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Perfect and imperfect strangers in social dilemmas

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Abstract

This paper focuses on social dilemma games where players may or may not meet the same partner again in the future. In line with the notion that contagion of cooperation is more likely the higher the likelihood of being re-matched with the same partner in the future, both a novel experiment and a meta-study document higher cooperation rates if this likelihood is sufficiently high.

JEL Codes: C70, C90, D70

Keywords: cooperation, contagion, matching protocol, laboratory experiment, meta-study

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1 Introduction

Many interactions qualify as social dilemma games, where cooperation between individuals in a group is needed to achieve outcomes that are optimal for the group. Think for example of voluntary public good provision, trade relationships or effort provision in the context of team production. Often, these interactions involve a certain degree of rotation of interaction partners. For example, the composition of teams within organizations typically varies over time, and often there is fluctuation in trading partners. This holds from trade relations in Europe or Sub-Saharan Africa during the Middle Ages (Greif, 1989; Milgrom, North, and Weingast, 1990) to today's Internet transactions (Bolton, Greiner, and Ockenfels, 2013). Laboratory experiments have contributed to our understanding of why people cooperate in such interactions and under which conditions they do so. For example, information about a partner's past choice(s) tends to increase cooperation (Bolton, Katok, and Ockenfels, 2005; Camera and Casari, 2009).[1] In this paper we study whether the likelihood of meeting the same partner again in the future has an effect on the willingness to cooperate in settings where partners may change. We report the results of a novel laboratory experiment and of a meta-study based on past experimental studies.

Participants in our experiment play ten rounds of a prisoner's dilemma and are re-matched after each round. We use a prisoner's dilemma because it is the simplest dilemma game in terms of action space and strategic interaction, and using simple games eases identification and interpretation of possible treatment effects. We run three treatments, each corresponding to a different matching protocol. In the first two treatments participants are randomly matched at the beginning of each round within matching groups of four and six participants, respectively. They are thus imperfect strangers to each other. In the third treatment participants are paired using the turnpike protocol due to Townsend (1980), which makes them perfect strangers. In the turnpike protocol matches are organized such that participants never encounter the same partner more than once and past interaction partners never encounter each other either (see Cooper, DeJong, Forsythe, and Ross, 1996; Kamecke, 1997; Aliprantis, Camera, and Puzzello, 2006, for discussions). Thus the crucial difference between the two random matching treatments and the turnpike treatment is that the probability of being re-matched with the same partner in the next period is strictly larger than zero in the former treatments — equal to 0.33 to 0.20 in matching groups of four and six, respectively — and equal to zero in the latter treatment.

In the random matching treatments, and not so in the turnpike treatment, an individual may be concerned that her choice may affect the future choices of others in the same matching group, which

[1]See also for example Andreoni (1988), Fehr and Gächter (2000) and Fischbacher and Gächter (2010) for applications of public goods games, and Andreoni and Miller (1993), Cooper, DeJong, Forsythe, and Ross (1996), Clark and Sefton (2001) and Duffy and Ochs (2009), for applications of prisoner's dilemmas. Examples of experiments where participants are randomly re-matched after playing a repeated or dynamic game with the same partner are Dal Bó (2005), Dal Bó and Fréchette (2011), and Calzolari, Casari, and Ghidoni (2016). Finally, strangers matching has also been used in oligopoly games characterized by a tension between individual and group interest (e.g. Huck, Normann, and Oechssler, 1999; Dufwenberg, Gneezy, Goeree, and Nagel, 2007).

may make it more likely that she cooperates. Ultimately, such concern may lead to the development of cooperative norms at the matching group level. This type of 'contagion' has been shown to be theoretically important in the context of an infinitely repeated prisoner's dilemma game where after each repetition of the stage game players are randomly re-matched within a finite population. Kandori (1992) and Ellison (1994) show that cooperative strategies can be part of an equilibrium if there is a sufficiently strong 'shadow of the future' even when players only observe the outcomes of their own matches and are not informed about the identity of past partners. Combining this insight with the notion that in a finitely repeated game cooperation is rationalizable if the share of reciprocal players in the population is above a certain threshold (Kreps, Milgrom, Roberts, and Wilson, 1982), contagion of cooperation may also occur in a finitely repeated prisoner's dilemma with random matching (see Andreoni and Miller, 1993; Cooper et al., 1996, for a discussion). Given that such threshold must be inversely related with the probability of being re-matched with the same partner in the next period, the expectation in the context of our experiment is that contagion is more likely to occur, and cooperation rates are (weakly) higher, the higher the re-matching probability.

The results are in line with the expectation. We find that the cooperation rate in our experiment is about 15 percentage points higher in the treatment where participants are randomly re-matched in matching groups of four than in the other two treatments. In part, this is because participants in that treatment tend to condition more strongly on the decisions of past partners. The results of the experiment are largely corroborated by a meta-study including 23 treatments from 14 linear public goods experiments with strangers matching, covering 59 independent observations and 923 participants.[2] The type of strategic interaction in public goods games is very similar to that of a prisoner's dilemma. In both games, it is a dominant strategy to defect (or to contribute zero). The meta-analysis shows that higher re-matching probabilities are associated with higher contributions to the public good. In particular, an increase in re-matching probability from below 0.2 to above 0.2, leads to a 15 percentage points increase in the share of the endowment contributed to the public good. On the basis of these two pieces of evidence, we conclude that a sufficiently high re-matching probability is likely to induce higher cooperation rates in dilemma games with random matching than a low re-matching probability.

Finally, we would like to point out that our paper also serves a methodological purpose. In particular, to study how people behave in one-shot interactions, laboratory experiments often use random matching designs with the purpose of allowing participants to gain experience with the interaction of interest while avoiding possible reputational effects (for example, some of the references in footnote 1). Our results indicate that this design choice may not be innocent. If the size of the matching group is relatively small as compared to the size of the interaction group, then the design cannot be taken to be free of reputational effects.

[2] A meta-study based on prisoner's dilemma experiments would be less appealing because strangers matching is not common in these experiments (see for example Mengel, 2017).

Table 1: The experiment.

(a) Payoffs in the PD.

	Cooperate	Defect
Cooperate	150, 150	210, 30
Defect	30, 210	68, 68

(b) The treatments.

	Random4	*Random6*	*Turnpike*
Number of sessions	4	4	4
Number of participants	72	66	80
Number of matching groups	18	11	-
Number of rounds	10	10	10
Re-matching probability	0.33	0.20	0

Notes: The relations between the PD payoffs are broadly similar to those of Andreoni and Miller (1993). The re-matching probability is the likelihood of meeting an interaction partner again in the next round.

2 The experiment

2.1 Design and procedures

Participants in our experiment played the prisoner's dilemma (PD) shown in panel (a) of Table 1 ten times. After each round, they received feedback about the outcome of their interaction. The type of strangers matching (perfect versus imperfect strangers) as well as the size of the matching group (four versus six members) varied across treatments in a between-subjects design. Panel (b) of Table 1 gives an overview of the treatments, including a number of key features.

