

Hönow, Nils Christian; Pourviseh, Adrian

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Intragroup communication in social dilemmas: An artefactual public good field experiment in small-scale communities

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Nils Christian Hoenow

Adrian Pourviseh

Intragroup Communication in Social Dilemmas: An Artefactual Public Good Field Experiment in Small-Scale Communities

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Hohenzollernstr. 1-3, 45128 Essen, Germany

Ruhr-Universität Bochum (RUB), Department of Economics

Universitätsstr. 150, 44801 Bochum, Germany

Technische Universität Dortmund, Department of Economic and Social Sciences

Vogelpothsweg 87, 44227 Dortmund, Germany

Universität Duisburg-Essen, Department of Economics

Universitätsstr. 12, 45117 Essen, Germany

Editors

Prof. Dr. Thomas K. Bauer

RUB, Department of Economics, Empirical Economics

Phone: +49 (0) 234/3 22 83 41, e-mail: thomas.bauer@rub.de

Prof. Dr. Ludger Linnemann

Technische Universität Dortmund, Department of Business and Economics

Economics – Applied Economics

Phone: +49 (0) 231/7 55-3102, e-mail: Ludger.Linnemann@tu-dortmund.de

Prof. Dr. Volker Clausen

University of Duisburg-Essen, Department of Economics

International Economics

Phone: +49 (0) 201/1 83-3655, e-mail: vclausen@vwl.uni-due.de

Prof. Dr. Ronald Bachmann, Prof. Dr. Manuel Frondel, Prof. Dr. Torsten Schmidt,

Prof. Dr. Ansgar Wübker

RWI, Phone: +49 (0) 201/81 49-213, e-mail: presse@rwi-essen.de

Editorial Office

Sabine Weiler

RWI, Phone: +49 (0) 201/81 49-213, e-mail: sabine.weiler@rwi-essen.de

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Nils Christian Hoenow and Adrian Pourviseh¹

Intragroup Communication in Social Dilemmas: An Artefactual Public Good Field Experiment in Small-Scale Communities

Abstract

Communication is well known to increase cooperation rates in social dilemma situations, but the exact mechanisms behind this have been questioned and discussed. This study examines the impact of communication on public good provisioning in an artefactual field experiment conducted with 216 villagers from small, rural communities in northern Namibia. In line with previous experimental findings, we observe a strong increase in cooperation when face-to-face communication is allowed before decision making. We additionally introduce a condition in which participants cannot discuss the dilemma but talk to their group members about an unrelated topic prior to learning about the public good game. It turns out that this condition already leads to higher cooperation rates, albeit not as high as in the condition in which discussions about the social dilemma are possible. The setting in small communities also allows investigating the effect of pre-existing social relationships between group members and their interaction with communication. We find that both types of communication are primarily effective among socially more distant group members, which suggests that communication and social ties work as substitutes in increasing cooperation. Further analyses rule out better comprehension of the game and increased mutual expectations of one's group members' contributions as drivers for the communication effect. Finally, we discuss the role of personal and injunctive norms to keep commitments made during discussions.

JEL-Codes: C71, C93, D8, D9, H41, Q5, Z1

Keywords: Communication; cooperation; field experiment; public good; social ties

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¹ Nils Christian Hoenow, RWI and University of Marburg; Adrian Pourviseh, University of Marburg. - The authors declare that there is no conflict of financial, general, or institutional competing interests. - All correspondence to: Nils Christian Hoenow, RWI, Hohenzollernstr. 1-3, 45128 Essen, Germany, e-mail: christian.hoenow@rwi-essen.de

1. Introduction

Social dilemmas, also known as collective action problems, occur when individual and collective interests diverge, for example due to externalities. Specifically, each single individual is always better off by being selfish and not cooperating, whereas for the group as a whole the best outcome can only be achieved if everyone cooperates. Tragically though, selfish individuals can still benefit from their cooperative partners' efforts, a problem known as free-riding. As real-world applications there are environmental problems, for instance, pollution and the use of natural resources, as well as many other issues, ranging from high-stake political ones down to people working together in everyday life on tasks that require cooperation to achieve common goals. Economists and psychologists use prisoner's dilemmas, public good and common-pool resource games to study decision making in social dilemma situations, often with the aim of finding ways and policies that are able to promote cooperative behavior.

One measure that has been identified as remarkably effective in increasing cooperation is giving participants in social dilemma situations the opportunity to talk to each other prior to making their decisions (Dawes 1980; Sally 1995; Balliet 2010; Ostrom 2010). Communication in the context of social dilemma problems and, in particular, in experimental settings, usually refers to unrestricted face-to-face discussions between participants (Bicchieri and Lev-On 2007).¹ While communication was found to raise cooperation, it may, however, also be associated with costs, especially between people who are spatially or socially distant from each other. To make the best use of communication as a tool to promote cooperation it is therefore helpful to understand how and in which contexts communication is beneficial.

In fact, there has been extensive research on the effect of communication on cooperation, yet exact mechanisms behind it are subject to discussion and, to date, remain largely unclear (Lopez and Villamayor-Tomas 2017; Koessler et al. 2020). A number of potential mechanisms to explain the conducive effect of communication on cooperation have been suggested and include (1) increased understanding of the dilemma problem and the consequences of individual decisions (Dawes et al. 1977; Kerr and Kaufman-Gilliland 1994), (2) changes in involved individuals' relationship to each other, such as through the promotion of group identity and decreases in social distance (Orbell et al. 1988; Dawes et al. 1990; Kerr and Kaufman-Gilliland

¹ Procedural standards in economic experiments further ensure anonymity of individual decisions which allows all participants to reveal their true preferences without having to worry about retaliation by other participants during or after the experiment. Participants consequently only get to know their own and the group outcome but are unable to find out about the other group members' individual decisions (unless all group members unambiguously defect or cooperate, in which case the others' behavior can be deducted from the group outcome). Real money is offered in economic experiments to make decisions salient.

1994; Bouas and Komorita 1996; Bicchieri 2002; He et al. 2017), and (3) finding consensus to cooperate, possibly including mutual reassurances, appeals, and even promises (Dawes et al. 1990; Kerr and Kaufman-Gilliland 1994; Bouas and Komorita 1996). Empirical evidence points towards consensus and commitments to cooperate as the most influential factor (Orbell et al. 1988; Kerr and Kaufman-Gilliland 1994; Bouas and Komorita 1996; Kerr et al. 1997).

Our study presents results from an artefactual public good field experiment that compares three conditions: ‘no communication’ as the control condition, an ‘unrelated communication’ treatment and a ‘dilemma-related communication’ treatment. The crucial distinction between the two communication treatments lies in whether participants already know about the upcoming public good game when they talk to their group members. While the relationship between group members may be affected by either type of communication, explanations to improve understanding of the situation as well as finding consensus and making commitments to cooperate are only possible when communication is dilemma related. This design therefore allows us to disentangle potential effects associated with each type of communication.

The experiment was conducted in northern Namibia with 216 villagers from small, rural communities. This field setting does not only constitute a novel and more natural environment than lab experiments, especially so when it comes to people talking to each other, but further allows us to analyze how communication interacts with previously existing social ties between participants. Since we hypothesize that a part of the communication effect works through changes in social relations between group members, we are thereby able to measure such effects for different social contexts. On the one hand, a certain level of social closeness might be necessary to make communication and, in particular, commitments effective (Ostrom and Walker 1991; Hoffman et al. 1996; Hardin 2003; Simpson 2007; Barbalet 2009). On the other hand, social closeness might already raise cooperation to a high level so that communication does not result in any additional effects (Ghate et al. 2013). Further, since participants already know each other, a communication effect cannot be attributed to simply identifying and getting acquainted with one’s group members, as would be the case in most lab settings (cf. Dawes et al. 1977; Kerr and Kaufman-Gilliland 1994; He et al. 2017). As another novelty, we measure if communication increases comprehension of the problem and whether this affects cooperation. Finally, we investigate if expectations of one’s group members’ contributions (also known as ‘beliefs’) are affected by each communication treatment and how they contribute to understanding mechanisms behind the communication effect.

Results show that talking about an unrelated topic is already conducive to cooperation, but the effect is mostly present among socially distant group members. Being able to discuss the social dilemma results in even higher rates of cooperation, yet also in this condition, the effect is strongest when group members are not yet socially close to each other. It can be concluded that pre-existing social ties and (either type of) communication work as substitutes in explaining cooperation. Our findings hence reassure the importance of dilemma-related elements, such as commitments, but also expose social relationships as a relevant mechanism in the effect that communication has on decisions to cooperate. We also show that dilemma-related discussions increase comprehension of the problem, but this cannot explain higher cooperation rates. Interestingly, expectations about one's group members' contributions remain virtually unaffected by communication, which hints towards the role of personal or injunctive norms of upholding commitments to cooperate made during discussions.

2. Discussion of the Literature

Several previous experimental studies have tried to separate the dilemma-related elements of the communication effect from the unrelated ones. One approach is to minimize any unrelated communication while only leaving the option to coordinate by sending written messages, often anonymously, for example on paper or in chats through computer terminals. Summing up findings, it turns out that, while written communication does indeed increase cooperation, it is not as effective as unrestricted face-to-face communication (Frohlich and Oppenheimer 1998; Bochet et al. 2006; Bicchieri and Lev-On 2007; Balliet 2010). In a meta study, Balliet (2010) observe the same across a large number of studies and pointed out the relevance of this finding: The mere content of a conversation can easily be exchanged by modern communication means like emails and telephone, yet, on many occasions in business, politics and science, meetings in person remain important, even though they involve higher costs and consume more time for travelling to meet each other. Jensen et al. (2000) and Brosig et al. (2003) tested even finer nuances by comparing various communication modalities like written messages, phone and video calls against face-to-face communication. It turns out that the broader or 'richer' a communication medium is, the better it is able to increase cooperation outcomes (Bicchieri and Lev-On 2007). The missing pieces in written communication in comparison to face-to-face communication are commonly explained by body language, facial expressions, eye gaze, the tone of voice and possibly other, more subtle cues (Roth 1995; Kurzban 2001; Bicchieri and Lev-On 2007).

However, even written messages may convey more than just the factual content. Visual and tonal cues are not available, but phrasing style and choice of words are still able to transmit information that go beyond the factual content and can affect the relationship between the conversation partners. Wilson and Sell (1997) went one step further and tested communication in a public good experiment over a computer terminal, where players could say nothing verbally but only signal their intended contributions as numbers. Interestingly, they did not find an increase in contributions, but, on the contrary, (forced) signaling of one's intentions resulted in lower contributions than what a control group achieved without any communication.² Similarly, Chen and Komorita (1994) as well as Bochet et al. (2006) conducted experiments that allowed participants to state their intended contributions to a public good but did not find any positive effects on cooperation compared to conditions without any communication, either. Results from these studies indicate that non-binding commitments or stated intentions to cooperate alone are insufficient for raising cooperation.

In order to find out how other elements in communication may be affecting cooperation, attempts have also been made to test the effect of unrelated communication only, i.e., without the possibility of discussing the social dilemma. This can be understood as the counterpart to the studies previously mentioned, as the idea is not to eliminate the unrelated but the dilemma-related elements; hence, testing whether unrelated discussions already affect decisions to cooperate. Dawes et al. (1977) did so by asking groups of participants to estimate the population proportions of different income levels of a particular US state as a communication task. Similarly, Bouas and Komorita (1996) hypothesized that finding consensus on any topic that was relevant to the participants could evoke a group identity after discussions. Both studies, however, find no effect of unrelated discussions. Higher cooperation rates in comparison to no communication were only achieved by groups that could actually discuss the dilemma. On the other hand, He et al. (2017) tested a communication condition in which participants were not allowed to make promises and found an effect on cooperation. Kurzban (2001) also found an increase in cooperation after allowing unrelated communication via computer messages, which indicates that there can still be some effect even if the communication does not happen face-to-face, but leaves us with an inconclusive overall picture on the role of unrelated communication. Looking at trust games, there is also empirical evidence supporting a positive effect of unrelated communication (Buchan et al. 2006).

² While, in the literature this type of non-binding signaling is often termed as 'cheap talk', we do not use the term to avoid confusion of 'cheap talk' with unrelated communication.

In conclusion, signaling one's (non-binding) intention alone, without free, unrestricted discussions, is not found to be effective in increasing cooperation, which highlights the importance of face-to-face communication and social relationships. At the same time, evidence for the effect of unrelated, social communication without the possibility to discuss the dilemma is, at best, mixed. The current state of the literature could hence imply that only communication that is both, unrestricted and dilemma-related, is effective in increasing cooperation.

With this study, we set up a novel experimental approach that varies the time at which group members talk to each other rather than externally restricting the topics of conversation. For our unrelated communication treatment, we ask the participating groups to discuss a given but not dilemma-related topic with their group members before introducing them to the public good game. The advantage is that, even though a discussion topic was specified to homogenize conversations, the content was not externally restricted and, possibly more importantly, not perceived as restricted by the participants. In contrast, previous studies prohibited talking about the dilemma problem while participants were already aware of the upcoming game (cf. Dawes et al. 1977; Bouas and Komorita 1996; Kurzban 2001, He et al. 2017). In our view, this could have unclear and detrimental behavioral side effects.

We further choose a field setting for the experiment, which allows a more natural environment with a heterogeneous pool of participants. Effects of communication can be expected to be different in field settings, where participants come from small communities, face cooperative dilemmas in their real lives and, to some extent, already know each other. Since social relations there are partly pre-defined, the setting in village communities allows measuring social ties and thereby makes it possible to analyze in more detail the role of social relations between participants regarding the effect of communication on cooperation. Specifically, we test for interactions between our experimental communication conditions and pre-existing social relations. Indeed, previous evidence on the communication effect from ("lab-in-the") field experiments is more heterogeneous than results from the lab: while positive effects of communication on cooperation are also regularly observed (e.g., Cardenas et al. 2004; Cardenas and Ostrom 2004; Velez et al. 2010), this was not always the case (Velez et al. 2012; Ghate et al. 2013). Ghate et al. (2013) even argue that communication is not necessary to increase cooperation if participants already show a high level of trust. Finally, potential communication effects as found in our study are not primarily attributable to simply identifying and getting to know each other.

3. Development of Hypotheses

In this section, we will introduce the underlying theories and develop a set of hypotheses to be tested by our experiment: We firstly aim to find out if unrelated communication is already able to increase cooperation. Since unrelated communication does not allow any exchange of information about the social dilemma, any potential communication effect on cooperation of this type of communication can, as a key assumption, only be attributed to changes in the relationships between group members. Unrelated communication entails topics like greeting each other, introducing oneself or others, and any type of small talk that is not about the social dilemma and the upcoming decision. It can also be on a specific, possibly (externally) specified, but unrelated topic. In particular, such group discussions are expected to decrease social distance between group members or even create or strengthen the feeling of belonging to the group. Social distance describes the relationship and closeness between groups or individuals, not spatially but in the degree of understanding and intimacy in their personal as well as social relationships to each other (Park 1924). Group identity, on the other hand, is understood as the perception of being part of a social group (Tajfel et al. 1971; Turner 1982; Spears 2011). According to theories of social identity and self-categorization, a stronger identification with the group shifts the focus of attention away from the individual towards the collective target, which means that members of a group with a strong perceived group identity are more likely to seek maximizing the group benefit instead of their own, individual one (Turner 1975; Tajfel et al. 1979; Turner 1982; Kramer and Brewer 1984; Brewer and Kramer 1986; Orbell et al. 1988; Dawes et al. 1990; Kerr and Kaufman-Gilliland 1994). From a less abstract perspective, decreases in social distance or the creation of a group identity, may make one's group members appear more relatable so empathy and concern for their welfare rises (cf. Schelling et al. 1968; Kerr and Kaufman-Gilliland 1994; cf. Bouas and Komorita 1996; Bohnet and Frey 1999). Indeed, several studies have observed higher cooperation rates and willingness to help each other with socially closer individuals (Essock-Vitale and McGuire 1985; Yamagishi and Sato 1986; Kollock 1998; Thompson et al. 1998; Bowles and Gintis 2004; Peters et al. 2004; Goette et al. 2006; Haan et al. 2006; Ruffle and Sosis 2006; Boone et al. 2008; Castro 2008; Apicella et al. 2012; Chuah et al. 2014). Similar preferences have also been found regarding trust and altruism (Glaseser et al. 2000; Buchan and Croson 2004; Cadsby et al. 2008; Rachlin and Jones 2008; Goeree et al. 2010; Etang et al. 2011; Binzel and Fehr 2013; Candelo et al. 2018) and can even be explained with evolutionary theories and kin selection (Hamilton 1964; Caporeale et al. 1989). Generally, in-group favoritism is a rather well-established finding in social psychology (Tajfel et al. 1979; Akerlof 1997; Buchan et al. 2006). We hence expect pre-existing social

relations, measured by the number of friends and family members in one's group, to positively affect contributions, but do not formulate it as one of our hypotheses as it seems confirmatory rather than novel and is, to begin with, understood as a direct effect independent of communication.

