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JEL Classification: C72, C73, C91, C92, L41

Keywords: Cooperation, repeated games, Prisoner’s dilemma, Multigame contact, Experiment

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Multigame contact and cooperation*

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July 6, 2021

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Many strategic situations involve players repeatedly interacting across multiple games. While there is an extensive experimental literature on the determinants of cooperation in indefinitely repeated games, there is no evidence on how multigame contact affects behavior in these games. In theory, multigame contact should foster cooperation by improving players' ability to punish. We study the effect of multigame contact on cooperation in two preregistered laboratory experiments, where the main treatment consists of subjects playing a pair of indefinitely repeated prisoner's dilemmas with either the same partner or with different partners, i.e., respectively with and without multigame contact. In contrast to the theoretical predictions, we find no evidence that multigame contact increases average cooperation rates. Yet, and now in line with theory, we observe that multigame contact has systematic effects on behavior: subjects often link the strategies in the two games when they play with the same partner. Such linkage turns out to be a double-edged sword: while it helps subjects reaching cooperation in both games, it makes cooperation in only one of the games less likely.

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1 Introduction

Many strategic situations involve a pair of players repeatedly interacting across multiple games. Even if two repeated games are payoff-independent, in the sense that the payoffs accruing in each stage game depend only on the actions chosen in that game, the two repeated games become connected if the players' actions in one game depend on the outcome of the other game. For instance, some coworkers are also neighbors and how loud they play their music at home may influence how they collaborate at work. Likewise, spouses that are business partners must still share household chores, and countries that are trading partners may hold different views about human rights.

Repeated interactions in a single game have been widely studied from both a theory and an experimental perspective. Most relevant to our work is the recent experimental literature on indefinitely repeated prisoner's dilemmas which studies the determinants of cooperation (see Dal Bó and Fréchette (2018) for a comprehensive overview). The main findings of this literature are that factors such as the continuation probability (Dal Bó, 2005; Duffy & Ochs, 2009; Normann & Wallace, 2012), communication possibilities (Cooper & Kühn, 2014), the monitoring structure (Aoyagi et al., 2019; Camera & Casari, 2009), costly personal punishment (Camera & Casari, 2009), continuous-time play (Bigoni et al., 2015; Friedman & Oprea, 2012), group decision making (Cason & Mui, 2019), as well as behavioral spillovers (Bednar et al., 2012) determine the extent to which players cooperate in these games.[1] None of these studies, however, examines how simultaneously playing two indefinitely repeated prisoner's dilemmas and strategically linking these games influences cooperation. This is our contribution to that literature.

We study cooperation in indefinitely repeated prisoner's dilemmas with multigame contact.[2] The theoretical understanding of the issue has been a staple in industrial economics, where it has been by now well understood that multimarket contact can increase collusive behavior among firms in pricing games that have a prisoner's dilemma structure. This idea was first formalized by Bernheim and Whinston (1990). The mechanism for deriving this result is that firms can pool their incentive constraints across markets, i.e., using the slack in the collusion incentives of one market to compensate for the lack of incentives to collude in another market. Collusive equilibria are then more easily sustainable, as captured by a reduction in the critical discount factor that equalizes the long term gains from continuing collusion with the gains from a deviating strategy.[3] Some empirical studies have found evidence which is consistent with multimarket contact leading to a less competitive environment in a variety of industries, but the endogeneity problem is a hard one to address.[4]

[1]There is also a recent literature on the behavioral effects of playing multiple finitely repeated (cooperation) games with different players at the same time (Cason et al., 2012; Falk et al., 2013; Savikhin & Sheremeta, 2013).

[2]The theoretical part of our analysis uses infinitely repeated games as workhorse. We will use the term indefinitely repeated games because that is what we can implement in the laboratory. The literature tends to use the two terms interchangeably (Dal Bó & Fréchette, 2018).

[3]With linear utility functions collusion is weakly more sustainable (Bernheim & Whinston, 1990) and becomes strongly more sustainable if utility functions are concave (Spagnolo, 1999a, 1999b).

[4]These industries include cement (Ghemawat & Thomas, 2008; Jans & Rosenbaum, 1997), telecommunications (Busse, 2000; Parker & Röller, 1997), radio (Waldfogel & Wulf, 2006), hotels (Fernandez & Marin, 1998), airlines (Ciliberto & Williams, 2014; Evans & Kessides, 1994; Miller, 2010; Singal, 1996), hospitals

We set up a laboratory experiment where each subject plays a pair of indefinitely repeated prisoner's dilemmas, which are played synchronously. Our main treatment consists in the presence or absence of multigame contact. Multigame contact is present when an agent interacts with the same partner in both indefinitely repeated prisoner's dilemmas, and it is absent when the agent faces a distinct agent in each of the two games.

In theory, multigame contact should have no effect on the critical discount factor above which cooperation is sustainable if players interact in multiple identical games.[5] For our main treatment variation to have some bite, we pair a *hard* with an *easy game* such that the incentives to deviate from the most cooperative path are higher in the former than in the latter. This implies that—in the absence of multigame contact—the critical discount factor at which cooperation is sustainable is lower in the *easy* than in the *hard game*.

In this framework, theory predicts that the effect of multigame contact varies for different discount factors. For this reason, we consider three distinct conditions with different levels of continuation probabilities such that, in theory: i) with a low a discount factor, cooperation is possible in neither of the games with and without multigame contact, ii) with a medium discount factor, cooperation is possible in both games with multigame contact (and otherwise only possible in the *easy game*), and iii) with a high discount factor, cooperation is possible with and without multigame contact.

In contrast to the theoretical predictions, we find no evidence that multigame contact facilitates overall cooperation. Yet, we should not conclude from this that multigame contact does not affect the structure of cooperation. Indeed, we find that multigame contact increases both all-out cooperation as well as all-out defection. In line with the theory, we find support for the fact that players more often link strategies across games in the presence of multigame contact. In particular, we observe that in those cases, i) subjects tend to revert to uncooperative behavior in all games in reaction to a deviation from cooperative behavior in a single game, and ii) cooperation in the *easy game* is more strongly linked with cooperative outcomes in the *hard game*. To the extent that people resort to uncooperative behavior at times, this implies that punishment occurs more often than theory would predict, and, in this particular experiment, the effect of multigame contact averages out.

This mechanism is reminiscent of the theoretical concept of contagion described by (Thomas & Willig, 2006). In a setting with imperfect monitoring, they show that strategically linking multiple games may be disadvantageous because a mistaken deviations from cooperation in one game trigger punishments with uncooperative behavior in all games. There, the losses due to this contagion outweigh the gains from strategic linkage when actions in one game are very imperfectly observed. In that case, players may want to avoid linkages if they have the possibility to do so.

To explore the mechanism further, we set up a second experiment with a sequential variant of the game, in which the cooperation enhancing effect of multigame contact is particularly strong in theory. We again find compelling evidence for linkage but no effect on overall cooperation. We conclude that multigame contact is a double-edged sword—a

(Schmitt, 2018), and banking (Coccorese & Pellecchia, 2009; Heggestad & Rhoades, 1978).

[5] While Spagnolo (1999a, 1999b) shows that the discount factor will be lower if utility is concave, for the monetary payoffs involved in a typical experimental setting, any significant reduction in the critical discount factor would require an extreme curvature of the utility function (akin to the calibration theorem (Rabin, 2000)).

benefit for some, a curse for others.

