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Cooperation in infinitely repeated games of strategic complements and substitutes[☆]

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ABSTRACT

We report the results of an experiment conducted to study the effect of strategic substitutability and strategic complementarity on cooperation in infinitely repeated two-player games. We find that choices in the first rounds of the repeated games are significantly more cooperative under strategic substitutes than under strategic complements and that players are more likely to choose joint-payoff maximizing choices in the former than in the latter case. We argue that this effect is driven by the fact that it is less risky to cooperate under substitutes than under complements. We also find that choices do not remain more cooperative under strategic substitutes than under complements over the course of the rounds *within* the repeated games. We show that this is because best-reply dynamics come into the picture: players are more inclined to follow cooperative moves of the partner under complements, offsetting the treatment effect observed in the first rounds.

1. Introduction

In infinitely repeated dilemma games opportunistic and cooperative behavior can both be supported in equilibrium. Experiments help to identify conditions under which cooperation prevails. In this paper we study the role of the strategic environment, that is, whether choices are strategic substitutes or strategic complements, for behavior in this class of games. The key difference between games of strategic substitutes and strategic complements is that the choices of players offset one another in the former games, whereas they mutually reinforce one another in the latter games. In other words, strategic complementarity refers to the property that best-response functions slope upward, while under strategic substitutability

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best-response functions slope downward.[1] The distinction is relevant for several applications. For example, depending on whether firms in oligopolistic markets with homogeneous goods are engaged in price or quantity competition, actions are strategic complements or substitutes, and *vice versa* in markets with complementary goods. Also, depending on whether skills of members in teams are complementary or substitutable, efforts of team members are strategic complements or substitutes.[2]

Which effects of the strategic environment can be expected? The related literature does not give an unambiguous answer to this question. On the one hand, based on the notion of strategic risk, as highlighted by Dal Bo and Fréchette (2011, 2017) and Blonski et al. (2011), less cooperation is expected with strategic complementarity than with strategic substitutes. Specifically, with strategic complementarity it is riskier to cooperate because the opportunity cost of a partner following a defection strategy is higher than with strategic substitutes. On the other hand, experimental evidence on related games suggests that environments with strategic complementarity are more conducive to cooperation. For instance, Potters and Suetens (2009) report significantly more cooperation in finitely repeated games with strategic complements than in equivalent games with substitutes.[3]

We run an experiment in which pairs of subjects play games with an indeterminate final round that feature either strategic complementarity or strategic substitutability. After each round the game proceeds to a next round with a fixed continuation probability (e.g. Roth and Murnighan, 1978). In order to allow for learning across games, subjects play at least 20 repeated games. [4]After a repeated game ends, players are randomly re-matched to play another repeated game with the same continuation probability in each round. We follow Potters and Suetens (2009) in keeping several variables constant across the strategic complements and substitutes settings, namely, actions and payoffs in the Nash equilibrium of the stage game and in the symmetric joint-payoff maximum, payoffs on the best-response function, and the absolute value of the slope of the stage-game best-response function. Mutual cooperation at the joint-payoff maximum can be sustained in equilibrium with the same critical discount factor across the two settings. There is thus no clear reason why the standard theory of infinitely repeated games would predict a difference in behavior between both settings.

We find that in the first rounds of the repeated games, players are on average more cooperative under strategic substitutes than under complements. If all rounds are considered, however, the difference disappears. We show that two countervailing behaviors hidden at the aggregate level but in line with the two above-mentioned predictions are responsible for these results. The first of these is that players more often take the risk of starting off with choices at the joint-payoff maximum under strategic substitutes than under strategic complements, and that, as a consequence, the percentage of joint-payoff maximizing choices is higher under substitutes than under complements. This behavior fits well with the notion that strategic risk related to cooperation at the joint-payoff maximum is lower under substitutes than under complements. A second finding is that choices of players who do not cooperate at the joint-payoff maximum tend to be lower, i.e., less cooperative, under strategic substitutes than under strategic complements (albeit this effect is statistically not significant). Overall, players are (significantly) more likely to follow a cooperative move of the other player under complements than under substitutes, and given that some players, in fact, make a cooperative move, this has an effect opposite to the strategic risk effect. The latter finding squares well with earlier-reported experimental evidence on the effect of the strategic environment.

Combining our findings with those in the literature, we hypothesize that the effect of the strategic environment on cooperation in repeated games may be different depending on whether players know when the game will end. A meta-study based on data from oligopoly experiments confirms this hypothesis. Specifically, we find a positive and significant interaction between strategic complementarity and whether or not the players know when the game will end; strategic complementarity has a positive effect on cooperation only in games with a known end. For application purposes, it thus seems crucial to know whether a finitely or infinitely repeated game best approximates the context to be modeled. In the final section of the paper, we discuss the interpretation of this finding in more detail. We elaborate on how our findings compare to the most closely related papers Potters and Suetens (2009) and Embrey et al. (2017, 2018).

The remainder of this paper is organized as follows. In Section 2 we introduce the experimental design and procedures. In Section 3 we develop the predictions regarding the behavior in our experiment, focusing on the comparative static predictions between the treatments with complements and substitutes. In Sections 4 and 5 we present the experimental results. In Section 6 we report the results of the meta-analysis. Section 7 offers a concluding discussion in the light of the existing literature.

[1] Formally, a game is characterized by strategic complements (substitutes) if for player i, payoff π_i and choice x_i with $i \neq j$, it holds that $\partial^2\pi_i/\partial x_i \partial x_j > 0$ (< 0), implying that the best-response functions are upward- (downward-) sloping (see Topkis, 1978; Bulow et al., 1985; Fudenberg and Tirole, 1984).

[2] Other examples include the production of a public good, which can either be characterized by increasing or decreasing returns, leading to contributions being strategic complements or substitutes, respectively, and R&D investment with low or high technological spillovers, implying strategic substitutability or complementarity, respectively.

[3] Similar results are reported by Boone et al. (2008). Moreover, in experimental games with Bertrand competition, in which choices are strategic complements, behavior is typically more collusive than with Cournot competition, which is characterized by strategic substitutability (Engel, 2007; Potters and Suetens, 2007).

[4] Allowing for learning is standard practice in the experimental literature on infinitely repeated games (see Dal Bo and Fréchette, 2017).

Table 1
Theoretical Benchmarks.

	Comp	Subs
$Choice_{NE}$	14.0	14.0
$Choice_{JPM}$	25.5	25.5
$Choice_{BRtoJPM}$	17.42	10.64
π_{NE}	27.71	27.71
π_{JPM}	41.97	41.97
$\pi_{BRtoJPM}$	60.14	60.14
$\pi_{MutualBRtoJPM}$	34.89	15.44
π_{Minmax}	−1.05	−1.07
π_{Sucker_1}	5.92	10.50
π_{Sucker_2}	−9.06	14.28
Slope of BR function	0.30	−0.30

Notes: This table summarizes the theoretical benchmarks regarding choices and payoffs in the stage games of our experiment. We include two versions of the sucker payoff. Π_{Sucker_1} corresponds to the payoff a player earns when he plays JPM and the other player best responds to JPM. Π_{Sucker_2} corresponds to the payoff a player earns when he plays JPM and the other player plays the stage-game NE. a Choices and payoffs corresponfing s and oa

