SAPIENZA
UNIVERSITÀ EDITRICE

ANNALI DEL DIPARTIMENTO DI METODI E MODELLI PER L'ECONOMIA IL TERRITORIO E LA FINANZA

2018

Sapienza Università Editrice
Piazzale Aldo Moro 5 – 00185 Roma

www.editricesapienza.it
editrice.sapienza@uniroma1.it

Iscrizione Registro Operatori Comunicazione n. 11420

ISSN: 2385-0825

Pubblicato a novembre 2018

Di Bartolomeo Giovanni*, Papa Stefano*

MISCOMMUNICATION IN AN INVESTMENT GAME+

Abstract. To varying degrees, empirical evidence shows that non-binding messages (cheap talk) are effective in enhancing cooperative behaviors when simple games are augmented with a communication pre-stage. We focus on games where the interactions are more complex and test the effect of communication. Specifically, we consider an investment game, where players can send free-form messages before taking their choices. Differently from simple games, where subjects have just to choose whether to cooperate or not, we find that messages do not matter—on average. Our rationale is that misunderstandings are more likely to be observed in a context where the space of strategies is larger.

Keywords: empty talk, investment game, promises, reciprocity, requests, trust.

1. Introduction

We aim to test the effect of communication in a laboratory experiment where outcomes are not trivial, and players have conflicting interests. Communication consists of free-form, non-binding messages sent before playing the game. Our idea is that, unlike other contexts, coordination through messages is more difficult here because, on the one hand, every player has an interest in being misunderstood and, on the other hand, misunderstanding is more likely, given the complex structure of possible actions.

In coordination games, to varying degrees, several experimental studies have found positive evidence that cheap talk is successful in achieving the Pareto-superior equilibrium. For instance, in two-person coordination games, Cooper et al., (1989; 1992) and Charness (2000) report that one-way communication increases the play of the Pareto-dominant equilibrium relative to the no communication baseline.[1] There is, however, less knowledge on its effects when risk dominance and payoff dominance conflict are present as in trust and investment games.

The effects of one-way messages have been investigated in different reduced forms of simple trust games, leading to mixed evidence. Charness and Dufwenberg (2006) and Vanberg (2008) found that promises enhance trust, cooperation, and efficiency.[2] By contrast, Charness and Dufwenberg (2010) found that one-way precompiled messages have no effect trust and cooperation.[3]

The above studies are usually based on binary choices (e.g., *In*/*Out*, *Roll*/*Don't Roll*) as the game described in Figure 1. In the games with binary choices, potential alternative outcomes of their actions are easily predicted by the players and the terms of potential (implicit or explicit) informal agreements easily stated or bargained. For instance, augmenting the game in Figure 1 with communication often leads to messages where B promises A to *Roll* (asking for *In*). In a two-player random dictator game, where the dictator (B) can take all the pot or split it, the outcome of bilateral communication usually is an informal agreement according to which each player agrees to split in the case the role of dictator is assigned to her/him.

* Sapienza University of Rome

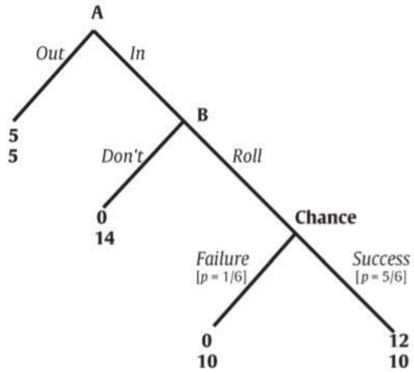
+The Authors are grateful to two anonymous referees, Parisio Di Giovanni, Maurizio Franzini, Ersilia Incelli, Valentina Meliciani, William S. Neilson, Thomas R. Palfrey, and Francesco Passarelli for useful comments on previous drafts.

[1] See also van Huyck et al. (1990), Blume and Ortmann (2007), Pogrebna and Blavatsky (2009), He et al. (2016) for similar results.

[2] See also Vanberg (2008) and Di Bartolomeo et al. (2018) to distinguish the underlying motivations of promise keeping.

