Cooperation, coordination and competition

Theory and experiments

He, S.

This dissertation explores how cooperation, coordination and competition are influenced by communication and population structure. The studies in this dissertation combine game theoretic modeling with laboratory experiments. The models' predictions provide a benchmark for the laboratory experiments; the laboratory experiments in turn test the theoretical predictions.

More specifically, the first study discussed in this dissertation finds that type detection and commitment value are the most important drivers of the effect of face-to-face communication on cooperation in social dilemmas. The second study suggests that sequential communication is very effective in the battle of the sexes game but ineffective in chicken games. The third study shows that being a member of the minority group can be beneficial in competitive environments but disadvantageous in cooperative environments.

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Contents

Chapter 1

Introduction

Cooperation, coordination and competition pervade our life. Understanding these topics is among the biggest challenges in our society. This thesis aims to contribute to a better understanding of these topics by asking how are they influenced by factors including communication and population structure. The first two chapters investigate when and why communication improves cooperation and coordination. This is motivated by the evidence of the huge power of communication in social dilemma games and coordination games. This can help understand why and when communication improves the outcomes in daily life, businesses and politics. The last chapter studies the role of being in the minority or majority group in a cooperative or competitive environment. This research is motivated by the observation that minority groups enjoy a benefit in competitive environments but suffer in cooperative environments. Understanding the sources of such majority-minority inequality can help design better policies to reduce it.

All three chapters employ laboratory experiments to answer questions that are somewhat difficult to address by using observational data. Laboratory experiments allow for a high degree of control, which is useful for both testing behavioral hypotheses and theoretical predictions. In the first chapter, laboratory experiments are used to understand why communication reliably boosts cooperation in social dilemmas (Sally 1995; Balliet 2009). In this chapter, we disentangle the relative importance of different mechanisms that contribute to the communication gap by manipulating the degree to which people can communicate. The other two chapters combine game theory and laboratory experimentation. These two methods go hand in hand: the theorys predictions provide a benchmark for the experimental parameterizations; the experiment shows which equilibrium can be expected.

The three chapters are independent studies. They aim to answer the following questions: (1) Why does face-to-face communication induce cooperation drastically in social dilemmas? (2) When and why does sequential communication improve coordination in mixed-motive games? (3) Do members of the minority group enjoy an advantage in competitive environments and bear a cost in cooperative environments?

Chapter 2 addresses the first research question. It is well known that face-to-face communication drastically increases cooperation in social dilemmas. We test which factors are the most important drivers of this communication gap. We distinguish three main categories. First, communication may decrease social distance by making the other person identifiable (e.g., Bohnet and Frey 1999; Hoffman et al. 1996). Second, communication may enable subjects to assess their opponent's cooperativeness ("type detection") and condition their own action on that information (Fischbacher et al. 2001; Eckel and Petrie 2011). Third, communication allows subjects to make promises, which create commitment for subjects who do not want to break a promise (e.g., Ellingsen and Johannesson 2004b; Charness and Dufwenberg 2006; Vanberg 2008).

In our experiment, subjects play a one-shot prisoner's dilemma game. In the Baseline treatment, subjects play this game without meeting each other before making their decisions. In the Silent treatment, subjects can identify the other, without the opportunity to communicate, before making their decisions. In the Restricted treatment, subjects are allowed to interact face-to-face before they make their decisions, but without being allowed to make promises. Finally, in the Unrestricted treatment, subjects are allowed to meet face-to-face and communicate freely before making their decisions. The effect of social identification (caused by a change in social distance) can be measured by the comparison between Baseline and Silent. The effect of type identification can be measured by the comparison between Silent and Restricted. And the effect of promises can be measured by the comparison between Restricted and Unrestricted. Compared to Bohnet and Frey (1999) and Brosig et al. (2003), our design is novel that it allows subjects to meet both in the Baseline and the Silent treatments; the meeting takes place either after or before they make decisions. This provides a clean identification of the effect of social distance without a potential confound of reputation concerns. The Restricted treatment is also novel compared to previous studies. Our results confirm that communication increases cooperation substantially. Moreover, we find that the possibility to commit with promises is an important factor in explaining the gap, but the largest part of the gap is attributed to type detection. We do not find evidence that social distance plays a role. To the best of our knowledge, this is the first study that decomposes the communication gap and estimates the importance of each of the three factors in a single design. It expands our knowledge of why face-to-face communication works so well in social dilemmas.

Chapter 3 answers the second question. In previous studies, two communication protocols were investigated. In one-way communication, only one of the players could send a message about the intended action to the other player (e.g. Cooper et al. 1989; Charness 2000; Duffy and Feltovich 2002). In two-way communication, players could simultaneously send messages in a limited number of rounds (e.g. Cooper et al. 1989). In this chapter, we study a more natural form of communication, where players talk sequentially and decide themselves for how long they want to talk. While it is well known that one-way and two-way simultaneous communication has a powerful effect in some coordination games, the effect of sequential communication is never studied. The goal of this chapter is to investigate whether sequential communication improves coordination and efficiency in conflicting interests coordination games. In particular, we compare the effect of sequential communication in the battles of the sexes game and in the Chicken game.

The model predicts that when the cost of communication is sufficiently small, communication can perfectly resolve the coordination problem in the battle of the sexes. Yet, communication may not always be efficient, as players may talk too much before they agree on how to coordinate. In Chicken games, we expect that communication fails to improve efficiency unless players are sufficiently averse to lying. We test these predictions by using three variations of the mixed motive games: one battle of the sexes and two variations of Chicken games. Subjects play one of the three games both with and without pre-play sequential communication. Consistent with the theoretical predictions, we find that communication drastically increases coordination rate and efficiency in the battle of the sexes, but not in any of the two Chicken games. In the battle of the sexes, subjects communicate very efficiently: the majority of the pairs agree on the first message. Since the first sender is almost always demanding for their preferred outcome, this leads to a first sender advantage. In the Chicken games, many subjects seem to anticipate that communication is ineffective and do not start the communication at the first place.

Chapter 4 studies the last research question. As there is a growing literature on the sources of the minority disadvantage such as discrimination and stereotype threat (Blau and Kahn 1992; Darity and Mason 1998; Altonji and Blank 1999; Bertrand et al. 2005), we know very little about the strategic formation of minority advantages. This chapter investigates how a minority advantage or disadvantage arises endogenously in different types of social interactions. We also examine how such an advantage or disadvantage is affected by the relative size of the majority and the minority group.

In our theoretical model, there is an exogenously determined minority group and majority group. Individuals in each group decide how to invest between two types of skills: in-group skills and out-group skills. In a competitive environment, in-groups skills are used to compete against in-group members, while out-group skills are used to compete against out-group members. The model shows that in such a competitive setting, individuals tend to invest more in competing against members of the majority group, which leads to a minority advantage. In a cooperative environment, in-groups skills are used to cooperate with in-group members, while out-group skills are used to cooperate with out-group members. The model shows that in such a cooperative setting, individuals tend to invest more in cooperating with members of the majority group, which yields a minority disadvantage.

These theoretical predictions are tested in a laboratory experiment. Subjects are either in a competitive environment or in a cooperative environment; they are assigned to either a majority or a minority group, and the relative size of the minority group is either small or large. They are endowed a budget to allocate between two skills and are then randomly paired within a matching group. The experimental results confirm the predicted differences in the payoffs between groups: a minority group member earns less than a majority group member in the cooperative treatments, but the opposite is found in the competitive treatments. Moreover, the payoff difference is larger when the ratio of the group size increases. Our results contribute to the understanding of the sources of the majority-minority inequality.

Chapter 2

The Sources of the Communication Gap

2.1 Introduction

Many people highly value face-to-face interactions. A survey among Forbes subscribers showed that the vast majority considers face-to-face meetings to be essential for negotiating contracts, closing deals, and building long-term relationships (Forbes, 2009). Many businessmen are willing to travel large distances to meet clients, thereby incurring considerable costs. They may be right to do so. Evidence from controlled laboratory studies shows that, compared to anonymous interactions, face-to-face communication has a major influence on peoples behavior. Most notably, face-to-face communication drastically increases the cooperation rate in social dilemma games.[1] On average, the option to meet and communicate with one another increases the cooperation rate by 40 percentage points (Sally 1995; Balliet 2009). We will refer to this as the "communication gap."

There are several reasons for why communication may have such a large impact. One possibility is that face-to-face interaction reduces the social distance between people, by making the other person identifiable (e.g., Bohnet and Frey 1999; Hoffman et al. 1996). It may be harder to free ride if it has been clarified who will be harmed. Another reason is that the content of

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[1]Many interactions within and across organizations have the character of a social dilemma. For instance, two employees who are engaged in team production may have an incentive to shirk if individual efforts cannot be monitored. Similarly, different divisions within a firm may to some degree be engaged in a competition with each other, even though this is not in the best interest of the firm as a whole. Or firms may try to find ways to cooperate with other firms instead of competing with them (e.g. on prices), but nevertheless have incentives to undercut their rivals. It is therefore of interest to managers to know how they can encourage cooperation in such situations.

communication can induce a commitment to cooperate. In particular, making a promise can create a commitment value to cooperate for people who are averse to deceiving others (e.g., Ellingsen and Johannesson 2004b; Charness and Dufwenberg 2006; Vanberg 2008).

The existing literature has mostly paid attention to social identification and commitment as explanations for the communication gap (e.g., Kerr and Kaufman-Gilliland 1994). There is, however, an alternative explanation, which may be equally, if not more, important: type detection. Face-to-face interactions can increase cooperation because it enables people to distinguish cooperative people from non-cooperative people. People often pay attention to the appearance and body language of someone else, looking for verbal and nonverbal cues that may reveal the intentions of the other person (Eckel and Petrie 2011).[2] Detecting others types is especially important for conditional co-operators. They are willing to cooperate provided that they perceive the other to be cooperative as well. Interacting with an anonymous person may make them reluctant to cooperate. Since a very large fraction of people are conditionally cooperative (Fischbacher et al. 2001), it is plausible that type detection is responsible for a substantial part of the communication gap.

The contribution of this chapter is to decompose the communication gap, estimating the importance of each of the above three factors in a single design; social identification, type detection, and commitment value.[3] We set up a laboratory experiment in which we let subjects play a social dilemma game. In the baseline treatment, subjects play this game without meeting each other before they make their decisions. In a second treatment, Silent, subjects can identify the other before making their decision, but they are not allowed to communicate. A novelty of our experiment is to implement the Restricted Communication treatment, in which subjects are given time to interact face-to-face before they make their decisions, but without being allowed to make promises. Finally, in the Unrestricted Communication treatment, subjects are given time to freely interact face-to-face before they make their decisions. By comparing Baseline and Silent, we can isolate the effect of social identification.[4] A comparison of Silent and Restricted enables us to measure the effect of type detection. A comparison of Restricted and Unrestricted allows us to estimate the effect of commitment value, as well as any additional type detection

[2]These cues can be very subtle and brief. Ekman (2009) argues that micro expressions often display emotions that people try to conceal, and that spotting such expressions can help to recognize intentions.

[3]There might be other factors that explain the importance of communication in real life. For instance, incurring large travel costs to meet a client may act as a signal of the value that the person attaches to a meeting, and being present forces the other to pay attention to you. De Haan et al. (2015b) provide evidence that messages are more credible when senders choose to incur a cost to communicate them, which is understood by receivers who anticipate that costly communication is more informative.

[4]We adopt the definition of social distance used by Bohnet and Frey (1999). Social distance is reduced if the other person has been identified. Note that there may also be an effect of an anticipated reduction in social distance, if subjects know that they will meet each other after deciding. We cannot rule this out, but, as our empirical analysis shows, the effect of social distance is in any case likely to be very modest.

based on promises.

In line with the existing literature, we find that face-to-face communication drastically increases the cooperation rate. The individual cooperation rate in Baseline is 21 percent, while it is 77 percent in Unrestricted, creating a communication gap of 56 percentage points in our experiment. The cooperation rate in Silent is only slightly higher than in the Baseline, and we therefore do not find that social identification is of any importance in our context. We do find an important role for promises, as the cooperation rate in Restricted is 43 percent, well below the cooperation rate in Unrestricted. Without any restrictions on the communication contents, a majority of subjects makes a promise to cooperate. Most people keep their promise, making it a reliable signal of cooperation. Thus, besides creating a commitment value, promises also facilitate type detection.

Since we also collected participants beliefs about their opponents choice, we can further break down the communication gap. If one is willing to make the assumption that beliefs have a causal impact on behavior, then changes in beliefs can be used to estimate the importance of type detection. This is not an innocuous assumption, as the causality may be reversed or there might be an omitted variables problem. With this caveat in mind, we find that type detection is by far the most important driver of the communication gap in our experiment. [5]

We also find evidence that subjects not only *believe* that they are able to predict their partners' decisions in the communication treatments, but are in fact to some extent able to do so. They use several cues that are correlated with behavior, and ignore some other cues that indeed lack predictive power. Interestingly, when we classify subjects as selfish or social on the basis of an independent test, we find that selfish people cooperate much less than social people in Baseline, but this difference is much smaller or even absent in the other treatments.

Existing work mostly focuses on the different factors in isolation. Studies that analyze the content of communication have found that promises can be a very reliable predictor of behavior (Ellingsen and Johannesson 2004b; Charness and Dufwenberg 2006; Belot et al. 2010; Van den Assem et al. 2012). Possible motives for keeping promises include guilt aversion, lying aversion, and shame (e.g., Gneezy 2005; Charness and Dufwenberg 2006; Vanberg 2008; Miettinen and Suetens 2008; Greenberg et al. 2015). Another related strand of literature studies peoples ability to predict the behavior of others, i.e., peoples ability to detect types. Within the context of social dilemmas, it has been shown that people have some ability to detect cooperators when there is a communication stage (Dawes et al. 1977; Frank et al. 1993b; Brosig 2002; Belot et al. 2012).[6] With the exception of Dawes et al. (1977), those studies do not make a comparison with

[5] Evidence from the existing literature suggests that there is indeed a strong impact of beliefs on actions. We discuss this issue in section 2.3.3.2.

[6] The ability to predict others' behavior above chance levels has now also been established in other games, such as the trust game and bargaining games. See, for instance, Van Leeuwen et al. (2014) for a list of studies.

a treatment without communication, and thus cannot establish the importance of type detection for cooperation rates.[7] The focus of those studies is on peoples actual ability to detect types, while the behavior of people depends more on their own perceived ability to detect types. We examine both subjects actual and perceived ability to detect types.

A few other studies have attempted to disentangle the determinants of the impact of communication. Studies that implemented a treatment in which subjects could only discuss game-irrelevant topics tend to find only a small impact compared to treatments with no communication. For instance, Dawes et al. (1977) find cooperation rates of 35% with restricted communication and 27% with no communication. Communication in the former treatment was restricted to topics unrelated to the game. This does not only exclude making promises, but also other ways of signaling the intention to cooperate, such as discussing fairness norms, and also the intentional exchange of personal information that may be perceived as providing reliable cues, e.g., their study or hobbies. The advantage of our approach is that we only exclude communication that is directly about intentions (see the design section). Frank et al. (1993a) implemented two treatments in which promises were not allowed. The two treatments differed in the length of communication. In this study, it is not clear what counted as a promise and how this was enforced. They compared these two treatments to a treatment with unrestricted communication, in which they explicitly told subjects that they could make a (nonbinding) promise, and found that unrestricted communication enhanced cooperation with 9 and 33 percentage points respectively. They did not have a treatment without communication.[8] Bohnet and Frey (1999) have no treatment with restricted communication, but they do have treatments with anonymity, mutual identification, and unrestricted communication. They attribute any difference between the mutual identification treatment and the anonymity treatment to a decrease in social distance. Brosig et al. (2003) have treatments with anonymity and mutual identification in a four player public game. They find that visual identification alone has no effect on cooperation. In both Bohnet and Frey (1999) and Brosig et al. (2003), the effect of social identification, if there is any, may be partly driven by reputation concerns. In our experimental design, we control for this potential confound.

In addition, even when the communication is about the game, allowing messages to be free-form may be essential. Charness and Dufwenberg (2010) find much less effect of communication when messages are more restricted. In a coordinated resistance game where two responders must jointly challenge the leader to prevent the exploitation of the victim, Cason

[7]In psychology, social dilemmas are often studied with groups of four players or more. This makes the study of type recognition more complicated, as subjects will have to take into account the characteristics of all other group members in forming their beliefs.

[8]Other studies have used a restricted communication treatment that was similar to the one of Dawes et al. (1977). These studies find little impact of irrelevant communication on cooperation (Bouas and Komorita 1996; Mulford et al. 2008; Bicchieri et al. 2010; Potters and Ismayilov 2012).

and Mui (2015) compare a treatment with free-form messages and some treatments with messages containing only intended choices. They find that the possibility of free-form messages is critical for coordinated resistance. It allows people to communicate their social motivations. Brosig et al. (2003) provide further support that rich communication may be a prerequisite for a positive effect on cooperation. In a four player public good game, communication facilitates cooperation only if subjects can visually identify each other while communicating. Similarly, peoples (perceived) ability to predict the intentions of others is not confined to face-to-face interactions. People also read cues in written messages (e.g., Charness and Dufwenberg 2006; Chen and Houser 2013) and different types of communication media yield different opportunities to assess others intentions and consequently result in different cooperation rates (e.g., Brosig et al. 2003). [9]

In addition to the decomposition of the effect of communication, we test some hypotheses that are suggested by the literature. Previous studies that examine the impact of own and opponents characteristics on cooperation in social dilemmas are somewhat mixed. For instance, both Belot et al. (2012) and Van den Assem et al. (2012) find that females are more likely to be cooperative, while Darai and Grätz (2013) do not find a significant gender effect. Belot et al. (2012) do not find evidence that a subjects own attractiveness or the others attractiveness is predictive of cooperation, but Darai and Grätz find that the others attractiveness increases cooperation in mixed gender pairs. We are not aware of any other studies that included risk-attitudes or social as controls. Another question that received attention in the literature is if selfish and social types differ in their ability to judge the person with whom they play the game. Brosig (2002) hypothesizes that conditional cooperators should be better at identifying the others willingness to cooperate, and finds that cooperative individuals are somewhat more accurate in their beliefs. Like these studies, in our analysis we investigate the roles that risk attitudes, gender and attractiveness play in peoples cooperation decision, and we investigate if conditional cooperators are better at predicting the others decision.[10]

The rest of the chapter is organized as follows. We describe the experimental setup in Section 2.2. Section 2.3 reports the results. Finally, Section 2.4 concludes.

[9]Bicchieri and Lev-On (2007) provide a more exhaustive discussion of the effect of the medium of communication on cooperation in social dilemmas.

[10]There is also a stream of literature that studies communication in a variety of other games, including coordination games, cheap talk games, and hold-up problems (e.g., Brandts et al. 2015; Charness and Dufwenberg 2011; Cooper et al. 1992; Ellingsen and Johannesson 2004a, Ellingsen and Johannesson 2004b, amongst others). Communication in those studies is typically in the form of messages.

2.2 Experimental setup

2.2.1 Treatment design

Table 2.1 presents the monetary payoffs of the specific prisoner's dilemma that we use in the experiment. Each of the two subjects makes a choice between X (cooperate) and Y (defect). The game is played only once. The experiment consists of three parts. Subjects receive the instructions of later parts only after finishing a part. In part 1, they are informed that earnings in the remainder of the experiment are completely independent of the decisions in part 1.

Table 2.1: Payoff Matrix

		Other's	decision
		X	Y
Your decision	X	8, 8	0, 12
	Y	12, 0	4, 4

Part 1 consists of a communication phase and a choice making stage. The communication stage is payoff-irrelevant. Our four treatments vary in the extent to which they allow subjects to communicate. In all treatments, subjects at some point meet the person with whom they are playing the game. In the Baseline treatment B, a short silent meeting of 10 seconds takes place *after* the subjects have chosen between X and Y. The other treatments allow subjects to communicate in varying degrees *before* they make a choice. In the Silent treatment S, subjects meet in silence for 10 seconds. In the silent meeting, subjects are not allowed to communicate in any way.[11] In the Restricted treatment R, subjects are allowed to talk face-to-face for two minutes. This communication is free form, except for the following restriction: subjects are not allowed to make a statement that would become a lie for any of the two choices. As an example, we inform subjects that they cannot promise to choose X, because that would become a lie if they would then choose Y instead. We also stress that no statement is allowed that *could* become a lie for any of the two choices, even if they are planning not to lie. In the Unrestricted treatment U, they are allowed to communicate with one another for two minutes without any restriction.

After subjects are informed of the specific game they are going to play, but before they have met the person with whom they are playing the game, subjects are asked to predict the likelihood

[11]The instructions of the Silent treatment were revised once. In early sessions, some subjects communicated by means of nonverbal signs (like making the sign "X" with their hands, to signal their intentions). After observing this, we adjusted the instructions by including an explicit statement that no type of communication was allowed. In sessions with the revised instructions, only 1 pair of subjects violated the instructions. We ran 3 sessions with 30 subjects with the original version of the instructions, and 8 sessions with 68 subjects with the revised version. We excluded subject pairs who used nonverbal signs in the first version from our analysis.

that the other will choose option X, on a scale from 0 to 100 (“beliefs before”).[12] Subjects are again asked to predict the choice of the other after they have met the other person and have made their decision (“beliefs after”). We chose not to incentivize these beliefs, because we felt that subjects would be intrinsically motivated to take the task seriously in such a short experiment. By incentivizing beliefs, we risked that subjects would hedge across the payoffs of the different tasks. For a discussion of the circumstances under which incentivized beliefs are desirable, see Schlag et al. (2015).

We use the subjects’ beliefs to study their ability to detect the type of the other player. In the experiment we asked to predict the other’s behavior instead of the other’s type, because we considered this question to be the clearest. The prediction of others behavior is based on a combined judgment of the other players type and of the other player’s belief. Although it would have been interesting to retrieve information on these concepts separately, we refrained from doing it because we feared that this would become too subtle.

A comparison of the Baseline and Silent treatments allows us to assess the effect of social identification, controlling for potential reputation or image concerns that our subjects may experience. That is, in the Baseline treatment subjects are aware that they will meet the other subject, just like in the Silent treatment. The comparison of the Silent and Restricted treatments enables us to measure a potential additional effect of type detection and the comparison of the Restricted and Unrestricted treatments allows us to judge a further effect of type detection as well as an incremental effect of commitment value. Table 2.2 provides a summary of treatments.

Table 2.2: Summary of treatments

Treatments (Label)	Timing of choice X, Y	Length of meeting	Communication restrictions	Number of subjects
Baseline (B)	Before meeting	10s	Silent	56
Silent (S)	After meeting	10s	Silent	98
Restricted (R)	After meeting	120s	No promises	100
Unrestricted (U)	After meeting	120s	None	80

Notes: In Restricted, any statement that would be a lie for any of the two choices was not allowed. The number in the last column is the total number of participated subjects before excluding subjects from the analysis (see Section 2.2.4).

Part 2 consists of two tasks. First, a version of the social value orientation test (e.g., Offerman et al. 1996) is conducted to acquire a measure of a subjects social preferences. We use the standard decomposed game method as developed by Griesinger and Livingston (1973) and Liebrand (1984). In this method, each subject makes 24 choices. In each of these choices, they choose between two own-other payoff vectors. Each of these payoff vectors assigns a certain

[12]In the Baseline treatment, this question was asked before subjects made their choice between X and Y.

amount of money to the subject and another amount to another subject. The payoff vectors are located at 24 equally spaced points around a circle when mapped in a two-dimensional own-other payoff space. Based on the choices, a subject is classified as aggressive, competitive, individualistic, cooperative, or altruistic, following (e.g. Griesinger and Livingston 1973; Liebrand 1984). Because some categories only contain a few observations, we group all subjects into two groups: *selfish* (aggressive, competitive, and individualistic subjects) and *social* (cooperative and altruistic subjects).

Second, risk attitudes are elicited using a similar task as Eckel and Grossman (2008), adding one option to capture risk-seeking behavior. In this method, a subject makes a choice among six 50-50 gambles that vary in the degree of risk and expected value. Gamble 1 is optimal for risk seeking subjects; gamble 2 for risk neutral subjects, and gambles 3-6 for subjects who are characterized by enhancing levels of risk aversion.

In part 3 of the experiment a questionnaire is administered which includes some questions on background information. Only at the very end of the experiment, the outcome of the prisoners dilemma game is revealed. One of the two tasks of part 2 is randomly selected for payment, and this payment is added to the payment of part 1.

2.2.2 Procedures

The experiment was conducted at the University of Amsterdam between April and May 2013. Dutch subjects were recruited from the CREED database. Subjects communicated in their native language. We ran 2-4 sessions each day and the experiment series lasted 12 days in total. Treatments were randomized at the session level. Each subject participated in one treatment only.

Upon arrival, each subject was directed to one of two separate rooms according to their random online recruitment assignment. Before the experiment started, subjects were informed that part of the experiment would be recorded on video, and that they could participate if they gave their consent or leave otherwise. The experiment only started when the two rooms had the same number of subjects. Each subject was randomly paired with one subject from the other room. Depending on show-up, the number of subjects per session varied between 4 and 12.[13] In each room, an experimenter read aloud the main instructions. The instructions are available as part of the online supplementary materials.

In the communication stage, subjects were called one by one into another room where they met the person with whom they were matched. They did not have the possibility to communicate whilst walking to the meeting room. The subjects were seated by the experimenter, and were

[13]There was one session with 4 subjects, and 5 sessions with 6 subjects. All other sessions were conducted with 8 to 12 subjects.

then left to communicate by themselves. An experimenter was always present in the L-shaped meeting room, but remained out of sight during the communication stage. Afterwards, they returned without any further communication one by one to the rooms where they originally came from. An experimenter was present when subjects returned to their rooms.

At the payment stage, each subject was paid in private in the meeting room. Subjects left the room one by one. The average payoff was €14.60, including a fixed show up fee of €6. The experiment took between 30 and 60 minutes, and was conducted with paper and pencil.

2.2.3 Coding of variables

Four research assistants independently coded the recorded conversations on several dimensions. Appendix 2.C contains the list of the variables that were coded. Appendix 2.B provides the list of variables that were used in the analysis. To code whether or not a subject made a promise to cooperate, we instructed the coders to use the same definition as was used in the instructions for the subjects (any statement that would be a lie for some choice is classified as a promise). Even though our coders tend to agree on their ratings, there are some inconsistencies. In 41 out of 74 cases (55%) all four coders agreed. In another 30 cases (38%), three out of four coders agreed. In 5 cases (7%) the coders were divided, with two out of four coders classifying the subjects' statement as a promise.[14] We classified promises into three categories: no promise (if at most one coder classified the statement as a promise), *weak promise* (if the coders were divided), and *strong promise* (if at least three coders classified the statement as a promise).

2.2.4 Observation and descriptive data

In total, we ran 38 sessions with 334 subjects (about the maximum number of Dutch speaking subjects that could be recruited). Subjects choices are independent on the individual level in the Baseline treatment and on the pair level in the other treatments. Therefore, we allocated relatively fewer subjects to the Baseline treatment to balance the independent observations per treatment.

In the analysis, we exclude subjects who violated the instructions. In the Silent treatment, one pair of subjects talked to each other, and eight other pairs communicated by means of nonverbal signs (all eight cases happened before we revised the instructions, see footnote 10). Another three pairs were excluded because they exchanged their identity cards, thereby creating an external commitment device. Regarding the Restricted treatment, we dropped 13 pairs who self-reported to have made promises in the communication phase. These self-reported results

[14]Cronbach's alpha (a measure of inter-rater reliability) for the variable promise is 0.75, which is usually considered as acceptable.

were consistent with independent coding results, suggesting that no more pairs violated the instructions.[15] Finally, we excluded four more pairs because they reported to be friends.[16]

After excluding these subjects, we are left with 276 subjects. Some characteristics of these subjects are listed in Table 2.3. The Kruskal-Wallis test results show that the randomization procedure was successful on these characteristics.

Table 2.3: Descriptive statistics

Treatment	B	S	R	U	*p*-values
Female, %	50.0	54.1	37.5	43.2	0.204
	(6.74)	(5.83)	(5.75)	(5.80)	
Age (years)	22.3	21.7	21.8	23.1	0.348
	(0.408)	(0.263)	(0.336)	(0.575)	
Attractiveness(1-6)	4.2	4.3	4.0	4.1	0.661
	(0.163)	(0.122)	(0.148)	(0.138)	
Risk-aversion (1-6)	3.5	3.2	3.4	3.0	0.267
	(0.202)	(0.169)	(0.186)	(0.165)	
Social types (%)	26.8	18.9	27.8	31.1	0.386
	(5.97)	(4.58)	(5.32)	(5.42)	
N	56	74	72	74	

Notes: Standard errors in parentheses. In the last column, *p*-values refer to Kruskal-Wallis tests of equality-of-populations between treatments. The number of subjects refers to the number after excluding some subjects (see Section 2.4). Social types according to the social value orientation test.

2.3 Results

In our analysis, cooperation rates and beliefs play an important role. We define the cooperation rate as the fraction of subjects who choose strategy X. We distinguish beliefs according to the moment at which they were elicited: beliefs elicited before the meeting are referred to as 'beliefs before' and beliefs elicited after the meeting as 'beliefs after.'

Table 2.4 lists the cooperation rates and the beliefs in each treatment. Consistent with the existing evidence, adding free format face-to-face communication increases the cooperation rate substantially, in our case from 0.21 to 0.77.[17] To understand the sources of this increase, we

[15]In two instances, one of the four coders coded the conversation as violating the instruction whereas the participants themselves did not report a violation. Dropping those observations does not affect our results.

[16]In the questionnaire, subjects were asked to rate their relation with their partner before the experiment. One of the options was 'I consider the other person to be a friend'. If subjects stated to be friends, they were excluded from the analysis. Their payments were unaffected by the answers in the questionnaire.

[17]Our results remain almost the same if we do not exclude the subjects for the various reasons mentioned in Section 2.4. Then, the cooperation rates are 0.21(B), 0.33(S), 0.40 (R) and 0.79 (U). If we exclude the subjects in

now take a close look at the role of social identification, commitment value, and type detection, respectively.

