

THE EFFECT OF ENDOGENOUS TIMING ON COORDINATION UNDER ASYMMETRIC INFORMATION: AN EXPERIMENTAL STUDY*

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ABSTRACT

This paper investigates the role of endogenous timing of decisions on coordination under asymmetric information. In the equilibrium of a global coordination game, where players choose the timing of their decision, a player who has sufficiently high beliefs about the state of the economy undertakes an investment without delay. This decision (potentially) triggers an investment by the other player whose beliefs would have led to inaction otherwise. Endogenous timing has two distinct effects on coordination: a learning effect (early decisions reveal information) and a complementarity effect (early decisions eliminate strategic uncertainty for late movers). The experiments that we conduct to test these theoretical results show that the learning effect of timing has more impact on the subjects' behavior than the complementarity effect. We also observe that subjects' welfare improves significantly under endogenous timing. JEL CLASSIFICATION NUMBERS: C72, C91, D62, D82, D83.

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1 INTRODUCTION

There are many economic activities that are characterized by strategic complementarities where individuals can achieve a desirable outcome if they coordinate their actions. Such activities include the agglomeration of businesses (Caplin and Leahy [10]), technology adoption (Katz and Shapiro [35, 36]), crop choice by subsistence farmers (Conley and Udry [13]), bank runs and currency attacks (Diamond and Dybvig [19], Morris and Shin [42] and Goldstein and Pauzner [28]), and foreign direct investment (FDI) (Jorjaan [34]), among others. In this paper, we frame our discussion around the FDI example. In fact, in the international trade literature it has often been argued that the probability of a FDI project's success increases if and only if the flow of FDI into the host market is high, because the presence of a large number of FDI projects can create positive externalities.

Another common characteristic of the examples mentioned above is the existence of asymmetric information. For instance, potential investors in foreign markets have myriad sources of information regarding the uncertainty that their decision involves. Uncertainty in FDI decisions can include political uncertainty (Rodrick [43], Alesina and Perotti [2], and Busse and Hefeker [7]), demand uncertainty (Goldberg and Kolstad [27], Aizenman and Marion [1]), exchange rate uncertainty (Cushman [16], Schmidt and Broll [44]), and cost uncertainty (Creane and Miyagiwa [15]). In many cases, individuals acquire private information from various sources regarding these uncertain events. For example, as Markusen [39] argues, “[... a] multinational firm may adopt some contractual agreement with a local agent as a means of exploiting any superior information the agent may possess regarding market characteristics.” As a result, each firm's information is determined by the contractual relationship with a local agent. This can create asymmetry because potential investors will pair with different local agents and will, therefore, have access to different sources of information.

The effect of strategic timing has been subject to analysis both under pure information externalities (Chamley and Gale [12], Gul and Lundholm [29]) and under strategic complementarities (Bolton and Farrell [5], Farrell [22], Farrell and Saloner [23, 24]). Under pure information externalities, time serves as a vehicle for disseminating information between individuals who make their decisions at different moments. We will refer to this effect of time as the *learning effect*. In the presence of strategic complementarities, time serves as a coordination device, because early movers eliminate strategic uncertainty by late movers, thus facilitating coordination. In the sequel, we will refer to this effect as the *complementarity effect* of strategic timing.

In the FDI example above, when there are many projects there will be higher returns on new projects because of strategic complementarities. A dearth of existing FDI projects,

on the other hand, in turn leads to stagnation because of strategic complementarities. In these cases, the complementarity effect of timing is evident. Suppose that a critical number of firms make an early decision to undertake an FDI project. The mere awareness of these projects eliminates the risk of coordination failure for other firms and leads them to undertake similar projects because of the existence of complementarities.

The outcome of economic activities can also be affected by the timing of when information is revealed by individuals' decisions. A firm that undertakes a FDI project reveals valuable information about the profitability of the project. Early investors therefore trigger a process through which valuable information aggregates. At the same time, the option of having access to this information in later periods (i.e., the learning effect) creates an incentive for the investors to wait and observe others' decisions. In fact, as Chamley and Gale [12] show, if the incentive to delay the investment is sufficiently strong, no investment is undertaken even though it is beneficial for all investors.

Despite the substantial amount of research analyzing the complementarity and learning effects separately, there is little theoretical work (viz. Brindisi et al. [6], Dasgupta [17], Heidhues and Melissas [30], Xue [46]) that studies the two effects together. Furthermore, to the best of our knowledge, there is no experimental research that does this either. Clearly, little is known about the interaction of these two effects. We aim to fill this gap by using a series of laboratory experiments to investigate the effects of strategic timing decisions on coordination under asymmetric information—i.e., when strategic complementarities and information externalities coexist.

Since the main focus of our paper is on strategic complementarities and asymmetric information about the fundamentals of the economy, we base the design of our experiment on the global coordination game introduced by Carlsson and van Damme [11]. Incorporating endogenous timing into this model allows us to analyze the two effects of timing, which we discussed above. The theoretical framework analyzed by Brindisi et al. [6] provides us with insights and hypotheses that we test in our experimental treatment. They characterize the equilibrium of a simple two-player global coordination game that allows endogenous timing. They demonstrate that the complementarity and the learning effects of timing allow players to internalize the returns from coordination, and strategic delay serves as a coordination device. In particular, they find that a player who has a high signal (i.e., is more optimistic) invests earlier, and a player who has a low signal (i.e., is more pessimistic) invests later, if at all.

The intuition for this is as follows. An optimistic investor, already expects a high return, making investment an attractive option. Beyond this, the optimistic investor also understands that his investment will make the other player more optimistic about the economic fundamentals, increasing the likelihood that the other player will invest in subsequent pe-

riods, if he has not already done so. Because of strategic complementarities, this further increases the optimistic investor's expectation of higher returns. The equilibrium behavior of the pessimistic investor is as follows: Because of his pessimistic beliefs, he not only expects a low return, but he also believes that the other investor holds pessimistic beliefs as well. Therefore, he finds it optimal to wait. If he observes investment, then in the next period he becomes less pessimistic, and adjusts his decision accordingly.

This characterization determines the effect of endogenous timing on welfare. It is well-known that the risk-dominant equilibrium is the limit equilibrium of the simultaneous game as the player's signal becomes fully informative. Brindisi et al. [6] show that when the state is high enough, the sequential game outperforms the simultaneous game, and conversely the simultaneous game outperforms the sequential game when the state is low. This finding is quite intuitive because the risk-dominant equilibrium presents less risk of coordination failure, which is more pronounced when the state is low. They also consider the difference in ex ante welfare between the sequential and simultaneous games as a function of the informativeness of the signals and demonstrate that the difference in ex ante welfare increases as the informativeness of signals decreases. Thus, endogenous timing has a particularly beneficial effect on welfare in relatively noisy environments.

The laboratory setting provides us with an environment in which we can observe empirical regularities while we control for asymmetric information—a variable that is hard to observe or identify (and hence, control for) in market data. We base the experiment on a special case of the model and vary the informativeness of private information in both simultaneous and sequential games to obtain a rich data set. A high level analysis of the data shows that the subjects' actual behavior is consistent with many properties of equilibrium behavior. For example, although there are three periods in which the subjects can make their decision, there are negligibly few subjects who delay the investment to the third period. Instead, if a subject has optimistic beliefs, he invests in the first period; if his beliefs are more pessimistic, he waits until the second period to invest (if at all). We also see that a subject who delays his decision invests in the second period, only if he observes that the subject invests, and if he is not too pessimistic. Otherwise, he never invests.

Another main finding of our experiment is that endogenous timing is a significant instrument for enhancing welfare, except when there is no uncertainty. When subjects have complete information, the coordination rates between simultaneous and sequential games are nearly identical. This suggests that the complementarity effect of timing is not a strong determinant of the outcome. When there is uncertainty, we find that coordination about the efficient outcome is uniformly higher in the sequential game than in the simultaneous game. Consistent with our theoretical findings, the gap between the coordination rates of simultaneous and sequential cases increases when private information becomes less

informative. That is, the more information is asymmetric between the players, the more endogenous timing facilitates coordination. We confirm the same results when making similar comparisons in terms of subjects' actual earnings. Overall, we conclude that in the lab the learning effect of timing is stronger than the complementarity effect of timing.

Looking more closely at behavior in our experiment, in the simultaneous game, we find that subjects coordinate on joint investment more than what theory predicts. This result is consistent with the findings of Heinemann et al. [31] and Duffy and Ochs [20]. In the sequential game, while the behavior of many subjects is consistent with the theory, we also find that there are some subjects who appear to have a strong preference to delay investment (in order to obtain more information), even though immediate investment would have been justified based on their private information. A follow-up within subjects experiment confirms that a non-negligible fraction of subjects actually have higher thresholds in the sequential game, when the possibility to delay exists.

The rest of the paper is organized as follows: in the remainder of this section, we discuss the related literature. Sections 2 and 3 discuss the positive and the normative predictions of the model. We pose the empirical questions in Section 4. Section 5 explains the experimental design and Sections 6 and 7 provide an analysis of the data. In particular, in Section 6, we examine only choice behavior, while in Section 7, we use elicited beliefs to gain further insights into subjects' decision making processes. Section 8 contains some further discussion of one potential unintended consequence of endogenous timing, while Section 9 concludes.

1.1 RELATED LITERATURE

1.1.1 THEORETICAL LITERATURE

Since our experimental design is based on Carlsson and van Damme [11], for a thorough review of global games literature we will refer the reader to Morris and Shin [41], Morris [40], and the references therein. The global games literature takes the view that the complete information assumption accounts for the multiplicity of equilibria inherent in coordination games. A key result in this literature is that multiplicity vanishes once the economy is perturbed by small failures of common knowledge. The goal of the present paper is different since we focus on questions that are concerned with the effects of endogenous timing on coordination under asymmetric information. In other words, asymmetric information is a defining feature of the environment rather than a vehicle to approximate the complete information model.

Dasgupta [17] and Xue [46] study similar questions with a theoretical approach.¹ Das-

¹Angeletos and Werning [4], Angeletos et al. [3] and Heidhues and Melissas [30] also share some similar-

gupta [17] provides an analysis of a two-period game with endogenous timing and a continuum of players. In the second period of the game, players who still have an option to invest observe an additional private signal that provides information about the aggregate level of investment in the first period. The second period signal in Dasgupta [17] differs from the information conveyed in our model by an investment decision in two respects: First, it is privately observed and second, it is subject to idiosyncratic noise. This modeling choice has implications in terms of the interplay between information and timing, which is the main focus of the present paper. In Dasgupta [17], players who invest in the first period know that they collectively play a role in determining second-period signals. Therefore, they take into account the aggregate influence on investment decisions in the second period, yet, because of the continuum of players, the individual signaling effect is negligible. Under specific distributional assumptions, Dasgupta [17] shows that if private information is sufficiently precise, the game has a unique equilibrium in monotone strategies. However, because the signaling effect is negligible the equilibrium does not achieve efficiency at the complete information limit.

Xue [46] also studies a similar model of dynamic coordination game under incomplete information. However, Xue [46] assumes that the types of players are not correlated. The correlation between the types (namely, player's private information regarding the common return of investment) is a critical feature of the current paper because it allows players to signal about the value of investment, when they choose to invest in early periods. Xue [46] also analyzes the role of irreversibility of actions in dynamic coordination games. Similarly, Dasgupta et al. [18], and Kováč and Steiner [37] focus on the role of irreversibility in dynamic global coordination games. However, they assume that players do not observe each other's actions. Hence, they do not focus on social learning dynamics as we do in the present paper.

Our paper also shares some common elements with the literature on observational learning with pure information externalities. A few papers focus on endogenous timing and hence, strategic delay (cf. Caplin and Leahy [9], Chamley and Gale [12], Gul and Lundholm [29] and Levin and Peck [38]). Under pure information externalities, an investor can wait and observe the behavior of other investors to gain additional information on the uncertain returns of investment. Investment is undertaken if the current expected returns are higher than the value of future information gains net of exogenous cost of delay. In our paper, the additional presence of payoff externalities causes a fundamental change;

ities. Among other results, the first paper shows that the endogenous transmission of information can lead to a multiplicity of equilibria, while the second shows in a dynamic model of regime change the interplay of endogenous and exogenous learning about the economy also generates a multiplicity of equilibria. Finally, Heidhues and Melissas [30] examine the role of cohort effects, and characterize the conditions under which uniqueness of equilibrium is established.

because now an investor must weigh the benefits of moving early to transmit information to others against the benefits of waiting to observe other investors' decisions.

