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Cheap Talk, Information, and Coordination - Experimental Evidence*

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Abstract: Costless and non-binding pre-play communication *(cheap talk)* has been found to often be effective in achieving efficient outcomes in experimental games. However, in previous two-player experimental games each player was informed about both his payoff and the action of the other player in the pair. In the field, people may engage in cheap talk and subsequently learn their payoffs, but frequently only learn their own payoffs and not the actions of other people. We model this uncertainty in the framework of a 2x2 coordination game, in which one choice leads to the same payoff regardless of the action of the other player. We vary whether messages about intended play are permitted, and whether participants are informed about the other person's play. Cheap talk is found to be effective, as there is much more coordination in both Signal treatments than in either of the No Signal treatments. We also find that information about the other person's play appears to increase coordination when messages are permitted. However, in the No Signal treatments, the round-to-round changes in choices induced by this additional information are unable to overcome the apparent pessimism about the feasibility of coordination without a signal.

Keywords: Cheap Talk, Coordination, Payoff Information, Risk Dominance

JEL Classification: C72, C91, C92
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1. INTRODUCTION

In many social and economic situations, actors must coordinate choices to achieve mutually beneficial outcomes. Often there are multiple equilibria and the selection process reflects the tension between payoff dominance and risk dominance (Harsanyi & Selten 1988). In such cases costless and non-binding pre-play communication, or *cheap talk*, can be useful in solving the coordination problem.

In the theoretical literature, Crawford & Sobel (1982) consider a class of signaling games with continuous types and message spaces; they show that the more closely players' interests are aligned, the more likely one-way cheap talk is to be informative. More recently, Blume (1998) and Arvan, Cabral & Santos (1999) characterize conditions under which cheap talk might be useful. A number of experimental studies (e.g., Cooper, DeJong, Forsythe & Ross 1992, Clark, Kay & Sefton 1997, Charness 1998, Blume & Ortmann 1999 and Duffy & Feltovich 1999a) have found that advance messages about play are successful (to varying degrees) in achieving the Pareto-superior equilibrium in 2x2 coordination games.

However, one issue that has been largely unexplored is the amount of information that must be conveyed for cheap talk to be effective when risk dominance and payoff dominance conflict. In some environments signals are possible and the outcomes are observable, but one cannot identify the set of individual actions that has led to the observed outcome.

An example might be a project that can be undertaken by two parties to potential mutual benefits. Both parties can engage in the endeavor with either high or low effort, where the effort level is not observable. Realized financial returns depend on the effort invested by each party. From an individual party's point of view, a sure return can be obtained by engaging in low effort. The project will pay out additional money if and only if both parties engage fully in the project (generate high effort). However, if one party engages in high effort and the other one doesn't, the party choosing high effort incurs additional costs that are subtracted from the sure gain. Thus, a party that chooses high effort will always be aware of the other party's action, but a shirking party will not know what the other firm did without other explicit information.

We model this uncertainty in the framework of an experimental game adapted from Cooper *et al.*(1992):

Figure 1 – The Game

	A	*B*
A	80,80	80,10
B	10,80	100,100

While the *(B,B)* outcome is preferred by both players, it is risky for either person to play *B*, since this is a best response if and only if the probability that the other player chooses *B* is at least 7/9. At first glance, one might expect that a *B* signal from one player to the other would achieve coordination, but there is a potential problem: The message sender weakly prefers that the receiver chooses *B* in all cases. As Aumann (1990) points out, one might therefore discount a *B* signal.[1] Cooper *et al.* (1992) find that *A* play is the rule without communication, while signals substantially increase the likelihood of *B* play.[2]

If one plays *B*, the outcome reveals the other player's action. However, notice that a person who plays *A* always receives 80, and so will not be able to deduce the other player's action from the observed outcome. In the Cooper *et al.* (1992) experimental setup, each player is nevertheless informed about the other player's action. We concentrate on the *ex post* ambiguity about the other player's action when one plays *A*. We vary whether messages about intended play are permitted and whether a participant is specifically told which play the other person has chosen. In our experimental setup, players are randomly rematched.

As expected, pre-play communication facilitates coordination. We also find that the payoff-dominant equilibrium occurs more frequently when people know that explicit information about actions will be revealed. Yet this information (or the knowledge that this information will be provided) does not help without a signal. It seems that *B* play is perceived to be too risky, regardless of whether one is informed about the counterpart's play.

The remainder of this paper is organized as follows: Section 2 presents the background to the issues in question, as well as relevant previous evidence. In Section 3, we describe our experimental design. Results are given in Section 4, and Section 5 concludes.

[1] In the game discussed by Aumann, the sender strictly prefers that the receiver play *B* in all cases. However, the spirit of the argument seems unchanged here.
[2] In their experimental game the lower off-diagonal payoff is 0, rather than 10.

2. BACKGROUND

Farrell & Rabin (1996) express the view that information transmission can facilitate efficiency, and that even costless and non-binding (and so "cheap") talk can convey information. While the possibility of a babbling equilibrium (where messages are ignored because they are completely uninformative) always exists, people typically use the usual meaning as a starting point and then assess credibility (p. 108). The outcome induced by a message really depends on beliefs, and the structure of a game is likely to influence these beliefs. Blume (1998) characterizes theoretical conditions under which one-sided communication can lead to the payoff-dominant equilibrium. His assumption that messages have some *a priori* information content formalizes the idea that if all players communicate and their messages express unanimity about an equilibrium, then this equilibrium becomes more attractive for the communicating players. Hurkens (1996) finds that the signaler (one-sided communication) achieves his preferred outcome if a message has an arbitrarily small cost (apparently inducing a higher credibility of the signal).

