Coordination with Communication under Oath*

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Abstract

We focus on the design of an institutional device aimed to foster coordination through communication. We explore whether the social psychology theory of commitment, implemented via a truth-telling oath, can reduce coordination failure. Using a classic coordination game, we ask all players to sign voluntarily a truth-telling oath before playing the game with cheap talk communication. Three results emerge with commitment under oath: (1) coordination increased by nearly 50 percent; (2) senders' messages were significantly more truthful and actions more efficient, and (3) receivers' trust of messages increased.

Keywords: Coordination game; Cheap talk communication; Oath.

JEL Classification: C72; D83.

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1 Introduction

A coordination game captures the idea that value can be created when people coordinate their non-cooperative actions in a strategic environment (see Schelling, 1960; Cooper, DeJong, Forsythe, and Ross, 1990; Van Huyck, Battalio, and Beil, 1990).[1] If people coordinate their otherwise sovereign actions, they can achieve the first best equilibrium among many suboptimal ones. Coordination failure arises when people fail to attain the first best outcome, typically due to strategic uncertainty – the uncertainty associated with not knowing how your opponent will play the game (see, *e.g.*, the survey by Devetag and Ortmann, 2007). Because the main challenge is to influence beliefs, rather than actions, better communication between players is the most frequently prescribed institution to overcome coordination failure.[2]

Accumulated evidence shows *(i)* communication can improve efficiency; but *(ii)* coordination failure still remains because the messages are non-binding cheap talk, *i.e.*, there are no real economic consequences to players who do not act in accordance to the message (see, *e.g.*, Cooper, DeJong, Forsythe, and Ross, 1992; Parkhurst, Shogren, and Bastian, 2004; Blume and Ortmann, 2007).[3] Our hypothesis is that pre-play communication in coordination games fails to fully resolve strategic uncertainty precisely because it is cheap – *i.e.* the link between words and actions is, or at least is perceived to be, too weak. To foster better coordination through communication, one needs to strengthen the tie between reported intentions and deeds from senders, in such a way that receivers do adapt to this information. Recent empirical evidence suggests cheap talk might be credible even if there are no monetary incentives at work to induce truth-telling (see *e.g.*, Gneezy, 2005; Hurkens and Kartik, 2009; Gibson, Tanner, and Wagner, 2013). While the properties of communication under such circumstances have been studied in the recent literature both in theory (*e.g.,* Kartik, Ottaviani, and Squintani, 2007; Kartik, 2009; Demichelis and Weibull, 2008; López-Pérez, 2012; Miettinen, 2013) and with experimental methods (*e.g.,* Vanberg, 2008; Erat and Gneezy, 2012; Sutter, 2009; Serra-Garcia, van Damme, and Potters, 2013), little is still known about how truthfulness can be fostered. This paper adds to the literature on communication in coordination games by focusing on the design of a non-monetary device – a solemn oath – that achieves real economic commitment through communication.

In social psychology, commitment is defined as a "binding of the individual to behavioral acts" (Kiesler, 1971, p.30). Commitment is obtained through preparatory actions, which are purposefully designed to induce a predictable change in subsequent decisions. In so called low-ball

[1]Other seminal contributions by John Van Huyck and his colleagues to improve our understanding of coordination problems include Van Huyck, Battalio, and Beil (1991, 1993); Battalio, Samuelson, and Van Huyck (2001).

[2]Crawford and Sobel (1982) are the first to theoretically show how costless communication can help players reach efficient outcomes. See Sobel (2013) for a recent survey of the theoretical literature.

[3]For instance, in a classic experiment that is closely related to ours, Cooper, DeJong, Forsythe, and Ross (1992) find that only 65% of outcomes are efficient during 11 periods of the stag hunt game with one-way communication. See also Charness (2000) for a discussion of these data and their replication.

experiments, for instance, subjects are asked for their willingness to perform a target behavior before knowing the full costs of the target behavior. Data show that low-balling significantly increases compliance relative to cases in which people are asked to perform the target behavior directly.[4] Following the social psychology theory of commitment, we use a truth-telling oath procedure introduced by Jacquemet, Joule, Luchini, and Shogren (2013) to strengthen the link between players' communication and actions: each player voluntarily signs an oath to tell the truth before he or she enters the lab.[5] Based on the social psychology of commitment, and the results from this previous work, we expect the prior action – an oath to tell the truth – to induce people to be consistent with their initial commitment to tell the truth in subsequent decisions in the experimental game. This commitment is expected to enhance the power of cheap talk communication, thereby reducing strategic uncertainty and facilitating efficient coordination. Thus, the oath is an institutional device that one can use to make talk less "cheap" so as to enhance the effect of communication on efficiency in a coordination problem.

To test these hypotheses, we apply this oath-as-commitment device to a classic coordination game of a sender and a receiver with Pareto rankable Nash equilibria (based on Selten, 1975; Rosenthal, 1981). In this game, it is in both players' monetary interest to be as credible as possible in their use of communication to reach the Pareto dominant outcome. Surprisingly, previous experiments using several variations of this game show that players commonly select dominated strategies, and continue to do so even after allowing for pre-play communication. Our result is that the oath procedure increases the likelihood of coordinating on the efficient outcome in the communication game by nearly 50 percent. This aggregate improvement results from shifts in both players' behavior. First, the senders' messages become more truthful and focused on efficiency, and their choices become more efficient. Second, the receivers show more trust in senders' messages and also act more efficiently. Yet, about 20% of the outcomes remain Pareto dominated. One might wonder whether this could be explained by factors like residual strategic uncertainty and non-standard decision making. We find that the behavior of receivers under oath is similar to the behavior of subjects playing the same game with automated players who create no uncertainty (studied by Hanaki, Jacquemet, Luchini, and Zylbersztejn, 2016). This means that residual coordination failures in the communication under oath treatment are not induced by

[4]See Cialdini, Bassett, Miller, and Miller (1978) for the seminal experiment, Cialdini and Sagarin (2005) for an overview and Joule and Beauvois (1998) for a comprehensive work on procedures that create commitment.

[5]In a series of experiments, Jacquemet, Joule, Luchini, and Shogren (2013) show that the oath works as a strong commitment device for preference elicitation given the oath is taken freely, expressed publicly and signed. Signed undertakings, such as the oath procedure we use here, have also been studied thoroughly in the experimental literature in social psychology. For instance, evidence from field experiments shows that people who have agreed to sign an undertaking to recycle more paper or save water and electricity become much more devoted to these tasks (see for example Pallack, Cook, and Sullivan, 1980; Wang and Katsev, 1990; Katzev and Wang, 1994; Joule, Girandola, and Bernard, 2007; Guéguen, Joule, Halimi-Falkowicz, Pascual, Fischer-Lokou, and Dufourcq-Brana, 2013).

Table 1: The experimental game

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

strategic uncertainty, but rather by deviations from standard rationality. It also suggests that our new institutional design cannot restrain this source of coordination failure.

2 Description of the experiment

We use the normal form game presented in Table 1. The game involves two players: player A, who chooses between actions R and L, and player B, who chooses between actions r and l. If R is chosen by player A, player B can maximize both players' payoffs by selecting action r. Alternatively, player B may choose action l, which slightly undermines her own payoff but sharply decreases player A's payoff. If, in turn, player A chooses L, both players' payoffs do not depend on player B's decision.

2.1 The open challenge

Rosenthal (1981) first introduced this game (under sequential rather than normal form) as a textbook pathological example of the reasons why subgame perfection may fail empirically. Under the assumption that one's own material payoffs summarize individual preferences, (R, r) is a Pareto dominant and a pure-strategy perfect Nash equilibrium. (L, l) also happens to be a Nash equilibrium, which is nonetheless imperfect – since it involves a weakly dominated strategy l from player B. Player A's best choice is R if player B is an own-payoff maximizer who seeks to use the weakly dominant strategy r; and L otherwise. Action L involves the least strategic uncertainty for player A. Depending on the stakes, player A's expected payoff from the decision L can dominate the expected payoff of a reliant decision R, even if the probability that player B uses dominated strategies is extremely low.[6]

We purposefully selected this game for the following reason. Like the classic 2×2 simultaneous-move coordination game – the stag hunt game – our game also exhibits Pareto rankable Nash equilibria. Unlike stag hunt, however, this game also involves dominance solvability: only two stages of removing the weakly dominated strategies suffice. This feature enables us to distinguish

[6]The cut-off probability of decision l by player B which makes player A indifferent between actions L and R equals 0.036.

between two layers of decision-making in coordination problems: the preference to act efficiently (highlighted for player Bs) and the uncertainty on whether others act efficiently (highlighted for player As). This separation is helpful to better understand how communication affects each of these components. In this setting, characterized by an asymmetric strategic uncertainty (born by player A about player B's intentions), one-way communication from player B to player A is a straightforward signaling mechanism.

