Equilibrium Cooperation in Three-Person, Choice-of-Partner Games

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Abstract

The experiment involves three-person games in which one player can choose which of two others to "do business with." The ability of the chooser to switch away from a defecting partner in a subsequent stage constitutes a salient action that may be used to punish static, noncooperative play. In this manner, cooperative outcomes that are not Nash equilibria in stage games can be supported by noncooperative behavior in a multi-stage game. As predicted by theory, cooperation rates are low in baseline single-stage games. Cooperation rates, however, are much higher in a ten-stage version of the game.

1. Introduction

Many policy debates turn on assumptions about the extent to which economic agents behave cooperatively. The desirability of challenging a horizontal merger, for example, depends largely on the likelihood of a post-merger conspiracy. Similarly, when product quality is unobserved prior to purchase, consumer-protection policies may be unnecessary if consumers and producers cooperate to avoid "lemons market" outcomes. In many specific situations of this type, the dominant strategy in a static sense involves noncooperation. With asymmetric information about product quality, for example, sellers may have unilateral incentives to "defect" and provide inefficiently low levels of quality in a single, non-repeated market encounter, even though the delivery of high quality at high prices may Pareto dominate the low-price/low-quality outcome.

When agents interact in a sequence of periods or stages, threats of subsequent punishments can generate dynamic equilibria with *cooperative outcomes*, or outcomes that Pareto dominate those determined by single-stage Nash equilibria. For example, an equilibrium for a game with 2 stages can yield a cooperative outcome in the first stage if the strategies incorporate

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threats that a departure from cooperative behavior in the first period will be followed by play that alters the second-period outcome in a way that punishes the defector.[2]

Following the original work of Friedman (1971), Green and Porter (1984) and others, there is considerable interest among theorists in specifying conditions in dynamic games under which equilibria involving cooperative outcomes will be selected from the set of equilibria.[3] Insights gained from laboratory data may assist in this process, by helping to predict when cooperative equilibrium outcomes will actually be observed. One clear primary result from laboratory investigation is that even extensive repetition of a stage game (with a probabilistic stopping rule) is not enough to ensure a cooperative outcome. Palfrey and Rosenthal (1992), for example, find that repetition does little to enhance the incidence of cooperative outcomes in a three-person public-goods game. This finding defies the conventional wisdom regarding the incidence of cooperation, and, as the authors remark, is "not encouraging news for those who might wish to interpret as gospel the oftspoken suggestion that repeated play with discount rates close to 1 leads to more cooperative behavior" (Palfrey and Rosenthal, 1992, p 4).

But noncooperative outcomes should not be regarded as a foregone conclusion, since factors other than the mere existence of equilibria with cooperative outcomes may be important. A variety of individual and market characteristics may affect the propensity to cooperate, by changing the structure of the relevant game. In particular, incidence of cooperation may be affected via alterations in potentially important properties of threat outcomes. Consider, for example, a symmetric, two-person game with two stages: the first stage is a prisoner's dilemma, and the second is a 2x2 game with two, Pareto-ranked Nash equilibria. Suppose further that the incentive for each player to defect unilaterally in the first stage is smaller than the reduction in payoffs that occurs when the outcome in the second stage is switched from the dominant to the dominated equilibrium outcome in the second stage. Then cooperative play in the first-stage

[2] Here, "defection" indicates a departure from cooperative behavior. In contrast, a "deviation" will indicate a departure from an equilibrium strategy. If the equilibrium generates a cooperative outcome, then a defection is also a deviation.

[3] The work cited in the text, which largely pertains to infinite-horizon, oligopoly applications, has been significantly generalized by Abreu (1986) and others. Van Damme (1983) and Benoit and Krishna (1985) discuss equilibrium cooperation in games with finite horizons. It is possible to induce an infinite horizon in a laboratory experiment, by using a probabilistic stopping rule, e.g., Roth and Murnighan (1978).

prisoner's dilemma can occur in a sequential equilibrium in which the players' strategies specify a switch from the Pareto-dominant to the Pareto-dominated equilibrium in the event of a defection in the first stage. Equilibrium cooperation can exist in this environment, even with a short, finite horizon.