1.1.2 EXPERIMENTAL LITERATURE

Like the theoretical literature on global games, the experimental literature can be categorized as dealing with either static or dynamic global games. Heinemann et al. [31], which tests Morris and Shin [42], is the pioneering experimental work on static global games. Heinemann et al. [31] present three main findings: First, differences in observed behavior under complete and incomplete information are not significant.² Second, behavior is consistent with the comparative statics of global games. Third, subjects cooperate more often than the predictions of the theory.

Duffy and Ochs [20] is the bridge between static and dynamic global games experiments. In a similar environment to the complete information treatments of Heinemann et al. [31], they study behavior in both static and dynamic games.³ Contrary to their focus on complete information, our experiments examine behavior in both static and dynamic games under incomplete information, allowing us to provide a clear test of the theory of dynamic global games that we consider.

In its static treatments, Duffy and Ochs [20] elicit subjects' strategies and find that actual decisions are often consistent with the implied, elicited cutoff strategies, though they also find that there is substantial variance in individual cutoffs and that cutoffs vary with changes in the environment in ways not predicted by the theory. Comparing the dynamic and static games, Duffy and Ochs [20] find that *cutoffs* do not differ in the two treatments. That is, while subjects have lower cutoffs than theory predicts in the static game, they have higher cutoffs than the efficient subgame perfect equilibrium threshold in the dynamic games. Moreover, in the dynamic games, they find that subjects adopt a "wait-and-see" approach, which often leads to substantial delay.

Our experimental data share some similarities with these papers. Like Heinemann et al. [31] and Duffy and Ochs [20], we find that subjects have lower thresholds than predicted by the global games theory in the static game. Also, like Duffy and Ochs [20], we find that subjects in our dynamic treatments have higher thresholds than predicted by our theory, and that the thresholds in the dynamic and static treatments are indistinguishable under complete information. Unlike Duffy and Ochs [20], our experiment involves incomplete information. Moreover, with incomplete information, we find that behavior is

²Similarly, Cabrales et al. [8] test Carlsson and van Damme [11] and provide further support that complete and incomplete information environments lead to similar behavioral patterns. They show that behavior converges to theoretical predictions as the subjects become more experienced.

³Fehr and Shurchkov [25] and Costain et al. [14] also conduct dynamic global games experiments, though because the underlying models that they study have multiple equilibria, they are separate from our focus.

distinguishable—in particular, thresholds are lower and the frequency of coordination is higher in the dynamic games than in the static games.

Finally, in a related strain of literature, SgROI [45], Ivanev et al. [32], and Ivanev et al. [33] study endogenous timing in social learning games. These experiments focus only on the role of timing decisions in information acquisition and investigate various behavioral phenomena that arise in social-learning environments with pure informational externalities. In contrast, our game has both payoff and information externalities. We focus on the interplay between these externalities as well as their effect on welfare.

2 THEORY

In this section, we lay out the theoretical model, which our experiment is based on, and discuss its implications. The model is based on our companion work Brindisi et al. [6]. Although our exposition is self-contained, we refer the reader to Brindisi et al. [6] for a thorough discussion of a more general model and formal proofs of the results.

2.1 PRELIMINARIES

Consider the following game à la Carlsson and van Damme [11]. Two players, $i = 1, 2$, make a binary investment decision. There are two actions available to them: I and W. We interpret action I as *investing* and W as *waiting*. While W is a safe action, whose payoff is normalized to s , the return of investment is determined by a random variable Θ . Given a realization θ , the payoffs are as in Figure 1. In particular, when both players take action I the return is θ , but if only one player takes action I his payoff is $\theta - r$ where $r > 0$. That is, the actions exhibit strategic complementarities: if a player invests, the return of investment is higher by an amount r when the other player invests as well.

FIGURE 1: PLAYER'S RETURNS FOR A REALIZATION OF Θ

	I	W
I	θ, θ	$\theta - r, s$
W	$s, \theta - r$	s, s

Each player's prior belief about Θ is represented by a uniform distribution over $[u, v]$. The true state, θ , is realized prior to investment decisions. Following the realization of θ , *but before taking any action*, each player receives a private signal. Player i 's signal is deter-

mined by the random variable X_i defined as

$$X_i := \Theta + E_i,$$

where E_i is a uniform distribution over $[-e, e]$, and $e > 0$. We assume that E_1 and E_2 are identically and independently distributed, and that E_i and Θ are independent for $i = 1, 2$.

We assume that players are Bayesian. Therefore, when player i receives a signal x_i he updates his belief about Θ by Bayes' rule. Player i 's posterior belief about Θ , conditional on observing x_i , is represented by the distribution function $F_\Theta(\cdot|x_i)$.⁴ A player also forms beliefs about the other player's signal. Player i , conditional on observing x_i , updates his belief about the signal of the other player, which we denote by $F_{X_j}(\cdot|x_i)$.

Finally, we assume that $v - r > s > u$. This assumption guarantees that there exist signals \underline{x} and \bar{x} such that $\mathbf{E}[\Theta|\bar{x}] = s + r$ and $\mathbf{E}[\Theta|\underline{x}] = s$. Therefore w is a dominant strategy for $x_i < \underline{x}$ and I is a dominant strategy for $x_i > \bar{x}$.

2.2 THE GAME

The game consists of τ periods: $t = 1, 2, \dots, \tau$. In each period, each player i takes an action $a_i^t \in \{I, w\}$. The action I is *irreversible*, while w is *reversible*. Therefore, if player i takes action I in period t ($a_i^t = I$), then his action is determined to be I in all future periods. If player i 's action was w up to period t , player i can still choose between actions I and w in period $t + 1$. However, switching to action I is costly in periods $t > 1$: If a player switches from action w to I in period t then he pays a cost of $(t - 1)c$, where $c \geq 0$.

For a given θ , payoffs are determined by the sequence of actions taken in τ periods:

$$u_i((a_1^t, a_2^t)_{t=1}^\tau) = \begin{cases} s & \text{if } a_i^\tau = w, \\ \theta - (t - 1)c - r \mathbb{1}_{a_j^\tau = w} & \text{if } a_i^{t-1} = w \text{ and } a_i^t = I, \end{cases}$$

where $a^t = (a_1^t, a_2^t)$ is the profile of actions taken in period t , and $\mathbb{1}_{a_j^\tau = w}$ is an indicator function that takes value 1 if $a_j^\tau = w$ and 0 otherwise. We assume that players have perfect information about the history and the solution concept is perfect Bayesian equilibrium.

A player's strategy determines his action at each history given his signal. As it is standard in the literature, in our equilibrium analysis, we will focus on *monotone* strategies: If a player invests for a signal x_i , then he also invests for signals greater than x_i . Thus, a monotone strategy is characterized by thresholds, one for each history. We denote the threshold of player i in period t by k_i^h . Under a monotone strategy, after a history h , a player i , invests in period t if $x_i \geq k_i^h$; otherwise, he waits.

⁴For a generic random variable Y , F_Y and f_Y denote the distribution and density functions respectively.

2.2.1 SIMULTANEOUS GAME ($\tau = 1$)

If $\tau = 1$, the game reduces to a simultaneous game (henceforth `sim`), which is analyzed extensively in the literature of global games. For completeness, we will briefly review the key aspects of the game.

Since there is only one period, a single threshold characterizes the players' monotone strategies. So, we will write κ_i to denote player i 's threshold. Note that, if player j follows a monotone strategy with threshold κ_j , then player i expects him to invest with probability $1 - F_{X_j}(\kappa_j|x_i)$, and to wait with probability $F_{X_j}(\kappa_j|x_i)$. Hence, player i 's expected payoff from action I is $\mathbf{E}[\Theta|x_i] - rF_{X_j}(\kappa_j|x_i)$. Consequently, player i 's best response is to invest if and only if his expected payoff from investment is at least as much as the safe payoff s . If we restrict our attention to symmetric equilibria in monotone strategies, an equilibrium with threshold $\kappa := \kappa_1 = \kappa_2$ must satisfy the indifference condition $\mathbf{E}[\Theta|\kappa] - rF_{X_j}(\kappa|\kappa) = s$. Simple computations yield the equilibrium threshold and, in fact, it is well-known that this is the unique equilibrium. We state this result in the following proposition.

PROPOSITION 1. *There exists a unique equilibrium of the `sim` game. The equilibrium is in monotone strategies and symmetric. It is characterized by the threshold $\kappa = s + r/2$.*

2.2.2 SEQUENTIAL GAME ($\tau \geq 2$)

Players' strategic considerations change dramatically when the game becomes sequential (henceforth `seq`). In the `seq` game, on one hand a player can wait in order to benefit from the information obtained in the other player's investment decisions; on the other hand, he can invest in the current period, avoid the cost of investment in the future, and more importantly, reveal information to the other player. This information may convince the other player to invest, allowing both players to benefit.

We begin our analysis with the preliminary observation that in any monotone-strategy equilibrium, once both players wait, there cannot be any investment in the future. The intuition behind this result is simple. At any point in time, a player decides whether or not to invest by comparing the expected payoff of investing, and the option value of waiting. In an equilibrium with monotone strategies, observing that the other player's action is `W` reveals *bad* news. This, in turn, makes the player more pessimistic, and hence, lowers the expected payoff of investing. As a result, a player who has not already invested, finds it even less profitable to invest after observing `w`. Since this argument applies to both of the players, there is never any investment after period two.

This observation greatly simplifies our equilibrium analysis because it implies that in `seq` investment will only occur in the first two periods, if at all. Therefore, in any monotone-strategy equilibrium, player i has two thresholds k_i^1 and k_i^2 for periods 1 and 2, respectively.

The first threshold determines the range of signals for which he immediately invests. If he does not invest in the first period, the second threshold determines whether or not he will invest in the second period, *after observing the other player invest*. We state the full equilibrium characterization in the next proposition⁵.

PROPOSITION 2. *There exists a $c^* > 0$ such that if $c < c^*$ there exists a unique equilibrium of the SEQ game in monotone strategies. The equilibrium is symmetric (i.e. $k_1^t = k_2^t = k^t$, for $t = 1, 2$) and characterized as follows:*

$$\text{Play} \begin{cases} \mathbf{I} & \text{in the first period if } x_i \geq k^1, \\ \mathbf{I} & \text{in the second period if } a_j^1 = \mathbf{I} \text{ and } x_i \geq k^2, \\ \mathbf{W} & \text{otherwise,} \end{cases}$$

such that

$$k^1 - r \left(\frac{3}{4e} \right)^2 (c + s + e - k^1)^2 + c - s - \frac{e}{3} = 0, \quad (1)$$

$$2k^2 + k^1 - 3(c + s) + e = 0. \quad (2)$$

If $c \geq c^*$, there exists a symmetric equilibrium of the SEQ game. The equilibrium strategies are monotone, symmetric and characterized as follows:

$$\text{Play} \begin{cases} \mathbf{I} & \text{in the first period if } x_i \geq k, \\ \mathbf{W} & \text{otherwise,} \end{cases} \quad \text{such that } k = s + \frac{r}{2}.$$

Proposition 2 fully characterizes the equilibrium of the SEQ game. There are two types of equilibria depending on the level of cost. The cutoff c^* is the cost, above which, even a player who waits in the first period and observes an investment would never invest. In other words, if the cost is higher than the option value of delay ($c \geq c^*$), the game is effectively simultaneous. Therefore, there is only one threshold as in the SIM game ($k = \kappa$.)

When the cost is small enough, the unique equilibrium of the game is characterized by two thresholds $k^1 \geq k^2$. A player invests in period one for signals above k^1 ; otherwise, he waits. If his signal is between k^2 and k^1 , he invests in period two only when he observes an investment, otherwise he waits. If his signal is less than k^2 , then he never invests.

The condition (2) in Proposition 2 determines the optimal threshold (k^2) of a player who did not invest in the first period but observed the other player investing. The observation that the other player invested reveals that his signal is above k^1 . Having updated

⁵The proof follows from the equilibrium characterization provided in Brindisi et al. [6]. The computations are available at <http://bogachancelen.com>.

his beliefs accordingly, the player invests in the second period for any signal above k^2 since the expected value of investing exceeds the safe payoff plus the cost of delayed investment.

The condition (1) determines the optimal threshold in the first period. Basically, it compares the ex ante expected value of waiting with the expected value of investing today.