Maintaining the strategic principles of Rosenthal's game while varying payoffs and decision-elicitation rule, numerous lab experiments report that suboptimal outcomes are omnipresent and arise from two kinds of puzzling behaviors (see the summary of existing experimental evidence in the online Supplementary Material A). First, many player As are reluctant to rely on player Bs. Second, many player Bs' actions are not oriented towards own-payoff-maximization. Altogether, the player Bs widespread use of dominated strategies seem to create strategic uncertainty within the player As – the As then tend to (over)react to this uncertainty. However, another possibility is that the personal monetary gain is too narrow a concept to capture players' preferences in a satisfactory way. In this case, coordination failure would no longer be a matter of strategic uncertainty – which may be overcame using communication – but rather a mismatch between players' actual preferences and the experimenter's assumption about these preferences (see, *e.g.*, Weibull, 2004, for a thoughtful discussion of this issue). For instance, given a highly asymmetric payoff structure maintained in most lab implementations of this game (making the payoff-maximizing outcome the most unequal one), the patterns of behavior observed in the previous studies may be explained by a classic model of other-regarding preferences: the inequality aversion model of Fehr and Schmidt (1999). Jacquemet and Zylbersztejn (2014) provide an empirical test of the hypothesis that coordination failure in this game stems from inequality aversion, and point to a negative answer. They vary the degree of inequality across outcomes, as well as the individual costs of deviating from the payoff-maximizing equilibrium, and observe that both players' behavior depends on their personal payoffs rather than the relative ones.

2.2 Experimental coordination game

The payoff structure we implement appears as Treatment 1 in the sequential-move game experiment of Beard and Beil (1994). Among several reported payoff schemes, this one induces the most striking behavior among participants: *(i)* the frequency of player As' unreliant choices related to this treatment is remarkable: 65.7%, and *(ii)* this is the only treatment in which deviations from the dominant strategy by player Bs were observed (in 17% of all cases in which player A made a reliant decision R). This treatment is also implemented as simultaneous-move normal form game by Jacquemet and Zylbersztejn (2013) who report that roughly 50% of all player As' decisions are unreliant, and that player Bs use dominated strategy in about 20% of all cases once a simultaneous-move game is repeated over multiple periods.

Pre-play communication. Prior to decision making, player B (hereafter, *the sender*) transmits a cheap talk signal to player A (hereafter, *the receiver*), indicating (truthfully or not) her intended decision. We are interested in a fixed-form communication, in which senders choose between three messages: "I will choose r", "I will choose l", or "I will either choose l or r." The first two messages are informative, while the last one is uninformative.

The cheap talk signal that announces action r has interesting theoretical properties: it is self-committing, but not self-signaling (Farrell and Rabin, 1996). It is self-committing because, if trusted, it induces the receiver to choose R, to which the sender's best response is r – exactly as the message announces. As argued by Farrell (1988), this is enough to assure the credibility of the signal. Yet, player B may still use message "I will choose r" to persuade her partner to choose R, and take any action afterwards. Such message is not self-signaling in the sense that it does not reveal sender's true intentions and, as pointed by Aumann (1990), it should be ignored by the receiver. Notwithstanding Aumann's critique, Crawford (1998) suggests that cheap talk communication may be used to *reassure* the receiver about sender's intentions and works to reduce strategic uncertainty in coordination problems. Subsequent experimental studies tend to confirm this point (see *e.g.* Cooper, DeJong, Forsythe, and Ross, 1992; Crawford, 1998; Charness, 2000; Duffy and Feltovich, 2002, 2006; Blume and Ortmann, 2007). Ellingsen and Östling (2010) formalize this idea using the level-k bounded rationality framework. Under the central assumption that agents display a weak preference for truthfulness which is common knowledge, they demonstrate that cheap talk messages convey information about sender's rationality – that is, whether he disregards dominated strategies or not.

In the context of our coordination game, Jacquemet and Zylbersztejn (2013) find that cheap talk communication helps overcome part of the coordination failure, but not enough to improve the efficiency of outcomes. Moreover, its performance is similar to another information-transmission mechanism, in which player A observes the entire set of decisions that player B took in interactions with his past partners. Our experiment seeks to enhance the effect of communication through having subject sign a truth-telling oath.

2.3 Experimental treatments

Baseline treatment (No Oath). Each experimental session consists of 10 rounds of the game presented in Table 1. Roles are fixed. Each participant makes 10 decisions as either receiver or sender. After each interaction, pairs are rematched according to a perfect stranger round-robin procedure that guarantees that each pair of subjects interacts only once throughout the entire experiment.[7] The experimental instructions state that there will be "several" rounds. At the beginning of each round, the sender sends one of the three messages to the receiver by clicking

[7] This procedure, also known as rotation matching, is optimal for our experimental design: for a given number of players and the one-shot nature of each interaction between subjects, it maximizes the number of rounds. See Kamecke (1997) and Duffy and Feltovich (2002) for a related discussion.

on a relevant button on her computer screen. We explain to the subjects that messages sent by senders do not affect their payoffs, and that they can be followed by any decisions. Once the receiver has confirmed receiving the message, the game moves to the decision-making stage, in which the receiver chooses between R and L, while the sender chooses between r and l. Instructions inform the participants that the receiver makes the first decision, followed by the sender. The final payoff depends only on the receiver's decision if L was chosen, and on both partners' decisions otherwise. At the end of a round, each subject is only informed about her own payoff. Next, subjects are told if another round is about to start, or that the experiment ends.[8]

Oath treatment. This treatment uses an identical experimental environment as the baseline treatment, except that each subject is asked to sign an explicit oath before entering the lab.[9] The oath is implemented as follows: prior to entering the lab, each subject is invited to a separate room adjoining the laboratory where she is welcomed by one of the monitors. The monitor offers each subject a form to sign entitled "solemn oath" as presented in Figure 1 – the word "oath" is written on the form and read by the subject, but never said aloud. The *Paris School of Economics* logo on the top of the form and the address at the bottom indicate that it is an official paper; the topic designation and the research number were added so to ensure the credibility. The monitor explicitly points out to the subject before she reads the form that she is free to sign the oath or not, that participation and earnings in the experiment are not conditional on signing the oath, and that whether she signs the oath or not would be private information that would not be revealed to anyone else within or outside the experiment. Subjects are, however, not informed about the topic of the experiment when asked to take the oath. The subject reads the form, which asks whether she agrees "to swear upon my honor that, during the whole experiment, I will **tell the truth and always provide honest answers**" (in bold in the original form). Regardless of whether the subject signs the oath, he or she is thanked and invited to enter the lab. The exact wording used by the monitors to offer the oath to respondents was scripted to standardize the phrasing of the oath. The monitor did not leave the room at any time, and another monitor remained in the lab until all subjects had been presented with the oath, to avoid communication prior to the experiment. Subjects waiting their turn could neither see nor hear what was happening at the oath-desk.

[8] An English translation of the original instructions in French is provided in the Online Supplementary Material, Section C.

[9] The procedures are the same as in Jacquemet, Joule, Luchini, and Shogren (2013) and Jacquemet, James, Luchini, and Shogren (2016).

Figure 1: Oath form used in the experiment

PARIS SCHOOL OF ECONOMICS
ECOLE D'ECONOMIE DE PARIS

SOLEMN OATH

Topic: "JZ"; Research number 1842A

I undersigned .... swear upon my honour that, during the whole experiment, I will:

Tell the truth and always provide honest answers.

Paris, ... Signature..................

Paris School of Economics, 48 Boulevard Jourdan 75014 Paris - France.

2.4 Experimental procedures

Our analysis relies on six experimental sessions (three for each experimental condition), each of them had 20 subjects – 10 receivers and 10 senders.[10] In the oath treatment, subjects first come one by one to the oath-desk and are exposed to the above described procedure. Although signing

[10] The data for the baseline treatment come from the communication treatment of Jacquemet and Zylbersztejn (2013). These sessions have been run in July and December 2009, and in March 2010. Sessions for the oath treatment have been run in March and September 2010, and in January 2012.

the oath was not mandatory, a large majority of subjects accepted to do so. Six subjects did not take an oath. This leads to a 90% acceptance rate. This is in line with previous experiments involving an oath procedure.[11] We apply the intention-to-treat strategy, so that these six subjects are kept in the statistical analysis.

In both treatments, participants are randomly assigned to their computers and asked to fill in a short 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 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 receiver or sender. Subsequently, they are anonymously and randomly matched to a partner. Then, the sender sends a message to the receiver, after which they are both asked for their choices, R or L for receivers, and r or l for senders. At the end of every round, each participant is informed solely about her own payoff. Once all pairs complete a round of the game, subjects are informed whether a new round starts. If this is the case, pairs are rematched. Otherwise, a single 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.

All sessions reported in this paper took place in the lab of University Paris 1 (LEEP). The recruitment of subjects has been carried out via LEEP database among individuals who have successfully completed the registration process on Laboratory's website.[12] The experiment involved a total group of 120 subjects, 72 males and 48 females. 102 subjects are students, among which 58 subjects are likely to have some background in game theory due to their field of study.[13] 28% were first time participants in economic experiment at LEEP. Participants' average age is about 24. No subject participated in more than one experimental session. Each session lasted about 45 minutes, with an average payoff of approximately 12 Euros.