Initial laboratory investigation in this type of environment by Davis and Holt (1992) has not consistently generated high rates of cooperation.[4] One problem is that the coordination of actions needed to achieve an equilibrium punishment does not reliably evolve, even with repeated rematchings of the subjects in a series of distinct pairings. In particular, the attempt to punish may backfire if the defector does not "accept" the punishment.[5]

But other types of punishment structures are possible. Many interesting economic situations differ from the case just described in the sense that (equilibrium) punishment does not involve the need for coordination. For example, a buyer who is dissatisfied with the quality delivered by one seller can unilaterally switch to another. A switch of this type is clearly interpreted as a punishment, and its effectiveness is not contingent on acceptance by the defector. Moreover, the ability to switch to a new partner, leaving the other with reduced earnings, is a more salient threat than the types of threats identified with play in the two-stage games discussed above. Ultimately, the effect of introducing a clear, salient punishment opportunity is an empirical issue. A laboratory experiment, where the underlying incentive structure of the game is induced (and hence known) represents an ideal environment for isolating these effects.

This paper reports the results of an experiment designed to assess the extent of cooperative behavior in multi-stage, three-person games in which there is one subject who must choose which of the others to deal with in the current stage. Section 2 contains a more detailed description of three variations of this switching game. Experimental procedures and results are

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[4] Although Davis and Holt do observe some equilibrium cooperation in two-stage games that are similar to the game described in the text, this cooperation is not as prevalent as had been expected. In the most-replicated design, the addition of a Pareto-undominated punishment to the second stage game increased cooperation in the first stage from about 12% of the time to about 30% of the time. Baseline cooperation rates improved with experience, and were higher in some trials using other designs. Overall, however, the increase in cooperation rates caused by the addition of a Pareto-undominated punishment averaged about 25 percentage points.

[5] In the Davis and Holt (1992) experiments, only about half of the players who defected in the first stage accepted the punishment by making the second-stage decision that would lead to the Pareto-dominated equilibrium in that stage.

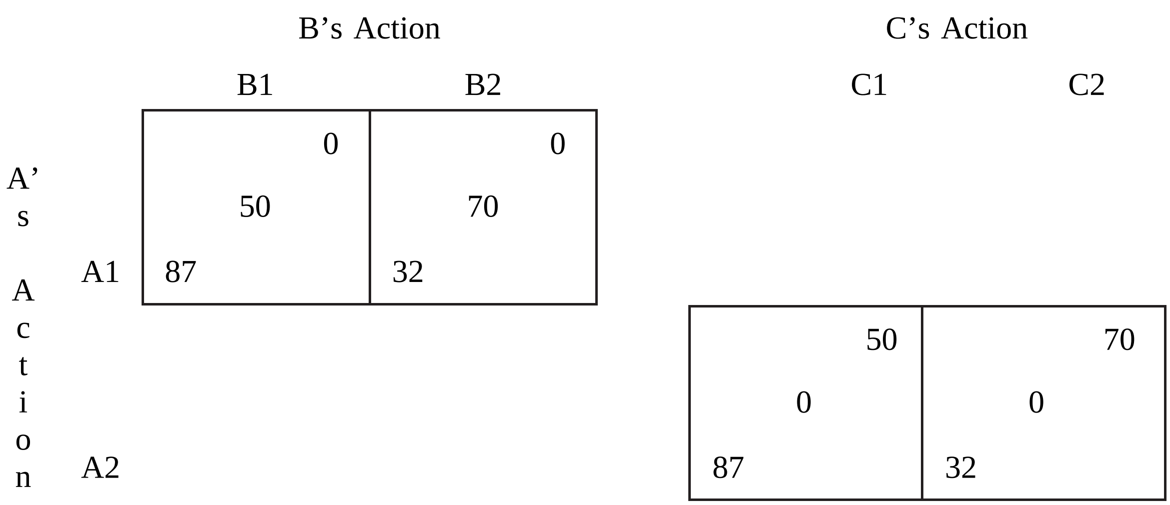
described in sections 3 and 4 respectively. The final section is a conclusion.

