An Experiment of Social Learning with Endogenous Timing*

Boğaçhan Çelen† Kyle Hyndman‡

Columbia University Maastricht University

May 2, 2012

Abstract

In this paper, we experimentally investigate a social learning model with endogenous timing. Specifically, we focus on a model, in which two subjects are supposed to make a binary decision. One alternative is a safe action with a fixed payoff, while the other alternative is a risky action. The subjects can make their decisions in three stages. The safe action is reversible, but the risky action is not. A subject who delays his decision can observe the decision of the other subject in the earlier stages, and as a result, acquire more information. We show that players do delay their decisions in order to obtain more information. Furthermore, they delay especially when their private information does not particularly support the risky action. We also find evidence which suggests that risk aversion plays an important role in timing decisions, often leading to ex post inefficient outcomes.

JEL Classification Numbers: C72, C91, D82, D83.

Keywords: Delay, information, experiment.

*This research was supported by the Center for Experimental Social Sciences and the C. V. Starr Center at New York University.

†Graduate School of Business, Columbia University, 3022 Broadway, 602 Uris Hall, New York, NY 10027. E-mail: bc2132@columbia.edu, url: http://celen.gsb.columbia.edu.

‡Department of Economics, (AE1), Maastricht University, Room A1.06, Tongersestraat 53, Maastricht 6211 LM, Netherlands. E-mail: k.hyndman@maastrichtuniversity.nl, url: http://www.personeel.unimaas.nl/k-hyndman.

1 Introduction

Timing of a decision is often at least as informative as the decision itself. In fact, an early investment decision reveals a lot about the information about the profitability of this investment, since an optimal decision balances the marginal cost of delay and the marginal benefit of information that may become available in time. The pioneering works of Chamley and Gale [7] and Gul and Lundholm [9] analyze the problem of endogenous timing in a simple social learning environment. In fact, these two papers are quite different in their approaches. Gul and Lundholm [9] study a model, in which players are supposed to make a forecast of the true state of the world. The timing of players' decision is important because it is more costly to make a forecast later. Also, the cost is higher for higher states. They show that when players choose the timing of their decisions, their actions become clustered together. Moreover, in this equilibrium, information is used efficiently. The fact that cost of delay depends on the state leads to a natural sequence of timing decisions among the players: Those who have higher information tend to move earlier. More importantly though, those players who move early update their forecasts, in order to account for the information that is revealed by the observation that other players *have not* make a decision yet. In addition, those players who move later, benefit from the information that is revealed by the early actions. This is the main reason that the game has an efficient equilibrium. In contrast, Chamley and Gale [7] study an investment model, in order to show the inefficiencies that arise, as a result of the strategic role of timing. In the model, the return of the investment is uncertain. Each player can randomly receive an option to invest. Moreover, the number of options is determined randomly but correlated with the return of the investment. For a player, receiving an option is a private information, and as a result, exercising the option reveals information regarding the return of investment. Since all agents—who have an option—are identical, either the equilibrium exhibits delay, in which case early investors cannot benefit from available information, or there is investment collapse and no information is revealed. In other words, the equilibrium cannot be efficient.

In this paper, we investigate the strategic role of timing in a controlled experiment. Specifically, we test a model of of pure information externality in which two subjects decide if and when they want to take a risky and irreversible action. The default option of the subjects is a safe action that yields a fixed payoff. The players have asymmetric information about the return of the risky action. Therefore, those who have information which favors the risky action, prefer not to delay in order to acquire information. The experiment allows us to test subjects' propensity to delay in order to acquire information. Also, we aim to detect other variables or behavioral patterns that are instrumental in timing decisions.

In our preliminary tests, we find that the subjects almost never make completely unreasonable decisions. Moreover, we observe that subjects exhibit significant consistency in using information. To be precise, subjects respond to information in a monotonic way: If a subject decides to take the risky action given his private information, then he does not reverse this decision in other independent rounds if his information favors the risky action more. In the experiment, there are three stages in which subjects can decide whether they want to take the risky action. In equilibrium, the third stage is redundant, and in fact we observe negligibly few instances (.08%) of risky action in the third stage. When we analyze the aggregate behavior in the first stage, we find that the theory predicts the actual behavior quite well. In addition, the behavior in the lab is consistent with all the comparative statics. On the other hand, when we analyze the data at the individual level, we find that there is heterogeneity in subjects' behaviors. We attribute this heterogeneity to differences in risk preferences. The analysis of the second stage behavior is consistent with this assessment. In sum, we show that subjects delay in order to acquire more information in the second stage. Moreover, there is strong evidence that the behavior is different across subjects due to their risk preferences: Those who are more risk averse delay more. This excessive delay often leads to inefficient outcomes.

Although there is a large experimental literature studying different aspects of social learning models that are pioneered by Banerjee [2] and Bikhchandani [3] et al., there are very few experiments that test endogenous timing in pure information externality environment.[1] Sgroi [12] is the first, and the only other, paper that experimentally analyzes timing decisions in a social learning environment. Consistent with our findings, Sgroi [12] shows that subjects delay especially when their private information is not strong enough. Moreover, he observes herding behavior, which sometimes occur on the wrong action.