As the first hypothesis to be tested with our experiment, we expect that unrelated communication increases cooperation compared to a control condition without any communication. Since unrelated communication does not entail discussing elements and behaviors specific to the social dilemma, such an effect must be attributed to changes in social relationships between participants or the creation of group identities (Orbell et al. 1988; Dawes et al. 1990; Kerr and Kaufman-Gilliland 1994; Bouas and Komorita 1996; Bicchieri 2002; He et al. 2017).³

H1: Communication unrelated to the social dilemma increases cooperation in comparison to no communication

Next, it is investigated if dilemma-related communication can, as found in previous studies, increase cooperation and if it is different from unrelated communication (e.g., Dawes et al. 1977; Bouas and Komorita 1996; He et al. 2017, see also Sally 1995; Balliet 2010). While dilemma-related conversation content may also affect the group members' relationships to each other, we expect that additional elements only present in dilemma-related communication, such as explanations, appeals and commitments, contribute to increases in cooperation even beyond the level achieved by unrelated communication (Dawes et al. 1977; Dawes et al. 1990; Kerr et al. 1997; Kerr and Kaufman-Gilliland 1994; Bouas and Komorita 1996). Hypotheses 2.1 and 2.2 are therefore formulated as:

H2.1: Dilemma-related communication (also) increases cooperation in comparison to no communication

H2.2: Dilemma-related communication increases cooperation more than unrelated communication

³ In theory, however, communication could also have a negative effect on cooperation if participants only learn through communicating with each other that they do not like their group members or do not find them trustworthy.

The setting and design of the experiment allow us to explore a number of potential factors that may contribute to explaining the mechanisms behind the communication effect on cooperation and, if applicable, why effects of dilemma-related communication would be different from those of unrelated communication.

3.1 Interactions of the communication conditions with pre-existing social ties

Considering that the effect of communication on cooperation is hypothesized to work, at least partly, through changes in social relationships between group members, it seems reasonable to investigate how communication interacts with already existing social ties between participants.

It can be hypothesized that communication is ineffective if social ties between group members are already strong as such groups may show high levels of cooperation even without talking to each other (cf. Ghate et al. 2013). This would imply that pre-existing social ties and communication work as substitutes, which should then show in negative interaction effects. If, however, social ties and communication, are both positively and independently associated with cooperation, their effects would simply add up (more or less) linearly. Finally, communication might also result in stronger increases of cooperation among socially close group members, i.e., communication and social closeness could work as complements. This seems particularly plausible for dilemma-related communication as certain levels of closeness and trust might be needed to make appeals and commitments effective (Ostrom and Walker 1991; Hoffman et al. 1996; Hardin 2003; Simpson 2007; Barbalet 2009).

3.2 Comprehension

As a mechanism that is only potentially effective in dilemma-related communication, we investigate if talking about the public good problem increases comprehension of the situation. If some participants have not entirely understood the nature of the social dilemma, dilemma-related discussions may help clarifying the rules and game mechanics (Dawes et al. 1977; Kerr and Kaufman-Gilliland 1994). It is, however, not ex-ante clear what effect increased comprehension might have on cooperation outcomes. Understanding how cooperation is in everyone's best interest could establish it as the preferable option. On the other hand, better comprehension of the social dilemma could also make individuals realize that defection always leads to higher individual payoffs and consequently to switch from an intuitive intention to cooperate to a deliberate decision to free ride (Kahneman 2011). Specifically, we firstly examine if dilemma-related communication increases comprehension. If it does, then we further investigate if increased comprehension affects cooperation. Unrelated communication should not be able to affect comprehension of the dilemma.

3.3 Expectations of others' contributions, trust and norms

To learn more about the motivations behind individual decisions to cooperate, we measure our participants' expectations (also known as 'beliefs') about their group members' cooperative behavior. Firstly, we anticipate that pre-existing social ties have a direct and positive effect on expectations. Regarding the communication treatments, both unrelated and dilemma-related discussions may affect expectations through changes in social relationships. Dilemma-related communication, however, may entail additional elements that could be relevant to building mutual expectations: During discussions about the dilemma, consensus between group members to cooperate can be found, appeals to cooperate can be made as well as pledges about one's own good intentions, all of which may increase mutual expectations and even promote social norms to cooperate (Dawes et al. 1977; Orbell et al. 1988; Bicchieri 2002; Bicchieri and Lev-On 2007).

Making pledges or even promises to cooperate in front of group members about one's intentions to cooperate is known as 'commitment' and has indeed repeatedly been suggested as the main driver for the conducive effect that communication has on cooperation (Orbell et al. 1988; Ostrom et al. 1992; Kerr and Kaufman-Gilliland 1994; Bicchieri 2002). Such commitments made in discussions are, however, not necessarily binding and plausible as the actual, individual decisions can, depending on the setting, not be enforced or monitored.⁴ A cunning free rider might even deliberately lie about their intention to cooperate. Keeping promises and not lying to people are, nonetheless, considered strong and rather universal social norms. In other words, the effectiveness of non-binding commitment may be based on the premise that lying and deliberately breaking promises are violations of norms far worse than a decision not to cooperate (cf. Orbell et al. 1988). Indeed, Kerr and Kaufman-Gilliland (1994) as well as Bicchieri (2002) argue that the communication effect on cooperation is based on the norm of promise keeping rather than on a general norm to cooperate, which may emerge after finding consensus to do so. Breaking norms may result in the feeling of guilt, which can be interpreted as an intrinsic cost that individuals try to avert (Posner and Rasmusen 1999; Ostrom 2000; Charness and Dufwenberg 2006; Kessler and Leider 2012).

⁴ The actual decisions as well as the final payments to each participant are generally kept anonymous in economic experiments. Free riders do therefore not need to fear social sanctions or reputational effects. This might be different in real situations where monitoring and enforcement are possible and, sometimes, economic experiments also allow (costly) punishment of deviators within the mechanics of the game. Such alterations of the dilemma situation would then likely also play a role in the respective conversation. Similarly, if decisions are to be made over several rounds, reciprocal effects are possible and would likely affect decisions as well as the content of the conversations.

Commitments made during group discussions might therefore raise expectations of high cooperation, even if they are not binding. According to the concepts of reciprocity and conditional cooperation, which are widely acknowledged in economic literature, individuals may condition their decision to cooperate on (the expectation of) their group members' behavior (Axelrod and Hamilton 1981; Orbell et al. 1988; Fischbacher et al. 2001; Croson 2007; Kocher et al. 2008, Fischbacher and Gaechter 2010; Chaudhuri 2011). In conclusion, there may be a double effect of making commitments in dilemma-related discussions: Firstly, based on mutual trust in such commitments, expectations about the other group members' intentions to cooperate increase, which, due to effects of reciprocity, boosts cooperation. Secondly, individuals might feel bound to fulfill their own promises due to social, and, possibly, personal norms. In contrast to social norms, which reflect someone's perception of how they are expected to behave or of what is considered as 'normal' behavior, a personal norm describes what one believes to be the right thing to do according to their own, personal standards (Schwartz 1973; Schwartz 1977; Ajzen 1991; Cialdini et al. 1991). Similarly, Cialdini et al. (1990) distinguish social norms into what is believed to be the normal (descriptive norms) and what is believed to be the appropriate behavior (injunctive norms).⁵ Beliefs about what is normal should consequently be reflected in one's expectations of other people's behavior, whereas this is not necessarily the case for personal or injunctive norms. In our context, expectations can hence be interpreted as a measurable manifestation of social norms.

Additionally, we elicit in the post-experimental survey a measure of trust in one's group members and hypothesize that, analogously to expectations, trust may be affected by social ties and either type of communication.

⁵ In similar terms, Ajzen (1991), Smith and McSweeney (2007), Ravis et al. (2009), Schram and Charness (2015) as well as Mittelman and Rojas-Méndez (2018) distinguish between social and moral norms.

4. Method

4.1 Research setting and participants

The experiments were conducted as part of the SASSCAL research project from April to June 2017 in 12 randomly selected rural villages in the Kapako district (Kavango West) and in the Ndiyona district (Kavango East). For the selection, villages that had formerly been visited for similar research projects were left out. Further preconditions were that there were more than 80 inhabitants, and the selected villages were not more than a day's drive away from the nearest tar road. The names and the positions of all villages are shown in the Supplementary Materials (A.1). The total sample size is 216 participants, 72 in each of the three experimental conditions. One participant from the control condition left earlier, so he could not complete the post-experimental survey. Table 1 presents some information on the variables included in the analysis and summarizes the socioeconomic characteristics of the sample.

Table 1: Summary statistics and variable description

variable	obs	mean	std. dev.	min	max	variable info
contribution	216	4.30	3.36	0	10	contribution to public good
expectation	216	5.97	2.72	0	10	exp. of others' avg. contribution
trust	171 ⁶	3.50	0.83	1	4	trust in group members (survey)
control q. wrong	216	0.20	-	0	1	control questions wrong answer
FdsFam	215	1.96	1.19	0	3	# friends and relatives in group
age	215	36.95	15.45	18	87	age of participant
female	216	0.60	-	0	1	gender (1 for female)
schooling years	215	6.60	3.97	0	17	years of schooling
hectares	215	2.51	2.05	0	15	hectares currently cultivated
bags yield	215	6.52	10.41	0	100	crop yield last season
farmer	215	0.87	-	0	1	profession farmer
migrant	215	0.18	-	0	1	moved to village (< 10 years ago)

Table 2 compares socioeconomic characteristics across the three experimental groups and tests for equality in the subsamples. It turns out that most, but not all, variables are equally distributed. It is therefore reasonable to add these as additional variables in the regression models.

⁶ The elicitation of trust was conducted as part of the post-experimental survey. However, the question was initially asked by some enumerators in an unclear manner. It was not explained whether the question was about trust in the other three players or in all other participants of the experimental workshop. This was only corrected after the first few villages had been visited. Invalid observations have hence been removed and the variable is not used in the main regression models.

Table 2: Split sample by experimental condition

Exp. condition:	control	treatment 1	treatment 2	
communication:	none	unrelated	dilemma-related	test for equality
	mean	mean	mean	p-value
FdsFam	2.20	2.10	1.58	0.002**
age	36.89	35.77	38.19	0.645
female	0.51	0.64	0.65	0.197
schooling years	6.70	6.71	6.39	0.860
hectares	2.27	3.03	2.21	0.026*
bags yield	6.74	7.26	5.57	0.610
farmer	0.85	0.94	0.83	0.077
migrant	0.23	0.14	0.18	0.407
observations	71	72	72	

p-value for one-way ANOVA or Fisher's-exact-test in case variable is dichotomous

FdsFam = number of friends and family members (in one's group)

Significance levels: * p<0.05, ** p<0.01, *** p<0.001

The majority of the rural population in Kavango is engaged in agriculture with crop farming as the primary component of their livelihood and cattle farming taking the second most relevant role (Namibian Ministry of Lands and Resettlements 2015). Farming is often on subsistence level and only partly integrated into markets. The Kavango region is further characterized by a young and growing population, most of whom enjoyed some years of school education. Villages are small in population size (with a mean of 642 and a median of 312.5 inhabitants) and villagers usually live in the same place for many years or even a lifetime, which means that the majority of the participants have known each other before the experimental workshops. In addition, there are village meetings as well as social and religious gatherings held regularly, and some households work together in agricultural tasks. Kinship relations are also present across many households.

In preparation of the experimental workshops, each village's headperson was visited several days ahead to arrange an appointment for a village meeting so that all villagers could be informed and invited in time. It was made clear beforehand that some monetary compensation would be offered for participating but also that only a certain number of participants would be able to take part in the workshops. At the beginning of each village meeting, 24 participants were randomly drawn by lot among those who expressed willingness to participate. This selection procedure was considered fair by almost everybody. The same lots also determined the allocation to one of two experimental groups per village. These experimental conditions of 12 players each were then spatially separated, and we explained to them the procedure of the workshop as well as the instructions of the public good game according to the respective experimental condition. For the public good game, the 12 persons per experimental condition were later again split into three groups of 4 players. We relied on a random allocation and did not externally stratify the group composition. Each experimental condition was supervised by

one experimenter and one local research assistant for interpretation. Local assistants were recruited in the town of Rundu and remained the same team over all visited villages. The allocation of assistants and experimenters to experimental conditions was randomized for each village. Experimental protocols and game instructions in English language can be found in the Supplementary Materials (B.). Protocols and instructions were priorly translated by the local assistants from English into the respective local languages.

4.2 The public good game

For the experiment, an unframed, single-round, standard public good game was chosen. Participants could earn real money according to their own and their group members' decisions. There were always 4 players in a group playing the game together. Each player received a private endowment of 10 coins and had to decide how much to keep and how much to contribute to a group account. The game was framed neutrally with coins to be allocated to a private and to a group account, so as to avoid associations with any particular, real-world applications. It was possible to contribute any discrete number of coins between 0 and 10. After all players had made their decision, contributions to the group account got doubled and then distributed equally among all four players. The socially optimal outcome was reached when everyone decided to contribute all of their endowment, i.e., 10 coins. Individually, however, one could always reach a higher payoff by not contributing at all (Nash equilibrium). Since the public good game was one-shot and anonymous, no reciprocity effects over rounds were possible and contributions supposedly measured the participants' pristine preferences (Rand and Nowak 2013). A formalized description of the public good game can be found in the Supplementary Materials (A.2).

The rules of the public good game were explained to the participants with the help of posters and by giving examples for outcomes (Supplementary Materials A.3 and B.4). Special attention was paid to making clear that the game was not a 'zero sum' situation about dividing the money, but that cooperating actually increased the total benefit for the group as a whole. To counterbalance, it was pointed out that not contributing always led to higher individual earnings. The assistants gave additional explanations, answered questions to the group if necessary and gave instructions to those who did not understand the game procedures right away. Even though studying the effect of comprehension after communication is part of this study's objectives, we did at no point deliberately aim for a certain share of player to misunderstand the rules of the game. As in all economic experiments, we tried to make all explanations as easily understandable as possible.

In the next step, participants were asked one after another to come forward to a secluded place (for example behind a building) to meet a research assistant that asked them two control questions for understanding. If one or both control questions were answered wrongly, the player would at this point no longer receive any additional explanation of the game but nonetheless participate in the experiment and receive their payment. The research assistant also asked the participant about their expectation of their other group members' average contribution. Correctly stated expectations were incentivized with an additional 20N\$ reward in the final payments.

The decision about the contributions to the public good were then made in another secluded place individually and anonymously. Plastic coins were used as game currency and could be put into two differently colored envelopes, one of which represented the individual and the other one the group account. Players then put their sealed envelopes that contained their contributions to the group account into a basket. By doing so, contributions were kept anonymous and could afterwards only be attributed to the players' ID numbers, guaranteeing some degree of anonymity in decision making not only towards the group members but also towards the experimenters. Participants' names were never asked and can therefore not be linked to their ID-numbers in the game. A research assistant stayed with the remaining group members to make sure they did not talk or communicate in any way while waiting for their turn. After making their decisions, players proceeded to the snack area for a break and were then interviewed individually. Survey questions can be found in the Supplementary Materials (C.) and include, among others, questions about socioeconomic characteristics and the relationship with their group members. Following Bogardus (1925), pre-existing social relationships were measured in four categories, as 'family (or member of the same household)', 'friend' and 'acquaintance' and 'stranger'. However, to avoid collinearity in the analysis, we only count the number of friends and family members as a single measurement for socially close group members versus acquaintances and strangers as the counterfactual for socially more distant group members. Further, it turned out that there were hardly any 'strangers' among the villagers so that this subgroup would have been very small. A detailed table on all reported social ties is shown in the Supplementary Materials A.4. The relation to group members was only reported 15 times as 'strangers', compared to 209 times as 'acquaintances', 152 times as 'friends' and 269 times as 'family'.