In treatment *Random4* participants were randomly matched within matching groups of four players in each round, and in *Random6* they were matched in matching groups of six players. In both of these treatments, participants were aware of the size of the matching group. In the *Turnpike* treatment, all participants in sessions of twenty participants were randomly allocated to two groups of ten. In each round they were matched with a different participant of the other group in a specific order. The matching order ensures that participants were never matched with the same partner more than once, and that the partners of each participant were never matched together.[3] As a consequence, in *Turnpike* the best-reply structure of the stage game is maintained so that contagion effects are ruled out (Kamecke, 1997).

The three treatments are characterized by differences in the probability in round t of being matched with the same participant in round $t+1$. This probability is 0.33 in *Random4*, 0.20 in *Random6*, and 0 in *Turnpike*. So, over ten rounds of play, participants could expect to interact with the same partner for 3.3 rounds in *Random4* and 2 rounds in *Random6*, which is close to what is typically observed in dilemma experiments.[4] In *Turnpike* participants were sure to interact with the same partner only once.

In all treatments we measured risk and social preferences before participants played the PD games.

[3]See Table C.6 in Appendix for an example of how participants were matched across rounds in *Turnpike*. Since the number of seats in the lab is limited to 20 positions, the turnpike matching protocol constrained the maximal number of repetitions of the PD to 10 rounds (or half the session size).

[4]To illustrate, the re-matching probability in the dataset on public goods games with strangers matching that we discuss in section 3 of this paper ranges between 0.08 and 0.44 (see Table 4).

Both are possibly relevant control variables because the choice to cooperate includes a risk and a social component. Risk preferences were elicited in a lottery task inspired by Holt and Laury (2002). Social preference parameters á la Charness and Rabin (2002) were elicited in a modified dictator game inspired by Kurschilgen (2017) and Iriberri and Rey-Biel (2011). In particular, we elicited ρ and σ for each participant, where ρ (σ) corresponds to the weight attached to a randomly matched anonymous other person's payoff if the participant is ahead (behind) the other person in monetary terms.[5] In the context of a PD game ρ can be taken as a determinant of the choice of a player to cooperate or defect given that the partner *cooperates* because the player earns (weakly) *more* than the partner then. In contrast, σ can be taken as a determinant of the choice of a player to cooperate or defect given that the partner *defects* because the player earns (weakly) *less* than the partner then. Thus, a player who expects her partner to cooperate is more likely to cooperate herself if she is characterized by a high ρ. A player who expects her partner to defect is less likely to cooperate if she is characterized by a high σ.

The experiment was conducted in CentERlab at Tilburg University. It was programmed using z-Tree (Fischbacher, 2007). The experimental instructions were provided in writing to the participants directly before each task was conducted and were read aloud by the experimenter (see Section A of the Appendix). Prior to beginning each task, participants were required to complete a short quiz to test their understanding of the instructions. Participants could not begin a task until they had correctly answered each question. Participants were paid for all tasks. To prevent income effects and updating of expectations about other participants, feedback about the outcomes of the risk and social preferences tasks was only provided to participants after they played the PD games. On average, sessions lasted approximately 50 minutes and participants earned 10.4€.

2.2 Results

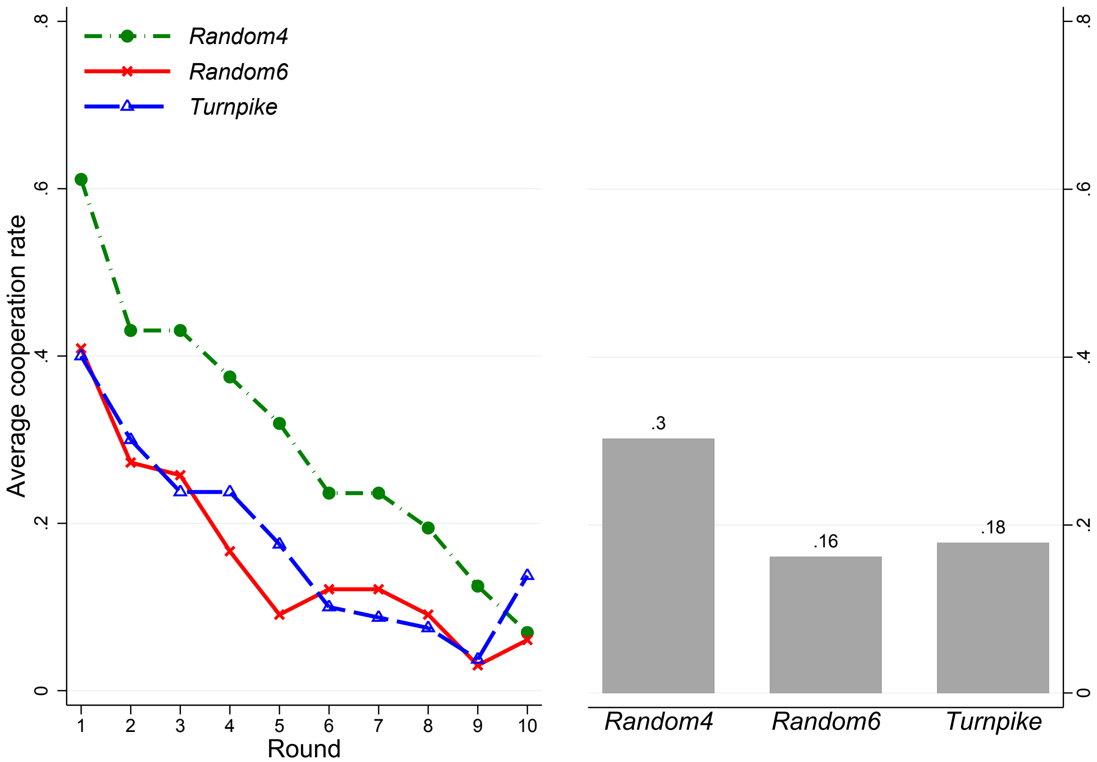
We conduct between-treatment analyses. We first focus on detecting treatment effects on cooperation rates and then study the extent to which the choice to cooperate in each treatment is influenced by the previous partner's decision.

The left-hand-side graph of Figure 1 shows the dynamics of the cooperation rates by treatment.[6] It can be seen that in every round but the last one the cooperation rate in *Random4* lies above the cooperation rates in *Random6* and *Turnpike*. Cooperation rates in *Turnpike* and *Random6* follow a similar trend over the rounds. In the last round, not much of a difference is left between the treatments. As can be seen in the right-hand-side graph of Figure 1, the overall cooperation rate is equal to 0.30 in *Random4*, 0.16 in *Random6*, and 0.18 in *Turnpike*.

[5]For details on the elicitation procedure, see Section B of the Appendix. Basically, we elicited intervals for ρ and σ using distributional decisions, and use the lower bounds of the intervals as proxies for ρ and σ.

