

Strategic and Social Pre-Play Communication in the Ultimatum Game*

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Abstract

Pre-play face-to-face communication is known to facilitate cooperation. Various explanations exist for this effect, varying in their dependence on the strategic content of the communication. Previous studies have found similar communication effects regardless of whether strategic communication is available. These results were so far taken to support a social-preferences based explanation of the communication effects. The current experiment provides a replication and extension of previous results to show that different processes come into play, depending on the communication protocol. Specifically, pre-play communication in an ultimatum game was either restricted to non-game-related content or unrestricted. The results show that strategic, but not social, communication affects responders' strategies. Thus, the existing results are cast in a new light. I conclude that pre-play communication effects may be mediated by qualitatively different processes, depending on the social context.

Keywords: pre-play communication, bargaining, ultimatum game,
video experiment social norms, social preferences

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1 Introduction

Early experiments studying face-to-face bargaining have found high levels of cooperation compared to those observed in anonymous bargaining. Face-to-face communication was found to lead to close to 100% rates of agreement, whereas substantial disagreement rates are commonly observed in experiments which study anonymous bargaining behavior (Roth, 1995).¹ Such effects of communication on cooperation are not restricted to face-to-face communication, but have also been found for written communication across various games (e.g., Charness and Dufwenberg, 2006; Koukoumelis et al., 2009, see Crawford, 1998 for a survey of experiments on communication). In dictator games, where there are no efficiency gains to be obtained from cooperation, written communication still increase cooperation in the form of more egalitarian outcomes (Andreoni and Rao, 2011; Mohlin and Johannesson, 2008). Nonetheless, face-to-face communication is typically associated with stronger effects than those observed following written communication (Brosig et al., 2003, 2004).

Several explanations have been suggested in the literature for the effects of communication in general, and face-to-face communication in particular. These explanations vary with different elements of the communication, specifically the *direction* and *content* of the communication. In this paper, I focus on the nature of the content of communication as a way to distinguish between qualitatively different effects of communication in the ultimatum game. In order to disentangle possible underlying processes, the strategic content of the communication is manipulated by excluding any game-related discussion in one treatment, thus controlling for effects that depend on strategic communication. In line with previous research by Roth (1995), restricted and unrestricted discussions were found to have a similar effect on offers. Conversely, the responders' strategies significantly differed between the two

¹Roth (1995) draws this conclusion based on the available literature in experimental economics available at the time. The experiments that allowed face-to-face bargaining in at least one treatment are Nydegger and Owen (1974), Hoffman and Spitzer (1982, 1986), and Radner and Schotter (1989). The conclusion is further supported by more recent work on bargaining (Bohnet and Frey, 1999a,b; Brosig et al., 2004). None of the experiments used the structured ultimatum game studied by Roth (1995) and in the current experiment.

communication treatments, with responders willing to accept lower offers when communication was restricted. The responder behavior observed in the experiment reveals that the similar proposer behavior under the different communication protocols is driven by different processes. I conclude that different processes can come into play in different situations. Thus, although it is appealing to look for a unified explanation for the observed effects of communication in different situations, it is likely that different studies tap into different processes. In the following, I briefly review possible explanations for communication effects, before proceeding to motivate and describe the current experiment.

Start by considering the effects implied by the elimination of anonymity with face-to-face communication. First, once a player is identified, she has incentives to cooperate at a personal cost in order to build her reputation and reap the indirect benefits of cooperation in future interactions. Second, the elimination of anonymity implies identifiability of the ‘other’. Schelling (1968) suggested that “the more we know, the more we care.” Indeed, various experiments show that people become more generous towards others as they gain personal information about them (Bohnet and Frey, 1999a,b; Charness and Gneezy, 2008).² Accordingly, social preferences may alter as a result of increased empathy towards other players once they are observed.³ In the domain of helping behavior and charitable giving, this effect has been termed the ‘identifiable victim’ effect (e.g., Jenni and Loewenstein, 1997; Kogut and Ritov, 2005a,b; Small and Loewenstein, 2003). Some evidence on the effects of identifiability in a dictator game was provided by Burnham (2003), who manipulated identifiability in a one-sided manner by showing subjects pictures of their partners. Dictators were significantly more likely

²These effects depend on cultural background (Buchan et al., 2006). Other studies manipulated social distance without affecting the information about other players, with mixed results (e.g., Bolton et al., 1998; Bolton and Zwick, 1995; Dufwenberg and Muren, 2006; Hoffman et al., 1996, 1999; Johannesson and Persson, 2000; Rigdon et al., 2009; Small and Loewenstein, 2003).