Payments according to the participants' and their group members' decisions were done in the very end individually and in private. The whole workshop took about 4 hours in each village. Payoffs averaged at 97N\$ (= 7.32US\$) per participant, which was more than an average local

wage for a day's work. The possible range of earnings was between 25 and 145N\$ (≈ 2 and 11 US\$), including the bonus payment for correctly stated expectations. Payoffs were set after pre-testing for calibration and allowing a reasonable final compensation for participating. A game currency was used to keep the number of coins used in the game low and with a range of possible contributions between 0 and 10 it can easily be compared to similar studies. The conversation rate was one to five (1coin = 5 N\$). For convenience, final payoffs were rounded to the next whole N\$ when necessary.

4.3 The experimental conditions

Table 3 summarizes the control and the two communication treatment conditions. In order to keep everything except the communication conditions comparable, participants in the control condition could visually identify their group members and were given a few moments of time before making their decisions to allow for silent deliberations about the explained social dilemma situation. While the first treatment only allows unrelated discussions, the second treatment can be considered as what is usually understood as communication in cooperation experiments.

Table 3: Experimental conditions

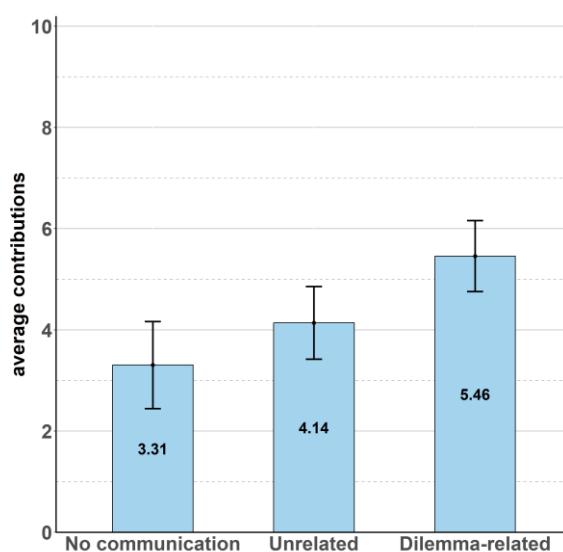
Control condition: No communication
<p><i>Participants are not allowed to talk to each other, but they can see who their group members are.</i></p> <p>In the control condition participants play the public good game with revealed identities of their group members. The participants are allocated in groups of four according to the numbers on their ID-cards. They are, however, not allowed to communicate with each other. Before decision making starts, a statement is made by a research assistant that the groups are playing the game together as allocated.</p>
Treatment 1: Unrelated communication
<p><i>Participants talk to their group members about an unrelated topic (climate change and farming) before learning about the public good game.</i></p> <p>In the unrelated communication treatment participants are asked to discuss a given but unrelated topic for five minutes in their group before learning about the public good game. They are allocated in groups of four according to the numbers on their ID-cards and asked to discuss how different rainfall conditions and changes in climate affects agricultural outputs and how adaptation measures could be taken. No further communication is allowed after learning about the game rules.</p>
Treatment 2: Dilemma-related communication
<p><i>Participants first learn the rules of the public good game and then talk to their group members before making the decision.</i></p> <p>In the dilemma-related communication condition participants learn about the rules of the public good game first and are then allowed to talk to their group members for five minutes before making their decisions. Hence, players have the opportunity to discuss the social dilemma and coordinate their actions. Decisions are still made in private.</p>

Discussions were never listened to or even recorded. In fact, for both communication treatment groups, experimenters and research assistants deliberately moved out of hearing distance from the groups so that they could talk freely. Groups were spatially divided for the discussions so that other groups could not be listened to and influence the content of discussions or the outcome. It should be noted that the communication conditions are to be understood as an ‘intention to treat’, which means that participants were given the opportunity to communicate but were not forced to do so. As a measure of compliance to the intended treatment conditions, we ex post asked participants in the survey about the content of their discussions. While this elicitation method might not be perfectly precise, it still turned out that 85% in the unrelated communication treatment complied with their task of discussing agriculture and 54% of participants in the dilemma-related communication treatment answered that they coordinated decisions with their group members, even though in this treatment no specific discussion topic was suggested by the experimenters. For privacy reasons, we did not ask more detailed questions about the content of discussion, such as whether agreements or promises were made. As post-experimental interviews were conducted in person by our research assistants, it seems improbable that such questions would have resulted in credible, truthful answers.

5. Results

Comparing the three conditions, average contributions to the public good were 3.31 coins in the control condition without communication, 4.14 coins after talking about the unrelated topic, and 5.46 coins after discussing the public good game (Figure 1, Table 4).

Figure 1: Average contributions to public good by experimental condition



Unrelated communication hence raises cooperation by 0.83 coins, which amounts to 39 percent of the average effect of 2.15 coins achieved by dilemma-related communication. While the differences according to simple t-test (Table 4) are significant between the dilemma-related communication and the control condition as well as between dilemma-related and unrelated communication, the difference between the unrelated communication and the control condition is not significant at the 5% level.⁷ Within-group variations of contributions were much smaller in the dilemma-related communication condition, i.e., behavior was more homogenous in those groups, which is likely a result of agreements on certain amounts to contribute (Table 4).

Table 4: Comparisons between experimental conditions and tests

Experimental condition:	control	treatment 1	treatment 2	differences		
communication:	none	unrelated	dilemma-related	T1-C	T2-C	T2-T1
	mean (std. dev.)	mean (std. dev.)	mean (std. dev.)	Δ (std. err.)	Δ (std. err.)	Δ (std. err.)
contribution	3.31 (3.66)	4.14 (3.06)	5.46 (2.99)	0.83 (0.56)	2.15*** (0.56)	1.32** (0.50)
contribution if control question correct ^A	2.92 (3.51)	3.85 (2.60)	5.31 (2.91)	0.93 (0.59)	2.38*** (0.59)	1.45** (0.51)
contribution if control question wrong ^A	4.37 (3.99)	5.06 (4.19)	6.86 (3.58)	0.69 (1.36)	2.49 (1.71)	1.80 (1.81)
variability of contributions^B	2.45 (1.50)	2.59 (1.44)	1.54 (1.21)	0.14 (0.49)	-0.90 (0.45)	-1.04* (0.44)
expectation	5.96 (2.80)	5.74 (2.85)	6.22 (2.69)	0.22 (0.47)	0.26 (0.46)	0.49 (0.46)
variability of expectations^B	2.01 (0.90)	2.27 (1.25)	1.70 (1.43)	0.26 (0.37)	-0.31 (0.41)	-0.57 (0.46)
correctly stated expectations^C	33%	38%	56%	8%	27%**	19%*
trust^D	3.46 (0.85)	3.50 (0.81)	3.56 (0.84)	0.04 (0.15)	0.10 (0.16)	0.05 (0.15)
# observations	72	72	72	-	-	-

Test results according to student's t-tests if not otherwise specified. All test results are two-sided.

A: Control questions for understanding of the public good game. The number of participants who correctly answered the control questions for understanding are 51 in the control condition, 55 in the unrelated communication condition, and 65 in the dilemma-related communication condition.

B: measured as mean of within-group standard deviation of contributions (18 observations per experimental condition)

C: Chi-squared tests for comparing shares of correctly stated expectations

D: Trust in group based on reduced sample of n=171 (57/60/54).

Significance levels: * p<0.05, ** p<0.01, *** p<0.001

⁷ For a power calculation, we draw on standard deviations found in a public good lab experiment similar to our design by Fehr and Gaechter (2000) and find that at 80% Power (1- β) and 5% type I error rate (α) a sample size of 72 per condition allows detecting an average 1.5 coins pairwise-difference in contributions in a two-sided t-test. Indeed, we observe a similar standard deviation as Fehr and Gaechter (2000), which corresponds to ~3 coins. However, the effect of unrelated communication is smaller than 1.5 coins difference so that it does not turn out as significant at the 5% level in a direct comparison (p-value t-test: 0.14). It does, however, become significant in some of the regression models (Table 5 and 6).

For the construction of the regression models, we use Tobit estimations as contributions must be considered to be censored at both the minimum (0 coins) and maximum (10 coins) possible choices that can be made in the public good game. The distributions of contributions for each condition are depicted in the Supplementary Materials (A.5) and confirm the presence of censoring at both endpoints. The basic model 1 only includes dummy variables for each communication treatment, whereas the number of friends and family members in one's group, individual expectations about one's group members' contributions as well as socioeconomic variables are added in model 2. The selection of these socioeconomic variables aims to capture potentially relevant socioeconomic characteristics including age, gender, and years of schooling. Information on regular incomes was difficult to obtain due to irregular seasonal flows over the year. We therefore include how many hectares of fields a household cultivates and how many bags of crop yields they produced in the last season. We further add whether the respective participant's main profession is farming and whether they have migrated to the village less than 10 years before. The full regression models, including all socioeconomic variables can be found in the Supplementary Materials (A.6). The same model specifications are repeated for model 3 and 4 but focus on the subsample of participants who have correctly answered both control questions for understanding of the public good game. While an intuitive understanding of the situation might still be present, wrongly answering the control questions is potentially associated with random, unclear decision behavior, thereby leading to less precise observed results. Indeed, the standard deviations of contributions are much larger for the subsample of participants who misunderstood the public good game (Table 4). Coefficients according to Tobit estimations reflect the slope of the latent variables rather than the observed outcome. In line with results from t-tests in Table 4, we find positive and significant effects of dilemma-related communication (T2) on contributions in all regression model specifications (Table 5). Coefficients for unrelated communication are smaller than the coefficients for dilemma-related communication, making up, depending on the model, between 42 and 59 percent of the latter. Unrelated communication (T1) further only reaches significance at the 5% level in model 4, after the inclusion of socioeconomic variables and when focusing on the sample of participants who correctly answered the control questions. The same regressions using Ordinary-Least Squares estimations can be found in the Supplementary Materials (A.7) and confirm the results found using Tobit models.

Table 5: Basic Tobit regression models for contributions to public good

Dep. var: contribution models without interaction terms	model 1 coefficient (std. err.)	model 2 coefficient (std. err.)	model 3 coefficient (std. err.)	model 4 coefficient (std. err.)
T1: Unrelated	1.331 (1.15)	1.647 (0.97)	1.474 (1.06)	1.906* (0.91)
T2: Dilemma-related	3.129* (1.28)	2.851** (1.06)	3.372** (1.20)	3.216** (0.99)
#FdsFam		-0.081 (0.27)		0.003 (0.24)
expectation		0.540*** (0.15)		0.497*** (0.15)
constant	2.635** (0.99)	2.028 (2.07)	2.175* (0.92)	1.375 (1.70)
'socioeconomics'	no	yes	no	yes
exclude misunderstood	no	no	yes	yes
var(e.contribution)	19.814*** (3.59)	15.027*** (2.58)	14.958*** (3.10)	11.738*** (2.30)
N	216	215	173	172

Tobit regression models with standard errors clustered on group (n = 4) level, censoring at the lower (0) and upper (10) endpoint for contributions

Socioeconomic covariates as shown in Table 2. Full regression model in Supplementary Materials A.6.1

#FdsFam = number of friends and family members (in one's group)

Expectation = one's expectations ('beliefs') of the other three group members' contributions

Significance levels: * p<0.05, ** p<0.01, *** p<0.001

5.1 Interactions of the communication conditions with pre-existing social ties

To obtain further insights about the effects of our two communication treatments, we next look at the role of pre-existing social ties and how they interact with each communication treatment (Table 6). The models with interaction terms consider social ties in two different ways: model 5 and model 7 treat social ties ('FdsFam') as continuous variables whereas models 6 and 8 treat them as factor variables with a dummy for each possible value (1, 2 and 3) other than zero, which works as the counterfactual.

Table 6: Tobit regression models for contributions to public good with interaction effects

Dep. var: contribution models with interaction terms	model 5 coefficient (std. err.)	model 6 coefficient (std. err.)	model 7 coefficient (std. err.)	model 8 coefficient (std. err.)
T1: Unrelated	5.567** (1.96)	6.741** (2.36)	5.633*** (1.41)	6.992*** (1.84)
T1 × [#FdsFam]	-1.791* (0.71)		-1.694** (0.59)	
T1 × [FdsFam = 1]		-4.905* (2.38)		-5.244* (2.09)
T1 × [FdsFam = 2]		-4.168 (3.07)		-6.125* (2.36)
T1 × [FdsFam = 3]		-6.487** (2.28)		-6.127** (1.89)
T2: Dilemma-related	6.530*** (1.94)	6.873** (2.28)	7.274*** (1.59)	8.103*** (2.08)
T2 × [#FdsFam]	-1.784** (0.67)		-1.988*** (0.57)	
T2 × [FdsFam = 1]		-2.720 (2.56)		-4.060 (2.41)
T2 × [FdsFam = 2]		-3.586 (2.87)		-5.445* (2.50)
T2 × [FdsFam = 3]		-5.713* (2.26)		-6.619** (2.06)
#FdsFam	1.181* (0.57)		1.370** (0.46)	
[FdsFam = 1]		2.962 (2.22)		4.195* (2.05)
[FdsFam = 2]		2.390 (2.74)		3.942 (2.34)
[FdsFam = 3]		4.083* (1.94)		4.929** (1.67)
expectation	0.521*** (0.15)	0.522*** (0.16)	0.456** (0.14)	0.469** (0.15)
constant	-0.643 (2.31)	-1.244 (2.55)	-1.437 (1.95)	-2.594 (2.30)
'socioeconomics'	yes	yes	yes	yes
exclude misunderstood	no	no	yes	yes
var(e.contribution)	14.332*** (2.43)	14.144*** (2.44)	10.964*** (2.04)	10.751*** (2.05)
N	215	215	172	172

Tobit regression models with standard errors clustered on group (n = 4) level, censoring at the lower (0) and upper (10) endpoint for contributions

Socioeconomic covariates as shown in Table 2. Full regression model in Supplementary Materials A.6.2

#FdsFam = number of friends and family members (in one's group)

Expectation = one's expectations ('beliefs') of the other three group members' contributions

Significance levels: * p<0.05, ** p<0.01, *** p<0.001

In the models that include interaction terms, the number of friends and family members in one's group shows a positive association with contributions, which, however, only describes the effect in the control condition. This positive association is also visible in Figure 2 and Table 7.⁸

⁸ As visible in Table 7, social ties, measured as friends and family members, are not perfectly balanced across the treatment conditions. The simple comparison of means for values of 'FdsFam' in Figure 2 may therefore give a

For the two communication treatments, no apparent relation between social ties and contributions stands out. For this reason, the coefficient for the number of friends and family members does not become significant across all conditions as in the basic models, either (Table 5).

Figure 2: Contributions by treatment conditions and social contexts

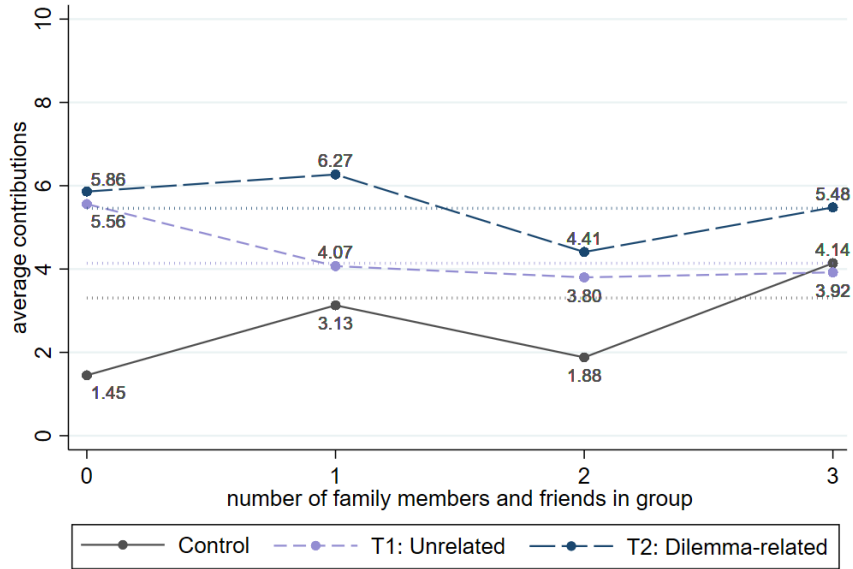


Table 7: Contributions by treatment conditions and social contexts

number of friends and family in group	control none		treatment 1 unrelated		treatment 2 dilemma-related	
	count	contribution mean (std. dev.)	count	contribution mean (std. dev.)	count	contribution mean (std. dev.)
0	11	1.45 (3.21)	9	5.56 (3.43)	21	5.86 (2.95)
1	8	3.13 (3.56)	14	4.07 (2.81)	11	6.27 (3.29)
2	8	1.88 (3.35)	10	3.80 (2.82)	17	4.41 (2.83)
3	44	4.14 (3.70)	39	3.92 (3.14)	23	5.48 (2.97)
N		71		72		72

In all models of Table 6, the addition of interaction terms between the communication condition dummies and pre-existing social ties leads to much larger and highly significant regression coefficients for both types of communication. They denote the effect at zero friends or family members in the group. The interaction terms between social ties and the communication

biased picture. Further, the sample size of this experiment might not yield enough statistical power for a detailed analysis of all single values for ‘FdsFam’.

treatments assume negative and, mostly, significant values. This implies that both unrelated and dilemma-related communication are most effective among socially distant people, while there is only a reduced effect among already close group members. The same can be seen more clearly in Table 8, which shows the marginal effects of the Tobit estimations including interaction terms at each possible value of friends and family members: the strongest and most significant effect of communication is found at low counts of friends and family members in one's group. Indeed, for the highest value of social ties ('FdsFam' = 3), i.e., groups consisting entirely of friends and family members, communication has no significant effect anymore. This shows that pre-existing social ties and communication work as substitutes. Finally, Table 8 also includes average marginal effects of the treatment conditions based on the Tobit models with interaction terms. Both types of communication conditions show significant and positive effects on cooperation in all four models. Across all model specifications and values for 'FdsFam', the effect on cooperation is distinctly larger for dilemma-related communication as compared to unrelated communication.

