BEHAVIORAL SPILLOVERS IN LOCAL PUBLIC GOOD PROVISION: AN EXPERIMENTAL STUDY

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April 4, 2018

Abstract

In a circular neighborhood, each member has a left and a right neighbor with whom (s)he interacts repeatedly. From their two separate endowment amounts individuals can contribute to each of their two structurally independent public goods, either shared only with their left, respectively right, neighbor. If most group members are discrimination averse and conditionally cooperating with their neighbors, this implies intra- as well as interpersonal spillovers which link all neighbors. Investigating individual adaptations in one's two games with differing free-riding incentives confirms, through behavioral spillovers, that both individual contributions anchor on the local public good with the smaller free-riding incentive. Therefore asymmetry in gaining from local public goods allows to establish a higher level of voluntary cooperation.

Keywords: Public goods, behavioral spillovers, experiments, voluntary contribution mechanism.

JEL: C91, C72, H41

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§The authors gratefully acknowledge the constructive criticism of our anonymous referees as well as of the editor, Andreas Ortmann. We further thank the Max Planck Institute for Collective Goods for supporting this research.

1 Introduction

While there is ample evidence that individuals respond to those with whom they interact repeatedly, it is still unclear how one interaction affects another structurally independent[1] interaction in which the same individual is involved. Recently, a wave of experimental studies has focused on the issue of behavioral spillovers by analyzing subjects' behavior in multiple games played either sequentially or simultaneously (see e.g. Bednar et al., 2012; Cason et al., 2012; Savikhin and Sheremeta, 2013; Cason and Gangadharan, 2013; Falk et al., 2013). Many of them have found evidence of spillovers, and thus prove that structural independence does not imply behavioral independence. These studies either confront different game types (like games of coordination and games of cooperation, e.g. Cason et al., 2012, or competitive and cooperative games, e.g. Cason and Gangadharan, 2013; Savikhin and Sheremeta, 2013) or let the same game type be played by overlapping player sets (e.g., one treatment of Falk et al., 2013).

In this paper we focus on behavioral spillovers across games of the same type (public good games). Our experimental setup embeds dual group membership in a circular neighborhood of eight participants playing two structurally independent linear public good games for finitely many periods, each with just one neighbor - the left or the right one - so that in total eight games with overlapping two-player sets are played in each round. Structural independence of these games is guaranteed by a separate endowment, separate payoffs, and a different co-player. Whereas the main, asymmetric, treatment features different left and right free-riding incentives, i.e. marginal per capita return, control treatments rely on symmetry.

Despite structural independence, we predict that one does not play each game separately and that his/her behavior can affect, over time, those with whom one is not directly interacting, since an individual's good or bad experience with one co-player may affect her or his interaction with the other co-player. When not only the games but also the free-riding incentives are the same,

[1] An interaction is structurally independent if all parties involved are concerned only with their own payoff and if these payoffs depend only on the behavior of the involved parties, that is if the set of all parties involved qualifies as a *cell* according to the terminology of Harsanyi and Selten (1988).

spillovers could not be attributed to their characteristics but only to behavioral effects. This may seem less obvious for the asymmetric treatment where different free-riding incentives could induce contributing differently, i.e. independently left and right, thus questioning behavioral spillovers. Nevertheless, if the asymmetric treatment induce symmetry via not discriminating between neighbors who contribute similarly, this would enhance behavioral spillovers.

The behavioral assumptions behind the existence of this kind of spillovers, in our view, are that individuals are discrimination averse and conditional cooperators. An individual is said to be discrimination averse when not wanting to treat neighbors differently. With such an individual, behavior in one interaction is likely linked to behavior in the other interaction what triggers an intra-personal behavioral spillover. Because of overlapping player sets, the conditionally cooperating pairs of neighbors, furthermore, trigger inter-personal spillovers. Due to this combination of intra- and inter-personal spillovers, to which we refer as purely behavioral spillovers, the neighborhood is predicted to evolve as a whole.

Participants receive feedback information only on own payoff relevant contributions by their two neighbors and are thus free to react independently to each of them. However, if they are discrimination averse, they may want to align their behavior in both games. When such intra-personal spillovers apply to several members, who are conditionally cooperating[2], inter-personal spillovers arise and possibly spread. In this case, individual behavior may, over time, affect more distant neighbors with whom one is not directly interacting. Therefore behavior may spread not only from one structural independent game to another but also affect the whole neighborhood.

We can describe individual group members as dual selves[3] with the two selves of each participant facing a different neighbor[4]. We illustrate the interplay between the contribution levels of the two selves in Figure 1: for each individual member i we denote by L_i and R_i the

[2] As in Fischbacher et al. (2001) and Fischbacher and Gächter (2010).

[3] Ideas of multiple selves date back to Plato (see Allen, 2006) who distinguished between passion and reason that can be related to systems 1 and 2 (see Kahneman, 2011); for recent discussions see Elster (2009).

[4] Due to dominance solvability (0-contributions are strictly dominant) each such self is a cell in the terminology of Harsanyi and Selten (1988), i.e. each of the 8 local 2-person linear public good games has two proper subcells, one for each self of two interacting neighbors (see footnote 1 above).

contribution level in the left, respectively right game with the left $i-1$, respectively the right $i+1$ neighbor. The dashed bi-directional arrows indicate possible intra-personal spillovers, triggered by discrimination aversion (da), whereas the two solid bi-directional arrows between neighbors symbolize possible conditional cooperation (cc).

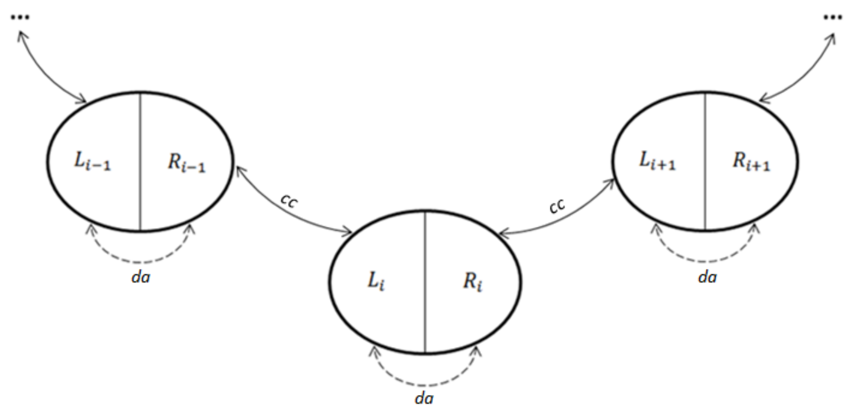

Figure 1: The interplay between intra-personal and inter-personal spillovers in our setup.

If one suffers when both selves are treating two equals differently, as postulated by discrimination aversion, an attempt to align both contribution levels could be triggered. Moreover, one can indirectly justify this by assuming inequity aversion[5]. As both neighbors are symmetric and should earn the same, affecting their payoffs differently could generate inequality which one wants to avoid. But then in the asymmetric treatment the question arises which free-riding incentive should trigger the same left and right contributions. If individual contribution - aligning anchors on the larger (lower) contribution in both games, voluntary cooperation could be enhanced (hindered) due to intra-personal spillovers.

We distinguish four (between-subjects) treatments: the main asymmetric treatment with different free-riding incentives on both sides and three control treatments featuring free-riding incentives equal to the lower, the higher, and the average of the asymmetric one, respectively. Discrimination aversion, in the sense of wanting to contribute similarly in both games, should be stronger when the incentives are symmetric but may still exist when they differ. To test this robustness of discrimination aversion, our main treatment allows for different free-riding

[5]See Fehr and Schmidt, (1999); Bolton and Ockenfels, (2000); for a survey, see Cooper and Kagel, (2016).

incentives in one's two games but maintains the symmetry of all eight members. Thus the asymmetric treatment presents a worst-case scenario for testing our main hypothesis of purely behavioral spillovers.[6] Our results, in fact, confirm purely behavioral spillovers, both for the asymmetric and the symmetric treatments.

The paper is organized as follows: Section 2 focuses on related literature. The experimental design is described in Section 3. Sections 4 states hypotheses and Section 5 analyzes the data. The conclusions in Section 6 discuss our findings and the methodological issues involved. The Appendix provides the English translation of the instructions for one treatment,[7] inter-group heterogeneity analysis, and additional data analysis.

2 On related behavioral spillovers studies

Spillover dynamics across different and structurally independent games[8], as in our setup is not a new topic. Our study shares some aspects with other experiments on behavioral spillovers[9] and, in particular, with Bednar et al. (2012), Cason et al. (2012), Savikhin and Sheremeta (2013), Cason and Gangadharan (2013) and Falk et al. (2013).

Bednar et al. (2012) focus on a class of infinitely repeated two-person binary action games with overlapping player sets to test if choices differ between isolated and simultaneous game play. Similarly to our design, they position (four) players on a circle, maintaining constant interaction with the left and right neighbor, but without informing participants about this. They consider the effects of cognitive load and behavioral spillovers linking individual choices across games. Their evidence of both processes is related to entropy, a novel measure of behavioral variance.

[6]As it is well known (see e.g. Isaac and Walker, 1988) there is a positive correlation between marginal per capita return and average contributions to public good when games are played in isolation but whether such correlation persists when public good games are jointly played is still an open question.

[7]The treatment differs only in free-riding incentives. The whole set of instruction is available from the authors upon request.