Our paper is related to the experimental literature on multimarket contact. There are a number of papers that investigate the theoretical predictions of Bernheim and Whinston (1990) in the laboratory albeit in different settings. These experiments provide mixed results: Feinberg and Sherman (1985, 1988), Phillips and Mason (1992, 1996), and Freitag et al. (2021) lend some support to the hypothesis that multimarket contact leads to more cooperation between firms while Güth et al. (2016) find no and Yang et al. (2016) find even a negative effect of multimarket contact on cooperation. Our approach differs from these studies in two important ways. First, we consider an indefinitely repeated setup with a commonly known probability of continuation of the game. A proper test of Bernheim and Whinston (1990) requires that the experimental subjects have identical beliefs regarding the possibility of future interaction when they evaluate game payoffs. None of the above studies except Yang et al. (2016) uses randomly terminated games. Second, our treatments consist of a ceteris paribus variation regarding the different contact schemes. We vary only the number of fronts on which the players interact—one versus two—while keeping all remaining aspects of the environment constant. Yang et al. (2016) compare a treatment where subjects play only one repeated prisoner's dilemma with a treatment where subjects play two repeated prisoner's dilemmas at the same time. Such a design is problematic because neither strategic linkages can be observed (they become apparent only if deviation and punishment strategies can be compared across treatments) nor can the effect of multiple contacts be disentangled from the effect of making decisions in a more complex environment. Our experimental design takes care of these shortcomings.

The article is organized as follows. Section 2 derives the theoretical predictions. Section 3 describes the experimental design. Section 4 discusses our results. Section 5 briefly concludes.

2 Theoretical predictions

In this section, we explain the theory underlying our experiment. We start by investigating the effect of multigame contact in indefinitely repeated prisoner's dilemmas when players choose their actions for both games at the same time. Later, we will examine a sequential variant of multigame contact, which is the game we use for our second experiment.

Consider the stage game in Table 1 where C stands for cooperation, and D for defection. The payoff matrix consists of four elements: the reward from joint cooperation (R), the temptation payoff earned from defection when the other player cooperates (T), the sucker's payoff from cooperation when the other player defects (S), and the punishment payoff from mutual defection (P). Under the restriction $T > R > P > S$, the game is a prisoner's dilemma with (D, D) as a unique Nash equilibrium in the stage game. Moreover, we assume $2R > T+S$ to ensure that a dynamic cooperative path with (C, C) in every period dominates a path with alternating strategies (C, D) and (D, C) across periods.

This stage game is repeated infinitely, and players discount the future with a common discount factor $\delta \in (0, 1)$. In our experiment, we interpret δ as the probability with which the game will continue into the next date t. In laboratory experiments, such games are known as indefinitely repeated games since players know the game can stop after any period with probability $(1 - \delta)$ but they cannot infer for sure how long the game will last.

Table 1: Payoff matrix of a single stage game

		Player 2	
		C	D
Player 1	C	R, R	S, T
	D	T, S	P, P

For sufficiently high discount factors, cooperation in every period can be sustained as a subgame-perfect equilibrium. The lowest δ at which cooperation is subgame perfect is achieved with the following grim trigger strategies: play C in every t, and play D forever after any deviation from (C, C). This critical threshold for δ is obtained by solving the incentive compatibility constraint below:

$$\frac{R}{1-\delta} \geq T + \frac{\delta P}{1-\delta} \Leftrightarrow \delta \geq \frac{T-R}{T-P}$$

The left-hand side of the inequality denotes the present discounted payoff from cooperation in every period while the right-hand side denotes the present discounted payoff from deviation. This critical threshold is lower the higher is R, and P, and the lower is T.

We are interested in the effect of multigame contact on cooperation. We now turn our attention to a situation where an agent plays two indefinitely repeated prisoner's dilemmas simultaneously and learns immediately the outcome of each stage game she plays. Playing simultaneously the stage games of two identical indefinitely repeated prisoner's dilemmas, with either the same or different partners, does not affect the critical discount factor at which cooperation is sustainable (Bernheim & Whinston, 1990).[6] We thus consider stage games with asymmetric payoffs, where we add a factor $z > 0$ to the temptation payoff T in one game and subtract z from T in the other one (see Table 2). Because the gain from deviating from (C, C) in the game where we added z is higher than in the game where we subtracted it, we call the former the *hard game* and the latter the *easy game*. To keep a similar incentive structure to the stage game above, we assume that $T - z > R > P > S$, and $2R > (T + z) + S$.

Table 2: Payoff matrices of the two stage games

Hard game

	C	D
C	R, R	$S, T + z$
D	$T + z, S$	P, P

Easy game

	c	d
c	R, R	$S, T - z$
d	$T - z, S$	P, P

In the absence of multigame contact, i.e., when facing a different partner in each game, a player's strategy in one game cannot affect the action of her partner in the other game.

[6]See also the discussion in the introduction.

Therefore, each game can be treated independently, and cooperation in each game is sustainable if:

$$\delta \geq \frac{(T+z)-R}{(T+z)-P} = \delta_{hard}, \quad \delta \geq \frac{(T-z)-R}{(T-z)-P} = \delta_{easy}$$

Note that cooperation is easier to sustain in the *easy* than in the *hard game*, i.e., $\delta_{easy} < \delta_{hard}$.

Consider now the situation where two players interact with each other in both the *easy* and the *hard game*, i.e., a situation of multigame contact. The two players may still sustain cooperation in each game by playing as if they faced a different partner in each game, i.e., as if the games were independent, so that the critical discount factors would still be the ones presented above. However, the two players may achieve cooperation in both games more easily if they link the strategies, as discussed next. When facing the same opponent, a player can use the threat of punishment in both games following any deviation. This threat will pool the two incentive constraints, which induces cooperation in both games if, for each player, the following incentive constraint is satisfied:

$$\frac{2R}{1-\delta} \geq (T+z)+(T-z)+\frac{\delta 2P}{1-\delta} \tag{1}$$

$$\Leftrightarrow \frac{2R}{1-\delta} \geq 2T + \frac{\delta 2P}{1-\delta} \Leftrightarrow \delta \geq \frac{T-R}{T-P} = \delta_{pool}$$

The payoff from perpetual cooperation in both games is given by the left-hand side. The payoff from defection, triggering perpetual punishment, is given by the right-hand side: as punishment is expected to occur in both games regardless of the form of deviation, a player will optimally defect in both the *easy* and the *hard game* simultaneously. Because $\delta_{pool} < \delta_{hard}$ cooperation in both games is indeed easier to sustain if strategies are linked.

The most cooperative outcomes are achieved, using grim trigger strategies, as follows: for $\delta_{pool} \leq \delta < \delta_{hard}$, players should link the strategies in the two games; for $\delta \geq \delta_{hard}$, players should cooperate in each game separately, and thus linkage becomes superfluous; for $\delta < \delta_{easy}$ players will not cooperate in either game and again linkage becomes superfluous; and finally, for $\delta_{easy} \leq \delta < \delta_{pool}$, players should not link the strategies to be able to cooperate in at least the *easy game*.

