are presented. Subjects work on the task. There is no time limit.]

**Divide Money**

Now, we ask you to divide 2 points between one randomly chosen other person in your own laboratory and one randomly chosen person in the other laboratory. The amount you give will be exchanged for euros at the end of the experiment.

Everyone else is making the same decision. This means that at the end of the experiment, you may receive money from someone in your own laboratory and from someone in the other laboratory.

How many points do you want to give to someone else in your own laboratory? [From 0 to 2, with an increment of 0.1 point]
How many points do you want to give to someone in the other laboratory? [From 0 to 2, with an increment of 0.1 point] [Subjects go to instruction in Part Three]
Instruction in Part Three

We now move to part 3. For this, you will be put in groups of three. Each group consists of two performers and one judge. Each of the three members of your group could be from your laboratory or from the other laboratory. You will be told later from which laboratory they are.

The two performers will perform a task. After they have finished, the judge will decide on the winner. The winner will receive 10 points (to be converted to euros at the end of the experiment). In addition, each performer will receive 5 points at the start of the task. The judge will not perform the task and will receive a fixed payment of 10 points.

Next page you will find the instruction of the task.

The task that the performers do is a matrix game in eight minutes. You will see two 10x10 matrices. Your job is to find the highest number in the left matrix and the highest number in the right matrix. Add these two numbers up and enter it. After you have entered a number, two new matrices will appear no matter if your answer was correct or incorrect. You will receive at most 30 matrices in 8 minutes.

The judge will be told the number of correct answers by the two performers in the group and must then determine the winner of the prize.

Questions about instruction in part three

Before the third part starts, we would like to ask you a few questions to check your understanding of the instruction. You can return to the instruction for this part on the bottom of this page.

How many roles there are for this task in part 3?
- Only one, we are either all performers or all judges.
- There are two roles: judges and performers.
- There are more than two roles.

Is it possible to play the task with another person from the other lab?
- Yes, I might play with another one or two persons from the other lab in this part.
- No, and I only play with the persons in this lab.
- Yes, and I only play with persons from the other lab.

What are the procedures for this task?
- First the computer will decide who are the judges and performers, and then performers start the matrix game and judges wait.
- Everybody will complete the matrix game first, and then the computer chooses judges in each group and the judge decides who is the winner between rest two performers.
- First the computer decides who are the judges and performers, and then everyone does
the matrix game.

How many judges and how many performers there are in this task?
- There are 12 judges and 12 performers, each performer is matched with one judge.
- There are 8 judges and 16 performers.
- The number of judges and performers are unknown.

How many minutes does the matrix game take for performers?

The matrices that you will see during the experiment will be much larger. For now, assume that one of the matrices on your screen consists of the numbers 19, 23, 41, 16, 25, 30, 12, 29, 22 and the other matrix consists of the numbers 31, 36, 20, 15, 28, 38, 17, 19, 31. What would be the correct number to enter?

[One the next page, their role of whether they are judges or performers will be assigned. To enhance the salience of group identity, the group slogan is mentioned.]

[Performers solve matrix task while judges are waiting for the results. When judges are informed about the results of performance and bribes, she nominates the winners.]
Exit Survey

Please fill out the following questionnaire.

1) Gender:
   - Male
   - Female

2) Age: (only numbers)

3) Have you participated in a CREED experiment before?
   - No.
   - Yes, once.
   - Yes, more than once.

4) Department where you study:
   - Faculty of Economics and Business
   - Faculty of Social and Behavioural Sciences-Psychology
   - Faculty of Social and Behavioural Sciences-non Psychology
   - Faculty of Science
   - IIS: beta gamma bachelor
   - Faculty of Law
   - Faculty of Humanities
   - Faculty of Medicine
   - Faculty of Dentistry
   - Another university
   - A Dutch “hogeschool” (HBO)
   - Other different places

5) You have done a lot of tasks today, individually and with others. Please rate how closely attached you felt to different labs throughout the experiment. On a scale of 0 to 10 where 0 means you don’t feel anything to this lab and 10 means you really feel like belonging to this lab.
   - How much you feel close to your own lab?  - How much you feel close to the other lab?

6) We will give you one more chance to make some money in this experiment. You can do so by predicting how well people did in the matrix task. How do you think people today in this experiment perform in this matrix game? There are 16 performers in total who played the matrix game in this experiment. Choose the number of people you think solve this number of matrices correctly. You are given 5 points for a fully correct answer. For every difference between your answer and the correct answer, we will subtract 0.25 points. If your answer differs by more than 20 from the correct answers, your payment for the survey would be 0. Your earnings in part 2 and part 3 will not be influenced. Make sure the total number of performers is 16. We know from previous experiments that the best record for the matrix game is 12 matrices in 8 minutes.
[On the last page, the results for matrix game, group tournament, other-other allocation, and the guesses for how well the others performed are revealed.]
Appendix B: Paintings Used in Part One

Figure 2.8: Painting Pair 1 in Part One

Figure 2.9: Painting Pair 2 in Part One

Figure 2.10: Painting Pair 3 in Part One
Appendix C: Paintings Used in Part Two

Figure 2.11: Paintings in Group Tournament: Pair One

Figure 2.12: Paintings in Group Tournament: Pair Two

Figure 2.13: Paintings in Group Tournament: Pair Three
Figure 2.14: Paintings in Group Tournament: Pair Four

Figure 2.15: Paintings in Group Tournament: Pair Five

Answers:
figure 2.11: left - child, right - professional ‘Sam Gilliam’;
figure 2.12: left - child, right - professional ‘Gerhard Richter’;
figure 2.13: left - professional ‘Gerhard Richter’, right - professional ‘Picasso’;
figure 2.14: left - child, right - child;
figure 2.15: left - professional ‘Gerhard Richter’, right - professional ‘Nick Mauss’.
## Appendix D: Tables

### Table 2.9: Tobit regression for amount bribed

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Y = Amount bribed</td>
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<td></td>
<td></td>
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<tr>
<td>Model: Tobit</td>
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<td></td>
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<tr>
<td>Both out-group treatment</td>
<td>0.399</td>
<td>0.414</td>
<td>0.511</td>
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<tr>
<td></td>
<td>(0.580)</td>
<td>(0.609)</td>
<td>(0.611)</td>
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<tr>
<td>One in-group treatment</td>
<td>-0.773</td>
<td>-0.757</td>
<td>-0.674</td>
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<tr>
<td></td>
<td>(0.705)</td>
<td>(0.723)</td>
<td>(0.671)</td>
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<td>Performance</td>
<td>0.235</td>
<td>0.148</td>
<td>0.197</td>
</tr>
<tr>
<td></td>
<td>(0.252)</td>
<td>(0.557)</td>
<td>(0.573)</td>
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<td>In-group membership</td>
<td>0.475</td>
<td>0.467</td>
<td>0.233</td>
</tr>
<tr>
<td></td>
<td>(0.797)</td>
<td>(0.800)</td>
<td>(0.793)</td>
</tr>
<tr>
<td>Performance (own correct matrices)× Both out-group treatment</td>
<td>0.045</td>
<td>0.098</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.652)</td>
<td>(0.709)</td>
<td></td>
</tr>
<tr>
<td>Performance × One in-group treatment</td>
<td>0.154</td>
<td>0.212</td>
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<td></td>
<td>(0.686)</td>
<td>(0.683)</td>
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<td>Beliefs of other performers’ ability</td>
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</tr>
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<td></td>
<td>(0.002)</td>
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<tr>
<td>Closeness towards in-group members</td>
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<td>Being female</td>
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</tr>
<tr>
<td></td>
<td>(0.519)</td>
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<td></td>
</tr>
<tr>
<td>Age</td>
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<td></td>
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<td>Major in economics</td>
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<tr>
<td></td>
<td>(0.670)</td>
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<td>Session fixed effects</td>
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<td>3</td>
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<tr>
<td>Constant</td>
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<td>2.221***</td>
<td>2.650</td>
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<td>(0.401)</td>
<td>(0.418)</td>
<td>(2.850)</td>
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<td>Log pseudolikelihood</td>
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<td>-260.765</td>
<td>-256.695</td>
</tr>
<tr>
<td>Observations</td>
<td>128</td>
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</tbody>
</table>

*Notes: Dependent variable is the amount performers send to the judge as a bribe. The value ranges from 0 to 10. Robust standard errors between parentheses allow for cluster at the triad level. * p < 0.1, **p < 0.05, *** p < 0.01.*
Chapter 3

High Social Status Induces Pro-social Behaviour

Great responsibility follows inseparably from great power.

— French National Convention, 1793

3.1 Introduction

More than a century ago in his book “The Theory of the Leisure Class”, Thorstein Veblen reported a distinction between classes in the modern American society. There were a leisure class which was exempted from manual work, and a subordinated working class that was employed in vulgar occupations. During the Second Industrial Revolution, the lower class accumulated wealth and sought to emulate the standards of leisure class life. He showed that the demand for luxury goods and services was increasing in price, which

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is a contradiction of the law of demand, and was seen as an attempt to sustain or attain a superior social status (Veblen 1899).

Such status-seeking behaviour is not only observed in the American society in the late nineteenth century, we encounter the impacts of social status everyday: why we want to tan during holidays? And why did pale skin lose its popularity in the ancient Greek, Roman and Elizabethan eras?

Without any doubt, social status shapes our behaviour in daily life. Defined as a ranking in a social group that is commonly recognised, social status typically “carries with it the expectation of entitlement to certain resources” (Ball et al. 2001). It has been recognised as “the most important incentive and motivating force of social behaviour” aside from economic payoffs (Harsanyi 1966). Recent studies in social psychology show that the desire for status is a fundamental and universal motive among individuals. In a wide range of studies across psychology, sociology, anthropology, economics, public health, and organisational behaviour, people are observed to pursue status for evolutionary benefit and attention, access to scarce resources, and more (Anderson et al. 2015).

A connection between social status and social preferences has been observed in previous studies, yet, little is known about the mechanism underlying how people are affected by social status. Will an individual with high status behave in a more altruistic manner? If so, what might be the reason for such prosocial behaviour?

There are three obstacles in setting up research to address these questions. First, present studies in the area rely on different definitions of social status; and as a result, the findings are incomparable. There is as of yet no consensus on the effects of having a higher or lower social status on prosocial behaviour. As will be discussed in more detail in the literature review, some studies show that high status individuals behave less generously, while others reveal the reverse.

In addition, the factors that compose a high or low social status are endogenous and may also affect behaviour directly (i.e., not mediated by social status). Therefore it is
hard to isolate the ‘pure’ effects of social status on behaviour. Education, for instance, is related to income. People with a high education often earn a high income, and often also have high expertise in some area. This makes it hard to distinguish the effects of social status from those of education, income or expertise. A laboratory experiment provides a useful tool to generate a status ranking among subjects and isolate the effects of ranks from endogenous variables such as income, education, and power.

Moreover, people behave differently, depending on the way social status has been assigned. In particular, subjects with high status, if earned and not randomly assigned, are more willing to exploit their power in bargaining (Hoffman and Spitzer 1985). How social status is assigned may matter for subsequent behaviour because people may perceive the status differently. Examples of a randomly assigned status are royal family members, IQ, beauty, and gender and examples of an earned status are educational degrees, expertise, and income. An individual might show respect to someone who accumulates wealth from hard work, but may not do so to someone who inherits wealth from parents.

In this chapter, I present experimental evidence on the effects of social status on prosocial behaviour. The experiment consists of two stages. At the first stage, subjects go through a trivial task in which they review a set of paintings. This task is used to divide subjects in a group of “winners” and a group of “losers”. Winners are either randomly determined (by table number, Random Status treatment, RS) or are determined by performance in the task (Earned Status treatment, ES). This procedure builds on the minimum group paradigm (Tajfel 1970, Tajfel et al. 1971, Turner et al. 1979, Chen and Li 2009) and minimises the objective differences in characteristics between two groups. There is an additional control group where subjects review the paintings without being made aware of any kind of social status. This setup yields three treatment groups: Baseline, RS, and ES.

Following the Ball et al. (2001) status inducement procedure, winners of the painting task are subsequently awarded with a symbolic gift and applause in a ceremony. I will
argue that performance in the painting task has no correlation with IQ (Bottorf 1946), the ability to solve numerical problems (Zheng et al. 2017a), or income in ways that might be related to social preferences. For this reason, winners in ES can be regarded as being just as randomly chosen as in RS, except that winners in ES may have different perceptions of having earned their status.

The second stage of the experiment consists of a one-shot task, in which two subjects, one with high status and the other with low status, jointly decide on their effort provision. The task requires a constant amount of effort in total. Exerting effort is costly such that one’s individual payoff is decreasing in one’s effort. The first player advises the second player, suggesting how much of the total effort player 2 should exert \((a)\). This advice is non-binding so the second player can follow or disregard \(a\). Rejecting the advice, however, is costly for the team, with the costs being proportional to the deviation from advice \(a\). Upon receiving \(a\), the second player chooses the final effort for both workers \(e_1\) and \(e_2\), and consequently, the final payoffs for both players. There is only one round of the task.

The experimental results reveal two main observations. First, the chosen effort is positively related to one’s status. Compared to Baseline, if player 1 has a higher status than player 2, she advises player 2 to choose 14.5% less effort in RS, and 12.2% less in ES. A similar effect is observed for players 2. Compared to Baseline, a low status player 2 in ES contributes 0.73 lower effort (which is a 34% decrease from the average effort level of 2.14 units in Baseline) than in Baseline; and in RS, this drop is 0.54 points (a 25% decrease from the Baseline average).

Second, the experiment shows that a high status makes players 2 less selfish and more efficiency-concerned. Interestingly, this is only observed in ES when status is perceived as having been earned. A selfish player 2 will always choose the minimum effort for herself no matter what advice she receives from player 1. As the decision of the second player is elicited by the strategy method, we can calculate the proportion of selfish players 2 in each treatment. In Baseline, 50% of player 2s are selfish. This percentage drops to 26.7%
among high status players 2 when their status is earned. The proportion of selfish players 2 in RS and low status players 2 in ES, is no different than in Baseline. Meanwhile, high status players 2 deviate less from the advice than their low status counterparts if their status is earned. In contrast, high status players 2 in RS do not deviate differently than their low-status counterparts.

To explain the results, I analyse the responses of subjects in a questionnaire about their beliefs on ability at the end of the experiment. The results show that that subjects with a high status reveal higher beliefs about their own ability compared to the low status counterparts; and that those with high status should exhibit more responsibilities in tasks. This difference is only significant in the treatment when status is considered earned and not assigned randomly.

This chapter contributes to several strands of literature. First of all, it adds new evidence to the “Noblesse oblige” effect – privilege entails responsibility. Being endowed with high status then makes one more prosocial. This makes sense from a social exchange point of view (Homans 1958, Blau 1964), because status – public recognition, admiration, and respect – can be treated as a reward or gift from i.e. nature, and a reciprocal person will behave in a more prosocial manner in order to complete the social exchange of favours. Various studies have investigated the effects of having a high status in economics experiments. Such high-status individuals have been found to receive higher offers from others in an ultimatum game (Eckel and Ball 1996). Their behaviours are more likely to be mimicked by others in a public goods game (Eckel et al. 2010). In a dictator game, male dictators with royal last names (thus considered to be from the elite social class) in Sweden are less likely to be monetarily punished by a third party, compared to low status male dictators (von Essen and Ranehill 2014). To the best of my knowledge, none of these previous studies addresses the response by the high-status individual to such beneficial treatment. In my experiment, individuals with high status enjoy privileges in the laboratory, including applause from other subjects, decorated computer cells, and a
symbolic gift from the experimenter. I study their response to such preferential treatment. Their prosocial behaviour can be explained by them being positively reciprocal.

To the best of my knowledge, this chapter is the first to credibly create earned social status in the laboratory. It adds to the status inducement procedure of Ball et al. (2001) where social status is exogenously assigned. The assignment procedure has the advantage of generating the feelings of “I earned it” while in fact “randomly” selecting the winners. More subjects in the ES than the RS think the way winners were selected reflected abilities. If subjects were to disagree on the criterion or they would have doubts about the procedure, the effects of the status inducement could be distorted.

Aside from their contributions to the literature, the findings in this chapter are also relevant to policy makers. Policy makers often enjoy a high social status, which, in many cases, entitles them with certain access to scarce resources and, consequently, shapes the distribution of wealth in a society. To better understand a society’s welfare, it is therefore crucial to understand how policy makers’ status affects their social preferences. For instance, Fisman et al. (2015) conduct a laboratory experiment that compares the redistribution attitudes between an elite cadre (Juris Doctor at the Yale Law School) and a diverse sample of Americans in the American Life Panel. The results show that Yale Law School subjects, who will enter future policy making with a high probability, focus less on fairness and are more efficiency-focused than the American Life Panel participants, who are more representative of the American population than Yale elites.

The remainder of this chapter is structured as follows. Section 2 reviews related studies that focus on the role of social status in social preferences. Section 3 introduces the team-production task. Section 4 presents the experimental design, discussing how status is manipulated and what the treatment groups are. Section 5 describes the main results. Section 6 explores the mechanisms underlying the status effects we observe. Section 7 concludes.
3.2 Literature review

Over the past few decades, many correlational studies in economics and psychology have examined the role of social status in social preferences. No consensus, however, has been reached on whether having a higher social status is associated with a more selfish preference or being more altruistic. In some studies, children with a higher status in terms of their educational background (which school they attend, the level of their parents’ education) are more altruistic and less selfish (Bauer et al. 2014), and are more generous in charitable giving (Liebe and Tutic 2010). Others find the opposite, that low-status individuals are more helping (Piff et al. 2010) and are involved with less unethical decision-making, e.g. taking items that do not belong to them (Piff et al. 2012).

There are two reasons why these empirical findings may be so different. First of all, the definition of social status varies in the choice of variables used to differentiate between people with high and low status. For instance, in the studies mentioned above, the proxies for high social status vary from income (Piff et al. 2010), vehicle status (Piff et al. 2012), family education (Bauer et al. 2014, Liebe and Tutic 2010) and weekly hours of work (Glaeser et al. 2000), to gender, race, and religion (Hong and Bohnet 2007).

The second reason is that variables with naturally-occurring social status are endogenous, making it almost impossible to disentangle the effect of social ranking in social preferences from confounders. It has been recognised by both economists and psychologists that there are gender differences in altruism (Andreoni and Vesterlund 2001), inequity-aversion, and trust and reciprocity (see for a review in Croson and Gneezy (2009)). Age is also found to be positively related to provision of public goods in the field (List 2004), and height is negatively related to altruism (Harbaugh et al. 2003). At the meantime, income affects inequity-aversion (see a review in Heffetz and Frank (2010)), and education is related to redistribution preferences (Fong 2001).

Experimental studies have made a breakthrough possible, by allowing one to isolate the effects of social ranks on behaviour from confounding factors. Instead of using naturally-
occurring social status, exogenous social status is introduced. This methodology has clear advantages. Not only does it create a ‘pure’ effect of social ranking on preferences which is independent of income, education, and other confounding factors, but it also creates an agreement across individuals on what constitutes high status. For instance, a football player might enjoy a high status among a group of athletes but may not do so among economists. Such differences will occur much less frequently when status is induced in the laboratory.

In economics, Hoffman and Spitzer (1985) were the first to compare the behaviour of individuals who earned a high status to those who were randomly appointed such. Before participating in a bargaining game, some players were either appointed as having ‘high status’ based on a coin flip or based on winning a hash mark game. These high status players were subsequently given the role of proposer in an ultimatum game. The authors find that when status is based on skills, proposers are more willing to exploit their power by suggesting less equal splits.

Nevertheless, even when status is assigned randomly, previous studies have not reached an agreement on the effect of a high status on behaviour. In a carpooling arrangement (Fiddick and Cummins 2007), individuals who are primed with high status are more willing to tolerate free-riding; in a similar car-pooling task, subjects who are primed with high status are more likely to tolerate cheating (Fiddick et al. 2013). However, in a series of experiments in Guinote et al. (2015), individuals with randomly high social status (by receiving false information about the high ranking of their department) are less willing to help someone in need and less likely to do volunteer work or public service.

The seminal paper by Ball et al. (2001) compares behaviour of subjects with high status and low status in a market setting, and in addition, differentiates the situations when status is randomly drawn from a basket or determined by a trivia quiz (Eckel and Ball 1996, Ball and Eckel 1998) with slight deception (the statuses in both treatments are in fact allocated randomly). Their results show that subjects with high status end up
with a larger share of the surplus than their low-status counterparts. However, random status has a stronger effect than earned status in their setting; the authors speculate on how this might have been affected by the experimental procedures in the earned status treatment.\footnote{The authors speculate that the reason why the ‘earned status’ treatment shows a weaker treatment effect is because subjects are asked to answer questions that they are not familiar with, and consider it unfair to base status on these. See footnote 14 in Ball et al. (2001).} Since its publication, the Ball et al. (2001) procedure used to assign earned status has been adopted in various studies. This has led to results showing that a high social status helps to achieve a Pareto superior outcome in a coordination game (Eckel and Wilson 2007), to induce social learning in public goods provision (Eckel et al. 2010), and to increase charitable giving in cooperative settings (Kumru and Vesterlund 2010).

This chapter also builds on the Ball et al. (2001) social status inducement method. However, there is no deception and all procedures are made transparent to subjects.

### 3.3 Team-production task

In this one-shot game, two players team up to work on a task where the total amount of effort is fixed at 10 units, with \( e_1 \) from player 1 and \( e_2 \) from player 2, see Equation 3.1. The aggregate effort of 10 yields a revenue of 48, to be divided between the two players.

\[
e_1 + e_2 = 10. \tag{3.1}
\]

There are two stages. First, player 1 gives a non-binding advice \( a \) to player 2 concerning how much effort she should choose. Upon receiving the advice, player 2 decides on her actual effort. Recalling that the total effort is constant, by choosing her own effort player 2 also determines player 1’s effort.

