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Promises and Image Concerns*

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Abstract

According to several psychological and economic studies, non-binding communication can be an effective tool to increase trust and enhance cooperation. This paper focuses on reasons *why* people stick to a given promise and analyzes to what extent image concerns of being perceived as a promise breaker play a role. In a controlled laboratory experiment, we vary the ex post observability of the promising party's action in order to test for social image concerns. We observe that slightly more promises are kept if the action is revealed than if it is not, yet the difference is not significant. However, a variation in the selection of pre-defined messages across treatments delivers another interesting finding. While most of the promises are kept, statements of intent tend to be broken.

Keywords: Promises, communication, social image concerns, guilt, shame, behavioral economics, experiment

JEL-Classification: C70, C91, D03, D82

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1 Introduction

Cooperation among interacting partners is essential for economic success in many situations, as joint value creation often exceeds individual achievements. These situations become challenging as soon as cooperation cannot be contractually enforced, but relies on mutual trust by the interacting partners. Among a large literature focusing on how to improve cooperation, various experimental studies show that communication can be an effective tool to enhance it (see, e.g. Bochet and Putterman, 2009; Cooper et al., 1992; Ellingsen and Johannesson, 2004). While several articles analyze whether cheap talk can be effective and how this depends on the communication protocol and the game structure (see for instance Blume and Ortmann, 2007; Camera et. al., 2011; Ellingsen and Östling, 2010; Kriss et al., 2011; Mohlin and Johanneson, 2008), we contribute to the literature focusing on *why* individuals stick to a commitment, given that rationality predicts a deviating behavior. In particular, we analyze whether and to what extent social image concerns motivate people to stick to a given promise. More precisely, as breaking a promise is deemed negative in society, avoiding the image of being a promise breaker might induce individuals to keep their word. Consequently, we study whether an individual is more likely to act in line with a given promise if its violation is more obvious to its receiver.

In order to test whether social image concerns influence promise keeping behavior, we conduct a controlled laboratory experiment. Here, subjects are randomly matched in pairs of two and play a one-shot sequential trust game similar to the one used in Charness and Dufwenberg (2006). A first mover (`A`) decides whether to enter the game or to opt out, the latter choice inducing a low outside option for both players. If `A` enters the game, a second mover (`B`) chooses between a selfish option, yielding a payoff of zero for `A`, and cooperation, in which case a chance move determines whether `A` gets a positive payoff or 0.[1] Prior to the strategic decisions, the second mover sends one out of three pre-defined messages to the first mover, one of which is a promise to cooperate. In order to test for social image concerns, we vary the ex-post observability of the second mover's action. While in condition *Rev* `A` learns `B`'s action choice, in condition *NoRev* she cannot infer whether a payoff of zero is due to `B` behaving selfish or just to bad luck.[2] We hypothesize that a higher share of `B`s cooperate if `B`'s action is revealed to `A` (*Rev*) than if it is concealed (*NoRev*), assuming that a fraction of `B`s has a preference for avoiding the image of being a promise breaker.

By the choice of our experimental design, we attempt to differentiate social image con-

[1]While rational behavior predicts the second mover to behave selfish, and therefore the first mover not to enter the game, mutual cooperation is the unique Pareto-optimal outcome, which generates the highest joint payoff.

[2]Conditions are assigned randomly to pairs.

cerns from other possible reasons for promise keeping by second movers. Up to now, the literature mainly provides two motivations why individuals might stick to their promises. Firstly, Charness and Dufwenberg (2006) explain promise keeping by simple guilt, i.e. the aversion to disappoint other people's expectations, as introduced by Batigalli and Dufwenberg (2007). If the second mover promises cooperation, the first mover expects a higher payoff, which increases the second mover's guilt in case he refuses to cooperate. However, in our experiment only the game structure and the payoffs are common knowledge, but `B`s are privately informed about the revelation condition. `A`s are not even aware that different conditions exist. Thus, `A`s' first order beliefs, and consequently `B`s' second-order beliefs should not vary across conditions, inducing the same amount of guilt for non-cooperation in both conditions.[3] Secondly, Vanberg (2008) claims that subjects have a preference for keeping their promises per se, independent of others' expectations. This assumption cannot explain a difference in `B`s' behavior across conditions either, as the preference for keeping a promise should be independent of `A`'s ex-post information.

Yet, as the revelation of `B`'s action choice might also induce a concern of being perceived as selfish (Tadelis, 2011), we conduct a control treatment without communication (*NoCom*). We claim that the effect of revelation on behavior in treatment *Com* is larger than the respective effect in *NoCom*, indicating that the differential effect is due to the mere aversion of being perceived as a promise breaker, additional to the aversion to an egoistic image.

With pre-play communication, we observe marginally significantly more cooperation in *Rev* than in *NoRev*. This effect does not seem to be driven by shame to be selfish alone, as without communication revelation even marginally decreases cooperation rates in *Rev* compared to *NoRev*. However, although conditions are identical at the pre-play communication stage, the number of promises sent is significantly higher in *Rev* than in *NoRev*. Thus, the higher *Roll* rate in *Rev* might only be driven by a higher number of promises and not by image concerns of being perceived as a promise breaker. When comparing the share of promises kept, we do observe a slightly higher rate in *Rev* (85%) than in *NoRev* (81%), however the difference is not significant. Thus, we fail to prove our hypothesis that avoiding the image of being perceived as a promise breaker plays a significant role in the individual decision to keep a given promise.

It is worth noting that the high promise keeping rate without revelation (81%) limits the scope for further increase. In treatment *Com1*, where `B`s can choose between a promise to cooperate, a statement of intent, and an empty message, this high promise keeping rate might be partly due to the fact that `B`s, who attempt to influence their interaction partner without planning to cooperate, have the possibility to send a statement of intent.

[3] Otherwise `A` might expect `B` to choose *Roll* with a higher probability if his choice is revealed, inducing higher simple guilt in *Rev* than in *NoRev* (if we assume consistent beliefs).

In order to reduce the promise keeping rate without revelation by forcing this type of subjects to either break a promise or refrain from influencing the interaction partner, we exclude the opportunity of stating an intention in a further treatment, *Com2*. However, we do not observe a significant effect of revelation in *Com2* either.

Still, this design variation provides another interesting finding. The menu of messages available to B seems to play a significant role for the effectiveness of communication, as Bs are significantly more likely to keep a promise than to stick to a statement of intent. Hence, intentions seem to be less costly to break than promises. In contrast, As, who are unaware of the available messages, seem to trust intentions to the same amount as promises.

Literature

This paper is mainly related to two strands of the economic literature. First, there is an expanding literature analyzing the effect of non-binding communication on behavior. Experimental studies show that communication can increase coordination (Blume and Ortmann, 2007; Ellingsen and Östling, 2010; Kriss et al., 2011), generosity in a dictator game (Andreoni and Rao, 2011; Mohlin and Johannesson, 2008), and most relevant for our study, cooperation (Bochet and Putterman, 2009; Charness and Dufwenberg, 2006, 2010; Cooper et al., 1992; Ellingsen and Johannesson, 2004; Vanberg, 2008). So far, mainly two reasons for the effectiveness of communication have been identified. On the one hand, guilt aversion in the sense of Batigalli and Dufwenberg (2007) has been found to induce promise keeping (see for instance, Charness and Dufwenberg, 2006, or Beck et al., 2013).[4] On the other hand, individuals can exhibit a preference for promise keeping per se, that is, promises have a commitment value (Ismayilov and Potters, 2012; Vanberg, 2008). Likewise, individuals might face costs of lying (Fischbacher and Föllmi-Heusi, 2013; Hurkens and Kartik, 2009; Lundquist et al., 2009; Mazar et al., 2008). However, none of these papers consider social image concerns as a reason why people stick to their promises.

A second related strand in the behavioral economics literature studies the effect of social image concerns on behavior. Following Ariely et al. (2009), "image motivation [...] refers to an individual's tendency to be motivated partly by others' perceptions." That is, individuals dislike to publicly violate a social norm, such as altruism or modesty. Correspondingly, evidence for individuals behaving more selfishly or greedily if their action is less likely to be observed, has for example been found in experimental dictator games (Andreoni and Bernheim, 2009; Broberg et al 2007; Dana et al., 2006; Dana et al., 2007; Grossman, 2010a, 2010b; Koch and Normann, 2008; Larson and Capra, 2009) and in the context of volunteering (e.g. Carpenter and Myers, 2010; Linardi and McConnell, 2008)

[4]However, the effect of guilt aversion has been found to be relatively small (Ellingsen et al., 2010).

or donations (Ariely et al., 2009; DellaVigna et al., 2012; Lacetera and Macis, 2010).