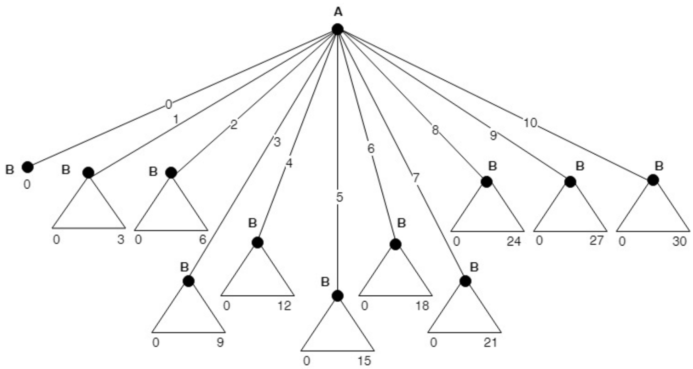
[3] See also Wilson and Sell (1997), Duffy and Feltovich (2002), Ellingsen and Johannesson (2004), Bochet et al. (2006), Bracht and Feltovich (2009), and Ridgon (2009).

Figure 1 – Binary-choice trust game (Charness and Dufwenberg, 2006)



We aim to test whether unilateral messages are effective in a more complex setup, where actions are not binary. Specifically, we consider an investment game *à la* Berg et al., (1995) and we investigate the effects of free-form one-way messages on the average amounts sent by subjects who trust and cooperate. The game is described in Figure 2.

Figure 2 – Investment game (Berg et al. 1995)



The figure reports actions only. Endowed with 10, Player A (investor) can send an amount between 0 and 10 to B. Player B (trustee) can then send back an amount between 0 and the amount received multiplied by 3. Payoffs follow. The space of strategies and the potential outcomes are clearly larger and more complex compared to the game described in Figure 1.

In contexts like that described in Figure 2, Ben-Ner et al. (2011) found that trust and trustworthiness are greater when written numerical messages are used than when numerical communication only or no-communication is assumed. In the same context, Ben-Ner and Putterman (2009) found that cooperation also increases with the number of interactions (one-side vs. two-side messages). The highest level of cooperation was found when chat is used for communication. Similarly, Bochet et al. (2006) found higher public goods contributions under a “chat room” (no restriction on messages) treatment than when only numerical messages were possible. In these games, it seems that more communication or more clear communications enhance cooperation.

Augmenting the game described in Figure 2 with pre-play communication, we find that messages do not support trust and cooperation. Messages are ineffective, no effect is detected on average when treatments with and without communication are compared. Our intuition is that the negative result depends on misunderstandings rather than by the fact that the messages are ignored as they are not binding. We further explore the communication by classifying the messages by type. Also, in this case, different kinds of messages do not lead to different effects on trusting behavior, while we find some difference in the trustworthiness behavior. In this case, our result is in line with Chen and Houser (2017). By using a mistress game, they find that the investors/receivers are unable to distinguish between messages that will be honored from those that will not have honored by senders. Like them, we found that people sent certain kinds of messages to cheat the trustors and they succeeded in doing so because once they received trust, they do not reciprocate.[4]

The rest of the paper is structured as follows. Section 2 describes experiment design and procedures. Section 3 illustrates and discusses our main results. Section 4 concludes.

2. The experiment

2.1 Design

We perform two treatments based on an investment game (Berg et al. 1995). The first treatment (TC) is a standard two-stage investment game; the second (TM) is a variant where pre-play communication is allowed. By comparing TC (control) to TM, we investigate the effects of cheap talk.

Investment games involve two players (A and B). For the sake of brevity, we refer to A as "she" and B as "he". In both treatments, A and B are initially endowed with 10 tokens. The two treatments can be then formally defined as follows.

Definition (TC). Subjects interact in two stages during which they can increase or decrease their initial endowments depending on their choices. In the first stage, A can transfer part, all or none of her endowment (i.e., 0 to 10 tokens) to B. Before being delivered to B, any amount transferred is multiplied by 3. In the second stage, B could transfer part, all or none of all the tokens received from A. The subjects' payoffs are the initial endowments plus the tokens received minus those sent.

Definition (TM). In the first stage subject B can send a free-form message to A; the message is not binding. Stage two and three are the same as stage one and two of TC.

In the above setup, we can then define trust and reciprocity.

Definition (trust). When A transfers some tokens to B, she trusts him.

Definition (reciprocity). When B sends back an amount greater than what he received by A, he reciprocates.

