Cooperative Institutions*

Piotr Evdokimov[1] and David Rahman[2]

[1]CIE-ITAM
[2]University of Minnesota

August 13, 2014

Abstract

This paper provides the first systematic experimental analysis of delay, communication, and reaction lags in a repeated prisoners' dilemma with frequent actions and imperfect monitoring. We independently manipulate delay of information and the ability of subjects to engage in limited communication and find that subjects earn significantly more *without* delay, a result that cannot be explained by standard repeated games models. We also find that communication always improves welfare and that average payoffs in one of our treatments (with communication and no delay) are significantly greater than the upper bound on public Nash equilibrium payoffs. We explore the possibility that this is driven by bounded rationality in the form of reaction lags and find that slowing down the experiment has no significant effect on behavior.

*We thank Drew Fudenberg, Thomas Holmes, Antonio Penta, John Quah, Itai Sher, Jan Werner and seminar participants at the University of Minnesota Applied Microeconomics and Mathematical Economics Workshops for helpful discussions. Please send any comments to pevdokim@gmail.com. Financial support from the National Science Foundation Grant No. SES 09-22253 and from the Heller-Hurwicz Economics Institute is gratefully acknowledged.

1 Introduction

Cooperative agreements are often complicated by limitations on how much information is available to the parties involved. Firms in an industry attempting to collude, for example, cannot reliably verify every relevant decision made by their competitors. Likewise, leaders of governments have an imperfect assessment of each other's actions. To sustain cooperation, firms form trade associations and heads of state hold regular meetings to share information and coordinate their actions. How can efficient outcomes be sustained in such settings? We address this question with a controlled experiment that assesses the effects of information delay, communication, and bounded rationality in a repeated prisoners' dilemma with imperfect monitoring and frequent actions.

Delay of information is ubiquitous. Company bonuses to CEOs are given on a yearly basis. The G20 meetings, which from 2008 to 2011 were held on a semi-annual basis, now take place annually. The Kyoto Protocol, a global initiative to reduce emissions of greenhouse gases, establishes two commitment periods for the member countries: 2008-2012 and 2013-2020. A theoretical justification for delay is that it can help overcome the bounds on welfare imposed by inefficient provision of incentives. For concreteness, consider a repeated game with imperfect monitoring in which a noisy public signal of the chosen action profile arrives every period. The monitoring technology can be such that under public equilibria,[1] welfare is bounded away from efficiency by a substantial amount (Fudenberg et al., 1994; Sannikov and Skrzypacz, 2007). Near efficiency, however, is possible if the signal is delayed, i.e., if players receive several signals at a time instead of receiving a signal every period. This point was first made by Lehrer (1989) and Abreu et al. (1991) and has since become a standard technique in the theoretical literature on repeated games, especially in the study of both private monitoring and private strategies.[2] This literature exploits the delay of endogenous information: private signals and actions of other players. Intuitively, without delay, public equilibrium requires that both players are punished for a "bad" signal in every period it is observed. If the signal is shown every two periods, punishment can be triggered (with some probability) by *two* instances of bad news. Longer delays allow performance to be reviewed more efficiently.

[1]Intuitively, public equilibrium means behavior only depends on public information. By imperfect monitoring, past behavior is not public information, so players cannot react to their own past actions.

[2]See, for instance, Compte (1998); Ely et al. (2005); Hörner and Olszewski (2006); Kandori and Obara (2006).

Communication, likewise, is a pervasive element of human interaction, and experiments have shown that it leads to improved welfare and coordination (a review of this literature can be found in Crawford, 1998). Although most of the theoretical literature on communication or "cheap talk" (Crawford and Sobel, 1982) emphasizes limitations to information sharing when incentives are misaligned, the literature concludes that some communication is often better than none at all. In the context of repeated games, however, subjects' payoffs are bounded away from efficiency if public equilibria are played, and this is true irrespective of the ability to communicate.[3]

We design an experiment around a prisoners' dilemma game played repeatedly with imperfect monitoring and frequent actions elapsing at a rate of 0.15 seconds per period. Because one of our main goals is to see whether players take advantage of delay, this environment is particularly appropriate. With our chosen parameters, the efficient level of welfare is 30 and welfare levels above 20 cannot be sustained in public equilibrium according to standard theory. In the treatments with delay, information arrives in 100 period blocks, making it possible to sustain welfare levels above 29. A game with a small number of periods would make the benefits of delay substantially less stark.

The experiment is described in detail in Section 3, but its basic features are the following. Subjects are randomly and anonymously matched into groups of two and earn points depending on the group's chosen action profile. Instead of observing the other player's actions, each subject observes a noisy public signal that has a positive drift if and only if both matched players cooperate. In the treatment without delay (treatment N), the public signal is shown in real time. In the treatment with delay (treatment D), the signal is shown in 100 period (15 second) windows. Two additional treatments allow subjects to their strategies with (treatment DC) and without (treatment NC) delay of information.

If actions cannot be changed every period (e.g., because of physical constraints on

[3]Some papers have explored communication as a useful tool for augmenting the set of equilibrium outcomes by allowing strategies to depend on the communicated information (e.g., Compte, 1998; Kandori and Matsushima, 1998; Obara, 2009). Kandori (2003) studies a repeated game with public monitoring and proves a folk theorem with communication in this environment. Although Kandori's result requires more than two players, one can ask if a version of his solution is applicable to our example. It is shown in Rahman (2013a), however, that a folk theorem in public communication equilibria requires that the drift when both players defect differs from the drift with unilateral defection, a condition which is violated by our monitoring technology. The public equilibrium bound on payoffs persists.

reaction times), then players observe several signals before making their decisions, and this bundling together of information makes greater levels of cooperation sustainable in equilibrium. For example, if it takes players five periods to respond, they can use a trigger strategy which starts off by cooperating and continues to do so as long as anything other than five bad signals is observed and defects with some probability if five bad signals are observed.[4] To test whether reaction lags affect behavior, our experiment includes a slow treatment (treatment S) that is identical to the baseline no delay, no communication case in all respects by two: a period lasts for a whole second, rather than 0.15 seconds, and the exchange rate between points and dollars is adjusted to equalize earnings per unit of time.

Our main results are the following:

Result 1. Delay of information hinders cooperation.

Result 2. Communication improves cooperation, allowing players to exceed the public equilibrium bound on payoffs.

Result 3. Giving players more time to think about their choices has no effect on behavior.

The finding that delay leads to a decrease in welfare cannot be explained by (public) ε-equilibria, where each player is a small distance away from playing a best response to the other player's strategy.[5] Friedman and Oprea (2012), the first paper to systematically examine behavior in a continuous time prisoners' dilemma (with perfect monitoring), provides a useful reference point for this observation. The paper finds median cooperation rates above 90% in continuous time and provides a theoretical model to explain this data, building on earlier work by Radner (1986) and Simon and Stinchcombe (1989). Focusing on cut-off strategies K(s) with conditional cooperation until time s and unconditional defection thereafter, the authors show that ε-equilibria are consistent with their experiment's results.

Our results provide a counterpoint to this conclusion. When imperfect monitoring is introduced in an otherwise similar environment, (public) ε-equilibria cannot explain

[4]Note that the effect of reaction lags is non-monotonic: If actions are held fixed for a sufficiently long period of time, *less* cooperation can be sustained in equilibrium. This complication may be ignored for our purposes.

[5]This is because the result that near efficiency can be sustained with delay is robust to small mistakes.

the observed behavioral regularities. The heart of Friedman and Oprea's argument was that frequent actions permitted players to punish deviations quickly, rendering them unprofitable. With imperfect monitoring, it takes time to recognize a deviation, and as a result reacting quickly loses its power. In theory, greater welfare is attainable with imperfect monitoring if subjects are *not* able to react quickly. In fact, we find no significant difference in cooperation rates between our slow (1 period per second) and fast (6 periods per second) treatment. Arguably, subjects could not react to information at the rate of 1/6th of a second, so via this form bounded rationality, their ability to react promptly to deviations was limited more in treatment N than in treatment S. Nevertheless, cooperation rates were not significantly different.

Thus, since Friedman and Oprea (2012) studied perfect monitoring, deviations could be detected precisely in their experiment. In our experiment, imperfect monitoring made it impossible for players to detect perfectly the behavior of their opponents–subjects needed repeated observations to make confident judgments regarding their opponents' behaviors.

We take the finding that delay of information leads to significant losses in welfare to be our paper's main contribution. It has been pointed out that the efficiency gains associated with delay in Abreu et al. (1991) may in practice be counteracted by the benefits of receiving frequent feedback (Levin, 2003). In the context of a laboratory experiment, we find that this is consistent with the evidence. Our results are in broad agreement with important findings in the industrial organization literature, which treats communication and information sharing as canonical ways of sustaining collusion (Feuerstein, 2005). In this line of research, there exist important examples of collusive institutions that choose *not* to delay noisy information. The Joint Executive Committee, a well-known railroad cartel which controlled much of railroad shipment in late nineteenth century United States, published weekly statistics that allowed cartel members to check on each other weekly (Ulen, 1980). Indeed, according to Porter (2005), "the cartel formation process [...] involves more than the issues studied in the repeated games literature. Dampening the short run incentives to cheat is only one facet of a cartels problems." We agree with this assessment and take our experimental results to point to the following basic fact: Contrary to standard theory, management of exogenous information can decrease welfare, while an institution that allows for additional information to be generated endogenously can lead to significant welfare benefits.

2 Related work

2.1 Experimental literature

There is a small but growing experimental literature on repeated games played with frequent actions. Friedman and Oprea (2012) showed that cooperation rates in a prisoners' dilemma are higher when the game is played in quasi-continuous time than when time is discrete. Bigoni et al. (2011) compared the effects of fixed and random termination times in the same setting, extending related experiments of Dal Bó (2005) conducted in discrete time. Oprea et al. (2011) used a continuous "hawk-dave" game in an experimental test of evolutionary game theory. We follow the basic methodology established in these studies: An action is assumed to be fixed until changed by the subject, while payoff stocks are updated every period, which in our case lasts 0.15 seconds.

While subjects observed their partner's choices in these studies, other experiments, in both discrete and continuous time, made use of imperfect monitoring. Aoyagi and Fréchette (2009) showed that welfare decreases in a repeated prisoners' dilemma as the public signal becomes more noisy. Ambrus and Greiner (2012) studied the relationship between welfare and the severity of a punishment technology in a public good game. Bigoni et al. (2012) found that action frequency has a nonlinear impact on collusion when payoffs are updated in a quasi-continuous manner and monitoring is noisy.

Our study is the first to implement imperfect monitoring and information delay in a theoretically structured manner. Cason and Khan (1999) delayed the announcement of other subjects' contributions in a public good game, interpreting information delay as an imperfect monitoring technology. As pointed out in Aoyagi and Fréchette (2009), such an interpretation of imperfect monitoring is at odds with the way the former is construed in theory. Moreover, information aggregation is irrelevant in a setting without noise.