To understand the last point, notice that, if a player expects her partner to use a grim trigger strategy following a deviation in one game, her optimal response is to deviate immediately in both games since incentive constraint (1) is violated. Then, cooperation will not be achieved in either game. However, cooperation in the *easy game* alone is still achievable if players were not to link the grim trigger strategies in the two games. This point might be missed in a casual reading of the literature: while linkage may help sustain full cooperation for intermediate discount factors, it may yet destroy partial cooperation when the discount factor is sufficiently low.[7]

Now turning to our main treatment of the absence and presence of multigame contact, and focusing on the most cooperative outcome, our theoretical predictions are summarized in Figure 1 below: if $\delta < \delta_{easy}$ then in both cases, with and without multigame contact, no

[7] We are not aware of other explicit mentioning of this point with the exception of Thomas and Willig (2006), who reaches a similar conclusion in a setting with imperfect monitoring. The argument exposed here shows that the desirability to uncouple strategies does not hinge on the existence of imperfect monitoring.

cooperation is sustainable in either game; if $\delta_{easy} \leq \delta < \delta_{pool}$, then cooperation is sustainable only in the *easy game* in both cases; if $\delta_{pool} \leq \delta < \delta_{hard}$, then cooperation is sustainable in both games in the presence of multigame contact while cooperation is only sustainable in the *easy game* in the absence of multigame contact; if $\delta \geq \delta_{hard}$, cooperation is sustainable in both games in the presence and in the absence of multigame contact.[8]

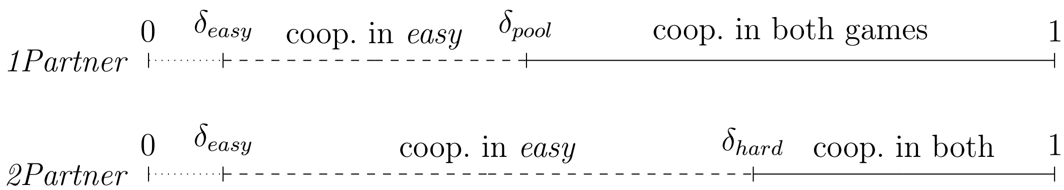

Figure 1: Most cooperative outcomes

Let us now consider the sequential variant of the game, which we use for our second experiment. In this variant, each period consists of two stages: players first interact in the *hard game* and then—knowing the outcome of that stage game—play the *easy game*. In the absence of multigame contact, cooperation is independent of playing the stage games sequentially or simultaneously, and there is no effect on the critical discount factors at which cooperation is sustainable in the *easy* and the *hard game*. However, in the presence of multigame contact, cooperation in both the *easy* and the *hard game* is achievable for lower discount factors when played sequentially rather than simultaneously. The reason is that when players can link the two games, the information on the outcome of the first game matters for how they play the second. Intuitively, cooperation becomes more easily sustainable because a partner who defects gets the temptation payoff in at most one game, i.e., she gets a lower benefit from deviation than in simultaneous play. Moreover, the punishment is weakly harsher: it is harsher if she deviates in the *hard game* (as then she will see her punishment start immediately within the same period in the *easy game*), and it is the same if she deviates later only in the *easy game* (as then the punishment is the same as in simultaneous play). These considerations alter the incentive constraint and make cooperation more easy to sustain. In particular, cooperation in both games is sustainable for

$$\delta \geq \begin{cases} \frac{T+z+P-2R}{T+z-P}, & \text{if} \quad 2z+P-R \geq 0, \\ \\ \frac{T-z-R}{T-z+R-2P}, & \text{otherwise.} \end{cases}$$

Moreover, with multigame contact, the critical discount factor at which cooperation in both games can be sustained in this sequential game is even lower than the critical discount factor for the *easy game* alone. Thus, in order to achieve the most cooperative outcome, with simultaneous play linkage is required for δ sufficiently high but will hurt for δ sufficiently low (meaning that linkage may need to be avoided), while with sequential play there is no region where linkage should be avoided.

[8]The qualitative predictions are identical using risk dominance instead of subgame perfection as an equilibrium selection criteria. See Table A.1 for the critical discount factors that emerge with the payoffs used in our experiments.

3 Experimental procedures

Subjects play a sequence of indefinitely repeated prisoner's dilemmas. In every round, they play two prisoner's dilemma games in parallel. Table 3 presents the stage game payoffs. Henceforth, we will refer to the *hard game* as *hard* and to the *easy game* as *easy*.[9] We call the combination of both indefinitely repeated prisoner's dilemmas a supergame. The first three rounds of a supergame are played for sure and at the end of the third round, a computerized stopping rule is introduced. From round three onward, the supergame either continues (with the continuation probability δ), or the computer stops the supergame. We differ from the standard approach in which only the first round is played for sure. Adding two guaranteed rounds of play enables us to observe how subjects deviate and react to deviations. This is especially helpful for low and intermediate continuation probabilities, where longer supergames are very rare. Indeed, at least three rounds are required to observe the effect of a deviation. In a round t, subjects have to cooperate, a deviation occurs in round $t+1$, and the reaction is observed in round $t+2$. For example, at least 50% of the indefinitely repeated games would not reach the third round if $\delta < 0.71$ if the probabilistic continuation would be in place right from the start.[10] After termination of a supergame, subjects are randomly rematched for the subsequent supergame. All this information is common knowledge to subjects.

The subjects within a session are randomly allocated to matching groups. At the beginning of each supergame, the computer randomly matches subjects with one or two partners depending on the treatment.[11]

Table 3: Payoff matrices of the experimental stage games

hard	C	D		*easy*	c	d
C	135, 135	45, 216		c	135, 135	45, 144
D	216, 45	60, 60		d	144, 45	60, 60

The main treatment variation manipulates multigame contact: Within a supergame, subjects either interact in both games with one partner (multigame contact, henceforth

[9]For half of the subjects, *hard* is always displayed on the left of the screen and *easy* on the right. The order is reversed for the other half. We use neutral labels (A, B and X, Y) for the actions in the games. We randomize by subject whether they see A, B or X, Y as labels for *hard* or *easy*.

[10]It is true that adding two certain rounds may alter subjects' perceptions of the true continuation probability. However, our main interest is in the effect of multigame contact and not in precisely estimating cooperation rates at different continuation probabilities. From a theoretical perspective the guaranteed rounds should not matter, as adding any finite number of rounds before introducing the random stopping rule does not affect the incentive to cooperate in round one and two by backward induction.

[11]Matching groups comprise 6 to 20 subjects. Subjects are only matched with other subjects in the same matching group. The matching group sizes are chosen such as to keep the expected number of times a subject interacts with another subject comparable across treatments. Labels do not allow subjects to identify with whom they interact. The other subject in the game is always labelled as "Your partner" in the treatments with one partner, and "Your partner 1" and "Your partner 2" in the treatments with two partners. At the beginning of each supergame, we inform subjects that a new partner or new partners 1 and 2 are drawn.

1Partner) or they play *hard* with one subject and *easy* with another subject from the matching group (no multigame contact, henceforth *2Partner*). The matching is fixed within the supergame. The second treatment variation is the expected length of the supergames: We implement three different continuation probabilities $\delta \in \{0.1, 0.5, 0.9\}$.

We run our two by three factorial design as a between subject design, i.e., subjects play only one of the six treatments. All subjects in a session play the same treatment. When the total number of rounds reaches 100 the experiment continues with the current supergame until this is terminated by the random stopping rule. Supergames within a matching group all stop and start at the same time but matching groups within a session go through an independent sequence of supergame durations. In order for all the matching groups of a session to finish the experiment at the same time, we add a finite supergame for matching groups, which are not last to finish. Data from these finite supergames are not part of the analysis.[12]

Participants were paid out the sum of the payoffs of all rounds. We measured earnings in points during the experiment and the exchange rate was 1000 points = 1 CHF ($\approx$ 1.10 USD). In addition, participants received a show-up fee of 10 CHF ($\approx$ 11 USD). Sessions were run in the laboratory of the University of Lausanne (LABEX) with undergraduate students from the University of Lausanne and the EPFL recruited with ORSEE (Greiner, 2015). The experiment was programmed in oTree (Chen et al., 2016).

A total of 436 subjects participated in the experiment. Table A.2 in the appendix provides detailed information about the observations per treatment. The average payment per participant was 31 CHF ($\approx$ 34 USD) and sessions lasted between 70 and 113 minutes. The design of the experiment and the hypotheses were preregistered prior to the data collection in an OSF registry.

4 Results

This section is organized in three parts. First, we test our preregistered hypotheses. Second, we perform an exploratory analysis of the data, which leads to new hypotheses. Finally, we present the design and results from the second experiment.

4.1 Cooperation rates across treatments

Given the monetary payoffs and the continuation probabilities we can predict cooperation rates in our experiment. In case of $\delta = 0.1$ the theory predicts no cooperation in *1Partner* and *2Partner*, for $\delta = 0.9$ cooperation can be part of an equilibrium in both cases and in both games. The beneficial effects of multigame contact should appear, however, in $\delta = 0.5$. While cooperation in *easy* is sustainable in both treatments, cooperation in *hard* should only be possible in *1Partner*.