[6]See Figure C.5 in the Appendix for dynamics of cooperation rates at the matching group level in *Random4* and *Random6*.

Figure 1: Cooperation rates by treatment.



Notes: The left-hand-side graph shows average cooperation rates by treatment by round and the right-hand-side graph shows the overall cooperation rate by treatment.

We test whether treatment effects are statistically significant in probit regressions where the choice to cooperate (yes/no) is regressed on treatment dummies. When considering all rounds, the regressions include random effects and standard errors clustered at the matching group level. Results are in Table 2. The cleanest comparison is based on first-round cooperation rates because in the first round subjects are not influenced by past experiences (see specification (1)). We find that the first-round cooperation rate is 19.3 percentage points higher in *Random4* than in *Random6* ($P = 0.012$) and 20.4 percentage points higher than in *Turnpike* ($P = 0.006$). The difference between *Random6* and *Turnpike* is statistically not significant.[7] Controlling for risk and social preferences does not have much of an impact on the estimated treatment effects (see specification (3)). Notice that ρ has a positive and strong effect on the cooperation rate and σ does not. The estimated treatment effects on the overall cooperation rate are

[7]These results are confirmed by one-tailed Fisher exact tests where the unit of observation is a participant: *P*-values are $P = 0.014$ for *Random4* versus *Random6*, $P = 0.007$ for *Random4* versus *Turnpike*, and $P = 0.523$ for *Random6* versus *Turnpike*.

Table 2: Estimated treatment effects.

Dep. var.: Cooperate in t (yes=1)	(1) Round 1	(2) Rounds 1 to 10	(3) Round 1	(4) Rounds 1 to 10
Random6	-0.193** (0.012)	-0.111** (0.012)	-0.170** (0.027)	-0.098** (0.032)
Turnpike	-0.204***(0.006)	-0.087* (0.057)	-0.194***(0.008)	-0.086* (0.062)
t		-0.040***(0.000)		-0.040***(0.000)
Degree of risk aversion			-0.006 (0.749)	-0.009 (0.293)
ρ			0.631*** (0.000)	0.401*** (0.000)
σ			-0.027 (0.562)	0.006 (0.722)
Wald test results				
Random6 − *Turnpike*	-0.011 (0.896)	-0.024 (0.489)	-0.025 (0.757)	-0.012 (0.746)
Observations	218	2180	218	2180
Matching groups		33		33

Notes: Marginal effects from probit regressions are reported. *Random6* and *Turnpike* are treatment dummies. In specifications (2) and (4) random effects are at matching group level and standard errors are clustered at matching group level. P-values are in parentheses; * $P < 0.1$, ** $P < 0.05$, *** $P < 0.01$. The variable ρ (σ) correspond to the weight attached to a randomly matched anonymous other person's payoff if the participant is in monetary terms ahead (behind) the other person. See Section B of the Appendix for details on the calculation of risk and social preferences.

smaller in size but still statically significant (see specifications (2) and (4) in Table 2). [8] [9] In the last round the cooperation rate is no longer significantly higher in *Random4* than in the other two treatments (not reported in the table; P-values are $P = 0.848$ [0.999] for *Random4* versus *Random6* and $P = 0.104$ [$P = 0.093$] for *Random4* versus *Turnpike* without [with] controls). We summarize our first result as follows:

Result 1. *The cooperation rate in matching groups of four is higher than that in matching groups of six, and than under a turnpike protocol. The difference disappears in the last interaction round.*

Next we investigate whether there are differences in the behavioral dynamics between the treatments. In particular, we study the reactions of participants to the previous partner's decision. We use a probit regression where we include the lagged value of a dummy variable tracking whether the partner from the previous round cooperated or not, the treatment dummies *Random6* and *Turnpike*, interaction terms between the lagged value and the two treatment dummies, and a linear time trend. The regression again includes random effects and standard errors clustered at the matching group level. The estimated effects of past partners' cooperative decisions in each treatment are reported under specification (1) in Table 3. Specification (2) keeps the same variables and, in addition, controls for participants' risk and

[8]When focusing only on participants with an estimate of σ larger or equal to 0, also the parameter σ is positively and statistically significantly correlated to the decision to cooperate, although to a lower extent than ρ. Instead, for individuals characterized by a negative estimate of σ, we do not detect any significant effect.

[9]We do not detect treatment differences in cooperation trends. When including interactions between the treatment dummies and a linear trend in our probit regressions, the estimated coefficients are not statistically significant ($P \geq 0.159$).

Table 3: Estimated effect of previous partner's decisions on cooperation.

Dep. var.: Cooperate in t (yes=1)	(1)		(2)	
Partner in $t-1$ cooperated	0.213***	(0.000)	0.205***	(0.000)
Random6	-0.053	(0.133)	-0.046	(0.217)
Turnpike	-0.028	(0.390)	-0.029	(0.390)
Random6 $\times$ Partner in $t-1$ cooperated	-0.090***	(0.009)	-0.077**	(0.027)
Turnpike $\times$ Partner in $t-1$ cooperated	-0.079**	(0.012)	-0.073**	(0.015)
t	-0.027***	(0.000)	-0.028***	(0.000)
Degree of risk aversion			-0.009	(0.322)
ρ			0.372***	(0.000)
σ			0.009	(0.591)
Observations	1962		1962	
Matching groups	33		33	

Notes: Marginal effects from a probit regression are reported. Random effects are at matching group level and standard errors are clustered at matching group level. P-values are in parentheses. * $P < 0.1$, ** $P < 0.05$, *** $P < 0.01$. The variable ρ (σ) correspond to the weight attached to a randomly matched anonymous other person's payoff if the participant is in monetary terms ahead (behind) the other person. See Section B of the Appendix for details on the calculation of risk and social preferences.

social preferences.

We find that part of the above-reported treatment effects can be explained by players conditioning their choice on the choice of the previous partner; the estimated coefficients of the treatment dummies are smaller in absolute value now and no longer statistically significant. The estimates indicate that participants in *Random4* were about 16 percentage points more likely to choose to cooperate when the person they were matched to in the previous round cooperated. A similar pattern is found also in *Random6* and *Turnpike* but to a weaker extent. More precisely, the estimated effect of the previous partner's choice to cooperate in respectively *Turnpike* and *Random6* is 6.5 and 8.3 percentage points lower than in *Random4*. Results are qualitatively similar when controlling for risk and social preferences.[10] We summarize our second result as follows:

Result 2. *In matching groups of four the previous partner's decision tends to influence the current decision to cooperate more strongly than in matching groups of six or than under a turnpike protocol.*

3 The meta-study

3.1 Description of the data

The data used in the meta-study is from laboratory experiments featuring a linear public goods game (PGG) that meets the following requirements: 1. players are randomly matched after each round of

[10]Conclusions are qualitatively similar when including two lags (see Table C.8 in the Appendix).

play (strangers matching)[11], 2. the experiment is incentivized, 3. the time horizon is finite and commonly known, 4. participants choose their contribution to the public good simultaneously, 5. the PGG is symmetric (same endowments, costs, etc.), 6. communication among participants is not allowed, 7. participants are not reported to have taken part in another task before the PGG, 8. the study is published in a scientific journal.

