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Choice Flexibility and Long-Run Cooperation

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Abstract

Understanding how incentives and institutions help scaling up cooperation is important, especially when strategic uncertainty is considerable. Evidence suggests that this is challenging even when full cooperation is theoretically sustainable thanks to indefinite repetition. In a controlled social dilemma experiment, we show that adding *partial* cooperation choices to the usual binary choice environment can raise cooperation *and* efficiency. Under suitable incentives, partial cooperation choices enable individuals to cheaply signal their desire to cooperate, reducing strategic uncertainty. The insight is that richer choice sets can form the basis of a language meaningful for coordinating on cooperation.

Keywords: experiments, repeated games, social dilemmas, strategy estimation.

JEL codes: C70, C90, D03, E02

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1 Introduction

Entrepreneurs often confront situations where they must ensure the cooperation of unfamiliar business associates without the support of legal and enforcement organizations. Even within large organizations—where certain aspects of cooperation can be formally contracted and enforced—many interactions take the form of temporary collaborations with strangers whose objectives are unclear or unpredictable. In these situations, cooperation cannot be incentivized through reciprocity, and strategic uncertainty may frustrate the attempts at coordinating on a common objective.[1] The question is thus: what interventions can we adopt to incentivize cooperation?

Social dilemma experiments suggest two possible solutions. One is to make cooperation highly valuable in economic terms (e.g., Charness and Rabin, 2002; Dal Bó and Fréchette, 2018; Isaac and Walker, 1988; Nosenzo et al., 2015). This direct intervention is not budget neutral and runs into tight constraints, in practice. Another solution is to invest resources in performance-monitoring technologies or individual sanctioning systems capable of supporting strategies that incentivize cooperation (e.g., Camera and Casari, 2009; Cinyabuguma et al., 2006; Fehr and Gächter, 2002; Nikiforakis, 2008). These indirect interventions are also costly, their implementation may be challenging (e.g., due to legal considerations) and, in fact, may negate the anticipated efficiency gains.[2] Hence, there is scope for exploring alternative interventions.

[1] See Bowles and Gintis (2011); Kaplan et al. (2018); Kimbrough et al. (2008); North (1991); Rodrik (2000); Seabright (2004) for a discussion of the problems associated with impersonal interaction with unfamiliar counterparts. See Ben-Ner and Putterman (2000) for a discussion of the problems of cooperation within organizations. See Bigoni et al. (2020) for a literature review of experiments where strategic uncertainty and lack of reciprocity prevent the emergence of cooperative equilibrium.

[2] For instance, peer punishment in social dilemmas reduces group welfare due to misuse in Cinyabuguma et al. (2006) and Nikiforakis (2008), and fails to increase efficiency in Camera and Casari (2009).

Here, we argue that flexibility in choices can provide a partial solution. We hypothesize that a richer choice set can form the basis of a language that – if meaningful to strangers – can reduce strategic uncertainty and help them coordinate on cooperation. This angle of inquiry is largely unexplored in the literature. To explain, social dilemma experiments oftentimes restrict choices to either full or no cooperation (binary choices), while sometimes partial cooperation is allowed (flexible choices). Binary choices are standard in indefinitely repeated social dilemmas—where economic incentives alone can support cooperation. By contrast, in public goods experiments, where economic incentives are typically insufficient for cooperation, subjects can often contribute fractions of their endowment (see Chaudhuri, 2011, for a literature review), but these studies—as well as other social dilemmas with flexible choices (e.g., Bigoni et al., 2012; Wright, 2013)—cannot establish causal effects of choice flexibility on cooperation because there is no binary control treatment.

Only two social dilemma experiments compare rigid and flexible choices, and both are limited to partners' designs. The finitely repeated VCM in Gangadharan and Nikiforakis (2009) finds that partners cooperate more when the action set is binary as compared to non-binary. Unlike them, we are interested in indefinitely repetition, where economic incentives *alone* can support efficient play as an equilibrium outcome. Furthermore, our games create a rich strategy set *even if* action sets are binary, giving rise to multiple equilibria, strategic uncertainty, and coordination problems. We ask: is being able to cooperate *less than fully* today helpful to coordinate on full cooperation in the future? We are only aware of one indefinitely repeated social dilemma contrasting binary to flexible actions (Lugovskyy et al., 2019). The experiment only considers fixed pairs and finds no effect from altering the choice set.[3]

[3]In a one-shot trust game, moving away from binary choices can provide a clearer signal

In our design, a group of twelve strangers confronts an indefinitely repeated social dilemma where full cooperation is an equilibrium. In each round these strangers are randomly paired to play a helping game—an asynchronous cooperation task where one player (the "donor") can cooperate by sustaining a cost to bestow a proportionally larger benefit upon the counterpart (the "recipient"). This is a binary-choice setup: donors can either cooperate or defect. After observing the outcome, players switch roles and play the next round in a new donor-recipient pair where they cannot leverage reciprocity or reputation because the counterpart's identity and past behavior are hidden. The game stops with a known probability after a certain number of rounds has been played, hence its exact duration is unknown. In a session, everyone also confronts this social dilemma in fixed pairs, a within-subject design used to establish if flexibility of choice has a differential impact in partners vs. strangers settings.

In many repeated social dilemma experiments players make simultaneous choices and can immediately reap the benefits of from cooperation (e.g., PD games). Instead, in our setup players *alternate* in making choices, so decision makers can benefit from cooperation only in future periods. This makes explicit to participants the *intertemporal* nature of the cooperation task. As noted in Bigoni et al. (2019), alternation in decision making also makes the distinction between partners and strangers' economies sharper (2 vs 12 players). It eliminates strategic uncertainty in fixed pairs, as the initial decision maker can select the equilibrium. This supports cooperation; Ghidoni and Suetens (2022) report increased cooperation when moving from simultaneous

of trust, raising repayment of trust but not trust (Gomez-Miñambres et al., 2021). In an indefinitely repeated trust game with exogenous player types, the opponents' type can be uncovered by gradually increasing the investment (Kartal et al., 2021).

to sequential actions in two-person repeated PD games.

Our *Baseline* is the binary-choice treatment. Here, donors can only adjust their cooperation frequency across periods—an extensive margin of choice. In flexible-choice treatments, instead, we expand the choice set so donors can also adjust their cooperation level *within* a period—an intensive margin of choice. A donor can choose cooperation levels going from 0% (full defection) to 100% (full cooperation) in twelve equal increments. This does not increase the return from full cooperation and full defection–it simply opens the door to earning returns from partial cooperation (interior choices). Theoretically, this should not increase the frequency of full cooperation, for two reasons. Interior cooperation choices are suboptimal from an economic incentives perspective: partial cooperation cannot maximize earnings in equilibrium, and does not maximize deterrence when used as a sanction off-equilibrium. Moreover, expanding the choice set to include interior cooperation levels adds Pareto-inferior equilibria, which should not facilitate coordination on efficient play.

The returns from full cooperation and full defection are identical in all treatments. In flexible-choice treatments we only intervene on the shape of returns from partial cooperation. In the *Linear* treatment the return from cooperation is constant and identical to that in *Baseline*: one extra unit of cooperation always generates 2.5 units of surplus. In the *Low* and *High* treatments the return from cooperation is nonlinear. At low cooperation levels, it generates 1.5 units of surplus; as soon as the cooperation choice exceeds a pre-specified threshold—respectively low and high—surplus shifts upward by 6 points. Thus, *Low* and *High* manipulate the social value of *partial* cooperation relative to the other treatments.

A main findings is that adding interior choices helped groups of strangers to achieve higher average cooperation levels as compared to *Baseline*. Consequently, realized efficiency also increased. The strongest effect is seen in *Linear* and *Low*, where modest cooperation levels generated substantial surplus gains. Here, strangers achieved up to a 50% increase in cooperation and efficiency. By contrast, cooperation did not increase in *High*, where modest cooperation levels only generated small surplus gains. An insight is that the economic benefit associated with partial cooperation has a significant impact on behavior in groups of strangers. Importantly, the analysis reveals that this effect is not present in fixed pairs, where cooperation did not improve with choice flexibility. These results suggest that flexibility in choice can be a powerful intervention when individuals face strategic uncertainty and cannot leverage reciprocity mechanisms to support cooperation.

The natural question is thus: how did flexibility of choices affect behavior among strangers? We use a finite-mixture model to estimate categories of behavior—conditional and unconditional on past actions—and their distribution in the subject population. Our random assignment of subjects to treatments allows us to analyze if and how this distribution varies across treatments. The proportion of free-riders—individuals who methodically defect without ever attempting to cooperate—is significantly lower in *Linear* and *Low*, as compared to *Baseline* and *High*, while conditional and unconditional cooperators increased. This shows that defection became a less attractive choice only when a modest cooperation level created sufficient gains. In this case, choice flexibility supported coordination on cooperation because — by choosing a modest cooperation level — donors could send a cheap *and* economically meaningful cooperative signal to their counterpart.

The paper is organized as follows. Section 2 describes the experimental design. Section 3 introduces the theory and offers some testable hypotheses. Section 4 presents the main results, and Section 5 offers a final discussion.

2 Experimental design

In the experiment, subjects face an indefinite sequence of pairwise interactions. Each pair confronts a variant of an individual decision problem known as "helping game" (Nowak and Sigmund, 1998): one individual is a "donor" who can sustain a costly effort to provide a benefit to the counterpart, an inactive "recipient." Importantly, the benefit is always greater than the effort cost and maximum effort is socially optimal.

Table 1: Payoffs in a meeting for a given effort e.

Donor	Recipient
$6 - e$	$3 + f(e)$

Notes: The donor chooses effort $0 \leq e \leq 6$; the recipient has no action to take. In the experiment $f(0) = 0$ and $f(6) = 15$; 1 point=\$0.18. All framing in the experiment is neutral.

Table 1 presents the decision problem in a general form that captures all treatments (screenshots and instructions are in Appendix B). The recipient earns a base payoff of 3 points and can receive a benefit of $f(e)$ additional points from the donor, which depends on the donor's effort $e \in E \subseteq \{0, 0.5, 1, \ldots, 6\}$. The donor earns $6 - e$ points. In all treatments, the "return function" f is strictly increasing with $f(0) = 0$, $f(e) > e$ for $e > 0$, and $f(6) = 15$. Payoffs are determined by the functional form of f, and the cardinality of the choice set E; these will be our two treatment variables.

We say that there is *(full) defection* in a meeting if $e = 0$, in which case the total payoff is at its minimum (9 points). There is *partial cooperation* if $0 < e < 6$, and *(full) cooperation* if $e = 6$. A meeting generates $f(e) - e \geq 0$ surplus, which is the benefit to the recipient minus the cost to the donor. In all treatments, $f(e') - f(e) > e' - e > 0$ holds, i.e., the surplus generated in a meeting monotonically increases in e. Hence, though the donor's dominant action is no effort, $e = 0$, the socially optimal action is maximum effort $e = 6$. For $e > 0$, the benefit/cost ratio $f(e)/e > 1$ defines the *return from cooperation.*

Session: Each session involves 24 subjects in the laboratory, all exposed to the same treatment. Everyone participates in five consecutive supergames of uncertain duration, determined by a random continuation rule (Roth and Murnighan, 1978). Each supergame starts and ends simultaneously for all subjects in the session. A supergame includes 16 periods after which subsequent periods occur with probability $\delta = 0.8$.[4] The continuation probability δ can be interpreted as the discount factor of a risk-neutral player.

Economy size and matching protocol: In each supergame subjects interact in fixed matching groups (or, economies) of even size $2N$. The size of groups in supergames 1-4 is $2, 12, 2, 12$ for half of the sessions and the reverse sequence for the remaining sessions. In supergame 5 we have again 12 players to study possible end-of-session effects on behavior in large groups. Hence, there are either twelve or two concurrent supergames being played in a session, depending on whether $2N = 2$ or $2N = 12$, respectively. Economies are

[4]The expected duration of each supergame is thus 20 periods because five additional periods are expected ($= \frac{1}{1-\delta}$), after period 15. In the experiment, at the end of each period a computer draws with equal probability an integer number between 1 and 100; this number is shown to all subjects. A draw above 80 determines the end of the supergame (otherwise, it continues). This experimental feature keeps the supergame duration random and reduces variation in subjects' experience across games, sessions, and treatments (see distribution of supergame lengths across treatments in Table B3 in Appendix B), which could be a possible confound. To fully control for possible effects of realized lengths (e.g., see Mengel et al., 2022), we include standardized supergame duration covariates in our econometric models. In addition, this experimental feature offers tighter control over the duration of a session, decreasing the variance in length.

constructed so that no one interacts with the same individuals in more than one supergame.[5] The within-subject alternation between size $2N = 2$ and $2N = 12$ allows us to establish individual differences between interaction as partners and strangers, respectively. It also gives subjects experience with the task through repeated exposure, and allows us to control for possible effects of starting as partners or not.

Figure 1: Layout of a Session.

2-player group	Rematch in 12-player group	Rematch in 2-player group	Rematch in 12-player group	Rematch in 12-player group
1	2	3	4	5

Supergames

In every economy, we create N meetings in each period, each with one donor and one recipient. Hence, in every period there is an equal number of recipients and donors. These roles are randomly assigned by the computer in the first period of a supergame, and then deterministically alternate (e.g., donor, recipient, donor, ...). This role assignment procedure ensures equal earning potential for all subjects. In economies with $2N = 2$, the partner is fixed and there is perfect monitoring. Instead, in economies where $2N = 12$, subjects are randomly re-matched in 6 donor-recipient pairs in each period, with uniform probability. In these large groups, subjects by design cannot identify their

[5] Groups of 12 are created as follows. Six are of type 1 (beginning donor) and six of type 2 (beginning recipient). Type 1 subjects can only meet type 2 subjects and viceversa. The 24 subjects in the session are partitioned in 4 sets of 6 each: $A = \{1, 2, 3, 4, 5, 6\}, \ldots, D = \{19, 20, 21, 22, 23, 24\}$. The sets A through D are fixed for the duration of the session. For each of the three supergames of large groups, subjects from one set are matched, round robin, to subjects from the other three sets. For each of the two supergames of fixed pairs, subjects are matched to a different player of its own set. This matching process supports only three supergames with large groups, so that no one interacts with the same individuals in more than one supergame. This procedure is discussed in Bigoni et al. (2019).

counterparts and cannot see their past actions, so they interact as strangers. Hence, subjects cannot build a reputation within their economy. The only information observed about others' behavior at the end of each period is average effort in the economy $\bar{e}$ (see screenshots from instructions in Appendix B). This is a minimal form of anonymous public monitoring, which is helpful to construct trigger strategies that support cooperation among strangers.

Treatments: We have four treatments, including the Baseline design; Table 2 provides summary details by treatment.[6]

Table 2: Treatments

Variable	Baseline	High	Linear	Low
Effort choice e	$0, 6$	$0, 0.5, \ldots, 6$	$0, 0.5, \ldots, 6$	$0, 0.5, \ldots, 6$
Maximum benefit	15	15	15	15
Effort for 50% max benefit	—	4.5	3	1.5
Sessions (subjects)	4 (96)	4 (96)	4 (96)	4 (96)
Large groups (fixed pairs)	24 (96)	24 (96)	24 (96)	24 (96)
Periods/Supergame (avg.)	21.3	19.8	23.3	21.9
Salient $ Earnings (avg.)	28.00	26.75	25.75	33.75
min, max	11.25, 47.50	11.25, 50.75	11.00, 35.75	15.25, 46.50

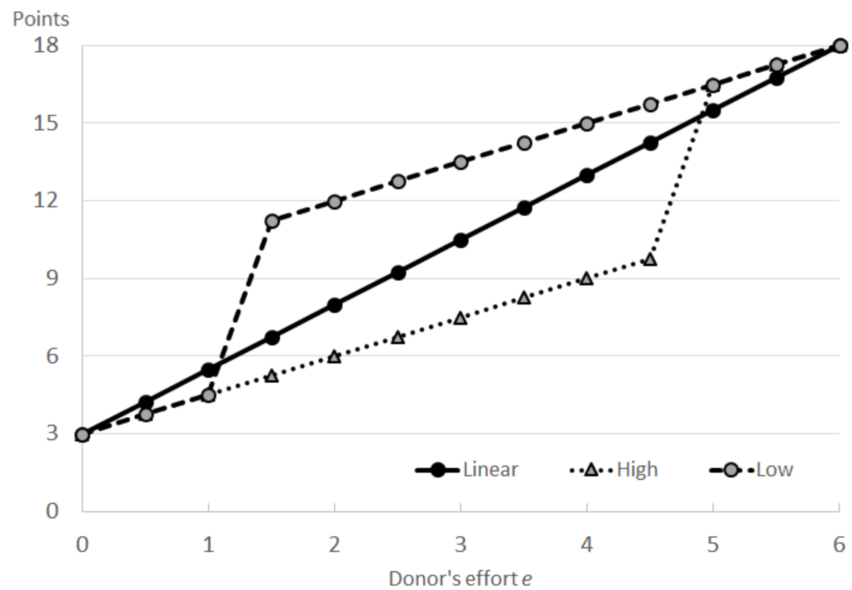
Notes: Maximum benefit is the highest value f attainable, corresponding to the efficient outcome $e = 6$. "Effort for 50% max benefit' is the e value creating (approximately or exactly) half of the maximum possible benefit. No. of economies per treatment: 96 fixed pairs, 24 groups of 12. Salient earnings are rounded up to the next quarter, exclude the $7 fixed participation amount, and the dollar earnings from the quiz ($2.25 average).

The treatments involve two incremental manipulations: first, the composition of the choice set E, and subsequently the shape of the function $f(e)$. In all treatments, the choice set E includes the boundary points $e = 0, 6$, and these extreme choices bestow benefits of $f(0) = 0$ and $f(6) = 15$ points on the recipient. The *Baseline* treatment is our control treatment. Here, $f(e) = 2.5e$ and the effort choice set $E = \{0, 6\}$ includes only the boundary values, as in

[6] We ran 4 sessions per treatment following similar experiments where treatment effects induced by changing institutions were detected; see the survey in Camera et al. (2013).

the standard helping game. Fig. 2 provides an illustration.

Figure 2: The recipient's payoff $3 + f(e)$ vs. the donor's effort e



Notes: Return from cooperation is $f(e)/e$. In *Baseline* and *Linear* $f(e) = 2.5e$, in *High* $f(e) = 1.5e + 6\mathbb{1}_{e>4.5}$, and in *Low* $f(e) = 1.5e + 6\mathbb{1}_{e>1}$.

