The sources of the communication gap

He, S.; Offerman, T.; van de Ven, J.

The Sources of the Communication Gap

Simin He, Theo Offerman, and Jeroen van de Ven*

April 2015

Abstract: Face-to-face communication drastically increases cooperation rates in social dilemmas. We test which factors are the most important drivers of this communication gap. We distinguish three main categories. First, communication may decrease social distance. Second, communication may enable subjects to assess their opponent's cooperativeness ("type recognition") and condition their own action on that information. Third, communication allows subjects to make promises, which create commitment for subjects who do not want to break a promise. We find that communication increases cooperation by 56 percentage points. Roughly 74% of this effect can be attributed to type recognition, the remaining 26% to a commitment value. We do not find evidence that social distance plays a role.

Keywords: communication, cooperation, prisoner's dilemma, social distance, type recognition, commitment

JEL codes: C72, C91

* Simin He: CREED, University of Amsterdam, s.he@uva.nl. Theo Offerman, CREED and Tinbergen Institute, University of Amsterdam, T.J.S.Offerman@uva.nl. Jeroen van de Ven (corresponding author): ACLE and Tinbergen Institute, University of Amsterdam, j.vandeven@uva.nl. We are very grateful to Harrie Beek, Aviva Heijmans, Joris Korenromp, and Jacco van Mourik who helped running the experiments and coding the communication. This paper benefited from comments by participants of the 2013 ABEE meeting (Amsterdam), the 1st IMEBESS conference (Oxford), the 2014 FUR meeting (Rotterdam), the 2014 CCC-meeting (Nottingham), the 2014 European Meeting of the ESA (Prague), and the 2015 Morality, Incentives and Unethical Behavior Conference (San Diego). Financial support from the Research Priority Area Behavioral Economics of the University of Amsterdam is gratefully acknowledged.

1 Introduction

Many people highly value face-to-face interactions. A survey among Forbes subscribers showed that the vast majority considers face-to-face meetings to be essential for negotiating contracts, closing deals, and building long-term relationships (Forbes, 2009). Many businessmen are willing to travel large distances to meet clients, thereby incurring considerable costs. They may be right to do so. Evidence from controlled laboratory studies shows that, compared to anonymous interactions, face-to-face communication has a major influence on people's behavior. Most notably, face-to-face communication drastically increases the cooperation rate in social dilemma games. On average, the option to meet and communicate with one another increases the cooperation rate by 40 percentage points (Sally, 1995; Balliet, 2009). We will refer to this as the "communication gap."

There are several reasons for why communication may have such a large impact. One possibility is that face-to-face interaction reduces the social distance between people, by making the other person identifiable (e.g., Bohnet and Frey, 1999; Hoffman et al., 1996). It may be harder to free ride if it has been clarified who will be harmed. Another reason is that the content of communication can induce a commitment to cooperate. In particular, making a promise can create a commitment value to cooperate for people who are averse to deceiving others (e.g., Ellingsen and Johannesson, 2004b; Charness and Dufwenberg, 2006; Vanberg, 2008).

The existing literature has mostly paid attention to social identification and commitment as explanations for the communication gap (e.g., Kerr and Kaufland-Gilliland, 1994). There is, however, an alternative explanation, which may be equally, if not more, important: type recognition. Face-to-face interactions can increase cooperation because it enables people to distinguish cooperative people from non-cooperative people. People often pay attention to the appearance and body language of someone else, looking for verbal and nonverbal cues that may reveal the intentions of the other person (Eckel and Petrie, 2011).[1] Recognizing others' types is especially important for conditional co-operators. They are willing to cooperate provided that they perceive the other to be cooperative as well. Interacting with an anonymous person may make them reluctant to cooperate. Since a very large fraction of

[1] These cues can be very subtle and brief. Ekman (2009) argues that micro expressions often display emotions that people try to conceal, and that spotting such expressions can help to recognize intentions.

people are conditionally cooperative (Fischbacher et al., 2001), it is plausible that type recognition is responsible for a substantial part of the communication gap.

The contribution of this paper is to decompose the communication gap, estimating the importance of each of the above three factors in a single design; social identification, type recognition, and commitment value.[2] We set up a laboratory experiment in which we let subjects play a social dilemma game. In the baseline treatment, subjects play this game without meeting each other before they make their decisions. In a second treatment, "Silent," subjects can identify the other before making their decision, but they are not allowed to communicate. A novelty of our experiment is to implement the "Restricted Communication" treatment, in which subjects are given time to interact face-to-face before they make their decisions, but without being allowed to make promises. Finally, in the "Unrestricted Communication" treatment, subjects are given time to freely interact face-to-face before they make their decisions. By comparing Baseline and Silent, we can isolate the effect of social identification. A comparison of Silent and Restricted enables us to measure the effect of type recognition. A comparison of Restricted and Unrestricted allows us to estimate the effect of commitment value, as well as any additional type recognition based on promises.

In line with the existing literature, we find that face-to-face communication drastically increases the cooperation rate. The individual cooperation rate in Baseline is 21 percent, while it is 77 percent in Unrestricted, creating a communication gap of 56 percentage points. The cooperation rate in Silent is only slightly higher than in the Baseline, and we therefore do not find that social identification is of any importance in our context. We do find an important role for promises, as the cooperation rate in Restricted is 43 percent, well below the cooperation rate in Unrestricted. Without any restrictions on the communication contents, a majority of subjects makes a promise to cooperate. Most people keep their promise, making it a reliable signal of cooperation. Thus, besides creating a commitment value, promises also facilitate type recognition. Based on the cooperation rates, and a further analysis of subjects' beliefs, we estimate that roughly 74 percent of the communication gap can be attributed to type recognition, and the remaining 26 percent to a commitment value.

[2] There might be other factors that explain the importance of communication in real life. For instance, incurring large travel costs to meet a client may act as a signal of the value that the person attaches to a meeting, and being present forces the other to pay attention to you. De Haan et al. (2015) provide evidence that messages are more credible when senders choose to incur a cost to communicate them, which is understood by receivers who anticipate that costly communication is more informative.

We also find evidence that subjects not only *believe* that they are able to predict their partners' decisions in the communication treatments, but are in fact to some extent able to do so. They use several cues that are correlated with behavior, and ignore some other cues that indeed lack predictive power. Interestingly, when we classify subjects as selfish or social on the basis of an independent test, we find that selfish people cooperate much less than social people in Baseline, but this difference is much smaller or even absent in the other treatments.

Existing work mostly focuses on the different factors in isolation. Studies that analyze the content of communication have found that promises can be a very reliable predictor of behavior (Ellingsen and Johannesson, 2004b; Charness and Dufwenberg, 2006; Belot et al., 2010; Van den Assem et al., 2012). Possible motives for keeping promises include guilt aversion, lying aversion, and shame (e.g., Gneezy, 2005; Charness and Dufwenberg, 2006; Vanberg, 2008; Miettinen and Suetens, 2008; Greenberg et al., 2014). Another related strand of literature studies people's ability to predict the behavior of others, i.e., people's ability to recognize types. Within the context of social dilemmas, it has been shown that people have some ability to recognize cooperators when there is a communication stage (Dawes et al., 1977; Frank et al., 1993; Brosig, 2002; Belot et al., 2012).[3] With the exception of Dawes et al., those studies do not make a comparison with a treatment without communication, and thus cannot establish the importance of type recognition for cooperation rates.[4] The focus of those studies is on people's actual ability to recognize types, while the behavior of people depends more on their own perceived ability to recognize types. We examine both subjects' actual and perceived ability to recognize types.

A few other studies have attempted to disentangle the determinants of the impact of communication. In an early study, Dawes et al. (1977) implemented different communication possibilities, including a treatment with restricted communication. Communication in that treatment was restricted to topics unrelated to the game. This does not only exclude making promises, but also other ways of signaling the intention to cooperate, such as discussing fairness norms, and also the intentional exchange of personal information that may be perceived as providing reliable cues, e.g., their study or hobbies. The advantage of our

[3] The ability to predict others' behavior above chance levels has now also been established in other games, such as the trust game and bargaining games. See, for instance, Van Leeuwen et al. (2014) for a list of studies.
[4] In psychology, social dilemmas are often studied with groups of four players or more. This makes the study of type recognition more complicated, as subjects will have to take into account the characteristics of all other group members in forming their beliefs.

approach is that we only exclude communication that is directly about intentions (see the design section).[5] Bohnet and Frey (1999) have no treatment with restricted communication, but they do have treatments with anonymity, mutual identification, and unrestricted communication. They attribute any difference between the mutual identification treatment and the anonymity treatment to a decrease in social distance. However, part of this effect may be driven by reputation concerns. In our experimental design, we control for this potential confound.