Our experiment also manipulates subjects' ability to communicate. The experimental literature on communication is vast and dates back to at least Dawes et al. (1977). Studies in this line typically find that communication increases cooperation rates amongst experimental participants. This finding, however, comes with some qualifications. Charness (2000), for instance, found that "minimalist" communication protocols that allow players to announce their strategies are ineffective at improving cooperation rates in a one shot prisoners' dilemma. Ben-Ner et al. (2007) found that numerical messages are much less effective than verbal ones at encouraging trusting and trustworthy behavior in a trust game. Charness et al. (2012) employed a design manipulating the subjects' ability to communicate in a freeform manner and the rate at which periods elapsed, and found that communication had a much greater effect on contributions in continuous than in discrete time. This study relates to ours only loosely. First, it utilizes a setting with *perfect* monitoring. Second, the communication technology employed in our study is closer to the "minimalist" protocol of Charness (2000) or the numerical protocol of Ben-Ner et al. (2007) than the type of free-form communication employed in Charness et al. (2012).

2.2 Theoretical literature

The theoretical literature on repeated games with frequent actions is also small, recent and growing. With perfect monitoring, Simon and Stinchcombe (1989) developed an influential idea for sustaining cooperation in a prisoners' dilemma with finite horizon, assuming a form of bounded rationality. Specifically, they assume that players can only react to their observations with some fixed delay. As actions become arbitrarily frequent, for any fixed delay in reaction times they show that it is possible to sustain cooperation in a prisoners' dilemma—even if the horizon is fixed and finite.

Although Radner (1986) focused on the discrete time case, his results apply[6] just as much to games with frequent actions. He points out the discontinuity in the equilibrium payoff correspondence from an arbitrarily large but finite horizon to an infinite horizon, and then offers three different ways of restoring continuity. First, by introducing reputation, as in the famous "gang of four" papers (e.g., Kreps et al., 1982), cooperation becomes possible. Second, relaxing the behavioral predictions to ε-equilibria allows for some cooperation in equilibrium. Third, if players' strategies are subject to being "executed" by finite state automata with a fixed upper bound on their number of states, thencontinuity of the equilibrium payoff correspondence is again restored with respect to the horizon.

Friedman and Oprea (2012) use these interesting results to understand their experimental results in a theoretically structured manner. They emphasize a combination

[6] Radner (1986) attributes some of the findings in his paper to others; see his paper for references.

of ε-equilibrium and delayed reaction as a way of explaining the behavior of subjects in their experiment. However, none of the arguments mentioned above generalize immediately to games with imperfect monitoring, and no such extension exists in the literature. Such a generalization is an interesting open problem that we leave for future research. On the other hand, our results seem to rule out both ε-equilibrium and finite automata arguments as drivers for cooperation in the prisoners' dilemma with imperfect monitoring: In theory, delay ought to add value even in ε-equilibrium and regardless of the feasible complexity of a strategy.

The existing literature on repeated games with imperfect monitoring has a long history by now, perhaps most notably Radner et al. (1986), Abreu et al. (1986, 1990), Abreu et al. (1991), as well as Fudenberg et al. (1994). Relatively recently, Sannikov (2007); Sannikov and Skrzypacz (2007, 2010) and Fudenberg and Levine (2007, 2009) extended these techniques and results to games with both frequent actions and imperfect monitoring. A crucial assumption that is made in all of these papers is that players behave according to (perfect) public Nash equilibrium. This restriction on the set of equilibria facilitates formal analysis of sustainable payoffs, often with stark behavioral predictions. For instance, according to Sannikov and Skrzypacz (2007), collusion is impossible in a repeated Cournot oligopoly with flexible production, and the amount of cooperation that is sustainable in the prisoners' dilemma is severely limited in public Nash equilibrium. This is shown in Proposition 1 below.

There is substantial theoretical precedent for the question of how exogenous delay of information helps players sutain cooperation. Starting from Lehrer (1989) and Abreu et al. (1991), the idea that players can attain better social outcomes by delaying and lumping information into blocks has been widely accepted and applied in various contexts. Thus, "block" strategies have been used to sustain socially desirable outcomes in Kandori and Matsushima (1998), Compte (1998), Obara (2008), Ely et al. (2005), Sugaya (2010) and others. Although none of these "cooperative institutions" survive in repeated games with frequent actions, it can be shown that even with frequent actions delay can still help (Rahman, 2013b)—at least in theory.

3 Experimental design

All treatments were programmed using zTree (Fischbacher, 2007) and implemented with a between-group design following all standard practices of experimental economics. The experiment had five treatments, described in detail below. Upon signing their consent forms, subjects in every treatment obtained a paper copy of the instructions and were shown a pre-recorded PowerPoint presentation explaining their task. They then played a two player repeated prisoners' dilemma with imperfect monitoring and frequent actions through a computerized interface. Appendix B provides the instructions to the NC treatment.[7]

In all treatments other than treatment S, a time period lasted $\Delta t = 0.15$ seconds. At the beginning of each match, subjects chose between pressing an orange button ("cooperate") and a purple button ("defect"). After their initial choice, they could change their selection at any time and as often as they wanted. Not pressing any buttons during a time period amounted to maintaining their last recorded choice. Following one practice match, subjects were randomly and anonymously matched several times. In every period, the probability of a match terminating was 1/700. Following Murnighan and Roth (1983), we identify the continuation probability with the discount factor. The first match to end after 45 minutes elapsed since the beginning of the experiment marked the end of a session. If subject were in mid-match at 50 minutes after the experiment began, we overrode the random termination rule and terminated the match at a randomly chosen period within the current 100-period block. Payment consisted of the final payoff from a randomly selected match, converted from points to dollars at the exchange raint of 40 (treatments N and D) or 20 (treatments NC and DC) points per cent.

Depending on whether she chose to cooperate (C_{it}) or defect (D_{it}), subject i's stock of points increased by u_{it} in period t according to the following table:

	C	D
C	15, 15	0, 20
D	20, 0	2, 2

Subjects did not find out their earnings until the end of the session, at which point they received their accumulated earnings $\sum u_{it}(a_t)$ from every match. Instead of observing her partner's actions, each subject was shown a public signal that could

[7] The instructions to treatments with delay differ in that the sentence "The process will be displayed in real time, in blocks of 100 periods" in the "Information" section is replaced by "You will only observe the evolution of this process at the end of each block of 100 periods." The instructions to treatments N and D omit the "Communication" section.

go up with probability $p(a_t)$ or down with probability $1-p(a_t)$, with $p(a_t)$ determined as in the table below:

	C	D
C	$\frac{3}{4}$	$\frac{1}{2}$
D	$\frac{1}{2}$	$\frac{1}{2}$

Conditional probability p of the good signal

In treatment N, information arrived continuously, and in treatment D it arrived at the end of each 100 period (15 second) block, when it was revealed in five three second-long lumps. In both treatments, points were converted to dollars at an exchange rate of 40 points per cent. In treatment S, information also arrived continuously, but a time period lasted for $\Delta t = 1$ second. The continuation probability (discount factor) was identical to that in the other treatments, but points were converted to dollars at an exchange rate of 6 points per cent. This ensured that earnings per unit of time were the same.[8] The slow treatment also had 150 (unpaid) periods in the practice match, compared to 250 in the other treatments. This ensured that the practice match does not go on for an unnecessarily long length of time, but that the players still get an opportunity to experience a match with more than one 100-period block.

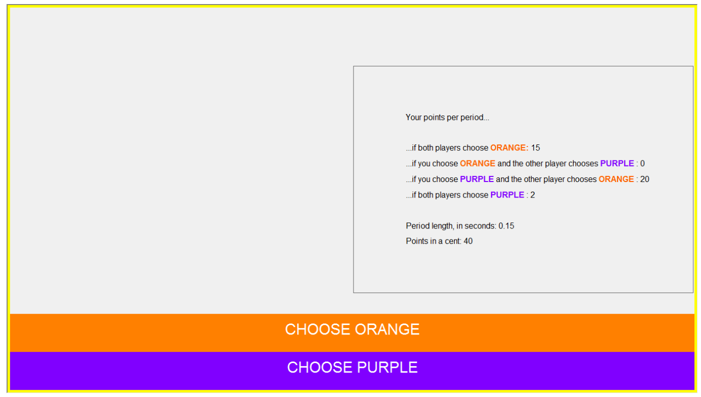
Treatments NC and DC allowed subjects to communicate cut-off strategies in an environment without (NC) and with (DC) delay of the public signal. Because communication made each match longer, we introduced a more generous exchange rate in these treatments to help smooth out earnings across sessions: 20, rather than 40 points, were converted into a cent. In all treatments of the experiment, subjects pressed a "continue" button every 100 periods–at the end of each 15 second block. This block structure was introduced to minimize the difference between the differences between ways in which information is presented in treatments with and without delay. Note that it leaves our theoretical predictions unaltered.

The communication in these treatments took place at the beginning of each match, before subjects took their initial actions, and at the end of each 100 period block. At the beginning of the match, subjects provided answers to the following questions:

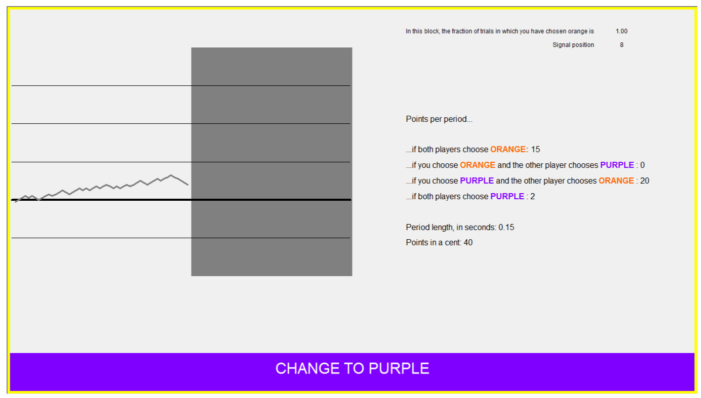
- This block, I will choose ORANGE this percentage of the time: ___%

[8](1/40) cents/point x 15 points/period x (1/.15) periods/second = 2.5 cents/second. To make the slow world the same, change the exchange rate to ((1/40) x (1/.15)) cents/point x 15 points/period x 1 period/second = 2.5 cents/second.

Beginning of match:



Mid-block:



End of block:

Figure 1: **Screenshots of the first block in treatment N (no delay, no communication).** In the beginning of each match, a subject selects her initial action (top). She then plays a prisoners' dilemma game with imperfect monitoring, in which a common signal of the chosen action profile is observed (middle, bottom). The player is allowed to change her action as often as she desires in the course of the game (middle). Every 15 seconds, the player presses a "continue" button (bottom), which erases past signals from the display.

- If this block's signal position is [above/below] the number ___, I will respond by choosing ORANGE this percentage of the time in the following block: ___%. Otherwise, I will choose ORANGE this percentage of the time: ___%

After everyone submitted their answers and initial actions, subjects saw their partner's answers displayed on the screen for 30 seconds. When this screen timed out, the game began to elapse. At the end of each 100 period block, subjects were given an opportunity to revise their answers. After everyone's new answers were submitted, each subject looked their partner's new answers for 15 seconds before the next block started.