The *Linear* treatment manipulates the *Baseline* design by expanding the choice set to $E = \{0, 0.5, 1, \ldots, 6\}$. The return from any cooperation level remains 2.5, i.e., we still have $f(e) = 2.5e$. It follows that the payoff set is not expanded beyond the *Baseline* boundaries; players simply have more strategies to choose from. Adding interior choices does not change the informational structure of the game: in particular, players still interact as partners in fixed pairs and as strangers in large groups. Hence, *Linear* allows us to study the pure effect of adding interior choices, all else equal.

Two treatments, *High* and *Low*, manipulate the *Linear* treatment by altering the transformation rate of effort *for interior choices*. In other words, the benefit generated by partial cooperation varies relative to the *Linear* treatment only away from the boundaries $e = 0$ and $e = 6$. This is done by altering the shape of the return function f, making it quasilinear as follows:

$$f(e) = 1.5e + 6\mathbb{1}_{e>4.5} \qquad \text{(High treatment)},$$
$$f(e) = 1.5e + 6\mathbb{1}_{e>1} \qquad \text{(Low treatment)}.$$

The return from cooperation is initially low, 1.5 points, and jumps above 2.5 when the effort level reaches a treatment-specific threshold—$e = 1.5$ in *Low* and $e = 5$ in *High*. Consequently, the return from partial cooperation jumps considerably and at a low effort level in *Low* (from 1.5 to 5.5), and jumps moderately and at a high effort level in *High* (from 1.5. to 2.7). As effort increases to 6 the return from cooperation gradually declines back to 2.5. As a result, the effort needed to attain about half of the maximum benefit varies by treatment: it is $e = 4.5$ in *High*, $e = 3$ in *Linear*, and $e = 1.5$ in *Low*. In *High* (resp. *Low*) donors must put *more than* (resp. less than) half of maximum effort in order to bestow on the recipient about half of the maximum benefit f. This formulation allows us to explore if choice flexibility affects behavior depending on the returns from partial vs. full cooperation.

Experimental procedures: We recruited a total of 384 undergraduate student subjects (44% males) through announcements at Chapman University. No participant had previous experience with this type of game. The experiment was conducted in the Economic Science Institute's laboratory. Each subject had a private terminal; neither communication nor eye contact was possible among subjects. The experimenter publicly read the instructions at the beginning of the session; each subjects had a paper copy of the instructions. During the supergame, subjects could consult an electronic record of their own past outcomes before making a decision, and could also use pen and paper to create records. The experiment was programmed and conducted using z-Tree (Fischbacher, 2007). On average, a session lasted 104 periods for a running time of approximately 120 minutes including instructions and payments. Instructions were followed by an incentivized comprehension quiz (reported in Appendix B), with responses used to control for possible differences in subjects' understanding of the game across treatments. At the conclusion of a session, one of the five supergame was selected for payment using a web-based randomization device—as explained in the instructions; the payment corresponded to the subject's cumulative earnings in that supergame, with points were converted into U.S. cash (1 point =\$0.18).[7] Average earnings were \$28.50 per subject ($\min = \$11.00, \max = \$50.75$) excluding a \$7 fixed participation payment and an average of \$2.25 ($\min = \$.50, \max = \$2.50$) from providing correct answers to the comprehension quiz (\$0.25 for each of 10 questions).

3 Theoretical benchmark

Here we offer a general formulation of the model and use it to study subgame perfect equilibrium. As multiple equilibria exist, we then enrich the analysis by discussing an equilibrium selection criterion based on risk dominance. These theoretical considerations are then used to derive hypotheses.

There is a group of $2N$ players. At the start of the game, N players are randomly assigned the role of donor (denoted by $i = 0$), and N the role of recipient ($i = 1$), corresponding to the neutral experimental terminology Red and Blue. Each player has equal probability of being either donor or recipient. In each subsequent period, players switch roles deterministically: if a player is i in period $t = 1, 2, \ldots$, then the player is $(i + 1) (\mathrm{mod}\ 2)$ in period $t + 1$.

[7]The goal of this incentive mechanism is to minimize possible wealth effects from point accumulation across supergames. The econometric analysis accounts for supergame effects.

This constant alternation of roles between donor and recipient implies that a player starting as a donor (resp. recipient) will be a donor in all odd periods, and a recipient (resp. donor) in all even periods; hence, in every period there are N donors and N recipients.

In each period, players are arranged in donor-recipient pairs using uniform random matching, and the following stage game is played in each pair. The donor chooses an action $e \in E \subseteq \{0, e(1), e(2), \ldots, b\}$. The recipient has an empty action set and observes the action e of the donor. The payoffs to donor and recipient in the stage game depend on (i) the action e of the donor and (ii) a fixed value $\hat{e} \in \hat{E} \subseteq E$. Specifically, define the payoff to the player with role i by $\pi_i : E \times \hat{E} \to \mathbb{R}$ where

$$\pi_0(e, \hat{e}) = b - e \qquad \text{and} \qquad \pi_1(e, \hat{e}) = a + f(e, \hat{e}),$$
$$\text{with } f(e, \hat{e}) := k(\hat{e})e + \mu \mathbb{1}_{e>\hat{e}}, \quad \mu = [k(b) - k(\hat{e})]b, \text{ and } k(b) \geq k(\hat{e}) > 1.$$

Given this, let the *total* payoff in a meeting be $\Pi(e, \hat{e}) := \pi_0(e, \hat{e}) + \pi_1(e, \hat{e}) = a + b + f(e, \hat{e}) - e$. It is clear that Π, is strictly increasing and piecewise linear in e. That is, for all $\hat{e} \in \hat{E}$, we have two extreme points:

$$\arg\min_{e \in E} \Pi(e, \hat{e}) = 0 \qquad \text{and} \qquad \arg\max_{e \in E} \Pi(e, \hat{e}) = b.$$

We say that $e = b$ is the socially efficient action. Since $\Pi(0, \hat{e}) = a + b$ and $\Pi(b, \hat{e}) = a + k(b)b$, the maximum surplus that can be created in a meeting is $[k(b) - 1]b$, independent of $\hat{e}$.

Remark: In the experiment $a = 3$, $b = 6$, $k(\hat{e}) = 1.5 + \mathbb{1}_{\hat{e}=b}$, and $\mu = 6$. We consider economies of size $N \in \{1, 6\}$, and study four parameterizations: (i) $\hat{e} = 6$ and $E = \{0, 6\}$ (*Baseline* treatment); (ii) $\hat{e} = 4.5$ and $E = \{0, 0.5, 1, \ldots, 6\}$ (*High* treatment); (iii) $\hat{e} = 6$ and $E = \{0, 0.5, 1, \ldots, 6\}$

(*Linear* treatment); (iv) $\hat{e} = 1$ and $E = \{0, 0.5, 1, \ldots, 6\}$ (*Low* treatment).

The ex-ante payoff to a player in the infinitely repeated game is the sum of the payoffs from the stage game. It is assumed that players discount future payoffs with common factor $\delta \in (0,1)$ starting only from period 16.[8]

We call *full defection* the outcome when $e = 0$ in all meetings and all periods, and *full cooperation* the outcome when $e = b$ in all meetings and all periods. Because surplus is maximum when $e = b$, and is unaffected by $\hat{e}$, full cooperation is the outcome that supports maximum surplus in all treatments and, therefore, maximizes social efficiency. We will thus refer to full cooperation as the *(socially) efficient* outcome.

3.1 Equilibria with constant cooperation

Consider outcomes characterized by a constant cooperation level $e^* > 0$ in the group. A first important result is that multiple outcomes of this kind can be supported as a subgame perfect Nash equilibrium. In order to do so let $\mathcal{D}_t$ denote the set of donors in period t, where $|\mathcal{D}_t| = N$ in all t. The mean effort of these donors in period t is $\bar{e}_t := \frac{1}{N} \sum\limits_{j \in \mathcal{D}_t} e_{j,t}$. Recall that player j has an empty action set in period t when $j \notin \mathcal{D}_t$ (she is a recipient).

We demonstrate how to support positive effort levels e^*, by considering the following trigger strategy:

Definition 1 (Grim Strategy)**.** *Let* $e^* \in E/\{0\}$*. A strategy* $\boldsymbol{e}_j(e^*, 0) = (e_{j,1}, e_{j,2}, \ldots)$ *for a player* j *is called a "grim strategy" if it satisfies:*
(i) $e_{j,1} = e^*$ *if* $j \in \mathcal{D}_1$*,*

[8] Specifically, the ex-ante payoff to player i in the infinitely repeated game is

$$\mathbb{E} \sum_{t=1}^{15} \pi_{i(t)}(e_t, \hat{e}) + \mathbb{E} \sum_{t=16}^{\infty} \delta^{t-16} \pi_{i(t)}(e_t, \hat{e}),$$

where $i(t) = i$ if $t = 1, 3, 5, \ldots$, and $i(t) = (i+1) (\mathrm{mod}\ 2)$ if $t = 2, 4, 6, \ldots$.

(ii) $e_{j,t} = e^*$ *in all* $t > 1$ *whenever* $j \in \mathcal{D}_t$ *and* $\bar{e}_\tau \geq e^*$ *for all* $\tau < t$,
(iii) $e_{j,t} = 0$ *in all* $t > 1$ *whenever* $j \in \mathcal{D}_t$ *and* $\bar{e}_\tau < e^*$ *for some* $\tau < t$.
Player j *has no action to take whenever* $j \notin \mathcal{D}_t$.

Following Kandori (1992), we say that the grim strategy profile $(\mathbf{e}_j(e^*, 0))_{j=1}^{2N}$ is a "social norm" of cooperation. This norm leverages information about average group effort $\bar{e}$ to monitor compliance with equilibrium play. In equilibrium, donors select $e = e^*$ from period 1, and continue to do so as long $\bar{e} \geq e^*$. Otherwise, donors select the "grim" sanction $e = 0$ forever after. If $\bar{e} < e^*$, then this reveals to the entire group that someone must have lowered their effort $e < e^*$. If everybody adopts this strategy, constant effort e^* is supported by *implicit* threat of a permanent and group-wide sanction for choosing an effort level less than the informally agreed-upon effort e^*.[9]

We now present a condition that ensures this social norm is a subgame perfect Nash equilibrium.

Proposition 1. *Consider an economy with* $2N \geq 2$ *players and fix a positive effort level* $e^* \in E$. *If*

$$\delta \geq \bar{\delta}(e^*, \hat{e}) := \frac{e^*}{f(e^*, \hat{e})},$$

then the social norm of cooperation associated with the grim strategy profile $(\boldsymbol{e}_j(e^*, 0))_{j=1}^{2N}$ *is a subgame perfect equilibrium.*

Proof of Proposition 1. See section A.1 in Appendix A. □

Given the assumptions on f, we have $\bar{\delta}(e^*, \hat{e}) \in (0, 1)$. Given the parameterizations selected for the experiment, direct calculation shows that $\max_{e^* \in E} \bar{\delta}(e^*, \hat{e}) \leq 2/3$ for $\hat{e} \in \{1, 4.5, 6\}$. Since in the experiment $\delta = 4/5$, this immediately implies the following:

[9]Effectively, the trigger strategy adopted describes a two-state automaton. In each period the player can be in one of two states, "cooperate" (C) or "defect" (D). In state C, as a donor the player chooses effort $e = e^* > 0$, and chooses $e = 0$ in the D state. The player starts the game in state C. In subsequent periods, the state is updated as follows: if the state is C, then the player switches state in the next period only if the mean group effort is below e^*. Otherwise, the player remains in the C state. State D is absorbing.

Corollary 1. *In the experiment, the grim strategy profile* $\left(\boldsymbol{e}_j^*\left(e^*,0\right)\right)_{j=1}^{2N}$ *is a subgame perfect equilibrium for any positive* $e^* \in E$, $\hat{e} \in \{1, 4.5, 6\}$ *and* $N \in \{1, 6\}$.

The key implication is that the grim strategy supports multiple equilibria in the experiment, where $e = 0$, or $0 < e = e^* < b$, or $e = e^* = b$ in all periods and all meetings (full defection, partial cooperation, and full cooperation, respectively). These equilibria can be easily Pareto-ranked: the higher the equilibrium effort e, the greater the total payoff in each round of play, hence the greater is the payoff in the repeated game to *every* player; see the proof of Proposition 1. Hence, full cooperation corresponds to the socially efficient outcome.[10] Since full defection is associated with the smallest social efficiency level, the sanction considered in the grim strategy of Definition 1 theoretically provides the biggest deterrent for deviations from equilibrium play. However, if the choice set E contains elements other than 0 and b, then milder sanctions could also work as a sufficient deterrent. To study if milder sanctions can support some cooperation, consider a modified trigger strategy with more lenient sanctions off equilibrium. Formally:

Definition 2 (Lenient Strategy)**.** *Let* $\epsilon, e^* \in E = \{0, e(1), e(2), \ldots, b\}$ *with* $0 < \epsilon < e^*$. *A strategy* $\boldsymbol{e}_j(e^*, \epsilon) = (e_{j,1}, e_{j,2}, \ldots)$ *for a player* j *is called a "lenient strategy" if it satisfies:*
(i) $e_{j,1} = e^*$ *if* $j \in \mathcal{D}_1$,
(ii) $e_{j,t} = e^*$ *in all* $t > 1$ *whenever* $j \in \mathcal{D}_t$ *and* $\bar{e}_\tau \geq e^*$ *for all* $\tau < t$,
(iii) $e_{j,t}$ *is the action prescribed by the grim strategy* $\boldsymbol{e}_j(\epsilon, 0)$ *in all* $t > 1$ *whenever* $j \in \mathcal{D}_t$ *and* $\bar{e}_\tau < e^*$ *for some* $\tau < t$.
Player j *has no action to take whenever* $j \notin \mathcal{D}_t$.

In words, the player starts the game by choosing effort $e^* > 0$ as a donor, and keeps selecting that effort in all subsequent periods as long as average

[10] Partial cooperation equilibria exists also in the Baseline treatment where choices are binary, which rely on alternating between $e = 0$ and $e = b$ across periods in the supergame (e.g., $e = b$ only in periods 1-4, 9-12, etc).

effort does not fall below e^* (no deviation is detected). This is identical to the grim strategy in Definition 1. The difference with that strategy occurs if a deviation is detected. In this case, the player switches to use the grim strategy involving positive but smaller effort $\epsilon < e^*$. This effort will be selected in all periods of the continuation game unless average effort falls below ϵ. If so, this triggers permanent zero effort.

Intuitively, a grim strategy with positive effort $\epsilon \in E$ is an equilibrium of the original game (Corollary 1). Hence, the lenient strategy triggers a low-effort grim strategy, as a "fall-back" option after a deviation. If, after this switch, someone deviates by lowering their effort even further (below ϵ), then the strategy prescribes a permanent switch to zero effort. For $\epsilon = 0$ the lenient strategy $\mathbf{e}_j(e^*, \epsilon)$ corresponds to the grim strategy in Definition 1.

In the experiment, if the lenient strategy is feasible (i.e., if the choice set is not just 0 and b), then the strategy can support full cooperation.

Proposition 2. *In the experiment, if $E = \{0, e(1), e(2), \ldots, b\}$, then the lenient strategy profile $(\boldsymbol{e}_j^*(b, \epsilon))_{j=1}^{2N}$ supports full cooperation in subgame perfect equilibrium for any $\hat{e} \in \{1, 4.5, 6\}$, $N \in \{1, 6\}$, and some $\epsilon \in E$ with $0 < \epsilon < b$.*

Proof of Proposition 2. See section A.2 in Appendix A. $\square$

Full cooperation can rely on the threat of a sanction that is not as harsh as the grim sanction (full defection). This is the key difference between our design and earlier indefinitely repeated helping game experiments. When interior choices are possible, players can sanction defections by switching to an inferior equilibrium albeit not the worse possible (in terms of social efficiency).[11] These results apply to all economies of our experiment. They suggest that

[11] The drawback of more lenient sanctions is a smaller deterrent effect as the payoff in the continuation game off equilibrium is larger that with the grim sanction. This drawback is similar to that from supporting cooperation with T-period punishments, which are also possible because subjects can coordinate actions through anonymous public monitoring.

full cooperation can be attained as easily in fixed pairs as in large groups, independent of choices being binary or not. If so, the impact of our treatment interventions should be similar in strangers and partners' settings. However, there are behavioral considerations suggest this might not be the case.

3.2 Behavioral considerations

A first consideration is related to a possible impact of choice flexibility on cooperation. It is reasonable to believe that some players might not want to adopt a strategy of cooperation unless the effort required ensures a minimum payoff in each period, which we call the "safe" payoff.[12] This would imply that the grim strategy cannot support cooperation in Baseline, while partial cooperation can be supported in the other treatments. To explain, recall that any level of cooperation can be supported by the grim strategy in the experiment (Corollary 1). A candidate safe payoff is $a = 3$, which is what a recipient earns even if the donor puts no effort. It is thus reasonable to imagine that players might wish to adopt strategies ensuring something close to a also when they are donors. If so, equilibrium effort must satisfy $b - e^* \simeq a$, thus restricting selection to grim strategies with e^* around 3 ($= b - a$). We call these strategies, "safe" grim strategies. Note that these strategies also involve a salient effort level—the middle of the choice set. These strategies are unavailable in Baseline; there, the only strategy that guarantees a positive payoff in every period is $e = 0$, which does not support any cooperation. This idea can be extended to safe payoffs that are positive but much smaller or bigger than a. This suggests that choice flexibility might in fact increase cooperation relative to the Baseline, which is summarized in the following:

[12] For example introducing a safe strategy prevents coordination on a high payoff equilibrium in a two player game, even if that strategy is dominated (Goeree and Holt, 2001).

Observation 1. *No "safe" grim strategy with $e^* > 0$ exists in Baseline, while multiple safe grim strategies exist in the other treatments.*

A second behavioral considerations is related to possible differential impact of choice flexibility on cooperation in fixed pairs vs. large groups. This conjecture is suggested by empirical evidence from indefinitely repeated social dilemmas with a design similar to our Baseline, which show that partners attain higher cooperation rates than strangers; see Camera et al. (2013) and Bigoni et al. (2019). If so, our large economies might also attain less cooperation than fixed pairs in our Baseline.