Table 8: Marginal effects for Tobit estimations with interaction effects

marginal effects <i>models with interaction terms</i>	for model 5 <i>coeff./ (std. err.)</i>	for model 6 <i>coeff./ (std. err.)</i>	for model 7 <i>coeff./ (std. err.)</i>	for model 8 <i>coeff./ (std. err.)</i>
marginal effects for given values of 'FdsFam'				
T1: Unrelated				
[FdsFam = 0]	5.567** (1.96)	6.741** (2.36)	5.633*** (1.41)	6.992*** (1.84)
[FdsFam = 1]	3.775** (1.38)	1.835 (2.03)	3.340*** (1.02)	1.748 (1.30)
[FdsFam = 2]	1.984* (0.99)	2.583 (1.84)	2.246* (0.88)	0.867 (1.13)
[FdsFam = 3]	0.192 (1.01)	0.254 (1.10)	0.553 (1.09)	0.865 (1.24)
T2: Dilemma-related				
[FdsFam = 0]	6.530*** (1.94)	6.873** (2.28)	7.274*** (1.59)	8.103*** (2.08)
[FdsFam = 1]	4.746*** (1.41)	4.153 (2.16)	5.286*** (1.17)	4.044** (1.53)
[FdsFam = 2]	2.961** (1.05)	3.289 (1.70)	3.300*** (0.93)	2.659* (1.08)
[FdsFam = 3]	1.177 (1.05)	1.16 (1.15)	1.311 (1.01)	1.485 (1.13)
average marginal effects				
T1: Unrelated	2.059* (1.00)	2.111* (1.01)	2.453** (0.88)	2.322** (0.83)
T2: Dilemma-related	3.036** (1.06)	3.055** (1.04)	3.54*** (0.94)	3.501*** (0.85)

#FdsFam = number of friends and family members (in one's group)
Significance levels: * p<0.05, ** p<0.01, *** p<0.001

5.2 Comprehension

Next, we investigate if comprehension works a mechanism in dilemma-related communication and may hence be able to explain why dilemma-related discussions result in even more cooperation than unrelated communication. It turns out that indeed more participants answered both control questions correctly after dilemma-related discussions (90%) as compared to the control condition (74%) and the unrelated discussions (76%). The differences are significant and show that talking with one's group members about the social dilemma increases comprehension (for details see Supplementary Materials A.8.1, A.8.2).

However, comparing contributions between those, who have answered both control questions correctly, and those, who have not, shows that contributions from the latter were higher in all experimental conditions (Table 4, Supplementary Materials A.8.1). Increased comprehension can therefore be ruled out as the relevant mechanism for higher contributions after dilemma-related communication. Also, the effects of the communication treatments on cooperation follow the same pattern across both subgroups (Table 4, Supplementary Materials A.8.3). Additional regression models using comprehension as the dependent variable can be found in the Supplementary Materials (A.8.4). They also reveal a positive and significant correlation between the dilemma-related communication condition and correctly answering the control questions and show that education, measured in years of schooling, increase comprehension of the social dilemma problem (Supplementary Materials A.8.5).

5.3 Expectations of others' contributions and trust

It turns out that neither the unrelated nor the dilemma-related communication condition has any effect on the expectations of one's group members behavior in the public good game. As evident in Table 4, there are only very small and insignificant differences in average expectations across all three experimental conditions and they do not follow a specific pattern that suggests any association with the treatment conditions (see also Supplementary Materials A.9.1). We also conduct regression analyses using expectations as the dependent variable and find no significant correlations with the treatment conditions, either (Supplementary Materials A.9.2, A.9.3). The variability of expectations, measured as means of standard deviations within groups, is slightly, but not significantly, lower after dilemma-related communication (Table 4).

We further measure the correctness of stated expectations: After dilemma-related discussion more than half of the participants (56%) correctly guessed the average contribution of their group members compared to 33% and 38% for the control condition and the unrelated communication, respectively (Table 4). It stands out, however, that average contributions are lower than average expectations of the other players' contributions, especially in the control

and the unrelated treatment conditions, which means that, on average, players deliberately contributed less than what they expected their group members to contribute.

Individual expectations were also added as an explanatory variable in most of the main regression models on contributions (Tables 5 and 6). This is possible because, as it has just been shown, the treatment conditions do not affect expectations.⁹ On individual level, expectations show a positive and significant correlation with contributions in almost all models, which reassures the concept of conditional cooperation and, vice versa, our elicitation of expectations.

In addition to expectations, we measure trust in one's group members in the post-experimental survey (Table 4, Supplementary Materials A.10.1). Trust was elicited on a 4-level scale from 1 (do not trust at all) to 4 (trust completely). It turns out that, similar to results found regarding expectations, trust is not significantly affected by the communication treatments. Univariate regression analyses find a positive, significant correlation between trust and expectations but not so between trust and contributions (Supplementary Materials A.10.2). While we do not find any associations of pre-existing social ties with expectations, there is a positive correlation with trust. This means participants trust their group members more if they consist of friends and family members, but do not have higher expectations of their contributions.

6. Discussion

With this study we aim to learn more about the mechanisms through which talking to one's group members before decision making can raise cooperation in social dilemma situations. Specifically, we hypothesize that changes in group members relationship with each other are part of the effect. To test this, we introduce an unrelated communication condition that does not allow discussing the social dilemma. We further conduct our experiment in a natural field setting and take into account effects of pre-existing social ties between participants.

Results show that unrelated communication is indeed able to increase cooperation, but the effect is mostly present among socially distant group members. As unrelated talk does not allow discussing the dilemma, which includes explanations, appeals or making commitments, increases in cooperation must be the result of changes in group members' relationship with each other, such as through decreases in social distance or the promotion of a group identity. For dilemma-related communication, we find even larger effects on cooperation than for unrelated

⁹ The same regression models as in Table 5 and 6 without expectations as an explanatory variable can be found in the Supplementary Materials A.7. Results on the effect of the communication treatments as well as their interactions with social ties remain virtually the same.

communication, which are, similarly, strongest among group members who are not already socially close to each other. Conversely, either type of communication has only a reduced or no additional effect on cooperation if group members already show a high level of cooperativeness due to their pre-existing social ties. Pre-existing social ties and communication, even on unrelated topics, hence work as substitutes in explaining cooperation. Technically, it can also be interpreted that social ties become irrelevant once communication is possible, but this view seems less intuitive.

Importantly though, dilemma-related communication results in even higher rates of cooperation than discussions about an unrelated topic. Concerning potential reasons for this difference, we are firstly able to rule out better comprehension after dilemma-related communication: While comprehension is increased by dilemma-related discussions, it cannot be associated with higher contributions. We further find that expectations of the other group members' contributions are not affected by either communication condition. Raised expectations are therefore not part of the mechanisms behind the effect of communication on cooperation which seems at odds with the literature's consensus on commitments as the main driver behind the communication effect (cf. Bornstein and Rapoport 1988; Orbell et al. 1988; Kerr and Kaufman-Gilliland 1994; Kerr et al. 1997; Bicchieri and Lev-On 2007). If commitments worked through trust in each others' pledges, reciprocity, or even the creation of social norms, they should be reflected in higher average expectations (cf. Bornstein and Rapoport 1988; Bicchieri and Lev-On 2007). An explanation for higher cooperation after dilemma-related than after unrelated communication, that is still in accordance with commitments as the major driver, can, however, be found in personal or injunctive norms. In this context, the relevant personal or injunctive norm would be the one to fulfill one's own appeals and promises to contribute made during discussion, even despite not necessarily believing in one's group members' pledges to do the same. In other words, individuals may feel bound to stick to their commitments due to their own standards and norms of appropriate conduct rather than due to the need to fulfill social contracts or the expectations of others. Also, Vanberg (2008) found that promises were kept due to the personal feeling of being obliged to do so, and not as to avoid disappointing others.

Supportive of this line of argumentation, we did not only find zero effects of either communication condition on expectations (and nonetheless communication led to higher cooperation rates) but also observed a preference to cooperate more with socially close group members without any links to raised expectations. This means that rather than following a social norm or a feeling of having to meet certain expectations, contributing more in the presence of socially close group members is, in a similar manner, one's very own preference. It can be

explained by in-group favoritism, and also finds support in evolutionary theories on kin selection (Hamilton 1964; Tajfel et al. 1971; Tajfel et al. 1979, Caporeale et al. 1989, Peters et al. 2004; Candelo et al. 2018).

As an alternative explanation, it could be hypothesized that is it not the deliberate adherence to a personal norm of keeping promises but an intuitive, and possibly irrational, stickiness to commitments made during discussions (Orbell et. al. 1988; Kerr et al. 1997; Kahneman 2011). However, previous experimental studies have found no effect of signaling one's (non-binding) intentions without the possibility to freely discuss the dilemma, which speaks against this theory (Dawes 1977; Chen and Komorita 1994; Wilson and Sell 1997; Bochet et al. 2006).

Based on our results, we can also not rule out the possibility that finding agreement on an important, relevant topic, such as the mutual consensus to cooperate, could affect social relations even further than just talking about unrelated issues, and hence even the additional effect of dilemma-related communication could work primarily through altered social relationships between participants (cf. Dawes et al. 1988; Orbell et al. 1988; Kerr and Kaufman-Gilliland 1994; cf. Bouas and Komorita 1996; Bicchieri 2002; Spears 2011). Spears (2011), in particular, points out that group identity can become salient depending on the content of the conversation in discussions. It could be an interesting aspect for future research to investigate if talking about something controversial like politics or religion leads to disputes and consequently a decrease in cooperation. Researchers could further try to analyze in more detail how group discussions affect social relationships by eliciting an empirical measurement for group identity (or the perception thereof). Also, more sophisticated measurements of norms could be implemented, especially with a distinction between social and personal norms. Finally, one could re-run similar experiments with different participants in additional settings, such as with strangers rather than community members. Our results show that the effect of communication on cooperation is largest among socially distant individuals and speak against the theory that social closeness is a prerequisite for effective communication (cf. Hoffman et al. 1996; Hardin 2003; Simpson 2007; Barbalet 2009). It should be kept in mind that, within the setting of our study, participants all came from the same village and hence even those who did not identify each other as friends or family members, were not real strangers but do, mostly, at least know each other, which one could interpret as 'weak' social ties. It could be investigated if communication effects are even stronger between actual strangers, who have never met each other. Given that social ties and communication work as substitutes, our setting would then also explain why we find a relatively small effect of dilemma-related face-to-face communication as compared to experiments conducted in the lab (e.g., Bochet et al. 2006). On the other hand,

the effect of communication on cooperation might also be non-linear and concave in social ties if communication is less effective again among very distant individuals.

Concerning policy relevance, our results show that communication promotes cooperative behavior, even if it is about unrelated topics and especially between socially distant group members. For socially close group members, on the other hand, cooperation is already high, and communication has less of an additional effect on cooperation. As another relevant insight, our findings on the effect on comprehension imply that increasing education and understanding of the social dilemma problem must not necessarily help in solving it. Indeed, in our setting, participants who had trouble to correctly answer our control questions contributed, on average, more to the public good. On a more abstract level, our results suggest investigating the potential of personal and injunctive norms and their application. Personal norms might work in certain situations independently from social norms and expectations about others' compliance, which could find use as a tool to further promote prosocial and cooperative behavior.

To conclude, we may recall and consider that average contributions were below the average expectations about one's group members' contributions in all experimental conditions. The effect of communication on cooperation could therefore also be worded differently: Individuals behave less selfishly after communication.



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Competing Interests Statement

The authors declare that there is no conflict of financial, general, or institutional competing interests.

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Supplementary Materials

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B. Experimental Protocols

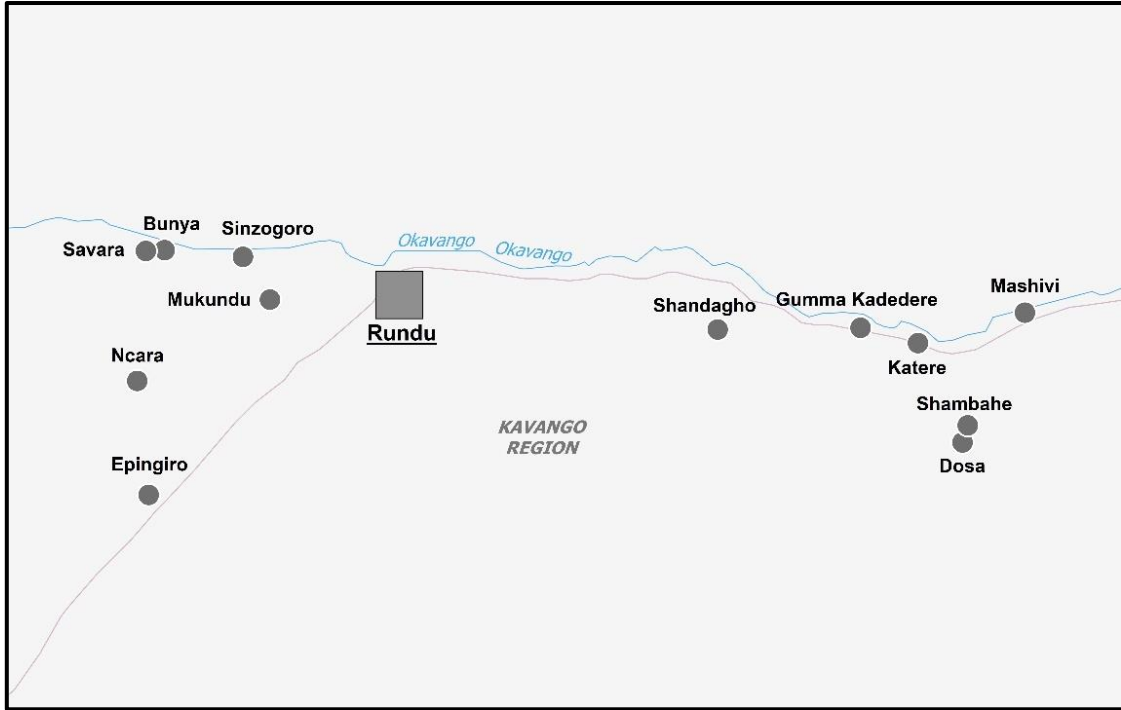
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Supplement A: Appendix

A.1 Map of selected villages



A.2: Description of the public good game payoffs

The public good game payoffs P_i can be formalized with the following equation for individuals (equation 1) and for the group as a whole S (equation 2):

Equation 1:

$$P_i(e_i, c_i, c_j) = e_i - c_i + \frac{m}{n} c_i + \frac{m}{n} \sum_{j=1}^{n-1} c_j$$

Equation 2:

$$S(e_i, c_i) = \sum_{i=1}^n P_i(e_i, c_i)$$

$$S(e_i, c_i) = n * P_i(e_i, c_i)$$

$$S(e_i, c_i) = n * e_i - n * c_i + m \sum_{i=1}^n c_i$$

P_i = Payoff of player i

e_i = endowment player i

c_i = contribution decision of player i , $\in \{0, \dots, 10\}$

c_j = contribution decision of player j , $\in \{0, \dots, 10\}$

m = multiplier of group account

n = number of players in the group

S = Sum of all players' payoffs

With an endowment (e) of 10 coins, a group size (n) of 4 player and the social multiplier (m) set to 2, the payoff equation becomes (equation 3):

