The Impact of the Termination Rule on Cooperation in a Prisoner's Dilemma Experiment*

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Abstract

Cooperation in prisoner's dilemma games can usually be sustained only if the game has an infinite horizon. We analyze to what extent the theoretically crucial distinction of finite vs. infinite-horizon games is reflected in the outcomes of a prisoner's dilemma experiment. We compare three different experimental termination rules in four treatments: a known finite end, an unknown end, and two variants with a random termination rule (with a high and with a low continuation probability, where cooperation can occur in a subgame-perfect equilibrium only with the high probability). We find that the termination rules do not significantly affect average cooperation rates. Specifically, employing a random termination rule does not cause significantly more cooperation compared to a known finite horizon, and the continuation probability does not significantly affect average cooperation rates either. However, the termination rules may influence cooperation over time and end-game behavior. Further, the (expected) length of the game significantly increases cooperation rates. The results suggest that subjects may need at least some learning opportunities (like repetitions of the supergame) before significant backward induction arguments in finitely repeated game have force.

Keywords Prisoner's dilemma, Repeated games, Infinite-horizon games, Experimental economics

JEL classification C72, C92, D21, D43

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1 Introduction

The game-theoretic predictions for repeated games crucially depend on whether a game is finitely or infinitely repeated. In a finitely repeated dilemma game, cooperation usually cannot occur (Luce and Raiffa 1957), but it can emerge if the game has infinitely many periods. There are several exceptions to this rule. Cooperation can be part of a subgame perfect Nash equilibrium in a finitely repeated game if, for example, the stage game has multiple Nash equilibria (Benoit and Krishna 1985; 1987), if there is uncertainty about players' preferences (Kreps et al. 1982), or if the number of periods to be played is not common knowledge (Samuelson 1987; Neymann 1999). Also, when players have other-regarding preferences (for example, if they are inequality averse), cooperation can emerge in finitely repeated games (Fehr and Schmidt 1999; Bolton and Ockenfels 2000). Nevertheless, a complete-information prisoner's dilemma game with a unique equilibrium in the stage game and where players have standard preferences requires infinitely many repetitions for cooperation to be possible.

Because the issue of finitely vs. infinitely many repetitions is crucial in theory, it needs to be carefully addressed in the design of laboratory experiments. However, whereas experimentalists have certainly paid close attention to the design of experimental termination rules, no consensus seems to exist regarding the most suitable experimental design as far as this point is concerned. As we will see, experimentalists use different rules and they seem to disagree about the pros and cons of them. We will also see that there are contradicting results about how the termination rules affect cooperation.

Which termination rules are used in experiments? The first termination rule, the *finite horizon*, is simply to repeat the stage game of the experiment a finite number of times and to inform participants about the number of repetitions in the instructions. This rule was used, for example, in the early experiments of Flood (1952) and Rapoport and Chammah (1965). The second rule (which we label *unknown horizon*) is to refrain from informing participants about the actual length of the experiment (e.g., Fouraker and Siegel 1963). The experimenter may tell participants that there will be a "large" number of repetitions, or that there will be a certain minimum number of periods they will play, but the actual number of periods is unknown. The third termination rule is to impose a *random-stopping rule* to terminate the experiment (Roth and Murnighan 1978; Axelrod 1980). The termination mechanism (for example, the throw of a die or a random computer draw) and the termination probability are explained in detail in the instructions.

Since experimentalists use these different methods for ending cooperation experiments, it seems useful to review which properties of the experimental termination rules they regard as (non-)desirable:

– Presumably influenced by Luce and Raiffa's (1957) theoretical result, experimentalists often saw a need to *avoid the unraveling of cooperation* that may occur due to the finiteness of the horizon. If the proposition that cooperation cannot occur in finitely repeated games has descriptive power in experiments, then the finite horizon is not suitable for cooperation experiments whereas the random stopping rule and the unknown horizon would be. Empirically, however, it is well known that stable cooperation does occur also in finitely repeated games.

