Learning from Inaction*

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Abstract

We experimentally study how agents extract information from the absence of a signal or action across three canonical settings: an individual state-guessing task, a two-player colored hats game, and an incomplete-information matching market. In each setting, inaction arises naturally from the environment rather than from strategic choice, allowing us to isolate the inferential challenge it poses. We find that learning from inaction is consistently more difficult than learning from action across all three environments. Participants underreact to inaction even when it carries the same informational value as an observed action. We further introduce a treatment that makes inaction explicitly observable. This intervention generally improves learning from inaction, suggesting that a key source of the difficulty is the failure to recognize absence as informative in the first place.

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"Is there any point to which you would wish to draw my attention?"

"To the curious incident of the dog in the night-time."

"The dog did nothing in the night-time."

"That was the curious incident," remarked Sherlock Holmes.

Silver Blaze

Arthur Conan Doyle (1892)

1 Introduction

People often depart from the Bayesian benchmark when forming and updating beliefs (Kahneman and Tversky, 1973; Grether, 1980; Benjamin, 2019). Such deviations are especially common when information is correlated, selected, or difficult to interpret. For example, individuals may overreact or underreact to correlated information (Enke and Zimmermann, 2019; He and Kučinskas, 2024), or fail to account for selection in observed samples (Esponda and Vespa, 2014, 2018; Enke, 2020; Barron, Huck and Jehiel, 2024). In many economic environments, however, relevant information may be conveyed not only by observed actions, but also by the absence of actions, whether generated by nature or by other people. This paper studies how individuals learn from such inaction.

Learning from inaction may be difficult because it requires decision makers to recognize that something informative has not occurred. Consider a university applicant who may receive good news, bad news, or no news from a university. While prior work documents a good-news–bad-news asymmetry in belief updating (Eil and Rao, 2011; Möbius et al., 2022), there may also be an asymmetry between bad news and no news: receiving no response may carry information similar to bad news, yet applicants may fail to update accordingly. A similar issue arises in job search. Individuals may update their beliefs about labor-market conditions when they observe friends signing job offers, but may draw weaker inferences when they observe no such activity. Inaction may therefore fail to trigger belief updating because decision makers must first recognize the situations under which an action would have occurred, and then infer from the fact that it did not.

In this paper, we examine whether individuals respond differently to action and inaction when the two have the same informational value. We first test whether learning from inaction is systematically weaker than learning from action, and then study the underlying mechanism by asking whether making inaction observable or salient improves belief updating.

We adopt three canonical settings: a state-guessing task, a colored hats game, and an incomplete-information matching market. These settings are intended to understand learning from inaction in three fundamental environments: individual decision-making, non-cooperative games, and cooperative games. In all three, inaction arises from the structure of the environment rather than from strategic choice.[1] We test the three settings in a series of laboratory experiments as described below.

State-guessing. Nature draws a state, Red or Blue, and sends an individual either a red or blue signal. The individual then guesses the state. A red signal favors Red, whereas a blue signal favors Blue. In some treatments, the blue signal is replaced by the absence of a signal. We measure learning from inaction by whether participants correctly infer Blue when no signal is displayed.

Colored hats. Two players each wear either a red or blue hat and observe only the other player's hat. After a public announcement that at least one hat is red, they infer their own hat color over sequential periods. When both hats are red, neither player can infer her own color in the first period, so both should remain silent. Correctly guessing red in the second period therefore requires learning from the other player's first-period inaction.

Matching market. Two informed workers and two uninformed firms form one-to-one matches in a decentralized market. In some configurations, an uninformed firm observes another worker-firm match and infers the state from this market activity. In others, the firm observes that no such match has formed and must infer the state from this inactivity. The second match forms only if the firm learns from inaction.

Across all three settings, we construct theoretically comparable cases that differ only in whether learning is based on action or inaction. These cases constitute our two main treatment conditions. If participants perform worse when learning from inaction, one possible mechanism

[1] In the colored hats game, for example, a player's failure to raise her hand is not a strategic message but a direct consequence of her epistemic state: she did not act because she does not know. Similarly, in the matching market, we design the matching payoffs to rule out complex strategic action and inaction.

is that inaction requires a two-step inference: a player must first recognize that an action could have occurred under certain states, and then update beliefs based on the fact that it did not occur. Failure at either step leads to the same observable outcome: underreaction to inaction.

To study this mechanism, we introduce a *Revealed* treatment in all three settings. In this treatment, the relevant inaction is made observable or salient: in the first two settings, it is represented as a distinct action to highlight the relevant situation; in the third, the acceptance and rejection decisions underlying match-formation attempts are revealed to clarify that an unchanged matching outcome may result from market activity. The *Revealed* treatment allows us to examine whether greater transparency helps decision makers extract the informational content of inaction, and whether this effect varies across environments.

Our experimental results show that learning from inaction is consistently more difficult than learning from action across settings. In the state-guessing task, this pattern emerges only when participants receive two different but balanced signals, that is, when inaction is accompanied by action. In the other two settings, the difficulty appears more broadly across configurations. Overall, participants underreact to inaction even when it has the same informational value as action. The *Revealed* treatment generally improves learning from inaction, or at least narrows the gap between learning from action and learning from inaction. This suggests that a key source of the difficulty is participants' failure to recognize inaction as informative. Once inaction is made observable or salient, participants respond to it more, though not always fully.

Overall, our paper makes three novel contributions to the literature. First, we study a novel type of behavioral bias in belief formation: individuals may fail to recognize that the absence of a signal or action is itself informative. In our settings, inaction is not merely missing data; rather, it is an event generated by the underlying information structure or induced by game contingencies. We therefore examine whether individuals under-use the information conveyed by unobserved signals, silence, or inaction. Second, we identify this bias across three distinct but complementary economic settings. Third, we show that interventions that make the informational content of inaction explicit, or reveal relevant hidden information, improve belief updating and behavior in these respective settings.

2 Related Literature

First, this paper contributes to the literature on biases in belief formation when information is complex, correlated, selected, or difficult to interpret.[2] For example, Enke and Zimmermann (2019) shows that people often treat correlated signals as independent and therefore overreact to repeated information, while He and Kučinskas (2024) finds that people underreact to correlated signals when they fail to recognize their relevance. Relatedly, Esponda and Vespa (2014) shows that subjects often fail to extract information from hypothetical or counterfactual events, and Esponda and Vespa (2018) documents failures to account for selection in observed samples. In an experimental investment environment, Barron, Huck and Jehiel (2024) shows that individuals can become overly optimistic when learning from others' selected outcomes because they do not properly account for the selection process. Together, these studies show that people often update non-optimally.[3]

Our paper is closest to work on belief formation when some information is not revealed. Enke (2020) studies selective reporting by information providers, which creates a sample-selection problem for receivers. He shows that individuals often neglect this selection process and form beliefs as if the observed information were representative of the full information set. Jin, Luca and Martin (2021) studies voluntary disclosure in a sender–receiver game and shows that senders strategically disclose favorable information while withholding unfavorable information, but receivers do not fully internalize the negative informational content of nondisclosure. Forsythe, Lundholm and Rietz (1999) examines communication in experimental financial markets with adverse selection, showing how communication regimes affect market efficiency when sellers have private information about asset quality. These studies show that individuals may draw biased inferences when relevant information is selectively revealed or strategically withheld. Unlike these settings, however, our paper studies whether individuals underuse no-news events when the absence of signals or actions arises directly from the information or game structure itself, rather than from strategic withholding by information

[2] Earlier work shows that people may fail to update optimally even when signals are explicitly presented (Kahneman and Tversky, 1973; Grether, 1980; Charness and Levin, 2005). Benjamin (2019) provides a broad survey of systematic deviations from Bayesian reasoning.

[3] A related but more distinct literature studies motivated or emotionally asymmetric belief updating. For example, Eil and Rao (2011) shows that individuals process positive and negative self-relevant information differently, and Möbius et al. (2022) documents biased updating about one's own ability.

providers.

Theoretically, our paper relates to the general economic tenet that rational players should update their beliefs based on the observed history of play, including both actions and inactions. This principle appears in many equilibrium concepts for games as well as more specific topics such as the colored hats game (Littlewood, 1953), voluntary disclosure (Grossman, 1981; Milgrom, 1981), and matching with incomplete information (Liu et al., 2014; Liu, 2020). In each case, rational agents are predicted to extract information from events that fail to occur, disclosures that are not made, or matches that are not formed. However, focusing on inaction in its most basic and transparent form, our experiments show that learning from inaction is difficult.

Our emphasis on the difficulty of learning from inaction is in spirit related to the unawareness literature (Heifetz, Meier and Schipper, 2006). However, unawareness formally requires that a player cannot conceive of a certain event, which is different from failing to draw an inference from an aware event. Similarly, our Revealed treatment echoes the salience framework of Bordalo, Gennaioli and Shleifer (2012, 2013), who show that agents overweight attributes that stand out relative to the background and underweight those that do not. However, their framework models salience as contrast relative to a reference point, which fundamentally differs from our comparison between an observed action, inaction, and revealed inaction.

Finally, our paper relates to experimental studies using settings similar to ours. Our individual state-guessing task is close to those in Charness and Levin (2005), Levin, Peck and Ivanov (2016) and Coutts (2019), but differs in introducing environments in which the absence of a signal is itself informative. Our colored hats (equivalently, dirty face) experiment relates to the work of Weber (2001), Bayer and Renou (2016) and Lin (2022), which studies reasoning depth, logical deduction, and the effect of game form. In contrast, we fix the depth of reasoning and focus on how subjects respond to action versus inaction. Our matching experiment is closely related to Agranov et al. (2025) and Gui, He and Hu (2025), who study whether and how stability emerges as a market outcome. Unlike these papers, we examine whether uninformed agents correctly update their beliefs from the absence of a match and whether revealing relevant hidden information improves their performance.

3 Games

We consider three settings where both *learning from action* and *learning from inaction* naturally arise. For theoretical descriptions, we assume players are rational. In each of the three settings, we vary state of the world (and relevant parameters) such that the two learning patterns can be identified by game dynamics.