Similar to our experimental design, Tadelis (2011) builds on the framework of CD (2006) and varies the ex post information of the first mover. He shows that image concerns to appear selfish (the "shame" effect) exist and increase cooperation, especially if anonymity is lifted, by announcing the second mover's action choice to all participants in the room. However, subjects in his setting are not able to communicate.

In our study, we combine these two strands of literature and investigate whether social image concerns are even more pronounced with communication, due to the aversion of being perceived as a promise breaker. Bracht and Regner (2011) also analyze social image concerns in a similar trust game with communication, however, they focus on the correlation of behavior to proneness to shame and guilt, which they elicit via a psychological test.[5] While Bracht and Regner (2011) analyze the effect of transparency and communication separately, we focus on how communication interacts with the effect of revelation on behavior. To the best of our knowledge, social image concerns have rarely been analyzed in the context of communication.

The remainder of this paper is structured as follows. Section 2 introduces the experimental design and the leading Hypotheses. In Section 3, we analyze and discuss the experimental results. We compare our results to previous research in Section 4, and Section 5 concludes.

2 Experimental Design and Hypotheses

2.1 Experimental Design

At the beginning of the experiment, role `A` is assigned to half of the subjects while the other half is assigned role `B`. One subject with role `A` and one subject with role `B` are randomly matched to form a pair.[6] Each pair subsequently plays the one-shot trust game depicted in Figure 1, which is akin to the one used by Charness and Dufwenberg (2006), henceforth CD (2006). The upper number refers to `A`'s payoff, the lower one to `B`'s.[7]

`A` ("she") decides whether to enter the game (*In*) or not (*Out*). Without learning `A`'s decision, `B` ("he") decides whether to keep a payoff of 30 tokens for himself while `A`

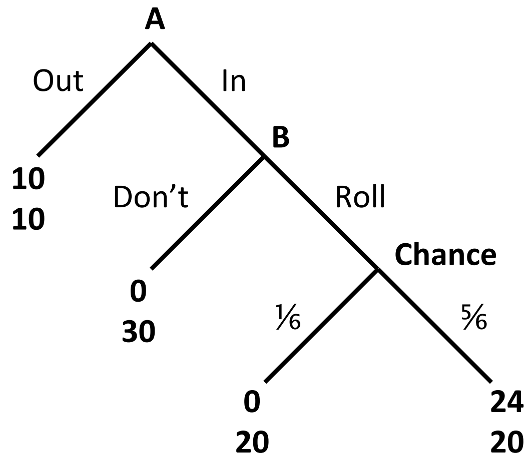
[5]Bracht and Regner (2011) find that disposition to guilt predicts behavior, but not disposition to shame.

[6]In the following, we refer to the player with role `A` (`B`) as `A` (`B`).

[7]In comparison to CD (2006), stakes are lower in our set-up, as one session consists of two separate experiments, which are both paid out (see Section 2.3). However, the proportions of the payoffs resulting from different strategies are similar.

receives nothing (*Don't Roll*), or to let a die decide over A's payoff (*Roll*). In this case, A receives a payoff of 24 tokens with probability $\frac{5}{6}$ and a payoff of 0 with probability $\frac{1}{6}$, while B earns a payoff of 20 tokens in any case. In order to elicit B's action choice we use the strategy method, i.e. B decides on his action independent of whether A enters the game or not. At the end of the experiment, one token is converted into 0.25 Euros.

Figure 1: The Trust Game

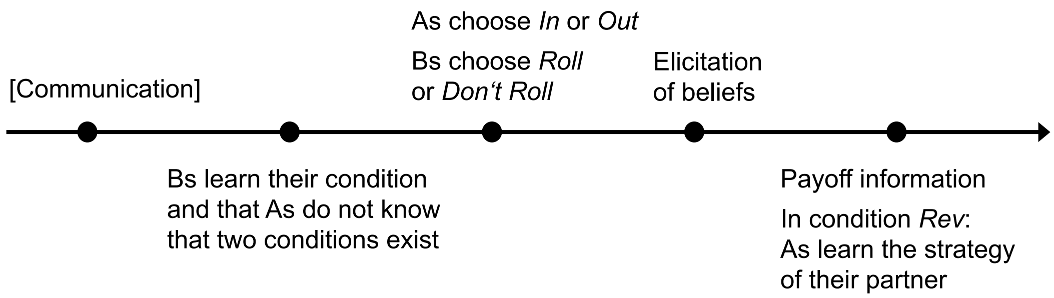


We conduct three treatments, called *Com1*, *Com2* and *NoCom*. In *Com1* and *Com2* B sends one out of three predefined messages to A, prior to playing the trust game. *Com1* and *Com2* differ only in the type of messages that can be sent. In *Com1* B can choose between a promise ("I promise to choose *Roll*."), an intention ("I will choose *Roll*.") or an empty message ("Hello, how are you? I'm fine."). In *Com2* B can choose between the same promise and two empty messages ("Hello!" and "How are you?"), i.e. B cannot send an intention in *Com2*. As the design of *Com1* and *Com2* is the same except for the message choices, we sometimes refer to the pooling of both communication treatments as *Com*. *NoCom* is a control treatment, which is identical to the other two treatments, but without pre-play communication.

Without any further information, A cannot infer whether B has chosen *Roll* or *Don't Roll* whenever she experiences a payoff of 0. However, we are interested in the influence of social image concerns on B's cooperative behavior (see Section 2.2), that is, whether B cares about how he is perceived by A. Consequently, we vary whether A can observe B's action choice at the end of the experiment or not, which yields two conditions within each treatment. Before playing the trust game, half of the pairs is randomly assigned to condition "Revelation" *(Rev)*, the other half plays condition "No Revelation" *(NoRev)*. In condition *Rev* B's choice will be revealed to A at the end of the experiment, whereas A does not learn B's behavior in condition *NoRev*.

B is informed about the condition he plays before choosing between *Roll* and *Don't Roll*, but after having sent a message to A. Thereby, we ensure that only the action choice, and not the type of message sent, is affected by the condition. In other words, when B chooses the message to be sent, both conditions are exactly equal and Bs' communication behavior should not differ across conditions. Hence, any difference in *Roll* rates across conditions is then due to the variation of the observability of B's action choice and not to a difference in messages across conditions. Figure 2 provides an overview of the course of the experiment.

Figure 2: The Sequence of the Experiment



A neither learns the condition she is playing in nor is she aware that two different conditions exist until the end of the experiment. The instructions are the same for A and B and inform the participants only about the course and the payoffs of the game, without commenting on information structures.[8] B receives private information about the condition he plays via his screen during the experiment. By not informing A, we ensure that A's first-order belief about B's behavior is constant across conditions. Furthermore, B is explicitly informed about A's unawareness that two conditions exist, thus his second-order belief about A's expectations should not vary across conditions. Therefore, guilt aversion, i.e. the aversion to disappoint A's expectations cannot cause a difference in B's behavior across conditions. We explain the concept of guilt aversion in more detail in Section 2.2.

A's first-order and B's second-order beliefs are elicited after the trust game, but before subjects learn their payoffs. As were asked: *"What do you think, how many of the x Bs in the room have chosen Roll?"*, where x was substituted by the number of Bs in the session. For Bs, eliciting beliefs is a bit more involved. In a sequential game like the one we consider, B's choice only becomes relevant for those As who choose *In*, thus only the first-order beliefs of those As should matter for B's behavior and his second-order belief. Hence, we asked all Bs: *"We asked all As: "What do you think, how many of the x Bs in the room have chosen Roll?" Consider only the As who chose In. What do you think is the average guess of those As?"*[9] Subjects earn a supplement of 6 tokens for a

[8]However, the instructions emphasize that all Bs throw a die such that Bs' decisions can not be inferred, which is likely to induce a prior of getting no information among As.

[9]This procedure is analogous to the one in CD (2006).

guess deviating by at most +/-1 from the correct answer. This way, we elicit an interim second-order belief conditional on the event of `A` choosing *In*.[10]

2.2 Hypotheses

In the following, we derive our hypotheses from a notion of social image concerns and subsequently exclude other possible behavioral explanations for our hypotheses.