Figure 2 shows mean cooperation rates and 95 percent confidence intervals in *hard* and *easy* for each treatment. The left panel contains data for all the rounds, whereas the right

[12] Rather than drawing a random number after each round for the stopping rule, all supergame durations are randomly drawn from a geometric distribution at the beginning of the session. This allows us to fix the duration of the finite game for each matching group beforehand.

panel only contains data for the last round in each supergame. Because of the random termination rule, subjects cannot infer for sure when the last round will take place in the supergame. Looking at the last round in the supergame is interesting because it gives us a sense to which decisions subjects converge to.

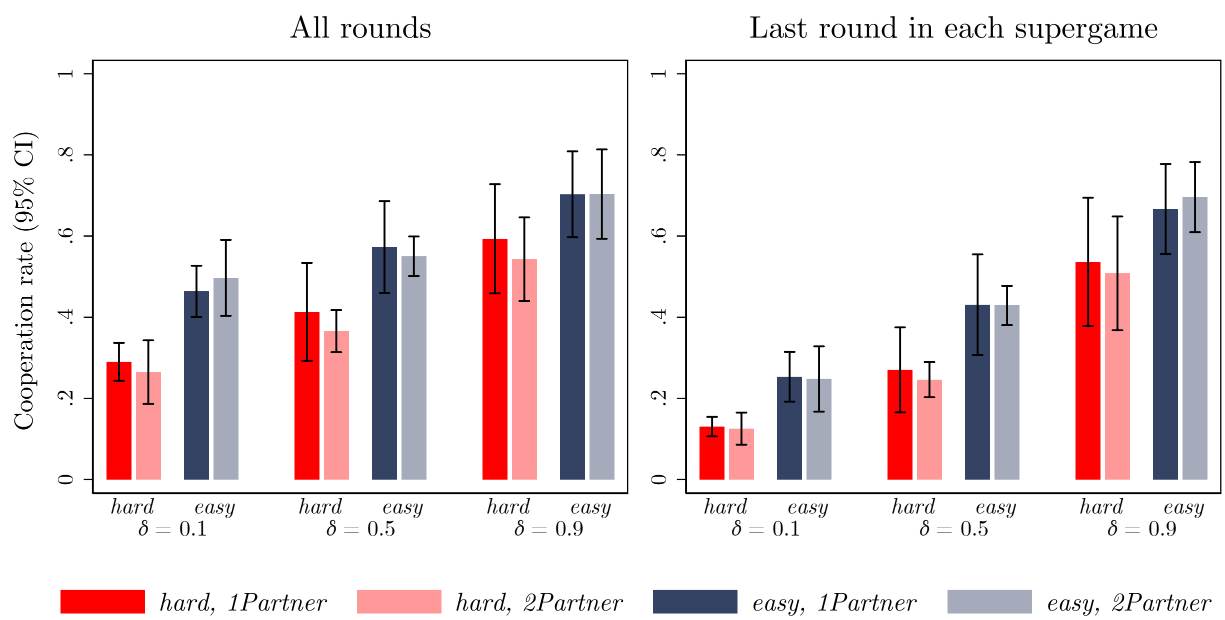

Figure 2: Cooperation rates by treatment. Mean cooperation rates and 95% confidence intervals are computed using the matching group averages. The left panel contains all the rounds; the right panel shows the last round in each supergame.

In line with the literature (Dal Bó & Fréchette, 2018) we observe an increase in cooperation for both games and for both partner treatments as the continuation probability increases. This is especially true when looking at the last supergame round. Comparing $\delta = 0.1$ to $\delta = 0.5$ and $\delta = 0.5$ to $\delta = 0.9$, we find large and statistically significant differences for both partner treatments.[13]

Comparing *1Partner* and *2Partner*, we find no consistent difference in the average cooperation rates. In particular, contrary to the theoretical predictions, we do not observe a systematic increase in cooperation in the hard game for $\delta = 0.5$. This is true for both when we look at all rounds in a supergame, or at the last round in each supergame.[14]

While a strict reading of the theory does not predict differences in any of the other comparisons, it is clear that we should not take these results too literally. After all, we observe quite some cooperation at $\delta = 0.1$, as well as defection in $\delta = 0.9$. Given this variation, it seems reasonable to suspect that the general mechanism of multigame contact should also

[13] $p < .05$ for all comparisons except the difference between $\delta = 0.1$ and $\delta = 0.5$ in *hard* for *1Partner* ($p = .054$), Wilcoxon rank-sum tests on matching group averages.

[14] Figure A.1 in the appendix shows that this also holds when we split the supergame between the first two rounds and from round three onward.

affect behavior in the remaining comparisons.[15] However, none of the comparisons between *1Partner* and *2Partner* is anywhere close significance in our data. This holds also for overall cooperation, i.e., pooling the actions of *hard* and *easy* ($p > .522$ at any δ, Wilcoxon rank-sum tests). This leads to our first result:

Result 1: Multigame contact does not affect overall cooperation.
Cooperation rates in both hard and easy are statistically indistinguishable between 1Partner and 2Partner, and the same holds for overall cooperation.

However, merely observing average cooperation rates in *hard* and *easy* does not tell us much about whether subjects link the two games when playing with the same partner. A first indication may come from the fact that, at least for $\delta = 0.5$, the confidence intervals shown in Figure 2 are substantially larger with multigame contact. Next, we take a closer look at individual decisions. Because we have no further preregistered hypotheses beyond the effects on average cooperation rates seen above, Section 4.2 has an exploratory character. This leads to new hypotheses which we test in Section 4.3, this time in a preregistered experiment.

4.2 Strategic linkage

The prediction that multigame contact helps to sustain simultaneous cooperation in both games relies on the assumption that players link the two games when matched with a single partner. Even though we find no difference in average cooperation across the partner treatments, subjects' behavior may still react to the treatment.

If subjects link the two games under multigame contact we should observe different reactions to the partner(s)' previous decisions between *1Partner* and *2Partner*. To better understand if and how subjects' reactions differ across treatments, we first restrict our attention to situations where theory helps us explain observed behaviors. Specifically, we look at how previously cooperative subjects react to different outcomes in the previous round.

Figure 3 shows heatplots with the decisions of a subject's partner(s) in the round $t-1$ of a supergame on the horizontal axis and the subjects' reaction in t on the vertical axis. Each label contains one or two action pairs referring to the decision in *hard* (capital letter) and *easy* (lowercase). To simplify matters we pool the action pairs Cd and Dc. The partner(s)' decisions are from the point of view of the subject. In *1Partner*, those are the decisions of her partner whereas for *2Partner*, the first letter is the decision of her partner in *hard* and the second is the decision of her partner in *easy*.

Each cell reports the mean proportion of the corresponding reaction (in percent) conditional on the decision of the partner(s). Cells within a column add up to 100.

To investigate whether subjects in *1Partner* link the two games, we will focus our attention on the middle column of each heatplot. This column shows how a subject having played Cc reacts after facing Cd or Dc. At any continuation probability, the reaction (Cd/Dc) is clearly the modal response in *2Partner*. Thus, the most common reaction is to treat the two games separately. In contrast, for *1Partner* we observe that the reaction Dd (deviation in

[15] Bruttel (2009) and Dal Bó and Fréchette (2018) argue that—rather than a stepwise increase—the distance between the implemented discount factor and the critical discount factor (δ^*) is a continuous predictor of cooperation. For discount factors below δ^* the cooperation rates are typically at a low level, while above δ^* cooperation gradually increases in δ.

