Cooperation with Finite-Lived Committee Members in Indefinitely Repeated Games*

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Abstract

We study cooperation in an indefinitely repeated trust game between an individual and committees of different organizational structure in the laboratory. More specifically, we study committees of trustees with limited terms. A game-theoretic analysis implies that cooperation may or may not emerge for a committee structure with overlapping terms, but that trust is impossible to achieve with synchronized terms or when the committee is replaced by a single finitely lived trustee. Using a laboratory experiment, we find that individuals choose different strategies in the different treatments and that cooperation is more stable under overlapping terms. However, cooperation arises in all three treatments with no differences in the average cooperation rates. This may indicate that cooperation is driven primarily by intrinsic behavioral motivation rather than a desire to influence the future decisions of others.

Keywords: cooperation, committees, repeated games, time inconsistency.

JEL codes: D71, C92.

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1 Introduction

The success of most organizations depends on their ability to establish cooperation with outsiders. Such cooperation can only be achieved when the outsiders trust the organization. Central banks can achieve low and stable inflation much more effectively if citizens are confident that the central bank will be successful at stabilizing inflation in the future.[1] The performance of other agencies such as the police, public administration, or commercial banks largely depends on the trust they receive from citizens or customers. For example, in the case of banks, depositors' concerns about the stability of the institution can lead to bank runs, which puts the survival of a bank at risk. In this study, we examine which institutional structures help inducing trust from the outsider, thereby promoting cooperation from both sides.

Specifically, we focus on two institutional dimensions. The first dimension concerns whether the organizational structure is individualistic or collectivist. Important decisions in an organization may be made by either an individual or a group of decision-makers. While most central banks have committees that take monetary policy decisions, they are located somewhere between an individualistic and a collectivist structure, as, in most cases, the president assumes a particularly powerful and prominent role.[2] In an individualistic organization, turnover may make it difficult to adopt policies that are desirable for both parties in the long term but costly in the short term. This may result in low levels of trust. On the other hand, turnover may also create opportunities to re-establish trust when past levels of trust were low. Compared to an individualistic organization, a collectivist organization provides more continuity, which may foster cooperation and trust.[3] However, the diffusion of responsibilities among multiple decision-makers may also make the organization opaque and hence less trustworthy.

Second, we focus on the potential role of announcement of an official strategy. Many organizations have mission statements. Relatedly, central banks typically announce official strategies such as a specific variant of inflation targeting. These statements may play a dual role. They serve as a guideline for decision-makers within the organization and influence their behaviors.

[1]This is a central implication of the new Keynesian model, where current inflation depends on expectations about future inflation. See Woodford (2003) for a textbook treatment.

[2]For example, it is common to label different periods of U.S. monetary policy after chairpersons of the FOMC ("Volcker Disinflation", "Volcker-Greenspan era", "pre-Volcker era",...).

[3]Kroszner and Stratmann (2000), for example, argue that committees facilitate interactions and thereby allow for reputation development in repeated interactions between legislators and interest groups.

These statements also signal the objectives of the organization to outsiders, which may be conducive to trust.

In this study, we propose a simple framework to examine how these two dimensions affect trust in organizations. In particular, we study repeated versions of a slightly adapted version of the canonical trust game (Berg et al., 1995) with binary choices. In each stage, an outsider (the trustor, "he") can send a share of his endowment to an organization (the trustee). If it does, the organization receives an additional surplus that captures the gains from cooperation. The decision-makers ("she") in the organization decide whether to send a given fraction of the received transfers back to the outsider. In this case, the outsider receives these transfers and an additional surplus. Trust facilitates cooperation in our framework, where cooperation is defined as a mutual exchange of transfers between the outsider and the organization. Cooperation is beneficial to both parties compared to a situation where no transfers are being made.

In the variant of our model that focuses on an individualistic organization, this stage game is embedded into a framework where the outsider interacts indefinitely often with an organization in which all decisions are made by a single individual with a fixed term in office. After the end of a decision-maker's term, she is replaced by a new decision-maker with the same fixed term length. The model variant formalizing the collectivist organization is identical, except that all decisions are made by a committee of overlapping generations of decision-makers with fixed individual terms.

We then examine the implications of this model, both theoretically and experimentally. In the case of an individualistic organization, standard backward-induction arguments imply that the outsider never considers the organization trustworthy and never transfers funds to the organization. This lack of trust is justified, as the organization would never reciprocate transfers. In the case of a collectivist organization, no unique equilibrium is obtained, even if we impose the refinement that non-pivotal players vote sincerely. Interestingly, there are equilibria with full cooperation; that is, the outsider always transfers his endowment to the organization, and the organization reciprocates the transfers. These equilibria implement Pareto-efficient allocations. However, in a collectivist organization, there are also equilibria in which no cooperation occurs and no transfers are made. Our theoretical predictions are thus not clear-cut.

This is one of the motivations for conducting an experiment. The second motivation is that cooperation occurs in experiments and even in one-shot interactions. The trustor transfers funds to the trustee based on the belief that they will reciprocate (see, for example, Berg et al., 1995). Hence, one might expect positive transfers and cooperation even in an individualistic organization. Moreover, from a theoretical perspective, cooperation is feasible only if the terms of members overlap but not if all members' terms end simultaneously.[4] However, in a lab experiment, Xu and Potters (2018) find overlapping terms are only mildly conducive to cooperation between members of an organization. Hence, it is instructive to examine whether an overlapping term structure has an effect under a collectivist organization. Therefore, our experimental analysis distinguishes between two different treatments of collectivist organizations: one with overlapping terms and one with synchronized terms.

We observe high average trust rates across treatments, with no significant differences in average trust and cooperation rates between individualistic and collectivist organizations. This suggests that organizational structures may play a minor role in shaping trusting behavior. Furthermore, in the individualistic treatment, despite a decrease in the sending rate in the last term, the trustor and decision-makers still exhibit a relatively high average sending rate. This suggests that a substantial fraction of decision-makers may act cooperatively even without anticipating future gains, suggesting that trust within a committee is influenced by underlying behavioral factors. However, there is an important difference between the treatments when it comes to the stability of cooperation. While the individualistic organization and the collectivist organization of synchronized terms show end-game effects, the collectivist organization of overlapping terms shows more stable cooperation rates.

As previously mentioned, we consider a second factor that potentially influences trust in an organization: mission statements. Examining mission statements is particularly relevant for a collectivist organization with overlapping terms, as mission statements may help players coordinate on Pareto-superior equilibria. The initial members of the organization are allowed to select one of the two pre-specified statements about the choices that the organization should make. One statement says that the organization will always cooperate, while the other statement says the opposite. Our empirical results show that the opportunity to make an announcement does not affect trust and cooperation. The trust and cooperation rates in the announcement treatment are comparable to those without an announcement.

[4] To be more precise, this statement is true only if we impose a refinement akin to trembling-hand perfection, which rules out equilibria where decision-makers vote against their interests as they are never pivotal.

Moreover, we estimate what strategies the participants play. While many studies have been conducted to analyze strategies in the indefinitely repeated Prisoners' Dilemma (for a recent overview, see, for example, Dal Bó and Fréchette, 2018), to the best of our knowledge, there are only three studies that experimentally examine indefinitely repeated trust games. Their focus is either on the comparison of finitely repeated and indefinitely repeated interactions of individual decision-makers (Engle-Warnick and Slonim, 2004, 2006) or on gradual trust building between individuals in a generalized version of the trust game (Kartal et al., 2021). They do not consider the institutional design addressed in this paper. According to our estimation, the trustor punishes the organizations when the organization was not trustworthy in the previous round. The length of punishment differs depending on the organizational structure. Dvorak and Fehrler (2019) study the effect of communication in the indefinitely repeated Prisoners' Dilemma and find, among other things, that pre-play communication is very effective in reducing strategic uncertainty and makes participants play cooperative and lenient strategies. In the experiment analyzed in this paper, the opportunity to select one out of two mission statements does not affect overall trust and cooperation.

Our study relates to the broad literature on social capital and trust between agents (Durlauf and Fafchamps, 2004; La Porta et al., 1997). Many empirical studies find that trust has benign economic consequences (Zak and Knack, 2001; Knack and Keefer, 1997; Guiso et al., 2004; Tabellini, 2010). Glaeser et al. (2000) focus on how to measure trust precisely and distinguish between trust and trustworthiness. The tension between long-lived organizations and relatively short-lived members is theoretically examined by Cremer (1986), Salant (1991) and Smith (1992). Xu and Potters (2018) and Offerman et al. (2001) study such set-ups in the laboratory. In contrast to the present study, these studies consider cooperation between members of an organization rather than between an organization and an outside player. Our study also contributes to literature which compares trust in both individual and collective settings, such as Holm and Nystedt (2010). A key distinction is that our study focuses on a repeated game.

When interpreting the outsider as the public, our study also relates to studies that consider the trust of the public in a specific organization, in most cases, a central bank. Empirical studies find that macroeconomic outcomes influence trust in the European Central Bank (Fischer and Hahn, 2008; Gros and Roth, 2010; Farvaque and Mihailov, 2012; Ehrmann et al., 2013; Bursian and Fürth, 2015). The role of the institutional characteristics of central banks in mitigating the time-inconsistency problem in monetary policy, which goes back to Barro and Gordon (1983) and Kydland and Prescott (1977), has been considered by several theoretical analyses.

In particular, Sibert (2003) and Hansen and McMahon (2016) examine the benefits of collective and individual decision-making in central banks. In contrast to our approach, these studies examine signaling models with different types of central bankers. Other contributions to time-inconsistency problems and monetary policy committees include Dal Bo (2006), who examines optimal super-majority rules, and Riboni (2010), who focuses on the role of the status quo as the default option in committees. Kugler et al. (2007), who experimentally study trust between individuals and between groups, find that individuals are as trustworthy as groups. We extend their finding to a set-up where groups potentially enjoy the additional advantage that they interact with outsiders over longer time horizons compared to individuals.

In the next section, we set up the model and derive theoretical predictions for the experiment. In Section 3, we describe our experimental design. In Section 4, we present our main hypotheses and research questions. Section 5 presents the results of the study. The Appendix includes the proof of one of our theoretical predictions, details of our simulations for the power calculation, screenshots, and instructions of the experiment.

2 Model

2.1 Set-Up

We consider four versions of an indefinitely repeated trust game with binary choices. The first scenario, which considers an individualistic organization with a single decision-maker, is labeled "I." The second scenario ("C") focuses on a collectivist organization with overlapping terms. Third, a collectivist organization with synchronized terms is described by model variant "CST." Finally, we consider a variant of the second scenario with the possible announcement of a mission ("CA").

We first describe the stage game for scenario I. The organization comprises three members: the decision-maker and two passive players. The decision-maker makes all decisions on behalf of the organization. The passive players make no choices in scenario I. The organization interacts with another player: the outsider. At the beginning of the stage, all players receive an endowment E. The outsider can choose to send T_1 ($T_1 < E$) to the organization. In this case, each of the

Figure 1: Trust Game with Binary Choices in Normal Form

	$Send_2$	$Not\ Send_2$
$Send_1$	$E-T_1+R_2, E-T_2+R_1$	$E-T_1, E+R_1$
$Not\ Send_1$	E, E	E, E

three members receives R_1. We assume $3R_1 > T_1$ to capture efficiency gains from cooperation. If the outsider does not transfer resources to the decision-maker, the stage game ends and all players' payoffs are E.

The decision-maker can choose simultaneously whether the organization should keep the entire transfer. If the outsider sends the transfer and the decision-maker decides not to send back the transfer, all members' payoffs are $E + R_1$ and the payoff of the outsider is $E - T_1$. If the organization receives a transfer while deciding to send back a transfer at fixed sum T_2 ($T_2 < R_1$) per member to the outsider, the outsider receives R_2. Thus the payoffs are $E_1 - T_1 + R_2$ for the outsider and $E + R_1 - T_2$ for members of the organization. Analogous to the assumption $3R_1 > T_1$, we impose $R_2 > 3T_2$ to describe efficiency gains from cooperation. We note that full cooperation, i.e. when both parties make transfers, maximizes aggregate payoffs. If no transfers are made, i.e. in the absence of cooperation, all players' payoffs are strictly lower in comparison.

It remains to describe how this stage game is extended to a repeated game in scenario I. The interaction between the outsider and the organization is of indefinite length: After every round there is a continuation probability $\delta \in (0, 1)$. With probability $(1-\delta)$ the interaction ends after every round.[5] Hence, the outsider has an indefinite time horizon, whereas all members of the organization have a fixed term in office, which we assume to be three periods. In period $t = 1$, the decision-maker starts in the first period of her term. One passive member is in the second period of her term and one passive member is in the last period of her term.

Retiring members are always replaced by new members with fixed terms of three periods. Passive members are replaced by new passive members. The decision-maker is replaced by a

[5]Note that this is equivalent to assuming that time $t = 1, 2, ...$ is infinite and payoffs in future periods are discounted by the common factor $\delta \in (0, 1)$.

new decision-maker. Members only receive payoffs while in office. New members always observe the entire history of choices made by all players.

Scenario C differs from scenario I only in that decisions are not made by a single individual but by all three members of the organization. Decisions on the transfers are taken by majority rule. As before, all members' payoffs are identical and terms overlap. The outsider observes the outcome of the vote but does not receive any information about individual votes.

Scenario CST, in turn, is almost identical to scenario C. The only difference is that the terms of the three decision-makers are synchronized. In period 1, all decision-makers are in their first term in office. After periods $t = 3, 6, 9, ...$, all decision-makers are replaced by new office holders.

For the last scenario, CA, we add an additional initial phase to scenario C. In this phase, the committee members who will be in charge in period 1 have the opportunity to vote on two available mission statements regarding the future choices of the organization. For simplicity, we allow only for the statements that "send will be chosen" and that "send will not be chosen." The statement selected by the majority will be revealed to the outsider and all future members of the organization. The exact pattern of votes will not be revealed.