Previous experimental work confirms that the extent to which communication can enhance coordination varies across different forms of games. Charness (1998) uses one-way signals (only one party can send a signal) that are potentially self-serving (the sender strictly prefers that the receiver plays *B*), and the payoff-dominant outcome prevails in a Stag Hunt game. While Isaac & Walker (1988) find communication is very effective in a public goods game, players in the Prisoners' Dilemma game almost completely ignore the "cooperate" signal in Charness (1998). Weber, Rottenstreich, Camerer & Knez (1999) use cheap talk in a minimum effort (multi-person Stag Hunt) game to investigate the effects of "leadership" on the likelihood of Pareto-optimal choices, which they find to be effective in small groups but not in large groups. Blume & Ortmann (1999) find that messages facilitate both quick convergence to, and participants' initial coordination on, the Pareto-dominant equilibrium in an order-statistic game adapted from Van Huyck, Battalio & Beil (1990).

All past studies of coordination in two-person games give participants feedback about the action chosen by one's counterpart (this is generally an inevitable by-product of the payoff calibration). In a game nearly identical to ours, Cooper *et al.* (1992) find a very high rate of coordination on *(B,B)* with signals simultaneously sent by both players, an intermediate rate when a signal sent by only one of the players, and *(A,A)* outcomes when no signal is allowed. On the other hand, Clark *et al.* (1997) find that the degree of effectiveness of simultaneous two-way messages in the Stag Hunt game depends on whether a signal for efficient play is potentially self-serving.

Duffy & Feltovich (1999a) consider the relative effectiveness of cheap talk and information about the previous action of one's current round counterpart in three games: Chicken, Stag Hunt, and Prisoners' Dilemma. They find that both devices are at least partially effective, but that the relative success depends on the game played. In Duffy & Feltovich (1999b), the two enhancements are combined. The sender's action in the previous period is revealed to the current-round receiver, allowing this receiver to evaluate the pre-play message in light of the sender's past behavior. In a second treatment, receivers get the same information about the senders, but they are also informed about the message that the sender had sent in the previous period. Their results suggest that messages then become more truthful, facilitating cooperation/coordination.

Our study investigates the effect of combining cheap talk with information about actions. We wished to avoid potential supergame issues inherent in attaching a reputation to each player, so only provide information about someone else's action after a player has already interacted with that person. Theoretically, this provision of information should not matter, since players are randomly rematched from a pool of players that is larger than the number of rounds. However, we suspect that the mere knowledge about whether current round's actions will be observable by current round's partners might affect behavior. Moreover, we feel it may be more realistic to assume that people will know whether their action will be observable by their current round partner than how prospective partners behaved in the past.

3. EXPERIMENTAL DESIGN

Our experimental design follows Charness (1998). The 144 participants were undergraduates majoring in either economics or humanities at the Universitat Pompeu Fabra in Barcelona. Average earnings were about 1400 pesetas (then about $9), including a 500 peseta show-up fee. Sessions lasted about 80 minutes and were conducted in June, 1999 and January, 2000.

In each session, there were 12 people seated at individual carrels. Instructions (presented in Appendix A; sample signal and decision sheets are shown in Appendix B) were given to each person and were also read aloud to the group.[3] There were 10 periods of play; participants were told that there would be

[3] The experiment was conducted in Spanish. Instructions shown in Appendix A and B are translations only.

random re-matching in each period, and that the role of the signaler was also randomly drawn for the period. We did use random matching, but only within subgroups of 6 players. This separation by sextuples enables us to have two completely independent observations for each session of 12 participants.[4]

In our four treatments, we used the game of Figure 1 (given in the introduction), with 1 unit = 1 peseta. We varied only whether sending a message (signal) was permitted ("Signal" versus "No Signal"), and whether we wrote the other player's action down on the decision forms before they were returned to the players ("Info" versus "No Info"). The 2x2 design is shown in Figure 2:

Figure 2 – Treatment Conditions

		Information about partner's choice	
		Yes	No
Cheap Talk (Signal)	Yes	Signal - Info (SI)	Signal - No Info (SNI)
	No	No Signal - Info (NSI)	No Signal - No Info (NSNI)

Prior to each period in the Signal treatments, every participant was given a piece of paper. Senders received a paper which read "I intend to play [*A* or *B*]." For receivers, the initial paper received was blank. Identical black pens were provided to each person to indicate a signal. In this way, no one knew which people were signalers in a period. After signal sheets were marked, these were collected and sorted; signals were distributed to the assigned receivers and senders received blank pieces of paper. At this point, players marked play choices on decision forms and these were collected. Payoffs were determined and marked (along with actions, if appropriate) on the forms, which were then returned to the participants. Payoffs were aggregated over the 10 periods in the session, and people were paid individually and privately.

4. RESULTS

Table 1 presents summary statistics for our Signal treatments (data by individual sextuple is presented in Appendix C). "*S(A)*" and "*S(B)*" refer to signals *A* and *B*; "*AA*", "*BB*", "*AB*", and "*BA*" refer to the plays of the signaler and receiver, respectively.