2. An Opponent Selection Game

Consider the game in figure 1, in which players A, B, and C receive, respectively, the lower-left, middle, and upper-right payoffs in each box. Decisions are made simultaneously by all three players. In this game, A chooses whether to have his/her earnings determined by B (in which case C earns 0) or by C (in which case B earns 0). Suppose that player A makes decision A1, thereby selecting B. Since decisions are simultaneous, B receives 70 points for having picked B2 and 50 points for having picked B1. Notably, the 20 points B gains by choosing B2 reduce A's earnings by 55 points. Player C's incentives are identical to B's.[6]

Figure 1. A Three Person Stage Game that is Repeated

Key: In each stage, player i plays i1 or i2, i = A, B, C.



In a one-stage version of this game, B2 and C2 are dominant strategies, and A should be indifferent as to which player to select. But suppose that the stage game in figure 1 is played twice, with all players observing their own (but not others') payoffs for the first play, prior to

6 Notice that figure 1 is a two-dimensional simplification of the normal form of the 3-person game. Since there are 8 possible outcomes, the normal form would typically be shown with 8 boxes, perhaps in a 3-dimensional array.

the second and final play.[7] In this two-stage game, a "cooperative" play of either B1 or C1 in the first stage can be part of an equilibrium strategy: player A chooses to deal with B (or C) in the first stage and only switches after a defection. For example, the strategy for player A may be to select B (decision A1) in the first stage and to stay with player B unless B2 is observed. The respective strategies for players B and C are to choose B1 and C1 in the first stage and to choose B2 and C2 in the second.[8] In this case, the gain of 20 that B could realize by defecting in the first stage is more than offset by the cost, 70, of not being selected in the final stage. This sequential Nash equilibrium is weak since A's second-stage payoff would not be any lower if the threat to switch following a defection were not carried out.

When the stage game is repeated more than once, a switch-to-punish and stay-to-reward strategy can be communicated more effectively. In a 3-stage game, for example, suppose B plays a straight defect strategy. Then a switch from B to C would increase A's expected payoffs if C cooperates in the second-stage with any positive probability. Multiple-stage variants of this game have the advantage that B and C can see whether A will punish defection in early stages. In this way cooperative outcomes might develop as a consequence of an A participant developing a reputation for employing a punish/reward strategy. Notice also that repetition of the stage-game increases the costs of defection. For example, B stands to lose twice as much for a first-stage defection in a 3-stage game (with 2 periods of punishment) as would be the case in a 2-stage game (with 1 period of punishment).[9]

This 3-person game has a very natural economic interpretation. Players B and C may be viewed as sellers of a product with a quality that cannot be determined until after the sale.

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[7] Thus, this repeated game is one of less than perfect information. For example if A chooses B, then neither A nor B learn C's choice. Moreover, C does not learn B's choice. Information is restricted in this way to parallel the information available to buyers and sellers in a "lemons market" game, discussed below.

[8] Obviously, the punishment strategy would also induce player C to cooperate if selected in the first stage. There are other, more asymmetric equilibria with the same cooperative outcome but in which the player not selected (B or C) defects in both stages and the one selected by player A cooperates in the first stage.

[9] In a 3-stage game, B stands to lose 100 as a punishment for defecting in the first stage, rather than 50 in the 2-stage game. In a 2-stage game, B gains 20 by defecting in the first stage (e.g., earns 70 rather than 50), but loses 70 by not being selected in the second stage. Player B's net loss is thus 50. The net loss increases to 100 in the 3-stage game, since B again gains 20 from defection in the first stage, but may lose 120: 50 from not being selected in the second stage, and 70 from not being selected in the final stage.

Player A is a buyer, and the seller selected by A can either deliver a high-cost, high-quality unit (generating a profit of 50), or deliver a low-cost, low-quality unit (generating a profit of 70). If the buyer makes repeated purchases, the delivery of high quality may be optimal for players B and C, because player A may punish low quality in one market period by shopping elsewhere in the next. Importantly, actions by the buyer can have a punishment effect, even if the buyer does not explicitly have strategic play in mind. For example, suppose that the buyer believes that there is some positive probability that at least one of the two sellers is an altruist. Following the realization of low quality in one period, the buyer might switch simply to see if the other is an altruist. Since the switch is costless, only a very low prior probability of altruism is necessary to motivate such a switch. The coincidence of strategic and non-strategic motivations for switching is an advantage of this game, for this is a context where the structure of available actions facilitates the delivery of punishments: regardless of the buyer's intent, the effect of switching is equally salient to a seller delivering low quality.[10]