The following procedure was followed to collect the data.[12] First, a list of experimental studies on PGGs was compiled from studies listed in Zelmer (2003) and studies found on Scopus in November 2012 using several combinations of keywords such as experiment(s), voluntary contribution(s), public goods, linear, laboratory, subjects, participants. Only published studies in English language were included to which there was access via the online library of Tilburg University. Second, e-mails were sent to the authors of the listed studies asking for details on their experiments that were hard to infer from the paper. Third, in February 2015 a query was made on the ESA Google Groups mailing list for experimental methods discussion to complete the list with missing references reporting on experiments fulfilling the above-mentioned conditions. This procedure yields 23 data points from 14 studies on PGG experiments with strangers matching where the unit of observation refers to the treatment level. Section D in the Appendix reports the list of studies.

For each study a number of variables was recorded at the treatment level. The variables include average contribution to the public good, size of the interaction group, total number of participants, number of matching groups (independent observations), number of rounds, and marginal per capita return of the public good (MPCR). We measure the cooperation rate by the average contribution to the public good across all rounds expressed as a percentage of the initial endowment. Since we are interested in studying how the likelihood of meeting the same partner(s) again affects cooperation, the re-matching probability will be the independent variable of main interest. We defined it as the probability of meeting at least one of the current group members again in the next round, and compute it as $\frac{(size\ of\ interaction\ group-1)}{(size\ of\ matching\ group-1)}$. The size of the matching group is not always constant within treatments. We proxy it by the total number of participants in a treatment divided by the number of matching groups collected for that treatment. Participants are typically aware of the size of the matching group, because matching groups typically correspond to sessions in public goods experiments. The upper panel of Table 4 reports a range of summary statistics for these data.

3.2 Results

Figure 2 shows scatter plots of the relation between the re-matching probability and first-round cooperation rates (left-hand-side panel) and overall cooperation rates (right-hand-side panel). As can be seen,

[11]We did not come across PGG experiments using a turnpike protocol.

[12]The data were collected at the same time and using the same procedure as the data used in Fiala and Suetens (2017).

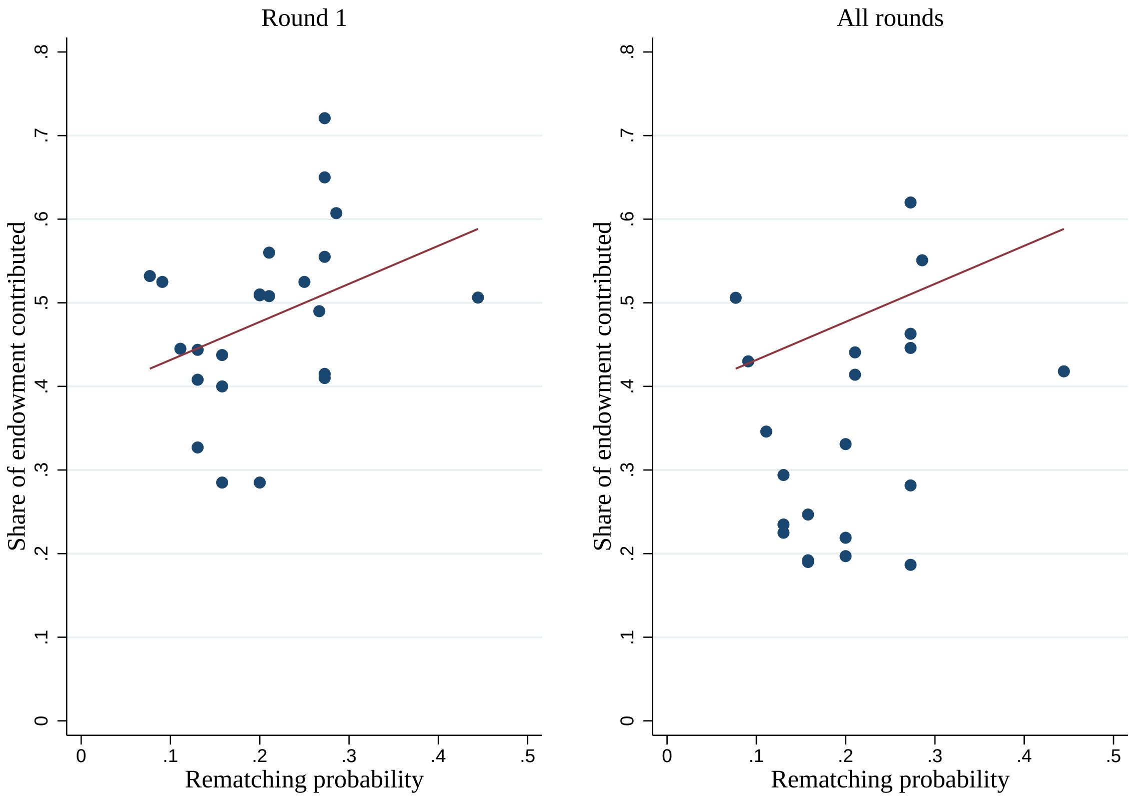
Table 4: Summary statistics of variables in the meta-study.

	Obs.	Mean	St. dev.	Min	Max
Share of endowment contributed in first round	23	0.48	0.11	0.28	0.72
Share of endowment contributed in all rounds	21	0.34	0.13	0.19	0.62
Rematching probability	23	0.21	0.08	0.08	0.44
MPCR	23	0.53	0.15	0.30	0.75
Size of interaction group	23	4.00	0.90	2	5
Size of matching group	23	16.26	4.49	10	24

Notes: We were unable to retrieve the average contribution in all rounds for the two treatments in the study of Carpenter (2004).

the fit line highlights a positive relationship in both panels.

Figure 2: Rematching probabilities and contributions to public good.



Notes: Left-hand-side: share of endowment contributed in first rounds as a function of re-matching probability. Right-hand-side: share of endowment contributed across all rounds as a function of re-matching probability. Each dot represents a treatment of a linear public good experiment (see Table 4). The red lines plot the prediction from a linear regression of the share of endowment contributed on the rematching probability.

Table 5 reports the estimates from linear regressions where the cooperation rate, measured by the share of the endowment contributed to the public good, is regressed on the re-matching probability (under 'Linear') or on a binary variable indicating whether the re-matching probability is strictly higher than the median, equal to 0.2 (under 'Median-split'). The reason why we include the latter results is

Table 5: Effect of re-matching probability on cooperation in public goods games.

