Experimental studies on cooperation and coordination in politics, firms and society

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Wenn Du eine weise Antwort verlangst, musst Du vernünftig fragen.

Johann Wolfgang von Goethe (1749-1832)

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Summary

People have different preferences, and when conflicts of preferences arise (as they invariably will), people face the challenge of coordinating their actions in order to cooperate successfully. The problems of coordination and cooperation are reflected in many areas: In political decision making, legislators with different preferences have to agree on joint actions. In firms, departments of companies or subsidiaries of conglomerates need to coordinate to succeed in cooperation. In society, different people need coordinate to provide public goods.

Classical economic theory assumes that successful coordination and cooperation among individuals with different preferences will only be achieved if binding agreements (usually explicit contracts) are used. All individuals involved in a decision have to be part of these agreements. In other words, coordination will only result in successful cooperation if people can commit to certain actions. Although binding agreements do not have to be in explicit form, they have to force individuals through a credible threat of punishment to comply with the relevant agreements.[1]

However, in many cases people face situations where contractual agreements with others are difficult or even impossible and credible threats of punishment for deviations from agreements (or credible rewards for compliance) are not feasible. Experimental studies have shown that coordination and cooperation is nevertheless possible. Social preferences, in particular trust and reciprocity, but also other considerations of fairness and equality may ensure that people cooperate with each other. A variety of theoretical (see, e.g. Rabin, 1993; Levine, 1998; Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006 and Cox et al., 2007) and experimental work (Fehr et al., 1998; Fehr and Gächter, 1998; Charness and Rabin, 2002; Falk et al., 2008 and Dohmen et al., 2009) has underlined the importance of social preferences. People not only reciprocate, they also trust in the reciprocity of others (see, e.g. Berg et al., 1995; McCabe et al., 1998 and

[1]See also Axelrod (1984), who explains how selfish individuals can succeed in cooperation, even without explicit contracts.

Strassmair, 2009). In this way, people can succeed in cooperation when commitment devices are missing.[2]

This thesis investigates by means of four experimental studies how social preferences, in particular trust and reciprocity, foster coordination of economic decision makers in different contexts. The studies were conducted and written during my doctoral studies at the University of Konstanz between February 2007 and December 2011.

The first two chapters originate from joint research with Urs Fischbacher. We investigate the role of trust and reciprocity in determining coordination in sequential political decision making. In the experiments, a committee votes sequentially on different projects, of which each project can be accepted or rejected. In a first step, it is shown that trust and reciprocity enable members of a committee to coordinate their voting behavior. Committee members succeeded in "log-rolling" although formal commitment devices and communication were missing.

In the first study, *Reciprocity and Resistance to Comprehensive Reform*, we show that with a transparent voting procedure, which allows for observing individual voting behavior, some legislators strategically impede comprehensive reforms. After comprehensive reform failure, these legislators engage in log-rolling activities. The log-rolling coalition passes only a subset of proposals preferred by its members. at the expense of others. By this means, the study also illustrates that successful cooperation of some (through trust and reciprocity) does necessarily result in outcomes preferred by all.

The second study, *Agenda Setting and Reciprocal Vote Trading*, illustrates an additional consequence of trust and reciprocity in sequential voting on different projects. Without trust and reciprocity among committee members there is no reason for a strategic manipulation of the agenda by the chairman of the committee. However, with reciprocal committee members, vote trading based on trust and reciprocity allows the agenda setter to strategically manipulate the agenda. The results of both studies call

[2]Nowak (2006) provides a broader discussion of when and how cooperation can evolve. He names five reasons for the evolution of cooperation: Kin-selection (see also Hamilton, 1964), direct reciprocity (see also Trivers, 1971), indirect reciprocity (Nowak und Sigmund, 1998), network reciprocity and und group-selection.

for the inclusion of additional reciprocal motives in theoretical models of political economy.[3]

The third chapter of this thesis, *Competition Within Firms*, originates from a joint research paper with Lisa Bruttel. It deals with the coordination of various subsidiaries of the same parent firm. The coordination problem in this case is the following: subsidiaries compete with each other in the same market, but may be better off if they collude. The parent firm, whose goal is to maximize joint profits, is required to coordinate its subsidiaries by setting appropriate implicit incentives. In the experiment, we examine how a parent firm can achieve successful coordination of its subsidiaries by redistributing joint profits ex-post. It can be shown that an appropriate choice of ex-post redistribution improves the chances for a successful coordination of subsidiaries significantly. The results not only have implications for the design of implicit incentive systems within firms but also contribute to the discussion regarding the evaluation of the "single entity" status in the field of competition law.[4] The results illustrate that implicit incentives are crucial for whether or not a “single entity” status can be meaningfully applied to members of the same conglomerate.

The fourth chapter of this thesis, *Heterogeneous Reactions to Heterogeneity in Returns from Public Goods*, originates from joint research with Urs Fischbacher and Sabrina Teyssier. It deals with coordination problems of the society when providing a public good. Experimental economic research has shown that people are willing to contribute to public goods, although the rational "homo economicus" would abstain from any contribution (see e.g. Fehr and Gächter, 2000). Different explanations for such behavior have been put forward: pure altruism, i.e. the wish to increase others’ utility in general, "warm glow" (see, e.g. Andreoni, 1990), i.e. the joy of the act of giving, reciprocity and inequality aversion (see for example Rabin, 1993; Levine, 1998; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006 and Cox et al., 2007). Fischbacher et al. (2001) have shown in a laboratory experiment that many people are conditionally co-operative, i.e. they contribute something to public goods if others do so as well. This

[3] Hahn and Mühe (2009) and Hahn (2009) provide interesting examples of how this can be done.

[4] For a discussion of the “single entity” status in the US see e.g. Williamson (2009), a discussion for Germany is found in Koch (2007).

behavior can be explained by both inequality aversion and reciprocity but not by pure altruism or warm glow.

Both in the experiment by Fischbacher et al. (2001) and in a variety of other experiments on the provision of public goods it was assumed that all individuals in society will benefit in the same way from the public good. The study in Chapter 4 focuses on the question of how inequality in the benefits from public goods affects unconditional and conditional cooperation. It can be shown that unconditional contributions decrease when heterogeneity in returns is introduced. Thus, on the one hand, social preferences lead to positive contributions, on the other, social preferences, in particular the avoidance of envy, may also cause people to reduce their contributions. Interestingly, heterogeneity in returns from public goods had only a minor effect on conditional contributions of participants. This suggests that heterogeneity in returns from the public good affect in particular the expectations about the contribution behavior of the other members and do not generally reduce the willingness to contribute. Further, we observed that reactions to heterogeneity were heterogeneous but systematic. A substantial part of heterogeneity in reactions can be explained by inequity aversion which accounts for different reference groups subjects compare to.

Zusammenfassung

Menschen haben unterschiedliche Präferenzen. Ergeben sich aus diesen Präferenzen Handlungskonflikte, so stehen Menschen vor der Herausforderung, ihre Handlungen zu koordinieren, um erfolgreich zu kooperieren. Die Problematik von Koordination und Kooperation spiegelt sich in vielen Bereichen wider: In politischen Abstimmungen müssen sich Akteure mit unterschiedlichen Präferenzen auf gemeinsame Maßnahmen einigen. In Unternehmen müssen einzelne Abteilungen oder ganze Unternehmensteile sich gegenseitig koordinieren, um miteinander erfolgreich kooperieren zu können. Bei gesellschaftlichen Problemen, wie die Bereitstellung öffentlicher Güter, steht eine Vielzahl von Beteiligten vor der Aufgabe eine kooperative Lösung durch koordiniertes Handeln zu finden.

In der klassischen ökonomischen Theorie wird davon ausgegangen, dass erfolgreiche Koordination und Kooperation bei Akteuren mit unterschiedlichen Präferenzen nur gelingen kann, wenn Abmachungen (in der Regel explizite Verträge) alle in einer Entscheidung involvierten Akteure an die getroffenen Vereinbarungen binden. Auf diese Weise werden klare Anreize für das Verhalten dieser Akteure gesetzt. Zwar müssen bindende Abmachungen nicht unbedingt in expliziter Form als Verträge bestehen, jedoch sollten sie die Akteure durch glaubhafte Sanktionen dazu zwingen, die entsprechenden Vereinbarungen einzuhalten.[5]

In vielen Fällen stehen Menschen jedoch vor Entscheidungen, bei denen vertragliche Abmachungen mit anderen nur schwer oder gar nicht möglich sind und auch glaubhafte Sanktionen bei Abweichungen von Abmachungen (bzw. glaubhafte Belohnungen bei Einhaltung) nicht möglich sind. Experimentelle Studien haben gezeigt, dass Koordination und Kooperation dennoch möglich ist: Soziale Präferenzen, insbesondere Vertrauen und Reziprozität, aber auch weitere Fairnessüberlegungen wie z.B. Ungleichheitsaversion können dafür sorgen, dass Menschen miteinander kooperieren. Eine Vielzahl theoretischer (siehe z.B. Rabin, 1993; Levine, 1998; Bolton und Ockenfels, 2000; Charness und Rabin, 2002; Dufwenberg und Kirchsteiger, 2004; Falk und Fischbacher, 2006 und Cox et al., 2007) und experimenteller Arbeiten (Fehr et al., 1998; Fehr und Gächter,1998; Charness und Rabin, 2002; Falk et al., 2008; Dohmen et al., 2009) hat die Bedeutung von sozialen Präferenzen, Vertrauen und Reziprozität

[5]Siehe auch Axelrod (1984), der zeigt, wie auch unter egoistischen Individuen Kooperation ohne explizite Verträge zustande kommen kann.

hervorgehoben. Menschen belohnen und bestrafen nicht nur, sie vertrauen in vielen Fällen auch in die Reziprozität anderer (siehe z.B. Berg et al., 1995; McCabe et al., 1998 und Strassmair, 2009). Auf diese Weise kann Kooperation und Koordination auch ohne bindende Abmachungen gelingen.[6]

Die vorliegende Arbeit zeigt anhand von vier experimentellen Studien, wie soziale Präferenzen, Vertrauen und Reziprozität die Koordination von ökonomischen Akteuren beeinflussen können. Alle vier Studien entstanden während meines Promotionsstudiums an der Universität Konstanz zwischen Februar 2007 und Dezember 2011.

Die ersten beiden Kapitel entstammen gemeinsamen Forschungsarbeiten mit Urs Fischbacher. Sie befassen sich mit der Rolle von Vertrauen und Reziprozität bei Entscheidungen in politischen Abstimmungsprozessen. Wir zeigen in diesen Arbeiten auf, dass Vertrauen und Reziprozität in sequentiellen Abstimmungen über unterschiedliche Projekte ein koordiniertes Abstimmungsverhalten der Kommitteemitglieder ermöglichen, selbst wenn formelle Abmachungen zwischen den Mitgliedern unmöglich sind. Diese Koordination ermöglicht einem Teil der Mitglieder, einen politischen „Kuhhandel", durch den die am Handel Beteiligten ihre bevorzugten Projekte auf Kosten Dritter umsetzen können.

In der ersten Studie, *Reciprocity and Resistance to Comprehensive Reform*, kann zudem gezeigt werden, dass in einem Abstimmungsverfahren, in dem das Wahlverhalten jedes Komitee-Mitgliedes nachvollziehbar ist, der Kuhhandel sogar so erfolgreich gelingt, dass selbst große Reformprojekte, die allen Komitee-Mitgliedern in gleichem Maße nutzen, strategisch zum Scheitern gebracht werden, um anschließend im politischen Stimmentausch nur die durch eine kleinere Koalition bevorzugten Projekte zu Lasten Dritter durchzusetzen. Die Studie veranschaulicht dabei, dass eine durch Vertrauen und Reziprozität entstandene Kooperation von einigen Akteuren nicht immer zum Besten aller Akteure sein muss.

Die zweite Studie, *Agenda Setting and Reciprocal Vote Trading*, verdeutlicht eine weitere Konsequenz von Vertrauen und Reziprozität in sequentiellen Abstimmungen

[6] Einen guten Ausgangspunkt für eine weiterführende Diskussion, wann und warum Kooperation gelingen kann, bietet die Arbeit von Nowak (2006). Er benennt insgesamt fünf Gründe, welche die Evolution von Kooperation erklären können: Kin-Selektion (Sippenselektion, siehe auch Hamilton, 1964), direkte Reziprozität (siehe auch Trivers, 1971), indirekte Reziprozität (Nowak und Sigmund, 1998), Netzwerk-Reziprozität und Gruppen-Selektion.

über unterschiedliche Projekte. Während ohne Vertrauen und Reziprozität der Mitglieder des Komitees in der beobachteten Situation keinerlei Grund für eine strategische Anpassung der Tagesordnung seitens des Vorsitzenden des Komitees besteht, sorgt der auf Vertrauen und Reziprozität basierende Stimmentausch dafür, dass der Vorsitzende (der Agenda–Setter) die Tagesordnung zu seinen Gunsten manipulieren kann. Die Ergebnisse beider Studien verdeutlichen die Notwendigkeit, Reziprozitäts-annahmen in Modellen der politökonomischen Theorie stärker zu berücksichtigen.[7]

Das dritte Kapitel der vorliegenden Arbeit, *Competition Within Firms*, entstammt einer gemeinsamen Forschungsarbeit mit Lisa Bruttel. Es beschäftigt sich mit der Koordination unterschiedlicher Tochterunternehmen derselben Muttergesellschaft. Das Koordinationsproblem besteht in diesem Fall darin, dass die Tochtergesellschaften in Konkurrenz zueinander stehen, sich aber durch Kollusion besser stellen können. Die Muttergesellschaft, deren Ziel die Maximierung des gemeinsamen Gewinns ist, ist gefordert, mit den ihr zur Verfügung stehenden Mitteln geeignete Anreize zu setzen, um dieses Ziel zu erreichen. Untersucht wird in einem Experiment, inwiefern Mutter-gesellschaften durch eine ex post Umverteilung des erwirtschafteten Gewinns der Tochtergesellschaften, eine erfolgreiche Koordination ihrer Tochtergesellschaften erzielen können. Es kann gezeigt werden, dass durch die richtige Wahl nachträglicher Umverteilung die Chancen für eine erfolgreiche Koordination der Tochterunternehmen deutlich verbessert werden. Die Ergebnisse haben dabei nicht nur Implikationen für die Ausgestaltung von Anreizsystemen innerhalb von Unternehmen, sondern tragen auch zur Diskussion hinsichtlich der Bewertung der „wirtschaftlichen Einheit“[8] im Bereich des Wettbewerbsrechts bei. Sie verdeutlichen, dass die wirtschaftliche Einheit eines Konzerns auch von impliziten Anreizen innerhalb des Unternehmens abhängt.

Das vierte Kapitel der vorliegenden Arbeit, *Heterogeneous Reactions to Heterogeneity in Returns from Public Goods* entstammt einer gemeinsamen Forschungsarbeit mit Urs Fischbacher und Sabrina Teyssier. Es befasst sich mit der Koordinationsproblematik der Gesellschaft bei der Bereitstellung öffentlicher Güter. Die experimentelle Wirtschaftsforschung hat gezeigt, dass Menschen bereit dazu sind, auch in Situationen Beiträge zu öffentlichen Gütern zu leisten, in denen positive Beiträge

[7] Beispiele, wie dies gelingen kann, finden sich in Hahn and Mühe (2009) sowie Hahn (2009).

[8] Koch (2007) diskutiert den Begriff der „wirtschaftlichen Einheit“ für Deutschland, Williamson (2009) setzt sich mit dem Begriff der „single entity” in den USA auseinander.

durch den rationalen „homo oeconomicus“ nicht getätigt würden (siehe z.B. Fehr und Gächter, 2000). Unterschiedliche soziale Präferenzen wurden als Erklärungen herangezogen. Neben „purem“ Altruismus, also dem Wunsch, anderen ein höheres Nutzenniveau zu ermöglichen, kamen „warm glow“ (siehe z.B. Andreoni, 1990), also die Freude des Aktes etwas zu geben, Reziprozität und Ungleichheitsaversion (siehe z.B. Rabin, 1993; Levine, 1998; Fehr und Schmidt, 1999; Bolton und Ockenfels, 2000; Charness und Rabin, 2002; Dufwenberg und Kirchsteiger, 2004; Falk und Fischbacher, 2006; Cox et al., 2007) als Erklärungsansätze in Betracht. Fischbacher et al. (2001) haben in einem Laborexperiment gezeigt, dass viele Menschen bedingt kooperativ sind, d.h. sie tragen insbesondere dann etwas zu öffentlichen Gütern bei, wenn andere dies auch tun. Dieses Verhalten ist sowohl durch Ungleichheitsaversion als auch durch Reziprozität erklärbar, jedoch nicht durch puren Altruismus oder „warm glow“.

Sowohl im Experiment von Fischbacher et al. (2001) als auch in einer Vielzahl anderer Experimente zur Bereitstellung öffentlichen Gütern wurde davon ausgegangen, dass alle Individuen in der Gesellschaft in gleichem Umfang von der Bereitstellung des öffentlichen Gutes profitieren. Die Studie in Kapitel 4 geht der Frage nach, inwiefern unbedingte, aber auch bedingte Kooperation durch Ungleichheit im Nutzen aus dem öffentlichen Gut beeinflusst werden. Es kann gezeigt werden, dass unbedingte Beiträge durch die Einführung von Ungleichheit zurück gehen. Das bedeutet, soziale Präferenzen können einerseits dafür sorgen, dass Menschen etwas zur Bereitstellung von öffentlichen Gütern beitragen, andererseits kann insbesondere die Vermeidung von Neid auch dazu führen, dass Menschen bei ungleichem Nutzen aus dem öffentlichen Gut eine geringere Kooperationsbereitschaft zeigen. Interessanterweise hatte die Heterogenität im Nutzen aus dem öffentlichen Gut jedoch nur eine geringfügige Wirkung auf die bedingten Beiträge der Teilnehmer. Dies deutet darauf hin, dass die Unterschiede im Nutzen aus dem öffentlichen Gut insbesondere die Erwartungen über das Beitragsverhalten der jeweils anderen Gesellschaftsmitglieder negativ beeinflusst, nicht aber die grundsätzliche Beitragsbereitschaft. Die Reaktionen auf Ungleichheit im Nutzen aus dem öffentlichen Gut sind heterogen aber systematisch. Ein Großteil der unterschiedlichen Reaktionen lässt sich durch ein Modell der Ungleichheitsaversion erklären, das unterschiedliche Referenzgruppen für unterschiedliche Personen zulässt.

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1 Reciprocity and Resistance to Comprehensive Reform

1.1 Introduction

Comprehensive reforms often fail or become piecemeal during the preparatory phase of legislation. Resistance to comprehensive reforms has been explained by uncertainty about outcomes of reforms (see e.g. Fernandez and Rodrik, 1991 and Cason and Mui, 2003), the existence of veto players (see e.g. Tsebelis, 2002 and Kagel et al., 2010), interest groups or wars of attrition (Alesina and Drazen, 1991 and Saint-Paul, 2002) and psychological constraints such as confirmatory bias (Samuelson and Zeckhauser, 1988 and Rabin and Schrag, 1999; for an excellent survey see also Heinemann, 2004). We provide a new rationale for resistance to reform: trust and reciprocity among legislators, which allow for vote trading without commitment devices.

Facing a comprehensive reform, legislators in favor of a subset of individual bills included in the comprehensive reform will be willing to impede the comprehensive reform if they expect their favorable subset to be implemented by a vote trade after comprehensive reform failure. For selfish legislators, such a vote trade requires formal commitment devices. Instead, if legislators trust and reciprocate commitment devices for vote trading will not be necessary to trade votes.

A vast amount of theoretical (see e.g. Rabin, 1993; Levine, 1998; Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006 and Cox et al., 2007) and experimental work (e.g. Fehr et al., 1998; Fehr and Gächter,1998; Charness and Rabin, 2002; Falk et al., 2008; Dohmen et al., 2009) has underlined the importance of reciprocity in human interactions. People reward kind actions and punish unkind actions. Humans not only reciprocate, but also trust in reciprocal behavior by others (see e.g. Berg et al., 1995; McCabe et al., 1998 and Strassmair, 2009).

The aim of this paper is first, to investigate under which circumstances trust and reciprocity among legislators facilitates vote trading and second, to study how vote trading based on trust and reciprocity affects the willingness to engage in resistance to efficient comprehensive reforms. To do so, we conduct a laboratory experiment in which a committee votes on an efficient comprehensive reform including three independent bills. The approval of each individual bill is efficient but only preferred by a minority.

The comprehensive reform requires unanimity.[9] If the comprehensive reform fails, the committee votes in a sequential bill-by-bill voting procedure on each independent bill under simple majority rule. Because in the experiment each bill is only preferred by a minority standard economic theory predicts no bill to be passed in the sequential bill-by-bill voting procedure. Consequently, selfish legislators will vote for the comprehensive reform. However, if legislators trust in others' reciprocity, legislators will expect vote trading on individual bills after reform failure to be possible. In turn, resistance to comprehensive reform may occur.

The importance of trust and reciprocity for vote trading depends on whether legislators can identify their potential vote trading partners in the sequential voting procedure. If the voting procedure provides information on each legislators' votes (open ballot), supporters of own bills will be identifiable. If, instead, information on individual voting behavior is missing (secret ballot), it will be impossible to direct reward to supporters. We study both institutional settings with and without the possibility of voting for a comprehensive reform.

We find that transparency in the sequential voting procedure is crucial for vote trading based on trust and reciprocity. In the open ballot treatments we frequently observe vote trades in which two legislators passed their preferred bills at the expense of the third legislator. In the secret ballot treatments, vote trading based on trust and reciprocity turned out to be difficult. Vote trading possibilities in the open ballot treatment were sufficient to cause resistance to the efficient comprehensive reform. Legislators who expected vote trading to be possible (and beneficial to them) strategically impeded comprehensive reforms when the sequential voting procedure was transparent. Thus, an imperfect device for vote trading, namely a transparent sequential bill-by-bill voting procedure, was sufficient to cause resistance to comprehensive reform. Instead, secret voting procedures (i.e. individual voting behavior is not observable) reduce legislators' trust and, in turn, resistance to comprehensive reform.

There has been an extensive discussion on the welfare effects of vote trading based on the seminal work by Tullock (1959). Buchanan and Tullock (1962) argue that

[9] Many nternational organizations such as the North Atlantic Treaty Organization (NATO), World Trade Organization (WTO) and MERCOSUR make use of unanimity rule and other supermajority rules. Also the European Union applies qualified majority rule for particularly sensitive issues (Maggi and Morelli, 2006).

vote trading allows for the representation of degrees of intensities of preferences: Legislators can trade votes on issues where their preferences are relatively weak against votes on issues where their preferences are relatively strong. Thus vote trading is mutually beneficial to those involved in the trade. Brams and Riker (1973) point out that although each vote trade may be beneficial to traders, there is a risk that third parties not involved in the trade bear a substantial part of the cost involved in the respective trade. When the cost for third parties exceeds the benefits from trades, subsequent vote trades may lead to a situation under which vote trades eventually make everyone worse off (the paradox of vote trading). McKelvey and Ordeshook (1980) were the first to test Brams and Riker's paradox of vote trading experimentally. They observe the vote trading patterns predicted by Brams and Riker in a series of experiments with three and five person committees and a face to face communication environment with formal commitment devices. However, in some of their experiments, committee members were allowed to cancel vote trades when all members involved in trading agreed. Consequently, Pareto-dominated outcomes were eventually replaced by canceling all trades under unanimous agreement or by a majority coalition implementing its preferred outcome. In contrast to the study by McKelvey and Ordeshook (1980) our subjects could not communicate face to face, nor in any other form. Further, our subjects were completely aware of the impact of their choices on others' payoffs whereas McKelvey and Ordeshook (1980) assured that their participants were not informed about the exact amounts earned by others in order to exclude non-selfish motivations for vote trading.

In contrast to the literature mentioned above, our contribution focuses on the impact of vote trading without commitment devices on resistance to comprehensive reforms. We demonstrate that legislators can succeed in vote trading even when formal commitment devices and communication possibilities are missing. Legislators trust in their counterparts' reciprocity and vote for bills contrary to their (induced) preferences. Transparency of the decision making procedure provides accountability and thus increases trust in others' reciprocity. In turn, when the alternative voting procedure was transparent, vote trading occurred and resistance to the comprehensive reform was profitable for those involved in trading.

1.2 Experimental design

In the experiment subjects form three-member committees and decide on an efficient comprehensive reform. The comprehensive reform includes three individual bills. Each bill is efficient, but preferred by only one member of the committee. A committee member's preferred bill yields 6 additional points for herself, whereas the other two members of the committee incur a loss of two points each. Thus, if an individual bill is passed, gross payoff will increase by two points. Because only one participant of the group gains from each individual bill, each individual bill is disadvantageous to a majority of the group. Table 1.1 shows how each bill affects the participants' payoffs. We inform all participants about their own and their counterparts' induced preferences.

When the comprehensive reform is approved, all bills are implemented, i.e. each participant receives two additional points. Passing the comprehensive reform requires unanimity. This allows us to elicit preferences against the comprehensive reform in an unambiguous way (voting the comprehensive reform down will always lead to comprehensive reform failure). We do not display information on who vetoes the comprehensive reform in order to minimize fear of revenge as a reason to accept the comprehensive reform.

If at least one member vetoes the comprehensive reform, the committee will decide sequentially on the three individual bills under simple majority rule. We decided for a sequential procedure because it allows us to study the implications of reciprocal behavior among committee members on vote trading in a controlled environment. In the sequential bill-by-bill procedure, each bill can be passed or failed. First, all committee members simultaneously cast their votes on the first bill. Then, we inform the committee about the outcome of the vote. Second, each member casts her vote for the second bill. The second vote is displayed and the group decides on the third bill. Eventually, the outcome of the third vote and the resulting payoff changes are displayed.

	Bill A	**Bill B**	**Bill C**
Member A	+6	-2	-2
Member B	-2	+6	-2
Member C	-2	-2	+6

Table 1.1: Bills and resulting payoff changes

To test whether trust in others' reciprocity leads to resistance to comprehensive reform, we vary the bill-by-bill voting procedure with respect to the information on individual voting behavior. We suppose transparency provides accountability and therefore increases trust in others' reciprocity. The committee either votes in a transparent procedure, in which individual voting behavior is observable (OpenBallot), or in a secret ballot, where only the outcome of each vote is displayed (SecretBallot).

Before committee members decide on the comprehensive reform, we inform them about the alternative voting procedure and about the (random) order of bills in the alternative procedure. This allows us to infer committee members' reaction to information about potential gainers and losers of each procedure. As further controls, we run additional treatments without the possibility to agree on a comprehensive reform. In these treatments the committee decides on the three bills either in an open (OpenNoCR) or secret (SecretNoCR) bill-by-bill voting procedure with simple majority rule. We can thus investigate the effect of transparency on trust in others' reciprocity in a controlled environment and test whether the failure of a comprehensive reform affects trust and reciprocity among committee members.

To control for learning effects and changes of voting behavior over time, participants vote on the three bills in 12 periods which are all payoff relevant. In each period, each participant is randomly sorted into a group of three participants. The random matching procedure assures that participants cannot infer any information on their current counterparts' voting behavior from past periods. Thus we exclude individual reputation building across periods but allow for learning effects. In sessions with at least 24 subjects we divided subjects into two separate matching groups. Table 1.2 summarizes the number of subjects, sessions and treatments in detail. None of the subjects participated in more than one session.

Each subject sat at a randomly assigned and separated PC terminal and was given a copy of instructions.[10] A set of control questions ensured the understanding of the game. If any participant repeatedly failed to answer correctly, the experimenter provided an oral explanation. No form of communication was allowed during the experiment. We conducted all sessions at the LakeLab (University of Konstanz,

[10] A copy of translated instructions can be found in the appendix.

Treatment	# Subjects	# Sessions	# Matching Groups
Comprehensive reform and open ballot (OpenBallotCR)	63	3	5
Comprehensive reform and secret ballot (SecretBallotCR)	51	2	4
No Comprehensive reform and open ballot (OpenNoCR)	51	2	3
No Comprehensive reform and secret ballot (SecretNoCR)	54	2	3

Table 1.2: Treatments, sessions and matching-groups

Germany). The data was collected over 9 sessions, with 219 participants in total. Participants received a show-up fee of 2 euros ($2.40 at that time).

The experiment took about one hour and 15 minutes, average income (including the show-up fee) was about 14.40 euros ($17.30). The experiment was programmed and conducted using z-Tree (Fischbacher, 2007). We recruited participants using the online recruiting system ORSEE (Greiner, 2004). Participants were part of the LakeLab subject pool, including undergraduate and graduate students of all fields of study.

1.3 Behavioral predictions

1.3.1 The sequential bill-by-bill voting procedure

Committees comprised of selfish members will vote down all bills in the sequential bill-by-bill voting procedure if commitment devices for vote trading are missing. This holds irrespective of whether the voting procedure is transparent or not. Instead, if legislators expect their counterparts to reward support (with a sufficiently high probability[11]) they will court for reward by voting for bills preceding their own bill on the sequential voting agenda. Assuming first, that legislators are reciprocal and do not support bills preferred by legislators who voted down their own preferred bill and second, that legislators do not treat counterparts about whom they have the same behavioral information differently, the approval of the first bill is weakly more likely than the approval of second and third bill. Assuming additionally that the legislator, who prefers the last bill on the agenda, does not vote more frequently for the first bill than the legislator

[11] For a risk neutral subject, the probability of reward p_r is sufficiently high if $p_r > 1/3$ because voting for another's bill costs 2 points whereas reward yields 6 additional points.

preferring the second bill, it follows that the approval of the second bill is weakly more likely than the approval of the third bill. [12]

1.3.2 The comprehensive reform

Standard economic theory predicts that the committee will approve the efficient comprehensive reform because committees comprised of selfish members will vote down all bills in the alternative sequential bill-by-bill procedure. For resistance to reform to be caused by something different than decision errors, at least one legislator needs to expect another legislator to vote for her bill in the sequential bill-by-bill voting procedure. If legislators indeed expect reciprocal behavior by their counterparts with a sufficiently high probability (such that counterparts are willing to vote for bills preceding their own bill on the alternative sequential bill-by-bill agenda), it will be worthwhile to veto the comprehensive reform. As explained above, the approval for the first bill is weakly more likely than the approval of the second and third bill on the alternative agenda. In turn the probability of a veto is (weakly) higher for legislators preferring the first bill than for legislators preferring the second or third bill on the agenda.

1.3.3 Transparency and reform failure

Standard economic theory predicts that the committee will approve the efficient comprehensive reform irrespective of transparency in the alternative bill by bill voting procedure. Similarly, if voting for another legislator's bill is expected to result from decision errors, it will occur irrespective of transparency and so does resistance to reform.

If voting for another legislator's bill instead is expected to result from other legislators courting for a reward and the probability of reward depends on transparency, resistance to reform will depend on transparency as well. We consider it as plausible to assume that transparency provides accountability and thus subjects expect a higher probability of reward in the OpenBallot treatments. Consequently, we expect legislators to trust more in their counterparts, i.e. we expect votes for bills preceding one's preferred bill on the alternative bill-by-bill agenda to occur more frequently in the OpenBallot treatments, in which the bill-by-bill voting procedure is

[12] Proofs of these predictions can be found in section 2.3. Note also that due to the random matching procedure, there is no incentive for individual reputation building across periods, which might induce any additional motives for supporting monetarily unfavorable reforms.

transparent. Resistance to the comprehensive reform is thus also more likely to occur in OpenBallotCR.

1.4 Results

To understand why some members may veto the comprehensive reform it is necessary to shed light on the behavioral pattern in the alternative bill-by-bill voting procedures. Therefore, we will first discuss the effects of transparency on voting behavior in the sequential bill-by-bill voting procedure and second present the results with respect to resistance to comprehensive reform.

1.4.1 Trust and reciprocity in the sequential bill-by-bill voting procedure

In the sequential bill-by-bill voting procedure all bills will fail if selfishness of committee members is common knowledge. However, if committee members trust in their counterparts' reciprocity, they will vote for bills detrimental to their induced preferences. Committee members in our experiment frequently vote for monetarily unfavorable bills. Figure 1.1 illustrates acceptance rates of monetarily unfavorable reforms in the bill-by-bill voting procedure for OpenBallotCR and SecretBallotCR, after the failure of the comprehensive reform, as well as for treatments OpenNoCR and SecretNoCR, in which no comprehensive reform was available. Each column represents the share of members voting for a reform bill which is monetarily disadvantageous to them. In the open ballot treatments (OpenBallotCR and OpenNoCR), the earlier a bill is voted on in the bill-by-bill procedure, the higher the probability of its approval. For the first bill two committee members have an incentive to court for positive reciprocity toward members preferring the bills second and third on the agenda. When voting on the second bill, the member in favor of the first bill has a strong monetary incentive to reject the second bill whereas the member preferring the third bill still has an incentive to vote for the second bill as long as she expects reward by the member preferring the second bill. Consequently, the actual approval rate for the second bill is clearly lower than for the first. The same logic applies for the third bill, which is clearly less frequently accepted than the first or second bill.

We observe 67 percent of members voting for the first bill in OpenNoCR although they incur a loss from the approval of this bill, and 40 percent for the second bill. In SecretNoCR only 35 percent vote for the first bill and 22 percent for the second. The regressions in Table 1.3 confirm that transparency, i.e. information on individual voting

behavior, increases the individual acceptance of the first and second reforms significantly, whereas transparency does not affect the acceptance of the third bill by members preferring earlier bills. We summarize this finding in Result 1.1.

Result 1.1 - Transparency increases the probability of acceptance of early bills (first and second) in the sequential bill-by-bill voting procedure.