³By social preferences I include any preferences over the payoffs or beliefs of other players (e.g., Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Fehr and Schmidt, 1999; Rabin, 1993). For example, the social context induced by the face-to-face communication may lead a player to prefer an outcome in which social welfare is maximized over an outcome that maximizes her own monetary payoff at a larger expense to others.

to donate half of their endowment both when they saw a picture of the recipient and when they themselves became identifiable, suggesting that a single process is not enough to explain the potential of communication. Other experiments failed to find a significant effect of mere visual identification on the tendency to give (Bohnet and Frey, 1999b; Greiner et al., *ming*), although decisions were found to be idiosyncratically sensitive to the communication (Greiner et al., *ming*).⁴ The conflicting results can be reconciled through the findings of Kogut and Ritov (2005a,b), who showed that charitable giving increases with identifiability only for a single beneficiary.

The effects of identifiability discussed above exist regardless of any content of communication, as illustrated by Burnham (2003). Allowing for strategic game-relevant information to be exchanged as part of the communication raises a new set of possible effects. When the game has multiple equilibria, communication can facilitate coordination (e.g., Blume and Ortmann, 2007; Charness, 2000; Clark et al., 2001). In the ultimatum game, the use of threats by the responder can lead the players to play a Nash equilibrium which is not subgame perfect.⁵ However, this does not explain the effects of the content of communication on games with a unique equilibrium (e.g., Brosig et al., 2003; Charness and Dufwenberg, 2011; Dawes et al., 1977),⁶ and specifically in the non-strategic dictator game (Andreoni and Rao, 2011).

The content of the communication can alter the preferences of the players by modifying the social perception of the outcomes. One way in which this can be achieved is through guilt aversion, by which players wish to avoid ‘letting down’ their partners (Battigalli and Dufwenberg, 2007; Charness and Dufwenberg, 2006, 2011). Thus, a player who updates her belief about the expectations of her partner following strategic communication now has a new incentive to comply with the perceived expectations. Another motive iden-

⁴Visual identification also did not have an effect on contributions to a public good (Brosig et al., 2003).

⁵Note that such equilibria in the strategy-method variant of the game to be described below are not consistent with a responder’s belief that attributes a positive probability to the offer being lower than the equilibrium one. Indeed, this is the motivation behind perfection refinements (Selten, 1975). However, whether perfection refinements are normatively relevant is under controversy (e.g., Binmore, 1999).

⁶Introducing social preferences may result in new equilibria, see below.

tified in the literature is a tendency to keep promises made as part of the communication with a specific partner (Ellingsen and Johannesson, 2004; Vanberg, 2008). When expecting responses from the recipient, communicating dictators in a dictator game generally promise to be, and consequently are, generous (Andreoni and Rao, 2011). Strategic content can also make social norms salient, in such a way that they cannot be ignored (cf. Konow, 2000; Rabin, 1994). Thus, preferences are altered without any new information regarding the immediate partner. In line with this notion, Mohlin and Johannesson (2008) found that messages from recipients increase generosity in a dictator game compared to a no-communication baseline even if these messages originated in a previous game with different players. Nonetheless, the effect was stronger when the message came from the actual recipient that the dictator was playing with.⁷ Additionally, Andreoni and Rao (2011) found that receiving messages from recipients and writing messages as recipients before the role allocation is announced had a similar effect on dictator's donations.

Thus, the generous offers observed in the ultimatum game with pre-play face-to-face communication can be explained by different underlying processes. These processes diverge with regard to their dependency on the source of the messages (self, partner, third-party) and on the strategic content of the messages. The current paper aims to add to the understanding of the effects of pre-play communication by testing the effects of free communication versus communication that is restricted to non-strategic content. In the context of social dilemmas, Dawes et al. (1977) have previously studied the role of the strategic content of face-to-face communication by restricting communication in one treatment to non-game related content, to find no significant differences due to communication restriction. A similar approach was applied to the ultimatum game by Roth (1995). The experiment included three experimental treatments. In the control treatment (henceforth NO-COM), subjects played an ultimatum game using the standard protocol. i.e. the players did not communicate with each other and remained anonymous.

⁷However, only the difference between the donations in the baseline and the within-game communication treatments was statistically significant.

In the unrestricted communication treatment (henceforth UNR-COM), the two players could communicate for two minutes before playing the game. In the restricted communication treatment (henceforth RES-COM) the subjects similarly communicated before playing the game, but were not allowed to refer to the experiment itself during the face-to-face communication phase.

