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Social Learning and Voluntary Cooperation among Like-Minded People

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Abstract

Many people contribute to public goods but stop doing so once they experience free riding. We test the hypothesis that groups whose members know that they are composed only of 'like-minded' cooperators are able to maintain a higher cooperation level than the most cooperative, randomly-composed groups. Our experiments confirm this hypothesis. We also predict that groups of 'like-minded' free riders do not cooperate. Yet, we find a high level of strategic cooperation that eventually collapses. Our results underscore the importance of group composition and social learning by heterogeneously motivated agents to understand the dynamics of cooperation and free riding.

Keywords

Public goods, social learning, conditional cooperation, free riding, experiments

JEL Classification

C91, H41, D23, C72

1. Introduction

Research in experimental economics and social psychology has repeatedly demonstrated that many people cooperate even in tightly controlled one-shot prisoner's dilemmas and public goods experiments, where the payoff structure entails a dominant strategy to free ride. However, an equally frequent observation is that cooperation declines to rather low levels in repeatedly played cooperation games (Ledyard 1995).

How can we explain these findings, which are puzzling from the viewpoint of rationality and the assumption of selfishness? One explanation is that people are confused to some degree and have to learn to play their dominant strategy, i.e., reduced errors explain the decay (e.g., Palfrey and Prisbrey 1997). Another explanation, which will be central to our paper, focuses less on learning how to play the game but on *social* learning: People differ in their cooperative attitude and learn, during repeat play, about the social behavior of others.[1] The background for this conjecture is the observation from numerous experiments that some people are 'conditional cooperators' whereas others are 'free riders'. The conditional cooperators cooperate if sufficiently many others cooperate as well (e.g., Keser and van Winden 2000; Fischbacher, Gächter, and Fehr 2001; Croson 2002; Falk and Fischbacher 2002; Burlando and Guala 2004; Fischbacher and Gächter 2004). A sizeable minority of people is best described as selfish, because they free ride, whenever this is in their material self-interest. If, as is typical in most experiments, membership in groups is randomly determined, cooperation is very likely to be fragile in repeatedly played experiments, despite the fact that some people are willing to cooperate. The reason is that conditional cooperators, who learn that others take a free ride, are likely to reduce their own contribution, because they do not want to be the 'suckers' (see Fischbacher and Gächter 2004).

This analysis suggests that cooperation is bound to be fragile if an agent's social learning about other group members is based on observing their cooperation decisions resulting from a *mixture* of motivations that are unknown to the agent. By contrast, if conditional cooperators, for instance, would know that the other group members are as well 'like-minded' conditional cooperators, then social learning would be confined to observing cooperative behavior. "Team reasoning" (e.g., Sugden 1993) and subsequent cooperation should be easy if the team players know that they are among like-minded team players. In this case social learning should sustain cooperation and prevent free riding. Likewise, if free rider types would know that they are among other free riders, free riding should be paramount.

This paper presents experimental evidence on the conjecture that cooperation among like-minded people is substantially different from cooperation in randomly composed groups. To this end, and as we will explain in detail in the next section, we first determine a subject's type and then sort subjects into homogeneous groups of similar types. Subjects are then informed that they will play ten rounds of the public goods game with the same group members who are of their type. We compare the 'sorted' like-minded groups to randomly composed control groups to determine the impact of knowing to be among 'like-minded' group members.[2]

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[1] In our context, social learning refers to learning *about* the behavior (or type) of others, whereas in other contexts, e.g., information cascades, social learning means learning *from* others.

[2] Thus, our experiments are related to studies that have also investigated group composition effects. Recent examples comprise Hayashi and Yamagishi (1998), Ehrhart and Keser (1999), Hauk and Nagel (2001), Page, Putterman and Unel (forthcoming), Coricelli, Fehr, and Fellner (2004), and Riedl and Ule (2004). These studies are mainly interested in *endogenous* partner selection. By contrast, in our study, group members are *exogenously* matched. In this respect, our paper is related to Ockenfels and Weimann (1999), Gunnthorsdottir et al. (2001), and Ones and Putterman (2004). However, these papers differ both

To get a further yardstick about the effectiveness of being among like-minded group members, we also conduct 'sorted' and 'random' experiments in which group members have a punishment option. Punishment is a mechanism that can sustain very high cooperation levels (e.g., Fehr and Gächter 2000; Sefton, Shupp, and Walker 2002; Masclet et al. 2003; Falk, Fehr, and Fischbacher 2004; Carpenter forthcoming; Page, Putterman, and Unel forthcoming).

Consistent with our conjectures, we find that, when among other cooperators, cooperation-minded people can sustain almost efficient cooperation, even in the absence of a punishment option. Contrary to our prediction, we also find that free riders manage to cooperate strategically at non-negligible levels. Yet, in the absence of a punishment opportunity cooperation among free riders collapses entirely by the final period.

Overall our results suggest that social learning rather than learning about the game drives cooperation and free riding. Because people are heterogeneous with respect to their cooperative attitudes, the exact dynamics entailed by social learning depends on group composition.

2. Procedures

Our goal is to study how 'like-minded' people, i.e., people who know that they share a similar attitude to the cooperation problem, actually cooperate. A suitable instrument to determine 'like-mindedness' should be simple and credibly reveal true preferences. For our purposes, we use a *one-shot* linear public goods game as the measurement instrument for cooperative attitudes. This game is simple and has the advantage that its payoff structure gives players a dominant strategy to free ride, i.e., to contribute nothing, if they only care about their monetary income.[3] We use the actual contribution level as a measure of the strength of cooperative attitude. The one-shot nature of the game makes a contribution choice unbiased (i.e., non-strategic) and therefore a credible revelation of true cooperation preferences.[4]

The details were as follows.[5] The one-shot public goods game was conducted among randomly generated groups of three people. We call this first experiment the *Ranking* experiment. All subjects were endowed with 20 ECU (experimental currency unit). Each subject *i* decided independently how many ECU (between 0 and 20) to contribute to a linear public good. The contributions of the whole group were summed up and subject *i*'s payoff was

in their research questions and a number of design details from our study. The paper closest to ours is Burlando and Guala (2004).