There are at least two reasons for this possible cooperation differential. On the one hand, strangers have less strategies to choose from as compared to partners—strategies based on reciprocity and reputation, which support cooperation in the laboratory. In our design, perfect monitoring of the partner's actions allows players to build reputations and engage in direct reciprocity, something that strangers cannot do. In fact, it also precludes indirect reciprocity, because counterparts' identity and past behavior is never revealed. On the other hand, strategic uncertainty affects large groups but not fixed pairs. Strategic uncertainty occurs in games with multiple equilibria when players cannot explicitly coordinate their actions—as in our design—and may be unsure about the strategy chosen by others. Strategic uncertainty can play an important role in the selection of an equilibrium strategy as it increases coordination difficulties, thus impairing coordination on efficient play (Bigoni et al., 2019; Blonski et al., 2011; Heinemann et al., 1989; Van Huyck et al., 1990). In our model, strategic uncertainty exist in large economies, while it is absent in fixed pairs where there is just one decision maker in period 1, who can select the socially efficient equilibrium by choosing $e = b$.

To formalize these ideas we enrich the theoretical analysis introducing risk dominance as an equilibrium selection criterion, following its recent application to indefinitely repeated social dilemmas (Bigoni et al., 2019; Blonski et al., 2011; Camera et al., 2020; Dal Bó and Fréchette, 2018). The approach we take is based on the analysis in Bigoni et al. (2019, Proposition 3), which shares a design close to ours. The main result is summarized in the following:

Observation 2. *The grim strategy with $e^* = b$ is risk dominant in fixed pairs but not necessarily in large groups where it requires:*

$$\delta \geq \tilde{\delta}(b, \hat{e}) := \frac{-p^{N-1} f(b, \hat{e}) + \sqrt{p^{2N-2} f(b, \hat{e})^2 + 4b^2(1 - p^{N-1})}}{2b(1 - p^{N-1})},$$

where $\tilde{\delta}(b, \hat{e}) > \bar{\delta}(b, \hat{e})$.

To derive this result, consider uncertainty over just two strategies, the grim strategy $e^* = b$ supporting full cooperation, and the diametrically opposed strategy of full defection. Denote the first strategy C and the second D. Suppose players see each strategy as being selected with probability p. The strategy that delivers the highest expected payoff is said to be risk dominant.[13] Denoting V_C and V_D the payoffs in the repeated game for an initial donor who chooses strategy C and D, we have:

$$V_D = b + \delta \frac{a + \delta b}{1 - \delta^2} \qquad \text{and} \qquad V_C = p^{N-1} \delta \frac{a + f(b, \hat{e})}{1 - \delta^2} + (1 - p^{N-1}) \delta \frac{a + \delta b}{1 - \delta^2}.$$

The second equation shows that the initial donor selects $e = b$ (earning zero payoff in the round) and switches to full defection if some other donor defects in the first round. This reaction is part of the sanction prescribed by the grim strategy. Given the beliefs of this player, a defection occurs with probability p^{N-1} since there are $N - 1$ other initial donors. In that case no matter how

[13] Our notion of risk dominance follows the conceptual argument that motivates the analysis in Harsanyi and Selten (1988). Simply put, an equilibrium is risk-dominant if it maximizes the expected payoff given that players have uniformly distributed second order beliefs on the best and worst equilibrium, which in our case are full cooperation and full defection.

many other donors have selected strategy C or D, everyone plays $e = 0$ in the continuation game. The first equation is similarly interpreted. The donor starts with $e = 0$ and no matter how many other donors have selected each strategy, everyone plays $e = 0$ in the continuation game.

We say that the grim strategy is risk dominant if

$$V_C \geq V_D \quad \Rightarrow \quad p^{N-1}\delta\frac{f(b,\hat{e}) - \delta b}{1-\delta^2} \geq b.$$

Recall that $\delta = 0.8$ in the experiment. Direct calculation reveals that the above inequality is satisfied for $N = 1$, for any probability $p > 0$. That is, given the experimental parameters, the grim strategy with $e^* = b$ is risk dominant in fixed pairs. The reason is that there is no strategic uncertainty in fixed pairs.

However, for $N > 1$, this inequality implies $\delta \geq \tilde{\delta}(b,\hat{e}) > \bar{\delta}(b,\hat{e})$. For all $\hat{e} = 1, 4, .5, 6$, direct calculation reveals that $\tilde{\delta}(b,\hat{e}) = 0.976$ when $p = 0.5$ (principle of insufficient reason), while $\tilde{\delta}(b,\hat{e}) < 0.8$ only when p is approximately above 0.76. In words, for the grim strategy to be risk dominant in large groups, players must believe that there is a strong bias toward the grim strategy. As this applies to all treatments, this suggests cooperation differentials should be observed between strangers and partners' settings.

A third behavioral considerations is related to possible differential impact of choice flexibility across treatments. This is motivated by extending the risk-dominance result to grim strategies that involve partial cooperation rates, $0 < e^* < b$, leading to an additional observation:

Observation 3. *The grim strategy with $0 < e^* < b$ is more likely to be risk dominant in large groups of the Low treatment as compared to High.*

To see this, consider a grim strategy with interior effort level $0 < e^* < b$.

The associated expected payoff is

$$V_C(e^*) = b - e^* + p^{N-1}\delta\frac{a + f(e^*, \hat{e}) + \delta(b - e^*)}{1-\delta^2} + (1 - p^{N-1})\delta\frac{a+\delta b}{1-\delta^2}.$$

For $N > 1$ we have $V_C(e^*) \geq V_D$ whenever

$$\delta \geq \tilde{\delta}(e^*, \hat{e}) := \frac{-p^{N-1}f(e^*, \hat{e}) + \sqrt{p^{2N-2}f(e^*, \hat{e})^2 + 4(e^*)^2(1-p^{N-1})}}{2e^*(1-p^{N-1})}.$$

This discount threshold now depends on the treatment because the function f is not always linear for interior strategies. In other words, risk dominance will involve different thresholds in different treatments. We say that risk dominance is more likely in a treatment where this threshold is the smallest.

Table 3: Threshold discount $\tilde{\delta}(e^*, \hat{e})$ under risk dominance

	Probability p of selecting the grim strategy									
	.5	.55	.6	.65	.7	.75	.8	.85	.9	.95
Effort $e^* = 6$										
All treatments	.976	.962	.941	.912	.873	.820	.754	.674	.583	.489
Effort $e^* = 3$										
High	.992	.987	.980	.970	.955	.935	.908	.870	.819	.751
Linear	.976	.962	.941	.912	.873	.820	.754	.674	.583	.489
Low	.961	.938	.904	.858	.798	.723	.635	.540	.445	.359
Effort $e^* = 1.5$										
High	.992	.987	.980	.970	.955	.935	.908	.870	.819	.751
Linear	.976	.962	.941	.912	.873	.820	.754	.674	.583	.489
Low	.931	.891	.835	.762	.674	.574	.472	.377	.297	.232

Notes: Calculations refer to large economies where the *High* treatment corresponds to $\hat{e} = 4.5$, *Linear* to $\hat{e} = 6$, and *Low* to $\hat{e} = 1$.

Table 3 reports $\tilde{\delta}(e^*, \hat{e})$ as a function of $p \geq 0.5$, for effort levels $e^* = 1.5, 3, 6$. The threshold $\tilde{\delta}(e^*, \hat{e})$ clearly varies across treatments for interior e^* levels. In particular, we generally have smaller thresholds $\tilde{\delta}$ in *Low* $(\hat{e} = 1)$ as compared to *High* $(\hat{e} = 4.5)$ where the grim strategy is risk dominant only for p close to 1. This suggests that, if strategic uncertainty is a behavioral obstacle to cooperation in large economies, then we should expect higher cooperation rates in *Low* as compared to *High*.

3.3 Testable hypotheses

Based on Section 3.1, we put forward three testable hypotheses, each associated to a behavioral alternative hypothesis derived from Section 3.2.

H 1. *Adding interior choices does not increase average cooperation relative to the Baseline treatment.*

Full cooperation is equally possible in fixed pairs and large groups, in all treatments (Section 3.1). Expanding the choice set relative to the *Baseline* treatment does not remove any of the equilibria available in *Baseline* and only adds Pareto-dominated equilibria that involve partial cooperation, in every economy. The socially efficient outcome is a subgame perfect equilibrium in all treatments. Hence, payoff-maximizing players can select this equilibrium in all treatments and all economies, whether interior choices are available or not. As this holds for fixed pairs as well as large groups, standard equilibrium analysis suggests we should not expect that adding flexibility of choice will affect cooperation. A *behavioral alternative hypothesis* is that adding interior choices might have a positive impact on cooperation in comparison to *Baseline* (Observation 1). It is also possible that the impact might be moderated by the increase in the size of the choice sets, if this increases coordination difficulties or if this presents cognitive challenges for some subjects.

H 2. *Adding interior choices does not have a differential effect in large groups versus pairs.*

Fixed pairs can support cooperation based on reciprocity and reputation, while large groups cannot. Adding interior choices does not alter this fundamental difference, because it does not alter the informational and matching structure of the economies (Section 3.1). Hence, if reciprocity and reputation differentials create a cooperation gap between fixed pairs and large groups of our Baseline, then we do not expect this gap to be affected by choice flexibility.

A *behavioral alternative hypothesis* is that adding interior choices might have a differential effect in large groups versus fixed pairs. The reason is the strategic uncertainty present in large groups (but not in fixed pairs), which can act as an additional possible channel creating cooperation gaps (Observation 2). Flexibility of choice might affect this channel and, hence, differentially impact cooperation in large groups vs. fixed pairs.

H 3. *The shape of the function f should not affect average cooperation.*

Section 3.1 proved that all functions f considered in the experiment support the fully efficient and inefficient equilibria ($e = b$ and $e = 0$). Importantly, the payoffs associated with these two equilibria do not vary across treatments, i.e., are invariant to the shape of f. It is true that in treatments where choices are flexible there exist partial cooperation equilibria ($0 < e < b$, see Corollary 1). The payoffs associated with these equilibria do depend on the shape of the function f, but are always lower than the full cooperation payoff. Hence, the shape of f should not affect the motivation to coordinate on full cooperation. A *behavioral alternative hypothesis* is that the shape of f might affect cooperation in large groups due to strategic uncertainty. The reason is that the non-linearity of f makes partial cooperation more likely to be risk dominant in *Low* as compared to *High* (Observation 3). An additional reason is that subjects might also take into account how much surplus is lost under partial cooperation relative to full. If choosing a modest cooperation level generates small surplus losses, then there might be little motivation to fully cooperate. However, the motivation to sanction a low cooperation effort might also be modest. In this case, the shape of f would also matter; our design allows us to determine which of these two motivations is dominant.

Outside of these three hypotheses, we also conduct an exploratory analysis motivated by the behavior heterogeneity observed in other indefinitely repeated social dilemmas experiments (e.g., Camera et al., 2012; Fudenberg et al., 2012). These experiments report three broad categories of behavior (or, "player types"): free-riders, unconditional cooperators, and conditional cooperators. It is an open question if the distribution of player types is affected by the environment and, in our particular case, by varying the choice set introducing partial cooperation. Thanks to our random assignment to treatments, we can uncover if and how the distribution of types is affected across treatments.

4 Results

Here we report the main results that emerge from the analysis of the experimental data. We focus on supergames 1-4 because by doing so we have a balanced sample of observations (two supergames for fixed pairs and for large groups, each). We also check the robustness of our results to including supergame 5—where groups were only large.

We start by giving an aggregate view at the economy level. We normalize effort choices dividing them by 6. The normalized variable $e/6$ defines a cooperation level between 0 and 1. We then calculate average cooperation levels in a generic economy i as follows:

$$\frac{1}{T_i}\sum_{t=1}^{T_i}\sum_{j=1}^{N/2}\frac{e_{jt}/6}{N/2} \in [0,1].$$

Here, $N/2$ is the number of donors in the economy, T_i is the realized duration of the supergame for economy i, and $e_{jt}/6$ is the cooperation level of generic donor $j = 1, \ldots, N/2$ (of that economy) in period $t = 1, \ldots, T_i$.

Table 4 reports summary statistics for economies in games 1-4 and in game 5, separately: average cooperation in all periods (*All*), in the first period (t=1), and the number of economies where $e = b$ in all pairs and all periods (100%), which is the socially efficient outcome.

Table 4: Cooperation: Summary Statistics

	Fixed Pairs Games 1-4			**Large Groups (12 players)** Games 1-4			Game 5		
Treatment	All	$t = 1$	100%	All	$t = 1$	100%	All	$t = 1$	100%
Baseline	0.81	0.78	0.51	0.44	0.65	0	0.47	0.58	0
	(.03)	(.04)		(.05)	(.06)		(.08)	(.07)	
High	0.70	0.77	0.39	0.43	0.52	0	0.44	0.60	0
	(.04)	(.04)		(.05)	(.06)		(.06)	(.07)	
Linear	0.83	0.80	0.57	0.58	0.60	0	0.64	0.80	0
	(.03)	(.04)		(.04)	(.03)		(.04)	(.05)	
Low	0.83	0.80	0.52	0.62	0.69	0	0.62	0.78	0
	(.03)	(.03)		(.06)	(.07)		(.07)	(.09)	

Notes: Unit of observation: an economy. *All*: all-periods average of normalized effort $e/6$; *t=1*: average of period 1 normalized effort. *100%*: fraction of economies where $e = b$ in all pairs and all periods. Standard errors in parentheses.

Realized (social) efficiency in economy i is in Table 5 and corresponds to:

$$\frac{1}{T_i}\sum_{t=1}^{T_i}\sum_{j=1}^{N/2}\frac{f(e_{jt}) - e_{jt}}{(N/2)(f(b) - b)} \in [0, 1].$$

If donor j chooses e_{jt}, then the economy creates $\sum_{j=1}^{N/2}(f(e_{jt}) - e_{jt}) \geq 0$ surplus in period t. In all treatments, the economies can attain the same maximum surplus $(N/2)(f(b) - b) = 4.5N$ points, in every period. Realized efficiency is reported in Table 5 (standard errors in parentheses). It is calculated as the ratio between average realized surplus and maximum surplus, ranging from 0% to 100%. In all economies full cooperation implies 100% efficiency, corresponding to a total payoff of 18 points in a meeting (18 to the recipient and 0 to the donor). Full defection implies 0% efficiency, corresponding to a total payoff of 9 points in a meeting (3 to the recipient and 6 to the donor).

Table 5: Realized Efficiency: Summary Statistics

	Fixed Pairs Games 1-4		**Large Groups (12 players)** Games 1-4		Game 5	
Treatment	All	$t = 1$	All	$t = 1$	All	$t = 1$
Baseline	0.81	0.75	0.44	0.46	0.47	0.58
	(0.03)	(0.09)	(0.05)	(0.125)	(0.08)	(0.07)
High	0.68	0.53	0.38	0.44	0.41	0.59
	(0.04)	(0.10)	(0.05)	(0.08)	(0.07)	(0.07)
Linear	0.83	0.64	0.58	0.45	0.64	0.8
	(0.03)	(0.09)	(0.04)	(0.04)	(0.04)	(0.05)
Low	0.86	0.68	0.70	0.41	0.7	0.84
	(0.03)	(0.09)	(0.05)	(0.05)	(0.06)	(0.07)

Since the return from cooperation f varies by treatment, partial cooperation levels do not generate identical payoffs across treatments and, hence, identical realized efficiency. For instance, if every donor selects $e = 1.5$ in every period, then realized efficiency is 8%, 25% and 75% for *High*, *Linear* and *Low*, respectively. Instead, if every donor selects $e = 5$ in every period, then realized efficiency is 94%, 83% and 94% respectively. This explains why realized efficiency is proportional to average cooperation in *Baseline* and *Linear*, and it is slightly below average cooperation in *High* and slightly above in *Low*.

To start we document that in the binary-choice *Baseline*, cooperation is difficult when reciprocity is ruled out by design. In this manner, *Baseline* replicates previous results (e.g., Bigoni et al., 2019; Camera and Casari, 2009; Camera et al., 2013) and offers a meaningful benchmark to study cooperation-enhancing institutions in groups of strangers.

Result 1. *In Baseline, average cooperation and realized efficiency are higher in fixed pairs as compared to large groups.*

Evidence appears in Table 4. Fixed pairs in *Baseline* attained 81% cooperation on average, while large groups about half that level. Consequently, realized efficiency was also lower in large groups as compared to fixed pairs; see Table 5. The statistical significance of these results is established using a GLM regression with a logit link function (i.e., a fractional logit model), estimating all treatments and economy sizes jointly, for games 1-4; see the estimated coefficient on the *Size 12* regressor in Table B1 of Appendix B.[14]

Result 1 refers to economies where subjects must exclusively rely on extreme actions to simultaneously motivate "good" and discourage "bad" behavior. We now examine if manipulating the choice set—adding interior partial-cooperation choices—has an effect. Does long-run cooperation increase when we introduce flexibility in choice *within* an interaction? We provide an answer by studying fixed pairs and groups of strangers separately. By studying fixed pairs, we can ascertain if flexibility in choice can complement the classical mechanism for cooperation of reciprocity and reputation. By studying large groups, we can determine whether flexibility in choice can enhance cooperation in the absence of these two classical mechanisms for cooperation.

Result 2. *Relative to the binary-choice Baseline, adding interior choices often increased cooperation and realized efficiency in large groups, but never did so in fixed pairs.*

Based on Result 2 we can reject H1 for large groups but not fixed pairs. As a consequence, H2 is also rejected. Consider games 1-4 in Tables 4 and 5. In fixed pairs, overall cooperation was 0.70, 0.83, and 0.83 in *High*, *Linear*,

[14]This estimation strategy is suitable as in our design all subjects play in fixed pairs and in large groups, in a session. To take into account the uncertainty associated with the sampling variation of our estimates, we also estimated this model specification bootstrapping the clustered standard errors. The coefficient on *Size 12* remains significant. Furthermore, when we use an econometric specification that takes into account the panel structure of the data and time fixed effects, the inferences do not change. These analyses, as well as additional robustness checks, are reported in Table B2, Appendix B.

and *Low*, respectively, which is not too different from the 0.81 of *Baseline*. Instead, in large groups we have 0.43, 0.58, and 0.62, respectively, levels that are generally above the 0.44 of *Baseline*. The same difference emerges if we consider game 5. The statistical significance of these observations is assessed using Table A1 in Appendix A, which reports the marginal effects from the aforementioned GLM regression in Table B1.