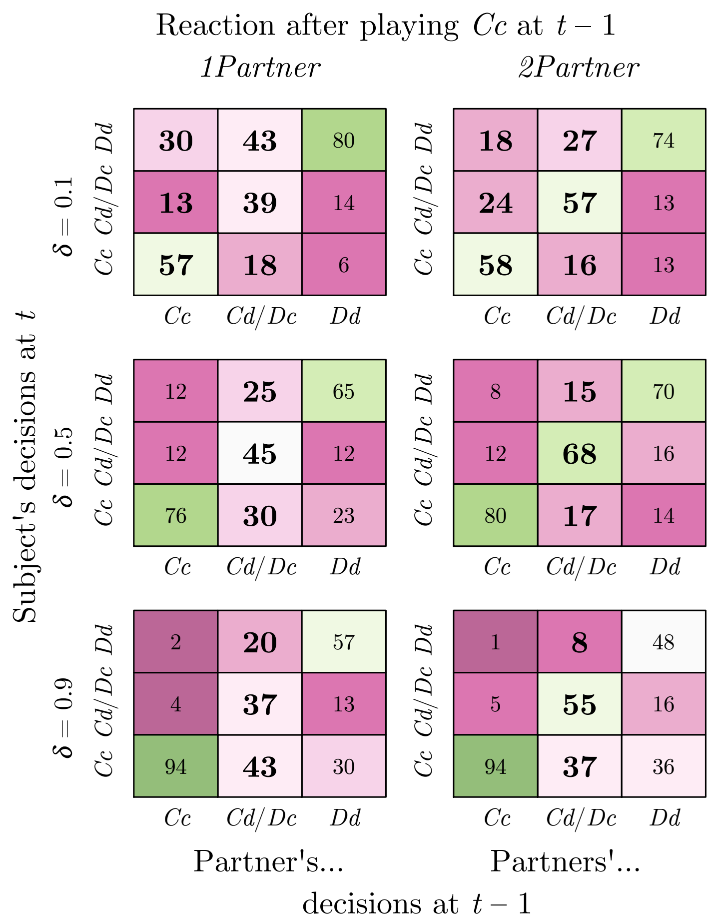

Figure 3: Reactions across treatments in percent. Partner's or partners' decisions in round $t-1$ of a supergame on the horizontal axis and the subject's reaction in (t) on the vertical axis. We restrict our attention to rounds where the subject played Cc in round $t-1$. Bold numbers indicate that the Pearson's χ^2 test for the differences in the reactions to a given partner's decisions between *1Partner* and *2Partner* are statistically significant ($p < .05$). The coloring indicates frequencies, going from purple for values close to 0 up to green for values close to 100.

one game is punished in both games) is substantially more frequent. Interestingly, the same holds for the forgiving action pair Cc. For all three δ these differences reach significance (indicated by the bold numbers in Figure 3).

When subjects face Cc or Dd in $t-1$ (first and third columns of each graph), we do not see important differences in the reactions between *1Partner* and *2Partner*. The only exception reaching significance is the reaction after having faced Cc at $\delta = 0.1$. In this case, subjects in both partner treatments are as likely to react with full cooperation (Cc), but we observe almost twice as often defection in both games (Dd) in *1Partner*. This is again in line with the concept of linkage, since a subject who wishes to deviate in *1Partner* should anticipate her partner's reaction Dd and should thus deviate in both games in the first place. This difference is not observed for higher continuation probabilities because the modal reaction to Cc is by a large margin to maintain cooperation (Cc).

In a next step, we will provide a more general test for linkage. In a broad sense, linkage between the two games can be understood as mutual dependence of the actions between the two games. There is no obvious reason to link the two games in *2Partner*. In *1Partner*, on the other hand, links can go in both directions. The strongest link is presumably the willingness to cooperate in the hard game. If two subjects are willing to maintain cooperation in this game, they should find it easy to cooperate in the easy game as well. On the other hand, if subjects do not link the two games, then the predominant determinant of cooperation should be the outcome of the particular game in the previous round. We investigate these effects in a regression analysis.

Table 4: Linkage across games

	DV: cooperation in *easy* (c_t)		
	(1)	(2)	(3)
$\delta = 0.1$	-0.114^{**}	-0.106^{**}	-0.108^{**}
	(0.040)	(0.017)	(0.018)
$\delta = 0.9$	0.216^{**}	0.103^{**}	0.100^{**}
	(0.044)	(0.015)	(0.015)
2Partner	0.007	0.004	-0.023
	(0.036)	(0.015)	(0.025)
$(C,C)_t$		0.180^{**}	0.296^{**}
		(0.019)	(0.026)
$(C,C)_t$ × *2Partner*			-0.162^{**}
			(0.031)
$(c,c)_{t-1}$		0.454^{**}	0.358^{**}
		(0.023)	(0.037)
$(c,c)_{t-1}$ × *2Partner*			0.130^{**}
			(0.043)
Constant	0.369^{**}	0.248^{**}	0.267^{**}
	(0.039)	(0.016)	(0.022)
Time controls	Yes	Yes	Yes
χ^2-test	462.4	3501.9	3901.7
p	0.000	0.000	0.000
R^2	0.109	0.398	0.401
N	44,834	33,964	33,964

Notes: Random effects estimates. Dependent variable is cooperation in *easy*. Independent variables are dummies for the continuation probability and a dummy for the treatments with two partners (with one partner and $\delta = 0.5$ as baseline case). $(C,C)_t$ indicates a cooperative outcome in *hard*; $(c,c)_{t-1}$ indicates a cooperative outcome in *easy* in the previous round of the supergame. Time controls are dummies for the first and second round of the supergame and the supergame round, as well as the overall round in the experiment. Robust standard errors, clustered on matching group, in parentheses. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table 4 shows linear probability models with random effects at the subject level and robust standard errors clustered on the matching groups. The dependent variable, c_t, is a binary variable taking the value one if a subject cooperates in *easy*. In Model (1) we regress c_t on the exogenous treatment variables only. The baseline is *1Partner* and $\delta = 0.5$. Cooperation in *easy* clearly increases with the continuation probability, and the coefficient estimate for $2Partner$ confirms the lack of differences in cooperation rate in *easy* between the partner treatments. In Model (2) we add two dummy variables: $(C, C)_t$ takes the value one if the cooperative outcome in *hard* is reached in the same round and $(c, c)_{t-1}$ takes the value one if the cooperative outcome in *easy* was reached in the previous round. Both variables predict a higher likelihood to cooperate in *easy*. Model (3) is our model of interest. In addition to Model (2), we add the interaction between $(C, C)_t$ and *2Partner* and another interaction between $(c, c)_{t-1}$ and *2Partner*. The coefficient estimate for $(C, C)_t$ remains statistically significant and increases compared to Model (2), indicating that for *1Partner* cooperation in *easy* strongly increases when subjects reach the cooperative outcome in *hard*. To our interest, the interaction term between $(C, C)_t$ and *2Partner* is negative and statistically significant. This means that the link between the outcome of *hard* and cooperation in *easy* is much weaker in *2Partner*. The reverse is the case considering $(c, c)_{t-1}$ and its interaction term with *2Partner*. The link between a cooperative outcome in *easy* in the previous round and the likelihood to cooperate in *easy* in the current round is much stronger for *2Partner* than for *1Partner*.[16] Both the conclusions from these linear probability models and from our investigation on subjects' reaction in Figure 3 led us to conclude the following:

Result 2: Under multigame contact subjects strategically link the two games.
In the presence of multigame contact, defection of other subjects is more likely to provoke full defection in response. The link between outcomes of the hard game and cooperation in the easy game is much stronger in the presence of multigame contact.