[3] To ensure that subjects understand their incentives we administered a set of eight control questions that tested the subjects' understanding of the payoff function. Subjects had to solve all questions successfully before the experiment could start. This was made public knowledge. In our experiments all subjects were able to solve all questions in due time. Before making a binding contribution decision, subjects could also use a 'what-if-calculator' that allowed them to calculate their payoffs by inserting combinations of own and others' contributions. Thus, we can safely assume that subjects' contributions in this initial game are well-considered.

[4] Ockenfels and Weimann (1999) report an experiment in which they sort people into 'cooperative' and 'less cooperative' groups. They do this on the basis of observing these people's contribution over ten rounds of a repeated public goods game. They do not get any effect of this sorting on cooperation levels. Since we do get strong effects as we will show below, this suggests that a repeated game and/or still rather heterogeneous 'cooperative' and 'less cooperative' groups may indeed dilute signals about cooperation *preferences*.

[5] For the instructions see Appendix B.

$$\pi_i = 20 - g_i + 0.6\sum_{j=1}^{3} g_j .$$

This is a standard linear public good where the marginal per capita return is 0.6 (and the social marginal return is 1.8). Therefore, payoffs give money-maximizing subjects a dominant strategy to free ride, i.e., to choose $g_i = 0$, whereas efficiency would require $g_i = 20$ by all i.

Subjects played this *Ranking* experiment just once. They were informed about this both in the instructions and by public announcement. After all participants had chosen their contribution the *Ranking* treatment ended. Subjects received *no* information about the decisions of the other group members and about their earnings at this time.

Subjects then received new instructions for the main experiment, which consisted of a ten period repeated public goods game with constant group memberships and the same parameters as in the *Ranking* experiment. Subjects were informed about the main experiment only at this stage of the experiment. (Before they started the *Ranking* experiment, they were told that some other part of the experiment would follow, but were not given further details.) This was necessary to ensure that the *Ranking* experiment measures subjects' cooperation preferences as accurately as possible. If subjects knew about the details of the main experiment and that their contribution in the *Ranking* experiment would influence the regrouping procedure then their choice in the *Ranking* experiment could have been strategically biased and would not have been a credible indication of cooperation preferences.[6]

In the instructions as well as in public announcements subjects were informed that groups would be rearranged as follows: All participants in a session were ranked according to their contribution to the project in the *Ranking* experiment. The first group consisted of the three subjects that had chosen the highest contributions in the *Ranking* experiment. The subjects with the fourth to sixth highest contribution constituted a second group and so on. The last group consisted of the three subjects who had chosen the smallest contribution in the *Ranking* experiment.

Subjects were then informed about their *new* group members' average contributions in the *Ranking* experiment. After the rearrangement of groups and the information about what the new group members contributed in the *Ranking* experiment, the main experiment started. We call this main experiment the *Sorted* experiment.

In order to identify the effect of the sorting mechanism we conducted two kinds of control treatments: the *Ranking-Unsorted* and *Simple* experiments. In the *Ranking-Unsorted* control experiments, subjects played the *Ranking* experiment but this had no effect on the regrouping procedure. The new groups for the main treatment were unsorted, i.e., formed randomly. As in the main treatment subjects received the information about their new group mates' average contributions in the *Ranking* experiment. The control experiment *Simple* consisted merely of the ten period public goods game of randomly composed groups, i.e., there was no *Ranking* experiment. We found no significant differences between the contributions in the *Ranking-Unsorted* and the *Simple* experiments. This also holds for the experiments with punishment described below (both $p>0.43$, Mann-Whitney tests, independent groups as observations). For the

[6] It is an interesting issue for further research to investigate how knowledge about the sorting procedures would influence contribution choices in the *Ranking* experiment and in the subsequent finitely repeated experiment. Similarly, a further interesting treatment would be to sort people as in our experiment but not to tell them about this sorting mechanism. This would allow teasing apart the effects of being sorted and knowing about it. The results by Gunnthorsdottir et al. (2001) and Burlando and Guala (2004) suggest that even resorting of which subjects are unaware can increase cooperation relative to random matching.

analysis we will therefore pool the data of the *Ranking-Unsorted* and *Simple* treatment and call these observations *Random*. In the following we will refer to the experiments with no punishment as *Sorted N* and *Random N*, respectively.

The experiments in which punishment was available (called *Sorted P* and *Random P*) had exactly the same structure as the *Sorted N* and *Random N* experiments described above. In the *Sorted P* experiments, subjects, after they were sorted into their new groups as a function of their contribution in the *Ranking* experiment, learned both that the public goods game would be played repeatedly with the same new group members and that a punishment option was available at the second stage. The *Ranking* experiment was exactly identical to the previous one (i.e., it involved no punishment). In the public goods game with punishment subjects at the first stage made simultaneous contributions to the public good as in the *Sorted N* and *Random N* experiments, respectively. They were then informed about their group member's individual contributions to the public good and could assign costly punishment points to each group member. One punishment point assigned cost the punishing subject 1 ECU and reduced the punished group member's income by 3 ECU. Each group member could assign up to ten punishment points to each other group member.

In total 231 subjects participated in the ten sessions of our experiments (54 in *Sorted N*; 51 in *Random N;* 72 in *Sorted P* and 54 in *Random P*). The experimental subjects were first-semester undergraduate students from the University of St. Gallen majoring in economics, business, law, or international relations. The experiment was programmed and conducted in z-Tree (Fischbacher 1999). It lasted about 1.5 hours and the subjects earned on average CHF 46 (about €30).

3. Hypotheses and Results

For expositional ease and the data analysis we will divide the newly formed groups in the *Sorted* experiments into three classes, each containing a third of the observations. The third of the groups with the highest average contribution in the *Ranking* treatment is called the class of TOP cooperators. The groups in the middle and lowest third are called MIDDLE and LOW cooperator groups, respectively. In the *Random* experiments, where there is no sorting, we classify *Random* groups *ex post* according to their average contributions over all ten periods. We classify them into the top, middle and least cooperative third of groups. They will serve as comparison classes for the *Sorted* experiments.

