Cooperation in Organizations

The Effects of Obeservation, Feedback and Leadership

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Abstract

Most organizational theories argue that formal organizational structures determine or at least influence individual behavior and therewith are crucial for the performance and survival capability of the organization. This thesis contributes to the research on cooperation in organizations by investigating the effects of basic elements of organizational structure within controlled laboratory experiments. It provides insight into the underlying processes that determine the team members' responses to feedback information and to formal structures of the decision environment. The organizational dilemma that arises from team production - as a conflict between individual and collective interests - provides the framework for the experiments presented in this thesis. The results show that unconfined information that is available to every member of a team might have detrimental effects on cooperation over time while retained or exaggerated information can foster and stabilize cooperation.

Cooperation, Organization, Public Good, Team Production, Monitoring, Feedback, Leadership, Experiment

Zusammenfassung

Eine Vielzahl von Organisationstheorien geht davon aus, dass die formale Struktur der Organisation das Verhalten der Organisationsmitglieder bestimmt oder zumindest beeinflusst und damit von großer Bedeutung für die Leistungs- und Überlebensfähigkeit der Organisation ist. Die vorliegende Arbeit leistet, durch die Untersuchung der grundlegenden Elemente dieser formalen Struktur in kontrollierten Laborexperimenten, einen Beitrag zum Verständnis von Kooperation in Organisationen. Die Ergebnisse erlauben einen Einblick in die zugrundeliegenden Prozesse, die die Reaktion der Teammitglieder auf Feedback-Informationen und formale Strukturelemente bestimmen. Es zeigt sich, dass die vollständige Information aller Teammitglieder negative Auswirkung auf den Kooperationsverlauf haben kann, während zurückgehaltene bzw. geschönte Informationen die Kooperation verstärken und stabilisieren.

Kooperation, Organisation, Public Good, Teamproduktion, Beobachtung, Feedback, Führung, Experiment

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Chapter 1

The effect of observation and rewards on cooperation[1]

> ...every team member would prefer a team in which no one, not even himself, shirks ...(*Armen A. Alchian and Harold Demsetz*)

1.1 Introduction - Team production as social dilemma

Team production, as described by Alchian and Demsetz (1972), is the joint use of inputs that are owned by different individuals. The result of team production is for one thing characterized by the fact that it exceeds the sum of individual contributions. At the same time the collective result does not allow any conclusions about the team members' marginal contribution. Therefore observing the team output and rewarding the members with regard to their collective result is no means to establish a reliable incentive structure. At the same time another problem arises from the structure of team production. Since the marginal contributions of the group members cannot be detected at zero costs team production fosters free riding on other team members' contributions as long as the shirker can expect that the reduced team output is not attributed to his action. In consequence, it is necessary to observe team

[1] This chapter is based on the working paper "Does management matter? The influence of observation and rewards on cooperative behaviour", joint work with Bettina Rockenbach and Peter Walgenbach. All authors contributed equally.

members' input behavior in order to establish a motivating connection between input and payoff and to prevent free riding. According to Alchian and Demsetz this should be done by a central monitor (hereafter the manager) who is rewarded with the residual[2] as an incentive for committed observation. In addition the manager decides about the team members' individual rewards and owns the right to sanction uncooperative behavior. Although observing input behavior and sanctioning shirking creates costs and therefore reduces the benefit of team members, they might agree to - or even choose - the appointment of a manager. Since “every team member would prefer a team in which no one, not even himself, shirks”[3] it seems plausible to expect the team members' willingness to comply with observation. We conducted an experiment to investigate whether subjects.

A similar argumentation can be found in several theoretical and experimental studies concerning the provision of public goods. Subjects confronted with the decision whether to invest their money in a public good or keep it for their private utility tend to free (or cheap)[4] ride on the contributions of their team mates (Ledyard 1995; Chaudhuri 2011). Every team member seeking individual payoff maximization will keep all units, such that the beneficial team output would not be produced. The most widespread means to counteract this behavior in laboratory experiments is punishment, either by a centralized institution or via decentralized peer-punishment that is executed directly by the team mates. A second distinctive feature of sanction mechanisms is the way they are implemented. It might be an exogenous part of the experimental design or endogenously formed or chosen by the participants.

A first strand of literature on punishment in voluntary contribution situations focuses on the exogenously imposed and decentralized alternative enforced by the group members themselves. Experimental results show that this is a feasible mean to counteract free riding and supports stable cooperation (Ostrom, Walker, and Gardner 1992; Fehr and Gächter 2000).

At the same time recent studies also point on the limits of peer-punishment

[2] The manager's residual is assumed to be created through the fact that his work reduces shirking among the team members and therewith extends the team output.

[3] Alchian and Demsetz (1972): 790

[4] In contrast to the free rider the cheap rider does not contribute zero but tries to contribute less than his teammates. There is experimental evidence that also a considerable number of conditional contributors behaves this way (see for example Fischbacher and Gächter (2010)).

and showed the problems and malfunctions around that mechanism, like reduced efficiency, misdirected and perverted punishment (Cinyabuguma, Page, and Putterman 2006; Carpenter 2007; Nikiforakis 2008), or detrimental effects on altruistic cooperation (Fehr and Rockenbach 2003).

Another part of the literature focuses on centralized and exogenously imposed sanction. Two prominent examples of these mechanisms are the Groves-Ledyard (Groves and Ledyard 1977) mechanism and the Falkinger mechanism. The former was investigated by (Chen and Plott 1996) and appeared as a solution for the free-riding problem as long as the punishment is sufficiently high. The Falkinger mechanism (Falkinger 1996; Falkinger, Fehr, Gächter, and Winter-Ebmer 2000) also enhances cooperation by automatically rewarding players who contribute above the group average and punishing those who contribute less than the average. Both mechanisms, however, ignore or at least under represent the costs of implementing and enforcing the sanctioning institution.

Gürerk, Irlenbusch, and Rockenbach (2006) report similar results for an endogenously chosen peer-punishment institution. The authors offer the subjects the opportunity to decide whether they play the public good game in a sanction-free world or within an individual punishment environment. Like the team members modeled by Alchian and Demsetz, the majority of the subjects choose the institution that provides the possibility of observing and sanctioning others behavior.

A study presented by Kosfeld, Okada, and Riedl (2009) investigates the endogenous formation of a centralized sanctioning institution and reveals that subjects are able to successfully form such institutions and therefore increase the efficiency of public good provision. In comparison to the afore-mentioned sanction mechanisms this study uses a considerably stricter one. Once established the institution does not allow any deviation from contributing the whole endowment. In fact, the subjects do no longer choose the amount of tokens they contribute but agree to stick to full contribution decision by voting for the institution.

Following this classification the design of our study can be described as an endogenously chosen and centrally executed sanction mechanism. Unlike the design presented by Kosfeld et al. (2009) the subjects in our study are not restricted to a certain contribution decision after electing the central institution.

In contrast to Fehr and Gächter (2000) and other peer-punishment experiments the subjects participating in our study cannot individually sanction uncooperative group members. Instead they can choose to assign this right to a manager who is rewarded for observing the team members input. Unlike most of the studies mentioned above, the sanction mechanism used here cannot efficiently annihilate or create payoff tokens. Instead the sanction mechanism rewards the group member(s), who contributed the most by reallocating the other part of the team surplus. The remaining part of the surplus is given to the manager not only to represent the costs for sanctioning that are usually considered in experiments. It is also given to take the costs of observing individual contribution into account that are regularly ignored when individual information is exogenously provided at zero costs.[5]

In our study we address three main questions to shed some light on individual behavior in a team production process modeled as a social dilemma situation. The first question is whether the subjects actually are able to choose an external institution that observes their input behavior and rewards high contributors but at the same time reduces the net benefits from team production. This question is directly motivated by the argument that shirking in team production necessarily leads to the need for a monitoring institution (Alchian and Demsetz 1972).

The next question concerns the actual influence from an appointed manager on team members' input behavior. According to Alchian and Demsetz, the existence of a manager will reduce or even eliminate shirking. In terms of the public good game the consequence would be an increase in contributions.

Numerous experiments have shown that in the absence of any institutional norms and regulations team performance and accordingly the provision of the public goods is suboptimal and deteriorates over time (for an overview see Ledyard 1995; Chaudhuri 2011). On the other hand there is also an extensive amount of literature providing evidence for the effectiveness of punishment mechanisms (see the examples above). In contrast to the efficient annihilation of payoff tokens in that literature, we conjecture that a less rigorous sanction

[5] In order to allow peer-punishment subjects need to know the individual contribution of their team members. According to Alchian and Demsetz (1972) this information is not costless. In fact it is part of the problem that creates the necessity of centralized monitoring.

mechanism leads also to higher contributions and on top of that it avoids the losses connected with punishment.

Finally, we will examine whether the manager's influence on subjects' contribution behavior is altered by subjects' voting decision. This could be the case if the group members' commitment to the team project is higher after actively voting for a central sanctioning institution than after voting against it. Since deciding against a sanctioning institution seems to be a good predictor for subjects' intention to contribute zero or very small positive amounts (see Gürerk et al. 2006), voting against the manager might also lead to a lower level of individual cooperation.

The experimental design, that allows us to answer the questions stated above and to test the conjectures of Alchian and Demsetz, is explained in the next section 1.2. Additional predictions with regard to the social dilemma structure of the game are described in section 1.3. We present the results in section 1.4. They show that the theory of team production can be widely confirmed by our data. The possible reasons for the remaining differences are discussed in the concluding section 1.5.

1.2 Experimental design and procedure

The experiment was conducted in the eLab[6] at the University of Erfurt. A total of 96 students from different disciplines participated in the experiment. Students who had been involved in similar experiments were not allowed to participate. The participants were randomly invited via Orsee (Greiner 2004) and distributed to the control treatment and the two manager treatments. In each treatment, we collected eight independent observations each with 4 subjects. Each subject was placed in a cubicle equipped with a computer which was connected to the experimenter's server. The experimental software was written with z-tree (Fischbacher 2007). The period payoffs were accumulated to a total payoff. The amount of earned tokens was converted into Euro at an exchange rate of Euro 1 for 70 tokens and paid to the participants at the end of the experiment. One experimental session lasted about an hour and a half and subjects earned on average Euro 12.20. This basic design and the

[6] Laboratorium für experimentelle Wirtschaftsforschung

described parameters were identical in all treatments. Written instructions (see appendix A.1.1) were handed out to all subjects and read aloud by one of the authors to publicly provide information about the payoff function, individual endowments and group size. All decisions and payoffs were completely anonymous. Thus, the participants were not able to draw any conclusions on the individual contribution of their group members in any treatment.

1.2.1 Control treatment (CT)

In each of the 20 identical rounds, each participant is endowed with 20 tokens (e_i) and then decides on the amount he or she wants to contribute (c_i) to the group project.[7] Every token invested in the group project was deducted from the contributor's private account and generated a surplus for the entire group (i.e. it was multiplied by 1.6). The group account was evenly distributed among all four group members (N). Hence, subjects' payoffs were known to be calculated according to the following payoff function:

$$\Pi_i = e_i - c_i + C_j, \text{with} C_j = 0.4 \sum_{i=1}^{4} c_i \tag{1.1}$$

After all subjects had entered their decision, they were informed about their individual payoff and the sum of contributions in their group.

1.2.2 The manager treatments (M4 and M2)

The subject's willingness to hand over the sanction possibilities to a manager and the expected change in contribution behavior is investigated in two treatments. Within each treatment the periods consist of two stages. The first one is identical with the situation described for the control treatment. The only difference is the modification of the multiplier (used to generate the surplus of team production) in order to implement (different) costs of monitoring into the experiment. Therefore only the simple sum (1 out of 1.6) of contributions is evenly distributed among the four team members. The remaining part of the original multiplier (0.6 out of 1.6) is distributed between the manager and those team member(s) who contribute the largest amount (n).

[7] Only integer numbers between 0 and 20 were accepted as valid inputs.

However, these changes did only take effect if the members of a group decided to hire the manager. The election was conducted with a simple majority rule at the end of every round after all subjects were informed about the average contributions and their individual payoff in the current period. If at least 3 out of 4 team members chose to vote for the manager, she was appointed for the following period.

The distinguishing feature of the manager used in the two treatments is the way she observes and rewards the team members' behavior and the costs caused by her work. In the M4 treatment, the manager observes the individual contribution of every team member and reallocates 0.2 times the sum of all contributions to those who contribute most. The remaining 0.4 represent her costs and are taken by the manager.

In the M2 treatment this ratio is reversed, i.e., the costs for observing are represented by one third of the team surplus (0.2) and the remaining surplus (0.4) is distributed to the high contributors. As a consequence of the lower costs the manager in this treatment is not able to observe every single contribution decision. Actually she only reveals the contributions of two employees directly and guesses the remaining two by assuming that both unobserved group members choose the same amount of tokens to put into the team project. Knowing the sum of tokens invested to the group account she is than able to rank the group member according to their contributions. Separating the sum

	CT	M4	M2
Manager available	no	yes	yes
Marginal per capita return	0.4	0.25	0.25
Management costs		0.20 C_i	0.40 C_i
Reward for top contributor(s)		0.40 C_i/n	0.20 C_i/n
MPCR for one top contributor		0.45 C_i	0.65 C_i
Observed group members		4	2
Idependent observations	8	8	8

Table 1.1: Summary of treatment design

of contributions from the surplus generated by team production is equivalent to the conception of the manager as residual claimant presented by Alchian and Demsetz (1972). Although they assign the whole residual to the manager, Alchian and Demsetz (1972) also admit that she owns the right to reallocate it to the team members. Moreover, giving the entire residual to the manager would destroy the incentive structure of the public good game. Therefore the surplus of team production is divided as described above to consider the costs of management as well as the incentives for the team members. The treatment variation can also be seen as different quality levels of management with respect to observation ability and costs. Table 1.1 summarizes the different treatment conditions.

1.3 Predictions

First of all we expect the contribution behavior in the control treatment (CT) to be in line with former results (Ledyard 1995; Camerer 2003; Chaudhuri 2011), i.e. subjects start contributing a positive amount of tokens but decline their contributions over time. As there are no means to assist or force cooperation there is no reason to expect higher contributions or stable cooperation in this repeated game.

1.3.1 Voting and contribution incentives

To appraise the rate and stability of cooperation in the manager treatments it is necessary to take a closer look on the voting incentives given by the experimental design. As the initial payoff function (1.1) is altered in the manager treatments with regard to the manager's appointment, the participants should have clear preferences for certain states of design given their original contribution preferences. These contribution preferences are regularly used to classify subjects as free riders or conditional contributors (see for exapmple Fischbacher and Gächter 2010).[8] While the free rider contributes nothing at all and takes advantage of other group members' input, the conditional contributor matches the other members' contributions over the entire strategy space. We restrict the description of the incentives on those two types of players.

[8] There is at least one other observable type of player that is characterized by a hump shaped response function, i.e. they behave like conditional cooperators until the median of the strategy space is reached and thereafter steadily reduce their contribution. Since only a smaller number of subjects reveal such preferences (about 14% reported in Fischbacher, Gächter, and Fehr (2001)) we forego the further considering of this type of player.

Since the initial and alternate state and the corresponding payoffs are common knowledge every subject can calculate its benefit from acting in one or another state. In all treatments the payoff function is given by equation 1 as long as there is no manager hired. The payoff function in M4 is given by equation 1.2 if the manager is hired where n is the number of top contributors. In the treatment with a manager who observes only two team members directly (M2) the payoff function is given by equation 1.3 as long as the manager is active.

$$\Pi_i = \begin{cases} e_i - c_i + \frac{1}{4}C_j + \frac{0.2}{n}C_j, & \text{for (one of) the top contributors} \\ e_i - c_i + \frac{1}{4}C_j, & \text{for all other group members} \end{cases} \tag{1.2}$$

$$\Pi_i = \begin{cases} e_i - c_i + \frac{1}{4}C_j + \frac{0.4}{n}C_j, & \text{for (one of) the top contributors} \\ e_i - c_i + \frac{1}{4}C_j, & \text{for all other group members} \end{cases} \tag{1.3}$$

The last term in equation 1.2 and 1.3 is only paid to the subject(s) with the highest contribution. If the manager identifies two or more subjects as the top contributors, this part of the surplus is evenly divided among those players. In general every subject that expects its own contribution to be marginally higher than the positive contribution from the second-best teammate should prefer (see 1.4: MPCR $0.4 < 0.45$) the existence of a manager. Because for every $C_j > 0$ the profit Π_i* is always higher than the profit Π_i (see equation 1.4). In contrast to that, every subject that expects its own contribution to be as high as the one form the second-best teammate has no incentive to vote for the manager (see 1.5 MPCR $0.4 > 0.35$) in M4. In M2 the incentive to vote persist as long as the number of top contributors is smaller than three (MPCR $0.4 > 0.383$).

The free-rider has no incentive at all to vote for the manager because both sanction mechanisms (M2 and M4) reduce the possibility to benefit from the others contributions (MPCR $0.4 > 0.25$). From that point of view voting for the manager can also be seen as a means to discipline free-riding group members if we allow for social preferences. Assuming that a subject is inequity avers and suffers if other group members receive higher payoffs than the subject itself (see Fehr and Schmidt 1999; Bolton and Ockenfels 2000); than the appointment of a manager offers contributors an additional way to ease their possible suffering from payoff disadvantages. Besides reducing the inequity by lowering the own contribution, cooperative players can shorten the distance to the free riders by (partly) excluding them from the benefits of the team project and use the potential observation as a threat of punishment.

$$\Pi_i = e_i - c_i + 0.4C_j < \Pi_i* = e_i - c_i + 0.25C_j + 0.20C_j \tag{1.4}$$

$$\Pi_i = e_i - c_i + 0.4C_j < \Pi_i* = e_i - c_i + 0.25C_j + \frac{0.2}{2}C_j \tag{1.5}$$

Nevertheless, the original Nash equilibrium of the public good game is not altered by one of the presented designs (M4 and M2). Voting against the manager and contributing nothing is the only strategy that leads to strategically stable payoffs.

The alternative strategy to contribute marginally above the second-best contributor (to gain the additional part of the surplus) does not lead to a higher payoff until the sum of other contributions (C_{-i}) is larger than 2.75 times the own contribution in M4 ($C_{-i} > 0.875c_i$ in M2, see Appendix A.2.1). If this is common knowledge every subject has an incentive to deviate by contributing one additional token to become the only top contributor (1.6 is always smaller than 1.7). Given the limited strategy space (0-20 tokens), this incentive does no longer exist if everybody contributes the entire endowment.

$$\Pi_i = e_i - c_i + 0.25C_j + \frac{0.4}{n}C_j, \text{with} \quad n > 1 \tag{1.6}$$

$$\Pi_i^* = e_i - (c_i + 1) + 0.25(C_j + 1) + \frac{0.4}{n^*}(C_j + 1), \text{with} \quad n^* = 1 \tag{1.7}$$

However, at this point each subject has an incentive to free-ride. If this again is common knowledge no subject has incentive to vote for the manager and as consequence there is also no incentive to contribute anything. Due to the managers estimation in the M2 treatment it is possible that a high contributor is not rewarded if the manager guesses his contribution together with the contribution of a free rider. Since the observed subjects are randomly chosen and the players do not know the probability of being observed, the only difference to M4 is an additional uncertainty that does not change the incentives for a rational, selfish, and risk-neutral player.

Beside these formal incentives provided by the experimental design there might be also a more implicit influence from the vote for a central sanctioning authority. Considering the voting decisions as a signal or a promise for higher contributions might also help to overcome the dilemma structure of the public good game and stabilize the cooperation within the teams. The results presented by Gürerk et al. (2006) show that the initial contributions are higher after the subjects entered the sanction world although they had not yet experienced any punishment. A deliberate vote for an institution that observes and judges all team members could not only be seen as a means to discipline the others but also as a way to establish cooperation as a social norm that binds the voting subject. On the other hand it is also conceivable that the announcement effect of the vote is strategically abused by the free riders to misguide the team mates and benefit from their higher contributions.

$$\Pi_i = e_i - c_i + \frac{1.4}{4}C_j + \frac{0.2}{n}C_j - \frac{0.4}{4}C_j < \tag{1.8}$$

$$\Pi_i^* = e_i - (c_i + 1) + \frac{1.4}{4}(C_j + 1) + \frac{0.4}{n^*}(C_j + 1) - \frac{0.4}{4}(C_j + 1) \tag{1.9}$$

Up to now we focused on the incentives created by the rewards but as a consequence of the payoff function (see 1.2 and 1.3) the management costs rise together with the contributions. Every additional token contributed to the team project enlarges the manager's share of the team surplus by 0.4 token in M4 and by 0.2 in M2. Equation 1.8 (with $n^* = 1$) shows that the best response function described above persists because the monitoring costs, just like the sum of contributions, are equally divided among the team members. In the M4 treatment (0.4), as well as in M2 (0.2), the marginal increase in the costs is always below the marginal increase in the share of the sum of contributions.

As shown in equation (1.10), the same applies for the group payoff.

$$\Pi_j = E_j - C_j + 1.4C_j + 0.2C_j - 0.4C_j \tag{1.10}$$

$$\Pi_i^* = e_i - (c_i + 1) + \frac{1.4}{4}(C_j + 1) + \frac{0.4}{n^*}(C_j + 1) - \frac{0.4}{4}(C_j + 1) \tag{1.11}$$

Again the marginal increase in the sum of contributions exceeds the one in monitoring costs. Nevertheless the group, as well as every team member, is better off if the same contributions are provided without monitoring.

Considering the voting and contribution incentives explained above, former experimental results (especially Gürerk et al. 2006; Kosfeld et al. 2009) and the signaling effect just mentioned there are several conceivable scenarios for the repeated game.

1.3.2 Cooperation without manager

If the very possibility of implementing a central sanctioning institution fosters cooperation, subjects should choose to contribute more in the manager treatments than in the control treatment without any voting. Fehr and Gächter (2002) report a similar effect if subjects have the opportunity to punish uncooperative behavior by reducing the free riders payoff. Even the mere opportunity of punishment fosters cooperation and reduces the number of free riding decisions in the early repetitions of the game. In this case even the teams' payoff will be higher than in CT since no costs accrue for observation and rewarding. According to what is known from the afore-mentioned repeated public good experiments this should not prevent contributions from declining over time. Therefore the positive influence from potential observation and sanctioning should not rule out actual manager appointments.

1.3.3 Cooperararation with manager

If there is no positive effect from potential observation, subjects should recognize a declining amount of contributions already in the first periods (Ledyard 1995; Chaudhuri 2011). The non-free-riding team members might then start voting for the manager and compete for the surplus generated and exclude the free-riders. Fehr and Gächter (2002) report that subjects are willing to counteract uncooperative behavior even if this reduces their own payoffs. If the manager is appointed subjects might raise their contributions to be rewarded by the manager (see equation 1.8). If players do not care about the costs and do not contribute their entire endowment, they might continue voting for the manager and remain on a high level of cooperation but on a low level of efficiency in terms of group payoff.

This case might occur as long as subjects' disutility from payoff inequity outruns the costs of observation. Since the degree of aversion against payoff inequity differs between the subjects (especially Fehr and Schmidt 1999; Bolton and Ockenfels 2000) it can only be used to explain subjects behavior if the individual parameters are elicited. For that reason we forego the further consideration of these models and leave this question open to further research.