Equation 3:

$$P_i(c_i, c_j) = 10 - c_i + \frac{2}{4}c_i + \frac{2}{4}\sum_{j=1}^3 c_j$$

$$P_i(c_i, c_j) = 10 - \frac{1}{2}c_i + \frac{1}{2}\sum_{j=1}^3 c_j$$

With c_i being the only variable that player i can manipulate, it becomes obvious that contributing nothing is the individually best option. For the group payoff (equation 4), on the other hand, there is:

Equation 4:

$$S(c_i) = 4 * P_i(c_i)$$

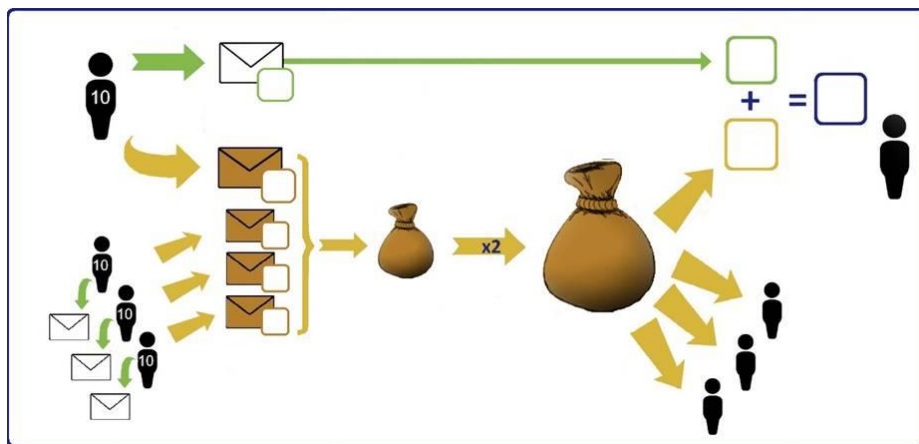
$$S(c_i) = 40 - 4 * c_i + 2 \sum_{i=1}^4 c_i$$

$$S(c_i) = 40 - 4 * c_i + 2 * 4 * c_i$$

$$S(c_i) = 40 + 4c_i$$

This shows that, for the group, contributing as much as possible leads to the highest payoff. Since c_i is capped at 10, this is the social optimum in the game. The minimum payoff in the experiment is 5 coins (25N\$) for someone, who contributes everything while in a group with three free riders. The maximum payoff is 25 coins (125N\$) for a free rider in a group with three cooperators. Plus, the bonus for correctly estimating the other players' contribution, the maximum amount that can be earned is $125 + 20 = 145\text{N\$}$.

A.3: Poster used to explain the public good game

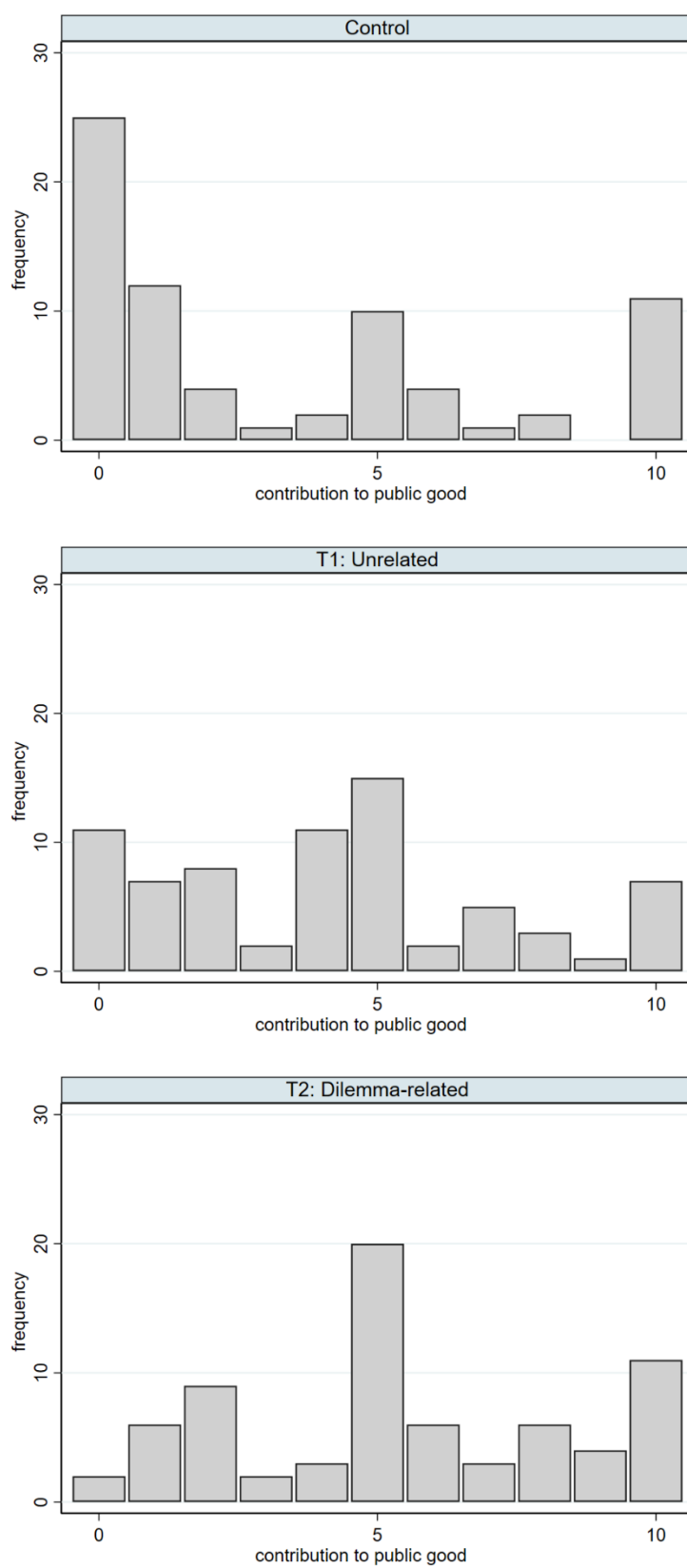


A.4 Distribution of social ties across experimental conditions

	Control	T1: Unrelated	T2: Dilemma-related	Total
strangers				
0	70	72	66	208
1	1	0	1	2
2	0	0	2	2
3	0	0	3	3
acquaintances				
0	45	39	27	111
1	7	10	18	35
2	8	14	11	33
3	11	9	16	36
friends				
0	40	42	42	124
1	17	20	12	49
2	4	7	12	23
3	10	3	6	19
family (or same HH)				
0	24	20	36	80
1	15	17	21	53
2	10	14	6	30
3	22	21	9	52

Numbers in parcels show the frequency of reported social ties. Given a sample size of 216 participants and each participant stating the relationship to three group members, 648 social ties should have been reported in total. Since one participant left the workshop before surveys were finished, we only have 645 reported social ties in our data.

A.5 Distribution of contributions across experimental conditions



A.6 Full Tobit regression models for contributions

A.6.1 Tobit regression: contribution - basic models

Dep. var: contribution <i>models without interaction terms</i>	model 1 <i>coeff./ (std. err.)</i>	model 2 <i>coeff./ (std. err.)</i>	model 3 <i>coeff./ (std. err.)</i>	model 4 <i>coeff./ (std. err.)</i>
T1: Unrelated	1.331 (1.15)	1.647 (0.97)	1.474 (1.06)	1.906* (0.91)
T2: Dilemma-related	3.129* (1.28)	2.851** (1.06)	3.372** (1.20)	3.216** (0.99)
#FdsFam		-0.081 (0.27)		0.003 (0.24)
expectation		0.540*** (0.15)		0.497*** (0.15)
age		0.008 (0.03)		0.002 (0.03)
female (d)		-0.055 (0.78)		-0.566 (0.78)
schooling years		-0.255** (0.08)		-0.168 (0.09)
hectares		-0.189 (0.15)		-0.069 (0.14)
bags yield		0.105** (0.03)		0.050 (0.04)
farmer (d)		-1.611* (0.66)		-1.238 (0.74)
migrant (d)		1.097 (0.76)		1.376 (0.74)
constant	2.635** (0.99)	2.028 (2.07)	2.175* (0.92)	1.375 (1.70)
<i>exclude misunderstood</i>	<i>no</i>	<i>no</i>	<i>yes</i>	<i>yes</i>
var(e.D_contribute)	19.814*** (3.59)	15.027*** (2.58)	14.958*** (3.10)	11.738*** (2.30)
N	216	215	173	172

Tobit regression models with standard errors clustered on group (n = 4) level, censoring at the lower (0) and upper (10) endpoint for contributions, #FdsFam = number of friends and family members (in one's group)

Expectation = one's expectations ('beliefs') of the other three group members' average contributions

Significance levels: * p<0.05, ** p<0.01, *** p<0.001

A.6.2 Tobit regression: contribution - models with interaction effects

Dep. var: contribution <i>models with interaction terms</i>	model 5 <i>coeff./ (std. err.)</i>	model 6 <i>coeff./ (std. err.)</i>	model 7 <i>coeff./ (std. err.)</i>	model 8 <i>coeff./ (std. err.)</i>
T1: Unrelated	5.567** (1.96)	6.741** (2.36)	5.633*** (1.41)	6.992*** (1.84)
T1 × [#FdsFam]	-1.791* (0.71)		-1.694** (0.59)	
T1 × [FdsFam = 1]		-4.905* (2.38)		-5.244* (2.09)
T1 × [FdsFam = 2]		-4168 (3.07)		-6.125* (2.36)
T1 × [FdsFam = 3]		-6.487** (2.28)		-6.127** (1.89)
T2: Dilemma-related	6.530*** (1.94)	6.873** (2.28)	7.274*** (1.59)	8.103*** (2.08)
T2 × [#FdsFam]	-1.784** (0.67)		-1.988*** (0.57)	
T2 × [FdsFam = 1]		-2.720 (2.56)		-4.060 (2.41)
T2 × [FdsFam = 2]		-3.586 (2.87)		-5.445* (2.50)
T2 × [FdsFam = 3]		-5.713* (2.26)		-6.619** (2.06)
#FdsFam	1.181* (0.57)		1.370** (0.46)	
[FdsFam =1]		2.962 (2.22)		4.195* (2.05)
[FdsFam =2]		2.390 (2.74)		3.942 (2.34)
[FdsFam =3]		4.083* (1.94)		4.929** (1.67)
expectation	0.521*** (0.15)	0.522*** (0.16)	0.456** (0.14)	0.469** (0.15)
age	0.006 (0.03)	0.006 (0.03)	-0.001 (0.03)	-0.001 (0.03)
female (d)	-0.165 (0.74)	-0.109 (0.74)	-0.647 (0.72)	-0.562 (0.72)
schooling years	-0.267** (0.08)	-0.264** (0.08)	-0.171 (0.09)	-0.175 (0.09)
hectares	-0.168 (0.14)	-0.155 (0.16)	-0.072 (0.13)	-0.007 (0.15)
bags yield	0.094** (0.03)	0.093** (0.03)	0.043 (0.03)	0.039 (0.04)
farmer (d)	-1.372* (0.66)	-1.372 (0.70)	-1.024 (0.78)	-0.995 (0.76)
migrant (d)	1.086 (0.81)	1.028 (0.87)	1.463 (0.79)	1.350 (0.84)
constant	-0.643 (2.31)	-1.244 (2.55)	-1.437 (1.95)	-2.594 (2.30)
<i>exclude misunderstood</i>	<i>no</i>	<i>no</i>	<i>yes</i>	<i>yes</i>
var(e.contribute)	14.332*** (2.43)	14.144*** (2.44)	10.964*** (2.04)	10.751*** (2.05)
N	215	215	172	172

Tobit regression models with standard errors clustered on group (n = 4) level, censoring at the lower (0) and upper (10) endpoint for contributions, #FdsFam = number of friends and family members (in one's group), Expectation = one's expectations ('beliefs') of the other three group members' average contributions, Significance levels: * p<0.05, ** p<0.01, *** p<0.001

A.7 Alternative regression model specifications: contributions

A.7.1 Tobit regression: contribution – basic models without expectations

Dep. var: contribution <i>models without interaction terms</i>	model 1 <i>coeff./ (std. err.)</i>	model 2 <i>coeff./ (std. err.)</i>	model 3 <i>coeff./ (std. err.)</i>	model 4 <i>coeff./ (std. err.)</i>
T1: Unrelated	1.331 (1.15)	1.540 (1.06)	1.474 (1.06)	1.935* (0.97)
T2: Dilemma-related	3.129* (1.28)	3.006* (1.23)	3.372** (1.20)	3.472** (1.10)
#FdsFam		-0.034 (0.31)		-0.014 (0.29)
age		0.007 (0.03)		0.004 (0.03)
female (d)		0.024 (0.88)		-0.644 (0.90)
schooling years		-0.286*** (0.09)		-0.186 (0.10)
hectares		-0.195 (0.16)		-0.104 (0.15)
bags yield		0.094** (0.03)		0.032 (0.04)
farmer (d)		-1.022 (0.75)		-0.803 (0.85)
migrant (d)		1.39 (0.76)		1.606* (0.74)
constant	2.635** (0.99)	4.848* (2.18)	2.175* (0.92)	4.100* (1.83)
<i>exclude misunderstood</i>	<i>no</i>	<i>no</i>	<i>yes</i>	<i>yes</i>
var(e.contribute)	19.814*** (3.59)	17.273*** (3.24)	14.958*** (3.10)	13.657*** (2.88)
N	216	215	173	172

Tobit regression models with standard errors clustered on group (n = 4) level, censoring at the lower (0) and upper (10) endpoint for contributions, #FdsFam = number of friends and family members (in one's group)

Model 1 and 3 are the same as reported in table 5 (did already not contain expectations)

Significance levels: * p<0.05, ** p<0.01, *** p<0.001

A.7.2 Tobit regression: contribution - models with i.e.s without expectations

Dep. var: contribution <i>models with interaction terms</i>	model 5 <i>coeff./ (std. err.)</i>	model 6 <i>coeff./ (std. err.)</i>	model 7 <i>coeff./ (std. err.)</i>	model 8 <i>coeff./ (std. err.)</i>
T1: Unrelated	6.161** (2.19)	7.166** (2.42)	6.999*** (1.57)	7.925*** (1.97)
T1 × [#FdsFam]	-2.115** (0.79)		-2.314** (0.71)	
T1 × [FdsFam = 1]		-4.850* (2.15)		-4.763* (2.17)
T1 × [FdsFam = 2]		-4.595 (3.25)		-6.354** (2.41)
T1 × [FdsFam = 3]		-7.319** (2.37)		-7.678*** (2.23)
T2: Dilemma-related	6.888** (2.31)	7.215** (2.55)	8.084*** (1.86)	8.626*** (2.21)
T2 × [#FdsFam]	-1.862* (0.79)		-2.209** (0.68)	
T2 × [FdsFam = 1]		-2.684 (2.48)		-3.365 (2.37)
T2 × [FdsFam = 2]		-3.749 (3.19)		-5.224* (2.37)
T2 × [FdsFam = 3]		-5.781* (2.52)		-6.922** (2.24)
#FdsFam	1.364* (0.65)		1.659** (0.50)	
[FdsFam =1]		2.86 (1.99)		3.504 (1.94)
[FdsFam =2]		2.227 (2.97)		3.455 (2.05)
[FdsFam =3]		4.543* (2.07)		5.531** (1.69)
age	0.004 (0.03)	0.001 (0.03)	0.001 (0.03)	-0.002 (0.03)
female (d)	-0.116 (0.83)	-0.039 (0.84)	-0.758 (0.82)	-0.67 (0.84)
schooling years	-0.298*** (0.09)	-0.298*** (0.08)	-0.181 (0.10)	-0.186 (0.09)
hectares	-0.177 (0.15)	-0.147 (0.17)	-0.116 (0.14)	-0.046 (0.16)
bags yield	0.084** (0.03)	0.080* (0.03)	0.027 (0.03)	0.020 (0.03)
farmer (d)	-0.795 (0.75)	-0.708 (0.76)	-0.592 (0.88)	-0.464 (0.87)
migrant (d)	1.369 (0.80)	1.326 (0.85)	1.724* (0.78)	1.659* (0.82)
constant	1.811 (2.59)	1.367 (2.71)	0.323 (2.01)	-0.381 (2.17)
<i>exclude misunderstood</i>	<i>no</i>	<i>no</i>	<i>yes</i>	<i>yes</i>
var(e.contribute)	16.424*** (3.06)	16.180*** (3.1)	12.540*** (2.43)	12.310*** (2.46)
N	215	215	172	172

