Journal of Behavioral and Experimental Economics 90 (2021) 101595

Contents lists available at ScienceDirect

Journal of Behavioral and Experimental Economics

journal homepage: www.elsevier.com/locate/jbee

It pays to be nice: The benefits of cooperating in markets

Nina Serdarevic*, Eirik Strømland, Sigve Tjøtta

Department of Economics, University of Bergen, Fosswinckels gate 14, 5007 Bergen, Norway

ARTICLE INFO

Keywords:
Cooperation
Partner choice
Preference evolution

JEL Codes:
D02
C91
C92

ABSTRACT

We contribute to the experimental literature by examining the causal effect of partner choice opportunities on the earnings of different cooperative types. We first elicit cooperative types and then randomly assign subjects to a repeated prisoner's dilemma game, with either mutual partner choice or random matching. In each period, the individual who fails to attain a partner is excluded from the group. The results from three experiments show that mutual partner choice enables cooperators to outperform free riders; cooperators tend to earn more than free riders and are less frequently excluded. Our findings are robust with respect to varying group size and whether subjects are reminded about their entire partner and earnings history or only their recent history.

1. Introduction

Most markets are open to opportunistic behaviour. This allows free riders to benefit more than cooperative individuals by exploiting opportunities for short-term gain. However, many market interactions resemble repeated prisoner's dilemma situations where there is the opportunity to choose a partner. When partnerships form through mutual consent, people tend to prefer those who keep their end of the bargain and shun those who do not. Those who fail to cooperate are left without a partner and may need to redeem themselves in future partnerships. Those who cooperate reap the benefits of future trade.

According to Van Vugt, Roberts and Hardy (2007), for cooperative sentiment to evolve, there must be a long-term benefit for the cooperating individual. The competitive altruism hypothesis, established by Roberts (1998), explains that partner choice may be an important driving force of cooperation. If individuals can choose with whom to interact at every step in the prisoner's dilemma game, cooperative individuals are preferred as partners. Although cooperation may not necessarily have a direct short-term benefit to the cooperating individuals, it may, nevertheless, help in the long-term through the development of a good reputation and access to cooperative partnerships. Moreover, the size of the society may matter for whether cooperation pays off or not (Boyd & Richerson, 1988). Whereas it is easier to keep track of free riders in smaller societies, in larger ones, subjects will have a higher incentive to invest in developing a good reputation, as the competition to be chosen for a partnership is higher in larger societies than in smaller societies.[1] Finally, in order to know who to attract and who to avoid, behaviour must be observable by others.

The indirect evolutionary approach, pioneered by Güth and Yaari (1992), explains how cooperative preferences, or "rules of behaviour" as they are also called in this literature, can be evolutionarily stable in a repeated prisoner's dilemma situation involving partner choice. With partner choice, cooperative preferences are evolutionarily stable if subjects endowed with these preferences out-earn subjects with free-riding preferences. We engage with this literature by introducing a two-step design experiment in which, in the first step, we elicit cooperative preference types and, in the second step, examine how these types perform in terms of earnings in a repeated prisoner's dilemma game.

Subjects' behaviour in the second step of the experiment may

We would like to thank the Associate Editor Daniele Nosenzo and two anonymous reviewers for their valuable suggestions and comments that helped improve the paper. We also thank Mathias Ekström, Tom G. Meling, Amanda Kvarven, Linn Magritt Skotnes, Oda K. Sund, Sondre V. Michelsen, Erik Ø. Sørensen, Daniele Nosenzo, Gernot Doppelhofer, Rita Ginja, Thomas De Haan, Ingar Haaland, Martin Sefton, Ernesto Gavassa Perez and participants at the Department of Economics at the University of Bergen and DIGSSCORE lunch seminars for their helpful comments. We thank various conference and workshop participants for their valuable discussions: CeDEx at the Nottingham School of Economics, ESA 2017 in Vienna, NCBEE 2017 in Gothenburg, Tax Day 2017 at Max Planck Institute for Tax Law and Public Finance, the 2017 Public Choice Conference in New Orleans and the Frisch Centre. We are grateful to the Statoil Academia Agreement, Småforsk and PROLEG for financial support. Nina Serdarevic thanks DIGSCCORE for financial support. Declarations of interest: none.

* Corresponding author.
E-mail address: Nina.Serdarevic@uib.no (N. Serdarevic).

[1] Statistically, large groups are more likely to suffer from free riding than small groups (Kerr, 1989).

https://doi.org/10.1016/j.socec.2020.101595
Received 26 February 2020; Received in revised form 11 July 2020; Accepted 18 August 2020
Available online 21 August 2020

deviate from their elicited cooperative preferences in the first step as they adapt to the repeated partner choice environment (e.g. Alger & Weibull, 2013, 2019; Alger, Weibull & Lehmann, 2020). When information is incomplete, free riders have an incentive to mimic cooperators in order to attract cooperative partners – in finitely repeated games with incomplete information, there are equilibria supporting cooperation (Kreps & Wilson, 1982). However, it may be psychologically difficult to implement such strategies, as a free rider in each round would be torn between choosing the rational, long-term strategy and the temptation to go for the short-term monetary payoff. Indeed, one of the reasons why cooperative preferences are often found to be evolutionarily stable is their ability to solve such a commitment problem (e.g. Frank, 2011; Samuelson, 2001).

Scholars from Adam Smith (1759) and Thomas Schelling (1960) to Robert Frank (1988) suggest that a commitment to cooperation can end up as an unintentional material benefit. In Smith's case, a commitment to cooperation arises from self-command – the ability to restrain our self-love that has been fostered by the approval and disapproval of others. In Frank (1988, 2011), moral sentiments function as internal self-control devices in that they make the individual less likely to fall for the temptation of going for a short-term monetary gain. This could lead individuals to cooperate independently of incentives provided by the present situation. Such a commitment to cooperation in a partner choice environment may yield higher monetary benefits than simply mimicking cooperative intents. Our empirical approach in this paper allows us to examine whether subjects elicited to be cooperative in a non-strategic environment are , in the repeated game, able to outperform subjects elicited to be non-cooperative.

The literature on partner choice and self-sorting is large (e.g. Bernard, Fanning & Yuksel, 2018; for a survey see Kosfeld, 2004, and for a meta-study, see Guido, Robbett & Romaniuc, 2019). Previous experimental literature has shown that the opportunity to choose one's partner increases the overall efficiency (Brekke, Hauge, Lind & Nyborg, 2011; Hauge, Brekke, Nyborg & Lind, 2019 Page, Putterman & Unel, 2005). Similarly, subjects who behave cooperatively in a social dilemma game tend to earn more (Gächter & Thöni, 2005; Sylwester & Roberts, 2010) and end up in partnerships and groups with like-minded individuals (Kamei & Putterman, 2017; Page et al., 2005) more often than their lower contributing counterparts do.

Our contribution to the literature on partner choice lies in examining the causal effect of partner choice on the earnings of different cooperative preference types. In the existing literature on partner choice, one cannot separate cooperative preferences from a person's behaviour in terms of choice of partners, contributions and earnings, making the association between types and performance endogenous (e.g. Dreber, Rand, Fudenberg & Nowak, 2008; Kurzban & Houser, 2005). That is, subjects who differ in their preferences in a non-strategic environment can still earn the same amount in a strategic one. Our two-step design allows us to study how different elicited types perform in an environment with partner choice compared to random matching. Our first conjecture is that the effect of partner choice is positive for subjects classified in step 1 as cooperators and that it is negative for those classified as free riders. Moreover, based on the existing literature, we expect subjects classified as cooperators to outperform those classified as free riders within the partner choice environment.

To address this conjecture, we ran three experiments with the following two-step design. In step 1, we classified subjects as either cooperators or free riders using the strategy method in a one-shot, sequential, continuous prisoner's dilemma game (Fischbacher, Gächter & Fehr, 2001; Kurzban & Houser, 2005; Selten, 1967). In step 2, the subjects were either randomly assigned to a fixed group resembling a society in which there was an opportunity for partner choice in each period or to a fixed group where there is random matching in each period. In both the partner choice and the random matching treatments, the subjects played a 20-period, repeated, simultaneous prisoner's dilemma game with another subject from their group. Our two-step procedure allowed us to separate cooperative types from their observed behaviour in two different matching environments (the benefits of a similar two-step design are discussed by Gächter, Kölle & Quercia, 2017).

In the repeated prisoner's dilemma game of step 2, subjects interacted within fixed groups consisting of an odd number of subjects, meaning that one subject was, by design, excluded in every period and only pairs of two subjects could continue playing the prisoner's dilemma game. In the literature, partner matching has been implemented in various ways, ranging from ranking, voting, bidding and unilateral choice to mutual choice of partners (e.g. Ahn, Isaac & Salmon, 2009; Bohnet & Kübler, 2005; Kamei & Putterman, 2017; Page et al., 2005; Riedl & Ule, 2002; Strømland, Tjøtta & Torsvik, 2018).[2] Building on Strømland et al. (2018), we based partner choice on mutual consent. Whereas ranking and bidding entails that one can state a preference for a partner, our approach aimed to resemble market settings where people choose partners directly through mutual consent and not through ranking or bidding systems. We consider that this is a more realistic description of real market interactions where a relationship between two individuals is established through mutual consent.

