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Game Experiments with Communication

A dissertation submitted to the Doctoral School in Economics and Management in partial fulfillment of the requirements for the Doctoral degree (Ph.D.) in Economics and Management

Andrew Musau

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Advisor

Prof. Luigi Mittone
University of Trento

Doctoral committee

Prof. Luca Corazzini
University of Messina

Prof. Jochen Jungeilges
University of Agder

Ass. Prof. Matteo Ploner
University of Trento

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Introduction

Neoclassical economic theory is characterized by complex models of super rationality. Aumann (1997) summarizes five main objections to such models:

- From casual empirical observation (or introspection), it is apparent that most economic agents are not maximizers (i.e., they do not consider the entire choice set and pick the maximal element from it).
- Maximization problems are complex, and even if one were willing, one would in practice not be able to carry them out.
- Conclusions from rational analysis, in some cases, seem unreasonable even on the basis of introspection.
- Laboratory experiments show that people fail to conform to some of the basic assumptions of rational decision theory.
- Laboratory experiments indicate that the conclusions of rational analysis sometimes fail to adhere to "reality".

The final two points relate to the contribution of behavioral and experimental economics in challenging the neoclassical paradigm. For a long time, there was growing skepticism about the assumptions employed in neoclassical models, and ensuing conclusions. Justifiably, if economics as a social science is concerned with the investigation of human groups and individuals, then the science should make a reasonable attempt to get this right. Thus, part of the growth in popularity of behavioral and experimental economics is its endeavor not only at identifying anomalies in neoclassical models, but also introducing psychological realism into these models.[1] Specifically, behavioral and experimental economics has proposed and tested a variety of formulations (such as reciprocity and inequality aversion), that capture the social nature of preferences.

One important element in this research field is determining how communication affects behavior, taking into account that individuals are not merely self-interested, but also value outcomes that accrue to other individuals. In the utility maximization framework, communication is on the most part predicted not to influence outcomes, since agents primarily pursue their private interests. For example, in the trust game (Kreps, 1990), if the second mover promises to honor trust conditional on the first mover offering trust, then the first mover would not find the second mover's promise to be credible since keeping such a promise entails sacrificing some material payoff for the second mover. Yet, if we stop at this prediction, we learn nothing about the real world where communication plays a crucial role in maintaining the functioning of the economic system. The present thesis therefore aims to identify the incremental effect of communication on behavior of participants in game experiments, taking into account social preferences, structures of games, and underlying power hierarchies in a given game. It would be incor-

[1]For both general and specific discussions on whether models in economics and psychology should represent "real world" phenomena, refer to Rubinstein (2006), Ainslie (2012), and our own perspective (Musau, 2014b).

rect, however, to imply that the level of importance given to communication started with the rise to prominence of behavioral economics. Therefore, before summarizing our contribution in the thesis, we briefly summarize the role that information has played in economics, because a primary role of communication is to convey information.

The central role of information in economics is attested by the existence of an entire branch of microeconomic theory devoted to the study of how information affects economic decision-making and outcomes (information economics). The branch was largely inspired by Hayek (1945)'s study which explored the uses of information in ordering effective employment of societal resources. Hayek argued that a centrally planned system was bound to fail since information that was needed to achieve an optimal allocation of resources was "not given to a single mind". For him, the social planner's problem would be reduced to one of logic if he started from a given system of preferences and possessed all relevant information and knowledge of available means. Subsequently, the notion of information as an economic commodity arose, and a number of studies attempted formalizations of this idea into theory. Robert Aumann, for example, considered a prototype in which a finite number of information commodities are transacted in a competitive environment. However, this and similar modeling attempts violated many standard assumptions in microeconomic theory which are identified and discussed in Allen (1990), such as, the indivisibility of information structures, price dependency of preferences for information, and valuation of identical copies of the same information. In game theory, the role of information is also central. Non-cooperative games, for example, are analyzed in terms of what information players possess, and are subdivided into perfect information games and imperfect information games. The economic literature on perfect information games is vast, encompassing the areas of signaling and screening.

With this background in hand, we summarize the organization of the thesis: Chapter 1 begins by providing a survey of game experiments with communication, focusing mainly on bargaining and trust games. It also considers public-good games, matrix games, and coordination games, where either there is substantial, or a growing body of research on the effect of communication. The implementation of game experiments in economics is described in detail, and the standard solutions of games are derived. Generally, communication is shown to have a positive effect on increasing donations in dictator games, increasing rates of cooperation in bargaining games, trust games, and public goods games, and increasing coordination rates in coordination games when the interests of players are closely aligned. However, some important considerations that have an effect on the result include, whether communication is face-to-face or written, whether communication is one-sided or two-sided, and the content of the communication (for example whether participants discuss the game that is played, or whether they engage in social communication).

Chapter 2 is based on the working paper “Anticipated communication in the ultimatum game”, with Mario Capizzani (IESE Business School), Luigi Mittone (University of Trento, Department of Economics and Management), and Antonino Vaccaro (IESE Business School). The paper investigates whether strategic considerations crowd out anticipatory effects of communication in an ultimatum game. Previous studies in the dictator game show that anticipated verbal feedback induces altruistic behavior. When an allocator donates an amount of money to a recipient, and the recipient sends an anonymous written message after learning of the amount, donations are higher in relation to the standard (no-communication) condition. However, in the extended environment of the ultimatum game, in determining what proportion of endowment to offer, the proposer is not only concerned about her pro-social behavior, but must factor in the possibility of her offer being rejected by the responder. Such strategic considerations have been shown to matter, and in a study examining the effect of anonymity on social distance, it is observed that reducing social distance significantly increases donations in the dictator game, whereas in the ultimatum game, there is no significant effect on offers. We show that anticipated communication effects still persist in the presence of two-sided communication in the ultimatum game.

Chapter 3 is based on the working paper “Communication, sequentiality and strategic power: A prisoners’ dilemma experiment”, with Luigi Mittone. Building on the study in Chapter 2, the study identifies and implements an experimental design that examines the role of first-mover anticipated communication on the inter-player strategic power dynamics that exist in a symmetric simultaneous move prisoners' dilemma, and a sequential move investment game. One shot two-player sequential game experiments are characterized by an asymmetry in the observed payoffs of participants. In the ultimatum game, for example, the distribution favors first-movers, whereas in the investment game, it favors second movers. A comparison to sequential move games are symmetric simultaneous move games, which entail symmetry in actions and payoffs. We show that first-mover anticipated communication has a significant effect in inducing payoff asymmetries in symmetric games.

Chapter 1

A survey of game experiments with communication

This chapter considers the role of communication in bargaining and trust game experiments. In particular, it identifies the incremental effect of communication on the behavior of participants in these experiments. To draw analogies, public-good games, and matrix games (e.g., prisoners' dilemma, coordination games, and zero-sum games) experiments are also considered, but the treatment is more condensed. For each category of experiments, both the structure of the underlying game (or a version of the general class of games) and the standard game theory solution are presented. The latter provides a basis for comparison with the experimental results. We attempt as much as possible to provide a non-technical description of the games that we consider and their solutions, in order to better focus attention on the behavioral aspects of the results. The introduction begins by first defining communication and discussing issues that may arise if the experimenter wishes to study its effect in an experiment.

The Merriam-Webster dictionary defines communication as "the act or process of using words, sounds, signs, or behaviors to express or exchange information or to express ideas, thoughts, feelings, etc., to someone else." This definition implies that communication can be split into verbal communication and nonverbal communication. The first category may be further subdivided into oral communication (where spoken words are used e.g., face-to-face conversations, telephone conversations, voice over internet, radio, video) and written communication (where written words are used e.g., letters, reports, memos, faxes, emails, sms).[1] Non verbal communication on the other hand involves the sending or receiving of wordless messages (or any form of communication other than oral or written), including, sign language (gestures), body language, facial expression, tone of voice, posture, etc.

What emerges from the above definition is the broad nature of communication, and how implementing it in a laboratory setting may be problematic in some instances. A key feature of laboratory experiments is that they enable the investigator to achieve a controlled variation of a variable of interest whilst keeping other (background) conditions fixed. The assumption is that an experiment that is well designed and executed exemplifies a situation where statistical relationships between variables represent underlying causal

[1] We do not include telegrams in this category because the year is 2015.

relations (Guala, 2005). Communication in an experiment, thus, is what is referred to as a "structural variable" (Camerer, 2003, p. 75). A structural variable changes the structure of the underlying game (typically by adding a move in the extensive-form representation of the game) as contrasted to a descriptive variable that simply changes the way strategies are described.

To illustrate how communication may result in false cause-and-effect inference in a game experiment setting, we consider an example where the investigator wants to study the effect of communication on how much the first mover allocates to the second mover in an dictator game.[2] However, before the allocation is done, the second mover is given the opportunity to talk face-to-face with the first-mover. In this instance, there may be non-pecuniary influences on the first-mover's preferences that the experimenter introduces by allowing face-to-face communication. Suppose that the participants in this experiment, as in most studies, attend the same college and are able to identify each other once they meet face-to-face. As Camerer (2003, p. 37) observes,

> If the subjects know the identity of the person they are bargaining with, their knowledge may influence what they do for many reasons. They may like how the person looks and want to make them happy, or fear retribution or embarrassment if they make a stingy offer and see the person after the experiment.

[2]For the reader unfamiliar with the dictator game, the structure is presented in the immediately following section.

Therefore, the amount the first-mover allocates to the second-mover may take into account future encounters between the two-parties outside the laboratory. In such an instance, what may be influencing observed behavior may not be so much what is said between the first-mover and the second-mover and its result (i.e. the effect of communication), but the effect of identification.

Other potential false inference issues that may arise if the experimenter chooses face-to-face communication relate to the various levels of face-to-face communication. If visual identification alone results in a non-significant difference in contributions relative to when participants talk to each other face-to-face, then the effect is not as a result of the content of the messages that are exchanged. An experimental design which does not include a visual-identification-only treatment may erroneously attribute the effect of observed behavior partly on the content of the messages, when in fact this is not the case. Finally, it is noted that nonverbal responses at times contradict verbal communication and hence affect the effectiveness of messages. A famous example is the first televised US presidential debate between the then Sen. John F. Kennedy and vice president Richard Nixon. Don Hewitt, who produced and directed the debate observed that

> If you were watching television on the night of Sept. 26, 1960, you probably thought that the young Sen. John F. Kennedy had won that night's presidential debate. Yet if you heard the event on radio, Vice President Richard M. Nixon was the clear winner

(CBS News , October 3, 2012).

The medium of communication may influence perception, in this case the message-content having a dominant effect over radio than over television. How the experiment is designed to take into account these particular factors is therefore important in ensuring that the experimenter achieves a controlled variation of communication. An easy way out is to focus on written communication and maintain anonymity between experimental participants, yet this does not fully represent real-world environments where behavioral factors such as trust are important, e.g., in buyer-seller encounters, which in a large number of cases are face-to-face.

The research strategy that we implement involves identifying major behavioral factors that have attracted a great deal of interest from experimental economists, such as, altruism, reciprocity, and trust. We then critically review design features and results of existing experimental studies that look at the interaction between these factors and communication in games. Before assessing the state of the art, we first begin by describing the implementation of game experiments.

1.1 Implementation of game experiments in economics

In a typical game experiment in economics, participants, usually students pursuing a program of study at the college where the experimental laboratory is located, are randomly recruited from a pool of interested participants who previously indicated their intent to take part in such experiments. In a number of cases, the recruitment process may impose some restrictions with respect to some attribute, e.g., maintaining a female to male gender ratio of at least $4:6$. Laboratories often utilize in-house recruitment and selection procedures, but increasingly, open source recruitment softwares such as ORSEE (Greiner, 2004) are employed to aid in the organization and management of the process.

Upon entering the laboratory, a participant is given a set of written instructions relating to the experiment to read through. The written instructions describe the decision tasks that the participant faces and the interaction setting of the experiment. To ensure that participants have adequately understood the instructions, they are usually required to answer a set of control questions relating to the contents of the instructions. The participants are not allowed to participate in the experiment until they have correctly answered all the control questions. Written instructions play an important role in experiments in economics, but more so in game experiments. An assumption in non-cooperative game theory is that players possess common knowledge of the structure of the game, embodied in the common knowledge of rationality assumption (Aumann, 1976). The instructions therefore serve to familiarize participants with the environment of the game, and this is reinforced by the experimenter reading these instructions aloud at the start of an experimental session.

Most game experiments in economics are incentivized, i.e., participants are paid according to their payoffs in the experiment. This mechanism serves to achieve control over preferences. However, as outlined in the introduction, there are other external non-monetary factors that may influence preferences which experimental designs control for, one example being identifiability. Experiments therefore implement single-blind (information that could introduce bias e.g, identity of the matched counterpart is withheld from the experimental participants) or double-blind (withheld from the experimental participants and the experimenter) designs to suppress such non-pecuniary influences on preferences. Additionally, a large number of game experiments are held over a computer interface. Open source software such as z-Tree (Fischbacher, 2007) are commonly used to program and implement experiments. The usual experiment involves one or two populations of participants repeatedly playing a one-stage game over several periods. For pairs of participants playing several times, "reputation building" (or a repeated game effect) can result in behavior that markedly differs from the stage-game equilibrium that the experimenter wishes to study (Camerer, 2003). Therefore, matching protocols aim to have participants play with each other only once in each experimental session. The most common matching protocol implements no-repeat matching (the so-called "stranger design")[3].

1.2 Dictator and ultimatum games

The most widely studied of the experimental games, the ultimatum game, takes a very simple form. The first-mover (henceforth proposer) is asked to divide a fixed endowment, usually a sum of money, between herself and the second-mover (henceforth responder). The responder can either accept or reject the offer. Fig. 1.1 provides an extensive form representation of the game. The endowment is set equal to 10 and the amount that the proposer offers to the responder is denoted x, where $x \in [0, 10]$. The continuum of possible offers is represented by the shaded region in the figure. If the responder accepts the proposer's offer, then he gets a payoff of x, and the proposer gets $10 - x$. If the responder rejects the offer, then both players get a zero payoff.

The dictator game on the other hand can be viewed as an ultimatum game where the responder's option to reject an offer is removed. In effect, the game is a one-player decision task where only the proposer has a substantive

[3]The strictest form of the stranger design is termed "perfect stranger" design where a participant is matched with another participant at most once.

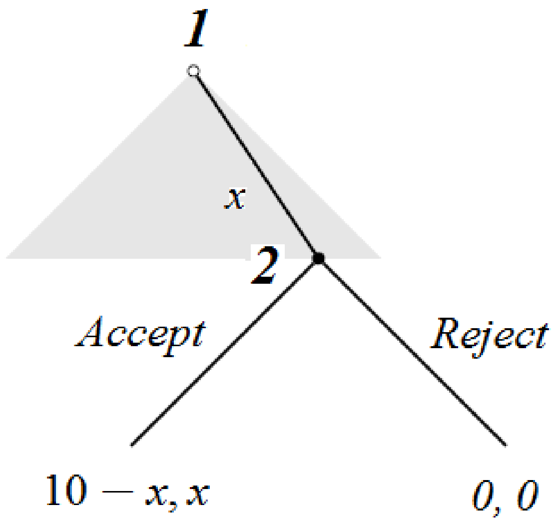

Figure 1.1: The ultimatum game

role.[4] Nevertheless, it is useful in identifying whether proposers' offers in the ultimatum game stem from purely altruistic preferences or are a result of the fear of rejection by responders.

Before reviewing the literature on behavior in ultimatum and dictator games with communication, we briefly summarize findings for these games without communication. For comparative purposes, it is useful to first consider what outcome predictions an analysis of these games would yield applying standard game theory. Selten's (?) notion of backward induction provides a simple method to analyze the ultimatum game in Fig. 1.1. How-

[4]Strictly speaking, the dictator game does not constitute a proper game in the pure game theoretic sense since the responder does not strategically influence the outcome of the game. To be a proper game, assuming a two-player game played by X and Y, the payoff of X should depend on the action (strategy) of Y. Similarly, the payoff of Y should depend on the action (strategy) of X.

ever, before one can apply the concept in search of the subgame perfect equilibrium (SPE), assumptions regarding players' preferences are needed. Standard game theory assumes that players maximize expected utility in games. Therefore, players have well defined preferences over outcomes, and these preferences are represented by utility functions defined over outcomes. They choose strategies that earn them the highest expected payoff considering the strategies of all other players in the game. What considerations enter a particular player's utility function in practice may not be fully enumerated, and standard game theory does not offer a guide as to what these considerations are.[5] Under standard theory, payoffs are exogenous and valued equally by all players in the game. In experiments which use money, a simple solution is to assume a one-to-one correspondence between utility and money. Figure 1.2 illustrates this assumption. Assume that the set of payoffs for each participant in an experiment involves four dollar amounts $\{\$1,\ \$2,\ \$3,\ \$4\}$ as represented in the figure. The assumption is that these amounts correspond to the utility set $\{1,\ 2,\ 3,\ 4\}$ where the following preference ordering holds: $u(4) \succ u(3) \succ u(2) \succ u(1)$. Thus, each player strictly prefers a higher dollar amount relative to a lower dollar amount, is self-interested, and maximizes monetary payoffs (since money corresponds to utility). We will refer to this model as the *money-maximizing* model.

[5]Attempts to explicitly explore considerations that enter into players' utility functions formed the basis of modern expected utility social preferences theories. It should be noted, however, that these theories were developed to explain observed behavior and are thus *post-experimental theories of social preferences*.

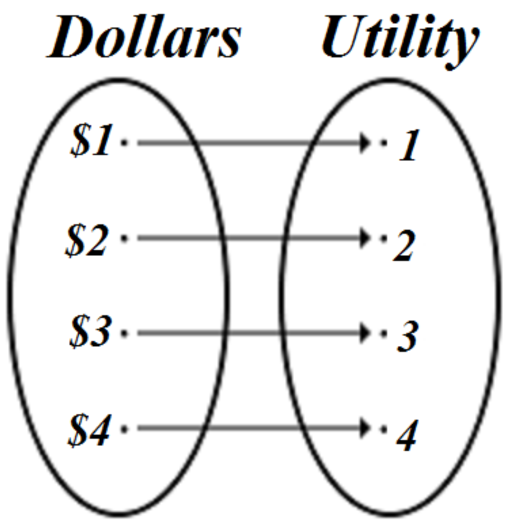

Figure 1.2: Money maximizing model:
One-to-one correspondence assumption

The purely selfish money-maximizing model predicts that the outcome where the proposer offers $x = \epsilon$, and the responder accepts is an SPE of the ultimatum game, where ϵ is the smallest monetary amount that can be offered (e.g. \$0.25 from a \$10 endowment). To see this, consider the following: If the proposer (she) offers $x = \$0$, she risks the responder rejecting the offer, since rejecting yields the same amount for the responder (he) i.e., \$0. The fact that the responder is self-interested implies that he does not consider the proposer's payoff when making his decision, and it does not matter whether the proposer ends up with \$10, if he accepts the $x = \$0$ offer, or \$0 if he rejects the $x = \$0$ offer. Thus, the responder will be indifferent between "accepting" or "rejecting" the offer and will toss a coin to determine which of the two options to choose. Tossing a coin yields an expected payoff of $\frac{1}{2}(\$0) + \frac{1}{2}(\$10) = \$5$ for the proposer. Could the proposer do better? Absolutely. She could offer $\$x = \epsilon$ (the smallest currency unit available) which the responder will immediately accept because $\$\epsilon > 0$. The proposer gets $\$(10 - \epsilon)$ (e.g., \$9.75 if $\epsilon = \$0.25$). Offers $x > \epsilon$ yield a lower payoff for the proposer. In expected payoffs terms, the outcome $(10 - \epsilon, \epsilon)$ yields the highest expected payoff for the proposer who would determine this using the backward induction process. She would subsequently use her first-mover advantage in the ultimatum game to achieve this allocation. In the dictator game, the money-maximizing model predicts that the proposer would keep the entire endowment since offers of $x > 0$ reduce the proposer's payoff.

Camerer (2003) compiles statistics from a large number of studies of ultimatum games and observes, contrary to predictions of the money-maximizing model, modal and median offers are mostly within 40–50 percent of the stakes on offer, and mean offers are 30–40 percent. In addition, there are very few offers of less than 10 percent, and of above 50 percent.

1.2.1 Dictator, ultimatum and other bargaining game experiments with communication

Rankin (2006) considers a dictator game experiment in which some responders can request some specific portion of a \$30 dollar initial endowment from proposers. He implements "anonymous" treatments and "face-to-face" treatments. Thus, in the anonymous treatments, responders fill in a reporting card, completing the sentence, "I request $US\$$...". The card is then forwarded to the proposer by a courier, and the proposer at no time knows the identity of the responder she is paired with. In the face-to-face treatment, the responder fills in the same reporting card, but delivers it himself to the proposer with the appropriate participant number. No other form of communication is allowed. In total, the experiment includes four treatments: anonymous-and-no-request, anonymous-and-request, face-to-face-and-no-request, and face-to-face-and-request. Rankin finds that mean requested amount by responders is higher under anonymous interaction relative to face-to-face interaction ($ 15.60 compared to $ 13.85). However, there is no significant difference in mean offers when requests are made anonymously or face-to-face ($ 7.90 and $ 8.10 respectively). Significantly, face-to-face interaction in the absence of a request results in the highest mean offer across all four conditions ($11.70) whereas the standard dictator game protocol of anonymous interaction and no request results in the lowest mean offer (only $1.89).

Mohlin and Johannesson (2008) investigate the effect of anonymous written communication on donation levels in the dictator game. In the experiment, the proposer has to decide how to allocate an endowment of SEK. 120 between herself and the responder. In the first of two communication treatments, responders send a written message to proposers prior to proposers deciding on the allocation, where there is no restriction on the content of the message. In the second treatment, the proposer receives a written message from a third party (a previous responder) before the allocation decision, and this is common knowledge. Mohlin and Johannesson find that written messages from responders to proposers increase the proportion of proposers who donate some amount of money relative to no-communication from 42 percent to 58 percent, whereas average donations increase from 12.73 percent to 22.13 percent respectively. When communication is from third parties to proposers, there is also an increase in the proportion of proposers who donate some amount of money relative to no-communication from 42 percent to 53 percent, and average donations increase from 12.73 percent to 18.18 percent. Therefore, Mohlin and Johannesson suggest that it is not the "relationship effect" of communication that is relevant in raising donations but the impersonal content of the communication.

Andersen *et al.* (2011) experimentally examine the effect of communication in a dictator game where either the proposer or responder can send a written message to the counterpart before the proposer decides on how to allocate a \$10 endowment between herself and the responder. The experiment includes four communication treatments: "Ask" – where the responder sends a written message along with a request for a given amount of the endowment to the proposer who reads it before making her decision; "Explain" – where the proposer sends a written message to the responder along with the allocation decision (responder stays silent); "Ask then Explain" – where the responder first sends a written message and a request for a given amount of the endowment and thereafter the proposer makes her decision accompanied by a written reply message to the responder; "Explain then Ask" – where the proposer first sends a written message along with a "non-binding allocation" to the responder who then responds with a written message and a numerical request, and then finally the proposer makes her decision. The base treatment is the standard protocol with no communication.