To summarize, we find no support for our hypothesis that multigame contact increases cooperation in *hard* (at $\delta = 0.5$ or any other continuation probability), but we find strong evidence for linkage in *1Partner* at all three continuation probabilities. In line with Bernheim and Whinston (1990), this often enables subjects to reach fully cooperative outcomes. However, frequent linkage leads subjects towards fully defective situations as well, which prevents linkages from producing clear overall benefits. This suggests that multigame contact is a double-edged sword—a blessing for some and a curse for others. As these results stem from an exploratory analysis we went back to the laboratory in order to provide further evidence on linkage under multigame contact and its effect on outcomes.

4.3 Powering multigame contact through sequential play

For our second experiment we decided to make a small but theoretically powerful change to the stage game: Instead of playing the two games simultaneously, subjects take their decision

[16] $(C, C)_t$ and $(c, c)_{t-1}$ are almost surely correlated, especially in *1Partner*, meaning the coefficient estimates in these models may be biased. To account for this correlation issue, we run Model (3) adding two interaction terms: $(C, C)_t \times (c, c)_{t-1}$ and $(C, C)_t \times (c, c)_{t-1} \times$ *2Partner*. The coefficients are very similar and our conclusion do not change.

sequentially. Subjects first choose their action in the *hard game*, after which they learn the outcome of the *hard game* and proceed to the *easy game*.

Recall from the theory in Section 2 that in *2Partner* playing the two games sequentially rather than simultaneously within a round has no impact on the sustainability of cooperation in theory. In *1Partner*, the sequential variant of the game should make cooperation in *hard* easier to sustain compared to the simultaneous variant. The reason is that defection is less profitable in the sequential game. While a defecting player can reap the temptation profit of both games when played simultaneously, this is not possible anymore. A player who wants to defect has two options. Either she deviates in *hard* and trigger strategies imply that the players revert to the stage Nash equilibrium already in *easy*, or she keeps cooperating in *hard* in order to deviate in *easy*. With our particular payoffs, waiting to deviate in *easy* is the optimal defection. This lowers the critical discount factor for cooperation in both games in *1Partner* relative to the simultaneous game.

Apart from making the stage game a sequential game, the second experiment uses identical procedures as the first experiment. We restrict our attention to $\delta = 0.5$, the case in which multigame contact matters most according to theory.[17] The order of events within a round is the following: Subjects take their decision simultaneously in *hard*, they are informed about the outcome in *hard*, they take their decision in *easy*, and are finally informed about the outcome in *easy*.[18] For both *1Partner* and *2Partner* we have data from six matching groups. A total of 128 subjects participated in this second experiment. Table A.3 in the appendix provides detailed information about the observations per treatment. We use the same subject pool but excluded subjects who did participate in the first series of sessions. The design of this second experiment and the hypotheses were preregistered anew prior to the data collection in an OSF registry.

Figure 4 shows mean cooperation rates and 95% confidence intervals. The left panel contains data for all the rounds whereas the right panel only contains data for the last round in each supergame. Within each panel, the four bars on the right are the results from the second experiment. For comparison we include the four bars on the left with the results of the first experiment for $\delta = 0.5$. Looking at the left panel, the strongest difference we observe between simultaneous and sequential games is the increase in cooperation rates in *hard* for *1Partner* (from 0.42 to 0.59, $p = .030$, Wilcoxon rank-sum test). Cooperation in *easy* for *1Partner* hardly changes (from 0.57 to 0.61) and is now almost indistinguishable from the cooperation rate in *hard* (0.59 and 0.61). For *2Partner*, both cooperation in *hard* and *easy* increase by 9 percentage points.[19] All these results also hold when looking at the last round in each supergame. From these results we conclude that the biggest effect of moving from simultaneous to sequential games is, as expected, the increase in cooperation in *hard* for *1Partner*. In line with our theoretical arguments, we observe very similar levels of

[17] To maximize comparability between the first and second experiment we do not generate the supergame lengths on the spot but use the realizations of the six matching groups of the first experiment at $\delta = 0.5$ in *1Partner* for all treatments.

[18] The screens on the computer have the same structure as in the first experiment. What changes is that at the beginning of the round the part of the screen for *easy* is shaded and inactive. This part of the screen becomes active when subjects have to take their decision in *easy*. The other part of the screen keeps displaying the results in *hard*.

[19] $p = .087$ in *hard* and $p = .117$ in *easy*, Wilcoxon rank-sum tests on matching group averages.

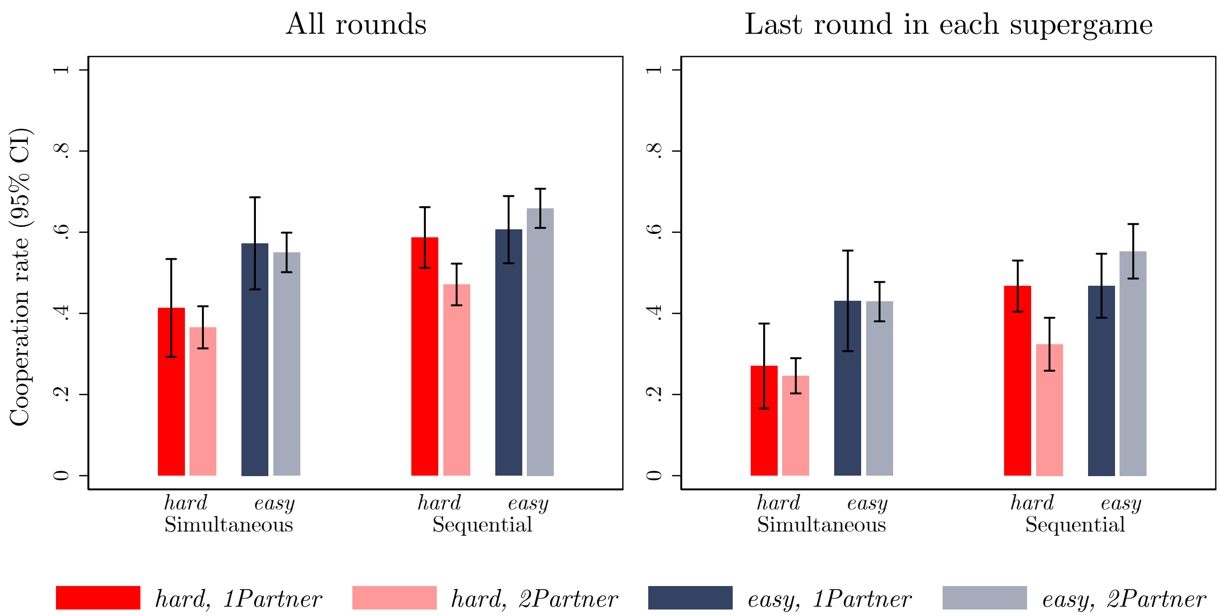

Figure 4: Cooperation rates by treatment ($\delta = 0.5$ only). Mean cooperation rates and 95% confidence intervals are computed using the matching group averages. The left panel contains all the rounds; the right panel shows the last round in each supergame.

cooperation in *hard* and *easy* for *1Partner*. The cooperation rate in *hard* is now significantly larger in *1Partner* than in *2Partner* ($p = .026$, Wilcoxon rank-sum test). Although the results of this experiment are closer to our theoretical predictions, cooperation rates have also increased in *2Partner* where cooperation in *easy* is even somewhat higher than in *1Partner*. Similar to the simultaneous sequence, we find no statistically significant difference in overall cooperation rates (0.60 vs 0.57, $p = .521$, Wilcoxon rank-sum test). We will postpone the question as to whether multigame contact improves subjects' payoffs and leads to more cooperative outcomes to the next section and first investigate the evidence for linkage in the sequential game.