4 Theoretical predictions

From the point of view of the theory, the modeling choice of imperfect monitoring with frequent actions is useful for three reasons. First, it is consistent with many real-world applications. Second, it disciplines the design of institutions considerably by forcing them to be robust to the friction of a fixed period length by discouraging infinitesimal deviations.[9] Third, it delivers a mathematically tractable analysis of the problem. We now describe the theoretical considerations relevant to our study.

4.1 Public equilibrium payoffs

The study of public equilibria is practically the norm in repeated games, especially those with frequent actions. As such, it is important to understand the restriction that such equilibria impose on equilibrium payoffs. Fortunately, their recursive nature delivers a simple, partial identification for public equilibria: The maximal payoff under public equilibria is given by 20 points per period. This claim is proved in the following proposition, for which Figure 2 provides a graphical illustration.

Proposition 1. *Let $\gamma(\delta) = \max\{v_1 + v_2 : v \in E(\delta)\}$, where $E(\delta)$ is the set of public equilibrium payoff vectors of the game with discount factor $\delta < 1$. Then, $\gamma(\delta) \leq 20$ for every δ. This bound continues to hold in public communication equilibrium.*

[9] This rules out most of the institutions in the game theory literature, including Abreu et al. (1991) to Compte (1998), Kandori and Matsushima (1998), Ely et al. (2005), Hörner and Olszewski (2006), Sugaya (2010) and beyond.

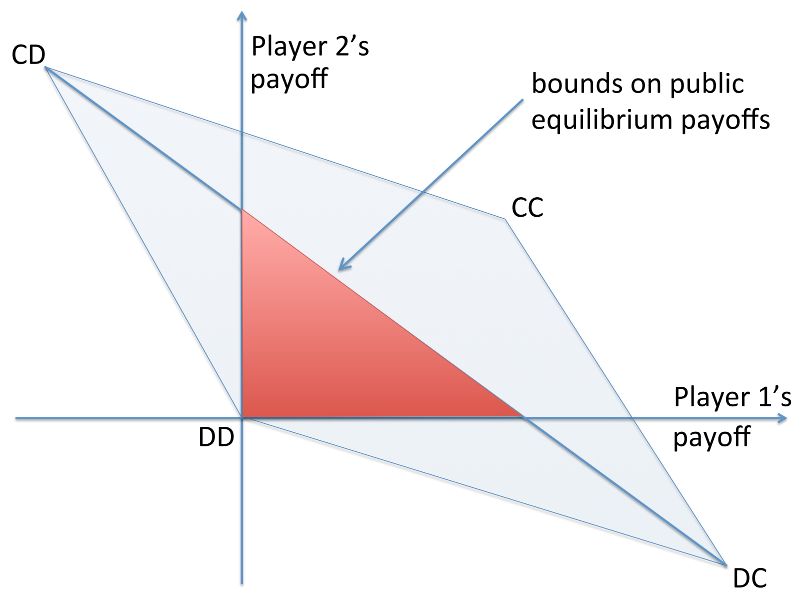


Figure 2: Flow payoffs in the prisoners' dilemma with two players

Note that Proposition 1 delivers the same theoretical bound for both public Nash equilibrium and public communication equilibrium. This is due to the particular choice of information structure in our experiment, where the probability of good news is the same if there is only one cooperator or none at all. Therefore, the fact that our treatment with communication exceeded the welfare bound of 20 is not consistent with public communication equilibrium.

4.2 How information delay can help

The insight that lumping information together may improve incentives is not new, dating back at least to Lehrer (1989).[10] For our purposes, the construction due to Abreu et al. (1991) is a particularly useful way of describing it.

Suppose that, instead of the signal arriving every period, it was possible to lump the information in such a way that the signal only arrived at the end of every T-period block. Abreu et al. (1991) show how players can improve upon a welfare of 20 by delaying information this way. Consider the following strongly symmetric strategies, to be called *AMP block strategies*. Every player cooperates for T periods.

[10] Lehrer (1989) studied repeated games without discounting, though.

At the end of the T-period block, the T public signals for each period in the block arrive to the players. If every signal was bad then continuation play consists of mutual defection henceforth with some probability α. Otherwise, they continue to cooperate for the next block with the same contingency.

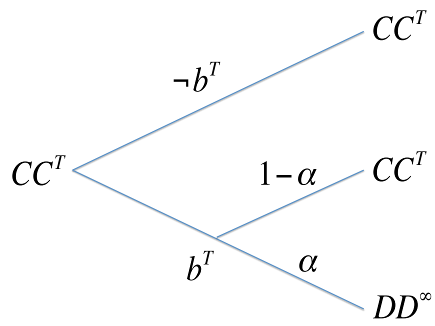


Figure 3: AMP block strategies ($b^T = T$ bad signals)

The probability of T consecutive bad signals equals q_2^T in equilibrium, that is, assuming mutual cooperation throughout the block. A player's lifetime utility under this strategy profile is therefore given by

$$v = (1-\delta^T)15 + \delta^T[(1-q_2^T)v + q_2^T((1-\alpha)v + 2\alpha)].$$

Rearranging,

$$v = 15 - \frac{\delta^T}{1-\delta^T} q_2^T \alpha(v-2). \tag{4.1}$$

Discouraging a deviation in the very first period of the block requires that the utility gained from defecting, $(1-\delta)5$, be outweighed by the associated loss in continuation payoff. This is given by the change in probability of punishment from the one-period deviation, $q_2^{T-1}(q_1 - q_2)$, times the opportunity cost of punishment, $\delta^T \alpha(v-2)$. Since $q_1 - q_2 = .25 = q_2$, this incentive constraint may be written as

$$(1-\delta)5 \leq \delta^T q_2^T \alpha(v-2). \tag{4.2}$$

A key insight behind the welfare properties of AMP block strategies is that discouraging one deviation discourages all others, as the next result shows. The intuition for it is this. The gains from deviating grow linearly, whereas the costs grow exponentially in the number of deviations. Therefore discouraging one deviation discourages them all.

Lemma 1. *If the AMP block strategies above discourage a deviation in any single period of a block then they discourage every deviation, that is, they constitute an equilibrium.*

Consider maximizing v, the strongly symmetric equilibrium payoffs above, with respect to α such that the AMP block strategies above remain an equilibrium. At an optimum, the incentive constraint (4.2) must bind, since otherwise by (4.1) we would be able to feasibly lower α further and increase v, contradicting optimality. If (4.2) binds then the maximum value of v equals

$$v = 15 - 5\frac{1-\delta}{1-\delta^T}.$$

On the other hand, feasibility requires that $\alpha \leq 1$, since it is a probability. Substituting for v and this inequality in (4.2) and rearranging gives

$$5(1-\delta)\left[\frac{1}{1-\delta^T} + \frac{1}{(\delta q_2)^T}\right] \leq 13. \tag{4.3}$$

This inequality places a restriction on the exogenous parameters of the game for the strategy profile above to be an equilibrium. Abreu et al. (1991) used a version of this bound to argue a result along the following lines.

Proposition 2. *For every block length $T \in \mathbb{N}$, there exists $\delta < 1$ sufficiently large that the strategies above constitute an equilibrium. Moreover,*

$$\lim_{T\to\infty}\lim_{\delta\to 1} v = 15.$$

Note that $15 + 15 > 20$. This shows that the public equilibrium bound on payoffs can be overcome with delay.

4.3 Delay with practical cut-offs

AMP equilibria suffer from the following basic problem: as T grows, the demands placed on δ for an equilibrium become unreasonable. For instance, if $T = 20$ (or 3 seconds) then (4.3) requires δ to be so unreasonably close to 1 that interpreting it as the probability of termination would imply an expected duration of a match to be more than 1,000 years, using the parameters from our experiment.

Essentially, the construction by Abreu et al. (1991) is too lenient over when players are punished. It takes T bad signal realizations to trigger punishment. A less lenient, but more practical, approach to making use of delay is the following one taken from Rahman (2013b). If T is large then the distribution of signals is close to normally distributed by the Central Limit Theorem, and the likelihood that in equilibrium there will be more than $\frac{1}{2}T$ bad signals is relatively low, since the expected number of bad signals is $\frac{1}{4}T$.[11] Therefore, punishment actually occurs relatively infrequently in equilibrium. Nevertheless, it is still the case that discouraging a single deviation discourages them all, which helps to establish a Folk Theorem.

Specifically, consider the following strategies, called *practical cut-off strategies*. As with AMP block strategies in Section 4.2, players cooperate over a T-period block with information delay. If the number of bad signals at the end of the block is greater than $\frac{1}{2}T$, then players choose mutual defection henceforth with some probability α. Otherwise, they continue to cooperate in the next block, with the same contingent plan. The only difference between AMP block strategies and practical cut-off strategies is in the number of bad signals that trigger punishment. In the former, this number is T, whereas in the latter it is $\frac{1}{2}T$. Nevertheless, Rahman (2013b) shows that these cut-offs lead to efficient outcomes with reasonable discount rates, in contrast with the previous objection to AMP block strategies. This shows that—at least in theory—it is possible for players to substantially improve on the public equilibrium benchmark of Proposition 1.

4.4 How bounded rationality can help

As noted in the introduction, bounded rationality can also help players overcome the bound on public equilibrium payoffs. Let $\tau = 2$ denote the number of periods that a player is unable to change her action, and consider the following strongly symmetric strategies. Every player cooperates for τ periods. At the end of the τ-period block, the players consider cooperating if anything other than τ bad signals is observed. If τ bad signals are observed, the players switch to defection with some probability α.

The probability of 2 consecutive bad signals equals q_2^2 in equilibrium. A player's

[11] This cut-off is lower than might be expected from a version of the mechanism proposed by Kandori and Matsushima (1998) in discrete time, closer to $\frac{1}{4}T$, which would not approximate full cooperation due to a non-vanishing punishment probability.

lifetime utility under this strategy profile is therefore given by

$$v = (1-\delta^2)15 + \delta^2[(1-q_2^2)v + q_2^2((1-\alpha)v + 2\alpha)].$$

Rearranging,

$$v = 15 - \frac{\delta^2}{1-\delta^2} q_2^2 \alpha (v-2). \tag{4.4}$$

Discouraging a deviation requires that the utility gained from defecting, $(1-\delta^2)5$, be outweighed by the associated loss in continuation payoff. Thus, the incentive constraint is

$$(1-\delta^2)5 \leq q_2^2 \alpha (q_1^2 - q_2^2)(v-2). \tag{4.5}$$

Consider maximizing v with respect to α such that the trigger strategies above remain an equilibrium. At an optimum, the incentive constraint (4.5) must bind, since otherwise by (4.4) we would be able to feasibly lower α further and increase v, contradicting optimality. If (4.5) binds then the maximum value of v equals

$$v = 15 - \frac{5}{\left(\frac{q_1}{q_2}\right)^2 - 1} = 15 - 5/3 \approx 13.33 > 10.$$

It is easy to check that the feasibility constraint, that is, $0 \leq \alpha \leq 1$, is satisfied given the parameters of the experiment.