3. Procedures

We conducted a series of 9 **sessions**; each session involved a separate group of 9 subjects who were anonymously matched in groups of 3 on a network of personal computers. Every session consisted of a series of 30 **stages** in which subjects made simultaneous decisions. Payoffs were determined by the stage game in figure 1. The payoff structure was presented to subjects in the format of figure 1, with no reference to the buyer/seller, high-quality/low-quality interpretation. In each session, each subject changed roles frequently, and made a total of 10 decisions as a type-A participant, 10 as a type-B participant, and 10 as a type-C participant.

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[10] Strategic and non-strategic behavior are often difficult to distinguish in multi-person games. Consider for example, an infinitely repeated n-person prisoner's dilemma. Following a defection by at least one player in a given stage of the game, it is impossible to determine whether a play of the stage-game Nash equilibrium strategy by another player in a subsequent stage is a punishment, a defection, or simply an effort to maximize minimum earnings (in the expectation that everyone else will defect). The difference between the n-person prisoner's dilemma and the seller-selection game is that a misinterpretation of strategic play as nonstrategic tends to stimulate noncooperative outcomes in the prisoner's dilemma, while such a misinterpretation in the seller-selection game may stimulate cooperation. This intuition suggests why it is reasonable for some economists to expect largely noncooperative behavior in some contexts (such as substantial free-riding in a public goods game, or little implicit conspiracy in an oligopoly), and simultaneously expect rather high rates of cooperation in others (such as high quality delivery in a quality-choice game with asymmetric information), even though cooperation may be supported by strategic play in each context.

The sessions are distinguished by **treatment**, i.e., the number of consecutive stages in the game. Three of the nine sessions involved single-stage games in which groupings were shuffled and participant roles were rotated after each play of the stage game in figure 1. In a second set of 3 sessions, participants remained paired and assumed the same role for two consecutive stages. In the final set of 3 sessions, participants remained matched with the same participants and assumed the same identities for 10 consecutive stages. To summarize, there were 9 sessions with 9 subjects each; 3 sessions involved 30 single-stage games, 3 sessions involved 15 two-stage games, and 3 sessions involved 3 ten-stage games.

The nature and timing of the rematching was explained to the subjects, but identification numbers and roles were changed in a manner that prevented anyone from knowing when or in what role they had encountered the two subjects with whom they were currently matched.[11] No subject was ever matched with the same pair of other subjects in the same roles.

Single-stage games were conducted as a control treatment, to evaluate the extent to which B and C play the cooperative option in the absence of punishments, out of altruism or fairness concerns.[12] The 2-stage games were conducted to assess the effects of the salient punishment that does not involve coordination with the subject being punished. The relatively long, 10-stage games were conducted to assess the reputation effects made possible when participants remained paired for many consecutive stages.

Participants were undergraduate business students at Virginia Commonwealth University, recruited from introductory economics and accounting courses. Participants were all inexperienced in the sense that they had not previously participated a session with these 3-person

[11] A copy of the roll and group assignment tables and a printout of the instructions are available from the authors on request.

[12] Cooper *et al*. (1991) observed some cooperative play in which subjects were anonymously rematched in a series of single-stage prisoner's dilemma games. They attribute this cooperation to altruism. In particular, the authors conduct an experiment to show that the cooperation is not due to a "contagion effect" in which subjects behave cooperatively in the hope that cooperation will spread throughout the group, which could be ultimately beneficial to them. A change in their matching procedure that eliminated the possibility of such contagion had no significant effect on observed cooperation levels.

games.[13] They were paid $3.00 for meeting their appointment, plus their earnings in the course of the session. Individual earnings ranged between $10.00 and $22.00 per session, which lasted approximately one hour. Payments were made in cash immediately after each session. A bank of networked personal computers was used to present interactive instructions, to control the matching process, and to record decisions.[14] Software was written in turbo-PASCAL by Davis.