Table 5 shows linear probability models with the decision to cooperate in *easy* in round t (c_t) as the dependent variable. As in Table 4, the explanatory variables are a dummy for the cooperative outcome in *hard* in the contemporary round, $(C, C)_t$, a dummy for the cooperative outcome in *easy* in the previous round, $(c, c)_{t-1}$, and two interaction terms for the dummies with *2Partner*. Since *hard* is played before *easy*, subjects already know the outcome in *hard* before taking their decision in *easy*. Comparing the coefficients with the results from Table 4 suggests that the reaction to a cooperative outcome in *hard* is considerably stronger in *1Partner* of the sequential game (0.519 vs. 0.296). The highly significant and negative interaction term indicates that the link between the hard and easy game is a lot smaller for *2Partner* ($0.519 - 0.408 = 0.111$). Again, having reached a cooperative outcome in *easy* in the previous round has a positive effect on cooperation in *easy* in the following round, and this effect is stronger in *2Partner*.

Table 5: Linkage in the sequential games

	DV: cooperation in *easy* (c_t)		
	(1)	(2)	(3)
2Partner	0.052	0.073**	0.010
	(0.044)	(0.018)	(0.037)
$(C,C)_t$		0.205**	0.519**
		(0.046)	(0.046)
$(C,C)_t$ × *2Partner*			−0.408**
			(0.046)
$(c,c)_{t-1}$		0.492**	0.186**
		(0.048)	(0.054)
$(c,c)_{t-1}$ × *2Partner*			0.383**
			(0.066)
Constant	0.487**	0.140**	0.196**
	(0.057)	(0.023)	(0.028)
Time controls	Yes	Yes	Yes
χ^2-test	383.1	1574.0	2951.2
p	0.000	0.000	0.000
R^2	0.081	0.433	0.463
N	13,076	9,796	9,796

Notes: Random effects estimates. Dependent variable is cooperation in *easy*. Independent variables are a dummy for the treatments with two partners (with one partner as baseline case); $(C,C)_t$ indicates a cooperative outcome in *hard*; $(c,c)_{t-1}$ indicates a cooperative outcome in *easy* in the previous round of the supergame. Time controls are dummies for the first and second round of the supergame and the supergame round, as well as the overall round in the experiment. Robust standard errors, clustered on matching group, in parentheses. $^+$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

To conclude, our second series of experiments strongly confirm that subjects link the two games in the presence of multigame contact. While multigame contact seems to help establish cooperation in the hard game, overall the results of Figure 4 still do not suggest dramatic improvements in terms of cooperation. In order to study the overall effect we combine the data sets from the first and second experiment and turn our attention to the payoffs, arguably the most relevant metric to gauge whether multigame contact is in the interest of the parties involved.

4.4 Payoffs and outcomes

Table 6 shows random effects regressions for payoffs. The dependent variable is an individual's payoff from both the *hard* and *easy* game in a given round. The first two models present the results for the simultaneous and sequential games separately. In all models we control for time effects with dummies for the first and second round, as well as a linear time trend afterwards. In both of the separate regressions the treatment dummy *2Partner* is far from significant, and the point estimate is very small when compared to the average payoff of the baseline case (i.e., the constant). The two dummies for the discount factors (with $\delta = 0.5$ as the baseline case) have the expected sign and are highly significant. Moving to the estimates on the full sample shows that the sequential variant enables subjects to realize somewhat higher payoffs, while the interaction with *2Partner* suggests that this holds irrespective of multigame contact. Finally, in the rightmost model we add a dummy for the second half of the supergames, again including the interaction with the dummy *2Partner*. Both coefficients do not reach significance and are very small, indicating that support for the beneficial effects of multigame contact does not arise with experience.

While multigame contact seems to have no effect on overall payoffs, our data provides strong evidence for the double-edged sword. Table 7 shows the frequencies of fully cooperative and fully non-cooperative outcomes, as well as intermediate outcomes. The left part of the table shows the results of the *1Partner* treatment ($\delta = 0.5$ only), the right part those of the *2Partner* treatment. In the experiment with simultaneous play we observe a cooperative outcome in both or neither game in 82 percent of the cases in *1Partner*, relative to 67 percent in *2Partner*. Conversely, the two intermediate outcomes (cooperative in only one of the two games) are more frequent when the two games are played with different partners. The differences between *1Partner* and *2Partner* are significant ($p = .031$, Pearson's χ^2-test with robust standard errors, clustered on matching group). The right part of Table 7 shows that we find qualitatively the same for the sequential variant of the game, but the effect is considerably stronger. With multigame contact, we observe 90 percent of the outcomes in the two extreme categories, compared to 63 percent for the *2Partner* treatment. The differences in the distribution of outcomes is highly significant ($p = .000$).

Result 3: Multigame contact is a double-edged sword.
Subjects in 1Partner are significantly more likely to cooperate in both or neither game, whereas subjects in 2Partner are more likely to realize partially cooperative outcomes. On average, multigame contact fails to increase cooperation.

Table 6: Multigame contact and payoffs

	DV: Payoff			
	Simultaneous	Sequential	Both	Both
2Partner	-2.412	-3.403	-2.392	-2.707
	(5.137)	(5.980)	(5.110)	(5.217)
$\delta = 0.1$	-20.044^{**}		-20.009^{**}	-19.999^{**}
	(5.801)		(5.788)	(5.810)
$\delta = 0.9$	33.684^{**}		34.222^{**}	34.006^{**}
	(6.232)		(6.290)	(6.311)
Sequential			16.321^{*}	16.342^{*}
			(7.401)	(7.410)
Sequential × *2Partner*			-1.065	-1.005
			(7.705)	(7.714)
2nd half				2.846
				(3.050)
2nd half × *2Partner*				0.537
				(4.491)
Constant	179.295^{**}	219.219^{**}	179.926^{**}	178.581^{**}
	(6.086)	(7.490)	(6.060)	(5.745)
Time controls	Yes	Yes	Yes	Yes
χ^2-test	561.9	670.9	825.1	1095.8
p	0.000	0.000	0.000	0.000
R^2	0.104	0.085	0.105	0.105
N	44,834	13,076	57,910	57,910

Notes: Random effects estimates. Dependent variable is individual payoff from both games in a round. Independen variables are dummies for the continuation probability and a dummy for the treatments with two partners (with one partner, $\delta = 0.5$, and simultanous as baseline case). 2nd half is a dummy for the second half of the supergames played. Time controls are dummies for the first and second round of the supergame and the supergame round. Robust standard errors, clustered on matching group, in parentheses. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table 7: Outcome of the stage games, $\delta = 0.5$

	Simultaneous		Sequential	
	1Partner	*2Partner*	*1Partner*	*2Partner*
Cooperative outcome …				
…in *hard* and *easy*	0.24	0.14	0.41	0.23
…only in *hard*	0.01	0.07	0.03	0.08
…only in *easy*	0.17	0.26	0.07	0.29
…in neither	0.58	0.53	0.49	0.40
N	5,924	11,574	4,496	8,580

Notes: Share estimates of the outcome of the stage games using all rounds.

5 Conclusion

Should you open a business with your spouse? According to a compelling and well-established theoretical argument the answer is yes. Interacting on multiple fronts should enable you to establish and maintain cooperation in situations where incentives to deviate are particularly strong. The trick is to link the games and use the slack in the incentive constraint of one game to enforce cooperation in the other.

Our empirical results suggest that things are more complicated when experimental subjects are exposed to multigame contact. In our treatments with $\delta = 0.5$, and especially when subjects first interact in the *hard game*, the difference in the critical discount factor for fully cooperative equilibria differ widely between multigame and single game contact. Given that a large experimental literature documents systematic variations in δ for standard prisoner's dilemma games, it is surprising that subjects do not seem to be able to realize the benefits of multigame contact. This points to a shortcoming of our theoretical argument. Linking the two games in the presence of multigame contact is beneficial as long as the players remain on the equilibrium path. Off the equilibrium path, linkage seems to produce adverse effects, because subjects cannot even cooperate in games where this should be easy. This led us to conclude that multigame contact is a double-edged sword.