We are now ready to formulate our hypotheses, which follow from the mounting evidence, mentioned in the introduction, that there is a large degree of heterogeneity in cooperative attitudes. We formulate our hypotheses for the LOW and TOP cooperator groups. For them, being among like-minded group members should matter the most. MIDDLE cooperator groups which consist of people with intermediate degrees of cooperativeness, should behave like the middle third of cooperative groups in *Random N* and *Random P*, respectively.

Hypotheses 1 and 2 formulate our expectations about cooperation levels in the experiments with no punishment (*Sorted N* and *Random N*).

Hypothesis 1: The average contribution of TOP cooperator groups in the *Sorted N* experiments is higher than the average contribution of the *top cooperative third* of groups in the *Random N* experiments.

Hypothesis 2: The average contribution of LOW cooperator groups in the *Sorted N* experiment is lower than the average contribution of the *least cooperative third* of groups in the *Random N* experiments.

Our next set of hypotheses concerns the experiments with punishment.

> **Hypothesis 3:** TOP cooperator groups do not need punishment to achieve high cooperation. Therefore, the average contribution of TOP cooperator groups in the *Sorted P* experiments is the same as in the *Sorted N* experiments. Since we predict no free riding among TOP cooperator groups, they do not punish.

> **Hypothesis 4:** The average contribution of LOW cooperator groups is the same as in the *Sorted N* experiment. Since punishment is costly, LOW cooperator groups do not punish.

Figure 1 contains our main results. We start with the *Random N* and *Sorted N* experiments. Panel A shows the average contribution (dashed line) in the *Random N* experiments, as well as the mean cooperation levels of the top, middle and lowest third of these randomly composed groups. The average contribution is relatively stable until period 8 and then shows the typical endgame effect known from many finitely repeated public goods experiments (e.g., Keser and van Winden 2000). In the final period, contributions are not significantly different between classes (Kruskal-Wallis test, p=0.412).[7]

Panel B depicts the contributions in the *Sorted N* experiments. Period 0 indicates the *Ranking* experiment. We find that contributions in the *Ranking* experiment vary over the whole strategy space. Subjects who were later on sorted into TOP contributor groups contributed on average 18.1 ECU in the *Ranking* experiment; MIDDLE contributors invested 10.1 and LOW contributors 0.8 ECU.

A comparison of average contributions in *Random N* and *Sorted N* shows that, overall, sorting people led to a substantial increase in contributions (see also Burlando and Guala 2004 who report a similar result). In *Random N* average contributions were 9.5 ECU, whereas in *Sorted N* they amounted to 13.9 ECU. The difference is significant (p=0.012, Mann-Whitney test with group average as observations).

We now test Hypotheses 1 and 2. We find unambiguous support for Hypothesis 1. TOP cooperators, when playing together, contributed significantly more than the most cooperative third of groups in the *Random N* experiments (Mann-Whitney-test, p=0.024). With 4.3 ECU, the difference in cooperation levels is quite substantial (14.1 vs. 18.4 ECU). This comparison is interesting, because the cooperation level by the top third of groups in *Random N* is the upper bound of cooperation that one can expect in randomly composed groups. Thus, when 'like-minded' cooperators are sorted together, one can expect a substantially higher and more stable cooperation level than in the best case of randomly composed groups.

Quite to our surprise – and contrary to our Hypothesis 2 – we find substantial contributions among the free riders who comprise the LOW contributor group. Although they contributed much less than the groups in the other classes, we find even *higher* contributions among the sorted LOW contributor groups than among the least cooperative third of groups in the *Random N* experiments. The difference is borderline insignificant (Mann-Whitney test, p=0.109).

We offer two (speculative) explanations for this observation. A first explanation is derived from the bounded rationality of subjects who do not backward induct but know that earning money requires cooperation (Selten and Stoecker 1986). LOW contributors have revealed to each other that they chose the money-maximizing strategy in the *Ranking* experiment. They may therefore believe that there are no cooperators around to free ride on. Thus, they understand that they need to cooperate among themselves if they want to earn money. The second explanation rests on the possibility that LOW

[7] All statistical tests are based on independent group averages. For the data see Appendix A.

contributors actually believe that some other LOW contributors invested nothing in the *Ranking* experiment not because they are free riders, but because they are conditional cooperators with pessimistic beliefs. Then LOW contributors have an incentive to cooperate strategically until the ninth period to induce the conditional cooperators to contribute. They free ride in the final period, when cooperation is not in their rational self-interest anymore. Thus, if for whatever reason LOW contributors believe that some others are conditional cooperators, then rational cooperation is possible even in a finitely repeated cooperation game (Kreps et al. 1982).