Table 2.4: Cooperation rates and beliefs

Treatment:	B	S	R	U
Choices				
Cooperation rate (fraction choosing X)	0.21	0.24	0.43	0.77
Test equal to B		(0.812)	(0.020)	(<0.001)
Test equal to S			(0.029)	(<0.001)
Test equal to R				(<0.001)
Coordination rate (fractions)				
Both subjects in pair cooperate (X/X)	0.04	0.08	0.22	0.68
Both subjects in pair defect (Y/Y)	0.61	0.60	0.36	0.14
Expected coordination if choices are independent	0.67	0.64	0.51	0.65
Test actual = expected coordination (p-value)	(0.776)	(0.399)	(0.238)	(0.003)
Beliefs				
Beliefs before (1-100)	37.5	40.4	44.5	51.9
Test equal to B		(0.486)	(0.168)	(<0.001)
Test equal to S			(0.391)	(0.001)
Test equal to R				(0.059)
Beliefs after (1-100)	34.4	37.1	55.0	70.2
Test equal to B		(0.683)	(0.001)	(<0.001)
Test equal to S			(0.002)	(<0.001)
Test equal to R				(0.008)

Notes: (1) Statistical tests report p-values of two-sided Mann-Whitney tests (cooperation rates and beliefs) or Chi-square tests (coordination rates). For cooperation rates and beliefs after, the independent unit of observation is the mean over a pair of subjects.

2.3.1 Social identification

Existing studies do not tease out the effects of social identification from reputation effects. If there is a chance of future interaction (outside the experiment), subjects may behave nicer to the identifiable other to protect their own reputation. We allow our subjects to identify each other in both the Baseline and the Silent treatments, keeping reputation concerns constant. These two treatments only differ in the timing of the meeting - whether it is before or after choosing in the

treatment Silent before we revised the instructions, the cooperation rate in Silent is 0.25.

prisoners dilemma. A difference in behavior between these two treatments can be attributed to social identification.

We do not find any role for social identification. The cooperation rate in the Silent treatment is not statistically different, and only slightly higher, from that in the Baseline treatment. There are also no systematic differences in reported beliefs between these two treatments. It is still possible that social identification plays some role, insofar as subjects in the Baseline treatment correctly anticipate the effects of social identification. Our results show that there is no effect from experiencing social identification compared to any effects of anticipated social identification.

This result differs from Bohnet and Frey's (1999) findings. They have documented a difference between a treatment with mutual identification and a treatment with anonymous interactions in a prisoners dilemma. Their design did not control for reputation effects: subjects in the anonymous treatments never identified each other, even not after making their decisions. Our results suggest that the effect they found might at least partly be due to this reputation effect, and not to social identification.[18] This is consistent with their findings from a dictator game, where they find that solidarity rates increase much more with two-way identification than one-way identification (in which reputation effect are absent).[19] This result is also in line with the findings in the four player public good game of Brosig et al. (2003). Without controlling for reputation, they found no difference in cooperation between a treatment with visual identification and a treatment with anonymity.

2.3.2 Promises

Communication gives people the opportunity to convey their own intentions and to learn about the intentions of others. Amongst all kinds of statements to express intentions, promises are particularly powerful. A distinctive feature of promises is that they relate intentions directly to actions. Unlike other statements, promises may create commitment value to the promise-maker, and the partner may understand the commitment value and become more trusting as a result. For instance, if the promise-maker is lying averse, it will be costly for him to break his promise to cooperate. Of course, promises may also facilitate the process of recognizing types. If others perceive promises as a credible sign of cooperation, even when promises are not credible, they may decide to cooperate as well.

We find clear evidence for the positive effect of promises on the cooperation rate. Table 2.4

[18] Since their study is a classroom experiment, future interactions are likely and reputation effects are therefore plausible. Bohnet and Frey (1999) acknowledge this potential drawback of their design (see their footnote 8). It is also possible that some subjects knew each other, and that their treatment difference is caused by friendships.

[19] Bohnet and Frey (1999) do find a substantial effect of one-way identification on solidarity rates if the dictator also learns some background information about the potential recipient, such as their name and major.

shows that the cooperation rate in the Restricted treatment, when promises are not allowed, is well below the cooperation rate in the Unrestricted treatment. In addition, after communicating, subjects are much more optimistic about the behavior of their partner in the Unrestricted treatment. Interestingly, the positive effect of the possibility to make promises is already anticipated by subjects before communicating. At first glance, the option to make promises seems to account for a substantial part of the communication gap.[20]

In the Unrestricted treatment, subjects eagerly use the possibility to make promises. Only 13 out of 74 subjects make no promises at all. Among the subjects who do make promises, 5 make weak promises and 56 strong promises. If promises create commitment value, one would expect that subjects who make promises are more likely to cooperate. This turns out to be the case. Among subjects who do not make promises, only three (23 percent) cooperate. By contrast, those who make promises cooperate much more often: 4 out of 5 (80 percent) for weak promises and 50 out of 56 (89 percent) for strong promises. Such large effects are consistent with findings of other studies (e.g. Ellingsen and Johannesson 2004b; Charness and Dufwenberg 2006; Belot et al. 2010; Van den Assem et al. 2012). Of course, these differences should not be interpreted as being necessarily causal; we did not exogenously vary whether or not a promise was made, but only the option to make promises.[21]

2.3.3 Commitment and type detection

Identifying or communicating with the other subject can help people to assess the intentions of others. Conditional cooperators may adjust their behavior according to their assessments. To form these assessments, people may rely on cues such as promises and gender, attractiveness, etc.[22] However, since those cues may fail to predict the actual behavior, we make a distinction between perceived and actual cues. If in the communication process conditional cooperators perceive positive cues that the other will cooperate, they may have more confidence in the other and cooperate more. This process may be an important source for the positive effect of communication.

A comparison between Silent and Restricted provides evidence that type detection is im-

[20] Almost all subjects who expressed their intention stated that they would cooperate; only one subject mentioned the intention to defect (according to more than one coder).

[21] An ingenious study by Potters and Ismayilov (2012) suggests the relationship is not causal. In a trust game setting, they exogenously vary whether a written promise is delivered to the matched subject. Even if subjects know that their message is not delivered, they tend to keep their promise. However, in a control treatment with irrelevant communication, they find similar levels of trustworthiness. They conclude that it is not promises per se that affects trustworthiness. Rather, trustworthy people are more likely to make a promise. By contrast, we do find a difference between Restricted and Unrestricted, suggesting at least some causality from promises on behavior.

[22] We measured attractiveness in the post experimental questionnaire. There, we asked the question: "How would you rate the beauty of the person with whom you were matched on a 1-7 scale (1 is ugly; 7 is beautiful)?"

portant. The difference in cooperation is 19 percentage points. This cannot be attributed to commitment or social identification. Type detection can also play a role in other treatments. Because we collected data on beliefs, we can study the effect of type detection in more detail. We present the analysis in two subsections. In Section 2.3.3.1, we check whether or not people change their belief on the basis of how they communicated; and if they do, how much it affects their own decision. We contrast the perceived ability to detect types with the actual ability to detect types, and we also investigate how beliefs map into decisions. In Section 2.3.3.2, we identify which part of the communication gap is due to commitment and which part to type detection. In Section 2.3.3.3 we show which cues subjects use when they change their beliefs, and we compare them to the cues that actually predict when a partner will cooperate.

In the analysis that follows, we assume that the elicited beliefs have a causal impact on behavior. This is not necessarily the case. There can be omitted variables and the causality may be reversed. We come back to this important point at the end of Section 2.3.3.2, where we discuss the potential consequences of this.

2.3.3.1 Changes in beliefs and behavior

Table 2.5 shows that subjects change their beliefs in all treatments. A revision of beliefs after meeting the matched partner suggests that subjects do rely on cues. In the treatments where verbal communication is allowed, the largest changes in beliefs are observed. The average absolute change in beliefs in those treatments is 27 percentage points, compared to 16 and 17 percentage points in the Baseline and Silent treatment, respectively. They are also more likely to change their beliefs with more than 10 percentage points when verbal communication is possible. In the communication treatments, 73% of the subjects change their beliefs with more than 10 percentage points, against 52% in the other treatments. In the final two columns we distinguish between pairs who made a mutual promise and those who did not in the Unrestricted treatment. Although pairs who made a mutual promise on average changed their beliefs to a larger extent, the difference is not significant (Mann-Whitney, p=0.48).

If subjects are able to update their beliefs about each other in the right direction, one expects that the choices of their partners are positively correlated when communication is possible.[23] In the first row of Table 2.6, the correlation between choices is presented for each treatment separately. In the Baseline treatments subjects make their decisions prior to meeting the other,

[23] A priori, this is not clear though. Belot et al. (2010) argue that altruism or efficiency concerns can result in a negative correlation. Since surplus is destroyed when no one cooperates, non-cooperative behavior becomes unattractive if an altruistic or efficiency minded subject expects the other to be non-cooperative. In an earlier working paper version, they analyse the game in a Bayesian framework with incomplete information about the cooperativeness of other people, and show that an increase in a subjects characteristic that makes the subject more likely to cooperate decreases the opponents equilibrium probability of cooperating.

Table 2.5: Updating of beliefs

Treatment:	B	S	R	U	U mutual	U no mutual
Average absolute changes in beliefs	15.5 (2.0)	16.6 (1.9)	27.0 (2.3)	27.4 (2.7)	28.9 (3.18)	20.7 (3.12)
Proportion of subjects with large changes in beliefs (%)	50.0	52.7	75.0	71.6	71.7	71.4

Notes: Change in beliefs is beliefs after - beliefs before. Standard errors in parentheses. A large change in beliefs occurs when a subject changes his or her beliefs by more than 10 percentage points in absolute terms. The column U mutual reports the changes in beliefs for the 30 pairs who made a mutual promise in U. The column U no mutual reports the changes in beliefs for the other 7 pairs.

and indeed their choices are not correlated. In the Silent treatment, the correlation is small and not significantly different from zero. In the Restricted treatment, the correlation is slightly stronger but still not significant. In the Unrestricted treatment, the correlation is sizable and significant.[24]

Table 2.6: Ability to detect types

Treatment:	B	S	R	U
Correlation between choices in pairs (p-value, Pearsons corr. coefficient)	-0.06 (0.657)	0.12 (0.312)	0.15 (0.208)	0.47 (< 0.001)
Accuracy of beliefs after meeting (α) (p-value, Mann-Whitney test)	5.90 (0.738)	1.75 (0.865)	16.09 (0.066)	36.54 (< 0.001)
Accuracy of beliefs after meeting (β) (p-value, Binomial test)	3.50 (0.662)	4.23 (0.537)	10.60 (0.104)	18.65 (< 0.001)

Notes: See the main text for definitions of α and β. All statistical test are based on two-sided tests.

It is possible that subjects are accurate when they change their beliefs, but nevertheless do not adjust their own decisions correspondingly. A selfish person, for instance, might never cooperate independent of his beliefs about the other. As a result, the correlation between choices may be an underestimate of the true ability to assess the intentions of others. We therefore examine if subjects' beliefs after meeting vary systematically with the actual choice of the

[24]Using data from game shows, both Belot et al. (2010) and Van den Assem et al. (2012) do not find evidence of a correlation in decisions, despite a free-format communication stage.

other. Our measure of the accuracy of beliefs is given by:

$$\alpha = \bar{p}^a(c) - \bar{p}^a(d),$$

where $\bar{p}^a(c)$ is the average beliefs about others who cooperate and $\bar{p}^a(d)$ is the average beliefs about others who defect. The measure varies between -100 and 100, where 100 would reflect a perfect ability to predict the other's choice, 0 would reflect random guessing, and -100 would reflect that people are completely wrong in telling the other's choice.

Values of α are presented in Table 2.6. We find evidence that subjects' beliefs have predictive value about the others decision when they can communicate (Restricted and Unrestricted treatments), but not otherwise. In the Baseline and Silent treatments, the values of α are 5.9 and 1.75 respectively, and we cannot reject random guessing (Mann-Whitney test). In the Restricted treatment, equals 16, and is significantly different from zero. Finally, in the Unrestricted treatment, α equals 36, and is again significantly different from zero.[25]

We think that our accuracy measure is very sensible as long as there is a sufficient number of people who cooperate and a sufficient number of people who defect. If for instance there is only one subject in a sample who defects, then the measure will be very sensitive to the report made by the person who was matched with this subject However, this problem did not materialize in our sample. In each of our treatments, at least 12 subjects defect or cooperate.

To investigate the robustness of the result, we include an additional measure of accuracy (β) which is also used by, e.g., Brosig (2002), Dawes et al. (1977) and Frank et al. (1993b). To construct this measure, we classify beliefs as correct if they are above 50$\%$ and the other cooperated, or if they are below 50$\%$ and the other defected. We then compare the fraction of correct beliefs ($\hat{p}$) to a benchmark of random guessing ($\hat{q}$), determined as $\hat{q} = pq + (1-p)(1-q)$, where p is the fraction of times that cooperation is predicted (i.e., beliefs above 50$\%$) and q is the actual fraction of cooperators. The measure of accuracy is then:

$$\beta = 100(\hat{p} - \hat{q}).$$

We use a binomial test to test if β differs significantly from 0. Note that, unlike the other measure α, this alternative measure is sensitive to the fraction of cooperators. In treatments in which the fraction of cooperators differs a lot from 0.5, there is less scope to predict better than the benchmark of random guessing.

The results are shown in Table 2.6. The results are qualitatively similar to the other measure

[25] We find that same gender-match improves accuracy: α equals 55.2 for the same-gender pairs, and equals 20.0 for the mixed-gender pairs. In a similar spirit, Coffman and Niehaus (2015) find that similarity between seller and buyer matters. In their paper, homophily facilitates persuasion by the sellers, who gain substantially more when the buyer has the same gender.

of accuracy. Subjects predictions are not distinguishable from random guessing in treatments Baseline and Silent. The accuracy is somewhat higher in Restricted (with $p = 0.104$). Again, only the Unrestricted treatment provides strong results.

There is another way to examine the accurateness of the reported beliefs. Figure 2.1 plots for each belief after meeting the corresponding cooperation rate of the other. The flat trend in the Baseline and Silent treatments provides further support for the claim that subjects are not able to accurately predict the behavior of the other there. There is some increasing trend in the Restricted treatment and a clear increasing trend in the Unrestricted treatment, illustrating that subjects predict better in these two treatments.

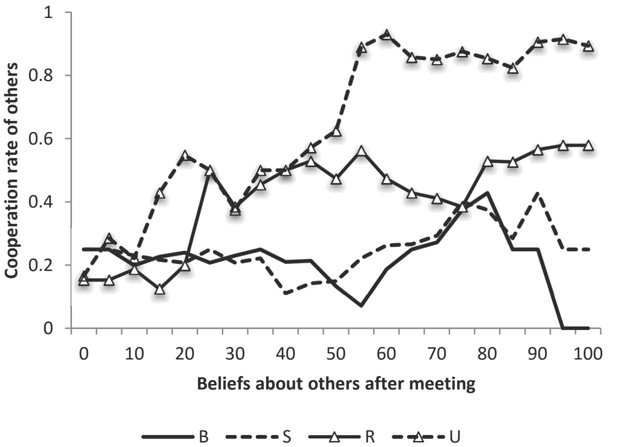

Figure 2.1: Average cooperation rate of others by beliefs after meeting. The plotted lines are moving averages, where for each belief we compute the uniformly weighted average over all beliefs within a distance of 10 percentage points of that belief.

2.3.3.2 Identifying the effects of commitment and type detection

Type detection can explain part of the communication gap if the resulting changes in beliefs affect the propensity to cooperate. To estimate this effect, we construct for each treatment a "reaction function" which maps subjects' beliefs about the likelihood that the paired subject will cooperate into (population average) cooperation rates. Figure 2.2 plots the smoothed reaction functions (see Appendix 2.A for a detailed description of the smoothing). In each treatment, the reaction function is increasing, reflecting that subjects are conditional cooperators, and that they are more inclined to cooperate when they are more optimistic that the other will cooperate. If type detection makes subjects on average more optimistic, this results in higher cooperation levels. Additionally, with a convex reaction function, cooperation increases when type detection results in more dispersed beliefs, even when on average beliefs stay the same.[26]

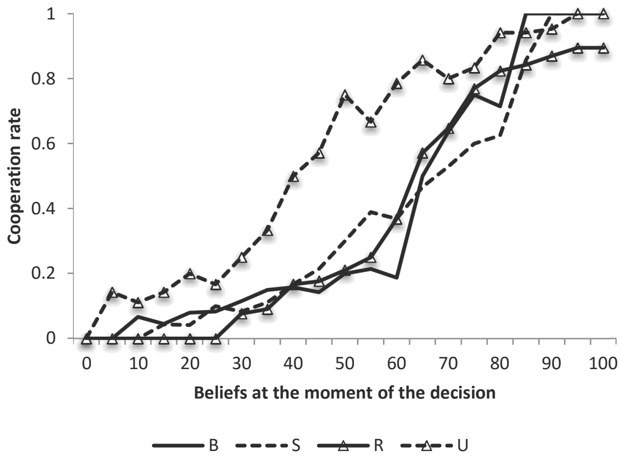

Figure 2.2: Average cooperation rate of subjects as a function of beliefs at the moment of the decision. These are beliefs before meeting in Baseline and beliefs after meeting in the other treatments. B, S, R and U refer to the Baseline, Silent, Restricted and Unrestricted treatments, respectively. The plotted lines are moving averages, where for each belief we compute the uniformly weighted average over all beliefs within a distance of 10 percentage points of that belief.

We use the reaction functions to estimate the effects of commitment value and type detection. If communication creates a commitment value, this would manifest itself as an upward shift *of* the reaction function; given any beliefs about the intentions of the paired subject, subjects become more willing to cooperate. If, on the other hand, people change their beliefs as a consequence of type detection, this would result in changes *along* the reaction function. Figure 2.2 shows that the reaction function of Unrestricted is indeed above that of the treatments in which subjects were not allowed to make promises, suggesting that commitment value plays some role.

We isolate the effect of commitment value by constructing the counterfactual cooperation rate for subjects in Unrestricted had they not been allowed to make promises. We do this by using those subjects beliefs and calculating the cooperation rate using the reaction function of the Restricted treatment, in which there is no commitment value. This gives an estimate of the expected cooperation rate if subjects have beliefs as in Unrestricted but would behave as

[26]Hugh-Jones and Reinstein (2012) also discuss how information about others' types can increase cooperation rates. In their setup, peoples types are revealed by their actions instead of the communication process.

subjects in Restricted. The counterfactual cooperation rate is 62.4, which is 14.6 percentage points below the actual cooperation rate in Unrestricted. Thus, we attribute 14.6 percentage points of the communication gap to a commitment value.

We attribute the remaining part of the communication gap (41.0) to type detection, i.e. changes in beliefs. Note that some of the changes in beliefs are indirectly caused by the commitment value if promises are perceived as a reliable signal of cooperation. We can thus distinguish between (*i*) type detection due to a commitment value, and (*ii*) type detection due to other factors. Since the cooperation rate in Restricted is 43.1 percent, while it would have been 62.4 percent if subjects had beliefs as in Unrestricted, the difference in beliefs between the two treatments yields an estimated difference in cooperation rates of 19.3 percentage points. This means that 19.3 of the 41.0 percentage points can be attributed to type detection as a result of a commitment value, and the remaining 21.7 to type detection as a result of other factors. This is summarized in Table 7. Of course, we do not want to give the suggestion that these exact numbers will generalize to other settings. The main point of these calculations is to illustrate that in our experiment type detection is the most important factor.

Table 2.7: Decomposition of the Communication Gap

Commitment value	14.6 (26.3%)
Type detection	41.0 (73.7%)
due to promises	19.3 (34.7%)
due to other factors	21.7 (39.0%)
Total communication gap	**55.6**

If our decomposition technique is correct, one should expect to find no commitment value among participants that did not make a promise in Unrestricted. This is indeed the case. If we compute the counterfactual cooperation rate for this subsample, we find that it is very close to their actual cooperation rate (23.3 versus 23.1, respectively). It is also comforting to observe that (at least visually), the reaction functions of Baseline, Silent, and Restricted are very similar, consistent with the idea that a commitment value is absent in those treatments. In Appendix A, we report some more robustness tests, and find that our estimates are not sensitive to alternative assumptions.

Before proceeding, we emphasize that our decomposition analysis rests on the presumption that we can give a causal interpretation to the estimated reaction function, where beliefs determine actions. As mentioned before, this is not necessarily the case. There can be omitted variables and the causality may be reversed. According to the consensus effect, people's beliefs are biased towards their own behavior (e.g., Blanco et al. 2014). Additionally, subjects who choose not to cooperate may report pessimistic beliefs to justify their behavior towards the experimenters or even towards themselves. Such behavior would result in an underestimation of the commitment effect: the commitment effect induces people to cooperate, and a justification of behavior would result in more optimistic beliefs, which we would then incorrectly attribute to type detection.[27] We cannot give a precise quantitative estimate of any bias, but some other studies have examined the causal role of beliefs on actions. Blanco et al. (2014) study the consensus effect in a sequential prisoner's dilemma. They find that the best response function based on subjects reported beliefs differs from the best-response function when subjects are given objective probabilities. They attribute the difference to the consensus effect. However, although the best-response function based on objective probabilities is substantially flatter around the sample mean, the average slope is very similar, so that the average bias may be small. Moreover, even with the objective probabilities, there is a strong correlation between beliefs and actions. Within the context of a trust game, Costa-Gomes et al. (2014) use an instrumental variable approach to detect a causal link from beliefs to actions. They find strong evidence of a causal effect, and their results indicate that there is no strong or significant omitted-variable problem. Of course, we cannot directly apply those results to our setting, but two features of our data suggest that also in our setting there is some link between behavior and beliefs. First, the fact that the measure of accuracy (α) is significantly different from zero in some of the treatments supports the idea that beliefs have a causal impact on behavior: we would not find a positive correlation between beliefs and the other's decision if beliefs would only reflect own intended behavior (reverse causality), or if some omitted variables would cause beliefs to differ between subjects. Second, the upward shift in the response function is consistent with the idea that commitment value plays a role in Unrestricted, but not in the other treatments. If subjects were just biasing their beliefs towards their actions, we would not expect to see a shift of the response function, but merely a movement along the response function.

2.3.3.3 Perceived and actual cues

To find out the role of *perceived* cues, we investigate the influence of observable cues on the beliefs that subjects report after the meeting. If subjects think that certain cues signal that their partners are going to cooperate, more optimistic beliefs are expected when such cues are present. We use OLS regressions to determine the effects of some observable cues.[28] The dependent variable is the belief reported after the meeting. Table 2.8 shows the independent variables together with the regression results.

In Table 2.8, the control variables include observable characteristics such as gender, age

[27]We thank an anonymous referee for pointing this out.

[28]We cluster the standard errors at the subject-pair level to account for interdependency within a pair. Estimations based on seemingly unrelated regressions yield similar results.

and attractiveness of the opponents. These features are easily observed when subjects meet. The regression results suggest that our subjects do not rely on any of these characteristics to update their beliefs. Another control variable is the promise that the partner may make, which only takes place in the Unrestricted treatment. Since promises are highly correlated within a pair, we cannot distinguish if a subject's own promise matters or that of the partner. We therefore construct the dummy variable Promise that is 1 if both subjects make a promise and 0 otherwise. We combine weak and strong promises, as their effect is nearly identical. A promise has a large and significant positive effect on the reported belief. We also include the level of risk-aversion and the variable social (based on the social value orientation test) as controls. Those are not directly observed by the subjects, but conceivably people can form an impression about someone's risk-attitude or social value orientation. We do not find that they influence beliefs though. Finally, we include the actual cooperation decision of the partner as a control variable. It has a positive effect on beliefs in the communication treatments, which suggests that our subjects in these treatments use some reliable cues that are not captured by any of the other control variables.

To determine the *actual* cues that influence the choice to cooperate, we use OLS regressions in which the dependent variable is the choice to cooperate. In the regression, the choice equals 0 when the subject defects and 100 when the subject cooperates. The independent variables consist of both own characteristics and the partner's characteristics.[29] We do not include the risk-aversion and social type of the opponent, as in the previous analysis we did not find evidence that these variables affect beliefs. Table 2.9 shows the regression results. Some own observable characteristics affect the decisions. Women tend to cooperate more than men when subjects do not meet before the decision. The gender coefficient in Restricted and Unrestricted is still fairly large, but ceases to have a significant effect. In Silent, the gender coefficient is even negative, though also not significant. We find some surprising results for the variable social. Subjects who are classified as social according to the social value orientation test are more likely to be cooperative, but the effect is much larger in the baseline than in the other treatments. It is also interesting to observe that risk-averse people are much less likely to cooperate than others in the baseline treatment, but the effect disappears in the other treatments. Possibly the communication process diminishes the perception that cooperation is a risky decision.[30]

We again include the variable promise as a joint characteristic (because we cannot distin-

[29]The other's characteristics are not included in the regressions for the Baseline treatment and the combined treatments. In the Baseline treatment, subjects decide before meeting the person with whom they play the game. Therefore, the subjects are not influenced by the others characteristics at the moment of decision.

[30]To assess the possibility of multicollinearity, we calculated the *variance inflation factor*. This factor provides an index that measures how much the variance of an estimated regression coefficient is increased because of collinearity. The regressions in Tables 2.8 and 2.9 have modest variance inflation factors between 1 and 2. These factors are far lower than 10, which is regarded as the threshold for (severe) multicollinearity.

Table 2.8: Beliefs after and cues

	(1)	(2)	(3)	(4)	(5)
Treatment:	All	B	S	R	U
Opponent's characteristics					
Female	1.213	-2.123	10.598	3.425	-3.660
	(4.163)	(8.463)	(6.528)	(10.976)	(6.039)
Attractiveness (1-7)	0.715	1.768	-0.374	0.401	0.456
	(1.632)	(2.944)	(3.559)	(4.000)	(3.036)
Risk-aversion (1-6)	-1.363	-2.698	-2.442	-0.809	1.390
	(1.399)	(3.123)	(3.385)	(3.128)	(1.811)
Social	-0.656	1.386	14.749	-0.376	-5.810
	(4.252)	(10.532)	(9.644)	(6.859)	(5.961)
Cooperates	25.701***	4.108	0.188	17.965*	20.013*
	(4.660)	(13.908)	(10.720)	(9.750)	(11.450)
Promise					34.297***
					(12.462)
Constant	40.822***	36.364**	38.418**	49.066**	24.006
	(8.135)	(17.481)	(15.218)	(18.204)	(18.060)
Observations	262	54	69	66	73
R-squared	0.156	0.046	0.065	0.084	0.433

Notes: OLS estimates. Dependent variable: Beliefs after (0-100). Robust standard errors in parentheses, clustered at the pair level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Social types according to the social value orientation test.

guish the effect of a subject's own promise from the promise of the partner). A promise is a very reliable predictor of cooperation: when promises are made within a pair, subjects are 45 percentage points more likely to cooperate.

Besides the variables included in Tables 2.8 and 2.9, we asked our coders to code the variables listed in Appendix 2.C. Several of those variables (rollcall, threat, trust, risk, justice, and caring) were very infrequently used (in less than 10% of the dialogues) and so we do not have enough observations to reliably identify any effects. For some other variables (first promise, discuss structure of the game) the interrater reliability was questionable or poor (see Table 2.14 in Appendix 2.B for the values of Cronbach's alpha). In Tables 2.15 and 2.16 of Appendix 2.B, we reproduce the regressions of Tables 2.8 and 2.9 when we include the variables for which the interrater reliability was acceptable (Cronbach's alpha of 0.7 or higher). Because we have missing observations for some of those variables, and because the sample size is relatively small, we included the variables one by one.[31] In most cases, the inclusion of those variables does not affect the estimated size and significance of the coefficients of the other variables. One exception is the coefficient of female, which becomes larger in some specifications although it never becomes significant and it is not consistent in sign across specifications. Perhaps the most interesting finding is that shaking hands before talking to one another decreases the beliefs that the matched participant will cooperate in Restricted, while shaking hands after talking has a positive effect on cooperation rates in Unrestricted. These results are consistent with the findings of Darai and Grätz (2013). Only one pair of participants shook hands at the end of the dialogue in treatment Restricted, while this happened relatively frequently in treatment Unrestricted. As a matter of fact, in all cases that they shook hands they also made a promise and they always kept their promise. It thus seems that participants shook hands on their promises, thereby "sealing the deal."

A striking result is that while selfish subjects are much less likely than social subjects to cooperate in the anonymous baseline treatment, selfish subjects catch up with social subjects in terms of the cooperation rate when they meet their partner. This cannot be explained by beliefs, as average beliefs are the same for both types in all treatments. This result is further illustrated in Table 2.10, which reports the correlation between own beliefs and own cooperation rates. Social types tend to behave as conditional cooperators in all treatments, and there is no noticeable trend when the possibilities for communication are enhanced. In contrast, for selfish types the correlation between own beliefs and own behavior is smaller in the Baseline treatment than in the other treatments. Selfish people behave in a more selfish way behind the veil of anonymity, but once the veil is lifted, they start responding to their beliefs like other people. In the Unrestricted treatment, 86% of the selfish and 74% of the social subjects make a weak or strong promise. A promise has remarkable commitment power even for selfish types. They keep their promise in 84% of the cases, while social subjects always keep their promise. One could also imagine that social subjects are more likely to make a promise first, and that selfish subjects follow the example.[32] This is related to the findings of Cason and Mui (2015), who find that potential victims are more likely than potential beneficiaries to start a conversation and to appeal to fairness arguments. We do not find evidence for this in our context, as social and selfish subjects are equally likely to make a promise first (51% of selfish and 47% of social subjects make a promise first), although we emphasize that the interrater reliability of first promise is too low to derive reliable conclusions (Cronbach's alpha 0.45). Furthermore, even in pairs where both subjects are classified as selfish, the percentage of promises is very high (85%).

[31]If three or four of the raters agreed then we coded a variable in accordance with the majority of the raters. If the raters were split, we coded the variable as a missing observation.

[32]We thank a referee for suggesting this.

Finally, we investigate if selfish and social types differ in their ability to judge the person with whom they play the game. Table 2.11 shows that, based on the measure α, selfish subjects predict slightly better than social subjects in the treatments without face-to-face communication. However, this result is reversed in the treatment with unrestricted communication. When face-to-face communication is present and selfish subjects start behaving more as social subjects, social subjects form more accurate beliefs. However, conditional on the choice of the other person, there are no significant differences in the beliefs of the two types at the 5% level. Using the measure β also does not reveal systematic differences between selfish and social types.