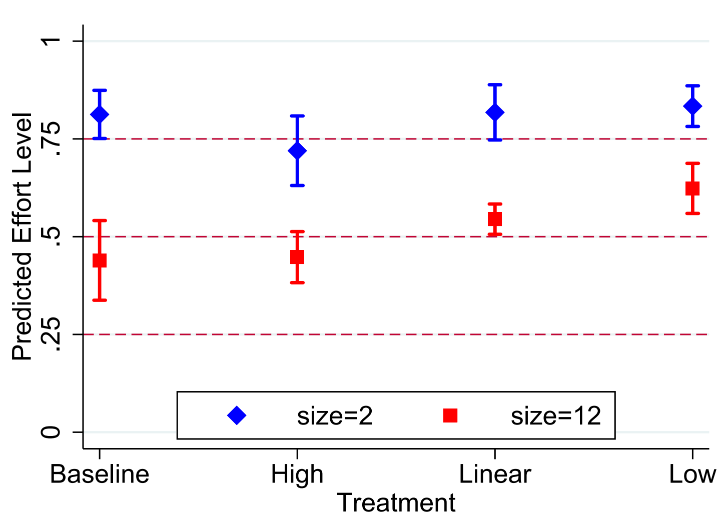
Fig. 3 plots the predictive margins for cooperation with 95% confidence intervals. Cooperation did not significantly increase in fixed pairs of any treatment as compared to *Baseline*. In fact, it decreased in *High*, though not significantly. Instead, cooperation significantly increased in large groups of *Linear* and *Low* as compared to *Baseline* (p-value: 0.06 and 0.004, respectively, Wald test using the marginal effects from Table A1); the difference is insignificant for *High*. The impact of choice flexibility on realized efficiency is qualitatively the same as for cooperation; see Table A1 and Fig. A1 in Appendix A. Table A2 in Appendix A reports p-values for all possible pairwise comparisons of marginal effects across treatments, for cooperation and efficiency.

Result 2 is reinforced when we consider (i) the first choice in a supergame and (ii) the dynamics of cooperation within a supergame. Introducing interior choices improved cooperation from the start of the game; we estimated the probabilities of defection ($e = 0$) and of cooperation ($e = 6$) in the first period of the supergame, in large groups.[15] Columns 1 and 2 in Table B4 of Appendix B show the estimated marginal change in the probability of defection and cooperation, relative to *Baseline*. The probability of defection significantly

[15] We use a logit specification with robust standard errors clustered at the session level, including a categorical variable for treatments, an indicator variable for supergame 5, and standard controls: an indicator variable for the order of play (large groups first, or fixed pairs first), total duration of all previous supergames, and individual characteristics (study major, sex, and number of wrong answers in the incentivized post-instruction quiz).

declines by about 20 percentage points in both *Linear* and *Low*.

Figure 3: Predictive Margins for Average Effort

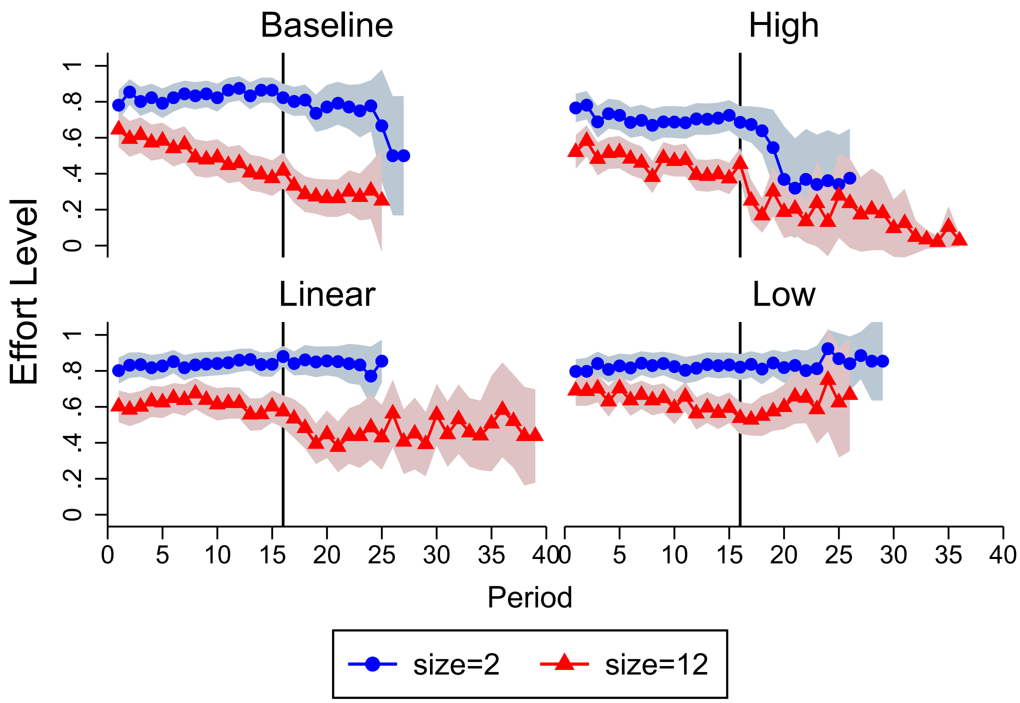


Notes: Unit of observation: an economy. Supergames 1-4 only (N=96 and N=16 per treatment for, respectively, groups of size 2 and 12). Each marker reports the mean normalized effort level with a 95% predictive margin generated using a GLM regression with robust standard errors clustered at the session level; see Table A1 in Appendix A. All treatments are jointly estimated. The regression includes treatment dummies interacted with a size dummy taking value 1 in supergames where size=12 (0, otherwise). The following additional controls are included: a dummy for each supergame 2-4, a dummy that controls for order effects (1, if supergames 1 and 3 have groups of 12, and zero otherwise), supergame duration, duration of previous supergame, and individual characteristics (major of study, sex, and the number of wrong answers in the incentivized understanding quiz).

The decline in *High* is insignificant (column 1). Instead, the probability of cooperation does not significantly vary in any of the treatments (column 2).

The effect of flexibility of choice in large groups is not limited to the first period of the supergame but also has a long-lasting effect. Two observations provide support. Consider choices across periods in a supergame; see Fig. 4.

Figure 4: Cooperation in small and large groups within a supergame



Notes: Unit of observation: an economy. Supergames 1-4. Each figure reports the average observation in a period, and the corresponding 95% confidence intervals. The vertical line at period 16 represents the number of fixed periods in the supergame. Confidence intervals widen past period 16 as the number of observations declines.

Cooperation remains high and stable in pairs independent of treatment, while in large groups we see treatment effects. As is typical in these indefinitely repeated social dilemmas, in large groups cooperation tends to decline (see *Baseline*). This decline was less pronounced in *Linear* and *Low* where, in fact, cooperation tended to stabilize over time. That is to say, the treatment effects appear to be long-lived within the supergame.

Now consider the distribution of choices in the last supergame; see Fig. B2 in Appendix B. *Linear* and *Low* first order stochastically dominate *Baseline* and *High*. This shows that flexibility of choice had a long-lasting effect on cooperation. As an additional check, we estimate a random effects model with time fixed effects as well as similar models with a richer set of covariates (see Table B2, Appendix B); the estimated time regressors show a stable effect for fixed pairs and a convex decline for large groups.

Naturally, because the treatment intervention raised cooperation in large groups but not in fixed pairs, the efficiency gap between large groups and pairs also shrank. Seen this way, our experiment shows that adding an *intensive margin* to cooperation choices can be a valuable tool to improve coordination on long-run cooperation. Yet, Tables 4-5 suggest that such intervention was not equally successful in every treatment. Hence, we can reject H3.

Result 3. *Adding interior choices increases cooperation in large groups of Linear and Low, but not in High.*

Evidence for this result comes from Wald tests on the coefficients from the marginal effects in Table A1; the various tests are summarized in Table A2. In both fixed pairs and large groups, the effect of *High* on cooperation is smaller than the other two treatments (p-values < 0.064). Furthermore, there is no difference between *Linear* and *Low* (p-values > 0.1). The impact on realized efficiency follows a similar pattern; see Table A2.

Whether or not adding interior choices boosts average cooperation depends on the shape of the returns from partial cooperation. The intervention succeeded in treatments where the return from moderate cooperation did not fall relative to full cooperation, i.e., when moderate efforts did not dissipate too much of the potential surplus. This is not the case in *High*, where the return from cooperation sharply fell as soon as players selected effort levels below 5 points. This inefficiency in transforming partial cooperation into surplus seems to generate a negative feedback effect on the motivation to cooperate.

Summing up, the analysis reveals that cooperation in groups of strangers is affected by flexibility of choice, under certain conditions. By contrast, fixed pairs are unaffected. Given these findings, the natural question is: How did the addition of interior choices affect behavior in groups strangers?

To address this question, we investigate how flexibility of choices affected behavior using a finite-mixture model to estimate categories of behavior (or, player types)—conditional and unconditional on past actions—and their distribution in the population. Our random assignment allows us to determine if and how our treatments affect this distribution. In particular, we can determine if there are individuals who methodically defect without ever attempting to cooperate, and if our interventions impact the share of these individuals.

Result 4. *The treatments affect the distribution of player types in large groups. Compared to Baseline: (i) there are less uncooperative types in all treatments; (ii) more cooperative types emerge in Linear and Low, but not in High.*

Evidence comes from estimating a Finite Mixture Model (FMM) where the unit of observation is a player in a period $t \geq 2$ of supergames 1-4. The estimation is based on maximum likelihood. See McLachlan and Peel (2000) for a standard reference, and see Bruhin et al. (2019) for an application to estimating social preferences from experimental data.

According to FMM, the economy is populated by K (player) types. Since the indefinitely repeated game admits a large number of strategies, one possible approach to estimate play is to put restrictions on these types so that their play must fall within a specific subset of possible strategies. However, this is problematic because there is no general consensus on what subset of strategies one should consider. Another approach is to not impose that types are restricted to adopt specific strategies, and instead allow for two very general types of behavior, conditional and unconditional on counterparts' choices. We follow this second route, which we then improve upon by estimating models enriched by imposing that some types also exist who play specific strategies.

More concretely, we estimate models where the choices of any player type are explained by the following covariates: (i) the "Counterpart" covariate, indicating how a donor in t responds to the effort of her donor counterpart in $t-1$; (ii) the "Group" covariate, indicating how the donor responds to the effort observed in all other meetings in $t-1$ (excluding her own); and (iii) the categorical variable "Repeat" taking values 1 or 2, depending on whether it is the first or the second supergame that the participant played in that group configuration, in the session.[16]

The first model we estimate has $K = 1$, i.e., it is a benchmark representative-agent model corresponding to a fractional logit model (as the ones used earlier). Hence, we define the right hand side of the regression as

$$\begin{aligned}\boldsymbol{x}'\boldsymbol{\beta} :=& \beta_0 + \beta_1 Repeat + \beta_2 Counterpart \\ &+ \beta_3 Counterpart \times Repeat + \beta_4 Group + \beta_5 Group \times Repeat.\end{aligned}$$

We do not want to impose that players behave identically in both supergames, hence, we interact the categorical variable "Repeat" with the "Counterpart" and "Group" covariates. Therefore, the vector of estimated parameters allows us to determine not only how the representative agent behaves, but also whether this behavior changes with experience.

To explain how to interpret the coefficients, a player who does not condition her cooperation on the behavior of others (=unconditional type) and behave

[16]The Counterpart and Group covariates are standardized (centered around their respective averages). This provides a measure of sensitivity and, since the covariates' mean value is zero, allows us to estimate the subject's intrinsic motivation to cooperate through the constant. In addition, because we focus on the first two supergames, the variable Repeat is a linear transformation of an indicator variable, so we adopt the usual interpretation of the associated parameters.

identically across supergames should have $\beta_i = 0$ for all $i > 1$. Her intrinsic motivation to cooperate is measured by β_0. Given the fractional logit model specification, a negative β_0 would imply an effort below 50%, while a positive β_0 an effort above 50%. If this player's intrinsic motivation changes in the second supergame, then the estimated β_1 should be different than zero.

Now consider a player who, as a donor, cooperates based on the behavior of others (=conditional type). Our model allows us to estimate how much importance this player places on the choices of her counterpart in the previous period vs. everyone else in the economy. If this player's cooperation is based on what their previous counterpart did, then we should have $\beta_2 \neq 0$ and $\beta_2 + \beta_3 \neq 0$, respectively, for the first and second large economy she participated in. Similarly, if she bases her cooperation on the effort of all other donors in the previous round, then we should have $\beta_4 \neq 0$ and $\beta_4 + \beta_5 \neq 0$, respectively.

To incorporate potential heterogeneity in behavior, we also estimate models where we allow $K \geq 2$ different types. Here, we must estimate K vectors of parameters $\boldsymbol{\beta}$ and to keep the model parsimonious, we impose that these K types exist in all treatments, although their distribution may vary.[17] More concretely, it is assumed that, although subjects belong to a type $k = 1, \ldots, K$, the classification of individual choices into one of these types is latent. A latent class indicator $c_k = 0, 1$ determines the type to which observations belong to. Furthermore, for each subject, the ex-ante probability of an individual choice belonging to type k is determined by the probability π_k. We assume that this probability is a function of the treatment indicator variables $\boldsymbol{z}$, according to a

[17] Our working hypothesis here is that experimental subjects belong to pre-existing behavioral types, which are unaffected by the specific experiment they volunteer for. Given random assignment to treatment, this is a natural working hypothesis, which is also useful to estimate possible differences in behavior outside the laboratory.

multinomial logistic distribution, i.e.,

$$\pi_k = \Pr(c_k = 1 \mid \boldsymbol{z}) = \frac{\exp(\boldsymbol{z}'\boldsymbol{\gamma}_k)}{\sum_{j=1}^{K} \exp(\boldsymbol{z}'\boldsymbol{\gamma}_j)} \qquad \text{for } k = 1, \ldots, K.$$

Here, $\boldsymbol{\gamma}_k$ denotes the vector of coefficients that must be estimated. Let $h_k(\boldsymbol{e} \mid \boldsymbol{x}, \boldsymbol{\beta})$ denote the conditional joint density function that a given vector of observed efforts $\boldsymbol{e}$ belongs to type k. The likelihood function is

$$\mathcal{L}(\boldsymbol{\beta}, \boldsymbol{\gamma}) = \sum_{k=1}^{K} \pi_k(\boldsymbol{z}, \boldsymbol{\gamma})\, h_k(\boldsymbol{e} \mid \boldsymbol{x}, \boldsymbol{\beta}).$$

Thus, we use maximum likelihood estimation to uncover the parameters $\boldsymbol{\beta}$ that characterize each type, and the parameters $\boldsymbol{\gamma}$ governing the relative frequency of that type in the population across treatments.

The question at this point is how many types K should be estimated, because the FMM estimation does not determine endogenously this number. This choice is left to the modeler. Hence, to determine K we consider two criteria that have been proposed in the literature. The first is called NEC, Normalized Entropy Criterion, first proposed by Celeux and Soromenho (1996). This criterion considers the relative quality of the classification—the separation between ex-post probabilities of belonging to each type—and the additional information from having $K > 1$ types rather than one. The second called ICL-BIC, Integrated Completed Likelihood-Bayesian Information Criterion, combines the well-know BIC criterion and adds a further penalization for poorly separated types (Biernacki et al., 2000; McLachlan and Peel, 2000).[18]

These two criteria suggest that we should stop at a model with 2 types,

[18] Other methods are discussed in the literature, but there is no consensus on which one is more informative. A common method is the Akaike Information Criterion (AIC). In the context of FMM, evidence suggests a poorer performance of AIC and BIC relative to NEC to determine the optimal number K (e.g., Celeux and Soromenho, 1996). Biernacki et al. (2000) offer evidence suggesting that ICL-BIC outperforms these other criteria.

without moving to a model with 3 types. At the bottom of Table 6, criteria with lower numbers indicate a more preferred model. The top of the Table reports the estimation results. The first three columns refer to models with 1, 2 and 3 types. The last two columns, refer to models estimated as a robustness check—where we constrain some types, as we discuss later.

Col. 1 reports the estimation of a representative-agent model, $K = 1$. This representative type has a natural inclination to choose low effort (*Constant*=0.364), which is slightly (but insignificantly) reinforced with experience in the game (*Repeat*=-.150). This representative agent is classified as a conditional cooperator who similarly reacts to the actions of direct counterparts (*Counterpart*=.621) and the other counterparts in her group (*Group*=.774); this behavior does not significantly change with experience (*Repeat* $\times$ *Counterpart*=-.031 and *Repeat* $\times$ *Group*=-.069).

Col. 2 refers to the two-types model, both of whom are classified as conditional cooperators. Type 1 resembles the representative agent (see above) and can be described as a *closet opportunist*. She has low intrinsic motivation to cooperate (*Constant* = -2.153), and motivation increases from meeting a cooperative counterpart or being in a cooperative group (*Counterpart* = .738 and *Group* = 1.544). This induced extrinsic motivation is insufficient to significantly overcome the intrinsic uncooperative bias (the sum of coefficients on *Counterpart* and *Group* is similar to *Constant*). This behavior appears to be stable: the variables associated with experience are insignificant (*Repeat*=-.438; *Repeat* $\times$ *Counterpart* = .62; *Repeat* $\times$ *Group* = -.118).

Type 2 can be described as a *responsive cooperator*: there is a strong natural inclination to cooperate (*Constant* = 10.048), a strong response to the counterpart's actions (*Counterpart* = 6.956), and a weak response to group behavior (*Group* = .931).[19] Experience in the first supergame negatively affect behavior in the second: intrinsic motivation declines (*Repeat*=-7.908) as well as the desire to cooperate in response to a cooperative action (*Repeat* $\times$ *Counterpart* = -5.841). In a way, this is a player who starts with all good intentions, but is quickly disappointed by the behavior of those around her.

The main take-away is that types 1 and 2 are very different in their cooperation levels: the estimation implies predicted efforts of, respectively, 0.259 and 0.846. This picture would not change if we used a model with three types (col. 3) as the coefficients are broadly consistent with the presence of "closet opportunist" and "responsive cooperator" types.[20]

[19]This cannot be ascribed to direct or indirect reciprocation. It is not direct reciprocity because the reaction to a cooperative action benefits a different counterpart. It is not indirect reciprocity because counterparts' past behavior is unobservable.

