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Lisa Bruttel
Werner Güth

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Repeated Prisoner's Dilemma Experiments with Third-Party Monitoring and Indirect Punishment

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Repeated Prisoner's Dilemma Experiments with Third-Party Monitoring and Indirect Punishment

Lisa Bruttel* Werner Güth†

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Abstract

Two pairs of two participants each interact repeatedly in two structurally independent but informationally linked Prisoner's Dilemma games. Neither pair receives feedback about past choices by their own partner but is fully informed about the choices by the other pair. Considering this as a four-person infinite horizon game allows for Folk-Theorem-like voluntary cooperation. We ask whether monitoring and indirect punishment with the help of others are comparable to direct monitoring and punishment in establishing and maintaining voluntary cooperation. The treatment effects we find are rather weak. Others' monitoring of own activities is only an insufficient substitute for direct observability.

Keywords: prisoner's dilemma, monitoring, experiment

JEL-Classification: C73, C91, D82, D84

*University of Konstanz, Department of Economics, Box 131, 78457 Konstanz, Germany, lisa.bruttel@uni-konstanz.de.

†Max Planck Institute of Economics, Strategic Interaction Group, Kahlaische Str. 10, 07745 Jena, Germany, email: gueth@econ.mpg.de

1 Introduction

Rather than demonstrating by examples the importance of social monitoring in the sense of "others" observing and reacting to own behavior, let us immediately introduce our experimental workhorse. Two pairs, let us denote them as $X = (X_1, X_2)$ and $Y = (Y_1, Y_2)$, interact repeatedly by each playing a structurally independent Prisoner's Dilemma game rendering the payoff of player X_i, resp. Y_i, dependent only on the choice of X_i and X_j, resp. of Y_i and Y_j, with $j \neq i$. Nevertheless, the two player pairs, X and Y, can strategically influence each other since in each round $t\,(> 1)$ for $i = 1, 2$ player X_i, resp. Y_i, never observes any of X_j's, resp. Y_j's, past choices, where $j \neq i$, but is fully informed about all past choices by both players in the other pair, what is commonly known among all four players X_1, X_2, Y_1, Y_2. In the following, we refer to such observability by players of the other pair as *social monitoring*.

There exists ample experimental evidence[1] that, even when T is finite and commonly known, participants in standard two-person Prisoner's Dilemma games cooperate frequently – contrary to backward induction – except for endgame effects (Selten and Stoecker, 1986) in the final rounds of the game. What we wonder about and study experimentally is whether such cooperation can also be observed in case of social monitoring and indirect sanctioning by "others," i.e., players in the other pair. Although the extent of voluntary cooperation may suffer from substituting *direct* by *social* monitoring, such evidence could demonstrate possible welfare gains from social monitoring, for example in the form of neighborhood gossip.

Folk Theorems (see, e.g., Aumann, 1981) can be applied to justify permanent voluntary cooperation by all four players in the infinitely repeated game. For instance, if players react to observing any defection of the other pair by permanent defection thereafter, any unilateral defector will not gain when payoffs for the infinitely repeated game are determined by the limes inferior of periodic payoffs or weakly discounted future earnings. Thus, the four players play grim trigger strategies, punishing any defection in the other, payoff-unrelated pair with own defection. Sanctioning is indirect due to "hidden actions" within a pair.

[1] See, e.g., the recent papers by Dal Bó (2005), Camera and Casari (2009), Dal Bó and Frechette (2011), Normann and Wallace (2012), or Angelova et al. (2013) and the therein cited literature.

Playing in constant four-player groups with an X and a Y pair might render establishing and maintaining voluntary cooperation more difficult in spite of the structurally independent games played by the two pairs.[2] For illustration, imagine a situation where all four players cooperate for some rounds until one player, say X_i, defects. This is not observed by X_j but by both Y_i and Y_j. Players in pair Y may view the defection of X_i as a reason to defect themselves in order to inform player X_j that his coplayer defected or because they interpret X_i's defection as an attempt to signal a defection by their own coplayer. At the same time, player Y_i and/or Y_j may be reluctant to react to a defection of player X_i by own defection because they run the risk to harm their own, possibly innocent coplayer. Without preplay communication, it is not obvious how to interpret that members of the other pair are defecting. Furthermore, they may totally neglect the other pair, e.g., due to complexity reduction. It may need time before all four players cooperate and, even when cooperation is established, endgame opportunism might still begin earlier.

