The Effect of Communication on Economic Behavior—Experimental Evidence and Methodological Reflections

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1. Overview

"First of all, there is a property of behavior that could hardly be more basic and is, therefore, often overlooked: behavior has no opposite. In other words, there is no such thing as nonbehavior or, to put it even more simply:
one cannot not behave*.*
Now, if it is accepted that all behavior in an interactional situation has message value, i.e., is communication, it follows that no matter how one may try,
one cannot not communicate*."*
Watzlawick et al. (1967, pp. 48–49)

1.1. Introduction

Since at least the 1960s it became apparent how much behavior and communication are interwoven. As the quote above from Watzlawick et al. (1967, pp. 48–49) briefly illustrates: wherever there is one, there is the other. From the perspective of economics in general and laboratory experiments in particular, communication is often used as a means to an end. That is, communication is used to induce specific behavior by e.g. enabling cooperation between agents (Brosig et al., 2003). This dissertation contributes to this general framework of research on communication in experimental economics. Thus, the questions which overarch the research of this thesis can be summarized as follows: How does communication affect human behavior? Does communication enhance cooperation? What elements of communication can be used to predict cooperation in a public goods experiment? Can communication barriers drive a wedge between people in bargaining situations? Furthermore, in addition to this content-related type of questions, the thesis tackles a recently emerging methodological issue: How can face-to-face communication be measured in order to be controlled for, as it is required in laboratory experiments? This is especially important since face-to-face communication is an integral part of our life yet is difficult to measure. This problem potentially limits the applicability and generalization of laboratory findings.

In order to tackle the questions raised, the first chapter of this thesis first introduces a short overview of relevant historical developments in communication research and experimental economics. Then some of the contemporary challenges in this research branch are summarized. The next paragraphs display how the objective of this thesis addresses these challenges. Subsequently, the laboratory experiments underlying the research are briefly introduced. As second to last, this chapter displays a new type of classification of communication in laboratory experiments. Finally, major findings of the following chapters of the thesis are briefly summarized.

Less than a century ago the central questions of this thesis would neither have had as much scientific background nor the contemporary relevance for two reasons: First, the technology (i.e. measurement devices and computing power) required for a thorough analysis of communication was not available at the time. Second, communication was not as omnipresent as it is in the 21st Century (e.g. video-telephony). That may be a reason why communication science, is a relatively young field of research – and so is experimental economics. Some of the first notable laboratory-type economic experiments are often traced back to Thurstone (1931), Chamberlin (1948), and Smith (1962).[1] However, after these early contributions it took until the late 1980s and 1990s for experimental economics to have a real breakthrough. Since then the numbers of research and publications involving laboratory experiments increased sharply as is stated in literature reviews on different types of economic experiments by e.g. Chaudhuri (2011) on public goods games, Güth and Kocher (2014) on the ultimatum games, and in the preface of the second volume of the seminal "Handbook of Experimental Economics" by Kagel and Roth (2016).

Similar to the origins of experimental economics, early communication models can be traced back to the same period. Lasswell (1948) provided a mostly informal model emphasizing the role of specific elements of communication being the communicator, message, medium, audience, and effect. Shannon and Weaver (1949) provided a more formal model of communication focusing on the sender, channel, and receiver. Schramm (1954) extended this linear model of Shannon and Weaver by avoiding a strict differentiation between sender and receiver. Instead both parties can function as receiver and sender, thus making the communication model a circular one. Berlo (1960) extended the model of Shannon and Weaver by emphasizing the role of the effect of communication. Starting from these simple models a new branch of literature emerged.

Comparable with laboratory experiments in economics, the analysis of communication became more prominent in science. Given the increasing relevance of these two research branches the combination of these became merely a matter of time. The day-to-day observation of

[1] This attribution is arguable, as is discussed in an overview of early economic experiments in Roth (1993). Thurstone is likely to be the first modern-day researcher to test economic theory with experimental methods. Chamberlin conducted a famous classroom experiment on markets and stressed what laboratory experiments can bring to economic research. Eventually, in 2002 Vernon Smith received the Nobel Memorial Prize in Economic Sciences for establishing laboratory experiments as a tool in empirical economic research.

communication in working groups quickly revealed the role of communication on cooperation and led to its analysis in laboratory experiments (Dawes et al., 1977; Isaac & Walker, 1988, 1991). Even before the rise of experimental economics in the 1980s and 1990s several researchers found communication to be a powerful tool to significantly increase cooperation: One of the most prominent examples for this observation illustrates the Prisoner's Dilemma which is basically a cooperation problem that can be tackled via communication. Early research provided by Loomis (1959), Deutsch (1960), Caldwell (1976), Dawes et al. (1977) and the meta-analysis by Sally (1995) supported this argument.

1.2. Challenges in the research on communication

Although there have been major developments in the research, communication is a highly complex process which is difficult to analyze. Foremost this refers to the issue that one and the same verbal statement can be understood differently with regards to its content by different individuals. In fact, the problem itself goes deeper since content is not the only source of information in communication. One of the most cited research on this topic and one whose message reached several popular science books was conducted by Mehrabian and Ferris (1967), Mehrabian and Wiener (1967), and Mehrabian (1971). They state that only 7% of the message are transferred via verbal communication, while the rest is transferred through facial expressions (55%) and voice (38%). Please note that these numbers were obtained from an experiment with specific focus on affective language, so no generalized remarks about communication should be done based upon them.

However, from the very beginnings of communication analysis it became apparent that individuals receive and interpret signals through body language and sound of the voice (Watzlawick et al., 1967). Similar to content, these channels of communication may have message value. Yet, they can be subject to misinterpretation, too. This induces a certain problem for laboratory experiments. One of the main advantages of laboratory experiments in economics is the ability to control all relevant factors as much as possible. This facilitates the basic groundwork of experimental research: to change only the one parameter of interest while keeping other variables constant. However, free face-to-face communication being often too complex to be analyzed in its entirety, collides with this agenda.

In fact, from the historic perspective several early economic experiments originally included communication, e.g. subjects saw and talked to each other prior to their decisions. The seminal experiment of Chamberlin (1948) is an example which, despite being groundbreaking at the time, would probably not live up to current methodological standards. In order to reduce the overlapping effects for many experiments, it was aimed to reduce the uncontrollable factors of communication to a minimum. This development went along with the foundation of experimental laboratories in economic research. In fact, many modern laboratories for economic research include soundproof boxes to separate the participants and avoid communication before and during the experiment, as was described in e.g. Weimann and Brosig-Koch (2019). On the one hand, it is therefore possible to reduce unintended communication between the participants of the experiments. On the other hand, it enables researchers to introduce communication stepwise into the respective experimental designs and to analyze it in a relatively controlled manner. The methodological developments accompanied the economic analysis of communication, starting from simple discussions by Loomis (1959), continuing with the more sophisticated experiments by Isaac and Walker (1988, 1991), Brosig et al. (2003) and Bochet et al. (2006) and eventually reaching its current state of the art analysis as in Penczynski (2019) and Andres et al. (2019) who use machine learning approaches to analyze text communication in laboratory experiments.

1.3. Communication in this thesis

This dissertation continues this tradition of laboratory experiments in economics with a focus on communication. Referring to the question how communication can enhance cooperation, this thesis shows the lasting effect of communication on behavior in a scenario where no further communication between agents is possible. Furthermore, it is analyzed how communication interacting with knowledge of the theoretical optima can drive a wedge between individuals. Referring to the issue of how communication in laboratory experiments can be measured, this thesis sheds light on the question whether the application of (formerly unavailable) technology, e.g. automatized facial expression detection, can support experimental economic research in the future. These questions are to be outlined in the following chapters.

In this dissertation communication takes on two different roles: it can be either a means or an end. In one part of this dissertation, communication is understood as a means to simulate an informal, behaviorally informed intervention which provides substantial gains in efficiency. In this context, it is important that the communication platform, comparable to potential behaviorally informed interventions in the field, does not affect the theoretical outcomes of the strategic behavior. Thus, the communication analyzed in the first part of this thesis was non-binding, unverifiable, and with one minor exception costless. Therefore, it complies with the traditional criteria of cheap talk (Farrell, 1987). It further complies with the more recent updates on the topic (Ying Chen et al., 2008; Kartik, 2009) since misbehavior basically cannot affect the reputation. In the other part, communication is analyzed directly in terms of what elements of communication (e.g. verbal or facial cues) can be used to predict cooperation behavior in an experiment and how these elements can be measured doing so. Two approaches shall be further distinguished: a traditional one and a recently emerging one. First, there is the analysis of the interaction between information and communication in a new setting of a multiplayer ultimatum game (Pirate Game), where different restrictions on communication are made. Second, the thesis illustrates how assessment methods like content analysis and automatic facial expression analysis can influence the future of laboratory experiments.

1.4. Experimental designs: Public Goods Game and Ultimatum Game

The following chapters go along with the general idea of discussing communication as a means *and* an end in experimental economics. By being a means to an end, communication is often introduced into a pre-existing design. This thesis makes use of two fundamental laboratory experimental designs in economics: The Public Goods Game and the Ultimatum Game. While the individual chapters will provide more specific information on previous research with respect to each of the research questions, the focus of the introduction is to simply display the general frameworks of these two experimental designs and present some stylized facts, starting with the public goods game.

Public Goods Game

At the very core of public goods games lies the mutual dilemma between individually and socially optimal behaviors. This design aims to simulate a commonly observed scenario when from the individual perspective it is beneficial to free-ride while hoping for others to contribute to the public good. Yet, when all members of the group free-ride, the outcome is severely worse than in case of cooperation. One of the most prominently addressed applications of a public goods game is tax compliance (Chaudhuri, 2011). A single individual would benefit by evading taxes and continuing to use e.g. the publicly provided infrastructure, education, or security. However, if nobody pays taxes these goods cannot be provided anymore. This dilemma was replicated countless times in laboratory experiments over the last decades as is indicated in prominent literature reviews by Ledyard (1995) and Chaudhuri (2011). In an utterly simplified manner the hitherto existing findings from experimental research can be summed up to several results: (i) On average individuals contribute between 40% and 60% of their endowment (Chaudhuri, 2011; Ledyard, 1995). (ii) The contributions decline over time towards the theoretical Nash Equilibrium, yielding a certain end-game effect (Andreoni, 1988). (iii) A large share of individuals act as conditional contributors, i.e. adjust their contributions depending on the contributions of other group members (Keser & Van Winden, 2000; Kocher et al., 2008). (iv) There is a variety of mechanisms that can induce the socially desired contribution behavior (Chaudhuri, 2011), with pre-play communication (Bochet et al., 2006; Brosig et al., 2003) being among these.

Ultimatum Game

In contrast to the public goods game, in the ultimatum game there is no fundamental social dilemma underlying the actual experiment. Rather, the ultimatum game focuses strongly on the inherent asymmetry of the task where the two individuals have two different types of decisive power: proposing the distribution of endowment (by a proposer) vs. accepting/rejecting the offer for both individuals simultaneously (by a receiver). So, on the contrary to the public goods game, the ultimatum game is not symmetrical for all players. Thereby, the experiment constitutes a basic decision situation, which under traditional assumptions would generate easily interpretable findings. However, the early results of ultimatum game experiments quickly illustrated the limits of traditional assumptions concerning rationality and financial selfishness (Güth et al., 1982) and the seemingly plain framework produced results that often puzzled researchers (Güth & Kocher, 2014). The most notable of these unexpected results are that neither the proposer nor the receiver act entirely rationally. The hitherto most relevant findings can be summed up as follows: (i) In the standard setup of the experiment average proposals lie approximately around 40-50% and are mostly accepted by the receivers (D. J. Cooper & Dutcher, 2011). (ii) The probability of acceptance decreases with smaller offers (Güth & Kocher, 2014). (iii) Communication has an ambivalent effect on proposals due to the interaction of strategic and social-affective processes (Zultan, 2012).

1.5. Dimensions of communication

With respect to communication being not only a means but also an end to experimental analysis it is first valuable to structure the vast literature on communication in laboratory experiments. The classification, however, shall not be based on the respective research topics but on the ways, communication takes place. This refers to the problem of communication being a highly versatile process which hardly can be held constant between different experimental treatments or experiments. One of the most robust findings is that communication affects different agents in different scenarios in different ways. This is supported by early literature (R. Cooper et al., 1992) and more recent analysis (Arechar et al., 2017; Cason et al., 2012, 2017; Ellingsen &

Östling, 2010). Even such a simple finding implies that the standard question of what has been communicated should be extended. Thus, the question arises what the general environment of the communication process is. This is tackled as early as by Lasswell (1948, p. 37) who simplified the description of communication to answering the question "Who says what in which channel to whom with what effect?" With respect to laboratory experiments Balliet (2010) briefly discusses three dimensions (communication medium, group size, and timing of communication) on which communication in laboratory experiments may differ. However, as the following sections will indicate, there are many more. In a simplified manner this paragraph shall briefly address some of the dimensions by which communication types can be defined and distinguished: (i) medium, (ii) direction, (iii) order, (iv) timing and frequency, (v) time restrictions (vi) content restrictions (vii) characteristics of participants (viii) number of participants (ix) endogeneity. In fact, this distinction is related to the recently published overview by Brandts et al. (2019). While that overview provides a more exhaustive and very recommendable literature overview on the topic, the following paragraphs indicate some additional dimensions worth investigating.

(i) *Medium:* Investigating the communication medium refers to distinguishing between e.g. text-messages, audio-communication, video-communication, or face-to-face communication. In the context of this thesis the latter is treated as synonymous with face-to-face communication using audio-video conference or in-person. These issues were investigated by e.g. Cason and Khan (1999), Brosig et al. (2003), and Bochet et al. (2006). Brosig et al. (2003) showed that in the context of a simple public goods game the differences between an audio-video conference and an in-person communication at a table did not yield significant differences. Supporting this idea, Bicchieri and Lev-On (2007) discussed that the closer the mediated communication reproduces the elements of an in-person FFC the more similar are the two effects. While in-person FFC may include some additional communication channels, i.e. body language (Van den Stock et al., 2007) or scent (Camps et al., 2014), these channels are not required to establish high levels of cooperation in simple experimental designs.

(ii) *Direction:* With respect to the direction of communication it is important to assess whether the communication is bi-directional or whether it operates in one direction only. In this context it further matters which type of agents act as sender and which as a receiver of information. Such differences become apparent analyzing the evidence obtained from dictator and ultimatum games by e.g. Rankin (2003), Xiao and Houser (2005, 2007), Ellingsen and Johannesson (2008), Andersson et al. (2010), and Capizzani et al. (2017). In the context of public goods game, Koukoumelis et al. (2012) illustrate how even one-directional communication substantially increases contributions.

(iii) *Order:* The next dimension, the order of communication, is highly related to the direction. In the context of economics, the problem can be related to a first-mover advantage/disadvantage. Thus, it may matter which party will start the communication (Ottaviani & Sørensen, 2001). Further, it may be valuable to distinguish between a simultaneously occurring communication e.g. Palfrey and Rosenthal (1991), Brosig et al. (2003) and a sequential one as in Valley et al. (2002).

(iv) *Timing and frequency:* Further, the timing of communication may be of importance as well. For simplicity, consider three different timings: pre-play communication, communication simultaneous to the economic behavior, and post-play communication. While the first is discussed extensively in the literature, e.g. Brosig et al. (2003), Rankin (2003), Bochet (2006) the other two are debated less often, e.g. in Xiao and Houser (2007), Ellingsen and Johannesson (2008). Furthermore, all of these can easily be used in combinations, which adheres to real-life situations more often. In this sense the timing also refers to the issue of frequency of communication.

(v) *Time restrictions:* The duration of communication is another criterion closely related to the timing. While it is possible to test the duration of communication in pilots for its functionality, little research tangles the question how long communication takes to achieve efficient results with respect to different levels of complexity of the problem and what happens if communication time implies costs per time or becomes otherwise restricted as in Karagözoğlu and Kocher (2016).

(vi) *Content restrictions:* Another possible type of restrictions refers to restricting the content. This enhances the distinction between e.g. social and strategic communication as was done by Roth (1995) or updated by Zultan (2012). However, imposing this type of restrictions is a complex issue as it is almost impossible to exclude subjects who violated these restrictions during the experiment. Furthermore, the line between strategic and non-strategic communication may be blurry. A possible solution for this problem was implemented by Centorrino et al. (2015) who drastically restricted communication by letting subjects memorize predetermined sentences with the only freedom left being some personal characteristics, e.g. name, age, and occupation.

(vii) *Characteristics of participants:* In addition to the question who starts the communication there is the question who are the agents that are taking part in it. This can refer to simple demographics, yet in economic laboratory experiments it is further meaningful to distinguish between aligned and conflicted interests of participants. The easiest distinction becomes apparent while differentiating between cooperation and competition games. Thus, the effects of introducing communication would depend on the incentives structure in the experiment as proposed by e.g. Blume et al. (2001), Cason et al. (2017) and Arechar et al. (2017). This further goes along with the question how different rules of communication can be used to elicit private information. For instance Blume et al. (2019) tested randomized responses in an experiment to elicit private information when truth-telling was socially stigmatized.

(viii) *Number of participants:* On a familiar note this also concerns the question of how many individuals take part in the communication, as was discussed in Lowry et al. (2006) with respect to accuracy of communication. The authors illustrated how the perceived accuracy of communication decreased in case of six participants as compared to three. This is plausible since adding more people to the conversation can increase the noise in the discussion and thus negatively affect the signal to noise ratio of communication. This topic is further briefly discussed in Balliet (2010).

(ix) *Endogeneity:* Another important question is whether communication is endogenous. This refers to whether the individuals are able to determine some of the eight previously discussed dimensions on their own. Baccara and Yariv (2013) and Calvó-Armengol et al. (2015) recently tackled this problem of endogeneity from the theoretical point of view. Cason et al. (2017) and Abbink et al. (2018) investigated different cases of endogenous emergence of communication experimentally. The underlying idea of agents choosing their interaction partners based on matching preferences, however, is not new. In fact, it could be traced back to Tiebout (1956).

The discussed nine contemplable dimensions illustrate how complex the implementation of communication in the laboratory can be and how much seemingly coherent studies and findings might differ in these dimensions. Further, it is mostly unknown how changes in these dimensions interact. Being aware of these versatile possibilities of communication two conclusions may be drawn. First, in order to apply communication as treatments, more information about the functionality of (different types of) communication is needed. This may have implications on the external validity of results as e.g. communication in small groups (in experimental laboratory) may differ from communication in large groups (outside world). Second, a more thorough planning of communication treatments may lead to new answers to new questions. For instance, being aware of the consequences of different types of communication on economic behavior in negotiation processes can be of high value for the negotiators.

Recapping the aforementioned distinctions, it appears challenging to provide some overarching, formalized insights on the effects of communication. Yet, some very general findings can be subsumed: Communication influences one's own beliefs about the behavior of other players and the state of the world. Therefore, it influences payoffs through the effect of beliefs on actions. More precisely, with respect to the experiments conducted within the scope of this thesis it is worth focusing on some possible explanations in the case of face-to-face communication. Brosig et al. (2003) discusses how face-to-face communication reduces social distance, enhances coordination between the agents. He et al. (2017) illustrates that it further facilitates type recognition of other agents. Ellman and Pezanis-Christou (2010) discuss the observer effect induced by face-to-face communication or the transmission of roles of ethicality. While there is not enough experimental evidence to assess which of these possible explanations is the most important one, it seems plausible to consider all of them as relevant. In fact, from the experience obtained in the process of this thesis it is likely that the exact explanation of the effects depends on the actual characteristics of communication as classified by the dimensions above.

1.6. Preview of the chapters

Before starting a brief overview of the results obtained in the course of this thesis: critic where critic is due. Looking back, the experiments underlying this dissertation did not consider any and all dimensions of communication that in hindsight could have been implemented in the experimental design. However, the conducted experiments made it apparent how inevitable it is to improve the systematic measurement of communication in future laboratory experiments. Contributing to this overall agenda, this thesis aims to proceed the analysis of communication in economic laboratory experiments in different aspects. Firstly, research conducted for this thesis replicates basic findings on the effectiveness of communication in public good games. Secondly, communication is introduced as a tool imitating an institution which has an economic value for the subjects in the experiment. Thirdly, the thesis illustrates new technological methods to analyze face-to-face communication in laboratory settings. Fourthly, communication and information are shown to strongly and interdependently affect financial allocations in a multi-agent variation of the ultimatum game. The following paragraphs provide a short outlook to the respective chapters: (2) The Incentive-driven Free-riding. The Effect of

Economic Education on Free-riding in Changing Institutional Setups[2] (3) The Reverberation Effect of a Behaviorally Informed Intervention [3] (4) Predicting free-riding in a public goods game – analysis of content and dynamic facial expressions in face-to-face communication[4] (5) Strategically distorted majority voting: The role of communication and information.[5]

The findings of the second chapter of this thesis are obtained from the combination of the previously discussed public goods game and communication. Making use of the illustrated classifications the communication applied in the underlying experiment was: (i) face-to-face, (ii) multidirectional communication – all group members talked to each other. It was conducted (iii) simultaneously, (iv) as one-time pre-play (v) time-wise restricted to three minutes, and (vi) not restricted content-wise. The interests of the participants were (vii) aligned on the group yet conflicting on the individual level. Per group (viii) four participants talked to each other. Eventually, (ix) the participants were not able to influence any of these criterions. With respect to the overall goal of the dissertation the aim of the first chapter was twofold. Firstly, the goal is to simply replicate a previously successful experimental setup of Brosig et al. (2003) to confirm the efficiency of pre-play face-to-face communication in a public goods game demonstrated in their research. Secondly, it aims to analyze the role of group composition focusing on the presence of economists in the groups. With respect to the first aim, replicating the results of Brosig et al. (2003) clearly confirms the powerful effect of face-to-face communication. Yet, the second goal of this chapter is more nuanced. In fact, it focuses on the effect subjects with economic education may have on the behavior individually or as a group. The overall idea is to test previous findings such as by Kirchgässner (2005) who illustrated that economists behave more selfishly in dilemmas similar to the case of a public goods experiment. The analysis, however, aims for a less philosophical question, i.e. whether economists are selfish or not, yet for a more behaviorally orientated one, i.e. whether economists react stronger to different incentives.

In order to observe any reactions to incentives it is important to establish different choice-architectures. During the experiment the subjects proceed a standard public goods game of ten periods in groups of four. Then the members of the groups are reshuffled, and the subjects

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[2] This Chapter is a personal advancement of the working paper of Altemeyer-Bartscher et al. (2017) and contains contributions from Martin Altemeyer-Bartscher, Philipp Schreck, and Florian Timme. The exact contributions are denoted in an additional document available to the doctoral committee.
[3] This Chapter was written by the author exclusively.
[4] This Chapter is a personal advancement of the working paper of Bershadskyy et al. (2019) and contains contributions from Frerk Saxen and Ehsan Othman. The exact contributions are denoted in an additional document available to the doctoral committee.
[5] This Chapter is an extended version of a paper draft and contains contributions from Florian Sachs and Joachim Weimann. The exact contributions are denoted in an additional document available to the doctoral committee.

repeat the same public goods game, however with pre-play face-to-face communication. Ultimately, some of the subjects are asked to fund the setup of the communication in an upcoming third part of the experiment. Thus, the experimental setup underlying for the chapter illustrates three types of architecture changes. First, it focuses on the differently strong incentives to contribute in a public goods experiment at the beginning and the end of the contribution stages. Second, the experiment uses communication as an efficiency providing institution. Making such an institution costly for the group imposes a second-order public good dilemma. Since the contribution environment for the funding of this institution differs from the one in the standard public goods game (first-order dilemma), these differences can be compared. Third, the funding process of the institution involves two treatments. One treatment includes a refund policy, i.e. if the group is not able to finance the communication platform the individuals will receive their money back. This implies higher incentives to fund the institution as compared to the no-refund treatment. If economists adapt more to explicit incentives than non-economists, the differences in these three cases would make this apparent. In fact, the results indicate differences for all three cases.

First, at the beginning, where potential benefits of cooperation are the highest, economists provide more socially optimal contributions to the public good experiment. In the final stage of the experiment, when the incentives to deviate are the highest, economists contribute significantly less than non-economists. Second, comparing the results of the first-order and the second-order public goods dilemma shows that while economists tend to contribute less in the first-order choice architecture, this does not hold for the second-order case. A possible explanation may be the experiences in similar scenarios. Third, the two different investment types illustrated that economists reacted stronger to the more investment-friendly treatment with the refund option. In this treatment the share of economists compensating for possible free-riders is higher than of non-economists. This speaks against the oversimplification of economists being simply more selfish yet supports the idea of economists reacting stronger to specific incentives.

The third chapter of this dissertation follows the experimental setup of the second chapter, yet it focuses on the question whether the positive effect of face-to-face communication is persistent after members of the groups are reshuffled and communication is taken away. For this purpose, it addresses the third part of the experiment, which was promised to the subjects, while funding the institution. It further utilizes two control groups where the individuals did not have an option to fund the communication but were obliged to repeat the setup with or without pre-play communication respectively. Therefore, there is a change in the endogeneity dimension, since in some treatments the subjects were able to influence future communication. In a nutshell the research bases on previous findings originating from the literature on path dependence and spillover effects in laboratory experiments. The results illustrate that the communication institution yields strong beneficial effects on the behavior in the third block. The cooperation is very high despite the group members being reshuffled. Further, the results indicate that individuals may have been subject to a simultaneous learning from the efficient public goods game and the inefficient one. This observation is based on the contributions being very high and stable even without the communication. Yet, the end-game behavior is much more severe than in the scenario prior to communication. This can be explained by a learning model of end-game behavior (Selten & Stoecker, 1986)

At the core of the fourth chapter lies the question whether communication information can be used to automatically predict free-riding behavior in a public goods experiment. The idea that contribution behavior in a public goods experiment can be predicted due to face-to-face communication is not new. Frank et al. (1993) showed how individuals were able to predict their partners' behavior in a one shot Prisoner's Dilemma experiment after a 30 minutes interaction. Brosig (2002) and Sparks et al. (2016) confirm these findings in slightly different settings. Further research shows that individuals are not only able to predict cooperation, yet are in general able to detect payoff relevant emotions. Van Leeuwen et al. (2018) indicate that independent observers are able to detect anger in an ultimatum game experiment. However, in most cases the precision of detection is not very high, or the individuals required a lot of time to make good predictions. One possible explanation may be that in the course of face-to-face communication a lot of different processes happen simultaneously and therefore are not detected accordingly by the human subjects.

This, however, would not be an issue for a software solution which, given enough computational powers and training data, could assist type detection of subjects in the experiments. Therefore, the fourth chapter shows how verbal and non-verbal communication can be hypothetically and practically used to predict contribution behavior in a group. From the practical perspective the analysis illustrates how content and facial expressions can be used as separate modalities to predict behavior. For the content analysis the communication was transcribed and scanned for specific buzz words and topics as were indicated in Brosig et al. (2003). The results indicate how simply mentioning specific topics can increase contributions in a group. Further, the magnitude of the discussion measured in the number of words spoken has a strong effect on contributions, too.

For technical reasons, (e.g. no automatic mapping of voices to the respective speakers was technically possible, no suitable library of words for a sentiment analysis was found, number of words spoken was too low) more modern types of analysis in this area, e.g. machine learning techniques were not used. In contrast to it, for the analysis of facial expressions it was possible to implement different types of machine learning analysis, e.g. support vector machine or random forest classification. The latter turned out to be more successful, likely due to large share of noise in the data. In the end, both types of analysis indicate predictions sufficiently higher than chance. From a more hypothetical perspective the paper illustrates the high potential of a combination of these approaches towards a multimodal analysis of face-to-face communication. However, due to the aforementioned technological constraints such a combination was not possible in the course of this thesis.

The fifth chapter focuses on another line of questions. While previous chapters analyze cooperation and free-riding behavior, this chapter focuses on the allocation of a financial endowment in a group. It focuses on the role of theoretical information and communication on the allocation schemes. With respect to the previously discussed communication dimensions the applied communication is (i) face-to-face and (ii) multi-directional. The individuals discuss the topic (iii) simultaneously (iv) up to two times before the economic action. Communication time (v) was restricted to three minutes and no restrictions on content (vi) were imposed. The design distinguishes between aligned and conflicted interests (vii) in a group. The discussions were conducted in groups (viii) of two, three, or five individuals respectively. The individuals were not allowed to influence (ix) any of these dimensions.

In the nutshell the chapter questions to what extent can a seemingly symmetrical majority voting process be distorted by imposing communication restrictions between some of the agents and providing information about the inherent power structure within the group. To analyze these questions, the chapter uses an extension of the ultimatum game towards a multiplayer scenario known as the Pirate Game. A major benefit of the design is that it enables the distinction of two groups of players (pivotal and non-pivotal) based on backward induction which yields the theoretical Nash Equilibrium. One of the central questions of the experiment is whether pre-play communication reinforces in-group favoritism among the subjects. This refers to communication being performed in different types of groups. Foremost it distinguishes between the conversations conducted in groups of two non-pivotal subjects and the conversations of three pivotal subjects. This further can be compared to the joint communication of the five subjects and a sequential communication, when subjects first communicate in separate groups and later enter a joint conversation. In a nutshell, the results indicate that communication strengthens in-group favoritism. This means that providing separated communication benefits one group and harms another. The second set of results illustrate a similar effect of enlightening the subjects with the theoretical equilibrium information. Pivotal players benefit from having a justification to extract a higher share from the total endowment. Combining communication and information leads to the severe disadvantages of the non-pivotal players.

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2. The Incentive-driven Free-riding. The Effect of Economic Education on Free-riding in Changing Institutional Environments

2.1. Introduction

One of the most plausible arguments in favor of better economic education is to improve the quality of financial decisions of the individuals. Well educated economists should be able to make unbiased strategic decisions based on their observation. From the perspective of behavioral economics this is related to the work of Thaler and Sunstein (2008). The authors emphasize the importance of the specific environment in which people make their decisions. These environments or so-called choice-architectures can heavily influence the decisions. In the same book the authors distinguish in a simplifying manner among two types of decisions makers. The authors argue that one type of them is often subject to fallacies or biases and thus errs predictably. The other type can also err, yet not in a systematic and hence predictable way. In simple terms this yields the first type of decision makers to react stronger to nudges and the second one stronger to real financial incentives. In this paper we focus on this differentiation with respect to economic education. Do economists focus more on financial incentives? Following the related distinction of passive and active savers, Chetty (2014) illustrates that individuals with higher economic education are more likely to response to changes in monetary subsidies for their savings.

The foundation for the question whether economists behave differently stems from the evidence from Kirchgässner (2005), Rubinstein (2006) and Brosig et al. (2010). The results indicate that economists often behave more selfishly than non-economists. While a share of literature on economic education focuses on the predominance of an education or a selection effect respectively, this paper aims to analyze the channels through which any of these two effects may operate. More concisely the goal of this research is to investigate whether economists react more sensitive to changing incentives in different choice-architectures. For this matter we implement a multi-stage public goods experiment. If the differences between economists and non-economists do not come from subjective fairness values or defaults but at least partially from the financial incentives of the dilemma, this should become observable in our laboratory setting. Public goods experiments fit this agenda for two major reasons. First, decentral provision of public goods typically causes a conflict of interest between individually rational behavior of market participants and socially desired outcomes. Further, economists were shown to behave more selfishly in related scenarios. Second, by the very structure of public goods experiments the financial incentives for high contributions decrease in the course of time as simultaneously the value of future cooperation within the group decreases. This is beneficial for our approach.

The major idea of public goods game research is to analyze why, in the absence of strong institutions that coordinate and enforce individual contributions to a public good, the contribution rates are inefficient. We contribute to the literature by further providing the subjects the opportunity to pay for an efficiency proven institution, i.e. the subjects know that the institution solves the dilemma and yields higher payoffs. Funding such an efficiency providing institution constitutes a second-order public goods dilemma. This is especially true if the formation of this efficiency providing institution entails costs for its founding members. If agreements on cost-sharing fail, the typical free-rider incentive problem remains unresolved as it simply shifts to a second-order level as is discussed by Yamagishi (1986). In our experimental setup we analyze the effect of economic education on the contributions of both, a first-order and a second-order public good. We are not aware of previous research on such differences.

In the experiment, we consider an institutional design that allows for video-conference based face-to-face communication before a public goods game is played, similar to the studies by Cason and Khan (1999), Brosig et al. (2003), and Bochet et al. (2006). As Brosig et al. (2003) did not observe differences between video-conference based and in person face-to-face communication and we replicated their experimental design, we will for simplicity refer to communication in our design as face-to-face.

In order to provide face-to-face communication in form of a second-order public good, the communication must be established endogenously. That is, the agents themselves may contribute to the provision of communication. Recent literature illustrates different approaches to build up the institution endogenously. Some of the most commonly applied tools are based on voting procedures (Kosfeld et al., 2009). In contrast to voting procedures we implement a design which involves the formation of a second-order public good. This is done for two reasons. First, it allows a comparison of contributions to a first-order and a second-order public good. Second, it facilitates the analysis of a refund option when contributing to the second-order public good. This is described in more details shortly after. In both cases the question arises whether economic education influences such investments. In our research, we analyze whether previous findings that economists contribute less to public goods hold with respect to changing institutional setups. Therefore, we aim to investigate three changes. First, the incentives to defect at the end of the contribution stages are higher than at the beginning.

Second, there are differences between the first-order and second-order public good. Third, we provide two different designs of institution formation. The difference is that one treatment includes a refund option, where the subjects will get their investments back if the group did not manage to fund the institution in total. Thus, the financial incentives to invest are higher, since there is less risk to lose money. In the other case there is no refund option. The financial incentives to invest are therefore lower. The differences in these three categories improve the analysis of the effect of economic education on contributions to public goods. We investigate potential differences in all three scenarios in a laboratory experiment. We further support the analysis by implementing a questions from the Cognitive Reflection Test (Frederick, 2005) and the World Values Survey (WVS). Questions from WVS address fairness, institutional trust, and attitudes towards specific free-riding activities (e.g. fare evasion, obtaining social benefits by fraud). Differences between economics and non-economists with regards to these questions might suggest that the two groups differ in some fundamental characteristics beyond economic education.

The experimental design can be divided into an *experience stage* and an *investment stage*. The experience stage makes sure subjects are aware of the benefits of the institution. In this stage players go through the standard (inefficient) public goods setup followed by a significantly more efficient setup with pre-play face-to-face communication. Thus, the players experience how effectively the communication institution overcomes the underlying free-rider problem. In the investment stage subjects are asked to jointly contribute money in order to proceed again with the effective institution. If groups fail to meet the established threshold, they have to continue with the inefficient version of the experiment.

With respect to the three aforementioned institutional changes, the experimental results indicate three findings. First, economists and non-economists differ in terms of the observed contribution patterns. Despite providing higher contributions at the beginning, in the end economists contribute significantly less than non-economists. Second, the contributions to the second-order public good do not depend on the economic education but on the previously experienced benefits. Third, economists react stronger to changes in financial incentives while funding the institution. With respect to the questionnaire no significant differences were found.

The paper outline is as follows. After a brief review of literature in section 2 the paper proceeds by describing the experimental setup in section 3. Subsequently we present our main results in section 4. The results are then discussed in section 5. Finally, section 6 concludes.

2.2. Literature

The standard result in the literature on public goods experiments is a too low provision of the public good. This has already been shown in an in-depth literature reviews by Ledyard (1995) and by Chaudhuri (2011). The literature further shows that there is a variety of mechanisms to induce socially optimal behavior. Falkinger (1996) and Falkinger et al. (2000) introduced a tax-subsidy procedure, which can negatively impact free-riding and induce a higher than average willingness to cooperate. Yet, there are several other mechanisms aiming to significantly increase contribution rates, e.g. Cason and Khan (1999), Gürerk et al. (2006).

Whereas early literature focused on efficiency consequences of different exogenous institutional backgrounds, more recent studies investigate the *endogenous* formation of such institutions. Gürerk et al. (2006) allowed the subjects to choose two institutional environments. Firstly, there was a simple voluntary contribution mechanism (VCM) without any regulation. Secondly, there was a VCM that includes sanctions, giving subjects the means to respond to co-players' free-riding behavior. The authors showed that the environment that allows for sanctions prevails relatively quickly as the predominant institution. Ertan et al. (2009) allowed players to vote on whether and who should be punished. The authors showed that only the low contributors were chosen to be punished and groups that in general allowed punishment achieved very high levels of contribution. In the experiment of Kosfeld et al. (2009) the subjects could choose on whether their group should be allowed to implement sanctions. Sutter et al. (2010) pointed out that the option to endogenously implement institutions has a positive effect on contributions as compared to exogenous alternatives. Further, the subjects preferred the reward option instead of the sanctioning option although the latter was more effective. In two different approaches, recent literature distinguishes the choices of subjects between formal and informal sanction schemes with (Kamei et al., 2015) and without (Markussen et al., 2014) endogenizing formal sanction schemes. Ramalingam et al. (2016) revealed the influence of the cost of an endogenously created institution on its effectiveness. Kriss et al. (2016) illustrated how even small communication costs decrease the usage of communication in Stag-Hunt games that was essential for coordination.

Our paper utilizes communication as an efficiency providing mechanism. From the early literature onwards (Dawes et al., 1977; Isaac & Walker, 1988) it was shown that face-to-face communication increases the cooperation in experiments. In addition, Isaac and Walker (1991) and Ostrom and Walker (1991) introduced an experimental setup where the communication is costly and at least a predefined share of participants had to fund it to be operational. Hereby the authors provided early experimental evidence on the contribution behavior in a second-order dilemma. Several studies (Bochet et al., 2006; Brosig et al., 2003; Cason & Khan, 1999) illustrated that the introduction of a video-conference for all players before VCM has a significant positive effect on individuals' contribution rates similar to in-person face-to-face communication. The authors distinguished between different types of communication and found the high relevance of face-to-face communication, yielding in cooperation rates of more than 90%. Extending research on the welfare-enhancing effects of an exogenously given communication platform, we analyze the endogenous formation of an institution that allows for pre-play communication.