Finally, there is a stream of literature that studies communication in a variety of other games, including coordination games, cheap talk games, and hold-up problems (e.g., Brandts et al, 2012; Charness and Dufwenberg, 2011; Cooper et al., 1992; Ellingsen and Johannesson, 2004a, 2004b, amongst others). Communication in those studies is typically in the form of messages.

The rest of the paper is organized as follows. We describe the experimental setup in Section 2. Section 3 reports the results. Finally, Section 4 concludes.

2 Experimental Setup

2.1 Treatments design

Table 1 presents the monetary payoffs of the specific prisoner's dilemma that we use in the experiment. Each of the two subjects makes a choice between X (cooperate) and Y (defect). The game is played only once. The experiment consists of three parts. Subjects receive the instructions of later parts only after finishing a part. In part 1, they are informed that earnings in the remainder of the experiment are completely independent of the decisions in part 1.

[5] Other studies have used a restricted communication treatment that was similar to the one of Dawes et al. (1977). These studies find little impact of irrelevant communication on cooperation (Bouas and Komorita, 1996; Mulford et al, 2008; Bicchieri et al, 2010; Ismayilov and Potters, 2014). Frank et al. (1993) implemented a treatment in which promises were not allowed. In the latter study, it is not clear what counted as a promise and how this was enforced. They compared this to a treatment with unrestricted communication, in which they explicitly told subjects that they could make a (nonbinding) promise.

Table 1: Payoff Matrix

		Other's decision	
		X	Y
Your decision	X	8,8	0,12
	Y	12,0	4,4

Part 1 consists of a communication phase and a choice making stage. The communication stage is payoff-irrelevant. Our four treatments vary in the extent to which they allow subjects to communicate. In all treatments, subjects at some point meet the person with whom they are playing the game. In the Baseline treatment B, a short silent meeting of 10 seconds takes place *after* the subjects have chosen between X and Y. The other treatments allow subjects to communicate in varying degrees *before* they make a choice. In the Silent treatment S, subjects meet in silence for 10 seconds. In the silent meeting, subjects are not allowed to communicate in any way.[6] In the Restricted treatment R, subjects are allowed to talk face-to-face for two minutes. This communication is free form, except for the following restriction: subjects are not allowed to make a statement that would become a lie for any of the two choices. As an example, we inform subjects that they cannot promise to choose X, because that would become a lie if they would then choose Y instead. We also stress that no statement is allowed that *could* become a lie for any of the two choices, even if they are planning not to lie. In the Unrestricted treatment U, they are allowed to communicate with the other for two minutes without any restriction.

After subjects are informed of the specific game they are going to play, but before they have met the person with whom they are playing the game, subjects are asked to predict the likelihood that the other will choose option X, on a scale from 0 to 100 ("beliefs before").[7] Subjects are again asked to predict the choice of the other after they have met the other person and have made their decision ("beliefs after"). We chose not to incentivize these beliefs, because we felt that subjects would be intrinsically motivated to take the task seriously in such a short experiment. By incentivizing beliefs, we risked that subjects would

[6] The instructions of the Silent treatment were revised once. In early sessions, some subjects communicated by means of nonverbal signs (like making the sign "X" with their hands, to signal their intentions). After observing this, we adjusted the instructions by including an explicit statement that no type of communication was allowed. 3 sessions with 30 subjects were ran with the first version of the instructions, 8 sessions with 68 subjects were ran with the adjusted version. We excluded subject pairs who used nonverbal signs in the first version from our analysis.

[7] In the Baseline treatment, this question was asked before subjects made their choice between X and Y.

hedge across the payoffs of the different tasks. For a discussion of the circumstances under which incentivized beliefs are desirable, see Schlag et al. (2014).

A comparison of the Baseline and Silent treatments allows us to assess the effect of social identification, controlling for potential reputation or image concerns that our subjects may experience. That is, in the Baseline treatment subjects are aware that they will meet the other subject, just like in the Silent treatment. The comparison of the Silent and Restricted treatments enables us to measure a potential additional effect of type recognition and the comparison of the Restricted and Unrestricted treatments allows us to judge a further effect of type recognition as well as an incremental effect of commitment value. Table 2 provides a summary of treatments.

Table 2: Summary of treatments

Treatment (Label)	Timing of choice X,Y	Length of meeting	Communication Restrictions	# Subjects
Baseline (B)	Before meeting	10s	Silent	56
Silent (S)	After meeting	10s	Silent	98
Restricted (R)	After meeting	120s	No promises	100
Unrestricted (U)	After meeting	120s	None	80

Notes: In Restricted, any statement that would be a lie for any of the two choices was not allowed. The number in the last column is the total number of participated subjects before excluding subjects from the analysis (see Section 2.4).

Part 2 consists of two tasks. First, a version of the social value orientation test (e.g., Offerman et al., 1996) is conducted to acquire a measure of a subject's social preferences. We use the standard decomposed game method as developed by Griesinger and Livingston (1973) and Liebrand (1984). In this method, each subject makes 24 choices. In each of these choices, they choose between two 'own-other' payoff vectors. Each of these payoff vectors assigns a certain amount of money to the subject and another amount to another subject. The payoff vectors are located at 24 equally spaced points around a circle when mapped in a two-dimensional own-other payoff space. Based on the choices, a subject is classified as aggressive, competitive, individualistic, cooperative, or altruistic, following (e.g. Griesinger and Livingston, 1973; Liebrand, 1984). Because some categories only contain a few

observations, we group all subjects into two groups: *selfish* (aggressive, competitive, and individualistic subjects) and *social* (cooperative and altruistic subjects).

Second, risk attitudes are elicited using a similar task as Eckel and Grossman (2008), adding one option to capture risk-seeking behavior. In this method, a subject makes a choice among six 50-50 gambles that vary in the degree of risk and expected value. Gamble 1 is optimal for risk seeking subjects; gamble 2 for risk neutral subjects, and gambles 3-6 for subjects who are characterized by enhancing levels of risk aversion.

In part 3 of the experiment a questionnaire is administered which includes some questions on background information. Only at the very end of the experiment, the outcome of the prisoner's dilemma game is revealed. One of the two tasks of part 2 is randomly selected for payment, and this payment is added to the payment of part 1.

2.2 Procedures

The experiment was conducted at the University of Amsterdam. Dutch subjects were recruited from the CREED database. Subjects communicated in their native language. We ran 2-4 sessions each day and the experiment series lasted 12 days in total. Treatments were randomized at the session level. Each subject participated in one treatment only.

Upon arrival, each subject was directed to one of two separate rooms according to their random online recruitment assignment. Before the experiment started, subjects were informed that part of the experiment would be recorded on video, and that they could participate if they gave their consent or leave otherwise. The experiment only started when the two rooms had the same number of subjects. Each subject was randomly paired with one subject from the other room. Depending on show-up, the number of subjects per session varied between 4 and 12.[8] In each room, an experimenter read aloud the main instructions. The instructions are presented in Appendix A.

In the communication stage, subjects were called one by one into another room where they met the person with whom they were matched. They did not have the possibility to communicate whilst walking to the meeting room. The subjects were seated by the experimenter, and were then left to communicate by themselves. Afterwards, they returned

[8] There was one session with 4 subjects, and 5 sessions with 6 subjects. All other sessions were conducted with 8 to 12 subjects.

without further communication one by one to the rooms where they originally came from. An experimenter was always present in the L-shaped meeting room, but remained out of sight during the communication stage.

At the payment stage, each subject was paid in private in the meeting room. Subjects left the room one by one. The average payoff was €14.60, including a fixed show up fee of €6. The experiment took between 30 and 60 minutes, and was conducted with paper and pencil.

2.3 Coding of variables

Four research assistants independently coded the recorded conversations on several dimensions. Appendix B contains the list of the variables that were coded. To code whether or not a subject made a promise, we instructed the coders to use the same definition as was used in the instructions for the subjects (any statement that would be a lie for some choice is classified as a promise). Even though our coders tend to agree on their ratings, there are some inconsistencies. In 41 out of 74 cases (56%) all four coders agreed. In another 21 cases (28%), three out of four coders agreed. In 12 cases (16%) the coders were divided, with two out of four coders classifying the subjects' statement as a promise.[9] We classified promises into three categories: no promise (if at most one coder classified the statement as a promise), *weak promise* (if the coders were divided), and *strong promise* (if at least three coders classified the statement as a promise).

2.4 Observations and descriptive data

In total, we ran 38 sessions with 334 subjects. Subjects' choices are independent on the individual level in the Baseline treatment and on the pair level in the other treatments. Therefore, we allocated relatively fewer subjects to the Baseline treatment to balance the independent observations per treatment.