3 Results

Table 2 summarizes aggregate results by round from both treatments. We see our first key result: the likelihood that players coordinate on the Pareto efficient outcome increases by nearly 50 percent due to oath – to 75.0 percent optimal coordination with the oath from 52.7% without. Each type of suboptimal coordination – either $(L; r)$ or $(R; l)$ – is less likely in the oath treatment than in the

[11] See Jacquemet, Joule, Luchini, and Shogren (2013); Jacquemet, James, Luchini, and Shogren (2016); this is also a standard acceptance rate for commitment experiments (see Joule and Beauvois, 1998; Burger, 1999).

[12] The recruitment uses ORSEE (Greiner, 2015); the experiment is computerized through a software developed under REGATE (Zeiliger, 2000).

[13] Disciplines such as economics, engineering, management, political science, psychology, mathematics applied in social science, mathematics, computer science, sociology, biology.

Table 2: Aggregate results of the main treatments

	Round										Overall
	1	2	3	4	5	6	7	8	9	10	
No oath:											
Reliant A (R)	50.0	36.7	50.0	53.3	66.7	70.0	66.7	70.0	60.0	70.0	**59.3**
Reliable B (r)	80.0	70.0	90.0	83.3	66.7	80.0	86.7	83.3	86.7	73.3	**80.0**
Coordination (L,l) or (R,r)	56.7	46.7	60.0	63.3	66.7	70.0	80.0	66.7	66.7	83.3	**66.0**
Efficient outcome (R,r)	43.3	26.7	50.0	50.0	50.0	60.0	66.7	60.0	56.7	63.3	**52.7**
Miscoordination (L,r)	36.7	43.3	40.0	33.3	16.7	20.0	20.0	23.3	30.0	10.0	**27.3**
Miscoordination (R,l)	6.7	10.0	0.0	3.3	16.7	10.0	0.0	10.0	3.3	6.7	**6.7**
Oath:											
Reliant A (R)	70.0	70.0	67.7	83.3	80.0	76.7	76.7	80.0	86.7	83.3	**77.3**
Reliable B (r)	96.7	90.0	90.0	90.0	93.3	90.0	96.7	96.7	96.7	93.3	**93.3**
Coordination (L,l) or (R,r)	73.3	80.0	76.7	86.7	80.0	80.0	73.3	83.3	83.3	76.7	**79.3**
Efficient outcome (R,r)	70.0	70.0	66.7	80.0	76.7	73.3	73.7	80.0	83.3	76.7	**75.0**
Miscoordination (L,r)	26.7	20.0	23.3	10.0	16.7	16.7	23.3	16.7	13.3	16.7	**18.3**
Miscoordination (R,l)	0.0	0.0	0.0	3.3	3.3	3.3	3.3	0.0	3.3	6.6	**2.3**

Note. Columns 1-10 summarize the frequencies of outcomes (defined in rows) as % of all outcomes observed in each round of a given experimental treatment. The last column provides overall results.

baseline. Accounting for both optimal and suboptimal coordination, the commitment-via-the-oath contributes to coordinating players' actions – the frequency of Nash equilibrium increases to 79.3% in the oath treatment from 66% in the baseline.

Result 1 *Commitment via the truth-telling oath increases coordination on the Pareto optimal Nash equilibria to 75 percent of games from 53 percent without the oath.*

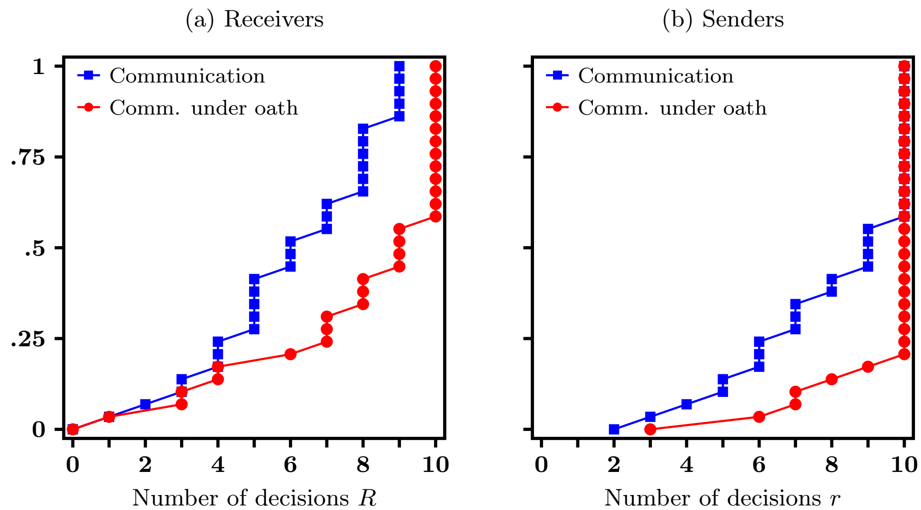
The oath induces an important change in the main outcome of the game: efficient coordination increases by one half once communication occurs under oath rather than alone. In the next sections, we describe the change in individual behavior behind this change in outcome, and how it is related to communication strategies.

3.1 Individual behavior under oath

The increase of coordination induced by the oath is explained by significant changes in individual behavior.[14] When subjects are inexperienced (in round 1), the oath significantly increases player

[14]In our data, each subject takes ten decisions which requires that our statistical analysis controls for within-subject correlation. We do so by carrying out a specific bootstrap proportion test that consists of bootstrapping with replacement subjects and their ten decisions, rather than bootstrapping on single decisions. This allows us to account in our tests for a within-subject correlation of unknown form. Apart from this, the test is based on a standard bootstrap procedure with 9999 draws that yields an empirical bootstrap distribution of players' sets

FIGURE 2: DECISIONS OF RECEIVERS AND SENDERS BY TREATMENT



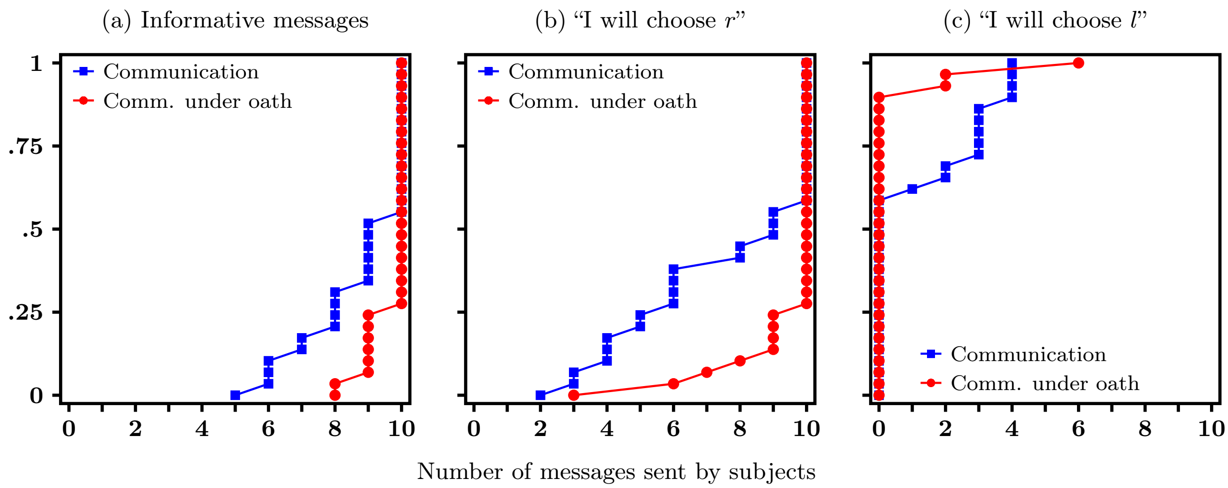
As' reliance and player Bs' reliability ($p = 0.099$, $p = 0.034$ using one-sided bootstrap proportion test).[15] Figure 2 summarizes both players' decision-making patterns across the 10 rounds of the game. Once again, the degree of receivers' reliance is larger in the oath treatment: out of 10 decisions, an average of 5.9 decisions R is observed in the baseline treatment, and 7.7 in the oath treatment. A mean difference bootstrap test indicates that the difference is significant ($p = .029$). Both EDF presented in Figures 2.a are similar on the low end, *i.e* for players who do not play R often, while discrepancies are visible on the upper end, where the oath treatment induces subjects to play R more often. We observe that 43.3% of receivers in the oath treatment choose to play R in all rounds while none of them do so in the baseline treatment (bootstrap proportion test: $p < .001$). The differences between the two EDF are highly significant: the EDF of receivers' behavior in the oath treatment first-order dominates the EDF of receivers' behavior in the baseline ($p = .003$).[16] Substantial differences also appear in senders' behavior, as revealed in Figure 2.b. Based on the empirical frequencies of decisions r, we find that the EDF from the oath treatment first-order

of choices. Like in every lab implementation of the round-robin matching scheme, the second issue arising in our data is the potential between-subject correlation at the session level, since all player As interact with each player B (and vice versa). For the sake of consistency, we apply this same bootstrap testing procedure to independent observations from round 1. For round 1 observations, we also report on the standard Fisher's exact test on both players' decisions that relies on the independence assumption.

[15]One-sided Fisher's exact test yields highly consistent results: $p = 0.094$ and $p = 0.051$.

