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Social Preferences and the Variability of Conditional Cooperation

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Abstract

We experimentally examine how incentives affect conditional cooperation (i.e., cooperating in response to cooperation and defecting in response to defection) in social dilemmas. In our first study, subjects play eight Sequential Prisoner's Dilemma games with varying payoffs. We elicit second mover strategies and find that most second movers conditionally cooperate in some games and free ride in others. The rate of conditional cooperation is higher when the own gain from defecting is lower and when the loss imposed on the first mover by defecting is higher. This pattern is consistent with both social preference models and stochastic choice models. In a second study subjects play 64 social dilemma games, and we jointly estimate noise and social preference parameters at the individual level. Most of our subjects place significantly positive weight on others' payoffs, supporting the underlying role of social preferences in conditional cooperation. Our results suggest that conditional cooperation is not a fixed trait but rather a symptom of the interaction between game incentives and underlying social preferences.

Keywords: sequential prisoner's dilemma, conditional cooperation, social preferences

JEL codes: A13, C91

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1 Introduction

Conditional cooperation is widely observed in social dilemmas. Whereas the pursuit of narrowly defined selfish interests would result in a lack of cooperation, many people are willing to forgo their selfish interests and cooperate, but only if others do so as well. This pattern of behavior is particularly clear in controlled experiments investigating contributions to public goods.[1] These experiments also reveal substantial heterogeneity: for example, in some of these studies (see Thöni and Volk (2018) for a review) some group members are classified as "free-riders" (i.e., defecting regardless of the behavior of others), others as "conditional cooperators" (i.e., cooperating if others do so), and still others as "unconditional cooperators" (i.e., cooperating independently of the behavior of others). Identifying these heterogeneities is crucial when trying to understand what makes individuals cooperate and which measures to take to further enhance cooperation.

Despite this commonly applied classification, not much is known, however, about whether it reflects stable personality traits whereby the participant would exhibit similar behavioral patterns in similar situations, or whether the classification applies only to the specific experimental setting and parameters. There is also surprisingly little evidence on how the specific material payoffs of the game affect conditional cooperation. In case that the degree of conditional cooperation varies with game parameters, it is fundamental to understand the mechanisms and account for them when studying group cooperation. In this study, we examine whether and how the behavioral pattern exhibited by an individual, such as conditional cooperation, varies in response to changes in the material incentives.

Examining the within-subject variability of conditional cooperation across payoff variations is important for at least two reasons. First, it allows us to understand the nature of conditional cooperation: whether conditional cooperation reflects underlying social preferences, or whether conditional cooperation reflects a desire to reciprocate the cooperation of others in a way that is robust to changes in material incentives.[2] Social preference models,

[1] See, e.g., Bilancini, et al. (2022); Brandts and Schram (2001); Chaudhuri and Paichayontvijit (2006); Croson (2007); Cubitt, et al. (2017); Fischbacher, et al. (2001); Fischbacher and Gächter (2010); Furtner, et al. (2021); Gächter, et al. (2017b); Gächter, et al. (2022); Isler, et al. (2021); Keser and van Winden (2000); Kocher, et al. (2008); Weber, et al. (2018); Weber, et al. (2023). For reviews see Chaudhuri (2011); Fehr and Schurtenberger (2018); Gächter (2007); and Thöni and Volk (2018).

[2] As discussed by Bardsley and Sausgruber (2005), Fehr and Fischbacher (2004), Gächter, et al. (2017a), and Katuščák and Miklánek (2023), conformity to what is perceived as "socially appropriate" and willingness to sacrifice material payoffs in order to follow such norms could also be a candidate explanation for conditional cooperation.

which define preferences over one's own and other's material payoffs (e.g., Andreoni and Miller (2002); Bolton and Ockenfels (2000); Charness and Rabin (2002); Cox, et al. (2007); Cox, et al. (2008); Fehr and Schmidt (1999); Fisman, et al. (2007)), are capable of explaining conditional cooperation, but at the same time these models predict that it will be influenced by material incentives. In contrast, if conditional cooperation reflects a principled stand against free-riding, eschewing material gains to reciprocate the cooperation of others, then conditional cooperation is expected to be robust across payoff variations.

Second, the efficacy of interventions to promote cooperation depends on whether conditional cooperation is influenced by payoff variations. For example, leading by example would be an effective mechanism to achieve cooperative outcomes if followers are generally conditionally cooperative (e.g., Gächter, et al. (2012)). On the other hand, if conditional cooperation is sensitive to payoffs, then this implies that there are settings where leading by example is ineffective.

We study within-subject variability of conditional cooperation using two experimental designs. Our first experimental design is based on a sequential prisoner's dilemma in which a First-mover (FM) chooses either cooperate or defect, and a Second-mover (SM) can condition their choice, cooperate or defect, on FM’s choice. Mutual cooperation maximizes combined earnings, but whatever FM’s decision, SM maximizes own earnings by defecting. Thus, a selfish SM who maximizes own earnings should defect regardless of FM's choice.

We elicit SM strategies in eight games with varying payoffs by asking how the subject would respond to defect and how they would respond to cooperate, with their actual decision being determined by their response to their opponent’s actual FM choice. This allows us to classify SM strategies as free-riding (i.e. defection regardless of FM’s choice), unconditional cooperation (i.e., cooperation regardless of FM’s choice), conditional cooperation (i.e., cooperation in response to cooperation and defection in response to defection), or mismatching (i.e., cooperation in response to defection and defection in response to cooperation).

We find that 72% of subjects change their SM strategy at least once across games, and 58% of subjects conditionally cooperate in at least one game and free ride in another. Moreover, changes in behavior are systematically related to payoffs: SM are more likely to conditionally cooperate when they have less to gain from free-riding, and when free-riding has a larger negative impact on the FM’s earnings.

This pattern is consistent with the predictions of several social preference models (e.g., Charness and Rabin (2002); Fehr and Schmidt (1999)), but it is also consistent with stochastic choice models where subject choices are determined by selfish preferences plus noise (e.g., Anderson, et al. (1998)). Thus, we developed a second experiment where subjects make choices in sixty-four sequential dilemma games with varying payoffs in order to jointly estimate individual-level noise and social preference parameters.

We find that relatively few SMs consistently respond to cooperation by maximizing their own payoff (12%) or by cooperating (6%). Instead, most SMs vary their responses to cooperation across games in a way that is systematically, though not deterministically, related to payoffs. For 72% of SMs we estimate a model incorporating social preferences and find that most of these (representing 66% of all SMs) have significantly positive social preference parameters that place a positive weight on their opponent's payoff.

Our findings from these experiments have two main implications. First, any classification of individuals as "conditional cooperators" or "free-riders" in one game should not be generalized to other games with different material payoffs: a conditional cooperator in one game may be a free-rider in another, and vice versa. Secondly, conditional cooperation varies with material payoffs in a systematic way, reflecting underlying social preferences.

The remainder of the paper is organized as follows. In Section 2, we place our contributions in the related literature that examines the variability of conditional cooperation and the relationship between social preferences and conditional cooperation. We describe the design and results of our experiments in Sections 3 and 4. In Section 5 we discuss related findings on conditional cooperation in public goods experiments. Section 6 concludes.

2 Related literature and our contributions

Several previous papers have examined the variability of conditional cooperation over time, by measuring conditional cooperation repeatedly but keeping payoff functions constant. The results are mixed. Brosig, et al. (2007) conducted sequential prisoners dilemma games three times within three months using the same subjects and random-matching and found that the rate of conditional cooperation diminished across repetitions. This finding is supported by Andreozzi, et al. (2020) who also found that conditional cooperation diminished with repetition. Exploring public goods games, Muller, et al. (2008) elicited subjects' strategies across five repetitions. Although only 37% of subjects always chose the same strategy across all five games, previous choices were useful predictors of subsequent choices. For example, 69% of subjects who conditionally cooperated in any of the first four games also conditionally cooperated in the fifth game. Volk, et al. (2012) elicited subjects' strategies in a public goods game three times over the course of five months and observed that conditional cooperation was remarkably stable over time. Half of their subjects chose the same strategy in all three games, and 71% of these conditionally cooperated. In a closely related analysis, Gächter, et al. (2022) report stability rates of 66% and 59% in their provision and maintenance versions of public goods games played four months apart.[3]

Our approach differs from this previous literature because we examine robustness to payoff variation across games. We are not aware of any previous study examining how within-subject variation of payoffs affects conditional cooperation.[4] We are only aware of three studies that examine whether between-subject payoff variation affects conditional cooperation. Thöni and Volk (2018) found that the proportion of conditional cooperators is similar across 17 public goods experiments, which employ different parameters (i.e., different marginal per capita returns from the public good and different group sizes). Gächter and Marino-Fages (2023) elicit preferences for conditional cooperation in one-shot public goods games that also vary marginal per capita returns and group sizes and found surprisingly little variation. In contrast, Clark and Sefton (2001), using a between-subjects sequential prisoners dilemma experiment in which subjects played repeatedly against changing opponents with feedback on the outcomes of each play, found that doubling the temptation payoff, T, resulted in a significantly lower rate of conditional cooperation. Our studies differ from these in that we ask subjects to make decisions in eight games with systematically varying payoffs and without feedback across games. This within-subject design allows us to examine how changes in payoffs affect conditional cooperation at the individual level.

Our paper also contributes to a literature using estimates of social preferences to explain decisions in experimental social dilemmas. One of the first papers in this literature is Blanco, et al. (2011). They measure parameters of disadvantageous and advantageous inequality aversion (Fehr and Schmidt (1999)) using ultimatum and modified dictator games and then have the same subjects play a sequential prisoners dilemma and a public good game. They find that the elicited preference parameters predict decisions at the aggregate level but not so much

[3] Two further studies (Eichenseer and Moser (2020) and Mullett, et al. (2020)) examine the variability of conditional cooperation across different contexts by comparing behavior in a public goods game and a sequential prisoners dilemma. Both studies report that subjects who are conditionally cooperative in the sequential prisoner's dilemma are also conditionally cooperative in the public goods game.

[4] Several studies examine how decisions in the simultaneous prisoner's dilemmas are influenced by payoff variations (e.g., Ahn, et al. (2001); Au, et al. (2012); Charness, et al. (2016); Engel and Zhurakhovska (2016); Mengel (2018); Ng and Au (2016); Schmidt, et al. (2001); Vlaev and Chater (2006)). See Gächter, et al. (2024) for a discussion of these papers and a systematic experimental analysis of the role of payoff parameters for cooperation in prisoner's dilemma experiments.

at the individual level. Hedegaard, et al. (2021) elicit distributional preferences in a representative Danish sample and then use them to explain behavior in trust and public goods games. Our approach differs from these papers. Unlike them, we do not elicit preference parameters in some games to predict behavior in others. Instead, we estimate preference parameters by observing behavior across a series of games with varying payoff parameters.

Our approach therefore directly relates to an experimental literature testing models of social preferences and estimating social preference parameters (see Cooper and Kagel (2016), for a review). Many experiments in this area are based on designs where individuals are randomly assigned to different treatments and tests of models are based on making treatment comparisons. It is typically the case that there are too few observations on individual subjects to estimate individual preference parameters, and so estimations are based on population regressions (e.g., Charness and Rabin (2002)). We take a fundamentally different approach by having subjects make many choices in a modified version of the sequential prisoner's dilemma game with varying payoffs, enabling us to estimate preference parameters at the individual level. In this regard, our Study 2 experiment is most closely related to a literature initiated by Palfrey and Prisbrey (1997) who estimate altruism and warm glow parameters in public goods games, and Andreoni and Miller (2002), and Fisman, et al. (2007), who estimate individual preferences for giving by having subjects make choices in modified dictator games with varying endowments/prices of giving.[5]

3 Study 1: Measuring the variability of conditional cooperation

3.1 Experimental design and procedures

In our first experiment subjects played eight sequential prisoner's dilemma games. In each game First-mover (FM) chooses either cooperate or defect, and Second-mover (SM) chooses a response, either cooperate or defect, to each of FM's possible choices. Thus, SM has four strategies to choose from: a *conditionally cooperative* (CC) strategy responds to cooperation with cooperation and responds to defection with defection, a *free-riding* (FR) strategy defects regardless of FM's choice, an *unconditionally cooperative* (UC) strategy cooperates regardless of FM's choice, and a *mis-matching* (MM) strategy responds to cooperation with defection and defection with cooperation. SM's actual choice is determined by her response to FM's actual

[5] Our approach is also similar to that used in a considerable literature on individual choice experiments where individual risk preferences are estimated from responses to a battery of lottery choices (see, for example, Hey and Orme (1994), Andersen, et al. (2008)). A more recent related paper is Bruhin, et al. (2019), who estimate structural social preference models from binary choices, although their emphasis is on finite mixture models.

choice. Thus, we use a strategy method to elicit SM strategies (Selten (1967); see Keser and Kliemt (2021) for a recent discussion).

We varied payoffs across the games. We denote the payoff from mutual cooperation by R and the payoff from mutual defection by P. In the case that one player defects and the other cooperates, we denote the payoff to the defector by T and the payoff to the cooperator by S. In all games payoffs were chosen to be strictly positive multiples of ten, with $T > R > P > S$ and $2R > T + S$, so that mutual cooperation maximizes combined earnings, but, assuming players are selfish own-earnings maximizers, the Nash equilibrium is always mutual defection.

Our main interest concerns how changes in payoffs affect conditional cooperation, focusing on two factors. First, LOSS $\coloneqq (R - S)/R$ refers to FM's loss when SM responds to cooperation by defecting rather than cooperating. Second, GAIN $\coloneqq (T - R)/R$ refers to SM's gains from responding to cooperation by defecting rather than cooperating. We also manipulate the efficiency gains from cooperation, EFF $\coloneqq (R - P)/R$. Table 1 shows the payoff parameters used in our experiment, the resulting values of EFF, LOSS, and GAIN, and the proportion of strategies in each class. R (500) is constant across all games while there are two distinct values of P (200, 400). Thus, we study games with two different levels of EFF. There are also two distinct values of T (600, 800) and four distinct values of S (20, 90, 40, 180). With this parameterization we study a 2x2 variation in LOSS and GAIN for each level of EFF. [6]

Table 1 Payoffs and strategy classifications for Sequential Prisoner's Dilemma Games

	Payoff parameters				*Payoff indices*			*Strategy classifications*			
Game	R	P	S	T	EFF	LOSS	GAIN	CC	FR	UC	MM
G1	500	200	90	600	0.60	0.82	0.20	37.0	46.2	12.5	4.4
G2	500	200	20	600	0.60	0.96	0.20	43.4	42.6	10.4	3.6
G3	500	200	90	800	0.60	0.82	0.60	30.1	54.6	10.0	5.2
G4	500	200	20	800	0.60	0.96	0.60	35.3	50.6	9.2	4.8
G5	500	400	180	600	0.20	0.64	0.20	39.8	50.6	6.8	2.8
G6	500	400	40	600	0.20	0.92	0.20	48.2	43.0	5.6	3.2
G7	500	400	180	800	0.20	0.64	0.60	30.1	57.0	9.6	3.2
G8	500	400	40	800	0.20	0.92	0.60	37.0	49.4	10.0	3.6
							Average	*37.6*	*49.3*	*9.3*	*3.9*

Note: EFF = $(R - P)/R$; LOSS = $(R - S)/R$; GAIN = $(T - R)/R$. CC: Conditional Cooperation; FR: Free-Riding; UC: Unconditional Cooperation; MM: Mis-matching

[6] This is the same parameterization used in Gächter, et al. (2024) for studying simultaneous prisoner's dilemmas.

We conducted the experiment online on (i) the platform Amazon Mechanical Turk (n = 106 participants) and (ii) at the University of Nottingham (n = 143). Participants were paired and played each of the eight games of Table 1, without feedback between games, and in both roles, as FM and as SM. At the end of the experiment one game was chosen at random for each pair, the roles were randomly determined, and subjects received payment based on their decisions for that game. Further procedural details are in Online Appendix A; instructions are in Online Appendix B; and demographic details of our subject pools are in Online Appendix C, Table C1.

3.2 Results

In line with our research question, our focus is on SM decisions. For our analysis here we pool the data from AMT and UoN and report the proportions of each strategy by game in Table 1. See Online Appendix C, Tables C2 for a breakdown of strategies by game and subject pool.

Conditionally cooperative strategies make up 37.6% of the strategies and varies between 30.1% and 48.2% across games. Free-riding strategies comprise 49.3% of the strategies and varies between 42.6% and 57.0%. There are relatively few unconditionally cooperative strategies (between 5.6% and 12.5%; 9.3% on average) and even fewer mis-matcher strategies (between 2.8% and 5.2%; 3.9% on average). Thus, as in other social dilemma experiments (e.g., Fischbacher, et al. (2001); Fallucchi, et al. (2019); Gächter, et al. (2022); Isler, et al. (2021); Miettinen, et al. (2020); Muller, et al. (2008); Thöni and Volk (2018); Weber, et al. (2023)), conditional cooperation and free-riding make up the bulk of elicited strategies (87% in aggregate).

These population averages do not tell us anything about the stability of classifications. For example, it may be that about half of our subjects are free-riders all the time, or it may be that all our subjects are free-riders half of the time. We find that, on average, 73.0% of subjects who free ride in one game also free ride in the next. Similarly, 66.0% of conditional cooperators in one game also conditionally cooperate in the next. Nevertheless, most subjects cannot be unambiguously classified as a 'free-rider' or 'conditional cooperator' because they vary their strategy between games: only 16.5% always free-ride and only 11.7% always conditionally cooperate. These results suggest that strategies are not a fixed trait but instead vary with the incentives of the game. How do strategies vary with the game payoffs?

We begin by considering *conditionally cooperative strategies*. A Cochrane's Q-test rejects the null hypothesis that the rate of conditional cooperation is the same across all eight games (Q = 51.11, p < 0.001). A detailed analysis reveals that the rate changes systematically with payoffs. Fig. 1(a) illustrates this finding for all eight games of Table 1. It compares the proportions of conditionally cooperative strategies in low GAIN (light bars) and high GAIN (dark bars), broken down by levels of LOSS and EFF. We find, for all four possible combinations of LOSS and EFF, a consistent pattern where conditional cooperation is significantly lower with high GAIN than low GAIN (McNemar tests: all p < 0.036). Except for the games with high EFF and high GAIN, where the effect of LOSS is insignificant (McNemar test: p = 0.118), we find conditional cooperation increases significantly with LOSS in the other three combinations (McNemar tests: all p < 0.085). Regarding EFF, none of the pairwise comparisons are significant (McNemar tests: all p > 0.201).

