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Learning, words and actions: experimental evidence on coordination-improving information*

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Abstract

This paper reports experimental results from a one-shot game with two Nash equilibria: the first one is efficient, the second one relies on weakly dominated strategies. The experimental treatments consider three information-enhancing mechanisms in the game: simple repetition, cheap-talk messages and observation of past actions from the current interaction partner. Our experimental results show the use of dominated strategies is quite widespread. Any kind of information (through learning, words or actions) increases efficiency. As regards coordination, we find that good history performs better than good messages; but bad history performs worse than bad messages.

Keywords: Coordination Game, Communication, cheap-talk, Observation.

JEL Classification: C72, D83.

Résumé

Cet article présente des résultats expérimentaux issus d'un jeu comportant deux équilibres de Nash : le premier est efficace, le deuxième repose sur l'usage de stratégies faiblement dominées. Les traitements expérimentaux introduisent trois mécanismes améliorants le niveau d'information dans le jeu : une simple répétition, des messages de type "cheap-talk" et l'observation des actions passées du partenaire. Les résultats expérimentaux établissent que l'utilisation de stratégies dominées est fréquente. Tout type d'information augmente l'efficacité des résultats. A l'égard de la coordination, un historique d'actions positif apparaÃ®t plus efficace que de bons messages, tandis qu'un historique d'action négatif est moins efficace que de mauvais messages.

Mots-clés: Jeu de coordination, Communication, Observation.

Table 1: The experimental game

	Player B	
Player A	l	r
L	(9.75;3)	(9.75;3)
R	(3;4.75)	(10;5)

1 Introduction

Many applications in finance, such as currency attacks, bank runs, or liquidity crises, involve multiple equilibria (see, e.g., Morris and Shin, 2003). In such situations, theoretical refinement concepts are characterized by assumptions on players' beliefs about other players' behavior. In theory, the idea that players rely on the payoff-maximizing behavior of others is enough to rule out equilibria that are supported by incredible threats, *i.e.* actions that may undermine the value of the interaction. It has long been recognized, however, that such a condition may fail empirically. As an illustration, Table 1 presents a variation of a game originally proposed by Selten (1975) and extensively discussed by Rosenthal (1981). In the original sequential version of the game, player A moves first and chooses between L and R. In case R is chosen, player B can maximize both players' payoffs by choosing r ; or undermine them both by choosing l. The only subgame perfect equilibrium is (R, r) which leads to the Pareto efficient payoff (10,5). On the other hand, the simultaneous move game has two Nash equilibria: (R, r) is still an equilibrium, but (L, l) is one as well because decision r is only weakly dominant for player B. Even if decision l is an incredible threat from the point of view of player A, the decision to play L involves less strategic uncertainty. For the payoff structure presented in Table 1, the secure option L dominates the expected payoff of reliance for probabilities as low as 0.036.

In his 1981 paper, Rosenthal conjectured that the imperfect equilibrium may well happen depending on: *(i)* the stakes of the game; and *(ii)* what is known about the interaction partner's behavior. Until now, experimental studies have focused on the first dimension, and confirmed Rosenthal's conjecture. In their seminal contribution, Beard and Beil (1994) study subjects behavior under varying stakes. They find that 54.5% (from 20% to even 80% across treatments) of players A prefer the mistrustful choice L, while the preference to maximize own gains is almost universal (97.8%) among subjects in the role of player B who are trusted by their partners. The results has been confirmed on Japanese subjects in Beard, Beil, and Mataga (2001). Goeree and Holt (2001) apply the strategy method to the decision of players B. When asked what they would do would player A choose R, the odds of players B choosing r vary from 53% to 100%.[1] The rate

[1]See also Camerer (2003)[pp. 199-209] for a related discussion.

of secure choice from subjects in the role of player A varies from 16% to 80%, depending on the stakes of the game.

In this paper, we explore the second dimension of Rosenthal's conjecture by assessing how better knowledge about player B likely behavior affects coordination in the game. Note, most studies focus on what appears as a failure of player A to rely on the payoff maximisation of player B. This happens if player A interacts with a perfect payoff maximizer and nonetheless chooses L (a Type-II error if one views A decision as a test of who player B is). This may be different, however, from the coordination failure arising when a player A mistakenly relies on a player B choosing l (Type-I error). Disentangling between the two requires to observe not only players A unreliance rate, but also player B behavior unconditional on what player A does. Our experimental design implements the normal form of the game to elicit decisions of both players in each interaction round. As noted by Duffy and Feltovich (1999), the flow of information between players contribute to fulfilling Aumann and Brandenburger (1995) condition of "mutual knowledge of the strategy choices"; a key assumption to guarantee that Nash equilibria are reached. Two further treatments implement specific information on the current interaction partner. Player B is allowed to send messages in the communication treatment. In the observation treatment, player A is informed of the complete history of past decisions of the current interaction partner before the decision stage.

