Exchanging Private Information to Sustain Cooperation in Noisy, Indefinitely Repeated Interactions

– Pre-Analysis Plan –

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1 Experimental Design

We implement different variants of a Prisoners' Dilemma game with imperfect monitoring in a laboratory experiment. In every round, two players choose their actions $a_i \in \{C, D\}$ simultaneously. Payoffs depend on the player's own action a_i and the received signal about the other player's action ω_{-i}. Under public monitoring, subjects are informed about $(a_i, \omega_i, \omega_{-i})$ at the end of every round. Under private monitoring, subjects are informed about (a_i, ω_{-i}) at the end of every round. The continuation probability δ of the repeated game is 0.8. In the treatments without correlation, signals are drawn independently for each of the chosen actions. Signals are noisy and indicate the wrong action with probability $\epsilon = 0.2$. Therefore, if both players play C, the probability that the two signals differ is 0.32. In the treatments with correlation, both players receive the same signals if they choose the same action. The signals are correct, that is: they indicate cooperation (defection) when both choose cooperation (defection), with probability $1 - \epsilon = 0.8$. However, if their actions differ, signals are drawn independently, as in the treatments without correlation.

In all treatments, subjects engage in a pre-play communication-stage before the first round of every supergame. In this stage, subjects can communicate via a chat-box interface for 120 seconds.

Under private monitoring, two treatments have a reporting stage, which is implemented in the form of a structured communication stage after every round. In this stage, subjects can report the received signal from the current round to their partner (or misreport it).

In every session of every treatment, subjects are randomly divided into 3 matching groups, with 8 each. Subjects play 7 supergames with pre-determined lengths. At the beginning of every supergame, each subject is matched with a new partner from his/her matching group using perfect stranger matching, so that they do not play with the same partner for a second time. To keep the length of supergames constant across treatments, we generated 3 sequences of random numbers beforehand, and used them to determine the length L_i of each supergame.[1] To increase the number of observations per supergame, we adapt the block-random-termination method (Fréchette and Yuksel, 2017). Subjects play a block of 5 rounds at the beginning of every supergame. If the true length L_i is smaller or equal than 5, the supergame ends at the end of round 5 and only the first L_i rounds are payoff relevant. If L_i is larger than 5, the supergame continues until round L_i has been reached and all rounds are payoff relevant. Before the end of round 5, subjects are not informed about whether the supergame ends or not.

Subjects are required to answer control questions before the game starts. At the end of the experiment, subjects answer a short survey to elicit basic socio-economic characteristics,

[1]We used Stata to generate 3 sequences of uniformly distributed random numbers between 0 and 1 with seeds 3, 4, and 5 (we used seeds 1 and 2 in: Dvorak and Fehrler, 2018). Denote the 3 sequences as $\{r_n\}_i = \{r_1, r_2, ..., r_x\}_i$, where $i = 3, 4, 5$ indicates the seed underlying the sequence and $n \in \mathbb{N}$. The first supergame has x_1 rounds if $r_{x_1} \leq 0.2$ and for all $n < x_1$, $r_n > 0.2$. The second supergame has $x_2 - x_1$ rounds if $r_{x_2} \leq 0.2$ and for all $x_1 < n < x_2$, $r_n > 0.2$. And so forth. The resulting (lengths of the) sequences are SQ1 $(2, 8, 1, 5, 7, 1, 7)$, SQ2 $(4, 2, 2, 21, 4, 3, 5)$ and SQ3 $(2, 3, 1, 1, 4, 6, 6)$.

such as age and gender.

Experimental Parameters

Figure 1 shows a screenshot of the decision interface.

Figure 1: Stage Game Parameters

Ihre Optionen	Ihr Einkommen bei Signal		Erwartetes Einkommen, wenn die andere Person	
	A	B	Option A waehlt	Option B waehlt
Option A	32	2	26	8
Option B	40	10	34	16

Notes: Screenshot from the experiment. Payoffs are in experimental currency units with an exchange rate of 50 ECU = 1 EUR.

The left two columns depict the stage-game payoff in experimental currency units. The payoff parameters do not vary across treatments. The last two columns show the expected stage-game payoffs and are calculated given a fixed error rate of 0.2 among all treatments. The parameters are chosen such that two conditions are satisfied:

1) Under imperfect private monitoring with signal correlation, there is a quasi-public perfect (truth-telling) equilibrium (QPPE) if reporting is allowed, in which both players play a "reporting grim-trigger" strategy. The reporting grim-trigger strategy prescribes the following

behavior: Start with C and report your received signals truthfully, continue cooperating as long as both reports in the previous round are the same, otherwise defect for all subsequent rounds. The reporting mechanism translates the private monitoring into (quasi) public monitoring. An analogous cooperative PPE exists under imperfect public monitoring, in which subject play the same grim-trigger strategy as the one sketched above (but without reporting).

2) No cooperative (Q)PPE exists if there is either no correlation or there is correlation but it cannot be detected due to the absence of a reporting stage.

