Choosing a Cellmate in the Prisoner's Dilemma

An experimental study

by

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Preface

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Eirik André Strømland, 01.09.2014

Abstract

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This thesis investigates cooperative behavior in a repeated Prisoner's Dilemma using experimental methods. In the experiment, we allow subjects to form voluntary partnerships by mutual choice, and to communicate through a chat room. Three main research questions were pursued. First, we wanted to show that mutual partner choice could increase cooperation in an environment with a simple matching mechanism. Also, we wanted to study whether there are positive spillover effects between partner choice and communication. Finally, we wanted to replicate a finding than partner choice opportunities induce strategic behavior ("competitive altruism") in humans.

Our study makes several novel contributions to the existing literature. We show that mutual partner choice increases cooperation. We find that through partner choice, the game is transformed from a random process to repeated and stable interactions. The competitive altruism hypothesis is supported. We find no effect of partner choice when chat room communication is allowed. We believe communication and partner choice both fail to increase cooperation in the most selfish subjects. Therefore, there is little room for a further effect of partner choice when communication is possible.

The experiment was computerized using the experimental software z-Tree 3.3.8. Results were analyzed using the statistical software STATA/IC 13.1 and Microsoft Excel 2010. The Meltzer Fund and the Department of Economics financed the project.

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Chapter 1: Introduction

"Where people seldom deal with one another, we find that they are somewhat disposed to cheat, because they can gain more by a smart trick than they can lose by the injury it does to their character. […] Wherever dealings are frequent, a man does not expect to gain so much by any one contract as by probity and punctuality in the whole, and a prudent dealer […] would rather choose to lose what he has a right to than give any ground for suspicion."

- Adam Smith (1766/1978: 538-539)

In the Prisoner's Dilemma, two players are stuck on a cell with a given partner and cannot influence who they are paired with. Defection is the dominant strategy, while cooperation is the social optimum. This makes the game a social dilemma. By pursuing their self-interest, both players loose.

In reality, we often choose our partners. Friendships form voluntarily, and we may either maintain our current friendships or form new ones. Maghreb traders in the Middle Ages used a "coalition" with membership granted based on individual reputations (Greif 1989). Jewelers situated in New York strive to attain membership in exclusive dealers' clubs to gain access to mutually beneficial trades (Bernstein 1992). On eBay, users may choose their sellers (Tennie et al. 2010). Repeated interactions, together with the opportunity for partner choice, constitute a reputation mechanism that makes defection unattractive. Cheating leads to exclusion from profitable partnerships. Smith's "discipline of frequent dealings" paints a less pessimistic picture of social dilemmas.

In this thesis, we study a repeated prisoner's dilemma game where subjects may choose their partners. Previous experimental studies on mutual choice have "filtered" choices through a complex algorithm (Coricelli et al. 2004; Bayer 2011). This makes it somewhat difficult to interpret their results. The treatment effect may partially reflect the effect of the "filter" they have chosen. In this thesis, we cut the "middle man" and study a game where individual choices are the sole determinant of a partnership.

We also implement an information structure where players attain information through private experience. This is similar to a recent study by Huck et al. (2012), but has not so far been extended to a mutual choice setting. This also means that we must allow players to explore the environment. We therefore design the experiment so that matching occurs in every period.

We also study chat room communication together with partner choice. Communication is often involved in partner choice. We use communication to enter agreements. When we enter partnerships, we discuss relevant problems and coordinate upon common strategies. To our knowledge, no former studies have studied these variables together in a controlled experiment. Tullock (1999) conducted a demonstrative experiment with partner choice and communication, but did not include control and treatment groups and so could not isolate treatment effects.

When partner choice is possible, the "competitive altruism" hypothesis predicts that cooperation should increase because players will engage in costly signaling in order to gain access to profitable partnerships (Roberts 1998). In contrast to former studies, we test for competitive altruism in a between-subjects design. And in our experimental game, the players do not know when they will reap the benefits from signaling cooperative intentions.

We make the following behavioral predictions. In addition to providing incentives to signal generosity, partner choice permits conditional cooperators to selectively interact and avoid free riders (Tiebout 1956; Page et al. 2005; Brekke et al. 2011). For these two reasons, partner choice should increase cooperation.

Communication should increase cooperation, because it allows conditional cooperators to coordinate their beliefs through universal promising and build group identity (Dawes et al. 1988). Communication may also assist coordination in the partner selection stage. In addition, communication potentially enhances the reach of a costly signal by allowing for information sharing (Alden Smith 2010). For these reasons, we predict a positive interaction effect between partner choice and communication.

The rest of this thesis proceeds as follows. In chapter 2, some relevant game theoretic predictions are derived. Chapter 3 discusses relevant previous literature, and explains the novelties in our design. Chapter 4 contains a summary of the experimental design and procedures. Chapter 5 features descriptive statistics and a data analysis. Chapter 6 summarizes and discusses the main findings and concludes the thesis.

Chapter 2: Theoretical considerations

This thesis investigates cooperative behavior in a repeated Prisoner's Dilemma game where players may communicate and choose their partner. We implement the payoff structure of the Public Goods Game (PGG). In the PGG, each player receives a sum of money either to share or to keep.[1] Players must decide how much to cooperate, rather than just make a binary choice. This continuous strategy space adds a layer of realism to the standard prisoner's dilemma. Often, the decision to cooperate is not a question of "whether to" but rather of "how much". The PGG is an N-player generalization of the Prisoner's Dilemma.[2] When we consider the game with two players, we may therefore use the terms interchangeably.

2.1 Game theoretic predictions

Stage game

Let the set $\mathcal{N}$ represent a population of selfish and rational individuals. From this population, we randomly draw a pair of individuals $\{i, j\}$. For the moment, we consider the one-shot stage game. Each player receives an endowment equal to e. She might keep this for herself, or invest some in a public good. Decisions are simultaneous. With two selfish players, the payoff function is as follows:

$$u_i = (e - c_i) + \alpha(c_i + c_j) \quad i, j = (1,2), (2,1) \qquad [2.1]$$

Here, c_i is the player's investment in the public good and α is the private marginal benefit of investing in the public good.[3] While the first part of the payoff is private, the second part is shared. Neither player can exclude the other from enjoying the public good, nor does this

[1] This type of game is called a "Voluntary Contribution Mechanism" game (VCM), which refers to the decision to contribute voluntarily to a public project.
[2] In fact, it can be shown that the behavioral predictions from standard assumptions are invariant to the number of players. The argument proceeds identically, but only adds additional notation.
[3] Often refered to as the "Marginal Per Capita Return" (MPCR).

enjoyment affect the "quality" of the good. In the two-player situation, we may think of this payoff component as positive externalities conferred between the players.[4]

The Nash equilibrium (c_i^*, c_j^*) of the stage game satisfies the following condition:

$$u_i\left(c_i^*, c_j^*\right) \geq u_i\left(c_i', c_j^*\right) \, \forall c_i' \in [0, e] \, for \; i, j = (1,2), (2,1) \qquad [2.2]$$

An equilibrium strategy profile is a vector of chosen strategies such that neither player could experience a payoff gain from altering her strategy. Player i's contribution choice must be an optimal response to player j's contribution choice, and vice versa.

The optimization problem for the individual i is as follows:

$$Max_{c_i}\{u_i\} = Max_{c_i}\left\{e - c_i + \alpha(c_i + c_j) : 0 \leq c_i \leq e\right\} \qquad [2.3]$$

Each individual maximizes her utility by making a contribution choice. Her contribution cannot be less than zero, and cannot exceed her total endowment.

Problem 2.3 solves for $c_i = \begin{cases} e \; if \; \alpha \geq 1 \\ 0 \; if \; \alpha < 1 \end{cases} \; \forall i \in \mathcal{N}$ [2.4]

Regardless of who we draw from the population, both defect in Nash equilibrium when $\alpha < 1$. This follows from the joint assumption of selfishness and rationality. The parameter α is the private marginal benefit from investing in the public good. The marginal cost of contributing is equal to one. If the marginal cost always exceeds the marginal private benefit, you lose on each contribution unit. Therefore, it is a dominant strategy for both players to contribute zero. The strategy profile $c^* = (0,0)$ is the unique Nash equilibrium of the stage game.[5]

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[4] For instance, one of two researchers may give much more effort in a research project than the other. As they share the publication credits, the second researcher may enjoy the gain from the other's effort without contributing much herself.

[5] Since the optimal response for each player is independent of the choices of other players, the game has a unique equilibrium in dominant strategies. The possibility of any mixed strategy equilibrium is ruled out, as no individual will be willing to randomize between a dominant strategy and another strategy.

In order to ensure that the game constitutes a social dilemma, private incentives to defect are necessary, but not sufficient. The collective payoff is the sum of individual payoffs ($W = u_i + u_j$). The "collective" optimization problem is then:

$$Max_{c_i}\{W\} = Max_{c_i}\left\{2e - c_i - c_j + 2\alpha(c_i + c_j) : 0 \leq c_i \leq e\right\}$$

This solves for

$$c_i = \begin{cases} e \ if \ 1 \leq 2\alpha \\ 0 \ if \ 1 > 2\alpha \end{cases} \qquad [2.5]$$

Taking into account solutions 2.4 and 2.5, the game constitutes a social dilemma if and only if

$$0 < \alpha < 1 < 2\alpha \ \Leftrightarrow \ \frac{1}{2} < \alpha < 1 \qquad [2.6]$$

If the private marginal benefit is less than one, no player will invest in the public good. But if $1 > 2\alpha$ (the social marginal cost is higher than the social marginal benefit), zero contribution is also socially optimal. Then, the players lose *collectively* on each unit. Similarly, when $\alpha > 1$, the Nash equilibrium is for each player to contribute their entire endowment, and there is no dilemma.

As long as condition 2.6 holds, the social marginal benefit exceeds the private marginal benefit, and the private marginal cost is greater than the individual benefits. Hence, there is a wedge between individual and collective interest, and the game is a social dilemma.

Repeated game with complete information

We now assume that the game is finitely repeated. The randomly formed pair $\{i, j\}$ drawn from the population $\mathcal{N}$ is either fixed in each round or randomly reshuffled. In addition to the former assumptions, we add the assumption that rationality and selfishness is common knowledge. This rules out strategic play.

Since the stage game equilibrium is unique, the subgame perfect equilibrium is for each player to play the stage game equilibrium on every subgame (Selten 1973). In sequential equilibrium, each player will therefore contribute zero of their endowment in each round. This result follows from backward induction. In the last node T, regardless of the interaction partner, each player knows that his actions cannot affect future play. The decision is strategically equivalent to the stage game decision. Applying the same logic, in round $T - 1$, each player knows that his actions do not affect play in the next period, and so on. This logic could be extended all the way back towards the first decision node.

Baseline prediction (Strong free rider hypothesis)

In the finitely repeated, two-player public goods game (prisoner's dilemma), assuming full rationality and selfishness (and that this is common knowledge), no player will contribute to the public good.

Within this framework, partner choice cannot affect the outcome. Since all players are selfish, and this is known, everyone knows that they will end up with a selfish partner. If partner choice is costly, no one will be willing to engage in it. If partner choice is free, every player is indifferent between all potential partners (because they are all identical). In the latter case, there are multiple matching equilibria but neither will affect cooperation levels.

Communication has no predicted effect either. It constitutes a non-binding threat ("cheap talk") within this framework, because it does not affect the payoff structure and so cannot influence the behavior of selfish players. Thus, one cannot extract promises, appeal to social norms or sanction defection by harsh verbal feedback. Neither strategy enters the payoff function directly.[6]

Repeated game with incomplete information

If we assume incomplete information, there are Bayesian equilibria with positive cooperation in the finitely repeated game (Kreps et al. 1982).[7] For simplicity, we now assume that the decision to cooperate is binary (cooperate or defect). We assume both players to believe that

[6] The baseline prediction also holds regardless of costly sanctioning opportunities or the information structure provided by the experimenters. No selfish player would incur costs to reduce the payoff of other players. The best response $c_i^* = 0$ is a dominant strategy for all players and thus independent of others' actions.

[7] Incomplete information refers to uncertainty concerning the "rules of the game" (e.g. payoff functions), while imperfect information concerns uncertainty about others' actions (Harsanyi 1967). As there is a unique equilibrium in dominant strategies in the prisoners' dilemma, information about actions cannot influence the outcome.

there are two types of players, “selfish” and “reciprocal”. A reciprocal player will play according to a “tit for tat” strategy.[8] He will cooperate initially, and then copy the previous action of his partner.[9]

If players assign a strictly positive probability that the other player they are facing is of the reciprocal type, cooperation might be rational in the repeated game. Denote this probability by δ. Then, $1-\delta$ is the probability that the co-player is selfish. The selfish player might cooperate in sequential equilibrium if he believes that his co-player is reciprocal. The cooperation level will therefore tend to increase with the level of δ (Andreoni & Miller 1993).

To maintain the above conclusion, repeated interactions are necessary. With random matching, the cooperation level will not be tied to the probability that a co-player is reciprocal (two players may meet only once, so reputational incentives are weak). However, partner choice is a possible mechanism for the *emergence* of repeated interactions. Over time, cooperators might enter repeated interactions with one another. This also creates additional incentives to maintain their reputations.

Anticipating cooperation from others, selfish types may attempt to mimic cooperators in order to gain access to profitable partnerships. Therefore, in presence of incomplete information and repeated interactions, partner choice may increase cooperation.[10]

Computer simulations support the speculation that partner choice helps cooperation. Hayashi & Yamagishi (1998) simulated a game where pairs play prisoner’s dilemma games within a larger group. A strategy termed “out for tat” was constructed. This strategy type cooperates unconditionally, but is reciprocal in its partner choice strategy. If the co-player cooperates, the out for tat player will stay with his partner. Else, he will leave his co-player and select a new partner. In this setting, it might be individually rational to signal generosity. This is because

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[8] We do not specify a utility function, but a reciprocal player may for instance be thought of as averse to inequality (Fehr & Schmitt 1999).

[9] Axelrod & Hamilton (1981) showed through computer simulations that this strategy outperforms “selfish” strategies in an infinitely repeated game.

[10] Models assuming complete information (see e.g. Ambrus & Pathak 2011) cannot account for the role of reputation. A “signal” is meaningless in a setting when types are known with certainty. Cooperators will simply sort, but will not increase their cooperation in order to signal their “quality”. Non-cooperators cannot mimic cooperators in such a setting, and so will not increase their cooperation either.

cooperation yields access to profitable partnerships. The “out for tat” strategy performed surprisingly well in these computer tournaments.

Communication may also increase cooperation in presence of incomplete information and heterogeneous player types. Reciprocal players may use communication as a coordination device in order to agree upon a strategy of cooperation. If players are reciprocal, we may think of the game as a coordination game where full cooperation is an efficient equilibrium (Rabin 1993; Rabin 1998). Communication may affect coordination through influencing expectations about intended actions.

We may also predict a positive interaction effect between partner choice and communication. If it is very difficult to coordinate in the partner choice stage, it might be less attractive to signal generosity. This is because the probability of achieving a “match” is lower. Even though you signal generosity, it is difficult to coordinate with your desired partner. Communication facilitates complex coordination among individuals, and may reduce this coordination problem (thus increasing the likelihood that a signal translates itself into future profits). This coordinative function of communication may make it easier for players to find each other and “re-coordinate” in the partner selection stage. Additionally, if partners are identifiable, communication permits information exchange about others. Communication may enhance the “broadcast efficiency” (the reach) of a costly signal (Alden Smith 2010). Thus, communication may enhance the efficiency of partner choice.

Chapter 3: Previous experimental results

The strong free rider hypothesis does not hold up to evidence. In one-shot public goods experiments, contributions on average tend to reach about 40-60% of the total endowment (Ledyard 1995).

Although data rejects the strong free rider hypothesis, we still observe free riding in repeated public goods games. The contribution level is initially high but declines towards the end of the experiment.[11] On average, about 70 percent contribute zero in the last period (Ostrom 2000; Fehr & Smith 1999). While contribution rates start off far from the free rider prediction, behavior converges towards considerable free riding as the game approaches the end.

In this chapter, some evidence from repeated public goods games and prisoner's dilemma experiments is considered. I first consider possible explanations for the standard contribution pattern, and then turn to experiments on partner choice and communication. We relate these studies to our own contribution.

3.1 What explains contribution patterns in finitely repeated games?

Learning

Learning may explain the decay in contributions in the repeated game. As the game progresses and the understanding of the experimental environment increases, subjects adjust their contributions towards the Nash equilibrium. This is known as the "learning hypothesis" (Andreoni 1988). However, Andreoni (1988) showed that when a 10-period public goods game started over, contributions increased. This "restart effect" should not be observed if the contribution pattern is due to learning only.[12]

[11] This result holds whether the number of periods is common knowledge or not (Andreoni 1988).
[12] See also Camerer (2013) for a recent discussion of the "learning hypothesis".

Signaling

Signaling is another possible explanation for the decay in contributions. The “strategy hypothesis” predicts that subjects will signal cooperation initially, and as the incentives to signal decrease, contributions will fall (Kreps et al. 1982). Andreoni (1988) showed that this signaling effect does not explain the standard contribution pattern. Subjects with random matching contributed more on average than subjects who played the game within a fixed group. If the strategy hypothesis was true, we should expect the opposite.

The latter result does not mean that signaling is unimportant in explaining cooperation. It merely means that it is insufficient to account for the observed patterns in the standard game. As we shall see below, signaling becomes very important when we introduce the opportunity for partner choice.