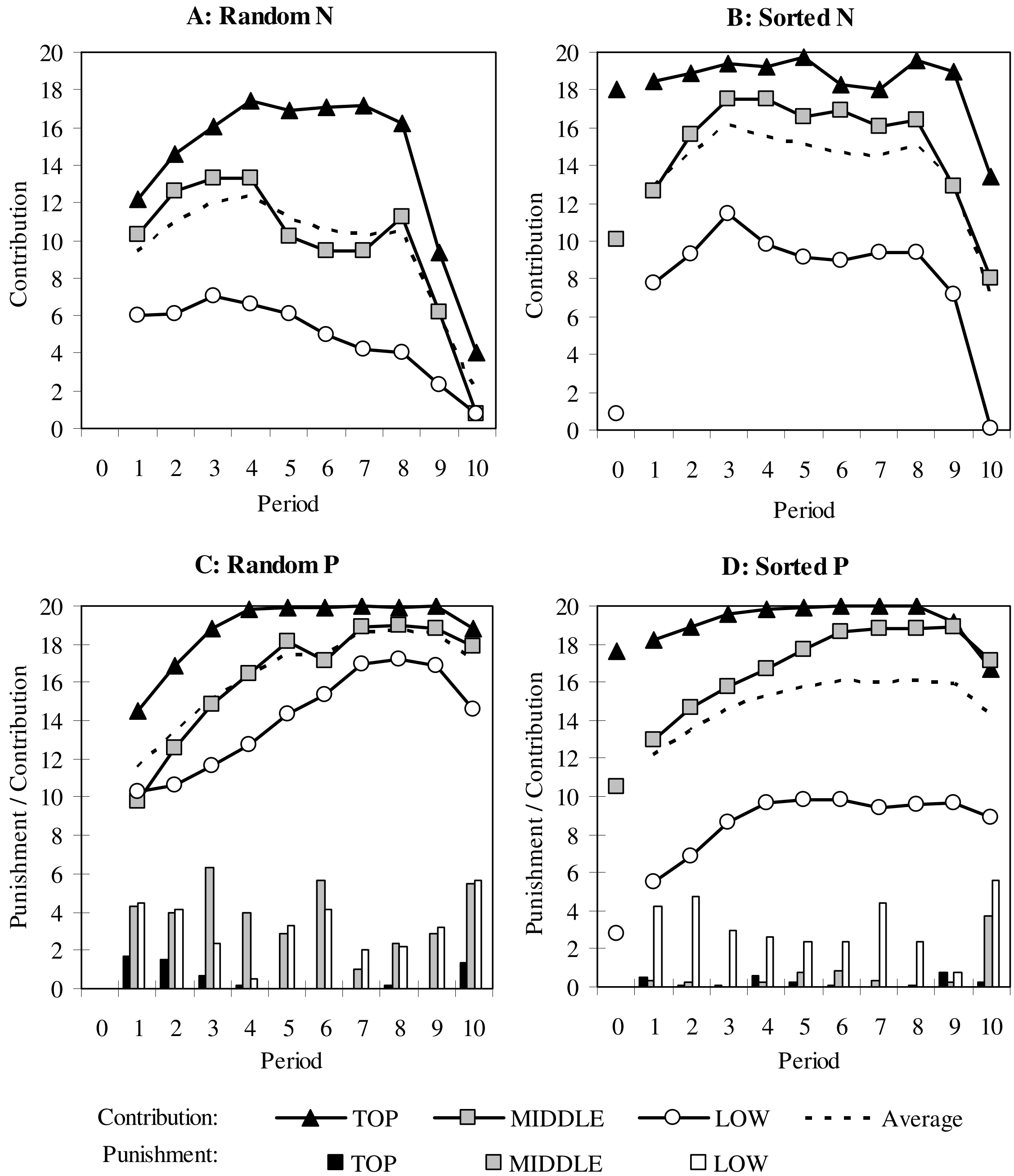

FIGURE 1. Social learning and cooperation in randomly composed groups and groups of like-minded subjects. Lines depict mean contribution levels. Bars denote mean income reduction due to punishment.

There is an endgame effect in all classes, but it is most pronounced among the LOW contributors. In the TOP cooperator groups we find some people who lower their contributions in the final period. Yet, the median contribution is still 20. Overall, we find – in contrast to the *Random N* experiments – that final period contributions differ highly significantly between classes (Kruskal-Wallis test, p=0.001, group averages in period 10 as independent observations).

We also find that MIDDLE contributor groups contributed substantially more than the middle third of groups in *Random N* (15.0 vs. 9.7 ECU on average). The difference is significant (Mann-Whitney test, p=0.022). A speculative explanation is that many people, who in principle are prepared to cooperate, are hesitant (because they fear free riders) and first want to test waters before they cooperate. Once they are playing together and learn that there are no strong free riders among them because they have been sorted out, MIDDLE cooperators quickly lose their hesitation and cooperate until the final rounds.

Since MIDDLE cooperator groups contributed on average the same as the middle third of groups in the first period of *Random N*, one may argue that the higher level of cooperation among MIDDLE groups in *Sorted N* is even stronger evidence for a 'like-minded' effect than the one observed among TOP cooperator groups. Through the sorting mechanism TOP cooperator groups already started out at a higher cooperation level than the top third of groups in the *Random N* experiments. Our results show that grouping them together allowed the like-minded TOP cooperators to maintain their high cooperation level. However, like-minded MIDDLE cooperator groups were even able to increase their contributions, probably because they expected no strong free riders among them. A similar observation holds for LOW contributor groups who started out at a similar level as the lowest third of groups in *Random N.* Apparently, knowing that they are among like-minded free riders allowed them to maintain a higher level of strategic cooperation than the lowest third of groups in *Random N* who did not have such clear-cut evidence that they are composed of free rider types.

Our next results concern the impact of punishment. We first look at the *Random P* experiment (panel C) and compare it with the *Random N* experiments (panel A). Consistent with previous experiments from finitely repeated public goods experiments with and without punishment we find that average contributions are substantially higher in the presence of the punishment option than in their absence. With punishment, average contributions amounted to 16.4 ECU; in the absence of punishment contributions were 9.5 ECU (p=0.000, Mann-Whitney test). Contributions were also much more homogeneous in the presence of punishment. The bars in panel C denote the average income reduction due to punishment in a particular period. We find that the highest cooperating groups punished only initially and in the final period. Groups in the other classes punished roughly equally and throughout all periods. In the top and middle third of groups punishment was almost exclusively targeted at free riders. In the lowest third of groups there was some punishment of free riders, in particular of strong ones, but also some non-negligible punishment of cooperators (see also Falk, Fehr and Fischbacher 2004).

Before we look at our Hypotheses 3 and 4 note that the contributions in the *Ranking* experiment (period 0) of the *Sorted P* in panel D were almost identical to those of the *Sorted N* experiment in panel B (p=0.819, Mann-Whitney test).

Hypothesis 3, which predicts that TOP cooperators' contributions are the same in *Sorted P* than in *Sorted N*, is confirmed (group average contributions are not significantly different; p=0.516, Mann-Whitney test). As predicted, we also find that TOP cooperators did not punish. They punished even less frequently (and less strongly) than the top third of groups in *Random P* (p=0.095, Mann-Whitney test). The presence of the punishment option did not affect TOP contributors because they did not need it to achieve cooperation.

It is also interesting to compare the highest cooperating groups in *Random P* (panel C) and TOP cooperators in *Sorted N*. The cooperation by the highest contributor class in *Random P* indicates the highest cooperation level that one can get in randomly composed groups. The cooperation level by TOP cooperators in *Sorted N* indicates the highest cooperation level that is achievable by like-minded cooperators. We find that TOP cooperators in *Sorted N*, who had no punishment option available, contributed the same as the most successful randomly composed groups who had a punishment option at their disposal. Since TOP cooperator groups in *Sorted N* also had no punishment costs, in terms of efficiency they did even better than the most successfully cooperating groups who could punish misbehavior.