In order to understand whether transparency increases reward, trust in others' reciprocity, or both, the further analysis focuses on reward behavior and trust in others' reciprocity (i.e. expected reward) across treatments. In the open ballot treatments reward refers to a situation in which a committee member rewards her helping counterpart by accepting a subsequent bill favored by the helping counterpart. In the secret ballot treatments, committee members cannot identify supporters. If one's preferred bill is passed, voting for an unfavorable subsequent bill will refer to *reward* with respect to the group. In order to make reward in the open and closed ballot

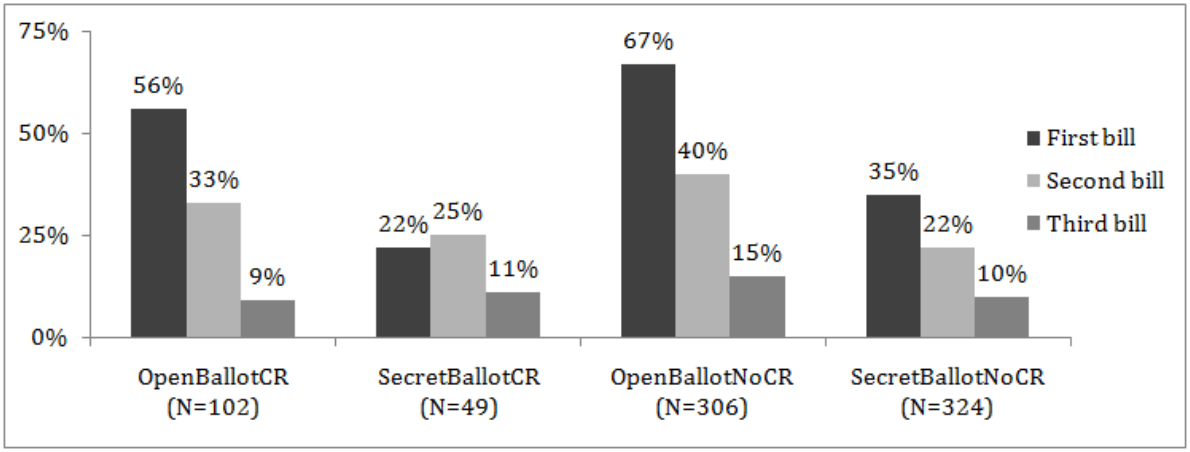


Figure 1.1: Share of members accepting monetarily unfavorable bills (when CR failed)

Probit (robust std. errors)	**CR Treatments - after failure of CR**		**No CR Treatments**	
Dep.Var.: Acceptance of…	…first bill	…second bill	…first bill	…second bill
Constant	-0.77*** (0.135)	-0.68*** (0.097)	-0.39*** (0.140)	-0.78*** (0.232)
Transparency	0.93 *** (0.165)	0.25* (0.135)	0.84*** (0.147)	0.53** (0.269)
Pseudo R^2	0.09	0.01	0.08	0.03
Number of Observ.	510	510	840	840
Std.Err. adjusted for # clusters	9	9	6	6

Table 1.3: Transparency and the acceptance of monetarily unfavorable reforms
*p-val.<.10, ** p-val.<.05, *** p-val.<.01

treatments comparable, we focus on the share of committee members who accept at least one subsequent bill, provided their preferred bill is approved. With respect to trust in others' reciprocity, we draw conclusions from the acceptances of preceding but monetarily unfavorable bills. We understand trust as a behavior based on an optimistic belief, namely based on the belief of reward by one's counterpart. We use the probability of the acceptance of at least one unfavorable preceding bill as a measure for trust. We are aware that efficiency concerns might also affect this probability because efficiency seekers will always vote for all bills. However, our data suggest efficiency concerns to be low. Only 5 percent of subjects vote for an unfavorable bill although their preferred bill failed.[13] Further efficiency concerns should not vary significantly across treatments and thus cannot explain treatments differences in the probability of the acceptance of at least one unfavorable preceding bill.

Table 1.4 summarizes the shares of members rewarding (accepting at least one subsequent unfavorable bill provided the approval of one's own bill) and trusting in others' reciprocity (accepting at least one preceding bill) for the four treatments. The share of members rewarding their counterparts is slightly higher in transparent than in secretive procedures. 25 percent of members reward their counterparts in OpenBallotCR, 18 do so in SecretBallotCR, 30 percent do so in OpenNoCR and 24 percent in SecretNoCR. In open ballot treatments a substantially higher share of members trusts in others' reciprocity. In OpenBallotCR 66 percent of members accept preceding bills detrimental to their induced preferences, in SecretBallotCR only 37 percent do so. In OpenNoCR this share amounts to 75 percent whereas in SecretNoCR it is only 41 percent.

Treatment	Share of members accepting at least one subsequent unfavorable bill when own bill is approved	Share of members accepting at least one preceding unfavorable bills
OpenBallotCR	.25	.66
SecretBallotCR	.18	.37
OpenNoCR	.30	.75
SecretNoCR	.24	.41

Table 1.4: Share of members rewarding / trusting

[13] This share has to be considered as a lower bound for efficiency concerns, because not accepting subsequent bills after one's own bill failed can be caused by negative reciprocity too.

Probit (robust std. errors)	**Dependent Variable: Reward** (Probability of the acceptance of at least one unfavorable subsequent bill when own bill is approved)		**Dependent Variable: Trust** (Probability of the acceptance of at least one unfavorable preceding bill)	
	CR treatments (after CR failed)	NoCR treatments	CR treatments (after CR failed)	NoCR treatments
Constant	-0.92***	-0.72***	-0.34***	-0.22
	(0.100)	(0.104)	(0.095)	(0.183)
Transparency	0.23	0.21	0.76***	0.86***
	(0.145)	(0.166)	(0.151)	(0.190)
Pseudo R^2	0.005	0.005	0.06	0.08
Prob>X^2	0.113	0.201	0.00	0.00
Number of Observ.	298	516	510	840
Std.Err. adjusted for # clusters	9	6	9	6

Table 1.5: Transparency, reward and trust in others' reciprocity

*p-val.<.10, ** p-val.<.05, *** p-val.<.01

Using Probit regressions in Table 1.5 we find no significant effects of transparency on reward. However, transparency significantly increases the probability of acceptance of preceding bills (trust). Both, members in favor of the second and third bill accept preceding bills more frequently provided that individual voting behavior is observable. We summarize our finding in Result 1.2.

Result 1.2 - Transparency increases trust in others' reciprocity, but does not increase actual reward.

1.4.2 Resistance to reform

We are now ready to turn to our main research question, namely whether trust in others' reciprocity is sufficient to induce resistance to comprehensive reform. Results 1.1 makes clear that vetoing a comprehensive reform is most attractive for members preferring the first bill on the alternative agenda, when the alternative bill-by-bill procedure is an open ballot. We observe committee members preferring the first bill on the alternative agenda vetoing the comprehensive reform in 47 percent of cases in OpenBallotCR and 25 percent in SecretBallotCR. Members in favor of later bills on the alternative agenda do so in about 16 percent of cases in OpenBallotCR and 20 percent in SecretBallotCR.[14] The regression results in Table 1.6 make clear that members preferring the first bill on the alternative agenda expect profitable vote trades to be

[14] The latter difference is not statistically significant (Probit with robust std. errors, p-val.>.10).

possible when the sequential alternative procedure is transparent and thus impede the comprehensive reform. Members preferring later bills mainly opt for the comprehensive reform and are not affected by the transparency of the alternative voting procedure. We summarize this finding in Result 1.3.

Results 1.3 – Legislators impede comprehensive reforms when they anticipate profitable vote trading possibilities on the basis of trust and reciprocity.

Using actual probabilities, the expected payoff of blocking the package for a member preferring the first bill amounts to 7.5 points in OpenBallotCR, whereas the acceptance of the package yields only 6 points. Thus, vetoing behavior by members preferring the first bill is profitable in the open ballot procedure (OpenBallotCR). The expected payoff of resistance to comprehensive reform in the secret ballot treatment (SecretBallotCR) is 5.96, which is slightly lower than the payoff of 6 points when the comprehensive reform is accepted. Thus, impeding the comprehensive reform in the secret ballot treatment does not pay off for members preferring the first bill. The same holds for members preferring later bills on the agenda. The expected payoffs from vetoing the package for members preferring the second bill (third bill) are 5.37 (2.27) in OpenBallotCR and 4.89 (3.11) in SecretBallotCR, i.e. these members have no monetary incentive to impede the comprehensive reform.

Why do members preferring the first bill succeed in vote trading after they block the comprehensive reform? The reason is that if the comprehensive reform fails,

Probit (robust standard errors)	**Dependent Variable: Probability to vote against Comprehensive Reform**		
	Member preferring first bill	Member preferring second bill	Member preferring third bill
Constant	-0.75*** (0.104)	-0.64 (0.141)***	-1.16 ***(0.310)
Transparency	0.68 *** (0.174)	-0.03 (0.212)	-0.27 (0.377)
Pseudo R^2	0.05	0.0001	0.0099
Prob>X^2	0.00	0.87	0.49
Number of Observ.	456	456	456
Std.Err. adjusted for # clusters	9	9	9

Table 1.6: Transparency and resistance to reform
*p-val.<.10, ** p-val.<.05, *** p-val.<.01

members preferring the second bill will gain from supporting the first bill in OpenBallotCR. By supporting the first bill, a member preferring the second bill loses 2 points with certainty but receives a reward of 6 points by the member preferring the first bill with a probability of 35 percent. Additionally, voting for the first bill increases the probability that the member preferring the third bill accepts the second bill from 36 to 48 percent. We cannot infer whether the latter increase is due to positive signaling or because members preferring the third bill expect the second bill to be passed anyway and therefore have nothing to lose by voting for the second bill. Irrespective of these motives we can say that the acceptance of the first bill yields a higher expected payoff for the member preferring the second bill than voting against it.

Figure 2 indicates that members preferring the first bill on the alternative agenda change their behavior over time. In open ballot treatments resistance to reform increases from about 20 percent in early periods to about 70 percent in late periods. The regression in Table 7 confirms that members preferring the first bill on the agenda learn to block the comprehensive reform when facing a transparent voting procedure whereas resistance to reform stays at a constant level in the secret ballot treatment.

Result 1.4 - Legislators whose favorable bill is first on the alternative bill-by-bill agenda learn that resistance to comprehensive reforms pays off

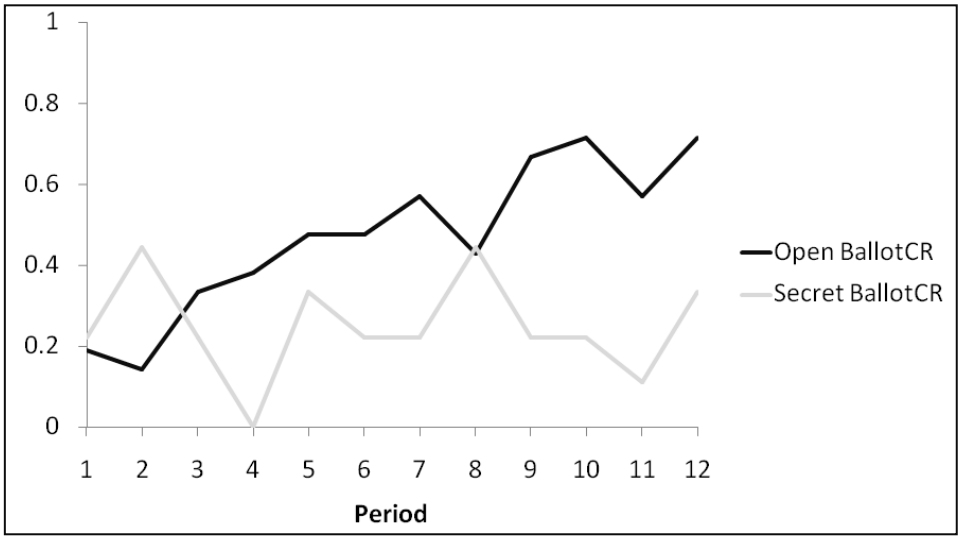


Figure 1.2: Resistance to reform by members preferring the first bill, over time (N=114)

Probit Regression (robust standard errors)	Vote against comprehensive Reform
Constant	-0.81*** (0.137)
Transparency	-0.09 (0.388)
Period	0.01 (0.008)
Transparency*Period	0.12*** (0.034)
Pseudo R^2	0.10
Number of Observ.	456
Robust Std.Err. adjusted for # clusters	9

Table 1.7: Resistance to comprehensive reform by members preferring the first bill
*p-val.<.10, ** p-val.<.05, *** p-val.<.01

Although we did not display individual voting decisions for the comprehensive reform, most participants seemed to be aware that in the open ballot treatment (OpenBallotCR) the incentive to block the comprehensive reform was strongest for the member preferring the first bill on the agenda. As Figure 1.1 already indicates, the support for the first bill by members preferring the second or third bill is significantly stronger in the NoCR treatments compared to the CR treatments after failure of the comprehensive reform.[15]

Probit (robust standard. errors)	Vote for first Bill	Vote for second Bill	Vote for third Bill
Constant	0.55*** (0.170)	-0.83*** (0.184)	-1.60*** (0.105)
Vote for CR	-0.51** (0.237)	0.66*** (0.197)	0.53*** (0.202)
Pseudo R^2	0.02	0.05	0.04
Number of Observ.	316	316	316
Robust Std.Err. adjusted for # clusters	5	5	5

Table 1.8: Discrimination after comprehensive reform failure (OpenBallotCR)
Dep. Variables: Acceptance of monetarily unfavorable bills (first, second or third bill)
*p-val.<.10, ** p-val.<.05, *** p-val.<.01

[15] Probit regressions with robust std. errors, p-values<0.06.

When the comprehensive reform fails, members in favor of the comprehensive reform vote less frequently for the first bill whereas they vote more frequently for the second and third bills (see Table 1.8). Discrimination against the member preferring the first bill does not increase over time and its quantitative effect on actual approval rates is small. Thus, discrimination does not change the incentive to block comprehensive reforms for members preferring the first bill on the agenda.

Finally, we turn to the question of whether the failure of the comprehensive reform causes a general loss in trust and reciprocity among committee members. To do so, we compare the share of members trusting/reciprocating in the treatments with the possibility of voting on a comprehensive reform with treatments in which no comprehensive reform is available. Table 4 illustrates that after the failure of comprehensive reform 25 percent of members reward others' support in OpenBallotCR and 18 percent do so in SecretBallotCR, whereas these shares amount to 31 percent in OpenNoCR and 24 percent in SecretNoCR. With respect to trust in others' reciprocity we observe 66 percent of members voting for at least one preceding bill in OpenBallotCR and 37 percent in SecretBallotCR after the comprehensive reform failed, whereas these shares amount to 75 percent in OpenNoCR and 41 percent in SecretNoCR when no comprehensive reform was available. Thus, the impact of comprehensive reform failure on reciprocity is weak and reduces trust in open ballot treatments only slightly (Table 1.9).

Probit (robust standard errors)	**Dependent Variable: Reward**		**Dependent Variable: Trust**	
	Open Ballot	Secret Ballot	Open Ballot	Secret Ballot
Constant	-0.51***	-0.72	0.64***	-0.22
	(0.126)	(0. 103)	(0.050)	(0.180)
Reform Failure	-0.17	-0.196	-0.23*	-0.12
	(0.164)	(0.145)	(0.129)	(0.205)
Pseudo R^2	0.004	0.004	0.006	0.002
Prob>X^2	0.28	0.19	0.076	0.55
Number of Observ.	532	282	724	626
Std.Err. adjusted for # clusters	8	7	8	7

Table 1.9: The Impact of Failure of Comprehensive Reform on Trust and Reciprocity

*p-val.<.10, ** p-val.<.05, *** p-val.<.01

1.5 Conclusion

We showed in a laboratory experiment that the expectation of profitable vote trades leads to resistance to comprehensive reform. According to standard economic theory, vote trading in our experiment is impossible because commitment devices for vote trading are missing. Consequently, resistance to comprehensive reform should not occur. Nevertheless we observe resistance to the comprehensive reform. Comprehensive reform failure results, because legislators expect profitable vote trades even with an imperfect device for vote trading, namely a transparent sequential bill-by-bill voting procedure. The transparent voting procedure provides accountability, which allows legislators to court for positive reciprocity. Because legislators expect others to trust in their reciprocity, they impede efficient comprehensive reforms and trade votes after comprehensive reform failure. Eventually majority coalitions emerge who trade votes at the cost of third parties.

Trust in others' reciprocity abates substantially when individual voting behavior is not observable. In turn, vote trading becomes difficult and resistance to comprehensive reform subsides. We do not claim that secretive procedures can solve the reform deadlock in real world policy making. Instead, we take our results as a warning that resistance to comprehensive reform can stem from any source which facilitates vote trades or secret agreements.

The importance of trust and reciprocity for resistance to reform might be smaller with asymmetric payoffs, private knowledge about preferences over bills and a larger number of decision makers. However, the importance of trust and reciprocity might also be larger, because in contrast to real world voting decisions, legislators in the experiment could not directly communicate to agree on a vote trade. Also, the small size of the voting body does not have to be interpreted as a small number of legislators in general. In representative democracies, usually only a small number of political parties exist. If party members are strictly adherent to the party line, decisions will eventually be made by a small number of groups. Resistance to reform may then result from parties (instead of individual legislators) strategically vetoing comprehensive reforms.

Our results shed new light on role of trust and reciprocity when designing optimal political decision making procedures. Typically trust and reciprocity are associated with positive welfare effects, for instance in trust games or public goods

games. However, it is not completely uncommon that social preferences can lower social welfare, in particular, because they foster coordination. Coordination decreases social welfare for instance in oligopolies, where it facilitates collusion among suppliers or in rent seeking contests between groups, in which within group coordination increases the amount of wasted money.[16] In a similar way, trust and reciprocity foster coordination in voting environments. In our setup, trust and reciprocity will increase total payoffs, if the possibility for a comprehensive reform is missing. If instead a comprehensive reform is available, trust and reciprocity will lead to resistance to the comprehensive reform.

[16] See e.g. Leibbrandt and Sääksvuori (2010).

1.6 Appendix to Chapter 1: Instructions (translated from German)

We present a full translation of the instructions for the OpenBallotCR. Instructions for OpenNoCR are identical, except for the decision on the package, which is missing there. In the treatments with a secret ballot we modified the instructions at the relevant parts. We indicate these modifications after the translated instructions for OpenBallotCR. The general information is identical in all treatments.

General information (Participant A)

Today you take part in an economic decision making experiment. If you read the following instructions carefully, you will be able to earn money additionally to your show-up fee of 2 Euro. Therefore it is important that you read the instructions completely.

For the entire duration of the experiment, it is not allowed to communicate with other participants. We therefore ask you not to talk to each other. If you have problems understanding the experiment, please have a second look at the instructions. If you still have questions, please give us a hand signal. We will come to your cubicle and answer your questions personally. During the experiment, we do not talk about Euro, we talk about points. The numbers of points you earn in the experiment are converted into Euro with the following exchange rate.

1 Point = € 0,20

At the end of the experiment, you will receive the 2 Euros show-up fee plus the equivalent of all points received in the experiment in cash. The following pages will explain the experiment in detail. At the end of the instructions we added some control questions helping you to understand the sequence of events. The experiment does not start until all participants solved the control questions and are completely familiar with the course of the experiment.

Summary

This experiment has 12 periods. In each period you will form a group with two randomly determined participants. At the beginning of a period each participant receives 4 Points. Then you and the other two members of the group decide on three different bills. The bills affect the points of each group member. A period ends when the group made a decision on all three bills. Then, a new period starts. You form a new group with two randomly chosen participants. All over you decide on three bills in 12 Periods. After the final period you will see a summary table on screen showing your points earned in each period.

At the end of the experiment you receive the 2 Euro show-up plus the euro equivalent of points earned in cash.

The Experiment

In this experiment we speak of three different participants, Participant A, B and C. **You are a Participant A.** In each period you form a group of three members with a randomly chosen participant B and a randomly chosen participant C. At the beginning of each period each participant receives 4 points. There are three bills to be voted on in each Period. We label them Bill A, B and C, respectively. First the group chooses if all three bills shall be accepted simultaneously or not.

If all members of the group decide to vote on the acceptance of all three bills, the vote on this package of bills takes place. Each member votes (simultaneously) for or against the acceptance of all three bills. If a majority of the group (at least two members) votes for the acceptance of all three bills, the bills are accepted.

If the group does not unanimously agree on voting on all three bills at once, the group decides sequentially on the three bills. If a majority (at least two members of the group) accepts a bill, it is passed.

The bills in detail: Each of the three bills yields 6 additional points for one group member but subtracts two points from each of the other two members.

Bill A: Participant A receives 6 additional Points, 2 points are subtracted from Participant B and C (each).

Bill B: Participant B receives 6 additional Points, 2 points are subtracted from Participant A and C (each).

Bill C: Participant C receives 6 additional Points, 2 points are subtracted from Participant A and B (each).

Each bill can be accepted or rejected by the group. Thus it is possible that more than one bill is accepted or rejected. The order in which the bills are voted on is determined randomly. The six possible sequences are:

Sequence	**1st Bill**	**2nd Bill**	**3rd Bill**
1	Bill A	Bill B	Bill C
2	Bill A	Bill C	Bill B
3	Bill B	Bill A	Bill C
4	Bill B	Bill C	Bill A
5	Bill C	Bill A	Bill B
6	Bill C	Bill B	Bill A

At the beginning of each Period, namely before the decision on the first bill, the sequence of bills is displayed on your computer screen.

Procedures are the following

Step 1 – Decision on the voting procedure

Participants of each group see the three bills and the possible sequence, given the group decides for the sequential procedure.

Each participant states in Step 1 if she is for or against voting on all bills at once. If all participants of a group decide to vote on all bills at once **Step 2a comes next**, otherwise it follows **Step 2b**.

Step 2a – Simultaneous Procedure (only if all members of the group agreed on voting on the three bills at once)

Each participant of the group states if she is for or against accepting all bills at once. It follows **Step 3**.

Step 2b – Sequential Procedure (only if at least one member of the group disagreed on voting on the three bills at once in Step 1)

In this Step, you see the bill put to vote. You state whether you are for or against this bill. **Step 2c** follows.

Step 2c – Outcome of the vote on a single bill (only if at least one member of the group disagreed on voting on the three bills at once in Step 1)

After all participants of a group made their choice, the result of the current vote will be displayed on the computer screen. A bill is accepted if a majority of the group voted for the bill, i.e. if two or three group members voted for the bill.

In this Step you can also see which participant of your group decided for/against the current bill.

It follows the decision on the next bill. This means you see the next bill put for vote and decide on this bill (see Step 2b). Then, you see the outcome of this vote (see Step 2c).

Then you decide on the third bill and see the result of the group decision.

Step 3 – Result

After voting on the three bills, a summary table is presented on your computer screen. It shows which bills passed or failed in this period. Also, it displays the number of points earned.

After Step 3 you are again randomly matched with two participants and form a new group.

When the new period starts, no participant receives any information on your voting behavior from previous periods. Also, you do not receive any information on the voting behavior from previous periods of the new group's participants. Neither before nor after the experiment, will you receive any information about your counterparts' identity. The randomly selected participants who interact with you do also not receive any information on your identity.

Payment

At the end of the experiment you receive the 2 Euro show-up plus the euro equivalent of points reached in cash.

We now present an example which helps you to understand the course of the experiment on screen in more detail. At the end of this example you will find some control questions. Please write down your answers to these questions. **Your answers to these questions will not affect the amount of money you will receive at the end of the experiment.**

Course of the experiment on the computer screen– an example

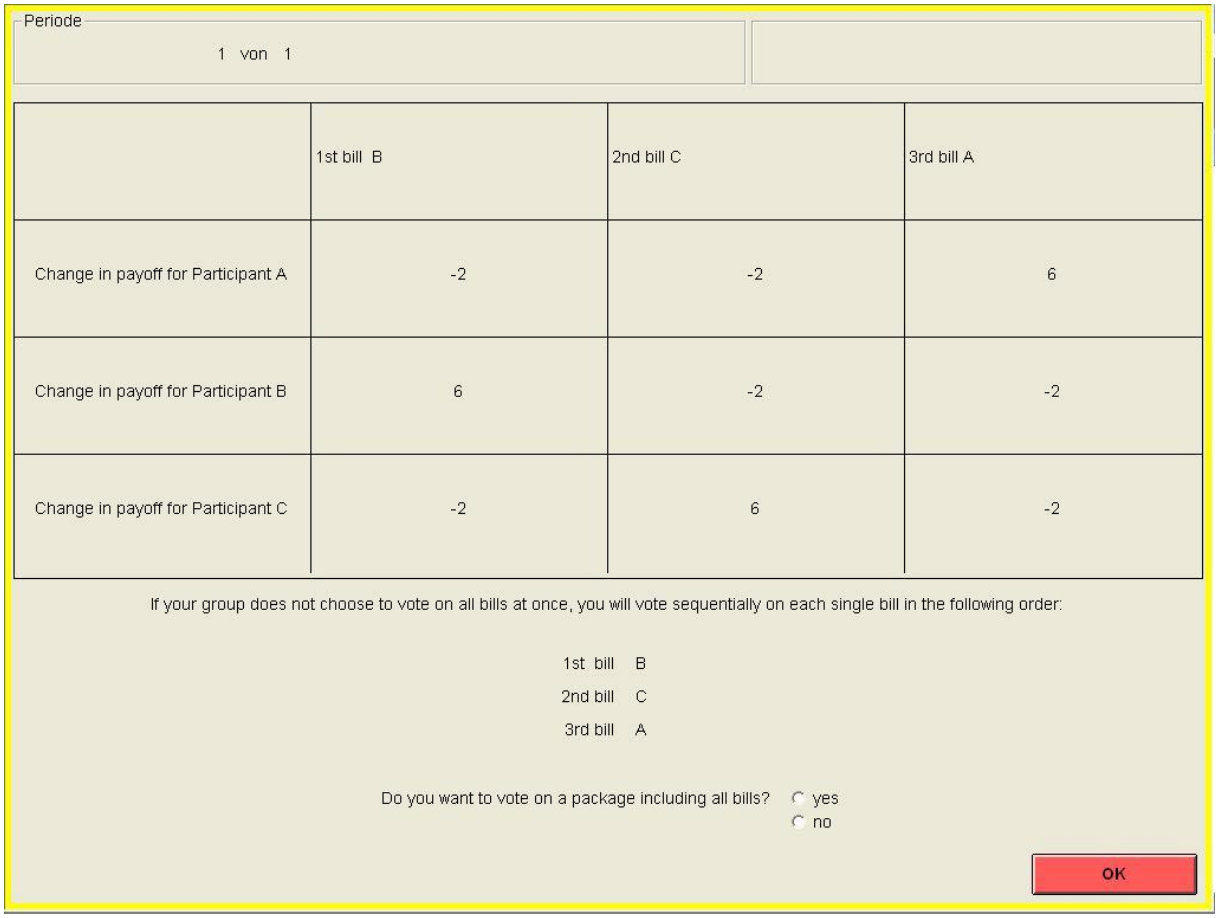
Step 1 – Decision on the voting procedure

All Participant of the group see the three bills and the randomly selected order in which they will be voted on in case of a sequential voting procedure.

Each participant decides in this Step, whether she is for or against voting on the three bills at once.

If all participants of a group decide for voting on the three bills at once **Step 2a** will follow, otherwise continue with **Step 2b**.

A screenshot of Step1 (in this example):



The first line in the table shows the randomly selected order of bills (in this example: 1st Bill B, 2nd Bill C and 3rd Bill A).

Below we present how each bill will change the amount of points of each participant, if a majority accepts this bill.

In this example:

1st Bill B: Participant B receives 6 additional Points, 2 points are subtracted from Participant A and C.

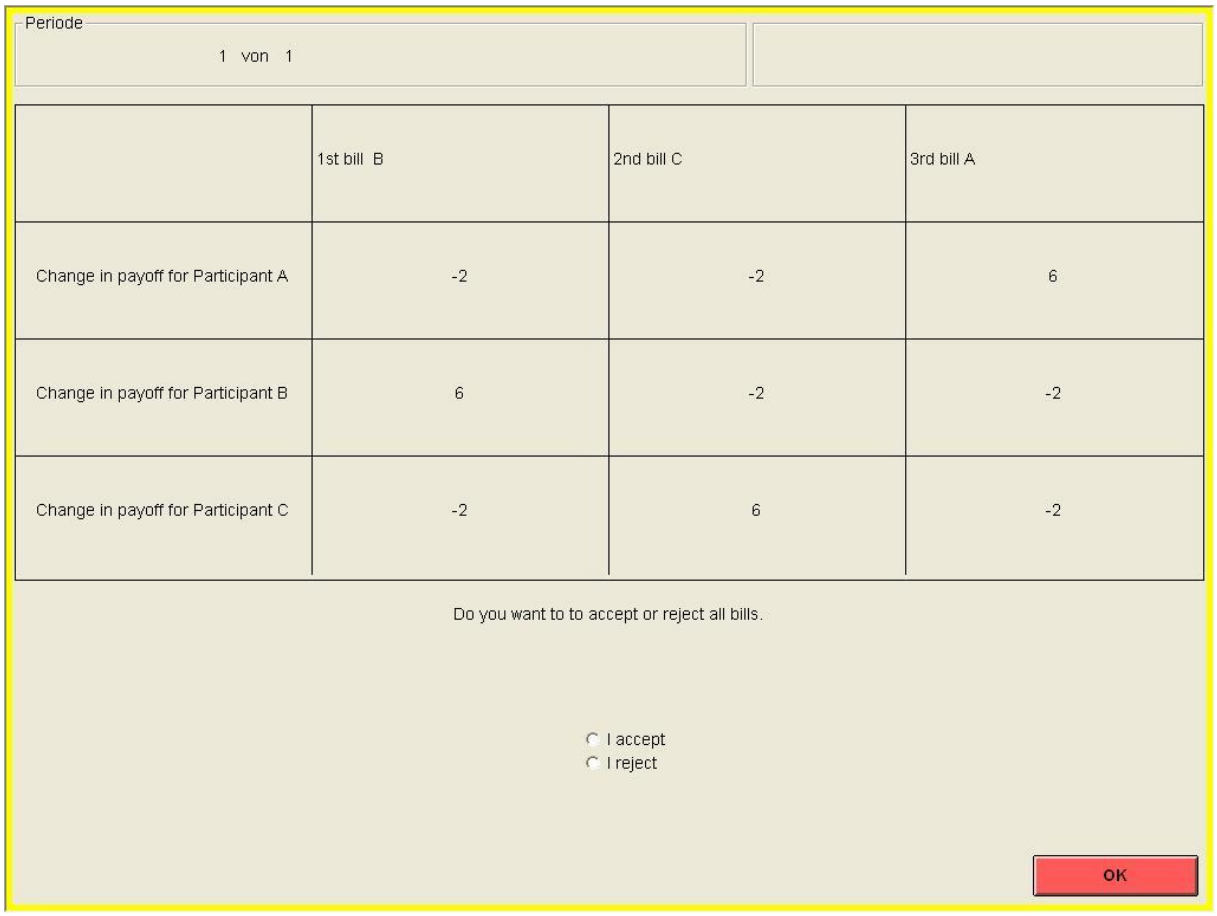
2nd Bill C: Participant C receives 6 additional Points, 2 points are subtracted from Participant A and B.

3rd Bill A: Participant A receives 6 additional Points, 2 points are subtracted from Participant B and C.

If a bill is not accepted by a majority, it does not affect the points of any participant.

Step 2a – Simultaneous voting Procedure

We now look at the case in which all participants of a group decided to vote on the three bills at once. Then the participants see the following screen:



Each Participant of the group now chooses to accept /or reject all bills at once.

If a majority, at least 2 participants of the group, decides to accept all bills at once, all three bills are accepted. If a majority votes against all three bills, the bills are rejected. It follows **Step 3**.

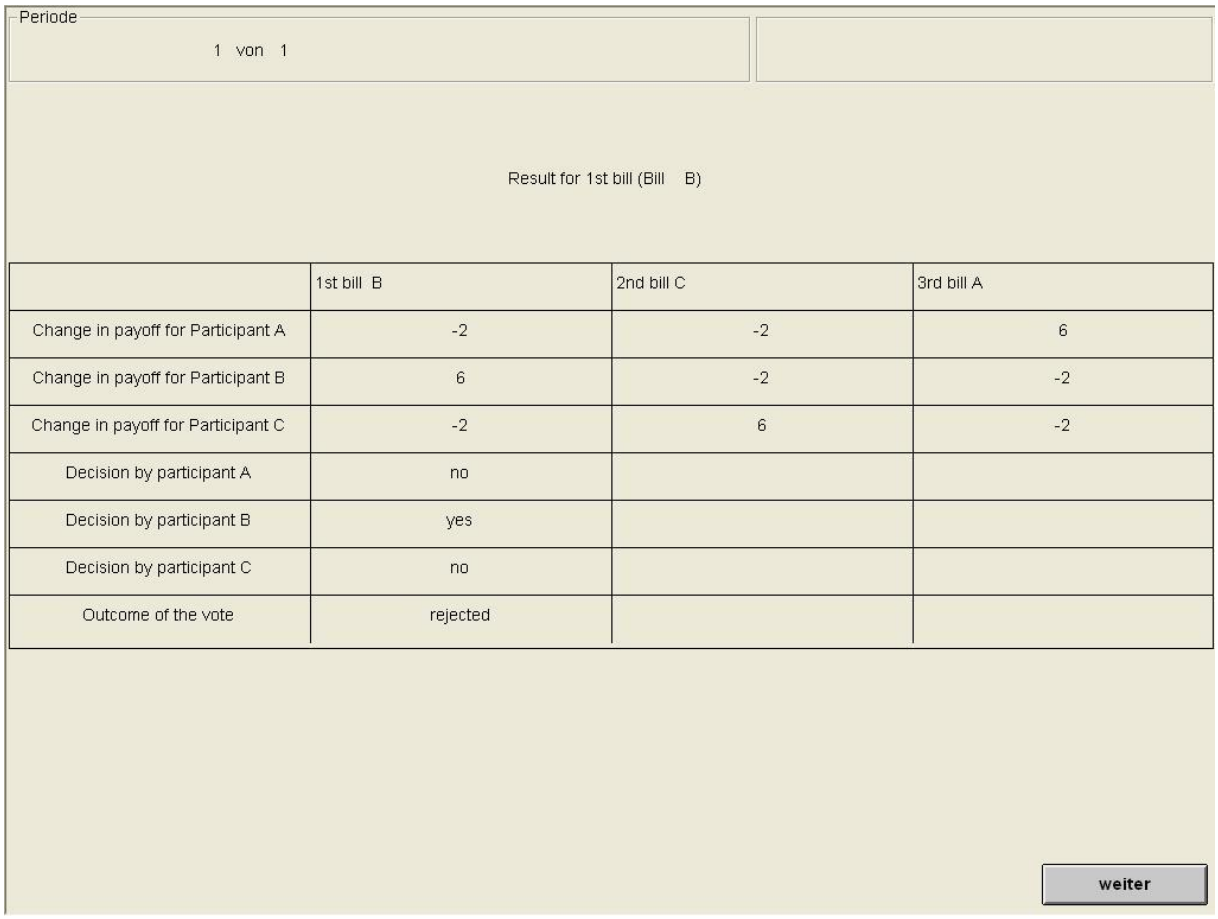
Step 2b – Sequential procedure

Now we look at the case in which at least one participant of a group decided not to agree on voting on the three bills at once. This means, bills are now voted on sequentially, in the previously displayed order. In our example, first Bill B, then Bill C and finally Bill A:

You decide whether to accept/reject the current bill and click the „OK"-Button. After all participants of the group made their decision, the voting result is displayed.