Section 2 describes the model (2.1), and briefly discusses the equilibrium of the game (2.2). Section 3 discusses the design of the experiment (3.1), and provides the analysis of the data (3.2 - 3.5). We conclude in Section 4.

2 Theory

2.1 Timing Game

There are two players $i = 1, 2$ who are supposed to make a decision between two alternatives A and B. The decision process consists of three periods. In each period, each player i is supposed to choose an action. The action A is *irreversible*, but B is reversible. That is,

[1] For experiments on social learning see Anderson and Holt [1], Hung and Plott [10], Çelen and Kariv [5] and Kübler and Weizsäcker [11].

when a player chooses the action A, he cannot change his action in subsequent periods. However, if he chooses B, he can still choose action A in later period(s). The action A is costly in periods $t > 1$: if a player plays A for the first time in period t he pays a cost $(t-1)c > 0$.

The return of action A is determined by a random variable Θ. However, action B is safe and its payoff is normalized to s. Therefore, if a player chooses B in all periods, his payoff is s. If, however, a player chooses A in period t his payoff is $\theta - (t-1)c$, where θ is a realization of Θ. Each player's prior beliefs about Θ is represented by the uniform distribution over the interval $[a, b]$. Let us assume that the safe payoff $s \in (a, b)$, so that the problem is not trivial. The true state, θ, is determined prior to any decision. Following the realization of θ, each player i receives a private signal $x_i = \theta + \epsilon_i$. We assume that ϵ_i is a realization of a draw from the uniform distribution over $[-e, e]$ for some $e > 0$. We assume that ϵ_i's are independent; therefore, conditional on the state θ, the signals are independent for $i = 1, 2$.

2.2 Equilibrium

The problem that a player faces in the above game is extensively and rigorously discussed in Chamley and Gale [7] and Chamley [6]: By delaying to commit to action A, a player can infer valuable information from the decision of the other player. Yet, delaying is costly. Therefore, the optimal decision is determined by this trade-off.

The equilibrium analysis of the model is quite standard. For more general models, we refer the reader to Chamley [6] and Brindisi et al. [4]). In what follows, we discuss the characterization of the perfect Bayesian equilibrium of the game, and summarize the main findings. In particular, we confine our attention to monotone strategies. A monotone strategy simply states that, given a history, if a player takes action A for a signal x_i, then so does he for signals greater than x_i. Thus, a monotone strategy is characterized by thresholds, one for each possible history. At a given history, if the signal is above the threshold, the optimal action is A, otherwise it is B.

The analysis is simplified by the feature that Chamley and Gale [7] call *one-step property*. This property states that if both players play B in the first period, then they should never revise their decision to A in any of the succeeding periods. The idea behind one-step property is simple. If a player uses a monotone strategy, observing him taking action B reveals that his signal is not high enough to play A. Therefore, this observation can only lead to more pessimistic beliefs about the payoff of action A. Fortunately, any Bayesian perfect equilibrium of the timing game in monotone strategies satisfies one-step property.

One immediate corollary of one-step property is that the game ends in two periods. To be precise, there is no equilibrium in which s player switches from B to A in period 3;

i.e. there is no decision to be made in period 3. As a result, an equilibrium in monotone strategies can be characterized by two thresholds, $\kappa_i^1 \geq \kappa_i^2$. If player i's signal is above κ_i^1, he finds it optimal to take action A in period one. If, however, his signal is between κ_i^1 and κ_i^2, then he plays B in the first period, and then plays A in the second period, if and only if, the other player played A in the first period. If player i's signal is below κ_i^2 he never takes action A.

For the rigorous characterization of the equilibrium, we refer the reader to Brindisi et al. [4]. For completeness, we state the equilibrium in the following proposition.

PROPOSITION 1. *Let $c^* = e/3$. If $c \leq c^*$ there exists a unique symmetric equilibrium of the game in monotone strategies. The equilibrium is characterized by thresholds $\kappa^1 \geq \kappa^2$ such that*

$$\begin{aligned} \kappa^1 &= \frac{e}{3} + s - c, \\ \kappa^2 &= 2c + s - \frac{2}{3}e. \end{aligned}$$

If $c > c^*$ then $\kappa^1 = \kappa^2 = s$.

In other words, if the cost of delay is small enough, those players who receive a signal between κ^2 and κ^1 prefer to wait, and observe the decision of the other player. However, if the cost is high, there is no private signal for which it is optimal to delay. As a result, a player i takes action A if and only if $x_i \geq s$.

3 Experiment

3.1 Experimental Design

The experiment was run at the Experimental Economics Laboratory of the Center for Experimental Social Sciences (C.E.S.S.) at New York University. The 52 subjects in this experiment were recruited from undergraduate classes at New York University, and had no previous experience in our experiments. In each session, after the subjects read the instructions, the instructions were also read aloud by an experimental administrator.[2,3] Each session lasted for about 75 minutes and each subject participated in only one session. An $8 participation fee and subsequent earnings, which averaged about $20, were paid in private at the end of the session. Throughout the experiment, we ensured anonymity and effective

[2] The instructions are available upon request from the authors.