Moreover, we used an asymmetric supply and demand for partners, as one subject was left without a partner in each period of both treatments. By linking partner choice to exclusion, we engage with the literature on the termination of partnerships and exclusion (Gaudeul, Crosetto & Riener, 2015; Huck, Lünser & Tyran, 2012; Wilson & Wu, 2017).[3] Honhon et al. (2020) show that the option to dissolve relationships acts as a sorting device, allowing those in cooperative relationships to maintain relationships, while defectors see their relationships terminated more frequently. By keeping the number of subjects without a partner constant in both the partner choice and random matching treatment, we cleanly test whether partner choice increases the probability of being without a partner for cooperators and free riders. While it may be difficult to disentangle whether subjects cooperate because they want to obtain a better partner or to avoid being excluded, in real-life market interactions these two are distinct, yet not mutually exclusive pathways that may facilitate cooperation.

Is partner choice more or less effective in promoting cooperation when groups become larger? Individuals in larger societies face more competition, which may create incentives for cooperation as a way to maintain a partnership and to better their reputation (Barclay, 2013; Roberts, 1998; Van Vugt et al., 2007; Wu, Balliet, Peperkoorn, Romano & Van Lange, 2020).[4] To explore whether the effect of partner choice

[2] Some studies allow subjects to move freely between groups based on the information about average group contributions (Brekke, Hauge, Lind & Nyborg., 2011; Ehrhart & Keser, 1999; Hauge et al., 2019). Another strand within this literature implements matching based on price auctions for the right to choose a partner (Coricelli, Fehr, & Fellner, 2004; Rigdon, McCabe, & Smith, 2007). Cooperation increases even when subjects are paired by experimenters based on the cooperative strategies they exhibit in repeated games (Gunnthorsdottir et al., 2007). Finally, some study the effect of opting in and out of games when the outside option is higher than the cost of potentially being exploited by staying put (Hauk & Nagel, 2001).

[3] For a discussion about costly punishment and social exclusion, see Barclay and Raihani (2016); Bayer (2016); Cinyabuguma, Page and Putterman (2005); Egas and Riedl (2008); Fehr and Gächter (2000); Feinberg, Willer, and Schultz (2014); Guala, (2012); Gürerk, Irlenbusch, and Rockenbach (2006); Leibbrandt, Ramalingam and Walker (2015); Masclet, Noussair, Villeval, and Tucker (2003); Nosenzo and Sefton (2014); Rigaud Maier, Martinsson, and Staffiero (2010); and Walker, (2004).

[4] Generally, the effects of group size on cooperation are mixed; large groups have been found to increase (e.g. Barclay, 2013; Carpenter, 2007), have no effect (e.g. Zelmer, 2003) and decrease (e.g. dos Santos and Wedekind, 2015; Van Lange et al., 2013) cooperation. This mixed evidence on group size and cooperation may also be driven by the payoff structure in a specific interaction (Bonacich, Shure, Kahan, & Meeker, 1976; Nosenzo, Quercia & Sefton, 2015).

varies between different competitive environments, we varied whether subjects interacted in fixed groups of five or nine. Assuming that larger groups may involve a lower chance for a person to be chosen as a partner, our second conjecture is that the effect of partner choice is higher in the groups of nine than in the groups of five.

We also compare the effect of partner choice in an environment in which subjects are reminded only about their current partner's id tag and earnings (e.g. Ahn, Isaac & Salmon, 2008; Charness & Yang, 2014) to an environment in which subjects get the entire history of earnings and their previous partners' id tags (e.g. Coricelli, Fehr & Fellner, 2004; Page et al., 2005). The evidence for competitive altruism indicates that cooperation increases when contribution behaviours are made public (Barclay & Willer, 2007; Sylwester & Roberts, 2010). Kamei and Putterman (2017) find that cooperation is higher when there is a more complete reputational history, such as reminding subjects about potential partners' fixed id tags and average previous contributions. Nevertheless, even when no information is provided about one's potential partners, some subjects succeed in establishing partnerships.

In our design, we restrict information dissemination to private experience only within a partnership. Thus, reputational information is revealed only to one's previous partners, not to the entire group and to potential partners. On the one hand, reminders about one's entire reputational history may create an incentive to invest in reputation by cooperating more. As Kamei and Putterman (2017) point out, the difficulty of altering one's reputation may be greatest when subjects carry with them their entire average past level of cooperation. On the other hand, whereas being reminded only about current behaviour allows one to create a new reputation over time, it may also increase the cost of searching for a new partner in cases where the current partnership terminates. This may make sustaining a current partnership through increased contributions more valuable.

Supporting our first conjecture and the existing literature on partner choice, we find that the effect of partner choice is positive for cooperators and negative for free riders. Comparing performance between types when partner choice is allowed, we show that cooperators outperform free riders; subjects who are elicited to be cooperators in the non-strategic environment earn more and are more likely to attain a partner than subjects elicited to be free riders. Regarding our second conjecture, we find higher earnings under partner choice than random matching only in the larger groups of nine. Although positive, we do not find that partner choice leads to statistically significant higher earnings in larger groups than in smaller ones. Finally, we do not find support for the conjecture that partner choice is more valuable when more reputational information is available. Rather, we find that subjects earn more when they are reminded of their current earnings and partner's id tag.

The rest of the paper is structured as follows: Section 2 describes the designs and procedures of our three experiments, Section 3 reports the results and Section 4 offers a discussion, while Section 5 concludes the paper.

2. Experimental design

We conducted three experiments with partner choice, varying group size and reminder of reputational history. In Group5History, subjects interacted in fixed groups of five and were reminded of their entire earnings and previous partners' id tags. In Goup9History and Group9Current, subjects interacted in groups of nine. However, in Group9Current, subjects were reminded only of their current earnings and partners' id tags, not their entire earnings and partners' id tags as in Group9History. We elaborate more on the features of the respective experiments in the subsequent sections. The experimental design in all three experiments consisted of two steps, as illustrated in Fig. 1.

Step 1: We obtained measures of cooperative types using the strategy method (Selten, 1967) in a one-shot, continuous prisoner's dilemma game.

Step 2*:* The participants were randomly assigned to a finitely repeated prisoner's dilemma game, which featured two possible matching procedures in each period—either partner choice (Choice) or random matching (Random).

Prior to steps 1 and 2, the subjects were informed about the payoff function and answered a set of control questions that aimed to ensure their understanding of the task (Fischbacher & Gächter, 2010; Fischbacher et al., 2001). In line with standard procedures (e.g. Fischbacher & Gächter, 2010), subjects were not allowed to proceed to step 2 until they had correctly answered all control questions. If they answered incorrectly, they were provided with the correct answers on the computer screen. The subjects were first informed about step 1, after which they made choices in relation to this step. We informed them that there would be a step 2 but did not provide further details until they had made their decisions in relation to step 1. During step 2, we did not inform the participants about the input or output of the strategy method. Thus, this design reduced strategic spill-over between the two steps and allowed for an exogenous treatment effect of the different types. In both steps, we used the following earnings function:

$$\pi_i = 10 - x_i + 0.7(x_i + x_j), \tag{1}$$

where x_i and x_j denote the subjects' own and their partner's contribution choices. The payoff in step 2 is conditional on having a partner, otherwise it is zero.[5] Hence, the outside option of not being in a partnership is zero.

2.1. Step 1: elicitation and classification of initial cooperative types

We used step 1 of the experiments to obtain the measures of cooperative preferences that are independent of the Choice and Random treatment. For the type elicitation procedure, we employed the strategy method (Fischbacher, Gächter & Quercia, 2012; Selten, 1967) and followed the seminal design of Fischbacher et al. (2001). This procedure consists of, first, an *unconditional contribution* choice and, thereafter, *conditional contribution* choices (See Thöni and Volk (2018) for studies using similar classification procedures). The subjects were informed that one of these two decisions would randomly be drawn to determine their actual payoff and that their partner would also be randomly drawn.

For the conditional contribution choice, the subjects filled out a conditional contribution table for each possible contribution choice made by their partner. Our classification procedure followed that of Kurzban and Houser (2005), who estimated their subjects' linear contribution profiles (LCPs) before classifying them into types.[6] We denote the 11 conditional contribution entries by y_i^k, $k = 0,1,2\ldots10$, and the subject i's OLS estimated contribution profile is given by: $y_i^k = \alpha_i + \beta_i y^k + u_i^k$ for $k = 0, 1, 2, \ldots10$, where u_i^k is the error term. We classified three types. Subjects were classified as **free riders** if their predicted contributions were below 25 percent of the endowment, that is, $\hat{y}_i^k < 2.5$ for $k = 0, 1, \ldots, 10$ where $\hat{y}_i^k$ is estimated contribution profile. Subjects were classified as **cooperators** consisting of unconditional cooperators who contribute most of the time and reciprocators who tend to match their partner's choices. Unconditional cooperators had LCPs that were always above 75% of the endowment, that is, if and only if, his or her predicted contributions were $\hat{y}_i^k \geq 7.5$ for $k = 0, 1, \ldots, 10$. Reciprocators were those whose LCPs were no further away from the 45-degree line than 25% of the endowment, that is, a subject i was classified as a reciprocator if and only if his or her predicted contributions were $-2.5 + k \leq \hat{y}_i^k \leq 2.5 + k$ for $k = 0, 1,\ldots,10$. The remaining subjects

[5] A translated copy of the instructions is provided in the Online Appendix 2.

[6] Similar classification methods are used by, for example, Burlando & Guala, 2005; Fichbacher & Gächter, 2010; and Fischbacher et al., 2001.