1.3.4 Cooperaration beyond manager

An alternative path might be reached if subjects realize the cost advantage of cooperation without observation. After some rounds under observation players may find the contributions considerably higher while the manager is hired and they compete for the surplus (see equation 1.8). From here on, subjects do not necessarily need to continue with permanent monitoring to establish cooperation. They also might skip the monitoring to save costs and rely on the threat of future manager engagements as described above. In contrast to the contribution without manager situation, the threat of reappointing the manager might have a stronger influence on contributions since the free riders already experienced the sanction mechanism.

To sum up our predictions, with regard to our research question stated at the end of section 1.1, we expect a frequent use of the sanctioning institution in both manager treatments and a higher number of manager appointment in M2 where the costs incurred by the manager are lower. Regarding the second research question we expect subjects' contributions to be higher whenever the group is under observation. Further we expect the contributions in the manager treatments to be higher than in the control treatment even if the manager is actually not hired. Finally we expect the managers' positive influence to be higher on those subjects who actually voted for his appointment.

1.4 Results

The first question to answer is whether the subjects are really willing to put themselves under the observation of the central sanctioning authority. Figure 1.1 shows the relative frequency of positive votes and actual manager appointments. The differences between M2 and M4 are significant for the positive votes ($p < 0.001$)[9] as well as the number of actual manager appointments ($p < 0.001$). The gap between votes and appointments in M4 is can be explained as result of the strict voting rule.[10] The difference in manager appointments between M4 and M2 supports the assumption that subjects are aware of the costs of control. After all it can be stated that considerable number of subjects indeed complies with the observation by a manager as it is predicted by Alchian and Demsetz (1972). According to our predictions (see equation 1.6) this also points to subjects' intention to contribute some positive amount of token to the public good.

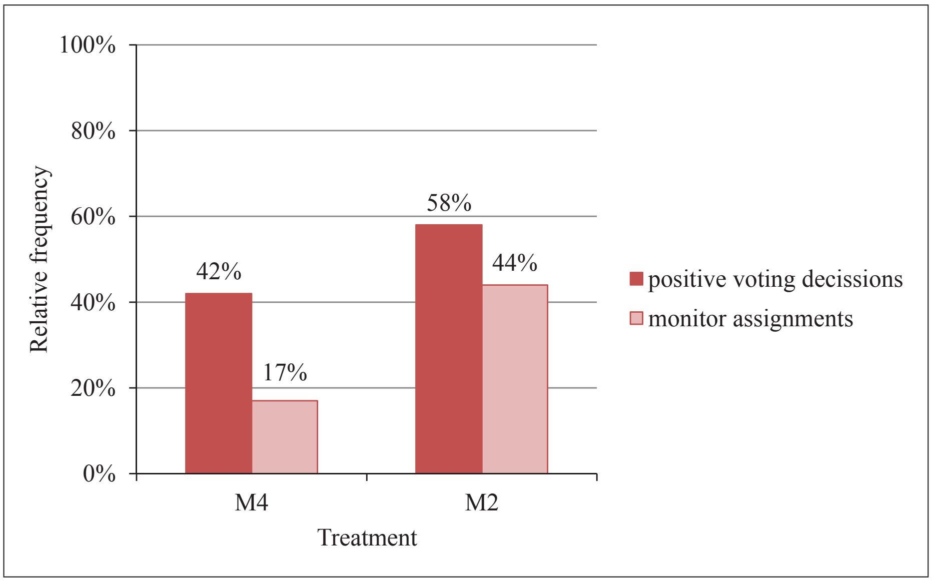

Figure 1.1: Positive voting decisions and manager appointment in M4 and M2

Taking a closer look to the voting decisions in the particular groups reveals that subjects are not able to coordinate on a stable use of the manager. On average the manager observes only 1.08 rounds in a row in the M4 treatment

[9] All comparisons between two independent Samples are tested with a Mann-Whitney-U-test (exact), two-tailed

[10] In the M4 treatment 21% and in the M2 treatment 18% of the positive votes vanish in tie results.

and 1.74 rounds in M2. The longest employment lasted 5 rounds and was reached only once by a single group in the M2 treatment.

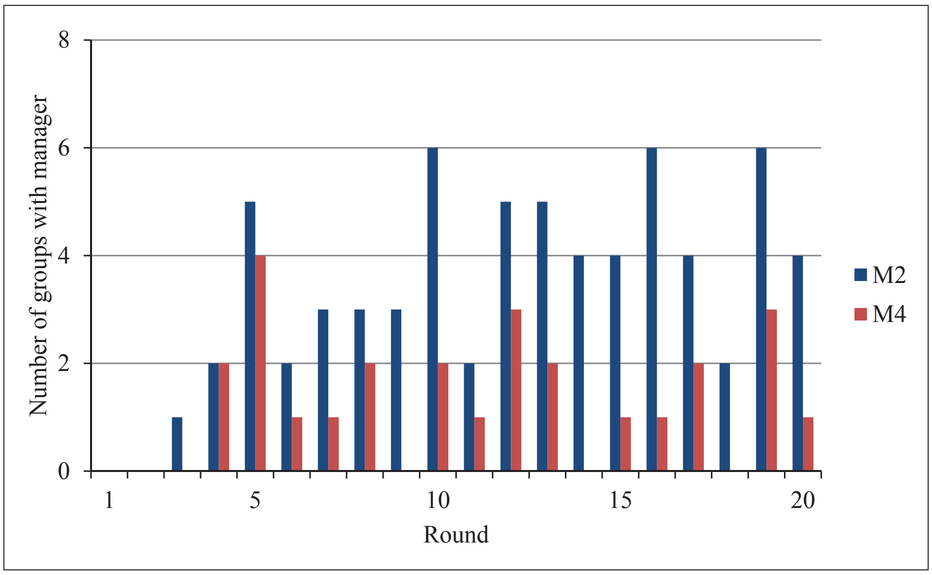

Figure 1.2: Manager appointment over time in M4 and M2

At the same time the number of rounds without observation between two manager appointments is 2.8 (M4) and 1.65 (M2) on average. One group in the M4 treatment played the entire experiment without appointing the manager. Figure 1.2 shows the number of groups that appointed a manager in the particular round and confirms the alternating pattern from described above. It also reveals a remarkable increase of manager appointment in M2 between the first and the second half of the game. The corresponding nonparametric test confirms the difference at $p < 0.05$.[11]

In the following part of the result section we compare the effect from of observation within the two manager treatments as well as between those and the control treatment.

Figure 1.3 shows the average contributions in the control treatment and the ones in M4 which are divided in contribution decisions under observation and those that have been done in the absence of a manager. It should be mentioned that the lines in Figure 1.3 as well as the group averages used in the Mann-Whitney-U-test consist of different numbers of independent observations

[11] All comparisons between two related samples are tested with a Wilcoxon-signed-rank test (exact), two tailed

depending on whether the group hired a manager or not.[12] Therefore the dashed line - representing contributions under observation - is disconnected for some rounds. Comparing the contributions in CT and M4 confirms the assumption that the existence of manager raises the cooperation rate. On average the contributions in M4 (6.9) are significantly higher ($p < 0.01$) than in the control treatment (3.8). This difference also maintains if we focus on the groups that did not hire a manager in the M4 treatment. Even without actual observation subjects contributed significantly more ($p < 0.05$) in M4 (6.5) than in CT. In other words, the mere opportunity of observation and rewarding fosters cooperation.

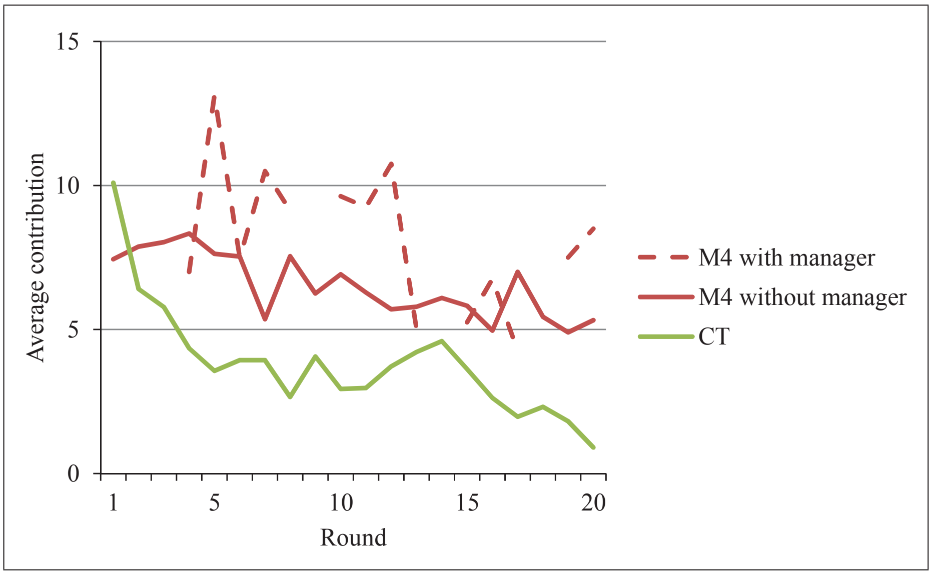

Figure 1.3: Average contributions in M4 and CT

Comparing the contributions within the M4 treatment reveals an additional increase in cooperation. If the manager is actually hired by the majority of one group the average input to the public good is 8.7 (44%) and therewith significantly above ($p < 0.05$) the average in the same group without observation. It is important to mention again that there are two groups in the M4 treatment that never agreed on hiring a manager. Nonetheless those subjects contributed 7.8 (39%) token on average and therewith match the treatment average.

[12] To dispel the worries about the equal variances assumption underlying the U-test, we conducted some Levene-tests and find the smallest probability > F at 0.272. Therefore we keep the H0 of equal variances and use the U-test to compare means.

In the next step we present the results of the M2 treatment in which the manager does only claim 0.2 of the surplus but on the other hand only observes one half of the group members and guesses the contributions from the other half. The pattern is similar to the one from M4. Again the average contributions (8.6 or 43%) are significantly higher ($p < 0.001$) compared to the control treatment and also the contributions without observation (6.8 or 34%) are above ($p < 0.001$) the values in CT.

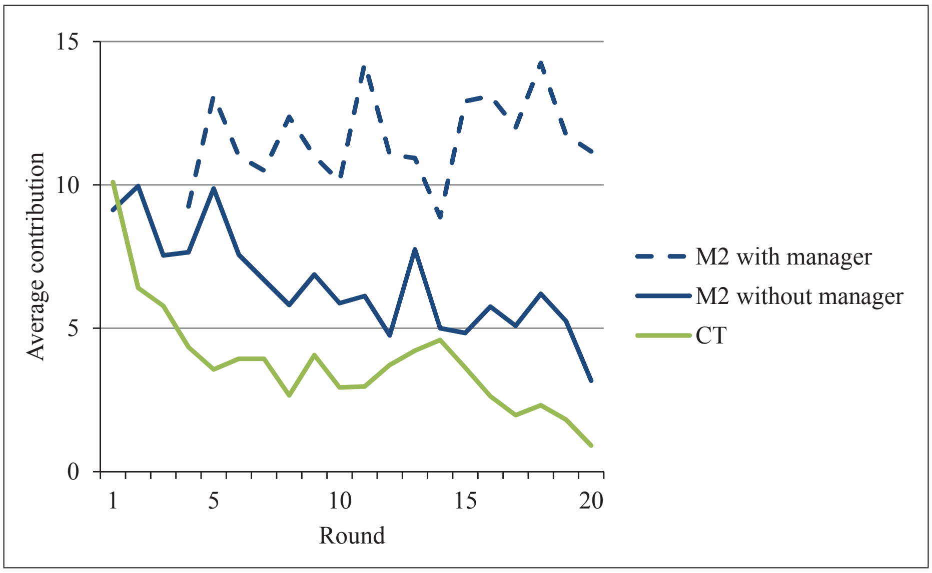

Figure 1.4: Average contributions in M2 and CT

As can be seen from Figure 1.4 the subjects in M2 contribute surprisingly high amounts of tokens (11.2 or 56%)[13] , if the manager is actually hired although the incentives for being the top contributor are below those in M4, since the manager might ignore the high contributor by guessing his contribution together with the one provided by a free rider.

To disentangle the difference between the positive effects of observation from the experience gained from the feedback about other group members contributions during the experiment we focus on the contribution difference in the first round when a group hires a manager. We find that in both manager treatments the average contributions raise with the manager's first entry.

While Table 1.2 reveals that the vast majority of subjects in both treatments contribute more if the manager comes into play for the first time, Table 1.3

[13] Which is again significantly above the contributions in CT. $p < 0.001$

shows a completely contrary result. As stated above, the manager was only hired for one round and subjects had to vote again in order to keep him or her employed or otherwise play along without observation. In the latter case subjects drop their contributions back to the original level.

	Average group contribution		
	last round without manager	first round with manager	difference (p-value)
M4	8.4	12.2	3.8 (<0.05)
M2	6.8	11.7	4.9 (<0.01)
	Subjects' response to managers entry		
	contribute more	no change	contribute less
M4	63%	8%	29%
M2	72%	6%	22%

Table 1.2: Responses to managers' first entry

This phenomenon does not only occur during the manager's first appearance but also in all later rounds.[14] Thus, subjects' behavior creates a pattern of alternating between rounds without manager and low contributions and rounds with manager and high contributions. Comparing the subjects' decisions before and after the manager is active reveals that going back to the situation without observation drops the contributions even behind the ones before the appointments of the manager. Although the differences are quite small in both treatments (0.41 in M4 and 0.42 in M2) this effect is significant ($p < 0.01$ in M4 and $p < 0.1$ in M2) and none of the 14 relevant groups contributed more than they did before they hired the manager.

Up to now we showed that the existence of a manager as well as his or her actual appointment fosters contributions in a public-good game. But being observed by a manager does not affect all subjects in the same way. Table 3 shows the results of a regression analysis that describes subjects' contribution as a function of time (round), initial contribution, feedback and observation by the manager.

[14] The average contributions in the last round before a manager was hired are 6.31 in M4 and 6.07 in M2. During the rounds with an active manager the subjects contributed 9.26 (respectively 11.53 in M2) on average. In the first round after the manager was not hired again the average contributions dropped back to 5.89 (5.64). The increase as well as the later degrease is significant in both treatments at least at the 5% level.

	Average group contribution		
	last round with manager	first round without manager	difference (p-value)
M4	11.1	7.7	3.8 (<0.05)
M2	11.8	7.2	4.6 (<0.05)
	Subjects' response to managers exit		
	contribute more	no change	contribute less
M4	25%	0%	75%
M2	19%	9%	72%

Table 1.3: Responses to managers' first exit

The initial contribution preference is operationalized as the subjects' unconditional contribution in the first round and feedback is the average contribution of the other group members in the previous round. The manager variable simply indicates whether a manager is hired or not. As can be seen from Table 1.4 manager's influence on subjects' contribution is significantly higher if the subject himself voted for her appointment while those subjects who voted against the manager raise their contributions to a lesser extent.

Dv: contribution	Spec I ($N = 608$)		Spec II ($N = 608$)	
Round	-0.155***	(0.045)	-0.157***	(0.045)
First contribution	0.557***	(0.110)	0.553***	(0.113)
Feedback	0.037	(0.059)	0.039	(0.059)
Manager	5.117***	(0.639)		
Vote for hired manager			5.501***	(1.040)
Vote against hired manager			3.707***	(1.017)
Cons	2.779***	(0.981)	2.796***	(0.971)
R^2	0.29		0.30	

Notes: GLS panel regression, random effects, robust errors (6 clusters in groups) in parentheses,* sign. at 10%; ** sign. at 5%; *** sign. at 1%. The Wald-test rejects the H0 of equal coefficients (Vote for the hired manager and Vote against the hired manager) at the 5% level. Since there is no sufficient number of observations of contributions with manager in M4,we only use the data from M2. (for M4 see A.1.3)

Table 1.4: Managers' influence on contributions

The regression also shows that the typical influence from the feedback about others players' previous contributions (see Fischbacher and Gächter 2010) is suppressed by the managers' impact. Instead of adapting their contributions towards other group members' behavior, subjects focus on whether their contribution is observed or not. As a consequence the downward trend in contributions, which results from contribution adaption towards the feedback, might be stopped or at least slowed down. If subjects do not consider their team mates' contribution decisions in the usual extent, the average contributions in later rounds should not be lower than those in the first rounds.

The results presented in Table 1.5 confirm that there is indeed no decline in subjects' contributions over time as long as the manager is hired.[15] In contrast to that the downward trend also persists in M2 if the manager is not active.

Dv: contribution	CT ($N = 640$)	M2 without manager ($N = 372$)	M2 with manager ($N = 268$)
Round	-0.254***	-0.245***	-0.002
	(0.046)	(0.029)	(0.100)
Cons	6.486***	8.938***	11.287***
	(0.536)	(0.294)	(1.115)

Notes: GLS panel regression, random effects, robust errors (6 clusters in groups) in parentheses, * sign. at 10%; ** sign. at 5%; *** sign. at %. Since there is no sufficient number of observations of contributions with manager in M4, we only use the data from M2. (for M4 see A.1.4)

Table 1.5: Downward trend in contributions

The next step is to investigate the managers' impact in terms of costs and efficiency. As described in section 1.2 the management costs are borne by the group members since one part of the return from their contributions is allocated to the manager. Thus the positive effect on average contributions is opposed by the managers' claim on subjects' payoffs.

Since the surplus from the public good is completely allocated to the four group members in the control treatment (2.29) they receive a significantly higher ($p < 0.05$) average return than the participants in the M4 treatment (1.37). In contrast to that, the total return in M4 (3.6) is significantly higher ($p < 0.05$) than in the control treatment. Due to the fact that the manager receives almost two third of the return the subjects' payoffs fall behind CT although their contributions are much higher.

As can be also seen from Figure 5 the participants in M2 receive the highest

[15] This result can also be found by using the complete number of observations in M2 and the interaction between time (round) and manager appointments as independent variable.

share in return and generate an overall return that is well above those in the other treatments. The difference in members' share in return between M2 and CT (as well as the difference between M2 and M4) is significant at the 1% level.

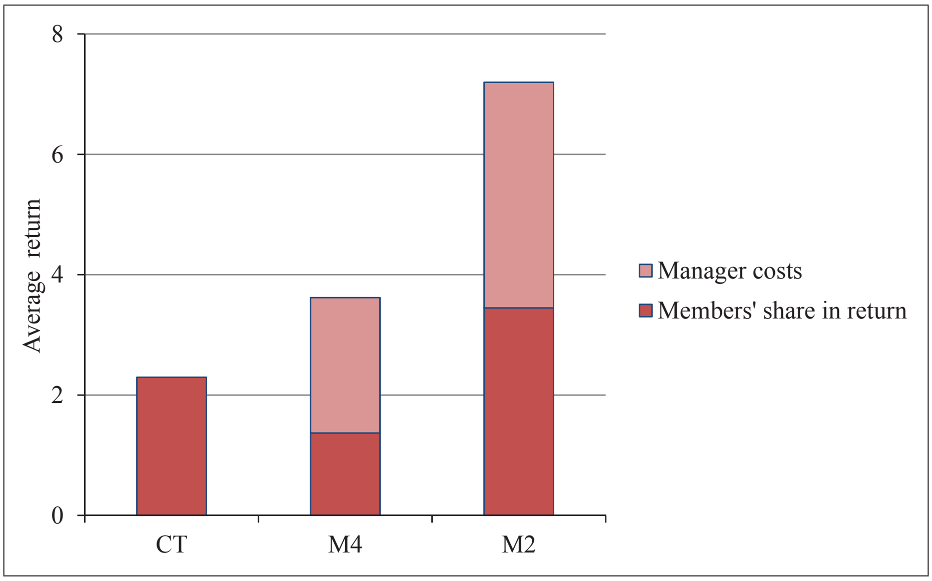

Figure 1.5: Average contributions in M2 and CT

The same is true for the whole return from the public-good game. Thus there is not only a payoff advantage for the entire group (included the manager who receives about 52% of the return) but also for the team members albeit the subjects do not benefit to the same extent. Actually the variance in payoffs is even higher in the M2 treatment (compared to CT $p < 0.05$). That is the low contributor takes more advantage from exploiting the high contributor than in CT, although he is (partly) excluded from the return. Additional support for this result comes from a comparison of variances within the treatments and between the states of observation. In both treatments, the variance in contributions (and in payoffs) is significantly higher ($p < 0.05$ in both treatments) if the manager is hired. According to our predictions derived from equation 1.4 and 1.5 the different contribution preferences should lead to different voting preferences. Our data supports this prediction at least for the first voting decision. We find a positive correlation (0.394)[16] between subjects' contribution in the first period and the vote they stated afterwards. The regression anal-

[16] Spearman-Rho, $p < 0.05$, two-tailed

ysis[17] shows that subjects' first choice has a positive influence on later voting decisions. In line with the afore-mentioned pattern of alternating manager appointment the regression also shows a negative influence from current monitoring. Given our data and the experimental design the question who votes for the manager cannot be conclusively answered. The elicitation of subjects' contribution preferences and beliefs about other team members' contributions in future experiments might help to close this gap. Compared to the widely used peer punishment mechanism (Fehr and Gächter 2002), the reallocation of the team surplus presented in this study benefits from reducing the payoff inequality from both directions. For one side the free riders are excluded from the surplus of team production. At the same time cooperative team members are compensated for the disutility they get from being exploited by the free riders. According to a theory of inequity aversion (Fehr and Schmidt 1999) it matters whether the payoff distance is reduced by lowering the free riders' payoff or by increasing the contributors' payoff. Since the subjects' utility function is assumed to be only partly determined by the relative payoffs, every increase in subjects own payoff increases subjects' utility more than a corresponding reduction of the payoffs for the free rider. The usual peer punishment mechanism reduces other team members' payoff as well as subjects own payoff regardless of the punishment efficiency. In consequence the utility gain provided through peer punishment is lower than the corresponding utility gain from a sanction mechanism that tackles payoff inequity from both directions.

1.5 Conclusion

We present the results of an experiment conducted to test the predictions of a theory of team production introduced by Alchian and Demsetz (1972). Based on a public-good game our design allows us to observe the actual willingness to be monitored by a manager and his influence on cooperation within the team. Our results support the assumption that team members indeed prefer the appointment of a manager if they had experienced free riding among their teammates before. We also find that the amount of compliance is reduced if monitoring costs are higher.

[17] For the detailed results of the probit regression see Appendix A.1.5

With view to the managers' influence on cooperation our results show that even the mere possibility of observation and sanctioning fosters contributions which rise again after the team members decided to hire a manager. We also find that subjects are not able to coordinate on hiring the manager on a regular basis. Instead they alternate their voting decisions as well as their contributions. One possible explanation can be found in the opposed voting incentives for different types of players. Although conditional contributors can hire the manager to discipline the free (or cheap) riders, they are better off if they provide the same amount of contributions without bearing the monitoring costs.

The positive influence of monitoring receives a drawback from the increasing variances in contributions and payoffs. Although the average contributions are higher in the presence of a manager, this does not lead to homogeneous cooperation. The comparatively weak punishment of free riders might explain this result. An additional treatment that uses the team surplus to punish the free riders instead of rewarding the high contributors could shed some light on this question.