3.1 State-Guessing

We first consider an individual state-guessing problem which is arguably the simplest setting with the aforementioned learning patterns. It is a two-stage game played by the individual and nature. More precisely, there are two possible states of the world, $\omega \in \{\text{Red}, \text{Blue}\}$, distributed according to a common prior $\Pr(\text{Red}) = \Pr(\text{Blue}) = 1/2$. In the first stage, nature draws a state and discloses a signal (or no signal) to the individual player according to the following structure:

$$\Pr(red \mid \text{Red}) = \frac{2}{3}, \qquad \Pr(red \mid \text{Blue}) = \frac{1}{3},$$

where *red* denotes a signal that favors the state Red.

In the second stage, whether the individual observes a signal or not, she guesses the state. Upon observing *red*, the individual shall derive a posterior belief by the Bayes' rule:

$$\Pr(\text{Red} \mid red) = \frac{\Pr(red \mid \text{Red}) \Pr(\text{Red})}{\Pr(red)} = \frac{2}{3},$$
$$\Pr(\text{Blue} \mid red) = \frac{\Pr(red \mid \text{Blue}) \Pr(\text{Blue})}{\Pr(red)} = \frac{1}{3}.$$

Therefore, a rational player guesses Red. In contrast, without observing a signal, a rational individual shall recognize that silence is itself informative and derive a posterior belief as follows:

$$\Pr(\text{Red} \mid absent) = \frac{1}{3}, \quad \Pr(\text{Blue} \mid absent) = \frac{2}{3},$$

where *absent* denote the absence-of-signal situation. In this case, a rational player guesses Blue. Table 1 summarizes the game, where r means the *red* signal and $\varnothing$ means no signal.

Overall, learning from action is measured by whether the individual correctly guesses Red in response to *red*, whereas learning from inaction is measured by whether the individual

Stage one	State	Red		Blue	
	Signal	r	$\varnothing$	r	$\varnothing$
$(\Pr(\text{Red} \mid \cdot), \Pr(\text{Blue} \mid \cdot))$		$(\frac{2}{3}, \frac{1}{3})$	$(\frac{1}{3}, \frac{2}{3})$	$(\frac{2}{3}, \frac{1}{3})$	$(\frac{1}{3}, \frac{2}{3})$
Stage-two best response		Red	Blue	Red	Blue

Table 1: State-guessing game with one signal draw.

correctly guesses Blue in response to silence.

The game can be extended from one signal to two independent signals (about the same state drawn by nature in stage one). The individual may thus observe two red signals, one red signal, or no signal. The signal structure is identical to before, and posteriors follow from Bayes' rule applied to independent draws. Table 2 summarizes all configurations. Here, learning from action is measured by whether the individual correctly guesses Red in response to rr, whereas learning from inaction is measured by whether the individual correctly guesses Blue in response to $\varnothing\varnothing$.[4]

Stage one	State	Red			Blue		
	Signals	rr	$r\varnothing$	$\varnothing\varnothing$	rr	$r\varnothing$	$\varnothing\varnothing$
$(\Pr(\text{Red} \mid \cdot), \Pr(\text{Blue} \mid \cdot))$		$(\frac{4}{5}, \frac{1}{5})$	$(\frac{1}{2}, \frac{1}{2})$	$(\frac{1}{5}, \frac{4}{5})$	$(\frac{4}{5}, \frac{1}{5})$	$(\frac{1}{2}, \frac{1}{2})$	$(\frac{1}{5}, \frac{4}{5})$
Stage-two best response		Red	Red/Blue	Blue	Red	Red/Blue	Blue

Table 2: State-guessing game with two signal draws.

3.2 The "Colored hats" Game

The "colored hats" game (or equivalently, the "dirty faces" game) swept Europe some time in the first half of the twentieth century (Littlewood, 1953, pp. 3-4). Here we take the textbook treatment from Osborne and Rubinstein (1994, p. 71). In particular, there are n players seated

[4] In this example, when the signal profile is $r\varnothing$, both Red and Blue are the individual's best responses. The uniform prior is chosen for expositional clarity: it isolates the key insight that silence (inaction of nature) is theoretically as informative as a red signal, without confounding it with asymmetric baseline beliefs. In our experimental design, we adjust the priors so that the rational best response is unique under every signal configuration. Moreover, correctly guessing Blue becomes both necessary and sufficient to identify learning from inaction, which is the main focus of our paper. See Appendix A.1 for parameter details.

around a table. Each player is wearing a hat that is either *red* or *blue*. Each can see the hats of the other $n-1$ players, but not her own. An observer announces: "Each of you is wearing a hat that is either *red* or *blue*; at least one of the hats is *red*. I will start to count slowly. After each number you will have the opportunity to raise a hand. You may do so only when you know the color of your hat." When, for the first time, will any player raise her hand?

The answer depends on the state of the world, i.e., the profile of colored hats. Formally, we consider the case of $n = 2$ and two possible states. First, suppose the state is $(\text{Red}, \text{Blue})$, meaning that Player 1 is wearing a red hat and Player 2 is wearing a blue hat. The game starts with the observer's public announcement and then proceeds in multiple periods (corresponding to the observer's count); Table 3 illustrate the game. Once the observer announces "at least one of the hats is *red*," Player 1 immediately knows that her hat must be *red* since she saw the other hat is *blue*. Therefore, in period one (when the observer counts one), Player 1 shall raise her hand and claim *red*, whereas Player 2 does not.

In period two, Player 2 would think in the following way: "If my hat *were* red, then Player 1 should have seen it in period one. Since she only knows that there is at least one red hat, she cannot tell whether her hat is red or blue, both of which are consistent with the public announcement. Now that Player 1 claimed *red* in period one, it must be the case that she has seen my *blue* hat." Therefore, Player 2 shall raise her hand in period two and claim *blue*. In this game, learning from action is measured by whether Player 2 correctly claims *red* in period two.

	Player 1	Player 2
State	Red	Blue
Announcement	at least one of the hat is Red	
Period-one actions	claim red	(inaction)
Period-two actions	N.A.	claim blue

Table 3: Learning from action in the colored-hats game.

Second, suppose the state is (Red, Red); this game is illustrated in Table 4. The game starts with the observer announcing "at least one of the hats is *red*." In period one, neither player can know her hat's color and make a claim: For each of them, the opponent's hat being red provides no information beyond the public announcement, which is consistent with both possible colors of their own hat. Therefore, no action is taken in period one.

In period two, Player 2 (Player 1 is symmetric) would think in the following way: "If my hat *were* blue, then Player 1 should have seen it in period one. Since she knows that there is at least one red hat, she must have known that her hat is red. Now that Player 1 toke no action in period one, it must be the case that she has seen my *red* hat." Therefore, Player 2 shall raise her hand in period two and claim *red*, so should Player 1 for the same reason. In this game, players' learning from inaction is measured by correctly claiming *red* in period two.

	Player 1	Player 2
State	Red	Red
Announcement	at least one of the hat is Red	
Period-one actions	(inaction)	(inaction)
Period-two actions	claim red	claim red

Table 4: Learning from inaction in colored hats game.

3.3 Matching Markets with Incomplete Information

We take a simplified version of the framework introduced by Liu et al. (2014) and Liu (2020). Consider a one-to-one matching market with one-sided incomplete information and without monetary transfer. Specifically, let $I = \{i_1, \ldots, i_n\}$ be a set of workers, and $J = \{j_1, \ldots, j_m\}$ be a set of firms. A *matching* is a one-to-one function $\mu : I \cup J \to I \cup J$ that pairs up workers and firms such that for each $i \in I$ and each $j \in J$, (1) $\mu(i) \in J \cup \{i\}$, (2) $\mu(j) \in I \cup \{j\}$, and (3) $\mu(i) = j$ if and only if $\mu(j) = i$. If $\mu(i) = i$ or $\mu(j) = j$, we say that the agent is *unmatched*. Assume that μ is observable.

A payoff-relevant *state* ω is drawn from a finite set Ω according to a prior distribution $\beta \in \Delta(\Omega)$. Particularly, let $a_{ij}(\omega) \in \mathbb{R}$ and $b_{ij}(\omega) \in \mathbb{R}$ be the ex post *matching values* worker i and firm j receive, respectively, when they are matched, i.e., $\mu(i) = j$, and when the realized state is ω. Normalize the unmatched values to zero, that is, $a_{ii}(\omega) = b_{jj}(\omega) = 0$. Assume that each worker can observe the realized state, whereas neither firm can. When a firm is uncertain about the state, she cares about the expected matching values. This assumption is for modeling completeness and is not crucial for our analysis.

In this paper, we consider matching markets with two workers i_1 and i_2, two firms j_1 and j_2, and two possible states ω and ω'. Our analysis shall be clear without formally introducing the theoretical concepts like blocking and stability in Liu (2020).[5] We vary the matching values to identify the two learning patterns.

The first matching market is described by the following matching-value table:

	j_1		j_2	
i_1	1	2	-10	-10
	-1	4	-10	-10
i_2	-10	-10	1	2
	-10	-10	1	-4

where the numbers in each box correspond to

$$\begin{array}{|cc|} \hline a_{ij}(\omega) & b_{ij}(\omega) \\ a_{ij}(\omega') & b_{ij}(\omega') \\ \hline \end{array}.$$ [6]

Suppose ω is the realized state, inducing the first row of payoffs in each box in the table. The matching market starts with no one matched (autarky) and proceeds in a totally decentralized way: Each worker (firm) can propose to any firm (worker), but only one so that matches are one-to-one. Each agent is free to accept or reject a proposal; when a proposal is accepted, a tentative match is formed (and any previous tentative match, if any, is dissolved). Each agent in a matched pair is free to dissolve the relationship. Agents aim to obtain as much payoff as possible.

We claim that the above matching market will end up with two matched pairs: (i_1, j_1) and (i_2, j_2). To see it, note first that "cross matches" are impossible because the payoffs are all negative, worse than standing alone. Therefore, we only need to verify the two claimed pairs. Although we do not impose a sequential structure to the market, the two pairs will emerge

[5]For theoretical studies of matching with incomplete information, see also Liu et al. (2014), Chen and Hu (2020, 2023, 2024), Pomatto (2022),Wang (2023), and Hu (2024).