Andersen *et al.* find that the average allocation with no communication is $1.53 which is significantly lower than in all communication treatments except "Explain" where the mean allocation is just $0.65. Mean allocation rates are highest in the treatments where there is two-way communication i.e. "Ask then Explain" and "Explain then Ask" (over $ 2.50 in both treatments), and over $2.25 in the "Ask" treatment. They analyze the content of responders' communication and find that the model message is a request for the equal split of the endowment. Notably, in treatments where the responder has a voice (all 3 communication treatments except "Explain"), the mean allocation and proportion of equal split offers is significantly higher than in the treatments where the responder can not speak (baseline no-communication and "Explain"). Andersen *et al.* thus conclude that asking is powerful. However, there is a caveat to this result: Andersen *et al.* observe that asking for more than the equal split typically results in nothing in return. They conclude that communication has a significant influence on altruistic behavior, and this appears to mainly work by increasing empathy.

Bohnet and Frey (1999a) implement three treatment conditions in the standard dictator game: "anonymity", "mutual identification" and "communication". In the anonymity condition, participants play the dictator game under standard procedure. The mutual identification condition entails participants visually identifying each other but there is no verbal communication between them, whereas in the communication condition, participants interact face-to-face with verbal communication allowed. Proposers are given envelopes containing an endowment of CHF. 13 and decide how much to allocate to responders. Bohnet and Frey find that there is an increase in the proportion of endowment allocated to the responder from 26 percent under no-communication to 50 percent under mutual identification. The proportion allocated under communication is 48 percent, which is higher than the proportion under no communication but not significantly different to the proportion under mutual identification. Therefore, they conclude that silent identification (and not face-to-face communication per se) is sufficient to raise contribution rates in dictator games. In particular, Bohnet and Frey note that identification strengthens social or cultural propensities for fairness and decreases social distance, thereby allowing emergence of empathy for the counterpart.

Croson *et al.* (2003) modify the standard ultimatum game to allow responders to receive an outside option in case of rejection.[6] They consider a two-sided imperfect information setting: known or unknown amounts to be

[6] An outside option in the ultimatum game can be seen as potentially having two effects: it allows a responder to punish a proposer who makes small offers without the responder hurting himself, and it makes explicit this possibility to the proposer, who must then factor it in when making an offer.

divided (endowments), and known or unknown amounts of the outside options. Participants play the same role with the same partner initially for two rounds, and then a further two rounds, and responders are allowed to send a written message to proposers prior to the proposers making an offer. In turn, proposers have the option to reply (in writing) to the responders' messages in conjunction with their offers. The design allows lies about private information to be uncovered and to be punished. In some treatments, the size of either the outside option, the initial endowment, or both are revealed to proposers and responders after the second round. Croson *et al.* find that about half of the responder's communication includes a threat to reject an offer across all four rounds of the experiment, and both responders' and proposers' messages include a lie, averagely about slightly less than 20 percent of the time in the first two rounds (before revelation of private information) and about 10 percent of the time in the final two rounds.

Responders in the experiment threaten to reject small offers about 50 percent of the time, and despite the threats, proposers make smaller offers (relative to what is requested) in 30 percent of the cases.[7] Croson *et al.* classify short-run effects of communication and long-run effects. In the short-run, responder's lies about their outside options significantly increase the offers

[7]In game theory, a player who makes a threat that he cannot stand up to is referred to as making an *incredible threat*. For example, in the ultimatum game in Fig. 1.1, assuming that the monetary values represent utilities, if the responder threatens to reject an offer $x = \epsilon > 0$, then the proposer would consider this as an incredible threat since ϵ is greater than the responder's payoff of zero in case of rejection. The proposer would proceed to offer $x = \epsilon$, an act that is termed as "calling the threat" (similar to "calling a bluff").

that they receive (relative to no-communication) whereas responder's threats increase offers only marginally but significantly reduce the probability of acceptance. Proposers' lies on the other hand result in significantly lower offers and no change in the probability of acceptance. In the long run, responder revealed lies result in smaller but not significantly different offers relative to no-communication in the proceeding rounds (i.e. round 3 and 4), whereas proposer revealed lies result in significantly larger offers and a significantly higher likelihood of rejection of the amounts offered. A possible reason that Croson *et al.* attribute to the lack of an observed effect as it relates to responder lies is the small size of outside options relative to the possible gains from trade (i.e. the relative unimportance of the lies as it relates to their effect on overall dollar payoffs in the game).

Roth (1995) examines how a particular form of face-to-face communication influences cooperation in bargaining. In the baseline treatment, bargaining is anonymous and there is no communication. The two communication treatments differ only in terms of restrictions imposed on the content of the communication: In the restricted treatment, participants are allowed to communicate to each other face-to-face prior to the start of play but they are not allowed to talk about any aspect of the bargaining game. For a 2-minute time interval, groups of participants are thus required to familiarize themselves with the names of other members in the group, and the year of study but cannot discuss the bargaining game. In the unrestricted treatment, there are no restrictions on what participants can talk about.

They thus can discuss the bargaining game or anything else. Roth finds that "social" communication results in the same level of cooperation (measured in terms of agreement frequencies) as "game related communication" in bargaining games. Overall, communication results in higher rates of cooperation relative to no communication with agreement frequencies of over 90 percent in both communication treatments compared to only 67 percent under no communication. However, Roth further notes that the presence of game-related communication although not resulting in significantly different rates of cooperation compared to social communication may have affected the distribution of offers in the bargaining game. He finds that the percentage of equal-split offers are significantly higher under game-related communication relative to both social communication and no communication, but due to a small sample size, the relevance of this result is unknown.

Zultan (2012) considers a similar experiment to Roth (1995) by comparing the effects of face-to-face restricted (social) communication and unrestricted (strategic) communication in the ultimatum game. In the "restricted communication" treatment, participants mainly discuss their fields of study alongside their experiences as students via a video link for a 2 minute duration, but they cannot discuss strategies in the ultimatum game. In the "unrestricted communication" treatment, participants can discuss strategies in the ultimatum game or anything else via a video link for 2 minutes. The no-communication treatment entails the standard ultimatum game protocol, with a 2 minute wait time before the proposer can make an offer. The proposer endowment is 90 experimental currency units (ECU) and the strategy method is employed to elicit the full vector of the responder. Therefore, the proposer chooses one of 17 possible divisions of the 90 ECU, and for each division, without knowing the actual choice of the proposer, the responder has to indicate whether he accepts or rejects the division.

Zultan finds that communication results in significantly higher offers than no communication, with no significant difference between offers in the restricted condition and the unrestricted condition (over half of offers are at the 50:50 split in both the restricted and unrestricted conditions compared to only 9 percent under no communication). The result qualitatively mirrors proposer behavior in Roth's experiment. However, he finds that responders behave significantly less cooperatively in the unrestricted condition compared to both under restricted communication and no communication. The mean acceptance threshold, defined as the smallest proportion of the endowment that a responder is prepared to accept, is 32.2 percent in the unrestricted condition, which is significantly higher than in both the restricted condition and under no communication (23.8 percent and 20.6 percent, respectively). Zultan thus concludes that pre-play communication effects may be the outcome of strategic and social-affective processes, or an interaction of both. Strategic (game-related) communication affects the way in which the players consider social norms, whereas social (non game-related) communication induces cooperative behavior through other-regarding preferences.

Rankin (2003) considers the effect of communication in the form of written requests from responders in the ultimatum game. His two experimental conditions include "request" – where responders can request a specific amount of the endowment by filling a reporting card, completing the sentence, "I request $...", and "no request" – which entails the standard ultimatum game procedure without communication. Both treatments maintain anonymity between participants. The endowment to proposers is equal to $ 30. Proposers record their donations on recording cards, completing the sentence "My offer is $...", and these are forwarded to responders who decide on whether to accept or reject the offers. Rankin finds that offers and rejection rates in the no request condition are consistent with past results from ultimatum games. Averagely, proposers contribute $ 12.67 or 42 percent of their endowment, and 24 percent of offers are rejected by responders. In the request condition, the mean offer is $ 10.09 or 33.6 percent of the endowment, significantly lower than in the no request condition, and 35 percent of offers are rejected. He concludes that requests adversely affect average offers in the ultimatum game.

Brosig *et al.* (2004) investigate the effect of communication in two-stage sequential-bargaining game experiments which incorporate reputation and punishment. In these games, first-movers choose one of two decision nodes, and second-movers choose one of two options which decide final payoff allocations. The games are such that there is a conflict between the players inherent in the monetary payoff structure (first-movers have a preference for one Nash equilibrium in one subgame whereas the second-movers have a preference for another Nash equilibrium in a different subgame). Punishment arises since the second-mover has a chance to choose the strategy that yields a lower payoff for both players if the first-mover chooses her preferred subgame (as opposed to the subgame that favors the second-mover).[8] Concurrently, second movers can reward first movers for choosing the subgame preferred by the former (through choosing the efficient-equal split outcome), although this again entails second-movers sacrificing some of their material payoffs. Only the first-movers preferred equilibrium is subgame perfect.

Two pre-play communication treatments are implemented in the experiment: written (email) communication and face-to-face (via a video link) communication, alongside the base no-communication treatment. In the written communication treatment (as well as no communication), anonymity between participants is maintained. Further, Brosig *et al.* consider games with different efficiencies of punishment. They find that in the absence of communication, first-movers overwhelmingly choose the subgame perfect strategy, over two thirds of the time across all punishment efficiency conditions. Under face-to-face communication, there is a reversal: first-movers play their preferred subgame perfect strategy in only 13 out of 106 games (less than an eighth of the time), and second movers exploit first movers by not choosing the efficient equal-split outcome only in 4 out of 93 cases (less than 5 percent of the time). In the anonymous written communication treatments,

[8]Therefore, this feature of the sequential-bargaining game is similar to ultimatum bargaining since second-movers sacrifice some of their payoffs in order to punish first movers.

first-movers play their preferred subgame perfect strategy less than a quarter of the time, and the results generally mirror those under face-to-face communication (although the rates are higher in the face-to-face condition). Brosig *et al.* note that if reputation drove the results in the face-to-face condition, then behavior in the anonymous written communication treatment should not be different from behavior under no-communication. Since the results overwhelmingly reject this conclusion, they rule out reputation effects as a significant driver of the communication effect.

Greiner *et al.* (2012) address the incongruence between Brosig *et al.*'s result that lifting anonymity (via a video screen) does not enhance contributions and Roth's result that face-to-face communication induces near equal-split offers in ultimatum bargaining. They note that either experiment in isolation cannot disentangle the effect of strategic effects of non-verbal communication (and reputation concerns) versus changes in preferences triggered by personalized information, since the former may still be present in ultimatum and public good games, and mere visual identification may not be sufficient to trigger the latter. Therefore, Greiner *et al.* (2012) suggest studying one-way responder communication in a dictator game (to rule out strategic effects of communication), and introduce two responders into the game (to study the effect of discrimination between two responders). Therefore, an anonymous proposer decides how to split an endowment of 17 Euros between herself and the two responders. The experiments includes two communication treatments: "visual," - where the proposer can see both responders via a video link but without any audio channel, and "audio-visual," where the proposer can see both responders but can only hear one of them speak. The baseline is the standard dictator game protocol with no communication. Greiner *et al.* find that donations are on average 8 percent higher in the visual treatment compared to no-communication, whereas they are 14 percent and 28 percent higher for the non-talking and talking responder, respectively, in the audio-visual treatment. Thus, the findings replicate the result that mere visual exposure does not greatly affect social giving. In addition, Greiner *et al.* speculate that guilt aversion may have driven increased giving in the audio-visual treatment, noting that video messages may serve as a channel to influence the proposer's beliefs about the responder's expectation, thereby influencing contributions. Proposers' discrimination (differences in allocations between the responder pair) on the other hand significantly increases with the addition of the visual channel, thus providing evidence that proposers respond to the communication, even if they do not generally become more generous towards responders.

Valley et al. (2002) study the effect of communication in a buyer-seller bargaining experiment under incomplete information. Buyers and sellers draw values in the range $0 - \$50$ and subsequently bargain by stating bids. The experiment consists of seven rounds, and buyers are matched with a different seller at each round (perfect stranger matching). In the no-communication treatment, players are given two minutes to arrive at a bid. There are two communication treatments: written-communication and face-

face communication. In the former, players send written messages to each other (through a courier) for a 13 minute duration before submitting a bid. In the latter, players are allowed to talk to each other face-to-face, and communication is unrestricted, after which they return to separate rooms and submit a final bid. Valley et al. find that communication generally enhances the efficiency of trade. In the communication treatments, only one third of participants truthfully reveal their true values and about the same proportion actively lie about their value. Therefore, Valley et al.'s analysis shows that efficiency does not emerge from mutual truth telling (mutual revelation of values) but through a different mechanism: specifically, communication aids bargainers to coordinate on a single price that is common (one-price equilibrium), and this occurs approximately 40 percent of the time under written communication and approximately 70 percent of the time under face-to-face communication.

A detail that is hidden in the above description relates to differences between written communication and face-to-face communication. Bargaining in the written-communication treatment is more efficient relative to the no-communication treatment only when the potential gains from trade are small. When potential gains from trade are large, the opposite holds: bargaining in the no-communication treatment is more efficient than under written communication. Valley et al. show that the higher efficiency is due to coordination to the one-price equilibrium discussed above, whereas the lower efficiency is due to high rates of deception in the written communication treatment.

This is not the case when communication is face-to-face, where bargaining is more efficient relative to no communication both when the potential gains from trade are large and when they are small. This increased efficiency is achieved through high levels of coordination coupled with low rates of deception when parties interact face-to-face.

1.2.2 Other relevant dictator and bargaining game experiments without verbal communication

Because face-to-face communication entails visual identification between the communicating parties, it is also useful to explore experiments that manipulate identifiability, but where participants do not communicate verbally. For example, as we have argued in the introductory section, if the experimenter observes significantly higher mean contributions in a face-to-face communication treatment in the dictator game where participants communicate verbally, then in the absence of a treatment which only includes visual-identification (without any verbal communication), she cannot attribute the effect to verbal communication. Yet, there is a communication effect (since participants are communicating face-to-face). The issue here is one of definition, i.e., constituting what makes up face-to-face communication. Therefore one should consider an aggregate face-to-face communication effect, and further subdivide this into a visual identification effect and a verbal communication effect (or what is sometimes referred to as a "content effect"). Here, we review some experiments that have attempted to isolate and investigate the former effect.

Bohnet and Frey (1999b) vary the degree of social distance in a dictator game experiment by considering two identification treatments: "one-way identification" – where the proposer can visually identify the responder but the responder does not know the identity of the proposer; and "two-way identification" – where both the proposer and responder can visually identify each other. There is no verbal communication of any sort in both treatments. They compare behavior in these two treatments to that in the baseline anonymity treatment. Bohnet and Frey find that out of an initial endowment of CHF. 13, on average, proposers allocate 26 percent of the endowment to responders in the anonymity condition, compared to 35 percent under one-way identification and 50 percent under two-way identification. Two-way identification results in over two thirds of donations at the equal-split compared to 39 percent under one-way identification and 25 percent under anonymity. In an additional one-way identification treatment where proposers are provided additional information on the responders that they are paired with, mean contributions increase to 52 percent of the endowment, exceeding the mean contribution under two-way identification. Bohnet and Frey note that one-way identification excludes the potential for future sanctions, and thus "transforms anonymous, faceless entities into visible, specified human beings" (p. 339). Their one-way identification results rule out reciprocity and fairness reference points, and thus the relevance of these factors as the driving force in decreasing social distance and increasing contributions in the dictator game.

Bolton and Zwick (1995) test two hypotheses of why results in the ultimatum game experiments divert from the perfect equilibrium prediction. The first, labeled "anonymity hypothesis," posits that participants' money maximizing objective is distorted by the act of experimental observation. The second, termed "punishment hypothesis," contends that participants are willing to punish those who treat them "unfairly" independent of any experimenter influence. Due to what Bolton and Zwick refer to as cost and time considerations, they run a modification of the standard ultimatum game where proposers can choose either an equal allocation of a \$ 4 endowment at any period, represented by the payoff vector $(2, 2)$, or they can choose an unequal allocation of the same amount at experimental period t, represented by the payoff vector $h_t = (h_{t,1}, h_{t,2})$, where $h_{t,1}$ is the proposer's share of the endowment at t and $h_{t,2}$ is the responder's share at t satisfying $h_{t,1}+h_{t,2} = 4$ and $h_{t,1} > h_{t,2} > 0$. The experiment is run under several alternative values of h_t, e.g., $(2.20, 1.80)$, $(2.60, 1.40)$, $(3, 1)$, etc. If the responder accepts at a period t, then the chosen allocation is implemented, either $(2, 2)$ or $(h_{t,1}, h_{t,2})$. If the responder rejects, the payoff vector $(0, 0)$ is implemented. The experiment includes three conditions, (i) participant-experimenter anonymity absent, (ii) participant-experimenter anonymity present, and (iii) the game minus the responder's punishment strategy and participant-experimenter anonymity absent.

Bolton and Zwick find that across the last 5 periods of the game, the results from condition (i) qualitatively mirror those of standard ultimatum game experiments, for example, 50 percent of offers are equilibrium offers, 20 percent of offers are rejected by responders, and equal-divisions are never rejected. In condition (ii) where anonymity is present, 66 percent of offers are equilibrium offers, and 20 percent of offers are rejected by responders, so perfect equilibrium outcomes rise to 46 percent relative to 30 percent in condition (i). The difference is significant, but Bolton and Zwick note that the anonymity hypothesis explains only a small fraction of non-equilibrium play (about 23 percent). Finally, in condition (iii), all offers are equilibrium offers in the final 5 periods despite the fact that participant-experimenter anonymity is absent. Bolton and Zwick thus conclude that anonymity is relatively unimportant in explaining non-equilibrium play in the ultimatum game, and observe that the punishment hypothesis explains the majority of deviation.

Burnham (2003) examines both the role of and perceptions of anonymity on pro-social behavior in a dictator game experiment. He implements two double blind treatments, where either the proposer or the responder can view a picture of the counterpart, and a baseline double blind pictureless treatment. The proposer endowment is equal to \$ 10. Burnham finds that in excess of half of the proposers give nothing to responders across all three treatments, implying that one-way identification under strict anonymity (double blind design) does not change the percentage of participants who give. The modal allocation is \$ 5 in both picture treatments relative to \$ 2 in the pictureless treatment. However, mean amounts do not vary much between treatments resulting from the high concentration of \$ 0 contributions, with the lowest amount of \$1.19 being contributed in the pictureless treatment compared to \$1.96 when the responder's photo is shown, and \$1.71 when the proposer's photo is shown. He concludes that contrary to Bolton and Zwick (1995)'s conclusion, anonymity is an important consideration in determining how much is contributed in the dictator game (altruism) since more than half of proposers keep everything under double blind conditions. However, for those proposers who do choose to contribute some positive amount, the modal division is an even split in the picture treatments similar to what is observed in face-to-face experiments.

Hoffman et al. (1994) conduct double blind dictator game experiments where the experimenter cannot identify the proposer. In their "contest entitlement" treatment, participants answer current events questions, and the top ranked participants who score highest in this quiz "earn" the right to be proposers in the game.[9] Additionally, participants earn \$0.25 extra for each

[9] To motivate this property rights treatment, Hoffman et al. note that in bargaining experiments, participants' expectations may be more compatible, and proposers less influenced by the possibility of punishment strategies by responders if proposers have earned the right to their roles in the game (as opposed to being randomly assigned the right), and if this right acquisition is common knowledge.

correct answer.[10] The "random entitlement" treatment entails the standard procedure of randomly assigning participants into the position of proposer and responder. Proposers are endowed with $ 10. Hoffman et al. find that the contest entitlement significantly lowers the distribution of donations in the dictator game relative to random entitlement. In the contest entitlement treatment and when the experimenter can identify proposers, about 40 percent of proposers offer $ 0 compared to 20 percent under random entitlement. 80 percent of proposers offer $ 2 or lower under contest entitlement, whereas about 20 percent of proposers allocate half of the endowment to responders under random entitlement. When the experimenter cannot identify proposers (double-blind treatments), Hoffman et al. observe by far the largest incidence of self-regarding contributions. For example, in one of the double blind treatments, over two-thirds of proposers donate $ 0 and 84 percent donate $1 or less. The significantly smaller offers are observed in both random entitlement and contest entitlement treatments.

Eckel and Grossman (1996) run a dictator experiment where the responder in one treatment is an anonymous individual, whereas in a separate treatment, the responder is an established charity. Proposers are asked to allocate a $ 10 amount, and behavior in the two treatments is compared. They find that 30 out of 48 participants (62.5 percent) keep the entire en-

[10]There is evidence that participants regard the role of proposer as more valuable than the role of responder in the ultimatum game (Güth and Tietz, 1986). This in itself should be motivation enough for participants to answer questions correctly. However, making no assumptions as to how participants value the relative positions, Hoffman et al.'s payment for each right answer acts to incentivize participants to compete for the role of proposer.

dowment, and the mean donation is 10.6 percent of the endowment when the responder is an anonymous individual. However, when the responder is an identified registered charity, only 27.1 percent of participants keep the entire endowment and the mean donation is 31 percent of the endowment. Eckel and Grossman note that when the responder is agreed to be "deserving," donations increase significantly and thus they conclude that participants are rational in the sense that they incorporate fairness into their decisions.

Charness and Gneezy (2008) vary anonymity and social distance in dictator game and ultimatum game experiments. Employing a 2×2 experimental design, they vary the game (dictator or ultimatum) and anonymity (family name of responder provided to proposer or not provided). Proposers were endowed with 100 points in all 4 conditions, and had to decide how many points to allocate to responders. They find that 43 percent (13 of 30) of the proposers in the name treatment allocate at least 50 percent of the endowment to responders, compared to only 20 percent (6 of 30) in the no name treatment. The mean contribution in the no-name condition is 18.3 points compared to 27.2 points when proposers know the family name of the responder. However, the results in the ultimatum game are markedly different. Charness and Gneezy find that mean amounts allocated in the ultimatum game do not significantly differ between the two anonymity conditions (43.2 points in the no-name treatment compared to 45.3 points in the name treatment). 50 percent of proposers allocate half of their endowment to responders in the no-name treatment compared to 63.3 percent in the name treatment. They conclude that the absence of a significant effect on the offers across treatments in the ultimatum game suggests that strategic considerations crowd out altruistic impulses.

Small and Loewenstein (2003) manipulate a weak form of identifiability where the proposer either donates to a responder who is already determined or one who is yet to be determined in the dictator game. In the determined treatment, each participant is given a $ 10 endowment in an envelope, and draws a number labeled 1–10 from a bag. Thereafter, participants draw cards labeled either "KEEP" or "LOSE" from a bag, and the KEEP participants get to keep their endowment and number cards. The LOSE participants return the envelopes containing their endowments to the experimenter, and deposit their number cards in a bag. Each KEEP participant then draws one of the number cards deposited by the LOSE participants, and is paired with that particular LOSE participant. The KEEP participant are then assigned the role of proposer and play the dictator game with the paired LOSE participant as responder. In the undetermined treatment, the KEEP participants are told that they will be linked with one LOSE participant by having the KEEP participant draw one of the LOSE participant's numbers from the bag. However, they are informed that they have to play the dictator game before the actual draw occurs. Therefore, in the determined condition, proposers already know the number (but not the identity) of the paired responder whereas in the LOSE condition, they are yet to know the number of the paired responder. Small and Loewenstein find that the mean contribution of \$ 3.42 in the determined condition is significantly higher than \$ 2.12 in the undetermined condition. 46.1 percent of donations in the determined condition are at the equal split of \$ 5 or above, compared to only 18.9 percent in the undetermined condition. They conclude that determining the responder without providing any personalizing information– increases caring.