In the analysis, we exclude subjects who violated the instructions. In the Silent treatment, one pair of subjects talked to each other, and eight other pairs communicated by means of nonverbal signs. Another three pairs were excluded because they exchanged their identity cards, thereby creating an external commitment device. Regarding the Restricted treatment,

[9] Cronbach's alpha (a measure of inter-rater reliability) is 0.77, which is usually considered as acceptable.

we dropped 13 pairs who self-reported to have made promises in the communication phase. These self-reported results were consistent with independent coding results, suggesting that no more pairs violated the instructions. Finally, we excluded four more pairs because they reported to be friends.[10]

After excluding these subjects, we are left with 276 subjects. Some characteristics of these subjects are listed in Table 3. The Kruskal-Wallis test results show that the randomization procedure was successful on these characteristics.

Table 3: Descriptive statistics

Treatment:	B	S	R	U	*p*-value
Female, %	50.0	54.1	37.5	43.2	0.204
	(6.74)	(5.83)	(5.75)	(5.80)	
Age (years)	22.3	21.7	21.8	23.1	0.348
	(0.408)	(0.263)	(0.366)	(0.575)	
Attractiveness, 1-7	4.2	4.3	4.0	4.1	0.661
	(0.163)	(0.122)	(0.148)	(0.138)	
Risk-aversion, 1-6	3.5	3.2	3.4	3.0	0.267
	(0.202)	(0.169)	(0.186)	(0.165)	
Social types, %	26.7	19.7	27.8	31.1	0.469
	(5.97)	(4.76)	(5.32)	(5.42)	
N	56	74	72	74	

Notes: Standard errors in parentheses. In the last column, *p*-values refer to Kruskal-Wallis tests of equality-of-populations between treatments. The number of subjects refers to the number after excluding some subjects (see Section 2.4).

3 Results

In our analysis, cooperation rates and beliefs play an important role. We define the cooperation rate as the fraction of subjects who choose strategy X. We distinguish beliefs according to the moment at which they were elicited: beliefs elicited before the meeting are referred to as 'beliefs before' and beliefs elicited after the meeting as 'beliefs after.'

[10] In the questionnaire, subjects were asked to rate their relation with their partner *before* the experiment. One of the options was 'I consider the other person to be a friend'. If subjects stated to be friends, they were excluded from the analysis. Their payments were unaffected by the answers in the questionnaire.

Table 4 lists the cooperation rates and the beliefs in each treatment. Consistent with the existing evidence, adding free format face-to-face communication increases the cooperation rate substantially, in our case from 0.21 to 0.77.[11] To understand the sources of this increase, we now take a close look at the role of social identification, commitment value, and type recognition, respectively.

3.1 Social identification

Existing studies do not tease out the effects of social identification from reputation effects. The effect of reputation on pro-social behavior comes from the concern for future interactions. If there is a chance of future interaction (outside the experiment), subjects may behave nicer to the identifiable other to protect their own reputation. To avoid a confounding effect of social image, we allow our subjects to identify each other in both the Baseline and the Silent treatments. These two treatments only differ in the timing of the meeting – whether it is before or after choosing in the prisoner's dilemma. As a result, a difference in behavior between these two treatments can be attributed to social identification.

We do not find any role for social identification. The cooperation rate in the Silent treatment is not statistically different, and only slightly higher, from that in the Baseline treatment. There are also no systematic differences in reported beliefs between these two treatments. Social identification by itself does not induce cooperation, and this is understood by our subjects. This result differs from Bohnet and Frey's (1999) findings. They have documented a difference between a treatment with mutual identification and a treatment with anonymous interactions. Their design did not control for reputation effects: subjects in the anonymous treatments never identified each other, even not after making their decisions. Our results suggest that the effect they found is due to this reputation effect, and not to social identification.[12]

[11] Our results remain almost the same if we do not exclude the subjects for the various reasons mentioned in Section 2.4. Then, the cooperation rates are 0.21(B), 0.33(S), 0.40 (R) and 0.79 (U). If we exclude the subjects in treatment Silent before we revised the instructions, the cooperation rate in Silent is 0.25.

[12] Since their study is a classroom experiment, future interactions are likely and reputation effects are therefore plausible. Bohnet and Frey (1999) acknowledge this potential drawback of their design (see their footnote 8). It is also possible that some subjects knew each other, and that their treatment difference is caused by friendships.

Table 4: Cooperation rates and beliefs

Treatment:	B	S	R	U
Choices				
Cooperation rate (fraction choosing X)	0.21	0.24	0.43	0.77
Test equal to B		(0.812)	(0.020)	(<0.001)
Test equal to S			(0.029)	(<0.001)
Test equal to R				(<0.001)
Coordination rate (fractions)				
Both subjects in pair cooperate (X/X)	0.04	0.08	0.22	0.68
Both subjects in pair defect (Y/Y)	0.61	0.60	0.36	0.14
Expected coordination if choices are independent	0.67	0.64	0.51	0.65
Test actual = expected coordination (*p*-value)	(0.776)	(0.399)	(0.238)	(0.003)
Beliefs				
Beliefs before (1-100)	37.5	40.4	44.5	51.9
Test equal to B		(0.486)	(0.168)	(<0.001)
Test equal to S			(0.391)	(0.001)
Test equal to R				(0.059)
Beliefs after (1-100)	34.4	37.1	55.0	70.2
Test equal to B		(0.375)	(0.028)	(0.001)
Test equal to S			(0.149)	(0.001)
Test equal to R				(0.012)

Notes: (1) Statistical tests report *p*-values of two-sided Mann-Whitney tests (cooperation rates and beliefs) or Chi-square tests (coordination rates). For cooperation rates and beliefs after, the independent unit of observation is the mean over a pair of subjects.

3.2 Promises

Communication gives people the opportunity to convey their own intentions and to learn about the intentions of others. Amongst all kinds of statements to express intentions, promises are particularly powerful. A distinctive feature of promises is that they relate intentions directly to actions. Unlike other statements, promises may create commitment value to the promise-maker, and the partner may understand the commitment value and become more trusting as a result. For instance, if the promise-maker is lying averse, it will be costly for him to break his promise to cooperate. Of course, promises may also facilitate the process of recognizing types. If others perceive promises as a credible sign of cooperation, even when promises are not credible, they may decide to cooperate as well.

We find clear evidence for the positive effect of promises on the cooperation rate. Table 4 shows that the cooperation rate in the Restricted treatment, when promises are not allowed, is

well below the cooperation rate in the Unrestricted treatment. In addition, after communicating, subjects are much more optimistic about the behavior of their partner in the Unrestricted treatment. Interestingly, the positive effect of the possibility to make promises is already anticipated by subjects before communicating. At first glance, the option to make promises seems to account for a substantial part of the communication gap.

In the Unrestricted treatment, subjects eagerly use the possibility to make promises. Only 13 out of 74 subjects make no promises at all. Among the subjects who do make promises, 12 make weak promises and 49 strong promises. If promises create commitment value, one would expect that subjects who make promises are more likely to cooperate. This turns out to be the case. Among subjects who do not make promises, only three (23 percent) cooperate. By contrast, those who make promises cooperate much more often: 11 out of 12 (92 percent) for weak promises and 43 out of 49 (88 percent) for strong promises. Such large effects are consistent with findings of other studies (e.g. Ellingsen and Johannessen, 2004b; Charness and Dufwenberg, 2006; Belot et al., 2010; Van den Assem et al, 2012). Of course, these differences should not be interpreted as being necessarily causal; we did not exogenously vary whether or not a promise was made, but only the *option* to make promises.[13]

3.3 Commitment and type recognition

Identifying or communicating with the other subject can help people to assess the intentions of others. Conditional cooperators may adjust their behavior according to their assessments. To form these assessments, people may rely on cues such as promises and gender, attractiveness, etc.[14] However, since those cues may fail to predict the actual behavior, we make a distinction between perceived and actual cues. If in the communication process conditional cooperators perceive positive cues that the other will cooperate, they may have more confidence in the other and cooperate more. This process may be an important source for the positive effect of communication.

[13] An ingenious study by Ismayilov and Potters (2014) suggests the relationship is not causal. In a trust game setting, they exogenously vary whether a written promise is delivered to the matched subject. Even if subjects know that their message is not delivered, they tend to keep their promise. However, in a control treatment with irrelevant communication, they find similar levels of trustworthiness. They conclude that it is not promises per se that affects trustworthiness. Rather, trustworthy people are more likely to make a promise. By contrast, we do find a difference between Restricted and Unrestricted, suggesting at least some causality from promises on behavior.