The equilibrium concept is subgame-perfect Nash equilibrium. As is well-known, voting games often have multiple equilibria as voters are indifferent when they are never pivotal. Thus we impose the following restriction: If a decision-maker is not pivotal with certainty, she votes for the option that would maximize her payoffs.[6]

This restriction will allow us to identify a unique equilibrium in scenario CST. However, scenario C admits multiple equilibria even if this refinement is imposed. It is of particular interest whether participants in the experiment are able to coordinate on an equilibrium with full cooperation, i.e. one that maximizes aggregate payoffs.

2.2 Theoretical Predictions

In the following, we derive the theoretical predictions for the different scenarios.[7]

[6]Obviously, this restriction has no bite in scenario I.

[7]The underlying assumption in this section is that subjects are purely selfish. While this is clearly at odds with the results of previous empirical studies of trust games, it helps to pin down the different strategic motives in the different organizational structures. In section 4, we address potentially relevant behavioral deviations (e.g., with respect to end game effects).

An individualistic organization (I) Applying a standard backward-induction argument leads to the finding that there is a unique subgame-perfect equilibrium, in which no transfers occur. We summarize this result as follows:

Theoretical Prediction 1. *An individualistic organization leads to no cooperation. In particular, the outsider never sends transfers to the organization. Decision-makers would never send transfers to the outsider.*

A collectivist organization with synchronized terms (CST) The equilibria are similar to the ones in scenario I. Consider, e.g., period 3, after which all decision-makers leave. If a decision-maker is pivotal with positive probability, it clearly maximizes her payoffs to send no transfers to the outsider. If she is pivotal with probability zero, her action cannot affect the outcome of the vote. Because of our equilibrium refinement, she nevertheless votes against sending transfers to the outsider. As a consequence, conditional on having received a transfer, all decision-makers vote in favor of not sending a transfer to the outsider. Anticipating this behavior, the outsider does not send transfers in the third period. Backward induction yields that also in periods 2 and 1, no player makes transfers. The theoretical prediction for the CST scenario is thus identical to the prediction for scenario I:

Theoretical Prediction 2. *A collectivist organization with synchronized terms leads to no cooperation. In particular, the outsider never sends transfers to the organization. Decision-makers would never vote in favor of sending transfers to the outsider.*

We would like to highlight that this prediction relies on our additional refinement about the behavior of non-pivotal players. Without this refinement, equilibria with cooperation would exist. Suppose all decision-makers always voted in favor of sending transfers to the outsider. Then no profitable deviation would exist for individual members of the organization. As a consequence, it would be optimal for the outsider to send transfers to the organization.

A collectivist organization with overlapping terms (C) As a next step, we consider a collectivist organization with overlapping terms. In every period, there is a majority of decision-makers who remain in office for at least one additional period and therefore potentially benefit from future cooperation. Due to this feature, cooperation can occur in equilibrium.

Theoretical Prediction 3. *In a collectivist organization with overlapping terms, multiple equilibria exist. In particular, the following behaviors may occur in equilibrium:*

1. *The outsider never sends transfers to the organization, i.e. there is no cooperation.*
2. *Suppose that*

$$T_2 \leq \frac{\delta}{1+\delta} R_1. \tag{1}$$

   Then the following behaviors are supported by an equilibrium: The outsider always sends transfers to the organization. The organization always sends transfers in return. Hence there is full cooperation and aggregate payoffs are maximal in every period.

Proof. See Appendix A. □

Thus Condition (1) ensures that cooperation is possible. Intuitively, it guarantees that the costs of cooperating incurred by members of the organization, which are associated with T_2, are sufficiently small such that decision-makers do not wish to forgo the future gains from cooperation, which are positively influenced by R_1. As explained in Appendix A, cooperation cannot occur if (1) is violated.

A collectivist organization with the announcement of a mission statement (CA) As scenario C involves multiple equilibria, it is possible to construct equilibria with mission statements where the announcement has an effect on future behavior despite the fact that the statements represent cheap talk. While no clear-cut theoretical prediction emerges, the following results can be plausibly expected. First, we would expect the organization to announce that it intends to achieve cooperation, i.e. that it will transfer funds to the outsider. Second, it appears possible that such an announcement will help the players to select the payoff-maximizing equilibrium with full cooperation.

3 Experimental Design

We implement an indefinitely repeated binary trust game in four treatments using a between-subject design. In all treatments, the outsider repeatedly interacts with the organization. For all supergames, the continuation probability after every round δ is 5/6.[8] Thus, the expected length of each supergame is six. We achieve indefinite repetition through random termination. To maintain a consistent length of supergames across treatments, we pre-generate three sequences and use them to determine the length of each supergame. There are nine, five, and nine supergames with a total of 41, 40, and 39 rounds for the three sequences, respectively. Each of the three sequences is implemented for one-third of the participants in each treatment.[9] The treatments differ in the organizational structure.[10]

I (Treatment with an individualistic organizational structure.) An individual decision-maker interacts repeatedly with the outsider for three periods. At the end of the third period, the current decision-maker is then replaced by a new one.

C (Treatment with a collectivist organizational structure and overlapping terms.) The committee comprises three decision-makers of overlapping generations. At the end of each period, the participant representing the oldest generation exits the committee, making room for a new member who enters at the beginning of each period as the youngest member.

CST (Treatment with a collectivist organizational structure and synchronized terms.) Three decision-makers interact repeatedly for 3 periods with the outsider. At the end of the third period, all members retire and are replaced by three new members.

CA (Treatment with a collectivist organizational structure, overlapping terms and mission announcement.) The treatment differs from C by including an additional mission statement stage. Before each supergame, the committee votes on one of the two announcements:

[8] A "supergame" is one indefinitely repeated interaction, including all rounds until its random termination.

[9] We use Stata to generate three sequences of uniformly distributed random numbers between 0 and 1. In previous research projects, we used seeds 1-6. Accordingly, we utilize seeds 7-9 for the three sequences. For each sequence, we identify the positions of the numbers smaller than or equal to 1/6. Denoting these positions as n_1, n_2, n_3, ..., we compute the lengths of the supergames as n_1, $n_2 - n_1$, $n_3 - n_2$, ... This procedure results in the following length sequences: $(3, 2, 1, 17, 1, 1, 7, 7, 2)$, $(16, 10, 2, 11, 1)$ and $(6, 2, 2, 15, 2, 1, 5, 3, 3)$.

[10] See Appendix F for screenshots and instructions.

"We will always play *Send*" and "We will always play *Not Send*". The committee announcement is determined by the majority rule and remains displayed on the screens of all participants in the same group for the entire supergame.

In all treatments, the outsider consists of a single participant who remains in position until the supergame concludes. In each period, participants participate in the normal-form binary trust game, wherein the outsider and decision-makers simultaneously decide on actions rather than sequentially. In treatments C, CST, and CA, the organization's decision (referred to as the committee in the instructions) is determined by simple majority rule.

At the end of each round in all treatments, the outsider is informed of the individual decision-maker's decision or committee decision only if the outsider chooses *Send*. The individual votes of committee members in C, CA, and CST remain confidential to the outsider. The individual decision-maker always receives feedback on the outsider's decision. Committee members receive feedback on individual votes, the committee decision, and the outsider's choice.

Matching Groups For treatment I, each session comprises 27 participants, organized into three matching groups. Each matching group is further divided into three subgroups, each consisting of one participant in the role of the outsider, an individual decision-maker, and a waiting participant. At the beginning of each session, participants are randomly assigned to the three matching groups, and within each matching group, they are then randomly assigned to the role of the outsider, decision-maker, or the waiting pool.[11] Participants assigned to the role of the outsider retain this role throughout the entire session. Before the start of each supergame, participants within their matching group are randomly rematched. Those not in the role of the outsider are once again randomly assigned to a role. Across all sessions, each matching group is assigned a different sequence. Consequently, the lengths and numbers of supergames vary among the three matching groups.

Recall that in the theoretical model, a decision-maker serves a finite number of terms and then retires. Implementing this setup directly in the laboratory would be challenging due to the need for a very large number of participants. To address this, we need to allow for re-entry, while ensuring that the chance of re-entering the same group remains sufficiently low. We chose the following implementation: the individual decision-maker interacts repeatedly with the outsider

[11] The role of the waiting pool will be explained in detail below.

in the same group for three periods. Afterwards, she is replaced by the waiting participant of that group. She then waits in the next group for three periods before becoming a decision-maker there. For example, a decision-maker retiring from group 1 proceeds to wait in group 2 for three periods before taking the role of decision-maker there. Likewise, a decision-maker retiring from group 3 enters three waiting periods in group 1 before becoming a decision-maker in that group. Consequently, it takes 13 periods for a retired decision-maker to re-enter her initial group (initially in the waiting pool), and 16 periods to become a decision-maker again in her initial group. With our continuation probability $\delta = 5/6$, the likelihood of the latter is approximately only 5%, a sufficiently low value that does not impact our theoretical predictions.

For treatments C, CST, and CA, each session comprises one matching group. Within each matching group, there are three subgroups, each consisting of one participant in the role of the outsider, three decision-makers in the committee with either overlapping or synchronized terms, and a waiting pool of three participants.

Similar to treatment I, participants have the opportunity to re-enter, but the likelihood is equally small. All retired decision-makers transition to the next group, where they spend three periods waiting before joining that group's committee. Consequently, it once again takes 13 periods for a retired decision-maker to re-enter her initial group and 16 periods to resume the role of a decision-maker in that group. This design ensures that the laboratory implementation closely aligns with the theoretical scenario and guarantees that the treatments are identical in terms of the low re-entry probability. In all treatments, the choice history of both the decision-makers and the outsider is visible to all waiting participants in the same group.

Payment A potential concern with the individualistic setup in Treatment I is that the outsider's choice only influences the payment of one decision-maker, whereas it affects three players in the other treatments. To address this, we designed the payment structure in Treatment I in such a way that the outsider's choice also influences three players. To achieve this, we randomly select two participants from another matching group as 'passive members' of the organization. Although they are paid the same amount as the decision-maker, they do not participate in decision-making. Participants are unaware of whether they are chosen as passive members until the end of the session.

In all sessions, participants receive a show-up fee of EUR 5 in addition to their accumulated earnings over all rounds. The points they earn are converted to euros at an exchange rate of 5 cents per point. Since participants not in the role of the outsider spend half of the rounds (on average) in the waiting pool in the C, CST, and CA treatments, their earnings would be considerably lower compared to participants in the role of the outsider. In the I treatment, additional points are earned by participants not in the role of the outsider due to their selection as passive assistants. To address this discrepancy and increase the average earnings of participants not in the role of the outsider in C, CST, and CA, waiting participants receive a fixed wage of five points for each round they spend in the waiting pool.

Experimental Parameters Figure 2 shows the stage-game payoffs for all treatments. The outsider is the row player and the decision-makers are the column players. Players are endowed with $E = 5$. If the outsider does not transfer, players end up with their endowments. If the outsider transfers $T_1 = 4$, the (three) committee members receive $R_1 = 6$; that is, the transferred amount is multiplied by a factor of 3*1.5. When a transfer is received, the decision-maker(s) can send back $T_2 = 2$, which reduces her payoff to 9. The outsider receives $R_2 = 8$, that is, the back transfer (from all three committee members) T_2 is multiplied by a factor of 4.

We choose these parameters for the following reasons. First, they ensure that Condition 1 (as stated in Theoretical Prediction 3) holds; specifically, in a collectivist organization with overlapping terms, cooperation between the outsider and decision-makers can be sustained with $\delta = 5/6$. Second, the outsider is indifferent between "Send" and "Not Send" (in the absence of repeated game effects) if they consider decision-makers to be trustworthy with a probability of 50%. Third, both parties receive the same payoffs if "Send" is chosen by both and if "Not Send" is chosen by the outsider. Finally, the cooperation payoff 9 is efficient. [12]

Figure 2: Stage Game Parameters

	$Send_2$	$Not\ Send_2$
$Send_1$	9, 9	1, 11
$Not\ Send_1$	5, 5	5, 5

Sessions All treatments are programmed in z-Tree (Fischbacher, 2007). Participants are recruited via hroot (Bock et al., 2014). In total, we conduct 20 sessions: 3 sets of 6 sessions

[12]Reciprocating transfers result in a loss of 2 for the decision-maker in the individualistic treatment and an aggregate loss of 6 in the collectivist treatments. At the same time, the outsider gains 8. Consequently, cooperation leads to an increase in efficiency.

each with one matching group of 21 participants for the C, CA, and CST treatments, and 2 sessions each with three matching groups of nine participants for the I treatment. This brings the total number of participants to 432.[13] We conducted 10 sessions at the WISO Experimental Lab of the University of Hamburg in 2021 and 2022, and 10 at LakeLab of the University of Konstanz in 2022 and 2023.[14] Each session lasted less than two hours, and participants were able to comprehend the matching protocol.[15] These questions prompted participants to reflect on their decision-making, addressing aspects such as the perceived importance of their decisions and whether organizational structures influenced their choices. The complete questionnaire and a summary of the answers can be found in the Appendix. No differences in answers were detected between the treatments.

4 Main Hypotheses and Research Questions

Based on the theoretical reasons outlined in the previous sections and drawing insights from previous studies on finite and indefinite repetitions of a stage game, we anticipate higher cooperation in the C treatment than in the CST and I treatments. Furthermore, we expect that an announcement of an organizational mission leads to higher trust in the CA treatment than in the C treatment. We formulate our hypotheses, which directly follow from the theoretical predictions in Section 3.

H1s: *Cooperation rates are expected to be higher in the C treatment compared to each of the CST and I treatments. Additionally, cooperation rates are anticipated to be higher in the CA treatment than in the C treatment during the last five supergames.*

We test the three corresponding null hypotheses (H0s) by comparing cooperation rates within the pairs of treatments C–I, C–CST, and CA–C. Each group is treated as an independent

[13] Our simulations (see Appendix B) suggest that we have enough power ($> 87\%$) to detect effect sizes of 15 percentage points at the 5% level with a one-sided $t-$test and 6 matching groups per treatment. For the simulations we assumed trust probabilities of 0.4 for I (CST or C) and 0.55 for C (C or CA). Power would increase substantially if we assumed trust probabilities closer to 0 or 1 (for example, 0.1 and 0.25 or 0.8 and 0.95). This is because the variance of the Bernoulli distribution, $p(1-p)$, is largest at $p = 0.5$.

