Do Actions Speak Louder Than Words? An Experimental Comparison of Observation and Cheap Talk*

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Abstract

How do individuals achieve "good outcomes" in one–shot strategic situations? One possibility is that they engage in some kind of preplay communication—cheap talk—in which they endeavor to convince one another of the actions they intend to play. Another, less explored, possibility is that individuals take account of their knowledge of the past behavior of others when deciding which actions to play. While these two possibilities have been considered separately, there has been no research examining the relative efficacy of cheap talk and observation for the achievement of good outcomes. This paper reports the results from an experiment with human subjects that allows for such a comparison. The effects of cheap talk and observation of past actions are compared with each other, and with a control, in which neither cheap talk nor observation is allowed. We consider three different 2×2 games and explain why cheap talk or observation is likely to be the more effective device for achieving good outcomes in each game. The experimental evidence suggests that both cheap talk and observation make cooperation and successful coordination more likely and increase payoffs relative to the control. The relative success of cheap talk versus observation in achieving such good outcomes depends on the game played, in accordance with our predictions. We also find that the signals players send are informative in the sense that they are correlated with their eventual actions, and that receivers of signals take this fact into account by conditioning their actions on the signal they receive. The results of this experiment can be used to extend game–theoretic models of how individuals make use of the different types of information available in strategic environments.

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1 Introduction

How do individuals achieve good outcomes in one–shot strategic situations where decisions must be made simultaneously? One much–explored possibility is that they use costless, nonbinding "cheap talk"; that is, they communicate their intended actions in the hope of swaying the expectations and actions of others. A second, largely overlooked, possibility is that individuals turn to information regarding the past actions of others when such information is available. Under this scenario, individuals recognize that their actions can be used by others to judge their likely behavior in future meetings. They may therefore choose to signal through their current choice of actions their intended future behavior.

There is good reason to believe that in our current "postmodern era" observed past actions "speak louder" than words—that cheap talk may be the relatively weaker device for achieving good outcomes. The linguist John Haiman has observed (Haiman (1998)) that the old "talk is cheap" maxim is more relevant today than ever. Haiman notes that "unplain speaking," which once carried a social stigma, is now commonplace. Individuals frequently say things that are very different from what they mean, and they recognize this same tendency in others. Haiman notes that the increasingly common use of such phrases as "yeah, right," "whatever," and "I couldn't care less" reflect a pervasive and articulated cynicism with all human communications that was simply not present just a couple of decades ago.

Whether individuals find observation of past actions or non–binding cheap talk more effective for achieving good outcomes is, essentially, an empirical issue, and is the question we seek to address in this paper. In particular, we ask: *do actions speak louder than words?* We attempt to answer this question by designing and conducting a laboratory experiment in which human subjects play three simple games under one of three information treatments: no information about opponents, cheap talk, or observation of opponents' previous–round actions.

Our experimental evidence suggests that both cheap talk and observation, while far from being perfect devices, do result in better outcomes—more cooperation, more coordination on pure–strategy Nash equilibria, and higher payoff efficiency—relative to the control case, in which players have no information about their opponent. Consistent with the work of Farrell and Rabin (1996), we find that the relative effectiveness of cheap talk or observation varies systematically with the game. In particular, when the strategic structure of a game implies that cheap talk messages ought to be credible, cheap talk is found to be more effective—words speak louder than actions. On the other hand, when the strategic structure of a game implies that cheap talk ought *not* to be credible, observation is more effective than cheap talk—actions speak louder than words. We also find, at the individual level, that subjects take additional information—cheap talk messages or observed previous–round actions—into account when choosing actions in each of the games.

2 The Games and Information Treatments

Figure 1 shows the games used in our experiment: Prisoner's Dilemma (PD), Stag Hunt (SH), and Chicken (CH). These games are alike in that they are well–known, symmetric, 2×2 games.[1] However, they differ in their strategic structure (such as their best–reply correspondence), in the number and nature of Nash equilibria, and as we will see in Section 2.1, in the predicted effect of cheap talk on play relative to that of observation of prior actions. In each game, players choose between two strategies: Cooperate (C) and Defect (D). We call the strategies "Cooperate" and

Figure 1: The Games

		Player 2 C	Player 2 D
Player 1	C	70,70	10,80
	D	80,10	40,40

Prisoner's Dilemma

		Player 2 C	Player 2 D
Player 1	C	70,70	10,55
	D	55,10	55,55

Stag Hunt

		Player 2 C	Player 2 D
Player 1	C	70,70	50,80
	D	80,50	40,40

Chicken

"Defect" due to a characteristic shared by all three games: playing Cooperate always weakly increases, and nearly always strictly increases, the *other player's* payoff, ceteris paribus. A high degree of cooperation, a high probability of coordination on a pure–strategy Nash equilibrium, and

[1] Mehlmann (1997) notes that Prisoner's Dilemma, Stag Hunt and Chicken can be thought of as different parameterizations of a broad class of games, which he refers to as "Löwe–Lamm Spiele" (lion–lamb games). "Lion" corresponds to our Defect strategy, and "Lamb" corresponds to Cooperate.

high payoffs are all desirable features of outcomes in the games we consider. By "good" outcomes, we shall mean outcomes in which as many as possible of these features are present. In Table 1 we

Table 1: Characteristics of Nash Equilibria of the Games

Game	Equilibrium	Prob(Cooperation)	Prob(Coordination)	Expected Payoffs	Payoff Efficiency
PD	(0,0)	0	—	(40,40)	0
SH	(1,1)	1	1	(70,70)	1
	(0,0)	0	1	(55,55)	.6
	(.75,.75)	.75	.625	(55,55)	.6
CH	(1,0)	.5	1	(50,80)	.833
	(0,1)	.5	1	(80,50)	.833
	(.5,.5)	.5	.5	(60,60)	.667

Nash equilibria are presented in the form (Prob(row player cooperates),Prob(column player cooperates)).

show the extent to which these features are present in the Nash equilibria of the three games we consider. Prob(Cooperation) is defined as the overall frequency of C choices by individual players. Coordination refers to play of a pure– strategy Nash equilibrium in games with multiple equilibria—(C,C) or (D,D) in Stag Hunt, and (C,D) or (D,C) in Chicken. Payoff efficiency is defined as the sum of row and column player payoffs, normalized so that the maximum possible joint payoff in a given game has an efficiency of one and the minimum possible joint payoff has an efficiency of zero. These features of the equilibria will be compared with the experimental results in Section 4.

2.1 The Information Treatments: Theory and Hypotheses

In the experiment, players play each game ten times against changing opponents in one of three information treatments. In the *control* treatment, players were told only that they faced a different opponent in each round of a game; they received no additional information about their opponents. In the *cheap talk* (or *communication*) treatment, one member of each pair of players was randomly selected to send a costless, nonbinding message to the other player, prior to the choice of actions, indicating the action she intended to play in that round.[2] In the *observation* treatment, one member

[2]Note that in our design, cheap talk amounts to sending a signal of an intended action; no other communication is allowed. We chose to adopt this convention for cheap talk so that the information players received would be comparable to the information received in the observation cells, where they observed the action played in the previous round.

of each pair of players was randomly selected to be informed of her opponent's action in the previous round (when the opponent was matched with a different player), prior to the choice of current–round actions. We use the term "signal" to refer to a message in the cheap talk treatment or an observed action in the observation treatment; a "receiver" is a player who sees her opponent's signal, and a "sender" is a player whose signal is observed.[3] In our cheap talk and observation treatments, both players were equally likely to be senders or receivers.