The results showed a significant decrease in ultimatum rejections (33% in NO-COM, 4% and 6% in UNR-COM and RES-COM, respectively) and an increase in mean offers (\$4.27 out of a pie of \$10 in NO-COM, \$4.85 and \$4.70 in UNR-COM and RES-COM, respectively). The increase in mean offers corresponded to the higher rates of equal split offers in UNR-COM, (75% compared to 31% in NO-COM and 39% in RES-COM). When offers around the equal split (\$4.50-\$5.50) were examined, high rates were observed in both communication treatments (83% and 82% in UNR-COM and RES-COM respectively, 50% in NO-COM). Focusing at the time on disentangling changes in social utility due to communication from coordination through strategic communication, Roth (1995) concluded that “the results cast doubts on the [strategic] communication hypothesis” (p. 298).

However, these results are not enough to reject the explanations that depend on the strategic content of the communication, as their predictions are fulfilled in the experimental data, even though they are not able to explain the results in RES-COM. In other words, all of the explanations delineated above predict that communication leads to more egalitarian offers by proposers. As they do not generate contradicting positive predictions, no results can be conclusive in terms of disentangling them.

Conversely, the responder behavior in the ultimatum game provides a better test for the processes associated with the strategic content of the communication. Effects of identifiability suggest that exposure triggers caring for the payoffs of others. Accordingly, responders should be less likely to hurt the proposers (as well as themselves) by rejecting offers. On the other hand, if the strategic content of the communication activates the egalitarian social norms, responders should be more likely to conform to these norms, as proposers do, i.e., reject low offers.⁸

⁸This is true for both the interpretation of social norms as focal points, which can be

It is important to note that the disagreement frequencies reported by Roth (1995) do not reflect the responders' strategies in a reliable way, as the responders are acting on vastly different offers in the different treatments. Since the proposers make generous offers in the two communication treatments, the likelihood to observe disagreement in actual play drops substantially regardless of the underlying strategy.

In the current experiment I use the strategy method (Selten, 1967) in order to elicit the full strategy vector of the responders.⁹ In other words, the responders choose whether to accept or reject each possible offer without knowing the actual offer made by the proposer. Although this turns the game into a simultaneous-moves game, the same treatment effects are to be expected as in the standard extensive-form game (Brandts and Charness, 2000, 2011).¹⁰ Thus, this experiment provides clean data with regard to the influence of pre-play face-to-face communication on the strategic behavior of players in the ultimatum game. If changes in social preferences due to identifiability indeed fully explain the results of Roth (1995), then the acceptance threshold in the two communication treatments should be similar, and equal to or lower than that in the control treatment. Conversely, if the proposer behavior in UNR-COM is driven by strategic communication, then the acceptance threshold in this treatment is predicted to be significantly higher than in the other treatments. The experimental data supports this interpretation of the previous findings. The remainder of the paper is organized as follows. The next section describes the experimental design and procedure. Section 3 discusses the experimental results, and section 4 concludes.

supported in equilibrium (Bohnet and Frey, 1999b; Schelling, 1960) and or as affecting preferences through considerations of self-image (Andreoni and Rao, 2011; Bodner and Prelec, 2003; Dana et al., 2007). Guilt aversion has no clear prediction, as the strategy of the responder has no effects on payoffs in equilibrium.

⁹This paper is a replication and expansion of the preliminary results reported in Schmidt and Zultan (2005). The new data allows for statistically significant results to be obtained, with somewhat altered conclusions and additionally investigates the effects of communication on the beliefs of the players.

¹⁰A meta analysis by Oosterbeek et al. (2004) shows that offers and demands are higher under a restricted strategy method, where responders provide an explicit acceptance threshold.

2 Experimental design and procedure

To provide control over the communication between subjects in the experiment, the experiment was conducted at the video laboratory of the Max Planck Institute for Economics in Jena. Sessions were conducted in March 2009 and 2010.

One hundred and twenty eight subjects played the ultimatum game with strategy method. In the game a pie of $p = 90$ ECU is to be allocated between the two players.¹¹ The proposer X chooses an offer x in multiples of 5 ECU, under the restriction that each player receives at least 5 ECU. Therefore 17 different offers are possible. Simultaneously, the responder Y indicates for each of the 17 possible offers whether she accepts or rejects it. I denote the response to a possible offer x by $y_x \in \{\text{accept}, \text{reject}\}$. If the responder receives the actual offer made by the proposer, the players receive their payoffs accordingly. Conversely, if the responder chooses to reject the offer made by the proposer, both players receive a payoff of zero. Thus, the payoff to the proposer is:

$$(1) \quad \pi^X = \begin{cases} p - x & \text{if } y_x = \text{accept} \\ 0 & \text{if } y_x = \text{reject}, \end{cases}$$

And the payoff to the responder is:

$$(2) \quad \pi^Y = \begin{cases} x & \text{if } y_x = \text{accept} \\ 0 & \text{if } y_x = \text{reject}. \end{cases}$$

I define the individual *acceptance threshold* \underline{y} to be the smallest amount that a responder is willing to accept, $\underline{y} = \min_{x: y_x=1} x$.¹²

The experiment includes three treatments, corresponding to the three

¹¹ECU stands for experimental currency units, which were converted into money at the end of the experiment at a conversion rate of 10 ECU = 1 Euro).