[8] Learning from repeatedly playing the same – as well as structurally different – (bidding) games has been referred to as robust learning. According to the evidence, reviewed by Güth (2002), learning in playing the same game quickly becomes weak, whereas conditioning on different rules (i.e., on game types) is strong and persistent.

[9]Theoretical analyses of multiple game plays have been provided by Samuelson (2001), Jehiel (2005) and Bednar and Page (2007).

More specifically, the authors argue (and provide consistent findings) that cognitive load has the greatest effect in games with high entropy while games with low entropy generate the largest spillovers onto games with high entropy.

Cason et al. (2012) analyze minimum and median effort games, played both simultaneously and sequentially with same and with different group composition, to assess how behavior in one game affects behavior in the other. For the treatment with repeated simultaneous plays of the two types of games, which is most comparable to our design, they find efficient coordination in the minimum effort game to occur less often than when the game is played after the median effort one, whereas there is no significant difference when it precedes the median effort game. For the treatments with simultaneous play and with the minimum effort game preceding the median effort one, they also fail to reject the hypothesis that average effort and average minimum effort are equal. Furthermore, they find that simultaneous choices in the median game positively affects the choice in the minimum game while the opposite does not occur. In treatments with sequential play experience with efficient coordination in the median effort game affects behavior in subsequent play of the minimum effort game with the same group of subjects and this effect persists, though weakened, when group composition changes. The authors relate the existence of these behavioral spillovers to two structural characteristics of the considered games, namely strategic uncertainty (measured by entropy) and path-dependence, with the former being higher in minimum effort game and the latter stronger in the median effort game.

Cason and Gangadharan (2013) focus on behavioral spillovers between a cooperative environment (a threshold public goods game with stochastic provision) and a competitive environment (a double auction market). Without communication, cooperation in public good provision is lower when subjects simultaneously interact in the double auction market whereas they do not find evidence that cooperation in public good game affects market price competition. The authors attribute this to the higher cognitive load of the simultaneous play.

Savikhin and Sheremeta (2013) analyze repeated plays of a lottery contest and a linear public good game by fixed groups of participants and find that overbidding in the contest is lower when it is played simultaneously with the public good game than when it is played in isolation. However, there are no significant differences in contributions to the public good when it is played together with the lottery contest or in isolation. The authors argue that behavioral spillovers can be attributed to differences in strategic uncertainty and path-dependence of the two game types. Since the contest features larger average volatility of bids, as compared to the average volatility of contributions to the public good, and contests are less path-dependent than public good contributions, the authors predict and confirm significant spillovers effects from the public good game onto the lottery contest when the two games are played simultaneously.

Falk et al. (2013) study social interaction effects both in a coordination and a cooperation game. The latter features two identical linear three-person public good games with one common player facing two different co-player sets with whom one interacts repeatedly. This common player confronts two structurally independent games, as each group member in our experiment, but without a neighborhood structure. Furthermore, differently from our setting, participants are not aware of being embedded in a larger matching group of nine participants who interact directly but also indirectly via common co-players. While the authors find evidence of social interaction effects (participants tend to contribute on average more to the group which has contributed more in the previous period), they do not find a statistically significant difference between average contributions in their two-group design and the control treatment with a single group.

In our view, similar behavior when playing one rather than two games can be due to both: that the two different co-players in both games react similarly to the same behavior and that they react differently to the only common player of both games when this player does not align choices across games. Only in the latter case the finding of Falk et al. (2013) denies intra-personal spillovers. Thus one would analyze whether and why the two disjoint co-player sets behave differently and how this affects their choices of the common co-player. Furthermore, in three-person games one's two co-players can behave differently but still be, on average, equally efficient.

Less related are studies of sequentially played games. Knez and Camerer (2000) study how precedent experience with efficient coordination in weak-link games affects play in a subsequent (repeated) Prisoner's Dilemma games with same subjects. Ahn et al. (2001) compare behavior in coordination and one-shot Prisoner's Dilemma games both with partner and with stranger matching protocols. Brandts and Cooper (2006) focus on repeated play of weak-link games with varying return from coordination by fixed group of participants. Bernasconi et al. (2009) are interested in the "unpacking effect" (see Rottenstreich and Tversky, 1997) and study "unpacked" vs. "packed" public goods games.[10] Evolutionary studies analyze or simulate how the population, composed of exogenous behavioral types, evolves when fitness is measured by actual profits (see Sethi and Somanathan, 1996; Noailly et al., 2007). Other experimental studies explore the different effects of specific networks structures on cooperation (see Suri and Watts, 2011 and Fatas et al., 2010) or compare direct reciprocity and reinforcement learning (see Biele et al., 2008). Finally, some studies do not maintain the structural independence of local public good provisions (see McCarter et al., 2014).

One may also wonder whether and how purely behavioral spillovers, intra-personal or inter-personal ones, are related to contagion effects [11] and social diffusion dynamics (see, e.g., Cassar, 2007). Such social dynamics for an exogenously given network are denied by the benchmark prediction based on common opportunism and backward induction based on finitely repeated elimination of (weakly) dominated strategies. In view of common opportunism, how one interacts with one neighbor should not matter for the interaction with the other neighbor, and backward induction should unravel all voluntary cooperation. This clear benchmark prediction is questioned not only by purely behavioral spillovers but also by contagion effects and their social diffusion dynamics.

[10] The "unpacking effect" is known to occur also in economic situations like the evaluation of private commodity bundles (see, e.g., Diamond and Hausman, 1994; and Bateman et al., 1997).

[11] Contagion can occur intra-personally, i.e. between one's two selves, as well as inter-personally. However, we do not refer to contagion which is better explored via repeated interaction experiments based on (random) strangers matching.

3 Experimental Setup

In the experiment, eight participants are randomly positioned in the circular neighborhood of Figure 2 locating an individual participant i (lighter color) in the bottom position. When interacting with the left $i-1$, respectively right $i+1$ neighbor, participant i's contribution is denoted c_i^L, respectively c_i^R (see Figure 3). Contributions are integers ranging from 0 to 9.

<Insert Figure 2 here>

<Insert Figure 3 here>

For each two-player games, a participant receives an endowment of 9 experimental currency units (ECU, with 1 ECU corresponding to 1 euro) in every period. In the left (right) interaction the constant gain from one unit of public provision – the so-called MPCR – is α_L (α_R). Participant i earns

$$\begin{aligned}&\left[9 - c_i^L + \alpha_L(c_i^L + c_{i-1}^R)\right] + \left[9 - c_i^R + \alpha_R(c_i^R + c_{i+1}^L)\right] \\ &\qquad = 18 - (c_i^L + c_i^R) + \alpha_L(c_i^L + c_{i-1}^R) + \alpha_R(c_i^R + c_{i+1}^L). \qquad (1)\end{aligned}$$

Imposing $0 < \alpha_n < 1 < 2\alpha_n$ for $n = L, R$ renders free-riding, i.e., $c_i^n = 0$ for $n = L, R$ and all $i = 1, ..., 8$, dominant and fully contributing, i.e. $c_i^n = 9$ for $n = L, R$ for all $i = 1, ..., 8$, efficient. The four treatments, T_a, T_l, T_m, and T_h, differ in their MPCRs. The main asymmetric treatment T_a varies the free-riding incentive in one's left and right public good game with $\alpha_L = 0.6$ and $\alpha_R = 0.8$, thus an additional contribution unit c_i^L, respectively c_i^R, generates a return of 0.6, respectively 0.8. Although quite many experimental studies vary more or less systematically the MPCR (for instance, Isaac and Walker, 1988; and more recently Cartwright and Lovett, 2014), we are not aware of attempts varying them intra-personally as in treatment T_a. Three symmetric treatments rely on aspects of the asymmetric one: treatment T_l with

$\alpha_L = \alpha_R = \min\{0.6, 0.8\} = 0.6$; treatment T_m with $\alpha_L = \alpha_R = \frac{(0.6+0.8)}{2} = 0.7$; and treatment T_h with $\alpha_L = \alpha_R = \max\{0.6, 0.8\} = 0.8$. We refer to T_a as our main treatment since it features the worst-case scenario to validate purely behavioral spillovers. The control treatments symmetrically capture the different free-riding incentives of the main treatment T_a, namely "lowest" via T_l, "mean" via T_m and "highest" via T_h.

A challenge of purely behavioral spillovers and a familiar topic of supergame experiments is endgame behavior, i.e. how far backward induction unravels voluntary cooperation. To allow behavioral spillovers to be more systematically challenged by endgame behavior we distinguish earlier and later termination by implementing a supergame experiment with an endogenous restart[12]: constant neighborhoods play either 8 or 16 periods. Participants know that a supergame will last for 8 periods with probability of 1/3 and for 16 periods with probability of 2/3 and only learn after period 8 whether the long or short horizon has been randomly selected. Constant groups (i.e. neighborhoods) with eight participants each experience four successive supergames. After each supergame, the same eight participants are randomly relocated within the neighborhood guaranteeing that each participant has at least one new neighbor, i.e. reshuffling occurs within neighborhoods and not between them.

In each period of each supergame, all eight participants choose their contributions (c_i^L, c_i^R) simultaneously being aware of α_L and α_R. After each period, feedback information is provided only on own and the neighbors' contributions as far as they concern the own payoff. To limit income effects across supergames, payment is the average payoff of one (after the experiment) randomly selected supergame.