Theoretically, the additional information we allow in the observation and cheap talk treatments need not affect play in the games we consider, as the subgame perfect equilibria of the 10–times–repeated game usually correspond closely to those of the stage game. For each game and information treatment, any sequence of stage–game Nash equilibria is a subgame perfect equilibrium of the corresponding finitely–repeated game. In the control treatment (where no extra information is available) and in Prisoner's Dilemma (which has a unique Nash equilibrium), these are the *only* subgame perfect equilibria of the finitely–repeated game. Allowing cheap talk in games with multiple equilibria (such as Stag Hunt or Chicken) enlarges the set of equilibria, but only in the trivial sense that players can condition the stage–game equilibrium they play on the information they send or receive. Allowing observation in these games enlarges the set of equilibria in this way, and also in a nontrivial way: subgame perfect equilibria of the finitely–repeated game may involve pairs of stage–game actions that are not Nash equilibria of the stage game.[4]

While the set of equilibria may be enlarged by the presence of observation or of cheap talk, standard game theory makes no prediction as to which equilibrium might be expected or even whether players take the additional information into account. Recently, however, theorists have considered the issue of when additional information (usually cheap talk) might aid coordination in

[3] Our cheap talk and observation treatments involved *one–way* signaling, in which (in each round) exactly one of the two players receives a signal sent by the other, as opposed to *two–way* signaling, in which both players receive signals from each other. Remaining silent in the cheap talk sessions, or remaining unobserved in the observation sessions, was not an option available to our subjects. Cooper et al. (1989) found that subjects rarely choose to remain silent in one–way cheap talk environments.

[4] For example, in Chicken, the action pair (C,C) could be sustained in every stage but the last by the (correct) belief that in the next stage, signal receivers will play the strategy: "if I observe C, I will choose C; if I observe D, I will choose D," in which case players will optimally play C in the current stage with an eye toward playing D in the next stage. These beliefs lead to equilibrium as long as players are sufficiently patient, as in our design, which induces a discount factor of unity among hypothetical expected–utility maximizing agents with time–separable preferences.

games with multiple equilibria.[5] Aumann (1990) and Farrell and Rabin (1996) propose conditions for cheap talk to facilitate coordination in situations where messages have literal meanings (that is, some convention exists for translating each message into a unique intended action) as in our design. We will use Farrell and Rabin's nomenclature here. One of their two conditions, *self–commitment*, is satisfied when the sender's message is part of a Nash equilibrium strategy profile. Such a message, if believed, binds the sender to playing the action she has signaled. For example, in Chicken, C is a self–committing message because the sender's best response to the receiver's best response (D) is the action C. By contrast, in Prisoner's Dilemma, C is not self–committing because the sender's best response to the receiver's best response (D) is not C, but D. Their other condition, *self–signaling*, is satisfied when the sender prefers the receiver to play the best response to a given message if and only if she (the sender) truly intends to play the signaled action.[6] For example, in Stag Hunt, C is a self–signaling message because the receiver's best response C gives the *sender* a higher payoff than she would have received had the receiver chosen D, and had the sender intended to play D instead, she would not have preferred the receiver to choose C. By contrast, in Chicken, C is not a self–signaling message because the receiver's best response, D, is *not* preferred by the sender; a C response by the receiver would have given the sender a higher payoff.

According to Farrell and Rabin (1996), "a message that is both self–signaling and self–committing seems highly credible." Indeed, one expects that when messages in a given game are self–signaling and self–committing, they should be truthful and believed. In Stag Hunt, both C and D messages are self–signaling and self–committing; in Chicken, both C and D messages are self–committing, but not self–signaling; and in Prisoner's Dilemma, C messages are neither self–signaling nor self–

[5]One commonly–used game is the Battle–of–the–Sexes (BoS) game, which is like our Chicken game, but with the (70,70) outcome replaced by (40,40). Like Chicken, BoS has multiple Nash equilibria, but none are payoff dominant. Unlike Chicken, in BoS both pure–strategy Nash equilibria payoff dominate the mixed–strategy Nash equilibrium. Farrell (1987) studies repeated two–way cheap talk in the context of an entry game that is qualitatively similar to BoS; Farrell argues that the presence of cheap talk allows for equilibria in which cheap talk messages are taken seriously if it is optimal for senders of such messages to keep their promises and if senders believe that receivers believe those messages. Arvan, Cabral, and Santos (1999) go further, showing that in games like BoS, if a few additional conditions are satisfied, payoffs *will* be improved by two–way cheap talk. Rabin (1994) shows that allowing a large number of rounds of pre–play two–way communication in similar games enables players to ensure at least their minimum payoff in any Pareto efficient Nash equilibrium. Using a different type of game—a class of signaling games with continuous type and message spaces—Crawford and Sobel (1982) show that the more closely players' interests are aligned, the more likely one–way cheap talk is to be informative (in a sense which they define).

[6]Aumann (1990) used the term *self–enforcing* to refer to a self–committing, self–signaling message.

committing, while D messages are self–committing, but not self–signaling.[7] We can therefore hypothesize that messages in the Stag Hunt are most often truthful and believed, messages in Chicken and D messages in Prisoner's Dilemma are less often truthful and believed, and C messages in Prisoner's Dilemma are least often truthful and believed.

The value of cheap talk as a coordination device has also been examined experimentally. While there has been some question as to which type of cheap talk works best (one–way or two–way), a large body of evidence suggests that coordination is further facilitated when some kind of communication is possible.[8] Surprisingly, cheap talk has also been found to induce greater cooperation in games such as Prisoner's Dilemma, where messages are neither self–committing nor self–signaling: specifically, collusion in oligopoly games (for a survey, see Holt (1995), pp. 409–411), public–good provision in public good games (for a survey, see Ledyard (1995), pp. 156–158), and the Prisoner's Dilemma itself (see, for example, Swensson (1967)). For other examples, see Dawes et al. (1977), Orbell et al. (1988), and Ostrom et al. (1994).

While many researchers have looked at the effects of cheap talk, there has been comparatively little research into other potential coordination devices, such as observed past actions.[9] By contrast with cheap talk, observed previous–round actions are credible by their very nature. However,

[7] It can be shown that in a symmetric 2×2 game, if a message is self–signaling, it is self–committing.