[3] At the end of the first round, the subjects were asked if there was anything they did not understand. No subject reported any problems with understanding the procedures or using the computer program.

isolation of subjects in order to minimize any interpersonal influences that could stimulate uniformity of behavior.[4] The experiment was programmed in z-Tree (Fischbacher [8]).

In the experiment the random variable Θ was uniformly distributed with a finite support of $[10, 40]$.[5] Conditional on the state, θ, each subject, i, received an independent signal x_i, which was distributed uniformly between $\theta - e$ and $\theta + e$, where e indexes the noisiness of signals. The experiment consisted of three sessions, each with a different noise level e. In each session, subjects interacted for 40 independent rounds. In each round, subjects were randomly matched with another subject in the laboratory.

TABLE 1: SUMMARY OF EXPERIMENT

Number of Subjects	c	e	Number of Rounds
36*	2	2	40
32*	2	5	40
18	2	10	40

Contains one session each with support $[10, 40]$ and $[20, 50]$.

We allowed subjects to decide whether they want to take action A in three stages. This allowed us to test the prediction of the theoretical model that all decisions of action A will occur in *at most* two stages. In each of the sessions, we fixed the payoff of the safe action B to be $s = 25$. Action A was costly in the sense that in stage 2 subjects paid $c(= 2)$ points, while in stage 3 they paid $2c$ points in order to take action A.

3.2 Basic Tests of the Theory

We begin our analysis by testing three basic rationality requirements in the model. First, note that a subject should take action B if his signal is below $25 - e$, and he should take action A if his signal is above $25 + e$, because in the former (latter) case even in the best (worst) possible state, action B (A) dominates action A (B). Hence, a fundamental test of rationality is whether subjects respect dominance. Second, the equilibrium predicts that

[4] Participants' workstations were isolated by cubicles, making it impossible for participants 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.

[5] We also ran two sessions, by mistake, with support of $[20, 50]$. For both $e = 2$ and $e = 5$, the estimated thresholds (at the aggregate level) are not significantly different despite the different supports ($p = 0.93$ and $p = 0.25$, respectively). Similarly, the standard deviation of the estimated thresholds do not differ ($p = 0.17$ and $p = 0.29$, respectively). This suggests that framing effects do not play a role, which can be taken as further support for our theory. Moreover, it also means that we can include these sessions in the analysis that we report in what follows.

action A should be taken in *at most* two stages. That is, a subject who did not take action A until the third stage has no reason change his mind. Finally, a subject, who plays B in the first stage, should never take action A after having observed that his match took action B in stage 1.

Table 2 provides the frequency with which these three predictions are violated. As can be seen, violations of these three predictions are extremely rare. Overall, dominance was violated in only 0.73% of the observations, in which there were only 5 instances of a subject playing A in stage 2 despite observing that her match played B stage 1. Finally, in only 3 observations did a subject took action A in stage 3.

Table 2: Basic Tests of the Theory

Violations of dominance	25 (3440)
Action A in stage 3	3 (3440)
Action A in stage 2 despite observing B.	5 (1151)

The number in parentheses below gives the relevant sample size.

Thus, subjects seem to have a quite strong grasp of certain fundamental aspects of the strategic problem they were faced with. Given this, we now delve more deeply into more specific aspects of behavior.

3.3 Behavior in Stage One

Given the extremely infrequent violations of very fundamental predictions of our theory, we now begin a thorough analysis of more specific aspects of observed behavior. In particular, our theory suggests that subjects should use threshold strategies. The next subsection looks at this in detail.

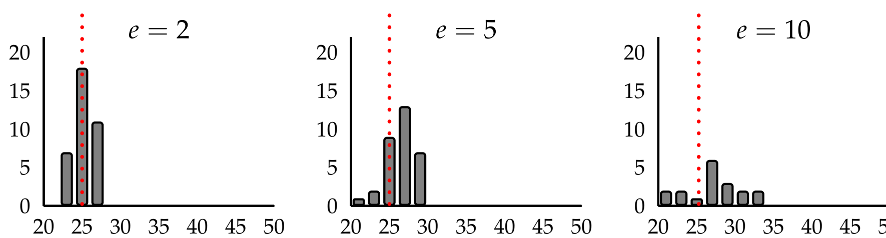
3.3.1 Thresholds at the Subject Level

For each subject i, we estimate a first stage threshold, $\hat{\kappa}_i^1$, as follows. In a round, for an arbitrary threshold κ_i^1, we say that subject i makes a mistake, if his estimate is greater than κ_i^1 *but* he does not take action A. Similarly, we also call it a mistake when the subject's estimate is less than κ_i^1 but the subject takes action A. We then search for the value of $\hat{\kappa}_i^1$

which minimizes the number of mistakes. This is our measure of subject i's threshold.[6, 7]

While Table 3 reports the average estimated thresholds for each treatment, Figure 1 displays the histograms of subjects' estimated thresholds. In order to find an answer to the question of whether the behavior is consistent with the use of a monotone strategy, we first look at the average number of mistakes. Table 3 shows that there is no treatment where the average number of mistakes exceeds 2. Furthermore, the percentage of subjects who make three or less mistakes is 94%.