[6]For convenience in discussion, we specify matching values directly in the examples, rather than specifying the states and functional forms of a and b.

(endogenously) sequentially, with (i_1, j_1) happens *before* (i_2, j_2). We argue this in three steps.

One the one hand, i_1 and j_1 will form a match regardless of who proposes to whom. For i_1, he know that the state is ω and thus the matching value is 1, so he would either propose to j_1 or accept j_1's proposal if any. For j_1, her matching value with i_1 is always positive no matter what the state is, so she would either propose to i_1 or accept i_1's proposal if any. Therefore, after the market starts, pursuing positive payoffs would drive i_1 and j_1 matched.

On the other hand, *relative to the autarky*, i_2 and j_2 will not form a match for the following reason. For i_2, his matching value with j_2 is always positive no matter what the state is, so he would either propose to j_2 or accept j_2's proposal if any. Nevertheless, for j_2 who does not know what the state is and thus what her matching value will be, she worries about the negative value -4 even if the positive 2 is also possible. So she would not propose to i_2 or reject i_2's proposal if any.

However, *once the match* (i_1, j_1) *takes place*, firm j_2 can observe it. She would think in the following way: "If the state *were* ω', then worker i_1 should be having a negative payoff -1 when matched with j_1. Then he should not have proposed to j_1 and should have rejected j_1's proposal if any. Now that I see the matched pair (i_1, j_1), it must be the case that worker i_1 is enjoying a positive payoff, i.e., the state must be ω." Therefore, firm j_2 should propose to i_2 or accept i_2's proposal if any, leading to the second match (i_2, j_2).

In this matching market, learning from blocking (LB) is measured by whether the match (i_2, j_2) is formed.

Now we consider a second matching market described by the following payoff table:

	j_1		j_2	
i_1	-1	2	-10	-10
	1	4	-10	-10
i_2	-10	-10	1	2
	-10	-10	1	-4

where we only switched the matching values of i_1 under two states. Suppose ω is the realized state. We claim that this matching market will end up with only one matched pair (i_2, j_2). Again, note first that "cross matches" are impossible. The potential match between i_1 and j_1

will not occur: Since worker i_1 knows that the state is ω and thus his matching value with j_1 is -1, he would not propose to j_1 and reject j_1's proposal if any. *Relative to the autarky*, i_2 and j_2 would not form a match for the same reason as in the first matching market.

However, as the market proceeds, j_2 will notice that i_1 and j_1 did not form a match. She would think in the following way: "If the state *were* ω', then worker i_1 should be having a positive payoff 1 when matched with j_1. Then he should have proposed to j_1 or should have accepted j_1's proposal if any. Since firm j_1 is always willing to be matched, this should have led to a matched pair (i_1, j_1). Now that I did not see such a pair, it must be the case that worker i_1 is avoiding a negative payoff, i.e., the state must be ω." Therefore, firm j_2 should propose to i_2 or accept i_2's proposal if any, leading to a matched pair (i_2, j_2).

In this second matching market, learning from no blocking (LNB) is measured by whether the match (i_2, j_2) is formed.

Remark 1. Unlike the colored hats games, the matching markets in this subsection exhibit "cooperative" feature. That is, what matters are the matches formed and their sequence, while the details such as "who proposes to whom" or "how long it takes" do not affect our analysis. As Liu (2023) points out, the cooperative approach, as a reduced form, proves useful in analyzing complex markets. It makes no excessive ad hoc assumptions while keeps the flexibility of doing so.

4 Experimental Design, Procedures, and Hypotheses

We conduct three laboratory experiments to study how individuals learn from inaction. Across the experiments, payoff-relevant information may be conveyed not only by observed actions, but also by the absence of actions. The three experiments examine this question in increasingly rich environments: an individual decision problem, a non-cooperative strategic game, and a cooperative matching market.

Experiment 1 isolates the basic mechanism in a non-strategic setting. Participants complete a state-guessing task in which information is generated by nature. This setting allows us to test whether individuals treat the absence of a signal in the same way as an informationally equivalent observed signal, and whether making the absence of a signal salient improves learning.

Experiment 2 moves to a non-cooperative environment. In a colored-hats game, participants must infer their own hat color not only from what the other player says, but also from the other player's silence. This setting allows us to study whether individuals draw the correct inference from another player's inaction in a dynamic game, and whether adding an explicit option "cannot tell" to the action space helps learning.

Experiment 3 examines learning from inaction in a matching environment. In an incomplete-information matching market, uninformed firms can infer the state from the behavior of other participants, such as forming a match or dissolving an existing match. This setting allows us to study whether individuals learn from the absence of changes in matching outcomes, and whether revealing otherwise hidden market activities facilitates such learning.

Taken together, this coherent design allows us to examine whether people have systematic difficulty or bias in learning from inaction across distinct but complementary economic settings. Moreover, by making the information behind inaction explicit or salient, or by revealing otherwise hidden information, we can compare how these mechanisms operate across settings.

4.1 Treatments and Design

4.1.1 Experiment 1: State-Guessing Task

Experiment 1 employs a state-guessing task. In each round, there are two possible states of the world, Red and Blue. Participants receive imperfect exogenous signals about the true state and then make a guess. A correct guess is rewarded if the round is selected for payment.

We use a between-subject design with three treatments. In the *Baseline* treatment, signals are displayed as either red or blue. A red signal is more likely to occur when the true state is Red, and a blue signal is more likely to occur when the true state is Blue. In the *Inaction* treatment, the red signal is displayed in the same way as in Baseline, whereas the blue signal is replaced by the absence of a displayed signal. Thus, in this treatment, the informational content of the blue signal is conveyed through inaction. In the *Revealed* treatment, the signal structure is the same as in Inaction, but participants are explicitly informed about the probability of receiving no signal and are notified whenever the signal is absent. This treatment allows us to examine whether making inaction salient mitigates underreaction to absent signals.

Across all treatments, we hold fixed the correlation structure between states and signals:

$$\Pr(\text{red signal} \mid \text{Red}) = \frac{2}{3}, \quad \Pr(\text{blue or absent signal} \mid \text{Red}) = \frac{1}{3},$$

and

$$\Pr(\text{red signal} \mid \text{Blue}) = \frac{1}{3}, \quad \Pr(\text{blue or absent signal} \mid \text{Blue}) = \frac{2}{3}.$$

Thus, the blue signal in Baseline and the absent signal in Inaction and Explicit are informationally equivalent.

Participants experience two parameter sets in a within-subject design. The two sets vary in the prior probabilities of the true state and in the number of signals participants receive. In Parameter Set A, the prior probabilities are

$$\Pr(\text{Red}) = 0.6, \quad \Pr(\text{Blue}) = 0.4,$$

and participants receive one signal in each round. In Parameter Set B, the prior probabilities are

$$\Pr(\text{Red}) = 0.4, \quad \Pr(\text{Blue}) = 0.6,$$

and participants receive two independent signals in each round.

Participants complete 15 rounds in total: 5 rounds under Set A and 10 rounds under Set B. The order of the two parameter sets is counterbalanced across participants: half of the participants complete Set A first and then Set B, while the other half complete Set B first and then Set A.

In each round, the true state and the signal realization are generated according to the relevant prior and signal structure. Among all possible signal configurations, we focus on pivotal cases in which underreaction to an absent signal relative to an explicit blue signal can lead to an incorrect choice. To ensure that each participant encounters such pivotal configurations, we impose an additional randomization constraint. Specifically, Set A must include at least one round with one blue or absent signal, and Set B must include at least one round with one red signal and one blue or absent signal.

At the end of the experiment, three rounds are randomly selected for payment. In each selected round, participants receive a reward if their guess matches the true state.

4.1.2 Experiment 2: Colored-Hats Game

Experiment 2 employs a colored-hats game with two players. Each player wears a hat that is either Red or Blue. Each player observes the other player's hat color but not her own. At the beginning of the game, both players receive a public announcement indicating that at least one of the two hats has a particular color. Players then infer their own hat color over multiple periods.

We use a 2×2 design that varies along two dimensions: game type and action space. The game type is determined by the realized hat colors and is varied within-subject. In the *different-color* case, the two players wear different hat colors. In the *same-color* case, the two players wear the same hat color. The action space is varied between-subject. In the *Inaction* treatment, participants can choose "Red", choose "Blue", or take no action. In the *Revealed* treatment, participants can choose "Red", choose "Blue", or choose "Cannot tell". Thus, the Revealed treatment makes the inability to infer one's own hat color explicit in the action space.

The public announcement is determined by the realized hat colors. If the two players have the same hat color, the announcement states that at least one hat has that color. For example, if both hats are Red, the announcement states that there is at least one Red hat. If the two players have different hat colors, the announcement states that there is at least one Red hat or at least one Blue hat, with equal probability.

The game proceeds over multiple periods. In each period, players first observe the other player's action from the previous period, if any, and then decide whether to guess the color of their own hat. Once a player makes a guess, the guess is final and cannot be changed in later periods. The game ends when both players have made a guess. In the Inaction treatment, choosing neither Red nor Blue corresponds to taking no action, and the game continues. In the Revealed treatment, choosing "Cannot tell" is theoretically equivalent to taking no action in the Inaction treatment, which allows the game to continue.

Payoffs are designed to reward correct guesses and penalize incorrect guesses. A correct guess yields a positive payoff, whereas an incorrect guess yields a negative payoff. Payoffs are discounted across periods: both the gain from a correct guess and the loss from an incorrect guess decrease in absolute value over time. From period 6 onward, the absolute values of the payoffs are reduced to zero, so the game ends automatically if it reaches period 6. Within each period, the loss from an incorrect guess is larger in absolute value than the gain from a correct guess, reducing the incentive to guess randomly. The detailed payoff matrix is presented in Appendix B.