– A related concern is to *avoid end-game effects*. Morehous (1966) observed that defection rates increase towards the end of the game when the horizon of the game is known to be finite. Thus, even though cooperation does usually not completely unravel with a finite horizon, some studies try to avoid this end-game effect by using the random stopping rule or the unknown horizon. For example, Axelrod's first tournament had a known and fixed duration of 200 periods whereas his second tournament used a probabilistic termination rule so that "end-game effects were successfully avoided" (Axelrod 1984, p. 42). Murnighan and Roth (1983, p. 284) argue that "consideration of end-game play is less critical" with the random termination rule. Holt (1985, p. 320) makes the same point. Alternatively, rather than avoiding end-game effects with the termination rule, some experimenters simply discard the finals period(s) of the game from the data so that no bias due to end-game effects can affect the data analysis.[1]

– Experimentalists are also concerned about *transparency* and *control*. Holt (1985) prefers to fully inform subjects about the things to come in an experiment. The lack of transparency is an argument against the unknown horizon. Also, with that termination rule, "subjects must form subjective probabilities greater than zero that a given period might be the last" (Roth and Murnighan 1978, p. 191). This rule thus lacks experimental control. The random termination rule and the finite horizon are transparent and enable control.

– Another goal mentioned in the literature is to *make the theory of infinitely repeated games applicable* to the experiment. With finitely many periods, the theory is bland; by contrast, the random termination rule "permits the nature of the equilibrium outcomes to be controlled" (Roth and Murnighan 1978, p. 191). However, Selten, Mikzewitz and Uhlich (1997) argue that infinitely repeated games cannot be played in the laboratory. Participants will be aware that experiments can only be of finite duration as the experimenter simply cannot continue "forever". Selten, Mikzewitz and Uhlich (1997, p. 517) point out that "the stopping probability cannot remain fixed but must become one eventually". From this perspective, efforts to match the theoretical requirements of infinitely repeated games would miss the point.

[1] For recent references, see Kaplan and Ruffle (2006), Orzen (2008) or Suetens and Potters (2007).

In this paper, our aim is to investigate how the termination rules affect cooperation empirically, rather than adding to the debate of potential (dis-)advantages of termination rules. We conduct a series of laboratory experiments comparing different termination rules for repeated-game experiments. How the experimental designs regarding the termination of the game affect cooperation rates is of significance for both theorists and experimentalists. For game theorists, it seems to be of some importance to learn to what extent the distinction of finite vs. infinite horizon is reflected quantitatively in the outcomes of cooperation experiments; observational experience can then be useful for a reflection on assumptions. For experimentalists, it is essential to learn about the effects of the experimental designs because they may affect cooperation rates and therefore bias the experimental results if different studies adopt different designs.

Here is a summary of what we know about how these experimental designs affect cooperation rates. We have already mentioned that stable cooperation emerges even with finitely many repetitions but that end-game effects occur. Selten and Stoecker (1983) further noted that subjects learn to anticipate the endgame effect in that this effect is shifted to earlier rounds when a supergame with a finite horizon is repeated several times (see also Andreoni and Miller 1993). In that case, there is more unraveling and backward induction arguments have more force. Roth and Murnighan (1978) found that a random stopping rule with higher continuation probability does lead to more cooperation in the prisoner's dilemma. However, in the modified setup analyzed in Murnighan and Roth (1983), this could not be confirmed.[2] Engle-Warnick and Slonim (2004) ran trust game sessions with a known horizon of five periods and sessions with a random stopping rule with a continuation probability of 0.8. Their data show that the level of trust does not vary in the two treatments with inexperienced players even though the supergame was played twenty times. Dal Bo (2005) found that the continuation probability of the random-stopping rule matters when various prisoner's dilemma supergames are repeated ten times. The (expected) number of periods to be played was one, two and four in Dal Bo's experiments. We compare our findings to those of Dal Bo (2005) in the Discussion below.[3]

[2]The experimental design in Roth and Murnighan (1978) and Murnighan and Roth (1983) deviates from standard prisoner's dilemma experiments (like ours). See the discussion in Roth (1995).