3 DISCUSSION OF EQUILIBRIA

The equilibrium analysis of the SEQ and SIM games sheds light on the *learning* and *complementarity* effects of timing. Let us first discuss the benchmark cases of SIM and SEQ under complete information. Throughout our discussion let us assume that $\theta \in [s + c, s + r]$, so that investing in the second period remains a feasible option.

The equilibria of SEQ and SIM under complete information are straightforward. While there are multiple equilibria in the SIM game, the SEQ game has a unique subgame perfect equilibrium in which both players invest in the first period if and only if $\theta \geq s + c$. In the SIM game, the reason for multiple equilibria is due to self-fulfilling beliefs: If both players believe that the other will play I (W) then it is optimal to play I (W). On the other hand, the uniqueness of the subgame perfect equilibrium in the SEQ game depends on players' reasoning that if they invest in the first period for $\theta \geq s + c$, then the other player—even if he has not already invested—will invest in the second period. So, under complete information, the possibility that the players can choose the timing of their decisions has an important impact on the equilibrium prediction of the game through the complementarity effect of timing.⁶

When there is uncertainty about the fundamentals, the equilibrium prediction changes dramatically in the SIM game under asymmetric information. In the Bayesian equilibrium of the game, when both players have a signal above the equilibrium threshold $\kappa > 0$, they coordinate on the outcome (I,I). Similarly, they coordinate on the outcome (W,W) when both signals are below κ . Finally, the players fail to coordinate when one signal is above, and the other one is below κ . The literature on global games shows that these failures vanish as the players' signals become perfectly informative. In fact, in the limit, there exists a unique equilibrium. Given our parameterization, this equilibrium is characterized by $\kappa = s + r/2$. An immediate consequence of this result is that even though the efficient outcome is to invest when the expectation of θ exceeds the safe return s , this does not necessarily occur in the limit equilibrium.

In the case of SEQ, under complete information, there is a unique subgame perfect equilibrium because a player *knows* that his investment in the first period *guarantees* coordina-

⁶Note that if $\theta < s + c$, the SEQ game practically becomes a SIM game since no player will invest in the second period. Therefore, for $\theta \in [s, s + c)$ equilibria of SEQ and SIM coincide.

tion. Under asymmetric information, the explanation behind the perfect Bayesian equilibrium is similar. Precisely, if a player moves first, the probability that the other player will invest in the second period increases. The investment in the first period affects the probability of coordination in two ways: First, as in the complete information case, there is no risk of miscoordination for a player who did not invest in the first period. Second, since a player invests in the first period only if his signal is high enough (above k^1), observing investment makes other player's beliefs more optimistic about the overall profitability of investment. As a result, the equilibrium exhibits *sorting*; the players whose signals are high enough move first, while those who do not invest in the first period benefit from the information revealed from the investment decision. The sequential structure of SEQ makes it possible to exchange information (learning effect), and use timing as a coordination device (complementarity effect). Note that in the limit case of SEQ when the private signals become increasingly informative and the cost of delay becomes vanishingly small, the unique limiting equilibrium is the subgame perfect equilibrium of the SEQ game under complete information. In other words, there is no discontinuity in the limit of the SEQ game, and full efficiency is achieved (i.e., both players invest if and only if $\theta \geq s$).

3.1 WELFARE

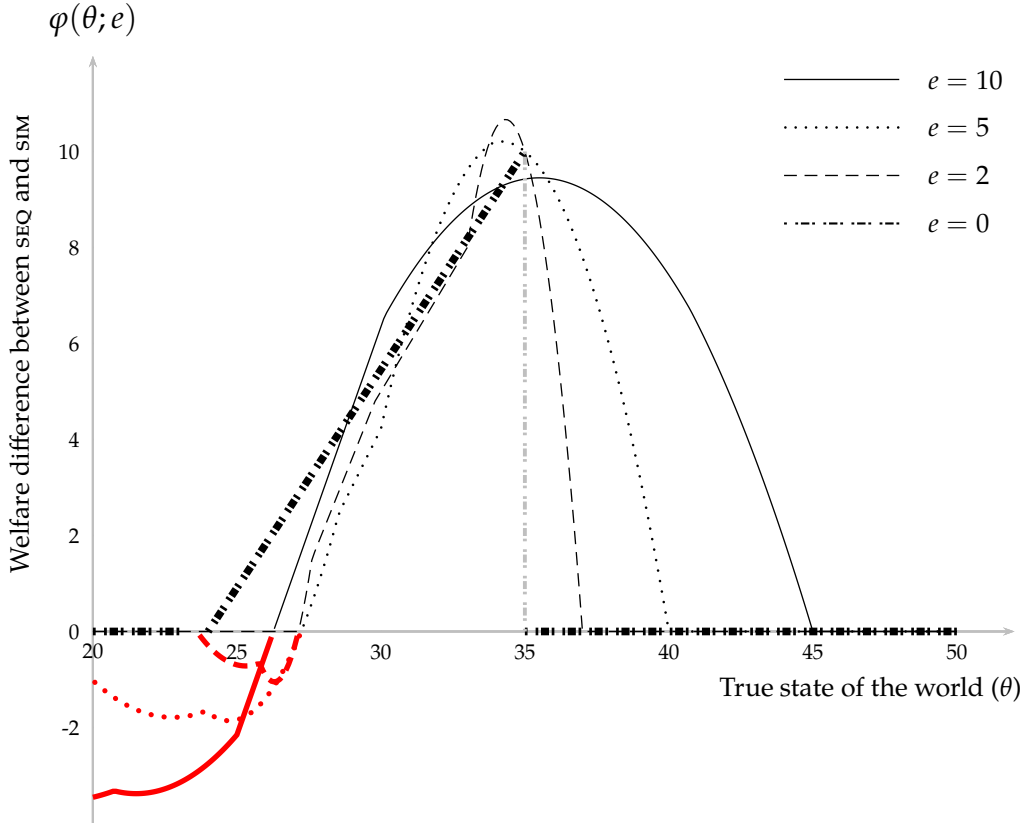
We now seek to quantify the welfare effects of endogenous timing. Let v_i^{SIM} and v_i^{SEQ} denote the value of the SIM and SEQ games in equilibrium, conditional on the true state θ . These are player i 's expected payoff when both players play the equilibrium strategy.⁷

Despite the conclusions we reached in the complete information case, we are unable to provide a general analytical comparison of v_i^{SIM} and v_i^{SEQ} for any given level of uncertainty. However, the following computational exercise helps us to understand welfare differences in both SEQ and SIM in the presence of noise. Figure 2 plots the computations of welfare differences of the two games for the parameters we employ in the experiment. Specifically, Θ is uniformly distributed between 20 and 50 (i.e. $u = 20$ and $v = 50$), and the noise level e , which determines the informativeness of the signals, takes values of 0, 2, 5, and 10. Finally, we assume that $s = 25$, $r = 20$, and $c = 2$. Let us define $\varphi(\theta; e) := v_i^{\text{SEQ}} - v_i^{\text{SIM}}$ to be the welfare difference between SEQ and SIM as a function of θ when the noise parameter is e .

For the above parameters, the threshold of the SIM game is $\kappa = 35$ for all values of e . Hence, in the limiting case where e approaches to 0, it is also 35. In the SEQ game, on the other hand, when $e = 0$, all players invest in period one if and only if their signal is above 25. Therefore, SEQ game generates at least the same welfare that the SIM game generates for all values of θ above 25. As the noise (i.e., e) increases, the learning effect leads to more co-

⁷The formulae for v_i^{SIM} and v_i^{SEQ} are in Appendix B.

FIGURE 2: WELFARE COMPARISON OF SEQ VS. SIM[†]



[†] The parameters used here correspond to those as used in our experiment. See Figure 3 for the precise game. The state variable was uniformly distributed on $[20, 50]$; given the true state, θ , each subject, i received a private signal $x_i = \theta + \epsilon_i$, where ϵ_i was an independent draw from the uniform distribution with support $[-e, e]$. Each line, represents the welfare difference between SEQ and SIM as a function of the true state for different signal noise parameters. The derivation of the welfare difference can be found in Appendix B.

ordination in the SEQ game. Although this coordination is valuable for large values of θ , for small values of θ , a player may invest upon seeing a high signal even though it would have been better not to invest at all. Therefore, for low realizations of θ , we observe that welfare in SIM is higher than in SEQ. The opposite is true for larger realizations of θ . Although there is no clear comparative static for $\varphi(\theta; e)$ as the noise changes, we find that, *on average*, the welfare difference between SEQ and SIM is increasing as the informativeness of the signals decreases. That is, the ex ante expected welfare difference $\tilde{\varphi}(e) := \int \varphi(\theta; e) dF(\theta)$ increases in e . More precisely, we find $\tilde{\varphi}(10) = 3.4$, $\tilde{\varphi}(5) = 2.3$, $\tilde{\varphi}(2) = 1.9$.

4 EMPIRICAL QUESTIONS

The theoretical predictions provide us with a number of empirical questions which will be rigorously analyzed in our experiment. In this section, we discuss and develop our main hypotheses and predictions.

THRESHOLDS. The equilibrium analysis is based on the assumption that players use monotone strategies. In the *SIM* game there is a unique equilibrium in monotone strategies, in the *SEQ* game we assume that the strategies are monotone and characterize the equilibrium. This leads to the obvious question of whether subjects' behavior is consistent with the use of monotone strategies. This is important for at least two reasons. First, it can help us understand whether the equilibrium that we characterize provides a meaningful benchmark to analyze the data. Second, we can analyze the data under the light of theoretical comparative statics, and if there are systemic deviations, we can investigate when and how subjects' behavior cause these deviations.

RATIONALITY AND VALUE OF INFORMATION. In the equilibrium of the *SEQ* game, all investment occurs in the first two periods. This is because both time is costly and investing reveals good news, while not investing reveals bad news. By allowing a longer time horizon in the experiment, we can test whether behavior is consistent with this result.

TIMING AND COORDINATION. Under complete information, while the *SIM* game has multiple equilibria, the *SEQ* game has a unique equilibrium in which players invest in the first period because they know that the other player will surely invest in the next period, provided that the return of investment exceeds safe return plus the cost of delay. This argument provides us with a straightforward way to understand the role of timing: In the absence of any information asymmetries, we can test whether time works as a coordination device by comparing *SEQ* and *SIM*.

In the asymmetric information case, timing has an additional role; namely an early investor reveals good news to his opponent about the profitability of investment. Thus, we can ask, do subjects use early investment to signal a profitable investment, and do subjects who observe investment respond appropriately to this signal?

WELFARE. Clearly, the role of timing has important implications in terms of welfare. We have a limit result that compares welfare measures in *SIM* and *SEQ*, which can be tested empirically. Although we do not have analytic results that compare welfare in the presence of asymmetric information, our simulations indicate that the average welfare difference

between `SEQ` and `SIM` increases as the asymmetry of information increases. That is to say, endogenous timing is *more* welfare enhancing when the environment is more noisy.

5 EXPERIMENTAL DESIGN

The experiment was run at the Center for Experimental Social Sciences (C.E.S.S.) of New York University. 308 subjects were recruited from undergraduate classes at NYU, who had no previous experience in our experiments. In each session, after the subjects read the instructions, they were also read aloud by an experimental administrator.⁸ Each session lasted for about 90 minutes, and each subject participated in only one session. An \$8 participation fee, and subsequent earnings, which averaged about \$21, were paid in private at the end of the session. Throughout the experiment, we ensured anonymity and effective isolation of subjects in order to minimize any interpersonal interactions and influences that could stimulate a specific pattern of behavior.⁹ The experiment was programmed in z-Tree (Fishbacher [26]). Appendix A contains the instructions for one of the sessions.

We report two treatments: `SIM` and `SEQ`. For each treatment, we ran different sessions in which we varied the informativeness of subjects' signals. In each session, subjects played the specified game for 40 rounds. Subjects were randomly matched with another subject in the laboratory at the beginning of each round.

We also ran a variant of `SIM` and `SEQ` treatments, in which we elicited subjects' beliefs about what their match will play, and about the state variable θ .¹⁰ We will label those sessions in which we elicited beliefs as Beliefs treatments, otherwise we will call them Regular treatments. Below, we will explain the differences between Beliefs and Regular treatments in more detail. We also ran one follow-up session, using a within-subject design in which subjects played both the `SIM` and `SEQ` games, with beliefs being elicited.