[16]This result comes from a bootstrap version of the univariate Kolmogorov-Smirnov test. This modified test provides correct coverage even when the distributions being compared are not entirely continuous (since ratios are discrete by construction) and, unlike the traditional Kolmogorov-Smirnov test, allows for ties (see Abadie, 2002; Sekhon, 2011). Note that we use one observation per subject, based on the number of decisions R made by a given player A throughout the game, so that the within-subject correlation is not an issue here.

Figure 3: Communication behavior of senders by treatment



dominates the EDF from baseline ($p = .003$). Senders are more likely to cooperate with receivers given the oath. 80% of senders choose to play r in all 10 rounds in the oath treatment, while only 43.3% do so in the baseline. A bootstrap test for equality of proportions indicates that the difference is significant at the 1% level ($p = .006$).

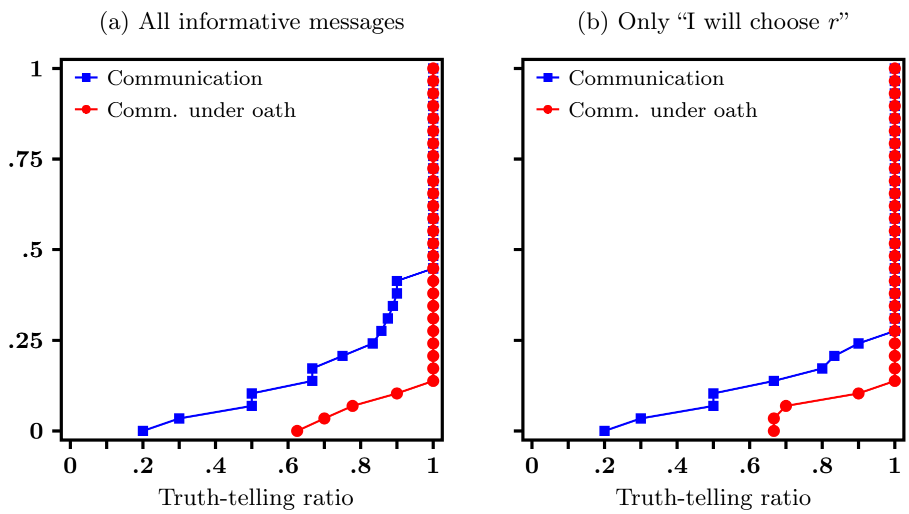
Result 2 *The oath fosters player As' reliance and player Bs' reliability.*

Communication under oath improves coordination through shifts in player's behavior towards more payoff-maximizing decisions. We now turn to an analysis of how this shift in behavior is related to communication.

3.2 Communication under oath

Figure 3 focuses on the patterns of communication by senders and shows the empirical distribution functions (EDF) of the number of messages sent over all 10 decision periods. Each dot inside the graph indicates an individual, on the x-axis we present the number of messages he has sent (between 0 and 10), the y-axis represents the probability of observing an individual who has sent at most a given number of messages. Figure 3.a depicts the empirical distribution of informative messages ("I will choose r" and "I will choose l") in both treatments. In both cases communication is widely used by senders, since no subject has abstained from sending at least one informative message. The number of informative messages sent by each subject is relatively high, ranging from 5 to 10 messages in the baseline treatment and 8 to 10 messages in the oath treatment. 14 of 30 subjects (46.7%) in the baseline treatment sent 10 informative messages, 22 subjects out of 30 (73%) do so in the oath treatment. The difference is significant according to a bootstrap test for equality of proportions ($p = .067$). Overall, senders in the oath treatment seem to use communication more

Figure 4: Truthfulness of senders by treatment (Empirical distribution function)



often than senders in the baseline treatment, first-order dominance of the EDF of informative messages in the oath treatment over informative messages in baseline is statistically significant ($p = .036$).

Figure 3.b presents the analogous EDF exclusively for messages "I will choose r". We find that subjects in the oath treatment display a stronger willingness to signal their credibility than subjects in the baseline treatment: the EDF from the oath treatment first-order dominates the baseline treatment ($p = .009$). Finally, Figure 3.c focuses on messages "I will choose l". These messages are seldom used – only 3 of 30 subjects sent this at least once in the oath treatment, whereas 12 of 30 do so in the baseline; this difference is significant at the 5% level ($p = .003$). Altogether, we find that individual communication behavior varies in quantity – since senders send more often informative messages in the oath treatment –and does change in quality – in the oath treatment messages "I will choose r" are sent significantly more often, while messages "I will choose l" are sent significantly less often.

Result 3 *The oath (1) increases the informativeness of transmitted signals and (2) changes the signal's structure by substantially increasing the use of reassuring announcements.*

We now explore the link between messages and actions through the truth-telling behavior of senders. For sender we calculate the proportion of cases in which the action is coherent with the message (given it is informative) for only the informative messages. We call this the "truth-telling ratio". Figure 4.a presents the EDF from both treatments. We find evidence that misinforming one's partner about intended move is substantially more widespread without the oath. First, 26 of 30 senders (87%) always reveal their actual intentions when sending an informative message in the oath treatment relative to only 17 of 30 player Bs (57%) in the baseline treatment. The increase is statistically significant with $p = .005$ according to a bootstrap proportion test. Second, the EDF from the oath treatment significantly first-order dominates the EDF from baseline ($p = 0.011$). In Figure 4.b, we represent the same truth-telling ratio using only messages "I will choose r". Here the difference between the two treatments is more ambiguous. The proportion of subjects always telling the truth conditional on sending this message is 73.3% and increases to 86.7% under oath. This increase is slightly insignificant ($p = 0.144$) using bootstrap proportion test. The EDF of the truth-telling ratio from the oath treatment still first-order dominates the EDF from baseline, but the result is not statistically significant ($p = 0.195$).[17]

Result 4 *The oath improves the truthfulness of announcements.*

Results 2, 3 and 4 can be summarized by the following descriptive statistics. In the baseline, we observe that 227 honest messages were sent – senders use truthful communication in 75.7% of interactions – whereas in the oath treatment, 281 honest messages are sent – truthful communication is used in 93.7% of interactions. More importantly, the proportion of truthful communication that announces the reliable decision r is only 68.3% in baseline (205) and increases to 91.0% in the oath treatment (273).

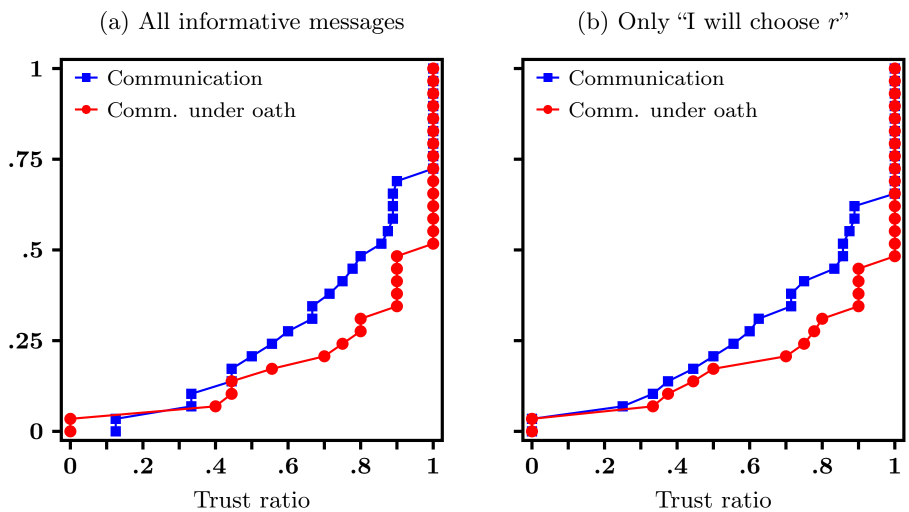
Last, Figure 5 illustrates the receivers' perception of information obtained from their partners, conditional on the presence of oath. For each receiver, and for the subset of informative messages, we calculate the proportion of cases in which the response is in line with the signal, *i.e.* the receiver plays R after the sender announces action r, or L after a message announcing move l. We call this proportion the "trust ratio", and present its EDF in Figures 5.a and 5.b. Figure 5.a suggests the oath affects how receivers react to messages: the EDF obtained in the oath treatment first-order dominates the EDF obtained in the baseline ($p = .020$). This hold even when we focus to messages "I will choose r", as it can be seen in Figure 5.b. The differences in behaviors are less pronounced but still significant ($p = .038$).

Result 5 *The oath improves the receiver's trust toward the announcement.*

Comparing coordination through communication with an without the oath, we observe two simultaneous shifts in behavior. First, both player As and player Bs take decisions leading to the efficient outcome more often. Second, this results in improved coordination because communication is more efficient: player Bs announce decision r more often and act more truthfully; and

[17] Comparing the data reported in Figures 4.a and b, we clearly see that the effect of the oath has the expected direction in both cases. We furthermore suspect that the effect reported in the latter is slightly insignificant due to a relatively high baseline rate that reduces the size of the treatment effect and brings down the statistical power of difference testing. Conventional power calculation (based on a standard one-sided proportion test supporting the significance of the effect of the oath on the truth-telling ratio, with $\alpha = 5\%$) for the data reported in Figure 4.b yields the power of 0.357. The same test performed on the data reported in Figure 4.a has a power of 0.838. We thank a referee for pointing this out.