[4] While it is true that people were sometimes paired more than once, no player was ever aware of the identity of the other player in the pair. Since there were 12 people in each session, participants had little reason to be

concerned with repeated-game effects, as they did not know that pairings were only formed within sextuples.

Table 1 – Signal Results

(Outcomes, aggregated over all rounds)

Signal, Info treatment					
	Outcome				
Signal	*AA*	*BB*	*AB*	*BA*	*Total*
S(A)	23	0	1	0	*24*
S(B)	0	142	3	11	*156*
Total	*23*	*142*	*4*	*11*	*180*

Signal, No Info treatment					
	Outcome				
Signal	*AA*	*BB*	*AB*	*BA*	*Total*
S(A)	40	0	3	1	*44*
S(B)	5	88	6	37	*136*
Total	*45*	*88*	*9*	*38*	*180*

It is immediately apparent that there is a substantially higher percentage of *BB* outcomes in the Signal - Info (SI) treatment than in the Signal - No Info (SNI) treatment: 79% (142 out of 180) to 49% (88 out of 180). Providing the information about the other person's play (and knowing that this information will be provided) seems to expedite coordination on *BB*.

We can analyze the data from the individual sextuples for a conservative statistical test of the difference between treatments, using either *BB* outcomes or *B* play. There are 6 sextuples in each treatment and the data is summarized in Table 2 below:

Table 2 – Signal Treatments by Individual Sextuple

	***BB* Outcomes** (30 possible per sextuple)			***B* Play** (60 possible per sextuple)	
Sextuple	Info	No Info		Info	No Info
1	12	15		29	41
2	23	20		48	49
3	30	15		60	33
4	21	7		50	22
5	27	18		54	44
6	29	13		58	34
Total	*142*	*88*		*299*	*223*

The robust rank-order test finds these two treatments to be statistically different at p = .01 (one-tailed tests) for both *BB* outcomes and *B* play.[5] This difference is comprised of two elements. First, *B* signals are treated as being less credible. The likelihood that the receiver of a *B* signal responds by playing *B* is 93% in the SI treatment (145 out of 156), but only 69% in the SNI treatment (103 out of 136, see Table 5). A second difference is that there are 11% fewer *B* signals in the SNI treatment (136 instead of 156).

The results in our No Signal treatments are quite different from our Signal results. Table 3 presents summary statistics for the No Signal (NS) treatments (data by individual sextuples are presented in Appendix C):

Table 3 – No Signal Results
(Outcomes, aggregated over all rounds)

No Signal, Info treatment			
AA	*BB*	*Off*	*Total*
82	22	76	*180*

No Signal, No Info treatment			
AA	*BB*	*Off*	*Total*
77	23	80	*180*

There is virtually no difference between the No Signal - Info (NSI) and No Signal - No Info (NSNI) cases, as confirmed by both visual inspection and the robust rank-order test. Table 4 shows outcomes and play by sextuples for the No Signal treatments:

Table 4 –No Signal Treatments by Individual Sextuple

	***BB* Outcomes (30 possible per sextuple)**		***B* Play (60 possible per sextuple)**	
Sextuple	Info	No Info	Info	No Info
1	2	3	14	15
2	2	3	9	22
3	1	5	8	23
4	8	6	28	30
5	7	4	34	21
6	2	2	27	15
Total	*22*	*23*	*120*	*126*

The information about the other player's action does not facilitate coordination in the absence of signals. However, there are significant differences between Signal and No Signal treatments. Comparing Table 2 with Table 4, a robust rank-order test on individual sextuples finds any comparison between a Signal and a No Signal treatment is significantly different at $p < .01$. Regardless of whether information about actions is provided, *B* play and *BB* outcomes are more frequent in any Signal treatment.

Returning to the issue of credibility of *B* signals, Table 5 shows that both receivers and senders are more likely to play *B* after (they either received or sent) a *B* signal when there will be information provided about the actions chosen. In this table, *P(sB|S(B))* indicates the relative frequency that a signaler plays *B* after sending a *B* signal and *P(rB|S(B))* indicates the relative frequency that the receiver plays *B* after receiving a *B* signal. The comparison across treatments strongly suggests that people seem to be more expectant of *B* play in the SI treatment.

Table 5 – Actions conditioned on signals

	Info	No Info	Test of equality of proportions (*Z*-values)[6]
P(sB|S(B))	154/156 (0.99)	126/136 (0.93)	2.61
P(rB|S(B))	145/156 (0.93)	103/136 (0.76)	4.10

[5] We also perform the more familiar Wilcoxon-Mann-Whitney test that relies on the assumption that the underlying distributions are the same. The results are quite similar. For the nonparametric tests used in this study see Siegal & Castellan (1988).

[6] The specific test statistic is $Z = (p_1 - p_2)/S_{p_c}$, where p_i is the proportion of B choices following a B signal in subsample *i*, and $S_{p_c} = \sqrt{p_c(1-p_c)(\frac{1}{N_1}+\frac{1}{N_2})}$ is an estimate of the standard error of $p_1 - p_2$. p_c is an estimate of the population proportion under the null hypothesis of equal proportions, $p_c = (p_1N_1 + p_2N_2)/(N_1 + N_2)$, where N_i is the total number of B signals in subsample *i*.