Conditional Cooperation

An alternative hypothesis to learning and signaling is that most subjects are initially cooperative. They have heterogeneous preferences, as well as different initial beliefs about others’ likely actions.[13] “Conditional cooperators” are reciprocal. They cooperate if others cooperate, and defect if others defect. As the game progresses, conditional cooperators adjust their initially positive beliefs downwards. This leads to a self-reinforcing negative spiral of beliefs and contribution levels (Fehr & Fischbacher 2003). Such type and belief heterogeneity may be reasonably inferred from the standard pattern of contributions in public goods games. However, this is an ex post rationalization and not a direct test of the hypothesis.

Fischbacher et al. (2001) *directly* identify conditional cooperation in a laboratory experiment.[14] They employ a variant of the “strategy method” in which subjects make their decisions for every possible average contribution level of other group members in a one-shot public goods experiment. This was implemented by making subjects fill out a contribution table, where they inserted their contribution choice for every possible average contribution level. The results of this study indicate that about 50% of the subject pool exhibit conditional

[13] Orbell & Dawes (1991) proposed a mechanism that we may use to explain the heterogeneity in initial beliefs. The “cognitive misers’ theory” suggests that people use a heuristic by which they project their own intended behavior upon their co-players. Cooperators will tend to project their own cooperative self-image onto their co-players, and will therefore cooperate initially.

[14] The hypothesis of conditional cooperation has also been supported in a field experiment (Frey & Meyer 2004).

behavior. About a third of the subjects chose zero contributions in all entries, and were classified as free riders. The results from this study have been widely replicated (Chaudhuri 2011).

The theory of conditional cooperation has motivated researchers to design different mechanisms able to increase cooperation (Chaudhuri 2011). If individuals manage to coordinate upon optimistic beliefs, cooperation will also stabilize. This suggests a role for communication as a coordination device. One may also induce cooperation by providing incentives to cooperate (for instance by providing cooperators with a punishment opportunity). Another way of increasing cooperation is to limit the interaction between cooperators and free riders. This suggests a role for partner choice.

3.2 Choosing a cellmate: Partner choice and cooperation

In real life prisoner's dilemmas, we often get to choose our "cellmate" (Tullock 1985). When partner choice is possible, conditional cooperators may self-select into groups or pairs of like-minded individuals. They may therefore escape free riders (Tiebout 1956).[15] This property of partner choice has led some researchers to speculate that partner choice may partly account for the evolution of cooperation (McNamara et al. 2008; Izquierdo et al. 2010; Baumard et al. 2013). These bold claims about the role of partner choice for cooperation should be met with thorough experimental evidence documenting its robustness in different environments.

In this thesis, we experimentally examine the effect of mutual partner choice on cooperation. To my knowledge, no previous studies have studied mutual partner choice in a setting where choices *directly* determine the matching process. Whereas former studies (Coricelli et al. 2004; Bayer 2011) filter choice through a complex matching algorithm, we cut this "middle man" and tie choices directly to partnership formation. We also implement an information structure where players only learn through private experience (similar to Huck et al. (2012)). This has not been implemented so far in a mutual choice setting. The restrictions we impose allow us to show that the coordination problem is not the reason for the previously

[15] Gunnthorsdottir et al. (2007) isolate the effect of "behavioral sorting" in an experiment where reputation building is not possible. Here, the experimenters control group formation and keep the participants in the dark about the assortment rule. When groups are formed based on the players' actions, groups of cooperative individuals tend to have greater and more stable contribution rates than other groups.

documented poor performance of mutual choice.

When we choose partners, reputation matters. The competitive altruism hypothesis suggests that opportunities for partner choice increase our willingness to cooperate, because costly signaling of generosity may yield increased access to profitable partnerships (Roberts 1998; Hardy & Van Vugt 2006).[16] This form of altruism is competitive in that people try to appear more generous than others, beyond just signaling a cooperative disposition (Barclay & Willer 2007).

We test the competitive altruism hypothesis by comparing initial contributions between the subjects with and without partner choice. Therefore, the baseline condition incorporates a possibility to signal cooperative intentions (players are identifiable and know that they are identifiable). In contrast to former studies, we use a "between-subjects" design which is less sensitive to experimenter demand effects (Charness et al. 2013). Additionally, in our design subjects do not know when they will be able to reap the benefits of a costly signal. Competitive altruism should be robust to such uncertainty.

3.2.1 Choosing a cellmate: Experimental evidence

Competitive altruism

Barclay & Willer (2007) provide direct evidence of competitive altruism. In their laboratory experiment, all participants first played a one-shot two-person continuous prisoner's dilemma. Next, they played the same one-shot game with a third party. Depending on the experimental condition, this third party was either a) randomly paired with one of these players and received no information about past play, b) randomly paired and received all information about past behavior or c) received this knowledge, and had the opportunity to select his or her partner for the one-shot game. This was common knowledge, such that the two participants in the first game faced potential reputational benefits. Thus, this experimental design measures both the effect of altruistic signaling (signaling cooperative intent), and the effect of competitive altruism, which measures the willingness to be perceived as more generous than others. Barclay & Willer observed that cooperation increased when behavior was observed and even further when partners were chosen.

[16] Here, altruism is defined behaviorally as incurring a short-term cost that is individually disadvantageous.

In the Barclay & Willer (2007) study, subjects knew with certainty when they would be able to reap the benefits of a costly signal (immediately). Additionally, choice was one-sided, so there was no coordination problem. These features of their design may make the potential benefits from signaling very transparent. In our experiment, the players do not know when they will be selected by a potential partner. They only know that their contributions are visible to previous partners, and so may choose to contribute if they wish to increase the probability of being chosen by this partner sometime in the future.

The finding of competitive altruism was replicated by Sylwester & Roberts (2010), who additionally showed that cooperators benefit through generous displays. The study shows that cooperation increases when partner choice is possible, and that cooperators earn greater profits through such behavior. They are also more likely to be selected as partners.

In the study by Sylwester & Roberts (2010), choice was mutual, but the authors announced all information about past play. This means that a given signal is broadcasted to all others in the group. When a given signal has a high reach, the incentives to engage in costly signaling are also high. In our study, we restrict the reach of a costly signal to a single individual. Thus, we test whether competitive altruism is still present under a low rate of feedback.

Both Barclay & Willer (2007) and Sylwester & Roberts (2010) used a within-subjects design. This means that all subjects participated in all conditions. This type of design may be especially sensitive to experimenter demand effects, because the subjects are not blind to the experimental condition (Charness et al. 2012). Substantial research in psychology indicates that the design type may interact with independent variables of interest (Erlebacher 1977). Results from within-designs may partially reflect experimenter demand effects because subjects gain information about several experimental conditions and therefore may infer the experimenter's motives (Zizzo 2010; Charness et al. 2012). For these reasons, competitive altruism needs to be replicated in a between-subject design in order to demonstrate its robustness as a behavioral pattern. Our experiment uses a between-subjects design, and so is well suited to address this question.

Partner choice experiments

There are essentially two main ways to study partner choice. The choice mechanism may be either one-sided or mutual. Either a single participant dictates partnership formation or which group to enter, or some degree of mutual agreement is required (Coricelli et al. 2004). Studies on one-sided partner choice are generally in unanimous agreement: Partner choice positively influences cooperation (Hauk & Nagel 2001; Coricelli et al. 2004; Huck et al. 2012). However, there are few studies on mutual partner choice. In our opinion, researchers have yet to unambiguously demonstrate that mutual partner choice influences cooperation.[17]

Erhart & Keser (1999) showed that when subjects were allowed to freely "move" in and out of groups of changing size (there were no restrictions on group size), cooperators are constantly "on the run" from non-cooperators. This provides suggestive evidence that cooperators want to avoid non-cooperators. Since in this study there were no restriction rules on who could enter, free riders tended to "chase" cooperators around. The groups with higher average contribution levels tended to grow in size, while those with lower contributions tended to shrink in size. However, because group size was endogenous, disentangling the effect of partner choice from the effect of group size on cooperation is difficult within this type of design.

Page et al. (2005) show that voluntary group formation through preference rankings increases contributions in a public goods game. This study holds group size fixed, so that causal inference is easier. Their study employs a baseline of fixed matching. This control group potentially overstates the effects of the regrouping treatment (a random matching protocol would allow for "restart" effects). The regrouping treatment was implemented by letting subjects express their preferences by ranking all other subjects (15 in total) on a scale from 1 to 15. A computer algorithm matched the subjects together in groups of four based on these rankings. The authors also compared regrouping with punishment, and found that the regrouping treatment was significantly more efficient than the punishment treatment. The combined treatment reached the highest level of contributions out of all experimental conditions. High contributors were sorted together, and low contributors were left with

[17] Mutual choice is used in some studies on endogenous networks (Riedl & Ule 2002; Wang et al. 2012). However, such studies introduce additional (and endogenous) sources of variation by letting the group or "partner pool" vary in size. This makes causal inference more difficult.

other low contributors.

Brekke et al. (2011) show that costly signaling through a donation to charity may function as a sorting device for cooperators. Individuals may choose two groups, red or blue. Those in the red group donate an amount to the Norwegian Red Cross in order to enter, whereas no donations are made in the blue group. The authors observed much higher and more stable contribution rates in the red groups. In the blue groups, they observed the conventional decaying pattern. Brekke et al. do not compare the general effect of such sorting on cooperation with a baseline with either fixed or random matching, but show that the red groups reach higher cooperation levels.

The above experiments differ from ours in that the subjects choose a group or many subjects at a time. In our experiment, we study a game where only two players interact at a time However; previous experiments clearly suggest that when people may influence their social partners, cooperation may increase due to behavioral sorting.

Partner choice experiments: Mutual partner choice

Partner choice often requires agreement between two parties. However, the number of studies on mutual partner choice is limited. Additionally, those studies either do not allow participants to "run" the matching process (Hauk & Nagel 2001), or choose to "filter" choices through a complex computer algorithm (Coricelli et al. 2004; Bayer 2011). It is somewhat ambiguous whether this actually captures the direct effect of partner *choice.*

Hauk & Nagel (2001) study one-sided and mutual partner choice in a standard binary Prisoners' Dilemma experiment. A baseline Prisoner's Dilemma with fixed partners is compared with conditions in which all participants are allowed to decide whether to enter or exit the game with a particular player. The two treatment conditions feature either one-sided or mutual choice. In the former, it is sufficient that one of the two players choose to enter the game. In the latter, mutual choice is required. The results show that one-sided choice is more efficient. Mutual choice lowers overall cooperation, but cooperation is higher in the "matched" pairs than the fixed matching pairs. However, this study did not allow players to influence partnership formation. They could simply choose to enter or refuse a game with a given partner.

In our study, the strategy space is that of the public goods game. Players may choose how much to contribute, rather than just to cooperate or defect. Additionally, we use a baseline of random matching in order to control for possible restart effects. But more importantly, it is the players themselves who drive the matching process.

In a repeated, two-player public goods game, Coricelli et al. (2004) show that one-sided partner choice increases cooperation relative to a baseline of random matching. Thus, their control incorporates restart effects. Their experimental treatments featured six sequences of five-round two-player Public Goods games, and a group consisted of six participants. Mutual partner choice does not increase cooperation. Additionally, the results from the Coricelli et al. study showed that one-sided choice was significantly more efficient than mutual choice. The inefficiency of mutual choice was attributed to the inherent coordination problem introduced. Coricelli et al. claim that signaling of a cooperative disposition is nearly unattainable in their design.

Bayer (2011) shows that a breakup opportunity combined with a mutual choice mechanism increases cooperation in a two-player public goods game relative to a baseline of random matching. However, this result only holds when either one of the following conditions are met. Either, partner choice must give extra benefits, or entire contribution histories must be published. In our experiment, neither of these conditions are met.

Both Coricelli et al. (2004) and Bayer (2011) study complex and highly artificial matching mechanisms, and it is somewhat ambiguous whether these studies actually measure the direct effect of choice. Coricelli et al. (2004) use a “bidding” process where players must assign endowments to different partners, expressing their willingness to be paired with this player. Then, a computer algorithm maximizes the sum of mutual assignments. Bayer (2011) allows subjects to express their preferences for different partners in a ranking procedure. Then, a stable marriage mechanism assigns everyone to a partner based on these rankings. The latter algorithm is so complex that the authors chose not to fully inform the subjects about how it worked, in order to avoid confusion. The problem with these approaches is that we cannot disentangle the effect of partner choice from the effect of the particular computer algorithm that “interprets” the individuals’ preferences.

To our knowledge, we provide the first experimental study of mutual partner choice in a situation where choices directly determine partnership formation. In the former studies, we have seen that choice only indirectly influences matching, through a complex algorithm. In this study, we remove this “middle man” by tying the matching algorithm directly to individual choices.

In real life, information about partners is often attained through experience, an issue generally not addressed in former studies on partner choice. Both Coricelli et al. (2004) and Bayer (2011) publish the entire contribution history of all players. Huck et al. (2012), however, show that one-sided partner choice increases trust and efficiency in a repeated trust game. This study is unique by showing that partner choice performs extremely well even when information is private. The players only receive feedback about their own past interactions. Co-players are identifiable. Given such private information, partner choice boosts cooperative play. Trust increases from 50 to 86% when partner choice is allowed. The “honor rate” (the rate by which trust is honored) increases from 72% to 92%. This is strong evidence for the disciplining effect of partner choice.

We implement the “private history” information structure implemented by Huck et al. (2012) in a mutual choice setting. We conjectured that even in this highly restrictive (although realistic) environment, mutual choice would increase cooperation. This is because a) there are reputational incentives to engage in costly signaling and b) the matching process is repeated each period, giving the subjects time to learn. The latter opportunity was not given in the study by Coricelli et al (2004) and Bayer (2011), even though their matching mechanisms were substantially more complex.

The information structure and mutual choice setup creates a substantial coordination problem in our experimental game. We are therefor able to show that the coordination problem is not the reason for the previously documented poor performance of mutual choice. However, improved coordination in the partner selection stage might still increase cooperation. One way to further improve coordination is through communication.

3.3 Communication: Not only "cheap talk"

In experimental settings, face-to-face communication was early shown to induce greater cooperation levels in prisoner's dilemma experiments (Sally 1995; Ostrom 2000). Also recently, chat room communication, which abstracts from visual and auditory cues, has been shown to increase cooperation (Bochet et al 2006). From a conventional game theoretic view, this is a surprise, because communication is considered "cheap talk" within this framework. Yet, few factors are as robust as communication in inducing cooperation among strangers (Sally 1995; Balliet 2010).

3.3.1 Cheap talk matters: Experimental evidence

Communication increases cooperation in social dilemmas. In a thorough meta-analysis of Prisoner's Dilemma experiments from 1958 to 1992, Sally (1995) concluded that communication increases cooperation by about 40 percentage points. The effect of communication appears to be more robust than other known factors, such as group size and the size of the incentive parameter (marginal private benefit). This robust effect of communication was confirmed in a more recent meta-analysis (Balliet 2010). This analysis additionally revealed that the effect of verbal communication appears to be larger than that for written communication.

In a seminal study, Isaac & Walker (1988) showed the efficiency of face-to-face communication in different environments in a public goods experiment.[18] The authors first ran ten experiments with different communication treatments, providing them with between-, within-, and sequencing comparisons of the effects of communication. In the four C/NC experiments (communication allowed in the first ten periods, and prohibited in the last ten), cooperation levels reached almost full efficiency. In the three NC/C (communication allowed in the second sequence of ten periods), communication still had a significantly positive effect, but less than that found in the C/NC experiments. Figure 1 displays the results from this study.

[18]Ostrom et al. (1992) show that communication also increases cooperation in a common pool-resource experiment. This game differs from the public goods game in that the common-pool resource is rival, but non-excludable (not a pure public good). Both one-shot and repeated communication increased cooperation.

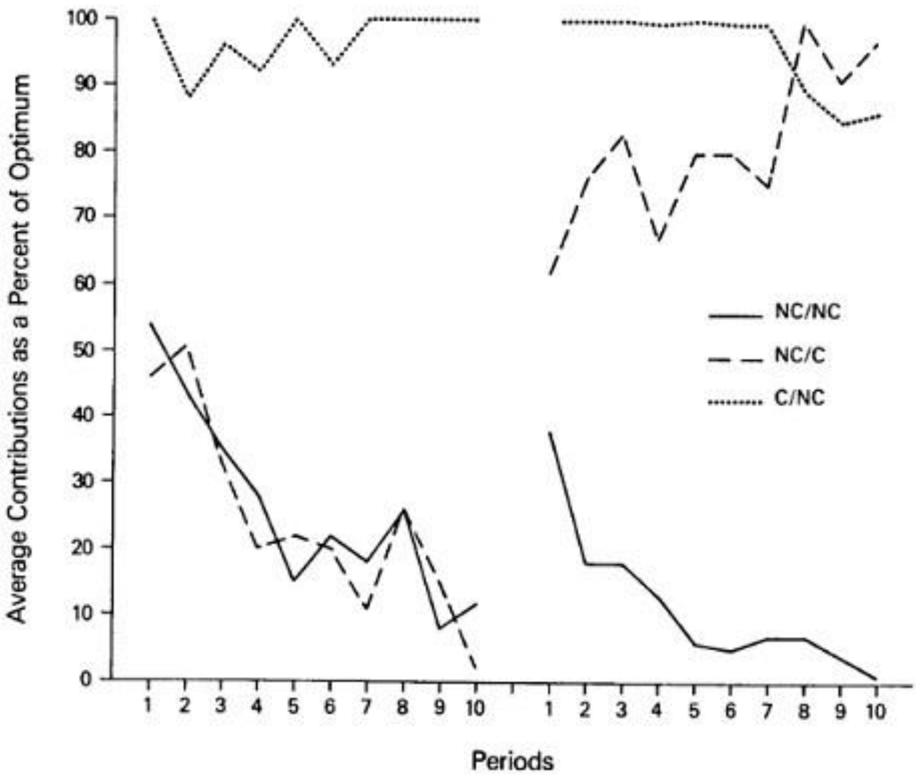

Figure 1 - The effect of face-to-face communication in Isaac & Walker (1988): 591

Isaac et al. also showed that communication influences cooperation in complex decision environments. Even when endowments where asymmetric (varied across subjects) or information about individual endowments was incomplete, the effect of communication is positive (although somewhat reduced). The conclusion was not affected by the production function; Communication had a positive effect when the incentive parameter was declining in the level of contributions. Even in this environment, the effect of communication was significantly positive in nine of ten periods.