In both treatments, if all the agents are selfish, the sub-game perfect Nash equilibrium implies that nothing is sent. The proof is trivial. However, experimental evidence usually documents different results

[4] The experimental literature on deception games include several papers. Gneezy (2005), e.g., study the role of communication in social relationships that often implies deception. Sanchez-Pages and Vorsatz (2007) show links between a subject's willingness to punish lies of others and their aversion to lying. Ederer and Fehr (2007) study the impacts of deception and aversion to lying. Sutter (2009) identifies sophisticated deception. Rode (2010) analyzes the effect of communication in a competitive and non-competitive (cooperative) context. He finds that people trust less in the competitive framework if communication is not explicit about the payoffs. Finally, by introducing the idea of guilt aversion, Charness and Dufwenberg (2006) provide an alternative interpretation of Gneezy's (2005) findings.

since people have other regarding preferences. Therefore, we expect that subjects will transfer some tokens due to conditional or unconditional motivations.[5]

By comparing the outcomes of two treatments (TM vs. TC), we study the effects of cheap talk on trust and reciprocity. Our assumption is that the extent of conditional or unconditional motivations is affected by the context—in particular, by pre-play communication. Trust and reciprocity are then investigated by looking at the averages sent by A and B subjects in TM and TC, respectively.

Referring to *a* and *b* as the average amounts respectively sent by A and B subjects, we test the following hypothesis:

H1: Announcement effect $a_{TM} - a_{TC} > 0$

where the subscript indicates the treatment. As $a_{TM} - a_{TC}$ is a measure of the announcement effect, if $a_{TM} - a_{TC} = 0$, there is not announcement effect on average trusting behavior.

As Cox (2004) and Cox et al., (2008) pointed out, tests for reciprocity should use a regression approach that is conditional on the amounts received by Bs. We estimate following Tobit regression:

H2: Reciprocity $b_i = \alpha + \beta D_i a_i + \gamma a_i + \varepsilon_i$

where *D* is a dummy which assumes values equal to 1 (0) when the observation *i* refer to TM (TC); a_i and b_i are the amounts sent by A and B of paired *i*.

The bounds for the Tobit estimation are those imposed by the experiment design 0 and $3a_i$. Following Cox (2004), we also take account for multiplicative heteroskedasticity. We estimate θ from $\sigma_i = \sigma e^{\theta a i}$. The coefficient β measures reciprocity (β>0). The null hypothesis is that β is equal to zero.

Similar to Charness and Dufwenberg (2006) or Vanberg (2008), we also distinguish messages according to their contents and investigate their specific effects on trust and reciprocity.

Messages were evaluated by research assistants. Classification is clearly subjective, and some messages were on boundary between different classes. Nevertheless, the overall pattern is robust to alternative classifications.[6] We consider three kinds of messages:

i) Promise (P). The trustee makes a promise to reciprocate to the investor.
ii) Request (R). The trustee asks the investor for something, such as a favor or a courtesy.
iii) Empty (E). The trustee sends a message that contains neither promise nor request, it simply attempts to achieve success or sympathy from investor.

We test whether the average of amounts sent for trust and reciprocity is different, once we controlled for the kind of message received. Regarding trust, we explore the announcement effects between the message of the same kind, i.e.,

H3: Announcement effect $a_i \neq a_{TC}$ $i \in \{P, R, E\}$

where the subscript *i* individuates the average amount sent conditional to the kind of message. We do not assume any ex ante direction in the difference between the message of kind *i* and the control group averages.

As above, reciprocity is explored on individual data by using three Tobit regressions indexed by $z \in \{P, R, E\}$. We estimate:

H4: Reciprocity $b_i(z) = \alpha + \beta D(z) a_i(z) + \gamma a_i(z) + \varepsilon_i$

[5] We do not distinguish other-regarding preferences from conditional to unconditional. To disentangle conditional and unconditional motivations further treatments (appropriate dictator games or methodologies) should be introduced (Di Bartolomeo and Papa, 2016).

[6] We use the classification of one research assistant randomly chosen before the experiment. However, results are robust when other researchers' classifications or averages are used. Results are available upon request.

where D is a dummy that takes value one when the observation i refers to the type of message z under scrutiny, while when it refers to an observation from the control group, it is set at zero.

The coefficient β measures the effect of type of unilateral communication, i.e., promises, requests, or empty talk compared to the associated control group. The null hypothesis is that β is equal to zero.

2.2 Procedures

The experiment was conducted in 2012 at the Sapienza University of Rome and the University of Teramo. Participants were undergraduate students recruited by e-mail using lists of voluntary potential candidates.[7] Subjects were randomly selected from the database. In each university, we ran two sessions for each treatment. Overall, 296 subjects participate the experiment.