Tobit regression models with standard errors clustered on group (n = 4) level, censoring at the lower (0) and upper (10) endpoint for contributions, #FdsFam = number of friends and family members (in one's group)
Significance levels: * p<0.05, ** p<0.01, *** p<0.001

A.7.3 OLS regression: contribution - basic models

Dep. var: contribution <i>models without interaction terms</i>	model 1 <i>coeff./ (std. err.)</i>	model 2 <i>coeff./ (std. err.)</i>	model 3 <i>coeff./ (std. err.)</i>	model 4 <i>coeff./ (std. err.)</i>
T1: Unrelated	0.833 (0.75)	1.165 (0.65)	0.93 (0.72)	1.382* (0.63)
T2: Dilemma-related	2.153* (0.87)	2.048** (0.73)	2.383** (0.84)	2.396** (0.71)
#FdsFam		-0.064 (0.18)		0.019 (0.17)
expectation		0.392*** (0.09)		0.395*** (0.10)
age		0.005 (0.02)		-0.001 (0.02)
female (d)		-0.224 (0.55)		-0.585 (0.57)
schooling years		-0.192** (0.06)		-0.142 (0.07)
hectares		-0.146 (0.11)		-0.079 (0.12)
bags yield		0.065*** (0.02)		0.039 (0.03)
farmer (d)		-0.919* (0.45)		-0.709 (0.56)
migrant (d)		0.723 (0.54)		0.983 (0.56)
constant	3.306*** (0.64)	2.848 (1.47)	2.925*** (0.61)	2.215 (1.25)
<i>exclude misunderstood</i>	<i>no</i>	<i>no</i>	<i>yes</i>	<i>yes</i>
N	216	215	173	172

Ordinary-Least-Squares regression models with standard errors clustered on group (n = 4) level

#FdsFam = number of friends and family members (in one's group)

Expectation = one's expectations ('beliefs') of the other three group members' average contributions

Significance levels: * p<0.05, ** p<0.01, *** p<0.001

A.7.4 OLS regression: contribution - models with interaction effects

Dep. var: contribution <i>models with interaction terms</i>	model 5 <i>coeff./ (std. err.)</i>	model 6 <i>coeff./ (std. err.)</i>	model 7 <i>coeff./ (std. err.)</i>	model 8 <i>coeff./ (std. err.)</i>
T1: Unrelated	3.347** (1.18)	4.029** (1.31)	3.456*** (0.84)	4.197*** (0.87)
T1 × [#FdsFam]	-1.017* (0.43)		-1.007** (0.26)	
T1 × [FdsFam = 1]		-2.947* (1.30)		-3.042* (1.2β)
T1 × [FdsFam = 2]		-1.993 (1.93)		-3.097* (1.26)
T1 × [FdsFam = 3]		-3.743** (1.27)		-3.531** (1.09)
T2: Dilemma-related	4.160*** (1.08)	4.165** (1.16)	4.893*** (0.89)	5.145*** (1.00)
T2 × [#FdsFam]	-1.060* (0.40)		-1.268*** (0.36)	
T2 × [FdsFam = 1]		-0.980 (1.44)		-1.971 (1.38)
T2 × [FdsFam = 2]		-1.769 (1.65)		-2.911* (1.21)
T2 × [FdsFam = 3]		-3.165* (1.25)		-3.894** (1.14)
# FdsFam	0.637* (0.31)		0.823** (0.26)	
[FdsFam =1]		1.507 (1.10)		2.270* (1.02)
[FdsFam =2]		0.670 (1.49)		1.551 (0.99)
[FdsFam =3]		2.149* (0.95)		2.788*** (0.78)
expectation	0.381*** (0.09)	0.379*** (0.10)	0.372*** (0.09)	0.375*** (0.10)
age	0.003 (0.02)	0.001 (0.02)	-0.003 (0.02)	-0.005 (0.02)
female (d)	-0.29 (0.52)	-0.233 (0.53)	-0.635 (0.54)	-0.548 (0.56)
schooling years	-0.203** (0.06)	-0.204** (0.06)	-0.148* (0.07)	-0.150* (0.07)
hectares	-0.132 (0.11)	-0.115 (0.12)	-0.079 (0.11)	-0.025 (0.13)
bags yield	0.059** (0.02)	0.056** (0.02)	0.035 (0.03)	0.029 (0.03)
farmer (d)	-0.792 (0.47)	-0.746 (0.51)	-0.57 (0.59)	-0.492 (0.61)
migrant (d)	0.686 (0.56)	0.685 (0.61)	1.002 (0.58)	0.942 (0.63)
constant	1.476 (1.46)	1.285 (1.51)	0.646 (1.32)	0.185 (1.36)
<i>exclude misunderstood</i>	<i>no</i>	<i>no</i>	<i>yes</i>	<i>yes</i>
N	215	215	172	172

Ordinary-Least-Squares regression models with standard errors clustered on group (n = 4) level

#FdsFam = number of friends and family members (in one's group)

Expectation = one's expectations ('beliefs') of the other three group members' average contributions

Significance levels: * p<0.05, ** p<0.01, *** p<0.001

A.8 Comprehension

A.8.1 Comparison correctly and incorrectly answered control questions

Experimental condition: communication:	control none	treatment 1 unrelated	treatment 2 dilemma-related	differences		
	mean (std. dev.)	mean (std. dev.)	mean (std. dev.)	T1-C Δ (p)	T2-C Δ (p)	T2-T1 Δ (p)
contribution if control question correct	2.92 (3.51)	3.85 (2.60)	5.31 (2.91)	0.93 (0.59)	2.38*** (0.59)	1.45** (0.51)
contribution if control question wrong	4.37 (3.99)	5.06 (4.19)	6.86 (3.58)	0.69 (1.36)	2.49 (1.71)	1.80 (1.81)
# correct control questions ^A (% share of 72)	53 74%	55 76%	65 90%	2	12**	10*
differences in contributions between correct and wrong control questions						
Δ (std. err.)	1.44 (0.97)	1.20 (0.84)	1.55 (1.18)			

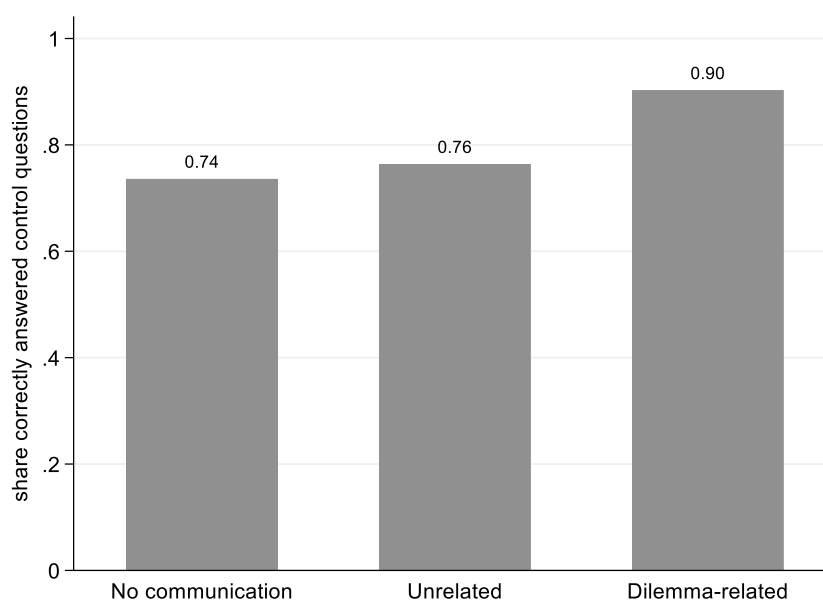
Control questions are considered as wrong if one or both control questions are answered incorrectly.

Test results according to student's t-tests. All test results are two-sided.

A: Chi-squared tests for differences in shares

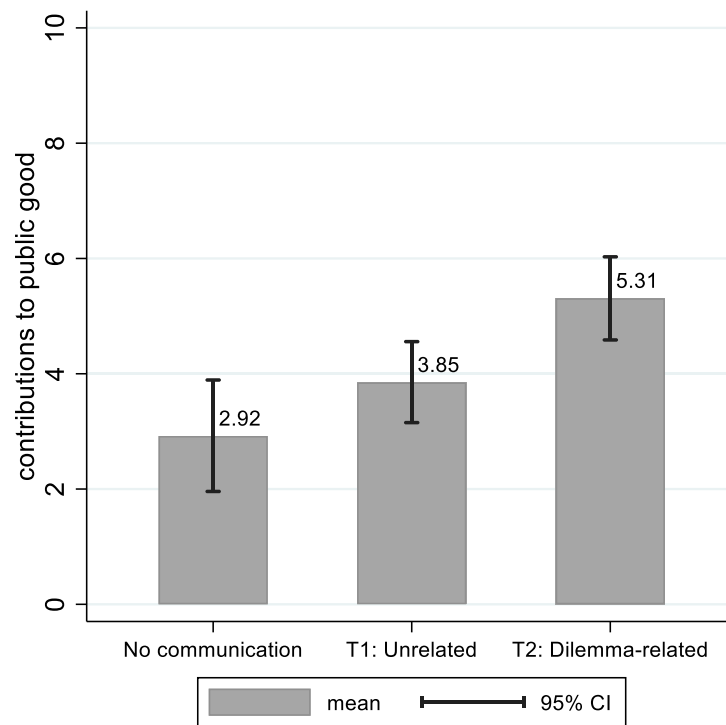
Significance levels: * p<0.05, ** p<0.01, *** p<0.001

A.8.2 Figure comprehension share

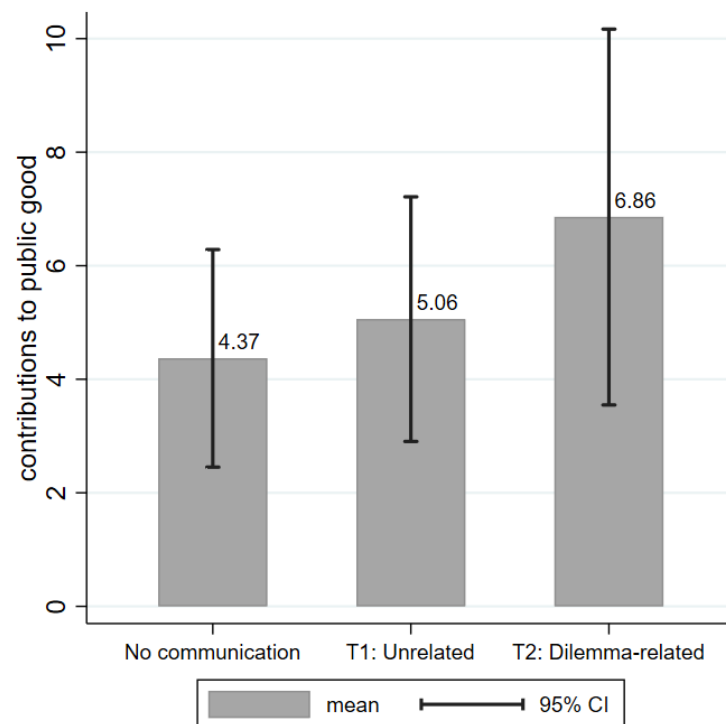


A.8.3 Figure comprehension and contributions

correctly understood only (n = 173)



misunderstood only (n = 43)



A.8.4 Regressions: comprehension - without socioeconomics

Dep. var:	control wrong	control wrong	contribution
estimation method:	OLS ^o	Probit	Tobit ^T
	coeff./(std. err.)	coeff./(std. err.)	coeff./(std. err.)
control question wrong			1.904* (0.96)
T1: Unrelated	-0.028 (0.09)	-0.087 (0.28)	1.403 (1.11)
T2: Dilemma-related	-0.167* (0.07)	-0.666* (0.30)	3.443** (1.25)
constant	0.264*** (0.06)	-0.631*** (0.19)	2.193*** (0.95)
var(e.contribute)			19.224*** (3.46)
N	216	216	216

^o Using Ordinary-Least-Squares regressions on a binary outcome variable is known as Linear-Probability-Model regression.

^T Tobit models for contributions with censoring at the lower (0) and upper (10) endpoint

All regression models with standard errors clustered on group (n=4) level

Significance levels: * p<0.05, ** p<0.01, *** p<0.001

A.8.5 Regressions: comprehension - with socioeconomics

Dep. var:	control wrong	control wrong	contribution
estimation method:	OLS	Probit	Tobit ^T
	coeff./(std. err.)	coeff./(std. err.)	coeff./(std. err.)
control question wrong			1.077 (0.88)
T1: Unrelated	-0.014 (0.09)	-0.095 (0.29)	1.561 (1.04)
T2: Dilemma-related	-0.173* (0.08)	-0.845* (0.30)	3.190** (1.21)
#FamFds	0.03 (0.02)	0.167 (0.10)	-0.065 (0.31)
age	0.000 (0.00)	0.000 (0.00)	0.006 (0.03)
female (d)	0.078 (0.06)	0.361 (0.23)	-0.053 (0.87)
schooling years	-0.031*** (0.01)	-0.129*** (0.03)	-0.256*** (0.08)
hectares	-0.018 (0.01)	-0.066 (0.06)	-0.174 (0.17)
bags yield	0.002 (0.00)	0.005 (0.01)	0.093** (0.03)
farmer (d)	-0.105 (0.09)	-0.433 (0.35)	-0.911 (0.77)
migrant (d)	-0.002 (0.06)	-0.046 (0.26)	1.397 (0.77)
constant	0.264*** (0.06)	-0.631*** (0.19)	4.362*** (2.21)
var(e.contribute)			17.110*** (3.196)
N	215	215	215

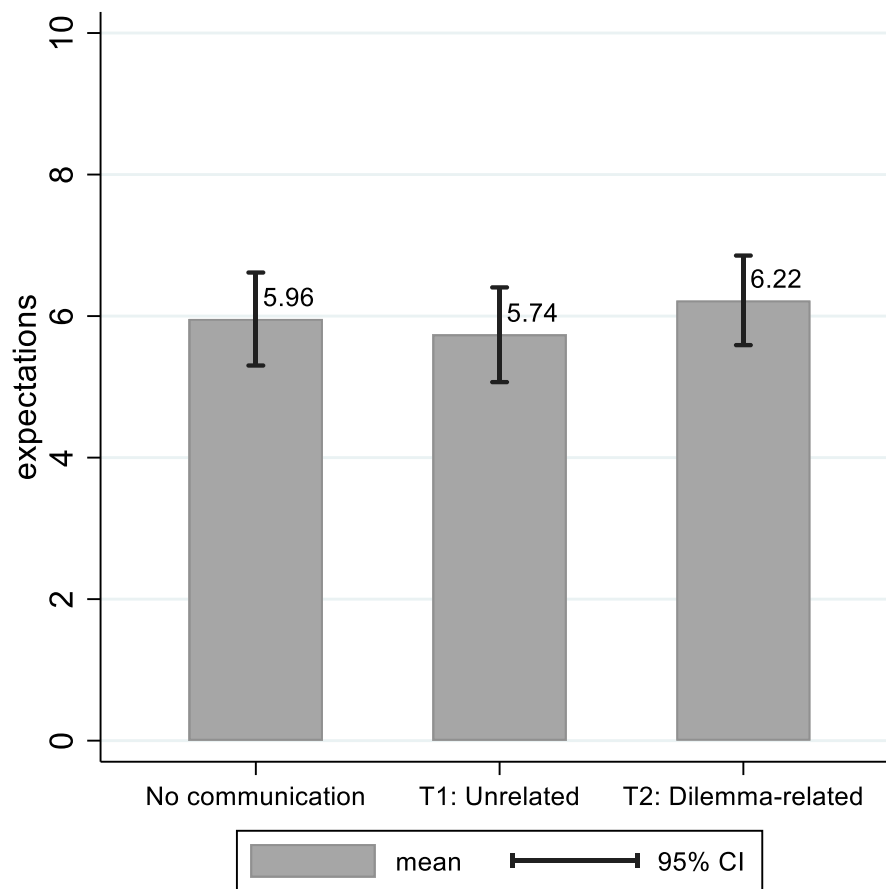
^T Tobit models for contributions with censoring at the lower (0) and upper (10) endpoint