Throughout the experiment we used a neutral language and replaced the terms *investing*, *waiting* and *signal* with *action A*, *action B*, and *estimate*, respectively.

Table 1 provides a summary of our design. The parameter e indicates the noisiness of signals. Henceforth, we will write `SEQ`(e) and `SIM`(e) to refer to the treatments with corresponding parameter e . In all `SEQ` treatments the cost of delay, c , was 2.

⁸At the end of the first round, the subjects were asked if there were any questions. No subject reported any problems with understanding the procedures or using the software.

⁹Participants' workstations were in isolated cubicles, making it impossible for them to observe others' screens or to communicate. We also made sure that all the participants remained silent throughout the session. At the end of a session, participants were paid in private according to the number on their workstation.

¹⁰We appreciate a referee's suggestions, which led to these follow-up sessions.

TABLE 1: SUMMARY OF TREATMENTS

Treatment	Number of Subjects		c	e
	Regular	Beliefs		
SEQ	22	20	2	0
SEQ	24	–	2	2
SEQ	28	20	2	5
SEQ	32	20	2	10
SIM	16	20	NA	0
SIM	16	–	NA	2
SIM	24	16	NA	5
SIM	36	14	NA	10
SIM/SEQ	–	20	2 [†]	10

[†] Applicable only when subjects played the SEQ game.

SEQUENTIAL (SEQ) In the Regular treatments, the precise game that subjects played was a parametrization of the problem that we analyzed theoretically. In each session, subjects played the game with the payoffs specified in Figure 3. The distribution of the state variable Θ was uniform over the support $[20, 50]$. Conditional on the state, θ , each subject i received an independent signal x_i which was drawn from a uniform distribution over the support $[\theta - e, \theta + e]$. The parameter $e \geq 0$ determines the informativeness of signals. The experimental software randomly generated θ , as well as signals x_1 and x_2 for each pair in every round. The length of the game was set to be $\tau = 3$.

FIGURE 3: PAYOFFS EXCLUDING COSTS OF DELAYED INVESTMENT

	I	W
I	θ, θ	$\theta - 20, 25$
W	$25, \theta - 20$	$25, 25$

In each period, subjects simultaneously chose I or W. Choosing I was irreversible, while choosing W was reversible. If a subject chose W in the first period, then in the second period, he would observe the first-period decision of his match. Similarly, in the third period, if the subject chose W in periods one and two, he would observe his match’s previous decisions. The cost of investment was only incurred if the subject chose I in period two or three, after having previously chosen W. The cost of investment in period two was 2 points, while it was 4 points in period three. Immediate investment was costless.

SIMULTANEOUS (SIM) In the `sim` treatment the payoffs were identical to those in the `seq` treatment. The only exception was that each round of `sim` consisted of a single period in which subjects could make their decision.

ELICITING BELIEFS In both `sim` and `seq` Beliefs treatments, after subjects received their private signal, they were asked to state the likelihood that they believed their match would invest in the first period. These beliefs were incentivized according to a quadratic scoring rule, and subjects were paid for one randomly chosen round. In addition, in the `seq` treatments with $e > 0$, after they have observed their match’s first period decision, subjects were asked for their best estimate of the state variable, θ . These predictions were incentivized according to a quadratic loss function parameterized so that subjects could not lose money. Like the other prediction, subjects were paid for one randomly chosen round.¹¹

We initially conducted the Regular treatments, and then conducted the Beliefs treatments as a follow-up. Since we found that subjects’ behavior regarding the timing of investment was consistent with the theoretical prediction that all investment should take place in the first two periods (see Table 2), in the Beliefs treatments subjects only had two periods to make their decision (i.e., $\tau = 2$).

A WITHIN-SUBJECT TREATMENT While most of our analysis uses a between-subjects design, we ran one additional treatment to investigate behavior *within* subjects. Specifically, we ran one experiment with 20 subjects in which subjects first played the `sim`(10) game for 30 rounds, and then played the `seq`(10) game for 25 rounds.¹² Subjects were informed that there would be two parts to the experiment, and were given details of the second part only after the first part was finished. This additional treatment was conducted in order to gain further insights into behavior and to test a conjecture that some subjects have an excessively strong desire, when given the opportunity as in the `seq` game, to wait in order to acquire information. We will come back to these findings in Section 8. Note that our main data analysis in Sections 6 and 7 exclude the data from this session.

6 EXPERIMENTAL RESULTS

In this section, we present our main experimental results. We first show that subjects understood the most fundamental aspects of the decision problem, and then show that

¹¹Observe that a third belief, namely, the one that the subject’s match will invest in period two after observing that he *did not* invest in period one, was intentionally not asked of subjects. Beyond the difficulties with implementing this belief elicitation (i.e., it is only relevant in some histories), we felt that it could introduce perverse incentives which would bias their play in the actual game.

¹²A network failure prevented us from completing the originally planned 30 rounds.

subjects' behavior was consistent with the use of monotone strategies. Finally, we test the main theoretical predictions by comparing observed behaviors in *SIM* and *SEQ*. All results are pooled across the Beliefs and Regular treatments. We will note any differences that we observe in the analysis of these two treatments whenever necessary.

6.1 BASIC TESTS OF SUBJECTS' UNDERSTANDING

In order to find out whether behavior is compatible with the basic predictions of the theory, Table 2 reports the number of failures in each of three fundamental tests of subjects' understanding of the problem. First, do subjects respect dominance? Given the parameters of our experiment, if a subject's signal is below $25 - e$ or above $45 + e$, then *W* and *I* are dominant actions, respectively. Our subjects violated dominance only 4.1% of the time in the *SEQ* treatment, and only 0.7% of the time in the *SIM* treatment. Second, do subjects in the *SEQ* game violate the prediction that there should not be any investment in period three? This is an important property of the equilibrium, and its frequent violation would imply a basic misunderstanding of strategic interaction in our game. In the Regular *SEQ* treatments, in only 0.47% of the observations did a subject invest in period three. Finally, do subjects invest in period two after observing that their match *did not* invest in period one? We observe this type of behavior in only 0.98% of all the cases in which a subject who did not invest in period one observes that his match did not invest in period one either.

TABLE 2: BASIC TESTS OF THE THEORY

	SEQ	SIM
Violations of dominance	42 (1021)	6 (818)
Invest in period 3	20 (4240)	NA
Invest in period two despite observing <i>W</i>	14 (1432)	NA

The numbers in parentheses indicate the relevant sample sizes.

Of this small number of errors, approximately half took place in the first 10 rounds and about 80% in the first 20 rounds. This indicates that not only these errors are rare, but they also vanish as subjects gain experience. Since the number of violations are extremely low, we can confidently conclude that the subjects in our experiment had a strong grasp of fundamental aspects of the strategic problem they were faced with.

6.2 DO SUBJECTS USE MONOTONE STRATEGIES?

The next fundamental question that we investigate is whether subjects' behavior in period one is consistent with the use of monotone strategies. That is, is there a k^1 such that a subject invests in period one if and only if his signal is $x \geq k^1$. For each subject, we construct a threshold as follows. For an arbitrary threshold, k^1 , we say that a subject makes a mistake if he receives a signal, $x \geq k^1$ but *does not* invest, or he receives a signal, $x < k^1$ but *does* invest. We then look for the threshold \hat{k}^1 that minimizes the number of mistakes.¹³ If, at \hat{k}^1 , a subject does not make any mistakes, then we say that the subject has a *perfect threshold*; that is, he uses a monotone strategy. Table 3 displays the results and the summary statistics on this exercise, where we focus on the last 20 rounds.¹⁴

TABLE 3: INDIVIDUAL-LEVEL PERIOD-ONE THRESHOLDS (LAST 20 ROUNDS)

Treatment	Average Threshold	Standard Deviation	Average Number of Mistakes	Fraction of Perfect Threshold	Equilibrium Threshold	p -value (theory)
SEQ(0)	27.93	3.24	0.17	87.8	27.00 [†]	0.07
SEQ(2)	28.93	2.13	0.25	87.5	27.79	0.02
SEQ(5)	30.61	2.88	0.13	89.3	28.92	0.00
SEQ(10)	32.12	7.05	0.81	48.1	30.74	0.17
SIM(0)	27.92	2.50	0.08	91.7	35.00 [†]	0.00
SIM(2)	30.49	2.10	0.18	87.5	35.00	0.00
SIM(5)	32.42	3.14	0.38	71.8	35.00	0.00
SIM(10)	33.41	3.67	0.70	46.0	35.00	0.00

[†] Observe that there are actually multiple equilibria in these treatments. In SEQ(0) any threshold $k^1 \in [25, 27]$ (with $k^2 = 27$) is an equilibrium and in the SIM(0) treatment any threshold $\kappa \in [25, 45]$ is an equilibrium.

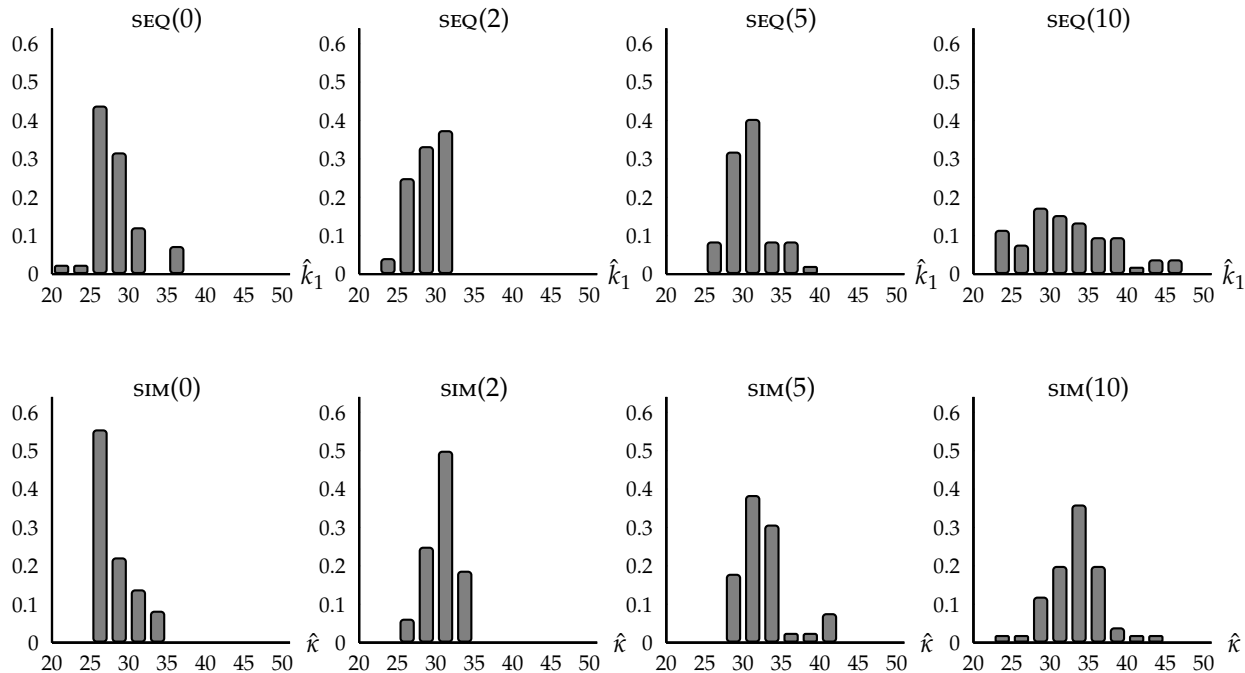
As the fifth column of Table 3 shows, in five of eight treatments, over 85% of the subjects has a perfect threshold, and only in SIM(10) and SEQ(10), did about half of the subjects has a perfect threshold, though even here, the average number of mistakes was less than 1. This observation lends more support that subjects understood the basic strategic interaction. In the SEQ game treatments, we see that the average threshold is always above the predicted equilibrium threshold. For SEQ(0) and SEQ(10), the difference is not significant at the 5% level, while for the other two SEQ treatments the difference is statistically significant. Thus, subjects appear not to invest often enough in the SEQ game. In contrast, in

¹³In the event of a tie, we take the average over all thresholds that minimize the number of mistakes.

¹⁴As noted above, the number of fundamental errors declined substantially after the first 20 periods. If we focused on the entire 40 periods, average thresholds are very similar, but the fraction of subjects using perfect thresholds is significantly lower.

the SIM treatments, subjects' thresholds are always significantly lower than the theoretical prediction, suggesting that subjects invest too often.