P(sA|S(A))	24/24 (1.00)	42/44 (0.95)	1.06
P(rA|S(A))	23/24 (0.96)	42/44 (0.95)	0.07
Round 8 only			
P(sB|S(B))	16/16 (1.00)	13/15 (0.87)	1.51
P(rB|S(B))	16/16 (1.00)	11/15 (0.73)	2.21

There is no significant difference with respect to *A* play and *A* signals. While the test statistics are quite high for the difference in contingent *B* play (first two rows of Table 5), these implicitly assume each observation is independent. Since this does not seem realistic and overstates the levels of significance, we also examine the play from an individual round, selected at random.[7] Even this more conservative test finds the differences in contingent *B* play to be significant at $p < .07$ (one-tailed test).[8]

We also perform clustered probit regressions on the Signal treatments.[9] Given that there was a *B* signal, the dichotomous variable *choice* is regressed on the information condition (Info or No info), the player's type (sender or receiver) and an interaction term between the information condition and the type of player (see Appendix E for details on the regression).[10] We find that the probability to play *B* after a *B* signal is about 13% higher in the information condition compared to the No Info treatment, and about 6% higher for senders than for receivers of messages. Both values are significantly different from zero at a significance level of 5%.

Overall, we observe that *B* signalers play *B* more often than do *B* receivers. *B* signalers appear to expect receivers to play *B* more than receivers expect *B* signalers to play *B*. This possibly reflects the tenuous nature of second-order beliefs. While the signaler need only expect the receiver to believe him, the receiver must be confident that the signaler expects the receiver to expect the signaler to play *B* after a *B* signal. Results from the *guessing game* (e.g., Nagel 1995) suggest that many people do not reason at this level of

[7] We rolled an 8-sided die to choose a period, excluding the first and last periods from the selection pool.
[8] We omit the entries for *A* signals, since only 2 (3) *A* signals were sent in the 8th round in the info (no info) treatment and all *A* signals were followed by *A* play for both sender and receiver. Test statistics for the other nine rounds are qualitatively similar to the reported ones of the 8th round.
[9] We conducted clustered regressions in order to relax the assumption of independence within a sextuple. *Choice* was encoded as (0=*A*, 1=*B*). Running clustered regressions implicitly uses robust (i.e. "correct") standard errors, even if the observations are correlated.

depth. A similar phenomenon is observed in Charness & Garoupa (2000), where sellers of inside information about an asset value generally tell the truth, but some potential information buyers doubt that this will happen, and so do not purchase this information.

Table 6 examines whether receivers' "trust" or "mistrust" in *B* signals is *ex post* justified in the SI and SNI treatments. We will call a *B* receiver who plays *B* "trusting", whereas a *B* receiver who does not play *B* will be called "mistrusting". Receivers are substantially more likely to trust a *B* signal in the SI treatment than in the NSI treatment - 145 times out of 156 *B* signals (93%), compared to 103 times out of 136 *B* signals (76%). This trust is generally justified, although this is less likely in the SNI case (99% vs. 85%). The difference in the ability of the receiver to correctly interpret the signal is somewhat striking: Receivers successfully coordinate with signalers in 92% of the time in the SI treatment (143 out of 156), but only 69% of the time in the NSI treatment (104 out of 136).

Table 6 – Receivers' trust and mistrust of *B* signals

	Info	No Info
Unjustified Mistrust *(pA, oB)*	11	27
Justified Mistrust *(pA, oA)*	0	6
Unjustified Trust *(pB, oA)*	2	15
Justified Trust *(pB, oB)*	143	88

(pX, oY) means a *B* receiver played *(p) X* and observed[11] *(o) Y*

We do not perform a learning analysis, as we feel that 10 periods are not really adequate for this purpose, particularly since people change their roles. Nevertheless, we can examine the trends over time. Figures 2 and 3 show the proportion of *BB* outcomes and *B* play, respectively:

Figure 2: Relative frequency of *BB* outcomes over time

[10] As can be seen from Table 5, almost all *A* signals were followed by an *A* play by both sender and receiver. We therefore restrict the regressions to choices that follow a *B* signal.

[11] In the No Info treatment "observed" means that this play was made but left unidentified by a receiver who played *A*.

