JENA ECONOMIC RESEARCH PAPERS

2011 – 066

Leading by Words in Privileged Groups

by

Johannes Weisser

www.jenecon.de

ISSN 1864-7057

The JENA ECONOMIC RESEARCH PAPERS is a joint publication of the Friedrich Schiller University and the Max Planck Institute of Economics, Jena, Germany. For editorial correspondence please contact markus.pasche@uni-jena.de.

Impressum:

Friedrich Schiller University Jena
Carl-Zeiss-Str. 3
D-07743 Jena
www.uni-jena.de

Max Planck Institute of Economics
Kahlaische Str. 10
D-07745 Jena
www.econ.mpg.de

Jena Economic Research Papers 2011 - 066

Leading by Words in Privileged Groups*

27.12.2011

Johannes Weisser – IMPRS Uncertainty – MPI for Economics Jena

Abstract

Koukoumelis et al. (2010, 2012) have shown that one-way communication enhances contributions to public goods. We investigate the effectiveness of one-way communication, when the benefits from the public good are asymmetric and the sender of a message is the main beneficiary of cooperation. Our results show that, in the absence of communication opportunities, contribution behavior may be inversely related to other group members' marginal benefits from the public good. The effectiveness of one-way communication, however, remains unaffected even though compliance with a sender's suggestion to cooperate generates unfavorable payoff inequalities for message receivers. The results also indicate that one-way messages have to relate to the experimental game to enhance cooperation. Merely "giving someone a voice" is not sufficient.

Keywords: Public goods; One-way communication; Privileged groups; Asymmetric marginal benefit

JEL Classification: C72; C91; C92; D74; H41

*Acknowledgments: This paper benefited greatly from discussions with Oliver Kirchkamp, Werner Güth, Vittoria Levati, Sebastian Krügel, Matthias Uhl, and all participants of the ESI and IMPRS seminars in Jena. Nadine Marmai, Jochen Bick, Tina Hilbig, and Evgenia Grishina provided valuable support in conducting the experiments.

1 Introduction

In public good provision, the dictate of monetary payoff maximization predicts inefficient outcomes for the collective (Olson, 1965). While decades of experimental research demonstrate the prevalence of social preferences (see, e.g., Fehr and Fischbacher, 2002), a general willingness to withstand selfish acts is not sufficient to resolve the inefficient provision of public goods. Since individual actors' decisions are typically highly interdependent (e.g., Fischbacher et al., 2001, Fischbacher and Gächter, 2010), successful collective action also requires a high level of coordination.

In two recent public goods experiments, Koukoumelis et al. (2010, 2012) have shown that one-way communication, i.e., a free-form text message sent from one group member to his co-players before contribution decisions are made, can provide the means for successful coordination. The main facts that have emerged from these experiments are (i) that most messages entail specific suggestions for efficient contributions and (ii) that participants generally follow these suggestions. Such one-way communication is a useful simplification of real world situations in which communication is uni- rather than multilateral. Speeches as a tool to encourage cooperation when formal authority is lacking are one example. It is therefore important to identify the circumstances which are conducive to the cooperation-enhancing effect of one-way communication as well as its underlying mechanisms.

This study reports on a one-shot experiment with asymmetric benefits from the public good. More specifically, we consider a situation in which the communicator, i.e., the sender of a message, is also the main beneficiary of cooperation. Apart from its intuitive appeal, this scenario might challenge the effectiveness of one-way communication if those who send messages rely on efficient contribution suggestions as witnessed in previous experiments. Arguably, such prompts are even more likely if communicators benefit disproportionally from cooperation. Compliance with such suggestions, however, might suffer for at least two reasons. One such reason is the inequality in payoffs associated with an efficient provision of the public good. Since compliance with any suggestion is voluntary, it is likely to depend on the desirability of the projected outcome. Inequality averse subjects (Fehr and Schmidt, 1999, Bolton and Ockenfels, 2000) might thus be less likely to follow prompts to cooperate when the benefits from cooperation are asymmetric. Another reason is that compliance with contribution suggestions might depend on the recipients' perception of the sender's intent. Parallel to findings on direct reciprocity, i.e., responses to actions (see, e.g., Falk et al., 2008), intentional ambiguity might diminish cooperative responses

to written prompts. While any message which encourages cooperation in a social dilemma might be perceived as selfish, this is arguably more likely if the sender also benefits disproportionally from the public good.

In his topology of groups, Olson (1965) identified the particular asymmetry in benefits from cooperation, which we described before, in what he called "privileged groups." Such collectives consist of two types of actors: high-benefit group members for whom the benefits from the public good outweigh the costs for providing it and low-benefit members, for whom the opposite is the case.[1] Olson's labeling already signifies the argument that, given actors care only about their own monetary outcome, privileged groups should enjoy higher quantities of the public good than normal, non-privileged groups since the latter uniquely consist of low-benefit members.[2] The only two experiments that explicitly examined privileged groups are not univocal with respect to this conjecture.[3] Glöckner et al. (2011) report that the presence of a high-benefit member inhibits the cooperation of low-benefit members, although the former makes efficient contributions. As a consequence, privileged and normal groups enjoy similar quantities of the public good. Reuben and Riedl (2009), in contrast, confirm Olson's conjecture as correct when punishment opportunities are unavailable. When sanctioning of (mis)behavior is allowed, however, privileged groups lose their status completely as low-benefit members largely refuse to react to punishment by high-benefit members. Reuben and Riedl (2009) thus demonstrate that the term "privileged" can be misleading when it comes to the effectiveness of measures against free riding.

This study reports on the first experiment comparing the effectiveness of one-way communication in normal vis-à-vis privileged groups.[4] The addition of baseline conditions without any opportunities to communicate also allows for insights with respect to the conjecture that privileged groups enjoy higher quantities of the public good. The results indicate no such advantage. On the contrary, low-benefit members contribute even less when matched with a high-benefit member, which reveals considerable inequality

[1] Examples for privileged groups include the international efforts to fight terrorism, where primary targets such as the U.S. and the U.K. would still have an incentive to provide the funds needed even if no other country had such an incentive. Scientific projects may share similar characteristics. Imagine, e.g., a graduate student who needs a co-authored study to be completed for his Ph.D.

[2] This argument is consistent with experimental evidence confirming that the marginal per capita return for providing the public good is a strong predictor of behavior (see, e.g., Marwell and Ames, 1979, Isaac and Walker, 1988, Ledyard, 1995, Fisher et al., 1995).

[3] Palfrey and Prisbrey (1996, 1997) and Brandts and Schram (2001) allow for the "accidental" formation of privileged groups. Due to the information structure in these experiments, however, participants were not aware what kind of group was formed in a given period.

[4] In fact, we are not aware of any public goods experiment that simultaneously allows for communication and asymmetric benefits from cooperation.

aversion. The effectiveness of one-way communication, however, is not affected by group composition. Even messages from senders who benefit disproportionally from cooperation elicit substantial increases in contributions from low-benefit members. This is also surprising because the post experimental questionnaire reveals that high-benefit members' messages are suspected of serving selfish purposes.

What makes one-way communication effective? Koukoumelis et al. (2010, 2012) emphasized the coordinative role of specific contribution suggestions. In this study, we test an alternative explanation which focuses on the basic fact that communication enables social interaction ex ante any binding decisions. This aspect is essential to some social psychological explanation attempts as to why (multilateral) communication enhances cooperation. Based on social identity theory (Tajfel and Turner, 1986), it is argued that communication may create affinity for the group and, hence, more cooperation (see, e.g., Orbell et al., 1988). Accordingly, well structured argumentation might not even be necessary for communication to strengthen contributions, and "giving someone a voice" might be sufficient. We test this conjecture for one-way communication. Specifically, we contrast a baseline with two communication conditions, only one of which allows for messages related to the experimental game. Our results show that only game-relevant messages may increase cooperation beyond the baseline condition. This finding is in accordance with results from previous studies (Dawes et al., 1977, Bouas and Komorita, 1996) which compared unrestricted multilateral communication to a condition that enabled discussion about predefined topics but not the dilemma situation itself.

The remainder of the study is organized as follows. Section 2 describes the experimental design and procedures. Qualitative hypotheses are discussed in Section 3. Section 4 reports on the experimental results regarding contribution decisions, the communication content, and the post experimental questionnaire. Section 5 summarizes the findings and concludes.

2 Experimental design

2.1 The basic public goods game

In all treatments, participants interact for a single period in a linear voluntary contribution mechanism (see, e.g., Isaac et al., 1984). Every group consists of three members, $i = \{1, 2, 3\}$, each of whom is endowed with E=25 ECU (Experimental Currency Units). All

group members simultaneously decide about their individual contributions, $c_i \in [0, 25]$, to the public good. Every ECU a decision maker keeps is worth 1 ECU to him. Additionally, he earns β_i ECU for every ECU he or any other group member contributes to the public good. We refer to β_i as individual i's marginal per capita return (MPCR) or marginal benefit (from cooperation). Individual i's payoff can be summarized by

$$(1) \quad \pi_i = 25 - c_i + \beta_i \times \sum_{j=1}^{3} c_j \quad ,$$

where $\sum_{j=1}^{3} c_j$ is the sum of contributions in i's group.