[3]Recently, this literature has seen a substantial growth. Related to our research question, albeit less relevant, are the following findings. Gonzales et al. (2005) have suggested a new termination method. In public-goods experiments, they contrast a known finite horizon with various treatments where the termination period is only given by an interval. They find that asymmetric information about this interval reduces end-game effects but replacing a definite endpoint by a commonly or privately known symmetric interval does not have a significant impact on overall cooperation. Bruttel, Güth and Kamecke (2011) analyze prisoner's dilemma settings where information about the horizon of the game is not common knowledge and where cooperation can be part of a subgame perfect equilibrium as a result. Finally, Bruttel et al. (2011) investigate finitely repeated games with and without multiple equilibria of the stage game. They find that the nature of the additional equilibrium matters (strict versus non-

Our research extends these findings by analyzing all three termination rules for repeated games within a unified framework. Our main treatments focus on a setting that is frequently applied in social dilemma experiments—a design with "many" periods (at least 22) and where the supergame is *not* repeated. For such a setting, we compare the impact of the termination rules on cooperation in a prisoner's dilemma. We employ the finite and known horizon, the unknown horizon, and the random-stopping rule is analyzed with a high and with a low continuation probability (where cooperation can occur in a subgame-perfect equilibrium only with the high probability). We check for the robustness of these results with additional treatments which have an (expected) length of only five and ten periods.

Our findings are that the termination rules do not significantly affect average cooperation rates, but they may influence cooperation over time and end-game behavior. Further, the (expected) length of the game significantly increases cooperation rates.

2 Theory and Experimental Design

The stage game underlying our cooperation experiments is the prisoner's dilemma in Table 1. This is a standard two-player prisoner's dilemma with $S_i = \{$*defect, cooperate*$\}$, $i=1, 2$, as strategy sets (in the experiment, a neutral labeling for the strategies was used). The static Nash equilibrium of the game in Table 1 is $\{$*defect, defect*$\}$.

	defect	*cooperate*
defect	350, 350	1000, 50
cooperate	50, 1000	800, 800

Table 1: The stage game

Our four treatments reflect the above discussion of termination rules. In treatment KNOWN, the end of the experiment was given to the participants simply by saying that the experiment would last for 22 periods. In treatment UNKNOWN, the length of the experiment (28 periods) was not mentioned to the participants and the instructions merely said that the experiment would last at least 22 periods. In RANDOMLOW, the instructions said that the experiment would last at least 22 periods, and then the experiment would continue with a probability of 1/6.[4] In treatment RANDOMHIGH, there were at least 22 periods

strict).

[4]Feinberg and Husted (1993) induce discounting in an alternative way. They have a random stopping rule but additionally they shrink gradually the payoffs in a reduced two-action Cournot game. Using experienced subjects, they find a quantitatively minor increase of cooperation with higher discount factors.

and then the experiment would continue with a probability of 5/6.[5] A copy of the instructions is contained in an appendix. In all four treatments, the matching of participants was fixed over the entire experiment. We have data from 15 pairs for each treatment.

Additionally, we ran three further treatments designed to test the impact of the length of the horizon of the game. We will report on the design of these treatments and the results in section 4 below.

The subgame perfect Nash equilibrium predictions for the treatments are as follows. The static Nash equilibrium, {*defect, defect*}, is also the unique subgame perfect Nash equilibrium of the finitely repeated game in treatment Known. In Unknown, we cannot control for subjects' prior on the termination of the experiment. The static Nash equilibrium may apply but possibly repeated-game arguments have bite as well. If we ignore Selten, Mikzewitz and Uhlich's (1997) argument, we can make predictions based on infinitely repeated games for the treatments with a random end. From Stahl (1991), {*cooperate, cooperate*} is a subgame perfect Nash equilibrium outcome of the infinitely repeated game if and only if the discount factor is larger than $4/13 \approx 0.31$. Cooperation among rational and risk neutral players may thus only emerge in RandomHigh. In RandomLow, the unique subgame perfect Nash equilibrium is {*defect, defect*}.