¹²Out of 64 subjects, 55 chose a monotonic strategy vector in all four periods (i.e., if $y_x = 1$, then $y_{x'} = 1$ for all $x' > x$). Three subjects rejected high offers as well as low offers in all four periods (cf. Bahry and Wilson, 2006; Güth et al., 2003; Hennig-Schmidt et al., 2008; Huck, 1999). Four subjects deviated from monotonicity in only one decision point across the four periods, probably due to a typing error. One subject chose a monotonic strategy vector in one periods, and rejected high as well as low offers in the

treatments of Roth (1995), in which the communication is conducted via video interface. To eliminate interactions between subject gender and communication treatment, only females were invited to participate in the experiment (Eckel and Grossman, 2001).

Four proposers and four responders participated in each experimental session. When showing up for the experiment, each subject was shown a list of the invited subjects and was asked whether she recognized any of the names on the list. A subject whose name was recognized by a subject who was in the other role received a 4 Euros show-up fee and was released. In order to guarantee that the proposers and the responders do not meet each other outside the experimental communication duration, the proposers and responders were invited in two cohorts, separated by 15 minutes. The proposers were led into the lab before any of the responders showed up, and were placed in insulated cabins. Each cabin includes, in addition to a computer terminal, a camera, a screen, a microphone, and a speaker. Once the responders arrived the experiment commenced.

First the subjects received the instructions for the communication phase and for the game.¹³ The instructions were (known to be) identical for all subjects in a given session. Four rounds of ultimatum game with pre-play communication were played throughout the session, so that each proposer played exactly once with each responder and vice versa. Each round included a communication phase and a game phase. In the communication phase each proposer was connected via video interface to the responder whom she was matched with in this round, so that the two players could see each other on screen and hear each other through the speaker. The subjects were then able to converse for a period of two minutes, after which the cabins were disconnected.¹⁴ In RES-COM, the experimenters monitored the conversation to check that the subjects do not discuss the game. In the control treatment,

other three periods, one of which includes one rejection point in the acceptance range. Only one subject chose seemingly arbitrary strategy vectors (in 3 of 4 periods), and was subsequently excluded from the analysis. This exclusion does not qualitatively alter the results.

¹³Translation of the German instructions is provided in the appendix.

¹⁴Unfortunately, due to technical problems only one session of UNR-COM and two sessions of RES-COM were taped, thus precluding analysis of the communication content.

the subjects had to wait for two minutes with no communication before the game phase commenced. Subjects in RES-COM typically discussed their field of studies and experiences as students. As expected, subjects in UNR-RES discussed possible allocations in the game, usually followed by some personal chat as in RES-COM. Following the communication phase, the subjects were asked to make a decision on screen. The proposers were asked to choose a division of the 90 ECU. The responders were presented with all 17 possible divisions on screen sequentially in random order, and they were asked to indicate for each one whether they accept or reject it. Once all 17 decisions were made, the responders were shown their decisions, and had the chance to make changes before making a final confirmation. No feedback regarding decisions and payoffs was provided until the end of the experiment. At the end of the last round, the subjects were asked about their beliefs regarding their partners in this round.¹⁵ The responders were asked to guess the division chosen by the proposer whom they interacted with in the fourth round, while the proposers were asked to guess the smallest amount that the responder whom they interacted with in the fourth round was willing to accept. For a correct guess a subject received an additional 5 ECU. The final payoff in the experiment was the payoff for one randomly chosen round (which was the same for all subjects in any given session), in addition to any payoffs for correct guesses in the fourth round, and a show-up fee of 4 Euros. Following the end of the last round, each subject was presented with the following information: the randomly chosen payoff round, the division chosen by the proposer in this round and the responder's corresponding decision, the subject's guess and whether it was correct, and the sum of ECU accumulated throughout the experiment. Next each subject who participated in one of the two communication treatments was shown a screenshot of the four subjects in the other role, and she was asked to rate each one of them on six scales corresponding to the semantic differential (Osgood et al., 1957), and to indicate whether she knew any of the subjects.¹⁶ These ratings did not

¹⁵Beliefs were elicited only after the final round to avoid an effect of the belief elicitation procedure on behavior.

¹⁶The options were do not know, have seen before, and know personally.

yield any interesting result, and are not discussed further.