Usually, public goods games are parameterized such that they satisfy $0 < \beta_i < 1, \forall i$ and $\sum_{i=1}^{3} \beta_i > 1$. The first inequality implies that a monetary payoff maximizing decision maker has a dominant solution to contribute nothing, since he incurs a net loss of $1-\beta_i > 0$ ECU for every ECU he contributes. However, due to the second inequality it is efficient to contribute the entire endowment in the sense that it maximizes the sum of payoffs in a group.

2.2 Experimental treatments

The experiment follows a 2×3 between subjects design. For the first dimension we vary the composition of the groups by introducing two types of players. A *normal group* consists of three *low-benefit members*, whose individual marginal benefit from cooperation is equal to $\beta_L = 0.6$. *Privileged groups*, in contrast, comprise two *low-benefit* and one *high-benefit member*.[5] The latter's MPCR is set to $\beta_H = 1.6$. Classification into player types takes place at random. Note that this implementation of privileged groups mirrors that in Reuben and Riedl (2009) and is consistent with the definition in the sense of Olson (1965).[6]

For the second dimension we vary the available communication technology. The baseline (or B) treatment offers no opportunity to communicate. In two further conditions, we allow for one-way communication. We give one randomly chosen group member, i.e., the communicator, the opportunity to send a written message to his co-players prior to the contribution decisions.[7] In privileged groups, the communicator is also the high-

[5] In the experiment, high- and low-benefit members were labeled A- and B-types, respectively.

[6] The only difference compared to Reuben and Riedl (2009) is that in their setup, the types' MPCRs are set to $\beta_L = 0.5$ and $\beta_H = 1.5$. This discrepancy is deliberate as we want to preserve the social dilemma character of the decision situation also for the subsets of two low-benefit members.

[7] In the following, we use the term "follower" to denote those group members who cannot communicate in treatments which allow for one-way communication.

benefit member. To ensure that messages are non-verifiable (and thus cheap talk), the communicators' contribution levels could not be identified. The two conditions allowing communication differ only with respect to the permissible communication content. While relevant one-way communication allows for messages that relate to the decision situation, irrelevant one-way communication does not.[8] The two treatments are abbreviated to *RC* and *IC*, respectively. Table 1 summarizes the experimental treatments.

Table 1: Experimental treatments

Treatment	**Normal groups** *(3 low-benefit members)*	**Privileged groups** *(1 high-, 2 low-benefit members)*
B	no communication	no communication
RC	relevant communication, low-benefit communicator	relevant communication, high-benefit communicator
IC	irrelevant communication, low-benefit communicator	irrelevant communication, high-benefit communicator

2.3 Procedures

The experiment was programmed in z-Tree (Fischbacher, 2007) and conducted in June and July 2011 in the experimental laboratory of the Max Planck Institute of Economics in Jena, Germany. The participants were undergraduate students from the Friedrich Schiller University Jena.[9] They were recruited using the ORSEE system (Greiner, 2004). Upon arrival, participants were seated at visually separated computer terminals. The instructions were distributed and then read aloud to establish common knowledge.[10] The comprehension of the experimental rules was tested by means of a control questionnaire. Any questions were answered privately at the participants' seats.

In both treatments allowing communication, the communicator had up to four minutes to compose his message but was also able to finish ahead of time. The implementation of relevant and irrelevant communication differed only with respect to the set of restrictions to the otherwise free-form messages. In both cases, the communicator was neither allowed to violate anonymity, nor to promise side payments, nor to threaten the other

[8] The labels *game-relevant* and *game-irrelevant* would be more appropriate as we do not mean to prejudge any treatment effects. The simplification is intended to serve readability and in accordance with previous studies (Dawes et al., 1977, Bouas and Komorita, 1996).

[9] None of the subjects had previously participated in a public goods game experiment with communication opportunities.

[10] All instructions can be found in Appendix C.

group members with anything that might occur after the experiment. As for irrelevant communication, the messages were additionally restricted to statements not relating to the experiment.[11] All messages were collected, screened by an experimenter and delivered simultaneously. If any restrictions were violated, the message was blocked and the communicator was notified about his misconduct. Such an incident happened only once in a normal group. It was common knowledge that (a) the messages were cheap talk (i.e., costless, non-binding, and non-verifiable), (b) all group members received exactly the same message from the group's communicator, and (c) only after having read the communicator's message could the group members decide simultaneously on their individual contributions.

The experimental procedure included four practice periods in which participants were matched with computerized agents programmed to choose contributions randomly. In treatments RC and IC, the practice periods did not include the communication stage. After the experiment, all participants received information about the other group members' individual contributions. Before the feedback was delivered, we distributed an unincentivized questionnaire, to elicit the subjects' identification with their group, first order action beliefs, and the followers' perception regarding the intention underlying the communicator's message.[12,13] The questionnaire items' exact wording can be found in Appendix A.

Sessions lasted 60 minutes on average. Payoffs were quoted in ECU, where 1 ECU = 20 euro cents. High- and low-benefit group members earned on average 16.60 and 10.10 euro, respectively, including a 2.50 euro show-up fee.

[11] Implementing irrelevant communication in this way has the shortcoming that communicators may try to circumvent the restriction, e.g., by using clever wording or metaphors. An alternative would be to record the communicators' messages before distributing the instructions. While the latter method precludes any reference to the specifics of the public goods game, it does not entirely prevent the communicator from relating his message to the experiment. More specifically, subjects experienced in experimental paradigms such as the trust game might expect cooperation to be an important element of the experiment. As communicators, they might then attempt to promote "full cooperation" even without knowledge of the game and its parametrization.

[12] Since our main focus was on contribution decisions, we followed the advice in Gächter and Renner (2010) and elicited beliefs without payment.

[13] Delivering feedback after the questionnaire precluded the possibility that the participants' answers were affected by statements about payoffs.

3 Hypotheses

3.1 No communication – baseline

Given preferences to maximize own monetary payoffs, group composition should not affect behavior since both types of players have dominant strategies. A high-benefit member should contribute his whole endowment since he earns a net benefit of $1.6 - 1 = 0.6$ ECU for every ECU he contributes. A low-benefit member, in contrast, is expected to contribute nothing to the public good. Decades of experimental research, however, have rejected the latter prediction (see, e.g., Chaudhuri, 2011) and it is thus reasonable to expect some positive contributions also from low-benefit members.

If we relax our assumptions and allow for preferences for equitable monetary outcomes, the behavioral predictions change. Outcome-based models (e.g., Fehr and Schmidt, 1999, Bolton and Ockenfels, 2000) take into account that the privileged member benefits disproportionally from cooperation within the group. In fact, the only uniform contribution decision by all group members which leads to strict equality in payoffs is when overall contributions equal zero. Strictly positive contributions from low-benefit types increase the inequality in payoffs between the decision maker and the high-benefit member by exactly the same amount since $\beta_H - \beta_L = 1$. Notice that by the same argument, it is exclusively the low-benefit group members' contributions which generate payoff discrepancies across player types. A low-benefit group member who is sensitive to the inequality in payoffs is thus predicted to contribute less in privileged than in normal groups.

In both, normal and privileged groups, a contribution always benefits the group more than it costs the contributor. The aggregate effect on overall payoffs, however, differs according to group composition. While a contribution of 1 ECU generates a total payoff of $3 \times 0.6 = 1.8$ ECU in normal groups, this figure amounts to $2 \times 0.6 + 1.6 = 2.8$ ECU in privileged groups. This discrepancy might lead to differences in behavior if participants' choices are guided by concerns for overall efficiency (see, e.g., Engelmann and Strobel, 2004). While a preference for overall efficiency coincides with monetary payoff maximization for high-benefit types, it counteracts the incentive to free ride for low-benefit members.

In summary, it can be said that high-benefit members are expected to contribute their whole endowment under monetary payoff maximization. Neither inequality aversion nor a preference for efficiency contradict this prediction. We propose our first hypothesis:

Hypothesis 1. *High-benefit group members contribute their entire endowment.*

Both, maximization of the own monetary payoff and preferences for equality in payoffs, predict lower contributions from low- than from high-benefit types, leading to Hypothesis 2:

Hypothesis 2. *High-benefit group members contribute more than low-benefit members.*

It is not straight-forward how the composition of groups will affect low-benefit types. Preferences for efficiency suggest higher contributions, while preferences for equality suggest lower contributions in privileged than in normal groups. While, strictly speaking, we are not in a position to propose a precise behavioral hypothesis ex ante, the observed behavior will help us to make an inference about the relative strength of the opposing effects ex post.

3.2 One-way communication

In social dilemma experiments, participants seize communication opportunities usually in an attempt to mitigate the free rider problem. In our setup, however, high-benefit members are essentially unproblematic since they themselves have an incentive to provide the public good. As a consequence, neither relevant nor irrelevant one-way communication are expected to change the behavior of high-benefit members.