Figure 1: Histograms of Estimated Individual Thresholds



In all the histograms, the horizontal axis is the estimated thresholds, and the vertical axis is the number of subjects. The dotted lines indicate the equilibrium thresholds.

We are not able to repeat the same exercise for the second period thresholds since we have less observations at the individual level. However, our findings about the first period threshold provide a strong support for the monotone strategy assumption, which in turn makes it plausible to rely on theoretical predictions as a benchmark for interpreting the experimental data.

Table 3 also shows that average estimated thresholds are pretty close to their corresponding theoretical values. For $e = 5$, we reject the hypothesis that the estimated threshold is equal to the theoretical threshold ($p = 0.02$), while for $e = 2$ and $e = 10$ we fail to reject the null hypothesis ($p > 0.25$). In addition, subjects respond to information in the same direction that theory predicts: as the information becomes less informative, the average first stage threshold level increases.[8] A more interesting finding that we observe in Figure 1 is that the distribution of estimates of cutoffs becomes more disperse as noise increases. This suggests that although average thresholds are very close to the theoretical predictions, the same is not necessarily true at the individual level as some subjects take

[6]Since we only have 40 observations, there is generally a continuum of possible thresholds. For each subject, we estimate the threshold 50 times and report the average of the 50 estimates.

[7]We also tested the question with an alternative metric: For each subject i, let $\max_i^{B}$ denote the largest signal for which subject i chose B during the course of the experiment. Similarly, let $\min_i^{A}$ denote the smallest signal for which subject i chose A. We say that subject i's behavior is consistent with monotone strategy if $\min_i^{A} > \max_i^{B}$. We do not report the detailed results here (available upon request), yet we find strong support that the vast majority of subjects use monotone strategy according to the above definition.

[8]A regression of threshold on noise gives a positive estimated coefficient, with a $p-$value of 0.013, though somewhat against the theory, most of the increase occurs when moving from $e = 2$ to $e = 5$.

the risky action for higher signals. In other words, we observe a particular type of heterogeneity among subjects' decisions to delay. This heterogeneity becomes more pronounced as the information becomes less informative. This observation is consistent with risk aversion. In fact, if we modify the model to take into account possible risk aversion, we would obtain higher theoretical cutoffs.[9] Therefore, we attribute this dispersion to heterogeneity among subjects' risk preferences.

Table 3: Analysis of Estimated Individual Thresholds Stage 1

$e =$	2	5	10
Average Estimated Threshold	24.60	25.75	26.31
Average Number of Mistakes	0.61	1.41	1.50

3.3.2 Thresholds at the Aggregate Level

In order to explore the data at the aggregate level, for each session, we use the logistic distribution in order to fit action A decisions in stage one. That is, we write the probability of taking action A in stage one conditional on observing an estimate x as:

$$\Pr\left(\text{A}|x\right) = \frac{\exp(\alpha + \beta x)}{1 + \exp(\alpha + \beta x)}. \tag{1}$$

The ratio $-\frac{\alpha}{\beta}$ is the mean threshold, while $\frac{\pi}{\beta\sqrt{3}}$ is the standard deviation of the estimated threshold. Estimation results of (1) for all sessions are reported in the second and third columns of Table 4. Robust z-statistics, which correct for clustering at the subject level, are reported below the estimated coefficients in parentheses. The fourth column reports the estimated mean threshold, the fifth column reports the standard deviation of the estimated mean threshold, and the sixth column reports the equilibrium threshold. The last column reports $p-$values of the hypothesis that the estimated threshold is equal to the theoretical threshold.

We report the results by using only the last 20 periods because a specific form of learning seems to take place. To see this, we estimated the same logit regressions for each session in five period increments. In Figure 2, we plot the results of this exercise. As shown in the figure, estimated thresholds appear to be constant over time. However, in all treatments, it appears that the standard deviation of estimated thresholds declines substantially after the first 5 periods. This suggests that while subjects' thresholds do not

[9] The model we present here assumes that the players are risk neutral by positing a utility function $u(x) = \mathbf{1}_x\theta + (1 - \mathbf{1}_x)25$, where $x \in \{\text{A}, \text{B}\}$ and $\mathbf{1_x}$ is the indicator function that takes value 1 if $x = \text{A}$ and 0 otherwise. Instead if we assume that $v(x) = \exp\{-au(x)\}$ for $a > 0$ the equilibrium first stage thresholds increase in the risk aversion coefficient a.