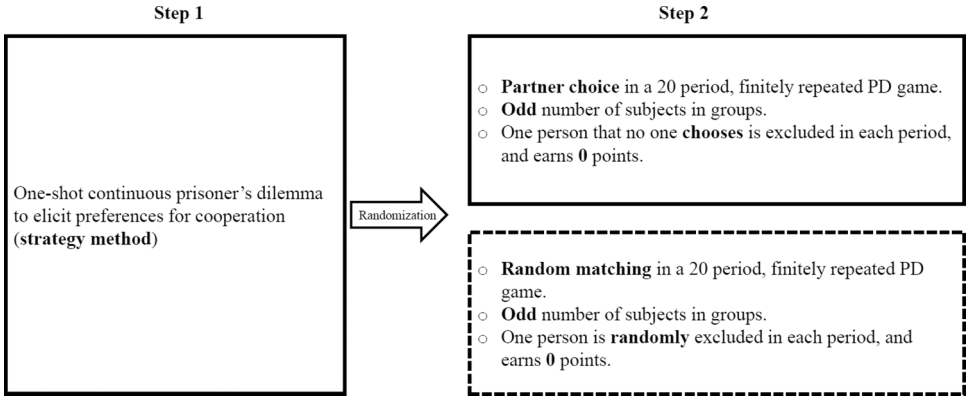

Fig. 1. A two-step experimental design.

were classified as **others**.[7] In the Online Appendix, we also offer distribution of types and regression analyses using a 0% and 50% bandwidth. Table 1 displays the distribution of the elicited types from step 1, in both the Choice and Random treatments, for the three experiments.

2.2. Step 2: the repeated prisoner's dilemma game

Experiment Group5History: *Groups of five subjects with a reminder of entire personal history*

In step 2, we used within-session randomisation, whereby the participants were randomly assigned to the **Choice** or the **Random** treatment. In both treatments, each participant in Group5History was placed in a fixed group of five subjects and informed that the group would remain fixed throughout the experiment. We used odd numbers of subjects in the fixed groups so that in each period, an individual who had not attained a partner would be excluded from the group for that period. Table A8 in the Appendix provides details concerning the number of participants, groups and sessions in the experiments.

In the **Random** treatment, the participants were informed that the group would be fixed throughout the experiment. They were assigned a numbered identity tag and were informed that this and other subjects' identity tags would be fixed. Although subjects were in groups of five, only pairs of two subjects within a group played the prisoner's dilemma game. In each period of the game, individuals received an endowment of 10 units and had to decide how much to contribute according to the payoff function [1]. The default contribution was set to zero so that subjects had to make an active choice about whether to contribute or not. Because contributions in the production stage were simultaneous, we set the decision-making time in both the Choice treatment and the Random treatment to 10 s to avoid subjects causing delays. In both treatments, the default contribution was set to zero if someone exceeded this time limit. The participants were informed that the person with whom they would be partnered in each period is determined by random assignment.

As there was always one extra participant in the group, one person was always randomly excluded from the production stage of a particular period. In case of exclusion, the payoff was zero for that period. Prior to each production stage, the participants who were not excluded were reminded of their partners' identity tags. After each production stage, a screen showed the participant's earnings and his or her partner's identity tag. In Group5History, the screen also reminded the subjects of all their preceding earnings and partners' identity tags. In the instructions, we referred to the other players as a "person" not a "partner".

The **Choice** treatment was identical to the Random treatment, except for the procedures given for partner assignment. Prior to each production stage, instead of random assignment, the participants were able to choose their preferred partner in the fixed group of subjects for that period. For the 10-second duration of the partner choice stage, the participants could freely enter a number on the computer screen. The default was set to their own identity tag. Notably, subjects could not use their phones, a pen or paper to keep track of their previous earnings or partners in any of the experiments.

The matching procedure in the Choice treatment was conducted in the following manner. Two participants were matched if there was mutual consent (both participants had entered each other's ID tags). The subject that failed to find a partner was excluded after reading the following message: "You will not participate in this production period. In this period, you earn 0 points". The subjects who managed to match mutually with a partner received the message: "You are person X. The person you chose also chose you. You are producing with person Y in this period".

If more than one subject failed to find a partner, then one was drawn to be excluded, while the others were randomly assigned an available partner. Those who did not match mutually with a partner received one of two possible messages. Either: "You are person X. The person you chose did not choose you. You will not participate in this production period. In this period, you earn 0 points" or "You are person X. The person you chose did not choose you. You are randomly paired with person Y, with whom you can produce in this period".

The Group5History experiment was conducted in November 2015. The experiment was programmed using the z-Tree software (Fischbacher, 2007). The participants were undergraduate students enroled at the University of Bergen, whom we recruited by email.[8] Each participant earned 100 NOK for showing up. On average, the sessions lasted 30 min and the participants earned 202 NOK (25.30 USD). This corresponds to hourly pay of 404 NOK

Table 1
Distribution of Types (%) between Treatments by Experiment.[14].

Types	Group5History		Group9History		Group9Current	
	Choice	Random	Choice	Random	Choice	Random
Cooperator	66.0	53.0	62.8	65.5	67.4	63.2
- Unconditional cooperator	11.0	12.0	11.7	7.6	9.7	7.6
- Reciprocator	55.0	41.0	51.1	57.9	57.6	55.6
Free rider	6.0	10.0	6.7	7.6	4.9	11.8
Other	28.0	37.0	30.6	26.9	27.8	25.0
Total number of subjects	100	100	180	171	144	144

[7] The estimated linear contribution profiles allowed for predicted contributions outside the feasible interval [0,10]. However, only 6.09% (134/2200) of the predicted contributions in Group5History, 6.00% (232/3861) in Group9History and 6.47% (205/3168) in Group9Current lie outside this range.

[8] We used the recruitment platform Expmotor to send out invitations, which was provided by Erik Ø. Sørensen from The Norwegian School of Economics.

[14] Table 4.2 in the Online Appendix shows the distribution of types by bandwidth and experiment, without distinguishing between treatments.

(48.6 USD) and is well above the average hourly pay for an undergraduate student in Norway.

Experiments Group9History and Group9Current: *Groups of nine subjects, varying with respect to personal history reminder*

The Group9History experiment was identical to the Group5History experiment, except that the number of subjects in the fixed group was increased from five to nine subjects. In this manner, we examined the effects of partner choice in larger groups in comparison to smaller groups, while maintaining a constant feedback channel. The subjects in the Random treatment were expected to receive the same partner every fifth period in the smaller groups and every ninth period in the larger groups of the follow-up experiment, making the matching environment of the follow-up experiment more challenging, and potentially more competitive.[9]

Finally, the Group9Current experiment corresponded to the Group9History experiment with respect to group size, except that subjects in Group9Current were reminded only of their personal information from the current period and not of their entire personal history as in the Group9History experiment. In the Group9History experiment, each subject's screen reminded them of the entire history of his or her contributions and payoffs, as well as of their previous partners' identity tags. Only reminding subjects of their current contribution, payoff and partner's identity tag could increase the search cost and make it more difficult to keep track of potential partners' behaviours, thereby creating incentives to cooperate.

The Group9Current experiment was conducted in March 2017 and Group9History was conducted in March 2019. In both experiments, we used the recruitment platform Hamburg Organizational Online Tool (HROOT; Bock, Baetge & Nicklisch, 2014), which was provided by DIGSSCORE at the University of Bergen. Each participant earned 100 NOK for showing up to the experiments. On average, the sessions lasted 30–45 min. In Group9Current, the participants earned 192 NOK (25 USD). In Group9History, participants earned 223.8 NOK (25.47 USD) on average. Despite varying the platform from which we sent out the invitations, the participant pool consisted of undergraduate students from the University in Bergen in all three experiments.[10]

3. Results

3.1. The benefits of being a cooperator

In line with the previous literature on partner choice, we find that the ability to choose one's partner increases earnings compared to random matching (e.g. Brekke et al., 2011; Hauge et al., 2019; Page et al., 2005). The overall estimated effect on earnings varied from 6.2 percentage points ($p = 0.151$) in Group5History to 10.20 percentage points ($p < 0.01$) and 19.0 percentage points ($p < 0.01$) in Group9History and Group9Current, respectively. For the treatment effects on overall earnings in each experiment, see the corresponding first rows of Table 2 below.

Our main research question concerned examining the causal effect of partner choice on the earnings of different cooperative types. That is, whether subjects who were elicited to have a distinct preference for cooperation in step 1 earned more in the repeated game of step 2 than subjects elicited to have a preference for free riding. We conjectured that the effect of partner choice would be positive for cooperators and negative for free riders. In line with this conjecture, we find that:

Result 1: *Cooperators earn more in the Choice treatment than in the Random treatment. Free riders earn less in the Choice treatment than in the Random treatment. Whereas cooperators out-earn free riders under partner choice, this pattern is reversed under random matching.*

Table 2 shows that the treatment effect on earnings for cooperators is positive in all three experiments, ranging from 11.9 percentage points ($p < 0.05$) in Group9History to 20.6 percentage points ($p < 0.05$) and 29.1 percentage points ($p < 0.01$) in Group5History and Group9Current.[11] On the other hand, free riders in the Choice treatment decrease their earnings in all three experiments compared to the Random treatment. In particular, the negative effects range from 72.0 percentage points ($p < 0.05$) in Group5History to 14.4 percentage points ($p = 0.319$) in Group9Current. For regression estimates, see Columns 1 and 2 in Tables A1, A3 and A5 in the Appendices.