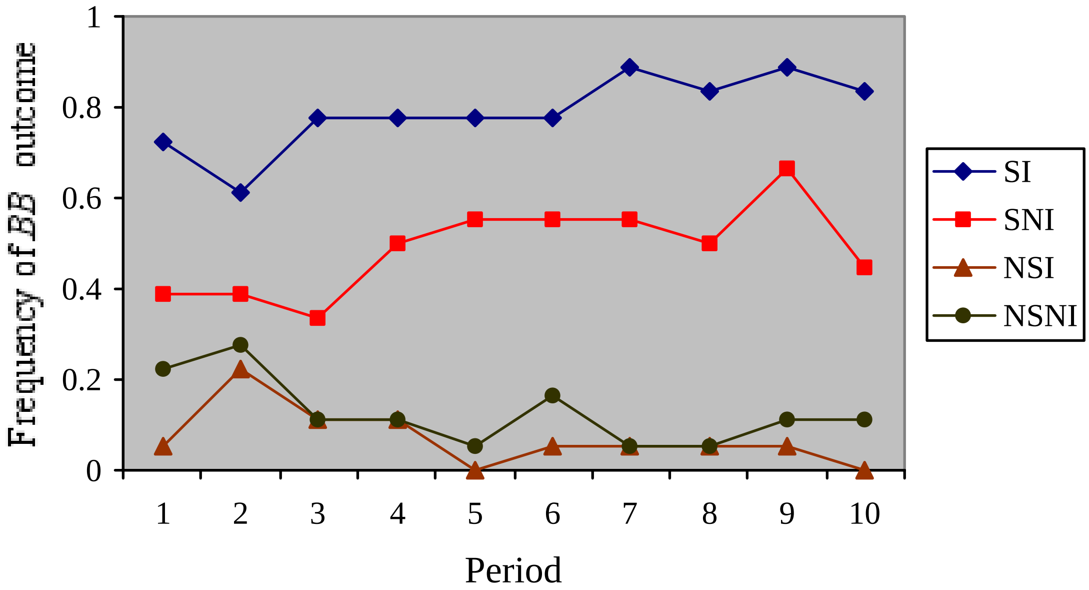
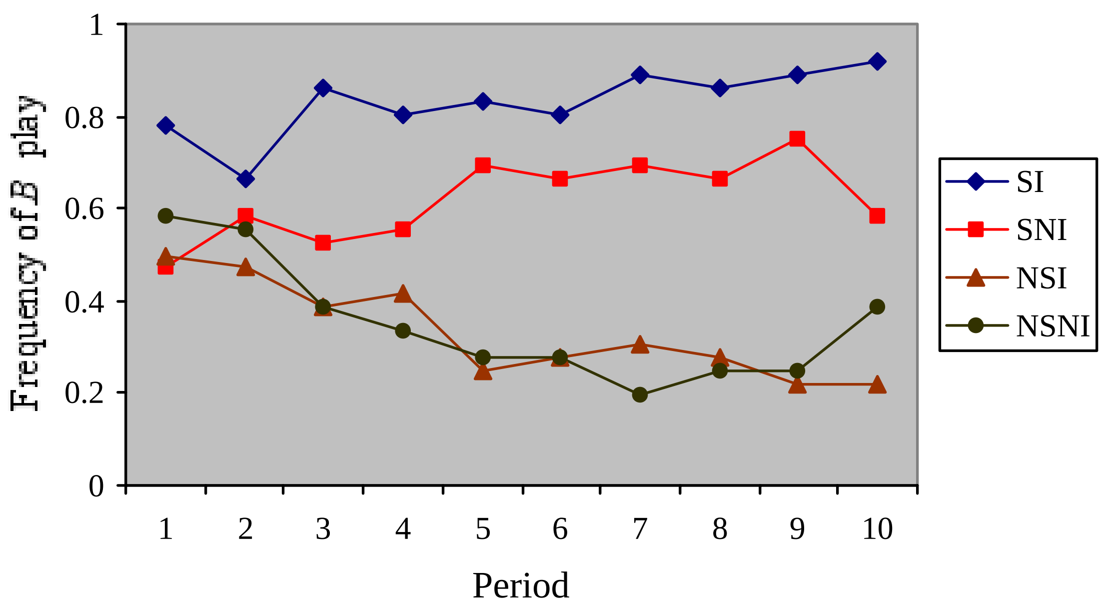


Figure 3: Relative frequency of *B* play over time



The Figures indicate that both outcomes and play diverge somewhat over time across the Signal and No Signal treatments. The trend with signals is toward increasing *B* play and *BB* outcomes, whereas the trend without signals is toward decreasing *B* play and *BB* outcomes (with some last-period effects). While we do not observe equilibrium behavior in either case, the Figures suggest that we are moving toward different equilibria.

In our clustered probit regressions (Appendix E), we regress the chosen play on the information condition, the time trend, and an information-time interaction term. For the Signal treatments we do not find any coefficient significantly different from zero, although they all have the anticipated positive sign. As for the No Signal data, we find that the probability of playing *B* decreases by 3% per round. This is significantly smaller than zero at a significance level of 1%. The other coefficients are negative, but not significantly so.

Aggregate behavior in the No Signal sessions does not differ much according to the different information scenarios. On an individual level we do find that the round-to-round behavior is influenced by foregone payoffs. However, the effect appears to be overwhelmed by the difficulty of the coordination problem without a signal. The No Signal treatments offer a fairly clean test of how transition (switching from *A* to *B* or *vice versa*) is affected by information about actions. Table 7 shows the likelihood of switching from *A* to *B* in the No Signal treatments in period *t+1*, conditional on information feedback in round *t*:

Table 7 – Propensity to change from *A* to *B* (No Signal)

Outcome in time *t*	Info	No Info
	% *B* choice in *t+1*	% *B* choice in *t+1*
played *A* + observed* *A*	19/144 (13.2%)	35/142 (24.6%)
played *A* + observed* *B*	19/68 (27.9%)	15/70 (21.4%)

*In the No Info treatment, "observed" means that this play was made, but was unidentified.