5 Results

Data was collected from 248 University of Minnesota undergraduate students at the Anderson Hall Social and Behavioral Sciences Laboratory. Table 1 reports select summary information.[12] To estimate the effect of our treatments on cooperation, we regressed each subject's average cooperation rate in non-practice matches[13] on three dummies: Delay (=1 for treatments with delay), Communication (=1 for treatments with communication) and Slow (=1 for treatment S). The regression was performed

[12]Notice that treatments N and D had more matches than treatments NC and DC. This is because communication took up a significant portion of time in the latter treatments, as discussed in Section 3.

[13]The analysis here is restricted to non-practice matches. We discuss behavior in practice matches and the dynamics of cooperation below.

with session-clustered standard errors. The results, presented in the left column of Table 2, show that delay of information led to lower and communication to higher cooperation rates.

		Delay Δt=0.15 sec.	*Delay* c=15 sec.
Communication	No	Treatment N 6 sessions $N = 64$ 287 matches	Treatment D 3 sessions $N = 46$ 169 matches
Communication	Yes	Treatment NC 3 sessions $N = 44$ 109 matches	Treatment DC 3 sessions $N = 52$ 83 matches

No delay *No communication* $\Delta t = 1$ sec.
Treatment S 4 sessions N=42 49 matches

Table 1: Summary statistics of the experimental treatments

While other studies found that communication improves cooperation in games, our result that delay hinders cooperation is entirely new; it is therefore important to verify its replicability. We found a similar result in a pilot experiment that we conducted in the summer of 2013 with a sample size of 66 subjects, where under different parameters delay has a significantly negative effect on welfare ($P < 0.05$ with session-clustered errors). This experiment also had frequent actions and imperfect monitoring, but subjects observed their own stocks of payoffs, rather than the noisy signal. In one treatment, the payoff stock was observed in real time, and the second treatment, it was observed with delay. The payoff stock, however, depended on one's own chosen action, the chosen action of the opponent, and noise. We highlight our first result below:

Result 1. Delay of information hinders cooperation.

The effects of the treatment variables on subjects' average attained payoffs are shown in the right column of Table 2. It is apparent the effects on welfare exactly parallel the effects on cooperation rates described above. The average payoff in treatments N and DC does not differ significantly from 10 (P-values of 0.742 and 0.937, respectively); the average payoff in treatment D is significantly below 10 ($P < 0.01$), and the average payoff in treatment NC is significantly above 10 ($P < 0.01$). This latter result suggests off-equilibrium behavior, private strategies, or equilibrium behavior with bounded rationality in treatment NC. We highlight this result below:

	Cooperation	Average payoff
Delay	-0.102***	-1.251***
	(0.0359)	(0.440)
Communication	0.111***	1.405***
	(0.0364)	(0.444)
Slow	-0.0264	-0.315
	(0.0392)	(0.463)
Constant	0.558****	9.882****
	(0.0288)	(0.353)
Observations	248	248

Session-clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

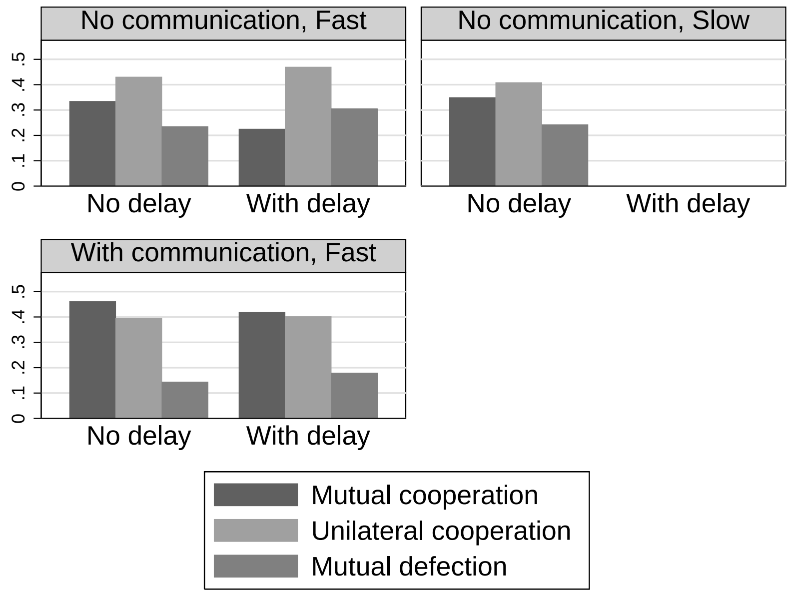
Table 2: Treatment effects on subjects' average cooperation rates and payoffs.

Result 2. Communication improves cooperation, allowing players to exceed the public equilibrium bound on payoffs.

Subjects' inability to change their actions in every period of the treatments with frequent actions raises the question of whether this sort of bounded rationality has an effect on behavior. That the subjects exceed the bound on public Nash equilibrium payoffs in treatment NC makes this question all the more salient. We find, however, that slowing down the experiment has no significant effect on subjects' cooperation rates or payoffs (Table 2). In magnitude, the effect of the Slow dummy on individual cooperation rates is less than 3%, and the attained payoffs levels are virtually the same. This is our third significant result:

Result 3. Giving players more time to think about their choices has no effect on behavior.

We also explored the effect of our treatment variables on *mutual* cooperation rates. To this end, we created three variables for each of the 697 matches in the dataset: percentage of time spent in action profile (C,C), percentage of time spent in action profile (C,D) or (D,C), and percentage of time spent in action profile (D,D,). These profiles of cooperation rates are plotted for different treatments in Figure 4. In the



	(C,C)	(C,D) or (D,C)	(D,D)
Delay	-0.0889**	0.0293	0.0596*
	(0.0419)	(0.0271)	(0.0329)
Communication	0.154***	-0.0488	-0.105***
	(0.0491)	(0.0309)	(0.0280)
Slow	0.0224	-0.0257	0.00331
	(0.0527)	(0.0323)	(0.0242)
Constant	0.327****	0.434****	0.239****
	(0.0378)	(0.0210)	(0.0203)
Observations	697	697	697

Session-clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Figure 4: Treatment effects on subjects' mutual cooperation rates.

bottom part of the figure, we report the results of regressions in which these variables are regressed against the treatment dummies. Delay made mutual cooperation less likely ($P < 0.05$) and mutual defection more likely ($P < 0.1$). Communication had the reverse effect, increasing mutual cooperation ($P < 0.01$) and decreasing mutual defection ($P < 0.01$). Interestingly, neither of these variables had an effect on unilateral cooperation rates. In the slow treatment, the profile of cooperation did not significantly differ from that in the baseline treatment (N), providing additional evidence for Result 3 above.

5.1 Dynamics

To study the dynamics of cooperation in the experiment, we changed the unit of analysis to average cooperation rate per block and re-ran the regression reported in Table 2 for all blocks and matches, including practice matches. We included a practice dummy, a block number variable (1 for the first 100 periods, 2 for the next 100 periods, etc.), and a match number variable. The results of this regression and an analogous regression in which the dependent variable is the subject's average payoff per block are reported in the left-most two columns of Table 3. Subjects cooperated significantly more often in the practice matches ($P < 0.05$), but the match and block dummies failed to reach significance. The average per-block cooperation rate in practice matches was approximately 65%, while the average in non-practice matches was approximately 55%.

We also re-ran the regressions reported in Table 2 for the practice matches. The results of these regressions are shown in the right-most two columns of Table 3. Unlike Table 2, Table 3 shows none of the treatment dummies as significant. I.e., subjects sustained comparably high cooperation rates and welfare levels in every treatment of the practice matches. Even in the slow treatment, where average payoffs above 10 cannot be sustained in public equilibrium with or without response time constraints,[14] we find that the average attained welfare level is significantly higher than 10 (mean=10.87, $P < 0.05$, session-robust standard errors). We summarize the findings described above as follows:

Result 4. There are significant differences in behavior between

[14] In this treatment, periods proceed at a slow enough rate that subjects can physically respond in every period.

	All data		Practice data	
	Cooperation (per block)	Average payoff (per block)	Cooperation (per subject)	Average payoff (per subject)
Delay	-0.0861**	-2.092**	-0.0373	-0.409
	(0.0312)	(0.777)	(0.0538)	(0.651)
Communication	0.0936***	2.302***	0.0590	0.583
	(0.0323)	(0.785)	(0.0542)	(0.655)
Slow	-0.0409	-1.021	-0.00979	-0.203
	(0.0346)	(0.815)	(0.0353)	(0.443)
Practice	0.0634**	1.554**		
	(0.0260)	(0.661)		
Match	-0.00548	-0.140		
	(0.00352)	(0.0866)		
Block	-0.000817	-0.0267		
	(0.00118)	(0.0291)		
Constant	0.595****	20.76****	0.648****	11.08****
	(0.0300)	(0.743)	(0.0315)	(0.386)
Observations	9674	4837	248	248

Session-clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 3: Dynamics of cooperation in the experiment. There was significantly more cooperation in practice than in paid matches, but little learning once the paid matches start (left column). In the practice matches, none of the experimental treatments affected cooperation rates (right column).

practice and paid matches (and evidence of off-equilibrium behavior in practice matches), but little learning once the paid matches start.

To explore the dynamics of cooperation further, we examined whether what happens in the very first period of the match explains what happens in the rest of the match. To this end, we controlled for the subjects' chosen profile of cooperation in the regressions reported in Figure 4 by introducing two dummy variables: a mutual cooperation dummy (=1 if the players chose (C,C) in the first period of the match) and a unilateral cooperation dummy (=1 if the players chosen (C,D) or (D,C) in the first period of the match). The results are shown in Table 4. Both of the new variables have highly significant coefficients. Moreover, the effect of delay loses significance both when mutual cooperation in the match ($P = 0.202$) and mutual defection in the match ($P = 0.595$) are dependent variables. The effect of communication remains significant but falls in magnitude, from 15.4% to to 9.13% when the percentage of match spent in mutual cooperation is the dependent variable, and from -10.5% to -6.53% when the dependent variable is mutual defection. Thus, first period profiles of cooperation largely explain the effects of the treatment variables. We highlight this result below:

Result 5. The effect of delay (communication) on cooperation in the course on the match is largely (partially) explained by differences in first period profiles of cooperation.

5.2 Strategies

We next looked at how subjects' behavior depended on the realized public signals. We averaged actions taken in each period (1-100) across all sessions, matches and blocks, taking into account the evolution of the public signal in the previous block. The average cooperation rates following blocks with different numbers of good news are plotted in Figure 5. This figure makes apparent a number of behavioral regularities.