Kogut and Ritov (2005a) run an experiment to investigate the generality of Small and Loewenstein's result that identifying responders results in increased donations in the dictator game. In the experiment, they employ a 2×4 design where they vary singularity of the responder (single vs. a group of eight individuals) and identifying information (unidentified; age only; age & name; age& name & picture). All participants (proposers) are told that the responder needs medical care, but the cost is beyond the means of the responder. They are then asked how much they are willing to donate to help the responder. Kogut and Ritov find that mean willingness to contribute (WTC) is highest when the responder is identified by age, name and a picture (63.69), and lowest when the responder is identified only by age, and when the responder is unidentified (36.87 and 45.68, respectively – where this difference is not significant). Further, they find that whether the responder is a single individual or whether the responder is a group of 8 individuals does not have a significant main effect on WTC. They note that the lack of a significant singularity effect indicates quantity neglect. Further analysis shows an interaction between the type of identifying information and singularity. Specifically, when the responder is a single individual (but not the group of individuals), identification by age, name, and picture results in a significantly higher WTC relative to both unidentified and identified by age only. Kogut and Ritov conclude that the effect of identification in the dictator game may be largely restricted to single responders, and that identification of the single responder is more effective the more vivid the representation.[11]

1.3 Trust games

Trust is another important dimension of social preferences and it has inevitably attracted a number of experiments with communication. However, as we did in the previous section, we will first summarize findings of trust games without communication and then assess how the behavior of players changes once communication is added into the mix. Earlier trust experiments employed versions of Kreps' (1990) trust game such as exhibited in Fig. 1.3. In this game, the first-mover (henceforth trustor) can choose one of two actions: "trust" or "don't trust". If the trustor chooses not to trust the second-mover (henceforth trustee), then both players get a payoff of five. If the trustor chooses to trust the trustee, then the payoffs depend on the action taken by the trustee. The trustee (she) can choose to honor the trustor's trust in which case each player gets a payoff of 10, or she may choose not to honor the trustor's trust. If this happens, then the trustor gets a zero payoff and the trustee gets a payoff of 14. The trust game in Fig. 1.3 cap-

[11] Kogut and Ritov (2005b) is a related paper that considers the singularity effect in separate versus joint evaluations.

tures an essential element of trust in the sense that the trustor (he) will choose the action “trust” only if he strongly believes that the trustee will honor trust. Before analyzing the predicted outcome of the game using the money-maximizing model described in the previous section, we identify a defect inherent in Kreps’ trust game and introduce a game which has become the standard behavioral measure of trust among experimental economists.

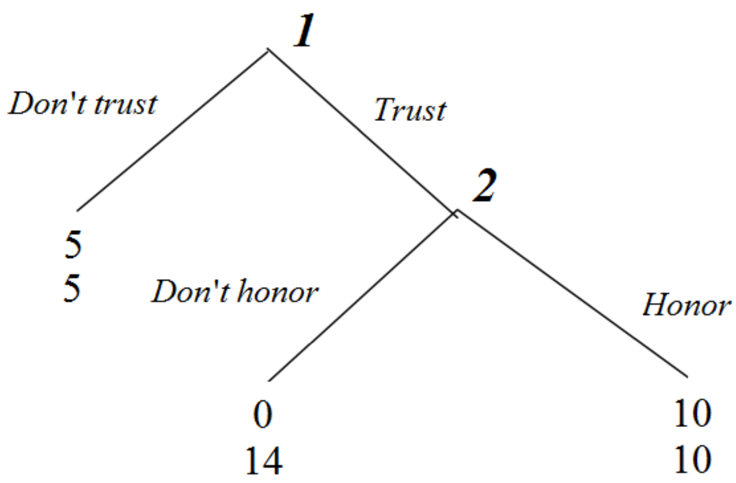

Figure 1.3: A version of Kreps’ trust game

Suppose, for example, that the trustor believes that there is a positive probability of the trustee honoring trust in the game in Fig. 1.3, but this belief is weak. What should he do? He has only two choices: to trust or not to trust. Let us assume that the trustor assigns a probability $p = 0.4$ to the trustee honoring trust. Being an expected utility maximizer, he will compare his outside payoff of 5 (payoff under the action "don't trust") to 0.4(10) + 0.6(0) =4 in which case he still decides not to trust. Notice that all beliefs with $p < 0.5$ automatically result in the trustee choosing the "don't trust" action – implying zero trust even though the trustor does weakly trust the trustee. The opposite argument can be advanced for beliefs with $p > 0.5$, where it might seem that the trustor fully trusts the trustee in cases where $p \in (0.5, 1)$, e.g., $p = 0.7$, even though the trustors' beliefs entail an element of mistrust.

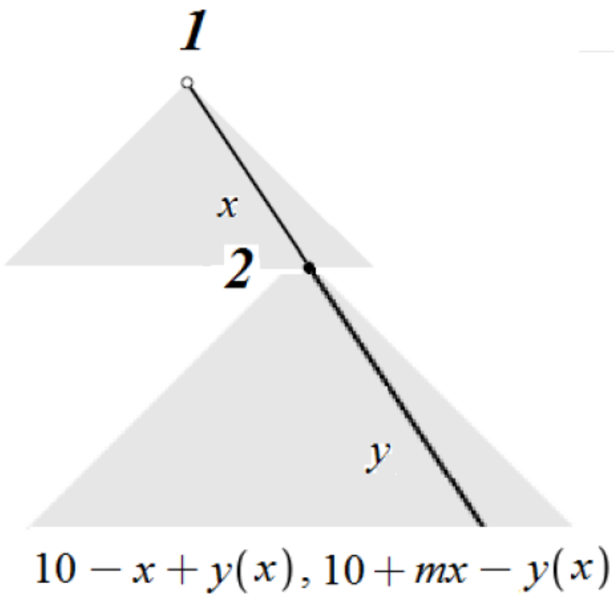

Figure 1.4: The investment game

Recognizing the fact that Kreps' trust game does not allow for the identification of intermediate trust preferences, Berg *et al.* (1995) suggested the investment game exhibited in Fig. 1.4. In this game, the trustor has the choice to send some part, none, or all of the initial endowment (set equal to 10 in Fig. 1.4). The amount that the trustor sends is denoted x where $x \in [0, 10]$. The continuum of possible trustor offers is represented by the shaded upper part of Fig. 1.4. Before the amount sent by the trustor reaches the trustee, it is multiplied by an efficiency factor of $m > 1$ (usually m is set equal to 3). Therefore, the trustee receives mx. The trustee (usually) has an initial endowment equal to that of the trustor, and she decides how much of her total wealth (endowment + mx) to send back to the trustor (trustee's endowment is set equal to 10 in Fig. 3.3). The amount sent back to the trustor is denoted y where $y \in [0, 10 + mx]$. The continuum of possible y amounts is represented by the shaded lower part of Fig. 1.4. It should be noted that the trustee's choice of y is a function of x in the sense $y(x)$.

The wider choice set available to the trustor in the investment game allows for more accurate and refined measurement of trust preferences. For example, a trustor who assigns a probability $p = 0.4$ of the trustee reciprocating may have chosen the "don't trust" action in Kreps' trust game in Fig. 1.3, but in the investment game, he has the flexibility to invest some of the money in "risky trust" (reflecting degrees of trust in the trustee) and keep the remainder to himself. Amounts $x < 3$ reflect weak trust, $x = 5$ reflects intermediate trust, and amounts $x > 7$ reflect strong trust. In case the trustee chooses not to reciprocate, the trustor still remains with part of the endowment not sent, and not a devastating zero payoff as is the case in Kreps' trust game. Additionally, the investment game also allows for the measurement of the strength of the trustee's trustworthiness. For every dollar invested by the trustor, it is possible to determine what proportion the trustee reciprocates, and higher proportions reflect higher levels of trustworthiness.

To determine the predictions of the money maximizing model for Kreps' trust game and the investment game, it is wise to simplify the analysis and look at the structure of the game once the trustor has made his move. If the trustor chooses the action "don't trust" in Kreps' trust game, then the game is over and the trustee does not have a move. However, if the trustor chooses the action "trust," then the game is effectively a dictator game in which the trustee has the choice of either keeping 14 to herself and allocate nothing to the trustor, or to allocate 10 to the trustor and reduce her own payoff by 4. Similarly, the trustee in the investment game in Fig. 1.4 is playing a dictator game in which she chooses how much of her wealth $(10 + mx)$ to allocate to the trustor. The money maximizing model predicts that the trustee will keep to herself the payoff of 14 in Kreps' trust game in Fig. 3.2, leaving the trustor with a zero payoff since $14 > 10$, and the trustee in the investment game will also keep $10+mx$ since any $y > 0$ reduces the trustee's final payoff. Using the backward induction process, the trustor who is the first mover in Kreps' trust game and in the investment game will anticipate this outcome, and therefore will choose the action "don't trust" in Kreps' trust game and will send $x = 0$ in the investment game. The trustee in the investment game will also send $y = 0$ (since the game does not end even if the trustor sends $x = 0$). Therefore, the SPEs in Kreps' trust game and the investment game are given by $(\textit{don't send}, \varnothing)$, and $(\textit{send } x = 0, \textit{send } y = 0)$, respectively.

Trust game experiments with communication

Charness and Dufwenberg (2006) modify the trust game (Fig. 1.3) by introducing a probability $p = \frac{1}{6}$ that the trustor gets a zero payoff if the trustee chooses to honor trust.[12] In a communication treatment, the trustee is allowed to send a written message to the trustor, and trust behavior in this treatment is compared to that in the base treatment where no communication occurs. Charness and Dufwenberg find that a majority of trustees' communication includes statements of intent (promises), and observe that the proportion of trustors who choose to trust their counterparts increases from 56 percent in the absence of communication to 74 percent when communication is allowed. Overall, there is an increase in the action profile in which the trustor offers trust and the trustee honors trust from 20 percent in the no communication treatment to 40 percent in the communication treatment. In a separate communication treatment where trustors send a written message to trustees prior to the start of play, there is no significant effect on

[12] This modification may be seen as representing *hidden action* in a principal-agent relationship. Consider an auto-maker in the red that hires a new CEO to transform the profitability of the firm. The success of this human resource acquisition depends on the strategies that the new CEO implements (i.e., effort that the CEO exerts). However, it may be the case that despite the CEO exerting maximum effort, an unexpected downturn in economic activity leads to weak consumer demand, and the firm remains in the red.

behavior.

The special contribution of Charness and Dufwenberg is to figure out why communication from trustees to trustors significantly enhances trust and cooperative behavior (trustworthiness), resulting in greater efficiency and not the other way round. Their design allows them to measure the beliefs of participants, and they find that communication potentially influences motivation and behavior by influencing beliefs about beliefs. Specifically, since a majority of trustees' communication includes promises, Charness and Dufwenberg find evidence consistent with trustees striving to live up to the expectations of trustors' (induced by their promises) in order to avoid guilt, a hypothesis they refer to as "guilt aversion".

Following Charness and Dufwenberg, Vanberg (2008) attempts to distinguish between "commitment-based" explanations and "expectations-based" explanations of why communication in the form of promises enhances cooperative behavior. In summary, commitment-based explanations contend that promises affect individuals' behavior because individuals possess a preference for keeping their word. Expectations-based explanations on the other hand posit an indirect effect of promises on behavior. Specifically, promises alter second-order beliefs of individuals, and this in turn has an effect on their behavior.[13] Vanberg's experimental design allows him to directly test these two explanations. Using essentially the same experimental setup as Charness and Dufwenberg, he achieves this by first matching trustors and trustees and allowing the trustees to send messages to trustors, and subsequently re-matching half of the trustees with different trustors before the trustees make any move. Trustors do not know at any time that trustees have been re-matched so their first-order beliefs depend on whether a promise had been made to them. Rematched trustees on the other hand know of the switch and are shown the messages that were sent to their new counterparts by a different trustee. Most of these messages contain promises, and in principle, the second-order beliefs of the re-matched trustees should be affected by these promises. Vanberg finds strong evidence in favor of the commitment-based explanations observing that an individual's own promise significantly affects her behavior towards the person whom the promise is made. In terms of the expectations-based account, Vanberg finds that promises do induce changes in second order beliefs, but these changes do not have any significant effect on behavior. The general result of Charness and Dufwenberg on the effect of communication on cooperative behavior is replicated in this experiment.

Goeree and Zhang (2014) introduce competition into Charness and Dufwen-

[13] Therefore, if I know that individual "X" had been promised something (either by myself or by someone else), my second order beliefs are altered by the promise. Consider a situation in which I have to make a decision with regards to whether or not to deliver on the promise to X. Then, is it just the mere fact that my second order beliefs have been altered that makes it more likely that I will deliver on the promise, or is there some other reason? If the answer is in the affirmative, then who makes the initial promise does not matter. Vanberg's experiment essentially seeks to answer this question.

berg's design by including treatments where the trustor can select one of two trustees prior to playing the trust game. To induce competition in the design, they offer a higher outside option for the trustee who is selected but not hired relative to the trustee who is not selected (a payoff of 10 relative to a payoff of 5). Goeree and Zhang replicate Charness and Dufwenberg's result that one way communication from trustees to trustors enhances cooperative behavior (the proportion of efficient outcomes rises from 30.1 percent under no-communication to 64.4 percent with communication). However, they find that the introduction of competition significantly lowers efficiency in the presence of communication, observing a higher proportion of efficient outcomes under no communication and with competition (53.5 percent) compared to under communication and with competition (37.5 percent). Their additional result that competition raises efficiency in the absence of communication leads them to conclude that communication and competition act as substitutes.

Buchan *et al.* (2006) employ the investment game to study the influence of communication, culture and social distance on trust and other regarding preferences. In an attempt to reconcile contrasting findings from Roth (1995) and Dawes *et al.* (1977) on the influence of non-strategy related face-to-face communication on cooperative behavior (where the former finds a positive influence and the latter no influence), they compare the influence of two types of non-strategy related communication on behavior. In the first condition ("personal-communication"), participants share personal information about themselves but are not allowed to discuss strategies in the investment game (similar to Roth's restricted treatment). In the second condition("impersonal-communication"), participants discuss facts but no personal information is shared (similar to the manipulation in Dawes *et al.* (1977)). Buchan *et al.* find that personal communication results in significantly higher amounts sent and returned in the investment game (or higher levels of trust and trustworthiness) relative to impersonal communication. Therefore, they speculate that what may be relevant in enhancing cooperation, for example as observed in Roth's experiment, may not merely be non-strategic communication but personal communication, through either reducing social-distance between communicating parties, increasing group identity, or promoting other regarding concerns.

1.4 Other games

Apart from dictator, ultimatum, and trust games, the role of communication has been experimentally studied in public good games, and matrix games (e.g., prisoners' dilemma, coordination games, and zero-sum games). Since the focus of this paper is primarily on the first three games, we will only give a brief summary of the role of communication in the latter three classes of games, and refer the reader to other sources where there has been a more thorough treatment of the literature. In a public goods game, players privately choose how much of their initial endowment (tokens) to contribute into a public pot. The tokens are multiplied by a factor greater than one but less than N (N representing the total number of players), and this "public good" payoff is evenly divided among players. Each player keeps the part of the initial endowment that they did not contribute.[14] Under a linear voluntary contribution mechanism, the payoff to player j is thus given by

$$\pi_j = p(z - c_j) + \frac{\alpha}{N}\gamma \text{ where } \gamma = \sum_{i=1}^{N} c_i.$$

where z represents the total number of tokens, p is the private value of tokens, c_j is the number of tokens that player j contributes, $\frac{\alpha}{N}$ is the marginal gain from contributing a token. The marginal per capita return (MPCR) of the public good is simply the marginal rate of substitution between the public and private good

$$\text{MPCR} = \frac{-\frac{\partial \pi_j}{\partial \gamma}}{\frac{\partial \pi_j}{\partial c_i}} = \frac{\alpha}{pN}$$

A social dilemma arises when $\frac{1}{N} < \text{MPCR} < 1$, and the money maximizing model predicts that player j will contribute $c_j = 0$.

Coordination games are games with multiple pure strategy Nash equilibria where players can choose either the same or corresponding strategies. They represent abstractions of coordination problems, a classical example be-

[14] A public good game is therefore just an N-person Prisoners' Dilemma (PD) where contribution by all N players corresponds to the strategy "Cooperate" and non-contribution corresponds to "Defect". It is profitable not to contribute to the public good if everyone else is contributing just as it is to defect in the one-shot PD.

ing the "battle of the sexes" – where one player (husband) prefers to watch a game of football, and the other player (wife) prefers to attend an opera concert, but both would rather attend the same event together than separate events apart. Fig. 1.5 exhibits the payoff matrix of the battle of the sexes game where the wife is the row player and the husband is the column player. Notice that the game is represented in normal form (payoff matrix) because unlike the ultimatum game and trust game which are sequential-moves games, coordination games are simultaneous-moves games.[15]

	Opera	***Football***
Opera	**2 1**	**0 0**
Football	**0 0**	**1 2**

Figure 1.5: Payoff matrix of the battle of the sexes

The two pure strategy Nash equilibria outcomes of the battle of the sexes game are (opera, opera) and (football, football). The husband prefers the latter equilibrium but would rather be in the (opera, opera) equilibrium

[15]Strategically, what is important in the classification of simultaneous-moves versus sequential-moves games is not the time that an action takes place but the information available to a player when she makes her move.

relative to coordination failure. Similarly, the wife prefers the (opera, opera) equilibrium but would rather be in the (football, football) equilibrium relative to coordination failure. There is a mixed-strategy Nash equilibrium in which the husband mixes his strategies opera and football with weights $(\frac{1}{3}, \frac{2}{3})$, and the wife mixes her strategies opera and football with weights $(\frac{2}{3}, \frac{1}{3})$. To see this, suppose that the husband (column player) assigns probability q to the action "opera" and probability $1-q$ to the action "football". Then, the wife's (row player's) expected payoff for the action "opera" against $(q, 1-q)$ is $2(q) + 0(1-q) = 2q$. Similarly, the wife's expected payoff for the action "football" against $(q, 1-q)$ is $0(q) + 1(1-q) = 1-q$. In particular, if a mixed strategy constitutes a Nash equilibrium, then all pure strategies in the mix must yield the same expected payoff (or each pure strategy must itself be a best response). Exploiting this fact, we have to equate the two payoffs and solve for q: $2q = 1 - q \Rightarrow q = \frac{1}{3}$. Since we defined q as the probability assigned by the husband to the action "opera", $1 - \frac{1}{3} = \frac{2}{3}$ is the probability he assigns to the action "football" in the mixed Nash equilibrium. Using the same method, the wife's weights are easily computed. Different variants of coordination games similar in structure to the battle of the sexes are used in experimental studies of coordination behavior. These experiments often establish if players coordinate on a particular Nash equilibrium (relative to another), fail to coordinate, or whether they play the mixed Nash equilibria of the games.

	cooperate	*defect*
cooperate	2, 2	0, 3
defect	3, 0	1, 1

Figure 1.6: Prisoners' Dilemma

The Prisoners' Dilemma, the most famous game in game theory, has also attracted a great deal of interest among experimental economists. In this game, both the row player and the column player can choose between two actions: *cooperate* or *defect*. If both players choose *cooperate*, then each obtains a payoff of $C \in \mathbb{R}$, and if both choose *defect*, then each obtains a payoff of $D \in \mathbb{R}$. If a player chooses *defect*, then she obtains a payoff of $A \in \mathbb{R}$ in the event that the other player chooses *cooperate*, and the latter obtains a payoff of $Z \in \mathbb{R}$. The following inequality holds with respect to the sizes of these payoffs: $A > C > D > Z$. Fig. 1.6 exhibits a version of the prisoners' dilemma where $A = 3$, $C = 2$, $D = 1$, and $Z = 0$. The game has one pure strategy Nash equilibrium, i.e., $(defect, defect)$, but no mixed strategy Nash equilibrium (this can be proved using the procedure we have previously outlined). A characterizing feature of the game is that the Nash equilibrium is Pareto-dominated by the outcome $(cooperate, cooperate)$. The money-maximizing model predicts that the Nash equilibrium would result if participants play this game in the laboratory, where the payoffs in the matrix represent some multiple of real monetary amounts. A famous result, referred to as the folk theorem, shows that the outcome $(cooperate, cooperate)$ can emerge as a subgame perfect equilibrium in an infinitely repeated version of the stage game (or where the number of repetitions is unknown by players). We derive conditions under which $(cooperate, cooperate)$ emerges as a subgame perfect equilibrium under the assumption that players use an exponential discount function (the standard case), and in cases where players use non-exponential discounting (Musau, 2014a).

	Left	***Right***
Left	**−1, 1**	**1, −1**
Right	**1, −1**	**−1, 1**

Figure 1.7: A zero-sum game

Zero-sum games are games in which the gains or losses of a set of players are exactly balanced by gains and losses of the other players in the game. In essence, once the payoffs of all players are added up, then they amount to a zero sum. Zero-sum games are therefore games of conflict, and any result of the game is Pareto optimal. Consider the game in Fig. 1.7 which we will refer to as the "shoot-out" game. In this game, the row player (henceforth striker) will attempt to score a goal from the penalty spot against the column player (henceforth goalkeeper). The striker has two strategies: he can either kick the ball to the left or to the right. The goalkeeper has also two strategies: he can either dive to the left or to the right. If the striker kicks to the left and the goal keeper dives to the left, then the spot kick is saved and the striker gets a payoff of -1 whereas the goal keeper gets a payoff of 1. However, if the striker kicks to the left and the goal keeper dives to the right, the striker scores and gets a payoff of 1 whereas the goalkeeper gets a payoff of -1. Thus, if both players pick the same action, this favors the goalkeeper whereas different actions favor the striker. Notice that the payoffs in each cell sum up to zero. In zero-sum games, there may be no pure strategy Nash equilibria as in our shoot-out game. However, there is a mixed strategy Nash equilibrium in which both the striker and goalkeeper mix their strategies "left" and "right" with weights $(\frac{1}{2}, \frac{1}{2})$. This can be easily computed using the procedure described previously. Experiments involving zero-sum games investigate whether participants follow minimax decision rules when playing these games.[16]

[16]The minimax theorem asserts that in a 2-player zero-sum game, there exists a mixed strategy and a payoff value "V" such that, given player $j's$ strategy, the best payoff possible that player i can guarantee is V and correspondingly, given player $i's$ strategy, the best payoff possible that player j can guarantee is $-V$ $(i \neq j)$. In our example, the minimax strategy involves both the striker and the goalkeeper randomizing their two strategies, half of the time playing "left" and half of the time playing "right" (the mixed strategy), yielding $V = -V = 0$.