The experiments were conducted in the experimental laboratory at Royal Holloway College (University of London) and University College London using z-Tree (Fischbacher, 2007). In total, 182 students participated. Average payments were £7.20 or roughly $14. Sessions lasted about 45 minutes including time for reading the instructions.

3 Experimental Results

We start by looking at cooperation rates in the four main treatments. Table 2 reports the number of *cooperate* choices per pair. We refer to the first 22 periods, so the maximum is 44 *cooperate* choices.[6] Average cooperation rates are 44.2% in Known, 55.0% in Unknown, 55.2% in RandomLow and 59.1% in RandomHigh.

In all treatments, there is a large variation in cooperation rates across the fifteen pairs. Some pairs cooperate in all (or nearly all) periods, others virtually never.

[5] We control for the minimum number of periods rather than the expected number of periods across treatments because an analysis of the impact of termination rules requires that subjects play the same number of periods before the termination rule is triggered. In Unknown, we cannot control for the expected number of periods anyway.

[6] In four out of the six sessions with a random end, play stopped after period 22. The remaining two sessions had a length of 23 and 26 periods and occurred in RandomHigh.

Treatment							*cooperate* choices per pair									rate
KNOWN	44	43	42	42	36	22	13	11	10	9	7	6	3	3	1	44.2%
UNKNOWN	44	43	43	36	35	34	24	21	21	16	13	11	11	9	2	55.0%
RANDOMLOW	43	43	43	43	42	37	30	19	16	14	13	12	8	1	0	55.2%
RANDOMHIGH	44	44	44	43	41	33	32	31	21	17	13	10	7	6	2	59.1%

Table 2: Results by (ordered) pairs

In order to take the possible dependence of observations between paired players into account, we count each participating pair as one observation. Taking all four treatments into consideration, we find that these cooperation rates do not differ significantly (Kruskal-Wallis test, H=1.929, $d.f.$=3, p=0.587). The four distributions of *cooperate* choices do not differ according to a median test either (χ^2=1.268, $d.f.$=3, p=0.737). From this and further robustness checks[7], we conclude that

Result 1 *The termination rule does not significantly affect average cooperation rates.*

Figure 1 shows the time path of *cooperate* choices in the experiments. The time paths of the four treatments often overlap, so we show the data in two separate figures. All treatments start at a level of around fifteen *cooperate* choices (or 50%). KNOWN and UNKNOWN stay at this level until about period 12. After that, the number of *cooperate* choices declines in KNOWN but it increases in UNKNOWN. Treatments RANDOMHIGH and RANDOMLOW seem very similar although, until period 18, *cooperate* choices in RANDOMHIGH appear to be increasing whereas they stay at or above the initial level of 15 in RANDOMLOW. After that, cooperation drops in both RANDOM treatments. We find a negative and significant time trend in KNOWN ($p = 0.002$) and RANDOMLOW ($p = 0.013$) but no other significant time trend.[8]

Result 2 *There is a negative and significant time trend in treatments* KNOWN *and* RANDOMLOW.

[7]We can compare the four treatments pairwise even though the Kruskal-Wallis *omnibus* test is not significant, but Mann-Whitney U tests do not indicate significant differences between distributions. The p values, not corrected for multiple comparisons, range between 0.65 in RANDOMLOW vs RANDOMHIGH and 0.24 in KNOWN vs RANDOMHIGH. Neither do Kolmogorov-Smirnov tests indicate significant differences between the distributions (p values, not corrected for multiple comparisons, range between 0.55 in RANDOMLOW vs RANDOMHIGH and 0.35 in KNOWN vs RANDOMHIGH).

[8]Time trends are analyzed by calculating Spearman correlation coefficients of *cooperate* choices over time separately for each pair. A sign test using the fifteen (statistically independent) correlation coefficients for each treatment indicates whether the time trend is significant.