Regarding the incentive system, all participants knew that they would be paid according to monetary payment or to a between-subject random-lottery incentive system.[8] The payment mode in the session was randomly drawn (50% probability for each) before the experiment and revealed at the end. In the case of monetary payment, the value of each token was equal to 0.50 euro; otherwise, the value of each token was equal to a ticket for a lottery that was rewarded with four electronic MP3 devices (40 euros each) for each session.[9]

At the beginning of each session, subjects were required to provide identification cards. A database with name verified that there was no repeat participation. Then all the participants were divided in two groups (A and B) and placed in two different rooms by a random sampling. Each subject A was matched to a subject B in a random and anonymous way.

All the decisions made during the experiment were anonymous. Anonymity was guaranteed by using identification codes; names remained unknown to all – including experimenter and controllers.[10] During the experiment, two controllers checked that the instructions were correctly followed by participants. We used a standard double-blind procedure. Using their codes, participants were paid by an administration office located in a separate building (the central administration of the university), which a payment summary from the examiners by email. Participants knew that officers were unaware of the details for the reason for the payments or anything concerning the experiment.

3. Results

Our results from TM and TC are reported in Table 1. The table displays the average amounts sent in the investment games. In the first row, we report the average amount sent by investors present that the average amount sent by trustees is 4.66 and 4.59 tokens in TM and TC, respectively. Standard deviations are reported in square brackets. The last two columns report the mean and median difference and one-tailed *p*-value (in parentheses) associated with a *t*-test and non-parametric tests (Mann-Whitney and Wilcoxon) based on the independent sample assumption, respectively.

The amount sent in TM and in TC is 4.43 and 4.20 tokens, respectively. Therefore, Table 1 provides evidence for the announcement effect, but its magnitude is small. The difference between the average amounts sent in TM and in TC is not significantly different from zero—as indicated by the tests. As a result, H1 must be rejected. Pre-play communication does not seem to play a role on the amount sent by investors.

---

[7] Lists were compiled in advance by advertisements placed on the universities notice boards and by using the universities mailing lists.

[8] In dynamic-multi-task environments, these lottery incentive systems might induce some biases. However, in a single task context, it performs well. For a detailed discussion, see Starmer and Sudgen (1991), Beattie and Loomes (1997), Cubitt et al. (1998), Baltussen et al. (2012).

[9] Participants who were not selected for the reward earned nothing.

[10] Each subject does not know the subject s/he is paired with-neither during nor after the experiment.

Table 1. Experiment results

	Sent TM (74 obs.)	Sent TC (74 obs.)	Mean tests	H1: Wilcoxon	MW
Average amount sent back by As	4.43	4.20	0.47	0.21	0.39
	[3.31]	[2.57]	{0.32}	{0.84}	{0.35}
Average amount sent back by Bs	4.66	4.59			
	[5.40]	[4.96]			
H2:					
	α	β	γ	θ	LR test
Tobit for second mover data	-1.88	-0.04	1.39	4.71	24.18
	(0.01)	(0.67)	(0.00)	(0.29)	(0.00)

Note: The column reports the means (and standard deviations between square brackets), and one-tail difference mean test based on a *t*-test assuming independent sample, non-parametric Wilcoxon sum-rank, and Mann-Whitney tests (*p*-values are reported between brace brackets).

Regarding trustworthiness, looking at the amount sent back by Bs and those sent by As, the return ratio is larger than one in TM. Thus, subjects who trust gained more tokens on average than those who did not. The same occurs in TC.[11] However, return ratios are a weak measure of reciprocity, for instance, they may be sample dependent because the inequality faced by second movers is endogenously determined by the investors' actions (Di Bartolomeo and Papa, 2016). As said, following Cox (2004), trustworthiness should be investigated by using individual data.

Looking at the Tobit estimates, as expected, we observe that larger paybacks sent by trustees are related to larger amounts sent by investors, given that γ is significantly different from zero. But, we do not observe any reciprocity effects associated with messages: β is not significantly greater than zero. Therefore, we cannot reject the null hypothesis (H2) that reciprocity is not affected by unilateral communication.

Summarizing, we find that unilateral messages have no effect on the average amounts sent. A possible explanation of our results compared to other studies on trust formation, such as Charness and Dufwemberg (2006) where the choice to trust is binary, could be related to the form of the trust game. Considering a non-binary trust game, where misunderstandings are more likely to be observed, in fact, in our case the number of strategies available is larger than in simple cooperate/non-cooperate trust games or similar ones. Therefore, one can infer that coordination is more difficult to achieve and miscommunication is more likely to be observed. However, different kinds of messages may have an impact on the participants' behavior. We now investigate this point by examining the messages by type.