Furthermore, the paper is reminiscent to a literature concerning the role and performance of economists in public goods experiments. Kirchgässner (2005) illustrated that economists and non-economists behave differently in the VCM. Laboratory experiments have shown that subjects with economic education have a lower voluntary willingness to cooperate. Marwell and Ames (1981) showed that American high school majors contribute twice as much in a VCM than students in the first year of their university education. In an ultimatum game by Carter and Irons (1991), economists did not behave completely selfishly, but their bids were significantly lower, and thus closer to Nash equilibrium than those of non-economists. Selten and Ockenfels (1998) exposed that economists have a much lower a priori willingness to commit themselves to compensation payments to players with a low payoff. A similar discrepancy was revealed by Frank and Schulze (2000) who displayed that economists have a much higher propensity for corruption than non-economists. However, gender effects could also be responsible for the results, since low cooperation is mainly due to the behavior of male economists (Ockenfels & Weimann, 1999). In other cultural contexts, such as Japan, differences between economists and non-economists could only be partially replicated (Iida & Oda, 2011). Further, some studies doubt the general finding of economists behaving more selfishly. Laband and Beil (1999) showed that economists were significantly more likely to make honest (i.e. correct) information on their income when compared to political students and sociologists. Further, it was shown that economists have a higher willingness to contribute to the provision of public goods of professional associations. Similar conclusions can be drawn from the interviews in Gandal et al. (2005).

However, as in most cases economists display less pro-social behavior, the question arose whether the differences stem from an education or a selection effect. Various studies (Brosig et al., 2010; Cipriani et al., 2009) provided evidence that the differences are mainly due to a selection bias rather an education effect. However, the results from the laboratory contrast some other related studies. Rubinstein (2006) discussed how mathematical methods in economics make students lean towards profit maximization. Applying a field experiment Frey and Meier (2003) observed more differentiated results. They point out that business students were less willing to donate as part of a charity campaign at the University of Zurich than fellows from other disciplines, although this finding does not hold for students in economics. Following up this research, Bauman and Rose (2011) indicated that in fact selection and education effects co-exist. Which of the two effects is predominant, depends on the actual major of students. Non-economics majors were subject to an education effect, whereas actual students in economics were subject to a selection effect. However, the authors could not exclude that students in economics simply have more economic experience prior to visiting the university. This effect is further discussed by Parsons and Mamo (2017) who illustrate that prior socio-economic experience influences the preconceptions of students entering economics. Hellmich (2019) provides a comprehensive literature review on the general topic, eventually concluding that the results on the topic are ambiguous.

Based on the findings in the literature, the standard hypothesis is that economists free-ride more often in a public goods game. Since this could be traced back to different origins, we analyze further hypotheses. First, economists react stronger to changing financial incentives. Second, economists have in general a higher acceptance of free-riding activities. Third, economists have higher cognitive reflection skills potentially enabling them to take advantage of others.

2.3. Experimental Design

In order to test these hypotheses, we conduct a laboratory public goods experiment consisting of three game blocks that are consecutively executed, followed by a questionnaire testing cognitive skills and assessing specific values associated with acceptance of free-riding. During the experiment participants are stepwise informed about the experimental design. The instructions for every block are distributed prior to the individual block and read aloud by the instructors. For instructions, see Appendix 2.1. Hence, participants learn the institutional details of every block immediately before its starting. In addition, we ask questions of understanding related to the experimental design. Only after all players answered the questions correctly the experiment starts. Individuals play the public goods game in a group of four. The pay-off function of individual j in period k is:

$$\pi_{jk}\left(g_{jk}\right) = 20\text{-}g_{jk}+\frac{1}{2}\sum_{j=1}^{n} g_{jk}, \quad j=1,\ldots 4$$

with g_{jk} representing tokens invested by subject j in period k.

After every period the subjects receive anonymous information on the payoff in their group. At the end of each block, the subjects are informed about their cumulative payoff that stems from all ten periods. Applying round-robin design, we rule out a repeated encounter of subjects in the same group in subsequent blocks. Thus, we ensure a perfect strangers set-up. The subjects are informed, that they will not meet each other in any block to come. To avoid income accumulation among the blocks, at the very beginning of the experiment the subjects are informed that in the end only one block will be randomly selected as cash-effective. Therefore, every block constitutes an independent set of observations.

In the first block, the participants play a typical VCM for ten rounds. Hereon, the groups are reshuffled. At the beginning of the second block, each group of four individuals attends a video conference for up to three minutes. During the conference, individuals can have an open discussion but do not have the power to make binding commitments. Furthermore, the subjects are not able to figure out which of the co-players decided to defect if they did. Since after the experiment the subjects are paid out in a group independent order and can leave the laboratory area in two different ways, their conversation constitutes cheap talk. After the video conference ends, participants play the same VCM as in the first block. The combination of communication and VCM is denoted as C-VCM.

Subsequently, participants are randomly assigned to new groups. Then, they receive the opportunity to fund the same communication platform. The subjects are informed that the communication platform can only be successfully installed if their group of four people jointly funds 32 Laboratory-Dollar (LD)[6]. Otherwise they have to wait for three minutes and then proceed without communication. The investment takes place simultaneously and their investment will be subtracted from future income in block three. The experimental procedure is depicted in Figure 2.1.

[6] This threshold level was calibrated during a pilot experiment prior to the main experiment. Hereby we randomized the threshold values for the participants and evaluated the corresponding investments in the institution.

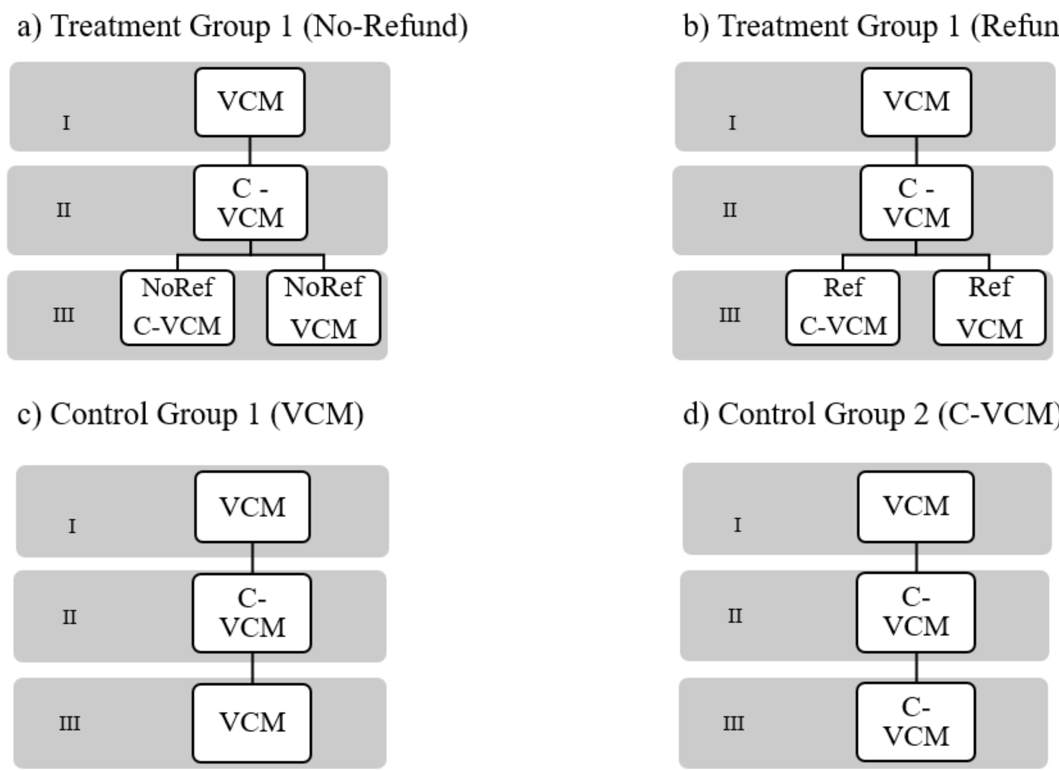

Figure 2.1 Experimental Design

Note: VCM: simple voluntary contribution mechanism; C-VCM: voluntary contribution mechanism with pre-play communication; NoRef: individuals pay their investment independent of whether the threshold was met; Ref: individuals get their investment refunded if threshold was not met.

We employ two different variants of the investment decision block three. In the less investment-friendly setup ("No Refund"), individuals must pay their investment independent of whether the institution is sufficiently funded in the end. In the more investment-friendly setup ("Refund") participants would get their investments refunded if the group threshold was not met. Assuming approximately fair share contributions of 8 LD per subject it yields two per cent of the payoff to be obtained from the socially optimal contributions (400 LD). Since the investment choice exhibits a simultaneous-move game, no subject knows *a priori* whether his or her contribution will suffice for the formation of the institution. After their investment decision, groups that reach the threshold of 32 Laboratory-Dollar proceed with the C-VCM setup and groups that did not reach the threshold wait for three minutes and continue with the standard VCM. Following another set of hypotheses not to be discussed in the subsequent chapter, we implemented the third block including two control treatments without endogenous institution funding between block two and block three. Finally, all subjects filled in a questionnaire consisting of several questions from the World Value Survey (WVS), the Cogntive Reflection Test (CRT) by Frederick (2005), and questions on demographics and their major. The questionnaire is presented in Appendix 2.2. The experiment was conducted among students from the University of Magdeburg at the experimental laboratory (MaXLab). The duration of the experiments in total was between 70 and 90 minutes. The payoff of one of the blocks was converted to euro (1 Laboratory Dollar = 4.5 Cents). The average payoff was around 16 €. The experimental design was executed in z-Tree (Fischbacher, 2007). The experiment was organized and recruited with the software hroot (Bock et al., 2014).

Table 2.1 Subjects pool statistics

	Treatment 1 VCM/C-VCM/NoRefund	Treatment 2 VCM/C-VCM/Refund	Control 1 VCM/C-VCM/VCM	Control 2 VCM/C-VCM/C-VCM
Sessions	8	8	4	4
Total Subjects	128	128	64	64
Economists	65	57	29	31
Non-Economists	63	71	35	33
Male	66	78	36	32
Females	62	50	28	32
Average age	23.96	24.10	23.28	22.92

The total sample consists of 384 subjects (see Table 2.1) that are distributed among the two treatment groups (128 subjects each) and two control groups (64 subjects each). Due to the aforementioned perfect stranger matching, it was necessary to obtain exactly 16 subjects per session. To investigate the effects of economic education we distinguish two different types of subjects. Firstly, we define individuals who study economics or business majors as economists (E) and the remaining students as non-economists (NE). We classify each participant based on the data provided in the questionnaire where the subjects had indicated their study program.

2.4. Results

Our experimental analysis focuses on three different decisions: the individual contributions of subjects in the VCM, C-VCM, and their contributions to the financing of the communication platform. The remainder of this section is organized as follows: Section 4.1. illustrates the result for the standard public goods game. Section 4.2. presents the influence of the communication platform. Section 4.3. analyzes differences between economists and non-economist in forming the communication platform. Section 4.4. briefly discusses the questionnaire based on the WVS and CRT.

2.4.1. Voluntary Contributions to the Public Good

We start with an analysis of the standard public goods game in Block I. The central question is whether economists contribute less to the public good. In compliance with the majority of the literature on the topic we observe that economists have lower contributions. However, when simply comparing the average contributions over 10 periods the results are not significant. Figure 2.2 visualizes the contributions of economists and non-economists over the entire time horizon of Block I. Hereby it becomes apparent that the differences in the average contributions yield from the last stages of the experiment. The average contributions are 14.37 LD (11.19 LD) for non-economists and 14.31 (9.58 LD) for economists in periods 1 to 5 (6 to 10). Therefore, the average contributions of economists and non-economists are virtually identical in the first half of the VCM, yet they are significantly different in the second one (p= 0.000) using the Mann-Whitney test.

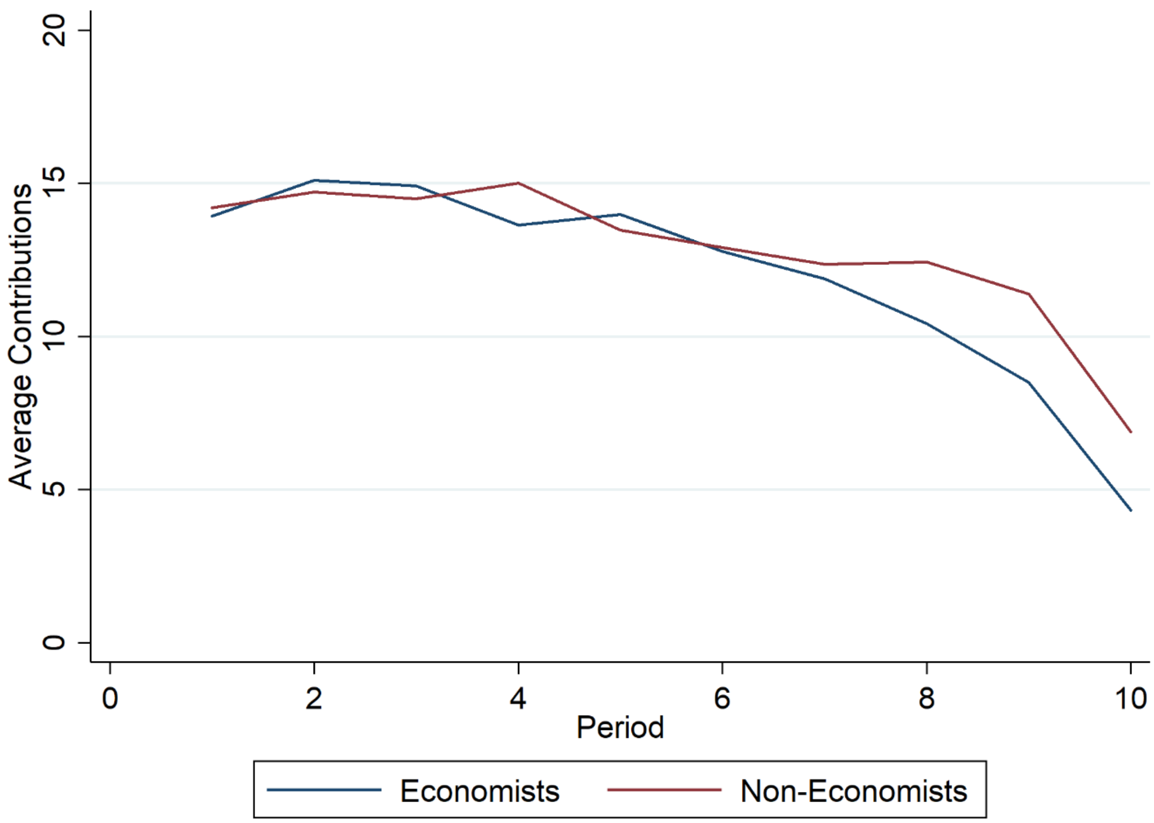

Figure 2.2 Contributions in the VCM by economists and non-economists

This observation implies that the contribution period and being an economist may interact. From the methodological perspective we consider two approaches. First, it is possible to analyze the contributions of individuals at different periods (e.g. first vs. last period) of the experiment using Mann-Whitney test (MW). This would however necessarily imply many tests and therefore increase the risk of multiple testing problem. The alternative we chose is to analyze the obtained data in the panel form. Hence, we establish four regression models in order to isolate how and when economists contribute differently to the public goods game. For reasons of completeness we simply include the statistics from MW-test in the appendix 2.4.

The obtained data constitutes a censored panel regression model. The presented results are all obtained from a panel Tobit regression with lower limit at 0 and the upper limit at 20 and are illustrated in Table 2.2. In order to control for the behavior of the co-players we introduce the variable of the sum of contributions of the other three players in the group in the previous period. The coefficient of this variable indicates how strong the individuals react to the observed contribution level of the co-players. From the theoretical point of view this variable is a proxy to whether the respective subject acts as a conditional cooperator. Since conditional cooperators adjust their contributions based on previously observed contribution behavior of other players, high contributions of others should yield in higher own contributions. However, including the lagged variable also limits the number of observations. Since in the first period there is no previous period, the analysis can only be conducted in periods 2-10[7]. Simultaneously, we implement a second lagged variable which controls for the own contributions in the prior period. Further, the analysis contains a simple time variable which in total yields the following random effects panel Tobit regression model:

$$\begin{aligned} Contribution_{it} = \beta_0 + \ & \beta_1 Economics + \beta_2 Period + \ \beta_3 AverageContr_{-it-1} \\ & + \beta_4 Contrbution_{it-1} + \ \beta_5 Economics * Period + \ \beta_6 Economics \\ & * AverageContr_{-it-1} + \beta_7 age_i + \beta_8 gender_i + u_i + \ \varepsilon_{it} \end{aligned}$$

The results from the basic model are presented in column (1). In the next columns we introduce the interaction between time and being economists (2), interaction between co-players' previous contributions and being economists (3), and both interactions jointly (4). The control variables age and gender are included in all regressions. Based on these regressions we observe a versatile image of how being an economist influences the contributions to the public good. The first regression illustrates that the contributions depend positively on the contributions in the previous round and decrease over time. The model further supports the hypothesis that economists provide lower contributions. On the one hand, including the interaction between economics and the period shows that this effect is largely driven by economists faster decreasing their contributions. Including this interaction turns the effect of simply being

[7] The differences in the first period are depicted in appendix 2.4.

economists positive. On the other hand, the interaction between the contribution of the co-players from the previous period and economics has exactly the opposite effect. Economists react stronger to contributions of their co-players. Combining both interactions in (4) illustrates that time is the dominant factor. The interaction between economics and co-players vanishes and the effect of being economists becomes insignificant. Yet, economists decrease their contributions stronger in the course of the experiment.

However, from the methodological perspective it is unclear whether the coefficients of the random effect panel Tobit regressions are biased. Poen (2009) and Merrett (2012) indicate that, despite this model being chosen very often in public goods games, if applied to VCM with feedback variables it can produce biased coefficients. However, there is no unambiguous remedy. Discrete approaches such as Finite Mixture Models or ordered Logit are less precise. Panel OLS approaches can be superior to panel Tobit if the amount of censoring is low. However, only the random effect model is meaningful in our case, since the fixed effect model removes time invariant variables. Another approach would be clustering pooled data. However, the bias may remain in case the origin of the bias is not the feedback variable but the difference between the true distribution of data and the assumed distribution of the Tobit model. Ashley et al. (2010) argue that dynamic censored models are the best choice for individual-level analysis of a VCM. Vossler (2013) states that there is no consensus for a covariance estimator which is robust to serial correlation without knowing its true form. The author further proposes an A-HAC estimator, which however would be similar to a clustered OLS with fixed effects. Kong and Sul (2018) provide a potentially working estimator, which however relies on a set of strict assumptions, e.g. knowing the true long-time decay rate of contributions. Given the changing institutional setups of this analysis this approach is unfeasible. Therefore, as a robustness check to the analysis in Table 2.2 we present a random effect Panel OLS and a pooled Tobit regression with the same specifications in Appendix 2.3. These illustrate that the findings – especially the interaction between the period and economic education – are strongly significant and robust.

One possible explanation is that economists are more prone to changes in contribution environment. Thereby, economists would pursue a time dependent type of a tit-for-tat strategy. As long as the cooperation is high, economists contribute strongly to the public good. Observing the decreasing contribution rates of co-players yields them to try pre-empting the others by decreasing their rates stronger. Simultaneously, economists may anticipate the end-game by observing the development of contribution rates or by being more aware of the actual dilemma.

Table 2.2 Panel Tobit regression results

	(1) Basis Individual	(2) Interaction Period	(3) Interaction co-players	(4) Interaction joint
Economics	-1.899** (0.966)	4.694*** (1.750)	-6.230*** (2.016)	1.630 (2.789)
Period	-1.465*** (0.129)	-0.977*** (0.166)	-1.473*** (0.129)	-1.020*** (0.168)
Economics x Period		-1.106*** (0.243)		-1.020*** (0.250)
Co-players	0.428*** (0.025)	0.428*** (0.025)	0.375*** (0.032)	0.397*** (0.033)
Economics x Co-players			0.111** (0.045)	0.066 (0.047)
Own previous contribution	0.802*** (0.062)	0.777*** (0.061)	0.795*** (0.062)	0.776*** (0.061)
Gender	0.544 (0.972)	0.581 (0.989)	0.575 (0.977)	0.593 (0.988)
Age	0.157 (0.145)	0.159 (0.148)	0.180 (0.146)	0.172 (0.148)
Constant	-5.515 (3.723)	-8.217** (3.836)	-3.952 (3.790)	-7.104* (3.911)
Observations	3456	3456	3456	3456

Note: Standard error is denoted in brackets. ***/**/* denote significance of the coefficients at 0.01/0.05/0.1 levels respectively.

Further support for this explanation can be derived from observing the first time of defection. This variable measures in which period the individual deviates from the socially optimal strategy (20 LD) for the first time. This analysis shows that economists deviate on average later (after 3.23 periods) than non-economists (after 2.43 periods) with a MW test p-value of 0.012. We further repeat the analysis only for all the individuals that did not deviate already in the first period. By excluding the individuals that deviated from the socially optimal behavior in the first period, we address the issue of confusion in the first period (Andreoni, 1995) and focus on individuals whose defection was induced by either the contributions of the other players or the approaching end-game. We obtain for economists and non-economists 5.78 and 4.63 periods respectively (p=0.035). The observation that in a VCM economists contribute on average less, but start deviating later, yields the conjecture that the differences between the two groups are more versatile than discussed in previous literature.

In addition to the analysis on the individual level, we further illustrate the differences between groups with different shares of economists. Therefore, we create dummy variables indicating groups with at least 50% economists[8] and test the group level contributions (see Table 2.3)[9]. In the standard public goods game, as predicted by previous research, groups with more economists have lower contributions. However, this difference is not significant when the analysis is pursued on group average contributions over 10 periods.

Table 2.3 Two-sided MW-test at 50% threshold of economists in a group

	≥50% Economists	<50% Economists
Block I		
Statistics (average)	47.61 LD	52.91 LD
p-value	0.1694	
Observations	61	35

With respect to the aforementioned discussion on whether the differences between economists and non-economists are caused by selection or education we investigate students with different academic achievements. We use the information obtained from the questionnaire where subjects indicated their highest educational achievements. Therefore, we distinguish two subgroups of economists depending on whether they finished their undergraduate studies.[10] We assume that economists with a bachelor's degree (63 subjects) have more economic training than those without a bachelor's degree (116 subjects). Figure 2.3 illustrates how the contributions of the two groups. It is worth noting that the general progress of contribution rates is similar to the one depicted in Figure 2.2 for economists and non-economists. Here, the more experienced students with a Bachelor's Degree start with slightly higher contribution rates yet end up with lower rates than students with the university entrance diploma. Table 2.4 summarizes the analysis of the contributions provided by both groups using the two-sided MW-test. We show that in total there is no significant difference between these groups. However, with respect to different stages of the VCM it can be stated that students without Bachelor's Degree start contributing less than those with it, yet over time the behavior switches. The differences are mostly insignificant. The Tobit regression models indicate that the variables period and contributions of co-players are predominant.

[8] Robustness checks are possible at (25%/75%/100%). However, thresholds other than 50% result in more unbalanced groups as a group of e.g. three or more economists is less likely to occur.
[9] Note that there are 96 groups with 10 observations per group.
[10] The total number of observations is reduced due to some subjects not filling out this question.

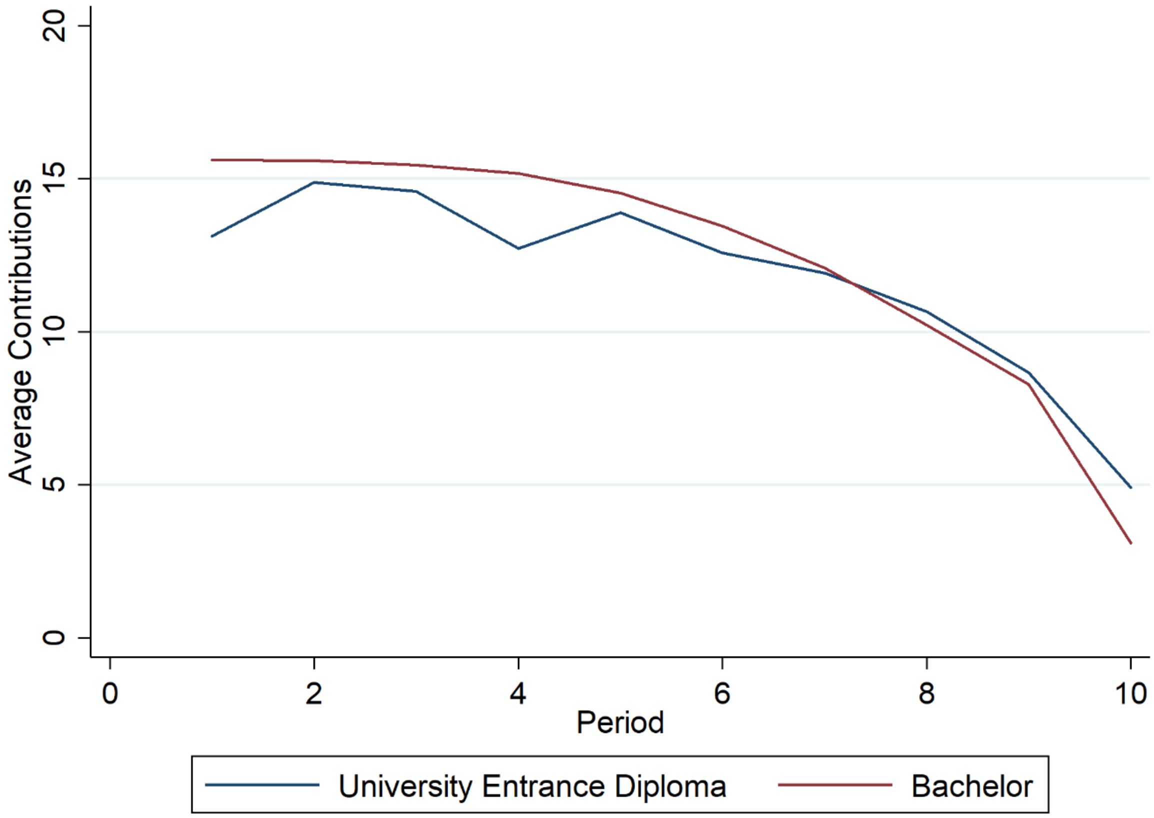

Figure 2.3 Contributions in the VCM by economists with University Entrance Diploma and bachelor’s Degree

Table 2.4 Two-sided MW-test for economists with different educational achievements

	First Period	Last Period
University Entrance Diploma	13.12 LD	4.92 LD
Bachelor’s Degree	15.60 LD	3.11 LD
MW	0.042	0.093
observations	179	179

Though being only partially significant we observed similar patterns for the distinctions between economists and non-economists on the one hand and economists with and without Bachelor’s Degree on the other. It is further worth noting that that this pattern holds for non-economists, as well. Likewise, non-economists with a Bachelor’s Degree start with higher contribution rates than their counterparts without degree yet finish at lower rates than them. Further, there is a possible explanation for this observation. It is reasonable to assume that students with a Bachelor’s Degree are more experienced than those without. Since experience is in general known to have this very type of effect in public goods games (Nax et al., 2016) it is plausible to think of economic education as ex-ante experience in this type of dilemmas.

2.4.2. Communication

Prior to analyzing the contributions in the second block, we first investigate the effect of the provided communication. Hereby we analyze contributions of 96 groups over 10 periods. The average contributions in the VCM and C-VCM are 49.54 and 77.16 LD respectively. This difference is highly significant (p=0.0000) and is further visualized in figure 2.4. We support the evidence from Brosig et al. (2003) having the identical set up that pre-play communication using a video-conference tool leads to high and stable contributions in a VCM.

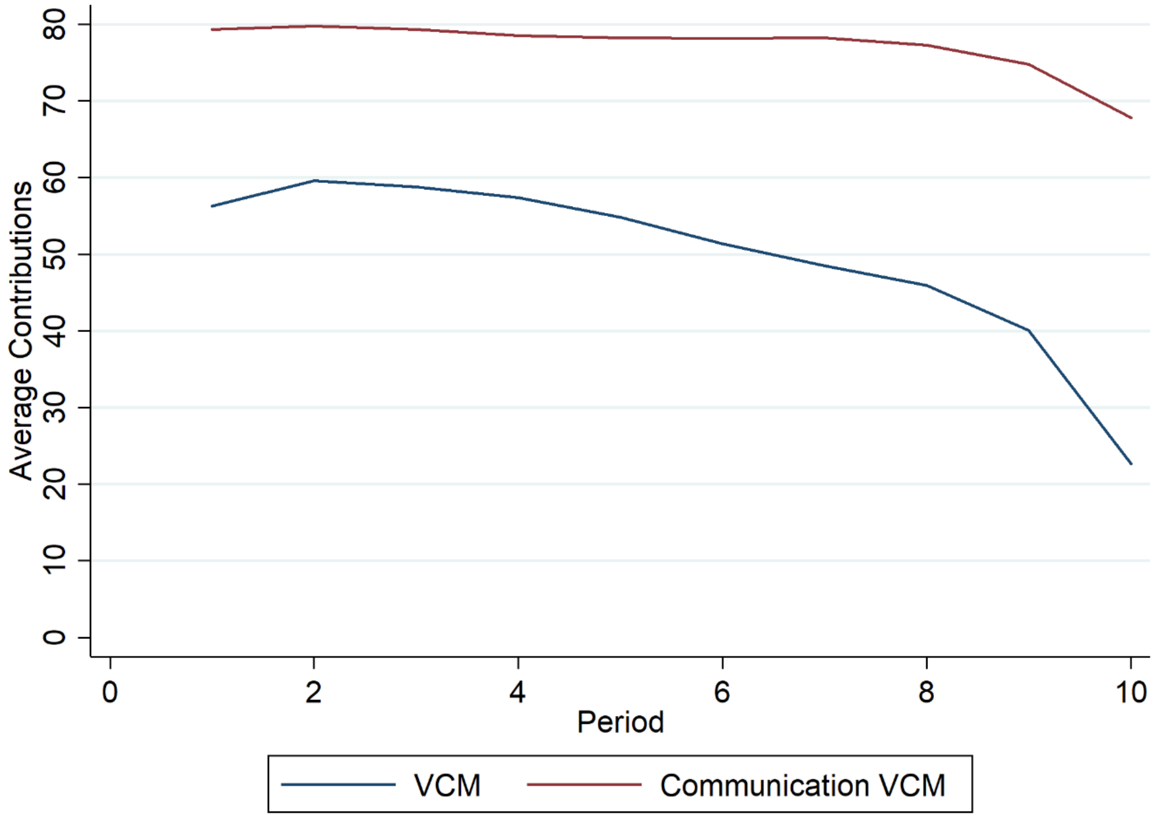

Figure 2.4 Contributions in the VCM and in the Communication VCM on group level

Table 2.5 provides evidence that groups with a high share of economists contribute at least as much as other groups (77.65 compared to 76.36). Appendix 2.4 displays the fact that both, economists and non-economists, contribute a high share of their endowment. Full contributions occur in 95.99% of all decisions. There are no significant differences between economists and non-economists. Similar to the results of the first block, economists start deviating from the socially optimal contributions later than non-economists, yet the differences are insignificant. In general the results obtained from the second block confirm previous findings (Bochet et al., 2006; Brosig et al., 2003; Cason & Khan, 1999) on the effectivity of pre-play communication as an efficiency enhancing tool in the voluntary contribution mechanism. Simultaneously, communication is so powerful in terms of increasing the efficiency of contributions that there is almost no variance among the contributions, making further analysis futile.

Table 2.5 Two-sided MW-test at 50% threshold of economists in a group in Block II

	≥50% Economists	<50% Economists
Statistics (average)	77.65	76.36
p-value	0.824	
Observations	60	36

2.4.3. Endogenous institution formation

Analyzing the investments to the communication institution we firstly take a closer look on the outcomes in the experience stage, i.e. block I and block II. This stage lays the foundation for the players' subsequent investment choice. Before making their investment decisions, individuals can consider the additional pay-off that stems from the higher cooperation rates due to pre-play communication setting of block II.

We classify investment levels as follows: zero investments or values near to zero indicate that individuals either do not have preference for communication or intend to free-ride on the building of the communication platform (second-order public goods problem). Investment levels of individuals who intend to actively build up the institution crucially depend on the functionality of the institution and the subjects' *a priori* expectations with respect to their co-players' willingness to invest. If they opt for a rule of thumb "fair-share" they would invest 32/4 LD=8 LD. If they however try to compensate possible zero-contributions of co-players they can apply Level-k thinking. Several options may arise of which we briefly mention two. Firstly, if the individual assumes no other person will try to compensate the free-rider, they increase own contributions by 8 LD assuring the formation of the institution in the case of one free-rider. Alternatively, if the individual expects other cooperation partner to have a similar line of thoughts, the extra costs caused by a free-rider can be split. In this case, an individual that expects k free-riders in the group, contributes $\frac{1}{4\text{-k}}$ of the threshold value. It can be tested whether economist opt more for any value suggested by the Level-k thinking. This would indicate a higher degree of purely strategical thinking. However, for neither of possible thresholds do we observe a significantly higher share of economists. Instead we provide a general overview for different benefit criterion, which can be related to different stages of Level-k thinking. These are listed in Table 2.6.

When it comes to the No Refund treatment, the benefit between block II and block I added up to an average of 59 LD per person. These 59 LD therefore constitute the average benefit of the institution for the individuals. In the Refund treatment this benefit amounted 69 LD. Despite efforts to keep the invitations to all treatments identical, the difference in the benefit between the two treatments is significant (MW p=0.0430). This is the reason we control for the treatment as a dummy variable and the individual benefits in the upcoming regression analysis. However, there are no differences in the benefit variable between economists and non-economists (MW p=0.8572).

Table 2.6 Benefits of communication and investments in communication

Benefit criterion		Number of subjects	Average benefit in LD	Average investment in LD
No Refund		128	59.04	5.78
	>0	119	64.83	5.97
	>8	112	68.70	6.13
	>32	82	86.45	6.65
Refund		128	69.15	6.42
	>0	112	84.53	7.16
	>8	105	89.95	7.37
	>32	93	98.66	7.71

Given the low financial threshold and the comparatively high gains from the institution, a high number of created institutions could be anticipated. However, only seven groups in the no refund treatment and eight groups in the refund treatment could attain the investment threshold to successfully build up the communication platform. For both treatments, we observe a large variety of values with different peaks. The mode is at zero indicating a large number of individuals without any willingness to contribute. Another frequent observation is the fair share value (8 LD). Furthermore, we observe a peak at 10, which could be attributed to the commonly observed behavior of choosing round numbers. Figure 2.5 and Figure 2.6 provide information on the distribution of the investments for both treatments. The combination of many zero contributions and only few high value contributions results in an inefficient provision of the institution, independent of the treatment. The most notable difference between the treatments is a shift from zero-contributions in the No Refund treatment to the fair share value in the Refund treatment. This can be attributed to decision structure, which includes less risk in the refund treatment.

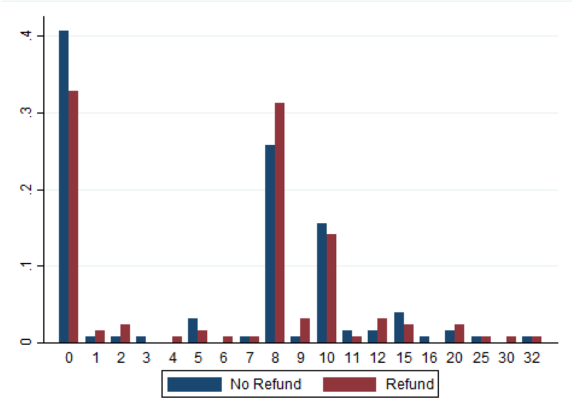

Figure 2.5 Histogram of investments

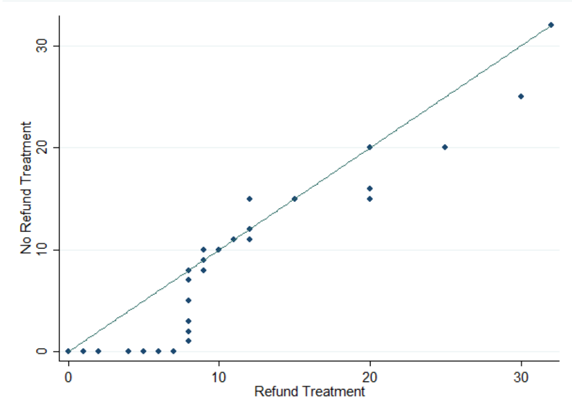

Figure 2.6 Quantile-Quantile plot of investments

Observing the investments yields a certain pattern, yet no significant differences (Kolmogorov-Smirnov p=0.832). However, the refund option loosens the dilemma and yields a more investment-friendly environment. Coming back to our initial research agenda we focus on the question whether economists reacted differently to this change in the dilemma.

Before focusing in more detail on the interaction between the refund option and economic education, we stress that the significance values are always close 0.05. For example, a simple diff-in-diff measure indicates an interaction between the refund option and economic education (p=0.045). Therefore, economists reacted stronger to the change in the payment scheme. Yet, enhancing the analysis by the means of clustering on group level the results turn slightly insignificant (p=0.055). For descriptive statistics on this issue see Appendix 2.5. To obtain as robust result as possible we apply two sets of regressions always clustered at the group level. First, we use a simple OLS model which is later extended by the aforementioned individual benefits from communication. In the last step we control for all variables obtained from the questionnaire (demographics, trust, etc.). Second, we apply a Tobit regression which is extended identically to the OLS. The Tobit regression is clustered at group level and includes a lower bound of zero.