[14] We measured attractiveness in the post experimental questionnaire. There, we asked the question: "How would you rate the beauty of the person with whom you were matched on a 1-7 scale (1 is ugly; 7 is beautiful)?"

A comparison between Silent and Restricted provides evidence that type recognition is important. The difference in cooperation is 19 percentage points. This cannot be attributed to commitment or social identification.[15] Type recognition can also play a role in other treatments. Because we collected data on beliefs, we can study the effect of type recognition in more detail. We present the analysis in two subsections. In Section 3.3.1, we check whether or not people change their belief on the basis of how they communicated; and if they do, how much it affects their own decision. We contrast the perceived ability to recognize types with the actual ability to recognize types, and we also investigate how beliefs map into decisions. In Section 3.3.2, we identify which part of the communication gap is due to commitment and which part to type recognition. In Section 3.3.3 we show which cues subjects use when they change their beliefs, and we compare them to the cues that actually predict when a partner will cooperate. In the analysis, we assume that beliefs have a causal impact on behavior. This is not necessarily the case. There can be omitted variables and the causality may be reversed. For instance, subjects who choose not to cooperate may report pessimistic beliefs to justify their behavior towards the experimenters or even themselves. We cannot rule out that this is the case, but some of our findings are inconsistent with reversed causality (see later) and there is some evidence from trust games suggesting that the problem is small (see Costa-Gomes et al., 2014).

[15] Studies that implemented a treatment in which subjects could only discuss game-irrelevant topics tend to find only a small impact compared to treatments with no communication. For instance, Dawes et al. (1977) find cooperation rates of 35% with restricted communication and 27% with no communication. As argued before, game-irrelevant topics exclude more than making promises, and we speculate that this may have hampered the recognition of types. In addition, even when the communication is about the game, allowing messages to be free-form may be essential. Charness and Dufwenberg (2010) find much less effect of communication when messages are more restricted. In a coordinated resistance game where two responders must jointly challenge the leader to prevent the exploitation of the victim, Cason and Mui (2015) compare a treatment with free-form messages and some treatments with messages containing only intended choices. They find that the possibility of free-form messages is critical for coordinated resistance. It allows people to communicate their social motivations. Brosig et al. (2003) provide further support that rich communication may be a prerequisite for a positive effect on cooperation. In a 4 player public good game, communication facilitates cooperation only if subjects can visually identify each other while communicating.

3.3.1 Changes in beliefs and behavior

Table 5 shows that subjects change their beliefs in all treatments. A revision of beliefs after meeting the matched partner suggests that subjects do rely on cues. In the treatments where verbal communication is allowed, the largest changes in beliefs are observed. The average absolute change in beliefs in those treatments is 27 percentage points, compared to 16 and 17 percentage points in the Baseline and Silent treatment, respectively. They are also more likely to change their beliefs with more than 10 percentage points when verbal communication is possible. In the communication treatments, 73% of the subjects change their beliefs with more than 10 percentage points, against 52% in the other treatments.

Table 5: Updating of beliefs

Treatment:	B	S	R	U
Average absolute change in beliefs	15.5	16.6	27.0	27.4
	(2.0)	(1.9)	(2.3)	(2.7)
Proportion of subjects with large changes in beliefs (%)	50.0	52.7	75.0	71.6

Notes: Change in beliefs is *beliefs after – beliefs before.* Standard errors in parentheses. A large change in beliefs occurs when a subject changes his or her beliefs by more than 10 percentage points in absolute terms.

If subjects are able to update their beliefs about each other in the right direction, one expects that the choices of their partners are positively correlated when communication is possible.[16] In the first row of Table 6, the correlation between choices is presented for each treatment separately. In the Baseline treatments subjects make their decisions prior to meeting the other, and indeed their choices are not correlated. In the Silent treatment, the correlation is small and not significantly different from zero. In the Restricted treatment, the correlation is

[16] A priori, this is not clear though. Belot et al. (2010) argue that altruism or efficiency concerns can result in a negative correlation. Since surplus is destroyed when no one cooperates, non-cooperative behavior becomes unattractive if an altruistic or efficiency minded subject expects the other to be non-cooperative. In an earlier working paper version, they analyse the game in a Bayesian framework with incomplete information about the cooperativeness of other people, and show that an increase in a subject's characteristic that makes the subject more likely to cooperate decreases the opponent's equilibrium probability of cooperating.

slightly stronger but still not significant. In the Unrestricted treatment, the correlation is sizable and significant.[17]

Table 6: Ability to recognize types

Treatment:	B	S	R	U
Correlation between choices in pairs	-0.06	0.12	0.15	0.47***
(*p*-value, Pearson's correlation coefficient)	(0.657)	(0.312)	(0.208)	(<0.001)
Accuracy of beliefs after meeting (α)	5.90	1.75	16.09*	36.54***
(*p*-value, Mann-Whitney test)	(0.738)	(0.865)	(0.066)	(<0.001)

Notes: *p*-values refer to statistical test that coefficient is zero. The accuracy of beliefs after meeting is defined by $\alpha \equiv \bar{p}^a(c) - \bar{p}^a(d)$, where $\bar{p}^a(c)$ is the average belief about others who cooperate and $\bar{p}^a(d)$ is the average belief about others who defect. * *p*-value <0.1, *** *p*-value <0.01.

It is possible that subjects are accurate when they change their beliefs, but nevertheless do not adjust their own decisions correspondingly. A selfish person, for instance, might never cooperate independent of his beliefs about the other. As a result, the correlation between choices may be an underestimate of the true ability to assess the intentions of others. We therefore examine if subjects' beliefs after meeting vary systematically with the actual choice of the other. Our measure of the accuracy of beliefs is given by:

$$\alpha = \bar{p}^a(c) - \bar{p}^a(d),$$

where $\bar{p}^a(c)$ is the average beliefs about others who cooperate and $\bar{p}^a(d)$ is the average beliefs about others who defect. The measure α varies between -100 and 100, where 100 would reflect a perfect ability to predict the other's choice, 0 would reflect random guessing, and -100 would reflect that people are completely wrong in telling the other's choice.

[17] Using data from game shows, both Belot et al. (2010) and Van den Assem et al. (2012) do not find evidence of a correlation in decisions, despite a free-format communication stage.

Values of α are presented in Table 6. We find evidence that subjects' beliefs have predictive value about the other's decision when they can communicate (Restricted and Unrestricted treatments), but not otherwise. In the Baseline and Silent treatments, the values of α are 5.9 and 1.75 respectively, and we cannot reject random guessing (Mann-Whitney test). In the Restricted treatment, α equals 16, and is significantly different from zero.[18] Finally, in the Unrestricted treatment, α equals 36, and is again significantly different from zero.[19]

There is another way to examine the accurateness of the reported beliefs. Figure 1 plots for each belief after meeting the corresponding cooperation rate of the other. The flat trend in the Baseline and Silent treatments provides further support for the claim that subjects are not able to accurately predict the behavior of the other there. There is some increasing trend in the Restricted treatment and a clear increasing trend in the Unrestricted treatment, illustrating that subjects predict better in these two treatments.

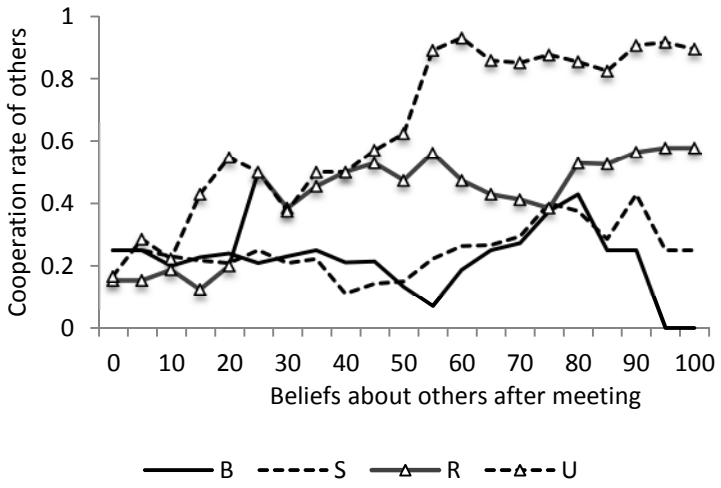

Fig. 1 Average cooperation rate of others by beliefs after meeting. The plotted lines are moving averages, where for each belief we compute the uniformly weighted average over all beliefs within a distance of 10 percentage points of that belief.

[18] Note that this result supports the idea that beliefs have a causal impact on behavior: we would not find a positive correlation between beliefs and the other's decision if beliefs would only reflect own intended behavior (reverse causality), or if some omitted variables would cause beliefs to differ between subjects.