Participants complete eight rounds. In each round, each player's hat color is randomly and independently determined, with equal probability of Red and Blue. Given the realized hat colors, the round is classified as either a same-color or different-color case. At the beginning of the experiment, participants are randomly paired, and pairs remain fixed across all eight rounds. Treatment assignment and the order of configurations are determined by pre-randomization.

4.1.3 Experiment 3: Matching Market

Experiment 3 employs a one-to-one matching environment with incomplete-information. Each market consists of two workers and two firms. Workers observe the payoff-relevant state of the market, whereas firms do not. Firms therefore need to infer the state from observed market activities before they form matches.

At the beginning of the experiment, participants are randomly divided into fixed four-player groups. Each group consists of two workers and two firms, and the group remains fixed across the eight rounds. The role of worker or firm remains fixed throughout the experiment. Workers are labeled Lemon and Mango, and firms are labeled Yellow and Green. In each round, worker-role participants are assigned to be Lemon or Mango with equal probability, and firm-role participants are assigned to be Yellow or Green with equal probability. The pre-randomization ensures that, across the eight rounds, each firm-role participant experiences a balanced mix of easier and harder firm roles. A worker can be matched only with a firm, and any participant may also remain unmatched.

Each matching market has two possible states, Sunny and Rainy, each occurring with probability one half. Workers observe the realized state, while firms do not. States are independently pre-randomized across rounds. All participants observe the payoff matrix of the current market, which displays the payoffs of workers and firms for each possible match under each possible state. Remaining unmatched yields a payoff of 10. Matching payoffs may be higher or lower than 10, depending on the match and the state.

We use a 2×2 design that varies along two dimensions: market type and information structure. The market type is varied within-subject. In market type *learning-from-blocking* (LB), from a theoretic perspective, participants can make correct belief updating about the state from active changes in matching outcomes (i.e., match formation by other players in the market). By contrast, in market type *learning-from-no-blocking* (LNB), participants can update their beliefs correctly from the maintenance of the unmatched status quo according to the theory.

The second treatment variation concerns what information is publicly displayed during the market, and is varied between-subject. In the *Inaction* treatment, public information includes only changes in matching outcomes, such as the formation or dissolution of a match. In the *Revealed* treatment, public information additionally includes the underlying acceptance and rejection decisions underlying match-formation attempts. For example, if Lemon makes a proposal to Green and Green accepts or rejects it, this information is immediately displayed to all participants in the market. Thus, the Revealed treatment makes observable the actions that may otherwise be hidden behind an unchanged matching outcome.

Participants complete eight rounds, each involving a different matching game as displayed in Appendix A.3. They consist of four games for each market type. The order of the eight games is randomly determined, subject to the restriction that both the first and second half contains two games of each type. This restriction balances exposure to the two market types between the early and later parts of the experiment. Treatment assignment and the order of market configurations are determined by pre-randomization.

4.2 Procedures

Experiments 1 and 2 were conducted at the Shanghai University of Finance and Economics in December 2025 and April 2026, respectively. Experiment 3 was conducted at Shanghai Jiao Tong University in April 2026. Subjects were recruited from the subject pool of the Economics Lab at the corresponding university.

In total, 152, 114, and 124 subjects participated in Experiments 1, 2, and 3, respectively. Table 5 summarizes the experimental design. In Experiment 1, subjects were assigned to one of three treatments: Baseline (51), Inaction (50), or Revealed (51). Because Experiment 1 involved individual decision-making, each subject served as an independent observation. Experiment 2 included 114 subjects: 58 in the Inaction treatment, forming 29 independent pairs, and 56 in the Revealed treatment, forming 28 independent pairs. Experiment 3 included 124 subjects: 64 in the Inaction treatment, forming 16 independent four-player groups, and 60 in the Revealed treatment, forming 15 independent four-player groups.

Table 5: Summary of subjects in the three experiments

Experiment	Baseline	Inaction	Explicit	Total subjects
1: State-guessing task	51	50	51	152
2: Colored-hats game	–	58	56	114
3: Matching market	–	64	60	124

Each subject participated once and in only one experiment. The participant pool consisted primarily of undergraduate students from a variety of majors at the corresponding universities. Treatments were randomized at the individual level in Experiment 1, at the pair level in Experiment 2, and at the four-player-group level in Experiment 3. This randomization allowed multiple treatments or parameter orders to be implemented within the same experimental session.

All experiments were computerized using oTree (Chen, Schonger and Wickens, 2016) and conducted in Chinese. English translations of the instructions and screenshots are provided in Appendix B. Upon arrival, subjects were randomly assigned a card indicating their table number and were seated in the corresponding cubicles. Instructions were displayed on participants' computer screens, and participants completed a set of control questions to ensure comprehension. The same experimenter oversaw all experimental sessions.

At the end of each experiment, subjects completed a short demographic survey. In Experiments 1, 2, and 3, respectively, three of 15 rounds, two of 8 rounds, and four of 8 rounds were randomly selected for payment. The experimental currency was denominated directly in Chinese yuan (CNY). Average earnings in Experiments 1, 2, and 3 were 40.39, 50.20, and 64.15

CNY, respectively. These amounts include a participation bonus of 20 CNY in Experiment 1 and 15 CNY in Experiments 2 and 3. Sessions lasted about 34 minutes in Experiment 1, 36 minutes in Experiment 2, and 45 minutes in Experiment 3.

4.3 Hypotheses

4.3.1 Experiment 1

In Experiment 1, the blue signal in the Baseline treatment and the absent signal in the Inaction and Revealed treatments are informationally equivalent. A Bayesian decision maker should therefore condition on the informational content of the signal rather than on whether the signal is displayed explicitly. However, if participants underreact to inaction, they will place less weight on an absent signal than on an explicit blue signal. This leads to the following hypothesis.

Hypotheses 1. *In the pivotal signal configurations, the correct rate is highest in the Baseline treatment and lowest in the Inaction treatment, with the Revealed treatment lying in between. Specifically, this ranking applies when participants receive one blue or absent signal in Set A, and when participants receive one red signal and one blue or absent signal in Set B. For non-pivotal signal configurations, there are no systematic treatment differences.*

The Revealed treatment is predicted to improve performance relative to the Inaction treatment because it makes the absence of a signal salient. However, if participants still process explicit signals more readily than absent signals, performance in the Revealed treatment may remain below that in the Baseline treatment.

4.3.2 Experiment 2

In the colored-hats game, the informational value of the opponent's inaction differs across game types. In the different-color case, a player can often infer her own hat color directly from the public announcement and the opponent's hat color. In the same-color case, correct inference requires greater reliance on the opponent's failure to guess in earlier periods. Thus, the same-color case places more weight on learning from inaction. This leads to the following hypothesis.

Hypotheses 2. *In the colored-hats game, the correctness rate is higher in the different-color case than in the same-color case. In the same-color case, the correctness rate is weakly higher in the Revealed treatment than in the Inaction treatment.*

The first part of the hypothesis follows from the fact that the same-color case involves more inference from inaction. The second part follows from the prediction that explicitly displaying "Cannot tell" as an action helps participants recognize the informational content of the opponent's inability to make a guess.

4.3.3 Experiment 3

In the incomplete-information matching environment, firms can infer the state from the behavior of others in the market.In the market type LB, information is conveyed through active changes in matching outcomes. In LNB markets, information is conveyed through maintaining the unmatched status quo. Since inaction is less salient than action, we expect participants to learn less effectively in the latter type.

The Revealed treatment makes the acceptance and rejection decisions underlying match-formation attempts publicly observable. This additional information should be especially useful in the second market type, where the absence of a change in matching outcomes may otherwise obscure the actions that led to the status quo. This leads to the following hypothesis.

Hypotheses 3. *In the incomplete-information matching environment, the correctness rate is higher in LB than in LNB markets. In the latter type, the correctness rate is higher in the Revealed treatment than in the Inaction treatment.*

The treatment difference generated by the information structure is predicted to be strongest in LNB markets because these markets rely most directly on learning from inaction. In LB markets, where information is conveyed by active changes in matching outcomes, the additional display of acceptances and rejections is expected to play a smaller role.

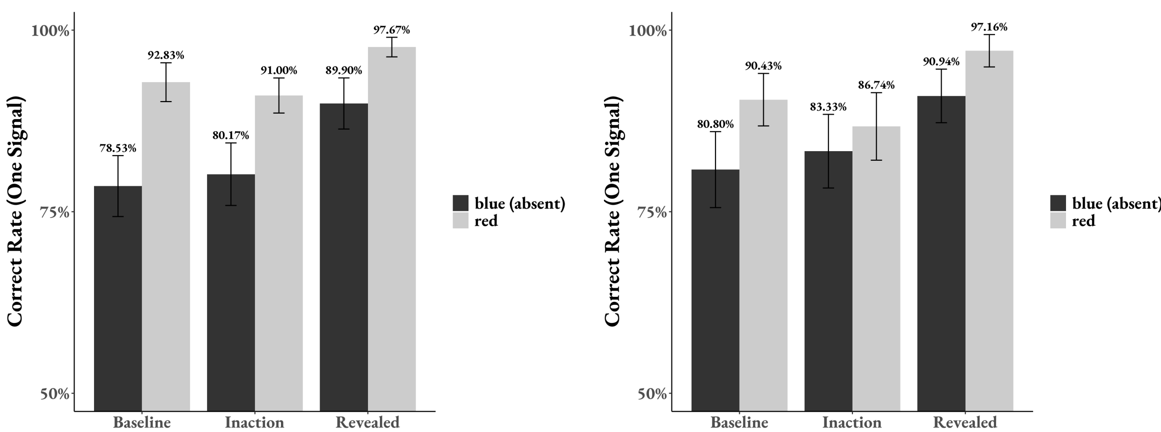
5 Results

In this section, we present and discuss the experimental results from Experiments 1-3 separately.

5.1 Experiment 1

For Experiment 1, we focus on Bayesian correctness rates by signal type and treatment. Bayesian correctness is defined as whether a participant's guess coincides with the optimal Bayesian choice implied by the information structure and the signals received. This choice may differ from the true underlying state. Bayesian correctness therefore provides a more appropriate measure of whether participants made the optimal decision given the information available to them.