Table 4: Estimated Thresholds (last 20 periods)

Treatment	`slope` (β)	`cons` (α)	Mean Threshold	S.D. of Threshold	Equilibrium Threshold	p-value (theory)
Stage 1 $e = 2$	1.31*** (4.47)	-32.80*** (-4.41)	25.04	1.38	25.00	0.86
Stage 1 $e = 5$	0.67*** (7.71)	-17.79*** (-7.78)	26.39	2.69	25.00	$\ll 0.01$
Stage 1 $e = 10$	0.46*** (7.08)	-12.48*** (-6.79)	27.37	3.98	26.33	0.28
Stage 2 $e = 2$			`no obs`		27.00†	
Stage 2 $e = 5$	0.57** (2.22)	-16.37** (-2.35)	28.56	3.17	27.00†	0.19
Stage 2 $e = 10$	0.166*** (2.57)	-4.529*** (-2.79)	27.28	10.93	22.33	0.15

Robust z-statistics are in parentheses (clustering at the subject level).
*** significant at 1%; ** significant at 5%; * significant at 10%.
† Note that according to Proposition 1 we have $\kappa^1 = \kappa^2 = 25$ in these treatments. However, if a subject mistakenly delays his decision, it is still optimal to take action A for $x_i \geq 25 + c$. This is why we write 27 for the second stage threshold.

change much over time, they appear to become sharper in later periods. Therefore, by focusing on the final 20 periods, we are able to gain sharper tests of our theory.

In the following discussion we will write $\hat{\kappa}^t(e)$ to denote the estimate of stage t threshold in the treatment e. The first three rows of Table 4 report the estimates for stage one. We see that the theoretical predictions continue to explain behavior adequately at the aggregate level. In sessions $e = 2$ and $e = 10$, we fail to reject the hypothesis that estimated thresholds are equal to theoretical thresholds. In terms of comparative statics, the connection to theory is somewhat loose. In particular, we find that:

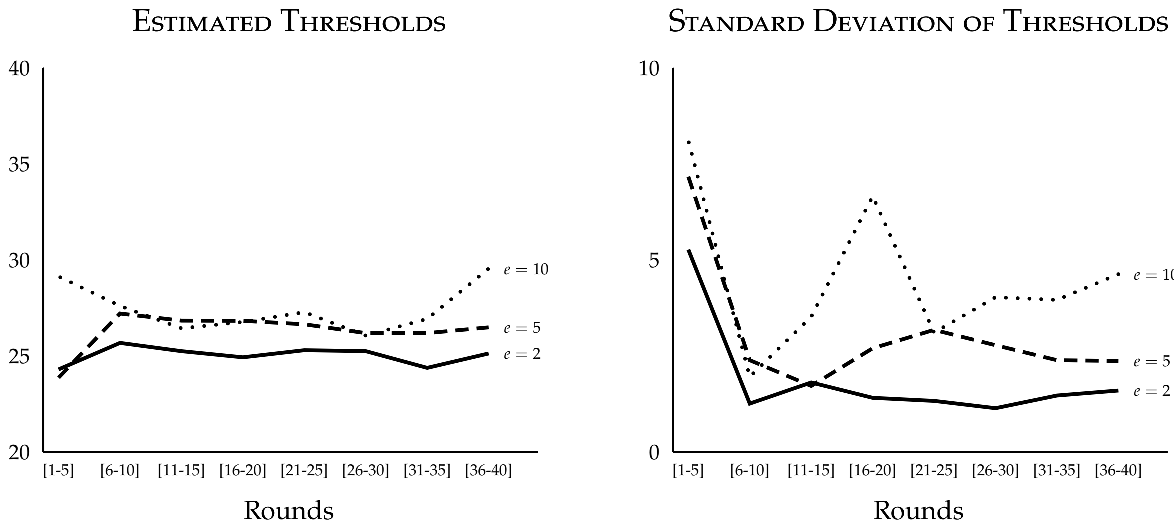
$$\hat{\kappa}^1(2) \underset{p \ll .01}{<} \hat{\kappa}^1(5) \underset{p = .33}{=} \hat{\kappa}^1(10),$$

but we do reject the hypothesis that $\hat{\kappa}^1(2) = \hat{\kappa}^1(10)$ ($\chi^2_1 = 5.85$ ($p = 0.016$)).

3.4 Behavior in Stage Two

We now turn to an analysis of behavior in the second stage. In the last three rows of Table 4, we recreate our logit regressions, restricting attention to those subjects who chose B in stage one, but who saw that their match chose A. Although we do not have enough

Figure 2: Estimated Period One Thresholds & S.D. of Thresholds (5-Round Increments)



observation to obtain any result for the $e = 2$ treatment, we report the results for $e = 5, 10$.

Although we do have the directionally correct comparative static that $\hat{\kappa}^2(10) < \hat{\kappa}^2(5)$, we fail to reject the hypothesis that $\hat{\kappa}^2(10)$ is equal to $\hat{\kappa}^2(5)$ ($p = 0.72$). Moreover, in both cases, the estimated threshold values are higher than (but not significantly) the theoretical thresholds ($p_{e=5} = 0.19$, $p_{e=10} = 0.15$). This deviation is consistent with our earlier observation of heterogeneity of risk preferences among subjects. If those who delay—despite their relatively high signal—do so because of risk aversion, then obtaining more information in the second stage convinces them to take action a. Put differently, those subjects who should have taken action a in the first stage according to the theory, delay their decision until after they observe their match taking action a. This leads the second stage estimated thresholds to be not less than the theoretical thresholds.