[8] For example, Cooper et al. (1989) showed that when one–way communication is used in a two–player BoS game, coordination becomes much more likely—more so than when two–way communication is used. Cooper et al. (1992) find that two–way communication can be more useful than one–way communication in certain types of coordination games, such as in a game similar to the Stag Hunt game that we consider. However, they also find that one–way communication always improves coordination relative to the case of no communication in all varieties of coordination games that they examine. Wilson and Rhodes (1997) provide further evidence that one–way communication is useful in achieving high–payoff equilibria in pure coordination games in which players' payoffs are common knowledge. In his survey of cheap–talk experiments, Crawford (1998) argues that one–way communication is preferable in coordination games such as BoS that require players to engage in "symmetry–breaking," that is, to play different actions, even though their action set and payoff functions are identical. (The two pure–strategy Nash equilibria in our Chicken game require such symmetry–breaking.) Crawford further notes that in some games that do not require symmetry–breaking, such as the Stag Hunt game we consider, cheap talk may play an important "reassurance role," allowing the sender to signal that she understands the structure of the game and the existence of the payoff dominant equilibrium. Cooper (1999) provides a survey of coordination game experiments.

[9] In addition to cheap talk and observation, other devices that have been examined as aids in solving experimental coordination games are (1) an outside option in which one player first chooses between a sure payoff and playing the coordination game (Cooper et al. (1993)), and (2) a preplay auction, in which the rights to play a coordination game are auctioned off to the highest bidders (Van Huyck et al. (1993), Crawford and Broseta (1998)). These devices, which are equivalent to giving some players an opportunity to choose a costly signal, typically do not reduce the number of Nash equilibria of these games, but "forward induction" refinements such as the Cho–Kreps (1987) intuitive criterion may eliminate all equilibria except those in which the sender sends a costly signal (bypassing the outside option in (1) or paying a high price for the right to play in (2)), and the Pareto efficient equilibrium of the stage game is then played.

observed actions differ from cheap talk in the extent to which they can be considered signals of the sender's likely action. In the case of cheap talk, there is no question that a message is a signal; that is its only function. While a previous–round action may be thought of by the observer (receiver) as a signal, the sender might not have intended for his action to be interpreted as a signal; after all, this action choice also affected his previous–round payoff. Previous–round actions play a dual role of both signal and action. Thus, the receiver of such a signal must bear in mind that while credible, observed previous–round actions are not a perfect forecast of the sender's current–round intended action.[10] Since observed actions play this dual role in all the games we consider, we hypothesize that the extent to which observed previous–round actions correlate with current–round actions will not vary with the game.

The experimental literature contains some evidence that players take observed actions of other players into account, when such information is available. Kahneman, Knetsch, and Thaler (1986), Eckel and Grossman (1996), and Fehr, Gächter, and Kirchsteiger (1997), for instance, show that players play more cooperatively toward opponents believed to have behaved cooperatively in the past, and less cooperatively toward opponents believed to have behaved uncooperatively in the past. Duffy and Feltovich (1999) provide evidence that in a simple bargaining game, observation of other players' actions (along with their payoffs) can change the frequency of rejections of unfavorable offers, by altering players' perceptions of what constitutes a fair outcome. Wilson and Sell (1997) consider *both* cheap talk and observation in a repeated public good game, and report that the combination of cheap talk and observation of past contributions resulted in a level of cooperation that was approximately the same as when cheap talk and observation were both absent, but that adding either cheap talk or observation by itself actually *decreased* the amount of cooperation.[11]

To summarize, we hypothesize that the extent to which "actions speak louder than words" largely depends on the credibility of cheap talk messages in the game played. As discussed earlier, in Prisoner's Dilemma, where cheap talk should not be credible, observation is hypothesized to

[10]By this logic, we can consider observation to be a type of costly signal, in contrast to costless cheap talk.

[11]There are major differences between the Wilson–Sell experimental design and our own that make comparisons difficult. One important difference is that their subjects play with the same opponents in every round. Consequently, their notion of "observation" includes not only opponents' previous–round actions, but also opponents' current–round actions, and the player's own payoff.

be the better device for achieving good outcomes. In Stag Hunt, where messages should be very credible, cheap talk is hypothesized to be the better device. In Chicken, where cheap talk is less credible than in Stag Hunt, but somewhat more credible than in Prisoner's Dilemma, the efficacy of cheap talk relative to observation in achieving good outcomes is hypothesized to lie somewhere between that found in the other two games.

3 Experimental Procedures

We use a 3×3 experimental design in which we vary the game—PD, SH, or CH—and the information condition—cheap talk, observation, or control.[12] In each experimental session, 20 subjects with no prior experience played 10 rounds of each of the 3 games (30 rounds total) under a single information condition.[13] Subjects were told in the instructions that there would be exactly ten rounds of each game. Subjects were primarily University of Pittsburgh undergraduate students. In each game, one–half of the subjects were randomly chosen to be "row players" and the other half were "column players." Subjects remained in the same role throughout a game. We used a round–robin matching format, so that each row player faced each column player exactly once in a ten–round game.[14]

The experimental sessions were conducted at the University of Pittsburgh, using a laboratory of networked personal computers. Each subject was seated at a computer and given written instructions, which were also read aloud in an effort to make the rules of the experiment common knowledge. A sample copy of the instructions can be found in the Appendix. The computer screen displayed the payoff matrix for each game, the results from the player's previous rounds of play of that game, and additional information depending on the treatment. The payoff matrix was also drawn on a blackboard for all subjects to see. Subjects input their actions by choosing which row, (R1 or R2) or column (C1 or C2) of the current game payoff matrix they wanted to play. R1 and

[12]More precisely, we use a $3 \times 3 \times 2$ design, because we also considered two different orders in which subjects played the three games: PD–SH–CH and CH–SH–PD. We found no systematic differences in play due to the ordering of the games, so in our results, we pooled the data from both orderings. For a similar reason, we pooled the row– and column–player data.

[13]In another paper (Duffy and Feltovich (2000)), we combine the two information conditions, cheap talk and observation, into a single treatment, so that receivers of cheap talk messages also observe the previous–round action played by the message sender.

[14]Kamecke (1997) shows that this matching technique ensures that the ten–round game maintains the one–shot character of the stage game, and does so efficiently in the sense that there is no way to increase the number of rounds, while keeping the same number of players and continuing to maintain the one–shot nature of the game.

C1 corresponded to C actions and R2 and C2 corresponded to D actions. In describing the actions available to subjects we avoided any reference to the words "cooperate" or "defect," and we referred to each player's opponent as his or her "partner."

No cheap talk or observation occurred in the first round of a game.[15] Beginning with the second round of each game of the cheap talk and observation treatments, and continuing with every round thereafter, cheap talk or observation took place before subjects chose their current–round actions. In both treatments, one member of each pair of players was chosen with probability one–half to send a cheap talk message or to have his previous–round action revealed to the other player.[16] In the cheap talk sessions, senders were limited to sending either the message C1 or C2 if they were a column player or R1 or R2 if they were a row player. The instructions asked senders to send a message "indicating the action they intend to choose in the next round." The instructions further explained that these messages were not binding; the sender could choose either action regardless of the message sent.

Messages were entered using the computer keyboard (there was no verbal communication) and were revealed to receivers on their computer screens prior to the play of the round. In the observation treatment, previous–round actions were also revealed to receivers via their computer screen. In both the cheap talk and observation treatments, both senders and receivers were asked to record, on record sheets, the signals that were sent and received. Our aim was to call subjects' attention to the information that they had either provided or received. After cheap talk or observation had taken place and the signals had been recorded, or following the pairing of subjects in all rounds of control treatment games and in the first round of cheap talk and observation games, each player

[15] In the first round of each game, observation of past actions is not possible. For comparison purposes, we chose to suspend cheap talk in the first round as well.