First, it suggests that subjects cooperate more in blocks that follow blocks with high realizations of the public signal, and that this is true for treatments with and without delay. The figure obscures the effect of the public signal, however, because past realizations of the signal are related to past cooperation rates. We therefore ran regressions in which a subject's cooperation rate in block b was regressed against the public signal at the end of block $b-1$, using the partner's cooperation rate in block

	(C,C)	(C,D) or (D,C)	(D,D)
Delay	-0.0435	0.0281	0.0154
	(0.0330)	(0.0226)	(0.0286)
Communication	0.0913**	-0.0260	-0.0653***
	(0.0336)	(0.0244)	(0.0213)
Slow	0.0325	-0.0381	0.00564
	(0.0800)	(0.0486)	(0.0355)
(C,C) in first period	0.414****	0.0396	-0.453****
	(0.0380)	(0.0400)	(0.0513)
(C,D) or (D,C) in first period	0.0761***	0.255****	-0.331****
	(0.0218)	(0.0393)	(0.0486)
Constant	0.124***	0.297****	0.579****
	(0.0377)	(0.0356)	(0.0488)
Observations	697	697	697

Session-clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 4: The action profile chosen by the players in the beginning of the match had a significant effect on the average action profile in the match. Note that the effect of the delay variable loses significance when players' first period choices are controlled for.

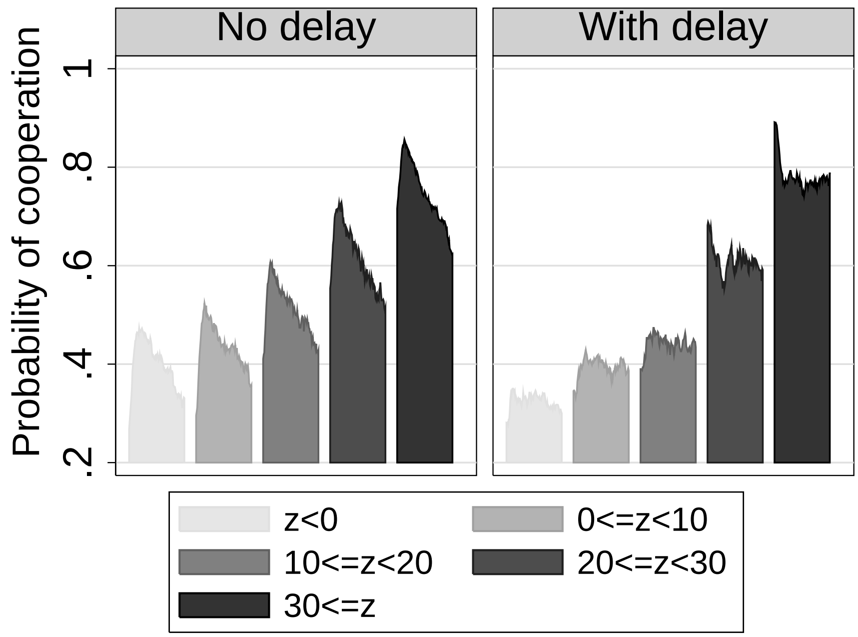


Figure 5: Behavior and the public signal. Each bar in plots average cooperation in rates in periods 1-100 of the block, with period number on the horizontal axis. Colors code the variable z, defined as the number of good news less the number of bad news received in the previous block.

$b-1$ as in instrument. Specifically, the model we estimated was

$$a_{ib} = \beta z_{i,b-1} + \alpha_i + \epsilon_{ib},$$

where a_{ib} is subject i's average cooperation rate in block b and $z_{i,b-1}$ is the number of good news minus the number of bad news observed in the previous block $b-1$. Because ϵ_{ib} may be related to $a_{i,b-1}$, which is strongly correlated with $z_{i,b-1}$, there is a potential endogeneity issue. To address it, we used $a_{-i,b-1}$, the cooperation rate of i's partner in block $b-1$, as an instrument for $z_{i,b-1}$. Because i never observes her partner's actions except through the public signal, the instrument is valid.

We ran these regressions separately for every treatment, including subject fixed effects as covariates, and clustering the standard errors by session. The results are shown in Table 5. Once past cooperation rates are controlled for, we find that past realizations of the signal have highly significant positive effects on behavior ($P < 0.001$) in every treatment without communication, and in the treatment with communication and no delay. The positive and highly significant relationship between signals and behavior constitutes our sixth major finding:

Treatment	N	D	S
Prev. block signals	0.00477****	0.00530****	0.00154****
	(0.000631)	(0.000590)	(0.000388)
Observations	2994	1776	362

Treatment	NC	DC	
Prev. block signals	0.00316****	0.00114	
	(0.000756)	(0.00249)	
Observations	676	810	

Session-clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 5: Behavior and signals received in the previous 100-period block. The partner's cooperation rate in the previous block is used as an instrument for previous block's final signal position. The first stage F-statistics in these regressions range from 151.346 to 2389.576.

Result 6. Subjects provide each other with incentives by cooperating more after receiving good news.

Note that the coefficient on the signal variable is almost two times smaller in magnitude in the treatments with communication. This suggests that when the subjects are able to communicate, they rely on old signals less, and that the messages have informational content. To confirm this, we looked at correlations between what the subjects communicated and how they behaved. For the 96 subjects who participated in the treatments with communication, the average promised cooperation rate in the non-practice matches was approximately 63% (promises averaged across blocks for each subject). The actual cooperation rate, in comparison, was approximately 69%. The correlation between promised and actual cooperation rates is strong and highly significant (correlation coefficient of .6214, $P < 0.001$). We interpret this as strong evidence that the messages in our data relate to behavior:

Result 7. The messages have informational content.

5.3 Periodicity of behavior

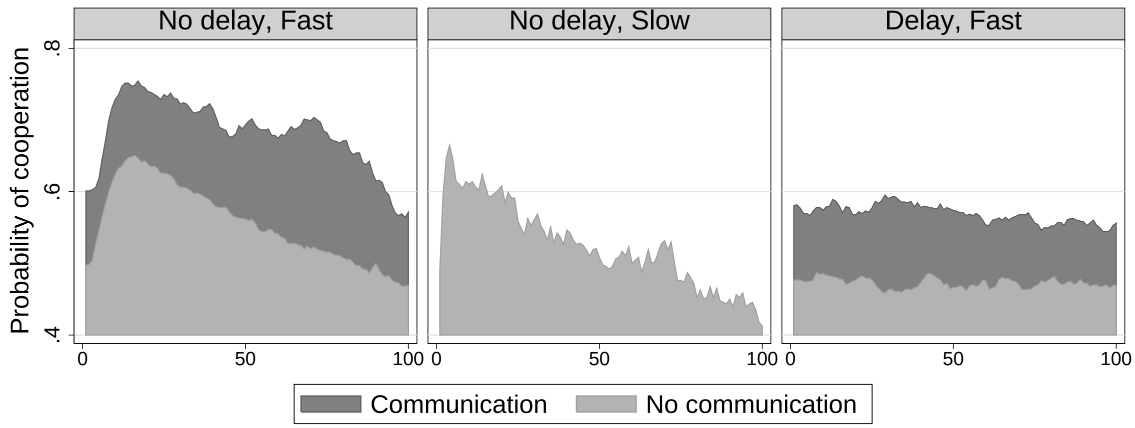
Figure 5 also suggests that behavior in treatments *without delay* has a highly periodic nature: regardless of the number of good signals observed in the previous block, subjects in these treatments are more likely to defect in the late periods of each block than they are in the early ones. This can also be seen in Figure 6, which aggregates the data for different past realizations of the public signal and plots the per-period cooperation rates for different treatments. Cooperation in these treatments is periodic with and without communication (top panel), with no clear time trend across blocks (bottom panel). Remarkably, cooperation rates in the slow treatment increase at about the same rate (per period) as they do in the fast treatments without delay, and then follow a similar gradual decline.

Result 8. Blocks in treatments without delay exhibit a striking periodicity in behavior.

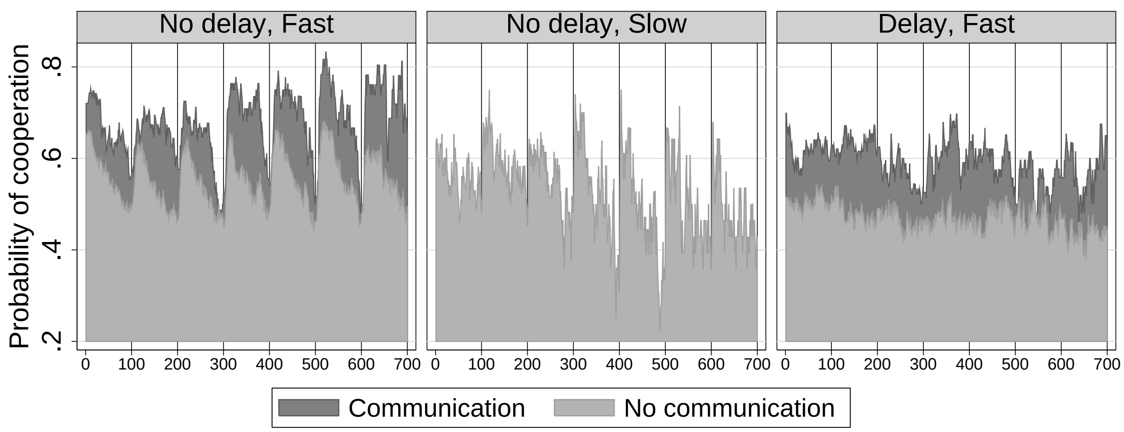
Note that the slope of the within-block trend is different in treatments with and without communication. Without communication, cooperation declines at a constant slope. With communication, it stays at a high level for a longer period of time, and then declines steeply. This observation sheds light on some of the dynamics of cooperation described above. Recall that the first period cooperation profile can explain most of the treatment effect of delay, but that the effect of communication remains significant even when the first period cooperation profile is controlled for. That cooperation declines faster without communication provides one avenue for a within block (and hence within-match) effect of the communication treatment.

One interpretation of this observed periodicity in behavior is that it reflects a focal point introduced by our design, which divides the match into 100-period blocks. This focal point may influence subjects to coordinate on mutual cooperation around the beginning of each block. Intuitively, a break (like a postman knocking on the door of a couple that is fighting) distracts the subjects, thereby restoring initial levels of cooperation. However, because behavior is not periodic in treatments with delay, this explanation is problematic.

An arguably more plausible interpretation is that players observe signals and choose their level of cooperation on the basis of the progress of the signal. For instance, suppose that in equilibrium the drift of the signal is fixed at some level while players are in a cooperative phase. As soon as the Brownian motion passes through a



(a) The average block



(b) First seven blocks of the average match

Figure 6: Periodicity of behavior. The top panels plot data averaged across all blocks and matches. In treatments without delay, cooperation rates start out high, decline over time, and refresh at the beginning of the next 100 period block. The decline in cooperation is faster for treatments without communication. No periodicity of behavior is observed in the treatments with delay. Behavior in the slow treatment follows the same pattern as behavior in the treatment with frequent actions and no communication.