Further insight into subjects' motives could be gained from the elicitation of contribution preferences and beliefs. Therewith it seems possible to answer the question whether the manager as described by Alchian and Demsetz is able to change the employees' attitude towards the organization beyond the short term changes in cooperative behavior.

Chapter 2

The effect of feedback on conditional cooperation[1]

Silence is a source of great strength. (*Lao Tzu*)

2.1 Introduction - Feedback information

Feedback about one's own decisions and especially about the decisions of relevant others is supposed to be an element of vital importance in many social interactions. Organizational theories emphasize its influence on team performance and organizational learning (Greve 2003; Vegt, Jong, Bunderson, and Molleman 2010). Educational sciences also point to the beneficial effects of feedback on sustainable learning success (Metcalfe, Kornell, and Finn 2009) and politi-cal sciences emphasize its importance for the viability of democracy (Easton 1975; Soss and Schram 2007). Immediate feedback is also one of basic elements of almost all repeated experiments in economic research and particularly in repeated public good games. Information about the other players' behavior is crucial for most economic theories of cooperation in social dilemma situations. It can be considered as a stylized fact that cooperation in repeated social dilemma situations diminishes over time, if there are no means to enforce it or to punish uncooperative actions (for a survey see Ledyard 1995; Chaudhuri 2011). There are several competing approaches to explain this phenomenon.

[1] This chapter is based on the working paper "Delayed feedback and conditional cooperation in repeated public good games.", joint work with Bettina Rockenbach and Peter Walgenbach. All authors contrib-uted equally.

One of the most frequently investigated explanation is that (I) people cooperate as long as they observe cooperation among the other group members[2] and reduce their contributions, as soon as the others do so. However, they do not perfectly match the others contribution. Instead they are supposed to display a selfish bias and contribute less than they observe (or expect) from others. The alternative approaches either invoke (II) erroneous[3] or (III) strategic[4] behavior, which induces positive contributions in early stages but vanishes in later repetitions. In each of these models the feedback information about others' behavior (or payoff) plays a crucial role, either as a necessity for belief updating or as an accelerator of equilibrium learning. We forego to consider further approaches like altruism (Andreoni 1993) and commitment (Bordignon 1990), since these models explain positive contributions as a deviation from the perfect rational and selfish payoff maximization rather than the decline of cooperation in repeated games.

Our study is focused on diminishing contributions through (imperfect) conditional cooperation, as described by Fischbacher and Gächter (2010)[5] and our research questions aim at the three basic elements that are considered in that model to explain the downward trend in repeated public good games. The first question investigates the role of feedback about others' contributions (I) that is necessary to update beliefs. Second, we investigate the effect of the elicitation of beliefs (II) that is known to affect subjects' contribution decisions in both directions. And finally, we explore subjects' contribution preferences (III) that are supposed to be the main reason for the downward trend because they are selfishly biased.

We use a standard repeated public good game (see section 2.2 for details) and extend the basic design in two directions. For one thing we implement a delayed feedback by adding further repetitions after a first phase without feedback (I). In addition, we vary the frequency of the elicitation of beliefs in order

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[2] see for example Keser and Van Winden (2000); Fischbacher et al. (2001); Croson (2007); Gächter (2007); Ashley, Ball, and Eckel (2010); Fischbacher and Gächter (2010)

[3] Andreoni (1988); Sonnemans, Schram, and Offerman (1999)

[4] Kreps, Milgrom, and RobertsRobert (1982); Andreoni (1995); Palfrey and Prisbrey (1997)

[5] Neugebauer, Perote, Schmidt, and Loos (2009) investigate the predictions of the three models in a public goods experiment with and without feedback information and do not find any support for the hypotheses of erroneous or strategic play. In line with Croson (2007) they identify conditional cooperation (reciprocity) and adaptive belief learning as the mainsprings of diminishing cooperation in repeated social dilemma situations.

to investigate the influence of explicit reflection about others' contribution behavior (II). Our results show that missing feedback interrupts the downward trend while delayed feedback hardly changes the decline in contributions. The results also show that subjects are not per se selfishly biased. The majority of subjects (about 70%) starts as perfect conditional contributors and stick to their preference until the end of the experiment. The remaining subjects contribute considerably less than they expect from other.

In the next section (2.2) we will discuss the related literature and describe the predictions and research questions derived from the model and the experimental results mentioned there. The main design elements are described in section 2.3 and the experimental results are analyzed and discussed in section (2.4). Section 2.5 provides a concluding discussion and implications for further research.

2.2 Related literature and research questions

The standard linear public good game faces the subjects with the following decision situation. The participants are randomly matched into groups of 4 players. Each player is endowed with 20 tokens (e_i) and asked to decide to either keep them or to contribute (c_i) them to a group project. The subjects can keep or contribute every integer amount between 0 and 20 tokens. Every token that is invested to the group project yields a marginal social benefit of 1.6 tokens, which is equally divided among the group members. Therefore, the marginal private return from every invested token is 0.4. If the game is played repeatedly, subjects receive feedback before they have to decide again. The payoff structure is summarized by the following function 2.1:

$$\Pi_i = e_i - c_i + C_j, \qquad \text{with} \qquad C_j = 0.4 \sum_{i=1}^{4} c_i \tag{2.1}$$

The homo economicus in a public-good game is neither influenced by any feedback nor does he adjust his behavior towards his or her beliefs about the other players' contribution. This completely rational and selfish player would simply contribute nothing. Therefore, the Nash-equilibrium in the one-shot game and also in every single stage of a finitely repeated game will be no cooperation at all (Shapiro 1989). As mentioned above, numerous laboratory public-good experiments draw a different picture. Subjects typically start with contributing about one half of their endowment to the public good and keep the other half in their private account. Without any cooperation enforcing mechanisms, like punishment or rewards, but with regular feedback about others' contributions, subjects start to decrease their input in later repetitions until there is almost no cooperation at the end of the game (Ledyard 1995; Chaudhuri 2011).

Adaptive belief learning and diminishing cooperation

Fischbacher and Gächter (2010) (hereafter FG) explain the downward trend in contributions over time with a two-stage model that is described in the following section. Subjects' beliefs about their teammates' next contributions are described as a function (2.2) of their previous beliefs (belief$_{t-1}$) and the feedback about the group members' previous contributions.

$$belief_t = \alpha_1 belief_{t-i} + \alpha_2 feedback \tag{2.2}$$

The contributions in turn are determined by subjects' belief (belief$_t$) and their predicted contribution derived from their contribution preferences that are elicited at the beginning of the game.

$$contribution_t = \beta_1 belief_t + \beta 2 predicted contribution \tag{2.3}$$

To elicit contribution preferences, the participants are asked to state their response on each average contribution (0-20 token) via the strategy method (Selten 1967). According to FG, these response functions reveal that conditional contribution is imperfect, i.e. subjects contribute below average contributions from the remaining group members. Therefore, FG argue that it is not necessary to assume free-riding since the conditional cooperators itself foster the downward trend through their selfishly biased behavior. By simulating subjects' behavior, based on the revealed contribution preferences, the authors identify this behavior as the main reason for declining contributions, even in the absence of free-riders (for details see Fischbacher and Gächter 2010).

According to this model, the given feedback does not directly enter subjects' contribution decision but is mediated through the beliefs about other players' contributions. At this point it seems plausible to assume that missing feedback will interrupt the process of belief forming and diminishing cooperation (2.2). Without any information about the group members' behavior, the subjects are neither able to update their beliefs nor is there any necessity to adapt contributions in later repetitions because the individual contribution preference should not change during the experiment. Therefore subjects' average behavior should remain on the initial level in terms of contributions as well as with view to the beliefs. It is important to mention that FG use their model to explain the behavior, which is observed in a stranger setting, i.e. the groups were randomly recomposed in every repetition. Thus subjects not only have to update their beliefs with view to their group members' behavior but also with respect to the actions of players that participate in the actual session.

However the experimental results, presented to describe subjects' behavior in repeated games without feedback, are ambiguous and do not show a clear pattern. Sell and Wilson (1991) designed one of the early public goods experiments with different feedback conditions without observing subjects' beliefs.[6] The authors report no contribution differences between the no information condition and the treatment with aggregated feedback information. They also report a downward trend in contributions in the no feedback condition. Weber (2003) presents the results of a repeated guessing game under no information conditions and shows that subjects' decisions are not stable but instead converge towards the predicted equilibrium without any feedback about other players' decisions. In contrast to Sell and Wilson (1991) he reports a significant difference between the treatment with regular feedback and those without feedback. The decline in subjects' choices over time is reported to be slower in the absence of feedback. In terms of a public good game, this might point to a (slow) decline in contributions even without information about other players, if subjects 'learn' to free-ride by simple repetition (Andreoni 1988). Again in contrast to Sell and Wilson (1991), the recent study presented by Neugebauer

[6] The authors compared the contributions in a repeated 4-person public-good game between the following three information conditions: (I) no information about other players' contributions, (II) aggregate information and (III) individual information.

et al. (2009) reports no significant decline in contributions, if there is no feedback about other group members' behavior.[7] The authors also record subjects' beliefs in every repetition of the game and report no change in the distance between beliefs and contributions over time.

On the other hand, there is also experimental evidence that uncertainty, which necessarily occurs in the absence of feedback information, reduces cooperative behavior in public-goods games. Wit and Wilke (1998) demonstrate that social uncertainty, i.e. uncertainty about their partners' cooperative behavior, has detrimental effects on cooperation. Wit and Wilke (1998) show that contributions are lower if the uncertainty about the outcome of the public good increases. A further study by Brennan, González, Guth, and Levati (2008) reveals a negative correlation between contributions and risk level in a public-good game. Finally, the results presented in chapter 4 provide further support for the detrimental effect of uncertainty on contributions. It turns out that subjects reduce their contributions, if the feedback information is fraught with uncertainty.

After all, it can be stated that missing feedback might affect cooperation in repeated interactions from two directions. On the one hand it interrupts adaptive belief learning and might therewith stop the decline in contributions; one the other hand it induces uncertainty that is known to lower contributions. Therefore we expect the contributions under missing feedback conditions to be stable over time but not considerably above those under regular feedback.

With view to the uncertainty and our second research question, it might be expected that the elicitation of beliefs contributes to a stabilization of cooperation. If subjects are repeatedly asked to state their expectations about others' contributions, they might use their beliefs to justify their own behavior and therewith reduce the uncertainty about the others. The results from a study by Schunk and Betsch (2006) suggest that deliberate decisions tend to be less risk-averse. This seems to support the assumption that an explicit reflection about others' behavior reduces uncertainty and stabilizes cooperation over time. Of course, there is no reason to assume that subjects omit concerning about other group members just because they are not asked to state their beliefs. However,

[7] Neugebauer et al. (2009) conclude that conditional cooperation and adaptive belief learning are the only plausible hypotheses to explain the downward trend in contributions under feedback conditions.

several studies point out that the elicitation of beliefs by itself has an effect on contribution decisions. Dudley (1993) and Croson (2000) report that the incentivized elicitation of beliefs leads to lower contributions compared to a condition, in which the subjects are not asked to state their expectations about others.[8] Contrary to that, Wilcox and Feltovich (2000) report no differences in a comparable setting. A recent study presented by Gächter and Renner (2010) provides evidence that also the way of belief elicitation matters for the average contribution levels. The authors show that contributions are higher if the belief elicitation is incentivized, while contributions are not affected by the non-incentivized elicitation compared to a treatment without belief elicitation. To the best of our knowledge this is the only work that investigates the effects of mere belief elicitation without any further incentives.[9] Again, the experimental results point to different directions. To gain further insight into the process of belief forming and its effect on contributions, we investigate two additional treatments with immediate feedback and varying belief elicitation. On the other hand we use the treatment without feedback and without repeated elicitation of beliefs. If there is a stabilizing effect from repeatedly stated beliefs, the contributions should decline in this treatment, unless the effect from interrupted belief updating is stronger.

As mentioned above, even conditional contributors are supposed to have selfishly biased contribution preferences (Fischbacher et al. 2001) that are revealed in the distance between their own contributions and their belief about the contributions of the other group members. If this is the case subjects' beliefs are crucial for two reasons with regard to the model by FG. For one thing, the initial belief determines the starting point of the belief formation process (equation: 2.2). On the other hand, the difference between current belief and contribution might be used to measure the contribution preference (equation: 2.3). If subjects' contribution is a weighted average of her current belief and the contribution that is predicted from the response function, than the difference between the current belief ($belief_t$) and the current contribution ($contribution_t$) should represent the contribution preference for this point on the response function. This implies that the distance between belief and con-

[8] The elicitation of beliefs is incentivized by paying the subjects additional money depending on the accuracy of their guesses.

[9] Neugebauer et al. (2009) also incentivized the elicitation of beliefs.

tribution remains stable, if subjects' contribution preferences do not change over time. We stress this stability through a higher number of repetitions and through stronger feedback with an additional summarize of aggregated results after 10 rounds.

2.3 Experimental design and procedure

A total of 72 students from different disciplines participated in the experiment. Students who had been involved in similar experiments were not allowed to participate. The participants are randomly invited via ORSEE (Greiner 2004) and matched into one treatment. The 24 subjects in each treatment are divided into groups with 4 members and solely act within these groups until the end of the experiment. Thus we collected 6 independent observations per treatment. The experiment was conducted in the "Laboratorium für experimentelle Wirtschaftsforschung (eLab)" of the University of Erfurt. The experimental software was written with z-tree (Fischbacher 2007). The period payoffs were accumulated to a total payoff. The amount of earned tokens was converted into Euro at an exchange rate of 1 Euro for 70 tokens and paid to the participants at the end of the experiment. Written instructions (A.2.1) are handed out to all subjects and read out by one of the authors. All decisions and payoffs were completely anonymous. Thus, the participants were not able to draw any conclusions with respect to the individual contribution of their group members in any treatment.

Treatments

The first treatment (B10F10) starts with 10 rounds without any feedback. Subjects' are asked to state their beliefs only once, at the beginning of round 1. This part is used to investigate the question, whether the process of belief updating is interrupted under no information conditions. At the end of round 10 the subjects received a feedback about their own average contributions (and payoff) and the average contributions from the other group members. Afterwards the second phase starts with another 10 rounds without feedback. The restart is not surprising, since it is announced in the instructions that there will be 3 phases each with 10 rounds. Each phase starts with the elicitation of beliefs and ends with an aggregated feedback. The only difference in the second treatment (B10F1) is the feedback frequency. In addition to the feedback at the end of each phase, subjects received the same information (own contribution, own payoff and average others' contributions) at the end of each round. The third treatment (B1F1) increases the frequency of belief elicitation. Subjects are now asked to state their beliefs about others' contributions at beginning of each round. The feedback frequency is the same as in the second treatment (B10F1). By comparing B1F1 and B10F10, we investigate the effect of the elicitation of beliefs. The evolution of the selfish bias is observed in the different phases of the B1F1-treatment. Table 2.1 summarizes the different feedback conditions and frequencies of belief elicitation.

Treatment	N	Belief frequency	Feedback frequency
B1F1	24	1 round	1 round
B10F1	24	10 rounds	1 round
B10F10	24	10 rounds	10 rounds
NOINFO*	18	1 round	10 rounds

* The NOINFO treatment was conducted by Neugebauer et al. (2009) and is used as a benchmark for the B10F10 treatment.

Table 2.1: Summary of experimental design

2.4 Results

First we turn to the effect of missing feedback on the decline in contributions in phase 1. As can be seen from Figure 2.1 and Table 2.2 there is no downward trend in the absence of feedback (B10F10) whereas the contributions decline in B10F1 and B1F1. The regression coefficients reveal that there is no time trend without feedback and the differences in the coefficients between the treatment without feedback and those with feedback are significant ($p < 0.01$)[10]. Thus it can be stated that missing feedback prevents the downward trend in average contributions.[11] With view to the model by FG this result might be interpreted as support for the importance of feedback for adaptive learning.

[10] Wald-test for equal-coefficients, The comparison of coefficients is still possible although single coefficients are not significant (Kennedy 2003)

[11] The observation of no significant decline is in line with Neugebauer et al. (2009) who studied a treatment comparable to our B10F10 treatment.

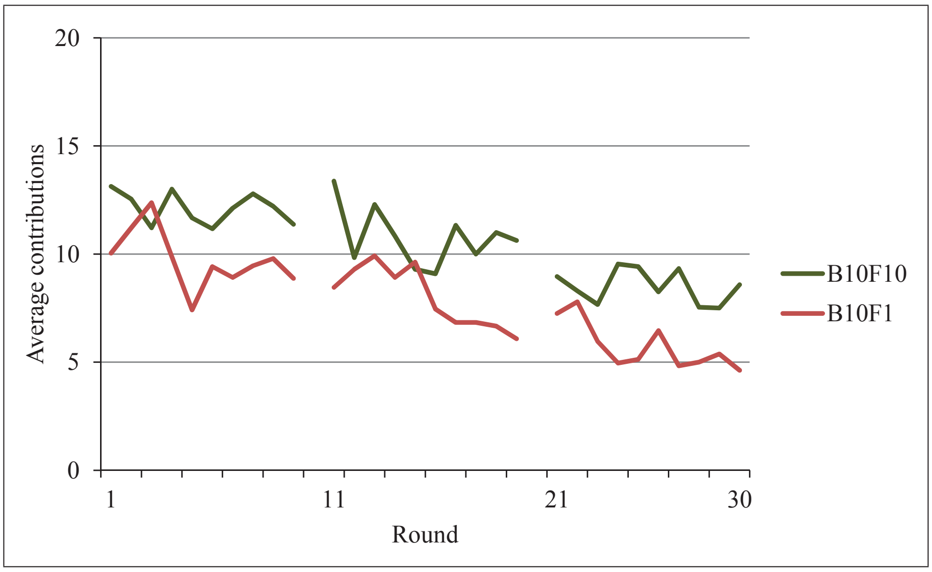

Figure 2.1: Average contributions under immediate and delayed feedback

The result also allows rejecting the hypothesis of learning without feedback (Weber 2003) in public good games. To be more precisely, it allows rejecting the assumption that learning can be observed in players' actions because considering the beliefs about others' behavior - regardless of whether they are elicited or not - might prevent subjects from playing the rational, selfish equilibrium solution.

Dv: contribution	Phase 1	Phase 2	Phase 3
Round in B1F1	-0.380***	0.286**	-0.168***
	(0.070)	(0.124)	(0.081)
Round in B10F1	-0.241*	-0.294***	-0.268***
	(0.127)	(0.079)	(0.088)
Round in B10F10	-0.004	-0.106	-0.069
	(0.089)	(0.079)	(0.084)
Const	11.312***	12.207***	12.572***
	(0.629)	(1.499)	(1.994)

Notes: GLS panel regression, random effects, robust errors (6 clusters in groups) in parentheses, * sign. at 10%; ** sign. at 5%; *** sign. at 1%. (N = 720)

Table 2.2: Downward trend in contributions in phase 1-3

The average contributions in B10F10 (12.12) are significantly ($p < 0.05$)[12]

[12] All comparisons between two treatments are tested as independent samples with the MWU-

above those in B10F1 (9.73). The variance in contributions within the groups, which might be expected to be smaller without feedback, does not differ between the treatments (B10F10: 29.67; B10F1: 36.19; $p = 0.394$).

Next we analyze the evolution of contributions in the later phases of the B10F10 treatment. As predicted by the model of adaptive learning (FG) the subjects reduce their average contributions (from 12.12 to 10.77) after the first feedback at the end of round 10. The difference in contributions between the first and the second phase is weakly significant ($p < 0.1$). In the last phase (after the second feedback) subjects contribute 8.51 tokens on average. This is again significantly below the contributions in the phase before ($p < 0.05$). As in phase one there is no significant influence of time on the contributions (see Table 2.2) in the second and the third phase.

Figure 2.1 also reveals that the decline in average contributions from phase one to three does not depend on the feedback frequency. Although the subjects in B10F10 received only delayed feedback, the distance (3.61) between the average contributions in phase one and those in phase three, is not significantly smaller ($p = 0.818$) than under immediate feedback conditions (B10F1: 4.01). Thus it can be stated that subjects adapt their behavior to the received feedback whereas they do not reduce their contributions if they have no current information about the others. In other words, adaptive learning is interrupted as long as there is no feedback but it takes place whenever it is possible and the extent of adaption is not altered by the feedback frequency.

With view to our second research question (the effect of elicitation of beliefs) we first compare the average contributions between the two treatments with immediate feedback but different belief elicitation frequencies. As can be seen from Figure 2.2, the average contributions are almost identical in B10F1 (7.83) and B1F1 (7.29). The corresponding Mann-Whitney-U-test confirms that there is no significant divergence over all three phases ($p = 0.937$). The same applies to the single phases.[13] With regard to the variance in contributions (within the groups), the assumption that the elicitation of beliefs contributes to a stabilization of cooperation cannot be confirmed. This applies to the overall variance within the groups (38.7 in B10F1 and 30.1 in B1F1)

test (exact, two tailed) and 6 independent observations in each treatment.

[13] $p = 0.699$; $p = 0.818$; $p = 1$ (phase 1-3)

as well as to each phase.[14] These findings are in line with those presented by Gächter and Renner (2010) and provide further support for the assumption that the mere elicitation of beliefs does not alter the subjects' behavior, for they consider the other group members' behavior either way.

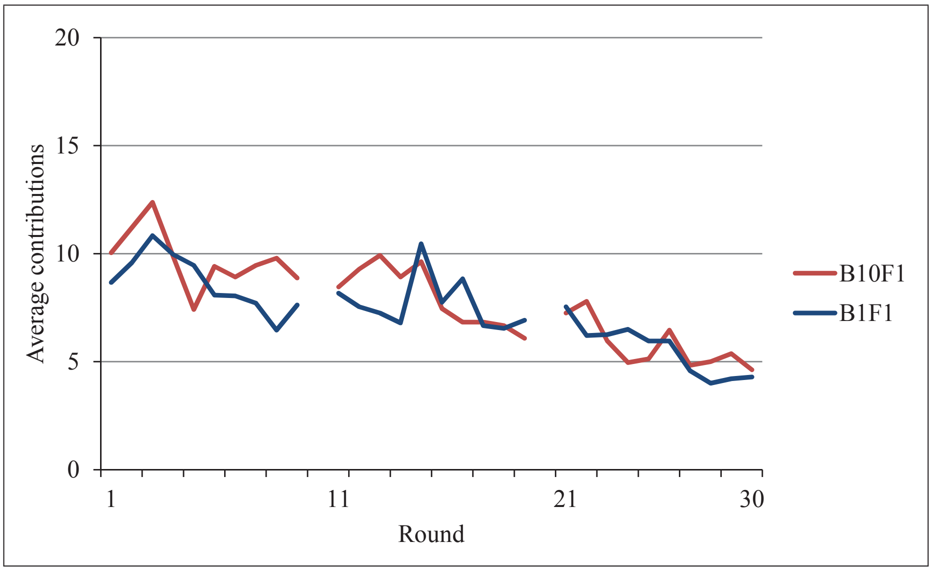

Figure 2.2: Average contributions over time in B10F1 and B1F1

Our last research question which concerns subjects' selfishly biased contribution preferences is investigated by comparing the different phases of the B1F1 treatment. Table 2.3 sheds a first light on subjects' initial difference between contribution and belief. As can be seen there, the majority of subjects (69.2%) do not have the intention to contribute below their beliefs at the beginning of the first round. Thus, most subjects are not per se selfishly biased conditional contributors. Further support comes from the evolution of the difference between average contributions and beliefs in B1F1 (Figure 2.3). Obviously there is almost no difference (-0.45) between beliefs and contributions in the first phase although subjects received a feedback after each round. The difference is twice as high (-0.97) in phase two but the beliefs are still not significantly above the contributions ($p = 0.219$)[15]. Only in the last phase there is a significant difference (-1.50) and subjects can be described as selfishly biased conditional contributors ($p < 0.05$).