Comparing the earnings of cooperators and free riders within the Choice treatment, we show that cooperators outperform free riders. In the Random treatment, this result is reversed. For instance, in the Choice treatment of Group5History, cooperators' earnings are 47.9 percentage points ($p < 0.10$) higher than those of free riders. In the Random treatment, cooperators earn 44.6 percentage points ($p < 0.01$) less than free riders. Together, this offers support that cooperative preferences are evolutionarily stable in the Choice treatment, whereas free riding is stable in the Random treatment.

To test whether subjects of the same type are more likely to interact in the Choice treatment than in the Random treatment, we constructed an assortative matching measure. It takes the value 1 if subjects meet the same types and 0 if they do not. We excluded from the analyses subjects who were in groups in which there was no potential partner of their own type, as group composition was random.

We observe positive overall assortative matching in the Choice treatment compared to the Random treatment, across the three experiments. In the Group5History experiment, assortativity is 22.0 percentage points ($p < 0.01$) higher in the Choice treatment than in the Random treatment. In the two experiments with larger groups of nine subjects, the assortativity is 11.3 percentage points ($p < 0.10$) and 1.2 percentage points ($p = 0.812$) higher when partner choice is possible than when matching is random. See Columns 1 and 2 in Tables A2, A4 and A6 in the Appendix for regression estimates.

Recall that subjects in the Choice treatment could meet a partner through either mutual matching with their chosen partner or by being randomly matched with an available subject within their group. The overall frequency of mutually matching with one's partner pooled across the three experiments increased from 5.4% in period 1 to 71.7% in period 20 in the Choice treatment. At the type level, we found that

[9] In the Random treatment, for a fixed group of five subjects, the probability of having the same partner from the previous period is $1/5 = 0.20$. Over 20 periods of play, the expected number of partnerships between the two same subjects is 4. For a fixed group of nine subjects, the probability of having the same partner from the previous period is $1/9 = 0.11$. During 20 periods of play, the expected number of partnerships between the same two subjects is 2.22, suggesting a more difficult matching environment with group sizes of nine subjects.

[10] To avoid inviting participants who had previously taken part in the Group9Current experiment, we used HROOT to screen and exclude the subjects who had signed up and participated in the Group9Current experiment. Unfortunately, we were unable to exclude the subjects who participated in the Group5History experiment when sending out invitations for the Group9Current experiment because we used a different recruitment platform and the participant emails from Group5History were unavailable. However, given that three years had passed between the first and last experiment, we believe that it is unlikely that the same subjects participated in the three experiments, as undergraduate programs in Norway last for three years.

[11] In the literature, there is not always a sharp distinction between cooperators and reciprocators. Sometimes, unconditional cooperators are not present in the data, as in Fischbacher et al. (2001). Other times, these subjects make up a broader group of cooperative individuals (e.g. Burlando & Guala, 2005). For ease of presentation, when discussing our main results, we do not distinguish between unconditional cooperators and reciprocators. However, in the Online Appendix, we do provide separate estimated effects of partner choice on earnings, exclusion and contribution for unconditional cooperators and reciprocators. Here we also provide the estimated effects for the different bandwidths used to classify the cooperative types. Though results differ in magnitude, the treatment effects are in the expected direction.

Table 2
Earning Levels and Treatment Effects Overall, by Type and Experiment.

Group5History	Random	Choice	Treatment effect	p-value
Overall Earnings	56.5	62.6	6.2	0.151
Cooperators	50.4	71.0	20.6**	0.012
-Unconditional cooperators	37.9	106.9	68.9***	0.008
-Reciprocators	54.0	63.8	9.8	0.387
Free riders	95.0	23.05	−72.0**	0.011
Others	54.7	51.4	−3.3	0.818
Group9History	**Random**	**Choice**	**Treatment effect**	**p-value**
Overall Earnings	59.5	69.7	10.2***	0.003
Cooperators	55.5	67.3	11.9**	0.014
-Unconditional cooperators	34.7	44.7	10.1	0.529
-Reciprocators	58.2	72.5	14.3**	0.015
Free riders	76.0	53.5	−22.5	0.365
Others	64.7	78.1	13.4	0.108
Group9Current	**Random**	**Choice**	**Treatment effect**	**p-value**
Overall Earnings	53.3	72.3	19.0***	0.000
Cooperators	46.5	75.5	29.1***	0.000
-Unconditional cooperators	48.5	53.8	5.3	0.837
-Reciprocators	46.2	79.2	33.0***	0.000
Free riders	88.8	74.4	−14.4	0.319
Others	53.7	64.0	10.3	0.332

Notes: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$. Estimates are based on an ordinary least squares regression with no controls. Standard errors are clustered at the group level. Earnings are normalised so that 0 is the minimum average and 100 is the maximum average of earnings. On the individual level, earnings could be below 0 and above 100.

cooperators mutually match more often with their preferred partner than free riders, irrespective of the partner's type, as depicted in Fig. 2. As a corollary result, Tables 1.7, 2.7 and 3.7 in the Online Appendix show that matching with one's preferred partner in the Choice treatment is associated with higher earnings than being matched with a subject in one's group who also failed to mutually match with someone.

3.2. Group size and reminder of personal history

Assuming that the competition to be chosen as a partner increases in larger societies and that signalling one's cooperativeness becomes more important, as suggested by Wu et al. (2020) and Barclay (2013), we conjectured that the effect of partner choice would be higher in larger groups than in smaller ones. The effect of partner choice on earnings over the 20 periods is depicted in Fig. 3. Increasing the group size from five to nine and keeping the reminder of the reputational history fixed increases the treatment effect from 6.2 percentage points in Group5-History to 10.2 percentage points in Group9History, but the difference is not statistically significant ($p = 0.444$). See Table 4.1 in the Online Appendix for regression estimates.

Result 2: *The effect of partner choice on overall earnings increases with group size, but the difference in the effect of partner choice between smaller and larger groups is not statistically significant.*

In terms of reputational history, we conjectured, based on the existing literature, that more complete information in Group9History may make investment in a good reputation more valuable as others can keep track of one's behaviour more easily. In contrast to this, Table 4.1 in the Online Appendix shows that subjects earned 8.8 percentage points ($p<0.10$) more in Group9Current than in Group9History. Compared to Kamei and Putterman (2017), we show that being reminded of less reputational information when choosing a partner increases earnings more than being reminded of more complete information. One explanation focuses on increased search costs; in Group9Current, subjects may cooperate more as a way to avoid being left without a partner and having to take the risk of choosing a bad partner when they have less information to base their choice on – provided they recall less accurate information about previous partners (Miller, 1956). In environments in which information about past decisions is less clearly provided, partner choice may resemble a mechanism similar to Güth and Kliemt's (1998) notion of a "technology" for detecting and keeping track of type-related cues. We summarise our finding in the following:

Result 3: *Partner choice increases earnings more when subjects are reminded only about their current earnings and partners' ID tag compared to being reminded about the entire history.*

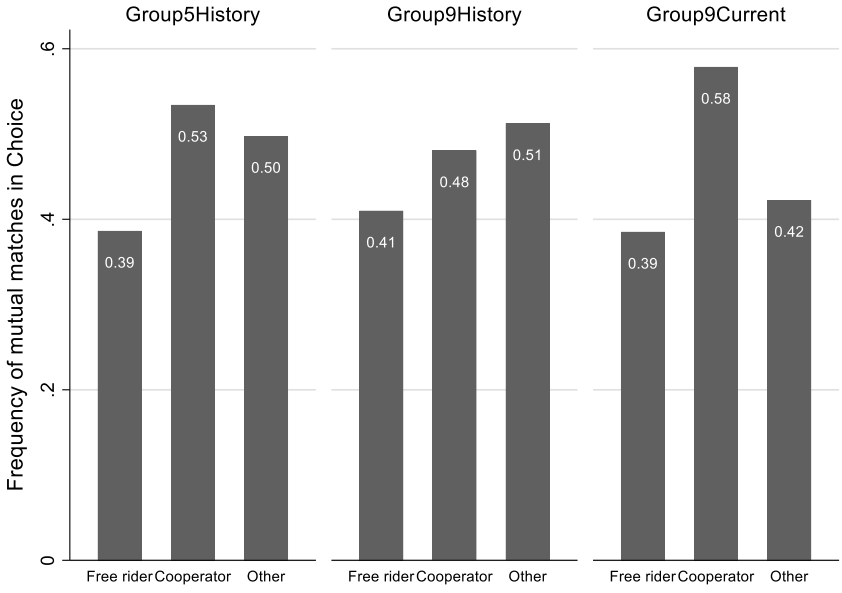

Fig. 2. Frequency of mutual matching in the Choice treatment, by type and experiment.