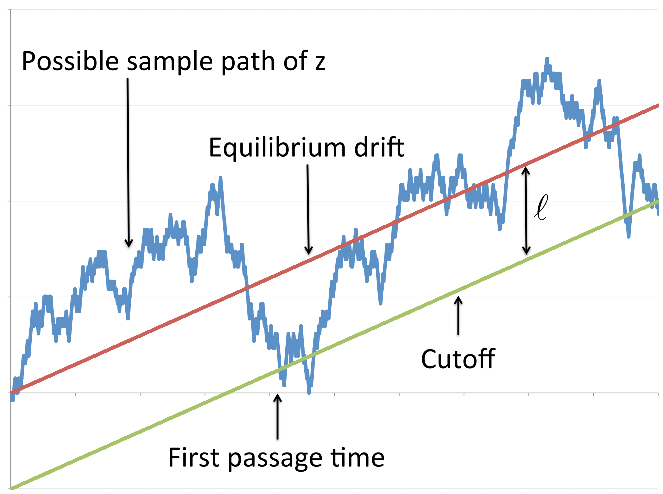


Figure 7: Illustration of strategies with first passage times determining switches in cooperation rates. The parameter ℓ describes the amount of leeway a player gives to the public signal before switching from a cooperative regime to a more defective regime.

window of some length ℓ from this drift, players move to a punishment phase for the rest of the block, and then restart their strategy in the next block, perhaps updating ℓ on the basis of the length of time it took for the Brownian motion to escape the previous cutoff in the last period. See Figure 7. Assuming a common threshold ℓ across matches and subjects, the density f of first passage times would take the form

$$f(t) = \frac{\ell}{\sqrt{2\pi t^3}} \exp\left(-\frac{\ell^2}{2t}\right).$$

Under this model, the rate of cooperation decreases gradually over time, in line with the experimental results of Figure 6(a) without delay. (The initial increase in cooperation rates in the fast treatments is likely due to subjects reacting relatively slowly to adjusting their action from the end of the previous block.) Of course, a richer model of passage times and cutoff strategies can be used to fit the data precisely by fitting an appropriate inverse Gaussian distribution to average cooperation rates and allowing for variation in thresholds ℓ.

6 Discussion

Our experiment provides the first systematic treatment of delay, communication, and reaction lags in a repeated game with frequent actions and imperfect monitoring. The results draw attention to several discrepancies with standard theory. The first finding is that delay unambiguously hurt subjects. This is at odds with the Abreu et al. (1991) argument that information delay can substantially help players to reduce the likelihood of inefficient punishments. The argument is robust—it holds in games with frequent and infrequent actions, with unbounded patience, even in approximate equilibrium, which makes the empirical finding all the more puzzling. One explanation of it is that delay makes it more difficult for subjects to learn the kind of opponent they are facing. Intuitively, if subjects face too much uncertainty over their opponents' planned behavior, then it may be difficult to justify cooperating with them. The absence of delay may help players to signal their intended behavior more effectively.

Our second finding is that communication unambiguously helps subjects. Although it is well known in the experimental literature that communication generally improves welfare in a wide variety of strategic contexts, there is no strong theoretical justification for it adding (or subtracting) value in our experiment, unless—again—it helps to reduce subjects' uncertainty over their opponents' intentions. This suggests several sources of possible gains from reducing uncertainty over opponents' strategies. First, it may motivate a subject to cooperate more if she is more confident that her opponent is likely to cooperate in return. Second, it may be easier to give incentives for cooperation to opponents if they can be made better aware of the consequences of their defection. In treatments with communication, where subjects announce their contingent plans at the beginning of every block, both of these channels should be facilitated—at least somewhat–and we find this to be the case. Subjects use messages to inform their opponents of future strategies, and reported and actual behaviors are significantly aligned.

We also find little evidence that reaction lags affect behavior in our experiment. This is a notable observation in light of the experiments reported in Friedman and Oprea (2012), where cooperation rates increase monotonically as players are given the opportunity to respond more quickly. Friedman and Oprea (2012) argued that response lags prevent players from quickly punishing deviations and that the gains to shorter lags are monotonic. Crucially, this argument only makes sense with *perfect* monitoring. In an environment like ours where information is noisy, reaction lags may allow players to gain a better idea of whether their partners are being cooperative, thereby supporting more efficient equilibria. We find, however, that cooperation does not seem to be affected in practice by how much time a player has to respond to a signal.

Our instrumental variable-based estimation shows that subjects behavior is driven by observations of the public signal. Using an opponent's strategic behavior as an instrument is justified by the assumption that subjects play mixed strategies. As a result, conditional on the public signal, random changes in a subject's behavior must be mutually independent. Since the coefficient on the public signal is estimated with an instrument, it is unbiased regardless of other omitted variables. If there are no omitted variables, then the regression equation is consistent with a first-order approximation of public strategies. Of course, if we assume that subjects' behavior is consistent with equilibrium, then players must be playing public equilibria. From a practical point of view, public equilibrium makes testable restrictions on feasible outcomes, such as the welfare bound of Proposition 1. However, our treatment with communication implies that the public equilibrium bound is violated. This could be for several reasons. First, it could be that subjects simply do not play equilibrium strategies. However, if the equilibrium assumption is dropped then it is not clear what structural predictions can be made. Secondly, it could be that their bounded rationality means that they are incapable of playing public equilibria, as they cannot react immediately to an individual bad news event. This may improve welfare, as illustrated in Section 4.4. However, it is not clear why players would exploit this bounded rationality in treatments with communication in order to exceed the public equilibrium bound but not in those without. Thirdly, subjects may be playing private—not public—strategies. From a technical point of view, public equilibrium is an assumption that often puts severe restrictions on behavior. That is, it precludes players from certain behavior that may be intuitively justified in some contexts. Rahman (2012, 2013a) explores this issue at some length and argues that public equilibria preclude secret monitoring and infrequent coordination amongst players. Both of these behaviors have the potential to improve their welfare significantly, so much so that the impossibility results of Sannikov and Skrzypacz (2007, 2010) can be completely overturned. Our interest in future work is to explore experimentally how infrequent coordination can help sustain cooperation in the laboratory.

A Proofs

Proof of Proposition 1. The proof follows a basic argument by Fudenberg et al. (1994). For a contradiction, assume that $\gamma > 20$. Choose $v \in E(\delta)$ such that $v_1 + v_2 = \gamma$. Player i's utility is given by

$$v_i = (1-\delta)u_i + \delta(pw_i^+ + qw_i^-),$$

where w_i^+ and w_i^- denote the continuation payoffs of player i after a good and a bad signal, respectively, u_i is player i's expected utility today and p and q are, respectively, the probabilities of a good and a bad signal today. Since $\gamma > 20$, it must be the case that the probability of both players cooperating is greater than zero after some history. Let μ_j, where $j \neq i$, denote player j's probability of defection. It will be incentive compatible for player i to cooperate if

$$(1-\delta)(1-\mu_j)15 + \delta\big[\big((1-\mu_j)p_2 + \mu_j p_1\big)w_i^+ + \big((1-\mu_j)q_2 + \mu_j q_1\big)w_i^-\big]$$
$$\geq$$
$$(1-\delta)\big((1-\mu_j)20 + 2\mu_j\big) + \delta\big[\big((1-\mu_j)p_1 + \mu_j p_0\big)w_i^+ + \big((1-\mu_j)q_1 + \mu_j q_0\big)w_i^-\big].$$

Write $\Delta p = (1-\mu_j)(p_2 - p_1) + \mu_j(p_1 - p_0)$. Rearranging yields:

$$(1-\delta)[5(1-\mu_j) + 2\mu_j] \leq \delta\Delta p(w_i^+ - w_i^-),$$

that is, current utility gains from deviating are outweighed future losses in continuation payoffs. Since $p_2 - p_1 = 1/4$ and $p_1 = p_0$, this inequality yields the following upper bound on w_i^-:

$$w_i^- \leq w_i^+ - \frac{1-\delta}{\delta}\frac{5(1-\mu_j)+2\mu_j}{\Delta p} = w_i^+ - \frac{1-\delta}{\delta}\frac{5(1-\mu_j)+2\mu_j}{(1-\mu_j)/4} \leq w_i^+ - 20\frac{1-\delta}{\delta}$$

Substituting into the previous expression for v_i,

$$v_i \leq (1-\delta)u_i + \delta\left[w_i^+ - 20q\frac{1-\delta}{\delta}\right].$$

Therefore,

$$v_1 + v_2 \leq (1-\delta)\left[30 - 40q\right] + \delta\gamma.$$

Since $q \geq 0.25$ and $v_1 + v_2 = \gamma$ by hypothesis, it follows that $\gamma \leq 20$, as claimed.

Finally, a proof that the bound remains in public communication equilibrium can be found in Rahman (2013a, Lemma 1). □

Proof of Lemma 1. Assume that (4.2) holds. If a player chooses to deviate for τ periods, the utility gained from such a deviation is clearly bounded above by $(1-\delta)5\tau$, since this bound ignores discounting of future deviation gains. In other words, deviation gains are linear in the number of deviations. On the other hand, punishment costs grow exponentially in the number of deviations. Indeed, the opportunity cost of punishment remains $\delta^T\alpha(v-2)$, but the change in punishment probability from τ deviations becomes

$$q_2^{T-\tau}(q_1^\tau - q_2^\tau) = q_2^T\left[\left(\frac{q_1}{q_2}\right)^\tau - 1\right],$$

which, since $q_1 > q_2$, clearly grows exponentially with τ. Now, by the Binomial Theorem, $(q_1/q_2)^\tau - 1 \geq \tau[(q_1/q_2) - 1] = \tau$, so the change in punishment probability is bounded below by $q_2^T\tau$. Therefore, the following inequality implies that τ deviations are discouraged:

$$(1-\delta)5\tau \leq \delta^T q_2^T \tau\alpha(v-2).$$

But this is just (4.2). The claim now follows because τ was arbitrary. □

Proof of Proposition 2. Fix $T \in \mathbb{N}$. As $\delta \to 1$, the left-hand side of (4.3) tends to $1/T$ by l'Hopital's rule, which is less than or equal to 1. Hence, there exists $\delta < 1$ sufficiently large that (4.3) holds, so the candidate equilibrium strategies above are indeed an equilibrium. Finally, by l'Hopital's rule, it follows that

$$v \to 15 - 5\frac{1}{T} \quad \text{as} \quad \delta \to 1.$$

Finally, it is now clear that $v \to 15$ as $T \to \infty$, as claimed. □

B Instructions to treatment NC

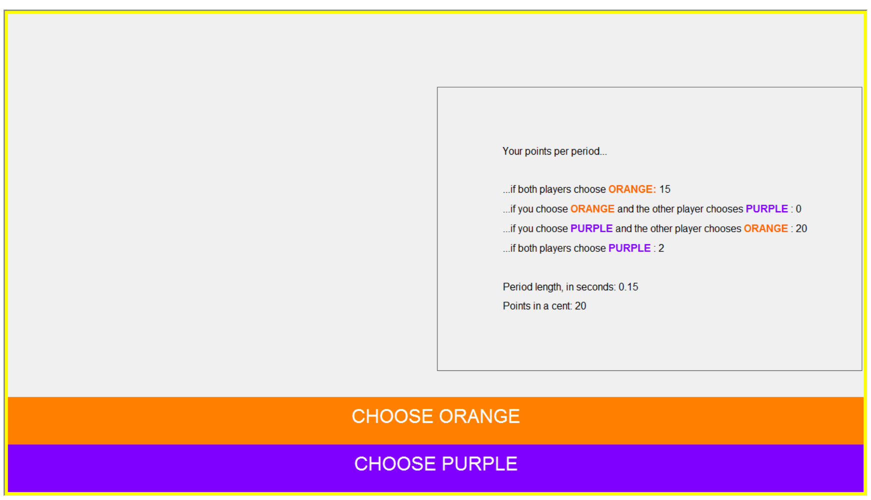
Instructions

Welcome and thank you for participating in the following experiment on strategic decision-making. This document explains what the experiment will entail. First, notice that **your show-up fee that is paid separately and is not affected in any way by the outcome of the experiment.**

Timing and Payoffs

You will be randomly and anonymously matched to another subject several times. Each time you are matched to a subject will be called a "match." During each match, you will interact with the other subject to whom you are matched through a computer program as described below. You will have the opportunity to earn money depending on the decisions made by you and this other subject.