[16] In some one–way cheap–talk experiments, such as those of Cooper et al. (1989), subjects alternated between the roles of sender and receiver in a known, deterministic manner. Such a design is fine for studying cheap talk, but it is less suited for the study of observation. If, as is likely, behavior when being observed is different from behavior when not being observed, an observed action will not be useful for forecasting an action that will not be observed. Our design avoids this problem. Also, since our design does not designate players as senders or receivers, all players can be treated similarly for data analysis purposes, yielding more observations. A disadvantage of our design is that subjects face an additional layer of complexity, as the 50% chance of being observed introduces more uncertainty. In Duffy and Feltovich (2000), we conduct an experiment in which we allow both cheap talk and observation prior to the play of the same games examined in this paper. In one cell, players are randomly chosen to be senders or receivers (of both types of signal) in every round, and in another, players assume a single role throughout the game. We find that there is no difference in aggregate play between these cells.

chose an action. After all choices were made, payoffs in points were revealed to all subjects.

Subjects were informed that each point in the payoff matrix represented a 1% chance of winning \$1.00. At the end of each round, an integer between 1 and 100 inclusive was randomly drawn. Subjects whose payoff in that round was greater than or equal to the chosen number earned \$1.00 for the round; those whose payoff was less than that number earned nothing for the round.[17] At the end of the session, subjects received in cash their total earnings from all rounds as well as a \$5.00 participation payment. Sessions lasted between 60 and 75 minutes. Subjects earned an average of about \$25.00 for participating in the experiment.

4 Results

Thirteen experimental sessions were conducted. Our experimental findings include a number of interesting results, some of which can be seen in Table 2. This table reports the aggregate relative frequencies of cooperation, coordination (where relevant), and payoff efficiency for each cell.

Table 2: Experimental Relative Frequencies and Payoff Efficiency (All rounds)

Game	Treatment	Cooperation		Coordination		Efficiency
PD	Control	$.222^a$	(222/1000)	—		$.113^a$
	Cheap Talk	$.400^b$	(320/800)	—		$.260^b$
	Observation	$.404^b$	(323/800)	—		$.266^b$
SH	Control	$.607^a$	(364/600)	$.513^a$	(308/600)	$.453^a$
	Cheap Talk	$.835^a$	(501/600)	$.840^b$	(504/600)	$.803^b$
	Observation	$.757^a$	(454/600)	$.667^{ab}$	(400/600)	$.636^{ab}$
CH	Control	$.537^a$	(430/800)	$.475^a$	(380/800)	$.696^a$
	Cheap Talk	$.564^{ab}$	(451/800)	$.532^b$	(426/800)	$.741^{ab}$
	Observation	$.634^b$	(507/800)	$.438^a$	(350/800)	$.780^b$

Result 1 *Subjects' actions are partially, but not completely, influenced by strategic considerations.*

The third column of Table 2 shows the aggregated relative frequencies of cooperation in each cell. Notice that differences in the level of cooperation are much more stark across *games* than

[17]This binary lottery procedure is intended to induce risk neutral behavior among hypothetical expected–utility maximizing agents. See, e.g., Roth and Malouf (1979) for a discussion.

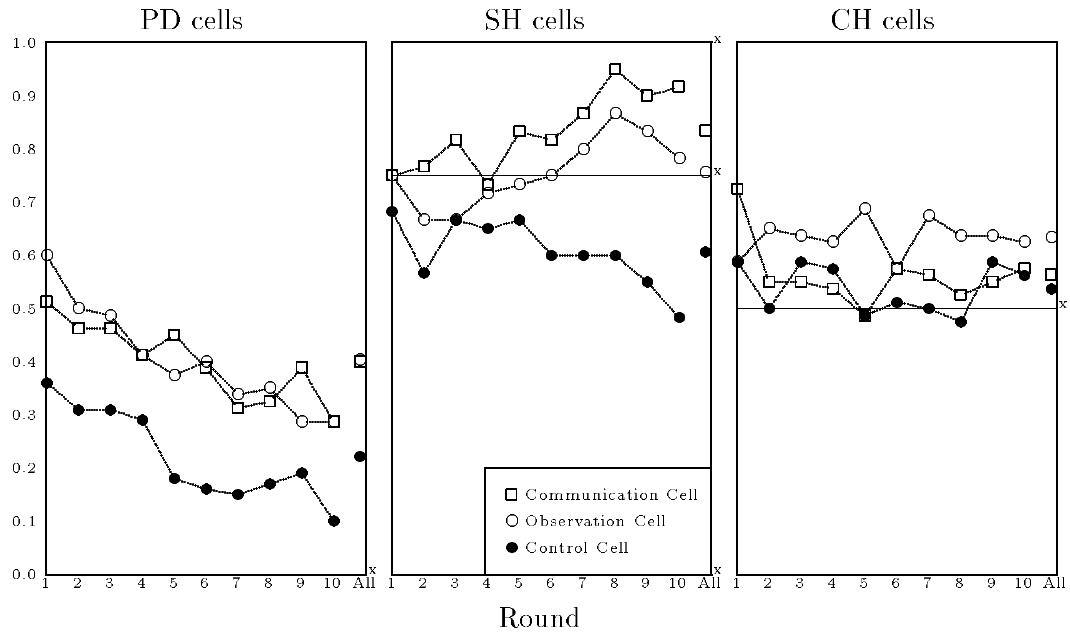
across *information conditions*; cooperation is generally highest in the SH cells and lowest in the PD cells. Recall that the unique Nash equilibrium in Prisoner's Dilemma involves no cooperation, while the three Nash equilibria in Chicken involve cooperation 50% of the time (differing only in how much each player cooperates), and two of the three Nash equilibria in Stag Hunt involve cooperation between 75% and 100% of the time. The ordering of the observed frequencies of cooperation across the three games is thus similar to the ordering of the equilibrium levels of cooperation across the three games. (Admittedly, the third Nash equilibrium of Stag Hunt has cooperation never occurring.)

The Nash equilibrium point predictions fare well in the SH cells, provided we choose the equilibrium that is closest to the observed data; the mixed–strategy equilibrium has cooperation occurring 75% of the time, and the actual relative frequency of cooperation in the three SH cells ranges from 0.607 to 0.835. In the CH and PD cells, there are substantial deviations from equilibrium, all in the direction of higher cooperation. The actual relative frequency of cooperation in the three CH cells ranges from 0.537 to 0.634, while the equilibrium prediction is exactly 0.5, and the relative frequency of cooperation in the three PD cells ranges from 0.222 to 0.404, while the equilibrium prediction is zero.