Treatments

We implement up to five different treatments. We begin with collecting data for two treatments with imperfect-public monitoring: one with correlation (T1) and one without (T2). In case we find a statistically significant treatment difference (see next section for details on the test), we continue with two private-monitoring treatments with correlated signals: one with reports (T3) and one without (T4). In case, we find a statistically significant treatment difference between T3 and T4, we continue with the final treatment T5, which is a private-monitoring treatment without correlation but with a reporting stage.

T1 Signals are public and independent.

T2 Signals are public and perfectly correlated if both actions are the same.

T3 Signals are private and perfectly correlated if both actions are the same. Participants can publicly report their private signal after each round.

T4 Signals are private and perfectly correlated if both actions are the same. Participants cannot report signals.

T5 Signals are private and independent. Participants can publicly report their private signal after each round.

2 Hypotheses Tests, Power, and Further Analyses

In a previous study of communication and cooperation in a noisy, indefinitely repeated Prisoner's Dilemma with uncorrelated signals, we saw high cooperation rates in the first rounds of the supergame with pre-play communication but then a steady and strong decline over the subsequent rounds (Dvorak and Fehrler, 2018). In a pretest session of treatment T1, we again saw high cooperation rates in the first round and a decline afterwards. However, the decline was much weaker.

Based on these observations and the existence (or absence) of cooperative (Q)PPEs in the different treatments, we formulate our main hypotheses:

H1a: *The cooperation rate will be higher in T1 than in T2.*

H1b: *The cooperation rate will be higher in T3 than in T4.*

H1c: *The cooperation rate will be higher in T3 than in T5.*

We test the corresponding H0s by comparing the cooperation rates in the first 5 rounds of the last 3 supergames between the treatments. We run one-sided t-tests, for which we average the cooperation rates within each matching group and then take these averages as our independent observations.

Our simulations (see next paragraph) suggest that we will have enough power ($> 80\%$) to detect effect sizes of 10 percentage points with 9 matching groups with 8 participants each per treatment.

Simulations for Assessing the Statistical Power

In the simulations, we iterate the following process 10,000 times for various effect sizes Δ:

1. Create a data set of 8 (subjects per matching group) * 9 (number of matching groups per treatment) * 2 (treatments) observations and an indicator variable for treatment 2.

2. Draw random numbers from the Bernoulli distribution with a success probability that starts at 1 in round 1 and then linearly declines to 0.9 in round 5 for treatment 1.[2]

3. Draw random numbers from the Bernoulli distribution with a success probability that starts at 1 in round one and then linearly declines to $0.9 - \Delta$ in round 5 for treatment 2.

4. Average the random draws from rounds 1-5 within each matching group. These are the simulated average cooperation rates.

5. Run a one-sided t-test on the matching-group averages. Return the $p-$value.

Finally, we compute the share of the $p-$values smaller than 0.05, which gives us the statistical power. We check the accuracy of the procedure by running it 10'000 times with the same success probabilities in both treatments, which results in a share of $p-$values smaller than 0.05 of 0.049, which is close to the 5% that we would expect for this scenario. The simulation results indicate that the power increases in Δ and is 80.1% with $\Delta = 0.1$. The power remains similar if we introduce matching-group-specific variation in the slopes of the declining cooperation probabilities.

Further Analyses

In addition to testing our three hypotheses, we explore subjects' strategies across the treatments to better understand the aggregate findings. These analyses are explorative in nature and we, therefore, refrain from specifying further hypotheses. The questions we are interested to explore are the following:

[2] 0.9 was the cooperation rate we observed in round 5 of the pretest of the T1 treatment.

- How are participants' choices in the private treatments with reports (T3 and T5) influenced by the reports of the previous period?
- Are the strategies used in the private treatment with reports (T3 and T5) similar to the strategies used in the the public treatment with correlated signals (T1 and T2)?

For the treatments T2 and T3, we are particularly interested to assess how many participants use a reporting grim-trigger strategy. It will further be interesting to compare the estimated strategies in T2 and T3 to the strategies in T1 and T5 for which the reporting grim-trigger strategy is theoretically not supported.

For the analysis of strategies, we build on the strategy frequency estimation method (SFEM) introduced by Dal Bó and Fréchette (2011), and use the R package stratEst (Dvorak, 2018), which was first used in Dvorak and Fehrler (2018). The SFEM is frequently used to obtain maximum-likelihood estimates of the shares of a candidate set of strategies in experimental data. However, the results of the SFEM are specific to this set and it is hard to know ex-ante which strategies should be included. To circumvent this problem, we will compute Maximum Likelihood estimates for an endogenously determined number of strategies where the structure of each strategy is the result of a model-selection process. Thus we will infer the strategies from the data rather than imposing a predefined set of strategies. The process will always start with a large number of such generic strategies, which will then be reduced step-by-step using the integrated-completed-likelihood criterion (ICL-BIC, Biernacki et al. (2000)). The ICL-BIC is an entropy-based selection criterion for mixture-models which has been used to estimate the dimensionality of the strategy space in other settings before (Breitmoser, 2015).

To assess whether strategies differ in two treatments, we fit a model on the pooled data of the two treatments and bootstrap the likelihood-ratio test statistic. If the distribution indicates that the likelihood-ration statistic is sufficiently extreme, we conclude that the strategies differ between the two treatments.

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