In the beginning of a match, you will see on the bottom of your computer screen an orange button and a purple button. At any time, you will have the choice of selecting either color by clicking on the corresponding button with the computer's cursor, using your mouse. The image below shows what your computer screen will look like before you make your first choice.



Initial screen prior to making first choice

Once you and the other subject have selected an initial color, **periods will start elapsing with a duration of 0.15 seconds per period**. You can change your selection at any time and as often as you want by selecting the corresponding button; so can the other subject to whom you are matched. The computer program will register your changes every period. Thus, in 15 seconds the program will register one hundred choices. **Unless and until you change your choice, your assumed choice for a given period will be the last choice you made.** For example, if you select orange by clicking on the orange rectangle on the screen with your mouse and then change to purple 5 seconds later by clicking on the purple rectangle with your mouse then the computer will register that you chose orange during the time between clicking orange and purple.

Your monetary payoff at the end of the experiment depends both on your color choices and those of the other subjects to whom you were matched. Every period, if you chose orange and the other subject chose orange, too, then you will each earn 15 points. If you chose orange and the other subject chose purple then you will earn zero points and the other subject will earn 20 points. If you chose purple and the other subject chose orange then you will earn 20 points and the other subject will earn zero points. Finally, if both you and the other subject chose purple then you will each earn 2 points. Your final payoff is the accumulation of all your points in all your matches. **Points will be exchanged for money at the rate of forty (40) points per cent, or 1000 points per 25 cents.** The table below summarizes this information.

		Other's choice	
		Orange	Purple
Your choice	Orange	15 points for you 15 points for other	0 points for you 20 points for other
	Purple	20 points for you 0 points for other	2 points for you 2 points for other

Average points per period depending on each subject's choices

To illustrate, consider the following example. If you and the other subject to whom you are matched both chose orange for 100 periods, then you would earn 15 x 100 = 1,500 points, translating into 1,500 x 1/40 = 37.5 cents. If you chose purple and the other subject chose orange in every period, you would earn a total of 20 x 100 = 2,000 points, which would translate into 2,000 x 1/40 = 50 cents. If you both chose purple, then you would earn 2 x 100 = 200 points, translating into 200 x 1/40 = 5 cents.

The number of periods in a match is selected as follows. Every period, a random process determines whether the match continues on to the next period. The continuation probability is held constant, so that the average duration of a match is 700 periods. Because termination is random, some matches will last longer than 700 periods and others will last less than that. As soon as a match ends, every subject will be randomly and anonymously re-matched with another subject. You will be re-matched several times. Your final payoff will consist of the accumulation of your payoffs across all matches.

Information

Throughout a match, you will observe neither your payoff, nor the other subject's payoff, nor the other subject's choices. Similarly, the other subject will observe neither his or her payoff, nor your payoff, nor your choices. You and the other subject will observe the outcome of a random **signal process**, graphically depicted on the left-hand side of your computer screen. The graph of the process will depend on your color selection, the other subject's selection, and an element of randomness, as follows.

Every period, the value of the signal process will either increase or decrease by one unit. If you and the other subject both chose orange, the value of the process will increase with 75% probability and decrease with 25% probability. Otherwise, if one or both of you chose purple then the process will increase and decrease with 50% probability.

The process will be displayed in real time, in blocks of 100 periods. On the top-right region of the screen there will be displayed the fraction of periods during which you chose orange in the current block as well as the **position** of the process, defined as the number of time it actually increased minus the number of times it actually decreased in the current block. If you and the other subject always chose orange, then, at the end of a block, the process will reach a position of around 50 on average. If one or both subjects always chose purple then, at the end of a block, the process will reach a position of around 0 on average. However, this score fluctuates randomly, and can in principle end up far away from these values. At the end of each block, a red "continue" button will appear at the bottom of the screen. You may press the button when you are ready to move on to the next block. There will be two practice blocks at the start to gain familiarity with the process.

To illustrate, see the figures below with possible paths of your earnings over time when you and the other subject make different color choices. Figure 1 below depicts possible paths of the signal process during two consecutive blocks given that both you and the other subject chose orange. The process starts at zero at the beginning of every block. The horizontal graph lines count 20 units of the process increasing or decreasing and the ticker line in the middle denotes the starting point of the signal. In this instance, the signal position exhibited a net rise of 50 units in the first block, followed by a rise of 46.

Figure 1: Possible path of the signal if both of you chose orange throughout

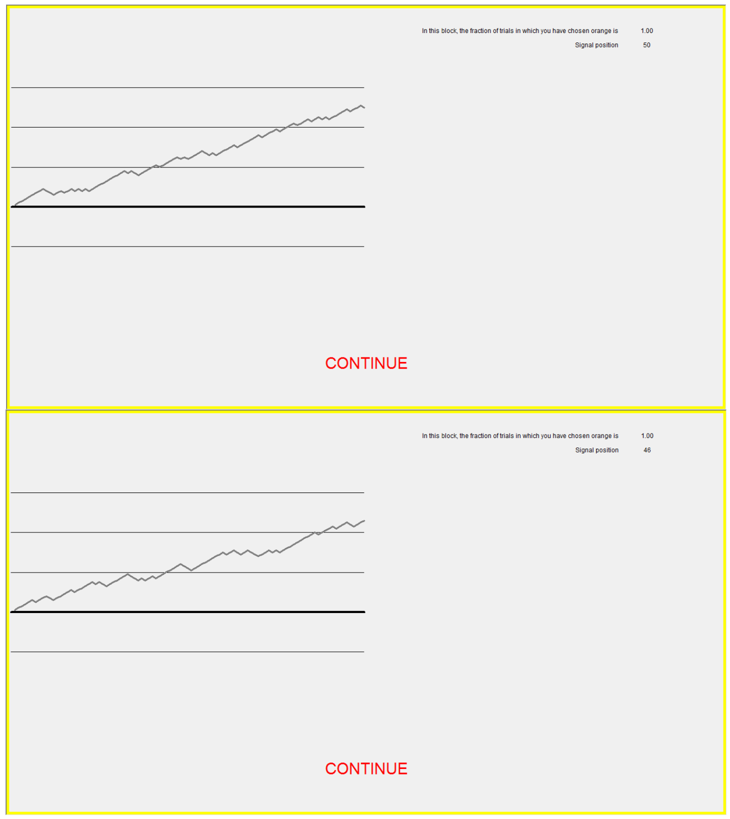


Figure 2: Possible path of the signal if one or both of you chose purple throughout

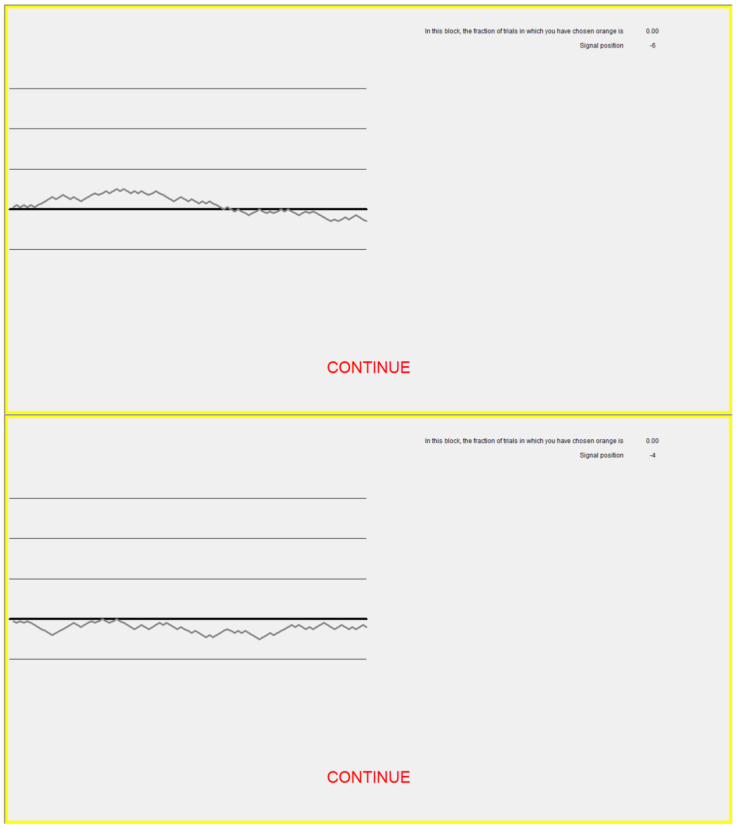
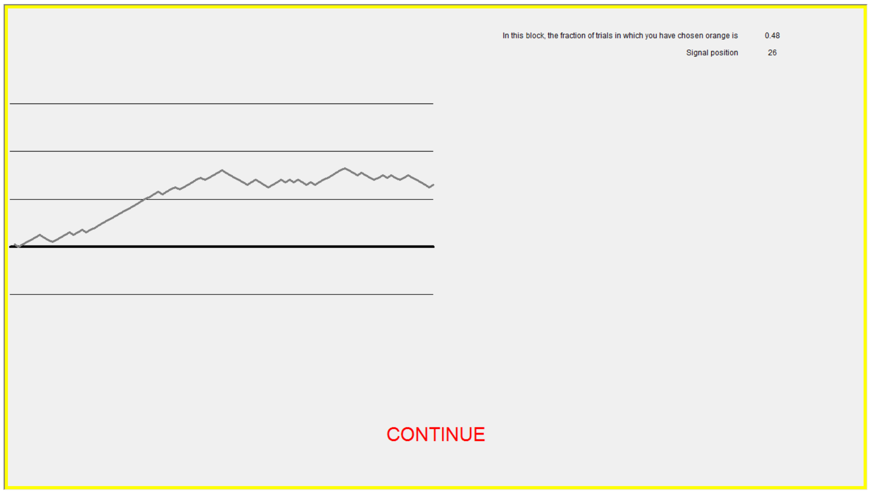


Figure 2 above depicts possible paths of the signal process for two consecutive blocks given that you chose purple and the other subject orange. In this instance, the signal position dropped 6 units in the first block, followed by a drop of 4 units in the next block.

Figure 3 below depicts the possible path of the signal process during a single block given that the other subject chose orange throughout the block and you switched from orange to purple halfway through the block.