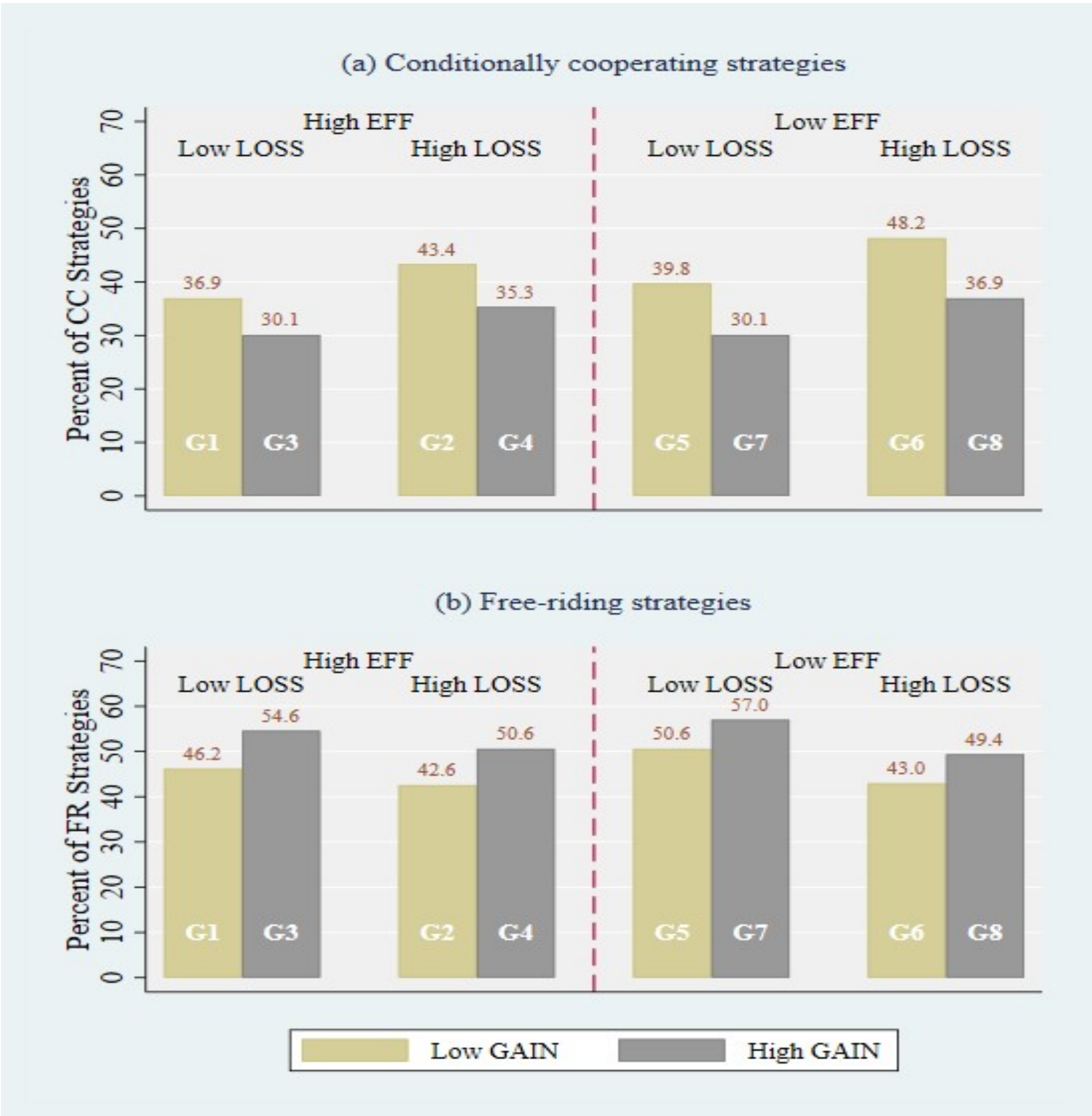

Fig. 1 Percentages of (a) conditionally cooperative and (b) free-riding strategies by game.

The rate of *free-riding strategies* also varies across games (Cochrane's Q = 34.45, p-value < 0.001). We find this particularly interesting as, in our experiment, a selfish player should free-ride regardless of LOSS, GAIN and EFF. Fig. 2(b) shows that the pattern is the mirror image of panel (a): free-riding significantly increases with GAIN (McNemar tests: all p < 0.085) and in the low EFF games decreases significantly with LOSS (McNemar tests: all p < 0.040). Again, regarding EFF none of the comparisons are significant (McNemar tests: all p > 0.550).

In Table 2, we report regressions that estimate the role of LOSS, GAIN, and EFF for each of the four strategies CC, FR, UC, and MM. We find that the rate of conditional cooperation increases in LOSS and decreases in GAIN, whereas free-riding decreases in LOSS and increases in GAIN. However, neither unconditional cooperation or mismatching is affected by GAIN nor LOSS. Contrasting our previous results, we find some significant effects of EFF: conditional cooperation decreases, and unconditional cooperation increases, with EFF.[7]

Table 2 Determinants of Strategy Choice for Study 1.

	(1) CC	(2) FR	(3) UC	(4) MM
LOSS	0.302***	-0.274***	-0.038	0.004
	(0.069)	(0.074)	(0.052)	(0.038)
GAIN	-0.213***	0.176***	0.020	0.015
	(0.042)	(0.044)	(0.026)	(0.019)
EFF	-0.140***	0.033	0.082**	0.036
	(0.046)	(0.046)	(0.033)	(0.022)
Observations	1,936	1,936	1,896	1,904

Note. Average marginal effects from probit regression with robust standard errors clustered at the individual level. LOSS = $(R - S)/R$, GAIN = $(T - R)/R$, EFF = $(R - P)/R$. Regressions include controls for individual characteristics, task characteristics, round and session effects.
** p < 0.1; ** p < 0.05; *** p < 0.01*

In summary, across our online experiments we find that conditional cooperation and free-riding vary across games. Most subjects change strategies across games, and this switching between strategies varies systematically with LOSS and GAIN. Second-movers are more likely to conditionally cooperate when free-riding imposes larger losses on the First-mover, or when

[7] The effect of EFF on SMs strategies reflects how SMs respond to defection. After cooperation, SM cooperation barely changes across levels of EFF (low: 46.8%; high: 47.0%). However, after defection, there is a noticeable increase in SM cooperation as EFF increases (low: 11.24%; high: 15.06%).

free-riding provides smaller gains for oneself. Our finding that strategies are sensitive to the cost imposed on the opponent as well as the gain to self suggests that some subjects care not only about their own material payoffs but also about the other's material payoffs. Moreover, the way conditional cooperation varies with LOSS and GAIN is consistent with the predictions of several distributional preference models. For example, consider the Fehr and Schmidt (1999) model of inequality aversion or the "distributional preference" model by Charness and Rabin (2002). According to these models the SM maximizes utility by defecting in response to defection, while the optimal response to cooperation depends on how much weight the SM places on the disadvantaged FM's payoff (Charness and Rabin's ρ parameter) or the marginal disutility from earning more than the FM (Fehr and Schmidt's β parameter). Applied to our game SM will conditionally cooperate if ρ (or β) > GAIN/(GAIN + LOSS), and free-ride otherwise. Thus, given a distribution of preference parameters in the population, more individuals in the population will conditionally cooperate when GAIN is lower, or LOSS is higher.

However, it should be noted that behavior is not perfectly aligned with these models. For example, 35.7% of individuals sometimes unconditionally cooperate. As another example, when GAIN increases some individuals switch in the opposite direction, from free-riding to conditionally cooperating (19.7%). At best, our data is consistent with noisy versions of these models.[8] In fact, the systematic effects of GAIN and LOSS are also consistent with stochastic choice models in which subjects maximize selfish utility with error. In Appendix D we present a quantal response equilibrium analysis and show that the QRE probability of conditional cooperation increases with LOSS and decreases with GAIN. Since our data does not allow us to distinguish which of these alternative models drives our results, we designed a further experiment to separately estimate the effects of social preferences and noise.

4 Study 2: Explaining the variability of conditional cooperation

Our second and main experimental design attempts to jointly estimate noise and preference parameters at an individual level. To obtain meaningful estimates it is necessary to have a subject play many games and so we modified our initial design in several ways. First, we used an in-person lab experiment to avoid problems of attrition and to enhance control. Second, we

[8] There is considerable evidence that errors and confusion play a significant role for behavior in public goods games. See, for instance, Andreoni (1995), Bayer, et al. (2013), Burton-Chellew, et al. (2016); Ferraro and Vossler (2010); Houser and Kurzban (2002); Palfrey and Prisbrey (1997). Errors may also affect conditional cooperation to some extent (e.g., Fosgaard, et al. (2017); Gächter, et al. (2022)).

simplified the task by having subjects play in fixed roles. Third, as in the sequential-move prisoner's dilemma game studied by Blanco, et al. (2014) we simplify the SM decision by having second movers only make a choice in response to cooperation – effectively, we hardwire defection as a response to defection (based on the results from our earlier design, where the response to defection is defect in over 85% of cases, we think not much is lost from this simplification). The game implemented in our lab experiment is shown in Fig. 2 and the instructions are in Online Appendix E.

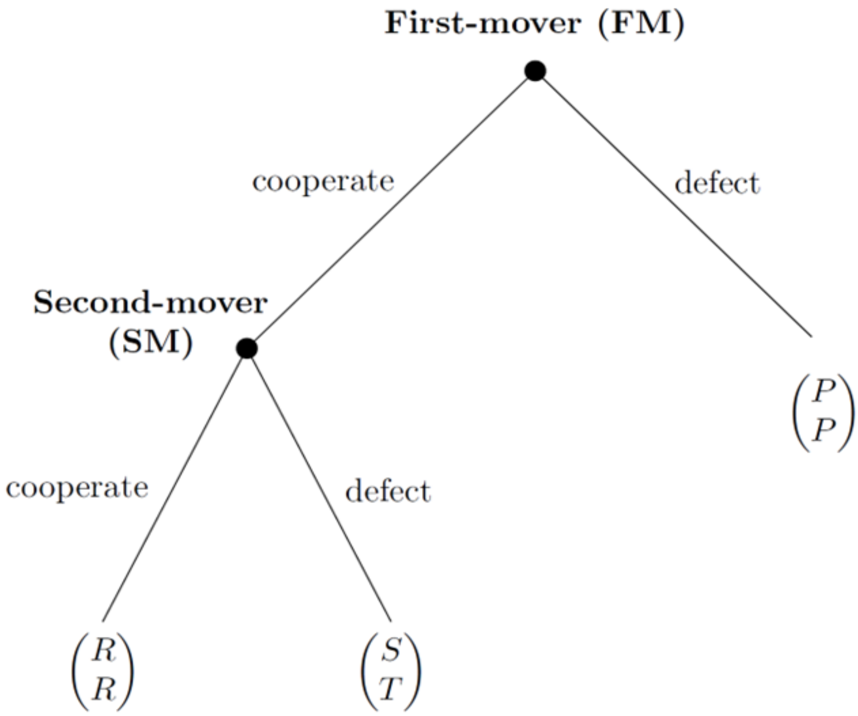

Fig. 2 The Modified Sequential Prisoner's Dilemma Game

Despite these changes, we kept many features identical to our first experiment. To elicit SM responses to cooperation we retained the contingent choice element of our earlier design. That is, FM and SM made choices at the same time and a SM choice was only relevant for payoffs if the FM cooperated. Thus, both players make binary choices in each game.

Additionally, as in our earlier design, we kept R (500) constant in all games and used the same four values of S (20, 40, 90, 180). We expanded the set of values of P (100, 200, 300, 400), and T (400, 600, 800, 1000), to obtain 64 games. These include the 8 games of our original design, and 22 more games satisfying the PD conditions $T > R > P > S, 2R > T + S$. In addition, there are 15 games where $T > R > P > S$, but $T + S > 2R$, so that the Nash equilibrium outcome is mutual defection, while combined earnings are maximized when FM cooperates and SM defects. In addition, there are 16 games where $R > T$, so that the SM maximizes own earnings by cooperating. For these games the Nash equilibrium outcome is mutual cooperation (and in one of these $S > P$ so the FM has a dominant strategy to cooperate). Finally, there are 3 more games where $S > P$ and so a FM has a dominant strategy to cooperate. (A complete list of games and parameters is provided in Appendix F, Table F1.) Thus, most of our games are dilemmas but the inclusion of other games means that a subject motivated to maximize own earnings cannot achieve this by using a simple heuristic of always defecting, and similarly a subject motivated to maximize combined earnings cannot use a simple heuristic of always cooperate. This feature of our experimental design provides us with an additional opportunity to examine the attentiveness of subjects and gain some insights into the rationalizability of choices.

4.2 Experimental procedures

We conducted our experiment in June 2022 in the CeDEx lab using University of Nottingham students. We conducted 13 sessions with a total of 194 participants (97 SMs). Subjects were recruited using ORSEE (Greiner, 2015) and the experiment was conducted with the software LIONESS Lab (Giamattei, et al. (2020)). 42% of the subjects were female and the average age was 22.1 years (s.d. 3.69 years). The experiment was pre-registered (AEARCTR-0009536).[9]

At the beginning of the session each participant was given a set of instructions (see Online Appendix E), and these were read aloud by the experimenter. Subjects then answered control questions before beginning the decision-making part of the session. As in the online experiments, subjects were anonymously paired with another subject and then played all 64 games with no feedback between games.

In contrast to our online study, we asked subjects to make their choices on a graphical implementation of the decision tree as outlined in Fig. 2. We again utilized neutral labels, where for each game, the FM was Person A and chose between options A1 and A2, while the SM was described as Person B and chose between options B1 and B2. In addition, we elicited beliefs about the other person's choice. As before, to control for potential order effects, we randomized the sequence of games at the pair level. Once subjects completed the tasks for all games, we

[9] https://www.socialscienceregistry.org/trials/9536. We aimed for 200 participants but due to show-up problems we ended up with 194.

asked them to complete a short post-experimental questionnaire eliciting basic demographic information.

At the end of the session two games were randomly chosen for each pair. One of these games was used to determine additional earnings based on game choices, applying an exchange rate of £0.02 per point. The other game was used to determine additional earnings based on beliefs. Subjects were rewarded in lottery tickets using a binarized scoring rule (Hossain and Okui (2013)), and these determined their chances of winning a prize of 200 points (i.e., £4). The instructions did not describe the precise binarized scoring rule to subjects. Instead, they were told that they maximized their chances of winning the prize by reporting their beliefs as accurately as possible. The instructions also offered to reveal the precise mechanism after the experiment to interested subjects (only one subject took up the offer). We adopted this procedure following Danz, et al. (2022) who show that despite a potential centrality bias using the binarized scoring rule, not outlining the details of the incentive mechanism results in most accurate belief elicitations.

Subjects received a £5 show up fee and earnings ranged from £5.40 to £25.00, averaging £16. On average, the experiment lasted about 60 minutes, including the completion of a post-experimental questionnaire. Subjects were informed of their payment immediately upon completion of the experiment and were paid within 24 hours.

4.3 Econometric model

To jointly estimate the effects of noise and social preferences we use a stochastic choice model incorporating social preferences. First, following Fehr and Schmidt (1999) and Charness and Rabin (2002) we assume SM's utility depends on both own-earnings and other's earnings as follows:

$$u_{SM}(\pi_{FM}, \pi_{SM}) = \rho\pi_{FM} + (1-\rho)\pi_{SM} = \pi_{SM} - \rho(\pi_{SM} - \pi_{FM})$$

The parameter ρ is the weight that SM places on FM's payoff when SM earns at least as much as FM in the Charness-Rabin model. It can also be interpreted as the marginal disutility from advantageous inequality in the Fehr-Schmidt model (their β parameter). In all 64 games SM earns at least as much as FM, and so we do not need to distinguish between the weights placed on the other's payoff when ahead and when behind, or between advantageous and disadvantageous inequality.

Second, we assume SM holds beliefs about FMs choice and assigns probability q to FM cooperating. Given these assumptions and our payoff parameterization, SM's expected utility from cooperating is

$$V_c = qR + (1-q)P$$

and the expected utility from defecting is

$$V_d = q(T - \rho(T-S)) + (1-q)P.$$

Third, we assume SM follows a stochastic choice rule, defecting if

$$V_d - V_c > Z, \text{ where } Z \sim N(\mu, \sigma^2),$$

and cooperating otherwise.

Note that a positive value of the parameter μ results in a bias toward cooperation that is independent of payoffs. That is, even if the choice rule is deterministic ($\sigma = 0$), and even if the expected payoff from defection exceeds the expected payoff from cooperation, SM may choose to cooperate if the expected utility difference is less than μ. The higher is μ, the more likely it is that SM will cooperate even when expected payoff maximization points toward defecting. Including μ in our econometric specification therefore accounts for potential individual bias toward cooperation and provides us with more robust estimates of the social preference parameter.

Note also that the model implies that SM choices are affected by their beliefs that FM cooperates. One might argue that those assigned to the SM role should know that their choice is relevant only if FM cooperates, and so should make the choice that is preferred in that contingency. In the stochastic choice model framework, beliefs matter because they affect the expected utility difference between cooperating and defecting. If the SM believes it is unlikely that FM will cooperate, then SM's choice is unlikely to be consequential and so the incentives for making one choice rather than another are diluted.

From this choice rule, letting $\Phi(\cdot)$ denote the standard normal distribution function, the probability of cooperating as a function of the payoffs, beliefs, preference, and noise parameters is:

$$\Pr\{SM\ cooperates\} = \Phi((\mu + V_c - V_d)/\sigma) = \Phi(\mu/\sigma + q(R-T)/\sigma + \rho q(T-S))/\sigma).$$

We then estimate μ, σ and the preference parameter ρ for each subject using maximum likelihood probit, providing us with individual estimates corresponding to our key dimensions of interest, social preferences (ρ) as well as noise (σ).