Let us assume that, in our example, Participant B accepted the 1st bill, You (Participant A) and Participant C, however, rejected the 1st bill. The intermediate result (Step 2c) is displayed on your computer screen:

Step 2c – Outcome of the vote on a single bill


Periode

1 von 1

Result for 1st bill (Bill B)

	1st bill B	2nd bill C	3rd bill A
Change in payoff for Participant A	-2	-2	6
Change in payoff for Participant B	6	-2	-2
Change in payoff for Participant C	-2	6	-2
Decision by participant A	no		
Decision by participant B	yes		
Decision by participant C	no		
Outcome of the vote	rejected		

weiter

The table again shows the order in which the bills are voted on. And who voted for or against a bill. In the last line you see whether a bill was accepted or rejected by the majority of the group. In our example a majority (You and participant C) voted against the first bill. Thus in this example the table shows that the 1st bill was rejected by a majority. Consequently, the points of all participants in your group are not affected. By clicking on the "continue" ("weiter") button you will come to the next decision.

Now voting on the second bill begins. Then you see the result of the group's decision on screen. Let's assume a majority of the group accepted the second bill.

Then voting on the third bill starts. You see the third bill and decide for or against it. Let's assume for our example that again a majority accepted the third bill.

We continue with **Step 3.**

Step 3 – Result

At the end of a period and independent from the voting procedure chosen by your group, you will see a summary table showing points received by you and your group members.

In the following we explain how the points received in the period of our example are calculated.

In case of voting on the three bills at once, (continued from **Step 2a**) there are two possible outcomes:

The majority accepts all bills at once.

This yields for each participant of the group:

4 points (endowment)

+6 points (by the one bill in favor of the participant)

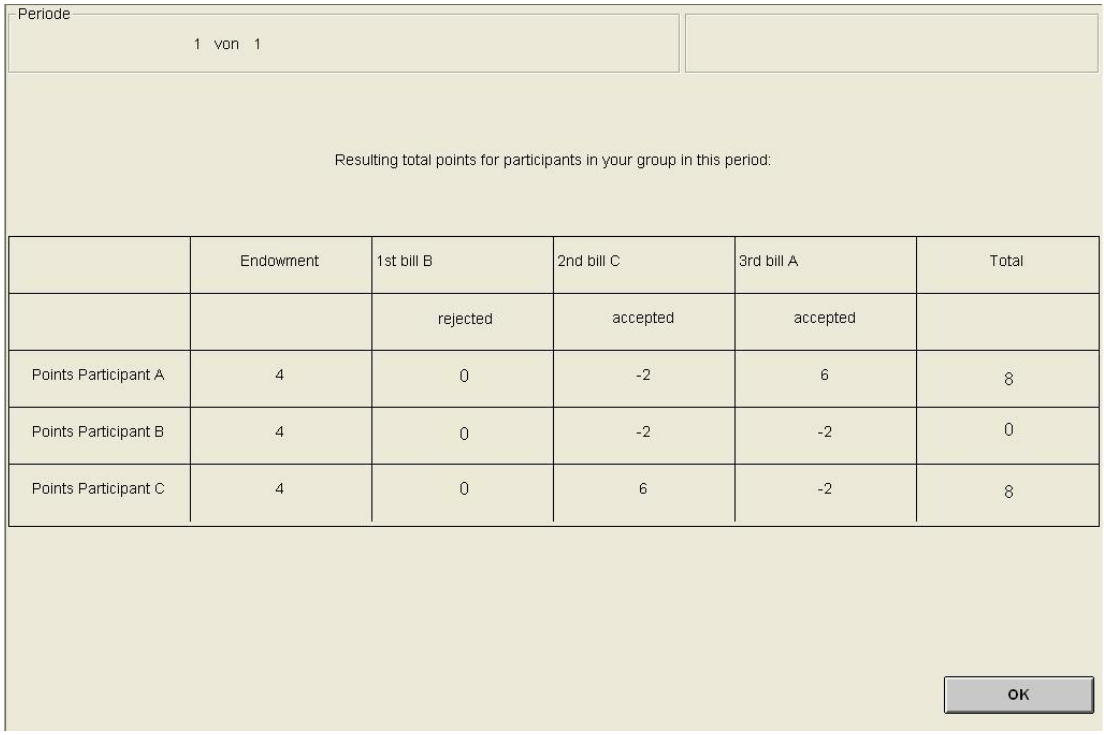
-4 points (by the two bills in favor of the other two group members)

= 6 points

All bills were rejected at once.

This yields 4 points for each participant (the endowment)

Given the group decided for the sequential procedure, (continued from **Step 2c**) points received at the end of a period are calculated as follows:


Periode

1 von 1

Resulting total points for participants in your group in this period:

	Endowment	1st bill B	2nd bill C	3rd bill A	Total
		rejected	accepted	accepted	
Points Participant A	4	0	-2	6	8
Points Participant B	4	0	-2	-2	0
Points Participant C	4	0	6	-2	8

OK

The table displays again the order in which bills where voted on. Additionally you see in the second line whether a bill was accepted or rejected by the majority of the group. In our example the first bill was rejected, whereas the other two bills were accepted. Endowment is 4 points.

The first bill did not affect the points received by participants in this group, because it was rejected. The second bill was accepted and yields participant C six additional points, whereas 2 points are subtracted from participant A and B each. The third bill was accepted too in our example. It yields six additional points for participant A, and subtracts two points from each of the other two participants.

Points received at the end of the period by each participant are calculated as follows:

Points Participant A = 4 + 0 - 2 + 6 = 8

Points Participant B = 4 +0 - 2 -2 = 0

Points Participant C = 4 + 0 + 6 – 2 = 8

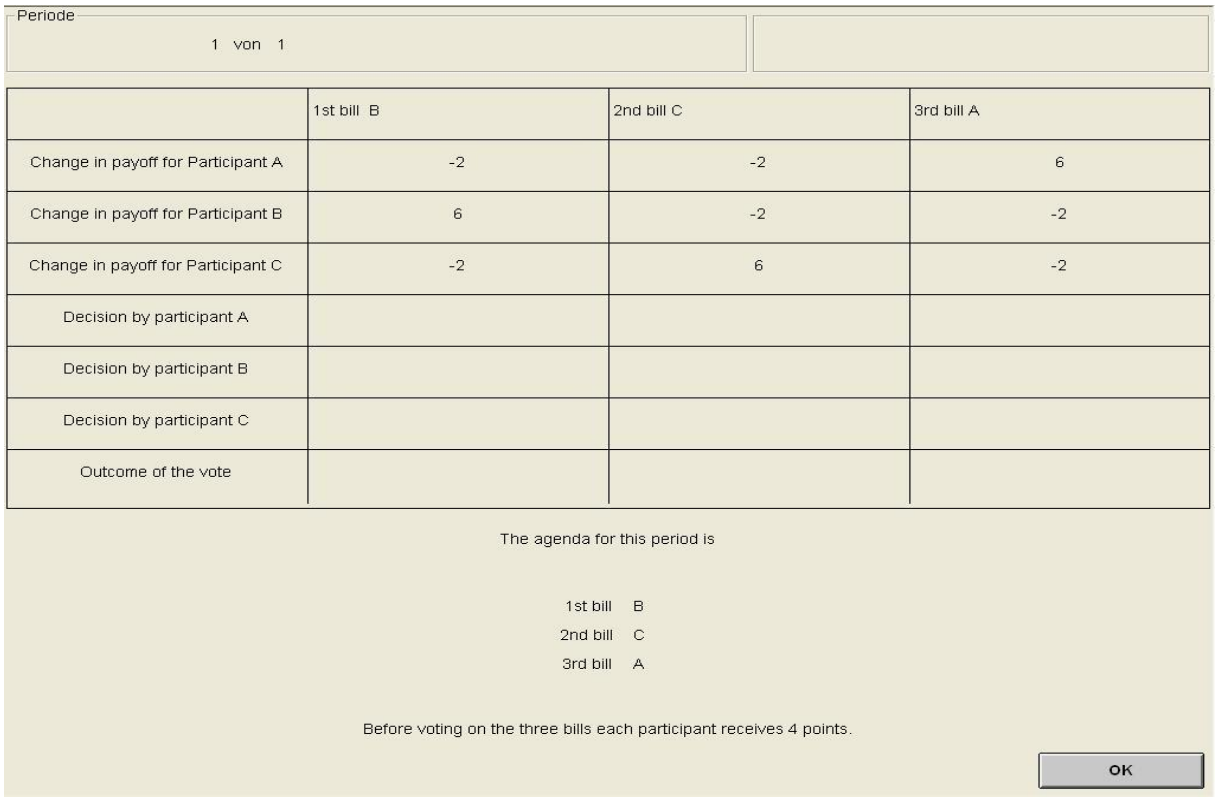
After clicking the „OK“– Button, you are randomly matched into a new group.

Control questions

Please read the new example on this page and answer the control questions.

Your answers to these questions will not affect the amount of money you will receive at the end of the experiment.

Example:


Periode

1 von 1

	1st bill B	2nd bill C	3rd bill A
Change in payoff for Participant A	-2	-2	6
Change in payoff for Participant B	6	-2	-2
Change in payoff for Participant C	-2	6	-2
Decision by participant A			
Decision by participant B			
Decision by participant C			
Outcome of the vote			

The agenda for this period is

1st bill B
2nd bill C
3rd bill A

Before voting on the three bills each participant receives 4 points.

OK

Assume that at least one participant of the group decided against voting at once on the three bills.
Assume further…
You accept the 1st and 3rd bill.
Participant B accepts the 1st and 3rd bill.
Participant C accepts the 1st and 2nd bill

Which bills are accepted by a majority of the group? _____
How many points do you receive in this period? _____
How many points does participant B receive in this period? _____
How many points does participant C receive in this period? _____
□correct or □wrong: „If two of three group members decide for voting on the three bills at once, the group will decide on the three bills at once.

[Change of instructions for Treatments SecretBallotCR OpenNoCR and Secret NoCR]

The Experiment

In this experiment we speak of three different participants. Participant A, B and C. **You are a Participant A.** In each period you form a group of three members with a randomly chosen participant B and a randomly chosen participant C.

At the beginning of each period each participant receives 4 points.

There are three bills to be voted on in each period and group. We label them Bill A, Bill B and C respectively. Each group chooses weather to pass or fail each bill sequentially. If a majority (at least two members of the group) accepts a bill, it is passed. The order of bills is random.

The bills in detail: … **[See instructions for OpenBallotCR]**

Procedures are the following…

Step 1 – Determination of the agenda
A random process determines the order of the bills.

Step 2 Agenda display
Participants can see the current order of bills on their computer screens. It follows step 3...

Step 3 – Sequential Procedure
In this step you see which bill is currently voted on. You decide if you accept or reject the bill.

Step 4 – Result
After all participants of a group made their choice, the result of the current vote will be displayed on the computer screen. A bill is accepted if a majority of the group voted for the bill, i.e. if two or three group members voted for the bill.
[Only the open ballot treatments OpenBallotCR and OpenNoCR:] In this step you can also see which participant of your group decided for/against the current bill

After voting on the three bills, a summary table is presented on your computer screen. It shows which bills passed or failed in this period. Also, it displays the number of points earned. After Step 4 you are again randomly matched with two participants and form a new group.

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2 Agenda Setting and Reciprocal Vote Trading

2.1 Introduction

Agenda setting allows chairmen to move the outcome of a political decision making procedure in the direction of their interest (e.g. Romer and Rosenthal, 1978; Baron and Ferejohn, 1989 and Cox and McCubbins, 2005). Tsebelis and Proksch (2007) even argue it was the use of agenda control which made the success of the European Convention in producing a constitutional treaty possible. Thus, agenda setting can be considered a powerful tool in political decision making.

Social-choice literature has extensively discussed under which conditions voting procedures might be subject to manipulation.[17] Traditionally, agenda setting has been studied in situations in which a voting body decides on different alternatives of a single decision.[18] Reciprocity plays a minor role in this context because legislators' possibilities to punish or reward are restricted. If a committee instead decides on a series of unrelated proposals, reciprocity among legislators may play a role for voting behavior and thus also for agenda setting possibilities. On the one hand reciprocity may lead to vote trading which increases the feasible set of outcomes in sequential decision making procedures and thereby provide additional grounds for agenda manipulation. Consequently, reciprocity among legislators may increase agenda setting possibilities. On the other hand, reciprocity among legislators may reduce agenda setting possibilities because agenda setting may be considered as morally problematic and reciprocators who dislike agenda manipulation may punish the agenda setter.[19]

To study the impact of reciprocal behavior on agenda setting possibilities in a sequential collective decision making procedure, we designed a laboratory experiment in which vote trading and agenda manipulation cannot occur when the committee is comprised of selfish members, but can occur if committee members are reciprocal. In the experiment, a committee decides in a bill-by-bill voting procedure under simple majority rule. All bills are efficient and any subset of bills can pass or fail. Each bill is

[17] For surveys on agenda manipulation see e.g. Cox (2006) and Cox and Shepsle (2007). For a recent survey on laboratory voting experiments including agenda setting see also Holt (2006).

[18] See early work by Gibbard (1973), Satterthwaite (1975), McKelvey (1976), Ordeshook and Palfrey (1988) and more recent work by Dutta, et al. (2004).

[19] In his doctorate, Satterthwaite (1973, p. 5-16) names five reasons why agenda setting may be perceived problematic: inequality in skills (some legislators are able to manipulate, others are not), inefficiency (resulting from gathering costly information about others' preferences in order to manipulate), non-transparency of voters' preferences (because some voters abstain or hide their true payoffs to avoid manipulation), non-transparency of preferences and representatives' preferences (voting decision may not reflect representatives' preferences), and randomness of voting outcomes (when dominant strategies are missing and several legislators try to manipulate the agenda). See van Hees and Dowding (2008) for a detailed discussion of these arguments.

preferred by exactly one committee member and preferences over bills are common knowledge. Bills can thus only be passed by vote trading coalitions. The agenda setter's power is restricted to the control over the order in which proposals are voted on,[20] and by design the order only matters if legislators trade votes.[21] If legislators are reciprocal, the order may make certain vote trading coalitions more (or less) likely and thus affect the voting outcome. In addition, reciprocity might matter because non-agenda setters may perceive manipulation negatively, first, due to outcomes resulting from specific agendas and second, due to the fact that only one person in each committee is allowed to determine the order of bills on the agenda.[22]

The main interest of this paper is to understand whether reciprocity among committee members provides additional grounds for agenda manipulation and how reciprocal legislators deal with agenda manipulation. The experimental setup allows us to show whether vote trading based on reciprocity leads to additional agenda setting possibilities in a controlled environment. Researchers have studied the occurrence of vote trading in real world policy making (see e.g. Stratmann, 1992) but its implications for agenda setting possibilities are difficult to disentangle. First, in the field different degrees of reciprocity among different legislators or parties cannot be controlled for. Second, legislators' voting decisions might be driven by individual reputation building. Third, it is very difficult to identify legislators' true preferences – even for legislators themselves. These problems are avoided in an experiment. First, an experiment allows for anonymous decision making. Preferences with respect to specific characteristics of other legislators or parties can thereby be excluded and reciprocity is restricted to the interaction in the experiment. Individual reputation building can be controlled for by using an appropriate matching procedure and preferences can be induced by using monetary incentives. Furthermore, preferences of different legislators can be made

[20] Note that the extent of the agenda setter's control can vary extensively. Agenda setters may determine what voting procedure is used, what subset of possible alternatives in addition to the status quo is voted on or may be the only person who is able to add alternatives to an otherwise fixed set (see also Miller, 1995).

[21] Casella (2011) also studies a situation in which a committee decides on a series of bills and (in some treatments) an agenda setter can determine the order of the bills. In her experiment, agenda setting does not significantly matter.for outcomes However, her analysis differs in several aspects. First, her committees vote under simple majority rule with bonus votes and second and more importantly, information on legislators' preferences is private and subjects vote in a secret ballot (i.e. they receive only information whether a proposal was passed or not, but not on who voted for or against it). The latter makes vote trading among committee members particularly difficult, which may explain why agenda setting does not affect outcomes strongly.

[22] Apart from this inequality in ability to manipulate the agenda, we excluded Satterthwaite's reasons for negative perception of agenda setting in the experiment. Information on all legislators' payoffs is common knowledge so neither the inefficiency nor the non-transparency nor the randomness argument is valid. We also do not set any default order of bills but "force" the agenda setter to choose an order irrespective of her intentions to manipulate or not.

common knowledge in order to control for information asymmetries. The small number of decision makers in experimental voting decisions may be considered as a drawback, but also in many real world voting procedures committees comprised of a few members make voting decisions. In addition, adherence to the own party line may reduce the number of actual decision makers drastically.

Our findings demonstrate that agenda setting possibilities can indeed result from reciprocity among legislators. Agenda setters are well aware of their counterparts' reciprocity and consequently manipulate the agenda. We find that agenda manipulation is most profitable when information on individual voting behavior is available. This is because transparency allows for the identification of supporters. Beneficiaries of a chosen agenda discriminate positively against the agenda setter whereas those suffering from the chosen agenda vote less frequently for the agenda setter's bill, in particular when individual votes are not observable.

The rest of the paper is organized as follows. In the next section we explain the experimental design and procedures. In Section 2.3 we provide predictions of our subjects' behavior. We report the results in Section 2.4 and conclude in Section 2.5.

2.2 Experimental design and procedures

In the experiment, three participants form a committee. The committee decides on three independent bills. Each bill is strictly preferred by exactly one member of the committee. A preferred bill yields 6 additional points for oneself, whereas the other two members of the committee lose two points each. Thus, if a bill is passed, the overall payoff will increase by two points. However, only one participant of the group gains from each bill. Consequently, each single bill is disadvantageous to a majority of the group. Table 2.1 shows how each bill affects the participants' payoffs. For the rest of the paper we will call a legislator who benefits from the first bill "first beneficiary" and legislators benefiting from the second (third) bill "second (third) beneficiary". The committee votes sequentially on each of the three bills using simple majority rule. Each bill can be passed or failed. First, all committee members simultaneously cast their votes on the first bill. Then, the committee is informed about the outcome of the vote. Second, each member casts her vote for the second bill. The second vote is displayed and the group decides on the third bill. Finally, the outcome of the third vote and the resulting payoffs are displayed.

	Bill A	Bill B	Bill C
Member A	+6	-2	-2
Member B	-2	+6	-2
Member C	-2	-2	+6

Table 2.1: Bills and resulting payoff changes

There are two dimensions in which we vary our experiment. The focus of this paper is on how an agenda setter influences the decision process. In the agenda setter condition, we randomly select one participant in each group who assumes the role of an agenda setter. The assignment takes place at the beginning of the experiment and subjects maintain their role during the whole experiment. In each period, one agenda setter is matched with two non-agenda setters.[23] The agenda setter can determine the order in which the bills are put for vote in her committee.

Information on individual voting behavior is likely to affect trust and reciprocity among legislators, because it allows for the identification of supporters. We therefore study agenda setting possibilities under two conditions, first when the voting procedure is transparent and second, when only the outcome of the vote is displayed. As control treatments, we use data from an earlier experiment (see Fischbacher and Schudy, 2010), in which subjects faced the identical situation but the order of bills was determined randomly. To summarize, an agenda setter determines the order in which bills are voted on either in a transparent voting procedure (*ASFI*= **A**genda **S**etting, **F**ull **I**nformation) or in a secret ballot (*ASPI*= **A**genda **S**etting, **P**artial **I**nformation). In the control treatments a random device determines the order of bill either under full (*RAFI*= **R**andom **A**genda, **F**ull **I**nformation) or partial information (*RAPI*= **R**andom **A**genda, **P**artial **I**nformation). In all treatments, the order of bills is displayed to the members of the committee before voting starts.

To control for learning effects and changes of voting behavior over time, participants voted on the three bills in 12 periods which were all payoff relevant. In each period, each participant was randomly sorted into a group of three participants. We use a random matching procedure, which assured that participants cannot infer any information on their current counterparts' individual voting behavior from past periods.

[23] Depending on the size of the sessions we formed matching groups of at least nine participants.

Treatment	# Subjects	# Sessions	# Matching-Groups
RAPI	54	2	3
ASPI	48	2	3
RAFI	51	2	3
ASFI	72	3	4

Table 2.2: Treatments, Sessions and Matching-Groups

Thus we exclude individual reputation building across periods. Each subject sat at a randomly assigned and separated PC terminal and was given a copy of instructions.[24] A set of control questions was provided to ensure the understanding of the game. If any participant answered incorrectly, the experimenter provided an oral explanation. No form of communication between subjects was allowed during the experiment.

We conducted all sessions at the LakeLab (University of Konstanz, Germany). The experiment took place between December 2008 and January 2009. One additional session was conducted in May 2009. Altogether, 225 subjects participated in nine sessions. Table 2 summarizes the number of subjects, sessions and treatments in more detail. None of the subjects participated in more than one session. Each session included exactly one treatment. Participants received a show-up fee of 2 euro ($2.40 at that time). The experiment took about one hour and 15 minutes, average income was about 12.50 euro ($17.50). The experiment was programmed and conducted using z-Tree (Fischbacher, 2007). We recruited participants using the online recruiting system ORSEE (Greiner, 2004). Participants were part of the LakeLab subject pool, consisting of undergraduate and graduate students of all fields of study.

2.3 Behavioral predictions

When all committee members are selfish, the order of bills does not affect voting behavior. Committees comprised of selfish members will vote all bills down because each bill is only preferred by a minority of the committee and commitment devices for vote trading are missing. However, when some committee members expect reciprocal behavior by their counterparts they may court for reward by voting on bills preceding their own bill on the agenda. With the following two assumptions, we derive Proposition 2.1:

[24] A copy of translated instructions can be found in the appendix of this chapter.

(i) legislators are reciprocal and do not support bills of legislators who turned down their own bill

(ii) legislators do not discriminate against particular counterparts, i.e., they treat agents differently only when they have different information about their behavior

Proposition 2.1 - The approval of the second bill and the approval of the third bill is not more likely than the approval of the first bill

Proof. If the first bill is not accepted then both the second and the third beneficiary have turned down the first bill. According to (i) the first beneficiary will not vote for any subsequent bill. Further, because of (ii), the third beneficiary will also vote against the second bill. Finally, also the third bill will be turned down because the third beneficiary did not support any preceding bill. Thus, it is not possible that the second or the third bill is approved more frequently than the first bill. □

If we additionally assume that

(iii) the third beneficiary does not vote more frequently for the first bill than the second beneficiary

we come to proposition 2.2.

Proposition 2.2 – The approval of the third bill is not more likely than the approval of the second bill.

Proof. We first prove the proposition for voting with partial information. Let us start with the case in which the first bill fails. As we have seen in the proof for proposition 2.1, both the second and the third bills will be turned down in this case. We now turn to the case in which the first bill is approved. Due to (ii) the first beneficiary either votes for both or none of the subsequent bills. If she accepts both bills, both the second and third bills are approved. If she votes against the second and third bills, the second bill can only fail if the third beneficiary votes against it. However, in this case the third bill will also receive no support by the second beneficiary (due to (i)) and will also fail.

Let us now turn to the full information case. So far, we have shown that when a bill fails, the subsequent bills fail as well. This is not necessarily true in the full

information condition. Here it is possible that the third beneficiary supports only the first beneficiary and receives reward by the first beneficiary whereas the second beneficiary does not support the first bill and receives support neither from the first beneficiary (due to (i)) nor from the third beneficiary because the third beneficiary knows that the second beneficiary turned down the first bill. Hence it is in general possible to observe the committee passing only the first and the third bills. We will now show that it is nevertheless not possible that the third bill is on average approved more frequently than the second due to (iii). Consider two different matching protocols: In matching 1, participant A is the first beneficiary, participant B is the second and participant C is the third beneficiary. Thus participant A received support from participant C and therefore also voted for participant C's bill. In matching 2 instead the participants are matched differently so that participant A is still the first beneficiary, but participant C is now second beneficiary and participant B is now the third beneficiary. This means that participant A received support by participant C, who is now second beneficiary and participant B who is now third beneficiary does not vote for the first bill, and due to (i) and (ii) does not receive any support. Thus with random matching we can conclude that on average the third bill cannot be approved more frequently than the second bill. □

Proposition 1 implies that when legislators are reciprocal and do not discriminate against specific legislators, it is a weakly dominant strategy for the agenda setter to put his preferred bill first. It is plausible to assume no discrimination when the sequence of the bills has been assigned by a random device. It is, however, less convincing when the agenda setter determines the sequence. The agenda setting option can affect voting behavior of the agenda setter as well as voting behavior of other legislators.

On the one hand non-agenda setters may positively discriminate against the agenda setter. Two reasons may drive positive discrimination. First, the agenda setter could feel more responsible for the voting outcome and thus reward other legislators more frequently.[25] Second, the agenda setter has more power than other legislators and thus receives a higher expected payoff. This could prevent her from compensating low income periods by exploiting other legislators. In both cases other legislators have no incentive to distrust the agenda setter but instead they should support the agenda

[25] The idea is related to "responsibility alleviation", which states that shifting responsibility from an outcome to an external authority reduces impulses towards generosity (see Charness, 2000).

setter's bill with a higher probability than legislators in the treatments with no agenda setter.

On the other hand, other legislators may discriminate negatively against the agenda setter because they either consider agenda setting per se as morally problematic, or because they fear exploitation when the agenda setter uses her power to place her own bill first on the agenda. Legislators may thus refrain from trusting the agenda setter and therefore do not support her bill. By not providing any default order of bills we "force" the agenda setter to choose an agenda which makes a general negative perception of agenda setting is less likely. Therefore punishment may mainly occur due to the outcomes resulting from specific agendas chosen.

Punishing the agenda setter by not voting for her bill is however risky, in particular when the voting procedure is transparent. Here, legislators are accountable for punishment and the agenda setter can directly reciprocate. Legislators who do not vote for the agenda setter's bill (when it is placed at the first position) risk not receiving help from other legislators for their own proposal.[26] If the agenda setter places her bill first in the partial information treatment, legislators not voting for the agenda setter's bill cannot be identified. Therefore, the rejection of the agenda setter's bill is more likely when it is in the first position and individual voting behavior is not observed. In particular, the third beneficiary could use this option since we expect her bill to be accepted with the lowest probability. Thus, if the agenda setter puts her own bill first, we expect little difference in behavior from that of other legislators in the full information treatment. In the partial information treatment we expect that fewer third beneficiaries will support the agenda setter when she puts her bill first on the agenda.

Positioning her own bill first is a weakly dominant strategy for the agenda setter as long assumptions (i) to (ii) are fulfilled. When legislators discriminate negatively against the agenda setter because of distrust it may be possible for the agenda setter to reduce distrust by positioning her bill second on the agenda. If an agenda setter decides to do so, it is clearly necessary that the agenda setter accompanies this decision by support for the first bill. Consequently, we expect higher efficiency when the agenda setter puts her bill second on the agenda.

[26] Mutual support for each other's bill by two non-agenda setters requires a lot of trust on the side of the third beneficiary. If the third beneficiary observed the first bill failing, she may distrust the second beneficiary, because the second beneficiary did not vote for the first bill.

2.4 Results

We first show how the order of bills matters for the voting outcome. Then, we address the question of whether the agenda setter manipulates the agenda. The next subsection deals with agenda setters' voting behavior and shows whether the agenda setter exploits other legislators. Further, we investigate non-agenda setters' behavior towards the agenda setter. Do they trust the agenda setter and do they reward or punish her? Finally, we discuss the optimality of the agenda setter's decisions.

2.4.1 The order of bills matters

We find that the earlier a bill is voted on, the higher is the probability of its approval, whether or not an agenda setter determines the order of the bills. Figure 2.1 illustrates individual acceptance rates of monetarily unfavorable bills across treatments. Each column represents the share of members voting for a bill that is monetarily disadvantageous to them.[27] As expected, the third bill is less frequently accepted than the second bill and the second bill is less frequently accepted than the first bill.

Whether legislators vote for a bill depends on the trust and reciprocity among legislators. We will give a short overview of the overall treatment differences in reciprocal behavior. Table 2.3 illustrates the relative occurrence of each possible outcome (A to H) across treatments. By voting on the first bill, the committee decides between outcomes including the approval of the first bill (outcomes A to D) and outcomes excluding the first bill (outcomes E to H). Provided the first bill is passed the

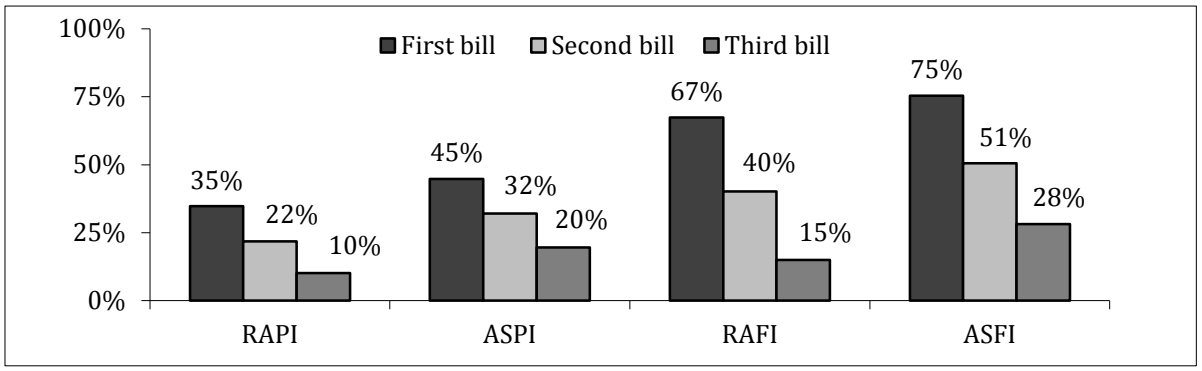


Figure 2.1: Shares of votes for unfavorable bills (by treatments)
Note: ASFI = Agenda Setter Full Information; *ASPI*= Agenda Setting, Partial Information; *RAFI*= Random Agenda, Full Information; *RAPI*= Random Agenda, Partial Information.

[27] Subjects accept bills which increase their own payoff in 99 percent of the cases.

	Bills passed						
Outcome	1st	2nd	3rd	*RAPI*	*ASPI*	*RAFI*	*ASFI*
A	✓	✓	✓	15	30	18	38
B	✓	✓	-	17	18	41	34
C	✓	-	✓	1	2	3	8
D	✓	-	-	24	20	28	16
E	-	✓	✓	2	1	1	1
F	-	✓	-	4	4	3	1
G	-	-	✓	1	1	0	0
H	-	-	-	36	27	5	2

Table 2.3: Outcomes observed across treatments (in percent)
Note: ASFI = Agenda Setter Full Information; *ASPI*= Agenda Setting, Partial Information; *RAFI*= Random Agenda, Full Information; *RAPI*= Random Agenda, Partial Information.

committee decides next between outcomes including the first and the second bill (A,B) and outcomes including the first but excluding the second bill (C,D), and so on.

First note that, when a bill fails, legislators reject subsequent bills with a high probability. We rarely observe outcomes E to G, in which later bills are accepted, although the first bill failed. When the second bill fails, the third bill is also very unlikely to be accepted in the partial information case, as shown by the rare occurrence of outcome C. Table 3 also indicates that legislators may expect reward most frequently when the voting procedure is transparent. Outcome H, in which no bill is passed, occurs only in about 4 percent in the full information treatments (*RAFI* and *ASFI*) whereas it occurs in about 30 percent of cases in the partial information treatments (*RAPI* and *ASPI*). We conclude with a first result:

Result 2.1 The later a bill is voted on, the less likely it is that the bill is accepted, irrespective of agenda setting.

Table 2.3 also indicates that treatments differ in their degree of reciprocity. In order to make reward in full and partial information treatments comparable, we present the share of committee members accepting at least one subsequent bill when their own bill was accepted. We summarize this share in the first column of Table 2.4. The agenda setting option has a stronger impact on reciprocity than transparency of the voting procedure (full information vs. partial information). This result is also confirmed econometrically. In the first column of Table 2.5, we present a probit regression analysis for reciprocal behavior, i.e. for the probability to vote for a bill after one's own bill has

been accepted. The regression reveals a significant positive effect of the agenda setting treatment and an insignificant effect of the information condition.

How do the treatments differ with respect to the trust that the subjects exhibit? To answer this question we focus on the acceptance of the first bill.[28] Columns 2 to 4 in Table 2.4 show the share of supporters of the first bill. Again, we see a positive impact of agenda setting and transparency on trust. However, only the latter is significant (see Table 2.5). Interestingly, for both, the second and third beneficiary trust is affected by transparency but not by the agenda setting option. The beneficiaries differ only in their level of trust; the second beneficiaries' level of trust is significantly higher than the third

Treatment	**Reward Behavior** Share of first and second beneficiaries voting for at least one subsequent bill when own bill was accepted	**Trusting Behavior** Share of second and third beneficiaries voting for the first bill		
		by 2nd and 3rd	by 2nd	by 3rd
RAPI	24	35	47	22
ASPI	38	45	58	31
RAFI	30	67	79	56
ASFI	44	75	86	65

Table 2.4: Reward and trusting behavior across treatments (shares in percent)
Note: ASFI = Agenda Setter Full Information; *ASPI*= Agenda Setting, Partial Information; *RAFI*= Random Agenda, Full Information; *RAPI*= Random Agenda, Partial Information.