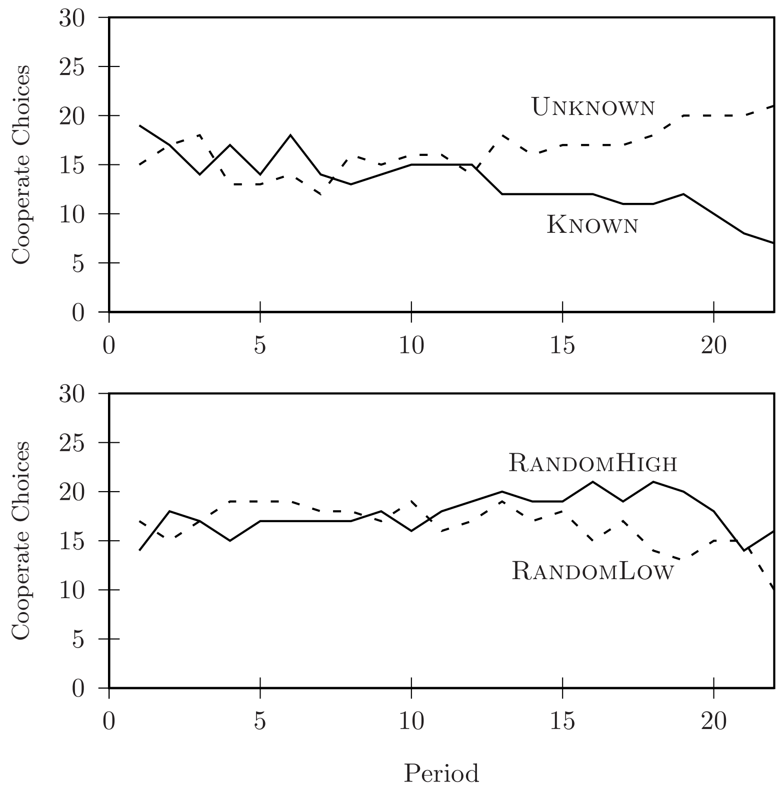

Figure 1: Cooperation over time

In all treatments but Unknown, cooperation declines as play approaches period 22. We follow a standard procedure to test for possible end-game effects. We compare cooperation rates in periods 10 to 19 to the average rate in periods 20 to 22 with a related-sample test and separately for all treatments. We find significantly lower cooperation rates in the last three periods in treatment Known (matched-pairs Wilcoxon, $Z = -3.06$, $p = 0.002$), and RandomLow ($Z = -2.14$, $p = 0.021$), and RandomHigh ($Z = -2.38$, $p = 0.017$). In Unknown, there is more cooperation in the last two periods. While this increase in not significant ($Z = -1.27$, $p = 0.203$), cooperation rates are higher beyond period 22 such that the cooperation rate over all periods is 58% Unknown. By contrast, in the one RandomHigh session with 26 periods, cooperation rates are 5 percentage points lower beyond period 22. We obtain virtually identical results when we compare periods 11 to 20 to the average rate in periods 21 to 22.

Result 3 *A significant end-game effect occurs in all treatments except* Unknown.

4 Treatments with a Shorter Horizon

There is one aspect of our design that, although common to cooperation experiments, could account for the results we found. With at least 22 periods, the length of our games may be said to be "long". In shorter games, the impact of the termination rule may be more significant. Thus, it seems useful to test the robustness of our results in additional treatments that vary the length of the prisoner's dilemma games. A richer set of treatments with different (expected) number of periods also allows to test whether the length of the horizon of the game *per se* has an impact on cooperation.