For control treatments T_l and T_h, we employed in total 48 and 40 subjects respectively, i.e. 6 and 5 independent groups; for treatments T_m and T_a a total of 96 subjects each, i.e. 12 independent groups each. All subjects played four supergames but because of random restart the number of observations differs across treatments with the same number of participants. Each session included two or three groups of eight participants and lasted about one hour.[13] No

[12]We refer to this uncertainty as an endogenous restart possibility. We expected purely behavioral spillovers to limit but not exclude endgame effects (see Selten and Stöecker, 1986).

[13]Our setting takes into account Manski's (1993) reflection problem.

subject participated in more than one session. Altogether, 280 participants self-registered for participation through ORSEE (Greiner, 2015) at CESARE lab (Luiss Guido Carli University). Earnings (including a show-up fee of 5 euros) range from 11.4 euros to 32.4 euros, with an average of 20.55 euros. The experiment was programmed and conducted using z-Tree (Fischbacher, 2007).

4 Hypotheses

We expect to confirm some well-established results in public good experiments as a robustness check for our design. Regarding the role of incentives we expect higher (lower) contribution levels and smaller (larger) free-riding in treatments with higher (lower) MPCR. We also expect declining voluntary cooperation up to endgame[14] but a less striking in period 8 than in period 16 endgame effect. Specifically, we expect a recovery of voluntary cooperation in period 9 when learning about the endogenous restart.

Our central hypothesis, also the basis of some more specific hypotheses, claims behavioral spillovers across structurally independent local two-person public good games. In our view, such spillovers arise through the interplay of both intra- and inter-personal spillovers as triggered by discrimination (and indirectly by inequity) aversion as well as by conditional cooperation of neighbor pairs.

Without intra-personal spillovers there would be no behavioral contamination between one's left and right game. Thus our first hypothesis presupposes

> Hypothesis 1: subjects do not play their two local games independently but rather correlate their contribution choice on their left and right side, even in T_a for which we expect the weakest confirmation.

One reason is equity theory as early discussed by Homans (1961). One wants to treat equal others equally, similarly to the equality before the law. Since in our experiment all neighbors are

[14]See Andreoni (1988), the meta-study by Zelmer (2003) and the survey by Chaudhuri (2011).

symmetric, this could be the main driver of intra-personal spillovers. Other possible reasons for behaving similarly on both sides may be harmony seeking or avoiding the cognitive and emotional costs of arbitrary discrimination or conditioning on the past.

Without inter-personal spillovers there would be no behavioral contamination between neighbors' contribution to the public good. Thus our next hypothesis presupposes

> Hypothesis 2: subjects are willing to reciprocate with both their neighbors in the spirit of conditional cooperation.

Like, for instance, Fischbacher et al. (2001), we expect inter-personal spillovers to arise because of conditional cooperation: subjects react to past choices of those with whom they directly interact due to feedback information on own past outcomes and their neighbors' past contributions on which they depend.

Hypotheses 1 and 2 together imply purely behavioral spillovers and possibly the co-evolution of the whole neighborhood. Individual contributions on one side affect, via intra-personal spillovers, one's other contributions and, via conditional cooperation, also one's neighbor's contributions, what allows for contamination across the whole neighborhood. Conversely, if behavioral independence between the two games is verified, behavior will depend only on payoff-relevant contributions (own and that of the direct neighbor on the same side) and there will not be contamination from one game to another. Behavioral spillovers thus presuppose confirmation of Hypotheses 1 and 2, which jointly imply

> Hypothesis 3: behavioral spillovers arise through the interplay of intra- and inter-personal spillovers and affect the whole neighborhood.

Figure 1 illustrates the interplay of intra- and inter-personal spillovers: intra-personal spillovers link left and right contributions although the two games are structurally independent. Furthermore, inter-personal spillovers resulting from conditional cooperation link contributions by neighbor pairs.

If left and right contributions are MPCR-dependent, this could imply much smaller left than right contribution for the asymmetric treatment. In the spirit of discrimination aversion, however, some participants may still abstain from treating neighbors rather unequally.

5 Results

To confirm the hypotheses stated in the previous section, the analysis proceeds as follows: after investigating how free-riding incentives shape contribution choices and their dynamics in the different treatments, we test our specific behavioral hypotheses with the help of individual choice data. Finally, we demonstrate how purely behavioral spillovers affect the evolution of voluntary cooperation of the whole neighborhood, based on average group behavior and its dynamics.

5.1 Treatment effects and contribution dynamics

Table 1 lists average and standard deviation of contributions across treatments and percentages of free-riding, $(c_i^L, c_i^R) = (0, 0)$ contributions, in left and right games.

<Insert Table 1 here>

There is not much variation in standard deviations but considerable differences in average contributions. As expected, contributions increase with MPCR: average contribution is lowest in T_l (2.591) and highest in T_h (3.872), with T_m in between (2.992)[15]. In spite of equal average free-riding incentives, average contribution in the asymmetric treatment T_a (3.478) exceeds that of T_m, so that the average contribution in T_a is closer to T_h - average than of T_l. The share of free riding in T_a (14.88%) is lower than that in T_m (28.75%), and again closer to that in T_h (17.40%)[16].

[15]Tables 11 and 12 in Appendix A also list average contributions separately for periods $1-8$ and periods $9-16$, showing similar results.

[16]These differences are significant ($p = 0.000$) using conservative two independent-sample t-test where the unit of observation is frequency of $(0, 0)$ contributions per supergame and aggregated across all periods.

Statistically robust confirmation of the effects of incentives on average contributions is reported in Table 2.

<Insert Table 2 here>

For treatment testing in case of non-independent observations we follow the approach proposed by Moffatt (2016, pp. 84-85) and run an OLS regression of average group contribution by period on single dummies for pairwise comparisons of treatments, clustering standard errors at the group level in order to adjust for time dependence. Due to having eight individuals per group and the rather low number of groups, the ultra-conservative approach (allowing only one aggregate observation per matching group) is not feasible. Our (second-best) approach, which clusters at the supergame per neighborhood level and results in 4 clusters per neighborhood, allows us to exploit the unique structure and reshuffling mechanism of our design[17].

This analysis confirms that, in the symmetric treatments, lower MPCR corresponds to a significantly lower level of average contribution and, interestingly, in spite of the same average MPCR, contributions in T_m are significantly lower than contributions in T_a (cluster-robust p-values in parenthesis).

<u>Result 1</u>:

- in symmetric treatments, average contributions increase when the MPCR is higher;
- the asymmetric treatment T_a triggers, in spite of the equal average productivity, higher average contribution and less free-riding than T_m.

How average contributions to both public goods evolve in all treatments is graphically presented in Figure 4. Average contributions in T_a ("Asymmetric" in the figure) are higher than in T_m ("Medium" in the figure) in every period, which is consistent with Result 1.

[17] Participants of a group are reshuffled between supergames and guaranteed at least one new neighbor, while most receive a completely new set of neighbors (91.66% have a different neighbor on both sides).

<Insert Figure 4 here>

As expected, average contribution declines over time with a more substantial drop in period 8, when participants do not know whether the supergame will end or not (first endgame effect). However, voluntary cooperation recovers quickly in period 9 when learning that interaction continues, although this recovery is nearly absent from treatment T_l. Contributions decline more drastically in the last possible period (second endgame effect)[18].

Given the hierarchical structure of our data, we statistically confirm these effects via a multilevel regression model[19]; in particular, when dealing with group contributions, we cluster at the session, supergame and group levels (with the addition of treatment level when the sample is pooled). Table 3 (and its full version, Table 13, in Appendix A) reports regression results of average group contribution on supergame and period dummies using period 8 as the reference category. The sample for this analysis is restricted to supergames lasting 16 periods (see last row of Table 3) in order to test endgame and endogenous restart effects on the same pool of participants.

<Insert Table 3 here>

The coefficients associated to period dummies statistically confirm the descriptive analysis of contribution dynamics. With the exception of treatment T_l, coefficients increase in absolute value until period 8 when contributions reach their first lowest level because of the first endgame effect. However, in period 9 (and 10) participants in treatment T_m and T_a revive cooperation[20], confirming the endogenous restart effect. Finally, contributions drop more substantially in the final period, confirming the second endgame effect.

[18] These dynamics of contributions are in line with other linear public good experiments, e.g. Andreoni (1988), and therefore represent another robustness check for our results.

[19] Even though our results are robust to other specifications (such as two-limit panel tobit estimation), we believe that, because of the specific experimental design, the multilevel approach is the most appropriate since it allows to handle both group and session effects. Not using a two limit panel tobit estimation (i.e. not taking into account the censored structure of our dependent variables) leads, in our case, to similar levels of significance but slightly reduces the magnitude of the effects; see Moffatt (2016, pp. 92-97) for a discussion.

[20] Figures 4, 6a and 6b suggest that a restart effect in period 9 is present also in T_h. This is not confirmed by regression analysis, possibly because of smaller number of groups playing 16 periods in this treatment.

Regarding the asymmetric treatment, we analyze separately the left and right MPCR effect on average contribution. Table 4 presents differences between average left (c^L), resp. right (c^R) contribution in T_a, and average (left and right) contribution in treatments with the same free-riding incentives, i.e. T_l, resp. T_h. Both differences are positive, specifically more than four times larger for the higher (T_l) than for the lower (T_h) free-riding incentive. The fact that average contribution in T_l is lower than average contribution in $T_{a,l}$ shows that in presence of asymmetry subjects tend to reduce the gap between right and left contributions, by anchoring their contributions on the interaction in which they are more efficient (right side). This suggests that, when facing different left and right free-riding incentives, repeatedly interacting participants link their contributions more to the larger MPCR.