Assuming selfish and risk-neutral players, the unique subgame-perfect equilibrium in the trust game illustrated in Figure 1 is (*Out, Don't Roll*). However, while the classical game theory claims that non-binding communication cannot influence the players' strategies if information is symmetric, it has been observed in the laboratory that communication indeed enhances cooperation in trust games – promises are made, taken as credible and frequently kept. While CD (2006) argue that subjects keep their promises due to guilt aversion, that is, to not disappoint the increased expectations of the truster, Vanberg (2008) claims that people have a tendency to keep their promise per se, independent of the truster's expectations. Still, in their experiments a considerable share of trustees break a given promise.[11] We analyze whether a change in the set-up, i.e. introducing transparency about the trustee's action induces more trustees to be true on their word. More precisely, we investigate whether social image concerns of being perceived as a promise breaker exist and induce individuals to stick to their word. This yields our main hypothesis, which we break down to testable hypotheses in the following.

Main Hypothesis. *The aversion of being perceived as a promise breaker exists and is one reason for why people keep a given promise.*

Indeed, it is frequently observed by Economists, Sociologists and Psychologists that people care for how they are perceived by others (e.g. Apsler, 1975; Grossman, 2010a; Lacetera and Macis, 2010; Lewis, 1995; Scheff, 1988; Smith et al., 2002; Tangney, 1995). Applied to our setting, we hypothesize that the trustee is more likely to cooperate if his action choice is revealed than if it is concealed, in a situation where communication is possible.[12]

Hypothesis 1. *The revelation of* `Bs`*' action choices induces more cooperation among* `Bs`*. In our setting, the Roll rate in* $[Com|Rev]$ *is higher than in* $[Com|NoRev]$.

[10] One could argue that observing the actual choice of the `A`-player is far more influential for beliefs than a hypothetical choice. However, we think that this effect is negligible given that the results show a high correlation between second-order beliefs and actual strategy choices.

[11] CD (2006) observe that 25% of promisers break their promise without revelation, Vanberg 2008 observes a share of 27% (*no switch* condition).

[12] We are aware that the experimenter always observes whether a promise is kept or not and that this can also evoke some social image concerns. However, the presence of the experimenter does not vary across conditions.

However, the presence of social image concerns does not necessarily rely on the possibility to communicate. In fact, even without communication evidence for social image concerns has been found, such as the aversion of being perceived as egoistic or greedy (e.g. Ariely et al., 2009; Dana et al., 2006, 2007; Güth et al., 1996; Koch and Normann, 2008; Tadelis, 2011). From a theoretical point of view, Tadelis (2011) proposes a model of “shame” inducing disutility of being perceived as a non-cooperator, in order to explain the effect he observes.[13] Besides the social disapproval of egoism, we are interested in the existence of another social norm which condemns promise breaking, thereby inducing additional social image concerns. Accordingly, we hypothesize that the effect of revelation on *Roll* rates is larger if subjects can communicate than without communication, indicating that the differential effect has to be due to an aversion to be regarded as a promise breaker. Hence, we compare the results of *Com* to the control treatment *NoCom* and state the following hypothesis.[14]

Hypothesis 2. *The effect of revelation on cooperation is larger if pre-play communication takes place. In our setting, the difference between* $[Com|Rev]$ *and* $[Com|NoRev]$ *is larger than the difference between* $[NoCom|Rev]$ *and* $[NoCom|NoRev]$.

Yet, communication might enhance cooperative behavior of Bs independent of the observability of Bs' action choice (CD, 2006; Vanberg, 2008). In order to contribute the hypothesized higher *Roll* rate in $[Com|Rev]$ compared to $[Com|NoRev]$ to revelation only, the share of promises has to be equal in both conditions. Given that Bs do not know the condition they play at the pre-play communication stage, and Bs are randomly assigned to both conditions, promising behavior should not differ across conditions. Still, if and only if this holds, we can conclude our main hypothesis from Hypotheses 1 and 2.

Elimination of Alternative Explanations

In the following, we show that, given Hypothesis 1 holds, it can neither be explained by simple guilt nor by promise-keeping per se.

Simple guilt. If B is subject to *simple guilt*, in the sense of Batigalli and Dufwenberg (2007), he is reluctant to cause a lower payoff for A compared to what he believes she expects to earn. Let thus $\alpha_{\mathtt{A}} := \Pr_{\mathtt{A}}(Roll)$ denote A's belief about the probability that B cooperates. Then A expects to earn a payoff of $\frac{5}{6} \cdot \alpha_{\mathtt{A}} \cdot 24 = 20\alpha_{\mathtt{A}}$ upon entering the game. In turn, B forms a belief about A's belief about his action choice, given that A chooses *In*. This results in B's interim second-order belief $\beta_{\mathtt{B}} := E[\alpha_{\mathtt{A}}|In]$. By choosing

[13] “Guilt from blame” (Batigalli and Dufwenberg, 2007) also accounts for more cooperation in the *Rev* condition, based on B facing disutility from A blaming him for a bad outcome.

[14] We consider the *Roll* rates of all trustees in *Com* rather than focusing on those of the promising trustees only, as this allows for a comparison of *Roll* rates to the behavior in the control treatment, *NoCom*.

Don't Roll conditional on A choosing *In*, B experiences simple guilt proportional to $20\beta_{\mathtt{B}}$, his belief about the difference between A's payoff expectation and her experienced payoff. In contrast, if B cooperates, any deception by A cannot be due to B's behavior, thus he doesn't feel guilty. Assuming that B's utility is additively separable in his material payoff and his experienced simple guilt, this yields

$$\begin{aligned} u_{\mathtt{B}}(In, Roll) &= 20 \\ u_{\mathtt{B}}(In, Don'tRoll) &= 30 - \theta^{SG} \cdot 20\beta_{\mathtt{B}}, \end{aligned}$$

where θ^{SG} denotes B's sensitivity to simple guilt.

Simple guilt can explain why communication is able to influence behavior. If B makes a promise, he believes that he influences A's belief about his behavior, i.e. $\beta_{\mathtt{B}}$ increases. Ceteris paribus, this induces a lower payoff for choosing *Don't Roll*, hence a larger share of Bs chooses *Roll* after having sent a promise.

While simple guilt delivers an explanation for why communication fosters cooperation, it cannot explain the effect in Hypothesis 1. As As do not learn the condition they play, their first-order beliefs cannot depend on whether Bs' behavior is revealed or not. Bs know about the unawareness among As and thus their second-order beliefs cannot depend on the condition either. Thus, ceteris paribus, guilt aversion predicts the same *Roll* rates for conditions *Rev* and *NoRev*. As the condition is not known to both players at the time communication takes place, the amount of promises should be the same in both conditions. Given this assumption, guilt aversion predicts the same *Roll* rates whether B's decision is revealed or not, which contradicts Hypothesis 1.

Self-image concerns ("Promise keeping per se"). Vanberg (2008) argues that there exists a preference for promise keeping per se independent of the truster's expectations. He shows that in case an individual faces a different player than the one he made a promise to, his action choice does not depend on whether the new partner has received a promise by another player before or not. In a similar vein, Ellingsen and Johannesson (2004) introduce the notion of "lying cost". They propose a model where inequity averse players suffer from a fixed personal cost of being inconsistent, $l \geq 0$, which in turn leads to a higher commitment power and credibility of promises.

However, whether B's action in the trust game is revealed to A in the end or not does not make a difference to B if he is a "promise-keeper per se". Thus, given an equal number of promises in both conditions, Hypothesis 1 can not be solely induced by promise-keeping per se.

2.3 Experimental Procedure

The experiment was conducted in the Munich Experimental Laboratory for Economic and Social Sciences (MELESSA). Subjects were recruited using the online recruitment system ORSEE (Greiner, 2004), and the 406 participants in 17 sessions consisted mainly of students. Upon entering the laboratory, subjects were randomly assigned to 24 visually isolated computer terminals. The instructions were distributed and read out loud by one of the experimenters. Questions were answered individually at the subjects' seats. Before the experiment started, subjects filled out a short questionnaire ensuring the comprehension of the rules.

The experiment was the first of two independent experiments conducted in one session. Before the experiment started, participants were informed that two independent experiments would be conducted without any further information about the second experiment. Both experiment were paid out at the end of the session, where the average earning was 12.6 EUR, including a fixed show-up fee of 4 EUR. In the first experiment, As received 3.5 EUR on average, while the mean among Bs was 5.2 EUR. The experiment was programmed and conducted with the software z-tree (Fischbacher, 2007). Each session ended with a detailed questionnaire on demographics and social preferences and lasted about 50 minutes.

3 Experimental Results

In this section we first analyze the effect of revelation on Bs' behavior (Section 3.1), followed by an investigation of the effects of communication (Section 3.2).