In two-person Prisoner's Dilemma experiments, a distinction between what "we" do and how "others" behave is impossible. In another treatment, we therefore consider two pairs playing two independent games with feedback information about their partner's choices and additional observability across pairs. Being observable by payoff-unrelated others might discourage deception and thereby stabilize voluntary cooperation beyond what is observed in usual two-person Prisoner's Dilemma experiments. A control treatment provides no feedback information at all and renders constant defection dominant by ruling out any "tit-for-tat." Nevertheless, behaviorally speaking, participants might shy away from constant defection with its dramatic efficiency losses. These treatments with full or no observability provide an upper, respectively lower, bound for how much voluntary cooperation can be expected in the main Prisoner's Dilemma treatment with social monitoring and indirect punishment only.

The results show that in order to enable voluntary cooperation, social monitoring is only an insuffient substitute for direct observation within pairs. Beliefs about the own partner's cooperation tend to be overoptimistic. Further differences between treatments are not systematic.

[2]Public goods experiments usually rely on more than two players (most frequently four players as in our experiment) but do not, to the best of our knowledge, distinguish between payoff relevant interaction partners and others who only observe choices which are not payoff relevant for themselves (see Ledyard, 1995, for an early survey and Chaudhuri, 2011, for a more recent one of public goods experiments).

Our paper relates to experimental studies on third-party observation and punishment such as Fehr and Fischbacher (2004), Goette et al. (2006), Carpenter and Matthews (2010), or Sutter et al. (2009). In these studies, the third party is usually a neutral observer of an interaction between two players. This additional player may punish the interacting players by reducing their income at a cost for the third player's income after the main interaction. The base game is often a Prisoner's Dilemma as in our study, but the interaction with the third party differs in various ways. First, different to the third party punishment, as implemented in these papers, punishment in our setup is indirect, because the third party cannot immediately affect the payoffs of those in the other pair. Second, our "third party" players are more closely involved due to simultaneously playing the same game with their own coplayer. Finally, observation in our case is bidirectional.

In section 2, we describe the symmetric two-person Prisoner's Dilemma game, played by each of the pairs in our experiment, and introduce the four main as well as the two control treatments. Section 3 specifies some obvious hypotheses. After analyzing the data in section 4, we conclude in section 5.

2 Experimental design and procedures

To test whether social monitoring matters for cooperation in symmetric Prisoner's Dilemma base games, and if so how, we always relied on the 2x2 Prisoner's Dilemma game in Table 1. The game excluded negative payoffs but allowed for a payoff level of 0, with one player choosing C, the cooperative move, and the other player in the same pair defecting, i.e., choosing D.[3] The payoffs in the bimatrix of Table 1 are listed in the order "own payoff, payoff of other player in the same pair."

In all six treatments, summarized in Table 2, this base game was played recursively by the same pair. Each treatment confronted its participants with two phases. Phase 1 consisted of either 10 or 20 successive rounds. Phase 2 always comprised 20 successive rounds. During the two phases treatments differed either in group size (two informationally linked pairs in the main

[3]In the instructions for the participants, we used neutral action labels.

		Choice by other player in the same pair	
		C	D
Own choice	C	80,80	0,140
	D	140,0	10,10

Table 1: The symmetric Prisoner's Dilemma game implemented as base game in all treatments

treatments versus one pair only in the control treatments) or in whether players were informed about the own partner's past choices.

In our main treatments, participants were randomly assigned to groups of four participants each who, furthermore, were randomly assigned to two pairs. In all main treatments, players received information feedback about the past choices of the own partner during phase 1 and only about the past choices of the other pair in phase 2. What differed between main treatments, in addition to the length of phase 1, was whether in phase 1 a pair additionally received information about the past choices of the other pair. Thus, we distinguish altogether four main treatments in a 2x2 factorial design. We also ran two control treatments, one with 10 and one with 20 rounds in phase 1 without informationally linking the two pairs. In phase 1, both players in a pair received feedback about the past choices of their partner, whereas, during phase 2, no feedback at all was provided. In each round, participants had to state their belief whether it was more likely that their coplayer would cooperate or defect.[4] In all treatments, there was a restart after both phases had been played. In the restart, matching was the same as before. Subjects were informed in the beginning that there would be a second part of the experiment, but learned that it was a repetition of the first part with the same partner(s) only when entering the second part.