2. Experimental design and procedures

2.1. Experimental design

Our experiment has two treatments: one where choices are strategic complements (Comp) and another where choices are strategic substitutes (Subs). In both treatments subjects play indefinitely repeated games that have a continuation probability of 0.9 in each round. The per-round payoffs in the repeated games are determined by the following quadratic payoff functions for Comp and Subs respectively (borrowed from Potters and Suetens, 2009):

$$\pi_i^{\text{Comp}}(x_i, x_j) = -28 + 5.4740x_i + 0.0100x_j - 0.2780x_i^2 + 0.0055x_j^2 + 0.1650x_ix_j, \tag{1}$$

$$\pi_i^{\text{Subs}}(x_i, x_j) = -28 + 2.9686x_i + 2.5154x_j - 0.0818x_i^2 + 0.0228x_j^2 - 0.0485x_ix_j, \tag{2}$$

where $i, j = 1, 2;\ i \neq j$. Both games have positive externalities, a unique and Pareto dominated Nash equilibrium (NE) and a symmetric socially efficient (joint-payoff maximizing) outcome (JPM). The coefficients in the payoff functions are chosen in order to ensure a fair comparison between the two treatments. First, in both treatments stage-game NE choices ($Choice_{NE}$) are the same and the JPM choices ($Choice_{JPM}$) are the same. Second, the payoffs corresponding to the stage-game NE (π_{NE}) and the JPM (π_{JPM}) are the same across the two treatments. Third, the payoff achieved by best responding to JPM play of the matched player, equivalent to the "temptation" payoff in a prisoner's dilemma ($Choice_{BRtoJPM}$), is the same in the two treatments. Lastly, the absolute value of the slopes of the best-response (BR) functions (*Slope of BR function*) are the same in the two treatments to guarantee that the same speed of convergence is generated by BR dynamics. Table 1 summarizes the main theoretical benchmarks of our design. In addition, it includes information on the following: "mutual temptation" payoff ($\pi_{MutualBRtoJPM}$), which is defined as the payoff a player earns who best responds to JPM, while the other does the same; "minmax" payoff (π_{Minmax}), which is defined by the minimum payoff a player gets in a worst case scenario; "sucker" payoff (π_{Sucker_1}), which is defined as the payoff a player earns who plays JPM, while the other player optimally defects (*i.e.*, best responds to JPM); and an alternative "sucker" payoff (π_{Sucker_2}) is defined as the payoff a player earns who plays JPM, while the other players plays the NE choice.

By using the payoff functions given in (1) and (2), we keep several actions and payoffs constant across treatments. We felt the same should be done with respect to the sequence of matches and their respective lengths. At the same time, because of possible order effects, we did not want to have just one sequence of matches to be played in each of the two treatments. We therefore decided to have five different draws of the lengths of matches prior to the start of the experiments, each of which was administered in one session of each of the two treatments Comp and Subs. The length of each match in a draw

[5] Note that one important difference between our design and the design of Potters and Suetens (2009) is about the repetition of the stage game. In their experiment the stage game is played for exactly 30 periods with the same partner, corresponding to the play of one "long" repeated game, while in our experiment subjects play at least 20 supergames with an expected length of 10 (due to the continuation probability of $\delta = 0.9$) with different partners.

was determined randomly with the continuation probability of 0.9. Figure X in Web Appendix F shows the distribution of realized match lengths across all five draws.[6]

Since there is always the possibility of continuing to a next round, the randomization generates a game that is strategically equivalent to an infinitely repeated game. In particular, the continuation probability δ is equivalent to the discount factor in an infinitely repeated game assuming that within the time slot of an experiment, there is no discounting(Roth and Murnighan, 1978).

2.2. Experimental procedures

The experiment consisted of 10 sessions (five for each of the two treatments Comp and Subs) that were conducted at CentERlab at Tilburg University during September-October 2011.[7] A total number of 160 students participated in the experiment. Participants were recruited through an E-mail list of students who are interested in participating in experiments. In each session, 16 subjects interacted anonymously in a sequence of matches, with a match corresponding to an indefinite repetition of the same stage game. In each round of a match the game continued to the next round with a 0.9 probability, and participants were informed about this. After a match participants were randomly re-matched, and started a new match (i.e. repeated game).[5] In each session subjects participated in as many matches as possible such that at least 20 matches were played. If at least 20 matches had already been played, a session ended after one and a half hours of play. Sessions lasted not more than two hours (including the time to read the instructions and payment of the subjects) and covered between 20 and 25 matches.

All participants in a treatment were given the same instructions (see Web Appendix A). The identity of the partners was not revealed to subjects. It was explained to the subjects that their final earnings depended on their own choices and the choices of the matched participants. The subjects were asked to choose a number between 0.0 and 28.0 (up to one decimal point) in each round of a match. Subjects were provided an earnings calculator on the computer screen enabling them to calculate their earnings in points for any combination of hypothetical choices, and a payoff table for combinations of hypothetical choices that are multiples of two (see Figure I and Figure II in Web Appendix A).

After choices were submitted in a round, subjects were informed about whether or not the match would continue to a next round. In the case the game continued to a next round, subjects received the message "*The match continues to the next round.*" on the computer screen. In the case the match ended, subjects received the message "*The match is over.*" on the computer screen. Moreover, after each round of a match subjects were shown on the screen their and the matched partner's choice and earnings. After subjects finished reading the instructions, we explained to them that the experiment itself would proceed for about 1.5 h.

The payoffs in the experiment were expressed in points. At the end of the experiment, the sum of a subject's earnings in points in all rounds of all matches were converted into Euro at the exchange rate of 480 points = 1 Euro, and privately paid to subjects. The average earning in the experiment was 16.45 Euro.

3. Predictions

A first prediction builds on the standard theory of infinitely repeated games. Based on a simple grim-trigger strategy,[8] this theory predicts that a sufficient condition under which full cooperation (JPM) can be supported as a subgame-perfect Nash equilibrium (SPNE) is the following (Friedman, 1971):

$$\frac{\pi_{JPM}}{1-\delta} \geq \pi_{Temptation} + \frac{\delta \pi_{NE}}{1-\delta}. \tag{3}$$

The left-hand side of (3) is the discounted sum of payoffs from full cooperation, while the right-hand side is the discounted sum of payoffs from a one-time deviation followed by stage-game NE play forever after. By design, the JPM payoff, the payoff from a best response to JPM, and the stage-game NE payoff are the same in both treatments. Rearranging condition (3) and using the numbers given in Table 1, we get $\delta \geq \underline{\delta} := 0.56$ for both treatments. Thus, the critical discount factor above which cooperation at the joint-payoff maximum is supported by a grim-trigger strategy is the same in both treatments.[9]

[6] The complete set of the randomly determined lengths of the matches actually played in the five different draws are shown in the notes of Table IV, which summarizes how many matches were actually played for each draw.

[7] We used the experimental software toolkit *Z-Tree* to program and conduct the experiment (see Fischbacher, 2007).

[8] According to this strategy a player cooperates in the first period and continues to cooperate until a single defection by the opponent, which triggers defection, i.e., stage-game NE play forever after.

[9] Note that since in our games the minmax payoffs are (roughly) the same and smaller than the stage-game NE payoffs, full cooperation can be supported for even lower δ than $\underline{\delta}$ (see, e.g., Fudenberg and Maskin (1986); Mailath and Samuelson (2006)). And these lower δ will also be the same in our games as long as critical thresholds only depend on those payoffs which we held fixed in our games (see Table 1). Furthermore, the range of actions that Pareto-dominate the stage-game Nash equilibrium, and thus also the range of actions that can be sustained in equilibrium in an infinitely repeated game, is larger under substitutes than under complements. This can be seen in Figure VIII in Web Appendix F that shows isopayoff contours in both cases. Given the findings of Gazzale (2009), we did not expect that this difference would lead to differences in the extent to which subjects succeed in fully cooperating. It may lead to larger variability in actions under substitutes than under complements, though.

A second prediction takes into account differences in the relative riskiness of cooperation in the two treatments, and thus depends on the "sucker" payoff. The sucker payoff can either be defined as the payoff of playing JPM against a partner best-responding to JPM, or as the payoff of playing JPM against a partner playing the stage-game NE. In either case the sucker payoff is lower in Comp than in Subs, as can be seen in Table 1.[10]. Intuitively, choosing an action that maximizes joint payoffs is less attractive in Comp than in Subs, because doing so is relatively more risky in the former than in the latter treatment. Recently, this intuitive idea received formal support by approaches put forward in, e.g., Dal Bo and Fréchette (2011) (who elaborate on the *basin of attraction* of a cooperative strategy in comparison to a defecting strategy) and Blonski et al. (2011) (who develop an *axiomatic approach to equilibrium selection* in infinitely repeated prisoner's dilemma (PD) games). Both of theses approaches reflect the influence of the sucker payoff on the incidence of fully cooperative play.