3.5 An Analysis of Delay

3.5.1 Comparing Actual With Expected Behavior Using Theory as a Guide

In Table 5, we report, for each of our treatments, a summary of individual behaviors using the theoretical predictions as a benchmark. Panel (a) focuses on the Stage 1 decision. As can be seen, for $e = 2$, 96.5% of the time when the signal is above the threshold, the subject takes action a. Yet, we still see that this is significantly different than 100% ($p < 0.01$). Moreover, as noise increases, the frequency of correct actions declines. The other type of error—taking action a when it is not optimal—is relatively small and stable across treatments. Next turn to Panel (b), which looks at behavior in Stage 2 conditional on taking

Table 5: Individual Behavior and Equilibrium Thresholds

(a) Stage 1: Frequency of a Choices

Treatment	$x_i \geq k^1$	$x_i < k^1$
$e = 2$	0.965	0.073
$e = 5$	0.903	0.057
$e = 10$	0.813	0.080

(b) Stage 2: Frequency of a Choices, Conditional on a in Stage 1

Treatment	$x_i \geq k^1 \wedge k^2$		$x_i \in [k^1 \vee k^2, k^1 \wedge k^2)$		$x_i < k^1 \vee k^2$	
	Obs. a	Obs. b	Obs. a	Obs. b	Obs. a	Obs. b
$e = 2$	0.250	—	0.0625	—	0.000	0.000
$e = 5$	0.381	0.143	0.313	0.050	0.100	0.036
$e = 10$	0.511	0.000	0.310	0.000	0.066	0.000

$k^1 \wedge k^2 \equiv \max\{k^1, k^2\}$ and $k^1 \vee k^2 \equiv \min\{k^1, k^2\}$.

action b in Stage 1. As can be seen in the first column, it is still quite common for subjects to receive a high signal (such that action a would actually be optimal in Stage 1), observe that their match took action a in Stage 1, yet still do not take action a in Stage 2. That being said, subjects are always more likely to take action a after observing a, as opposed to observing b. Moreover, comparing the first, the third and the fifth columns, we see that subjects are more likely to take action a (after observing a from their match) when their own estimate is higher.

We now turn from an analysis of group behavior to one which focuses on how well the behavior of the matched subjects accords with the equilibrium path. Specifically, in Table 6, for each of our treatments, we report the observed frequencies of each of the four types of equilibrium outcomes: (i) (a_1, a_1): both subjects take action a in stage 1; (ii) (a_1, a_2): one player takes action a in stage 1, with the other doing so in stage 2; (iii) (a_1, b): one subject takes action a in stage 1 and the other subject takes action b; and (iv) (b,b): neither subject takes action a. The final column reports the frequency of any other type of behavior, such as taking action a in stage 3 or in stage 2 after having observed b in stage 1. Below each reported frequency, in brackets, we also report the expected frequency of each outcome if subjects were using the theoretical thresholds. Below this number, we report the fraction of times that the correct action was observed according to the theoretical predictions. Perfect accordance to theory would be denoted by a 1.

We observe that there are less immediate a decisions by both subjects than would be optimal. For $e = 2$, the difference is small but still statistically significant $p < 0.01$. As the

TABLE 6: ACTUAL VS PREDICTED FREQUENCIES OF OUTCOMES

Treatment	(A_1, A_1)	(A_1, A_2)	(A_1, B)	(B, B)	OTHER
$e = 2$	62.22	0.28	8.19	29.17	0.14
	[64.17]	[-]	[5.28]	[30.56]	[-]
	0.932	-	0.712	0.869	-
$e = 5$	55.15	2.66	11.41	29.84	0.94
	[62.34]	[-]	[9.53]	[28.12]	[-]
	0.886	-	0.412	0.875	-
$e = 10$	26.94	8.33	16.94	47.50	0.28
	[33.06]	[8.61]	[12.22]	[46.11]	[-]
	0.735	0.280	0.514	0.881	[-]

The numbers in brackets represent the frequencies that would have occurred if subjects were using the equilibrium thresholds. A "-" indicates that the event is either not a possible outcome or off-the-equilibrium path.
The numbers in the third row for each treatment indicate the fraction of times that the given outcome was observed when, according to the estimates received, this was the theoretical prediction. Perfect accordance to theory would be denoted by a 1. All frequencies are significantly less than 1 at the 1% level according to a sign rank test (subject average as unit of observation).

informativeness of signals decreases, the frequency of correct decisions decreases[10] and, in all cases, it is statistically significant ($p < 0.01$). This means that subjects fail to take action A when it is optimal and are more prone to doing this when the noise is higher. For those instances where the optimal action was (B, B), we see that the optimal action was taken about 87%-88% of the time across all noise levels. It is more appropriate to label these cases as errors rather than a sign of something more (such as heterogeneous risk preferences), as suggested by the results for when (A_1, A_1) is optimal. Finally, we see substantially less frequent occurrences of either (A_1, A_2) or (A_1, B) than is optimal. The former suggests that the subjects do not fully appreciate the information contained in their signal and the information learned by waiting to observe action A. Without going deeper, it is difficult to understand the source of the bias in the latter case because there are several possible outcomes when (A_1, B) is optimal but not observed.