4. Results

Cooperation Rates. Our initial analysis focuses on observed cooperation rates, or the proportion of times that the player selected by A chose the cooperative (B1 or C1) decision.[15] Our particular interest is in cooperation rates for non-terminal stages, where cooperation can be part of a Nash equilibrium strategy. In the sessions with 2-stage games, this would be the initial stage of each matching, while in the 10-stage games the relevant proportion is based on cooperation rates in the first 9 stages of each matching (recall that cooperation is not part of an equilibrium strategy in terminal game stages).

Average cooperation rates for the 9 sessions are listed in table I. In the table, sessions are identified by a 2-part code in the left-hand column. The first part (1s, 2s, or 10s) indicates the number of stages between rematchings, while the last character indicates the session number (identified in chronological order). For example, 2s-3 indicates the third session involving 2-stage games.

Cooperation rates for nonterminal stages of sessions with 2-stage and 10-stage games are listed as bolded numbers in the middle column. Cooperation rates for terminal stages, as well

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[13] Some participants, however, had previously participated in other economics experiments involving 2-person games.

[14] However, rather than having participants read through interactive instructions at their own pace, we read the instructions aloud while subjects followed on their screens, using the keyboards as necessary. In pilot tests of software created for an unrelated experiment, we found that reading instructions aloud increased comprehension. Reading instructions also creates more common knowledge, i.e. an assurance that everybody is playing by the same rules. We decided that these advantages outweighed the risk that the person reading the instructions could somehow bias the results with a suggestive tone of voice.

[15] As noted above, in footnote 7, there are equilibria with cooperative outcomes in which the player not selected defects in all stages. Therefore, the cooperation rate is calculated on the basis of decisions made by the subject actually selected.

Table I. Cooperation Rates
Key: **Non-Terminal Periods**, Terminal Periods[a]

Session Identifier	Cooperation Rate by Session (average of all individuals in a session)	Cooperation Rate by Treatment (average of all individuals in 3 sessions)
1s-1	.33[a]	
1s-2	.20[a]	
1s-3	.26[a]	.26[a]
2s-1	**.49**, .35[a]	
2s-2	**.09**, .13[a]	
2s-3	**.18**, .24[a]	**.25**, .24[a]
10s-1	**.63**, .22[a]	
10s-2	**.73**, .89[a]	
10s-3	**.53**, .44[a]	**.63**, .52[a]

[a] Provided for reference. Cooperation is not part of an equilibrium strategy in single-stage games, or in the terminal stages of multi-stage games.

as well as cooperation rates in sessions with 1-stage games are also provided in the middle column, for reference. Bolded or unbolded, each entry represents the average of the cooperation rates realized by each of the 9 participants in a session when they were a type-A player. The rightmost column in table I presents overall cooperation rates, pooled across all individuals in the 3 sessions in each treatment. Entries in rightmost column are formatted as in the middle column.

Inspection of the mean cooperation rates for each session, in the middle column of table

I, reveals some heterogeneity across sessions within treatments, particularly in the repeated games. But despite this heterogeneity, the overall mean cooperation rates listed in the rightmost column of table I reveal a dramatic treatment effect. Starting at the top of the rightmost column, the mean cooperation rate of .26 in the 1-stage games suggests some basic concern for altruism or fairness.[16] The addition of a second stage did nothing to increase the first-stage cooperation rate, as evidenced by the slightly lower .25 mean cooperation rate for the 2-stage games. In the 10-stage games, however, the mean rate of cooperation in nonterminal stages increased substantially, to .63.

The improvement in cooperation rates in the 10-stage games over either the 1-stage or the 2-stage games is statistically significant. A Mann-Whitney rank-sum test of the null hypothesis that the mean cooperation rate is no higher in the 10-stage games than in either the 1-stage games, or in the 2-stage games can be rejected at a confidence level of 95%.[17] The improvement in cooperation rates in the 10-stage game over the 2-stage and 1-stage games is our first conclusion: *Although repeating the stage game twice does not increase cooperation rates, cooperation rates are substantially increased when the stage game is repeated 10 times*.