2.4 Conclusion

Communication has a profound effect on how people behave in social dilemmas. The main goal of this chapter is to investigate in one setup why communication has such a strong effect on people's behavior in social dilemmas. Previous papers have reported effects of the various factors influencing cooperation in isolation. We find that perceived type detection is the most important factor, while the remainder of the gap can be attributed to a commitment value. We find no evidence for a positive effect of social identification. Promises stand out as a powerful predictor for both the belief that the other will cooperate and the actual decision to cooperate. In fact, the option to make promises not only creates commitment value, but also further facilitates the process of recognizing types. In our study, we find that approximately half of the effect of type detection is due to promises. We expect our results to be robust qualitatively. We would not want to assign too much importance to the precise quantitative effects that we obtain though, as these may vary when the payoffs of the prisoners' dilemma are changed, and they may also change if beliefs are (partly) affected by subjects' own behavior.

What matters for subjects' decision is their *perceived* ability to gauge the type of the subject with whom they are matched, provided that beliefs have a causal impact on behavior. Interestingly, we find that perceived type detection is supported by an ability to actually detect types if subjects are allowed to communicate face-to-face. The evidence in favor of actual type detection is strong when subjects are able to make promises. But even when their talk is restricted and they cannot make promises, there is some degree to which they can predict the behavior of their partner (Table 2.6).

We think that our results are important for understanding managerial processes. Our study not only contributes to understanding why communication works, but it may also help managers to pay attention to cues that actually work (like explicit promises) and to avoid cues that have feeble predictive value.

In this chapter, we assume that reputational concerns are constant across the four treatments.

An alternative possibility is that treatment effects are partly driven by different strengths of reputational concerns. If a person defects after a restricted conversation she may perceive more damage for her reputation than when she defects after visual identification. Similarly, breaking a promise after face-to-face communication may be perceived to damage a person's own reputation more than to defect after restricted talk. Figure 2.2 showed that the "response function" is almost identical in the Baseline, Silent and Restricted treatments. This suggests that the reputational effect does not differ between these treatments. When we compare Restricted and Unrestricted, we do observe a shift in the response function. We expect that the shift is mainly caused by the fact that subjects can make promises in the latter treatment, but we cannot exclude that it is partly due to differential reputational concerns. We leave it to future studies to distinguish between the two.

It is likely that type detection also plays an important role in many other settings, including coordination games, competitions, and bargaining situations. In all these settings, people will assess the intentions and characteristics of others, and condition their own behavior on these assessments. We see it as an important avenue for future research to study the process of type detection in such contexts.

Appendix 2.A Decomposition method

In this appendix we outline in more detail how we decompose the total communication gap into type detection and a commitment value. We also present the results of some robustness checks.

We constructed the reaction functions as follows. We calculated the average cooperation rate for different possible beliefs. The relevant beliefs are those that subjects reported right after they made their decision to cooperate or defect. Since our sample is relatively small, this results in some variability in the reaction function. We therefore smoothen the function by calculating the average cooperation rate of all subjects reporting a belief within a distance of 10 from any particular belief. That is, for any belief $p \in \{0, 1, , 100\}$, we calculate the average cooperation rate of all subjects with beliefs in the interval $[max\{p - 10, 0\}, min\{p + 10, 100\}]$. We do this separately for each treatment (see Figure 2.2 in the main text). We denote the resulting reaction function as $F^j(p)$, where $j \in \{B, S, R, U\}$ is an index of the treatment.

Consider then subject *i* in treatment *j*, and denote the reported belief by this person as $p_i j$. The counterfactual cooperation rate of subjects in the Unrestricted treatment is then calculated as:

$$\frac{1}{N} \sum_i F^R(p_{iU})$$

That is, for every subject in treatment Unrestricted, we calculate the expected cooperation rate using that subject's reported belief and using the reaction function of the *Restricted* treatment. This tells us what cooperation rate could have been expected given the distribution of beliefs of subjects in Unrestricted, in the counterfactual case that they were in the treatment in which they were not allowed to make promises.

The commitment value is then calculated as:

$$\frac{1}{N}\sum_{i} F^{U}(p_{iU}) - \frac{1}{N}\sum_{i} F^{R}(p_{iU})$$

which we estimate to be 77.0 - 62.4 = 14.6.

Note that the counterfactual cooperation rate (62.4) differs from the actual cooperation rate in Restricted (43.1). This is because promises not only create a commitment value, but also facilitate type detection: a promise is a reliable signal of cooperation. This affects the average beliefs of subjects, which are higher in Unrestricted than in Restricted. We attribute the difference between cooperation rates ($62.4 - 43.1 = 19.3$) to type detection due to promises. The remaining difference in cooperation rates between Restricted and Baseline ($43.1 - 21.4 = 21.7$) is attributed to type detection that is not due to promises. Overall, we attribute 41.0 percentage points of the communication gap to type detection, and 14.6 to a commitment value (see column 1 of Table 2.12).

Robustness check 1. Our claim is that the shift in the reaction function from Restricted to Unrestricted is due to a commitment value of making promises. If so, one should expect to find no evidence of a commitment value for subjects who did not make a promise. As a robustness check, we estimated the counterfactual cooperation using the same procedures as described above, but including only subjects who did not make a promise. We indeed find no evidence of a commitment value for that subsample: their actual cooperation rate (23.1 percent) matches the counterfactual cooperation rate if they were not allowed to make promises (23.3 percent). While admittedly this is based on a small sample, it give some extra support for our decomposition method.

Robustness check 2. Instead of using the reaction function based on the Restricted treatment only, we also decompose the total effect using a reaction function that is based on the combination of the Baseline, Silent, and Restricted treatments. Combining the reaction functions potentially eliminates some of the variability that is due to noise. Column 2 of Table 2.12 shows the results. As can be seen, this method yields virtually identical results.

Robustness check 3. As another check, we estimated the reaction function of the Restricted treatment based on a regression analysis. We used a Linear Probability Model, based on the following specification:

$$C_i = \beta_0 + \beta_1 p_i + \beta_2 p_i^2 + \epsilon_i$$

Where C_i is a binary outcome variable (0 or 100) indicating whether or not the subject cooperated. We included a quadratic term, as the reaction function appears to be convex. Table 2.13 shows the result. We then decompose the total effect using the estimated reaction function as the counterfactual. Column 3 of Table 2.12 shows the results. This method yields again virtually identical results.

Robustness check 4. All three methods above use the Unrestricted treatment as the baseline. As a final robustness check, we compute the commitment value using the Restricted treatment as the baseline. In this case, we use the subjects' beliefs of the Restricted treatment and then use the reaction function of the Unrestricted treatment as the counterfactual. Because the distributions of beliefs differ between Restricted and Unrestricted, this can potentially yield different estimates. Column 4 of Table 2.12 shows the results. While this method attributes a somewhat larger part of the communication gap to a commitment value (19.7 percentage points), the estimates are of similar magnitude.

Appendix 2.B Coded variables

Table 2.14 reports the interrater reliability of variables that were coded by our raters. We exclude variables that were very infrequently observed in the dialogues (in less than 10% of the dialogues). Tables 2.15 and 2.16 include these variables as control variables. We constructed dummy variables (coding the variables as 1 if more than 50% of the raters coded the variable as 1, as 0 if less than 50% of the raters coded the variable as 1, and as missing if raters were split equally).

Appendix 2.C Coding of Variables

Four research assistants independently coded the recorded conversations on the variables listed below. Each participant was recorded on one video camera. Each research assistant watched only one of the videos. The variable related to the ethnicity of the participant was therefore only visible to two of the four research assistants. To code promises, we instructed the coders to use the same rule that we used in the instructions for the participants (any statement that would be a lie for one of the choices is classified as a promise).

PROMISE: 1 if participant makes a simple promise, 2 if participant makes a conditional promise

(if you then I also), 3 if participant makes a promise to defect, 0 otherwise
FIRSTPROMISE: 1 if the participant was the first to make a promise, 0 otherwise
ROLLCALL: 1 if they ended with both making a promise, 0 otherwise
THREAT: 1 if participant makes a threat, 2 if the participant makes a positive threat (reference to the future in a positive way), 0 otherwise
TRUST: 1 if the participant says that (s) he trusts the other, 0 otherwise
STRUCTURE: 1 if the participants makes a remark about structure of the game or explains game, 0 otherwise
RISK: 1 if the participants makes a remark about risk, 0 otherwise
JUSTICE: 1 if the participant make some reference to morality, such as "it is the fair thing to do", 0 otherwise
CARING: 1 Stating that they care about the payoff of the other (such as "I like you to make money (as well)", "We both deserve to make money"), 0 No statement of that kind
ASKSTUDY: 1 if the participants inquires about the study of the other, 0 otherwise
SMALLTALK: 1 if the participants engage in other chat (not related to the game), 0 otherwise
NONVERBALAGREEMENTSIGN: 1 if they shake hands (or something else) at the beginning, 2 if they shake hands (or something else) later in the communication, 0 otherwise
LAUGHTER: 1 if they laughs out loud, 0 otherwise
ETHNICMINOR 1 if from ethnic minority (non-white), 0 otherwise
PROBLEM: 1 at least one did not in accordance with instructions, 0 otherwise
EXPLANATIONPROBLEM: if problem, briefly explain it in words

Appendix 2.D Experiment Instructions

The experiment instructions in below are translated from Dutch.

WELCOME PAGE
Thank you for participating in todays experiment. Please read the following instructions carefully. At the start, you will receive €6 for your participation. During the experiment, you will have an opportunity to earn additional money, depending on your own choices and the choices of other participants. You will be paid in private at the end of the experiment. Do not communicate with other participants during the experiment unless we explicitly give permission to do so. We also ask you to not use your mobile phones during the experiment. Always make sure to write your number on every answer sheet. The experiment has three parts. What follows are the instructions for the first part. The instructions for the other parts will be distributed later.

PART 1 INSTRUCTIONS

There is an even number of participants in this experiment. We have divided the participants into two rooms. Each of the participants in a room will be randomly matched to one other participant in the other room. In part 1, the number of rounds will be equal to the number of pairs that participate. In each round one pair of participants will make decisions. The number on your card shows in which round you will make a decision. Each person will make a choice between X and Y. If you and the other person both choose X, each of you will receive 8 euros. If you choose Y and the other person chooses X, then you receive 12 euros and the other person receives nothing. If you choose X and the other person chooses Y, then you receive nothing and the other person receives 12 euros. If you and the other person both choose Y, each of you will receive 4 euros. The possible decisions and earnings are also shown in the following matrix. In each cell of the matrix, the first number shows the amount of money in euro for you, and the second number shows the amount of money in euro for the other participant.

You and the other participant will make your decisions simultaneously, without knowing what the other will decide. Before you make your decision, we ask you to make a prediction about the choice of the other. We ask you how likely you think it is that the other person will choose X on a scale from 0 to 100. If you indicate 0, this means that you are sure that the other person will not choose X. If you indicate 100, this means that you are sure that the other person will choose X. If you indicate 50, it means that you don't know, and that you think it is equally likely that the other person will choose X or Y. You can indicate any number from 0 to 100 in steps of 1 (0 and 100 are admitted).

SILENT,RESTRICTED, UNRESTRICTED. In the round in which it is your turn to make a decision, we will call you and the participant from the other room to the meeting room. This happens after you made your prediction but before you make your decision between X and Y. Once you are in the meeting room, you and the other participant have the opportunity to talk to each other for 2 minutes.

RESTRICTED. The communication is free format. The only restriction is that you are not allowed to make any statement that could become a lie if afterwards you would make a choice that is not in accordance with that statement. For instance, you are not allowed to say I promise to choose X, because that would become a lie if you would then choose Y instead. Note that the criterion is that the statement could become a lie, and even if you plan to act in accordance with your statement, such a statement is not allowed. The communication between you and the other participant will be recorded on video. After these 2 minutes, you will come back to this room to make your choice between X and Y.

UNRESTRICTED. The communication is free format. The communication between you and the other participant will be recorded on video. After these 2 minutes, you will come back to this room to make your choice between X and Y.

SILENT. Once you are in the meeting room, you and the other participant have the opportunity to see each other, but you are not allowed to communicate with each other by any means. This part will be recorded on video.

BASELINE. In the round in which it is your turn to make a decision, we will call you and the participant from the other room to the meeting room. This happens after you made your prediction and after you made your decision between X and Y. Once you are in the meeting room, you and the other participant have the opportunity to see each other, but you are not allowed to speak to each other. This part will be recorded on video. After 10 seconds have passed, you will come back to this room.

You will make your decision between X and Y only once. When all participants have made their decisions and all participants have returned from the meeting room, the second part of the experiment will start in which we will ask you some additional questions. Your earnings in the second part will not depend on any decision made in the first part, and you will not be matched to the same participant. At the end of the experiment, we will reveal the decision of the other participant to you, and pay you in private. Participants will be asked to leave the building one by one. Please raise your hand if you have any questions.

PART 2 INSTRUCTIONS
In this part, you will make some choices for the following questions. At the end of the experiment one of these questions will be randomly selected and your earnings will depend on your choice for that question. Your earning in part 2 will be added to your earnings in part 1 and to your participation fee. After this part there is a final part where you will be asked to fill out a short questionnaire. The payoffs in this part are in points, and every point is worth €0.10.

Question 1 We show 24 items to you on the next page (see Figure 2.3). For each item there are two options (A and B) specifying amounts of money for you and another person. We ask you to indicate which option you prefer. Please indicate your preferred option for each of the 24 items. There is no right or wrong answer, it depends on your personal preferences. Afterwards, we will randomly match you to a person in the other room. This will be another person than the person you met in the meeting room in part 1. This person faces the same decisions as you do. If this question is selected for payment, we will add the payoffs that correspond to the 24 choices made by you and we will do the same for the person with whom you are matched. You will get paid depending on your own choices as well as the choices of the other person.

Question 2 In the table below (see Figure 2.4), we present six different options. You are asked to select one of the options. Your earnings will depend on the outcome of a fair coin toss. Every option shows the amount in points you earn in case a head shows up or a tail shows up. The amount you can earn is always higher when heads shows up, except in option 6 in which you always earn 28 points. If this question will be selected for payment, we will toss a coin and pay you according to the outcome of the toss and the choice you made. Please indicate which one of the six options above you prefer.

Table 2.9: Cooperation and cues

	(1)	(2)	(3)	(4)	(5)
Treatment:	All	B	S	R	U
Own characteristics					
Female	6.195	23.556*	-13.803	10.834	12.995
	(6.998)	(12.275)	(9.878)	(15.864)	(10.987)
Attractiveness (1-7)	0.415	-1.560	-0.889	3.734	-0.016
	(2.751)	(3.685)	(6.221)	(7.846)	(3.878)
Risk-aversion (1-6)	-2.172	-10.942**	4.128	3.224	0.704
	(2.101)	(4.040)	(5.796)	(3.555)	(3.679)
Social	17.598***	40.935***	18.299	-9.021	13.798*
	(6.983)	(13.917)	(16.687)	(12.821)	(7.448)
Opponent's characteristics					
Female			0.199	16.387	-6.176
			(12.881)	(14.459)	(10.166)
Attractiveness (1-7)			-0.268	-5.622	2.035
			(6.288)	(6.048)	(4.198)
Cooperates			-3.909	21.185	24.583
			(19.586)	(19.683)	(21.086)
Joint characteristics					
Promise					44.995***
					(16.056)
Constant	41.169***	43.472**	21.291	22.760	3.485
	(13.782)	(20.283)	(37.796)	(36.348)	(35.918)
Observations	264	56	65	60	72
R-squared	0.028	0.259	0.067	0.124	0.399

OLS estimates. Dependent variable: Cooperates (0 or 100). Robust standard errors in parentheses, clustered at the pair level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Social types according to the social value orientation test.

Table 2.10: Correlation between cooperation rates and own beliefs by type and treatment

	B	S	R	U
Selfish types	0.41	0.62	0.71	0.62
	(0.008)	(<0.001)	(<0.001)	(<0.001)
Social types	0.56	0.67	0.77	0.66
	(0.030)	(0.009)	(<0.001)	(<0.001)

Notes: Spearman rank correlations. p-values testing coefficient equals zero in parentheses. Classification of selfish and social types based on social value orientation test.

Table 2.11: Ability to detect types by own type

		B	S	R	U
Accuracy of beliefs after meeting (α)	Selfish	14.6	2.6	15.3	31.4***
	Social	-18.3	-1.6	21.8	46.9**
Accuracy of beliefs after meeting (β)	Selfish	5.7	5.6	11.3	15.5**
	Social	-4.2	-2.0	5.6	25.7***
Differences in beliefs selfish and social	If other defects	-15.9*	-2.9	-1.5	9.6
	If other cooperates	17.1	1.3	-7.9	-5.9

Notes: The upper and middle panels present accuracy levels α and β by treatment and type. Types classified based on social value orientation test. The lower panel presents differences in beliefs between the selfish and social types, conditional on the choice of the other. In both panels, Mann-Whitney tests are used to test equality of coefficients to zero. *** $p < 0.01$,** $p < 0.05$, * $p < 0.1$.

Table 2.12: Decomposition of the Communication Gap

Method	1	2	3	4
Commitment value	14.6	14.3	16.2	19.7
Type detection	41.0	41.3	39.4	35.9
due to promises	19.3	19.6	17.7	14.3
due to other factors	21.7	21.7	21.7	21.6
Total effect	**55.6**	**55.6**	**55.6**	**55.6**

Table 2.13: Decomposition of the Communication Gap

	(1) Cooperates
Belief	0.196
	(0.363)
Belief^2	0.009**
	(0.004)
Constant	-3.947
	(2.822)
Observations	72
R-squared	0.557

Notes: LPM model. Robust standard errors in parentheses clustered at the pair level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2.14: Interrater reliability of variables

	Cronbach's alpha
Promise	0.752
First to make promise	0.510
Discuss structure	0.607
Ask about study	0.957
Engage in small talk	0.729
Laughter during conversation	0.765
Shake hands	0.882

Table 2.15: Beliefs after and cues

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment:	Restricted				Unrestricted			
Opponent's characteristics:								
Female	-2.345	7.420	7.114	4.488	-2.492	-5.287	0.872	-3.857
	(12.387)	(13.960)	(14.510)	(10.887)	(6.375)	(8.079)	(7.278)	(6.347)
Attractiveness (1-7)	0.209	-1.593	0.043	-0.281	0.163	1.156	1.615	0.398
	(3.772)	(4.918)	(5.105)	(3.924)	(3.038)	(3.446)	(3.794)	(3.216)
Risk-aversion (1-6)	-0.063	-0.703	-1.635	-0.615	1.472	1.944	0.361	1.458
	(3.299)	(3.470)	(3.821)	(3.342)	(1.923)	(2.652)	(2.429)	(1.892)
Social†	1.744	-0.332	2.623	-0.327	-6.922	0.304	-4.815	-5.130
	(7.098)	(6.996)	(7.742)	(6.640)	(6.103)	(5.684)	(7.911)	(6.323)
Cooperates	15.301	19.982*	13.338	16.031	19.769	23.417*	15.607	20.408*
	(9.738)	(11.164)	(11.677)	(9.772)	(12.439)	(11.586)	(14.689)	(11.825)
Meetings								
Handshake (before)	-13.197*				5.172			
	(7.399)				(5.934)			
Handshake (after)					4.565			
					(5.621)			
Engage in small talk		1.078				9.989		
		(8.928)				(6.271)		
Laugh			11.733				-2.897	
			(11.420)				(6.007)	
Discuss study				-0.532				4.875
				(8.376)				(6.616)
Promise					33.735**	32.427**	31.436*	33.146**
					(12.723)	(12.694)	(16.105)	(12.611)
Constant	58.975***	53.625**	43.962*	51.558***	20.307	12.411	28.696	22.955
	(16.078)	(19.970)	(22.204)	(17.972)	(17.428)	(18.886)	(25.476)	(18.846)
Observations	66	58	48	64	73	63	59	72
R-squared	0.111	0.102	0.105	0.070	0.444	0.485	0.355	0.440

Notes: OLS estimates. Dependent variable: Beliefs after (0-100). Robust standard errors in parentheses, clustered at the pair level. *** p<0.01, ** p<0.05, * p<0.1. †Social types according to the social value orientation test.

Table 2.16: Cooperation and cues

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment:	Restricted				Unrestricted			
Own characteristics								
Female	0.326	4.465	-11.977	18.293	12.023	13.584	8.393	13.090
	(15.696)	(16.422)	(20.834)	(17.513)	(10.544)	(14.902)	(9.492)	(11.050)
Attractiveness	3.489	4.115	3.548	2.688	0.587	-1.866	2.511	-0.081
	(7.535)	(8.467)	(8.641)	(8.496)	(4.064)	(4.080)	(4.557)	(3.863)
Risk-aversion	4.497	3.182	8.636	2.521	1.842	1.696	1.664	0.692
	(3.715)	(3.547)	(5.292)	(4.073)	(3.959)	(4.697)	(3.541)	(3.791)
Social	-3.115	-8.500	-4.343	-9.143	13.446*	7.134	10.367	13.124*
	(12.915)	(13.609)	(17.164)	(13.951)	(7.804)	(6.235)	(11.935)	(6.840)
Opponents' characteristics								
Female	7.904	8.570	11.442	20.025	-5.413	-11.071	-3.989	-6.075
	(14.100)	(15.771)	(20.012)	(14.654)	(10.286)	(12.285)	(10.183)	(10.320)
Attractiveness	-6.496	-3.924	-3.179	-7.127	2.363	3.680	5.313	2.216
	(6.214)	(6.418)	(7.140)	(5.927)	(4.175)	(4.689)	(4.493)	(4.265)
Cooperates	19.172	17.692	16.063	16.809	20.232	29.503	28.520	23.657
	(20.277)	(21.522)	(21.315)	(20.124)	(22.771)	(24.155)	(22.026)	(20.310)
Meetings								
Handshake (before)	-22.628				-12.263			
	(15.388)				(8.940)			
Handshake (after)					12.128			
					(8.654)			
Engage in small talk		-16.957				-2.561		
		(15.475)				(8.444)		
Laugh			23.930				3.550	
			(17.358)				(9.520)	
Ask about study				6.388				-8.916
				(16.746)				(8.733)
Promise					43.645**	40.483**	34.126	47.424***
					(16.948)	(19.074)	(20.346)	(15.028)
Constant	44.315	31.574	-9.694	30.809	7.089	7.427	-13.330	4.850
	(40.869)	(38.717)	(35.304)	(41.728)	(36.214)	(38.871)	(42.209)	(35.196)
Observations	60	54	44	58	72	62	58	71
R-squared	0.151	0.133	0.140	0.134	0.420	0.385	0.351	0.407

Notes: OLS estimates. Dependent variable: Beliefs after (0-100). Robust standard errors in parentheses, clustered at the pair level. *** p<0.01, ** p<0.05, * p<0.1.
[†]Social types according to the social value orientation test.

	Option A		Option B		Your choice
Item	Amount Self	Amount Other	Amount Self	Amount Other	
1	+15,00	+0,00	+14,50	-3,90	
2	+14,50	-3,90	+13,00	-7,50	
3	+13,00	-7,50	+10,60	-10,60	
4	+10,60	-10,60	+7,50	-13,00	
5	+7,50	-13,00	+3,90	-14,50	
6	+3,90	-14,50	0,00	-15,00	
7	0,00	-15,00	-3,90	-14,50	
8	-3,90	-14,50	-7,50	-13,00	
9	-7,50	-13,00	-10,60	-10,60	
10	-10,60	-10,60	-13,00	-7,50	
11	-13,00	-7,50	-14,50	-3,90	
12	-14,50	-3,90	-15,00	0,00	
13	-15,00	0,00	-14,50	+3,90	
14	-14,50	+3,90	-13,00	+7,50	
15	-13,00	+7,50	-10,60	+10,60	
16	-10,60	+10,60	-7,50	+13,00	
17	-7,50	+13,00	-3,90	+14,50	
18	-3,90	+14,50	0,00	+15,00	
19	0,00	+15,00	+3,90	+14,50	
20	+3,90	+14,50	+7,50	+13,00	
21	+7,50	+13,00	+10,60	+10,60	
22	+10,60	+10,60	+13,00	+7,50	
23	+13,00	+7,50	+14,50	+3,90	
24	+14,50	+3,90	+15,00	0,00	

Figure 2.3: This figure shows the task for Question 1 in Part 2.

Option	If the coin indicates	You earn in points
1	 Head Tail	**66** **0**
2	 Head Tail	**60** **12**
3	 Head Tail	**52** **16**
4	 Head Tail	**44** **20**
5	 Head Tail	**36** **24**
6	 Head Tail	**28** **28**

Figure 2.4: This figure shows the task for Question 2 in Part 2.

Chapter 3

The Power and Limits of Sequential Communication in Coordination Games

3.1 Introduction

Many situations require some coordination to achieve an efficient outcome. At the level of firms, companies have to agree on technology standards, their marketing and production departments have to collaborate on the design and launch of new products, and division managers have to agree when and where to meet. It has been recognized for a long time that communication can be effective in solving such coordination problems, even without the use of formal contracts (Farrell 1987, 1988).

We examine the effectiveness and limits of communication in a class of two-player mixed-motive games; games in which players can benefit from coordinating, but also have conflicting objectives. We study this theoretically and put the predictions to an experimental test. The main game structure is illustrated below. Without pre-play communication, and for $c < 200$, this game has two pure-strategy Nash equilibria, (H, L) and (L, H), and a unique equilibrium in mixed strategies. Without communication, the mixed-strategy may be focal as players have no way to coordinate on either one of the pure-strategy equilibria (cf. Farrell 1995). With communication, one may expect players to be able to coordinate. Our main interest lies in examining how the effectiveness of communication depends on the level of c.

This chapter is based on He et al. (2016a). Financial support of the Research Priority Area Behavioral Economics of the University of Amsterdam is gratefully acknowledged. We are grateful to Jieqiong Jin who helped running the experiment and coding the communication.

Table 3.1: The game

		Player 2	
		H	L
Player 1	H	0, 0	200, 50
	L	50, 200	c, c

Notes: $0 \leq c < 200$.

Previous theoretical and experimental work has focused on two simple models of communication, (i) one-way communication and (ii) simultaneous two-way communication. With one-way communication, one of the players is allowed to send a non-binding message of his intended action, after which the players play the coordination game. In a regime of simultaneous two-way communication, players have a limited number of rounds in which they simultaneously send non-binding messages before they play the coordination game. Intuitively, one-way communication gives too much power to the player who is allowed to communicate in coordination games with conflicting objectives. It precludes the possibility to express disagreement for the other player, who might otherwise not want to concede. On the other hand, two-way communication allows for the possibility that players communicate the same intended action, after which they still face the same coordination problem. Such coordination failure in the messages seems less likely to occur in the real world where players tend to communicate sequentially.

A distinctive feature of our setup is that players can *sequentially* send messages. The communication can be both about intended actions and about desired outcomes (in our experiment it is even free format), and while players have the opportunity to talk, they are not obliged to send any messages and they decide themselves when to stop talking. To avoid that players talk without limits, we impose a small cost on sending messages.[1]

Theoretically, pre-play communication can be very effective in helping players to coordinate their actions and achieve higher payoffs when c is small. For $c < 50$, both players have incentives to coordinate on one of the pure-strategy equilibria, as they Pareto-dominate the mixed-strategy equilibrium. We identify the equilibria where players divide the pie efficiently, and either the first or the second mover receives the preferred outcome (almost) immediately. We also identify an equilibrium where the players haggle for a long time before they coordinate. In this equilibrium a substantial part of the pie is lost.

The picture looks very different for $c > 50$. In that case, the mixed-strategy equilibrium is not Pareto-dominated. Players prefer the mixed-strategy equilibrium to their least-preferred pure-strategy equilibrium. With standard preferences, communication does not help, as one of

[1]By making communication costly, our environment has some similarities to bargaining models with costly bargaining, such as Rubinstein (1982). A crucial difference is that in those models the division of the surplus is predetermined once an agreement is reached, while in our setup any agreements reached in the communication stage are not binding.

the players will prefer to disagree and play the mixed-strategy equilibrium instead of reaching an agreement on their least preferred pure-strategy equilibrium. We also analyze what happens when players are lying-averse and experience disutility when they deviate form an agreement (e.g., Gneezy 2005). Given the free-form communication, players may agree to play the outcome (L, L). In equilibrium, players will then choose L if and only if their lying cost exceeds a common threshold. The threshold depends on c. The larger c, the more players will conform to the agreement.

These predictions are somewhat in contrast with those of the theory developed in Ellingsen and Östling (2010). They study the effect of communication in both the Battle-of-the-Sexes game and the Chicken game by using a level-k model. They predict that one-way communication will powerfully resolve the coordination problem in such coordination games if players have some depth of thinking, even when $c > 50$, the case where our approach predicts communication to be ineffective unless lying aversion plays a sufficient role.[2]

We test the theoretical predictions in a laboratory experiment. We let subjects play this game, exogenously varying the level of c, using the values 0, 75, and 150. For $c = 0$, the game is Battle-of-the-Sexes. For the other values of c, it is a Chicken game. In addition, we vary whether or not subjects can communicate.

Our main finding is that communication is indeed very effective for $c = 0$, but (at least on aggregrate) becomes ineffective for the higher values of c. For $c = 0$, subjects benefit from the option to communicate, and typically coordinate immediately on the first senders' preferred equilibrium. Subjects almost always make use of the possibility to communicate in this case.

More surprising is that communication is largely ineffective for the higher values of c. Subjects seem to anticipate the ineffectiveness of sending messages, as they frequently forgo the option to communicate. A consequence of all this is that, even if without communication higher values of c always make subjects better off, with communication higher values of c can make subjects worse off by making communication ineffective.

An analysis of the contents of messages tells us that first-senders frequently send a message expressing an intention to play H when $c = 0$, and this happens much less often in the other treatments. This is consistent with our model predictions, at least qualitatively. With higher c, we find that subjects frequently reach an agreement to both play L, in particular when $c = 150$.