We examine this issue in Table 7, which also assists us to assess the validity of our earlier hypothesis about the effect of risk aversion on delay behavior. The fourth column shows the fraction of the cases the actual observations concur with the theoretical predictions. The last column shows the case where no subject takes action A. First, observe that for all values of e, this is the most frequent error that occurs. Second, observe that the frequency of this error is substantially higher for $e \in \{5, 10\}$ than for $e = 2$. Both of these

[10]Specifically, if we regress the frequency of the correct decision on the noise, then the estimated coefficient on noise is negative with a $p-$value of 0.002.

Table 7: Fractions of Deviations from (a1, b) Path

Treatment	(a1, a1)	(a1, a2)	(a1, b)	(b, b)	Other
$e = 2$	.158	0	.658	.184	0
$e = 5$	0	.098	.459	.410	.033
$e = 10$	.045	.114	.477	.364	0

Note that numbers in the fourth column are slightly different than those reported in Table 6 because those report the average of the subject average, while the results reported here are based on the raw data.

results are consistent with our risk aversion hypothesis: subjects tend to take action a as the information becomes less noisy. Similarly, as the second column of the Table 7 shows, the fraction of subjects who took action a in the first stage decreases as the information becomes less valuable.

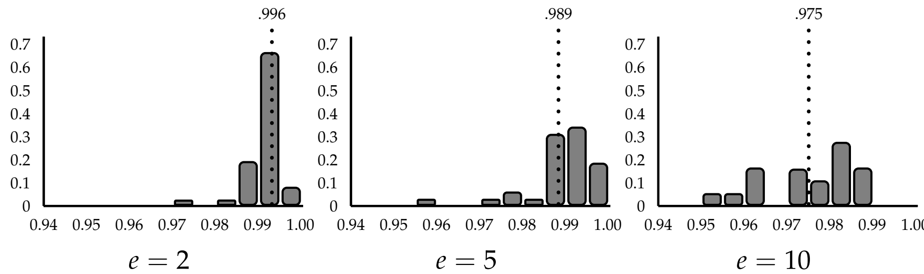
Finally, in the third column, we see that in treatments $e = 5$ and $e = 10$, the fraction of subjects who delayed action a until the second stage is higher than the treatment $e = 2$. Note that those subjects should have never played a in the first place. So, this seems to suggests that the potential to acquire more information in the second stage is a driving force to delay as the theory suggests.

3.6 Welfare

In an attempt to understand welfare implications of delay we now study the level of efficiency achieved by subjects in our experiment. Let π_t^a denote the actual payoff received by a subject in round t, including any costs that the subject incurred due to delay. Next, let $\pi_t^{max} := \max\{25, \theta\}$ denote the ex post efficient outcome in round t. That is, when a subject takes action a when $\theta \geq 25$ and b otherwise. We define our measure of welfare for a subject as $\pi = \sum_{t=1}^{40} \pi_t^a / \pi_t^{max}$.

For each of our treatments, Figure 3 displays the histogram of individual welfare measure. First, we observe that average welfare measures are quite high. However, this is somewhat misleading. Recall that our subjects did not violate basic requirements of rationality such as taking action a (b) for a very small signal where θ is revealed to be less (higher) than 25. In all those instances, the ratio is always 1 inflating the welfare measure. Still we are able to observe the changes across treatments. As would be expected, the average welfare measure decreases as information becomes less informative. More importantly, we observe that the individual welfare measures become more dispersed as e increases. Indeed, this is what we would expect given our hypothesis about the heterogeneity of risk preferences. Clearly, a risk averse subject who is hesitant to take action a will not be able to receive high payoffs in high states.

Figure 3: Histograms of Individual Welfare Measures



In all the histograms, the horizontal axis is the welfare measure, and the vertical axis is the fraction of subjects.

It is important to emphasize that this does not mean that risk averse subjects' ex ante welfare is also relatively low.

4 Conclusion

In this paper, we reported the result of an experiment that tested a simple social learning model with endogenous timing. Our goal was to explore individuals' timing decisions, and analyze whether they delay their decisions in order to acquire more information. Our analysis suggests that subjects do delay their decisions in order to obtain information, perhaps more often than the theory predicts. Furthermore, subjects delay especially when their private information does not particularly support the risky action. We also find evidence which suggests that risk aversion plays an important role in timing decisions. We show that subjects' delay decisions are more disperse when private information is less informative. Excessive delay and reluctance to take the risky action at all lead to ex post welfare losses.

References

[1] Anderson, L. and C. Holt (1997). "Information Cascades in the Laboratory." *American Economic Review* 87(5): 847-862.

[2] Banerjee, A. (1992). "A Simple Model of Herd Behavior." *Quarterly Journal of Economics* 107(3): 797-817.

[3] Bikhchandani, S., D. Hirshleifer and I. Welch (1992). "A Theory of Fads, Fashion, Custom, and Cultural Change as Informational Cascades." *Journal of Political Economy* 100(5): 992-1026.

[4] Brindisi, F., B. Çelen and K. Hyndman (2011). *The Effect of Endogenous Timing on Coordination Under Asymmetric Information: An Experimental Study*. Mimeographed, Columbia University.

[5] Çelen, B. and S. Kariv (2004). *Distinguishing informational cascades from herd behavior in the laboratory*. *American Economic Review* 94(3): 484-498.