[19] We find that same gender-match improves accuracy: α equals 55.2 for the same-gender pairs, and α equals 20.0 for the mixed-gender pairs. In a similar spirit, Coffman and Niehaus (2014) find that similarity between seller and buyer matters. In their paper, homophily facilitates persuasion by the sellers, who gain substantially more when the buyer has the same gender.

3.3.2 Identifying the effects of commitment and type recognition

Type recognition will only explain part of the communication gap if a change in beliefs translates into a change in the propensity to cooperate. Figure 2a plots for each treatment subjects' "reaction functions." These reaction functions plot the (population average) cooperation rate as a function of the subjects' beliefs that the paired subject will cooperate. In each treatment the reaction function is increasing, which reflects that subjects are more inclined to cooperate when they become more optimistic that the other cooperates. The cooperation rate is as low as 0 when the belief is very low and almost reaches 1 when the belief is rather high. This pattern in in agreement with the idea that subjects behave like conditional cooperators.

If communication creates a commitment value, this would manifest itself as an upward shift *of* the reaction function; given any beliefs about the intentions of the paired subject, subjects become more willing to cooperate. If, on the other hand, people change their beliefs as a consequence of type recognition, this would result in changes *along* the reaction function. The Baseline, Silent and Restricted treatments do not allow people to commit, and should therefore induce the same reaction function. Figure 2a shows that the reaction functions of these treatments are indeed remarkably similar. In contrast, the Unrestricted treatment has higher cooperation rates for any given belief, in agreement with the upward shift expected on the basis of the commitment value that it offers.

A positive effect of type recognition on the cooperation rate can manifest itself in two ways. First, if the communication process makes subjects on average more optimistic about the extent to which the other cooperated and the reaction function is upward sloping, the upward shift along the reaction function will translate to higher cooperation levels. Second, if communication makes subjects less uncertain about the extent to which the other cooperates and the (population average) reaction function is convex, type recognition will enhance cooperation even when the belief stays on average the same.[20]

[20] Hugh-Jones and Reinstein (2012) also discuss how information about others' types can increase cooperation rates. In their setup, people's types are revealed by their actions instead of the communication process.

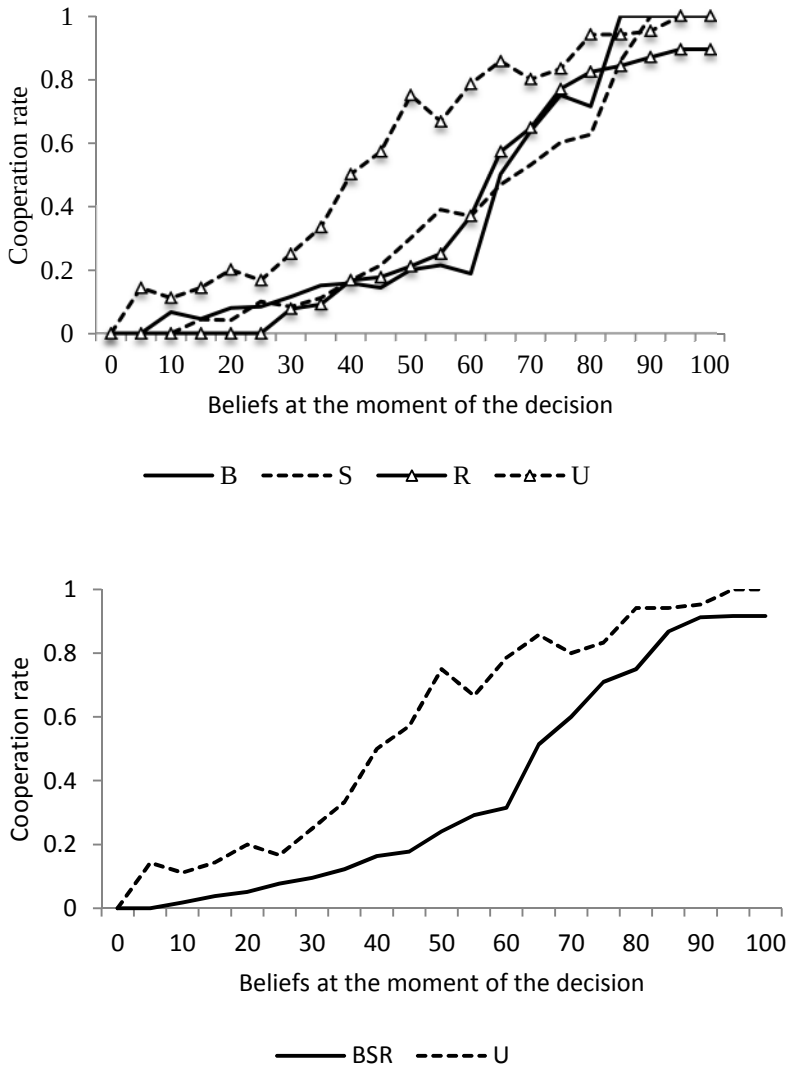

Fig. 2a (upper panel) and 2b (lower panel) Average cooperation rate of subjects as a function of beliefs at the moment of the decision. These are beliefs before meeting in Baseline and beliefs after meeting in the other treatments. B, S, R and U refer to the Baseline, Silent, Restricted and Unrestricted treatments, respectively. In the lower panel, the graph of BSR includes all subjects in treatments B, S and R. The plotted lines are moving averages, where for each belief we compute the uniformly weighted average over all beliefs within a distance of 10 percentage points of that belief.

Figure 2b shows the reaction function for people in Unrestricted and the average reaction function of the other treatments combined. We use this figure to identify the effects of commitment and type recognition. For every subject in Unrestricted, we estimate the hypothetical expected cooperation rate without commitment power using the subject's reported belief and the estimated reaction function of the treatments without commitment. This yields a predicted counterfactual cooperation rate of 62.4 percent. The difference with the actual cooperation rate in Unrestricted is 14.6 percentage points, which we attribute to the power to commit. A comparison of the Baseline and Unrestricted treatment shows that the total effect of allowing face-to-face communication is 56 percentage points. Therefore, the largest part of the effect of verbal face-to-face contribution is attributed to type recognition, which explains approximately 74% ((56-14.6)/56) of the gap. The rest of the communication is attributed to the commitment value that unrestricted communication offers.

3.3.3 Perceived and actual cues

To find out the role of *perceived* cues, we investigate the influence of observable cues on the beliefs that subjects report after the meeting. If subjects think that certain cues signal that their partners are going to cooperate, more optimistic beliefs are expected when such cues are present. We use OLS regressions to determine the effects of some observable cues.[21] The dependent variable is the belief reported after the meeting. Table 7 shows the independent variables together with the regression results.

In Table 7, the control variables include observable characteristics such as gender, age and attractiveness of the opponents. These features are easily observed when subjects meet. The regression results suggest that our subjects do not rely on any of these characteristics to update their beliefs. Another control variable is the promise that the partner may make, which only takes place in the Unrestricted treatment. Since promises are highly correlated within a pair, we cannot distinguish if a subject's own promise matters or that of the partner. We therefore construct the dummy variable Promise that is 1 if both subjects make a promise and 0 otherwise. We combine weak and strong promises, as their effect is nearly identical. A promise has a large and significant positive effect on the reported belief. We also include the

[21] We cluster the standard errors at the subject-pair level to account for interdependency within a pair. Estimations based on seemingly unrelated regressions yield similar results.

level of risk-aversion and the variable social as controls. Those are not directly observed by the subjects, but conceivably people can form an impression about someone's risk-attitude or social value orientation. We do not find that they influence beliefs though. Finally, we include the actual cooperation decision of the partner as a control variable. It has a positive effect on beliefs in the communication treatments, which suggests that our subjects in these treatments use some reliable cues that are not captured by any of the other control variables.

Table 7: Beliefs after and cues

	(1)	(2)	(3)	(4)	(5)
Treatment:	All	B	S	R	U
Opponent's characteristics:					
Female	1.213	-2.123	10.598	3.425	-3.660
	(4.163)	(8.463)	(6.528)	(10.976)	(6.039)
Attractiveness (1-7)	0.715	1.768	-0.374	0.401	0.456
	(1.632)	(2.944)	(3.559)	(4.000)	(3.036)
Risk-aversion (1-6)	-1.363	-2.698	-2.442	-0.809	1.390
	(1.399)	(3.123)	(3.385)	(3.128)	(1.811)
Social	-0.656	1.386	14.749	-0.376	-5.810
	(4.252)	(10.532)	(9.644)	(6.859)	(5.961)
Cooperates	25.701***	4.108	0.188	17.965*	20.013*
	(4.660)	(13.908)	(10.720)	(9.750)	(11.450)
Promise					34.297***
					(12.462)
Constant	40.822***	36.364**	38.418**	49.066**	24.006
	(8.135)	(17.481)	(15.218)	(18.204)	(18.060)
Observations	262	54	69	66	73
R-squared	0.156	0.046	0.065	0.084	0.433

Notes: OLS estimates. Dependent variable: Beliefs after (0-100). Robust standard errors in parentheses, clustered at the pair level. *** p<0.01, ** p<0.05, * p<0.1

To determine the *actual* cues that influence the choice to cooperate, we use OLS regressions in which the dependent variable is the choice to cooperate. In the regression, the choice equals 0 when the subject defects and 100 when the subject cooperates. The independent variables consist of both own characteristics and the partner's characteristics.[22] We do not include the risk-aversion and social type of the opponent, as in the previous analysis we did not find evidence that these variables affect beliefs.