The form of communication also matters for cooperation. Frolich & Oppenheimer (1998) compare the effects of e-mail communication with face-to-face communication in a five-person Prisoner's Dilemma experiment. This study also suggests that the effect of face-to-face communication is significantly greater than e-mail communication.

In a more extensive study, Brosig & Ockenfels (2003) show that face-to-face communication is more effective than other forms of communication. Fostering "group identity" by visual identification does not appear to be important, as this study includes a treatment where co-players are visually identifiable, but communication is prohibited. In all communication treatments, communication appeared to perform the same coordinative function. Thus, the relative efficiency of face-to-face communication appears to lie in the nature of the communication medium itself. For instance, communication in a "table conference" treatment had a significantly greater effect than in an "audio conference" treatment, where participants communicated verbally but did not see each other.

Bochet et al. (2006) show that chat room communication is also very efficient in creating cooperation in public goods games. This study also considers face-to-face communication and signaling through numerical messages.[19] They also compare punishment and communication. Face-to-face communication had a strong effect, and adding a punishment opportunity on top of this only slightly increased contributions and efficiency. The chat room treatment reached high contributions levels, but 15 percentage points less than the face-to-face treatment. When a punishment option was added, contributions increased. An examination of the content of the chat revealed that most messages sent were commitments to a common strategy, and about a fourth a discussion on the optimal strategy.

The study by Bochet et al (2006) is similar to ours in the way communication is implemented (chat room communication). Therefore, this study is a natural benchmark against which we may compare our study. The main difference lies in the fact that the dilemma is a number of isolated prisoner's dilemma games rather than an N-person game. There are multiple dilemmas at a time, rather than just one.

3.3.2 Why cheap talk matters

In our thesis, we employ chat room communication. This communication medium abstracts from visual cues shown to increase cooperation. However, as we have seen, communication also affects cooperation even though facial and auditory cues are removed (Brosig & Ockenfels 2001; Bochet et al. 2006).

Communication may foster commitment to generalized behavioral norms (Orbell et al. 1988). Another possibility is that communication lets individuals exchange promises which

[19] In the chat room treatment, participants were allowed to communicate in a common chat room before the first, fourth and seventh of in total ten periods. In the numerical cheap talk treatment, subjects could type numerical contribution levels before each actual production period.

affect the beliefs about others' actions (Kerr & Gilliland 1994). If such promises are believable, subjects might avoid the decay of cooperation. Yet another possible explanation is that communication fosters group identity (Dawes et al. 1988). For instance, discussion might lead the subjects to distinguish less sharply between individual and collective interest (Kollock 1998).

Orbell, Dawes & Kragt (1988) investigate the hypotheses of generalized norms and promises as explanations for the effect of communication. If the former hypothesis is correct, then after group discussion in the subject's own group, cooperation should also increase towards subjects beyond the discussion group. This hypothesis is thus tested by examining whether people cooperate towards "out-group" subjects after discussing the dilemma "in-group". Discussion did not significantly affect such "out-group" cooperation, which was interpreted as a rejection of the hypothesis of generalized norms.

In a review of several studies, Dawes et al. (1988) suggests two possible explanations for the effect of communication in absence of strategic incentives. One possibility is that universal promising increases cooperation. Another possibility is that universal promising affects cooperation by establishing solidarity (group identity) among group members. This increases cooperation, not promises per se. These two possibilities imply that sharply distinguishing among mechanisms is inherently difficult.

Our communication condition is closely related to the one implemented in Bochet et al. (2006). Subjects communicate through a chat room. This abstracts from perceptual cues such as facial cues or auditory cues. We believed that the effect of communication would be positive, although somewhat lower than in an N-person game. This is because interaction is in random dyads, and not on the group level. This might lower the prospect for coordination through communication. The private information structure might also make it more difficult to ensure that commitments to a common strategy are kept. In an N-person dilemma, a defecting player affects the payoffs of all others in his group. In a set of two-player games this information is scattered around in the group. Additionally, a strategy such as "everyone please contribute to the group" might be a simpler heuristic to coordinate upon than "everyone please be nice to each other". However, things look differently when we combine partner choice and communication.

3.4 Coordinating through cheap talk: Communication and partner choice

Communication is often involved in partner choice. When we enter a partnership, we voluntarily agree. We may also discuss relevant problems during the interaction, and this might help us achieve our goals. This suggests that these variables should be studied together.

However, there are also theoretical reasons why combining communication and partner choice might increase cooperation. Communication facilitates complex coordination. This unique feature of human communication makes it easier for large groups to solve difficult problems (Alden Smith 2010). This may make it easier to find a partner and commit to a partnership. Communication allows individuals to enter informal agreements beforehand, and also possibly agree upon a common strategy before they choose each other. Partner choice therefore allows communication to work through an extra channel. Additionally, when information sharing is possible (if players are identifiable), communication may enhance the reach (the “broadcast efficiency”) of a costly signal (Alden Smith 2010).

Tullock (1999) conducted a demonstrative experiment in which participants played repeated prisoner’s dilemma games in pairs. Participants were allowed to communicate freely without any restrictions. The design allowed subjects to choose their partner, and they could change their partner at any time. However, this study did not include control and treatment conditions, because the purpose of the study was merely to illuminate the fact that as the game more closely approximates real world settings, the prisoner’s dilemma is less of a dilemma. Therefore, we cannot identify treatment effects in the Tullock (1999) study.

Our study expands the literature by providing, to our knowledge, the first controlled study of partner choice and communication. This allows us to compare the effects of these variables. But more importantly, it allows us to ask whether communication improves the efficiency of partner choice.

Chapter 4: Experimental design and procedures

In our experiment, we test the hypothesis that partner choice has no effect on cooperation against the one-sided alternative that it increases cooperation. We also test the two hypotheses that communication improves cooperation, and increases the efficiency of partner choice. The experiment was designed so that the partner choice mechanism was simple and transparent. As we were interested in testing for competitive altruism, care was taken to ensure that first-period behavior would function as a measure of this mechanism.

Additionally, there was a coordination problem involved in partner selection. However, in one condition subjects could also communicate with all others in their group. This was a direct consequence of the research question. For communication to assist coordination in the partner selection stage, they should have the opportunity to communicate with all their potential partners.

4.1. Experimental design

The experimental design features two exogenous explanatory variables with two associated "levels". This gives us a total of four treatment combinations. This type of design is commonly defined as a 2 x 2 factorial design, which permits researchers to study the isolated effects of two treatment variables, as well as possible interaction effects (Friedman & Sunder 1994: 26-29). The experiment was computerized using the software z-Tree (Fischbacher 2007). The program that was used is available upon request.[20]

There were two treatment variables, Chat and Choice. The experimental conditions are described more fully below. Table 1 illustrates the main features of the experimental design.

Table 1 - Experimental design, number of subjects and sessions

Design	*No Chat*	*Chat*
Random	32 subjects, 2 sessions, 4 groups	32 subjects, 2 sessions, 4 groups
Choice	32 subjects, 2 sessions, 4 groups	32 subjects, 2 sessions, 4 groups

[20] Large parts of the program I used was developed by Fest (2011)

I. *Baseline (No Chat, Random)*

In each session of the baseline treatment, subjects were randomly assigned to two groups of eight which were fixed until the end of the experiment. The subjects were informed about their identity (a number between one and eight) and that this identity number would remain fixed for the entire duration of the experiment. They then played two-person public goods games (prisoner's dilemmas) in pairs. These pairs were randomly generated in each period. The stage game was repeated for 30 periods, and this was made common knowledge. Before each production period, the subjects were shown a screen informing them of the identity of their partner for that period. Figure 2 displays the user interface in the partner display stage.[21]

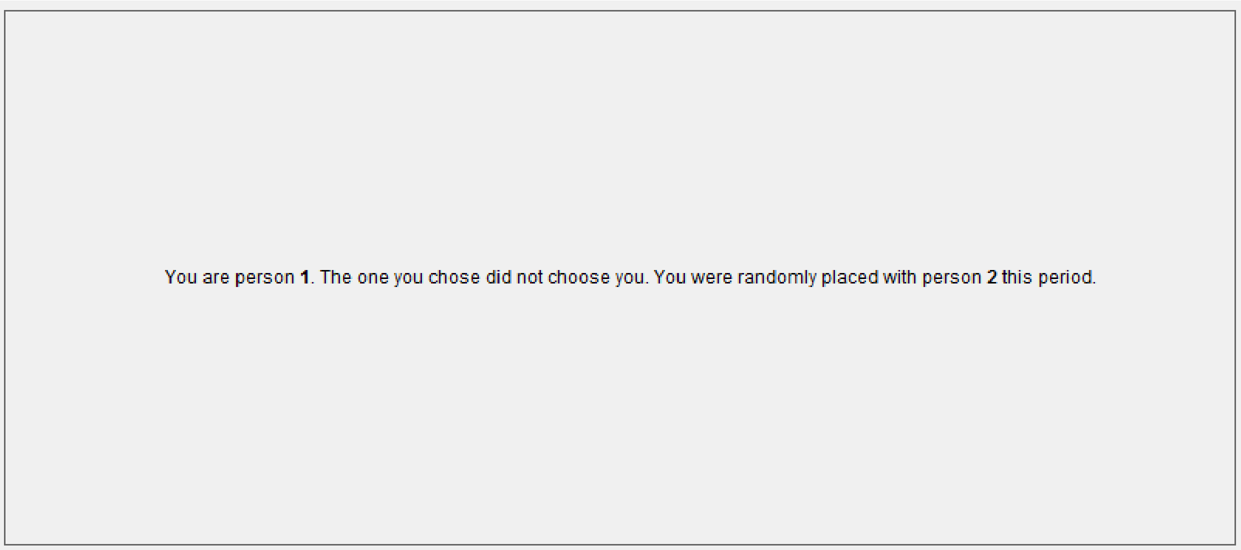

Figure 2 - The partner display stage

When the production stage ends, all participants see on a private screen their personal payoff and the numerical identity of their partner that period. Because of the two-player structure in this dilemma, all players may infer their partner's action. This private screen also contains all previous personal information, so that players remember their history. This creates a possibility to signal cooperative intentions.

[21] Here, the display stage is shown for the "Choice" condition. In the "Baseline" and "Chat" condition, subjects were simply informed about who they were randomly paired with that period. In both "Choice" conditions, players also got to know whether they had been chosen by their preferred partner.

The payoff function was identical for all subjects in all experimental conditions. The payoff function in each period is as follows:

$$\pi_i = 20 - c_i + 0.7(c_i + c_j) \qquad [4.1]$$

In each period, all subjects received an endowment of 20 blue units (experimental currency units), which could be used for production of red items. Producing red items was in this setting equivalent to producing a pure public good (conferring positive externalities to a partner). The private marginal benefit of contributing was set to 0.7, creating a social dilemma. Figure 3 displays the user interface in the production stage.

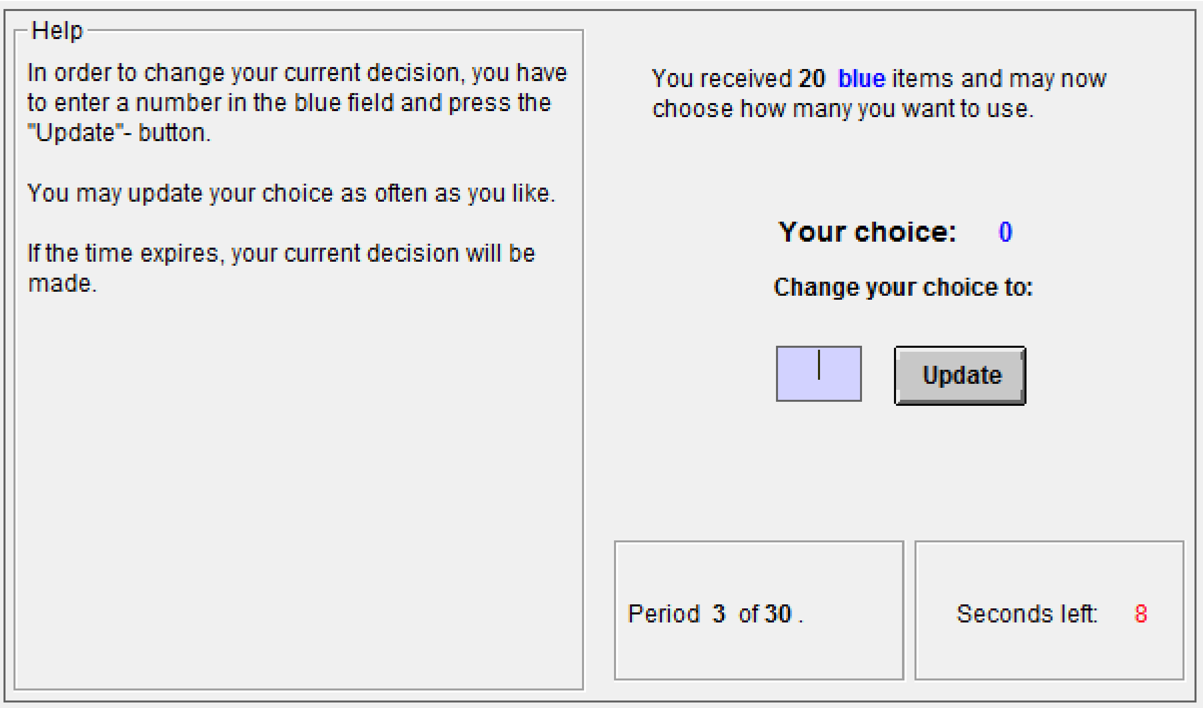

Figure 3 - The production stage

The default contribution choice in the production stage was set to zero. This means that the subjects had to update this default in order to contribute a positive amount. The production stage lasted for ten seconds in each period.

II. Choice condition

The “Choice” treatment was identical to the baseline, with one exception. Subjects were permitted to choose their preferred partner. In the first stage of each period, all subjects were enabled to make a choice of partner (a number between one and eight). No subjects could observe each other’s choices, and the choice was only revealed to another player if the other player had chosen the subject back. This introduces a substantial coordination problem within the dilemma. Given your desired partner, the probability that he will pick you at random is 1/8. In order for an established partnership to endure, one therefore needs a way of communicating the intent of reinforcing the relationship. In the “Choice” condition, the only way of doing this is through individual contributions.

The default partner choice was set to the value of the subject’s own identity tag. When the available time (ten seconds) had expired, the number entered was registered as their choice of partner. Subjects who chose each other were paired together in the next period. Subjects who chose themselves or failed to achieve a mutual match were randomly paired together with a remaining subject. This treatment is the reason why we chose a two-player game. It makes the matching algorithm simpler, and easier to tie directly to individual choices.

The matching algorithm was implemented as follows. If two players mutually chose each other in a given period, they were assigned a common, numerical value (the minimal value of the Subject variable for the two subjects who chose each other). Since no subject had the same identity, and identities were fixed over time, this algorithm ensured that all partnerships consisted of exactly two subjects. If the subject’s choice did not correspond to the one who chose her, she was assigned a random value. If a subject chose herself, she was also assigned a random value.[22] Figure 4 displays the user interface in the partner choice stage.

[22] The source code for the matching algorithm is presented in appendix E.

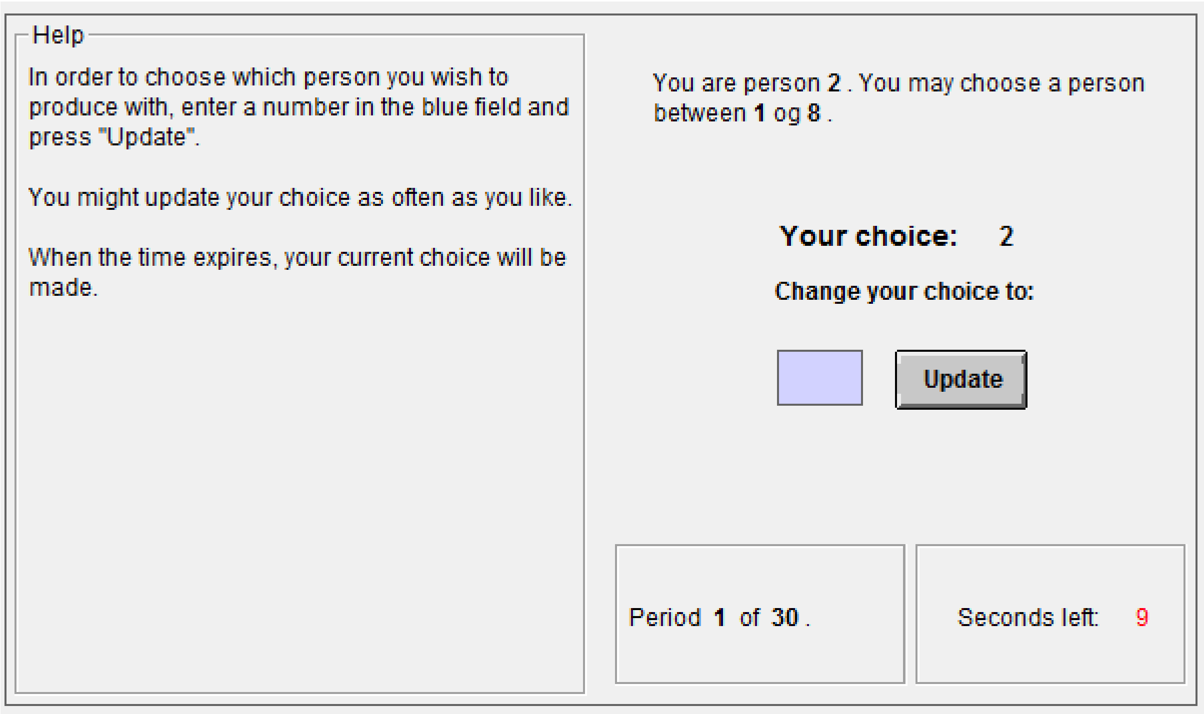

Figure 4 - The partner choice stage

All subjects had an initial ten second opportunity to choose their production partner for that period before they entered the production stage. This choice was made by entering a number between 1 and 8 in the blue field on the screen (thus a subject could either choose himself or another subject in his group). The default choice was set to the value of the subject's own identity tag. This means that if a player did not make a choice, he "chose" himself that period. He would then be randomly matched with one of the remaining subjects. The partner choice stage lasted ten seconds.