Figure 3: Possible path of the signal if you switch from orange to purple after 48 periods



Communication

In addition to observing the signal process, you will be able to send messages to and receive messages from the other subject. At the beginning of every block, you will be able tell the other subject the percentage of time you plan to choose orange in the next block. You will be able to tell the other subject a plan for choosing orange some percentage of time if the signal position is above or below some number of your choosing. Once you have entered and submitted your answers, your message will be sent to the other subject and you will receive the other subjects' message. You will see on the right-hand side of your screen both your most recent message and the other subject's most recent message throughout the next block. At the end of every block you will observe your most recent message as well as the other subject's most recent message while completing your next message for the subsequent block. You will have the option of submitting the same message as before or submitting a different message. Below are some screenshots to illustrate.

Figure 4: Screenshot of initial message screen at the beginning of the first block

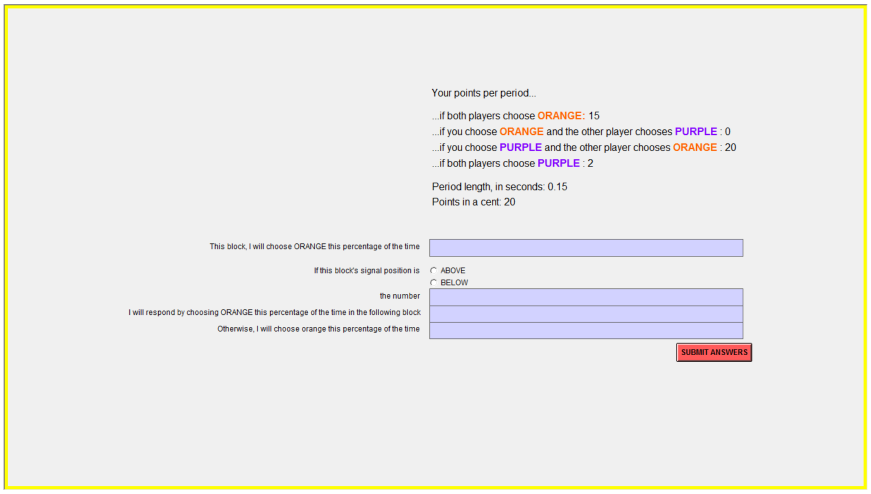


Figure 5: Screenshot of messages sent and received at the beginning of the first block

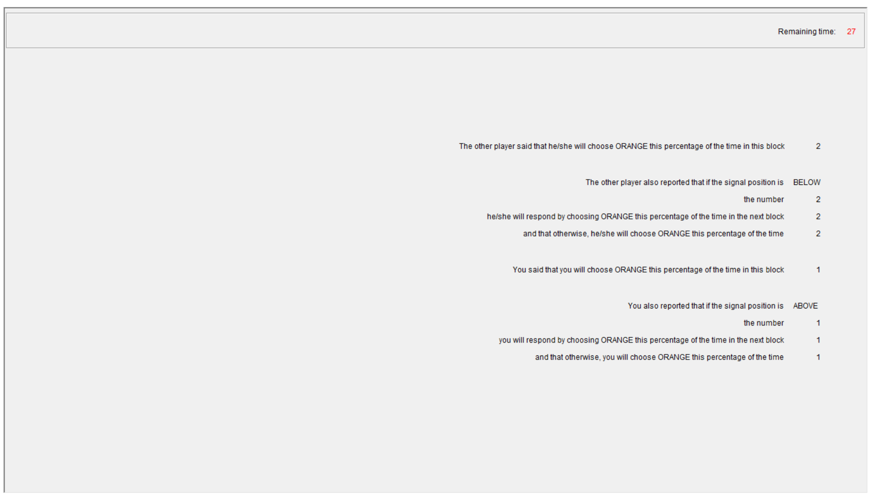


Figure 6: Screenshot of a subject at the end of a block

Figure 6: Screenshot of a subject at the end of a block if changing message

Ground Rules

Please wear the headphones provided throughout the experiment, except when instructed to do so by the experimenters. We also ask that you disconnect your cellphones throughout the duration of the experiment.

References

Abreu, D., P. Milgrom, and D. G. Pearce (1991): "Information and Timing in Repeated Partnerships," *Econometrica*, 59, 1713–33. 1, 4, 7, 11, 12, 14, 15, 29

Abreu, D., D. Pearce, and E. Stacchetti (1986): "Optimal cartel equilibria with imperfect monitoring," *Journal of Economic Theory*, 39, 251–269. 7

Abreu, D., D. G. Pearce, and E. Stacchetti (1990): "Toward a Theory of Discounted Repeated Games with Imperfect Monitoring," *Econometrica*, 58, 1041–1063. 7

Ambrus, A. and B. Greiner (2012): "Imperfect Public Monitoring with Costly Punishment: An Experimental Study," *The American Economic Review*, 102, 3317–3332. 5

Aoyagi, M. and G. Fréchette (2009): "Collusion as public monitoring becomes noisy: Experimental evidence," *Journal of Economic theory*, 144, 1135–1165. 5

Ben-Ner, A., L. Putterman, and T. Ren (2007): "Lavish returns on cheap talk: non-binding communication in a trust experiment," Working paper. 6

Bigoni, M., M. Casari, A. Skrzypacz, and G. Spagnolo (2011): "Time horizon and cooperation in continuous time," Working paper. 5

Bigoni, M., J. J. Potters, and G. Spagnolo (2012): "Flexibility and Collusion with Imperfect Monitoring," Working paper. 5

Cason, T. N. and F. U. Khan (1999): "A laboratory study of voluntary public goods provision with imperfect monitoring and communication," *Journal of Development Economics*, 58, 533–552. 5

Charness, G. (2000): "Self-serving cheap talk: A test of Aumann's conjecture," *Games and Economic Behavior*, 33, 177–194. 5, 6

Charness, G., R. Oprea, and D. Friedman (2012): "Continuous Time and Communication in a Public-goods Experiment," Working paper. 6

Compte, O. (1998): "Communication in Repeated Games with Imperfect Private Monitoring," *Econometrica*, 66, 597–626. 1, 2, 7, 11

Crawford, V. (1998): "A survey of experiments on communication via cheap talk," *Journal of Economic theory*, 78, 286–298. 2

Crawford, V. P. and J. Sobel (1982): "Strategic information transmission," *Econometrica: Journal of the Econometric Society*, 1431–1451. 2

Dal Bó, P. (2005): "Cooperation under the shadow of the future: experimental evidence from infinitely repeated games," *The American Economic Review*, 95, 1591–1604. 5

Dawes, R. M., J. McTavish, and H. Shaklee (1977): "Behavior, communication, and assumptions about other people's behavior in a commons dilemma situation." *Journal of personality and social psychology*, 35, 1. 5

Ely, J. C., J. Hörner, and W. Olszewski (2005): "Belief-Free Equilibria in Repeated Games," *Econometrica*, 73, 377–415. 1, 7, 11

Feuerstein, S. (2005): "Collusion in industrial economics—a survey," *Journal of Industry, Competition and Trade*, 5, 163–198. 4

Fischbacher, U. (2007): "z-Tree: Zurich toolbox for ready-made economic experiments," *Experimental economics*, 10, 171–178. 7

Friedman, D. and R. Oprea (2012): "A continuous dilemma," *The American Economic Review*, 102, 337–363. 3, 4, 5, 6, 29

Fudenberg, D. and D. Levine (2007): "Continuous time limits of repeated games with imperfect public monitoring," *Review of Economic Dynamics*, 10, 173–192. 7

——— (2009): "Repeated Games with Frequent Signals," *The Quarterly Journal of Economics*, 124, 233–265. 7

Fudenberg, D., D. Levine, and E. Maskin (1994): "The Folk Theorem with Imperfect Public Information," *Econometrica*, 62, 997–1039. 1, 7, 31

Hörner, J. and W. Olszewski (2006): "The Folk Theorem for Games with Private Almost-Perfect Monitoring," *Econometrica*, 74, 1499–1544. 1, 11

Kandori, M. (2003): "Randomization, Communication, and Efficiency in Repeated Games with Imperfect Public Monitoring," *Econometrica*, 71, 345–353. 2

Kandori, M. and H. Matsushima (1998): "Private Observation, Communication, and Collusion," *Econometrica*, 66, 627–652. 2, 7, 11, 15

Kandori, M. and I. Obara (2006): "Efficiency in Repeated Games Revisited: The Role of Private Strategies," *Econometrica*, 74, 499–519. 1

Kreps, D. M., P. Milgrom, J. Roberts, and R. Wilson (1982): "Rational cooperation in the finitely repeated prisoners' dilemma," *Journal of Economic theory*, 27, 245–252. 6

Lehrer, E. (1989): "Lower equilibrium payoffs in two-player repeated games with non-observable actions," *International Journal of Game Theory*, 18, 57–89. 1, 7, 12

Levin, J. (2003): "Relational Incentive Contracts," *American Economic Review*, 93, 835–847. 4

Murnighan, J. K. and A. E. Roth (1983): "Expecting Continued Play in Prisoner's Dilemma Games A Test of Several Models," *Journal of Conflict Resolution*, 27, 279–300. 8

Obara, I. (2008): "Folk Theorem with Communication," *Journal of Economic Theory*, forthcoming, mimeo. 7

——— (2009): "Folk theorem with communication," *Journal of Economic Theory*, 144, 120–134. 2

Oprea, R., K. Henwood, and D. Friedman (2011): "Separating the Hawks from the Doves: Evidence from continuous time laboratory games," *Journal of Economic Theory*, 146, 2206–2225. 5

Porter, R. H. (2005): "Collusion in industrial economics: a comment," *Journal of Industry, Competition and Trade*, 5, 231–234. 4

Radner, R. (1986): "Can bounded rationality resolve the prisoners dilemma?" *Essays in honor of Gerard Debreu*, 387–399. 3, 6

Radner, R., R. Myerson, and E. Maskin (1986): "An Example of a Repeated Partnership Game with Discounting and with Uniformly Inefficient Equilibria," *Review of Economic Studies*, 53, 59–69. 7

Rahman, D. (2012): "Mediating Collusion with Flexible Production," Mimeo. 30

——— (2013a): "Frequent Actions with Infrequent Coordination," Working paper. 2, 30, 31

——— (2013b): "Information Delay in Games with Frequent Actions," Working paper. 7, 15

Sannikov, Y. (2007): "Games with imperfectly observable actions in continuous time," *Econometrica*, 75, 1285–1329. 7

Sannikov, Y. and A. Skrzypacz (2007): "Impossibility of collusion under imperfect monitoring with flexible production," *The American Economic Review*, 97, 1794–1823. 1, 7, 30

——— (2010): "The role of information in repeated games with frequent actions," *Econometrica*, 78, 847–882. 7, 30

Simon, L. K. and M. B. Stinchcombe (1989): "Extensive form games in continuous time: Pure strategies," *Econometrica: Journal of the Econometric Society*, 1171–1214. 3, 6

Sugaya, T. (2010): "Folk Theorem in Repeated Games with Private Monitoring," Tech. rep., mimeo. 7, 11

Ulen, T. S. (1980): "Cartels and Regulation: Late Nineteenth-Century Railroad Collusion and the Creation of the Interstate Commerce Commission," *The Journal of Economic History*, 40, pp. 179–181. 4