To illustrate the approach of Dal Bo and Fréchette (2011) (see also Dal Bo and Fréchette, 2017), we follow the literature by simplifying strategy choices in the repeated game and adjust them to the context of our games. In particular, we assume that players can either play a (cooperative) grim trigger strategy or a defective strategy, and that this is common knowledge. We use two different types of defective strategies. First, we consider the defective strategy that starts by best-responding to an assumed JPM choice by the other player and then unconditionally switches to the stage-game NE forever after.[11] A player needs to determine which of the two strategies generates the higher expected payoff given the belief that with probability p the other player plays the cooperative grim trigger strategy and with probability $1-p$ plays the defective strategy. The basin of attraction of the cooperative strategy is the set of beliefs p for which playing this strategy gives a higher expected payoff than the defective strategy. The expected payoff of the cooperative grim trigger strategy is larger than that of the defective strategy if:

$$p\frac{\pi_{JPM}}{1-\delta}+(1-p)\left(\pi^k_{Sucker}+\frac{\delta\pi_{NE}}{1-\delta}\right)>p\left(\pi_{BRtoJPM}+\frac{\delta\pi_{NE}}{1-\delta}\right)+(1-p)\left(\pi^k_{MutualBRtoJPM}+\frac{\delta\pi_{NE}}{1-\delta}\right), \tag{4}$$

where k refers to Comp or Subs and the payoff terms are as described in Table 1. Inequality (4) is satisfied if $p>p^*_k$ where p^*_k is the threshold above which playing the grim trigger strategy is the payoff maximizing strategy. That is, the lower p^*_k, the larger the basin of attraction of the grim trigger strategy, and the more likely it is that subjects find it optimate to choose an action that maximizes joint profit. For a continuation probability of 0.9, which we use in our experiment, we find $p^*_{\text{Comp}}=0.208$ and $p^*_{\text{Subs}}=0.063$. Full cooperation is thus predicted to emerge for a larger range of beliefs in Subs than in Comp. Second, consider the defective strategy that prescribes the choice of the stage-game NE in each period of the repeated game. The expected payoff of the grim trigger strategy is larger than that of the alternative defective strategy if:

$$p\frac{\pi_{JPM}}{1-\delta}+(1-p)\left(\pi^k_{JPM,NE}+\frac{\delta\pi_{NE}}{1-\delta}\right)>p\left(\pi^k_{JPM,NE}+\frac{\delta\pi_{NE}}{1-\delta}\right)+(1-p)\left(\frac{\pi_{NE}}{1-\delta}\right), \tag{5}$$

where $\pi^k_{JPM,NE}$ is the payoff for a player if he plays JPM and the other player chooses the stage-game NE choice. In this case, we find $p^*_{\text{Comp}}=0.080$ and $p^*_{\text{Subs}}=0.063$, so that full cooperation is again predicted to emerge for a larger range of beliefs in Subs than in Comp.[12]

A third prediction is based on the literature that studies the interaction between the strategic environment (complements versus substitutes) and heterogeneity of players (see Haltiwanger and Waldman, 1991 or Camerer and Fehr, 2006)), as well as its application to repeated-game experiments. The intuition is as follows. In games of strategic complements a change in the matched player's choice gives a payoff-maximizing player an incentive to move in the *same* direction, while in games of strategic substitutes the incentive is to move in the *opposite* direction. Given that several experiments have shown that some individuals are (conditionally) cooperative in the sense that they follow cooperation when established by others, even when there is no future interaction or the chance of future interaction is low (see Fischbacher et al., 2001; Fischbacher and Gächter, 2010; Reuben and Suetens, 2018), it is plausible to assume that players are heterogeneous in their cooperativeness and defection strategies. Consider, for example, a cooperative player who is matched with a defector in the above-described games of complements and substitutes. If the cooperative player makes a cooperative choice (higher than the stage-game NE), and the matched defector is an optimal defector in the sense that he best-responds to this move, then, in sum, choices will be higher (more cooperative) in Comp than in Subs. This is because in Comp, the best response to a cooperative move is to (partly) follow the move and make a higher choice as well, whereas in Subs the best response is to make a less cooperative choice. This mechanism may facilitate cooperation in Comp and may hamper it in Subs. A similar mechanism

[10] A formal analysis of the generality of this property is beyond the scope of this paper. However, a numerical analysis reveals that this property holds for a broad range of parameters in our payoff functions. In this numerical analysis, we employed a grid-search for $5*200^5$ different parameter combinations in our payoff functions, which is discussed in detail in E of the Web Appendix.

[11] Note that this strategy is chosen so that the influence of the sucker payoff on the likelihood of cooperative play can be illustrated in a brief and compact way. Of course, more elaborate examples are conceivable.

[12] Blonski et al. (2011) suggest an axiomatic approach to equilibrium selection in indefinitely repeated 2×2 prisoner's dilemma (PD) games. They show that a set of five axioms leads to a discount factor δ^* that is strictly larger than the standard discount factor $\underline{\delta}$ derived in inequality (3) and that, importantly for our purposes, also reflects the influence of the sucker payoff on the incidence of fully cooperative play. If one is willing to make the strong simplifying assumption that the action space of our stage games consist of just two actions (a cooperative and a defective one), one can derive the result that $\delta^*_{\text{Comp}}>\delta^*_{\text{Subs}}$, so that full cooperation can be sustained for a larger range of discount factors in treatment Subs than in treatment Comp.

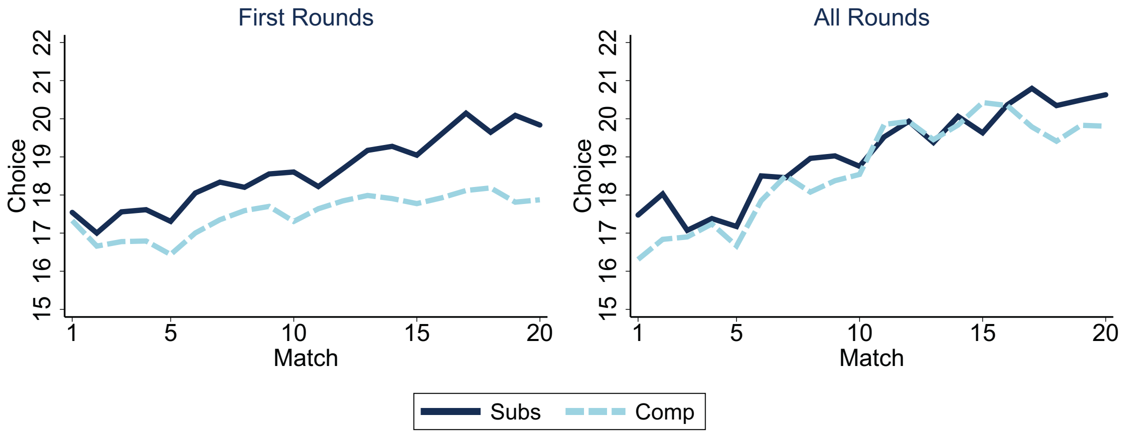

Fig. 1. Evolution of Average Individual Choices. *Notes:* This figure shows the evolution of average individual choices across matches based on first rounds (left-hand panel) and all rounds (right-hand panel). Note that, the Nash equilibrium choice and the joint profit maximizing choice of the static game are $Choice_{NE} = 14.0$ and $Choice_{JPM} = 25.5$ respectively.

occurs when a cooperative player is matched with a spiteful defector who aims at maximizing the payoff difference between himself and the cooperator. In order to employ the same level of punishment (in payoff terms), a spiteful defector must choose much lower choices in the Subs treatment than in the Comp treatment. So here as well, choices will, on average, be higher, i.e., more cooperative in Comp than in Subs. Potters and Suetens (2009) provide evidence for this intuition in the context of a finitely repeated game.

Summarizing, based on theory and earlier experimental results no unambiguous prediction can be made regarding the higher prevalence of cooperation in our two treatments. Hence, we formulate the following research question:

Research Question Which environment is more conducive to cooperation in the context of an infinitely repeated game: strategic substitutes or strategic complements?