In the first period, matching is practically random in both the "Baseline" and "Choice" condition, regardless of whether the latter succeed in finding a match or not. As the baseline treatment incorporates a possibility to signal cooperative intentions, (subjects are identifiable and may infer their partner's action) the additional source of variation in the "Choice" condition is the opportunity to choose a partner. Therefore, first-period differences necessarily reflect competitive altruism.

III. Chat condition

The “Chat” condition was identical to the baseline treatment, with one exception. In the last stage of each period, all subjects were given the possibility to communicate in a common chat room of eight participants. The chat room consisted of all subjects in a fixed group of eight subjects. All subjects could see the messages typed by all others in the same group.

The chat room had only two rules: The subjects were not allowed to reveal their personal identity, and improper language was not permitted. The subjects were told that breaking these rules would result in them being expelled from the experiment, and that they would lose the opportunity to receive their payment. The intention behind these rules was to signal towards the subjects that we expected civil behavior.[23] The user interface in the communication stage is displayed in figure 5. This figure also shows the information screen displayed to subjects in all conditions.

[23] This might be argued from an ethical perspective, or from a conventional “common pool” stance. If someone perceives the experiment as an unpleasant experience, they might choose not to assign next time they are invited. As researchers, we should be concerned about how our choice of experimental design influences the future recruitment base.

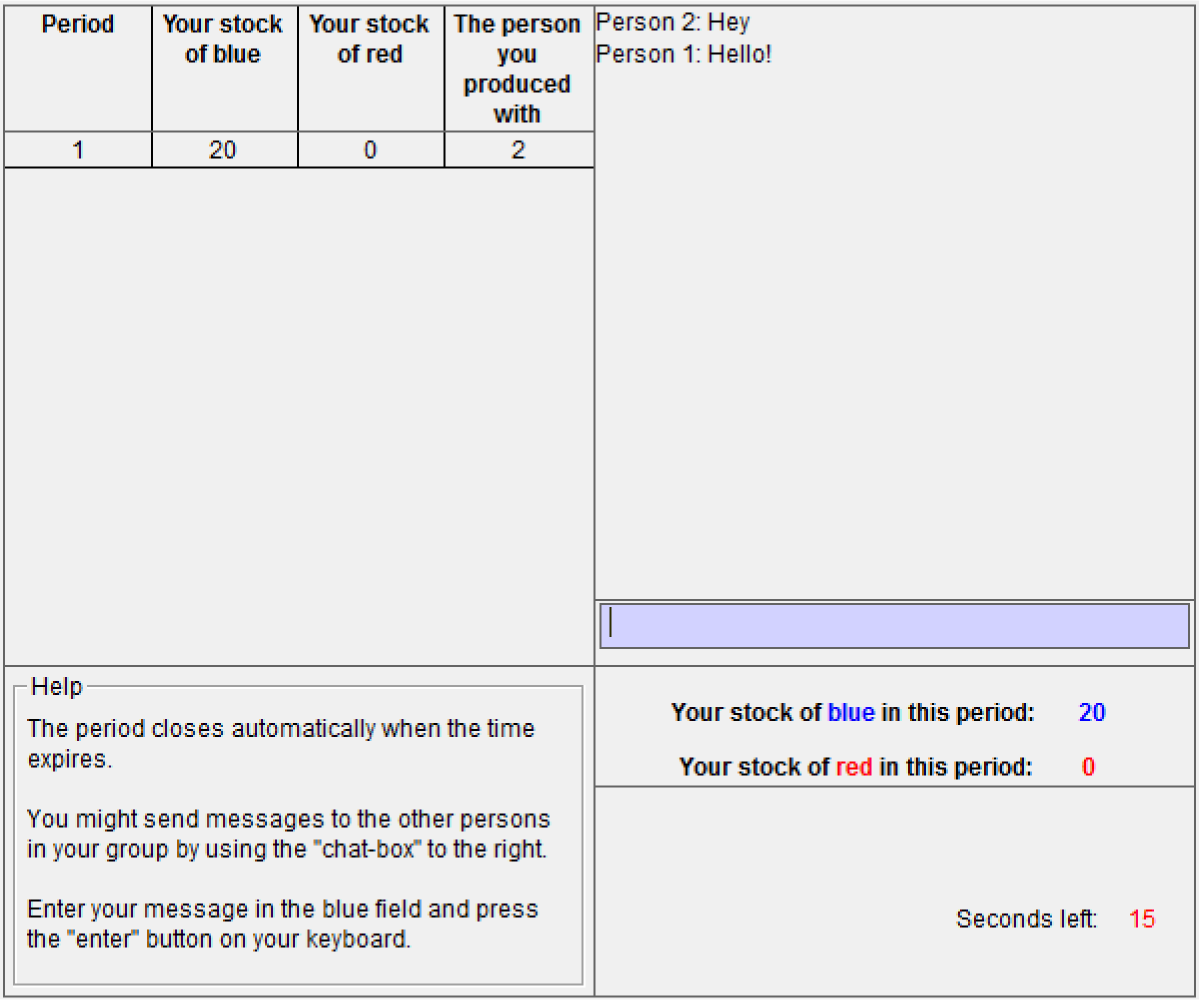

Figure 5 - The communication stage

All messages were entered in the blue field in the upper right corner on the screen. To display the message, the subject needed to press the "Enter"-button on the keyboard. All subjects could see the messages typed by either subject in their assigned group. This communication screen was almost identical to the profit display stage in the "Baseline" condition, except that in this latter treatment the "chat field" of the screen was blank. The communication stage lasted for 25 seconds in each period.

IV. Choice + Chat condition

The last treatment was identical to the baseline, except that subjects were both allowed to choose their production partner, and to communicate through a chat room in the last stage of each period. Thus, the sequence of events in each period was as follows:

1. *Partner Choice*
2. *Partner Display*
3. *Production Stage*
4. *Chat*

When the chat stage had expired, subjects immediately started a new period with a new choice of partner.

4.2. Experimental procedures

All participants were recruited by e-mail using the recruitment software Expmotor, specifically developed for this purpose.[24] An invitation e-mail was sent simultaneously to a list of 2500 individuals.[25] The recruitment base was all students registered for the exams in examen philosophicum, a mandatory course for all first year students enrolled at Norwegian universities. This was done in order to minimize the probability of possible social ties among the experimental subjects. Additionally, such a broad recruitment base might make the results easier to generalize towards the general student population.

In the recruitment e-mail, potential attendees were informed that everyone would receive a fixed payment of 100 NOK (approximately 16.7 USD), and that they could earn more money during the experiment. Thus, the total payment consisted of a fixed and a variable (incentivized) component.[26]

The experiment consisted in a total of eight sessions over two days. We ran the first four sessions on May 6. and the four last sessions on May 7.[27] Because of the differing time length of the experimental conditions, we had to run one treatment at a time in the laboratory. However, all subjects were randomly assigned to one of the four conditions. This was ensured by drawing the order of the treatments in advance. All four treatments were run each day, and each day all treatments were randomly assigned to different times. Thus, the subjects were "blind" to the condition they were in, and were randomly assigned to a condition.

A potential challenge for experimental studies is experimenter demand effects, subtle cues provided by the experimenter that might induce subjects to behave differently in different conditions.[28] This effect might itself represent an uncontrolled source of variation which is confounded with the treatment effects (Zizzo 2010). With these concerns in mind, a double blind procedure was considered, but not applied, due to the practical difficulties involved.

[24] This service is due to professor Erik Sørensen, Norwegian School of Economics.
[25] A translated version of this e-mail is provided in Appendix B
[26] One justification for such a payment scheme is that in order to avoid selection of "risk takers" into the experiment, you need to pay subjects a fixed sum. Additionally, in order to avoid selection of particularly "nice" subjects, you need a total expected payment exceeding the opportunity costs for the average undergraduate student. Finally, the variable part is directly tied to the subjects' actions, such that behavior is properly incentivized. The social dilemma is defined by the presence of such incentives (Dawes 1980).
[27] The four session were scheduled at 09.45, 11.15, 12.45 and 14.15.
[28] Orne (1962) explains and shows the relevance of experimenter demand effects from the perspective of the psychology literature.

Instead, the subjects were visually separated from the experimenter for the entire duration of the experiment, minimizing the possibility for experimenter demand effects.[29]

Upon arrival in the laboratory, all subjects were welcomed by an assistant, who asked them to draw a scrap of paper from a box. This box contained scraps of paper indexed from A-P. Each letter corresponded to a client PC. This procedure ensured that the subjects were seated randomly across the room. All client PCs were visually separated by dividing walls, and the PCs were set up in such a way as to avoid that subjects from the same group could see each other's screen (This was done by setting up the subjects from the two groups in the order 1-2-1-2-1-2 etc.). When participants had located their PCs and were seated ready, the experimenter read aloud prewritten instructions.[30] This was important in order to ensure that we did not create additional variation between the experimental conditions. Subjects were allowed to ask questions, but nobody asked questions at this point. When the experimenter had finished reading the prewritten instructions, the PCs were started. Subjects were allowed to quietly raise their hand for questions during the experiment. In total, this happened in three of the eight sessions.

The experiment on average lasted for 45 minutes, counting the time it took from subjects to get seated until they left the room with their payment. In total, three sessions were started manually because one subject did not click the "ready" button after all other subjects had waited several minutes. I followed the same procedure in all these three cases: I waited until 15 minutes had passed, walked to the back of the room and quietly informed the subject that I would start the experiment. I then quietly returned to the experimenter's PC.

When subjects had finished making all decisions, they answered some questions and filled out a questionnaire asking for their assigned letter. The program then wrote a payment file. I then quietly texted the assistant, who was waiting in a separate room, that the payment file was written. The assistant then started placing the payments received in separate envelopes labeled from A-P. When he had finished this procedure, he knocked on the door, gave me the envelopes and then returned to the other room. I then instructed the subjects to leave one by one, and exchange their scrap of paper with the corresponding envelope. This procedure

[29] Experimenter demand effects might still be present in the experimental instructions or the user interface, but as this is public information, the reader might judge for herself.
[30] Copies of these instructions are provided in Appendix C

ensured that the payment was anonymous. I did not know the link between the letter a subject had received and the numerical tag they had received in the experiment. The assistant had not observed the behavior of any subject, so the letters had no meaningful interpretation. Additionally, in this way subjects could in no way discover the numerical identity assigned to the other participants (in absence of voluntary consent).

On average, subjects earned 204 NOK (34.2 USD). This constitutes an average hourly payment of 272 NOK (45.6 USD), well above the average hourly wage rate (and thus the assumed opportunity cost) for undergraduate students in Norway.[31]

[31] For instance, the hourly wage of a teaching assistant at the University of Bergen is 169.8 KR (27.68 USD)

4.3 Instructions

The experimental instructions were written in line with the conventional approach in the literature.[32] All subjects received full information concerning the underlying payoff mechanism. They were initially informed that they had been placed in a group of eight subjects, and that this group would remain fixed for the entire experiment. They were also informed about their assigned numerical identity tag.

There was some additional information provided in the "Chat" and "Choice" conditions compared to the baseline. In the "Choice" condition, players were informed that they would have the opportunity to choose their partner in the first stage of each period. In order to avoid experimenter demand effects, we chose to refer to other players as "persons".

In the "Chat" condition, subjects received information that they would be able to communicate with all other seven players in their group at the end of each period. They were also informed that they were not allowed to reveal their personal identity, or use improper language. In the "Chat + Choice" condition, players received information about both the partner choice mechanism and the function and rules of the chat room.

In all experimental conditions, the last phase of the instructions consisted of a set of control questions in order to even out the understanding of the game on part of the players. This is a common procedure, but not really necessary when the experiment is repeated (this allows for learning). However, as the chat might also function partially as a learning device, this gives us an additional reason to provide all subjects with control questions. This learning effect of communication is not the focus of our research question. If we provide control questions, then we might partially control for the learning advantage present in the conditions with communication.

We tried to keep the instructions given to the different experimental conditions as similar as possible, in order to avoid the chance that features of the instructions would drive the overall results. Additionally, we took great care not to "push" subjects in the desired direction according to our research hypotheses. As previously mentioned, we referred to the choice of partner as "choosing another person". In order to avoid experimenter demand effects, we kept

[32] Copies of the instructions are presented in appendix A.

the instructions concerning the chat identical between the chat condition and the combined condition. This ensured that if subjects would use the chat for partner selection, we would have a stronger result than if we had hinted towards this behavior initially (although this comes at a high price).

Chapter 5: Results

This chapter features an analysis of the collected data. First, I provide descriptive statistics on the average contribution patterns and measures of partnership stability, as well as results from non-parametric tests. Finally, I provide an econometric analysis of individual behavior over time. Key findings are displayed in italics throughout the chapter.

5.1 The effect of chat room communication

Finding 1: *Chat room communication increases overall contributions by 28 percentage points compared to the "Baseline" condition. The difference increases over periods.*

Figure 6 displays and compares the contribution patters in the "Baseline" and "Chat" condition.

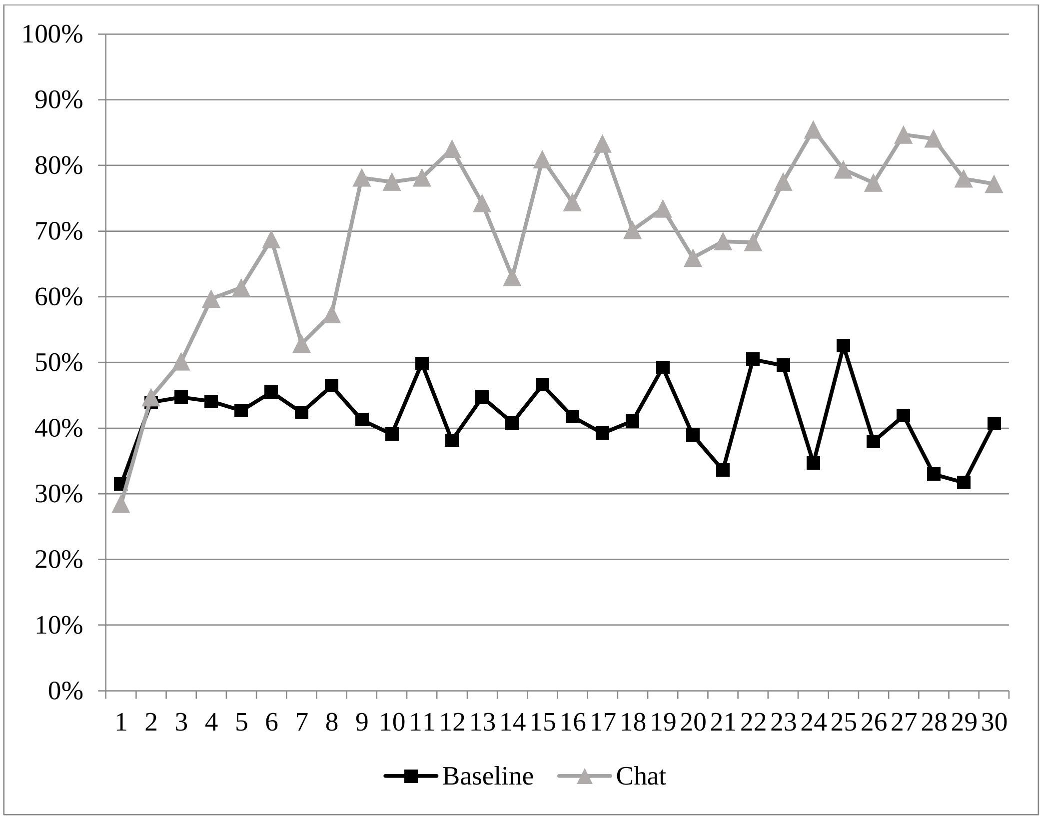

Figure 6 - Average cross sectional contributions (%), "Baseline" vs "Chat"

In the "Baseline" condition, average contributions amount to 41.9% of the total endowment. In the "Chat" condition, average contributions reach 70.2%. This is a difference of about 28 percentage points (a 67.5% increase). This strong effect of the chat is in line with previous experimental results (Bochet et al 2006).