Within the context of our game, cheap-talk messages from player B to player A have strong theoretical properties. First, the message to play r from player B to player A is highly credible according to the Farrell and Rabin (1996) characterization, both self-signalling – because the sender wants to select the action it signals – and self-committing – because it subsequently creates an incentive for the sender to fulfil it. One further feature of our game is that the efficient equilibrium entails greater strategic risk than the inefficient one. Ellingsen and Östling (2010) point out that cheap-talk provides reassurance in such a situation, thus enhancing coordination on the efficient outcome. Those properties should reinforce the abundant empirical literature confirming the ability of cheap-talk communication to improve efficiency.[2] Our third experimental condition follows a recent experimental literature contrasting the coordination properties of communication with the performance of observation. Duffy and Feltovich (2002) introduce both cheap-talk and observation of partner's most recent action in three 2 x 2 games: Prisoner's Dilemma, Stag Hunt and Chicken. While both treatments lead to an increase in frequency of the Nash equilibria whatever the structure of the game, cheap-talk appears more effective than observation when communication is highly credible – "words speak louder than actions" – and observation brings about better results when sender's message is not reliable enough – "actions speak louder than words". Bracht and Feltovich (2009) apply the same experimental treatments to a gift-exchange

[2]See, *e.g.*, Crawford (1998) for an earlier survey, and Charness (2000); Charness and Grosskopf (2004) for experimental evidence in strategic contexts close to ours.

game. The results show a striking contrast between treatments: while observation is effective in reinforcing cooperation, the effect of communication visibly lags behind.

We rely on a between-treatment design that compares the coordination performance of information based on learning, words and actions (Section 2). Our results, detailed in Section 3, show that unreliability (through decision r) is quite widespread among subjects in the role of player B, and unsensitive to the treatment variables. All three kinds of information channel happen to improve efficiency. Specific signals about the current interaction partner (through messages or observation) perform much better than, and are substitute to, repetition-based learning. The main difference between the two information-transmission mechanisms lies in their ability to induce coordination on either equilibrium: actions provide more informative signals on the odds of successful reliance; but strongly decreases the odds of accurately adjusting for unreliable partners.

2 Experimental design

For the sake of replication, our core game relies on the original experiment of Beard and Beil (1994). Among the various payoff combinations they use, we chose treatment 1, presented in Table 1, that has several attractive features: *(i)* as in the original setting, it does not lead to any conflict of interests between partners; *(ii)* the rate of players A unreliant choices related to this treatment is remarkable: 65.7% and *(iii)* this is the only treatment where deviations from the dominant strategy by players B were observed (in 17% of all cases where player A made a reliant decision R). The baseline condition implements a simple repetition of the one-shot game over an undefined number of rounds. We study the role of information in the game through two information boosting devices: cheap-talk messages from B to A before the game is actually played; and historical information on what player B has decided in previous periods.

2.1 Baseline game

Our focus on enhancing information in the game led us to introduce several modifications to the original experiment: *(i)* the one-shot game is played repeatedly by experimental subjects, and *(ii)* we implement the normal form of the game rather than the genuine sequential form.

The experiment involves 10 rounds, each consisting of the core game presented in Table 1. Roles are fixed, so that each subject takes 10 decisions as either player A or player B. The experiment was designed so as to remain as close as possible to a one-shot game. First, the pairs are rematched each round using a perfect stranger design (each session involves 20 subjects). Second, although the number of repetitions is pre-determined, we avoid end-game effects by providing no information about that in the experimental instructions – except for the repetition itself. Last, we associate take-home earnings from the experiment with only one round out of the ten. For that matter, one round is randomly drawn at the end of the experiment (the same for all subjects).

To ensure the homogeneity of rounds despite repetition, we also modify the sequentiality of the game originally introduced by Rosenthal (1981). As pointed out by Binmore, McCarthy, Ponti, Samuelson, and Shaked (2002, p.55-56), the repetition of one-shot multi-stage games may induce some unwarranted heterogeneity and selection bias in observed behavior, because players are induced to distinguish between rounds based on the decisions made in earlier stages of the game. Unlike the original Beard and Beil (1994) experiment, we thus ask both player A and player B to take a decision each period. To make it as close as possible to the original sequential game, we describe the decision phase to subjects as follows: player A is first asked to choose between L and R, then player B chooses between l and r, and last payoffs depend only on player A's decision if L is chosen, or on both players's decision otherwise.

To sum up, our baseline game implements a repeated version of the one-shot game originally analyzed by Beard and Beil (1994). We use a perfect stranger design, the normal form of the game, an unknown termination rule and a one-round compensation rule to avoid that subjects compute the expected value of the entire game. This should induce players to maximize their utility in each repetition of the one-shot game. As a first step towards assessing the role of information in the Rosenthal puzzle, intra-comparisons in this baseline game hence allows us to assess the robustness of the results to repetition – without reputation. We further increase the amount of information in two subsequent experimental treatments.

2.2 Experimental treatments

The experimental treatments introduce some flow of information in the baseline game under two different forms: pre-play communication and observation of partner's actions from the past.