The experiment was computerized using z-Tree (Fischbacher 2007). A total of 288 students from various disciplines recruited via ORSEE (Greiner 2004) took part in the experiment, 48 per treatment. Thus, we relied on 24 independent pairs in the control treatments. For the main treatments, there were 12 independent groups of four participants. Each participant could register for only one session. The experiment took place in *Lakelab*, the laboratory for experimental economics at the University of Konstanz, in April and May 2013. A session

[4]They had to choose one of three options, "C is more likely," "D is more likely," and "Entirely Uncertain."

Number of rounds in phase		Information feedback in phase		Group size
1	2	1	2	
10	20	own&other	other	4
		own	other	4
		own	no	2
20	20	own&other	other	4
		own	other	4
		own	no	2

Table 2: Treatment differences by length of phase 1, group size (one vs. two pairs) and information feedback

took about an hour, including reading the instructions, answering control questions, and final payment. The earned points were converted at a rate of 1 euro for 400 points. Both phases were paid in addition to the show-up fee of 3 euros. In case of the shorter phase 1, participants received an additional flat payment of 700 points at the end of phase 1 as compensation for the reduced earning possibilities to limit the endowment differences between treatments in phase 2. The average payment, including the show-up fee, was 14 euros. Before the start of our experiment, subjects first received written instructions on their computer screen.[5] Participants had to answer a few control questions before actually interacting. At the end of each session, participants were individually called to the exit to receive their payment in cash outside the laboratory. By allowing sufficient time between the participants exiting, we ensured privacy with respect to the amount of money each of them received.

3 Hypotheses

An obvious null hypothesis (0) claims no treatment effects at all in phase 2. The reasoning behind this is that players are only interested in the behavior of those with whom they are structurally related, i.e., their coplayer in the same pair. The necessary information for this, i.e., feedback on past behavior of the own pair, is provided in phase 1 but not in phase 2, what applies to all treatments. However, we expect to confirm the following alternative hypotheses, namely (i) that a longer phase 1 will create a longer history of voluntary cooperation and,

[5]See the Appendix for a translated version of our instructions.

due to habit formation, increase voluntary cooperation in phase 2 merely by social monitoring and indirect punishment, and (ii) that information about the other pair will promote own cooperation. We expect the latter since monitoring by others may have a disciplining effect, e.g., when participants generally dislike being detected as free riders.

However, observing a defection in the other pair is an ambiguous signal. It might be an exploitation attempt but also a reaction to the defection of the own coplayer. This ambiguity could be avoided, or at least reduced, when a defection in the own pair triggers defection by both players in the other pair.[6] Of course, such a common reaction to what happens in another pair is more likely in case of preplay communication, which we exclude. However, we nevertheless expect (iii) parallel reactions of coplayers to defection in the other pair.

Without specifying this as an additional hypothesis, we, furthermore, predict similar qualitative aspects of play, like initial voluntary cooperation and endgame behavior as in usual recursive Prisoner's Dilemma experiments, but, in case of purely social monitoring, significantly less efficiency than in the standard information setup.

4 Data analysis

In Table 3, we report the cooperation rate of phase 1 and 2 using the same representation format as in Table 2. (The appendix also includes the share of rounds with the C, C outcome as well as the cooperation rate in the first and last five rounds of each phase.) The dynamics of the cooperation rate across rounds for the four main treatments is visualized in Figure 1 and for the two control treatments in Figure 2.

Let us first focus on cooperation in phase 1. Comparing "own&other, other," and "own, other" reveals that information about the decisions by another pair increases cooperation, in particular in the repetition (2nd supergame). The difference is significant only in the second supergame of the treatment with a short phase 1 ($p = 0.03$).[7] The phase 1 differences between

[6] Based on the intuitive idea that individual exploitation attempts in the sense of defections not justifiable by previous defections cannot rely on a common observation.

[7] If nothing else is stated, the reported p-values refer to two-sided Wilcoxon rank sum test, treating informa-

Rounds in phase		Info in phase		1st supergame % C in phase		2nd supergame % C in phase	
1	2	1	2	1	2	1	2
10	20	own&other	other	0.76	0.46	0.90	0.37
		own	other	0.76	0.28	0.75	0.20
		own	no	0.73	0.34	0.83	0.33
20	20	own&other	other	0.81	0.26	0.90	0.23
		own	other	0.72	0.35	0.78	0.32
		own	no	0.67	0.23	0.88	0.27

Table 3: Cooperation rates across treatments and phases

"own, other," and "own, no" in Table 3 are significant only in the second supergame, irrespective of the length of phase 1 ($p(short) = 0.06$, $p(long) = 0.04$), with more cooperation in phase 1 if there is zero information in phase 2. It seems that in phase 1, when anticipating no feedback information in phase 2, participants of the "own, no" treatment reasoned along the lines of: 'if we care for cooperation, we better do it right now.'