We also see that the effect of the chat appears to be increasing over periods. The difference between the two conditions grows as the experiment progresses. The "Chat" condition reaches the highest average contribution level in the last period of all experimental conditions, 77.2 % of the total endowment.

An analysis of the chat transcripts reveals that a lot of written messages are attempts to extract promises from other subjects and commitments to common strategies. This is also in line with previous literature (Dawes et al. 1988; Bochet et al. 2006). The subjects try to persuade the others in the group to contribute their entire endowment, because this will result in a fair outcome where everyone is equally well off. We also observe praise and punishment of previous actions, as well as expressions of disapproval upon receiving a lower payoff than expected. Subjects appear willing to share reputational information about non-cooperators, even though this occurs in a setting where the "targets" observe them.

Results from non-parametric tests, "Baseline" vs "Chat"

In order to formally assess the differences between the two treatment conditions, we conducted a non-parametric Wilcoxon-Mann-Whitney test. Non-parametric tests rely on weaker assumptions than parametric tests.[33] The Wilcoxon-Mann-Whitney test assumes that we have two random, independent samples drawn from the same distribution and that the observation units are mutually independent (Siegel & Castellan 1988: 129). We test the null hypothesis of identical distributions against the one-sided alternative that the "Chat" sample is drawn from a stochastically larger distribution (that the bulk of observations from this sample are larger).[34]

[33] Parametric tests either make full distributional assumptions or assumptions about some moments of the distribution. For instance, in linear regression analysis one assumes (in finite samples) normality of the error terms (Wooldridge 2009: 118). In contrast, non-parametric tests do not specify the distribution from which the sample is drawn. A parametric test should only be considered when the assumptions are reasonable and the level of measurement is at least as strong as interval scaling. If this is not the case, we may impose false information upon the data when making parametric assumptions (Siegel & Castellan 33-35).

[34] For all tests reported, we also conducted robust rank order tests as robustness checks (Fligner & Policello 1981). This test only assumes equal medians (not variances or shapes of the population distribution as the WMW test) in the null hypothesis. None of the results we report were affected by this procedure.

The test proceeds as follows. We rank the individual observations (contributions) in the joint sample within each period in orders of increasing size (corrected for ties). The rank sum is then calculated for each condition, and the probability of observing this rank sum computed.[35] This implies that we "reinterpret" the test in each period. For each period, the "treatment" is defined as the chat opportunity and the history created by this exogenous source of variation. Figure 7 displays the one-sided p-value from the Wilcoxon-Mann-Whitney test conducted for each period of the experiment.

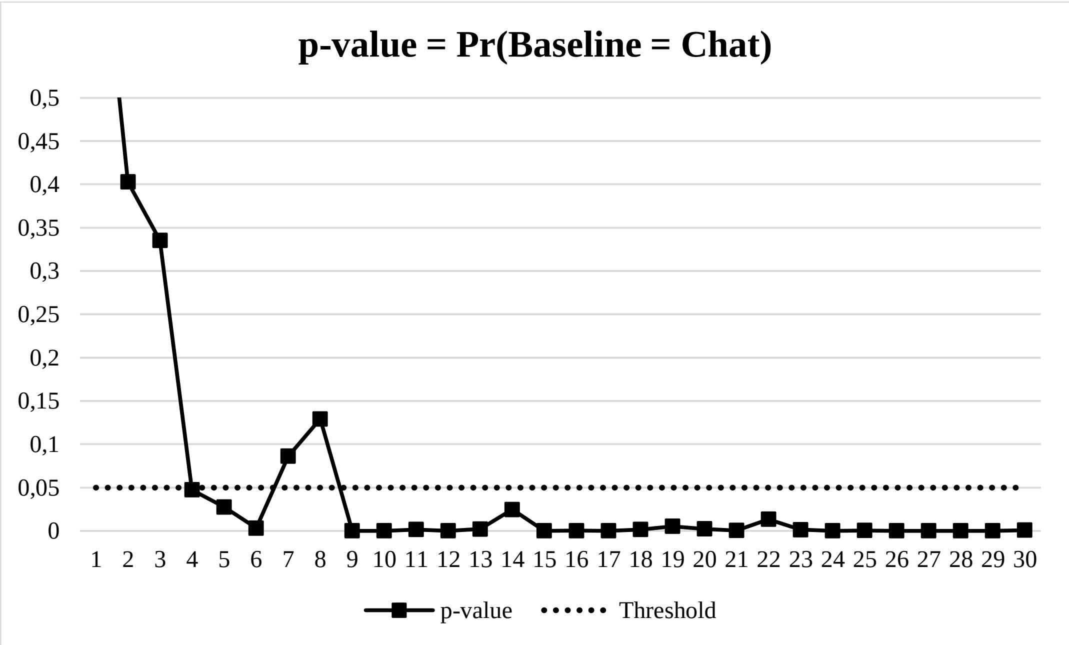

Figure 7 - One-sided p-values from a WMW test, "Baseline" vs. "Chat"

The test indicates that the effect of chat room communication is significant; The rank sum is significantly larger than expected in 25 of 30 periods ($p < 0.05$, Wilcoxon-Mann-Whitney, one-sided), and $p = 0.01$ in 21 of 30 periods. Thus the test indicates a location shift in the distribution in all but five periods.

Communication also significantly affects contribution levels when we compare the "Chat + Choice" to the "Choice" condition. The same test indicates a location shift in 17 of 30 periods ($p < 0.05$, Wilcoxon-Mann-Whitney, one-sided). Almost all of these location shifts are detected in the last part of the experiment.

[35] For this test, the normal approximation was used because the sampling distribution of the rank sum converges rapidly to the normal distribution as the sample size increases (Siegel & Castellan 1988: 132).

We also checked that the results are robust to different ways of conducting the test. A challenge in experiments with repeated observations is that the individual observations in the same group will be correlated (for instance due to feedback effects or learning) (Friedman & Sunder 1994: 98-99; Fréchette 2012). If time-dependence is present, the samples from period 2 onwards are not random (Friedman & Sunder 1994: 98). At the same time, we cannot perfectly control for possible pre-interaction among subjects within a given session. Particular features of a given group of subjects may yield correlation within this group, and this may not fully be attributed to the treatment variable. In the “Chat” condition these concerns are especially valid as there is interaction among all group members in every period.[36]

A common approach in the literature to correct for such problems is to use session or group averages as the unit of observation (Cinyabuguma et al. 2005; Fréchette 2012) as these units probably exhibit statistical independence. A drawback of this procedure is that the sample size is reduced, so that the probability of type II error increases (Fréchette 2012). In our case, when we conduct the test with group averages, there are only four observations per treatment. This yields a high probability of failing to reject a wrong null hypothesis (Siegel & Castellan 1988: 10).

Using group averages as the unit of observation, the test yields a p-value of 0.0147 (one-sided test).[37] The effect of chat room communication is so large that the reduction in sample size does not matter for detecting a location shift in the distribution.

[36] In the “Baseline” and “Choice” condition, feedback and interaction is restricted to pairs of individuals.
[37] Here, we report exact p-values, as the normal approximation is not reliable in small sample applications.

5.2 The effect of partner choice

Finding 2: *Partner choice increases overall contributions by 14 percentage points compared to the baseline condition, a significant difference according to non-parametric tests. The treatment effect appears to be a level effect.*

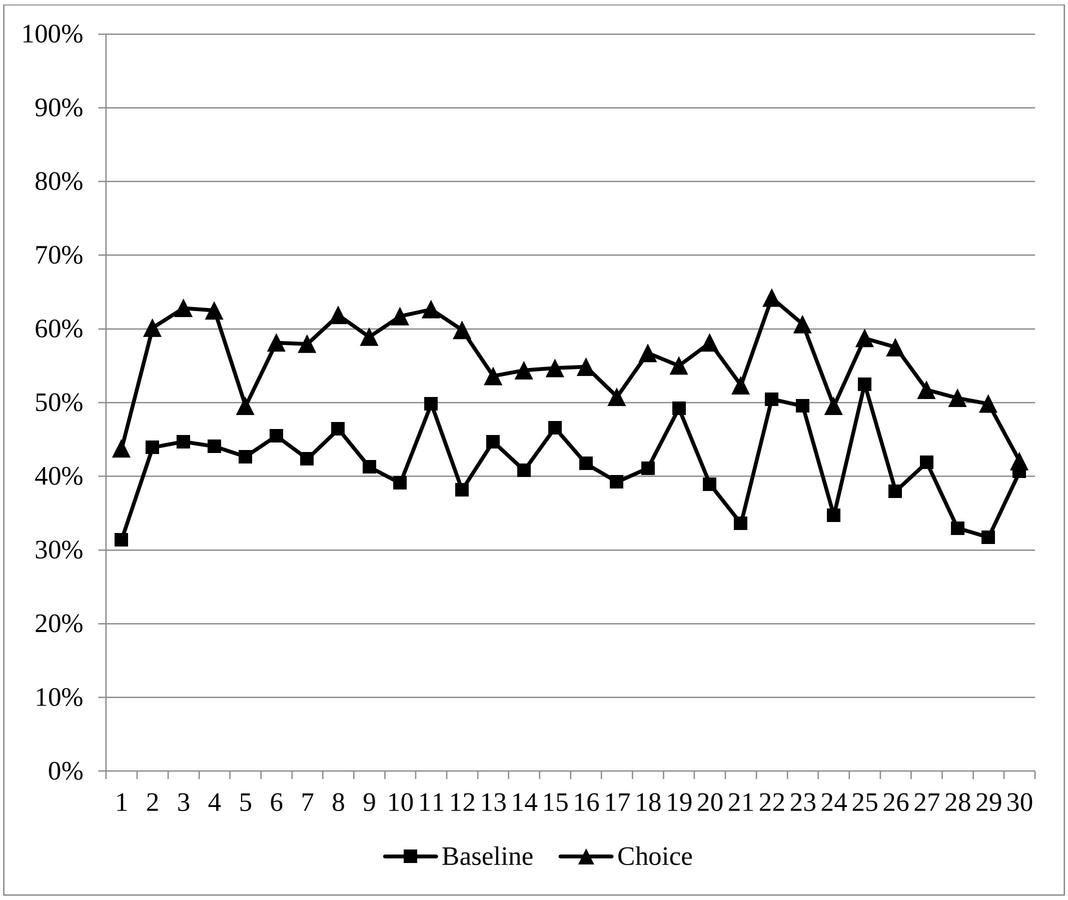

Figure 8 – Average cross sectional contributions (%), "Baseline" vs. "Choice"

The partner choice condition reaches higher average contribution levels than the baseline, 55.8 percent of the total endowment compared to 41.9 percent. The difference is 13.9 percentage points (a 33 percent increase). Average contributions start higher (about 12.3 percentage points). There is a slightly declining time trend, and contributions fall to the level of the baseline condition in the last period.

Results from non-parametric tests, "Baseline" vs "Choice"

Figure 9 displays the results from a one-sided Mann-Whitney test, conducted identically as above.

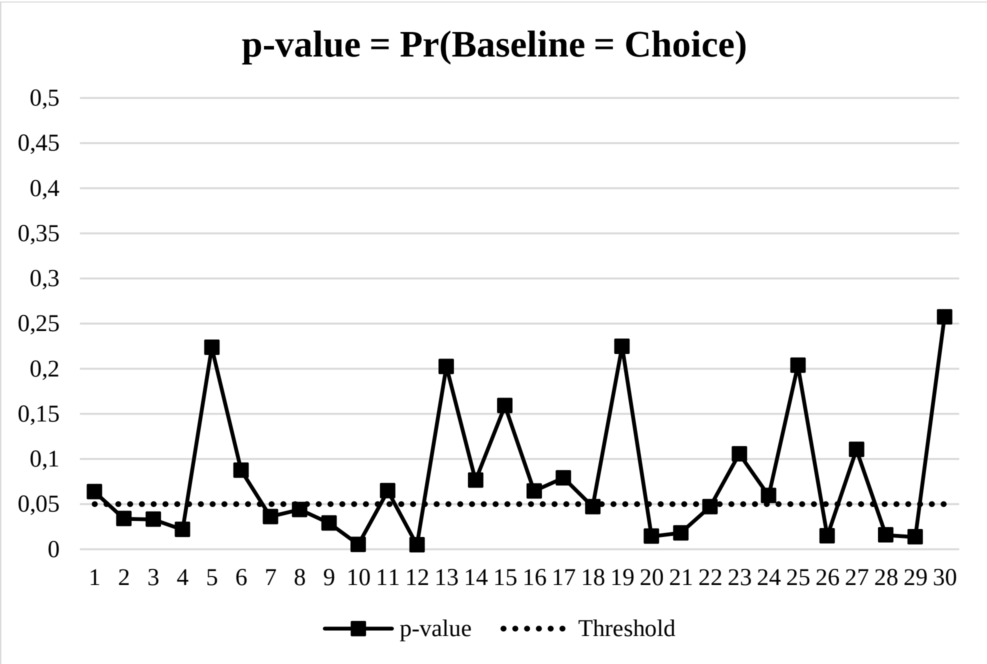

Figure 9 - One-sided p-values according to a WMW test, "Baseline" vs. "Choice"

The Wilcoxon-Mann-Whitney test supports the visual "test". The rank sum is higher than expected for the "Choice" condition in all periods, and the test indicates a significant location shift in the distribution in 15 of 30 periods ($p < 0.05$, Wilcoxon-Mann-Whitney, one-sided).[38] The effect does not appear to be changing with time.

The competitive altruism hypothesis is supported by our data. The "Choice" condition starts higher than the baseline, and the "Choice + Chat" starts higher than the "Chat" condition. Overall, average contributions are 13.4 percentage points higher in the conditions with partner choice. The rank sum test indicates a significant location shift in the first period when we compare the "Chat" and "Choice + Chat" condition ($p = 0.03415$, Wilcoxon-Mann-Whitney, one-sided).[39] When we compare the "Baseline" and "Choice" condition, the null is rejected at

[38] In seven additional periods, $0.05 < p < 0.10$. In two periods, $p < 0.01$

[39] Some might argue that this reflects an interaction effect. For instance, if subjects expect the chat to enhance "broadcast efficiency" (the reach) of a costly signal, the incentives to signal are higher in the "Chat + Choice" condition (Alden Smith 2010)). However, first period contributions are actually slightly higher in the "Choice" condition than the "Chat + Choice" condition, but the "Chat" condition starts a little lower than the baseline. This makes it easier to detect a location shift when we compare the "Chat" and "Chat + Choice" condition.

the 10% level but not the 5% level (p = 0.06365, Wilcoxon-Mann-Whitney, one-sided). If we pool together the two samples in the first period without partner choice and treat this as one sample, and do the same for those with partner choice, the test yields a p-value of less than 0.01 (p = 0.0091, Wilcoxon-Mann-Whitney, one-sided test). This is evidence for competitive altruism.[40]

When we conduct the test with group averages, the rank sum test yields a one-sided p-value of 0.0571 (Wilcoxon-Mann-Whitney, one-sided).[41] When we treat individual means as the unit of observation, the test indicates a significant location shift at the conventional level (p = 0.034, Wilcoxon-Mann-Whitney, one-sided). When we conduct the test with group averages, the null hypothesis is thus clearly rejected at the 10% level, but not at the 5% level. When we use individual means, the null hypothesis is also rejected at the 5% level. The latter result also holds when we take individual means for the first five periods, when matching is practically random in both conditions. In order to address the problem of low statistical power in the first test, the sample size should be increased (Davis & Holt 1993: 528).

Finding 3: *The subjects transform the game from random matching to repeated interactions.*

Did subjects make use of the partner choice opportunity? We may address this question by examining how pairs form over time in the "Baseline" compared to the "Choice" condition. If assortment is random in both conditions, we should see no differences in "partnership" stability between these conditions. For this purpose, a variable called $change_{it}$ was defined. This variable registers whether a subject changed his or her partner from the last period to the current. It takes the value of one if the partner changed, and zero otherwise. Figure 10 displays the distribution of this variable in the "Baseline" and "Choice" condition, respectively.

[40]A sceptic might argue that if subjects in the "Choice" condition succeed in finding a match in the first period, first-period behavior would partly reflect a reward for being chosen or other potential confounds. However, in our experiment no subjects attained a mutual match in the first period, indicating that this is not a valid objection.
[41] With this small sample size (m = n = 4), exact p-values below p = 0.10 are only available at the following levels: 0.0143, 0.0286 and 0.0571 (Siegel & Castellan 1988: 340).