[6] Chamley, C. (2004). *Rational Herds: Economic Models of Social Learning*. Cambridge, UK, Cambridge University Press.

[7] Chamley, C. and D. Gale (1994). "Information Revelation and Strategic Delay in a Model of Investment." *Econometrica* 62(5): 1065-1085.

[8] Fischbacher, U. (2007). "z-Tree: Zurich toolbox for ready-made economic experiments." *Experimental Economics* 10(2): 171-178.

[9] Gul, F. and R. Lundholm (1995). "Endogenous Timing and the Clustering of Agents Decisions." *Journal of Political Economy* 103(5): 1039-1066.

[10] Humg, A. and C. Plott (1995). "Information Cascades: Replication and an Extension to Majority Rule and Conformity-Rewarding Institutions." *American Economic Review* 91(5): 1508-1520.

[11] Kübler, D. and G. Weizsäcker (2004). "Limited Depth of Reasoning and Failure of Cascade Formation in the Laboratory." *Review of Economic Studies* 71(2): 425-441.

[12] Sgroi, D. (2003). "The Right Choice at the Right Time: a Herding Experiment in Endogenous Time." *Experimental Economics* 6: 159-180.

Appendix

General Instructions

This is an experiment on the economics of decision-making. Your earnings will depend partly on your decisions 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 and your match earns 25 points.
- If you choose B and your match chooses A: You earn 25 and your match earns Q 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 and your match earns 25 .

	A	*B*
A	**Q**, *Q*	**Q**, *25*
B	**25**, *Q*	**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 10 and 40 randomly determined by the computer. That means any number between 10 and 40 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.

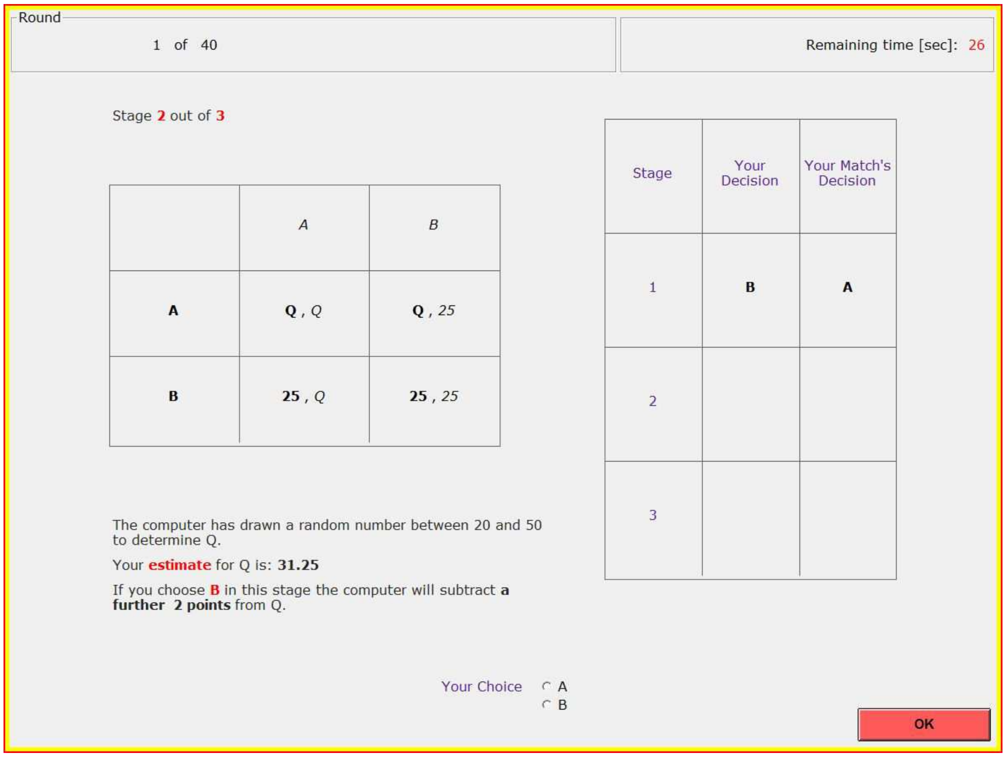
Before you make a decision you will not be told what Q is but instead you will receive an estimate of Q, which we will denote by E. Let's be more precise. After the computer randomly determines Q, it also picks a random number (up-to two decimals) between $Q - 5$ and $Q + 5$. This is your estimate E. 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 $E = 20.15$, then you know that Q is **not less than** $20.15 - 5 = 15.15$ and it is **not more than** $20.15 + 5 = 25.15$.

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(s). 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, while you chose **B** in the first stage your match chose **A**; you also see that your estimate of Q is 31.25. However, you do not see your match's choice in stage 2. Since B is reversible, you can choose between A and B in stage 2. But **A** is irreversible, so your match cannot change his/her decision.



Payoffs

Your potential earnings in each round depends on your 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 31.25, its true value was 32.11. You also see that your profit was 30.11: since you eventually chose A, you received $Q = 32.11$ points, but because you chose B in stage 1, 2 points were 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.