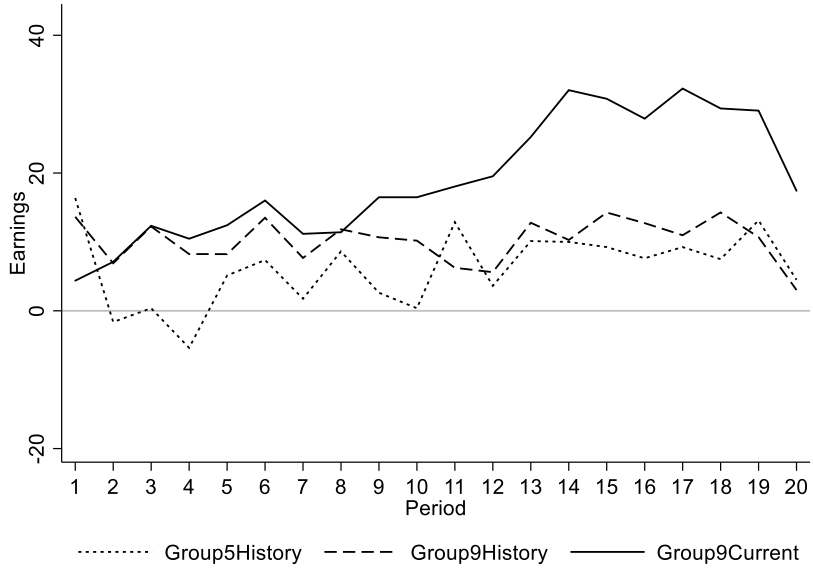

Fig. 3. Treatment effect on earnings, over period and by experiment.

4. Discussion

We conclude our analysis by discussing the role of partner choice with respect to group composition, stability of partnerships and observed contributions.

4.1. Cooperators benefit from partner choice even in a society with no free riders

We have already noted that the estimated overall effect of partner choice is positive in all three experiments. Similarly, partner choice allows for positive assortative matching, enabling cooperators to match with other cooperators and thereby avoid free riders. However, does partner choice lead to efficiency gains even when there are no free riders to avoid in a society? To explore this question, we exploited the fact that the subjects were randomly assigned into either the Choice or the Random treatment within each session. This allowed us to take advantage of the exogenous variation in the group composition in our experiments and to test how other types' earnings were affected by having free riders in a group, compared to being in a group with no free riders.

We found that random assignment of free riders to a group lowered the earnings of all other types across all experiments, as shown in Table A7 in the Appendix. However, in line with the assumption that partner choice may help subjects better keep track of their partners' cooperative types, we showed that the Choice treatment counteracts the negative effects on earnings of having free riders in the group. Notably, partner choice also had a positive effect on the earnings of cooperators and other types, even when there were no free riders in the group.

4.2. Cooperators tend to keep their partners

Because one subject was, by design, left without a partner in each period, the exclusion process was an indirect consequence of partner choice—subjects who failed to attain a partner risked the possibility of being left without a partner and earning 0 units. In general, we found that partner choice decreased the estimated probability of being left without a partner for cooperators compared to random matching, whereas it increased for free riders. This result is in line with the existing literature on endogenous matching, which shows that low contributors tend to be avoided compared to high contributors (e.g. Feinberg, Willer & Schultz, 2014; Page et al., 2005; Ule, 2005). More specifically, the difference in treatment effects between cooperators and free riders is 14.4 percentage points ($p<0.05$) and 7.1 percentage points ($p<0.05$) in Group5History and Group9Current, respectively. In the Group9History experiment, we found no statistically significant difference in the effect of partner choice on the exclusion probability of cooperators compared to free riders. Columns 5 and 6 in Tables A1, A3 and A5 in the Appendix provide the regression estimates.[12]

Cooperators were better not only at attaining partners but also at keeping their existing partners. Regardless of the partner's type, the estimated probability of keeping one's partner was significantly increased in the Choice treatment over the course of 20 periods in comparison to the Random treatment for each of the three experiments. This result is in line with Strømland et al. (2018) and Brown, Falk and Fehr (2004). In addition, at the type level, we found that cooperators in the Choice treatment were 40.2 ($p<0.01$) and 45.9 ($p<0.01$) percentage points more likely to keep their previous partner in the Group5History and Group9History experiment, respectively. In the Choice treatment of the Group9Current experiment, cooperators were 55.7 ($p<0.01$) more likely to keep their previous partner than in the Random treatment. The treatment effect was 26.6 percentage points ($p<0.05$) higher for cooperators than for free riders in the Group9Current experiment. See Columns 7 and 8, Tables A1, A3 and A5 in the Appendix for regression estimates.

4.3. Evidence of mimicking?

As we did not inform subjects in our experiments about individual types from step 1, non-cooperators could find it in their interest to "mimic" cooperators in step 2. Increased first-period contributions in the Choice treatment in comparison to the Random treatment could inform us about whether subjects actually are mimicking to signal their attractiveness as potential partners. Results show that the free riders did not increase their average first-period contributions in the Choice treatments of any of the three experiments. See Column 4, in Tables 1.1, 2.1 and 3.1 in the Online Appendix for regression estimates.

Over all periods, both free riders and cooperators in the repeated

[12] For regression estimates considering the effect of partner choice on the probability of exclusion for unconditional cooperators and reciprocators separately, see Tables 1.6, 2.6 and 3.6 in the Online Appendix. Here we also provide the estimated effects for the different bandwidths used to classify the cooperative types.

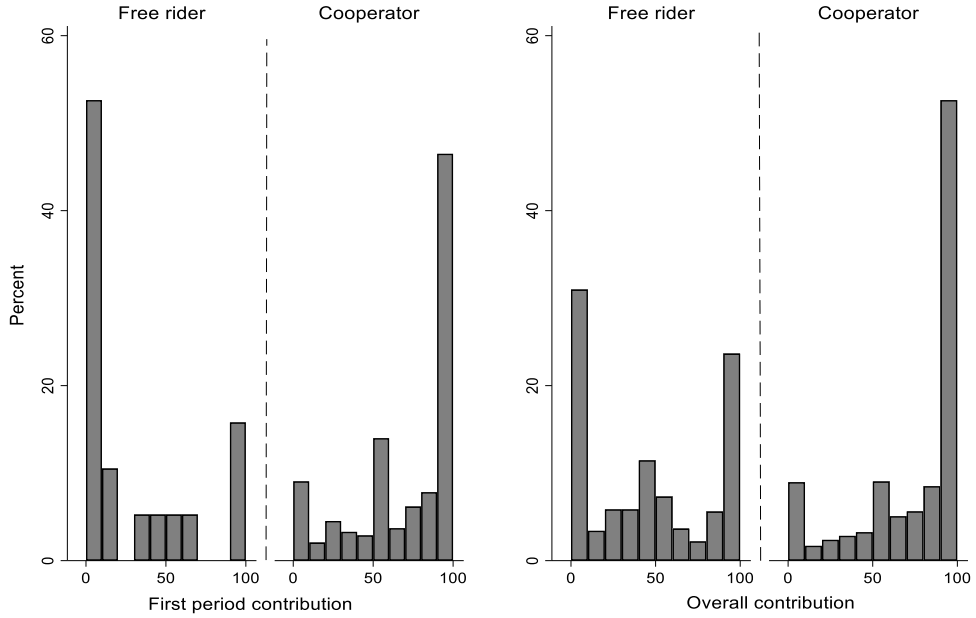

Fig. 4. Contributions (%) in the Choice treatments in the first period and overall, by type. Pooled data across the three experiments. Fig. A1 in the Appendix illustrates first period and overall contributions in the Random treatments.

game contributed more in the Choice treatment than in the Random treatment in the Group9History and Group9Current experiments. However, we cannot distinguish whether this was due to a change in behaviour of free riders or whether it was because of a selection of more cooperative free riders into partnerships. Still, seen in light of result 1 (free riders earn less in the Choice treatment than in the Random), and assuming no selection bias, an increase in contribution does not seem to help free riders increase their average earnings.[13] Fig. 4 offers a more thorough inspection of the distribution of contributions in the Choice treatment. We pooled data on contributions across the three experiments, and in general, there seems to be little overlap between free riders' and cooperators' contributions in the step 2 of the experiments. This may suggest that subjects with distinct elicited cooperative preferences have, on average, distinguishable contribution levels in the Choice environment, both in the first period and in the repeated game.

5. Concluding remarks

Many market situations are open to opportunistic behaviour, where individuals have an incentive to cheat each other. In this paper, we draw on the observation that an integral part of market interactions is the ability to voluntarily choose a partner. We conjectured – based on the competitive altruism hypothesis by Roberts (1998) and the indirect evolutionary approach literature (e.g. Güth & Kliemt, 1998; Güth & Yaari, 1992) – that competition for partners provides individuals endowed with cooperative preferences with the means to outperform free riders, as being avoided in subsequent interactions imposes costs upon non-cooperating subjects. We additionally conjectured that in larger groups resembling larger societies, there would be more competition for partners, increasing the effect of partner choice. Finally, we speculated whether a more complete set of reputational information would make investment in a good reputation more valuable than in situations where less information is provided.

To investigate these conjectures, we conducted three laboratory experiments. All experiments consisted of a two-step design, but varied in terms of group size and whether subjects were reminded about their entire history or just their recent earning and partner history. In all three experiments, we found support for the conjecture that cooperators benefit from partner choice opportunities, while free riders obtain lower earnings. Related to the second conjecture, we found that the effect of partner choice on overall earnings increases with group size. Still, the difference in the effect of partner choice between smaller and larger groups is not statistically significant. We also did not find support for the conjecture that partner choice is more efficient when more information is available about one's partners' previous actions.

Our results are important from a methodological point of view; to identify whether indeed cooperators earn more than free riders when partner choice opportunities exist, as is suggested in the existing experimental literature, it becomes important to credibly measure how different preference types perform across a variety of institutional arrangements. We propose a clean identification of types that we subsequently used to analyse subjects' choices in terms of partners, their earnings and their probability to be left without a partner.