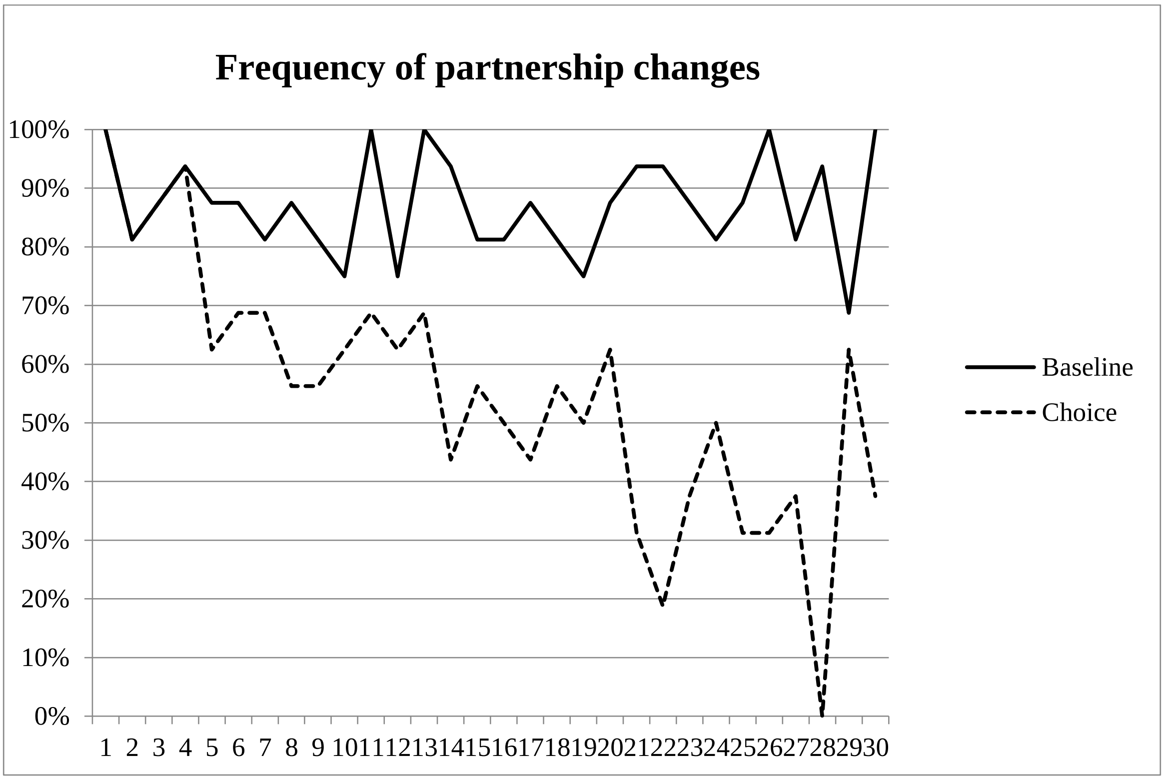

Figure 10 - Frequency of partnership changes, "Baseline" vs. "Choice"

Figure 10 suggests that assortment is random in the "Baseline" condition. This is no surprise because the program was designed to generate random numbers in this condition. Assuming assignment to the categories is random, the $change_{it}$ variable is bionomically distributed with mean p and variance $p(1-p)$. The theoretical probability of changing partner in a given period is $p = \frac{6}{7} \approx 0.857$. This is very close to the overall observed frequency in the "Baseline" condition.

The clear downward trend in the "Choice" condition indicates that partnership formation is far from random here. The overall observed frequency of partnership changes in this condition is 0.55, substantially lower than the expected value of 0.857.

If we look at the distribution in the last period and treat each pair of subjects as one observation, the observed distribution is extremely unlikely to occur at random according to a Fisher-test (p = 0.00013, two-sided test).[42]

[42] For the basis of Fisher's exact test, see Siegel & Castellan (1988: 104-105).

Within the “Choice” condition, we may also split between those who participate in voluntarily formed pairs (“matched” pairs) and those who are randomly matched within the residual pool (“unmatched” pairs). The only way to achieve greater stability is for some subjects to engage in repeated interactions with the same partner by voluntary consent. But this also limits the pool of residual subjects, so the unmatched pairs should also become more stable with time. Figure 11 shows the frequency of matched and unmatched pairs that change with time in the “Choice” condition.

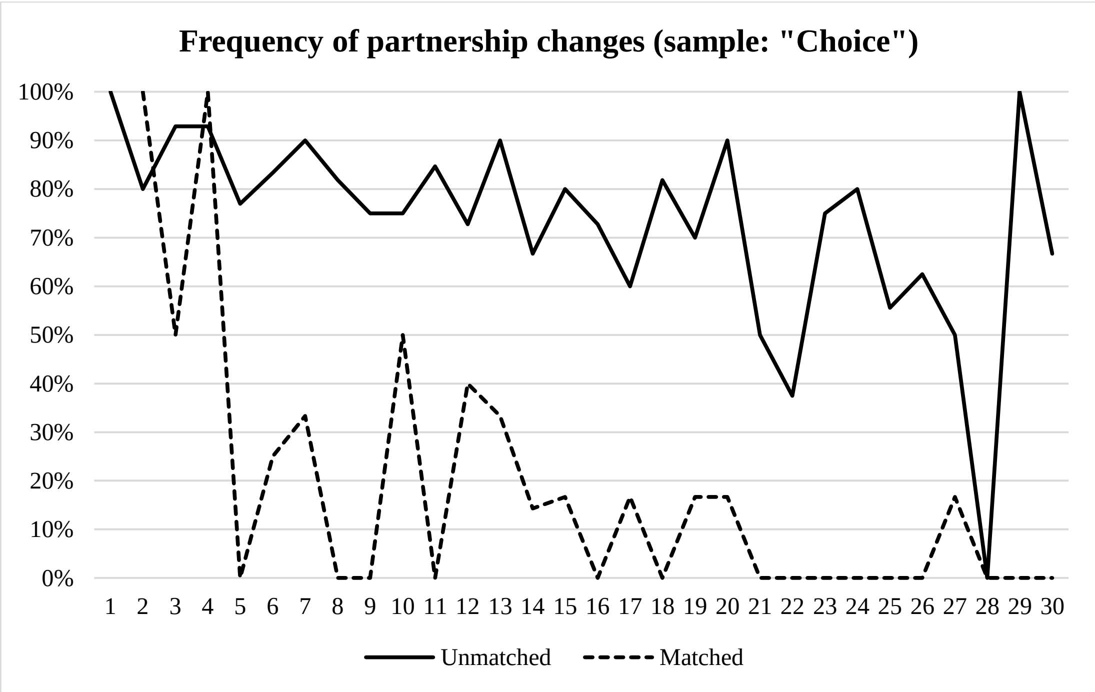

Figure 11 - Frequency of partnership changes, unmatched vs. matched pairs

Figure 11 reveals two interesting findings. First, the matched pairs “transform” the game from a random matching design to stable, repeated interactions. Within these pairs, almost no subjects change their partner in the last part of the experiment. Second, the unmatched pairs become increasingly stable with time. This is because the matched individuals make themselves unavailable by entering voluntary associations. By doing this, the “residual” pool of subjects is reduced, lowering the probability of changing partner. We also see that the upwards jump in figure 10 in the end is due to randomness, not a breakdown of stability in the matched pairs.

Finding 4: *Average contributions in matched pairs are substantially higher than in unmatched pairs.*

Why do the subjects transform the game? Previous research suggest that conditional cooperators, when allowed to do so, will avoid free riders by entering cooperative groups (Erhart & Keser 1999; Page et al. 2005). Those who cooperate are most likely to be selected (Barclay & Willer 2007; Sylwester & Roberts 2010). If this is the case, we should see clear differences in average contribution levels between the matched and unmatched pairs. That is, we should expect cooperative subjects to self-select (sort) into pairs with other cooperators. Figure 12 displays the difference in average contributions between the matched and unmatched pairs.

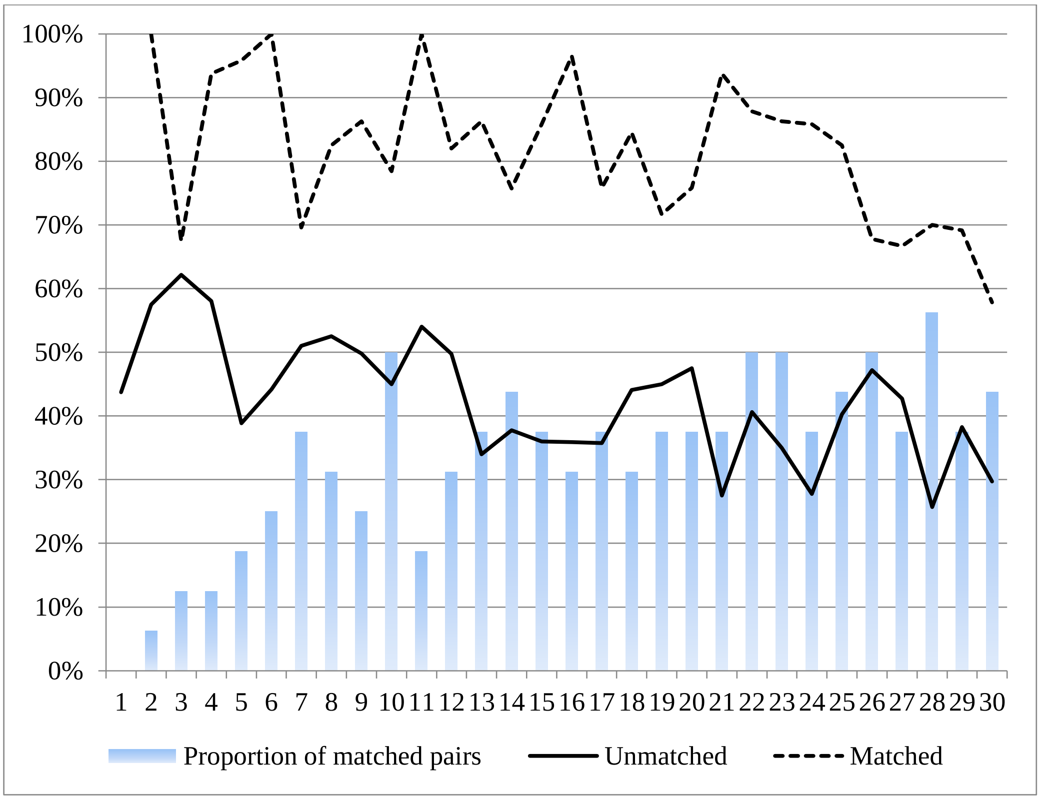

Figure 12 - Average contributions (%), unmatched vs. matched pairs, “Choice” sample

These results support the conjecture of behavioral sorting. Average contributions in the matched pairs are substantially higher than in the unmatched pairs, and the proportion of matched pairs increases over periods. Average contributions in the matched pairs amount to

79.9 percent of the total endowment, compared to 43.7 % in the unmatched pairs.[43] This is a difference of 36.2 percentage points, almost a 100 % increase. However, the contribution level declines over time in both partnership types. This is consistent with the idea that conditional cooperators have a "self-serving bias". They do not fully reciprocate their partner's actions (Fehr & Fischbacher 2003). The partner choice opportunity alone is not sufficient to stabilize the decaying contribution pattern.

Figure 12 reveals strong evidence for behavioral sorting (selective interaction) in our data. As the matched pairs are remarkably stable over time, this suggests that in addition to inducing strategic behavior (competitive altruism), the partner choice opportunity presents an enormous cooperators' advantage. As we saw previously, the chat reached average contributions of about 70 percent of the total endowment. Thus, the chat is incredibly efficient on the aggregate level. But if we look at the gains to cooperation, the partner choice opportunity is about equally efficient. This is because cooperators interact preferentially amongst themselves (they transform the game), while in the "Chat" condition, a cooperator can only extract promises and hope for a cooperative partner in the production stage.

Our theoretical speculations suggested that cooperators would be most likely to be selected. In other words, signaling should give you access to cooperative partners (Roberts 1998). Then, those who tend to participate in matched pairs should have higher initial contributions than those who participate less in such pairs. Comparing initial contributions between those who participated in matched pairs for at least ten periods with those who tended to participate less in such pairs, the difference is 19 percentage points. This is a significant difference according to a Wilcoxon-Mann-Whitney test ($p = 0.02075$, one-sided). This links competitive altruism to the probability of being selected.

[43] Within the unmatched pairs, I also compared those who express a desire for a partner to those who do not (those who choose themselves are assumed not to desire a particular partner). There are no clear, discernible differences between these two groups. However, even when we pool the two "Choice" conditions, the number of subjects who chose themselves is very low, so it is difficult to make any meaningful comparisons.

5.3 Partner choice and communication

Finding 5: *Adding partner choice to communication does not increase contributions compared to communication alone.*

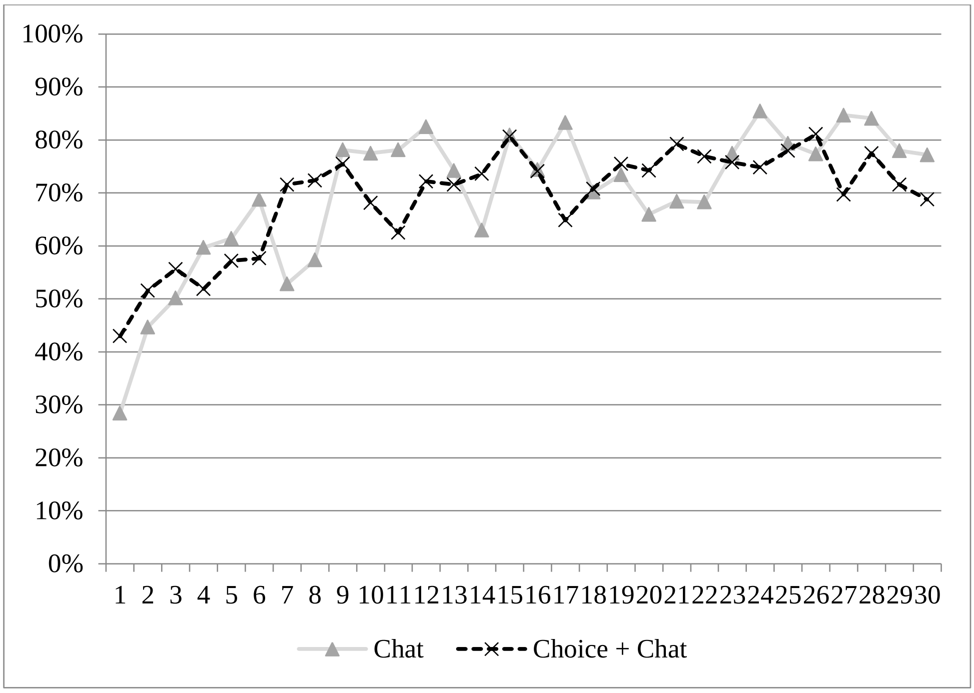

Figure 13 - Average contributions (%), “Chat” vs. “Choice + Chat”

The average contribution level in the “Chat” and “Choice + Chat” condition is almost identical. The former condition reaches an average of 70% of the total endowment, while the latter reaches 69 % on average. Contributions start higher in the “Choice + Chat” condition (14.5 percentage points), but else there is obviously no effect of partner choice here.

Whereas we predicted that communication might improve the efficiency of partner choice through improved coordination, we find no support for this hypothesis. In fact, even more surprisingly, the effect of partner choice entirely disappears when we allow for communication. It thus seems that the chat is so efficient that it leaves no room for a further effect of partner choice.

Looking at the chat transcripts, we find some evidence that the chat is used for partner selection, but only in two out of four groups in this condition. The chat is rather used for coordination at the group level. If players enter repeated interactions with the same player, the chat transcripts reveal that these players use “targeted coordination”. They extract promises within their partnerships, and not at the level of the group. Additionally, many of these pairs reinforce good behavior through verbal rewards.

Did subjects in the combined condition make use of the partner choice opportunity? Maybe the partner choice opportunity was superfluous. Figure 14 displays the distribution of partner updates across time in the “Chat” and “Choice + Chat” condition, respectively.

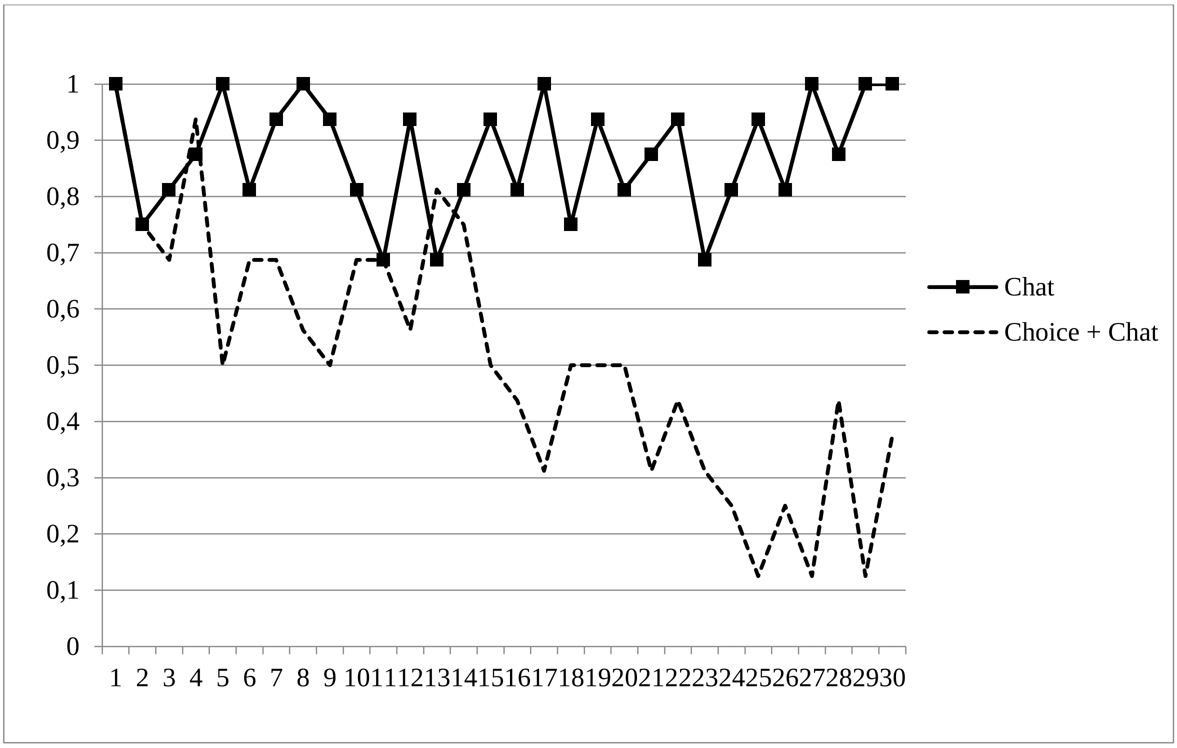

Figure 14 - Frequency of partnership changes, “Chat” vs. “Choice + Chat”

Figure 14 reveals exactly the same patterns here as we found when we compared the “Baseline” and “Choice” condition. Subjects transform the game from low stability to substantial stability.[44] Thus, even when communication is possible, people express a strong willingness to choose their partner.