The analysis in Table 2.7 illustrates that the results are not robust to different specifications, yet the effect sizes are remarkably high. Please recall that the average investments were around 6 LD. The interaction terms for the refund option and economic education are always over 3.0 LD in OLS models and over 3.9 LD in Tobit models. However, due to high variance the results are not always significant, which could be due to certain heterogeneity within either group or the insufficient sample size. The regressions further show that individual benefits of communication are low yet always significant and robust. Thus, it is reasonable to assume that the institution has to provide a better or a more a salient benefit-cost-ratio in order to be sufficiently funded.

Table 2.7 Analysis of investment

	(1) Basis	(2) +Benefit	(3) +Controls	(4) Tobit	(5) +Benefit	(6) +Controls
Refund	-0.763	-1.188	-2.206⁺	-0.658	-1.532	-2.966⁺
	(1.13)	(1.06)	(1.26)	(1.63)	(1.51)	(1.65)
Economics	-0.962	-1.010	-2.163	-1.211	-1.329	-2.938
	(1.09)	(1.04)	(1.40)	(1.62)	(1.52)	(1.91)
Refund x	3.017⁺	3.101*	3.270*	3.964⁺	4.026⁺	4.079⁺
Economics	(1.54)	(1.46)	(1.52)	(2.28)	(2.16)	(2.15)
Individual Benefit		0.038*	0.036*		0.060*	0.057*
		(0.01)	(0.01)		(0.01)	(0.01)
Constant	6.270*	4.049*	1.889	4.034*	0.642	0.466
	(0.95)	(0.90)	(5.00)	(1.34)	(1.34)	(7.18)
Controls	N	N	Y	N	N	Y
Observations	256	256	211	256	256	211

Note: Standard error is denoted in brackets. */+ denote significance at 0.05/0.1 levels respectively

2.4.4. The values and cognition of economists

The analysis of this chapter does not rely on incentivized experimental data, yet it originates from the questionnaire implemented directly after the final block of the experimental setup. The questionnaire consists of three major areas. Besides asking for simple information on age, gender, or the study program, the questionnaire includes questions on specific values and trust measures from the WVS and the CRT. As the demographics were already included in the standard analysis procedure in terms of controls, we now focus on the question whether there are any differences between economists and non-economists with respect to the other two areas. The CRT consists out of three different questions. It is possible to check whether any specific answer is correct or to build a total score of correct answers. We are further able to control, whether the individuals already knew any of the CRT questions before. Yet, controlling for this knowledge does not change the results. This yields a total of four variables. Further, we implemented 16 questions from the WVS asking for general trust, specific trust in particular institutions (e.g., police, churches, banks, environmental organizations), and the attitude towards specific behavior (e.g., theft, fare evasion, bribery). The most intuitive way to analyze whether there are differences in responses would be to perform MW tests.

Table 2.8 Correlations between economic education and answers from questionnaire

	CRT1	CRT2	CRT3	CRT Score	Trust 1
Economics	0.0374	-0.0314	-0.0493	-0.0164	0.0873

	Trust 2	Churches	Courts	Government	Parliament
Economics	0.0120	-0.0813	-0.1540	-0.0396	-0.0451

	Police	Civil Service	Environmental organizations	Humanitarian organizations	Television
Economics	-0.1470	-0.0573	0.0228	-0.0596	0.0478

	Banks	Unjust benefits	Fare evasion	Stealing property	Accepting bribes
Economics	-0.1138	-0.0410	0.0029	-0.0146	0.1241

Given the large amount of categories this raises the issue of multiple hypothesis testing. To avoid it, we first illustrate the correlation of the all categories focusing on our Economics variable (Table 2.8). Here it becomes apparent that economic education does not strongly correlate with any of the collected data. The strongest correlation measured is for the trust in courts (-0.1540). Except for trust in banks, police, and the attitude towards bribery all other correlations have an absolute value of below 0.1. These four categories are the only ones to yield significant differences between economists and non-economists applying the MW-tests. However, the significance completely disappears when applying the Bonferroni correction. In a final step it is possible to check whether any of the 20 parameters are a good predictor of economic education using a logit model. In this case only one variable (Attitude towards accepting bribes) is significant (p=0.004). Summing up the evidence from the questionnaire, we do not observe major differences between economist and non-economists.

2.5. Discussion

In this paper we analyzed whether the economists act more like the stylized incentives-driven beings from Thaler and Sunstein (2008). If so, economists should be less prone to the status quo bias and be able to adapt to altering investment environments. In order to investigate whether economists react stronger to changing choice-architectures we focused on three differences: (i) the difference between the first and last stage of the standard VCM, (ii) the difference between the first-order and second-order public goods dilemma, (iii) the difference in cognitive power and values.

With respect to the first topic, we illustrate that economists adapt more to a changing dilemma environment using results from the standard public goods game. Concerning the question whether economists provide less socially optimal contributions we partially support the previous finding of lower contribution rates of economists by e.g. Marwell and Ames (1981) and Kirchgässner (2005). However, we illustrate that the differences in behavior are not trivial. This indicates that economists and non-economists do not simply have different contribution rates but that those rates evolve differently over time. We show that economists start defecting from the social optimum strategy significantly later than non-economists, yet they converge faster to the Nash Equilibrium strategy. Especially towards the end-game phase of the dilemma, when the incentives to defect are higher, economists show significantly higher free-riding patterns than non-economists. However, we illustrate that this cannot be traced back easily to the typically discussed education effect of economics studies. In contrast, it mirrors findings from Dzionek-Kozlowska and Rehman (2017) who illustrated that students in economics *and* sociology are less likely to cooperate in the later years of their respective study programs. We observed a very similar pattern, distinguishing between students with and without a Bachelor's degree independent of studying economics.

Further, we extend the standard research towards the contribution behavior in the second-order public goods dilemma. In our setup the subjects first experienced an institution that was proven to effectively overcome the inefficiencies of the standard (first-order) public goods game. Afterwards we tested how much the individuals would pay to obtain this institution – which composed the second-order dilemma. The results obtained from the second-order dilemma are more ambiguous and depend on the choice-architecture. These differences are stronger in the case of economists. While both economists and non-economists mostly underfund the institution, we find minor evidence for the adjusting behavior of economists applying a small change in the payment scheme. In the refund treatment, where individuals only must pay for the institution if it is sufficiently funded, non-economists do not show any significant reaction to these changes in the dilemma (and in fact slightly decrease their payments). In contrast, economists increase their contributions in the refund treatment when they know they would retrieve their investment if no institution is formed.

This leads to the aforementioned differences between the first-order and the second-order public goods dilemmas in our experiment. The most notable difference, besides the order itself, is that in the former the dilemma is a repeated game, whereas the latter is a one-shot game. Thus, in the second-order dilemma there is no opportunity to learn. This however allows to draw a parallel to the first-order dilemma. When analyzing the first part of the standard VCM there were no major differences between economists and non-economists. It is reasonable to assume that repeating this second-order dilemma may change the way the subjects act. It is further worth noting that typical VCM (first-order dilemma) occurs more often in laboratory experiments than second-order dilemma. Thus, especially subjects with economic education, who visit the laboratory more often, could have had more experience with solving the standard VCM. This is doubtful for the second-order dilemma. This stresses the importance of experience. For the second-order dilemma the advantage from experience is smaller and so are the differences between economists and non-economists in their respective behavior.

We further confirm findings of Decker et al. (2003) stressing the role of the differences in profit for the establishment of an institution solving the initial dilemma. Okada (2008) focuses on the effects of accumulation and demographical changes in the setup of second-order public goods. However, our setup does not allow to draw parallel to this theoretical approach, as we (i) mostly avoid capital accumulation and (ii) do not have an increasing population but simply reshuffle the groups. Still, we are not aware of experimental analysis of a repeated second-order dilemma. If we were to consider economic education and economic experience as related, our findings at least suggest that this issue is worth of deeper investigation. In a nutshell, people realizing the consequences of the failure to solve the second-order dilemma can affect their decision process the next time with respect to the first-order and the second-order dilemma.

Finally, we discuss whether the differences in the observed behavior in the experiment correlate with specific variables taken out the WVS and CRT. The general idea that specific questions from the WVS correlate with behavior in public goods experiments is not new. Thöni et al. (2012) focus on trust variables and utilize two questions from WVS that are also included in our setup. The authors apply CRT as a further control. Fosgaard (2019) analyzes contributions to public goods to the political attitudes and controls for CRT and duration of education. We further extended the type of questions with respect to confidence in certain governmental and non-governmental institutions and attitudes towards specific misbehavior. However, we are not aware of an analysis explicitly focusing on whether individuals with economic education systematically differ in these questions. In our questionnaire we do not observe such differences. Only the attitude towards accepting bribes was significant, yet this finding was not very robust. Thus, we consider this as evidence that the differences we observed in the experiment do not originate from parameters that can be measured by these questions.

2.6. Conclusion

With respect to the initial questions on differences between economics and non-economists three statements can be made. Firstly, economists remain longer at the socially benevolent contribution rate than non-economists. However, with an increasing threat of exploitation, e.g. towards the end-game, economists strongly decrease their contributions. Secondly, we do not confirm economists being more selfish in general. On the contrary, given a more investment friendly payment scheme, economists contribute slightly more to the funding of an efficiency providing institution. Both observations can be explained by economists being better aware of the underlying incentives for the respective behavior. Thus, economists can perceive the choice-architecture better and adapt accordingly. Thirdly, with respect to general measures of trust, cognitive reflection and certain attitudes towards misbehavior economists do not differ from non-economists. Being aware of the origin of differences in the behavior may help policy makers to find the right policy measures. Further, for the educators, it emphasizes the similarities between economic education and economic experience. It may be helpful to consider economic education as a formal summary of prior economic experiences stemming from different sources inside and outside of academia. This can improve dealing with preconceptions of students entering their studies in economics.

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Appendix 2.1 Instructions

Instructions Experiment “Yellow“ (Block 1)

Please read the instructions diligently. If questions arise, open the door to your cabin and remain seated. The experiment “Yellow” is carried out at the computer.
Your fellow participants will only play with you within the experiment “Yellow“.

After reading the instructions you will receive four control questions. The control questions are not considered for your final payment. As soon as you have answered the control questions the part of the experiment relevant for your final payment will start. Please be aware that either experiment „Yellow“, „Blue“ or „Red“ will be paid out. Which experiment will be eventually relevant is decided by chance.

Within the experiment, we will use laboratory dollars as the used currency. The underlying exchange rate is the following: 100 laboratory dollars = 4.5 EUR.

You and three other participants receive each **20 laboratory dollars** per round of contribution. You can contribute these laboratory dollars either to a private or a group account.

Private Account (P): The deposited laboratory dollars are being kept.

Group Account (G): Each of the four players can deposit money in this account. The sum of the deposits is doubled by the experimenter and redistributed equally to the four players. Hence, each player receives 0,5 laboratory dollars per contributed laboratory dollar.

The laboratory dollars can be split up in between the two accounts. You take your decision anonymously. None of the other players will learn how you split your laboratory dollars up. Profit of player i is calculated accordingly:

$$\text{Profit}=(20\text{-}G)+0{,}5\cdot\sum_{1}^{4}G_i$$

Please turn!

Instructions Experiment „Red" (Block 2)

Please read the instructions diligently. If questions arise, open the door to your cabin and remain seated. The experiment "Red" is carried out at the computer.
Your fellow participants will only play with you within the experiment „Red".

After reading the instructions you will receive four control questions. The control questions are not considered for your final payment. As soon as you have answered the control questions the part of the experiment relevant for your final payment will start. Please be aware that either experiment „Yellow", „Blue" or „Red" will be paid out. Which experiment will be eventually relevant is decided by chance.

Within the experiment, we will use laboratory dollars as the used currency. The underlying exchange rate is the following: 100 laboratory dollars = 4.5 EUR.

You and three other participants receive each **20 laboratory dollars** per round of contribution. You can contribute these laboratory dollars either to a private or a group account.

Private Account (P): The deposited laboratory dollars are being kept.

Group Account (G): Each of the four players can deposit money in this account. The sum of the deposits is doubled by the experimenter and redistributed equally to the four players. Hence, each player receives 0,5 laboratory dollars per contributed laboratory dollar.

The laboratory dollars can be split up in between the two accounts. You take your decision anonymously. None of the other players will learn how you split your laboratory dollars up. Profit of player i is calculated accordingly:

$$\text{Profit}=(20\text{-}G)+0{,}5\cdot\sum_{1}^{4}G_i$$

Video conference: Before you take your decision on how to split the laboratory dollars you will be talking to the three other players in a video conference for three minutes. During this time, you can see and talk to each other. The duration of the call can neither be reduced nor prolonged.
Subsequently to the video conference, each player makes the above described decision.

Please turn!

Instructions Experiment „Blue" (Block 3, No Refund)

Please read the instructions diligently. If questions arise, open the door to your cabin and remain seated. The experiment "Blue" is carried out at the computer.
Your fellow participants will only play with you within the experiment „Blue".

After reading the instructions you will receive six control questions. The control questions are not considered for your final payment. As soon as you have answered the control questions the part of the experiment relevant for your final payment will start. Please be aware that either experiment „Yellow", „Blue" or „Red" will be paid out. Which experiment will be eventually relevant is decided by chance.

Within the experiment, we will use laboratory dollars as the used currency. The underlying exchange rate is the following: 100 laboratory dollars = 4.5 EUR.

You and three other participants receive each **20 laboratory dollars** per round of contribution. You can contribute these laboratory dollars either to a private or a group account.

Private Account (P): The deposited laboratory dollars are being kept.

Group Account (G): Each of the four players can deposit money in this account. The sum of the deposits is doubled by the experimenter and redistributed equally to the four players. Hence, each player receives 0,5 laboratory dollars per contributed laboratory dollar.

The laboratory dollars can be split up in between the two accounts. You take your decision anonymously. None of the other players will learn how you split your laboratory dollars up. Profit of player i is calculated accordingly:

$$\text{Profit}=(20\text{-}G)+0{,}5\cdot\sum_{1}^{4} G_i$$

Set-up of the video conference: At the beginning of the experiment you are asked whether you want to make the experiment this time with or without communication. Communication will be subject to a fee. To make the experiment with communication you must raise a required amount jointly as a group. This amount will pop up on your screen at the beginning of the experiment. The decision on how much you contribute will then be again taken anonymously. The deposited money for setting up communication is being deducted from your profit in the experiment „blue" at the end of it – whether communication is successfully set up or not. If the group raises the required amount, a three-minute video conference is being set up, see previous round. Otherwise, all group members have to wait for three minutes until other groups have finished their communication period, respectively. Subsequently, the decision on how to split up the laboratory dollars between private and group account are being made.

Please turn!

Instructions Experiment „Blue" (Block 3 Refund)

Please read the instructions diligently. If questions arise, open the door to your cabin and remain seated. The experiment "Blue" is carried out at the computer.
Your fellow participants will only play with you within the experiment „Blue".

After reading the instructions you will receive six control questions. The control questions are not considered for your final payment. As soon as you have answered the control questions the part of the experiment relevant for your final payment will start. Please be aware that either experiment „Yellow", „Blue" or „Red" will be paid out. Which experiment will be eventually relevant is decided by chance.

Within the experiment, we will use laboratory dollars as the used currency. The underlying exchange rate is the following: 100 laboratory dollars = 4.5 EUR.

You and three other participants receive each **20 laboratory dollars** per round of contribution. You can contribute these laboratory dollars either to a private or a group account.

Private Account (P): The deposited laboratory dollars are being kept.

Group Account (G): Each of the four players can deposit money in this account. The sum of the deposits is doubled by the experimenter and redistributed equally to the four players. Hence, each player receives 0,5 laboratory dollars per contributed laboratory dollar.

The laboratory dollars can be split up in between the two accounts. You take your decision anonymously. None of the other players will learn how you split your laboratory dollars up. Profit of player i is calculated accordingly:

$$\text{Profit}=(20\text{-}G)+ 0{,}5\cdot \sum_{1}^{4} G_i$$

Set-up of the video conference: At the beginning of the experiment you are asked whether you want to make the experiment this time with or without communication. Communication will be subject to a fee. To make the experiment with communication you must raise a required amount jointly as a group. This amount will pop up on your screen at the beginning of the experiment. The decision on how much you contribute will then be again taken anonymously. The deposited money for setting up communication is being deducted from your profit in the experiment „blue" at the end of it – only if communication is successfully set up. If the group raises the required amount, a three-minute video conference is being set up, see previous round. Otherwise, all group members have to wait for three minutes until other groups have finished their communication period, respectively. Subsequently, the decision on how to split up the laboratory dollars between private and group account are being made.

Please turn!

Appendix 2.2 Questionnaire

1. How many of the persons you communicated with in the second sub-experiment did you already know before (by first name)?
   0/1/2/3

2. How many of the persons you communicated with in the third sub-experiment did you already know before (by first name)?
   0/1/2/3/Communication did not take place.

3. A bat and a ball cost together 1.10 Dollar. The bat costs one dollar more than the ball. How much does the ball cost?

4. If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to produce 100 devices?

5. In a lake, there is a patch of lily pads. Each day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take to cover half of the lake?

6. Did you already know one or several of the three previous questions?
   Yes/No

7. Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people? (Code one answer):
   1 Most people can be trusted. / 2 Need to be very careful.

8. Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair? Please show your response on this card, where 1 means that "people would try to take advantage of you," and 10 means that "people would try to be fair" (code one number):

People would try to take advantage of you People would try to be fair

1 2 3 4 5 6 7 8 9 10

9. I am going to name a number of organizations. For each one, could you tell me how much confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very much confidence or none at all? (Read out and code one answer for each):

	A great deal	Quite a lot	Not very much	None at all
The churches				
Television				
The Police				
The courts				
The government				
Parliament				
The Civil Service				
Banks				
Environmental Organisations				
Humanitarian Organisations				

10. Please tell me for each of the following actions whether you think it can always be justified, never be justified, or something in between, using this card. (Read out and code one answer for each statement):

Never justifiable							Always justifiable		
1	2	3	4	5	6	7	8	9	10

Claiming government benefits to which you are not entitled

Avoiding a fare on public transport

Stealing property

Someone accepting a bribe in the course of their duties

11. What is your age?

12. Are you male or female?

13. Please indicate your highest educational level:

– No formal education

– Incomplete secondary school: technical/vocational type

– Complete secondary: university-preparatory type

– Bachelor Degree

– Master Degree

– PhD

14. In which major are you enrolled in?

Appendix 2.3 Robustness Check

Table 2.9 Robustness Check of the Panel Tobit model using OLS Panel and Pooled Tobit regressions

	Tobit Panel				OLS Panel				Pooled Tobit			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Economics	-1.899*	4.694**	-6.230**	1.629	-0.670**	0.775	-1.047*	0.698	-1.572+	4.070*	-4.876*	1.229
	(0.97)	(1.75)	(2.02)	(2.79)	(0.21)	(0.53)	(0.51)	(0.80)	(0.81)	(1.74)	(2.03)	(2.68)
Period	-1.465***	-0.977***	-1.473***	-1.020***	-0.499***	-0.386***	-0.499***	-0.387***	-1.287***	-0.855***	-1.292***	-0.893***
	(0.13)	(0.17)	(0.13)	(0.17)	(0.04)	(0.06)	(0.04)	(0.06)	(0.16)	(0.18)	(0.16)	(0.18)
Economics		-1.106***		-1.020***		-0.241**		-0.238**		-0.947***		-0.875***
x Period		(0.24)		(0.25)		(0.08)		(0.08)		(0.26)		(0.26)
Co-players	0.428***	0.428***	0.375***	0.397***	0.139***	0.139***	0.134***	0.138***	0.405***	0.406***	0.365***	0.377***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.04)	(0.04)	(0.04)	(0.04)
Economics			0.111*	0.066			0.010	0.002			0.086+	0.063
x Co-players			(0.05)	(0.05)			(0.01)	(0.01)			(0.05)	(0.05)
Own prev.	0.802***	0.777***	0.795***	0.776***	0.438***	0.436***	0.438***	0.436***	1.158***	1.152***	1.156***	1.151***
contribution	(0.06)	(0.06)	(0.06)	(0.06)	(0.02)	(0.02)	(0.02)	(0.02)	(0.11)	(0.11)	(0.11)	(0.11)
Gender	0.544	0.582	0.575	0.593	0.090	0.091	0.094	0.092	0.163	0.176	0.186	0.191
	(0.97)	(0.99)	(0.98)	(0.99)	(0.21)	(0.21)	(0.21)	(0.21)	(0.69)	(0.70)	(0.70)	(0.70)
Age	0.157	0.159	0.180	0.172	0.010	0.010	0.012	0.011	0.084	0.083	0.101	0.095
	(0.15)	(0.15)	(0.15)	(0.15)	(0.03)	(0.03)	(0.03)	(0.03)	(0.12)	(0.12)	(0.12)	(0.12)
Constant	-5.515	-8.217*	-3.952	-7.104+	3.999***	3.333***	4.134***	3.362***	-8.804**	-11.342***	-7.653*	-10.302**
	(3.72)	(3.84)	(3.79)	(3.91)	(0.85)	(0.88)	(0.87)	(0.91)	(3.27)	(3.41)	(3.25)	(3.40)
sigma_u	7.038***	7.267***	7.095***	7.255***								
Constant	(0.64)	(0.65)	(0.64)	(0.65)								
sigma_e	14.799***	14.695***	14.779***	14.696***								
Constant	(0.40)	(0.40)	(0.40)	(0.40)								
sigma									15.929***	15.889***	15.924***	15.888***
Constant									(1.14)	(1.14)	(1.14)	(1.14)

Note: Standard error is denoted in brackets. ***/**/*/+ denote significance levels of 0.001/0.01/0.05/0.1

Appendix 2.4 Analysis of contributions in different periods

Table 2.10 MW-tests of contributions at different stages of the experiment and the time of first deviation behavior in Block 1

Block 1	First period (N=384)		Last period (N=384)		Joint block (N=3840)	
	Economists	Non-Ec.	Economists	Non-Ec.	Economists	Non-Ec.
Extreme points	0.6429	0.5297 (0.0249)	0.8516	0.7426 (0.0084)	0.7286	0.6366 (0.0000)
Free-riding	0.1319	0.0743 (0.0624)	0.6978	0.5248 (0.0005)	0.2720	0.1980 (0.0000)
Full-contributions	0.5110	0.4554 (0.2774)	0.1538	0.2178 (0.1092)	0.4566	0.4386 (0.2632)
First Deviation Time (in periods)					3.23	2.43 (0.0352)

Note: The numbers represent the share of individuals in the respective group, p-values of the MW-Test are in brackets. The analysis of first deviation considers only individuals that have deviated at least once. Since some individuals contributed the socially optimal amount over the entire 10 rounds, the sample decreases to 348 individuals in block one and only 64 in block two.

Table 2.11 Analysis of contributions at different stages of the experiment and the time of first deviation behavior in Block 1

Block 2	First period (N=384)		Last period (N=384)		Joint block (N=3840)	
	Economists	Non-Ec.	Economists	Non-Ec.	Economists	Non-Ec.
Extreme points	0.9945	0.9802 (0.2175)	0.9890	0.9802 (0.4874)	0.9940	0.9832 (0.0019)
Free-riding	0.0000	0.0050 (0.3425)	0.1593	0.1287 (0.3929)	0.0341	0.0272 (0.2184)
Full-contributions	0.9945	0.9752 (0.1292)	0.8297	0.8515 (0.5598)	0.9599	0.9559 (0.5433)
First deviation time (in periods)					8.33	7.19 (0.1427)

Note: The numbers represent the share of individuals in the respective group, p-values of the MW-Test are in brackets. The analysis of first deviation considers only individuals that have deviated at least once. Since some individuals contributed the socially optimal amount over the entire 10 rounds, the sample decreases to 348 individuals in block one and only 64 in block two.

Appendix 2.5 Analysis of the Refund option

Table 2.12 Different types of investments in the two Refund options

	No Refund Treatment		Refund Treatment	
	Economists	Non-Economists	Economists	Non-Economists
Free riding	41.53%	39.68%	28.07%	36.62%
Min fair share	53.85%	52.38%	64.91%	54.93%
Fair share	29.23%	22.22%	28.07%	33.80%
Compensator	24.61%	30.16%	36.84%	21.13%
Average investment	5.3077	6.2698	7.5614	5.5070
		(0.6769)		(0.1064)

Note: Numbers in brackets represent p-values from a two-sided MW-test. This table illustrates the shares of economists and non-economists that were free-riders (investment=0 LD), compensators (investment >8 LD) or provided exactly the fair share (= 8LD) to the institution. The last two can be summarized as "min fair share", i.e. individuals who contributed at least 8 LD. The biggest change for the non-economists is the tendency to contribute more often the fair share values instead of compensating other players (increase in fair share contributions from 22.2% to 33.8%). For the economists two major observations can be done. Firstly, the number of free-riders strongly decreased in the refund treatment (from 41.5% to 28.1%). Secondly, economists compensated more often for other players (24.61% to 36.8%). Though insignificant, the economists increased their investments in the Refund Treatment.

3. Reverberation Effect of a Behaviorally Informed Intervention

3.1. Introduction

Over the last years the application of behavioral science insights into policy became more and more frequent. According to OECD (2020) more than 200 institutions officially announced the application of behaviorally informed tools. One of the sub-branches where such tools are applied is the collection of due payments, including taxes. Besides the original formal measures such as increasing fines for frauds or improving the detection of tax evaders, behavioral research provided less formal alternatives. For example, personalizing reminders to non-compliant debtors increased the payment of fines to the UK Ministry of Justice (Haynes et al., 2013). In another setup providing people information on the high tax payment behavior in their area and its importance for local services increased tax payments (Hallsworth et al., 2017).

From the experimental perspective these findings are not surprising. In general the insufficient provision of public goods was experimentally shown using voluntary contribution mechanisms (VCM) in various setups (Chaudhuri, 2011; Ledyard, 1995). The behaviorally informed interventions used in the aforementioned policy applications worked through decreasing social distance between the individuals and raising trust in payment behavior of other members of the group. While being experimentally, they were in praxis shown to reduce the original public goods dilemma. From the experimental point of view, one of the best approaches to simultaneously achieve both effects is to provide face-to-face communication among subjects prior to the contribution to the public good (Brosig et al., 2003; Isaac & Walker, 1988). Thereby, communication providing platforms are at least as efficient as formal institutions introducing punishments for misbehavior or subsidizing benevolent behavior. In a field study on common-pool resource dilemma conducted in rural areas of Columbia, Cardenas et al. (2000) illustrated that communication leads to more efficient choices than incentivized government regulation. However, it is yet unclear whether this type of behavioral intervention has persistent effects in the population. This is of importance since even behavioral measures induce costs which in the best case should be minimized. If the changes are permanent the intervention can be removed to save spending or to avoid the impression of persistent paternalism.

Applying a public goods experiment with face-to-face communication, the aim of this paper is to analyze whether the positive effects wear off over time. In the context of the paper the platform enabling face-to-face communication will be referred to as a behavioral institution and the contribution behavior after removing the institution is addressed as the reverberation effect.

An important characteristic of this paper is that the institution chosen for the experimental design does not involve punishment for free-riders nor subsidies for socially benevolent agents. Thereby, it contributes to the general analysis of behaviorally informed tools. The behavioral institution simply facilitates non-binding pre-play communication in a video-conference. The experimental result that cheap talk during video-conferences helps overcome the public goods dilemma has been established by e.g. Cason and Khan (1999), Brosig et al. (2003), Bochet et al. (2006). Altemeyer-Bartscher et al. (2017) raised the question on how much the individuals would pay for such an institution to be established after it was demonstrated to be an effective way to increase outcome for the participants. Applying the same experimental data set that was obtained in the experiment of Altemeyer-Bartscher et al. (2017), which analyzed the establishment of such an institution, this paper focuses on the consequences of removing the institution. The novelty of the experimental design lies in the combination of pre-play communication and the treatments involving restarts with strangers as applied in Andreoni (1988), Croson (1996), and Andreoni and Croson (2008). The experimental setup allows us to focus on two major questions.

Firstly, do efficiency gains of a behavioral institution prevail after its removal? In a broader sense this questions to what extent past outcomes influence future contribution behavior. To address this issue, the simplified experimental setup is as follows. In the first block of the experiment, all individuals play the standard VCM.In the second block all individuals play the VCM with pre-play communication (C-VCM), which provides much higher payoffs. Thus, the subjects experience the changes in contribution rates caused by pre-play communication. The participants in the third block are either asked to finance the institution or are left without this choice and forced to either join the standard VCM or the C-VCM (Altemeyer-Bartscher et al., 2017). Comparing the contribution patterns between the blocks within the respective treatments enables the analysis of the reverberation effect.

While previous research in the area often focused on the effects of changing the “good” institutional environment to a “bad” one and vice versa, this paper adds the focus on endogenizing such a change. Therefore, the second question arises, whether failing or succeeding in establishing an institution influences future contribution behavior. This question is addressed in the design as the participants receive information on whether their group met the financial threshold to fund the communication platform. Funding or failing to fund the platform can have signaling effects for future cooperation. In order to investigate such potential signaling effects it is possible to compare groups that failed (succeeded) to fund the communication platform and continued with the VCM (C-VCM) with those who did not have any choice and continued with VCM (C-VCM) anyway.

The paper provides experimental evidence that there is a positive reverberation effect of the behavioral intervention after its removal. However, the gains are abating. The increased contributions, induced by pre-play communication, yielded higher contribution rates even after regrouping the team members and removing the communication. The combination of standard VCM and the VCM with pre-play communication is shown to yield two major effects for subjects without institution in block 3. First, prior to the last period of the block the contributions are higher and more stable than in block 1. Second, the end-game effect is more severe, as the contributions decrease very strongly in the last period of the block. Further, the research shows that there are no differences between having the choice and being forced to repeat VCM/C-VCM respectively. Thus, no signaling effects of the institution formation were found. However, the type of institution can yield a short-term mistrust effect. Those individuals that factually lost money after the failure to fund the institution as the group, significantly reduced their contributions to the public good. Yet, this effect is limited only to the first contribution period after the funding stage.

The outline of the paper is as follows. After providing an overview of relevant literature in section 2, the paper illustrates the experimental setup in section 3. The presentation and discussion of the main findings takes place in section 4 and is followed by the final conclusion.

3.2. Literature

This paper joins previous literature on contributions to a public good in an experimental setup. In line with the standard prediction, the common laboratory finding, as it was surveyed by Ledyard (1995) and Chaudhuri (2011), is that the VCM yields an inefficient provision of the public good and a downward spiral of individual contributions over time. As the focus of this paper lies on an institution solving the original dilemma, it is important to discuss several findings including the contribution behavior in environments without institutions, different types of institutions and the specific effect of communication. Subsequently the literature review will address the topics of end-game behavior, institutions, communication, path dependence (prior experiences), and the issue of field relevance.

At first, we will have a look at the literature concerning the simple scenarios where no efficiency providing institution is present and the contributions to the public good are low. There, a large share of individuals exhibits a specific reaction to low contribution rates of co-players. They behave as conditional cooperators, i.e. people who provide more to the public good when the other members of the group provide high contribution rates as well (Fischbacher et al., 2001; Kocher et al., 2008). Another important observation is the decrease of cooperation rates in the last contribution periods, which is referred to as the end-game effect (Andreoni, 1988; Selten & Stoecker, 1986). From the theoretical perspective this effect is not surprising since the standard Nash Equilibrium contributions of zero for all periods are obtained by using backward induction. In the final periods the expected benefits from a longstanding cooperation are lower than at the beginning which leads participants to deviate from their originally higher contribution rates. Further, the end-game effect is robust with respect to different designs, e.g. non-definite time horizons in Gonzales et al. (2005) and sequential contributions in Figuieres et al. (2012).

In order to increase the contributions and therefore solve the initial dilemma, different types of institutions can be applied. In line with North (1990), institutions are constraints that limit or define the set of choices of individuals. In laboratory experiment this can refer among other things to formal institutions involving punishments or rewards in the VCM or choosing between different institutional mechanisms. Falkinger (1996) and Falkinger et al. (2000) illustrated how positive (negative) taxes for selfish (non-selfish) players lead to the high contribution rates approximating the social optimum. Gürerk et al. (2006) allowed competition between sanctioning and non-sanctioning institutions and illustrated how individuals chose the sanctioning one. Sutter et al. (2010) allowed the individuals to choose between a sanctioning and a rewarding institution. The individuals chose the rewarding option despite it being inferior.

Another experimental approach to increase contributions which does not include formal institutions is to implement restarts. Focusing on the distinction between strangers and partners in a public goods experiment Andreoni (1988), Croson (1996), and Andreoni and Croson (2008) illustrated a restart effect. After completing a VCM of several rounds the VCM was simply restarted. The contributions of the first period of the restart were then higher than the final periods of the previous VCM. However, despite this increase, the average contribution rates do not achieve the level from before the restart. Furthermore, as Duffy and Lafky (2016) argued, this “restart effect” does not occur when the subjects are replaced periodically by new members.

In contrast to restarts another informal measure has a much stronger and more permanent effect. In their seminal paper Isaac and Walker (1988) showed that face-to-face communication increased contribution rates in public goods experiments to approximately 100%. Following these results several approaches have been started to isolate the drivers for the increases in contribution rates. Frohlich and Oppenheimer (1998) differentiated between E-Mail communication and face-to-face communication. The main finding was that while communication via E-Mails increased cooperation, the contribution rates did not reach the level of face-to-face communication. Further, the authors established that the role of the communication channel decreases in importance when there is no conflict of interest among the participants in the group. Cason and Khan (1999) provided evidence that face-to-face communication increases efficiency independent of the ability to monitor prior contribution quality. Brosig et al. (2003) extended the findings from Frohlich and Oppenheimer (1998) by establishing a higher variety of different ways of communication in a public goods experiment. The authors illustrated that the combination of verbal and audio-visual communication enhances efficiency the most. Simple audio communication, passive communication in form of a short lecture or a visual identification without communication did not achieve the results that were obtained from the actual face-to-face communication or the video conference. The differences between the face-to-face communication and the video conference were negligible. In the same regard, Bochet et al. (2006) compared different types of communication. The authors found strong and long-lasting effect of face-to-face communication. In comparison, textual communication affected the contributions only shortly after the respective communication period. Brosig (2006) provided an overview of a variety of economic experiments implementing cheap talk through face-to-face communication. This draws the conclusion that cheap talk conducted via face-to-face communication, including video conferences, is an effective tool to increase efficiency in different types of experiments and is of importance for real-world applications. Haruvy et al. (2017) distinguished among the environments in which communication and visibility take place. While supporting previous findings on the effect of communication in laboratory experiments, the authors showed that the channels through which the effect materializes can differ in a virtual world environment. To conclude, communication is proven to be a highly effective measure to increase contributions in public goods experiments.

Most of the aforementioned experiments analyzing face-to-face communication focused on costless communication. As any type of intervention, the establishment of a communication platform would cause costs in real world. Yet, as was shown early by Isaac and Walker (1991), Ostrom and Walker (1991), and Ostrom et al. (1992) the positive effect of communication persisted even when the communication became costly. Recent analysis of communication costs illustrated that the use of communication devices strongly decreases whenever communication is not for free (Altemeyer-Bartscher et al., 2017; Kriss et al., 2016). Similar results were found by Ramalingam et al. (2016) despite the efficiency providing institution being not based on communication but on a sanctioning mechanism.

With respect to the experimental evidence on how previous experience affects present behavior in a changed environment, there are few studies that focus on this question and do so using starkly different environments with different results. Hamman et al. (2007) and Brandts et al. (2016) provided evidence that coordination failure in minimum-effort games can be resolved by changes in financial incentives even without changing the equilibrium outcomes, implying that past experience has no decisive effect on the following behavior. In a platform competition experiment Hossain and Morgan (2009) tested the QWERTY phenomenon. The authors showed that subjects readily switch to a more efficient platform whenever it becomes available. Thus, the threat to get caught in a bad equilibrium as originally described by David (1985) caused by first mover advantage did not find empirical support in the laboratory.

While these studies did not find evidence for path dependence, others concluded that groups can fail to adapt perfectly in a changing environment. Smerdon et al. (2016) and Wilkening (2016) traced this back to the issue of incomplete information. According to Andreoni et al. (2017) the path dependence arose when the preferences of the subjects change gradually yet separately. Kamm et al. (2017) argued that even under complete information a commonly known change in an institution fails to affect the expectations of the subjects and therefore their behavior. Further evidence of path dependence in different setups can be found in e.g. d'Adda et al. (2017) who investigated the differences in spillover effects between nudges and push measures in different Ultimatum and Prisoner's Dilemma Games. Engl et al. (2018) demonstrated the spillover effects of an efficiency providing institution on a simultaneously existing inefficient institution. Buckenmaier et al. (2018) indicated lasting spillover effects of a sanctioning institution providing leniency to whistleblowers even after the removal. In addition, Peysakhovich and Rand (2016) illustrated how being exposed to more cooperative environments in a repeated prisoner's dilemma changed individuals to become more prosocial and punish selfish behavior in a subsequent one-shot game. Bruttel and Friehe (2014) implemented a public goods experimental design that is comparable to the one in this research. The authors analyze whether initially providing higher financial incentives to cooperate yields the subjects to keep high cooperation rates even after the incentives decrease. The results indicate that after removing high incentives the cooperation deteriorates and may become smaller than in the non-treated group. Summing up, the evaluation of path dependence is not completely unambiguous, yet the majority of cases indicates that prior institutional forms influence the behavior after an institutional change.