Effort is costly. The payoff for player \( i \), \( \pi_i \), is 24 minus the cost of effort. The marginal cost of effort is 2 points. The payoff for player 1 is then \( \pi_1 = 24 - 2e_1 \). If player 2 follows player 1’s advice \((e_2 = a)\), the payoff for player 2 is \( \pi_2 = 24 - 2e_2 \). Aggregate payoff under
each effort allocation, if no deviation, is then constant and equal to $28 (= 48 - 2(e_1 + e_2))$. See Nikiforakis et al. (2014) for a similar task with constant aggregate payoff. Any allocation where player 2 follows the advice is then socially efficient.

It is possible that player 2 deviates from the advice. Deviation is costly, however. In particular, each unit deviation from the advice gives rise to a 1 point decrease in player 2’s payoff; hence, a deviation by $P$ units yields $\pi_2 = 24 - 2e_2 - P$. Player 1’s payoff is not affected. Note that this means that deviation form the advice yields an inefficiency at the team level. In summary, payoffs in this game are given by

\begin{align*}
P &= |e_2 - a|, \\
\pi_1 &= 24 - 2e_1, \\
\pi_2 &= 24 - 2e_2 - P. 
\end{align*}

(3.2)

In the experiment, effort choices are restricted to the set of odd-numbered integers 1, 3, 5, 7, 9. Table 3.1 summarises the payoffs for both players under five advice schemes, if no deviations occur. In Table 3.1, I also report two equilibria assuming, respectively, pure selfishness and pure inequity-aversion for player 2. Player 1’s preferences are inconsequential because player 2 decides about the final payoff. If player 2 is purely selfish, she will always choose the minimum effort for herself because she is at least as well-off as when she follows the advice. Therefore the sub-game perfect Nash equilibrium involves advice $a \in \{1, 3, 5, 7, 9\}$, and $e_2 = 1$. A different equilibrium occurs when player 2 is completely inequity averse. Suppose player 1 knows that player 2 cares about fairness and will minimise inequality by choosing $(5, 5)$. She will advise $a = 5$. The reason is that for any other advice, player 2 will deviate towards the equitable outcome and player 1’s payoff will be reduced. Therefore the equilibrium for inequity averse players is $a = 5$, and $e_2 = 5$. 

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Table 3.1: Payoffs if no deviation

<table>
<thead>
<tr>
<th>$e_2$ ($= a$)</th>
<th>1</th>
<th>3</th>
<th>5$^b$</th>
<th>7</th>
<th>9</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\pi_1$</td>
<td>6</td>
<td>10</td>
<td>14</td>
<td>18</td>
<td>22</td>
</tr>
<tr>
<td>$\pi_2$</td>
<td>22</td>
<td>18</td>
<td>14</td>
<td>10</td>
<td>6</td>
</tr>
</tbody>
</table>

*Notes:* $\dagger$ is the equilibrium for self-regarding player 2. $^b$ is the equilibrium assuming an inequity-averse player 2.

### 3.4 Experimental design

In this experiment, there are two parts and three treatment groups. In all treatments, subjects first review 10 pairs of paintings. If they are in the treatments with social status (to be explained below), results from the painting task are revealed. A ceremony following the first part awards the winners from the painting task. The way in which winners are appointed depends on the treatment, as explained below. There is no such ceremony in a control treatment where no status is available. In the second part of the experiment, subjects team up in pairs to conduct the team production task described above. In treatments with social status, all pairs consist of one high status person and one low status person. At the end of the experiment, I collect social and economic characteristics in a questionnaire. I adopt a between-subject design. The selection procedures of winners are made transparent to subjects and the payoff scheme is common knowledge. Subjects are also asked to answer questions to check their understanding of the instructions and the calculation of payoffs.\(^2\)

#### 3.4.1 Part 1: Minimum status

In part one, subjects review 10 pairs of paintings that are painted either by a child under 15 years old or by a professional adult painter. Their task is to tell the sources of each painting with four possibilities: both are painted by children, both are painted by

\(^2\)For detailed information of instructions and questionnaire, please see Appendix A.
professional adult painters, the painting on the left is painted by a child and the painting on the right is painted by a professional, and the painting on the left is painted by a professional and the painting on the right is painted by a child.\textsuperscript{3} In Baseline, subjects complete the painting task and directly go to the second part. They are not informed about their performance in the painting task. In RS and ES, subjects know how the winners are determined. There are either selected randomly by table number (RS; among the 20 subjects in a session, those with a table number below 11 are the winners) or by the number of correct answers (ES; the 10 subjects with most points are the winners).

The procedures of the ceremony are standard and identical for both RS and ES. Once the painting task has finished, subjects are seated in the reception room next to the laboratory. Then the experimenter publicly announces the winners’ table numbers. To better recognise the winners, the experimenter attaches a star on winners’ station IDs.\textsuperscript{4} In addition, the experimenter gives a small gift to each winner.\textsuperscript{5} Finally, all subjects are asked to applaud the winners. Winners enjoy two privileges for the rest of the experiment: first, they will be seated at a ‘VIP’ area that is decorated with stars. Questions from the VIP seats will be answered first if a winner and a loser have questions at the same time. Second, winners will be the first to be paid by the end of the experiment.

Using a high score on the painting task to determine the winners aims at generating a feeling of entitlement without selecting subjects on unobservables that might be correlated with social preferences such as wealth, education, and cognitive abilities. Bottorf (1946) shows that the ability to appreciate art is uncorrelated with IQ. A similar task in Zheng\textsuperscript{6}.

\textsuperscript{3}Screenshots for the paintings are available in Appendix B.
\textsuperscript{4}The station ID is used in every CREED experiment. It is part of the standard procedures at the CREED laboratory to ensure complete anonymity. Upon arrival at the laboratory and before the experiment starts, each subject randomly draws a station ID to determine at which station she sits during the experiment. After subjects have finished the experiment, they receive their payments according to the station ID.
\textsuperscript{5}The small gift is a pen from the shop of the University of Amsterdam with the university’s name and logo. The pens are common to students and staffs. Sometimes, students get the pens for free at workshops, seminars, and conferences. I chose the pen as presents to avoid possible income effects. The price for 50 pens was 37.5 euro at the time we purchased them. For pictures of the VIP area, stars, and the pen during the experiment, see Appendix C.
et al. (2017a) shows that there is no correlation between the performance in the painting task and mathematical ability (see chapter 4). We thus expect no differences between the two treatments in terms of relevant characteristics (see also Table 3.3).

### 3.4.2 Part 2: Treatments

There are three treatment groups in our experiment. They are a baseline treatment where there is no status, a Random Status treatment group where the winners are randomly chosen, and an Earned Status treatment group where the winners are chosen by their performance.

Since in the effort task, each high status player is paired with a low status player, two situations occur in the treatments with status: Player 1 is high status and player 2 is low status and player 1 is low status and player 2 is high status. A brief summary of all treatment groups is presented in Table 3.2. Because there is no status in Baseline, players in Baseline are paired according to their station ID, which is used in RS to determine winners.

Subjects all know that there is only one round in the team-production task. After player 1 has given a piece of advice, player 2 chooses the actual effort. The answers of player 2 are elicited by the strategy method. That is, for each possible advice, player 2 should give her choice for $e_2$. The final payoff is determined by the actual advice $a$ and actual effort level $e_2$, using equations (3.1) and (3.2).

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Player 1</th>
<th>Player 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Random Status</td>
<td>High</td>
<td>Low</td>
</tr>
<tr>
<td></td>
<td>Low</td>
<td>High</td>
</tr>
<tr>
<td>Earned Status</td>
<td>High</td>
<td>Low</td>
</tr>
<tr>
<td></td>
<td>Low</td>
<td>High</td>
</tr>
</tbody>
</table>

---

6Zheng et al. (2017a) – chapter 4 – shows an insignificant negative correlation (-0.04, with 208 observations) between scores from the painting task and scores from a mathematical task.
The difference between Baseline and RS will shed light on whether even a minimal status differentiation has behavioural effects. Since it is made clear that winners in RS are selected completely randomly (by station ID), there I expect no differences between RS and Baseline. The distinction between RS and ES aims at giving insights in the effects of earning a high status.

3.5 Results

3.5.1 Descriptive statistics

The experiment took place in the CREED laboratory of the University of Amsterdam. 300 subjects participated in the experiment. There were 20 subjects in each of 15 sessions: 3 sessions in Baseline, and 6 each in RS and ES. On average, each session lasted 45 minutes and the average payment was 14.13 euro per person, including a 7 euro show-up fee.

The randomisation of subjects across treatments seems to have worked quite well. Table 3.3 reports the descriptive statistics. Tests for differences in observable characteristics across treatments show no such differences between RS and ES. There are, however, differences between these two treatments and Baseline. The major difference occurs in ethnicities (with the baseline having relatively more Asian participants), while subjects in RS are also marginally significantly younger than in Baseline. Table 3.3 shows that subjects are around 22 years old. The gender ratio is reasonably balanced in all treatments: The average percentage of female subjects is about 50%. We observe no differences in educational background, average payoff, and the performance of the painting task across treatment groups.

---

7The sample size is much smaller in Baseline. Subjects are recruited online from the subjects pool at the CREED laboratory and the experimenters could not control for the ethnicities of the subjects ex ante. I will control for the differences in the parametric analysis.
Table 3.3: Descriptive statistics

<table>
<thead>
<tr>
<th>Treatments</th>
<th>Baseline</th>
<th>RS</th>
<th>ES</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>22.9</td>
<td>22.09*</td>
<td>22.72</td>
</tr>
<tr>
<td></td>
<td>(3.82)</td>
<td>(2.53)</td>
<td>(3.00)</td>
</tr>
<tr>
<td>%Female</td>
<td>55.00%</td>
<td>45.00%</td>
<td>48.33%</td>
</tr>
<tr>
<td></td>
<td>(0.51)</td>
<td>(0.50)</td>
<td>(0.50)</td>
</tr>
<tr>
<td>%White</td>
<td>45.00%</td>
<td>72.50%***</td>
<td>64.17%**</td>
</tr>
<tr>
<td></td>
<td>(0.50)</td>
<td>(0.45)</td>
<td>(0.48)</td>
</tr>
<tr>
<td>%Asian</td>
<td>40.00%</td>
<td>16.67%***</td>
<td>24.17%*</td>
</tr>
<tr>
<td></td>
<td>(0.50)</td>
<td>(0.37)</td>
<td>(0.43)</td>
</tr>
<tr>
<td>%Other Race</td>
<td>15.00%</td>
<td>10.83%</td>
<td>11.67%</td>
</tr>
<tr>
<td></td>
<td>(0.36)</td>
<td>(0.31)</td>
<td>(0.32)</td>
</tr>
<tr>
<td>%Econ Major</td>
<td>70.00%</td>
<td>67.50%</td>
<td>48.33%</td>
</tr>
<tr>
<td></td>
<td>(0.46)</td>
<td>(0.47)</td>
<td>(0.48)</td>
</tr>
<tr>
<td>Performance painting task</td>
<td>3.68</td>
<td>3.65</td>
<td>3.73</td>
</tr>
<tr>
<td></td>
<td>(1.32)</td>
<td>(1.52)</td>
<td>(1.62)</td>
</tr>
<tr>
<td>Average payoff</td>
<td>14.5</td>
<td>14.05</td>
<td>14.04</td>
</tr>
<tr>
<td></td>
<td>(3.28)</td>
<td>(2.54)</td>
<td>(2.25)</td>
</tr>
<tr>
<td>N (Player 1)</td>
<td>30</td>
<td>60^nb1</td>
<td>57^nb2</td>
</tr>
<tr>
<td>N (Player 2)</td>
<td>30</td>
<td>60</td>
<td>63</td>
</tr>
<tr>
<td>N</td>
<td>60</td>
<td>120</td>
<td>120</td>
</tr>
</tbody>
</table>

Notes:

*nb1: The number of high and low status players 1 and players 2 in RS is 30. The number of high (low) status players 1 in ES is 30 (27), and the number of high (low) status players 2 in ES is 30 (33).

*nb2: The unequal number of players 1 and players 2 in ES is due to a programming mistake. The behaviour of those players, however, are not affected.

Standard deviations are between parentheses. “Other race” refers to Black, Hispanics, and other races. Significance (MW) of the differences between Baseline and RS/ES are reported.

We observe no significant difference between RS and ES. *** *p < 0.01 ** *p < 0.05 * *p < 0.10.

3.5.2 Treatment effects

Behaviour of player 2

There are three major findings for players 2. First, there are fewer selfish players 2 when they have high status in ES. Second, a player 2 contributes more in the team-production
task if she has high status, regardless of how the high status is obtained. Last, high status players 2 deviate less from player 1’s advice than low status players 2 when status is earned.

Figure 3.1: Scatter plot of $e_2$ for player 2: Baseline

Notes: The size of the dots represents the frequency of $e_2$. There are 30 players 2 in Baseline. Player 2’s decision is elicited by the strategy method. The horizontal axis gives the “hypothetical” advice $a$ from player 1, suggesting how many units of effort player 2 should exert for the team. The vertical axis gives the actual effort $e_2$ chosen by player 2.

Figure 3.1 shows the distribution of players 2’s effort $e_2$ in Baseline. Since player 2’s decision $e_2$ is elicited by the strategy method, Figure 3.1 describes her choices of $e_2$ for each possible advice. Each player makes five decisions, namely, an effort level for each advice $a = 1, 3, 5, 7, 9$. Figure 3.1 clearly shows that many players 2 in Baseline choose the minimum effort level $e_2 = 1$, regardless of the advice. Even in the fair-split case when
$a = 5$, almost half of the subjects choose $e_2 = 1$. Some players 2 choose a 50 - 50 split $e_2 = 5$ but the proportion is smaller than for the minimum effort. Other choices are made much less frequently. The fraction of subjects who choose $e_2 = 1$ is 73.33% ($a = 1$), 66.67% ($a = 3$), 50% ($a = 5$), 56.67% ($a = 7$), and 80% ($a = 9$).

Figure 3.2: Scatter plot of $e_2$ for player 2: RS

Notes: The size of the dots represents the frequency of $e_2$. There are 60 player 2 in RS, 30 are high-status and 30 are low-status. Player 2’s decision is elicited by the strategy method. The horizontal axis gives the “hypothetical” advice $a$ from player 1, suggesting how many units of effort player 2 should exert for the team. The vertical axis gives the actual effort $e_2$ chosen by player 2.
Figure 3.3: Scatter plot of $e_2$ for player 2: ES

Notes: The size of the dots represents the frequency of $e_2$. There are 63 player 2 in ES, 30 are high-status and 33 are low-status. Player 2’s decision is elicited by the strategy method. The horizontal axis gives the “hypothetical” advice $a$ from player 1, suggesting how many units of effort player 2 should exert for the team. The vertical axis gives the actual effort $e_2$ chosen by player 2.

Figures 3.2 and 3.3 show the distribution for player 2 in the two treatments with status. Figure 3.2 shows how players 2 choose $e_2$ in RS depending on the advice, with the choices of high-status players 2 on the left and those of low-status players 2 on the right. Figure 3.3 does the same for ES.

Consistent with Baseline, we observe that the majority of players 2 chooses the minimum effort $e_2 = 1$ in RS and ES, independently of their status. In RS, the fraction of subjects who choose $e_2 = 1$ is 63.33% ($a = 1$), 50% ($a = 3$), 40% ($a = 5$), 46.67% ($a = 7$), and 80% ($a = 9$) if status is high; and, the proportions are 76.67% , 60%,
43.33%, 53.33%, and 83.33%, respectively, if status is low. Likewise in ES, the fraction of minimum effort subject chosen is 50%, 50%, 30%, 33.33%, and 83.33% if status is high; and 69.7%, 63.64%, 51.52%, 57.58%, and 75.76% if status is low.

Figure 3.4: Percentage of self-regarding player 2

Notes: “Status” refers to own status. Player 2 is selfish if she always chooses the minimum effort no matter what advice player 1 gives. The percentage of selfish player 2s in ES and with high status is significantly lower than in baseline ($p = 0.065$, MW). Other comparisons are statistically insignificant.

Next, I compare player 2’s behaviour across treatments. To start, Figure 3.4 shows the percentage of selfish players 2 in the team-production task. Player 2 is defined as being selfish if she always chooses the minimum effort, independent of player 1’s advice. When status is absent (Baseline) 50% of players 2 (15 out of 30) are selfish in this way. This proportion is slightly lower in RS – about 40% – and this proportion is identical between low status (40%, 12 out of 30) and high status players 2 (40%, 12 out of 30). When status is earned, however, fewer high status players 2 fall into this category. Specifically, 27% (8 out of 30) of high status players 2 in ES respond with minimum effort to each advice. The drop from 50% in Baseline to 27% is significant at the 10% level ($p = 0.065$, MW).
In contrast, if the earned status is low, the ratio of selfish subjects is 45.5%, 15 out of 33 and it is not significantly different from Baseline \((p = 0.72, \text{MW})\). In ES, the fraction of selfish player 2s is higher if status is low but the difference with high status individuals in ES is statistically insignificant \((p = 0.125, \text{MW})\).

To further analyse the treatment effects for player 2, I adopt the econometric model depicted in equation 3.3, below. I focus on two dimensions of player 2’s behaviour during the experiment: average effort and average absolute deviation. The dependent variable \(Y_i\) is either the average effort in the advice scenarios or the average deviation in the same five scenarios. The dependent variable \(Y_i\) is regressed on constant term \(\alpha_i\) and four dummies which describe the social status and treatment group a player 2 belongs to: high status player 2 in RS \((HR_i)\); low status player 2 in RS \((LR_i)\); high status player 2 in ES \((HE_i)\), and low status player 2 in ES \((LE_i)\). The dummies will all take the value of 0 if player 2 is in Baseline. I also include a set of socioeconomic background variables \((Z_i)\) in the regression, to wit, gender, age, ethnicity, and whether the subject has a major in economics.\(^8\)

\[
Y_i = \alpha_i + \beta_1 HR_i + \beta_2 LR_i + \beta_3 HE_i + \beta_4 LE_i + \gamma Z_i + \varepsilon. \tag{3.3}
\]

This model specification identifies the treatment effects between players 2 with unequal social status. On the one hand, the coefficients of the four dummies identify how players 2 with status in a certain treatment group behave differently from the Baseline when there is no status. For example, \(\beta_1 = Y_i(H = 1|RS = 1) - Y_i(Baseline = 1)\) represents the difference between behaviour in the baseline and that of high status player 2s in the random status treatment, \(\beta_2 = Y_i(H = 0|RS = 1) - Y_i(Baseline = 1)\) describes how low status player 2s in RS behave differently from baseline; likewise, \(\beta_3 = Y_i(H = 1|ES = 1) - Y_i(Baseline = 1)\) and \(\beta_4 = Y_i(H = 1|ES = 1) - Y_i(Baseline = 1)\) illustrate the differences between ES and baseline, with \(H\) as the dummy for being high status.

\(^8\)There is attrition of 20 observations because some variables were not saved properly. In particular, in the first two sessions the variables for gender and ethnic background were not stored. I checked the regressions without the two sessions and the results are not significantly different.
On the other hand, \((\beta_1 - \beta_2)\) describes how high and low status players 2 in RS behave differently from each other; similarly, \((\beta_3 - \beta_4)\) represents the difference in behaviour between a high and a low status player 2 in ES. Another set of interesting comparisons is \((\beta_1 - \beta_3)\) and \((\beta_2 - \beta_4)\) where the former investigates the differences among high status player 2s between RS and ES and the latter studies the behavioural differences among low status player 2s between RS and ES.

I report the regression results about treatment effects on effort and deviation in Table 3.4 on the full sample. I choose a Tobit model in all cases to account for the fact that the effort variable is defined to be between 1 and 9 and the deviation variable is bounded between 0 and 8. The results in column (1) and (2) show that, compared to the Baseline, having a low *earned* status decreases the average effort. After controlling for personal characteristics, on average, a low status person chooses 1.07 points less (out of 10) than Baseline, if in ES. In column (3) and (4) where the dependent variable is the average deviation from the socially-efficient allocations, I find that subjects in RS do not deviate differently from Baseline, nor do low status player 2s in ES; however, high status subjects in ES deviate significantly less than in Baseline, suggesting that they choose more socially efficient outcomes. After controlling for personal characteristics, on average, a high status player 2 in ES deviates 0.493 points less than in Baseline. Other personal characteristics suggest that older subjects tend to choose higher effort. And caucasian subjects seem to choose lower effort. Caucasian and asian subjects deviate more from the socially-efficient effort outcomes than other races.