4. Main results

Before reporting the main results, we note the following. The number of played matches varied due to variation in the random draws. In some of the sessions exactly 20 matches were played, while in other sessions more than 20 matches were played. To keep the number of matches balanced across the sessions, all data analyses reported in the main text are based on data from the first 20 matches, but our conclusions do not hinge on this data selection.

Fig. 1 illustrates the evolution of average choices over matches under strategic complements and strategic substitutes. The left-hand panel uses data from first rounds only and the right-hand panel is based on data from all rounds. We examine first and all rounds of a match separately since cooperation might evolve differently within a match, depending on the number of rounds in that match (see Dal Bo and Fréchette, 2011). In addition, in the first rounds of each match subjects are playing with a new partner, and thus have no experience with their partners' behavior (or cannot know it due to random matching). In this respect, subjects' behavior in first rounds of each match is mainly driven by the fundamentals of the game they are playing (and possibly their experiences in the previous matches), and not by the current partner's behavior.

As can be seen in the left-hand panel of Fig. 1, first-round choices increase more strongly over matches in Subs than in Comp. Overall, the mean first-round choice is 18.63 in Subs and 17.50 in Comp. In matches 11–20 means across the first rounds are 19.30 and 17.85, respectively. The right-hand panel shows that the average choice across all rounds is strongly increasing over the matches in both treatments, and that there is no longer a clear difference between the treatments. Overall, the mean choice across all rounds is 19.09 in Subs and 18.70 in Comp. In matches 11–20 the mean choice in Subs is 20.12 and that in Comp is 19.87.[13] The average choice is thus roughly the same in the two treatments.[14]

To formally quantify treatment differences, and to test their statistical significance, we estimate the effect of strategic complementarity on the individual choice by regressing the choice of an individual on a treatment dummy. The econometric analysis is based on a mixed-effects regression model with standard errors corrected for clustering at the session level.[15] Results based on first rounds and all rounds are reported in columns (1) and (2), and columns (3) and (4) of Table 2, respectively. The regression results confirm what is visualized in Fig. 1. As can be seen in column (1), the choice in Comp

[13] The summary statistics for average choices are presented in Table VI in Web Appendix G.

[14] Payoffs are also very similar in the two treatments, as reported in Table V in Web Appendix G.

[15] Unless otherwise mentioned, we use this methodology in all reported regressions. In Tables IX to XI in Web Appendix G, we report results based on data from matches 11–20 only. Note that the results presented in Tables 2 to 4 are quantitatively and qualitatively similar if we use linear models and compute bootstrapped p-values.

Table 2
Regression results on choice.

	First rounds		All rounds			
VARIABLES	(1) $Choice_{it}$	(2) $Choice_{it}$	(3) $Choice_{it}$	(4) $Choice_{it}$	(5) $Choice_{it}$	(6) $Choice_{it}$
Comp	−1.124**	−0.275	−0.318	−0.734	−3.535***	−3.194***
	(0.526)	(0.477)	(0.808)	(0.570)	(0.381)	(0.430)
$Choice_{jt-1}$					0.623***	0.609***
					(0.024)	(0.020)
Comp×$Choice_{jt-1}$					0.187***	0.176***
					(0.026)	(0.024)
Match		0.154***		0.193***		0.078***
		(0.019)		(0.042)		(0.019)
Comp×Match		−0.081**		0.038		−0.013
		(0.035)		(0.059)		(0.024)
Constant	18.625***	17.000***	19.210***	17.113***	7.302***	6.717***
	(0.332)	(0.401)	(0.703)	(0.487)	(0.303)	(0.310)
Observations	3,200	3,200	33,024	33,024	29,824	29,824

Notes: This table reports results from mixed effect regressions with standard errors (in parentheses) clustered at the session level. *** (**) [*] indicate that the estimated coefficient is significant at the 1% (5%) [10%] level. The dependent variable is a subject's choice in all specifications. Specifications (1) and (2) are based on observations from the first rounds of matches only and specifications (3), (4), (5) and (6) are based on all observations.

in first rounds is estimated to be 1.124 units lower than the choice in Subs, and the treatment effect is significant at the 5% level. If allowing for treatment effects on learning patterns across matches (column (2)), we see that the stronger increase in first-round choices in Subs is significant at the 5% level. These results are supportive of the prediction based on the riskiness of a cooperative strategy; cooperation less risky in Subs than in Comp. As shown in column (3), the treatment difference across all rounds is much smaller in size (−0.318) and not statistically significant. Controlling for the match and the interaction between treatment and match does not change much to this result (column (4)).

Next, we analyze the adjustments across rounds within repeated games. During a match, subjects observe the previous choice of the matched subject and are likely to adjust their own behavior. If at least some of the subjects (noisily) best-respond it should be the case that in Comp the estimated response function has a higher slope than in Subs (see Table 1). Columns (5) and (6) of Table 2 report estimates of the observed response functions. The reported results come from regressions where the choice of a player is regressed on the choice of the matched player in the previous round (in the same match) as well as the interaction of the other subject's previous choice and a treatment dummy. In column (6) additional controls are included for the match and the interaction between match and treatment. Response functions being positively sloped in both treatments can be explained by endogenous complementarity that arises when subjects use reciprocal strategies (see also Potters and Suetens, 2009); reciprocal players following cooperative (and other) choices by others generates a form of strategic complementarity. This endogenous strategic complementarity leads subjects to follow each other in both treatments.

The estimations show that it is indeed the case that the extent to which subjects follow each other is significantly greater in Comp than in Subs, which shows in a positive dynamic effect of complementarity. Despite the choices being more cooperative initially under Subs than under Comp, the positive dynamic effect of complementarity helps cooperation to build up more in Comp than in Subs during the course of a match. Also in Subs, the past choice of the partner has a significant effect on one's own choice, though. To illustrate, an increase in the choice by a subject by one unit increases the choice of the matched subject in the next round by 0.62 units in Subs and by 0.81 units in Comp (cf. column (5)). The effects are very similar when we control for the match and the interaction between match and treatment (column (6)).[16] From columns (5) and (6) in Table 2 it can also be seen that once we control for the past choice of the partner, Comp has a negative effect on choices (significant at the 1% level).

In sum, the nature of the strategic environment seems to influence behavior in different ways. To understand behavior better, we provide a more detailed analysis in the following section.

5. Detailed analysis

First, it is useful to consider Fig. 2, which depicts the distributions of choices in our two treatments. The figure shows that the modal choice in both treatments is a choice at or very close to the JPM level of 25.5. This is particularly accentuated in Subs, where almost 30% of the choices are at the JPM level or in the interval [25,26]. In Comp we only observe about

[16] The reported effects are unchanged when we include the additional controls round (within a match) and the interaction between round and treatment to the estimation in column (6) of the Table 2. In this case, the estimated coefficient of $Choice_{jt-1}$ remains exactly the same as in column (6) with $p < .001$, and the estimated coefficient of Comp×$Choice_{jt-1}$ becomes 0.177 with $p < 0.001$.

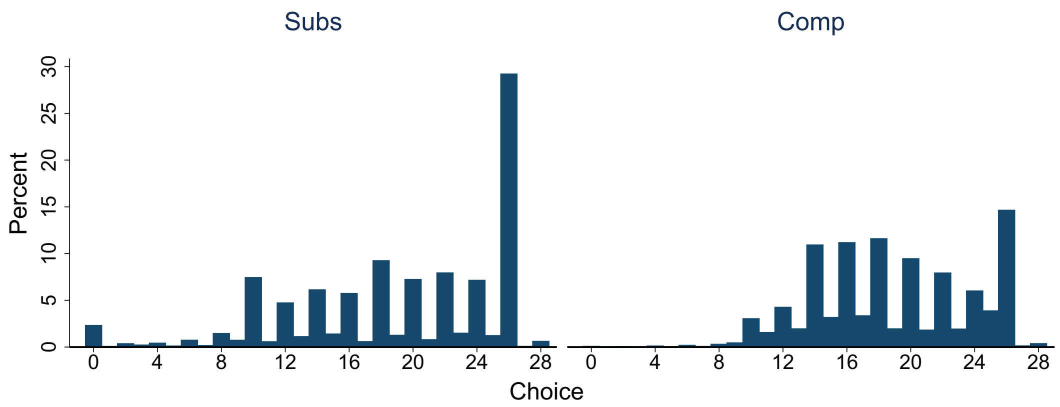

Fig. 2. Distribution of Choices. *Notes:* The figure shows the distribution of individual choices in the experiment by treatment.