[14] We divided our sessions so that each lab conducted half of the sessions of all treatments for every random sequence.

[15] After the sessions in Hamburg had been completed, we decided to include seven additional questions.

observation, and we cluster at the matching group level for one-sided t-tests comparing cooperation rates between treatments. Additionally, we conduct a two-sided t-test to assess the null hypothesis that there is no difference in the trust rate between I and CST, as predicted.

Apart from our main hypotheses, we answer the following research questions:[16]

Question 1: Does the tenure of decision-makers influence their choices?

Theoretical considerations suggest that in treatment C, decision-makers in the final term lack an incentive to send back the transfer, while those in the first and second terms have an incentive to do so. Consequently, we anticipate a lower frequency of sending back for decision-makers in the final term. In treatments CST and I, our first and second theoretical predictions posit that cooperation is not possible, leading decision-makers to consistently decide "Not Send" in each term. Thus their tenure is irrelevant to their decisions. However, participants' choices may be term-dependent due to behavioral motivations. Since decision-makers serve for three terms, they may consider sending back in the first term or the first two terms if this allows them to signal pro-social preferences (for reciprocity) to the outsider and keeping the money only in the last term. This end-game effect results in a lower frequency of sending back in term 3 compared to the first two terms. Finally, the interaction between the mission announcement and tenure is uncertain. If decision-makers adhere to group announcements, we anticipate less inconsistency in their choices among terms, as they are likely to vote for the same decision which has been announced in every term. However, the decision-maker in the last term has an incentive to deviate from the announced choice, particularly when this announcement is made by former organization members.

Question 2: How does cooperation change over rounds?

In a collectivist organization with overlapping terms, the composition of the committee remains stable, ensuring that there is always one member who has just joined, one member who joined one period ago, and one member who is about to leave. As a result, the frequency of playing "Send" at the committee level and the overall cooperation level should be stable over time. In contrast, an individualistic organization and an organization of synchronized terms lack this feature. Consequently, the outsider's trust may decrease in round 3 if the expectation is for the organization not to send back, leading to a reduction in cooperation rates in round 3.

[16]These research questions are pre-registered as explorative studies.

Question 3: Which strategies do participants play?

While strategy choices have been extensively studied in the repeated Prisoners' Dilemma (for an overview of the findings, see for example, Dal Bó and Fréchette, 2018), there is a notable gap in research on strategy choices in repeated trust games, with the exception of Engle-Warnick and Slonim (2004, 2006). To address this gap in the context of our repeated trust game, we build on the strategy frequency estimation method (SFEM) introduced by Dal Bó and Fréchette (2011) and utilize the R package stratEst, developed by Dvorak (2023) and first employed in Dvorak and Fehrler (2024). The SFEM is commonly employed for obtaining maximum-likelihood estimates of the shares of a candidate set of strategies in experimental data. Our estimation builds upon a candidate set of 18 pure strategies for trustors from Engle-Warnick and Slonim (2006). Additionally, we introduce one pure strategy in which the outsider sends as long as the committee sends; otherwise, it plays "Not Send" until the end of rounds 3 and 6, then returns to "Send". This strategy serves as a potential punishment strategy for treatment I and CST.

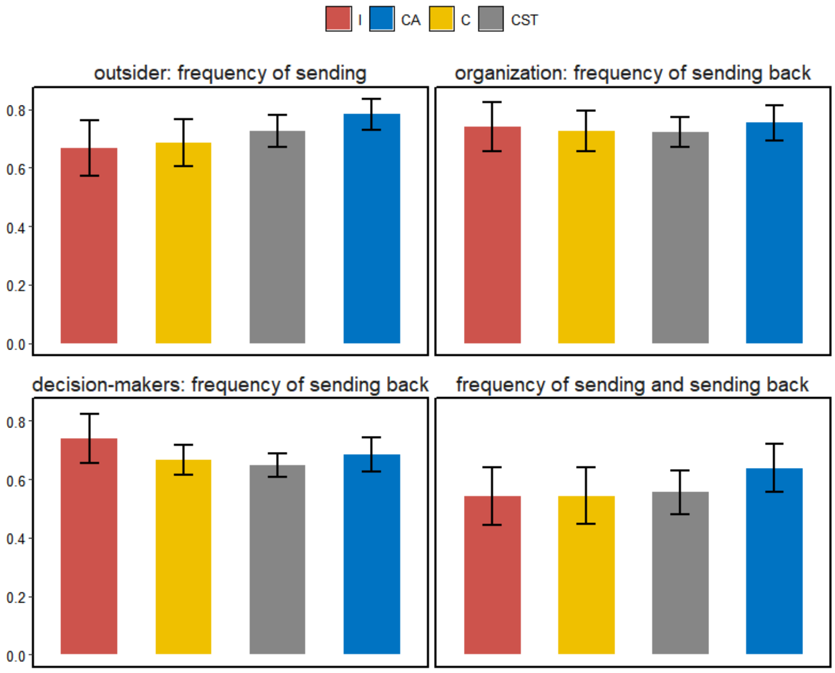
5 Results

Following the common approach in the analysis of experiments with indefinitely repeated games, we take into account that participants need some time to learn. Thus when we test our hypotheses, we exclude the first two supergames from each sequence in the dataset. This leaves us with seven supergames for analysis in sequences 1 and 3, and three supergames in sequence 2. We initiate the analysis by testing our main hypotheses.

5.1 The Frequency of Sending

As the binary trust game exhibits asymmetry, we analyze the frequency of playing "Send" for the outsider and decision-makers separately. The first column in Figure 3 presents the average frequency of sending for both the outsider and decision-makers. In the second column, we depict the committee's mean rate of sending and their mean rate of cooperation, defined as the rates of mutual sending. To calculate the average, we treat each committee (group) as an independent observation, clustering at the matching group level to obtain clustered standard errors. Table 1 provides a summary of the mean differences and $p-$values between the treatments for each plot. In treatment CA, the majority of committees announce their intention to send back the transfer ($M = 0.89$, $SD = 0.03$).

Figure 3: Plot of the average frequency of sending



Note: This figure plots the average sending frequency of the outsider, organization, decision-makers, and the cooperation rates between the outsider and the committee for each treatment. The average is calculated at the committee level and clustered at the matching group level. Error bars indicate clustered standard deviation.

For the outsider, the average rate of sending serves as a measure of their average trust in the committee. Overall, trust rates are consistently high across all treatments, with no significant difference in mean trust rates between treatments. Notably, the outsider tends to transfer funds more frequently when the committee announces its intention to send back, compared to scenarios where no announcement is made or when the committee promises not to "Send".

Regarding decision-makers, the frequency of sending is not higher in treatment I compared to treatment C, nor is it higher than in treatment CST. There is no difference between treatment CST and C, and similarly, no significant distinction between treatment C and CA. We further examine the mean frequency of sending at the committee level. No significant differences in means are observed across the four treatments without announcements. Finally, we define

Table 1: Mean differences and $p-$values for treatment comparisons

treatments		mean difference	p-value
I	C	-0.02	.438
	CST	-0.06	.561
CST	C	0.04	.325
C	CA	-0.10	.141

(1) outsider

treatments		mean difference	p-value
I	C	0.01	.451
	CST	0.02	.847
CST	C	< 0.01	.475
C	CA	-0.03	.376

(2) committee

treatments		mean difference	p-value
I	C	0.07	.218
	CST	0.09	.314
CST	C	-0.02	.395
C	CA	-0.02	.408

(3) decision-makers

treatments		mean difference	p-value
I	C	< 0.01	.493
	CST	-0.01	.906
CST	C	0.01	.461
C	CA	-0.09	.218

(4) cooperation rates

Note: The four tables present the mean differences in the frequency of sending and the corresponding p-values between treatments. They are based on the choices of the outsider, the committee, the decision-makers, and the cooperation rates. The average is calculated at the committee level and clustered at the matching group level. The p-values between treatments I-C, CST-C, and C-CA are calculated using one-way tests, whereas the p-values between treatments I-CST are calculated using two-way tests.

cooperation as mutual sending. The lower right part of Table 1 displays the mean differences in frequency of cooperation and the corresponding $p-$values. No statistically significant differences in means are observed between the treatments. We cannot reject our null hypotheses as the means of sending and cooperating do not show statistically significant differences.

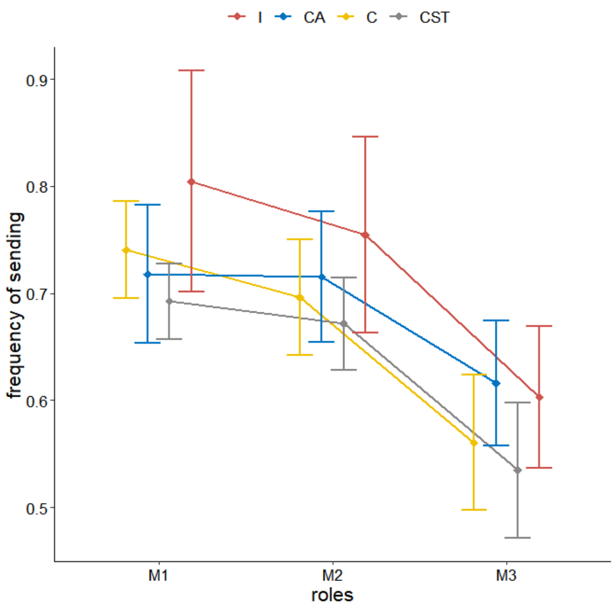
Result 1: Organizational structures, whether individualist or collectivist, do not influence the mean trust and cooperation rates between the treatment I and C. The turnover structures of the organization, whether synchronized or overlapping, do not impact the mean trust and cooperation rates between the treatment CST and C. Overall, the frequency of trust remains high across all treatments.

Result 2: The opportunity to make an announcement does not improve the mean rates of trust and cooperation.

5.2 Tenure

We proceed to compare the decisions of decision-makers with the same tenure across treatments and their decisions with different tenures within the same treatment. The results are presented in Figure 4. Decision-makers in the first, second, and last terms are abbreviated as M1, M2, and M3, respectively. For each tenure, the mean differences between treatments are summarized in Table 2. We find no significant differences between treatments.

Figure 4: Plot of the average frequency of sending of decision-makers in term 1, 2 and 3



Note: This figure depicts the average frequency of sending for decision-makers in the first, second, and third terms. The averages are computed at the committee level, and clustering on matching groups is applied for standard deviation. Error bars represent the clustered standard deviations.

Within each treatment, we analyze the impact of being in different terms on the mean rate of sending by regressing the average frequency of sending on the term in office, treating M1 as the base group. The results are presented in Table 3.

According to Table 3, in treatment I and treatment CST, being in the final term in the committee is associated with a decrease in the mean sending frequency. For treatment C, where theory predicts that both M1 and M2 should send back due to the future benefit of cooperation, the mean frequency of sending decreases as tenure increases, with a more pronounced decrease observed in M3. Announcements, however, alter this pattern, as the mean sending rate does not

Table 2: Frequency of sending for each tenure

treatments		mean difference	p-value
I	C	0.06	.552
	CST	0.11	.282
CST	C	-0.05	.381
C	CA	0.02	.763

(1) M1

treatments		mean difference	p-value
I	C	0.06	.563
	CST	0.08	.389
CST	C	-0.02	.711
C	CA	-0.02	.806

(2) M2

treatments		mean difference	p-value
I	C	0.04	.630
	CST	0.07	.436
CST	C	-0.03	.761
C	CA	-0.06	.499

(3) M3

Note: The three tables present the mean differences in the frequency of sending and their corresponding *p*-values between treatments. They are based on the choices of decision-makers with three different tenures. The average frequency is calculated at the committee level and clustered at the matching group level. *p*-values are calculated using two-way tests.

Table 3: Linear regression results of mean sending frequency on tenure

	I	C	CST	CA
constant	0.80***	0.74***	0.69***	0.72***
	(0.11)	(0.05)	(0.03)	(0.07)
M2	-0.05	-0.04***	-0.02	<-0.01
	(0.07)	(0.02)	(0.05)	(0.01)
M3	-0.20***	-0.18***	-0.15***	-0.10**
	(0.08)	(0.04)	(0.05)	(0.04)
N	54	54	54	54
adjusted-R^2	0.09	0.20	0.18	0.04

Note: This table shows the results of linear regressions. The dependent variable is the mean sending frequency at the committee level, and the explanatory variable is the tenure, with M1 as the base group. Standard errors in parentheses are clustered at the matching group level. $***$ ($**$,$*$) indicates the significance at the 1 (5,10)% level.

decrease from M1 to M2. Although announcements help M2 maintain their "Send" frequency as in the previous term, this effect is limited and does not extend to M3.

Our findings reveal a last term effect in treatments I, CST, and CA, where decision-makers tend to play "Send" in the first two periods but keep the funds in the last period. This results in a higher mean rate of sending for M1 and M2 compared to M3. However, as indicated by Figure 4, the average sending rate for M3 remains relatively high. This observation suggests that a considerable proportion of decision-makers do not do backward induction but play rather cooperatively. Additionally, in treatment C, both M1 and M2 have an incentive to send based on our stage game parameters. However, empirically, we find that M2 sends less often than M1. This difference in sending rates between M1 and M2 diminishes in other treatments, indicating that the decrease is more stable in treatment C.

Result 3: Contrary to our theoretical prediction, the mean frequency of playing 'Send' decreases over tenure in treatment C. Although announcements eliminate the difference in sending between M1 and M2, this effect is limited and does not extend to M3.

Result 4: A last-term effect is also observed in treatments I, CST, and CA. Decision-makers in these treatments frequently send funds in terms 1 and 2, but opt to keep the funds in the last term.