The round–by–round observed relative frequencies of cooperation in the PD, SH, and CH cells are shown in Figure 2. (Note that about half the subjects played the games in the sequence shown in Figure 2, while the rest played the games in the reverse sequence.) Also shown are the 10–round average frequency of cooperation, at the far right of each box, and the frequency of cooperation in each Nash equilibrium, as a horizontal line marked by an "x" (see also Table 1). The round–by–round frequencies of coordination and payoff efficiency are not shown, but follow similar patterns. Notice that some cooperation occurs in all PD cells, even in the last round. It thus appears that play is influenced, at least to some extent, by nonpecuniary considerations, forward–looking behavior, or both.[18]

[18] We note that the amount at stake in any single decision is small—never more than thirty cents, and sometimes as little as ten cents in expected value. Thus, even in a game such as the Prisoners' Dilemma where cooperation is strictly dominated, it is never terribly costly. We will see shortly, however, that the observed frequency of cooperation cannot be ascribed to non–salient payoffs or nonpecuniary considerations alone; it varies systematically with not only the game played, but also the information treatment and the particular piece of information sent or received.

Figure 2: Average Relative Frequency of Cooperation (All Sessions)
Rounds 1–10, 10–Round Averages, and Equilibrium Predictions



The next result concerns the effect of additional information on aggregate play.

Result 2 *The addition of either observation or communication increases the frequency of cooperation, the frequency of coordination, and payoffs relative to the control, though the relative efficacy of observation and communication depends on the game.*

Supporting evidence for this result is provided in Table 2. In addition to the levels of cooperation, coordination, and payoffs in each cell, we report the results of robust rank–order tests of differences in these statistics across information conditions of each game.[19] Within each box of Table 2, frequencies with a b superscript are significantly larger at the 5% level than those with only an a superscript, while neither is significantly different at the 5% level from frequencies with an ab superscript. Thus, in the PD cells, adding either cheap talk or observation leads to significant

[19]We use the robust rank–order test instead of the more commonly used Wilcoxon–Mann–Whitney test because the latter assumes that the two samples being compared come from distributions with identical second– and higher–order moments, which we have no reason to believe a priori. See Siegel and Castellan (1988) for a discussion of this issue, as well as more thorough descriptions of the nonparametric statistical tests used in this paper. All of our statistical tests are performed on data at the session level, rather than at the level of the individual subject. While this has the disadvantage of reducing the power of the tests, it avoids the problem of the likely interdependence of play among subjects in the same session.

increases in the frequency of cooperation and payoff efficiency relative to the control, but there is no significant difference in the frequency of cooperation or payoff efficiency between the PD-cheap talk and PD-observation treatments. In the SH cells, there are no significant differences in cooperation frequencies across treatments.[20] On the other hand, both the frequency of coordination and payoff efficiency are higher in the cheap talk cell than in the control, though none are significantly different from their counterparts in the observation treatment. In the CH cells, there is little difference in either the frequency of cooperation or payoff efficiency between the control and cheap talk treatments. However, observation yields a significant increase in the frequency of cooperation and payoff efficiency relative to the control. Cooperation and payoffs in the observation cell are not significantly higher than those in the cheap talk cell (though they would be if we had instead used the 10% level of significance).

The differences across cells can also be seen when the data are disaggregated by round (see Figure 2). In addition, we can see that, consistent with many Prisoner's Dilemma experiments, cooperation decreases over time in all three PD cells. Also, in the SH–control cell, cooperation decreases over time, while in the other two SH cells, cooperation increases (particularly in the SH–cheap talk cell). In the CH–control and CH–observation cells, the level of cooperation stays roughly constant over time, while in the CH–cheap talk cell, it drops between the first and second rounds and stays roughly constant thereafter.

A very interesting finding from the cheap–talk treatments of Stag Hunt and Chicken is the high frequency of successful coordination relative to that which would be expected *given the observed frequency of cooperation*. In Stag Hunt, coordination means play of either a (C,C) or a (D,D) strategy pair. If q denotes the observed frequency of cooperation, and if there is no correlation between the actions of Row and Column players, then the probability of a (C,C) pair should be q^2 and that of a (D,D) pair should be $(1-q)^2$, so that the total probability of successful coordination should be $q^2 + (1-q)^2$. In the SH–cheap talk cell, the observed frequency of cooperation (0.835)

[20]This lack of significance may seem surprising, since Table 2 suggests that adding either observation or cheap talk *increases* the overall frequency of cooperation substantially. The explanation for this discrepancy is that there is a lot of variance in the control sessions; in two of the three control sessions, the frequency of cooperation is close to 50%, while in the other one, it is much higher (81%) and comparable to the non–control cells. Since we have only three control sessions, the high variance across these sessions accounts for the statistically insignificant results.

would thus imply a *predicted* frequency of coordination of 0.724. However, the *actual* frequency of coordination, 0.840, was considerably higher. In contrast, the actual frequency of coordination in the SH–observation cell was only slightly higher than the predicted frequency (0.667 vs. 0.632), and in the SH–control cell, the actual frequency was slightly lower than the predicted frequency (0.513 vs. 0.523). Similarly, in Chicken, coordination means play of either a (C,D) or a (D,C) strategy pair. If q is the observed probability of cooperation and there is no correlation between the actions of Row and Column players, then either pair should occur with probability $q(1-q)$, implying a total probability of successful coordination equal to $2q(1-q)$. In the CH–cheap talk cell, the frequency of cooperation (0.564) would imply a predicted frequency of coordination of 0.492. However, the actual frequency of coordination was higher, at 0.532. In the other two cells, the predicted and actual frequencies of coordination were roughly the same—0.497 and 0.475 in the control cell, and 0.464 and 0.438 in the observation cell. According to either the sign test or the Wilcoxon summed–ranks test, actual coordination frequencies are significantly higher than predicted coordination frequencies in the cheap talk treatment ($p < .008$), while in the other two treatments, there is no significant difference between predicted and actual frequencies of coordination.

We now turn to the effect of signals on the behavior of individual receivers.

Result 3 *Receivers condition their actions on the information they receive.*

Table 3 reports the aggregate relative frequency of cooperation in response to (a) receiving a C signal, (b) receiving a D signal, (c) having sent a C signal, or (d) having sent a D signal. In every game, in both communication and observation treatments, receivers of C signals cooperate more than receivers of D signals, though the difference is not always large. Consider first the SH cells. In the SH–communication cell, players almost always (over 97% of the time) choose C after receiving a C signal, and they usually (over 80% of the time) choose D after receiving a D signal. In the observation cell, players choose C over 80% of the time after receiving a C signal, and they choose D about 54% of the time after receiving a D signal. According to either the sign test or the Wilcoxon signed–ranks test, players are significantly more likely to cooperate after receiving a C signal than a D signal (p=0.125, the smallest p–value possible when there are three sessions for

Table 3: Conditional Relative Frequencies of Cooperation (Rounds 2–10)

Game	Cheap Talk Treatment			Observation Treatment		
PD	receive C	.504	(119/236)	observe C	.597	(89/149)
	receive D	.161	(20/124)	observe D	.213	(45/211)
	send C	.513	(121/236)	C observed	.631	(94/149)
	send D	.153	(19/124)	D observed	.223	(47/211)
SH	receive C	.978	(226/231)	observe C	.827	(167/202)
	receive D	.128	(5/39)	observe D	.559	(38/68)
	send C	.918	(212/231)	C observed	.871	(176/202)
	send D	.282	(11/39)	D observed	.412	(28/68)
CH	receive C	.623	(132/212)	observe C	.599	(136/227)
	receive D	.304	(45/148)	observe D	.579	(77/133)
	send C	.623	(132/212)	C observed	.780	(177/227)
	send D	.568	(84/148)	D observed	.526	(70/133)

each treatment) in both cells.