Pre-play communication. Our first treatment allows players B to provide information to players A about what they intend to play. In every round, prior to the decision-making phase, player B has to send a message to player A. In the experimental implementation of cheap-talk messages, several trade-off must be solved. As raised by, *e.g.*, Farrell and Rabin (1996) cheap-talk ought to be meaningful, *i.e.* to have a precise meaning. Messages of the "I will do ..."-type, which might be considered as a bit oversimplified are however highly meaningful. Voluntary free-form communication, by contrast, improves the informational content of communication but always gives the sender an opportunity to send an empty message or a message that is either meaningless or imprecise – which is hard to interpret for both the receiver and the experimenter.[3] Given that our primary goal is to boost the information, we want to encourage players to communicate in a precise and clear manner. We hence implement a fixed-form communication and limit the set of

[3] Experimental results from Charness and Dufwenberg (2006, 2008) substantiate that impersonal messages, that have been prefabricated by the experimenter, work effectively in coordination games, whereas in trust games a more customized free-form communication seems to be needed.

possible messages to three options only, out of which two contain a precise information, while the third is empty. Before any decision takes place in the round, players B are asked to choose one out of the three following messages:

☐ I will choose r ☐ I will choose l ☐ I will choose either r or l

by clicking on the relevant field on her computer screen. This message is then displayed on player A's computer screen. Once player A confirmed the reception of the message, the round moves to the decision phase. It is highlighted in the written instructions that messages are not binding (decisions from players B can be anything following any of the messages) and do not affect experimental earnings.

Observation of historical information. In the third condition, we allow subjects in the role of players A to inspect all the decisions made by their current interaction partner in earlier rounds of the game. In every round, before the decision-making phase, player B is asked to wait while player A is provided with the history of choices made by player B. Following, *e.g.*, Bolton, Katok, and Ockenfels (2004) we make available the full history of past decisions rather than only the last one (see *e.g.,* Bracht and Feltovich, 2009). In each round, players A thus receive a table with the whole list of decisions from their current interaction partner in previous plays. Since pairs are rematched before each round, this information is updated and extended accordingly. Once player A has confirmed to be aware of player B's history, the decision-making phase starts.

2.3 Experimental procedures

For each game, we ran three experimental sessions, each involving 20 subjects. All sessions took place in the Laboratoire d'Economie Experimentale de Paris (LEEP) at University Paris 1 Panthéon-Sorbonne in between June 2009 and March 2010. The recruitment of subjects has been carried out via LEEP database of individuals who have successfully completed the registration process on Laboratory's website.[4] We intended to invite only those who never took part in any economic experiment in LEEP before. No subject participated in more than one experimental session. Each session lasted about 45 minutes, with an average payoff of 12 Euros.

Upon arrival, participants are randomly assigned to their computers and asked to fill in a small personal questionnaire containing basic questions about their age, gender, education, etc. The written instructions are then read aloud. Players are informed that they will play some (unrevealed) number of rounds of the same game, each round with a different partner, and that their own role will not change during the experiment. Before starting, subjects are asked to fill in

[4]The recruitement uses Orsee (Greiner, 2004); the experiment is computerized through a software developed under Regate (Zeiliger, 2000).

Table 2: Summary of experimental evidence on Rosenthal's game

Experiment	Payoff			Observed outcomes				Nb.
	(L)	(R,r)	(R,l)	L	(R,r)	(R,l)	$Pr(r\|R)$	obs.
Beard and Beil (1994)	(9.75; 3)	(10; 5)	(3; 4.75)	66%	29%	6%	83%	35
Beard et al. (2001)	(1450; 450)	(1500; 750)	(450; 700)	79%	18%	3%	83%	34
Goeree and Holt (2001)	(80; 50)	(90; 70)	(20; 68)	52%	36%	12%	75%	25
Baseline, round 1	(9.75; 3)	(10; 5)	(3; 4.75)	77%	23%	0%	100%	30
Baseline, round 2-10	(9.75; 3)	(10; 5)	(3; 4.75)	48%	43%	9%	84%	270

a quiz assessing their understanding of the game they are about to play. Once the quiz and all remaining questions are answered, the experiment begins.

Prior to the first round, players are randomly assigned to their roles – either A or B. They are then anonymously and randomly matched to a partner and asked for their choice, R or L for players A, r or l for players B. At the end of each round, each player is informed only about her own payoff. Once all pairs complete a round of the game, subjects are informed whether a new round starts. In this case, pairs are rematched according to a perfect stranger matching procedure (any pair meets only once in the session). At the end of the experiment, one round is randomly drawn and each player receives the amount in Euros corresponding to her gains in that round, plus a show-up fee equal to 5 Euros.