Prior to estimation we conducted Monte Carlo simulations to explore the properties of the estimators under alternative data generating processes (see Online Appendix F). The main take-aways from our simulations are, first, that sometimes the model fails to produce sensible estimates. Partly this reflects the familiar under-identification problem in discrete choice models, whereby maximum likelihood parameters cannot be estimated when the data is perfectly predicted by the regressors. This occurs if a subject cooperates in every decision, or defects in every decision. But it also occurs, for example, if choices follow deterministically from the social preference model so that a subject cooperates if and only if $\rho > (T-R)/(T-S)$ for some value of ρ. Another reason for failure to produce sensible estimates is that estimates of σ may be negative. Related to this, when estimates of σ are very close to zero, corresponding estimates of ρ can vary wildly. Thus, in our data analysis we treat individuals with $\hat{\sigma} < 0$ or $|\hat{\rho}| > 1$ as outliers.

Second, controlling for potential bias in choices, $\mu \neq 0$, by including a constant in the regression, and controlling for variability in beliefs across games by including beliefs in the regression, are important parts of our estimation strategy. When the data generating process includes a bias term, $\mu \neq 0$, estimates from a model without a constant are severely biased, whereas when the data generating process does not include a bias term, $\mu = 0$, estimating a model with a constant comes at a small price (mainly, adding a constant term increases the chance of running into identification problems). Similarly, when choices are based on expected utility differences and depend on q, estimating a model assuming $q = 1$ leads to severely biased estimates.

Third, standard errors can be very large, particularly when σ is large. An implication is that point estimates may be very imprecise estimates of underlying parameters. On the other hand, the finite-sample bias in the estimate of ρ is small, and so the average estimate of ρ across many individuals gives a useful estimate of the underlying mean parameter in the population.

4.4 Results

4.4.1 Descriptive statistics

Before estimating our model, we give some summary information on choices and beliefs.[10] Fig. 3 shows how SM cooperative choices relate to GAIN and LOSS. As in our online experiment,

[10] See Appendix G, Table G1, for a complete table of average beliefs and average choices for each of the 64 games.

the effect of GAIN is very clear: the higher is GAIN, the lower is the cooperation rate. Recall, that in our design SM's payoff from defecting is lower than the payoff from cooperating in 16 games (i.e., in the $T = 400$ games, which implies GAIN = -0.2)). As it turns out, SM almost always cooperates in these games. Note, also that in these games the private gain from cooperating is quite small (100 points), and the same as the private gain from defecting when $T = 600$. The fact that the cooperation rate in the $T = 400$ games is almost 100% while the cooperation rate in the $T = 600$ games is substantially above zero is a first hint that cooperation reflects more than just selfish motives plus error.[11]

Fig. 3 Cooperation Rate by GAIN, LOSS, and EFF

Also as in our online experiment, there is a positive relationship between LOSS and SM cooperation. However, the effect of LOSS is much less pronounced than in the case of GAIN. With respect to EFF, again in line with our online experiment, we see no clear relationship between SM cooperation and EFF. In Table 3, we show that these results are supported by a probit regression model. Column (1) reports results from a simple model estimating the effects of GAIN, LOSS, and EFF on SM cooperation, and we find that cooperation is not influenced by EFF, increases significantly with LOSS, and decreases significantly with GAIN, with the effect size of GAIN substantially greater than that of LOSS.

[11] This is not conclusive evidence because it ignores the role of beliefs. SM may have a higher expectation that FM cooperates when $T = 400$ than when $T = 600$, and so the expected private gain from cooperating when $T = 400$ may be higher than the expected private gain from defecting when $T = 600$.

These results are robust to adding controls for demographic, round, and session effects (see Column (2)).

Table 3 Determinants of SM Cooperation (64 games)

	(1) SM Cooperation	(2) SM Cooperation
LOSS	0.135***	0.138***
	(0.039)	(0.039)
GAIN	-0.552***	-0.553***
	(0.016)	(0.014)
EFF	-0.012	-0.013
	(0.018)	(0.018)
Controls	No	Yes
Observations	6,208	6,144

Notes: Average marginal effects from probit regression with robust standard errors clustered at the individual level. Dependent variable = 1 if SM conditionally cooperated, 0 otherwise. LOSS = (R – S)/R, GAIN = (T – R)/R, EFF = (R – P)/R. Controls: demographic variables, round and session effects. * *p < 0.1; *** *p < 0.05; **** *p < 0.01*

Next, we examine how *beliefs* relate to choices in our 64 games. Fig. 4 shows how beliefs about the other player's choice are related to the other player's actual choice. The left panel shows SM beliefs against the actual cooperation rates of FMs. The Spearman correlation is 0.984 ($p < 0.001$), suggesting that SM beliefs are quite well-calibrated, although there is a tendency for SM to somewhat over-estimate low FM cooperation rates and under-estimate FM high cooperation rates.

In the right panel of Fig. 4 we present FM beliefs about SM choices. Again, there is a high correlation across the 64 games: the Spearman correlation coefficient is 0.949 ($p < 0.001$). For FMs, there is a clear clustering of beliefs, and it reflects differences in GAIN. Thus, for the $T = 400$ games FMs expect SMs to cooperate at a high rate (the average belief is 85%), although the actual SM cooperation rate is in fact even higher than this (97%). At $T = 1000$ on average FM expect SMs to cooperate 24% of the time (slightly over-estimating the cooperation rate of 20%).

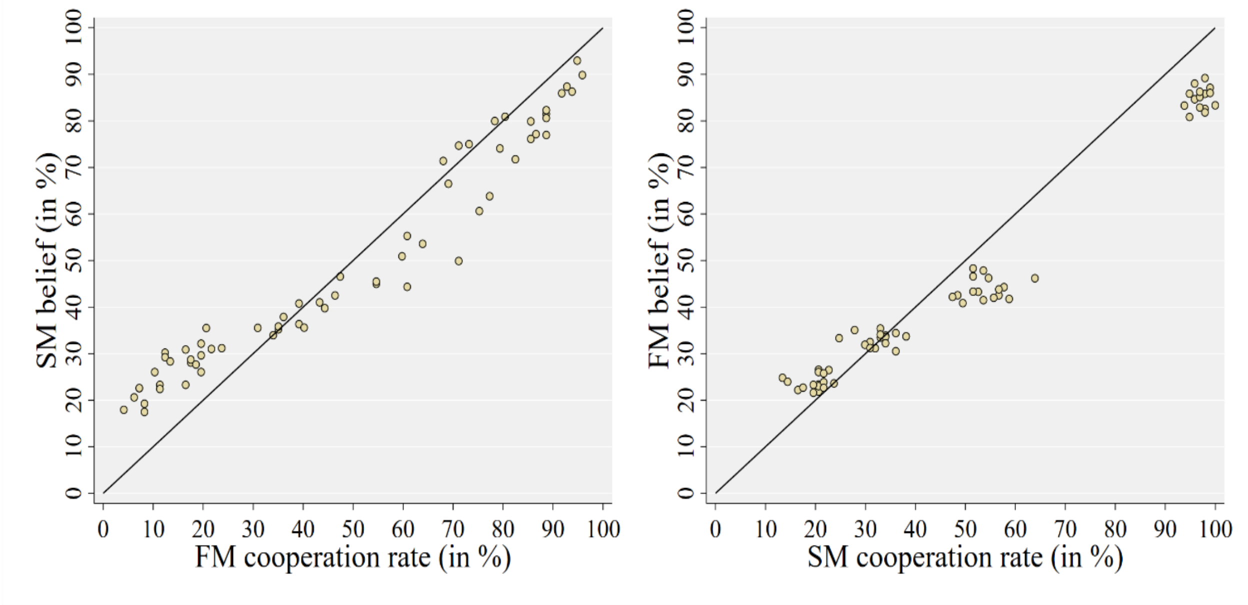

Fig. 4 Beliefs and Choices

4.4.2 Estimation results

Next, we turn to the estimation of our social preferences model using maximum likelihood probit regressions. We are unable to estimate parameters for 26 of 97 SMs for whom a linear combination of regressors perfectly predict choices. For example, 6 SMs always chose to cooperate, and 12 SMs always chose to maximize own-earnings (i.e., defect when $T > R$ and cooperate when $T < R$). Of the remaining 71 SMs for whom we can estimate parameters, one is estimated with $|\hat{\rho}| > 1$, which we exclude as an outlier. The rest of our analysis of SMs is based on the remaining sub-sample of 70 SMs. A complete list of the individual estimates is provided in Table G2 in Appendix G.

Fig. 5 presents a histogram of the 70 ρ parameter estimates. The average estimate is 0.41 (s.d. 0.25). Using this to estimate the mean ρ parameter in the population, we conclude that the mean parameter is statistically significantly different from zero (p < 0.001).[12] Of the 70 subjects, 64 have significantly positive estimates of ρ. Thus, the majority of our second movers have significantly positive social preference parameters and place positive weight on FM's payoff. This presents clear evidence in line with models of social preferences to explain variability in conditional cooperation under changing payoff parameters. It is interesting to

[12] To test this hypothesis, we use the average estimate in our sample, 0.41, as our estimate of the population mean, and for a standard error of this estimate we use $se(\sum\hat{\rho}/n) = (1/n)\sqrt{(\sum se(\hat{\rho}_i)^2)} = 0.1137$. This analysis can only be carried out with the subjects for whom we are able to estimate parameters. If we regard the twelve subjects who always free ride as having $\hat{\rho} = 0$, the average estimate of the population mean is reduced to 0.35.

compare these results with previous experiments that use modified dictator games to estimate the Fehr-Schmidt β parameter. In line with our results, Blanco et al. (2011) give an average estimate of β of 0.47 (s.d. 0.31), while Beranek, et al. (2015) (using UoN students) find an average estimate of $\beta = 0.48$ (s.d. 0.29).

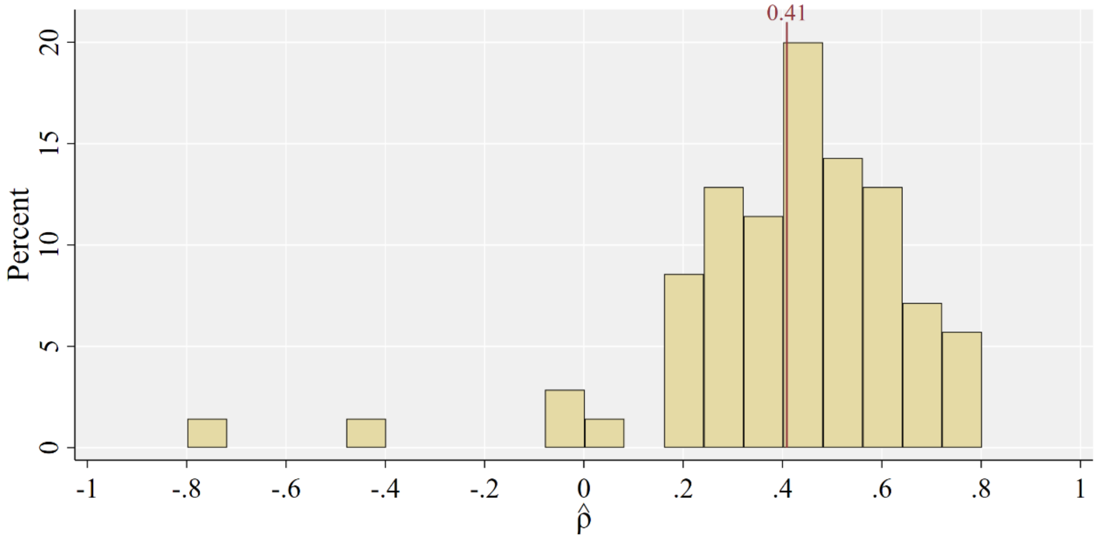

Fig. 5 Distribution of SM social preference parameter (ρ) estimates

For the noise parameters, the average estimate of μ is 5.36 (s.d. 289.76) and the average estimate of σ is 62.48 (s.d. 109.93). Note, the average bias estimate is very small, and we cannot reject the hypothesis that the mean bias in the population is zero ($p = 0.978$).[13] However, there is substantial variability in the sample: many subjects have a large estimated bias and 38 estimates are significantly negative (i.e., displaying a bias toward defection). Both additional pieces of evidence further support that social preferences are in fact the main driver of our previously established results.

For our sub-sample of 70 subjects Table 4 summarizes the predictive accuracy of our model and compares it with the predictive accuracy of two alternative models. For our first alternative, we simply predict cooperation using a probit model with a constant. That is, for each SM we predict cooperation (defection) in all games if that subject cooperates (defects) in most games. Note that this model must successfully predict at least 50% of an SM's choices. Across our sub-sample we find it predicts 67% of choices correctly. For a second alternative, we predict cooperation using the model with ρ constrained to be zero (i.e., for each SM we

[13] We again follow the same approach as before, using our average estimate, $\sum\hat{\mu}/n = 5.36$, as our estimate of the mean bias and for a standard error we use $se(\sum\hat{\mu}/n) = (1/n)\sqrt{(\sum se(\hat{\mu}_i)^2)} = 195.77$.

predict decisions based on a constant and expected utility differences, where utility depends only on own payoff). This increases predictive success considerably, and correctly predicts 80% of SM's choices, on average. Adding social preferences to the model increases the predictive success to 89%. We also report another widely used measure of predictive success, the pseudo R^2 measure. Since these measures of predictive success generally increase with the number of predictors in the model, we also report McFadden's adjusted Pseudo R^2, which penalizes for the number of predictors. Even using this measure, the model with social preferences improves predictive success relative to the other models.

Table 4 Measures of Predictive Success

Model	Average Hit Ratio	Average Pseudo R^2	Average adjusted Pseudo R^2
Constant	0.67	0	-0.03
Random utility with selfish preferences	0.80	0.31	0.25
Random utility with social preferences	0.89	0.56	0.47

4.4.3 Reciprocity

Our social preference model as outlined above is based on preferences defined over the distribution of payoffs. This means that when deciding how to respond to cooperation by FM, SM weighs up the utility of cooperation, which depends on R, and the utility from defection, which depends on S and T. The payoff parameter P does not directly enter SM's utility. However, it is possible that P does in fact matter for SM choices if subjects have reciprocal preferences. That is, SM considers that FM is being kind by cooperating (see, e.g., Falk, et al. (2003)), and so SM cooperates to reward this act. How kind FM is to SM could be measured in alternative ways that depend on P, the payoff that FM forgoes by choosing to cooperate. One can argue that $P-S$ is a relevant measure of kindness, as by cooperating FM forgoes P and risks getting S. Alternatively, one could argue that $R-P$ is a more relevant measure of kindness from cooperating as these are the cooperative gains being offered to SM.

To test whether some form of reciprocation is playing a role in our setting, we do not model reciprocal preferences explicitly, but rather we simply test whether the weight SM places on FMs payoff changes with P. That is, we suppose that SM's probability of cooperating is given by:

$$\Pr\{SM\ cooperates\} = \Phi\left(\mu/\sigma + q(R-T)/\sigma + q\sum_k \rho_k\, \mathbb{I}_{P=P_k}(T-S))/\sigma\right)$$

where $\mathbb{I}_{P=P_k}$ is the indicator function for the four possible values of P. We estimate separate ρ_k parameters for each value and test the hypothesis that all four are equal.

We can estimate this model for 68 subjects (70%). Of these, there are only four subjects for whom the weights significantly vary with P at the 10% level. Thus, in our specification, we can only find limited evidence of reciprocity as an explanation of conditional cooperation. [14] It appears that most of the variation in cooperation we observe can be explained by heterogeneity in social preferences rather than due to additional reciprocal concerns.

The result that play is not sensitive to P is perhaps surprising given the results from papers such as Falk, et al. (2003) and Brandts and Sola (2001), which report striking evidence of reciprocity. However, it should be noted that our design (using a strategy method, cold decisions, and 64 tasks without feedback) may favor distributional concerns and make concerns about intentions reciprocity less salient. Thus, the lack of strong effects of P in our design should not be taken to imply that reciprocity generally plays no role in sequential dilemmas.

4.4.4 First Mover choices

Although our main focus is on SM's response to cooperation, FM cooperation rates also vary substantially across the 64 games, ranging from as little as 4.1% up to 95.9% in another.[15] To further examine a potential explanation behind FM cooperation, we can also apply the random utility model with social preferences to FM choices. Let FM's utility be given by

$$u_{FM}(\pi_{FM}, \pi_{SM}) = \tau\pi_{SM} + (1-\tau)\pi_{FM} = \pi_{FM} + \tau(\pi_{SM} - \pi_{FM}).$$

Here, the parameter τ is the weight FM places on SM's payoff when SM earns more than FM (the Charness-Rabin σ parameter). (Recall, in our games SM always earns at least as much as FM.) It can also be interpreted as minus one times Fehr-Schmidt's disadvantageous inequality aversion parameter.

[14] The finding that SM cooperation is insensitive to P does not rely on our stochastic choice model specification. As seen in Table 3, the estimated effect of EFF, and hence, P, from a simple probit regression is small. We also regressed SM cooperation on P for each group of 4 games that vary P while holding R, S, and T constant. As we show in Online Appendix H, cooperation varies a lot across the 16 groups of games, but it varies very little within a group.

[15] Table G1 in Appendix G reports average FM cooperation rates for each of the 64 games.

With this utility function, FM's utility from defecting is P, and since this determines the outcome with certainty FM's expected utility from defecting is

$$V_d = P.$$

FM's expected utility from cooperating is

$$V_c = qR + (1-q)(S + \tau(T-S))$$

where q is the probability FM assigns to SM cooperating. Using the stochastic choice rule, FM defects if

$$V_d - V_c > Z, \text{ where } Z \sim N(\mu, \sigma^2),$$

and cooperates otherwise. From this it follows that FM cooperates with probability:

$$\Pr\{FM\ cooperates\} = \Phi(\mu/\sigma + (qR + (1-q)S - P)/\sigma + \tau(1-q)(T-S)/\sigma).$$

As before, we estimate the parameters μ, σ and τ using maximum likelihood probit regressions. We can estimate individual parameters for 84 of 97 FMs. As is the case for SM, for FM we also find large heterogeneity in our estimated bias with a small and insignificant population mean of $\hat{\mu}$ = -0.18 (s.d. 126.84; p = 0.980).[16] A| histogram of our estimates of τ is shown in Fig. 6.