[20]The sign of the relevant significant coefficients in col. 2 and 3 is the same, and their size are quite similar.There is a new third type of player, who learns to cooperate during the session: she consistently free-rides in the first supergame but conditionally cooperates in the second.Yet, including a third type reduces the model performance.

Table 6: Endogenous Types According to FMM Estimation.

		Unconstrained models			Constrained models	
	Regressor	1 type	2 types	3 types	3 types	4 types
Type 1	Repeat	-.150	-.438	-.045	-.142	.466
		(.103)	(1.025)	(1.855)	(.142)	(.970)
	Group	.774***	1.544***	1.974***	.961***	1.608**
		(.086)	(.403)	(.558)	(.172)	(.734)
	Counterpart	.621***	.738***	1.128***	.822***	.748***
		(.048)	(.218)	(.273)	(.098)	(.263)
	Repeat × Group	-.069	-.118	-1.023	-.181	-.261
		(.109)	(.524)	(.673)	(.162)	(.573)
	Repeat × Counterpart	-.031	.620	.373	-.074	.377
		(.056)	(.458)	(1.835)	(.089)	(.259)
	Constant	.364***	-2.153**	-2.135**	.013	-2.176
		(.098)	(.866)	(.924)	(.330)	(1.481)
Type 2	Repeat		-7.908**	-8.233**		-7.200*
			(3.201)	(3.258)		(3.796)
	Group		.931***	1.233***		.974***
			(.202)	(.226)		(.231)
	Counterpart		6.956***	7.104***		6.032**
			(2.418)	(2.335)		(2.764)
	Repeat × Group		.007	-.377		-.466
			(.391)	(.442)		(.323)
	Repeat × Counterpart		-5.841**	-6.170***		-5.386*
			(2.293)	(2.312)		(2.827)
	Constant		10.048***	10.122***		8.170**
			(3.453)	(3.333)		(3.894)
Type 3	Repeat			9.421		
				(6.053)		
	Group			-3.892		
				(2.882)		
	Counterpart			-9.050**		
				(3.710)		
	Repeat × Group			7.206*		
				(3.896)		
	Repeat × Counterpart			10.434***		
				(3.946)		
	Constant			-14.010***		
				(5.402)		
NEC		—	**26.84**	29.20	28.01	42.59
ICL-BIC		—	**17555.78**	19870.68	19416.95	24988.36

Notes: Unit of obs.: a subject in a period (large groups, supergames 1-4, $N = 7752$, number of subjects =384). Standard errors clustered at the individual level, in parentheses. Unconstrained models: all types are estimated from the data. Constrained models: two types are constrained to behave as unconditional defectors and unconditional cooperators, by setting the intercept to -100 and 100, respectively, to avoid numerical overflow in the estimation; types 1 and 2 are estimated from the data. The lowest number for each information criterion, NEC and ICL-BIC, is in bold. We also calculated BIC and AIC, which also recommend an unconstrained model (BIC recommends 2 types, AIC recommends 3 types).

None of the estimations identify types consistent with unconditional defectors and unconditional cooperators—players who consistently defect or cooperate in all supergames. However, if we look at the raw data we do see individuals who unconditionally select one of the two extreme choices – but almost no-one who unconditionally selects some interior choice; see Table 7.

Table 7: Unconditional players in the raw data

	Baseline	**High**	**Linear**	**Low**
Always $e = 0$	0.226	0.125	0.111	0.087
Always some $e = e^* \in (0, 6)$	0.000	0.003	0.003	0.024
Always $e = 6$	0.160	0.076	0.184	0.271

The question is thus: how sensitive is the model to imposing the presence of unconditional types? Would the overall picture change? As a robustness check, we thus estimate constrained models where we impose the presence of these two types and allow for a third or fourth type to be present in the population; see the last two columns in Table 6. Following the previous definition, an unconditional type is defined as someone who has $\beta_i = 0$ for all $i > 1$; for computational reasons we impose $\beta_0 = 100$ for an unconditional cooperator and $\beta_0 = -100$ for an unconditional defector. The results are reassuring because the estimation identifies conditional cooperator types that behave quite similarly to those identified in the unconstrained models. This suggests that the unconstrained estimation of conditional cooperators is robust to the possibility that unconditional types are present, although the distribution of types might be affected.[21] Table 8 reports the estimated proportions of types

[21] We thank an anonymous referee for suggesting this analysis. Additional robustness checks are in Table B5 in Appendix B, which shows that the estimated types are robust to controlling for trigger strategies, and for the initial donor/recipient role assignment. Table B6 reports the estimation results when behavior is fixed across supergames.

1 and 2 in the unconstrained two-type model, and the constrained four-type model.

There are two main observations. First, the proportion of the most cooperative types—Type 2 and Unconditional Cooperator—increase in *Low* and *Linear*, as compared to *Baseline*. For example, considering the unconstrained model, Type 2 goes from 39% in *Baseline* to 72.5% in *Low* (Wald test p-value < 0.001), while Type 1 players decline. Second, if we impose the presence of unconditional players (supposing that these players should in fact exist), treatments have little to no effect on the distribution of unconditional defectors. In fact the constrained model uncovers that there is a very small number of these players—the highest share is 4.5% in *Baseline.* By contrast, the constrained model mostly reassigns some players from Type 2 to unconditional cooperators: 10.6% in *Baseline*, 33% in *Linear* and 21.2% *Low.* In other words, moving from an unconstrained to a constrained model mostly shifts weight from conditional to unconditional cooperator types. This shift is small in *Baseline*, but is very large in *Linear* and *Low*, which might explain the higher cooperation observed in those two treatments as compared to *Baseline* (Result 3).

Table 8: The Distribution of Player Types

	Baseline	**High**	**Linear**	**Low**
Unconstr. model—2 types				
Type 1	0.610	0.391	0.312	0.275
Type 2	0.390	0.609	0.688	0.725
Constr. model—4 types				
Uncond. Defector	0.045	0.008	0.000	0.000
Type 1	0.587	0.305	0.348	0.263
Type 2	0.262	0.686	0.322	0.525
Uncond. Cooperator	0.106	0.000	0.330	0.212

Notes: The cells report predicted posterior probability of belonging to a specific type, from the estimation. Types 1 and 2 are conditional cooperators as per Table 6.

Finally, to uncover possible differences in behavior and distribution of the player types, we also performed a separate estimation for fixed pairs, for all three unconstrained model specifications (omitting the variable Group as it no longer applies). The results, in Table A3 in Appendix A, reveal that the preferred unconstrained model still is the one with two types $K = 2$. When we focus on this model, we find two main differences in behavior relative to large groups. Type 1 players—the *closet opportunists*—behave a bit differently than in large groups, while type 2 players—the *responsive cooperators*—behave as they did in large groups. Type 1 players exhibit a more pronounced intrinsic motivation to cooperate with a partner as compared to strangers—probably because negative direct reciprocity is now available (see the coefficients on the constant terms). Yet, their motivation remains weak as compared to that of type 2 players. Another difference is that in fixed pairs there are more players of type 2 and less type 1 (the proportions are reported in the notes to Table A3). These differences are responsible for the increases in average levels of cooperation in fixed pairs, relative to large groups.

5 Discussion

This study contributes to the experimental literature on long-run cooperation among strangers, i.e., settings where individuals cannot rely on reciprocity to coordinate on the cooperative equilibrium. The institutions typically considered in the literature to support cooperation may not always be practically feasible, may be costly, or may not perform as intended. For instance, technologies for monitoring behavior are expensive to set-up and operate, while peer punishment systems can be misused and lower efficiency. It is thus meaningful to explore alternative interventions that can improve cooperation without dissipating most or all of the surplus that the institution is supposed to create.

The intervention considered in this experiment consists of setting subjects free to modulate the intensity of their cooperative effort *within* a period, instead of constraining them to two extremes actions—full and no cooperation—as in the typical binary-choice social dilemma. In our experiment this choice flexibility significantly increased cooperation (Results 1-2), which is remarkable because by design it could not increase potential earnings relative to a binary-choice setting, nor reduce strategic uncertainty. We see no effect in fixed pairs—where the high cooperation rates observed with binary choices did not further increase with flexible choices. Seen this way, flexibility in choice seems to act as a partial substitute for the lack of reciprocity mechanisms to support cooperation. We cannot exclude, however, that it might also prove beneficial in fixed pairs, for stage-game parameters less conducive to cooperation.

Was choice flexibility *per se* sufficient? Simply put, no. The trade-off between cooperation effort and surplus creation proved to be a key motivating factor. In *High*, attaining reasonably high payoffs required more effort as compared to the other treatments, where modest effort was sufficient. As a result, we find that the modest cooperation rates observed in *Baseline* significantly improved in *Low* and *Linear*, but not in *High* (Result 3).

How can we rationalize these results? It is possible that expanding the choice set to include interior choices facilitated coordination on high cooperation levels. We think that this mechanism is not the primary reason behind the cooperation increase observed when moving away from the binary-choice design. The reason is that including interior choices adds Pareto-inferior equilibria, which does not go in the direction of reducing strategic uncertainty or facilitating coordination. A second possibility is that some interior choice served as a focal point for coordination on partial cooperation. Examples of natural salient points are $e = 3$ in *Linear*, as it represents a middle ground and ensures the safe payoff of 3 points in every period; $e = 1.5, 5$ are salient in *Low* and *High*, as at these levels the return from cooperation jumps to a higher level. The data show no evidence that these values served as focal points. If subjects used these salient effort levels as focal points, then we should observe high cooperation in *High* and low cooperation in *Low*, in contrast with our findings. Consider also that in period 1 of a supergame, the frequencies of salient choices $e = 1.5, 3, 5$ are very small in all treatments: between 5 and 8 in fixed pairs (out of 384, per treatment), and 8 to 19 in large groups (out of 576). The frequencies of interior salient choices remain low when we consider all periods; see Fig. B1 for the distribution of choices.

A third possibility is that by selecting full instead of partial cooperation, subjects managed to more strongly demonstrate their trustworthiness and commitment to cooperation (see Gomez-Miñambres et al., 2021). This might have acted as a trust-building mechanism, pushing the more apprehensive individuals, and possibly some poorly motivated ones, to give cooperation a chance. Indeed, the data reveal that the probability of defection ($e = 0$) fell in period 1 of a supergame in large groups, when we introduced choice flexibility, and the share of full defectors declined (Result 4). Moreover, cooperation levels remained higher than in *Baseline* as the supergame progressed. It is in this sense that flexibility in choice might have supported trust among strangers. This may also explain why fixed pairs were unaffected by choice flexibility. In fixed pairs trust is easier to build because, unlike groups of strangers, reciprocity is possible, interaction is not sporadic and uncertain, and counterparts' actions are perfectly monitored.

An important observation is that in our flexible-choice setting individuals are not free to choose the return on partial cooperation, which is exogenously determined. In other words, the “price of cooperation” is exogenously imposed in our design. A natural question is whether allowing individuals the freedom to endogenously select not only the cooperation level in a meeting, but also its price, could help crowding-out free-riding behavior and support coordination on efficient play. We think this is a fruitful avenue of future research about long-run cooperation among strangers.

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A Appendix

A.1 Proof of Proposition 1

Fix a positive effort level $e^* \in E$ and consider the social norm based on the grim strategy profile $(\mathbf{e}_j(e^*, 0))_{j=1}^{2N}$. Here, according to Definition 1, the strategy $\mathbf{e}_j^*$ neither depends on the number of players, $2N$, nor on privately observed actions. This social norm consists of a rule of cooperation $e = e^*$, taken along the equilibrium path, and a rule of punishment $e = 0$, taken off the equilibrium path.

Consider equilibrium payoffs. Discounting starts with the random termination rule, on date $T \geq 1$, so only payoffs from rounds $t \geq T+1$ are discounted at rate $\delta \in (0, 1)$. Let $v_{i,t}(e^*, \hat{e})$ denote the equilibrium payoff in the repeated game at the start of $t = 1, 2, \ldots$ to a player in role $i = 0, 1$.

By direct calculation, letting $h = 1, 2 \ldots$, in equilibrium we have

$$v_{i,t}(e^*, \hat{e}) := \begin{cases} \Pi(e^*, \hat{e}) \times \dfrac{T-t}{2} + v_i(e^*, \hat{e}), & \text{if } \ T-t = 2h \\ \Pi(e^*, \hat{e}) \times \dfrac{T-t+1}{2} + \delta v_i(e^*, \hat{e}), & \text{if } \ T-t = 2h-1, \\ v_i(e^*, \hat{e}), & \text{if } \ T-t \leq 0, \end{cases}$$

and

$$v_i(e^*, \hat{e}) = \frac{\delta^i(b - e^*) + \delta^{1-i}[a + f(e^*, \hat{e})]}{1 - \delta^2}, \qquad \text{for } i = 0, 1.$$

In words, we must consider the two cases $t \geq T$ and $t < T$ separately. Recall that players deterministically alternate between the roles of donor and recipient. Suppose we are in period $t \geq T$. Here the payoff in the repeated game is time-invariant. Direct calculation gives us $v_i(e^*, \hat{e})$. In equilibrium each player puts effort e^* as a donor and receives $f(e^*)$ as a recipient on alternating dates. Omitting the arguments e^* and $\hat{e}$ when understood, the period payoffs are $\pi_0 = b - e^*$ to a donor and $\pi_1 = a + f(e^*, \hat{e})$ to a recipient, appropriately discounted.

Now consider periods $t < T$. Those who are recipients (resp., donors) on $t = 1$ earn π_1 in odd (resp., even) periods and π_0 in even periods (resp., odd). Hence, $v_{i,t}$ depends on whether $T-t$ is odd or even. If it is even, then the player earns Π for $\dfrac{T-t}{2}$ times, which accounts for all periods up to and including $T-1$; in period T the continuation payoff is v_i. Instead, if $T-t$ is odd, then the player earns Π for $\dfrac{T-t+1}{2}$ times, which accounts for all periods up to and including T; in $T+1$ the continuation payoff is v_i, discounted by δ since in T discounting starts (the random termination rule starts). It follows that $v_{i,t}$ falls in T for $i = 0, 1$ and achieves a minimum when $t \geq T$.

To determine the optimality of the proposed strategy we must check that, given $\mathbf{e}^*$ in equilibrium a donor has no incentive to choose $e \neq e^*$, and out of equilibrium has no incentive to select $e > 0$.

Let $\tilde{v}_{i,t}$ denote the continuation payoff to a player in role i on date t, when someone (maybe the player herself) moved off equilibrium in period $t-1$ by $e < e^*$. Recall that this move is publicly observed via the average effort, as $\bar{e}_{t-1} < e^*$. Given the strategy profile $\mathbf{e}^*$, off-equilibrium every donor selects $e = 0$ so $\pi_0 = b$ and $\pi_1 = a$ in all periods. It follows that if $t \geq T$ we have

$$\tilde{v}_{i,t} = \tilde{v}_i = \frac{\delta^i b + \delta^{1-i} a}{1-\delta^2}, \qquad \text{for } i = 0, 1.$$

For $h = 1, 2, \ldots$, the continuation payoff off-equilibrium satisfies

$$\tilde{v}_{i,t} := \begin{cases} (b+a) \times \dfrac{T-t}{2} + \tilde{v}_i & \text{if } \ T-t = 2h \\ (b+a) \times \dfrac{T-t+1}{2} + \delta\tilde{v}_i & \text{if } \ T-t = 2h-1, \\ \tilde{v}_i & \text{if } \ T-t \leq 0. \end{cases}$$

Importantly, payoffs in the repeated game are independent of the size of the group N off equilibrium, because all donors set $e = 0$ immediately after observing a defection from the strategy in Definition 1.

A donor would not deviate to $e > e^*$ because in equilibrium others will not respond by increasing their effort. Hence raising effort above e^* is individually suboptimal, since it simply lowers the payoff π_0 to the donor.

Now consider a deviation to $e < e^*$. The proposed action e^* is a best response for a donor in period $t \geq 1$ if

$$\begin{aligned} & v_{0,t} - (b - e) - \delta \tilde{v}_{1,t} \geq 0 \qquad \text{for all } e < e^*, \\ \Rightarrow \quad & v_{0,t} - \tilde{v}_{0,t} \geq 0, \end{aligned}$$

where the second line follows from noticing that the best deviation is $e = 0$, as $\tilde{v}_{1,t}$ is independent of the deviation observed.

Using the definitions above we have

$$\Delta(e^*, \hat{e}) := v_0(e^*, \hat{e}) - \tilde{v}_0 = \frac{\delta f(e^*, \hat{e}) - e^*}{1 - \delta^2}.$$

Now define

$$\begin{aligned} \Delta_t(e^*, \hat{e}) &= v_{0,t}(e^*, \hat{e}) - \tilde{v}_{0,t} \\ &= \begin{cases} [f(e^*, \hat{e}) - e^*] \times \dfrac{T - t}{2} + \Delta(e^*, \hat{e}) & \text{if } \ T - t = 2h \\ [f(e^*, \hat{e}) - e^*] \times \dfrac{T - t + 1}{2} + \delta\Delta(e^*, \hat{e}) & \text{if } \ T - t = 2h - 1, \\ \Delta(e^*, \hat{e}) & \text{if } \ T - t \leq 0. \end{cases} \end{aligned}$$

It is immediate that $\Delta_{t=T-2h} > \Delta_{t \geq T}$; note that $f(e^*, \hat{e}) - e^* > 0$ by assumption. Similarly, $\Delta_{t=T-2h+1} > \Delta_{t \geq T}$. Hence, Δ_t attains a minimum for $T - t \leq 0$. It follows that $e = e^*$ is a best response for all t whenever it is a best response for $t \geq T$. The latter requires

$$\Delta(e^*, \hat{e}) \geq 0 \qquad \Rightarrow \qquad \delta \geq \bar{\delta}(e^*, \hat{e}) = \frac{e^*}{f(e^*, \hat{e})}.$$

Finally, it should be clear that $e = 0$ is always a best response off equilibrium because everyone else plays $e = 0$ in that case. Indeed, indefinite repetition of the static Nash equilibrium—$e = 0$ for every producer—is always a subgame perfect equilibrium of the indefinitely repeated game. This implies that (i) no player would deviate from the action prescribed by the strategy in Definition 1 off equilibrium, and (ii) the strategy profile $\mathbf{e} = (\mathbf{e}_j)_{j=1}^{2N}$ where $\mathbf{e}_j$ is such that the player always chooses $e = 0$ as a donor is also an equilibrium of the original game, corresponding to the "full defection" outcome. It is immediate that this equilibrium outcome has payoffs $\tilde{v}_{i,t}$, as defined above.