[22] The other's characteristics are not included in the regressions for the Baseline treatment and the combined treatments. In the Baseline treatment, subjects decide before meeting the person with whom they play the game. Therefore, the subjects are not influenced by the other's characteristics at the moment of decision.

Table 8 shows the regression results. Some own observable characteristics affect the decisions. Women tend to cooperate more than men when subjects do not meet before the decision. The gender coefficient in Restricted and Unrestricted is still fairly large, but ceases to have a significant effect. In Silent, the gender coefficient is even negative, though also not significant. We find some surprising results for the variable social. Subjects who are classified as social are more likely to be cooperative, but the effect is much larger in the baseline than in the other treatments. It is also interesting to observe that risk-averse people are much less likely to cooperate than others in the baseline treatment, but the effect disappears in the other treatments. Possibly the communication process diminishes the perception that cooperation is a risky decision.[23]

We again include the variable promise as a joint characteristic (because we cannot distinguish the effect of a subject's own promise from the promise of the partner). A promise is a very reliable predictor of cooperation: when promises are made within a pair, subjects are 45 percentage points more likely to cooperate.

[23] The results from other studies that examine the impact of own and opponents' characteristics on cooperation in social dilemmas are somewhat mixed. For instance, both Belot et al. (2012) and Van den Assem et al. (2012) find that females are more likely to be cooperative, while Darai and Gratz (2013) do not find a significant gender effect. Belot et al. (2012) do not find evidence that a subject's own attractiveness or the other's attractiveness is predictive of cooperation, but Darai and Gratz find that the other's attractiveness increases cooperation in mixed gender pairs. We are not aware of any other studies that included risk-attitudes or social as controls.

Table 8: Cooperation and cues

	(1)	(2)	(3)	(4)	(5)
Treatment:	All	B	S	R	U
Own characteristics					
Female	6.195	23.556*	-13.803	10.834	12.995
	(6.998)	(12.275)	(9.878)	(15.864)	(10.987)
Attractiveness (1-7)	0.415	-1.560	-0.889	3.734	-0.016
	(2.751)	(3.685)	(6.221)	(7.846)	(3.878)
Risk-aversion (1-6)	-2.172	-10.942**	4.128	3.224	0.704
	(2.101)	(4.040)	(5.796)	(3.555)	(3.679)
Social	17.598**	40.935***	18.299	-9.021	13.798*
	(6.983)	(13.917)	(16.687)	(12.821)	(7.448)
Opponent's characteristics					
Female			0.199	16.387	-6.176
			(12.881)	(14.459)	(10.166)
Attractiveness (1-7)			-0.268	-5.622	2.035
			(6.288)	(6.048)	(4.198)
Cooperates			-3.909	21.185	24.583
			(19.586)	(19.683)	(21.086)
Joint characteristics					
Promise					44.995***
					(16.056)
Constant	41.169***	43.472**	21.291	22.760	3.485
	(13.782)	(20.283)	(37.796)	(36.348)	(35.918)
Observations	264	56	65	60	72
R-squared	0.028	0.259	0.067	0.124	0.399

Notes: OLS estimates. Dependent variable: Cooperates (0 or 100). Robust standard errors in parentheses, clustered at the pair level. *** p<0.01, ** p<0.05, * p<0.1

A striking result is that while selfish subjects are much less likely than social subjects to cooperate in the anonymous baseline treatment, selfish subjects catch up with social subjects in cooperation rate when they meet their partner. This cannot be explained by beliefs, as average beliefs are the same for both types in all treatments. This result is further illustrated in Table 9, which reports the correlation between own beliefs and own cooperation rates. Social types tend to behave as conditional cooperators in all treatments, and there is no noticeable trend when the possibilities for communication are enhanced. In contrast, for selfish types the correlation between own beliefs and own behavior is smaller in the Baseline treatment than in the other treatments. Selfish people behave in a more selfish way behind the veil of anonymity, but once the veil is lifted, they start responding to their beliefs like other

people. In the Unrestricted treatment, 86% of the selfish and 74% of the social subjects make a weak or strong promise. A promise has remarkable commitment power even for selfish types. They keep their promise in 84% of the cases, while social subjects always keep their promise.

Table 9: Correlation between cooperation rates and own beliefs by type and treatment

	B	S	R	U
Selfish types	0.41	0.62	0.71	0.62
	(0.002)	(<0.001)	(<0.001)	(<0.001)
Social types	0.56	0.67	0.77	0.66
	(0.022)	(0.018)	(<0.001)	(<0.001)

Notes: Correlations are Pearson's correlation. *p*-values testing coefficient equals zero in parentheses. Classification of selfish and social types based on social value orientation test.

Finally, we investigate if selfish and social types differ in their ability to judge the person with whom they play the game. Brosig (2002) hypothesizes that conditional cooperators should be better at identifying the other's willingness to cooperate, and finds that cooperative individuals are somewhat more accurate in their beliefs. Table 10 shows that selfish subjects predict slightly better than social subjects in the treatments without face-to-face communication. However, this result is reversed in the other two treatments. When face-to-face communication is present and selfish subjects start behaving more as social subjects, social subjects form more accurate beliefs.

Table 10 Ability to recognize types by own type

	B	S	R	U
Selfish	14.6	2.5	15.3	31.4***
	(0.28)	(0.85)	(0.20)	(0.01)
Social	-18.3	-1.6	21.8	51.9**
	(0.35)	(0.79)	(0.22)	(0.01)

Notes: Values of α by treatment and type. Types classified based on social value orientation test. Mann-Whitney tests to test equality of coefficients to zero.

*** p<0.01,** p<0.05, * p<0.1

4 Conclusion

Communication has a profound effect on how people behave in social dilemmas. In this study, we find that face-to-face communication enhances the cooperation rate from 21% to 77%. The main goal of our paper was to investigate why communication has such a strong effect on how people behave in social dilemmas. Promises stand out as a powerful predictor for both the belief that the other will cooperate and the actual decision to cooperate. In fact, the option to make promises not only creates commitment value, but also further facilitates the process of recognizing types. We find a very strong effect of type recognition, which we estimate to explain 74% of the communication gap. The remainder of the gap can be attributed to a commitment value. We find no evidence for a positive effect of social identification.

It is likely that type recognition also plays an important role in many other settings, including coordination games, competitions, and bargaining situations. In all these settings, people will assess the intentions and characteristics of others, and condition their own behavior on these assessments. Similarly, people's (perceived) ability to predict the intentions of others is not confined to face-to-face interactions. People also read cues in written messages (e.g., Charness and Dufwenberg, 2006; Chen and Houser, 2013) and different types of communication media yield different opportunities to assess other's intentions and consequently result in different cooperation rates (e.g., Brosig et al., 2003). We see it as an important avenue for future research to study the process of type recognition in such contexts.

References

Balliet, D. (2009). Communication and cooperation in social dilemmas: A meta-analytic review. *Journal of Conflict Resolution*, *54*(1), 39-57.

Belot, M., Bhaskar, V., & Van de Ven, J. (2010). Promises and cooperation: Evidence from a TV game show. *Journal of Economic Behavior & Organization*, *73*(3), 396-405.

Belot, M., Bhaskar, V., & Van de Ven, J. (2012). Can observers predict trustworthiness? *Review of Economics and Statistics*, *94*(1), 246-259.

Bicchieri, C., Lev-on, A., & Chavez, A. (2010). The medium or the message? Communication relevance and richness in trust games. *Synthese*, *176*(1), 125–147.

Bohnet, I., & Frey, B. S. (1999). The sound of silence in prisoner's dilemma and dictator games. *Journal of Economic Behavior & Organization*, *38*(1), 43-57.

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

Brandts, J., Charness, G., & Ellman, M. (2012). Let's talk: How communication affects contract design. *Available at SSRN 2112945.*

Brosig, J. (2002). Identifying cooperative behavior: some experimental results in a prisoner's dilemma game. *Journal of Economic Behavior & Organization*, *47*(3), 275-290.