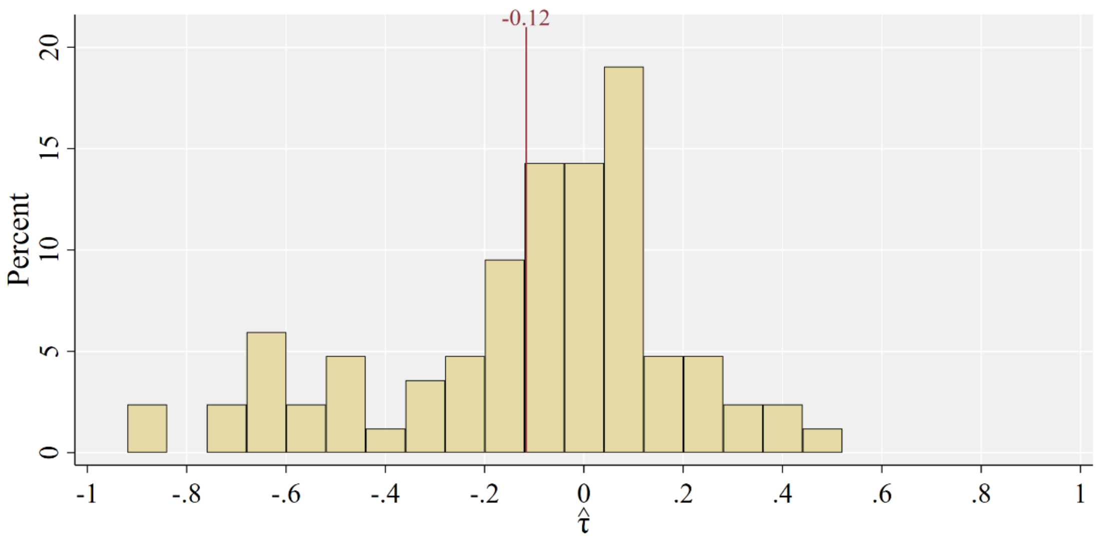

Fig. 6 Distribution of FM social preference parameter (τ) estimates

The average estimate of τ is -0.12 (s.d. 0.30). Sixty-two of the estimates are insignificantly different from zero. Eleven are significantly negative and 13 are significantly

[16] Following our previous approach, we compute the standard error of our estimate as $se(\sum\hat{\mu}/n) = (1/n)\sqrt{(\sum se(\hat{\mu}_i)^2)} = 10.70$.

positive. Despite this heterogeneity we find that the population mean of τ is significantly different from zero (p = 0.010) providing evidence for other-regarding preferences.[17]

Most notably, we find that the weight FM places on SM's payoff tends to be lower than the weight SM places on FM's payoff. Recalling that SM always earns at least as much as FM this is consistent with the assumption in Charness and Rabin (2002) that the weight placed on the other's payoff depends on whether a player is ahead or behind. This is also consistent with the results reported in Bruhin et al. (2019), where subjects, on average, display *asymmetric altruism*, placing more weight on another's payoff when ahead and less weight on another's payoff when behind. Further, note that about half the estimates of τ are negative. These FMs place a negative weight on SMs payoff, consistent with an aversion to disadvantageous inequality in the sense of Fehr and Schmidt. However, this leaves roughly another half of FMs who place a positive weight on SM's payoff, even though SM is ahead. These subjects are not consistent with difference aversion models, and this suggests a substantial proportion of departures from selfish behavior cannot be accounted for by inequality aversion.

5 Discussion

Given our results, an interesting question is how they relate to the large literature on eliciting strategies in public goods experiments. Following the design of Fischbacher, et al. (2001), many studies classify subjects as conditional cooperators, free riders, and others (see Thöni and Volk (2018) for a review of this literature). As in our sequential prisoner's dilemma games, a robust finding in this literature is that conditional cooperator and free rider strategies are the most frequent. Across 18 studies reviewed in Thöni and Volk (2018) (n=7,107), 61.3% of elicited strategies are conditionally cooperative and 19.2% are free-rider strategies, making up over 80% of subjects.

Interestingly, conditional cooperation is more prevalent and free riding less frequent in these public goods experiments than in our sequential prisoner's dilemma games. Thöni and Volk note that conditional cooperation varies between 40% and 70% and free riding varies between 6% and 30%, whereas we see more free riding than conditional cooperation in most of our games. Eichenseer and Moser (2020) report a direct within-subject comparison of strategies in a sequential prisoner's dilemma and a public goods game, and find that more subjects free ride in the sequential dilemma game.

[17] In line with the SM analysis, we compute the standard error of our estimate as $se(\sum\hat{\tau}/n) = (1/n)\sqrt{(\sum se(\hat{\tau}_i)^2)} = 0.044$.

This should be interpreted cautiously because of differences in the way the strategies are classified across the two types of game. However, it is suggestive of at least two alternative possibilities worth discussing. One, consistent with our findings, is that the difference in rates of conditional cooperation across the two games may reflect that the structure of payoffs and payoff parameters are more conducive to conditional cooperation in their public good setting than in their sequential dilemma setting. For example, it may be that Eichenseer and Moser's results would have been reversed had they used sequential prisoner's dilemma payoffs with lower gain and higher loss. Alternatively, the lower prevalence of free-riding in public goods experiments might be because the psychology of groups with more than two players (and more levels of cooperation) is substantially different from the two-player, two-choice SPD setting. Further research would be needed to separate these explanations.

Perhaps more important that comparing aggregate levels of cooperation is within-subject patterns in cooperation across treatments. Eichenseer and Moser find that subjects who conditionally cooperate in one game are more likely to conditionally cooperate in the other. This is also seen in our experiments where subjects who cooperation in one game are more likely to cooperate in another. This is consistent with the view that, even if conditional cooperation is not a stable trait, conditional cooperation reflects stable underlying social preferences.

In fact, we are not aware of any previous public good game experiments that have classified subjects while varying, within-subject, payoff parameters (such as group size, or marginal per capita return), and so it is premature to make any claims about the stability of public good game strategies to changing payoffs. In a recent study Gächter and Marino-Fages (2023) vary group size and marginal per capita return in between-subjects experiments. In contrast to the findings reported here, they find that the distribution of strategy types is unaffected by game parameters. Whether the effects of variation in payoffs are stronger in within-subject designs, where they are perhaps more salient, or whether classifications of strategies are more robust in public good games is again a subject for further research.

6 Conclusion

To our knowledge, our study is the first to empirically examine the within-subject variability of conditional cooperation across games with varying payoffs. In our first study, we have subjects play eight different one-shot sequential prisoner's dilemma games. Although strategies are correlated across games, we find that conditional cooperation varies across games, and most subjects change strategies at least once across games. This switching between strategies varies systematically with the distributional consequences of free-riding relative to conditionally cooperating. Subjects conditionally cooperate more often when free-riding imposes larger losses on the first mover, or when free-riding provides smaller gains for oneself. In our second and main study, we jointly estimate social preference parameters and noise parameters at the individual level and find that most of our subjects place a significantly positive weight on others' payoff.

These findings provide two important implications. First, the within-subject variation of conditional cooperation with payoffs suggests that conditional cooperation should be viewed as an endogenous behavior arising from interaction between underlying motives and payoff variations, rather than a preference itself. Relatedly, because a majority of subjects change their second mover strategy when material payoffs change, classifications of individuals as conditional cooperators or free riders should not be generalized to other games with different material payoffs.

Second, our findings have implications for the modelling of social preferences. We find that simple distributional preferences, where utilities depend only on the distribution of material payoffs can explain a lot of conditional cooperation, and we find little support for reciprocity. The lack of support for reciprocity may reflect features of our design (e.g. our use of a strategy method) or our particular focus on a sequential dilemma game in which only positive reciprocity can play a role (several studies, e.g., Abbink, et al. (2000) and Offerman (2002), suggest that positive reciprocity concerns are generally weaker than negative reciprocity). We also find that weights placed on other's payoffs vary substantially between first movers and second movers. Second movers, who earn at least as much as first movers in any outcome of our games, place a higher weight on first mover payoffs. This finding is consistent with the Charness and Rabin (2002) assumption that individuals place less weight on other's payoff when others are ahead and more weight on other's payoffs when others are behind. This result is also qualitatively consistent with the findings of asymmetric altruism reported in Bruhin, et al. (2019): they also find subjects place more weight on other's earnings when others are behind. In addition, estimates of first mover preference parameters show an almost even split between subjects who place a negative or a positive weight on second mover payoffs. A first mover who places a negative weight on the second mover's payoff can be interpreted as exhibiting an aversion to disadvantageous inequality, in the sense of Fehr and Schmidt (1999). However, those placing a positive weight on second mover payoffs cannot be viewed as inequality averse, and so inequality aversion alone cannot capture a significant portion of the departures from selfish behavior.

Data and code availability

Data and analysis code are available at https://osf.io/8us67/

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Online Appendix for

Social Preferences and the Variability of Conditional Cooperation

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Contents

Appendix A *Details on the experimental procedures of Study 1*

Here we provide procedural details for Study 1 reported in the main text. We conducted our initial online interactive experiment in Spring 2019 using Amazon MTurk across five sessions with a total of 138 participants. We refer to this as our AMT experiment. To further examine the robustness of our first data collection, we replicated the AMT experiment with a different subject pool in Summer 2021, referred to as our UoN experiment, using students from the University of Nottingham who had signed up to a subject database for participating in experiments. For this experiment we used ORSEE (Greiner (2015)) to recruit subjects and conducted an additional three sessions with a total of 152 participants.

Both online experiments were programmed using LIONESS Lab (Giamattei, et al. (2020)), and the same program was used for both experiments, with only minimal changes to the instructions related to the subject pools (for the instructions, see Appendix B).

Each participant was paired with another subject after they had read the instructions and passed some control questions. Each pair then played all eight games of Table 1 (main text) with no feedback between games. Before making decisions in a game subjects had to answer additional control questions about the payoffs to ensure that subjects recognized the payoff changes across games (see Screenshot 2 in Appendix B). These additional control questions were intended to ensure that participants understood the implications of their decisions and recognized the payoff changes across games. Participants then made decisions as FM as well as SM. Both decision tasks were presented on the same screen.

For the FM decision, participants simply chose whether to cooperate or to defect. For the SM decision, we asked participants to decide in the following two situations: i) if FM cooperates, and ii) if FM defects. Rather than use the terms "cooperate" or "defect", we labeled options neutrally as A or B, with labeling randomly chosen at the pair level in each game. To control for potential order effects, we randomized the sequence of games and the order of tasks (i.e., placing the FM or SM decision at the top of the screen) at the pair level. Once participants completed the tasks for all games, we asked them to complete a short post-experimental questionnaire eliciting basic demographic information.

We paired subjects with another participant on a real-time basis, and they made decisions in each game at the same time. That is, they could not proceed to the next game until both had completed their decisions for the current game.[1]

To elicit participants' responses in an incentive-compatible way, we implemented the following payment scheme. At the end of the session, one of the eight games was randomly chosen at the pair-level for payment. If both subjects completed the entire experiment, they were paid according to the outcome of this game as follows. One of the pair was randomly chosen to be FM, and the other was selected to be SM. Then, participants were reminded of their decisions and informed about the outcome for this game. As mentioned above, for SM's decision we used their conditional response to FM's decision. If one of the pair had dropped out during the experiment, the computer randomly selected the payoff-relevant game for the remaining subject. Then the computer randomly selected one out of four monetary outcomes (i.e., T, $R, P,$ or S) of the chosen game for payment to the remaining subject. We explained this payment scheme clearly in the instructions. This payment procedure gives subjects a monetary incentive to take both FM decisions and SM decisions seriously in all games as any of these decisions can become payoff relevant.

In line with other online experiments, there was a non-negligible attrition rate: 32 out of 138 AMT subjects (23%) and 9 out of 152 UoN subjects (6%) dropped out during the experiment.[2] For subjects who completed the AMT experiment, the average age was 34.2 years (s.d. 10.2 years) and 37% were female, while for the UoN experiment the average age was 22.5 years (s.d. 4.6 years) and 58% were female. On average, the experiments lasted about 30 minutes and subjects were informed of their payment immediately upon completion of the experiment and were paid within 24 hours. AMT subjects' earnings ranged from \$1.20 to \$9.00, averaging \$4.59, so somewhat higher than the median hourly rate of ~\$2 reported in Hara, et al. (2018). UoN subjects' earnings ranged from £1.20 to £9.00, averaging £4.53, similar to average hourly rates for similar online experiments and slightly lower than average hourly rates for lab experiments for that subject pool.

[1] To reduce the risk of decreased attention due to long waiting times as subjects waited for their opponent to decide, we took the following measures. Before participants entered the experiment, we told them to avoid distractions during the experiment. In addition, participants who were inactive for more than 30 seconds (i.e., no mouse movement or no keyboard input) got an alert voice message and a blinking text on their browser. If an inactive participant did not respond to the alert message for a further 30 seconds, such an inactive participant was removed from the experiment and the remaining person was able to continue the experiment.

[2] The dropout rate in our AMT sessions is similar to that of related interactive online experiments. For example, Arechar, et al. (2018) report a 20% dropout rate in their interactive four-player public goods game, and Gächter, et al. (2023) report a 24% dropout rate in their interactive eight simultaneous prisoner's dilemma games.

Appendix B *Instructions for Study 1*

These are the instructions for the AMT experiment. Instructions for UoN online experiment were identical except that the terms 'HIT', 'dollar' and $ were replaced with 'experiment', 'pound', and '£', respectively.

Welcome

Thank you for accepting this HIT. To complete this HIT, you must make some decisions. Including the time for reading these instructions, the HIT will take about 30 minutes to complete. If you are using a desktop or laptop to complete this HIT, we recommend that you maximize your browser screen (press F11) before you start.

It is important that you complete this HIT without interruptions. During the HIT, please **do not close this window or get distracted from the task.** If you close your browser or leave the task, you will not be able to re-enter and we will not be able to pay you.

In this HIT, you will be matched with one other participant. Each of you will make decisions for 8 decision situations. In each situation, each of you will earn Tokens depending on your decisions.

At the end of the HIT, one of the decision situations will be randomly chosen. Your earnings from this situation will be converted from Tokens to Dollars at a rate of 1**00 Tokens = $ 1.** This will be added to **your participation fee of $1.00.** Depending on your decisions, you may make up to $8.00 more in addition to the $1.00 participation fee. In the same way, Tokens earned by the person matched with you in that same situation will also be converted to Dollars at a rate of 100 Tokens = $ 1.

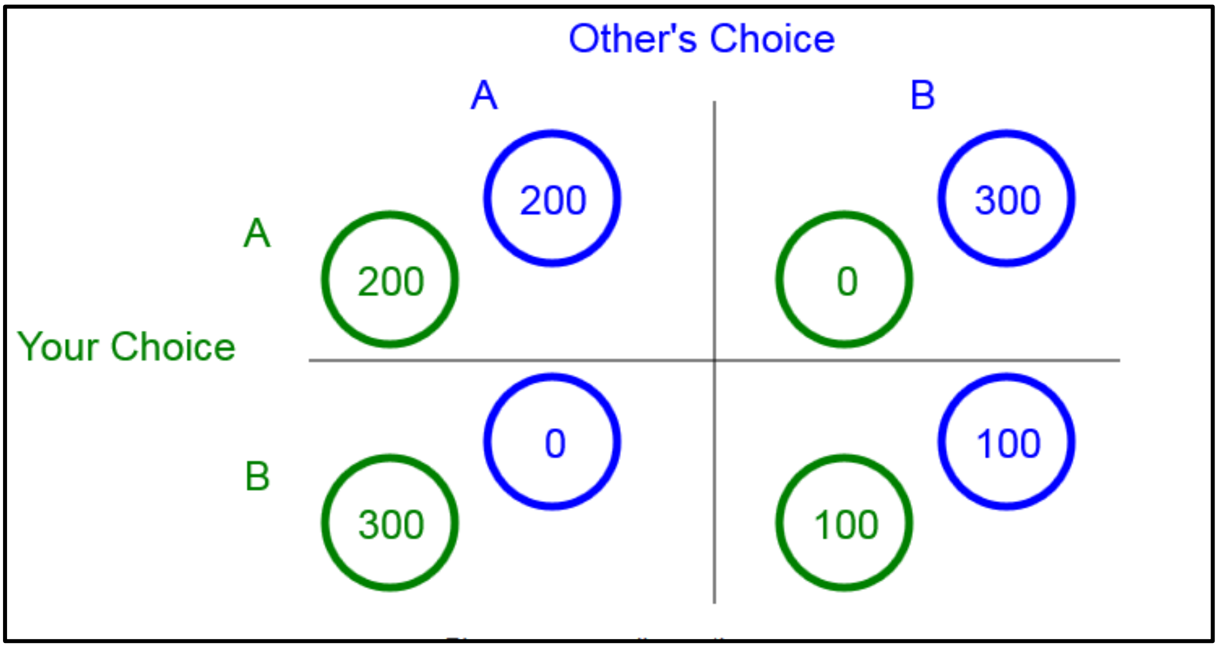
You will receive a code to collect your payment via MTurk upon completion.

Please click "Continue" to start the HIT.

Instructions

The HIT consists of 8 decision situations.

Each decision situation will be presented on a screen like the **example screen** below.



You and the other person will be making choices between **A** and **B**. Your earnings are the values in the green circle, and the other person's earnings are the values in the blue circle. The table is read as follows:

- If you choose A and the other person chooses A, you will earn 200 Tokens and the other person will earn 200 Tokens.
- If you choose A and the other person chooses B, you will earn 0 Tokens and the other person will earn 300 Tokens.
- If you choose B and the other person chooses A, you will earn 300 Tokens and the other person will earn 0 Tokens.
- If you choose B and the other person chooses B, you will earn 100 Tokens and the other person will earn 100 Tokens.