Similarly, from an evolutionary perspective, it is essential to understand the benefit of cooperative preferences, as this may offer insights to their formation. If we view preference formation as an endogenous process, it becomes key to identify the circumstances under which they are evolutionarily stable. Randomly assigning preference types to two matching environments allowed us to study how these different environments exogenously affected the fitness of cooperative and free riding types. Supposing that the elicited cooperative preferences in the first step of the experiments represent true preferences, our results show that in a random matching environment, free riders earn more than cooperators, leading to the conclusion that it does not necessarily pay to be committed to cooperation. Conversely, being a cooperator pays off when there are opportunities for mutual partner choice.

[13] See Tables 1.2, 2.2 and 3.2 in the Online Appendix for the estimated effect of partner choice on contributions for unconditional cooperators and reciprocators separately. Here we also provide the estimated effects for the different bandwidths used to classify the cooperative types.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.socec.2020.101595.

Appendix A. Results

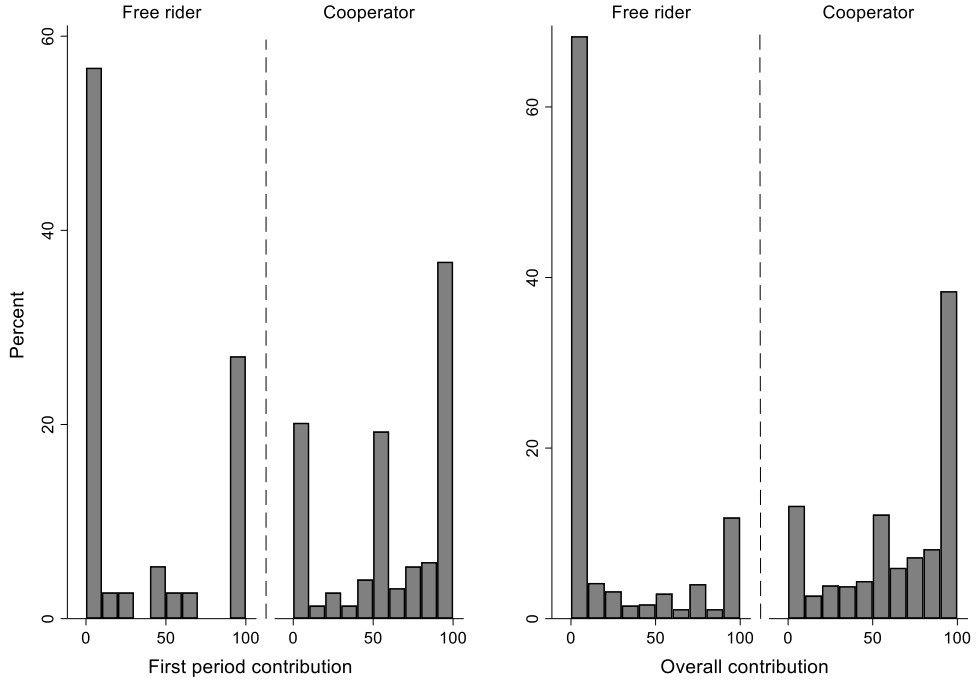

Fig. A1. Contributions (%) in the Random treatments in the first period and overall, by type. Pooled data across experiments.

Table A1
Treatment effects on Earnings Contributions and Exclusion, by Type in Group5History.

	(1) Earnings	(2) Earnings	(3) Contribution	(4) Contribution	(5) Exclusion	(6) Exclusion	(7) Keep	(8) Keep
Choice	−71.953**	20.605**	0.127	0.410	0.122***	−0.022	0.219	0.402***
	(26.819)	(7.855)	(0.973)	(0.502)	(0.041)	(0.014)	(0.135)	(0.046)
Freerider	*Ref.*	44.634***	*Ref.*	−3.937***	*Ref.*	−0.056**	*Ref.*	0.043
		(12.490)		(0.683)		(0.027)		(0.026)
Cooperator	−44.634***	*Ref.*	3.937***	*Ref.*	0.056**	*Ref.*	−0.043	*Ref.*
	(12.490)		(0.683)		(0.027)		(0.026)	
Other	−40.253***	4.381	2.829***	−1.108**	0.069**	0.013	−0.037	0.006
	(14.736)	(8.181)	(0.824)	(0.456)	(0.031)	(0.018)	(0.027)	(0.022)
Choice × Freerider	*Ref.*	−92.558***	*Ref.*	−0.283	*Ref.*	0.144***	*Ref.*	−0.184
		(29.333)		(1.006)		(0.048)		(0.137)
Choice × Cooperator	92.558***	*Ref.*	0.283	*Ref.*	−0.144***	*Ref.*	0.184	*Ref.*
	(29.333)		(1.006)		(0.048)		(0.137)	
Choice × Other	68.641**	−23.918	−0.044	−0.326	−0.099*	0.044	0.141	−0.043
	(30.103)	(19.712)	(1.120)	(0.641)	(0.050)	(0.043)	(0.111)	(0.061)
Constant	95.000***	50.366***	2.532***	6.469***	0.145***	0.201***	0.239***	0.196***
	(12.791)	(4.503)	(0.755)	(0.375)	(0.024)	(0.007)	(0.030)	(0.019)
N	4000	4000	3200	3200	4000	4000	3040	3040
Reference group	F	C	F	C	F	C	F	C
adj. R^2	0.005	0.005	0.095	0.095	0.003	0.003	0.151	0.151

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Coefficients from OLS regressions with no controls. Cluster-robust standard errors in parentheses (clustered at the group level). Reference category in Columns (1), (3), (5) and (7): Free rider. Columns (2), (4), (6) and (8): Cooperators. In these regressions, we do not distinguish between unconditional cooperators and reciprocators.

Table A2
Treatment effects on Overall Assortativity, Contributions and Earnings in Group5History.

	(1) Assortativity	(2) Assortativity	(3) Contribution	(4) Contribution	(5) Earnings	(6) Earnings
Choice	0.220***	0.133	0.617	0.264	6.175	2.644
	(0.078)	(0.084)	(0.422)	(0.456)	(4.219)	(4.557)
Period		−0.006**		−0.027		−0.274
		(0.003)		(0.025)		(0.255)
Choice × Period		0.008*		0.034		0.336
		(0.004)		(0.034)		(0.335)
Constant	0.445***	0.512***	5.645***	5.933***	56.450***	59.330***
	(0.037)	(0.048)	(0.300)	(0.339)	(2.997)	(3.389)
N	2666	2666	3200	3200	4000	4000
adj. R^2	0.049	0.051	0.006	0.007	0.000	−0.000

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Coefficients from OLS regressions with no controls. Cluster-robust standard errors in parentheses (clustered at the group level). Columns (1) and (2) report the effect of partner choice on overall assortativity. Columns (3) and (4) report the effect of partner choice on overall contributions. Columns (5) and (6) report the effect of partner choice on overall earnings in the Group5History experiment.

Table A3
Treatment effects on Earnings, Contributions and Exclusion, by Type in Group9History.

	(1) Earnings	(2) Earnings	(3) Contribution	(4) Contribution	(5) Exclusion	(6) Exclusion	(7) Keep	(8) Keep
Choice	−22.452	11.873**	2.110*	0.875**	0.055	0.002	0.353***	0.459***
	(24.499)	(4.622)	(1.201)	(0.340)	(0.055)	(0.007)	(0.111)	(0.053)
Freerider	*Ref.*	20.512	*Ref.*	−3.855***	*Ref.*	−0.001	*Ref.*	0.024
		(12.412)		(0.905)		(0.024)		(0.021)
Cooperator	−20.512	*Ref.*	3.855***	*Ref.*	0.001	Ref.	−0.024	*Ref.*
	(12.412)		(0.905)		(0.024)		(0.021)	
Other	−11.246	9.266	3.278***	−0.578	−0.004	−0.005	−0.024	−0.000
	(12.740)	(6.624)	(1.016)	(0.353)	(0.023)	(0.013)	(0.023)	(0.012)
Choice × Freerider	*Ref.*	−34.325	*Ref.*	1.234	*Ref.*	0.053	*Ref.*	−0.106
		(26.197)		(1.259)		(0.059)		(0.112)
Choice × Cooperator	34.325	*Ref.*	−1.234	*Ref.*	−0.053	*Ref.*	0.106	*Ref.*
	(26.197)		(1.259)		(0.059)		(0.112)	
Choice × Other	35.865	1.540	−1.106	0.128	−0.071	−0.018	0.142	0.036
	(27.620)	(9.794)	(1.358)	(0.448)	(0.061)	(0.020)	(0.114)	(0.051)
Constant	75.980***	55.468***	2.545***	6.401***	0.112***	0.112***	0.122***	0.098***
	(11.852)	(3.278)	(0.851)	(0.270)	(0.021)	(0.004)	(0.023)	(0.008)
N	7020	7020	6240	6240	7020	7020	5928	5928
Reference group	F	C	F	C	F	C	F	C
adj. R^2	0.003	0.003	0.076	0.076	0.001	0.001	0.241	0.241

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.0$. Coefficients from OLS regressions with no controls. Cluster-robust standard errors in parentheses (clustered at the group level). Reference category in Columns (1), (3), (5) and (7): Free rider. Columns (2), (4), (6) and (8): Cooperators. In these regressions, we do not distinguish between unconditional cooperators and reciprocators.

Table A4
Treatment effects on Overall Assortativity, Contributions and Earnings in Group9History.