[2]The early theoretical literature on the effect of communication in mixed motive coordination games has focused on one-way or two-way communication. Farrell (1987) investigates the effect of two-way communication in a coordination game with conflict, e.g. the Battle-of-the-Sexes game, and finds that a finite-round of two-way simultaneous communication improves the coordination rate, but never fully resolves coordination failure. Later on, Farrell (1988) argues that communication requires credible communication, e.g. communication in the Battle-of-the-Sexes, and that communication may not help in many games. Rabin (1994) shows that communication does not necessarily lead to Pareto-efficient Nash equilibrium, and that with sufficient two-sided communication each player gets a payoff at least as great as that of his worst Pareto-efficient equilibrium. Farrell and Rabin (1996) adds that communication is effective only if the message is self-signaling under the game structure.

In agreement with the equilibrium that allows for lying aversion, we find that subjects play L more often after agreeing on (L, L), but the effect is small and subjects still often choose H. The data support the prediction that more players conform to the agreement when c is larger.

A key feature of our theoretical analysis is that players communicate when communication helps them reach an outcome that is beneficial to both players. Thus, each player must earn more after communicating compared to what she would receive without communication, otherwise they simply refuse to communicate. Another approach states that cheap talk is only effective if two conditions are fulfilled (Aumann 1990, Farrell and Rabin 1996). Communication is credible when the sender's message is both *self-committing* and *self-signaling*. Self-commitment requires that if the message is believed, the sender wants to play in agreement with the message. Self-signaling demands that if the receiver believes the message, the sender only wants so send this message if she plans to play in agreement with it.

The experimental evidence on the empirical bite of the concepts self-signaling and self-committing is mixed. In agreement with these two conditions, subjects communicate successfully when messages are self-committing and self-signaling. For instance, in the Stag-Hunt game, where players' preferences are aligned but they face a problem to coordinate on the risky Pareto dominant equilibrium or on the safe equilibrium with inferior payoffs, communication helps when the message to cooperate is self-signaling (Cooper et al. 1992, Duffy and Feltovich 2002). However, counter to the theoretical predictions, communication tends to remain effective in situations where messages are not self-signaling. In Stag-Hunt games where the message to cooperate is not self-signaling, Charness (2000) finds a strong effect of one-sided messages on subjects' willingness to choose the risky action while Clark et al. (2001) find a lesser but still substantial effect of two-sided communication.[3] Notice that the positive effect of communication in all these studies is in agreement with our less demanding assumption that communication is effective when it helps *both* players to reach a better equilibrium outcome than without communication.[4]

In terms of game structure, the two closest experimental papers are Cooper et al. (1989) and Duffy and Feltovich (2002). Cooper et al. (1989) show that one-way communication in a Battle-of-the-Sexes game is very effective in inducing coordination on the equilibrium preferred by the sender. Compared to the situation without communication, it increases the frequency of

[3]Theoretically, Aumann (1990) argues that the risk of the underlying game negatively influences the effect of communication. Blume (1998) shows that only one-sided communication is sensitive to the risk in the game whereas multi-sided communication is not.

[4]Burton and Sefton (2004) extend the results to 3x3 games and Blume and Ortmann (2007) to larger groups of people and also find cheap talk to be effective. Existing experimental studies on social dilemmas establish the positive effect of costless pre-play communication on cooperation (see e.g. Bicchieri and Lev-On 2007). In the Prisoners' Dilemma, some players may feel guilty to play defect when others play cooperatively. For them, the Prisoners' Dilemma is in essence a Stag-Hunt game, and pre-play communication may help because both players can gain compared to the mixed equilibrium without communication.

equilibrium play from 0.48 to 0.95. Two-sided communication is much less effective though. One round of two-way communication raises equilibrium play to 0.55, and three rounds yields an equilibrium rate of 0.63. Duffy and Feltovich (2002) have treatments that allow them to investigate the effect of one-way pre-coded cheap talk in the Chicken game. They find that cheap talk somewhat increases coordination on the pure-strategy equilibria, but there is no significant effect on efficiency.[5]

In terms of experimental communication protocol, our study has two main contributions. First, we deviate from the previous literature by letting players send messages sequentially and for as long as they see fit. This seems more natural than one-way and two-way simultaneous communication, and has the advantage that people can explicitly express (dis)agreement in response to another player's message. By contrast, having simultaneous messages creates another coordination problem: even if players want to (dis)agree with each other, they can only do so if they correctly anticipate the message of the other player. We think that the possibility to talk sequentially is most relevant in mixed-motive coordination games. In coordination games with aligned preferences (like Stag-Hunt), there is no need to contradict the other player if the other proposed to play cooperatively.

Second, in the studies mentioned above, communication is restricted to messages that specify the intended own action, with the help of pre-coded messages. This excludes the possibility that subjects talk about desired outcomes. In the Chicken game, for instance, players may want to signal a willingness to play (L, L). Pre-coded messages also lack all the subtleties that can signal a player's sincerity or understanding. We therefore allowed players to send free-format messages. In different contexts, it has been found that free-format messages are much more effective in changing behavior than pre-coded messages (Charness and Dufwenberg, 2010, Palfrey et al., 2015). Closest to our work in this respect is Cason and Mui (2015), who find that the possibility of free-form messages is critical for coordinated resistance in a "resistance game."

The remainder of the paper is organized in the following way. Section 2 describes the game and the theory. Section 3 presents the experimental design. Section 4 discusses the experimental results and Section 5 concludes.

[5] In most of their treatments, they also reveal some of the player's history to the opponent. Duffy and Feltovich (2006) extend the analysis by investigating how results change when subjects' messages can contradict previous actions.

3.2 Theoretical Background

3.2.1 Preliminaries

We consider a two-player simultaneous-move normal-form game *G*. Each player chooses some action $A_i \in A = \{H, L\}$, with payoffs $u_i(A_i, A_j)$. The payoffs are given in Table 3.2. We assume that $a > b > 0$ and $a > c$. For $c = 0$, it reduces to a "Battle-of-the-Sexes" game. For $c > 0$, it has the structure of a "Chicken" game. Let S^* be the set of pure-strategy Nash equilibria of the game *G*. Note that the game has two Nash-equilibrium outcomes in pure strategies (H, L) and (L, H), and a mixed strategy Nash-equilibrium in which each player randomizes between H and L, playing H with probability $p = (a-c)/(a+b-c)$ and expected payoff $\mu = ab/(a+b-c)$.

Before choosing their actions, there is a pre-play communication stage *C*. In the experiment, communication is free-format. Here, we assume a more restricted message space, but one that is rich enough to capture the most important messages. Players can send messages $m_i \in M = \{\hat{A}_i, \{\hat{A}_i, \hat{A}_j\}, \text{OK}, \emptyset\}$, where $\hat{A}_i, \hat{A}_j \in \{H, L\}$. A player can thus send a message indicating her own intended action $\hat{A}_i$, or a combination of her own intended action and the expected action of the other player $\{\hat{A}_i, \hat{A}_j\}$. A player can send "OK" to signal agreement with the other player's message. Players send messages in turns, starting with player 1, where it is randomly determined which of the players is player 1. The communication stage ends with one of the player sending $m = \emptyset$. The message $m = \emptyset$ is costless, sending any other message costs $\gamma > 0$ to *each* player.

We refer to game *G* including the communication stage as the extended game $G^*(G, C)$. The communication stage consists of multiple periods. In each period, one of the players can send one message. Strategies in the game $G^*(G, C)$ are messages in the communication stage (possibly mixed and contingent on time and the opponent's messages) followed by probability distributions (possibly degenerate) over elements of A, where the probabilities can depend on the messages sent in the communication stage. Payoffs in G^* are $u_i(A_i, A_j) - \gamma T$, where T is the total number of non-empty messages sent by both players.

Table 3.2: Payoff matrix of Game *G*

		Player 2	
		H	L
Player 1	H	0, 0	a, b
	L	b, a	c, c

Notes: $a > b > 0$ and $a > c \geq 0$.

Some terminology:

Definition 3.1. *A **conversation** is the sequence of messages* $(m_1, m_2, ..., m_{T1})$ *until one of the players terminates the communication stage by sending* $m_{T+1} = \emptyset$ *at time* $T + 1$.

Definition 3.2. *Messages* m_h *and* m_k *are **conflicting** if they stipulate actions* (A_i *or* A_j) *or outcomes* (A_i, A_j) *that are incompatible, i.e.,* $A_{ih} \neq A_{ik}$ *and/or* $A_{jh} \neq A_{jk}$, *where* A_{ik} *is the action for player* i *specified in message* k. *A conversation ends with non-conflicting messages if (i) it only contains a single message, or (ii) it has length* $T \geq 2$ *and messages* m_{T-1} *and* m_T *are non-conflicting.*

Definition 3.3. *An **agreement** is reached when a conversation ends with non-conflicting messages.*

Definition 3.4. *A **credible agreement** is reached when a conversation ends with non-conflicting messages, and the last message(s) specifies actions* (A_i, A_j) *that constitute an equilibrium set of pure strategies of the game G, i.e.,* $(A_i, A_j) \in S^*$.

Note that an agreement is not binding. Notice also that we only allow agreements on pure-strategies or outcome. In principle, players could also agree on randomizing their strategies.

We call an agreement **demanding** if player i proposed the outcome and it is player i's most preferred outcome. This is the case for conversations such as $\{..., \{\hat{H}, \hat{L}\}, \text{OK}\}$. If it is in the players' best interest to live up to the agreement in the decision stage, we call this a **demanding equilibrium**. We call an agreement **conceding** if player i proposed the outcome and it is player i's least preferred outcome. This is the case for conversations such as $\{..., \{\hat{L}, \hat{H}\}\}$ and $\{..., \{\hat{L}\}\}$. If it is in the players' best interest to live up to the agreement, we call this a **conceding equilibrium**. An agreement to play (L, L) is a **compromise**. Finally, we say that an **agreement is immediate** if it is not preceded by any other messages in the conversation.

3.2.2 Equilibrium

In this section we characterize the set of equilibrium strategies if players have standard preferences. In principle, the set of equilibrium strategies can be very large. To narrow down the set of possible equilibria, we assume a natural language interpretation of messages, such that $\hat{A}$ corresponds to the intention to play A, and make several behavioral assumptions.

Our first behavioral assumption is that players will play the mixed-strategy equilibrium if no messages are sent or no agreement is reached. It seems reasonable to assume that they play the mixed-strategy equilibrium if they end the conversation without reaching an agreement, since they have no way of coordinating (cf. Farrell 1987). This assumption is largely supported by the data.

Our second behavioral assumption is that if players reach a credible agreement, they will only act in accordance with the agreement if it gives a higher utility to both players than if they disregard the conversation and play the mixed-strategy equilibrium. Thus, they will ignore the conversation if at least one of the players is better off by doing so than by acting in accordance with the agreement.

We now make the above more precise and summarize the main elements in Assumption 3.1. Suppose that the conversation ended with a credible agreement that specifies $(\tilde{A}_i, \tilde{A}_j)$ as the outcome.[6] Let $\tilde{u}_i^*$ represent player i's expected payoff in the outcome that results from the credible agreement. Recall that μ is the expected payoff from playing the mixed strategy in game G.

Assumption 3.1. *If a conversation ends with a credible agreement, and $\tilde{u}_i^* \geq \mu$ for both players, then each player believes that the other player will act in accordance with the agreement. Otherwise, each player believes the other player will play according to the mixed-strategy equilibrium of the game G.*

Many potential equilibrium strategies are eliminated under this assumption. In particular, it rules out correlated equilibria, in which the random assignment of players to roles is used as a coordination device.[7] Assumption 3.1 also restricts admissable beliefs and thereby the set of equilibria. Under the assumption, players always interpret certain (sequences of) messages as an agreement or disagreement. For instance, when a player sends $\{\hat{H}, \hat{L}\}$ and the other player responds with "OK," an agreement is reached, and players will never interpret this sequence of messages as mere babbling or disagreement.

The following Proposition presents the equilibria in which the players use pure strategies in the communication stage. Let $\gamma_1 = \frac{b(b-c)}{a+b-c}$ and $\gamma_2 = \frac{a(a-c)}{a+b-c}$.

Proposition 3.1. *Under Assumption 3.1, there exist only two possible subgame perfect Nash equilibrium outcomes of conversations in pure strategies (in the communication stage) when $b > c$: (i) immediate concession is an equilibrium conversation outcome if and only if $\gamma \leq \gamma_1$, and it is followed by the Nash-equilibrium outcome (L, H) of game G; (ii) immediate demanding is an equilibrium conversation outcome if and only if $\gamma \leq \gamma_2$, and it is followed by the Nash-equilibrium outcome (H, L) of game G. When $b < c$, communication is ineffective and players refrain from sending costly messages.*

[6]For games such as ours, in which there is a unique best-response to the action of the other player, it is enough for a player to specify an action to reach mutual understanding. When this is not the case, players need to specify the outcome to avoid ambiguity about the intended outcome.

[7]For instance, the first player could terminate the communication stage immediately, and both players could then believe that the first player will choose H and the other player will choose L. Assumption 3.1 rules this out by specifying that the mixed-strategy equilibrium is played after immediately terminating the communication stage (something that is supported by the data).

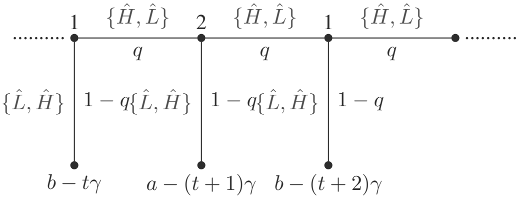

Figure 3.1: Part of a possible communication stage tree when period ≥ 4.

(The proofs of Proposition 3.1 and Proposition 3.2 are in Appendix 3.A.) Note that, if they exist, the above equilibrium messages are both self-committing and self-signaling.

The advantage of the above equilibrium conversations is that they quickly result in agreement. Under some conditions, there also exists an equilibrium in which players potentially take a long while before they reach an agreement. In the communication stage of such an equilibrium, players are indifferent between conceding (and get the low payoff b) and demanding in the hope that the other player will concede (getting the high payoff a but at the cost of sending more messages). In the following proposition, we characterize this equilibrium.

Let $q_1 = \frac{a-b}{a-b+2\gamma}$, $q_2 = \frac{a-b-2\gamma}{a-b+\gamma}$, $q = \frac{a-b-\gamma}{a-b+\gamma}$ and $N = 1 + 2q_1^2 + \frac{q_1^2 q_2}{1-q}$.

Proposition 3.2. ***Mixed strategy equilibrium.*** *Under Assumption 3.1 and for $\gamma \leq \gamma_1$, there exists a mixed strategies equilibrium in C in which players mix between a demanding message and a conceding message in each period. In the first two periods, both players are demanding with probability q_1. In the third period, player 1 is demanding with probability q_2. From the fourth period, each player is demanding with probability q whenever it is her turn to send a message.* [8] *The player that concedes plays L in game G, the other player plays H in game G. The expected length of the conversation is N messages.*

We do not think that players will literally randomize at each instance where they can send a message. Instead, the equilibrium described in Proposition 3.2 may approximate a situation where players at the start of the communication stage decide to be tough negotiators, and play a mixed strategy with regard to the maximum number of periods in which they are willing to send a message L before they concede. They can determine this maximum before they start communicating. If the mixed strategy for the maximum agrees with the randomization process described in Proposition 3.2, an equilibrium results in which players are tough bargainers.

[8] The initial phase of periods 1, 2 and 3 differs from the remainder of the game because player 2 in period 2 has the possibility to concede by simply terminating the communication. In the other periods, to avoid conflicting messages, players concede by sending the costly message $\{\hat{L}, \hat{H}\}$. We assume that player 1 mixes with probability q_1 in the first period, though any probability is supported in equilibrium.

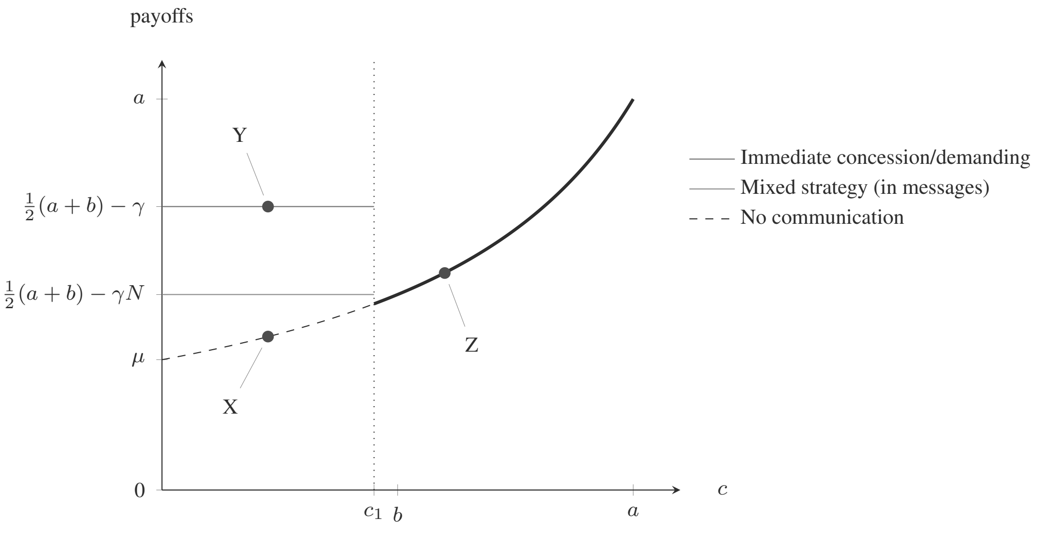

Figure 3.2: Average payoffs with and without communication for different equilibria. Notes: c_1 is the level that makes γ_1 equal γ, that is $\gamma = \frac{b(b-c_1)}{(a+b-c_1)}$. For $c > c_1$ there are no equilibria in which players send messages. The figure is drawn for (a, b, γ)=$(200, 100, 5)$.

The following corollary is an immediate implication of the above propositions.

Corollary 3.1. *For $c > b$, costly communication cannot be supported in equilibrium.*

The reason behind the result in this corollary is that players will anticipate that the player who is worse off after communication prefers to ignore the communication and to play the mixed-strategy equilibrium ($\mu > b$) for any $c > b$.

Given that communication can help for $b > c$ but not otherwise, it is possible that players can be worse off for a higher value of c, because communication becomes ineffective. This is illustrated in Figure 3.2, which shows the equilibrium expected payoffs for different values of c. For relatively low values of c, there exist equilibria in which players communicate, increasing average payoffs compared to a situation where communication is not possible (from X to Y, for instance). For high values of c, no equilibra exist in which players communicate. Even though without communication, payoffs are higher for higher values of c (compare Z to X), communication is ineffective for large c and average payoffs are lower than for lower values of c when players can communicate (compare Z to Y).

3.2.3 Extension: lying aversion

So far we only considered the direct costs of sending messages. Several studies show that there can be psychological costs related to talking. In particular, many people do not break promises because of lying or guilt aversion (e.g., Gneezy 2005, Charness and Dufwenberg 2006, Vanberg 2008, Gneezy et al. 2013). Introducing a cost of lying does not affect any of the previously characterized equilibria, as in those equilibria players act in accordance with the agreement reached.[9] However, one may speculate that costs of lying can make communication more effective, and this turns out to be the case.

In this Section we analyze the effects of lying aversion. We assume that people differ in the lying costs that they suffer whenever they deviate from what was intended in an agreement. In such a case, player i experiences lying cost k_i, which means that an amount of k_i is subtracted from her monetary outcome. Each player knows that players' lying costs are independently drawn from a continuous and strictly increasing cumulative distribution function $F(\cdot)$ that has full support on $[0, \bar{k}]$, and each player i is only privately informed of her own k_i. We refer to this Bayesian game as $G^*(BG, C)$.

We now modify our main assumption on behavior to account for the presence of lying costs and incomplete information. First, we modify the expected utility to $\tilde{u}_i(k_i)$, which is player i's utility including the lying cost of deviating from an agreement. Second, with the presence of incomplete information, we rely on the Bayesian Nash Equilibrium as a solution concept. Let $\tilde{u}_i^*(k_i)$ represent the expected payoff of type k_i of player i in the Bayesian Nash Equilibrium after reaching a credible agreement. Without a credible agreement, each type of each player mixes with the same probability as in the game G with perfect information, and the expected payoff from playing the mixed strategy equilibrium in game BG is still μ.

Assumption 3.2. *If a conversation ends with a credible agreement, and $\tilde{u}_i^*(k_i) \geq \mu$ for each type of each player, then each type of each player believes that the other player will act in accordance with the equilibrium strategies in the Bayesian Nash equilibrium of the associated agreement. Otherwise, each type of each player believes that all the types of the other player will play according to the mixed-strategy equilibrium of the game BG.*

This assumption is in the same spirit as the definition and assumption for the case with complete information. The approach for complete information is a special case of the analysis in this section. We assume that if players reach an agreement to play (L, L), they understand that play will be in agreement with the Bayesian Nash equilibrium of that agreement, which means

[9]Once an agreement is reached, they can still be supported with lying costs as players have no incentives to deviate even without lying costs. The threshold values of γ are also unaffected, as they are determined by possible deviations to other messages in the communication stage, and players have no lying costs from sending any message as long as no agreement exists.

that players with a relatively high cost of lying will conform to the agreement while players with a relatively low cost of lying will deviate. If the fraction of the deviators is sufficiently low, a Bayesian Nash equilibrium exists in which any type of player finds it profitable to agree on (L, L).

In the symmetric Bayesian Nash equilibrium, there exists a threshold k^*, such that players with a cost of lying lower than k^* deviate from the agreement (choose H) and players with a cost of lying higher than k^* conform to the agreement (choose L). Suppose that other players choose H if and only if their cost of lying is smaller than k^*, then player i who has a lying cost k_i is indifferent between H and L when:

$$F(k^*)b + (1 - F(k^*))c - \gamma = F(k^*)(-k_i) + (1 - F(k^*))(a - k_i) - \gamma \tag{3.1}$$

The LHS of equation 3.1 is the expected payoff of playing L after an agreement on (L, L), the RHS of equation 3.1 is the expected payoff of playing H after an agreement on (L, L). $F(k^*)$ is the fraction of players who will choose H after an agreement on (L, L). In a symmetric equilibrium it must be optimal for player i to choose H if and only if her cost of lying is smaller than k^*. This will be the case when player i is indifferent when $k_i = k^*$, which implies

$$F(k^*)b + (1 - F(k^*))c - \gamma = F(k^*)(-k^*) + (1 - F(k^*))(a - k^*) - \gamma \tag{3.2}$$

In this case it is optimal for player i to choose H if $k_i < k^*$ and to choose L otherwise. Solving equation 3.2, we have:

$$F(k^*) = \frac{a - c - k^*}{a + b - c} \tag{3.3}$$

Note that $F(0) < \frac{a-c}{a+b-c}$, $F(\bar{k}) > \frac{a-c-\bar{k}}{a+b-c}$. Given that both functions are continuous, and $F(k^*)$ is strictly monotonically increasing in k^*, $\frac{a-c-k^*}{a+b-c}$ is strictly monotonically decreasing in k^*, there must be a unique solution k^* for any non-negative values of a, b and c.

Another condition that must be fulfilled is that players find it more profitable to agree on (L, L) than to avoid any conversation. Since players with $k_i > k^*$ will not lie, and players with $k_i < k^*$ have lower costs of lying than a player with $k_i = k^*$, the player with $k_i = k^*$ has the highest incentive to deviate. A sufficient condition is therefore that a player with $k_i = k^*$ does not want to deviate:

$$F(k^*)b + (1 - F(k^*))c - \gamma \geq \frac{ab}{a + b - c} \tag{3.4}$$

Depending on the values of a, b, c, γ, and the distribution function of $F(k)$, this condition may or may not be satisfied. When the condition is satisfied, the agreement on (L, L) can be supported in equilibrium. The following proposition summarizes the equilibrium.

Proposition 3.3. *Under Assumption 3.2, an agreement to play (L, L) can be supported as a perfect (Bayesian) equilibrium conversation outcome if and only if (i) $c > b$, and (ii) $\gamma \leq c - \frac{ab}{a+b-c} - (c-b)F(k^*)$, where k^* is the solution to $F(k^*) = \frac{a-c-k^*}{a+b-c}$. After reaching an agreement to play (L, L), players with a cost of lying lower than k^* choose H and players with a cost of lying above k^* choose L.*

What is the effect of enhancing c on the probability that players deviate from the agreement? In equation 3.3, suppose a, b are fixed and $c_1 < c_2$. The corresponding solution for c_1 is k_1^*, such that $F(k_1^*) = \frac{a-c_1-k_1^*}{a+b-c_1}$. It is straightforward that $F(k_1^*) > \frac{a-c_2-k_1^*}{a+b-c_2}$. In order to satisfy equation 3.3 for c_2, we must have either a smaller LHS or a greater RHS, both of which demand $k_2^* < k_1^*$, which will allow $F(k_2^*) = \frac{a-c_1-k_2^*}{a+b-c_2}$. Therefore, the solution k^* decreases in c, and players will deviate less often from the agreement when c increases. This implication is summarized in the following corollary.

Corollary 3.2. *When c increases, k^* decreases.*

3.2.4 Summary

In the result section we will investigate which equilibrium is played by our subjects (if any). In addition, we will consider the following testable insights of the model.

1. When players communicate, they must reach some credible agreement;

2. When players do not reach a credible agreement, or when the credible agreement is worse than the mixed strategy equilibrium for at least one of the players, they play in accordance with the mixed-strategy Nash equilibrium;

3. Without lying costs, there can be no communication in equilibrium for any $c > b$;

4. With (heterogeneous) lying costs, an agreement to play (L, L) can be an equilibrium outcome for $c > b$, and not all players conform to the agreement. When c increases, more players will conform to the agreement;

5. Without communication, payoffs are increasing in c. With communication, players can on average be better off with lower values of c.

3.3 Experimental Design and Procedures

3.3.1 Treatment design

In the experiment, we implemented the payoff matrix of Table 3.2. Table 3.3 summarizes the different treatments. Payoffs were presented in points. We always set $a = 200$ and $b = 50$, and varied the value of c. For $c = 0$, the game reduces to a Battle-of-the-Sexes game (treatment *BoS*). For $c = 75$ (treatment *C-Small*) and $c = 150$ (treatment *C-Large*) it has the structure of a Chicken game. Subjects simultaneously made a choice between H and L.

In the communication condition, the game was played after one of the players ended the conversation. In that condition, subjects could send free-form messages to each other. Each subject in a pair had to pay a cost $\gamma = 2$ for every message that was sent, no matter who sent the message. It was randomly determined which subject in a pair would be sender 1. After that, they alternated. They could only end the communication if it was their own turn to send a message. They did so by clicking on the Leave chat button which ended the communication without affecting the subjects' earnings.[10]

Each subject participated in only one of the treatments. They played the game for 20 rounds: 10 in the condition without communication and 10 in the condition with communication. We changed the communication condition every five rounds, balancing the condition in which they started. This gives a 3x2 design: three treatments (between-subject) and two communication conditions (within-subject).

Subjects were rematched to a different opponent in every round, and were informed that they would never meet the same opponent twice within each communication condition. At the end of each round, each subject received feedback about the decision of the other person and her own payoff.

At the end of the experiment, we administered a short survey, collecting some background information. 4 out of the 20 rounds were then randomly selected for payment. Subjects also received a starting capital of 300 points to cover any possible losses. Every points was worth 0.025.

[10] Before running the main experiment, we ran a few pilot sessions (with 48 subjects) of *BoS*. In those sessions, we had a higher cost per message ($\gamma = 5$ instead of 2) or a lower value of a ($a = 75$ instead of 200). Coordination rates were high and subjects sent very few messages. To make sure that these results were not driven by high message costs or small losses of coordinating on one's least preferred equilibrium, we adjusted the values.

Table 3.3: Overview of Treatments

Treatment	Parameter values			
	a	b	c	γ
BoS	200	50	0	2
C-Small	200	50	75	2
C-Large	200	50	150	2

Notes: a, b, and c correspond to the payoff matrix in Table 3.2. γ is the cost per message (to each sender).

3.3.2 Procedures

The experiment was conducted in the CREED laboratory of the University of Amsterdam. A total of 288 subjects were recruited from the CREED database. We conducted 13 sessions with 12 or 24 subjects each. Treatments were randomized at the session level. Each treatment had 96 subjects. Subjects were divided into matching groups of 12 subjects, so that we have 8 independent matching groups per treatment. 48% of subjects were female, and approximately 68% were majoring in economics or business.

The experiment was computerized using PHP/MySQL and was conducted in English. Subjects were randomly assigned to a cubicle. Instructions were given on their screen. They also received a hardcopy sheet with a summary of the instructions. Subjects could not continue until they correctly answered a set of test questions. The same experimenter was always present during the experiment.

Subjects received their earnings in private. Average earnings were 16.40. A session lasted between 45 and 65 minutes.

3.3.3 Coding of messages

Three research assistants independently coded the messages on several dimensions. Coders were asked to code if a subject expressed an intention to play H or L, making a distinction between strong and weak expressions of intentions. An expression is considered strong if the sender emphasizes that this is definitely what he or she will do. We also asked coders if a pair of subjects reached an explicit agreement on the outcome (L, L) or any of the outcomes (L, H) or (H, L). To classify a conversation as an explicit agreement, we used the criteria that: (*i*) senders were aware of each others intentions, and (*ii*) they showed some approval or confirmation. The exact coding instructions can be found in Appendix 3.B.

Each coder coded all 1009 conversations. At the end, 50 randomly selected conversations

[10] Experimental instructions are provided in Appendix 3.B.

were shown again and recoded, to check each coders individual consistency. The intra-rater consistency is very high. If we combine the weak and strong expressions into a single category, then each rater gives the same assessment in the retest question as in the original question in at least 48 out 50 cases. The inter-rater consistency is also very high. The values of kappa (a measure of inter-rater consistency) is between 0.89 and 0.93 for the different categories, which is commonly regarded as excellent. In our analysis, we classify messages according to the majority of coders. If all coders disagreed with each other, we treat the conversation as missing value. This is the case for 32 out of 1009 cases.

It took coders roughly eight to ten hours of work to complete the task. They worked at their own pace, taking breaks as they saw fit, and were paid a flat amount of 120.

3.4 Experimental Results

3.4.1 Effects of Communication

Without communication, the average proportion of *H*-choices and corresponding payoffs are fairly close to the mixed-strategy equilibrium outcome in all three games. Figure 3.3 shows the actual payoffs (solid line) and the theoretically predicted payoffs if subjects play the mixed-strategy equilibrium (orange dots). As expected, mean payoffs are increasing in the value of c: In *BoS* ($c = 0$) mean payoffs are 54, in *C-Small* ($c = 75$) mean payoffs are 73, and in *C-Large* ($c = 150$) mean payoffs are 111.