Table 3.5 reports the F-test p-values for behavioural difference between high and low status player 2s within the same treatment and the differences between RS and ES for given status. On the one hand, I find insignificant differences for the effort regression. The effort provision between a low and a high status player 2 is not significantly different, neither in RS \((p = 0.364)\) nor in ES \((p = 0.226)\). On the other hand, I find in the deviation regression that first, a high status player 2 deviates more than a low status counterpart
in ES ($p = 0.002$); second, the deviation behaviour is different between low status player 2s in RS and ES ($p = 0.063$).

\[
BM 1a U p = 0.002 \quad Vc b2+QM/- i?2 /2pBiBQM #2?pBQm` Bb /Bz2`2Mi #2ir22M HQr biimb THv2`
\]

Table 3.4: Treatment effects: Effort and deviation by player 2

<table>
<thead>
<tr>
<th>Dependent variable = average of ...</th>
<th><strong>Effort (Tobit)</strong></th>
<th><strong>Deviation (Tobit)</strong></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>High status player 2 in RS</td>
<td>0.648</td>
<td>-0.161</td>
</tr>
<tr>
<td></td>
<td>(1.135)</td>
<td>(0.592)</td>
</tr>
<tr>
<td>Low status player 2 in RS</td>
<td>0.106</td>
<td>-0.676</td>
</tr>
<tr>
<td></td>
<td>(0.961)</td>
<td>(0.435)</td>
</tr>
<tr>
<td>High status player 2 in ES</td>
<td>0.654</td>
<td>-0.369</td>
</tr>
<tr>
<td></td>
<td>(0.995)</td>
<td>(0.507)</td>
</tr>
<tr>
<td>Low status player 2 in ES</td>
<td>-0.073</td>
<td>-1.070***</td>
</tr>
<tr>
<td></td>
<td>(0.915)</td>
<td>(0.284)</td>
</tr>
<tr>
<td>Female</td>
<td>0.204</td>
<td>0.066</td>
</tr>
<tr>
<td></td>
<td>(0.405)</td>
<td>(0.123)</td>
</tr>
<tr>
<td>Age</td>
<td>0.101**</td>
<td>-0.012</td>
</tr>
<tr>
<td></td>
<td>(0.048)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>Caucasian</td>
<td>-1.480**</td>
<td>0.955**</td>
</tr>
<tr>
<td></td>
<td>(0.617)</td>
<td>(0.461)</td>
</tr>
<tr>
<td>Asian</td>
<td>-0.721</td>
<td>0.785**</td>
</tr>
<tr>
<td></td>
<td>(0.535)</td>
<td>(0.372)</td>
</tr>
<tr>
<td>Econ major</td>
<td>-0.785</td>
<td>0.278</td>
</tr>
<tr>
<td></td>
<td>(0.480)</td>
<td>(0.213)</td>
</tr>
<tr>
<td>Constant</td>
<td>2.397***</td>
<td>2.134***</td>
</tr>
<tr>
<td></td>
<td>(0.140)</td>
<td>(0.131)</td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td>-259.493</td>
<td>-223.111</td>
</tr>
<tr>
<td>N</td>
<td>153</td>
<td>133</td>
</tr>
</tbody>
</table>

Notes: Robust standard errors are clustered at the session level and are between parentheses.

* $p < 0.1$, **$p < 0.05$, ***$p < 0.01$. 

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Table 3.5: P-values for treatment effects

<table>
<thead>
<tr>
<th></th>
<th>Effort regression</th>
<th>Deviation regression</th>
</tr>
</thead>
<tbody>
<tr>
<td>$(\beta_1 - \beta_2)$</td>
<td>RS: High vs. Low</td>
<td>0.452</td>
</tr>
<tr>
<td>$(\beta_3 - \beta_4)$</td>
<td>ES: High vs. Low</td>
<td>0.136</td>
</tr>
<tr>
<td>$(\beta_1 - \beta_3)$</td>
<td>High status: RS vs. ES</td>
<td>0.784</td>
</tr>
<tr>
<td>$(\beta_2 - \beta_4)$</td>
<td>Low status: RS vs. ES</td>
<td>0.329</td>
</tr>
</tbody>
</table>

Notes: The coefficients are from Table 3.4, where $\beta_1$, $\beta_2$, $\beta_3$, and $\beta_4$ are coefficients for variables of “High status player 2 in RS”, “Low status player 2 in RS”, “High status player 2 in ES”, and “Low status player 2 in ES”, respectively. F-test p-values are reported.

Because Figure 3.4 shows a difference in the percentage of selfish players 2, it is worthwhile to explore the treatment effects among the ‘non-selfish’ players 2 who sometimes choose more than the minimum effort.

Table 3.6 reports the results with non-selfish players 2. Similar to the previous regression in the full sample, I report the analysis for effort in columns (1) and (2), and that for deviation in columns (3) and (4). The regression about effort shows that among non-selfish players and compared to Baseline, high earned status players 2 choose lower effort; this is robust after controlling for personal characteristics. The corresponding p-value in Table 3.7 shows that high status players 2 with earned status choose lower effort than those with random high status. This effect is different from the regression in Table 3.4 where it is the low status subjects who choose lower effort. The difference can be explained by the fact that there are fewer selfish players 2 with high earned status. Columns (3) and (4) show that among non-selfish players 2 and compared to Baseline, high status – either random or earned – subjects deviate less from the socially efficient effort combinations, and the effect is robust to personal characteristics. In addition, high status players 2 in ES deviate less than their low status counterparts in ES and the difference is significant at 1% level, as shown in Table 3.7.
Table 3.6: Treatment effects among non-selfish players 2

<table>
<thead>
<tr>
<th></th>
<th>Effort (Tobit)</th>
<th>Deviation (Tobit)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>High status player 2 in RS</td>
<td>0.196</td>
<td>0.366</td>
</tr>
<tr>
<td></td>
<td>(0.358)</td>
<td>(0.272)</td>
</tr>
<tr>
<td>Low status player 2 in RS</td>
<td>-0.604*</td>
<td>-0.422</td>
</tr>
<tr>
<td></td>
<td>(0.320)</td>
<td>(0.298)</td>
</tr>
<tr>
<td>High status player 2 in ES</td>
<td>-0.627***</td>
<td>-0.526**</td>
</tr>
<tr>
<td></td>
<td>(0.215)</td>
<td>(0.225)</td>
</tr>
<tr>
<td>Low status player 2 in ES</td>
<td>-0.493</td>
<td>-0.375</td>
</tr>
<tr>
<td></td>
<td>(0.323)</td>
<td>(0.297)</td>
</tr>
<tr>
<td>Female</td>
<td>0.228</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.227)</td>
<td></td>
</tr>
<tr>
<td>Age</td>
<td>0.055</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.040)</td>
<td></td>
</tr>
<tr>
<td>Caucasian</td>
<td>-0.299</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.336)</td>
<td></td>
</tr>
<tr>
<td>Asian</td>
<td>0.004</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.326)</td>
<td></td>
</tr>
<tr>
<td>Econ major</td>
<td>-0.160</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.316)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>1.234***</td>
<td>1.211***</td>
</tr>
<tr>
<td></td>
<td>(0.076)</td>
<td>(0.084)</td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td>-148.244</td>
<td>-135.288</td>
</tr>
<tr>
<td>N</td>
<td>91</td>
<td>84</td>
</tr>
</tbody>
</table>

Notes: Robust standard errors are clustered at the session level and are between parentheses.  
* $p < 0.1$, **$p < 0.05$, ***$p < 0.01$.  

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Table 3.7: P-values for treatment effects in Table 3.6

<table>
<thead>
<tr>
<th></th>
<th>Effort regression</th>
<th>Deviation regression</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\beta_1 - \beta_2$</td>
<td>RS: High vs. Low</td>
<td>0.095</td>
</tr>
<tr>
<td>$\beta_3 - \beta_4$</td>
<td>ES: High vs. Low</td>
<td>0.292</td>
</tr>
<tr>
<td>$\beta_1 - \beta_3$</td>
<td>High status: RS vs. ES</td>
<td>0.009</td>
</tr>
<tr>
<td>$\beta_2 - \beta_4$</td>
<td>Low status: RS vs. ES</td>
<td>0.890</td>
</tr>
</tbody>
</table>

Notes: The coefficients are from Table 3.6 columns (2) and (4), where $\beta_1$, $\beta_2$, $\beta_3$, and $\beta_4$ are coefficients for variables of “High status player 2 in RS”, “Low status player 2 in RS”, “High status player 2 in ES”, and “Low status player 2 in ES”, respectively. F-test p-values are reported.

The patterns for deviation are demonstrated graphically in Figure 3.5. The horizontal axis shows each possible scenario player 2 faces and the vertical axis gives the deviation player 2 chooses. The general shape appears similar in all treatment groups. The effect of status, however, differs between treatments. In particular, in ES, the deviation by a low status player 2 first-order stochastically dominates that of high status players.

Figure 3.5: Behaviour of player 2: Deviations from player 1’s advice

Notes: “Status” refers to own status. In ES, low status subjects on average deviate significantly more than their high status counterparts (MW, $p = 0.01$) and there is first-order stochastic dominance. High status players 2 in RS do not deviate differently from their low status counterparts (MW, $p = 0.668$).

In summary, the treatment effects on players 2’s effort choices depend largely on the proportion of selfish players. The deviation from player 1’s advice is consistent when selfish players are included and excluded from the analysis. Players 2 with earned high status deviate significantly less than players without status and their low status counterparts.

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Behaviour of player 1

As shown in Figure 3.6, player 1 in Baseline advises player 2 to choose, on average, 6 out of 10 units of effort in the team production. High status players 1 in RS advise, on average, 5.13, a significant decrease of 14.5% from Baseline (Mann-Witney, $p = 0.033$). The advice from a high status player 1 in ES is 5.27 points, a significant decrease of 12.2% compared to Baseline (MW, $p = 0.045$). I find no statistically significant difference between the advice from a low status player 1 in either status treatment and the Baseline (MW, $p = 0.765$ in RS vs. Baseline, and $p = 0.609$ in ES vs. Baseline).

Figure 3.6 also allows us to compare advice between a high status player and a low status player 1 in RS and ES. Low status player 1 in RS, on average, gives an advice of 5.8 points to player 2. This advice is statistically insignificantly different from player 1 with high status (MW, $p = 0.103$). In ES, a low status player 1 gives an average advice of 5.74 points to player 2. This advice is marginally significantly different from her high status counterparts (MW, $p = 0.092$).

These nonparametric tests thus show that players 1 with high social status advise less effort for player 2 (thus leaving more effort to exert by themselves) than when they have no differences in social status; their low-status counterparts behave in a similar pattern as when there are no differences in social status.

In addition, the behaviour of player 1 shows less selfishness when player 2 has an earned status than in Baseline. Table 3.8 reports the distribution of types of player 1 in the three treatments. Based on the advice given, three types of player 1 are distinguished: self-regarding ($a \geq 7$), inequity averse ($a = 5$), and altruistic ($a \leq 3$). Two findings emerge about self-regarding and inequity-averse players. First, if status is earned, there are fewer high status players 1 who gives a self-regarding advice. Row 1 in Table 3.8 shows that there are significantly fewer self-regarding subjects in ES than in Baseline. The proportion of selfish players 1 in RS is not significantly different from Baseline. Second, if status is earned, there are more high status players 1 choosing an equal split of effort than in
Baseline. The proportion of inequity-averse player 1 is the same (63.33%, 19 out of 30) in Baseline and RS. In ES the proportion rises to 80% (24 out of 30, \( p = 0.076 \) compared to Baseline) if status is high. Low status players 1 in ES do not show significantly more inequity aversion than in Baseline.

Table 3.8: Distribution of player 1 types

<table>
<thead>
<tr>
<th></th>
<th>Baseline (95)</th>
<th>Random Status</th>
<th>Earned Status</th>
</tr>
</thead>
<tbody>
<tr>
<td>Self-regarding ((a \geq 7))</td>
<td>33.33%</td>
<td>20%</td>
<td>33.33%</td>
</tr>
<tr>
<td>Inequity-averse ((a = 5))</td>
<td>63.33%</td>
<td>63.33%</td>
<td>63.33%</td>
</tr>
<tr>
<td>Altruistic ((a \leq 3))</td>
<td>3.33%</td>
<td>16.67%**</td>
<td>3.33%</td>
</tr>
<tr>
<td>N</td>
<td>30</td>
<td>30</td>
<td>30</td>
</tr>
</tbody>
</table>

Notes: P-value (MW) compared to the Baseline treatment is reported. ** \( p < 0.05 \) * \( p < 0.1 \).
3.6 Discussion

In general, the results from the laboratory experiment show that it is the procedure of earning one’s status rather than gaining a status by luck that makes people with high status behave differently from those with low status; especially high status players are more generous than their low status counterparts. This suggests that it is the belief that status has been earned that leads to the result. It is possible that this makes someone feel a sense of ability and responsibility that makes her act more generously, in other words, ‘Noblesse oblige’.

3.6.1 Possible mechanism

To investigate this possibility, I consider responses of my subjects in the questionnaire administered at the end of the experiment. I asked subjects questions concerning their perceptions towards subjects with low and high status. Using a 7-point Likert scale (from totally disagree to totally agree), subjects indicate their (dis)agreement with various attitudinal statements regarding attitudes. The statements are:
(1) People with stars in my experiment session are more powerful.
(2) People with stars in my experiment session are cleverer.
(3) People with stars in my experiment session are more aggressive.
(4) People with stars in my experiment session deserve to get more in team-production task.
(5) The role I am playing in this experiment is generally better.
(6) The role I am playing in this experiment performs better.
(7) The method to allocate stars in this experiment is fair.
(8) The method to allocate stars reflects abilities.
(9) People with stars have unfair advantages in this experiment.
(10) I am satisfied with my performance.
(11) I prefer the star groups.
Statements (1), (2), (4), (8), and (11) allow me to study whether people perceive status differently depending on the way it has been assigned. I assume that ‘Star’ means higher status. In Figures 3.7 to 3.10, I plot the average perception of players 2 in RS and ES treatments, separately for low status subjects and high status subjects.⁹

There is a clear perception difference in ability and smartness between RS and ES. Figures 3.7 and 3.8 show that players 2 in ES have a higher tendency than in RS to agree that high status subjects are smarter and more able. In addition, high status players 2 attribute more power and cleverness to having stars than their lower status counterparts, regardless of how the status is assigned; see Figures 3.7 and 3.9. However, our subjects do not think people with stars deserve higher payoffs in the team production task; see in Figure 3.10.

Therefore, the way I assign status induces differences in beliefs on the ability and cleverness of people. Specifically, if the status is determined by a well-recognised rule, people think that the winners have higher ability and are cleverer than winners who are randomly nominated. The difference in beliefs on how able high status people are might be the explanation for the treatment effects in the experiment.

⁹Because there is no differences in terms of status subjects in the control treatment (Baseline) are not asked these attitudinal questions. For the perceptions of players 1, please see Appendix C.
Figure 3.7: “People with stars are cleverer”

Figure 3.8: “The method to allocate stars reflects abilities”
Figure 3.9: “People with stars are more powerful”

![Bar graph showing social status comparison between random and earned status for low and high status players.]

Figure 3.10: “People with stars deserve to get more in the team-production task”

![Bar graph showing social status comparison between random and earned status for low and high status players.]

3.6.2 Alternative mechanisms

Are there any other explanations for the treatment differences?
To begin with, the behaviour itself of player 1 cannot explain the treatment effects. This is because of the strategy method, where player 2 does not know the decision by a player 1.

Is it the belief of generosity of players 1 that drives the behaviour of players 2 in different treatments? For instance, a high status player 2 may believe her low status teammate (player 1) is so nice that she will suggest an advice in favour of an equal split, while this belief is stronger if the status is earned than with random status allocation; this player 2 subsequently reciprocate the (expected) nice advice by choosing a higher effort.

To test whether or not the belief has an effect on the behaviour of players 2, I elicited the beliefs of players 2 about the advice suggested by the player 1 she was paired with in the questionnaire. This question is incentivised by giving 1 euro cash for the correct answer. Table 3.9 summarises the result. This clearly shows that, on average, player 2 believes the advice is between 5 and 6 units. Importantly, the beliefs are neither statistically different between treatments nor between statuses. Therefore this alternative mechanism cannot explain our treatment effects either.

<table>
<thead>
<tr>
<th></th>
<th>Random Status</th>
<th>Earned Status</th>
</tr>
</thead>
<tbody>
<tr>
<td>High</td>
<td>5.47</td>
<td>5.73</td>
</tr>
<tr>
<td></td>
<td>(2.21)</td>
<td>(2.85)</td>
</tr>
<tr>
<td>Low</td>
<td>5.18</td>
<td>5.00</td>
</tr>
<tr>
<td></td>
<td>(2.42)</td>
<td>(2.41)</td>
</tr>
</tbody>
</table>

Notes: Standard deviations are between parentheses.

### 3.7 Concluding remarks

In this chapter, I generate ranks in the laboratory, either randomly or assigned with a perception of deservedness, and additionally I study the causal relationship between a high social status and prosocial behaviour. In a one-shot team production task, I pair a
high status worker with a low status worker. Worker 1 first gives an advice to worker 2 suggesting how many units of effort worker 2 should exert. This message, however, is not binding, though deviation from the advice is costly. The total effort is constant and effort is costly; the parameters are chosen such that a rational and selfish worker 2 will always choose the minimum effort level for herself.

The results show a clear pattern. People with high status are more generous than their low status counterparts, both as worker 1 and worker 2. However, there are some differences related to the way in which status is assigned: there are fewer selfish high-status players 2 when status is earned. Moreover, only when status is earned, do high status players 2 deviate much less than their low status counterparts, achieving a more socially efficient outcome. My conjecture for the findings is that subjects in the laboratory form different beliefs about others as well as themselves. Unlike in the random rule case, earning a better rank in the task induces the feeling of having higher abilities or being superior to the others. The belief might well result in a higher aversion for inequalities and may even cultivate feelings of responsibility for the team.

This study focuses on the situation when social interactions take place among individuals with unequal status, such as employer-employee, professor-student, interviewer-interviewee. It would be interesting for future research to compare prosocial behaviour among individuals with the same status, e.g., both high or both low, to investigate whether similar patterns appear.
Appendix A: Instructions in the experiment

Outline of experimental instructions

Instruction in Part One

Welcome to our experiment!

This is an experiment in decision-making. The amount of money you earn will depend upon the decisions you make and on the decisions other people make. This experiment has 2 parts and in total there are 20 participants. Notice that you might seat on a different table in the second part, so please keep your stuffs together with the table card with you while you move.

Now you have already got 7 Euro for showing up here. Your total earnings will be the sum of your payoffs and the show-up fee. In this experiment, we use experimental points (200 points=1 euro). At the end of the experiment you will be paid IN CASH. Everyone will be paid in private and you are under no obligation to tell others how much you earn.

You will receive separate instructions for the two parts before each part begins. Please read all instructions carefully and do NOT communicate with each other during the experiment. If you have a question, feel free to raise your hand, and an experimenter will come to help you.

[Subjects enter the next page of instructions]

Part One

For this experiment, we have randomly assigned you a personal ID (in capital). Yours is A. Please remember your ID because you will use it later. Also please enter below a three-digit number between 99 and 1000 (for example: 123) as your personal password to relog-in your page.

Create your personal password: (a three-digit number between 99 and 1000.)

[When subjects correctly create their own passwords for their user ID, they enter the next page]

Instruction Question in Part One (in Entitled Status treatment)

[If subjects are in the entitled treatment, they receive the instructions as follows]

In this part you and other participants in this room will review ten pairs of paintings.
Answers will be ranked among all participants. The results of this part will determine your role in the second part. Those paintings are selected randomly from 30 paintings—15 by famous adult professional painters and 15 by children under the age of 15. Your task is to find out which painting is painted by whom. There is no time limit in part 1.

After answering all questions about paintings, we will randomly select half of people in this lab to be the winners today. So your answers to the paintings will not determine your role in the second part. Winners of Part 1 will be awarded with small gifts from the experimenter. Also they will sit at the front of the lab (VIP area) so that they can be answered quickly from the experimenter.

Before the experiment starts, we will ask you some questions to check your understanding about the first part.

How many paintings you are going to review in part one?
- 10.
- 15.
- It depends.

What will you know about your ranking in the end of part one?
- I will know my personal ranking.
- I will know whether I am at the top 50% or not.
- I will know nothing about my performance in part one.

How the role in Part 2 determined?
- Based on the results from Part 1.
- Computer randomly determines it.
- It will be determined by table numbers.

[If subjects are in random treatment, they receive the questions as follows]

**Instruction Question in Part One (in Random Status treatment)**

In this part you and other participants in this room will review ten pairs of paintings. Those paintings are selected randomly from 30 paintings—15 by famous adult professional painters and 15 by children under the age of 15. Your task is to find out which painting is painted by whom. There is no time limit in part 1.

After answering all questions about paintings, we will randomly select half of people in this lab to be the winners today. So your answers to the paintings will not determine your role in the second part. Winners of Part 1 will be awarded with small gifts from the experimenter. Also they will sit at the front of the lab (VIP area) so that they can be answered quickly from the experimenter.

Before the experiment starts, we will ask you some questions to check your understanding
about the first part.

How many pairs of paintings you are going to review in part one?
–10.
–15.
–It depends.

What will you know about your ranking in the end of part one?
–I will know my personal ranking.
–I will know whether I am randomly chosen as winners or not.
–I will know other people’s results.

How the role in Part 2 determined?
–Randomly decided.
–Based on the results from part 1.
–I don’t know.

[When subjects correctly answer all questions, they will review 10 pairs of paintings.]

[Painting pairs 1 to 10]

Next, subjects will see the results from part one, depending on the treatment. The winners receive small gifts from the experimenter. They also have the privilege to sit on the “VIP” area in the lab.

Instruction Part Two

In this part you will be paired with another participant as a team to do a task together. This task only has one round.

Team-production Task:
You and the other participant will be involved in a team-producing task. Both of you need to provide effort for this task. \( e_1 \) is the effort units provided by player 1, and \( e_2 \) is effort units provided by player 2. In total, the team has to provide 10 units of effort. So \( e_1 + e_2 = 10 \).

The team production will yield a team payoff of 2800 points which will be split by the two players. Providing effort is costly for the player who provides it; each unit provided by a player will cost that player 200 points.

Roles:
Player 2 is the one who will decide how much effort will be provided by player 1 and how much by player 2. Before player 2 makes the decision about the two effort levels, player 1 can send an advice about how much effort is to be provided by player 2. There will be
some costs to player 2 if he or she does not follow the advice. Below we will explain the
timing of events, and how the decisions affect the payoffs of the players.

**Timing of events:**
(1) Player 1 provides an advice to player 2 about the effort level of player 2. We call this
advice “advice of $e_2$”, because it specifies the effort level that player 1 wants player 2 to
provide for the team;
(2) Player 2 chooses the two effort levels $e_1$ and $e_2$. The two effort levels must sum up to
be 10. Each effort level is at least 1 and at most 9. Only effort levels 1, 3, 5, 7 or 9 are
allowed.
(3) The payoffs are determined.