With regard the model by FG these findings are surprising because subjects

[14] $p = 0.818$; $p = 0.598$; $p = 0.589$ (phase 1-3); $p = 0.699$ (overall)
[15] Wilcoxon-test for related samples, exact, two-tailed, N=6

	B1F1 (N=24)	B10F1 (N=24)	B10F10 (N=24)	CT (N=48)	Overall % (N=120)
Contribution ≥ Belief	16	17	17	33	69.20%
Contribution < Belief	8	7	7	15	30.08%

The CT treatment is a standard public good game (2 x 20 rounds) used as a control treatment in another experiment which is reported in chapter 4.

Table 2.3: Differences between initial beliefs and initial contributions

should reveal their selfishly biased preferences already at the beginning of the game and the number of subjects who contribute less than they expect from their teammates should be larger.

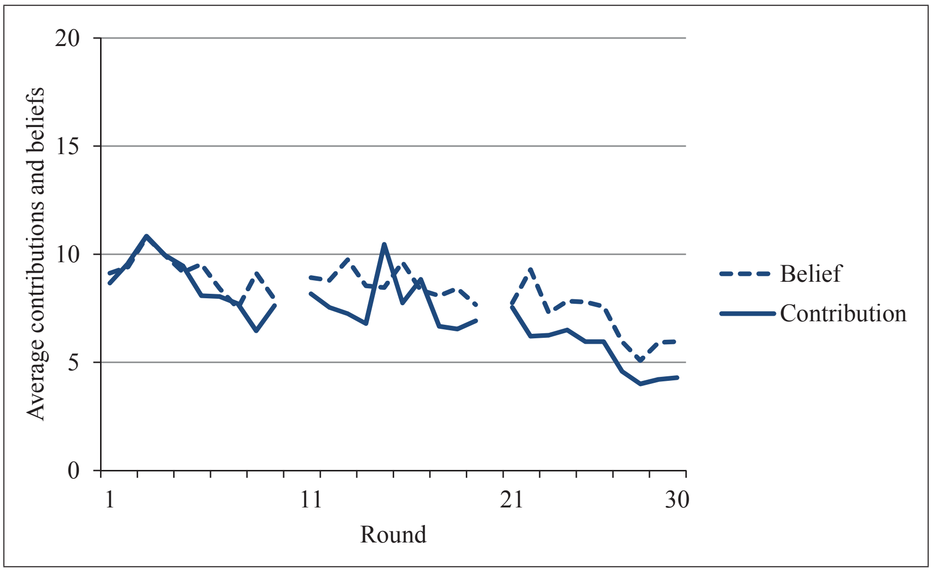

Figure 2.3: Average contributions and beliefs in B1F1

According to the model by FG there are two possible explanations for the growing differences between beliefs and contributions. First, the influence of feedback on the process of belief updating might change. As a consequence the influence of subjects' beliefs on their actual contribution decision might also change over time if subjects' initial contribution preferences become less important in later repetitions. The second possible explanation is that subjects might not only use the (negative) feedback to update their beliefs about others' future actions but also to compensate the relative 'losses' they received in the previous rounds. In other words the observed difference between average beliefs and contributions might be explained by reciprocal behavior rather than by intentional selfishness.

The panel regression presented in Table 2.4 provides support for the first assumption. As in Fischbacher and Gächter (2010) the model describes subjects' beliefs as a function of the beliefs in the previous round (Belief$_{t-1}$) and the feedback in the different phases.[16] It can be seen there that the influence of feedback is considerably larger in the second phase than in the first one.[17] The same applies on the third phase although the difference in the coefficients is smaller ($p < 0.05$). It seems that subjects belief updating changes over time.

Dv: belief	B1F1	
Round	-0.067**	(0.021)
$Belief_{t-1}$	0.405***	(0.082)
Feedback in phase 1	0.266***	(0.044)
Feedback in phase 2	0.357***	(0.030)
Feedback in phase 3	0.408***	(0.043)
Const	3.427***	(0.657)
R^2	0.40	.

Notes: GLS panel regression, random effects, robust errors (6 clusters in groups) in parentheses, * sign. at 10%; ** sign. at 5%; *** sign. at 1%. (N=648)

Table 2.4: Feedback effects on beliefs in B1F1 (phase 1 - 3)

If we follow the model suggested by FG, the updated belief enters subjects' contribution decision on top of their initial contribution preferences. In order to explore the effect of initial preferences on later contribution decisions we classify the subjects according to the difference between their beliefs and contributions at the beginning of round 1. These preferences are independent from feedback information about other group members and can be interpreted as the unconditional intention to contribute at a certain belief level. As reported in Table 2.3 less than 30% of the subjects ($B > C$; N=7) can be classified as selfish players who contribute below their belief whereas the remaining subjects ($B \leq C$; N=17) perfectly match their beliefs or even contribute above their

[16] To dispel worries about multi-collinearity we considered the variance inflation factors and find them below the critical value of 5: average vif = 2.17.

[17] Wald-test: coefficients for Feedback in phase 1 and Feedback in phase 2; $p < 0.001$

expectations about the others. Figure 2.4 compares the average contributions and beliefs of the both player types over all rounds.

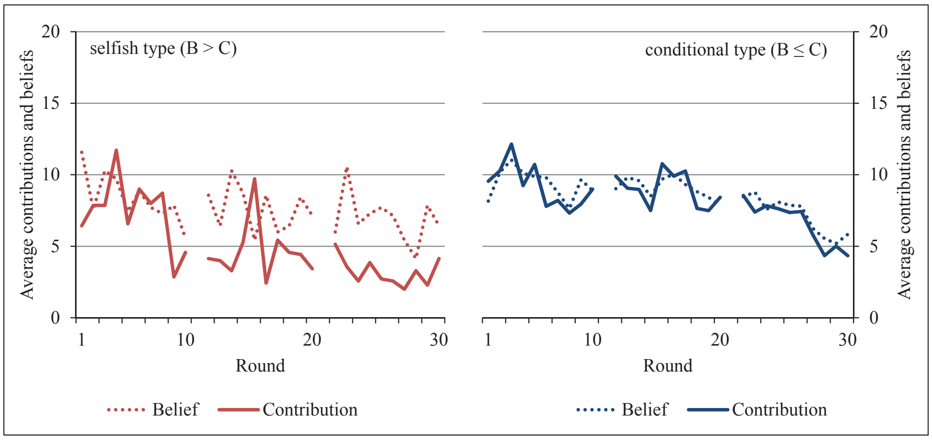

Figure 2.4: Average beliefs and contributions by type

This comparison yields two interesting results. First, the average contributions provided by selfish players (Type $B > C$: 5.08 tokens) are lower than those by subjects who can be classified as conditional contributors (Type $B \leq C$: 8.21).[18] Second, the distance between average contributions and beliefs is considerably larger (2.55) and grows over time if subjects are selfish whereas conditional contributors match their beliefs closer (0.32) in all three phases.[19] These results show that neither the selfish players nor the conditional cooperators resign from their initial contribution preferences. Nevertheless, the average contributions from conditional players decline over time and converge towards the selfish players' contributions. This seems to confirm the growing influence of feedback on the process of belief forming that is reported in Table 2.4 because the conditional players adjust their beliefs (and their contributions) to the feedback that is lowered by selfish players.

Another result from the comparison of selfish and conditional players can be seen in Figure 2.5 that only shows the last phase. While there is an obvious difference in contributions ($p < 0.1$) both types share very similar beliefs. ($p = 1$). Thus, the selfish players do not only foster the downward trend

[18] The difference in average contributions (phase 1-3) between both types of players is significant at the 10% level (Wilcoxon-test, exact, two-tailed). We use the Wilcoxon-test for paired samples because the classified subjects participated in different groups and cannot be treated as independent observations.

[19] The average distance between beliefs and contributions is also significant at the 10% level. The distance in phase 3 is significantly larger ($p < 0.1$) than in phase 1 for selfish players.

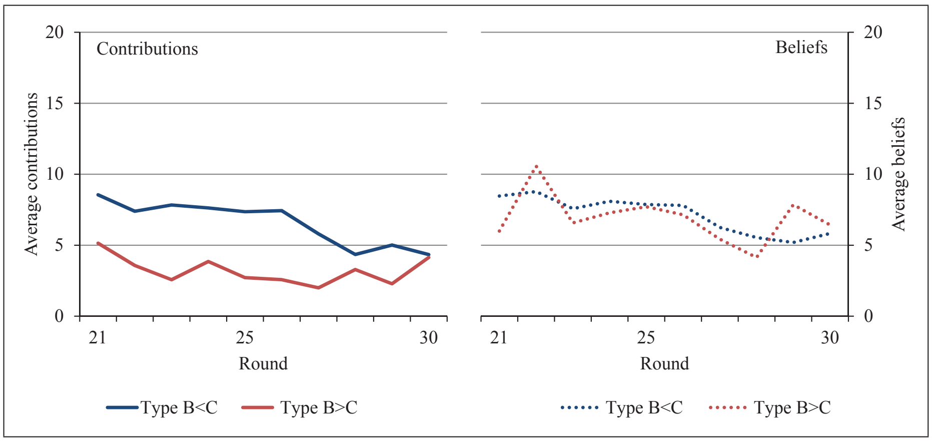

Figure 2.5: Differences in contributions and beliefs in phase 3

in contributions but are also the main explanation for the difference between average beliefs and contributions. As a consequence the differences between both types of players should be supposed in a different processing of beliefs and preferences rather than in a different processing of the received feedback. In other words, both types of players draw similar conclusions from the information they receive about their teammates but they respond with different contributions. This result receives further support from the regression that is reported in Table 5. To describe the process of belief forming we use the model that was applied in Fischbacher and Gächter (2010) but include separate independent variables with regard to the player types (indicated by S for selfish players and C for conditional contributors). It shows that the influence of feedback on actual belief does not differ between selfish and conditional subjects (Feedback_S and Feedback_C). Since the same applies to the influence of subjects' previous beliefs (Belief_{St-1} and Belief_{Ct-1}) it can be stated that both types of players update their beliefs in a similar way.[20]

The crucial difference between selfish and conditional subjects is the way in which both types of players process their beliefs in their contribution decisions. The influence of actual beliefs is considerably smaller ($p < 0.1$) for those subjects who are classified as selfish players (Belief_S) than for those who are classified as conditional cooperators (Belief_C). In contrast to that there is no difference in the influence of subjects' previous contributions ($p = 0.427$). In

[20] Wald-test: coefficients for Feedback_S and Feedback_C; $p = 0.949$; coefficients for Belief_{St-1} and Belief_{Ct-1} i; $p = 0.358$

	Dv: belief		Dv: contribution	
Round	-0.187***	(0.015)	-0.069***	(0.018)
Belief$_{St-1}$	0.371***	(0.081)		
Belief$_{Ct-1}$	0.436***	(0.102)		
Feedback$_S$	0.354***	(0.076)		
Feedback$_C$	0.347***	(0.055)		
Belief$_S$			0.253***	(0.129)
Belief$_C$			0.644***	(0.124)
Contribution$_{St-1}$			0.295***	(0.098)
Contribution$_{Ct-1}$			0.173***	(0.108)
Const	2.460***	(0.544)	2.429***	(0.847)
R^2	0.41		0.42	

Notes: GLS panel regression, random effects, robust errors (6 clusters in groups) in parentheses, * sign. at 10%; ** sign. at 5%; *** sign. at 1%. (N=696)

Table 2.5: Belief forming and contribution decision

summary, it can be stated that the impression of general selfishness among subjects conveyed by the average contributions and beliefs (see Figure 2.4) is not confirmed by our data if we consider the initial preferences. Instead it shows that the downward trend is triggered by a minority of selfish subjects.

2.5 Conclusion

We present the results of an experiment conducted to test the different elements of the process that was brought forward to explain the decline in contrxibutions in repeated social dilemma situations. Based on a standard public good game our design allows us to investigate the effects of feedback, elicitation of beliefs and contribution preferences. Our results show that missing feedback prevents the downward trend in contributions by interrupting the process of belief updating. Delayed feedback does not sustainably change this process. Subjects adjust (decline) their contributions over 30 rounds to a similar extend regardless of whether they received feedback after each round or after 10 rounds. This result emphasizes the importance of feedback learning in social interactions.

The non-incentivized elicitation of beliefs has no effect on average contributions because there is no stabilizing effect in terms of contributions or variances. From a methodological point of view, this result shows that the elicitation itself does not foster the behavior that is measures afterwards.

With view to contribution preferences we found that the majority of subjects do not show selfishly biased behavior at the beginning of the game. Therefore, selfishly biased contribution preferences as reported by Fischbacher and Gächter (2010) are not sufficient to explain the downward trend. According to our data the reason for diminishing cooperation is not the majority of subjects who contribute a little less than they expect from others but a smaller group (30%) of players with a distinctive preference to contribute below their beliefs. These selfish players cannot be observed by investigating the average contributions (and beliefs) because their beliefs do not differ from those of conditional contributors. Nevertheless, their contribution behavior seems to trigger the downward trend by creating negative feedback for the conditional contributors. Further support for this assumption comes from experiments that regrouped the participants with regard to their contribution preferences. The results show that contributions are stable if a group is only composed of conditional contributors (Page, Putterman, and Unel 2005; Burlando and Guala 2005; Gächter and Thöni 2005). However, even a group of perfect conditional contributors might reduce cooperation over time if they start with different beliefs about each other. Composing groups of (perfect) conditional contributors with different beliefs might answer the question whether cooperation can be stable if subjects start with different expectations.

There is another methodological issue that should be investigated in further experiments. Comparing the initial differences between beliefs and contributions in the B1F1 treatment with those reported by Fischbacher and Gächter (2010) reveals that there is a considerable distance already in the first round. Since both experiments used a standard linear public good with identical parameterization the only difference is the elicitation of contribution preferences via strategy method at the beginning of the game. Thus, it should be investigated whether this way of elicitation fosters selfishly biased behavior. The results also imply that investigating average behavior and beliefs is not sufficient to understand the dynamics of diminishing cooperation in repeated social dilemma situations.

Chapter 3

Feedback and leading by example

A leader is a dealer in hope. (*Napoleon Bonaparte)*

3.1 Introduction - Leading by example

Leadership is one of the most fundamental elements of organizations, not only in the field of enterprises but also with view to political and cultural institutions. According to a classical theory of the firm presented by Alchian and Demsetz (1972), leadership is a vital element in every team production. The necessity of leadership is justified with regard to the teammates' incentives to reduce their efforts and free-ride on the performance of other teammates. An effective means to overcome this inefficient situation is the appointment of a leader who has the right to sanction shirking. In this case the main source of the leaders' influence is authority. However, besides these formal elements of organizational structure like hierarchies, authority and obligatory rules, organizations can draw on personal leadership as a means to adjust their members' behavior towards the aims of the organization. It is widely accepted that leadership is more than the (re)allocation of resources and the disciplining of shirkers (Schein 2004; Bass and Riggio 2006; Kearney and Gebert 2009). Leadership also transforms the teammates' attitude through trust, loyalty and individual acknowledgement. Another element of transformational leadership is leading by example, which is described as an implicit way to change followers' behavior through the imitation of the leaders' actions (Yukl 2009). Beyond that it is not only considered as an alternative style of personal leadership but also as a catalyst for the formal elements of organizational structure (Mastrangelo, Eddy, and Lorenzet 2004). The remainder of this chapter is organized as follows. In the next section (3.2) the related economic literature is reviewed and previous result are summarized. Section 3.3 describes the treatments in detail. Afterwards, section 3.4 discusses the predictions derived from the basic public good game and the implemented leadership mechanisms. The results are presented and discussed in section 3.5. Finally, section 3.6 concludes and discusses possible implications.

The positive influence of leadership on the behavior of followers has also received growing attention in economic literature. Hermalin (1998) presents a theory of leading by example in the context of team production and shows that a leader can induce cooperative behavior by taking the first move. Vesterlund (2003) draws similar conclusions from modeling fundraising behavior. She demonstrates that announcing the first contributions can help to foster later contributions if the information costs are low enough. Another important feature of leading by example is put forward by Foss (2001). He argues that the example given by the leader does not only influence the followers' actual cooperative behavior but also their beliefs about the other followers. That suggests that there is an additional source of influence from leaders to followers beyond imitation and reciprocity.

We investigate the influence of personal leadership within an experimental setting in which the leader has private information about the followers' behavior. Thus, we are able to provide quantitative support for the assumption that the leader is able to change followers' attitudes and beliefs towards the other followers and in a wider sense also towards the organization. Furthermore, our results show that the leaders' influence also changes the followers' behavior and in consequence improves the team performance.

3.2 Related literature

Several experimental studies confirm the predicted rise in cooperation in the presence of a personal leader who acts as primus inter pares. In each of following experiments leadership is implemented as a sequential public goods game[1], i.e. the leader states her contribution in a first move while all followers decide simultaneously after they know about the leaders' choice. An early experiment conducted by Gächter and Renner (2004) reveals a positive correlation between leader and follower contributions but only a small positive effect[2] from leading by example. A stronger effect is reported by Güth, Levati, Sutter, and Van Der Heijden (2007) in a similar setting. Besides that, this study also reports a higher influence from those leaders that are equipped with additional authority.[3] In contrast to this heterogeneity within the followers' endowments showed to be opposed to leaders' impact and reduce cooperation to the level of a simultaneous public-good. The same applies to missing information about the distribution of these endowments within the group (Levati, Sutter, and Van Der Heijden 2007).

The question whether the positive effect is actually induced by the leader and not by the mere existence of an additional reference point that can be used by the followers to adapt their behavior, is addressed by Houser, Levy, Padgitt, Peart, and Xiao (2007). The results presented there show that the signal itself does not increase contributions if it is not sent by a human leader. The insight that there is a need for a personal leader necessarily raises the question how this leader can be selected. Due to the nature of leading by example or at least due to its implementation in laboratory experiments the leader has to be a member of the group. At this point it does not seem to matter whether one player continuously takes the lead or the position is rotating among the group members (see Güth et al. 2007). In contrast to that endogenous self-selection results in considerably higher efficiency compared to an exogenous random selection (Arbak 2007; Rivas and Sutter 2009).

Another strand of literature tries to disentangle the different motives group

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[1] Moxnes and Heijden (2003) investigated leading by example in a public bad setting and also find a positive effect from leadership.

[2] Gächter and Renner (2004)report a significance difference between the leader and his followers as well as between the leader and the subjects in the control treatment (without leader). However, they report no (significant) difference between the followers and the control subjects.

[3] Güth et al. (2007) conducted a treatment, in which the leaders have the power to exclude one follower from the group. This result is also confirmed by Rivas and Sutter (2009), who investigated the impact from rewards and punishment in a leading by example setting. They report, that punishment through exclusion yields higher contributions than rewarding the followers.

members might have to follow the leader. Potters, Sefton, and Vesterlund (2007) investigated a two-person-game, in which the leader has private information about the actual value of the public good. They conclude that the leaders influence is rather driven by signaling than by reciprocity. In contrast to that, Meidinger and Villeval (2002) favor reciprocity as the mainspring of followers' behavior in a similar (two-person) setting. Although the results are not consistent, they point at the crucial role of private information. Unfortunately, the two-person setting also eliminates the others follower's behavior as a source of information and therewith blocks the view on the more realistic situation in which one person leads a group of followers.

After all, there is some evidence for the positive effect from leading by example. The high positive correlation between leaders' and followers' contributions is obvious in all studies (although the contribution differences are rather small). However, the question whether this correlation is the result of signaling, reciprocity or mere imitation received no concluding answer. This study investigates another obvious reason that is not only crucial for the connection between leaders' and followers' contributions but also for the success of leading by example. In each of the studies (with more than one follower) mentioned above leaders and followers rely on the same source of information to decide about their contributions, namely the feedback information about the average contributions in the previous round. According to what is known about the processing of this information (see Fischbacher and Gächter 2010), it seems plausible to assume that they also derive similar conclusions from it and as a consequence they should show similar behavior. Even if we allow for heterogeneous contribution preferences, there is no reason to expect any differences since the distribution of conditional contributors and free-riders should not differ between leaders and followers. Especially not if the leader is selected by chance as it is done in most of the aforementioned studies.

This study tries to contribute an additional explanation to the nature of leading by example with an experimental setting that treats followers' contributions as private information, which is only available to the leader. In comparison to the usual setting explained above, this simple change seems suitable to provide insight into the relationship between leaders and followers. The results show that a leader, who has private information about her followers' contributions, is not only able to lift up the level of cooperation but also to stop the downward shift in contributions.

3.3 Experimental design and procedure

The experiment was conducted in the eLab[4] at the University of Erfurt, using the software z-tree (Fischbacher 2007). A total of 48 students from different disciplines were randomly invited via ORSEE (Greiner 2004) and participated in the experiment. Written instructions (see Appendix A.3.1), handed out to all subjects and read out by the author, provide information on the payoff function, individual endowments and group size. All decisions and payoffs were completely anonymous. Thus, subjects were not able to draw any conclusions with respect to the individual contribution of their group members in any treatment.

To shed some light on the questions, whether additional information alters group members' contribution behavior or not, two different treatments were conducted based on a standard public-good game. The game is repeated for 20 rounds in the first phase. Another 20 rounds are played in a second phase. At the beginning of the experiment the participants are assigned to groups of four members each. The group composition remains unchanged (partner design) until the end of the experiment. Each subject receives an endowment of 20 tokens (e_i) in each round and has to decide how much he or she contributes (c_i) to the public project and how much to keep for the private account. Each token invested in the group project is deducted from the contributor's private account and generates a surplus for the entire group (as it is multiplied by 1.6). The group account (C_j) is evenly distributed among all four group members (N).

As soon as all contribution decisions are made the individual payoffs for the recent period are calculated using the following payoff-function (3.1):

$$\Pi_i = e_i - c_i + C_j, \qquad \text{with} \qquad C_j = 0.4 \sum_{i=1}^{4} c_i \tag{3.1}$$

[4] "Laboratorium für experimentelle Wirtschaftsforschung"

To establish a leader-follower-relation the contribution decisions are not made simultaneously. After all members of the group stated their beliefs about the next contribution provided by the leader and by the other group members, the leader decides first about her contribution to give an example to her followers. The followers are informed about their leaders' decision and thereafter provide their own contributions simultaneously. After that the leader is informed about her followers' average contributions. The only difference is the additional information given to the follower. While the followers in the full information treatment (hereafter FI) receive the same feedback (about the other followers' contributions) as the leader, the followers in the private information treatment (hereafter PI) do not come to know how many token the remaining followers provided for the public good.

Both treatments start with the selection of a leader from the group members. Without regard to further concerns about procedural fairness and legitimation the only aim of the selection procedure presented here is to avoid a random choice and to implement a leader with some kind of qualification for the actual game. For that reason the first task for each subject is to estimate the average contribution provided by the other group members. After that each subject is asked to state how many tokens he or she wants to contribute to the group project. These announcements are used to calculate the actual average contribution within the group and to compare it with the estimations stated before. According to this comparison the group member with the smallest estimation error becomes the leader of the group. If there is more than one subject with the smallest estimation error, the leader is randomly selected from these subjects. The results of the estimations are not made public until the end of the experiment.