Other game experiments with communication

Brosig *et al.* (2003) study the effect of communication media on cooperation in standard public goods games. They manipulate two factors: verbal communication and anonymous interaction. Thus, their seven treatments include combinations of these factors, for example, no-communication (i.e. no verbal communication and anonymous interaction), visual identification with no communication (i.e. no verbal communication but non anonymous interaction), anonymous communication (verbal communication but anonymous interaction), face-to-face communication (verbal communication and non anonymous interaction). Brosig *et al.* find that the lowest contributions result under both no communication and under visual identification with no communication with cooperation rates of less than 50 percent. When verbal communication is introduced but the interaction is non-anonymous, there is a significant rise in contributions with cooperation rates of between 50 and 60 percent. The highest contributions are observed when there is face-to-face communication with cooperation rates in excess of 90 percent. Brosig *et al.* conclude that various forms of nonverbal communication such as facial expression, and body language as well as the tone of voice play a significant part in accounting for the observed communication effects.

Isaac and Walker (1988) implement a variant of the four-person voluntary contribution mechanism design with MPCR = 0.3. In the experiment, experienced participants decide on how much to contribute to the public good and are endowed with identical sums at the start of each period. The

"NC" treatment involves 10 periods with no communication. The "C/NC" treatment involves an initial 10 periods with communication followed by 10 periods without communication whereas the "NC/C" treatment involves 10 periods without communication followed by 10 periods with communication. In the communication periods, participants are allowed to discuss face-to-face for four minutes about anything, except that they cannot reveal private information about their endowments and cannot arrange side payments or issue threats. Thereafter, they return to their terminals and privately make their contribution decisions. Isaac and Walker find that communication significantly increases mean contributions at levels well above 60 percent in the communication periods compared to well below 50 percent in the baseline NC treatment. However, they observe that contributions decline very slowly in the last half of the C/NC treatment (i.e. periods where there is no communication), and they also increase slowly in the last half of the NC/C treatment (periods where there is communication), suggesting that there is a reinforcing effect, i.e., a history of free-riding tends to discourage cooperation and a history of cooperation tends to discourage free-riding.

Koukoumelis et al. (2012) consider a voluntary contribution mechanism with one way communication. In the experiment, groups of four participants interact for a specified number of periods, where the constitution of the group does not change across periods. The baseline treatment entails the standard protocol where there is no communication, and in the communication treatments, one of each group member is randomly selected to communicate with the other 3 group members in writing. Each participant is endowed with a sum of 25 ECU at the start of each period. Koukoumelis et al. find that the mean contribution per participant is significantly higher in the communication treatments ($<$ 20 ECU) compared to the no communication treatment (12.27 ECU). Thus, they conclude that in contrast to the usual explanation that communication enhances cooperation through mutual exchange of promises, one-way communication significantly raises contributions and lowers their variability. In cases where communication occurs only once, contribution levels persist in later periods, and the effectiveness of one-way communication is robust to the absence of strategic concerns.

Bohnet and Frey (1999a) implement the same three treatments described previously in the Prisoners' Dilemma (PD). They find that the outcome (Cooperate, Cooperate) emerges 12 percent of the time in the anonymity condition compared to 23 percent under mutual identification, and 78 percent under communication. Unlike the dictator game where cooperation rates were not significantly different between mutual identification and communication, their results show that face-to-face verbal communication is powerful in enhancing cooperation in the PD.

A result that seemingly contradicts Roth (1995) on the effect on no-strategy relevant communication on behavior was found a few decades earlier in an experiment by Dawes *et al.* (1977). In this experiment, participants take part in a 10-minute period where they discuss face-face the percentage of individuals at certain income levels in a major city of the Pacific Northwest region of the U.S. (i.e. participants engage in "fact-related" communication). As in Roth's experiment, participants cannot discuss strategies in the game. Dawes *et al.* find that fact-related communication results in the same levels of cooperation as no-communication in social-dilemma games.

A very brief summary on coordination games with communication

One area that deserves a mention in the current thesis is the role of communication in coordination games. While this area in our opinion has received a more thorough treatment in the literature, it is important to highlight an area where costless communication is effective in achieving desirable outcomes. Much of the work on costless communication ("cheap talk") in coordination games follows the pioneering work of Crawford and Sobel (1982). In their sender-receiver model, an informed sender learns her "type" and can convey this information via cheap talk to a receiver. The receiver after receiving the sender's message, takes an action which affects the payoffs of both players. The model predicts that cheap talk is positively related to efficiency the greater the degree to which interests of players are aligned. Thus, an equilibrium exists in which cheap talk is informative. Farrell (1993) subsequently noted that meaning cannot be established from introspection. This leads to a multiplicity of perfect Bayesian equilibria in a cheap talk game, which cannot be reduced by any standard equilibrium refinement. The reason is that standard refinements need to impose a restriction on the interpretation of messages that were not expected in equilibrium. One example of an equilibrium that always exists in a cheap talk game is the so called "babbling" equilibrium where cheap talk does not convey any useful information.[17]

Studies that have looked at the form of communication and outcomes in coordination games have yielded contrasting results. Ellingsen and Östling show that two-sided communication in general results in increased coordination on equilibrium outcomes and better payoffs than does one-sided communication in symmetric 2×2 games. However, in two-player common interests games, they observe both one-sided and two-sided communication always facilitate coordination on the best equilibrium outcome if players make at least two thinking steps. The duration of communication has also been observed to be important. Aumann and Hart (2003) show long cheap talk (or long conversations) convey substantive information that cannot be conveyed by a single message, and this expands the set of outcomes thus affecting the equilibria preferred by players. Experimental results that support a number of these theoretical predictions are covered in Crawford (1998).

1.5 Conclusion

This chapter has reviewed a substantial body of literature on the effect of communication in game experiments in economics. Previous findings have shown that behavior in game experiments does not conform to predictions

[17]A babble is a continuous, murmuring sound that is mostly incomprehensible. Thus, the sender may indeed send a message to the receiver, but if message does not convey any useful information that helps the receiver to coordinate, it is as if the sender just babbled, leaving the receiver no wiser.

of the purely selfish money maximizing model. Subsequent research has advanced from simply focusing on departures from the standard model to looking at the incremental effect of structural variables such as communication on observed behavior. Generally, results show that communication has a positive effect on increasing donations in dictator games, increasing rates of cooperation in bargaining games, trust games, and public goods games, and increasing coordination rates in coordination games when the interests of players are closely aligned. The general result masks a lot of detail in its simplicity, and it is tempting to conclude that communication has a unidirectional effect on behavior. In reality, as the experiments that have been reviewed show, there are a number of varied and at times conflicting effects of communication, some of which need further experimentation in order to disentangle.

An attempt to delve into the full spectrum of issues raised in the review would be futile, but here we can motivate how we want to advance the literature in the proceeding chapters, as well as offer some avenues for future research. One important point that we have raised in the review is the difficulty in achieving a controlled variation of communication if the experimentalist chooses to focus on face-to-face communication. Therefore, as the pioneering works of Bohnet and Frey implore, a standard that should be adopted in the literature is that all face-to-face communication experiments should include a visual-identification-only treatment. In the ultimatum game, if only visual identification of the proposer and responder results in behavior not different to face-to-face communication, then this renders an analysis of the informational content of messages in the communication treatments redundant. Therefore, it would be interesting to see a replication of the results by Roth and Zultan including a visual-identification only treatment.

The vast majority of ultimatum game and trust game studies reviewed have emphasized the role of strategic communication in achieving higher levels of cooperation compared to the standard no communication condition. For example, Charness and Dufwenberg emphasize the effect of first movers' promises to cooperate in the trust game, and Andreoni and Rao focus on the power of asking by recipients in the dictator game. Apart from a few studies such as Roth (1995), the role of non-strategic communication has not been analyzed to the same level as strategic communication. Therefore, more studies that look at the impact of non-strategic communication are needed, so that one can evaluate a general communication effect as opposed to simply a strategic communication effect.

An additional consideration is that a number of games feature varying strategic power dynamics between players, which inevitably affect the nature and content of communication. In the dictator game, for example, the allocator has full control of the initial endowment, and unilaterally decides how to split it between herself and the recipient. On the other hand, the recipient initially has nothing and plays no role in determining how the initial endowment is split. Thus, the inequality in roles implies that all strategic power is in the hands of the allocator, and one can infer that if given a voice, as Andreoni and Rao find, the recipient is reduced to asking for a portion of the endowment, or in less kind terms, begging. To the best of our knowledge, no communication study has explored these power dynamics that are present in sequential move games, and how they affect the ensuing communication. Given that economic actors have different levels of strategic power, for example in buyer-seller bargaining, exploring how power dynamics affect communication and cooperation is an important element in understanding individual behavior.

The proceeding chapters in one way or another attempt to capture one or several elements of the issues that we raise. In both Chapters 2 and 3, we change the sequence of communication, so that its effect is non-strategic. Therefore, unlike the standard pre-play communication sequence, communication occurs after the first-mover has made a binding decision, but before the second-mover has made any decisions. This makes it impossible for communication in a given period to affect the first mover's decision in that period, and this is common knowledge to both the first- and second-mover. Additionally, this manipulation allows us not to explicitly constrain the content of communication as in Roth (1995). We rely on written anonymous communication in the studies in both chapters to obtain clean data on the effect of communication, because, as we argue, there may be some identification based confounds if one chooses face-to-face communication and does not include a visual-identification-only treatment. Finally, in Chapter 3, we compare behavior in a symmetric simultaneous move game and a related sequential move game. As we have stated, positional power differences exist in sequential move games, but not symmetric simultaneous move games, so this allows us to investigate the effect of communication on strategic power differences.

Chapter 2

Anticipated communication in the ultimatum game

with Marco Capizzani, Luigi Mittone, and Antonino Vaccaro

2.1 Introduction

There is evidence that anticipated verbal feedback induces altruistic behavior. Xiao and Houser (2009), and Ellingsen and Johannesson (2007), find that in a dictator game where the allocator donates an amount to a recipient, and the recipient sends an anonymous written message after learning of the amount, donations are significantly higher in relation to the standard (no-communication) condition. In both studies, there is a strong emotional response by recipients who receive what they perceive to be unfair donations (below the 50:50 split), and this is expressed in terms of expressions of disapproval, and even at times foul language. For Ellingsen and Johannesson, individuals are motivated by concerns for pride and blame, whereas Xiao and Houser argue that allocators in the dictator game have a preference for avoiding written expression of disapproval, or negative emotions.

One suspects that such motivations persist in similar decision problems where individuals are required to make allocation decisions. However, in the extended environment of the ultimatum game, in determining what proportion of endowment to offer, the proposer is not only concerned about her pro-social behavior, but must factor in the possibility of her offer being rejected by the responder. Previous studies have shown that such strategic considerations have a significant effect on allocation decisions. Charness and Gneezy (2008), for example, compare how anonymity and social distance affect behavior in dictator- and ultimatum-games, and find contrasting effects. In the dictator game, reducing social distance significantly increases donations, whereas in the ultimatum game, there is no significant effect on offers. Thus, for them, it appears that strategic considerations crowd out impulses toward generosity or charity.

In this study, we thus propose to study whether strategic considerations crowd out anticipatory effects of communication. We achieve this by implementing Xiao and Houser's communication sequence in the ultimatum game with strategy method. In this setup, the proposer makes a binding proposal that cannot be subsequently changed. Thereafter, the responder, without knowing the actual choice of the proposer, has to indicate for each possible offer whether she "accepts" or "rejects". We include three treatments. The base treatment *No Communication* (henceforth NC) is the standard condition where participants are anonymous and are not allowed to communicate, with a three minute time-gap between choices of the proposer and responder. The two communication treatments *One-Sided Communication* (henceforth OSC), and *Two-Sided Communication* (henceforth TSC), consist of a three minute communication phase in between the proposer and responder choices. The treatments differ only in terms of how we manipulate communication: In the OSC treatment, the proposer unilaterally communicates to the responder in writing, whereas in the TSC treatment, both the proposer and responder communicate with each other in writing.

In a related paper, Xiao and Houser (2005) allow responders in the standard ultimatum game to attach a written message to their "accept" or "reject" decision, after learning of the offer from the proposer. They find that proposer offers do not differ compared to the standard condition, but responders reject unfair offers significantly less frequently. One fundamental difference between our designs is that the proposer's offer focalizes the communication content in Xiao and Houser's study, whereas in our study, the responder has an uninformative prior. As Xiao and Houser infer, responders' expression of negative emotions for what they perceive to be unfair offers decreases the likelihood that they reject such offers, and thus it appears that communication supplements costly punishment (by providing an additional medium where an aggrieved responder can express her negative emotions). Our study allows communication in the form of expressions of negative emotions, such as may occur if a proposer reveals that she offered an amount perceived to be unfair by the responder, in the two-sided communication condition. However, such communication is neither exclusive nor even expected to be significant in quantity. Therefore, since offers are never revealed prior to the conclusion of play, our design allows us to examine the effect of anticipated communication that is not constrained in some predefined way.

Furthermore, the strategy method (Selten, 1967) allows us to elicit the full strategy vector of the responder. In the ultimatum game, little is known about responder choices corresponding to especially high offers since such offers are rarely observed in practice, and previous studies have mostly elicited responses using the direct-response method. Considering a sample of 75 standard ultimatum game experiments, Oosterbeek *et al.* (2004) find that less than a fifth elicited responses using the strategy method. As Zultan (2012, p. 18) observes, "... [responder behavior in ultimatum games] 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)". Empirically, Brandts and Charness (2011) conclude that both the strategy- and the direct response-method generally yield similar results, thus consistent with the standard theoretical view. In addition, from the view-point of a single study, given that an experimentalist applies the same method consistently across treatments, any existential differences between the methods would not invalidate conclusions from that particular study.[1]

The content of the communication is not restricted in OSC and TSC, but because identifiability may introduce nonpecuniary influences on preferences, participants are not allowed to divulge information in the messages that can lead to them being identified. In contrast to the result of Xiao and Houser (2005), we find that anticipated communication effects still persist in the presence of strategic considerations with two-sided communication, with offers in TSC being significantly higher to either OSC or NC. Offers in NC and OSC do not differ, suggesting that anticipation effects also crucially depend on the form of communication. In terms of responder behavior, we observe a significant amount of non-monotonicity in responder choices across all three treatments, with higher conditional rejection frequencies for high (above equal-split) and low (below equal-split) offers in TSC, relative to either OSC or NC. However, as was the case for offers, responder rejection rates do not differ between NC and OSC. An analysis of the informational content of communication in OSC and TSC reveals that the vast majority of participants communication are statements or discussions of the ultimatum game being played (such as references to “offer”, “accept”, “reject”). In addition, we find evidence that proposers restrict the content of communication to conversations that exclude references to fairness in OSC, when they uni-

[1] In experiments where methods yield different results, one can distinguish between “hot” effects and “cold” effects. However, the responsibility rests on the experimentalist to explain if and how such observed differences matter.

laterally communicate to responders, which may account for why we observe behavioral differences in TSC relative to either OSC or NC, but not in OSC relative to NC.

The paper is organized as follows: Section 2.2.1 presents our experimental design. Section 2.2.2 outlines our behavioral predictions. Section 2.2.3 describes the experimental procedures and protocols. Section 2.3 presents the results of the experiment, and provides an analysis of the informational content of messages in OSC and TSC. Eventually, Sect. 2.4 concludes.

2.2 Method

2.2.1 Experimental Design

We investigate behavior in the ultimatum game with strategy method. In our setting, the proposer (henceforth X) chooses an amount x to offer to the responder (henceforth Y) from a pie of 10 *Euros*, with the restriction that each player gets at least 1 *Euro*. This results in nine possible offers: $x \in \{1, 2, ..., 9\}$. In turn, Y, without knowing the actual choice of X, indicates for each possible offer whether she accepts or rejects. We denote Y's choice $y_x \in \{accept, reject\}$. A strategy of Y assigns y_x to each x choice of X, and is a 9-element vector collected by having Y fill in a table similar to Table 2.1, with either "accept" or "reject" at each blank box.

An outcome of the game is a matched pair (x, y_x) with the following payoffs:

Table 2.1: Ultimatum game with strategy method: Y's decision task

x	1	2	3	4	5	6	7	8	9
y_x	□	□	□	□	□	□	□	□	□

$$\pi^X = \begin{cases} (10 - x)\ Euros & \text{if } y_x = \text{accept} \\ 0\ Euros & \text{if } y_x = \text{reject} \end{cases} \tag{2.1}$$

$$\pi^Y = \begin{cases} x\ Euros & \text{if } y_x = \text{accept} \\ 0\ Euros & \text{if } y_x = \text{reject} \end{cases} \tag{2.2}$$

where π^X and π^Y denote the payoffs to X and Y respectively.

Before Y makes her choices, but after X has chosen a binding offer x, there is a three minute communication phase in each of the game experiments. We distinguish three treatments:

1. *NC* (***No Communication***): Standard anonymous no-communication condition with a time gap during the communication phase.

2. *OSC* (***One-Sided Communication***): X has the option to anonymously and unilaterally communicate in writing with Y during the communication phase.

3. *TSC* (***Two-Sided Communication***): Both X and Y have the option to anonymously communicate with each other in writing during the communication phase.

2.2.2 Behavioral Predictions

Previous ultimatum game experiments have shown that even in the absence of communication, proposers on average offer about 40 percent of stakes on offer, and responders frequently reject offers of less than 20 percent, independent of the size of initial endowment (refer to the meta analysis by Oosterbeek *et al.*). Therefore, based on these studies, we predict the following for the NC treatment:

H1 X participants will make positive offers, on average in excess of 30 percent of endowment.

H2 Y participants will both frequently reject offers of less than 20 percent of endowment, and will accept offers in excess of 30 percent of endowment.

A few ultimatum bargaining experiments have examined the effect of communication on bargaining outcomes, and have for the most part found that communication induces more egalitarian offers, and lower rates of disagreement between bargaining parties (see, for example, Roth, 1995; Zultan, 2012). However, apart from Xiao and Houser (2005), none has examined the effect of anticipated communication on proposer behavior, or provided a detailed analysis of responder choices when communication proceeds the offer. Nevertheless, these studies alongside the dictator game studies of Ellingsen and Johannesson (2007), and Xiao and Houser (2009), provide us with some insights on expected behavior in our experiment.

Xiao and Houser (2005) does not find an anticipated communication effect (present in the two dictator game studies), suggesting that strategic considerations crowd out such anticipated communication effects in the standard ultimatum game. However, our design allows for a stronger effect to be observed in TSC, since two-sided communication implies that the proposer plays an active role (participates) in the communication, as opposed to where the proposer is passive and simply receives the accept/reject decision, along with a written message from the responder. Participation of the proposer implies that the responder can ask directly about how much the proposer offered, and subsequently the proposer may incur the wrath of the responder if the offer is judged to be unfair. On the other hand, the proposer may choose to lie about the offer to avoid any emotional backlash from the responder, but the proposer may feel guilty about lying later on. Therefore, due to guilt aversion, the proposer may simply choose to be more other-regarding. Therefore, we expect the fact that the proposer is active in TSC will result in an anticipated communication effect.

H3 X participants will make higher offers in TSC relative to either NC or OSC.

It is clear from the dictator game studies of Ellingsen and Johannesson (2007), and Xiao and Houser (2009), that the crucial aspect is the feedback mechanism of (anticipated) communication. Since it is the proposer that makes the offer, we do not expect that she will increase or decrease her offers if she unilaterally communicates, but does not expect feedback of any form. Charness and Dufwenberg (2006), for example, argue that communication in the form of promises from second movers in the trust game is what is crucial in inducing first movers to cooperate. They observe that one-sided written communication from first movers has no effect on cooperation levels, in comparison to the no communication condition. Similarly, Andreoni and Rao (2011), in the dictator game, find that the amount allocated is lowest in the standard no communication condition, and when the allocator communicates unilaterally to the recipient in writing, compared to all other conditions (one-sided communication from the recipient, and two-sided communication). Therefore, we expect that one-sided communication from the proposer will have no effect on offers, or responder choices, compared to no communication.

H4 Offers of X participants will not differ between OSC and NC.

H5 Choices of Y participants will not differ between OSC and NC.

Finally, Xiao and Houser (2005) note that communication supplements costly punishment in the standard ultimatum game, with responders being less likely to reject what they perceive to be unfair offers from proposers if they can show their disdain for the offers in the form of written messages. However, we note that since our design does not allow for the responder to definitively learn of the proposer's offer prior to the conclusion of the one-shot ultimatum game, it is not clear that the content in Xiao and Houser's study will be the same as that in our study. In a face-to-face communication treatment of an ultimatum game with strategy method similar to ours, Zultan (2012) observes that responders act less cooperatively when communication is unrestricted, compared to the standard no-communication condition, whereas restricting communication to social conversations results in responder behavior not different to the standard condition. Therefore, since we impose no restrictions on the content of communication in TSC, it is highly likely that behavior will be similar to Zultan's unrestricted communication result.

H6 Rejection frequencies will be significantly higher in TSC relative to either NC or OSC.

2.2.3 Participants and Procedures

The experiment was conducted at the Cognitive and Experimental Economics Laboratory (CEEL) at the University of Trento. A total of 120 participants took part in six experimental sessions compromising two sessions per treatment. The participants were recruited from the undergraduate populations at the University. None of the participants had previously taken part in this series of experiments.

On their arrival, participants were allocated separate computer terminals, and given a copy of the instructions for the experiment. Time was allocated for private reading of these instructions. Thereafter, a member of the experimental staff read aloud the instructions in an attempt to make the rules and procedures of the experiment common knowledge.

Each participant was then randomly assigned either the role X or Y, and maintained that role for the remainder of the experiment. To ensure comprehension with the instructions, participants had to answer a set of control questions relating to the contents of the instructions prior to the actual start of the experiment.

The experiment consisted of five periods. At the start of each period, each participant with the role X was randomly paired with a participant with the role Y. No participant was paired with the same participant at a subsequent period, and this detail of the matching protocol was explicit in the instructions.

In the communication treatments, a member of the experimental staff monitored the contents of the messages to make sure that they complied with the instructions. Participants in these treatments were explicitly made aware of this fact prior to the actual start of the experiment.

2.3 Results

2.3.1 Descriptive Statistics

Choices of X

We begin with a summary of the choices of X participants across the three treatments. Figure 2.1 exhibits the distribution of individual-level average offers over the five periods of the experiment.

Most offers are between $40-50$ percent of endowment in TSC, and $30-40$ percent of endowment in NC and OSC. Both the first-period- and overall-mean-offer across periods are higher in TSC compared to either NC or OSC. A series of Mann-Whitney U tests on period averages shows that offers differ between NC and TSC ($\rho = .010$), and OSC and TSC ($\rho < .001$). However, the tests do not reveal a statistically significant difference between offers in NC and OSC.

Observation 1 *Behavior of X participants in NC and OSC does not differ. However, offers are significantly higher in TSC.*

These results are consistent with our behavioral hypotheses H1, H3, and H4, and show that anticipated two-sided communication induces higher offers from proposers. Whether two-sided communication results in higher levels of cooperation, generally, depends on the behavior of Y participants analyzed below.