[44] When we split between matched and unmatched subjects, the results are also qualitatively identical, so they were not included here.

Finding 6: *Both in the presence and absence of communication, matched pairs have higher average contributions than unmatched pairs.*

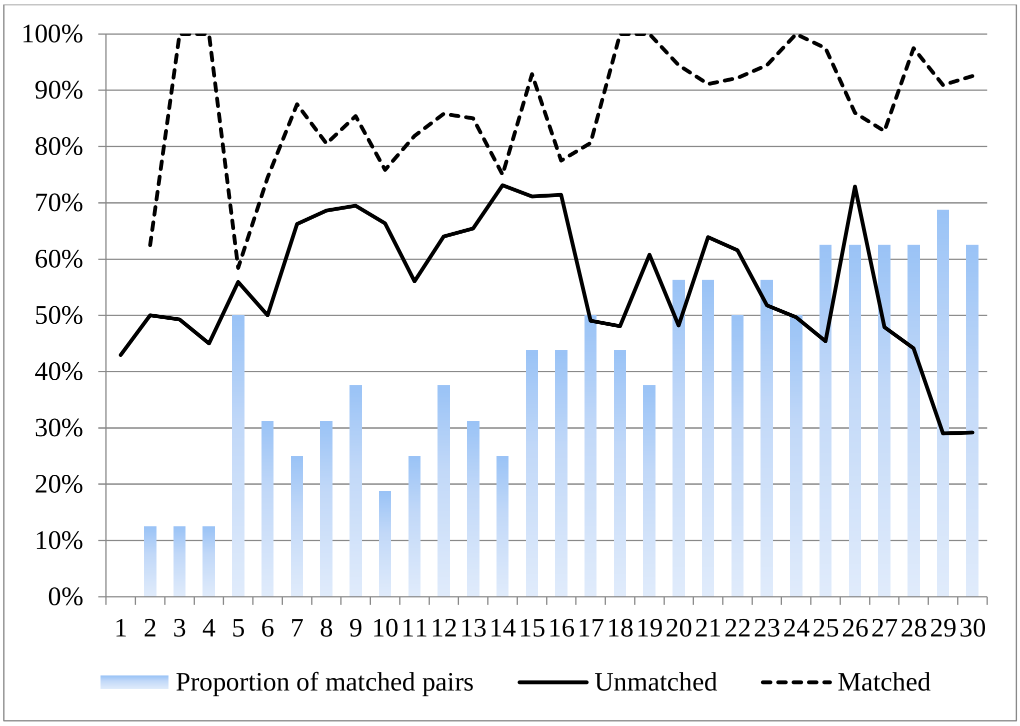

Figure 15 - Average contributions (%), matched vs. unmatched pairs, "Chat + Choice" sample

We also observe behavioral sorting. The matched pairs reach an average of 88.1 percentage points, while the "unmatched" pairs average 56.3 percentage points. This is a difference of 31.8 percentage points. Thus, cooperators use the partner choice opportunity to selectively interact even when communication is allowed.[45]

Finding 7: *The matched pairs with chat stabilize contributions almost at the level of the social optimum. The chat has no such stabilizing effect for the unmatched pairs.*

We also compared the "transformed" pairs within the "Chat + Choice" sample to other groups. It makes sense to talk about "transformed" pairs in the last ten periods, because subjects in matched pairs play the game more or less with the same partner here (they play a set of finitely repeated games). In these "transformed" pairs with chat, average contributions in the last ten periods amount to 92.3% of the total endowment, 42 percentage points higher

[45] Comparing initial contributions between those who tend to participate in "matched" pairs with others yields the same findings as above. Initial cooperators tend to get selected by their desired partner more often.

than for the unmatched pairs with communication. This is also higher than the "Chat" condition, which reaches 78% in the last ten periods, and the matched pairs without chat, which reaches 76.6%. If we conduct a rank sum test in the last period, the test indicates a significant location shift when we compare the matched pairs with and without chat ($p < 0.0009$, WMW, one-sided), when we compare the unmatched and matched pairs with chat ($p = 0.00005$, WMW, one-sided), and when we compare the matched pairs with chat and the "Chat" sample ($p = 0.04145$, WMW, one-sided). Thus the "transformed" pairs with chat "close" the game with significantly higher contribution levels than in any other comparison group.

In contrast, there is *no* effect of chat room communication in the last period when we compare the unmatched pairs with and without chat. In the last period, the unmatched pairs reach an average contribution level of about 29%, both with and without chat. Thus, while the chat is extremely efficient in stabilizing the decay in contributions in the matched pairs, it has *no* such stabilizing effect in the unmatched pairs. From figure 15, we also see that we witness a stabilization of cooperation near the social optimum in the "transformed" pairs. In the unmatched pairs, average contributions fall from 70% to 30% in the last five periods. This is a decline of 40 percentage points.

This suggests that the chat room works selectively in the "Chat + Choice" condition. In the "Chat" condition, promises may affect the beliefs of all others in the group concerning the behavior of their future partner. But in the "Chat + Choice" condition, the players who enter matched pairs are unavailable, so their promises do not influence the beliefs of other subjects.

5.4 Econometric analysis

Our data consists of 128 cross-sectional units (individuals) observed over 30 periods. As an additional robustness check on the results, we therefore conduct a panel data analysis. This analysis supports the previous results.

In repeated public goods games and prisoner's dilemmas, we expect that feedback effects and learning influence behavior. Panel data allows us to model such interdependencies. If we capture these in our theoretical model, panel data yields consistent estimators for the treatment effects (Davis & Holt 1993: 527). With panel data, we may therefore address the problem of interdependent observations without throwing away information about individual behavior. We may model the role of endogenous regressors (Verbeek 2012: 373-375) and control for intragroup correlation by computing cluster-robust standard errors (Verbeek 2012: 389-390; Fréchette 2012). However, this analysis comes at a cost; we make auxiliary assumptions regarding the structure of the error terms (Davis & Holt 1993: 528).

5.4.1 Choice of regression model

For the econometric analysis of the individual contribution patterns, the linear random effects (RE) estimator was chosen. Compared to a Pooled OLS procedure, this estimator exploits the autocorrelation in the composite error term and therefore is more efficient (Verbeek 2012: 381).[46] The time-invariant effects are not directly identified in the fixed effects (FE) model, because it only exploits the variation within individuals (Wooldridge 2010: 328). The RE model is justifiable in our case because the subjects were randomly assigned to the experimental conditions.[47] This avoids selection effects and ensures that individual heterogeneity is distributed randomly across conditions (Friedman & Sunder 1994: 24; Wooldridge 2010: 907).

Due to the upper and lower limit of the dependent variable, the true data generating process is nonlinear in the parameters. In our data set, a little more than 50 percent of the observations are observed at either the upper or lower limit. A linear model therefore only gives an

[46] This comes at the cost of a stronger identification assumption. The linear RE model assumes strict exogeneity, while OLS only requires contemporaneous exogeneity for consistency (Verbeek 2012: 384).

[47] Additionally, the practical differences in the coefficients on the time-varying covariates between the RE and FE model were small. The Hausman test is not valid in this case, because it assumes that at least one of the models are consistent under both the null and alternative hypothesis (Wooldridge 2010: 329).

approximation of the true marginal effects (Wooldridge 2010: 668).[48] However, this procedure may yield a good approximation if the covariates are close to their mean values (ibid.), as is necessarily the case for the time-invariant treatment effects.

Due to the "corner solution" response variable, public goods game data are often estimated using Tobit random effects (Merrett 2012). However, this procedure yields biased and inconsistent estimates when the errors are non-normal or exhibit heteroscedasticity (Arabmazar & Smith 1982; Wooldridge 2010: 685-686).[49] A recent cross-validation study also showed that while the predictive performance of the linear FE and RE models are about equal to that of the RE Tobit model, the latter has a greater bias (Merrett 2012). Additionally, interpretation of marginal effects is straightforward in the linear model.

We allow for within-group correlation by reporting the results with cluster-robust standard errors. Each group of eight constitutes a cluster. This allows for heteroscedasticity as well as autocorrelation within an individual and within clusters (Verbeek 2012: 389-391).

We assume the following theoretical population model:

$$y_{it} = \alpha_0 t + \gamma_1 y_{j,t-1} + \gamma_2 y_{j,t-1} * k_{it} + k_{it} + \boldsymbol{x\beta} + c_i + u_{it} \qquad [5.1]$$

$$u_{it} \sim IID(0, o_u^2); \qquad c_i \sim IID(0, o_c^2) \qquad [5.2]$$

The dependent variable y_{it} is the contribution of individual i in period t. This variable has a lower bound at 0 and an upper bound at 20. We control for a linear time trend. $\boldsymbol{x}$ is the vector of treatment dummies, interaction terms and demographic controls. We interact the treatment variables with the trend term in order for allow for time-varying treatment effects. We control for the subject's gender and previous participation in economic experiments. In order to capture a possible end game effect, we also add time dummies for the two last periods.

[48] Estimating the data with a linear model implies assuming a mean linear in $\boldsymbol{x}$, but this can only hold when the explanatory variables take on a very limited range of values (Wooldridge 2010: 668).
[49] Semi-parametric methods (CLAD estimators) relax the distributional assumptions in the Tobit model (Chay & Powell 2001). However, I am unaware of panel data applications of such models.

The partner choice opportunity allows for repeated interactions. This means that reciprocity (conditional cooperation) is more behaviorally relevant in this condition. We therefore interact the previous partner's contribution with a dummy indicating whether the subject kept his previous partner this period. The term $y_{j,t-1} * k_{it}$ therefore captures reciprocity involved in repeated interactions. We also control for "naïve" feedback, captured in the term $y_{j,t-1}$. The coefficient on the dummy k_{it} should turn negative when controlling for feedback effects. This is equivalent to stating that changing one's partner should increase contributions, and so reflects a "restart effect" (Andreoni 1988).

The error component c_i is an individual-specific effect, assumed constant over time, and u_{it} is an idiosyncratic error component allowed to vary over time. The key identification assumptions in the linear RE model are stated in expressions 5.3 and 5.4.

$$E(u_{it}|\boldsymbol{x}_i, c_i) = 0 \ \forall t \quad [5.3]$$

$$E(c_i|\boldsymbol{x}_i) = 0 \ \forall t \quad [5.4]$$

Here, $\boldsymbol{x}_i = (\boldsymbol{x}_{i1}, \boldsymbol{x}_{i2}, \dots, \boldsymbol{x}_{iT})$ is just the vector containing all the regressors in equation 5.1 for all time periods (Wooldridge 2010: 292). Assumption 5.3 states that the expected value of the idiosyncratic error term, conditional on $\boldsymbol{x}_i$ and the individual-specific random effect is zero. This is a strict exogeneity assumption, which rules out correlation between the error term and the regressors for past, present and future values of the regressors. Assumption 5.4 is specific to the RE model, and rules out correlation between the regressors and the individual-specific error term (Wooldridge 2010: 292).

5.4.2 Regression results

Table 2 - RE regression results on individual contribution (Contribution in period t)

	Entire sample	No chat	Chat	Random	Choice
Period	-0.0101 (0.0200)	-0.0271* (0.0160)	0.179*** (0.0242)	-0.0225 (0.0180)	-0.0737*** (0.0205)
Prev.partnercontr.	0.0423* (0.0249)	0.00557 (0.0289)	0.0674 (0.0419)	0.0302 (0.0294)	0.0537 (0.0427)
Kept partner	-1.075*** (0.372)	-1.198** (0.498)	-1.259* (0.720)	-0.708 (0.700)	-0.964** (0.479)
Kept partner Prev.partnercontr.*	0.174*** (0.0344)	0.232*** (0.0630)	0.155*** (0.0453)	0.0602 (0.0477)	0.211*** (0.0445)
Choice	3.273** (1.299)	2.996** (1.407)	0.429 (1.187)		
Chat	2.541** (0.998)			2.303** (1.007)	-0.249 (1.568)
*Choice*Chat*	-3.172* (1.783)				
*Period*Choice*	-0.0668*** (0.0255)	-0.0554** (0.0246)	-0.0748 (0.0484)		
*Period*Chat*	0.180*** (0.0227)			0.203*** (0.0212)	0.148*** (0.0360)
Control variables					
Period 29	-1.270*** (0.488)	-1.075 (0.668)	-1.456** (0.707)	-1.513*** (0.323)	-0.979 (0.967)
Period 30	-1.342** (0.628)	-0.813 (0.733)	-1.849* (1.018)	-0.786 (0.840)	-1.920** (0.938)
Female	0.716 (0.667)	1.047 (1.189)	0.367 (0.691)	0.625 (0.754)	0.830 (1.130)
Participated before	-0.143 (0.790)	-0.448 (0.682)	0.110 (1.391)	0.0675 (0.938)	-0.334 (1.261)
Constant	7.958*** (0.932)	8.367*** (1.076)	10.20*** (1.080)	8.320*** (1.036)	10.79*** (1.312)
*N*T*	3712	1856	1856	1856	1856

Cluster-robust standard errors in parentheses (Cluster variable: Group)
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The results from the random effects estimation procedure support the previous analysis. The estimated effect of partner choice is positive and significant. The results indicate that the positive effect of partner choice is a level effect. All else equal, partner choice is predicted to increase contribution by 3.27 units on average (about 16 percentage points increase compared to the baseline). However, this effect is weakly declining over time, and the end game effect is significantly negative. The effect of communication is significant, and strongly increasing with time (for instance, the effect is predicted to double after ten periods). As noted previously, a conventional interpretation of the effect of communication suggests such a trending treatment effect (extracting promises and building group identity takes time). The overall time trend is predicted to be negative, but not statistically significant.

The feedback term is significant, given repeated interactions (reflecting reciprocity). The "naïve" feedback term is significant at the 10% level. Thus, the previous partner's contribution affects behavior more when partners are kept. The data suggests a "restart effect". Keeping one's partner decreases contributions, but the effect turns positive when the previous partner's contribution is high.

It is worth noting that the effect of partner choice disappears when chat is allowed. The interaction term is significant at the 10% level, but not at the 5% level. Both partner choice and communication significantly affect cooperation, but the level effect disappears when the other is allowed. However, the effect of the chat is still positively trending over periods, and the interaction term is always significantly positive.

Are the results robust to choice of estimator?

As an additional robustness check, we estimated equation 5.1 through Tobit Random Effects and OLS, respectively. The former takes into account the corner solution nature of the response variable (Wooldridge 2010: 670). The latter does not require strict exogeneity for consistent estimation (Wooldridge 2010: 291-292).

The results are also robust to the choice of estimator. The robustness checks yield no change in the direction of the predicted effects, but the significance level on some variables varies slightly across models. The coefficient on the "Choice" dummy is always significant. The results from these robustness checks are reported in appendix D.

Finding 8: *Partnership formation is mainly driven by past contributions, not communication.*

We mentioned previously that there were some tendencies towards using the chat for partner selection. However, this was not a prevailing pattern in all groups considered. In order to model the coordination process (what affects the probability of a mutual match), I estimated the following model through a linear probability model with random effects:

$$match_{it} = \alpha_0 t + \gamma_1 y_{i,t-1} + \boldsymbol{x\beta} + c_i + u_{it} \qquad [5.5]$$

The identification assumptions for the random effects model are as before. Here, $match_{it}$ is a binary variable registering whether an individual entered a partnership that period. It is constructed such that if the desired partner chooses you, a mutual match is granted. Thus, we may interpret the regression results as estimating the probability that the subject is chosen by her desired partner. The vector $\boldsymbol{x}$ contains other variables of interest and demographic controls. We control for communication and a linear time trend, as well as gender and previous participation in experiments. We also allow for an interaction effect between past contributions and communication.

If subjects use the contribution stage as a signaling opportunity, and this signal is what drives the coordination process, then the individual's lagged contribution variable should work as a reasonable predictor for whether he or she is chosen by her desired partner in a given period. Additionally, one should expect that as the experiment progresses, this "signaling effect" should become relatively more important over time. I therefore ran the regression in 5.5 for the first, middle and last ten periods of the experiment. The standard errors were clustered at the level of the group of eight subjects, as before. This is especially necessary in the LPM because the error term exhibits heteroscedasticity by construction (Wooldridge 2010: 562). The results are presented in table 3.