Figure 1: Cooperation rate over time in the main treatments

In the two main treatments of short duration, cooperation in both supergames is significantly ($p = 0.05$ and $p = 0.08$, respectively) higher in phase 2 when subjects received feedback about

tionally related groups as the unit of observation.

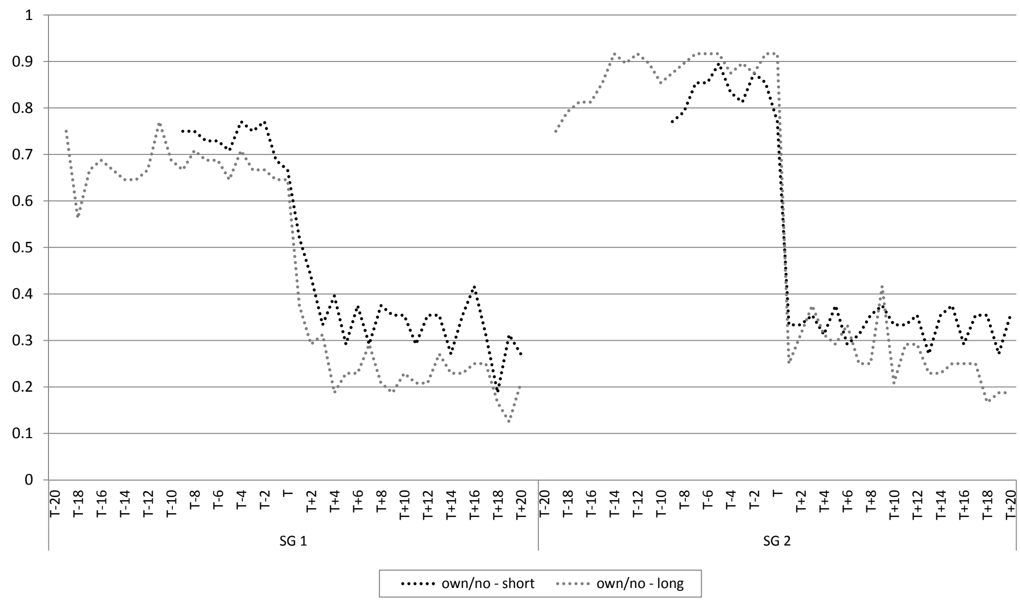

Figure 2: Cooperation rate over time in the control treatments

another pair in phase 1, despite the fact that information in phase 2 is the same in both treatments. However, in the treatments with the long phase 1 of 20 rounds, the effect of information about the other pair is different. Whereas information about the other pair in phase 1 still enhances cooperation during phase 1, though not significantly ($p = 0.88$ and $p = 0.23$), cooperation rates in phase 2 are (insignificantly, $p = 0.20$ and $p = 0.26$) reduced.

Differences in endgame effects across treatments may explain these differences in cooperation rates. As can be seen in Figure 1, there is a significant drop in cooperation from round 19 to round 20 in the second supergame of the "own&other, other" treatment of long duration (Wilcoxon signed rank test, one-sided, $p = 0.01$). The next largest drop in cooperation is observed in the second supergame of treatment "own, no" of short duration ($p = 0.06$). In the other treatments, the endgame effect is much weaker ($p > 0.15$). Why are there different endgame effects? Our intuition is that participants in the "own&other" treatment of long duration have more time to anticipate endgame effects and that full feedback helps to propagate, in the longer phase 1, more frequent preemption attempts.[8]

[8]By preemption we mean that a player is first (not second) in terminating voluntary cooperation.

Situations in which one player of one pair qualifies as the first unique defector occur quite rarely in general. Many four-player groups do not perfectly cooperate in round T, and in the groups, starting into phase 2 with 100% cooperation, there is often more than one defector. In total, there are only 17 cases with a unique first defector across treatments and supergames. In these cases, players of the other pair did not react at all to the defection 10 times, did both defect 4 times, and reacted by only one defection 3 times. In general, defections occur quite early in phase 2, mostly in the first or second round of this phase.