The results presented so far do not address how sending rates evolve over rounds. To determine whether sending rates are more stable in treatment C, we will examine changes in the frequency of sending over the rounds in the next subsection. This analysis will focus not only on the choices of decision-makers within a committee but also on outsider's trust and cooperation rates.

5.3 Changes Over Rounds

Figure 5 illustrates the changes in the average frequency of sending and cooperation over the rounds. The four panels correspond to the choices of the outsider, committee, decision-makers, and the cooperation rates. The averages are computed at the committee level across all supergames for each round. We specifically focus on the initial six rounds due to the continuation probability of our indefinitely repeated trust game being 5/6. This probability results in an expected length of six for each supergame.

Figure 5 reveals a notable decrease in the sending of decision-makers and the frequency of cooperation in rounds 3 and 6 for treatments I and CST. This end-game effect is confirmed in Table 4. We define a dummy variable MULT3 which takes on value 1 if the round is a multiple of 3. We regress the average frequency of sending and cooperation on this dummy, on the treatment category that treats treatment C as the base, and on the interaction of the round dummy and treatment category. The standard errors in parentheses are two-way clustered at both the committee and matching group levels. Our regression results confirm the presence of a last-round effect in cooperation in treatment I and CST compared to treatment C. Notably, the last-round effect is not for the outsider. This observation is surprising, as one might expect the outsider to exhibit corresponding reductions in trust during rounds 3 and 6.

The stability of cooperation between treatments is compared using Levene's test for the homogeneity of variance, which indicates that treatment C is significantly more stable than treatments I and CST ($p_{Ivs.C} = .009$, $p_{CSTvs.C} = .0176$). No significant differences are detected in the outsider's trust or in the committee's voting decisions across treatments. This implies that the end-game effects lead to instability of cooperation in the non-overlapping structured committees. The difference in treatment is not reflected in the overall frequency of sending and cooperation, but rather in the stability of these behaviors.

Result 5: Compared to treatment C, the trust of the outsider in the committee remains unaffected by the last round. A last-round effect is observed in treatments I and CST in the cooperation rates.

Result 6: The end-game effect leads to instability in cooperation in treatments I and CST compared to treatment C.

5.4 Strategies

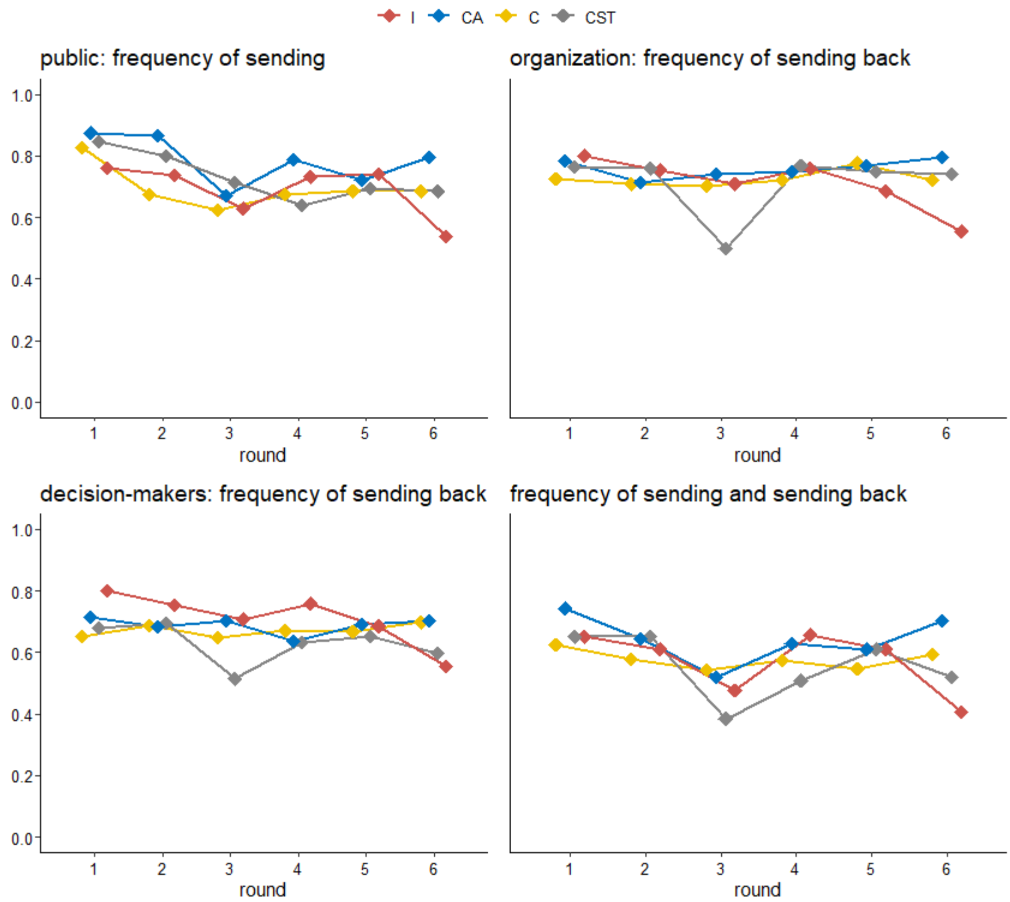
In this section, we examine the specific strategies employed by the outsider. We estimate strategies for the outsider rather than for the committee or decision-makers for several reasons. First, strategies specify actions after each possible history. To differentiate among strategies, we need sufficient observations of a sequence of actions following different histories. It is thus difficult to estimate what strategies each decision-maker plays because they interact with a certain outsider for a maximum of three rounds, which does not provide enough repetitions for estimation. Second, estimating strategies with observations of only three rounds restricts our candidate strategies to no more than three transitions among states. Lastly, despite committees having indefinite interactions with the outsider, the changing composition of decision-makers

Table 4: The end-game effect on the mean rate of sending and cooperation

	outsider	committee	decision-makers	cooperation
constant	0.72***	0.73***	0.67***	0.58***
	(0.07)	(0.08)	(0.06)	(0.10)
MULT3	-0.06	-0.02	<0.01	-0.01
	(0.06)	(0.02)	(<0.01)	(0.04)
I	0.03	0.02	0.08	0.05
	(0.12)	(0.13)	(0.12)	(0.15)
CST	0.03	0.03	<0.01	0.03
	(0.09)	(0.10)	(0.07)	(0.12)
CA	0.10	0.02	0.01	0.08
	(0.08)	(0.11)	(0.08)	(0.13)
MULT3×I	-0.10	-0.10	-0.12*	-0.18**
	(0.10)	(0.07)	(0.07)	(0.07)
MULT3×CST	0.02	-0.12	-0.11*	-0.14**
	(0.08)	(0.10)	(0.06)	(0.06)
MULT3×CA	-0.02	0.04	0.02	-0.03
	(0.07)	(0.05)	(0.04)	(0.05)
N	432	432	432	432
adjusted-R^2	0.02	0.003	0.02	0.02

Note: This table presents the regression results. The independent variable for the first three columns is the mean sending frequency of the outsider, the committee, and the decision-makers of each committee in every round over all supergames. The independent variable for the last two columns is the mean rate of cooperation at the committee level for all supergames in every round. The base group for comparison is treatment C. The explanatory variables include: 1) MULT3, a dummy variable that takes on the value 1 if the round is a multiple of 3; 2) treatment categories; and 3) the interaction of MULT3 and the treatments. Numbers in parentheses represent standard deviations that are two-way clustered at both the committee and matching group levels. Significance levels are denoted as $***$ ($**$,$*$) indicating significance at the 1 (5, 10)% level.

Figure 5: Plot of average frequency of sending over rounds



Note: This figure illustrates the changes in the mean frequency of sending and cooperation over the rounds for all treatments.

over time and the committee's choices being the collective result of all decision-makers may lead to the absence of a consistent pure strategy at the committee level.

Tables D3–D5 in the Appendix provide a summary of candidate strategies, all modeled using finite automata.[17] The initial 18 strategies are pure strategies adapted from Engle-Warnick and Slonim (2006). These strategies are contingent on the committee's choices, with state transitions responding to the committee's decisions in the preceding round.

[17]In Appendix E, we estimate the outsider's generic strategies based on memory-one histories from the data, rather than relying on a predefined candidate set. The results show that leniency and the willingness to restore trust vary across treatments.

In addition, we introduce a pure strategy, T-MULT3. Participants employing this strategy choose to send as long as the committee reciprocates. However, if the committee fails to send back, the outsider switches to a "Not Send" strategy and continue with it until the end of rounds 3 and 6. This strategy acts as a form of punishment on the committee level, specifically designed for treatment I and CST. In rounds 4 and 7, as the committee undergoes a change, the outsider reverts to the "Send" strategy. The analogous punishment strategy for treatment C and CA is T2, a trigger strategy with two rounds of punishment. Here, the outsider sends when the committee reciprocates; otherwise, they adopt a "Not Send" strategy for two rounds until a new committee is in place. Afterward, they resume the "Send" strategy. It's worth noting a potential overlap between T2 and T-MULT3. Participants in treatments I and CST may be employing T-MULT3, and due to their partner's failure to reciprocate in the initial round, the outsider enforces punishment in rounds 2 and 3. This scenario will be estimated as if T2 is played. Consequently, T-MULT3 can be viewed as the lower bound for the prevalence of individuals employing a trigger strategy at the committee level.

Table 5: Descriptions of the selected strategies

Acronym	Description
ALLS	Always play "Send".
ALLN	Always play "Not Send".
GRIM	Play "Send" as long as the committee returns, otherwise play "Not Send" forever.
TFT	Play "Send" as long as the committee returns, otherwise play "Not Send" for one round and return to "Send".
NTFT	Start with "Not Send", and play "Send" in the next round. Keep playing "Send" as long as the committee returns, otherwise go back to "Not Send".
FN	Start with "Not Send", then play "Send" forever.
T2	Play "Send" as long as the committee returns, otherwise play "Not Send" for two rounds and return to "Send".
T3	Play "Send" as long as the committee returns, otherwise play "Not Send" for three rounds and return to "Send".
T4	Play "Send" as long as the committee returns, otherwise play "Not Send" for four rounds and return to "Send".
T-MULT3	Play "Send" as long as the committee returns, otherwise play "Not Send" until the end of round 3 and round 6, then return to "Send".

The estimation method, inspired by Dal Bó and Fréchette (2011), employs the Maximum Likelihood approach to estimate the shares of each strategy within the candidate set for every treatment. Subsequently, a subset of strategies that best captures participants' behavior is selected based on the Bayesian Information Criterion (BIC). The prevalence of these strategies is presented in Table 6, while the selected strategies are detailed in Table 5.

Table 6: Results of strategy frequency estimation for the outsider

	I	C	CST	CA
ALLS	0.08	0.23	-	0.30
	(0.08)	(0.11)		(0.13)
ALLN	0.17	0.06	0.06	-
	(0.09)	(0.06)	(0.05)	
GRIM	0.08	-	-	-
	(0.07)			
TFT	-	-	0.30	-
			(0.13)	
NTFT	-	-	0.06	-
			(0.05)	
FN	-	-	-	0.06
				(0.05)
T2	0.21	0.39	-	0.42
	(0.11)	(0.13)		(0.14)
T3	0.09	0.22	0.31	0.22
	(0.09)	(0.12)	(0.14)	(0.11)
T4	-	0.09	-	
		(0.09)		
T-MULT3	0.37	-	0.28	-
	(0.13)		(0.11)	
γ	0.08	0.14	0.10	0.12
BIC	341.65	440.85	363.31	393.77
ln$L$	-162.15	-213.20	-174.43	-191.10

Note: This table reports the maximum likelihood shares of pure strategies. The estimation procedure assumes constant strategy use across all supergames. γ is the estimated tremble probability, which avoids the likelihood shares of zero when the participants deviate from a choice pattern. Strategies are selected based on Bayesian Information Criterion. Strategies that attract zero shares are omitted (-). The standard errors are reported in parentheses. Values may not add up to 1 because of rounding.

Strategies TFT (tit-for-tat), T2 and T3 are trigger strategies with one, two, and three rounds of punishment. While T3 is played in all treatments, T2 is played in collectivist organizations with overlapping generations and in individualistic organizations. Once trust is not returned, the outsider from these treatments punishes for two rounds, until the committee has been completely replaced. The one-round punishment strategy tit-for-tat (TFT) is played only in treatment CST.

T-MULT3 is prevalent in both treatments I and CST. This suggests that the outsider in these treatments punishes the decision-makers at the committee level. Notably, the outsider in treatment I plays unforgiving strategies. GRIM, a grim-trigger strategy with the initial action set to “Send”, is played in treatment I. In this strategy, trust is discontinued if the organization does not send back. Conversely, treatment CST sees a high prevalence of the one-shot forgiving strategy TFT. It appears to be easier to restore trust under a collectivist organizational structure than under an individualistic organizational structure, although the underlying reason remains unclear. The forgiving strategy NTFT is played with a small share in treatment CST. This is a tit-for-tat variant that starts with “Not Send”, with punishment triggered by a subsequent “Not Send”.

In treatments C and CA, trust is increased by boosting the prevalence of ALLS (always send). Trust is further increased in treatment CA by reducing the occurrence of ALLN (always not send). The outsider in CA also employs FN (first-round not send), a variant of ALLS with the initial action being “Not Send”.

The prevalence of T2 in treatments with overlapping-structured committees may explain why we observe no difference in trust rates between treatments C and I, or C and CST. The punishment rounds last two rounds due to T2, but they can be shorter with T-MULT3 in treatments I and CST if defection occurs in the second or third round. The lower forgiveness of T2 compared to T-MULT3 results in similar average trust frequencies across different committee structures.