3.1 The Effect of Revelation on Bs' Behavior

In the following, we pool the data of *Com1* and *Com2* to *Com* in order to analyze the differences between $[Com|Rev]$ and $[Com|NoRev]$. This procedure is justified as the effect of revelation on Bs' behavior does not differ between the two communication treatments. Data considering each treatment separately is gathered in the appendix.

Our first result provides some evidence for Hypothesis 1.

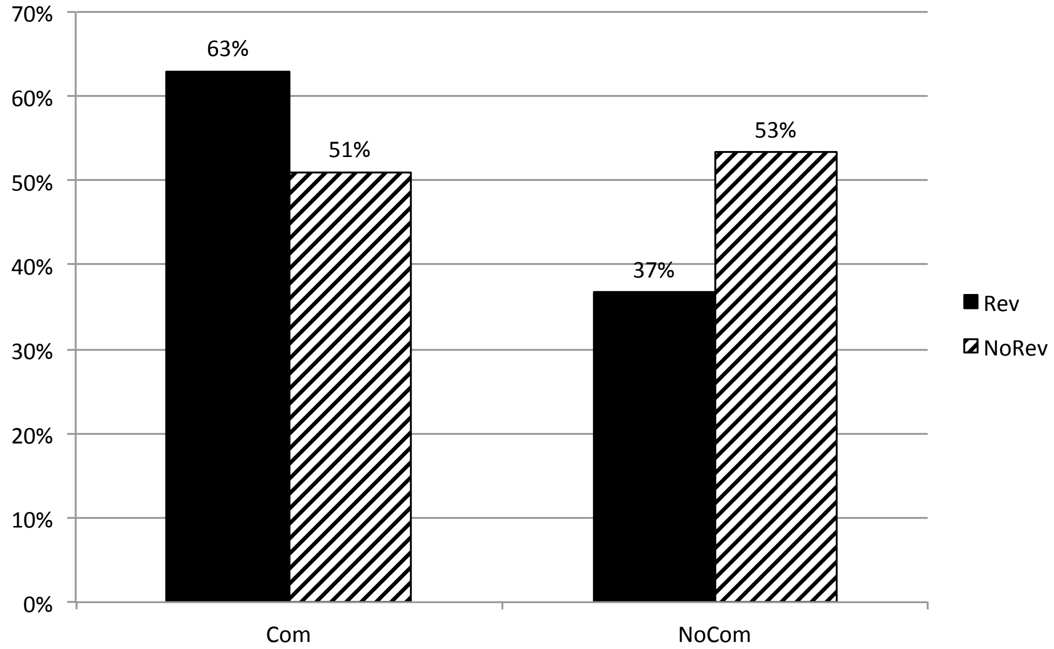
Result 1. *The Roll rate in* $[Com|Rev]$ *is higher than in* $[Com|NoRev]$*, with the difference being marginally significant.*

Indeed, while 63% of Bs choose *Roll* in $[Com|Rev]$, this share amounts to only 51% in

[$Com|NoRev$] (test of proportions, one-tailed, Z=1.450, p=0.074).[15] Thus, Hypothesis 1 is confirmed on a marginally significant level, indicating that subjects in a situation where communication is possible behave more cooperatively when their action is revealed than when it is not.

The next step is to take a closer look at the source of this marginally significant effect. We claim that Bs behave more cooperatively in [$Com|Rev$] than in [$Com|NoRev$] as they do not want to be perceived as a promise breaker. In order to confirm this claim, we have to confirm that the higher *Roll* rate in [$Com|Rev$] is not only caused by an image concern of being perceived as selfish, but rather induced by the combination of communication and revelation (Hypothesis 2). Therefore, we compare the observed effect of revelation in *Com* to the one in *NoCom*. Figure 3 illustrates the shares of Bs choosing *Roll* in *Com* and *NoCom* separated by condition.

Figure 3: *Roll* Rates of Bs Separated by Condition



First, we consider the *NoCom* treatment separately and find no evidence for an image concern of being perceived as selfish.

Result 2. *The Roll rate in* [$NoCom|Rev$] *is marginally significantly lower than the one in* [$NoCom|NoRev$]*. Hence, there is no evidence for the existence of image concerns of being perceived as selfish.*

[15]If we consider *Com1* and *Com2* separately, the effect goes in the same direction, but is no longer significant (*Com1*: 54% vs. 42%, Z=1.062, p=0.144; *Com2*: 72% vs. 61%, Z=1.000, p=0.159, one-tailed test). Throughout this paper, the Z-Statistics reflect the test of proportions (see Glasnapp and Poggio, 1985) and p-values are on one-tailed tests, because we use our underlying hypotheses, except when reported otherwise.

Indeed, only 37% of Bs choose *Roll* in [$NoCom|Rev$] whereas 53% cooperate in [$NoCom|NoRev$]. This difference is marginally significant (test of proportions, one-tailed, Z=1.292, p=0.098). Thus, if revelation changes Bs' behavior in *NoCom*, it rather decreases cooperative behavior.[16] This rather unexpected result is unlikely to be a demand effect as Bs are only informed about their own condition, i.e. that their behavior is revealed or not revealed to A, but not about the existence of the other condition. The low *Roll* rate in [$NoCom|Rev$] might be a sullen behavior due to the sudden announcement that B's action will be revealed to A, which was not mentioned in the instructions.

While Result 1 and Result 2 already suggest the confirmation of Hypothesis 2, i.e. that the effect of revelation on cooperation is larger in *Com* than in *NoCom*, we conduct a probit regression to compare the differences across conditions in *Com* and *NoCom*, delivering the following result (Hypothesis 2).

Result 3. *The difference in Roll rates between [$Com|Rev$] and [$Com|NoRev$] is significantly larger than the one between [$NoCom|Rev$] and [$NoCom|NoRev$]. Hence, Hypothesis 2 is confirmed.*

The results of the probit regression are reported in Table 1. The dependent variable is 1 if B chooses *Roll* and 0 otherwise. The independent variables are a dummy for *Com*, a dummy for *Rev*, and an interaction of the two.

Table 1: Regression of Choosing *Roll*

	PROBIT Coefficient (p-value)	OLS Coefficient (p-value)
Com	-0.049	-0.019
	(0.770)	(0.775)
Rev	-0.424	-0.167
	(0.105)	(0.108)
*Com*Rev*	0.731	0.287
	(0.030)	(0.040)
Constant	0.084	0.533
	(0.279)	(0.000)

We cluster standard errors on sessions (17 sessions). Number of observations is 203. In the Probit regression Pseudo R-squared is 0.023 and log Pseudo Likelihood is -136.961. In the OLS regression R-squared is 0.031.

We observe that the only significant coefficient is the one of the interaction term *Com*Rev* (p=0.030), which is positive, showing that cooperation among Bs is increased by revelation only in treatment *Com*. The negative and almost marginally significant coefficient of

[16] This result is in contrast to Tadelis (2011).

Rev indicates the negative effect of revelation on cooperation without communication. The results are robust to an OLS regression, which is also reported in Table 1. Thus, Hypothesis 2 is confirmed.

Yet, it remains to show that Bs' communication behavior does not differ between [*Com*|*Rev*] and [*Com*|*NoRev*]. For no apparent reason, we are not able to confirm this.

Result 4. *The share of promises among messages in [Com|Rev] is significantly higher than the one in [Com|NoRev]. Hence, we are not able to conclude the main hypothesis about the existence of an image concern of being perceived as a promise breaker.*

Table 2 provides an overview of the messages sent from B to A in *Com*.

Table 2: Overview of Messages Sent in *Com*

		Promise	Intention	Empty
Com	*Rev*	48/71 68%	6/71 9%	17/71 24%
	NoRev	37/72 51%	13/72 18%	22/72 31%
	Z stat. (p-value)	1.975 (0.024)	-1.692 (0.045)	0.888 (0.187)

The Z Stat reflects the test of proportions for the two treatments or conditions (see Glasnapp and Poggio, 1985). The p-value is on one-tailed tests.

There is neither a difference in the design nor in the instructions of the two conditions. B does not even know that two different conditions exist when sending his message. Still, we observe a significantly higher share of Bs sending a promise in [*Com*|*Rev*] than in [*Com*|*NoRev*] (68% vs. 51%, one-tailed test of proportions, Z=1.975, p=0.024).[17] On the other hand, we also observe a significantly smaller share of intentions in [*Com*|*Rev*] than in [*Com*|*NoRev*] (Z=1.692, p=0.045), yielding a similar share of intentions and promises (pooled) in both conditions (76% in [*Com*|*Rev*] vs. 70% in [*Com*|*NoRev*], p=0.448, two-tailed test). However, as further analyzed in Section 3.2 and reported in Table 3, subjects sending a promise choose *Roll* significantly more often than subjects sending an intention or an empty message (in *Rev* Z=5.568, p=0.000, in *NoRev* Z=5.183, p=0.000). Thus, we cannot pool intentions and promises, and the communication behavior has to be considered as largely different in both conditions, indicated by a significantly higher share of promises in *Rev* than in *NoRev*.