<Insert Table 4 here>

Table 5 reports statistical robust confirmation of the effect of the asymmetric free-riding incentives on average contribution[21]. The analysis confirms that contributions in T_l are lower than those in $T_{a,l}$ and also reveals that there is no significant difference between contributions in T_h and those in $T_{a,r}$.

<Insert Table 5 here>

Figure 5 visualizes the percentage share of zero, low $(1, 2, 3)$, medium $(4, 5, 6)$, and high $(7, 8, 9)$ contributions for all four treatments. In case of the asymmetric treatment, it distinguishes left and right contributions. The share of zero contributions, as discussed in Table 1, is lowest in $T_{a,r}$ ("Asymmetric right" in the figure) and T_h ("High" in the figure). The peaks are at low contributions for all treatments, except for T_h where the peak is at the medium level; finally, the share of high contributions in $T_{a,r}$ is higher than in T_h.

<Insert Figure 5 here>

[21] The same methodological remark concerning Table 2 applies here, therefore we once again follow the procedure suggested by Moffatt (2016, pp. 84-85).

How average contributions to left and right public goods in treatment T_a evolve is represented in Figures 6a and 6b. It is striking that left average contribution in T_a ("Asymmetric left" in the figure) is nearly always above that of T_m ("Medium" in the figure) in spite of T_a's larger free-riding incentive. Moreover there are almost no differences in the right average contributions between T_a ("Asymmetric right") and T_h ("High") for the first eleven periods, which further dynamically confirms that participant anchor their behavior towards lower free-riding incentive.

<Insert Figures 6a and 6b here>

Result 2:

- participants in T_a tend to close the gap between left and right contributions;
- behavior in T_a is closer to that of T_h than to that of T_l, i.e. participants tend to anchor on the lower free-riding incentive in repeated interaction.

Overall, Results 1 and 2 constitute a robustness check for our design, since they provide evidence of well-established patterns of behavior in public good experiments; moreover they show some novel findings related to the innovations that characterize our design, such as the endogenous restart effect. Furthermore, these results provide preliminary evidence that participants in the main asymmetric treatment T_a tend to be guided more by the lower free-riding incentive, thus suggesting that intra-personal spillover may have occurred. To investigate these issues, in the next section we provide an in-depth analysis of behavioral spillovers triggered by the interplay of inter- and intra-personal ones.

5.2 Behavioral spillovers analysis

To demonstrate the existence of intra-personal spillovers, Table 6 reports correlations between individual left and right average contributions together with the significance level in parentheses. Although structurally independent, the two public good games are not played independently:

the correlation between left and right individual average contributions are high and significant. Albeit generally higher than 50%, correlation is lowest for T_a in all supergames due to its asymmetric incentives.

<Insert Table 6 here>

Result 3: in spite of their structural independence, and in accordance to Hypothesis 1, average contributions to both local (left and right) public goods are behaviorally interdependent.

Table 7 (a) displays the average contribution received by participants who contributed on average at least seven ECU to both their neighbors in all but the last period (hereafter High contributors) and the average contribution received by participants who are not high contributors (Everyone Else). Furthermore, Table 7 (b) presents the average contribution received by participants who contributed on average at most two ECU to both their neighbors in all but the last period (hereafter Low contributors) and the average contribution received by participants who are not low contributors (Everyone Else).[22] As the analysis presented in Table 7 (and Tables 7, 14 and 15 in Appendix A) is based on reactions to feedback on neighbors' choices, we exclude the last period of play, either 8 or 16, when defining High and Low contributors, as neighbors can not react to that feedback. For the same reason we exclude period 1 when defining average contributions of High and Low contributors' neighbors.

Tables 7 (a) and (b) suggest that High (Low) contributors trigger significantly higher (lower) conditional cooperation levels by their neighbors, compared to "Everyone Else" ($p=0.000$ for both independent-sample t-test). In our view, this results is consistent with Hypothesis 2 since it confirms the prevalence of conditional cooperation among our participants.

Table 8 reports average left (right) contribution by participants whose right (left) neighbor is either High Contributor (a), Low Contributor (b) or Everyone Else. Tables 8 (a) and (b)

[22]For High contributors, the threshold of at least seven ECU on both sides corresponds to the ninetieth percentile of the distribution of average own left and right contributions. For Low contributors, the threshold of two ECU is below the average contribution of all four treatments.

reveal that participants with a High (Low) Contributor on one side contribute more (less) on the opposite side than other participants (p=0.000 for Low contributors vs. Everyone else, using a two independent-sample t-test; High contributors vs. Everyone else is also significant but the low number of observations, when aggregated, does not allow for enough test power). This result is, in our view, evidence of intra-personal spillovers, as postulated by Hypothesis 1, as it indicates that participants link their two contribution choices.

<Insert Table 7 here>

<Insert Table 8 here>

Tables 14 and 15 (in Appendix A) apply the analysis of Tables 7 and 8 only to treatment T_a by distinguishing between left and right neighbor (due to difference in free-riding incentives). The qualitatively similar results in T_a as in the other treatments – even for the low MPCR ($\alpha_L = 0.6$) – reveal a striking interaction effect of intra-personal and inter-personal spillovers: in spite of their different free-riding incentives, participants in T_a reciprocally behave in similar ways in their two games.

> Result 4: High (low) contributors positively (negatively) affect the contributions of their neighbors, who are not only conditionally cooperating and thereby inspiring inter-personal spillovers, but also contribute more (less) to their other neighbor, indicating intra-personal spillovers. This holds even in case of different free-riding incentives as in T_a. Altogether, this evidence supports Hypothesis 3 which postulates that behavioral spillover arise through interplay of intra- and inter-personal spillovers.

To validate these findings econometrically, we regress individual left (right) contribution in period t on own lagged left (right) contribution, supergame, period, and contributions made by both neighbors in period $t-1$. We use a multilevel model with clusters at session, supergame, group and individual levels (see Table 9).

<Insert Table 9 here>

Table 9 shows that an individual's contribution on one side is significantly affected by the same-side neighbor's (lagged) contribution, as suggested by the evidence of conditional cooperation, and also by the other neighbor's (lagged) contribution (except for the left contribution in T_m and T_h), in line with intra-personal spillover effects. Finally, "supergame" is not systematically significant whereas "period" has a small negative but significant coefficient (except for c_i^R in treatment T_h).

> Result 5: one's left and right contributions depend significantly on feedback (i.e. on own lagged contribution, supergame and period) as well as, for most treatments including T_a, on past contributions by both neighbors. On average, higher past contribution by one's neighbor triggers higher present contributions to both neighbors, thereby confirming Hypotheses 1 and 2, which are jointly required by Hypothesis 3.

Summing up, the support for Hypotheses 1 and 2 strongly confirm purely behavioral spillover effects, claimed by Hypothesis 3, for most treatments and both, left and right, games including treatment T_a whose unequal free-riding incentives could have weakened, even questioned, such spillovers.

5.2.1 Diffusion of behavioral spillovers

Due to the evidence of behavioral spillovers, each member's choice can indirectly affect, over time (periods), all other members. An important feature of our experimental design, involving overlapping sets of players, is that it allows to analyze if and how behavioral spillovers spread throughout the whole neighborhood (i.e. Hypothesis 3). To shed light on such indirect influences we analyze how contributions are affected by the relative distance of public good games. Since with longer delay, via periodic feedback, more contributions can influence present ones, our assessment of distance effects concentrates on the shortest delay by which one group member i can possibly influence another one.

Due to periodic feedback information it takes one period for member i to influence through behavioral spillovers, members $i+1$ and $i-1$, while it takes at least four periods for her to influence the most distant member $i+4$. For example, member i's contributions in period 1 can affect her neighbors' (members $i+1$ and $i-1$) contributions to their other neighbors (members $i+2$ and $i-2$) in period 2, etc. until lastly the most distant $(i+4)$ member's contributions can be influenced, via members $i+3$ and $i-3$'s contributions, only in period 4 (see Figure 7).

To trace distance effects we compute the sum of contributions (potentially) affected by member i's contribution choices in $k = 0, 1, 2, 3$ lags. For example, when $k = 1$ (respectively, $k = 2$, $k = 3$) we measure the sum of contributions to the public good games which take one (respectively, two, three) period(s) to be affected by member's i contributions. Obviously, when $k = 0$ we compute the sum of contributions to the two public good games in which member i is directly involved in.

The sum of contributions is denoted by $F_i(t+k)$, where $t = 1, ..., 15$ denotes period of play, and it is computed as follows:

$$F_i(t+k) = c^R_{i-(k+1)}(t+k) + c^L_{i-k}(t+k) + c^R_{i+k}(t+k) + c^L_{i+(k+1)}(t+k) \quad \text{for } k = 0, 1, 2, 3,$$

With the help of this notation, we define the distance difference D^k_i as the absolute value of the difference in aggregate contributions when $k = 0$ and when $k = 1, 2, 3$, as follows:

$$D^k_i(t) = |F_i(t+k) - F_i(t)|, \quad \text{for } k = 1, 2, 3,$$

The more or less delayed behavioral spillovers can be traced across the neighborhood via the distance differences $D^1_i(t)$, $D^2_i(t)$ and $D^3_i(t)$ for all members $i = 1, ..., 8$ and for all periods t (excluding period 16[23]), supergames, and treatments: to reiterate, as feedback on neighbors' contributions is received after every period, it takes one period to possibly affect a public good game that is one lag away, measured by $D^1_i(t)$, two periods to affect a public good game that

[23] We constrain our data analysis to all periods except for period 16, where endgame effect should overpower any spillover effects.

is two lags away, $D_i^2(t)$, and three periods to affect a public good game that is three lags away, $D_i^3(t)$. These distance differences are based on minimal delay by which $F_i(t)$ may influence $F_i(t+1)$, $F_i(t+2)$ and finally $F_i(t+3)$.