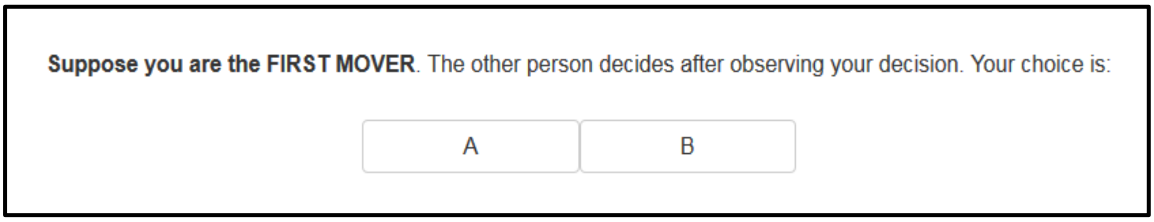
Please note that the values in the table will differ in each decision situation.

Tasks

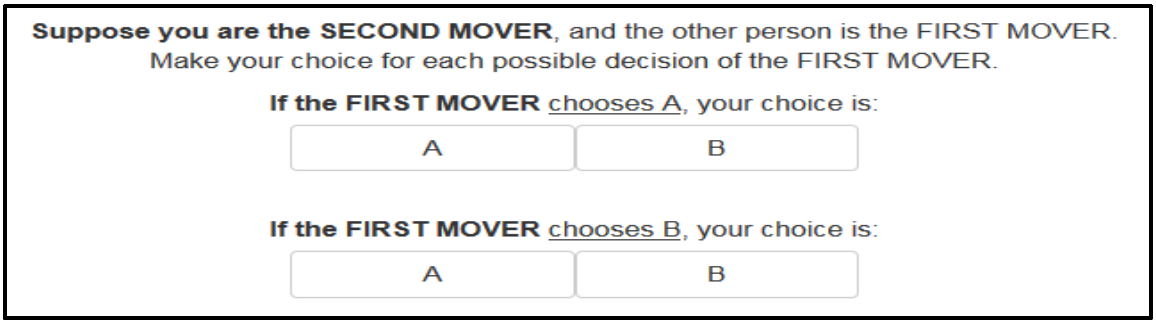
In each decision situation, you must complete **two types** of tasks, which we will refer to below as the "FIRST MOVER's decision" and "SECOND MOVER's decision". The FIRST MOVER decides first whether to choose A or B. The SECOND MOVER is then informed of the FIRST MOVER's decision. The SECOND MOVER then decides whether to choose A or B.

We want to know what you would do in the role of the FIRST MOVER and what would you do in the role of the SECOND MOVER. Thus you will be prompted to make decisions in both roles.

● For the "FIRST MOVER's decision" task, you will see the following screen and you must choose A or B:



● For the "SECOND MOVER's decision" task, You will see the following screen and you must choose A or B in two possible cases: (1) if the FIRST MOVER chooses A (2) if the FIRST MOVER chooses B



During the HIT, you will not receive any feedback on the other person's choice or the outcomes of the decision situations.

Your dollar earnings

On completion of the HIT, you will be paid your participation fee of $ 1.

In addition, one of the decision situations will be randomly chosen for your additional dollar earnings. The computer will randomly choose either you or the other person to be the first-mover. If you are chosen to be the first-mover, your first-mover's decision will be matched with the second-mover's decision of the other person. If the other person is chosen to be the first-mover, your second-mover's decision will be matched with the first-mover's decision of the other person. Your earnings and the other person's earnings will be determined depending on choices of you and the other person in that situation. Two examples should make this clear.

Example 1. Assume that **the computer randomly selects you to be the first-mover. This implies that your payoff relevant decision will be your first-mover's decision.** Assume that you choose A as the first-mover's decision in the above example screen. Assume that the other person matched with you makes the following second-mover's decisions: he/she chooses A if you choose A, and chooses B if you choose B. As a consequence, you will earn 200 Tokens and the other person will earn 200 Tokens.

Example 2. Assume that **the computer randomly selects the other person to be the first-mover. This implies that your payoff relevant decision will be your second-mover's decision.** Assume that you make the following second-mover's decisions: you choose B if the FIRST MOVER chooses A, and choose B if the FIRST MOVER chooses B in the above example screen. Assume that the other person matched with you chooses A as the first-mover's decision. As a consequence, you will earn 300 Tokens and the other person will earn 0 Tokens.

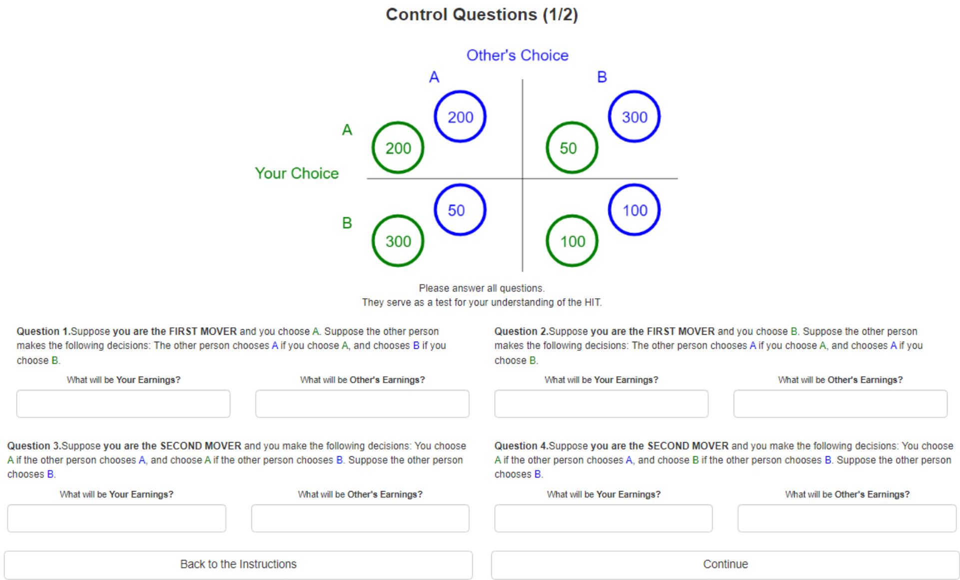
At the end of the HIT

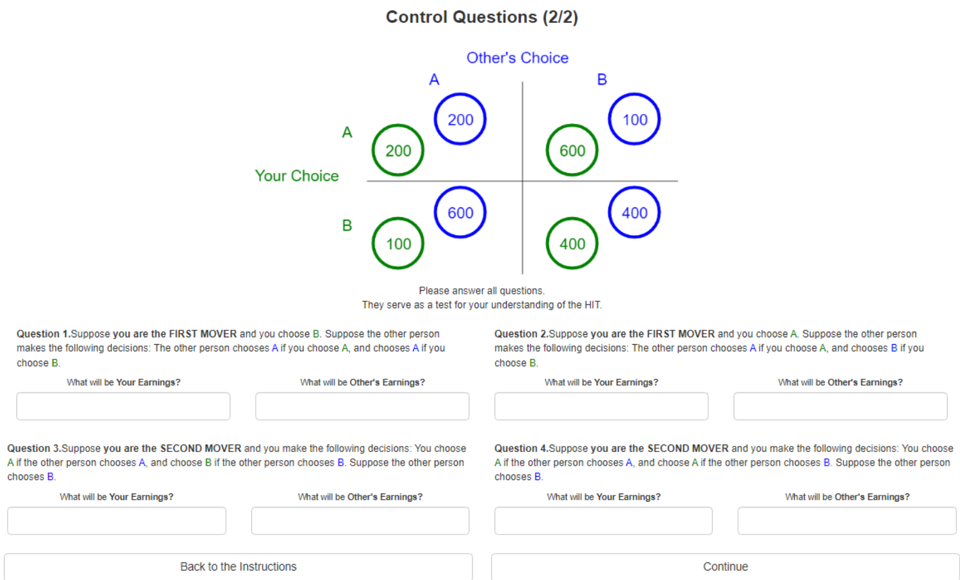
On completion of the HIT, one of the decision situations will be randomly chosen as explained above. You will be informed of your choices and earnings for that decision situation, and you will be paid these earnings in addition to your participation fee.

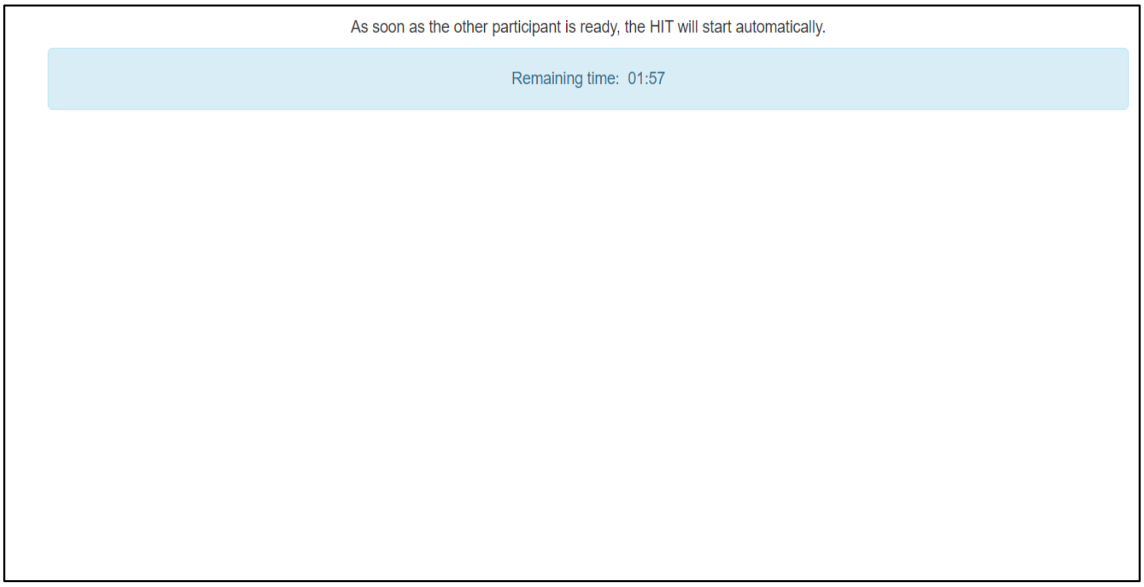
Note that we will not be able to pay you if you do not complete the HIT. If the person you are matched with does not complete the HIT, the computer will randomly select one of the four possible earnings in the randomly chosen decision situation, and you will be paid these earnings in addition to your participation fee.

Your participation fee and the additional earnings will be paid to you within two working days.

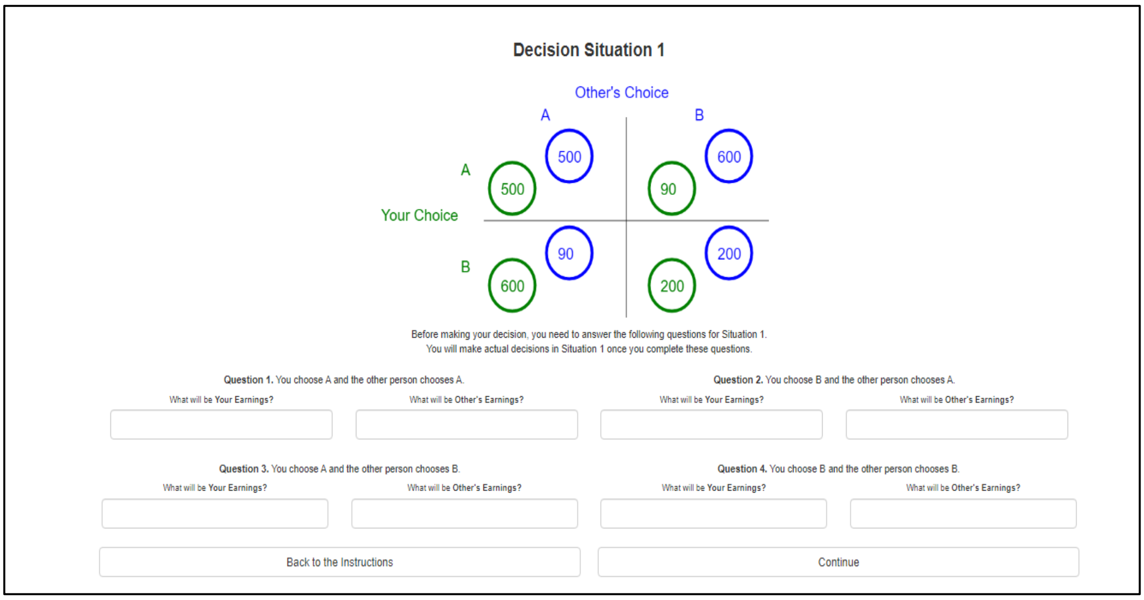
Comprehension Questions


Control Questions (1/2)
Other's Choice
A
B
Your Choice
A
B
200
200
300
50
50
300
100
100
Please answer all questions.
They serve as a test for your understanding of the HIT.
Question 1.Suppose you are the FIRST MOVER and you choose A. Suppose the other person makes the following decisions: The other person chooses A if you choose A, and chooses B if you choose B.
What will be Your Earnings?
What will be Other's Earnings?
Question 2.Suppose you are the FIRST MOVER and you choose B. Suppose the other person makes the following decisions: The other person chooses A if you choose A, and chooses A if you choose B.
What will be Your Earnings?
What will be Other's Earnings?
Question 3.Suppose you are the SECOND MOVER and you make the following decisions: You choose A if the other person chooses A, and choose A if the other person chooses B. Suppose the other person chooses B.
What will be Your Earnings?
What will be Other's Earnings?
Question 4.Suppose you are the SECOND MOVER and you make the following decisions: You choose A if the other person chooses A, and choose B if the other person chooses B. Suppose the other person chooses B.
What will be Your Earnings?
What will be Other's Earnings?
Back to the Instructions
Continue

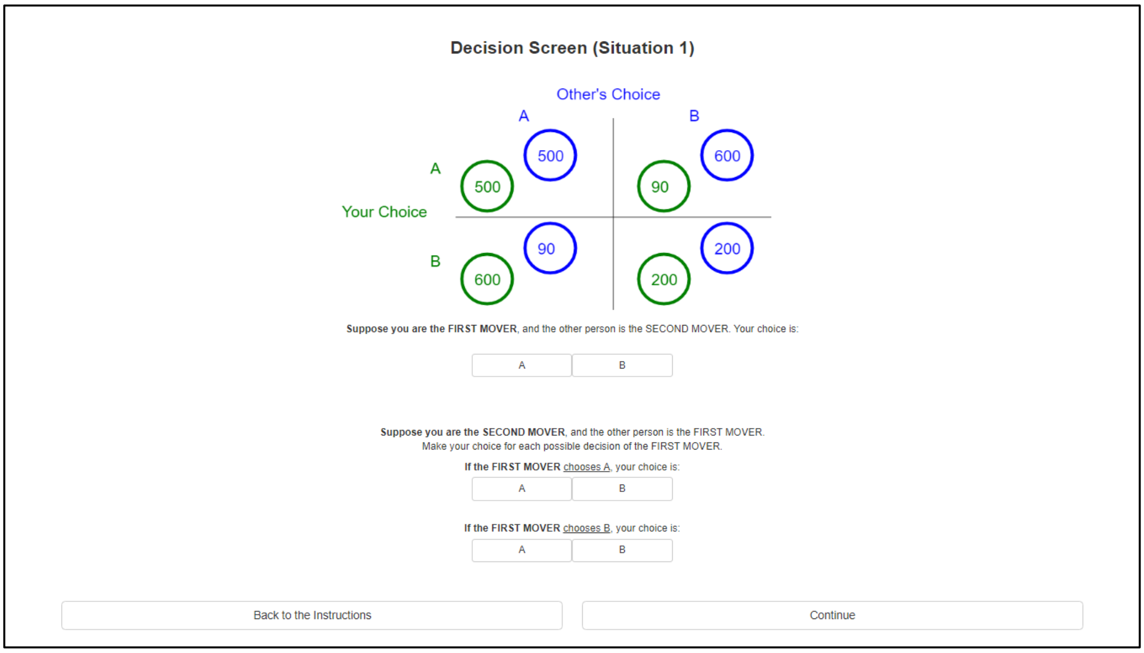

Control Questions (2/2)
Other's Choice
A
B
Your Choice
A
B
200
200
100
600
600
100
400
400
Please answer all questions.
They serve as a test for your understanding of the HIT.
Question 1.Suppose you are the FIRST MOVER and you choose B. Suppose the other person makes the following decisions: The other person chooses A if you choose A, and chooses A if you choose B.
What will be Your Earnings?
What will be Other's Earnings?
Question 2.Suppose you are the FIRST MOVER and you choose A. Suppose the other person makes the following decisions: The other person chooses A if you choose A, and chooses B if you choose B.
What will be Your Earnings?
What will be Other's Earnings?
Question 3.Suppose you are the SECOND MOVER and you make the following decisions: You choose A if the other person chooses A, and choose B if the other person chooses B. Suppose the other person chooses B.
What will be Your Earnings?
What will be Other's Earnings?
Question 4.Suppose you are the SECOND MOVER and you make the following decisions: You choose A if the other person chooses A, and choose A if the other person chooses B. Suppose the other person chooses B.
What will be Your Earnings?
What will be Other's Earnings?
Back to the Instructions
Continue



Screenshot 1: Matching stage



Screenshot 2: Comprehension Question before Decision Screen



Screenshot 3: Decision screen

Appendix C *Additional data from Study 1*

Table C1 Comparison of demographic characteristics in Study 1 subject pools

	(1) Total	(2) AMT	(3) UoN	(4) Difference
Age	27.46	34.19	22.47	11.72***
	(9.50)	(10.22)	(4.64)	(0.97)
Gender				
Female	0.49	0.37	0.58	-0.21***
	(0.50)	(0.48)	(0.50)	(0.06)
Male	0.50	0.62	0.41	0.21***
	(0.50)	(0.49)	(0.49)	(0.06)
Other	0.01	0.01	0.01	0.00
	(0.09)	(0.10)	(0.08)	(0.01)
Ethnicity				
Asian	0.23	0.11	0.32	-0.21***
	(0.42)	(0.31)	(0.47)	(0.05)
Black or African	0.06	0.02	0.09	-0.07**
	(0.23)	(0.14)	(0.28)	(0.03)
Latin American	0.02	0.03	0.01	0.02
	(0.13)	(0.17)	(0.08)	(0.02)
Native Hawaiian	0.00	0.01	0.00	0.01
	(0.06)	(0.10)	(0.00)	(0.01)
White	0.62	0.80	0.49	0.31***
	(0.49)	(0.40)	(0.50)	(0.06)
Other	0.07	0.04	0.10	-0.06*
	(0.26)	(0.19)	(0.30)	(0.03)
Political view (0 - left to 10 – right)				
Political view	4.06	4.33	3.86	0.47
	(2.50)	(3.04)	(2.00)	(0.32)
Participations in previous studies				
Never	0.21	0.32	0.13	0.19***
	(0.41)	(0.47)	(0.33)	(0.05)
Once or Twice	0.37	0.32	0.41	-0.09
	(0.48)	(0.47)	(0.49)	(0.06)
3-10 Times	0.31	0.15	0.43	-0.28***
	(0.46)	(0.36)	(0.50)	(0.06)
11-50 Times	0.09	0.17	0.03	0.13***
	(0.29)	(0.38)	(0.18)	(0.04)
More than 50 Times	0.02	0.04	0.00	0.04**
	(0.13)	(0.19)	(0.00)	(0.02)
Observations	249	106	143	249

Note. Table includes demographics collected in both online experiments. Significance of differences is based on t-tests examining equality to zero. Some additional collected demographics are not reported in the table, but available in published data.