**Rules of payoffs:**
(1) Player 1’s payoff is equal to $2400 - 200e_1$. That is, for each 1 unit of effort he or she
provides, player 1 must pay for 200 points.
(2) Player 2’s payoff follows the same rule except that player 2 will receive a penalty if his
or her choice of $e_2$ is different from player 1’s advice—“advice of $e_2$”. For each unit effort
that player 2 differs from the advice, player 2 will lose 100 points.
(3) So player 2’s payoff is equal to $2400 - 200e_2 - penalty$. The Table below lists the
payoffs of the two players conditional on the advice of player 1 and the decision of player 2.

For example, if player 1 advises to player 2 to choose an effort level of 9 and the ad-
vice is implemented by player 2, so that $e_2 = 9$ and $e_1 = 1$, then the payoff is 2200
($= 2400 - 200 \times 1$) for player 1 and 600 ($= 2400 - 200 \times 9 - 0$) for player 2.

If in this case, player 2 instead chooses a lower $e_2$, say $e_2 = 7$ (so that $e_1 = 3$ accordingly),
then player 2 will receive a penalty that depends on the difference between the actual $e_2$
and player 1’s advice. In this example, the difference is 2 so that player 2’s payoff becomes
800 ($= 2400 - 200 \times 7 - 100 \times 2$). Player 1’s payoff is 1800 ($= 2400 - 200 \times 3$).
Before the team production task starts, we will ask you some questions to check your understanding about the second part.

How many people are there in total in one team?
- 1.
- 2.
- 3.

What will player 1 do in this task?
- To propose how to divide the revenues between two players.
- To propose how to divide the efforts between two players.
- To accept or reject player 2’s proposal.

Who will decide the final revenue?
- Player 1 always.
- Player 2 always.
- Sometimes player 1 sometimes player 2.

Find out the revenues for both players for the following scenario:
Player 1 proposes to divide the effort between (player 1, player 2) to be (5, 5) and player 2 agrees.
- 1400, 1200: 1400 for player 1 and 1200 for player 2.
- 1400, 1400: 1400 for player 1 and 1400 for player 2.
- 1600, 600: 1600 for player 1 and 600 for player 2.

Find out the revenues for both players for the following scenario:
Player 1 proposes to divide the effort between (player 1, player 2) to be (3, 7) and player 2 decides to give the effort (9, 1).
- 1000, 1400: 1000 for player 1 and 1400 for player 2.
-600, 1600: 600 for player 1 and 1600 for player 2.
-1600, 600: 1600 for player 1 and 600 for player 2.

Find out the revenues for both players for the following scenario:
Player 1 proposes to divide the effort between (player 1, player 2) to be (9, 1) and player 2 decides to give the effort (1, 9).
-600, 2200: 600 for player 1 and 2200 for player 2.
-2200, 200: 2200 for player 1 and 200 for player 2.
-2200, -200: 2200 for player 1 and -200 for player 2.

[After every subject answers the questions correctly, they will be led to the page where their roles will be assigned.]

[If this subject is player 1, then her page is]

**Team Production Task: Player 1**

Now you can make a decision on the team production task. You can refer to the revenue table at the bottom of this page if you need.

You are player 1. You can make a proposal to player 2 how to divide the effort between you two.

- (1, 9): Player 1 makes an effort of 1, player 2 makes an effort of 9.
- (3, 7): Player 1 makes an effort of 3, player 2 makes an effort of 7.
- (5, 5): Player 1 makes an effort of 5, player 2 makes an effort of 5.
- (7, 3): Player 1 makes an effort of 7, player 2 makes an effort of 3.
- (9, 1): Player 1 makes an effort of 9, player 2 makes an effort of 1.

[If this subject is player 2, her page would be]

**Team Production Task: Player 2**

Now you can make a decision on the team production task. You can refer to the revenue table at the bottom of this page if you need.

You are player 2. Here we will ask you what effort you want to make under all situations. Your final revenue depends on player 1’s proposal and your decision. In other words, one of the decisions you make below will be realised.
The screenshot shows the decisions player 2 has to make. Condition on every possible choice player 1 could choose, player 2 decides what her response is.

[Before subjects receive the choices chosen by their opponent they are asked about their characteristics, beliefs about their opponent, and the attitudes to their opponents.]

Exit Survey

Please fill out the following questionnaire.

1) Gender:

2) Age:

3) What do you consider your racial or ethnic background is:
   - White/Caucasian
   - Black
   - Hispanic
   - Asian
   - Other

4) Have you participated in a CREED experiment before?
   - No
   - Yes, once
   - Yes, more than once

5) Have you ever done similar tasks (distinguish paintings from professional ones and unprofessional ones) before?
   - No
   - Yes, once
   - Yes, more than once

6) Department where you study:
   - Faculty of Economics and Business
   - Faculty of Social and Behavioural Sciences-Psychology
   - Faculty of Social and Behavioural Sciences-non Psychology
   - Faculty of Science
   - IIS: beta gamma bachelor
   - Faculty of Law
   - Faculty of Humanities
   - Faculty of Medicine
   - Faculty of Dentistry
   - Another university
   - A Dutch “hogeschool” (HBO)
- Other different places

7) To what extent do you agree with the following statements? (Number 1 to 7 measure the degree of agreement, where 1 = “Totally Disagree”, 7 = “Totally Agree”)
Statement 1: People with stars in my experiment session are more powerful.
Statement 2: People with stars in my experiment session are cleverer.
Statement 3: People with stars in my experiment session are more aggressive.
Statement 4: People with stars in my experiment session deserve to get more in team-production task.
Statement 5: The role I am playing in this experiment is generally better.
Statement 6: The role I am playing in this experiment performs better.
Statement 7: The method to allocate stars in this experiment is fair.
Statement 8: The method to allocate stars reflects abilities.
Statement 9: People with stars have unfair advantages in this experiment.
Statement 10: I am satisfied with my performance.
Statement 11: I prefer the star groups.

8) (Extra opportunity to earn money) What is your guess for your opponent’s choice in the team-production task (correct guess will bring you extra 100 points)?
Appendix B: Paintings for the painting task

Figure 3.11: Pair 1

Figure 3.12: Pair 2

Figure 3.13: Pair 3
Figure 3.17: Pair 7

Figure 3.18: Pair 8

Figure 3.19: Pair 9
Figure 3.20: Pair 10
Figure 3.21: Stars, VIP Area, and Award in the Experiment
Appendix C: Figures and tables

Distribution of Effort Levels for Player 1 and Player 2

Table 3.10: Frequency of Advice (a) by Player 1

<table>
<thead>
<tr>
<th></th>
<th>Baseline</th>
<th>Random status</th>
<th>Earned status</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>–</td>
<td>High</td>
<td>Low</td>
</tr>
<tr>
<td>$a = 9$ of player 1</td>
<td>7</td>
<td>2</td>
<td>4</td>
</tr>
<tr>
<td>7</td>
<td>3</td>
<td>4</td>
<td>6</td>
</tr>
<tr>
<td>5</td>
<td>19</td>
<td>19</td>
<td>19</td>
</tr>
<tr>
<td>3</td>
<td>0</td>
<td>4</td>
<td>0</td>
</tr>
<tr>
<td>1</td>
<td>1</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>N</td>
<td>30</td>
<td>30</td>
<td>30</td>
</tr>
</tbody>
</table>

Notes: This table reports the frequency of effort choices by player 1 in different treatments. There are 3 less player 1 in Earned Status treatment.
Measures for Social Status: Different attitudes towards a higher status ("star")

Figure 3.22: “People with stars in my experiment session are more powerful”
Figure 3.23: “People with stars in my experiment session are cleverer”
Figure 3.24: “People with stars in my experiment session deserve to get more in team-production task”
Figure 3.25: “I prefer the star groups”
Figure 3.26: “The method to allocate stars reflects abilities”
Chapter 4

*Homo Reciprocans* Revisited

4.1 Introduction

Reciprocity matters in the workplace. Recent field experiments have shown that employers can increase employees’ productivity by giving gifts (monetary or non-monetary) to employees. Meanwhile, employees’ productivity might decrease if employers treat employees in an unkind way. The former tendency is often referred to as positive reciprocity and the latter as negative reciprocity.¹ Many laboratory experiments show similar tendencies among workers to reward “generous offers” with higher effort to their employer and decrease effort when their employer pays a wage that is perceived to be unfair (see Fehr and Gächter (2000) for a review).

While reciprocal behaviours are well-documented in the laboratory and with a few

¹Examples for such reciprocal reactions from employees are ample. For instance, receiving gifts from the employer increased the amount of data-entry by up to 30% in the university library (Kube et al. 2012), tree planters increased their daily productivity by 10% when they received a lump-sum of $80 for one day in addition to their regular wage (Bellemare and Shearer 2009), and bicycle messengers raised their work time by 33.5% after getting a 25% increase in the commission rate (Fehr and Goette 2007). In contrast, policemen decreased the arrest rates after experiencing an unexpected pay cut in New Jersey (Mas 2006), and one-time salesmen affected by a pay cut reduced their sales (Cohn et al. 2014b).
controlled field experiments, evidence remains scarce whether the same holds in non-experimental settings, with diverse occupations, different social contexts, and longer time spans. In fact, evidence is mixed: some field-experimental studies suggest that reciprocal responses to kind acts may be short-lived (Gneezy and List 2006).

This chapter follows Dohmen et al. (2009), who take a further empirical step by departing from the experimental settings, and studying reciprocity in a representative sample of the population. More precisely, I aim to discover how the effort a worker exerts relates to whether her wage is higher or lower than that of similar others; and in addition, how this relationship interacts with her inclinations to reciprocate. Moreover, I will investigate how job satisfaction relates to relative wage and reciprocal tendencies. Will employees with higher positive reciprocal inclinations be willing to offer more effort, if their wages are higher than the average? Does it hold that the more a worker earns than the average the more effort she will exert, compared to her peers who are equally positively reciprocal? How do these responses affect the satisfaction a worker feels with her job?

Throughout this chapter I assume that individuals compare their wages to others (Festinger 1954). This requires a reference level to be used for the comparison. To this end, I use the German Socio-Economic Panel (GSOEP) to construct a reference wage for each individual based on a set of characteristics. The reference wage represents the average wage a worker with the same characteristics in the economy earns. I subsequently assume that the larger the difference between a worker’s wage and the reference wage is, the more generously the employer is treating a worker. I also follow Dohmen et al. (2009) and construct measures of each individual’s inclinations to reciprocate others’ action, both for positive and negative reciprocal tendencies. For this purpose, I use individual responses to a set of attitudinal questions posed in GSOEP. Details are provided below.

Since this chapter builds on Dohmen et al. (2009) and extends their work, I shortly review their findings. Using the GSOEP dataset, Dohmen et al. (2009) investigate how a
worker’s reciprocal tendencies are linked to her working life, *e.g.*, overtime work, income, job satisfaction, and personal success. To study the relationship between reciprocal tendencies and effort, the authors focus on two groups of employees, using a question in the survey about the fairness perception of their wage.\(^2\) Depending on the fairness perception, they examine how effort is related to reciprocal tendencies in both groups. Their results show that if employees perceive their wage to be fair, there is a positive correlation between positive reciprocity and the likelihood of working overtime. The relation is insignificant, however, between negative reciprocity and effort in the group where the wage is perceived to be unfair. Therefore, relying on workers’ subjective perception about wage fairness, Dohmen et al. (2009) confirm – partially – the experimental findings that workers in the economy reciprocate fair wage offers by their employers.

In this chapter, I adopt two continuous variables in my empirical analysis which distinguishes my work from Dohmen et al. (2009).\(^3\) First, for the dependent variable of work effort, instead of a binary variable of doing overtime or not, I choose the number of hours of unpaid overtime. This variable has the advantage of containing richer information on the amount of effort exerted by the worker. Second, and more importantly, I adopt a (continuous) measure for how well workers are treated by their employers that takes into account the heterogeneity in the worker population. In particular, it is important to take into account that what is a fair wage to one worker might not be a fair wage to another. This measure is constructed by predicting a reference wage for workers with similar characteristics. The comparison between a worker’s wage and the reference allows me to shed light on how well an employee is treated by her employer compared to similar other workers in the economy. Consequently, by looking at otherwise similar workers with different reciprocal traits, we can not only observe how hard a worker works when she is

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\(^2\)The original question in English (translated from German) is “Is the income that you earn at your current job just, from your point of view?” The questionnaire questions are available online. See GSOEP website [www.diw.de/en/soep](http://www.diw.de/en/soep).

\(^3\)My work also distinguishes itself from Dohmen et al. (2009) by considering the effects of reciprocity and wages on job satisfaction.
treated well relative to her peers, but also investigate among equally well-treated workers how effort relates to reciprocal tendency.

My theoretical predictions about work effort are made based on reciprocity theory. A positively reciprocal worker responds to generous payments with more effort; moreover, the higher her general tendency to reciprocate is, the stronger this positive relationship will be. In contrast, a negatively reciprocal employee decreases her effort when she receives wage that is perceived to be unfair; likewise, the stronger her tendency towards negative reciprocity is, the stronger this negative relationship will be. The prediction that a positively reciprocal worker with generous wage exerts more effort is consistent with two theories. One is recorded in Levine (1998) where an altruistic employer signals her type by paying a high wage to the worker. Upon receiving the signal, the worker internalizes the employer’s utility into her utility function, and thus works harder to achieve a higher joint utility. Another theory is developed by Blau (1964) where if employees are reciprocal, a generous wage gives rise to feelings of obligations on the part of the employee, which can lead to her providing higher effort.

One could argue that, even in the absence of any reciprocal tendencies, a worker’s effort will be affected by other factors, such as relative wages or a threat of dismissal. To avoid being laid off, a rational and selfish worker will exert more effort when her relative wage increases, regardless of how reciprocal she is. To avoid shirking, the employer is paying a wage which is higher than the market clearing level. This is the efficiency wage hypothesis and was first formulated by Shapiro and Stiglitz (1984). I will argue in section 2 that, even with efficiency wages, we should still observe that reciprocal workers respond stronger to higher wages than nonreciprocal workers.

The predictions for job satisfaction are more complex than those for effort. If a worker has no reciprocal tendencies, she will experience an increase in her job satisfaction when the pay is generous. If workers do have such tendencies, however, the mechanisms may conflict, leading to an ambiguous prediction. In particular, a generous wage may go
hand in hand with a higher or lower job satisfaction for workers with positive reciprocal tendencies. In some cases, for example, a worker experiences an increase of satisfaction when her employer offers a generous wage. An alternative scenario follows from Blau (1964). Workers may perceive the generous condition as a mental burden, and suffer from the perceived obligation to do extra work for the high wage.

My empirical analysis looks at the role of reciprocal tendencies on work effort and job satisfaction conditional on a worker’s income relative to that of her peers. The results show that a worker exerts more effort if she compares well to her peers, and even more so if she is positively reciprocal. For example, for a worker with a high tendency - 6.88 out of 7 - towards positive reciprocity according to my measure, a 1 percent increase in her hourly wage (13 euro cents), goes with an increase of 1.8 minutes of monthly unpaid overtime. Compared to the baseline, this is a 0.45% increase. In contrast, my results show no comparable effects of negative reciprocity. More precisely, a worker with relatively low wage and strong negative reciprocity does not decrease her unpaid overtime hours than an otherwise similar worker without such reciprocal tendencies.

The analysis of job satisfaction shows that satisfaction is related to both reciprocal tendencies and relative wages. However, my data show no evidence that a more reciprocal worker is more satisfied with her job after a wage increase even when her wage is higher than her peers. This maybe due to the conflicting theories discussed above.

This chapter adds to the literature on reciprocity in analysing the role of reciprocal inclinations in the workplace. Instead of comparing the effort levels of reciprocal workers with perceived fair or unfair wages, I use a more objective measure of how ‘fair’ a worker’s wage is. This allows for a practical implementation of this chapter’s results. It suggests how employers can motivate reciprocal workers to increase productivity and effort.

The rest of the chapter is organised as follows. Section 2 sets out the theoretical predictions. Section 3 introduces the data and the empirical approach. Section 4 describes the main results, and Section 5 concludes.
4.2 Predictions

Without threat of dismissal, a rational and selfish worker will not exert more effort if her wage is higher than her peers. She derives utility directly from her wages and if the utility is affected by effort, the relationship will typically be negative. If there is a threat of dismissal, a rational and selfish worker may increase her effort when she faces higher wages. In comparison, however, workers with reciprocal tendencies will respond even stronger to relative wages.

A positively reciprocal worker works more if her wage is higher than her peers; in contrast, a negatively reciprocal worker will exert less effort if she earns less than her peers. This prediction views the employment relationship as a process of exchanging favours as in social exchange theory. After receiving generous wages from the employer, employees work harder so as to return the generosity of the employer (Blau 1964). An alternative theory that assumes conditional altruism leads to the same prediction. Here, the main idea is that people are more altruistic towards those who are altruistic towards them, so that they internalize the other people’s utility. As a consequence, employees work harder when they receive generous payments from an altruistic employer (Levine 1998).\(^4\)

Following the same theories, the prediction for job satisfaction is less clear. On the one hand, according to social exchange theory, in addition to the raised job satisfaction from more income, employees may experience a decrease in their job satisfaction if the wage is generous. This is because they feel an obligation to do extra work. Therefore in this theory, the overall effect of an increase in income on job satisfaction is ambiguous. For negatively reciprocal employees, predictions for satisfaction are not mentioned in social exchange theory. Nevertheless, recent studies in neuroscience show that taking revenge brings satisfaction and deservingness to the one who punishes an unfair act (Quervain 2006).

\(^4\)Though people might evaluate the kindness of intentions differently (Falk and Fischbacher 2006), it is generally assumed that paying a generous wage reflects good intentions in gift-exchange settings.
et al. 2004, Knutson 2004, Gollwitzer et al. 2011). So, aside from the reduced satisfaction due to a low wage, workers who are negatively reciprocal might also experience an increase in job satisfaction by taking revenge on the employer (by working fewer overtime hours). Therefore the overall effect on job satisfaction for a low wage is also unclear for negatively reciprocal employees.

On the other hand, employees may experience an additional increase in their satisfaction. An altruistic worker might experience an increase in her job satisfaction when her wage is generous as she has a positive regard for the employer; in contrast, a spiteful worker will experience a reduced utility when the wage is perceived as unfair and she puts a negative regard for her employer (Levine 1998). This preference could be justified with an adjusted utility function which reflects both the worker’s own utility and her regard for the employer such that the objective of individuals is to maximise their joint utility, incorporating the regards for their opponents.

To sum up in Table 4.1, with a higher than average wage, a positively reciprocal worker exerts more effort; and the more she earns than her peers the stronger the effect would be. With a lower wage, a negatively reciprocal worker will exert less effort; and the less she earns than her peers, the less effort we could expect from her. I do not form predictions for the interaction between positive reciprocal tendencies with lower relative wages, neither is there a prediction for the interaction between negative reciprocal tendencies with higher wages. This is because I assume that lower than average wages are not considered a ‘gift’ that is to be reciprocated, and, similarly, above-average wages are not considered a negative act that is to be punished. Finally, the prediction for job satisfaction derive from the interaction between social exchange theory and conditional altruism discussed above.
Table 4.1: Theoretical predictions

<table>
<thead>
<tr>
<th>Wage above reference</th>
<th>Positive reciprocity</th>
<th>Negative reciprocity</th>
</tr>
</thead>
<tbody>
<tr>
<td>E: +</td>
<td></td>
<td>N.A.</td>
</tr>
<tr>
<td>S: ? (social exchange)</td>
<td>+ otherwise</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Wage below reference</th>
<th>N.A.</th>
<th>E: -</th>
<th>S: ? (revenge brings joy), - otherwise</th>
</tr>
</thead>
</table>

Notes: “E” stands for effort and “S” stands for satisfaction. “N.A.” indicates ‘not applicable’. A “+” (“-”) indicates an increase (decrease) in the variable concerned. A “?” indicates that the effect is ambiguous.

4.3 Data and empirical approach

4.3.1 Data

I use data from the German Socio-Economic Panel in the year 2005, the same dataset as in Dohmen et al. (2009). This dataset has two advantages for the study. It is a national survey and contains information on employment, health, and education from over 20,000 individuals who have been living in Germany since 1984.\(^5\) Also, in 2005 for the first time, respondents were asked about reciprocity. I use the same measure of reciprocal tendencies as in Dohmen et al. (2009), which is derived from six attitudinal questions in the survey.\(^6\) Specifically, from the list of statements below, the first three hypothetical statements are related to positive reciprocity, and the other three pertain to negative reciprocity. Respondents need to indicate their attitude on a scale of 1 to 7, ranging from “does not apply to me at all” to “applies to me perfectly”. I follow Dohmen et al. (2009) and use the average scores for questions 1), 2) and 3) as a measure of the strength of positive reciprocity. Likewise, the average score for question 4), 5) and 6) provide the

\(^5\)For more information about the questionnaire, see the GSOEP website www.diw.de/en/soep.

\(^6\)In the 2010 wave, the questions on reciprocal tendencies were asked for the second time. The principal component analysis in Dohmen et al. (2009) shows that positive reciprocity and negative reciprocity are distinct traits for individuals. Positive reciprocity and negative reciprocity have a weak correlation of 0.025. Such reciprocity measures have been used to study incentive contracts (Dur et al. 2010), trust (Dohmen et al. 2008), and job motivation (Montizaan et al. 2015).
measure for negative reciprocity. The questions are:

1) If someone does me a favour, I am prepared to return it.
2) I go out of my way to help somebody who has been kind to me before.
3) I am ready to undergo personal costs to help somebody who helped me before.
4) If I suffer a serious wrong, I will take revenge as soon as possible, no matter what the cost.
5) If somebody puts me in a difficult position, I will do the same to him/her.
6) If somebody offends me, I will offend him/her back.\(^7\)

There are approximately 20,000 respondents in the 2005 wave of the GSOEP survey. Table 4.2 describes the main variables of relevance for this study. On average, the respondents are 39 years old with 17 years of working experience, and 12 years of education. 37\% are part-time employees, 52\% are female. My analysis is restricted to employed (either full-time or part-time) respondents, excluding the self-employed, those in compulsory military or community services. This leaves me with 10,055 respondents. Respondents earn on average a (gross) hourly wage of 12.81 euro.