<Insert Figure 7 here>

Table 10 (a) displays the average and standard deviation of the absolute value differences $D_i^1(t), D_i^2(t)$ and $D_i^3(t)$ in contributions to public good games which are 1 (i.e., closest), 2, or 3 (i.e., furthest) steps away. It shows that despite being structurally independent, closer games reveal a smaller absolute difference in contributions, compared to games further apart. We view this as strong, albeit indirect, evidence of spillover effects: without behavioral spillovers there would not be such systematic differences in contributions depending on proximity from one another.

Table 10 (b) validates econometrically this influence by a multilevel regression (clustered at the treatment, session, group, supergame, and individual levels) which compares the differences in contribution sums based on the proximity to the two public good games in which member i is involved, for all members $i = 1, ..., 8$. It confirms that structural independence of games does not guarantee behavioral independence: proximity of games significantly affects their contribution differences.

The significant coefficient between $D_i^2(t)$ and $D_i^3(t)$ in Table 10 (b) suggests that games which are two steps away are more similar than those three steps away and this is an indication that the interplay of intra- and inter- personal spillovers affects the whole neighborhood and let it evolve as a whole, supporting Hypothesis 3.

<Insert Table 10 here>

Figure 8 depicts absolute differences in contribution sums between public goods which are 1, 2, 3 steps apart, without lag, across all periods except the 16th. Contribution differences increase mainly in the first four periods of a supergame, which is the minimal number of periods needed for the whole neighborhood to become "affected" via behavioral spillovers by a group member. After period 4, contribution differences between different distances stabilize. It seems that contributions adjust across games with the shortest possible delay.

<Insert Figure 8 here>

The fact that the differences in contributions stabilize without decreasing questions that neighborhoods become homogeneous in the level of cooperation due to behavioral spillovers (see Appendix B). Such heterogeneity is in line with the robust evidence of usual repeated public good experiments, especially with results showing that most but not all participants can be defined as reliable conditional cooperators.

6 Conclusion

We experimentally demonstrate that a constant neighborhood with eight members, who each repeatedly plays two structural independent games, is more than the parallel play of isolated games. Society members, although only bilaterally interacting, seem discrimination averse and are often conditionally cooperating, letting their group evolve as a "whole". Specifically, they try to establish a high level of voluntary cooperation which generally quickly recovers when learning that the game goes on.

The main conclusion from our data analysis is that behavioral spillovers are pervasive. As one's left play evolves strictly with right play, on the basis of such individual positive correlation, most participants seem discrimination averse and link their behavior in both games. Therefore, when participants are also conditionally cooperating, this also spills over interpersonally.

More specifically, this proves that:

- even across completely unrelated interactions we nevertheless generate our choice behavior in a holistic way,

- local experiences, even when restricted to local feedback information only, can become gradually appreciated by more and more others, and

- unequal free-riding incentives as in treatment T_a may foster voluntary cooperation when participants interact repeatedly.

The last point suggests that in repeated collective-action tasks we may be more influenced by good, e.g. efficiency enhancing, experiences than by worse ones and that discrimination averse participants anchor more on their better experiences. This could have an interesting policy implication for reducing the costs of fostering cooperation when free-riding incentives can be manipulated.

Our analysis distinguishes between intra-personal spillovers, due to discrimination aversion, and inter-personal spillovers, due to conditional cooperation. Together they let neighborhoods with eight participants evolve as a whole interrelated society in spite of its eight local games being structurally independent. Actually, this allows us to trace how behavioral spillovers affect even more distant members across time. In future research it could be beneficial to study more closely how behavior first spills over intra-personally and then inter-personally by considering an experimental design which can provide even more informative data. In our neighborhood setting, a player could react to the contribution of both neighbors via the strategy (vector) method[24] which would directly reveal intra-personal spillovers, even in simultaneous (left or right) contributions. In particular, this will allow to better understand how a good or bad experience in one's left or right game can immediately affect also how one behaves in the other game.

[24]See Fischbacher and Gächter (2010) and Di Cagno et al. (2016) for experimental methods employing leader/independent as well as follower/conditioning contributions.

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Tables and Figures

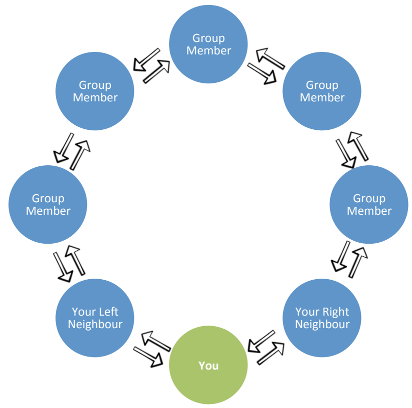


Figure 2: The circular neighborhood

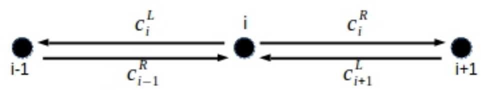


Figure 3: The interacting neighbors

	T_l	T_m	T_a	T_h	Total
Average	2.591	2.992	3.478	3.872	3.183
Std. Dev.	2.327	2.762	2.523	2.808	2.650
Freq.	2,688	5,568	4,672	1,856	14,784
Share of (0,0)	23.85%	28.75%	14.88%	17.40%	22.05%

Table 1: Average contribution by treatment

	T_l	T_m		T_a		T_h	
T_l	-	-0.401	(0.154)	-0.887***	(0.004)	-1.281***	(0.001)
T_m		-		-0.486**	(0.055)	-0.880***	(0.009)
T_a				-		-0.394	(0.260)
T_h						-	

Table 2: Difference in average contribution. Cluster-robust p-values in parentheses

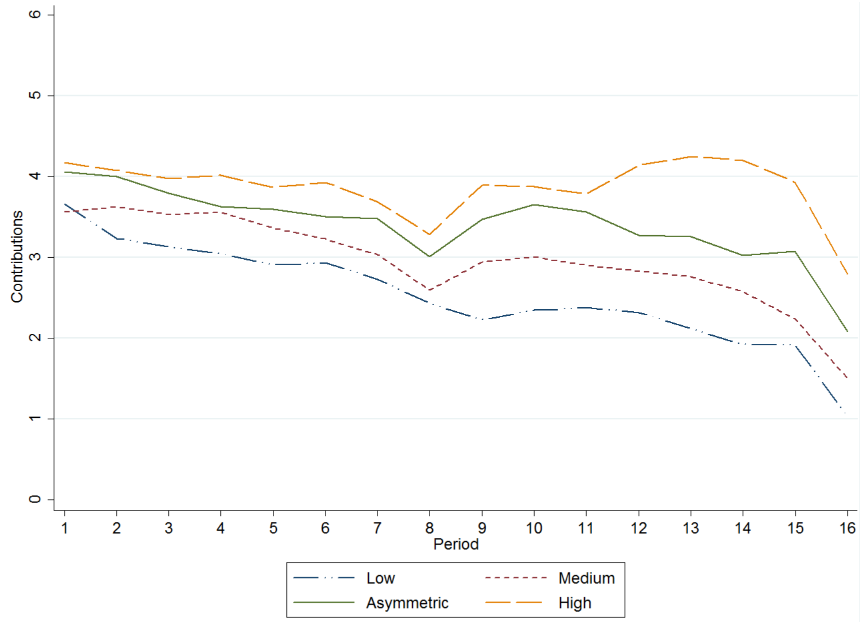


Figure 4: Average contributions to both PG

Dependent variable: average group contribution at period t					
	Pooled$^{(i)}$	$T_l^{(ii)}$	$T_m^{(ii)}$	$T_a^{(ii)}$	$T_h^{(ii)}$
Supergame	-0.194***	-0.080	-0.315***	-0.141	0.180
	(0.059)	(0.121)	(0.085)	(0.109)	(0.136)
Period dummies. Ref. category: period 8					
Periods 1-6	✓	✓	✓	✓	✓
Period 7	0.486***	0.399*	0.410***	0.542***	0.826**
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 9	0.338***	0.024	0.446***	0.328*	0.528
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 10	0.437***	0.142	0.510***	0.512***	0.507
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Periods 11-16	✓	✓	✓	✓	✓
Constant	3.221***	2.444***	3.362***	3.159***	2.603***
	(0.295)	(0.566)	(0.365)	(0.605)	(0.665)
Observations	1,456	288	624	400	144
Supergames lasting 16 periods	65%	75%	81%	52%	45%
Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1					
(i) Five level estimation (treatment, session, supergame and group)					
(ii) Four level estimation (session, supergame and group)					

Table 3: Multilevel regression of average group contribution. Sample is restricted to supergames lasting 16 periods only

$c^L(T_a) - \dfrac{c^L(T_l) + c^R(T_l)}{2}$	0.646
$\dfrac{c^L(T_h) + c^R(T_h)}{2} - c^R(T_a)$	0.153

Table 4: Differences in left $\left(c^L\right)$ and right $\left(c^R\right)$ contributions (average values)