More surprising (from a game–theoretic standpoint) is the extent to which subjects in the PD cells consider their additional information. Consistent with similar results reported by other researchers, subjects in the observation cell chose to cooperate much more often when matched with someone known to have cooperated in the previous round than when matched with someone known to have defected (cooperating about 60% of the time in the former case, versus just over 20% of the time in the latter) It certainly appears that subjects are willing to forgo some (expected) monetary payoff by cooperating with people who have previously cooperated. Subjects in the cheap talk cell also take their additional information into consideration. They are much more likely to cooperate with someone who sends a cooperate signal than with someone who sends a defect signal (50.4% vs. 16.1%). Again, players are significantly more likely to cooperate after receiving a C signal than a D signal (p=0.0625, the smallest p–value possible when there are four sessions of each treatment) in both treatments.

In the CH cells, subjects also appear to take their additional information into account. In the observation cell, subjects cooperate slightly more after receiving a C signal than after receiving a

D signal (59.9% vs. 57.9%), but this difference is not significant.[21] Subjects in the communication cell are also more likely to cooperate after receiving a C signal than after receiving a D signal (62.3% vs. 30.4%), though again, this difference is not significant. The fact that subjects in both CH treatments are at least as likely to cooperate after receiving a C signal as when they receive a D signal runs counter to the Nash equilibrium prediction: both pure–strategy equilibria of this game require the two players to choose different actions, so if players believe that messages and previous–round actions are positively correlated with current–round actions, they should do the opposite—choose D when receiving a C signal and choose C when receiving a D signal. This result suggests that subjects' choices of actions may be influenced by nonpecuniary aspects of outcomes.

Having examined the effect of signals on receiver behavior, we now consider the effect of signals on sender behavior.

Result 4 *Subjects often send truthful signals even when they have no monetary incentive to do so.*

In every cell of the communication and observation treatments, senders of C signals cooperate more frequently than senders of D signals. As noted in Section 2.1, Stag Hunt is the game in which we expect cheap talk messages to be the most truthful. Indeed they usually are; subjects who send C messages actually choose C over 90% of the time, and those who send D messages actually choose D over 70% of the time. According to either the sign test or the Wilcoxon signed–ranks test, players in the cheap talk cell are significantly more likely to cooperate after sending a C signal than a D signal (p=0.125, the smallest p–value possible when there are three sessions of each treatment).

In Chicken, cheap talk is less credible than in Stag Hunt, but players still have an interest in coordination, so there remains some incentive to send truthful messages. In fact, messages in the CH–communication cell do tend to be truthful, but not nearly as frequently as in the SH–

[21] When the CH–observation data are broken down by sessions, an interesting pattern emerges. In three of the four sessions, players whose previous–round D signals were observed usually defect in the current round (more than 75% of the time in each of the three sessions) and observers of D signals usually cooperate (at least two thirds of the time in each of the three sessions). In the fourth session, however, the opposite happens. Players whose previous–round D signals were observed *cooperate* over 70% of the time in that session, and observers *always* defect. The degree of cooperation by senders of D signals in this session is even higher than that by senders of C signals in that session (the only session in which this is true), and is enough to make the difference in the level of cooperation between senders of C and D signals in the CH–observation cell insignificant. These profiles of sender/receiver behavior are examples of the two possible types of the "symmetry–breaking" phenomenon that one–way observation allows, and which has been observed in BoS experiments.

communication cell; in CH, 62.3% of C messages are followed by C actions, but only 43.2% of D messages are followed by D actions, so the overall fraction of truthful messages is just over one–half (54.4%). In this case, the difference in cooperation between senders of C messages and senders of D messages is not significant.

In Prisoner's Dilemma, messages are not self–signaling, and C messages are not even self–committing. Indeed, given the way message receivers react (Result 3), there is a strong incentive to signal Cooperate and then choose the Defect action. Nevertheless, in the PD–communication cell, D messages are truthful almost 85% of the time, and even C messages are truthful about half the time.[22] Once again, according to either the sign test or the Wilcoxon signed–ranks test, players in this cell are significantly more likely to cooperate after sending a C signal than a D signal (p=0.0625, the smallest p–value possible when there are four sessions of each treatment).

In the SH–observation cell, subjects who sent C signals subsequently cooperated around 87% of the time, while those who sent D signals subsequently cooperated only about 40% of the time. In the PD–observation cell, the frequencies of cooperation are 63.1% and 22.3%, and those in the CH–observation cell are 78.0% and 52.6%. In all three games, the difference in the level of cooperation is significant (p=0.125, 0.0625, and 0.0625, respectively, the smallest p–value possible for the number of sessions run, in each case). We note, however, that the correlation between observed previous–round actions and current–round actions does not differ substantially from the correlation between *unobserved* previous–round actions and current–round actions in any of our cells. For example, in the PD–control cell, subjects who chose C in the previous round chose C in the current round 37.3% of the time, as opposed to only 15.6% of those who chose D in the previous round. In the SH–control cell, the numbers were 81.2% and 24.9%, and in the CH–control cell, they were 72.5% and 31.0%, respectively. In other words, players' actions tend to be positively autocorrelated, and at least some of this autocorrelation exists irrespective of whether or not individuals are being observed. Of course, this does not reduce the utility of observed actions in forecasting current–round actions.

[22]It is odd that anyone would send a D message and then cooperate, given the likely behavior of receivers. Indeed, the frequency with which the sender of a D message cooperates stays roughly the same from rounds 2 through 10, so it cannot be ascribed to misunderstanding of the game which eventually goes away. For lack of a better explanation, we suppose this is due to noisy behavior by subjects, possibly from low incentives per decision.

Given Result 4, that subjects' signals tend to be truthful and hence informative, Result 3, that receivers condition their actions on the signals they receive, can be justified. To get at this issue, we use senders' observed relative frequencies of cooperation, conditional on their signals (the bottom two rows of Table 3), to calculate receivers' expected payoffs to cooperating or defecting. If players' choices depend primarily on strategic considerations, we would expect to see that the probability of cooperation depends on the difference in expected payoff between C and D. The higher this difference, the more likely cooperation ought to be. If players' choices depend solely on strategic considerations, we would expect them to choose C if and only if this difference is positive.[23]

Table 4: Payoff Differences and Receiver Strategy Choices (Rounds 2–10)

Cell	Signal Received	Expected Payoff (C)	Expected Payoff (D)	Difference	Frequency of C
PD–communication	C	40.76	60.51	−19.75	0.504
PD–communication	D	19.19	46.12	−26.94	0.161
PD–observation	C	47.85	65.23	−17.38	0.597
PD–observation	D	23.36	48.91	−25.55	0.213
PD–control	–	22.40	48.27	−25.87	0.207
SH–communication	C	65.06	55.00	+10.06	0.978
SH–communication	D	26.92	55.00	−28.08	0.128
SH–observation	C	62.28	55.00	+7.28	0.827
SH–observation	D	34.71	55.00	−20.29	0.559
SH–control	–	45.89	55.00	−9.11	0.598
CH–communication	C	62.45	64.91	−2.45	0.623
CH–communication	D	61.35	62.70	−1.35	0.304
CH–observation	C	65.59	71.19	−5.60	0.599
CH–observation	D	60.53	61.05	−0.53	0.579
CH–control	–	61.14	61.68	−0.54	0.532

These expected–payoff differences are shown in Table 4. For each cell and for both signals received, the difference in expected payoff between C and D is shown, given the observed conditional relative frequencies of C and D by signal senders over all rounds.[24] The actual relative frequency

[23] More accurately, we are examining whether players' choices depend on *myopic* strategic considerations. In the observation cells, non-myopic strategic considerations would include the effect a player's action choice has on a potential next–round observer.