3 Results

The last two rows of Table 2 provide a summary of observed behavior in our baseline treatment along with results from previous experimental studies using the same game (top part of the Table). Our results are in line with what has been observed in other studies, despite the differences in the design described in Section 2: the average rate of unreliance is 51%, very close to the one observed in Goeree and Holt (2001) who apply the strategy method to the sequential game. Interestingly, the one-shot sequential games of Beard and Beil (1994); Beard, Beil, and Mataga (2001) are much better replicated by the first round of our baseline than by the overall rate produced by the repetition of the game. Once all repetitions of the baseline are pooled, the outcomes are in line with all previous studies in terms of both efficiency – outcome (R,r) – and coordination – (R,r) U (L,l). Two striking features emerge: first, a few number of attempts to rely on players B failed; second, this risk to be let down leads almost half players A to choose the secure option. In what follows, we describe whether and why information helps to overcome such coordination failures.

Table 3: Overall treatment effects of information

	Round 1			Rounds 2-10		
Outcome	Baseline	Comm.	Obs.	Baseline	Comm.	Obs.
Reliant A (R)	23.3%	50.0%	16.7%	51.9%	60.4%	57.0%
Reliable B (r)	80.0%	80.0%	76.7%	80.7%	80.0%	82.6%
Efficiency (R, r)	23.3%	43.3%	3.3%	43.3%	53.7%	52.2%
Coordination $((L, l) \cup (R, r))$	43.3%	56.7%	13.3%	54.1%	67.0%	64.8%
Type I error (L, r)	56.7%	36.7%	73.3%	37.4%	26.3%	30.4%
Type II error (R, l)	0.0%	6.7%	13.3%	8.5%	6.7%	4.8%

Note. For each treatment (in the sub-columns), the left-hand side presents the outcomes observed in the first round (30 observations for each treatment) and the right-hand side pools data from rounds 2-10 (270 observations). The first two rows display the proportions of unconditional behavior: decision R from player A on the first row, decision r from player B on the second row. Outcomes resulting from the interaction are split into four categories in the bottom part of the Table.

3.1 Aggregate treatment effects

Table 3 summarizes aggregate behavior elicited in each of the three treatments. We separate data into two groups – initial round on the left hand side, and pooled subsequent decisions in the right hand side. The first two rows of the table summarize unconditional average behavior of players A and B. The bottom part of the table describes the resulting outcomes: positive ones (efficiency and coordination) in the top panel, and failures in the bottom part. Thanks to our design, we are able to observe two sources of coordination failure: beyond the outcome arising when a player A mistakenly relies on player B, which we classify as Type-II errors, we are also able to observe Type-I errors, *i.e.* players A who should have relied on player B, since player B would have proved reliable in this case – outcome (L, r).

The likelihood of each outcome depends on both players' behavior. As shown in the second row of Table 3, the behavior of B players is fairly stable regardless of time and experimental treatments.[5] As a result, any difference we observe between treatments and between rounds is very unlikely to be driven by changes in players B behavior. If any, the treatment effects of information occur because of changes in the way players A perceive players B, rather than through discrepancies between populations of players B.

We first focus on repetition-based learning by comparing outcomes across rounds within the baseline game. The rate of reliance from players A almost double between round 1 and the

[5]Our between- and within- treatments comparisons rely on one-tailed proportion tests. For players B, all differences are insignificant. For each game, we first test differences in the likelihood of decision r in round 1 against aggregate rounds 2-10. The p-values are p=0.46 for the baseline game; p=0.50 for the communication game and p=0.21 for the observation game. As regards comparison between treatments, a Kruskal-Wallis test does not reject the null hypothesis that decisions of players B in all games come from the same distribution, neither in round 1 (p=0.94) nor in aggregate rounds 2-10 (p=0.73).

subsequent occurrences of the game (Z=2.96, p<0.01). Given the stability of players B actual decisions, this suggests that over time players A update beliefs about the population of players B. This increase in reliance induces a significant rise in terms of efficiency, from 23% of first round outcomes to 43% of subsequent repetitions (Z=2.11, p=0.02). Although a slight improvement in terms of coordination results as well, the difference between rounds is not significant (Z=1.14, p=0.13). The increase in efficiency comes at a price in terms of coordination failure: while the risk of Type-I errors falls (Z=2.05, p=0.02), Type-II error gets much more likely (Z=1.66, p=0.05).

In the communication treatment, all outcomes become insensitive to the repetition of the game.[6] As compared to the baseline situation, cheap-talk induces a strong increase in the reliance rate at any stage of the experiment: from 23% in the first round of the baseline to half decisions in the communication treatment; from 52% to 60% in further repetitions of the game. This mainly results in an improvement of the rate of efficient outcomes, which reaches at the very beginning of the game the same level as the one observed after several repetitions in the baseline – the p-value on proportion differences with the baseline are p=0.05 in round 1, and p<0.01 for rounds 2-10. Coordination between players also slightly improves – p=0.15 and p<0.01. As in the baseline, the increase in reliance and efficiency comes with a small rise in Type-II errors – the proportion is significantly higher than in the baseline for the first round, p=0.08, and equal in rounds 2-10 – $p = 0.21$. Rather than helping newly matched partners to better coordinate their decisions, the main effect of cheap-talk is hence to allow them to implement more often the efficient solution when it seems possible. This is reflected in the strong decrease of the proportion of Type-I errors – the decrease is significant at any stage of the game: p=0.06 in the first round, p<0.01 for rounds 2-10.