Table C2 Strategy classifications in Study 1 across subject pools

	AMT sample (n=106)				**UoN sample (n=143)**			
Game	CC	FR	UC	MM	CC	FR	UC	MM
G1	35.8	42.5	17.9	3.8	37.8	49.0	8.4	4.9
G2	42.5	39.6	12.3	5.7	44.1	44.8	9.1	2.1
G3	33.0	49.1	13.2	4.7	28.0	58.7	7.7	5.6
G4	38.7	43.4	9.4	8.5	32.9	56.0	9.1	2.1
G5	44.3	43.4	8.5	3.8	36.4	55.9	5.6	2.1
G6	48.1	42.5	6.6	2.8	48.3	43.4	4.9	3.5
G7	27.4	54.7	13.2	4.7	32.2	58.7	7.0	2.1
G8	34.9	43.4	17.0	4.7	38.5	53.8	4.9	2.8
Average	*38.1*	*44.8*	*12.2*	*4.8*	*37.2*	*52.5*	*7.1*	*3.1*

Note: AMT: Amazon MTurk sample. UoN: University of Nottingham student sample. CC: Conditional Cooperator; FR: Free-Rider; UC: Unconditional Cooperator; MM: Mismatcher. Each entry is the percentage of subjects using that strategy in that game.

Appendix D *Effect of GAIN and LOSS in stochastic choice models*

In this appendix we examine the effects of GAIN and LOSS on the probability of SM conditional cooperation in a sequential prisoner's dilemma using a stochastic choice model without social preferences. In our experiment R is constant and so the effect of changes in GAIN = $(T - R)/R$ on the probability of conditional cooperation has the same sign as the effect of changes in T. The effect of LOSS = $(R - S)/R$ has the opposite sign as the effect of S.

Let q denote the probability that FM cooperates, and let V_j denote SM's expected (selfish) payoff from strategy j. The expected payoffs from conditional cooperation (cc), free riding (fr), unconditional cooperation (uc), and mis-matching FM's choice (mm) are:

$$V_{cc} = qR + (1-q)P,$$
$$V_{fr} = qT + (1-q)P,$$
$$V_{uc} = qR + (1-q)S,$$
$$V_{mm} = qT + (1-q)S.$$

Let $Z_j = V_j + \varepsilon_j$ be the perturbed payoff from strategy j, where the ε_j error terms are independent draws from an extreme value distribution with scale parameter $1/\lambda$. Assuming the individual chooses the strategy giving the largest perturbed payoff, this induces a multinomial logit choice structure where the probability of conditional cooperation, p_{cc}, is given by

$$p_{cc} = \frac{\exp\{\lambda V_{cc}\}}{\sum \exp\{\lambda V_j\}} = \frac{1}{1 + \exp\{\lambda(V_{fr} - V_{cc})\} + \exp\{\lambda(V_{uc} - V_{cc})\} + \exp\{\lambda(V_{mm} - V_{cc})\}}$$

$$= \frac{1}{1 + \exp\{\lambda q(T-R)\} + \exp\{\lambda(1-q)(S-P)\} + \exp\{\lambda q(T-R) + \lambda(1-q)(S-P)\}}$$

$$= (1 + \exp\{\lambda q(T-R)\})^{-1}(1 + \exp\{\lambda(1-q)(S-P)\})^{-1}$$

For fixed q the first term is decreasing in T and so p_{cc} decreases with T and with GAIN. For fixed q the second term is decreasing in S and so p_{cc} decreases with S and with LOSS.

In order to sign the comparative statics with endogenous q, we need a model of FMs choice. Using a QRE model (McKelvey and Palfrey (1995)), the choice probabilities are given by the solution to the system of equations:

$$p_{cc} = (1 + \exp\{\lambda q(T-R)\})^{-1}(1 + \exp\{\lambda(1-q)(S-P)\})^{-1}$$
$$p_{fr} = (1 + \exp\{\lambda q(R-T)\})^{-1}(1 + \exp\{\lambda(1-q)(S-P)\})^{-1}$$
$$p_{uc} = (1 + \exp\{\lambda q(T-R)\})^{-1}(1 + \exp\{\lambda(1-q)(P-S)\})^{-1}$$
$$p_{mm} = (1 + \exp\{\lambda q(R-T)\})^{-1}(1 + \exp\{\lambda(1-q)(P-S)\})^{-1}$$
$$q = \left((1 + \exp\{\lambda\left((P-S)p_{fr} + (P-R)p_{cc} + (T-S)p_{mm} + (T-R)p_{uc}\right)\}\right)^{-1}$$

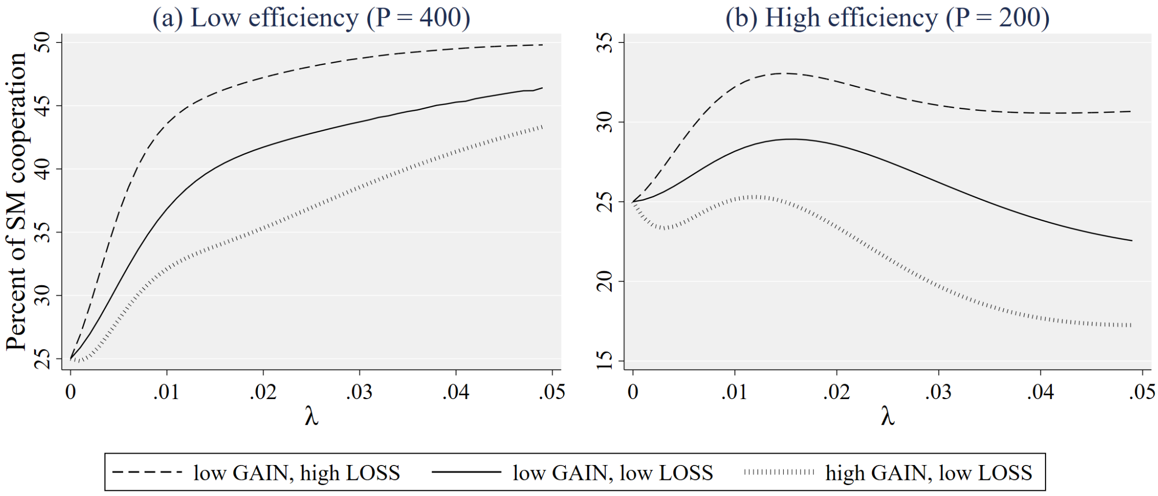
The solution gives the QRE probabilities as functions of the payoff parameters and precision parameter λ. We use numerical methods to solve these equations for our payoff parameters.

Figure D1(a) below shows the QRE probability of conditional cooperation as a function of λ for the *low efficiency* games ($R = 500, P = 400$). As a baseline take the low GAIN and low LOSS game ($T = 600, S = 180$). The QRE probability of conditional cooperation in this baseline is shown in Figure D1(a) as the middle line. Keeping $S = 180$ and increasing T to 800 gives the high GAIN and low LOSS game, and this results in a lower probability of conditional cooperation, as shown by the lowest line. Keeping $T = 600$ and reducing S to 40 gives the low GAIN and high LOSS game, and this results in a higher probability of conditional cooperation, as shown by the uppermost line.

Figure D1(b) shows the QRE probability of conditional cooperation for the high efficiency games ($R = 500, P = 200$). Again, take the low GAIN and low LOSS game ($T = 600, S = 90$) as a baseline. The QRE probability of conditional cooperation is the middle line in Figure D1(b). Keeping $S = 90$ and increasing T to 800 gives the high GAIN and low LOSS game, and this results in the lowest line. Keeping $T = 600$ and reducing S to 20 gives the low GAIN and high LOSS game, and results in the uppermost line.

In summary, for any given λ, the QRE probability of conditional cooperation increases with LOSS and decreases with GAIN.

Figure D1 QRE probability of conditional cooperation for low EFF (left panel) and high EFF (right panel) games



Appendix E *Instructions for Study 2*

Welcome

Thank you for participating in this experiment. At the end of this experiment you will receive £5 for your participation, in addition to other money to be paid as a result of decisions made in the experiment. These instructions explain how the additional money depends on decisions, so please read them carefully. If you have a question at any time, please raise your hand and an experimenter will come to you to answer it. During the session, please do not use mobile phones or try to communicate with any of the other participants. If you do not follow these rules, you will be excluded from the study and will not be paid.

General Instructions

In this session, you will be anonymously matched with one other participant. Each of you will make decisions for 64 decision situations. In each situation, each of you will earn Tokens depending on your decisions.

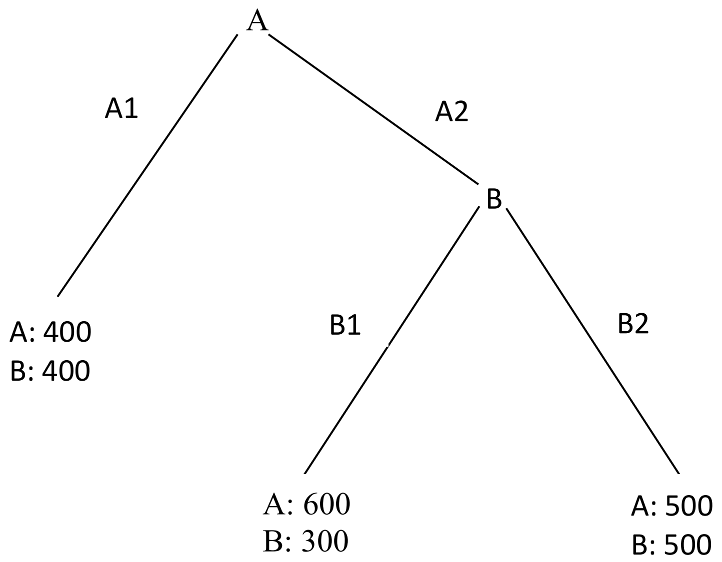
At the end of the session, one of the decision situations will be randomly chosen. Your earnings from this situation will be converted from Tokens to pounds at a rate of 1 Token = £0.02. This will be added to your participation fee of £5.00. In the same way, Tokens earned by the person matched with you in that same situation will also be converted to pounds at a rate of 1 Tokens = £0.02, and they will earn this amount in addition to their £5 participation fee. Payments will be made individually and privately via PayPal.

The Decision Situation

In each decision situation there are two "roles", Person A and Person B. You will either be assigned the role of Person A or Person B at the beginning of the first decision situation and you will keep this role for all 64 situations. The person you are matched with will be assigned the other role for all 64 situations.

Each decision situation will be presented on a screen like the example screen below. The choices available to each player are represented by lines and the earnings are given by the numbers at the end of the lines. The earnings values will differ in each decision situation. Neither your choices, nor the choices of the person you are matched with, will affect the earnings values in each decision situation.

Example Screen



Person A will be making choices between A1 and A2. Person B will be making choices between B1 and B2. Earnings will depend on the choices as follows.

If Person A chooses A1 the token earnings are as shown at the end of the leftmost branch: Person A will earn 400 Tokens and Person B will earn 400 Tokens.

If Person A chooses A2 then Person B's choice will determine the outcome. If Person B chooses B1, Person A will earn 600 tokens and Person B will earn 300 Tokens. If Person B chooses B2, Person A will earn 500 Tokens and Person B will earn 500 Tokens.

Person A and Person B will make their choices at the same time. Thus, Person B will make a choice in every decision situation, even though Person B's choice only affects the outcome if Person A chooses A2.

You will not receive any feedback on the other person's choice or the outcomes of the decision situations until the end of the session.

To make a choice on the experimental screen, you can simply click on the line that you wish to choose. Once selected the chosen line will be highlighted.

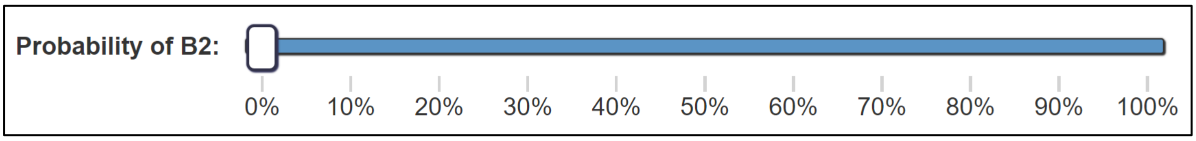
Probability task

For each decision situation you are also asked to provide your best guess that the other Player selects A2 or B2.

This means, Player A is asked to guess the likelihood that Player B chooses B2 and Player B is asked to guess the likelihood that Player A chooses A2.

Your guess is a percentage probability from 0 to 100 - with 0 indicating a 0-out-of-100 chance that the other Player chose A2/B2, and 100 indicating a 100-out-of-100 chance.

You choose Your Guess by clicking a response bar on your screen as can be seen below.



The width of the bar represents 100% and you can select the probability corresponding to Your Guess by clicking on the bar. Larger values represent a greater chance that the other Player chooses A2/B2 and smaller values represent a smaller chance that the other Player chooses A2/B2.

Your guess and the actual choice of the other Player will be used to determine your chance of winning an additional 200 Tokens. Your chance of winning the Tokens is set so that more accurate guesses lead to a higher chance of winning.

The payment rule is designed so that you can secure the largest chance of winning the additional 200 Tokens by reporting your most-accurate guess. That is, if you think there is a 20% chance the other Player chooses A2/B2, you maximise your chances of winning the additional 200 Tokens by submitting a guess of 20%; if you think there is a 78% chance the other player chooses A2/B2, you maximise your chances of winning the additional 200 Tokens by submitting a guess of 78%. Etc. The precise payment rule details are available by request at the end of the experiment.

Ending the Session

At the end of the session, two different decision situations will be randomly chosen as explained above.

You will be informed of the choices and token earnings of you and the person you are matched with for the first chosen decision situation.

You will also be informed about the choices of the person you are matched with in the second chosen decision situation. This will determine the chance of winning the 200 additional Tokens in the probability task.

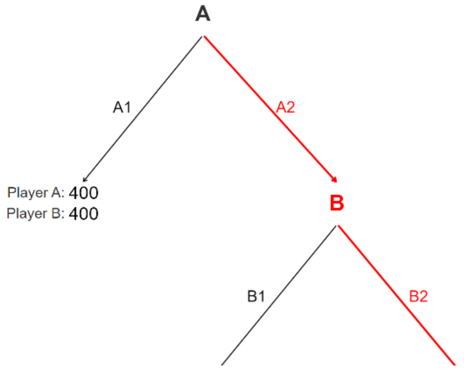
For both decisions you will be paid £0.02 per token in addition to your £5 participation fee.

Comprehension Questions

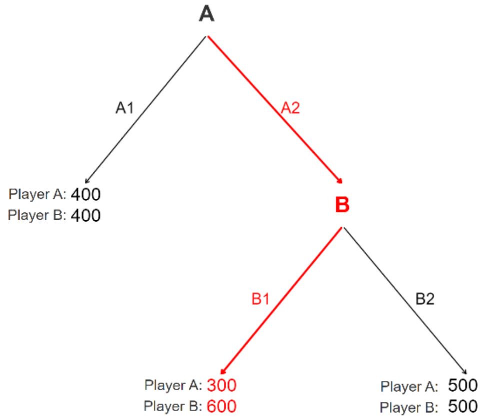
1. With whom are you grouped for each decision situation?
    a. With the same participant for the whole experiment.
    b. Each situation with a different participant.
2. Which "role" are you playing as?
    a. A different "role" each decision situation.
    b. The same "role" for the entire experiment

The following 4 images show possible decisions made by **Person A and Person B.**

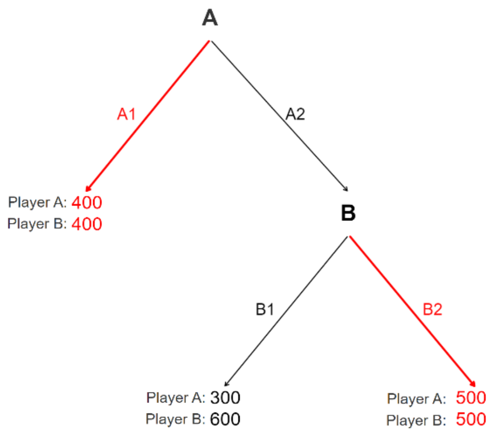
For each image, please use the boxes on the right to insert how many tokens Person A and Player B would receive with the decisions shown in the image.



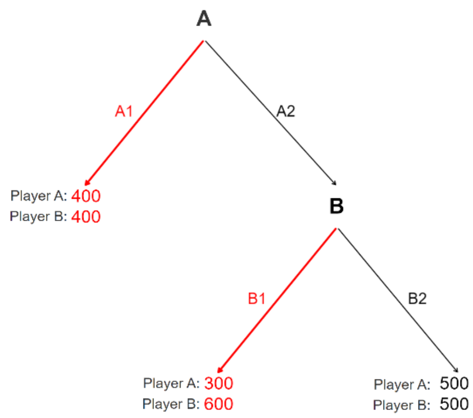
3a) How many Tokens would Person A receive?
3b) How many Tokens would Person B receive?