To analyze whether different types of messages affect trust and cooperation, we classify messages in three different types: promises (P), requests (R), and empty messages (E). Table 2 displays the average amounts sent by investors and trustees paired to a control group.

We test our H3 by comparing the behavior of investors in the treatment where messages were allowed, to those in the control treatment. In the subsample of people who sent promises compared to the paired control group, the average amount sent in TM and in TC is 5.05 and 4.03 tokens, respectively. This difference is not significantly different from zero—as indicated by the *t*-test and the non-parametric Wilcoxon test. In the same way, in the subsample of people who sent empty messages compared to the paired control group, the former sent on average 4.48 and the latter 4.33 tokens, respectively. We do not find any significantly different from zero—as indicated by all tests. Finally, in the subsample of people who sent requests compared to the paired control group, the average amount sent in TM and in TC is 2.94 and 4.44 tokens, respectively. Again, we do not find significant differences—as indicated by the *t*-test and the non-parametric Wilcoxon test. All the differences are not significantly different from zero.

[11] Results on return ratios are mixed, as survived by Glaeser et al. (2000), Capra et al. (2008), Cardenas and Carpenter (2008), Johnson and Mislin (2011). In Cox (2004), Di Bartolomeo and Papa (2016a), and Di Bartolomeo and Papa (2019) the ratio is below one. Instead, in Di Bartolomeo and Papa (2016) and Di Bartolomeo and Papa (2017) subjects who trust gained more money on average than those who did not, as the return ratio is larger than one.

Table 2. H3: Parametric and nonparametric tests (amounts sent and sent back)

Message by kind vs. Control	TM_i	TC_j	Mean test	Wilcoxon
P vs. C: Investors (37 vs 37)	5.05	4.03	1.38	0.47
	[3.79]	[2.49]	{.172}	{.638}
P vs. C: Trustees (37 vs 37)	5.19	4.11		
	[6.22]	[4.19]		
E vs. C: Investors (21 vs 21)	4.48	4.33	0.18	0.57
	[2.66]	[2.61]	{.861}	{.566}
E vs. C: Trustees (21 vs 21)	6.09	5.90		
	[6.66]	[6.05]		
R vs. C: Investors (16 vs 16)	2.94	4.44	-1.59	-1.69
	[2.52]	[2.83]	{.123}	{.090}
R vs. C: Trustees (16 vs 16)	1.81	4.00		
	[1.17]	[5.07]		

Note: The column reports the means (and standard deviations between square brackets), and two-tail difference mean tests based on a *t*-test, assuming independent samples, and non-parametric Wilcoxon rank-sum test (*p*-values are reported between brace brackets).

Compared to the control group, the return ratio of people who sent promises and empty messages, is larger than one. While, the return ratio of trustees who sent requests is lower than one.

Regarding reciprocity, we control the effects of different communication types by using a Tobit regression approach (H4). The results are shown in Table 3.

Table 3. H4: Tobit analysis on amounts sent back (obs. 148)

Censored Tobit analysis for second-mover data (obs. 74)					
	α	β	γ	θ	LR test
P vs. C	-1.54	0.07	1.11	5.29	23.36
	[0.20]	[0.76]	[0.00]	[0.49]	[0.00]
Censored Tobit analysis for second-mover data (obs. 42)					
	α	β	γ	θ	LR test
E vs. C	-3.22	-0.13	2.11	3.88	11.16
	[0.01]	[0.58]	[0.00]	[0.44]	[0.00]
Censored Tobit analysis for second-mover data (obs. 32)					
	α	β	γ	θ	LR test
R vs. C	-1.14	-0.50	1.19	2.84	1.76
	[0. 22]	[0.03]	[0.00]	[0.38]	[0.42]

Note: In the Tobit models, we drop the observations associated with individuals that received zero from the investors, since they are both left- and right-censored.

Source: Author calculations.

Larger paybacks sent by trustees are related to larger amounts sent by investors, given that all γ is significantly different from zero. These results confirm that the more is the amount sent by the investor, the more is the cooperation in each type of message.