Hypothesis 4, which predicts that (i) LOW cooperators contribute the same in *Sorted N* and *Sorted P* but (ii) do not punish, is partially confirmed. We find support for (i) but have to reject (ii). LOW contributors punished by far the most. Given the observation from the *Random N* experiments that LOW contributors apparently cooperate strategically, it is not too surprising that LOW contributors punished to induce further cooperation. Most punishment was targeted at the free riders, but, as in *Random P*, there was also some punishment of cooperators.

LOW contributors punished about the same as the least cooperative third of groups in *Random P*. Despite this, LOW contributors contributed less in *Sorted P* than the least cooperative third of groups in *Random P* (8.8 ECU vs. 14.1 ECU on average; p=0.053, Mann-Whitney test). The time trend of cooperation was also different. Punishment among like-minded LOW contributors in *Sorted P* only stabilized cooperation. Yet, it strongly increased contributions in the lowest third of groups in *Random P*. Like-minded free riders seem not to be too impressed by punishment inflicted on them by other like-minded free riders.

4. Concluding Remarks

The results of this paper are hard to reconcile with an error-hypothesis but are consistent with social learning by heterogeneous types. The reason is that an error hypothesis would not easily predict that group composition effects matter for cooperation behavior. Yet, this is exactly what we find. Since people are heterogeneous with respect to their attitudes to cooperation, our results suggest that the dynamics of cooperation as produced by social learning will depend very strongly on the extent to which group members are 'like-minded' (see also Ones and Putterman 2004, who aptly talk about an "ecology of collective action"). Our results also confirm that social norms of cooperation are quite easy to sustain in homogeneous groups of people who are aware that others share their attitudes. Like-minded cooperators do not need punishment to uphold cooperation. It is only in heterogeneous groups where punishment is helpful in sustaining cooperation.

We believe that our results are not only of theoretical interest but may also shed light on some management practices that emphasize team spirit. For instance, Ghemawat (1995) describes the group incentive schemes of a large US steel producer. To prevent free riding, management at this company prefers recruiting "farm boys" (p. 697) who share a similar set of values that is thought to hold free riding at bay. The morale is that successful teamwork not only requires the right mix of complementary abilities, but also "team players" who do not shirk their responsibilities even if they safely could.

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6. Appendix A: Data

Data of the experiments without the punishment option:

				Contribution				Profit
Treatment	Group	Rank	Class	Ranking treatment	Main treatment	Last period	St. dev. main treat.	main treatment
Sorted N	1	1	T	20.0	19.3	13.3	1.15	35.5
	2	2	T	20.0	19.3	13.3	1.15	35.5
	3	3	T	20.0	19.3	13.3	1.15	35.5
	4	4	T	16.7	14.4	6.7	4.21	31.5
	5	5	T	16.0	18.4	14.0	1.52	34.7
	6	6	T	15.7	19.7	20.0	0.23	35.8
	7	7	M	13.7	17.7	12.7	1.81	34.2
	8	8	M	12.0	18.3	6.7	1.52	34.6
	9	9	M	10.0	16.5	13.3	2.20	33.2
	10	10	M	10.0	10.1	8.7	5.07	28.1
	11	11	M	10.0	11.3	0.3	1.96	29.0
	12	12	M	5.0	16.3	6.7	3.90	33.0
	13	13	L	3.3	15.6	0.3	2.55	32.5
	14	14	L	1.3	6.2	0.0	6.09	24.9
	15	15	L	0.3	10.8	0.0	5.99	28.7
	16	16	L	0.0	2.3	0.0	3.27	21.9
	17	17	L	0.0	8.3	0.0	5.10	26.7
	18	18	L	0.0	6.2	0.0	6.42	25.0
Random N	19	1	T	8.3	16.0	0.3	2.48	32.8
	20	2	T		16.0	0.0	1.67	32.8
	21	3	T		14.4	5.3	5.14	31.5
	22	4	T		14.4	0.0	2.37	31.5
	23	5	T	4.7	13.1	14.7	4.09	30.5
	24	6	T		10.7	3.7	4.33	28.5
	25	7	M	5.0	10.3	0.0	2.36	28.2
	26	8	M	4.0	10.1	0.0	5.08	28.1
	27	9	M		10.1	2.3	5.03	28.1
	28	10	M		9.0	0.0	5.92	27.2
	29	11	M	6.3	8.9	1.7	3.37	27.1
	30	12	L	2.3	6.5	1.0	5.34	25.2
	31	13	L	5.3	5.6	3.3	6.45	24.5
	32	14	L		4.9	0.0	1.84	23.9
	33	15	L	4.3	4.0	0.0	1.14	23.2
	34	16	L	13.0	3.9	0.3	3.51	23.1
	35	17	L		3.8	0.0	4.86	23.0

Table A1: The table shows the independent group observations for the *Sorted N* and the *Random N* treatment. In the case of the *Sorted N* treatment, the observations are sorted according to the average contributions in the *Ranking* treatment. The observations of the *Random N* treatment are ranked according to the average contributions during the ten periods of the main treatment. Recall that the observations of the *Random N* treatments stem from two different treatments, the *Ranking-Unsorted N* and the *Simple N* treatment. The column *Ranking* treatment identifies the origin of the observation, i.e. groups from the *Ranking-Unsorted N* treatment have an entry while groups that played the *Simple N* treatment do not have an entry. The table also shows the average contribution in the last period and the mean standard deviation of the contributions in the main treatment. The last column shows the average profit that was earned in the main treatment.