Result 7: Punishment strategies are prevalent in all treatments, but the duration of punishment varies based on committee structures. The outsider is found to punish the committee until the committee is replaced. They punish in treatments I and CST until the end of rounds 3 and 6, whereas they punish for two consecutive rounds in treatments C and CA. Moreover, when comparing treatment CST to treatment I, collectivist structures tend to make the outsider more forgiving toward a non-cooperative outcome. Finally, announcements contribute to sustaining trust by increasing the frequency of the outsider playing ”always send.”

6 Conclusion

Cooperation and trust from outsiders is essential for the success of many organizations. For example, the effectiveness of central bank policies depends on people's expectations about future policies. Therefore, understanding which institutional structures foster trust is crucial. This study examines the effects of institutional characteristics on trust and cooperation using an indefinitely repeated binary trust game in the lab.

We focus on two dimensions of institutions. The first dimension concerns whether the organization is individualistic or collectivist, where a collectivist organization involves more than one decision maker. We further distinguish between two specific structures: collectivist organizations with overlapping terms and collectivist organizations with synchronized terms. In organizations that are individualistic or have synchronized terms, our game-theoretic analysis predicts that it should be difficult to enable cooperation. By contrast, in collectivist organizations with overlapping terms cooperation is a possible equilibrium outcome as long as a majority of decision makers receive a sufficiently high net gain from cooperation in the future. In our experiment, however, institutional structures do not affect the average frequency of trust and cooperation. Trust and cooperation rates are high in both individualist and collectivist structures. Our results also show that a sizable proportion of individualistic decision makers and individuals in the role of the "outsider" continue to transfer funds even in the final period. However, we find important treatment differences in the stability of cooperation. In a collectivist structure with overlapping terms, cooperation rates are more stable than in an individualist structure and in a collectivist structure with synchronized terms. Thus an overlapping term structure may be desirable in environments where the stability of outcomes is an end in itself.

The second dimension concerns communication of the organization with the outsider. We introduce a simple announcement stage to a collectivist organization with overlapping terms. Decision-makers vote on two pre-specified non-binding statements: either always returning the trust or always not returning the trust. Our findings suggest that an opportunity to make an announcement increases neither the trust rate nor the cooperation rate.

Future research could delve deeper into the behavioral effects that contribute to our results. For example, it is possible that the diffusion of responsibilities among multiple decision-makers makes the collectivist organization appear less trustworthy, resulting in a similar level of trust in both individualistic and collectivist organizations. Our experimental design does not permit a direct measurement of the perceived diffusion of responsibility's effect on outsider's trust. Finally, exploring the impact of announcements on trust by allowing decision-makers to formulate their own messages could be another interesting avenue for future studies.

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A Proof of Theoretical Prediction 3

The first part of the prediction is easy to show. Consider the following strategy profile: The outsider never transfers resources to the organization. Irrespective of the history, members of the organization always vote in favor of not sending transfers to the outsider. It is obvious that such a strategy profile constitutes a subgame-perfect Nash equilibrium and that it satisfies our sincere-voting requirement.

To show that equilibria with full cooperation exist, consider the following strategies. Decision-makers in their final terms always vote against sending transfers to the outsider, conditional on the organization having received transfers from the outsider. The other decision-makers vote in favor of sending transfers to the outsider unless the outsider did not submit transfers in the previous round or, in the previous round, the organization did not submit transfers to the outsider. In these cases, all decision-makers vote against sending transfers. The outsider always sends transfers to the organization unless it itself or the organization did not send transfers in the previous round.

The behavior of the outsider is optimal, as it is payoff-maximizing to send transfers to the organization exactly in those periods where the organization is expected to reciprocate this behavior. The behavior of decision-makers in their last terms is optimal as well, as not sending transfers to the outsider maximizes the payoffs in the last period. The other decision-makers have to weigh the current gains from defecting, which are $E + R_1 - (E + R_1 - T_2)$, against the discounted forgone future gains from cooperating, i.e. $\delta\,(E + R_1 - T_2 - E)$. The assumed behavior is optimal if $E + R_1 - (E + R_1 - T_2) \leq \delta\,(E + R_1 - T_2 - E)$, which is equivalent to (1).

It may be interesting to discuss strategies where defections lead to more extended periods of punishments. Suppose, for example, cooperation would break down for two periods following a defection, i.e. a period where the outsider or the organization do not send transfers. Could cooperation be sustained for larger sets of parameters than the one characterized by (1)? Actually, this is not the case because the middle-aged decision-maker faces only one additional period. As a consequence, stricter punishments like a two-period break down of cooperation after defection or even grim-trigger strategies do not increase the scope for cooperation. This implies that cooperation cannot occur if (1) is violated. $\square$

B Simulations for Assessing the Statistical Power

In the simulations, we iterated the following process 20,000 times for various effect sizes:

1. Create a data set of 5 (supergames) * 3 (number of matching groups per treatment) * 3 (number of groups per matching groups) * 2 (treatments) observations and an indicator variable for treatment 2.
2. Draw random numbers from the Bernoulli distribution with success probability 0.4 for treatment 1.
3. Draw random numbers from the Bernoulli distribution with success probability 0.4 + (effect size) for treatment 2.
4. Average the random draws within each group. These are the simulated average cooperation rates.
5. Regress the average cooperation rates on the indicator variable to get the difference and the cluster-adjusted standard error (clustering on the matching group). Use these for a one-sided $t-$test. Return the $p-$value.

Finally, we computed the share of the $p-$values smaller than 0.05, which gave us the statistical power.

Our simulations suggest that we will have enough power ($> 87\%$) to detect effect sizes of 15 percentage points at the 5% level with a one-sided $t-$test and 6 matching groups per treatment. We checked for the accuracy of the method by running a simulation with an effect size of 0 (again with 20,000 iterations of the process described above). The relative frequency of $p-$values smaller 0.05 was 0.051, which is very close to the 0.05 that we would expect in this case.

C Probability of Playing "Send" After Memory-one Histories

We estimate the probability of playing "Send" based on memory-one histories, with a specific focus on the voting decisions of the committee rather than at the participant level. The calculated probabilities of playing "Send" after each memory-one history are summarized in Table C1.

Table C1: Cooperation rates after memory-one histories

	$\sigma_{\emptyset}$	σ_{ss}	σ_{sn}	σ_{n}	ln$L$
I	0.75	0.92	0.18	0.27	-220.91
	(0.10)	(0.03)	(0.08)	(0.10)	
C	0.81	0.95	0.12	0.33	-197.81
	(0.06)	(0.02)	(0.07)	(0.06)	
CST	0.86	0.96	0.22	0.43	-180.30
	(0.06)	(0.03)	(0.07)	(0.12)	
CA	0.87	0.94	0.41	0.51	-195.85
	(0.05)	(0.02)	(0.10)	(0.07)	

(1) outsider

	$\sigma_{\emptyset}$	σ_{ss}	σ_{sn}	σ_{ns}	σ_{nn}	ln$L$
I	0.75	0.79	0.59	0.73	0.38	-277.33
	(0.07)	(0.03)	(0.08)	(0.06)	(0.10)	
C	0.69	0.84	0.39	0.72	0.58	-272.099
	(0.07)	(0.02)	(0.07)	(0.07)	(0.04)	
CST	0.77	0.80	0.48	0.77	0.56	-268.99
	(0.05)	(0.03)	(0.08)	(0.06)	(0.08)	
CA	0.78	0.82	0.47	0.77	0.52	-260.12
	(0.05)	(0.04)	(0.10)	(0.06)	(0.11)	

(2) committee

Note: These tables present the estimated probabilities of playing "Send" after memory-one histories. The two sub-tables, display the estimated results for the outsider, and committee. The numbers in parentheses indicate the standard errors, and ln$L$ represents the log likelihood of the model.

The outsider and the committee condition on different memory-one histories. Let σ represent the round probability of playing "Send". For the outsider, σ conditions on one of the four possible memory-one histories ($\emptyset$, ss, sn, n). $\emptyset$ indicates the first round with no history. ss occurs when both the outsider and the committee played "Send" in the previous round. sn represents the scenario where the outsider played "Send", but the committee did not. n

corresponds to the case in which the outsider chose not to send the fund in the previous round, and they could not observe the committee's decision. The corresponding cooperation rates after these histories are denoted as ($\sigma_{\emptyset}$, σ_{ss}, σ_{sn}, σ_{n}). We estimate the probabilities, and the results are presented in the sub-table (1) of Table C1.

We assume that the committee's choice in the current round are influenced by its decision in the previous round. Additionally, the committee's decision is influenced by the outsider's choice. Thus, our defined memory-one histories are ($\emptyset$, ss, sn, ns, nn), where the first letter denotes the outsider's choice in the previous round, and the second letter indicates the committee's decision in the previous round. The estimated results for the committee are presented in the sub-table (2) of Table C1.

The probability σ_{sn} represents the likelihood of trusting even when the committee was not trustworthy in the previous round, thus it measures how lenient the outsider is. As indicated in sub-table (1) of Table C1, while leniency is generally low in all treatments, treatment CA stands out with a more lenient outsider than in the other treatments. The low probabilities of σ_n indicate that once the outsider decides not to trust, it is unlikely for them to return to trust. An exception is observed in CA, where $\sigma_n = 0.51$, reflecting approximately 50% of cases where trust is restored. Taken together, the outsider in treatment CA is both more lenient and more forgiving than in other treatments.

For the committees across all treatments in the sub-table (2) of Table C1, the likelihood of choosing "Send" in the current period is lower if they did not send in the last period compared to when "Send" was chosen in the previous period. This pattern holds consistently across treatments, irrespective of the outsider's decisions, as the probabilities of sending after histories ns and ss are consistently high across treatments. This suggests leniency on the part of the committees, indicating that even if the outsider opted not to send funds in the previous round, the committee is still likely to send if they did so in the last round.

For the decision-makers, we assume that they base their choices on the outsider's choice, the committee's choice, and their own choice in the previous round. The possible memory-one histories for M1 differ from those for M2 and M3 because M1 has just entered the committee and thus does not have a history of their own choice in the previous round. The memory-one histories for M1 are thus ($\emptyset$, ss, sn, ns, nn), where the first letter denotes the choice of the outsider in the previous round, and the second letter denotes the choice of the committee in the previous round. These histories further imply that the choices made by the committee when M1 is not in the committee affect the decisions of M1. For M2 and M3, the memory-one histories are (∅, *sss*, *ssn*, *sns*, *snn*, *nss*, *nsn*, *nns*, *nnn*). The first letter denotes the choice of the outsider, the second letter is the choice of the committee, and the third letter is one's own choice in the previous round. Notice that for treatment I, the committee's choice is the decision-maker's choice; the only possible histories are thus (*sss*, *snn*, *nss*, *nnn*). For treatments I and CST, M2 and M3 do not have an empty history because they made decisions as M1 and M2 in the previous round.

Table C2 shows the probability of playing "send" for M1, M2, and M3 for each treatment. The probability for M1 to play "Send" is high irrespective of the choices made either by the outsider or by the committee in the last round, with the only exception in treatment CA, following the history *nn*. M2 and M3's probability of sending is influenced by their own decisions in the previous round. If they did not send in the previous round, their likelihood of sending remains low, even if the committee has sent.

Table C2: Sending rates after memory-one histories for M1, M2 and M3

	$\sigma_{\emptyset}$	σ_{ss}	σ_{sn}	σ_{ns}	σ_{nn}	ln$L$
I	0.75	0.84	0.93	0.68	0.57	-110.54
	(0.07)	(0.08)	(0.07)	(0.14)	(0.14)	
C	0.72	0.77	0.68	0.66	0.72	-285.39
	(0.05)	(0.04)	(0.07)	(0.06)	(0.07)	
CST	0.68	0.71	0.64	0.75	0.78	-373.00
	(0.04)	(0.05)	(0.07)	(0.07)	(0.07)	
CA	0.75	0.77	0.64	0.64	0.38	-281.83
	(0.05)	(0.04)	(0.07)	(0.08)	(0.08)	

(1) M1

	$\sigma_{\emptyset}$	σ_{sss}	σ_{ssn}	σ_{sns}	σ_{snn}	σ_{nss}	σ_{nsn}	σ_{nns}	σ_{nnn}	ln$L$
I	-	0.85	-	-	0.32	0.86	-	-	0.26	-74.16
		(0.07)			(0.15)	(0.06)			(0.13)	
C	0.65	0.92	0.20	0.86	0.14	0.80	0.27	0.90	0.26	-217.42
	(0.06)	(0.03)	(0.08)	(0.10)	(0.06)	(0.05)	(0.15)	(0.05)	(0.08)	
CST	-	0.88	0.28	0.77	0.28	0.82	0.25	0.71	0.18	-224.74
		(0.03)	(0.07)	(0.11)	(0.06)	(0.05)	(0.10)	(0.19)	(0.09)	
CA	0.75	0.88	0.26	0.88	0.17	0.82	0.44	0.85	0.31	-226.08
	(0.05)	(0.03)	(0.09)	(0.08)	(0.06)	(0.06)	(0.18)	(0.11)	(0.09)	

(2) M2

	$\sigma_{\emptyset}$	σ_{sss}	σ_{ssn}	σ_{sns}	σ_{snn}	σ_{nss}	σ_{nsn}	σ_{nns}	σ_{nnn}	ln$L$
I	-	0.69	-	-	0.60	0.55	-	-	0.22	-74.20
		(0.08)			(0.22)	(0.12)			(0.11)	
C	0.51	0.76	0.17	0.69	0.20	0.72	0.19	0.69	0.19	-277.95
	(0.06)	(0.05)	(0.08)	(0.14)	(0.06)	(0.07)	(0.10)	(0.14)	(0.07)	
CST	-	0.77	0.16	0.75	0.23	0.81	0.12	0.67	0.23	-189.31
		(0.04)	(0.06)	(0.12)	(0.08)	(0.08)	(0.13)	(0.12)	(0.07)	
CA	0.64	0.77	0.12	0.47	0.22	0.61	0.50	1.00	0.44	-275.81
	(0.05)	(0.04)	(0.06)	(0.15)	(0.06)	(0.09)	(0.24)	-	(0.09)	

(3) M3

Note: Estimated probabilities of playing “Send” after memory-one histories. The three sub-tables present the estimated results for M1, M2, and M3 in all treatments. Numbers in parentheses are the standard errors. ln$L$ represents the log-likelihood of the model.