In contrast with non-biding communication, the observation treatment does not provide specific information to players A at the beginning of the game: the signal becomes available starting at round 2. The treatment appears anticipated by players A: the rate of reliance dramatically decreases at the first round compared to the baseline, resulting in significant falls in both cooperation (p=0.01) and coordination (p<0.01). Future observation has neither a disciplining nor a detrimental effect on players B behavior. Less reliance in the first round hence induces an important increase in the proportion of Type-I errors (p=0.09). Once information becomes available – in round 2-10 – outcomes increase as compared to the baseline (p<0.01 for coordination, p=0.02 for cooperation) to reach levels that are similar to those observed with communication (p=0.29 for the differences as regards coordination, p=0.37 for cooperation). Similarly, both types of errors are reduced compared to the baseline (p=0.04 for both types of errors) to the same extent as they are by communication between players (p=0.18 for Type-I error, p=0.15 for Type-II error).

[6] Comparing round 1 to further repetitions of the game, the tests of proportion lead to p=0.14 for the share of efficient outcomes, p=0.13 for coordination, p=0.11 for the probability of Type-I errors and p=0.50 for Type-II errors.

Table 4: Informational content of signals

	Baseline	Communication			Observation		
		$m(r)$	$m(l)$	$m(r/l)$	B_P	B_{IP}	Unknown
		75.7%	12.0%	12.3%	54.8%	45.2%	(round 1)
Reliant A (R)	49.0%	72.2%	16.7%	21.6%	77.7%	32.0%	16.7%
Reliable B (r)	80.7%	90.3%	38.9%	56.8%	93.9%	68.9%	76.7%
Efficiency (R, r)	41.3%	65.6%	8.3%	16.2%	75.7%	23.8%	3.3%
Coordination $((L, l) \cup (R, r))$	53.0%	68.7%	61.1%	54.1%	79.7%	46.7%	13.3%
Type I error (L, r)	39.3%	24.7%	30.6%	40.5%	18.2%	45.1%	73.3%
Type II error (R, l)	7.7%	6.6%	8.3%	5.4%	2.0%	8.2%	13.3%
Nb of observations	300	227	36	37	148	122	30

Note. For each treatment in column, the rows provide the proportion of observed decisions (first two rows) and outcomes (last four rows). The first column pools all observations from the baseline. In the middle columns, data from all rounds of the communication game are split according to the message received by player A: "I will play r", denoted $m(r)$, "I will play l", $m(l)$, and "I will play either r or l", $m(r/l)$. For the observation game (right-hand side of the table), observations are classified according to the reputation of player B: in rounds 2-10, the reputation is perfect, and denoted B_P, if all previous decisions are r; and imperfect otherwise, denoted B_{IP}. Reputation is unknown in round 1.

3.2 Informational content of signals

Table 4 reorganizes data according to the flow of information. As a benchmark, the first column of the table summarizes the outcomes observed in the baseline once all rounds are pooled. For the communication game, the observations are conditioned on the message received by player A – "*I will play r*", "*I will play l*", "*I will play either l or r*". For the observation game, we use the reputation of each player B to separate the population into two groups: highly reliable ones and others. For that matter, we construct a reputation index for each player B equal to the rate of decisions r amongst all decisions made prior to the current round. We classify players B in each round by comparing their reputation to the cut-off probability of the game (0.964), which makes a risk neutral player A indifferent between choosing L and R. Each player B (in each round) is accordingly classified as entering the interaction with either a perfect reputation (denoted B_P) or an imperfect record (B_{IP}).[7] The last column provides observations from the first round, in which no information is available. For each treatment involving specific information about the partner, those classifications hence organize data according to three kinds of informational content delivered to players A: a positive signal – $m(r)$ in the communication game and B_P in the observation game, a negative signal – $m(l)$ and B_{IP} – or an inarticulate signal – $m(r/l)$ and unknown type of player B.

In both treatments, positive signals operate a strong screening of players B: the rate of reliable

[7]Note, this way of separating players B induces that the first group gathers only those players that constantly played r before the current interaction. As a result, any player B with a perfect record who chooses l once in the game drops out from this category permanently, and becomes B_{IP} ever since.

partners is much higher among those sending a positive signal – through either the message to play r or perfect reputation – than others; in the communication treatment, 90.5% of players pre-announcing decision r happen to be reliable, more than twice the reliability rate amongst players sending one of the two other messages. The screening is weaker in the observation treatment, essentially because of a much higher rate of reliable players B among those associated with a negative signal: the reliance rate increases from 69% to 94% when comparing players B according to their observed reputation. As compared to the baseline, players A appear to account for this information by becoming more reliant on players B with a positive signal: from 50% in the baseline, the reliance rate increases up to 72% against a positive message and 78% against perfect reputation. As a result, any positive signal induces a significant increase in the rate of efficient outcomes, along with a fall in Type-I errors (all comparisons with the baseline are significant at the 1% level).