Data of the experiments with the punishment option:

Treatment	Group	Rank	Class	Contribution				Punishment		Profit
				Ranking treatment	Main treatment	Last period	St. dev. main treat.	Average reduction	Frequency of use	Main treatment
Sorted P	36	1	T	20.0	20.0	20.0	0.00	0.0	0.00	36.0
	37	2	T	20.0	18.7	6.7	1.15	0.0	0.00	34.9
	38	3	T	20.0	18.0	6.7	2.31	0.6	0.07	33.6
	39	4	T	18.7	20.0	20.0	0.00	0.0	0.00	36.0
	40	5	T	16.3	19.7	20.0	0.10	0.0	0.00	35.8
	41	6	T	15.7	18.6	20.0	0.52	1.2	0.27	33.3
	42	7	T	15.3	19.4	20.0	0.39	0.2	0.07	35.3
	43	8	T	15.0	19.3	20.0	0.29	0.2	0.03	35.2
	44	9	M	12.3	17.5	20.0	0.83	0.0	0.00	34.0
	45	10	M	11.7	16.9	18.3	1.27	0.7	0.17	32.6
	46	11	M	11.3	17.7	20.0	0.41	0.3	0.10	33.7
	47	12	M	11.3	18.4	13.3	1.62	1.1	0.07	33.3
	48	13	M	10.0	16.6	13.3	1.94	1.5	0.10	31.3
	49	14	M	10.0	19.8	20.0	0.25	0.1	0.03	35.7
	50	15	M	9.3	16.2	19.0	1.18	0.2	0.07	32.7
	51	16	M	8.3	13.0	13.0	1.71	1.7	0.30	28.2
	52	17	L	6.3	8.8	11.0	2.10	1.9	0.50	24.5
	53	18	L	5.3	9.5	6.7	1.97	1.7	0.37	25.3
	54	19	L	4.3	5.8	6.0	3.32	2.4	0.37	21.5
	55	20	L	3.7	15.6	19.7	2.16	1.2	0.27	30.9
	56	21	L	2.0	10.8	13.0	4.69	8.8	0.87	16.9
	57	22	L	0.7	3.2	1.3	2.88	2.3	0.23	19.5
	58	23	L	0.0	1.4	0.0	2.48	3.5	0.43	17.2
	59	24	L	0.0	15.0	13.3	3.57	4.2	0.37	26.6
Random P	60	1	T		19.7	20.0	0.35	0.2	0.07	35.5
	61	2	T		19.5	18.0	0.81	0.2	0.07	35.4
	62	3	T	15.0	19.1	20.0	1.08	0.3	0.07	34.9
	63	4	T	15.0	18.5	20.0	0.96	1.1	0.20	33.4
	64	5	T		18.5	20.0	0.52	1.0	0.13	33.4
	65	6	T		17.9	15.0	1.20	0.5	0.03	33.6
	66	7	M	9.0	16.9	20.0	0.71	0.4	0.03	33.0
	67	8	M		16.6	17.7	1.50	2.6	0.30	29.8
	68	9	M	4.0	16.6	20.0	1.28	1.3	0.27	31.5
	69	10	M		16.3	13.7	4.66	5.9	0.57	25.2
	70	11	M	7.0	15.9	20.0	3.12	10.2	0.57	20.1
	71	12	M		15.8	16.0	4.58	2.9	0.27	28.7
	72	13	L	10.3	15.7	19.7	1.93	3.9	0.47	27.4
	73	14	L	8.7	15.7	20.0	1.26	0.0	0.00	32.6
	74	15	L		15.6	13.3	2.41	3.4	0.53	28.0
	75	16	L	12.3	14.4	3.7	3.30	3.6	0.43	26.7
	76	17	L		14.3	20.0	0.92	0.3	0.10	31.0
	77	18	L	9.3	8.7	11.0	3.70	8.0	0.37	16.3

Table A2: The table shows the independent group observations for the *Sorted P* and the *Random P* treatment. In the case of the *Sorted P* treatment, the observations are sorted according to the average contributions in the *Ranking* treatment. The observations of the *Random P* treatment are ranked according to the average contributions during the ten periods of the main treatment. Recall that the observations of the *Random P* treatments stem from two different treatments, the *Ranking-Unsorted P* and the *Simple P* treatment. The column *Ranking* treatment identifies the origin of the observation, i.e. groups from the *Ranking-Unsorted P* treatment have an entry while groups that played the *Simple P* treatment do not have an entry. The table also shows the average contribution in the last period and the mean standard deviation of the contributions in the main treatment. Two columns provide information about the use of the punishment option. The show the average reduction chosen and the frequency of punishment acts. The last column shows the average profit that was earned in the main treatment.

7. Appendix B: Experimental Instructions

Instructions of the *Ranking* experiment:

You are now taking part in a series of economic experiments, financed by several research promoting foundations. If you read the following instructions carefully you will be able to earn – according to your decisions – a considerable amount of money. On this account, it is very important that you read these instructions very closely.

The instructions we have distributed to you are solely for your private information. During the experiment conversation is strictly prohibited. If you have any questions, please ask us. Violation of this rule will lead to exclusion from the experiment and all payments. If you have questions, please raise your hand. We will answer your question personally.

During the experiment we will not speak of Francs but rather of Guilders. Your income will be calculated in Guilders. At the end of the experiment, the amount of guilders you have earned will be converted to Swiss Franks and paid out in cash at the following rate:

1 Guilder = 0.07 Swiss Francs.

All participants will be randomly divided into groups of three members. Apart from us, the experimenters, nobody knows the composition of the groups. Neither before, nor after the experiment you will learn which people are/were in your group. On the following pages we will describe the experiment in detail.

The Experiment in Detail

At the beginning of the experiment, every participant receives an endowment of 20 Guilders. Your task is to decide about the allocation of your endowment. You have to decide how much of the 20 guilders you want to contribute to a project and how much you want to keep for yourself.