Since we implement a sophisticated experiment with several stages it is important to briefly analyze literature on specific learning and signaling effects. With respect to learning, the designs with and without communication are sufficiently different to assume the differences in contributions are less due to learning and more due to the communication. However, there is one possible exception. Communication does not solve the problem of the end-game behavior, as presented in Brosig et al. (2003). Thus, despite randomization, subjects could start learning this behavior. Selten and Stoecker (1986) introduce a learning theory for end-game behavior in repeated finite Prisoner's Dilemma games. According to this theory individuals learn when the other players start defecting from the optimal behavior and try to anticipate it in the next repetition. Finally, it is necessary to address the issue of signals. Due to the applied perfect stranger design and the structure in which only one block is chosen to be payoff relevant we do not assume major signaling between the blocks. However, in block three where individuals are first asked to fund the institution and then continue dependent on the size of their funding, signals can occur. As trust and regulation can operate bi-directionally (Aghion et al., 2010; Sliwka, 2007), several explanations become possible. An insufficient funding of communication can be interpreted as subjects expecting that they do not need communication anymore to achieve high cooperation rates. Alternatively, a failure to fund communication may be a signal of distrust, i.e. a group that insufficiently funds a second-order public good is more likely to insufficiently fund a first-order public good, as well.

The discussed literature leads to two hypotheses that are tackled by the experimental design. First, experiencing the more efficient VCM with pre-play communication yields the subjects to contribute more to the public good even after its removal and despite reshuffling groups as compared to the initial less efficient VCM. Second, failing to build an efficiency providing institution sends a signal of distrust within the group.

3.3. Experimental Design

In order to analyze potential long-time benefits of a behaviorally informed institution after its removal the paper utilizes the second part of the experiment conducted in Altemeyer-Bartscher et al. (2017). The design of the total experiment consists of three blocks[11]. At the beginning, the individuals are informed that only one of the three blocks will be cash-effective after the experiment. Therefore, the design ensures that every subject has the incentive to perform in the best possible manner in every block and that there is no income accumulation effect between

[11] Altemeyer-Bartscher et al. (2017) focuses on the analysis of the first two blocks and the investment in the institution, leaving aside the observations that followed in the third block of the experiment.

the blocks. Furthermore, the design includes a randomization process between the blocks. Following a round-robin design it is ensured that subjects cannot encounter each other after they were jointly placed in one group.

The block specific instructions are distributed and read aloud by the instructor prior to every block. The instructions are listed in Appendix 3.1. The core of every block is the standard VCM which is kept unchanged in every block for all participants. Hereby the pay-off function of individual *j* in period *k* is defined as:

$$\pi_{jk}\left(g_{jk}\right) = z\text{-}g_{jk} + \frac{\alpha}{n}\sum_{j=1}^{n} g_{jk}\,, \quad j=1,\ldots4$$

with the initial endowment (z) = 20 Laboratory Dollar (LD), the efficiency multiplier (a) = 2, g_{jk} representing the amount of LD subject j invested in period k. In every block, participants repeat this VCM for 10 periods in constant groups of four individuals. After every period the participants receive anonymous information on how much the other members of their group contributed. After the last period of the block the individuals receive information about their payoff for this block. While this procedure is identical for each of the three blocks, the second and third block contain additional communication mechanisms (i.e. an institution). Specifically, block one contains only the standard VCM, and in block two participants can engage in a non-binding pre-play communication prior to the standard VCM. The complete mechanism in block two is called Communication-VCM (C-VCM). All participants play through block one and block two, as the differences among treatments originate prior to block three. After finishing block two, participants are randomly assigned to different treatment groups: One sixth of the subjects is forced to repeat the VCM (i.e. the gameplay of block 1). Another sixth of subjects has to repeat the C-VCM (i.e., the gameplay of block 2) respectively. The remaining two thirds of the subjects are given the opportunity to jointly and simultaneously finance the communication institution.[12] This group of subjects is again divided equally between two different payment options: Half of them receive a refund option in case their group does not achieve the threshold value for the institution. The others have to hand in their invested money independent of whether the institution is formed or not (no refund). Those groups that achieve the threshold value proceed with the C-VCM (as in block 2) and those who do not continue

[12] The costs of the institution were in total 32 LD for a group of four individuals (8 LD per person on average). For reference please note that the theoretical outcomes for all individuals following the Nash Equilibrium strategy is 200 LD, all individuals following the socially optimal strategy is 400 LD, and the factual average benefit of the institution for every individual was approximately 64 LD. Therefore, the average benefit of communication was on average eight times higher than the average costs.

with the standard VCM (as in block 1). After the end of the third block the participants are informed which block became cash-effective and answer a questionnaire. The complete experimental structure is depicted in Figure 3.1.

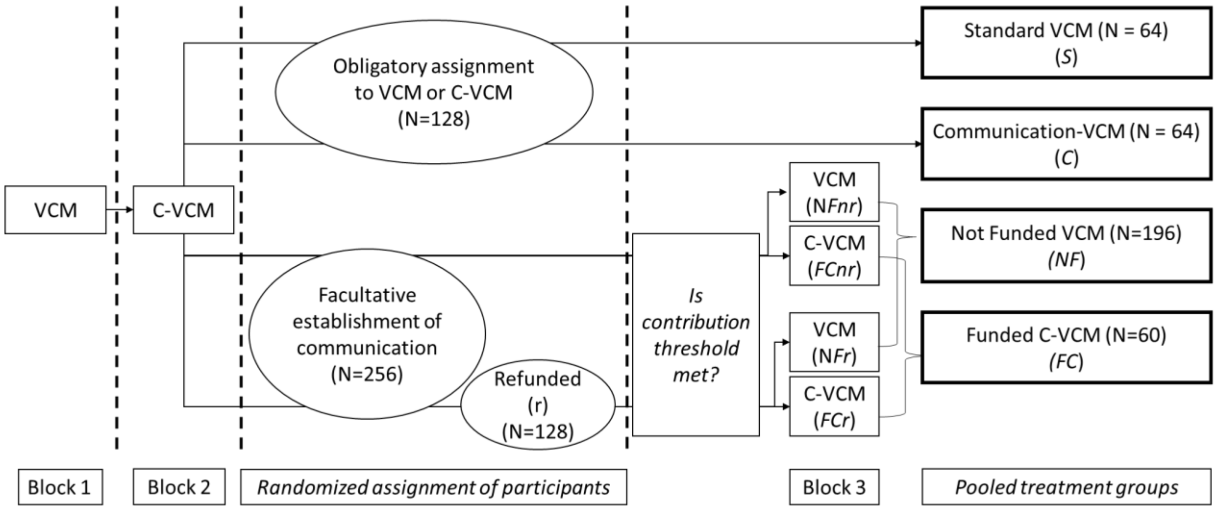

Figure 3.1 Experimental Design

Note: VCM: simple voluntary contribution mechanism; C-VCM: voluntary contribution mechanism with pre-play communication; No refund (nr): Individuals pay their investment independent of whether the threshold was met; Refund (r): Individuals pay their investment only if threshold was met. N depicts the number of subjects.

This design allows a within subject analysis with respect to the reverberation effect, i.e. how the prior experience of an efficient institution influences participants' later in-game behavior, and a between subject analysis to analyze potential signaling effects with respect to the funding of the institution, i.e. differences of contributions in groups where the institution formation was successful vs. groups where it was unsuccessful.

In total, the paper distinguishes between two major treatments and two endogenously formed groups which are illustrated in Table 3.1. The standard treatment (S) is composed of individuals that simply repeated VCM in block three. In the communication treatment (C) they repeated the C-VCM procedure. The Not Funded group (NF) consists of subjects who had the chance to fund the communication platform but whose group in total did not achieve the threshold. The subjects stem from both payment options (Refund and No Refund). Likewise, in the group Funded Communication (FC) there are those who met the financial threshold and therefore had C-VCM in the third block independent of having had the Refund option. The explanation for pooling the refund and non-refund options is provided in the result section. [13]

[13] As will be shown in the results section the two different payments schemes (Refund vs. no-Refund) do not influence average contribution behavior. Please note, that the significance of results would not change if regarded separately. This is further discussed in section 3.4.1. and displayed in tables 3.2 and 3.3.

Table 3.1 Overview of Groups

	Communication	No Communication
Funded with no Refund	FCnr } **FC**	NFnr } **NF**
Funded with Refund	FCr	NFr
Exogenous Provision	**C**	**S**

The participants were recruited from the subject pool of the Magdeburg Experimental Laboratory of Economic Research (MaXLab) and consisted of students from the Otto-von-Guericke University Magdeburg (Germany). In total 384 students took part in the experiment. For the composition of participants with respect to some demographic characteristics and different treatments see Appendix 3.2. The duration of experiments in total was between 70 and 90 minutes. After the end of the experiment, the payoff of one of the blocks was converted to euro (1 Laboratory Dollar = 4.5 Cents). The average payoff was around 16 €. The experimental design was executed in z-Tree (Fischbacher, 2007). The experiment was organized and recruited with the software hroot (Bock et al., 2014).

3.4. Results

The result section is divided with respect to the different types of analysis. Firstly, from the between-subject perspective, the paper examines whether having the option to choose to have an efficient institution matters. Secondly, from the within-subject perspective, the paper focuses on the effects of removing the efficiency providing institution.

3.4.1. The value of choice

To examine the effects of having the option to finance communication, the analysis focuses on the behavior of subjects in the third block of all four treatments. As depicted in table 3.2 and illustrated in figure 3.2 there are significant differences between subjects with communication in the third block and those without. However, to get a clear picture of the value of choice it is necessary to compare the treatments in which the participants had a choice with those where they were exogenously assigned to a game mode with or without communication. This means to compare: FC vs. C (with communication) and NF vs. S (without communication). The results from two-sided Mann-Whitney (MW) tests, which were conducted on the aggregates of all ten periods on the group level of contributions, are presented in table 3.2. Therefore, there were no significant differences between groups that had the opportunity to form an institution by meeting a financial threshold and those groups that were not given a choice to invest into the institution. This is true for repeating VCM (NF and S) and repeating C-VCM (FC and C).

However, in total, the groups that had communication (C-VCM) had a significantly larger average contributions than the groups that played without (VCM).

Table 3.2 Differences of Contributions in Block Three Aggregated on the Group Level over 10 periods and p-values of the MW-test.

	NF	S	FC	C	NF+S	FC+C
Mean	642.898	587.750	783.467	788.750	629.323	786.194
Observations	49	16	15	16	65	31
MW-test	0.2142		0.2966		0.0000	

This result has two implications: Firstly, giving the participants the option to finance the platform did not influence their behavior. Secondly, in statistical terms it allows us to pool the two groups for the analysis following in chapter 4.2. Further, it is important to stress that the two different payment schemes (refund and no refund) did not generate any significant differences for the contribution behavior in the third block as a whole. Table 3.3 illustrates that neither in the case of a successfully funded institution nor an unsuccessfully funded one did the type of choice have a significant effect. However, as the results are conducted on the group level there is only a limited number of observations, especially in the case of FC (15 observations). The findings are more robust for NF (49 observations). Therefore, the results need to be considered with caution.

Table 3.3 Differences between refund and no-refund treatments in Block Three aggregated on the Group Level over 10 periods and p-values of the MW-test.

	FCnr	FCr	NFnr	NFr
Mean	790.29	777.50	648.00	637.58
Observations	7	8	25	24
MW-test	0.4490		0.7793	

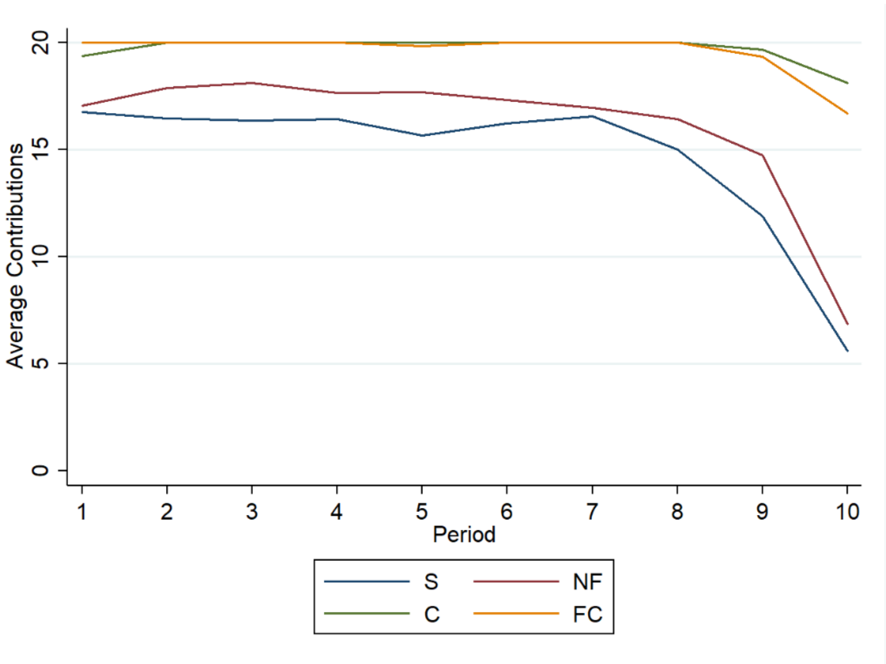

Figure 3.2 Contributions in Block Three

The only significant difference can be observed when analyzing the first contributions after the unsuccessful formation of the communication platform. Hereby, there is a certain mistrust effect. The individuals, who were willing to sufficiently contribute to the communication platform but whose group did not meet the threshold, contributed less to the public good at the beginning of block three. These results, displayed in table 3.4, are obtained from the following Tobit regression model:

$$FCB3_i = \beta_0 + \beta_1 INV_i + \beta_2 FCB1_i + \beta_2 FCB2_i + \beta_4 InstBen_i + \alpha_i + \varepsilon_i$$

with FCB3 representing the first contribution in block 3, INV the investment (financial contribution) into the communication platform, FCB1 the first contribution in block 1, FCB2 the first contribution in block 2, InstBen the benefit of having the communication[14], α set of control variables for the individual i.

Analyzing the first period of block three of those groups that did not successfully fund the institution independent of the refund option, it becomes apparent that high contributions to the institution have a negative effect on the first contributions. This is depicted in Table 3.4 columns (1) and (2). In the treatment with refund (3) and (4) the effect of high but futile contribution to the communication platform on first contributions in block three is negligible. This is reasonable since no participant factually lost their investment. However, withdrawing the refund option in the other treatment group induces the disappointment effect (5) and (6). The

[14] The individual institutional benefit is obtained from the individual differences in payoffs between the second and the first block.

effect of reduced contributions due to the failed institution however is strongly limited in time. Starting from the second contribution period the prior-game investments to the communication do not matter. Instead, the individual contributions depended more on the contributions of their co-players in the actual VCM as is shown in the next chapter (table 3.6).

Table 3.4 Mistrust Effect after the failed institution funding

	(1) NF	(2) + Controls	(3) NFr	(4) + Controls	(5) NFnr	(6) + Controls
Investment	-1.222** (0.537)	-1.264** (0.535)	-0.385 (0.657)	-0.363 (0.650)	-2.264** (0.965)	-2.349** (0.993)
FCB1	2.009*** (0.405)	1.982*** (0.400)	1.777*** (0.479)	1.779** (0.472)	2.435*** (0.729)	2.401*** (0.729)
FCB2	3.278** (1.660)	3.191* (1.662)	2.724* (1.545)	2.705* (1.551)	(omitted)	(omitted)
InstBen	0.009 (0.040)	0.008 (0.040)	-0.016 (0.048)	-0.018 (0.047)	0.029 (0.071)	0.024 (0.071)
Treatment	0.665 (1.517)	0.455 (1.512)				
Constant	-51.807 (33.699)	-38.694 (35.428)	-40.001 (29.998)	-25.195 (32.399)	15.315** (7.664)	27.084 (30.802)
Controls	N	Y	N	Y	N	Y
Observa-tions	196	196	96	96	100	100

Note: Standard error is denoted in brackets. ***/**/* denote significance at 0.01/0.05/0.1 levels respectively. Control variables include: gender, age, study program. The coefficients of FCB2 in the no refund treatment are omitted statistically due to lack of any variance (every participant contributed 20 LD).

3.4.2. The Reverberation effect

To examine the reverberation effect of an efficient institution after its removal, the contributions of the participants in block three are compared to the prior blocks one and two. The participants can be divided into two groups: those who played without an institution in the third block (forced or by choice) on one hand, and on the other hand the individuals that played with the pre-play communication (forced or by choice) before the standard VCM.

In short, the analysis (see table 3.5) shows that individuals who did not benefit from an institution in block three behaved differently than in the second block. Furthermore, the participants who played with an institution in block 3 showed almost the same behavior as in the second block (which was also a C-VCM-scenario). Both observations are illustrated in Figure 3.3 and are discussed separately in the next sections of this paper.

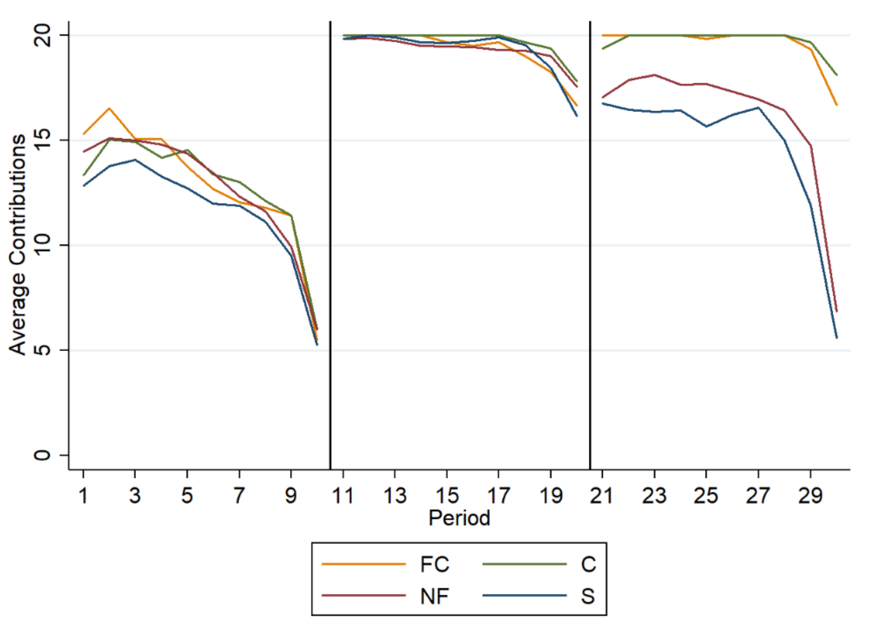

Figure 3.3 Average Contribution Rates in all Blocks for Respective Treatments.

Note: Periods 1-10 constitute block one, 11-20 block two, and 21-30 block three respectively.

Table 3.5 Differences in Contributions Between Second and Third Blocks

	Block 2 NF+S	Block 3 NF+S	Block 2 FC+C	Block 3 FC+C
Average	767.2269	629.3231	781.0081	786.1935
Observations	65		31	
Mann-Whitney	0.0000		0.4301	

3.4.3. The Reverberation effect for repeating VCM in block 3

In the analysis of repetition of the standard VCM (see Figure 3.3: NF and S in block one compared to NF and S in block three) two major observations can be made: firstly, the initial contributions are significantly higher in the third block than in the first one. This general observation contrasts the findings from laboratory experiments where the participants simply repeated the VCM as presented by e.g. Andreoni (1988), Andreoni and Croson (2008). This implies that the experiences in the second block (C-VCM), though being technically independent of the first and third blocks, induced positive spillover effects. Simultaneously, the contributions do not achieve similar rates as in the C-VCM itself (see Table 3.5, Block 2 NF+S & Block 3 NF+S). This is important as it shows that the one-time experience of the efficient C-VCM is not sufficient to induce equally strong long-term efficient behavior. Secondly, another notable difference between the two standard VCMs in block one and three is that in the former the contributions follow a steady decrease over time until there is a sharp decrease towards the end-game phase. In the latter, the contributions in the first seven periods are very stable and remain comparably high. However, in the end there is no difference between contributions in the last periods of block one and three. Thus, the end-game behavior is much more severe in block three than in block one (Figure 3.3). This is striking as there is no simple explanation for this behavior. Since by design the individuals do not know which block will be paid out and are incentivized to perform in the best possible manner in every block respectively. Thus, the approaching end of the total experiment is not a good explanation for such a sharp decrease in contributions.

Table 3.6 Panel Tobit Regression Results on Group and Individual Level for Individuals Without Communication (NF and S) in the Third Block

Dep. Variable: Contributions	(1) Basis Individual	(2) Interaction Individual	(3) + Control	(4) Basis Group	(5) Interaction Group	(6) + Control
Period	-3.060*** (0.173)	-1.889*** (0.218)	-1.889*** (0.219)	-4.704*** (0.275)	-3.745*** (0.393)	-3.739*** (0.388)
Block = 2	29.737*** (1.594)	41.875 *** (4.232)	41.837 *** (4.229)	69.273*** (3.375)	88.345*** (7.020)	89.313*** (7.076)
Block = 3	7.950*** (0.9574)	24.358*** (2.421)	24.378*** (2.422)	22.897*** (2.707)	31.452*** (4.324)	32.697*** (4.330)
Block (=2) x Period		-1.927*** (0.538)	-1.923*** (0.538)		-2.923*** (0.881)	-2.935*** (0.878)
Block (=3) x Period		-2.658*** (0.350)	-2.659*** (0.350)		-1.516** (0.592)	-1.551*** (0.586)
AvContr Co-players (-1)	0.941*** (0.036)	0.956*** (0.0363)	0.956*** (0.036)			
Constant	-0.922 (1.932)	-8.485*** (2.189)	-13.762* (7.879)	80.535*** (3.217)	75.332*** (3.573)	131.841*** (21.476)
Controls	N	N	Y	N	N	Y
Observations	7020	7020	7020	1950	1950	1950

Note: Standard error is denoted in brackets. ***/**/* denote significance at 0.01/0.05/0.1 levels respectively. Control variables include gender, age, study program on individual level or aggregates of these on the group level respectively. For the coefficients obtained for the variable block the first block was taken as the reference.

The results are further confirmed using two dynamic panel Tobit regression models with random effects (Table 3.6). The specification is based on findings of Ashley et al. (2010) who stresses its benefits and the fact that no other practically feasible estimator outperforms this specification (for an in depth discussion see chapter 2.4.1). For robustness we introduce a group level version of the estimator.[15] The individual level model is represented by:

$$Contribution_{it} = \beta_0 + \beta_1 AvContr_{-it-1} + \beta_2 t + \beta_3 Block + \beta_4 Block * t + \alpha_i + u_i + \varepsilon_{it}$$

with $Contribution_{it}$ being the contributions individual i provided at period t (1-10), $AvContr_{-it-1}$ the average contributions of the other three players in the group at the prior period, Block the respective block (1-3) which determines different institutional setups, α_i several control variables on the individual level. The limits of the model are at 0 and 20 LD. The group level model (with limits at 0 and 80 LD) is similar, yet leaves out the average contributions of the other members, since they are incorporated in the group contribution variable and uses control variables α_j on the level of the group j.

$$GroupContribution_{jt} = \beta_0 + \beta_1 t + \beta_2 Block + \beta_3 Block * t + \alpha_j + u_j + \varepsilon_{jt}$$

Consequently, the results from the regressions confirm that contributions in block three were significantly higher than in block one. They further show that the contributions in the end-game-phase decreased stronger than in the first block. To sum up, the communication platform has a lasting positive effect even for the third block for most of the rounds despite the randomization between the blocks. However, this effect is not stable but decreases in the course time towards the contribution rates obtained in the first block. Therefore, the efficiency providing institution in block two reverberates to the levels achieved before.

3.4.4. The Reverberation effect for repeated C-VCM in block 3

Turning the focus on the repetition of the C-VCM in the third block (see Figure 3.3: FC and C in block two compared to FC and C in block three) implies the question whether the participants were learning from repeating the C-VCM. Since there is no previous literature on repeating C-VCM no well-grounded presumption was a priori possible.

[15] Independent of the method chosen the results here remain robust. In both model types Tobit regressions are used due to censored data (0-20 on the individual and 0-80 on the group level). However, from the methodological perspective it is worth noting the different advantages and disadvantages of the models. The individual level model does not provide entirely independent observations, as the contributions of the individuals depend on the decision of others from period two onwards. This effect is diminished by controlling for the lagged behavior of other players. However, this also reduces the number of observations, since for the first period there are no average contributions of the other players in the previous period. The group level model provides independent observations. However the randomization of groups between the blocks yields technically unbalanced samples and thus leaving out a certain number of observations.

Table 3.7 Panel Tobit Regression Results on Group and Individual Level for Individuals With Communication (FC and C) in the Third Block

Dep. Variable: Contributions	(1) Basis Individual	(2) Interaction Individual	(3) +Control	(4) Basis Group	(5) Interaction Group	(6) + Control
Period	-2.703*** (0.257)	-7.763*** (1.261)	-7.766*** (1.260)	-4.193*** (0.351)	-6.760*** (1.139)	-7.180*** (1.211)
Block = 1	-29.990*** (2.361)	-76.430*** (11.477)	-76.446*** (11.465)	-69.287*** (4.713)	-91.448*** (10.162)	-96.849*** (10.995)
Block = 3	3.058 (2.321)	34.264 (21.197)	34.446 (21.193)	1.927 (3.595)	-6.482 (12.127)	-8.748 (12.745)
Block (=1) x Period		5.948*** (1.275)	5.946*** (1.273)		3.146*** (1.203)	3.571*** (1.270)
Block (=3) x Period		-3.323 (2.294)	-3.343 (2.294)		1.129 (1.524)	1.539 (1.579)
AvgContr (t-1)	0.639*** (0.055)	0.658*** (0.054)	0.657*** (0.054)			
Constant	39.862*** (4.744)	80.069*** (11.691)	70.858*** (14.660)	144.046*** (5.401)	163.443*** (10.127)	80.000*** (29.520)
Controls	N	N	Y	N	N	Y
Observations	3348	3348	3348	930	930	930

Note: Standard error is denoted in brackets. ***/**/* denote significance at 0.01/0.05/0.1 levels respectively. Control variables include: gender, age, study program on individual level or aggregated on the group level respectively. For the coefficients obtained for the variable block the second block was taken as the reference.

However, since the contributions in block two are on average at 96.46% there is almost no room for improvements. The regression results are depicted in Table 3.7. Further, the analysis of average contributions indeed does not yield any significant results. In fact, the contributions in block 3 appear to be slightly higher and more stable for a longer period. However, the end-game effect is equally present in the repetition of the C-VCM and thus, at the end the contributions declined to the same levels as in block two.

3.5. Discussion

The paper presented several findings worth of further discussion. This mainly addresses the issues of signaling and repeated voluntary contribution mechanisms. Before discussing these issues separately, it is important to address some methodological concerns. Parts of the analysis rely on small observation numbers. This is mainly due to the endogenous decision process. Thus, it was a priori difficult to precisely estimate the numbers of groups that will fund the institutions. However, this limitation does not concern the main findings on the reverberation effect but merely parts of the findings on the signals caused by the formation of the institution.

With respect to signals, the paper did not find any signaling effect of the (un)successful funding of the institution. The differences in contribution behavior between either having the option to fund the institution or being simply allocated to the respective scenario are shown to be not significant, i.e. contributions did not differ in the third block for participants in the C-VCM (FC & C) as well as in the VCM (NF & S). It is worth noting that the potential effect is a priori limited. Since the contribution rates in the second block were very high (see figure 3.2) the only measurable effect would have been decreasing rates. Yet, this was not observed.

However, the result from the VCM (without pre-play-communication, regardless of being forced or having a choice, i.e. NF & S) in the third block is more ambiguous than the results from the C-VCM in the third block. A priori it was reasonable to assume that not being able to fund the institution may send out a trust signal. Yet, as trust and regulation can operate bi-directionally (Aghion et al., 2010; Sliwka, 2007), it is not a priori clear whether the signal would be positive (i.e. not funding institution as a signal that the individuals do not require it and aim to achieve high contribution rates without spending the money) or negative (not funding the institution signals a lack of cooperation in a similarly structured dilemma within the group). Observational evidence (see figure 3.2) strengthens the positive interpretation of the signal, as contribution rates in NF were consistently higher than in S throughout all ten periods of block three. However, since the differences between the NF and S were insignificant ($p=0.2142$) this result should be viewed with caution. Yet, to conclude, no “endogeneity premium” (Bó et al., 2010) was found. However, in the case of this experiment the formation was costly and the motives in favor or against repeating communication differed among subjects due to different experiences.

With respect to the reverberation effect which is observed in a repeated VCM structure it is worth recalling the results from Andreoni (1988), Croson (1996), and Andreoni and Croson (2008). There the authors repeated the VCM procedure including randomizing the group members between the retakes, as it was done in the present paper. The authors observed that after every restart the contributions went up, yet they remained lower than the initial contributions in the first block. Furthermore, the contributions followed a similar pattern after the restart. These observations were not made in the experimental setup of this paper when repeating the VCM in block three. In lieu thereof, subjects who repeated the standard VCM had higher initial contributions and had a slightly different contribution pattern over time. This effect is likely to originate from block 2 which involved communication. One suitable explanation is that the combination of the inefficient VCM and the efficient C-VCM led to a more frequent emergence of a tit-for-tat type of strategy as described by e.g. Axelrod and Hamilton (1981) or Nowak and Sigmund (1992). The combined experiences of the VCM in the first and C-VCM in the second block potentially induced two observations: Firstly, it is financially beneficial to have high and stable group contributions. Secondly, once members of the group start free-riding it is evident that the cooperation will be breaking down. In order to not be exploited by free-riders the individuals adapted by faster reducing their own contributions in block three, while generally starting at higher contribution rates than in the first block's VCM. These two observations combined may be the cause for the illustrated reverberation effect of the institution. Further, these findings are in line with the learning theory of end-game behavior (Selten & Stoecker, 1986) and the research on multiple games environment of Grimm and Mengel (2012). The latter illustrated learning spillover effects and demonstrated how subjects learn to behave in strategically equivalent games in the same way. Yet, it is also worth noting that these findings are in contrast to Bruttel and Friehe (2014) who did not find such a reverberation effect. However, the design of this experiment differs in terms of how the subjects experienced the efficiency of the intervention. Thus, subjects first experienced the inefficient state and then the efficient one. Only after this experience stage, the paper discusses how the positive effect of communication reverberates in the next block. Further, in contrast to this paper, the authors applied formal measures to increase efficiency, i.e. higher marginal return on contributions or punishments. These measures did not achieve the high contribution rates that were obtained in this research due to pre-play communication and eventually backfired once removed.

3.6. Conclusion

The focus of the research was to analyze whether positive effects of a behavioral intervention are prevalent after its removal and how individuals react to the failure to finance the renewal of the institution. From the presented results of the experiment several conclusions can be inferred. Firstly, there is strong evidence that the efficiency providing behaviorally informed institution affected the contribution behavior even after it was removed – despite the groups being randomized. The positive experiences gathered in the C-VCM design led to positive spill-over effects in the following standard VCM design. However, the individuals also experienced the end-game effect and behaved accordingly to the fact that even the C-VCM design does not provide complete protection of being exploited towards the end of the game. Compared to the first VCM, in the VCM after having experienced the efficient institution the contributions were higher but were linked to a steeper drop of the contribution rates at the end of the game. As briefly discussed, this may be due to overlapping learning processes in different environments. This type of learning may be the main driver of the reverberating positive effect of the institution after its removal. Repeating the efficiency providing institution did not have any significant effects. This is foremost due to the fact that the contribution rates were already at an extraordinary high level. Concluding this element of the analysis, the experiment illustrated that the subjects were able to learn simultaneously from positive and negative experiences of the mostly identical mechanisms.

Furthermore, the paper provides evidence that it is not possible to draw a distinction between subjects opting for the repetition of the communication and subjects being forced to repeat it. The same result holds for potential positive signaling effects of meeting the financial threshold for the institution. Despite the observation that there is no significant signaling effect of (un-) successfully financing the institution, the exact mechanism design applied to finance it can matter yet only in a very specific case. Using treatments with and without refund showed that individuals in the treatment without refund who heavily invested into the institution but whose groups did not meet the threshold showed a short-time disappointment effect: They reduced their contributions to the public good at the beginning of the last block. However, the effect was only observable for the first period as afterwards the individuals focused on the contribution behavior of their co-players in the VCM.

In total the paper contributes to the analysis of behavioral institutions or more broadly behaviorally informed interventions in a public goods setup. It illustrates that behavioral institutions, even in case of being highly efficient, lose their beneficial effect over time. Previous research showed that behavioral actions, such as reminding free-riders to contribute or threaten them with already existing legal consequences, have an incremental effect on the contributions. The implications of this paper are twofold. On the one hand, it illustrates that even after removing the behaviorally informed tool the positive effects remain to a certain degree. This implies that policy makers would not have to apply such instruments continuously. On the other hand, the paper stresses the possibility of this effect to reverberate and eventually fade away completely if no measures are taken to solve the initial dilemma. Consequently, long-term path dependence based on the quality of the institution was not observed. However, the discussion is limited to behavioral measures and no implications are being made on formal institutions (e.g. punishing free-riders) since these often operate on a different channel.

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Appendix 3.1 Instructions

Instructions Experiment “Yellow“ (Block 1 + in treatment S: Block 3)

Please read the instructions diligently. If questions arise, open the door to your cabin and remain seated. The experiment “Yellow” is carried out at the computer.
Your fellow participants will only play with you within the experiment “Yellow“.

After reading the instructions you will receive four control questions. The control questions are not considered for your final payment. As soon as you have answered the control questions the part of the experiment relevant for your final payment will start. Please be aware that either experiment „Yellow“, „Blue“ or „Red“ will be paid out. Which experiment will be eventually relevant is decided by chance.

Within the experiment, we will use laboratory dollars as the used currency. The underlying exchange rate is the following: 100 laboratory dollars = 4.5 EUR.

You and three other participants receive each **20 laboratory dollars** per round of contribution. You can contribute these laboratory dollars either to a private or a group account.

Private Account (P): The deposited laboratory dollars are being kept.

Group Account (G): Each of the four players can deposit money in this account. The sum of the deposits is doubled by the experimenter and redistributed equally to the four players. Hence, each player receives 0,5 laboratory dollars per contributed laboratory dollar.

The laboratory dollars can be split up in between the two accounts. You take your decision anonymously. None of the other players will learn how you split your laboratory dollars up. Profit of player i is calculated accordingly:

$$\text{Profit}=(20\text{-}G)+0{,}5\cdot\sum_{1}^{4}G_i$$

Please turn!

Instructions Experiment „Red" (Block 2 + in treatment C: Block 3)

Please read the instructions diligently. If questions arise, open the door to your cabin and remain seated. The experiment "Red" is carried out at the computer.

Your fellow participants will only play with you within the experiment „Red".

After reading the instructions you will receive four control questions. The control questions are not considered for your final payment. As soon as you have answered the control questions the part of the experiment relevant for your final payment will start. Please be aware that either experiment „Yellow", „Blue" or „Red" will be paid out. Which experiment will be eventually relevant is decided by chance.

Within the experiment, we will use laboratory dollars as the used currency. The underlying exchange rate is the following: 100 laboratory dollars = 4.5 EUR.

You and three other participants receive each **20 laboratory dollars** per round of contribution. You can contribute these laboratory dollars either to a private or a group account.

Private Account (P): The deposited laboratory dollars are being kept.

Group Account (G): Each of the four players can deposit money in this account. The sum of the deposits is doubled by the experimenter and redistributed equally to the four players. Hence, each player receives 0,5 laboratory dollars per contributed laboratory dollar.

The laboratory dollars can be split up in between the two accounts. You take your decision anonymously. None of the other players will learn how you split your laboratory dollars up. Profit of player i is calculated accordingly:

$$\text{Profit}=(20\text{-}G)+0{,}5\cdot\sum_{1}^{4} G_i$$

Video conference: Before you take your decision on how to split the laboratory dollars you will be talking to the three other players in a video conference for three minutes. During this time, you can see and talk to each other. The duration of the call can neither be reduced nor prolonged.

Subsequently to the video conference, each player makes the above described decision.

Please turn!

Instructions Experiment „Blue" (Block 3 with No Refund)

Please read the instructions diligently. If questions arise, open the door to your cabin and remain seated. The experiment "Blue" is carried out at the computer.
Your fellow participants will only play with you within the experiment „Blue".

After reading the instructions you will receive six control questions. The control questions are not considered for your final payment. As soon as you have answered the control questions the part of the experiment relevant for your final payment will start. Please be aware that either experiment „Yellow", „Blue" or „Red" will be paid out. Which experiment will be eventually relevant is decided by chance.

Within the experiment, we will use laboratory dollars as the used currency. The underlying exchange rate is the following: 100 laboratory dollars = 4.5 EUR.

You and three other participants receive each **20 laboratory dollars** per round of contribution. You can contribute these laboratory dollars either to a private or a group account.

Private Account (P): The deposited laboratory dollars are being kept.

Group Account (G): Each of the four players can deposit money in this account. The sum of the deposits is doubled by the experimenter and redistributed equally to the four players. Hence, each player receives 0,5 laboratory dollars per contributed laboratory dollar.