	Reward behavior	**Trusting behavior**		
Dependent Variable	Vote for at least one subsequent bill	Vote for first bill		
	by first and second	by second and third	by second	by third
Full Information	0.077	**0.323*** **	**0.299*** **	**0.347*** **
	(0.058)	(0.052)	(0.048)	(0.086)
Agenda Setting	**0.149** **	0.102	0.096	0.109
	(0.064)	(0.119)	(0.085)	(0.177)
Full Information*	-0.017	-0.0108	-0.005	-0.016
Agenda Setting	(0.102)	(0.140)	(0.109)	(0.201)
Observations	1,235	1,800	900	900
Pseudo R-squared	0.020	0.087	0.097	0.097
#Clusters	13	13	13	13

Table 2.5: Reward and trusting behavior. Probit regression (marginal effects) with robust standard errors and clustering on matching groups; *** p<0.01, ** p<0.05, * p<0.1

[28] We focus on the first bill only, because we want to compare trust by the second and third beneficiary in the first beneficiary. We obtain similar results if when using the acceptance of all preceding bills as a measure of trust.

beneficiaries' level of trust. We summarize this discussion in Result 2.2.

Result 2.2 Transparency has a positive influence on trust and agenda setting has a positive influence on reciprocity in the voting decisions.

Do agenda setters manipulate the agenda?

According to the assumptions of non-discrimination and reciprocity it is a weakly dominant strategy for the agenda setter to put her bill first on the agenda. Indeed, this is what a majority of agenda setters do. Figure 2.2 shows the agenda setters' choices over time. In all matching groups the committee on average votes more frequently first on the agenda setter's bill. Committees vote in about 53 percent of the cases for the agenda setter's bill first and this share increases over time, irrespective of the partial or full information condition (see Table 2.6). A robust share of about one fourth of the agenda setters chose the second position, while the third position was chosen by almost no agenda setter.

Result 2.3 Agenda setters put their preferred bill most frequently first on the agenda. They do so irrespective of information on individual voting behavior.

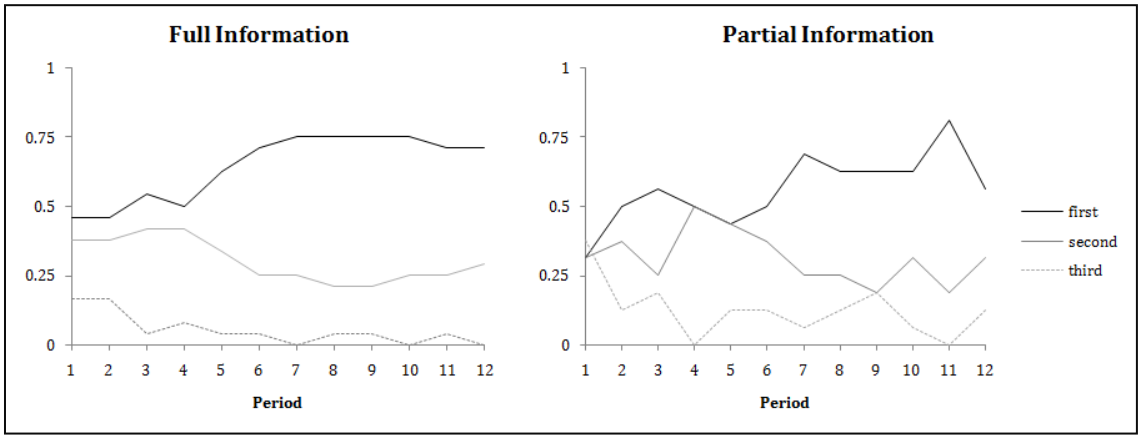


Figure 2.2: Position of agenda setter's bill over time

Dependent Variable:	**Agenda Setter's bill is voted on first**
Full Information	0.274
	(0.287)
Period-12	**0.066*** **
	(0.012)
Full Information*	0.011
(Period-12)	(0.021)
Constant	**0.526*** **
	(0.089)
# clusters	7
Observations	480
Pseudo R-squared	0.033

Table 2.6: Probability of voting on the agenda setter's bill first. Probit regression with clustering on matching groups, robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

2.4.2 How do agenda setters vote?

We first address the question of whether and how much agenda setters reward other legislators when these legislators supported the agenda setter's bill. To answer this question, we discuss direct reciprocity by agenda and non-agenda setters in the full information treatments and reciprocity with respect to the group in the partial information treatments. In the full information treatments, we compare the probability of acceptance of the second (or third) bill by beneficiaries of the first bill when the beneficiary of the second (third) bill voted for the first bill.[29] The first two rows of Table 2.7 show the shares of second and third bills accepted by beneficiaries of the first bill. We present agenda setters' reward behavior in the first column, non-agenda setters' reward behavior in the second column and reward behavior by beneficiaries of the first bill in the treatments without agenda setting in the third column. With full information and agenda setting, there is no difference in direct reward by agenda setters and non - agenda setters (48 percent) with respect to beneficiaries of the second bill on the agenda. Also the share of third bills accepted by agenda setters (44 percent) is only insignificantly[30] higher than the share of third bills accepted by non-agenda setters (32 percent) and the share of third bills accepted by beneficiaries of the first bill in the

	Agenda Setting Treatments		**Treatments without agenda setter**
	by agenda setter	by non-agenda setter	
Full Information			
Reward for beneficiary of the second bill	48	48	39
Reward for beneficiary of the third bill	44	32	25
Partial Information			
Reward for beneficiary of the second bill	46	30	24
Reward for beneficiary of the third bill	48	13	17

Table 2.7: Reward by agenda and non-agenda setters preferring the 1st bill on the agenda (shares in percent)

Note: In Full Information treatments reward refers to the share of 2nd (or 3rd) bills accepted by beneficiary of the 1st bill when the beneficiary of the 2nd (or 3rd) bill voted for the 1st bill. In Partial Information treatments reward refers to the share of 2nd or 3rd bills accepted by the beneficiary of the 1st bill when 1st bill was approved by the committee.

[29] In full information treatments we observe a typical tit-for-tat behavior. Subsequent bills are mainly accepted when their beneficiary supported a preceding bill. First beneficiaries vote for the second (third) bill in only 4.8 (2.6) percent when they received no support by the second (third) beneficiary.

[30] See Table 2.8 model (1) and (2).

	Full Information		Partial Information	
	(1)	(2)	(3)	(4)
Dependent Variable	Vote for second bill	Vote for third bill	Vote for second bill	Vote for third bill
Agenda setting	0.081	0.056	**0.086****	-0.023
	(0.109)	(0.106)	(0.042)	(0.068)
by Agenda Setter	-0.011	0.108	**0.149*****	**0.334*****
	(0.0863)	(0.097)	(0.054)	(0.092)
Positive experience in past periods	0.111	0.106	0.018	0.048
	(0.109)	(0.163)	(0.108)	(0.077)
Observations	382	281	228	228
Pseudo R-squared	0.009	0.026	0.038	0.098

Table 2.8: Reward for beneficiaries of second and third bills by the beneficiary of the first bill

Probit regression, robust standard errors in parentheses (marginal effects) *** p<0.01, ** p<0.05, * p<0.1
Note: In Full Information treatments reward refers to the share of 2nd (or 3rd) bills accepted by beneficiary of the 1st bill when the beneficiary of the 2nd (or 3rd) bill voted for the 1st bill. In Partial Information treatments reward refers to the share of 2nd or 3rd bills accepted by the beneficiary of the 1st bill when 1st bill was approved by the committee.

treatments without agenda setting possibilities (25 percent). In the regression analysis (Table 2.8) we additionally use a measure of positive past experience as a control variable. It is simply the share of accepted own bills until the current period. However, positive experience also does not affect reward significantly. This shows that with full information the immediate experience is more important than the general positive experience. The third and fourth row of Table 2.7 show the shares of accepted second and third bills for the partial information treatments by beneficiaries of the first bill (when the first bill was accepted). In this situation, agenda setters support later bills significantly more frequently than non-agenda setters (46 vs. 30 percent for the second bill and 48 vs. 13 percent for the third bill) and also significantly more frequently than beneficiaries of the first bill in the partial information treatment without agenda setting (17 percent).[31] Thus, we obtain Result 2.4.

Result 2.4 Agenda setters reward more frequently than non-agenda setters when voting on bills in a secret ballot.

We now turn to trusting behavior by agenda setters and other legislators. Table 2.9 shows the frequency of support of the first bill by agenda setters, by non-agenda setters and by subjects in the treatment without agenda setting. As expected table 2.9 suggests

[31] See Table 2.8 model (3) and (4).

that agenda setters trust more than non-agenda setters and they trust in particular more than subjects in the condition without agenda setting. Table 2.10 presents a probit regression with the vote for the first bill as dependent variable. Again, we include our measure for positive experience in past periods as an explanatory variable. The regression reveals that positive experience is the driving force for trust in the first beneficiary's reciprocity. Comparing treatments with and without agenda setting we find that trust in the first beneficiary is significantly higher in the agenda setting treatments when full information on individual voting behavior is observed, indicating that second beneficiaries trust more in agenda setters. We obtain result 2.5.

Result 2.5 Trust in others' reciprocity is mainly driven by positive experiences in the past.

	Agenda Setting Treatments		**Treatments without agenda setting**
	by agenda setter	**by non-agenda setter**	
by beneficiaries of the 2nd bill			
Full Information	87	85	79
Partial Information	72	52	47
by beneficiaries of the 3rd bill			
Full Information	63	65	56
Partial Information	42	30	22

Table 2.9: Shares of 1st bills voted for by beneficiaries of the 2nd (3rd) bill on the agenda.

	Full Information treatments	**Partial Information treatments**
Dependent Variable	Vote for 1st bill by beneficiaries of the 2nd bill	Vote for 1st bill by beneficiaries of the 2nd bill
Agenda Setting	**0.062****	-0.003
	(0.030)	(0.137)
by Agenda Setter	0.008	0.154
	(0.032)	(0.160)
Positive Experience	**0.241****	**0.329*****
	(0.096)	(0.121)
Observations	451	374
Pseudo R-squared	0.060	0.039

Table 2.10: Votes for first bill by beneficiaries of the second bill

Probit regression, robust standard errors in parentheses (marginal effects); *** p<0.01, ** p<0.05, * p<0.1 ; Dep. Variable: Pr(Acceptance of 1st bill by beneficiaries of the 2nd bill

Vote by…	Beneficiary of 2nd bill			Beneficiary of 3rd bill		
Treatment	**Agenda Setting**		**No agenda setting**	**Agenda Setting**		**No agenda setting**
	First bill preferred by			First bill preferred by		
	agenda setter	non-agenda setter		agenda setter	non-agenda setter	
Full Information	87	63	79	60	76	56
N	185	16	204	185	87	204
Partial Information	54	46	47	19	48	22
N	54	76	216	86	76	216

Table 2.11: Votes for 1st bill by beneficiaries of 2nd or 3rd bill (shares in percent)

2.4.3 Reciprocal behavior toward the agenda setter

The agenda setters are in an advantageous situation, which can make the other subjects envious. This problem occurs in particular when the agenda setter chooses the first position, which is what most agenda setters do. Apart from envy, other legislators could also fear exploitation. For this reason the agenda setters risk that their bills are accepted with lower probability than the other legislators' bills. In Table 2.11, we show the frequency with which the agenda setter's bill is supported when she chooses the first position. First, note that the beneficiary of the second bill supports the agenda setter more frequently than a non-agenda setter in the same position (as indicated before). This shows that second beneficiaries indeed expect higher reward by agenda setters. Third beneficiaries instead discriminate negatively against the agenda setter, in

	Trust in first beneficiary by second beneficiary	**Trust in first beneficiary by third beneficiary**
Full Information	**0.309***** (0.050)	**0.339***** (0.061)
Agenda Setting	**-0.122**** (0.059)	**0.218***** (0.080)
Agenda Setter is first beneficiary	**0.176***** (0.024)	**-0.186***** (0.041)
Positive experience	**0.310***** (0.070)	**0.497***** (0.076)
Observations	692	795
# Clusters (MatchingGroups)	7	6
Pseudo R-squared	0.145	0.187

Table 2.12: Trust in the beneficiary of the 1st bill by non-agenda setters benefitting from the 2nd and 3rd bill. Probit regression with clustering on matching groups, robust standard errors (in parentheses), *** p<0.01, ** p<0.05, * p<0.1

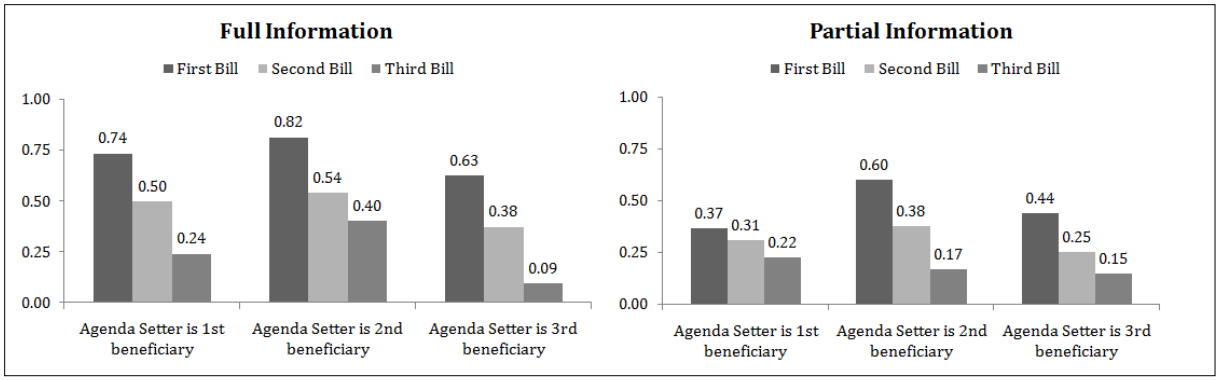


Figure 2.3: Approval rates according to agenda setter's position

particular in the partial information condition where distrust is not necessarily visible. Both results are statistically significant as the regression analysis in Table 2.12 reveals. It shows that beneficiaries of the 3rd bill distrust, and they distrust even more in the partial information condition.

How do non-agenda setters treat the agenda setter when she does not choose the first position? Is the agenda setter additionally rewarded in this case? This would mean that the non-agenda setters accept her bill with a higher probability than if she chose the first position. Figure 2.3 shows the acceptance rates of the bills, conditional on the chosen position of the agenda setter. It shows that in the full information treatment, the first position yields the highest probability to pass a bill followed by the second position and the third. In the partial information treatment, the second position yields about the same probability to receive support as the first position. However, if the agenda setter chooses to put her bill second, she has to support the first bill, which results in a lower expected income. At least, as shown in Table 2.13, if the agenda setter chooses the second position, the highest total number of bills is passed. Consequently, average payoffs are higher in this case than when there is no agenda setter at all. We conclude with Result 2.6.

Result 2.6 Agenda setters choosing to position their bill first on the agenda are punished by those suffering from the chosen agenda. Punishment by third beneficiaries is higher in the partial information treatment but never creates a sufficiently strong threat for agenda setters to abstain from positioning their own bill first.

Let us conclude the results section with some remarks on the efficiency effects of our treatment. As Table 13 shows, transparency of the voting procedure has a positive

	Agenda Setting		No agenda setting possibility	
Agenda setter is..	**Full Information**	**Partial Information**	**Full Information**	**Partial Information**
First Beneficiary	2.10 (0.76)	1.44 (1.25)		
Second Beneficiary	2.36 (0.80)	1.70 (0.99)		
Third Beneficiary	1.68 (0.58)	1.46 (1.16)		
Total	2.16 (0.78)	1.53 (1.17)	1.75 (0.81)	1.14 (1.06)

Table 13: Average number of bills passed according to agenda setter's position (std. dev.)

and significant impact on the number of bills accepted.[32] Agenda setting also increases the number of bills accepted but the increase due to agenda setting is only significant when the agenda setter places her bill second. Agenda setters positioning their bill second give up a potential gain for themselves because it is only reasonable to place the own bill second when the agenda setter also supports the first bill. This suggests that agenda setters who position their bills second do not only intend to increase their own but also others' profits.

2.5 Conclusion

We conducted a three member committee voting experiment to study whether reciprocity among legislators provides additional grounds for agenda manipulation in sequential voting decisions on unrelated proposals. We hypothesized that reciprocity may on the one hand increase agenda setting possibilities through vote trading but on the other hand also reduce them, because reciprocal legislators are likely to punish manipulating agenda setters. In the experiment, a three person committee had to vote on a series of three bills using simple majority rule. We induced symmetric and publicly known preferences over the bills on the agenda. Each member only preferred one bill on the agenda. We did not allow our participants to communicate nor did we provide any commitment devices to trade votes. However, the sequential voting procedure allowed committee members to court for positive reciprocity by voting for bills detrimental to their own preferences but beneficial to a counterpart and hope for reward.

The results underline the importance of reciprocity among legislators for vote trading and agenda control. First, reciprocity enables legislators to trade votes even without commitment devices and in turn, vote trading provides additional grounds for agenda manipulation. Second, agenda setters clearly take reciprocity among legislators

[32] Regression with clustering on matching groups controlling for agenda setting option.

into account when setting the agenda. Third, agenda manipulation is punished by those suffering from it, in particular when the voting procedure is secretive and fourth, agenda setters reward support by other legislators more frequently than non-agenda setters, in particular when the voting procedure is secretive. Thus, in the experiment, agenda setters not only make use of their agenda power but also take responsibility for the committee. Nevertheless, additional reward by agenda setters only weakly increased total payoffs because beneficiaries of the last bill on the agenda discriminated negatively against agenda setters. Total profits were highest when the agenda setter provided a signal and generously chose to position her bill second.

There has been a long debate about what circumstances lead an agenda to become subject to manipulation. Scholars agree that agenda setting possibilities hinge crucially on the information about other committee members' preferences (see e.g. Ordeshook and Palfrey, 1988), the specific voting procedure of the decision making process (e.g. forward vs. backward agendas, see Wilson, 1986) and committee members' voting behavior (sincere versus sophisticated voting). Sophisticated voting is closely related to vote trading (see also Brams and Riker, 1973). Vote trading activities influence agenda setting possibilities by increasing the number of feasible outcomes. We showed that reciprocity – through vote trading – can yield additional agenda setting possibilities. Although agenda setting gave one person the option to exploit the other subjects, we found that discrimination against agenda setters did not suffice to prevent agenda manipulation completely. Also, agenda setters did not fully exploit their counterparts. While in the agenda setting decision, most of them behaved selfishly, in the voting decision, they partly compensated their counterparts by rewarding support more frequently than non-agenda setters.

2.6 Appendix to chapter 2: Instructions (translated from German)

We present a full translation of the instructions for the agenda setting full information treatment (ASFI). Instructions for *RAFI* are identical, except for the decision on the agenda, which was determined randomly by the computer. In the agenda setter treatments and random order treatments with partial information we modified the instructions at the relevant parts. We indicate these modifications after presenting the translated instructions for ASFI. The general information is identical in all treatments.

General information (Participant A)

Today you take will part in an economic decision making experiment. If you read the following instructions carefully, you will be able to earn money additional to your show-up fee of 2 euros. Therefore it is important that you read the instructions completely.

For the entire duration of the experiment, communication with other participants is not allowed. We therefore ask you not to talk to each other. If you have problems understanding the experiment, please have a second look at the instructions. If you still have questions, please give raise your hand. We will come to your cubicle and answer your questions personally. During the experiment, we do not use the term euros, we use the term points. The number of points you earn in the experiment are converted into euros with the following exchange rate.

1 point = € 0.20

At the end of the experiment, you will receive the 2 euro show-up fee plus the equivalent of all points received in the experiment in cash. The following pages will explain the experiment in detail. At the end of the instructions we added some control questions to help you to understand the sequence of events. The experiment does not start until all participants solved the control questions and are completely familiar with the course of the experiment.

Summary

This experiment has 12 periods. In each period you will form a group with two randomly determined participants. At the beginning of a period each participant receives 4 points. Then you and the other two members of the group decide on three different bills. The bills affect the points of each group member. A period ends when the group has made a decision on all three bills. Then, a new period starts. You form a new group with two randomly chosen participants. Altogether you decide on three bills in 12 periods. After the final period you will see a summary table on screen showing your points earned in each period. **At the end of the experiment you receive the 2 euro show-up fee plus the euro equivalent of points earned in cash.**

The Experiment

In this experiment we will talk about three different participants, Participants A, B and C. **You are Participant A.** In each period you form a group of three members with a randomly chosen participant B and a randomly chosen participant C.

At the beginning of each period each participant receives 4 points.

There are three bills to be voted on in each period. We label them Bill A, B and C, respectively. The group decides sequentially on the three bills. If a majority (at least two members of the group) accepts a bill, it is passed.

The bills in detail: Each of the three bills yields 6 additional points for one group member but subtracts two points from each of the other two members.

Bill A: Participant A receives 6 additional Points, 2 points are subtracted from Participant B and C (each).

Bill B: Participant B receives 6 additional Points, 2 points are subtracted from Participant A and C (each).

Bill C: Participant C receives 6 additional Points, 2 points are subtracted from Participant A and B (each).

Each bill can be accepted or rejected by the group. Thus it is possible that more than one bill is accepted or rejected.

The order in which the bills are voted on is determined by participant A.

In the control treatments: [The order in which the bills are voted on is determined randomly.

The six possible sequences are:

Sequence	**1st Bill**	**2nd Bill**	**3rd Bill**
1	Bill A	Bill B	Bill C
2	Bill A	Bill C	Bill B
3	Bill B	Bill A	Bill C
4	Bill B	Bill C	Bill A
5	Bill C	Bill A	Bill B
6	Bill C	Bill B	Bill A

At the beginning of each period, namely before the decision on the first bill, the sequence of bills is displayed on your computer screen.]

The course of the experiment is the following:

Step 1 – Decision on the order of bills

Participant A determines the order in which the three bills will be voted on. After his decision all participants of the group see the chosen order.

Step 2 – Overview

Participants of each group see an overview of the consequences of each bill and the order of bills.

Step 3 – Voting

In this step you see the current bill and enter whether you vote for or against the current bill.

Step 4 – Result

After all participants of a group have voted on the current bill, they see the outcome of the vote for this bill. A bill is accepted when the majority of participants voted for the bill. That is, the bill is accepted when at least two participants in a group voted for it.

[You see whether a bill was accepted and who voted for or against the bill. **]**

Only in full information treatments

Afterwards, the group votes on the second bill, i.e. you see the next bill and decide on voting for or against it (see step 2). Then you see the outcome of the vote for the second bill (see step 3). Then you vote on the third bill and see the result.

After the vote on the third bill took place, you are again randomly matched with two participants and form a new group.

When the new period starts, no participant receives any information on your voting behavior from previous periods. Also, you do not receive any information on the voting behavior from previous periods of the new group's participants. Neither before nor after the experiment will you receive any information about your counterparts' identities. The randomly selected participants who interact with you do also not receive any information on your identity.

Payment

At the end of the experiment you will receive the 2 euro show-up plus the euro equivalent of points reached in cash.

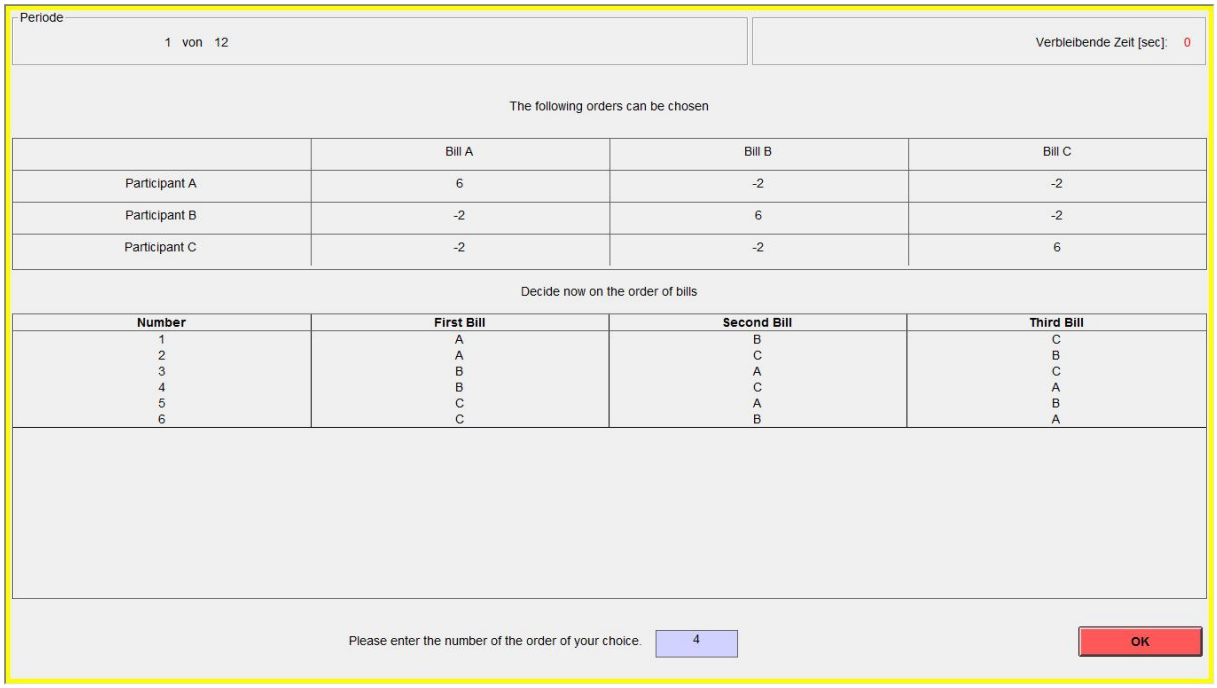
We now present an example which will help you to understand the course of the experiment on screen in more detail. At the end of this example you will find some control questions. Please write down your answers to these questions. **Your answers to these questions will not affect the amount of money you receive at the end of the experiment.**

Course of the experiment on the computer screen– an example

Step 1 – Decision on the order of bills

First Participant A sees the following screen:

A screenshot of Step 1 (in this example):



Participant A chooses the order of bills by entering a number and pressing the "ok" - button. In this example we assume that participant A decides for order "4". That is, the group votes first on bill B, then on bill C and finally on bill A.

Step 2 - Overview

After participant A's choice all participants in the group see the following screen.

The first line in the table shows the [randomly] selected order of bills (in this example: 1st Bill B, 2nd Bill C and 3rd Bill A).

In control treatments without agenda setting

Below we present how each bill will change each participant's number of points if a majority accepts this bill.

In this example:

1st Bill B: Participant B receives 6 additional points, 2 points are subtracted from Participants A and C.

2nd Bill C: Participant C receives 6 additional points, 2 points are subtracted from Participants A and B.

3rd Bill A: Participant A receives 6 additional points, 2 points are subtracted from Participants B and C.

If a bill is not accepted by a majority, it does not affect the points of any participant.

Step 2 – Voting

Now bills are now voted on in the previously displayed order, In our example, first Bill B, then Bill C and finally Bill A.


Periode

1 von 1

	1st bill B	2nd bill C	3rd bill A
Change in payoff for Participant A	-2	-2	6
Change in payoff for Participant B	6	-2	-2
Change in payoff for Participant C	-2	6	-2
Decision by participant A			
Decision by participant B			
Decision by participant C			
Outcome of the vote			

The first bill to be voted on is: Bill B

Please vote now on the first bill: accept / reject

OK

You decide whether to accept/reject the current bill and click the "OK" Button. After all participants of the group have made their decision, the voting result is displayed.

Let us assume that, in our example, Participant B accepted the 1st bill, you (Participant A) and Participant C, however, rejected the 1st bill. The result is then displayed on your computer screen (Step 3).

Step 3 – Result

The table again shows the order in which the bills are voted on [and who voted for or against a bill]. In the last line you see whether a bill was accepted or rejected by the majority of the group. In our example a majority (you and participant C) voted against the first bill. Thus in this example the table shows that the 1st bill was rejected by a majority. Consequently, the points of all participants in your group are not affected. By clicking on the "continue" button you will come to the next decision.

Now voting on the second bill begins. Then you see the result of the group's decision on screen. Let's assume a majority of the group accepted the second bill.

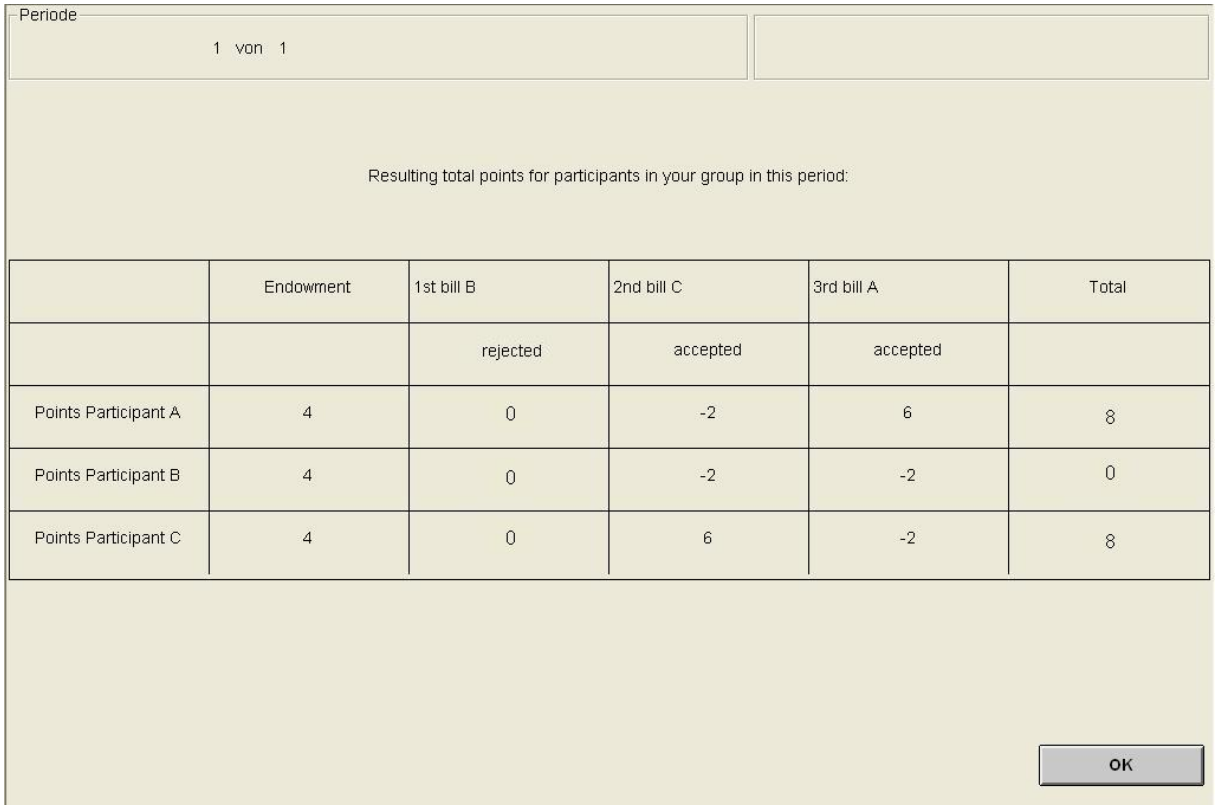
Then voting on the third bill starts. You see the third bill and decide for or against it. Let's assume for our example that again a majority accepted the third bill.

We continue with **Step 4**.

Step 4 – Result

At the end of a period you will see a summary table showing points received by you and your group members.

In the following we explain how the points received in the period of our example are calculated. Points received at the end of a period are calculated as follows:


Periode

1 von 1

Resulting total points for participants in your group in this period:

	Endowment	1st bill B	2nd bill C	3rd bill A	Total
		rejected	accepted	accepted	
Points Participant A	4	0	-2	6	8
Points Participant B	4	0	-2	-2	0
Points Participant C	4	0	6	-2	8

OK

The table displays again the order in which bills where voted on. Additionally you see in the second line whether a bill was accepted or rejected by the majority of the group. In our example the first bill was rejected, whereas the other two bills were accepted. Endowment is 4 points.

The first bill did not affect the points received by participants in this group, because it was rejected. The second bill was accepted and yields participant C 6 additional points, whereas 2 points are subtracted from participants A and B each. The third bill was accepted too in our example. It yields 6 additional points for participant A, and subtracts 2 points from each of the other two participants.