The comparisons across treatments suggest that positive signals coming from observation are perceived as harder information than positive messages. Interactions with players B_P in the observation game are substantially more fruitful than with senders of message "I will play r" in the communication game. Even though the differences in both players' behavior are not statistically different (p=0.12 and p=0.11), the discrepancies across outcomes are substantial: the likelihoods of both an efficient outcome and successful coordination are significantly greater (p=0.02 and p<0.01), while the odds of Type-I and Type-II errors are considerably reduced (p=0.07 and p=0.02).

The two information transmission mechanisms work in different directions as regards coordination. In the communication treatment, a negative message leaves unchanged the rate of coordination and the proportion of errors as compared to the baseline: the slight increase in coordination, from 53% to 61%, the marginal increase in mistaken reliance (from 7.7% to 8.3%) and the decrease in the proportion of failures to implement potentially efficient interactions are not significant ($p = 0.18$, $p = 0.44$ and $p = 0.15$). In the observation treatment, by contrast, the negative signal induces a fall in the rate of coordination and an increase of Type-II errors. Both are significantly different from the outcomes achieved following the message "*I will play l*": coordination achieved through a negative signal falls from 61% in the communication treatment to 47% in the observation treatment ($p = 0.06$). This seems mainly driven by the inability of players A to account for the very high rate of reliable players B among those associated with an imperfect reputation: this results in a much higher rate of Type-I errors ($p = 0.06$).

While both information-transmission mechanisms provide accurate signals about the nature of the current interaction, they perform quite differently in terms of outcomes: a good reputation performs better than a good message, but a bad reputation performs worse than a bad message. The main driving force of this difference is the ability of players B to reverse their signal from one round to the other. With communication, players B never lose the chance to signal their coopera-

tive behavior, regardless of their past decisions. Thanks to this "clean-sheet" effect, coordination goes well beyond the interaction with those players B that always behave as payoff-maximizers. When information comes from observation, players B are strongly held responsible for their past decisions due to the strong distrust from players A towards any player B who has ever deviated from the payoff maximizing strategy. Whatever their future intentions to behave as payoff maximizers, players B with imperfect reputation are trapped into the inefficient outcome. As a result, a lot of potentially efficient interactions fail to be implemented.

3.3 The determinants of reliance

We test the marginal significance of the effects highlighted above by estimating treatment-specific models on the probability of a reliant decision from players A in each round. We include individual random effects to account for individual heterogeneity and condition observed behavior on time-dummies, observed past behavior and both group and individual information about players B. Due to the conditioning on lagged variables, all models are estimated on data from rounds 2-10. The results from Probit regressions are presented in Table 5.

In the baseline game, the round dummies indicate small variations over time. This effect of time reflects repetition-based learning about players B behavior. The variable $Population_B$ is constructed as the proportion of decisions r among all decisions made in the entire population of players B in the earlier rounds of the experimental session. The effect of this variable thus measures how the true behavior of the population of players B is accommodated for in the experiment – although this information is never available as such to players A in this treatment. Both the coefficient and the marginal effect are significant. In the baseline, repetition-based learning induces a positive correlation between reliance and the overall rate of reliable partners in the population of players B. Interestingly, this effect of time vanishes in the two information-enhancing treatments. Once any information flow becomes available (through either cheap-talk or observation) trustworthiness becomes driven only by this specific information.

In the communication game, the information appears to rely solely on positive messages ("I will play r"), which substantially increase the odds of action R. Note, this effect mainly comes from those players A who already experienced reliance in the past: the joint impact of variables $Message_r$ and its interaction with $\mathbf{1}_{Never\ Trusted}$ does not turn out statistically significant (χ^2=0.41, p=0.52). Message "I will play l" does not change the probability of decision R, as compared with an empty message. This remains true when we test the joint nullity of both the direct effect of this message and its interaction with whether player A has never tried to rely on partners met in the past (χ^2=0.66 p=0.41). For the observation game, we discretize the reputation observed by player A in 6 intervals, and estimate separately the effect of perfect reputation. As compared to the reference category of a bad reputation (past reliability lower than 50%), only very high levels of reputation (higher than a 80% past reliability rate) are able to induce a significant

Table 5: Probit regressions on players A reliance in the three experimental games