Dep. var.: Share of endowment contributed	Linear				Median-split			
	Round 1		All rounds		Round 1		All rounds	
Rematching probability	0.475*	(0.085)	0.761***	(0.009)	0.086*	(0.094)	0.143**	(0.010)
MPCR	0.186	(0.224)	0.756***	(0.001)	0.175	(0.254)	0.786***	(0.001)
Constant	0.271**	(0.015)	-0.212	(0.103)	0.323***	(0.002)	-0.154	(0.191)
R^2	0.198		0.535		0.191		0.530	
Adjusted R^2	0.117		0.483		0.120		0.478	
Observations	23		21		23		21	

Notes: Results from linear regressions weighted by the number of independent observations by treatment are reported. The unit of observation is a treatment in a study. P-values are in parentheses; * $P < 0.1$, ** $P < 0.05$, *** $P < 0.01$.

that this allows a more direct comparison of the marginal effect of the re-matching probability with the results (marginal effects) obtained in the PD experiment. The regressions are weighted by the number of independent observations (matching groups) to account for the fact that this number is quite heterogeneous across the experiments.[13] We report specifications where the share of endowment contributed (the cooperation rate) is calculated across first rounds of play as well as across all rounds. Given that the MPCR has been shown to have a large effect on contributions (e.g. Zelmer, 2003; Fiala and Suetens, 2017), it is included as a control.[14]

As can be seen in Table 5, the re-matching probability has a large and positive effect on the contribution rate in first rounds as well as in all rounds. In the first rounds, the marginal effect is about 48 percentage points and is significant at the 10% level. Across all rounds, the effect is equal to 76 percentage points and significant at the 1% level. If we take the contribution rate in last rounds as a dependent variable (not reported in the table) we find that the positive effect persists.[15] The results of the 'median-split' regressions show that an increase in the re-matching probability from below 0.2 to strictly above 0.2 leads to an increase in the cooperation rate of 8.6 percentage points in first rounds and 14.3 percentage points in all rounds. The latter effect is qualitatively similar to that in the PD experiment. To conclude, except for the last round, the re-matching probability seems to have a similar effect as in the above-reported PD experiment. We summarize our third result as follows:

Result 3. *Contributions to a public good are higher when group members face a higher probability of meeting at least one group member again in the next round of decision-making.*

[13]Results are qualitatively similar in unweighted regressions.

[14]We also ran regressions controlling for the size of the interaction group or the size of the matching group to rule out the concern that the effect of our re-matching probability would capture the effect of either of these variables on contributions. Results of these regressions are qualitatively similar (see Table C.9 in the Appendix).

[15]The marginal effect of the re-matching probability on the contribution rate in the last round is equal to about 73 percentage points $P = 0.036$.

4 Conclusion

We find evidence consistent with the conjecture that contagion effects can influence behavior under random re-matching in a PD experiment. Our experiment shows that under imperfect strangers, first-round and overall cooperation rates are substantially higher in matching groups of four, where the probability of being matched with the same player in the future is relatively high, than in matching groups of six, and than under a perfect strangers turnpike protocol. Likewise, in a meta-analysis using data from public goods experiments higher re-matching probabilities are associated with significantly higher contributions.

The data of the PD experiment allow to study in more detail how players react to past choices of others. In particular, we find that in our random matching treatment with matching groups of four, the choice to cooperate tends to depend more strongly on previous partners' cooperation choices than in the other two treatments, where the probability of meeting the same player again is relatively low. This is consistent with Mengel and Peeters (2011) who find that under partner matching and 'hot' decision making, players tend to condition more strongly on the partner's past behavior than under random matching. The result is also consistent with the notion that contagion may be more likely to play a role, the larger the re-matching probability. Given that first-round cooperation rates are also higher when matching groups have only four players than in the other two treatments, players appear to foresee (absence of) possible contagion.

We find that the treatment effect persists in the last round of game play in the meta-study, and not so in the PD experiment. We speculate that this may be driven by the following mechanism. Due to the wide range of possible levels of contribution in public goods games it is less clear than in PDs what should be taken as a 'defection'. If in the last round players 'defect' by contributing a bit less than the partners rather than by contributing nothing at all (see for example Fischbacher and Gächter, 2010), behavioral differences between treatments that differ in terms of re-matching probabilities may persist also in the last round. This is in contrast to the PD, where players can defect in just one way.

Our results contribute to the literature that identifies conditions under which contagion may occur in dilemma games where players may meet the same partner again in the future. The results are relevant for theoreticians. Theoretical models show that cooperation and contagion effects can arise under random matching in the context of an infinitely repeated game (Kandori, 1992; Ellison, 1994; Camera and Casari, 2009). We show that these effects can also arise with a finite and commonly known horizon. This suggests that a mechanism of reputation building of the kind of Kreps, Milgrom, Roberts, and Wilson (1982) may not only be at work in games with partner matching—where the re-matching probability is equal to one—but also in settings where partners change with some probability after each round of decision-making. In this sense, the results of our PD experiment fit well with the finding that cooperation rates in experiments where participants are matched with the same partner throughout a repeated game are typically higher than in experiments with random matching (for example, Andreoni and Miller, 1993; Keser and van Winden, 2000; Mengel, 2017).

Our results are also relevant for experimental economists. The choice for a random matching design rather than a perfect strangers design as a means of ruling out reputational effects or contagion may not be innocent. Moreover, although it is an open question whether differences in re-matching probability influence the extent to which cooperation spreads or diminishes across supergames of randomly matched individuals, our results may help to understand why cooperation rates sometimes increase across supergames, and sometimes decrease. For example, they may help to explain why (first-round) cooperation rates in the experiment of Camera and Casari (2009)—that has a relatively high re-matching probability—increase across supergames whereas in Duffy and Ochs (2009)—that has a relatively low re-matching probability—cooperation tends to diminish across supergames.

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Appendix

A Experimental Instructions

General instructions

You are participating in a study on economic decision making. Your earnings will depend on your decisions and the decisions of the other participants. You will be paid in cash and in private at the end of this study.

The entire study is anonymous: that is, your identity will not be revealed to others and the identity of others will not be revealed to you. You are not allowed to communicate with other participants during the study. Should you have any questions at any time during the study, please raise your hand and your question will be answered in private.

The experiment will consist of three Tasks. You will be given detailed instructions about each of the Tasks before they begin. Please, follow these instructions carefully.

During the experiment your earnings will be expressed in points. Your earnings will depend on the points you earn in the three tasks. Points will be converted to EUR at the following rate: 140 points = €1. In addition, you will also receive a €2 show-up fee.

Instructions for Task 1

CHOICES AND POINTS

In Task 1 you must make **ten** decisions. In each decision you must choose between two lotteries: **Lottery A** and **Lottery B**. Each lottery has two possible outcomes: a low outcome and a high outcome. The two possible outcomes of Lottery A are different from the two possible outcomes of Lottery B.

The low and high outcomes within Lottery A and Lottery B are always the same in all ten decisions. However, the chance of these outcomes occurring does change across decisions.