Figure 5: Messages trusted by player As (Empirical distribution function)



such announcements are trusted and followed by player As. The oath unambiguously improves communication, which translates into more efficient coordination between the players. Our interpretation of these results is that the oath helps build commitment that strengthens the link between subjects' words and actions – cheap talk no longer happens to be cheap. For the senders, this implies more truthful messages; for the receivers who accurately account for this change, this comes with increased trust in the messages received. In the next section, we report on additional data aimed at assessing the reliability of this interpretation and the robustness of the effect of the oath.

4 Robustness treatments and sensitivity analysis

In this section, we complement our analysis by addressing two potential concerns. First, we test whether the oath enhances the power of communication to induce cooperative behavior, or simply induces subjects to behave more cooperatively. Second, we explore the link between players' commitment and strategic uncertainty by comparing the results of the coordination game with communication (with and without an oath) to a behavioral benchmark in which strategic uncertainty is ruled out from player As' decision making.[18]

[18] A third concern is that the oath stimulates cooperation in the communication treatment because the communication technology we use inherently lacks commitment: subjects can use neutral announcements rather than promises (Bochet and Putterman, 2009; Lundquist, Ellingsen, Gribbe, and Johannesson, 2009). To explore this hypothesis, we designed another robustness treatment in which messages are worded as promises (with no oath). We fail to find any effect of such framing, as reported in detail in the online supplementary Material, SectionB.

TABLE 3: COORDINATION UNDER OATH, WITHOUT COMMUNICATION: AGGREGATE RESULTS

	Round										**Overall**
	1	2	3	4	5	6	7	8	9	10	
No oath:											
Reliant A (R)	23.3	36.7	50.0	50.0	60.0	60.0	56.7	53.3	56.7	43.3	**49.0**
Reliable B (r)	80.0	80.0	90.0	83.3	73.3	80.0	76.7	76.7	80.0	86.7	**80.7**
Coordination (L, l) or (R, r)	43.3	43.3	53.3	60.0	66.7	60.0	60.0	43.3	63.3	36.7	**53.0**
Efficient outcome (R, r)	23.3	30.0	46.7	46.7	50.0	50.0	46.7	36.7	50.0	33.3	**41.3**
Miscoordination (L, r)	56.7	50.0	43.3	36.7	23.3	30.0	30.0	40.0	30.0	53.3	**39.3**
Miscoordination (R, l)	0.0	6.7	3.3	3.3	10.0	10.0	10.0	16.7	6.7	10.0	**7.7**
Oath:											
Reliant A (R)	33.3	40.0	40.0	40.0	46.7	46.7	43.3	46.7	40.0	50.0	**42.7**
Reliable B (r)	86.7	66.7	83.3	93.3	80.0	83.3	86.7	90.0	80.0	90.0	**84.0**
Coordination (L, l) or (R, r)	46.7	46.7	36.7	40.0	60.0	43.3	43.3	43.3	40.0	46.7	**44.7**
Efficient outcome (R, r)	33.3	26.7	30.0	36.7	43.3	36.7	36.7	40.0	30.0	43.3	**35.7**
Miscoordination (L, r)	53.3	40.0	53.3	56.7	36.7	46.7	50.0	50.0	50.0	46.7	**48.3**
Miscoordination (R, l)	0.0	13.3	10.0	3.3	3.3	10.0	6.7	6.7	10.0	6.7	**7.0**

Note. Columns 1-10 summarize the frequencies of outcomes (defined in rows) as % of all outcomes observed in each round of a given experimental treatment. The last column provides overall results.

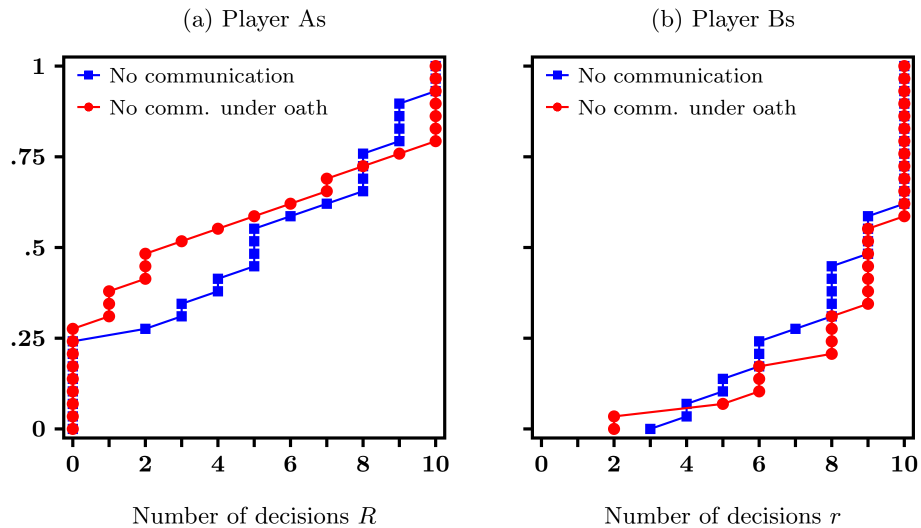
4.1 Commitment without communication

We observe two simultaneous shifts in behavior in our previous treatments. First, players behave more efficiently under oath: both player As and player Bs take decisions leading to the Pareto-Nash equilibrium outcome more often. Second, coordination improves because communication is more efficient: player Bs announce decision r more often, act more truthfully, and enjoy more trust from player As. What remains to be understood is whether improved coordination results from a commitment to keep one's word or if the oath also induces subjects in both roles to behave more cooperatively. To find this out, we design a robustness experiment to capture the direct effect of the oath on behavior and run the coordination game without communication.

This **no communication** condition introduces one change to the previous design: now, player Bs cannot announce their intention to player As. To contrast the effect of the oath on actions without communication, we run two treatments one *No-oath*, one *Oath.*[19] Table 3 summarizes

[19]For each of the two conditions, our data come from 3 sessions, involving 20 subjects each. The data for the baseline-no communication condition are taken from the baseline treatment of Jacquemet and Zylbersztejn (2013). These sessions have been run in June and December 2009, and in March 2010. The oath-no communication sessions have been run in October 2012, with all subjects but two (58/60) freely deciding to sign the oath. Among the total of 120 participants (56 males and 64 females), 105 are students – with 54 students enrolled in programs in economics, engineering, management, political science, psychology, mathematics applied in social science, mathematics, computer science, sociology, biology. Subjects' average age is 23, 54% took part in an experiment before. The average payoff is approximately 12 Euros including the show-up fee of 5 Euros.

Figure 6: Observed decisions without communication



aggregate results by round from the no-communication treatments. We observe that the oath does not induce differences in aggregate behavior. The likelihood that players coordinate on the efficient outcome is 41.3% in the baseline-no communication and 35.7% in the oath-no communication treatment. Coordination, optimal or not, accounts for 53.0% of the outcomes in baseline and 44.7% when subjects took an oath prior to the experiment.

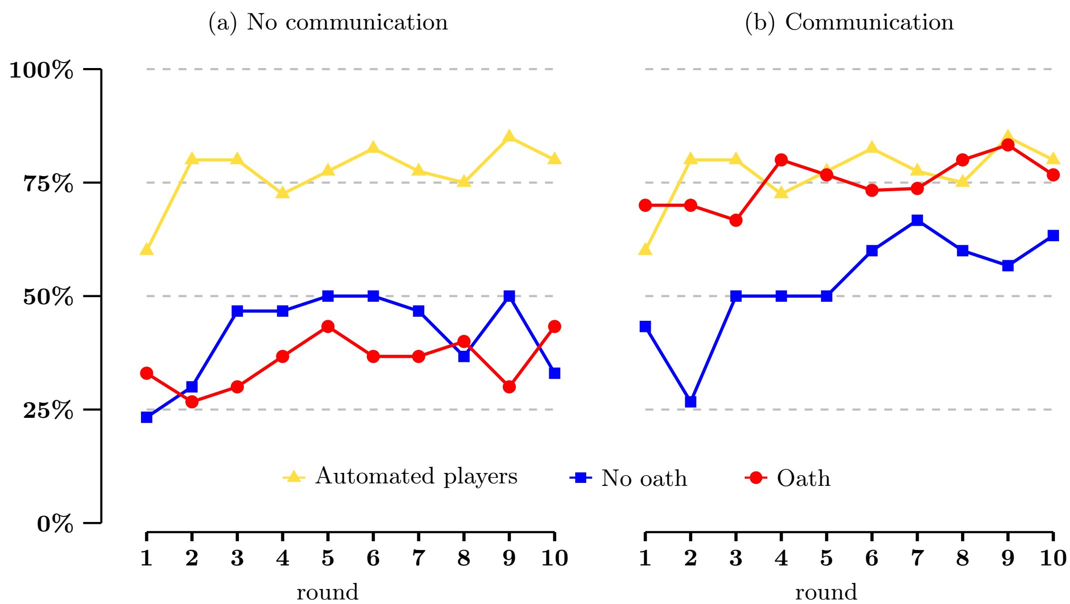
In the absence of communication, one does not observe that the oath significantly increases the frequencies of actions R or r among inexperienced players, *i.e.* in round 1 ($p = 0.372$, $p = 0.411$ using a one-sided bootstrap proportion test).[20] In addition, Figure 6 summarizes the decision-making pattern of player As and player Bs during the entire game. These data point to the same conclusion: the oath does not induce any change in behavior when communication is not allowed. Out of 10 decisions, 4.9 decisions R are on average taken by player As without oath, as opposed to 4.3 with an oath –the difference is not significant ($p = .535$). The average number of decisions r from player Bs equals 8.1 without oath and 8.4 with an oath– the difference is not significant, $p = .564$. Bootstrap distribution tests indicate that the EDF are not significantly different with $p = .416$ for player As and $p = .676$ for player Bs.