Hypothesis 3. *High-benefit group members contribute their entire endowment irrespective of any communication opportunities.*

Since high-benefit types should remain unaffected, we exclusively concentrate on the low-benefit types when discussing the potential effects of one-way communication.

3.2.1 Relevant communication

If we assume common knowledge of rationality and preferences to maximize monetary outcomes, then costless, non-binding and non-verifiable messages are, of course, pure cheap talk. Consequently, they should not affect the behavior of low-benefit members. However, previous studies on (relevant) one-way communication in normal groups have rejected this prediction. Even for the case in which subjects interact for a single period, relevant one-way messages have a strong and positive effect on overall cooperation (Koukoumelis et al., 2012). Since our setup is different from theirs only with respect to the parametrization of the public goods game, we expect to replicate the effect for normal groups.

Hypothesis 4. *In normal groups, contributions are higher with relevant one-way communication than with no communication.*

Koukoumelis et al. (2010, 2012) observe that the majority of messages contain specific contribution suggestions, which mostly point to full cooperation, and that these suggestions are followed by the majority of participants. The authors conjecture that messages function as a coordination device, which is plausible as, e.g., preferences for conditional cooperation can transform the social dilemma into a coordination game with multiple Pareto-ranked equilibria (see, e.g., Sen, 1967, Fehr and Fischbacher, 2002).[14] It is also consistent with a stream of literature in social psychology (Kelley and Thibaut, 1978, Kelley, 1979) which argues that the (experimentally) "given" payoff matrix might diverge from the "effective" matrix. Or, as the sociologist Peter Kollock (1998, p. 193) put it: "*There is, after all, no guarantee that subjects play an experimental game as intended by the researcher ....*"

There is little reason to expect that high-benefit communicators will be less likely than low-benefit communicators to try and promote high cooperation. After all, they have an even greater interest to do so, as they benefit disproportionally from the public good. Whether they are similarly successful will depend on the desirability of high cooperation for low-benefit types in privileged groups. Assume, for instance, that a communicator suggests full cooperation. Unanimous compliance with such a suggestion would lead to higher overall payoffs in privileged than in normal groups. The distribution of payoffs, however, would be equal in normal and maximally unequal in privileged groups since the total surplus is solely earned by the high-benefit communicator. Thus, if low-benefit types strive for efficiency, their compliance with high contribution suggestions might be high also in privileged groups. Compliance and thus the cooperation-enhancing effect of one-way communication might seriously suffer, though, if low-benefit types dislike payoff inequalities.

Contribution suggestions by communicators are not only cheap talk but may also exhibit some self-serving character. If a communicator believes (with positive probability) that high contribution suggestions will, on average, lead to higher contributions, he will always try to promote cooperation. While this is true for both types of groups, the self-serving character of contribution suggestions seems intuitively more salient in the case of asymmetric marginal benefits, where communicators are also the main beneficiaries of

[14] According to Fischbacher et al. (2001) conditional cooperation can either be viewed as a consequence of, e.g., fairness motives or as a preference in itself.

cooperative play. We know, e.g., from research on trust games, that intentions are crucial for reciprocal action (see, e.g., McCabe et al., 2003, Falk et al., 2008). We are not aware of a similar result which connects the effectiveness of communication to the perceived intention underlying the communication content. If such a relationship exists, however, a higher saliency of selfish intentions is likely to lead to lower compliance with suggestions to cooperate.

Depending on the desirability of efficiency or payoff equality and the sensitivity to perceived intentions, one-way communication, or, more precisely, specific contribution suggestions, might be more or less effective in privileged vis-à-vis normal groups. We suspect that the net effect will be correlated with how behavior of low-benefit members compares across both types of groups. Lower cooperation in the presence of the high-benefit member, for instance, would signal inequality aversion, which should weaken the effectiveness of one-way communication.

3.2.2 Irrelevant communication

It has been a long-standing conjecture in social psychology that communication affects cooperation in social dilemmas by manipulating the social environment (see, e.g., Orbell et al., 1988). The same may be true for one-way communication. By sending their message, communicators might create an environment in which participants perceive their group no longer as anonymous and randomly assembled but rather as a social entity bound together by fate. Research in sociology and social psychology suggests that even arbitrary categorization can lead to higher cooperation in social dilemmas (see, e.g., Simpson, 2006, Yamagishi and Mifune, 2008). Thus, it might not be necessary that the communicators' messages relate to the experimental game for one-way communication to be efficiency-enhancing. "Giving one group member a voice" might be sufficient to elicit identification with the group and enhance cooperation. This line of reasoning is equally applicable to normal as well as privileged groups and suggests the following hypothesis.

Hypothesis 5. *Irrelevant one-way communication, i.e., messages unrelated to the experimental game, leads to higher cooperation than no communication.*

Previous empirical findings cast doubt on the validity of this hypothesis. In two separate experiments, Dawes et al. (1977) as well as Bouas and Komorita (1996) find that even irrelevant multilateral communication (i.e., communication among all group members on a topic other than the experiment) does not lead to higher cooperation rates

than no communication at all. If "giving everyone in a group a voice" does not suffice to promote cooperation, it is doubtful that giving only one person a voice will.

4 Results

We conducted one session per treatment with 30 participants each. The data analysis is based on 10 group averages per treatment since communication allows for correlated decisions within groups.[15] Consequently, data is also averaged by group for analysis on a more disaggregate level, i.e., for low-benefit members or followers. Unless stated otherwise, we will use two-sided Wilcoxon rank sum tests for comparisons across treatments and two-sided Wilcoxon signed rank tests to compare matched pairs within treatments.[16] We first investigate contribution decisions. Afterward, we advance to the analysis of the communication content and the data from the post experimental questionnaire.

4.1 Contribution decisions

Figure 1, panel a, depicts the average group contributions in normal groups for all three conditions. In the baseline, average contributions amount to 12 ECU, or almost half the endowment. This is consistent with previous findings (see, e.g., Ostrom, 2000). Relevant and irrelevant one-way communication elicit average contributions of 19.2 and 9.5 ECU, respectively. We thus find support for Hypothesis 4 but not for Hypothesis 5 since only relevant one-way communication seems to lead to an increase in cooperation. Non-parametric tests confirm this impression. While relevant messages lead to significantly higher contributions both with respect to the baseline ($p = 0.01$) and irrelevant messages ($p = 0.004$), the latter two conditions show no significant differences ($p = 0.44$). Moreover, the same result holds if we exclude the communicators' decisions ($p = 0.02$, B vs. RC; $p = 0.65$, B vs. IC; $p = 0.02$, RC vs. IC).

The general picture for privileged groups, which is depicted in panel b of Figure 1, is qualitatively similar to that of normal groups. It is again relevant communication which elicits the highest average contributions, namely 16.8 ECU. The relative advantage with respect to the baseline and irrelevant communication, however, seems smaller

[15] In case of irrelevant communication, we exclude the group for which the message was blocked from our entire analysis. All results are robust to its inclusion.

[16] The null hypothesis of a Wilcoxon rank sum test states that both independent samples are drawn from the same population. A Wilcoxon signed rank test requires for any observation in one sample a unique counterpart in the paired sample. It tests the null hypothesis that the difference in medians between the samples is equal to zero.

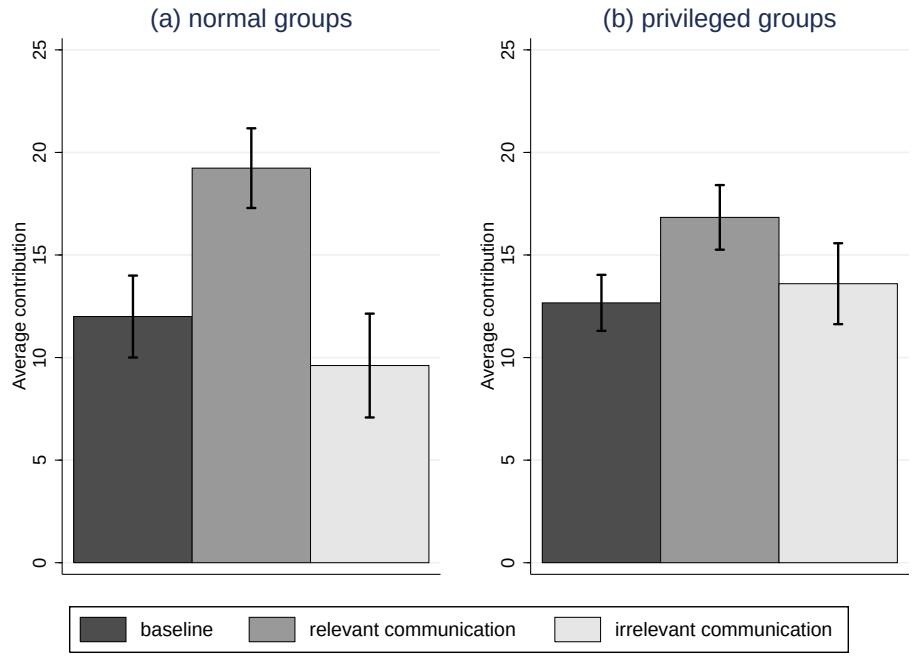


Figure 1: Average group contributions and 95% confidence intervals

than in normal groups as contributions in the latter two conditions average 12.6 and 13.6 ECU, respectively. The null hypothesis of equality in distributions is rejected on the 10% significance level only for the comparison between relevant communication and the baseline condition ($p = 0.053$). No other pairwise comparison reveals a significant difference ($p = 0.16$, RC vs. IC; $p = 0.82$, B vs. IC). We summarize with Result 1.