5.1.1 Aggregate results: pooled orders



(a) All 5 rounds

(b) Last 3 rounds

Figure 1: Individual Correct Rate (One Signal)

We begin by presenting the aggregate results, pooling across both treatment orders, for the one-signal task. Figure 1 reports average Bayesian correctness rates by treatment and signal type, separately for all five rounds and for the last three rounds. Table 6 reports the corresponding pairwise non-parametric tests. As shown in Figure 1, correctness rates are above 50% in all conditions, indicating that participants perform better than random guessing.

Figure 1a shows that, across all rounds, Bayesian correctness is consistently higher after a red signal than after a blue signal or no signal. These differences are significant at the 5% level in the Baseline and Revealed treatments and marginally significant in the Inaction treatment. This pattern is expected: in the one-signal task, correct inference is more difficult when the signal is blue or absent. A red signal reinforces the prior, making Red the straightforward Bayesian choice. By contrast, a blue or absent signal requires participants to update against the prior and infer that Blue is more likely. In the last three rounds, shown in Panel (b), these differences are no longer statistically significant in any treatment, suggesting that learning reduces the performance gap between red and blue or absent signals.

Table 6: Individual correct rates: pairwise tests in the one-signal task

	Baseline		Inaction		Revealed	
	blue	red	blue	red	blue	red
	($n = 51$)	($n = 50$)	($n = 50$)	($n = 50$)	($n = 51$)	($n = 50$)
Within-treatment p-value	0.002 ($n = 50$)		0.090 ($n = 50$)		0.035 ($n = 50$)	
blue: Baseline vs Inaction			0.680			
blue: Baseline vs Revealed			0.014			
blue: Inaction vs Revealed			0.033			
red: Baseline vs Inaction			0.358			
red: Baseline vs Revealed			0.112			
red: Inaction vs Revealed			0.013			

(a) All 5 rounds

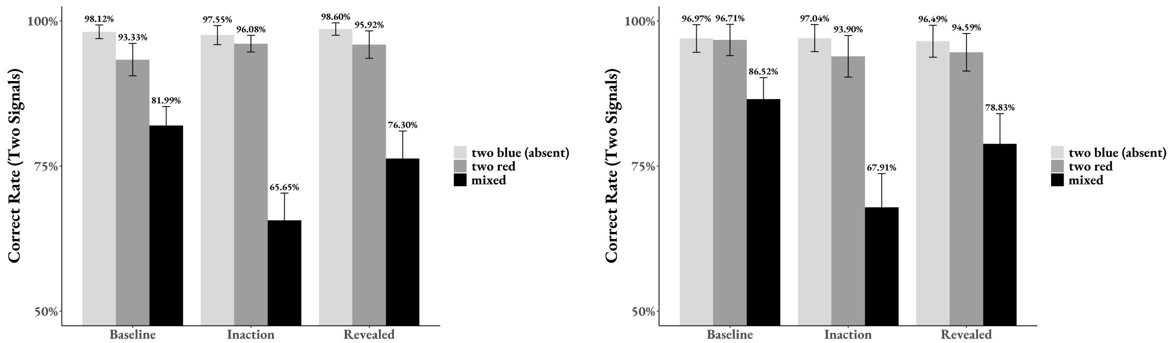
	Baseline		Inaction		Revealed	
	blue	red	blue	red	blue	red
	($n = 46$)	($n = 47$)	($n = 47$)	($n = 44$)	($n = 46$)	($n = 47$)
Within-treatment p-value	0.142 ($n = 42$)		0.533 ($n = 41$)		0.109 ($n = 42$)	
blue: Baseline vs Inaction			0.627			
blue: Baseline vs Revealed			0.115			
blue: Inaction vs Revealed			0.274			
red: Baseline vs Inaction			0.644			
red: Baseline vs Revealed			0.079			
red: Inaction vs Revealed			0.033			

(b) Last 3 rounds

Notes: Within-treatment p-values compare correct rates between blue and red signals within each treatment using two-sided Wilcoxon signed-rank tests. Between-treatment p-values compare correct rates across treatments within each signal category using two-sided Wilcoxon–Mann–Whitney rank-sum tests. Column-level sample sizes report the numbers of observations in each treatment–signal cell. For within-treatment tests, the sample size in parentheses reports the number of matched pairs used in the signed-rank test.

Next, we compare treatment differences by signal type. When the signal is blue or absent, performance across all rounds is significantly higher in the Revealed treatment than in the other two treatments at the 5% level, while the Baseline and Inaction treatments do not differ significantly from each other. In the last three rounds, however, there are no significant differences between any pair of treatments. When the signal is red, all-round performance is significantly higher only in the Revealed treatment than in the Inaction treatment, and this difference persists in the last three rounds.

Our main interest lies in pivotal configurations in which the signal is blue or absent, because these are the cases in which participants observe either an explicit blue signal, no displayed signal, or an explicit reminder that no signal is present. The results suggest that there is no persistent treatment effect in these cases. When a treatment effect does emerge, it indicates that making the absence of a signal explicit improves performance, even relative to directly displaying the blue signal.



(a) All 10 rounds

(b) Last 5 rounds

Figure 2: Individual Correct Rate (Two Signals)

We next discuss the aggregate results for the two-signal case, pooling across the two treatment orders. Figure 2 presents the average Bayesian correctness rates by treatment and signal type, separately for all 5 rounds and for the last 3 rounds. Table 7 reports the corresponding pairwise non-parametric test results. As shown in Figure 2a, performance over all rounds is significantly lower when participants receive mixed signals than when they receive two identical signals; these differences are significant at the 1% level in all treatments.

This result confirms that, in the two-signal task, correct inference is most difficult when the two signals differ. When the two signals are identical, they provide strong evidence in favor of the corresponding state. By contrast, when the signals differ, participants must perform a more subtle Bayesian update, under which the optimal guess is Blue, the state with the higher prior. Turning to the last 3 rounds, Figure 2b shows that these differences persist at the 5% level in all but one comparison: Mixed versus Two red in the Revealed treatment. This suggests that the difficulty associated with mixed signals does not substantially diminish over time.

Table 7: Individual correct rates: pairwise tests in the two-signal task

	Baseline	Inaction	Revealed
Within-treatment comparisons			
Two blue vs Mixed	< 0.001	< 0.001	< 0.001
	$(n = 48)$	$(n = 49)$	$(n = 50)$
Two blue vs Two red	0.091	0.163	0.167
	$(n = 45)$	$(n = 47)$	$(n = 46)$
Mixed vs Two red	0.004	< 0.001	0.001
	$(n = 48)$	$(n = 48)$	$(n = 47)$
Between-treatment comparisons			
Two blue: Baseline vs Inaction	1.000 $(n = 48$ vs. $49)$		
Two blue: Baseline vs Revealed	0.621 $(n = 48$ vs. $50)$		
Two blue: Inaction vs Revealed	0.625 $(n = 49$ vs. $50)$		
Two red: Baseline vs Inaction	0.905 $(n = 48$ vs. $48)$		
Two red: Baseline vs Revealed	0.369 $(n = 48$ vs. $47)$		
Two red: Inaction vs Revealed	0.414 $(n = 48$ vs. $47)$		
Mixed: Baseline vs Inaction	0.010 $(n = 51$ vs. $50)$		
Mixed: Baseline vs Revealed	0.711 $(n = 51$ vs. $51)$		
Mixed: Inaction vs Revealed	0.039 $(n = 50$ vs. $51)$		

(a) All 10 rounds

	Baseline	Inaction	Revealed
Within-treatment comparisons			
Two blue vs Mixed	0.021	< 0.001	0.005
	$(n = 40)$	$(n = 42)$	$(n = 37)$
Two blue vs Two red	0.577	0.636	0.531
	$(n = 31)$	$(n = 36)$	$(n = 24)$
Mixed vs Two red	0.009	0.002	0.094
	$(n = 34)$	$(n = 38)$	$(n = 36)$
Between-treatment comparisons			
Two blue: Baseline vs Inaction	0.982 $(n = 44$ vs. $45)$		
Two blue: Baseline vs Revealed	0.881 $(n = 44$ vs. $38)$		
Two blue: Inaction vs Revealed	0.863 $(n = 45$ vs. $38)$		
Two red: Baseline vs Inaction	0.693 $(n = 38$ vs. $41)$		
Two red: Baseline vs Revealed	0.615 $(n = 38$ vs. $37)$		
Two red: Inaction vs Revealed	0.922 $(n = 41$ vs. $37)$		
Mixed: Baseline vs Inaction	0.016 $(n = 47$ vs. $47)$		
Mixed: Baseline vs Revealed	0.569 $(n = 47$ vs. $50)$		
Mixed: Inaction vs Revealed	0.081 $(n = 47$ vs. $50)$		

(b) Last 5 rounds

Notes: The within-treatment rows report p-values from two-sided Wilcoxon signed-rank tests, with the number of matched pairs shown below each p-value. The between-treatment rows report p-values from two-sided Wilcoxon–Mann–Whitney rank-sum tests within each signal category, with sample sizes shown in parentheses. The table reports normal-approximation p-values. In treatments where blue corresponds to the absence of a signal, Two blue denotes two no-signal realizations.

Finally, we compare treatment differences by signal type in the two-signal task. When participants receive two blue (absent) signals or two red signals, we find no significant treatment differences. By contrast, when participants receive mixed signals, performance over all rounds is significantly lower in the Inaction treatment than in both the Baseline and Revealed treatments, with differences significant at the 1% and 5% levels, respectively. Turning to the last 5 rounds, the same pattern persists, although the differences are significant at the 5% level for the comparison with Baseline and at the 10% level for the comparison with Revealed. This stable pattern suggests that, when signals are mixed, replacing blue signals with their absence leads participants in the Inaction treatment to underreact to the informational content of the blue signal. Making the absence of a signal salient in the Revealed treatment improves learning, bringing performance close to the Baseline treatment, where blue and red signals are displayed in the same manner.