Once selected, the leader keeps her position until the end of phase one. In the second phase the leader becomes a common member of the group and decides simultaneously with the former followers about her contribution. Table 3.1 summarizes the procedure in phase one and two.

Each subject, regardless of its later role, is informed about the leader selection procedure and all other steps described in Table 3.1 (except the treatment difference). Beyond that subjects are told that there will be a second phase. This is done without providing information about the particular task. After

Phase	Step	Leader	Follower
1 - leading by example	1	Leader selection procedure (1st round only)	
	2		Belief → leader
	3	Belief → follower	Belief → follower
	4	Contribution decision	
	5		Feedback ← leader
	6		Contribution decision
	7	Feedback ← follower	FI Feedback ← follower PI: No feedback
2 - no leader	1		Belief → follower
	2		Contribution decision
	3		FI: Feedback ← follower PI: Feedback ← follower

Table 3.1: Procedure in phase 1 and 2

the first phase subjects are instructed to play another 20 rounds of a simultaneous public goods game with regular feedback about the other group members' average contributions in both treatments.

3.4 Predictions

Assuming perfect rational and payoff maximizing individuals, everybody, regardless of whether he or she is a leader or a follower, should play the dominant strategy and contribute nothing to the public good. However, it is a well known result from numerous experiments that subjects deviate from that strategy and provide positive contributions to the team project (for an overview see Ledyard 1995; Chaudhuri 2011). One prominent explanation has been put forward by Fischbacher et al. (2001). They show that a considerable number of individuals behave as conditional contributors, i.e. they are willing to contribute if others contribute, too. Under perfect information conditions this behavior can explain the decline of cooperation rates in repeated public goods experiments. Subjects adapt their own contributions towards the contribution average in their group, which is continuously lowered by a number of free riders. In consequence the overall contributions to the public good diminish over time. In a recent study Fischbacher and Gächter (2010) provide empirical evidence for the assumption, that not even conditional cooperators match the contribution of their teammates perfectly. Instead subjects' behavior is described as selfishly biased, i.e. the group members tend to contribute below the group average and therewith accelerate the downward trend in cooperation.

A similar phenomenon is regularly observed in leading by example settings. Followers contribute (significantly) less than their leader as long as she has no (additional) means to sanction her followers. Nevertheless, contributions of both types show a high positive correlation (see for example Gächter and Renner 2004; Güth et al. 2007). At the same time the imperfect response to the other followers' contributions might enlarge the difference between leader and follower, if (some) group members want to contribute not only less than the leader but also below the average of the other followers. As a consequence the followers' contributions under full information conditions (FI) should be lower than the contributions in the PI-treatment, since followers do not know the average contributions of others.

There are several other reasons that point to higher average contributions in the private information treatment. The additional information, created by the feedback about other followers' contributions, also acts as an alternative anchoring point (Tversky and Kahneman 1974; Mussweiler 2000) and allows subjects to choose whether they take the leaders' or the followers' behavior as a reference. Considering the selfish bias mentioned above, this might result in inconsistent signals, towards which the followers have to adapt their behavior. If there are two sources of feedback, it might happen that a follower contributes more than the other followers but less than the leader or vice versa. This situation can be supposed to induce further uncertainty to the followers' environment and there is some evidence that uncertainty reduces contributions in repeated interactions. For example Au, Chen, and Komorita (1998) report that cooperation is lower if there is uncertainty about the outcome of the public good.[5] On the other hand, this uncertainty is also present if there is no additional reference point. Relying on a single source of information might create additional distrust towards the leaders' motives among the followers.

[5] Other results, which confirm the negative effect of uncertainty on contributions can be found in Wit and Wilke (1998) or Brennan et al. (2008).

There is no clear prediction from theoretical works or former experiments that disentangles both kinds of uncertainty. Furthermore, the PI condition might also influence leaders' behavior. If the leader recognizes the importance of her inputs for the group performance, she might improve her example. In other words, being the exclusive source of feedback might strengthen the leaders' commitment to her role modeling function (see Walumbwa, Avolio, Gardner, Wernsing, and Peterson 2008).

According to Foss (2001), leadership might also influence the group members' beliefs. The author argues that a leader is able to solve coordination problems because she creates a belief structure that substitutes common knowledge.[6] Following this argumentation, it seems plausible to assume that the followers in the PI-treatment are able to learn about other followers' behavior by drawing conclusions from the leaders' behavior. At this point the question arises whether the leader influences these conclusions or not. If the leader has private information on which the followers have to rely it should be easier for her to convince the followers of a cooperative culture. However, this gives rise to the temptation to misguide the followers. A selfish leader can use the asymmetry in information to free ride on the contributions of her followers if the teammates' cooperation is independent from the leaders' example. This case will occur if the followers' beliefs about their teammates have a stronger influence on their behavior than the example given by the leader. In contrast to that, the altruistic or efficiency oriented leader can use her information advantage to the benefit of the entire group. If she continues to give a good example by contributing high amounts of her endowment even if not all followers reciprocate, she might finally convince the group of a high cooperation level, which will in turn raise the contributions of conditional cooperative followers. Howsoever, the questions of leader motives are not primarily addressed in our experiment, since it was designed with the focus on followers' behavior.

The different information conditions should also alter behavior in the simultaneous game (phase 2); in a way that subjects in PI contribute more than the ones in the FI-treatment. With this second phase we should be able to shed some light on the sustainability of the transformation process in followers' belief structure. If average contributions are indeed higher under private

[6] Foss (2001): page 2f

information conditions, PI-subjects will have experienced 20 rounds of stable cooperation. Although they are now able to update their beliefs with regard to the received feedback, they might coordinate on higher contributions. In contrast to that the cooperation rates in the FI-treatment should decline for several reasons. First of all these participants should have experienced lower contributions in phase one and beyond that they are still able to adapt their behavior with reference to the feedback they receive in phase two. Considering that the positive influence from leading by example does not foster contributions anymore, the average inputs should be even lower than in the first 20 rounds.

If leaders are aware of their importance for the group and to some extend even disposed to get exploited by the followers, as described by Güth et al. (2007), their behavior should even differ from those of the followers in the second phase. Since former leaders still can give a good example to some extent, they might stick to their previous behavior and contribute more than the other group members.

3.5 Experimental results

The results are presented in two subjections according to the two phases of the experiment. The first one focuses on the contributions and beliefs in the leading by example setting (phase 1) and analyses the behavior and interrelation of leaders and followers under different feedback conditions. In the second subjection the simultaneous part of the experiment (phase 2) is discussed with view of the change in contribution behavior within the treatments and remaining differences between the two feedback settings.

The results from the leader selection procedure show no differences between the treatments. The average estimations are 9.6 in the FI treatment and 9.0 in PI and the same applies for the average contributions, which are 10.42 in FI and 10.08 under private information conditions. None of the differences is significant, neither within the treatments nor between the feedback conditions.[7] Therefore we are convinced that the subjects are randomly distributed to the

[7] The difference between the average estimations and the actual average contributions is 0.82 ($p = 0.355$) in FI and 1.08 ($p = 0.678$) in PI. The average estimation error does not differ between the treatments ($p = 0.980$).

treatments with regard to their contribution preferences.

	Overall (round 1-20)		1st half (round 1-10)		2nd half (round 11-20)		Difference* 1st vs. 2nd	
FI	6.73	(33%)	8.29	(41%)	5.16	(26%)	-3.13	(0.094)
PI	10.84	(54%)	9.58	(47%)	12.11	(61%)	2.53	(0.313)
Diff.**	4.11	(0.180)	1.29	(0.818)	6.95	(0.041)		

Notes: Average contributions (% of endowment) and differences (p-value) in phase 1; *tested with Wilcoxon-test, two-tailed (exact); **tested with Mann-Whitney-U-test, two-tailed (exact)

Table 3.2: Average contributions in phase 1

As can be seen from Table 3.2, the average contribution in phase one is 10.84 in the PI-treatment and therewith 4.11 tokens above the one in the FI-treatment (6.73). This might give a first cue to the positive influence of private information.

However, the distance between the treatments changes over time. While there is only a small difference in contributions in the first 10 rounds (1.29), the distance is more than five times higher (6.95) in the second half of phase one (see also Figure 3.1).

Obviously contribution differences are driven by the dynamics of the feedback conditions and not by different expectations at the beginning of the experiment. This becomes clear if the initial beliefs, stated after the leader selection procedure, are taken into account. One might expect that leaders anticipate their limited influence in the FI-treatment and therefore reduce their initial expectations towards the followers' contribution.

The same might hold true for the followers and their belief about the leaders' input to the public good. If there is a different appraisal of the information conditions announced in the instructions, there also might be a difference in initial beliefs. Actually this is not the case[8] and therefore it seems plausible to assume that the contribution differences observed in phase one are a result of adaption towards the feedback which subjects received during the game.

[8] There are no differences (between the treatments) in the initial beliefs of followers towards other followers ($p = 0.589$), the initial beliefs towards the leader ($p = 0.589$) and not in the leaders belief towards followers' contributions ($p = 0.699$), according to the Mann-Whitney-U-test (exact, two-tailed).

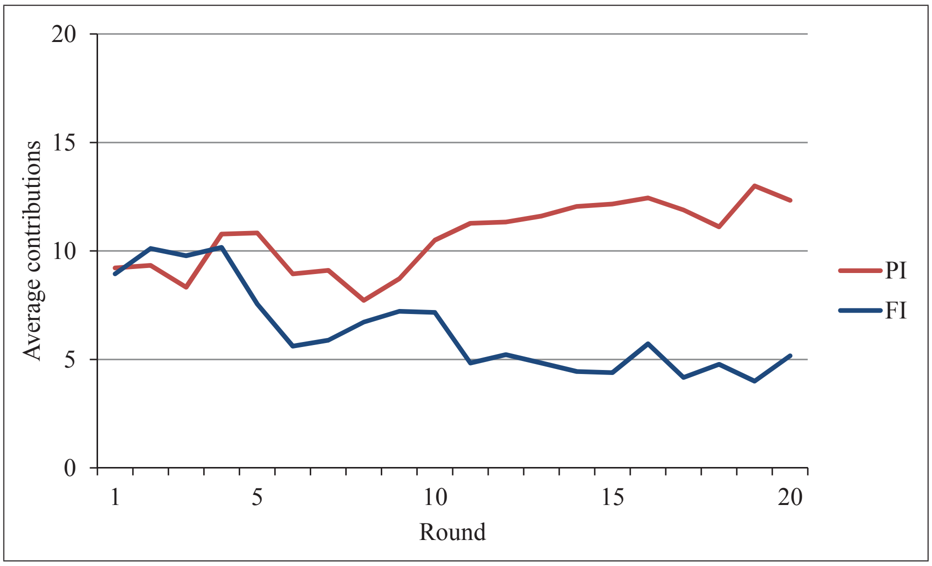

Figure 3.1: Average contributions in phase 1

At this point the prediction of higher contributions under private information conditions can only be partly confirmed. Thus, the next step is to explain the different evolutions of contributions.

One first explanation for the difference in contributions might be found in the distance between leaders and followers. If followers do not have feedback information about their teammates, they will exclusively rely on the leaders' contribution. As a consequence the difference between leaders' and followers' contributions should be smaller in the PI-treatment. The results tent to that assumption since the distance is smaller if the leader has private information[9]. Furthermore the comparison between the first and the second half of phase one shows an increase in the difference between leaders' and follower's contributions in the FI-treatment, while the difference decreases in PI (see also Figure 3.2).

In the PI-treatment the average distance in the first phase is only 0.83 token while the average difference between leader and follower contributions is almost twice as high in the FI-treatment (1.56).

Further support for the difference in leaders' influence on followers' contributions comes from the panel regression presented in Table 3.3. Contributions are described as a function of time (round), leaders' contribution and the belief about the other followers' contribution.[10] Model C1 captures the first 10

[9] see Appendix A.3.2

[10] In order to check for possible multi-collinearity we take the variance inflation actors into

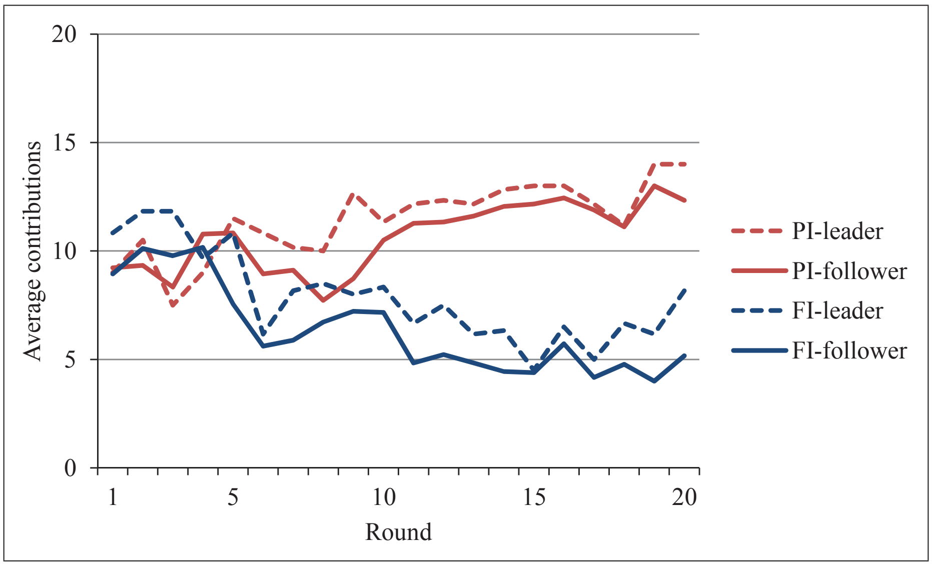

Figure 3.2: Average leader and follower contributions in phase 1

rounds and shows no considerable difference in leaders' influence between the treatments. However, the model C2 which captures the rounds 11 to 20 shows that followers contributions are significantly more affected by leaders' actions under private information (PI).[11]

Dv: followers' contribution	C1 (round 1-10)		C2 (round 11-20)	
Round	-0.194**	(0.093)	-0.004	(0.041)
Leaders' contribution PI	0.435***	(0.087)	0.624***	(0.084)
Leaders' contribution FI	0.313***	(0.036)	0.402***	(0.092)
Belief (Followers)	0.265***	(0.086)	0.314***	(0.107)
Cons	3.545***	(0.931)	0.445	(0.911)
R^2	0.34		0.66	

Notes: GLS panel regression, random effects, robust errors (6 clusters in groups) in parentheses, * sign. at 10%; ** sign. at 5%; *** sign. at 1%. Wald-test: equal coefficients for Leaders' contribution in C2. F = 2.82 p < 0.1. (N = 360)

Table 3.3: Leaders' influence on followers' contributions

The higher influence of leaders' contribution also provides first support for the assumption that followers' attitudes and beliefs change over time. Therefore the next step is to analyze the process of belief forming. As stated above,

account. They are below the critical value (5) in both models: 1.42 for the C1 model and 1.64 for the C2 model.

[11] Wald-test: coefficients for leaders' contribution PI and leaders' contribution FI $p < 0.1$

theories of transformational leadership predict that the personal leader does not only influence teammates' behavior but also their belief structure. Our model of belief updating is similar to the one that is used by Fischbacher and Gächter (2010). The belief about the other followers' next contribution is described as a function of the belief about the leaders' next action and the feedback information from the previous round.[12] Due to the design, this feedback consists of two values: the average contributions of the remaining followers and the leaders' contribution in the previous round. This distinction is important because it provides insight to what extend the teammates in the PI treatment draw conclusions from changes in leaders' behavior. The results of the panel regression are presented in Table 3.4.

Analyzing the process of belief forming reveals to things. First, followers in the FI-treatment use both sources of feedback to update their expectations about other followers' behavior. Although the belief about the followers is widely determined by the belief about the leader, the feedback from the previous round is also taken into account. It seems that followers expect their teammates to deviate from leaders' next choice. Otherwise there should be no additional effect of feedback on the beliefs about followers' contributions.

Dv: belief → follower	Full info (FI)		Private info (PI)	
Round	-0.011	(0.019)	0.066	(0.042)
Belief → leader	0.699***	(0.056)	0.870***	(0.042)
Leaders' contribution FI	0.313***	(0.036)	0.402***	(0.092)
Change in leaders' contr.	0.067	(0.044)	0.122***	(0.032)
Feedback ← followers	0.138***	(0.049)		
Cons	0.841**	(0.410)	0.043	(0.664)
R^2	0.74		0.85	

Notes: GLS panel regression, random effects, robust errors (6 clusters in groups) in parentheses, * sign. at 10%; ** sign. at 5%; *** sign. at 1%. (N = 342)

Table 3.4: Belief forming under full and private information

The results for the PI treatment show that followers seem to compensate the missing feedback by taking the change in leaders' behavior into account. The results support the assumption that leadership influences the followers'

[12] The variance inflation factors are again below the critical value in both models: 1.22 for the FI model and 1.52 for the PI model.

belief structure.

The transformation in the belief structure becomes obvious, if we look at the distances between the beliefs about followers, the beliefs about the leader and the actual contribution decision. As can be seen in Figure 3.3, there is an almost constant distance between followers' expectations of the other teammates and the leader in the FI treatment.

At the same time followers' actual contributions are below their belief about the remaining teammates in every round. Figure 3.3 shows the same variables for the PI treatment and the first rounds are comparable to the ones in FI. However, the differences decline over time and the belief about followers' behavior as well as the actual contribution converge to the belief about the leaders' next choice.

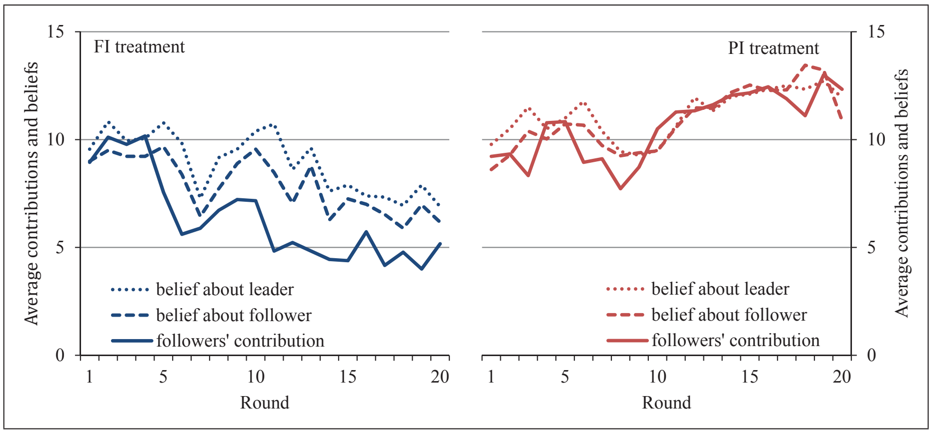

Figure 3.3: Beliefs and contributions in FI and Pi treatment

The average difference between the beliefs about the leader and those about the followers is 1.01 in FI and 0.28 in the PI treatment. The average distance between the followers' contributions and their beliefs about their teammates is 1.56 in FI compared to 0.26 in PI. Finally, the gap between the belief about the leader and the average contribution is 2.58 in FI and 0.54 in PI. All differences between the treatments are significant at least at the 10% level.[13]

In summary it can be stated that the different evolution of contributions is result of several effects that occur under no information conditions. First of all, the leaders' example has a stronger influence on followers' contributions if

[13] All comparisions between two treatments are tested with the Mann-Whithney-U-rest, two-tailed, exact.

the later do not have a second source of information. Furthermore, the missing feedback about the teammates' behavior conflates the followers' beliefs about leaders and teammates. Finally, contributions converge towards beliefs.

The remaining part of this section reports the results from the second phase, in which all group members, including the former leader, contribute simultaneously. Figure 3.4 shows the average contributions separated with regard to the former player types. Although contributions in the PI treatment (9.41) are still significantly ($p < 0.1$) above those in FI (4.97), both treatments exhibit a considerable downward trend.

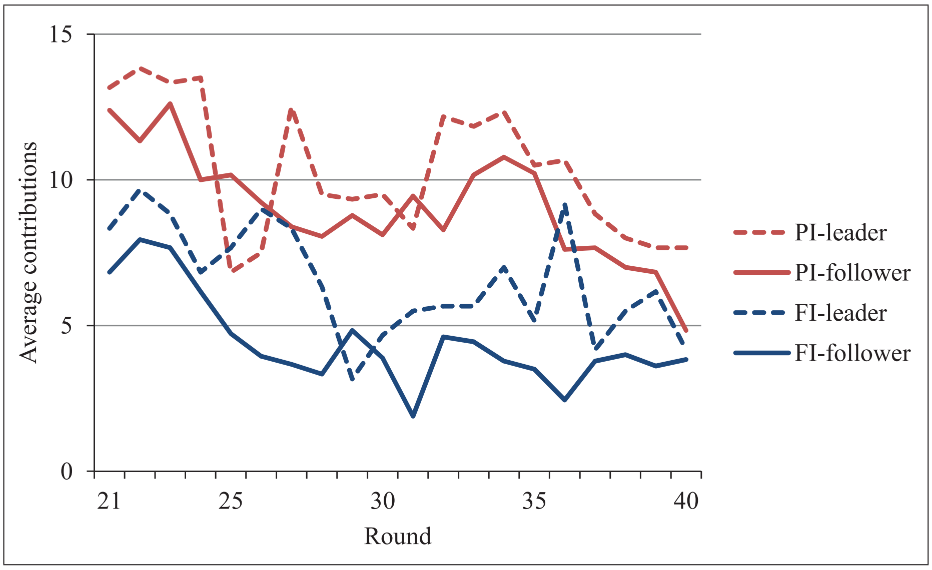

Figure 3.4: Former leader and follower contributions in phase 2

Obviously, the change in followers' belief structure that is induced by leading by example, cannot withstand the incentive structure of a standard public goods game. The average contributions provided by former leaders are only slightly above the ones of the group members and the distance does not differ between the treatments. During the second phase the gap between the treatments contracts from 5.38 in round 21 to 1.63 in round 40.

These results receive further support from the panel regression presented in Table 3.5, which shows that the process of belief updating is very similar in both treatments. The only considerable determinant is the feedback about others' contributions and previous beliefs do not have any impact.

The additional explanatory variable (Avg. C-B in P1) is thought to measure the effects of subjects' former attitude observed in the first phase. The rationale behind this is that followers might have formed a certain picture about their teammates, which also persists in a modified environment. As can be seen, there is no effect of former attitudes on belief updating in the second phase.

Dv: belief → follower	Full info (FI)		Private info (PI)	
Round	-0.094*	(0.042)	-0.098**	(0.042)
Feedback	0.464***	(0.159)	0.666***	(0.173)
Avg. $C - P$ in P1	0.092	(0.055)	-0.036	(0.039)
Cons	4.612***	(1.013)	4.911***	(1.831)
R^2	0.43		0.57	

Notes: GLS panel regression, random effects, robust errors (6 clusters in groups) in parentheses, * sign. at 10%; ** sign. at 5%; *** sign. at 1%. (N = 456)

Table 3.5: Belief forming in phase 2

However, there is a considerable influence at the beginning of the simultaneous part of the game. The average distance (phase 1) between subjects' contributions and their beliefs about the other followers is highly correlated with their initial difference in the second phase.[14] Thus, it appears that subject's transfer the attitudes which they formed during the first phase, into the second phase of the game but change it afterwards with regard to the new decision situation.