Points received at the end of the period by each participant are calculated as follows:

Points for Participant A = 4 + 0 - 2 + 6 = 8

Points for Participant B = 4 +0 - 2 -2 = 0

Points for Participant C = 4 + 0 + 6 – 2 = 8

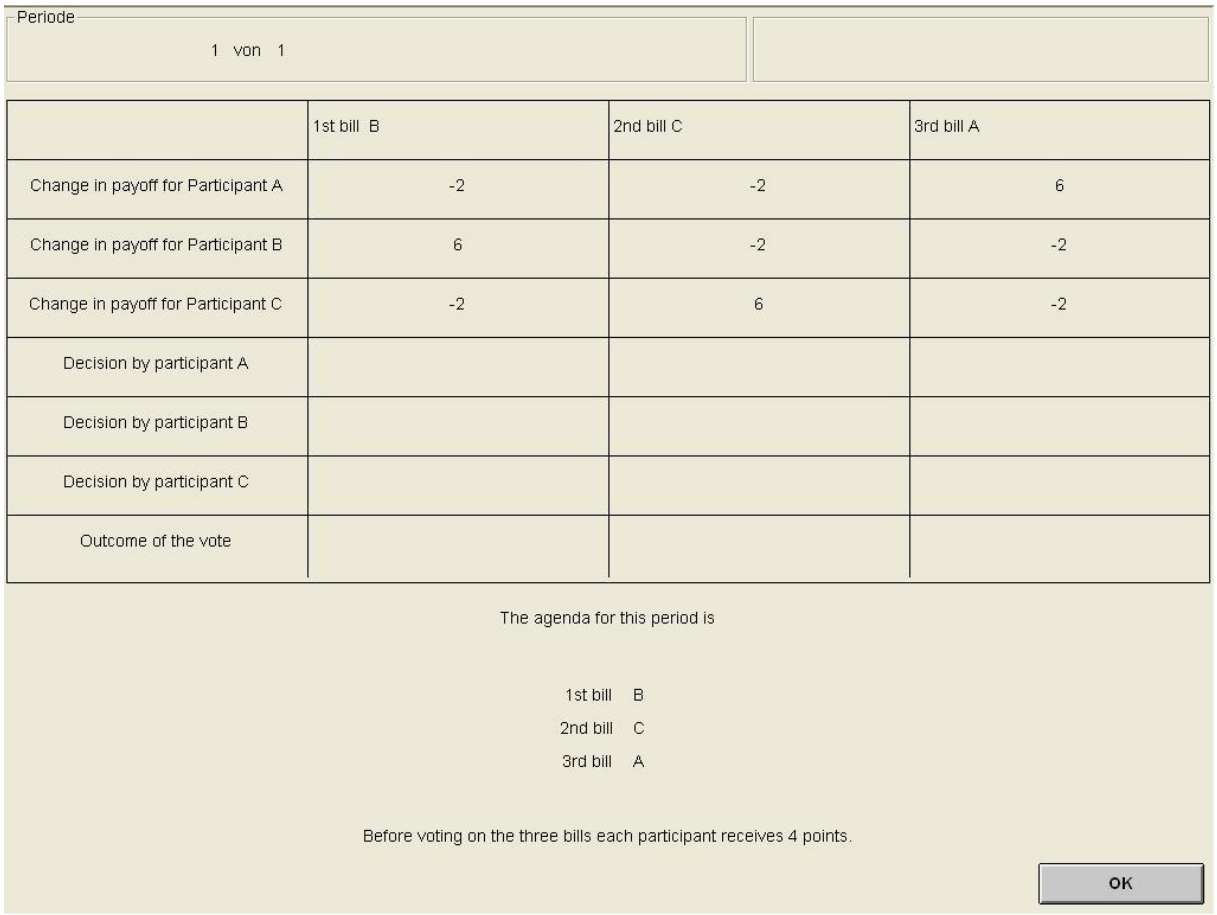
After clicking the “OK” – Button, you are randomly matched into a new group.

Control questions

Please read the new example on this page and answer the control questions.

Your answers to these questions will not affect the amount of money you receive at the end of the experiment.

Example:



Periode

1 von 1

	1st bill B	2nd bill C	3rd bill A
Change in payoff for Participant A	-2	-2	6
Change in payoff for Participant B	6	-2	-2
Change in payoff for Participant C	-2	6	-2
Decision by participant A			
Decision by participant B			
Decision by participant C			
Outcome of the vote			

The agenda for this period is

1st bill B
2nd bill C
3rd bill A

Before voting on the three bills each participant receives 4 points.

OK

Assume further…
You accept the 1st and 3rd bill.
Participant B accepts the 1st and 3rd bill.
Participant C accepts the 1st and 2nd bill.

Which bills are accepted by a majority of the group? _____

How many points do you receive in this period? _____

How many points does participant B receive in this period? _____

How many points does participant C receive in this period? _____

2.7 References to Chapter 2

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3 Competition within firms

3.1 Introduction

When evaluating the competitiveness of a market, cartel authorities assume that subsidiary companies with the same parent do not compete with each other or with their parent. According to US antitrust law subsidiaries in a single entity pursue the goals of the parent. Subsidiaries in a single entity are thus not legally capable of conspiring with their parent firm[33] or with each other under Section 1 of the Sherman Act, because any conspiracy between firms by definition requires at least two separate firms involved. Similarly, antitrust law in the EU presumes that firms belonging to the same owner always act in the owner's interest.[34]

If subsidiaries commit a market infringement, a parent firm will at least have the chance to prove that the subsidiary in fact did act independently. This possibility does not exist in the field of merger control. When judging whether to allow or forbid a merger, cartel authorities have to forecast whether the planned merger will lead to a concentration of the market structure.[35] If the merger is generally considered to reduce competition in that market, it will not be permitted. Authorities assume that firms who are allowed to use their market power will always do so. Firms do not have the option to prove that the merger will not affect competition and that they are planning the merger for other reasons such as efficiency improvements only.

The prediction of perfect cooperation between merged firms seems a strong simplification and probably does not match the variety in actual behavior. Legal ownership and actual control may in fact be effectively separable, no matter whether this separation occurs intentionally or by inability of the parent firm to control subsidiaries. For example, high monitoring costs may impede direct control over subsidiaries by parent firms. Instead of direct control, parent firms may use incentive schemes to coordinate their subsidiaries. We are interested in how such intra-firm incentives evolve and whether these incentives affect competition among subsidiaries. Do endogenously determined incentive schemes eventually lead to collusive behavior among subsidiaries as presumed by law? In order to answer these questions we

[33] See Copperweld Corp. v. Independence Tube Corp., 467 U.S. 752, 771 (1984).

[34] See the AKZO Nobel case: "In the specific case of a parent company holding 100% of the capital of a subsidiary which has committed an infringement, there is a simple presumption that the parent company exercises decisive influence over the conduct of its subsidiary.'' (European Court reports 2009 Page I-08237, Case C-97/08 P, ad para 60).

[35] Commission, guidelines horizontal cooperation agreements, ABl. EG C31/5, Rz. 4.

designed a laboratory experiment in which a non-producing parent firm sets intra-firm incentives to coordinate its producing subsidiaries by redistributing profits.[36] We study the intra-firm coordination problem in an unambiguous way and isolate the effects of endogenously determined incentives: Subsidiaries of a parent firm operate in a Cournot oligopoly excluding other competitors.

We find that almost all parent firms converge to a specific incentive scheme, mainly to one of two simple profit sharing rules: proportional and equal profit sharing. Subsidiaries operating under equal profit sharing rules are able to collude whereas subsidiaries operating under proportional profit sharing generate profits close to the Cournot level. Our results show that the prediction of perfect cooperation between subsidiary firms belonging to the same owner might be too restrictive. Only around half of the firms in our experiment collude. The other half of firms instead maintain Cournot competition between their subsidiaries.

Our paper contributes to the discussion in the literature on what constitutes a single entity. Since the *Copperweld*[37] case there have been many attempts of firm agglomerates to be declared as a single entity, because this legalizes otherwise forbidden agreements between them. Some of them were successful in receiving the desired declaration as a single entity even though they were not under perfectly concentrated ownership and control.[38] Prominent examples for disputed decisions are litigations involving sports leagues, where the joint marketing of the intellectual properties of the different teams was only sometimes considered not to be conspiratorial according to Section 1 of the Sherman Act, because the "sum" of teams could be considered a single entity (see American Needle v. NFL).[39] Our paper adds to the discussion on the definition of a single entity in two ways. First, our experiment illustrates that not only the formal ownership and control structure of the firm agglomerate determines their coordinated action, but also the implicit, non-written incentives set by the parent firm through ex-post redistribution. Second, we provide a flexible experimental tool suitable to compare

[36] This hierarchical division of owners (parent firms) and decision-makers (producing subsidiaries) also relates our work to a study on strategic delegation in a Cournot duopoly by Huck, et al. (2004a).

[37] See Copperweld Corp. v. Independence Tube Corp., 467 U.S. 752, 771 (1984).

[38] See Williamson (2009). He provides a comprehensive overview over the decisions of courts and classifies them in a two-stage single entity test. Broadly speaking, the firm agglomerate cannot only be categorized as a single entity, when there is in fact concentrated ownership and control, but also when the court comes to the conclusion that the firms are no potential competitors, for example because they contribute complementary inputs.

[39] See also the discussion in Edelman (2008).

behavior under different incentive systems. This may support judgment in future cases on whether a specific agglomerate of firms constitutes a single entity or not.

The use of the Cournot game in our experiment relates our study to research on group decision making in experimental Cournot oligopolies by Raab and Schipper Raab and Schipper (2009). While focusing on the comparison of individual decisions and the decisions of groups, the authors also contrast treatments with different exogenously determined incentive schemes (profit sharing rules) within firms. Raab and Schipper find no significant differences in production quantities of firms under the different incentive schemes. However, this result was obtained by implementing an additional effort cost in the proportional sharing treatment but not in the treatments with equal profit sharing. The introduction of additional costs was necessary in order to maintain the same theoretical predictions for individual and group treatments, but it makes the comparison of intra-firm incentives difficult. We suppose that proportional sharing should generally yield higher production quantities in a Cournot game but additional effort costs (born individually) may induce risk averse subjects to choose lower production quantities under proportional sharing than under equal profit sharing. Further, the weak treatment differences in Raab and Schipper may also result from the use of an option in subjects' profit calculators that allowed the automatic calculation of the best response. Requate and Waichman (2011) have recently shown that the addition of a best-response option to the profit calculator tends to increase the aggregate output to the Cournot level, the level of production observed by Raab and Schipper in both treatments. In our experiment the individual's cost structure does not vary with the implemented profit sharing rule and no best-response option is provided in the profit calculator. Also, we let parent firms decide on the distribution of profits among their subsidiaries instead of exogenously manipulating the profit sharing rule. This allows us to study the endogenous evolution of intra-firm incentives when the firm's subsidiaries compete on the same market.

Intra-firm incentives have also been studied in Bertrand oligopoly experiments. Bornstein and Gneezy Bornstein and Gneezy (2002) and Bornstein et al. (2008) underline the suggestion that intra-firm incentives matter for coordination within firms as well as for coordination among different firms in the same market. They find that incentives within the firm matter for the dynamics of prices in Bertrand duopolies. The specific structure of their game reverses the effects of equal and proportional profit

sharing compared to Cournot games. Proportional profit sharing in their context means an allocation according to subsidiaries' own asking prices (the prices of intermediate products which add up to the total price of the final product). Sharing in proportion to asking prices thus creates a free rider problem within the firm which tends to increase market prices over time. This is in contrast to the effect of proportional sharing in the Cournot case we consider, in which proportional sharing leads to higher production quantities (and thus to a lower market price).

The remainder of this paper is organized as follows. In section 3.2 we introduce the experimental design and briefly discuss the theoretical benchmark solutions of this framework. Section 3.3 presents the results of the experiment and section 3.4 concludes.

3.2 Design and Procedures

3.2.1 Design

We study a linear symmetric Cournot oligopoly market in which four subsidiaries of the same parent firm operate. From Huck et al. (2004b) we know that experimental Cournot oligopolies with four firms competing against each other are never fully collusive but produce aggregate quantities at or above the Cournot outcome. We consider whether and to what extent such markets become collusive if the Cournot firms do not operate independently but are governed by a parent firm. In our experiment, this parent firm is not involved in production. Its task is to decide after each round about the distribution of the group profit among the four subsidiaries. The parent firm has an incentive to maximize the joint profit of the four subsidiaries, because it receives a predetermined fixed share (20%) of the group profit. Our design focuses on the evolution of incentives within the firm and how intra-firm incentives affect coordination among subsidiaries. In order to exclude any confounding effects from market competition, no firms other than the four subsidiaries operate in the market.[40]

Market demand in the experiment was simulated according to the function $P = max\,(100 - Q, 0)$, where P denotes the market price and Q the total quantity produced by the four subsidiaries ($Q = \sum q_i$ with q_i denoting the quantity produced by subsidiary i). Total production costs were equal to $C = \sum c(q_i)$. The instructions[41]

[40] Examples for such a market structure can be found in the real world for instance in major sports leagues such as Major League Soccer (MLS) (see Edelman, 2008).

[41] For a translation of the instructions see the appendix of this chapter.

provided subjects with verbal information about demand and cost conditions. Additionally we offered a "profit calculator" on their computer screens at the beginning of each round which allowed all subjects to calculate how changes in individual quantities affected profits.

The experiment consisted of 25 rounds. At the beginning of the experiment five subjects were randomly matched to form a group. One of the five was randomly selected to be the parent firm. Group composition and roles remained the same until the end of the experiment. We used a partner matching procedure to study the evolution of incentives within the firm and the corresponding changes of profits over time.

We structured each round in three stages: a production stage, a redistribution stage and a summary stage. In the production stage each subsidiary chose a production quantity. We approximated a continuous action space for subsidiaries by allowing for one decimal point when entering quantities between 0 and 100. The production costs for each subsidiary was $c(q_i) = q_i$ and consequently the profit generated by a subsidiary was equal to $\pi_i = (P(Q) - 1)q_i$. Note that because the parent firm could redistribute the sum of profits of the four subsidiaries at the end of each round costs were not necessarily being born privately by the subsidiaries. Further note that overproduction ($Q > 99$) could lead to a loss. When the loss occurred within the first rounds, subjects' show-up fees were converted into points (1000 points = 1 euro) and automatically invested to cover the loss. In later rounds profits from past periods covered losses.

In the redistribution stage, parent firms saw the profits (or losses) generated by each subsidiary and the total quantity produced. 20% of total profit (or loss) was automatically transferred to the parent. Parents then decided freely on how to assign the remaining 80% to the subsidiaries (i.e. parent firms could redistribute but could not increase profits of specific subsidiaries by creating "new" losses for other subsidiaries).

In the summary stage, all subjects saw the subsidiaries' individually generated profits and the details of the redistribution decision of the parent firm. After the summary stage the next round started immediately. The whole procedure was common knowledge.

3.2.2 Procedures

We conducted all four sessions at the LakeLab (University of Konstanz, Germany). The experiment took place in June and July of 2010. 20 participants participated in each

session, thus we had a total of 80 participants. Each participant sat at a randomly assigned and separated PC terminal and was given a copy of instructions. A set of control questions was provided on-screen to ensure the understanding of the game. If any participant answered incorrectly, the experimenter provided an oral explanation to the participant. No form of communication between participants was allowed during the experiment.

Participants received a show-up fee of 2 euros (2.50$ at that time). The experiment took about one hour and 30 minutes, average income was about 13.40 euros ($16.75). The experiment was programmed and conducted using z-Tree (Fischbacher, 2007). We recruited participants using the online recruiting system ORSEE (Greiner, 2004). Participants were part of the LakeLab subject pool, consisting of undergraduate and graduate students of all fields of study.

3.2.3 Theoretical Benchmarks and Behavioral Predictions

The symmetric Cournot Nash equilibrium for the experimental markets is characterized by an individual equilibrium output of $q_i^N = 99/(n+1)$ and an individual equilibrium profit of $\pi_i^N = (q_i^N)^2$. The total Cournot equilibrium output in this case is $Q^N = 99n/(n+1)$ and industry profit is given by $\pi^N = n(q_i^N)^2$. Other benchmark outcomes are the collusive case with $Q^C = 99/2$ and the rivalistic (competitive) outcome with $Q^R = 99$. Table 3.1 summarizes the numerical values of these benchmarks for $n = 4$ subsidiary firms.

Actual quantity choices in the experiment may depend on which profit sharing rule parent firms employ. As parent firms have an incentive to maximize total profit we expect that they will use ex post redistribution as an instrument to coordinate quantity choices of their subsidiaries. In order to maximize the joint profits, redistribution has to fulfill two conditions: First, it has to be systematic in the sense that subsidiaries can predict from past experience how future redistribution will depend on their quantity choices. Eventual coordination can only be successful if the parent firm's past allocations

	Total Quantity	Group Profit
Collusion	49.5	2450.25
Cournot-Nash	79.2	1568.16
Perfect Competition	99	0

Table 3.1: Theoretical benchmarks with n=4

predict future allocations and if subsidiaries have the according beliefs. As redistribution is the only means of communication for the parent firm, we expect that (after some initial unsystematic trials) parent firms will persistently use one specific profit sharing rule. A first step in our analysis of behavior will be finding out whether there is convergence to such systematic behavior. Second, a systematic redistribution by the parent firm has to create appropriate incentives to coordinate the subsidiaries. Parent firms can communicate their agreement or disagreement with certain quantity choices by punishing or rewarding subsidiaries when reallocating profits.[42]

There are two particularly prominent sharing rules which we expect to observe predominantly: proportional and equal profit sharing. Given the appealing fairness of the allocation rule "everybody gets back what he or she contributed to the joint profit of the group" we expect that some parent firms will implement proportional sharing rules which reward high contributions to the joint profit with a high share.[43] Ongoing redistribution proportional to contributed profits gives all subsidiaries the same incentives as firms in a Cournot oligopoly and will therefore induce quantities close to the Cournot equilibrium.

Parent firms who understand the detrimental effect of proportional profit sharing will seek allocation rules avoiding this effect. Maximization of the joint profit can be reached with any allocation rule that positively relates the individually assigned shares to the industry profit but not to the contributed profit share. The simplest example for such an allocation rule (and therefore the one we will focus on in the following) is an equal sharing of profits such that each subsidiary receives 20% of the joint profit. The relation of the assigned share to the contributed share could also be uncorrelated, which we expect to be rare because such rules may lead to confusion among subsidiaries or induce envy among them. When parent firms face excessive competition among their subsidiaries they may also use an anti-proportional sharing rule, which punishes firms with high production quantities and rewards firms with low production quantities. Continuing anti-proportional allocation, however, would make quantities converge to zero, which leads us to expect rare use of this rule in exceptional cases of dramatic

[42] Incentives set by the parent firms can also be interpreted as indirect communication between the parent firm and its subsidiaries. For an experiment on the effect of direct communication between firms on collusion, see Andersson and Wengström (2007) and also Fonseca and Normann (2011).

[43] Rey Biel (2008) shows that relative rewards can be used by parent firms to provide effort incentives to inequity averse subsidiaries. However, this result holds only in a context with individual effort costs born by the subsidiaries. In our design, in contrast, it has a detrimental effect on total profit.

overproduction only. Equal sharing, in contrast, sets total profit maximizing incentives independent of the current quantities.

To summarize, different incentives require different levels of reasoning (see also Arruñada and Casari, 2007). A rule that yields "correct" incentives requires more cognitive effort or ability from the parent firm than just giving back what a subsidiary contributed to the joint profit. As we expect that cognitive effort and ability vary across subjects, we predict convergence to different sharing rules. We expect equal sharing rules to lead to collusion and proportional profit sharing to foster competition among the subsidiaries.

3.3 Results

Average profits provide a first descriptive insight as to whether parent firms in our experiment are able to make their subsidiaries optimize profits jointly. Figure 3.1 illustrates that at an aggregate level firms do not achieve collusion. We observe 10 out of 16 firms with average profits closer to the Cournot than the collusive level and only 6 with average profits closer to the collusive level. We can thus reject the hypothesis of perfect collusion among subsidiaries of the same parent firm in our experiment (Binomial test, p-value < 0.01).[44] We obtain result 3.1.

Result 3.1 There is competition among subsidiaries of the same parent.

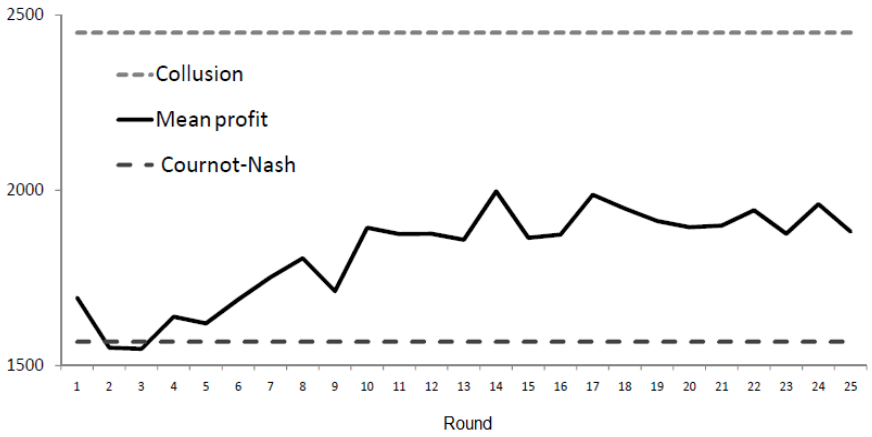


Figure 3.1: Mean profit over time

[44] For this test we used 95 percent collusion as the benchmark, allowing for 5 percent decision errors. However, p-value < 0.01 still holds as long as one expects at least more than 71 percent of subsidiaries to be closer to the collusive than to the Cournot level.

Coordination failure between the subsidiary firms can have two main reasons. Either the parent firm sets incorrect incentives or the subsidiaries do not respond to correct incentives accordingly. We cluster redistribution rules employed by the parent firms (and therefore the incentives set by them) into two broad categories. "Correct" incentives imply a sharing rule that gives subsidiaries incentives to maximize joint profits, e.g. equal profit sharing.[45] "Incorrect" incentives, in contrast, are set with any sharing rule which rewards a high contributed profit with a higher assigned profit share when the total production quantity exceeds the optimal level (49.5) or rules which induce lower production quantities when total quantity lies below the optimum. We concentrate on the first case, because in all groups, quantities (if at all) converge from above the collusive level. To classify the actual sharing rules employed by our parent firms into "correct" and "incorrect" incentives, we judge the distance to a specific sharing rule in the following way. In each round we calculate the sum of differences in percentage points of the actual share each firms receives to the point prediction of the equal (DiffE) and proportional (DiffP) sharing rule, $\text{DiffP}_i = \sum_j |b_j - a_j|$ and $\text{DiffE}_i = \sum_j |0.25 - a_j|$, with i = parent $_i$ and j = subsidiary$_j$, a_j = assigned profit share, $b_j = \frac{q_j}{Q}$, q_j = quantity produced by subsidiary$_j$ and Q = total quantity produced by the four subsidiaries.[46]

In a next step, we compare for each round and each parent firm, which of the two values DiffE and DiffP is smaller. Taking the average over all rounds, we derive for each parent firm a measure whether its redistribution scheme is on average over all rounds "closer to" an equal or a proportional sharing rule. This gives us a first, broad measure for different profit sharing rules. Next, we relate the relative closeness to one of the two sharing rules to the average industry profits realized by the firms. Table 3.2 shows that average industry profits of parent firms employing the equal sharing rule in more than 50% of their decisions are in significantly more cases closer to the cartel level than profits by firms employing the proportional sharing rule in more than 50% of their decisions (Fisher exact test, p-value <0.05).

[45] In one of the groups the parent firm did not allocate profits equally in every round but took turns in giving the whole group profit to one of the subsidiaries in cyclical order. We treat this rule as "correct'' or equal sharing as well.

[46] Note that the two sharing rules make the same prediction when all subsidiaries produce exactly the same amount. However, we rarely observe this case. Differences between minimum and maximum quantities are smaller than two for only 1.5 percent of our observations.

	Industry profit closer to	
Sharing rule closer to	Cartel	Cournot
equal	6	4
proportional	0	6

Table 3.2: Sharing rules and industry profits of groups

Result 3.2 Average profits of firms employing a proportional sharing rule are always below the collusive level. Profits close to the collusive level are only attained by firms employing sharing rules close to equal sharing.

Learning within sessions might bring all groups to combined profit maximization outcomes sooner or later. Figure 3.1 illustrates that mean industry profits are higher in the second half of the game than profits in the first rounds. We now turn to the question of whether it is convergence to specific rules which eventually leads to higher profits. To find out whether certain groups converge to the equal sharing rule we run 16 independent regressions (one for each group) to see whether the distance of the actual to the equal sharing rule DiffE gets smaller over time. With $\text{DiffE}_t = \beta \cdot t + \varepsilon_t$ a β significantly smaller than zero indicates that the distance of the actual to the equal sharing rule is decreasing over time. In this case, we say that the group is converging to equal sharing.[47]

According to this clustering we can treat eight groups as converging to equal sharing and six groups as not converging to the equal sharing rule. According to our convergence measure two remaining groups converge to both rules.[48] Figure 3.2 shows the groups' profits over time, separated by the above convergence criterion. The pictures indicate that parents converging to an equal profit sharing rule succeed in increasing profits over time whereas profits of groups with distribution rules not converging to equal sharing follow no time trend. We also regress group profits for the two clusters on the round index. Table 3.3 shows that profits are significantly increasing over time only for groups converging to an equal sharing rule.

[47] As convergence (if any) will stop once the profit maximum has been reached, we restrict the convergence analysis to sharing rules in the first 15 rounds of the game. As a robustness check we also analyzed convergence within the first 10 rounds. Using this criterion we identify 6 firms, which converge to an equal sharing rule. This does not qualitatively change the results reported in Table 3.3.

[48] The two groups are not contained in Figure 3.2. The regression results in table 3.3 do not qualitatively change if we include these two groups into the cluster of groups converging to equal sharing.

Result 3.3 Groups with distribution rules converging to an equal sharing rule have increasing profits over time. Profits of groups with distribution rules not converging to equal sharing follow no time trend.

The "closer to" and the convergence measure are relatively broad ones as they do not account for the absolute distance between the prediction of the (equal or proportional) benchmark and the actual sharing rule. A certain distribution rule might be relatively closer to one of the two rules but far away from the predictions of both rules in absolute

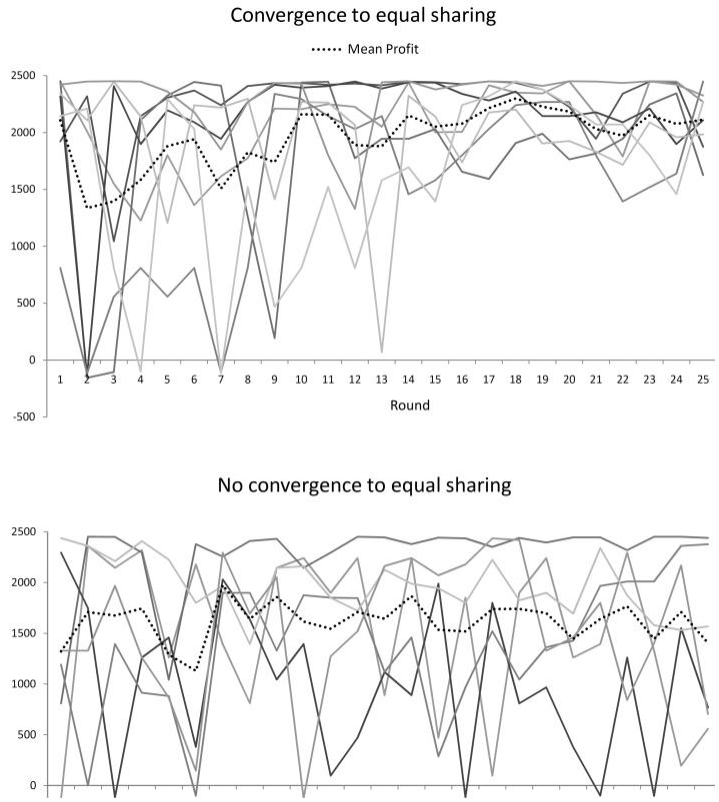


Figure 3.2: Development of profits over time (by convergence to equal sharing)

	Not converging	Converging	Converging (incl. group 6 & 9)
Round	1.629	23.49***	22.75***
	(8.904)	(6.448)	(5.880)
Constant	1,593***	1,652***	1,652***
	(133.2)	(116.5)	(104.5)
Observations	150	200	250
R-squared	0.000	0.075	0.064

Table 3.3: Regression results (OLS): group profit and convergence to the equal sharing. Robust standard errors in parentheses; *** p < 0.01, ** p < 0.05, * p < 0.1

	Group profit in t			
	(1)	(2)	(3)	(4)
DiffP - DiffE in t-1	848.3***	545.7***		
	(135.7)	(170.3)		
DiffP - DiffE in t-2		484.6***		
		(145.3)		
DiffP in t-1			545.0***	309.4
			(154.9)	(226.2)
DiffP in t-2				221.8
				(189.0)
DiffE in t-1			-1,170***	-920.9***
			(211.5)	(218.8)
DiffE in t-2				-750.7***
				(203.6)
Constant	1,782***	1,779***	1,909***	2,006***
	(35.79)	(35.09)	(52.21)	(50.88)
Observations	384	368	384	368
R-squared	0.130	0.157	0.150	0.204

Table 3.4: Regression results (OLS): group profit explained by past profit allocation rules. Robust standard errors in parentheses; *** p < 0.01, ** p < 0.05.

terms. To capture the absolute difference between predicted and actual share, we use an OLS regression explaining current profits with the sharing rule in the previous two rounds. "DiffP - DiffE" denotes the relative closeness to one of the allocation rules. The closer (farther) the sharing rule in a certain round is to equal (from proportional) sharing, the larger is the value of this difference. We therefore expect a positive impact of "DiffP - DiffE" on group profit. "DiffP" and "DiffE" measure the impact of closeness to the two allocation rules separately. We expect that a large difference to equal sharing in t-1 (and t-2) has a negative impact on group profit in round t and that a large difference to proportional sharing has a positive impact on group profit. The regression specifications (1) and (3) include only allocation rules from the previous round while specifications (2) and (4) take t-2 into account as well.

All results point in the expected direction and most of them are strongly significant (see Table 3.4). Sharing equally in the previous round increases profits in the current round (note that the negative sign of DiffE indicates that profits decrease the farther an observed sharing rule is away from equal sharing). Sharing proportionally leads on average to an increase in quantities and thereby reduces profits. The positive effect of equal sharing on profits is stronger than the negative effect of proportional sharing. Furthermore, with proportional sharing, the effect of "DiffP in t-1" is only significant if we do not include t-2 in the regression as well. This indicates that

proportional sharing on group profit in round t-1 is highly correlated with proportional sharing in t-2 (Spearman's $\rho = 0.68$, p-value <0.01). With equal sharing, the effect rather accumulates over time. Including "DiffE in t-2" in the regression reduces the impact of "DiffE in t-1" only to a small extent and the twice lagged variable is highly significant itself. We conclude with Result 3.4.

Result 3.4 The closer the profit allocation is to equal sharing in one round, the higher are group profits in the two following rounds.

3.4 Discussion and conclusion

We investigated the evolution of incentives set by parent firms for their subsidiaries and their role for competition among subsidiaries in a Cournot oligopoly experiment. We observed most parent firms converging to specific profit redistribution schemes. Parent firms chose simple profit sharing rules which created strong incentives for subsidiaries: Most of the parent firms implementing equal profit sharing rules were able to coordinate their subsidiaries to act in the firm's joint interest. When the sharing rule employed by the parent firm was proportional to subsidiaries' contributions to the joint profit, subsidiaries did compete. Furthermore, our data showed that the repeated use of a specific redistribution scheme helps to circumvent the problems caused by lack of commitment. Incentives set by parent firms were in fact decisive for the degree of competition among subsidiaries.

The interpretation of our results depends on the reader's viewpoint. From a regulator's perspective it is promising that half of the firms in the experiment did not manage to collude but it is worrying that the other half did. From a coordinating parent firm's perspective the good and bad news are reversed: It can be considered good news that around half of the firms manage to coordinate even under difficult circumstances. The other half, however, did not manage to coordinate, sometimes even despite the parent firm setting correct incentives for cooperation among the subsidiaries. Accordingly, the implications of our experiment are twofold.

From a regulator's perspective our experiment recommends taking planned intra-firm incentives into account when investigating whether a planned merger will lead to a concentration of the market structure. In light of our results the assumption of perfect cooperation between merged firms seems a strong simplification. For example, high monitoring costs may impede perfect control over subsidiaries by parent firms.

Instead of direct control, parent firms may use incentive schemes to coordinate their subsidiaries. The simple presumption of collusion among subsidiaries of the same parent may then be too restrictive. Instead, the likelihood of concerted action depends on the incentives for the subsidiaries within the merged firm. This relates our study also to the concept of league based common property systems discussed in Edelman (2008, p. 900-903). A prominent example for league based common property systems is the original model for Major League Soccer ("MLS"). It was designed to serve as a "single entity" league for anti-trust advantages by generating common interests of clubs playing in the league. Its incentives were exactly designed such that the league's "subsidiaries" (the clubs) maximize the league's instead of individual clubs' profits. However, eventually the MLS became a mix-mode model because wealthy investors did not want to become "faceless" but own champions, which was not possible in the common property system.[49]

From an organizational behavior perspective the results illustrate the trade-off corporate management faces when deciding on incentive schemes for the executives of the different divisions. Bonus payments at the executive level typically depend on the division's profit and on the total profit of the company as well.[50] The organization of divisions as independent profit centers is quite popular as it allows the parent firm to determine the subsidiaries' individual profitability and reduces monitoring costs.[51] However, this organizational structure will not provide incentives to maximize the total profit of the company if divisions compete. Our experiment can reproduce this trade-off at a very stylized level and may therefore serve as a basic design to study the effects of different variants of such combined bonus payment rules in the future.

[49] It was envisaged that investors own shares of the league entity and centrally set prices for tickets, concessions, and broadcasting et cetera in a board of directors (Edelman, 2008, pp. 901-902).

[50] For a formal model see also Friebel and Raith (2010).

[51] See also Mookherjee and Reichelstein (2001).

3.5 Appendix to Chapter 3: Instructions (translated from German)

General information

Today you are taking part in an decision-making experiment. If you read the following instructions carefully, you will be able to earn money. The amount you earn depends on your own and on other participants' decisions.

For the entire duration of the experiment, communication with other participants is not allowed. We therefore ask you not to talk to each other. Breaking this rule leads to exclusion from the experiments and any payoffs.

If you have problems understanding the experiment, please have a second look at the instructions. If you still have questions, please raise your hand. We will come to your cubicle and answer your questions personally. During the experiment, we do not use euros, we use points. The number of points you earn in the experiment are converted into euros with the following exchange rate.

1000 Points = 1 euro

At the end of the experiment, you will receive all points earned during the experiment converted into euros.

The following pages will explain the experiment in detail. First we explain the general procedure. Then we explain the different decision making screens. Before the experiments starts, control questions will appear on the screen in order to help you to fully understand the procedure. The experiment does not start until all participants have solved the control questions and are completely familiar with the course of the experiment.

The Experiment

At the beginning of the experiment you will be randomly matched with 4 other participants. That is, you make decisions in a group of 5 people. Neither you nor other participants learn anything about your identity, either during or after the experiment.