5.1.2 Order effect

In the experiment, we implement two treatment orders: either the one-signal task is played first or the two-signal task is played first. In this section, we examine whether the results are affected by order effects. Figures 3 and 4 present correctness rates in the one-signal and two-signal tasks by order. Table 8 reports Mann–Whitney tests for order effects by signal type and treatment, separately for the one-signal task in Panel (a) and the two-signal task in Panel (b). The table shows evidence of an order effect in only one condition: when participants receive two red signals in the Inaction treatment. Figure 4 shows that performance in this condition is higher when the one-signal task is played first, suggesting that experience with the one-signal task may facilitate learning in the two-signal task, but only for the two-red-signal case. Since this signal configuration is not central to our treatment-effect analysis, we base our main conclusions on the pooled sample.

Table 8: Order-effect tests for individual correct rates

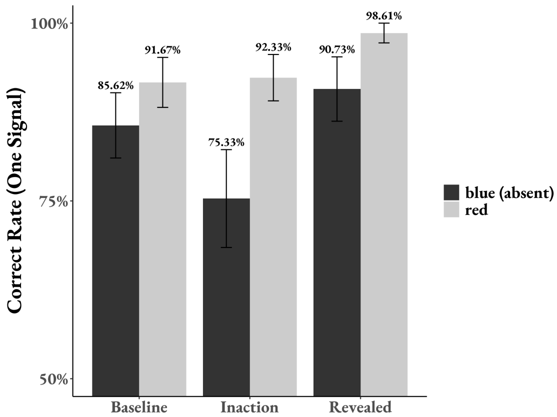
	blue (absent)	red
Baseline	0.181	0.336
	($n = 24$ vs. 27)	($n = 23$ vs. 27)
Inaction	0.331	0.551
	($n = 25$ vs. 25)	($n = 25$ vs. 25)
Revealed	0.935	0.587
	($n = 25$ vs. 26)	($n = 24$ vs. 26)

(a) One-signal task

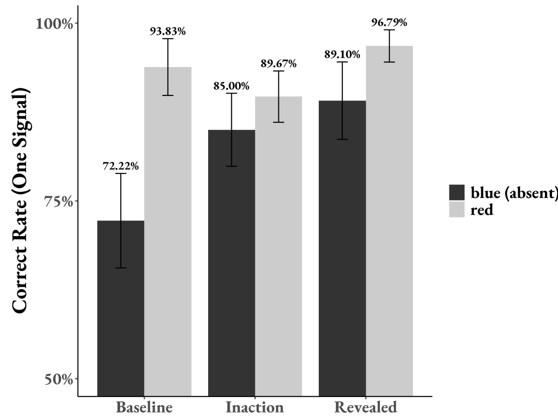
	Two blue (absent)	Mixed	Two red
Baseline	0.539	0.544	0.840
	($n = 24$ vs. 24)	($n = 24$ vs. 27)	($n = 22$ vs. 26)
Inaction	0.500	0.440	0.005
	($n = 25$ vs. 24)	($n = 25$ vs. 25)	($n = 24$ vs. 24)
Revealed	0.137	0.595	0.366
	($n = 24$ vs. 26)	($n = 25$ vs. 26)	($n = 22$ vs. 25)

(b) Two-signal task

Notes: Each cell reports the normal-approximation p-value from a two-sided Wilcoxon–Mann–Whitney rank-sum test comparing order 1 and order 2 within the corresponding treatment and signal category. The sample sizes in parentheses report the number of observations in order 1 vs. order 2. In treatments where blue corresponds to the absence of a signal, blue denotes No Signal.

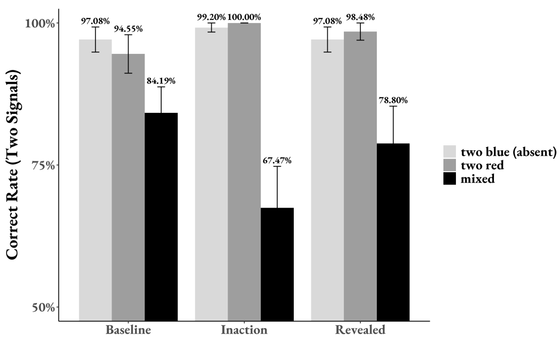


(a) One-Signal First

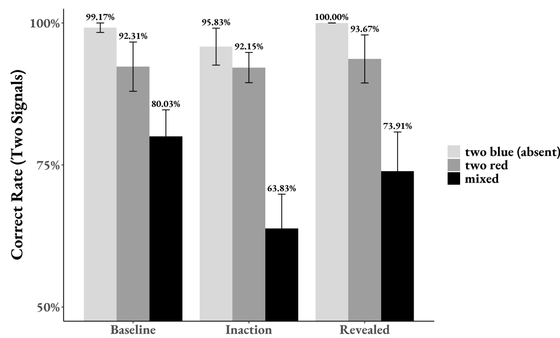


(b) Two-Signal First

Figure 3: Individual Correct Rate (One Signal) by Order



(a) One-Signal First



(b) Two-Signal First

Figure 4: Individual Correct Rate (Two Signals) by Order

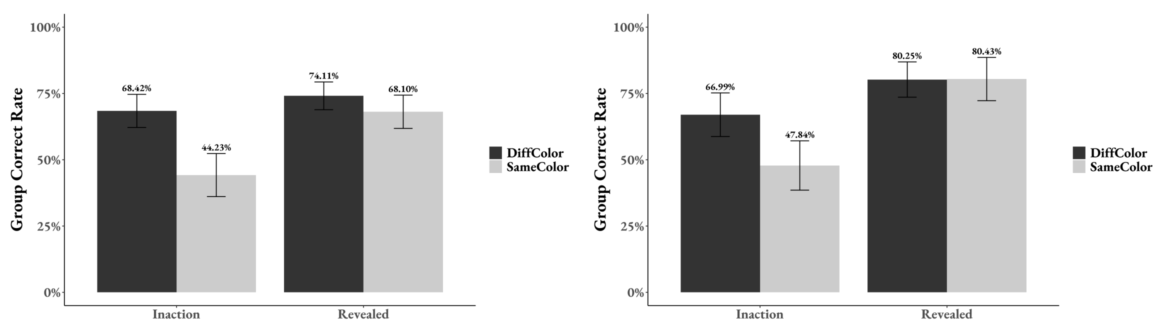
Result 1 (Experiment 1)**.** *In the pivotal signal configuration of the one-signal task, we do not find persistent treatment differences. In the pivotal signal configuration of the two-signal task, the correct rate is statistically indistinguishable between Baseline and Revealed, but is significantly lower in Inaction. These results partially support Hypothesis 1.*

This result yields two main insights. First, participants exhibit difficulty in belief updating when information is conveyed by the absence of a signal, but only when the absent signal is accompanied by a direct signal of the opposite type. Second, drawing attention to the absence of the signal improves performance, bringing it to the same level as in Baseline, where all signals are displayed directly.

5.2 Experiment 2: Colored-Hats Game

For Experiment 2, recall that the first treatment variable is whether inaction is represented as the absence of a guess or as the displayed choice "Cannot tell," corresponding to the Inaction and Revealed treatments, respectively. The second treatment variable is game type: different-color versus same-color games. Since learning from inaction is predicted to arise in period 2 of the same-color game, we examine whether participants find it more difficult to draw inferences from another player's inaction than from another player's stated guess, and whether representing inaction as "Cannot tell" improves performance. We consider two outcome measures: game-level correctness and individual mistake rates by period.

5.2.1 Game-level correctness



(a) All 8 Rounds (b) Last 4 Rounds

Figure 5: Market Correct Rates

At the game level, correctness coincides with correctness relative to the true hat colors: a pair is classified as correct only if both players correctly infer their own hat colors in the predicted period. Figure 5 reports game-level correctness rates by treatment and game type—the different-color game and the same-color game—separately for all eight rounds and the last four rounds. Table 9 reports the corresponding pairwise non-parametric tests. Across all eight rounds, the game-level correctness rate in the Inaction treatment is 68.42% in the different-color game but only 44.23% in the same-color game; this difference is statistically significant ($p = 0.003$). The same pattern persists, although more weakly, in the last four rounds, where the corresponding difference is marginally significant ($p = 0.072$). This result provides support for Hypothesis 2: in the Inaction treatment, the same-color game is more difficult than the different-color game, as it requires players to reason from their partner's inaction in period 1, whereas players in the different-color game can infer from their partner's stated guess.

Table 9: Game correctness rates: pairwise tests

	Inaction		Revealed	
	Diff ($n=29$)	Same ($n=28$)	Diff ($n=28$)	Same ($n=28$)
Within-treatment p-value	0.003 ($n=28$)		0.356 ($n=28$)	
Diff: Inaction vs Revealed	0.610			
Same: Inaction vs Revealed	0.053			

(a) All 8 rounds

	Inaction		Revealed	
	Diff ($n=26$)	Same ($n=27$)	Diff ($n=27$)	Same ($n=23$)
Within-treatment p-value	0.072 ($n=24$)		0.707 ($n=22$)	
Diff: Inaction vs Revealed	0.207			
Same: Inaction vs Revealed	0.010			

(b) Last 4 rounds

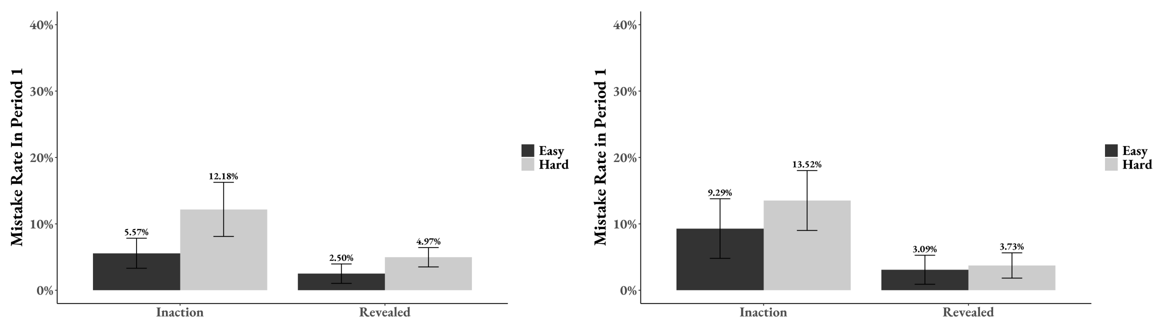
Notes: Each cell reports the p-value from a two-sided Wilcoxon signed-rank test for within-treatment comparisons (Diff vs. Same), and from a two-sided Wilcoxon–Mann–Whitney rank-sum test for between-treatment comparisons (Inaction vs. Revealed). Column-level sample sizes report the numbers of observations in each treatment–game-type cell. For within-treatment tests, the sample size in parentheses reports the number of matched pairs used in the signed-rank test. Observations are aggregated at the game-pair level.