Table 3 - LPM regression on coordination behavior (Match in period t)

	Entire sample	**Period 1-10**	**Period 11-20**	**Period 21-30**
Period	0.0118*** (0.00231)	0.0422*** (0.00929)	0.0135 (0.00878)	0.00469 (0.0163)
Prev.Contr.	0.0330*** (0.00507)	0.0153*** (0.00285)	0.0232*** (0.00238)	0.0239*** (0.00317)
Chat	0.143 (0.0899)	0.310*** (0.0932)	0.00589 (0.266)	-0.194 (0.519)
*Period*Chat*	0.00261 (0.00436)	-0.0245 (0.0152)	0.0105 (0.0168)	0.0121 (0.0197)
Prev.Contr. Chat*	-0.0139** (0.00536)	-0.0139*** (0.00343)	-0.0124*** (0.00310)	-0.00374 (0.00478)
Control variables				
Female	-0.0174 (0.0420)	0.0600* (0.0345)	-0.0678 (0.0797)	-0.0346 (0.0577)
Participated before	-0.00341 (0.0397)	-0.0594 (0.0596)	0.0226 (0.0436)	-0.00511 (0.0753)
Constant	-0.204*** (0.0377)	-0.189*** (0.0521)	-0.108 (0.108)	0.0759 (0.429)
*N * T*	1856	576	640	640

Cluster-robust standard errors in parentheses (Cluster variable: Group), Sample: “Choice” and “Choice + Chat”
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The results indicate a signaling effect (the coefficient on own previous contribution is significant). The chat appears to have a large initial effect on the probability of a mutual match. The chat is estimated to increase the probability of finding a mutual match by 31 percentage points in the first ten periods. However, this does not hold in the rest of the experiment. In fact, when examining the chat transcripts, the initial effect of the chat may be attributed to one group of eight subjects who are particularly active in using the chat for partner selection in the first ten periods. Even when we control for the chat, the coordination process in the partner choice stage appears to be mainly driven by signaling through contributions. The higher your previous contribution, the lower is the effect of communication on the probability of a mutual match. The “signaling effect” becomes somewhat more important as the experiment progresses, suggesting that the signal is perceived as increasingly credible. This is consistent with reputation building.

The latter finding is remarkable. Subjects only seem to need a minimal amount of feedback in order to coordinate with their desired partner. This allows them to signal their intent of cooperating, and therefore to be perceived as a desirable partner. Let subjects explore their environment and accumulate private information, and they will transform the “random” process to a set of highly structured ones.

Are the results robust to choice of estimator?

The LPM often does a very good job in approximating partial effects (Wooldridge 2010: 563). Additionally, in the LPM we do not have to impose restrictive distributional assumptions for consistent estimation (Wooldridge 2010: 608). However, a problem with the LPM is the linearity assumption. This feature of the LPM leads to the possibility of obtaining predicted probabilities outside the unit interval. Additionally, the LPM assumes constant partial effects, which cannot literally hold when the response variable is binary (Wooldridge 2010: 562-563).

No predicted probabilities fall outside the unit interval in out model, and we have made our inference robust to heteroscedasticity. However, as an additional robustness check we estimated equation 5.5 through a Probit Random Effects model. This model does not assume linearity, but an additional assumption is that the individual random effect is normally distributed (Wooldridge 2010: 612). The results were robust to the estimation procedure.

Chapter 6: Discussion and conclusion

This thesis used experimental methods to examine the effect of partner choice and communication on cooperation. The study differs from previous literature in tying individual choices directly to the matching process, and by studying communication (in a chat room) and partner choice together. We also test for competitive altruism in a between subjects design. In contrast to former studies, the potential gains from costly signaling are not necessarily immediately realized, and a signal is only observed by a single individual. This means that a given signal has a lower reach than when contribution histories are published.

The main results show that both partner choice and chat room communication increase contribution levels in isolation, but there is no further increase in cooperation when partner choice is allowed in addition to communication. The competitive altruism hypothesis is supported.

Communication

The strong effect of communication is in line with previous literature (e.g. Dawes et al. 1988; Bochet et al. 2006). Subjects appear to use the chat in order to ensure that they face a cooperative group environment. That is, they try to achieve agreement upon a common goal (for instance, equal contributions from everyone towards everyone), and then try to enforce this goal through follow up messages. This strategy gives a substantial efficiency gain on average. The interpretation of this effect is in line with previous interpretations in the literature (Dawes et al. 1988). Universal promising is essential for the efficiency of communication. Interestingly, our results show that it does not matter whether subjects play a lot of isolated games in pairs or at the level of the group. The problem is still addressed in the same manner.

While Bochet et al. (2006), who also employ chat room communication, find a clear downwards trend and an endgame effect in the chat condition, this does not happen in our experiment. A possible explanation is that they only allow for chat room communication before the first, fourth and seventh period, while we allow for continuous communication. After the seventh period in their experiment, contributions drop. This suggests that continuous chat room communication is needed in order to sustain contributions through communication.

This is in consistent with findings from studies on face-to-face communication. Isaac & Walker (1988) found an end game effect when the initial communication opportunity was removed (although contributions were sustained at impressive levels towards the end). This end game effect was less pronounced in the groups with communication in the last sequence.

Mutual choice

The fact that mutual partner choice is efficient on the aggregate level also separates our results from previous findings. Coricelli et al. (2004) could not confirm higher average contributions from mutual partner choice compared to a random matching in a public goods game. This was attributed to the coordination problem introduced by mutual choice. However, we have incorporated a substantial coordination problem. While Coricelli et al. use groups of six, we use groups of eight (thus lowering the probability of being chosen at random by the desired partner). While they publicize all information about previous players, we only inform subjects about their previous payoffs and interaction partners. This means that acquiring relevant information takes more time. We also force subjects to make a choice in every period, making them have to "re-coordinate" constantly. Signaling is only possible through individual contributions.

A recent study by Bayer (2011) shows the efficiency of mutual partner choice. But this result holds only when contribution histories are published or staying in a partnership gives extra benefits. In our study, none of these requirements are met. Nevertheless, contributions increase when partner choice is allowed.

As previously mentioned, the latter studies suffer from the same basic problem. Choices only indirectly determine partnership formation, and they are filtered through a complex algorithm. To our knowledge, our study is the first to address partner choice where a mutual match is solely determined by individual choices. Our design therefore allows for a clean identification of the effect of mutual partner choice. We implemented the choice mechanism in an environment where information is attained through private experience, similar to Huck et al. (2012). They found that given such information, partner choice increases efficiency. We extend this finding to a mutual choice setting. Given identifiable players and private information, mutual partner choice increases cooperation among individuals.

Through partner choice, repeated and stable interactions evolve out of an initially random process. This suggests that partner choice presents a possible mechanism for the emergence of repeated interactions. Axelrod & Hamilton (1981) showed that cooperation is sustainable among egoists if interactions are infinitely repeated among the same partners. But a problem with this approach is the arbitrary assumption of guaranteed interactions. Our results throw some light on this issue. Partner choice allows for the formation of repeated interactions. As long as we continue to cooperate, our desired social partners continue to choose us as partners.

The matched (voluntarily formed) pairs have substantially higher contributions than the unmatched pairs. This is strong evidence for endogenous behavioral sorting. Thus, humans appear very able to achieve behavioral sorting even in a highly restrictive environment (information is private) where coordination is difficult (choice is mutual). Non-cooperators tend to get shut out of such repeated interactions, and are left with the residual pool of non-cooperators.

Future studies should try to identify the factors that moderate the effect of partner choice. What are the necessary conditions for mutual choice to increase cooperation? Another question is whether these "moderating" effects also work the same way for one-sided choice. This should be addressed in future studies in order to increase our knowledge of different choice mechanisms.

Competitive altruism

Our data supports the competitive altruism hypothesis, the suggestion that partner choice opportunities induce costly signaling in order to gain access to profitable partnerships (Roberts 1998; Hardy & Van Vugt 2006). Initial contributions are higher in the conditions with partner choice opportunities. As the baseline condition incorporates possibilities for reputation building (subjects are identifiable and players receive information about their payoff) differences between the "Choice" condition and the former necessarily reflect competitive altruism. Past contributions also appear to drive the matching process. Thus cooperators are more likely to be chosen by their preferred partner. This is in line with former studies (Barclay & Willer 2007; Sylwester & Roberts 2010).

To my knowledge, this is the first evidence for competitive altruism in a between-subjects design. Previous studies (Barclay & Willer 2007; Sylwester & Roberts 2010) have used within-designs. Thus competitive altruism carries over across research design.

Additionally, in former studies either the potential gains from costly signaling were very transparent (Barclay & Willer 2007), or information on contribution histories was simply "given" (published by the experimenters) (Sylwester & Roberts 2010). In our study, a given signal is more costly than in previous studies, because a signal is only viewed by a single observer at a time (because you may infer your partner's action). Nevertheless, we find clear evidence for competitive altruism, confirming its robustness as a behavioral pattern.

Partner choice and communication

We find no additional effect on cooperation when we allow for partner choice in addition to communication. But cooperators seem to fare relatively better in the combined condition due to the option of both avoiding free riders and coordinating within a partnership through the chat room (rather than at the level of the group). The chat stabilizes cooperation levels near the social optimum for the matched pairs, but has no stabilizing effect for the subjects "outside" these pairs. The matched pairs start to coordinate selectively within their partnerships, instead of at the level of the group.

Punishment has previously been shown to affect contributions on top of already existing chat room communication (Bochet et al. 2006). The fact that partner choice fails to achieve this may imply that punishment is more efficient in incentivizing cooperation in selfish types. In anticipation of altruistic punishment, free riders face clear incentives to cooperate. In our game, there are some incentives for free riders to mimic cooperators. However, the cost of signaling a cooperative disposition is perhaps too high relative to the perceived gains. Communication is probably more efficient in fostering cooperation in conditional cooperators, thus leaving little room for a further effect of partner choice.

This result does not mean that partner choice is irrelevant for cooperation. For instance, on e-Bay, coordination at the "group" level is infeasible. The group size is endogenous and immensely large. In such situations, partner choice and reputation mechanisms may be crucial for avoiding individual defection.

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Appendix A: Experimental instructions

Page 1:

This is an experiment on decisions. We pay out real money. You will receive 100 kroner as show up payment. In addition you will earn points which will be converted into money. The total amount in kroner you earn is paid out anonymously in a closed envelope at the end of the experiment.

The experiment consists of three parts: First you read the instructions. Then you go through the experiment. Finally, you will be asked to fill out a question form.

Page 2:

In this experiment you are person **i**. This number will be fixed the entire time. You have been randomly placed in a group of in total 8 persons. This group is the same during the entire experiment. All the other persons have also received a random number between 1 and 8, which will be fixed the entire time.

The experiment consists of several periods. In each period, you and 1 other person may produce together two fictitious items, blue and red.

Page 3:

The experiment consists of 30 periods in total.

{*Only Treatment II and IV*}:
In each period, you must first decide which of the 7 other persons you wish to produce together with this period. You may only choose one person. The other person also has to choose you. If you both choose each other, you produce together in this period. If you do not find another person, you will be randomly placed with one of the other available persons in your group. This also applies if you choose yourself.

{*Only Treatment I and III*}:
In each period, you will be randomly placed with one of the other persons in your group.

Page 4:

The period continues with a production decision. You and the other person receive 20 blue items each. In order to produce red items you must use blue items. The number of red items produced depends upon how many blue items you and the other person use.

The production stage lasts for 10 seconds. During these seconds you must decide how many blue items you will use in the production of red items. This is done by writing a number in the blue field on the screen. You press the Update-button to decide how many blue you want to use in order to produce red.

The production stage automatically closes after 10 seconds, and the number of blue behind Your choice will be made as your final decision.

Page 5 {*only treatment III and IV*}

At the end of each period, you will have the opportunity to chat electronically with the other participants in your group of 8. This chat lasts for 25 seconds per period.

The rules for the chat are as follows:

You are not allowed to reveal your personal identity. Improper language is not permitted either. Violations of these rules will lead to exclusion from the experiment, and you will lose your opportunity to receive payment.

Page 6

At the end of each period, you will see an overview of your stock of blue and red items, and who you produced with this period. Only you may see this overview.

The monetary value of the production in a period is dependent upon the stock of blue and red items. You receive 15 øre for 1 blue item and 15 øre for 1 red item.

Page 7

As mentioned earlier, you and the other person will in each period receive 20 blue items. Your task is to decide how many of your 20 blue items you will use in order to produce red items and how many blue you want to keep for yourself. Correspondingly, the other person will decide how many of her items to use in order to produce red items, or keep for herself.

The number of red items produced is decided as follows:

Number of red items = (The number of blue you use + The number of blue the other uses) x 0,7

Page 8

If, for instance, you use 20 blue and the other uses 20 blue, you and the other person will receive 40 x 0,7 = 28 red items each. If you use 10 blue, and the other uses 10 blue, both will receive 20 x 0,7 = 14 red items each.

Your total income in each period is the sum of your stock of blue and red items:
Total income = Income from the stock of blue (= 20 – your choice of blue for production of red items) + Income from the stock of red (= 0,7 x sum of blue items).

We ask you to answer the following 3 questions. This will help you to understand how your income depends upon your stock of red and blue items.

Page 9
Question 1

You and the other person have 20 blue items each. Assume that both use 0 of their 20 blue items in order to produce red.

a) How large is your stock of blue items?

b) How large is your stock of red items?

c) How large is the stock of blue items for the other person?

d) How large is the stock of red items for the other person?

Page 10

Question 2

You and the other person have 20 blue items each. Assume that both use 20 of their 20 blue items in order to produce red.

a) How large is your stock of blue items?

b) How large is your stock of red items?

c) How large is the stock of blue items for the other person?

d) How large is the stock of red items for the other person?

Page 11

Question 3

You and the other person have 20 blue items each. The other person uses 20 of his 20 blue items in order to produce red. How many red items do you have if you use:

a) 0 blue in the production of red – in addition to the 20 blue the other person uses?
b) 10 blue in the production of red – in addition to the 20 blue the other person uses?
a) 20 blue in the production of red – in addition to the 20 blue the other person uses?

Page 12

You have answered all questions correctly and this is the end of the instructions. Please raise your hand if you have any questions. If you are ready to continue the experiment, press the Ready-button. The experiment starts when everyone has pressed the Ready-button.

Appendix B: Invitation e-mail

You are invited to participate in an experiment. You will receive a show up payment of 100 kroner. In addition, you may earn additional money, which will be paid out in the end.
The experiment involves making decisions on a PC-screen, and no prior knowledge is required. All information gathered will be anonymous.

The experiment will last for approximately 40 minutes and will be held in room 305 and 315 at Ulrike Pihls Hus (Professor Keysers gate 1)

The dates you may sign up for are the following:
- Tuesday 06.05.2014
- Wednesday 07.05.2014

Go to registration by clicking on the link below and register for a session with free capacity.

Tuesday 06.05:
http://thomas.nhh.no/dj/expmotor/new_participant/61/

Tuesday 07.05:
http://thomas.nhh.no/dj/expmotor/new_participant/63/

Best regards, Eirik André Strømland

Appendix C: Instructions read aloud to the participants

Welcome to the experiment. We thank you for showing up. The experiment will last for approximately 40 minutes from the moment I have started the PCs.

The rules for the experiment are as follows:

You are not permitted to talk during the experiment. You are neither permitted to close the program I have opened on the computers, or to open other programs.

If you have any questions before I start the computers, I ask you to ask them now. If you are stuck during the experiment, I ask you to raise your hand. I will then come to you.

[Questions and answers]

I will now start the PCs, and I ask you to be quiet during the experiment.

Appendix D: Robustness checks

Table 4 - Robustness checks on individual contributions

	RE GLS	OLS	RE Tobit
Period	-0.0101 (0.0200)	-0.00799 (0.0191)	-0.0130 (0.0420)
Prev.partnercontr.	0.0423* (0.0249)	0.0902*** (0.0294)	0.0733** (0.0326)
Kept partner Prev.partnercontr.*	0.174*** (0.0344)	0.313*** (0.0574)	0.446*** (0.0638)
Kept partner	-1.075*** (0.372)	-1.430** (0.634)	-3.072*** (0.902)
Choice	3.273** (1.299)	3.088** (1.108)	8.578*** (2.760)
Chat	2.541** (0.998)	2.508** (0.910)	4.817* (2.780)
*Choice*Chat*	-3.172* (1.783)	-3.164* (1.495)	-7.412** (3.746)
*Period*Choice*	-0.0668*** (0.0255)	-0.102*** (0.0219)	-0.107** (0.0490)
*Period*Chat*	0.180*** (0.0227)	0.157*** (0.0213)	0.429*** (0.0485)
Control variables			
Period 29	-1.270*** (0.488)	-1.037* (0.542)	-2.977** (1.172)
Period 30	-1.342** (0.628)	-0.954 (0.662)	-2.671** (1.203)
Female	0.716 (0.667)	0.759 (0.649)	1.262 (1.907)
Participated before	-0.143 (0.790)	-0.146 (0.729)	0.278 (1.965)
Constant	7.958*** (0.932)	7.375*** (0.959)	5.153** (2.291)
*N * T*	3712	3712	3712

Cluster-robust standard errors in parentheses (In RE Tobit ordinary standard errors are reported)
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix E: The Matching Algorithm

```
program{
                    table = subjects;
                    do{
                              PartnerChoice = find (Subject ==: Choice, Choice);

                              if ( Choice == Subject) {
                              Match = 0;
                              }

                              elsif ( PartnerChoice == Subject ) {
                              ProdGroup = ( (Subject + Choice)/2 - ( abs (Subject - Choice)/2 ) );
                              Match = 1;
                              }

                              else {
                              Match = 0;
                              }
                    }
          }
          program{
                    table = subjects;
                    do{
                              if ( Match == 0) {
                              r = random ();
                              }
                    }
          }
          program{
                    table = subjects;
                    do{
                              if (r > 0) {
                              RandomOrder = count (r >: r) + 1;
                              }
                    }
          }
          program{
                    table = subjects;
                    do{
                              if (Match == 0) {
                              ProdGroup = rounddown (( RandomOrder -1)/numInProd, 1) + 100;
                              }
                    }
          }
          program{
                    table = subjects;
                    do{
                              Partner = find ( same ( ProdGroup ) & not (same ( Subject) ), Subject);
                    }
          }
}
```