All regression models with standard errors clustered on group (n=4) level

Significance levels: * p<0.05, ** p<0.01, *** p<0.001

A.9 Expectations

A.9.1 Figure Expectations



A.9.2 Tobit regression: expectations – basic models

Dep. var: expectations <i>models without interaction terms</i>	E model 1 <i>coeff./ (std. err.)</i>	E model 2 <i>coeff./ (std. err.)</i>	E model 3 <i>coeff./ (std. err.)</i>	E model 4 <i>coeff./ (std. err.)</i>
T1: Unrelated	-0.259 (0.90)	-0.513 (0.91)	-0.21 (1.06)	-0.196 (1.03)
T2: Dilemma-related	0.348 (0.92)	0.245 (0.92)	0.486 (0.99)	0.385 (0.91)
#FdsFam		0.125 (0.28)		-0.086 (0.30)
age		-0.003 (0.02)		0.008 (0.02)
female (d)		0.263 (0.58)		-0.114 (0.65)
schooling years		-0.077 (0.08)		-0.028 (0.10)
hectares		-0.008 (0.14)		-0.106 (0.14)
bags yield		-0.033 (0.02)		-0.053 (0.04)
farmer (d)		1.713* (0.75)		1.232 (0.83)
migrant (d)		0.575 (0.71)		0.479 (0.77)
constant	6.377*** (0.69)	5.353*** (1.58)	6.133*** (0.77)	5.773** (1.73)
<i>exclude misunderstood</i>	<i>no</i>	<i>no</i>	<i>yes</i>	<i>yes</i>
var(e.contribute)	14.330*** (2.48)	13.470*** (2.32)	13.421*** (2.61)	12.587*** (2.41)
N	216	215	173	172

Tobit regression models with standard errors clustered on group (n = 4) level, censoring at the lower (0) and upper (10) endpoint for expectations, #FdsFam = number of friends and family members (in one's group)

Expectation = one's expectations ('beliefs') of the other three group members' average contributions

Significance levels: * p<0.05, ** p<0.01, *** p<0.001

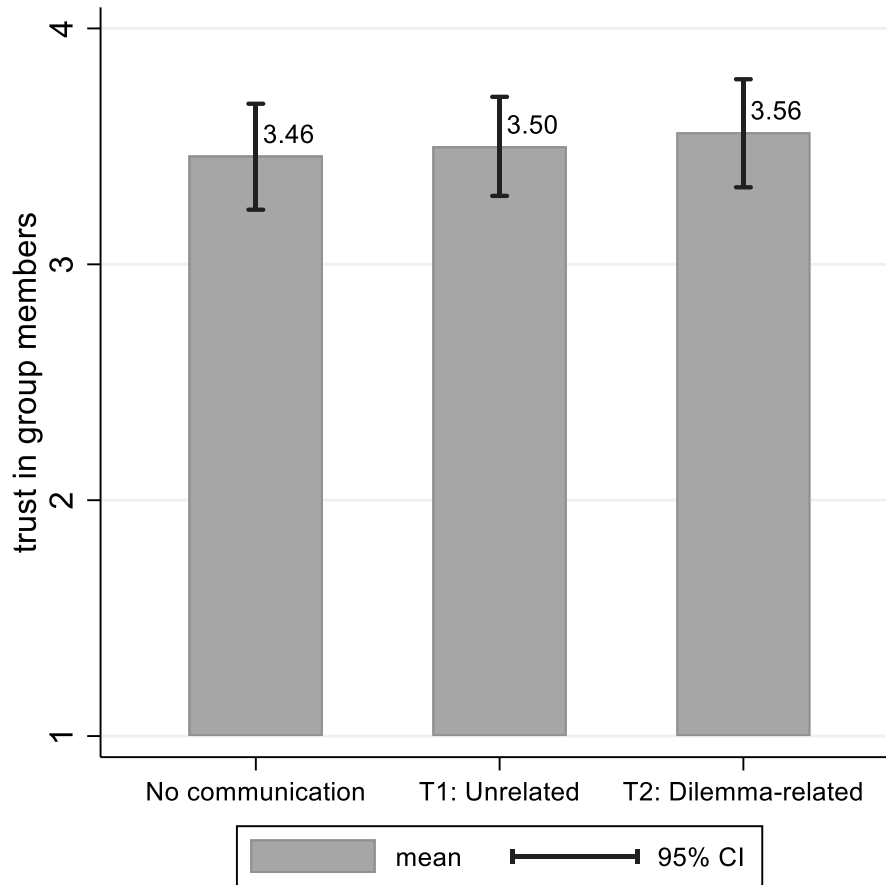
A.9.3 Tobit regression: expectation – models with interaction effects

Dep. var: expectations <i>models with interaction terms</i>	E model 5 <i>coeff./ (std. err.)</i>	E model 6 <i>coeff./ (std. err.)</i>	E model 7 <i>coeff./ (std. err.)</i>	E model 8 <i>coeff./ (std. err.)</i>
T1: Unrelated	0.954 (1.78)	0.386 (2.48)	3.139 (1.65)	1.557 (2.06)
T1 × [#FdsFam]	-0.682 (0.66)		-1.578* (0.62)	
T1 × [FdsFam = 1]		0.833 (2.98)		2.963 (2.58)
T1 × [FdsFam = 2]		-1.004 (2.88)		0.159 (2.59)
T1 × [FdsFam = 3]		-1.562 (2.52)		-3.524 (2.14)
T2: Dilemma-related	0.537 (1.60)	0.613 (1.84)	1.418 (1.48)	0.412 (1.66)
T2 × [#FdsFam]	-0.085 (0.60)		-0.393 (0.61)	
T2 × [FdsFam = 1]		-0.012 (2.04)		3.088 (2.11)
T2 × [FdsFam = 2]		-0.617 (2.13)		1.394 (1.83)
T2 × [FdsFam = 3]		0.179 (1.94)		0.004 (1.93)
# FdsFam	0.363 (0.40)		0.543 (0.35)	
[FdsFam =1]		-0.616 (1.82)		-3.467 (1.91)
[FdsFam =2]		-0.379 (1.79)		-2.202 (1.47)
[FdsFam =3]		0.853 (1.34)		0.686 (1.12)
age	-0.003 (0.02)	-0.009 (0.02)	0.01 (0.02)	0.005 (0.02)
female (d)	0.219 (0.57)	0.257 (0.58)	-0.19 (0.62)	-0.238 (0.64)
schooling years	-0.075 (0.08)	-0.08 (0.08)	-0.015 (0.09)	-0.009 (0.09)
hectares	-0.013 (0.14)	0.025 (0.16)	-0.143 (0.13)	-0.138 (0.15)
bags yield	-0.034 (0.02)	-0.039 (0.02)	-0.052 (0.04)	-0.061 (0.04)
farmer (d)	1.749* (0.75)	1.999* (0.79)	1.297 (0.81)	1.532 (0.89)
migrant (d)	0.585 (0.69)	0.649 (0.73)	0.575 (0.71)	0.829 (0.84)
constant	4.819** (1.69)	5.182** (1.81)	4.259** (1.59)	5.556*** (1.58)
<i>exclude misunderstood</i>	<i>no</i>	<i>no</i>	<i>yes</i>	<i>yes</i>
var(e.contribute)	13.353*** (2.32)	13.102*** (2.28)	12.030*** (2.33)	11.475*** (2.23)
N	215	215	172	172

Tobit regression models with standard errors clustered on group (n = 4) level, censoring at the lower (0) and upper (10) endpoint for expectations, #FdsFam = number of friends and family members (in one's group), Expectation = one's expectations ('beliefs') of the other three group members' average contributions, Significance levels: * p<0.05, ** p<0.01, *** p<0.001

A.10 Trust

A.10.1: Figure trust



A.10.2 Regressions: trust

dep. var:	Expectation	Contribution	Trust
estimation method:	Tobit ^T	Tobit ^T	Oprobit
	coeff./(std. err.)	coeff./(std. err.)	coeff./(std. err.)
T1: unrelated			0.142 (0.21)
T2: dilemma-related			0.412 (0.30)
FdsFam			0.326*** (0.08)
trust	0.677* (0.30)	0.569 (0.40)	
constant	3.975*** (1.02)	2.513 (1.37)	
cut1			-1.015*** (0.25)
cut2			-0.446 (0.24)
cut3			0.326 (0.24)
var(e.contribute)		23.542*** (5.35)	
var(e.expectation)	14.117*** (2.97)		
N	171	171	171

^T Tobit models for contributions and expectation with standard errors clustered on group (n = 4) level, censoring at the lower (0) and upper (10) endpoint

#FdsFam = number of friends and family members (in one's group)

Expectation = one's expectations ('beliefs') of the other three group members' average contributions

Significance levels: * p<0.05, ** p<0.01, *** p<0.001

Supplement B: Experimental Protocols

B.1 Village meeting

[VILLAGE MEETING]

[freely presented by Christian, interpreted by Moses (assistant)]

To begin with, we would like to thank you all for coming here today. My name is Christian Hoenow. I am from the Marburg University in Germany. Together with the Ministry of Agriculture, Water and Forestry we are conducting research under the SASSCAL project. [NAME OF EXPERIMENTERS] are also part of the project.

Doing research means we are just here to collect data, but we do not bring any type of development project into the village. What you answer in the workshop will not have any impact on future projects.

Today we would like to conduct two small workshops with a certain number of people. During this workshop we will also ask you several questions. Unfortunately, not everyone from this village can participate since each workshop can only include a certain number of participants.

Since we want everyone to have the same chance to participate, we have prepared a bag with as many cards as people present. Each adult that is older than 18 years now will draw a card. We will ask you to fully concentrate on the workshop and we will be asking many questions. If you already know that you cannot attend for up to 5 hours, or do not wish to answer many questions, you should please not draw. Participation is, of course, voluntary!

- If you draw a red card, you will participate in the first workshop, which is conducted by Christian
- If you draw a blue card, you will participate in the second workshop, which is conducted by Adrian
- If you draw a white card, you unfortunately cannot participate in any of the events.

Do you have any questions?

[let every adult draw a card]

Now that everyone has drawn a card, we would like to ask all participants with a white card to leave the area. Thank you very much for attending the meeting.

Those who have drawn a blue card, please follow Adrian. He will right away start with the meeting.

[wait for everyone to leave except Christian's participants]

[continue with general instructions in each group]

[GENERAL INSTRUCTIONS]

To begin with, we would like to thank you all for coming here today.

We will conduct a workshop and at the end you will receive some payment for participating.

The money is not our private money, but it is provided by the German government.

All information collected today will be used for research only. Neither the government of Namibia, Germany nor any other organization will receive the data for other purposes. Also, neither your names nor any village-specific information will be linked to the results. All answers will remain anonymous to others.

The schedule for today looks as follows:

1. We will explain the procedure of the workshop.
2. We will conduct a small workshop.
3. After the games each of you answers a short questionnaire.
4. Finally, you will receive the money.

Before starting, I would like to give you some general information:

1. If at any time, you think that this is something that you do not wish to participate in for any reason, you are free to leave. You will however only receive a payment if you stay until the end of the workshop.
2. If you already know that you will not be able to stay for at least 5 hours, then you should leave right away.
3. We require your complete and undistracted attention. Please, follow the instructions carefully and do not use your phone or engage in any other distracting activity.
4. It is not allowed to talk to each other during the workshop, unless we tell you to. You can ask questions after raising your hand. If you talk to each other when you are not allowed to, you will be excluded from the workshop and the payments.
5. Everyone of you has received a unique ID card. Please keep this ID until the end. You must return the ID before receiving the money at the end of the workshop.

After knowing these rules, is there anybody who does not want to participate?

Do you have any questions?

[continue with treatment conditions]

[COMMUNICATION GAME INSTRUCTIONS]

We will now explain the procedure of the workshop. Please pay attention as for participating it is necessary that you understand everything. Also, we will later ask you questions individually to check whether you understood everything correctly. Each one of you will now receive an envelope that contains 10 Experimental Coins (EC). Each EC is worth 5 N\$. **[show coins]**. You will have to decide whether to contribute that money to a group account or not. What you put in the brown envelope is what you want to contribute to the group account, whereas what you wish to keep must be put in the white envelope **[show envelopes]**.

You can contribute any amount between 0 and 10 EC. The coins that you do not contribute are yours and you can keep them for sure. After the game we will change them for you: 5N\$ for every EC. [See graph with exchange rate] In total you can get between 25 and 125N\$, depending on your decisions and the decision of the others players in your group. You are playing the game with three other players, i.e., in groups of four.

- The number of ECs that were contributed to the group account are doubled. This doubled amount is then equally divided by all four players in the group.
- That means every player receives one fourth of the doubled group account.
- In total you will earn the ECs that you keep plus the share that you receive from the group account
- Note that the game is not about luck and not about being better than others. Everyone will receive exactly the amount as determined by the rules explained.

B.4 Examples

Example 1

[use poster and fill with example numbers]

4 players contribute half of their endowment to the group account.

- There are then $5 \times 4 = 20\text{EC}$ in the group account.
- The 20EC in the group account are then doubled ($20\text{EC} \times 2 = 40\text{EC}$) and divided equally to all 4 players.
- This means each player in the group receives $40\text{EC} / 4 = 10\text{EC}$ from the group account.

Each player then ends up with the amount that he/she kept, which is 5EC and the amount that he/she received from the group account, which is 10EC. In total it results in 15EC for all players.

Example 2

[use poster and fill with example numbers]

3 players contribute all of their ECs to the group account and 1 player does not contribute anything.

- There are then 30EC in the group account.
- The 30EC in the group account are then doubled ($30\text{EC} \times 2 = 60\text{EC}$) and divided equally to all 4 players.
- This means each player in the group receives $60\text{EC} / 4 = 15\text{EC}$ from the group account.

The one player that did not contribute receives 15EC from the group account plus 10EC that he/she kept for himself, which is 25 EC in total.

The three players that contributed everything receive 15 EC each from the group account.

Example 3
[use poster and fill with example numbers]

Imagine now that the one player also contributes. So, everyone contributes everything.

- Then the total contributions are $4 \times 10 = 40$. Multiplied by 2 = 80. 80 divided equally amongst all four players is 20EC for everyone.
- Then the three players get 20 instead of 15, and the one player who now also contributed also receives 20, instead of the 25 he/she would receive if NOT contributing.

B.5 Control questions for public use

We would now like to ask you a few questions to check if everybody understood:

[try to involve all participants]

1. If no one contributes anything, that means everyone keeps his/her initial ECs. Then how much does every player end up with?
[10]
 2. If everyone contributes all of his/her initial ECs, then how much does every player get? **[20]**
 3. Are the payoffs for everyone higher, lower or the same if all 4 players contribute 8EC, compared to when all players contribute 5EC?
[higher]
 4. If you do not contribute anything are your own payoffs higher, lower or the same compared to when you contribute?
[always higher]
 5. What is your payoff if you contribute all of your 10EC but no one else contributes anything?
[then only 10EC in the group account, $10 \times 2 = 20$, divided by four = 5ECs for everyone. Since you did not keep any of your initial EC, your final payoff is 5EC.]
 6. How much does everyone else receive in this case?
[keep 10 for themselves + get 5 from your contribution = 15]
 7. If you end up with 10 coins, how many N\$ will you get for that later?
[$5 \times 10 = 50$ N\$]
- Very good. Is there anything unclear about the rules or how the payoffs are calculated?
- Should we have another example?

B.6 Treatment conditions

[CONTROL: NO COMMUNICATION]

[read Game Instructions]

In the following game, we will divide you into groups of four. That means each of you is playing the game with three other players. The other groups are playing the same game, but what they do does not influence your group or your payoffs. They do just play the same game simultaneously.

[allocate groups]

[groups should sit together, but keep distance to avoid communication between groups]

This is the group you will be playing with. Remember that you are not allowed to talk to each other.

[then wait for 2 minutes in silence before starting the game]

[TREATMENT 1: UNRELATED COMMUNICATION]

[allocate groups]

[groups should sit together, but keep distance to avoid communication across groups]

We ask you to now please talk to each other for 5 minutes with your group members about how the different weather in this and in the last years affected the harvest. Also, think about which types of crops are doing good and which are doing bad in the different weather conditions.

[move away, wait 5 minutes, return to groups]

Please stop talking now as we are going to explain the rules of the game to you.

We will now explain what we are going to do in the workshop, you will be doing the workshop in groups of four, i.e., with the same 3 people that you just talked with.

The other groups are playing the same game, but what they do does not influence your group or your payoffs. They do just play the same game simultaneously.

[continue with game instructions]

[after reading out game rules and examples note this:]

your group is the group as allocated in the beginning during the discussions!