Finally, the subjects were informed of their final payoff in the experiment. First the proposers were paid and released. Next the responders were paid and asked to wait for an additional 10 minutes before being released. In total, 128 subjects The sessions lasted approximately 75 minutes from the arrival of the proposers until the release of the responders (approximately 60 minutes for each cohort). The average earnings were 8.06 Euros for proposers and 7.82 Euros for responders. All earnings include a show-up fee of 4 Euros.

3 Experimental results

In the two communication treatments, subjects who are playing in the same role and in the same session communicate in different rounds with the same subjects who are playing in the opposite role. Despite the fact that the subjects never directly interact with each other and receive no feedback about game decisions throughout the experiment, this indirect communication may yet create dependencies in decisions.¹⁷ To account for this issue, I report in the following non-parametric tests conducted both on the session and on the individual levels.¹⁸ The non-parametric tests are complemented and supported by regressions that allow for the error terms to be correlated within the sessions.

Although offers in the control treatment were lower than those observed by Roth (1995, p. 297), the effects in the proposers' Data are qualitatively replicated; generally, communication induces higher offers and a higher rate of near-equal offers, leading to lower disagreement frequencies (although rejection rates are not lower in the first period. See Table 1).¹⁹ Average offers do not differ between UNR-COM and RES-COM ($p_{\text{SES}} = .313, p_{\text{IND}} = .579$,

¹⁷Note that this problem is absent in treatment NO-COM, and is mitigated in treatment RES-COM, where no game-related information is passed between the communicators.

¹⁸With five independent observations in NO-COM and UNR-COM and six independent observations in RES-COM, each comprised of four proposers and four responders. Significance levels are denoted by p_{SES} and p_{IND} when based on session and individuals, respectively.

¹⁹The rejection rates are presented in Table 1 for consistency with the presentation of Roth (1995).

Mann-Whitney test), although a slightly higher proportion of equal-split offers is observed in UNR-COM compared to RES-COM.²⁰ Recall that Roth (1995) interpreted these results as support for the social preferences basis of communication effects.

Table 1: Proposer behavior

Treatment	\bar{x}/p^a	N	Prop. of $x = 45$	Prop. of $40 \leq x \leq 50$	Rejection rate
All periods					
NO-COM	.365 (.114)	20	.09	.49	.18
UNR-COM	.472 (.051)	20	.66	.84	.06
RES-COM	.443 (.114)	24	.50	.79	.07
First period					
NO-COM	.378 (.124)	20	.15	.55	.20
UNR-COM	.475 (.061)	20	.45	.90	.30
RES-COM	.449 (.116)	24	.54	.83	.21

^a Standard deviations (based on N subjects) in parentheses.

Although the proposers' behavior replicates, by and large, that observed by Roth (1995), a different pattern emerges once the responders' strategy vectors obtained by using the strategy method are examined. Even though similar offers are observed in the two treatments, as in Roth (1995), responder strategies observed in the new data show that responders behave significantly less cooperatively in UNR-COM compared to NO-COM as well as RES-COM. This analysis focuses on the acceptance threshold \underline{y} extracted from the full acceptance vectors. Following Roth's (1995) analysis of offers, I look

²⁰This difference is significant if the dependencies between offers made by the same proposer are ignored ($p = .033$, Fisher's Exact test) and when the means of individuals are compared ($p_{\text{IND}} = .018$, Mann-Whitney test), but not when the session means are compared ($p_{\text{SES}} = .268$, Mann-Whitney test).

at the share of equal-split and near-equal-split acceptance thresholds.

Table 2: Responder behavior

Treatment	\bar{y}/p^a	N	Prop. of $\underline{y} = 45$	Prop. of $40 \leq \underline{y} \leq 50$	Prop. of $\underline{y} = x$
All periods					
NO-COM	.206 (.125)	20	.03	.06	.10
UNR-COM	.322 (.153)	19	.24	.47	.28
RES-COM	.238 (.150)	24	.05	.12	.06
First period					
NO-COM	.222 (.131)	20	.00	.10	.20
UNR-COM	.295 (.160)	19	.16	.37	.26
RES-COM	.243 (.151)	24	.04	.12	.24

^a Standard deviations (based on N subjects) in parentheses.

Table 2 summarizes the results pertaining to the acceptance thresholds. The mean acceptance threshold is significantly higher in UNR-COM than in both other treatments ($p_{\text{SES}} = .020, p_{\text{IND}} = .035$, Kruskal-Wallis test); whereas the mean acceptance threshold is not significantly higher in RES-COM compared to in the baseline ($p_{\text{SES}} = .580, p_{\text{IND}} = .528$, Mann-Whitney test), in contrast to the results obtained in the preliminary study reported in Schmidt and Zultan (2005). The same result is reflected in the higher proportions of equal-split thresholds in UNR-COM compared to NO-COM and RES-Com ($p_{\text{SES}} = .051, p_{\text{IND}} < .001$ and $p_{\text{SES}} = .102, p_{\text{IND}} < .001$, respectively, Mann-Whitney test).