The laboratory dollars can be split up in between the two accounts. You take your decision anonymously. None of the other players will learn how you split your laboratory dollars up. Profit of player i is calculated accordingly:

$$\text{Profit}=(20\text{-}G)+ 0{,}5\cdot \sum_{1}^{4} G_i$$

Set-up of the video conference: At the beginning of the experiment you are asked whether you want to make the experiment this time with or without communication. Communication will be subject to a fee. To make the experiment with communication you must raise a required amount jointly as a group. This amount will pop up on your screen at the beginning of the experiment. The decision on how much you contribute will then be again taken anonymously. The deposited money for setting up communication is being deducted from your profit in the experiment „blue" at the end of it – whether communication is successfully set up or not. If the group raises the required amount, a three-minute video conference is being set up, see previous round. Otherwise, all group members have to wait for three minutes until other groups have finished their communication period, respectively. Subsequently, the decision on how to split up the laboratory dollars between private and group account are being made.

Please turn!

Instructions Experiment „Blue" (Block 3 with Refund)

Please read the instructions diligently. If questions arise, open the door to your cabin and remain seated. The experiment "Blue" is carried out at the computer.
Your fellow participants will only play with you within the experiment „Blue".

After reading the instructions you will receive six control questions. The control questions are not considered for your final payment. As soon as you have answered the control questions the part of the experiment relevant for your final payment will start. Please be aware that either experiment „Yellow", „Blue" or „Red" will be paid out. Which experiment will be eventually relevant is decided by chance.

Within the experiment, we will use laboratory dollars as the used currency. The underlying exchange rate is the following: 100 laboratory dollars = 4.5 EUR.

You and three other participants receive each **20 laboratory dollars** per round of contribution. You can contribute these laboratory dollars either to a private or a group account.

Private Account (P): The deposited laboratory dollars are being kept.

Group Account (G): Each of the four players can deposit money in this account. The sum of the deposits is doubled by the experimenter and redistributed equally to the four players. Hence, each player receives 0,5 laboratory dollars per contributed laboratory dollar.

The laboratory dollars can be split up in between the two accounts. You take your decision anonymously. None of the other players will learn how you split your laboratory dollars up. Profit of player i is calculated accordingly:

$$\text{Profit}=(20\text{-}G)+0{,}5\cdot\sum_{1}^{4}G_i$$

Set-up of the video conference: At the beginning of the experiment you are asked whether you want to make the experiment this time with or without communication. Communication will be subject to a fee. To make the experiment with communication you must raise a required amount jointly as a group. This amount will pop up on your screen at the beginning of the experiment. The decision on how much you contribute will then be again taken anonymously. The deposited money for setting up communication is being deducted from your profit in the experiment „blue" at the end of it – only if communication is successfully set up. If the group raises the required amount, a three-minute video conference is being set up, see previous round. Otherwise, all group members have to wait for three minutes until other groups have finished their communication period, respectively. Subsequently, the decision on how to split up the laboratory dollars between private and group account are being made.

Please turn!

Appendix 3.2 Group composition in the experiment

Table 3.8 Group composition

Block	VCM/C-VCM/noRefund	VCM/C-VCM/Refund	VCM/C-VCM/VCM	VCM/C-VCM/C-VCM
Session	8	8	4	4
Subjects	128	128	64	64
Economists	65	57	29	31
Non-Economists	63	71	35	33
Male	66	78	36	32
Females	62	50	28	32
Average age	23.96	24.10	23.28	22.92

4. Predicting free-riding in a public goods game – analysis of content and dynamic facial expressions in face-to-face communication

4.1. Introduction

Communication is an elementary component of our society. It is an important factor in order to achieve stable cooperation among members of a group. A common way to investigate cooperation in groups is the public goods game which contains a conflict between individual interests and group interests. In the real world such public goods game scenarios occur in different contexts and various sizes. It may be as trivial as paying money into the kitty at the working place or as sophisticated as paying taxes. From the perspective of a social planner who needs the kitty to buy the coffee and the taxes to build the infrastructure, communication among group members may offer some assessment of the contribution behavior. Such an assessment can be used to predict whether the group is going to fund the public good sufficiently.

In order to illustrate this public goods dilemma within the scope of economic laboratory experiments, researchers apply the voluntary contribution mechanism (VCM) in which individuals form a group to fund a public good everyone benefits from. Socially optimal behavior implies all individuals to fund the public good consistently to the maximum extent. However, due to the mechanism's design, from the individual perspective it is beneficial to free-ride and simply take advantage of the contributions of the other group members. A very specific sub-topic of this issue refers to the end-game. The end-game in the context of the public goods game is the time when less and less contribution stages are left ahead and the incentives to deviate from the socially optimal contribution rates become higher. In practice this addresses everyday situations such as (not) contributing to the kitty yet drinking coffee paid out of it shortly before switching jobs or more general issues, such as (not) paying taxes shortly before leaving the jurisdiction of the tax officers.

The main practical economic question is how to ensure all individuals show the socially optimal behavior. Communication between the group members has been shown to play a crucial role in achieving this goal, as is discussed in the comprehensive overviews on public goods games by Ledyard (1995) and Chaudhuri (2011). Therein, face-to-face communication (FFC) was shown to be a very intuitive hands-on solution against free-riding. Several researchers focused on the question why communication prior to the standardized VCM leads to very high and stable contribution rates. Previous research, by e.g. Cason and Khan (1999), Brosig et al. (2003), Bochet et al. (2006) narrowed down the cause for this effect using different treatments (e.g. audio-communication without video, video-communication without audio), finding that the simultaneous exchange of information through verbal content and facial cues has the strongest effect to increase cooperation rates.

This paper adds to the research on the influence of preceding communication on the contributions to a public good by examining two factors: facial cues and verbal content of the communication (topics discussed and length of communication in number of words). In contrast to prior studies, the approach applied in this paper is partially based upon new technological possibilities trained specifically on the underlying data (Othman et al., 2019).

Using these methods, we extend the line of research from the general question of what influences contributions to a public good to: is it possible to identify groups that are going to provide socially sub-optimal contribution rates to the public good prior to their actual contributions? Based on these findings, the ultimate goal, therefore, could be to introduce additional interventions only when the prediction based on a priori available information concludes that the group needs another push (e.g. nudges, formal institution) towards the social optimum. Since it is favorable to limit this type of public interventions to a minimum it is beneficial to identify sub-optimally performing groups as precisely as possible. In the context of the underlying experiment in this paper, we will show that the predictions can be based on a priori available information: communication data (e.g. content or facial expressions) which precedes the contribution stage.

The general research goal to identify well-performing groups goes along with two questions. First, whether simply seeing and being seen by the other members of the group while discussing the problem increases the contributions and second, whether the efficiency gains yield from the contents discussed. FFC, being superior to other types of communication (Bochet et al., 2006; Brosig et al., 2003) not only enables coordination but also reduces the social distance among the participants. More recent analysis appended another explanation being that communication enables type detection of the interacting subjects (He et al., 2017). Since these aspects are barely tangible, we are not aware of successful measurements of them using currently existing technology. Thus, this paper aims to answer the question whether these processes can be approached technically using a facial action unit detection algorithm. To tackle this, a newly developed algorithm described by Othman et al. (2019) is applied and additionally supplemented by classifying simple content information transcribed from the original communication .

Given previous research, it is an expedient hypothesis that contributions depend on the content of the communication, since it helps coordinating the strategies. Contributions also seem to depend on facial expressions, which might serve as a channel to reduce social distance. The results of this paper indicate that some content-related information indeed provides explanation to group contributions at the end of the experiment. The experimental results indicate that groups which specifically mentioned that they aim to remain at the full contribution strategy until the end had significantly higher contribution rates in the last periods of the experiment Thus, they demonstrated much less of the otherwise typical end-game behavior. The evaluation of facial expressions enhances the analysis of which groups provided full contribution rates. Although there was only little deviation in the contribution rates over time, optimizing the model using the random forest algorithm led to increases in predictive power as compared to the baseline (so-called "trivial" classifier). The following analysis of the correctly identified sub-optimally contributing groups ("true negatives") supports the findings from the verbal content analysis. Groups that stopped active communication earlier, i.e. spoke fewer words or instead of communicating stared into the empty space at the end of the communication period, contributed less at the last stage of the VCM.

The remaining chapter is organized as follows. Section 2 provides a literature review. Section 3 briefly explains the experimental design and focuses on the data obtained from the transcribed communications, sections 4 and 5 present and discuss the results, and section 6 concludes.

4.2. Literature

Being aware of face-to-face communication mostly referring to in-person face-to-face communication we consider this paper as a contribution to the general FFC literature, as well. Bicchieri and Lev-On (2007) discussed that the closer the mediated communication comes to reproducing elements of in-person FFC the more similar are the two effects. While in-person FFC may include some additional explicit or implicit communication channels, i.e. body language (Van den Stock et al., 2007) or scent (Camps et al., 2014), these channels are not required to establish high levels of cooperation in a simple dilemma game such as the public goods game. This is supported by Brosig et al. (2003) whose computer mediated FFC treatment (video-conference) we replicate. Further, Brosig et al. (2003) were able to compare the in-person FFC with the computer mediated FFC and found no significant differences. Therefore, we simplistically assume that the important factors which increase the contributions in a public goods game are transferred through the content (e.g. by improving coordination) and mutual visual identification (e.g. by inducing trust and reducing social distance). It is theoretically possible that communications develop different patterns based on the medium as shown by e.g. van der Zwaard and Bannink (2014) in the case of video-conference and chat. Yet, we are not aware of analyses indicating that subjects discuss other topics or display facial expressions differently in public goods experiments when communicating via video-conferences as compared to sitting at a table. Based on this assessment we consider in-person FFC and computer mediated FFC likewise, at least in the simple context of a public goods experiment.

From early on, communication was subject to experimental research. From the perspective of economic research, it gained on importance with communication devices becoming more and more widespread (Bordia, 1997). The contributions by Dawes et al. (1977) and Isaac and Walker (1988) are hereby the first illustrating the unambiguous effect of communication in prisoner's dilemma problems with multiple players. Following the argumentation of Frank (1988) the clues for this beneficial behavior may occur due to different reasons such as facial or verbal expressions. The results were partially unexpected because the communication used in the experiments was de facto cheap talk and the theoretical effect of cheap talk in scenarios with strong incentives to lie is expected to be low (V. Crawford, 1998; Farrell, 1987; Farrell & Rabin, 1996). We argue that the communication constituted cheap talk not only in this traditional depiction but also in the more updated descriptions (Ying Chen et al., 2008; Kartik, 2009) as lying costs based on reputation are marginal, given the subjects are not able to identify free-riders after the experiment. Due to this observation, several studies were conducted in order to distinguish the transmission channels of the effect of communication.

Brosig et al. (2003) and Bochet et al. (2006) use separate treatments for each form of communication, e.g. face-to-face, audio-video, audio only, video only, chat. Both analyses confirm face-to-face types of communication (table conference and video conference) as the superior means of communication. Brosig (2006) provided an overview of different types of experiments which involved communication illustrating the effect of communication in several different experimental designs. One possible idea is that, besides assisting the coordination of group behavior, FFC reduces social distance. This can be interpreted as a degree of reciprocity individuals believe in within social interactions and which affects the individuals' behavior (Hoffman, McCabe, Smith, et al., 1996). However as Brosig et al. (2003) showed, simply reducing the social distance by providing short time (the individuals saw each other for 10 seconds) visual identification did not provide significant increments in contribution behavior. Nonetheless the hypothesis that facial expressions, such as seeing a happy face, affect human behavior in economic experiments found support (Eckel & Wilson, 2003).

Further, Haruvy et al. (2017) illustrated the interaction between communication and visibility in the laboratory and in a virtual world. In order to focus on the aspect of identification Andreoni and Petrie (2004) explicitly excluded the effect of changes in facial expressions. Using photos they argue that identification alone reduces free-riding and when combined with information it increases contributions to a charity. While these studies focused on static facial expressions, more recent analyses, e.g. Belot et al. (2012), Konrad et al. (2014), Sparks et al. (2016), Belot and van de Ven (2017), investigated dynamic expressions in the context of trust and deception detection using human assessment methods. Further, general evidence on the advantages of using incentivized economic experiments to analyze deception or cooperation was discussed by ten Brinke et al. (2016) and more specifically for facial expressions by Bonnefon et al. (2017).

Although some technologically advanced methods have been utilized in the past, they were sometimes used for other purposes. Fiedler et al. (2013) provided a study that focuses on the way humans gather information in public goods experiments by tracking the eye-movements of the subjects. In a simple sender-receiver experiment with biased transmission by Wang et al. (2010), deceiving senders had dilated pupils and reduced information gathering of the payoffs of the deceived receivers. The authors illustrated how obtaining and applying this information would increase the predictions of the true state and change the payoff allocation between the players. Further, research focused on the measurement of specific physiological characteristics and their effect on decision making as it is outlined in e.g. Sanfey et al. (2003), Kenning and Plassmann (2005), Glimcher et al. (2009), Dimoka et al. (2012), Al Osman et al. (2014) for neuro-economics or different types of biofeedback. Yet, in contrast to the analysis of facial cues, this type of analysis relies on less easily obtainable data.

Automatic analysis of social behavior using computer vision and machine learning algorithms is an emerging field of research (Aran & Gatica-Perez, 2011; Gatica-Perez, 2009; George & Leroux, 2002). The ultimate aim is to infer human behavior by observing and analyzing the interaction of the group conversation taken from the audio and video channels. Hopfensitz and Mantilla (2019) analyzed the performance of FIFA World Cup players based on their portraits. Jaques et al. (2016) trained deep neural networks using the facial expression of one-minute segments of the conversation to predict whether a participant will experience bonding up to twenty minutes later. In contrast to previous research we aim to predict contribution behavior in a financially incentivized public goods game after three-minute FFC applying automatic dynamic analysis of facial expressions. For the best of our knowledge this has not been done so far.

While the analysis of facial expressions in experimental economics is a young research branch which, due to technological improvements, is gaining on relevance, the content of communication is more researched. Brosig et al. (2003) used a simple yet effective classification of the content in a very similar experimental setup. The authors identified groups that discussed e.g. the optimal strategy, threats and end-game effect. Chaudhuri et al. (2006) implemented intergenerational advices in a public goods experiment and looked at the content of the messages and how much public these were. However, no statistical analysis was pursued in order to investigate the effect of the content on the contribution behavior. In a more recent analysis, e.g. Lopez and Villamayor-Tomas (2017), Palfrey et al. (2017), Arechar et al. (2017) looked deeper into the content of communication focusing on the relevance of information, strategic decisions communicated in it, or the level of truthfulness respectively. Using communication restrictions Zultan (2012) differentiated the effects of social and strategic communication prior to the ultimatum game. In another setup Chen and Houser (2017) analyzed the ability of individuals to detect deception with one major finding being that the number of words increases the trustworthiness. Further, the authors illustrate how mentioning specific content relevant words, e.g. money, influences the credibility. However, we are not aware of a content analysis of unrestricted and simultaneously happening face-to-face communication in a public goods game that is directly linked to the contribution behavior of the subjects.

4.3. Design and Data

The data was collected alongside a larger experimental setup described in Altemeyer-Bartscher et al. (2017). The total experiment consisted out of three blocks of which only block two and block three are useful since only those involved pre-play communication. For instructions please see Appendix 4.1[16]. The experiment was conducted at the University of Magdeburg. German subjects were recruited from a pool of participants at the Magdeburg Experimental Laboratory of Economic Research. Subjects communicated in German. To ensure a certain standard in German language only subjects that spoke German fluently took part in the experiment. Nobody was allowed to repeat the experimental procedure. Every session contained 16 subjects being randomly allocated to four groups of four individuals. After the experiment ended the individuals were paid off individually (in a group-independent order) in a separate room and had two different ways of leaving the laboratory area. In total 384 students

[16] The experiment consisted of three blocks. Every block had its own instructions which were color-coded (Yellow, Red, Blue). The first block (instructions Yellow) did not involve any communication. The instructions from Yellow are added for completeness only.

took part in the experiment. The duration of every complete session was on average around 80 minutes. The subjects were incentivized using real money with the conversion rate of 1 Laboratory Dollar = 4.5 Cents. Only one of the three blocks was paid out. However, since the subjects were informed that the decision which block is payoff relevant is conducted randomly after the end of the experiment, it is ensured that every block is correctly incentivized. After the experiment, the subjects filled out a questionnaire including some demographics. The experimental design was executed in z-Tree (Fischbacher, 2007). The experiment was organized and recruited with the software hroot (Bock et al., 2014).

The relevant parts of the experiment for the analysis in this paper are mainly block two (Instructions Red: including the first time communication – FTC) and parts of block three (Instructions Blue: second time communication – STC) of the complete experiment[17], where the participants had the chance to communicate face-to-face via audio-video communication software prior to the VCM. During the communication period of three minutes, the participants were free to discuss anything. The duration of the group communication was determined by using pilot sessions and in accordance with Brosig et al. (2003). Since the discussions were non-binding the communication constituted cheap talk. The VCM is described by the following profit function for individual j in period k:

$$\pi_{jk}\left(g_{jk}\right) = z\text{-}g_{jk} + \frac{\alpha}{n}\sum_{j=1}^{n} g_{jk}\,, \quad j=1,\ldots 4$$

with the initial endowment (z) = 20 Laboratory Dollar (LD), the efficiency multiplier (α) = 2, g_{jk} representing the amount of LD subject j invested in period k. The individuals repeat this decision 10 times in constant groups of four individuals.[18] After every of the 10 periods the participants receive anonymous information on the contributions of other subjects in their group. After the last period the individuals are informed about their total payoff for this block.

The face-to-face communication was recorded digitally. In order to analyze the content of the discussions, the communication was first transcribed and in a second step independently rechecked.[19] In order to assess several aspects of the discussion in a way that is less prone to

[17] By design of the complete experiment some participants were allowed to have the identical type of communication in the third block. However, due to randomization between the experimental blocks it was ensured that no individual in STC can meet anybody they have talked in FTC again.
[18] Given this design, the dilemma occurs as the payoffs are encircled within the three simplified scenarios: (a) 40 LD if all subjects contribute fully, (b) 20 LD if no one contributes anything, (c) 50 LD for the individual that freerides while the all others contribute fully and receive 30 LD respectively.
[19] Due to technical constraints it was not possible to automatize voice-to-text-transcription. The audio channel was codified by the recording software which strongly impedes distinguishing the different speakers. The video recordings and the manually transcribed discussion protocols are available on request.

subjective perspective of content, several meta parameters were chosen that are purely objective including the total word count of the conversations and the individual word counts of the respective subjects. Another set of variables was obtained using a classification scheme similar to the one conducted in Brosig et al. (2003). Table 4.1 summarizes these obtained variables[20] as well as how they are coded. Hereby it is important to differentiate between who these parameters address. The majority of the parameters focus explicitly on the group behavior. However, the individual word counts and the categorization on the main provider of information in the group refer to specific subjects and are analyzed on the individual level.

Table 4.1 Overview of content variables.

Parameter	Definition	Coding
Full Investment	The participant(s) mentioned to invest full contributions	"0" – no, "1" – yes
End-game awareness	The participant(s) mentioned that they should contribute fully until/in the end. (No explicit agreement required)	"0" – no, "1" – yes
Previous experiences	The participant(s) discussed experiences from previous block or prior experiments	"0" – no, "1" – yes
Threats and Consequences	The participant(s) "threatened" potential free-riders by explaining consequences, e.g. they will reduce their contributions.	"0" – no, "1" – yes
Disagreement	How many players (temporarily) disagreed with the optimal solution after it was mentioned	Numbers from 1 to 4
Information provider	Which player in the team explained the dilemma/solution of the dilemma (first).	Numbers 1 to 16 (linked to the specific individual in every session)

[20] The classification was conducted by two coders blindly and later rechecked using a semi-automatic approach. Hereby, we distinguished words and phrases (e.g. "last period", "last stage", "until the end" and so on) that are likely to appear when discussing the respective parameter (e.g. End-Game) and used a search function. In a second step the differently categorized results of the approaches were analyzed by the authors prior to and independent of linking the data to the experimental data. Although subjectively arguable, the categorization by the coders is more nuanced.

In order to assess the reproducibility of the coding it is essential to check for the interrater agreement rates. The agreement percentage rates for all the content variables lie between 87% and 100%. Given the variety of different measures for interrater agreement (Krippendorff's Alpha, Cohen/Conger's Kappa, etc.) some of the most prominent measures are listed in Appendix 4.2. In total the agreement rates are at a high level. This can be explained by the easy identification of parameters since most of the issues just needed to be mentioned at least once in order to be coded as present. The weakest agreement is found for the variable End-Game. This may be due to different ways to mention the end-game problem. While some of the subjects explicitly mentioned terms like "last period" others discussed the issue of decreasing contributions over time in more general. This made the decisions of the coders to be more subjective. For this case and any other where the vote of the coders was not identical several approaches are possible. First, we can proceed with the analysis of only those cases where the decision of the coders was identical. This however would lead to many different sample sizes. Second, we can add up the binary votes of the coders and consider them as a measure of salience of a verbal statement, i.e. if only one coder identified this as true the statement was less salient than when both coders identified the statement as true. Therefore, we implement the second approach for our analysis. The only parameter where this cannot be done is the question who provided the information. For this question we only consider unanimous decisions of the coders.

4.4. Results

Similar to previous research, face-to-face communication had a strong effect on contribution rates. This, however, caused a problem with respect to the evaluation of the possible explanations. Figure 4.1 illustrates that there is not much variance in contributions. Instead, contributions remained stable at a very high level throughout the first nine periods. The last period symbolizes the typical end-game behavior of subjects. Therefore, there are much more defectors, which causes more variance in the contribution data. Thus, it is only statistically plausible to focus the analysis on the last period of the experiment. Therefore, the result section focuses basically on the end-game behavior and the question what parameters are interrelated with making contributions more stable over time. In the first part of the results section, the predominant issue is the content-related analysis of the experiment. The second part of the section discusses the effect of facial expressions on the contribution behavior based on the approach the authors presented in Othman et al. (2019).

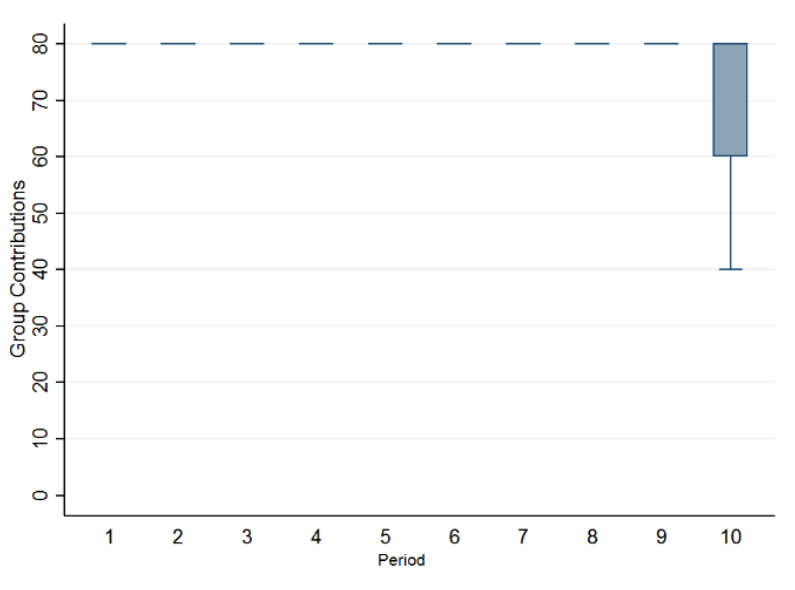

Figure 4.1 Boxplot of group contributions over ten periods (outliers not displayed). Due to communication almost all groups contributed socially optimal rates of 80 LD.

Before the actual analysis it is necessary to stress two limitations. First, the analysis focuses only on the final period. Theoretically this imposes a problem since players wo act as conditional cooperators adjust their contributions with respect to contributions of the other players. We argue that this is less of a problem since there is almost no variance in the first nine periods and thus no reasonable adjustments can be made. Still, there is a certain distance between the communication and the final contribution period. However, as the individual contributions periods did not take much time (a total of approximately 3 minutes for the first nine periods), the likelihood that subjects forgot the content of the discussion is relatively low.

Second, it is important to stress that we refrain from making causal claims. Despite the analysis being founded on a laboratory experiment, there are evidently no treatments in place to test for causality. The research is mainly exploratory. The goal is to assess whether facial visual cues and buzz topics can help predict future cooperation behavior and how these currently independently conducted approaches can be linked in future research. Further, the question whether the communication affects group contributions or the individual preferences towards contribution influence the communication cannot be answered based on this data set. Yet, it is plausible to assume an interaction between these factors.

4.4.1. Content Analysis

The analysis of the content is based on the transcribed communication protocols of the group discussions.[21] Since it is arguable whether combing first time communication and second time communication is generally possible, the analysis is conducted separately for these cases. This reduces the observations to 96 for FTC and 31 for STC. The descriptive statistics of the variables, obtained from the communication protocols, are displayed in Table 4.2. Several findings can be obtained from this summary. Virtually every group was able to find and agree on the socially optimal solution of full contributions. The variables End-game, Experiences, and Threat vary among the groups. However, only the first one has a significant effect on the contribution behavior as it is depicted in Table 4.3 for both blocks. This is less surprising since the dependent variables provide variance only in the end-game part of the experiment and the parameter End-game specifically focuses on whether the group is aware of the problem. Groups that discussed the issue had significantly higher contribution rates (FTC: 75.9 LD; STC: 78.2 LD) in the last period than groups that did not mention this issue (FTC: 64.3 LD; STC: 65.7 LD). Discussing previous experiences or threatening co-players in case of free-riding did not have any significant effect. The combination of these observations yields the possible conclusion that the experimental setup might have been too easy for the communication. In fact, it appears to be enough to simply discuss the optimal strategy and recollect the end-game problem to achieve extraordinarily high and stable contribution rates. The benefits of this strategy are high. Furthermore, in the end, virtually all subjects agree on the strategy. The conjecture of the VCM being too easy for the communication goes along with previous research (Brosig, 2006) mentioning the role of the complexity of the problem.

Furthermore, using simple metadata such as the total number of words spoken by the group enhances the findings. The mean of words spoken in the communication (248) is taken as a naive threshold to distinguish between two groups. Running the tests on the group level on these binary variables yields the conclusion that the more words were spoken the better was the cooperation in the last periods. Surprisingly this holds only for the FTC but not for the STC. Here the group contributions are virtually identical as is displayed in Table 4.3.

[21] Likewise to the subsequent analysis of facial expressions it is theoretically possible to automatize the process of content analysis, as e.g. proposed in (Penczynski, 2019). This was not done mainly for two reasons. First, in the course of the recording process the voices were saved using one channel though coming from four different sources. An automatic a posteriori mapping of voices to the individuals in the group was not possible with a sufficient reliability. Second, the language used was often very colloquial German. Any other automatic or semi-automatic analysis would have required establishing or improving the word libraries as well as a certain classification for methods like e.g. sentiment analysis. This, in turn, should be similar to the method chosen in this paper, yet require a substantially higher effort.

Table 4.2 Descriptive statistics of content variables and meta parameters for FTC and STC

	First Time Communication					Second Time Communication				
Variable	Obs	Mean	SD	Min	Max	Obs	Mean	SD	Min	Max
End Game	96	.323	.435	0	1	31	.387	.460	0	1
Invest All	96	.979	.144	0	1	31	1	0	1	1
Disagree-ment	96	.083	.268	0	1	31	.097	.301	0	1
Prev. Expe-rience	96	.411	.459	0	1	31	.774	.405	0	1
Threats/ Conse-quences	96	.281	.422	0	1	31	.355	.469	0	1
Total word count	96	244.760	118.015	33	516	31	260.710	133.283	18	470
Ind. word count	382	61.521	55.108	0	306	31	65.715	62.004	1	237

Note: The values for content parameters stem from two coders and are normalized to 1. Thus, the mean value denotes the percentage of cases in which the respective variable was identified by coders. We provide the observation numbers on group level in all categories but the last one (individual word count). There, observations are coded on the individual level.

Table 4.3 Effect of mentioning the End-Game and length of communication on group level contributions.

Group Contributions	End Game = 1	End Game = 0	Above-average word count	Below-average word count
FTC	75.862 (0.004)	64.328	71.392 (0.053)	63.756
STC	78.2 (0.033)	65.714	69.1 (0.454)	70.909

Note: p-values of two-sided MW-tests are depicted in brackets. For the binary analysis of the end game, we only include cases where both coders voted unanimously.

In order to provide robustness with respect to the naively chosen threshold and ensure the effect was not driven by other variables, this paper presents regression results with different control variables on the group level. Due to dealing with censored data on group level the analysis utilizes Tobit regressions with boundaries at 0 and 80. To complete the analysis on group level several demographic values were aggregated to a group level, i.e. number of males or economists in the group and the sum of the ages of the individuals. The results are provided in Table 4.4 for the two blocks separately. Hereby the length of communication is significant in

FTC and insignificant in STC. This holds despite adding the strongest content variable End-Game. Still, Table 4.4 indicates that there might be some hidden differences in communication in FTC and STC.

The table further shows that the End-Game variable, on contrary to the word count, is significant in FTC and STC. A possible explanation is that breaking the factually non-binding promise to keep high cooperation until the very end induces psychological costs to the individuals. Therefore, making such an agreement increases the cooperation. From the logical point of view, such a promise can only come up at the end of a specific train of thought. First, the group has to find the socially optimal strategy being fully cooperative. Second, the group members have to agree on their future contribution behavior. Only then it is reasonable to discuss the specifics towards the end of the game. This further provides a possible explanation why the length of communication matters only in FTC. The groups could have learned from their FTC and discussed the end-game earlier. Based on this idea it is plausible to assume that discussing the more negative experiences in FTC yields contributions to decrease while discussing the more positive experiences in STC yields contributions to increase.

While prior results were conducted on the group level this paragraph pursues an analysis on the individual level. The analysis focuses on the question who explained the contribution strategy in the team. Besides analyzing the information providers, it was possible to analyze the number of words communicated and relate both to basic demographic information, such as gender and field of study. In a simplified manner this implies the analysis of who became the leader during the communication. The distribution of demographics in the experiment can be found in Appendix 4.3. The results of this analysis are depicted in Table 4.5. Despite the logical idea that economists are more familiar with the public goods dilemma due to their study or presumably higher experience in public goods experiments, the analysis does not indicate economists taking the lead neither in the qualitative variable (information provider) nor in the quantitative variable (talker – the person who talked the most in the group). This holds for the first communication (in block two) and the second communication period (selected groups in block three).

Even though according to MW-test there is a mild effect for economists in STC ($p=0.062$) the significance does not withhold for Bonferroni adjustment. Yet, while there is no effect of economic education, there is a gender effect. Male subjects contributed significantly more to the communication. In block two (three) 32.0% (31.3%) of all males were coded as information providers while the share of information providers among females was 15.9% (12.2%)[22]. The results obtained for the variable talker are almost identical due to a high correlation between these variables, i.e. the individuals that explained the dilemma were also those who spoke the most in the group. However, we indicate both variables to illustrate the robustness. As it was necessary to conduct several MW-tests we further implement a simple OLS regression clustered at the group level. The results remain significant.

Table 4.4 Tobit regressions of contributions on group level for FTC and STC.

	FTC			STC		
	(1)	(2)	(3)	(4)	(5)	(6)
Total word count	0.160** (0.064)	0.167** (0.064)	0.205*** (0.071)	0.025 (0.083)	0.028 (0.082)	-0.171 (0.101)
Number of economists		4.827 (7.242)	0.326 (6.478)		2.341 (14.372)	3.366 (13.182)
Number of males		-3.243 (6.593)	-5.477 (6.137)		-6.036 (11.332)	-16.140 (11.513)
Aggregate of age		-0.506 (0.976)	-0.745 (0.869)		3.289* (1.893)	2.879 (1.706)
End-game			17.042** (8.400)			43.374** (18.523)
Invest all			34.902* (17.841)			(omitted)
Subjects against			-16.692 (11.040)			8.842 (21.194)
Previous Experience			-12.475 (7.653)			16.947 (11.651)
Threats and consequences			-6.736 (7.967)			28.534 (17.839)
Constant	63.492*** (15.054)	107.305 (89.452)	70.092 (82.141)	95.985*** (24.448)	-202.146 (171.12)	-164.268 (151.935)
Number of Observations	96	96	96	31	31	31

Note: Standard error is denoted in brackets. ***/**/* denote significance of the coefficients at 0.01/0.05/0.1 levels respectively. The variable *Invest All* was omitted in STC due to lack of variation. See Table 4.2 for descriptive statistics.

[22] Please note that by definition one out four individuals was coded as information provider. The fact that the respective numbers do not on average yield 0.25 is due to a slightly higher share of males in the total sample.

Table 4.5 Analysis of communication leadership with respect to specific demographics.

		Male	Female	Economist	Non-Economist
FTC	Talker	0.344	0.145 (p=0.000) (p=0.000)	0.269	0.243 (p=0.550) (p=0.650)
	Information provider	0.320	0.159 (p=0.000) (p=0.015)	0.237	0.257 (p=0.666) (p=0.295)
STC	Talker	0.362	0.109 (p=0.001) (p=0.017)	0.295	0.206 (p=0.256) (p=0.398)
	Information provider	0.313	0.122 (p=0.017) (p=0.027)	0.158	0.305 (p=0.062) (p=0.238)

Note: p-values in brackets. The first p-values are obtained from two-sided MW-tests where votes of coders were unanimous. The second p-values are obtained from simple OLS regression clustered at group level with votes of coders added together. Results are based on 384 observations in FTC and 124 observations in STC.

The results presented so far represent standard analysis approaches with respect to the VCM. In order to achieve comparability with the subsequent analysis of facial expressions, which by its structure purely aims to predict group contribution behavior, we extend the section by providing a very simple prediction model based on all content variables that were obtained on the group level. Hereby the focus lies upon the binary distinction whether groups contributed the full amount or deviated from it. This strict threshold is in line with the initial research question this aims to distinguish groups that do not need further institutionary support to achieve socially optimal contribution rates and those that do. Thus, theoretically any group that is predicted to show non-optimal contributions may become subject to an intervention from a social planer. The eventual prediction originates from a simple logit model with the following structure:

$$\widehat{FC} = \beta_0 + \beta_1 NW + \beta_2 EG + \beta_3 IA + \beta_4 SA + \beta_5 PE + \beta_6 TC + \varepsilon_i$$

with FC being the binary prediction whether the group provided full contributions, NW – number of words, EG – end-game, IA – invest all, SA – subjects against, PE – previous experience, TC – threats and consequences. Since the initial analysis of contents indicated some potential differences between FTC and STC the predictions are conducted for the two blocks separately. As the results of the logit model do not provide binary estimates but the probability for the value to be one (i.e. full-contribution) the accuracy of the predictions depends on a specific decision rule. This refers to the question from which probability onwards to assign the binary variable as a prediction for full-contribution. For example, a naïve threshold may be a probability of 50%. Thus, whenever the probability, that the binary estimate is one, is at least 50%, the final prediction is one. As there is no prior knowledge on the choice of threshold in such scenarios, it is reasonable to investigate different thresholds and the respective effects on the accuracy. The subsequent Figures 2 and 3 present the results for the FTC and STC respectively. Several findings can be obtained from this analysis. Firstly, the predictions for FTC are on average worse than for STC. The most efficient thresholds lies between 30% and 60% and yield accuracy rates of 75% and 74% for the two blocks respectively. It is arguable whether it is suitable or necessary to analyze the two blocks separately. Yet, we can provide a possible rationale. Investigating the simple descriptive statistics (Table 4.2) there is one major change to observe. There is a strong and significant (p-value of two sided MW-test: 0.0003) increase in the number of groups that discussed previous experiences. While it is not possible to link the increment in accuracy to this single variable, there are reasonable differences with respect to what experiences the individuals had at the different points in time. In the FTC the only experience from the experiment was the communication-free experimental set up that yields lower payoffs and second-tier experience from previous experiments in the laboratory. In the STC the experience of the more successful pre-play communication setup of the experiment is more salient. As these past experiences influence the content, this justifies a distinct analysis of FTC and STC. The results are depicted in Figures 4.2 and 4.3.

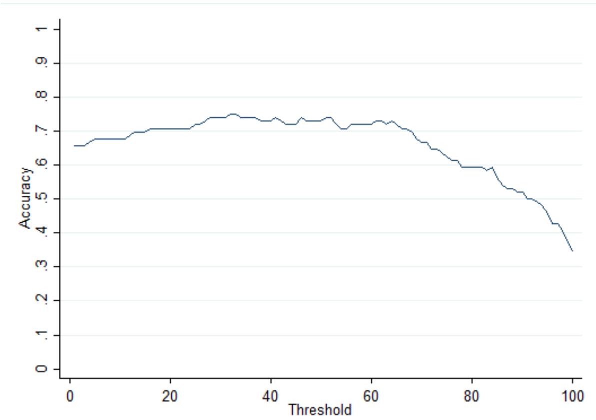

Figure 4.2 Accuracy of logit predictions depending on the classifying probability threshold in percent for FTC *excluding* demographics

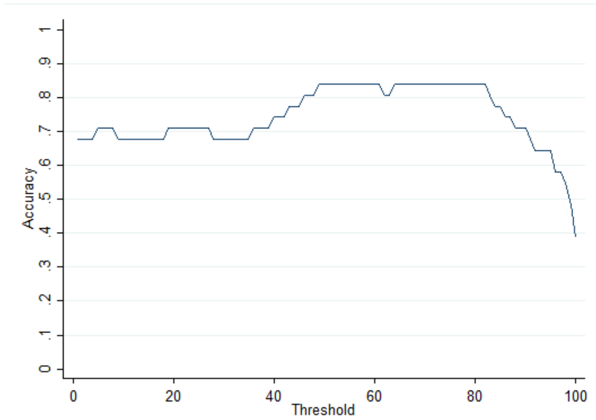

Figure 4.3 Accuracy of logit predictions depending on the classifying probability threshold in percent for STC *excluding* demographics.