Switching Behavior. The sizable difference in cooperation rates across treatments raises the question of whether switching/staying choices by the type-A players motivated the difference. Table II presents information regarding switching and reselection decisions. Each row in table II reports the data for a given treatment, by session. The proportions on the left side of the table

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16 Given the simplicity of the game, we found the incidence of disequilibrium cooperative outcomes in the 1-stage games somewhat surprising. These results, however, are consistent with those analyzed by Cooper, *et al.* (1991), who observe about 20% cooperative choices after subjects have gained experience in 1-stage prisoner's dilemma games with different partners. Note also that while the sample size is to small to make any statistical claims, cooperation rates in sessions with 10-stage games appear to be somewhat higher than in either the 1-stage games, or in the terminal stages of the 2-stage games, suggesting that repetition of the stage game may generate some residual-goodwill or "warm glow" effect that dominates subgame perfect equilibrium play.

17 Since individual experiences within a session are not likely to be independent, we prefer to use the session as the unit of observation. The intuition behind the 95% confidence level is that of the $\binom{6}{3} = 20$ possible rankings of two 3-session treatments that could have occurred, the most extreme outcome was observed: all 3 10-stage cooperation rates exceed the 3 cooperation rates of the comparison sessions (2-stage and 1-stage). If cooperation rates are in fact drawn from the same distribution (as is assumed under the null hypothesis), the probability of an outcome this extreme is 1/20 = .05. We also conducted parallel Mann-Whitney tests using the individual as a unit of observation (See Siegel and Castellan, 1988, pp. 128-137). The relevant test statistics (in unit-normal form) for pair-wise comparisons of the 10-stage/1-stage and 1-stage/2-stage games, are 4.71 and 4.35, respectively. These statistics allow rejection of the null hypothesis of no treatment effect at a confidence level in excess of 99%.

are the averages of individual propensities for type-A participants to reward cooperation by reselecting that participant in a subsequent period. Similarly, the proportions on right side of table II are the averages of individual propensities for type-A participants to punish defection by a B or C participant by switching away from that participant in a subsequent stage. Each session entry represents an average of all individual responses in a session, while the columns labelled "Summary" report an overall average of responses for all individuals that participated in the 3 sessions of a treatment.[18] Neither switching nor reselecting a player is possible in the 1-stage games, and the numbers provided for purposes of reference. The (fictitious) switch/stay rates in the 1-stage games are calculated by comparing the choice of a subject in the role of type-A with the subject's choice in the most recent game in which the subject was a type-A.

Again, despite the heterogeneity across sessions within treatments in table II, comparison of mean punish and reward rates across treatments allows some interesting observations. Notice first the reference punish and reward propensities for the 1-stage sessions. If the decisions of A-type participants were random (and in particular, independent of other's choices in previous games), "punish" and "reward" rates of .5 would be observed. But the .59 and .39 values listed in the summary columns for the 1-stage treatment indicate that responses are not random. Rather, there is something of a "lever bias." When indifferent, participants tend to repeat decisions (reselect) about 60% of the time. Inspection of reward and punish propensities across treatments in the second and third rows of table II indicates that this lever bias persists to some extent in the repeated games: In each case, the tendency to remain with a cooperator exceeds the tendency to switch away from a defector (.77 vs. .63 in the 2 stage games, and .77 vs. .65 in the 10 stage games).

Nevertheless, this lever bias is dominated by a treatment effect in each of the repeated games: Comparing across rows on the left side of in table II, it is seen that the tendency to reselect a cooperator increases over the baseline 1-stage treatment by 18 percentage points (from .59 to .77) in both the 2-stage and the 10-stage games. Despite these aggregate differences,

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[18] In some sessions, some type-A participants had no opportunity to make a relevant response. For example, 10 of the 27 participants in the 2-stage trials never received a cooperative outcome, and therefore were unable to "reward" cooperation. Reported session and treatment proportions are average decisions for those who actually had a response opportunity. Thus, for example, the reported "reward" propensity for the 2-stage trials is based on 17 observations.