Finally, the proportion of Nash equilibrium play under money-maximization (i.e. the acceptance threshold equals the offer) is higher in UNR-COM, when the players are able to explicitly coordinate on an equilibrium, than in NO-COM and RES-COM ($p_{\text{SES}} = .070, p_{\text{IND}} = .005$ and $p_{\text{SES}} = .023, p_{\text{IND}} = .000$,

respectively, Mann-Whitney test) In 17 of 21 (.81, 95% CI [.64-.98]) Nash equilibrium plays in UNR-COM, players coordinated on the equal split equilibrium. Conversely, equal-split equilibrium play occurred only 3 times in RES-COM and never in NO-COM.

The results in the first period are in the same direction as overall, but are weaker and noisier, and thus do not reach significance. The lack of statistical significance cannot be explained merely by the loss of power compared to the analysis based on subject means (which eliminate some of the within-subject variance) as the effects are pronounced and significant in the fourth period. The mean relative threshold in UNR-COM is significantly higher than in NO-COM in period 4 (.33 vs. .19, $p_{SES} = .012$, $p_{IND} = .008$, Mann-Whitney test) as well as than that in RES-COM (.33 vs. .23, $p_{SES} = .005$, $p_{IND} = .054$). The difference between NO-COM and RES-COM is not significant ($p_{IND} = .429$). Therefore some adaptation is taking place, as some responders in NO-COM become more accepting between the first and the last periods ($p_{IND} = .077$, Wilcoxon signed ranks test) while some responders in UNR-COM become more demanding ($p_{SES} = .042$, $p_{IND} = .016$). Since responders in NO-COM receive no feedback and do not engage in communication throughout the experiment, this adaptation can not be attributed to learning due to new information, but is more likely to be due to reflection (Iyengar and Schotter, 2008; Weber, 2003). The stronger pattern apparent in UNR-COM, however, suggests that there might be a cumulative effect of communication, for example, if one proposer has an effect on her responder partners that last for future periods (although it's easier to imagine that this has the opposite effect i.e., an effect of an aggressive bargainer rather than of a generous bargainer). Therefore I conduct an additional test, comparing the first-period thresholds in UNR-RES to the fourth-period thresholds in NO-COM. Since the time trend in NO-COM is not associated with any new information, this can be taken as an alternative test for the treatment effect. This test shows a significant difference ($p_{IND} = .030$, Mann-Whitney test).²¹

²¹One may argue that reflection leads to subgame-perfect equilibrium play (Weber, 2003), hence this test reflects the time trend more than the treatment effect. Recall, however, that the opposite trend is apparent in UNR-RES.

Table 3: Linear and probit regressions of offers and acceptance thresholds by treatment and period

	Offer					
	Share of the pie ^a		$x = 45^b$		$40 \leq x \leq 50^b$	
	Coeff.	Robust Std. Err.	Coeff.	Robust Std. Err.	Coeff.	Robust Std. Err.
Intercept	.351 ^{***}	.017	-1.741 ^{***}	.220	-.126	.181
UNR-COM	.119 ^{***}	.027	2.432 ^{***}	.423	1.018	.618
RES-COM	.091 ^{***}	.022	1.647 ^{***}	.347	.894 ^{***}	.241
Period ^c	-.009 [*]	.004	-.227 [*]	.109	-.063	.048
UNR-COM x Period ^c	.008	.006	.404 ^{***}	.142	-.000	.133
RES-COM x Period ^c	.008	.004	.165	.127	.033	.057
N	256		256		256	
	Acceptance threshold					
	Share of the pie ^a		$\underline{y} = 45^b$		$40 \leq \underline{y} \leq 50^b$	
	Coeff.	Robust Std. Err.	Coeff.	Robust Std. Err.	Coeff.	Robust Std. Err.
Intercept	.193 ^{***}	.033	-1.701 ^{***}	.270	-1.913 ^{***}	.336
UNR-COM	.146 ^{***}	.039	1.136 ^{***}	.332	2.045 ^{***}	.384
RES-COM	.039	.034	.237	.416	.762	.423
Period ^c	-.009	.007	.206 ^{***}	.021	-.221	.139
UNR-COM x Period ^c	.020 [*]	.009	-.102	.070	.355 [*]	.156
RES-COM x Period ^c	.005	.007	-.090	.130	.221	.139
N	252		252		252	

^a Linear regression with robust standard errors clustered on sessions.