Ultimately it is worth noting that the given predictions are based purely upon the content variables. On the contrary to the aforementioned Tobit-regressions the available demographics are not added into the model. This limitation is used as the demographics were obtained from a questionnaire following the entire experiment. Therefore, it is arguable whether this information is a priori obtainable and can be used to predict contribution before they take place. However, we analyzed the effect of including the demographics for one major reason that builds the bridge to the subsequent analysis of facial expressions. Research has shown that it is possible to predict gender and age with a substantial accuracy even based on random photos available on the internet (Levi & Hassner, 2015). By improvements this classification became better and can be applied on videos (Han et al., 2018). If the algorithms are to achieve the same quality as self-reported data in the questionnaire this would have an effect on the prediction accuracies in FTC (to 75%) and STC (to 84%) as is illustrated in Figures 4.4 and 4.5. However, we cannot provide logical explanation why there is only an increase in accuracy rates for the STC.

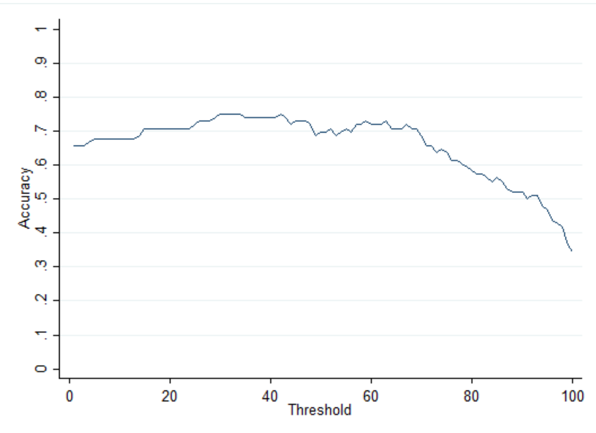

Figure 4.4 Accuracy of logit predictions depending on the classifying probability threshold in percent for FTC *including* demographics

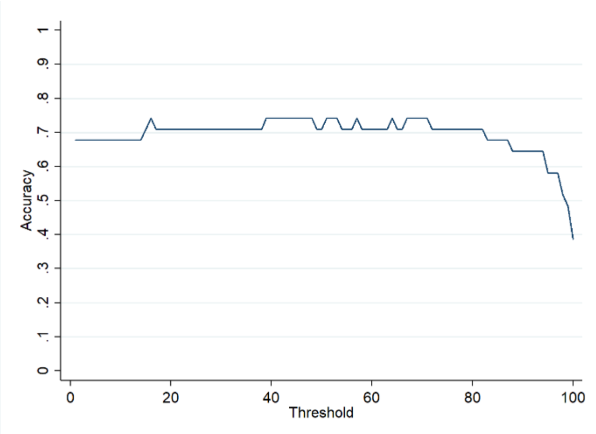

Figure 4.5 Accuracy of logit predictions depending on the classifying probability threshold in percent for STC *including* demographics

4.4.2. Analysis of facial expressions

Subsequently, a video-based automatic facial expression analysis is presented examining 3 minutes of communication to predict the end-game behavior of the groups. This chapter briefly displays the chosen method that is described in more detail in our methodological analysis (Othman et al., 2019). To predict the end-game behavior of the groups, a binary classifier was trained that predicts whether all 4 participants of a group will contribute fully in the very last period of the experiment or if anyone deviates, i.e., the binary classifier doesn't predict the contribution of each participant but likewise to the analysis in section 4.1 for the entire group. The dataset consists of 127 different groups divided into 24 sessions. The same subject might appear at most in two groups, but only within the same session. Therefore, to train person-independent models the analysis uses leave-one-session-out cross-validation. This ensures that no subject appears in the training and test set simultaneously.

Each face-to-face communication (FFC) video has four participants. First, the facial activity descriptors are calculated for each individual face and video frame. Then, the activity descriptors of every individual are concatenated in all 24 possible ways to form the group activity descriptors. All group activity descriptors get the same label (see next paragraph FADs and GADs). This approach increases the dataset, which is favorable in this case since the 127 FFC videos constitute a comparatively small dataset for a machine learning application. Finally, all 3048 (= 24 * 127) group activity descriptors are classified individually, and the classification outcome is averaged for every group to obtain the prediction score.

Using OpenFace (Baltrusaitis et al., 2016) facial features from each individual face and frame are extracted. OpenFace first detects the face, facial landmarks, estimates eye-gaze, head pose, and extracts facial action units (AUs) (see Figure 4.6). The list of facial features used in our analysis is depicted in Table 4.6. The list does not contain all possible features. Instead, only those features were used that OpenFace can estimate robustly (Baltrusaitis et al., 2016): 3 head pose features (yaw, pitch, and roll), 8 AU intensities features, and 10 AUs presence features, leading to a total of 21 frame level features. The FAD of each individual is extracted from the selected facial features for all frames in the FFC videos using the method of (Saxen, et al. 2017). After calculating the 4 FADs of the group, they are concatenated in all 24 possible ways (e.g. 1234, 1243, 1432, etc.) to form the GADs. Each GAD is given a group label.

Table 4.6 List of well-performing facial features based on OpenFace's own analysis

Head pose	Action Units			Action Units *(cont.)*		
	AU	AU Full name	Prediction	AU	AU Full name	Prediction
Yaw	AU1	Inner brow raiser	P	AU15	Lip corner depressor	I
Pitch	AU2	Outer brow raiser	I&P	AU17	Chin raiser	I
Roll	AU4	Brow lowerer	P	AU20	Lip stretched	I&P
	AU5	Upper lid raiser	P	AU23	Lip tightener	I&P
	AU7	Lid tightener	I	AU26	Jaw drop	P
	AU9	Nose wrinkler	P	AU28	Lip suck	P
	AU14	Dimpler	I	AU45	Blink	I&P

Note: I – Action Unit intensity: 0 (absence of action unit), 1 (faint) to 5 (strong), P -Action Unit presence (0 absent, 1 present).

Our model uses leave-one-session-out cross-validation using Random Forest classifiers[23], i.e. the GADs of one session are held out for testing while the rest form the training set. In total, 24 sessions provide 24 results. The classification outputs for the 24 GADs per FFC videos are simply averaged and thresholded, whereas an optimal threshold is calculated based on the training set. The performance measure used is accuracy. For reference, an informed guess (trivial classifier) was calculated in each fold, which always votes for the majority class (usually full contribution). It provides 24 different results, depending on the distributions of the test sets. Based on the entire dataset, the average precision of the trivial classifier was 64.47%.

Since it is unprobable that the whole FFC is equally likely to predict future behavior, in the best case scenario the analysis could be limited to the facial expressions occurring in the aftermath of specific contentwise relevant statements. However, due to the aforementioned technical difficulties, it was not possible to synchronize the content with the facial expressions. This yields another approach. To investigate which part of the FFC video is more important for predicting contribution behavior, we process the FADs from different parts of the FFC video. First, we divide each FFC video into 3, 4, and 5 equal long videos (splits). Second, we extract the FADs and GADs of these 12 different splits (3 + 4 + 5 = 12) and train a model for each split and multiple combinations (33 in total, see the details in Othman et al., 2019). We introduce three different categories containing models from different split combinations. These are referred to as beginning models, end models, and combination of beginning and end models. Each category includes 11 splits or combinations that belong to the particular part (beginning, end or both parts) of the FFC video. Each category provides 264 (11 splits * 24 results / split) different results. Table 4.7 shows a completeoverview of different parameters for the automatic analysis of facial expression applied in our analysis.

[23] For more detailed information on the utilized parameters consult Othman et al. 2019

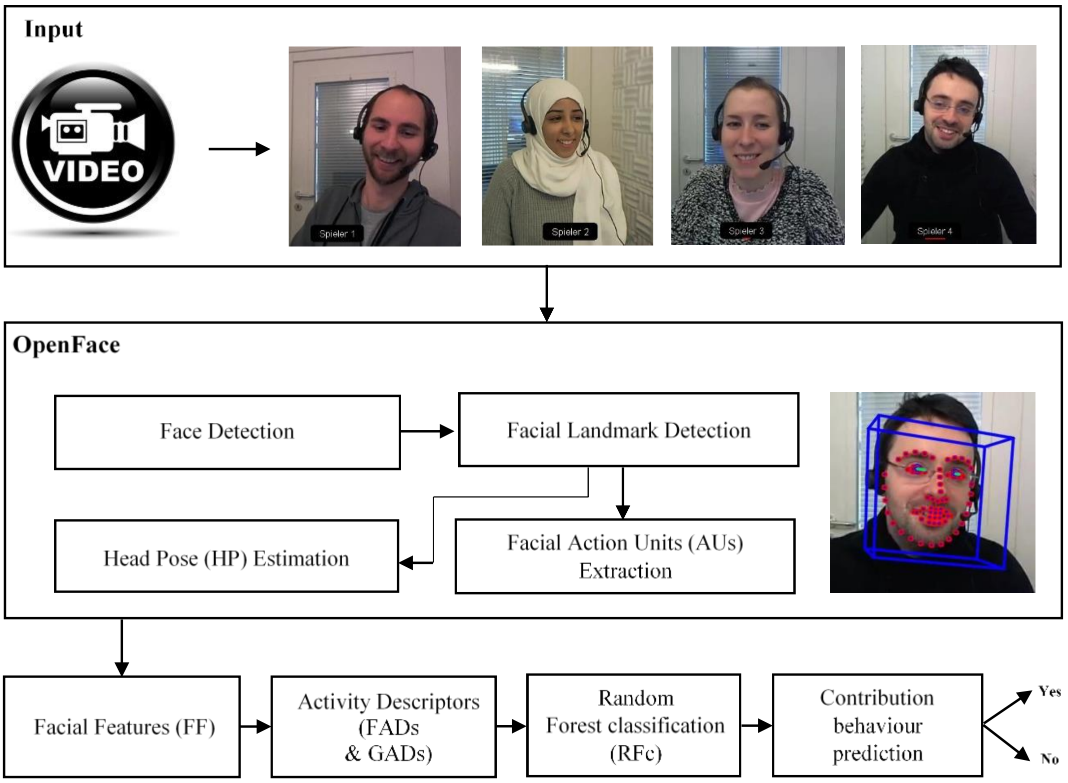

Figure 4.2 Automatic analysis of facial expressions

Note: The Automatic analysis of facial expressions from FFC video (4 players per video) using OpenFace, FADs, GADs and RFc to predict the contribution behavior of groups in the last period. OpenFace includes: face detection, facial landmark detection, head pose tracking, and facial Action Unit estimation (see Othman et al. 2019). Pictures were obtained from a replication of the original setup.

Table 4.7 Overview of different parameters for the automatic analysis of facial expression.

<table>
<tr><th colspan="2">Entire dataset</th><th colspan="3">Each FFC video</th><th colspan="2">Each training set</th></tr>
<tr><td rowspan="2">Videos</td><td rowspan="2">127</td><td colspan="2" rowspan="2">Splits
(division of the video into 3, 4, 5 splits)</td><td rowspan="2">12</td><td>categories</td><td>3</td></tr>
<tr><td>Models</td><td>33</td></tr>
<tr><td>Sessions</td><td>24</td><td rowspan="3">Categories
(multiple combinations of 12 splits)</td><td>Combined splits</td><td>11</td><td colspan="2">each model</td></tr>
<tr><td colspan="2">Leave-one-session-out cross-validation</td><td>Beginning splits</td><td>11</td><td>Frame level features</td><td>21</td></tr>
<tr><td>Training set</td><td>24</td><td>End splits</td><td>11</td><td colspan="2">Each test set</td></tr>
<tr><td>Test set</td><td>24</td><td colspan="3">Each split</td><td rowspan="2">Results of each category</td><td rowspan="2">11</td></tr>
<tr><td colspan="2">Final result: average accuracy of the test set</td><td colspan="2">GADs</td><td>24</td></tr>
</table>

The results show that predicting group behavior based on facial visual cues from the FFC video is complex and only slightly better than guessing (with the knowledge of the distribution – see trivial classifier). This task was expected to be especially difficult, since the decisions are subject to much more hidden influences. Nevertheless, on average, end models predict about 70% of the decisions, which is significantly more than guessing (see Figure 4.7 and Table 4.8). Moreover, looking at the correctly and wrongly classified FFC videos, little difference in the behavior between groups was found at the beginning of the FFC videos. However, groups that do not contribute fully show less engagement later on in the FFC videos. The current results seem promising, and one possible explanation for these results is that the last part of FFC videos can be used to predict the contribution behavior of groups since it is easier to tell if the group is communicating well when the introductory phase already ended. Furthermore, participants might control their facial expressions more at the beginning of communication, while their facial expressions at the end of the communication are potentially more relaxed and revealing. Although the results indicate that the last part of the FFC video is much more informative for predicting the group behavior, we have no proof that last part of the FFC video is better than the beginning due to lack of data. Our findings suggest further research is needed.

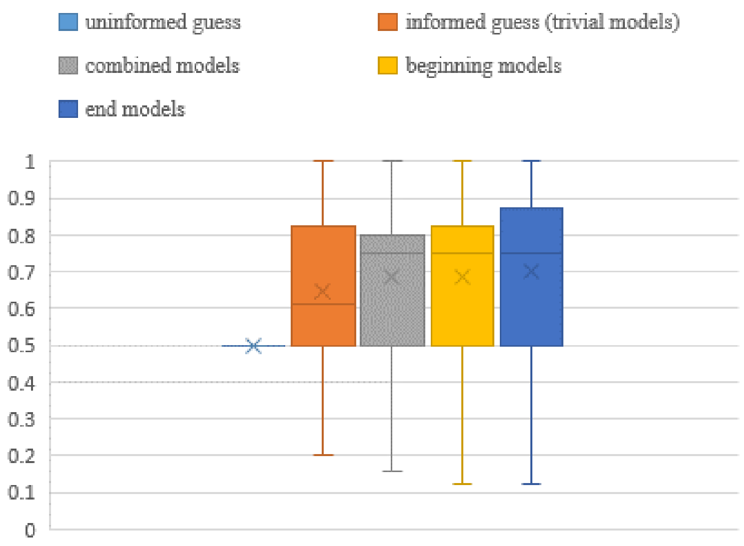

Figure 4.3 Boxplot of the accuracy rates

Note: Boxplot of the accuracy of uninformed guess and trivial models with three different RFc models (combined, beginning and end models). The accuracy rates stem from the leave-one-session-out cross-validation, i.e. 24 rates per model. The accuracy of RFc models is better than uninformed guess and trivial models. End models have an average accuracy of about 70%. Beginning and combined models get similar accuracy of about 68% (technical details are presented in Othman et al. 2019). Crosses represent mean values, boxes show 25% and 75% quantiles and median, whiskers show minimum and maximum values.

Table 4.8 MW-Test for comparing the results of uniformed guess, trivial models, combined models, beginning models and end models.

Models	p-value
uninformed guess & trivial models	0.0000
uninformed guess & combined models	0.0000
uninformed guess & beginning models	0.0000
uninformed guess & end models	0.0000
trivial models & combined models	0.0530
trivial models & beginning models	0.0519
trivial models & end models	0.0082
combined models & beginning models	0.9872
combined models & end models	0.4435
beginning models & end models	0.5147

Note: The tests were applied on the respective accuracy rates coming from the leave-one-session-out cross-validation. The rank of these models from the best to worst based on p-value is the end models, the beginning models, combined models, trivial models, and uninformed guess respectively.

In the following we want to address some methodological concerns. While we stress that the major contribution of this analysis is to show that facial expressions can be used to predict cooperation, we acknowledge there is another question which is evidently very intriguing: which expressions are associated with greater cooperation? Unfortunately, this question cannot be answered by the methodology of the analysis. Something that comes close to it is a depiction of feature importance which is provided in Table 4.9. It provides an overview about the feature importance of head posture and different action units distinguishing between the presence of the action unit and its intensity.[24] The table shows that the posture of the head has the highest feature importance, followed by the intensity features and presence features. Yet, this depiction has to be considered with great caution for two reasons. First, these values are technical ones and do not imply that head posture can be used to predict free-riding behavior. The depicted features are likely to occur interdependently, which limits the interpretation of individual features taken out of the context of the complete random forest classification. Second, even assuming that the feature importance is used to identify those facial expressions which correlate the most to contribution behavior, it still only applies to computer vision. It remains unclear whether and if so how much of these features can be detected by humans in a similar context.

[24] A more detailed analysis of the action units can be found in Othman et al. (2019).

Thus, we stress that no simplified connections between individual features and the contribution behavior should be made.

Table 4.9 Features importance for the FF4 by using RFc (5k)

Imp	Feature	Value	Imp	Feature	Value	Imp	Feature	Value	Imp	Feature	Value
1	pose_y	0.1387	7	AU17_I	0.1029	13	AU07_I	0.0759	19	AU26_P	0.0696
2	pose_p	0.1367	8	AU02_I	0.1022	14	AU20_P	0.0744	20	AU09_P	0.0667
3	pose_r	0.1349	9	AU15_I	0.1012	15	AU45_P	0.0736	21	AU01_P	0.0640
4	AU45_I	0.1060	10	AU23_P	0.0769	16	AU02_P	0.0721			
5	AU20_I	0.1036	11	AU05_P	0.0766	17	AU28_P	0.0717			
6	AU23_I	0.1033	12	AU14_I	0.0763	18	AU04_P	0.0707			

Note: The features in table ordered by their importance from most important to least important. I – intensity, P – presence.

4.5. Discussion

This paper provides further evidence on a strong positive effect of FFC on contribution levels in a public goods experiment. Applying a detailed analysis of content and facial expressions during the three minute long communication period, the paper illustrates several results. In conformance with previous literature both elements of communication have an effect on the contributions in the end-game phase. The strongest effect has the discussion or simple mentioning of the end-game phenomenon. Even though the discussions constituted cheap talk, an informal agreement builds a certain protection against free-riding at the end of the experiment. Therefore, it is plausible to assume an interaction with the visual identification of the co-players. Breaking an agreement after reducing the social distance by visual identification is hereby less likely. Another measure of mutual cooperation in the end-game is the length of discussions. The content analysis showed that groups with longer discussions provided on average higher contributions as compared to those with shorter ones. Combining all content variables to provide a forecast of free-riding in the last period of the experiment increases the precision as compared to naive distribution-based guesses. At the same time, the analysis of facial expressions yields comparable results in terms of precision. Further results illustrate that having economic education does not affect group communication in the experiment. Despite being more likely to have more experience in this type of problems, economists do not actively lead the way out of the dilemma by narrating the solution. However, it is apparent that, as compared to females, male subjects more actively take lead in the discussion and propose the optimal strategy.

With respect to the dyadic analysis approach, it has to be mentioned that the dilemma in the underlying experiment was too easy for the subjects. This yielded the variance of the dependent variable to be very low. Despite this challenging initial position, the results obtained are conclusive with respect to previous literature and the approach of analysis. They further allow predictions on the experiment-specific contribution behavior with accuracies between 70% and 80%. We are aware that besides the imbalanced data the biggest shortfall is the absence of a combined analysis. While, in the framework of this paper, this was not possible for technical reasons, the basic results hint at such interdependencies, e.g. discussing the end-game behavior occurs at the end of a logical train of thoughts, the length of the discussion matters, and the end models of the facial expressions analysis perform better than the beginning models.

Based on the obtained results several improvements can be proposed on how to advance this type of research. First, it is essential to provide a larger variance of the dependent variable. With respect to the applied experimental setup, possible changes would refer to decreasing the efficiency multiplier in the experiment or limiting the information on the contributions which both should diminish cooperation rates. Second, it is crucial to reduce the noise in the data. Therefore, several solutions can be thought of. One that also tackles the issue of simultaneously reducing the cooperation rates would be to decrease the duration of the FFC. Another possible solution might be the aforementioned combination of the two approaches by focusing on facial expression at specific points in time after a content relevant statement was made. This would make the analysis of large parts of the video section unnecessary and therefore decrease the noise. However, this requires more research since it is unclear how long these time frames shall be. Another way to combine the different strands of research is to focus on the prediction of specific characteristics that are known to correlate with the dependent variable. With respect to facial expression analysis, this refers to estimating the demographics of the individuals in the group. This information can then be applied to the content analysis. The combination of content variables and demographics lead to prediction rates between 75% and 84%. Third, it is necessary to increase the total number of observations in order to enhance the analysis of the data. This could enable deep learning approaches for the facial expression analysis as well as the application of machine learning on the content analysis. An experimental setup respecting these conditions shall yield deeper insights about the causes for the superiority of FFC as compared to other forms of communication, as well as help analyzing and predicting economic behavior in payoff-relevant settings.

4.6. Conclusion

In a nutshell, the goal of this research project was to investigate whether it is possible to predict socially sub-optimal contribution behavior in a laboratory public goods game focusing on facial expressions and simply classified contents. This would assist identifying groups in need of further interventions. The main finding is that both approaches separately provide improvements of predicting contribution behaviors, yet are currently limited in their scale, especially given a very noisy environment and an imbalanced sample. Further, the paper discusses how the amalgamation of the two applied techniques could be used to achieve better prediction accuracies. While using content measures to predict results in a simple experiment can be interpreted as an extension of the existing literature, obtaining similar results with simple machine learning algorithms based on facial expressions is a novelty. Assuming ongoing improvements in facial recognition algorithms, the research field of human-machine interaction – and therefore the economic importance of the technology – is expected to grow. Hence, it is crucial to understand whether and how human behavior can be predicted. This paper takes a stance on the issue from the perspective of economically meaningful interactions in a stylized setting of a public goods experiment.

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Appendix 4.1 Instructions

Instructions Experiment “Yellow“

Please read the instructions diligently. If questions arise, open the door to your cabin and remain seated. The experiment “Yellow” is carried out at the computer.
Your fellow participants will only play with you within the experiment “Yellow“.

After reading the instructions you will receive four control questions. The control questions are not considered for your final payment. As soon as you have answered the control questions the part of the experiment relevant for your final payment will start. Please be aware that either experiment „Yellow“, „Blue“ or „Red“ will be paid out. Which experiment will be eventually relevant is decided by chance.

Within the experiment, we will use laboratory dollars as the used currency. The underlying exchange rate is the following: 100 laboratory dollars = 4.5 EUR.

You and three other participants receive each **20 laboratory dollars** per round of contribution. You can contribute these laboratory dollars either to a private or a group account.

Private Account (P): The deposited laboratory dollars are being kept.

Group Account (G): Each of the four players can deposit money in this account. The sum of the deposits is doubled by the experimenter and redistributed equally to the four players. Hence, each player receives 0,5 laboratory dollars per contributed laboratory dollar.

The laboratory dollars can be split up in between the two accounts. You take your decision anonymously. None of the other players will learn how you split your laboratory dollars up. Profit of player i is calculated accordingly:

$$\text{Profit}=(20\text{-G})+0{,}5\cdot\sum_{1}^{4}G_i$$

Please turn!

Instructions Experiment „Red"

Please read the instructions diligently. If questions arise, open the door to your cabin and remain seated. The experiment “Red” is carried out at the computer.
Your fellow participants will only play with you within the experiment „Red“.

After reading the instructions you will receive four control questions. The control questions are not considered for your final payment. As soon as you have answered the control questions the part of the experiment relevant for your final payment will start. Please be aware that either experiment „Yellow“, „Blue“ or „Red“ will be paid out. Which experiment will be eventually relevant is decided by chance.

Within the experiment, we will use laboratory dollars as the used currency. The underlying exchange rate is the following: 100 laboratory dollars = 4.5 EUR.

You and three other participants receive each **20 laboratory dollars** per round of contribution. You can contribute these laboratory dollars either to a private or a group account.

Private Account (P): The deposited laboratory dollars are being kept.

Group Account (G): Each of the four players can deposit money in this account. The sum of the deposits is doubled by the experimenter and redistributed equally to the four players. Hence, each player receives 0,5 laboratory dollars per contributed laboratory dollar.

The laboratory dollars can be split up in between the two accounts. You take your decision anonymously. None of the other players will learn how you split your laboratory dollars up. Profit of player i is calculated accordingly:

$$\text{Profit}=(20\text{-G})+ 0{,}5\cdot \sum_{1}^{4} G_i$$

Video conference: Before you take your decision on how to split the laboratory dollars you will be talking to the three other players in a video conference for three minutes. During this time, you can see and talk to each other. The duration of the call can neither be reduced nor prolonged.
Subsequently to the video conference, each player makes the above described decision.

Please turn!

Instructions Experiment „Blue" (Without Refund)

Please read the instructions diligently. If questions arise, open the door to your cabin and remain seated. The experiment "Blue" is carried out at the computer.
Your fellow participants will only play with you within the experiment „Blue".

After reading the instructions you will receive six control questions. The control questions are not considered for your final payment. As soon as you have answered the control questions the part of the experiment relevant for your final payment will start. Please be aware that either experiment „Yellow", „Blue" or „Red" will be paid out. Which experiment will be eventually relevant is decided by chance.

Within the experiment, we will use laboratory dollars as the used currency. The underlying exchange rate is the following: 100 laboratory dollars = 4.5 EUR.

You and three other participants receive each **20 laboratory dollars** per round of contribution. You can contribute these laboratory dollars either to a private or a group account.

Private Account (P): The deposited laboratory dollars are being kept.

Group Account (G): Each of the four players can deposit money in this account. The sum of the deposits is doubled by the experimenter and redistributed equally to the four players. Hence, each player receives 0,5 laboratory dollars per contributed laboratory dollar.

The laboratory dollars can be split up in between the two accounts. You take your decision anonymously. None of the other players will learn how you split your laboratory dollars up. Profit of player i is calculated accordingly:

$$\text{Profit}=(20\text{-}G)+ 0{,}5\cdot \sum_{1}^{4} G_i$$

Set-up of the video conference: At the beginning of the experiment you are asked whether you want to make the experiment this time with or without communication. Communication will be subject to a fee. To make the experiment with communication you must raise a required amount jointly as a group. This amount will pop up on your screen at the beginning of the experiment. The decision on how much you contribute will then be again taken anonymously. The deposited money for setting up communication is being deducted from your profit in the experiment „blue" at the end of it – whether communication is successfully set up or not. If the group raises the required amount, a three-minute video conference is being set up, see previous round. Otherwise, all group members have to wait for three minutes until other groups have finished their communication period, respectively. Subsequently, the decision on how to split up the laboratory dollars between private and group account are being made.

Please turn!

Instructions Experiment „Blue" (With Refund)

Please read the instructions diligently. If questions arise, open the door to your cabin and remain seated. The experiment "Blue" is carried out at the computer.
Your fellow participants will only play with you within the experiment „Blue".

After reading the instructions you will receive six control questions. The control questions are not considered for your final payment. As soon as you have answered the control questions the part of the experiment relevant for your final payment will start. Please be aware that either experiment „Yellow", „Blue" or „Red" will be paid out. Which experiment will be eventually relevant is decided by chance.

Within the experiment, we will use laboratory dollars as the used currency. The underlying exchange rate is the following: 100 laboratory dollars = 4.5 EUR.

You and three other participants receive each **20 laboratory dollars** per round of contribution. You can contribute these laboratory dollars either to a private or a group account.

Private Account (P): The deposited laboratory dollars are being kept.

Group Account (G): Each of the four players can deposit money in this account. The sum of the deposits is doubled by the experimenter and redistributed equally to the four players. Hence, each player receives 0,5 laboratory dollars per contributed laboratory dollar.

The laboratory dollars can be split up in between the two accounts. You take your decision anonymously. None of the other players will learn how you split your laboratory dollars up. Profit of player i is calculated accordingly:

$$\text{Profit}=(20\text{-}G)+ 0{,}5\cdot \sum_{1}^{4} G_i$$

Set-up of the video conference: At the beginning of the experiment you are asked whether you want to make the experiment this time with or without communication. Communication will be subject to a fee. To make the experiment with communication you must raise a required amount jointly as a group. This amount will pop up on your screen at the beginning of the experiment. The decision on how much you contribute will then be again taken anonymously. The deposited money for setting up communication is being deducted from your profit in the experiment „blue" at the end of it – only if communication is successfully set up. If the group raises the required amount, a three-minute video conference is being set up, see previous round. Otherwise, all group members have to wait for three minutes until other groups have finished their communication period, respectively. Subsequently, the decision on how to split up the laboratory dollars between private and group account are being made.

Please turn!

Appendix 4.2 Interrater Agreement

Table 4.10 Interrater Agreement

	Full Invest-ment	End-game awareness	Previous expe-rience	Threat & conse-quences	Disagree-ment	Information provider
Percent Agreement	1.0000	0.8730	0.8810	0.9048	0.9841	0.9508
Krippendorff's Alpha	1.0000	0.7187	0.7628	0.7748	0.9008	0.9473
Brennan and Prediger	1.0000	0.7460	0.7619	0.8095	0.9683	0.9477
Cohen/Conger's Kappa	1.0000	0.7181	0.7635	0.7749	0.9000	0.9471
Scott/Fleiss' Pi	1.0000	0.7165	0.7619	0.7729	0.8997	0.9472
Gwet's AC	1.0000	0.7700	0.7619	0.8360	0.9811	0.9478
Number of raters	2	2	2	2	2	2
Ratings per rater	127	127	127	127	127	127

Appendix 4.3 Group composition in the experiment

Table 4.11 Group composition

Block 3 after VCM/C-VCM	FTC + possible STC		Only FTC (=VCM)	Always FTC and STC (=C-VCM)
	No Refund	Refund		
Sessions	8	8	4	4
Subjects	128	128	64	64
E	65	57	29	31
NE	63	71	35	33
Male	66	78	36	32
Females	62	50	28	32
Average age	23.96	24.10	23.28	22.92

5. Strategically distorted majority voting: The role of communication and information

5.1. Introduction

The majority rule is the most commonly used mechanism in democracies to bring about collective decisions. One reason why this rule is so popular is probably its inherent symmetry. For each decision, the group of voters can be divided into two subgroups. The majority group is the pivotal group, the minority group is the non-pivotal group. Symmetry consists in the fact that each member of the group can a priori be a member of both subgroups. Which group members form the pivotal group is basically arbitrary. The majority rule does not prejudge a certain composition of the subgroups, all group members are symmetrical in their rights and possibilities. This is the core of the famous principle of "one man one vote".

However, in reality there are many democratic decisions where complete symmetry is not given because there are voters who have a better chance of becoming members of the pivotal group than others. The reasons for this can be very diverse. Often some strategic opportunities are open for certain subgroups yet are closed for others. This happens, for example, when voters are not symmetrical in dimensions other than the weight of their vote. Two examples from the world of politics make this point clear:

The EU member states are not symmetrical in many respects. They differ in terms of population size, country size and, not least, economic power. There are also net contributors and net recipients. Due to these asymmetries, it may well be the case that net payers, for example, have a better chance of belonging to the pivotal group in a majority decision than net recipients do, because their net payer position gives them greater strategic weight. This may be one reason why many decisions in the EU have to be taken unanimously, because under this rule all voters have to be in the pivotal group.

On the world political stage, there are always sub-groups of states that emerge because their members occupy a prominent strategic position, because they have economic or military strength. The G7 are just as much an example as the G20 or the OECD. Further, pivotality may arise when certain members of the group reach preliminary agreements before the formal meeting. As an example, if the G7 reaches a joint agreement and resumes the topic in the G20, they are likely to become pivotal. Likewise, G20 is likely to become pivotal in the OECD and a coordinated strategy of G20 and OECD has a certain power in the United Nations.

In cases like these, the majority rule is often applied (in the EU Commission or in the UN), but the symmetry described above does not exist because there is a strategic distortion which leads to the probabilities of belonging to the pivotal group no longer being equal. In this paper, we examine the question of how, in such a case of a strategically distorted majority decision, two aspects are affected that we expect to play a role in such decisions: the information about the strategic distortion and the possibility to communicate. We use a simple distribution game known as "Pirate Game", which goes back to Stewart (1999). The Pirate Game has the advantage that a strategically distorted majority decision can easily be described in abstract terms with it, because the game has a perfect Nash equilibrium that creates this distortion. This, in turn, opens up the possibility to experimentally examine the game and to analyze the effects of information and communication.

In the basic Pirate Game experiment there are five strictly hierarchically ordered individuals (Player 1 to Player 5) in a group that have to allocate a certain endowment. The individual with highest rank (Player 1 - P1) has to propose an allocation scheme for the entire group. After the proposal all subjects, including the proposer, vote on this allocation scheme. Whenever, the voting in favor of it reaches at least 50% ($\geq$ 50.0%) the proposal is accepted and yields the respective payoffs for the subjects. Whenever the proposal is rejected the proposing P1 leaves the experiment without further payment (keeping only the show-up fee). The next player in the hierarchy (P2) takes over the task to propose the allocation for the entire group of remaining four players. The 50% decision rule and the total money available (in the case of our experiment: 120 Laboratory Dollar) remain constant. In case of acceptance, the allocation yields the payoffs. In case of rejection, P2 leaves the experiment and P3 takes over. This procedure would theoretically proceed until only one player is left.

Given this structure, a theoretical equilibrium assuming perfect rationality can be calculated by the means of backward induction. Table 1 represents the summary for the different stages of the backward induction. In the last stage there is only one player (P5) who can keep the total endowment. In a two player scenario (stage 4) the experimental setup resembles a simple Dictator Game. The proposer (P4) has enough votes on its own to single-handedly accept any proposal. In stage 3 where there are three individuals (P3 - P5), the proposing P3 needs another vote in favor of the proposal. In order to incentivize a further vote with certainty it is the cheapest and therefore most rational to make one of the other subjects a proposal that is higher than what they would receive otherwise at stage 4, yet P5 can be incentivized to vote in favor of any proposal larger than 0 LD. A proposal of 1 LD hereby implements the maximization principle of the proposer. This structure can be followed for the next stages ultimately yielding the optimal allocation scheme for stage 1 (see Table 5.1). The proposing P1 incentivizes P3 and P5 to vote in favor of the proposal by offering them 1 LD respectively and keeps 118 LD. Thereby, from the theoretical perspective P1, P3, and P5 become pivotal players at the first stage of the experiment whereas P2 and P4 are non-pivotal and go away empty-handed.

Table 5.1 Optimal allocation scheme depending on the respective stage.

	Player 5	Player 4	Player 3	Player 2	Player 1
Stage=5	**120**	-	-	-	-
Stage=4	0	**120**	-	-	-
Stage=3	1	0	**119**	-	-
Stage=2	0	1	0	**119**	-
Stage=1	1	0	1	0	**118**

The theoretically rational distribution among the three pivotal players is very unequal, because the proposer gets a very large share for himself and has to leave only a minimal share to players 3 and 5. The Nash equilibrium creates a strategic distortion of the majority rule, because in the equilibrium the members of the pivotal group are clearly identified. The game is therefore ideally suited to deal with the research question of how information and communication affect a strategically distorted majority decision. However, this question only really makes sense if we move away from the assumption that the players act fully rationally. Strictly rational players will play the equilibrium they know (they do not need information) regardless of whether communication is allowed or not.

In the next section, we will explain, with reference to the literature, why information about the Nash equilibrium is likely to have an influence on behavior in the Pirate Game Experiment (PGE) and why communication will play a role. In section 5.3 we will then derive the concrete hypotheses and present the treatments of our experiment. Section 5.4 describes the experimental design. Section 5.5 presents the results that are discussed in section 5.6. Section 5.7 concludes.

5.2. Information and communication in the PGE

Can we assume that limited rational players are able to perform the backward induction over five steps necessary to determine the subgame perfect equilibrium of the pirate game? Dufwenberg and van Essen (2018) investigate the ability of backward induction with a game that bears resemblance to the Pirate Game. If we summarize their results and those of Camerer (2003) and Crawford et al. (2013), it is not to be expected that players in a PGE are aware of the equilibrium. This is why it makes sense to study the effect of information in a PGE. How does knowing that players (1, 3, 5) are pivotal in the equilibrium change behavior? Any group of three players is pivotal in PGE, because the majority rule is used. The question is therefore whether the information that the group (1, 3, 5) is pivotal in equilibrium changes its behavior compared to a situation where the group feels that it is only a randomly assembled group of three.

If no information about the equilibrium is provided, the players are in a situation that corresponds to a multiplayer ultimatum game. After its introduction by Güth et al. (1982), the ultimatum game has become one of the best studied games in experimental economic research.[25] Ultimatum experiments with more than two players are particularly relevant for a PGE. In the experiments of Bolton and Ockenfels (1998) and Güth et al. (2007) as well as Güth et al. (1996) the players are equipped with different decision-making options and there are so-called dummy players who cannot make any decisions themselves. An important finding from these experiments is that the lower the influence of players on the outcome of the game, the lower their payoff in these experiments. Especially the dummy players often go away empty-handed. For a PGE, this suggests that the non-pivotal players will receive low payoffs - especially if the pivotal players are very aware of their power.

From the literature, we know two effects that can be important for the consequences of informing subject about the equilibrium. On the one hand, this information can create a group identity in the group of the pivotal players. On the other hand, this information can cause an entitlement effect, in the sense that pivotal players develop the feeling of having the right to receive a higher payoff than the non-pivotal players. The economic importance of group identification is shown in the work of Akerlof and Kranton (2000, 2002, 2005). The observation that there is a preference for in-group members is very robust (Bernhard et al., 2006; Goette et al., 2006). Chen and Li (2009) show that it is relatively easy to create a group identity. In their experiment, the subjects had to indicate whether they preferred a painting by Klee or by

[25] For an early overview, see Roth (1995), for more recent one Güth and Kocher (2014).