The experiment consists of 25 rounds. In each round you interact with the same people. Each participant in your group represents a firm. There are four producing firms (Firm 1 to 4) and one firm (Firm 5), which does not produce but instead decides on the distribution of profits between the five firms in your group. At the beginning of the experiment you see on the screen which firm you represent. You will represent the same firm in all 25 rounds.

Firms 1, 2, 3 and 4 produce the same (virtual) good and sell it on the same market. Production costs per unit are equal to one point. All producing firms decide simultaneously on how many units of the good they produce. There is one important rule: The higher the total quantity produced by the four firms, the lower is the market price. If some maximum of total output is reached the price will be equal to zero.

The per unit profit of each producing firm ("generated profit") equals the difference between the market price and the unit cost of production. The "generated profit"' (in points) thus equals:

Generated profit of a producing firm = (Market price - 1) x Quantity produced by the firm

In each round, firms 1 to 4 decide simultaneously on how many units of the good they produce. The computer calculates the total quantity produced by the four firms and the corresponding market price. The "generated profit" is not equivalent to payoffs earned by the firms in this round. Firm 5 decides on the actual payoff to each producing firm after the production decision: Firm 5 receives information about the total quantity produced and the profits generated by each firm as well as information about the resulting total profit. Firm 5 automatically receives 20% of the total profit. Then, firm 5 decides on how to distribute the remaining 80% of the total profit among firms 1 to 4. Firm 5 can freely choose how to distribute the profit among the firms 1 to 4.

If the total profit is negative, Firm 5 automatically bears 20% of the loss and decides on how to distribute the remaining 80% of the loss among firms 1 to 4 (in this case the 2 euros you received for participating will be automatically converted into points and used to compensate for the loss)

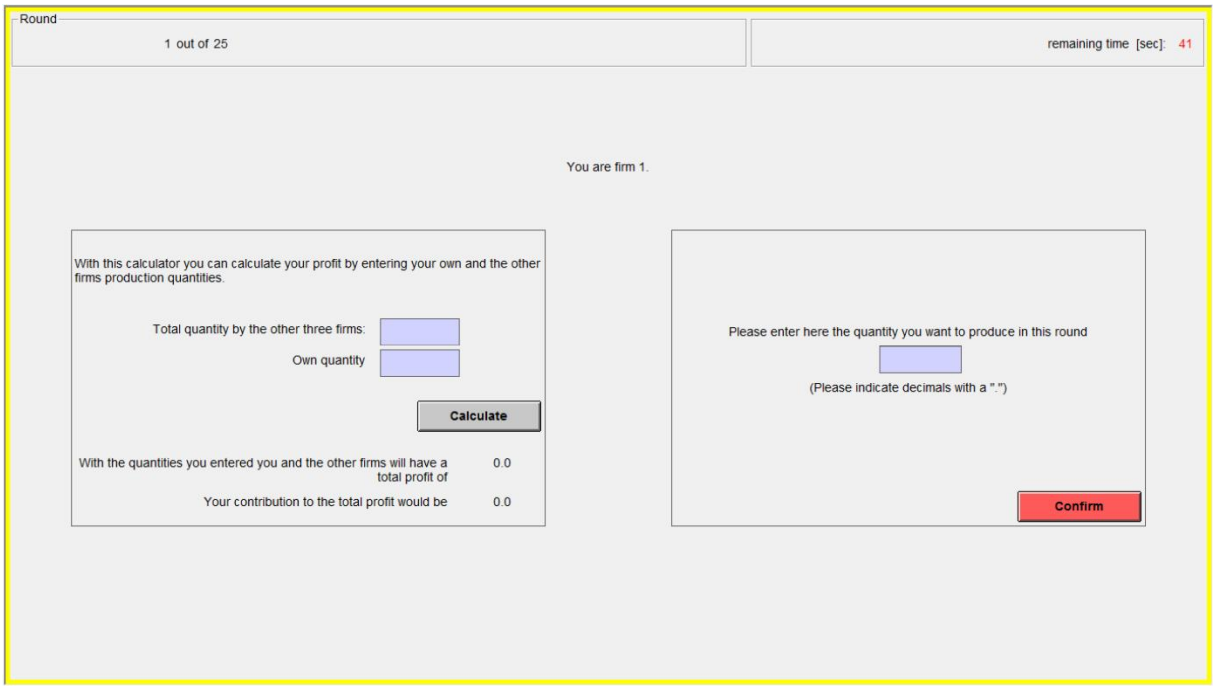
Importantly, firm 5 can only redistribute profits or losses from the current round. That is, it is also impossible to transfer a loss to one firm in order to transfer additional profits to another firm.

After firm 5 made the distribution choice all firms receive feedback on the chosen distribution. Then the next round starts. Again firms 1 to 4 make a production decision and firm five decides on the distribution.

The experiment consists of 25 rounds. After the last round all participants see a summary of their points received in each round on the computer screen. You receive all points received converted into euro.

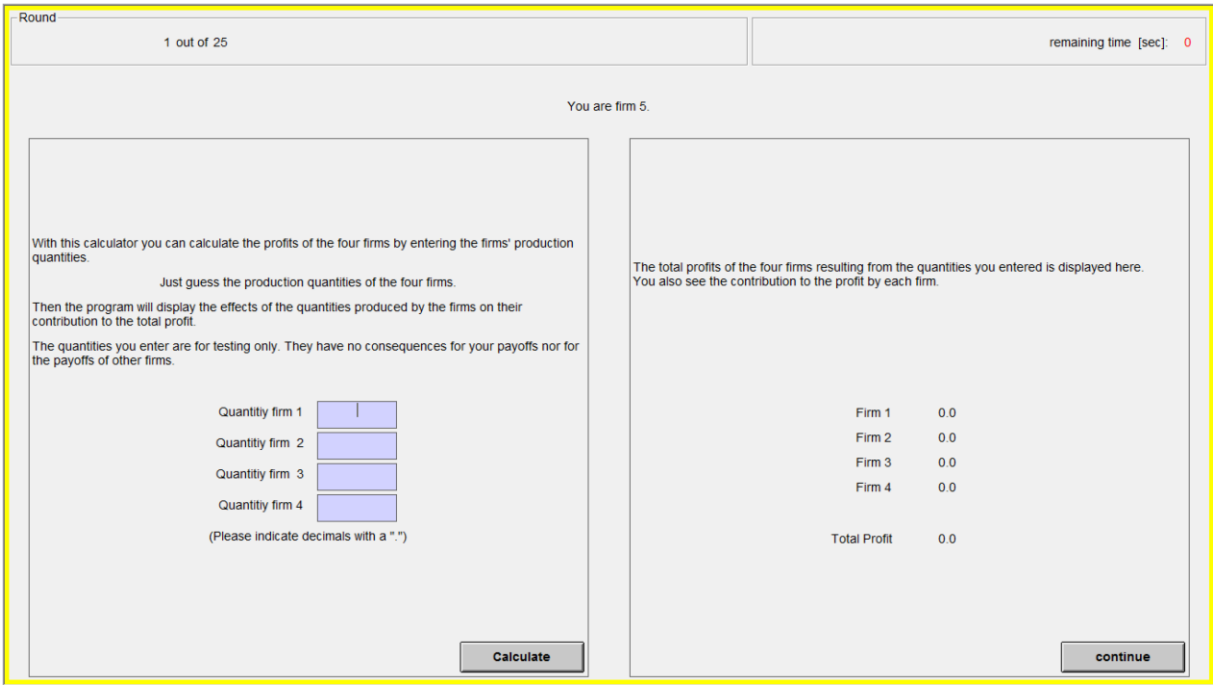
Procedure on screen

Firms 1 to 4 see the following screen. (Example for firm 1):

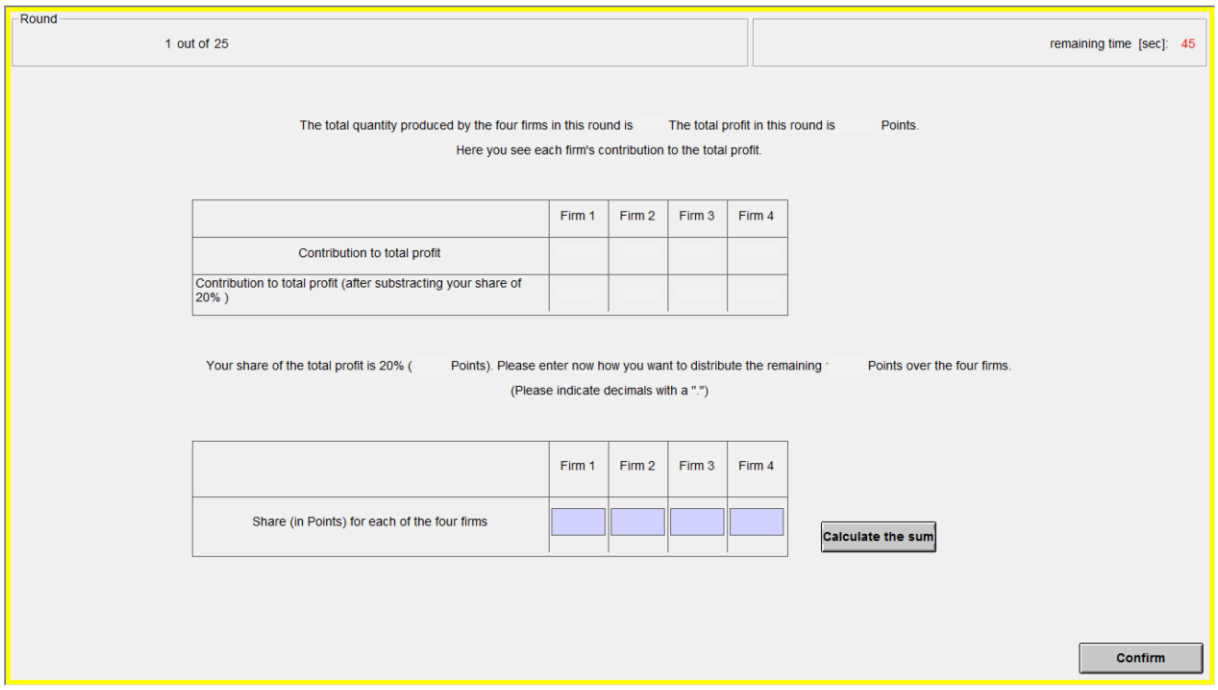


On the left hand side you see a profit calculator. With this calculator you can test how your “generated profit” varies in production quantities. You can try out how the generated profit changes when you change your own production quantity or the total quantity produced by the other three firms. The quantities you enter in the calculator are completely hypothetical. You can use the calculator to receive information on the consequences of your own and others' decisions. You can try as many calculations as you like. When you have decided on how much to produce, enter the amount on the right hand side and click on “confirm”. You cannot decide on the production quantity of other firms.

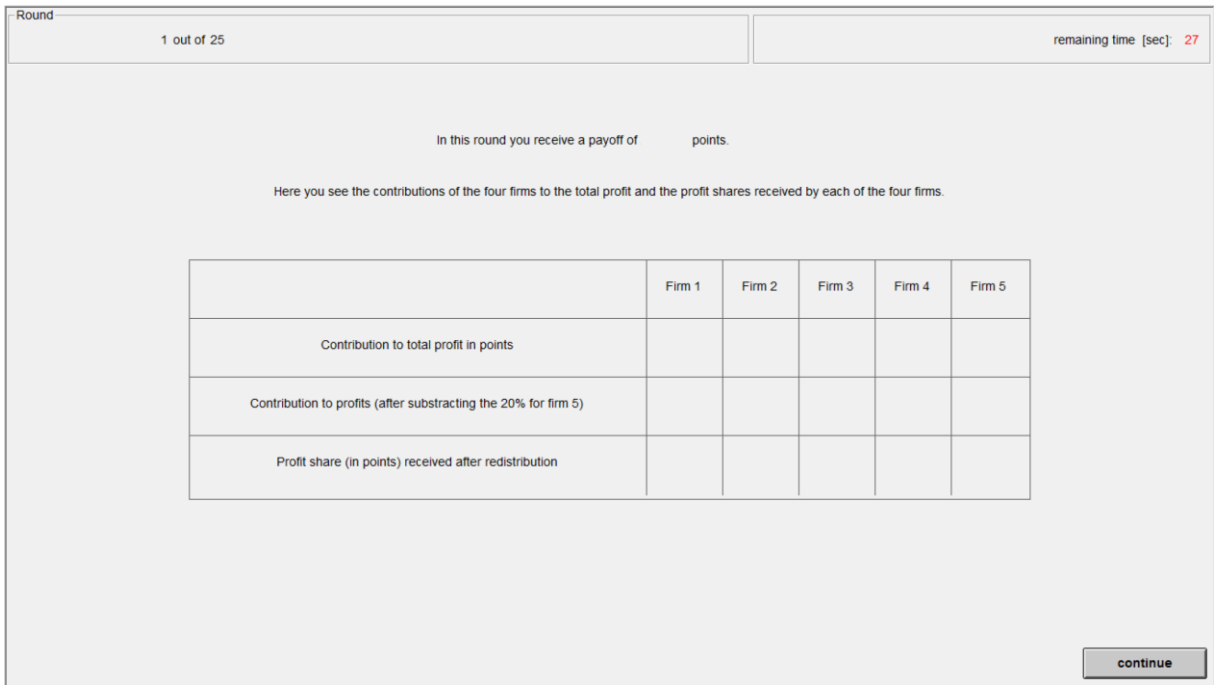
Firm 5 can also use a profit calculator. While firms 1 to 4 choose their production quantities, Firm 5 can try out, how the profits of each firm and the total profit vary when production quantities vary. The screen for firm 5 looks as follows:



After firms 1 to 4 have made their production decisions, firm 5 sees the following screen.



The screen shows the "generated profits" by each producing firm and the "generated profits" after the deduction of the 20% for firm 5. Firm 5 then decides on how many points it will assign to firms 1 to 4. In order to do so, firm 5 enters the number of points for each producing firm in the blue-shaded fields. A click on calculate shows the sum of assigned points and the number of points left for distribution. Clicking on "confirm" implements the chosen distribution of points. Then all firms see the following screen.



Control Question

Please click now on the “continue” button on your computer screen. We will display some control questions on screen. Please answer these questions. The questions only serve for the understanding of the course of the experiment. **Your answers do not affect the payoffs received in the experiment.**

If you have any questions, please raise your hand. We will come to your cubicle and answer your questions.

3.6 References of chapter 3

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4 Heterogeneous Reactions to Heterogeneity in Returns from Public Goods

4.1 Introduction

Investments in public goods (e.g. investments in energy-saving measures) benefit the investor and others. The value of obtained benefits (e.g. individual cost savings, a reduction in CO2 emissions, clean air or better water quality) is in many cases difficult to assert and different individuals benefit differently from the public good. In order to develop policies to sustain the provision of public goods, it is thus crucial to understand how uncertainty and heterogeneity in returns from public goods affect contribution behavior. Previous experimental work has focused on aggregate effects of heterogeneous returns from public goods on people's unconditional contributions to public goods (see e.g. Fisher et al., 1995) and uncertainty of returns (see e.g. Dickinson, 1998 and Levati et al., 2009). However, unconditional contributions depend on beliefs about others' contributions. Further if people have heterogeneous preferences or differ in their reference points (i.e. they compare to different reference groups), individual reactions to heterogeneity will differ in systematic ways. Studying aggregate effects may then lead to wrong conclusions and entail wrong policy implications. The aim of our paper is therefore to focus on type-specific and belief-independent reactions to heterogeneity.

The novelty of our experimental design is twofold: First, on top of unconditional contributions we elicit conditional contributions of subjects and thereby isolate belief-independent reactions to heterogeneity. Second, we use a within-subject design which allows us to identify type specific reactions to heterogeneity. Additionally, we provide insights on how people perceive heterogeneity in returns by relating our results to theoretical predictions based on two social preference models which we extend to allow for different reference groups to which people may compare.

In the experiment participants play several one-shot linear public goods games in groups of four. The social return from the public good is identical in all the games but we vary the marginal per capita returns (MPCRs). Subjects make unconditional and conditional contributions with certain and homogeneous MPCRs, certain and heterogeneous MPCRs and uncertain and heterogeneous MPCRs. In each game with heterogeneity in MPCRs, two group members receive a high MPCR while the two others receive a low MPCR. Uncertainty only concerns subjects' own MPCRs whereas the distribution of MPCRs is always known.

We find that unconditional contributions are negatively affected by the introduction of heterogeneity in MPCRs from the public good. Conditional contributions are however only weakly affected by heterogeneity. This indicates that negative effects of heterogeneity on contributions to public goods mainly stem from pessimistic beliefs about other's contributions. In heterogeneous environments, uncertainty about the own MPCR does not add a further decrease in unconditional contributions. Conditional contributions are also only slightly reduced when the own MPCR is not known and returns are heterogeneous.

Further we find that individual reactions to heterogeneity differ systematically. Selfish subjects and one third of conditional cooperators do not modify their conditional contributions to the public good when heterogeneity in returns is introduced. Around 17 percent of conditional cooperators increase contributions when receiving the high return and decrease contributions when receiving the low return. Additionally, we observe that 27 percent of conditional cooperators react only to either high or low MPCRs. Another 25 percent of conditional cooperators show the same reaction (an increase or a decrease) regarding both returns.

Since the early experiments reported in Bohm (1972), a vast experimental literature on public goods has grown, showing that individuals invest in public goods even though the individual marginal return from investments to the public good is lower than the individual marginal cost.[52] Because contributions vary with the own returns from the public good (see e.g. Ledyard, 1995), heterogeneity in returns may affect contribution behavior. An early experiment by Fisher et al. (1995) focused on the comparison of contributions to a public good by subjects with the same MPCR under homogeneity and heterogeneity in MPCRs. They neither find strong support for so-called "seeding" (i.e. higher contributions by subjects with low MPCRs in case of heterogeneity in MPCRs) nor for a "poisoning of the well" (i.e. lower contributions by subjects with high MPCRs in case of heterogeneity in MPCRs). However, in their experiment, subjects were only told that heterogeneity in returns is possible. Subjects did not know whether returns were actually different. Other experimental studies indicate that heterogeneous valuations of the public good lead less frequently to the efficient outcome (see e.g. Marwell and Ames, 1980; Bagnoli and McKee, 1991; Chan et al., 1999; Carpenter et al.,

[52] See e.g. Ledyard (1995), Anderson (2001) or Gächter (2007) for surveys.

2009 and Reuben and Riedl, 2009).[53] However, these studies do not elicit conditional contributions and thus cannot disentangle whether the decrease in average contributions is due to pessimistic beliefs about other group members' contributions or due to "pure" inequity considerations. Our experimental design allows us to go beyond this limitation. It indicates that heterogeneity matters for unconditional but not for conditional contributions and thus heterogeneity primarily affects beliefs about others' contributions.

Heterogeneity in returns is closely related to uncertainty about returns because uncertainty about returns involves different possible returns by construction. Dickinson (1998) and Levati et al. (2009) study the effects of uncertainty in MPCRs and find significantly lower individual contributions when the MPCR is stochastic compared to a certain return. Gangadharan and Nemes (2009) differentiate between situations in which the probabilities for low and high MPCRs are known by the subjects and situations with unknown probabilities. In both situations, contributions are significantly lower when there is uncertainty in the returns compared to a certain homogeneous return. However, these studies do not separate the effects of uncertainty from the effects of heterogeneity in returns. We isolate the effect of uncertainty by comparing contributions to the public good when there is heterogeneity in returns and the own returns are known with contributions when there is heterogeneity in returns but own returns are uncertain. Our findings indicate that heterogeneity matters most whereas uncertainty about the own MPCR plays a minor role.

The remainder of this paper is organized as follows. In section 2 we present the experimental design. In section 3 we propose theoretical predictions and highlight the importance of subjects' reference group. Section 4 presents the results of the experiment and section 5 concludes.

4.2 Experimental Design and Procedures

Subjects played six different versions of a standard one-shot linear public good game in groups of four. At the beginning of the experiment we informed subjects that they would participate in several experiments, but we did not inform them in advance about the

[53] Note that we only consider heterogeneity in valuations of public goods. For heterogeneity in productivity see e.g. Tan (2008) or Fellner, et al. (2010) and for heterogeneity in valuations of the private good see e.g. Falkinger, et al. (2000). For a meta study on determinants of contributions in linear public goods games see Zelmer (2003). Her findings indicate that heterogeneity decreases contributions; strongly for endowment heterogeneity and weakly for heterogeneity in MPCRs.

specific features of the six versions of the linear public goods game. Subjects received feedback only after the last game. In all six games, subjects received an endowment of 20 points each and the monetary payoff function was the following:

$$y_i = 20 - g_i + \gamma_i \sum_{j=1}^{4} g_j \qquad (1)$$

with y_i representing subject i's monetary income, g_i denoting i's contribution to the public good, and γ_i equal to the marginal per capita return (MPCR) of an investment by subject i. In the first three public good games subjects made unconditional contribution decisions (UC games). In the second three public good games we elicited conditional contributions (CC games).

In treatment UC04, all group members received the same MPCR from the public good: γ_i = 0.4. Each subject decided on her unconditional contribution and the game ended. In UCu0305, we introduced heterogeneity of MPCRs with uncertainty about each subject's own MPCR. Two subjects received $\gamma_L = 0.3$ and two subjects received $\gamma_H = 0.5$. When making their contribution decisions, subjects did not know whether they would receive γ_L or γ_H but they did know that two subjects in the group would receive γ_L and two would receive γ_H. Thus, there was uncertainty about the own MPCR, but the distribution of MPCRs was known. Note further that the marginal social return from the public good is unchanged. In the third game, there is heterogeneity of MPCRs but each subject knew her own MPCR. Subjects again faced a situation in which two subjects received γ_L and two subjects received γ_H. We used the strategy method in this decision. Subjects stated their contribution conditional on having the low (UC03) or high (UC05) MPCR.

In the CC games, we elicited conditional contributions which do not depend on subjects' beliefs about the average contribution of their group members. We used the procedure introduced by Fischbacher et al. (2001) in order to elicit conditional contributions. The procedure uses a variant of the strategy method (Selten, 1967). Subjects first decide on their unconditional contribution and then fill in a conditional contribution table. They state how many points they wish to contribute dependent on the average contribution of

their group members' ($\bar{g}$). [54] For each group, a random device (a die) selects one subject for whom the conditional contribution is relevant and three subjects for whom the unconditional contribution is relevant. MPCRs and information about possible MPCRs are equivalent to the information in the UC games. Table 4.1 summarizes the treatments.

In all sessions, CC games were conducted after UC games to have a progression of complexity in games. However, we altered the order among UC and CC games to control for changes in subjects contributions as the session progresses. In six sessions, the order was first UC04, then UCu0305 and finally UC03/UC05 (first homogeneity then heterogeneity) while in four sessions the order was UC03/UC05, UCu0305 and finally UC04 (first heterogeneity then homogeneity). The order in CC games followed the order in UC games. At the end of the session, we selected one of the games to be payoff relevant.[55] Subjects received no feedback until the end of the last experiment and were informed about this at the beginning of the experiment. Because we distributed the instructions for each game just before the game started, subjects' decisions in each public good game did not depend on any of the characteristics of the subsequent public good games.

We computerized the experiment using z-Tree (Fischbacher, 2007). Each subject sat at a randomly assigned and separated computer terminal and was given a copy of instructions.[56] A set of control questions was provided to ensure the understanding of the game. If any participant repeatedly failed to answer correctly, the experimenter

Type of game and MPCR	**Name**
Unconditional cooperation games (UC games)	
γ_i =0.4	UC04
γ_i =0.3 or γ_i =0.5, with uncertainty	UCu0305
γ_i =0.3 (with heterogeneity)	UC03
γ_i =0.5 (with heterogeneity)	UC05
Conditional cooperation games (CC games)	
γ_i =0.4	CC04
γ_i =0.3 or γ_i =0.5, with uncertainty	CCu0305
γ_i =0.3 (with heterogeneity)	CC03
γ_i =0.5 (with heterogeneity)	CC05

Table 4.1: Treatments

[54] Averages are rounded to integer numbers, i.e. subjects have to fill in 21 values. The instructions in the appendix provide a screenshot.

[55] We do not report results on a seventh decision (a donation decision) made by our subjects which was also elicited and included in the random selection of payoffs.

[56] A copy of translated instructions can be found in the appendix.

provided an oral explanation. No form of communication between the subjects was allowed during the experiment. We conducted all sessions at the LakeLab (University of Konstanz, Germany). The data were collected over ten sessions with 228 participants in total. The sessions took place between November 2009 and January 2010 and in February 2011. The experiment lasted about 1 hour and 30 minutes. Participants received on average 21.96 euros including a show-up fee of 4 euros. We recruited participants from the local subject pool including undergraduate and graduate students of all fields of studies (46 percent male) using ORSEE (Greiner, 2004).

4.3 Theoretical predictions

Selfish subjects have a dominant strategy not to contribute in the UC games. In the CC games, the conditional cooperation of selfish subjects is also zero for all contribution levels of other subjects. These predictions do not depend on our treatment variations. However, experimental research on public goods games has shown that people are willing to contribute significantly more to the public good than suggested by the assumption of selfishness. Several models have been suggested to explain such behavior: reciprocity models (e.g. Rabin, 1993; Dufwenberg and Kirchsteiger, 2004 and Falk and Fischbacher, 2006) or models of inequity aversion (e.g. Fehr and Schmidt, 1999 and Bolton and Ockenfels, 2000). All these theories predict some form of conditional cooperation if the players have a sufficiently strong social motive.

In this section, we discuss the theoretical predictions for conditional contributions by players with non-selfish preferences in our versions of the linear public goods game. Theoretical predictions focus on conditional contribution behavior, because players' unconditional contributions in the CC games depend on players' beliefs about other players' contributions. We present predictions of two well known inequity aversion models by Fehr and Schmidt, 1999 and Bolton and Ockenfels, 2000). Additionally, we discuss how players will behave according to these models if they have specific reference groups to which they compare themselves.

In the model of Fehr and Schmidt (1999) (from now on FS-model), individuals maximize a utility function of the following type:

$$U(y_i, y_j) = \begin{cases} y_i - \frac{\alpha_i}{n-1} \sum_{j \neq i} (y_j - y_i) & y_j > y_i \\ y_i & \text{if } y_j = y_i \\ y_i - \frac{\beta_i}{n-1} \sum_{j \neq i} (y_i - y_j) & y_j < y_i \end{cases} \quad (1),$$

with $\alpha_i \geq \beta_i$ and $0 \leq \beta_i < 1$. α_i is a parameter representing individual i's disadvantageous inequity aversion (or envy) while β_i corresponds to her advantageous inequity aversion. In the linear public good game with four players the monetary payoff of individual i is $y_i = 20 - g_i + \gamma_i \sum_{j=1}^{4} g_j$. y_j denotes the income of players j. According to the FS-model, subjects with a sufficiently high disutility from advantageous inequality (β_i) are willing to contribute to the public good in order to reduce the advantageous inequality (given others contribute). In particular, players will contribute positive amounts if their β is larger than or equal to $\beta_{crit} = \frac{1-\gamma i}{1-\gamma_i+\bar{\gamma}}$ with $\bar{\gamma} = \frac{1}{n-1} \sum_{j \neq i} \gamma_j$. Because of the linearity of the public good game and of the FS-model, for all but a finite set of values of border case parameters the best reply is zero contribution, full contribution or a contribution that generates equal payoffs with some player. In particular, if all players have the same MPCR, then conditional cooperation is either zero or perfect (i.e. $g_i = \bar{g}$). For the homogeneous case with MPCR $\gamma = 0.4$, it is perfect for players with a value of β >0.6.

In the heterogeneous case, players with higher MPCRs have to contribute more than players with low MPCRs to reduce inequality resulting from positive contributions (and players expect this in an equilibrium with positive contributions in which beliefs match actions[57]). The logic of the FS-model can be put in a nutshell as follows: First, each player never wants to be materially worse off than the richest of its three group members[58] and second, players who are sufficiently advantageously inequity averse will contribute as much as is necessary to realize payoff equalization with the richest of the other players[59]. Thus, in an equilibrium with positive contributions all payoffs have to be the same. Players with $\gamma_i = 0.5$ achieve payoff equality if they contribute 7/3 times as much as the players with $\gamma_i = 0.3$ (and vice versa). In other words, equilibria with positive contribution are characterized by the fact that players with an MPCR of 0.5

[57] Assuming all members make the same contribution with MPCR heterogeneity is not plausible in an equilibrium with positive contributions. With equal contributions, it is optimal for individuals with the high MPCR to contribute the same amount as the group average but for individuals with the low MPCR it is optimal to contribute 1/3 of the group average. Consequently in an equilibrium with positive contributions in which beliefs match actions we will not observe same contributions by high and low MPCR individuals. Note also, that in our experiment, subjects do not have explicit information about inequity in contributions of the other group members but only condition on the average contribution of their group members. Cheung (2011) shows however, that information on individual contributions will also affect conditional contributions.

[58] It can be shown that for every player, the marginal utility of contributing is strictly smaller than zero as soon as one other player receives a higher payoff due to $\alpha_i \geq \beta_i$ and $0 \leq \beta_i < 1$.

[59] The marginal utility of contributing will be strictly positive for a low MPCR player who is richer than any other player, if her $> \beta_{crit0.3} = \frac{21}{34} \sim 0.618$. The marginal utility of contributing will be strictly positive for a high MPCR player who is richer than any other player, if her $\beta > \beta_{crit0.5} = \frac{26}{45} \sim 0.577$.

contribute 61.5% above the average of the other players and players with an MPCR of 0.3 contribute 52.9% of the average contribution of the other players. Because the threshold for a payoff equalizing contribution is lower for players with an MPCR of 0.5 than for players facing homogeneity and an MPCR of 0.4, which is again higher than the threshold for the situation in which all individuals face heterogeneity with an MPCR of 0.3, we should also observe more people contributing positive amounts in situation CC05 than in CC04, and more in CC04 than in CC03.

As a further benchmark, we extend the FS-model by assuming that some players compare only to a specific reference group. Players who compare only to counterparts who have the same MPCR (although there is heterogeneity in MPCRs) have a threshold for positive contributions of $\beta_{crit0.3} \geq 0.7$ or $\beta_{crit0.5} \geq 0.5$ and contribute exactly the average contribution of their group members. If players compare only with group members who have a different MPCR, players with $\gamma = 0.3$ who compare only to players with the high MPCR have $\beta_{\text{crit}0.3} \geq 0.5833$ and contribute 1/3 of the average contributions whereas players with $\gamma = 0.5$ who compare only to players with the low MPCR have $\beta_{\text{crit}0.5} \geq 0.625$ and contribute twice the average when facing the high MPCR.[60] Consequently the qualitative predictions of the FS-model about the number of players contributing to the public good do not differ for different reference groups. However, optimal contribution levels are different, as we show in figure 4.1.

In the game with uncertain MPCRs (UCu0305) players have identical information ex-ante. It is thus plausible to assume that players' unconditional contributions will be the same. Although players equalize expected payoffs ex-ante, a low MPCR player will experience disutility from inequality toward the two richer players ex-post and therefore will prefer to contribute 0. This implies that no equilibrium with positive unconditional contributions exists in the FS-Model. For conditional contributions, it can be shown that an expected utility maximizer who cares sufficiently strongly about advantageous inequality ($\beta>0.6$) will contribute positive amounts as long as she is not

[60]Note that the threshold is smaller for a player with the low MPCR who compares only to high MPCR individuals than for an individual with a high MPCR comparing only to low MPCR individuals because it is less costly for the player with the low MPCR to reduce inequality (he loses 0.7 by contributing one unit and each member of his reference group gains 0.5. A player with the high MPCR loses only 0.5 when contributing 1 unit while his reference group members gain only 0.3 each).

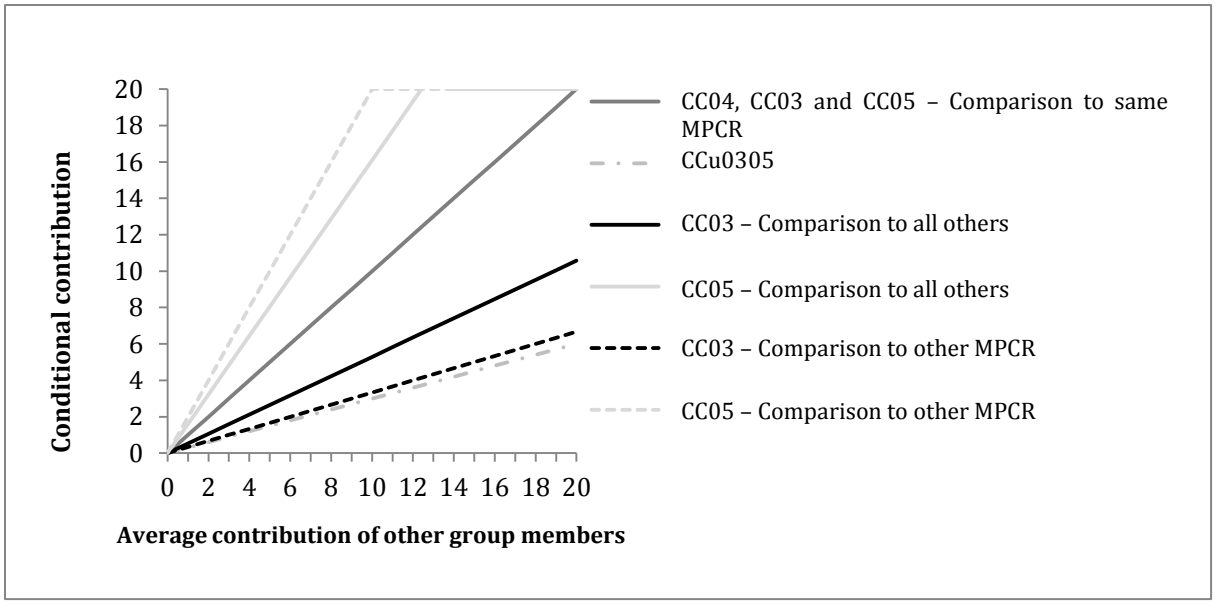


Figure 4.1: Optimal conditional contributions (FS-model, β=0.8)

poorer than the richest other player in her group when facing the low MPCR. The FS-model allows for positive conditional contributions under uncertainty but predicts a strong decrease in total conditional contributions if uncertainty is introduced. However, positive conditional contributions will in any case be lower or equal to 35% of the group members' average contribution (see also Figure 4.1).