Specifically, we conducted two further treatments with a known finite horizon of five (Known5) and ten periods (Known10), respectively, and we also ran a treatment called Random5+5 in which there were at least five periods, after which the experiment would continue with a probability of 5/6. The expected number of periods was ten in Random5+5 which corresponds to the number of periods in Known10; and the minimum number of periods in Random5+5 corresponds to Known5. The game theoretic predictions for these treatments are the same as those derived above for Known and RandomHigh, respectively. In Known5, we had nine pairs participating and in both Known10 and Random5+5 eleven pairs participated.[9]

Treatment					*cooperate* percentage per pair							rate
Known5			100	50	40	40	20	20	20	20	0	34.4%
Known10	95	90	85	60	25	25	20	20	15	15	5	41.4%
Random5+5	100	100	80	80	50	30	20	20	10	0	0	44.5%

Table 3: Results by (ordered) pairs, cooperation rates in percent.

Table 3 shows the results. As the number of periods differs here, we report percentages rather than absolute numbers (average over 5 periods in Known5, 10 periods in Known10 and the first 5 periods in Random5+5). The cooperation rates in Known10 and Random5+5 (which have the same expected number of periods) are very similar in their averages, while Known5 exhibits a lower average. Testing for differences in cooperation with all treatments jointly does not suggest significant results (Kruskal-Wallis test, H=0.137, $d.f.$=2, p=0.853), nor do any pair-wise comparisons.

Cooperation rates in Known5 and Known10 drop to a level of 22% and 18%, respectively, in the last period. This confirms the end-game effect found above. The negative time trend observed in Result 3 can be confirmed only for

[9]Each of these treatments was played twice. The second round of repeated games was not announced and was conducted as a "surprise restart". Subjects were rematched after the first supergame. In the second run of the experiment, cooperation rates go up by a moderate but insignificant amount in all treatments (even if we ignore the possible dependence of observations across the two supergames). Importantly, the differences between treatments do not get bigger. We thus refrain from reporting further details of the second round.

Known10 ($p = 0.012$) but not for Known5 ($p = 0.754$). In Random5+5, cooperation rates are never below 36%, which does not confirm the end-game effect found above for RandomLow and RandomHigh. There is no negative time trend in Random5+5 ($p = 0.508$), although cooperation rates decrease to 40% after period 5 (there were 26 periods in total). As an aside, we note that, if we discard the data from the last period(s), cooperations rates would be even more similar between treatments.

Result 4 *The termination rule does not significantly affect average cooperation rates in treatments* Known5*,* Known10*,* Random5+5*.*

Finally, we analyze whether the (expected) number of periods has an impact on cooperation. To do this, we include all treatments (except for Unknown, where we cannot control for subjects' beliefs about the length of the game[10]) and we use the expected number of periods (as opposed to the actual realization) in each treatment. It turns out the length of the game matters. In support of the hypothesis that a longer horizon leads to more cooperation, we can reject that the data come from the same distribution using a Jonckheere-Terpstra test (J-T statistic = 1.896, $p = 0.029$).[11]

Result 5 *The length of the horizon of the game significantly increases cooperation rates.*

Treatment	Known5	Known10	Random5+5	Known	RandomLow	RandomHigh
Exp. length	5	10	10	22	22.2	27
Coop. rate	34.4%	41.4%	44.5%	44.2%	55.2%	59.0%

Table 4: Expected length of the game and cooperation

5 Conclusion

In this paper, we analyze three termination rules for repeated-game prisoner's dilemmas. We find that the termination rule does not have a significant effect on average cooperation rates. Employing a random termination rule does not cause significantly more cooperation compared to a known finite horizon. Comparing the random termination rule with a low and a high continuation probability,

[10] If we include the data from Unknown with the actual game length (which was not known to subjects), the below result still hold.

[11] The Jonckheere-Terpstra test is a non-parametric test for more than two independent samples, like the Kruskal-Wallis test. Unlike Kruskal-Wallis, Jonckheere-Terpstra tests for ordered differences between treatments and thus requires an ordinal ranking of the test variable. See, e.g., Hollander and Wolfe (1999).

we find that the continuation probability does not significantly affect average cooperation rates either, as did Murnighan and Roth (1983).