^b Probit regression with robust standard errors clustered on sessions.

^c Last period as baseline.

^{*}, ^{**}, ^{***} Significant on the $p < .05$, $p < .01$, and $p < .005$ level, respectively.

Table 4: Linear regressions of beliefs by treatment

	Offer			
	\hat{y}/p		\hat{x}/p	
	Coeff.	Robust Std. Err. ^a	Coeff.	Robust Std. Err. ^a
Intercept	.203 ^{***}	.020	.436 ^{***}	.019
UNR-COM	.172 ^{***}	.048	.035	.021
RES-COM	.103 ^{***}	.031	.043	.021
N	64		63	

^a Robust standard errors clustered on sessions.

*** Significant on the $p < .005$ level.

These results are further supported by the linear and probit regressions reported in Table 3. Most importantly, the mean offer is significantly higher in both UNR-COM and RES-COM when compared to NO-COM, whereas the combination of coefficients reveals no significant difference between the two communication treatments ($\beta = .029$, $RSE = .026$, $p = .285$). Furthermore, the two communication treatments do not exhibit a decline in offers over the four periods as in NO-COM. In contrast, the acceptance threshold in UNR-COM is significantly higher than in NO-COM, but no significant difference is observed between RES-COM and NO-COM. The combination of coefficients further reveals that the responders are more demanding under unrestricted compared to restricted communication ($\beta = .108$, $RSE = .021$, $p < .001$).

Table 4 presents linear regressions conducted on the beliefs collected in the fourth period. The responders are generally optimistic, with expected offers in NO-COM almost as high as in the communication treatments. As the actual offers in this treatment are rather low, only 10.0% of the expectations are correct, compared to 52.6% and 45.8% in UNR-COM and RES-COM, respectively ($\chi^2(2) = 8.583$, $p < .05$).

Somewhat incongruous with the actual behavior, demands expected by proposers in RES-COM are, on average, 10.3% higher in NO-COM. However, expectations are 6.9% lower than in UNR-COM), and less accurate (only 3 of 24 [12.5%] correct in RES-COM vs. 7 of 20 [35.0%] in UNR-COM,

$\chi^2(1) = 3.145$, $p < .10$). This surprising result suggests that proposers do not expect responders to become more cooperative following face-to-face communication, as implied by the social preferences hypothesis. Apparently, proposers who are observed feel to be more accountable to their partners and expect them to be more demanding as a result.

4 Conclusion

The experimental results provide strong support for the hypothesis that different processes are at play in the different communication treatments, and thus call into question the previous interpretation of communication effects in ultimatum bargaining. The experiment reported by Roth (1995), applying play method, could not uncover responders' strategies, as the observed disagreement rate was driven primarily by proposers' offers. Treatment comparisons of the responders' decisions were not indicative of responders' strategies, as the responders in separate treatments were faced with different decision tasks. Thus, the study of responder behavior can serve to reveal differences in behavior, thereby also illuminating the observed proposer behavior.

In the current study, the use of the strategy method enabled comparisons of responders' strategies in an informative way. The similar behavior observed by Roth (1995) under unrestricted and restricted pre-play communication is now shown to reflect different processes, as responders' strategies are notably sensitive to the (restriction of) strategic content of the communication.²² When strategic communication is allowed, responders become significantly more demanding compared to lack of communication or even social communication. This reflects a strategic effect of coordination on the egalitarian social norm, as can be seen by the relatively high proportion of plays that achieved coordination in UNR-COM.²³ The social norm can be interpreted as a Schelling (1960) focal point, in which case the high demands

²²The generality of this conclusion may be restricted to the female population from which the subjects were sampled.

²³Restricted communication lead to a lower-than-baseline mean acceptance threshold in Schmidt and Zultan (2005), suggesting an effect of identifiability. This difference was not replicated with the larger sample employed in the current experiment.

are part of a Nash equilibrium, or as a result of an intrinsic preference for complying with the social norms, which has previously been assumed for allocator decisions (Andreoni and Rao, 2011; Konow, 2000). The existence of similar effects in dictator games favors the latter interpretation. Conversely, when communication is restricted, no significant effect is observed for the responders' behavior (and coordination on a Nash equilibrium is almost never achieved). Thus, different processes are at play when the communication lacks strategic content. In this case, the results are in line with an identifiability-based process, as proposers become more generous when social distance to the responders is reduced.