We now turn to the predictions of the ERC model by Bolton and Ockenfels (2000). In the BO-model it is assumed that each agent i maximizes the following utility function:

$$U_i = U_i\left(y_i, \frac{y_i}{c}\right) \tag{2}$$

The utility of each agent depends on her monetary payoff y_i and her relative payoff $\frac{y_i}{c}$. The sum of all group members' monetary payoffs is represented by c, $c = \sum_{j=1}^{n} y_i$. Based on Bolton and Ockenfels (2000), in our framework each agent maximizes the following utility function:

$$U_i = y_i - \vartheta_i \left(\frac{y_i}{y_i + 3\bar{y}} - \frac{1}{4}\right)^2 \tag{3}$$

with $\bar{y}$ being the average payoff of the other group members. The parameter ϑ_i represents an individual preference parameter and expresses the importance of disutility from inequality. It is assumed to be weakly positive. If this parameter is sufficiently high, players will conditionally contribute to the public good because if the

other players contribute, the own contribution reduces the difference between the own and the other players' payoffs. For this reason, conditional cooperation is also weakly increasing in ϑ_i. If there is heterogeneity in the MPCRs, it is not generally true that an increase in the own MPCR and a decrease in the other players' MPCRs increases the own contribution. However, it can be shown numerically to hold for the parameters chosen in the experiment ($g_{CC05} \geq g_{CC04} \geq g_{CCu0305} \geq g_{CC03}$). The logic of the numerical analysis works as follows. First, it can be shown that conditional contributions are monotonically increasing in ϑ_i. Because this is the case, it is sufficient to show in a second step that increasing ϑ_i leads to a successive increase in the components of the vector of conditional contributions in the different situations ($g_{cc05}, g_{CC04}, g_{CCu0305}, g_{CC03}$). Using this procedure reveals also that according to the BO-model conditional contributions are only weakly higher in CC04 than in CCu0305, but the difference amounts to at most one point.

In order to understand systematic differences in reactions to heterogeneity, we extend also the BO-model by allowing subjects to differ in their reference group, i.e. we differentiate cases in which subjects compare their own payoffs to the average payoff of all other group members from cases where subjects compare their own payoffs only to the payoff of other group members who have a specific MPCR. Formally, we replace the value of $\bar{y}$ in (3) by the average payoff of the respective reference group. Figure 2 presents subjects' optimal conditional contributions depending on the average contribution of other group members, for a value $\vartheta_i = 2000$. The order of the conditional contribution schedules shown in Figure 1 does not depend on the parameter value of ϑ_i. Figure 2 includes optimal conditional contributions for all games with certainty about the own MPCR.[61] Additionally, for treatments CC03 and CC05, we include optimal conditional contributions for subjects who compare to a specific reference group only. The spread between conditional contributions in CC03 and CC05 will be larger if subjects compare their own payoff only to the average payoff of group members with the other MPCR than if they compare their payoff to all group members. Instead,

[61] We do not include the optimal contributions for CCu0305, which are weakly below optimal contribution in CC04, in order not to charge the figure unnecessarily here.

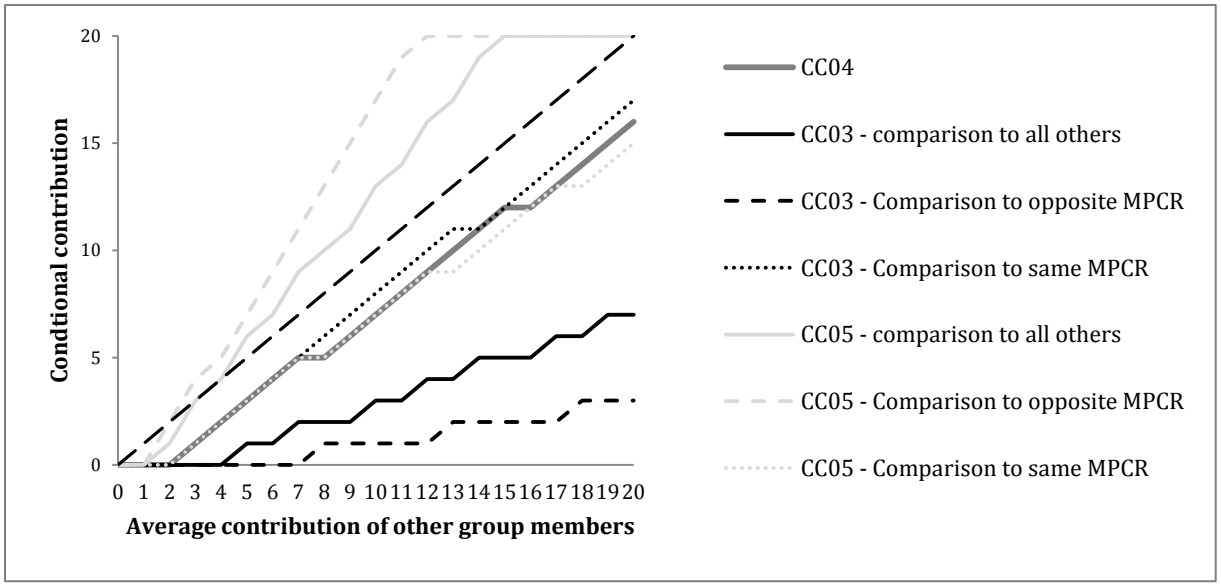


Figure 4.2: Optimal conditional contributions (BO-model, with $\vartheta_i = 2000$)

they will roughly contribute the same in CC03, CC04 and CC05, if they compare their own payoff to the payoff of other group members receiving the same MPCR.

Both the FS- and the BO-model predict that individuals may modify their contribution behavior when heterogeneity in returns from the public good is introduced. In the FS-model, participants have to be less inequity averse to make positive conditional contributions in CC05 than in CC04 than in CC03 (i.e. critical level of β is lower in CC05 than in CC04 and higher in CC03 than in CC04) and also subjects, who contribute positive amounts, contribute less with the low MPCR than with the high MPCR. The BO-model comes to a similar conclusion, because each individual will contribute weakly higher amounts in CC05 than in CC04 than in CC03 for a positive inequity parameter ϑ. Therefore, we should expect on average higher contributions in CC05 than in CC04 than in CC03.

Hypothesis 1 (MPCR effect): On average, compared to the homogeneous MPCR of 0.4, average conditional contributions are higher in CC05 and lower in CC03.

Further both models suggest that heterogeneity affects conditional contributions on average negatively

Hypothesis 2 (Heterogeneity effect): On average, the average of conditional contributions in CC05 and CC03 is lower than the conditional contributions in the homogeneous case with an MPCR of 0.4.

Because different players may perceive the game differently, we also derived predictions for the FS- and BO-model for subjects who compare only to a specific reference group. Both models suggest that conditional contributions should strongly react to heterogeneity if subjects compare themselves only to group members with the other MPCR, and that reactions to heterogeneity are rather weak if subjects compare only to group members with the same MPCR. This leads us to Hypothesis 3.

Hypothesis 3 (Type-specific reactions): Conditional cooperators' reactions to heterogeneity differ such that one fraction of conditional cooperators strongly increase contributions in CC05 and strongly decrease contributions in CC03 whereas another fraction reacts only weakly to heterogeneity in returns.

The predictions with respect to conditional contributions under uncertainty about the own MPCR differ strongly: the FS-model predicts that subjects strongly reduce conditional contributions whereas the BO-model predicts that contributions in CCu0305 are only weakly smaller than contributions in CC04 and do not differ by more than one point. Therefore, we formulate hypotheses 4a and 4b with respect to conditional contributions under uncertainty.

Hypothesis 4a (Uncertainty effect): Contributions in CCu0305 do strongly differ from conditional contributions in CC04.

Hypothesis 4b (Uncertainty effect): Contributions in CCu0305 do not differ by more than one point from conditional contributions in CC04.

4.4 Results

4.4.1 Results from unconditional cooperation games (UC games)

Figure 4.3 presents average unconditional contributions in the UC games as well as the mean of UC03 and UC05 as an additional benchmark. We observe significantly higher contributions to the public good when MPCRs are homogeneous rather than heterogeneous, irrespective of uncertainty (Wilcoxon signed-rank test: UC04 vs. UCu0305, $z=5.526$, $p<0.001$ and UC04 vs. MeanUC03UC05, $z=3.894$, $p<0.001$).[62] Subjects on average contribute positive amounts even under uncertainty about the own MPCR.

[62] This results holds irrespective of the order in which subjects played the game.

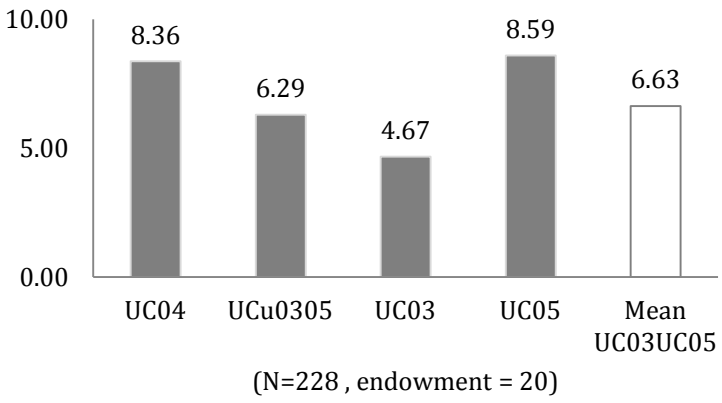


Figure 4.3: Average unconditional contributions to the public good in UC games

The introduction of uncertainty in addition to heterogeneity only slightly lowers subjects' contributions (Wilcoxon signed-rank test: UCu0305 vs. MeanUC03UC05, *z=2.316*, *p=0.021*). In UC03, average unconditional contributions are lower than in UC04 (Wilcoxon signed-rank test: *z=8.094*, *p<0.001*) and weakly higher in UC05 than in UC04 (Wilcoxon signed-rank test: *z=1.775*, *p=0.076*). Nevertheless, the decrease of contributions between UC04 and UC03 is much stronger than the increase of contributions between UC04 and UC05 (Wilcoxon signed-rank test: *z=3.894*, *p<0.001*). Isaac and Walker (1988) showed that MPCRs and contributions are positively related in homogeneous environments. We cannot completely confirm this finding for heterogeneous environments. We find that lower returns induce a decrease of contributions when MPCRs are heterogeneous; we only observe a weak increase in contributions with high MPCRs in the heterogeneous environment. Thus the (positive) effect of the value of the MPCR seems to interact with the (negative) effect of heterogeneity of group members' MPCRs.

The results on unconditional contributions give the global effect of heterogeneity in returns on average contributions to the public good. However, the decrease in unconditional contributions might be driven by pessimistic beliefs about other group members' contributions. Therefore we focus next on subjects conditional contributions, which are independent of beliefs about group members' average contributions.

4.4.2 Results from conditional cooperation games (CC games)

Figure 4 shows average conditional contributions for all subjects in all treatments. Subjects on average increase their conditional contributions in CC05 compared to CC04 whereas they decrease conditional contributions in CC03. Average conditional contributions in CC04 are 5.81, in CC05 6.31, in CC03 5.10 and in CCu0305 5.45.[63] Thus we cannot reject Hypothesis 4.1.

Result 4.1: On average, conditional contributions are higher in CC05 and lower in CC03 compared to the homogeneous MPCR of 0.4.

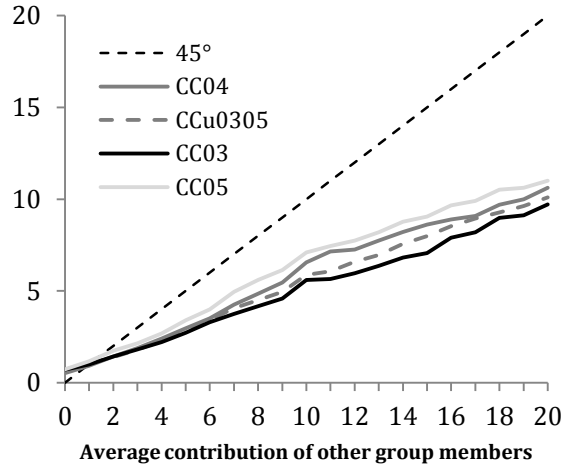


Figure 4.4: Average conditional contributions to the public good (N=228)

To investigate whether subjects adjust conditional contributions by changing their conditional contribution for every given average contribution level (i.e. they adjust the slope of their contribution schedule) or whether subjects simply become more or less generous when heterogeneity is introduced (i.e. they shift their schedule), we regress subjects' conditional contributions in model (1) of table 4.2 on the average contribution by their group members for the different treatments. The first column of table 4.2 shows that in CC03 subjects decrease their slope significantly by 0.067 whereas they do not significantly increase their slope in CC05. Instead, they behave more generously by shifting up the intercept of their schedule by 0.311.

In order to test hypothesis 2, we consider the average contribution of CC03 and CC05 for each individual subject to measure the aggregate effect of the introduction of heterogeneity in returns in model (2). Interestingly, heterogeneity does not affect

[63] All averages are significantly different from another at the 5 percent level according to Wilcoxon sign rank tests.

conditional contributions of subjects significantly, suggesting that heterogeneity in particular affects subjects' beliefs about others' contributions. We summarize this finding in result 4.2.[64]

Result 4.2: Conditional contributions of subjects with homogeneous MPCRs do not significantly differ from the average of subjects' conditional contributions with heterogeneous MPCRs.

Dependent variable :	**Model (1)**		**Model (2)**	
Conditional contribution	All subjects	Conditional Cooperators	All subjects	Conditional Cooperators
Average contribution of other group members	**0.523*** **	**0.816*** **	**0.523*** **	**0.816*** **
	(0.031)	(0.028)	(0.031)	(0.028)
CC04	Ref.	Ref.	Ref.	Ref.
CCu0305	-0.048	-0.009	-0.048	-0.009
	(0.108)	(0.110)	(0.108)	(0.110)
MeanCC03CC05			0.134	**0.285** **
			(0.121)	(0.143)
CC03	-0.044	0.045		
	(0.151)	(0.183)		
CC05	**0.311** **	**0.524*** **		
	(0.134)	(0.150)		
Group average × CC04	Ref.	Ref.	Ref.	Ref.
Group average × CCu0305	**-0.031***	**-0.078*** **	**-0.031***	**-0.078*** **
	(0.016)	(0.020)	(0.016)	(0.020)
Group average × MeanCC03CC05			-0.024	**-0.097*** **
			(0.022)	(0.026)
Group average × CC03	**-0.067*** **	**-0.146*** **		
	(0.023)	(0.028)		
Group average × CC05	0.019	**-0.048***		
	(0.024)	(0.028)		
Constant	**0.582*** **	-0.205	**0.582*** **	-0.205
	(0.208)	(0.193)	(0.208)	(0.193)
Observations	19,152	12,096	14,364	9,072
# clusters	228	144	228	144
R^2	0.225	0.489	0.229	0.516

Table 4.2: OLS regressions on conditional contributions[64]
Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

[64] We also controlled for order effects. We find that subjects become less generous as the experiment progresses. Considering model (1) for all subjects, those who first face the heterogeneous case contribute in CC04 about the average of their conditional contribution in CC03 and CC05. Subjects who face the homogeneous situation first contribute slightly less than the average of their conditional contribution in CC03 and CC05. For conditional cooperators we find qualitatively similar results with both orders.

Figure 4.4 also shows that with uncertainty about the own MPCR, average conditional contributions are lower than conditional contributions in CC04 but higher than in CC03. We can thus reject hypothesis 4a. Further contributions in CC04 are by more than 1 point higher than in CCu0305 for almost all group average contributions larger than 10.[65] We also find that uncertainty does not make subjects less generous, but reduces the slope of their contribution schedule (see also column 1 of model (1) in table 4.2). We summarize these findings as follows.

Result 4.3: Uncertainty in MPCRs reduces conditional contributions on average by not more than 1.5 points.

To study individual and type-specific reactions, we classify subjects based on their behavior in CC04 for the subsequent analysis. We define preference types according to the procedure introduced by Fischbacher et al. (2001): Selfish subjects are subjects who always contribute zero to the public good; conditionally cooperative subjects are subjects who monotonically increase their contribution to the public good as the average contribution of other group members increases or whose contributions are significantly positively correlated to the average contribution of other group members. The last type of subject shows a hump-shaped contribution pattern, i.e. these subjects' contributions are increasing in the average contribution of other group members until a specific value and then decrease in it.

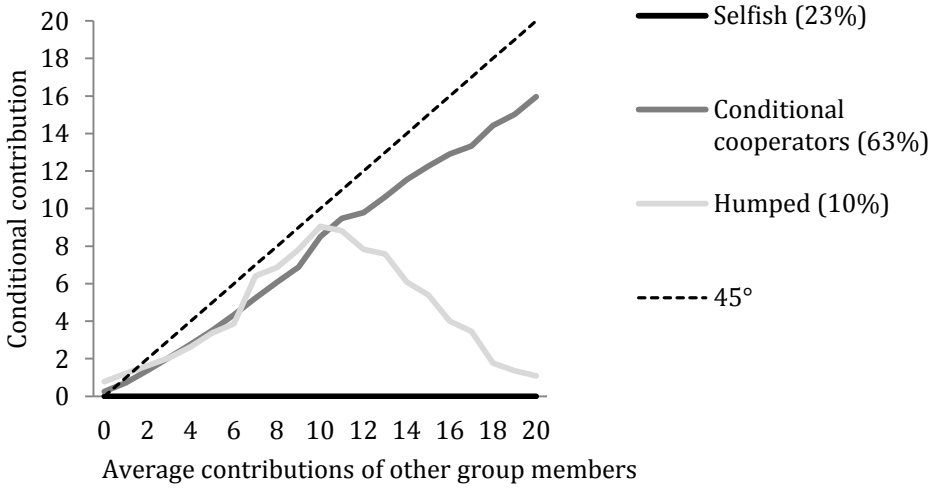


Figure 4.5: Average of conditional contributions in CC04 by type

[65] The exceptions are group averages of 12 and 17.

Over the 228 participants, we observe 23% selfish subjects, 10% show a humped-shaped pattern and 63% are conditional cooperators. Only 4% of the participants do not fit in any of these categories.[66] The contribution of each type for all potential average contributions of other group members in CC04 is depicted in Figure 4.5. Subjects with a humped-shaped pattern and subjects who do not follow a specific strategy are few (in total they represent 14% of the subjects) and display behavior that is not consistent with stability of other-regarding preferences. We briefly report the results for subjects categorized as humped-shaped and selfish but concentrate our analysis on conditionally cooperative subjects.

4.4.3 Selfish subjects and subjects with a humped-shaped contribution schedule

Almost all subjects who are classified as selfish in CC04 contribute zero to the public good for any average contribution of other group members in CC03, CC05 and CCu0305. Thus heterogeneity does not significantly affect contribution behavior by these subjects.[67] Conditional contributions schedules with humped-shaped patterns are rare (22 out of 228 subjects). Subjects with such schedules contribute on average 4.43 in CC04. Average contributions are higher when heterogeneity is introduced, weakly in CC03 (5.66, Wilcoxon signed-rank tests, $p = 0.0998$) and strongly in CC05 (7.02, $p = 0.0002$). Changes in average contributions are mainly caused by 8 subjects, who show a humped-shaped pattern in CC04 but are conditionally cooperative either in CC03, CC05 or both. Subjects showing a humped-shaped pattern in all three situations (CC04, CC03, CC05) are only weakly affected by heterogeneity.

4.4.4 Conditional cooperators - At the aggregate level

Figure 4.6 presents conditional contributions for subjects classified in CC04 as conditional cooperators. The figure shows that the slope in CC03 and CCu0305 is flatter than the slope in CC04. However, the slope is almost identical in CC05 and CC04. We test with a linear regression whether these results are significant (see Table 4.2).

Model (1) shows that if MPCRs are homogeneous, an increase of the average contributions of group members by one point will lead to an increase in conditional contribution by 0.816. However, the positive effect of the average contribution of other group members is significantly lower when heterogeneity of MPCRs is introduced (with

[66] As a comparison, Fischbacher, et al. (2001) find about one third of subjects classified as free riders whereas about 50 percent are conditionally cooperative.

[67] Six out of 52 as selfish classified subjects contribute more than zero in CC03, UC05. Among them 4 who slightly increase contributions in both UC03 and UC05 and two who only increase their contributions in UC05.

and without uncertainty about the own MPCR). When the own MPCR is certain, the positive effect of group members' average contribution is significantly smaller in CC03 than in CC05. Besides, a high MPCR makes conditional cooperators on average more generous (+0.417 points irrespective of the group average compared to CC04) but a low MPCR makes subjects on average less generous (-0.572 points irrespective of the group average compared to CC04). We summarize the findings in result 4.4.

Result 4.4: At the aggregate level, the slope of the contribution schedule of conditional cooperators decreases when they receive the low MPCR but does not increase when they receive the high MPCR compared to homogeneous MPCRs.

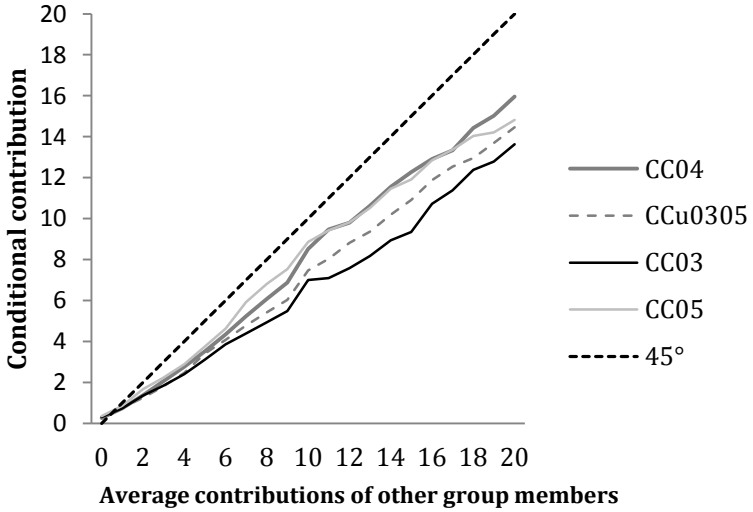


Figure 4.6: Average of conditional contributions by conditional cooperators

Model (2) shows that heterogeneity slightly increases generosity of conditional cooperators but reduces the slope of conditional cooperators' contribution schedules on average by almost 0.1 points. Thus, for average contributions higher than 4, heterogeneity reduces conditional contributions of conditional cooperators although the social return of the public good is identical to the homogeneous case. Comparing the coefficients of *CCu0305* and *MeanCC03CC05* as well as *Group average × CCu0305* and *Group average × MeanCC0305* allows us to infer whether uncertainty in addition to heterogeneity in MPCRs changes behavior of conditional cooperators. Clearly, conditional cooperators are more generous when they know their own MPCR (Wald test, p=0.013) but the slope coefficients do not significantly differ (Wald test, p=0.359). We summarize this finding in result 4.5.

Result 4.5: In case of heterogeneity of MPCRs, conditional cooperators are significantly less generous when facing uncertainty about their own MPCR but do not additionally reduce the slope of their contribution schedule.

4.4.5 Conditional cooperators - At the individual level

Hypothesis 2 suggests that some conditional cooperators react to the introduction of heterogeneity in MPCRs but others do not, because this type of subject compares her payoff only to the payoff of the subject with the same MPCR. To test hypothesis 2, we use the hierarchical cluster analysis of Ward (1963). This method is based on the minimization of the intra-group variance. At each step in the analysis, the union of every possible cluster pair is considered and the two clusters whose fusion results in minimum increase in variance are combined. To classify subjects, we use two variables reporting how subjects' conditional cooperation differs between CC04 and CC03 and between CC04 and CC05. The first variable is the average of each subject's difference in conditional contributions in CC03 and CC04. We call this variable Diff03 (Diff03 = average of ($g_{i03} - g_{i04}$). Diff03 being negative indicates that subjects' conditional cooperation is less perfect when the subject's MPCR equals 0.3 (with heterogeneity of MPCRs) than when it equals 0.4 (with homogeneity of MPCRs). To compute the second variable, we do the same but replace the low MPCR by the high MPCR. We name this second variable Diff05 = $g_{i05} - g_{i04}$.

On average, the difference between the average contribution of other group members and the conditional contribution of a conditional cooperator is equal to 2.04 in CC04. When there is heterogeneity in MPCRs, this value is equal to 3.45 if subjects receive the low MPCR, 2.00 if subjects receive the high MPCR and 2.83 if they do not know which MPCR they will receive. We have thus Diff03 = -1.41 and Diff05 = 0.04. We identify six categories of subjects with Ward's classification method. For each category of subjects, the average and standard deviation of Diff03 and Diff05 as well as the share of conditional cooperators it includes are presented in table 4.4. From Ward's classification of conditional cooperators, we can infer whether behavior corresponds to the classifications suggested by theory. On the one hand, 31.9% of conditional cooperators behave as if they compare their payoff to the payoff of the other group member receiving the same MPCR. They do not significantly change their behavior as heterogeneity in MPCRs is introduced (t-test for difference with 0, $p = 0.178$ for Diff03 and $p = 0.852$ for Diff 05, Wilcoxon signed rank test, p-value=0.649). On the other hand,

17.4% of conditional cooperators behave as if they compare their payoff to the average payoff of all other group members, or to the two group members having the opposite MPCR. These subjects modify their conditional contributions to the public goods as heterogeneity of MPCRs is introduced: they significantly decrease their contributions to the public goods when they receive the low MPCR and increase it when they receive the high MPCR (Wilcoxon signed rank test for difference with 0, $p < 0.01$ for Diff03 and Diff05). Hence, we cannot reject hypothesis 2.

Ward's classification yields two further categories, in which behavior corresponds roughly to the theoretical prediction by the BO-model for subjects who compare their payoff only to payoffs of subjects with a specific MPCR of either 0.3 or 0.5. According to the theoretical model, for subjects who only compare to others with an MPCR of 0.5 it should hold that $g_{CC04} > g_{CC03}$ *and* $g_{cc04} \geq g_{cc05}$. Indeed, 12.5% of conditional cooperators roughly behave in this way. They significantly and strongly decrease their contribution when they receive the low MPCR and do not significantly

	Share	Average Diff03	Average Diff05	SD Diff03	SD Diff05
Comparison with the same MPCR subject[68]	31,9%	0,06	0,01	0,29	0,45
Comparison to all others & to opp. MPCR[69]	17,4%	-1,60***	0,59***	0,65	0,61
Comparison to 05 subject(s)	12,5%	-5,39***	0,76	2,07	1,55
Comparison to 03 subject(s)	14,6%	0,77***	1,75***	1,04	0,83
Heterogeneity averse[70]	16,7%	-4,95***	-4,70***	2,56	2,78
Heterogeneity lover	6,9%	3,39***	5,28***	2,43	2,96

Table 4.3: Classification of conditional cooperators
Stars indicate whether values are significantly different from zero according to Wilcoxon sign rank tests, with *= p-value < 0.10, **= p-value < 0.05 and ***= p-value <0.01

[68] No reaction to heterogeneity in returns may also result from comparisons in contributions instead of final payoffs.

[69] We cannot separate subjects comparing themselves to subjects with the opposite MPCR from subjects comparing to all others, because the theoretical predictions do not differ qualitatively.

[70] Heterogeneity averse people are actually classified into two different clusters. Although average Diff03 and average Diff05 have the same sign in both clusters, the magnitude is different. We group these two clusters because for both Diff03 and Diff05 are strongly negative. Each cluster presents 8.3% of the population. In the first cluster, average Diff03 is -6.94 and average Diff05 is -6.86 while in the second cluster these values are respectively -2.97 and -2.54.

change their behavior if they receive the high MPCR (Wilcoxon signed rank test for difference with 0, $p < 0.01$ for Diff03 and p=0.123 for Diff05). Behavior of another 14.6% of conditional cooperators roughly coincides with the prediction of the BO-model for subjects who compare their payoff only to payoffs of members receiving an MPCR of 0.3. They significantly increase their contribution when they receive the high MPCR and slightly increase it when they receive the low MPCR (Wilcoxon signed rank test for difference with 0, $p < 0.01$ for Diff03 and Diff05).

The two last categories include subjects who are affected by the introduction of heterogeneity in MPCRs in the same way by both CC03 and CC05. We name 16.7% of our subjects "heterogeneity averse" because they significantly decrease their contribution when heterogeneity is introduced irrespective of their own MPCR (Wilcoxon signed rank test for difference with 0, $p < 0.01$ for Diff03 and Diff05). We have called the smallest (6.9%) and last category of subjects "heterogeneity loving" as they significantly increase their contribution in UC03 and UC05 (Wilcoxon signed rank test for difference with 0, $p < 0.01$ for Diff03 and Diff05).

4.5 Conclusion

We investigated whether the introduction of heterogeneity and uncertainty in returns from public goods affects unconditional and conditional contribution behavior. Unconditional contributions depend on beliefs whereas conditional contributions are belief-independent. A within-subject design allowed us further to analyze reactions to heterogeneity in MPCRs from the public good at the individual level. Based on the assumption that subjects may compare to different reference groups, we hypothesized that individuals react differently to heterogeneity in returns.

The results show that, at the aggregate level, heterogeneity in MPCRs from the public good reduces unconditional contributions significantly, regardless of whether the own MPCR from the public good was certain or uncertain. However, conditional contributions are less strongly affected by heterogeneity, suggesting that negative effects of heterogeneous environments may in particular result from more pessimistic beliefs about others' contribution behavior. To sustain contributions to public goods with heterogeneous returns, policies may thus aim at clarifying that those who gain a lot from the public good may indeed willing to contribute more.

Decomposing our results on conditional contributions shows that reactions to heterogeneity in returns are heterogeneous. Differences in reactions are systematic. Heterogeneity does not affect selfish subjects' behavior significantly. Conditional cooperators' reactions are mixed. We detect around one third of conditional cooperators who do not react to heterogeneity in MPCRs. 17 percent of conditional cooperators decrease their contributions when they receive the low MPCR and increase it when they receive the high MPCR. Additionally, some conditional cooperators mainly react to only high or low returns while others have the same reaction regarding both returns when heterogeneity is introduced. A substantial part of this variation can be explained by accounting for different reference groups subjects may compare to.

The decomposition of results on conditional contributions yields an important insight: Heterogeneity decreases conditional contributions mainly for two types of conditional cooperators. The first type dislikes heterogeneity in general. The second type behaves as if comparing only to group members with higher returns from the public good. Thus, in order to mitigate the negative effects of heterogeneity, public policy may aim at counterbalancing reference groups by specific communication policies.

4.6 Appendix to Chapter 4: Instructions (translated from German)

Instructions

You are about to participate in an experiment on decision-making. During this session, you can earn money. The amount of your earnings depends on your decisions and on the decisions of the participants you will interact with.

In the experimental session, you will make decisions in seven different experiments. One experiment will be randomly chosen to determine your payment. At the very beginning of the experimental session, one participant will be randomly selected to throw a die at the end in order to select the experiment that will be paid and to make all other random selections. The chosen experiment will be announced at the end of the experimental session. The experiment selected for payments is the same for all participants in the session. The payment you will receive will be your income in the selected experiment. In addition, you will receive a show-up fee of 4 Euros. You will be paid in cash at the end of the experimental session.

Each experiment is independent of the previous experiment you play. The next experiment starts as everybody in the room has made his decision in the previous experiment.

Please read the instructions carefully. To make sure that all participants have understood correctly, you will have to answer questions about the instructions.

You are not allowed to communicate during the experiment. If you have any questions, please ask us. Violation of this rule will lead to the exclusion from the experimental session and all payments. If you have questions, please raise your hand. A member of the experimenter team will come to you and answer them in private.

Thank you for your participation.

We will not speak in Euros during the experimental session, but rather in points. Your whole income will first be calculated in points. At the end of the experiment, the total amount of points you earned will be converted to Euros at the following rate:

1 point = 0.75 Euro

All participants will be divided in groups of four members. Except from us – the experimenters – no one knows who is in each group.

We describe the exact experiment process below.

<u>The basic decision situation</u>

We first introduce you to the basic decision situation. Further instructions will be distributed during the session. You will find control questions at the end of the description of the basic decision situation that help to understand the basic decision situation.

You will be a member of a group consisting of **4 people**. These groups will be reconstituted when a new experiment starts. Nobody knows the composition of the groups. Neither before, nor after the experimental session you will learn which people are/were in your group. You will receive a membership number in the group (1, 2, 3 or 4) that will remain the same for the whole experiment.

Each group member has to decide on the allocation of 20 points. You can put these 20 points into your **private account** or you can invest them **fully** or **partially** into a project. Each point you do not invest into the project will automatically remain in your private account.

<u>Your income from the private account</u>

You will earn one point for each point you put into your private account.

Income from your private account = *20 – your contribution to the project*

For example, if you put 20 points into your private account (and therefore do not invest in the project), your income will amount to exactly 20 points out of your private account. If you put 6 points into your private account, your income from this account will be 6 points. **No one except you earns something from your private account.**

Your income from the project

Each group member will profit equally from the amount you invest into the project. On the other hand, you will also get a payoff from the other group members' investments. The income for each group member will be determined as follows:

Income from the project = *sum of all contributions × 0.4*

If, for example, the sum of all contributions to the project is 60 points, then you and the other members of your group each earns 60 × 0.4 = 24 points out of the project. If four members of the group contribute a total of 10 points to the project, you and the other members of your group each earns 10 × 0.4 = 4 points.

Total income

Your total income is the sum of your income from your private account and that from the project:

Your total income =
Income from your private account (= 20 – your contribution to the project)
+ Income from the project (= sum of all contributions to the project × 0.4)

Control questions

Please answer the following control questions. They will help you to gain an understanding of the calculation of your income, which varies with your decision about how you distribute your 20 points. *Please answer all the questions and write down your calculations.*

1. Each group member has 20 points. Assume that none of the four group members (including you) contributes anything to the project.
   What will *your* total income be? _____

What will the total income of the *other* group members be? _____

2. Each group member has 20 points. You invest 20 points in the project. Each of the other three members of the group also contributes 20 points to the project.