FIGURE 4: HISTOGRAMS OF INDIVIDUAL-LEVEL PERIOD ONE THRESHOLDS (LAST 20 ROUNDS)



In all the histograms, the horizontal axis shows the estimated thresholds and the vertical axis shows the fraction of subjects.

We estimate individual thresholds \hat{k} in the SIM treatment by applying the same procedure that we used in estimating \hat{k}_1 's.

In order to understand how these estimated thresholds are distributed, we look at the results at the individual level. Figure 4 displays the fraction of subjects' whose estimated threshold is in an interval of size 2.5 (e.g., 30-32.5). We see that there is a substantial amount of heterogeneity. For instance, in the SEQ(10) treatment, estimated first period thresholds range between 24.6 and 44.44. Moreover, heterogeneity increases as the level of noise increases. Also heterogeneity in SEQ is more pronounced than in SIM treatments.¹⁵

6.3 SEQ VS. SIM TREATMENT

In this subsection we start comparing subjects' behavior and outcomes in SEQ and SIM treatments. Our theoretical analysis highlighted three main predictions: (i) first period thresholds should be lower in SEQ than in SIM; (ii) coordination on joint investment should be

¹⁵ Including both Beliefs and Regular treatments, the variance of thresholds is higher in the SEQ game than in the SIM game except when $e = 5$; however, the difference is only significant for $e = 10$ ($p < 0.01$). If we focus only on Regular treatments, then the difference is also significant when $e = 0$ ($p < 0.01$).

more frequent and miscoordination should be less frequent in SEQ than in SIM; and (iii) welfare should be higher in SEQ, with the difference between SEQ and SIM increasing as signals become noisier.

6.3.1 THRESHOLDS

Table 3 confirms the first prediction. In fact, we see that the first period SEQ thresholds are generally lower than the SIM thresholds. This is true except when $e = 0$, where the inference is clouded by the presence of multiple equilibria.¹⁶ An analysis of variance with factors for treatment (i.e., SEQ vs. SIM), noise (i.e., e), and whether or not beliefs were elicited, shows that thresholds are 1.44 points lower in SEQ, which is statistically significant ($p = 0.012$).^{17,18} Therefore, although the difference between the treatments is smaller than the theory predicts, allowing subjects to choose the timing of their investment lowers the threshold for investment.

Interestingly, the same ANOVA also suggests that subjects have lower thresholds—on average 1.54 points ($p = 0.018$)—in Beliefs treatments than Regular treatments. Most of this difference is attributable to the SEQ treatment. If we condition the data to Regular treatments, only in the SEQ(2) treatment thresholds are significantly lower (at the 5% level) than in the SIM(2) treatment. Therefore, the act of eliciting beliefs may have caused these subjects to think more carefully about the strategic setting, allowing them to come closer to equilibrium behavior.¹⁹

Let us look at how thresholds vary with noise. In both treatments, we observe that thresholds increase with the noisiness of the signals (see Table 3.) This is consistent with our theoretical prediction for the SEQ game, yet inconsistent with the invariant threshold prediction for the SIM game under the assumption of risk neutrality. Increasing thresholds is consistent with subjects being risk averse. However, our subsequent analysis of beliefs casts doubt on the ability of risk aversion to rationalize our data.

6.3.2 COORDINATION RATES

We now investigate our second theoretical prediction: Endogenous timing should lead to significantly higher coordination rates (and significantly lower miscoordination rates) in

¹⁶Recall that for $e = 0$, any threshold $k_1 \in [25, 27]$ ($k_1 \in [25, 45]$) is an equilibrium for SEQ (SIM).

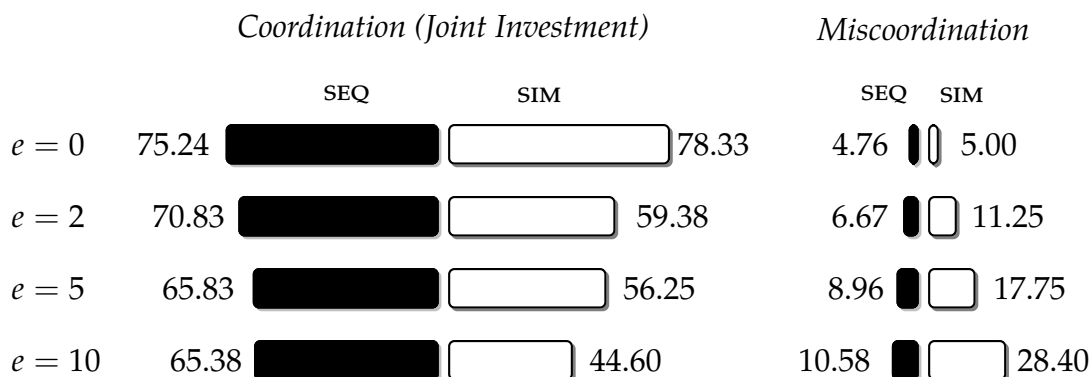
¹⁷The full results can be found in Table 9 in Appendix C.

¹⁸Pairwise tests of SEQ(e) vs. SIM(e) for $e \in \{2, 5, 10\}$ indicate that the difference is statistically significant for $e = 2$ ($p = 0.028$) and $e = 5$ ($p = 0.006$) but not for $e = 10$ ($p = 0.251$).

¹⁹Focusing only on the SIM treatment, although it was not significant, subjects had lower thresholds in Beliefs treatments. Therefore, eliciting beliefs did not cause a significant change in behavior — either towards or away from the theoretical prediction.

the SEQ game than in the SIM game. Also, coordination rates should go down, and miscoordination rates should go up as the noise increases.²⁰ Figure 5 documents our results and strongly confirms these predictions. For the SEQ treatments, we say that a pair of subjects coordinate if they have both invested by the last period, and they miscoordinate if one of them invests while the other never invests. In the left panel of Figure 5, we see the the frequency of coordination in each treatment. Similarly, the right panel displays frequencies of miscoordination. Indeed, the figure gives support to all of the theoretical predictions. In order to provide some statistical support, an analysis of variance with factors for treatment, noise, and whether or not beliefs were elicited shows that coordination rates are, on average, 0.113 higher in the SEQ game, and miscoordination rates are, on average, 0.095 lower in the SEQ game (see Table 9 in Appendix C for the ANOVA results). In both cases, taking the subject average as the unit of observation, the result is significant at $p < 0.01$.

FIGURE 5: COORDINATION & MISCOORDINATION RATES (LAST 20 ROUNDS)



To test the prediction that coordination rates are decreasing and miscoordination rates are increasing in the noisiness of signals, we run a regression of the coordination rate on the noisiness of signals as well as treatment (SEQ vs. SIM) and beliefs (Regular vs. Beliefs) dummies. We find that the coefficient of noise is significantly negative ($p < 0.01$), indicating that coordination rates go down as signals become noisier. The same regression, with the miscoordination as the dependent variable gives us a positive coefficient on the noise parameter, indicating that miscoordination rates go up as the noisiness of signals increase.

Of course, the reason that coordination rates are higher in the SEQ game is because subjects who choose to wait in period one have an additional period (or two) in which

²⁰Note that when $e = 0$, if subjects can agree on which of the continuum many equilibria to coordinate on, there should be no miscoordination. However, as can be seen in Figure 5, we still observe miscoordination rates of about 5%. It appears that much of this miscoordination arises because of multiple equilibria. For example, in SEQ(0), of the 20 instances of miscoordination over the las 20 rounds, in 11 of them the signal (and hence state) was between 25 and 27, which coincides with the set of equilibrium thresholds. The remaining 9 instances had signals outside of this range and can, therefore, be viewed as mistakes.

to revise their decision to invest. We do not have enough data to replicate our individual threshold analysis for the second period. Therefore, we take a regression approach, similar to that used by Heinemann et al. [31]. Specifically, we specify a logit model that determines the probability that a subject invests in period two after having waited and observed his match investing in period one by:

$$\Pr(\mathbf{I}|x, \mathbf{I}) = \frac{\exp(a + bx)}{1 + \exp(a + bx)}, \quad (3)$$

where x is the subject's signal. Note that for $x = -a/b$, we have $\Pr(\mathbf{I}|x, \mathbf{I}) = 1/2$; hence $-a/b$ is taken as the estimate of the threshold. The standard deviation of the threshold is $\pi/(b\sqrt{3})$. Therefore, the parameter b captures the sensitivity to one's signal. That is, higher values of b indicate *sharper* thresholds.

TABLE 4: ESTIMATED PERIOD-TWO THRESHOLDS (LAST 20 ROUNDS)

Treatment	a	b	Mean Threshold	S.D. of Threshold	Equilibrium Threshold	p -value (theory)
SEQ(0)	-42.49*** (-3.62)	1.56*** (3.61)	27.24	1.16	27.00	0.58
SEQ(2)	-6.65 (-1.38)	0.22 (1.22)	30.79	8.40	25.61	0.15
SEQ(5)	-9.09** (-2.43)	0.28** (2.00)	33.00	6.58	23.54	0.01
SEQ(10)	-3.35*** (-3.59)	0.14*** (4.36)	23.48	12.73	20.13	0.11

Robust z-statistics are in parentheses (clustering at the subject level).
 *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 4 shows that, in all four treatments, the estimated average threshold is higher than the theoretical prediction, but the difference is only statistically significant in the SEQ(5) treatment. There are two possible explanations. First, subjects' thresholds in period one were generally higher than the theoretical prediction, which means that the signal realizations are necessarily skewed rightward. Second, and more fundamentally, it appears that subjects only had a partial understanding of the information contained in observing their match invest in period one and may not have updated their beliefs about the state strongly enough.²¹ When we analyze beliefs in the next section, we will further investigate this latter claim.

²¹We say partial because the estimated threshold in SEQ(10) is less than 27, which is the cutoff for subjects to earn more than by choosing W , and also covering the cost of delayed investment.

6.3.3 WELFARE

The theoretical results suggest that as the noise, e , approaches zero, the equilibrium of the SEQ game becomes fully efficient, while in the SIM game inefficiency remains for a range of values of the state. Moreover, the difference of average welfare between the two treatments is increasing in e : In other words, as noise increases, endogenous timing enhances welfare.

For the analysis that follows we introduce some more notation. Let π^a denote the actual payoff received by a subject, including any costs due to delayed investment, and $\pi^{max} := \max\{25, \theta\}$ denote the ex post efficient payoff; i.e., both subjects invest in the first period if and only if $\theta \geq 25$. Also, let π^{eq} denote the payoff that a player would have received if subjects played according to the theoretical prediction.

For each of our treatments, Table 5 reports the ratios π^a / π^{max} and π^a / π^{eq} . Looking first at the left side of the table where we report π^a / π^{eq} , one can see that subjects in the SIM treatments actually earned more than the equilibrium payoff. This is because, subjects in these treatments used lower thresholds than the theoretical predictions. In exactly the opposite way, subjects in the SEQ treatments generally earned less than their equilibrium payoff because the subjects used higher thresholds than predicted by the theory.

TABLE 5: EFFICIENCY RATIOS (LAST 20 ROUNDS)

Noise	π^a / π^{eq}		π^a / π^{max}		COMPARISON	
	SIM	SEQ	SIM	SEQ	SEQ - SIM	PREDICTED
0	1.040	0.979	0.978	0.977	-0.001	0.059
2	1.009	0.976	0.943	0.963	0.020*	0.063
5	1.006	0.965	0.913	0.953	0.041***	0.073
10	0.980	0.985	0.858	0.928	0.070***	0.096

*** significant at 1%; * significant at 10%. $H_0 : \text{SEQ} - \text{SIM} = 0$.

The middle columns of Table 5 report π^a / π^{max} . We see that, except for $e = 0$, consistent with theoretical predictions, subjects in the SEQ game earn more than subjects in the SIM game, and the difference is statistically significant for all $e \geq 2$.²² Notice also that as noise increases, the difference in efficiency between the SEQ and the SIM increases, though not by as much as the theory predicts. In particular, if we estimate the following model via OLS,

$$\pi^a / \pi^{max} = \beta_0 + \beta_1 e + \beta_2 (e \times \mathbb{D}_{\text{SEQ}}) + \beta_3 \mathbb{D}_{\text{SEQ}} + \epsilon,$$

where \mathbb{D}_{SEQ} is the dummy variable that takes value 1 if it is a SEQ session, and 0 otherwise. We obtain $\beta_1 < 0$, $\beta_2 > 0$, both of which are significant at the 1% level. Thus, welfare

²²Letting t_{df}^e denote the t statistic when noise is e and the degrees of freedom are df , we have that $t_{76}^0 = -0.15$ ($p = 0.878$), $t_{38}^2 = 1.85$ ($p = 0.073$), $t_{86}^5 = 4.01$ ($p = 0.0001$) and $t_{100}^{10} = 7.67$ ($p = 0.000$).

declines as noise increases but, consistent with the theory, it declines significantly more slowly in the `SEQ` treatment.