Therefore, we cannot confirm our main hypothesis via Hypotheses 1 and 2. In order to further investigate what drives the higher *Roll* rate in [*Com*|*Rev*] in comparison to

[17]This difference is not driven by one or two sessions, but occurs in all sessions of both communication treatments. It is only marginally significant if we consider *Com1* and *Com2* separately (see the appendix).

[*Com*|*NoRev*], we examine the behavior of subjects sending a promise separately and compare it between conditions, thereby accounting for the different number of promises.

If the combination of revelation and communication drives the higher *Roll* rate in [*Com*|*Rev*], the share of promise keepers should be higher in condition [*Com*|*Rev*] than in [*Com*|*NoRev*]. As shown in Result 2, revelation itself does not lead to a higher *Roll* rate in comparison to no revelation, hence image concerns of being perceived as selfish play a negligible role in our setting. This allows us to conduct a separate analysis on the set of Bs sending a promise and attribute a difference in *Roll* rates among promising Bs across conditions to the image concern of being perceived as a promise breaker.[18]

Result 5. *The share of Bs keeping their promise among Bs who give a promise is slightly higher in [Com|Rev] than in [Com|NoRev], however the difference is not significant.*

From Result 5 we conclude that the higher *Roll* rate in [*Com*|*Rev*] in comparison to [*Com*|*NoRev*] is mostly driven by the higher number of promises, and not by social image concerns of being perceived as a promise breaker. Table 3 reports the *Roll* rates for each type of message sent in both conditions.

Table 3: *Roll* Rates by Type of Message Sent in *Com*

		Promise	Intention	Empty
Com	*Rev*	41/48 85%	1/6 17%	3/17 18%
	NoRev	30/37 81%	4/13 31%	3/22 14%
	Z stat. (p-value)	0.534 (0.270)	-0.650 (0.258)	0.344 (0.365)

The Z Stat reflects the test of proportions for the two treatments or conditions (see Glasnapp and Poggio, 1985). The p-value is on one-tailed tests.

In *NoRev* already 81% of promising Bs stick to their word, which leaves little scope for further increase by revelation. Still, in *Rev* the share is even higher with 85%. Although the effect goes in the predicted direction, the difference is not large enough to be significant (Z=0.534, p=0.270).

Result 5 is further supported by probit regressions of the decision to choose *Roll*, which are reported in Table 4. Here, we categorize messages into promises and no promises, where we categorize intentions as "no promise", as Bs' behavior after having sent an intention is not significantly different from the behavior after having sent an empty message (see Table 3 and Section 3.2). Column 1 of Table 4 reports the results of *Com*, Column 2 of

[18] If there was a higher *Roll* rate in [*NoCom*|*Rev*] than in [*NoCom*|*NoRev*], this analysis would not be meaningful since we cannot compare the effect of revelation among Bs sending a promise in *Com* to the overall effect in *NoCom*. Therefore, we started off with considering overall *Roll* rates in *Com*.

NoCom and Column 3 (4) reports the results of a regression including both treatments with (without) controls.[19]

Table 4: Probit of Bs Decision to Choose *Roll*

	Coefficient (p-value)			
	Com	*NoCom*	All	All
Promise	1.742		0.808	0.715
	(0.000)		(0.000)	(0.000)
Rev	-0.059	-0.322	-0.382	-0.424
	(0.846)	(0.157)	(0.142)	(0.105)
Promise**Rev*	0.216		0.527	0.623
	(0.561)		(0.158)	(0.085)
NoPromise			-0.639	-0.535
			(0.116)	(0.233)
NoPromise**Rev*			-0.264	-0.347
			(0.464)	(0.346)
Risk	0.097	0.205	0.132	
	(0.064)	(0.039)	(0.003)	
Female	0.486	-0.235	0.259	
	(0.022)	(0.339)	(0.150)	
# of observations	131	60	191	191
# of sessions	11	5	16	16
Pseudo R-squared	0.314	0.106	0.240	0.212
Log Pseudo Likelihood	-61.519	-36.908	-100.433	-104.052

The regressions cluster on sessions. The reference category is *NoRev*, or [*NoCom*|*NoRev*] respectively. The sample consists of all Bs in all sessions, except of one session of *Com2*, which we exclude due to a lack of controls. Results (in column 4) do not change if we include the session. Results for *Com* and *NoCom* (columns 1 and 2) do not change when excluding the controls.

In all 4 regressions the dependent variable is B's decision, represented by a dummy variable which takes the value 1 if B chooses *Roll* and 0 otherwise. Promise (NoPromise) is a dummy variable for sending a (no) promise in *Com*, *Rev* is a dummy for the condition *Rev*, and Promise**Rev* (NoPromise**Rev*) is an interaction dummy of the two. In Column 1-3, we also include two controls, a measurement of risk and a female dummy.[20]

We observe that the probability to choose *Roll* is significantly higher if B sends a promise

[19] Due to a lack of controls we excluded one session of *Com2*. Results for *Com* and *NoCom* do not change when excluding controls and/or including the excluded session. Results in Column 4 do not change when including this session either. Moreover, the results are robust to OLS regressions.

[20] We elicited risk preferences through subjects' self-assessment on a scale from 0 to 10, with 0 indicating that a subject has a very weak willingness to take risks, while a score of 10 means that a subject has a strong willingness to take risks. Dohmen et al. (2011) show that this general risk question is a good predictor of actual risk-taking behavior.

(p=0.000) than if he sends another message or does not communicate.[21] However, the coefficient of the interaction dummy Promise**Rev* is far away from being significant (p=0.561), indicating that the probability to choose *Roll* when having sent a promise is not further increased by revelation. Moreover, *Rev* does neither have a general significant effect in *Com* (p=0.846) nor in *NoCom* (p=0.157). As shown by the non-parametric test, if anything, revelation without communication even leads to less cooperative behavior as the coefficient of *Rev* in Column 2 is negative and the p-value is not far from being marginally significant.

In column 3 we report the results of the probit regression including both treatments. The reference category is a subject in [*NoCom*|*NoRev*]. In order to account for the different number of promises in the two conditions, we separate the subjects in *Com* into promisers and non-promisers and include a dummy for each group. Thus, NoPromise only takes the value 1 for `B`s not promising in *Com* and it is 0 for subjects in *NoCom*. Altogether, we have 6 categories, with [*NoCom*|*NoRev*] being the base case including dummies for all other cases. Similar to *Com*, we observe that `B`s sending a promise have a higher probability to choose *Roll* (p=0.000). The coefficients of *Rev* and and NoPromise**Rev* are not significant, showing that revelation does not change behavior when no promise has been sent. The coefficient of Promise**Rev* is positive, but not significant (p=0.158). Still, it becomes marginally significant (p=0.085) when excluding the two control variables.[22] It seems that *Rev* marginally increases the probability of choosing *Roll* conditional on sending a promise, yet, the effect is very small and not robust. Thus, we are not able to prove our main hypothesis.

3.2 The Effect of Communication on Cooperation

One major reason for our effects being only marginally significant might be that in [*NoCom*|*NoRev*] already 81% of all `B`s sending a promise stick to it, restricting the scope for further increase in promise keeping with revelation.

We started off with conducting treatment *Com1*, where `B`s choose between a promise, a statement of intent, and an empty message, and observe a very high promise keeping rate even without revelation. In order to achieve more promise breaking in the baseline without revelation, we conducted a second communication treatment, *Com2*, allowing `B`s to choose only between the same promise and two empty messages. Thereby, `B`s attempting to influence `A`s while planning to take the non-cooperative decision are forced to break a promise.[23] However, this change in the set of messages failed to generate a

[21]However, the causality is not clear. `B` might send a promise as he knows he will choose *Roll*, or he might choose *Roll* due to the promise sent. We will address this point in Subsection 3.2.

[22]Results for column 1 and 2 do not change when excluding risk and female.

[23]`As` are only informed that there are three messages to choose from, but that they are unaware of the

higher rate of promise breaking in the baseline, such that we are not able to confirm our main hypothesis in *Com2* either.[24] Yet, the comparison of *Com1* and *Com2* reveals some interesting findings about the the effect of communication on cooperation and the differences between promises and intentions, which we will address in this section.