We compare the cases where *A* was played in time *t*, as information about the other player's action is superfluous when *B* is played.[12] Overall, we see a modest divergence. The No Info column illustrates the general propensity to change from *A* play to *B* play, seen to be about 24% (50 out of 212). In the Info treatment, only 13% (19 out of 144) change from *A* play after learning the other player actually chose *A*, whereas 28% (19 out of 68) change after learning a *B* play was chosen in time *t*. These sequential changes seem intuitive and turn out to be the ones we had expected. However, as can be seen in the general trend,

[12] As one would hope and expect, there was no significant difference across treatments when *B* is played at time *t*.

they are not strong enough to overcome the apparent pessimism about being able to coordinate without a signal.

Individual behavior of receivers is given in Appendix D. In general, individual behavior confirms the patterns observed in the aggregate. In the SI treatment, 89% of all receivers (32/36) responded with *B* to all *B* signals and one other person responded with *B* to a *B* signal 4 of 5 times. In the SNI treatment, only 56% of all receivers (20/36) always responded with *B* to *B* signals, and 36% (13/36) responded with *B* to a *B* signal less than 70% of the time. Players' individual experiences also confirm the difference across signal treatments. In the SNI treatment, there was only 1 case (out of 36) where a player witnessed a *B* signal and *(B,B)* play in every period; there were 12 such cases in the SI treatment. Similarly, there were only 16 cases where a player experienced a *(B,B)* outcome more than half the time in the SNI treatment, compared to 32 cases in the SI treatment.

5. DISCUSSION

Coordinating on a risky, but Pareto-superior, outcome is not an easy task. Past experimental studies have found that cheap talk can be effective in coordination games, but have not explored whether it is necessary to specifically provide information about the actions that lead to one's revealed payoff. We use a game that allows us to isolate this factor, and find that knowledge that one will be informed about the other player's action substantially enhances coordination when there is also a signal about intended play.

From a purely game-theoretic perspective, there is no obvious reason why the additional information about actions should affect behavior in this repeated one-shot setting. Nevertheless, there are different channels through which information could matter here. When one is playing *A*, the realization that others are actually playing *B* after a *B* signal is sent might tempt one to play *B*. In addition, since choices are highly sensitive to beliefs about the likelihood that *B* play will follow a *B* signal, one's initial propensity for B play may be affected if one anticipates a difference in the behavior of the group.

We see that in the SI treatment, 98% of *B* signalers and 93% of *B* receivers play *B*. In the SNI treatment, 92% of *B* signalers still play *B*, but *B* receivers are less certain about matters, and only 69% play *B*. It is somewhat surprising that the difference in the Signal treatments is already present in the first period, rather than developing over time. Although we do see some evidence of round-to-round changes in choices in immediate response to obtained and foregone payoffs (in the No Signal treatments, where this is not confounded), the difference in behavior seems driven more by the participants' anticipation of the credibility of a *B* signal. Nevertheless, the time trend is positive in both Signal cases, so that it may be that cheap talk would ultimately be successful in inducing *(B,B)* outcomes even without this information provision.

Information about actions does not appear to help when there is no signal, however. Even though *A* play is more common (and so information provision should be more directly beneficial), the apparent pessimism about the likelihood of successful coordination swamps the sequential round-to-round change in behavior in response to foregone payoffs, so that attempts at coordination on *(B,B)* diminish over time and *A* play becomes the rule in both No Signal treatments. The coordination problem appears to be just too difficult without a signal.

Our study contributes to the growing literature on the effectiveness of cheap talk in a variety of games and environments. Clearly, beliefs (and therefore behavior) are sensitive to the structure of the game. In the terminology of Aumann (1990) and Farrell & Rabin (1996), cheap talk is more likely to be helpful when a message is both self-committing and self-signaling.[13] Duffy & Feltovich (1999b) explore the interaction of signals and information about actions and find that when the strategic structure of a game implies that cheap talk messages ought to be credible, cheap talk is found to be the relatively better device. However, when the strategic structure of a game implies that cheap talk messages ought not to be credible, "actions speak louder than words."

Extensions of our study include combining signals about intended play with degrees of information about individual action in order-statistic games (minimum games, median games) and in the voluntary contribution mechanism. This would build upon work such as the dilemma games of Dawes, McTavish, & Shaklee (1977) and Isaac & Walker (1988), as well as the minimum-effort games of Van Huyck, Battalio, & Beil (1990), and Weber, Rottenstreich, Camerer & Knez (1999).

As for a contribution toward the learning literature, it might well be worthwhile to conduct another (possibly longer) sequence of experiments, in which subjects do not change their roles, i.e. are either senders

[13] Self-commitment is satisfied when the sender's message, if believed, binds the sender to playing the signaled action (the action is a best response to the receiver's best response). A message is self-signaling

or receivers during the entire duration of the experiment. A truncated reinforcement learning model as used by Duffy & Feltovich (1999a) might more clearly disentangle effects that can be attributed to differences in initial propensities versus round-to-round adaptation effects.

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when the sender prefers the receiver to play the best response to a given message if and only if the sender truly intends to play the signaled action.

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APPENDIX A - SAMPLE INSTRUCTIONS

(Signal Treatments)

Thank you for participating in this experiment. For your participation today, you will earn some positive amount of money. This amount of money will be determined by the decisions that you make and by the way that these decisions interact with the decisions of others. You will receive 500 Ptas. in any case, i.e. additional to the amount of money that you accumulate during the experiment.