On your computer screen you will be shown a table. In each row of the table you must make a choice between Lottery A and Lottery B. The table below provides two examples of such choices:

Make your choices here

Decision	Lottery A	Lottery B	Choice
1	20% chance of 15, 80% chance of 20	20% chance of 0, 80% chance of 40	A ◉ ○ B
2	10% chance of 15, 90% chance of 20	10% chance of 0, 90% chance of 40	A ○ ◉ B

In Decision 1:

- If you choose Lottery A, you will have a 20% chance of receiving 15 points, and an 80% chance of receiving 20 points.
- If you choose Lottery B, you will have a 20% chance of receiving 0 points, and an 80% chance of receiving 40 points.

In Decision 2:

- If you choose Lottery A, you will have a 10% chance of receiving 15 points, and a 90% chance of receiving 20 points.
- If you choose Lottery B, you will have a 10% chance of receiving 0 points, and a 90% chance of receiving 40 points.

Please choose either Lottery A or Lottery B in each of the ten rows. You may choose different lotteries in different rows.

EARNINGS

The points you earn for Task 1 will depend on one of the ten decisions you make which will be selected at random by the computer. Specifically, the computer will:

- randomly select one of the ten lotteries you chose in Task 1; and
- determine the outcome of the selected lottery and the points you receive.

The outcome of the lottery and your earnings from Task 1 will be shown to you after all tasks have been completed.

QUIZ

Before proceeding with Task 1, we ask you to answer two questions to verify your understanding of the instructions.

Instructions for Task 2

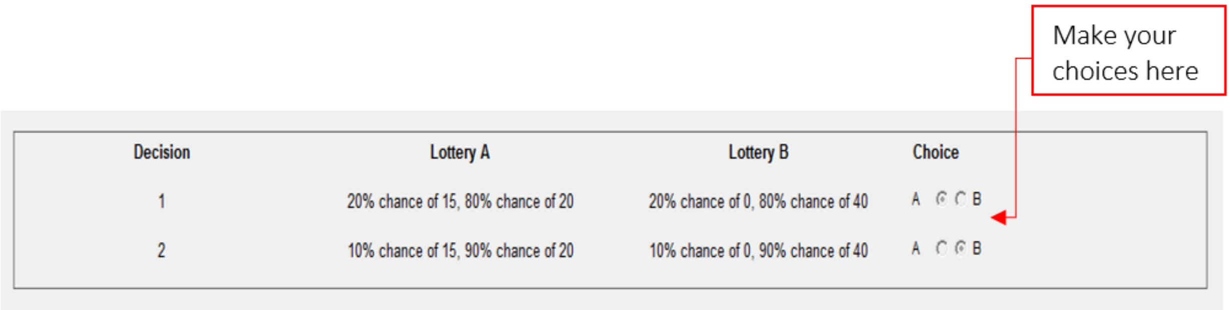
In Task 2, there are two roles: **Decider** and **Recipient**. At the beginning of Task 2, you will be randomly assigned to one of these roles. You will not know which role you have been assigned until the end of the study. However, you will make decisions as the **Decider**.

On your computer screen you will be shown four tables, one after the other. In each table you must make **5 choices** between two options: **Option A** and **Option B**. The two options define how many points the decider (you) get and how many points the recipient gets. Each one of the 5 choices in a period is associated with different points for you and for the participant you are matched with.

An example choice is shown below.

In this example:

- If you choose Option A, the Decider (you) receive 15 points and the Recipient 5 points.
- If you choose Option B, the Decider (you) receive 12 points and the Recipient 10 points.



Decision	Lottery A	Lottery B	Choice
1	20% chance of 15, 80% chance of 20	20% chance of 0, 80% chance of 40	A B
2	10% chance of 15, 90% chance of 20	10% chance of 0, 90% chance of 40	A B

EARNINGS

After you have completed your choices, the computer will:

- randomly select one of the four tables;
- randomly select one of the five choices within the selected table;
- allocate earnings to you and the other participant you were matched with in the selected choice based on the roles you were randomly assigned at the beginning of the experiment.

If you were assigned the role of decider, then you and the recipient you were matched with will receive points based on **your** choice. If you were assigned the role of recipient, you will receive the amount of points as determined by the choice of the **other** participant.

QUIZ

Before proceeding with Task 2, we ask you to answer two questions to verify your understanding of the instructions.

Instructions for Task 3 (*Turnpike* treatment)

MATCHES

Task 3 consists of 10 consecutive rounds of interaction. In each round, you will interact with one other participant. At the beginning of each round, you will be matched with a new participant. You will not be matched with the same participant more than once throughout the entire task. In addition, the participants you are matched with will not interact with each other. This means you will not be matched with a participant that was paired with someone that was paired with you, or with someone that was paired with someone that was paired with someone that was paired with you, and so on.

As a result of this, the decision you make in one round can have no effect on the decisions of individuals you are matched with in later rounds.

You will not know the identity of the participant you are matched with, nor will matched participants know your identity.

CHOICES AND POINTS

In each round, both you and the other participant will have two possible choices: X or Y.

As you can see in the table below, the points you and the other participant receive are based on the decisions you both make.

	You receive	The other participant receives
You both choose X	150	150
You choose X and the other participant chooses Y	38	210
You choose Y and the other person chooses X	210	38
You both choose Y	68	68

If both you and the other participant choose X, you will both receive 150 points.
If both you and the other participant choose Y, you will both receive 68 points.
If you choose X and the other participant chooses Y, then you will receive 38 points and the other participant will receive 210 points.
If you choose Y and the other participant chooses X, you will receive 210 points and the other participant will receive 38 points.

You and the other participant will make your choices simultaneously, and you will only learn the choice of the other participant at the end of the round.

The results screen will display the number of points you have earned for the current round along with your choice, and the choice of the participant you were matched with.

EARNINGS

Your final earnings for Task 3 depends on all the points you accumulate over all 10 rounds.

QUIZ

Before proceeding with Task 3, we ask you to answer two questions to verify your understanding of the instructions.

B Measurements of risk and social preferences

B.1 Risk preferences

To measure risk preferences we used a lottery task based on Holt and Laury (2002) multiple price list lotteries. Participants are presented with a choice list of ten paired lotteries (Figure B.3). In each choice, participants must select one of the two lotteries. The payoffs of the two possible lotteries are constant across the ten choices. Lottery A is characterized by a smaller gap in terms of payoffs from the two possible outcomes than Lottery B. On the one hand, Lottery B allows to win up to 192 tokens, while the highest payoff from Lottery A is 100 tokens. On the other hand, Lottery B has a minimum payment of only 5 tokens against the 80 tokens of Lottery A. The probabilities of the lotteries' outcomes vary across decisions so that the lotteries' expected values are different in every choice. The choice at which a participant switches from Lottery A to Lottery B gives an indication of her degree of risk aversion. Participants in the experiment received points based on one of the ten choices selected at random.