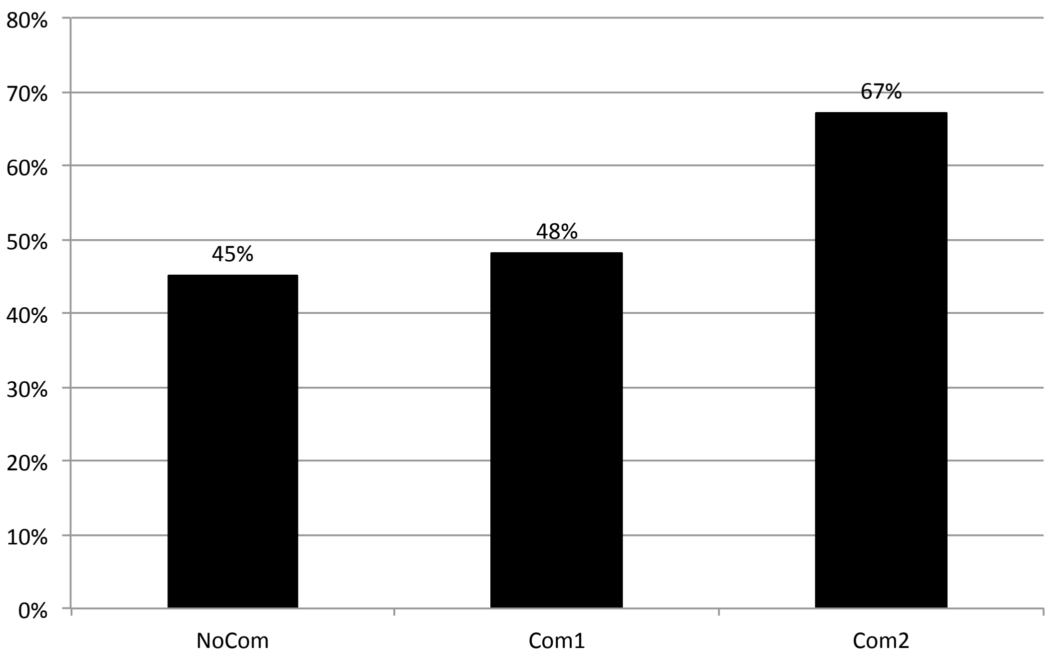
Bs' Behavior and the Choice of Messages

Considering Bs' behavior, it turns out that the set of messages available to B highly influences the effectiveness of communication on cooperation. In the following analysis, we pool the data of *Rev* and *NoRev*, as there is no significant difference in Bs' behavior between both conditions.

Result 6. *While the share of Bs choosing Roll in Com2 is significantly higher than in NoCom, the share in Com1 is not.*

We conclude that the possibility to send an intention in *Com1* constrains the effectiveness of communication on cooperation. Figure 4 illustrates the shares of Bs choosing Roll in all 3 treatments.

Figure 4: *Roll* Rates of Bs Separated by Treatment



In *Com2* 67% of Bs choose *Roll*, which is significantly higher than the share of 45% in *NoCom* (Z= 2.502, p=0.006), and than the share of 48% in *Com1* (Z=2.270, p=0.012). In contrast, Bs in *Com1* are as likely to cooperate as Bs in *NoCom* (Z=0.330, p=0.371).

type of messages or the wording. This was complete information.

[24]Note that the unchanged communication behavior of Bs allows us to pool *Com1* and *Com2* to *Com* in the analysis of Section 3.1.

In order to identify the driving forces behind these effects, we analyze the data separated by types of messages. Table 5 reports the shares of Bs sending each of the three types of message separately and the corresponding shares of Bs choosing *Roll*.

Table 5: Overview of Messages Sent and Subsequent Behavior

	Messages sent			
	Promise	Intention	Empty	Total
Com1	34/71 (48%)	19/71 (27%)	18/71 (25%)	71 (100%)
Com2	51/72 (71%)	–	21/72 (29%)	72 (100%)

	Shares choosing *Roll*			
	Promise	Intention	Empty	Total
Com1	26/34 (77%)	5/19 (26%)	3/18 (17%)	34/71 (48%)
Com2	45/51 (88%)	–	3/21 (14%)	48/72 (67%)

The sample consists of all B-Persons in *Com1* and *Com2*.

Result 7. *The share of Bs sending a promise is significantly higher in Com2 than in Com1, where roughly one quarter of Bs choose to send an intention. While the majority of promises is kept, the majority of intentions is broken.*

While 71% of Bs send a promise in *Com2*, only 48% do so in *Com1*, with the difference being highly significant (Z=2.794, p=0.003). In *Com1* 26% of Bs send an intention, which is not possible in *Com2*. In both treatments the majority of promises are kept, in *Com1* 77%, in *Com2* even 88%. In contrast, only 26% of Bs sending an intention stick to it. This share is significantly smaller than the share of Bs keeping their promise (*Com1*: Z=3.554, p=0.000; *Com*: Z=5.083, p=0.000), but not significantly different from the share of cooperating Bs conditional on sending an empty message (*Com1*: Z=0.713, p=0.238; *Com*: Z=0.997, p=0.160).

Note that the share of promises being kept in *Com2* (88%) is even marginally significantly higher than in *Com1* (77%) (Z=1.433, p=0.076), although more Bs send a promise in *Com2* than in *Com1*. This observation, together with the fact that most intentions are broken, yields the following result.

Result 8. *Not sticking to an intention seems to be less costly than breaking a promise.*[25]

Result 8 can be caused by the diction of the message itself or by the comparison to message alternatives, indicating that breaking an intention is not the strongest lie. However, such

[25]This result does not follow from the mere observation that promises are kept and intentions are broken in *Com1* as this might be caused by selection into messages (altruistic subjects send a promise and selfish subjects send an intention). However, the fact that a higher share of promises is sent *and* kept in *Com2* than in *Com1* yields the result.

a difference in behavior when sending a promise compared to sending an intention does not seem to occur in CD (2006), who use free-form messages. Therefore, we suggest that the latter reason is more likely to explain the observed phenomenon. Subjects who send an intention might not think "I am indicating to my partner that I will choose Roll", but more likely "I did not promise anything". Thus, unused alternatives seem to play a role, not as a signal to others, but as a self-justification device to behave selfish.[26] To conclude, the set of messages available to subjects in an experimental setting seems to play a crucial role for their behavior.

Finally, the fact that *Roll* rates after sending an intention are not significantly different from *Roll* rates after sending an empty message, but are significantly different from *Roll* rates after sending a promise in *Com1* speaks against the relevance of guilt aversion in our setting. `B` knows that `A` is not aware of the different available messages. Therefore, he should anticipate that both, a promise and an intention message, increase `A`'s first-order belief in comparison to receiving an empty message. Guilt aversion would predict a more cooperative behavior upon sending an intention than upon sending an empty message, and a similar behavior upon sending a promise or an intention.[27] In turn, promise keeping per se, as suggested by Vanberg (2008), is likely to play a role in our experiment, given that a high share of `B`s stick to a given promise even if their action choice is not observable.

`A`s' Behavior

As `A`s do not know which messages can be sent by `B`s, we pool *Com1* and *Com2* to *Com* for the analysis of `A`s' behavior. We do not differentiate between conditions either, as `A`s do neither know about the existence of two conditions nor does the experimental design vary across conditions from `A`'s point of view.

Table 6 gives an overview of `A`s' behavior in *Com* and in *NoCom*. For *Com* we report the overall behavior (total) and separated by the message received.

Result 9. *The share of `A`s choosing In is increased by communication for all kinds of messages. Furthermore, `A`s are equally more likely to cooperate after receiving a promise or an intention than after receiving an empty message.*

We observe a large effect of communication on `A`s' behavior. The share of `A`s choosing *In* increases significantly from only 35% in *NoCom* to 71% in *Com* (Z=4.833, p=0.000). This effect is driven by both promises and intentions. After receiving a promise, 79% of `A`s choose *In*, and after receiving an intention 74% do so (Z=0.488, p=0.313). These

[26] It would be interesting to test whether the share of people sticking to an intention, if the only options are an intention or two empty messages, is similar to the share keeping their promise in Com2.

[27] We cannot directly test for a difference in second-order beliefs as we ask for averages. We can only compare second-order beliefs across *Com1* and *Com2*. These are not significantly different (MWU, 2-sided, p=0.995).