D Candidate Strategies

Table D3: Strategies 1-9 for the outsider

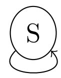
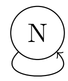
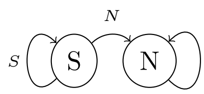
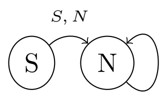
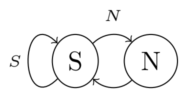
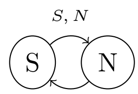
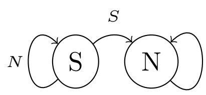
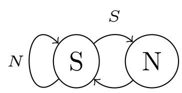
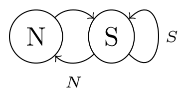
Acronym	Description	Automaton
ALLS	Always play “Send”.	
ALLN	Always play “Not Send”.	
GRIM	Play “Send” as long as the committee returns, otherwise play “Not Send” forever.	
FS	Play “Send” for one round, and irrespective of what the committee plays, play “Not Send” forever.	
TFT	Play “Send” as long as the committee returns, otherwise play “Not Send” for one round and return to “Send”.	
SN	Start with “Send”, then alternate between “Send” and “Not Send”.	
GRIMS	Play “Send” as long as the committee keeps, otherwise play “Send” forever.	
TFTS	Play “Send” as long as the committee keeps, otherwise play “Not Send” for one round and return to “Send”.	
NTFT	Start with “Not Send”, and play “Send” in the next round. Keep playing “Send” as long as the committee returns, otherwise go back to “Not Send”.	

Table D4: Strategies 10-18 for the outsider

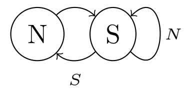
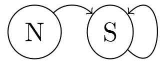
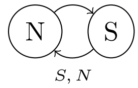
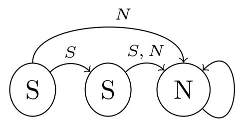
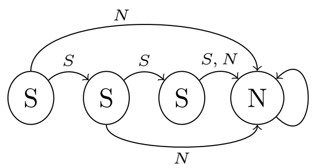
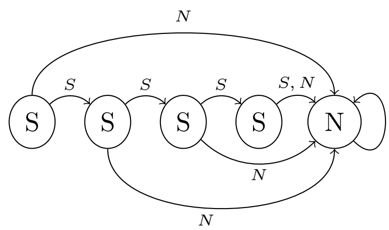
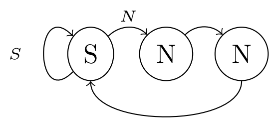
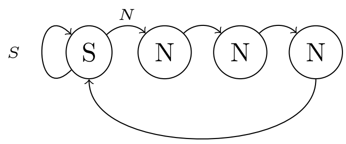
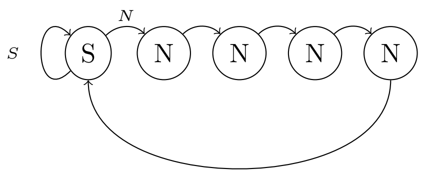
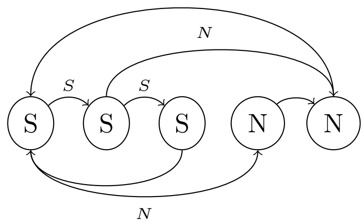
Acronym	Description	Automaton
NTFTS	Start with "Not Send", and play "Send" in the next round. Keep playing "Send" as long as the committee keeps, otherwise go back to "Not Send".	
FN	Start with "Not Send", then play "Send" forever.	
NS	Start with "Not Send", then alternate between "Send" and "Not Send".	
FS2	Play "Send" twice as long as the committee sends, then play "Not Send" forever.	
FS3	Play "Send" three times as long as the committee sends, then play "Not Send" forever.	
FS4	Play "Send" four times as long as the committee sends, then play "Not Send" forever.	
T2	Play "Send" as long as the committee returns, otherwise play "Not Send" for two rounds and return to "Send".	
T3	Play "Send" as long as the committee returns, otherwise play "Not Send" for three rounds and return to "Send".	
T4	Play "Send" as long as the committee returns, otherwise play "Not Send" for four rounds and return to "Send".	

Table D5: Strategy 19 for the outsider

Acronym	Description	Automaton
T-MULT3	Play "Send" as long as the committee returns, otherwise play "Not Send" until the end of round 3 and round 6, then return to "Send".	

Notes: circles represent the states of an automaton. The first state from the left is the starting state. The labels S and N inside the nodes represent the choice of the outsider. Arrows represent deterministic state transitions. The labels on the arrow indicate the choices of the committee that trigger this transition. An unlabeled arrows indicates an unconditional transition that occurs independent of the observed profile.

E Memory-one Generic Strategies

In this section, we test the robustness of our strategy estimation results by deriving the outsider's generic strategies directly from the data, rather than relying on a predefined set of candidate strategies. We assume these strategies are memory-one, meaning they are based on the previous round's history. We do not account for the end-game effect because doing so would require considering additional histories. The histories considered are $(\emptyset, cc, cd, d)$. Here, $\emptyset$ represents the initial round with no prior history, cc and cd indicate that the outsider sent in the previous round, with the committee either sending back (cc) or not sending back (cd), and d describes the situation where the outsider retained the funds in the previous round. We then infer the probabilities of the outsider choosing to send after each of these four histories, represented as $(\sigma_\emptyset, \sigma_{cc}, \sigma_{cd}, \sigma_d)$. Additionally, we estimate the strategy shares across the four treatments. The strategies estimated here are thus behavioral in nature.

Table E6: Memory-one behavioral strategies

	I					C				
	share	$\sigma_\emptyset$	σ_{cc}	σ_{cd}	σ_d	share	$\sigma_\emptyset$	σ_{cc}	σ_{cd}	σ_d
s1	0.24	0.90	0.81	0.46	0.68	0.42	0.75	0.87	0.00	0.39
	(0.10)	(0.10)	(0.07)	(0.15)	(0.12)	(0.13)	(0.07)	(0.04)	(0.00)	(0.04)
s2	0.54	1.00	1.00	0.08	0.28	0.52	0.98	1.00	0.27	0.43
	(0.12)	(0.00)	(0.00)	(0.09)	(0.16)	(0.13)	(0.02)	(0.00)	(0.18)	(0.08)
s3	0.22	0.04	0.71	0.00	0.13	0.06	0.14	1.00	0.00	0.04
	(0.10)	(0.11)	(0.26)	(0.00)	(0.13)	(0.05)	(0.49)	(0.00)	(0.00)	(0.18)
BIC	358.32	-	-	-	-	385.53	-	-	-	-
ln$L$	-158.93	-	-	-	-	-172.53	-	-	-	-
	CST					CA				
	share	$\sigma_\emptyset$	σ_{cc}	σ_{cd}	σ_d	share	$\sigma_\emptyset$	σ_{cc}	σ_{cd}	σ_d
s1	0.06	0.14	0.88	1.00	0.00	0.76	0.91	0.95	0.20	0.42
	(0.05)	(0.51)	(0.06)	(0.57)	(0.34)	(0.12)	(0.02)	(0.03)	(0.09)	(0.04)
s2	0.47	0.81	0.91	0.28	0.79	0.24	0.78	0.90	0.83	0.93
	(0.12)	(0.08)	(0.08)	(0.10)	(0.07)	(0.12)	(0.20)	(0.07)	(0.11)	(0.08)
s3	0.48	1.00	1.00	0.08	0.34	-	-	-	-	-
	(0.13)	(0.00)	(0.00)	(0.08)	(0.08)	-	-	-	-	-
BIC	323.49	-	-	-	-	389.81	-	-	-	-
ln$L$	-141.51	-	-	-	-	-181.90	-	-	-	-

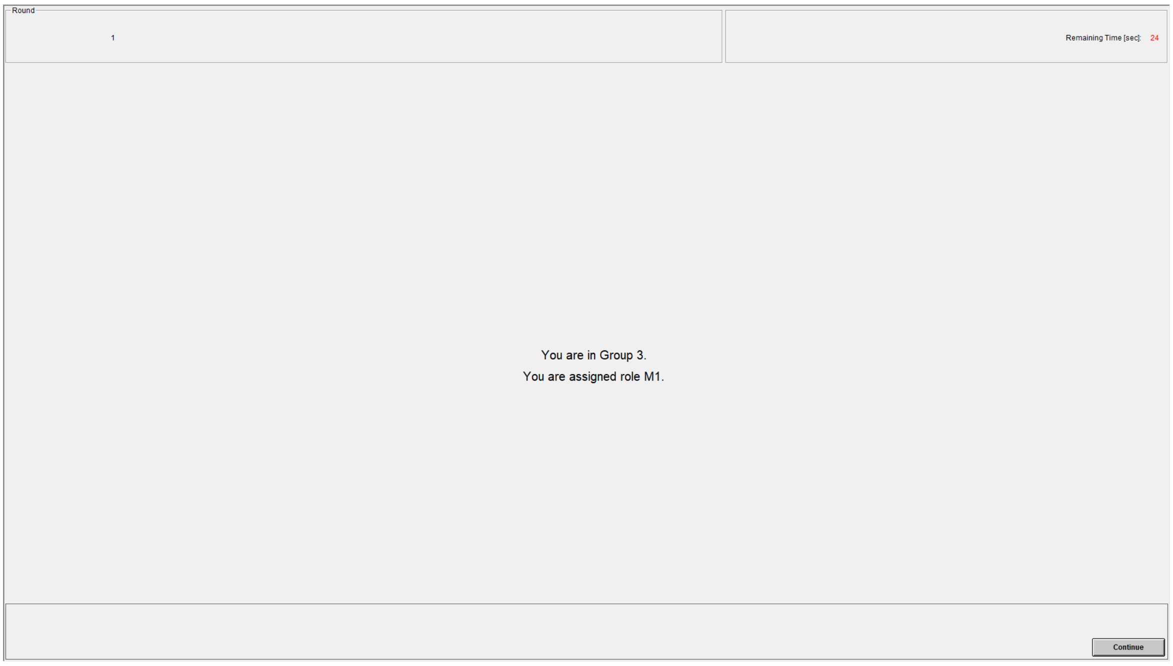
This table summarizes the estimated probability of trust for the outsider across all four treatments. The estimation is based on memory-one histories, and strategy selection is determined using the Bayesian Information Criterion (BIC). Reported standard errors are bootstrapped over 10,000 repetitions.

F Experimental Procedure

In this Section, we provide further information on the experimental procedure, including screen shots and the instructions for the C and the I treatment. The instructions for the other treatments are very similar and, therefore, we omit them here.

We ask participants to read through instructions, and to answer quiz questions to make sure they understand before experiment starts. Once the experiment starts, participants are informed about their role and group assignment in the current interaction on the first screen. Figure F1 is an example of what a participant would see when they are assigned into Group 3 as a decision-maker in the first year of their tenure.

Figure F1: Assignment Stage



In the treatments without mission statements, the outsider and decision-makers decide between ‘Send’ and ‘Not Send” which are represented by options A and B for the outsider, and options X and Y for the decision-makers. Figure F2 and F3 show the decision screens for the outsider and decision-makers, respectively in treatments C and CST.