Kandinsky. The groups formed in this way showed markedly different behavior towards members of their own and the other group. Entitlement effects have been repeatedly demonstrated, especially in connection with the ultimatum game. Güth and Tietz (1986) give out the role of the proposer by holding an auction. The result was a significant increase in unequal distributions. In Hoffman et al. (1996) those subjects became proposers who beforehand had won a knowledge quiz. Here, too, the entitlement leads to an increase in payments to the proposers. In a more general context, Gächter and Riedl (2005) show that knowledge-based entitlement influences behavior in negotiations. All these indications suggest that informing the pivotal players about their pivotal position in the equilibrium could lead to group identity and an entitlement effect.

So, there is some evidence that the provision of equilibrium information changes behavior in PGE. The same is true for communication between players, either in the subgroups (1, 3, 5) and (2, 4) or in the group of all players. In general pre-play face-to-face communication decreases social distance, builds social capital, further enhances cooperation among the agents (Brosig et al., 2003) and enhances mutual player type detection (He et al., 2017). In bargaining situations communication leads to higher fairness concerns as is discussed in Brosig et al. (2003, 2004). For the ultimatum game Capizzani et al. (2017) illustrated that one-sided communication (by receivers) does not increase the offers in ultimatum games yet two-sided communication does. These findings suggest that communication in the subgroups leads to a strengthening of group identification and a more even distribution within the group of pivotal players. Communication in the large group suggests that the offers to the non-pivotal players will be more generous than without this communication. Further literature shows, that providing subjects the opportunity to exclude others from communication leads to collusion of communicating partners against the excluded member (Abbink et al., 2018).

Based on the effects reported in the literature, it can be assumed that information and communication in a PGE will have effects. However, both aspects also play an important role in real negotiation situations. The recognition of a strategic advantage is sometimes not trivial even in reality. Therefore, the dissemination of information about such advantages can also play a role there. Further, it is obvious that communication is very important in real negotiations and that this communication can take place in the "small circle", in the “big round”, or in both sequentially. The PGE is therefore suitable for disclosing information and communication effects that can also be of great importance for real negotiations. In the next section, we will introduce the treatments we use to study these effects and present the hypotheses that we derived from the literature for the respective treatments.

5.3. Treatments and hypotheses

Building upon previous research we propose seven hypotheses with respect to communication and information, which are tested in the respective treatments. The central underlying idea is that bidirectional pre-play face-to-face communication enhances coordination and reinforces the group identity among communication partners. This implies that due to such communication we should observe in-group favoritism and simultaneously more equal splits among communication partners. With respect to information we assume that becoming aware of the power provided by the experimental design, the pivotal players will try to extract higher profits as they consider these to be more justified. Hereinafter we present the individual hypotheses.

1. Basic Treatment (BT)

In the Basic treatment the individuals were not allowed to communicate with each other and did not receive any information on the theoretic solution. Based on the literature review we assume that the players are not able to identify the Nash Equilibrium, as there are too many steps of backward induction required. Thus, the players are unaware of any strategic distortion. In the basic treatment we, therefore, assume to observe a similar behavior as in a typical (multiplayer) Ultimatum game. The proposer is aware of his special position and can try extracting a higher share than other players. Yet, he potentially anticipates that an excessively selfish proposal may result in rejection. This results in the following hypothesis:

Hypothesis BT:

In the BT, the proposer will claim a slightly higher share than the other four players. His offer to the other four players will be the same for all four.

2. Information Treatment (IT)

In the IT all subjects receive textbook styled information on the Nash Equilibrium of the game. Thus, the proposer knows his special strategical power and further is aware that all the other players know that, as well. However, without communication, there is no coordination and less identity building involved. Knowing the structure, the proposer can pay the other pivotal players to gain their votes and simultaneously keep up a share higher than theirs. Non-pivotal players are expected to receive only very small shares. This leads to the hypothesis:

Hypothesis IT

In IT, the proposers will demand higher shares for themselves. The two non-pivotal players will receive even lower offers than players 3 and 5. Players 3 and 5 will be offered significantly lower shares compared to GCT.

As already mentioned, communication in the context of real negotiations takes place both in small circles and in the big round. To investigate the effect of collusion in small groups, we first introduce communication in the subgroups (GCT) and then combine this with information about the Nash equilibrium of the game (GCIT). In this way, we can better isolate the effects of the two influencing variables.

3. Group Communication Treatment (GCT)

In this treatment the pivotal players can communicate with each other. Thus, they should realize that they have a majority within the group and thus are pivotal. At the same time, through communication they have the opportunity to agree and coordinate their behavior. Furthermore, communication leads to a decrease of social distance within the pivotal group and the emergence of a group identity. This is associated with an increasing social distance to the non-pivotal group. Based on literature on in-group favoritism, we expect that the pivotal group will make a proposal that discriminates against the non-pivotal group and favors its own group. Within the pivotal group we do not expect any differentiation of shares. This yields the hypothesis:

Hypothesis GCT:

In the GCT, the members of the pivotal group will grant themselves higher proportions than the members of the non-pivotal group. Within the pivotal group, the proportions will not differ significantly.

4. Group Communication Information Treatment (GCIT)

Similar to the GCT, the group of three can communicate and become aware of the fact that they form a majority and are therefore pivotal. Yet in GCIT, the group is additionally informed that it is pivotal in the Nash equilibrium and thus it knows that the composition of the group is not random but an expression of the strategic distortion of the majority decision. This information will give the group additional legitimacy in that it is pivotal not only because of its numerical superiority, but because the rules of the game make it pivotal in a strategic sense. The Nash equilibrium thus legitimizes in a certain way the exploitation of the non-pivotal players by the pivotal ones. At the same time, the communication effect works as in the GCT, i.e. no strong differentiation between the pivotal group members is to be expected. This leads to:

Hypothesis GCIT:

In this treatment, the pivotal players will give each other higher percentages than in GCT.

5. All Communication Treatment

If no information about the Nash Equilibrium is provided, joint communication in the large group affects all members. This means that there can be a coordination of behavior and that the social distance between all five players is reduced. In contrast to the Basic Treatment, the resulting group identity and the tendency towards a fair distribution should ensure that the prominent position of the proposer decreases. This leads to the hypothesis:

Hypothesis ACT:

Without information but with communication in the large group the proposers' offers will provide a largely even distribution of shares. The special position of the proposer from the BT will be reduced.

6. All Communication Information Treatment

If the communication in the whole group is supplemented by information about the Nash Equilibrium, this will result in the offers to the pivotal players (1, 3, 5) being higher than in the ACT. However, in this treatment the pivotal players do not have the possibility to coordinate their behavior separately and above all the communication among all five players still works towards an even distribution. This yields the hypothesis:

Hypothesis ACIT

The proportion of pivotal players will be larger than in the ACT, but it will be smaller than in the GCIT.

7. Sequential Group Communication Information Treatment

In real negotiations among informed agents it is likely to be the exception that communication is either only in a small circle or only in a large round. As a rule, both will take place: First agreements are made in the small groups and then they meet in the big round. This situation is illustrated in our seventh treatment.

When information about the Nash Equilibrium is provided and communication first takes place in two separate groups and then jointly in the whole group, two opposite effects occur. First, as in GCIT, the pivotal players can coordinate and generate a high degree of legitimacy for giving themselves a high share. Second, communication in the group as a whole works in the direction of a fair distribution. The reference to fairness arguments will prevent the pivotal players from showing their strength in the same way as in GCIT. Nevertheless, the behavioral coordination that takes place beforehand will ensure that the pivotal players can achieve a higher share of the score than in the ACIT. This leads to the hypothesis:

Hypothesis SGCIT

The offers to the pivotal players will be between those in the GCIT and the ACIT.

One further hypothesis can be obtained from the literature. For simplicity it only focuses on the share the proposers claim for themselves. Communication should lead to a fairer allocation among communication partners and thus, it would harm the proposers' share. This effect should be higher whenever there is no justification for an unfair allocation, as it would be provided in the information treatments. Simultaneously, this implies that information itself would benefit the proposer. These two mechanisms could lead to a ranking for the share of proposers in all seven treatments, where IT should lead to highest shares for the proposer followed by GCIT. In no treatment we would expect the proposer to receive less than in ACT. For other treatments the interaction between communication and information is too unknown to make sophisticated predictions. Therefore, a possible order may be:

Hypothesis proposer share

IT > GCIT > SGCIT ≥ ACIT ≥ GCT ≥ BT > ACT

5.4. Experimental Design

In order to achieve comparability, the general experimental process in all seven treatments was organized as follows. Whenever applicable, the subjects first received information on optimal solutions, followed by the communication. In the end, the basic version of the game – from the proposal until the final acceptance – started. This is depicted in Figure 5.1.

Further, we implemented the strategy approach in the experiment. Using the strategy method in treatments without communication (BT and IT), the subjects were first left uninformed about their actual position in the hierarchy and were all asked to make a proposal from the perspective of the proposer. Subsequently, every subject voted on all five proposals, i.e. they were shown their potential position in the hierarchy on the screen and needed to accept or reject the proposal. Ultimately, the individuals were informed about their actual position in the hierarchy and whether the proposal of the actual P1 was accepted. In case of acceptance the experiment was over. In case of rejection the positions of the subjects remained constant and the experiment proceeded with P2 making the next proposal.

In order to apply the strategy method in all other treatments, the subjects were first divided in two groups (P1, P3, P5 and P2, P4). They were informed that members of the first group will ultimately become P1, P3 or P5 and members of group two will become P2 or P4 respectively. Then the groups communicated in the respective way depending on the treatment. After communication every member of the first group made a proposal from the perspective of P1. Subsequently, the individuals were informed about their possible position in the hierarchy and voted on all three proposals from the perspective of their position. Ultimately, the acceptance or refusal of the actual P1's proposal was displayed, and the experiment continued as described. For detailed experimental procedures in every treatment see Appendix 5.1. For the instructions consult the Extract of the Online Appendix.

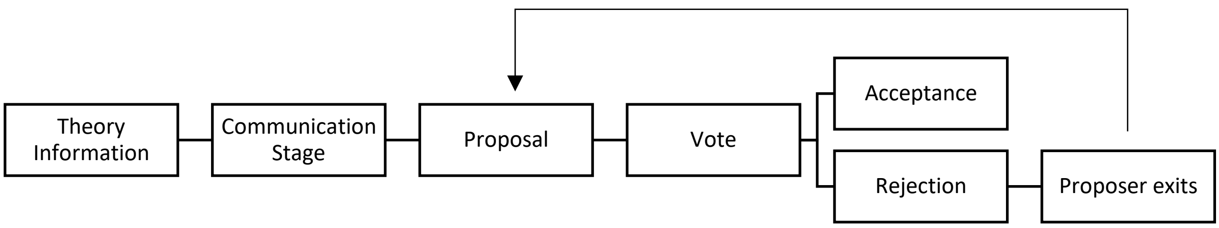

Figure 5.1 Experimental Design

We further provide a basic overview of demographical and otherwise potentially relevant variables in Table 5.2. In addition to age and gender (female=0, male=1), subjects filled in information on their highest educational achievement[26]. The choice on the numbers of observations depended power analysis. However, it only based on the main variable "proposer". This variable depicts the proportion of the endowment the proposers keep for themselves. Based on the results of the related literature (Agranov & Tergiman, 2014) and pilots for the treatments BT, IT, GCT this led to assumptions for the different treatments. The power analysis indicated sample sizes of approximately 20 - 25 observations per treatment which are lower than actually

[26] (0= no degree/1=college entrance qualification/2= bachelor's degree/ 3= master's degree/ 4= PhD).

chosen (42-81 observations). The increase is necessary to be better able to investigate possible interactions as well as analyze other variables for which a power analysis was not reasonably possible. The power analysis (Appendix 5.2) therefore illustrates the smallest effect sizes observable given the numbers of observations at a power of 0.8 at a 0.05 significance level.

Table 5.2 Treatment overview

	BT	IT	GCT	GCIT	ACT	ACIT	SGCIT
Age	24.30	24.40	23.69	23.84	22.90	23.17	23.35
Gender	0.53	0.50	0.53	0.52	0.50	0.46	0.47
Educational Achievement	1.42	1.56	1.40	1.41	1.33	1.41	1.52
Economic Education	0.21	0.27	0.24	0.22	0.26	0.16	0.27
Number of subjects	75	70	135	130	70	70	90
Number of independent observations	75	70	81	78	42	42	54

Note: The differences in numbers of subjects and number of independent observations in all treatments but BT and IT stem from the application of the strategy approach. There, only three out of every five players made a proposal.

The duration of the experiment was approximately 45 minutes. On average the individuals received 15.08€, which consisted of a show-up fee of 5€ and the payoff resulting from the experiment. The participants were recruited from the subject pool of the Magdeburg Experimental Laboratory of Economic Research (MaXLab) and consisted of students from the Otto-von-Guericke University Magdeburg (Germany). The experimental design was executed in z-Tree (Fischbacher, 2007). The experiment was organized and recruited with the software hroot (Bock et al., 2014). The instructions were handed out in paper form and read aloud. The experiment started only after all participants had answered the control questions correctly. Individuals took their seats in separated cubicles. At the end, the subjects answered a short questionnaire on their beliefs in just world (Dalbert, 1999) and demographics. Finally, the subjects were called into a separate room to be paid out individually and left on one of the two pathways out of the laboratory area.

5.5. Results

The results chapter is divided in three sections. First, we provide some descriptive statistics on the allocation schemes for all treatments. Second, we present tests for the aforementioned hypotheses. Third, we investigate further data obtained from the experiment, such as rates of acceptance, proximity to the Nash Equilibrium, and proximity to fair allocations. In terms of analysis, we implement Mann-Whitney tests on comparisons between treatments and paired Wilcoxon signed-rank test for the analysis of different types of individuals within every group. For further robustness, we provide regression results. Corresponding to the stated hypotheses we focus on different subgroups of individuals with respect to their position in the game. Player 1 is the "proposer". Jointly with players 3 and 5 they constitute the group of "pivotal players". The sole group of players 3 and 5 (without the proposer) we shall refer to as "other pivotal" players. Players 2 and 4 constitute the group of "non-pivotal" players.

5.5.1. Descriptive Statistics

Starting with the descriptive analysis, in Table 5.3 we first present the average shares for the respective types of players in all treatments. (For a graphical depiction of these average shares see Appendix 5.3). The shares of the proposer are mostly in line with the hypothesized order. In fact, the proposers profit the most from IT followed by GCIT. In ACT we observe an almost fair allocation. Thus, the proposers were not able to profit from the theoretical distortion of the game. The shares in the other four treatments (BT, GCT, ACIT, SGCIT) are almost identical (29.16-29.31 LD). In more general, the findings illustrate that different combinations of communication and information benefit different players. At this stage it further becomes apparent that in none of the treatments did the proposers extract their full equilibrium power, corresponding to 118 LD.

Table 5.3 Average shares for different groups of individuals in different treatments

	BT	IT	GCT	GCIT	ACT	ACIT	SGCIT
Proposer	29.31	53.30	29.16	36.37	24.67	29.21	29.19
	(16.35)	(30.37)	(8.78)	(8.22)	(2.75)	(15.06)	(7.15)
Other Pivotal	22.51	21.26	27.06	34.72	23.77	23.55	29.46
	(4.31)	(11.35)	(6.43)	(7.04)	(0.88)	(4.06)	(7.95)
Non-pivotal	22.84	12.09	18.36	7.10	23.89	21.85	15.94
	(4.27)	(11.10)	(9.35)	(10.29)	(0.68)	(6.18)	(10.41)

Note: Standard deviation is denoted in brackets.

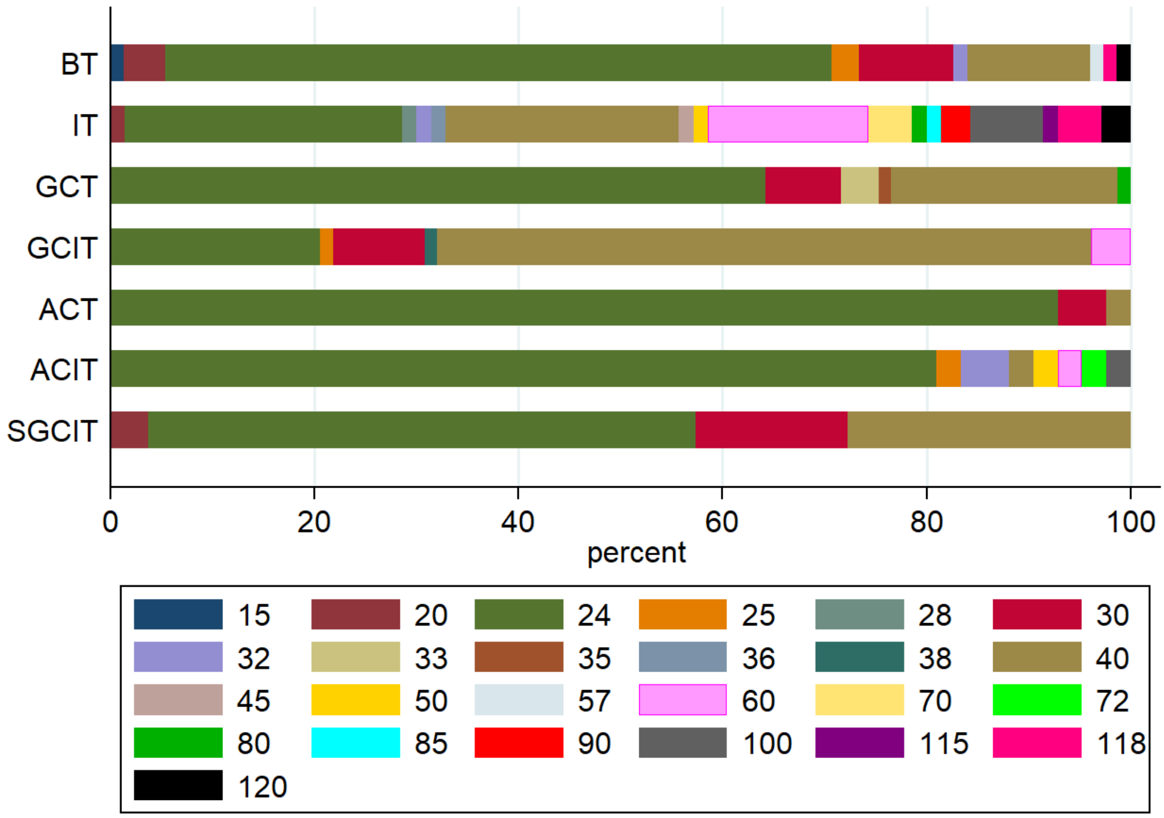

Figure 5.2 Overview of different proposals for the own share.

However, looking only at the average values leaves out additional information on the distribution of proposals. For this purpose, we illustrate the distribution of the shares, the proposers kept for themselves, in Figure 5.2 and the corresponding illustrations for other pivotal and non-pivotal players in Appendix 5.4. These figures indicate what type of money allocation were proposed, e.g. fair share for all (24/24/24/24/24), fair share among pivotal players (40/0/40/0/40). The figure illustrates that 24 LD is the mode in most of the treatments. Only in the GCIT 40 LD becomes the mode. Another observation is that the number of different proposals varies. While there were 10 different proposals for the own share in BT, the number of proposals decreased to 6 in GCT and GCIT respectively and 3 in ACT. In contrast to it, the number of different proposals increased to 17 in IT and 8 in ACIT, which may be interpreted as a sign of insecurity about the acceptance of the rational solutions of the other players. This is further supported by the observations in SGCIT, where only 4 different proposals occurred. Here the pivotal players knew the theoretical equilibrium and had the opportunity to coordinate on their strategy. In addition to the visual demonstration of these differences, we refer to the standard deviations denoted in Table 5.3.

5.5.2. Main results

In this section we will trace the individual hypotheses presented in chapter 5.3. Starting with the first hypothesis on the share the proposers keep for themselves, we will display the results from the individual tests. To test the hypotheses, we conducted 15 tests. Out of the 15 tests 13 tests are significant at 0.05 level after Holm and 11 after Bonferroni correction. We present the respective p-values in the text and sum up the significance values of Bonferroni and Holm corrections in Appendix 5.5.

Hypothesis BT

Based on findings from the Ultimatum Game we assumed that the proposer would keep a slightly higher share of the total endowment, while not distinguishing between the other pivotal and non-pivotal players. In fact, we observe that the average share of the proposers is 29.31 LD. This amount is significantly (p=0.0005) higher than the average share for other pivotals (22.51 LD) and higher than for non-pivotals (22.84 LD) at a mild level of significance after Bonferroni correction (p=0.0043).

Hypothesis IT

To check the Hypothesis for IT, assuming a certain entitlement effect of information, we focus first on the differences between the shares of proposers in BT and IT. On average proposers in BT kept 29.31 LD. The share of proposers in IT was significantly higher (53.30 LD, p=0.0000). We then investigate the shares of pivotal (21.26 LD) and non-pivotal (12.09 LD). Similar to BT, the differences between the proposer and other pivotal players are significant (p=0.0000) Yet, in contrast to BT the difference between other pivotal and non-pivotal is significant (p=0.0000), too. This supports the assumption that subjects need additional information to detect the equilibrium. Furthermore, the other pivotal players receive significantly (p=0.0003) less than the other pivotal players in GCT (27.06 LD).

Hypothesis GCT

If instead of receiving the information on their pivotality, the group of pivotal players simply received the chance to communicate jointly, we assumed bonding would occur. Such a joint identity would lead to in-group favoritism. Therefore, we compare the share of pivotal players in GCT (27.76 LD) with the share of non-pivotals (18.36). The difference is significant (p=0.0000). Further, we assumed that communication would have a levelling effect within the group and thus, no differences between the proposer and other pivotal players occur. However, we observe that the proposers in fact keep a slightly larger share (29.16 LD compared to 27.06 LD). Yet the difference is not significant after Bonferroni correction (p=0.0078)

Hypotheses GCIT

Likewise, to the comparison between BT and IT, we assumed a certain entitlement effect to occur between GCT and GCIT. This means that the pivotal players and especially the proposer become aware of their pivotally. First, we compare the shares of pivotal agents in GCT and GCIT. In GCT the players received on average 27.75 LD which is significantly (p=0.0000) less than in GCIT (35.27 LD). Similar to GCT, we do not observe large differences between the proposer (36.37 LD) and the other pivotal players (34.72) in the GCIT treatment (p=0.0391).

As long as the group (1, 3, 5) is not informed about the equilibrium, it is (from the viewpoint of the group members) a randomly composed group of three. However, this group forms a majority, i.e. it is pivotal. This is important because it shows that the information about the equilibrium has an effect that goes beyond the information about being pivotal. The group cannot become more "pivotal" than it already is in GCT. Nevertheless, the information about the equilibrium has a massive effect.

Hypothesis ACT

Based on prior literature we assumed that communication enables group identity. In contrast to the BT, this should result in a more equal distribution. While being aware of their special position, the proposers would have to argue with others why they deserve a larger share of the endowment. To analyze this, we construct a simple dummy variable which indicates whenever the proposer has the highest share of all players in the group. Using a Chi-Square test we illustrate that in ACT the special position of proposers is much weaker than in BT (p=0.0000). This confirms our hypothesis.

Hypothesis ACIT

Similar to the analyzed differences between BT/IT and GCT/GCIT we assumed an entitlement stemming from knowing the Nash Equilibrium. In this case however, the treatment implies that the entitlement effect of information and the levelling effect of joint communication operate in different directions. In comparison to ACT (24.07 LD) the pivotal agents had a share of 25.44 LD. This is only slightly and insignificantly (p=0.1009) more. Thus, information did not have a strong effect on the allocation. However, comparing ACIT (25.44 LD) with GCIT (35.27 LD) allows to look at the effect of joint communication, which is significant (p=0.0000). Thus, the communication in the “general assembly” limits the power of the pivotal group to exploit their strategic advantage at the expense of the non-pivotal players.

Hypothesis SGCIT

In the SGCIT treatment we wanted to analyze whether any preliminary agreement as it was made in group of three pivotal players (similar to GCIT) would persist despite the following joint communication (as in ACIT). The prediction was that the share of pivotal players in SGCIT should be between the level of GCIT and ACIT. In SGCIT the pivotal players received on average 29.37 LD which is significantly more than in ACIT (25.44 LD, p=0.0009) yet significantly less than in GCIT (35.27 LD, p=0.0000). This confirms our hypothesis. Once again, the communication in the large group limits the power of the pivotal group. Nevertheless, communication and information still cause a significant advantage for the pivotal group.

Briefly summing up the results it is apparent that different types of players benefit differently from communication type, further depending on the information state. To provide a more robust analysis we run a Tobit regression (Table 5.4) that is censored at 0 and 120 LD and is clustered at group level. We provide three models for the three types of agents (proposer, other pivotal, and non-pivotal). The regression model supports previous analysis. Focusing on the proposers, we observe that information has a strong positive effect on proposers. In contrast to it, communication reduces the share of proposers, yet mostly in treatments with information. The regressions further support the evidence that the effects are different for the different types of players. In contrast to the proposer, the other pivotal players do not benefit from information alone. However, they benefit when the communication excludes the non-pivotal agents as in GCT, GCIT, or partially in SGCIT. The situation is different for other pivotal players. They experience losses from grouped communication and information. Yet, in an informed state they strongly benefit from a joint communication.

Table 5.4 Tobit regression for three different types of players

	Own Share			Other Pivotal			Non-pivotal		
	Basis	Interaction	+dem.	Basis	Interaction	+dem.	Basis	Interaction	+dem.
Information	12.92***	24.06***	23.50***	5.17***	-2.72	-3.10	-23.28***	-26.24***	-25.00***
	(2.52)	(5.28)	(2.82)	(1.25)	(2.36)	(2.40)	(3.27)	(4.26)	(3.86)
GroupCom	-8.36***	-0.180	-0.375	17.94***	9.15**	9.00*	-12.82***	-10.58***	-10.16***
	(0.85)	(2.48)	(2.79)	(0.88)	(3.04)	(3.53)	(1.61)	(1.18)	(1.38)
AllCom	-14.24***	-4.67*	-5.036*	3.59***	2.59*	2.54*	13.01***	2.21	2.71**
	(0.98)	(2.28)	(2.07)	(0.86)	(1.15)	(1.23)	(1.67)	(1.26)	(0.87)
StageCom	-18.45***	-24.21***	-24.11***	12.61***	16.68***	17.55***	8.09	9.706	8.46
	(3.03)	(3.46)	(2.83)	(3.51)	(3.76)	(4.02)	(8.33)	(8.78)	(8.46)
Info * GC		-16.84***	-16.30***		18.04***	18.57***		-4.98***	-6.45***
		(4.96)	(3.36)		(4.45)	(4.85)		(1.20)	(0.813)
Info* AC		-19.51***	-18.97***		2.267	3.10*		21.75***	19.95***
		(3.70)	(3.37)		(1.33)	(1.51)		(4.27)	(2.68)
Male=1			-2.29***			1.27***			1.46
			(0.68)			(0.08)			(0.77)
Experience			0.08			1.15			-1.63
			(1.00)			(0.63)			(1.30)
Education			2.65			0.32			-4.25
			(1.37)			(1.58)			(2.64)
Age			-0.09			-0.63***			0.94***
			(0.17)			(0.07)			(0.27)
Constant	34.72***	29.34***	29.07***	41.15***	44.96***	56.57***	44.13***	45.57***	31.28***
	(1.32)	(2.66)	(6.57)	(1.31)	(1.25)	(1.88)	(1.53)	(1.54)	(8.54)

Note: Standard errors are in brackets. * p<0.05, ** p<0.01, *** p<0.001. Due to the lack of SGCT the coefficients in StageCom are likely to be biased.

5.5.3. Further Evidence

In addition to the proposed hypotheses, the experimental setup allows a wide range of further analysis. In this section we briefly discuss some of them. First, we investigate the acceptance rates, i.e. how often were the proposals accepted by the majority of the group. Second, we analyze the proximity of the proposals to (i) Nash Equilibrium, (ii) fair allocation among all five players, and (iii) fair allocation among the pivotal players. Since information and communication affected the proposals for different players in different ways, the paragraphs aim to summarize this multidimensionality from a different perspective.

Starting with the acceptance rates, we first pursue the analysis on the group level. Therefore, acceptance means that at least three out of five individuals accepted their respective shares. As is depicted in Table 5.5 communication has a strong increasing effect on the acceptance of the initial proposal. Information alone increased the acceptance rate only slightly and insignificantly. Likewise, the same analysis is possible on the individual level, i.e. analyzing the percentage of votes in favor of what had been allocated to oneself. These results are also depicted in Table 5.5. Further note, that differences in the two measures are a proxy of a certain dissatisfaction of individuals with the accepted proposals, e.g. in GCIT 92% of proposals were accepted, yet only 66% of players voted in favor of them.

Table 5.5 Group acceptance rates

Level of Acceptance	BT	IT	GCT	GCIT	ACT	ACIT	SGCIT
Group Acceptance	0.6	0.6429 (0.5981)	0.7901 (0.0095)	0.9231 (0.0000)	0.9762 (0.0000)	0.8810 (0.0013)	0.9815 (0.0000)
Individual Acceptance	0.5573	0.5886 (0.1555)	0.6889 (0.0000)	0.6590 (0.0000)	0.8714 (0.0000)	0.8286 (0.0000)	0.7889 (0.0000)

Note: The p-values in brackets result from a two-sided MW-test in comparison to the BT.

At this point we need to address the issue of the other stages. Based on the experimental design, groups that rejected the proposer proceed with stage two, where the proposer leaves the game and Player 2 becomes the new proposer. Deducing from the acceptance rates, it is apparent that several groups proceeded to stage two of the experiment. While the results indicate very interesting behavior, e.g. providing oneself less money than other participants, we cannot use statistical analysis to support this observation for three reasons. First, even in the BT where only 60% of groups accepted the proposals, this leaves us with a smaller number of independent observations. Second, we calculated the acceptance rates based on the strategy approach in our experimental design. This means that some of the rejected proposals were not picked as payoff relevant. Thus, there are proposals that were declined, yet there is no information on proposals in the second stage. Third, in stage two it is not possible to utilize the strategy approach anymore, since every position is permanently determined by then. This reduces the number of observations even further. For all treatments there are only 21 observations of contributions in the second stage. Therefore, we simply illustrate the basic results in Appendix 5.6 and conclude that considered by itself they reveal further interesting research questions.

Further, we would like to address the issue of whether individuals tried to disguise unfair allocation as fair ones. This could be possible since every participant only receives information on their respective offer. Theoretically the proposer could have made a verbal statement in favor of a fair allocation but then give only two other players 24 LD each while keeping the rest (72 LD) for himself. However, this behavior was observed exactly once. In ACIT one individual in fact disguised the allocation as a fair one, yet kept 72 LD for oneself. A more detailed analysis of the proposals close to 72 LD yields no indications of any similar disguising behavior. It is further conceivable that the proposer would incorporate the share of only one other player in order to be sure to receive the required majority. Yet, proposals of 48 LD were not observed.

Finally, we try to assess the different proposed schemes from a global perspective. The goal is to provide a measure of how the proposals in the different treatments are related to each other with respect to plausible outcomes. This means we investigate how close the proposals were to the Nash Equilibrium (118/0/1/0/1) or the fair share allocations (24/24/24/24/24 and 40/0/40/0/40). In order to measure how close the proposals were to the NE or the two types of fair share allocations we provide a proximity measure, being the sum of squared differences to the respective proposals in the group.[27]

The analysis indicates that providing information to the subjects increases the proximity to NE (see Table 6). Implementing communication has the opposite effect. Looking at the distance to the Fair Share, providing information leads to more unfair proposals. However, this is not true for the In-Group Fair share. Here it becomes clear, that the ability to communicate within the group of three yields the subjects to offer proposals closer to the in-group fair share of 40 LD per person. Having a second communication stage where everybody communicates jointly

[27] For the Nash Equilibrium the formula would be: $(120\text{-}P1)^2+(0\text{-}P2)^2+(1\text{-}P3)^2+(0\text{-}P4)^2+(1\text{-}P5)^2$, with P1-P5 being the offers made for the respective Players 1-5. For the Fair Share: $(24\text{-}P1)^2+(24\text{-}P2)^2+(24\text{-}P3)^2+(24\text{-}P4)^2+(24\text{-}P5)^2$. For the in-group Fair Share: $(40\text{-}P1)^2+(0\text{-}P2)^2+(40\text{-}P3)^2+(0\text{-}P4)^2+(40\text{-}P5)^2$.

reduces this effect, yet it does not nullify it. The proximity to the Fair Share in ACT further supports the Hypothesis for the ACT treatment.

Table 5.6 Proximity to Nash Equilibrium, Fair Share, In-group Fair Share

	BT	IT	GCT	GCIT	ACT	ACIT	SGCIT
Nash Equilibrium	10699.8	7065.0	10716.5	9767.7	11321.9	10556.5	10800.4
Fair Share	541.4	2660.1	541.3	1353.9	55.0	380.1	640.7
In-Group Fair Share	2275.8	2673.8	1559.6	569.2	1957.9	1955.4	1271.9

Note: The results are denoted in LD² on the group level.

In addition to the description in Table 5.6 we provide another way to compare the average solutions between the treatments. The values in Table 5.6 display the distance to one specific solution. Therefore, the comparison is one-dimensional. However, it is possible to consider every distance as a vector in a three-dimensional space, e.g. BT = (10699.81, 541.44, 2275.84) and then calculate the Euclidian distances.[28] Table 5.7 indicates the proposals in several treatments (BT, GCT, ACT, ACIT, SGCIT) are relatively close together. IT is very distant to other treatments. GCIT is less distant than IT, yet it is too far away from the five mentioned treatments. The SGCIT bridges the distance between GCIT and ACIT, which is exactly what one might expect, since SGCIT is a combination of the other two types.

Table 5.7 Euclidian distances between distances to specific solutions

	BT	IT	GCT	GCIT	ACT	ACIT	SGCIT
BT	0						
IT	**4225.96**	0					
GCT	**716.48**	4366.28	0				
GCIT	2107.43	<u>3666.07</u>	1594.13	0			
ACT	851.31	5041.83	**872.68**	2455.75	0		
ACIT	**<u>386.32</u>**	4231.41	456.34	1868.61	<u>831.58</u>	0	
SGCIT	1013.89	4471.75	**<u>315.77</u>**	<u>1438.29</u>	1041.93	<u>771.12</u>	0

Note: The smallest distance in every row is printed bold and the smallest distance in every column is underlined.

5.6. Discussion

In this section we discuss the results with respect to the initial question of how communication and information affect a strategically distorted majority voting. The presented results can be

[28] Please note that calculating the distance to the zero vector is not meaningful as in this game it is not possible to offer a proposal that is simultaneously a NE and is fair.

summed up into two major statements. First, information on how the majority voting is distorted and group communication based on such a strategic distortion benefit different types of subjects in the group. Pivotal players benefit from a separated communication. Non-pivotal players benefit from joint communication. Information benefits foremost the proposer, yet also other pivotal players. Second, the acceptance rates are driven by communication, but not the information. We now turn to discussing these issues individually.

First, our analysis indicates that while grouped communication and equilibrium information both hurt the non-pivotal players, they affect the pivotal agents differently. The provided information illustrates the strategic distortion in the voting procedure. It stresses the theoretical power of the proposers the most. In monetary terms the proposers are entitled to 98.33% of the total endowment and the other pivotal players to 0.83% respectively. Despite providing other allocation schemes to think about in the instructions, the theoretical information stresses the power of the proposer more than of the other two pivotal players. This may be an explanation for the strong increase in the own share of proposers in IT. In contrast to this, communication strengthens the group identity of the pivotal agents. First, the players are able to coordinate their strategy. Second, group communication is very likely to decrease social distance among the members. In accordance with previous literature this can lead to in-group favoritism as was observed by e.g. Chen and Li (2009). This is likely the reason for why non-pivotal players strongly benefitted from the joint communication.

Second, in reality there may be some types of asymmetries (based on economic, historical, cultural reasoning) dividing the subjects into a majority and a minority group. We illustrate that a simple communication barrier is enough to display such an asymmetry and that the pivotal groups make use of it. In addition to decreasing social distance, communication assists the coordination within the group. This is supported by our analysis of the acceptance rates. In the course of the discussion the agents are able to find common ground. This should decrease the risk of any of them deviating from the proposals. Therefore, the acceptance rates increase. In the treatment without information (GCT) some of the pivotal agents may lack the theoretical justification for their entitlement. In contrast to this, in GCIT it is easier for pivotal players to agree on solutions which disregard the interests of non-pivotal players, which is exactly what we observe in the data. Thus, if the pivotal agents are aware of their pivotality and can coordinate separately they largely disregard the financial interests of the non-pivotal group. The effect remains even when the separate communication is followed by a joint discussion as in our SGCIT treatment. Thus, we illustrate the general power of preliminary talks within a special group of interests, as they can happen in G7, G20, OECD or other formats. Nevertheless, the importance of communication in the general assembly must be stressed. Although it could not completely break the power of the pivotal group, it did lead to a noticeable weakening of the assertiveness of the pivotal group. This insight is of great importance for real negotiations. Against this background, full meetings of the UN or deliberations in the EU Parliament take on new significance.

Finally, we would like to add some remarks concerning the limitations of our study. While we implemented several reasonable steps of communication in terms of group composition (no communication, communication in two groups, joint communication, and the staged communication) there are of course more. Further, there are also other ways to implement information into this experimental design. We would like to discuss two major ways left aside. First, it is possible to provide the information as private to the proposers only. This way it should be easier to differentiate the entitlement effect. The proposers would be aware of their initial power and the fact that other subjects are not aware of it. However, the implementation would require more changes to the experimental design (e.g. stepwise introduction of this information in following stages for the new proposers or stopping after the first stage even in case of rejected offers) making this a slightly different research topic. The second alternative is to change the allocation of subjects to groups, i.e. subjects 1, 2, and 3 become one group whereas 4 and 5 are the second group. In this way, it should be possible to investigate whether it is the information itself driving the changes or the pure awareness of being pivotal as a group of three individuals. However, we argue that the individuals experienced almost exactly this state in one of our treatments. Based on communication protocols no group in treatments without information identified the theoretical optimum of the game. Thus, they were not aware of the reason why subjects 1, 3, and 5 built one group. Therefore, putting other subjects into the three-person group should lead to identical results. However, mixing pivotal and non-pivotal players while providing information on the Nash Equilibrium may be worth further investigation.