Table 6: A's Behavior

	NoCom	*Com*			
		Total	Promise	Intention	Empty
In	21 / 35%	102 / 71%	67 / 79%	14 / 74%	21 / 54%
Out	39 / 65%	41 / 29%	18 / 21%	5 / 26%	18 / 46%
	60 / 100%	143 / 100%	85 / 100%	19 / 100%	39 / 100%

The sample consists of all As.

two shares are (marginally) significantly higher than the share of As choosing *In* after receiving an empty message, which amounts to 54% (empty vs. intention: Z=1.450, p=0.074; empty vs. promise: Z=2.845 p=0.002).

Interestingly, the share of As choosing *In* after receiving an empty message is significantly different from the respective share in *NoCom* (54% vs. 35%, Z=1.854, p=0.032). It seems as if As receiving an empty message might not have considered the possibility of a promise or an intention, and react to a friendly, though meaningless message.

The difference in As' behavior across treatments is reflected by their first-order beliefs about Bs' behavior. While without communication As believe that on average 45% of Bs choose *Roll*, this belief amounts to 58% with communication (MWU, 2-sided, p=0.001).[28] Hence, similar to CD (2006), we observe that communication increases As' first-order beliefs, thus enhances trust among As.

Does Communication Enhance Mutual Cooperation?

We observe a significant increase of mutual cooperation in *Com* compared to *NoCom*, represented by the share of pairs choosing (*In, Roll*) in each treatment (45% vs. 13%, Z=4.270, p=0.000). These shares, reported by type of message sent, are stated in Table 7.

Table 7: Shares of Pairs Choosing (*In, Roll*)

NoCom	*Com*			
	Total	Promise	Intention	Empty
8/60	64/143	57/85	3/19	4/39
13.3%	44.8%	67.1%	15.8%	10.3%

Result 10. *Communication increases mutual cooperation. However, while promises increase the share of pairs choosing (In, Roll), intentions do not.*

[28] In particular, the average first-order belief is 63%, conditional on receiving a promise, 57% conditional on receiving an intention and 47% conditional on receiving an empty message. The first order belief is significantly higher after receiving a promise than after receiving an empty message (MWU, 2-sided, p=0.001), however not significantly higher than after receiving an intention (MWU, 2-sided, p=0.179).

While promises lead to a very high cooperation rate, intentions do not. This difference is mainly driven by the fact that Bs keep their promises, but break their intentions, while As trust both.[29] We conclude that the set of messages available to B plays a crucial role for the effectiveness of communication in experimental settings.

4 Comparison to Previous Research

The present experimental design is based on the work by Charness and Dufwenberg (2006), who analyze the effect of free-form communication on cooperation. While their design informs A only about her payoffs, we vary the revelation of B's action choice in order to test for social image concerns. However, if we restrict our data to the *NoRev* condition, we find largely different results. In this section, we therefore analyze these discrepancies to the work by Charness and Dufwenberg,[30] incorporating their follow-up treatment with predefined messages (Charness and Dufwenberg, 2008 and 2010; henceforth CD, 2010). In CD (2010), Bs could choose between sending a sheet saying "I promise to choose *Roll*." or an empty sheet, which is closest to our *Com2* treatment without revelation. Apart from the communication protocol, our design differs from CD (2006) and CD (2010) only in B's relative payoff for choosing *Don't Roll*, which we slightly increased in order to reduce *Roll* rates without communication (see Section 3.2).[31]

Considering Bs' *Roll* rates, we do not find any difference between *NoCom* (53%) and *Com* (51%) in the *NoRev* condition (Z=0.179, p=0.429). Though slightly more Bs cooperate if we restrict the sample to *Com2* (61%), there is still no significant difference to *NoCom* (Z=0.637, p=0.262).[32] Similarly, communication fails to significantly influence Bs' behavior in CD (2010) either. While in their experiment the average *Roll* rate increases from 44% without communication to 58% allowing for predefined messages, this difference is only marginally significant on a one-tailed test (Z=1.339, p=0.090).[33]

In contrast, Bs in CD (2006) are significantly more likely to choose *Roll* after free-form communication than without communication (44% vs. 67%, Z=2.083, p=0.019). At first glance, this indicates that Bs feel more committed to a free-form promise than to a predefined one, yielding an increase in *Roll* rates in CD (2006). However, while in

[29]This might have been different, if As had been aware of the messages available to B (compare Charness and Dufwenberg, 2010).

[30]More precisely, we only use the (5,5) treatment for comparison as it reflects our payoff structure.

[31]Furthermore, Charness and Dufwenberg (2006 and 2010) conduct a pen-and-pencil experiment in the classroom while we use the laboratory and computer screens. However, as we can not identify any idiosyncratic effect of this design feature, we neglect it in the following analysis.

[32]Note that the difference was significant pooling *Rev* and *NoRev* (Section 3.2), but we restrict the sample to *NoRev* here.

[33]Results considering the whole sample in the communication treatment are only reported in CD (2008).

CD (2006), 57% of Bs send a promise in the communication treatment, we only observe 51% in *Com* and 39% in *Com1*, the latter difference being almost marginally significant on a two-tailed test (*Com*: 51% vs. 57%, Z=0.594, p=0.552; *Com1*: 39% vs. 57%, Z=1.608, p=0.108, two-tailed tests).[34] Hence, the higher promise rate in CD (2006) might also account for part of the increased effect on cooperation.

It is striking that, though CD (2010) find that 85% of all Bs send a promise, which differs statistically from our promise rate in *Com2* (64%, Z=2.293, p=0.022, two-tailed test), their effect of communication on cooperation is only marginally significant. Compared to our result, it seems that promises in CD (2010) induce less commitment among Bs. Indeed, while in *Com2* 87% of all Bs who send a promise keep it, this share is significantly lower in CD (2010) (61%, Z=2.183, p=0.029, two-tailed test).[35] This might be due to the fact that the messages available to Bs are common knowledge in their design, while we leave As unaware of message choices, yielding many Bs to send a promise just in order to avoid the mistrusting signal of an empty sheet.

As to As' behavior, we do not find any evidence that predefined messages in our design dampen cooperation compared to free-form communication. In fact, *In* rates among As in our experiment achieve similar levels as in CD (2006) with free-form messages (71% in *Com* vs. 74% in CD (2006), Z=0.341). Furthermore, as cooperation among As is relatively low without communication in our setting (33%),[36] we observe a highly significant effect of communication on As' behavior (71% in *Com*, Z=3.520, p=0.000), exceeding the effect with free-form messages in CD (2006) (56% without communication vs. 74% with communication, Z=1.777, p=0.038). In contrast, predefined messages in CD (2010) do not induce As to choose *In* more often, if at all, *In* rates decrease (56% without communication vs. 52% with communication, Z=0.336).[37]

While this finding seems to be unintuitive at first sight, it shows that besides differentiating between free-form and predefined messages, subtle design differences can account for huge changes in the credibility of messages. First, while Bs in our experiment choose an empty message if no promise is made (and can not refuse to send a message), the only alternative to a promise in CD (2010) is an empty sheet. It might thus be the case that empty talk in our experiment, though through predefined messages, contains some general pleasantry, thus inducing As to cooperate more often in the present setting compared to CD (2010). Second, the explicit announcement in CD (2010) that promises are not binding might create a social norm reducing both self- and social image concerns

[34] Though in CD (2006), also intentions were classified as promises, we exclude intentions in *Com1* from the comparison. This is reasonable as Bs in our experiment break intentions more often than promises, and behave similarly after sending an intention as after sending an empty message (see Section 3.2).

[35] In contrast, the promise keeping rate in *Com* (81%) is similar to the one in CD (2006) (75%, Z=0.567, p=0.571, two-tailed test).

[36] Note that we restrict the sample to *NoRev* only.

[37] There is no effect despite the higher promise rate in CD (2010).

for non-cooperation among Bs, which in turn might be anticipated by As. Finally, As in our experiment are not aware of the kind of possible messages, while the exact wording and procedure is common knowledge in CD (2010). As an empty message thus signals uncooperative behavior by B in their setting and might induce As to opt out of the game, it is likely that some Bs in CD (2010) send a promise who would not have done so in other circumstances. If As anticipate this cheap-talk nature of promises, the credibility of a promise is reduced, which is why As seem to trust less in CD (2010) than in our setting. The fact that communication has a larger influence on Bs in our setting than with free-form messages in CD (2006) can only be explained by the strong wording of our predefined promise, as compared to the diverse statements of intent in CD (2006).