Your income depends on how many Guilders you have contributed to the project and how many Guilders the other members of your group have contributed. Your income consists therefore of two components:

(1) The Guilders that you kept for yourself ("**Income from kept Guilders**")

(2) The "**Income from the project**". This is calculated according to the following formula:

Income from the project = 0.6 times the sum of the contributions of all group members

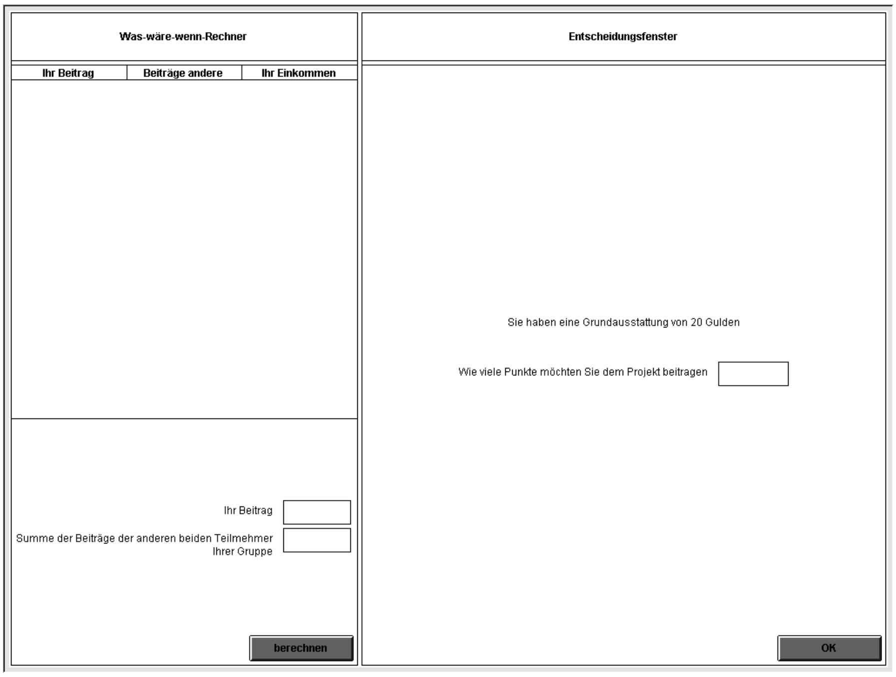
Your total income is therefore:

(20 – your contribution) + 0.6 * (sum of the contributions of all group members)

The income from the project is calculated identically for the other group members, i.e., all members of a group receive the same income from the project. If, for example, the sum of all contributions is equal to 50 Guilders then you and your other group members each receive an income of 0.6 * 50 = 30 Guilders from the project. If the members contributed a total of 8 Guilders then all group members receive 0.6 * 8 = 4.8 Guilders from the project.

If you contribute one Guilder of your endowment to the project of your group then the sum of all contributions rises by 1 Guilder and your income from the project rises by 0.6 * 1 = 0.6 Guilders. At the same time, the income of the other group members also rises by 0.6 Guilders. Therefore the total income of the group rises by 0.6 * 3 = 1.8 Guilders. The other group members profit from your contributions to the project, and, likewise, you profit from their contributions to the project. For each Guilder another member of your group contributes to the project you earn 0.6 * 1 = 0.6 Guilders.

You will see the following input-screen on your computer:



The screen consists of two panels:

1) The left panel shows the "**Income-Calculator**". Here you can calculate your income for different combinations of your contribution and the contribution of the other two group members. Indicate the values in the corresponding fields and press the button "calculate". The result will be displayed in the table in the upper part of the left panel. The values you enter in the Income-Calculator have no influence on the experiment and on your final payment.

2) The right panel is the "**Decision-Window**". Here you have to indicate your contribution to the project. If you have made up your mind about your contribution then enter the number in the corresponding field and confirm your entry by pressing the OK-button.

The experiment is conducted only once.

Do you have any further questions?

Instructions of the *Sorted P* Treatment

For the moment you will not receive information about the contributions of the other group members in the preceding experiment. We will provide this information at the end of the experiment. We have recorded the number of Guilders you have earned and we will pay them out to you together with the earnings of the second experiment. The exchange rate is still:

1 Guilder = 0.07 Swiss Francs

For the second experiment new groups will be formed. However, each group still consists of three participants.

The formation of the new groups occurs according to the following rule:

All participants of the experiment will be ranked according to their contribution to the project in the first experiment. The participant with the highest contribution receives rank 1. The participant with the second highest contribution has rank 2 and so on. If two or more participants chose the same contribution then the corresponding ranks are randomly assigned.

The formation of the new groups of three participants is such that the participants with rank 1 to 3, 4 to 6, 7 to 9 etc. form a group together.

In the first group there are now the three participants that chose the highest contributions in the first experiment. The second group consists of the participants with the 4th to 6th highest contributions. In the last group there are the three participants with the lowest contributions in the first experiment.

When the experiment starts you will be informed about the composition of your new group. You will be informed about how many Guilders your new group members have contributed to the project in the first experiment.

The second experiment will begin after you have seen the contributions of your new group members and have pressed the "continue" button in the corresponding screen.

The Second Experiment in Detail

Unlike the first experiment the second experiment is repeated 10 times. In every period you have to make the same decisions. Each period consists of two stages. In the first stage you have to make the same decision as in the first experiment. Again you receive an endowment of 20 Guilders and you have to decide how many of them you want to contribute to the project and how many Guilders you want to keep for yourself.

In the second stage you will be informed about the contributions of the other two members in your new group. You can then choose if and how much you want to reduce the income of the other two group members by assigning "negative points". In the following we give a detailed description of the experiment.

The First Stage

In the first stage you have to take the same decision as in the first experiment. You have to indicate how many of your endowment of 20 Guilders you want to contribute to the project. What is new is that you also have to indicate what you expect the other group members to do. You have to guess the average contribution of your other two group members. If you think, for example, that the two other group members will contribute 5 and 15 Guilders then you have to enter the average of 10 in the corresponding field on the screen. This second entry has no influence at all on your income in Guilders and no participant in the experiment will learn it.

The calculation of the income in the first stage follows the same rules as in experiment 1:

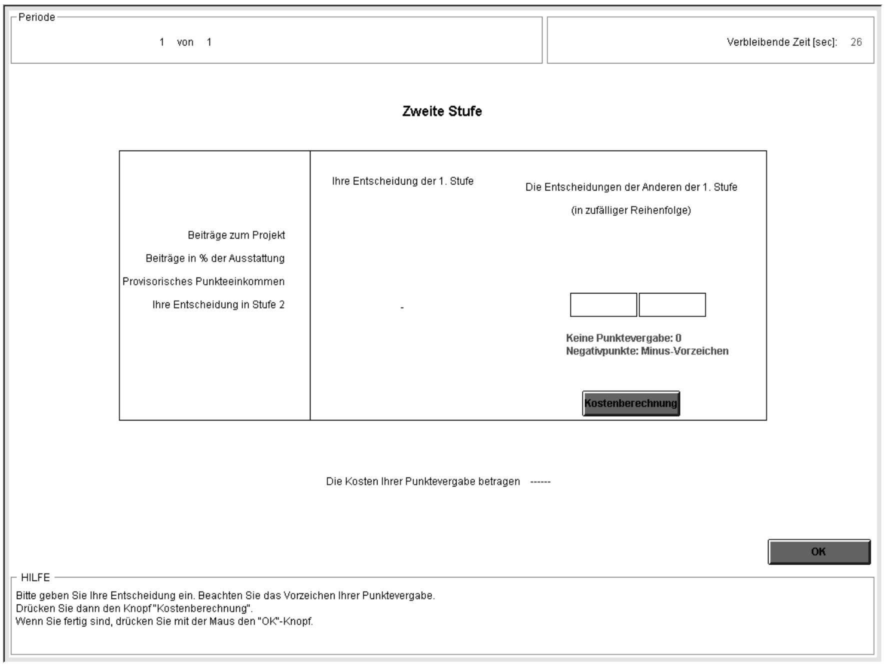
Income in Guilders in the first stage =
(20 – Your contribution) + 0.6 * (sum of the contributions of all group members)

The income of the first stage is, however, only a provisional income since it can be changed in the second stage.

The second stage

In the second stage you will learn the contributions of the other two members of your group. In addition to that you can reduce the income of every other group member by assigning **Negative-points**. The other members of your group also can reduce **your** income if they wish to do so. The Input-screen of the second stage shows how this works:

The input screen of the second stage:



On this screen you see the contributions of all group members. Your own contribution is always in the first column, followed by the contributions of the other two group members. The order of the other two group members is chosen at random in every period. Aside from the contribution, the table contains also information about the percentage of the endowment that was contributed and the provisional income. This income is provisional because it still can be reduced by the assignment of negative points.

You have to decide in each period and for each of the other two group members how many negative points you want to assign. You have to indicate a number in any case. If you do not want to change the income of a specific group member then you have to enter a 0. Please note that the negative points must be entered as a negative integer number. If you want, for example, assign 3 negative points to a group member then you should enter –3 in the corresponding field.

Costs of the negative points

The assignment of negative points has costs for you in Guilders. These costs depend on the number of assigned negative points. Each negative point that you assign costs you 1 Guilder, so the more negative points you assign the higher are your costs. The negative points you assign to the two other group members are added up. If you do not assign any negative points (i.e., you enter 0 in both fields) then you do not have to bear any costs. You can calculate the costs by pressing the button "Cost calculation" on the screen. As long as you have not pressed the OK-button you still can change your entries.

Effect of the negative points

If you assign 0 negative points to a member of your group then you do not change his income. For every negative point you assign to a group member the income of this group member is reduced by 3 Guilders. If you assign one negative point (i.e., enter –1 in the corresponding field) then you reduce the income of this group member by 3 Guilders. If you enter –2, i.e., you assign two negative points, then the income reduction is 6 Guilder.

How much the income of a group member is finally reduced depends on the total number of received negative points. If someone receives a total of 3 negative points then his or her income will be reduced by 9 Guilders. If someone receives a total of 4 negative points then the income reduction is 12. Your total income in Guilders is calculated by the following formula:

Calculation of the period income in Guilders

Income from stage 1 (1)
- 3 * (Sum of received negative points) (2)
- Cost of the assigned negative points
= Income in Guilders

Special case: If your income reduction due to received negative points is larger than your income from stage 1, i.e., (2)>(1), then the negative points do not count fully and you receive an income of 0. However, you must always bear the cost of the negative points you assigned. In this case your income in Guilders is

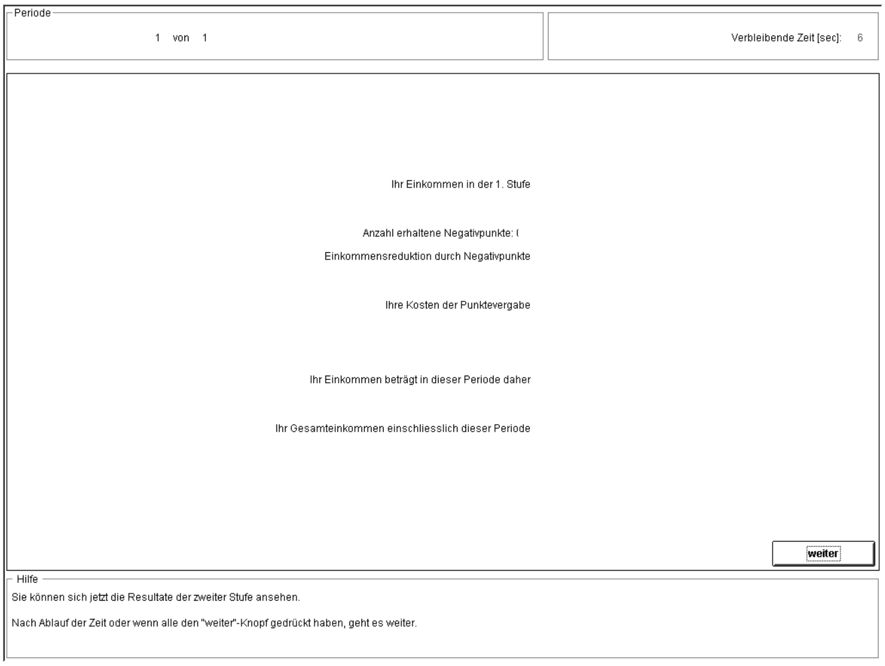
= 0 – cost of the assigned negative points

Please note that the income at the end of stage 2 can be negative. This is the case if the cost of the negative points you have assigned exceeds your income from stage 1 minus the reduction due to received negative points. However, you can take your decisions such that losses are ruled out for certain.

After all participants have assigned the negative points you are asked to guess the number of negative points that you will receive. If, for example, you believe that you will receive –7 points from one group member and –3 points from the other group member then enter –10 in the corresponding field.

Subsequently a screen appears that informs you about your income in a period.

The income screen at the end of the second stage:



That followed the experiment continues with the next period. There are 10 periods and you are grouped together with the same two participants during these 10 periods.

Do you have any questions?