	(1) Assortativity	(2) Assortativity	(3) Contribution	(4) Contribution	(5) Earnings	(6) Earnings
Choice	0.012	0.045	1.020***	1.029***	10.202***	10.291***
	(0.052)	(0.050)	(0.318)	(0.360)	(3.184)	(3.603)
Period		0.001		0.010		0.099
		(0.001)		(0.014)		(0.142)
Choice × Period		−0.003		−0.001		−0.008
		(0.002)		(0.020)		(0.203)
Constant	0.505***	0.499***	5.952***	5.848***	59.520***	58.480***
	(0.033)	(0.033)	(0.241)	(0.274)	(2.406)	(2.736)
N	5817	5817	6240	6240	7020	7020
adj. R^2	−0.000	0.000	0.021	0.020	0.001	0.001

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Coefficients from OLS regressions with no controls. Cluster-robust standard errors in parentheses (clustered at the group level). Columns (1) and (2) report the effect of partner choice on overall assortativity. Columns (3) and (4) report the effect of partner choice on overall contributions. Columns (5) and (6) report the effect of partner choice on overall earnings in the Group9History experiment.

Table A5
Treatment effects on Earnings, Contributions and Exclusion, by Type in Group9Current.

	(1) Earnings	(2) Earnings	(3) Contribution	(4) Contribution	(5) Exclusion	(6) Exclusion	(7) Keep	(8) Keep
Choice	−14.392	29.061***	3.689**	1.538***	0.052*	−0.019*	0.291**	0.557***
	(14.221)	(5.972)	(1.541)	(0.361)	(0.026)	(0.011)	(0.123)	(0.044)
Freerider	*Ref.*	42.301***	*Ref.*	−4.847***	*Ref.*	−0.042**	*Ref.*	0.000
		(9.341)		(0.634)		(0.017)		(0.016)
Cooperator	−42.301***	*Ref.*	4.847***	*Ref.*	0.042**	*Ref.*	−0.000	*Ref.*
	(9.341)		(0.634)		(0.017)		(0.016)	
Other	−35.075***	7.226	4.000***	−0.847	0.033**	−0.008	−0.002	−0.002
	(9.648)	(6.731)	(0.700)	(0.568)	(0.014)	(0.016)	(0.016)	(0.013)
Choice × Freerider	*Ref.*	−43.453***	*Ref.*	2.151	*Ref.*	0.071**	*Ref.*	−0.266**
		(15.404)		(1.536)		(0.031)		(0.120)
Choice × Cooperator	43.453***	*Ref.*	−2.151	*Ref.*	−0.071**	*Ref.*	0.266**	*Ref.*
	(15.404)		(1.536)		(0.031)		(0.120)	
Choice × Other	24.677	−18.776	−2.477	−0.326	−0.026	0.045	0.091	−0.175**
	(18.183)	(13.856)	(1.660)	(0.681)	(0.036)	(0.034)	(0.123)	(0.070)
Constant	88.774***	46.473***	1.287**	6.134***	0.076***	0.118***	0.097***	0.097***
	(8.558)	(3.867)	(0.491)	(0.272)	(0.013)	(0.006)	(0.017)	(0.009)
N	5760	5760	5120	5120	5760	5760	4864	4864
Reference group	F	C	F	C	F	C	F	C
adj. R^2	0.010	0.010	0.162	0.162	0.001	0.001	0.290	0.290

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.0$. Coefficients from OLS regressions with no controls. Cluster-robust standard errors in parentheses (clustered at the group level). Reference category in Columns (1), (3), (5) and (7): Free rider. Columns (2), (4), (6) and (8): Cooperators. In these regressions, we do not distinguish between unconditional cooperators and reciprocators.

Table A6
Treatment effects on Overall Assortativity, Contributions and Earnings in Group9History.

	(1) Assortativity	(2) Assortativity	(3) Contribution	(4) Contribution	(5) Earnings	(6) Earnings
Choice	0.113*	0.067	1.900***	0.566	18.996***	5.663
	(0.065)	(0.083)	(0.350)	(0.424)	(3.496)	(4.244)
Period		0.002		−0.096***		−0.957***
		(0.002)		(0.023)		(0.225)
Choice × Period		0.004		0.127***		1.270***
		(0.004)		(0.029)		(0.290)
Constant	0.511***	0.495***	5.327***	6.332***	53.273***	63.324***
	(0.047)	(0.052)	(0.230)	(0.289)	(2.297)	(2.889)
N	4798	4798	5120	5120	5760	5760
adj. R^2	0.013	0.015	0.062	0.073	0.005	0.006

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.0$. Coefficients from OLS regressions with no controls. Cluster-robust standard errors in parentheses (clustered at the group level). Columns (1) and (2) report the effect of partner choice on overall assortativity. Columns (3) and (4) report the effect of partner choice on overall contributions. Columns (5) and (6) report the effect of partner choice on overall earnings in the Group9Current experiment.

Table A7
The Effect of Free Riders on Earnings of Cooperators and Others, by Experiment.

	(1) Earnings	(2) Earnings	(3) Earnings
Free riders in group	−19.997***	−13.019***	−15.977***
	(5.935)	(4.405)	(3.972)
Choice	5.166	10.578**	17.869***
	(4.748)	(4.120)	(2.816)
Choice × Free riders in group	20.278	6.321	2.730
	(13.156)	(6.800)	(6.918)
Constant	59.943***	63.769***	58.838***
	(3.456)	(3.256)	(1.651)
N	3680	6520	5280
adj. R^2	0.002	0.004	0.010

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors in parentheses (clustered at the group level). The regressions report the estimated effect of having free riders in the group on the earnings of Cooperators and Others. The variable "Free riders in group" takes the value 1 if there is a positive share of free riders in the group, and the value 0 if there are no free riders in the group. Column (1): Group5History, Column (2): Group9History and Column (3): Group9Current experiment, respectively.

Table A8
Main Features of experiments.

Features	Group5History	Group9History	Group9Current
Choice	20 groups, 100 subjects	20 groups, 180 subjects	16 groups, 144 subjects
Random	20 groups, 100 subjects	19 groups, 171 subjects	16 groups, 144 subjects
Fixed group size	5 members per group	9 members per group	9 members per group
Reminder	Reminded of current and previous periods	Reminded of current and previous periods	Reminded of current period

References

Ahn, T.K., Isaac, R.M., Salmon, T.C., 2009. Coming and going: Experiments on endogenous group sizes for excludable public goods. Journal of public Economics 93 (1–2), 336–351.

Ahn, T.K., Isaac, R.M., Salmon, T., 2008. Endogenous group formation. Journal of Public Economic Theory 10 (2), 171–194.

Alger, I., Weibull, J.W., 2019. Evolutionary models of preference formation. Annual Review of Economics 11, 329–354.

Alger, I., Weibull, J.W., Lehmann, L., 2020. Evolution of preferences in structured populations: Genes, guns, and culture. Journal of Economic Theory 185, 1–45 104951.

Alger, I., Weibull, J., 2013. Homo moralis-preference evolution under incomplete information and assortative matching. Econometrica 81 (6), 2269–2302.

Barclay, P., 2013. Strategies for cooperation in biological markets, especially for humans. Evolution and Human Behaviour 34 (3), 164–175.

Barclay, P., Raihani, N., 2016. Partner choice versus punishment in human prisoner's dilemmas. Evolution and Human Behaviour 37, 263–271.

Barclay, P., Willer, R., 2007. Partner choice creates competitive altruism in humans. Proceedings of the Royal Society B: Biological Sciences 274 (1610), 749–753.

Bayer, R.C., 2016. Cooperation in partnerships: The role of breakups and reputation. Journal of Institutional and Theoretical Economics 172 (4), 615–638.

Bernard, M., Fanning, J., Yuksel, S., 2018. Finding cooperators: Sorting through repeated interaction. Journal of Economic Behaviour & Organization 147, 76–94.

Bock, O., Baetge, I., Nicklisch, A., 2014. hroot: Hamburg registration and organizational online tool. European Economic Review 71, 117–120.

Bohnet, I., Kübler, D., 2005. Compensating the cooperators: Is sorting in the prisoner's dilemma possible. Journal of Economic Behaviour & Organization 56 (1), 61–76.

Bonacich, P., Shure, G.H., Kahan, J.P., Meeker, R.J., 1976. Cooperation and group size in the n-person prisoners' dilemma. The Journal of Conflict Resolution 20 (4), 687–706.

Boyd, R., Richerson, P.J., 1988. The evolution of reciprocity in sizable groups. Journal of Theoretical Biology 132 (3), 337–356.

Brekke, K.A., Hauge, K.E., Lind, J.T., Nyborg, K., 2011. Playing with the good guys: A public good game with endogenous group formation. Journal of Public Economics 95 (9), 1111–1118.

Brown, M., Falk, A., Fehr, E., 2004. Relational contracts and the nature of market interactions. Econometrica 72 (3), 747–780.

Burlando, R., Guala, F., 2005. Heterogeneous agents in public goods experiments. Experimental Economics 8, 35–54.

Carpenter, J., 2007. Punishing free-riders: How group size affects mutual monitoring and the provision of public goods. Games and Economic Behaviour 60 (1), 31–51.

Charness, G., Yang, C.L., 2014. Starting small toward voluntary formation of efficient large groups in public goods provision. Journal of Economic Behaviour & Organization 102, 119–132.