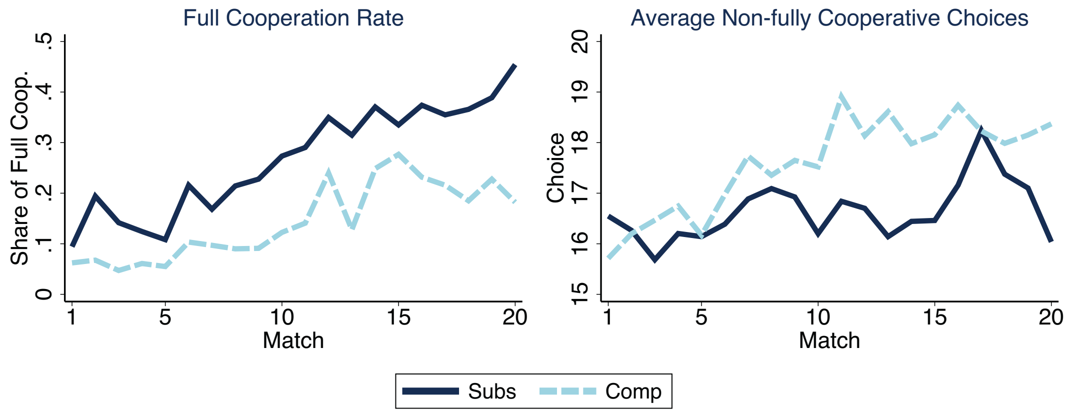

Fig. 3. Cooperative vs Non-Cooperative Behavior. *Notes:* This figure shows the evolution of cooperative and non-cooperative behavior. The left-hand panel depicts the evolution of full cooperation rate across matches and the right-hand panel depicts the evolution of average non-fully cooperative choices across matches. Note that, the Nash equilibrium choice and the joint profit maximizing choice of the static game are $Choice_{NE} = 14.0$ and $Choice_{JPM} = 25.5$ respectively.

15% of such choices. The figure also shows that choices in Subs are spread over the whole interval, while choices in Comp are somewhat more concentrated.

To further explore possible differences between Subs and Comp, we distinguish “fully cooperative” and “non-fully cooperative” choices. We define a choice to be fully cooperative if it lies within the interval [25,26] and call a choice to be non-fully cooperative if it lies outside the interval [25,26].[17] The left-hand panel of Fig. 3 shows for both treatments the share of fully cooperative choices across matches, and the right-hand panel depicts the evolution of averages of non-fully cooperative choices across matches. From the graph on the left in Fig. 3 it becomes clear that the share of fully cooperative choices is higher in Subs than in Comp. In addition, the share of fully cooperative choices increases in both treatments, but more so in Subs than in Comp. To illustrate, in matches 11–20, the percentage of fully cooperative choices is around 40% in Subs, while it is around 25% in Comp.

In the right-hand graph in Fig. 3 we observe that the average non-fully cooperative choice of subjects is lower in Subs than in Comp. The effect of strategic complementarity on behavior thus seems to switch—higher choices imply more cooperative behavior—when we focus on non-fully-cooperative choices. To illustrate, in matches 11–20, the average non-fully cooperative choice is 16.85 in Subs and 18.33 in Comp.

In sum, aggregate behavior seems to be driven by two countervailing forces. To understand these forces better, we analyze fully cooperative behavior and non-fully cooperative behavior in more detail, in Sections 5.1 and 5.2, respectively.[18]

[17] The choice of such a range is to some extent arbitrary, and one may argue that choices above 26 are also fully cooperative. For example, 28, which is the maximum choice possible, can serve as a focal point for subjects to coordinate on (almost) full cooperation. Enlarging the fully-cooperative interval to [25,28], does not affect any of our qualitative results. Choices above 26 correspond to 0.68% of all choices in the experiment.

[18] We also explore learning across matches, see Web Appendix C. We find, among other things, that the previous match length and partners' full cooperation in the previous match significantly increases the likelihood of full cooperation in the current match in both treatments, with a stronger effect in Subs.

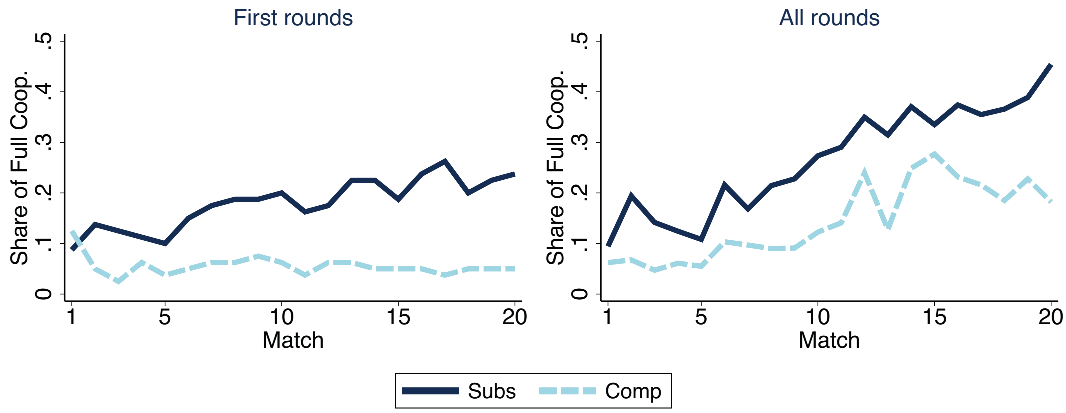

Fig. 4. Full Cooperation Rate. *Notes:* This figure shows the evolution of full cooperation rate across matches, on the left-hand panel for the first rounds only and on the right-hand panel for all rounds.

Table 3
Regression results on full cooperation.

	First rounds		All rounds	
VARIABLES	(1) $FullCoop_{it}$	(2) $FullCoop_{it}$	(3) $FullCoop_{it}$	(4) $FullCoop_{it}$
Comp	−0.124***	−0.037*	−0.118***	−0.109**
	(0.029)	(0.021)	(0.042)	(0.044)
Round				0.005***
				(0.002)
Comp×Round				0.003
				(0.002)
Match		0.007***		0.018***
		(0.002)		(0.004)
Comp×Match		−0.008***		−0.003
		(0.002)		(0.006)
Constant	0.180***	0.103***	0.300***	0.065***
	(0.020)	(0.019)	(0.025)	(0.030)
Observations	3,200	3,200	33,024	33,024

Notes: This table reports results from mixed effect regressions with standard errors (in parentheses) clustered at the session level. *** (**) [*] indicate that the estimated coefficient is significant at the 1% (5%) [10%] level. Specifications (1) and (2) are based on observations from the first rounds of matches only and specifications (3) and (4) are based on all observations.

5.1. Full cooperation rate

In this section we take a closer look at the full cooperation rate, which we define as the percentage of choices in the interval [25,26].[19] In doing so, we again examine first and all rounds of a match separately. Fig. 4 shows the evolution of the full cooperation rate across matches, in the left-hand panel for the first rounds and in the right-hand panel for all rounds (where we display the latter from Fig. 3 again to ease comparison). The left-hand panel of Fig. 4 shows that in the first rounds of a match subjects make fully cooperative choices more frequently under Subs than under Comp. In addition, the first-round full cooperation rate follows an increasing trend in Subs, while in Comp it is more steady across matches. To illustrate, it reaches the level of about 25% in Subs by the end of the experiment, while it remains at around 5% in Comp. These trends are very similar to those observed in the left-hand panel of Fig. 1.

In order to test whether these differences are statistically significant, we ran two specifications of a regression in which the dependent variable is a dummy equal to one if a subject makes a choice in the interval [25,26]. In specification (1) shown in Table 3 we include as an independent variable a treatment dummy. As can be seen, the treatment dummy has a negative sign—the full cooperation rate in Comp is thus lower than the one in Subs—and is statistically significant. The estimated effect is −0.124.