By contrast, the Revealed treatment substantially reduces the difficulty gap between the different-color and same-color games. Across all rounds, game-level correctness rates in the Revealed treatment are 74.11% in the different-color game and 68.10% in the same-color game. In the last four rounds, the corresponding correctness rates are almost identical: 80.25% and 80.43%, respectively. None of these differences is statistically significant. Thus, representing the absence of a guess as an explicit action appears to eliminate the additional difficulty of the same-color game.

Between-treatment comparisons provide further evidence that the Revealed treatment mitigates the difficulty of the same-color game. In the same-color game, the performance is better in Revealed than in Inaction. The difference is marginally significant across all rounds, with correctness increasing from 44.23% in Inaction to 68.10% in Revealed ($p = 0.053$). It becomes statistically significant in the last four rounds, where correctness increases from 47.84% to 80.43% ($p = 0.010$). These results indicate that the Revealed treatment improves performance specifically in the difficult same-color game.

5.2.2 Individual mistake rates by period

We now examine how individuals in different roles perform over time. The game features two roles: an *easy role* and a *hard role*. A player is in the easy role when the public announcement differs from the partner's observed hat color, allowing her to infer her own hat color immediately in period 1. A player is in the hard role when the public announcement coincides with the partner's observed hat, so she cannot infer her own hat color in period 1 and should therefore refrain from making a color guess. Since the theoretical prediction is that both game types should end by period 2, we focus on participants' behavior in the first two periods. We pay particular attention to period-2 choices by *hard-role* players, who must infer their own hat color from their partner's period-1 action or inaction.



(a) All 8 Rounds

(b) Last 4 Rounds

Figure 6: Individual Mistake Rate in Period 1

Figure 6 reports individual mistake rates in period 1 by treatment and role, separately for all eight rounds and the last four rounds. Table 10 reports the corresponding pairwise tests. Period-1 mistake rates are generally low, suggesting that the first period is not the main source of difficulty. Across all rounds, mistake rates in the Inaction treatment are 5.57% in the easy role and 12.18% in the hard role. In the Revealed treatment, the corresponding rates are 2.5% and 4.97%. Hard-role mistake rates are higher in both treatments, but not significantly so. The same pattern holds in the last four rounds.

Table 10: Individual mistake rates in period 1: pairwise tests

	Inaction		Revealed	
	Easy	Hard	Easy	Hard
	($n = 29$)	($n = 29$)	($n = 28$)	($n = 28$)
Within-treatment p-value	0.265 ($n = 29$)		0.157 ($n = 28$)	
Easy: Inaction vs Revealed	0.285			
Hard: Inaction vs Revealed	0.235			

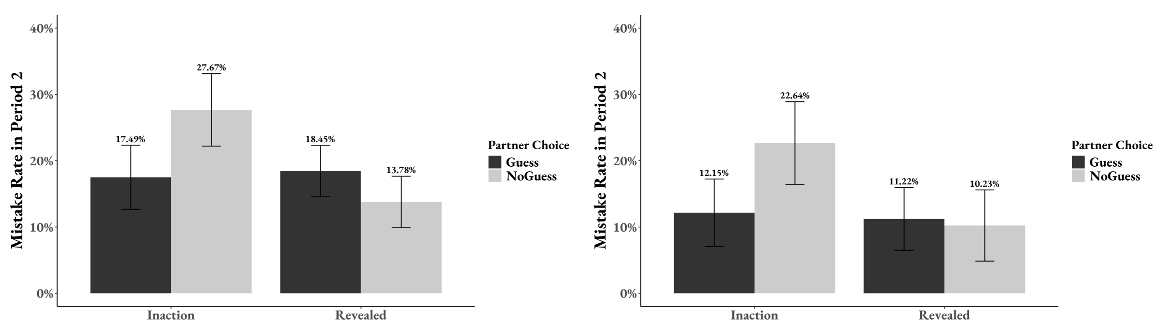
(a) All 8 rounds

	Inaction		Revealed	
	Easy	Hard	Easy	Hard
	($n = 26$)	($n = 29$)	($n = 27$)	($n = 28$)
Within-treatment p-value	0.339 ($n = 26$)		0.681 ($n = 27$)	
Easy: Inaction vs Revealed	0.215			
Hard: Inaction vs Revealed	0.069			

(b) Last 4 rounds

Notes: Each cell reports the p-value from a two-sided Wilcoxon signed-rank test for within-treatment comparisons (Easy vs. Hard), and from a two-sided Wilcoxon–Mann–Whitney rank-sum test for between-treatment comparisons (Inaction vs. Revealed). Column-level sample sizes report the numbers of observations in each treatment–role cell. For within-treatment tests, the sample size in parentheses reports the number of matched pairs used in the signed-rank test. Observations are aggregated at the game-pair level.

We find no significant treatment difference for the easy role in period 1. For the hard role, mistake rates are lower in Revealed than in Inaction, but the difference is not significant across all rounds and is only marginally significant in the last four rounds ($p = 0.069$). Overall, the limited differences across roles and treatments suggest that period 1 is not the main source of difficulty.



(a) All 8 Rounds (b) Last 4 Rounds

Figure 7: Individual Mistake Rate in Period 2

We therefore turn to period-2 decisions by hard-role players who are correct in period 1—that is, those who do not make a color guess—and who do not receive misleading information from their partner, meaning that the partner does not make an incorrect guess in period 1. Figure 7 reports mistake rates of hard-role players by treatment and information condition. We distinguish between two conditions: *Guess*, in which the partner makes a correct color guess in period 1, and *No Guess*, in which the partner does not make a color guess in period 1.

Conditional on the partner being correct, Guess corresponds to the different-color game, whereas No Guess corresponds to the same-color game. However, because an easy-role player may mistakenly fail to make a correct period-1 guess, we classify the information condition based on the hard-role player's realized information: the partner's actual period-1 action or inaction. In the Guess condition, a hard-role player is correct if her period-2 guess differs from her partner's period-1 guess. In the No Guess condition, she is correct if her period-2 guess differs from her partner's hat color. Table 11 reports the corresponding pairwise tests.

Table 11: Individual mistake rates in period 2: pairwise tests

Treatment	Inaction		Revealed	
Partner's Period-1 Choice	Guess ($n = 28$)	No Guess ($n = 25$)	Guess ($n = 28$)	No Guess ($n = 28$)
Within-treatment p-value	0.046 ($n = 25$)		0.104 ($n = 28$)	
Guess: Inaction vs Revealed	0.442			
No Guess: Inaction vs Revealed	0.049			

(a) All 8 rounds

Treatment	Inaction		Revealed	
Partner's Period-1 Choice	Guess ($n = 24$)	No Guess ($n = 23$)	Guess ($n = 26$)	No Guess ($n = 22$)
Within-treatment p-value	0.130 ($n = 19$)		0.981 ($n = 20$)	
Guess: Inaction vs Revealed	0.876			
No Guess: Inaction vs Revealed	0.054			

(b) Last 4 rounds

Notes: Each cell reports the p-value from a two-sided Wilcoxon signed-rank test for within-treatment comparisons (Guess vs. No Guess), and from a two-sided Wilcoxon–Mann–Whitney rank-sum test for between-treatment comparisons (Inaction vs. Revealed). Column-level sample sizes report the numbers of observations in each treatment–choice cell. For within-treatment tests, the sample size in parentheses reports the number of matched pairs used in the signed-rank test. Observations are aggregated at the game-pair level.

Consistent with the game-level findings, hard-role players in the Inaction treatment make significantly more period-2 mistakes in the No Guess condition than in the Guess condition across all rounds ($p = 0.046$), suggesting that participants have difficulty extracting information from their partner's inaction. By contrast, this gap is not significant in the Revealed treatment, indicating that representing inaction as "Cannot tell helps participants recognize its informational content. The same qualitative pattern persists in the last four rounds.

Between-treatment comparisons further show that Revealed improves performance specifically in the No Guess condition. Across all rounds, period-2 mistake rates in the Guess condition are similar across treatments: 17.49% in Inaction and 18.45% in Revealed. In the No Guess condition, however, the mistake rate is substantially higher in Inaction than in

Revealed: 27.67% versus 13.78% ($p = 0.049$). The same pattern persists in the last four rounds, where the No Guess mistake rate remains lower in Revealed than in Inaction, 10.23% versus 22.64%, with the difference marginally significant ($p = 0.054$). Since Revealed does not provide additional information in the Guess condition, where the partner's color guess is already observable, its effect arises primarily when the partner does not guess and the informational content of inaction must be inferred.