Cinyabuguma, M., Page, T., Putterman, L., 2005. Cooperation under the threat of expulsion in a public goods experiment. Journal of Public Economics 89 (8), 1421–1435.

Coricelli, G., Fehr, D., Fellner, G., 2004. Partner selection in public goods experiments. Journal of Conflict Resolution 48 (3), 356–378.

dos Santos, M., Wedekind, C., 2015. Reputation based on punishment rather than generosity allows for evolution of cooperation in sizable groups. Evolution and Human Behaviour 36 (1), 59–64.

Dreber, A., Rand, G.D., Fudenberg, D., Nowak, A.M., 2008. Winners don't punish. Nature 452, 348–351.

Egas, M., Riedl, A., 2008. The economics of altruistic punishment and the maintenance of cooperation. Proceedings of the Royal Society B: Biological Sciences 275 (1637), 871–878.

Ehrhart, K.M., Keser, C., 1999. Mobility and Cooperation: On the Run. CIRANO Working Papers. CIRANO.

Fehr, E., Gächter, S., 2000. Cooperation and punishment in public goods experiments. American Economic Review 90 (4), 980–994.

Feinberg, M., Willer, R., Schultz, M., 2014. Gossip and ostracism promote cooperation in groups. Psychological Science 25, 1–9.

Fischbacher, U., 2007. z-Tree: Zurich toolbox for ready-made economic experiments. Experimental Economics 10 (2), 171–178.

Fischbacher, U., Gächter, S., 2010. Social preferences, beliefs, and the dynamics of free riding in public goods experiments. American Economic Review 100 (1), 541–556.

Fischbacher, U., Gächter, S., Fehr, E., 2001. Are people conditionally cooperative? Evidence from a public goods experiment. Economic Letters 71 (3), 397–404.

Fischbacher, U., Gächter, S., Quercia, S., 2012. The behavioural validity of the strategy method in public good experiments. Journal of Economic Psychology 33 (4), 897–913.

Frank, R.H., 1988. Passions within reason: The strategic role of the emotions. WW Norton & Co, New York.

Frank, R.H., 2011. The strategic role of the emotions. Emotion Review 3 (3), 252–254.

Gächter, S., Kölle, F., Quercia, S., 2017. Reciprocity and the tragedies of maintaining and providing the commons. Nature Human Behaviour 1 (9), 650–656.

Gächter, S., Thöni, C., 2005. Social learning and voluntary cooperation among like-minded people. Journal of the European Economic Association 3 (2–3), 303–314.

Gaudeul, A., Crosetto, P., Riener, G., 2015. Of the stability of partnerships when individuals have outside options, or why allowing exit is inefficient. Jena Economic Research Papers 1.

Guala, F., 2012. Reciprocity: Weak or strong? What punishment experiments do (and do not) demonstrate. Behavioural Brain Science 35 (1), 1–15.

Guido, A., Robbett, A., Romaniuc, 2019. Group formation and cooperation in social dilemmas: A survey and meta-analytic evidence. Journal of Economic Behaviour & Organization 159, 192–209.

Gunnthorsdottir, A., Houser, D., McCabe, K., 2007. Disposition, history and contributions in public goods experiments. Journal of Economic Behavior and Organization 62 (2), 304–315.

Gürerk, Ö., Irlenbusch, B., Rockenbach, B., 2006. The competitive advantage of sanctioning institutions. Science 312 (5770), 108–111.

Güth, W., Kliemt, H., 1998. The indirect evolutionary approach: Bridging the gap between rationality and adaptation. Rationality and Society 10 (3), 377–399.

Güth, W., Yaari, M., 1992. An evolutionary approach to explain reciprocal behaviour in a simple strategic game. In: Witt, U. (Ed.), Explaining process and change–Approaches to evolutionary economics. University of Michigan Press, Ann Arbor, MI, pp. 23–34.

Hauge, K.E., Brekke, K.A., Nyborg, K., Lind, J.T., 2019. Sustaining cooperation through self-sorting: The good, the bad, and the conditional. Proceedings of the National Academy of Sciences 116 (12), 5299–5304.

Hauk, E., Nagel, R., 2001. Choice of partners in multiple two-person prisoner's dilemma games: An experimental study. Journal of Conflict Resolution 45 (6), 770–793.

Honhon, D., Hyndman, K., 2020. Flexibility and reputation in repeated prisoner's dilemma games. Management Science 1–17.

Huck, S., Lünser, G.K., Tyran, J.R., 2012. Competition fosters trust. Games and Economic Behaviour 76 (1), 195–209.

Kamei, K., Putterman, L., 2017. Play it again: Partner choice, reputation building and learning from finitely repeated dilemma games. The Economic Journal 127 (602), 1069–1095.

Kerr, N.L., 1989. Illusions of efficacy: The effects of group size on perceived efficacy in social dilemmas. Journal of Experimental Social Psychology 25 (4), 287–313.

Kosfeld, M., 2004. Economic networks in the laboratory: A survey. Review of Network Economics 3 (1), 20–42.

Kreps, D., Wilson, R., 1982. Reputation and imperfect information. Journal of Economic Theory 27 (2), 253–279.

Kurzban, R., Houser, D., 2005. Experiments investigating cooperative types in humans: A complement to evolutionary theory and simulations. Proceedings of the National Academy of Sciences of the United States of America 102 (5), 1803–1807.

Leibbrandt, A., Ramalingam, A., Sääksvuori, L., Walker, J., 2015. Incomplete punishment networks in public goods games: Experimental evidence. Experimental Economics 18, 15–37.

Masclet, D., Noussair, C., Villeval, M., Tucker, S., 2003. Monetary and nonmonetary punishment in the voluntary contributions mechanism. American Economic Review 93 (1), 366–380.

Miller, G.A., 1956. The magical number seven, plus or minus two: Some limits on our capacity for processing information. Psychological Review 63, 81–97.

Nosenzo, D., Quercia, S., Sefton, M., 2015. Cooperation in small groups: The effect of group size. Experimental Economics 18 (1), 4–14.

Nosenzo, D., Sefton, M., 2014. Promoting cooperation: The distribution of reward and punishment power. In: Van Lange, P.A.M., Rockenbach, B., Yamagashi, T. (Eds.), Social dilemmas: New perspectives on reward and punishment. Oxford University Press, Oxford, UK, pp. 87–114.

Page, T., Putterman, L., Unel, B., 2005. Voluntary association in public goods experiments: Reciprocity, mimicry and efficiency. The Economic Journal 115 (506), 1032–1053.

Riedl, A., Ule, A., 2002. Exclusion and cooperation in social network experiments. Working Paper. University of Amsterdam, CREED.

Rigaud Maier, F.P., Martinsson, P., Staffiero, G., 2010. Ostracism and the provision of a public good: Experimental evidence. Journal of Economic Behaviour and Organization 73, 387–395.

Rigdon, M., McCabe, A.K., Smith, L.V., 2007. Sustaining cooperation in trust games. Economic Journal 117 (522), 991–1007.

Roberts, G., 1998. Competitive altruism: From reciprocity to the handicap principle. Proceedings of the Royal Society of London. Series B: Biological Sciences 265 (1394), 427–431.

Samuelson, L., 2001. Introduction to the evolution of preferences. Journal of Economic

Theory 97 (2), 225–230.
Schelling, T., 1960. The strategy of conflict. Harvard University Press, Cambridge, MA.
Selten, R., 1967. Die Strategiemethode zur Efrorschung des eingeschränkt rationalen Verhaltens im Rahmen eines Oligopolexperiments. Beiträge zur experimentellen Wirtschaftsforschung 136–168.
Smith, A., 1759. The theory of moral sentiments. 1976. Liberty Fund, Indianapolis.
Strømland, E., Tjøtta, S., Torsvik, G., 2018. Mutual choice of partner and communication in a repeated prisoner's dilemma. Journal of Behavioural and Experimental Economics 75, 12–23.
Sylwester, K., Roberts, G., 2010. Cooperators benefit through reputation-based partner choice in economic games. Biology letters 6 (5), 659–662.
Thöni, C., Volk, S., 2018. Conditional cooperation: Review and refinement. Economics Letters 171, 37–40.
Ule, A., 2005. Exclusion and cooperation in networks (No. 354). Rozenberg Publishers, Amsterdam, The Netherlands: Tinbergen Institute.
Van Lange, P.A., Joireman, J., Parks, C.D., Van Dijk, E., 2013. The psychology of social dilemmas: A review. Organizational Behaviour and Human Decision Processes 120 (2), 125–141.
Van Vugt, M., Roberts, G., Hardy, C., 2007. Competitive altruism: Development of reputation-based cooperation in groups. In: Barrett, L., Dunbar, R. (Eds.), Oxford handbook of evolutionary psychology. Oxford University Press, Oxford, England, pp. 531–540.
Walker, J.M., Halloran, M.A., 2004. Rewards and sanctions and the provision of public goods in one-shot settings. Experimental Economics 7 (3), 235–247.
Wilson, A.J., Wu, H., 2017. At-will relationships: How an option to walk away affects cooperation and efficiency. Games and Economic Behaviour 102, 487–507.
Wu, J., Balliet, D., Peperkoorn, L.S., Romano, A., Van Lange, P.A., 2020. Cooperation in groups of different sizes: The effects of punishment and reputation-based partner choice. Frontiers in Psychology 10, 2956.
Zelmer, J., 2003. Linear public goods experiments: A meta-analysis. Experimental Economics 6 (3), 299–310.