Result 1. *We confirm that relevant one-way communication elicits higher average cooperation than no communication in a public goods game with normal groups. The effect also proves to be robust in privileged groups. The efficiency-enhancing effect of one-way communication is, however, confined to (game-)relevant messages. Merely "giving one group member a voice" does not suffice for one-way communication to increase cooperation. We do (do not) find significant differences between the effects of relevant and irrelevant messages in normal (privileged) groups.*

In order to assess the alleged advantage of privileged over normal groups, we compare the contributions across the two panels of Figure 1. While average contributions are virtually identical in the two baseline conditions, contributions are higher in normal than in privileged groups when relevant communication is allowed. This relation is reversed when we consider irrelevant communication. Pairwise comparisons, however, fail to reject the null hypothesis of equality in distributions for all three conditions ($p = 0.97$ for B; $p = 0.18$ for RC; $p = 0.22$ for IC). We summarize with Result 2.

Result 2. *Irrespective of whether we allow for no communication, relevant, or irrelevant one-way communication, we do not find support for the claim that contributions are higher in privileged than in normal groups.*

Both, results 1 and 2, have to be treated cautiously, as average group contributions may mask considerable heterogeneity in behavior across different player types. Figure 2 depicts type-specific contributions, where those of low-benefit members represent group averages.[17] Let us consider high- and low-benefit members in privileged groups. Hypotheses 1 and 3 hold that high-benefit members contribute their entire endowment irrespective of any communication opportunities. Low-benefit types are expected to contribute significantly less (see Hypothesis 2) but might be affected by one-way communication. Our data lends support to these conjectures. High-benefit members contribute, on average, between 88% and 98% of their endowment. In fact, 24 out of 30 individual observations are exactly equal to the full endowment. Two-sided t-tests do not detect a location shift away from 25 ECU in any of the conditions ($p = 0.12$ for the B, $p = 0.26$ for RC, $p = 0.34$ for IC). Furthermore, pairwise comparisons fail to reject the null hypothesis of equal distributions across conditions with and without communication opportunities (smallest p-value: $p = 0.28$ for RC). While high-benefit group members' contributions are very close to full efficiency, the contributions from low-benefit members fall short of that mark. On average, they contribute 29%, 56.8%, and 32.6% of their endowment in treatments B, RC, and IC, respectively. Comparing contributions between the different player types, the null hypothesis is rejected on the 1% significance level for both treatments B and IC (both $p = 0.005$) and on the 10% level for treatment RC ($p = 0.08$). Unlike high-benefit members' decisions, those of low-benefit members are sensitive to the availability of communication opportunities. This is already visible on the group level and becomes even more evident on the individual level, where the efficient contribution is observed only once in each treatment B and IC but nine times in treatment RC. Pairwise comparisons of low-benefit types' contributions reveal that relevant one-way messages lead to higher cooperation than the baseline treatment, though irrelevant messages do not ($p = 0.04$, B vs. RC; $p = 0.97$, B vs. IC). As for relevant communication, contributions are marginally significantly higher than for irrelevant communication ($p = 0.095$). Figure 2 also suggests that relevant messages lead to similar increases in low-benefit members' contributions in both types of groups. These findings qualify Result 1 and show that

[17] The bars for normal groups are identical to those in panel a of Figure 1, since normal groups exclusively consist of low-benefit members.

relevant one-way communication is effective in privileged groups, specifically as it leads to higher contributions by low-benefit members. We summarize with Result 3.

Result 3. *In privileged groups, behavior is type specific. High-benefit group members contribute almost their entire endowment irrespective of any communication opportunities. Low-benefit members contribute significantly less than high-benefit members, unless relevant one-way messages are possible. Relevant one-way communication affects low- but not high-benefit members' decisions. Irrelevant messages are never effective.*

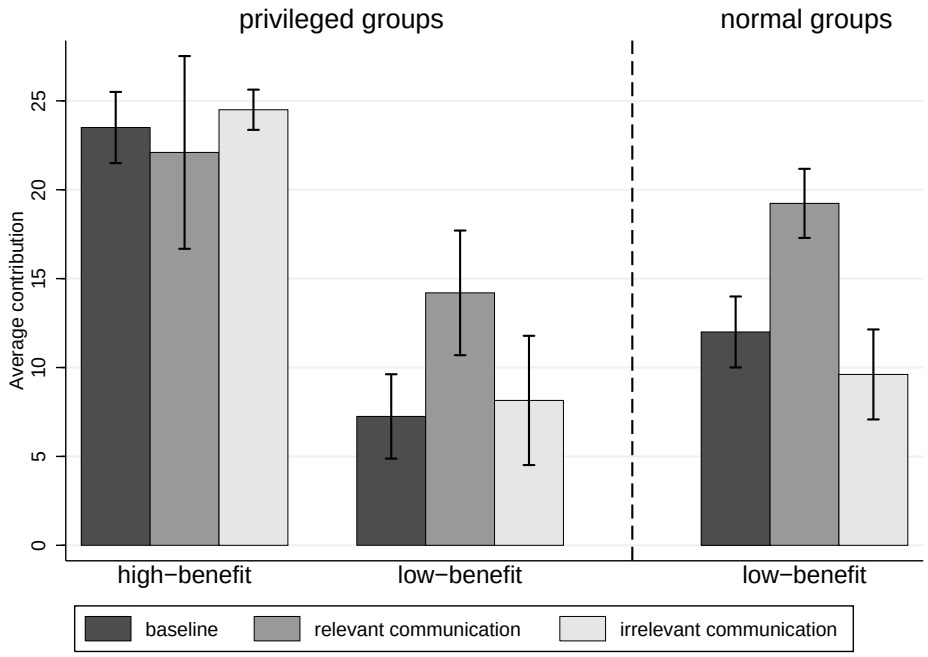


Figure 2: Type-specific contributions and 95% confidence intervals

Result 2 does not support the presupposed advantage for privileged groups but, according to Result 3, this is not due to the lack of contributions from high-benefit members. It is thus of interest to compare the behavior of low-benefit members across the different types of groups. Figure 2 shows that low-benefit members' contributions are lower in privileged groups for all three treatments. The discrepancy between privileged and normal groups is more pronounced in treatments B (4.7 ECU) and RC (5.0 ECU) than in treatment IC (1.5 ECU). Pairwise comparisons reveal that the difference is significant in the baseline treatment ($p = 0.03$), weakly significant with relevant messages ($p = 0.09$), and far from being significant with irrelevant messages ($p = 0.57$). We summarize with Result 4.

Result 4. *Low-benefit members' contribution decisions are sensitive to the presence of the high-benefit member as contributions tend to be lower in privileged than in normal groups when communication opportunities are either absent or relevant messages are allowed.*

This result deserves some discussion. First, note that Reuben and Riedl (2009) do not find that low-benefit members' decisions are sensitive to group composition. This discrepancy might be explained by their choice of a repeated partners design which allows for reputation building. The opportunity to act strategically might induce low-benefit members to cooperate even if they dislike payoff inequalities. Glöckner et al. (2011) report that the presence of a high-benefit member deters cooperation of low-benefit members but only after a restart following ten initial periods. Our result is complementary as it shows that this effect does not rely on long sequences of repetitions. Result 4 is also informative with respect to the effectiveness of relevant one-way communication in privileged groups. It demonstrates that low-benefit types' contribution decisions are sensitive to payoff inequalities. We argue in Section 3 that such sensitivity could weaken the cooperation-enhancing effect of one-way communication in privileged groups. Results 1 and 3, however, show that relevant one-way messages are similarly effective in both types of groups. One possible explanation is that efficiency concerns, when voiced by the communicator, become increasingly important in the decision process. We discuss this in more detail in Section 4.2.

Results 1 through 3 showed that relevant one-way communication yields an increase in contributions to the public good irrespective of group composition. But who benefits from this increase in cooperation? In order to answer this question, we investigate participants' payoffs which are summarized in Table 2.

Table 2: Payoffs by treatment and player type

	Treatment	Average	Communicator / high-benefit	Follower / low-benefit
Normal groups	B	9.46	–	9.46
	RC	10.66	10.64	10.67
	IC	9.08	9.67	8.79
Privileged groups	B	12.09	14.99	10.64
	RC	13.61	19.27	10.78
	IC	12.42	15.67	10.79

Note: Payoffs are denoted in Euro and include the 2.50 euro show-up fee.

In normal groups, group-averaged payoffs are significantly higher in treatment RC than

the baseline and treatment IC (both $p = 0.01$). The additional earnings benefit followers and, to a minor extent, also communicators. While followers in RC earn significantly more than both followers in IC and participants in the baseline treatment (both $p < 0.02$), the respective comparisons are weakly significant ($p = 0.07$) and insignificant ($p = 0.18$) for communicators.