7 USING BELIEFS TO UNDERSTAND OBSERVED BEHAVIOR

The results presented so far demonstrate that behavior is consistent with the theoretical predictions in many ways: Subjects do not violate dominance; subjects do not invest in later periods unless they observe investment by their match; subjects use monotone strategies, and thresholds vary in the correct way with noise, at least for the `SEQ` treatment; and, coordination rates are higher while miscoordination rates are lower in the `SEQ` game. Combining these results, we find that welfare is higher in the `SEQ` than in the `SIM`, with the difference increasing as signals become noisier.

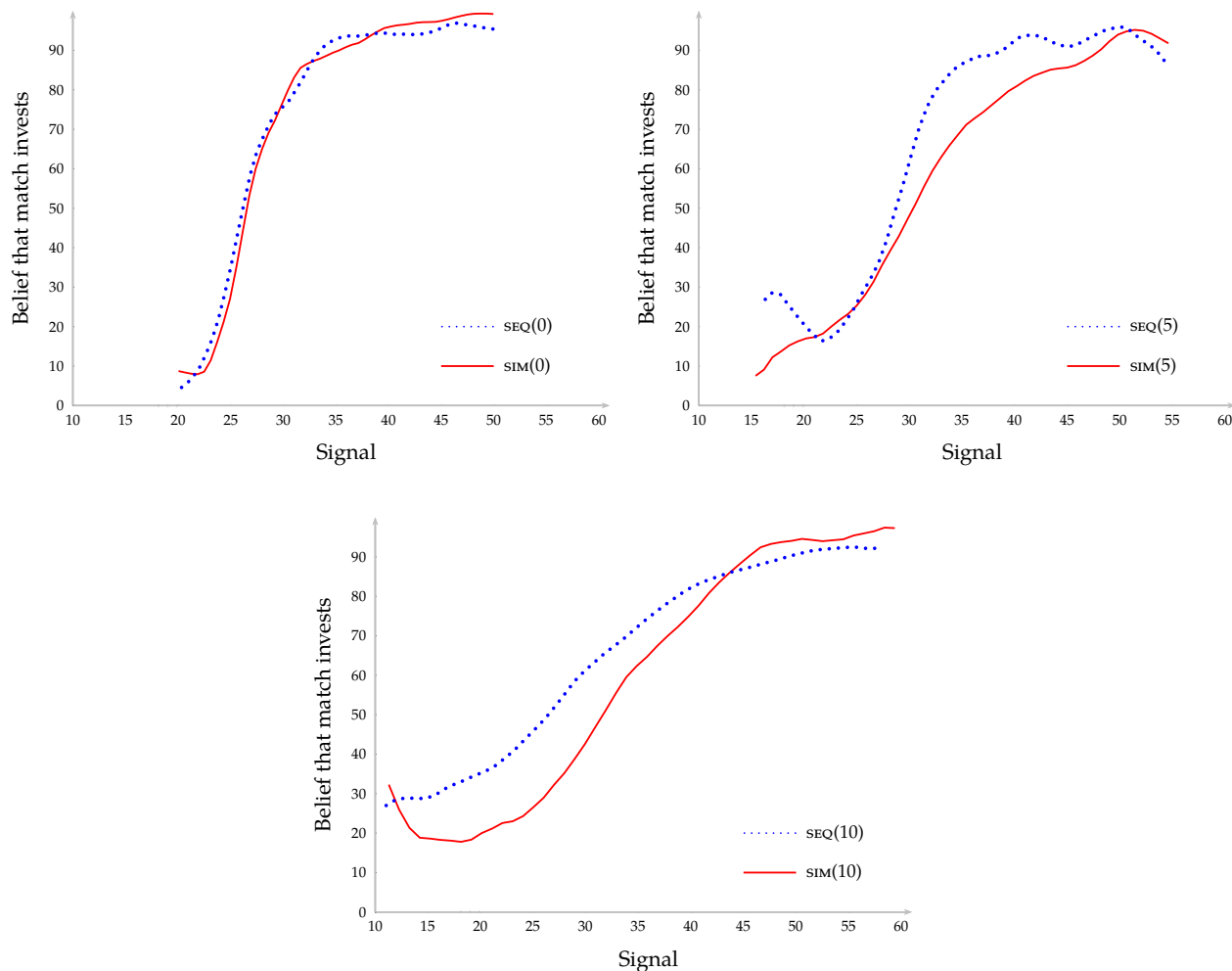
While these results are promising, there are some important questions that remain unanswered: (i) What is the source of the substantial heterogeneity in thresholds? (ii) Why are thresholds higher than predicted by the theory in the `SEQ` game but lower than predicted by the theory in the `SIM` game? (iii) Why don't the subjects fully appreciate the signaling value of observing their match invest in an earlier period? (iv) Why do thresholds vary so strongly with noise in the `SIM` treatment, contrary to the theoretical prediction? The Beliefs treatments were conducted to try to answer these questions.

7.1 BELIEFS IN PERIOD ONE

Figure 6 provides non-parametric estimates of beliefs about the likelihood that one's match would invest in period one. Not surprisingly, beliefs are monotone in signals, and they also appear to be sharper the lower the noise is. Furthermore, we observe that, subjects' beliefs in the `SEQ` treatment are, over a wide range of signals, higher than subjects' beliefs in the `SIM` treatment. In other words, for the same signal, subjects believed that it was more likely their match would invest in the sequential game. This is consistent with the theoretical results and our earlier experimental results.

Table 6 reports the results of a series of logit regressions where the dependent variable takes value 1 if the subject invested in the first period and the explanatory variables are the signal received and the subject's stated belief. The table reports marginal effects. Both a subject's signal about the state and his beliefs about the likelihood of his match investing have a positive effect on his investment decision. An increase in the signal by 1 point increases the probability of investing by between 0.75 and 2.3 percentage points. The effect of beliefs is much smaller: A one percentage point increase in one's belief increases the probability of investing by between 0.14 and 0.34 percentage points.

FIGURE 6: NONPARAMETRIC REGRESSIONS OF BELIEFS IN PERIOD ONE



The parameters used here correspond to those we used in our experiment. See Figure 3 and Section 5.

It also turns out that heterogeneity in beliefs explains a great deal of heterogeneity in thresholds. In particular, if we regress the threshold of a subject, reported in Table 3, on subjects' average beliefs that their match would invest, then the coefficient is negative and significant: For every percentage point increase in the average belief, the threshold decreases by 0.12 points ($p < 0.01$). That is, *confidence matters*: More confident subjects invested at lower thresholds. The relationship is present in both the `SEQ` and the `SIM` treatments, though it is stronger and has greater explanatory power in the `SIM` treatments.

We can also ask how frequently subjects best respond to their beliefs about their match's action. Denoting the belief that the match will invest by b , and the signal by x , we can write the expected payoff from investing as $EV_I = bx + (1 - b)(x - 20) = x - (1 - b)20$ in the `SIM` game. The expected payoff from not investing is always $EV_W = 25$ because of the cost

TABLE 6: HOW BELIEFS & SIGNALS JOINTLY DETERMINE INVESTMENT (LAST 20 ROUNDS, MARGINAL EFFECTS)

Parameter	SEQ(0)	SEQ(5)	SEQ(10)	SIM(0)	SIM(5)	SIM(10)
signal	0.0125*** (2.69)	0.0187*** (7.52)	0.0172*** (5.29)	0.0075*** (3.42)	0.0232*** (3.42)	0.0186*** (5.59)
belief	0.0015*** (7.90)	0.0014*** (4.73)	0.0029*** (3.41)	0.0012*** (8.35)	0.0029** (2.17)	0.0034*** (5.71)

Robust z-statistics are in parentheses (clustering at the subject level).
 *** significant at 1%; ** significant at 5%; * significant at 10%.

of waiting. The subject is best-responding if $EV_I \geq EV_W$ and he invests, or if $EV_I < EV_W$ and he does not invest. For the SEQ game, we cannot determine whether a subject is best-responding in period one, because the relevant belief is the probability that his match will invest in period one *or period two*, which should be weakly larger than the belief we elicited. Moreover, the expected value from waiting will differ from 25. Table 7 reports exact best-response rates for the SIM game and approximate best-response rates (using the exact criterion in the SIM game) for the SEQ game. We also report the frequency with which subjects chose to invest conditional on not best responding to their beliefs.

TABLE 7: (APPROXIMATE[†]) BEST-RESPONSE RATES (LAST 20 ROUNDS)

Criteria	SEQ(0)	SEQ(5)	SEQ(10)	SIM(0)	SIM(5)	SIM(10)
Best-Response Rate	0.945 (0.086)	0.910 (0.105)	0.803 (0.159)	0.977 (0.041)	0.881 (0.120)	0.900 (0.114)
Frequency of I Conditional on Not BR	0.950 (0.158)	0.929 (0.267)	0.747 (0.426)	1.000 (0.000)	0.804 (0.394)	0.731 (0.446)

Standard deviations in parentheses.

[†] We classified a decision as a best-response if, according to the elicited period one belief, $EV_I \geq EV_W$ and the subject invested, or $EV_I < EV_W$ and the subject did not invest. For the SIM treatments, this is the definition of a best-response, while for the SEQ treatments, as noted in the main text, this measure is only approximate.

We see in Table 7 that, in all six treatments, the best-response rate is over 80%. Thus, even though subjects in the SIM treatments invest more frequently than the theoretical predictions, their actions are largely consistent with their stated beliefs. Similarly, for the sequential game, actions are quite consistent with the beliefs according to this approximate measure of best-response. In the SEQ treatment, best-response rates decline as the noisiness of signals increases, while in the SIM treatment, the relationship is non-monotonic. However, there is a large difference between complete information ($e = 0$) and incomplete information ($e > 0$). We can gain further insight by examining behavior when subjects were not best responding to beliefs. We observe that, when subjects were not best-responding,

between 73% and 100% of the time they chose to invest when not investing was optimal according to their beliefs. Both results suggest that risk aversion cannot rationalize behavior in the `SIM` game as it would make *not investing* more attractive for the same beliefs.

REMARK 1. *To further investigate the ability of risk aversion to explain our results, for the `SIM` treatment, we estimated a simple model of stochastic best-response where we assume the utility function is $u(w) = w^\gamma$. In this case, the expected utility of investing is $\mathbb{E}[u|x] = b \int_{x-e}^{x+e} \theta^\gamma f(\theta|x) d\theta + (1-b) \int_{x-\eta}^{x+\eta} (\theta - 20)^\gamma f(\theta|x) d\theta$ where x is the signal received and b is the belief that their match will invest, while the utility of not investing is $u^w = 25^\gamma$. We estimated $\hat{\gamma} > 1$, indicating that risk-seeking behavior better-explains the data. A similar result for a `CARA` utility function holds.*