Communication is very effective in *BoS* but largely ineffective in *C-Large* and *C-Small*. For the Chicken games, mean payoffs remain the same when subjects have the opportunity to communicate (see the dashed line in Figure 3.3). In *BoS*, the opportunity to communicate increases payoffs considerably, from 54 to 98, an increase of 82 percent ($p < 0.001$, two-sided Mann-Whitney test).[11] This increase is so large that with communication subjects are on average better off in *BoS* than in *C-Small* ($p = 0.003$). Consequently, mean payoffs are no longer monotonically increasing in the value of c when subjects have the option to communicate. These results are in line with the theoretical prediction that communication is effective in *BoS* (where $c < b$) but not in the Chicken games (where $c > b$), at least if lying costs are sufficiently small.

[11] Unless specified otherwise, tests reported are based on taking the matching group as the independent unit of observation.

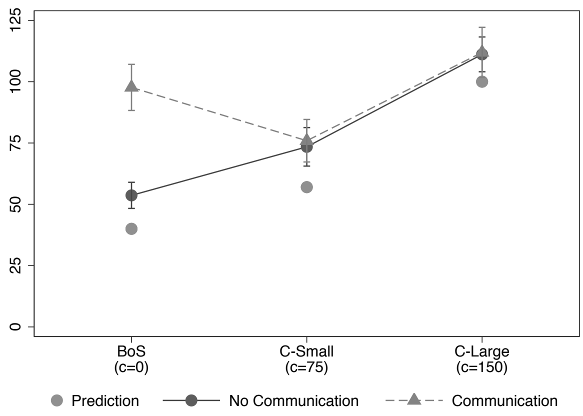

Figure 3.3: Average payoffs by treatment and communication. "Prediction" is the expected payoff if subjects play the mixed-strategy Nash equilibrium of game *G*. Bars are the 95% confidence intervals.

Table 3.4: Percentage of times positive payoffs and efficiency

Treatment	No Communication		Communication		Test Difference
	Payoffs>0	Efficiency	Payoffs>0	Efficiency	(Payoffs>0)
BoS	43	43	80	78	$p<0.001$
C-Small	65	59	67	61	$p=0.673$
C-Large	83	74	84	74	$p=0.711$

Notes: Payoffs>0 indicates the percentage of times that actions led to positive net payoffs (including communication costs). Efficiency is percentage of maximum joint payoffs. p-values based on two-sided Mann-Whitney tests.

Communication increases average payoffs and efficiency in *BoS* because it allows subjects to coordinate their actions on outcomes with positive payoffs, i.e., outcomes (H, L) or (L, H). This is shown in Table 3.4. Without communication, subjects end up with positive payoffs roughly 43 percent of the time. With the option to communicate, they coordinate on outcomes with positive payoffs 80 percent of the time. By contrast, coordination rates on positive outcomes in the Chicken games (i.e., (H, L), (L, H), or (L, L)) are unaffected by the option to communicate. Similar patterns apply to the achieved efficiency (earnings in a pair relative to the maximum joint payoffs). The efficiency is highest in *BoS* with communication, although it is not significantly higher than in *C-Large* ($p = 0.529$).

Result 3.1. *In the aggregate, the option to communicate increases coordination on positive outcomes and has a large impact on average earnings in the Battle-of-the-Sexes game, but is completely ineffective in the Chicken games. Without communication, payoffs are increasing in c. In the presence of the possibility to communicate, increasing the payoffs when both concede can make subjects worse off on average.*

Table 3.5 presents more detailed information about the outcomes and choices. Without communication, subjects' choices respond to c in agreement with the mixed Nash equilibrium. The higher c, the lower the probability that they choose H. Overall, they choose H with a smaller probability than in the mixed-strategy equilibrium though. In the absence of communication, the frequencies of H choices straightforwardly translate to outcomes, because subjects have no means to correlate their choices.

Table 3.5: Distribution of outcomes and choices

	% of outcomes and choices					
	(H, H)	(H, L)	(L, H)	(L, L)	H	Predicted H
No Communication						
BoS	49	22	21	8	70	80
C-Small	35	25	25	15	60	71
C-Large	17	27	26	30	44	50
Communication						
BoS	19	70	10	1	59	–
C-Small	33	40	16	12	61	–
C-Large	16	28	21	35	40	–

Notes: Entries are frequencies of each outcome and choice of H in percentage points. (H, L) [(L, H)] presents the percentage of outcomes that favor first [second] sender. The last column shows the theoretically predicted percentage points of H choice.

A different picture emerges when communication is allowed. Communication diminishes the frequency of H choices in *BoS*. There, the major effect of communication is that it helps subjects coordinate on the outcome that favors the person who can first send a message. Interestingly, even though communication does not affect aggregate payoffs in the Chicken games, it does lead to an increase in coordination on the equilibrium preferred by the first sender at the expense of the second sender, in particular in *C-Small*. Furthermore, when subjects are allowed to communicate there is a slight increase in the relative frequency of (L, L) outcomes in *C-Large*, but there is no such increase in *C-Small*.

Table 3.6 illustrates how payoffs depend on role and treatment. In *BoS*, only first-senders benefit from coordination, as subjects tend to coordinate on (H, L). In fact, first-senders in *BoS* obtain the highest payoffs (143, on average) of all subjects in all roles. Although this is not the preferred equilibrium for second-senders, the decrease in coordination failures ensures that they are not made worse off. First-senders in the Chicken games benefit from communication at the expense of second-senders, although the difference between roles is only significant in *C-Small*. Second-senders in *C-Small* are significantly worse off with communication than without (58 vs 73, $p = 0.003$), because the subjects coordinate somewhat more on the first-sender's preferred equilibrium.

Table 3.6: Average payoffs of first and second sender

Treatment	No Communication	Communication			Test Difference
		All	Sender 1	Sender 2	(sender 1 vs 2)
BoS	54	98	143	53	$p < 0.001$
C-Small	73	76	94	58	$p = 0.002$
C-Large	111	112	117	107	$p = 0.142$

Notes: Average net payoffs (including cost of messages). Reported p-values based on two-sided Mann-Whitney tests.

Result 3.2. *In the Battle-of-the Sexes game, communication favors the first sender. In C-Small, communication increases the payoff of the first sender at the expense of the payoff of the second sender.*

3.4.2 What causes the (in)effectiveness of communication?

What explains the differences in effectiveness of communication between the games? One possibility is that the length of the communication differs. Another possibility is that the content of the communication differs, or that given any content subjects behave differently in the different games. We next review these different possibilities in turn.

3.4.2.1 Conversation length

The option to communicate possibly does not result in higher coordination rates in the Chicken games in part because subjects communicate less than in *BoS*. The mean number of messages is 0.9 in *C-Small* and 1.0 in *C-Large*, against 1.4 in *BoS* (See Table 3.7). More importantly, however, is that subjects in the Chicken games are much less likely to send any messages at all:

44% of pairs in *C-Small* and 38% in *C-Large* do not communicate. In *BoS*, only 8% of pairs do not communicate at all. Subjects seem to understand that communication is rather ineffective in the Chicken games, and therefore avoid sending costly messages. This result is consistent with the theory. More specifically, it is predicted that players always communicate in *BoS*, but that they only talk in the Chicken games if they are averse to lying.

Remarkably, the ineffectiveness of communication in the Chicken games does not appear to be driven by the fact that fewer pairs sent messages. Even among the pairs that do send messages, average earnings are not higher than without communication. This is illustrated in Figure 3.4, that shows average earnings by the length of the conversation. In *BoS*, average earnings are highest when only one message is sent. Sending more messages is associated with lower average earnings, probably because longer conversations are a consequence of disagreement. In the Chicken games, average earnings are close to the average earnings without communication when two or less messages are sent. For three or more messages, average earnings drop, but this happens relatively rarely.

Table 3.7: Distribution of messages

Treatment	Mean	# messages (%)			
		0	1	2	3+
BoS	1.4^b	8	58	23	12
C-Small	0.9^a	44	32	16	7
C-Large	1.0^a	38	27	30	4

Notes: Entries with different superscripts are significantly different at the 5% level (two-sided Mann-Whitney test).

3.4.2.2 Contents of the Messages and Behavior

We next look at the contents of the messages and subsequent behavior in more detail. Based on the results from the coders, we group conversations into four main categories. In the first group are conversations in which sender 1 is conceding (i.e., expresses an intention to play L) while the other player silently or explicitly agrees. In the second group are conversations in which sender 1 is demanding (i.e., expresses an intention to play H) while the other player is silent or explicitly agrees. In the third group are conversations in which both players are demanding, such that there is conflicting messages. In the fourth group are conversations in which sender 1 suggests to play (L, L) and the other player is silent or explicitly agrees.[12]

[12] In a few cases, coders coded that a sender in Chicken indicated to play L. In these cases, we assume that the sender suggested to play (L, L).

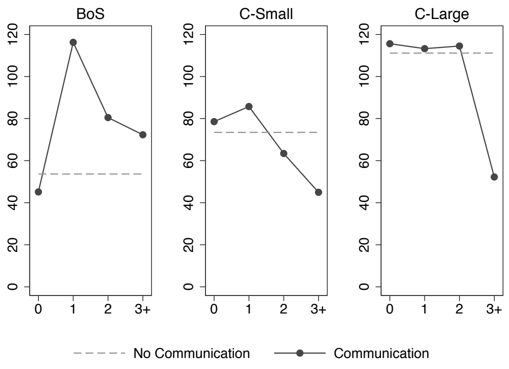

Figure 3.4: Average earnings by number of messages sent. The horizontal (dashed) lines are reference lines showing the average earnings without communication.

Table 3.8 reports the frequency of the different types of conversations, and how often the players choose H. The first thing we note is that subjects almost always reach an agreement when they talk. However, there are some conversations where both Senders are demanding and the conversation ends in disagreement. This occurs somewhat regularly in *BoS* (in 20% of the cases), while it is more rare in *C-Small* (10%) and *C-Large* (4%).

Result 3.3. *When subjects communicate, they reach some agreement in the large majority of cases.*

Table 3.8: Detailed contents of conversations and behavior

Treatment	Condition	% of conversations	% Choosing H:	
			Sender 1	Sender 2
BoS	No Communication		70	70
	Communication			
	No messages	8	78	69
	Sender 1 conceding	3	0	100
	Sender 1 demanding	69	100	7
	Both senders demanding H	20	73	77
	Suggestions to both play L	0	–	–
C-Small	No Communication		60	60
	Communication			
	No messages	44	60	50
	Sender 1 conceding	2	45	100
	Sender 1 demanding	28	98	28
	Both senders demanding	10	79	85
	Suggestions to both play L	14	62	53
	Not classified	2	–	–
C-Large	No Communication		44	44
	Communication			
	No messages	38	39	40
	Sender 1 conceding	2	38	50
	Sender 1 demanding	8	100	39
	Both senders demanding	4	81	81
	Suggestions to both play L	45	36	28
	Not classified	3	–	–

Notes: Without communication, there is no distinction between the two players.

Another regularity in our data is that behavior is fairly similar when no messages are sent as when communication is impossible. The data support our assumption that subjects play the mixed-strategy equilibrium when they immediately end the conversation without talking. When both players are demanding, our assumption that without agreement subjects play the mixed-strategy equilibrium fits the *BoS* observations reasonably well, but not the Chicken observations.

There, subjects choose H with a remarkably higher probability than expected in the mixed equilibrium. A similar deviation from the assumption occurs when only sender 1 is demanding in the Chicken games. In those cases, sender 1 also chooses H with a substantially higher probability than expected in the mixed equilibrium. Second senders take these conversations with a grain of salt. They choose L to a lesser extent than when a similar conversation occurs in *BoS*, but they choose L more frequently than when no conversation takes place.

Result 3.4. *When subjects do not reach an agreement, they tend to play in agreement with the mixed Nash equilibrium. The exceptions are the cases in the Chicken games in which sender 1 or both subjects are demanding. There, the demanding subjects choose H with higher probability.*

There seems to be an understanding that communication favors the first sender. First senders concede only very rarely in all three treatments. Instead, we find that demanding behavior by the first sender is very prevalent in *BoS* (89$\%$ of the time). It is much less often observed in the Chicken games, though (38$\%$ and 12$\%$ of the time). Remember that the theory predicts that we should only observe demanding behavior in *BoS*.

Even when the content of the messages is sometimes the same between games, behavior can be very different. When sender 1 is the only demanding player in *BoS*, there is almost perfect coordination: sender 1 always chooses H and sender 2 almost always chooses L (93$\%$ of the time). In the Chicken games, sender 2 is substantially less likely to play L in such a case (72$\%$ of the time in *C-Small* and 61$\%$ in *C-Large*).

Suggestions to play (L, L) are rare in *C-Small* (14$\%$), but quite common in *C-Large* (45$\%$). Interestingly, when a conversation ends with the suggestion to play (L, L), a large fraction of subjects still choose H, roughly 58$\%$ in *C-Small* and 32$\%$ in *C-Large*, suggesting that subjects differ in their lying costs.[13] The rate to play H in *C-Large* after (L, L) is suggested, 32$\%$, is well below the rate when there is no communication (44$\%$), but still far above zero.

It remains a question whether an agreement on (L, L) is effective, and makes subjects more likely to play L, or only captures a self-selection of subjects who would otherwise also have chosen L at the same rate. To examine this, we make a within-person comparison, comparing the likelihood of choosing H after an agreement on (L, L) with the likelihood of choosing H when they are allowed to communicate but no message is sent. We find a small and insignificant increase of 2 percentage points in the likelihood of choosing H after an agreement on (L, L) in *C-Small* ($p = 0.834$, two-sided Wilcoxon signed rank test), and a significant decrease of 12 percentage points in *C-Large* ($p = 0.035$). This suggests that in *C-Large*, an agreement on

[13] At the pair level, we find that in only 43$\%$ of cases both subjects behave in conformity with the agreement in *C-Large*. If subjects within a pair would independently deviate from the agreement, we would expect that 44$\%$ of pairs ends up choosing (L, L). The actual percentage is 43, suggesting that subjects do not have a way of telling if their opponent will stick to the agreement.

(L, L) does not just reflect subjects' type, but changes behavior to some extent.[14]

Result 3.5. *In the Chicken games, there is some tendency to agree on (L, L). After an agreement on (L, L), subjects tend to choose H to a somewhat lesser extent than without an agreement, but H choices still occur quite regularly.*

Overall, the data clearly support that subjects in *BoS* immediately coordinate on the equilibrium that favors first sender. Subjects almost always use the possibility to communicate. Conversations are very short (much shorter than in the haggling equilibrium) and they only boost the payoff of the first sender. In the next subsection we will investigate if behavior in the Chicken games conforms to the equilibrium described in Proposition 3.3.

Result 3.6. *In BoS, subjects tend to coordinate on the equilibrium in which communication favors first sender.*

We are now in a position to address the question why communication is ineffective in Chicken games. Although in *C-Small* first senders are often demanding, the other player is less likely to concede than in *BoS*. As a consequence, they end up both choosing H at roughly the same rate as without communication. In *C-Large*, players often agree on both choosing L but many players deviate from the agreement. Moreover, players are still sometimes demanding, and the opponent is relatively unlikely to concede. Ultimately, they only manage to increase coordination on (L, L) by a small percentage (5 percent). This only slightly increases payoffs, and it does not outweigh the communication costs by much.

3.4.2.3 Lying aversion

We start with some statistics that illustrate the heterogeneity in lying aversion in our sample. In *C-Small*, among the 67 subjects who at least once make an agreement on (L, L), 29 (43$\%$) always choose H and 23 (34$\%$) always choose L, while the other 15 (22$\%$) mix between choosing H and L. In *C-Large*, among the 93 subjects, 10 (11$\%$) always choose H and 42 (45$\%$) always choose L, while the other 41 (44$\%$) mix. This high level of heterogeneity between subjects agrees with our assumption that people differ in their cost of lying if they deviate from an agreement.

According to Proposition 3.3, subjects will agree on (L, L) if some conditions are met. An agreement on (L, L) will only be honored by those subjects who have a lying cost above a threshold that depends on c.

[14]In our experiment, most agreements are implicit. Behavior is quite similar when explicit agreements are reached (compared to when agreements are implicit). Sometimes our subjects actively avoided the costs of an explicit agreement by saying that if the other agreed, there was no need for another message.

To investigate how well Proposition 3.3 agrees with the data in the Chicken games, we first classify our subjects as being *lying neutral* or *lying averse*. We perform a within-person analysis on how often the subject conforms to an agreement on (L, L). If a subject conforms to the agreement more than half of the time, we classify him as *lying averse*. That is, his cost of lying is above k^*. Similarly, if a subject conforms to the agreement less than half of the time, we classify him as *lying neutral*. As for subjects who conform to the agreement exactly half of the time, we say that their cost of lying is exactly $\tilde{k}^*$ and therefore equally split them to lying averse people and lying neutral people. The proportion of lying neutral people is our estimate of $F(\tilde{k}^*)$. This rate is 0.52 in *C-Small* and 0.25 in *C-Large*.[15] Given the estimated rate of $F(k^*)$, we can then derive $\tilde{k}^*$ by the constraint in Proposition 3.3: The threshold above which subjects would conform is about 34 in *C-Small* and about 25 in *C-Large*. Thus, in agreement with the theory, more subjects are classified as lying-averse when c is higher. The data support Corollary 1.

Next, we check whether the estimated $F(\tilde{k}^*)$ satisfies the conditions in Proposition 3.3. The first condition $c > b$ is satisfied in both Chicken games. The second condition $\gamma \leq c - \frac{ab}{a+b-c} - (c-b)F(k^*)$ requires that the RHS is at least as large as the cost of message 2. The estimated $F(\tilde{k}^*)$ yields a RHS that equals 4.8 and 25, in *C-Small* and *C-Large* respectively. Note that although both values are greater than 2, the condition in Proposition 3.3 is more amply met in *C-Large* than in *C-Small*.

This is reflected in the modest rate of suggestions on (L, L) in *C-Small*, where an agreement of (L, L) is observed in only 14% of the cases. In *C-Large*, we observe agreements on (L, L) in 45% of the cases. This is substantially more than in *C-Small* but also far from the predicted 100%. Possibly, some of our subjects may find the gain in profit not sufficient to compensate the risk of suggesting (L, L). This may be behind the lack of (L, L) agreements, particularly in *C-Small*.

Result 3.7. *In the Chicken games, some subjects ignore the possibility of communication. Others focus on the equilibrium in which they agree on (L, L), while they do not necessarily conform to the agreement. In agreement with this equilibrium, the proportion of lying averse subjects increases with c.*

3.4.3 Learning

Although the game itself is simple, subjects may have to learn how to interpret others' messages and how their own messages are interpreted by others. In this section, we briefly look at learning effects.

[15] Note that in both Chicken games, there are subjects who never made an agreement (34% in *C-Small* and 3% in *C-Large*). As the subjects do not reveal their lying attitude, they are excluded from the estimation of $F(\tilde{k}^*)$.

We do not find much evidence of learning. The length of communication and the effect of communication on earnings change little over time. Figure 3.5 shows the length of communication over time in the different treatments. There is no discernible time trend in any of the treatments. Almost right from the start, the average number of messages is higher in *BoS* than in the Chicken games (left panel), and the percentage of cases where subjects do not send messages is lower in *BoS* than in the Chicken games (right panel). In terms of earnings, we find that in *BoS* there is a stable earnings gap between the communication and no-communication conditions, while communication is ineffective in the Chicken games in all rounds (see Figure 3.6). Although earnings vary somewhat over time, the effectiveness of communication does not.

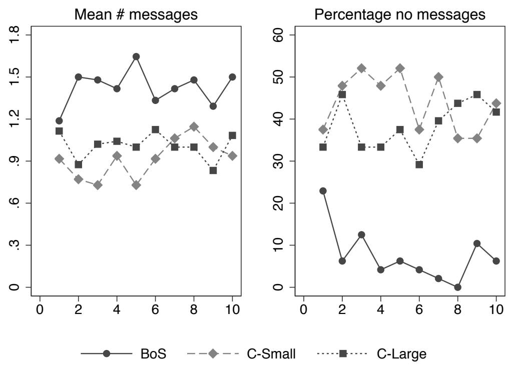

Figure 3.5: Average number of messages over rounds.

3.5 Conclusion

In this paper, we investigated how people play coordination games with conflicting interests when they have the possibility to sequentially send non-binding messages. Theoretically, we found that the effectiveness of sequential communication depends crucially on the comparison of c (the payoff when both players concede) and b (the payoff of the disadvantaged player in a pure equilibrium). If $c < b$, as in the Battle-of-the-Sexes, then sequential communication is predicted to solve the coordination problem. Theoretically, it may happen that agreement is

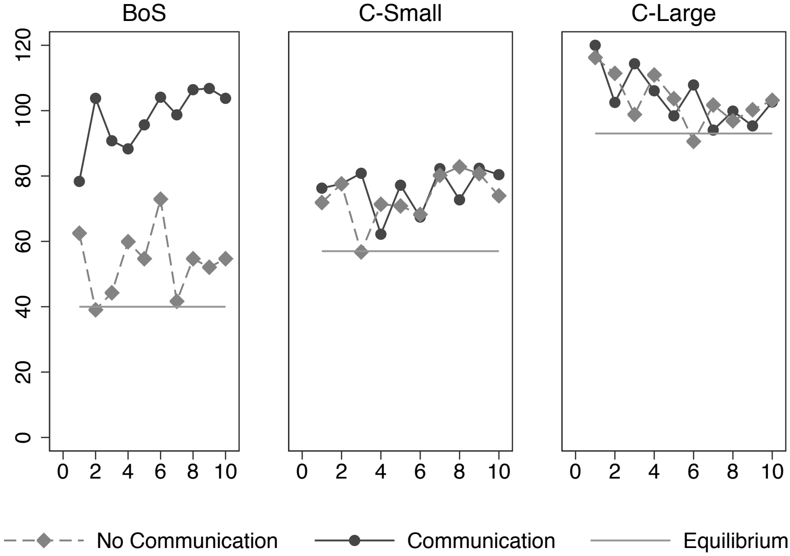

Figure 3.6: Average earnings over rounds. "Equilibrium" is the expected payoff if subjects play the mixed-strategy Nash-equilibrium of game *G*.

immediate and that either first or second sender is advantaged. It may also happen that people haggle for a long time and dissipate a substantial part of the available pie.

If on the other hand $c > b$, as in the Chicken games that we studied, the prediction with standard preferences is that communication is ineffective and no conversation will be started. Notice that this prediction is quite surprising. It deviates for instance from Ellingsen and Östling (2010) who predict that communication will powerfully resolve the coordination problem if players have some depth of thinking.

Theoretically, communication may also be effective in the Chicken games when players are lying-averse. If players suffer a cost when they deviate from an agreement, they may agree to both concede. Lying averse players will then conform to the agreement, while lying neutral players deviate. The higher c, the more conforming behavior is to be expected.

In the experiment, we find that communication works like a charm in the Battle-of-the-Sexes. There appears to be a common understanding that play should favor the first sender. Subjects do not lose much time to coordinate on this equilibrium.

In contrast, the possibility of communication is ineffective in the Chicken games. In the aggregate, it does not allow subjects to benefit and subjects often simply forgo the possibility to talk. When they do talk, they often agree on the outcome that gives them both c. As predicted, such agreements are only partly followed, and the extent to which they are followed responds positively to c. In the Chicken games we also observe some tendency to focus on the outcome that benefits the first sender. Demanding the good outcome is not without risk though, since it may easily happen that both subjects are demanding, after which the bad outcome frequently occurs.

One result that puzzled us is why so many subjects deviate from an agreement to both play L. In other experiments using different games, subjects are often very cooperative and trustworthy after communicating with one another (e.g., Bicchieri and Lev-On 2007, Balliet 2009). Why doesn't that happen in our environment? One possibility is that chatting is less forceful than face-to-face communication (e.g. Jensen et al. 2000, Brosig et al. 2003). Another reason might be that the wording in the conversations is different. Although we did not perform a formal text analysis, our impression is that most subjects do not make any promises. Instead, they only make a suggestion or a statement about what would be fair to do. In other experiments, subjects often make promises and those are a reliable sign of a person's trustworthiness (e.g. see Charness and Dufwenberg 2006, Belot et al. 2010, He et al. 2016b). Of course, that begs the question why subjects are more reluctant to make promises in our game than in other games. We do not have a clear answer to this, but possibly subjects feel no need to make promises because it is obvious what the desired course of action is; (L, L) gives high payoffs to both in *C-Large*, and the gains from deviating is relatively small. In other games, subjects have more to gain from deviating from an agreement, and might be more compelled to make a convincing case that they can be trusted.

Appendix 3.A Proofs

Proof of Proposition 3.1. First of all, we show that any equilibrium conversation can be at most 2 messages long if players use pure strategies for sending messages. Clearly, it cannot be optimal to never stop sending messages, as this is costly and players can deviate to $m = \emptyset$ (in their first message) making them strictly better off. Thus, in any equilibrium the number of messages is finite. There also has to be a credible agreement that yield higher payoffs than mixed strategy equilibrium without communication, otherwise players can deviate to $m = \emptyset$ earlier in the game. Note that the only possible credible agreement that yields better payoff than mixed strategy equilibrium for both players result in playing (H, L) or (L, H) and require $b > c$. Now suppose there would be $n \geq 3$ messages, after which a player terminates the communication stage. But then the player that receives L could deviate to sending message $\{\hat{L}, \hat{H}\}$ (or $\{\hat{L}\}$) at $n - 2$ messages, after which the other player best-responds by ending the communication stage. By ending the communication stage earlier both players receive the same payoffs in G and save on message costs. This implies that in equilibrium $n \leq 2$. Suppose that $n = 2$, then it must be that the second message confirms with the first message, hence, it is better off for the second sender to not to send the second message and save the communication cost. Therefore, the only equilibrium outcome is that the first sender demands or concedes in the first message, and the second sender leaves the communication immediately and they both play according to the first message. $\{\{\hat{L}, \hat{H}\}\}$ can, for instance, be supported as an equilibrium outcome by the strategies "send $\{\hat{L}, \hat{H}\}$" for player 1 after conflicting messages and "send $m = \emptyset$" after a credible agreement, and "send $\hat{H}$ until player 1 sends $\{\hat{L}, \hat{H}\}$ and then send $m = \emptyset$" for player 2. Player 2 clearly does not want to deviate to sending another non-empty message at any point. Player 1 should also not wish to deviate to $m = \emptyset$, yielding the threshold level $\gamma_1 = \frac{b(b-c)}{a+b-c}$. The conversation $\{\{\hat{H}, \hat{L}\}\}$ can, for instance, be supported as an equilibrium outcome by the strategies "send $\hat{H}$ until player 2 sends $\{\hat{L}, \hat{H}\}$ and then send $\emptyset$" for player 1, and "send $\emptyset$" if there is a credible agreement and "send $\{\hat{L}, \hat{H}\}$ if there is conflicting messages" for player 2. Player 2 clearly does not want to deviate to sending another non-empty message at any point. Player 1 should not wish to deviate to $m = \emptyset$, yielding $\gamma_2 = \frac{a(a-c)}{a+b-c}$ and $b > c$.

Proof of Proposition 3.2. In the second period, player 2's probability q_1 of demanding must make player 1 indifferent between demanding (by sending $\{\hat{H}, \hat{L}\}$) and conceding (by sending $\{\hat{L}, \hat{H}\}$) in the first period, thus we have: $(1-q_1)(a-\gamma) + q_1(b-3\gamma) = b-\gamma$, which gives $q_1 = \frac{a-b}{a-b+2\gamma}$. In the third period, player 1's probability q_2 of demanding must make player 2 indifferent between demanding (by sending $\{\hat{H}, \hat{L}\}$) and conceding (by sending $\emptyset$) in the second period, thus we have: $(1-q_1)(a-3\gamma) + q_1(b-4\gamma) = b-\gamma$, which gives $q_2 = \frac{a-b-2\gamma}{a-b+\gamma}$. If the conversation continues after three periods, there must be conflicting messages. A player must be indifferent between inducing agreement (by sending $\{\hat{L}, \hat{H}\}$) or let the communication stage continue (e.g., by sending $\{\hat{H}, \hat{L}\}$). thus we have: $(1-q)(a-2\gamma) + q(b-3\gamma) = b-\gamma$, which gives $q = \frac{a-b-\gamma}{a-b+\gamma}$.

Figure 3.1 provides an illustration of this situation after three periods. Sending $\{\hat{L}, \hat{H}\}$ yields a payoff of $b - t\gamma$ to player 1. After sending $\{\hat{H}, \hat{L}\}$, player 2 sends $\{\hat{L}, \hat{H}\}$ with probability $1-q$ (yielding a payoff of $a-(t+1)\gamma$ to player 1) and sends $\{\hat{H}, \hat{L}\}$ with probability q. Denote the continuation payoff of reaching period $t+2$ by V. Player 1 must be indifferent between $b - t\gamma$ and $(1-q)(a-(t+1)\gamma + qV$. At period $t+2$, if reached, player 2 should again be indifferent between sending $\{\hat{H}, \hat{L}\}$ and $\{\hat{L}, \hat{H}\}$. Sending $\{\hat{L}, \hat{H}\}$ yields $b-(t+2)\gamma$, so this must be equal to her continuation payoff V. Hence, we must have:

$$b - t\gamma = (1-q)(a-(t+1)\gamma) + q(b-(t+2)\gamma).$$

It follows that $q = \frac{a-b-\gamma}{a-b+\gamma}$. The same is true at any other period in the communication stage, except for the first message and the second message. Player 1 is indifferent between inducing agreement or continuing, and any probability of inducing agreement will support the equilibrium. Player 2 does not need to send $\{\hat{L}, \hat{H}\}$ or $\{\hat{L}, \hat{H}\}$ to signal agreement (leaving the conversation signals agreement), since there is no conflicting messages yet. However, for the sake of simplicity, we assume that player 1 also mixes with probability q and $1 - q$ in the first message, and that player 2 sends $\{\hat{L}, \hat{H}\}$ or $\{\hat{L}, \hat{H}\}$ in the second message as if there had been conflicting messages. Finally note that no player should wish to deviate to sending $m = \emptyset$, which is the case if $\gamma \leq \gamma_1$.