[19] In Web Appendix B, we present results from a comparison at the pair level. We find that JPM pairs (those pairs succeeding in maximizing joint payoffs) make higher choices in Subs than in Comp, while non-JPM pairs make higher choices in Comp than in Subs.

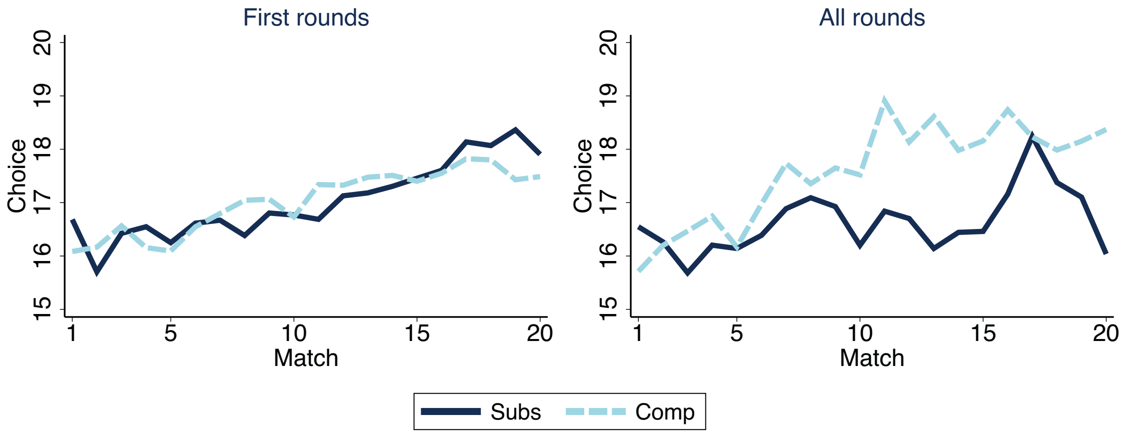

Fig. 5. Average Non-Fully Cooperative Choices. *Notes:* This figure shows the evolution of non-fully cooperative choices (i.e., choices outside the range [25,26]) across matches, on the left-hand panel for the first rounds only and on the right-hand panel for all rounds. Notice that the right-hand panel is the same as the right-hand panel of Fig. 3. Note that, the Nash equilibrium choice and the joint profit maximizing choice of the static game are $Choice_{NE} = 14.0$ and $Choice_{JPM} = 25.5$ respectively.

In specification (2), next to the treatment dummy, we control for the match, and the interaction between treatment and match. The estimated effect of the treatment dummy is now −0.037 (significant at the 10% level). More importantly, column (2) shows that the first-round full cooperation rate significantly increases over the matches in Subs, but not so in Comp.

Next, we focus on the right-hand panel of Fig. 4. As illustrated in the figure, there is again a clear difference between the two treatments in the full cooperation rate. Moreover, in contrast to the first rounds, the full cooperation rate now increases over matches in Comp as well. The full cooperation rate rises up to about 25% in Comp and up to about 45% in Subs. The results of the regressions that we report in columns (3) and (4) of Table 3, indicate that the treatment effect is statistically significant. Moreover, as shown in column (4), the full cooperation rate increases significantly over the matches in both treatments.[20]

Summarizing, we find significantly more initiation of full cooperation as well as more fully cooperative choices in general in Subs than in Comp. This treatment effect is in line with what is predicted if appealing to the notion of strategic risk, as for example proxied by the basin of attraction of a cooperative strategy discussed in Section 3.

5.2. Non-fully cooperative behavior

We now analyze the effect of strategic complementarity on non-fully cooperative behavior. In doing so, we focus on those observations that are not in the fully cooperative range of [25,26]. We study these data from two different angles. First, we perform a general analysis of non-fully cooperative choices. Second, we focus on punishment of deviations from full cooperation.

5.2.1. General analysis

Fig. 5 depicts the evolution of the average non-fully cooperative choice over matches, in the left-hand panel for the first rounds and in the right-hand panel for all rounds of a match (where we display the latter again from Fig. 3 to ease comparison). The left-hand panel shows that in the first rounds of the matches there is no clear difference in non-fully cooperative behavior between the two treatments. In both treatments the average non-fully cooperative choice in the first rounds starts off a bit above the static Nash equilibrium choice of 14, and increases over the matches. As shown in Table 4, presenting results from regressions where first-round non-fully cooperative choices are regressed on a treatment dummy, the treatment effect is small and not significant. In addition, as shown in column (2) of this table, the average choice significantly increases over matches in both treatments.

If we consider average non fully-cooperative choices across all rounds, shown in the right-hand panel of Fig. 5, a different picture emerges. When averages are taken across all rounds instead of just the first rounds of a match, the average non-fully cooperative choice is *lower* in Subs than in Comp, although this difference is statistically not significant (see column (3) in Table 4). In addition, the response function estimation results in columns (4) and (5) of this Table, where the past choice of the partner and its interaction with the treatment are regressors, give a similar picture as columns (5) and (6) of Table 2.

[20] In Figure IX in Web Appendix F, we provide a comparison of the collusion index across our two treatments, which is a relevant measure for competition authorities. The main takeaway is that the share of cases in which the collusion index is equal to 1 (and, thus, indicating JPM choices) is much higher in Subs than in Comp.

Table 4
Regression results on non-fully cooperative choices.

	First rounds		All rounds		
VARIABLES	(1) $Choice_{it}$	(2) $Choice_{it}$	(3) $Choice_{it}$	(4) $Choice_{it}$	(5) $Choice_{it}$
Comp	−0.038	0.080	0.895	−3.680***	−3.623***
	(0.577)	(0.458)	(0.820)	(0.434)	(0.466)
$Choice_{jt-1}$				0.469***	0.466***
				(0.030)	(0.027)
Comp×$Choice_{jt-1}$				0.249***	0.230***
				(0.037)	(0.030)
Match		0.108***			0.044
		(0.014)			(0.030)
Comp×Match		−0.019			0.025
		(0.030)			(0.036)
Constant	17.098***	16.041***	16.594***	8.739***	8.370***
	(0.452)	(0.411)	(0.682)	(0.318)	(0.350)
Observations	2,823	2,823	25,061	22,238	22,238

Notes: This table reports results from mixed effects regressions with standard errors (in parentheses) clustered at the session level. *** (**) [*] indicate that the estimated coefficient is significant at the 1% (5%) [10%] level. Specifications (1) and (2) are based on observations from the first rounds of matches only and specifications (3), (4) and (5) are based on all observations.

To illustrate, an increase in the choice by a subject, increases the choice of the matched subject in the next round by 0.47 units in Subs and by 0.71 units in Comp.[21]

The positive (albeit not significant) effect of Comp shown in column (3) of Table 4 in combination with the result that the extent to which subjects follow each other is greater in Comp than in Subs (cf. columns (4) and (5) in Table 4), suggest that at least some subjects try to induce cooperation, to which others (noisily) best-respond. For example, if a subject who increases its choice above the stage-game NE, with the intention to move towards full cooperation, is matched with a (noisily) best-responding subject or a spiteful subject, choices in this pair will on average end up to be higher (more cooperative) in Comp than in Subs, which is exactly what we observe. This is the mechanism behind the prediction based on heterogeneity of subjects' types put forward in Section 3.

Summarizing, when we focus on non-fully cooperative choices, we find that behavior is in agreement with the mechanism based on heterogeneity of subjects, leading to a higher average (non-fully cooperative) choice in Comp than in Subs.

5.2.2. Analysis of punishments

Some of the non-fully cooperative choices may be due to subjects punishing each other for not cooperating. In this sense, the opposing effects that we find—higher cooperation rates in Subs and higher average non-fully cooperative choices in Comp—may stem from a common underlying behavior where cooperative subjects punish deviators in a harsher way in Subs than in Comp by making lower choices. Therefore, we study whether there are treatment differences in the extent to which subjects who fully cooperate "punish" their partner for not cooperating. We conjecture that punishment choices are lower in Subs than in Comp because one needs to choose a lower action in the former treatment than in the latter in order to induce *ceteris paribus* the same level of punishment for a cheating partner.