What will *your* total income be? _____

What will the total income of the *other* group members be? _____

3. Each group member has 20 points. The other 3 members contribute a total of 30 points to the project.
   a) What will *your* total income be, if you – in addition to the 30 points – invest 0 points into the project?

Your **Income _____**

   b) What will *your* total income be, if you – in addition to the 30 points – invest 8 points into the project?

Your **Income _____**

   c) What will *your* total income be, if you – in addition to the 30 points – invest 15 points into the project?

Your **Income _____**

4. Each group member has 20 points at his or her disposal. Assume that you invest 8 points to the project.

a) What is your total income if the other group members – in addition to your 8 points – contribute another 7 points to the project?

***Your* Income _____**

b) What is your total income if the other group members – in addition to your 8 points – contribute another 12 points to the project?

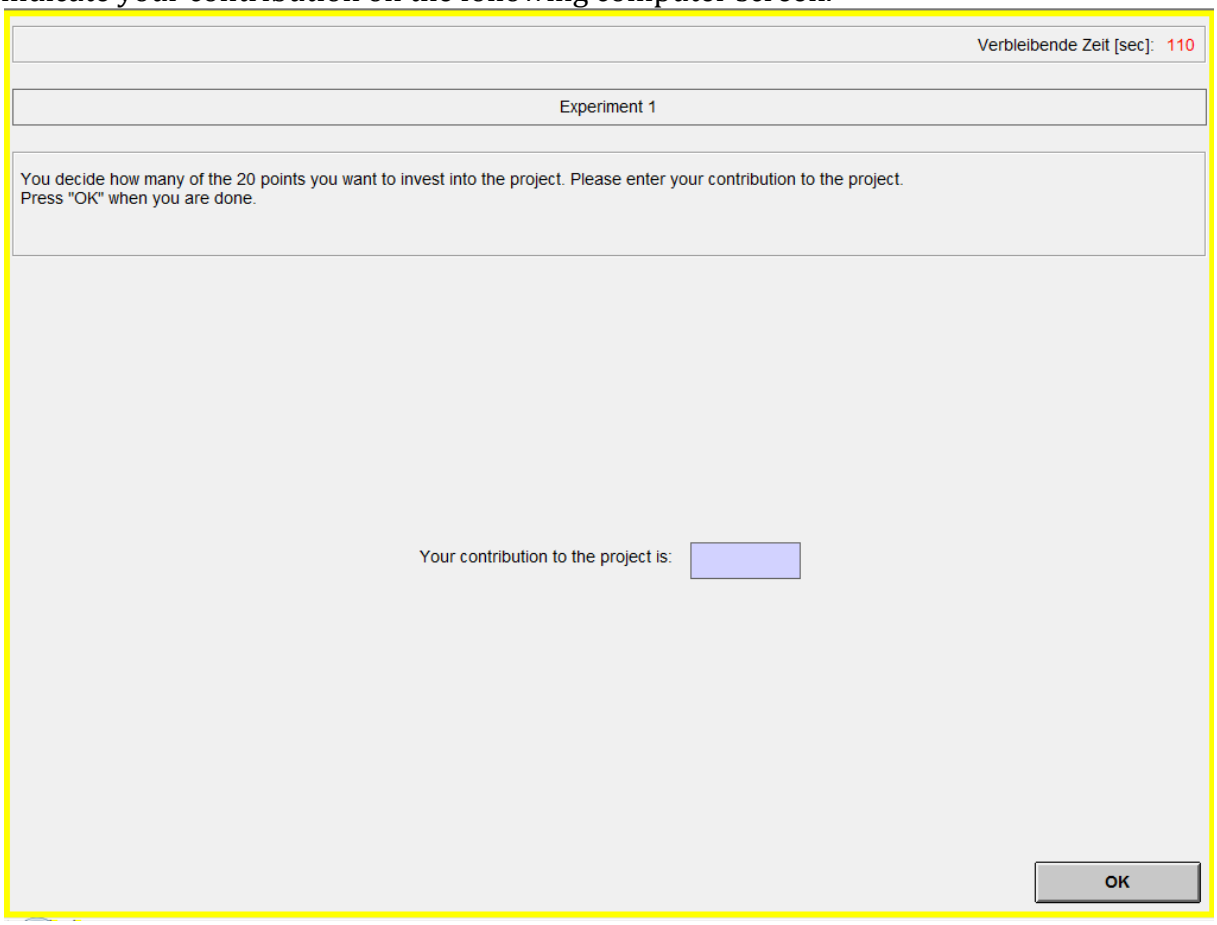
***Your* Income _____**

c) What is your income if the other group members – in addition to your 8 points – contribute another 22 points to the project?

***Your* Income _____**

Experiment 1

The experiment 1 includes the decision situation just described to you.
As you know, you will be a member of a group consisting of 4 persons and you will have 20 points at your disposal. You can put them into a private account or you can invest them into a project.
You decide how many of the 20 points you want to invest into the project. Please indicate your contribution on the following computer screen.



After you have determined your contribution, please click “OK”.

Experiment 2

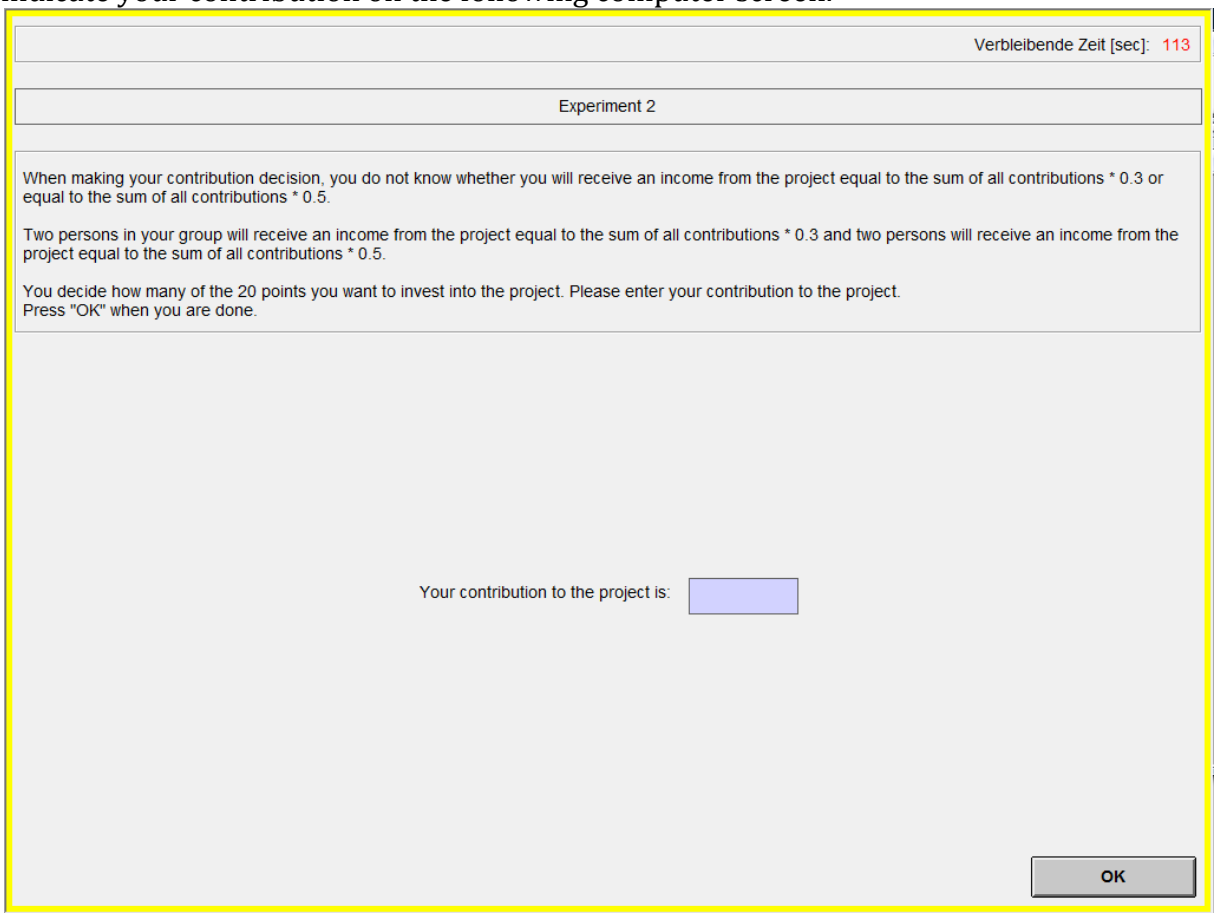
The experiment 2 consists of the basic decision situation, except for one change.
As you know, you will be a member of a group consisting of 4 persons and you will have 20 points at your disposal. You can put them into a private account or you can invest them into a project.
Your income from the project is different from the basic decision situation. In your group, two persons will receive an income from the project equal to:

***Income from the project** = sum of all contributions × 0.3*

And, two persons will receive an income from the project equal to:

***Income from the project** = sum of all contributions × 0.5*

When making your contribution decision, you do not know whether you will receive an income from the project equal to the sum of all contributions × 0.3 or equal to the sum of all contributions × 0.5. But you know that two persons in your group will receive an income from the project equal to the sum of all contributions × 0.3 and two persons will receive an income from the project equal to the sum of all contributions × 0.5.
You decide how many of the 20 points you want to invest into the project. Please indicate your contribution on the following computer screen.



After you have determined your contribution, please click “OK”.

The random selection of the income from the project will be implemented as follows. Each group member is assigned a number between 1 and 4. As you remember, a participant was randomly selected at the beginning of our experimental session. This participant will throw a 6-sided die at the very end of the experimental session. The resulting number will be entered into the computer.

Your income from the project will be equal to the sum of all contributions × 0.5 or × 0.3, depending on the result of the 6-sided die and on your membership number according to the following table:

Your income from the project will be equal to the sum of all contributions × ...

If the result of the die is:	If your membership number is: 1	2	3	4
1	**0.3**	**0.3**	**0.5**	**0.5**
2	**0.3**	**0.5**	**0.3**	**0.5**
3	**0.3**	**0.5**	**0.5**	**0.3**
4	**0.5**	**0.3**	**0.3**	**0.5**
5	**0.5**	**0.3**	**0.5**	**0.3**
6	**0.5**	**0.5**	**0.3**	**0.3**

Control questions

Please answer the following control questions. They will help you to gain an understanding of the calculation of your income, which varies with your decision about how you distribute your 20 points. *Please answer all the questions and write down your calculations.*

Assume that your membership number is 1.

1. Each group member has 20 points. Assume that none of the four group members (including you) contributes anything to the project. The result of the 6-sided die thrown at the end of the experiment is 4.

What will *your* total income be? _____
What will the total income of the group member 2 be? _____
What will the total income of the group member 3 be? _____
What will the total income of the group member 4 be? _____

2. Each group member has 20 points. You invest 20 points in the project. Each of the other three members of the group also contributes 20 points to the project. The result of the 6-sided die thrown at the end of the experiment is 2.

What will *your* total income be? _____
What will the total income of the group member 2 be? _____
What will the total income of the group member 3 be? _____
What will the total income of the group member 4 be? _____

3. Each group member has 20 points. The other 3 members contribute a total of 30 points to the project. The result of the 6-sided die thrown at the end of the experiment is 1.

a) What will *your* total income be, if you – in addition to the 30 points – invest 0 points into the project?

***Your* Income _____**

b) What will *your* total income be, if you – in addition to the 30 points – invest 8 points into the project?

***Your* Income _____**

c) What will *your* total income be, if you – in addition to the 30 points – invest 15 points into the project?

***Your* Income _____**

4. Each group member has 20 points at his or her disposal. Assume that you invest 8 points to the project. The result of the 6-sided die thrown at the end of the experiment is 5.

a) What is your total income if the other group members – in addition to your 8 points – contribute another 7 points to the project?

***Your* Income _____**

b) What is your total income if the other group members – in addition to your 8 points – contribute another 12 points to the project?

***Your* Income _____**

c) What is your income if the other group members – in addition to your 8 points – contribute another 22 points to the project?

***Your* Income _____**

Experiment 3

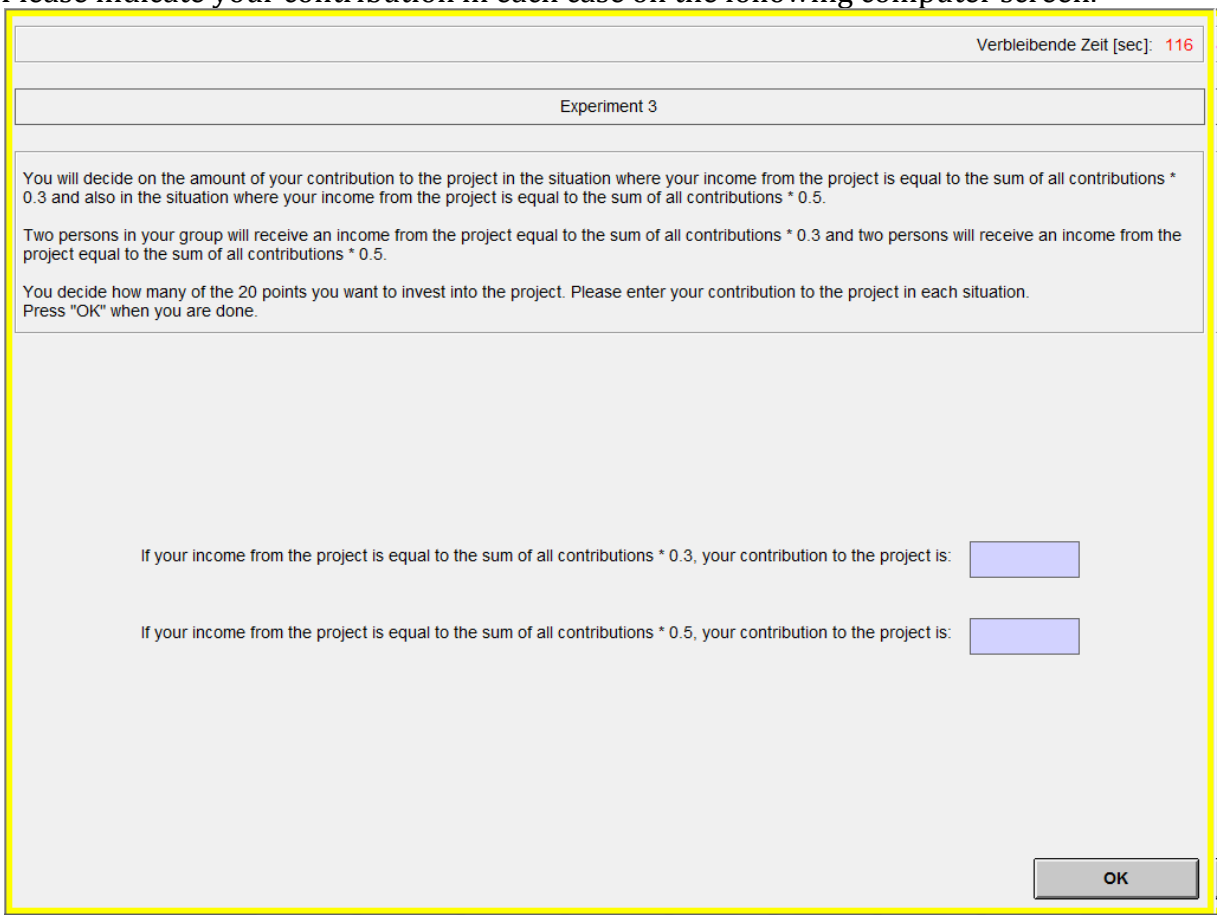
The experiment 3 consists of the situation in the experiment 2 with one change.
As you know, you will be a member of a group consisting of 4 persons and you will have 20 points at your disposal. You can put them into a private account or you can invest them into a project.
As in experiment 2, in your group, two persons will receive an income from the project equal to:

Income from the project = *sum of all contributions* × *0.3*

And, two persons will receive an income from the project equal to:

Income from the project = *sum of all contributions* × *0.5*

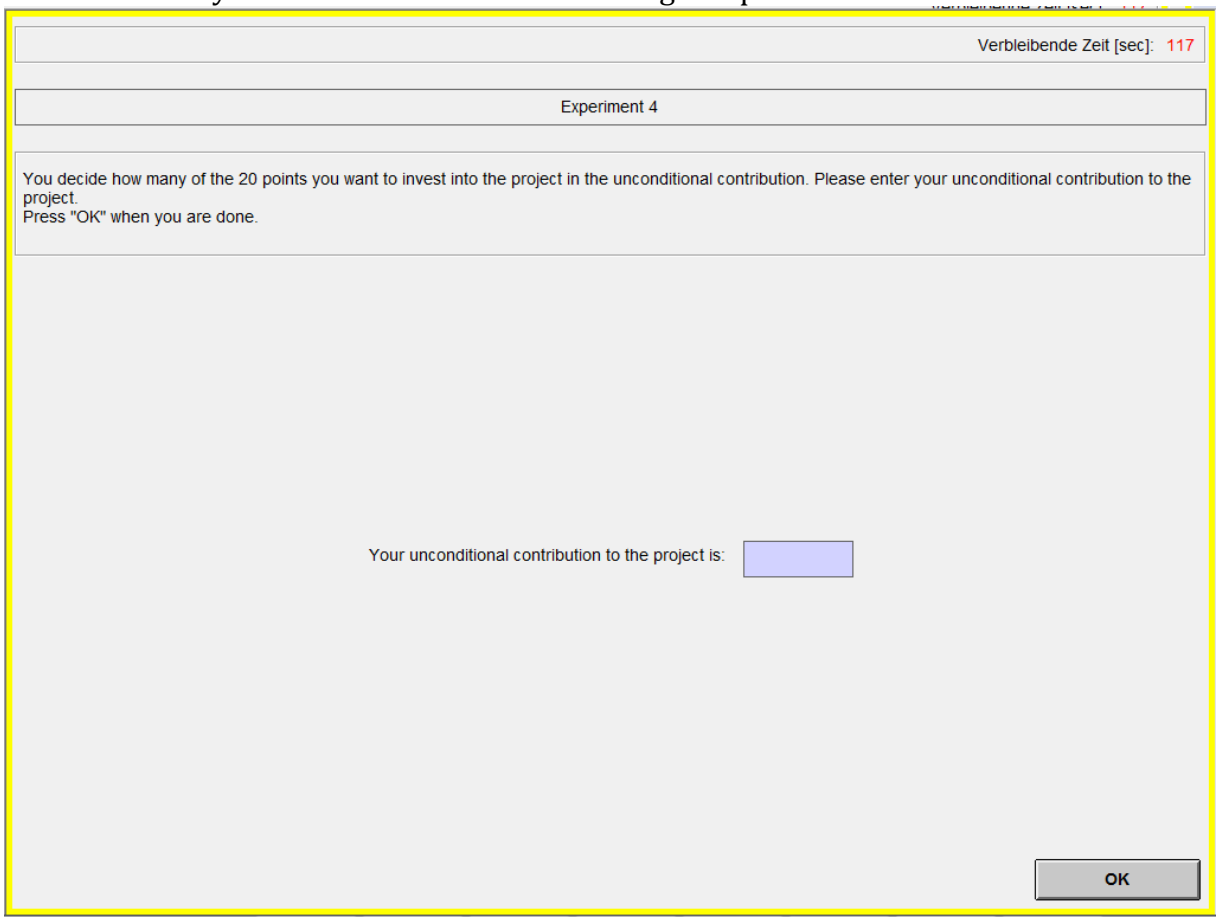
Differently from experiment 2, you will decide on the amount of your contribution to the project for each situation, i.e. if your income from the project is equal to the sum of all contributions × 0.3 and also if your income from the project is equal to the sum of all contributions × 0.5. Recall that two persons in your group will receive an income from the project equal to the sum of all contributions × 0.3 and two persons will receive an income from the project equal to the sum of all contributions × 0.5.
You decide how many of the 20 points you want to invest into the project if your income from the project is equal to the sum of all contributions × 0.3 and also if it is equal to the sum of all contributions × 0.5.
Please indicate your contribution in each case on the following computer screen.



After you have determined your contributions, please click “OK”.
The random selection of the income from the project is implemented as in experiment 2.

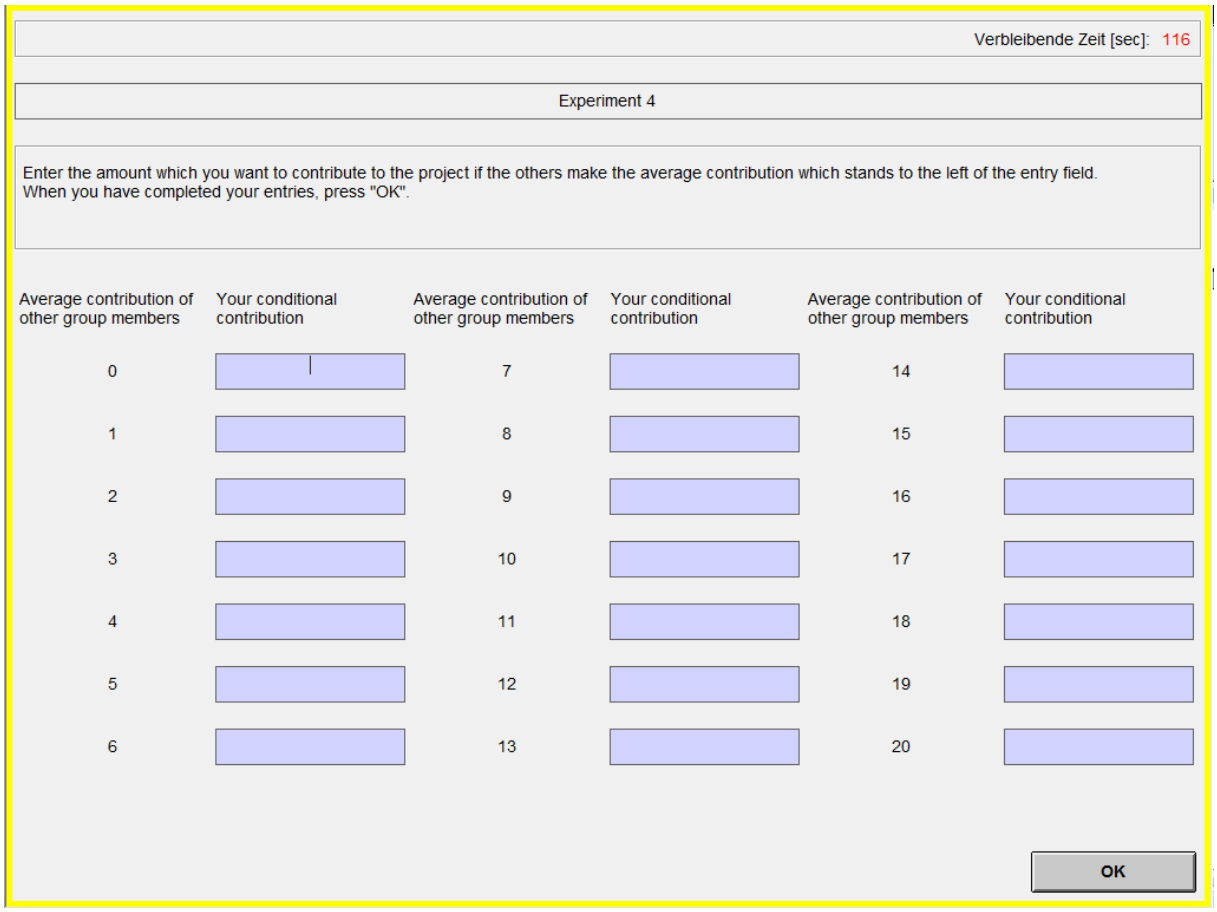
Experiment 4

The experiment 4 includes the basic decision situation just described to you at the beginning of the experimental session.
As you know, you will be a member of a group consisting of 4 persons and you will have 20 points at your disposal. You can put them into a private account or you can invest them into a project.
In this experiment 4, each subject has to make **two types** of decisions, which we will refer to below as the **"unconditional contribution"** and **"contribution table"**.
• You decide how many of the 20 points you want to invest into the project in the **unconditional contribution**.
Please indicate your contribution in the following computer screen:



After you have determined your unconditional contribution, please click "OK".

• Your second task is to fill in a **contribution table** where you indicate how many points **you want to contribute** to the project **for each possible average contribution of the other group members** (rounded to the next integer). You can condition your contribution on that of the other group members. This will be immediately clear to you if you take a look at the following table. This table will be presented to you in the experiment:



The numbers are the possible (rounded) average contributions of the **other** group members to the project. You simply have to insert how many points you will contribute to the project into each input box – conditional on the indicated average contribution. **You have to make an entry into each input box**. For example, you will have to indicate how much you contribute to the project if the others contribute 0 points to the project, how much you contribute if the others contribute 1, 2, or 3 points, etc. You can insert **any integer numbers from 0 to 20** in each input box. Once you have made an entry in each input box, click "OK".

After all participants have made an unconditional contribution and have filled in their contribution table, a random mechanism will select a group member from every group. Only **the contribution table will be the payoff-relevant decision for the randomly determined subject**. Only the **unconditional contribution** will be the payoff-relevant decision for the **other three group members** not selected by the random mechanism. You obviously do not know whether the random mechanism will select you when you make your unconditional contribution and when you fill in the contribution table. You will therefore have to think carefully about both types of decisions because both can become relevant for you. Two examples should make this clear.

EXAMPLE 1: Assume that **the random mechanism selects you. This implies that your relevant decision will be your contribution table**. The unconditional contribution is the relevant decision for the other three group members. Assume they made unconditional contributions of 0, 2, and 4 points. The average contribution of these three group members, therefore, is 2 points. If you indicated in your contribution table that you will contribute 1 point if the others contribute 2 points on average, then the total contribution to the project is given by 0+2+4+1=7. All group members, therefore, earn 0.4×7=2.8 points from the project plus their respective income from the private account. If, instead, you indicated in your contribution table that you would

contribute 19 points if the others contribute two points on average, then the total contribution of the group to the project is given by 0+2+4+19=25. All group members therefore earn 0.4×25=10 points from the project plus their respective income from the private account.

EXAMPLE 2: Assume **that the random mechanism did not select you,** implying that **the unconditional contribution is taken as the payoff-relevant decision** for you and two other group members. Assume your unconditional contribution is 16 points and those of the other two group members are 18 and 20 points. Your average unconditional contribution and that of the two other group members, therefore, is 18 points. If the group member whom the random mechanism selected indicates in her contribution table that she will contribute 1 point if the other three group members contribute on average 18 points, then the total contribution of the group to the project is given by 16+18+20+1=55. All group members will therefore earn 0.4×55=22 points from the project plus their respective income from the private account. If, instead, the randomly selected group member indicates in her contribution table that she contributes 19 if the others contribute on average 18 points, then the total contribution of that group to the project is 16+18+20+19=73. All group members will therefore earn 0.4×73=29.2 points from the project plus their respective income from the private account.

The random selection of the participants will be implemented as follows. Each group member is assigned a number between 1 and 4. As you remember, a participant was randomly selected at the beginning of the experiment. This participant will throw a 4-sided die at the very end of the experiment. The resulting number will be entered into the computer. If the die indicates the membership number that was assigned to you, then your contribution table will be relevant for you and the unconditional contribution will be the payoff-relevant decision for the other group members. Otherwise, your unconditional contribution is the relevant decision.

Experiment 5

The experiment 5 consists of the decision situation you just played in experiment 4, except for one change.

Your income from the project is different from the basic decision situation. In your group, two persons will receive an income from the project equal to:

Income from the project = *sum of all contributions × 0.3*

And, two persons will receive an income from the project equal to:

Income from the project = *sum of all contributions × 0.5*

When making your contribution decision, you do not know whether you will receive an income from the project equal to the sum of all contributions × 0.3 or equal to the sum of all contributions × 0.5. But you know that two persons in your group will receive an income from the project equal to the sum of all contributions × 0.3 and two persons will receive an income from the project equal to the sum of all contributions × 0.5.

As in the experiment 5, you have two tasks to complete.

• Your first task is to decide how many of the 20 points you want to invest into the project in the **unconditional contribution**. After you have determined your conditional contribution, please click "OK".

• Your second task is to fill in a **contribution table** where you indicate how many points **you want to contribute** to the project **for each possible average contribution of the other group members** (rounded to the next integer). You can condition your contribution on that of the other group members. Once you have made an entry in each input box, click "OK".

As in experiment 2, **the random selection of the income from the project** will be implemented as follows. Each group member is assigned a number between 1 and 4. As you remember, a participant was randomly selected at the beginning of our experimental session. This participant will throw a 6-sided die at the very end of the experimental session. The resulting number will be entered into the computer.

Your income from the project will be equal to the sum of all contributions × 0.5 or × 0.3, depending on the result of the 6-sided die and on your membership number according to the following table:

Your income from the project will be equal to the sum of all contributions × ...

If the result of the die is:	If your membership number is: 1	2	3	4
1	**0.3**	**0.3**	**0.5**	**0.5**
2	**0.3**	**0.5**	**0.3**	**0.5**
3	**0.3**	**0.5**	**0.5**	**0.3**
4	**0.5**	**0.3**	**0.3**	**0.5**
5	**0.5**	**0.3**	**0.5**	**0.3**
6	**0.5**	**0.5**	**0.3**	**0.3**

The random selection of the participants is identical as just presented in experiment 4.

Experiment 6

The experiment 6 consists of the situation in the experiment 5 with one change.
As you know, you will be a member of a group consisting of 4 persons and you will have 20 points at your disposal. You can put them into a private account or you can invest them into a project.
As in experiment 5, in your group, two persons will receive an income from the project equal to:

Income from the project = *sum of all contributions × 0.3*

And, two persons will receive an income from the project equal to:

Income from the project = *sum of all contributions × 0.5*

Differently from experiment 5, you will decide on the amount of your contribution to the project for each situation, i.e. if your income from the project is equal to the sum of all contributions × 0.3 and also if your income from the project is equal to the sum of all contributions × 0.5. Recall that two persons in your group will receive an income from the project equal to the sum of all contributions × 0.3 and two persons will receive an income from the project equal to the sum of all contributions × 0.5.
As in the experiments 4 and 5, you have two tasks to complete.
• Your first task is to decide how many of the 20 points you want to invest into the project in the **unconditional contribution** when your income from the project is equal to the sum of all contributions × 0.5 and also when it is equal to the sum of all contributions × 0.3. After you have determined your conditional contribution, please click "OK".
• Your second task is to fill in a **contribution table** where you indicate how many points **you want to contribute** to the project **for each possible average contribution of the other group members** (rounded to the next integer). You will enter first the contribution table if your income from the project is equal to the sum of all contributions × 0.5 and second the contribution table if your income from the project is equal to the sum of all contributions × 0.3. Once you have made an entry in each input box, click "OK".

The **random selection of the income from the project** and the **random selection of the participants** are organized as previously.

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Erklärung

Ich erkläre hiermit, dass ich die vorliegende Arbeit mit dem Titel

Experimental studies on cooperation and coordination in politics, firms and society

ohne unzulässige Hilfe Dritter und ohne Benutzung anderer als der angegebenen Hilfsmittel angefertigt habe. Die aus anderen Quellen direkt oder indirekt übernommenen Daten und Konzepte sind unter Angabe der Quelle gekennzeichnet. Weitere Personen, insbesondere Promotionsberater, waren an der inhaltlich materiellen Erstellung dieser Arbeit nicht beteiligt.[71] Die Arbeit wurde bisher weder im In- noch im Ausland in gleicher oder ähnlicher Form einer anderen Prüfungsbehörde vorgelegt.

Konstanz, den 1.Dezember 2011 _____

(Simeon Schudy)

[71] Siehe hierzu die Abgrenzung auf der folgenden Seite.

Abgrenzung

Ich versichere hiermit, dass ich die Kapitel 1 bis 4 der vorliegenden Arbeit ohne unzulässige Hilfe Dritter und ohne Benutzung anderer als der angegebenen Hilfsmittel angefertigt habe.

Kapitel 1: *Reciprocity and Resistance to Comprehensive Reform* entstammt einer gemeinsamen Arbeit mit Herrn Prof. Dr. Urs Fischbacher (Universität Konstanz) und basiert auf meiner Idee. Das experimentelle Design wurde gemeinsam erarbeitet, die Programmierung und Durchführung des Experiments habe ich selbstständig durchgeführt. Die Analyse wurde in gemeinsamer Zusammenarbeit erstellt. Die erste Version des Artikels entstammt meiner Hand und wurde gemeinsam überarbeitet.

Kapitel 2: *Agenda Setting and Reciprocal Vote Trading* entstammt ebenfalls einer gemeinsamen Arbeit mit Herrn Prof. Dr. Urs Fischbacher. Es basiert auf einer Idee von Urs Fischbacher. Das experimentelle Design wurde gemeinsam erarbeitet. Programmiert und durchgeführt wurde das Experiment durch mich. Auch die Analyse stammt zum überwiegenden Teil von mir. Die erste Version des Artikels entstammt meiner Hand und wurde gemeinsam überarbeitet.

Kapitel 3: *Competition Within Firms* entstammt einer gemeinsamen Arbeit mit Frau Prof. Dr. Lisa Bruttel (Universität Konstanz). Die Grundidee stammt von Lisa Bruttel. Die Umsetzung in ein experimentelles Design, die statistische Analyse sowie der Artikel wurden gemeinsam erarbeitet.

Kapitel 4: *Heterogeneous Reactions to Heterogeneity in Returns from Public Goods* entstammt einer gemeinsamen Arbeit mit Frau Dr. Sabrina Teyssier (INRA – ALISS Paris) und Herrn Prof. Dr. Urs Fischbacher. Das experimentelle Design wurde gemeinsam erarbeitet. Sabrina Teyssier und ich programmierten das Experiment. Ich führte die Experimente durch und erstellte große Teile der statistischen Analyse sowie eine erste Version des Artikels, die gemeinsam überarbeitet wurde.

Konstanz, den 1.Dezember 2011 _____

(Simeon Schudy)