Figure F2: Choice Stage for outsider

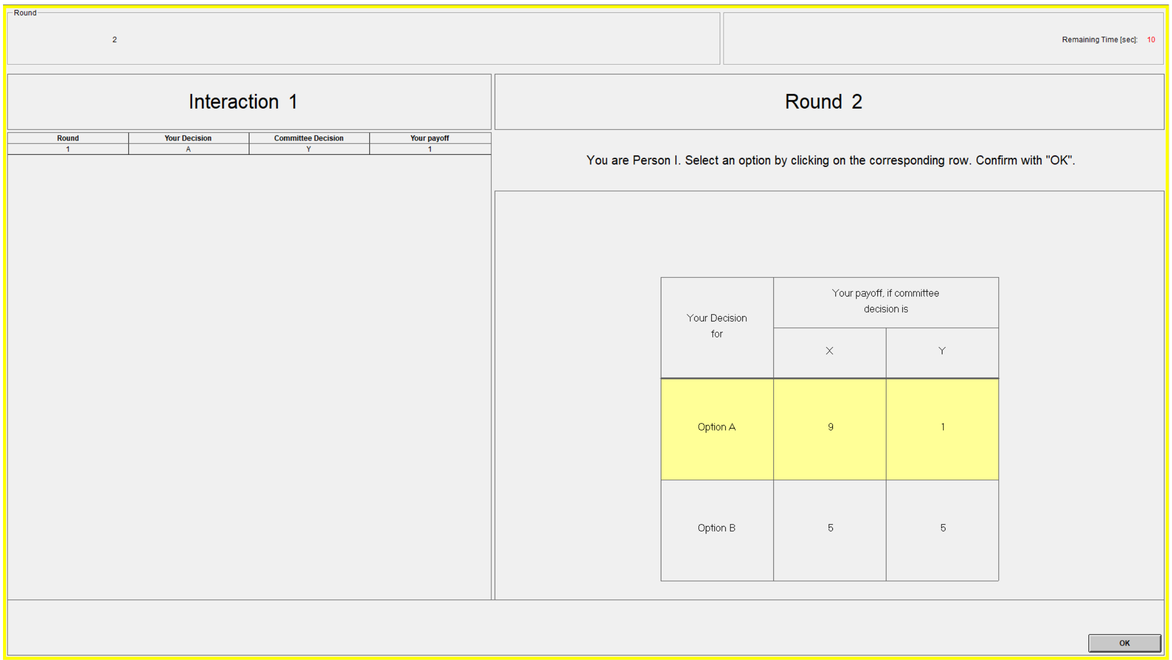
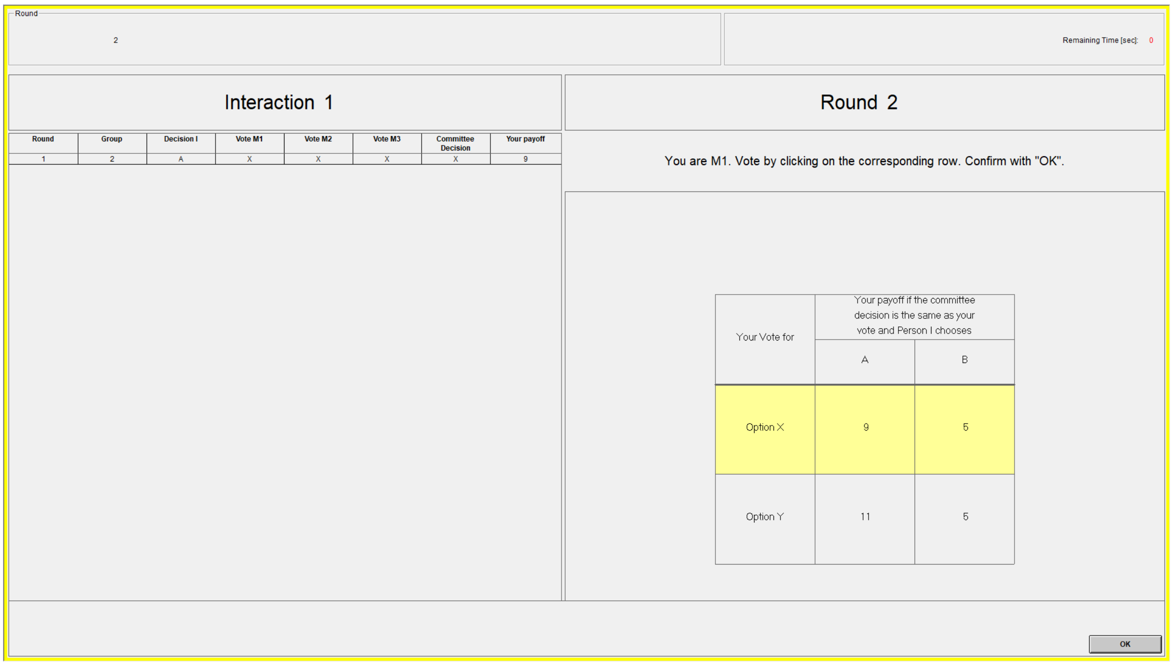
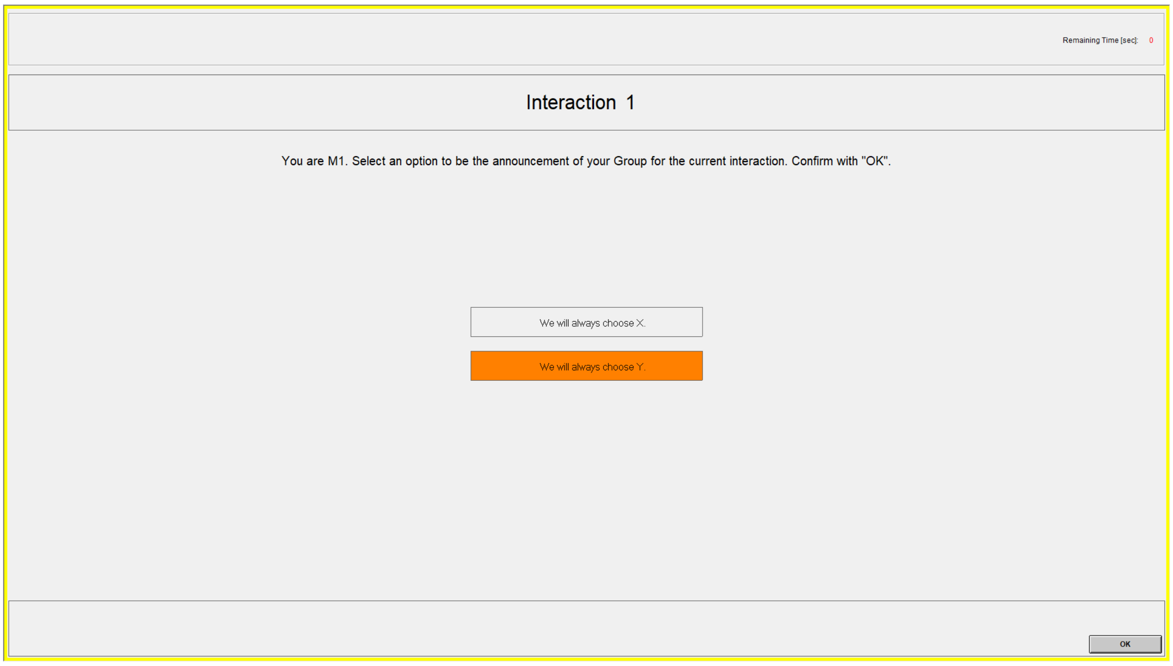


Figure F3: Choice Stage for Decision-makers



In treatment CA, decision-makers engage in an additional voting stage on the announcement (group mission) before making their choices in the stage game. They vote between the two options “We will always choose X” and “We will always choose Y”, as depicted in Figure F4.

Figure F4: Announcement Stage for Decision-makers



The committee decision regarding the announcement will appear as “Annoucement” on screens of all participants of that group in choice and feedback stages. Figure F5 gives an example of a choice screen with announcement. Figure F6 is a feedback stage that immediately follows the choice stage.

Between rounds, the continuation decision of the current supergame is represented by the rolling of a dice. As Figure F7 shows, the current interaction does not end because the roll of the die is larger than “1”.

At the end of the session, we present participants a payoff table with payment from all periods. Afterwards, participants answer a standard socio-economic survey to elicit basic demographic information of the participants of our experiment.

Figure F5: Choice Stage with Announcement

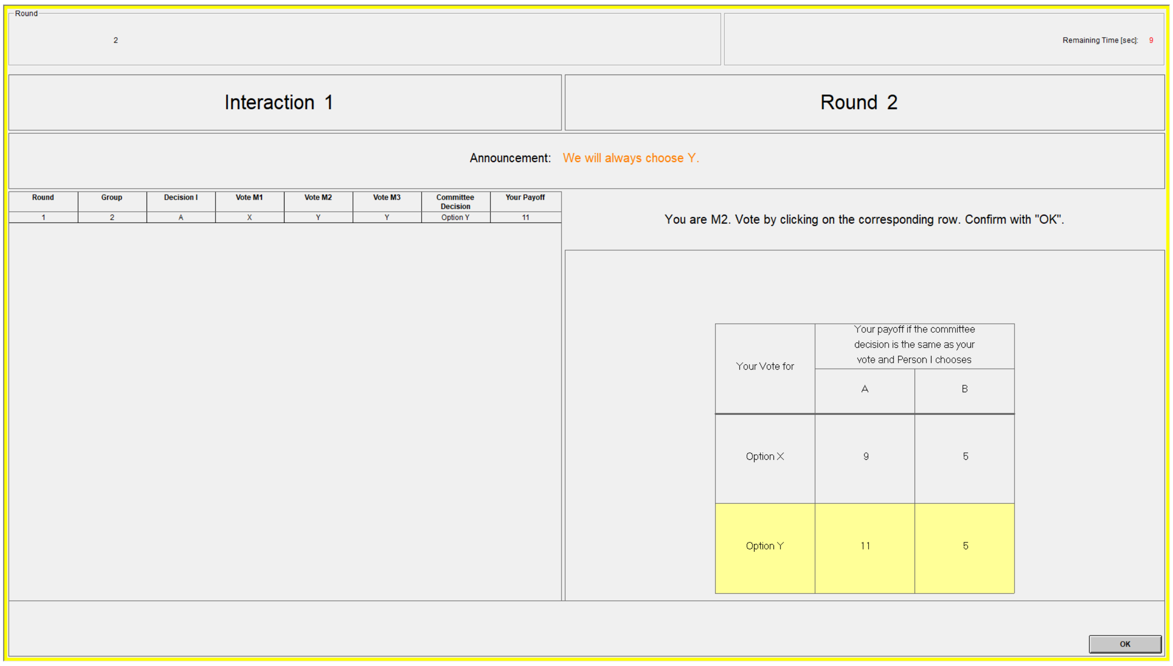


Figure F6: Feedback Stage with Announcement

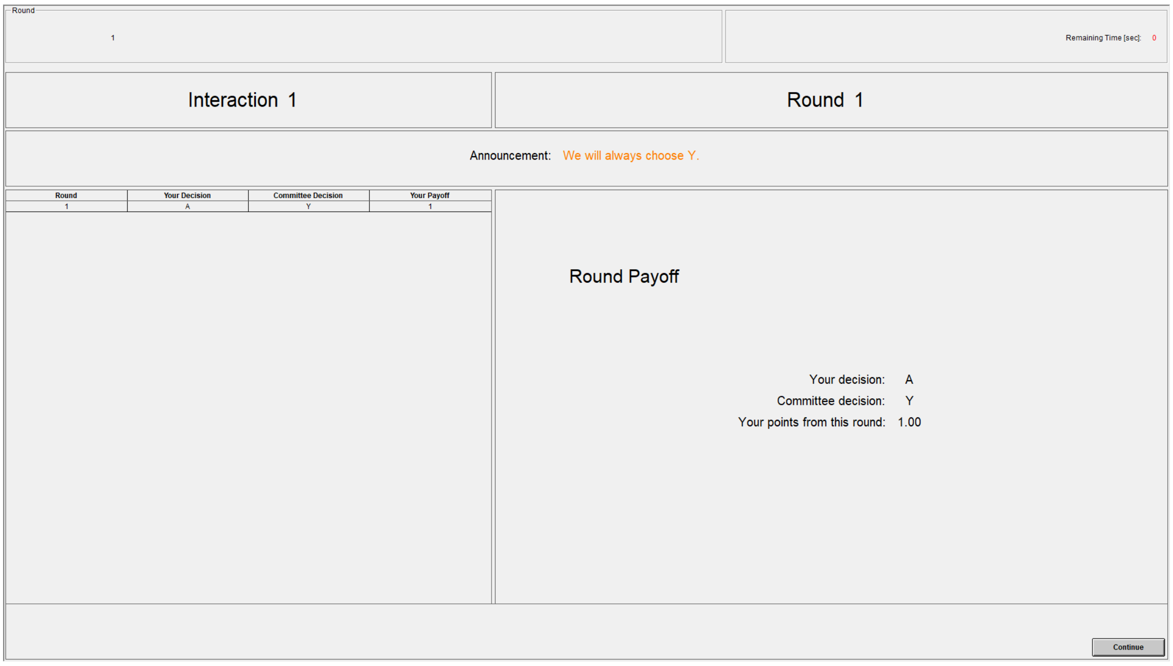
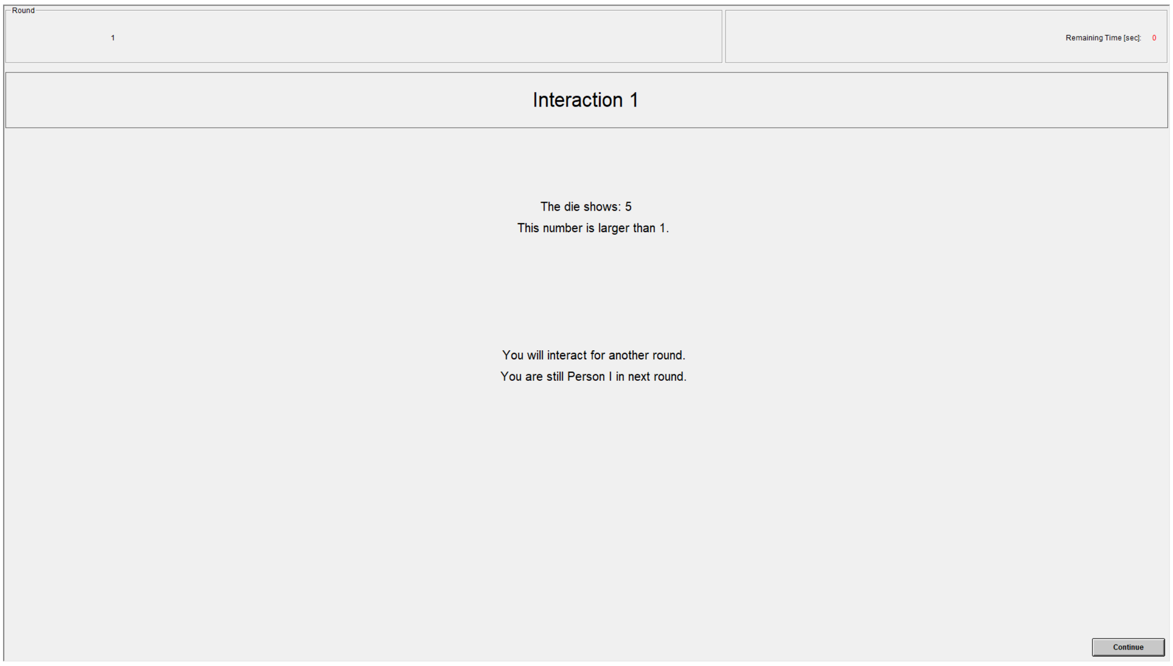


Figure F7: Die Rolling for Continuation



[Instructions for Treatment C]

Overview

Welcome to this experiment. We please you not to talk with other participants during this experiment and to switch off your mobile devices.

You will be paid in cash for today's participation at the end of the experiment. The amount of money you receive depends on your own decisions, the other participants' decisions, and pure chance. It is important that you understand the instructions before the experiment starts.

In this experiment, every interaction between the participants runs through the computers you are sitting in front of. They will interact with each other anonymously. Neither your name nor the names of other participants will be announced. Also, for the evaluations only the anonymized data are used.

Today's session consists of several interactions, which typically consist of several rounds. Your payoff amount is the sum of all points earned, converted into euros, plus a **show-up fee of 5 EUR**. The points shall be converted into euro as follows. Every point is worth **5 cents**, so that:

20 Points = 1,00 EUR.

All participants are paid privately, so other participants cannot see how much you have earned.