Finally, there is a positive cooperation rate of about 30 percent even without any "tat" at all in phase 2 of the "own, no" treatments, which is insignificantly smaller in case of the longer phase 1 (first supergame: $p = 0.28$, second supergame: $p = 0.40$). Such substantial cooperation without any "tat" may be attributed to habit formation: having experienced voluntary cooperation and relying on "tit for tat" in phase 1, triggers a reluctance in players to immediately terminate voluntary cooperation in phase 2 when "tit for tat" is no longer available.[9]

Rounds in phase		Info in phase		1st supergame % C in phase		2nd supergame % C in phase	
1	2	1	2	1	2	1	2
10	20	own&other	other	0.78	0.52*	0.90	0.43*
		own	other	0.76	0.31	0.77	0.21
		own	no	0.75	0.30	0.82	0.34
20	20	own&other	other	0.84	0.27	0.91	0.20
		own	other	0.74	0.42*	0.79	0.37*
		own	no	0.70	0.37**	0.86	0.37**

Table 4: Average beliefs about other's cooperation across treatments and phases, excluding "uncertain" as answer. * denotes that the belief is 5-10 percentage points larger than the actual cooperation rate, ** denotes that it is more than 10 percentage points larger.

Table 4 summarizes beliefs about the likelihood of the own partner cooperating. Believing "the other player will more likely choose C" is coded as 1, whereas D is coded as 0. Answers indicating "uncertain" are excluded. (Table 8 in the Appendix includes the uncertain beliefs, coding them as 0.5.) Comparing the beliefs to the outcomes, reported in Table 3, we find that beliefs reflect average cooperation rates in phase 1 quite well (in the sense of the difference

[9]One may speculate that even a "no, no" treatment with no information feedback in both phases would reveal some phase 2 cooperation. We did not run this further control treatment but strongly suspect that it would produce lower cooperation rates in phase 2 than our "own, no" treatments.

between belief and actual outcome being smaller than 5 percent). However, participants systematically overestimate their coplayer's cooperation in the second phase of both successive supergames (see the first and last two rows of Table 4).[10] Whether the first phase is short or long and whether there is "other" or "no" in phase 2 has no systematic effect on overoptimistic beliefs in the coplayer's cooperativeness.

5 Conclusions

One essential and, in our view, new finding is that observing only one other pair in phase 2, i.e., social monitoring, cannot substitute own monitoring, although it allows for Folk-Theorem-like voluntary cooperation based on "tit for others' tat." The cooperation rates decrease significantly from phase 1 to phase 2 across the board (Wilcoxon signed rank test, two-sided, $p \leq 0.01$ for all comparisons in Table 3). Furthermore, this substantial decline seems rather immune to circumstances such as whether one has experienced mutual observation of both pairs before ("own&other") or not ("own") in phase 1 or the length (10 versus 20 rounds) of phase 1. We can thus quite generally conclude that third-party monitoring and indirect punishment are no reliable substitutes for direct monitoring and punishment by those who structurally interact.

Whether this is good or bad news is debatable. On the one hand, encouraging neighbors to spy on the own partner seems futile. On the other hand, hoping to harvest the fruits of voluntary cooperation without being able to directly monitor one's partner seems equally futile. Before jumping to far-reaching conclusions, one should keep in mind that in our setup, neighborhoods (the two pairs) are only informationally linked (via "other" in phase 2 or in both phases of the main treatments). It seems possible that supplementing this informational link with some neighborhood interaction, e.g., by letting the four players, X_1, X_2, Y_1, Y_2, of the main treatments engage in an additional solidarity game (see Selten and Ockenfels, 1998) or four-person public goods game, renders social monitoring (more) important. The reason could be that one is more likely to trust social monitoring when having to rely on neighborhood cooperation also otherwise.

[10] Such overoptimism might also result from wishful thinking (e.g., Bar-Hillel and Budescu, 1995).

Appendix A: Instructions for "own&other, other"

For consistency reasons within the paper, we label the two actions C and D. Subjects in the experiment saw the actions labeled X and Y in their instructions.

Welcome and thank you for participating in this experiment. Please read these instructions carefully. From now on, you are asked to remain seated and stop communicating with other participants. If you have any questions, please raise your hand. We will come to your place and answer your questions in private.

These instructions are the same for all participants.

Today, you will participate in two experiments. Here you see the instructions for the first experiment. We will inform you about the content of the second experiment after the end of the first one. The first experiment consists of two stages. The first stage includes 10 rounds, the second 20 rounds.

Your earnings in this experiment will be counted in points. For every 400 points you earn, you will receive 1 euro in cash at the end of the experiment, plus 3 euros for showing up. After the end of the first stage of this experiment, you will additionally receive 700 points to be credited on your points account.