7.2 BELIEFS IN PERIOD TWO

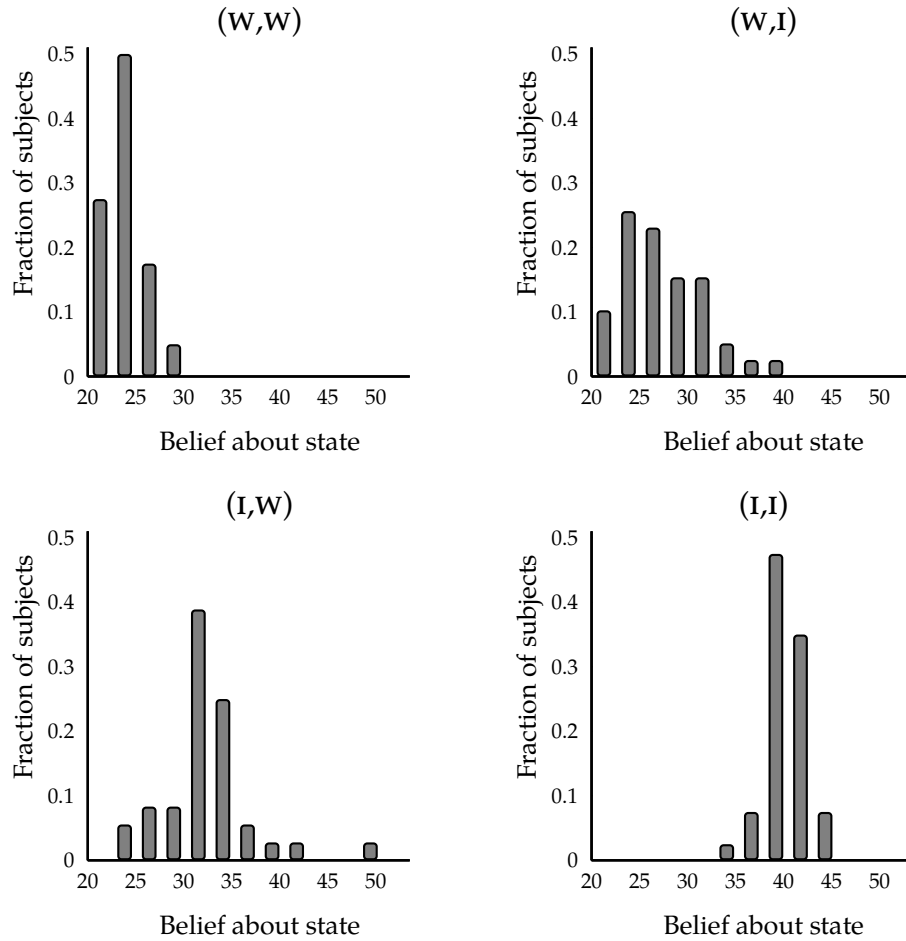
In the `SEQ` treatments, the period one choice of one's match contains valuable information: Investment implies that the match received a signal above a given threshold, which should increase the subject's belief about the state. On the other hand, observing that the match did not invest, reveals that his signal was lower than a given threshold, which should lower the subject's belief about the state. Figure 7 plots histograms of subjects' average beliefs about the state after observing period one choices, conditional on each of four possible period one outcomes. The heading above each subfigure indicates the history in the form (own action, match's action). Therefore, for each row, comparing across columns gives the impact of observing that one's match invested. As can be seen, there is a clear rightward shift, indicating that subjects became more optimistic upon observing investment. Conditional on not investing in period one, the average belief about the state when observing that one's match did not invest was 23.77, while it was 26.91 when observing that one's match did invest.²³ Moreover, the difference is statistically significant (paired t -test): $t_{38} = 5.14, p < 0.01$. The difference in beliefs about the state is even larger when conditioning of one having invested in period one: 32.12 when observing that one's match did not invest vs. 39.70 when observing that one's match did invest ($t_{35} = 11.01, p < 0.01$). Thus, observing one's match invest leads to significantly greater optimism about the state.

Table 8 reports a regression that determines the relationship between the period one outcome and beliefs about the state in period two, which allows us to capture the role that one's signal plays in determining period two beliefs. In all cases, the dependent variable is one's belief about the state and we estimate random effects models.²⁴ The first two

²³ Breaking up the average belief about the state after observing investment by the noise parameter, we see that it is only 26.3 when $e = 5$, while it is 27.5 when $e = 10$. Therefore, on average, beliefs about the state are lower than the safe payoff plus the cost of delayed investment when $e = 5$. This may partly explain the non-monotonicity in period two thresholds (as a function of e) that we reported in Table 4.

²⁴ Beliefs were constrained to being on $[\max\{20, est - e\}, \min\{50, est - e\}]$ but censoring was a rare occurrence. In any case, random effects Tobit regressions give qualitatively similar results.

FIGURE 7: BELIEFS ABOUT THE STATE AFTER OBSERVING PERIOD ONE CHOICES



The heading above each subfigure denotes (own action, match's action).

columns condition on the subject having chosen to wait in period one, while columns (3) – (6) include all data.

Looking at columns (1) and (3), we see that the direct effect of observing one's match invest is generally positive, though it is only significant when we include all the data. Columns (2) and (4) paint a more subtle picture. In particular, there is a positive interaction between one's signal and observing investment: The effect of observing investment is stronger, the higher one's own signal was. Somewhat surprisingly, even controlling for the signal and the behavior of the match, we see again that confidence matters. Specifically, we see that subjects who invested in period one have higher beliefs about the state.²⁵ The effect is similarly larger, the larger one's own signal is.

Figure 8 plots the estimated increase in beliefs due to observing investment as a func-

²⁵The direction of causality may be difficult to determine. In theory, higher beliefs about the state should make investment more likely; however, since we elicited beliefs about the state after the period one decision, it could be that subjects stated beliefs that "justify" their period one decision.

TABLE 8: BELIEFS ABOUT THE STATE, CONDITIONAL ON PERIOD ONE ACTIONS

Parameters	(1)	(2)	(3)	(4)	(5)	(6)
$e = 10$	1.765*** [0.447]	1.692*** [0.423]	0.202 [0.302]	0.324 [0.299]	0.384 [0.295]	0.372 [0.304]
match inv.	0.892 [0.762]	-5.098*** [1.643]	1.791*** [0.501]	-1.725* [0.901]	-2.952*** [0.956]	-1.409 [0.900]
signal	0.616*** [0.047]	0.472*** [0.060]	0.792*** [0.025]	0.708*** [0.034]	0.636*** [0.033]	0.554*** [0.044]
match inv. \times signal		0.242*** [0.065]		0.117*** [0.030]	0.148*** [0.029]	0.101*** [0.026]
inv.					1.877*** [0.395]	-2.561** [1.179]
(inv.) \times signal						0.153*** [0.038]
constant	8.847*** [1.007]	12.231*** [1.319]	5.608*** [0.687]	7.824*** [0.889]	9.019*** [0.856]	10.979*** [1.052]
R^2	0.69	0.71	0.87	0.87	0.87	0.88
N	489	489	1,600	1,600	1,600	1,600

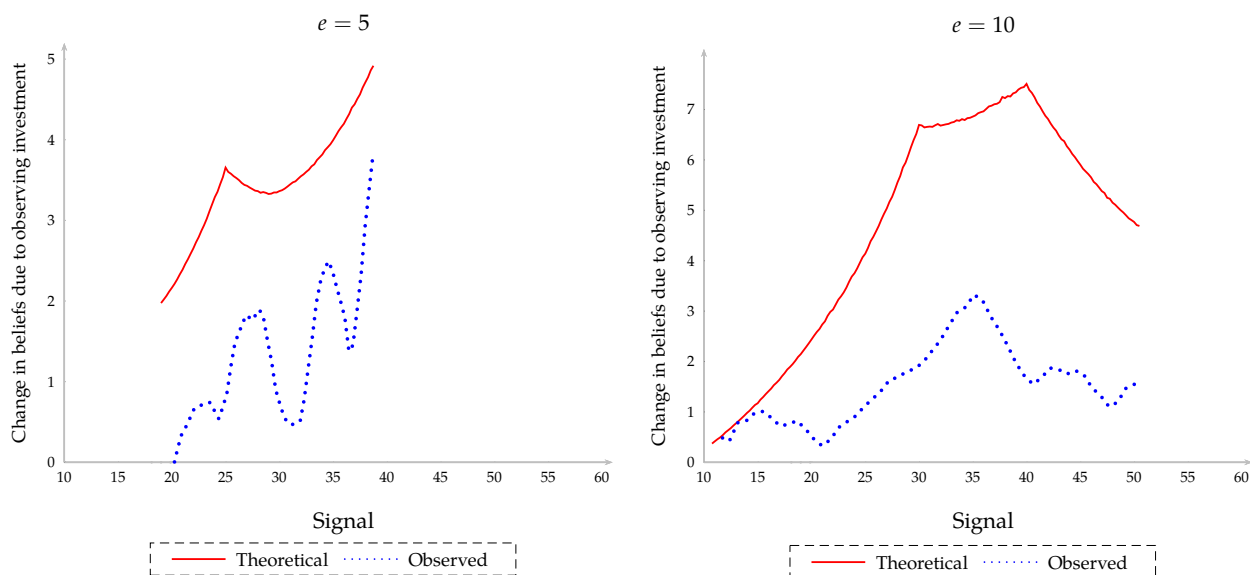
Robust standard errors are in brackets (clustering at the subject level).
 *** significant at 1%; ** significant at 5%; * significant at 10%.

tion of the signal, as well as the increase in beliefs that should occur if Bayesian subjects were playing according to the equilibrium.²⁶ For both values of the noise parameter, the estimated relationship matches closely to the theoretical benchmark in terms of functional form, yet there is a strong level effect; i.e. subjects do not update their beliefs about the state as much as the theory predicts. That is, subjects fail to appreciate the signaling value of observing the investment of their match.

One remaining question is how the interaction between beliefs and the observed investment decision of the match in period one affects the period two investment decision. A logit regression of the period two decision on one's signal, one's belief about the state, and one's match's period one investment decision shows that, by far, the largest effect is the decision of the match. Specifically, observing that one's match invests increases the probability of period two investment by 26.6%; hence, despite the fact that subjects do not fully appreciate the value of observing investment, it still is a crucial determinant of the second period decision. Additionally, beliefs matter: For every unit increase in beliefs, the

²⁶The estimate was obtained by estimating a local polynomial regression of beliefs on signal conditional upon observing one's match invested and conditional on not observing investment, and then taking the difference. Theoretical values were obtained via simulation assuming equilibrium behavior.

FIGURE 8: THE IMPACT OF OBSERVING INVESTMENT ON BELIEFS (ESTIMATED VS. THEORETICAL)



probability of investing increases by almost 1% (and the effect is 2.85% if we restrict the sample to only those who observed period one investment by their match). Both effects are significant at the 1% level. There is also weak evidence (i.e., significant at the 10% level) that subjects responded more strongly to observing their match invest in $\text{SEQ}(10)$ than in $\text{SEQ}(5)$, and that subjects were more likely to invest after observing their match invest in later rounds. The former result is consistent with the theory: The period one threshold is higher in $e = 10$, which means that observing investment is a *stronger* signal, which should encourage more period two investment. The latter result is consistent with learning. Interestingly, when controlling for beliefs and the decision of one's match, one's signal does not have a direct effect on the probability of investing.²⁷ Thus, one important factor holding down the efficiency of the SEQ game is the fact that subjects *under-appreciate* the information revealed by their match's investment. Moreover, heterogeneity in belief updating (as seen in Figure 7) can explain some of the heterogeneity in period two behavior.

8 DISCUSSION

The results of the previous two sections paint an interesting picture. Subjects seem to grasp many of the underlying strategic principles of the game, yet there are notable differences and substantial subject-specific heterogeneity. Our analysis of beliefs has also given us several insights. First, confidence matters: Subjects with higher average beliefs had lower thresholds for investment in period one. Second, heterogeneity in beliefs explains a part of the heterogeneity in thresholds. Third, although subjects deviate from the theory,

²⁷Of course, there is an indirect effect via beliefs.

their actions are highly consistent with stated beliefs. Moreover, as Table 7 and Remark 1 show, risk aversion cannot explain the data. Fourth, subjects do not fully appreciate the information revealed by their match’s investment. This is a key factor in explaining why period two thresholds are higher than predicted by the theory.

Our analysis of beliefs also points to one reason why the difference between the *SIM* and *SEQ* games is smaller than predicted: Subjects in the *SIM* game are over-confident relative to the global games prediction about the likelihood that their match will invest, and this leads to lower observed thresholds. However, we believe that there is more to the story. In particular, we conjecture that there are some subjects that we will call *confidence seekers* who, upon receiving a signal for which immediate investment is optimal, prefer to wait to observe the decision of their match. Confidence seekers have been shown to exist in other experiments. For example, Eliaz and Schotter [21] show that many subjects are willing to pay for information, even though the *ex ante* optimal action remains optimal *regardless* of the information revealed.

Confidence seeking behavior may express itself in a couple of distinct ways. First, our theory predicts that the period one threshold of the *SEQ* should be below the threshold of the *SIM*. To the extent that this is not true, it is suggestive of confidence seeking. That is, confidence seeking subjects in the *SEQ* may prefer to use the same (or possibly higher) threshold than in *SIM* in order to observe their match’s decision. In our Regular treatment, only when $e = 2$ do we find significantly lower thresholds in the *SEQ* game than in the *SIM* game ($p = 0.028$).²⁸ Thus, thresholds are not consistently lower in the *SEQ* than in the *SIM*, which provides some suggestive evidence in favor of our confidence seekers hypothesis.