To conclude, the results of this experiment suggest that pre-play communication effects may be the outcome of strategic and social-affective processes, as well as an interaction of both, depending on the protocol of the communication. Game-free social communication induces cooperative behavior through other-regarding preferences, while game-relevant strategic communication affects the way in which the players consider social norms. The influence of the protocol may come about by means of inducing a different framing for the interaction. When players make a decision following a bargaining discussion, they become more sensitive to the strategic structure of the interaction, whereas when the decision making follows a social interaction, the players become more sensitive to social cues. Although strategic communication may also increase the salience of social norms, this effect is mediated by the strategic- dependent dynamic of the communication.

This paper adds to the vast literature on behavior in ultimatum bargaining. I focus on the recipient behavior, which has received relatively little attention in previous studies, when compared to proposer behavior, possibly because “The Recipients’ action[s],... are easier to interpret” (Thaler, 1988, p. 197).²⁴ The existing studies stress the importance of fairness and adherence to social norms as a motivation for rejections. When the responders have incomplete information regarding the size of the pie, and hence regarding the size of a ‘fair’ offer, they are considerably less likely to reject an offer

²⁴Out of 75 results of the standard ultimatum game included in the meta-analysis by Oosterbeek et al. (2004), only 12 used the strategy method to collect responders’ strategies.

that is *potentially fair* (Güth and Huck, 1997; Mitzkewitz and Nagel, 1993). Huck (1999) has convincingly showed that responders reject offers that are perceived as unequivocally unfair, even if they are willing to accept lower offers that are potentially fair. The current study suggests that strategic communication exacerbate this adherence to social norms, by that increasing the cost that responders are willing to bear for not deviating from what they perceive to be fair. An interesting extension to this line of research would be to combine the design used in Güth and Huck (1997) with different protocols of communication.

The conclusions drawn here suggest that future study and application of pre-play communication should allow for different types of communication effects, and take into account the social context in which the communication takes place. Different social contexts apparently play a crucial role in determining the direction and amplitude of the effects of communication on social decision making.

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Appendix: Experimental instructions

GENERAL INSTRUCTIONS

Please read the following instructions carefully. The instructions are identical to all participants. The experiment consists of 4 rounds. In the experiment you can earn money. How much money you earn depends on your decisions and the decisions of the other participants. All money amounts will be stated in ECU (Experimental Currency Unit). 10 ECU equal to 1 €.

Eight participants participate in the experiment. The four participants in cabins 1-4 decide as type X. The other participants in cabins 5-8 decide as Y. The number of your cabin is on the cabin door. In each round one X interacts with one Y. During the following four rounds you do not interact with any participant twice.

At the end of the experiment, one of the four rounds will be randomly chosen for the calculation of your payoff. The amount you earn in this round will be calculated in € and paid in cash together with the 4€ showup fee. Participants X and Y will receive their money separately and will be shown out of the building separately. Since the X- and Y-participants have already been shown into the laboratory at separate times, you do not meet the participants of the other type at any time point.

Treatment UNR-COM

At the beginning of each round you have the possibility to communicate with the participant of the other type that you are matched with for 2 minutes by video conference. During this time the two participants can see as well as hear each other, and are free to talk about anything.

Treatment RES-COM

At the beginning of each round you have the possibility to communicate with the participant of the other type that you are matched with for 2 minutes by video conference. During this time the two participants can see as well as hear each other. They are, however, not allowed

to talk about the experiment. This will be controlled by us. In case anyone does not comply, they will not be paid.

Only female participants are participating in the experiment.

You will receive a separate page, which describes the exact procedure in a round.

ROUND INSTRUCTIONS

Each X/Y pair interacts via the computer per the following rules:

In each round, X proposes how a pie of 90 ECU should be divided between him and Y. X enters on-screen the share of Y (i.e. the remainder of the pie remains for X).

Y is not informed of this offer. Y states for all possible offers, whether he accepts or rejects. I.e. for $X=85$ and $Y=5$, . . . , $X=5$ and $Y=85$. These allocations will be presented in random order. At the end it is also possible to review and revise them.

The payoff is determined by comparing the offer of X with the corresponding decision of Y. If Y accepts, then X and Y receive each the amount proposed to him by X. If Y rejects, then both participants receive nothing. Therefore each decision of Y may be relevant to the payoff.

Control questions

With the three following questions we check that you have understood the rules. Please answer the questions as best you can. Before the beginning of the experiment, we will check that you have answered the questions correctly.

1. Imagine that X has offered Y 15 ECU, and that his offer was accepted.
How much will X and Y earn?

X earns _____ ECU Y earns _____ ECU

2. Imagine that X has offered Y 70 ECU, and that his offer was rejected.
How much will X and Y earn?

X earns _____ ECU Y earns _____ ECU

3. Imagine that X has offered Y 5 ECU, and that his offer was accepted.
How much will X and Y earn?

Y earns _____ ECU X earns _____ ECU