	Baseline		Communication		Observation	
	Coeff. (s.d)	ME (s.d)	Coeff. (s.d)	ME (s.d)	Coeff. (s.d)	ME (s.d)
Intercept	-3.56**	—	-0.25	—	-1.94*	—
Gender_A	0.71	0.28	0.39	0.14	0.51	0.19
$\mathbf{1}_{Round=3}$	0.35	0.14	0.14	0.05	0.32	0.12
$\mathbf{1}_{Round=4}$	-0.06	-0.02	0.14	0.05	0.39	0.14
$\mathbf{1}_{Round=5}$	0.07	0.03	0.19	0.07	-0.14	-0.05
$\mathbf{1}_{Round=6}$	-0.24	-0.10	0.22	0.08	-0.28	-0.11
$\mathbf{1}_{Round=7}$	-0.75	-0.27	-0.64	-0.25	0.08	0.03
$\mathbf{1}_{Round=8}$	-1.12*	-0.38**	0.06	0.02	-0.11	-0.04
$\mathbf{1}_{Round=9}$	-1.12	-0.38**	-0.90	-0.35	-0.71	-0.28
$\mathbf{1}_{Round=10}$	-2.07***	-0.54***	-0.86	-0.33	-1.13	-0.42*
$\mathbf{1}_{Trusted\ Before}$	0.33	0.13	-1.05	-0.31*	-0.10	-0.04
$\times$ Round $\times$ Ratio_Rr	0.28**	0.11**	0.21**	0.08**	0.23***	0.09***
Population_B	3.20*	1.28*	-1.05	-0.38	-0.13	-0.05
Message_ r	—	—	2.27***	0.74***	—	—
——$\times\mathbf{1}_{Never\ Trusted}$	—	—	-1.86**	-0.64***	—	—
Message_ l	—	—	-0.44	-0.17	—	—
——$\times\mathbf{1}_{Never\ Trusted}$	—	—	1.14	0.28**	—	—
Reputation $\in [0.5; 0.6)$	—	—	—	—	0.62	0.21
Reputation $\in [0.6; 0.7)$	—	—	—	—	-0.04	-0.01
Reputation $\in [0.7; 0.8)$	—	—	—	—	-0.10	-0.04
Reputation $\in [0.8; 0.9)$	—	—	—	—	0.79*	0.27**
Reputation $\in [0.9; 1)$	—	—	—	—	1.44**	0.37***
Perfect_Reputation	—	—	—	—	2.12***	0.70***
N	270		270		270	
Log L	-110.68		-108.54		-119.13	

Legend. *, **, *** indicate statistical significance at the 10%, 5%, 1% level.
Note. Panel probit regressions with individual random effects. The dependent variable is a dummy indicating whether player A makes a reliant decision R, observations come from rounds 2-10. Marginal effects (ME) are calculated under the assumption that the individual factors u_i are equal to 0. Gender_ A indicates whether player A is a male. The variables $\mathbf{1}_{Round=3}, \ldots,$ $\mathbf{1}_{Round=10}$ are Round fixed effects – Round 2 is omitted. $\mathbf{1}_{Trusted\ Before}$ is a dummy variable switched to one once player A has chosen R in the past. *Population_B* is computed as the rate of past reliable decisions among the whole population of B players in the experimental session. For the communication treatment (middle part) The flow of information is included through the content of the message received ("I will play r" or "I will play l") and its interaction with $\mathbf{1}_{Never\ Trusted}$ (= 1- $\mathbf{1}_{Trusted\ Before}$). For the observation treatment (right-hand side) the reputation of the current interaction partner is accounted for through dummies for each 10% range (past reliability lower than 50% is the reference).

increase in reliance. Partners' perfect reputation induces the highest increase in the use of a reliant strategy, corresponding to a 70 points increase of the probability to rely on the partner.

In all games, players A who become better informed (through the repetition of the game in the baseline, self-committing messages in the communication treatment and the quality of reputation

Table 6: Expected payoffs from the use of the dominant strategies

	Reliability rate			Expected payoffs					
		$Pr(r\|B)$		$E(Payoff(B)\|r)$			$E(Payoff(A)\|R,B)$		
	$Pr(B_P)$	B_P	B_{IP}	B_P	B_{IP}	B	B_P	B_{IP}	B
Baseline	55.6%	92.0%	66.7%	4.07€	4.08€	4.02€	9.56€	7.93€	8.90€
Communication	53.0%	92.3%	66.1%	4.42€	4.21€	4.32€	9.78€	8.41€	9.21€
— Following m(r)	62.1%	97.6%	77.9%	4.48€	4.50€	4.45€	9.85€	8.63€	9.36€
Observation	54.8%	93.9%	68.9%	4.61€	3.69€	4.15€	9.82€	8.21€	9.25€

Note. In each column,the whole population of players B (B) are separated into two subgroups, depending on whether they enter the interaction with a perfect reputation (B_P) or not (B_{IP}). For each treatment in row, the left-hand side provides the average observed payoff earned by players B who choose the payoff maximizing decision r, organized by type. The right-hand side provides the observed payoff earned by players A who rely on their partner by choosing R.

in the observation treatment) are significantly more willing to rely on partners. One virtue of this change in behavior is to strengthen efficiency through reinforcement-based learning. The dummy variable $\mathbf{1}_{Trusted\ Before}$ indicates whether player A has already relied on her partner in the past. This variable *per se* is a very poor predictor of the likelihood of current reliance. We capture reinforcement through interacting this variable with time (variable *Round*) and a measure of success, $Ratio_Rr$, constructed as the proportion of outcomes (R, r) among all decisions R. In all regressions, estimated coefficients indicate that decision R becomes more likely over time the more fruitful are the historical attempts to rely on the partner. This reinforcement-based learning is significant with comparable magnitude in terms of the marginal effects in all treatments.