The average choice at which a participant switches from Lottery A to Lottery B is choice 7 with a standard deviation of roughly 2. 10 participants failed to deliver a consistent set of choices, switching among the two lotteries more than once. For these participants we only consider the first choice where they switch to Lottery B.

Figure B.3: Payoffs for the lottery choice task.

Choice	Lottery A				Lottery B				Diff.in expected value
1	1 in 10 of	100	9 in 10 of	80	1 in 10 of	192	9 in 10 of	5	58.3
2	2 in 10 of	100	8 in 10 of	80	2 in 10 of	192	8 in 10 of	5	41.6
3	3 in 10 of	100	7 in 10 of	80	3 in 10 of	192	7 in 10 of	5	24.9
4	4 in 10 of	100	6 in 10 of	80	4 in 10 of	192	6 in 10 of	5	8.2
5	5 in 10 of	100	5 in 10 of	80	5 in 10 of	192	5 in 10 of	5	-8.5
6	6 in 10 of	100	4 in 10 of	80	6 in 10 of	192	4 in 10 of	5	-25.2
7	7 in 10 of	100	3 in 10 of	80	7 in 10 of	192	3 in 10 of	5	-41.9
8	8 in 10 of	100	2 in 10 of	80	8 in 10 of	192	2 in 10 of	5	-58.6
9	9 in 10 of	100	1 in 10 of	80	9 in 10 of	192	1 in 10 of	5	-75.3
10	10 in 10 of	100	0 in 10 of	80	10 in 10 of	192	0 in 10 of	5	-92

B.2 Social preferences

To elicit social preferences we used a modified dictator game based on Kurschilgen (2017) and Iriberri and Rey-Biel (2011). In the games, a dictator is asked to make 20 choices between two options, each of which allocates different combinations of payoffs to the dictator and the recipient (Figure B.4). In all choices, Option A is profit maximizing for the dictator, while Option B costs 10 points to the dictator, but allows her to create additional income for the recipient or destroy part of the recipient's income. The number of points that dictators can create or destroy changes across the 20 choices. In the experiment, the 20 choices were presented in four sequential tables. Each table corresponds to one of the four quadrants depicted in Figure B.4. All participants made each of the 20 choices in the role of dictator (strategy method). At the end of the experiment the computer randomly selected one of the 20 choices for payment, matched participants in pairs, and randomly assigned them to the role of dictator or recipient.

The choice setting allows to analyze participants' social preferences in the framework proposed by Charness and Rabin (2002). The utility function of individual i is defined as

$$U_i(a_i) \equiv \begin{cases} (1-\rho)\pi_i(a_i) + \rho\pi_j(a_i) & \text{if } \pi_i \geq \pi_j \\ (1-\sigma)\pi_i(a_i) + \sigma\pi_j(a_i) & \text{if } \pi_i \leq \pi_j \end{cases} \tag{1}$$

where a_i is the choice of individual i in the role of dictator between Option A and Option B, and the parameters $\sigma,\ \rho \in \mathbb{R}$ allow to rationalize choices that deviate from pure self-interest (this occurs whenever a dictator selects the costly Option B). More precisely, the parameter ρ measures the weight individual i puts on the payoff of j if i is ahead of j in monetary terms. The parameter σ measures the weight individual i puts on the payoff of j if i is behind j in monetary terms. When $\pi_i \geq \pi_j$, one can hence write

$$\pi_i^B - \pi_i^A \geq \rho \left[(\pi_i^B - \pi_i^A) - (\pi_j^B - \pi_j^A) \right] \tag{2}$$

When $\pi_i \leq \pi_j$, the same goes through for σ. It is now straightforward to derive the minimum values that σ and ρ must take to rationalize a dictator's choice for the costly Option B, for each one of the 20 choices. These threshold values for σ and ρ are reported in Figure B.4.

Out of 142 participants, none have a negative ρ, while 16 of them have a negative σ. The average estimated ρ was 0.26 with a standard deviation of 0.18. Participants are more heterogeneous for what concerns parameter σ, with an average estimated σ of 0.06 and a standard deviation of 0.78. Overall, 7 participants are inconsistent with respect to ρ, i.e. they exhibit both a positive and a negative estimate of ρ because their choices were inconsistent between Panel 1 and Panel 3 of Figure B.4. With respect to σ, 9 participants are inconsistent because their choices were inconsistent between Panel 2 and Panel 4 of Figure B.4. In the empirical analyses we include these participants and assign them their positive estimate of both ρ and σ.

Figure B.4: Payoffs and social preference estimates for the modified dictator game.

		Ahead					Behind				
		Panel 1					Panel 2				
Create	Option A	π^A_{Dec} 170	π^A_{Rec} 70				π^A_{Dec} 110	π^A_{Rec} 120			
	Decision	π^B_{Dec}	π^B_{Rec}	Δ_{Dec}	Δ_{Rec}	$\rho \geq$	π^B_{Dec}	π^B_{Rec}	Δ_{Dec}	Δ_{Rec}	$\sigma \geq$
	1	160	82	-10	12	0.45	100	132	-10	12	0.45
	2	160	88	-10	18	0.36	100	138	-10	18	0.36
	3	160	102	-10	32	0.24	100	152	-10	32	0.24
	4	160	124	-10	54	0.16	100	174	-10	54	0.16
	5	160	154	-10	84	0.11	100	204	-10	84	0.11
		Panel 3					Panel 4				
Destroy	Option A	π^A_{Dec} 140	π^A_{Rec} 130				π^A_{Dec} 90	π^A_{Rec} 180			
	Decision	π^B_{Dec}	π^B_{Rec}	Δ_{Dec}	Δ_{Rec}	$\rho \leq$	π^B_{Dec}	π^B_{Rec}	Δ_{Dec}	Δ_{Rec}	$\sigma \leq$
	1	130	118	-10	-12	-5.00	80	168	-10	-12	-5.00
	2	130	112	-10	-18	-1.25	80	162	-10	-18	-1.25
	3	130	98	-10	-32	-0.45	80	148	-10	-32	-0.45
	4	130	76	-10	-54	-0.23	80	126	-10	-54	-0.23
	5	130	46	-10	-84	-0.14	80	96	-10	-84	-0.14

Notes: π^A_{Dec} is the payoff for the decider and π^A_{Rec} is the payoff for the receiver if the decider chooses Option A. π^B_{Dec} is the payoff for the decider and π^B_{Rec} is the payoff for the receiver if the decider chooses Option B in a particular row. Δ_{Dec} and Δ_{Rec} is the differences between the the decider and receiver's payoffs of Option B relative to Option A.

C Additional figures and tables

Table C.6 shows an example of turnpike matching. Participants 1–3 and participants 4–6 are never matched together. In addition, from participants 1's perspective, each of their matches goes on to match with participants 3 and then 2 such that their matches cannot indirectly influence the behavior of future matches.