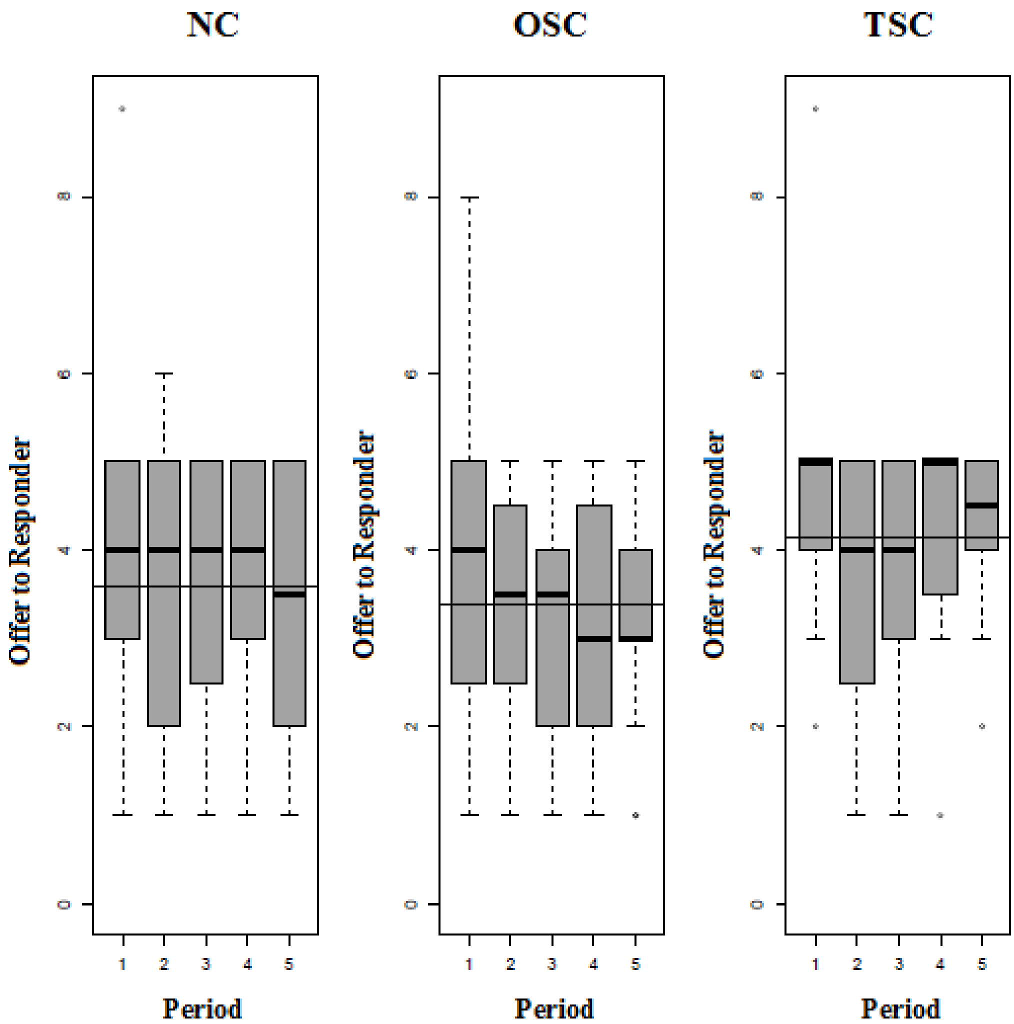

Figure 2.1: Choices of X (average at the individual level)

Choices of Y

Figure 2.2 exhibits a grouped bar plot of conditional acceptance rates of Y participants across the three experimental treatments.

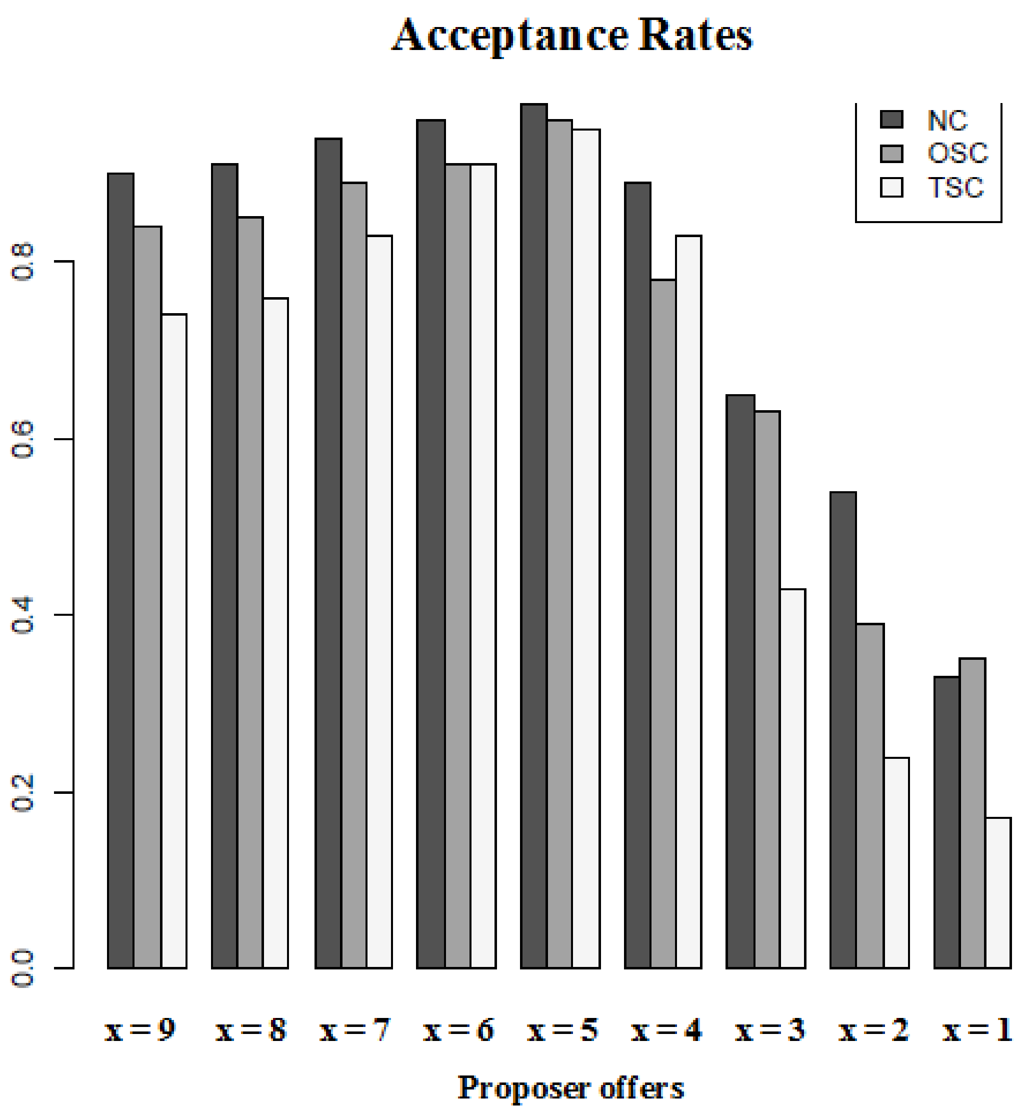

Figure 2.2: Conditional choices of Y (comparison across treatments)

What is immediately apparent from the bar plots is the non-monotonicity of Y choices across all treatments. A monotonic strategy of Y requires that if $y_x =$ "accept", then $y_{x'} =$ "accept" for all $x' > x$. Therefore, for example, if a participant accepts an offer $x = 5$, to adhere to monotonicity, she must also accept all offers $x > 5$. In addition, the bar plots reveal that acceptance rates are consistently lowest in TSC. For low offers, i.e., $x \in [1, 3]$, Mann Whitney U-tests on period averages show that acceptance rates differ between TSC and NC ($\rho = .003$), and TSC and OSC ($\rho = .007$). However, the tests do not reveal a difference in acceptance rates between NC and OSC ($\rho = .767$).

Across all treatments, acceptance rates are highest for intermediate offers, i.e., $x \in [4, 6]$. The maximum acceptance rate is observed at the equal split of the pie, $x = 5$, with nearly all such offers accepted by Y participants. For this range of offers, Mann Whitney U-tests do not highlight a significant difference in acceptance rates between treatments. For high offers, i.e., $x \in [7, 9]$, acceptance rates differ only between NC and TSC ($\rho = .024$).

Observation 2 *Y participants are significantly less cooperative in TSC if offers are either low or high. For intermediate offers, there are no behavioral differences between treatments.*

Hypothesis H2 is not fully supported due to the non-monotonicity of a large number of participant Y strategy vectors. Even though we observe that Y participants frequently reject offers of less than 20 percent of endowment, the frequency of rejection of high offers is also high (relative to the expectation that no high offer is rejected). In NC, for example, 10 percent responder choices corresponding to the maximum offer of $x = 9$ were "reject", which is significantly different to the case where all choices are "accept" (Wilcoxon signed-rank test, $\rho = 0.020$). Hypothesis H5 is supported, and hypothesis H6 holds with the qualification that rejection rates do not differ between OSC and TSC for the range of high offers.

2.3.2 Regression Analysis

We specify regression and probability models in this section in order to gain an in-depth analysis of choices of X, and rejection behavior of Y. The models take into account dependencies that arise from the matching protocol implemented, and repeated play across periods.

Analysis of Proposer behavior

To analyze choices of X, we specify a random effects linear model. The dependent variable *offer* represents the *Euro* value of the offer made by participant i, and takes on values in the range [1,9]. The model takes the form:

$$offer_i = \beta_0 + \beta_1 OSC_i + \beta_2 TSC_i + \beta_3 period_i + u_i$$

where as explanatory variables, we include indicator variables for the OSC and TSC treatments, and a period variable. In addition, as a robustness check, we estimate two random effects probit models where the dependent variable *offer* takes on the value one if $x = 5$, and $4 \leq x \leq 6$, respectively, and equals zero otherwise. The baseline treatment in the regression analysis is NC. Table 2.2 summarizes the results of the regressions.[2]

Table 2.2: Choices of X (Random Effects: linear and probit)

	Coeff (Std. Err.)		
Offer∼	*All* ($x \in [1, 9]$)	$x = 5$	$4 \leq x \leq 6$
(Intercept)	3.775 (0.236)***	-0.476 (0.262)*	0.332 (0.239)
OSC	-0.210 (0.189)	-0.417 (0.206)**	-0.260 (0.188)
TSC	0.550 (0.189)***	0.443 (0.192)**	0.434 (0.192)**
Period	-0.062 (0.054)	-0.002 (0.057)	-0.023 (0.055)
*W-st*1	16.250***	17.300***	13.010***
No. of observations (groups)	300 (20)	300 (20)	300 (20)
Random intercept of Level–2$^{\diamond}$	0.479	0.629	0.493

*W-st*1 : Wald statistic for the hypothesis that *OSC*= *TSC*
$^{\diamond}$Experimental participants
***(0.01);** (0.05); *(0.1); significance level

The results show that across all specifications, anticipated two-sided communication has a positive and significant effect on participant X offers relative to either no communication or one-sided communication (see $W - st1$ statistics). In general, anticipated one-sided communication has no effect on X offers relative to no communication, but significantly decreases the likelihood that participant X splits the endowment fifty−fifty. Experience has no effect on participant X offers.

Observation 3 *X participants offer more if they anticipate two-sided communication. Anticipated one-sided communication generally has no effect*

[2]The random effects linear model is estimated using the two-stage FGLS estimator (Balestra and Nerlove, 1966), and the random effects probit models are estimated using maximum likelihood.

on X offers, but decreases the probability that X will split the endowment equally.

Analysis of Responder behavior

To analyze the rejection behavior of Y, we specify a multilevel logit model. The dependent variable *reject* takes the value one if Y rejects an offer of X, and equals zero if she accepts. To account for the non-monotonicity of a significant proportion of participant Y strategies, we include in our set of explanatory variables dummies for the possible offer levels of X (x_LOW for $x \in [1, 3]$, and x_HIGH for $x \in [7, 9]$).[3] Other explanatory variables as previously defined are *OSC*, *TSC*, and *Period.* The baseline offer level in the regression analysis is x_INT where $x \in [4, 6]$, and the baseline treatment is NC. The logit model takes the form:

$$p_i = prob(reject_i = 1) = f(\beta_0 + \beta' X_i)$$

where

$$\beta' X_i = \beta_1 OSC_i + \beta_2 TSC_i + \beta_3 x\_LOW_i + \beta_4 x\_HIGH_i + \beta_5 Period_i.$$

The model is estimated with GLLAMM (Stata), and we allow error terms to be correlated within sessions. Table 2.3 summarizes the results of the regressions.

[3] A feasible estimation approach with monotonic strategy-vectors is to define a minimum acceptance threshold for a responder, i.e., the minimum amount that the responder is willing to accept, and then analyze how it changes across treatments (see, for example, Zultan 2012).

Table 2.3: Analysis of Responder Choices (GLLAMM: logit)

	Coeff (Std. Err.)	
reject ∼	(1)	(2)
(Intercept)	-3.640 (0.744)***	-3.496 (0.382)***
OSC	0.646 (0.917)	0.345 (0.290)
TSC	0.944 (0.888)	0.851 (0.289)***
x_LOW	1.344 (0.740) *	1.386 (0.320)***
x_HIGH	3.487 (0.722)***	3.283 (0.315)***
Period	-0.088 (0.034)*	-0.088 (0.034)***
*OSC*x_LOW*	-0.075 (0.913)	
*OSC*x_HIGH*	-0.385 (0.891)	
*TSC*x_LOW*	0.155 (0.880)	
*TSC*x_HIGH*	-0.189 (0.862)	
*W-st*1	0.16	3.11*
*W-st*2	96.04***	318.35***
No. of observations (sessions)	2700 (6)	2700 (6)
Log-likelihood	-1301.80	-1302.77
Random effects variance◇	0.067	0.069

*W-st*1 : Wald statistic for the hypothesis that *OSC*= *TSC*
*W-st*1 : Wald statistic for the hypothesis that *x_LOW*= *x_HIGH*
◇Sessions
***(0.01);** (0.05); *(0.1); significance level

Beginning from a very general model in column (1) of the table, we eliminate insignificant interactions until we are left with the desired model in column (2). The results show that two-sided communication has a significant effect in increasing the likelihood that Y rejects an offer relative to either no communication or one-sided communication (see $W-St1$ statistic), whereas one-sided communication has no effect on the rejection behavior of Y relative to no communication. The non-monotonicity of Y choices exhibited in Figure 2.2 is confirmed by the probability model, where it emerges that either low- or high-offers of X increase the likelihood of rejection by Y participants, relative to intermediate offers. In addition, low offers significantly increase the likelihood of rejection relative to high offers (see $W - st2$ statistic), while rejection rates decline across periods.

Observation 4 *Y participants are more likely to reject X offers with two-sided communication, whereas one-sided communication has no effect on the rejection behavior of Y.*

Observation 5 *Unequal offers (both high and low) are more likely to be rejected by Y participants, whereas experience makes it less likely that an offer is rejected.*

2.3.3 Analysis of the informational content of messages

In the behavioral predictions section, following the anticipated communication results of Ellingsen and Johannesson, and Xiao and Houser, as well as the communication studies of Andreoni and Rao in the dictator game, and Charness and Dufwenberg in the trust game, we correctly predicted that one sided communication will have no effect on behavior of proposers in our experiment. We argued that from these studies, what appears to be of crucial importance is the feedback mechanism of communication, which can only occur if the responder is active in the communication. However, our experimental design differs from those implemented in previous studies in the sense that communication occurs after the proposer has committed to a binding offer, whereas the responder never definitively learns about the offer until the end of the game. Therefore, there is no way to determine a priori what the content of communication will be. We therefore implement a qualitative analysis of the informational content of communication in OSC and TSC, to see if we can spot differences in the content of communication between the treatments, which may in turn explain observed behavioral differences.

Our analysis strategy is to identify communication-content classifications that have previously been identified as having an effect on observed behavior in experiments. Roth (1995), and subsequently Zultan (2012), identified differences in responder behavior between game-related communication, and non-game-related communication (social conversations) in the ultimatum game.[4] Such a classification is feasible in our experiment because even though the content of communication in OSC and TSC are unrestricted ex-ante, proposers in OSC may choose to restrict content to non-game-related conversations since they unilaterally communicate with responders.

We categorize the content of communication as either *game-related* or *non-game-related*. Specifically, we define game-related communication as any communication that includes reference to a parameter in the ultimatum game, such as, offer, accept, reject, payoff, and earning. Non-game-related communication on the other hand is any communication that does not fall into the defined category. We add a third category, *No communication*, since participants in both treatments are given the option of not engaging in com-

[4]Recall that Zultan (2012) finds that responders behave less cooperatively under game-related communication. Therefore, the preliminary hypothesis is that the there is a significantly higher percentage of conversations that exclude game-related content in OSC relative to TSC.

munication if they so wish. The categorization was conducted using Stata, and Appendix 2.5.2 presents details of the procedure.

In total, there were twenty proposers in OSC, and twenty proposer-responder pairs in TSC, communicating over five periods resulting in a potential total of one hundred conversations per treatment. Figure 2.3 summarizes the breakdown of the conversations by content for the treatments.

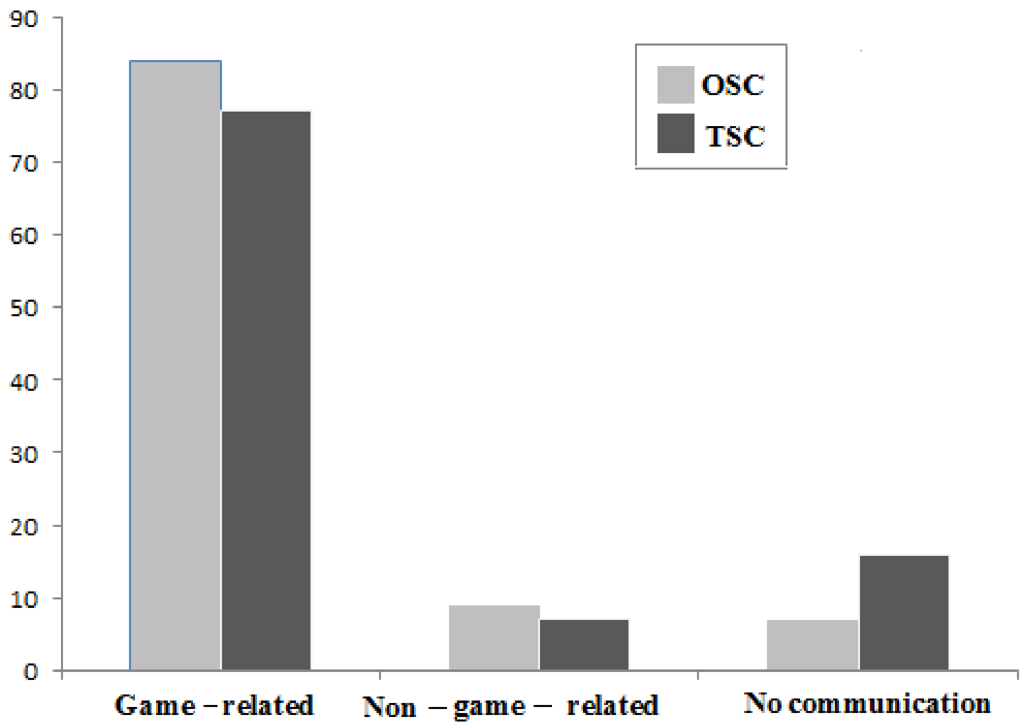

Figure 2.3: Communication breakdown by content

84/100 conversations in OSC relative to 77/100 in TSC included game-related content. Only 9/100 conversations were classified as non-game-related in OSC, relative to 7/100 in TSC.

Observation 6 *The vast majority of proposer and proposer-responder pair conversations is game-related.*

Since there is no significant difference in the proportion of game-related communication across treatments, we further refine the game-related category and consider the proportion of conversations in which the proposer explicitly stated the offer amount. In OSC, there was an explicit statement of the amount in 49/93 cases, relative to 49/84 cases in TSC. Out of this, 2/49 cases in OSC involved deception, compared to 1/49 in TSC, implying that most proposers who stated their offer amount were being truthful. The percentage differences in the number of conversations falling into either subcategory is not significant to explain differences in responder choices in OSC relative to TSC. In addition, we cannot attribute the differences to deception on the part of proposers.

An alternative classification

The previous categorization does not explain differences in behavior across our communication treatments, and therefore, it is likely that an alternative process is in effect. As in many instances involving social preferences, Fehr and Schmidt's inequality aversion model is robust in explaining behavior across a wide range of games. Following this theory, we test the degree to which communication included notions of fairness across treatments, thus potentially explaining the presence of higher offers and higher disagreement frequencies in TSC relative to OSC.

Our hypothesis is that there were more fairness-oriented conversations in TSC relative to OSC. With more notions of fairness prominent, responders have an induced expectation of fairness, and if this expectation is not met, then they are more willing to punish proposers, compared to absent the expectation. Concurrently, proposers anticipate responders' expectations, and respond positively to these expectations (the feedback mechanism).

To test whether there is evidence in support of the hypothesis, we analyze the content of messages in the communication treatments, focusing on the whether it included any fairness-oriented language. Therefore, we categorize a message as fairness-oriented if it includes a term referencing fairness such as fair, equal, equitable, even, half, fifty-fifty, same, and identical.[5] Otherwise, we categorize the message as non-fairness oriented. As with the previous case, the categorization was conducted using Stata, and Appendix 2.5.2 presents details of the procedure.

Figure 2.4 exhibits the distribution of messages that fall into our defined categories. In total, 32/100 conversations in OSC included fairness-oriented content relative to 53/100 in TSC. The mean offer for this category in OSC

[5]As a disclaimer, we acknowledge that equal is not necessarily fair. However, for most participants in the ultimatum game, there is a high positive correlation between the two concepts.