We can rank the equilibrium payoffs in the repeated game according to the equilibrium effort. Direct calculation shows that

$$\tilde{v}_{i,t} < v_{i,t}(e^*, \hat{e}) < v_{i,t}(e^{**}, \hat{e}) \qquad \text{for all } 0 < e^* < e^{**}, i = 0, 1, \text{ and } t \geq 1,$$

which is immediately implied from observing that the total payoff $\Pi(e, \hat{e})$ in the stage game is increasing in the effort e.■

A.2 Proof of proposition 2

Fix a grim strategy profile $(\mathbf{e}_j(\epsilon, 0))_{j=1}^{2N}$ for some positive $\epsilon \in E$ with $\epsilon < b$. Corollary 1 establishes that this is an equilibrium in the experiment.

Consider a lenient strategy profile $(\mathbf{e}_j(e^*, \epsilon))_{j=1}^{2N}$ with $e^* = b$. We know that the actions prescribed by the lenient strategy are a best response off equilibrium, since the grim strategy profile $(\mathbf{e}_j(\epsilon, 0))_{j=1}^{2N}$ is an equilibrium of the original game. From the proof of Proposition 1, letting $h = 1, 2 \ldots$, off-equilibrium payoffs are

$$v_{i,t}(\epsilon,\hat{e}) := \begin{cases} \Pi(\epsilon,\hat{e}) \times \dfrac{T-t}{2} + v_i(\epsilon,\hat{e}), & \text{if } \; T-t=2h \\ \Pi(\epsilon,\hat{e}) \times \dfrac{T-t+1}{2} + \delta v_i(\epsilon,\hat{e}), & \text{if } \; T-t=2h-1, \\ v_i(\epsilon,\hat{e}), & \text{if } \; T-t \leq 0, \end{cases}$$

and

$$v_i(\epsilon,\hat{e}) = \frac{\delta^i(b-\epsilon)+\delta^{1-i}[a+f(\epsilon,\hat{e})]}{1-\delta^2}, \qquad \text{for } i=0,1.$$

Now we prove that a player would not deviate from the action $e^* = b$ prescribed in equilibrium. Again, from the proof of Proposition 1, letting $h = 1, 2\ldots$, equilibrium payoffs under the lenient strategy $\mathbf{e}_j(b,\epsilon)$ satisfy $v_{i,t}(b,\hat{e})$. Consider a deviation to $e < b$. We already know that the best deviation is $e = 0$. It follows that the proposed equilibrium action $e^* = b$ is a best response for a donor in period $t \geq 1$ if

$$v_{0,t}(b,\hat{e}) - v_{0,t}(\epsilon,\hat{e}) \geq \epsilon,$$

since $v_{0,t}(\epsilon,\hat{e}) = b - \epsilon + \delta v_{1,t+1}(\epsilon,\hat{e})$.

For $t \geq T$ we have

$$v_0(b,\hat{e}) - v_0(\epsilon,\hat{e}) = \frac{\delta[f(b,\hat{e}) - f(\epsilon,\hat{e})] - (b-\epsilon)}{1-\delta^2}.$$

Following the proof of Proposition 1, we can show that $v_{0,t}(b,\hat{e}) - v_{0,t}(\epsilon,\hat{e})$ is minimized when $t \geq T$. Hence, deviating in equilibrium is suboptimal if

$$\frac{\delta[f(b,\hat{e}) - f(\epsilon,\hat{e})] - (b-\epsilon)}{1-\delta^2} \geq \epsilon.$$

Now fix $\epsilon \leq \hat{e}$. From the definition of f we have:

$$f(b, \hat{e}) = k(\hat{e})b \qquad \text{and} \qquad f(\epsilon, \hat{e}) = k(\hat{e})\epsilon.$$

It follows that deviating in equilibrium is suboptimal if

$$\frac{\delta[k(\hat{e}) - 1](b - \epsilon)}{1 - \delta^2} \geq \epsilon.$$

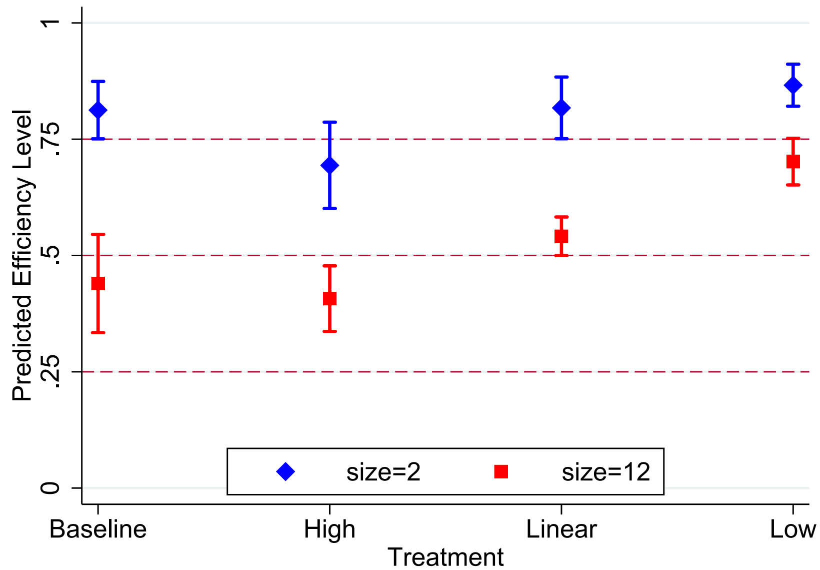
Direct calculation shows that this inequality holds in all treatments where the lenient strategy is feasible, for some $\epsilon \leq \hat{e}$ (e.g., $\epsilon = 0.5, 1$).■

Table A1: GLM Regression: Marginal Effects

Dep. variable:	Cooperation	Efficiency
High		
Size 2	-0.095	-0.122**
	(0.058)	(0.060)
Size 12	0.009	-0.036
	(0.067)	(0.071)
Linear		
Size 2	0.005	0.005
	(0.048)	(0.046)
Size 12	0.116*	0.113*
	(0.062)	(0.065)
Low		
Size 2	0.021	0.051
	(0.040)	(0.037)
Size 12	0.202***	0.288***
	(0.070)	(0.067)
N	448	448

Notes: Unit of observation: an economy in supergames 1-4. Additional controls include: an *order* indicator variable soaking up order effects (1, if supergames 1 & 3 have groups of 12, and zero otherwise), a *Repeat* categorical variable that controls for learning effects (1 if the supergame is 1 or 2, and 2 if supergame is 3 or 4), supergame duration and duration of previous supergame to soak up additional learning effects, and individual characteristics (major of study, sex, and the number of wrong answers in the incentivized understanding quiz). We use a logit link function. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively. Robust standard errors clustered at the session level.

Figure A1: Predictive Margins for Realized Efficiency.



Notes: Unit of observation: an economy. Supergames 1-4 (N=96 and N=16 per treatment for, respectively, groups of size 2 and 12). Each marker reports the mean realized efficiency level with a 95% predictive margin. The predictions are generated using a GLM regression, with estimates reported in Table A1. In fixed pairs, efficiency did not increase when the choice set is expanded as compared to the binary-choice *Baseline*, and significantly decreased in *High* (p-value=0.043). In large groups, efficiency significantly increased in *Linear* and *Low* as compared to *Baseline*, (p-value=0.082, p-value< 0.001, respectively), while the difference is insignificant for *High*.

Table A2: P-values of pairwise comparisons for marginal effects

	Treatment vs. Baseline						**Within treatment**
	High		*Linear*		*Low*		
	2	12	2	12	2	12	2 vs. 12
Cooperation							
Baseline	0.103	0.892	0.913	0.060	0.607	0.004	<0.001
High			0.096	0.008	0.033	<0.001	<0.001
Linear					0.724	0.051	<0.001
Low							<0.001
Efficiency							
Baseline	0.043	0.615	0.918	0.082	0.172	<0.001	<0.001
High			0.039	0.001	0.002	<0.001	<0.001
Linear					0.241	<0.001	<0.001
Low							<0.001

Table A3: Endogenous Types According to FMM estimation (Fixed Pairs)

	Regressor	1 type	2 types	3 types
Type 1	Repeat	0.060	-.373	.075
		(.123)	(.403)	(.329)
	Partner	1.631***	1.135***	.928***
		(.095)	(.314)	(.276)
	Repeat × Partner	.281**	1.041**	.572
		(.130)	(.443)	(.488)
	Constant	1.861***	.615	.921*
		(.116)	(.654)	(.484)
Type 2	Repeat		-5.190*	-8.373**
			(2.679)	(4.196)
	Partner		6.967***	10.412***
			(1.734)	(2.902)
	Repeat × Partner		-3.839*	-5.862**
			(1.995)	(2.981)
	Constant		9.223***	13.494***
			(2.410)	(3.866)
Type 3	Repeat			-1.732
				(1.561)
	Partner			3.265**
				(1.374)
	Repeat × Partner			-1.694
				(1.270)
	Constant			4.609**
				(1.865)
NEC		—	45.98	**37.46**
ICL-BIC		—	**11061.28**	11906.91

Notes: Unit of obs.: a subject in a period (fixed pairs, supergames 1-4). Number of obs. = 7512, Number of subjects =384. Standard errors clustered at the individual level, in parentheses. The lowest number for each information criterion, NEC and ICL-BIC, is in bold. We also calculated BIC and AIC, which recommend the 2 types and 3 types model, respectively. When we focus on the estimation of the 2 types model, the resulting proportions of type 1 are 0.446, 0.340, 0.145, and 0.136 in, respectively, *Baseline*, *High*, *Linear*, and *Low* (the complementary proportions are associated to type 2 players).

B Supplementary Material (for online publication)

Table B1: GLM Regression: Estimates

Dep. variable:	Cooperation	Efficiency
High	-0.573*	-0.714**
	(0.342)	(0.343)
Linear	-0.038	-0.035
	(0.350)	(0.339)
Low	0.158	0.433
	(0.306)	(0.311)
Size 12	-1.649***	-1.644***
	(0.178)	(0.183)
High×Size12	0.610**	0.567*
	(0.297)	(0.299)
Linear×Size12	0.429	0.418
	(0.276)	(0.281)
Low×Size12	0.667***	0.783***
	(0.156)	(0.163)
Repeat	0.296	0.302
	(0.303)	(0.326)
Repeat ×Size12	-0.487*	-0.511**
	(0.257)	(0.258)
Constant	1.479***	1.457***
	(0.259)	(0.267)
Additional Controls	Yes	Yes
N	448	448

Notes: Unit of observation: an economy in supergames 1-4. Additional controls include: an *order* indicator variable soaking up order effects (1, if supergames 1 & 3 have groups of 12, and zero otherwise), an *Repeat* variable that controls for learning effects (1 if the supergame is 1 or 2, and 2 if supergame is 3 or 4), supergame duration and duration of previous supergame to soak up additional learning effects, and individual characteristics (major of study, sex, and the number of wrong answers in the incentivized understanding quiz). We use a logit link function. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively. Robust standard errors clustered at the session level.

Table B2: Panel Logit: robustness checks for controls and time

Dep. var: cooperation	Model 1	Model 2	Model 3	Model 4	Model 5
Treatment & size effects					
High	-0.329	-0.214	-0.212	-0.407	-0.290
	(0.713)	(0.698)	(0.702)	(0.551)	(0.568)
Linear	1.935***	1.907***	1.910***	1.785***	1.743***
	(0.654)	(0.603)	(0.603)	(0.565)	(0.545)
Low	1.840***	1.801***	1.802***	1.745***	1.616***
	(0.544)	(0.526)	(0.535)	(0.443)	(0.396)
Size 12	-3.544***	-2.092***	-2.092***	-2.429***	-3.804***
	(0.337)	(0.621)	(0.617)	(0.642)	(0.321)
High $\times$ Size 12	1.600**	1.516**	1.513**	1.510*	1.421**
	(0.664)	(0.663)	(0.662)	(0.829)	(0.715)
Linear $\times$ Size 12	0.941**	1.040***	1.036***	1.070**	0.827*
	(0.379)	(0.327)	(0.326)	(0.544)	(0.470)
Low $\times$ Size 12	1.549***	1.625***	1.623***	1.686***	1.617***
	(0.328)	(0.321)	(0.323)	(0.355)	(0.336)
Dynamics within session					
Repeat	0.011	-0.205	-0.211		
	(0.367)	(0.342)	(0.341)		
Repeat $\times$ Size 12	-1.141***	-1.058**	-1.057**		
	(0.430)	(0.418)	(0.419)		
Cum_duration	0.849***	0.958***	0.962***		
	(0.157)	(0.134)	(0.120)		
Order	-0.066	-0.056			
	(0.341)	(0.333)			
Dynamics within supergame					
Period		0.019	0.019	0.062	
		(0.049)	(0.049)	(0.057)	
Period^2		-0.002	-0.002	-0.004*	
		(0.002)	(0.002)	(0.003)	
Period$\times$Size 12		-0.199***	-0.199***	-0.237***	
		(0.067)	(0.067)	(0.071)	
Period$^2\times$Size 12		0.005*	0.005*	0.006**	
		(0.002)	(0.002)	(0.003)	
Constant	4.661***	4.002***	3.979***	3.567***	3.321***
	(0.582)	(0.593)	(0.622)	(0.512)	(0.337)
Individual controls	YES	YES	YES	NO	NO
Other time indicators	YES	NO	NO	NO	NO
N	16,032	16,032	16,032	16,032	16,032

Notes: Unit of observation: a subject in a period, supergames 1-4. *Individual controls* include: major of study, sex, and the number of wrong answers in the incentivized understanding quiz. *Other time indicators* include a series of indicator variables for each period. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively. Robust standard errors clustered at the session level.

Table B3: Realized Supergame Durations

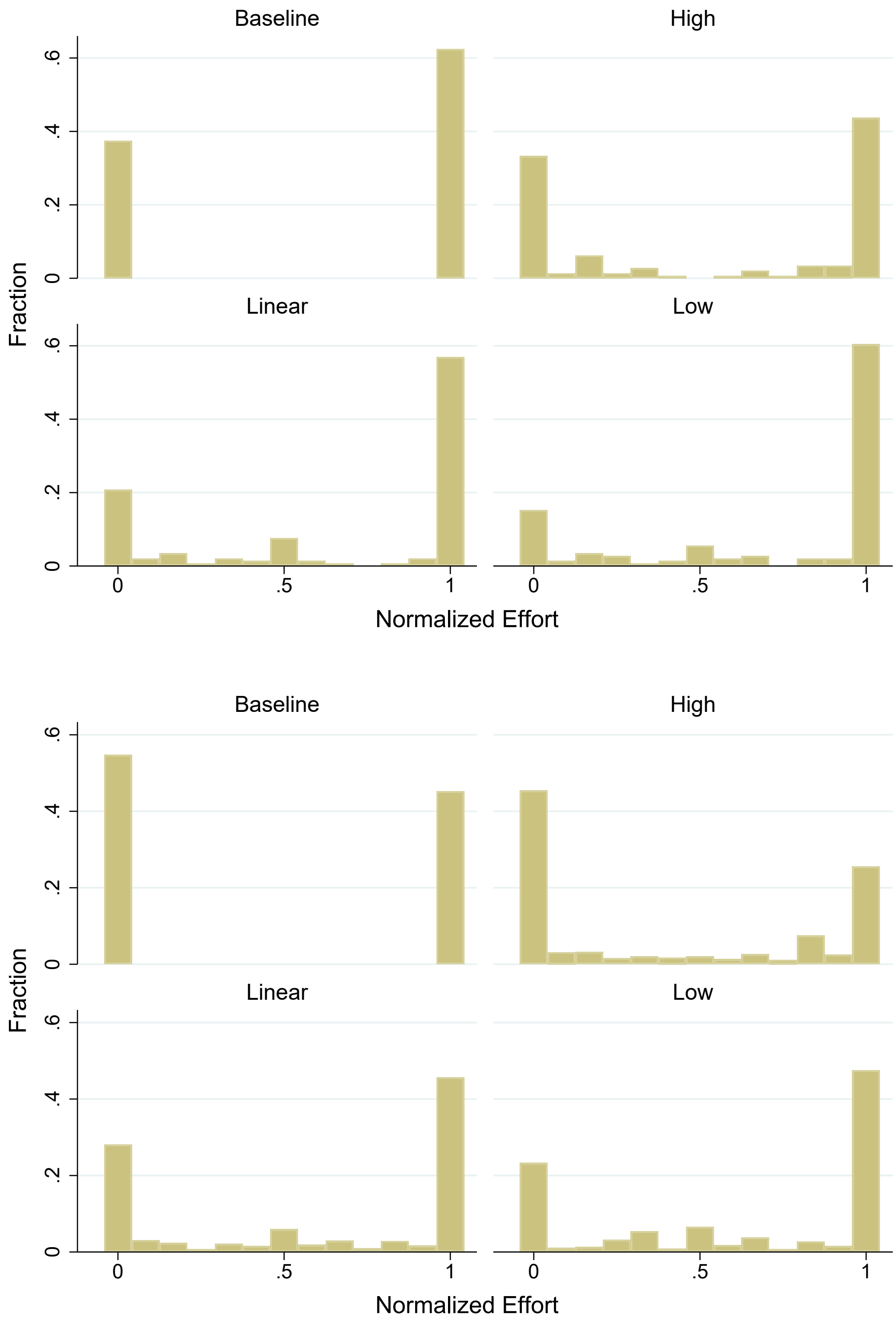
	Baseline		High		Linear		Low	
Rounds	2	12	2	12	2	12	2	12
16			24	14		6	12	2
17	12	4	24		36	4	12	6
18	12	4	24	2				
19	24	4	12	4		4		2
20						2	12	4
21		2			24		12	2
22	12	2		2		2	12	2
23		2			12		12	2
24	24	4						
25+	12	2	12	2	24	6	24	4
N	96	24	96	24	96	24	96	24

Table B4: Choices in period 1, Large Groups: Marginal Effects

Dep. variable= effort in period 1	Defection ($e = 0$)	Cooperation ($e = 6$)
High	-0.064	-0.156*
	(0.071)	(0.090)
Linear	-0.222***	-0.042
	(0.060)	(0.053)
Low	-0.299***	0.006
	(0.079)	(0.095)
Other Controls	Yes	Yes
N	576	576

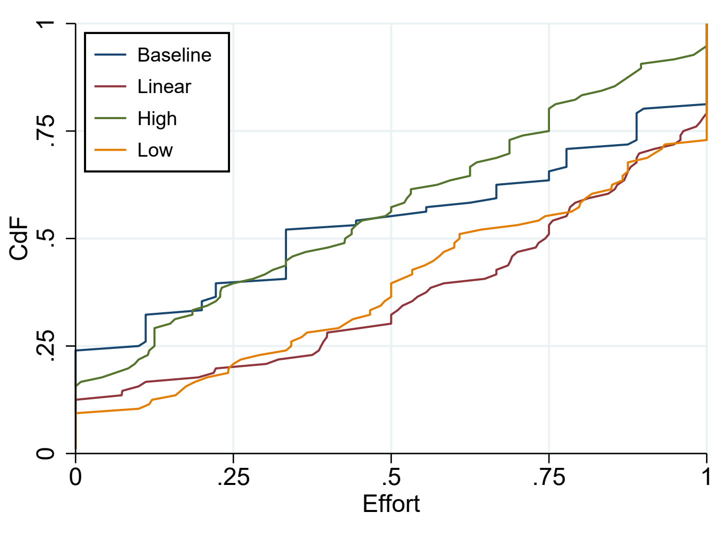
Notes: Unit of observation: A subject in period 1. Data for supergames 1-5, large groups. Logit regression with robust standard errors clustered at the session level. *Defection* $= 1$ if $e = 0$, and 0 if $e > 0$; *Cooperation* $= 1$ if $e = 6$, and 0 if $e < 6$. Other controls include: a dummy that controls for order effects (1, if supergames 1 and 3 have groups of 12, and zero otherwise), a dummy that controls for the supergame in the session (first, second or third, first is the base of the regression), cumulative duration of previous supergames, and individual characteristics (major of study, sex, and the number of wrong answers in an incentivized understanding quiz).