In privileged groups, the increase in average earnings associated with relevant one-way communication is less pronounced. In particular, average payoffs are only weakly significantly higher in treatment RC than in B and not significantly different for all other pairwise comparisons ($p = 0.054$, B vs. RC, $p = 0.82$, B vs. IC, $p = 0.16$, RC vs. IC). Since high-benefit members benefit disproportionally from the public good, it is not surprising that they earn significantly more than low-benefit members in all three treatments (all $p < 0.01$). A comparison between treatments, however, also reveals that high-benefit members are the only beneficiaries of the opportunity to send (game-)relevant messages. While low-benefit members in privileged groups earn virtually the same in all three treatments (all $p > 0.12$), high-benefit members earn significantly more in treatment RC than in the baseline ($p = 0.04$).[18] In fact, while the average earnings differential between player types amounts to 4.35 and 4.88 euro in B and IC, respectively, it roughly doubles to 8.49 euro in treatment RC.[19]

4.2 Communication content

We categorize the messages in the treatment RC according to the scheme described in Table 3. All methodological details can be found in Appendix B. Table 4 reports the categories' relative frequencies of appearance in the messages' argumentation.

On average, a message entails arguments according to 4.8 and 4.2 categories in normal and privileged groups, respectively. We treat this as a sign that most communicators took their task seriously, trying to make an impact with their messages. In many respects, the messages are very similar in both types of groups. In normal as well as privileged groups, the majority of messages incorporates suggestions for concrete (category 1) and efficient (category 2) contributions coupled with an emphasis on the importance of conformity within the group (category 3).[20] Statements pointing to fairness (category 7) or team

[18] No other pairwise comparison for high-benefit members reveals significant differences ($p = 0.93$, B vs. IC; $p = 0.11$, RC vs. IC).

[19] The earnings differential between high- and low-benefit members is significantly larger in RC than in B ($p = 0.04$), weakly significantly so in RC than in IC ($p = 0.096$), and not different for B and IC ($p = 0.97$).

[20] While this is in accordance with the results for treatment C in Koukoumelis et al. (2012), note that

spirit (category 8), which are most likely meant as auxiliary arguments to substantiate the contribution suggestions, are less frequent but present for both types of groups.[21] Half of all communicators signal their contribution intentions (category 9). Most of them specifically promise to contribute their entire endowment. Note that such a promise is credible in privileged groups, as communicators are high-benefit members, but not in normal groups.

Apart from these similarities there are some noticeable differences in the messages' content between the different types of groups. In fact, comparing the distributions of arguments between types of groups, a one-sided Fisher exact test gives some indication that the distribution of category appearance depends on group composition ($p = 0.07$). Most strikingly, we observe payoff calculations for seven out of ten normal groups but only for one privileged group. This difference is significant according to a two-sided Fisher exact test ($p = 0.02$). One rationalization for this observation might be that payoff calculations facilitate the demonstration of possible gains from cooperation but, at the same time, may also highlight the salience of payoff inequalities. High-benefit communicators might thus avoid computations altogether if they expect low-benefit types to be inequality averse. Instead, they might try to point out the possibility of efficiency gains in different ways, which is what we observe. Six high-benefit communicators mention group payoff maximization (category 5) – twice as many as in normal groups. This difference is, however, not significant ($p = 0.37$, two-sided Fisher exact test).

We have not categorized messages in treatment IC since they tend to be very heterogenous in their content. Many of them include only one "argument," if any. Three messages, for instance, merely incorporate the wish that the other group members may enjoy the experiment. Other messages just consist of nursery rhymes. Four messages are seemingly intended to entertain the receivers as they contain a joke or humorous summary of the daily news. The only message that was blocked referred to the upcoming contribution decision and the possible payoffs.[22] Perhaps most rich in content, six messages relate to general fairness ideas ("love your neighbor as yourself," "harm set, harm get") or invoke solidarity principles (three senders mention the slogan of Dumas' musketeers "one for all,

the relative frequency of suggestions is somewhat lower in the present study.

[21] Statements which relate to satisfaction (category 7) are absent in normal as well as privileged groups. We incorporate thie category in order to facilitate comparison with the results in Koukoumelis et al. (2010, 2012).

[22] We observe only one obvious attempt to circumvent the restrictions and still hint to to some desired contribution level. The sender of this message fabricates a story and mentions the number 25 wherever possible, but he never relates to the experiment. The average contribution from low-benefit members in that particular privileged group is 7.5 ECU.

Table 3: Description of the communication content categories

Category	Argument	Description
1	Suggestion	Suggestion (point or interval) of how much to contribute to the project. The suggestion, whether implicitly or explicit, must be unambiguous.
2	Efficient suggestion	Implicit or explicit suggestion to contribute the whole endowment.
3	Conformity	Emphasis on the need that all group members conform to the suggestion.
4	Payoff calculation	Calculation of the payoff associated with the suggestion.
5	Group payoff maximization	Explicit argument that the suggested amount maximizes the group payoff, or conjecture that participants are interested in maximizing the group payoff.
6	Satisfaction	Explicit argument that people should be content with following the communicator's suggestion.
7	Fairness	Explicit reference to fairness or just behavior.
8	Team spirit	Statement promoting the willingness to cooperate as part of a team.
9	Promise	Pledge to contribute some specific amount.

Table 4: Relative frequency of the arguments' presence with relevant communication

Category	Argument	Frequency	
		Normal groups	Privileged groups
1	Suggestion	0.8	0.7
2	Efficient suggestion	0.7	0.6
3	Conformity	0.9	0.6
4	Payoff calculation	0.7	0.1
5	Group payoff maximization	0.3	0.6
6	Satisfaction	0	0
7	Fairness	0.3	0.1
8	Team spirit	0.3	0.4
9	Promise	0.4	0.6

Note: The relative frequencies are based on 10 observations for each type of group.

all for one").

4.3 Post experimental questionnaire

4.3.1 Group identification

Some studies in social psychology argue that the effects of communication are driven by enhanced group identification. If this was the case, we should expect group members to identify more with their group in treatment RC than in the baseline. The ineffectiveness of irrelevant one-way communication, in turn, might be explained by the lack of such an increase in identification. The data from the post experimental questionnaire permits us to investigate these conjectures. We calculate a participant's mean identification score as his average response to all four items. Comparing group averages across treatments, we find for normal groups that identification is indeed weakly significantly higher in RC than B ($p = 0.06$) and not significantly different otherwise ($p = 0.97$, B vs. IC; $p = 0.12$, RC vs. IC). This result proves robust if we focus on group averages of non-communicators ($p = 0.096$, B vs. RC; $p = 0.73$, B vs. IC; $p = 0.32$, RC vs. IC). The data from normal groups is thus not inconsistent with previous conjectures.

The picture changes when we consider privileged groups. While, on the group level, mean identification scores are weakly significantly higher in RC than in the baseline ($p = 0.06$), this is not the case for low-benefit members ($p = 0.21$). Since it is precisely the low-benefit members who react to relevant communication in privileged groups, the

latter observation is inconsistent with the claim that communication works via enhancing group identification. A further puzzling observation is that irrelevant one-way messages lead to significantly higher identification than the baseline for group averages as well as for averages of low-benefit members ($p = 0.02$ and $p = 0.03$, respectively).[23] If communication was to affect cooperation via group identification, we should observe higher contributions in IC than in the baseline for privileged groups. The fact that we do not, casts doubt on the conjectured link between (higher) identification and enhanced cooperation. It is thus also possible that the increased identification in treatment RC for normal groups is a by-product rather the driving force behind the effectiveness of relevant one-way messages.

4.3.2 Message perception

In treatment RC, the majority of communicators makes specific contribution suggestions. Although these may always be interpreted as serving the communicator's monetary interest, we conjectured in Section 3 that such an interpretation was more likely in privileged groups, where the incentive structure highlights the communicator's interest in the public good. We also surmised that the perception that a suggestion serves a selfish purpose might reduce the rate of compliance. Result 3 shows, however, that relevant messages increase the cooperation of low-benefit members also in privileged groups. The question arises whether this is due to the fact that followers are unaware of the messages' potentially selfish character or that this property is unimportant for their decisions.

The questionnaire data (cf. item 9, Appendix A) might help to shed light on this question. Followers had to rate their degree of consent to a statement portraying the message they received as intended to maximize the communicator's own payoffs. The data shows that followers (averaged by group) have a significantly higher tendency to agree with this statement in privileged than in normal groups ($p = 0.001$).[24] Since it is not the lack of awareness, followers' choices must be insensitive with respect to perceived selfish purpose of messages. And indeed, the correlation between average follower contributions and their average responses to this questionnaire item turns out to be virtually zero ($\rho = 0.02$, $p = 0.97$). This insight is somewhat surprising as we know that perceived intentions are a crucial determinant of reciprocity (Falk et al., 2008). Despite the obvious discrepancy that the latter result deals with reactions to actions and not written statements, both situations

[23] For privileged groups, identification is not different for treatments RC and IC, neither for group averages nor for averages of low-benefit members.