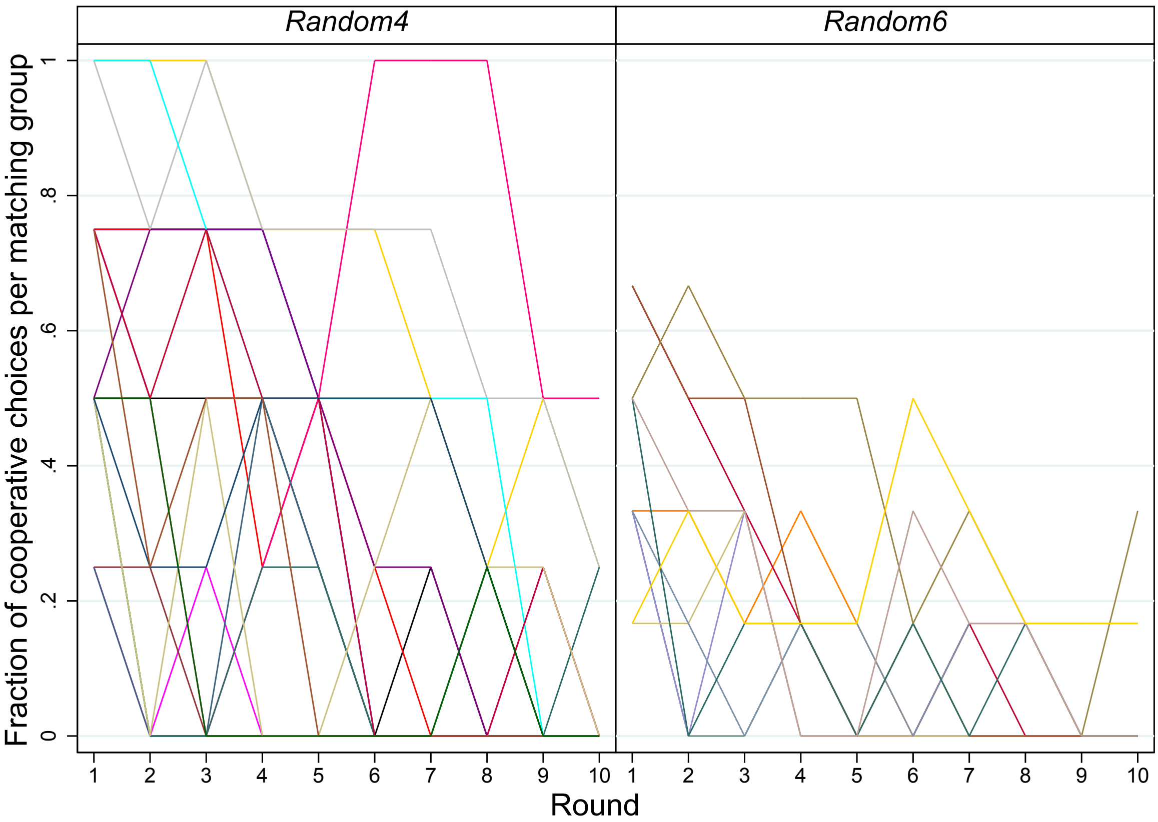
Table C.6: Example of turnpike protocol with six participants and three rounds.

Round	Participant	Partner	Pair
1	1	4	1
1	2	5	2
1	3	6	3
1	4	1	1
1	5	2	2
1	6	3	3
2	1	5	1
2	2	6	2
2	3	4	3
2	4	3	3
2	5	1	1
2	6	2	2
3	1	6	1
3	2	4	2
3	3	5	3
3	4	2	2
3	5	3	3
3	6	1	1

Table C.7: Experimental sessions.

Treatment	Session	Participants	Dates and times
Turnpike	1	20	16-06-2016, 10:30am
	2	20	08-09-2016, 2:30pm
	3	20	29-11-2017, 11:30am
	4	20	29-11-2017, 2:00pm
Random6	1	18	18-10-2016, 11:00am
	2	12	18-10-2016, 2:00am
	3	18	29-11-2017, 10:00am
	4	18	29-11-2017, 4:00pm
Random4	1	16	22-06-2016, 10:30am
	2	20	22-06-2016, 2:00pm
	3	20	22-06-2016, 4:00pm
	4	16	23-06-2016, 2:00pm

Figure C.5: Cooperation dynamic at the matching group level.



Note: Each line refers to one matching group.

Table C.8: Estimated effect of past partner's decisions with two lags.

Dep. var.: Cooperate in t (yes=1)	(1)		(2)	
Partner in $t-1$ cooperated	0.220***	(0.000)	0.210***	(0.000)
Partner in $t-2$ cooperated	0.151***	(0.000)	0.145***	(0.000)
Random6	-0.021	(0.525)	-0.018	(0.607)
Turnpike	0.016	(0.563)	0.013	(0.663)
Random6 $\times$ Partner in $t-1$ cooperated	-0.090***	(0.000)	-0.079***	(0.002)
Random6 $\times$ Partner in $t-2$ cooperated	-0.033	(0.289)	-0.020	(0.541)
Turnpike $\times$ Partner in $t-1$ cooperated	-0.089***	(0.000)	-0.084***	(0.000)
Turnpike $\times$ Partner in $t-2$ cooperated	-0.071**	(0.012)	-0.065**	(0.015)
t	-0.022***	(0.000)	-0.022***	(0.000)
Degree of risk aversion			-0.007	(0.381)
ρ			0.358***	(0.000)
σ			0.013	(0.482)
Observations	1744		1744	
Matching groups	33		33	

Notes: Marginal effects from a probit regression are reported. Random effects are at matching group level and standard errors are clustered at matching group level. P-values are in parentheses. * $P < 0.1$, ** $P < 0.05$, *** $P < 0.01$. The variable ρ (σ) correspond to the weight attached to a randomly matched anonymous other person's payoff if the participant is in monetary terms ahead (behind) the other person. See Section B of the Appendix for details on the calculation of risk and social preferences.

Table C.9: Effect of re-matching probability on cooperation in public goods games with additional controls.

Dep. var.: Share of endowment contributed	Round 1 (1)		Round 1 (2)		All rounds (3)		All rounds (4)	
Rematching probability	0.567	(0.107)	0.286	(0.416)	0.601*	(0.094)	0.794*	(0.055)
MPCR	0.192	(0.221)	0.168	(0.278)	0.829***	(0.002)	0.768***	(0.004)
Number of rounds								
Size of interaction group	-0.017	(0.655)			0.035	(0.465)		
Size of matching group			-0.006	(0.403)			0.001	(0.906)
Constant	0.319**	(0.045)	0.422*	(0.053)	-0.355	(0.139)	-0.241	(0.392)
R^2	0.206		0.227		0.550		0.535	
Adjusted R^2	0.081		0.105		0.470		0.453	
Observations	23		23		21		21	

Notes: Results from linear regressions weighted by the number of independent observations by treatment are reported. The unit of observation is a treatment in a study. P-values are in parentheses; * $P < 0.1$, ** $P < 0.05$, *** $P < 0.01$.

D List of public goods studies

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