Our results shed light on both the beneficial and detrimental effects of linking the actions in one game to the outcome of the other game. However, it remains a conundrum why the negative effects of linkage cannot be avoided. After all, if linking the two games leads to unfavorable outcomes, rational players are free to unlink the two situations. In other words, what is possible with single game contact should be possible with multigame contact as well (and more).

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Appendix

Table A.1: Cooperation as a Subgame-Perfect Equilibrium (SPE) and a Risk Dominant (RD) Strategy

Continuation prob.	$\delta = 0.1$	$\delta = 0.5$	$\delta = 0.9$
($\delta^{SPE}_{easy} = 0.11$, $\delta^{RD}_{easy} = 0.24$, $\delta^{SPE} = 0.38$, $\delta^{RD} = 0.44$, $\delta^{SPE}_{hard} = 0.52$, $\delta^{RD}_{hard} = 0.56$)			
- 1 partner			
- *easy* game	✗ SPE ✗ RD	✓ SPE ✓ RD	✓ SPE ✓ RD
- *hard* game	✗ SPE ✗ RD	✓ SPE ✓ RD	✓ SPE ✓ RD
- 2 partners			
- *easy* game	✗ SPE ✗ RD	✓ SPE ✓ RD	✓ SPE ✓ RD
- *hard* game	✗ SPE ✗ RD	✗ SPE ✗ RD	✓ SPE ✓ RD

Table A.2: Summary of the sessions (simultaneous)

	$\delta = 0.1$		$\delta = 0.5$		$\delta = 0.9$	
	1Part.	*2Part.*	*1Part.*	*2Part.*	*1Part.*	*2Part.*
# sessions	3	6	3	6	2	3
# matching groups	6	6	6	6	5	5
# subjects	60	116	58	114	34	54
avg. # supergames by subject	33	33	26	25	10	9
total # decisions in each game	6030	11754	5924	11574	3752	5800

Table A.3: Summary of the sessions (sequential)

	$\delta = 0.5$	
	1Part.	*2Part.*
# sessions	3	6
# matching groups	6	6
# subjects	44	84
avg. # supergames by subject	26	26
total # decisions in each game	4496	8580

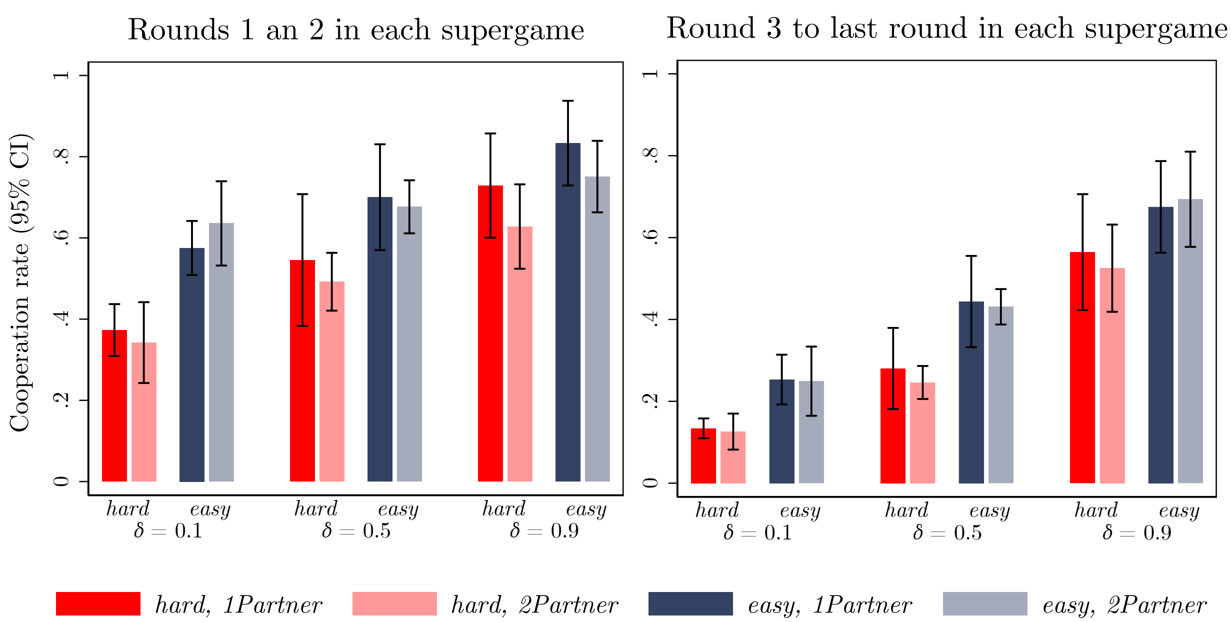

Figure A.1: Cooperation rates by treatment: Cooperation rates refer to the share of participants who choose to cooperate. Matching groups are the unit of observation and we assume independence between them. Mean cooperation rates and 95% confidence intervals are computed using the matching group averages. The left panel restricts to rounds 1-3 in each supergame and the right panel contains round 4 to the last round in the supergame.

Table A.4: Outcome of the stage games by treatment

	$\delta = 0.1$		$\delta = 0.5$		$\delta = 0.9$	
	1Part.	*2Part.*	*1Part.*	*2Part.*	*1Part.*	*2Part.*
Cooperative outcome …						
… in *hard* and *easy*	0.10	0.07	0.24	0.14	0.46	0.32
… only in *hard*	0.02	0.04	0.01	0.07	0.01	0.09
… only in *easy*	0.17	0.26	0.17	0.26	0.14	0.27
… in neither	0.71	0.63	0.58	0.53	0.39	0.33
N	6,030	11,754	5,924	11,574	3,752	5,800

Notes: Share estimates of the outcome of the stage games using all rounds. Pearson's χ^2-test with robust standard errors, clustered on Unique number for each group, for the difference between *1Partner* and *2Partner* yields $p = .170$ at $\delta = 0.1$, $p = .031$ at $\delta = 0.5$, and $p = .060$ at $\delta = 0.9$.

Table A.5: Outcome of the stage games in the last supergame round by treatment

	$\delta = 0.1$		$\delta = 0.5$		$\delta = 0.9$	
	1Part.	*2Part.*	*1Part.*	*2Part.*	*1Part.*	*2Part.*
Cooperative outcome …						
… in *hard* and *easy*	0.02	0.01	0.11	0.06	0.45	0.27
… only in *hard*	0.01	0.02	0.01	0.06	0.00	0.10
… only in *easy*	0.07	0.08	0.15	0.22	0.13	0.32
… in neither	0.90	0.89	0.73	0.66	0.42	0.31
N	1,970	3,770	1,488	2,812	330	500

Notes: Share estimates of the outcome of the stage games restricting to the last round in the supergame. Pearson's χ^2-test with robust standard errors, clustered on Unique number for each group, for the difference between 1Partner and 2Partner yields $p = .387$ at $\delta = 0.1$, $p = .049$ at $\delta = 0.5$, and $p = .030$ at $\delta = 0.9$.

Table A.6: Outcome of the stage games of the last round in the supergame, $\delta = 0.5$

	Simultaneous		Sequential	
	1Partner	*2Partner*	*1Partner*	*2Partner*
Cooperative outcome …				
… in *hard* and *easy*	0.11	0.06	0.27	0.11
… only in *hard*	0.01	0.06	0.03	0.05
… only in *easy*	0.15	0.22	0.06	0.30
… in neither	0.73	0.66	0.64	0.53
N	1,488	2,812	1,128	2,152

Notes: Share estimates of the outcome of the stage games restricting to the last round in the supergame. Pearson's χ^2-test with robust standard errors, clustered on Unique number for each group, for the difference between *1Partner* and *2Partner* yields $p = .049$ when the two games are played simultaneously and $p = .001$ when the two games are played sequentially.