Experiment

Interactions and role assignment

This experiment consists of several interactions, which are identical in their sequence. Each interaction consists of one or more rounds of committee meetings. The number of rounds of an interaction is random.

At any given time, there are three committees, with each committee consisting of three current members M1, M2 and M3. Each committee is assigned three potential waiting members W1, W2 and W3 and a person I who is not a committee member (I stands for "Individual").

At the beginning of every interaction all participants are randomly reassigned to the three committee groups (with three M, three W and one person I each). These committee groups exist for the duration of the interaction. In addition, the roles are randomly reassigned at the beginning of each interaction. The only exception is Role I. Whoever is assigned this role in the first interaction of the experiment will keep it throughout the entire experiment. Persons who are assigned a role other than person I at the beginning of the first interaction cannot be assigned role I later.

Both the membership in the committee and the possible waiting period is a maximum of three rounds. The number after the letter M indicates the term of office. After each round the term of office is increased by 1, i.e. M1 becomes M2 and M2 becomes M3. Person M3 leaves the committee at the end of the round and is assigned to another committee with the role W1. In the same way, the role of the persons in the waiting state is changed. W1 becomes W2 in the next round and W2 becomes W3. A person with W3 has finished the waiting time after the corresponding round and starts as a new member with the role M1. At any given time, there is exactly one role for each of the three committees.

Length of an interaction

The length of an interaction is random. After each round there is a 1/6 chance that the interaction ends, and all committees are dissolved. In this case, a new interaction starts with a new random allocation of roles (whereby - as already explained - participants in the role of person I always keep this role).

To determine the length of the interactions, a random number generator was used before the experiment to generate for each interaction a series of equally distributed random numbers from the set 1, 2, 3, 4, 5 and 6, as with the throws of an ordinary six-sided dice. It was determined after how many throws the number 1 appears for the first time. The number of these throws gives the length of the interaction.

Example: The random numbers 4, 6, 3, 4, 1 were generated. So, the number 1 appeared for the first time on the fifth throw. Thus, the interaction has the length 5.

If an interaction is very long, it is possible that a member who retires from a committee will become a member of the same committee again at a later date. The probability that the member M3 of a committee, who would thus retire in the next round, would ever re-enter that committee is about 5%. For the other members this probability is even lower.

Interaction und round schedule

In each round, person I decides between action A and action B by clicking on the corresponding row in the table on the screen (see Fig. 1, left panel). At the same time the committee members M1, M2 and M3 vote simultaneously for action X or Y by clicking on the corresponding row in the table on the screen (see Fig. 1, right panel). Abstentions are not possible. The option that receives more votes, is the decision of the committee. For example, if M1 votes for X, M2 for Y, and M3 for Y, the committee decision is Y.

Fig. 1: Decision masks of a person I (left) and a person M1, M2 or M3 (right)

Your Decision for	Your payoff, if committee decision is	
	X	Y
Option A	9	1
Option B	5	5

Your Vote for	Your payoff if the committee decision is the same as your vote and Person I chooses	
	A	B
Option X	9	5
Option Y	11	5

In case person I chooses option B, her payoff is 5 points and the payoff of every committee member is also 5 points, irrespective of the committee's decision. In case person I chooses option A, her payoff and the payoff of all committee members depends on the committee's decision. In case the committee decides for option X, person I receives 9 points and each committee member also receives 9 points. In case the committee decides for option Y, person I receives 1 point and each committee member receives 11 points.

The persons in the waiting state W1, W2 and W3 do not make a decision and receive a fixed payment of 5 points.

At the end of the round, all players assigned to a committee (M1, M2 and M3 and the waiting persons W1, W2, W3) receive information about the committee's collective decision and the individual votes of all committee members as well as person I’s choice and the resulting payoff. Person I receives information about her payoff and in case she chose action A, she also learns the committee’s decision (X or Y) but not the individual votes of the committee members. In case person I chose B, she does not receive information about the committee’s decision

All rounds are identical in terms of the procedure. The progress of the current interaction is displayed in tabular form in each round for the committee to which you are currently assigned.

End and final payoff

As soon as chance ends the last interaction, the experiment is over.

At the end of the experiment all interactions are paid off. The total amount of points from all rounds will be converted into Euros and paid out privately.

On the last screen of the last round of the last interaction, you can see how much you have earned in Euros.

Questions?

If you have any questions, please contact us. An experimenter will then come to your place.

If you think you have understood everything well, you may start the quiz on the screen. This quiz is only to make sure that everyone has understood the instructions well. The answers will not affect your payoff.

[Instructions for Treatment I]

Overview

Welcome to this experiment. We please you not to talk with other participants during this experiment and to switch off your mobile devices.

You will be paid in cash for today's participation at the end of the experiment. The amount of money you receive depends on your own decisions, the other participants' decisions, and pure chance. It is important that you understand the instructions before the experiment starts.

In this experiment, every interaction between the participants runs through the computers you are sitting in front of. They will interact with each other anonymously. Neither your name nor the names of other participants will be announced. Also, for the evaluations only the anonymized data are used.

Today's session consists of several interactions, which typically consist of several rounds. Your payoff amount is the sum of all points earned, converted into euros, plus a **show-up fee of 5 EUR**. The points shall be converted into euro as follows. Every point is worth **5 cents**, so that:

20 Points = 1,00 EUR.

All participants are paid privately, so other participants cannot see how much you have earned.

Experiment

Interactions and role assignment

This experiment consists of several interactions that are identical in terms of their sequence. Each interaction consists of one or more rounds. The number of rounds of an interaction is random.

At the very beginning, before the first interaction, you and other participants are randomly assigned to a matching group of 9 people in total, which will remain for the entire experiment. You will only interact with the members of this matching group during the whole experiment.

At any given time, your matching group consists of three subgroups, each subgroup consisting of three people. Before each interaction, the members of your matching group are randomly distributed among the three subgroups. Of the three subgroup members, one person is the current decision maker (role E1, E2 or E3) and one person is waiting (roles W1, W2 or W3). The waiting person may replace the decision-maker in the future. The third person has the role person I. The letter I stands for "Individual". In addition, there are two passive assistants of the decision maker P1 and P2, who are randomly chosen from a different matching group, never make any choices in your matching group but receive payoffs at the very end of the experiment which depend on the choices made by E and I. You might also be selected to be such a passive assistant for a group in another matching group in addition to the role that you have in your own matching group but you will only learn about this at the very end of the experiment. In other words, there is no way in which you can influence decisions in other matching groups and your choices cannot be influenced by people outside your matching group either.

At the beginning of the first interaction you will be randomly assigned one of the roles E1, W1 or A. If you are assigned the role of person I, you will keep it throughout the experiment. If you are assigned the role E1 or W1, you will be randomly reassigned one of these roles at the beginning of each new interaction.

The maximum term of office for decision-makers is three periods. The number after the letter E indicates the term of office for the decision-maker. After each round, the term of office is increased by 1, that is, E1 becomes E2 and E2 becomes E3. A person with role E3 leaves the subgroup at the end of the round and is assigned to another subgroup with role W1. In the same way, the role of persons in the waiting state is changed. W1 becomes W2 and W2 becomes W3 in the next round. A person with W3 has finished the waiting time after the end of the corresponding round and starts as a new decision maker of his or her subgroup with the role E1 (unless the interaction ends after this round).

Length of an interaction

The length of an interaction is random. After each round there is a 1/6 probability that the interaction ends, and all subgroups of the matching groups are dissolved. In this case, a new interaction starts with a new random allocation of roles (whereby - as already explained - participants with the role of person I always keep this role and can only meet the other 8 people in your matching group).

To determine the length of the interactions, a random number generator was used before the experiment to generate for each interaction a series of equally distributed random numbers from the set 1, 2, 3, 4, 5 and 6, as with the throws of an ordinary six-sided dice. It was determined after how many throws the number 1 appears for the first time. The number of these throws gives the length of the interaction.

Example: The random numbers 4, 6, 3, 4, 1 were generated. So, the number 1 appeared for the first time on the fifth throw. Thus, the interaction has the length 5.

If an interaction is very long, it is possible that a decision-maker who leaves a subgroup will later become a decision-maker again in the same subgroup with the same person I. The probability that a decision maker E3, who would thus leave in the next round, would meet the same person again during this interaction is about 5%. This probability is even lower for decision-makers with a lower term of office, i.e. for E1 and E2.

Interaction and round schedule

In each round, person I decides between action A and action B by clicking on the corresponding row in the table on the screen (see Fig. 1, left panel). At the same time the decision-maker E (E1, E2 or E3) chooses action X or Y by clicking on the corresponding row in the table on the screen (see Fig. 1, right panel).

In case person I chooses option B, her payoff is 5 points and the payoff of E and her passive assistants P1 and P2 is also 5 points each, irrespective of E's decision. In case person I chooses option A, her payoff and the payoff of E, P1 and P2 depend on the E's decision. In case E decides for option X, person I receives 9 points and E, P1 and P2 also receive 9 points each. In case E decides for option Y, person I receives 1 point and E, P1 and P2 receive 11 points.

The person in the waiting state (W1, W2 or W3) does not make a decision and does not earn any points.

Fig. 1: Decision masks of a person I (left) and a person E1, E2 or E3 (right)

Your Decision for	Your payoff, if Person E chooses	
	X	Y
Option A	9	1
Option B	5	5

Your Decision for	Your payoff, if Person I chooses	
	A	B
Option X	9	5
Option Y	11	5

At the end of the round, the decision-maker E receives information about her payoff and person I's choice. Person I receives information about her payoff and in case she chose action A, she also learns E's decision (X or Y). In case person I chose B, she does not receive information about the E's decision. The passive assistants P1 and P2 do not receive any information until after the last round of the last interaction of the experiment. Then they are informed about their additional earnings.

All rounds are identical in terms of the procedure. The progress of the current interaction is displayed in tabular form in each round for the committee to which you are currently assigned.

End and final payoff

As soon as chance ends the last interaction, the experiment is over.

At the end of the experiment all interactions are paid off. The total amount of points from all rounds will be converted into Euros and paid out privately.

On the last screen of the last round of the last interaction, you can see how much you have earned in Euros.

Questions?

If you have any questions, please contact us. An experimenter will then come to your place.

If you think you have understood everything well, you may start the quiz on the screen. This quiz is only to make sure that everyone has understood the instructions well. The answers will not affect your payoff.

Additional Questions

Questions for the public:

PUB1. My decisions had a strong influence on the policy in subsequent periods.

- totally agree
- agree
- neutral
- do not agree
- totally do not agree
- no answer

PUB2. I had the impression that the fact that decision-makers were replaced by new individuals had a great influence on the outcome.

- totally agree
- agree
- neutral
- do not agree
- totally do not agree
- no answer

PUB3. How my choices would affect the policy-makers' payoffs was an important factor for my decisions.

- totally agree
- agree
- neutral
- do not agree
- totally do not agree
- no answer

PUB4. I made my choices in order to reward or punish the behavior of the policy-makers.

- totally agree
- agree
- neutral
- do not agree
- totally do not agree
- no answer

Questions for the committee members:

POL1. My individual choices had a strong influence on the behavior of the public in subsequent periods.

- totally agree
- agree
- neutral
- do not agree
- totally do not agree
- no answer

POL2. How my choices would affect the public's payoffs was an important factor for my decisions.

- totally agree
- agree
- neutral
- do not agree
- totally do not agree
- no answer

POL3. I made my choices in order to reward or punish the behavior of the public.

- totally agree
- agree
- neutral
- do not agree
- totally do not agree
- no answer

G Results of Questionnaire

Responses to the post-experiment questionnaire are presented in Table G7. Ratings ranging from "totally agree" to "totally do not agree" are coded from 1 to 5. Responses marked as "No answer" have been excluded from the analysis.

Table G7: Summary of answers to questionnaire

	PUB1	PUB2	PUB3	PUB4	POL1	POL2	POL3
I	3.20	3.33	3.50	4.25	3.38	3.43	3.00
	(1.79)	(1.53)	(1.29)	(0.96)	(1.41)	(1.34)	(0.93)
C	2.83	3.71	2.00	4.50	3.66	3.34	3.59
	(1.17)	(0.95)	-	(0.93)	(1.29)	(1.13)	(0.84)
CST	3.00	2.80	3.00	4.33	3.57	3.31	3.46
	(1.41)	(1.64)	(1.10)	(1.21)	(1.22)	(1.18)	(0.92)
CA	3.62	3.33	2.43	4.00	3.23	3.31	3.54
	(1.19)	(1.53)	(0.79)	(1.15)	(1.14)	(1.12)	(0.90)

Note: This tables summarizes the mean and standard deviations of answers to the post-experiment questionnaire. Answers "totally agree", "agree", "neutral", "do not agree" and "totally do not agree" are coded from 1 to 5. "No answer" is removed from the analysis.

Table G8: Summary of answers to questionnaire

	df	N	F-value	p
PUB1	3	25	0.44	.726
PUB2	3	18	0.45	.725
PUB3	3	20	1.83	.183
PUB4	3	25	0.28	.841
POL1	3	93	0.61	.609
POL2	3	123	0.05	.984
POL3	3	138	1.70	.170

We conducted ANOVA tests to examine potential differences in responses across treatments, and the results are summarized in Table G8. No significant treatment differences are observed in responses to all questions. The outsider perceives that their decisions have a neutral effect on the committee's policies in the subsequent period, and the replacement of decision-makers by new individuals is also considered to have a neutral impact on outcomes. While the outsider acknowledges the importance of decision-makers' payoffs as an incentive for decision-making, they do not express a tendency to make decisions with the intent of rewarding or punishing decision-makers. Decision-makers, likewise, do not believe that their choices strongly influence the subsequent decisions of the outsider. The outsider's payoff is not viewed as a crucial factor in their decision-making, and they do not make choices with the intention of rewarding or punishing the outsider.