Brosig, J., Weimann, J., & Ockenfels, A. (2003). The effect of communication media on cooperation. *German Economic Review*, *4*(2), 217-241.

Cason, T. N., & Mui, V. L. (2015). Rich communication, social motivations, and coordinated resistance against divide-and-conquer: A laboratory investigation. *European Journal of Political Economy*, *37*, 146-159.

Charness, G., & Dufwenberg, M. (2006). Promises and partnership. *Econometrica*, *74*(6), 1579-1601.

Charness, G., & Dufwenberg, M. (2010). Bare promises: An experiment. *Economics Letters*, *107*(2), 281-283.

Charness, G., & Dufwenberg, M. (2011). Participation. *American Economic Review*, *101*(4), 1211-1237.

Chen, J., & Houser, D. (2013). Promises and Lies: Can Observers Detect Deception. *Available at SSRN 2280211.*

Coffman, L., & Niehaus, P. (2014). Pathways of Persuasion. Mimeo.

Cooper, R., DeJong, D. V., Forsythe, R., & Ross, T. W. (1992). Communication in coordination games. *Quarterly Journal of Economics*, *107*(2), 739-771.

Costa-Gomes, M. A., Huck, S., & Weizsäcker, G. (2014). Beliefs and actions in the trust game: Creating instrumental variables to estimate the causal effect. *Games and Economic Behavior*, *88*, 298-309.

Darai, D., & Grätz, S. (2013). Attraction and cooperative behavior. *University of Zurich Department of Economics Working Paper*, (82).

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

De Haan, T., Offerman, T., & Sloof, R. (2015). Money talks? An experimental investigation of cheap talk and burned money. Forthcoming in *International Economic Review*.

Eckel, C. C., & Grossman, P. (2008). Forecasting risk attitudes: An experimental study using actual and forecast gamble choices. *Journal of Economic Behavior and Organization*, 68(1), 1–17.

Eckel, C. C., & Petrie, R. (2011). Face value. *American Economic Review*, *101*(4), 1497-1513.

Ekman, P. (2009). Telling Lies: Clues to Deceit in the Marketplace, Politics, and Marriage (Revised Edition). WW Norton & Company.

Ellingsen, T., & Johannesson, M. (2004a). Is There a Hold-up Problem? *Scandinavian Journal of Economics*, *106*(3), 475-494.

Ellingsen, T., & Johannesson, M. (2004b). Promises, Threats and Fairness. *Economic Journal*, *114*(495), 397-420.

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

Frank, R. H., Gilovich, T., & Regan, D. T. (1993). The evolution of one-shot cooperation: An experiment. *Ethology and Sociobiology*, *14*(4), 247-256.

Gneezy, U. (2005). Deception: The role of consequences. *American Economic Review*, *95*(1), 384-394.

Greenberg, A. E., Smeets, P., & Zhurakhovska, L. (2014). Lying, Guilt, and Shame. *Available at SSRN 2544349*.

Griesinger, D. W., & Livingston, J. W. (1973). Toward a model of interpersonal motivation in experimental games. *Behavioral science*, *18*(3), 173-188.

Hoffman, E., McCabe, K., & Smith, V. L. (1996). Social distance and other-regarding behavior in dictator games. *American Economic Review*, *86*(3), 653-660.

Hugh-Jones, D., & Reinstein, D. (2012). Anonymous rituals. *Journal of Economic Behavior & Organization*, *81*(2), 478-489.

Ismayilov, H., & Potters, J. (2014). Promises as commitments. *Available at SSRN 2209662*.

Kerr, N. L., & Kaufman-Gilliland, C. M. (1994). Communication, commitment, and cooperation in social dilemma. *Journal of Personality and Social Psychology*, *66*(3), 513.

Liebrand, W. B. (1984). The effect of social motives, communication and group size on behaviour in an N-person multi-stage mixed-motive game. *European Journal of Social Psychology*, *14*(3), 239-264.

Miettinen, T., & Suetens, S. (2008). Communication and Guilt in a Prisoner's Dilemma. *Journal of Conflict Resolution*, *52*(6), 945-960.

Mulford, M., Jackson, J., & Svedsäter, H. (2008). Encouraging Cooperation: Revisiting Solidarity and Commitment Effects in Prisoner's Dilemma Games. *Journal of Applied Social Psychology*, *38*(12), 2964-2989.

Offerman, T., Sonnemans, J., & Schram, A. (1996). Value orientations, expectations and voluntary contributions in public goods. *Economic Journal*, *106*(437), 817-845.

Sally, D. (1995). Conversation and Cooperation in Social Dilemmas: A Meta-Analysis of Experiments from 1958 to 1992. *Rationality and Society*, *7*(1), 58-92.

Schlag, K. H., Tremewan, J., & Van der Weele, J. J. (2014). A penny for your thoughts: a survey of methods for eliciting beliefs. Forthcoming in *Experimental Economics.*

Van den Assem, M. J., Van Dolder, D., & Thaler, R. H. (2012). Split or steal? Cooperative behavior when the stakes are large. *Management Science*, *58*(1), 2-20.

Van Leeuwen, B., Noussair, C. N., Offerman, T. J. S., Suetens, S., Van Veelen, M., & Van de Ven, J. (2014). Predictably Angry: Facial Cues Provide a Credible Signal of Destructive Behavior. *Available at SSRN 2535057*.

Vanberg, C. (2008). Why do people keep their promises? An experimental test of two explanations. *Econometrica*, *76*(6), 1467-1480.

Appendix A: Instructions (translated from Dutch)

Thank you for participating in today's experiment. Please read the following instructions carefully. At the start, you will receive €6 for your participation. During the experiment, you will have an opportunity to earn additional money, depending on your own choices and the choices of other participants. You will be paid in private at the end of the experiment.

Do not communicate with other participants during the experiment unless we explicitly give permission to do so. We also ask you to not use your mobile phones during the experiment. Always make sure to write your number on every answer sheet.

The experiment has three parts. What follows are the instructions for the first part. The instructions for the other parts will be distributed later.

Part 1 Instructions

There is an even number of participants in this experiment. We have divided the participants into two rooms. Each of the participants in a room will be randomly matched to one other participant in the other room.

In part 1, the number of rounds will be equal to the number of pairs that participate. In each round one pair of participants will make decisions. The number on your card shows in which round you will make a decision.

Each person will make a choice between X and Y. If you and the other person both choose X, each of you will receive 8 euros. If you choose Y and the other person chooses X, then you receive 12 euros and the other person receives nothing. If you choose X and the other person chooses Y, then you receive nothing and the other person receives 12 euros. If you and the other person both choose Y, each of you will receive 4 euros.

The possible decisions and earnings are also shown in the following matrix. In each cell of the matrix, the first number shows the amount of money in euro for you (in blue), and the second number shows the amount of money in euro for the other participant (in orange).

		The other participant	
		X	Y
You	X	8, 8	0, 12
	Y	12, 0	4, 4

You and the other participant will make your decisions simultaneously, without knowing what the other will decide.

Before you make your decision, we ask you to make a prediction about the choice of the other. We ask you how likely you think it is that the other person will choose X on a scale from 0 to 100. If you indicate 0, this means that you are sure that the other person **will not** choose X. If you indicate 100, this means that you are sure that the other person **will** choose X. If you indicate 50, it means that you don't know, and that you think it is equally likely that the other person will choose X or Y. You can indicate any number from 0 to 100 in steps of 1 (0 and 100 are admitted).

{RC}

In the round in which it is your turn to make a decision, we will call you and the participant from the other room to the meeting room. This happens after you made your prediction but before you make your decision between X and Y.

Once you are in the meeting room, you and the other participant have the opportunity to talk to each other for 2 minutes.

The communication is free format. **The only restriction is that you are not allowed to make any statement that could become a lie if afterwards you would make a choice that is not in accordance with that statement**. For instance, you are not allowed to say "I promise to choose X," because that would become a lie if you would then choose Y instead. **Note that the criterion is that the statement *could* become a lie, and even if you plan to act in accordance with your statement, such a statement is not allowed.**

The communication between you and the other participant will be recorded on video.

After these 2 minutes, you will come back to this room to make your choice between X and Y.

{FC}

In the round in which it is your turn to make a decision, we will call you and the participant from the other room to the meeting room. This happens after you made your prediction but before you make your decision between X and Y.

Once you are in the meeting room, you and the other participant have the opportunity to talk to each other for 2 minutes.

The communication is free format.

The communication between you and the other participant will be recorded on video.

After these 2 minutes, you will come back to this room to make your choice between X and Y.