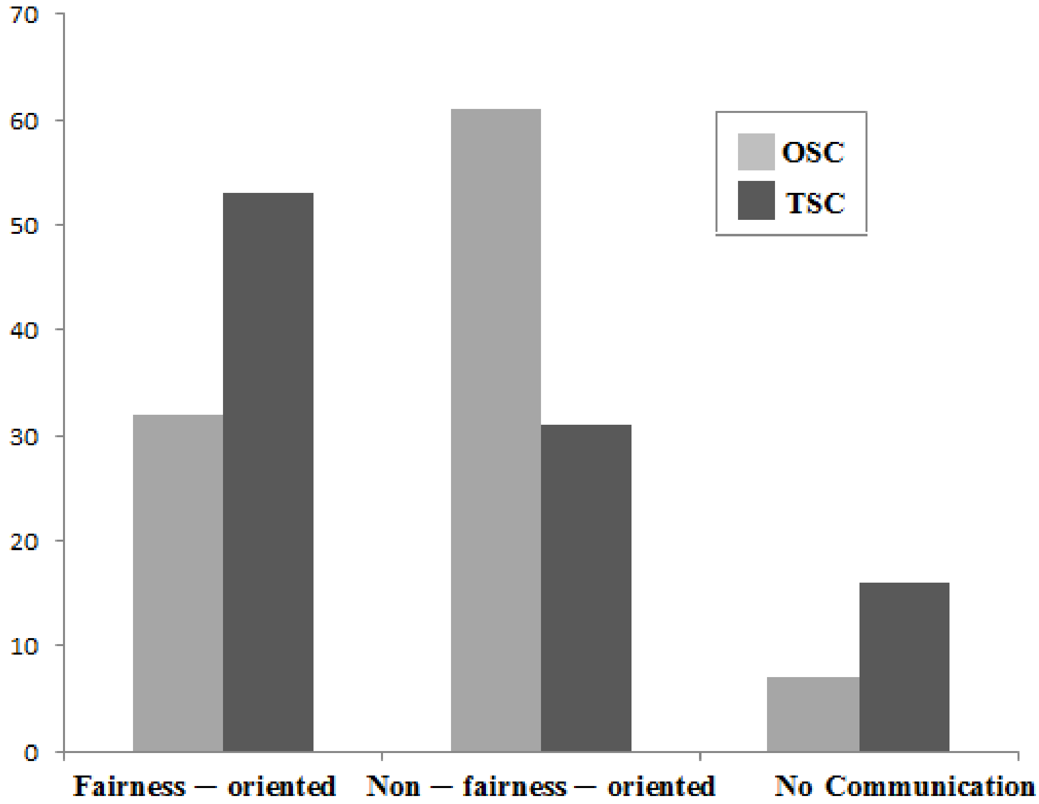

Figure 2.4: Fairness-oriented vs. Non-fairness-oriented content

and TSC was, respectively, ≈4.19 and ≈4.39. Conversely, 61/100 conversations in OSC, relative to 31/100 in TSC included non-fairness oriented content. The mean offer for this category in OSC and TSC was, respectively, ≈2.90 and ≈3.55.

Observation 7 *A significantly higher proportion of conversations in TSC includes fairness-oriented content. In both OSC and TSC, mean offers are significantly higher when the content of communication is fairness-oriented.*

When the content of communication is fairness-oriented, there is no significant difference in the amounts offered in OSC and TSC, but there is a significant difference in the relative frequency of such conversations between the treatments. One, however, has to exercise caution when reading this result as it relates to OSC. It is possible that causality runs from offer to communication, in the sense that proposers who offer higher amounts, and unilaterally communicate to responders, are more likely to engage in fairness-oriented conversations. However, what is not in doubt is that there is a clear anticipation effect of communication in TSC. The inclusion of responders into the conversation in TSC results in a significantly larger number of fairness oriented conversations in comparison to OSC, and proposers appear to anticipate such conversations, offering higher amounts on average, compared to OSC. The result provides support for the view that communication in unrestricted bargaining enhances cooperation by focusing participants' attention on a small number of fairness norms.

2.4 Conclusion

This paper has investigated whether the anticipated communication result of Xiao and Houser (2009), and Ellingsen and Johannesson (2007), is robust to the presence of strategic considerations. In these studies, it is observed that donations in the dictator game are significantly higher in relation to the standard (no-communication) condition when the allocator knows that she will receive feedback in the form of an anonymous written message from the recipient. We show that such anticipation effects still persist in the ultimatum game if communication is two-sided. Thus, even though one might imagine that the proposer in the ultimatum game mainly focuses on whether the responder will accept or reject her offer, it is also apparent that if the proposer knows that she will have to interact with the responder by exchanging anonymous written messages, then this additionally affects her allocation behavior. In other words, strategic considerations do not fully crowd out the anticipated communication effect.

Eliciting choices using the strategy method allowed us to study responder behavior at offer levels that are rarely observed in the actual course of play. Interestingly, this revealed a great deal of non-monotonicity in responder choices both in the presence, and absence of communication. Inequality aversion models such as that of Fehr and Schmidt implicitly assume that individuals suffer a psychological cost if they are either better off or worse off than other individuals (i.e., they dislike inequality). We provide evidence that a significant number of responders are willing to sacrifice their own material payoff to avoid inequality, even if this inequality is in their favor.

One-sided communication from the proposer does not result in behavior that differs with no communication, a result that mirrors Andreoni and Rao's result of one-sided communication from allocators to recipients in the dictator game. By analyzing the informational content of communication in our experiment, we observe that proposers restrict the content of communication to non-fairness oriented content when they unilaterally communicate with responders. This behavior can seemingly be explained by guilt aversion. Knowing that they have offered lower amounts (relative to the equal split), proposers want to avoid conversations that remind them of fairness, since such conversations may trigger a guilty conscience within them.

2.5 Appendix

2.5.1 Instructions (Translation from Italian)

[text corresponding to OSC treatment is shown in square brackets]

{text corresponding to TSC treatment is shown in braces}

Thank you for taking the time to attend this session. If you have any question at any point before, during or at the end of the experiment, please raise your hand and one of the experimenters will assist you. You are not allowed to talk to anyone else in the room except for the experimenters.

You will receive a show-up fee of €2.50 for taking part in this session. In addition, you have the opportunity to earn more money depending on the decisions that you and others make during the session. At the end of the session, you will personally be paid the total sum of your show-up fee and earnings in private.

The experiment will take place on a computer where you will be paired with a different individual at each period. There will be a total of five periods. At no point during or after the experiment will any individual know the identities of individuals that he or she is paired with across the periods.

Decision Tasks

At the very beginning, the computer will randomly assign you either the role of X or Y. Once this assignment is complete, you will remain in that role for the remainder of the session. If you are assigned the role of X, you will be paired with an individual assigned the role of Y and vice-versa. Your earnings will depend on the decisions that you make in your pair.

On the computer screen, each individual X will select one of nine divisions of €10. These divisions are (€1, €9), (€2, €8), (€3, €7), (€4, €6), (€5, €5), (€6, €4), (€7, €3), (€8, €2), (€9, €1), where the Euro amounts within the parenthesis represent (Amount to X, Amount to Y) respectively.

Not knowing the choice of X, for each of the nine divisions, Y has to indicate whether he or she accepts or rejects. If Y accepts, then X and Y both receive the amounts as per the division. If Y rejects, then both X and Y receive €0. Note that there will be a three minute time-gap between the choice of X, which temporally comes first, and Y's decision.

[A message

X has the option of sending a message(s) to Y prior to Y choosing whether to accept or reject the offers corresponding to each division. However, the message(s) will be after X has chosen one of the nine divisions. The following sequence illustrates the chronology of events:

$$X\text{'s choice} \rightarrow \text{optional message(s) from } X \text{ to } Y \rightarrow Y\text{'s decision}$$

There will be a message window on the computer screen where X can write a message(s) to Y within the three minute time-gap between X's choice and Y's decision. If X does not intend to send a message(s) to Y, then he or she can click on the button labeled "no message" at the bottom right hand corner of the screen. If this happens, then Y will be notified that X has chosen not to send any message(s). At any point within the allotted three minutes, X can send a message(s) to Y regardless of whether he or she had earlier opted not to.

In the message(s), X is not allowed to identify him or herself. Therefore, he or she cannot include personal details such as name, gender, appearance, age, address, phone number, and program or year of study. (Experimenters will monitor the message(s). Violations (to the discretion of the experimenters) will result in X forfeiting the €2.50 show-up fee and leaving the session with no earnings. The paired Y will receive the average amount received by other Y's.) Apart from these restrictions, X may say anything that he or she wishes in the message(s).

{Messages

Both X and Y have the option of sending messages to each other prior to Y choosing whether to accept or reject the offers corresponding to each division. However, the messages will be after X has chosen one of the nine divisions. The following sequence illustrates the chronology of events:

$$X\text{'s choice} \rightarrow \text{optional messages between } X \text{ and } Y \rightarrow Y\text{'s decision}$$

There will be a message window on the computer screen and both X and Y can send messages to each other within the three minute time-gap between X's choice and Y's decision. If either X or Y does not intend to send a message(s) to the other, then he or she can click on the button labeled "no message" at the bottom right hand corner of the screen. If this happens, then the paired participant will be notified that either X or Y has chosen not to send any message and subsequently, he or she will decide whether to send a message(s) to him or her. At any point within the allotted three minutes, X and Y can send a message(s) to the paired participant regardless of whether they had earlier opted not to.

In the messages, both X and Y are not allowed to identify themselves. Therefore, they cannot include personal details such as name, gender, appearance, age, address, phone number, and program or year of study. (Experimenters will monitor the messages. Violations (to the discretion of the experimenters) will result in the violator forfeiting the €2.50 show-up fee and leaving the session with no earnings. The paired individual will receive the average amount received by other participants.) Apart from these restrictions, both X and Y may say anything that they wish in the messages.

Earnings

Out of the five periods, one period will be randomly selected for payment. Total earnings at the end of the experiment for both X and Y will be the sum of the show-up fee and earnings in the period that is randomly selected.

2.5.2 Categorization procedure

All statistical analysis and categorization of messages in this paper was conducted using Stata. Included in each observation, which is a row in Stata, was a string variable of maximum length 244 characters (Stata type str244), which contained the entire message of the proposer in OSC, and proposer-responder pair in TSC in a given period. All punctuation marks were removed from the messages prior to the creation of the variable, and all characters were converted into lower-case. In the instances where a participant(s) chose not to engage in communication, the variable had the entry "NO MESSAGE".

For the first category, we identified a list of key words that reference a parameter in the ultimatum game, including ultimatum, game, offer, accept, reject, payoff, euro, currency, period, earnings (and different combinations of these: in Italian), as well as numbers representing monetary amounts.

Following identification of key words, we used the -inlist- command in Stata to generate an indicator variable that took the value one if a message included game-related content, and zero otherwise. However, prior to this, a loop was used to separate the message variable into constituent word variables using Stata's string function -word-, so that the software could handle the length of messages.

For the sake of illustration, we translate three messages from the list of messages in TSC and OSC, and explain the procedure.

Example message from TSC:

X I decided to offer you 6 leaving me with 4.

Y You did not split half-half?

X No I chose a little less for myself.

This message exchange is entered into Stata as follows:

"i decided to offer you six leaving me with four you did not split half half no i chose a little bit less for myself"

Example message from OSC:

X Hello, it's hot outside today... isn't it?

This message is entered into Stata as follows:

"hello it is hot outside today is it not"

Example message from OSC or TSC:

X or X-Y pair [No communication occurred]

This message is entered into Stata as follows:

"NO MESSAGE"

The commands below enter the data above into Stata, and separate the message variable into constituent word variables using a loop (note that here we generate a maximum of 25 word variables because the longest message has 25 words):

```
input str244 message
"i decided to offer you six leaving me with four you did not split
half half no i chose a little bit less for myself"
"hello it is hot outside today is it not"
"NO MESSAGE"
end
forvalues i = 1(1)25 {
gen word‘i’=word( message, ‘i’)
}
```

Finally, we use another loop to identify the occurrence of our defined keywords in the messages. A version of the following (expanded to include all combinations of words, e.g., accepts, accepted, etc.) generate an indicator variable which we label "game_related" that tells us how many conversations included our defined keywords.

```
forvalues i = 1(1)25 {
gen game_r‘i’ = inlist(word‘i’, "ultimatum", "offer", "accept", "reject",
"payoff", "euro", "currency", "earn")
}
egen game_related= rowtotal( game_r1 - game_r25)
replace game_related=1 if game_related>1
list message game_related
```

The resulting Stata output is as shown below. It is clear from the messages that only the first one has a game related content.

```
. list  message game_related

  1.  | message                                                              |
      | i decided to offer you six leaving me with four you did not split ha.|
      |----------------------------------------------------------------------|
      |                                                      game_r~d        |
      |                                                             1        |

  2.  | message                                                              |
      | hello it is hot outside today is it not                              |
      |----------------------------------------------------------------------|
      |                                                      game_r~d        |
      |                                                             0        |

  3.  | message                                                              |
      | NO MESSAGE                                                           |
      |----------------------------------------------------------------------|
      |                                                      game_r~d        |
      |                                                             0        |
```

Figure 2.5: Stata Output

For the second category, we included all synonyms of the word "fair" that we could identify as keywords, including, reasonable, equal, half, and generous. The ensuing categorization followed the above description.

Chapter 3

Communication, sequentiality and strategic power: A prisoners' dilemma experiment

with Luigi Mittone

3.1 Introduction

In two-player sequential game experiments, there is usually an asymmetry in the observed payoffs of players. For example, the average distribution of stakes on offer favors first-movers in the ultimatum game (see, for example, Güth *et al.*, 1982; Cameron, 1999; Henrich, 2000; Andersen *et al.*, 2011). A meta-analysis of over 30 standard ultimatum game experiments shows that the average first-mover offer to the second-mover is 40 percent of the pie, and this share is even smaller for larger pie-sizes. Concurrently, second-movers only frequently reject offers of less than 20 percent, independent of the size of the pie (Oosterbeek *et al.*, 2004). In the investment game, on the other hand, the picture is reversed and the distribution of final payoffs favors second-movers (refer to Berg *et al.*, 1995; Willinger *et al.*, 2003; Buchan *et al.*, 2008). Based on a meta-analysis of over 130 investment game experiments, Johnson and Mislin (2011) find that the average fraction of first-mover endowment sent to second-movers is 50 percent, whereas the average proportion sent by second-movers to first-movers is 37 percent. In the usual multiplier condition where the amount sent by the first mover is trebled, it can be shown that for any positive endowment, this ratio will always lead to higher average payoffs for second movers.[1]

Differences in average payoffs may proxy strategic power, and have been shown to influence how experimental participants value positions in two-player sequential games. In an earlier study of the ultimatum game, Güth and Tietz (1986) employed the second-price auction in an experiment eliciting position values in the game, and found the position of first-mover is twice as valuable as that of second-mover. Other studies such as Hoffman et al. (1994)

[1]Denote the initial endowment amount as π where $\pi > 0$. The 50:37 ratio implies that in cases where the second mover is allowed to send part of her endowment, her final payoff is 1.575π (i.e. $0.63(\pi + (3 \times 0.5\pi))$, compared to 1.425π (i.e. $0.5\pi + 0.37(\pi + (3 \times 0.5\pi))$ for the first mover. When the second mover cannot send her endowment (which is the standard case), the difference is even more pronounced, with the first mover having a final payoff of 1.055π (i.e. $0.5\pi + 0.37(3 \times 0.5\pi)$), compared to 1.945π (i.e. $\pi + 0.63(3 \times 0.5\pi)$) for the second mover.

have considered the strategic first mover advantage in the ultimatum game as implicit, allowing experimental participants to compete for the position of first mover in an entitlement condition, as opposed to that of second mover.

One variable that has been observed to influence the first-mover-second-mover payoff asymmetry in sequential games is pre-play communication. In trust games, Charness and Dufwenberg (2006) argue that communication in the form of promises from second movers enhance cooperation due to "guilt aversion". Second movers promise to cooperate conditional on first movers cooperating. In a large number of cases, first movers do cooperate, and second movers are thus bound to make good on their promises because failure to do so triggers a psychological cost in terms of guilt. In dictator games, where the first mover has absolute power and the second mover is obliged to accept whatever fraction of initial endowment that is allocated, Andreoni and Rao (2011) show that communication in the form of requests from second movers is significant in increasing donations. For them, communication from the second mover to the first mover increases the first mover's empathy with the second mover.

In both studies above, among a list of others in the literature, explanations emphasize the strategic role of communication in enhancing pro-social behavior, and thereby influencing payoff asymmetries in two-player sequential game experiments. However, a lagging question that remains is whether non-strategic communication is just as efficient in inducing similar outcomes. Along these lines, Roth (1995) showed that in a two-player bargaining game, if experimental participants are not allowed to discuss the bargaining game at hand, then this results in significantly higher levels of cooperation between bargaining parties compared to the absence of communication, but not significantly different to when communication is strategic. However, Roth relied on face-to-face communication, and subsequent studies have emphasized the need to include a visual-identification-only treatment in face-to-face communication experiments, to detect any identification-based confounds. Bohnet and Frey (1999a) showed that in the dictator game, mutual identification, which entails the first- and second-mover visually identifying each other but no verbal communication, results in significantly higher first-mover amounts allocated relative to no communication and anonymity, but not significantly different to communication (i.e. where participants both visually identify each other and communicate verbally). For Bohnet and Frey, identification strengthens social or cultural propensities for fairness and decreases social distance, thereby allowing the emergence of empathy for the first mover in the dictator game.

To obtain clean data on the effect of communication, this study employs written anonymous communication. Additionally, so as not to explicitly constrain the content of communication as in Roth (1995), we propose changing the sequence of communication so that its effect is non-strategic. Therefore, communication occurs after the first mover has made a binding decision, but before the second mover has made any decisions. Communication thus cannot change the first-mover's decision in the period that it occurs, and this is common knowledge to both the first- and second-mover. We distinguish between two communication forms: one-sided communication from the second mover to the first mover, and two-sided-communication.

A natural comparison to sequential games, where resulting first-mover-second-mover payoff asymmetries are interpreted as strategic power differences, are simultaneous move symmetric games. Such games are characterized by symmetry of payoffs and actions, and no player has a strategic power advantage. In experiments involving the games, there is no distinction between row- and column-participants, as defined in the normal form representations of the games. Thus, the experimentalist will usually collect responses of paired participants and pool them when conducting subsequent analysis, signaling an implicit belief that the row and column roles are equivalent. We thus choose a simultaneous move prisoners' dilemma to represent a balanced strategic power environment. The logical complement to the prisoners' dilemma is the sequential prisoners' dilemma (a version of Kreps' 1990 trust game). However, we choose a sequential move investment game, since it is very similar to the sequential prisoners' dilemma, and it is the standard measure of trust in the experimental economics literature. Furthermore, it allows us to collect more detailed data on the behavior of the second mover using the strategy method (Selten, 1967).

The investment game can be considered a form of continuous prisoners' dilemma, and we calibrate payoffs in the game such that all payoff pairs in the prisoners' dilemma can be obtained by executing a particular sequence of play. However, there is a fundamental difference between the two games. As we have suggested above for symmetric simultaneous move games, in the prisoners' dilemma, there is no first-mover and second-mover distinction that is present in the investment game due to symmetry of actions and payoffs. Part of the reason behind why the second-mover has more strategic power in the investment game is the fact that following the first-movers move, the game is effectively a dictator game in which the second-mover decides how to allocate her total wealth between herself and the first-mover.[2]

Introducing our communication sequence to the prisoners' dilemma allows us to determine which set of players move first, and which set move second. Even though, temporally, this allows us to categorize first-movers and second-movers in this game, it still remains a simultaneous move game because what is of strategic importance is the information that players posses about actions of other players, which is never revealed before play concludes. Furthermore, communication in theory allows the first-mover to reveal her action to the second mover. However, there is always an incentive not to reveal the truth, which is apparent to the second mover. Thus, unlike pure coordination games with a dominant Pareto-efficient Nash equilibrium where our sequence of communication would allow players to coordinate on this equilibrium, in the prisoners' dilemma this would not occur.

We show that communication has a significant effect in inducing payoff

[2]The term "dictator" in the dictator game explicitly hints at the extreme situation where one player (the allocator) has absolute power.

asymmetries in two-player symmetric games where a priori, no player has an apparent power advantage. In the absence of communication, as we would expect, outcomes do not differ between (row) first- and (column) second-movers in the prisoners' dilemma, and there are no payoff asymmetries. However, first-movers behave more cooperatively in the presence of two-sided communication relative to either the absence of communication, or one-sided communication. Second movers on the other hand do not behave as cooperatively in the prisoners' dilemma with two-sided communication, thereby resulting in a payoff advantage accruing to them. The sequential structure of the investment game in the absence of communication results in significantly different outcomes for first- and second-movers, in line with findings from previous studies. The results of communication for first movers mainly mirror those from the prisoners' dilemma where we observe that two-sided communication induces higher offers. One-sided communication results in significantly lower levels of cooperation relative to either no-communication, or two-sided communication, a result that mirrors that of Andreoni and Rao (2011) in the dictator game.

The paper is organized as follows: Sec. 3.2.1 presents the experimental design. Sec. 3.2.2 outlines our behavioral predictions. Sec 3.2.3 describes the experimental procedures and protocols. Sec 3.3 presents the results. Eventually, Sec. 3.4 provides a discussion and concludes.

3.2 Method

The introduction of communication changes the structure of the investment game and prisoner's dilemma by adding a move in the extensive form representations of the games. The following experimental design describes the games and details the experimental treatments and protocols.

3.2.1 Experimental design

In our setting, X represents the first-mover in the investment game, and row player in the prisoners' dilemma. Correspondingly, Y represents the second-mover, and column player, respectively.

We employ the strategy method to elicit the full strategy vector of Y in the investment game. In this game, X chooses an amount x to send to Y from an endowment of 10, in units of 1, resulting in eleven possible amounts: $x \in \{0, 1, 2, \ldots, 10\}$. Before being forwarded to Y, the amount x is multiplied by a factor of 2, so that Y receives $2x$. Y, also endowed with 10, decides on an amount y to send to X from her total wealth, that is, $10 + 2x$. The only restriction that we impose is that y must be an integer within the bounds ($y \in \mathbb{Z}$ such that $0 \leq y \leq 10 + 2x$). A strategy of Y assigns y to each x choice of X, and is an eleven-element vector collected by having Y fill in a table similar to Table 3.1. The payoffs to X and Y, respectively, are:

$$\pi^X = 10 - x + y \tag{3.1}$$

$$\pi^Y = 10 + 2x - y. \tag{3.2}$$

Table 3.1: Investment game with strategy method: Y's decision task

x	0	1	2	3	4	5	6	7	8	9	10
$2x$	0	2	4	6	8	10	12	14	16	18	20
$10 + 2x$	10	12	14	16	18	20	22	24	26	28	30
y	□	□	□	□	□	□	□	□	□	□	□

In the prisoners' dilemma, X chooses one of two actions: $a_1^X = up$, or $a_2^X = down$. In turn, Y chooses one of two actions: $a_1^Y = left$, or $a_2^Y = right$. An outcome of the game is an action pair, which we denote a, where: $a^1 = (a_1^X, a_1^Y)$, $a^2 = (a_1^X, a_2^Y)$, $a^3 = (a_2^X, a_1^Y)$, and $a^4 = (a_2^X, a_2^Y)$. The payoffs to X and Y, respectively, are: $\pi^X = 15$ and $\pi^Y = 15$ if the outcome is a^1; $\pi^X = 0$ and $\pi^Y = 30$ if a^2; $\pi^X = 30$ and $\pi^Y = 0$ if a^3; and, $\pi^X = 10$ and $\pi^Y = 10$ if a^4. The normal form representation in Figure 3.1 exhibits payoffs in the game. As illustrated in the 2×2 matrix, the prisoners' dilemma is symmetric in the sense that payoffs for playing a particular action depend only on the other actions employed, not on who is playing them. We rename actions in this game to explicitly distinguish between the actions of the row player and the column player.