[24] Of course, the exact values of the expected payoffs to C and D reflect a lot of information that subjects do not know, such as play in future rounds and in other sessions. Therefore the results we report here, though suggestive, should be interpreted with some caution.

of C by receivers is also shown. Also shown, for comparison, are the corresponding unconditional expected payoffs and cooperation frequencies from the control cells. The results in Table 4 clearly indicate that receivers' actions cannot be explained by myopic strategic considerations alone. In the PD cell, for example, the payoff–maximizing action is always D (since the payoff difference is negative in every situation), but cooperation often occurs, in some cases more than half the time. However, differences in expected payoffs across treatment do appear to matter. In the five cases within the PD game, there is a perfect ordinal relationship between the difference in expected payoff and the observed frequency of C. In the SH game, this relationship is also perfect. These relationships correspond to a Spearman rank–order correlation coefficient of 1.000, which is significantly different from zero at the 5% level. In the CH game, there is not a perfect relationship between the difference in expected payoff and the observed frequency of cooperation. Indeed, in the two situations in which the difference in expected payoff is lowest, the actual frequency of C is highest. The Spearman coefficient of the CH data is −0.500, which is not significantly different from zero.[25]

4.1 Do Actions Speak Louder Than Words?

While both cheap talk messages and observed previous–round actions are helpful in predicting current–round actions in all three games, the question asked in the title of this paper remains: *do actions speak louder than words?* That is, which type of signal is a better indicator of the action the sender actually plays? One obvious measure of the value of a signal is its truthfulness—the likelihood that the signal is the same as the action subsequently played. According to this criterion, in the SH cells, words speak louder than actions; cheap talk messages are the same as current–round actions 88.9% of the time, while observed previous–round actions are the same as current–round actions only 80.0% of the time. On the other hand, actions speak louder than words in the other

[25]One possible explanation for these findings is reciprocity. Notice that in all three games, ignoring the control cells in which signals are not seen, receivers are more likely to play C when receiving a signal of C (either a message or an observed action) than when receiving a signal of D. Since we know from Table 3 that senders' signals tend to be truthful, we can hypothesize that receivers have some taste for cooperating with senders whom they believe are likely to cooperate, and defecting when senders are likely to defect. Conditional on this assumption about preferences, the relationship between expected payoff differences and the frequency of cooperation becomes perfect; for a given game and signal, the cell (communication or observation) with the higher expected payoff difference is always the cell with the higher frequency of cooperation.

two games. In the PD cells, observed actions are the same as current actions 71.7% of the time, while messages are the same as current actions only 62.8% of the time. In the CH cells, observed actions are the same as current actions 66.7% of the time, while messages are the same as current actions only 54.4% of the time. The fact that the difference in truthfulness across observation cells is small relative to the difference across cheap talk cells is roughly consistent with our hypothesis, from Section 2.1, that the correlation between messages and actions would vary with the game, while the correlation between previous–round actions and current–round actions would not.[26] The manner in which messages and actions vary with the game is also consistent with our hypotheses.

Another criterion for measuring the value of signals is their *resolution*, their usefulness as forecasts of senders' current–round actions. The resolution of a set of signals can be defined roughly as their ability to partition the sample of prediction–event pairs into subsets in which the events are mostly of one type; for a given signal, the actual frequency of subsequent Cooperate actions should be either close to zero or close to one.[27] Mathematically, resolution is defined to be $Pr(\mathrm{C})Pr(\mathrm{C}|\mathrm{C})(1-Pr(\mathrm{C}|\mathrm{C}))+Pr(\mathrm{D})Pr(\mathrm{C}|\mathrm{D})(1-Pr(\mathrm{C}|\mathrm{D}))$ where $Pr(\mathrm{C})$ ($Pr(\mathrm{D})$) is the probability of a C (D) signal being sent, and $Pr(\mathrm{C}|\mathrm{C})$ ($Pr(\mathrm{C}|\mathrm{D})$) is the probability of the current–round action being C after a C (D) signal is sent. Lower values reflect better resolution; a resolution of 0 means that following a signal, the frequency of C is either zero or one, and a resolution of 0.25 indicates a rule that is completely uninformative (C and D are equally likely).[28] In the PD game, the resolution of messages is .208, while that of observed actions is .198. In the SH game, the resolution of messages is .094, while that of observed actions is .145, and in the CH game, the resolution of messages is .239, while that of observed actions is .200. Thus, in the SH game,

[26] Because there is a nonnegligible difference in "truthfulness" across observation cells, support for our specific hypotheses is only rough.

[27] The resolution of a signal may be related to its truthfulness, but it need not be. For example, if Subject A cooperates 50% of the time following either signal, while Subject B always defects after sending a C signal and always cooperates after sending a D signal, Subject A's signals are more truthful (they are truthful 50% of the time, whereas B's never are), but Subject B's signals have a better resolution (his current–round actions can be perfectly predicted from his signal, while Subject A's signal provides no information at all).

[28] Yates (1982) provides a general description of the notion of resolution, and a second notion, calibration, which is related to our "truthfulness". Murphy and Winkler (1977) use these concepts to evaluate the quality of meteorologists' forecasts of the probability of precipitation, and Lichtenstein, Fischhoff, and Phillips (1982) do the same for the quality of students' judgments concerning general–knowledge questions (see also Camerer (1995, pp. 590–593)). Feltovich (2000) uses these concepts to test the forecasting ability of several models of individual learning in relation to experimental data.

messages are better resolved than observed actions, while the opposite is true in the other two games.

We see, then, that the answer to the question, *do actions speak louder than words?* is, "it depends on the game." According to either truthfulness or resolution, in Stag Hunt, words are more useful than actions, while in Chicken and Prisoner's Dilemma, actions are more useful than words. It is worth noting that there is a correspondence between the usefulness of a set of signals and its ability to effect high–payoff outcomes; in all three games, the more useful set of signals is also the one that leads to higher payoffs (recall the last column of Table 2), though the difference in payoffs is not always significant.

5 Conclusion

Understanding how individuals achieve good outcomes in strategic situations is crucial to addressing a number of important questions in economics including issues of contract and mechanism design, the origin of standards and conventions, even the possibility of self–fulfilling macroeconomic fluctuations. Previous experimental studies have focused primarily on the role of cheap talk. In this paper we propose for comparison an alternative mechanism for achieving good outcomes: observation of an opponent's past actions.