	T_l	$T_{a,l}$		T_m		$T_{a,r}$		T_h	
T_l	-	-0.646**	(0.031)	-0.401	(0.154)	-1.128***	(0.000)	-1.281***	(0.001)
$T_{a,l}$		-		0.245	(0.309)	-0.482***	(0.000)	-0.635*	(0.066)
T_m				-		-0.727***	(0.002)	-0.880***	(0.009)
$T_{a,r}$						-		-0.153	(0.648)
T_h								-	

Table 5: Difference in average contribution. Cluster-robust p-values in parentheses

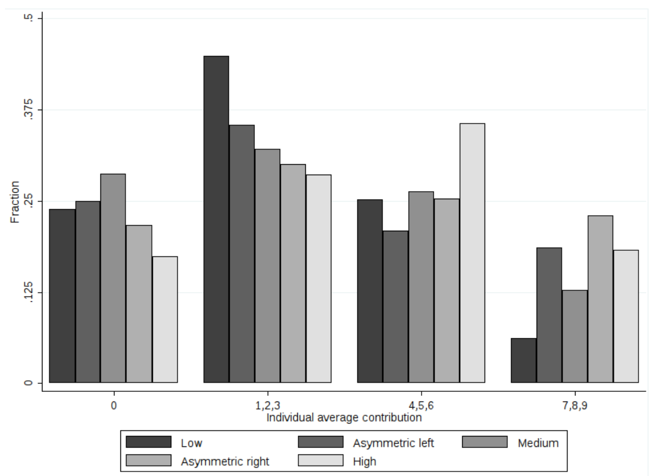
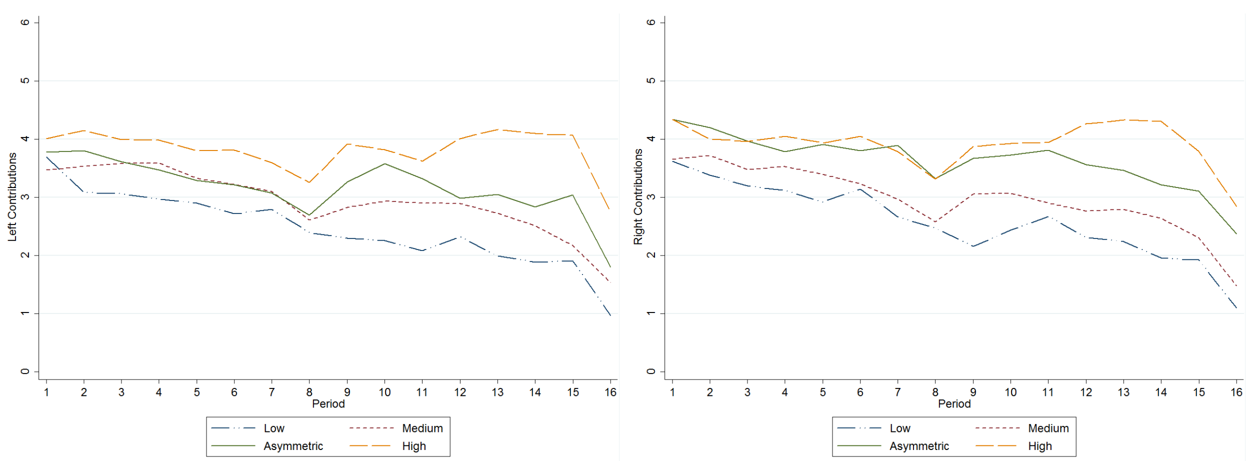


Figure 5: Contribution ranges by treatment



(a) Average contributions to left PG

(b) Average contributions to right PG

Figure 6: Average per-period Contributions to left (a) and right (b) PG

	T_l	T_m	T_a	T_h	All
Supergame 1	0.687*** (0.000)	0.639***(0.000)	0.562***(0.000)	0.802***(0.000)	0.632***(0.000)
Supergame 2	0.609*** (0.000)	0.552***(0.000)	0.507***(0.000)	0.731***(0.000)	0.581***(0.000)
Supergame 3	0.535*** (0.000)	0.630***(0.000)	0.463***(0.000)	0.755***(0.000)	0.593***(0.000)
Supergame 4	0.660*** (0.000)	0.585***(0.000)	0.615***(0.000)	0.514***(0.001)	0.607***(0.000)
All	0.637*** (0.000)	0.609***(0.000)	0.548***(0.000)	0.675***(0.000)	0.609***(0.000)

Table 6: Correlation of left and right individual average contributions of a given supergame and across supergames for each treatment and across treatments; p - values in parentheses.

	Average	Std.Dev.	Freq.
High contr.	6.528	2.218	866
Everyone else	3.042	2.202	12,798

(a) Contributions to High contributors

	Average	Std.Dev.	Freq.
Low contr.	1.603	1.774	4,128
Everyone else	3.982	2.217	9,536

(b) Contributions to Low contributors

Table 7: Average contribution received by High/Low contributors vs. by others in the neighborhood

	Average	Std.Dev.	Freq.
High contr.	4.531	2.267	866
Everyone else	3.182	2.226	12,798

(a) Spillover effect of High contributors

	Average	Std.Dev.	Freq.
Low contr.	2.476	2.167	4,128
Everyone else	3.610	2.202	9,536

(b) Spillover effect of Low contributors

Table 8: Effect of neighbor's type (High/Low contributors) on individual contribution to other neighbors

Dependent variable: left contribution $c_i^L(t)$								
	T_l		T_m		$T_{a,l}$		T_h	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
$c_i^L(t-1)$	0.300***	(0.018)	0.319***	(0.013)	0.303***	(0.014)	0.381***	(0.022)
Supergame	-0.114**	(0.057)	-0.124**	(0.056)	-0.101*	(0.052)	-0.020	(0.081)
Period	-0.038***	(0.009)	-0.059***	(0.007)	-0.056***	(0.008)	-0.037***	(0.014)
Neighbors' contributions, first lag								
$c_{i-1}^R(t-1)$	0.287***	(0.017)	0.236***	(0.012)	0.198***	(0.014)	0.252***	(0.022)
$c_{i+1}^L(t-1)$	0.066***	(0.018)	0.015	(0.012)	0.042***	(0.014)	0.008	(0.022)
Dependent variable: right contribution $c_i^R(t)$								
	T_l		T_m		$T_{a,r}$		T_h	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
$c_i^R(t-1)$	0.320***	(0.018)	0.338***	(0.012)	0.346***	(0.014)	0.397***	(0.022)
Supergame	-0.057	(0.056)	-0.124**	(0.053)	-0.166***	(0.053)	0.121	(0.077)
Period	-0.044***	(0.009)	-0.051***	(0.007)	-0.051***	(0.008)	-0.027**	(0.014)
Neighbors' contributions, first lag								
$c_{i-1}^R(t-1)$	0.041**	(0.017)	0.037***	(0.012)	0.046***	(0.014)	0.052**	(0.021)
$c_{i+1}^L(t-1)$	0.303***	(0.019)	0.265***	(0.013)	0.241***	(0.014)	0.267***	(0.022)
Observations	2,496		5,184		4,288		1,696	
Number of subjects	48		96		96		40	
Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1								

Table 9: Five-nested multilevel regression of individual left (upper subtable) and right (lower subtable) contributions (clustered at session, group, supergame, and subject levels).

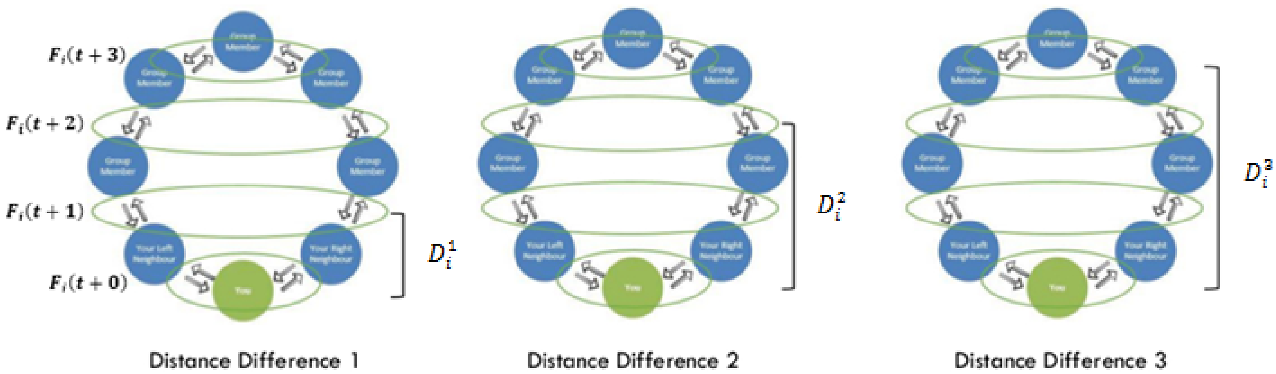


Figure 7: Distance Difference D_i^k for $k = 1, 2, 3$

	$D_i^k(t)$		
k	Average	Std. Dev.	Freq.
1	7.150	6.000	12,936
2	8.536	6.844	11,816
3	9.215	7.304	10,696
Total	8.153	6.851	36,480

(a)

	$D_i^1(t)$	$D_i^2(t)$	$D_i^3(t)$
$D_i^1(t)$	-	1.377***	2.038***
		(0.000)	(0.000)
$D_i^2(t)$		-	0.661***
			(0.000)
$D_i^3(t)$			-

(b)

Table 10: Summary statistics (a) and multilevel regression (b) of contribution differences between public goods which are $k-$distances away from each-other