3.4 Empirical efficiency of reliance

Table 6 summarizes the average payoffs earned by and against players B with different reputation when the decisions of the payoff maximizing strategy, r for player B and R for player A, are actually played. As compared to the benchmark, the introduction of either communication or observation leaves unchanged the overall welfare of trustful players A, while substantially benefiting players B.[8] The welfare of reliant players A significantly increases, though, when reliance is conditioned on either positive signals – received through a message (p=0.04) or an observed perfect reputation (p<0.01). Confronting both kinds of positive signals, observation happens to be more welfare improving for both players than communication. Keeping the message to play r brings on average 4.45€ to reliable players B, while observed perfect reputation leads to 4.61€ (p=0.05); for reliant players A, the payoff increases from 9.36 € to 9.82€ (p=0.01).

[8]Payoffs comparisons are based on student t tests. The p-values of differences of the average payoff earned in the baseline are p=0.12 against communication and p=0.10 against observation for player As who play R; and p<0.01 and p=0.08 for player Bs who play r.

Recall that player A can always guarantee 9.75 € in the game by choosing L, in which case player B experiences a 3 € payoff. As compared to this threshold, reliance never dominates the secure choice: the expected earnings are 8.90€ in the baseline, 9.21€ in the communication treatment and 9.25€ in the observation treatment. Such a payoff structure closely reflects the rather low rate of reliability of the population of players B. The use of information allows players A to discriminate players B. Whatever the treatment, the value of the interaction with a B_P player is always higher than with a B_{IP} one, due to huge differences in the likelihood that r is played in the current round (the difference between reliability rates of both types of players B is significant at the 1% level in all treatments). As detailed on the left-hand side of Table 6, only B_P players exhibit a reliance rate that is on average close to the 0.964 threshold which equalizes the expected theoretical payoffs of players A from the two strategies.

With communication, players A are only partly able to accurately adjust decisions to their current interaction partner: although positive messages signal a higher reliability rate, such information operates an imperfect screening of future strategies. Among B players sending a positive message, only those who played as perfect payoff maximizers in the past happen to be reliable enough: they choose r 97.6% of the time, while B_{IP} players are reliable 77.9% of the time. Since players A do not observe both reputation and messages in this experimental condition, reliance is empirically dominated even if conditioned on positive messages.

Observation, by contrast, allows players A to reward good reputation and punish for bad reputation. This results in a strong inter-type gap in cooperative players B expected gains ($p<0.01$). The substantial difference in players A expected payoffs from relying on either type of players B justify such a discriminatory behavior. Still, players A appear not conservative enough since their average gains from action R remain lower than the 9.75€ generated by decision L.[9]

4 Conclusion

We implement in the laboratory a 2 x 2 game with two Nash-equilibria, one of which is imperfect and arises if one player is reluctant to rely on the rationality of the current interaction partner. In his discussion of this game, Rosenthal (1981) conjectured the imperfect equilibrium may arise depending on: the stakes of the game, and what is known about the interaction partner. Accumulated evidence from experiments confirms the first part of the conjecture Beard and Beil (1994); Beard, Beil, and Mataga (2001); Goeree and Holt (2001). To assess the role of information, we replicate one of the payoff structures used by Beard and Beil (1994) under three information enhancing conditions. The game is repeated in the baseline so as to allow players to learn about the population of their partners. Two further treatments provide specific information about the

[9]Note that in the first round, where no observation is possible yet, the expected payoff of reliant players A is only 4.40 € . This undermines the overall expected payoff, which equals 9.41 € in rounds 2-10 – still lower than the cutoff payoff.

current interaction partner through either one-way cheap-talk communication or observation of partners' historical actions.

Our results show that information greatly contributes to overcoming coordination failures. Repetition of the game as well as information transmission mechanisms improve the efficiency of outcomes. The provision of specific information appears as a substitute to repetition-based learning. The main difference between the two information transmission mechanisms lies in their ability to induce coordination on either equilibrium: actions provide more informative signals on the odds of successful reliance; but strongly decreases the odds of accurately adjusting for unreliable partners.One attractive feature of our design is we elicit decisions of both players in each occurrence of the game. This allows us to assess the empirical efficiency of reliance induced by the likelihood of meeting a reliable player B. We find the unreliability rate is high enough to make unreliance efficient in most situations. In this regard, the transmission of information appears as way to restore the efficiency of reliance, thanks to the individual screening of players B. Interestingly, reputation and communication seems complementary in the screening of future actions from players B, substantiating previous evidence in favour of combining various information devices (see, for instance, Duffy and Feltovich, 2006). Explicitly combining the two information-device hence appears as a natural extension of our analysis.

More importantly, all our treatments conclude to the neutrality on players B behavior of information flows towards players A. This means that neither forward-looking information from cheap-talk messages, nor even backward looking information from observation manage to discipline payoffs minimizing behavior. This strengthens the puzzle associated to the behavior of players B in our and previous implementations of this game. We leave this important question open for future research.

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