[24] This is not the case for irrelevant messages ($p = 0.73$).

share a seemingly crucial aspect. The sum and distribution of payoffs always depend on some agents' willingness to react to one agent's attempt to stimulate cooperation.

4.3.3 Expected contributions

In Section 3 and in previous studies (Koukoumelis et al., 2010, 2012), we have argued that relevant one-way messages, and, more specifically, contribution suggestions may serve as a coordination device. This concept assigns a critical role to expectations as suggestions supposedly affect prior beliefs about what to expect from other group members. Conditional cooperators should be sensitive to such changes in expectations. The questionnaire data reported in Table 5 provides some support for these conjectures.[25] On the group level, average expectations proof to be significantly higher in treatment RC than in the baseline for both types of groups (both $p = 0.01$). Irrelevant messages, on the other hand, do not significantly affect average expectations compared to the baseline treatment ($p > 0.41$, for both types of groups).[26] Interestingly, we do not find significant differences between expectations of communicators in treatment RC and those of average participants in the baseline treatment ($p = 0.59$) for normal groups. The same holds for high-benefit members' expectations in treatments RC and B ($p = 0.29$).[27] This result

Table 5: Expected contributions by treatment and player type

	Treatment	Average	Communicator / high-benefit	Follower / low-benefit	
				about: high-benefit	about: low-benefit
Normal groups	B	12.4	–	–	12.4
	RC	18.1	16.2	–	19.1
	IC	11	8.2	–	12.4
Privileged groups	B	12.1	9.5	19.3	7.5
	RC	15.4	11.4	23.6	11.1
	IC	13.4	11.2	21.4	7.6

Note: In order to obtain one (average) measure per privileged group, we first calculate a low-benefit member's average expectation as the mean of what he expects from both his peers. Then, we average the expectations of all three group members.

[25] Cf. items 5 to 8 in Appendix A.

[26] Relevant messages elicit significantly higher average expectations than irrelevant ones only in normal but not in privileged groups ($p = 0.01$ and $p = 0.16$, respectively).

[27] Communicators of normal groups report significantly lower expectations for treatment IC than communicators in RC or the average member in the baseline ($p = 0.02$ and $p = 0.048$, respectively). There are no significant differences in expectations for high-benefit members for any combination of treatments ($p > 0.29$, for all cases).

is remarkable as it indicates that communicators in treatment RC did not believe that their messages would enhance cooperation. Instead, it is followers who trust in the effectiveness of relevant one-way messages. In normal groups, they report significantly higher expectations in treatment RC than the average member in the baseline and followers in treatment IC (both $p < 0.01$). In privileged groups, this tendency is less marked as low-benefit members' expectations about the other low-benefit member's action are only weakly significantly higher in treatment RC than in the baseline and not significantly different otherwise ($p = 0.095$, B vs. RC; $p = 0.88$, B vs. IC; $p = 0.12$, RC vs. IC). Surprisingly, however, their expectations about high-benefit members are affected by relevant messages ($p = 0.01$, B vs. RC; $p = 0.21$, B vs. IC; $p = 0.16$, RC vs. IC).[28] This indicates that enhanced cooperation of low-benefit members in treatment RC might partially be due to a conditionally cooperative reaction to a falsely assumed (cf. Result 3) increase in contributions from high-benefit members. In summary, the belief data suggests that relevant but not irrelevant one-way communication generates more optimistic expectations about others' contributions. Most astonishingly, this effect is exclusively visible for followers.

5 Conclusions

In this article, we use a one-shot public goods game to investigate contribution behavior in privileged vis-à-vis normal groups with and without one-way communication. The contribution to the literature is three-fold.

First, we provide evidence that contribution behavior can be inversely related to other group members' marginal benefits from the public good when interaction is one-shot. While efficiency concerns would predict the opposite, the result is in accordance with fairness theories (Fehr and Schmidt, 1999, Bolton and Ockenfels, 2000) which account for payoff inequalities resulting from asymmetric marginal benefits. The lack of personal sacrifice in contributions from high-benefit members constitutes an alternative explanation (Glöckner et al., 2011). If personal sacrifice is defined dichotomously, a future experiment might succeed in distinguishing these two explanations by comparing normal and privileged groups to intermediate groups, in which marginal benefits are asymmetric but the dominance of free riding is preserved. Our results have implications for the alleged advan-

[28] Separate t-tests reject the null hypothesis that followers expected high-benefit members to contribute the entire endowment for all three treatments ($p < 0.02$, for all cases).

tage of privileged groups (Olson, 1965). They show that the underprovision of the public good in privileged compared to normal groups depends on two opposing effects. The first effect is that high-benefit members contribute higher amounts than low-benefit members in normal groups. The other is that the presence of a high-benefit member inhibits cooperation by low-benefit members. Which of these effects dominates might depend on group composition. When they have many low- and few high-benefit members, privileged groups might even enjoy lower quantities of the public good than normal groups.

Second, we confirm that one-way communication yields enhanced cooperation even if a sender of the message benefits disproportionally from the public good. Followers comply with communicators' persuasion attempts (1) despite their awareness that these might be based on self-serving intentions rather than a desire to benefit the group and (2) despite the fact that such behavior inevitably yields unfavorable payoff inequality for themselves. The first aspect suggests that, in contrast to reciprocity with respect to actions, compliance with contribution suggestions does not depend on perceived intentions. The second aspect indicates that followers accept payoff inequalities more readily when one-way communication is available. One possible explanation for this observation is that communicators' arguments increase the weight of efficiency considerations in the decision making process. Overall, our experimental evidence makes a compelling case for the robustness of the effectiveness of one-way communication. This property should not be taken for granted, however. Reuben and Riedl (2009) have already shown that even the power of punishment opportunities tumbles in privileged groups.

Third, we test and reject the conjecture that one-way communication affects cooperation merely by "giving someone a voice." Messages which are restricted to topics other than the experiment do not affect contribution decisions irrespective of group composition. This result implies that the mechanism underlying the effectiveness of one-way messages resides within the communication content.

References

Bolton, G. E., Ockenfels, A., 2000. ERC: A theory of equity, reciprocity, and competition. The American Economic Review 90 (1), 166–193.

Bouas, K. S., Komorita, S. S., 1996. Group discussion and cooperation in social dilemmas. Personality and Social Psychology Bulletin 22 (11), 1144–1150.

Brandts, J., Schram, A., 2001. Cooperation and noise in public goods experiments: Applying the contribution function approach. Journal of Public Economics 79 (2), 399–427.

Chaudhuri, A., 2011. Sustaining cooperation in laboratory public goods experiments: A selective survey of the literature. Experimental Economics 14, 47–83.

Cooper, D. J., Kagel, J. H., 2005. Are two heads better than one? Team versus individual play in signaling games. The American Economic Review 95 (3), 477–509.

Dawes, R. M., McTavish, J., Shaklee, H., 1977. Behavior, communication, and assumptions about other peoples' behavior in a commons dilemma situation. Journal of Personality and Social Psychology 35 (1), 1–11.

Engelmann, D., Strobel, M., 2004. Inequality aversion, efficiency, and maximin preferences in simple distribution experiments. The American Economic Review 94 (5), 857–869.

Falk, A., Fehr, E., Fischbacher, U., 2008. Testing theories of fairness – intentions matter. Games and Economic Behavior 62 (1), 287–303.

Fehr, E., Fischbacher, U., 2002. Why social preferences matter – the impact of non-seflish motives on competition, cooperation and incentives. The Economic Journal 112, C1–C33.

Fehr, E., Schmidt, K. M., 1999. A theory of fairness, competition, and cooperation. Quarterly Journal of Economics 114 (3), 817–868.

Fischbacher, U., 2007. Zurich toolbox for readymade economic experiments. Experimental Economics 10 (2), 171–178.

Fischbacher, U., Gächter, S., 2010. Social preferences, beliefs, and the dynamics of free riding in public good experiments. The American Economic Review 100 (1), 541–556.

Fischbacher, U., Gächter, S., Fehr, E., 2001. Are people conditionally cooperative? Evidence from a public goods experiment. Economics Letters 71 (3), 397–404.

Fisher, J., Isaac, R. M., Schatzberg, J. W., Walker, J., 1995. Heterogenous demand for public goods: Behavior in the voluntary contributions mechanism. Public Choice 85, 249–266.

Gächter, S., Renner, E., 2010. The effects of (incentivized) belief elicitation in public goods experiments. Experimental Economics 13, 364–377.

Glöckner, A., Irlenbusch, B., Kube, S., Nicklisch, A., Normann, H.-T., 2011. Leading with(out) sacrifice? A public-goods experiment with a privileged player. Economic Inquiry 49 (2), 591–597.

Greiner, B., 2004. An online recruitment system for economic experiments. In: Kremer, K., Macho, V. (Eds.), Forschung und wissenschaftliches Rechnen 2003. GWDG Bericht 63. Gesellschaft für Wissenschaftliche Datenverarbeitung, Göttingen, pp. 79–93.