Second, observe that while we would expect subject-specific heterogeneity in both the *SEQ* and *SIM* treatments, only in the *SEQ* treatment, where subjects have the opportunity to delay investment, can confidence seeking behavior be expressed. Therefore, to the extent that there is greater variation in behavior in the *SEQ* treatment than in the *SIM* treatment, this is further indication that such behavior may explain our results. As noted in Footnote 15, the standard deviation of the estimated thresholds is higher in the *SEQ* treatment than in the corresponding *SIM* treatment except when $e = 5$. Furthermore, the difference is statistically significant for $e \in \{0, 10\}$.

To further investigate our confidence seekers hypothesis, we now look briefly at the results of our within-subjects session, in which subjects first play 30 rounds of the *SIM*(10) game, followed by 25 rounds of the *SEQ*(10) game. Consistent with the theoretical prediction, the average threshold is significantly lower in the *SEQ* game ($p < 0.01$). However, 7 out of 20 subjects actually have a *higher* threshold in the *SEQ* game. This is all the more

²⁸Recall that eliciting beliefs appears to influence behavior asymmetrically between the *SEQ* and the *SIM* games, so we focus here on the Regular treatment.

surprising because subjects first play the SIM game, which, in our opinion, should make it even more likely that subjects lower their threshold in the SEQ game. We, therefore, feel confident labeling these 7 subjects as confidence seekers, and that their presence, along with the general over-confidence in the SIM game are the two main factors explaining why the SEQ game leads to a more modest welfare improvement.

9 CONCLUDING REMARKS

We analyze a simple global coordination game with endogenous timing in order to understand the effect of timing on coordination under asymmetric information. The theoretical analysis highlights two forces behind timing decisions: learning and complementarities. We show that in the equilibrium, optimistic players invest first and convey information about the profitability of investment. In addition to this learning effect, the investment eliminates the strategic uncertainty, and encourages investment through strategic complementarities. The normative analysis indicates that endogenous timing leads to an efficient outcome, especially as players' private information become infinitely informative. Moreover, the average difference in welfare between the simultaneous and the sequential game increases as signals become less informative.

Our experiment confirms many of the main theoretical predictions: Subjects respect dominance, they do not invest after period two and they only invest in period two, after observing investment in period one by their match. Moreover, majority of subjects use monotone strategies. Beyond this, many treatment differences go in the direction predicted by the theory: Thresholds are lower in the SEQ than in the SIM, coordination rates are higher and miscoordination rates are lower in the SEQ than in the SIM, and welfare is higher in the SEQ than in the SIM, with the difference increasing as signals become noisier.

Despite this strong concurrence with the theoretical predictions, there is substantial subject-specific heterogeneity and instances where behavior and theory do not match. For example, part of the reason for our welfare result is due to the fact that behavior in the SIM treatment is influenced by noise, which is not predicted by the theory. By eliciting beliefs, both about the likelihood that their match would invest in period one, and also about the value of the state variable (after observing period one decisions), we are able to gain a deeper understanding for the observed behavior. In particular, a great deal of the heterogeneity in thresholds arises due to heterogeneity in beliefs: More confident subjects invest at lower thresholds. Moreover, while thresholds both in the SEQ and the SIM treatments deviate from theory, the vast majority of the time, subjects' investment decisions are actually best responses, given their beliefs. Thus, the significantly lower thresholds in the SIM treatment appear to be due to over-confidence about the likelihood that their match would

invest (relative to the theoretical prediction). Interestingly, this over-confidence appeared to decline as signals became noisier, leading to thresholds closer to the equilibrium prediction. The precise reason for this remains unclear. Furthermore, although observing one's match invest in period one is the decisive factor determining one's period two investment, our analysis of beliefs show that subjects fail to sufficiently update their beliefs about the state. These last two points go a long way in explaining why the differences between the simultaneous and sequential games are smaller than predicted by the theory.

Given our results, we believe that the behavioral differences between static and dynamic global investment games are sufficiently strong to justify a continued focus on behavior in dynamic games, especially in environments with asymmetric information. We also believe that further study designed to understand the belief formation process in greater detail in such games would be fruitful.

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APPENDIX

A. EXPERIMENTAL INSTRUCTIONS

GENERAL INSTRUCTIONS

This is an experiment on the economics of decision-making. Your earnings will depend partly on your decisions, partly on the decisions of others and partly on chance. By following the instructions and making careful decisions you will earn varying amounts of money, which will be paid at the end of the experiment. Details of how you will make decisions and earn money are explained below.

In this experiment, you will participate in 40 **independent** decision problems (rounds). In all rounds, you will be **randomly** matched with another participant. In what follows, we will refer to the person with whom you are matched as your *match*. After each round, you will be **randomly** matched with another participant for the next decision problem, and so on. At no point in the experiment will you know the identity of your matches.

DECISION PROBLEM

In each round you will be asked to make a choice between two alternatives A and B. Your match will face the same choice problem. Your decision and your match's decision result in the following earnings (the explanation of Q will be given later):

- If you choose A and your match chooses A: You earn Q and your match earns Q points.
- If you choose A and your match chooses B: You earn $Q - 20$ and your match earns 25 points.
- If you choose B and your match chooses A: You earn 25 and your match earns $Q - 20$ points.
- If you choose B and your match chooses B: You earn 25 and your match earns 25 points.

The following table lists your alternatives A and B in the rows, and your match's alternatives in the columns. For example, the situation in which you play A and your match plays B corresponds to the upper right cell. The numbers in that cell indicate the payoffs. The first number is your payoff (**boldfaced**) and the second number following the comma is your match's payoff (*italicized*). For instance, in the previous example, you earn a payoff of $Q - 20$ and your match earns 25 .

	A	B
A	Q, Q	$Q - 20, 25$
B	$25, Q - 20$	$25, 25$

WHAT IS Q ?

When you play A, your earnings depend on your match's decision and on Q . Q is a number (up-to two decimals) between 20 and 50 randomly determined by the computer. That means any number

between 20 and 50 is equally likely to be picked by the computer.

The computer picks Q before each round and the numbers are independent across rounds. That is, the Q chosen by the computer in the first round does not play any role on what Q will be in other rounds.

Before you make a decision you will not be told what Q is but instead you will receive an estimate of Q , which we will denote by E . Let's be more precise. After the computer randomly determines Q , it also picks a random number (up-to two decimals) between $Q - 5$ and $Q + 5$. This is your estimate E . Any number between $Q - 5$ and $Q + 5$ is equally likely to be picked by the computer. Although E does not tell you what Q exactly is, it gives an estimate of it. For example if you receive an estimate $E = 32.73$, then you know that Q is **not less than** $32.73 - 5 = 27.73$ and it is **not more than** $32.73 + 5 = 37.73$.

Note that although Q will be the **same** for both you and your match, your **estimates** can be different. That is, for the same Q , the computer also randomly picks another estimate exactly in the same manner for your match. Your estimate and your match's estimate are chosen independently. Therefore, it is very likely that they will be different numbers; however, both estimates will be between $Q - 5$ and $Q + 5$.

YOUR DECISION

After you are given your estimate, E , you are ready to make a decision. There are 3 stages in which you can finalize your decision. Note that both Q and E are **fixed** for all three stages for both you and your match. In each stage, you can choose either A or B . Choosing A is **irreversible**, while choosing B is **reversible**. That means choosing A in any stage ends the round and your earnings for that round are determined according to the table we discussed above. However, if you choose B , in either stage 1 or stage 2, then you will be allowed to revise your choice in the following stage. Note that for each stage that you choose B , your payoff will be reduced by **2 points** in case you end up choosing A . For example, if you choose B in stages 1 and 2, and then choose A in stage 3, $4 = 2 \times 2$ points will be subtracted from your earnings. On the other hand, if you also chose B in stage 3, then no extra points will be subtracted.

In any given stage, you will not observe the decision taken by your match in that stage, but you will observe decisions from **earlier** stages. For example, consider the screen below. It is currently the second stage, and as you can see, both you and your match chose **B** in the first stage; you also see that your estimate of Q is 34.02. However, you do not see your match's choice in stage 2. Since B is reversible, both you and your match can choose between A and B in stage 2.

PAYOFFS

Your potential earnings in each round depends on your choice, on your match's choice, and on Q as well as the timing of your choices. After both you and your match have made your choices, you will see the following screen. On the left, you see your estimate of Q , the true value of Q , and your profit; while on the right, you see the choices of both you and your match made in each of the 3

stages. In this example, you see that while your estimate of Q was 34.02, its true value was 33.87. You also see that your profit was 31.87: since both you and your match eventually chose A , you received $Q = 33.87$ points, but since you chose B in stage 1, 2 point was subtracted from this total.

At the end of the 40 rounds, we will add all your earnings in order to determine your total points. This total will be converted to a dollar amount according to the rule:

$$\$1 = 100 \text{ points}$$

This amount will then be added to the \$8.00 participation fee to give your final payment. Payments will be made in private via petty cash vouchers after the completion of the experiment.

RULES

Please do not talk with anyone during the experiment. We ask everyone to remain silent until the end of the last decision problem.

Your participation in the experiment and any information about your earnings will be kept strictly confidential. Your receipt of payment and the consent form are the only places on which your name will appear. This information will be kept confidential in the manner described in the consent form.

If you have any questions please ask them now. If not, we will proceed to the experiment.

B. WELFARE

The value of `sim` for a player i is:

$$v_i^{\text{sim}}(\theta) := (1 - F_{X_i}(\kappa|\theta))(\theta - rF_{X_j}(\kappa|\theta)) + sF_{X_i}(\kappa|\theta).$$

Conditional on θ , player i receives a signal above the threshold κ with probability $(1 - F_{X_i}(\kappa|\theta))$. Note that player j also invests if his signal is above κ . Therefore, in this case, player i 's investment yields an expected payoff of $(1 - F_{X_j}(\kappa|\theta))\theta + F_{X_j}(\kappa|\theta)(\theta - r) = (\theta - rF_{X_j}(\kappa|\theta))$. Otherwise, with probability $F_{X_i}(\kappa|\theta)$, he does not invest and receives the safe payoff s .

Similarly, we define the value of `seq` for player i as

$$v_i^{\text{seq}}(\theta) := (1 - F_{X_i}(k^1|\theta))(\theta - rF_{X_j}(k^2|\theta)) + (F_{X_i}(k^1|\theta) - F_{X_i}(k^2|\theta)) \left[(1 - F_{X_j}(k^1|\theta))(\theta - c) + sF_{X_j}(k^1|\theta) \right] + sF_{X_i}(k^2|\theta).$$

Conditional on θ , a player receives a signal above k^1 with probability $(1 - F_{X_i}(k^1|\theta))$ and invests with an expected value of $(\theta - rF_{X_j}(k^2|\theta))$. If his signal is between the thresholds k^1 and k^2 —which happens with probability $(F_{X_i}(k^1|\theta) - F_{X_i}(k^2|\theta))$ —his investment decision depends on the action of player j in the first period: (i) Player j invests in period one with probability $(1 - F_{X_j}(k^1|\theta))$, and Player i receives a payoff of $\theta - c$ by investing in the second period, (ii) Player j does not invest in period one with probability $F_{X_j}(k^1|\theta)$, and Player i receives s by not investing at all. Finally, Player i receives a signal below k^2 with probability $F_{X_i}(k^2|\theta)$ and he never invests.

C. SUPPORTING REGRESSION TABLES

TABLE 9: ANOVA RESULTS FOR THRESHOLDS, COORDINATION AND MISCOORDINATION (LAST 20 ROUNDS)

Factors	Threshold	Coordination	Miscoordination
TREATMENT			
SEQ	BASE	BASE	BASE
SIM	-1.440** [0.567]	0.113*** [0.024]	-0.095*** [0.016]
NOISE			
0	NA	BASE	BASE
2	BASE	-0.076* [0.042]	0.016 [0.028]
5	2.423*** [0.856]	-0.154*** [0.032]	0.055** [0.022]
10	3.576*** [0.824]	-0.207*** [0.031]	0.120*** [0.021]
BELIEFS			
No	BASE	BASE	BASE
Yes	-1.538** [0.646]	0.004 [0.026]	0.005 [0.020]
CONS	30.420*** [0.753]	0.695*** [0.030]	0.112*** [0.020]
N	228	280	280
R^2	0.118	0.212	0.219