Overall, the results support the first of the two explanations stated above: the oath changes behavior through its effect on communication, rather than on behavior itself.

Result 6 *The oath alone does not improve efficient coordination, rather the oath works through communication.*

[20] One-sided Fisher's exact test yields highly consistent results: $p = 0.284$ and $p = 0.365$.

Figure 7: Efficient coordination in human subjects and automated players treatments



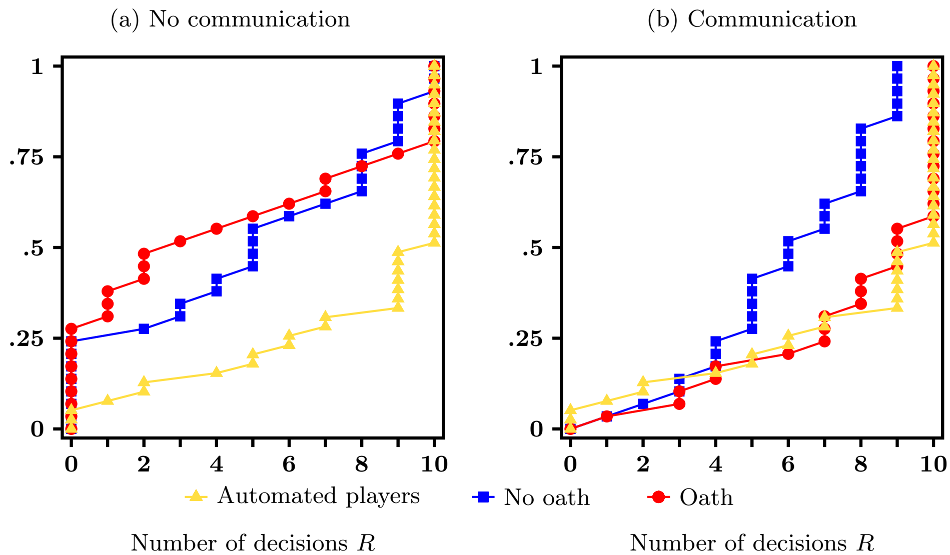
4.2 Behavioral benchmark: coordination without strategic uncertainty

Communication under oath increases efficiency by 50% and subjects coordinate on the Pareto efficient outcome 75% of the times. Although this is a significant improvement in efficiency, the open question is whether one could further reduce coordination failures through an even more efficient communication technology. To provide evidence on that issue, we borrow the results from Hanaki, Jacquemet, Luchini, and Zylbersztejn (2016) based on a game without communication identical to our **baseline-no communication** treatment but with one important exception: player As face automated players instead of human subjects in the role of player Bs. Automated players are programmed so as to always play the payoff-maximizing strategy r, and this is common knowledge to the subjects in the experiment. Apart from this change, the whole experimental design is identical.[21] Note that player As now have no "rational" reason to use action L as the automated players treatment completely removes strategic uncertainty. Since no participant in the lab actually receives the computerized player's payoffs, it also eliminates any form of other-regarding preferences. This treatment provides a behavioral benchmark –*i.e.* the maximum level of efficiency one can expect from the actual behavior of subjects to the experiments. Two sessions for this treatment (involving 60 player As) have been carried out in October 2012.

In Figure 7, we present the likelihoods of attaining the Pareto efficient outcome in the automated treatment and our four human treatments: no-communication treatments in Figure 7.a and

[21] An English translation of the original instructions in French is provided as Online Supplementary Material, Section D.

Figure 8: Player As' behavior against automated players



communication treatments in Figure 7.b. At the aggregate level, efficient coordination amounts to 77% of observed outcomes in the automated players treatment and is relatively stable across all rounds after round 1.[22] Efficient coordination without communication, with or without oath, falls bellow the automated players treatment, reaching a maximum of merely 50% in the no oath condition. When communication is allowed but no oath taken, efficient coordination is still below the level obtained in the automated players treatment. But when subjects communicate under oath, we observe an increase in efficient coordination to attain the level observed in the automated benchmark.

To further explore this comparison at the individual level, Figure 8 reports the EDF of the number of decisions R taken by each player A throughout the experiment in the automated players treatment together with the EDF observed in our four treatments. As shown in Figure 8.a, the EDF from the automated players treatment first order dominates that of **baseline-no communication** ($p = .001$) and that of **oath-no communication** ($p = .004$). In Figure 8.b, the EDF of the automated players treatment is plotted with the communication treatments. The EDF of decisions in the **baseline-communication** condition is significantly dominated by the EDF in the automated players treatment ($p < .001$). EDF from the **oath-communication** condition and

[22] As reported by Hanaki, Jacquemet, Luchini, and Zylbersztejn (2016), subjects' inability to attain perfect efficiency is persistent across different payoff structures. The data furthermore suggest that inefficient behavior cannot be accounted for by such factors as subjects' distrust towards the experimenter or if they get tired of facing the same decision problem many times. Interestingly, they also find that the degree of inefficient behavior is related to subjects' reasoning and information processing skills (measured by their score in Raven's progressive matrices tests).

in the automated players treatment are not significantly different ($p = 0.988$).

To summarize, player As facing automated players and player As exposed to communication under oath exhibit exactly the same pattern of behavior. By construction, strategic uncertainty plays no role in player As' behavior towards automated player Bs. This comparison suggests that committed communication via the oath manages to reduce failures to reach the efficient outcome due to strategic uncertainty.

Result 7 *Communication under oath achieves the same outcome and behavior as when subjects play against perfectly efficient automated players; the residual inability to reach efficient coordination is not due to strategic uncertainty.*

5 Conclusion

Overcoming coordination failure through economic design can be enhanced with more insight into non-monetary commitment devices. Evidence suggests that communication alone does not eradicate coordination failure, especially if the person's words are not clearly backed up by deeds. Our paper investigates whether the social psychological concept of commitment can improve the degree to which communication reduces strategic uncertainty and thereby increases coordination on efficient outcomes. We create commitment by having our players sign a voluntary solemn oath to tell the truth prior to entering the laboratory.

Our results suggest that the truth-telling oath increases optimal coordination by nearly 50 percent. In our communication game, senders are more likely to send informative and reassuring messages, and then to do what they said they would. More receivers trusted the messages received under oath than without it, but some remained wary. The oath appears to create a commitment in the sense that senders adhere to actions they signaled. The communication game with oath attains an efficiency level that is the same as the behavioral equilibrium when strategic uncertainty is completely eliminated as a decision-making motive. This suggests that coordination under oath works well to remove strategic uncertainty, but not bounded rationality.

In line with some recent literature on lying behaviour we show that the decision process matters beyond consequences. We find that performing certain actions, even those that have no monetary consequences, has the potential to subsequently induce certain behaviors from the decision-makers. In our case, an oath that is freely signed by nearly all subjects, (*i*) commits the senders to do what they say, and (*ii*) the receivers to put trust in this commitment. In our experiment, the monetary incentive and the oath are aligned in the sense that they both point at the efficient outcome. One interesting avenue for future research would be to disentangle the two motives and to study truth-telling under oath when lying is beneficial. This would also help clarifying the reasons behind increased willingness to tell the truth when acting under oath.

Finally, while we provide evidence substantiating the commitment interpretation of enhanced truth-telling through the oath, the fact that the oath makes reference to truth-telling may point to a different interpretation, based on "priming". A key difference, however, between the oath and priming in social psychology is that priming is purely cognitive and works by introducing a stimulus such that it goes unnoticed by the subject. The goal of priming is *"to sensitize the nervous system to a later presentation of the same or a similar stimulus"* (Kolb and Whishaw, 2009, p.494), whereas commitment-via-oath is a behavioral act. Priming is an implicit memory effect that occurs without the person's awareness and direct consent. As a matter of fact, priming experiments in social psychology typically include a test of awareness (see, e.g. Doyen, Klein, Simmons, and Cleeremans, 2014) and the common practice is to drop subjects who happen to be aware of the presence of priming from the data. Our oath procedure, in contrast, explicitly refers to truth-telling and gives each subject the freedom to sign the oath or not. Although we think this interpretation is unlikely to explain our results, addressing this question is next on our agenda. We plan to study the combined effect of priming and the truth-telling oath in a context in which lying might be beneficial to the liar.