Figure B1: Distribution of Effort: period 1, and all periods—Large Groups



Notes: Unit of observation: one subject in a period. The top panel refers to period 1 in a supergame, while the bottom panel considers all periods.

Figure B2: Cumulative Distribution Effort–Last Supergame



Notes: Unit of observation: one subject in the last supergame (N=96 per treatment).

Table B5: Endogenous Types According to FMM Estimation (Robustness checks)

	Regressor	2 types	2 types
Type 1	Repeat	-.388	-.003
		(1.039)	(.730)
	Group	1.509***	1.375***
		(.388)	(.461)
	Counterpart	.719***	.583***
		(.236)	(.224)
	Repeat × Group	-.095	-.317
		(.498)	(.472)
	Repeat × Counterpart	.567	.440
		(.447)	(.296)
	Initial Donor	-.227	
		(.347)	
	Trigger		-.471*
			(.285)
	Constant	-1.985**	-1.533
		(.859)	(1.045)
Type 2	Repeat	-7.994**	-7.940**
		(3.297)	(3.348)
	Group	.921***	.900***
		(.208)	(.196)
	Counterpart	6.925***	6.686***
		(2.422)	(2.452)
	Repeat × Group	-.026	-.063
		(.398)	(.309)
	Repeat × Counterpart	-5.885**	-5.870**
		(2.353)	(2.417)
	Initial Donor	.368	
		(.254)	
	Trigger		-12.289
			(44.901)
	Constant	9.820***	21.921
		(3.467)	(48.101)
NEC		26.66	27.29
ICL-BIC		17683.74	17935.06

Notes: Unit of obs.: a subject in a period (large groups, supergames 1-4). Number of obs. = 7752, Number of subjects =384. Standard errors clustered at the individual level, in parentheses. The results for each information criterion, NEC and ICL-BIC, appear at the bottom.

Table B6: Endogenous Types: FMM Estimation with Fixed Behavior Across Supergames

	Regressor	1 type	2 types	3 types
Type 1	Group	.735***	1.405***	2.326**
		(.062)	(.258)	(1.012)
	Counterpart	.604***	.963***	1.246*
		(.037)	(.203)	(.647)
	Constant	.290***	-2.084***	-4.087*
		(.086)	(.611)	(2.186)
Type 2	Group		.978***	.734***
			(.203)	(.236)
	Counterpart		7.899**	18.231
			(3.445)	(28.395)
	Constant		11.699**	26.450
			(4.928)	(40.349)
Type 3	Group			.849***
				(.203)
	Counterpart			.717***
				(.115)
	Constant			-.211
				(.430)
NEC		—	**27.336**	39.155
ICL-BIC		—	**17450.008**	22175.517

Notes: Unit of obs.: a subject in a period (large groups, supergames 1-4). Number of obs. = 7752, Number of subjects = 384. Standard errors clustered at the individual level, in parentheses. We estimate the same unconstrained models from Table 6 but restricting behavior to be the same across supergames. The lowest number for each information criterion, NEC and ICL-BIC, is in bold. We also calculated BIC and AIC, which recommend the 2 types and 3 types model, respectively.

Instructions-BASELINE

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Overview of the experiment

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There are 24 anonymous participants in the room. At the start of each **cycle**, a computer program will form groups and you will only interact with someone from **your group**.

- In some cycles there will be **random pairings** inside groups of 12 participants:

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Groups change in each cycle so that **you cannot interact with anyone for more than one cycle**.

How do you earn money in a period?

You will earn points that depend on your choices and the choices of others **in your group**. Points will be converted into dollars at the end of the experiment in a manner that we explain later.

In each period you will be **paired** with one person from your group, called your **"match."** If you are not in a fixed pair, then your match is **a random person** from your group. Your match is always **anonymous**.

In each pair, one person will be **red** and the other **blue**. The **blue** person starts with 3 points and has no choice to make. The **red** person starts with 6 points and must make a choice that determines the point **earnings** in the pair. Choices carry different costs and benefits, as illustrated below.

Table 1: Your possible choices when you are red, and the earnings (in points) in your pair

Cost of choice	Payoff to **red**	Payoff to **blue**
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What happens in each period?

Each **period** has the following timeline:

1. You see your color and you are paired with someone from your group.
2. You may be called to make a choice.
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Now, we discuss each of these steps.

1. Your color and your match

In each period, half of the persons in your group are **red** and the others **blue**. Your initial color is random and then your color alternates from period to period: blue, red, blue, red, … or: red, blue, red, blue, …

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2. Your choices

- If you are **blue**, then you have no choice to make.
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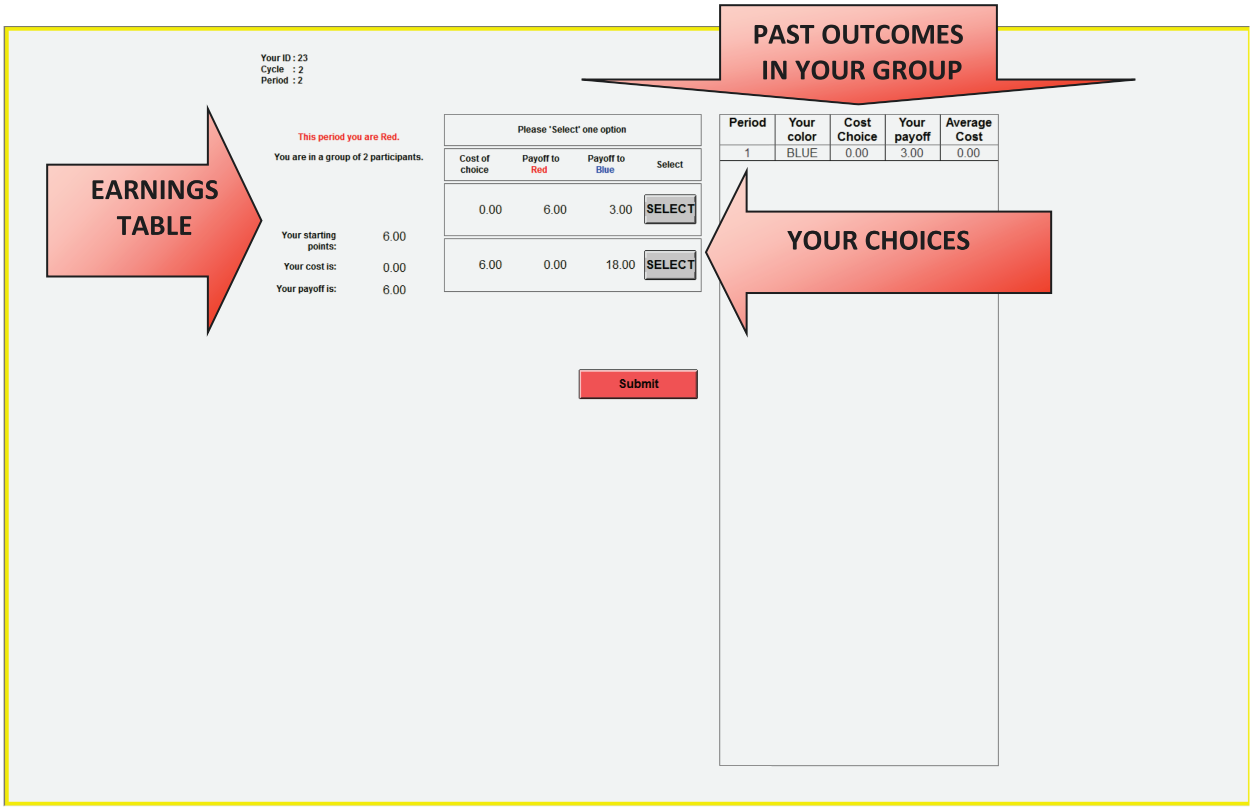

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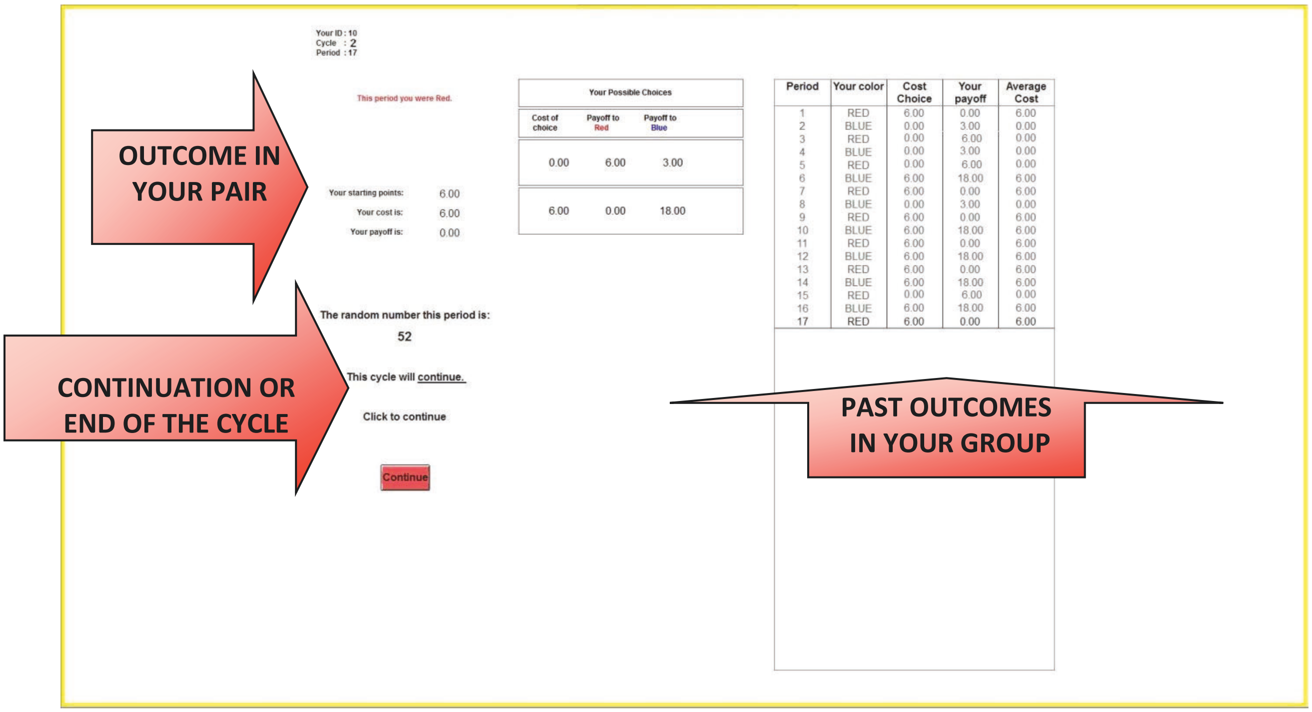
The results for the period will be displayed after everyone makes a choice (see figure below). You will see the **outcome** and the points you **earned**. You can write the results on your record sheet, if you wish. Results from past periods of that cycle will be visible at the right on your screen.



4. Ending of a cycle

Each cycle has many periods but their number is **unknown** to all of us because it is **random**.

Each cycle will have at least 16 periods. From period 16 on, at the end of each period the computer selects a number between 1 and 100. Each number is equally likely to be selected:

- If the number selected is less than or equal to 80, then the cycle will continue for all groups.
- If the number selected is 81 or more, then the cycle will end for all groups.

So: starting in period 16, the cycle has always a chance to continue. To see whether the cycle continues or ends look at the results screen; you will see the random number selected by the computer.

Note: The number of past periods does not influence the chance that a cycle will end. In each period, any number between 1 and 100 has the same chance of being selected, independently of the numbers selected before. The chance that a cycle will end, say, after you have completed period 23, is 20%, which is exactly the same as the chance that the cycle will end after you have completed period 16. Hence:

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Will there be fixed pairs or random pairs?

In cycles 2 and 4 you will be in a fixed pair. In cycles 1, 3 and 5 you will be randomly paired inside a group of 12 participants. **Recall**: participants that you meet in a cycle cannot be met in future cycles.

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- If you are **red**, then you make a choice that determines the points in your pair.
- If you are **blue**, then you have no choice to make.
- Each cycle has an **uncertain** number of periods. Starting in period 16, there is **always** an 80% chance of an additional period, and a 20% chance of ending.

Before we start the experiment, you will be asked to answer ten questions designed to verify your understanding of the instructions. You will receive $0.25 for each question you answer correctly on the first try. If you have a question at any time, then please raise your hand and someone will come to answer it.

Instructions-LINEAR

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How do you earn money in a period?

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2.5	3.5	9.25
3	3	10.5
3.5	2.5	11.75
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5	1	15.5
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What happens in each period?

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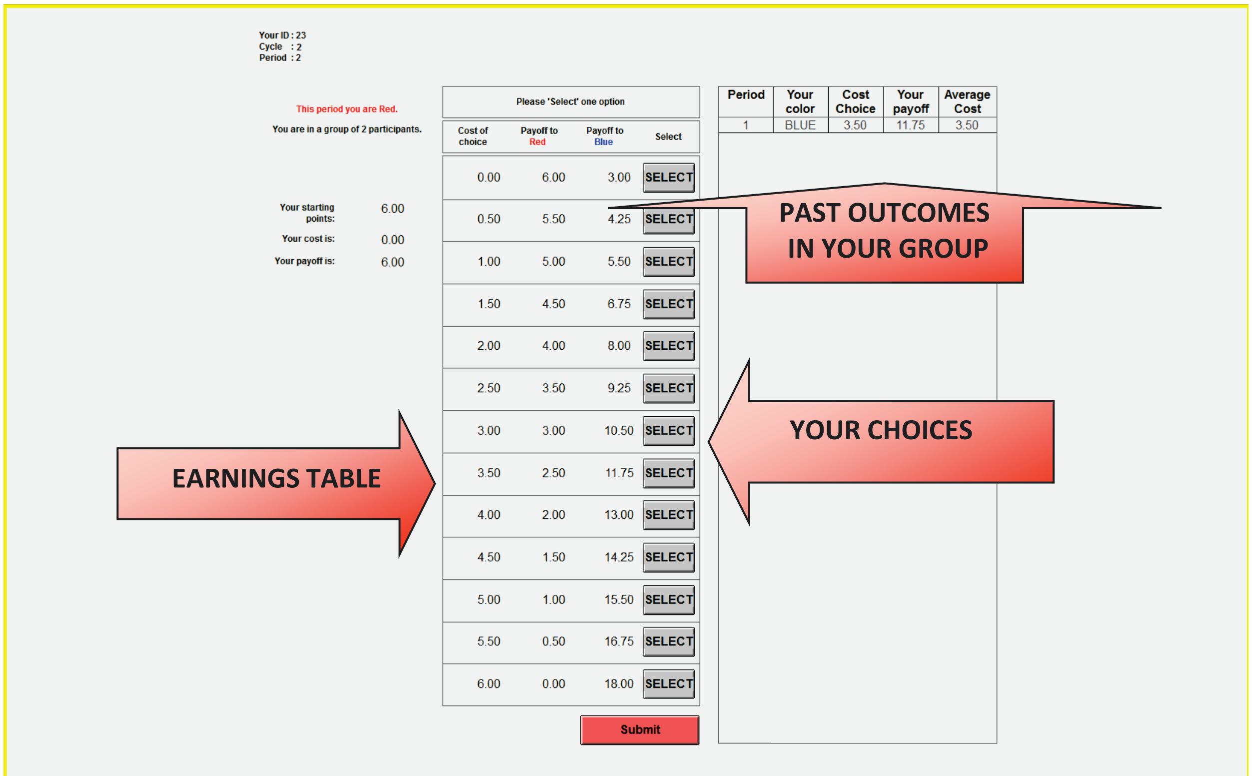