To calculate the expected number of messages, we first display the probability of each length of communication. The probability that the conversation ends with one message is $(1 - q_1) + q_1(1 - q_1)$; the probability that the conversation ends with two message is 0; the probability that the conversation ends with three messages is $q_1^2(1 - q_2)$; the probability that the conversation ends with four messages is $q_1^2 q_2(1 - q)$; the probability that the conversation ends with five messages is $q_1^2 q_2 q(1 - q)$; the probability that the conversation ends with six messages is $q_1^2 q_2 q^2(1 - q)$;...and the probability that the conversation ends with t (any $t \geq 4$) messages is $q_1^2 q_2 q^{t-4}(1 - q)$. We therefore derive the expected number of messages $N = 1 + 2q_1^2 + \frac{q_1^2 q_2}{1-q}$.

Appendix 3.B Experiment and Coding instructions

Experiment Instructions (for subjects)

Welcome to this experiment on decision-making. Please read the following instructions carefully. During the experiment, do not communicate with other participants unless we explicitly ask you to do so. If you have any question at any time, please raise your hand, and an experimenter will come and assist you privately. Your earnings depend on your own choices and the choices of other participants. During the experiment, your earnings are denoted in points. At the start of the experiment you will receive a starting capital of 300 points. In addition you can earn points during the experiment. At the end of the experiment, your earnings will be converted to euros at the rate: 1 point = 0.025. Hence, 40 points are equal to 1 euro. Your earnings will be paid to you privately.

You will be randomly matched with another person in the room. Each person will make a choice between H and L. If you and the other person both choose H, you will both receive nothing. If you choose H and the other person chooses L, then you receive 200 points and the other person receives 50 points. If you choose L and the other person chooses H, then you receive 50 points and the other person receives 200 points. If you and the other person both choose L, you will both receive 0 points. The possible decisions and payoffs are also shown in the following matrix. In each cell of the matrix, the first number shows the amount of points for you, and the

second number shows the amount of points for the other participant. In total, there will be 20 rounds. In each round, you are randomly rematched to another participant. At the end of each round, you will receive feedback about the decision of the other person and your payoffs.
Payoff matrix in Table 3.1 is displayed here.
In some rounds, you and the other person will have the opportunity to communicate before deciding between H and L. This happens in rounds 6-10 and 16-20. The communication works as follows. You and the other person can send messages to each other. There are four important rules for the communication:

- Only one person can send a message at a time. It will be randomly determined who can send the first message (you and the other person have an equal chance on being able to send the first message, independent of what happened in previous rounds). After that, you will take turns.
- Each of you have to pay 2 points for every message that is sent, no matter who sent the message. These points will be subtracted from your earnings. It is possible that your earnings in a round are negative. Any losses will be deducted from your starting capital.
- If it is your turn to send a message, you can also decide not to send any messages (by clicking on the Leave chat button). This will end the communication without affecting any of your earnings, and you and the other person will not be able to send any more messages in that round.
- You are not allowed to identify yourself in any way. If you identify yourself (for instance, by giving your name or describing what you look like or what you are wearing) you will be excluded from the experiment and lose all earnings including the starting capital.

In the rounds with communication, you will be paired with a different person in each round, so you will never chat with the same person twice. Likewise, in the rounds without communication, you will also be paired with a different person in each round, so you will never meet the same person twice in these rounds. At the end of the experiment, 4 out of the 20 rounds will be randomly selected for payment. Your earnings equal the sum of the starting capital 300 points and your earnings in the 4 selected rounds. If your total earnings are negative, you will receive 0.

Coding Instructions (for coders)
Thank you so much for helping us, your work is very valuable to us. Below are the instructions. Please read them carefully. If after reading you have any questions, please don't hesitate to ask any questions. Please work individually and do not discuss your choices with other people

while you are working on this task. We will show you chat conversations between people that participated in an experiment. In the experiment, participants were paired and randomly assigned the role of "Sender 1" or "Sender 2." Each person had to make a choice between two options: "H" and "L." They made their choices at the same time, without knowing what the other person did. Before they made their decisions, they could send messages to each other. Sender 1 could start by sending a message, and after that they alternated. Sometimes only Sender 1 sent a message. Your task will be to classify messages. Always read the entire conversation before answering any questions. The first question is about the intended choice that Sender 1 expresses in his or her messages.

Question 1: Which intention does Sender 1 express?

Choose from: *Weak intention to choose H; strong intention to choose H; Weak intention to choose L; Strong intention to choose L; None of the above/I don't know.*

If Sender 1 writes "I will play H" or "I choose H, up to you" or "I will play H, you should play L" , then he or she expresses intentions to play H. If instead Sender 1 writes: "Let's both play L" or "Let's choose L", then he or she expresses intentions to play L. We ask you to make a distinction between weak and strong expressions of intentions. An expression is strong if the sender emphasizes that this is definitely what he or she will do. Examples of strong expressions are "I will definitely play H", "I play H no matter what", "I will choose H and that is final." If you cannot infer any intention based on Sender 1's messages, or if the intention doesn't fit with the above two categories, you can indicate this by selecting the bottom option ("None of the above/I don't know").

The second question is the same as the first question, but for the other sender:

Question 2: Which intention does Sender 2 express?

The third question is whether or not they made some agreement.

Question 3: Did the two senders reach an agreement?

Choose from: *No; Yes, on both choosing L; Yes, on one choosing L and the other choosing H; I don't know.*

By reaching an agreement we mean that the senders know about each other's intentions, and they show some approval or confirmation (such as "ok" or "I agree" or "yes let's do that"). For instance, if Sender 1 wrote: "Let's both play L" and Sender 2 wrote "Ok", then they reached an agreement. If Sender 1 wrote "I play H" and Sender 2 wrote "I play L" and Sender 1 responded by writing "Ok" then they also reached an agreement. You should only classify the chat as reaching an agreement if the intentions of both players are clear. For instance, if Sender 1 writes "I play H" and Sender 2 writes "okay", then it is not clear what Sender 2 will choose, and therefore this should not be classified as an agreement. Similarly, you should only classify the chat as reaching an agreement if at least one of the players shows approval or confirmation.

If Sender 1 writes “I play H” and Sender 2 writes “me too”, then they did not reach an agreement because none of the players shows any approval or confirmation.

Please also pay attention to the following: What matters is the written intention at the end of the conversation. It can happen that players change their mind. In such cases, please classify messages according to the most recent statement of a player. For instance, suppose Sender 1 writes: “I will play H no matter what,” Sender 2 responds with “Let’s both play L”, after which Sender 1 writes “Ok.” In this case, we would classify Sender 1’s message as “Will choose L” and we would classify this as an agreement to both play L. Sometimes players will speak of “High” and “Low” instead of “H” and “L”, but they mean the same thing. You always need to answer all three questions. If there is no message by Sender 2, then please select “I don’t know.” You will see many chats. Please try to stay focused and take a break if you need to. After you have finished coding all chats, we will ask you to recode 50 randomly chosen chats. We will use this to measure the consistency of coders.

Chapter 4

Minority Advantage and Disadvantage in Competitive and Cooperative Environments

4.1 Introduction

Our society comprises majority and minority groups based on diverse categories. Interestingly, members of minority groups can have disadvantages in certain situations but have advantages in some others. For example, left-handers might suffer in the industrialized environment designed for a right-handed majority, but they also enjoy a benefit in many sports such as tennis, boxing and fencing.[1] Or, linguistic minorities are likely to be isolated from the majority language group in today's multilingual workplaces, but they may find it easier to transmit secret messages during wartime.[2] Finally, though ethnic minorities face more difficulties to find a job, they tend to have stronger social ties.[3]

While there is a growing literature on the sources of minority disadvantages,[4] we know very

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[1]See Hardyck and Petrinovich (1977), Coren and Halpern (1991), and Aggleton et al. (1993) for the costs of being left-handed in a right-handed world; see Raymond et al. (1996), and Voracek et al. (2006) for left-handedness in sports.

[2]Starting in World War I, the U.S. government hired Native Americans who spoke little-known languages to transmit messages using codes based on their languages as a form of secret communication.

[3]See Bertrand and Mullainathan (2004) for job discrimination; see Mehra et al. (1998) for social ties.

[4]For example, see Blau and Kahn (1992), Darity and Mason (1998), Altonji and Blank (1999), Bertrand et al. (2005), and De Haan et al. (2015a), among others.

little about the formation of minority advantages. This study first investigates how a minority advantage or disadvantage arises in different social interactions. Answering this question contributes to the understanding of majority-minority inequality, and helps in forming public policy to reduce it.

The key innovation is that individuals strategically invest in different skills that are useful in interactions with different types of people. Depending on the characteristics of the interactions, being competitive or cooperative, the minority group may acquire an advantage or a disadvantage. In competitive environments, members of the minority group enjoy an advantage as individuals tend to pay more attention to competing against the majority group. In cooperative environments, by contrast, members of the minority group are at a disadvantage since they tend to conform to the majority group. This idea is formalized by a game-theoretical model and the predictions are tested in a laboratory experiment.

In the theoretical model, there are two groups of people of unequal size. Every person belongs to either the minority or the majority group. Individuals spend their time investing in skills. There are two types of skills: in-group skills and out-group skills. A key assumption is that investing in out-group skills is more costly. Another assumption is that people are randomly matched with each other.

The first setting is a competitive environment. This environment resembles interactions such as sports. In sports such as tennis, players can train different skills to compete against different types of opponents. In this setting, in-group skills are relevant whenever a person interacts with a member from her own group, while out-group skills are relevant for interactions with members from the other group. In the competition, only the person with the highest skill level wins and receives a payoff.

In such a setting, the model shows that people from the minority group have an advantage. Intuitively, if the minority group is sufficiently small, so that it is rare to meet a member from the minority group, it does not pay to invest in out-group skills for members of the majority group. People from the minority group, on the other hand, will find it profitable to invest in out-group skills, as they are often matched with out-group members. This implies that whenever a minority group member is matched with a majority group member, the minority group member will beat the majority group member. People from the majority group will nevertheless find it unprofitable to invest in out-group skills, as this would reduce their chances of winning when matched with another majority group member, which happens relatively often.

The second setting is a cooperative environment, which is motivated by interactions such as communication. Individuals can learn different languages. The ability to communicate is limited by the person with the lowest proficiency, and two persons will choose to converse in the language for which the common proficiency is highest. In this setting, two persons coordinate skills in their interactions. In coordination, both persons receive a payoff that is equal to the lowest skill level, and they coordinate the skills that yield the highest payoff.

In such a setting, the model predicts that people from the minority group have a disadvantage. Naturally, members of the majority group shy away from investing in out-group skills for two reasons: first, it is more costly compared to investing in in-group skills; and second, chances of meeting an out-group member are smaller than meeting an in-group member. People from the minority group, on the other hand, face a trade-off: though investing in in-groups skills is less costly, they are also less likely to meet an in-group member. If the minority group is sufficiently small, members of the minority group will prefer to invest only in out-group skills. This implies that whenever a minority group member is matched with a majority group member, they will use the majority group's in-group skills to coordinate. Since members of the minority group invest in their most costly skills, they receive a lower payoff compared to those of the majority group. This is referred to as *conforming equilibrium*. Such a phenomena can be observed in societies with a dominant majority group. For instance, though US is a multilingual country, two persons from different language groups will most likely communicate in English. By contrast, if the minority group is large enough, another equilibrium exists in which members of the minority group invest everything in their in-group skills. Thereby coordination fails when two persons from different groups meet. This is referred to as *segregating equilibrium*. For instance, in Switzerland, there still exists a language barrier between German-speaking and French-speaking citizens.

To test these predictions, the model is brought to the laboratory. The objective of using a laboratory experiment in this study is twofold. First, it allows to perfectly control for the share of the minority group while keeping other things equal. While in the real world, it is virtually impossible to have two identical societies that only differ in their population composition. Second, with empirical data we often do not know the values of the model parameters. For example, it is hard to measure the cost difference between skills. In contrast, in the experiment all relevant parameters can be controlled.

In the experiment, the model is implemented in a straightforward manner. Subjects are either in a competitive environment or in a cooperative environment; they are assigned to a majority or a minority group, while the relative size of the minority group is either small or large. They are endowed a budget to allocate between two skills and are then randomly paired within a matching group.

The experimental results reveal the predicted differences in majority-minority inequality: In the cooperative environment, a minority group member earns less than a majority group member. Yet, this result *reverses* in the competitive environment. As expected, in both environments an increase in the share of the minority group reduces the payoff inequality. In particular, in the cooperative environment, the relatively small minority groups always conform to the majority, whereas the large minority groups sometimes remain segregate. Hence, it appears that the majority-minority gap can arise in different environments, as individuals strategically invest between their skills to achieve economic success.

Apart from contributing to the understanding of the sources of majority-minority inequality, the results also provide important insights into the design of public policies. They suggest that a policy-maker interested in reducing the majority-minority gap should help the disadvantaged group before they invest in skills, when the policy might be most effective. For example, worldwide immigrants and refugees fall behind their local counterparts in education, labor market as well as wellbeing. A significant part of the gap is due to cultural and linguistic barrier. A policy-maker can narrow the gap by reducing the cost of assimilation upon them settling down in the host country, which can be realized by helping them learn local language and adapt to local culture. Not only can this policy reduce inequality gap, but it might also increase the total welfare of the society by preventing segregation.

The remainder of the chapter is organized as follows. Section 4.2 discusses related literature. Section 4.3 presents the model. Section 4.4 reports the experimental design and procedures. Section 4.5 illustrates the experimental findings. Section 4.6 concludes.

4.2 Related Literature

To the best of my knowledge, this chapter is the first to establish a majority-minority equality by incorporating individuals' strategic skill investment choices. The inequality manifests in the equilibrium of a game-theoretical model and is found in the laboratory. This is new to the literature on majority-minority inequality. Economists often argue that the income gap between the majority and minority group is largely caused by discrimination (see Blau and Kahn 1992; Darity and Mason 1998; Altonji and Blank 1999; Bertrand et al. 2005; De Haan et al. 2015a). In particular, there is evidence on labor market discrimination against ethnic minority group (Reimers 1983; Riach and Rich 2002; Bertrand and Mullainathan 2004; Carlsson and Rooth 2007; Kaas and Manger 2012), linguistic minority group (Lang 1986; Dustmann and Fabbri 2003), and homosexual men (Badgett 1995; Elmslie and Tebaldi 2007; Drydakis 2009). Psychologists attribute the academic gap between the majority and the minority groups to stereotype threat (Spencer et al. 1999; Steele et al. 2002). This study contributes to this literature by adding another type of minority disadvantage, while other factors are absent, that is caused merely by the strategic choices of the minority and majority groups.

This chapter is also the first to compare minority advantage and minority disadvantage by characterizing the economic environment. The existing literature mostly studies such an advantage or disadvantage of a particular minority group in isolated situations. In the research about handedness, for example, some studies find that left-handed people bear a higher risk of accidents and a shorter lifespan, probably due to the industrialized environment designed for a right-handed majority (Coren and Halpern 1991; Aggleton et al. 1993). On the other hand, some other studies show that the left-handed athletes enjoy an advantage in sports, as the top-ranked left-handed athletes are overrepresented in many sports compared to the general population (Raymond et al. 1996; Voracek et al. 2006; Abrams and Panaggio 2012). This chapter, however, connects the seemingly unrelated literature by noting that being a minority can have a reversed effect, depending on the social or economical environment.

The chapter is related to the large body of literature on contests. In particular, the all-pay auction is the benchmark model for the study of competitive environment. This is because the all-pay auction game captures the general sunk investments inherent in scenarios such as labor-market tournament and sports competition (Szymanski 2003; Siegel 2009). A comprehensive survey about all-pay auction model can be found in Dechenaux et al. (2014). The cooperative environment is captured by minimum-effort coordination games. This game is selected because it is often used in situations where the weakest link determines the outcome of a joint task, such as public goods provision and language usage (Van Huyck et al. 1990; Anderson et al. 2001; Riechmann and Weimann 2008). Experimental surveys on this class of games can be found in Mehta et al. (1994), Devetag and Ortmann (2007).

In the model, it is assumed that both the assignment and the proportion of types is exogenous, and the payoff inequality is endogenous. This assumption is supported when it is unfeasible or costly for individuals to change their types. However, a stream of papers in biology study the effect of majority-minority in a different way. In these studies, the proportions of the majority or the minority are endogenous and the costs and benefits associated with each type are exogenous. In turn, the equilibrium proportion of the minority group can be derived (Ghirlanda and Vallortigara 2004; Billiard et al. 2005; Ghirlanda et al. 2009). These models are motivated by an evolutionary assumption that the fitness (payoff) of each type determines the dynamics of their population shares.

In line with the existing literature, this chapter shows that the minority group members are less likely to conform to the majority when the relative size of the minority group increases. Such a relationship has been characterized by some economic studies (Lazear 1999; Bisin and Verdier 2000; Kuran and Sandholm 2008; Advani and Reich 2015).[5] These studies find that, virtually without exception, the larger the relative size of a minority, the less likely that members of the minority group will assimilate. In particular, Lazear (1999) argues that in a pluralistic

[5] In Brown and Fuguitt (1972), Tienda and Lii (1987) and Kahanec (2006), it is found that the larger the relative size of a minority, the greater the majority-minority disparities in income.

society, the value of assimilation is larger to an individual from a small minority than to one from a large minority group.

4.3 Model

This section presents a simple and stylized model of skill investment that abstracts from all but the bare essentials necessary to illustrate the motivating ideas. The baseline setup is that an individual may meet his or her in-group members or out-group members during interactions. The type of the paired person is *ex ante* unknown. Therefore, an individual's investment decision is a trade-off between in-group and out-group skills. Further, individuals are either in a competitive environment or a cooperative environment. In the competitive environment, in-group (out-group) skills promote success when interacting with in-group (out-group) members. In the cooperative environment, individuals coordinate skills with their paired partners.

Population structure. Consider a population consisting of n risk-neutral individuals indexed by $i \in \mathbf{N} := \{1, ..., n\}$. An individual's type is characterized by his or her population share: $t \in \{\theta, \tau\}$, with θ the *majority type* and τ the *minority type*. The population share of the minority type defines a *population state* ϵ with $\epsilon \in (0, \frac{1}{2})$.

Matching process. Consider a uniform random matching process, that is, the chance of meeting anyone in the population is the same. This matching process captures environments in which one cannot choose a specific partner, such as in sports competition and workplace interaction. When the population is large, the chance of meeting oneself is negligible. This implies that the probability of meeting a θ is approximated as $1 - \epsilon$, and the probability to be matched with a τ is approximated as ϵ. For the remainder of this section, this approximation is employed by assuming a large population size. For small populations, the robustness of the model is discussed in Appendix 4.B.

Strategy. Individuals are endowed with ω units of time to allocate between two types of skills: in-group skills and out-group skills. Without loss of generality, let 1 and c denote the unit cost for in-group skills and out-group skills respectively. Assume that it is easier to invest in the in-group skills than to invest in the out-group skills, or, in mathematical form, $c > 1$.[6] Within the strategy set X, let $x \in X$ denote the level of out-group skills obtained by an individual, and the level of in-group skills acquired by the individual is simply $(\omega - cx)$. Levels of skills are integer numbers.[7] That is, $X = \{0, 1, ..., \bar{x}\}$, where $\bar{x}$ is the largest integer number such that

[6]This assumption is made due to the fundamental idea of in-group skill and out-group skills. In most social interactions, one tends to find it easier to obtain in-group skills than out-group skills. This can be caused by intrinsic difference or physical costs.

[7]The reason to restrict the strategy set to discrete choices is to induce the existence of Nash equilibrium. If the level of skill is a continuous choice variable, as is shown later in the competitive environment, members of the

$c \cdot \bar{x} \leq \omega$.

Payoff. When individual i of type t_i playing strategy x_i is matched with an individual j of type t_j playing strategy x_j, individual i receives payoff $\pi(t_i, t_j, x_i, x_j)$.

The expected payoff of individual i is the average payoffs when meeting everyone in the population. This implies that one's payoff is a function of n strategies, each of which is used by each individual. Suppose that individuals of the same type use the same strategy. For each state $\epsilon \in (0, \frac{1}{2})$ and any strategy $x \in X$ used by θ and any strategy $y \in X$ used by τ, the resulting expected payoff of each type is

$$\begin{cases} \Pi_\theta(x, y, \epsilon) = (1-\epsilon) \cdot \pi(\theta, \theta, x, x) + \epsilon \cdot \pi(\theta, \tau, x, y) \\ \Pi_\tau(x, y, \epsilon) = (1-\epsilon) \cdot \pi(\tau, \theta, y, x) + \epsilon \cdot \pi(\tau, \tau, y, y) \end{cases} \tag{4.1}$$

4.3.1 Competitive environment

Consider a simple situation where individuals compete for limited resources (food, prizes, jobs, partners, etc.). When two persons meet, the one with higher skills wins and receives a payoff. This is captured by all-pay auction models. In all-pay auction models, a group of people compare one type of score. However, in many competitive situations such as sports, individuals acquire many different skills to compete against different types of opponents. This can be achieved by adding competitor types and skill types. In the competitive environment of my model, there are two types of individuals, the majority type and the minority type. Each individual acquires two types of skills, in-group skills and out-group skills. Individuals use the in-group skills to compete against someone of the her own type and use the out-group skills to compete against someone of the other type.

More exactly, when two individuals from the same group are matched, they both use their in-group skills $(\omega - cx)$ to compete against each other. Therefore, individual with a higher level $(\omega - cx)$ wins and receives a payoff v while the other loses and receives nothing. When two individuals have the same level $(\omega - cx)$, chance decides the winner, with an expected payoff of $\frac{v}{2}$.

When two individuals from different groups are matched, they both use their out-group skills x to compete against each other. Therefore, individual with a higher x wins and receives a payoff v while the other loses and receives nothing. When two individuals have the same level

minority group will choose the smallest positive quantity to invest in out-groups skills. However, there does not exist a smallest positive number in the continuous choice set.

x, chance decides the winner. The payoff function is summarized below:

$$\pi^{comp}(t_i, t_j, x_i, x_j) = \begin{cases} v \text{ if } (t_i = t_j \text{ and } x_i < x_j) \textit{ or } (t_i \neq t_j \text{ and } x_i > x_j) \\ \frac{v}{2} \text{ if } x_i = x_j \\ 0 \text{ if } (t_i = t_j \text{ and } x_i > x_j) \textit{ or } (t_i \neq t_j \text{ and } x_i < x_j) \end{cases} \tag{4.2}$$

Equilibrium. For the equilibrium strategies, consider only the possibility that individuals of the same type use the same strategy, which is referred to as *symmetric strategy*. The reason behind this is that this population game is considered to be played by *two types* of individuals, where individuals of the same type are not distinguishable. Eventually, only the difference between each type of individuals is observed. When individuals use symmetric strategies in an equilibrium, the equilibrium is referred to as *symmetric equilibrium*. Definition 4.1 characterizes the conditions of symmetric Nash equilibrium.

Definition 4.1 (Nash Equilibrium). *In any state $\epsilon \in (0, \frac{1}{2})$, a strategy pair $(x^*, y^*) \in X^2$ is a (symmetric) Nash Equilibrium if*

$$\begin{cases} x^* \in \underset{x \in X}{argmax}[(1-\epsilon) \cdot \pi(\theta, \theta, x, x^*) + \epsilon \cdot \pi(\theta, \tau, x, y^*)] \\ y^* \in \underset{y \in X}{argmax}[(1-\epsilon) \cdot \pi(\tau, \theta, y, x^*) + \epsilon \cdot \pi(\tau, \tau, y, y^*)] \end{cases} \tag{4.3}$$

Turning to equilibrium analysis, it can be argued that the majority group are only willing to invest on the out-group skills when the share of the minority is sufficiently large. Suppose that the share of the minority group is sufficiently small, a member of the majority group would invest everything in her in-group skills, so that she can make sure not losing against her group members. Subsequently, members of the minority group would easily beat a majority group member by investing a positive amount of time in his out-group skills. However, he would also maximize his chance of winning when meeting his in-group members.[8] This implies that members of the minority group would eventually spend the smallest positive amount to invest in their out-group skills. Such a small share of the minority group yields the equilibrium $x^* = 0$, $y^* = 1$.

Once the share of the minority group is sufficiently large, members of the majority group would find it profitable to invest in the out-group skills. Subsequently, members of the minority would also increase their investment on out-group skills. Under this condition, there exists no pure-strategy equilibrium. For subsequent sections, the symmetric mixed-strategy equilibrium that corresponds to the experimental setup will be characterized in Appendix 4.C.

[8]For the sake of convenience, in this chapter 'she' is referred to someone from the majority group, and 'he' is referred to someone from the minority group.

Proposition 4.1 (Nash Equilibrium in competitive environment). *In any state $\epsilon \in (0, \frac{1}{3}]$, there exists a unique symmetric equilibrium $x^* = 0$, $y^* = 1$. In any state $\epsilon \in (\frac{1}{3}, \frac{1}{2})$, there exists no symmetric pure-strategy equilibrium.*

The intuition behind Proposition 4.1 goes as follows. When the share of the minority is small enough, members of the majority spend nothing on their out-group skills, while members of the minority obtain the smallest positive level on their out-group skills. When two individuals from different groups meet, the one from the minority group wins. When two individuals from the same group meet, they make a tie. Note that this proposition is silent about any differences at individual level, rather, it focuses on the difference between the two groups. To capture individual difference, the model has to be extended by adding heterogeneous individual abilities. Nevertheless, to resolve the question asked in this chapter, I focus on the payoff difference between the two population groups.

According to Proposition 4.1, when their relative size is sufficiently small, members of the minority group has a higher chance of winning compared to members of the majority group. This leads to a minority advantage: in equilibrium a member of the minority group receives a higher payoff than a member of the majority group. This helps explain the puzzle of "left-handed advantage" in most interactive sports: As it is common knowledge that left-handed people consist of a small share among human population, athletes spend less time to practice against them than to practice against the right-handed majority. As a result, left-handed athletes enjoy a benefit since their opponents are unfamiliar with their strategies. They can thereby achieve a higher rank at group level, conditioning on their abilities.

4.3.2 Cooperative environment

Consider a simple situation where individuals coordinate with each other (communication, joint tasks, social norms, etc.). When two persons meet, the one with a lower skill level determines the payoffs of both. This is represented by minimum-effort models. In a minimum-effort game, a group of individuals compare a single score. However, in many cooperative situations such as communication, individuals possess many types of skills such as different languages. In these situations, they can choose to coordinate the skills that yield the best outcome. This can be achieved by adding cooperator types and skill types. In the cooperative environment of my model, two types of individuals invest in their in-group skills and out-group skills, and are then randomly matched with each other. When two persons meet, they can choose to coordinate the skills that yields the highest payoffs.

More precisely, when two persons from the same group are matched, they can use both of their in-group skills $(\omega - cx)$ or both of their out-group skills x to coordinate. In either case, the one with a lower skill level determines the payoffs of both persons. Thus, the maximal payoff is realized when two persons choose the skills that yield the highest payoffs.

When two persons from different groups are matched, one person can use his in-group skills $(\omega - cx)$ to coordinate with the other person's out-group skills x; or the other way around. Either way, the person with a lower skill level determines the payoff of both persons. To maximize their payoffs, two persons eventually coordinate the skills that yield the highest payoffs. The payoff function is summarized below.

$$\pi^{coop}(t_i, t_j, x_i, x_j) = \begin{cases} Max\{Min\{\omega - cx_i, \omega - cx_j\}, Min\{x_i, x_j\}\} \text{ if } t_i = t_j \\ Max\{Min\{\omega - cx_i, x_j\}, Min\{x_i, \omega - cx_j\}\} \text{ if } t_i \neq t_j \end{cases} \tag{4.4}$$

Equilibrium. By Definition 4.1, in equilibrium none of the individuals can gain by deviating. Consider that the share of the minority group is very small, both groups of individuals would match their strategies with the majority group. This implies that it is optimal for a majority group member to do the same as her group members, while it is optimal for members of the minority group to mirror the strategies used by the majority group. There are two possible equilibria types: (1) when two individuals meet, they use the in-group skills of the majority group and (2) when two individuals meet, they use the in-group skills of the minority group. In both cases, one group conform to the other by investing an adequate amount in the out-group skills. As investing in out-group skills is more costly than investing in in-group skills, members of the conforming group receive a lower payoff.

Once the share of the minority group is sufficiently large, members of the minority group will find it too costly to mirror the strategies used by members of the majority group. This is because investing in out-group skills is more costly than investing in in-group skills. Given that the minority group is large enough, it is more profitable to focus on in-group coordination. As a result, another set of equilibria arises: both types invest an adequate amount of time in their in-group skills, and the two groups fail to coordinate with each other.

Proposition 4.2 (Nash Equilibria in cooperative environment). *In any state $\epsilon \in (0, \frac{1}{2})$, there exists two sets of equilibria:*
(1) the minority group conform to the majority group: $x^* \leq \frac{\omega}{c+1}$, $y^* = min(\omega - cx^*, \bar{x})$,
(2) the majority group conform to the minority group: $x^* = min(\omega - cy^*, \bar{x})$, $y^* \leq \frac{\omega}{c+1}$,
In any state $\epsilon \in [\frac{1}{1+c}, \frac{1}{2})$, there exists another set of equilibria:
(3) the two groups segregate from each other: $x^* \leq \frac{\omega}{c+1}$, $y^* \leq \frac{\omega}{c+1}$.

Due to the characteristics of the minimum-effort game, there are multiple equilibria in each set of (1), (2) and (3). This is because unilateral deviations often pays exact the same payoff: no loss, but no gain either. Equilibria of this property can be eliminated by implementing sensible refinements. To reduce the number of equilbria in this game, Definition 4.2 introduces strict Nash equilibrium.