{SC}

In the round in which it is your turn to make a decision, we will call you and the participant from the other room to meeting the room. This happens after you made your prediction but before you make your decision between X and Y.

Once you are in the meeting room, you and the other participant have the opportunity to see each other, **but you are not allowed to communicate with each other by any means**[24]. This part will be recorded on video.

After 10 seconds have passed, you will come back to this room to make your choice between X and Y.

{NC}

After you made your prediction, we ask you to decide between options X and Y.

In the round in which it is your turn to make a decision, we will call you and the participant from the other room to the meeting room. This happens after you made your prediction and after you made your decision between X and Y.

Once you are in the meeting room, you and the other participant have the opportunity to see each other, but you are not allowed to speak to each other. This part will be recorded on video.

After 10 seconds have passed, you will come back to this room.

[A diagram summarizing the steps was included.]

{End specific instructions}

You will make your decision between X and Y only once. When all participants have made their decisions and all participants have returned from the meeting room, the second part of the experiment will start in which we will ask you some additional questions. Your earnings in the second part will not depend on any decision made in the first part, and you will not be matched to the same participant.

At the end of the experiment, we will reveal the decision of the other participant to you, and pay you in private. Participants will be asked to leave the building one by one.

Please raise your hand if you have any questions.

Part 2 Instructions

In this part, you will make some choices for the following questions. **At the end of the experiment one of these questions will be randomly selected and your earnings will depend on your choice for that question.** Your earning in part 2 will be added to your earnings in part 1 and to your participation fee. After this part there is a final part where you will be asked to fill out a short questionnaire.

The payoffs in this part are in points, and every point is worth €0.10.

[24] This is the revised version. The old version is: Once you are in the meeting room, you and the other participant have the opportunity to see each other, but you are not allowed to speak to the other.

Part 2 Question 1

We show 24 items to you on the next page. For each item there are two options (A and B) specifying amounts of money for you and another person.

We ask you to indicate which option you prefer. Please indicate your preferred option for each of the 24 items. There is no right or wrong answer, it depends on your personal preferences.

Afterwards, we will randomly match you to a person in the other room. This will be another person than the person you met in the meeting room in part 1. This person faces the same decisions as you do. If this question is selected for payment, we will add the payoffs that correspond to the 24 choices made by you and we will do the same for the person with whom you are matched. You will get paid depending on your own choices as well as the choices of the other person.

	Option A		Option B		Your choice
Item	Amount Self	Amount Other	Amount Self	Amount Other	
1	+15,00	+0,00	+14,50	-3,90	
2	+14,50	-3,90	+13,00	-7,50	
3	+13,00	-7,50	+10,60	-10,60	
4	+10,60	-10,60	+7,50	-13,00	
5	+7,50	-13,00	+3,90	-14,50	
6	+3,90	-14,50	0,00	-15,00	
7	0,00	-15,00	-3,90	-14,50	
8	-3,90	-14,50	-7,50	-13,00	
9	-7,50	-13,00	-10,60	-10,60	
10	-10,60	-10,60	-13,00	-7,50	
11	-13,00	-7,50	-14,50	-3,90	
12	-14,50	-3,90	-15,00	0,00	
13	-15,00	0,00	-14,50	+3,90	
14	-14,50	+3,90	-13,00	+7,50	
15	-13,00	+7,50	-10,60	+10,60	
16	-10,60	+10,60	-7,50	+13,00	
17	-7,50	+13,00	-3,90	+14,50	
18	-3,90	+14,50	0,00	+15,00	
19	0,00	+15,00	+3,90	+14,50	
20	+3,90	+14,50	+7,50	+13,00	
21	+7,50	+13,00	+10,60	+10,60	
22	+10,60	+10,60	+13,00	+7,50	
23	+13,00	+7,50	+14,50	+3,90	
24	+14,50	+3,90	+15,00	0,00	

Part 2 Question 2

In the table below, we present six different options. You are asked to select one of the options.

Your earnings will depend on the outcome of a fair coin toss. Every option shows the amount in points you earn in case a head shows up or a tail shows up. The amount you can earn is always higher when heads shows up, except in option 6 in which you always earn 28 points.

If this question will be selected for payment, we will toss a coin and pay you according to the outcome of the toss and the choice you made.

Option	If the coin indicates	You earn in points
1	Head	66
	Tail	0
2	Head	60
	Tail	12
3	Head	52
	Tail	16
4	Head	44
	Tail	20
5	Head	36
	Tail	24
6	Head	28
	Tail	28

Please indicate which one of the six options above you prefer:

Part 2 Questionnaire

Finally, please take your time to answer the following questions. Your answers will remain confidential.

Age:

Gender (Male/Female):

Number of siblings (brothers and sisters):

Birth order (1 = you are eldest, 2 = you are second, etc.):

Country of birth:

Country (countries) of birth of grandparents:

Field of study (if applicable):

Did you know the other person with whom you were matched in Part 1 of the experiment (the person that you met in the meeting room? Please indicate below the extent to which you were familiar with the other person.

1: I did not know the other person	
2: I have seen the other person before, but never talked to her or him	
3: I have seen the other person before and talked to her or him, but I do not consider her or him to be a friend	
4: I consider the other person to be a friend	

If you feel that the above possibilities do not describe your relation with the other person, please describe it below in your own words.

How would you rate the attractiveness of the person with whom you were matched on a 1-7 scale (1 is very unattractive; 4 is average compared to the general Dutch population; 7 very attractive)? (If you do not remember or prefer not to answer, please leave this open.).

	1	2	3	4	5	6	7	
very unattractive								very attractive

{RC only}

Recall that for the communication phase you were instructed not to make a statement that would be a lie if afterwards you would make a choice that is not in accordance with that statement (for instance by making a promise).

Please indicate to what extent you agree with the two statements below (if you mark a 3 or 4, please also explain why).

The other participant complied with the request not to make such statements.

Your agreement:

1: completely agree	
2: agree	
3: disagree	

4: completely disagree	

If you marked a 3 or 4, please explain why you disagree:

You yourself complied with the request not to make such statements.

Your agreement:

1: completely agree	
2: agree	
3: disagree	
4: completely disagree	

If you marked a 3 or 4, please explain why you disagree:

{End specific part RC only}

{FC only}

Did the person with whom you communicated make a statement that would be a lie if afterwards he or she made a choice that was not in accordance with that statement (for instance by making a promise).

Please indicate to what extent you agree with the two statements below.

The other participant made such statements (such as promises).

Your agreement:

1: completely agree	
2: agree	
3: disagree	
4: completely disagree	

Did you yourself make such statements (such as promises).

Your agreement:

1: completely agree	
2: agree	
3: disagree	
4: completely disagree	

{End specific part FC only}

Have you heard about this particular experiment before taking part today? If so, what did you hear?

Can you tell us how you decided between options X and Y in part 1?

Do you have any other remarks about the experiment?

Appendix B: Coding of Variables

Four research assistants independently coded the recorded conversations on the variables listed below. Each participant was recorded on one video camera. Each research assistant watched only one of the videos. The variable related to the ethnicity of the participant was therefore only visible to two of the four research assistants. To code promises, we instructed the coders to use the same rule that we used in the instructions for the participants (any statement that would be a lie for one of the choices is classified as a promise).

Promise:	1 if participant makes a simple promise 2 if participant makes a conditional promise (if you … then I also) 3 if participant makes a promise to defect 0 otherwise
Firstpromise:	1 if the participant was the first to make a promise 0 otherwise
Rollcall:	1 if they ended with both making a promise 0 otherwise
Threat:	1 If participant makes a threat 2 if the participant makes a positive threat (reference to the future in a positive way) 0 otherwise
Trust:	1 if the participant says that (s) he trusts the other 0 otherwise
Structure:	1 if the participants makes a remark about structure of the game or explains game 0 otherwise
Risk:	1 if the participants makes a remark about risk 0 otherwise
Justice:	1 if the participant make some reference to morality, such as "it is the fair thing to do" 0 otherwise
Caring:	1 Stating that they care about the payoff of the other (such as "I like you to make money (as well)" , "We both deserve to make money") 0 No statement of that kind
Askstudy :	1 if the participants inquires about the study of the other 0 otherwise
Smalltalk :	1 if the participants engages in other chitchat (not related to the game) 0 otherwise

NonverbalAgreementSign: 1 if they shake hands (or something else) at the beginning
2 if they shake hands (or something else) later in the communication
0 otherwise

Laughter: 1 if laughs out loud
0 otherwise

Ethnicminor: 1 if from ethnic minority (non-white)
0 otherwise

Problem: 1 at least one did not in accordance with instructions
0 otherwise

ExplanationProblem: if problem, briefly explain it in words