There will be 10 rounds in this experiment. In each round, you will be paired with a individual randomly selected from the other people in the room. There is a new random drawing for each round. You will never be informed of the identity of the person with whom you are paired in any round, either during or after the experiment; similarly, no one will know that they are, or have been, paired with you at any point in time.

In each round, you will be asked to choose either action A or action B. The person with whom you are paired in a round will simultaneously choose action A or action B, without knowing the action you have selected. The payoffs you receive are shown in the game matrix below:

		Player Y	
		A	B
Player X	A	80,80	80,10
	B	10,80	100,100

All payoffs are in pesetas. The first payoff in each pair of payoffs is that of player X; the second payoff is that of player Y. Thus, if both players choose A, each receives 80 pesetas. If player X chooses A and player Y chooses B, then player X receives 80 and player Y receives 10. If player X chooses B and player Y chooses A, then player X receives 10 and player Y receives 80. If both players choose B, each receives 100 pesetas. We will go over these possibilities after everyone has read the instructions, to ensure that they are understood by all people present.

Before each round begins, each person will be handed a slip of paper. For half of the people in the room (and the identity of these people will vary from round to round), there will a statement on the sheet "I indicate my play to be:" and a place to check either box A or box B. If you have such slip, please select one of the two boxes. This choice is a statement to the other person in the pair about the intended action for the round to be played. Regardless of which box is checked, the person who checked the box is free to subsequently choose either action in that round. The other half of the people in the round will receive a blank slip. Please do not indicate any choices on the blank slips.

After a moment or two, someone will come around and collect all of the slips of paper. The slips with the statements will be distributed to people who were originally given blank slips in that round, according to the assignments that were randomly drawn earlier. The blank slips will be given to people who were originally given statement slips, according to the assignments that were randomly drawn earlier. This procedure insures that no other participant in the experiment will know whether or not you were originally given a sheet with or without a statement to make. After the exchange of sheets, indicate your decision for the round on your decision sheet. After everybody has made a decision, these sheets will be collected and payoffs will be calculated.

[Signal - No Info treatment]
At the end of each round, you will be informed of your payoff for that round.

[Signal - Info treatment]
At the end of each round, you will be informed of your payoff for that round and the choice of the player you were paired with in the current round.

At the end of ten rounds, payoffs for each person will be added up and each person will be paid individually and privately. Please feel free to ask questions before the beginning of this experiment. After the experiment begins, there is to be no communication between the participants in the experiment, except for the information on the slips of paper. Are there any questions?

(No Signal Treatments)

Thank you for participating in this experiment. For your participation today, you will earn some positive amount of money. This amount of money will be determined by the decisions that you make and by the way that these decisions interact with the decisions of others. You will receive 500 Ptas. in any case, i.e. additional to the amount of money that you accumulate during the experiment.

There will be 10 rounds in this experiment. In each round, you will be paired with a individual randomly drawn from the other people in the room. There is a new random drawing for each round. You will never be informed of the identity of the person with whom you are paired in any round, either during or after the experiment; similarly, no one will know that they are, or have been, paired with you at any point in time.

In each round, you will be asked to choose either action A or action B. The person with whom you are paired in a round will simultaneously choose action A or action B, without knowing the action you have selected. The payoffs you receive are shown in the game matrix below:

		Player Y	
		A	B
Player X	A	80,80	80,10
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All payoffs are in pesetas. The first payoff in each pair of payoffs is that of player X; the second payoff is that of player Y. Thus, if both players choose A, each receives 80 pesetas. If player X chooses A and player Y chooses B, then player X receives 80 and player Y receives 10. If player X chooses B and player Y chooses A, then player X receives 10 and player Y receives 80. If both players choose B, each receives 100 pesetas. We will go over these possibilities after everyone has read the instructions, to ensure that they are understood by all people present.

At the beginning of each round, each person will be handed decision sheet in which you are asked to indicate your choice. After everybody has made a decision these sheets will be collected and payoffs will be calculated.

[No Signal - No Info treatment]
At the end of each round, you will be informed of your payoff for that round.

[No Signal - Info treatment]
At the end of each round, you will be informed of your payoff for that round and the choice of the player you were paired with in the current round.

At the end of ten rounds, payoffs for each person will be added up and each person will be paid individually and privately. Please feel free to ask questions before the beginning of this experiment. After the experiment begins, there is to be no communication between the participants in the experiment, except for the information on the slips of paper. Are there any questions?

APPENDIX B - SAMPLE SIGNAL AND DECISION SHEETS

(SIGNAL SHEET)

PLAYER 8 PERIOD 7

I INTEND TO PLAY

A B

DECISION SHEET - No Info

PLAYER 8 PERIOD 7

MY DECISION MY PAYOFF

A B

DECISION SHEET - Info

PLAYER 8 PERIOD 7

MY DECISION OTHER PERSON'S CHOICE MY PAYOFF

A B

APPENDIX C - RESULTS FOR 6-TUPLES

Table C1: Play in Signal treatments (Rows correspond to the play of signalers)

SNI - Signal No Info

														Totals	
		A	**B**	**A**	**B**	**A**	**B**	**A**	**B**	**A**	**B**	**A**	**B**	**A**	**B**
S	**A**	4	1	1	0	12	1	15	0	4	3	9	4	45	9
	B	10	15	9	20	2	15	8	7	5	18	4	13	38	88