3.6 Conclusion

This chapter provides quantitative support for the effect of leadership on the belief structure of followers. The experimental design is focused on the interactions between leaders and followers and allows investigating a situation in which private information becomes an additional source of power for the leader. Based on the argumentation that has been put forward by Foss (2001), it sheds some light on the question whether leadership can substitute common knowledge and change the followers' attitudes towards their teammates.

The results show that leadership under private information conditions does not only change followers' beliefs but also their cooperative behavior. First,

[14] Spearman's rank correlation, FI: $\rho = 0.833$, $p < 0.05$ and PI: $\rho = 0.706$, $p < 0.05$

followers do no longer suspect their teammates to contribute less than the leader, as they do under full information conditions. Second, their own contribution preferences are no longer selfishly biased, i.e. they do not want to contribute below their beliefs' about other followers. As a consequence average contributions exceed those in the full information setting.

However, these changes in beliefs and behavior are not sustainable. The result from the second part of the experiment, in which all group members decided simultaneously without the example from a leader, shows that subjects readapt their expectations quickly with regard to the new decision environment. The process of belief forming and the contribution preferences drop back to the standards of the public good game and in consequence the contributions diminish over time.

With regard to theoretical approaches of transformational leadership, our results support the assumption that there is an additional effect of personal leading beyond the mere management of resources. After all, the results also reveal several new questions. The effect of leaders' contribution preferences could be investigated in further treatments with preselected leaders and might contribute further insight into the interdependences between leaders' attitudes and their ability to transform the beliefs of their followers. The question whether the transformation process can be accelerated through means of additional authority might be answered with a treatment in which the leader can draw on sanctions to discipline the followers.

Chapter 4

The effects of uncertainty and exaggerated feedback[1]

> Then if anyone at all is to have the privilege of lying, the rulers of the State should be the persons; and they, in their dealings either with enemies or with their own citizens, may be allowed to lie for the public good. (*Plato*)

4.1 Introduction

The provision of feedback is common practice both in the public as well as in the private sector. Politicians do not become tired of emphasizing what they have achieved and how well for example the economy and the labor market are developing. Managers report statistics with key performance indicators that prove how successful their departments and companies are. Hearing all this - as stimulating and mood-elevating as it sounds- who did not sometimes notice the shadow of a doubt whether the provided information might not be only part of the whole truth and the picture that is painted might be too optimistic?

Related literature shows that the issue of false or imperfect information provision has intensely been studied by economists. The major part of existing research focuses on two aspects. The first is whether people provide false or imperfect information in order to increase their individual profit and how their

[1] This chapter is based on the working paper "Effects of informational uncertainty on cooperation in social dilemmas", joint work with Mareike Hoffmann and Bettina Rockenbach. All authors contributed equally.

partners react to that behavior. The second aspect deals with the question whether people provide false or imperfect information to increase collective profit.

Results of experimental research show that people indeed provide false and imperfect information in order to increase their individual profit. In dictator games, subjects announce that a split was determined by chance instead of admitting that they actively chose a split that favored themselves (Ackert, Church, Kuang, Qi, and Li 2007). In ultimatum games, proposers cheat about the size of the pie and responders cheat about their outside option when given the chance to do so (Croson, Boles, and Murnighan 2003). Concerning the consequences in the short run, responder threats and lies increase the size of offers made, while proposer lies decrease them. In the long run, after the revelation of false information, proposers' lies both increase offers and decrease acceptances. Responders' revealed lies lead to smaller, but not significantly smaller offers. Besides also finding support for people's propensity to lie in order to increase individual profit, Gneezy (2005) argues that people are sensitive to their gain when making the decision to lie. According to Gneezy, people care less for a lie the bigger their personal gain from that lie is. Further, they do not only care how much they gain from that lie but also how much their partner will lose. Hurkens and Kartik (2009) reinterpret Gneezy's results claiming that people belong to one of two types. They either never lie or they lie as long as the outcome obtained by lying is preferred to the outcome obtained by telling the truth. According to their reasoning, in the latter case people will always lie and the decision to do so is not influenced by considerations about relative changes of the outcome for the partner.

With regard to the aspect of the provision of false information to increase the collective profit, there is evidence that leaders in public goods provide false information about the MPCR[2] in order to induce cooperation and gain efficient payoffs (Serra-Garcia, Damme, and Potters 2011). Despite findings like these, to the best of our knowledge, there is hardly any research on the question how people react to the revelation of having received false information that served to increase collective profit.

We set out to answer this question by investigating contribution behavior

[2] marginal per capita return

in a public good game with different feedback conditions. Due to our intention to exaggerate the feedback in two treatments we had to inform all subjects that the information about the other group members' contribution displayed on their screen might deviate from the actual values. In order to avoid confounding exaggeration and uncertainty we first have to investigate the effect of mere uncertainty. In the next step we analyze how contribution behavior is influenced by feedback about partners' contributions that is either exaggerated in absolute terms or in relation to subjects' own contribution, given it is common knowledge that the feedback might deviate from actual contributions. Finally, we analyze how the revelation that the feedback was exaggerated and will continue to be so affects future contribution behavior. Our results show that uncertainty lowers the average contributions, while exaggerated feedback leads to higher contributions. However, it also turns out that uncertainty creates selfish biased behavior, i.e. subjects prefer to contribute less than they expect from the other group members. We also find that exaggerated feedback, which prevents subjects' from feeling exploited by others results in stable cooperation on a high level. Comparing the two types of exaggeration reveals that the behavior of subjects who received negative feedback is a main reason for the downward trend in repeated games.

The remainder of this chapter is organized as follows. In the next section (4.2) we explain the different treatment conditions in detail. Afterwards the predictions, derived from former experimental results and models of cooperation in repeated games, are discussed (section 4.3). Section 4.4 presents and discusses our experimental results and the last section (4.5) concludes.

4.2 Experimental design and procedure

The experiment was designed as a standard public good game. Subjects were assigned to groups of four and randomly distributed to the baseline treatment with perfect feedback (PF) and the test treatments with imperfect feedback (IF). Each treatment was conducted with twelve groups and therefore provides twelve independent observations. The experiment was run at the eLab of the University of Erfurt. 192 subjects were recruited using the Orsee system (Greiner 2004). All participants were placed in separate compartments and the interaction with the other group members was done anonymously via the computer interface. The experiment was programmed and conducted with the software z-Tree (Fischbacher 2007). Subjects were paid privately after the end of the experimental session with an exchange rate of 1 Euro, for each 80 tokens. A session lasted approximately 70 minutes and average earnings were 11.75 Euro.

4.2.1 Procedure

The experiment was made up of two phases, each consisting of 20 identical rounds. Prior to the first period subjects only received instructions for phase 1 and were informed that there will be a second phase and that payoffs and decisions will be independent in the two phases.[3] Each subject received an endowment of 20 tokens (e_i) per round. At the beginning of every round subjects had to state their belief about the other group members' next average contribution decision. After that they made a decision about how many tokens to contribute (c_i) to the public good. Tokens contributed were deducted from a player's private account, multiplied by 1.6 and evenly distributed among all four members (N) of a group. Tokens kept remained in player's private account. Equation 4.1 summarizes the individual payoff-function and emphasizes that every contributed token yields 0.4 token for the player notwithstanding whether it was contributed by himself or any other group member. In other words, every token contributed by the player generates a return of 0.4 for the all group members, whereas every kept token has a return of 1 for the player.

$$\Pi_i = e_i - c_i + C_j, \qquad \text{with} \qquad C_j = 0.4 \sum_{i=1}^{4} c_i \tag{4.1}$$

4.2.2 Treatments

After all members of a group had made their decisions, subjects were informed about the average contribution of the other group members in every round. This feedback varied by treatment. In the PF treatment the feedback always displayed the actual contributions. The mere effect of uncertainty about the

[3] See the instructions in Appendix A.4.1

correctness of the feedback on contribution behavior was analyzed in treatment IF0. Here, subjects were informed that the feedback might deviate from actual values while in fact the displayed feedback always corresponded to actual contributions, i.e. was not exaggerated.[4] In treatment IF25 the feedback overestimated the other group members' contributions uniformly by about 25% based on the average inputs of the remaining group members. More precisely the feedback was calculated as the sum of others' contributions multiplied by 1.25 and divided by 3. To avoid unrealistic information the maximum feedback was limited to 20 (the endowment) and the exaggerated sum was rounded to be a credible quotient of a division by three. Since our main purpose was to analyze whether the announcement of higher values fosters cooperation and to avoid very small and hardly effective exaggerations, we implemented a minimum feedback of five. At the end of phase one of every treatment, subjects saw a summary of the first phase that provided information about their own average contributions, the average exaggerated feedback if relevant and the actual average contributions of the other three group members.[5] After seeing the summary, all the subjects in IF0 and IF25 were asked to rate the received feedback with regard to moral and monetary aspects. The first question was "How do you evaluate the deviation between the presented and the actual feedback information from a moral point of view?" The second question was "How do you evaluate the deviation between the presented and the actual feedback information with respect to your own monetary payoff?" Both questions were answered on a seven point scale from negative (-3) to positive (3). At the beginning of phase 2 there was another short introduction read out to the subjects to inform them that the second part of the experiment will be identical to phase 1 and that they are a member of the same group again. In other words in phase 2 subjects in the IF0 treatment now knew that the feedback they received was correct and subjects in the IF25 treatment knew that the feedback was systematically overestimated by about 25% and that the overestimation would continue in phase 2. Table 4.1 summarizes the different

[4] The original sentence from the instructions was: "From the beginning of the second period you will be informed about the average contribution of the other three members of your group in the previous period at the beginning of each period. Please note that this information might deviate from the true contribution!"

[5] For an example of a screenshot see Appendix A.4.2

feedback conditions.

Treatment	Uncertainty	Exaggeration	Feedback
PF	No	No	C_j / 3
IFO	Yes	No	1.25 C_j / 3
IF25	Yes	Yes	C_j / 3

Table 4.1: Summary of feedback conditions

4.3 Equilibria and predictions

In this section we first discuss the predictions given by standard theory of rational selfish decision makers and the influence of inequality and risk aversion as well as uncertainty on subjects' strategies in the game. We then explore the arguments of conditional cooperation towards our research questions stated above and finally specify some predictions to render our questions more precisely.

4.3.1 Equlibria

In a one-stage public-good game, without any means to foster cooperation, the rational selfish player will contribute nothing independent of what she beliefs about the other group members' input. Therefore the Nash-equilibrium will be no cooperation at all. This is also true for every single stage of finitely repeated game (Friedman 1986).

However, experimental research reveals that only a limited number of players in public good games behave this way, especially if the game is repeated for a known number of rounds. Laboratory public goods experiments typically show that subjects contribute roughly one half of their endowment in first rounds and start to decrease their input over time until there is almost no cooperation at the end of the game (Ledyard 1995; Chaudhuri 2011). One prominent explanation for this pattern is the approach of conditionally cooperative behavior (for an overview see Fischbacher and Gächter 2010). According to this approach, people are willing to contribute as long as they believe (or observe) that other group members will do the same and as long as the feedback -

used to update these beliefs - provides no reason to expect lower contributions in the future. This behavior is commonly referred to as conditional cooperation. In a recent study Fischbacher and Gächter (2010) provide empirical evidence that exposes conditional behavior as imperfect because subjects are selfishly biased, i.e. they prefer to contribute a little less than the other group members. This seems to be one major reason for declining contributions.

Another explanation for the adaption of subjects' behavior towards others' contributions derives from the models of inequity aversion presented by Fehr and Schmidt (1999) and Bolton and Ockenfels (2000). Both models claim that behavior is not only driven by a subject's own monetary outcome but also by the relative outcome (Bolton and Ockenfels 2000) and the non-monetary utility created or destroyed by payoff differences (Fehr and Schmidt 1999). Subjects evaluate their payoffs in comparison to their partners' earnings and try to reduce inequity. Despite the fact that the models of inequity aversion focus on the players' payoff distribution rather than on their beliefs, these comparisons lead to similar contribution patterns in public good games as predicted by conditional cooperation. Since the partner's payoff is calculated from the same contributions that are used to display the feedback, exaggerating the feedback will also lead to higher payoffs from the other group members' point of view.

A further approach was put forward by Kreps et al. (1982). This model predicts at least temporarily cooperation in repeated games even if there are no individual social preferences. They argue that there might be some sort of incomplete information about the other players strategies either in the way that at least one group member plays tit-for-tat or that others might have additional utility from acting altruistic. It is not necessary that these 'irrational players' actually participate as long as all subjects belief in their existence. The beliefs are updated with respect to the information gathered about other group members' contribution behavior. Under those conditions cooperative strategies might emerge even among absolutely selfish players as long as they expect to gain profit from others' reciprocal behavior. Cooperative behavior might emerge even more often if the probability subjects attribute to the existence of irrational players is higher. Since exaggerated feedback partly hides the selfish nature of their teammates and distort the belief updating, subjects might cooperate for a longer time to gain profit the expected reciprocal behavior of other group members. A study presented by Croson (2007) shows that the vast majority of subjects (about 92%) indeed behave in a reciprocal way rather than acting as defined in altruism or commitment models. Neugebauer et al. (2009) falsify the strategic play hypotheses (Selten and Stöcker 1986; Kreps et al. 1982; Sonnemans et al. 1999) and present additional evidence for reciprocal or conditional behavior.

Further, there is experimental evidence that uncertainty reduces cooperative behavior in public-goods games. Wit and Wilke (1998) demonstrate that environmental uncertainty about the provision point of a public-good has detrimental effects on cooperation, if subjects also face social uncertainty, i.e. uncertainty about their partners' cooperative behavior. Au et al. (1998) show that a greater uncertainty about the outcome of a public good decreases cooperation, if subjects perceive that their contribution is critical for the outcome of the public good. A recent study by Brennan et al. (2008) also reveals a negative correlation between contributions and risk level in a public goods game.

4.3.2 Predictions

Implementing feedback exaggeration without misleading the participants obviously creates an extra amount of uncertainty that added to the usual inevitable question how the other group members might behave. We first analyze the mere effect of uncertainty, i.e. without exaggeration. The uncertainty in the IF0 treatment that is caused by the imperfect feedback is similar to the risk subjects face in Brennan et al. (2008) study insofar as subjects know that they will not be perfectly informed. We expect that subjects account for the higher uncertainty level in the IF0 treatment than in the PF treatment by discounting their contribution decisions by some amount to cover against the possibility that actual contributions of fellow players are smaller than announced by the feedback. Thus we assume that contributions are smaller in the IF0 treatment than in the PF treatment.

The combination of uncertainty and exaggerated feedback in IF25 might lead to the two possible outcomes predicted. As the available information and thus the uncertainty in phase 1 is the same in IF0 and in IF25, we can again apply the logic of uncertainty reducing contributions to the public-good

(Au et al. 1998; Wit and Wilke 1998; Brennan et al. 2008). This means that even if the announced feedback is exaggerated, due to the existing uncertainty contributions might be lower in the IF25 treatment than in the PF treatment.

On the other hand, Fischbacher and Gächter (2010) findings suggest that people are conditionally cooperative and contribute slightly less than what they believe that the other group members will contribute. The feedback mechanism in the IF25 treatment exaggerates others' actual contributions which in turn might raise beliefs. Research on decision making has shown that under uncertainty people often apply simplifying heuristics which can result in anchoring effects, i.e. assimilations of a numeric estimate to a previously considered standard (e.g. Tversky and Kahneman 1974; Galinsky and Mussweiler 2001; Mussweiler 2000). Although our feedback is imperfect it is the only information that the subjects receive and therefore might cause such an anchoring effect with regard to the belief about other players' contributions. If the anchoring effect is stronger than the cooperation-decreasing effect of uncertainty, inputs to the public good should be higher in the IF25 treatment than in the PF treatment.

After phase 1, the information structure is the same in IF25 and IF0. While subjects in the IF0 treatment know that they can rely on the feedback information, subjects in the IF25 treatment now know that they need to discount 25% of the feedback in order to calculate their partners' actual contributions. Again applying the argument of conditional cooperation, we expect that subjects adjust their behavior and discount the feedback information. If this is the case, contributions in phase 2 should not differ between the treatments and second phase contributions in IF25 should be below those of the first 20 rounds.

If subjects understand that the exaggeration mechanism might induce higher contributions among conditionally cooperative players and at the same time a higher overall payoff, they should evaluate the exaggeration positive from a monetary point of view. However, research provides evidence that people are generally lying-averse (e.g. Brandts and Charness 2003; Holm and Kawagoe 2010; Gneezy 2005). Therefore we expect that despite the monetary advantage the exaggeration will be evaluated negatively from a moral point of view, indicating that the end does not justify the means.

4.4 Results and discussion

The results are presented in two subsections according to the two phases of the experiment. The first one focuses on the contribution behavior and the changes in beliefs about other subjects' contributions under the different feedback conditions. In the second subsection we focus on the long-run effects of exaggerated feedback und discuss the influence from revelation after phase 1. First we turn to the question whether uncertainty reduces cooperation. Figure 4.1 shows the average contributions during the first phase. In comparison to the control-treatment with perfect feedback the contributions are significantly ($p < 0.1$)[6] lower under imperfect feedback conditions (average contribution of 4.9 in IF0 and 6.5 in PF).

With view to the results presented by Wit and Wilke (1998) this difference seems to confirm the detrimental effect of uncertainty on cooperation in social dilemma situations. Nevertheless, it is important to notice that the uncertainty that we implemented in IF0 is broader than the uncertainty presented by Wit and Wilke because our feedback mechanism does not differentiate between environmental and social uncertainty.

Next we analyze whether exaggeration is able to increase cooperative behavior in the presence of uncertainty. The second treatment (IF25) maintains the uncertainty about the feedback but exaggerates the other players' average contribution uniformly by about 25%. In comparison to the situation with mere uncertainty (IF0) this leads to higher contributions.

On average subjects who received exaggerated feedback contributed 6.8 token and therewith 1.8 token more than those who participated in the IF0 treatment without exaggeration (see Figure 4.2). This difference is only weakly significant in the later rounds of phase 1 (round 5 - 20 $p < 0.1$) but not significant over all rounds of the first phase (round 1 - 20 $p = 0.198$). Compared to our baseline treatment (PF), the exaggeration mechanism is not sufficient to overcome the negative effect of uncertainty.

Subjects' contributions to the public good in IF25 are not significantly ($p = 1$) different from contributions under perfect feedback conditions (see Figure 2b). While the mere uncertainty in the IF0 treatment reduces contributions by

[6] All comparisons between two treatments are tested as independent samples with the MWU-test (exact, two tailed) and 12 independent observations in each treatment.

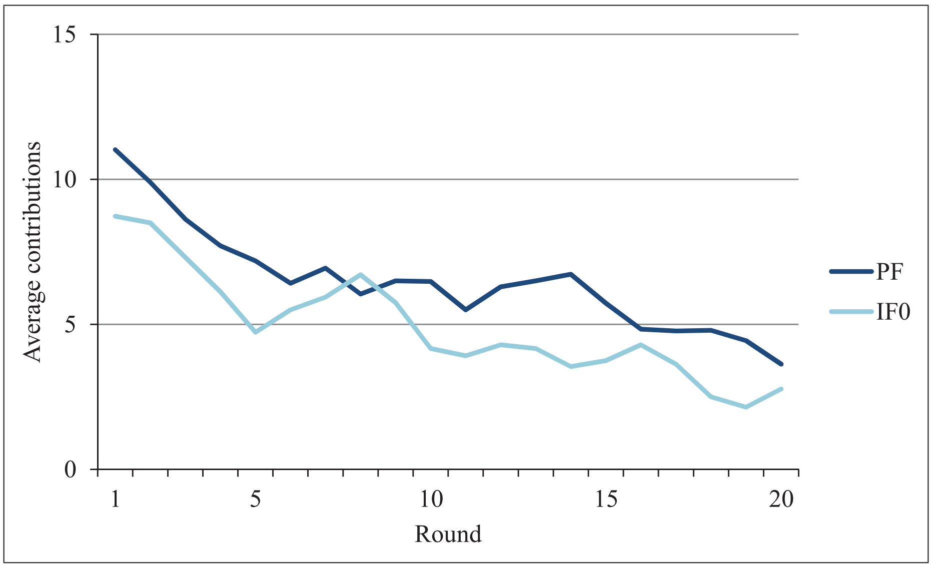

Figure 4.1: Average contributions in phase 1 under perfect feedback (PF) and imperfect feedback without exaggeration (IF0)

about 24% on average compared to the control treatment PF, the exaggeration in IF25 raises the contributions by about almost the same scale (26%). To shed some further light on the differences induced by uncertainty and exaggeration Figure 4.3 shows the average variances of contributions in phase 1. Especially in the first 10 rounds both treatments with imperfect feedback show a higher level of variance in contributions than the treatment with perfect feedback.

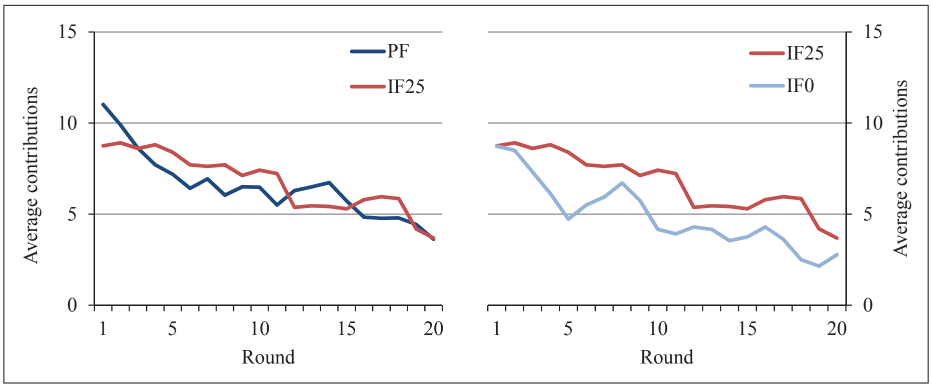

Figure 4.2: The effects of uncertainty and exaggeration on average contributions

The contribution variance in IF0 is 36.6 and even 41.4 in IF25 while it is only 26.7 in the PF treatment. Levene's test rejects ($p < 0.001$) the hypothesis of equal variances under perfect and imperfect feedback conditions.

The information that feedback might deviate from actual values does not only lower contributions on average but also increases the scattering of contribution decisions no matter whether there actually is an exaggeration or not.

The next section deals with the long-run effects and subjects' evaluation of the exaggeration. After revealing the exaggeration mechanism and the actual contributions at the end of phase 1 subjects were informed that neither their group nor the feedback conditions would be changed during the second phase. After that there is no informational difference between the PF and the IF25 treatment (and also not in IF0) in phase 2 because there is no uncertainty in any of the treatments, since subjects now know how to calculate the actual average contribution of the other group members, contributions are different (see Figure 4.4). Subjects in the IF25 treatment contributed significantly less than subjects in PF ($p < 0.05$). The decline of cooperation between phase one and phase two is significant in all three treatments ($p < 0.001$ in IF25, $p < 0.001$ in IF0 and $p < 0.05$ in PF)[7].