We do not observe reciprocity for promises and empty messages compared to the control group. In fact, parameter β is not significantly different from zero in these two Tobit regressions, therefore, H4 must be rejected. It is worth noting that those results may derive from the different initial distributions endogenously generated by the experiment and a larger sample should be used to better analyze reciprocity.[12] However, when we analyze reciprocity for requests, we observe that the coefficient is

[12] Problems in testing reciprocity are due to not conditioning reciprocity to initial inequality which is endogenous when the first stage of investment game is played (Di Bartolomeo and Papa, 2016).

significantly different from zero, but it is negative. So, people who sent requests, once trust is received, are less likely to reciprocate. This behavior reveals that people who sent requests may be self-interest subjects. Investors could be cheated by this type of message since they misunderstand it. Our result is in line with Chen and Houser's (2017) study on whether it is possible to detect deception or trustworthiness from messages. Like them, we find that the investors/receivers are unable to discern between simple requests, and that senders/trustees are not willing to reciprocate, from other messages such as promises or empty messages.

Summarizing, we find that the kind of message does not matter on trust behavior. But trustees who sent requests reveal that they were not interested in cooperation. Uncooperative trustees are usually unrecognizable by investors who often trust them. A feasible explanation of our results is related to the misunderstanding of messages by investors who are unable to distinguish the underlying "uncooperative" meaning of this kind of message.

4. Concluding remarks

We investigated the effects of unilateral non-binding messages on trust and cooperation in investment games by a counterfactual experiment; i.e., comparing the outcomes of an investment game with pre-play communication to those of a canonical one (control group).

Different from the binary trust game with two feasible strategies (e.g., cooperative or uncooperative actions) which find positive effects of cheap talk, at a first glance, we do not find any effect of communication on the amounts sent on average. A feasible explanation is that in investment games (Berg et al., 1995) where more than two strategies are available, the coordination is more difficult to achieve and miscommunication, in terms of the amount to send and to receive, is more likely to be observed.

After classifying the messages according to their contents as promises, requests, and empty messages, we observe that the kind of message does not matter for trust. Regarding reciprocity, trustees who sent requests reveal that they were not interested in cooperation as the return ratio is lower than one, whereas trustees who sent promises or empty messages enhance cooperation as the return ratio is larger than one. However, people who sent requests, once they received trust, send back an amount lower than the control group. People sending requests are uncooperative trustees that are usually unrecognizable by investors who always trust them. A possible explanation is related to the misunderstanding of the real content of their messages. Investors are unable to distinguish the underlying meaning of different kinds of messages as they seem to misread requests as cooperative messages.

References

BALTUSSEN G., POST G.T., VAN DEN ASSEM M.J., WAKKER P.P. (2012), Random incentive systems in a dynamic choice experiment, *Experimental Economics*, 15, 418-443.

BEATTIE J., LOOMES G. (1997), The impact of incentives upon risky choice experiments, *Journal of Risk and Uncertainty*, 14, 155-168.

BEN-NER A., PUTTERMAN L. (2009), Trust, communication and contracts: An experiment, *Journal of Economic Behavior and Organization*, 70, 106-121.

BEN-NER A., PUTTERMAN L., REN T. (2011), Lavish returns on cheap talk: Non-binding communication in a trust experiment, *Journal of Socio-Economics*, 40, 1-13.

BERG J., DICKHAUT J., MCCABE K. (1995), Trust, reciprocity and social history, *Games and Economic Behavior*, 10, 122-142.

BLUME A., ORTMANN A. (2007), The effects of costless pre-play communication: Experimental evidence from games with Pareto-ranked equilibria, *Journal of Economic Theory*, 132, 274-290.

BOCHET O., PAGE T., PUTTERMAN L. (2006), Communication and punishment in voluntary contribution experiments, *Journal of Economic Behavior and Organization*, 60, 11-26.