If punishment choices are taken as choices of players in round t who made a JPM choice in round $t-1$ and whose partner did not make a JPM choice in round $t-1$, we find that the punishment choice in Comp is estimated to be 1.18 units higher than in Subs. The treatment effect is (weakly) significant at $p = 0.062$ (see the estimation in Table VII in Web Appendix G). Furthermore, we consider cases where the cooperating player is more patient and gives her partner the chance to revise the non-JPM choice; we take punishment choices as choices of players in round t who made a JPM choice in rounds $t-1$ and $t-2$ and whose partner did not make a JPM choice in any of these rounds. In this case, we find that the punishment choice in Comp is estimated to be 1.90 units higher than in Subs. The treatment effect is significant at $p = 0.007$.

We conclude this section by pointing out that in Web Appendix D we use agent-based simulations to illustrate the behavioral mechanisms that may be at work in our experiment. The simulation model is based on two types of players (a cooperative and a non-cooperative one) and replicates the key features of our experimental findings: On the one hand, average choices are similar in both treatments. On the other hand, full cooperation rates are higher under Subs than under Comp, while average non-fully cooperative choices are higher under Comp than under Subs.

[21] Similar qualitative and statistical effects are obtained in hurdle models in which the *binary* choice to fully cooperate and the *level* of choice are jointly estimated as a function of the variables listed in Tables 3 and 4, respectively.

Table 5
Summary statistics of variables in the meta-study.

	Obs.	Mean	St. dev.	Min	Max
Strategic substitutes					
Degree of collusion	49	−0.219	0.513	−2.397	0.402
Known end	52	0.865	0.345	0	1
Group size	52	3.15	1.16	2	8
Friedman index	52	0.944	0.476	0.349	3
Payoff feedback	52	0.346	0.480	0	1
Number of periods	52	40.2	29.5	10	100
Strategic complements					
Degree of collusion	18	0.273	0.353	−0.133	0.903
Known end	21	0.619	0.498	0	1
Group size	21	3.19	1.25	2	5
Friedman index	17	0.737	0.383	0.134	1.714
Payoff feedback	21	0.286	0.463	0	1
Number of periods	21	36.9	26.8	10	120

6. Meta-study results

In this section we use data of oligopoly and related experiments to study whether there is an interaction between the nature of the strategic environment and the type of repeated game (known versus unknown end), or between the nature of the strategic environment and the length of the game, given that players know. We use the data collected by Fiala and Suetens (2017) (henceforth, FS). The data are from laboratory experiments in which participants play a repeated game with the same partner(s) (partner matching), and make either price or quantity choices in a simultaneous-move setting. The unit of observation is the treatment, and the total number of data points used in the meta-analysis is equal to sum of the number of treatments in each study. Details on the procedure of data collection are in FS.

In addition to the data reported in FS, which only involve oligopolies with more than two players, we include data from duopoly experiments. Also, next to the variables listed in FS, information was collected about whether or not the participants were aware of the number of periods the repeated game would last (referred to as “Known end”). In some experiments, the number of periods was not communicated to the participants. The data set does not have oligopoly experiments with an indefinite number of rounds.

The dependent variable in the regressions is the average degree of collusion observed in a treatment, defined as the average choice minus the stage-game NE choice divided by the JPM choice minus the stage-game NE choice. The degree of collusion measures the extent to which participants deviate from the stage-game NE in the direction of JPM. In the first set of regressions, our main independent variables are a “Strategic complements” dummy and a “Known end” dummy, and their interaction. In the second set of regressions, the main independent variables are a "Strategic complements” dummy and the number of periods of repeated game play (given that participants are aware), and their interaction. Relevant control variables are the group size, a variable indicating the incentive to deviate from full collusion (“Friedman index”)[22], and a variable measuring whether or not participants receive feedback about the past payoff(s) of the matched players(s) (“Payoff feedback”). The latter variable turned out to be highly predictive of cooperation in groups with at least three players (see FS).

Table 5 shows summary statistics of the variables we use in the meta-analyses. As can be seen, there seems sufficient variation across the variables within the complements and substitutes games.

Table 6 reports the results of a first set of regressions in which the average degree of collusion across all rounds is regressed on the variables of interest. All specifications apart from (1) include an interaction between the strategic complements dummy and the known end dummy. Group size and Friedman index are not included in the same regression to avoid collinearity problems; for the same cost and demand parameters, the Friedman index is a negative function of group size.

Specification (1) in Table 6 shows that if no interaction is included, there is a (large and positive) main effect of strategic complements. Moreover, for the other specifications we find that the interaction between Strategic complements and Known end is positive and statistically significant. The interpretation is that the large and positive effect of strategic complements shown in specification (1) stems from experiments where participants are informed about the number of rounds they would play, that is, from settings where strategic risk is relatively low. In experiments with an unknown number of rounds, where strategic risk is arguably higher, strategic complementarity does not lead to a higher degree of cooperation.

The results of a second set of regressions, in which we test for an interaction of strategic complementarity and number of periods in games with a known end, are reported in Table 7. The dependent variable is again the average degree of collusion across all rounds. All specifications apart from (1) include an interaction between both variables. The table shows

[22] The Friedman index is equal to (JPM payoff - NE payoff)/(Deviation payoff - JPM payoff) and it measures the scope for tacit collusion along the lines of Friedman (1971).

Table 6
Meta-study results on interaction of strategic complementarity and known end.

Dep. var.: Average degree of collusion across all rounds					
	(1)	(2)	(3)	(4)	(5)
Strategic complements	0.376***	0.017	0.052	0.045	0.082
	(0.136)	(0.226)	(0.217)	(0.207)	(0.224)
Known end	−0.015	−0.255	−0.160	−0.117	−0.150
	(0.142)	(0.184)	(0.181)	(0.173)	(0.186)
Strategic complements × Known end		0.551*	0.590**	0.552**	0.533*
		(0.279)	(0.269)	(0.256)	(0.282)
Payoff feedback			−0.283***	−0.339***	−0.272**
			(0.112)	(0.109)	(0.117)
Group size				−0.135***	
				(0.050)	
Friedman index					0.156
					(0.130)
Constant	−0.157	0.051	0.074	0.462**	−0.085
	(0.138)	(0.171)	(0.165)	(0.213)	(0.215)
R^2	0.127	0.178	0.255	0.334	0.247
Adj. R^2	0.100	0.139	0.207	0.280	0.181
N	67	67	67	67	67

Notes: The table reports results from linear regressions based on data from oligopoly experiments. The numbers of independent observations by unit of observation (treatment) are used as weights (p-values in parentheses). Stars ***, ** or * indicate that the effect of the variable is statistically significant at the 1%, 5% or 10% level, respectively.

Table 7
Meta-study results on interaction between strategic complementarity and length of game.

Dep. var.: Average degree of collusion across all rounds					
	(1)	(2)	(3)	(4)	(5)
Strategic complements	0.547***	0.704	0.750	0.573	0.556
	(0.181)	(0.511)	(0.482)	(0.457)	(0.528)
Number of periods	−0.002	−0.001	0.001	0.002	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Strategic complements × Number of periods		−0.005	−0.002	0.003	0.003
		(0.016)	(0.015)	(0.014)	(0.016)
Payoff feedback			−0.378**	−0.500***	−0.358**
			(0.142)	(0.141)	(0.151)
Group size				−0.166***	
				(0.061)	
Friedman index					0.235
					(0.182)
Constant	−0.137	0.051	−0.091	0.406*	−0.285
	(0.121)	(0.171)	(0.118)	(0.214)	(0.192)
R^2	0.178	0.180	0.287	0.385	0.283
Adj. R^2	0.145	0.129	0.226	0.318	0.197
N	52	52	52	52	48

Notes: The table reports results from linear regressions based on data from oligopoly experiments. The numbers of independent observations by unit of observation (treatment) are used as weights (p-values in parentheses). Stars ***, ** or * indicate that the effect of the variable is statistically significant at the 1%, 5% or 10% level, respectively.

that in none of the specifications there is evidence for an interaction between strategic complementarity and length of the game; the interaction term is by far not statistically significant.[23]

Finally, in order to study whether strategic complementarity only increases the degree of cooperation if players have the chance to interact with each other and react to each other's choices, we ran regressions as in Table 6 where the dependent variable is the average degree of collusion observed in first rounds, that is, before any interaction has taken place (see Table XII in the Appendix). Results are that the effect of strategic complements is not significant if not interacted with Known end ($p = 0.506$, $N = 60$), and neither is the interaction with Known end in specifications that includes an interaction ($p > 0.695$). This suggests that the above-reported positive effect of strategic complements in games with a known end stems from actual interaction having taken place, and not from a difference in initial inclination to cooperate, which is consistent with findings in our experiment.