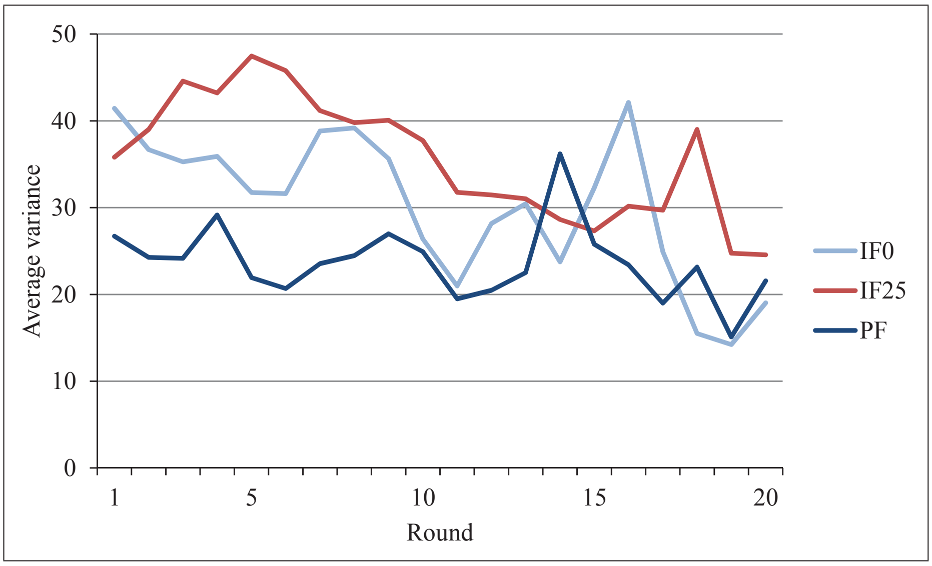

Figure 4.3: Average variances in contributions in phase 1

The fact that contributions in phase 2 are lower in the IF25 treatment than in the PF treatment, although the decline from phase 1 to phase 2 was significant in both treatments and the contributions did not differ in phase 1, reflects the strong negative effect that the revelation of the exaggeration

[7] All comparisons within one treatment are tested as related samples with the Wilcoxon test (exact, two-tailed) and 12 independent observations.

mechanism had on subjects' contribution behavior.

The act of overestimating contributions might have lowered subjects' perceived sense of obligation to contribute and might to have shifted their focus on the maximization of their individual benefits. The finding is also in line with the results presented by Wilson and Kelling (1982) that became known as the "broken window theory" and go back to an experiment conducted by the psychologist Zimbardo in (1973). Zimbardo abandoned one car without license plates and with the front lid open in the Bronx and another identical car in Palo Alto. The car in the Bronx was completely destroyed within one day while the one in Palo Alto remained untouched for a week. However, after breaking one of the windows, the car in Palo Alto was also completely desolated only within a few hours (Zimbardo 1973). Thus, criminal activity substantially increased even in this otherwise quiet neighborhood as soon as there were cues that enhanced this behavior. Likewise, the act of providing exaggerated feedback information might have triggered subjects to engage in less moral, i.e. less collectively beneficial behavior in the second phase.

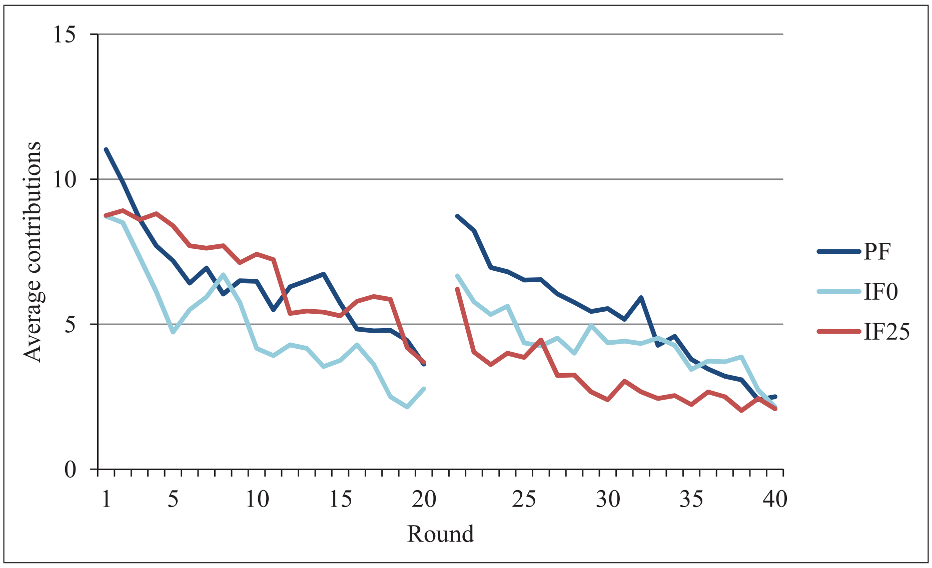

Figure 4.4: Average contributions in phase 1 and 2

Moral evaluations of the exaggerated feedback are not significantly different from monetary evaluations in IF25 ($p = 0.941$) after phase 1. Although the correlation between the two evaluations is not significant ($r = 0.168$, $p = 0.255$), the exaggeration mechanism is negatively evaluated from both points of view by about 40% of subjects. Obviously, the exaggeration mechanism was not appreciated as a means that is justified by the end because it served to increase contributions and collective benefit.

4.4.1 Beliefs

A surprising difference between the treatments can be found if the beliefs that subjects stated about the others' contribution before they announced their own contribution decision are taken into account. Figure 4.5 shows the average contributions, beliefs and feedbacks in phase 1 under perfect feedback. It can be seen that there are no noteworthy differences.

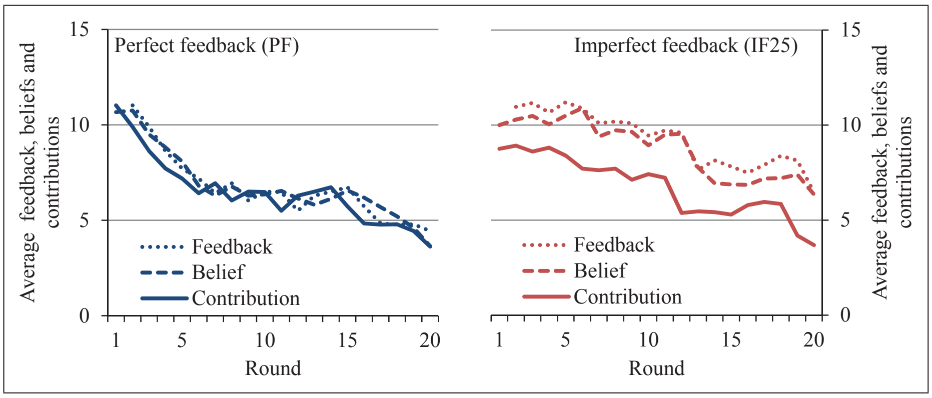

Figure 4.5: The difference between belief and contribution in PF and IF25.

Under imperfect feedback conditions, when subjects know that the displayed feedback might not represent actual values, one might expect a visible deviation of beliefs from the feedback that was received. Contrary to this expectation Figure 4.5 shows that there is a close connection between the presented feedback and beliefs. However, subjects systematically discount (Wilcoxon, $p < 0.001$)[8] their own contributions from the input they expect from their group members.

If this discount is driven by the uncertainty it should diminish in the second phase and indeed the removal of feedback uncertainty reduces the gap between beliefs and contribution ($p < 0.05$). In the second phase subjects know how the exaggeration mechanism works and do not need to subtract an amount from their beliefs in order to cover against the uncertainty that the feedback

[8] The discount from belief to contribution is also significant in the IF0 treatment (Wilcoxon: $p < 0.001$) and the differences are significantly higher in those treatments with imperfect feedback than with perfect feedback. (MWU: PF/IF0 $p < 0.05$ and PF/IF25 $p < 0.001$).

might deviate from actual values anymore. While the average distance between feedback and contribution remains unchanged due to the exaggeration mechanism, the average belief moves away from the presented feedback and towards contributions (Figure 4.6).

One might argue that not the absence of uncertainty but the possibility to calculate the actual average contribution reduces the distance between beliefs and contribution in the second phase. This is doubtful for two reasons. The first one is that the treatment with mere uncertainty (IF0) also shows a significant distance between beliefs and contributions ($p < 0.001$) in phase 1, which diminishes, just like in the IF25 treatment, once the uncertainty is removed in the second phase ($p < 0.05$).

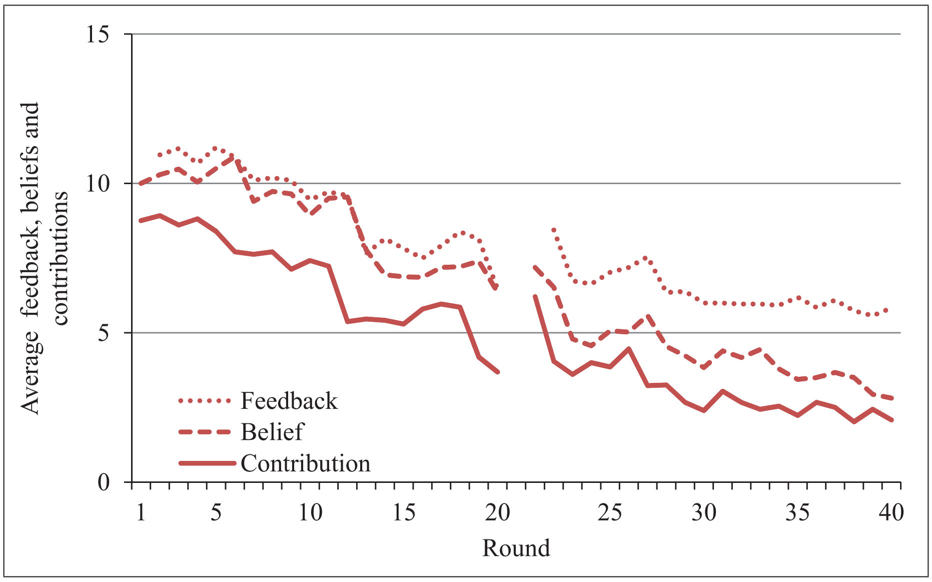

Figure 4.6: Beliefs and contributions under imperfect feedback conditions

The second reason can be found in the exaggeration treatment (IF25) itself. Although subjects can easily calculate actual values by subtracting 25%, the average beliefs are significantly above that calculated value ($p < 0.001$). Since this is not the case under perfect feedback conditions (PF) the differences in the IF treatments seem to be a result of some remaining uncertainty rather than a result of imperfect conditional cooperation.

To proceed with more detailed analysis of the influence from feedback on the beliefs and contributions we use the model applied by Fischbacher and Gächter (2010). It describes the individuals' belief about others contribution in the next round as a function (4.2) of the displayed average contributions of the other group members in the previous round (*feedback*) and the subjects' own belief in the previous round (Belief$_{t-1}$).

$$belief_t = \alpha_1 Belief_{t-1} + \alpha_2 feedback \tag{4.2}$$

Table 4.2 presents the result of the panel regression for the different information conditions. The Arellano-Bond dynamic panel-data estimation method, which we also applied to this model (Arellano and Bond 1991), might provide a better fit to our data structure but the reported estimates seem to be inconsistent (significant second order correlation in the residuals, $p < 0.05$, Arellano-Bond test). As a consequence we forego the interpretation of the Arellano-Bond model and report the results from the panel regression.

Dv: belief	Cert (N = 912)	Uncertain (N = 1824)	Joint (N = 2736)
Round	-0.017	-0.006	-0.008
	(0.013)	(0.015)	(0.009)
Feedback	0.580***	0.348***	
	(0.023)	(0.055)	
Belief$_{t-1}$	0.325***	0.586***	
	(0.025)	(0.060)	
Certain x feedback			0.589***
			(0.021)
Certain x Belief$_{t-1}$			0.334***
			(0.023)
Uncertain x feedback			0.345***
			(0.055)
Uncertain x Belief$_{t-1}$			0.584***
			(0.059)
Cons	0.686**	0.372	0.454***
	(0.290)	(0.325)	(0.253)
R^2	0.68	0.65	0.65

Notes: GLS panel regression, random effects, robust errors (12 clusters in groups) in parentheses, * sign. at 10%; ** sign. at 5%; *** sign. at 1%.

Table 4.2: Belief forming under certain and uncertain information conditions

The Cert model confirms that the results presented by Fischbacher and Gächter (2010) for repeated one-shot interaction (stranger matching) also hold true for repeated situations with partner matching. Applying the same model to the treatments with imperfect feedback (Uncert: IF0 and IF25) reveals a shift in the influences on subjects' belief. In contrast to the perfect feedback conditions the impact from others' contribution in the previous period drops down while the influence from subjects' prior beliefs rises at the same time. To rule out that this inversion occurs by chance we estimated both conditions in a JOINT model and find that feedbacks' influence is significantly lower ($p < 0.001$)[9] if subjects are in doubt about the information they receive about others' contributions. All the more subjects rely on their own prior beliefs under Uncert conditions ($p < 0.001$). This can also be seen as an additional explanation for the limited effect of exaggerated feedback on contribution level.

The next step is to investigate the determinants of subjects' contribution decision. We use a model similar to that applied by Croson (2007) and extend it by an additional independent variable, which is thought to be similar to the predicted contribution variable used by Fischbacher and Gächter (2010). Thus subjects' contribution in the current round is described as a function (4.3) of subjects' first contribution ($Contributioni_{t=1}$) and the belief about other group members' contribution (Belief_{i_t}).

$$contribution_t = \beta_1 belief_t + \beta_2 predictedcontribution \tag{4.3}$$

Comparing the first four models in Table 4.3 reveals two things. First, adding subjects' first contribution as a determinant for later contributions improves the predictions in both feedback conditions (Cert+ and Uncert+). In both cases the unexplained variance is reduced and the constant term is no longer significant under certain information conditions. As with the belief forming there is a shift in influences if the information about other players' contributions contains uncertainty (Uncert+). The belief about others' contributions loses influence on subjects' decisions if it is based on imperfect feedback. At the same time the initial and unconditional contribution in round 1 at

[9] Wald-test for linear hypotheses with H0 = equal parameters.

least partly substitutes that influence. As can be seen from the JOINT model the explanation provided by belief under uncertain conditions is significantly below ($p < 0.001$)[10] the one provided under certain conditions. The opposite is true for the impact from first round contributions that influence current contributions significantly stronger ($p < 0.01$) under uncertain conditions.

So far our results show that the exaggeration of group members' contributions not only fosters cooperative behavior but also alters the determinants of belief forming and contribution decisions due to the uncertainty it induces to the feedback. Uncertainty systematically reduces the impact from other-regarding variables (Belief) and raises the importance of self-regarding variables (First contribution). However, we also found that a uniform exaggeration of the average group members' contributions by about 25% is not sufficient to overcome the drop in contributions caused by the associated feedback uncertainty.

If subjects are in doubt about the reliability of information they receive, the exaggeration cannot induce a higher level of cooperation in comparison to the perfect feedback condition. In the long run, after the revelation of actual contributions, subjects are even less cooperative if the exaggeration mechanism is further applied. This might be due to the fact that the exaggeration is neither appreciated from a monetary nor from a moral point of view and rather increases the tendency to engage in less moral and more selfish behavior. To disentangle the opposing effects of uncertainty and exaggeration we changed the feedback mechanisms in an additional treatment that is presented in the following section.

4.4.2 Follow up: Subject based exaggeration

As subjects' contribution behavior to the public good in phase 1 is almost the same in PF as in IF25 although the given information is not the same, there must be a difference in the way the feedback is processed. For this reason we analyzed how subjects adjust their contribution behavior in the next round depending on the feedback they had received. Figure 4.7 shows that in each feedback round contributors can be classified into three types. Subjects who received the feedback that their input to the public good was

[10] Wald-test for linear hypotheses with H0 = equal parameters.

Dv: belief N	Cert 960	Uncert 1920	Cert+ 960	Uncert+ 1824	Joint 2736
Round	-0.059*	-0.161***	-0.050*	-0.162***	-0.129***
	(0.029)	(0.023)	(0.023)	(0.023)	(0.020)
Belief	0.763***	0.447***	0.761***	0.541***	
	(0.034)	(0.056)	(0.034)	(0.020)	
1st contr.			0.159***	0.368***	
			(0.044)	(0.016)	
Belief$_{t-1}$	0.325***	0.586***			
	(0.025)	(0.060)			
1st contr (C)					0.199***
					(0.037)
Belief (C)					0.702***
					(0.035)
1st contr. (U)					0.355***
					(0.055)
Belief (U)					0.465***
					(0.057)
Cons	1.893***	4.291***	0.154	1.103*	0.774
	(0.445)	(0.477)	(0.572)	(0.576)	(0.468)
R^2	0.42	0.36	0.45	0.48	0.47

Notes: GLS panel regression, random effects, robust errors (12 clusters in groups) in parentheses, * sign. at 10%; ** sign. at 5%; *** sign. at 1%. Wald-test: equal coefficients for Leaders' contribution in C2. F = 2.82 p < 0.1.

Table 4.3: Contribution decisions under certain and uncertain information conditions

below the average of the other group members' input do not have the feeling that the other players might have taken advantage of them, i.e. they are no suckers anyway and are called “IF25 below average”. These subjects adjust their contributions in the next round slightly upwards by 2-3 points. Subjects who received the feedback that their own contribution was above the average of their group members' contribution are the ones who experience the feeling that the other players took advantage of them. These players adjust their next round contributions strongly downwards. They are called “IF25 above average aware” in Figure 4.7. Finally, there are subjects who actually were suckers but who were prevented from knowing it because the 25% exaggeration increased the feedback of the average group members' contribution to an amount that was just above the subject's own contribution. As Figure 4.7 clearly shows, these players, called "IF25 above average unaware", do hardly change their contributions in the next round and their behavior does not differ significantly from those of the "IF25 below average" subjects ($p = 0.561$). The difference in the adjustment to the feedback between "IF25 above average unaware" suckers and "IF25 above average aware" suckers is significant ($p < 0.5$).

Thus, the analysis of the feedback adjustment seems to show that the downward shift is mainly driven by those subjects who experience feedback that points to exploitative behavior of their team mates, i.e. by strong downward adjustment of "IF25 above average aware" suckers.

Two other possible explanations for the downward shift in contributions are presented by Fischbacher and Gächter (2010). The first one is the existence of free riding subjects who contribute zero or a very small amount of tokens and therefore lower the average group contributions. The other one is due to the observation that majority of people behave as imperfect conditional cooperators and contribute (a little) less than they expect from their group members and therewith also lower the average group contributions. However our data suggest that there is also a relevant difference between the subjects who contributed below that average and those who contributed above. Since the initial contribution preference measured by the contribution belief ratio in the first round does not differ between our treatments (Jonckheere-Terpstra-Test $p = 0.556$)[11], we can rule out that subjects initially have different contribution preferences depending on what they know about the reliability of the presented feedback.

Departing from our assumption that suckers were responsible for the decline of cooperation, we predict that preventing subjects from feeling to be the sucker will lead to an increase in cooperation. In order to test whether this is true, we conducted a follow up study where all subjects whose contribution was above the group members' average were prevented from realizing that they were the suckers (IF no suckers). The experimental design of this treatment is identical to the IF25 treatment except for the exaggeration mechanism. Instead of exaggerating other group members' contributions uniformly by 25%, the exaggeration in IF no sucker was based on each subject's own contribution.

[11] The same holds true for first round contributions ($p = 0.972$) and first round beliefs ($p = 0.513$). The contribution belief ration is used here since it is comparable to the "predicted contribution" variable applied by Fischbacher and Gächter (2010).

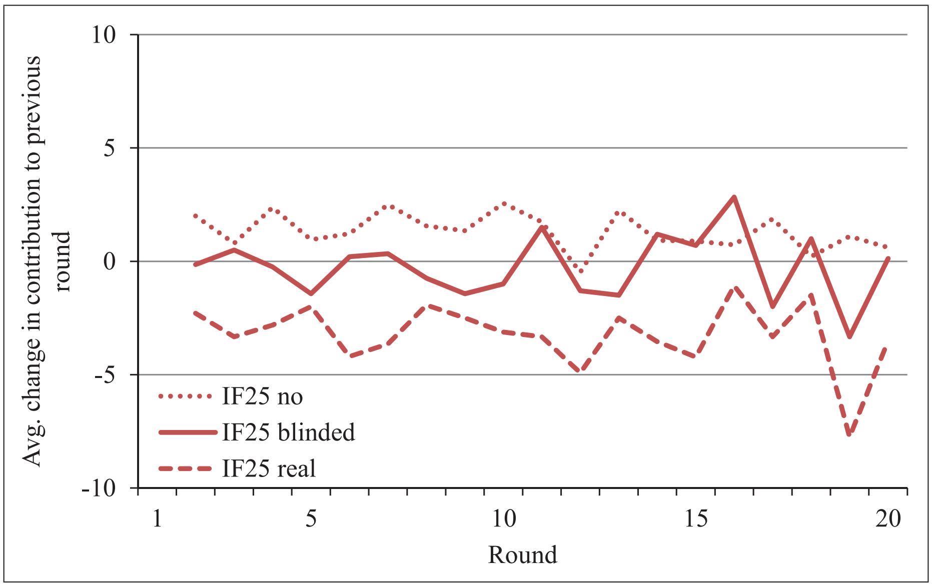

Figure 4.7: The influence from exaggeration on contribution adaption

We announced subjects' own contribution as feedback to every subject that contributed more than the other group members did on average. Otherwise the true average contribution of the other group members was announced. In other words, no subject received a feedback that indicates exploitation by other group members. As in the other IF-treatments subjects were told that the displayed feedback value might deviate from real one. It is important to mention that this feedback mechanism does not rule out a downward shift driven by imperfect conditional cooperation or free riders. If the subjects have selfish biased contribution preferences, there should still be a decline in average contributions, since each conditional contributor prefers to contribute below his or her belief about the others.

Indeed our results show that if no subject has the feeling to be exploited by other group members the downward shift can be avoided and contributions remain on the initial level. Contributions in the IF no sucker treatment are significantly higher the ones in IF0 ($p < 0.001$). Indeed there is no cue that contributions follow a continuous downward trend in phase 1 (see Figure 4.8). Neither do the last 5 rounds differ from the first 5 ($p = 0.775$) nor is there a significant difference between the first and the second half of phase 1 ($p = 0.677$). One might speculate that the inversed u-shape might be a result of growing mistrust towards the announced contributions of other group members. While subjects follow the exaggerated feedback in the first 10 rounds they start to mistrust the ongoing high values and start lowering their contributions in the second half. However, this interpretation is not supported by the data at all.

The regression coefficients hardly change between the first and the second half of phase 1, neither with regard to the belief forming[12] nor with view on the determinant of actual contribution decisions.[13] Instead, the well known end-game-effect might provide a better explanation for the decline in contributions at the end of phase 1. Further, although the informational uncertainty given in the introduction was the same in the IF25 and the IF no sucker treatment, a post hoc calculation shows that the average amount by which group members' contributions were overestimated in IF no sucker was only 22.67%. This amount is less than the amount that was necessary to lift cooperation in IF25 to the level that was achieved in the control treatment PF without uncertainty about the feedback (25%).