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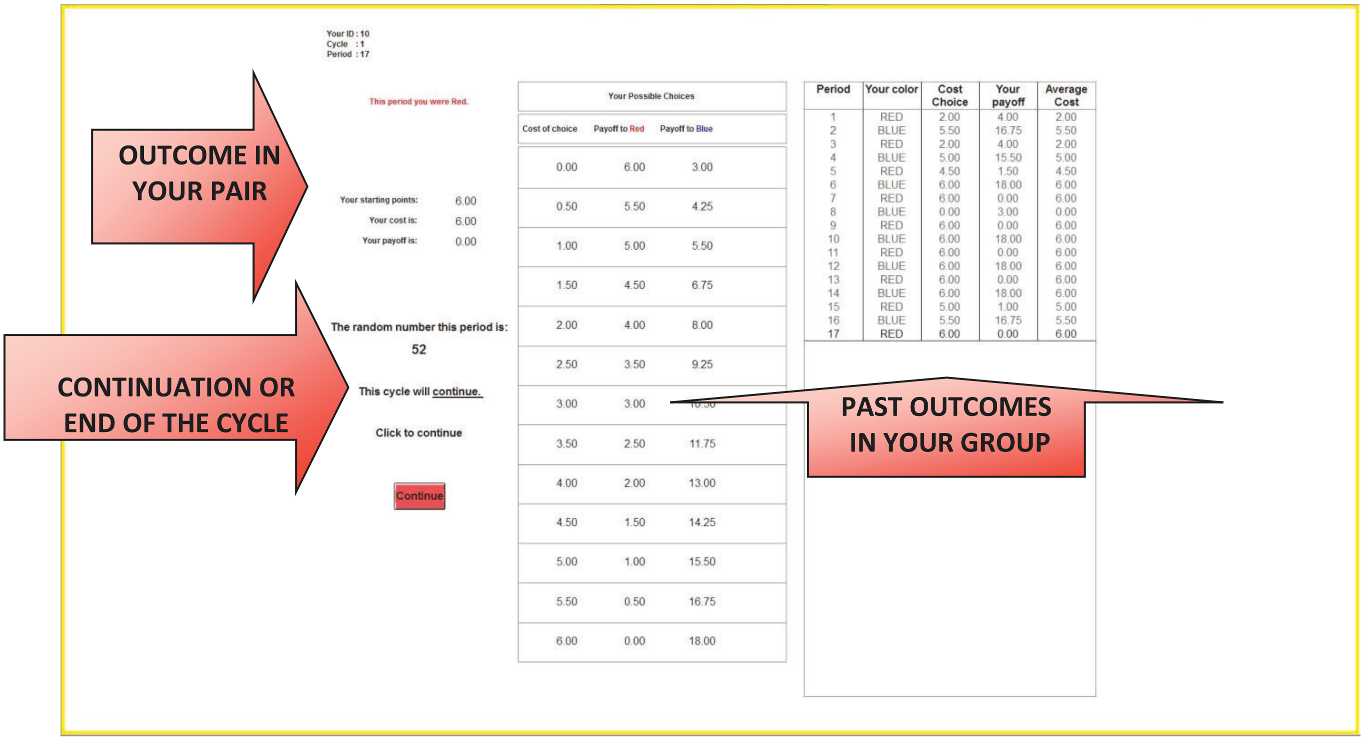

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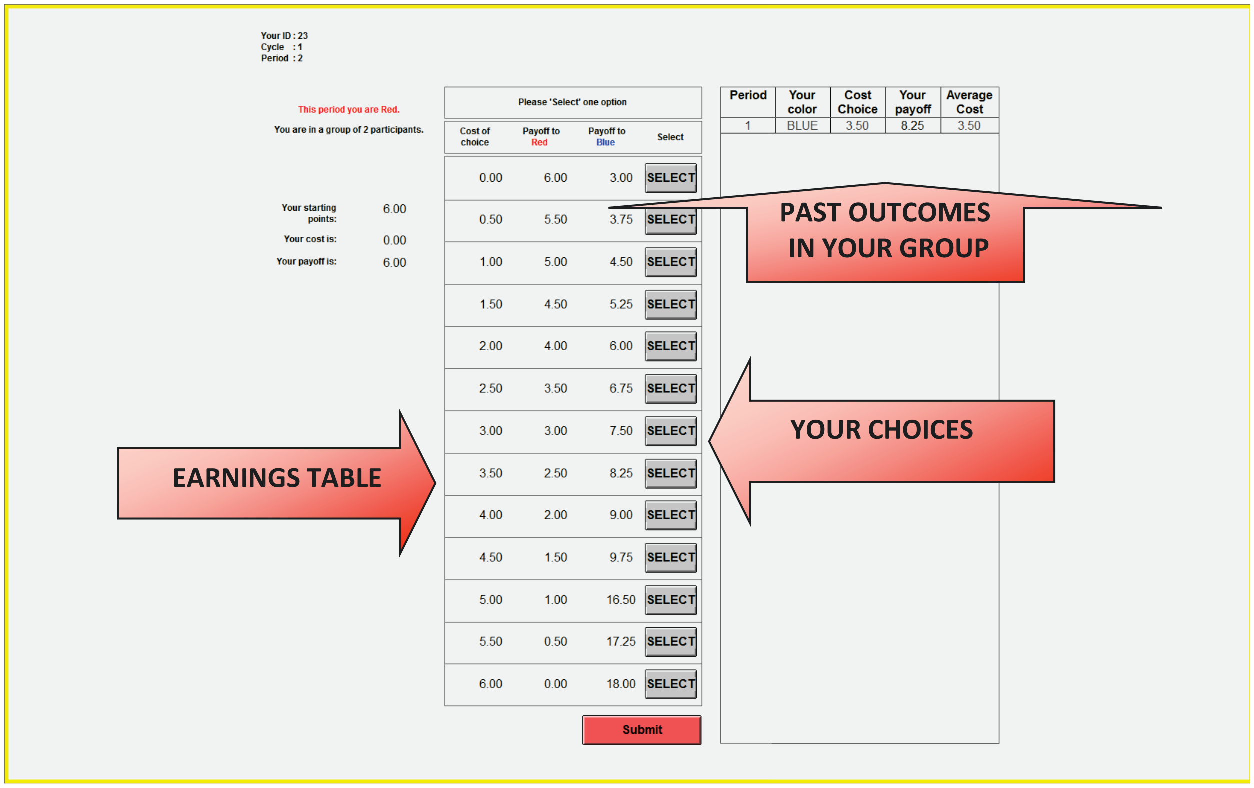

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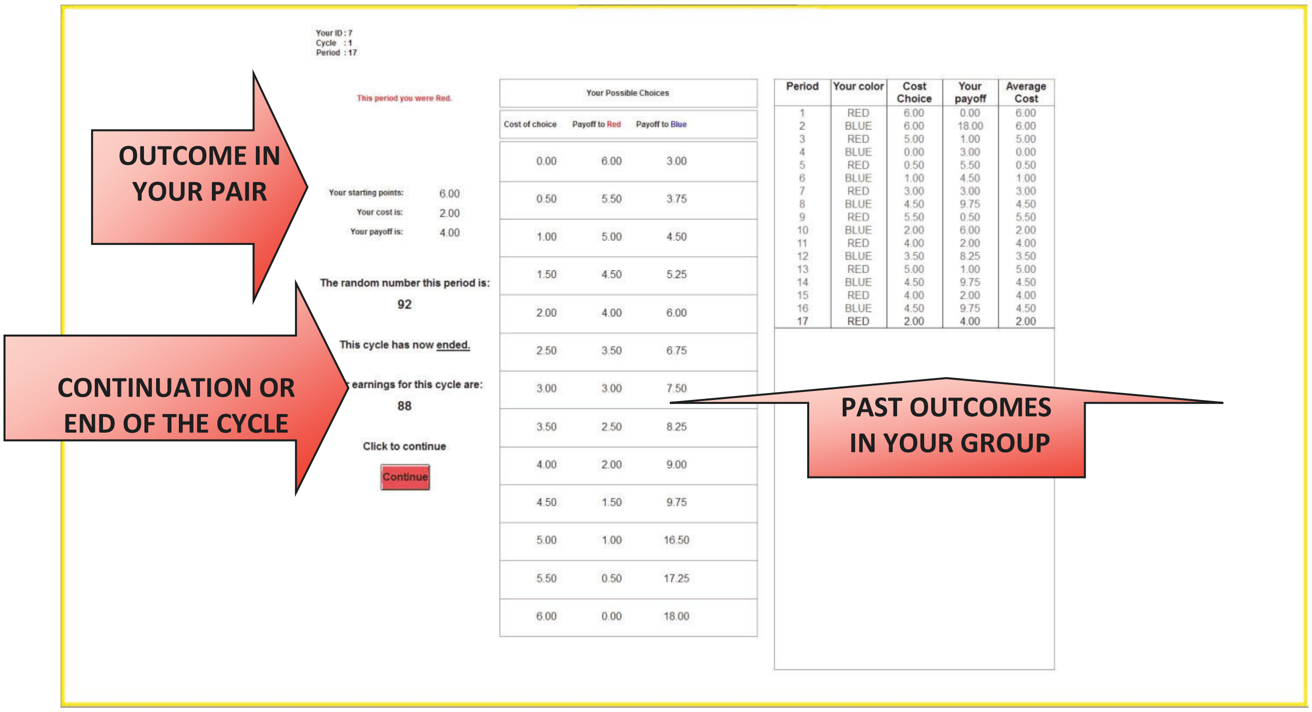

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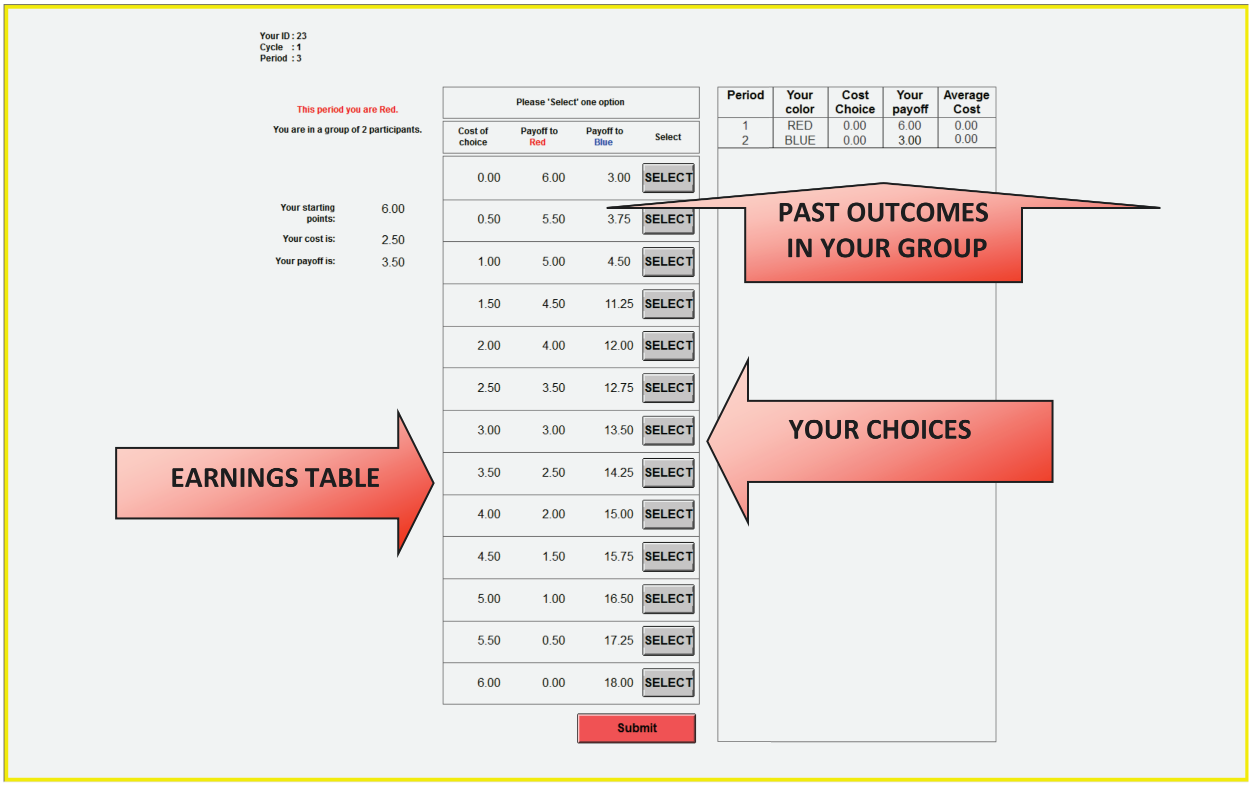

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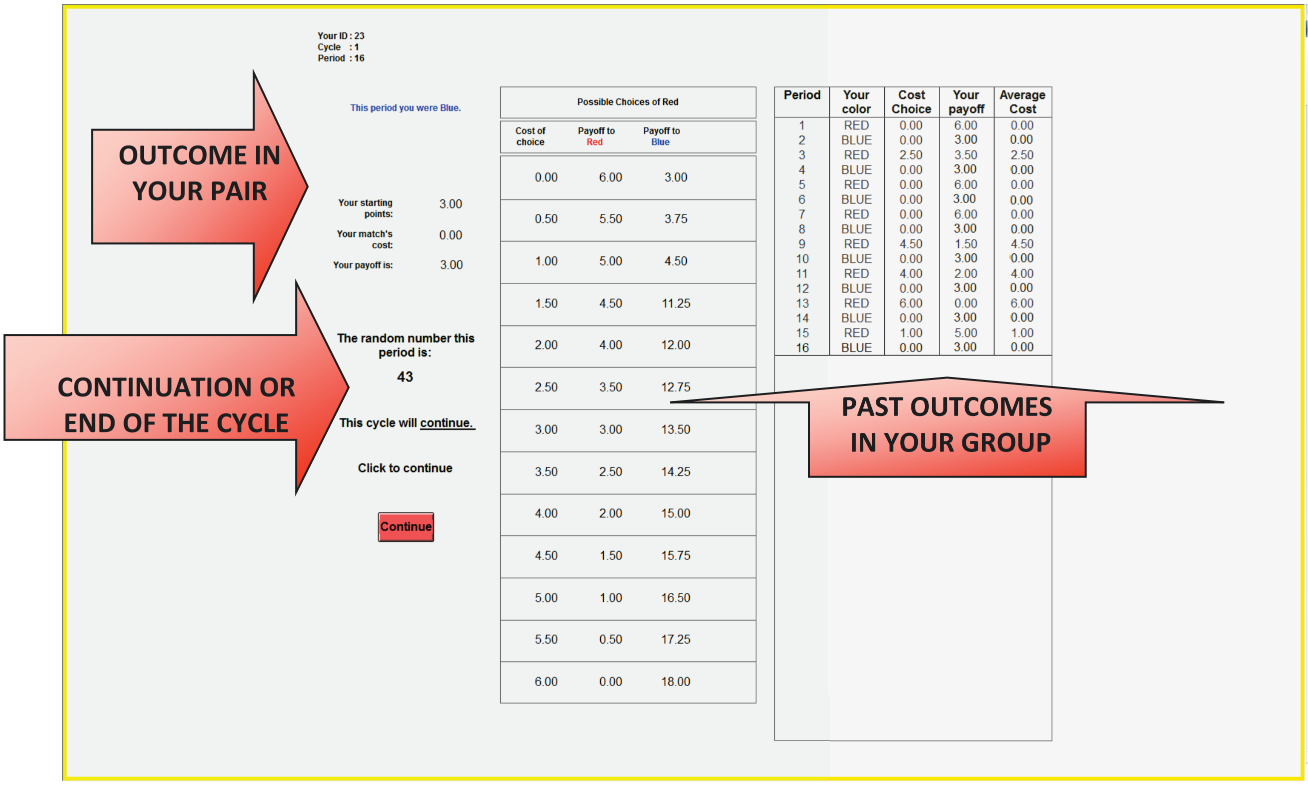

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ID _____ RECORD SHEET

Period	Your Color (B or R)	Outcome in your pair	Payoff	Notes (optional)

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Incentivized Post-Instructions Comprehension Quiz

We would like you to answer **10 questions**, to make sure that instructions are clear. You will receive $ 0.25 for each question answered correctly at the first try. Press OK to start. Thank you.

1. How many periods are there in each block?

A) 16 periods, exactly.

B) 16 periods plus an uncertain number of additional periods that depend on a random process.

C) 16 periods, more or less.

2. We have reached period 22 of a cycle. Can the cycle continue?

A) Yes. The chances that it will continue are 8 out of 10 (=80% probability).

B) Yes. The chances that it will continue are 2 out of 10 (=20% probability).

C) No. It will stop for sure.

3. Consider any cycle. Will your match be the same person in each period of the cycle, or will it randomly change from period to period?

A) In a group of 12, my match is fixed; in a group of 2, my match is selected at random.

B) In a group of 12, my match is randomly selected from those 6 participants who have a color different than mine. In a group of 2, my match is always the same in each period.

C) I get to choose who my match is in each period.

4. Consider any cycle. Can any of your matches in that cycle be your match in any other cycle?

A) No, it is impossible. I am matched to different sets of participants in each cycle.

B) Yes, for sure.

C) It is possible, but it is not certain.

5. If you are red in period 4, which color will you be in period 5, 6 and 7?

A) It depends. I will be blue or red with 50% chance in each of these periods.

B) I will be red in periods 5, 6 and 7.

C) I will be blue in period 5, red again in period 6 and blue again in period 7.

6. Suppose your match selects a cost of 6 when you are blue. How many points will you and your match earn in that period?

A) You: 6 points. Your match: 0 points.

B) You: 18 points. Your match: 3 points.

C) You: 18 points. Your match: 0 points.

7. Suppose you select a cost of 0 when you are red. How many points will you and your match earn in that period?

A) You: 0 points. Your match: 18 points.

B) You: 6 points. Your match: 3 points.

C) You: 6 points. Your match: 0 points.

8. How is your final payment determined?

A) You are paid for the points your earn in each cycle.

B) You are paid for the points you earn in one random period. Every period has the same chance to be selected at the end of the experiment.

C) You are paid for all the points you earn in one cycle, chosen at random. Every cycle has the same chance to be selected at the end of the experiment.

9. Suppose a cycle lasts 20 periods, you always selected a cost of 6 when you were red, and every red match you met selected a cost of 6, also. How many points will you earn in that cycle?

A) 10 periods x 6 points + 10 periods x 3 points= 90 points.

B) 10 periods x 0 points + 10 periods x 18 points = 180 points.

C) 10 periods x 6 points + 10 periods x 0 points = 60 points.

10. Suppose a cycle lasts 20 periods, you always selected a cost of 0 when you were red, and every red match you met selected a cost of 0, also. How many points will you earn in that cycle?

A) 10 periods x 6 points + 10 periods x 3 points= 90 points.

B) 10 periods x 0 points + 10 periods x 18 points = 180 points.

C) 10 periods x 6 points + 10 periods x 0 points = 60 points.