Definition 4.2 (**Strict Nash Equilibrium**). *In any state $\epsilon \in (0, \frac{1}{2})$, a strategy pair $(x^*, y^*) \in X^2$ is a strict (symmetric) Nash Equilibrium if*

$$\begin{cases} x^* = \underset{x \in X}{argmax}[(1-\epsilon) \cdot \pi(\theta, \theta, x, x^*) + \epsilon \cdot \pi(\theta, \tau, x, y^*)] \\ y^* = \underset{y \in X}{argmax}[(1-\epsilon) \cdot \pi(\tau, \theta, y, x^*) + \epsilon \cdot \pi(\tau, \tau, y, y^*)] \end{cases} \tag{4.5}$$

In mathematical form, Definition 4.2 differs from Definition 4.1 in that, given the strategies used by other players, the equilibrium strategy is the *unique* optimal strategy. Put differently, in Definition 4.1, a pair of strategies consists an equilibrium if unilateral deviation is unprofitable. In Definition 2, by contrast, a pair of strategies consists an equilibrium *if and only if* any unilateral deviation yields a strict loss. This strengthens the equilibrium condition and thereby reduce the set of the equilibria: By Definition 2, only one equilibrium ($x^* = 0$, $y^* = \bar{x}$) in set (1) survives as a strict equilibrium. Likewise, in set (2) only equilibrium ($x^* = \bar{x}$, $y^* = 0$) survives. In these two equilibria, one group invest only in out-group skills while the other group invest only in in-group skills. Finally, one equilibrium ($x^* = 0$, $y^* = 0$) in set (3) survives under Definition 4.2. (Proofs are provided in Appendix 4.A.)

Proposition 4.3 (**Strict Nash Equilibrium in cooperative environment**). *In any state $\epsilon \in (0, \frac{1}{2})$, there exists two equilibria:*
(a.1) the minority group conform to the majority group ($x^* = 0$, $y^* = \bar{x}$),
(a.2) the majority group conform to the minority group ($x^* = \bar{x}$, $y^* = 0$).
In any state $\epsilon \in [\frac{1}{1+c}, \frac{1}{2})$, there exists another equilibrium:
(b) the two groups segregate from each other ($x^* = y^* = 0$).

In Proposition 4.3, both (a.1) and (a.2) consist of one group conforming to the other. In (a.1), the majority group invest in their in-group skills, whereas (a.2) occurs when the majority group invest in the out-group skills. As is established in Van Huyck et al. (1990), coordination in the minimum-game becomes harder to achieve as the cost of effort (c) increases. Arguably, (a.2) is unlikely to occur as it requires the majority group to coordinate on the most costly skills, while a less costly skill is available. Therefore, though sustaining as a strict equilibrium, (a.2) has little predicting power compared to (a.1).[9] In the following analysis and experimental sections, I focus on the two strict equilibria (a.1) and (b). With some abuse of terminology, these two equilibria are referred to as *conforming equilibrium* and *segregating equilibrium* respectively.

[9]This conjuncture is supported by the experimental results in this chapter. The subjects never converge select the equilibrium in (a.2).

Finally, to compare the conforming equilibrium (a.1) and the segregating equilibrium (b), I look at the payoff gap and the efficiency of each equilibrium. The payoff gap is represented by $\Pi_\theta - \Pi_\tau$.[10] The efficiency is represented by the total payoffs of all players in the population, and is presented by $n(1-\epsilon)\Pi_\theta + n\epsilon\Pi_\tau$.

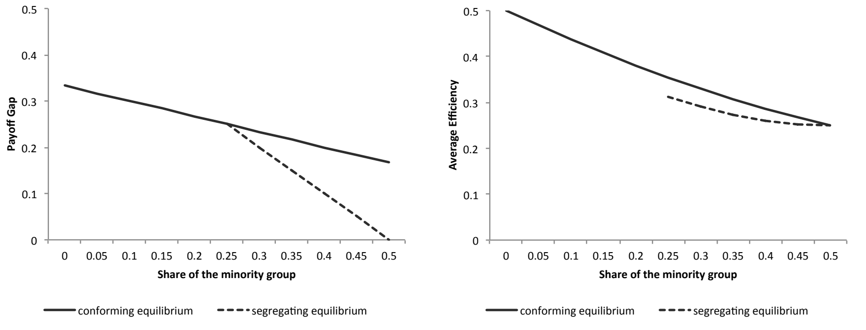

Figure 4.1: The left panel shows the payoff gap between the majority and the minority type in each equilibrium. The right panel shows the efficiency in each equilibrium. In both panels, the x-axis is the population share of the minority group. Values of parameters in this plot are $c = 3$ and $\omega = \frac{1}{2}$, tie-breaking point is $\bar{\epsilon} = \frac{1}{1+c} = \frac{1}{4}$, and the population size n is normalized to 1.

Figure 4.1 plots the payoff gap (left panel) and the efficiency (right panel) of each equilibrium. As can be seen from the left panel, in the conforming equilibrium, members of the majority earn more than members of the minority, and the payoff gap decreases in the share of the minority group (ϵ). This also holds for the segregating equilibrium. Comparing these two equilibria, it can be seen that the payoff inequality is always bigger in the conformity equilibrium at any $\epsilon \in (\frac{1}{4}, \frac{1}{2})$. Interestingly, the payoff gap only reaches zero in the segregating equilibrium when the share of the minority approaches half. At this point, the size of the majority and minority group is (almost) equal, and therefore the payoff gap vanishes in segregation.

Turning to efficiency, as can be seen in the right panel, the rank of the equilibrium is clear: the efficiency is always higher in the conforming equilibrium. In both equilibria, the efficiency decreases in the share of the minority and eventually equalize when the share of the minority group is half. Intuitively, the society reaches highest efficiency when there is only one type present.

To conclude, if equilibrium is selected according to efficiency, the conforming equilibrium will stand out. However, if the society cares about equality as well, one may expect the segregating equilibrium to be selected when the share of the minority is large. In both equilibria, a minority disadvantage can be expected.

[10] Since $\Pi_\theta \geq \Pi_\tau$ in both equilibria, this is equivalent to a more general form $|\Pi_\theta - \Pi_\tau|$.

To this extent, the model establishes a minority advantage in the competitive environment and a minority disadvantage in the cooperative environment. To note, these results are derived under two critical assumptions. First, it is more costly to invest in the out-group skills than the in-group skills. Second, individuals in a population are matched uniformly randomly. These two assumptions can in turn set limitations to the results.

The assumption that the out-group skills is more costly holds by its nature: individual are physically or socially less familiar with the out-group skills, or have less available resources to obtain such skills compared to their in-group skills. For instance, it is more difficult for right-handed people to train their skills against the left-handed people, as they naturally play in a different way; or, it is more costly for the children of the immigrants to learn the local language than the children of native-born parents, as it is harder for them to obtain equal resources. Therefore, as cost relationship which is not captured by this assumption is irrelevant within the scope of this study, it is sensible to leave them.

The uniform random matching process captures situations in which one cannot or does not select her counterparts in interactions. This perfectly resembles some situations. For instance, on the tennis court, one cannot choose his opponent; on the job market, one also does not choose against whom to compete. However, in some other scenarios people may actually choose their interacting counterparts. For example, in a multilingual environment, it is common that people from the same language group are more likely to interact with each other. To capture this, the model needs to incorporate an assortative matching process.[11] Depending on the degree of the assortativity, the magnitude of the minority advantage or disadvantage is expected to be reduced. Nevertheless, the direction of the results would remain intact. Furthermore, assortative matching in the cooperative environment would favor the segregating equilibrium over the conforming equilibrium. The intuition is that if one is less likely to meet people from the other group, one is then less motivated to invest in the out-group skills. After all, the assortative matching process can be added to the model and may give rise to some interesting results.

4.4 Experimental design and procedures

4.4.1 General setup

The design of the experiment closely follows the theoretical model described in section 3. The parameters in the model are chosen to generate comparative statistics. The size of the population

[11]It is also possible that people are more likely to interact with someone from the other group than someone from her own group, this can be captured by disassortative matching process. This is not commonly observed nor studied in sociology or economic literature.

is $n = 12$, as this size allows both small minority group and large minority group.[12] Members of the majority group receive a role color "Red" and members of the minority group receive a role color "Blue".[13] The minority group has two different sizes. The large minority group consists of 5 individuals while the small minority group consists of 3 individuals.

Individuals are endowed $\omega = 30$ points to allocate on two types of skills: skill blue and skill red.[14] Skill blue is the in-group skill for Blue individuals and is the out-group skill for Red individuals. Likewise, skill red is the in-group skill for Red individuals and is the out-group skill for Blue individuals. The unit cost of out-group skills is $c = 3$ and the unit cost of in-group skills is 1. The selection of these two parameters ω and c yields a set of choice bundles. In each choice bundle, the first number represents the level of the in-group skill and the second number represent the level of the out-group skill. For example, individuals who choose a bundle (21, 3) has level 21 of in-group skill and level 3 of out-group skill. There are in total eleven choice bundles, in which the level of out-group skill takes a value from $\{0, 1, .., 10\}$. Finally, the winning payoff in the competitive environment is $v = 30$ points.

Red players	Blue players
0, 30	30, 0
0, 30	18, 4
0, 30	3, 9
0, 30	
0, 30	
0, 30	
0, 30	
3, 27	
6, 12	

Figure 4.2: This figure shows a screenshot of decisions overview of the previous round within a matching group in treatment S-comp. The column separates the players by their role color. Each cell indicates the level of skill blue and the level of skill red of one player. The blue (red) number indicates the level of skill blue (red).

The experiment consists of four parts. The first part provides the instructions. The second

[12] As is mentioned later, the small minority group has 3 individuals. It is problematic to have only 2 subjects in the minority group, as they will be able to identify each other's strategy.

[13] The model's predictions is robust with small population, the analysis is presented in Appendix 4.B.

[14] The unit of the endowment is 'point' in the experiment.

part assigns a role color to each player, and the same color is kept throughout the experiment.[15] Then, players make decisions and payoffs are obtained. This part is repeated for 30 rounds. At the beginning of each round, every player chooses from the eleven bundles to determine their skills. After everyone made their decisions, the players are randomly and anonymously matched into pairs. At the end of each round, players learn the role color of the paired player, the decision of the paired player and the realized payoff. To allow for learning, they are also provided with a table illustrating the decisions in the previous round within their matching group. An example of the decisions overview is shown in Figure 4.2. Finally, the experiment is finished by a short questionnaire.

4.4.2 Treatments

The experiment consists of a two-by-two factorial design. Treatments are varied between subjects. The structure of the treatments is shown in Table 4.1. The first dimension determines the size of the minority group. The second dimension determines the competitive or the cooperative setting.

Table 4.1: Experimental Design

Conditions	Competitive	Cooperative
Small minority group	S-comp	S-coop
(3 Blue players, 9 Red players)	(N=6)	(N=6)
Large minority group	L-comp	L-coop
(5 Blue players, 7 Red players)	(N=6)	(N=6)

Notes: The cell entries show the acronyms used for the between subjects treatments (N = the number of matching groups).

Varying the population composition between treatments enables me to investigate whether the share of the minority group matters. Varying the competitive or cooperative setting allows me to look at whether the economic environment matters. This simple design tests the two central predictions of the model. First, how does a change in the share of the minority affect the equilibrium; second, how does the environment bring advantage or disadvantage to different types of individuals.

[15] An alternative design is to randomly assign the role color to each player in each round. However, the advantage of using fixed role is twofold. First, to exclude the possibility that subjects adopt a strategy that maximizes their overall payoffs. For example, in the cooperative environment, it is possible that the minority subjects converge to the conforming equilibrium because they expect to receive a majority role color in the other rounds. Second, since the reason to implement repeated rounds is to allow for learning, assigning role color in each round would reduce the possibility of learning.

4.4.3 Hypotheses

According to the theoretical analysis in section 3, the predicted strategies are presented in Table 4.2. Note that with the exception of treatment L-comp, all treatments consist pure-strategy predictions. In treatment L-comp, the prediction is that both types of players mix between two pure strategies, and the corresponding probability distribution of the strategies is included in Table 4.2.[16] Denote r as the ratio of the predicted payoff of the Red players to the predicted payoff of the Blue players. By implementing the predicted strategies, the ratio r can be obtained. For each type of players in each treatment, the equilibrium payoff ratio r is presented in the last column of Table 4.2. This gives rise to two hypotheses that directly tackles the questions of this chapter.

Table 4.2: Predictions Overview

Treatments	Blue players	Red players	r
S-comp	0.1	0	0.4
L-comp	$(0.1 : \frac{1}{5}, 1 : \frac{4}{5})$	$(0 : \frac{4}{7}, 1 : \frac{3}{7})$	0.71
S-coop	1	1	2.45
L-coop	1 or 0	1	2.1 or 1.5

Notes: This table shows the predicted decisions for each type of player. The cell entries show the predicted proportion spent on skill red. In L-comp, decisions are in mixed-strategy format, in which the probability is presented next to the strategy.

1. Payoff inequality: the Blue players earn a higher (lower) payoff in treatments S-comp and L-comp (S-coop and W-coop) compared to the Red players.

2. Relative size effect: treatment S-comp (S-coop) yields a bigger payoff inequality than treatment L-comp (L-coop).

4.4.4 Procedures

The experiment was conducted in the CREED laboratory of the University of Amsterdam in May 2015. In total, 312 subjects participated in the experiment, of which 24 subjects participated in the pilot and 72 subjects participated in each of the four treatments.[17] Subjects were

[16]The calculation procedure is provided in Appendix 4.C.

[17]In the pilot session, treatment L-coop was ran with an alternative design, where subjects were not provided with the decisions overview table. However, learning hardly took place: decisions did not converge to the prediction even till the last round. It implied that with this setup, subjects may not be able to select any equilibrium. The rest of the sessions were ran with providing the decisions overview tables. Therefore, the 24 subjects in the pilot session were excluded from the analysis.

recruited from the CREED database, which consists mostly of undergraduate students from various fields of studies. Of the subjects in my experiment, 56% were female, and approximately 68% were majoring in economics or business. Every subject received 7 euros show-up fee in addition to her earnings in the experiment. During the experiment, 'point' was used as currency. These points were exchanged to euros at the end of each session at an exchange rate of 50 points per euro. The experiment lasted between 50 to 70 minutes with average of one hour; the earnings varied between 9.7 to 24.7 euros with average of 16 euros.

The experiment was computerized using PHP/MySQL and was conducted in English.[18] After all subjects arrived at the laboratory, each was randomly assigned to a cubicle. Once everyone was seated instructions appeared on their screen. Subjects had to answer control questions to make sure that they fully understood the instructions. Communication between subjects was prohibited during the experiment. In case of questions, subjects raised hand and the experimenter answered them privately. After everyone had successfully answered the control questions, a printed summary of the instructions was distributed. After distributing the summary handouts to everyone, the experimenter announced and started the experiment. At the end of each session, subjects answered a short questionnaire and were subsequently paid their earnings privately. The same experimenter was always present in the experiment for all sessions.

In each session of the experiment, either 12 or 24 subjects participated, which formed either 1 or 2 matching groups. Subjects kept their role color and stayed in the same matching group throughout the entire session. In each round, matching is random and independent, i.e. the probability of meeting any of the other 11 players in the matching group is the same in every round.

4.5 Results

This section first presents comparative statics predictions across treatments. Next, it turns to each treatment to investigate the degree to which the Nash equilibrium predictions are supported and, in case of multiple equilibria, which equilibrium is selected.

4.5.1 Comparative statics across treatments

In this section the focus is on how the payoff inequality varies with the treatments. Figure 4.3 presents in a bar graph the average total earnings of each type across treatments.[19] Recall that r is the ratio of the average payoff of the Red players to the average payoff of the Blue players.

[18]See Appendix 4.D for the experimental instructions and the summary handout.

[19]The unit is point. The maximum earnings are 30 points in each round and there are 30 rounds.

Table 4.3 presents the actual r and compares it with the predictions in each treatment. The actual r is calculated from the payoffs of all players in all rounds.

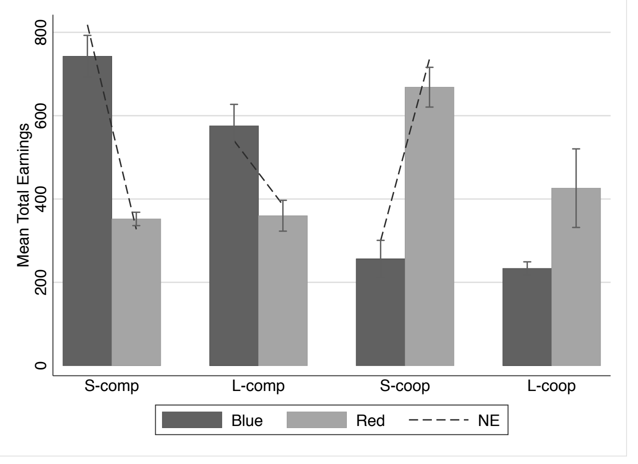

Figure 4.3: The figure shows the total earnings in points averaged over types for each treatment. In each treatment, the dark bar represents the earnings of the Blue players, the light bar represents the earnings of the Red players, and the dashed line connects the Nash predictions if applicable.

The payoff inequality in hypothesis (a) predicts that the Blue players earn more in the competitive treatments and earn less in the cooperative treatments compared to the Red players. This is supported by the results. In treatments S-comp and L-comp, the Blue players' earnings are significantly higher than the Red players' earnings (Wilcoxon signed-rank test, $p = 0.028$ in both treatments). In treatments S-coop and L-coop, the Blue players' earnings are significantly lower than the Red players' earnings (Wilcoxon signed-rank test, $p = 0.027$ (S-coop), $p = 0.028$ (L-coop)).

Table 4.3: Ratio of earnings (r)

	Actual	NE
S-comp	0.48 (0.02)	0.4
L-comp	0.63 (0.05)	0.71
S-coop	2.69 (0.23)	2.45
L-coop	1.84 (0.17)	2.1 or 1.5

Notes: Each cell in the middle two columns shows the actual ratio of earnings (standard errors in parentheses) between Red player and Blue players. Each cell in the right column presents the ratio of earnings in Nash equilibrium.

The relative size effect in hypothesis (b) predicts that the payoff inequality is higher when the minority group is small. This is supported by the results. In the competitive treatments, the Red players earn 352 points in S-comp and 359 points in L-comp; the Blue players earn 743 points in S-comp and 576 points in L-comp. The payoff difference between the two types of players is larger in L-comp than in S-comp (Mann-Whitney test, $p = 0.010$). In cooperative treatments, the Red players earn 668 points in S-coop and 426 points in L-coop; the Blue players earn 256 points in S-coop and 233 points in L-coop. Again, the payoff difference between the two types of players is larger in S-coop than in L-coop (Mann-Whitney test, $p = 0.004$).

Next, the actual payoff ratio r is compared to the model's predictions. It can be seen from Table 4.3 that in treatment S-comp and S-coop, when there is a unique pure-strategy equilibrium, the actual ratio is close to the equilibrium. In the last treatment, when there are multiple equilibria, the actual ratio lies between the two predictions.

4.5.2 Within treatment analysis

Figures IV-VII present the over-rounds average decisions of the Blue players and the Red players in each matching group. The decisions are the proportion of endowment spent on skill red. These figures provide both actual decisions of the subjects and the model's predictions. This makes it possible to see whether subjects' decisions converge to the predictions; and if not, in which direction they deviate from the predictions. Table 4.4 presents the observed frequency of the equilibrium strategy chosen by subjects in treatments with unique pure-strategy predictions.

4.5.2.1 Treatment S-comp

The theoretical prediction is that the Red players spend everything on skill red and that the Blue players obtain one unit of skill red. As can be seen from Figure 4.4 the Red players tend to spend most of their endowments on skill red starting from early rounds. The Blue players tend to overspend on skill red compared to the prediction.[20] From Table 4.4, we can see that the Red players use the equilibrium strategy 9 out of 10 times, and the Blue players use it well above chance.[21] Overall, the decisions are consistent with the prediction for Red players but not so for Blue players. Taking averages over all rounds and all matching groups, the Red players spent 93.8% on skill red and the Blue players spent 47.8% on skill red. Using Wilcoxon sign-rank test with the group average as unit of observation, it is found that the Red players spend a higher proportion on skill red than the Blue players ($p = 0.028$).

A major feature of the theoretical prediction is that the Blue players beat all the Red players: the predicted winning probability of Blue players is $9/11 = 81.8\%$. The actual winning

[20]In groups 1 and 3, decisions converge to the equilibrium over rounds. In groups 2 and 4, decisions stabilize substantially above the equilibrium. In groups 5 and 6, decisions do not seem to converge.

[21]Chance is 1 out of the 11 strategies, which is roughly 9.1%

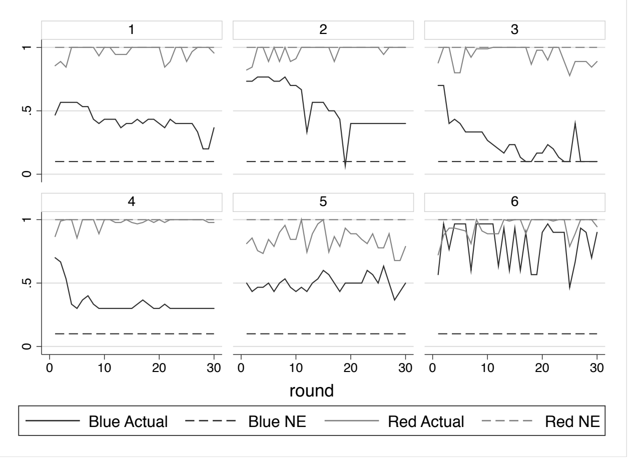

Figure 4.4: The figure shows the average fraction of endowment spent on skill red. The solid line represents the actual decisions of the subjects over rounds. The dashed line represents the predictions of Nash equilibrium. The dark lines represent the Blue players. The light lines represent the Red players. Each diagram shows the averaged decisions within one group.

probability of Blue players is 78.9%. On the other hand, the Red players win only 8.2% of all cases. This is consistent with hypothesis (a).

The observed frequency in Table 4.4 suggests that the Red players use the predicted choice more frequently than the Blue players. This result is not explained by the model as there is a unique equilibrium. One possible interpretation is that the Blue players learn little on how to play the strategy that maximizes their payoff since the matching between two Blue players rarely happen. Another possible interpretation is that a Blue player loses 10% on average, given the choices of all other players, by not playing the optimal strategy. On the other hand, a Red player may lose from 25% to all of their profit, given the choices of other players, by deviating from spending everything on skill red.

Table 4.4: Proportion of equilibrium strategy

	Red players		Blue players	
	all rounds	last 5 rounds	all rounds	last 5 rounds
S-comp	90.1	91.1	25.4	43.3
S-coop	84.5	97.0	73.9	88.9

Notes: Each cell shows the observed frequency (in percent) with which a subject chose the equilibrium strategy. If subjects chose the equilibrium strategy by chance, the frequency is 9.1 percent (1 out of 11 choices).

4.5.2.2 Treatment L-comp

The theoretical prediction is that there exists no pure-strategy Nash equilibrium, and all players use a mixed-strategy. Table 4.2 provides a benchmark prediction, in which the Blue players mix between spending either 10% or 100% on skill red and the Red players mix between spending either 0 or 100% on skill red. To provide an example of how individuals formulate strategies, decisions in one of the six matching groups is presented in Figure 4.5.

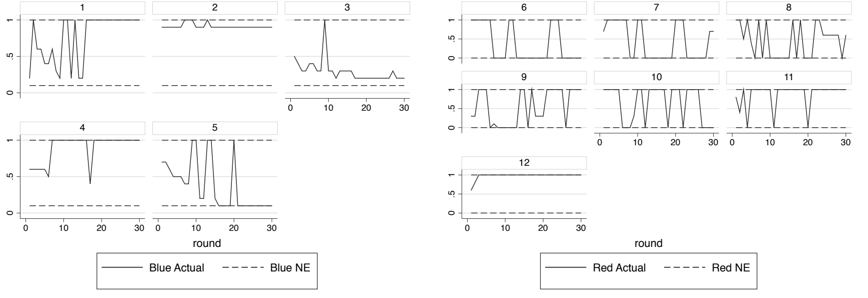

Figure 4.5: The figure shows individual decisions of endowment spent on skill red. The invididuals are within the same matching group: the left panel shows the decisions of the Blue players, the right panel shows the decisions of the Red players. The solid lines represent the actual decisions and the dashed line represent the predicted decisions in the mixed-strategy equilibrium.

As can be seen from the left panel, Blue players adopt different strategies. Players 3 and 5 converge to invest 10% on skill red, while players 1, 2 and 4 converge to invest 100% on skill red. On the other hand, we can see from the right panel that all the Red players mix between spending either 0 or 100% on skill red except for player 12. Overall, the Blue players manage to mix between the two predicted pure strategies at group level, while the majority of the Red players mix between the two predicted pure strategies at individual level. However, they fail to mix (at group level) the predicted strategies with the predicted probability distribution: both the Blue players and the Red players invest everything on skill red more often than predicted. As a result, Red players on average earn as much as 60% of Blue players, which is lower than the predicted ratio 71%. All together, this gives some support for the Nash predictions.

4.5.2.3 Treatment S-coop

The theoretical prediction is that both types spend everything on skill red. As can be seen from Figure 4.6 this equilibrium is selected in all matching groups. Both types converge to the equilibrium and after sufficient number of rounds. From Table 4.4, we can see that both types use the equilibrium strategy at least three fourths of all time. Taking averages of all matching groups, the Red players spent 94.5% on skill red and the Blue players spent 84.3% on skill red. The Wilcoxon sign-rank test rejects that the Red players spent a higher proportion on skill red than the Blue players ($p = 0.345$).

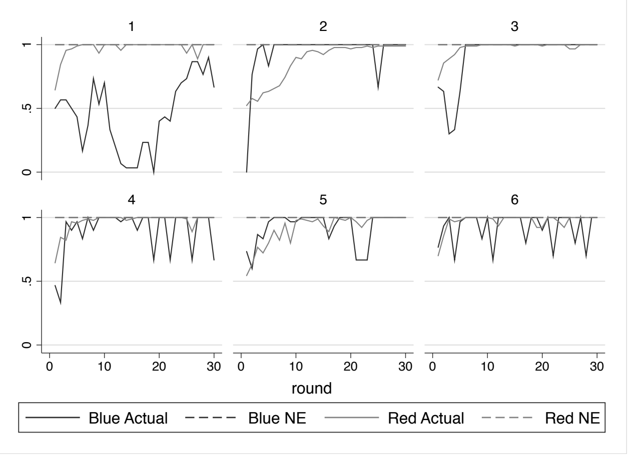

Figure 4.6: The figure shows the average fraction of endowment spent on skill red. The solid line represents the actual decisions of the subjects over rounds. The dashed line represents the averages of predictions in Nash equilibrium. The dark lines represent the Blue players. The light lines represent the Red players. Each diagram shows the averaged decisions within one group.

The observed frequency in Table 4.4 suggests that the Red players behave systemically better than the Blue players in playing the equilibrium strategy. This result is not explained by the model as there is only one unique equilibrium. One possible interpretation lies on the fairness concern due to the significant payoff inequality between types. Blue players may have an aversion to being disadvantaged as the minority type, and refuse to conform even if it is of their best interests to do so. It can be seen especially in the first group, where Blue players only conform until the last few rounds.

4.5.2.4 Treatment L-coop

The theoretical prediction consists of two equilibria. In the conforming equilibrium both types spend everything on skill red. In the segregating equilibrium both types spend everything on their own skill. To see whether a matching group select one of these two equilibria, a criterion is imposed with the following conditions: the conforming equilibrium is selected if (i) Blue players spent on average at least 75% on skill red and (ii) the equilibrium strategy is selected at least 40% of the time; the segregating equilibrium is selected if (i) Blue players spent on average at least 75% on skill blue and (ii) the equilibrium strategy is selected at least 40% of the time.

Using this criterion, as can be seen in Figure 4.7, the conforming equilibrium is selected by group 4. In this case, both types converge to the equilibrium, but the Red players converge faster and closer to the equilibrium than the Blue players. The segregating equilibrium is selected by groups 2 and 6. Here, both the Red players and the Blue players converge to the equilibrium after a sufficient number of rounds, while the Red players converge closer to the equilibrium. For the other three groups, it can be seen qualitatively that groups 1 and 5 start with a segregating pattern but turning to a conforming trend in the last few rounds; group 3 does not show a clear pattern for either of the two equilibria.

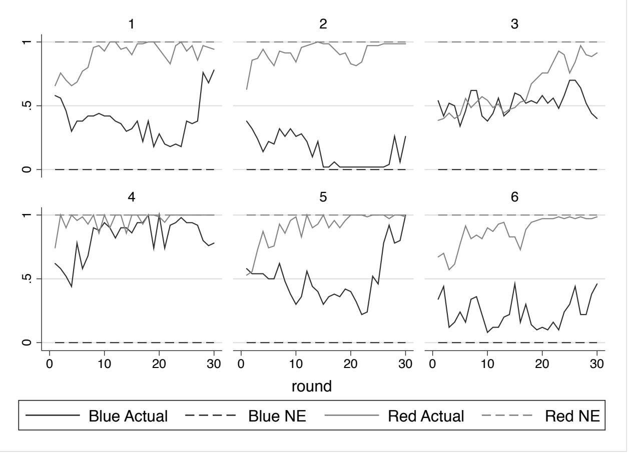

Figure 4.7: The figure shows the average fraction of endowments spent on skill red. The solid line represents the actual decisions of the subjects over rounds. The dashed line represents the predictions of Nash equilibrium. The dark lines represent the Blue players. The light lines represent the Red players. Each diagram shows the averaged decisions within one group.

One interest of this treatment is to see which equilibrium is selected among multiple equilibria. Surprisingly, none of the equilibrium seems to be strictly favored over the other: both equilibria are selected, while the segregating equilibrium is selected slightly more often than the conforming equilibrium.[22] It implies that the segregating equilibrium, which only arises when the minority group is large, is of empirical importance. It further suggests that efficiency fails to become a primary factor in the selection of the equilibrium, as segregating equilibrium is less efficient than conforming equilibrium.

[22]Since there are only six matching groups and the frequency of each equilibrium being selected is sensitive to the criterion, I would not claim that one equilibrium is favored over the other.

4.6 Discussion

When and why do minority groups have an advantage or a disadvantage? In this study I offer a new mechanism behind the formation of majority-minority inequality by testing a skill investment model in the laboratory. It is shown that in the competitive environment, members of the majority group invest predominately in beating their in-group members, which leads to a winning position for the minority group members. In the cooperative environment, by contrast, members of the minority group maximize profit at the cost of conforming to the majority group, which puts them at a disadvantaged position.