Isaac, R. M., Walker, J. M., 1988. Group size effects in public goods provision: The voluntary contributions mechanism. The Quarterly Journal of Economics 103 (1), 179–199.

Isaac, R. M., Walker, J. M., Thomas, S. H., 1984. Divergent evidence on free riding: An experimental examination of possible explanations. Public Choice 43 (2), 113–149.

Kelley, H. H., 1979. Personal Relationships: Their Structures and Processes. Lawrence Erlbaum Associates, Hillsdale, NJ.

Kelley, H. H., Thibaut, J. W., 1978. Interpersonal Relations: A Theory of Interdependence. Wiley & Sons, New York.

Kollock, P., 1998. Social dilemmas: The anatomy of cooperation. Annual Review of Sociology 24, 183–214.

Koukoumelis, A., Levati, M. V., Weisser, J., 2010. A voluntary contribution experiment with one-way communication and income heterogeneity. Jena Economic Research Papers 094.

Koukoumelis, A., Levati, M. V., Weisser, J., 2012. Leading by words: A voluntary contribution experiment with one-way communication. Journal of Economic Behavior and Organization 81 (2), 379–390.

Ledyard, J. O., 1995. Public goods: A survey of experimental research. In: Kagel, J., Roth, A. E. (Eds.), The Handbook of Experimental Economics. Princeton University Press, pp. 111–194.

Marwell, G., Ames, R. E., 1979. Experiments on the provision of public goods. I. Resources, interest, group size, and the free-rider problem. American Journal of Sociology 84 (6), 1335–1360.

McCabe, K. A., Rigdon, M. L., Smith, V. L., 2003. Positive reciprocity and intentions in trust games. Journal of Economic Behavior & Organization 52 (2), 267–275.

Olson, M. L., 1965. The Logic of Collective Action: Public Goods and the Theory of Groups. Harvard University Press, Camebridge, MA.

Orbell, J. M., van de Kragt, A. J., Dawes, R. M., 1988. Explaining discussion-induced cooperation. Journal of Personality and Social Psychology 54 (5), 811–819.

Ostrom, E., 2000. Collective action and the evolution of social norms. Journal of Economic Perspectives 14 (3), 137–158.

Palfrey, T., Prisbrey, J., 1996. Altruism, reputation and noise in linear public goods experiments. Journal of Public Economics 61 (3), 409–427.

Palfrey, T., Prisbrey, J., 1997. Anomalous behavior in public goods experiments: How much and why? The American Economic Review 87 (5), 829–846.

Reuben, E., Riedl, A., 2009. Public goods provision and sanctioning in privileged groups. Journal of Conflict Resolution 53 (1), 72–93.

Sen, A. K., 1967. Isolation, assurance and the social rate of discount. Quarterly Journal of Economics 81 (1), 112–124.

Simpson, B., 2006. Social identity and cooperation in social dilemmas. Rationality and Society 18 (4), 443–470.

Sutter, M., Strassmair, C., 2009. Communication, cooperation and collusion in team tournaments – an experimental study. Games and Economic Behavior 66 (1), 506–525.

Tajfel, H., Turner, J. C., 1986. The social identity theory of intergroup behavior. In: S. Worchel and W. Austin (Eds.), Psychology of Intergroup Relations. Chicago: Nelson-Hall.

Yamagishi, T., Mifune, N., 2008. Does shared group membership promote altruism? Fear, greed, and reputation. Rationality and Society 20 (1), 5–30.

Appendix

A Post experimental questionnaire

For items 1 to 4 and item 9, participants were asked to state their degree of agreement ("not at all" to "very much") on a 7-point Likert scale. All other items asked for an integer input between 0 and 10. Belief elicitation was tailored with respect to the player type indicated in italics after items 5 to 8. Except for the first four items which measured group identification, every item appeared on a separate screen. Originally, all items were in German.

1. I feel committed to my group.
2. I am glad to be in my group.
3. I feel solidarity with my group.
4. It is pleasant to be in my group.
5. According to your estimation, what is the other group members' average contribution to the project? *(normal groups)*
6. According to your estimation, what did the two members of type B contribute on average to the project? *(high-benefit members)*
7. According to your estimation, what did the member of type A contribute to the project? *(low-benefit members in privileged groups)*
8. According to your estimation, what did the other member of type B contribute to the project? *(low-benefit members in privileged groups)*
9. The communicator's message was constructed as to maximize his own payoff. *(non-communicators)*

B Categorization methodology

Our categorization methodology follows Cooper and Kagel (2005) and Sutter and Strassmair (2009). The set of categories is based on those obtained for treatment C in Koukoumelis et al. (2012). The new messages were screened in order to identify potentially distinct arguments, which were not represented in the original set of categories. Since none were found, we adopt the previous set of categories and report it in Table 3. For an exact description of how the original categories where established, see Appendix A in Koukoumelis et al. (2012).

Two undergraduate research assistants separately coded all messages obtained from treatment RC. If one message contained the argument(s) specified by a category, then that category was assigned the value of 1 (otherwise, it was assigned the value of 0). The correlation coefficient between the assistants' codings was 0.64 and 0.85 for the sets of normal and privileged groups, respectively. Finally, the two coders gathered and discussed their individual assessments and arrived at a common coding. The result is reported in Table 4.

C Instructions

This appendix reports the instructions (originally in German) for all treatments. Those for normal groups in the baseline treatment are displayed in full length below. They contain all parts which are common to all six treatments. The instructions for privileged groups and the treatments RC and IC can be obtained by inserting and replacing the appropriate paragraphs. The place holder *[for <treatment name>, replace the following paragraph]* indicates which paragraphs have to be replaced, where the replacement always has the same heading. The place holder *[for <treatment name>, insert paragraph <paragraph name> here]* prescribes where new paragraphs have to be inserted.

C.1 Instructions for normal groups - baseline treatment

INSTRUCTIONS

Welcome! You are about to participate in an experiment funded by the Max Planck Institute of Economics. Please remain silent and switch off your mobile.

You will receive €2.50 for showing up on time. Beyond this you can earn more money. In order to do this, please read these instructions carefully. The €2.50 show-up fee and any additional amounts of money you may earn will be paid to you in cash at the end of the experiment. Payments are carried out privately, i.e., without the other participants knowing the extent of your earnings. During the experiment, we shall not speak of euros but of ECUs (Experimental Currency Units). ECUs are converted to euros at the following exchange rate: 1 ECU = €0.20.

It is strictly forbidden to speak to other participants. If you have any questions during the experiment please raise your hand.

Detailed information on the experiment

Group formation

You will be placed in a group of three players. You will never learn the identity of the other members of your group.

Decisions

The experiment consists of one period only. In this period, you (as well as the other members of your group) receive an endowment of 25 ECUs. You have to decide **how many of these 25 ECUs you want to contribute to a project**. The ECUs contributed to the project yield income for you as well as for the other members of your group (you will learn more about the "income from the project" below). You can keep the ECUs that you do not contribute for yourself.

[for privileged groups, replace the following paragraph]

Period-earnings

More specifically, your period-earnings consist of two parts:

a) "Income from the project" = 0.6 × sum of all group members' contributions (in words, the income from the project is determined by multiplying the sum of the contributions of all group members by 0.6);

b) "ECUs you keep" = 25 − your contribution to the project.

Thus, your period-earnings summarized in a formula are

Your period-earnings	=	**Income from the project**	+	**ECU you keep**
		(0.6 × sum of group's contributions)	+	(10 − your contribution)

Example:

Suppose that all three group members contribute 5 ECUs. Then both you and your group members receive an "income from the project" of 9 ECUs (= 0.6 × 15). The "ECUs you keep" are 20 (= 25 − 5). Hence, your period-earnings are 9 + 20 = 29 ECUs.

[for treatments IC and RC, replace the following paragraph]

Interaction with your group members

You as well as the other two members of your group decide simultaneously and privately about the amount of ECUs you want to contribute to the project.

[for treatments IC and RC, insert paragraph Communication here]

The information you receive at the end of the experiment

You will receive information about 1) the number of ECU contributed by each of your group members, with the individual contributions being sorted in descending order, 2) the income from the project, and 3) your corresponding period-earnings.

Your final payoff

At the end of the experiment, your period-earnings will be converted into euros and paid out to you in cash, together with the show-up fee of 2.50 euros.

Before the experiment starts, you will have to answer some control questions to verify your understanding of the rules of the experiment. Once everybody has answered all questions correctly, four practice periods will be played *(only for treatments IC and RC:), which will only include the decision situation, but not the communication stage.* During these four practice periods, you will not be matched with other persons in this room, but with a computer that will determine the others' decisions randomly. You will get no payment for these practice periods.

Please remain quietly seated until the experiment starts. If you have any questions, please raise your hand now.