[23] Notice that if a more robust indicator of length of game is used, for example, a dummy referring to the number of periods being higher than the median, the interaction is also far from statistically significant.

7. Discussion

In our experiment subjects play infinitely repeated dilemma games of strategic substitutes or complements. We find that under strategic substitutes choices are on average higher, so more cooperative, in the first rounds of the repeated games than under complements, and they increase across repeated games. Our data indicate that this is because under substitutes subjects more often take the risk to initiate full cooperation at the beginning of each repeated game, and they do so more frequently, the more repeated games they play. To illustrate, in the second half of the substitutes treatment the percentage of full cooperation in the first rounds has increased to a level above 20%. In contrast, under complements, subjects rarely take this risk, and the percentage remains at about 5% in the second half. The finding is in line with the notion that strategic risk has an effect on behavior in infinitely repeated games. Loosely speaking, how much a player loses by cooperating in the case the other player defects, has an impact on whether this player will choose to cooperate or not. See, for example, Blonski et al. (2011), Dal Bo and Fréchette (2011), Blonski and Spagnolo (2015) and Dal Bo and Fréchette (2017) for applications in the context of a prisoner's dilemma, and Van Huyck et al. (1990) and Schmidt et al. (2003) in the context of a coordination game. In our games, it is less risky to fully cooperate or initiate full cooperation with strategic substitutes than with strategic complements: the basin of attraction of a cooperative strategy is larger under substitutes than under complements.

Furthermore, we also find that choices under strategic substitutes do not remain higher than under complements over the course of the rounds *within* the repeated games. Indeed, across all rounds, we find no significant difference in choices between the two environments. This is due to an effect of the strategic environment that runs counter to the effect of strategic risk. In particular, if we focus on choices of subjects who do not succeed in fully cooperating, that is, who do not make joint-payoff maximizing choices, we find that, on average, choices tend to be *less* cooperative (lower) under strategic substitutes than under complements (although not statistically significantly so). Relatedly, under complements, the slope of the estimated response function is (significantly) higher than under substitutes. These findings are in line with behavior in the experiments of Potters and Suetens (2009) who find that choices are *less* cooperative under strategic substitutes than under complements, thereby confirming predictions of the literature on the interaction between the strategic environment and heterogeneity of players. This literature posits that if players are heterogeneous, aggregate outcomes tend to be more cooperative under strategic complements than under strategic substitutes (see Haltiwanger and Waldman, 1991; 1993; Camerer and Fehr, 2006).

It seems that the higher strategic risk inherent in the games of strategic complements as compared to substitutes has played a fundamental role in our experiment and not so in Potters and Suetens (2009) (PS). We hypothesize that this is due to the different "nature" of interactions. The repeated game in PS is a "long" finitely repeated game. It is played with the same partner for 30 rounds, and subjects know this. Full cooperation, if it occurs, is typically built up gradually: subjects gradually increase their choice towards the level that maximizes joint payoffs. To illustrate, in the experiment of PS, it often takes around 10 rounds to reach full cooperation. In contrast, in our experiment, the expected length of each repeated game is just 10 rounds, and subjects do not know when exactly the games end. Gradual build-up is therefore more difficult to obtain. Overall, one can argue that in our experiment players are exposed to more strategic risk than in that of PS.

In order to shed light on the role of the length of the game and the ending rule for the effect of strategic complementarity on cooperation, we performed meta-analyses based on data from oligopoly experiments. The results are threefold. First, strategic complementarity only facilitates cooperation if players know *ex ante* when the game ends, that is, if they know the exact number of rounds of the repeated game(s). In games with an unknown end, strategic complementarity has a small and statistically insignificant effect on cooperation. Second, given that players know when the game ends, there is no further interaction between the number of rounds in a repeated game and strategic complementarity. Third, strategic complementarity only increases cooperation (in games with a known end) after interaction between players has taken place: it has no significant effect on the cooperation rate in the first round, in which strategic risk is arguably highest.[24]

Next, consider our findings in relation to Embrey et al. (2018) (EMP). This paper studies experimentally the effect of strategic commitment on cooperation in infinitely repeated games of strategic complements and substitutes. Subjects choose an initial action and a strategy (a "machine") at the beginning of each repeated game. In the low-commitment treatments subjects can revise their strategy in each round for a small cost. In the high-commitment settings the costs to revise the strategy are high. Findings are that in the low-commitment setting cooperation rates are (slightly) higher under strategic complements than under substitutes, whereas in the high-commitment setting cooperation rates are lower under strategic complements than under substitutes. There are two things we would like to point out. First, it is interesting to see that EMP report an interaction effect: strategic substitutes lead to a higher (lower) cooperation rate under high (low) commitment, where high (low) commitment is arguably associated with more (less) strategic risk. Second, we find the opposite from what EMP find in the low-commitment setting, which is most comparable to our setting. Although there are several differences between both settings (e.g., in EMP the action space is limited to four actions and subjects submit strategies for the repeated

[24] We also find a positive and significant interaction effect of strategic complementarity and our payoff feedback indicator (reported in Table XIII in the Web Appendix). More precisely, in games in which players do not receive direct feedback about past earnings of their game partner(s), the effect of strategic complementarity on cooperation is not significant, whereas in games where such feedback is provided, the effect is positive and significant. Given that it is plausible to assume that strategic risk is lower in the latter than in the former case, this result also seems consistent with a strategic risk interpretation of the results.

game) we conjecture a plausible explanation for this latter difference in results is again related to a difference in strategic risk. Specifically, EMP chose games with the same "sucker" payoff across their complements and substitutes treatments, which makes their (low-commitment) setting arguably less prone to strategic risk than our setting.[25]

Overall, if we combine the evidence from our experiment with that of PS, EMP and the meta-study, a pattern seems to emerge in which strategic risk plays a key role. In particular, whether cooperation is higher under strategic substitutes or complements seems to depend on the strategic environment and key factors of experimental designs. An implication that is potentially important for policy makers is that the likelihood of collusion does not only depend on the nature of the strategic environment *per se*, but rather on the interaction between the strategic environment and strategic risk. We leave it for future research to further test this hypothesis. For example, a testable hypothesis could be that in environments where strategic risk is an important factor (for example, relatively short games or indefinitely repeated games) games with strategic substitutability are relatively more conducive to cooperation than games with strategic complementarity, in particular after learning. Moreover, in games in which strategic risk tends to be less important (for example, long repeated games or finitely repeated games), more cooperation can be expected under strategic complements than under strategic substitutes.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2021.05.004.

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[25] Note that EMP constructed their payoffs as follows. In a first step, EMP work with two versions of a differentiated-goods linear duopoly game. They then choose the parameters of the corresponding payoff functions with continuous action choices in such a way that payoffs for three benchmarks are the same across the two games (Nash equilibrium payoffs, joint payoff maximizing payoffs and the optimal deviation payoff against the other player playing the joint payoff maximizing action). They then derive two 4×4 matrices in which three of the choices correspond to the joint profit maximizing quantity/price, the Nash equilibrium quantity/price, the optimal deviation to the other player choosing the joint payoff maximizing action. In these discretized 4×4 games, the "sucker" payoff in the Comp treatment is smaller than in the Subs treatment (as in our design). However, as EMP state, they then rounded and changed some payoffs so that in the end the "sucker" payoffs in their two adjusted 4×4 games were the same.