In this experiment you interact repeatedly with the same other participant. You will not be informed who this other participant is, nor will the other participants learn your identity.

In each round you and the other participant will be asked to choose one of two possible decisions C and D. Depending on your and the other player's decision, the payoffs are determined according to the following table:

My decision	Decision of the other	My earnings	Earnings of the other
C	C	80	80
D	D	10	10
C	D	0	140
D	C	140	0

- If you and the other participant both choose C, each of you receives 80 points.
- If you and the other participant both choose D, each of you receives 10 points.
- If one of you chooses C and the other D,
  - the one who chose C receives 0 points and
  - the one who chose D receives 140 points.

Before you make your decision between C and D, you will be asked in each round to tell us if you think it is more likely that the other participant chooses C or D in the respective round.

In stage 1, you will in each round learn both previous decisions. Furthermore, you will be informed about the decisions of two other participants in this round, who interact with each other the way you do with the other participant. Again, those two participants will be informed about the decisions you and the other participant, you are interacting with, choose. This additional information exchange always occurs between the same participants. In stage 2, you will only be informed about the decisions of the other two participants but not anymore about the decision of the participant you are interacting with.

You will be informed about how many points you earned in the two experiments only at the end of the second experiment. Your profits from both experiments will be added up at the end, converted into euros, and paid to you in cash. After you finished reading the instructions, we will ask you to answer some control questions. The experiments starts only after all participants have answered the questions correctly. After the experiment we will ask you to answer a short questionnaire.

Appendix B: Additional tables

Rounds in phase 1	Rounds in phase 2	Info in phase 1	Info in phase 2	1st supergame % C,C in phase 1	1st supergame % C,C in phase 2	2nd supergame % C,C in phase 1	2nd supergame % C,C in phase 2
10	20	own&other	other	0.65	0.26	0.86	0.17
		own	other	0.66	0.10	0.65	0.06
		own	no	0.60	0.17	0.76	0.16
20	20	own&other	other	0.73	0.07	0.85	0.04
		own	other	0.63	0.18	0.70	0.18
		own	no	0.58	0.07	0.83	0.10

Table 5: Share of rounds with **X,X outcome**

Rounds in phase 1	Rounds in phase 2	Info in phase 1	Info in phase 2	1st supergame % C in phase 1	1st supergame % C in phase 2	2nd supergame % C in phase 1	2nd supergame % C in phase 2
10	20	own&other	other	0.73	0.51	0.92	0.38
		own	other	0.76	0.38	0.78	0.27
		own	no	0.73	0.40	0.83	0.34
20	20	own&other	other	0.77	0.36	0.90	0.30
		own	other	0.65	0.47	0.77	0.41
		own	no	0.67	0.28	0.80	0.31

Table 6: Cooperation rate in the **first five rounds** of a phase

Rounds in phase 1	Rounds in phase 2	Info in phase 1	Info in phase 2	1st supergame % C in phase 1	1st supergame % C in phase 2	2nd supergame % C in phase 1	2nd supergame % C in phase 2
10	20	own&other	other	0.80	0.38	0.88	0.33
		own	other	0.75	0.18	0.73	0.17
		own	no	0.73	0.30	0.83	0.33
20	20	own&other	other	0.82	0.22	0.85	0.19
		own	other	0.75	0.28	0.71	0.22
		own	no	0.67	0.20	0.90	0.21

Table 7: Cooperation rate in the **last five rounds** of a phase

Rounds in phase 1	Rounds in phase 2	Info in phase 1	Info in phase 2	1st supergame % C in phase 1	1st supergame % C in phase 2	2nd supergame % C in phase 1	2nd supergame % C in phase 2
10	20	own&other	other	0.76	0.51*	0.88	0.44*
		own	other	0.74	0.35*	0.76	0.25*
		own	no	0.73	0.36	0.80	0.38*
20	20	own&other	other	0.82	0.30	0.89	0.25
		own	other	0.72	0.44*	0.77	0.40*
		own	no	0.69	0.40**	0.85	0.40**

Table 8: Average beliefs about other's cooperation across treatments and phases. * denotes that the belief is 5-10 percentage points larger than the actual cooperation rate, ** denotes that it is more than 10 percentage points larger.

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THURGAU INSTITUTE
OF ECONOMICS
at the University of Konstanz

Hauptstr. 90
CH-8280 Kreuzlingen 2

Telefon: +41 (0)71 677 05 10
Telefax: +41 (0)71 677 05 11

info@twi-kreuzlingen.ch
www.twi-kreuzlingen.ch