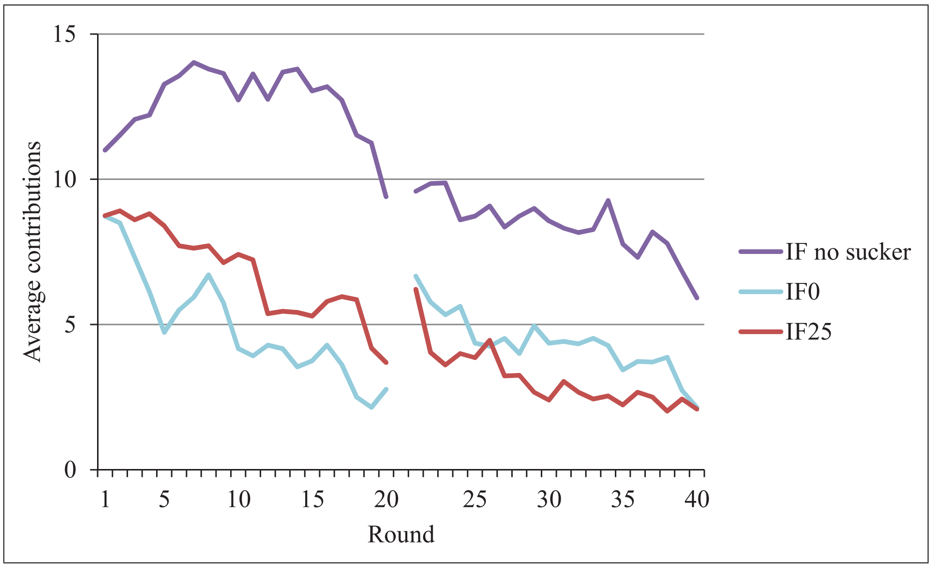

Figure 4.8: Average contributions in IF no sucker in phase 1 and 2

Following the same reasoning as outlined in section 4.3, we assume that cooperation will decline once the exaggeration mechanism is revealed.[14] In the

[12] The coefficient for feedback as explanatory variable for subjects' beliefs changes from 0.409 in round 1-10 to 0.411 in the last 10 rounds of the first phase.

[13] The coefficient for belief as explanatory variable for subjects' contributions changes from 0.704 in round 1-10 to 0.685 in the last 10 rounds of the first phase.

[14] The original instruction was: "Please note: The feedback on the others' average contribution in phase 1 was at least as high as your own contribution in each of the 20 periods. In phase

IF no sucker treatment subjects cannot exactly calculate the percentage of the exaggeration because others' contributions are not multiplied by an absolute number but depend on the subject's own contribution. Nevertheless, in phase 2 subjects know that actual contributions might be lower than announced and therefore should discount their contributions.

The comparison of the first and the second phase shows that contributions in the IF no sucker treatment significantly decline if the exaggeration mechanism is revealed ($p < 0.001$). However, the cooperation is still significantly higher in the IF no sucker treatment than in the PF treatment ($p < 0.001$) in the second phase.

Assuming that this way of exaggerating the feedback increases cooperation despite uncertainty, we expect that subjects evaluate the exaggeration positively from a monetary but negatively from a moral point of view.

Both moral and monetary judgments of the exaggeration are significantly better in the IF no sucker treatment than in the IF25 treatment ($p < 0.05$ and $p < 0.05$). The moral and the monetary evaluation of the exaggerated feedback in IF no sucker are again not significantly different ($p = 0.955$). Instead, moral and monetary evaluations are significantly correlated ($r = 0.515$, $p < 0.001$). The fact that not only the monetary but also the moral evaluation of the exaggeration is better in the IF no sucker treatment and that the two evaluations are correlated suggests that subjects do not regard the exaggeration as bad anymore as soon as their profit is higher. Apparently, subjects realized that the exaggeration mechanism induced more cooperation and appreciated this positive effect. In other words in this case the end seems to justify the means.

Beliefs in IF no sucker

The difference between beliefs and contributions (see Figure 4.9) in the first phase is also significantly larger in the IF no sucker treatment than in the PF treatment ($p < 0.001$). The information structure from round 1 to round 20 is the same in IF no sucker and in IF25 because subjects know that the feedback they receive might not be true but they do not know in what way. Subjects consider this uncertainty in their contribution decision by making a discount

2 will the information about the average contribution of the other three members of your group in the previous period also be at least as high as your own contribution in each of the 20 periods."

from what they believe that their partners contribute.

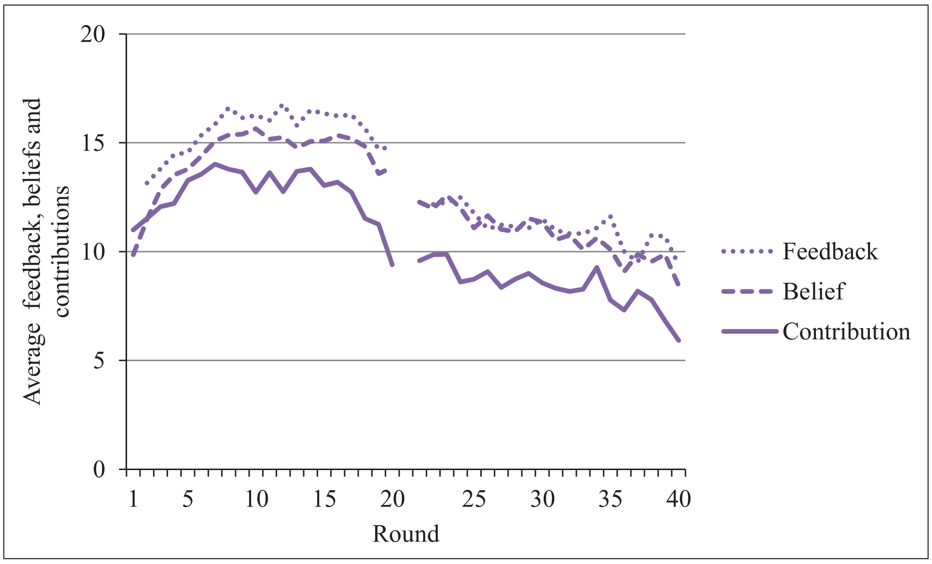

Figure 4.9: The difference between belief and contribution in IF no sucker in phase 1 and 2

In phase 2, subjects in IF no sucker behave differently from subjects in IF25. In IF no sucker, the difference between beliefs and contributions remains from phase 1 to phase 2 ($p = 0.146$). Assuming that uncertainty is the major reason for the discount from beliefs to contributions as already outlined before, this finding is not surprising. In IF no sucker the uncertainty is equivalently present in phase 1 as in phase 2 because even after the revelation how the exaggeration mechanism works subjects were by design not able to calculate the actual average contribution of the other group members.

4.5 Conclusion

We study whether the attempt to "paint the world pink" helps to overcome the conflict between individual and collective payoff maximization in a social dilemma situation and analyze the effects in the long run. Assuming that people are conditional cooperative and therefore are more likely to put effort in a task if they know that other group members do so, too, we sugarcoated the feedback about other group members' contributions in a public-good game. Our results show that as long as there is uncertainty about the reliability of the feedback, a uniform exaggeration of 25% about other people's average contributions does not increase cooperation above the level reached in a control treatment without uncertainty about the reliability of the feedback on group members' contributions.

We also find that uncertainty not only alters average contributions but also the way how subjects form their beliefs about other players' contributions. At the same time the importance of beliefs and contribution preferences as explanatory variables for contributions is reversed under uncertain feedback conditions.

The different contribution adjustments of those subjects who felt exploited by others and those who exploited their group members seems to be an alternative explanation for the downward shift in repeated social dilemma situations. While imperfect conditional cooperation as presented by Fischbacher and Gächter (2010) implies some sort of selfishness our observations suggest that players' decision to contribute less than the average is a result of (or an answer to) disappointing feedback.

In the long run, after the announcement of actual contributions, a further application of the uniform exaggeration mechanism leads to an even stronger decline in cooperation. The exaggeration is much more effective if it is done contingently on each subjects' own contribution, i.e. if no person feels to be exploited by others. In that case, stable cooperation can be established and even remains high once the exaggeration mechanism has become common knowledge. Additionally, this mechanism is more efficient in terms of possible exaggeration costs because the necessary amount of exaggeration is smaller.

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Appendix A

Appendix

A.1 The effect of observation and rewards on cooperation (Chapter 1)

A.1.1 Instructions

(translated from German)

General Information: You are a member of a group with four members. During the whole experiment you will only interact with the members of your group. The experiment consists of 20 rounds. At the end of the first period as well as at the end of all following rounds the group has to decide whether they hire a manager for the following round. The manager observes the group members' contributions and rewards those group members with the highest contributions. Hiring a manager creates costs.

Procedure: The experiment consists of 20 periods and every period consists of two stages.

Stage 1: Every group member receives an endowment of 20 tokens at the beginning of every period. You have to decide how many tokens you contribute to the group project. The remaining tokens are paid to your private account. The group result is 1.6 times the sum of contributions from all group members. The part of the group result that exceeds the sum of members' contributions is called surplus. Therefore the surplus is 0.6 times the sum of contributions. At the end of stage 1 you will receive information about the sum of contributions and your own payoff for this period. If the manager was appointed in this period, you will receive information about the allocation of the surplus and the costs of management.

Stage 2: The members of the group vote for or against the appointment of a manager in the following period. The manager will be appointed if three out of four members vote for him. Otherwise the manager will not be appointed. After every group member casted his/her vote the voting result will be announced.

The manager: The manager is not selected from the group members. The manager is an automated agent that decides according to the following rules. The manager observes the sum of contributions and the contributions of two out of four group members. The observed group members are randomly chosen. The choice is independent from former and future choices. It is also independent from the group members' voting and contribution decision. The manager guesses the remaining (unobserved) contributions by assuming that both are equal. [M4: The manager observes the sum of contributions and the contributions of each group member.]

Example:	Member 1	Member 2	Member 3	Member 4
Contribution:	5	7	11	17
Managers' guess:				
Sum of contributions:				5 + 7 + 11 + 17 = 40
Observed contributions:				7 + 11 = 18
Guess of unobserved contributions				(40 - 18) / 2 = 11
Guessed contribution:	11	7	11	11

The manager allocates the surplus (deducting the manager costs) to the group member with the highest contribution. If there is more than one group member with the highest contribution, the surplus (deducting the manager costs) is divided equally among these group members.

The manager costs are 0.2 [M4: 0.4] times the sum of contributions and are deduced from the surplus. The remaining surplus is therefore 0.4 [M4: 0.2] times the sum of contributions.

Calculation of payoff (period): The sum of contributions is always equally divided among the group members.

If the *manager is appointed* the payoffs are calculated as follows:

The group member(s) with the highest contribution receive(s):
Payoff = 20 - contribution + sum of contributions / 4
+ 0.4 x sum of contributions / number of members with highest contribution

The other group member(s) receive(s):
Payoff = 20 - contribution + sum of contributions / 4
+ 0.4 x sum of contributions / number of members with highest contribution

If the *manager is not appointed* the payoffs are calculated as follows:

Payoff = 20 - contribution + 1.6 x sum of contributions / 4

Total payoff: Your total payoff from the experiment is the sum of payoffs from the 20 periods. At the end of the experiment your total payoff will be converted into Euro with an exchange rate of 1 Euro per 70 tokens.

Please note: Communication is not allowed during the whole experiment. If you have a question please raise your hand out of the cabin. All decisions are made anonymously, i.e. no other participant is informed about the identity of someone who made a certain decision. The payment is anonymous too, i.e. no participant learns what the payoff of another participant is.

A.1.2 Incentives

The incentive to contribute marginally above the second-best player (if the manager is appointed) arises if:

$$\begin{array}{llcl} (M4) & \Pi_i = e_i + 0.25C_{-i} & < & \Pi_i^* = e_i - c_i + (0.25 + 0.2)(C_{-i} + c_i) \\ (M2) & \Pi_i = e_i + 0.25C_{-i} & < & \Pi_i^* = e_i - c_i + (0.25 + 0.4)(C_{-i} + c_i) \end{array}$$

Simplified to:

$$\begin{array}{llcl} (M4) & 0.25C_{-i} & < & -0.45c_i + 0.45C_{-i} \\ (M2) & 0.25C_{-i} & < & -0.35c_i + 0.65C_{-i} \end{array}$$

And solved for C_{-i}:

$$\begin{array}{llcl} (M4) & C_{-i} & > & 2.75c_i \\ (M2) & C_{-i} & > & .875c_i \end{array}$$

A second condition is necessary (in both treatments) to ensure that the player is the only top contributor: $c_i > max(c_{j1}, c_{j2}, c_{j3})$. If both conditions are fulfilled the player receives the higher payoff Π_i^*

A.1.3 Panel regression (I) on contribution in M4

Dv: contribution	Spec I ($N = 608$)		Spec II ($N = 608$)	
Round	-0.169***	(0.039)	-0.169***	(0.39)
First contribution	0.666***	(0.128)	0.664***	(0.129)
Feedback	0.046	(0.048)	0.047	(0.059)
Manager	3.269***	(0.616)		
Vote for hired manager			3.459***	(0.414)
Vote against hired manager			2.651***	(1.580)
Cons	2.862***	(1.098)	2.875***	(1.101)
R^2	0.33		0.33	

Notes: GLS panel regression, random effects, robust errors (6 clusters in groups) in parentheses,* sign. at 10%; ** sign. at 5%; *** sign. at 1%. The Wald-test rejects the H0 of equal coefficients (Vote for the hired manager and Vote against the hired manager) at the 5% level. We forego the interpretation of this data because of the small number of observations with manager in M4.

A.1.4 Panel regression (II) on contribution in M4

Dv: contribution	M4 without manager ($N = 536$)		M4 with manager ($N = 104$)	
Round	-0.152***	(0.036)	-0.311	(0.076)
Cons	7.919***	(0.739)	12.402***	(1.767)

Notes: GLS panel regression, random effects, robust errors (6 clusters in groups) in parentheses, * sign. at 10%; ** sign. at 5%; *** sign. at %. The Wald-test does not reject the H0 of equal coefficients (Vote for the hired manager and Vote against the hired manager) at the 10% level. We forego the interpretation of this data because of the small number (104) of observations with manager in M4.

A.1.5 Probit-regression on individual voting decisions

Dv: contribution	M2 ($N = 608$)		M4 ($N = 608$)	
Round	0.017*	(0.039)	0.169	(0.39)
First voting decision	0.323***	(0.111)	0.747***	(0.162)
Manager in current round	-0.220**	(0.110)	-0.173**	(0.143)
Feedback	-0.029***	(0.016)	-0.034**	(0.008)
Cons	0.271***	(0.191)	0.066***	(0.211)

Notes: Probit regression with robust standard errors in brackets. (LR $\chi 2 = 18.25$; $Prob > \chi 2 = 0.001$; significance level: *10% **5% ***1%) We forego the interpretation of the data from M4 because of the small number (104) of observations with manager in M4.

A.2 The effect of feedback on conditional co-operation (Chapter 2)

A.2.1 Instructions

(translated from German)

General Information: You are a member of a group that consists of four members. During the whole experiment you will only interact with the members of your group.

Procedure: The experiment consists of **3 phases** and every phase consists of **10 periods**. Every group member receives an endowment of **20 tokens** at the beginning of each period. You have to decide how many tokens you contribute to the group project. The remaining tokens are paid to your private account. All points that have been contributed to the public good will be multiplied by the factor **1.6** and equally split among all 4 players, i.e. every player receives **0.4 (=1.6/4)** for each point that has been contributed to the public good by a player. At the end of the $10^t h$, $20^t h$ and $30^t h$ period you will receive information about:

- your average contributions and payoff in the last 10 periods
- the average contributions of the other 3 group members in the last 10 periods

Calculation of payoff per period: Your payoff in each round consists of two parts:

- Points that you did not contribute
- Your share of the public good

Payoff per period = your endowment (20) - your contribution + sum of contributions + sum of contributions x 1.6/4

Total payoff: Your total payoff from the experiment is the sum of payoffs from the 20 periods. At the end of the experiment your total payoff will be converted into Euro with an exchange rate of 1 Euro per 70 tokens.

Please note: Communication is not allowed during the whole experiment. If you have a question please raise your hand out of the cabin. All decisions are made anonymously, i.e. no other participant is informed about the identity of someone who made a certain decision. The payment is anonymous too, i.e. no participant learns what the payoff of another participant is.

A.3 Feedback and leading by example (Chapter 3)

A.3.1 Instructions

(translated from German)

General Information: The experiment consists of two phases. First you will be informed about phase 1. Instructions for phase 2 will be announced after the end of phase 1. Your decisions in phase 1 will neither have any influence on the possibilities of your decisions nor on your payoffs in phase 2.

Information for phase 1: You are member of a group that consists of **4 members** in total. During phase 1, you will only interact with members of your group. Phase 1 consists of 20 periods. The structure of all rounds is identical. Every group member receives an endowment of **20 tokens** at the beginning of each period. You have to decide how many tokens you contribute to the group project. The remaining tokens are paid to your private account. All points that have been contributed to the public good will be multiplied by the factor **1.6** and equally split among all 4 players, i.e. every player receives **0.4 (=1.6/4)** for each point that has been contributed to the public good by a player. Points that have not been contributed to the public good are kept by the player.

Calculation of payoff per period: Your payoff in each round consists of two parts:

- Points that you did not contribute
- Your share of the public good

Payoff per period = your endowment (20) - your contribution + sum of contributions + sum of contributions x 1.6/4

Selection of the early contributor: At the beginning of the experiment each group member guesses the average contributions of the other 3 group members and states his/her own contribution.

The group member with the smallest deviation between his/her guess and the actual contribution of the other group members is selected as the early contributor. If there is more than one best guess, the early contributor is randomly selected.

You will see on your screen whether you are selected as the first mover or not.

The first mover is selected for the entire first phase.

Procedure for phase 1:

1. The early contributor decides about his/her own contribution.

2. Being informed about the decision of the early contributor, the other group members decide simultaneously and privately about their own contribution.

3. The early contributor [FI: each member] receives information about the average contributions of the other group members and the own payoff.

Total payoff: Your total payoff from phase 1 is the sum of payoffs from the 20 periods. At the end of the experiment your total payoff will be converted into Euro with an exchange rate of 1 Euro per 90 tokens.

Please note: Communication is not allowed during the whole experiment. If you have a question please raise your hand out of the cabin. All decisions are made anonymously, i.e. no other participant is informed about the identity of someone who made a certain decision. The payment is anonymous too, i.e. no participant learns what the payoff of another participant is.

Information for phase 2: Phase 2 consists of another 20 periods of the game you played in phase 1. The members of your group will not change. All group members decide simultaneously and privately about their own contribution. There is no early contributor. At the end of each period you will receive information about the average contributions of the other group members and your own payoff.

Income from phase 2: Your total income from phase 2 is the sum of payoffs of all periods. At the end of the experiment your total income from phase 2 will be converted into Euro with an exchange rate of 1 Euro for 90 points.

A.3.2 Average distances between leader and follower contributions

Round	Overall 1-20	1st half 1-10	2nd half 11- 20	Difference 1st vs. 2nd
Full information	1.56	1.50	1.61	0.11
Private information	0.83	0.90	0.76	-0.24
Difference	0.73	0.60	0.85	

Notes: The differences are not significant according to a Mann-Whitney-U-Test for independent samples (differences in columns) and a Wilcoxon-Test for related ones (differences in rows).

A.3.3 Standard deviation in contribution decisions

Round	Overall 1-20	1st half 1-10	2nd half 11- 20	Difference 1st vs. 2nd
Full information	6.94	6.51	7.41	0.09
Private information	5.93	5.78	5.68	-0.10
Difference	1.01	0.73	1.73	

Notes: The differences are not significant according to a Mann-Whitney-U-Test for independent samples (differences in columns) and a Wilcoxon-Test for related ones (differences in rows).

A.4 The effects of uncertainty and exaggerated feedback (Chapter 4)

A.4.1 Instructions

(translated from German)

General Information: The experiment consists of **two phases**. First you will be informed about phase 1. Instructions for phase 2 will be announced after the end of phase 1. Your decisions in phase 1 will neither have any influence on the possibilities of your decisions nor on your payoffs in phase 2.

Information for phase 1: You are member of a group that consists of 4 members in total. During phase 1, you will only interact with members of your group.

Procedure of phase 1: Contributions of group members

- **Phase 1** consists of **20 periods**. The structure of all rounds is identical.
- In **each period** every player receives an endowment of **20 points**.
- Every player has to decide how many of the 20 points he/she wants to contribute to the public good.
- All points that have been contributed to the public good will be **multiplied** by the factor **1.6** and **equally** split among all 4 players, i.e. **every player receives 0.4 (=1.6/4)** for each point that has been contributed to the public good by a player.
- Points that have not been contributed to the public good are kept by the player.

Information about other group members' contributions: From the beginning of the second period you will be informed about the **average contribution** of the other three members of your group in the previous period at the beginning of each period.

Pleas note: that this information might deviate from the actual contribution!

Calculation of the payoff per period: Your payoff in each round consists of two parts:

- Points that you did not contribute
- Your share of the public good

Payoff per period = your endowment (20) - your contribution + sum of contributions + sum of contributions x 1.6/4

Example for the case, that **you contributed 10** and the **other three members** of your group contributed **12, 8 and 4** points: = (20 - 10) + (10 + 12 + 8 + 4) x 1.6 / 4 = 10 + 13.6 = 23.6

Total income from phase 1: Your total income from phase 1 is the sum of payoffs of all periods. At the end of the experiment your total income from phase 1 will be converted into Euro with an exchange rate of 1 Euro for 80 points.

Please note: Communication is not allowed during the whole experiment. If you have a question please raise your hand out of the cabin. All decisions are made anonymously, i.e. no other participant is informed about the identity of someone who made a certain decision. The payment is anonymous too, i.e. no participant learns what the payoff of another participant is.

Information for phase 2: Phase 2 consists of another 20 periods of the game you played in phase 1. The members of your group will **not** change.

[**IF 0**] **Please note!** The feedback on the others' **average contribution** in phase 1 **corresponded to the actual average contribution in each of the 20 periods**.

In phase 2 the information about the **average contribution** of the other three members of your group in the previous period **will also correspond to the average of the actual contribution in each of the 20 periods**.

[**IF 25**] **Please note!** The feedback on the others' **average contribution** in phase 1 **was by 25% higher** than the actual average contribution in **each of the 20 periods**.

In phase 2 the information about the **average contribution** of the other three members of your group in the previous period will **also be by 25% higher** than the actual average contribution in **each of the 20 periods**.

[**IF no sucker**] **Please note!** The feedback on the others' **average contribution** in phase 1 w**as at least as high as your own contribution in each of the 20 periods**.

In phase 2 the information about the **average contribution** of the other three members of your group in the previous period **will also be at least as high as your own contribution in each of the 20 periods**.

Total income from phase 2: Your total income from phase 1 is the sum of payoffs of all periods. At the end of the experiment your total income from phase 1 will be converted into Euro with an exchange rate of 1 Euro for 80 points.

Total income from phase 1 and phase 2: Your total income from phase 1 and phase 2 is the sum of payoffs of both phases. At the end of the experiment your total income will be converted into Euro with an exchange rate of 1 Euro for 80 points.

A.4.2 Feedback screen

(translated from German)