[TREATMENT 2: DILEMMA-RELATED COMMUNICATION]

We will now explain to you the rules of the game.

In the following game we will divide you into groups of four. That means each of you is playing the game with three other players. The other groups are playing the same game, but what they do does not influence your group or your payoffs. They do just play the same game simultaneously.

[read Game Instructions]

[allocate groups]

[groups should sit together, but keep distance to avoid communication across groups]

You are now allowed to talk to your group members for 5 minutes. After the 5 minutes you will make your decisions in private. You may talk about anything you like.

[move away, wait 5 minutes, return to groups]

Please stop talking now as we are going to start with the decisions.

[DECISION MAKING]

Your contribution will not be disclosed to the other participants. You will find out about the total contributions in your group at the end when we pay you, but no one will find out about how much other single players contributed. That means your own contribution is also anonymous to the other players. We will not disclose your decisions and you are under no obligation to tell anyone about how much you contributed.

In order to ensure anonymity in decision making, you will one-by-one come to the booth and make the decision there, in private. Please do not show other players how much you contributed, also not after you have made your contribution decision.

[show both envelopes and how to do it]

- Are there any questions about the procedure?

Before we start with the decisions, we would like to ask you two control questions, in order to check whether you have really understood the game. The answers you give here will not affect the money you earn; it is just for us as additional feedback information. [Assistant (me)] will ask you these questions, then you go directly to the booth and make your decision, then put the brown envelope, which contains your contribution to the group account into the box.

Please now come to the booth. We will call you one by one. Please remember to not talk to each other or communicate in any other way while waiting until everyone has made their decision. Also remember that there is no right or wrong in this game.

After the decision you may directly move to the snack area. There, you may talk again freely.

[one-by-one to assistant to answer two control questions in private, then to booth to make decision, in convenient order]

[have a break with snacks and cold drinks for everyone]

Thank you all for participating. You will now answer some short questionnaire and afterwards you will get the payments.

Supplement C: Survey Questions (Sheets made with Kobo-Toolbox: “https://www.kobotoolbox.org/”)

Post-experimental individual survey after PG game	
by_ enumerator ENUMERATOR <input type="radio"/> James <input type="radio"/> Moses <input type="radio"/> Zypriaan <input type="radio"/> Other SPECIFY OTHER.	
PLAYER ID <i>put the exact ID number here</i>	
TREATMENT GROUP <input type="radio"/> 1. Group Anonymity <input type="radio"/> 2. Identification <input type="radio"/> 3. Smalltalk <input type="radio"/> 4. Coordination	
PLAYER GENDER <input type="radio"/> Male <input type="radio"/> Female	
General AGE	
YEARS OF SCHOOLING (INCLUDING ALL SCHOOLS)	
SINGLE, MARRIED, DIVORCED, WIDOWED, PARTNERSHIP <input type="radio"/> Single <input type="radio"/> Married <input type="radio"/> Partnership <input type="radio"/> Divorced <input type="radio"/> Widowed	

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Post-experimental individual survey after PG game	
ARE YOU THE HEAD OF HOUSEHOLD? <input type="radio"/> Yes <input type="radio"/> No	
WHAT IS YOUR POSITION IN THE HOUSEHOLD? <input type="radio"/> wife <input type="radio"/> brother/sister <input type="radio"/> son/daughter <input type="radio"/> elder <input type="radio"/> other	
HOW MANY ADULTS LIVE IN YOUR HOUSEHOLD ? <i>Members of the same household: People who sleep and eat in the same place. Adult = 16 years and above</i>	
HOW MANY CHILDREN LIVE IN YOUR HOUSEHOLD? <i>child = 0 to 15 years</i>	
WHAT IS YOUR RELIGION? <input type="radio"/> Lutheran <input type="radio"/> Catholic <input type="radio"/> Traditional <input type="radio"/> Evangelic <input type="radio"/> None <input type="radio"/> Don't know <input type="radio"/> Refuse to answer <input type="radio"/> Other SPECIFY OTHER.	

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Post-experimental individual survey after PG game	
General Information	
<p>IS FARMING YOUR MAIN PROFESSION?</p> <p><input type="radio"/> Yes</p> <p><input type="radio"/> No</p>	*
<p>HOW MANY BAGS OF CROP YIELDS DO YOU NORMALLY PRODUCE PER YEAR? (1BAG = 50KG)</p> <p><i>number of bags (or add unit if interviewee wants to answer in NAD or other unit instead)</i></p>	*
<p>HOW MANY BAGS OF YOUR PRODUCTION DO YOU SELL? (1BAG = 50KG)</p> <p><i>number of bags (or add unit if interviewee wants to answer in NAD or other unit instead)</i></p>	*
<p>AND THAT IS HOW MUCH INCOME PER YEAR FROM SELLING FARMING YIELDS?</p> <p><i>income from the WHOLE year or season in NAD</i></p>	*
<p>DOES YOUR HOUSEHOLD RECEIVE ANY REMITTANCES FROM PEOPLE WORKING ELSEWHERE? (E.G. IN RUNDU)</p> <p><input type="radio"/> Yes</p> <p><input type="radio"/> No</p>	*
<p>IS YOUR HOUSEHOLD OR SOMEONE IN YOUR HOUSEHOLD RECEIVING ANY PENSIONS (E.G. OLD OR HANDICAPPED)</p> <p><input type="radio"/> Yes</p> <p><input type="radio"/> No</p>	*
<p>TOTAL INCOME INCLUDING EVERYTHING YEARLY? IN NAD</p> <p><i>Income from: farming, remittances, pensions and other income</i></p>	*
<p>WHICH OF THE FOLLOWING ASSETS DOES YOUR HOUSEHOLD OWN?</p> <p><input type="checkbox"/> electricity from powerline</p> <p><input type="checkbox"/> generator for electricity</p> <p><input type="checkbox"/> radio</p> <p><input type="checkbox"/> television</p> <p><input type="checkbox"/> refrigerator</p> <p><input type="checkbox"/> motorized vehicle</p> <p><input type="checkbox"/> bicycle</p> <p><input type="checkbox"/> phone</p>	*

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Post-experimental individual survey after PG game	
<p>WHAT MATERIAL IS YOUR HOUSE MADE OF?</p> <p><input type="radio"/> tent</p> <p><input type="radio"/> reed house (nsugo sonombu)</p> <p><input type="radio"/> timber and termite mud</p> <p><input type="radio"/> corrugated iron</p> <p><input type="radio"/> stone</p> <p><input type="radio"/> bricks</p>	*
<p>Agriculture</p> <p>IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WERE GOOD HARVEST YIELDS?</p> <p><input type="checkbox"/> 2007</p> <p><input type="checkbox"/> 2008</p> <p><input type="checkbox"/> 2009</p> <p><input type="checkbox"/> 2010</p> <p><input type="checkbox"/> 2011</p> <p><input type="checkbox"/> 2012</p> <p><input type="checkbox"/> 2013</p> <p><input type="checkbox"/> 2014</p> <p><input type="checkbox"/> 2015</p> <p><input type="checkbox"/> 2016</p> <p><input type="checkbox"/> 2017</p> <p><input type="checkbox"/> none</p> <p><input type="checkbox"/> don't know</p>	*

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<p>DO YOU HAVE ANY PLANS TO CHANGE LANDS OR EXPAND YOUR CULTIVATION AREA IN THE NEXT FIVE YEARS?</p> <p> <input type="radio"/> Yes <input type="radio"/> No </p>
<p>WOULD YOU SAY THERE IS SUFFICIENT LAND FOR EVERYONE?</p> <p> <input type="radio"/> Yes <input type="radio"/> No </p>
<p>DO YOU REMEMBER IN WHICH YEARS YOU CLEARED FOREST FOR A NEW FIELD?</p> <p> <input type="checkbox"/> none <input type="checkbox"/> 2000 <input type="checkbox"/> 2001 <input type="checkbox"/> 2002 <input type="checkbox"/> 2003 <input type="checkbox"/> 2004 <input type="checkbox"/> 2005 <input type="checkbox"/> 2006 <input type="checkbox"/> 2007 <input type="checkbox"/> 2008 <input type="checkbox"/> 2009 <input type="checkbox"/> 2010 <input type="checkbox"/> 2011 <input type="checkbox"/> 2012 <input type="checkbox"/> 2013 <input type="checkbox"/> 2014 <input type="checkbox"/> 2015 <input type="checkbox"/> 2016 <input type="checkbox"/> 2017 <input type="checkbox"/> never <input type="checkbox"/> don't know </p>
<p>HOW MUCH ON AVERAGE WHEN YOU DID SO PER YEAR?</p> <p><i>(surface in hectare or meters, add until, put 0 if you did not clear,</i></p>
<p>DO YOU NEED PERMISSION FOR CLEARING?</p> <p> <input type="radio"/> no <input type="radio"/> yes, from headman <input type="radio"/> other </p>

Post-experimental individual survey after PG game	
SPECIFY OTHER.	
* HAVE YOU BEEN DENIED PERMISSION TO CLEAR IN THE LAST YEARS? <input type="radio"/> Yes <input type="radio"/> No	
* DO YOU FEEL THERE IS SOME TYPE OF RIVALRY OR CONFLICTS IN ACQUISITION OF NEW LAND? <input type="radio"/> Yes <input type="radio"/> No	
* DO YOU FEEL SAFE AND SECURE ABOUT YOUR OWN LAND (TENURE)? <input type="radio"/> very safe <input type="radio"/> pretty safe <input type="radio"/> somewhat safe <input type="radio"/> worried <input type="radio"/> unsafe	
* DO YOU THINK YOU WILL STILL USE THE LAND YOU USE NOW IN 10 YEARS? <input type="radio"/> Yes <input type="radio"/> No	
Environment	
* HAS THERE BEEN ANY CHANGE IN THE WEATHER OVER THE LAST YEARS? <input type="checkbox"/> more rain <input type="checkbox"/> less rain <input type="checkbox"/> hotter <input type="checkbox"/> colder <input type="checkbox"/> no change <input type="checkbox"/> don't know	
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Post-experimental individual survey after PG game	
* IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WAS PARTICULARLY MUCH RAIN? <input type="checkbox"/> 2007 <input type="checkbox"/> 2008 <input type="checkbox"/> 2009 <input type="checkbox"/> 2010 <input type="checkbox"/> 2011 <input type="checkbox"/> 2012 <input type="checkbox"/> 2013 <input type="checkbox"/> 2014 <input type="checkbox"/> 2015 <input type="checkbox"/> 2016 <input type="checkbox"/> 2017 <input type="checkbox"/> none <input type="checkbox"/> don't know	
* IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WAS PARTICULARLY LITTLE RAIN? <input type="checkbox"/> 2007 <input type="checkbox"/> 2008 <input type="checkbox"/> 2009 <input type="checkbox"/> 2010 <input type="checkbox"/> 2011 <input type="checkbox"/> 2012 <input type="checkbox"/> 2013 <input type="checkbox"/> 2014 <input type="checkbox"/> 2015 <input type="checkbox"/> 2016 <input type="checkbox"/> 2017 <input type="checkbox"/> none <input type="checkbox"/> don't know	
* DO YOU THINK THE FOREST WILL STILL BE THERE AND ROUGHLY THE SAME SIZE IN 10 YEARS? <input type="radio"/> Yes <input type="radio"/> No	
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Post-experimental individual survey after PG game	
Extra	
<p>ARE YOU BORN IN THIS VILLAGE?</p> <p><input type="radio"/> Yes</p> <p><input type="radio"/> No</p>	*
<p>FOR HOW LONG HAVE YOU LIVED IN THIS VILLAGE?</p> <p><input type="radio"/> more than 20</p> <p><input type="radio"/> more than 10</p> <p><input type="radio"/> more than five</p> <p><input type="radio"/> less than five</p>	*
<p>WHERE DID YOU LIVE BEFORE? (OPTIONAL)</p> <p><input type="radio"/> neighbour village</p> <p><input type="radio"/> far away village in Kavango</p> <p><input type="radio"/> Rundu</p> <p><input type="radio"/> other part in Namibia</p> <p><input type="radio"/> Angola</p> <p><input type="radio"/> other country</p>	*
<p>WHY DID YOU COME HERE? (OPTIONAL)</p> <p><input type="checkbox"/> expectation of better farming conditions</p> <p><input type="checkbox"/> marriage</p> <p><input type="checkbox"/> relatives</p> <p><input type="checkbox"/> better education for the kids</p> <p><input type="checkbox"/> job</p> <p><input type="checkbox"/> Other</p> <p>SPECIFY OTHER.</p>	*
<p>DO YOU SOMETIMES THINK ABOUT MIGRATING TO ANOTHER PLACE?</p> <p><input type="radio"/> Yes</p> <p><input type="radio"/> No</p>	*
<p>WHERE WOULD YOU CONSIDER MOVING TO?</p> <p><input type="radio"/> another village</p> <p><input type="radio"/> Rundu</p> <p><input type="radio"/> another part of Namibia</p>	*

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Post-experimental individual survey after PG game	
<p>WHAT ARE THE REASONS FOR MIGRATING?</p> <p><input type="checkbox"/> expectation of better farming conditions</p> <p><input type="checkbox"/> marriage</p> <p><input type="checkbox"/> relatives</p> <p><input type="checkbox"/> better education for the kids</p> <p><input type="checkbox"/> job</p> <p><input type="checkbox"/> Other</p> <p>SPECIFY OTHER.</p>	*
<p>DO YOU PLAN TO OR HAVE YOU ALREADY TALKED TO OTHER PLAYERS ABOUT THE DECISIONS YOU MADE IN THE GAME?</p> <p><input type="radio"/> Yes</p> <p><input type="radio"/> No</p>	*
<p>IF YOU HAD TO DECIDE BETWEEN THE FOLLOWING TWO OPTIONS WHICH WOULD YOU PREFER? RECEIVING 1000 NAD FOR YOURSELF OR EVERY HOUSEHOLD IN THE VILLAGE RECEIVING 100NAD INCLUDING YOURSELF?</p> <p><input type="radio"/> 1000 NAD for yourself</p> <p><input type="radio"/> 100 NAD for everyone</p>	*
<p>IMAGINE A LADDER WITH 10 RUNGS. THE RICHEST PERSON IN THIS VILLAGE STANDS ON THE HIGHEST RUNG AND THE POOREST AND THE LOWEST. WHERE ON THAT LADDER DO YOU SEE YOURSELF?</p> <p><i>one is the poorest ten is the richest</i></p> <p><input type="radio"/> 10</p> <p><input type="radio"/> 9</p> <p><input type="radio"/> 8</p> <p><input type="radio"/> 7</p> <p><input type="radio"/> 6</p> <p><input type="radio"/> 5</p> <p><input type="radio"/> 4</p> <p><input type="radio"/> 3</p> <p><input type="radio"/> 2</p> <p><input type="radio"/> 1</p>	*
<p>DO YOU THINK, GENERALLY SPEAKING, MOST PEOPLE CAN BE TRUSTED OR THAT YOU NEED TO BE VERY CAREFUL IN DEALING WITH PEOPLE?</p> <p><input type="radio"/> Most people can be trusted</p> <p><input type="radio"/> Need to be very careful</p>	*

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Post-experimental individual survey after PG game

<div><div>*</div><div>DO YOU TRUST PEOPLE IN YOUR VILLAGE COMPLETELY, SOMEWHAT, NOT VERY MUCH OR NOT AT ALL?</div><div><div><input type="radio"/> completely</div><div><input type="radio"/> somewhat</div><div><input type="radio"/> not very much</div><div><input type="radio"/> not at all</div></div></div>	<div><div>*</div><div>DO YOU TRUST THE OTHER PLAYERS IN THE WORKSHOP OF TODAY COMPLETELY, SOMEWHAT, NOT VERY MUCH OR NOT AT ALL?</div><div><div><input type="radio"/> completely</div><div><input type="radio"/> somewhat</div><div><input type="radio"/> not very much</div><div><input type="radio"/> not at all</div></div></div>	<div><div>*</div><div>DO YOU TRUST THE OTHER THREE PLAYERS IN YOUR GROUP COMPLETELY, SOMEWHAT, NOT VERY MUCH OR NOT AT ALL?</div><div><div><input type="radio"/> completely</div><div><input type="radio"/> somewhat</div><div><input type="radio"/> not very much</div><div><input type="radio"/> not at all</div></div></div>
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Supplement D: Information for data and analysis script request

Dataset and script for the data preparation and analysis (“do-file”) can be made available upon request from the corresponding author. Game protocol and instruction are also available in the local languages spoken in Kavango, Namibia.