5.7. Conclusion

Our research indicates how a seemingly symmetrical majority voting procedure on a financial allocation can be distorted by the inherent power structure of the institution or communication barriers. Using the PGE we illustrate that informing individuals about their pivotality leads to an entitlement effect. Then, pivotal players and foremost the proposer, who has the highest power in the setup, try to extract a higher share of the total endowment. Meanwhile communication is shown to be a social distance reducing, fairness concerns raising, coordination enhancing, and identity creating measure. Therefore, in our setup communication led to two major effects. First, it reduced the inequality among the communication partners. Second, it led to major in-group favoritism, implying that pivotal agents had less concerns for non-pivotal players. The findings can be applied to negotiations where power is distorted by common interests of agents, e.g. net payers in EU. They further shed light on the issue of preliminary agreements in subgroups. Although a discussion in the general assembly reduces the assertiveness of pivotal agents, they are still able to extract more of the total endowment.

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Appendix 5.1. Experimental procedures in all treatments

Basic Treatment

In the basis treatment (BT)[29] the subjects were uninformed of the Nash Equilibrium and did not communicate with each other. Using the strategy method, the subjects were first left uninformed about their actual position in the hierarchy and were all asked to make a proposal from the perspective of the proposer. Subsequently, every subject voted on all five proposals from the perspective of a certain position in the hierarchy shown on the screen. Ultimately, the individuals were informed about their actual position in the hierarchy and whether the proposal of the actual P1 was accepted. In case of acceptance the experiment was over. In case of rejection the positions of the subjects remained constant and the experiment proceeded with P2 making the next proposal.

Information Treatment

The procedure of the information treatment (IT) was identical to (BT). However, all subjects received information on the theoretical outcome of the experiment on the instruction sheets. The instructions are formulated in textbook style and illustrate the functionality of backward induction step by step for every stage. Further, it refrained from providing only the 118-0-1-0-1 solution but also listed three more possible solutions (100-0-10-0-10; 60-0-30-0-30; 40-0-40-0-40) acknowledging the existence of pivotal and non-pivotal players. However, it was stressed, that any other distribution is also feasible.

Group Communication Treatment

Identical with BT in the group communication treatment (GCT) there was no information on the theoretical outcome. In order to apply the strategy method and provide communication prior to the contribution stage, the subjects were first divided in two groups (P1, P3, P5 and P2, P4). They were informed that members of the first group will ultimately become P1, P3 or P5 and members of group two will become P2 or P4. Then both groups communicated separately for three minutes. After the communication period every member of the first group made a proposal from the perspective of P1. Subsequently, the individuals were informed about their actual position in the hierarchy and voted on all three proposals from the perspective of their position. Ultimately, the acceptance or refusal of the actual P1's proposal was displayed.

[29] For instructions please see Appendix 5.5.

Group Communication and Information Treatment

The group communication and information treatment (GCIT) was a combination of the procedure in GCT and the information status of IT.

All Communication

The procedure in the all communication treatment (ACT) is identical to GCT with only one change. After the subjects were informed about their group affiliation the communication took place jointly as a group of five subjects. During the communication, there was a note indicating who belonged to the group of three players and who was in the two-person group.

All Communication and Information Treatment

The procedure in the all communication information treatment (ACIT) is identical to ACT yet includes the information likewise to IT and GCIT.

Staged Group Communication and Information Treatment

The procedure in the staged group communication information treatment (SGCIT) is a combination of GCIT and ACIT. The informed subjects first communicate separately for three minutes and subsequently they communicate all together for another three minutes.

Appendix 5.2. Power Analysis

Table 5.8 Minimum detectable effects of proposer share

BT	IT	GCT	GCIT	ACT	ACIT	SGCIT
7.53	14.48	3.89	3.71	1.70	9.32	3.89

Note: The effect sizes were calculated for power of 0.8 and alpha of 0.05 in LD at given sample sizes.

Appendix 5.3. Graphical representation of average contributions

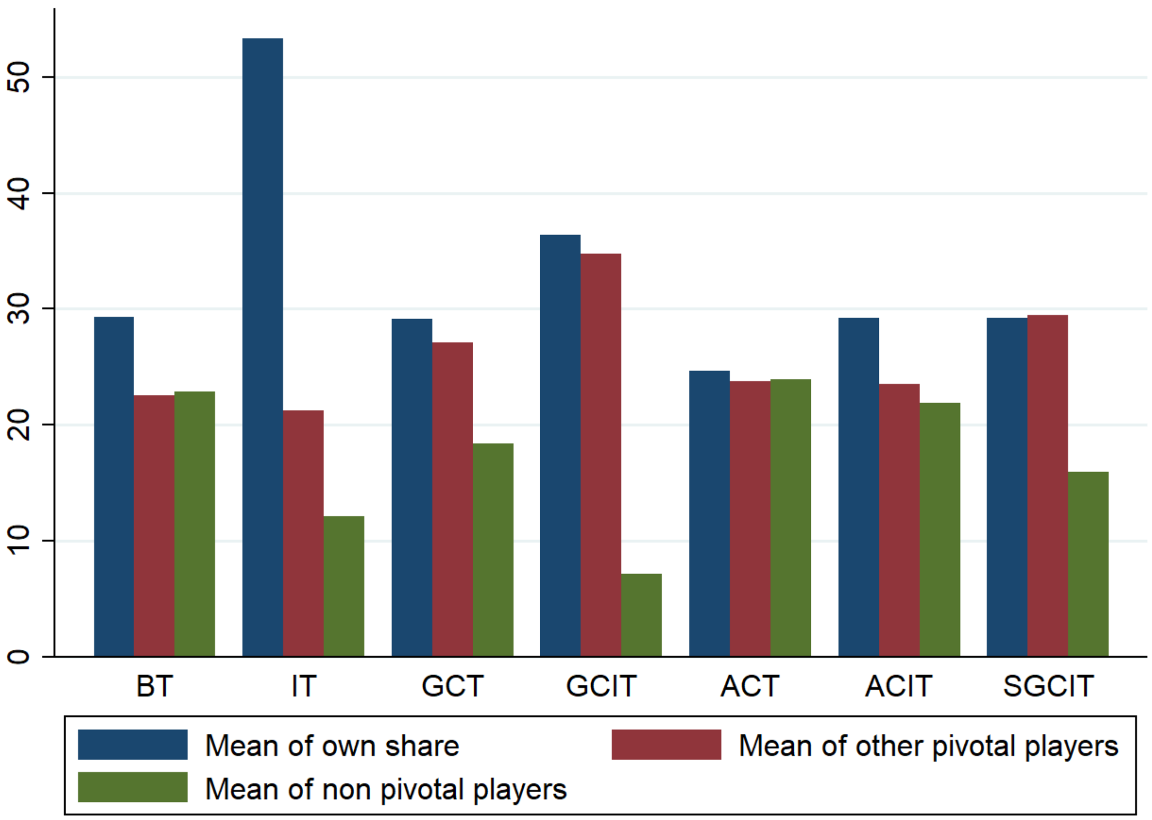


Figure 5.3 Overview of proposals for different interest groups in different treatments.

Appendix 5.4. Overview of different proposals for other pivotal and non-pivotal players.

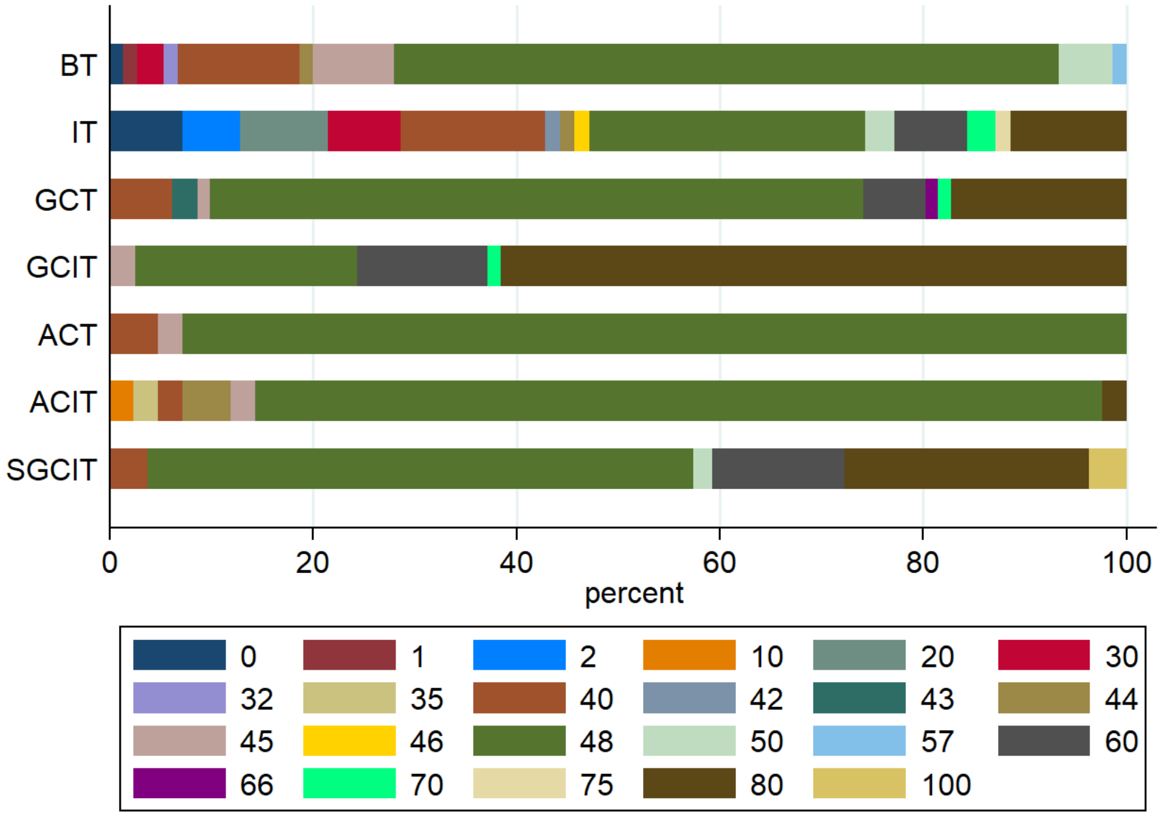


Figure 5.4 Overview of shares for other pivotal players

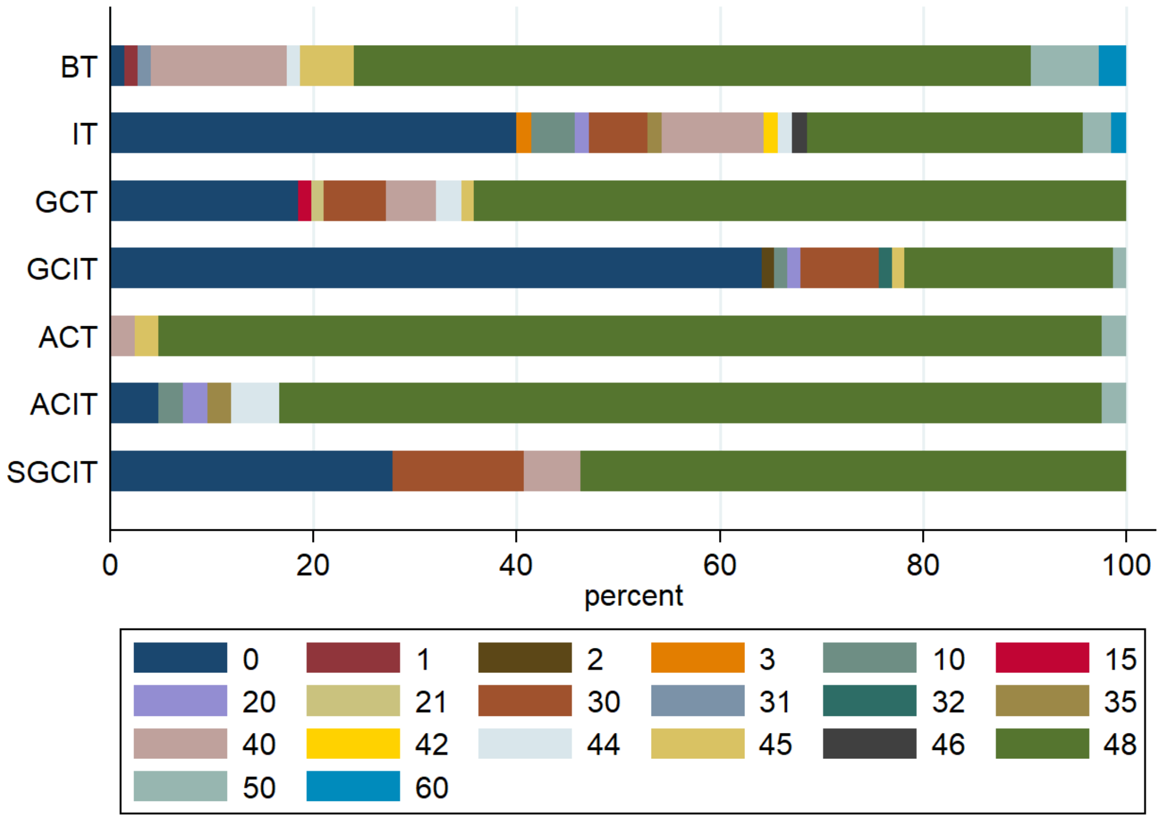


Figure 5.5 Overview of proposals for different interest groups in different treatments.

Appendix 5.5. Bonferroni and Holm corrections

Table 5.9 Bonferroni and Holm correction for multiple hypothesis testing

Test Nr.	Treatment	Unadjusted	Bonferroni	Holm
1	BT	0.0005	0.0075	0.0030
2		0.0043	0.0645	0.0172
3	IT	0.0000	0.0000	0.0000
4		0.0000	0.0000	0.0000
5		0.0000	0.0000	0.0000
6		0.0003	0.0045	0.0021
7	GCT	0.0000	0.0000	0.0000
8		0.0078	0.1170	0.0234
9	GCIT	0.0000	0.0000	0.0000
10		0.0391	0.5865	0.0782
11	ACT	0.0000	0.0000	0.0000
12	ACIT	0.1009	1.0000	0.1009
13		0.0000	0.0000	0.0000
14	SGCIT	0.0009	0.0135	0.0045
15		0.0000	0.0000	0.0000

Appendix 5.6. Results of further stages

Table 5.10 Results of stages two to five

	BT	IT	GCT	GCIT	ACT	ACIT	SGCIT
Stage 2	6 43.33	7 65.71	4 49.25	3 40	0 -	1 48	1 65
Stage 3	2 52.5	3 56.67	0	0	0	0	0
Stage 4	2 85	0	0	0	0	0	0
Stage 5	0	0	0	0	0	0	0

Note: Numbers in the first line depict the number of observations of the stage in the respective treatment. Numbers in the second line of every cell are the average proposals for the combination of the stage and treatment

Extract of the Online Appendix

For reasons of shortness I include only the instructions from one treatment (GCI). The differences in the experimental procedure are explained in the text and in Appendix 5.1.

The Experiment (GCI)

Description of the experiment:

In the experiment today, five people each form a group and are supposed to distribute 120 coins among themselves. One coin corresponds to 0.42€. The money will be divided among the participants if at least 50% of the players agree to the proposed distribution. All players are numbered from 1 to 5. The five persons in a group remain in the same group during the experiment. The assigned number also represents the hierarchical position of a participant, with player 1 representing the highest level und player 5 the lowest.

In the beginning player 1 plays the role of the one who proposes the money distribution, in the following called "Proposer". After player 1 has proposed a distribution, the other players, called "Receivers", and the "Proposer" vote on whether the distribution is accepted or not. If at least 50% of the players agree, i.e. 3 players in this round, this distribution is accepted and paid out and then the experiment is finished.

If the proposition does not win a majority, the Proposer, player 1, is excluded from the game. Player 2 takes his place and proposes a new distribution of the 120 coins. To reach an agreement, at least 50% of the participants (2 players) must still agree, as there are 4 players left (3 Receivers and 1 Proposer). If the distribution proposed by player 2 is accepted, it will be paid out and the experiment is finished.

However, if the distribution of player 2 is not accepted, player 2 will also be excluded from the game. Player 3 takes his place and proposes a distribution of the 120 coins. Again, to reach an agreement, 2 players need to unite 50%of the votes. If an agreement is reached, the distribution proposed by player 3 is accepted and paid out and the experiment is finished.

If there is no agreement, player 4 becomes the Proposer. The Proposer proposed a new distribution of the 120 coins. From the remaining two players, Receiver and Proposer, one player is required to receive at least 50% of the votes. If the proposed distribution is accepted, it is paid out and the experiment is finished.

If it is not accepted, player 4 is also excluded from the game and player 5 receives the full amount of 120 coins.

Please note additionally:

If all players behave rationally and expect the same from their fellow players, the theory predicts the following optimal distribution. Player 1, the "Proposer", receives 118 coins. Player 2 receives 0 coins. Player 3 receives 1 coin. Player 4 receives 0 coins and player 5 gets 1 coin.

This can be shown by looking step by step at different scenarios. For one player, the player would receive 120 coins. If there are two players, player 4, the "Proposer" keeps 120 coins and player 5 gets 0 coins. This is valid because player 4 votes for the proposed distribution and thus the required 50% majority is reached. In the three-player round, the "Proposer" player 3 must win another vote to gain the majority. Player 3 can win Player 5 by offering him 1 coin. This is more than player 5 would receive from player 4 if only these two players remain in the game in the next round.

If four players are in the game at the beginning of the round, the "Proposer" (player 2) must win another player. The best way to do so is to propose 1 coin to player 4, as this player would end up empty in the next round. It should be noted that it is cheaper to win player 4 instead of player 5 with a coin. Since player 5 would also receive 1 coin in the next round, the "Proposer" would have to offer him 2 coins to ensure his vote.

If at the beginning of the round all five players are still in the game, it becomes clear that the "Proposer" (player 1) needs two more players besides his own vote to accept his distribution. For the Proposer, it is best to offer player 3 and player 5 each 1 coin, otherwise they would end up empty in the next round. These results of the theoretical model are summarized in the following table. The bold letters indicate who the Proposer is in each round. It becomes clear that at the beginning of the experiment player 1 (as the Proposer) needs two more votes in addition to his own to enforce his proposition, which according to the theoretical model come from player 3 and 5.

	Player 5	Player 4	Player 3	Player 2	Player 1
Count=1	**120**	-	-	-	-
Count=2	0	**120**	-	-	-
Count=3	1	0	**119**	-	-
Count=4	0	1	0	**119**	-
Count=5	1	0	1	0	**118**

The following additional suggestions for the Proposer in the first round can be derived from the theoretical solution. Further distributions are also possible.

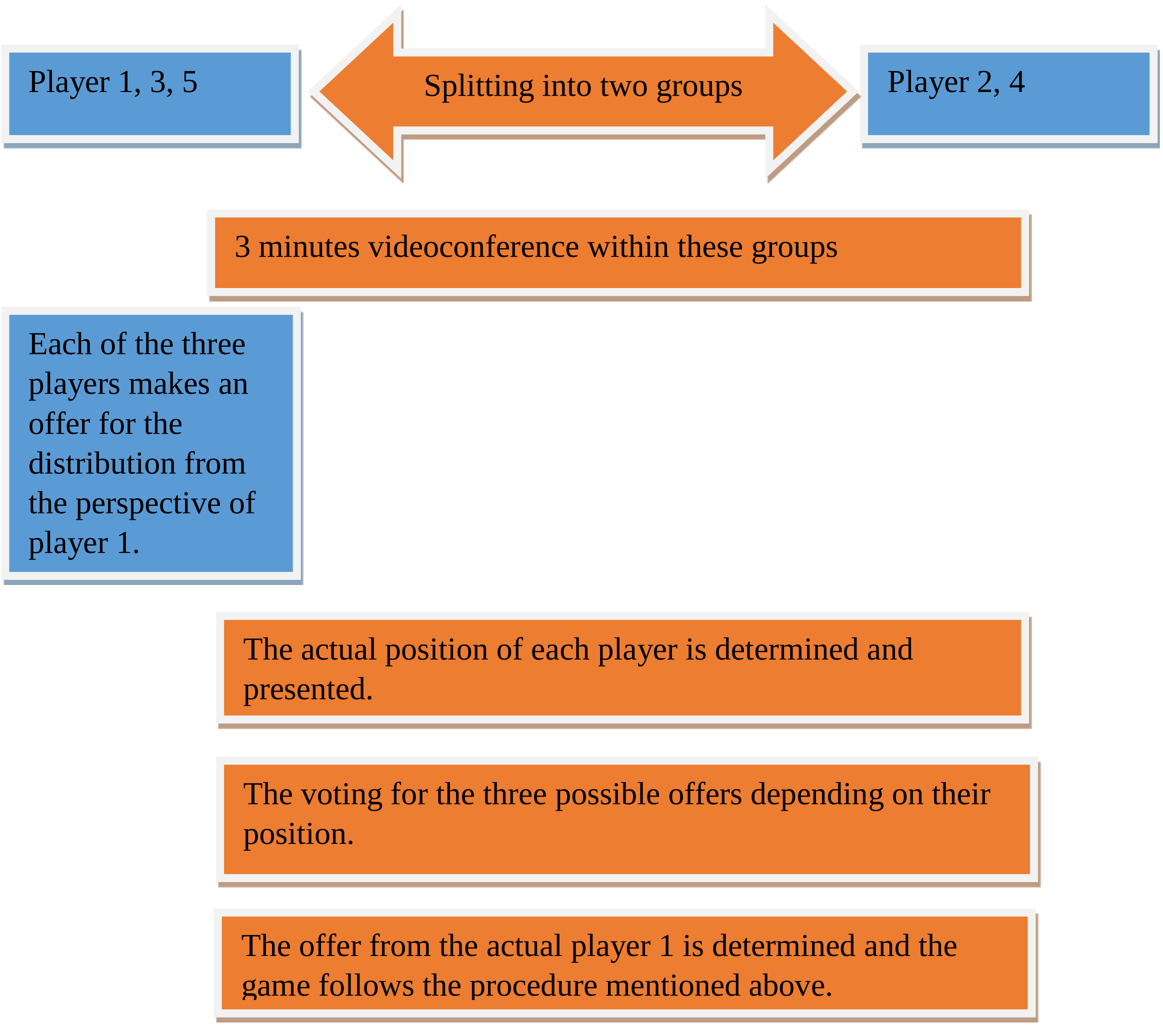
	Player 5	Player 4	Player 3	Player 2	Player 1
	10	0	10	0	**100**
	30	0	30	0	**60**
	40	0	40	0	**40**

Procedure of the experiment:

At the beginning of the experiment the players are divided into two groups. In the first group there are players 1,3,5 and in the second group there are players 2 and 4. The information who exactly has which position will be given to you a little later. After the division into these two groups you get the possibility to communicate for 3 minutes in your group of five via videoconference. The three players (1,3,5) get the addition “group of three” and the two players (2,4) get the addition “group of two” on their screen.

Following the communication, each of the players in the group of three (1,3,5) must make an offer from the Proposer’s perspective. The order of the players in the hierarchy is then determined. The players then vote on the three possible offers according to their position. Then the offer of the actual player 1 (the Proposer) is used. Thus, the offer of the player who has been determined the Proposer is valid and the result of the vote of all five players on this offer. If the offer is accepted, the distribution will be paid out accordingly. If the offer is not accepted, the players remain in their positions in the game and the experiment continues as described above.

Note that you will be filmed during the experiment. Your decisions and your video recording will not be associated with your name. You therefore act anonymously.



6. Concluding remarks

Eventually it is time to recapitulate and conclude the outcomes of this thesis. Therefore, the goal of the last chapter of this thesis is to analyze how the findings of the thesis fit to the literature and what can be learned for future experiments involving communication. In doing so, this chapter will first in short discuss previous chapters respectively. Then the findings of these chapters will be put together. The last part of this chapter will discuss possible applications and come back to the questions raised in the very first chapter of this thesis.

The first chapter presented a very short overview of the hitherto development of experimental economics and research on communication. The main purpose of the chapter was to illustrate the importance of a precise classification of communication in laboratory experiments as communication can have a variety of different effects due to its complex nature. It further briefly referred to the rise of economic laboratories. The laboratories were set up to exclude uncontrollable influences from communication, thus improving the quality and replicability of research in experimental economics. The last paragraphs of this thesis will revisit this line of thoughts.

The second chapter of this thesis addressed the issue of economic education in public goods experiments. Concerning economic education, it can be assumed that the total effect of the educational background is more complex than originally thought (Kirchgässner, 2005). While previous findings often showed economists to contribute less to public goods in experiments, the findings in the second chapter draw attention to something broader than economic education: the economic experience. In fact, the contribution patterns of subjects with economic education are similar to subjects with more experience. While in hindsight it was unfortunately not possible to precisely investigate the experience of the participants in laboratory experiments, two issues support this observation. First, analyzing undergraduate students without a Bachelor’s Degree (less experienced) to students holding a Bachelor’s Degree (more experienced) illustrates similar difference in contribution patterns as Non-Economists vs. Economists, respectively. Second, a paper by Nax et al. (2016) provides very similar contribution patterns while illustrating the stepwise implementation of experience. This chapter did not intend to answer the question on whether the differences between economists and non-economists are driven by an education or a selection effect, instead it presented a new perspective. Considering economic education to be a way to obtain specific experience mirrors the standard explanation of an education effect. Likewise, individuals with a higher economic experience may be self-selecting into economic study programs. This would be a typical explanation based on the selection effect. The sometimes applied, yet oversimplifying, critique of economists being selfish, however, cannot be supported by these observations. In contrast, in the first periods of the public good game the economists contributed the socially optimal rates more often than non-economists. Experiencing the steady decline in contributions of their co-players led them to decrease their contributions even faster. This resembles a type of tit-for-tat strategy. Further, economists were more eager to invest in the communication platform which evidentially provided efficiency. However, these differences occurred only in the non-risky investment scenario. In total, the first chapter indicates that the effect of economic education, while not being a major focus in general research of economics in the past years, remains of interest and contributes to the more general framework of experience.

The third chapter departed from the simple framework of economic experience and financial incentives in a public goods game. Previous literature showed that when different financial incentives - which elevated contributions to very high rates - are taken away, they can backfire (Bruttel & Friehe, 2014) leading to contributions levels lower than in the untreated groups. These findings were further in line with Fehr and Gächter (2000) who found the same effect when investigating the introduction and removal of a sanctioning mechanism. Since a lot of institutions in the real world operate with such financial incentives these findings are of major practical relevance. Nevertheless, the number of behavioral interventions in economics increases, possibly due to the high return to cost ratio whenever applied right. Thus, it is of interest to analyze whether such informal interventions can induce permanent changes and avoid backfiring effects that potentially come with financial incentives.

The third chapter therefore addressed the question whether the effect of experiencing an almost perfectly functioning behavioral institution outlives its removal and the randomization of subjects between the groups. In a nutshell, the results indicated that the behavioral changes are more permanent than expected. While the contributions did not yield the high rates induced through communication, individuals showed significantly higher rates than in the prior blocks without communication.

Another remarkable result of the analysis was the large end-game effect. The otherwise stable contributions decreased strongly in the last period of the game. This can be explained by the fact that communication de facto cannot solve the end-game problem. One of the reasons why communication works well is that by talking to each other and seeing each other’s faces the subjects develop trust for future cooperation and in fact provide high contribution rates. At the end there is no future cooperation and it becomes beneficial to collect the maximum profits.

This effect is probably amplified by the communication being cheap talk. Since most groups experienced the end-game problem twice before the final period in the last block, they possibly expected end-game behavior to happen again. Thus, by trying to avoid being taken advantage of by free-riders, a lot of subjects themselves started free-riding in the last period of the last block. This is in line with the theoretical learning model of end-game behavior by Selten and Stoecker (1986). Such an adaptation of contribution patterns is further in line with the findings on the role of experience provided in the first chapter of this dissertation. Further, the analysis showed that the subjects were not affected by the signal of successfully or unsuccessfully funding the communication when deciding on their contributions. Alternatively, the effect of this signal was too ambiguous to influence their decisions significantly. Thus, groups who failed to fund the communication platform and those in the control treatment who did not have the chance to fund it illustrated similar contribution patterns. The research of chapter three therefore has several potential implications: First, at least in the context of public goods experiments, behavioral interventions infuse a surprisingly permanent behavioral change. Second, this allows for a periodic implementation of behavioral tools in similar cases. This means that the behavioral interventions need not to be implemented permanently for all subjects, but a more punctual implication may work to a sufficient degree, improving the cost-benefit ratio of possible applications in the real-world.

The fourth chapter of the thesis turned the focus on the communication itself. Deviating from the standard analysis tools for economic research, it applied the analysis of facial expressions and the contents to demonstrate the prediction accuracy of such a technology. In the specific setup, the communication took place before the actual economic action. If successful, this approach offers a major advantage: The opportunity to observe and analyze the communication in order to predict whether the subsequent economic action will be the desired one, e.g. from the perspective of a social planner. Once a prediction is made, the observer can decide whether to intervene into the following contribution process. This tailoring of interventions based on predictions informed by the communication process is another step towards a more efficient and precise application of interventions - may these be formal (e.g. punishments, rewards) or informal (e.g. provision of information, communication). While from the purely efficiency concerned perspective of economics this may be a major improvement, there is also a potential downside to such developments. In a nutshell, this application of technology entails several ethical concerns. A behavioral intervention specifically designed for an individualized application is likely to violate the current norms and laws concerning privacy. Therefore, the chapter shall not be considered as a plain ready-made instruction on how to identify free-riders. Instead, it simply describes what is technologically possible even in a setup which was not explicitly designed for this type of analysis. Still the results in the chapter are especially important for four major reasons:

First of all, the analysis illustrates how specific buzzwords or buzz topics can indicate future behavior. In a nutshell, being more aware of a specific problem influences your solution to it. While this statement is particularly logical, it is not self-evident to be able to verify it in a laboratory experiment with unrestricted face-to-face communication. However, the analysis of content in experimental economics is still in its infancy. Publications on the matter utilizing machine learning, such as by Penczynski (2019), provide a first glimpse of what is going to be possible in near future.

Second, it is important to stress that although the approaches used in the fourth chapter do not allow any causal claims, their explorative value should not be dismissed easily. The missing theoretical underpinnings for a causal link may at first sound troublesome, especially in experimental economics. However, approaches like machine learning were not designed to provide causality but to shed light on factors that were potentially left aside for different reasons. It is thus the job of the experts to identify whether the results of the analysis actually make sense and matter. While this may cause critique as it affects the puristic perspective on experimental economics, the problem is not a new one. The dispute between the puristic approach of analyzing only the precise research hypotheses under which the experiment was designed and the pragmatic approach of analyzing further important results incidentally discovered in the data is not new. The dispute can be illustrated by an example attributed to Reinhard Selten: In this allegory he discusses a group of astronauts, on a mission to find out whether there are red stones on Mars. The astronauts are indeed able to validate this hypothesis. Yet, under every stone they also do find worms. It sounds too illogical to start a new expedition to capture the worms the next time (Weimann & Brosig-Koch, 2019). Such incidental findings can lead to important discoveries and machine learning is just a tool to extract them in a more structured way. However, luckily experimental economics is not astronomy. If there are interesting findings based on results from machine learning, it is much easier to design an experiment to test the new hypothesis. Applied to the fourth chapter of this dissertation several hypotheses can arise. With respect to the content it is possible to restrict the topic of conversations to something not related to the experiment. The question would be then whether groups would still have very high contributions. Another approach is to gradually reduce the duration of communication. Up to a certain threshold this should lead to a more condensed information flow and thus a higher signal-to-noise ratio. With respect to facial expressions, it is worth investigating whether the lead of end models (models evaluating the last parts of the videos) over beginning models (focusing on the first parts of the videos) prevails depending on different duration of communication. This can be further enhanced by either investigating whether the individuals are more stressed at the beginning of the conversation (e.g. by measuring the pulse through a webcam) or by linking content to the respective facial expressions.

Third, the results from the fourth chapter indicate that even communication channels, which are less condense than speech, such as facial expressions, can be used to predict contribution rates in an experimental setup. This is a novel finding. While it is tedious to causally link specific facial action units to cooperation for different reasons, the exact display of correlations is not always recommendable either, due to interpretation problems for humans (e.g., computer vision and human vision are not the same, action units operate in groups, etc.). Nonetheless, the main result is that despite these restrictions machine learning, or more broadly speaking artificial intelligence, can indeed provide useful predictions for cooperation. Looking at the ongoing development in detection algorithms and the quality of data, these predictions are very likely to improve in the next years, if not months. The research indicates that experimental laboratories can become an important source for high quality and highly relevant data. Yet, the experimental designs may be in need of some changes for that matter.

Fourth, the chapter provided examples on what can be improved in order to prepare experimental laboratories for a new wave of research in experimental economics. And it stresses the importance of open data. The more data on communication is available, the easier it is to build content libraries for all experimenters. This however would become a highly ambitious long-term project due to several obstacles. First, since research in experimental economics is not only conducted in English, this basically requires at least one library per language. Additionally, the libraries in the different languages should be comparable to each other in terms of the involved experimental designs, coding procedures, and other relevant factors. Second, even simplifying the agenda to one specific language, there still remain hundreds of communications in laboratory experiments which would have to be classified either by means of e.g. (un)supervised machine learning or structured analysis of experimental economists and linguistic experts. Besides content libraries, voice to text software will ease the transcriptions and eventually set higher standards. Eventually, subjective coders may become redundant.

Well-trained prediction models could make live predictions on the subsequent behavior and potentially confront the subjects in the laboratory itself. While such things may have sounded too futuristic a couple of years ago, they do not now. The fourth chapter of this thesis is just a first outlook into the quickly approaching future.

The fifth and penultimate chapter of this thesis turns the focus back to the more traditional economic frameworks, yet it keeps on investigating the effects of communication. It makes use of the classification of communication discussed in the introduction of this thesis and analyzes the effect of communication with respect to different group compositions and the information state of the participants involved. The results support findings that communication reduces social distance, raises fairness concerns and enhances coordination. Furthermore, communicating in separate groups contributes to group identity and thus causes in-group favoritism. Such in-group favoritism becomes stronger whenever these individuals are entitled to a higher share of the total endowment due knowing the power structure of the game. This constitutes another novelty in the literature, as it allows for the group identity to rely on a Nash Equilibrium result. The findings indicate how the different combinations of communication and information state influence different stakeholders in completely different ways. This is another vivid example of the versatility of communication in laboratory experiments.

Concerning the overall implications of this dissertation, the last paragraphs shall try to relate the results to each other and to the research question in a broader manner. While the thesis in parts focuses on the effects of economic education, in hindsight it appears to be more useful to additionally refer to economic experience and information. The results of the second chapter confirm that economists and non-economists behave differently, yet the differences change with respect to the specific problem. This would be in line with some mixed findings in the literature on the effect of economic education. However, more research is needed to test this hypothesis. The results of the fifth chapter illustrate how the provision of a previously unknown economic information on complex problems shifts the decisions towards the theoretically optimal results. Given the increasing amount of literature on how information or its provision can affect decision makers, it becomes expedient to assume that economic information can influence the behavior. In the fifth chapter this thesis discusses an entitlement effect as a possible mechanism which affects the decision-making process. Such an entitlement can be augmented by an accordingly chosen structure of communication. Therefore, the thesis emphasizes the importance of a well-thought-out communication structure planned before its implementation. Communication is in no way a simple instrument to achieve certain goals, e.g.

increasing own profits or achieving socially optimal results. Communication simultaneously operates through different effects, e.g. reduces the social distance, enables coordination among agents, and facilitates the detection of the type of other agents. The way any of these effects operate heavily depends on the precise implementation of communication. Some channels can be even switched off by the right choice of the communication, e.g. in the case of post-play communication, type detection does not play any role, yet the reduction of social distance can still operate through anticipation. Therefore, it is of eminent importance to further analyze communication to understand its effects on economic decision making more thoroughly. Yet, not only can we learn a lot about economic behavior caused by communication, but we can also learn a lot about the way people communicate due to observed economic behavior.

At the very beginning of this thesis I stated that research on communication and research on behavior in experimental economics started separately yet were destined to converge later. However, this conversion is a process which is yet to conclude. Referring to the initial quote by Watzlawick et al. (1967), behavior and communication are just two sides of the same medal. It is impossible to not behave, and it is impossible to not communicate. Behavior affects communication. Communication affects behavior. But communication is difficult to control without over-artificial restrictions. This thesis illustrates how modern technology can help solving this problem in different ways. And if machine learning and artificial intelligence can help analyzing communication it should also be able to advance behavioral research in general. In fact, Camerer (2019) argues that artificial intelligence and machine learning as a tool have the potential to revolutionize behavioral economics in many different ways. This certainly contains the upcoming possibility to design individualized nudges or the emerging research on deception detection. However, this is by far not the only opportunity. Altmejd et al. (2019) use machine learning to predict the replicability of studies in psychology and behavioral economics. Considering such ongoing developments, it is difficult to imagine any future of behavioral economics without machine learning and artificial intelligence in it. Until now it is difficult to predict which frontiers these approaches will reach. In this spirit, this thesis unfortunately answers less questions than it poses for the future.

But I guess we can always discuss these findings face-to-face.

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