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Online Supplementary Material

A Overview of existing experimental evidence

The table below presents a summary of existing experimental evidence. Several representations of the game have been applied so far, as stated in column 1: simultaneous-move strategic-form game (Str), simultaneous-move extensive-form game (Ext), sequential-move game (Seq). The monetary payoffs displayed in columns 2-4 are in USD in Beard and Beil (1994) and Cooper and Van Huyck (2003), in cents of USD in Goeree and Holt (2001), in Yens in Beard, Beil, and Mataga (2001), and in Euros in Jacquemet and Zylbersztejn (2014).

Experiment	Form	Payoff			Outcomes (%)				
		(L)	(R, r)	(R, l)	L	R, r	R, l	$r\|R$	r
Beard, Beil–Tr.1	Seq	(9.75; 3.0)	(10; 5.0)	(3; 4.75)	66	29	6	83	—
Beard, Beil–Tr.3	Seq	(7.00; 3.0)	(10; 5.0)	(3; 4.75)	20	80	0	100	—
Beard, Beil–Tr.4	Seq	(9.75; 3.0)	(10; 5.0)	(3; 3.00)	47	53	0	100	—
Beard et al.–Tr.1	Seq	(1450; 450)	(1500; 750)	(450; 700)	79	18	3	83	—
Beard et al.–Tr.2	Seq	(1050; 450)	(1500; 750)	(450; 700)	50	32	18	64	—
Goeree, Holt–Tr.1	Ext	(80; 50)	(90; 70)	(20; 10)	16	84	0	100	—
Goeree, Holt–Tr.2	Ext	(80; 50)	(90; 70)	(20; 68)	52	36	12	75	—
Goeree, Holt–Tr.3	Ext	(400; 250)	(450; 350)	(100; 348)	80	16	4	80	—
Cooper, Van Huyck–Tr.9	Str	(4; 1)	(6; 5)	(2; 4)	27	—	—	—	86
Cooper, Van Huyck–Tr.9	Ext	(4; 1)	(6; 5)	(2; 4)	21	—	—	—	84
JZ, 2014–Baseline (BT1), round 1	Str	(9.75; 3.0)	(3.0; 4.75)	(10; 5.0)	77	23	0	100	80
JZ, 2014–Baseline (BT1), rounds 2-10	Str	(9.75; 3.0)	(3.0; 4.75)	(10; 5.0)	48	43	9	84	81
JZ, 2014–Baseline (BT1), overall	Str	(9.75; 3.0)	(3.0; 4.75)	(10; 5.0)	51	41	8	84	81
JZ, 2014–ET1	Str	(9.75; 5.0)	(5.0; 9.75)	(10; 10.0)	54	33	13	72	73
JZ, 2014–ET3	Str	(9.75; 5.5)	(5.5; 8.50)	(10; 10.0)	39	48	13	79	76
JZ, 2014–ET4	Str	(8.50; 5.5)	(5.5; 8.50)	(10; 10.0)	25	61	14	82	82
JZ, 2014–ET2	Str	(8.50; 8.5)	(6.5; 8.50)	(10; 10.0)	26	70	4	94	94
JZ, 2014–BT2	Str	(8.50; 7.0)	(6.5; 7.00)	(10; 8.5)	26	70	4	94	94

B Robustness check: Neutral announcements versus promises

The **promises** treatment is a variation of the basic communication treatment, with no oath, in which only the wording of the messages is modified: cheap talk messages (*"Je vais prendre la décision ..."* / "I will choose ...") are turned into promises (*"Je m'engage à prendre la décision ..."* / "I promise to choose ...").[23]

We find no significant differences either in senders' decisions or in their patterns of communication. Figure 9 compares senders and receivers' behavior in both treatments. Figure 9.b indicates that senders' decisions do not vary significantly between the two communication treatments. First order dominance of the EDF from the baseline communication treatment is not significant with $p = .230$. In round 1, the rate of actions r equals 80% in both samples.[24] Overall, the mean number of decision r by subject across

[23]We carried out 2 additional sessions with this new form of communication, each involving 20 subjects and carried out in the lab of University Paris 1 (LEEP) in December 2009 and September 2010. Among 40 participants (14 males, average of about 26), 28 were students with various fields of specialization. 16 subjects were first time participants in economic experiment in LEEP.

[24]Two-sided bootstrap proportion test yields $p = .934$ and two-sided Fisher's exact test yields $p = 1.000$.

Figure 9: Promises robustness treatment: Decisions of receivers and senders by treatment

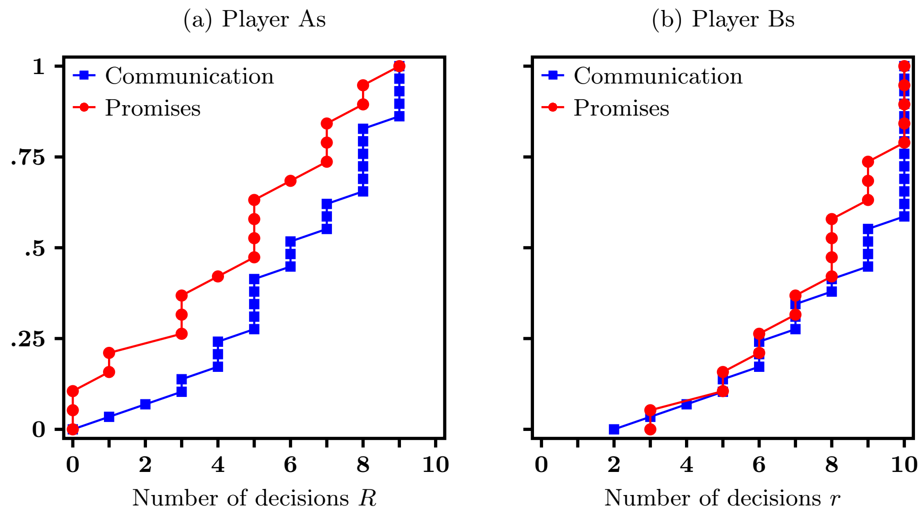
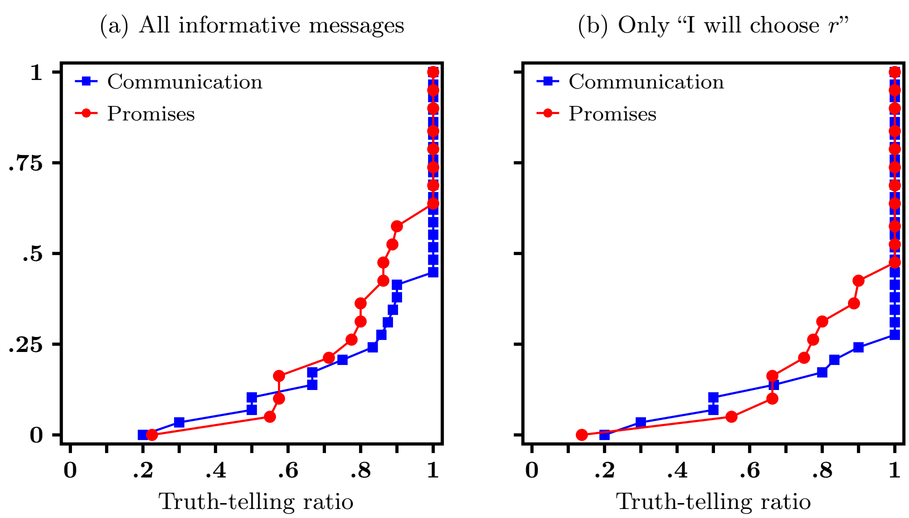


Figure 10: Promises robustness treatment: Truthfulness of senders



10 rounds is 8 with neutral announcements and 7.5 with promises ($p = .500$, two-sided bootstrap mean test). The next two figures report on the truthfulness of senders' messages. Figure 10.a presents the EDF from both treatments for all informative messages and Figure 10.b presents the EDF solely for messages announcing action r. The proportion of senders always sending truthful messages is slightly greater in the baseline communication treatment than in the promises treatment (56.7% vs 40.0%), but the difference is not significant (two-sided proportion test, $p = .240$). The EDF from baseline communication indicates that deceitful messages are less frequent in the former but first order dominance is not statistically significant ($p = .202$). The same conclusions hold when focusing only on the truthfulness of messages announcing r.[25]

[25]The p-values are .178 and .196.