4a) How many Tokens would Person A receive?
4b) How many Tokens would Person B receive?

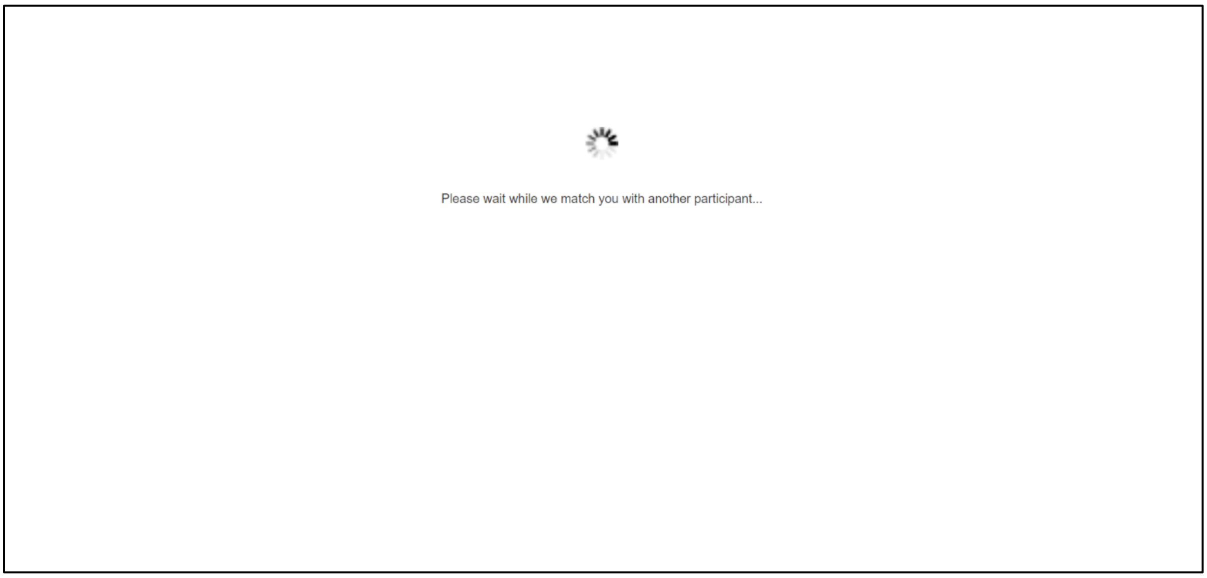


5a) How many Tokens would Person A receive?
5b) How many Tokens would Person B receive?

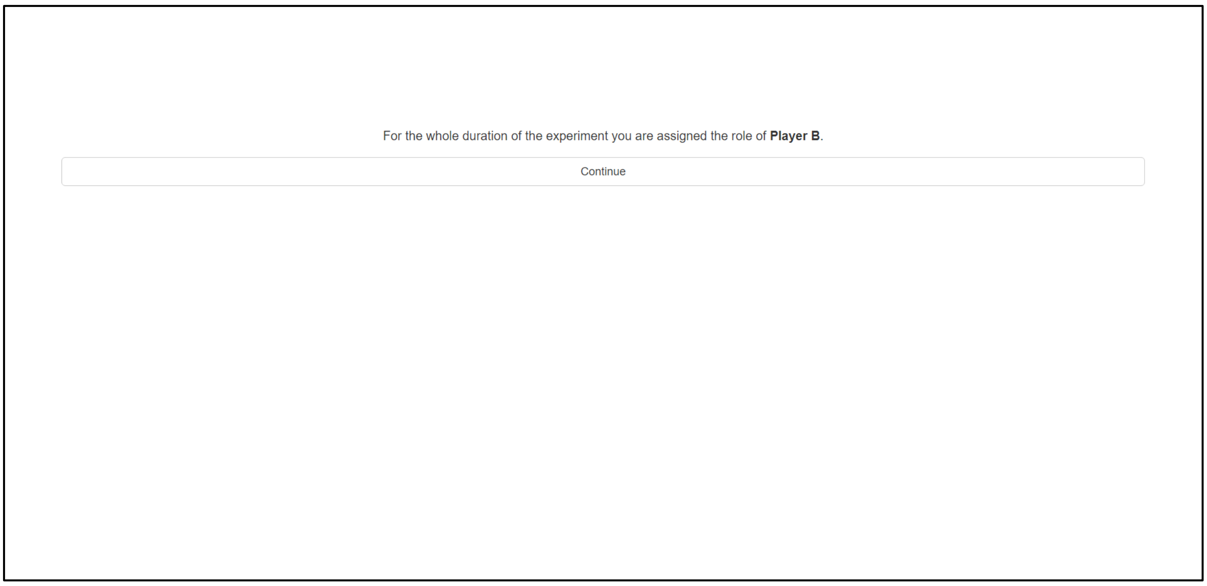


6a) How many Tokens would Person A receive?
6b) How many Tokens would Person B receive?

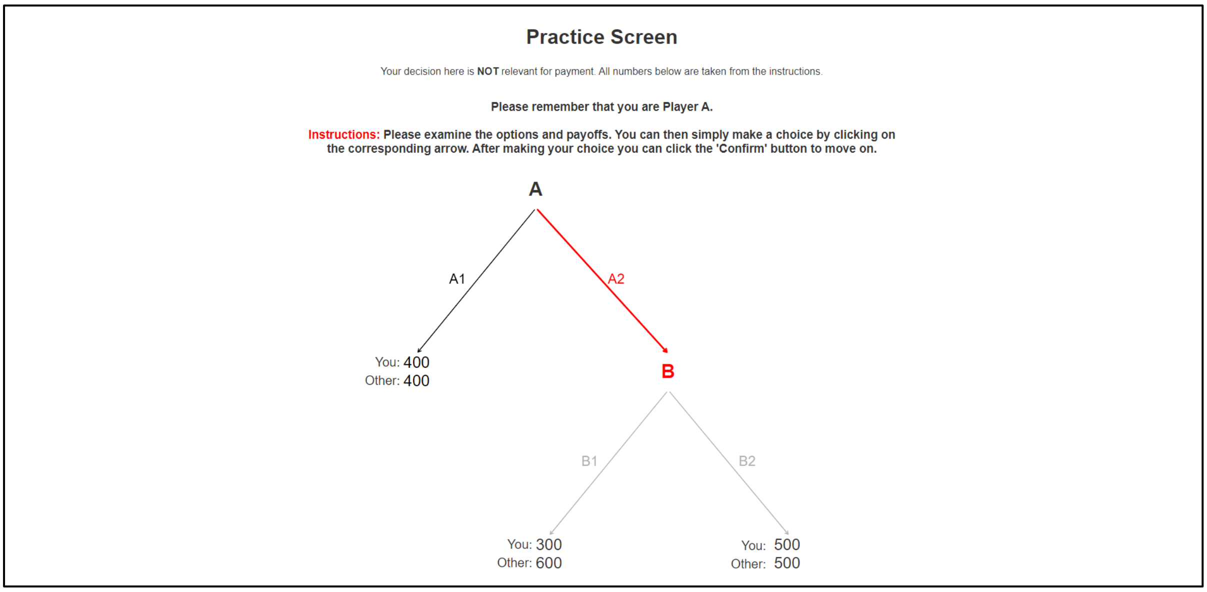
Screenshots of Lab Experiment



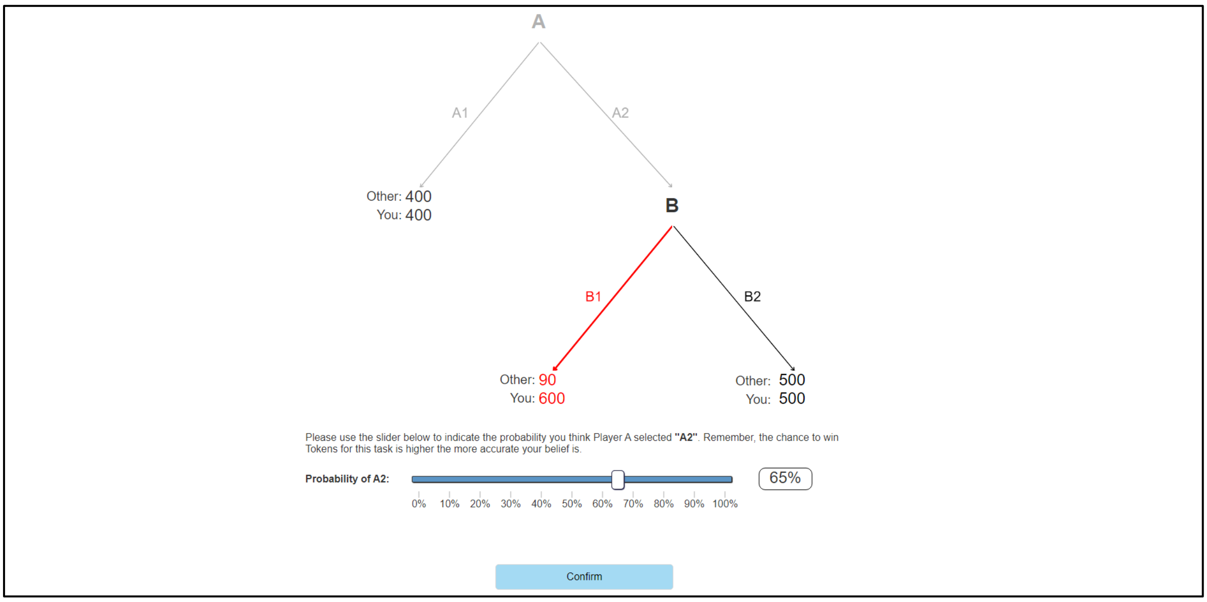
Screenshot 1: Matching stage



Screenshot 2: Role assignment



Screenshot 3: Practice screen



Screenshot 4: Decision screen

Questionnaire

1. How often have you previously participated in the experiments including similar decision problems (e.g. dividing an amount of money between yourself and another person)?
    a. Never
    b. Once or Twice
    c. 3-10 times
    d. 11-50 times
    e. More than 50 times
    f. Prefer not to say
2. What is your gender
    a. Female
    b. Male

c. Other
d. Prefer not to say

3. What is your age?
4. What is your nationality?
5. What is your citizenship?
    a. United Kingdom
    b. European Union
    c. United States
    d. Latin American Countries
    e. African Countries
    f. Asian Countries
    g. Other
    h. Prefer not to say
6. Which of the following describes you best?
    a. Asian
    b. Native Hawaiian or Other Pacific Islander
    c. Black or African
    d. Middle Eastern
    e. Latin American
    f. White
    g. Mixed background
    h. Other
    i. Prefer not to say
7. What is your current year of university?
    a. Year 1
    b. Year 2
    c. Year 3
    d. MA/MSc
    e. PhD
    f. Diploma
    g. Foundation Programme
    h. International Exchange Programme
    i. Other
    j. Prefer not to say
8. What is your major in the university?
9. How much do you spend per month, excluding rent?
    a. Less than £200
    b. £200 to £399
    c. £400 to £599
    d. £600 to £799
    e. £800 to £999
    f. £1,000 or more
    g. Prefer not to say
10. In politics, people sometimes talk about the ‘left’ and the ‘right’. Where would you place your own views on a scale from 0 to 10? (0 means the most left and 10 means the most right)

Appendix F *Monte Carlo simulations*

Prior to estimation we conducted Monte Carlo simulations to explore the properties of the estimators under alternative data generating processes. First, we generated data from model 1:

$$\Pr\{cooperate\} = \Phi(\mu/\sigma + (R-T)/\sigma + \rho(T-S))/\sigma).$$

using our Study 2 payoff parameters and various combinations of μ, σ and ρ. We used values of $\rho \in \{0,0.2,0.4\}$ corresponding to no, low and high weights on other's payoff. Low and high noise processes are represented by $\sigma \in \{100,200\}$, and negative, zero, and positive bias toward cooperation are represented by $\mu \in \{-100,0,100\}$.

We generated 1000 replications for each parameter combination, where a replication generates a sequence of 64 choices for our games. For each replication we ran two regressions. The first is a probit regression without a constant, estimating $\Phi(\beta_1(R-T) + \beta_2(T-S))$. Our estimate of ρ is $\hat{\rho} = \widehat{\beta_2}/\widehat{\beta_1}$ and standard errors are determined using the delta method. The second regression includes a constant and estimates $\Phi(\beta_0 + \beta_1(R-T) + \beta_2(T-S))$. Our estimate of μ is $\hat{\mu} = \widehat{\beta_0}/\widehat{\beta_1}$ and we again estimate ρ as $\hat{\rho} = \widehat{\beta_2}/\widehat{\beta_1}$, using the delta method to calculate standard errors.

Table F1 reports results. $\%id$ gives the proportion of 1000 replications in which we obtain 'sensible' estimates. We exclude cases where the model fails to converge due to under-identification and cases where $\hat{\sigma} < 0$ or $|\hat{\rho}| > 1$. $Av\ \hat{\rho}$ is the average estimate of ρ over the remaining cases, and $Av\ se(\hat{\rho})$ is the average of the associated standard errors. *Power* gives the proportion of 1000 replications that resulted in a significantly positive estimate of ρ (i.e. $\hat{\rho}$ / $se(\hat{\rho}) > 1.96$).

Note that neither the data generation process nor the regression models examined in Table F1 use beliefs and are essentially based on the model in the text with $q = 1$.

We studied the role of beliefs by generating data from an additional, belief-augmented, model

$$\Pr\{cooperate\} = \Phi(\mu/\sigma + q(R-T)/\sigma + \rho q(T-S))/\sigma).$$

We wanted to use beliefs that vary with game payoffs in a reasonable way, and so to fix beliefs we use Rapoport's K-index (Rapoport (1967)), $q = (R-P)/(T-S)$, which has been shown to be systematically related to cooperation rates in prisoner's dilemma experiments (e.g., Balliet and Van Lange (2013); Spadaro, et al. (2022); Gächter, et al. (2023)). We generated 1000 replications for each combination of parameters and ran two regressions for each replication. The first regression estimates $\Phi(\beta_0 + \beta_1(R-T) + \beta_2(T-S)))$, i.e., ignoring beliefs. The second model estimates $\Phi\big(\beta_0 + \beta_1 q(R - T) + \beta_2 q(T - S)\big)$ using the belief data. In both models we estimate μ as $\hat{\mu} = \widehat{\beta_0}/\widehat{\beta_1}$ and ρ as $\hat{\rho} = \widehat{\beta_2}/\widehat{\beta_1}$, using the delta method to calculate standard errors. Results are given in Table F2.

Table F1 Monte Carlo Results: Model 1

			No constant in regression				With constant in regression			
μ	ρ	σ	*%id*	*Av* $\hat{\rho}$	*Av se*($\hat{\rho}$)	*power*	*%id*	*Av* $\hat{\rho}$	*Av se*($\hat{\rho}$)	*power*
-100	0	100	.278	-.340	.213	0	.219	-.011	.675	.033
-100	0	200	.889	-.320	.269	0	.723	.113	.677	.156
-100	.2	100	.954	-.054	.087	.009	.885	.164	.397	.254
-100	.2	200	.989	-.072	.181	.044	.84	.214	.579	.246
-100	.4	100	.988	.210	.051	.909	.966	.337	.278	.506
-100	.4	200	.999	.196	.105	.613	.891	.359	.490	.398
0	0	100	.866	-.003	.065	.017	.72	.009	.474	.1
0	0	200	.998	-.026	.128	.061	.779	.048	.686	.124
0	.2	100	.845	.198	.045	.806	.786	.161	.386	.232
0	.2	200	1	.191	.077	.709	.845	.186	.590	.245
0	.4	100	.989	.398	.030	.989	.973	.342	.276	.511
0	.4	200	1	.400	.054	.996	.888	.357	.487	.392
100	0	100	.509	.187	.044	.487	.428	-.007	.515	.06
100	0	200	.998	.180	.065	.753	.762	.048	.661	.133
100	.2	100	.952	.349	.029	.952	.902	.141	.390	.243
100	.2	200	1	.349	.047	.998	.826	.184	.589	.244
100	.4	100	.978	.517	.025	.978	.94	.334	.300	.472
100	.4	200	1	.520	.049	1	.885	.354	.512	.381

1000 replications from the data generating process $\Pr\{cooperate\} = \Phi(\mu/\sigma + (R - T)/\sigma + \rho(T - S))/\sigma)$; $\%id$ is proportion of replications where parameters satisfying $\hat{\sigma} > 0$ and $|\hat{\rho}| < 1$ can be estimated. *power* is the proportion of replications with significantly positive ρ̂ at the 5% level.

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Table F2 Monte Carlo Results: Belief-augmented model

			No Beliefs in regression				With Beliefs in regression			
μ	ρ	σ	*%id*	*Av* $\hat{\rho}$	*Av se*($\hat{\rho}$)	*power*	*%id*	*Av* $\hat{\rho}$	*Av se*($\hat{\rho}$)	*power*
-100	0	100	.541	.069	.916	.067	.996	-.025	.232	.007
-100	0	200	.614	.311	1.021	.188	.960	-.007	.399	.016
-100	.2	100	.737	.108	.761	.120	1.000	.195	.198	.164
-100	.2	200	.642	.306	.995	.195	.959	.184	.340	.070
-100	.4	100	.783	.153	.747	.178	.997	.391	.176	.625
-100	.4	200	.652	.322	.976	.192	.940	.367	.341	.186
0	0	100	.754	.148	.794	.179	1.000	.005	.178	.046
0	0	200	.655	.348	.925	.229	.964	-.000	.358	.029
0	.2	100	.799	.197	.713	.217	1.000	.203	.165	.327
0	.2	200	.653	.337	.977	.213	.974	.171	.345	.097
0	.4	100	.806	.239	.758	.243	.997	.402	.162	.706
0	.4	200	.671	.356	.960	.239	.930	.372	.346	.251
100	0	100	.801	.238	.696	.229	.999	-.012	.200	.057
100	0	200	.670	.348	.915	.221	.971	.008	.356	.047
100	.2	100	.788	.300	.698	.273	.999	.196	.206	.293
100	.2	200	.662	.384	.947	.252	.964	.209	.367	.150
100	.4	100	.732	.375	.795	.292	.975	.397	.238	.558
100	.4	200	.664	.378	1.015	.228	.923	.361	.381	.286

1000 replications from the data generating process $\Pr\{cooperate\} = \Phi(\mu/\sigma + q(R-T)/\sigma + \rho q(T-S))/\sigma))$, where $q = (R-P)/(T-S)$; $\%id$ is proportion of replications where parameters satisfying $\hat{\sigma} > 0$ and $|\hat{\rho}| < 1$ can be estimated. *power* is the proportion of replications with significantly positive ρ̂ at the 5% level.