Table II. Reward and Punish Behavior

Treatment	Reward Behavior (Average Propensity for A to Reselect B or C Following a Cooperative Play)				Punish Behavior (Average Propensity for A to Switch Away from B or C Following a Defection)			
	Session Average			Summary[a]	Session Average			Summary[a]
	1	2	3	Sessions 1-3	1	2	3	Sessions 1-3
1 Stage	.77[b]	.68[b]	.32[b]	.59[b]	.20[b]	.52[b]	.47[b]	.39[b]
2 Stage	.86	.44	1.00	.77	.71	.57	.53	.63
10 Stage	.77	.62	.91	.77	.71	.60	.65	.65

[a] Average of individual reselection or switch rates pooled over all sessions in a treatment.

[b] Provided for reference only. The (fictitious) reselection and switch rates in the 1-stage games are calculated by comparing the choice of a subject in the role of type-A with the subject's choice in the most recent game in which the subject was a type-A.

application of the Mann-Whitney test to the session averages does not allow rejection of the null hypothesis of no increase in reward propensities at conventional significance levels.[19] But punishment propensities are even more pronounced. As seen on the right side of table II, the overall incidence of "punish" behavior increases by 24 percentage points in the 2-stage games over the 1-stage games (from .39 to .63), and by 26 percentage points in the 10-stage games over the 1-stage games (from .39 to .65). Using a Mann-Whitney rank-sum test, these differences in

[19] The null hypothesis of no treatment effect cannot be rejected even if individual reward propensities are used as the unit of observation. Relevant Mann-Whitney test statistics (in unit-normal form) are 1.58 and 1.40, for the 2-stage/1-stage and for the 10-stage/1-stage comparisons, respectively.

punish propensities are significant at a 95% confidence level (direction not predicted).[20] These ratios motivate our second conclusion: *When the game is repeated even once, type-A participants find it very natural to reward cooperation, and in particular, to punish defection by switching. Increasing the length of the game does not appear to affect the tendency of type-A participants to engage in punish/switching behavior.*

Switching Behavior and Cooperation Rates in 10-stage games. The difference in cooperation rates across 2-stage and 10-stage treatments, coupled with virtually identical punish/reward rates across those same treatments, indicates that switching behavior does not alone ensure high levels of cooperation. Rather cooperation is motivated a combination of switching behavior and the effect of increasing the number of stages in the game.

To gain some insight about the dynamics of cooperation, it is instructive to consider data from some specific 10-stage games. Figure 2 summarizes outcomes for the 9 participants who made decisions as type-A players in the final 10-stage sequence of each session. (Thus, the figure presents the final third of the 10-stage game outcomes.) In the figure, both the experiment identifier and a type-A participant identifier are presented on the left side of each row. For example, the "10s-1 #1," listing in the top row of figure 2 indicates that this row is a record of outcomes for the type-A participant #1 in the final 10-stage sequence of trial 10s-1. Moving rightward within each row, the 10 stages in each 3-person group are enumerated horizontally. Player A’s selection of A1 or A2 determines whether the B or C row is relevant, and the "c" and "d" letters represent the "cooperate" (B1 or C1) and "defect" (B2 or C2) choice by the selected player. Player A’s behavior is consistent with a punish/reward strategy if A repeats a "purchase" from a player who cooperates, and switches from a player who defects.

Inspection of these data suggest that the increase in cooperation may be explained in part by the increased opportunity to establish a reputation as a player who rewards cooperation and punishes defection. For example the type-A player in 10s-1 #3 switched in response to

[20] These tests use the session as the unit of observation. The intuition behind the 95% confidence level is identical to that explained for tests regarding treatment effects in cooperation rates. See footnote 16. Using the individual as the unit of observation (and implicitly assuming individual observations are independent) generates very similar results: Mann-Whitney test statistics (in unit-normal form) of the null hypothesis of no treatment effect are 2.67 for the 2-stage/1-stage comparison, and 3.05 for the 10-stage/1-stage comparison. These statistics allow rejection of the null hypothesis of no treatment effect at a confidence level in excess of 95%.