Before Y makes her decision(s), but after X has made a binding deci-

	left	*right*
up	15, 15	0, 30
down	30, 0	10, 10

Figure 3.1: Payoffs in the prisoners' dilemma

sion, there is a communication phase in each of the game experiments. We distinguish three treatments:

NC (No Communication): The standard investment game with strategy method, and prisoners' dilemma with a time gap between X's decision, which occurs first, and Y's decision(s).

OSC (One-Sided Communication): Following X's decision, Y can choose to unilaterally communicates to X in writing for two minutes, after which Y makes her decision(s).

TSC (Two-Sided Communication): Following X's decision, both X and Y can choose to communicate to each other in writing for two minutes, after which Y makes her decision(s).

The treatments are implemented in a between-subject design. However, due to the minimal decision tasks in the prisoners' dilemma (one decision per participant per period), we implement both games within subjects. Thus, participants in one session take part in a investment game followed by a prisoners' dilemma.

Payoff Equivalence

Our design of the games is such that all payoff pairs in the prisoners' dilemma can be obtained by executing a particular sequence of play in the investment game. This is summarized below:

- If X chooses $x = 10$ and Y chooses $y = 15$ in the investment game, then the payoff corresponds to outcome a^1 in the prisoners' dilemma;
- If $x = 10$ and $y = 0$, outcome a^2;
- If $x = 10$ and $y = 30$, outcome a^3;
- and, if $x = 0$ and $y = 0$, outcome a^4.

3.2.2 Behavioral predictions

The rational selfish prediction of behavior in the investment game and prisoners' dilemma is represented by the last combination of payoffs above ($x = 0$ and $y = 0$, and outcome a^4, respectively). Applying backward induction to determine the subgame perfect strategy, X evaluates π^Y in Eq. 3.2 and recognizes that it is declining in y, i.e., $\left(\frac{\partial \pi^Y}{\partial y}\right) < 0$. Anticipating that Y will set $y = 0$, now X's payoff in Eq. 3.1 becomes $\pi^{X'} = 10 - x$, and she sets $x = 0$ because $\pi^{X'}$ is declining in x, i.e., $\left(\frac{\partial \pi^{X'}}{\partial x}\right) < 0$. An analogous argument holds for Y. In the prisoners' dilemma in Fig. 3.1, only $a^4 = (down,\ right)$ is a pure strategy Nash Equilibrium outcome since $\pi^X(down,\ right) > \pi^X(up,\ right)$, and $\pi^Y(down,\ right) > \pi^Y(down,\ left)$, implying that neither X nor Y has an incentive to deviate from the outcome.[3] Neither outcome is Pareto efficient; for example, both X and Y are better off if X sends her full endowment and Y returns half of her total wealth in the investment game, and both cooperate in the Prisoners' dilemma ($x = 10$ and $y = 15$, and outcome a^1, respectively). Additionally, under selfish preferences, communication does not influence the predicted outcome.

Numerous studies, however, have shown that participants in one-shot investment game and prisoners' dilemma experiments do not conform to the rational selfish prediction.[4] Furthermore, communication has been observed to increase cooperation levels in a wide range of games (see reviews by Crawford, 1998; Roth, 1995). Based on previous investment game and prisoners' dilemma studies, and the symmetry property of the prisoners' dilemma, we predict the following outcomes for the NC treatment

H1 Some X and Y participants will send positive amounts in the investment game, but average final payoffs of Y will be higher.

H2 Some X and Y participants will choose the cooperative action in the prisoners' dilemma, but there will be no difference in average final payoffs.

[3] In the first quadrant of the matrix, both players have an incentive to deviate from the outcome since $15 > 0$. In the second (third) quadrant, X (Y) has an incentive to deviate since $30 > 10$.

[4] In repeated games, the folk theorem asserts that cooperative behavior can emerge and be sustained if the game has an unknown end-point, and players are sufficiently patient. Therefore, with frequent repeated and non-anonymous interaction, socially beneficial behavior may endogenously develop as a result of reputation (and the ensuing threat of punishment).

The fact that cooperative behavior emerges even in the absence of communication, and under double-blind conditions (see for example, Berg *et al.* (1995), in the investment game) suggests that trust and reciprocity is a social norm. In the investment game, the first-mover signals trust by sending a positive amount, whereas the second mover signals trustworthiness by reciprocating trust. The introduction of communication additionally influences the trust and reciprocity dynamics in the investment game. Charness and Dufwenberg's study reviewed in the introductory section shows how written pre-play communication of a strategic nature can induce higher contributions by first movers, and enhance cooperative behavior generally. Our design rules out effects of strategic communication on behavior, so to enable us formulate predictions for the communication treatments, we rely on a few studies have implemented a communication sequence similar to ours.

Xiao and Houser (2005) allow the second mover to attach a written message to her accept/ reject decision in the ultimatum game after receiving a binding offer from the first mover. They observe that first mover offers do not differ to the standard condition, but second movers reject unfair offers significantly less frequently. Thus, it appears that second movers expression of negative emotions for what they perceive to be unfair offers decreases the likelihood that they reject such offers. This result suggests that communication supplements costly punishment by providing an additional medium where an aggrieved party can express her emotions. In the dictator game, Ellingsen and Johannesson (2007) find that first movers (allocators) if anticipating written messages from second movers (recipients) after their allocation decisions, will act more other-regarding, with donations in the communication treatment being significantly higher to donations in the standard treatment. Xiao and Houser (2009) replicate this result, and speculate that allocators have a preference for avoiding written expression of disapproval, or negative emotions, which explains their enhanced pro-social behavior.

An additional hypothesis articulated by Hoffman *et al.* (1996) is that communication induces cooperative behavior by reducing the social distance between interacting parties. In the absence of communication and with anonymity, there is social isolation of an individual's decision. On the other hand, with communication, there is a social interaction, and individuals potentially have to justify their past and future choices. In dictator game studies (for example, some of those reviewed in the introductory section), decreasing social distance has been shown to increase contributions by first movers. However, since second movers unilaterally communicate to first movers in our OSC treatment, if they choose not to exercise this right (since communication is optional in the experiments), then this leaves open the possibility that communication will not reduce the social distance between pairs of participants. In the TSC treatment, for this to happen, it must be the case that both paired participants choose not to communicate simultaneously, and the odds of this happening are small. Therefore, we expect a less strong communication effect in OSC relative to TSC. Based on the anticipated written communication results highlighted, and the social distance hypothesis, we formulate the following hypothesis for the communication treatments:

H3 X and Y participants will act more cooperatively in TSC relative to either OSC or NC in the investment game and prisoners' dilemma.

H4 X and Y participants will act more cooperatively in OSC relative to NC in the investment game and prisoners' dilemma.

Cooperation rates do not automatically translate to differences in payoffs between players because it is possible to observe high levels of cooperation in one treatment relative to another, whereas within treatments, no differences in first- and second-mover payoffs are observed. Therefore, we formulate the following hypothesis regarding payoff asymmetries in the investment game:

H5 Differences in the average payoffs of X and Y participants will always favor Y participants, but the largest difference will be observed in the NC treatment, and the smallest difference in the TSC treatment.

Therefore, the above hypothesis states that higher levels of cooperation between players in TSC will reduce the observed asymmetry in the average payoffs of the first- and second-movers. By virtue of symmetry of actions and payoffs, we do not expect that there will be an asymmetry in the average payoffs of first- and second-movers in the prisoners' dilemma within each treatment, but based on the predicted levels of cooperation, we expect that average payoffs will differ between treatments.

H6 There will be no difference in the average payoffs of X and Y participants within each treatment, but average payoffs will be highest in TSC and lowest in NC.

3.2.3 Experimental procedures

Experimental sessions took place at the Cognitive and Experimental Economics Laboratory (CEEL), at the University of Trento. Participants were undergraduate students from the institution were recruited from an in-house list of volunteers who had signed up to be considered for future participation in experiments. Three sessions representing the aforementioned treatments were conducted, and a session was divided into two parts, each of five periods. The first part of a session involved play of the investment game with strategy method, while the second part, the prisoners' dilemma.

Upon arrival at the laboratory, each participant was randomly assigned the role of X or Y, and maintained this role for the remainder of the session. Before the start of each part of a session, participants were given a set of written instructions to read through privately (see Appendix 3.5.1 for the translated text). A member of the experimental staff thereafter read aloud the instructions, and once it was ascertained that everyone had understood them, the experiment started. A single-blind design was utilized, and the $X-Y$ pairs across periods were determined using a stranger matching protocol.

At the end of each part of a session, one of the five periods was randomly selected for payment. Each part of a session accounted for half of the earnings from the session, alongside a show-up fee of € 2.50. At the end of the session, Experimental Currency Units (ECU), the exchange medium used in the experiment, were converted to Euros at a rate of 3 ECU= € 1. Participants received their dues in private prior to leaving the laboratory.

3.3 Results

3.3.1 Descriptive statistics

Choices of X

We begin with an overview of the choices of X participants across the three treatments. Figure 3.2 summarizes the distribution of individual level average amounts sent by first-movers in the investment game, and the proportion of row-players choosing the cooperative action in the prisoners' dilemma.

In the former, the average amount is about twice as high in TSC compared to NC and OSC. The non-parametric Wilcoxon rank-sum test indicates that this difference (from pair-wise comparisons) is statistically significant ($p < 0.001$). However, there is no significant difference in average amounts in NC and OSC ($p = 0.123$). Correspondingly, the proportion of row player participants who choose the cooperative action $a_1^X = up$ in the prisoners' dilemma is higher in TSC (approx. 24%) compared to NC and OSC (approx. 7% and 9%, respectively). Wilcoxon rank-sum tests confirm that TSC and

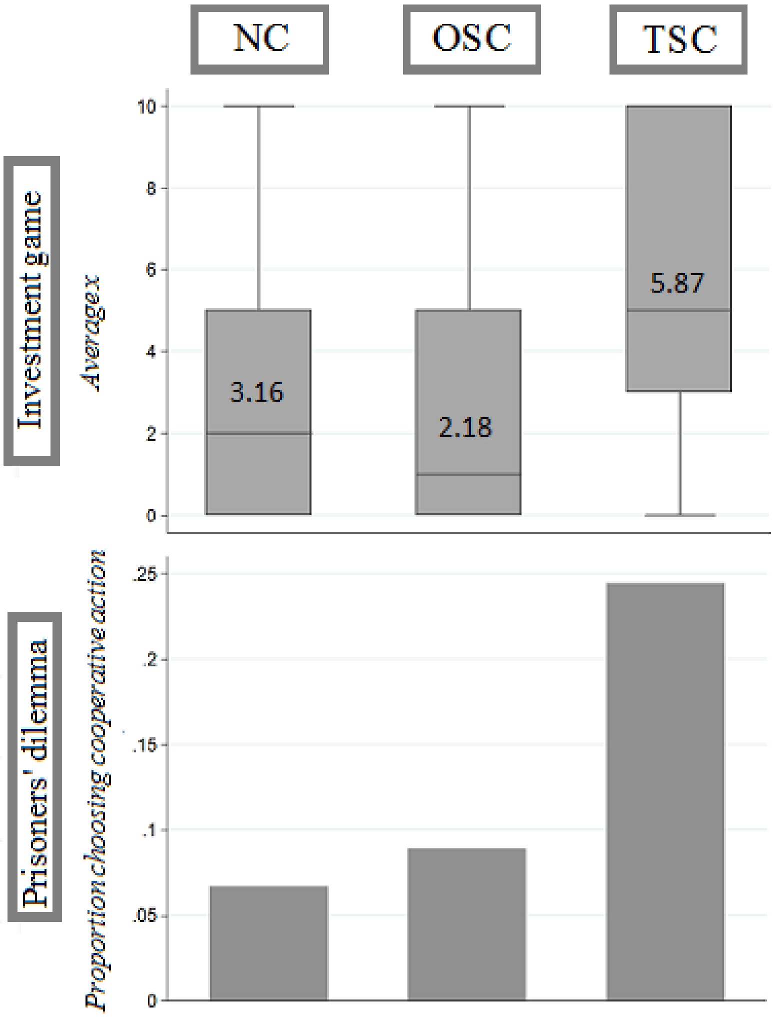

Figure 3.2: Choices of X participants (average at individual level for investment game). Figures within boxes represent mean values.

NC, and TSC and OSC differ ($p = 0.021$, and $p = 0.049$, respectively), whereas NC and OSC do not differ ($p = 0.696$).

Observation 8 *X participants behave more cooperatively in the presence of two-sided communication. One-sided communication has no effect on the* *behavior of X relative to no communication.*

The above result establishes some consistency with regards to the effect of communication on the behavior of X participants across both games. Whether or not two-sided communication results in more cooperation generally, depends also on the behavior of Y participants analyzed below.

Choices of Y

Figure 3.3 exhibits the distribution of individual-level average choices of Y in the investment game conditional on potential choices of X.

For low and intermediate levels of x (i.e., $0 - 3$ and $4 - 6$, respectively), average amounts are generally higher in TSC relative to either NC or OSC, and NC relative to OSC, whereas for high levels of x (i.e., $7 - 10$), average amounts are higher in both NC and TSC relative to OSC. A series of Wilcoxon rank-sum tests indicate that the differences are significant for low and intermediate levels. For high levels, the rank-sum tests show that choices of Y do not differ between OSC and TSC, but differ between NC and OSC and between NC and TSC.

Observation 9 *The effect of two-sided communication is conditional on the level of trust displayed by X: For low levels, Y participants cooperate more relative to no communication, whereas behavior does not differ for high levels.*

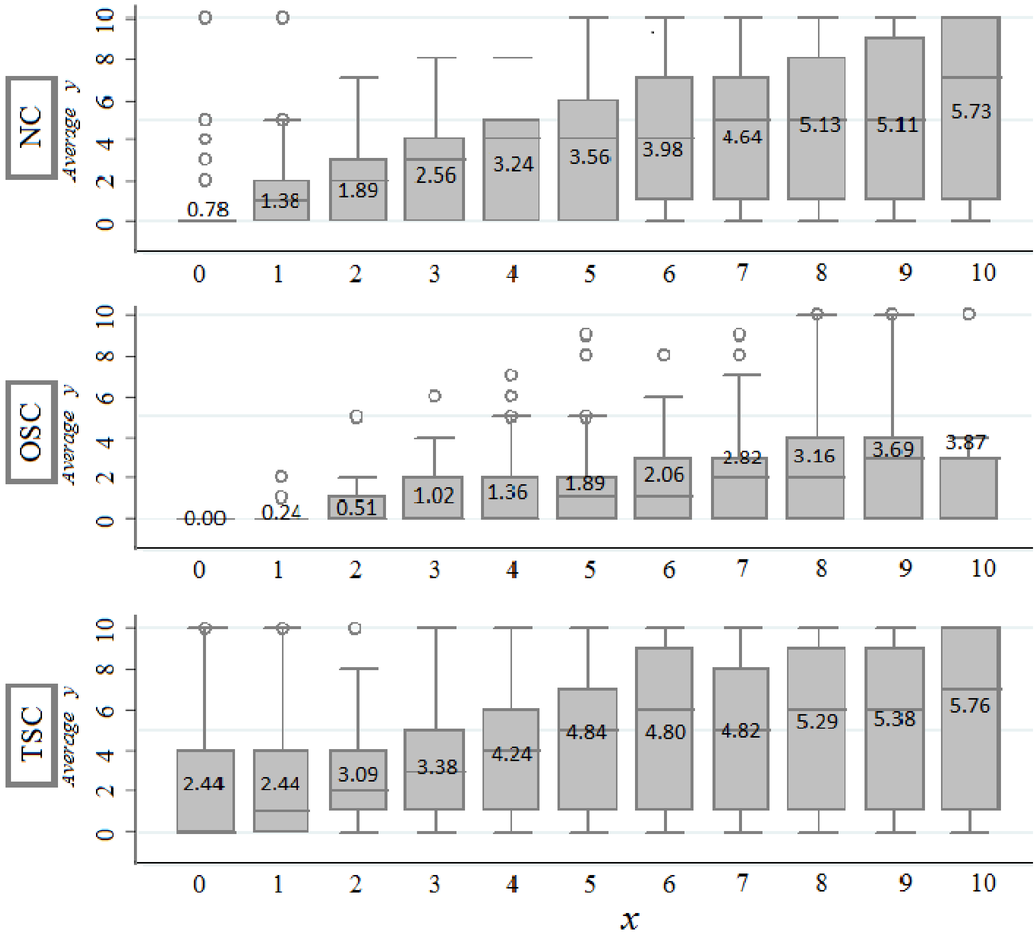

Figure 3.3: Choices of Y participants in the investment game (average at the individual level). Figures within boxes represent mean values.

Observation 10 *One-sided communication induces lower-contributions relative to either no communication or two-sided communication, irrespective of the level of trust displayed by X.*

The proportion of column-player participants choosing the cooperative action $a_1^Y = left$ in the prisoners' dilemma is exactly equal in NC and TSC

($\approx 9\%$), and slightly lower in OSC ($< 5\%$). Wilcoxon rank-sum tests show that the difference between OSC and either of the other two treatments is not significant ($\rho = 0.170$).

Observation 11 *Generally, Y participants display low levels of cooperation in the prisoners' dilemma, with no behavioral differences across treatments.*

Within treatment $X - Y$ payoff asymmetries

Figure 3.4 displays the average payoffs of X and Y participants in the prisoners' dilemma across treatments.

Wilcoxon rank sum tests indicate that there is no asymmetry in X and Y average payoffs in NC ($\rho = 0.561$), whereas average payoffs are significantly higher for Y participants in both OSC and TSC ($\rho = 0.059$ and $\rho = 0.005$, respectively). A between treatment comparison of payoffs for X participants reveals that average payoffs are just only higher in NC relative to TSC ($\rho = 0.099$). For Y participants, average payoffs are higher in TSC relative to NC ($\rho = 0.030$).

Observation 12 *Average payoffs of row and column participants do not differ in the absence of communication in the prisoners' dilemma. However, with communication, average payoffs are higher for column participants.*

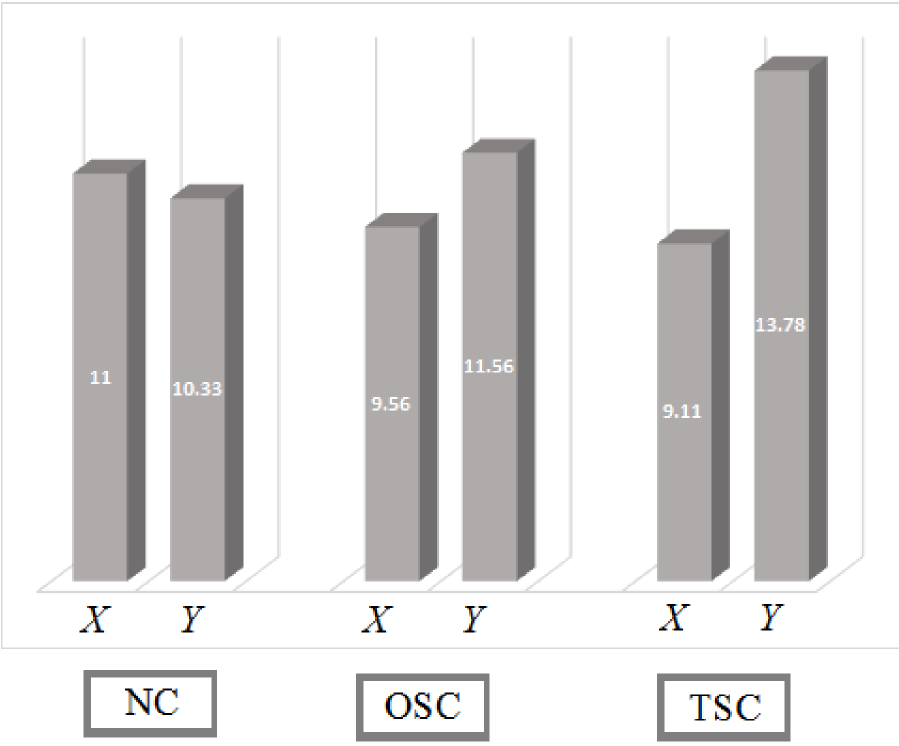

Figure 3.4: Average payoffs of X and Y participants across treatments in the prisoners' dilemma. Figures within boxes represent ECU amounts.

Figure 3.5 exhibits the average payoffs of X and Y participants in the investment game across treatments.

Wilcoxon rank sum tests indicate that average payoffs are significantly higher for Y participants relative to X participants in all three treatments ($\rho < 0.001$). A between treatment comparison of average payoffs of X participants reveals that they differ between NC and OSC ($\rho < 0.001$), OSC and TSC ($\rho < 0.001$), but not between NC and TSC ($\rho = 0.112$). For

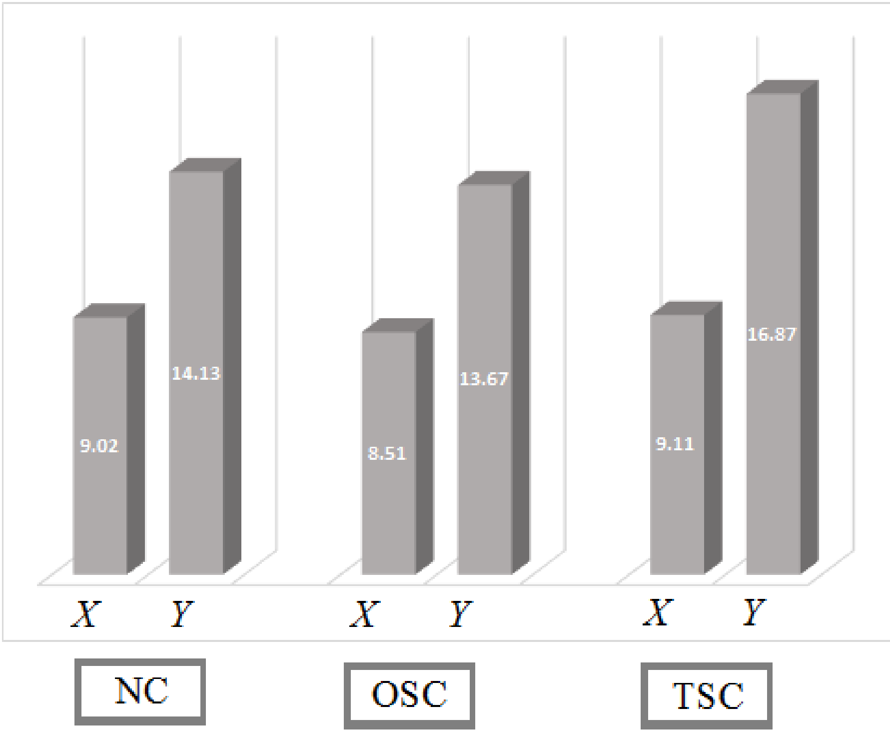

Figure 3.5: Average payoffs of X and Y participants across treatments in the investment game. Figures within boxes represent ECU amounts.

Y participants, average payoffs are higher in TSC relative to NC and OSC ($\rho < 0.001$), but do not differ between NC and OSC ($\rho = 0.254$).