Appendix G *Study 2 data*

Table G1 Cooperation Rates and Beliefs in Cooperation by Game

R	P	S	T	GAIN	LOSS	EFF	FM_C	SM_C	FM_q	SM_q
500	100	20	400	-.2	.64	.2	0.711	0.969	0.829	0.747
500	200	20	400	-.2	.64	.4	0.887	0.969	0.863	0.770
500	300	20	400	-.2	.64	.6	0.938	0.959	0.880	0.863
500	400	20	400	-.2	.64	.8	0.948	0.990	0.860	0.929
500	100	40	400	-.2	.82	.2	0.825	0.948	0.858	0.718
500	200	40	400	-.2	.82	.4	0.784	0.979	0.818	0.800
500	300	40	400	-.2	.82	.6	0.887	0.969	0.851	0.823
500	400	40	400	-.2	.82	.8	0.959	0.979	0.892	0.898
500	100	90	400	-.2	.92	.2	0.732	0.959	0.846	0.750
500	200	90	400	-.2	.92	.4	0.794	0.948	0.808	0.741
500	300	90	400	-.2	.92	.6	0.856	0.938	0.833	0.761
500	400	90	400	-.2	.92	.8	0.918	0.990	0.871	0.859
500	100	180	400	-.2	.96	.2	0.680	1.000	0.834	0.714
500	200	180	400	-.2	.96	.4	0.804	0.979	0.826	0.809
500	300	180	400	-.2	.96	.6	0.866	0.969	0.853	0.772
500	400	180	400	-.2	.96	.8	0.887	0.979	0.858	0.815
500	100	20	600	.2	.64	.2	0.165	0.495	0.409	0.309
500	200	20	600	.2	.64	.4	0.392	0.515	0.433	0.408
500	300	20	600	.2	.64	.6	0.753	0.515	0.483	0.606
500	400	20	600	.2	.64	.8	0.928	0.474	0.422	0.874
500	100	40	600	.2	.82	.2	0.196	0.557	0.420	0.296
500	200	40	600	.2	.82	.4	0.351	0.485	0.425	0.352
500	300	40	600	.2	.82	.6	0.546	0.526	0.433	0.450
500	400	40	600	.2	.82	.8	0.773	0.515	0.466	0.638
500	100	90	600	.2	.92	.2	0.124	0.567	0.438	0.292
500	200	90	600	.2	.92	.4	0.309	0.536	0.415	0.355
500	300	90	600	.2	.92	.6	0.433	0.567	0.425	0.410
500	400	90	600	.2	.92	.8	0.711	0.536	0.479	0.499
500	100	180	600	.2	.96	.2	0.124	0.588	0.418	0.302
500	200	180	600	.2	.96	.4	0.340	0.577	0.443	0.340
500	300	180	600	.2	.96	.6	0.464	0.639	0.462	0.425
500	400	180	600	.2	.96	.8	0.608	0.546	0.462	0.444

R	P	S	T	GAIN	LOSS	EFF	FM_C	SM_C	FM_q	SM_q
500	100	20	800	.6	.64	.2	0.134	0.309	0.312	0.283
500	200	20	800	.6	.64	.4	0.206	0.340	0.322	0.355
500	300	20	800	.6	.64	.6	0.608	0.278	0.351	0.553
500	400	20	800	.6	.64	.8	0.887	0.247	0.333	0.806
500	100	40	800	.6	.82	.2	0.072	0.320	0.312	0.226
500	200	40	800	.6	.82	.4	0.216	0.299	0.319	0.310
500	300	40	800	.6	.82	.6	0.351	0.309	0.325	0.358
500	400	40	800	.6	.82	.8	0.691	0.330	0.341	0.665
500	100	90	800	.6	.92	.2	0.113	0.340	0.336	0.233
500	200	90	800	.6	.92	.4	0.175	0.381	0.337	0.287
500	300	90	800	.6	.92	.6	0.361	0.330	0.335	0.379
500	400	90	800	.6	.92	.8	0.474	0.361	0.344	0.466
500	100	180	800	.6	.96	.2	0.072	0.340	0.339	0.226
500	200	180	800	.6	.96	.4	0.175	0.361	0.305	0.281
500	300	180	800	.6	.96	.6	0.392	0.381	0.337	0.364
500	400	180	800	.6	.96	.8	0.546	0.330	0.354	0.455
500	100	20	1000	1	.64	.2	0.103	0.216	0.258	0.260
500	200	20	1000	1	.64	.4	0.196	0.206	0.261	0.321
500	300	20	1000	1	.64	.6	0.598	0.144	0.240	0.509
500	400	20	1000	1	.64	.8	0.856	0.134	0.248	0.799
500	100	40	1000	1	.82	.2	0.082	0.175	0.227	0.192
500	200	40	1000	1	.82	.4	0.165	0.216	0.227	0.233
500	300	40	1000	1	.82	.6	0.237	0.196	0.233	0.312
500	400	40	1000	1	.82	.8	0.639	0.206	0.265	0.536
500	100	90	1000	1	.92	.2	0.041	0.196	0.216	0.179
500	200	90	1000	1	.92	.4	0.062	0.216	0.239	0.206
500	300	90	1000	1	.92	.6	0.196	0.206	0.230	0.261
500	400	90	1000	1	.92	.8	0.443	0.206	0.234	0.398
500	100	180	1000	1	.96	.2	0.082	0.165	0.222	0.175
500	200	180	1000	1	.96	.4	0.113	0.237	0.236	0.224
500	300	180	1000	1	.96	.6	0.186	0.206	0.218	0.277
500	400	180	1000	1	.96	.8	0.402	0.227	0.265	0.356

Table G2 SM Parameter Estimates

ID	$\hat{\sigma}$	se($\hat{\sigma}$)	$\hat{\rho}$	se($\hat{\rho}$)	$\hat{\mu}$	se($\hat{\mu}$)	Coop	Comment
4	1.70	1.04	0.22	0.02	-7.01	3.00	0.36	
6	n/a	n/a	n/a	n/a	n/a	n/a	1.00	Always cooperator[A]
7	11.13	9.09	0.58	0.10	-80.14	23.87	0.31	
8	1.47	2.77	0.26	0.01	-10.63	2.68	0.33	
10	61.26	17.29	0.49	0.09	-44.38	30.80	0.53	
12	22.55	5.85	0.45	0.05	-32.89	8.96	0.39	
14	n/a	n/a	n/a	n/a	n/a	n/a	1.00	Always cooperator[A]
16	n/a	n/a	n/a	n/a	n/a	n/a	0.28	underidentified[C]
18	58.44	30.45	0.26	0.18	145.13	101.69	0.84	
21	22.97	16.10	0.69	0.21	-33.11	23.07	0.48	
24	4.48	1.64	0.55	0.01	-6.76	2.98	0.80	
25	n/a	n/a	n/a	n/a	n/a	n/a	0.27	underidentified[C]
26	37.10	15.67	0.42	0.24	-106.41	68.34	0.25	
29	n/a	n/a	n/a	n/a	n/a	n/a	0.25	Payoff maximizer[B]
30	29.99	7.43	0.69	0.05	-90.13	19.82	0.66	
32	n/a	n/a	n/a	n/a	n/a	n/a	0.25	Payoff maximizer[B]
34	66.17	15.41	0.44	0.10	-57.57	26.04	0.42	
36	59.28	13.90	0.47	0.09	-130.50	35.24	0.27	
40	n/a	n/a	n/a	n/a	n/a	n/a	0.25	Payoff maximizer[B]
41	n/a	n/a	n/a	n/a	n/a	n/a	0.23	underidentified[D]
43	55.84	13.35	0.43	0.12	-87.56	36.39	0.33	
44	n/a	n/a	n/a	n/a	n/a	n/a	0.25	Payoff maximizer[B]
47	48.31	15.76	0.40	0.06	-10.51	14.15	0.53	
48	60.53	17.54	0.49	0.05	7.42	26.50	0.73	
50	57.36	12.97	0.38	0.07	-94.62	28.56	0.31	
52	20.36	5.93	0.73	0.09	-82.24	23.86	0.64	
54	38.83	19.32	0.31	0.08	-65.42	39.66	0.31	
56	15.41	6.86	0.22	0.21	-51.58	25.05	0.20	
58	89.99	46.71	-0.02	0.25	130.00	76.93	0.84	
60	24.83	6.61	0.41	0.04	-16.39	7.73	0.50	
62	n/a	n/a	n/a	n/a	n/a	n/a	0.52	underidentified[C]
64	47.54	11.17	0.52	0.06	-54.32	24.09	0.56	
67	61.25	12.93	0.59	0.06	-105.90	36.96	0.61	
68	11.71	3.01	0.46	0.03	-10.63	4.55	0.55	
71	3.45	1.52	0.26	0.02	-1.22	1.82	0.47	
72	58.30	15.82	0.67	0.09	-108.94	45.18	0.67	
74	34.70	7.98	0.53	0.07	-37.03	27.53	0.67	
76	9.22	3.32	0.40	0.03	2.51	2.59	0.69	
78	224.58	134.04	0.50	0.30	-222.47	100.79	0.25	
80	92.42	25.87	0.75	0.11	-121.75	32.87	0.42	
82	67.57	15.18	0.29	0.09	-33.97	27.15	0.42	
86	n/a	n/a	n/a	n/a	n/a	n/a	0.98	underidentified[C]
88	67.67	15.68	0.57	0.09	-131.87	32.02	0.33	
89	15.33	4.77	0.50	0.03	-6.45	7.64	0.73	
90	41.07	25.40	0.53	0.11	-102.48	58.86	0.25	
93	34.63	9.40	0.61	0.05	-50.47	21.55	0.72	
95	63.57	18.72	0.57	0.14	-54.09	35.65	0.58	
99	n/a	n/a	n/a	n/a	n/a	n/a	0.25	Payoff maximizer[B]
100	87.77	21.50	0.32	0.11	-85.55	39.44	0.28	
102	n/a	n/a	n/a	n/a	n/a	n/a	0.25	Payoff maximizer[B]
103	108.11	34.74	0.17	0.18	78.88	54.85	0.66	
104	45.50	15.73	0.36	0.17	-80.41	43.37	0.28	
107	5.03	2.92	0.40	0.01	-2.23	2.31	0.56	
108	n/a	n/a	n/a	n/a	n/a	n/a	0.27	underidentified[C]
112	89.54	58.72	0.36	0.19	127.50	103.22	0.92	
114	n/a	n/a	n/a	n/a	n/a	n/a	1.00	Always cooperator[A]
115	43.05	15.06	0.28	0.08	-2.24	12.13	0.52	
116	867.48	5387.21	-0.46	7.01	2091.30	13444.71	0.98	

121	334.13	766.01	-0.80	3.64	1006.42	2631.20	0.94	
122	0.52	0.40	0.25	0.00	-3.43	0.67	0.41	
124	12.66	4.78	0.45	0.03	-14.22	11.05	0.63	
125	37.86	11.92	0.37	0.11	-63.74	37.19	0.34	
127	n/a	n/a	n/a	n/a	n/a	n/a	0.25	Payoff maximizer[B]
129	64.16	20.48	0.44	0.07	-47.93	20.53	0.48	
130	n/a	n/a	n/a	n/a	n/a	n/a	1.00	Always cooperator[A]
132	80.39	62.12	0.67	0.21	42.34	162.68	0.97	
134	57.73	14.28	0.54	0.07	-60.84	17.35	0.39	
137	11.35	6.58	0.23	0.03	-14.15	9.60	0.31	
139	64.18	15.60	0.73	0.07	-133.02	28.49	0.39	
142	39.53	10.47	0.60	0.06	-71.42	19.33	0.47	
144	27.01	6.55	0.22	0.08	28.67	18.76	0.55	
145	n/a	n/a	n/a	n/a	n/a	n/a	0.25	Payoff maximizer[B]
146	16.74	7.24	0.67	0.05	-202.86	19.98	0.27	
149	n/a	n/a	n/a	n/a	n/a	n/a	0.98	underidentified[D]
150	n/a	n/a	n/a	n/a	n/a	n/a	1.00	Always cooperator[A]
152	n/a	n/a	n/a	n/a	n/a	n/a	0.27	underidentified[C]
155	31.06	10.21	-0.05	0.13	-11.57	20.54	0.28	
158	n/a	n/a	n/a	n/a	n/a	n/a	0.25	Payoff maximizer[B]
159	53.04	13.20	0.07	0.24	-8.88	47.65	0.27	
161	31.92	7.76	0.47	0.08	-61.37	19.37	0.34	
163	31.65	8.03	0.50	0.03	-23.27	13.77	0.67	
164	9.11	3.53	0.21	0.02	-1.26	4.13	0.38	
166	81.22	32.15	0.36	0.12	90.24	59.85	0.83	
168	32.48	9.56	0.64	0.05	-36.33	12.38	0.70	
170	19.18	5.86	0.45	0.05	-3.84	16.91	0.69	
172	n/a	n/a	n/a	n/a	n/a	n/a	0.25	Payoff maximizer[B]
174	170.96	62.77	0.73	0.19	-122.79	85.22	0.63	
176	61.14	18.38	0.62	0.09	-120.80	37.76	0.28	
181	n/a	n/a	n/a	n/a	n/a	n/a	1.00	Always cooperator[A]
182	16.45	4.59	0.43	0.04	-17.12	10.51	0.53	
184	84.20	24.71	0.46	0.06	-31.14	21.01	0.52	
187	68.89	21.06	0.43	0.08	-4.05	25.20	0.66	
189	33.65	10.21	0.59	0.05	-44.72	17.79	0.70	
190	34.83	20.85	0.30	0.17	-56.78	39.67	0.28	
192	n/a	n/a	n/a	n/a	n/a	n/a	0.25	Payoff maximizer[B]
193	n/a	n/a	n/a	n/a	n/a	n/a	0.25	Payoff maximizer[B]
194	2714.81	15354.74	-1.10	9.73	511.17	3379.62	0.52	Outlier

Notes. The estimates above are for SM and follow the econometric specification as outlined in the paper. For 26 individuals we cannot obtain maximum likelihood estimates because a linear combination of the regressors perfectly predicts outcomes. Of these cases of underidentification,
A: 6 always cooperate – choices perfectly predicted using a model with only a constant;
B: 12 maximise own payoff – choices perfectly predicted using a model with only $\boldsymbol{q}(\boldsymbol{R}-\boldsymbol{T})$;
C: 6 further underidentified cases can be perfectly predicted using a model with a constant and $\boldsymbol{q}(\boldsymbol{R}-\boldsymbol{T})$
D: 2 further underidentified cases can be perfectly predicted using a constant, $\boldsymbol{q}(\boldsymbol{R}-\boldsymbol{T})$ and $\boldsymbol{q}(\boldsymbol{T}-\boldsymbol{S})$.
One individual is treated as an outlier as the estimate of $\boldsymbol{\rho} < -\mathbf{1}$.

Appendix H *Further analyses regarding reciprocity (through EFF)*

1) Among the 64 games in study 2, we have 16 quadruples in each of which R, S and T are constant and EFF (through P) varies. Within each quadruple, SM cooperation only varies by 5.15% on average, and there is no monotonic relation between EFF and SM cooperation.
2) Fig. H1 plots fitted regression lines for each of the 16 quadruples to examine whether changes in EFF affect cooperation in each subset of games. Each line corresponds to a game where GAIN and LOSS are constant and only EFF varies across four levels. Observe that there is quite a lot of variation in cooperation across different values of GAIN and LOSS, but not much with respect to EFF.

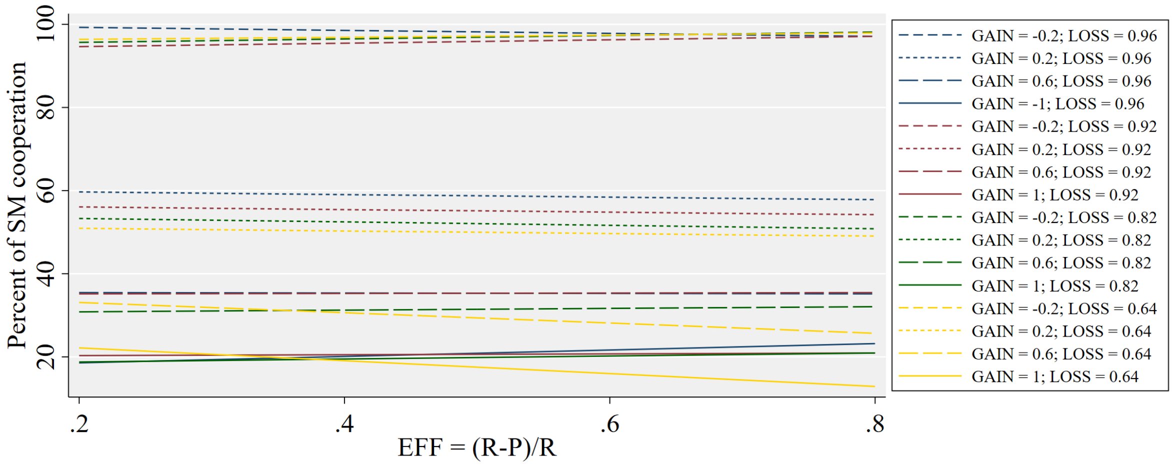

Fig. H1 The effect of EFF for each quadruple

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