\(^7\)Those questions were translated from German, for original German questions please see www.diw.de/en/soep.
Table 4.2: Descriptive statistics

<table>
<thead>
<tr>
<th>Variable name</th>
<th>Variable description</th>
<th>Mean</th>
<th>Std.</th>
<th>#Obs.</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Dependent variables</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unpaid overtime hours</td>
<td>Hours of unpaid overtime last month</td>
<td>6.69</td>
<td>13.21</td>
<td>9892</td>
</tr>
<tr>
<td>Job satisfaction</td>
<td>On a scale from 0 to 10</td>
<td>6.85</td>
<td>2.22</td>
<td>12189</td>
</tr>
<tr>
<td><strong>Independent variables</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Positive reciprocity</td>
<td>Average score on statements (from 1 to 7)</td>
<td>5.88</td>
<td>0.91</td>
<td>20774</td>
</tr>
<tr>
<td>Negative reciprocity</td>
<td>Average score on statements (from 1 to 7)</td>
<td>3.11</td>
<td>1.46</td>
<td>20774</td>
</tr>
<tr>
<td>Log of hourly gross wage</td>
<td>Natural logarithm of gross hourly wage</td>
<td>2.55</td>
<td>0.69</td>
<td>10055</td>
</tr>
<tr>
<td>Hourly wage - reference</td>
<td>Reference predicted by the income regression (conditional on earning more than the reference wage, and 0 otherwise)</td>
<td>0.29</td>
<td>0.28</td>
<td>4390</td>
</tr>
</tbody>
</table>
| Reference - hourly wage              | Difference between the reference predicted by the income regression and the actual wage (conditional on earning less than the reference wage, and 0 otherwise) | 0.31
| Part-time                            | Dummy: 1 if one works part-time                          | 0.37   | 0.48   | 12209 |
| Public                               | Dummy: 1 if one works in public sector                  | 0.26   | 0.26   | 10910 |
| Female                               | Dummy: 1 if female                                      | 0.52   | 0.50   | 20774 |
| Age                                  | Age in years                                            | 39.89  | 21.76  | 26774 |
| Working experience full-time         | Years of full-time work                                  | 16.86  | 14.06  | 20750 |
| Working experience part-time         | Years of part-time work                                 | 2.51   | 5.49   | 20750 |
| Tenure                               | Years of working at current place                        | 10.63  | 9.97   | 11624 |
| Education (in years)                 | Years of education                                      | 12.12  | 2.69   | 19475 |
| General education                    | Dummy for general school degree                         | 0.22   | 0.41   | 20774 |
| University education                 | Dummy for at least a university degree                   | 0.20   | 0.40   | 20774 |
| Vocational education                 | Dummy for a vocational education degree                 | 0.63   | 0.48   | 20774 |

Notes: nb: The number is defined to be positive in the regression analysis.
Other independent variables include the following dummy variables: marital status (5 statuses in total, including single, married with/without kids, divorced, and widowed), firm size (5), industry (9), occupation (3), and German states (16). The actual average hourly wage in this sample is $e^{2.55} = 12.81$ euro. The gross average hourly wage in Germany in 2005 is 16.98 euro, covering all kinds of jobs throughout the country (own calculation, data source: OECD). The average of a higher hourly wage is $e^{0.29} \approx 1.34$ times more than the average wage in the relevant wage group. For those who earn a lower wage, on average, their hourly wage is $e^{-0.31} \approx 0.73$ of the reference wage.

The key dependent variables in the analysis are: Effort ($E_i$) which is measured as hours of unpaid overtime work, and job satisfaction ($S_i$) which is the answer to the question on a scale from 1 to 10. The average score of positive reciprocity is 5.88 on a scale of 1 to 7 and the average score of negative reciprocity is 3.11 on the same scale.\(^8\)

\(^8\)The measures for reciprocal inclinations are also stable over time. The mean and standard deviation of positive reciprocity measure in 2010 wave (number of observations: 13,418) is 5.81 (0.91), with a correlation of 0.366 (Pearson correlation coefficient, $p < 0.01$) with the measure in 2005. Likewise, the mean and standard deviation of negative reciprocity measure is 3.03 (1.41), with a correlation of 0.48 ($p < 0.01$) with that measure in 2005.
The distributions are shown in Figure 4.1, see also Figure 2 in Dohmen et al. (2009). Figure 4.1 depicts the marginal effects of relative wage changes on effort, which will be discussed in detail later. The shaded area gives the distribution of reciprocity in the population; the left panel shows the distribution of positive reciprocity and the right panel shows the distribution of negative reciprocity. Approximately half of the respondents earn more than the average wage, as will be explained in detail in Section 3.2. Table 4.3 reports the Pearson correlations between important explanatory variables in the analysis. Older individuals, people with higher education, and high earners exhibit higher positive reciprocity. Low earners, males, younger people, less educated respondents, full-time workers, and private sector workers report a higher score on negative reciprocity.9

---

9Some of these correlations are highly consistent with Falk et al. (2015) in which middle-aged people are more positively reciprocal and women are self-reported to be less negatively reciprocal. They derive the correlations from the Global Preference Survey with 80,000 individuals from 76 countries, covering 90 percent of both the world’s population and global income.
Figure 4.1: Marginal effect of wage on effort
Table 4.3: Correlation table of important explanatory variables

<table>
<thead>
<tr>
<th></th>
<th>Positive reciprocity</th>
<th>Negative reciprocity</th>
<th>Wage</th>
<th>Female</th>
<th>Age</th>
<th>Education</th>
<th>Part-time</th>
<th>Public</th>
</tr>
</thead>
<tbody>
<tr>
<td>Positive reciprocity</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Negative reciprocity</td>
<td>0.025***</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log of hourly wage</td>
<td>0.041***</td>
<td>-0.060***</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>-0.002</td>
<td>-0.129***</td>
<td>-0.187***</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age (in years)</td>
<td>0.047***</td>
<td>-0.138***</td>
<td>0.361***</td>
<td>0.013*</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education (in years)</td>
<td>0.036***</td>
<td>-0.085***</td>
<td>0.373***</td>
<td>-0.074***</td>
<td>-0.081***</td>
<td>1.000</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Part-time</td>
<td>-0.007</td>
<td>-0.033***</td>
<td>-0.254***</td>
<td>0.382***</td>
<td>-0.072***</td>
<td>-0.098***</td>
<td>1.000</td>
<td></td>
</tr>
<tr>
<td>Public sector</td>
<td>0.004</td>
<td>-0.073***</td>
<td>0.151***</td>
<td>0.126***</td>
<td>0.101***</td>
<td>0.192***</td>
<td>0.016</td>
<td>1.000</td>
</tr>
</tbody>
</table>

Notes: Female, part-time and public sector are dummy variables. Other variables are continuous. Pearson coefficients for the correlations are reported. ***, * indicate significance at 1%, and 10% level, respectively.
I start the analysis by developing a reference wage for each employee based on observable characteristics. I follow the literature that constructs reference wages to compare subjective well-being among similar people (for a review, see Dahlin et al. (2014)). The variables in my reference wage regression are age (McBride 2001), region (Ferrer-i Carbonell 2005), education (Clark et al. 2010), public or private sector, full-time or part-time (Stadt et al. 1985), gender, industry, occupation (Clark and Oswald 1996), marital status (Dohmen et al. 2009); I add higher order terms of age, education, and tenure to account for nonlinear effects.\footnote{Firm size is not included in the income regression because of the missing values. However, I control for firm sizes in the main analysis about effort and job satisfaction.} From the residuals of the income regression, I obtain a measure of how much a worker is earning more (or less) than a worker with similar characteristics in the labor market. The income regression has a typical Mincer-income equation form:

\[
\ln(y_i) = \ln(y^*_i) + \epsilon_i,
\]

\[
\ln(y^*_i) = \delta D'(gender_i, age_i, education_i, etc.).
\] (4.1)

The dependent variable income \(y_i\) in the reference wage regression is the natural log of gross hourly income. The fitted value from the regression \(y^*_i\) is thus the income a typical employee would receive. Residuals from this regression take the form of the log of the income ratio: \(\ln(y_i/y^*_i)\). Therefore, a worker with a positive residual earns more than the reference wage, and one with a negative residual has a lower wage than the reference wage.\footnote{Residual \(\epsilon_i = \ln(y_i) - \ln(y^*_i) = \ln(y_i/y^*_i)\). Perfect prediction leads to \(\epsilon_i = 0\) when \(\frac{y_i}{y^*_i} = 1\) so that \(\ln(y_i/y^*_i) = 0\).}

Table 4.4 reports the regression results of the wage regression. Consistent with previous findings (such as Bonin et al. (2007); Table 1 in Dohmen et al. (2009)) on labor income in Germany, more education is related to higher income. In contrast, being female and working part-time are related to a lower hourly wage. Age and tenure both have a concave
effect on the hourly wage.

Table 4.4: Reference wage regression

<table>
<thead>
<tr>
<th>Dependent variable: Log of hourly wage</th>
<th>OLS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Female</td>
<td>-0.205***</td>
</tr>
<tr>
<td>Education (in years)</td>
<td>0.271**</td>
</tr>
<tr>
<td>Education^2/100</td>
<td>-1.852*</td>
</tr>
<tr>
<td>Education^3/1000</td>
<td>0.508**</td>
</tr>
<tr>
<td>Job tenure (in years)</td>
<td>0.042***</td>
</tr>
<tr>
<td>Job tenure^2/100</td>
<td>-0.147***</td>
</tr>
<tr>
<td>Job tenure^3/1000</td>
<td>0.018***</td>
</tr>
<tr>
<td>Age (in years)</td>
<td>0.079***</td>
</tr>
<tr>
<td>Age^2/100</td>
<td>-0.138***</td>
</tr>
<tr>
<td>Age^3/1000</td>
<td>0.007**</td>
</tr>
<tr>
<td>Part-time</td>
<td>-0.066***</td>
</tr>
<tr>
<td>Public sector</td>
<td>0.042***</td>
</tr>
<tr>
<td>Other controls</td>
<td>3</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.304</td>
</tr>
<tr>
<td>R^2</td>
<td>0.447</td>
</tr>
<tr>
<td>N</td>
<td>8508</td>
</tr>
</tbody>
</table>

*Note: Robust standard errors allow for clusters at the household level. Other control variables include occupation (3), industry (9), and province (16). The dependent variable is the log of hourly wage. ***, **, * indicate significance at 1%, 5%, and 10% level, respectively.
4.3.2 Empirical strategy

In order to investigate how a worker’s effort and job satisfaction are related to her reciprocal tendencies, and how this relationship depends on her relative earnings compared to similar others in the economy, I specify the following econometric model with effort ($E_i$ hours of unpaid overtime) and job satisfaction ($S_i$) as dependent variables. To ease interpretations, all variables in interactions are non-negative and measures for reciprocal tendencies are centred at their means.

I focus on the interaction terms of reciprocal tendencies and relative wages in the regression; specifically, the interaction between the measures for positive reciprocity ($P_i$) and higher relative wage ($W_{i+}$), and the interaction between the measures for negative reciprocity ($N_i$) and lower relative wage ($W_{i-}$). $W_{i+}$ and $W_{i-}$ are the residual terms derived from the income regression in model (4.1), i.e., $W_{i+}$ captures the extra wage that could not be explained by observables and $W_{i-}$ does the same for observed wage below the reference. I write the model specification in equation (4.2) for effort and equation (4.3) for satisfaction. The coefficients for the interaction terms capture my hypothesis. I control for various characteristics $D_i$ of the employees, including age, gender, years of education, sector, part-time or full-time job, years of work experience, firm size, occupation, industry, marital status, province of residence, higher order terms of age, education, and work experience. $\varepsilon_i$ captures unobserved characteristics for overtime work and job satisfaction, and are assumed i.i.d. error terms. Note that by construction, $W_{i+}$ is zero for wages below the reference wage. For these cases, $\beta_1$ captures the effects of positive reciprocity. I predict no effects for this case. The same holds, mutatis mutandis, for $\beta_2$. To account for nonlinearity of reciprocity, I include the squared terms of reciprocity, as well as their interaction with reciprocity and relative earnings in equation (4.2). I expect a positive sign for $\sigma_1$ and a negative sign for $\sigma_2$ in equation (4.2).
\[ E_i = \sigma_1(P_i \times W_{i+}) + \sigma_2(N_i \times W_{i-}) + \sigma_3(P^2_i \times W_{i+}) + \sigma_4(N^2_i \times W_{i-}) \\
+ \beta_1 P_i + \beta_2 N_i + \beta_3 P^2_i + \beta_4 N^2_i + \theta_1 W_{i+} + \theta_2 W_{i-} + D'_i\delta + \varepsilon_i. \tag{4.2} \]

The regression for satisfaction has a similar structure. The dependent variable is self-reported job satisfaction. Respondents rank their answers on an interval from 0 to 10. This is regressed on reciprocity, relative income, and their interactions. I also control for various characteristics \( D_t \) of the employees. \( \zeta_i \) accounts for the discrepancy between actual observed overtime work and job satisfaction and estimated values. I include the squared terms of reciprocity, and the interactions in equation (4.3) to control for nonlinearity of reciprocity measures. The model is specified as follows:

\[ S_i = \eta_1(P_i \times W_{i+}) + \eta_2(N_i \times W_{i-}) + \eta_3(P^2_i \times W_{i+}) + \eta_4(N^2_i \times W_{i-}) \\
+ \gamma_1 P_i + \gamma_2 N_i + \gamma_3 P^2_i + \gamma_4 N^2_i + \kappa_1 W_{i+} + \kappa_2 W_{i-} + D'_i\delta + \zeta_i. \tag{4.3} \]

The signs of \( \eta_1 \) and \( \eta_2 \) in equation (4.3) do not follow straightforwardly from theory. If employees are conditionally altruistic, I expect a positive \( \eta_1 \) and a negative \( \eta_2 \). If employees are behaving more in line with the social exchange theory, we would expect that \( \eta_1 < 0 \) and \( \eta_2 > 0 \).

### 4.4 Results

#### 4.4.1 Reciprocity and effort

Table 4.5 reports the results on how effort is associated with employees’ relative income, depending on reciprocity. Key variables of interests are the interactions between reciprocity and relative income. OLS coefficients are reported.
In general, the results show that among positively reciprocal employees, higher wages are related to more unpaid overtime hours. However, there is no evidence for a significant correlation between low wage and effort among negatively reciprocal workers.

We start with column (1) in Table 4.5, with only relative wages and reciprocity and their interaction terms as the explanatory variables. The coefficient for the interaction between positive reciprocity and relative wages is positive and statistically significant, showing that more unpaid work hours are related to higher earners with above-average positive reciprocal tendencies. In contrast, the interaction between negative reciprocity and relative wages is insignificantly correlated with effort. Because of the demeaning of the reciprocity variables and the quadratic terms used, it is hard to directly see the marginal effects of reciprocity on overtime from the coefficients in Table 4.5. I will discuss these effects in detail using Figure 4.1. An average worker scores 5.88 on positive reciprocity, 3.11 for negative reciprocity; she earns 12.81 euro per hour and works 6.69 hours of overtime without payments; for more statistics see Table 4.2. Since in the regression reciprocity variables are centred at their means, a worker with 1 point of positive reciprocity actually has 6.88 points (5.88 + 1), which is close to the maximum on a scale of 7. For such highly reciprocal workers, a 1% unit increase in hourly wage – 13 euro cents – results in an increase of 2.04 minutes (0.034 hours) of unpaid overtime, an increase of 0.51% (= \( \frac{0.034}{6.88} \)) over the average of 6.69 hours per month, compared to an equally highly reciprocal worker with similar characteristics.\(^{12}\) I also look at the interactions between positive reciprocity measures and lower earnings, that is, ‘Positive reciprocity \( \times \) (Reference wage - Hourly wage)’; and the interaction between negative reciprocity measures and higher earnings into the regression, that is, ‘Negative reciprocity \( \times \) (Hourly wage - Reference wage)’. The coefficients for these two interaction terms are insignificant, showing

\(^{12}\)Recall that in the Mincer-form income regression, both the actual hourly wage \( y_i \) and the reference wage \( y^* \) take the natural logarithms. Therefore a 1% increase in the relative wage means that \( y_i = e^{0.01} \approx 1 + 0.01 \) \( y^* \). Therefore, \( \lim_{x \to 0} x \approx 1 + x \) with Taylor expansion. Since the average reference wage is 12.81 in Table 4.2, an employee needs to earn an hourly wage of 12.93 (1.01 × 12.81) so as to increase working hours by 0.034.
that low earners do not exert more effort even if they are positively reciprocal, nor do high earners make less effort when they are negatively reciprocal. These results correspond to the ‘N.A.’s in Table 4.1. More detailed results of this robustness check are available upon request.

In column (2) of Table 4.5, I control for personal characteristics such as gender, age, education, and work related variables. The relationships persist in column (2) and the main interaction effects are very similar. Finally, I add other control variables such as firm size, occupation, industry, and region in column (3); in addition, to account for a possible nonlinear effect of reciprocity, I include squared terms of reciprocity into the regression. The main effects are very comparable to the other columns. Specifically, a 1% increase (13 cents) of the hourly income goes with an increase in the unpaid overtime hours by 1.8 minutes (0.03 hours) every month, which is a 0.45% increase from the average hours of unpaid overtime.

My findings are partially consistent with Dohmen et al. (2009) who regress the likelihood of doing overtime on measures of reciprocity. They do the same regression separately for two groups, depending on their perceptions of fair wages. Their results show that reciprocity seems to only matter when the wage is perceived to be fair: with 1 unit increase in positive reciprocity among workers who think they are receiving a fair wage, on a scale of 7, a worker with positive reciprocity is 3.8% more willing to work overtime and a worker with negative reciprocity is 1.2% less willing to do so. However, among workers who perceive the wage to be unfair, the authors find no significant relations between reciprocity and effort.

I plot the marginal effect of having one percent increase in income on the number of hours of unpaid overtime on the left panel of Figure 4.1. The marginal effects are derived from the regression coefficients from column (4) in Table 4.5 with a 95% confidence interval in dashed lines. In particular, this marginal effect is plotted with the distribution
Table 4.5: Effort and Reciprocity

<table>
<thead>
<tr>
<th></th>
<th>Y=Hours of Unpaid Overtime Work</th>
<th>OLS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Positive reciprocity × (Hourly wage - Reference wage)</td>
<td>3.373*** (1.059)</td>
<td>2.829*** (0.998)</td>
</tr>
<tr>
<td>Negative reciprocity × (Reference wage - Hourly wage)</td>
<td>-0.075 (0.471)</td>
<td>-0.092 (0.450)</td>
</tr>
<tr>
<td>Positive reciprocity</td>
<td>0.110 (0.214)</td>
<td>0.154 (0.206)</td>
</tr>
<tr>
<td>Negative reciprocity</td>
<td>-0.232* (0.125)</td>
<td>-0.256** (0.124)</td>
</tr>
<tr>
<td>Hourly wage - Reference wage</td>
<td>-0.190 (0.928)</td>
<td>1.525* (0.901)</td>
</tr>
<tr>
<td>(when hourly wage is above the reference)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reference wage - Hourly wage</td>
<td>1.930** (0.892)</td>
<td>2.902*** (0.867)</td>
</tr>
<tr>
<td>(when hourly wage is below the reference)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>-2.050*** (0.370)</td>
<td>-2.934*** (0.407)</td>
</tr>
<tr>
<td>Age (in years)</td>
<td>-0.738 (0.549)</td>
<td>-0.172 (0.653)</td>
</tr>
<tr>
<td>Age²/100</td>
<td>2.148 (1.337)</td>
<td>0.935 (1.597)</td>
</tr>
<tr>
<td>Public sector</td>
<td>-1.380*** (0.361)</td>
<td>-1.785*** (0.503)</td>
</tr>
<tr>
<td>Part-time</td>
<td>-4.550*** (0.411)</td>
<td>-3.701*** (0.430)</td>
</tr>
<tr>
<td>Education (in years)</td>
<td>2.485 (3.088)</td>
<td>8.184** (3.636)</td>
</tr>
<tr>
<td>Education²/100</td>
<td>-10.200 (24.820)</td>
<td>-61.950** (28.270)</td>
</tr>
<tr>
<td>Squared term of reciprocity</td>
<td>7 7 3</td>
<td></td>
</tr>
<tr>
<td>Other controls</td>
<td>7 7 3</td>
<td></td>
</tr>
<tr>
<td>Work experience controls</td>
<td>7 3 3</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>7.131*** (0.234)</td>
<td>-2.385 (14.440)</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.004 (0.083)</td>
<td>0.130 (0.130)</td>
</tr>
<tr>
<td>Observations</td>
<td>7451 7446</td>
<td>6635</td>
</tr>
</tbody>
</table>

Notes: Robust standard errors in brackets allow for cluster at the household level. Reciprocity variables are demeaned from the average. Work experience control variables include years of full-time work experience, years of part-time work experience, years of tenure, and squared terms of these variables. Cubed terms of age, years of education, tenure, and work experience are also included. Other control variables are: types of education (3), firm size (5), occupation (3), industry (9), marital status (5), and province (16). ***, **, * indicate significance at 1%, 5%, and 10% level, respectively.
of positive reciprocity in the population.\textsuperscript{13} The graph clearly indicates that the marginal effect of higher wage on effort changes with the extent of positive-reciprocal tendencies: when positive reciprocity increases especially above the average of 5.88, the marginal effect on effort becomes significantly positive.