Figure 2. Performance in the final 10 periods of each session composed of 10-stage games

10s-1 #1	Period	1	2	3	4	5	6	7	8	9	10
A's Choice:	B	c	c	d			c	c	d	d	
	C				d	d					d

10s-1 #2	Period	1	2	3	4	5	6	7	8	9	10
A's Choice:	B	c	c	c	c	c	c	c	c	c	d
	C										

10s-1 #3	Period	1	2	3	4	5	6	7	8	9	10
A's Choice:	B	c	c	c	c	d		c	c	c	d
	C						d				

10s-2 #1	Period	1	2	3	4	5	6	7	8	9	10
A's Choice:	B	c	c	d	c	c	c	c	c	c	c
	C										

10s-2 #2	Period	1	2	3	4	5	6	7	8	9	10
A's Choice:	B	d				c		c		c	d
	C		c	c	c		c		d		

10s-2 #3	Period	1	2	3	4	5	6	7	8	9	10
A's Choice:	B	c	c			c		d			
	C			c	c		c		c	c	c

10s-3 #1	Period	1	2	3	4	5	6	7	8	9	10
A's Choice:	B	c	d								
	C			c	c	c	c	c	c	c	d

10s-3 #2	Period	1	2	3	4	5	6	7	8	9	10
A's Choice:	B			d				d	d	d	
	C	d	d		c	d	d				c

10s-3 #3	Period	1	2	3	4	5	6	7	8	9	10
A's Choice:	B			c	c		c			c	
	C	c	d			c		c	d		d

defections by both B and C, and managed to realize a cooperative outcome in each of the remaining periods except the last (where cooperation is not part of an equilibrium strategy). Conversely, the type-A player in 10s-3 #2 punished defection rather inconsistently, "rewarding" defection by reselecting a seller who delivered low quality in 4 of 8 instances. This player received cooperative outcomes in only 1 of the 9 instances where cooperation is part of an equilibrium strategy.

But the results of other games suggest that a reputation as a punisher/rewarder is not the only explanation for high cooperation rates. In 10s-2 #1, for example, the type-B participant continued to deliver high quality following a defection in stage 3, even though the type-A participant did not punish the defection. Similarly, high rates of cooperation are observed in sessions 10s-2 #2, 10s-2 #3 and 10s-3 #3, despite rather inconsistent reward behavior by the type-A participant. One possible motivation for the high-quality delivery in these sessions is the increased costs to type-B and type-C players of a switch by a type-A in the early stages of a 10-stage game. For example, a type-B or type-C player who delivers low quality in the first stage of the 10-stage game stands to lose $4.70 if the type-A player "shops" elsewhere (50 cents per period for periods 2 - 9, and 70 cents in period 10). This maximum potential loss drops by 50 cents per period, but still dwarfs the 20 cent gain from defection until the final stages. Thus, even if the type-A player turns out to be less than entirely consistent in the game, type-B or type-C players have a stronger incentive to make a "c" decision in a game with many stages. These alternative motivations for increased cooperation rates motivate our third conclusion: *Some factor other than punish/reward behavior drives the increase in cooperation rates in the 10-stage game over the 2-stage game. Inspection of outcomes suggests that some of the increase may be due to the opportunity for type-A players to develop a reputation as a player who punishes defection and rewards cooperation. High rates of cooperation may also be explained by the high costs to type-B and type-C players of a switch in early stages of the 10-stage game.*

5. Conclusion

The structure of a game can prominently affect the selection of cooperative outcomes, independent of the structure of the underlying equilibria. For example, in many games with cooperative equilibria, strategies that incorporate threats may not always be recognized, either by the person who could use the threat strategy or by the person being threatened. This paper reports the results of an experiment in which subjects are placed in 3-person, choice-of-partner situations. The subject who gets to choose which of the other two to "do business with" is able to administer a salient punishment, i.e. switching partners, in a multi-stage game. This punishment does not involve coordination or acceptance of the punishment in order for it to be effective. Moreover, switching imposes costs on a seller (and provides incentives to offer high quality) independent of whether the buyer switches from a defector for strategic motives or for non-strategic motives (such as the search for an altruist).

In two-stage games, the chooser frequently uses punishments (switching after a partner defects) and rewards (staying with a partner who cooperates), but the level of cooperative play by the selected partners is no higher than in the single-stage control games. Significantly higher rates of cooperation are observed in the 10-stage version of the game. Increasing the number of stages both gives type-A players the opportunity to establish a reputation as a player who will punish defection and reward cooperation, and increases the costs to type-B and type-C players of being switched away from in non-terminal stages.

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