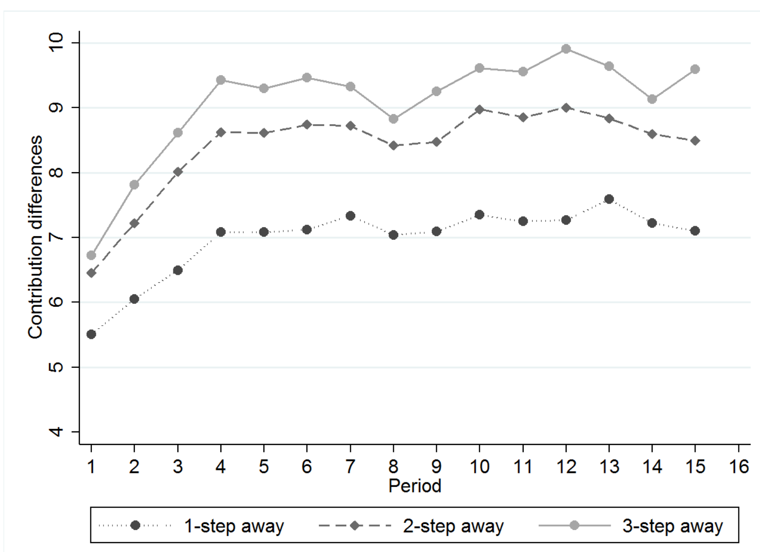


Figure 8: Absolute contribution differences by k-distances away (D_i^k for $k = 1, 2, 3$)

Appendix A - Additional Analysis

	T_l	T_m	T_a	T_h	Total
Average	3.009	3.314	3.635	3.878	3.452
Std. Dev.	2.393	2.739	2.526	2.691	2.618
Freq.	1,536	3,072	3,072	1,280	8,960
Share of (0,0)	16.99%	23.24%	12.92%	14.38%	22.05%

Table 11: Average contribution by treatment in periods 1 to 8

	T_l	T_m	T_a	T_h	Total
Average	2.033	2.596	3.177	3.859	2.769
Std. Dev.	2.111	2.739	2.491	3.053	2.646
Freq.	1,152	2,496	1,600	576	5,824
Share of (0,0)	32.99%	35.54%	18.63%	24.13%	29.26%

Table 12: Average contribution by treatment in periods 9 to 16

Dependent variable: average group contribution at period t					
	Pooled$^{(i)}$	$T_l^{(ii)}$	$T_m^{(ii)}$	$T_a^{(ii)}$	$T_h^{(ii)}$
Supergame	-0.194***	-0.080	-0.315***	-0.141	0.180
	(0.059)	(0.121)	(0.085)	(0.109)	(0.136)
Period dummies. Ref. category: period 8					
Period 1	1.229***	1.413***	1.138***	1.272***	1.132***
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 2	1.089***	0.906***	1.157***	1.125***	1.063***
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 3	0.941***	0.795***	0.958***	0.942***	1.153***
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 4	0.871***	0.705***	0.994***	0.720***	1.090***
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 5	0.737***	0.497**	0.796***	0.682***	1.111***
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 6	0.630***	0.490**	0.655***	0.518***	1.111***
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 7	0.486***	0.399*	0.410***	0.542***	0.826**
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 9	0.338***	0.024	0.446***	0.328*	0.528
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 10	0.437***	0.142	0.510***	0.512***	0.507
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 11	0.366***	0.174	0.405***	0.425**	0.417
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 12	0.279***	0.115	0.335**	0.133	0.771**
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 13	0.216**	-0.087	0.264*	0.118	0.882**
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 14	0.029	-0.281	0.080	-0.118	0.833**
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 15	-0.130	-0.288	-0.258*	-0.068	0.563
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Period 16	-1.002***	-1.170***	-0.990***	-1.055***	-0.569*
	(0.098)	(0.205)	(0.140)	(0.191)	(0.346)
Constant	3.221***	2.444***	3.362***	3.159***	2.603***
	(0.295)	(0.566)	(0.365)	(0.605)	(0.665)
Observations	1,456	288	624	400	144
Supergames lasting 16 periods	65%	75%	81%	52%	45%

Standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

(i) Five level estimation (treatment, session, supergame and group)

(ii) Four level estimation (session, supergame and group)

Table 13: Multilevel regression of average group contribution. Sample is restricted to supergames lasting 16 periods only

	Left neighbor		Right neighbor		
	Avg.	Std.dev.	Avg.	Std.dev.	Freq.
High contr.	6.578	2.712	6.966	2.486	206
Everyone else	3.653	2.985	3.136	2.823	4,082

(a)

	Left neighbor		Right neighbor		
	Avg.	Std.dev.	Avg.	Std.dev.	Freq.
Low contr.	2.334	2.889	1.573	2.188	955
Everyone else	4.212	2.949	3.821	2.917	3,333

(b)

Table 14: Average contribution received by High/Low type vs. by others in the neighborhood (T_a only)

	Left neighbor		Right neighbor		
	Avg.	Std.dev.	Avg.	Std.dev.	Freq.
High contr.	4.960	3.097	4.610	3.307	326
Everyone else	3.199	2.875	3.740	3.008	3,962

(a)

	Left neighbor		Right neighbor		
	Avg.	Std.dev.	Avg.	Std.dev.	Freq.
Low contr.	2.428	2.777	2.926	2.993	1,075
Everyone else	3.635	2.917	4.101	2.999	3,213

(b)

Table 15: Effect of neighbor's type (High/Low contributors) on individual contribution to other neighbors (T_a only)

Appendix B - Inter-group Heterogeneity analysis

This analysis illustrates that purely behavioral spillovers do not exclude heterogeneity of different neighborhoods, even within the same treatment. We visualize average individual contribution across periods[25] by "Low" "Medium" and "High" levels, using the same intervals as in Figure 5. The visual representations of neighborhoods illustrate how voluntary contributions can vary within and between neighborhoods and across treatments, and provide an intuitive and immediate impression. Neighborhoods are ordered according to the following criteria:

- Homogeneity in contributions (the same color shade across all eight members);

- Local concentration of high contributors (connected dark color spots); and

- Singular high contributors (isolated dark color spots).

In order to inspire intuition a few remarks are stated.

<u>Remark 1</u>: The left and the right neighborhoods in Figure 9 are quite homogeneous albeit differing considerably in their degree of voluntary cooperation.

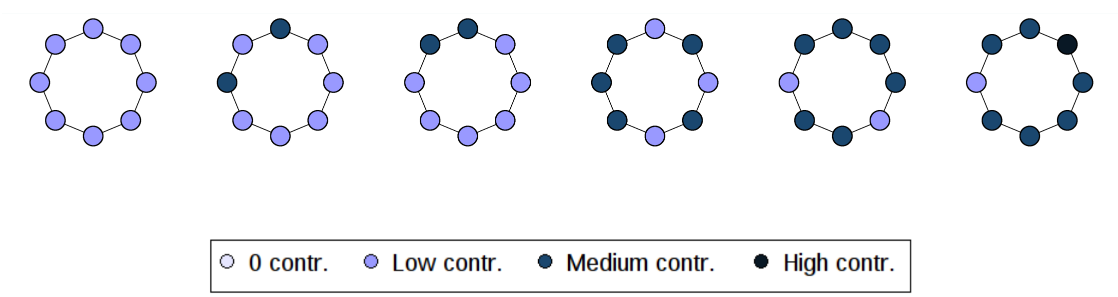


Figure 9: Treatment T_l

<u>Remark 2</u>: The upper left and lower right neighborhoods in Figure 10 differ most in their contributions.

[25] Average contribution does not include periods 8 and 16 to exclude endgame behavior.

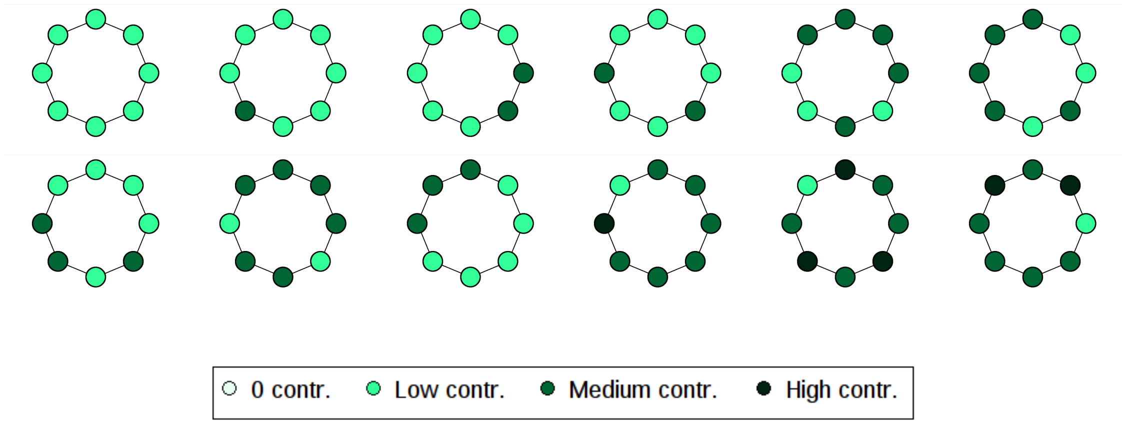


Figure 10: Treatment $T_{a,l}$

Remark 3: There exist isolated low and high contributors in Figure 11 even who may co-exist with clusters of similar contribution levels.