The right panel of Figure 4.1 shows for those with a below-reference income the marginal effects of reduced income by one percent. The marginal effect of lower wage on effort is plotted with the distribution of negative reciprocity in the population. Although we observe a downward-sloping marginal effect as negative reciprocity gets stronger, the graph does not give the predicted significant negative effect. On the contrary, it shows that when negative reciprocity is below approximately 5.0, the marginal effect of lower wage on effort is positive.\textsuperscript{14} When negative reciprocity becomes stronger, the marginal effect becomes negative with wide confidence intervals. However, because of lack of data on employees with strong negative reciprocity, the marginal effect of having lower wages among strongly negatively reciprocal workers on effort seems insignificant.

### 4.4.2 Reciprocity and satisfaction

Table 4.6 reports the results for job satisfaction. Coefficients from an interval regression are reported. The results show that job satisfaction is insensitive to the interactions between relative wage and reciprocity. The insensitivity is reflected by insignificant coefficients for the interaction terms. In column (2) and (3), I also control for a series of personal characteristics and higher order terms of reciprocity tendencies. The insensitivity of interaction effects stays throughout the various versions of the model. The signs

\textsuperscript{13}The distribution of positive reciprocity measures among the workers whose wage is above the reference is very similar to the distribution of the full sample. The same is to the distribution of negative reciprocity measures. See Figure 4.2 in Appendix.

\textsuperscript{14}This is because the marginal effect of lower wage has two parts: the interaction term “Negative reciprocity × (Reference wage - Hourly wage)” and the relative wage term “Reference wage - Hourly wage”. In Table 4.5, the former has a negative yet insignificant coefficient of -0.211; and the latter is 3.573 for employees with the average score of negative reciprocity. In total, the sum gives a positive number because negative reciprocity is demeaned from the average so that “Negative reciprocity × (Reference wage - Hourly wage) = 0”. 
of the interaction terms make it difficult to pin down which theory (social exchange theory or conditional altruism) dominates.

The insignificance of the interaction between reciprocity measures and relative wages is, however, supported by the theories mentioned earlier. Because two forces (conditional altruism and social exchange) are working against each other here, it is difficult to pin down the direction of the total effect. The positive sign for “Positive reciprocity × (Hourly wage - Reference wage)” throughout all columns in Table 4.6 suggests that employees with high wages are more conditionally altruistic than being engaged in a social exchange process.

Nevertheless, I find that positive reciprocity is related to higher job satisfaction and negative reciprocity is linked to lower satisfaction. These relations are robust after controlling for various characteristics and with different model specifications.

They correspond to what Dohmen et al. (2009) report about the overall success of *Homo Reciprocans*: for example, positively reciprocal employees have more friends and they enjoy a higher overall life satisfaction; while negative reciprocity is associated with fewer friends and lower life satisfaction.
Table 4.6: Job Satisfaction and Reciprocity: Interval Regression

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Positive reciprocity × (Hourly wage - Reference wage)</td>
<td>0.025</td>
<td>0.030</td>
<td>0.087</td>
</tr>
<tr>
<td></td>
<td>(0.105)</td>
<td>(0.106)</td>
<td>(0.137)</td>
</tr>
<tr>
<td>Negative reciprocity × (Reference wage - Hourly wage)</td>
<td>-0.028</td>
<td>-0.021</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.068)</td>
<td>(0.068)</td>
<td>(0.079)</td>
</tr>
<tr>
<td>Positive reciprocity</td>
<td>0.155***</td>
<td>0.155***</td>
<td>0.172***</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.031)</td>
<td>(0.039)</td>
</tr>
<tr>
<td>Negative reciprocity</td>
<td>-0.129***</td>
<td>-0.128***</td>
<td>-0.144***</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.019)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>Hourly wage - Reference wage</td>
<td>0.507***</td>
<td>0.530***</td>
<td>0.597***</td>
</tr>
<tr>
<td></td>
<td>(0.093)</td>
<td>(0.094)</td>
<td>(0.132)</td>
</tr>
<tr>
<td>Reference wage - Hourly wage</td>
<td>-0.293***</td>
<td>-0.287***</td>
<td>-0.619***</td>
</tr>
<tr>
<td></td>
<td>(0.097)</td>
<td>(0.098)</td>
<td>(0.172)</td>
</tr>
<tr>
<td>Female</td>
<td>-0.087*</td>
<td>-0.173***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.049)</td>
<td>(0.055)</td>
<td></td>
</tr>
<tr>
<td>Age (in years)</td>
<td>-0.098***</td>
<td>-0.120***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.024)</td>
<td></td>
</tr>
<tr>
<td>Age^2/100</td>
<td>0.105***</td>
<td>0.122***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.026)</td>
<td></td>
</tr>
<tr>
<td>Public sector</td>
<td>0.158***</td>
<td>0.147**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.051)</td>
<td>(0.071)</td>
<td></td>
</tr>
<tr>
<td>Part-time</td>
<td>-0.114*</td>
<td>-0.050</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.063)</td>
<td>(0.071)</td>
<td></td>
</tr>
<tr>
<td>Education (in years)</td>
<td>-0.031</td>
<td>-0.144</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.086)</td>
<td>(0.121)</td>
<td></td>
</tr>
<tr>
<td>Education^2/100</td>
<td>0.242</td>
<td>0.650</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.307)</td>
<td>(0.432)</td>
<td></td>
</tr>
<tr>
<td>Squared term of reciprocity</td>
<td>7</td>
<td>7</td>
<td>3</td>
</tr>
<tr>
<td>Other controls</td>
<td>7</td>
<td>7</td>
<td>3</td>
</tr>
<tr>
<td>Work experience controls</td>
<td>7</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>Constant</td>
<td>0.664***</td>
<td>0.658***</td>
<td>0.641***</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td>-17510.667</td>
<td>-17452.112</td>
<td>-14524.102</td>
</tr>
<tr>
<td>Observations</td>
<td>8408</td>
<td>8402</td>
<td>7051</td>
</tr>
</tbody>
</table>

Notes: I choose an interval regression to account for the fact that the dependent variable is both left and right censored. Robust standard errors in brackets allow for clustering at the household level. The dependent variables are the answer to questions on job satisfaction on a scale from 0 to 10, where 0 means “completely dissatisfied” and 10 means “completely satisfied”. Interval regression is performed to account for the fact that the dependent variable is both left- and right- censored. Reciprocity variables are demeaned from the average. Work experience control variables include years of full-time work experience, years of part-time work experience, years of tenure, and squared terms of these variables. Cubed terms of age, years of education, tenure, and work experience are also included. Other control variables are: types of education (3), firm size (5), occupation (3), industry (9), marital status (5), and province (16). ***, **, * indicate significance at 1%, 5%, and 10% level, respectively.
4.5 Concluding remarks

Employees’ tendency to reward and retaliate can deeply shape the working environment and the employee-employer relationship. This chapter provides quantitative evidence on the economic relevance of reciprocal tendencies, in interaction with relative wages, for work effort and job satisfaction. By comparing the wage to a reference for each employee in the economy, I find an asymmetric relation between reciprocal tendencies and effort. This not only shows the importance of reciprocal inclinations in the work place, but also how the impact differs among similar employees with different earnings. My results may be of interest to researchers in organisational psychology or management to better understand personality traits, and their consequences for labor market outcomes. Human resource policy makers might also benefit. The fact that only some employees with high earnings respond with more effort to higher wages implies that future incentive policies, i.e., performance pay, might be more effective if they target more accurately employees with specific personality traits.

Future research on reciprocity calls for causal evidence. Throughout this chapter I assume that employees know not only their own wage but also know how much workers in Germany with similar characteristics earn. This may seem a strong assumption e.g., because wage information is not always transparent, sometimes ‘no disclosure contracts’ even prohibit making one’s wage known (Card et al. 2012). Nevertheless, the results that I find do suggest that the reference wages that I estimate matter. Of course, in additional research, one could strengthen this by providing explicit information about the relevant reference wage to some workers and subsequently comparing their effort and job satisfaction to a group without this explicit information. One could then also distinguish between long-run and short-run effects of such information provision. Such experiments on social comparison could be informative for the development of wage disclosure policies.
Appended
Chapter 5

Are Workers More Satisfied if Their Wage Compares Well to That of Their Peers in the Economy? A Survey Experiment

5.1 Introduction

Pay disclosure policies, either to the public or to employees, have been implemented in several countries. For instance, in California, the salary information and pension records of more than 300,000 public employees, including university faculty and staff members, have been available online since 2008 after the implementation of the “Right to know”

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This chapter is based on the work of Zheng et al. (2017b). We registered this work on the AEA RCT Registry website. To see our document, please follow the registry link https://www.socialscienceregistry.org/trials/289. IT support from the Wage Indicator website, especially Brian Fabo and Tomas Kahina is well recogised.
law.\textsuperscript{1} Another case is Norway where all Norwegians’ taxable incomes have been available online to the public since 2001.

Despite such actions to reveal wage information, no consensus has been reached about whether this is desirable. Arguments for and against such policies have been made in economics and other social sciences. On the one hand, some studies reason that disclosure of wage information increases accountability, especially in the public sector where wage transparency is associated with less corruption and better government (Djankov et al. 2010).\textsuperscript{2} On the other hand, public disclosure of wages enables wage comparison and relative deprivation theory (Runciman 1966) in sociology argues that a worker’s well-being may be negatively affected by wage comparison with her colleagues, especially when employees earn less than others. These workers may get demotivated which could negatively affect their performance. Mas (2017) finds that after a disclosure policy of municipal salaries in California, there is a substantial increase in the quit rate and a reduction of compensation of top managers. On the other hand, employees who observe that they earn more than comparable co-workers may be stimulated to work harder. Under such conditions, employers may have a strong incentive not to reveal wage information if the welfare loss from the behavioural responses by low-earning employees outweighs the possible welfare gain from the behavioural responses by high-earning employees.

Recent experimental studies have shed some light on the effects of income comparison on subjective well-being and attitude and performance at work. Some experiments find that disclosing salary information causes higher quit rates and lower job satisfaction.

Card et al. (2012) study information provision about peer wages to university employees. They find heterogeneous treatment effects with respect to whether or not an employee earns more or less than the median. They use an experiment across three campuses at

\textsuperscript{1}News link: \textit{Los Angeles Times}, California public records. \url{http://documents.latimes.com/sunshine/salaries/}. There is also an online survey engine that allows anyone to search California public employee pensions or salary information. Transparent California. \url{http://transparentcalifornia.com/}

\textsuperscript{2}“Sunshine or colonoscopy?” in \textit{The Economist} article argues that transparency enhances accountability in local government. \url{http://www.economist.com/node/21538774}
the University of California and inform a random group of UC staff and faculty members about a website that allows them to check their co-workers’ salaries. They find that while employees who earn more than the median are not significantly affected, employees whose earnings are below median express lower job satisfaction and higher intention to quit than comparable workers who have not been pointed to the website. Moreover, by linking the experiment to administrative data, it is shown that their high intention to resign from the job is followed up by a high actual turnover rate in the next half year. Rege and Solli (2014) obtain the same findings using a large Norwegian longitudinal dataset. In the Fall of 2001, all Norwegians’ taxable income was made available online. Rege and Solli (2014) investigate how the information ‘shock’ affects workers and they find that it increases the job turnover rate of low earning workers.

Not only resignation behaviour and job satisfaction can be affected by having access to peers’ wages, productivity may be as well. In a sample of one-time salespersons in Germany, Cohn et al. (2014a) show that a wage cut decreases effort of a worker and even more so when the salesperson’s peer does not experience the cut. The results show that, on average, effort decreases by 15% if both wages are reduced, while it decreases by more than 30% if only one person’s wage is decreased. Moreover, in their study, the effect on pay satisfaction is consistent with that on effort: compared to the general wage-cut treatment where both workers’ pay is cut, workers show lower pay satisfaction if only one worker experiences a wage cut; there is an insignificant decrease in pay satisfaction between the general wage-cut treatment and the treatment without wage cut.

Besides the influence on employees’ performance in the workplace, having access to information about relative income might change their political attitudes as well. A growing body of experimental research shows that knowing one’s own rank, or place in the income distribution affects one’s preference for income redistribution policies. Cruces et al. (2013) investigate how the perception of income distribution affects the redistribution attitude with a face-to-face survey experiment in Argentina. The authors randomise half of the
subjects into a treatment group where the ‘real’ distribution information is revealed, while
the other half does not receive this information. They find that once the real income dis-
tribution information is revealed, people have different demand for income redistribution
policies depending on their original belief about their income position relative to the ac-
tual distribution. Specifically, those who overestimate their income demand more income
redistribution while others do not. Kuziemko et al. (2014) use a laboratory experiment to
show that sometimes people with low income rank might oppose rather than support an
income redistribution policy. Specifically, the authors show that individuals have a “last-
place aversion”: if people have a full ranking based on their income, the second-to-last
ranked person has a preference for keeping the last place person’s rank last, such that
they will not support a redistribution policy which would potentially benefit people who
fall behind.

A weakness of such experiments is that they are exclusively conducted among either
college students (Kuziemko et al. 2014), or among employees from one specific occupation
like university employees (Card et al. 2012), public sector workers (Mas 2017), one-time
salespersons (Cohn et al. 2014a), etc. We conduct an online survey experiment in the
Netherlands with participants from thirteen occupations. Our aim is to get a more com-
prehensive view and to better understand the mechanisms behind the effect of disclosure of
peers’ wage information on workers’ behaviour and satisfaction. We address the question
of how employees are affected by information about their position in the wage distribution
of similar peers. Similar to Cruces et al. (2013), we collected data on (and, hence, can
control for) the beliefs of workers about their position in the wage distribution. We have
one treatment group where the subjects receive their true wage position in the distribution
calculated by the website after their beliefs have been elicited, and one control treatment
group where participants do not receive this piece of information.

We collaborate with a Dutch website called Loonwijzer where a survey about the
labor market runs all year long. Questions in the survey are about a respondent’s current
situation in the labor market, including the wage, work experience, job satisfaction, the employer, health situation, house composition, and so on. Every year, one of the survey respondents is selected to win a lottery prize.\footnote{The website is \url{www.loonwijzer.nl} and it has been running in the Netherlands for more than 15 years. It also has other versions for different countries or regions. For practical reasons, we chose 13 occupations with the most observations from our historical dataset to construct wage peer groups. Based on tenure, industry, and education we are able to form 117 peer wage cells. See section 2.1 for more details.}

We add a few elements to the 2014 wave of the survey. First, we randomise survey participants into two groups, namely, a treatment and control group. Randomisation starts from the very beginning of the survey: the survey switches from the ‘control-version’ to ‘treatment-version’ every 8 minutes.\footnote{It is an online field experiment. Participants log in at different times.} Second, to learn about the mechanisms underlying possible effects of peer wage information, we include questions regarding reciprocity and the perceptions of how employers treat under-performing employees (details of such measures will be introduced in the next section). Third, before the possible treatment, we ask all participants to report their beliefs about their position in the wage distribution of similar workers in the economy. Up to this point, everything is the same for the treatment and control group. The only difference between the treatment and the control group takes place after answering the questions about beliefs: Subjects in the treatment group are provided with the information stating the ‘true’ position of their wage in the wage distribution of peers, while subjects in the control do not receive this information. Subsequently, all subjects proceed to questions about job satisfaction, wage satisfaction, work effort, and intention to stay with the current employer.

Our experimental design has several unique features. To begin with, we make use of a subsample of the Dutch population that is more representative than previous experimental studies. Specifically, we have a sample of employees with thirteen different occupations across a range of industries. Second, we control for subjects’ beliefs on their position in the wage distribution. Subjective beliefs on their relative ranking is important because it could lead to different treatment effects of income comparison if a worker overestimates
or underestimates her position, even though she receives the same information. Note that our design ensures anonymity by comparing participants to a more general peer group instead of those whom they know.

Our randomisation works. The control and treatment groups are balanced on almost all observable characteristics. The number of subjects is also balanced between control and treatment.

We start our analysis by distinguishing between different types of information workers receive. We denote news as a situation where there is a difference between the true wage position provided by the website and workers’ beliefs. We further denote ‘good news’ as information that the true wage position is higher than the worker believed. Likewise, we speak of ‘bad news’ when a worker learns that the true position in the wage distribution is worse than he or she initially believed. The data show that around 70% of our subjects receive good news (initially underestimate their position in the distribution wage), 20% of them receive bad news (initially overestimate their position), and approximately 10% have accurate beliefs about their position and so receive neutral news. These proportions are similar in the treatment and the control groups.

Surprisingly, we find no significant treatment effects on job satisfaction, wage satisfaction, and intention to stay, neither for good news nor for bad news. We conjecture why we fail to get significant treatment effects. Subjects in our experiment may simply not trust the information provided by the website. Moreover, the reference group we chose for our subjects is defined as similar workers with the same occupation, tenure, and educational background. Subjects in our experiment might care more about comparisons with workers who are even more similar, e.g. direct colleagues in the same firm, office, on the same floor, or living nearby. Another conjecture is that the timespan for the information treatment to have any effect might be insufficient. Since we did not find any effect of additional wage information on wage satisfaction, it appears to us not useful to pursue the planned analysis of heterogeneous effects of the information intervention on effort, job
satisfaction, and intention to quit.

Our experiment contributes to the empirical and experimental literature on the effects of income comparison among peers, especially with exogenous information intervention as the treatment variable. Our design, combining survey methodology and experimental randomisation, provides an alternative and less expensive way for future researchers to shed light on such causal relationships.

The remainder of this chapter is organised as follows. Section 2 describes our experimental design, followed by predictions in section 3. Section 4 presents the main results. Section 5 discusses possible explanations and section 6 concludes.

## 5.2 Experimental design

### 5.2.1 The website

We collaborate with a widely used online labor market survey in the Netherlands (50,000 visitors annually) called WageIndicator (‘Loonwijzer’ in Dutch, www.loonwijzer.nl). Web visitors are invited to complete a web survey about work and wages. Each year, one of the survey participants who has completed the survey wins a prize worth 1,400 euros. During the survey, participants fill in their personal characteristics, including gender, age, birth place, residence, and so on; they are also asked to provide job-related information such as income, industry, occupation, work experience, firm size, sector, etc.

We added questions measuring reciprocity, both positive and negative reciprocity, and we also added a question about the way the employer deals with underperforming employees. Reciprocity is measured by a standard set of questions developed by Perugini et al. (2003) and included in the German Socio-Economic Panel Study (GSOEP) surveys. Specifically, respondents indicate on a five-point scale about the extent to which a set of statements apply to them.\(^5\) We measure how employers treat employees who under-

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\(^5\)Detailed descriptions of the questions are reported in chapter 4. Alternatively, please check the information about the GSOEP questionnaire following the link [http://www.die.de/en/soep](http://www.die.de/en/soep).
perform using a question from the “NIDI” (Netherlands Interdisciplinair Demografisch Instituut) survey (Van Dalen et al. 2007). The question is “If in your organisation employees perform poorly, are they mostly...”, participants choose from the list of “fired”, “retrained”, “insulated”, “accepted”, “kicked upstairs (being isolated)”, “put in a lower position”, and “I don’t know” to describe the action their employer might take in the situation.6

Since the Wage Indicator survey has been running in the Netherlands for more than fifteen years, we have a rich dataset of individuals with different backgrounds. This is useful for us to construct wage distributions for groups of similar workers using historical data. To make the wage distribution as comparable as possible, we use the most recent data waves, namely, from the January 2012 to August 2013 waves of the survey.

5.2.2 Information treatment

The only difference between the control group and the treatment group is that only the treatment group receives information about their actual position in the wage distribution of peers. On the website, the version of the survey is switched between control (without information treatment) and treatment group every 8 minutes.

While completing the online questionnaire, respondents are asked at what position they think they are in the (hourly) wage distribution measured in deciles, compared to similar peers (defined as those with similar education, tenure, and occupation) in the Dutch economy. More specifically, they are asked to choose X (ranging from 0 to 9) in the question “In your opinion, X out of 10 people like me earn more than I do.” We ask this question to all participants. After answering that question, participants in the treatment group receive information about their true position in this wage distribution, stating the true X according to our historical data. We focus on the largest thirteen occupations

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6The original question and multiple choices are in Dutch. NIDI is a research group focusing on population issues. It belongs to the Royal Netherlands Academy of Arts and Sciences (KNAW) and is affiliated with the University of Groningen (RUG). See the website of NIDI for more information. https://www.nidi.knaw.nl/en/
in the dataset so that we have sufficient observations for each peer wage group. People from other occupations and the unemployed are not included in our experiment (so they fill out the regular online survey).

The method used in this article to construct the reference wage group is taken from empirical labor studies. In this chapter, the reference wage group is a group of workers with similar occupation, tenure, and education background, and thus we could construct for each worker a wage distribution for the reference group and locate her position in this distribution.

After this question, subjects in the control group are asked about job satisfaction, wage satisfaction, intention to quit their current job, and work effort in the future. Subjects in the treatment group receive a piece of information about their ‘real’ position in the wage distribution of similar workers, and they are told that this information is provided based on the historical data collected by the website. After receiving the news, they answer the same questions as the respondents in the control group about satisfaction, intention to quit, and effort to finish the survey. Satisfaction is measured on a one-to-five scale where ‘one’ stands for ‘highly dissatisfied’ and ‘five’ means ‘highly satisfied’. Work effort and intention to stay with the current employer are asking subjects to express their willingness to work hard and willingness to stay with their current employer, respectively. These questions are measured on the same one-to-five scale where ‘one’ means ‘fully disagree’ and ‘five’ means ‘fully agree’.

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7 The 13 occupation are general office clerks, shop sales assistants, accounting associate professionals, commercial sales representatives, secretaries (general), construction supervisors, administrative and executive secretaries, advertising and marketing professionals, systems analysts, statistical, finance and insurance clerks, motor vehicle mechanics and repairers, sales and marketing directors and chief executives. We differentiate between 3 different education levels using data on years of education. We also have three tenure groups (less than 4 years, 4 to 10 years, and more than 10 years). In total, we have 35,399 observations from the waves from January 2012 to August 2013 to construct $13 \times 3 \times 3 = 117$ peer wage groups.