BRACHT J., FELTOVICH N. (2009), Whatever you say, your reputation precedes you: Observation and cheap talk in the trust game, *Journal of Public Economics*, 93, 1036-1044.
CAPRA C.M., LANIER K., MEER S. (2008), *Attitudinal and behavioral measures of trust: A new comparison,* Department of Economics, Emory University, mimeo.
CARDENAS J.C., CARPENTER J. (2008), Behavioural development economics: Lessons from field labs in the developing world, *Journal of Development Studies*, 44, 311-338.
CHARNESS G. (2000), Self-serving cheap talk: A test of Aumann's conjecture, *Games and Economic Behavior*, 33, 177-194.
CHARNESS G., DUFWENBERG M. (2006), Promises and partnership, *Econometrica*, 74, 1579-1601.
CHARNESS G., DUFWENBERG M. (2010), Bare promises: An experiment, *Economics Letters*, 107, 281-283.
CHEN J., HOUSER D. (2017), Promises and lies: Can observers detect deception, *Experimental Economics*, 20, 396-419.
COOPER R., DE JONG D.V., FORSYTHE R., ROSS T. (1989), Communication in the battle of the sexes game, *Rand Journal of Economics*, 20, 568-587.
COOPER R., DE JONG D.V., FORSYTHE R., ROSS T. (1992), Communication in coordination games, *Quarterly Journal of Economics*, 107, 739-771.
COX J. C. (2004), How to identify trust and reciprocity, *Games and Economic Behavior*, 46, 260-281.
CUBITT R.P., STARMER C., SUGDEN R. (1998), On the validity of the random lottery incentive system, *Experimental Economics,* 1, 115-131.
DI BARTOLOMEO G., PAPA S. (2016), Trust and reciprocity: Extensions and robustness of triadic design, *Experimental economics,* 19, 100–115.
DI BARTOLOMEO G., PAPA S. (2016a), Does collective meditation foster trust and trustworthiness in an investment game?, *International Review of Economics*, 63, 379–392.
DI BARTOLOMEO G., PAPA S. (2017), Conditional and unconditional other regarding preferences: A qualitative analysis, *Annali del Dipartimento di Metodi e Modelli per l'Economia, Il Territorio e la Finanza*, 47-57.
DI BARTOLOMEO G., PAPA S. (forthcoming 2019) The effects physical activity on social interactions: The case of trust and trustworthiness, *Journal of Sport Economics,* 20, 50-71.
DI BARTOLOMEO G., DUFWENBERG M., PAPA S., PASSARELLI F. (forthcoming 2018), Promise, expectations and causation, *Games and Economics Behavior*.
DUFFY J., FELTOVICH N. (2002), Do actions speak louder than words? An experimental comparison of observation and cheap talk, *Games and Economic Behavior*, 39, 1-27.
EDERER F., FEHR E. (2007), Deception and incentives: How dishonesty undermines effort provision, *Discussion Paper No. 3200*, IZA, Bonn.
ELLINGSEN T., JOHANNESSON M. (2004), Promises, threats and fairness, *The Economic Journal*, 114, 397-420.
GLAESER E.L., LAIBSON D. I., SCHEINKMAN J. A., SOUTTER C. L. (2000), Measuring trust, *Quarterly Journal of Economics*, 115, 811-846.
GNEEZY U. (2005), Deception: The role of consequences, *American Economic Review*, 95, 384-394.
HE S., OFFERMAN T., VAN DE VEN J. (2016), The sources of the communication gap, *Management Science,* 63, 2832-2846.
JOHNSON N.D., MISLIN A.A. (2011), Trust games: A meta-analysis, *Journal of Economic Psychology*, 32, 805-889.
POGREBNA G., BLAVATSKYY P.R. (2009), Coordination, focal points and voting in strategic situations: A natural experiment, *Public Choice*, 140, 125-143.
RIGDON M. (2009), Trust and reciprocity in incentive contracting, *Journal of Economic Behavior and Organization*, 70, 93-105.
RODE J. (2010), Truth and trust in communication: Experiments on the effect of a competitive context, *Games and Economic Behavior*, 68, 325-338.
SANCHEZ-PAGES S., VORSATZ M. (2007), An experimental study of truth-telling in a sender-receiver game, *Games and Economic Behavior*, 61, 86-112.

SUTTER M. (2009), Deception through telling the truth? Experimental evidence from individuals and teams, *The Economic Journal*, 119, 47-60.
STARMER C., SUGDEN R. (1991), Does the random-lottery incentive system elicit true preferences? An experimental investigation, *American Economic Review*, 81, 971-978.
VAN HUYCK J.B., BATTALIO R.C., BEIL R.O. (1990), Tacit coordination games, strategic uncertainty, and coordination failure, *American Economic Review*, 80, 234-248.
VANBERG C. (2008), Why do people keep their promises? An experimental test of two explanations, *Econometrica*, 76, 1467-1480.
WILSON R.K., SELL J. (1997), Liar, liar... Cheap talk and reputation in repeated public goods settings, *Journal of Conflict Resolution*, 41, 695-717.