Subjects in our experiment play three simple games under one of three information treatments: no information about opponents, one–way cheap talk, or one–way observation of an opponent's previous–round action. The games we use differ substantially in the extent to which cheap talk ought to be credible. We find that both cheap talk and observation result in better outcomes—more cooperation, more coordination on pure–strategy Nash equilibria (in games with multiple equilibria) and higher payoffs—than when no additional information about an opponent is available, but that the effectiveness of cheap talk relative to observation depends on the game played. In answer to the question posed by our title, *do actions speak louder than words*, we find that in games such as Stag Hunt, where messages ought to be credible, cheap talk is the relatively better device—words speak louder than actions. On the other hand, in games such as Chicken and Prisoner's Dilemma, where there is less (or no) reason to believe senders' messages, observation is relatively more effective than cheap talk—actions speak louder than words.

The results of this experiment shed some light on the inadequacy of standard game theory and provide us with further insight as to how the theory might be extended. Two of the major deficiencies of game theory are (1) its indeterminacy of prediction in games with multiple equilibria, and (2) its inability to account for the degree of cooperation observed in social dilemmas. Experimental evidence and casual empiricism suggest that individuals often find ways in which to solve coordination problems and that cooperation is frequently observed. Our contention is that the presence in many environments of additional information, such as cheap talk or observation of prior behavior, plays a role in the solution of these coordination problems and in the fostering of cooperation. Game theory has little to say about how additional information may help to promote these "good outcomes." On the one hand, additional information can expand the set of equilibria to include some in which cooperation occurs, so it is possible to theoretically rationalize cooperative outcomes. On the other hand, an expansion in the set of candidate equilibria only serves to exacerbate the problem of multiplicity of equilibria, leaving us to ponder how it is that individuals achieve coordination on a particular equilibrium.[29] Our results suggest that not every outcome in this multiplicity is equally likely; allowing additional information generally leads to better outcomes. We hope that our results will encourage other theorists and experimenters to consider the role played by not only communication, but also observation of others' past actions, when thinking about how economic agents go about solving coordination problems.

[29] As pointed out in Section 2, in games such as Prisoner's Dilemma, it would be necessary to augment the standard theory even further in order to rationalize the cooperation observed in this game. Several researchers have suggested extending the theory to allow for preferences for nonpecuniary aspects of outcomes; see, for example, Rabin (1993), Levine (1998), Fehr and Schmidt (1999), or Bolton and Ockenfels (2000). After such an extension, the problem of multiple equilibria is often even further exacerbated.

Appendix

The following instructions were used in all experimental sessions involving "one–way observation". The instructions used in the "control" and "cheap talk" sessions were similar.

General Instructions

You are about to participate in an experiment in the economics of decision–making. Funding for this experiment has been provided by the National Science Foundation. If you follow these instructions carefully and make good decisions you might earn a considerable amount of money that will be paid to you in cash at the end of the session. If you have a question at any time, please feel free to ask the experimenter. We ask that you not talk with one another during the experiment. This experimental session consists of three different games. Each game consists of a number of rounds. At the start of each game, you will be randomly assigned a player type, either "row player" or "column player." Your type will not change during the course of game. In each round of this experiment you will be randomly matched to a player of the opposite type. You will be matched with a *different* player in every round of a game. We will refer to the person you are paired with in a round as your "partner." Your score in each round will depend on your choice and the choice of your partner in that round. You will not know the identity of your partner in any round, even after the end of the session.

Sequence of Play in a Round

- At the beginning of each round, the computer program randomly matches each player to a partner.
- Following the first round of each game, and every round thereafter, one member of each newly matched pair of players will be able to observe the action their partner chose in the last round of play, when this partner was matched with another player. The other member of the pair will not be able to observe the action chosen by his partner in the last round of play. In each round, there is a 50% probability that you get to observe the action your partner chose in

the last round of play and there is a 50% probability that your partner gets to observe the action that you chose in the last round of play.

- You and your partner play the game. If you are a row player, you choose which row of the payoff table to play, R1 or R2. If you are a column player, you choose which column of the payoff table to play, C1 or C2.
- After all players have chosen actions, each player's payoff or *score* is revealed. Your score is determined by your action and the action of your partner according to the given payoff table.
- A random number is drawn. You earn $1.00 if your score is greater than or equal to this random number, and you earn nothing if your score is less than this random number.
- Provided that the last round of the game has not been reached a new round of the same game will then begin. You will be matched with a different partner in the new round.

The Games

The payoff table for each game you play will be shown on your computer screen and will also be drawn on the chalkboard. We will begin by playing the first game for 10 rounds. We will then play the second game for 10 rounds followed by 10 rounds of the third and final game. In every round of a game, both you and your partner have a choice between two possible actions. If you are designated as the row player, you must choose between actions R1 and R2. If you are designated as the column player, you must choose between actions C1 and C2. Your action together with the action chosen by your partner determines one of the four boxes in the payoff table. In each box, the first number represents your score and the second number represents your partner's score.

Observation

Beginning with the second round of each game, some players will be shown the action that their current partner chose in the previous round when matched with a different player. Either you will see your partner's action in the previous round, or your partner will see your previous round action.

These previous–round actions will be observed *before* players are called on to make decisions in the current round. We ask that you record the action you observed or the action your partner observed at the beginning of each round on your record sheets under the heading "Action Observed."

Earnings in Each Round

Your score (payoff) in a round is a number between 0 and 100. This is your percent chance of earning $1.00 in that round. Once your score is determined, we ask that you record your score for that round on your record sheet under the heading "Score." After all players have recorded their scores, a random number between 1 and 100 will be chosen and announced. Record the random number that is announced on your record sheet under the heading "Lottery Number." If your score is greater than or equal to the announced number (the lottery number), you earn one dollar ($1) or that round. If your score is less than the randomly chosen number, you earn zero for that round. Record your earnings for each round (either $1 or 0) in the last column of your record sheet under the heading "Earnings." Notice that the more points you earn in a round the greater is your probability of winning the $1 prize.

The Computer Screen

The top of your computer screen shows your player type, either row player or column player, your player ID number, and the round number. Please write your ID number on your record sheet. The middle of your computer screen contains the payoff table for the current game. Following this information is a prompt asking you to choose an action. If you are a row player you will choose between R1 and R2 and if you are a column player you will choose between C1 and C2. To indicate your choice, you type either 1 or 2 at the prompt and then you press the Enter key. After making your decision you must confirm your decision by typing Y for yes and then pressing Enter. If you want to change your decision, type N for no at the confirm prompt and then press Enter. If you choose not to confirm your decision you will have the opportunity to change the row or column you want to play. Following the first round, the bottom of the screen will show information about the results of your earlier rounds of play.

Payments

If you complete this experiment, you are guaranteed to receive a $5 participation payment. You will also be paid the sum of your earnings from each round. At the end of the session we will ask you to total

up your earnings from all 30 rounds and record the sum at the bottom of your record sheet. All earnings will be paid in cash at the end of the session.

ARE THERE ANY QUESTIONS BEFORE WE BEGIN?

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