Observation 13 *Average payoffs are higher for second movers relative to first movers in the investment game across all three treatments.*

In relation to our behavioral predictions in Sec. 3.2.2, the results support hypotheses H1-H2. Hypothesis H3 is supported for X participants, but not for Y participants. Hypothesis H4 is completely rejected. Hypothesis H5 is supported with the following qualification: differences in average payoffs are greatest in TSC, and do not vary much between OSC and NC. Apart from there being no differences in $X - Y$ payoffs in NC, and average Y payoffs being significantly higher in TSC , Hypothesis H6 is overwhelmingly rejected.

3.3.2 Regression analysis

We further analyze the effect of communication on participants' decision to cooperate in the prisoners' dilemma, examining whether there are differences between row- and column-player participants' decisions across treatments. Furthermore, exploiting the broader range of data on the intended behavior of Y participants across all levels of X offers (provided by the strategy method), we investigate the effect of communication on reciprocity. Our emphasis in the investment game is not on how communication affects how much Y participants reciprocate, but rather on how it affects their decision to reciprocate. Therefore, we define an indicator outcome variable

$$y^* = \begin{cases} 1 & \text{if } y \geq x \\ 0 & \text{otherwise} \end{cases} \tag{3.3}$$

Our estimation model thus takes the form

$$p_i = \text{Probability}(y^* = 1) = f(\beta_0 + \beta' Q_i) \tag{3.4}$$

where

$$\beta' Q_i = \beta_1 \text{OSC}_i + \beta_2 \text{TSC}_i + \beta_3 \text{x\_INT}_i + \beta_4 \text{x\_HIGH}_i + \beta_5 \text{Period}_i.$$

The specification controls for the main effects of the treatments (where NC is the baseline category), the impact of experimental periods, and the level of trust displayed by X (where x_INT is an indicator variable that takes the value one if $x \in [4, 6]$, x_HIGH for $x \in [7, 10]$, x_LOW for $x \in [0, 3]$; and zero otherwise). We estimate a random intercept binomial logit model using GLLAMM (Stata), where we account for nested random effects arising from repeated choices of participants and their random assignment into matching groups at each experimental period. Table 3.2 presents estimates of the model. Beginning from a very general model (column 1), we eliminate the insignificant interaction effects and are left with the desired model in column 2.

The results show that one-sided communication decreases the probability of Y reciprocating relative to no communication, whereas two-sided communication increases this probability. Relative to one-sided communication, two-sided communication significantly increases the probability of Y reciprocating (see W-st1). Both intermediate and higher levels of X offers decrease the probability of Y reciprocating relative to low levels of X offers, with higher offers having a stronger effect (which is significant also relative to one-sided communication, indicated by W-st2). The interaction between

Table 3.2: Communication and Reciprocity (Random-intercept logit)

Reciprocity∼	Coeff (Std. Err.)	
	(1)	(2)
(Intercept)	1.081 (0.569)*	1.173 (0.556)**
OSC	-2.364 (0.325)***	-2.456 (0.303)***
TSC	0.530 (0.299)*	0.334 (0.158)**
x_INT	-0.526 (0.286)*	-0.545 (0.206)***
x_HIGH	-0.972 (0.270)***	-1.188 (0.196)***
Period	-0.227 (0.163)	-0.226 (0.162)
*OSC*x_INT*	0.863 (0.441)*	0.882 (0.394)**
*OSC*x_HIGH*	1.296 (0.416)***	1.511 (0.372)***
*TSC*x_INT*	-0.046 (0.413)	
*TSC*x_HIGH*	-0.440 (0.386)	
*W-st*1	81.27***	82.84***
*W-st*2	13.04***	37.70***
Log likelihood	-720.5	-721.4

*W-st*1 : Wald statistic for the hypothesis *OSC= TSC*
*W-st*2 : Wald statistic for the hypothesis *x_INT= x_HIGH*
***(0.01);** (0.05);* (0.1) significance level

one-sided communication and the offer levels of X are significant, whereas the interactions with two-sided communication are not. The probability of Y reciprocating decreases with time, but the effect is not significant.

Observation 14 *Reciprocity of Y participants depends both on the form of communication and the trust level displayed by X. Two-sided communication has a positive effect, and one-sided communication has a negative effect. Higher participant X offer levels decrease the likelihood of reciprocity.*

To analyze the choice to cooperate in the prisoners' dilemma, we specify the following model:

$$p_i = \text{Probability}(a_1^j = \text{Cooperate}) = f(\beta_0 + \beta' S_i); \quad j \in \{X, Y\} \tag{3.5}$$

where

$$\beta' S_i = \beta_1 \text{OSC}_i + \beta_2 \text{TSC}_i + \beta_3 \text{X\_Part}_i + \beta_4 \text{Period}_i.$$

X_Part in the specification above is a participant X indicator variable which allows us to compare differences between cooperative behavior of X and Y participants. As previously stated, the cooperative action is $a_1^X = \text{up}$ for X participants (row-players), and $a_1^Y = \text{left}$ for Y participants (column players).

Table 3.3: Communication and Cooperation (Random-intercept logit)

Cooperate∼	Coeff (Std. Err.)	
	(1)	(2)
(Intercept)	-1.619 (0.789)**	-2.268 (0.755)***
OSC	-1.576 (1.179)	-0.400 (0.638)
TSC	-0.001 (0.787)	0.999 (0.530)*
X_Part	-0.361 (0.884)	0.887 (0.520)*
Period	-0.411 (0.195)**	-.397 (0.187)**
*OSC*X_Part*	1.925 (1.452)	
*TSC*X_Part*	1.777 (1.104)	
*W-st*1	2.01	6.85**
Log likelihood	-77.6	-79.2

*W-st*1 : Wald statistic for the hypothesis *OSC*= *TSC*
***(0.01);** (0.05);* (0.1) significance level

Beginning with the very general model in column 1, we eliminate insignificant interactions until we are left with the desired model in column 2. From the results, we observe that one-sided communication has no significant effect on the probability that a participant chooses the cooperative action in the prisoners' dilemma relative to no communication, whereas two-sided communication increases this probability. In addition, two-sided communication relative to one-sided communication also increases the probability that a participant chooses the cooperative action (see W-st1). Being an X participant increases the probability of choosing the cooperative action relative to being a Y participant. Finally, it emerges that the probability of choosing the cooperative action declines over time as participants acquire more experience.

Observation 15 *The probability of choosing the cooperative action in the prisoners' dilemma is positively related to two-sided communication and being temporally the first mover, and negatively related to experience.*

Our final analysis looks at the effect of communication on how profitable trust is for X participants in the investment game. To measure this profitability of trust, we define the following outcome variable which computes the rate of return on the amount that X forwards to Y.

$$\text{Rate of return (\%)} = \left(\frac{y}{x} - 1\right) * 100; \quad x \neq 0 \tag{3.6}$$

We estimate a random intercept linear model with the same set of regressors in Table 3.2, restricting our sample to strictly positive values of y (i.e., $y > 0$).

Table 3.4: Communication and profitability of trust (random-intercept linear)

Rate of return (%)∼	Coeff (Std. Err.) Reciprocators ($y > 0$)
(Intercept)	55.989 (15.846)***
OSC	-73.244 (15.931)***
TSC	75.206 (12.610)***
x_INT	-59.753 (12.379)***
x_HIGH	-77.884 (11.559)***
Period	-1.162 (4.111)
*OSC*x_INT*	39.255 (20.261)*
*OSC*x_HIGH*	61.491 (19.078)***
*TSC*x_INT*	-52.341 (17.172)***
*TSC*x_HIGH*	-76.055 (16.088)***
*W-st*1	97.52***
*W-st*2	46.84***
Log likelihood	-5430.5

*W-st*1 : Wald statistic for the hypothesis *OSC*= *TSC*
*W-st*2 : Wald statistic for the hypothesis *x_INT*= *x_HIGH*
***(0.01);** (0.05);* (0.1) significance level

The results show that like reciprocity, both one-sided communication, intermediate- and high-offer levels of X (relative to low offer levels) have a negative effect on the profitability of trust, whereas two-sided communication has a positive effect. All interaction terms are significant, implying that effect of communication depends on the level of trust displayed by X, in both cases with higher levels corresponding to stronger effects of communication. Period is not significant, as was the case in Table 3.2.

Observation 16 *The profitability of trust depends mainly on the same factors that affect reciprocity of* Y *participants. However, unlike reciprocity, the form of communication interacts with the levels of trust displayed by* X*.*

3.4 Discussion

Before delving into other themes, it may be beneficial to first address the main focus of this study which is the strategic advantage that arises from the structure of sequential games, and how communication affects this. As expected, we find that our results for the no communication treatments are consistent with those from previous studies: there are no behavioral differences between first-movers (row players), and second-movers (column players) in the prisoners' dilemma, whereas second-movers are significantly better off (in payoff terms) relative to first-movers in the investment game. However, we observe that once we introduce communication into the mix, first-mover behavior in the investment game does not differ between the no-communication- and one-sided-communication treatments, whereas first-movers make significantly higher offers in the presence of two-sided communication. In the prisoners' dilemma, we also observe a similar pattern: first-movers (row players) choose the cooperative action more than twice as often in the two-sided communication treatment relative to either the one-sided- or no-communication-treatment. This result leaves us to explain why two-sided communication significantly induces first movers to cooperate across both games, whereas one sided communication does not. Previous studies suggest that our communication sequence can account for this pattern of behavior. The anticipated communication findings of Ellingsen and Johannesson (2007) and Xiao and Houser (2009) reviewed under our behavioral predictions (Sec. 3.2.2) showed that in dictator games, if first movers anticipate written communication from second movers following their allocation decision, their donations are significantly higher relative to the standard no-communication condition. However, unlike the dictator game where the only motivation for the first mover to donate is altruism, it appears from our results that this anticipation effect extends to environments where there are strategic considerations, for example, whether the second mover reciprocates in the investment game, or whether she in turn cooperates in the prisoners' dilemma.

The one-sided communication result for first movers replicates the behavior of second movers under the same condition, and is best addressed in a more general context. Second movers in the prisoners' dilemma choose the cooperative action with significantly less frequency, resulting in no differences in cooperation rates across all three treatments. In the investment game, on the other hand, the effect of two-sided communication is conditional on the first mover's offer level: for low levels ($0-30\%$ of endowment), second movers send higher amounts relative to both one-sided and no-communication. However, for high levels ($70-100\%$ of endowment), behavior in the two-sided communication treatment does not differ with the no-communication treatment. On the other hand, one-sided communication, relative to either two-sided-communication or no-communication, results in significantly lower amounts sent by second movers. This result that one sided communication leads to the same or lower levels of cooperation relative to no communication replicates what could have been an easily overlooked result by Andreoni and Rao (2011). In their study, they found that in dictator games where the allocator sends a written message to the recipient along with the allocated amount (and the recipient stays silent), the amount allocated is significantly lower relative to all other treatments (no communication, two-sided communication, and one-sided communication from recipient). To summarize this result in a general context, when a decision maker is allowed to unilaterally communicate prior to making her allocation decision, she behaves less other-regarding. Our results additionally suggest that this self-regarding behavior extends to environments where strategic considerations are present, and is not isolated to the dictator game.

We follow Andreoni and Rao's intuition of this result. A better way to think about this is to ask why are cooperation levels higher with two sided communication? Somehow, it must be the case that two-sided communication is affecting the utility function of the decision maker. If we consider an Andreoni and Bernheim (2009) utility function for the decision maker, where utility is split between consumption utility and social utility, two sided communication affects the social utility component. In this framework, the decision maker not only cares about fairness, but also gains additional utility if others perceive her to be fair, and perceptions depend on how much she decides to give. Andreoni and Rao hypothesize that if the allocator unilaterally communicates to the recipient in the dictator game, then this disrupts the empathy-altruism mechanism, or more generally self- and social-signalling.

Thus, our replication of Andreoni and Rao's result in a non-dictator-game context suggests that social utility remains an important consideration even in the presence of strategic considerations, such as reciprocity.

The rich data afforded by the strategy method in the investment game allowed us to gain an in depth analysis of how communication affects the reciprocity of second movers, and the profitability of trust, which is dependent on first mover offer levels and the corresponding responses of second movers. We found that intermediate- ($40 - 60\%$ of endowment) and high-first mover offers decrease the probability that second movers reciprocate, as does one-sided communication, whereas two-sided communication increases the probability that second movers reciprocate. The profitability of trust on the other hand is mainly influenced by the same factors, except that the form of communication interacts with the levels of first mover offers, implying that effect of communication is dependent on how much first movers choose to send.

In conclusion, this paper has identified and implemented an experimental design that examines the effect of non-strategic communication form on the strategic advantage that arises from the structure of sequential games. A sequential move investment game, and a simultaneous move prisoners' dilemma were chosen to represent an asymmetric power environment, and a balanced power environment, respectively. Previous studies have shown that second-movers in the investment game are strategically better off in payoff terms relative to first-movers. In the prisoners' dilemma, even though players can make their moves at different points in time applying our communication sequence, symmetry of actions and payoffs, and the information available to players at the time they make their moves implies that there is no strategic advantage to being either a first- or second-mover (i.e., the game is still a simultaneous move game despite the fact that moves are not themselves simultaneous in time). We show that anticipated communication has a significant effect in inducing payoff asymmetries (and hence power differences) in symmetric games. This anticipated communication effect cannot be attributed to the fact that we temporally separate the move of the row- and column-player in the prisoners' dilemma, since the no-communication treatment shows that even with such a separation, in the absence of communication, outcomes do not differ between first- and second-movers.

3.5 Appendix

3.5.1 Instructions (Translation from Italian)

PART A: General

Thank you for taking the time to attend this session. If you have any questions, please raise your hand and one of the experimenters will assist you. You are not allowed to talk to anyone else in the room except for the experimenters.

You will receive a show-up fee of €2.50 for taking part in this session. In addition, you have the opportunity to earn more money depending on the decisions that you and others make during the session. At the end of the session, you will personally be paid the sum of your show-up fee and earnings in private.

The experiment will take place on a computer where you will be paired with a different participant at each period. There will be a total of ten periods. At no point during or after the experiment will you know the identities of participants that you are paired with across the periods.

PART A: Decision tasks - NC

At the very beginning, the computer will randomly assign you either the role of X or Y. Once this assignment is complete, you will remain in that role for the remainder of the session. If you are assigned the role of X, you will be paired with a participant assigned the role of Y, and vice-versa. Your earnings will depend on the decisions that you make in your pair.

The session will be divided into two sections of 5 periods. When the first five periods are over, you will receive instructions for periods 6–10. Therefore, the following decision tasks relate ONLY to the first five periods.

All participants will be endowed with a sum of 10 Experimental Currency Units (ECU) at the start of each period. On the computer screen, participant X will decide how much of his or her endowment to send to participant Y by choosing one the following eleven options:

0 ECU	1 ECU	2 ECU	3 ECU	4 ECU	5 ECU	6 ECU	7 ECU	8 ECU	9 ECU	10 ECU

The choice of participant X is denoted x. The amount x will be multiplied by a factor of 2 before being sent to participant Y, so that participant Y receives $2x$. Not knowing the actual choice of participant X, for each of the eleven possible x choices of participant X, participant Y has to decide on an amount y to send to participant X from his or her total wealth, that is, the initial endowment of 10 ECU plus double the amount sent by participant X. Participant Y will thus fill in the y values in a table similar to the one below:

x	0	1	2	3	4	5	6	7	8	9	10
$2x$	0	2	4	6	8	10	12	14	16	18	20
$10 + 2x$	10	12	14	16	18	20	22	24	26	28	30
y	□	□	□	□	□	□	□	□	□	□	□

The only restrictions on the y values is that they must be whole numbers. Otherwise, participant Y can fill in any amount ranging from 0 ECU to his or her maximum wealth, indicated by the value of $10 + 2x$ at each column in the table.

Participant$X$'s choice $\rightarrow$ Participant$Y$'s decisions.

PART A: Decision tasks - OSC

[Decision task text as in NC above]

A Message

Participant Y has the option to send a message(s) to participant X prior to deciding the amounts y corresponding to each of the eleven possible x choices of participant X. However, the message(s) will be after participant X has chosen the amount x to send to participant Y. The following sequence illustrates the chronology of events:

$$X \text{ chooses} \rightarrow \text{optional message(s) from } Y \text{ to } X \rightarrow Y\text{'s decisions}$$

There will be a message window on the computer screen where participant Y can write a message(s) to participant X within the two minute time-gap between participant X's choice and participant Y's decision. If participant Y does not intend to send a message(s) to participant X, then he or she can click on the button labeled "no message" in the computer screen. If this happens, then participant X will be notified that participant Y has chosen not to send any message(s). At any point within the allotted two minutes, participant Y can send a message(s) to participant X regardless of whether he or she had earlier opted not to do so.

In the message(s), participant Y is not allowed to identify him or herself.

Therefore, he or she cannot include personal details such as name, gender, appearance, age, address, phone number, and program or year of study. In addition, the use of offensive language is not allowed. (Experimenters will monitor the message(s). Violations (to the discretion of the experimenters) will result in participant Y forfeiting the €2.50 show-up fee and leaving the session with no earnings. The paired participant X will receive the average amount received by other X participants.) Apart from these restrictions, participant Y may say anything that he or she wishes in the message(s).

PART A: Decision tasks - TSC

[Decision task text as in NC above]

Messages

Both participant X and participant Y have the option of sending messages to each other prior to participant Y deciding the amounts y corresponding to each of the eleven possible x choices of participant X. However, the messages will be after participant X has chosen the amount x to send to participant Y. The following sequence illustrates the chronology of events:

$$X \text{ chooses} \rightarrow \text{optional message(s) between } X \text{ and } Y \rightarrow Y\text{'s decisions}$$

There will be a message window on the computer screen where both participant X and participant Y can send messages to each other within the two minute time-gap between participant X's choice and participant Y's decisions. If either participant X or participant Y does not intend to send a message(s) to the other, then he or she can click on the button labeled "no message" in the computer screen. If this happens, then then the paired participant will be notified that either participant X or participant Y has chosen not to send any message(s). At any point within the allotted two minutes, participant X and participant Y can send a message(s) to the paired participant regardless of whether they had earlier opted not to do so.

In the message(s), both participant X and participant Y are not allowed to identify themselves. Therefore, they cannot include personal details such as name, gender, appearance, age, address, phone number, and program or year of study. In addition, the use of offensive language is not allowed. (Experimenters will monitor the message(s). Violations (to the discretion of the experimenters) will result in the violating participant forfeiting the €2.50 show-up fee and leaving the session with no earnings. The paired participant will receive the average amount received by other X or Y participants.) Apart from these restrictions, both participant X and participant Y may say anything that they wish in the messages.

PART A: Earnings - General

Out of the five periods, one period will be randomly selected for payment. ECU will be exchanged at a rate of 3 ECU = €1. The earnings in the randomly selected period will constitute half of the earnings from the experiment, the other half having being determined in periods 1–5. Therefore, final payments at the end of the experiment for each participant X and participant Y will be the sum of the show-up fee, 0.5 $\times$ (earnings in periods 1–5), and 0.5 $\times$ (earnings in periods 6–10).

PART B: General

The following instructions relate to the second section of the experiment (periods 6–10). If you were assigned the role of X or Y in periods 1–5, you remain in this role for the remainder of the experiment.

You will be paired with a different participant at each period, and at no point during or after the experiment will you know the identities of participants that you are paired with across the periods. Your earnings will depend on the decisions that you make in your pair.

PART B: Decision tasks - NC

Participant X will choose one of two actions – *up* or *down* – from the following matrix:

	left	*right*
up	15, 15	0, 30
down	30, 0	10, 10

where numbers in the matrix represent ECU. Not knowing the action chosen by participant X, participant Y will choose an action – *left* or *right* – from the same matrix. The payoff at each period is determined as follows:

- If participant X chooses *up* and participant Y chooses *left*, then each gets 15 ECU.
- If participant X chooses *up* and participant Y chooses *right*, then participant X gets 0 ECU and participant Y gets 30 ECU.
- If participant X chooses *down* and participant Y chooses *left*, then participant X gets 30 ECU and participant Y gets 0 ECU.
- Finally, if participant X chooses *down* and participant Y chooses *right*, then each gets 10 ECU.

Participant X's choice $\rightarrow$ Participant Y's choice.

PART B: Decision tasks - OSC

[Decision task text as in NC above]

A Message

Participant Y has the option to send a message(s) to participant X prior to choosing an action. However, the message(s) will be after participant X

has chosen an action. The following sequence illustrates the chronology of events:

X chooses an action $\rightarrow$ optional message(s) from Y to X $\rightarrow$ Y chooses an action

There will be a message window on the computer screen where participant Y can write a message(s) to participant X within the two minute time-gap between participant X's choice and participant Y's choice. If participant Y does not intend to send a message(s) to participant X, then he or she can click on the button labeled "no message" in the computer screen. If this happens, then participant X will be notified that participant Y has chosen not to send any message(s). At any point within the allotted two minutes, participant Y can send a message(s) to participant X regardless of whether he or she had earlier opted not to do so.

In the message(s), participant Y is not allowed to identify him or herself. Therefore, he or she cannot include personal details such as name, gender, appearance, age, address, phone number, and program or year of study. In addition, the use of offensive language is not allowed. (Experimenters will monitor the message(s). Violations (to the discretion of the experimenters) will result in participant Y forfeiting the €2.50 show-up fee and leaving the session with no earnings. The paired participant X will receive the average amount received by other X participants.) Apart from these restrictions, participant Y may say anything that he or she wishes in the message(s).

PART B: Decision tasks - TSC

[Decision task text as in NC above]

Messages

Both participant X and participant Y have the option of sending messages to each other prior to participant Y choosing an action. However, the messages will be after participant X has chosen an action. The following sequence illustrates the chronology of events:

X chooses an action $\rightarrow$ optional message(s) between X and Y $\rightarrow$ Y chooses an action

There will be a message window on the computer screen where both participant X and participant Y can send messages to each other within the two minute time-gap between participant X's choice and participant Y's choice. If either participant X or participant Y does not intend to send a message(s) to the other, then he or she can click on the button labeled "no message" in the computer screen. If this happens, then then the paired participant will be notified that either participant X or participant Y has chosen not to send any message(s). At any point within the allotted two minutes, participant X and participant Y can send a message(s) to the paired participant regardless of whether they had earlier opted not to do so.

In the message(s), both participant X and participant Y are not allowed to identify themselves. Therefore, they cannot include personal details such as name, gender, appearance, age, address, phone number, and program or year of study. In addition, the use of offensive language is not allowed. (Experimenters will monitor the message(s). Violations (to the discretion of the experimenters) will result in the violating participant forfeiting the €2.50 show-up fee and leaving the session with no earnings. The paired participant will receive the average amount received by other X or Y participants.) Apart from these restrictions, both participant X and participant Y may say anything that they wish in the messages.

PART B: Earnings - General

Out of the five periods, one period will be randomly selected for payment. ECU will be exchanged at a rate of 3 ECU = €1. The earnings in the randomly selected period will constitute half of the earnings from the experiment, the other half having being determined in periods 1–5. Therefore, final payments at the end of the experiment for each participant X and participant Y will be the sum of the show-up fee, 0.5 × (earnings in periods 1–5), and 0.5 × (earnings in periods 6–10).

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