To summarize, Bs in our experiment as well as in CD (2010) do not seem to be influenced by communication, while in CD (2006), free-form messages increase *Roll* rates. In contrast, *In* rates in our setting highly increase with communication, with this effect being even stronger than in CD (2006), while messages do not influence As' behavior in CD (2010). Hence, starting from a slightly lower cooperation level without communication than CD (2006), we obtain a similar effect of communication on (*In, Roll*) rates, which is also highly significant (13% in *NoCom* vs. 40% in *Com*, 50% in *Com2*, $p<0.01$ in both cases, two-tailed test). In general, while messages are most influential when they are free-form, predefined messages have a larger impact in our experiment than in CD (2010). This might be due to very subtle changes in the communication protocol, such as A's unawareness of message wording or the possibility of empty talk. We conclude that the effect of communication is not robust to slight changes in the experimental design.

5 Conclusion

Non-binding communication is at the heart of many economic interactions, especially if cooperation cannot be contractually enforced, for example because writing fully contingent contracts is impossible or too costly, or because cooperation is not verifiable. Hence, we contribute to the literature exploring why and in which environments "cheap talk" can be influential in two-player trust games.

In this paper we experimentally analyze whether individuals stick to their promised action, in contrast to the rational prediction, due to the aversion of being perceived as a promise breaker. While we observe slightly more cooperation of the promising party if the receiver of the promise can observe its compliance, the results are not significant. We find that 81% of subjects stick to their promise, even if their action is not observable to their interaction partners.[38] On the one hand, this result limits the scope for a further

[38]This even exceeds the shares reported in CD (2006) and Vanberg (2008).

increase in cooperation with revelation. On the other hand, it highlights subjects' preference for promise keeping per se (Vanberg, 2008), which in our experiment seems to play a more important role than social image concerns.

We find that the preference for sticking to one's word does only exist for promises and not for statements of intent. While most of the promises are kept, statements of intent tend to be broken. In line with this result, we find that the set of available predefined messages yields different results regarding cooperation by the communicating party, the second mover. While the possibility to communicate increases cooperation by second movers if they can only choose between sending a promise or an empty message, communication has no effect on second movers' behavior if they have the additional option of sending a statement of intent. However, the receivers of messages trust both a promise and a statement of intent in the same way. This finding allows us to exclude guilt aversion as an explanation for promise keeping, as the communicating party seems to be aware that a statement of intent does influence his partner the same way as a promise, but still does not stick to it.

To the best of our knowledge, our study belongs to one of the first economic studies analyzing the combined effect of communication and social image concerns on cooperation, suggesting a high potential and the need for further research. While we fail to prove the existence of social image concerns in our anonymous experimental set-up, one should not transfer this finding to other settings. We rather want to point out the crucial role of the design of the experiment, when trying to identify such subtle behavioral patterns. Lifting anonymity (see e.g. Tadelis, 2011) might increase the relevance of social image concerns, just like repeating the game and allowing for reputation building.

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Appendix

Separated Results for ***Com1*** and ***Com2***

In all three tables the Z Stat. reflects the test of proportions (see Glasnapp and Poggio, 1985). The p-value is on one-tailed tests.

Table 8: Bs' Average *Roll* Rate by Treatment and Condition

		Treatment				Z Stat.
		Com1	*Com2*	***Com***	***NoCom***	(p-value)
Condition	***Rev***	19/35 54%	26/36 72%	**45/71** **63%**	**11/30** **37%**	2.468 (0.007)
	NoRev	15/36 42%	22/36 61%	**37/72** **51%**	**16/30** **53%**	-0.179 (0.429)
Z Stat. (p-value)		1.062 (0.144)	1.000 (0.159)	1.450 (0.074)	-1.292 (0.098)	

The statistics in the last column test for the difference between *Com* and *NoCom*.

Table 9: Overview of Messages Sent

		Promise	Intention	Empty
Com1	*Rev*	20/35 57%	6/35 17%	9/35 26%
	NoRev	14/36 39%	13/36 36%	9/36 25%
	Z stat. (p-value)	1.540 (0.062)	-1.805 (0.036)	0.069 (0.472)
Com2	*Rev*	28/36 78%	– –	8/36 22%
	NoRev	23/36 64%	– –	13/36 36%
	Z stat. (p-value)	1.296 (0.097)	– –	1.296 (0.097)
Com	*Rev*	48/71 68%	6/71 9%	17/71 24%
	NoRev	37/72 51%	13/72 18%	22/72 31%
	Z stat. (p-value)	1.975 (0.024)	-1.692 (0.045)	0.888 (0.187)

Table 10: *Roll* Rates by Type of Message Sent

		Promise	Intention	Empty
Com1	*Rev*	16/20 80%	1/6 17%	2/9 22%
	NoRev	10/14 71%	4/13 31%	1/9 11%
	Z stat. (p-value)	0.580 (0.281)	-0.650 (0.258)	0.633 (0.264)
Com2	*Rev*	25/28 89%	– –	1/8 13%
	NoRev	20/23 87%	– –	2/13 15%
	Z stat. (p-value)	0.257 (0.400)	– –	-0.183 (0.427)
Com	*Rev*	41/48 85%	1/6 17%	3/17 18%
	NoRev	30/37 81%	4/13 31%	3/22 14%
	Z stat. (p-value)	0.534 (0.270)	-0.650 (0.258)	0.344 (0.365)

General Instructions[39]

We welcome you to this experiment. Please read these instructions carefully and follow the instructions on your screen after the start of the experiment.

At the end of the experiment you will get paid according to your decisions and the decisions of the other participants, as described below. In addition, you will get a fixed payment of 4 Euro for your attendance.

During the whole experiment you are not allowed to talk to other participants, to use mobile phones, or to start other programs on your computer. If you disobey these rules, we have to exclude you from the experiment and from all payments. If you have any questions, please raise your hand. An experimenter will come to your seat to answer your questions.

During the experiment, we are not talking about euros but about points. Your payment will be calculated in points. At the end of the experiment your overall score will be converted to Euro, where

1 Point = 25 euro cents.

The experiment consists of two parts and a questionnaire. Part 1 will be explained below. Once all participants have finished Part 1, you will get the instructions for Part 2. A questionnaire follows after Part 2.

Instructions Part 1

At the start of the experiment, either role A or role B will be assigned randomly to each participant. You will be informed on your screen which role was assigned to you. One person A and one person B, respectively, form an interaction pair. The allocation is random and anonymous. No participant will get to know the identity of his partner during or after the experiment. Your payment in Part 1 depends on the decisions made within your interaction pair.

Decisions:
Each person A chooses between IN and OUT. If A chooses OUT, A and B get 10 points each. If person A chooses IN, the payments depend on B's decision. Every person B chooses between ROLL THE DIE and DON'T ROLL THE DIE. At the time of decision, Person B doesn't know whether A has chosen IN or OUT. But as B's decision is only relevant if A chose IN, every person B should make her decision under the assumption that A has chosen IN.

[39] Original instructions were in German and are available upon request. Passages occurring only in the communication treatments are indicated by [. . .].

If A chose IN and B chooses DON'T ROLL THE DIE, B gets 30 points and A 0 points. If A chose IN and B chooses ROLL THE DIE, B gets 20 points and rolls a die at the end of the experiment in order to determine A's payoff. If the die shows 1, A gets 0 points, if the die shows 2,3,4,5 or 6, A gets 24 points.

The following table summarizes the payments, depending on the decisions made within an interaction pair and the result of rolling the die.

Decisions	**Payoff A**	**Payoff B**
A chooses OUT	10	10
A chooses IN, B chooses DON'T ROLL THE DIE	0	30
A chooses IN, B chooses ROLL THE DIE, Die=1	0	20
A chooses IN, B chooses ROLL THE DIE, Die=2,3,4,5,6	24	20

Please note: Every participant with role B, regardless if she chose ROLL THE DIE or DON'T ROLL THE DIE, will roll a die at the end of the experiment, such that the role of the die won't reveal the decision made by B. The result of rolling the die however is only relevant for those interaction pairs, where A chose IN and B chose ROLL THE DIE.

[**Message:**
Before A and B make their decision, B has the opportunity to choose one of three predefined messages and send it to A.]

Bonus questions:
During the experiment every participant has the opportunity to earn extra points by answering bonus questions correctly. The earnings out of these bonus questions will be displayed separately at the end of the experiment. You will get more detailed information during the experiment.

Control questions:
Before the start of the experiment control questions will appear on your screen to check that you understood the instructions. When all participants have answered these questions correctly, Part 1 of the experiment starts.