8 For a detailed description of the general method to calculate the reference wage groups, see chapter 4. Alternatively, see Dahlin et al. (2014) for a review.
5.3 Predictions

Intuitively, a better than expected wage position will increase a worker’s job satisfaction and, therefore, will make her more willing to stay with the current employer. In contrast, a worse than expected wage position will decrease a worker’s job satisfaction and thus reduces the probability of staying with the current employer.

The predictions for reciprocal workers will be carefully described in chapter 4. Reciprocity theory states that workers return the generosity from the employer by exerting more effort, and they shirk at work if the wage is not generous. The former is described as positive reciprocity and the latter as negative reciprocity (see more examples of reciprocal behaviours (Fehr and Gächter 2000)). We predict that a better than expected position in the peers’ wage distribution increases intended work effort of workers who are positively reciprocal. A worse than expected position decreases intended work effort of workers who are negatively reciprocal. The size of the response increases with the strength of the reciprocal inclinations as well as with the difference between expected and real position. The predictions are consistent with two theories: one is the social exchange theory developed by Blau (1964) in which workers return the ‘favour’ by the employer by exerting more effort, and the other is the conditional altruism model by Levine (1998) in which workers care for the employer conditional on the employer’s kindness.

We also predict that workers’ effort response is stronger if the employer dismisses underperforming workers. A better wage position than expected would increase effort in order to reduce the chance of being dismissed. This is because the threat from the employer reminds the employees that it is important to work hard so as not to be dismissed, under the assumption that there is efficiency wage (Shapiro and Stiglitz 1984). The size of this response increases with the difference between expected and real position. This effect only holds for employees who earn higher than expected wage because the higher than expected wage makes the efficiency wage hypothesis plausible to workers, in which workers are offered a wage that is higher than the market clearing wage and they exert
effort in order not to be dismissed and become involuntarily unemployed.

5.4 Results

1,468 subjects started the survey during the experimental period from March 5th, 2014 until April 30th, 2015. After the belief-elicitation stage and before the information treatment, the number of subjects in the control and the treatment group were 761 and 707, respectively (51.8% vs. 48.2%, z-statistic = -1.41, p = 0.16). The number of subjects dropped when they answered questions about satisfaction and effort, which left us with 454 subjects in the control group and 488 subjects in the treatment. We will focus on these subjects, who answered all five questions about satisfaction, intended effort, and intention to stay with the current employer.

5.4.1 Data

We first report information about attrition rates, however. Table 5.1 reports the attrition rate for the control and treatment group in our experiment. We define \( Attrition = \frac{N_1 - N_2}{N_1} \), where \( N_1 \) is the number of participants who successfully entered their answer to the belief question: \( N_1 = 707 \) for the treatment group and \( N_1 = 761 \) for the control. \( N_2 \) is the number of participants who continued answering the remaining questions. Participants sequentially answer questions about job satisfaction, wage satisfaction, intention to work hard, intention to work overtime unpaid, and intention to stay with their current employer. Therefore, the value of \( N_2 \) might differ after each question in both groups. We thus report the cumulative attrition rates for those five questions and we compare the differences in the attrition rates between the control and treatment group.
Table 5.1: Cumulative attrition rates of outcome variables

<table>
<thead>
<tr>
<th>Outcome variables</th>
<th>Control</th>
<th>Treatment</th>
<th>p-value for difference</th>
</tr>
</thead>
<tbody>
<tr>
<td>Job satisfaction</td>
<td>0.16</td>
<td>0.16</td>
<td>0.613</td>
</tr>
<tr>
<td>Wage satisfaction</td>
<td>0.16</td>
<td>0.17</td>
<td>0.515</td>
</tr>
<tr>
<td>Intention to work overtime unpaid</td>
<td>0.40</td>
<td>0.28</td>
<td>0.000</td>
</tr>
<tr>
<td>Intention to work hard</td>
<td>0.40</td>
<td>0.29</td>
<td>0.000</td>
</tr>
<tr>
<td>Intention to stay with current employer</td>
<td>0.40</td>
<td>0.31</td>
<td>0.000</td>
</tr>
</tbody>
</table>

Notes: Subjects sequentially answer the five questions after the belief elicitation question (and information treatment if treated). The cumulative attrition rate is defined as the number of subjects who failed to proceed to the question divided by the number of subjects who were supposed to answer the questions.

The results from Table 5.1 suggest that knowing about the real position in the wage distribution prevents subjects from leaving the survey. In the first row of Table 5.1, almost 16% of subjects in both groups quit the survey when answering questions about job and wage satisfaction. When the questions go on to intentions to work hard the attrition rate increases to 40% in the control group and to 29% in treatment.

We are surprised by and do not have a direct answer for why fewer subjects stopped answering the survey questions in the treatment group as compared to the control. A possible reason might be that the information treatment triggered the curiosity of the participants in the treatment group so that they continued answering the questions to get more information about their relative wage.

While it is difficult to rule out the possibility that subjects drop out on the basis of unobservables, we can check whether they do so on the basis of observables. We therefore compare the differences in observable variables between the control and the treatment group if they manage to complete all questions. Table 5.2 summarises descriptive statistics about the main observable variables of the participants, with 454 in control and 488 in treatment. Most differences in characteristics between the control and treatment groups are insignificant except for age ($p = 0.05$, Mann-Whitney test), hourly wage ($p = 0.06$, MW), and negative reciprocity ($p = 0.09$, MW). Subjects in the treatment group are one year older than in the control (35.92 v.s 34.54) and the difference in their negative reciprocal inclinations is not large: 2.36 in treatment and 2.45 in control. However, the
difference in hourly wage is rather substantial: on average, subjects in the treatment group earn 21.85 euro per hour while subjects who are randomised into the control group earn 16.56 euro per hour. This difference might have serious consequences for our analysis. For example, we may not be able to observe the treatment effects for subjects who earn a very high hourly wage because there are no observations in control who earn a comparable wage.

In our sample, on average, 50% of subjects are females; subjects on average are 35 years old, with 16 years of working experience; more than 70% of them are employed with full-time jobs; 88% of them are working in the private sector, and on average they earn between 16 and 22 euros per hour. In terms of other personal characteristics, 44% of our subjects are highly educated (ISCED 5-6, first/second stage of tertiary education), and 43% of our participants are married. In both the control and treatment groups they assess themselves as positively reciprocal, having an average score of more than 4 on a scale of 5.

Finally, there is heterogeneity in people’s beliefs about their wage position. In Figure 5.1, we find that more than 70% of participants in the treatment group receive good news in the experiment, meaning that their real position in the wage distribution provided by the website is better than their own estimate; nearly 20% of participants in the treatment overestimate their position in the wage distribution of similar workers and therefore they are defined to receive bad news; approximately 10% of them receive neutral news because they have an accurate belief about their wage position. These fractions are the same in the control group where participants would have received good, bad, or neutral news were they in the treatment. We do not find statistical differences between the control and treatment groups among all three news-type groups. The insignificant difference in news

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9The difference in hourly wage between the control and treatment group is insignificant before subjects quit the survey. On average, the control group earned 18.18 euro per hour while the treatment group earned 22.03 euro per hour ($p = 0.24$, MW). Note that more subjects in the control group quit the survey than the treatment and the hourly wage dropped to 16.56 after the quit. Hence, it appears that the difference in hourly wage is due to the low quit rate of high earners in the treatment group.
### Table 5.2: Descriptive statistics

<table>
<thead>
<tr>
<th></th>
<th>Control group</th>
<th>Treatment group</th>
<th>p-value for difference</th>
</tr>
</thead>
<tbody>
<tr>
<td>Good news</td>
<td>75% (0.44)</td>
<td>71% (0.45)</td>
<td>0.19</td>
</tr>
<tr>
<td>Neutral news</td>
<td>8% (0.27)</td>
<td>9% (0.28)</td>
<td>0.54</td>
</tr>
<tr>
<td>Bad news</td>
<td>18% (0.38)</td>
<td>20% (0.40)</td>
<td>0.30</td>
</tr>
<tr>
<td>Female</td>
<td>46% (0.50)</td>
<td>51% (0.50)</td>
<td>0.20</td>
</tr>
<tr>
<td>Age (in years)</td>
<td>34.54 (11.31)</td>
<td>35.92 (11.63)</td>
<td>0.05</td>
</tr>
<tr>
<td>Full-time employment</td>
<td>76% (0.43)</td>
<td>71% (0.45)</td>
<td>0.11</td>
</tr>
<tr>
<td>Hourly wage (in euro)</td>
<td>16.56 (15.74)</td>
<td>21.85 (86.55)</td>
<td>0.06</td>
</tr>
<tr>
<td>Tenure (in years)</td>
<td>15.50 (10.59)</td>
<td>16.30 (11.05)</td>
<td>0.33</td>
</tr>
<tr>
<td>Private sector</td>
<td>88% (0.33)</td>
<td>88% (0.33)</td>
<td>0.97</td>
</tr>
<tr>
<td>Low educated (ISCED 0-2)</td>
<td>1% (0.11)</td>
<td>2% (0.13)</td>
<td>0.52</td>
</tr>
<tr>
<td>Mid educated (ISCED 3-4)</td>
<td>48% (0.50)</td>
<td>45% (0.50)</td>
<td>0.44</td>
</tr>
<tr>
<td>High educated (ISCED 5-6)</td>
<td>44% (0.50)</td>
<td>44% (0.50)</td>
<td>0.99</td>
</tr>
<tr>
<td>Married</td>
<td>43% (0.50)</td>
<td>42% (0.49)</td>
<td>0.72</td>
</tr>
<tr>
<td>Positive reciprocity</td>
<td>4.12 (1.03)</td>
<td>4.03 (1.07)</td>
<td>0.20</td>
</tr>
<tr>
<td>Negative reciprocity</td>
<td>2.45 (0.99)</td>
<td>2.36 (1.00)</td>
<td>0.09</td>
</tr>
<tr>
<td>Job satisfaction</td>
<td>3.65 (1.23)</td>
<td>3.68 (1.20)</td>
<td>0.70</td>
</tr>
<tr>
<td>Wage satisfaction</td>
<td>2.86 (1.25)</td>
<td>2.86 (1.19)</td>
<td>1.00</td>
</tr>
<tr>
<td>Intention to work overtime</td>
<td>2.72 (1.46)</td>
<td>2.74 (1.52)</td>
<td>0.88</td>
</tr>
<tr>
<td>Expected to work harder</td>
<td>4.37 (1.08)</td>
<td>4.39 (1.09)</td>
<td>0.46</td>
</tr>
<tr>
<td>Intention to stay</td>
<td>3.33 (1.48)</td>
<td>3.29 (1.49)</td>
<td>0.66</td>
</tr>
<tr>
<td>Observations</td>
<td>454</td>
<td>488</td>
<td>0.12</td>
</tr>
</tbody>
</table>

Notes: Standard deviations are between parentheses. Mann-Whitney test p-values are reported. Variables such as job satisfaction, wage satisfaction, intention to work overtime unpaid, intention to work harder, intention to stay with their current employers, positive reciprocity, and negative reciprocity are measured on scales from 1 to 5, indicating completely disagree to completely agree.
types between the control and treatment also suggests that our randomisation works.

Figure 5.1: Types of ‘news’ in control and treatment

![Graph showing types of news](image)

**Notes:** Subjects in the treatment received good news when the real wage position is higher than their beliefs, and bad news when their belief is higher than the real position; those with accurate beliefs with the real wage position received neutral news. Subjects in the control group did not receive news but they would have received this were they in treatment.

Table 5.3 provides the correlations between important variables in our analysis. Positive reciprocity is negatively correlated with negative reciprocity. Working in the private sector is correlated with being male, younger, less experienced, and less highly educated. Female workers in our experiment seem to have a higher positive reciprocity and a lower negative reciprocity; they seem to underestimate their wages so that they are more likely to receive good news rather than bad news. Older participants are more experienced at work and they tend to show lower negative reciprocity. Highly educated subjects tend to exhibit higher positive reciprocity, they are more likely to underestimate their wages, they are younger, less likely to work in the private sector, and less experienced at their current job.

Depending on which type of news they receive, we further summarise the main outcome
### Table 2.3: Correlations of Independent Variables

<table>
<thead>
<tr>
<th>Education</th>
<th>Female</th>
<th>Private Sector</th>
<th>Good News (CN)</th>
<th>Bad News (en)</th>
<th>Treatment</th>
<th>Negative Reciprocity</th>
<th>Positive Reciprocity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Education</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>0.95</strong></td>
<td>***</td>
<td><strong>0.16</strong></td>
<td>-</td>
<td>-</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>0.00</td>
<td><strong>0.09</strong></td>
<td><strong>0.23</strong></td>
<td>-</td>
<td>-</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>0.00</td>
<td><strong>0.17</strong></td>
<td><strong>0.18</strong></td>
<td><strong>0.02</strong></td>
<td><strong>0.04</strong></td>
<td><strong>0.05</strong></td>
<td><strong>0.01</strong></td>
<td><strong>0.01</strong></td>
</tr>
<tr>
<td><strong>0.01</strong></td>
<td></td>
<td><strong>0.06</strong></td>
<td></td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>0.00</td>
<td><strong>0.16</strong></td>
<td><strong>0.15</strong></td>
<td>-</td>
<td>-</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>1.00</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
</tbody>
</table>

Notes: Pearson correlation coefficients are reported. Positive reciprocity and negative reciprocity are measured on a scale of 0-1. Age and gender are continuous variables. The other variables are dummy variables. Subjects with degree from college or above are defined to have high education.
variables in the control and treatment groups. Table 5.4 presents the average scores of job satisfaction, wage satisfaction, intention to work unpaid overtime, intention to work hard, and intention to stay with their current employer in both groups. Given the news type, the average scores between the control and the treatment groups are surprisingly close. We notice that knowing that one’s wage position is better than expected decreases wage satisfaction (second row in Table 5.4 under the column of “Good news”), and knowing one’s wage position is worse than expected increases wage satisfaction (the same row under column of “Bad news”). Similarly, receiving good news about one’s wage position decreases the intention to stay with the current employer (row 5, from 3.36 to 3.32) and receiving bad news about the wage position increases job satisfaction (row 1, from 3.73 to 3.90). Although these changes are never statistically significant, they suggest we failed to change people’s beliefs.

Table 5.4: Descriptive statistics of outcome variables (scale 1–5)

<table>
<thead>
<tr>
<th></th>
<th>Good news</th>
<th>Neutral news</th>
<th>Bad news</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Control</td>
<td>Treatment</td>
<td>Control</td>
</tr>
<tr>
<td>Job satisfaction</td>
<td>3.63</td>
<td>3.63</td>
<td>3.58</td>
</tr>
<tr>
<td></td>
<td>(1.24)</td>
<td>(1.23)</td>
<td>(1.29)</td>
</tr>
<tr>
<td>Wage satisfaction</td>
<td>2.82</td>
<td>2.79</td>
<td>3.29</td>
</tr>
<tr>
<td></td>
<td>(1.26)</td>
<td>(1.16)</td>
<td>(1.27)</td>
</tr>
<tr>
<td>Intention to work unpaid</td>
<td>2.64</td>
<td>2.70</td>
<td>2.77</td>
</tr>
<tr>
<td></td>
<td>(1.46)</td>
<td>(1.51)</td>
<td>(1.52)</td>
</tr>
<tr>
<td>Expected to work hard</td>
<td>4.36</td>
<td>4.41</td>
<td>4.43</td>
</tr>
<tr>
<td></td>
<td>(1.12)</td>
<td>(1.06)</td>
<td>(0.88)</td>
</tr>
<tr>
<td>Intention to stay</td>
<td>3.36</td>
<td>3.32</td>
<td>2.83</td>
</tr>
<tr>
<td></td>
<td>(1.48)</td>
<td>(1.49)</td>
<td>(1.44)</td>
</tr>
<tr>
<td>Obs.</td>
<td>339</td>
<td>346</td>
<td>35</td>
</tr>
</tbody>
</table>

Notes: Standard deviations are between parentheses. News (good, neutral, and bad) variables are dummy variables. Good news is received when the position in the peer wage distribution subjects receive from the website is better than their beliefs. Neutral news refers to the cases when subjects have accurate beliefs about their position in the peer wage distribution. Bad news is defined as information about the position in the peer wage distribution that is worse than their beliefs. All outcome variables are measured on a scale from 1 to 5, ranging from completely disagree to completely agree.

We will now use regression analysis to examine the information treatment effects.
5.4.2 Treatment effects

We regress the satisfaction and intention variables on the treatment dummy $T_i$, the type of news, and the interaction between the treatment dummy and news variable. Recall that we elicit for each subject their beliefs about the relative wage position in the distribution of similar workers by filling in the value $X_i$ in the question “$X_i$ out of 10 workers like me earn more than I do”. Then we calculate the reference wage $\hat{X}_i$ based on historical data and in this way we know the ‘true’ wage position $\hat{X}_i$ and how it differs from the belief $X_i$. Subjects in the treatment were offered this additional information about $\hat{X}_i$ while those in control did not. To capture the size of this ‘information shock’ between $\hat{X}_i$ and $X_i$, we define the “Good news” variable as $G_i = X_i - \hat{X}_i$ and the “Bad news” variable as $B_i = \hat{X}_i - X_i$ in model 5.1. We expect our treated subjects to respond positively to good news and negatively to bad news, that is, $\alpha_1 > 0$ and $\alpha_2 < 0$.

$$Y_{Satisfaction_i} = \alpha_0 + \alpha_1 T_i \times G_i + \alpha_2 T_i \times B_i + \delta T_i + \zeta G_i + \eta B_i + \sum_i \delta_i X_i + \epsilon_i, \quad (5.1)$$

where $T_i$ is a dummy variable that equals 1 if the subject receives the information. $G_i$, meaning “good news”, is the extent of good news when the wage position is better than the expectation and zero otherwise. $B_i$ is the extent of “bad news” when the wage position is worse than the expectation and zero otherwise. Both news variables are non-negative. Subjects who have consistent beliefs with the true distribution are defined to receive “neutral news”. Neutral news is our baseline for the regression. We also control for a set of personal characteristics $X_i$ and error term $\epsilon_i$.

Note that $\delta$ reflects the treatment effect when there is no news, that is, when the respondent is informed that her true wage position is the same as her belief; the coefficients $\zeta$ and $\eta$ give us a baseline measure of satisfaction for a subject who would have received good news and bad news when in treatment.
We also have a regression model about intended effort variables, namely, intention to work overtime without payments and intention to work hard. We regress effort variables on the interactions between reciprocity measures and the extent of news variables in the model.

In Table 5.5 we use two measures for satisfaction and one intention variable as the dependent variable. We use the answer to a question of job satisfaction, a question about wage satisfaction, and the intention to work with the current employer for the next year, respectively, in column (1), (2), and (3). All questions are measured on a scale from 1 to 5. In addition, we control for individual characteristics such as gender, age, marital status, and job market related characteristics such as firm size, education, and so on. OLS coefficients are reported.\(^\text{10}\)

We find no significant effects of the information treatment on satisfaction and intention to stay with the current employer, after controlling for individual characteristics. In column (1), when the dependent variable is job satisfaction, we do not find any significant treatment effects. In fact, the signs of the treatment effects are in the opposite direction of our predictions. We again find insignificant treatment effects of receiving wage information on wage satisfaction in column (2) and intention to stay with the current employer in column (3).

In light of the failure to affect wage satisfaction, we do not find it worthwhile to examine heterogeneous treatment effects with respect to reciprocity or when employers dismiss underperforming employees.

### 5.5 Possible explanations

Our experiment provides little evidence for the effect of disclosing peer wage information on satisfaction, which is opposite to our theoretical prediction. Here, we offer some

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\(^\text{10}\)We also run the same analysis with a Tobit model to account for the fact that the dependent variables are restricted between 1 and 5. The results are both qualitatively and quantitively similar to OLS models. See the Tobit regression table in the appendix.
Table 5.5: OLS regression: Treatment effects of news on satisfaction

<table>
<thead>
<tr>
<th></th>
<th>Job Satisfaction</th>
<th>Wage Satisfaction</th>
<th>Intention to Stay</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Treatment × Good News</td>
<td>-0.009 (0.038)</td>
<td>0.023 (0.034)</td>
<td>0.006 (0.042)</td>
</tr>
<tr>
<td>Treatment × Bad News</td>
<td>0.045 (0.064)</td>
<td>0.059 (0.063)</td>
<td>-0.093 (0.076)</td>
</tr>
<tr>
<td>Treatment</td>
<td>0.004 (0.171)</td>
<td>-0.175 (0.161)</td>
<td>-0.057 (0.194)</td>
</tr>
<tr>
<td>Good News</td>
<td>-0.046 (0.033)</td>
<td>-0.148*** (0.029)</td>
<td>-0.073** (0.034)</td>
</tr>
<tr>
<td>Bad News</td>
<td>0.001 (0.044)</td>
<td>-0.039 (0.041)</td>
<td>0.065 (0.053)</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Constant</td>
<td>7.194*** (1.894)</td>
<td>7.787*** (1.887)</td>
<td>5.984*** (2.023)</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.072</td>
<td>0.184</td>
<td>0.077</td>
</tr>
<tr>
<td>N</td>
<td>826</td>
<td>822</td>
<td>942</td>
</tr>
</tbody>
</table>

*Notes: Dependent variables are measured on 5-point (from 1 to 5) scales. OLS coefficients are reported. Treatment is a dummy and the other independent variables take positive values. Good news and bad news are measured by the extent of the news, representing the difference between one’s actual and expected position in the wage distribution. Control variables are age, age squared, gender, tenure, tenure squared, hourly wage, marital status, private sector or public sector, full-time job or part-time, occupation, industry, and firm size. Robust standard errors are between parentheses. * p < 0.1, **p < 0.05, *** p < 0.01.
conjectures why this is the case. Our first conjecture is that subjects might not trust the
information provided by the website.\footnote{Though Loomwijzer has participants who revisit the survey each year and it rarely receives questions or complaints – according to a personal conversation with the website manager Paulien Osse – we cannot rule out the possibility that people may have concerns about the information in our experiment, especially because it is the first time that the information about the wage distribution among similar workers is provided on the website.}

We also suspect that we might need a longer timespan for the information treatment to have an effect. Compared to other studies, subjects in our experiment were exposed to the information treatment for a few seconds, and then responded to our questions, which is much shorter than the “3 to 10 days” in Card et al. (2012), or more than a year in Rege and Solli (2014). Finally, the relevant group for workers to compare their wages to might not perfectly match the one in our experiment. Dahlin et al. (2014) show that family members, friends, coworkers, and those with similar age are much more likely to be used as references rather than neighbours, residents in the same town, in the same country, or across the world. If such findings can be extended to the Netherlands, our subjects would be more affected by wage comparisons with a colleague than with someone they barely know.