C.2 Additional instructions for normal groups

Communication

During the communication stage the communicator can use his/her keyboard to type one message to the others. The communicator is free to send the message (s)he likes, *(only for relevant communication:), including what (s)he thinks is the best approach to the experiment, what (s)he plans to do, or what (s)he would like the others to do. (Only for irrelevant communication:). Its content may e.g. be related to a current topic or may be intended to entertain the other group members.*

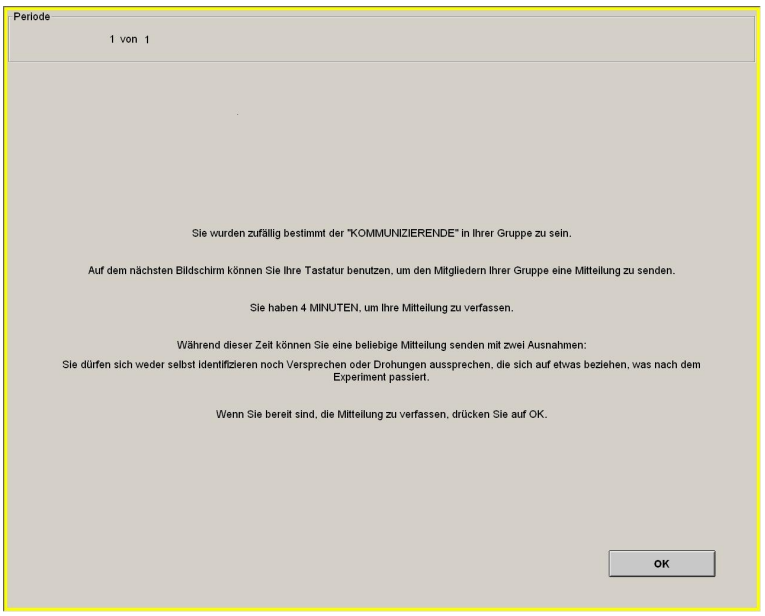
However, there are two *(only for irrelevant communication:) three* restrictions on the kind of messages that the communicator can send:

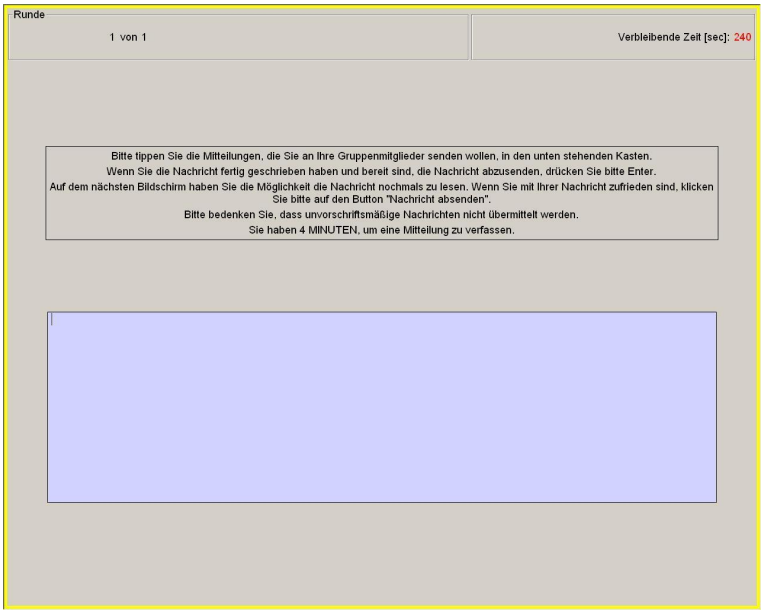
1. The communicator is not allowed to identify him/herself to the others. Thus, (s)he cannot reveal his/her real name, nicknames, or any other identifying feature such as gender, hair, or seat number.
2. There must be neither threats nor promises pertaining to anything that is to occur after the experiment.
3. *(Only for irrelevant communication:) Third, the communicator is not allowed to write about the upcoming decision situation. Thus, (s)he, e.g., must not indicate what (s)he thinks is the best approach to the experiment, what (s)he plans to do, or what (s)he would like the others to do.*

To enforce compliance with the above restrictions, all messages, before being sent, are checked by a monitor (a member of the experiment team). Improper messages are not delivered. Instead, the sender receives a warning informing him/her of his/her misconduct.

The communicator has 4 minutes to write his/her message, but (s)he is free to send it ahead of time. A clock will inform the communicator of the remaining time.

The screen-shots that you will see if you are the communicator in your group are shown below.





Please, remember to remain quiet during the whole experiment or the session will be terminated and all payments will be canceled.

C.3 Additional instructions for privileged groups

Period-earnings

More specifically, your period-earnings consist of two parts:

a) "Income from the project" = **type-factor** × sum of all group members' contributions (in words, your income from the project is determined by multiplying the sum of the contributions of all group members by your **type-factor**);

b) "ECUs you keep" = 25 − your contribution to the project.

Thus, your period-earnings summarized in a formula are

Your period-earnings =	**Income from the project**	+	**ECUs you keep**
	(**type-factor** × sum of group's contributions)	+	(25 − your contribution)

Before the experiment starts, you will be randomly assigned to either of two types: type A or type B. Each group consists of one member of type A and two members of type B. The types differ exclusively in their "income from the project." More specifically:

- If you are of **type A**, your **type-factor** is **1.6**.
- If you are of **type B**, your **type-factor** is **0.6**.

(Only for the baseline condition:) At the beginning of the experiment, one member of each group is randomly selected to be the "type A member." Every participant will be informed whether he or she is going to act as the "type A member" in an "Information Window."

Example:

Suppose that all three group members contribute 5 ECUs. Then the "ECUs you keep" are 20 (= 25 − 5) for both you and for your group members. The "income from the project" is 24 ECU (= 1.6 × 15) if you are of type A and 9 ECU (= 0.6 × 15) if you are of type B. Hence, your period earnings are 24 + 20 = 44 ECUs if you are of type A and 9 + 20 = 29 ECUs if you are of type B.

Interaction with your group members

You as well as the other two members of your group decide simultaneously and privately about the amount of ECUs you want to contribute to the project.

Before making your contribution decision, the type A member of every group is given the opportunity to communicate with his/her fellow members (how communication is carried out is described below). In the following, we shall refer to the type A member

also as the "communicator."

At the beginning of the experiment, one member of each group is randomly selected to be the "type A member / communicator." Every participant will be informed whether he or she is going to act as the "type A member / communicator" in an "Information Window."

Communication

During the communication stage the communicator can use his/her keyboard to type one message to the others. The communicator is free to send the message (s)he likes *(only for relevant communication:), including what (s)he thinks is the best approach to the experiment, what (s)he plans to do, or what (s)he would like the others to do. (Only for irrelevant communication:). Its content may e.g. be related to a current topic or may be intended to entertain the other group members.*

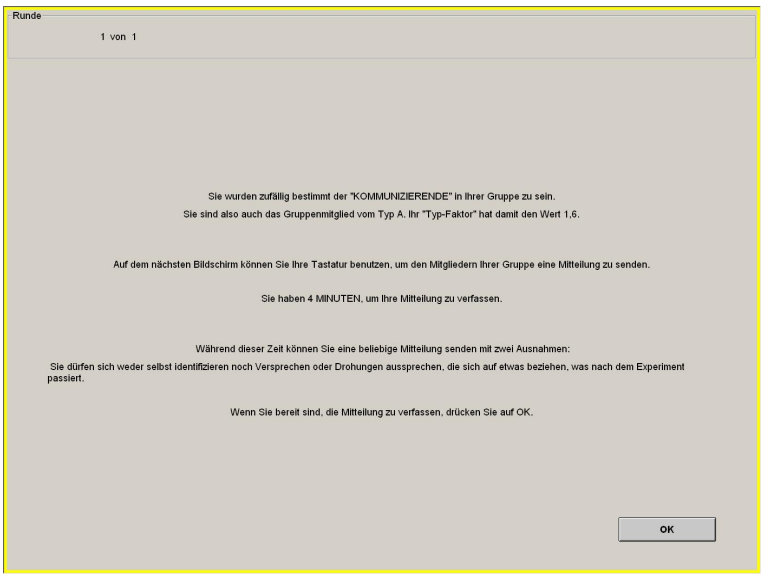
However, there are two *(only for irrelevant communication:) three* restrictions on the kind of messages that the communicator can send:

1. The communicator is not allowed to identify him/herself to the others. Thus, (s)he cannot reveal his/her real name, nicknames, or any other identifying feature such as gender, hair, or seat number.
2. There must be neither threats nor promises pertaining to anything that is to occur after the experiment.
3. *(Only for irrelevant communication:) The communicator is not allowed to write about the upcoming decision situation. Thus, (s)he, e.g., must not indicate what (s)he thinks is the best approach to the experiment, what (s)he plans to do, or what (s)he would like the others to do.*

To enforce compliance with the above restrictions, all messages, before being sent, are checked by a monitor (a member of the experiment team). Improper messages are not delivered. Instead, the sender receives a warning informing him/her of his/her misconduct.

The communicator has 4 minutes to write his/her message, but (s)he is free to send it ahead of time. A clock will inform the communicator of the remaining time.

The screen-shots that you will see if you are the communicator in your group are shown below.



Please, remember to remain quiet during the whole experiment or the session will be terminated and all payments will be canceled.