In treatments with a known finite horizon, there is an end-game effect with cooperation rates dropping as the experiment approaches the minimum possible number of periods for the experimental duration. An end-game effect also occurred with the random termination rule in our treatments with at least 22 periods but not with the shorter horizon. Cooperation over time is also affected by the termination rule. A known finite horizon and a random stopping rule with a low continuation probability exhibit a negative time trend. We also find that the length of the game does affect cooperation rates significantly. This is consistent with Dal Bo (2005) where this result occurred for both finitely repeated games and in games with a random stopping rule (although, in his setup, this can be rationalized with standard theory). Morehous (1966) and Bruttel et al. (2009) also find that longer horizons promote cooperation.

Dal Bo (2005) found that the continuation probability of the random-stopping rule matters. In addition to differences in the base game, there are two differences to our setting: Dal Bo's (2005) games were shorter (the expected length of the games were one, two and four, respectively) and the supergames were repeated ten times. It is difficult to assess to what extent the two differences account for differences in the results. However, it appears that treatment differences in Dal Bo's (2005) early supergames were not as pronounced as they were in the later during the experiment, and most of the learning seems to occur in the first two or three supergames. Thus it seems that subjects do need at least some learning opportunities before significant backward induction arguments in finitely repeated game have force. A shorter horizon may support the learning process. In such settings, the "shadow of the future" matters—whereas it does not in our longer experiments that were not repeated many times.

The conclusion that subjects learn about the finiteness of the game and that thus cooperation rates drop if the supergame repeated is consistent with the possibility that, when subjects read in the instructions that the horizon is finite, this may not imply that the finite horizon is really common knowledge. If so, even rational and selfish players may cooperate with finitely many periods (Samuelson 1987; Neymann 1999; Bruttel, Güth and Kamecke, 2011), as mentioned above. With repetitions of the supergame, eventually the finite duration will become common knowledge and unraveling of cooperation may occur. But this learning process may be slow if there are, in addition, subjects with other-regarding preferences that try to cooperate even when the game is commonly known to be finite.

Appendix

Here are the instructions for the RandomHigh treatment. The instructions for the other treatments are similar and are available from the authors.

Experimental Instructions

Welcome to the experiment! Take the time to read carefully the instructions. A good understanding of the instructions and well thought out decisions during the experiment can earn you a considerable amount of money. All earnings from the experiment will be paid to you in cash at the end of the experiment. If you do have any questions, please raise your hand and one of the co-ordinators will come to you and answer it privately. Please do not talk to anyone during the experiment.

You are participating in an experiment in which you interact with one other participant. The person with whom you interact is always the same.

At the beginning of the experiment, each participant is assigned one of two roles, either A or B. Everybody keeps his or her role throughout the entire experiment. Before the experiment starts, each participant with role A is randomly matched with a participant with role B. This matching is then maintained throughout the entire experiment.

The experiment is repeated for at least 22 rounds. After the 22nd round (and each subsequent round), a dice roll decides whether the experiment continues or not. The experiment terminates if a 6 is thrown and continues otherwise. The computer throws the dice.

Each round is the same.

During each round, the following happens: A has to choose between Left and Right and B has to choose between Up and Down. Decisions are made without the knowledge of the other participants decision (i.e. they are made simultaneously). The payoffs are then given in the following table:

		A's decision	
		Left	Right
B's decision	Up	A: 350, B: 350	A: 50, B: 1000
	Down	A: 1000, B: 50	A: 800, B: 800

So, for example, if A chooses Left and B chooses Down, A receives a payoff of 1000 points and B receives a payoff of 50 points.

After both A and B have made a decision, both participants are told what happened in the round, being informed of each participants decision and the payoffs to each participant. In addition participants will see their own points total so far.

Your total earnings from the experiment will be £1 for each 2000 points you get during the rounds of the experiment.

At the end of the experiment you will be paid your total earnings in cash and asked to sign a receipt. You will also be asked to fill in a short online questionnaire.

You have role A

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