What are the drivers of the results? The experimental design makes it possible to disentangle behavioral factors from strategic reasons. In the competitive environment, the majority spends nothing on the out-group skills only when it is strategically optimal to do so; they start to invest a sufficient amount on the out-group skills when the share of the minority group is large. This can only be attributed to strategic reasons. On the other hand, members of the minority learn to converge to the equilibrium strategy but hardly reach it, possibly because they do not lose much by not playing optimally. In the cooperative environment, the contrast is clear: the segregating equilibrium is selected only when the minority group is large. This is again explained by strategic reasons; that is, when it is strategically beneficial to stay segregate, the minority groups are able to resist conforming. Finally, according to the observed large payoff inequality, inequality aversion does not seem to have a bite in explaining the experimental results.

What are the welfare implications of the results? The Pareto efficiency criterion is silent about which equilibrium, in the cooperative environment, is better; the conforming equilibrium and the segregating equilibrium cannot be ranked on this criterion. From the perspective of total efficiency in society, one would rather conclude that when there are two (or more) population types, society is well served with conformity towards the majority group. This holds because conformity may efficiently solve the coordination problem of which type of skills to be invested in. For example, if two group speaking different languages are merged to a single society, in the most efficient equilibrium everyone learns the language of the bigger group. For another, most tools in our society are designed for right-handed majority and the lefties have to conform and use them in a right-handed way. However, the maximization of total welfare goes together with substantial social inequality. Therefore, some societies start to converge to the segregating equilibrium instead, despite its inefficiency. On the other hand, societies driven by efficiency may make the conforming equilibrium more focal by changing the beliefs of people.

Beyond the specific setting of the experiment, the results may help explain many daily life observations. For instance, a higher proportion of left-handers are seen in top ranks of many sports such as tennis, boxing, baseball and fencing, but not in sports such as golf or swimming (Hagemann, 2009). The difference between these two kinds of sports is that the first class involves direct interaction between two or more athletes, whereas the latter are individual sports. For another, left-handed people brought up in the western culture use their right hands to hold knives but use left hands to hold chopsticks in dining; the opposite is observed among left-handed people brought up in eastern culture. The difference between these two types of left-handed people lies in the dining culture they grow up with – they both conform to the majority habits in their culture.

Appendix 4.A Proofs

Proof of Proposition 4.1. For any $\epsilon \in (0, \frac{1}{3}]$, $x^* = 0$ and $y^* = 1$, a type θ ties with other θ and loses against all type τ, her payoff is $(1-\epsilon)\frac{v}{2}$; a type τ ties with other τ and beats all type θ, his payoff is $(1-\epsilon)v+\epsilon\frac{v}{2}$. If a type θ deviates to $x > x^*$, she will lose against other type θ, tie with or beat all type τ, she can gain a maximum of: $-(1-\epsilon)\frac{v}{2}+\epsilon v = (3\epsilon-1)\frac{v}{2} < 0$. If a type τ deviates to $y < y^*$, he will tie with all type θ and beat other τ, and gains $-(1-\epsilon)\frac{v}{2}+\epsilon\frac{v}{2} = (2\epsilon-1)\frac{v}{2} < 0$. If a type τ deviates to $y > y^*$, he still beats all type θ but loses against other τ, and this yields a strict loss. Thus, $x^* = 0$ and $y^* = 1$ is Nash equilibrium.

For any $\epsilon \in (0, \frac{1}{3}]$, suppose that there exists a pure-strategy equilibrium x and y, with $x \neq 0$ or $y \neq 1$. If $x > 0$, a type θ can gain by deviating to $x = 0$, as she will beat all other type θ, which ensures a gain of at least: $(1-\epsilon)\frac{v}{2} - \epsilon v = (1-3\epsilon)\frac{v}{2} > 0$. If $x = 0$ and $y = 0$, a type τ can gain by deviating to $y = 1$, as he will beat all type θ and lose against other type τ, which yields a strict gain of $(1-2\epsilon)\frac{v}{2}$. If $x = 0$ and $y > 1$, a type τ can gain by deviating to $y = 1$, as he will still beat all type θ as well as other type τ, which yields a strict gain of $\epsilon\frac{v}{2}$. Thus, no other pure-strategy equilibrium exists.

For any $\epsilon \in (\frac{1}{3}, \frac{1}{2})$, suppose that there exists a pure-strategy equilibrium x and y. If $x < y < \bar{x}$, a type θ can gain by deviating to $y+1$:$\epsilon v - (1-\epsilon)\frac{v}{2} > 0$. If $0 < x < y = \bar{x}$, a type θ can gain by deviating to 0, as she will beat other type θ. If $0 = x < y = \bar{x}$, a type τ can gain by deviating to 1, as he will beat other type τ. If $x = y < \bar{x}$, a type τ can gain by deviating to $x+1$, as he will beat type θ. If $x = y = \bar{x}$, a type θ can gain by deviating to 0, as she will beat other type θ. If $y < x < \bar{x}$, a type τ can gain by deviating to $x+1$, as he will beat type θ. If $0 < y < x = \bar{x}$, a type τ can gain by deviating to 0, as he will beat other type τ. If $0 = y < x = \bar{x}$, a type θ can gain by deviating to $x-1$, as she will beat other type θ. Thus, no pure-strategy equilibrium exists.

Proof of Proposition 4.2. The Nash equilibria can be distinguished by the skills that is used by indviduals from the same group. There are four different cases:

Case I. Suppose that there exists pairs of x^* and y^*, such that two type θ individuals use their in-group skills to coordinate ($\omega - cx^* \geq x^*$), two type τ individuals use their out-group skills to coordinate ($\omega - cy^* \leq y^*$).

As $\omega - cx^* \geq x^*$ and $\omega - cy^* \leq y^*$, it follows that $min[x^*, \omega - cy^*] \leq min[\omega - cx^*, y^*]$. Therefore, a type θ and a type τ use type θ's in-group skills to coordinate and both receive $min[\omega - cx^*, y^*]$.

In equilibrium, any unilateral deviation yields the same or a lower payoff. Consider deviation $x > x^*$, it yields a lower payoff when meeting type θ and no gain when meeting type τ. Consider deviation $x < x^*$, it yields equal payoff when meeting type θ and no gain when meeting type τ if $\omega - cx^* \geq y^*$. Consider deviation $y > y^*$, it yields no gain when meeting θ if $y^* = \bar{x}$ or $y^* = \omega - cx^*$. Finally, deviation $y < y^*$ yields no gain when meeting θ or meeting τ. Together the following conditions are required for the equilibrium set:

$$\begin{cases} x^* \leq \frac{\omega}{c+1} \\ y^* = min(\omega - cx^*, \bar{x}) \end{cases} \tag{4.6}$$

Case II. Suppose that there exists pairs of x^* and y^*, such that two type θ individuals use their out-group skills to coordinate ($\omega - cx^* \leq x^*$), two type τ individuals use their in-group skills to coordinate ($\omega - cy^* \geq y^*$).

Mirroring case I, the equilibria set is characterized by the following conditions:

$$\begin{cases} x^* = min(\omega - cy^*, \bar{x}) \\ y^* \leq \frac{\omega}{c+1} \end{cases} \tag{4.7}$$

Case III. Suppose that there exists pairs of x^* and y^*, such that two type θ individuals use their in-group skills to coordinate ($\omega - cx^* \geq x^*$), two type τ individuals use their in-group skills to coordinate ($\omega - cy^* \geq y^*$).

In equilibrium, any unilateral deviation yields the same or a lower payoff. Consider deviation $x > x^*$, it yields a loss of $(1 - \epsilon)c\Delta x$ when meeting type θ and a maximal gain of $\epsilon\Delta x$ when meeting type τ, and the net is always negative. Consider deviation $x < x^*$, it yields equal payoff when meeting type θ and a loss when meeting type τ. Consider deviation $y > y^*$, it yields a gain of $(1 - \epsilon)\Delta y$ when meeting θ and a loss of $\epsilon c\Delta y$ when meeting type τ, and the net is non-positive if $\epsilon \geq \frac{1}{1+c}$. Finally, deviation $y < y^*$ yields equal payoff when meeting θ and meeting τ.

Together the following conditions are required for the equilibrium set:

$$\begin{cases} x^* \leq \frac{\omega}{c+1} \\ y^* \leq \frac{\omega}{c+1} \\ \epsilon \in [\frac{1}{1+c}, \frac{1}{2}) \end{cases} \tag{4.8}$$

Case IV. Suppose that there exists pairs of x^* and y^*, such that two type θ individuals use their out-group skills to coordinate ($\omega - cx^* \leq x^*$), two type τ individuals use their out-group skills to coordinate ($\omega - cy^* \leq y^*$).

In equilibrium, any unilateral deviation yields the same or a lower payoff. Consider deviation $x > x^*$, it yields equal payoff when meeting type θ and when meeting type τ. Consider deviation $x < x^*$, it yields a loss when meeting type θ and no gain when meeting type τ. Consider deviation $y > y^*$, it yields a loss when meeting θ and equal payoff when meeting type τ. Finally, deviation $y < y^*$ yields a gain of $(1-\epsilon)c\Delta y$ when meeting θ and a loss of $\epsilon\Delta x$ when meeting τ, the net is always positive.

Thus, no equilibrium exists in this case.

Proof of Proposition 4.3. By Definition 4.2, a pair of strategies consists a strict equilibrium if and only if any unilateral deviation yields a strict loss. This implies that, for equilibria characterized in sets (1)-(3), the ones in which unilateral deviations may yield the same payoff can be eliminated.

In set (1), for any $x^* > 0$, a type θ receives the same payoff by deviating to $x = 0$. For any $y^* < \bar{x}$, a type τ receives the same payoff by deviating to $y = \bar{x}$. Therefore, strict equilibrium consists only $x^* = 0$ and $y^* = \bar{x}$.

Similarly, in set (2), only $x^* = \bar{x}$ and $y^* = 0$ survives as a strict equilibrium.

In set (3), for any $x^* > 0$, a type θ receives the same payoff by deviating to $x = 0$. And for any $y^* > 0$, a type τ receives the same payoff by deviating to $y = 0$. Therefore, strict equilibrium consists only $x^* = y^* = 0$.

Appendix 4.B Small population size

This section discusses the robustness of the model with small population size. Note that if the population size is large, the probability that one meets oneself is negligible. If the population size is small, on the other hand, the probability to meet each type of individual is affected by one's own type. The population state is defined by both n and ϵ. The matching probabilities for a population state (n, ϵ) are presented in the following equations.

$$\begin{cases} Pr[\tau|\theta, n, \epsilon] = \epsilon\frac{n}{n-1} \\ Pr[\theta|\theta, n, \epsilon] = (1 - \epsilon - \frac{1}{n})\frac{n}{n-1} \\ Pr[\theta|\tau, n, \epsilon] = (1 - \epsilon)\frac{n}{n-1} \\ Pr[\tau|\tau, n, \epsilon] = (\epsilon - \frac{1}{n})\frac{n}{n-1} \end{cases}$$

This modifies Propositions 4.1, 4.2 and 4.3 regarding the tie-breaking population share. Recall that in Proposition 1, the tie-breaking point is $\frac{1}{3}$. This point occurs when a type θ finds it profitable to deviate from $x^* = 0$. The equilibrium payoff at $x^* = 0$ and $y^* = 1$ can be derived using the probability system: $\frac{1}{2}(1 - \epsilon\frac{n}{n-1})$. The maximum payoff by deviating is achieved when a type θ beats all type τ, which is equal to $\epsilon\frac{n}{n-1}$. By equalizing these two payoffs, the tie-breaking point is obtained as $\frac{n-1}{3n}$. Note that this number approaches $\frac{1}{3}$ as n increases, and can be approximated by $\frac{1}{3}$ for a fairly large n.

Similarly, in Propositions 2 and 3, the tie-breaking population share occurs when a type τ finds it unprofitable to deviate in the segregating equilibrium ($x^* = y^* = 0$). Using the matching probability, the payoff of a type τ in the segregating equilibrium is equal to $(\epsilon - \frac{1}{n})\frac{n}{n-1}\omega$, and the maximum deviation payoff is achieved when a type τ deviates to $y = \bar{x}$, which yields $(\epsilon - \frac{1}{n})\frac{n}{n-1}\omega + y\frac{n}{n-1}$. By equalizing these two payoffs, the tie-breaking population share is obtained: $\frac{n+c}{nc+n}$. Again, when n is sufficiently large, this point can be approximated by $\frac{1}{1+c}$.

The rest predictions of the model hold for small population size.

Appendix 4.C Experimental predictions

In the experimental setup, the parameter values are: $\omega = 30$, $c = 3$, $\bar{x} = \frac{\omega}{c} = 10$, $n = 12$, $v = 30$, and the size of the minority group is 3 or 5. The detailed calculation for the experimental predictions are presented for each treatment.

Competitive environment. In treatment S-comp, the population state yields a unique Nash equilibrium $x^* = 0$ and $y^* = 1$. In equilibrium, the minority type receives a higher payoff than the majority type, and the payoff ratio $\frac{\Pi^*_\theta}{\Pi^*_\tau} = 0.4$.

In treatment L-comp, the population state yields a mixed-strategy Nash equilibrium. The mixed-strategy Nash equilibrium can be derived from the pure-strategy Nash equilibrium in S-comp. As $x = 0$ is no longer an equilibrium strategy for members of the majority, they are motivated to mix $x = 0$ with $x = 10$ to win against members of the minority. As a result, $y = 1$ is no longer an equilibrium strategy by members of the minority, and they are motivated to win against or tie with members of the majority by mixing $y = 1$ with $y = 10$.

In such a mixed-strategy Nash equilibrium, players mix between two pure strategies. To compute this Nash equilibrium, probability $(p_0, 1 - p_0)$ is assigned to strategies $x = 0, x = 10$ for the majority type, probability $(q_0, 1 - q_0)$ is assigned to strategies $y = 1, y = 10$ to the minority type. This strategy pair is a mixed-strategy Nash equilibrium if it satisfies the following conditions: (1) $x = 0$ and $x = 10$ yields the same expected payoff, while all other strategies yield a lower payoff (2) $y = 1$ and $y = 10$ yields the same expected payoff, while all other strategies yield a lower payoff.

These conditions leads to the mixed-strategy Nash equilibrium $x^* = (0 : \frac{3}{7}, 10 : \frac{4}{7})$, $y^* = (1 : \frac{1}{5}, 10 : \frac{4}{5})$. In this Nash equilibrium, the minority type receives a higher payoff than the majority type, and the payoff ratio $\frac{\Pi^*_\theta}{\Pi^*_\tau} = 0.71$. This equilibrium is used as the benchmark theoretical predictions for the experiment.

Cooperative environment. In treatment S-coop, the population state yields a conforming equilibrium $x^* = 0$ and $y^* = 10$. In equilibrium, the minority type receives a lower payoff than the majority type, and the payoff ratio $\frac{\Pi^*_\theta}{\Pi^*_\tau} = 2.45$.

In treatment L-coop, the population state allows another segregating equilibrium $x^* = y^* = 0$. In this equilibrium, the minority receives a lower payoff than the majority type, and the payoff ratio $\frac{\Pi^*_\theta}{\Pi^*_\tau} = 1.5$. In the conforming equilibrium $x^* = 0$, $y^* = 10$, the payoff ratio is 2.1.

The above illustration serves the comparative statics for the experiment and the results provide the theoretical predictions for the experiment.

Appendix 4.D Experiment instructions

WELCOME PAGE

Welcome to this experiment on decision-making. You will be paid €7 for your participation plus what you earn in the experiment. During the experiment you are not allowed to communicate with each other. If you have any question at any time, please raise your hand. An experimenter will assist you privately. In this experiment you will make a number of decisions. Your earnings depend on your own decisions and the decisions of other participants. During the experiment, all earnings are denoted in points. Your earnings in points are the sum of the payoffs in every round. At the end of the experiment, your earnings will be converted to euros at the rate: 1 point = €0.02. Hence, 50 points are equal to 1 euro. Your earnings will be privately paid to you in cash.

INSTRUCTIONS PAGE 1

The instructions are given in 2 pages. While reading them, you will be able to go back and forth by using the menu on top of the screen. A summary of these instructions will be distributed before the experiment starts.

Roles and Rounds At the beginning of the experiment, each participant will be randomly assigned to a role denoted by a color: Blue player or Red player. These roles will remain fixed throughout the experiment. For example, if you are assigned the color Blue, you will be a Blue player in each round of the experiment. Participants are divided into groups. Each group has 12 players. Among these 12 players, 3 are Blue players and 9 are Red players. Your group will stay the same throughout the experiment. The experiment consists of 30 rounds. In each round, these 12 players are randomly paired. The pairing is completely random in each round. Thus, in each round, one player is equally likely to be paired with any of the other 11 players in the group. At the end of each round, you will receive feedback about the role and the decision of the other player, your earnings and the group decisions overview in that round.

Decisions In each round, you will be asked to make one decision. The decision is about how to train your skills. There are two types of skills: skill blue and skill red. At the beginning of each round, each player receives an endowment of 30 skill points. This endowment will be the same in each round and for every player. You decide how to allocate the points on skill blue and skill red. The skill points associated with a given skill indicates how difficult it is to train that is, how many points it will require to reach each level. The skill points per level are given below.

- Skill blue: 1 point for Blue players; 3 points for Red players
- Skill red: 1 point for Red players; 3 points for Blue players

For example, if you are a Blue player, the points required to train skill blue is 1 point per level; the points required to train skill red is 3 points per level that is, it is more difficult for you to train the skill that is different from your color.

You must spend your entire endowment to train these two skills. It is completely up to you how many levels you want to reach for either of the two. For example, you may spend the entire points in skill blue, or the entire points in skill red, or any possible combination of the two. During the experiment, you will be given a list of all the possible choices. As soon as everyone has finished making a decision, you will be randomly paired with someone from the group and the payoff will be determined.

INSTRUCTIONS PAGE 2 (COMPETITIVE TREATMENTS)

Each player has two skills: blue and red. These skills are used in different situations. Skill blue is used when the player is paired with a Blue player; skill red is used when the player is paired with a Red player. Two players in a pair compete for a prize of 30 points. The winner receives the entire 30 points and the loser receives nothing. In case of a tie, both players receive half of the prize =15 points. We will now describe how it works.

- If two Blue players are in a pair, their skills blue are compared; whoever has the higher level wins. If the skill levels are equal, it is a tie.
- If two Red players are in a pair, their skills red are compared; whoever has the higher level wins. If the skill levels are equal, it is a tie.
- If a Blue player and a Red player are in a pair, the Blue players skill red and the Red players skill blue are compared; whoever has the higher level wins. If the skill levels are equal, it is a tie.

Examples (notice that all the numbers in the examples are randomly chosen and do not provide any indication about how to play the game):
Two Blue players in a pair. The first (skill blue: level 12, skill red: level 6) The second (skill blue: level 3, skill red: level 9) The first has the higher level skills blue than the second ($12 > 3$), and so the first receives 30 points, the second receives nothing.
One Blue player and one Red player in a pair. The Blue player (skill blue: level 12, skill red: level 6) The Red player (skill blue: level 3, skill red: level 21) The Blue player has the higher level skills red than the Red players skill blue ($6 > 3$), and so the Blue player receives 30 points, the Red player receives nothing.
One Blue player and one Red player in a pair. The Blue player has (skill blue: level 12, skill red: level 6) The Red player has (skill blue: level 6, skill red: level 12) The Blue players has the same level skill red as the Red players skill blue ($6 = 6$), and so both players receive half of the prize =15 points.

INSTRUCTIONS PAGE 2 (COOPERATIVE TREATMENTS)

Each player has two skills: blue and red. The two skills have different purposes. Skill blue is only used to work on project blue; skill red is only used to work on project red.Two players in a pair work on one of the two projects: blue or red. The outcome of project blue is equal to the minimum of the two skills blue. The outcome of project red is equal to the minimum of the two skills red. We will now describe the payoffs.

- If project blue yields a better outcome than project red, each player receives payoff = minimum of the skills blue;
- If project red yields a better outcome than project blue, each player receives payoff = minimum of the skills red;

- If project blue yields the same outcome as project red, each player receives payoff = minimum of the skills blue = minimum of the skills red.

Examples (notice that all the numbers in the examples are randomly chosen and do not provide any indication about how to play the game):
Two Blue players in a pair. The first (skill blue: level 12, skill red: level 6) The second (skill blue: level 3, skill red: level 9) Project blue yields outcome 3 (min of 12 and 3) and project red yields outcome 6 (min of 6 and 9), and so the two players work on red and each receives 6 points.
One Blue player and one Red player in a pair. The Blue player (skill blue: level 12, skill red: level 6) The Red player (skill blue: level 3, skill red: level 21) Project blue yields outcome 3 (min of 12 and 3) and project red yields outcome 6 (min of 6 and 21), and so the two players work on red and each receives 6 points.
One Blue player and one Red player in a pair. The Blue player has (skill blue: level 12, skill red: level 6) The Red player has (skill blue: level 6, skill red: level 12) Project blue yields outcome 6 (min of 12 and 6) and project red yields outcome 6 (min of 6 and 12), and so the two players work on blue or red and each receives 6 points.

Chapter 5

Summary

Understanding cooperation, coordination and competition is among the biggest challenges in our society. This thesis explores how cooperation, coordination and competition are influenced by communication and population structure. This thesis present the results of three independent studies that all make use of laboratory experiments.

In Chapter 2 we investigate why face-to-face communication induces cooperation in social dilemmas. We conduct a series of experiments to disentangle the effect of three mechanisms: social identification, type detection and commitment value. Subjects in all treatments play a one-shot prisoners dilemma game. We manipulate their opportunity to communication before making decisions: In No communication, subjects meet in silence only after making their decisions. In Silent communication, subjects meet in silence before making their decisions. In Restricted communication, subjects can talk without making a promise before making their decisions. In Unrestricted communication, subjects can talk freely before making their decisions. The idea here is that by increasing the degree of communication from No communication to Unrestricted communication, we are able to investigate how much each of the three factors contributes to the effect of communication.

The experimental results show that communication substantially increases cooperation from No communication to Unrestricted communication. However, Silent communication only slightly increases the cooperation rate compared to No communication, and the difference is not significant. The cooperation rate in Restricted communication is significantly higher than No communication, but falls well below Unrestricted communication. We control for the beliefs reported by subjects both before and after they communicate. We find that type detection and commitment value plays an important role in explaining the communication gap, whereas there is no evidence for social identification. This result departs from the literature, as it first decomposes the relative importance of each factor. It suggests the importance of type detection in other settings.

In Chapter 3 we investigate when and why pre-play sequential communication improves coordination in coordination games with conflicting interests. In the game-theoretic model, players can send messages in turn before making decisions in the coordination games. Players bear a small cost for each message in the conversation. We show that in the battle of the sexes game, in equilibrium communication always induces complete coordination. In Chicken games, by contrast, communication fails to affect coordination or efficiency. According to the theory, the reason is that in the battle of the sexes game, players rather agree on the equilibrium that is most preferred by the other player than having no agreement. In Chicken games, players prefer to play the mixed strategy equilibrium without agreement rather than to agree on the equilibrium most preferred by the other player.

We conduct experiments on mixed motive games in which we vary the size of the "disagreement" payoffs across treatments. One of the three games is the battle of the sexes and the other two are Chicken games. Subjects play one of the three games both with and without pre-play sequential communication. In agreement with the theory, we find that communication is very effective in the battle of the sexes, but not at all in the Chicken variations. Moreover, the first senders have a big advantage; they are three times as likely to achieve their preferred outcomes as the second senders. In Chicken games, messages are much less credible than in the battle of the sexes. The model predicts that without communication, increasing the "disagreement" payoffs always makes subjects better off. By contrast, with the option to communicate, increasing the "disagreement" payoffs can make subjects worse off on average, as it makes communication ineffective. This prediction is supported by the experimental data.

Finally, Chapter 4 explores the sources of majority-minority inequality in cooperative and competitive environments. We use a game-theoretic model to capture individual decisions in how to allocate time between investing different skills. These skills are used differently depending on the role of the opponent in social interactions. In the competitive environment, the skills are used to compete against the opponent. In the cooperative environment, the skills are used to cooperate with the opponent. An interesting feature of this model is that members of the minority group enjoy a higher surplus in the competitive situation but a lower surplus in the cooperative situation. These results are endogenously driven by the asymmetry of the population structure.

The experiment implements the model in a straightforward manner. In a 2x2 design, one dimension concerns the environment, which is competitive or cooperative, and the other dimension concerns the relative size of the two groups. To a large extent, the experimental results agree with the predictions of the model. That is, members of the minority group earn more in competitive environments compared to members of the majority group, but the reverse holds in cooperative environments. Besides, when the size gap of the two groups is bigger, the payoff difference also increases. The intuition is that subjects invest more toward the majority group and this leads to a minority advantage in the competitive situation but a minority disadvantage in the cooperative situation. When the relative size of the two groups is enhanced, this advantage or disadvantage is further increased.

To sum up, this thesis demonstrates that communication (Chapter 2 and 3) and population structure (Chapter 4) has a fundamental influence on how people cooperate, coordinate or compete with each other.

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Samenvatting (Summary in Dutch)

Het begrijpen van samenwerking, coördinatie en concurrentie is een van de grootste uitdagingen voor onze maatschappij. Mijn onderzoek, gebaseerd op laboratorium experimenten, laat zien wat de invloed is van communicatie en samenstelling van de bevolking is in deze drie fenomenen.

In hoofdstuk 2 onderzoeken we waarom face-to-face communicatie leidt tot meer samenwerking. In de literatuur worden drie mogelijke mechanismen genoemd: sociale identificatie, weten met welk type je communiceert en de mogelijkheid van commitment. In ons onderzoek onderzoeken we de waarde van elk van de genoemde mechanismen. In alle treatments spelen de deelnemers een one-shot prisoners dilemma. Hierbij worden de mogelijkheden om te communiceren voordat een beslissing wordt genomen gemanipuleerd. In de 'No communication' treatment zien de deelnemers met wie ze te maken hebben na het nemen van de beslissing en is geen mogelijkheid tot communicatie. In 'Silent Communication' zien ze elkaar voor de beslissing, maar kunnen ze nog steeds niet met elkaar communiceren. In 'Restricted communication', mogen de deelnemers weliswaar vrij vooraf communiceren, maar is het hen niet toegestaan om zich te committeren. In 'Unrestricted communication' is het ook mogelijk zichzelf vast te leggen. Door de mogelijkheden tot communicatie geleidelijk te verruimen van 'No communication' naar 'Unrestricted communication', onderzoeken we of en in welke mate genoemde mechanismen bijdragen aan het tot stand komen van de samenwerking.

Het experiment laat zien dat samenwerking toeneemt met de mate waarin de mogelijkheid tot communicatie toeneemt. Echter is het niet zo dat elke stap evenveel bijdraagt. Zo is er geen significant verschil tussen de samenwerking in 'No communication' en 'Silent communication'. In 'Restricted communication' is de mate van samenwerking significant hoger dan in 'No communication', maar belangrijk minder dan in 'Unrestricted communication'. Als we controleren voor wat de deelnemers denken voor en na de communicatie dan blijkt dat het kunnen zien met welk type wordt gecommuniceerd en de mogelijkheid van commitment een belangrijke rol speelt in de verklaring van genoemde verschillen. Daarentegen speelt sociale identificatie geen rol. Dit resultaat verrijkt de literatuur door het relatieve belang van iedere factor uit te splitsen en suggereert daarnaast dat ontdekking van het type van de persoon waarmee gecommuniceerd wordt ook in andere settingen van belang kan zijn.

In hoofdstuk 3 kijken we naar bilaterale en sequentiële communicatie voordat een beslissing wordt genomen een in coördinatie game met tegenstrijdige belangen voor de verschillende spelers. In het model dat we gebruiken om voorspellingen te doen, sturen de deelnemers om de beurt een boodschap voordat ze een coördinatie game spelen. Hierbij kost het de deelnemers geld om een boodschap te versturen. We zien dat in de zogenoemde battle of de sexes deze sequentilee communicatie zou moeten leiden tot perfecte communicatie terwijl dat in de chicken game niet bijdraagt tot een betere coördinatie. Volgens de theorie kiezen de deelnemers in het eerste geval liever voor een uitkomst die gunstig is voor de andere partij dan voor een evenwicht waarbij ze hun keuzes randomiseren en bij de chicken game is de voorspelling juist andersom.

In ons experiment laten we de deelnemers drie verschillende games spelen. Een battle of de sexen en twee chicken games. Deelnemers spelen een van de drie games zowel met als zonder communicatie. In overeenstemming met de theorie zien we dat die communicatie substantieel bijdraagt aan coördinatie in de battle of de sexen en totaal niet aan de coördinatie in de chicken games. Verder zien we dat de speler die het eerste een bericht mag versturen een groot voordeel heeft boven de andere speler; deze first-mover is driemaal vaker in staat om de voor hem/haar beste uitkomst binnen te slepen dan de second-mover. Een andere voorspelling van het model is dat het vergroten van het verschil in potentiele verdiensten tussen de spelers een negatief effect heeft op de coördinatie.

Ten slotte, onderzoeken we in hoofdstuk 4 de bron van ongelijkheid tussen meerderheden en minderheden zowel in een omgeving gericht op samenwerking en een omgeving gericht op concurrentie. Hiertoe wordt met behulp van een spel theoretisch model een experiment ontwikkeld waarin gekeken wordt hoe deelnemers verschillende vaardigheden inzetten om hun doel te bereiken. Het model voorspelt dat deze vaardigheden verschillend worden ingezet afhankelijk of de setting is gericht is op concurrentie dan een setting die gericht is op samenwerking. Uit het model volgt dat minderheden het beter doen in een competitieve setting en meerderheden het beter doen in een setting gericht op concurrentie.

Het experiment implementeert het model een op een. In een 2x2 ontwerp, beschrijft de ene dimensie de setting (samenwerking of concurrentie) en de andere dimensie de relatieve grootte van de minderheid. De resultaten van het onderzoek bevestigen voor een groot deel het model. Dus leden van de minderheid doen het relatief beter in een competitieve omgeving en de leden van de meerderheid doen het relatief beter in een setting gericht op samenwerking. Daarnaast zien we dat als het verschil in aantal tussen de groepen groter wordt de effecten sterker worden. Een verklaring zou kunnen zijn dat de deelnemers meer investeren in de meerderheidsgroep en dat leidt tot een voordeel voor de minderheidsgroep in een concurrentie setting en een nadeel als samenwerking centraal staat.

Samenvattend, dit proefschrift laat zien dat communicatie en populatiesstructuur van fundamentele invloed zijn op de manier waarop mensen met elkaar samenwerken, coördineren en concurreren.

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