\section*{5.6 Concluding remarks}

In this survey experiment, we study the effects on job and wage satisfaction of providing information about one’s wage position among similar others. We conduct the experiment in an online survey in the Netherlands. After answering job relevant questions and eliciting beliefs on the position in a wage distribution of peers, treated subjects are shown ‘real’ wage position. We expected that receiving ‘good’ news would increase workers’ satisfaction and receiving ‘bad’ news would reduce satisfaction. We also predicted that a better than expected wage position would increase the effort of worker who are positively reciprocal and a worse than expected wage position would decrease effort of workers who are negatively reciprocal. Additionally, we predicted that a worker with higher than
expected wage would exert more effort if the employer tends to dismiss underperforming workers while a worker with lower than expected wage would exert less effort under the same situation. We do not find any significant effect of ‘news’ on job satisfaction, wage satisfaction, or intention to stay with the current employer. Given these results, we decided against exploring heterogeneous treatment effects.

Three possible reasons for the lack of effect are that subjects did not trust the source of information about their relative income, that they are comparing their income mainly with other reference groups, and that there was insufficient time for the information to have an effect.
# Appendix

Table 5.6: Tobit regression: Treatment effects of news on satisfaction

<table>
<thead>
<tr>
<th>Y=</th>
<th>Job Satisfaction</th>
<th>Wage Satisfaction</th>
<th>Intention to Stay</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment × Good News</td>
<td>-0.019</td>
<td>0.042</td>
<td>0.010</td>
</tr>
<tr>
<td>(0.059)</td>
<td>(0.046)</td>
<td>(0.081)</td>
<td></td>
</tr>
<tr>
<td>Treatment × Bad News</td>
<td>0.083</td>
<td>0.079</td>
<td>-0.186</td>
</tr>
<tr>
<td>(0.103)</td>
<td>(0.080)</td>
<td>(0.147)</td>
<td></td>
</tr>
<tr>
<td>Treatment</td>
<td>-0.014</td>
<td>-0.252</td>
<td>-0.099</td>
</tr>
<tr>
<td>(0.265)</td>
<td>(0.208)</td>
<td>(0.379)</td>
<td></td>
</tr>
<tr>
<td>Good News</td>
<td>-0.065</td>
<td>-0.203***</td>
<td>-0.129*</td>
</tr>
<tr>
<td>(0.051)</td>
<td>(0.039)</td>
<td>(0.067)</td>
<td></td>
</tr>
<tr>
<td>Bad News</td>
<td>-0.005</td>
<td>-0.045</td>
<td>0.153</td>
</tr>
<tr>
<td>(0.070)</td>
<td>(0.050)</td>
<td>(0.105)</td>
<td></td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Constant</td>
<td>9.662***</td>
<td>9.276***</td>
<td>10.16**</td>
</tr>
<tr>
<td>(2.910)</td>
<td>(2.534)</td>
<td>(3.996)</td>
<td></td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td>-1322.160</td>
<td>-1313.189</td>
<td>-1554.639</td>
</tr>
<tr>
<td>N</td>
<td>826</td>
<td>822</td>
<td>942</td>
</tr>
</tbody>
</table>

*Notes: Dependent variables are measured on 5-point (from 1 to 5) scales. Treatment is a dummy and the other independent variables take positive values. Good news and bad news are measured by the extent of the news. Control variables are age, age squared, gender, tenure, tenure squared, hourly wage, marital status, private sector or public sector, full-time job or part-time, occupation, industry, and firm size. Robust standard errors are between parentheses. * p < 0.1, **p < 0.05, *** p < 0.01.
Chapter 6

Summary

This thesis focuses on the relationship between social identity and social preferences. Each chapter focuses on a different type of identity – group identity in chapter 2, social status in chapter 3, relative earnings in the wage distribution among similar workers in chapters 4 and 5.

In chapter 2, I study the role of social distance in a bribery setting. In the laboratory, I create two groups between which the social distance is larger than that within the same group, so that subjects feel closer towards their in-group members than those from out-group. After generating different social distances, I introduce a one-shot, three-player game: two performers perform a real effort task and compete for an award for the winner; while the task is objective and the results will be revealed to the judge, the judge has the authority to decide which performer wins the task. In some of the treatments, performers have the opportunity to bribe the judge. There are three situations among three players regarding the relationship among them: all three of them are from the same group, one performer and the judge are from the same group and the other performer is from a different group, and two performers are from the same group and the judge is from a different group. We compare the bribing behaviour and the chance of winning for players with different social distances. We find that performers bribe a substantial amount (more than 60% bribed and they bribed more than 3 euro out of 10 euro) to the judge, despite of
their relationship to the other players. When bribes are not allowed, the chance of winning for a performer who has an in-group judge in the triad increases significantly compared to a performer whose judge is from a different group. However, if bribes are available, the in-group performer’s advantage vanishes completely. It suggests that a closer social distance works in different ways in judgements, depending on whether or not bribery is available.

Chapter 3 discusses how social status affects pro-social preferences. It is also an laboratory experiment. In the first part of the experiment, subjects are assigned with either a high social status or a low social status, depending on their ranking in a task. Moreover, the ranking is either randomly assigned by the table number or by the number of correct answers so that subjects perceive the ranking/status differently. In the next part of the experiment, pairs of players conduct a one-shot game. Player 1 proposes a division of effort of a team task to player 2. Player 2 learns about the advice and decides the actual effort of both players. Since the total effort is constant, the higher the effort a player makes the lower the other player will make. Additionally, it is costly to exert effort so a rational selfish player will choose the minimal effort. The results show that there are fewer selfish players 2 if they have assigned high status in the previous part, compared to their low status counterparts. Furthermore, when status is considered earned, high status players 2 deviate less from players 1’s advice which achieves a socially efficient outcome. The results suggest that in a teamwork setting, it makes a difference to achieve higher efficiency if high social status is assigned to those with higher ability.

In chapter 4, I move from the laboratory to observational data of a representative national survey (German Socio-Economic Panel). With the help of the dataset, I aim to investigate the empirical relationship between reciprocity and effort and that between reciprocity and job satisfaction. Reciprocity measures are constructed by questions in the survey. I find that positive reciprocity is positively correlated with longer hours of unpaid overtime work; and the higher a worker earned than her similar peers, the stronger
the correlation is. However, there is no such interaction for negative reciprocity. Job satisfaction is correlated to reciprocity but insensitive to the interaction between relative income and reciprocity.

Chapter 5 extends the idea of chapter 4 with an online survey experiment. In this artificial field experiment, we try to examine the effect of revealing a worker’s position in a wage distribution of similar workers on work effort and satisfaction. We collaborate with a nationwide website in the Netherlands. During the survey experiment, participants filled in their personal information such as age, gender, education, region, etc. and job-related information, for instance, industry, occupation, years of working, income, working hours, and so on. We also elicited respondents’ beliefs on their own position in the wage distribution of peers. There are one treatment group and one control group in the experiment. We provided the ‘real’ position in the wage distribution of similar workers to the treatment group and the control group did not receive this information. The online survey switched from the treatment version to the control version in eight minutes so that it would be impossible to have one participant in the treatment group and the control group at the same time. We compare job satisfaction, wage satisfaction, the intention to work hard, the intention to work overtime without payments, and the intention to stay with the current job in the treatment group, to those employees in the control group. We find marginal and insignificant treatment effect of providing additional information on satisfaction. Our treatment does not significantly affect wage satisfaction, therefore we decide not to examine heterogeneous treatment effects with respect to reciprocity or when employers dismiss underperforming employees. It seems that in order to get significant treatment effects, subjects have to trust the information from the experimenter, they should compare they wages to a more comparable peer group such as direct colleagues, and there should be enough time for subjects to ‘digest’ the treatment. Future research should learn from our experiment when they try to study the effect of providing wage information.
The overall analyses in this thesis show that social identity has explanatory power in bribery behaviour, pro-social preferences, reciprocity, and social comparisons of income. This adds new evidence to the literature of social identity theory, showing how identity influences economic outcomes.

Though the results presented in this thesis elaborate the economic relevance of social identity, future research following the direction in this thesis seems desirable. Researchers, for instance, could consider the long-run effect of in-group membership on bribery, or the effect of a high social status on preferences if status is not publicly known, or the effect of revealing wage information among different peers defined by the strength of connections, etc. This framework may expand our understanding of the implications of social identity in economic outcomes.
Samenvatting (Summary in Dutch)

Dit proefschrift concentreert zich op de relatie tussen sociale identiteiten en sociale preferenties. Hoofdstuk 2 gaat over groepsidentiteit, hoofdstuk 3 over sociale status, hoofdstuk 4 en 5 gaan over identiteiten, ontleed aan de relatieve verschillen in beloning voor mensen met gelijksoortig banen.

In hoofdstuk 2, bestuderen we de rol van groepsidentiteit sociale bij omkoping. In het laboratorium creëren we twee groepen waarbij de sociale afstand (die gevoeld wordt) tussen de groepen groter is dan de sociale afstand binnen de groepen. Na het creëren van deze verschillen, spelen de deelnemers in drietalen een one-shot game. Twee deelnemers, de taakspelers, voeren eenzelfde taak uit en de derde deelnemer, de scheidsrechter, bepaalt wie van de twee taakspelers de prijs wint. Terwijl de scheidsrechter precies geïnformeerd wordt over de prestaties van de taakspelers, houdt de scheidsrechter de volledige vrijheid om te bepalen wie van de twee anderen de prijs krijgt. In sommige treatments hebben de twee taakspelers de mogelijkheid om de scheidsrechter om te kopen. We vergelijken daarbij drie situaties met elkaar: één situatie waarbij alle drie uit dezelfde groep komen, één waarbij een van de taakspelers uit een andere groep komt en één waarbij de scheidsrechter uit een andere groep komt. Kijken we naar de resultaten, dan zien we dat veel spelers (60%) gebruik maken van de mogelijkheid om de scheidsrechter om te kopen en daar meer dan 3 van de 10 euro’s waar mee ze beginnen voor gebruiken. We zien dat als omkoping niet mogelijk is, dat het dan een significant voordeel oplevert voor een taakspeler waar de scheidsrechter uit dezelfde groep komt, vergeleken met een taakspeler waar de
scheidsrechter niet uit dezelfde groep komt. Als er wel omkoping mogelijk is, speelt het verschil in groepsaflomst geen rol meer. Dit wekt de suggestie dat het effect van sociale afstand afhankelijk is van de beschikbaarheid van omkoping.

In hoofdstuk 3 wordt door middel van een ander laboratorium experiment gekeken naar de invloed van sociale status op sociale preferenties. Hier krijgen spelers in het eerste deel van het experiment een hoge dan wel een lage status toegekend afhankelijk van een ranglijst. De plaats van spelers op de ranglijst is in sommige treatments random en in andere treatments afhankelijk van de resultaten in een taak. In het tweede deel worden spelers ingedeeld in tweetallen. Van dit tweetal doet 1 speler een voorstel hoeveel inspanning elk van het tweetal moet leveren om een bepaalde gezamenlijke taak uit te voeren. De andere speler neemt daarover vervolgens een besluit. De taak vereist een bepaalde hoeveelheid inspanning en als de ene speler meer inspanning moet leveren zal de andere speler minder inspanning hoeven leveren. Omdat het experiment zo is opgezet dat het leveren van inspanning kosten met zich mee brengt zal een rationeel zelfzuchtige speler kiezen voor het zelf leveren van een minimale inspanning en het werk overlaten aan de andere speler. De resultaten laten zien dat de speler die het besluit neemt minder zelfzuchtig optreedt als deze speler in het eerste deel een hogere status heeft toegekend gekregen dan de andere speler. Daarnaast als de status in het eerste deel niet random is toegekend, maar op basis van de resultaten van de taak in het eerste gedeelte, zullen spelers met een hoger status minder snel afwijken van het voorstel van de andere speler als dat voorstel voor de groep als geheel de minste kosten met zich mee brengt. Dit resultaat suggereert dat het uitmaakt of een hogere status al dan niet verdiend is.

In hoofdstuk 4 verlaten we het laboratorium en onderzoeken we op basis van Duitse data het verband tussen wederkerigheid en inspanning en tussen wederkerigheid en tevredenheid over het werk. Op basis van de data concluderen we dat positieve wederkerigheid positief samenhangt met de mate waarin onbetaald overwerk wordt verricht en dat deze samenhang sterker wordt des te groter het verschil in salaris is tussen twee werknemers.
Met betrekking tot negatieve wederkerigheid is er weliswaar ook een samenhang met onbetaald overwerk, maar die samenhang hangt niet af van het verschil in salaris.

Voor hoofdstuk 5 gebruikten we een online enquête om verder te borduren op het onderzoek van hoofdstuk 4. In hoofdstuk 5 proberen we te onderzoeken of het transparant maken van de loon positie van werknemers ten opzichte van gelijksoortige werknemers effect heeft op werk inspanning en werk tevredenheid. Voor dit onderzoek gebruiken we een Nederlandse website. Tijdens het onderzoek vullen deelnemers persoonlijke informatie als leeftijd, gender, opleiding, woonregio etc. in en informatie die te maken heeft met hun werk zoals bedrijfstak, beroep, ervaring, inkomen, werk uren etc. Ook vragen we waar de deelnemers in het loongebouw denken te staan in vergelijking tot soortgelijke werknemers. Er is een treatment groep en een controle groep. In de treatment groep krijgen de deelnemers te horen welke plaats zij in nemen ten opzichte van soortgelijke deelnemers en in de controle groep, krijgen deelnemers die informatie niet. Vergelijken we vervolgens de resultaten van de treatment groep met betrekking tot de werk tevredenheid, de loon tevredenheid, de bereidheid om in de toekomst hard te werken, de bereidheid tot onbetaald overwerk en de intentie om bij de huidige baan te blijven, met de resultaten van de controle groep, dan vinden we slechts marginale en niet significante verschillen tussen beide groepen. Ons laatste experiment laat geen significant effect zien van informatie op loon tevredenheid en daarom zijn we ook niet verder gaan kijken naar wederkerigheid en het effect van ontslagen. Het zou kunnen zijn dat om significante resultaten te krijgen het nodig is dat experimentatoren geloofwaardiger overkomen of dat deelnemers meer tijd krijgen om de informatie te verwerken. Toekomstig onderzoek naar het effect van dit soort informatie kan van deze ervaringen leren.

Dit proefschrift laat zien dat sociale identiteit een rol speelt bij omkoping, pro-sociale voorkeuren, wederkerigheid en het vergelijken van salarissen. Dit voegt bewijs toe aan de sociale identiteitstheorie, die stelt dat sociale identiteiten economische resultaten beïnvloeden. Er wordt duidelijk gemaakt dat sociale identiteiten van belang zijn, maar ook
dat het wenselijk is dat er verder op het onderzoek in dit proefschrift wordt gebouwd. Onderzoekers zouden bijvoorbeeld kunnen kijken naar lange termijn groepseffecten op omkoping, naar het effect van een hoge status op preferenties als deze status niet algemeen bekend is en/of naar het effect van transparantie met betrekking tot het loon op de sociale banden tussen werknemers. Het in dit proefschrift geschetste kader zou het begrip van het effect van sociale identiteit op economische uitkomsten dan wel eens kunnen vergroten.
结语 (Summary in Chinese)

本论文探讨了社会身份和社会偏好的关系。本书每章集中于不同的社会身份的表现形式——在第二章中的群体身份，第三章中的社会地位，以及第四和第五章中在相似的雇员里相对收入分布的状况。

在第二章中，我研究了行受贿赂情境里的社会距离的作用。在实验室里，实验的参加者被分成两个团队，经过一些任务之后，这两个团队里的成员之间的社会距离大于队内成员之间的社会距离，也就是说，与队外成员相比，每个成员感觉和队内成员更加亲密。之后，我们引入了一个三人游戏：在这个游戏里，两个选手做一项任务并且竞争属于获胜者的奖励，这个任务的结果是客观的并且会呈现给第三人——裁判，而另一方面，裁判会决定这二人中谁会赢得这个游戏。在一些实验组里，选手们可以贿赂裁判。这三人之间的关系有三种：三个人都是来自于同一个队；一个选手和裁判来自于同一个队，而另一个选手来自于不同的队；两个选手来自于相同的队，裁判来自于不同的队。我们比较了行贿行为在不同的实验组的不同，以及比较由迥异的社会距离导致的选手的获胜几率。我们发现，当行贿行为被允许的时候，选手们给裁判一份相当可观的贿赂（多于 60%的选手选择了行贿，在 10 欧元的资金总数里，他们选择贿赂裁判的贿赂数额多于 3 欧元），不管他们和另一个选手的关系如何。当行贿行为不被允许的时候，有一个来自于同一个队伍裁判的选手比另一个没有同队裁判的选手有更大的概率成为赢家，但当行贿被允许的时候，这个选手的优势迅速消失。这似乎表明更亲近的关系在人们的判断决策里会随着情景（例如贿赂是否被允许）不同而发挥不同的作用。

第三章讨论了社会地位如何影响亲社会化偏好。这也是一个实验室实验。在实验的第一个部分，实验的参加者根据在一个任务中的排名被赋予高社会地位或者低社会地位。此外，
在这个排名或者被实验当日的桌号编号决定，或者被完成任务的正确个数决定，所以参加者对于排名/地位的分配方式有不同的理解。在接下来的实验环节中，两人一组进行一轮游戏。玩家1向玩家2提出一个如何分担一个团队任务的出力程度的建议。玩家2得知这个建议后最终决定两个玩家实际承担的努力。由于需要承担的总的努力程度是恒定的，所以一个玩家出力越多，另一个玩家就能越少的付出努力，并且，因为付出努力是有成本的，所以一个理性自私的玩家会选择出最少的力。实验结果显示，与低社会地位的玩家2相比，高社会地位的玩家2更多选择自私的努力程度。另外，如果这个地位被认为是凭自身能力获得的，和低社会地位的玩家2相比，高社会地位的玩家2更多的利益玩家1的建议，而这个建议是具有高社会效率的。这些结果暗示在团队协作的设定中，将高社会地位赋予高能力的成员会带来更高的社会效率。

在第四章中，我的研究视角从实验室转移到了实际的观测数据，这些数据来自于一个具有代表性的全国问卷（德国社会经济面板数据）。基于这些数据，我试图探索互惠和工作努力程度以及互惠和工作满意度之间的实证关系。利用问卷中的问卷问题，我构建了互惠的程度。我发现正向互惠和更长的无选择加班时间有正相关的关系；并且，一个雇员比相似的同行挣得越多，这种正相关关系就越强烈。但是，负向互惠和非努力程度没有类似的相关关系。工作满意度和互惠相关，但是我没有发现其与互惠与相对收入的交互作用的相关关系。

第五章在第四章的基础上将其构想用一个在线的问卷实验进行拓展。在这个实际实验里，我们试图考察提供雇员在类似同行的收入分布的位置这个信息会对员工的工作努力程度和满意度的影响。我们和在荷兰全国通用的一个网站合作进行了这项实验。在实验里，问卷的参与者在问卷上填写例如年龄、性别、教育背景、区域等的个人信息，和比如产业、工龄、收入、工作时间等和工作相关的信息。我们还收集了参与者对于他们自己在同行收入分布的位置的想法。在我们的实验里，有一个实验组和一个控制组。我们给实验组提供了在同行收入分布中的“真实”位置，而控制组里的参加者不会收到这个信息。这个线上问卷每八分钟会在实验组版本和控制组版本之间切换，因此确保了在同一时间同一个参加者不会进入多个实验组参加实验。我们比较了两组参加者在工作满意度、工资满意度、工作努力程度意愿、无偿加班的愿望和继续留在当前的工作岗位的意愿。我们发现了提供额外的信息对工作满意度产生了微弱且不显著的实验作用。我们的实验没有显著的影响工资满意度，因此
我们决定不继续考察与互惠相关的或者当雇主解雇表现不好的雇员的情形下的异质性的实验作用。这似乎表明为了得到显著的实验效果，实验的参加者必须相信实验者提供的信息，他们应该同更为相似的人例如直接日常接触的同事进行工资的比较，以及他们应该有足够多的时间来“消化”实验处理。未来考察提供工资信息效果的研究可以从我们的实验中汲取教训。

概括而言，这本论文的分析展示出了社会身份在行贿行为、亲社会化偏好、互惠，以及收入的社会比较中的解释能力。这为社会身份理论的文献提供了新的证据，展示了身份是如何影响经济活动的结果。

尽管呈现在本论文集中的结果阐述了社会身份的经济重要性，未来需要更多的与本论文相同志趣的工作。比如，研究者们可以考虑在行贿行为里的“自己人”的长期作用，或者在社会地位无法被公开化情况下的高社会地位的影响，又或者是提供用关系强弱定义的同事的工资信息带来的影响，等等。这个框架也许能拓展我们对于社会身份在经济活动中的意义的理解。
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