SI - Signal Info

														Totals	
		A	**B**	**A**	**B**	**A**	**B**	**A**	**B**	**A**	**B**	**A**	**B**	**A**	**B**
S	**A**	13	0	5	1	0	0	1	3	3	0	1	0	23	4
	B	5	12	1	23	0	30	5	21	0	27	0	29	11	142

Table C2: No Signal treatments

								Totals
NSNI	**AA**	18	11	12	6	13	17	77
	BB	3	3	5	6	4	2	23
	Off	9	16	13	18	13	11	80
NSI	**AA**	18	23	23	10	3	5	82
	BB	2	2	1	8	7	2	22
	Off	10	5	6	12	20	23	76

Table C3: Transitions in play in No Signal treatments

	No Info												Info											
	choice in *t+1*												choice in *t+1*											
choice in *t*	A						B						A						B					
Sextuple	1	2	3	4	5	6	1	2	3	4	5	6	1	2	3	4	5	6	1	2	3	4	5	6
pA + oA	28	13	13	6	19	28	4	9	9	6	5	2	27	36	39	14	4	5	5	4	1	4	0	5
pA + oB	8	11	7	11	10	8	1	2	5	4	1	2	6	4	6	8	11	14	3	1	0	2	7	6
pB + oA	6	8	7	6	6	5	3	5	5	9	5	5	7	5	3	4	8	9	2	0	3	6	10	11
pB + oB	1	4	6	5	2	1	3	2	2	7	6	3	2	2	1	4	2	2	2	2	1	12	12	2

Note: pX and *oY* refer to a subject that played *X* and observed *Y* (in the No Info treatement "observed" actually means undetected play of the respective other). Entries for each sextuple in columns A_i and B_i sum to 54 (6 players *x* 9 transitions)

APPENDIX D - INDIVIDUAL RESPONSES TO *B* SIGNALS

Signal - Info

Most receivers (32/36) responded with *B* to all *B* signals. Only 3 of 36 failed to respond to a *B* signal with a play of *B* at least 80% of the time.

Table D1: SI - # of receivers who responded with *B* to a *B* signal *X*% of time

Rate	Sextuple					
	1	2	3	4	5	6
100%	4	5	6	5	6	6
80%		1				
75%						
66.6%	1					
50%						
10-40%						
0%	1			1		

Cells show # of players having the corresponding rate in each sextuple.

Signal - No Info

Just over half of all receivers (20/36) responded with *B* to all *B* signals. 13/36 failed to respond to a *B* signal with a play of *B* at least 75% of the time.

Table D2: SNI - # of receivers who responded with *B* to a *B* signal *X*% of time

	Sextuple					
	1	2	3	4	5	6
100%	2	4	2	2	5	4
80%	2	1				
75%			1			
66.6%			2	1		1
50%	1		1	1		
10-40%	1	1			1	
0%				2		1

Cells show # of players having the corresponding rate in each sextuple.

APPENDIX E - ROBUST REGRESSION RESULTS

Choices conditional on a B signal

The regression equation is

$$choice = b\_0 + b\_1\ info + b\_2\ \ sender\ \ + b\_3\ info \cdot sender,$$

where *choice* = 1 if *B* was selected, 0 otherwise, *info* = 1 if the treatment was the info treatment, 0 otherwise and *sender* = 1 if the subject was a sender of messages, otherwise 0. Table F1 shows the probit estimates, where *dF/dx* can be interpreted as the probability of a discrete change of the dummy variable from 0 to 1.

Table E1: Robust Regression Results 1

Variable	*dF/dx*	*p*-value
(b_1) info	.13	.021
(b_2) sender	.06	.007
(b_3) info·sender	.04	.469

Note: p-values are derived from a test of the underlying coefficients being 0, two-tailed test.

The original probit estimates are given in Table F2.

Table E2: Robust Regression Results 2

Variable	Coefficient	*p*-value
(b_1) info	.77	.021
(b_2) sender	.35	.007
(b_3) info· sender	.25	.469
(b_0) constant	.70	.000

Note: p-values are derived from a test of the underlying coefficients being 0, two-tailed test.

Choices over time

The regression equation is

$$choice = b\_0 + b\_1\ info + b\_2\ \ time\ \ + b\_3\ \ info \cdot time,$$

where *choice* = 1 if *B* was selected, 0 otherwise, *info* = 1 if the treatment was the info treatment, 0 otherwise and *time* = *t* equivalent to the round index. Table F3 shows the probit estimates.

Table E3: Robust Regression Results 3

	Signal		No Signal	
Variable	*dF/dx*	*p*-value	*dF/dx*	*p*-value
(b_1) info	.18	.187	-.01	.877
(b_2) sender	.02	.451	-.03	.007
(b_3) info·sender	.01	.784	-.00	.906

Note: p-values are derived from a test of the underlying coefficients being 0, two-tailed test.

The original probit estimates are given in Table E4.

Table E4: Robust Regression Results 4

	Signal		No Signal	
Variable	Coefficient	*p*-value	Coefficient	*p*-value
(b_1) info	.05	.187	-.02	.877
(b_2) sender	.05	.451	-.08	.007
(b_3) info·sender	.02	.784	-.01	.906
(b_4) constant	.02	.950	.05	.574

Note: p-values are derived from a test of the underlying coefficients being 0, two-tailed test.