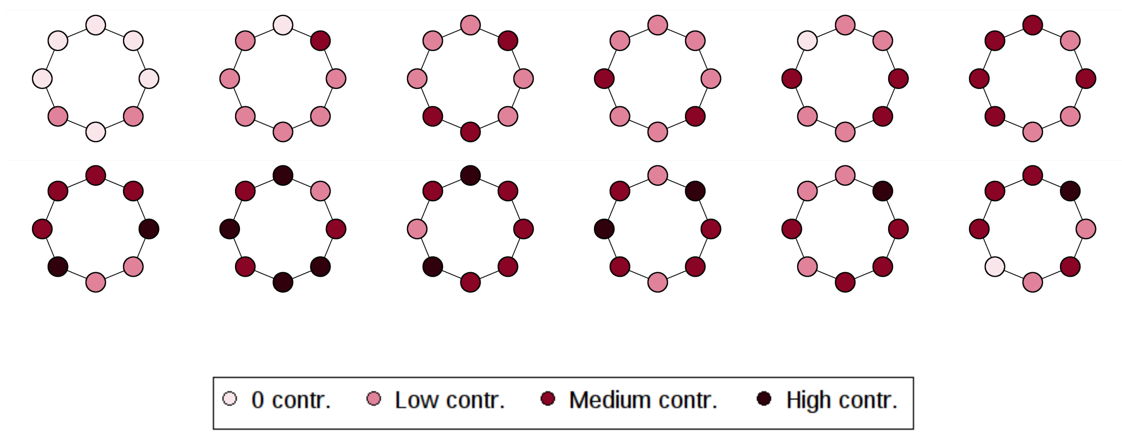


Figure 11: Treatment T_m

Remark 4: The upper left corner in Figure 12 is the least cooperative one. It appears that high voluntary cooperation never had a chance in this neighborhood.

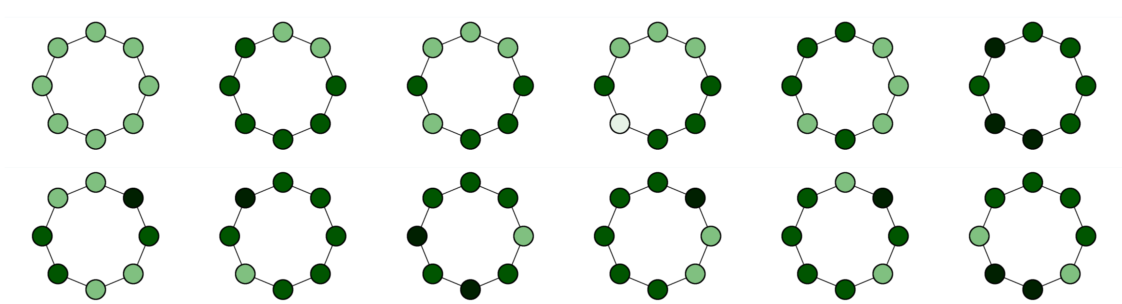


Figure 12: Treatment $T_{a,r}$

Remark 5: Homogeneous "medium" neighborhoods exist (also in treatment T_h, see Figure 13).

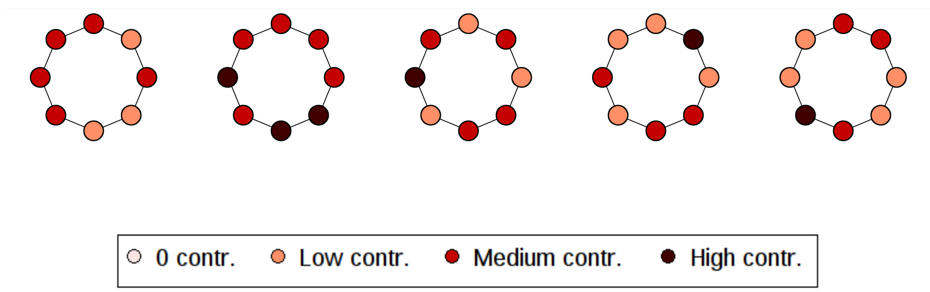


Figure 13: Treatment T_h

Across almost all treatments one can identify (at lest sub)neighborhoods of homogeneous contribution levels, whose average contributions become higher from T_l to T_m, from T_m to T_a and from T_a to T_h.

Note also how MPCR affects individual behavior: when the free-riding incentive is symmetric and high, only one in 48 subjects (2.08%) is a high contributor; the number of high contributors increases (6.25%) when considering the left side of the asymmetric treatment, in spite of the same free-riding incentive. This partly accounts for the similar percentage of low contributors in T_a (34.37%) and in T_h (35%), another corroboration that participants do not discriminate between their two neighbors as the difference in MPCR would suggest. The positive effect of asymmetry suggests it could be exploited in order to boost cooperation while maintaining the same average incentives.

Appendix C - Instructions

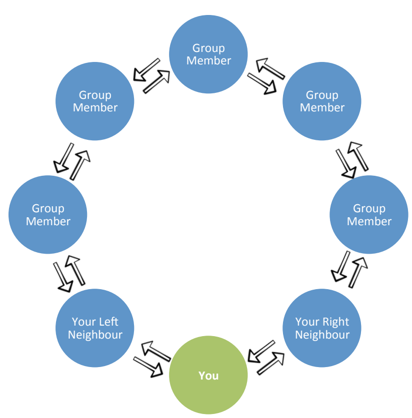
You are participating in an experiment about economic decision-making. During the experiment, you can earn money. Your earnings will depend on your decisions and the decisions of others. These instructions describe the decisions you and other participants should take and how your earnings are calculated. Therefore, it is important to read them carefully.

During the experiment, all interactions between the participants will take place through computers. It is forbidden to communicate with other participants by any other means. If you have any questions, please raise your hand and one of us will come to answer it. Keep in mind that the experiment is anonymous, i.e., your identity will not be disclosed.

During the experiment, your earnings will be calculated in points. At the end of the experiment, the points will be converted to Euros at the following exchange rate:

$$1 \text{ point} = 1€ .$$

In the experiment, you will be a member of a group containing a total of eight members, including you. For the purpose of this experiment, you and the rest of the members in the group are positioned in a circular manner. This means that each member has a neighbor to the left and a neighbor to the right.



During the experiment, each of you will interact with your two neighbors. These two neighbors will be the same two individuals for one supergame. In the experiment, there will be a total of four supergames. One supergame lasts either 8 or 16 periods (as will be explained later). Therefore, you will have to make either 8 or 16 decisions before the supergame ends. At the end of each supergame, your group consisting of eight members will be reshuffled randomly. For every member, at least one neighbor will be different from the previous supergame. *Keep in mind that you do not know the identity of your neighbors so you will not know if both of your neighbors are new, or just one of them.*

How many periods a supergame lasts depends on chance. A supergame will last for 8 periods with a probability of 1/3, and 16 periods with probability of 2/3.

In each period, you and your two neighbors will be endowed with points. More specifically, nine (9) points will be assigned to you for the interaction with your left neighbor, and nine (9) points will be assigned to you for the interaction with your right neighbor. The same number of points will be assigned to both of your neighbors, and all other members in your group.

In each period, you will have to decide, individually and independently, how many of the nine points you are endowed with you will want to contribute to a project with your left neighbor. In what follows, this is referred to as Project L. Similarly, in each period you will have to decide, individually and independently, how many of the nine points you are endowed with you will want to contribute to a project with your right neighbor. In what follows, this is referred to as Project R.

Keep in mind that you can invest a maximum of 9 points to Project R and a maximum of 9 points to Project L; moreover, you cannot invest your points for Project R into Project L, and vice versa.

You will retain for yourself the points that you decide not to invest in either project. Therefore, you will keep for yourself 9 −Your contribution to Project L; similarly you will keep for yourself 9 −Your contribution to Project R. For example, you can invest 8 points in project R, and keep $9 - 8 = 1$ for yourself, or invest 3 points in Project L and keep $9 - 3 = 6$ to yourself.

Every member is going to make the decisions simultaneously.

PAYOFFS

Your payoff in each supergame will depend only on your own choices and on those of your two neighbors

At the end of each period, your payoff is computed in the following manner:

For Project R: $(9 -$Your contribution$) + 0.7 *$ (Your contribution $+$ Your right neighbor's contribution)

For Project L: $(9 -$Your contribution$) + 0.7 *$ (Your contribution $+$ Your left neighbor's contribution)

EXAMPLE: Let's try to compute your payoff with the example given above. For the purpose of the example we imagine that both your right and left sided neighbors contribute 8 points. If you contribute 8 points into Project R, your payoff will be $0.7 * (8 + 8) + 1 = 0.7 * 16 + 1 = 11.2 + 1 = 12.2$. Similarly, if you contribute 3 points into Project L, your payoff will be $0.7 * (3 + 8) + 6 = 7.7 + 6 = 13.3$.

In each of the successive periods, all group members will simultaneously choose their contributions to Project R and to Project L. *Keep in mind that you play multiple periods with the same participants and that you decide about your own contribution without knowing the contributions of your neighbors.*

At the end of each period, each group member will be informed about own payoffs from Project L and from Project R, contributions by both left and right neighbors, and accumulated earnings from both projects.

What you will actually earn is:

At the end of the experiment the computer will randomly select the average payoff you obtained in one of the four supergames as a final payment. Thus your payment will be equal to the average payoff of supergame 1, or to the average payoff of supergame 2, or to the average payoff of supergame 3, or to the average payoff of supergame 4. Such a payoff will be converted to Euros at the exchange rate of 1 point = 1 €.