Figure 11: Promises robustness treatment: Communication behavior of senders by treatment

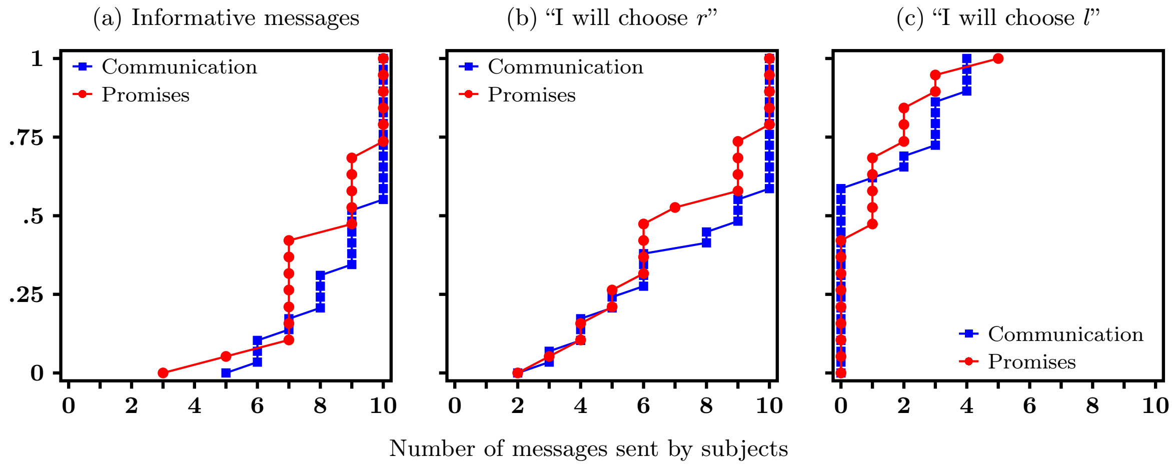


Figure 11 provides a comparison of communication behavior of senders at the individual level. Figure 11.a depicts the empirical distribution of informative messages ("I will/promise to choose r" and "I will/promise to choose l") in both treatments. In both cases communication is widely used by senders, but the EDF indicate that subjects in the promises treatment send fewer informative messages, showing more reluctance to communicate when messages express promises. The number of informative messages sent by subject ranges from 5 to 10 for neutral messages and from 3 to 10 in the promises treatment. This small decrease in informative communication is significant at a 10% threshold with the EDF of informative messages with baseline communication first order dominating the EDF of informative messages in the promises treatment ($p = .093$). The decrease in informative communication is induced by a decrease (albeit small) in both the number of r messages and l messages sent. Figure 11.b presents the EDF exclusively for messages "I will choose r" and Figure 11.c focuses on messages "I will choose l". We observe a slight decrease in the frequency of these messages but, taken separate this decrease is not significant with $p = .254$ and $p = .266$ (using the one-sided bootstrap test presented above).

Overall, the main outcome for the purpose of this paper is that the use of promises instead of neutral messages does not achieve the same outcome as neutral messages under oath. Both the signaling behavior and actions of the senders are only marginally affected by the new treatment.[26] This result is in line with Bochet and Putterman (2009) and Lundquist, Ellingsen, Gribbe, and Johannesson (2009) who suggest that, although cheap talk in open form format is sometimes interpreted as promises, the effect of announcements is not driven by the way they are phrased – fixed-form messages expressing "promises" do not bring any improvement as compared to a neutral cheap talk type of communication.

[26] At the aggregate level, our results even suggest that efficient coordination is less likely with promises than with cheap talk messages– with the frequency of efficient coordination (R, r) decreasing from 52.7% in the baseline communication treatment to 33.5% in the promises treatment. This result is mainly driven by the receivers' reluctance to rely on senders' messages, which makes them choose R less often in the promises treatment. One interpretation is that false promises may be more detrimental to efficient coordination than false neutral messages. Additional results are available upon request.

C Written instructions: communication treatments

You are about to take part in an experiment in which you can earn money. The amount of your gains will depend on your decisions, as well as on decisions made by other participants.

Before starting, we would like to ask you to answer a few standard questions (concerning your age, education, profession, ...) which will help us to get to know you better. **This information, as well as the amount of your gains from this experiment, will remain strictly confidential and anonymous.**

Please, fill in the questionnaire using the interface on your computer screen, which is divided into three parts:

- In the *top* section, you will find information that might help you in making decisions.
- In the *middle* section, you will submit your decisions by clicking on a relevant button.
- In the *bottom* section, you will see all your decisions and gains from previous rounds of the experiment.

Thank you.

The experiment

The experiments consists of several identical rounds. In each round, participants are divided by groups of two. Each pair consists of one player A and one player B. You will be randomly assigned to your role — player A or player B — at the beginning of the experiment, and retain it throughout the experimental session. A message on your computer screen will inform you about your role. **Your role will remain unchanged throughout the entire experiment.**

What happens in each round

At the beginning of each round, participants are be matched into pairs: if your are player A, then a player B is randomly selected to your complete pair; analogously, if your are player B, then a player A is randomly selected to complete your pair. Your pair will **change after each round**, and two participants in opposite roles **may interact at most once during the experiment.**

Each round consists of 6 stages.

Stage 1. At the beginning, a participant is randomly matched to your group.

Stage 2. Player B is asked **to send a message to player A** by choosing one of the options displayed on his/her computer screen and submitting it by clicking 'OK'. **This message does not affect neither players' earnings.**

Stage 3. Player A reads the message from player B and then clicks 'OK' in order to proceed to the next stage.

Stage 4. Player A chooses between L and R by clicking on a relevant button on his/her computer screen.

Stage 5. Player B chooses between l and r by clicking on a relevant button on his/her computer screen.

Stage 6. End of the round and each player is informed about his/her earnings:

- If **player A chose** L, then **regardless of player B's decision**:
  - ▶ Player A earns 9.75 €in this round;
  - ▶ Player B earns 3 €in this round;
- If **player A chose** R then:
  - – if **If player B chose** l:
    - ▶ Player A earns 3 €in this round;
    - ▶ Player B earns 4.75 €in this round;
  - – if **player B chose** r:
    - ▶ Player A earns 10 €in this round;
    - ▶ Player B 5 €in this round;

At the end of each round, a message on your computer screen will inform you that either a new round is about to start, or that the experiment ends.

Payment of your earnings

At the end of the experiment, **one round is picked at random**. Each participant receives a sum in EUR corresponding to the amount he/she earned in this round, plus a bonus of 5 €for completing the experiment. Payments are made individually and in cash.

For obvious reasons, **you are not allowed to talk during the experiment**. Participants who violate this rule will be excluded from the experiment and all payments. It is crucial that you understand perfectly the rules of this experiment. Should you have any questions to ask, please raise your hand.

Thank you for your participation.

D Written instructions: additional no-communication treatments – Human and Automated players

*English translation of the original instructions in French. Variations according to Human/Robot Treatments appear in italics preceded by **Robots.** / **Humans.***

You are about to take part in an experiment in which you can earn money. *The amount of your gains will depend on your decisions, as well as on decisions made by other participants.*

Before starting, we would like to ask you to answer a few standard questions (concerning your age, education, profession, ...) which will help us to get to know you better. **This information, as well as the amount of your gains from this experiment, will remain strictly confidential and anonymous.**

Please fill in the questionnaire using the interface on your computer screen, which is divided into three parts:

- In the top section, you will find information that might help you in making decisions.
- In the middle section, you will submit your decisions by clicking on a relevant button.
- In the bottom section, you will see all your decisions and gains from previous rounds of the experiment.

Thank you.

The experiment

Humans. *The experiment consists of several identical rounds. In each round, participants are divided into groups of two. Each pair consists of one player A and one player B. You will be randomly assigned to your role — player A or player B — at the beginning of the experiment, and retain it throughout the experimental session. A message on your computer screen will inform you about your role.* ***Your role will remain unchanged throughout the entire experiment.***

Robots. *The experiment consists of several identical rounds. In each round, participants are divided into groups of two. Each pair consists of one player A and one player B. Player B is an automated player whose behavior is described below. Throughout the experiment,* ***you will thus play as player A, and the computer will play as player B.***

What happens in each round

Humans. *At the beginning of each round, participants are divided into pairs: if you are player A, then a player B is randomly selected to complete your pair; analogously, if you are player B, then a player A is randomly selected to complete your pair. Your pair will* ***change after each round****, and two participants in opposite roles* ***will interact at most once during the experiment.***

Each round consists of 4 stages.

Stage 1. At the beginning, a participant is randomly matched to your group.

Stage 2. Player A chooses between L and R by clicking on a relevant button on his/her computer screen.

Stage 3. Player B chooses between l and r by clicking on a relevant button on his/her computer screen.

Stage 4. End of the round and each player is informed about his/her earnings:

- If **player A chose** L, then **regardless of player B's decision**:
  - ▶ Player A earns 9.75 €in this round;
  - ▶ Player B earns 3 €in this round;
- If **player A chose** R then:
  - – if **player B chose** l:
    - ▶ Player A earns 3 €in this round;
    - ▶ Player B earns 4.75 €in this round;
  - – if **player B chose** r:
    - ▶ Player A earns 10 €in this round;
    - ▶ Player B earns 5 €in this round;

At the end of each round, a message on your computer screen will inform you either that a new round is about to start, or that the experiment is over.

Robots. ***In this experiment the computer chooses*** r ***at each round, without exception.***

Payment of your earnings

At the end of the experiment, **one round is picked at random**. Each participant receives a sum in EUR corresponding to the amount he/she earned in this round, plus a bonus of 5 €for completing the experiment. Payments are made individually and in cash.

For obvious reasons, **you are not allowed to talk during the experiment**. Participants who violate this rule will be excluded from the experiment and all payments. It is crucial that you fully understand the rules of this experiment. Should you have any questions, please raise your hand, a staff member will answer you in private.

Thank you for your participation.