Result 2 (Experiment 2)**.** *In the Inaction treatment, the same-color game is significantly more difficult than the different-color games. The Revealed treatment significantly improves performance in the same-color game. These results support Hypothesis 2.*

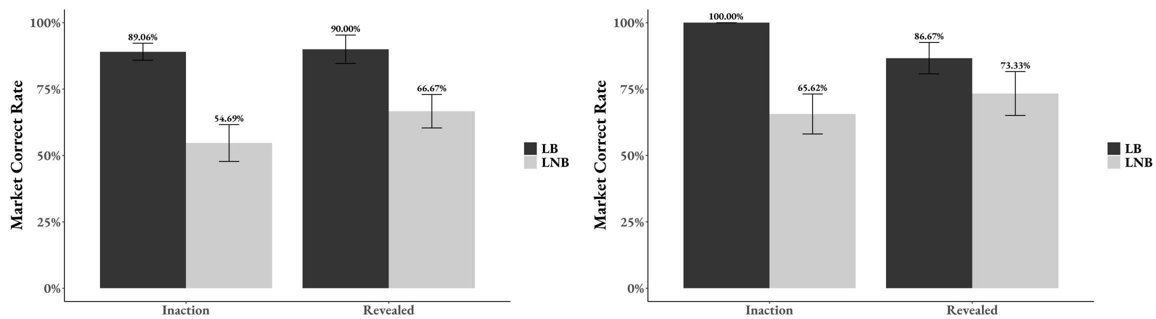
These findings yield two main insights. First, participants have difficulty inferring information from their partner's inaction. This difficulty appears both at the game level, where the same-color case is harder in the Inaction treatment, and at the individual level, where No Guess condition generates higher mistake rates. Second, the Revealed treatment, which represents the no-choice action as "Cannot tell," improves inference from inaction and largely eliminates the difficulty gap observed in the Inaction treatment.

5.3 Experiment 3

For Experiment 3, we examine whether participants find it more difficult to learn from market inaction, namely the absence of blocking activity, than from market action, namely observed blocking activity. We also examine whether this difficulty can be mitigated by revealing otherwise hidden market actions, such as rejections and acceptances, in the Revealed treatment. We consider two outcome measures: market-level correctness and individual mistake rates.

5.3.1 Market Outcome

We first examine whether matching markets converge to the theoretically correct outcome. A 4-player market is classified as *correct* if all four players reach their predicted matches. Figure 8 reports market-level correctness rates by treatment and learning pattern, separately for all eight rounds and the last four rounds. Table 12 reports the corresponding pairwise non-parametric tests. We distinguish between two learning patterns. In *Learning from Blocking* (LB) markets, firms can infer the state from the formation of a match. In *Learning from No Blocking* (LNB) markets, correct inference instead requires firms to learn from the absence of blocking activity.



(a) All 8 Rounds

(b) Last 4 Rounds

Figure 8: Market Correct Rates

Table 12: Market correct rates: pairwise tests across learning patterns

	Inaction ($n = 16$)		Revealed ($n = 15$)	
	LB	LNB	LB	LNB
Within-treatment p-value	0.001		0.003	
LB: Inaction vs Revealed	0.421			
LNB: Inaction vs Revealed	0.152			
LB-LNB: Inaction vs Revealed	0.304			

(a) All 8 rounds

	Inaction ($n = 16$)		Revealed ($n = 15$)	
	LB	LNB	LB	LNB
Within-treatment p-value	0.002		0.102	
LB: Inaction vs Revealed	0.029			
LNB: Inaction vs Revealed	0.438			
LB-LNB: Inaction vs Revealed	0.073			

(b) Last 4 rounds

Notes: Each cell reports the normal-approximation p-value from a two-sided Wilcoxon signed-rank test for within-treatment comparisons (LB vs. LNB), and from a two-sided Wilcoxon–Mann–Whitney rank-sum test for between-treatment comparisons (Inaction vs. Revealed). Treatment-level sample sizes are reported in the column headers. Observations are aggregated at the matching-group level.

We begin with the Inaction treatment, in which participants observe changes in matching outcomes only. Across all eight rounds, the market-level correctness rate is substantially higher in LB markets than in LNB markets: 89.06% versus 54.69%, respectively. This difference is statistically significant ($p = 0.001$). This gap persists in the last four rounds, where correctness reaches 100.00% in LB but remains only 65.63% in LNB markets ($p = 0.002$). These results indicate a persistent difficulty in learning from market inaction relative to market action.

We next examine whether Revealed mitigates this difficulty by making acceptance and rejection decisions observable. Across all eight rounds, correctness remains significantly higher in LB than in LNB markets: 90.00% versus 66.67% ($p = 0.003$). Between-treatment comparisons show no significant differences in either LB or LNB markets, and the change in the LB–LNB gap is not statistically significant. Thus, over all eight rounds, we find no clear evidence that the Revealed reduces the relative difficulty of LNB.

In the last four rounds, however, the LB–LNB difference is no longer statistically significant in Revealed. The narrowing of the gap is not driven by a significant improvement in LNB markets, where correctness increases from 65.63% in Inaction to 73.33% in Revealed. Instead, it partly reflects lower correctness in LB markets, which decreases from 100.00% to 86.67% ($p = 0.029$). As a result, the LB–LNB gap is marginally smaller in Revealed than in Inaction ($p = 0.073$). A difference-in-differences regression using the full round-level data confirms this pattern: the interaction term is statistically significant, indicating that Revealed reduces the relative difficulty of LNB markets (see Table 13).

Overall, the market-level results show persistent difficulty in learning from the absence of blocking activity. Revealing acceptance and rejection decisions attenuates the relative LB–LNB gap in later rounds, although this is partly driven by weaker performance in LB markets under Revealed. We therefore turn to individual mistake rates to better understand how Revealed affects participants' decisions.

Table 13: Difference-in-Differences Estimates of Market Correctness

	All 8 Rounds	Last 4 Rounds
β_0: Intercept	0.706^{***}	1.031^{***}
	(0.091)	(0.042)
β_1: LNB	-0.351^{***}	-0.341^{***}
	(0.063)	(0.073)
β_2: Revealed	0.003	-0.136^{**}
	(0.062)	(0.061)
β_3: LNB $\times$ Revealed	0.124	0.217^{**}
	(0.081)	(0.105)
Round fixed effects	Yes	Yes
R^2	0.162	0.127
Num. obs.	248	124
N Clusters	31	31
$H_0 : \beta_1 + \beta_3 = 0$	$p < 0.001$	$p = 0.092$
$H_0 : \beta_2 + \beta_3 = 0$	$p = 0.175$	$p = 0.463$

Notes: The dependent variable is market-level correctness. The estimated model is

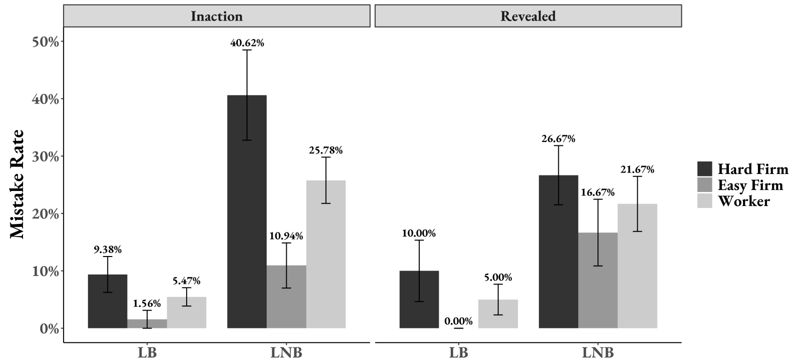
$$Y_{gr} = \beta_0 + \beta_1 \,\text{LNB}_{gr} + \beta_2 \,\text{Revealed}_g + \beta_3 \,(\text{LNB}_{gr} \times \text{Revealed}_g) + \gamma_r + \varepsilon_{gr},$$

where g indexes matching groups and r indexes rounds. The omitted category is an LB market in the Inaction treatment. Round fixed effects γ_r are included in all specifications. Standard errors, clustered at the matching-group level, are reported in parentheses. The test $H_0 : \beta_1 + \beta_3 = 0$ evaluates whether an LB–LNB gap remains in the Revealed treatment. The test $H_0 : \beta_2 + \beta_3 = 0$ evaluates the treatment effect of Revealed within LNB markets.

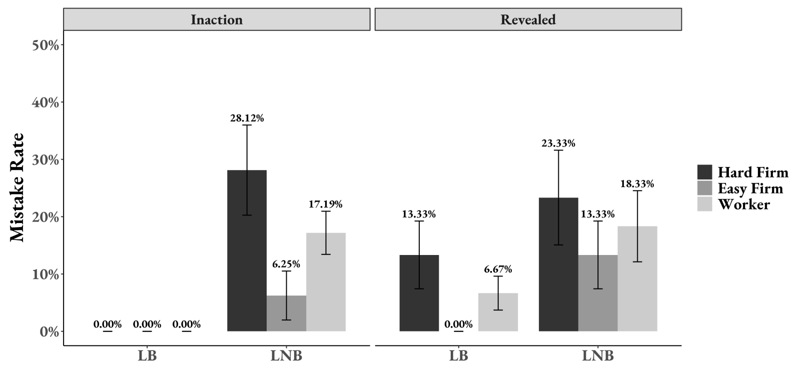
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

5.3.2 Individual Mistakes By Role

Given that markets often fail to reach the fully correct matching outcome, we next examine which roles drive these failures. Figure 9 reports mistake rates by treatment, learning pattern, and role, separately for all eight rounds and the last four rounds. We classify firms as *hard* or *easy* depending on whether they must learn from others' market activities to reach the correct match. We do not analyze workers' mistake behavior separately, since these are mechanically reflected in firms' mistakes. Overall, Figure 9 shows that hard firms account for most mistakes across market conditions.



(a) All 8 rounds



(b) Last 4 rounds

Figure 9: Individual mistake rates by treatment, by learning pattern, and by role

We start with easy firms. For easy firms, a matching decision that is consistent with theoretical predictions is always correct. For hard firms, by contrast, whether a decision is correct depends on the activities generated by the easy firms. Table 14 reports the corresponding pairwise tests. Easy-firm mistakes are concentrated in LNB markets, with almost no mistakes in LB markets. Across all eight rounds, mistake rates in LNB markets are significantly higher than in LB markets in both treatments. In the last four rounds, easy firms make substantial mistakes only in LNB markets under Revealed; the LB–LNB difference remains significant in Revealed but not in Inaction. Thus, easy-firm mistakes contribute to the lower market-level performance in LNB markets, particularly under Revealed. To understand market-level failures, it is therefore crucial to focus on hard firms.

Table 14: Mistake rates for easy firms: pairwise tests across learning patterns

	Inaction ($n = 16$)		Revealed ($n = 15$)	
	LB	LNB	LB	LNB
Within-treatment p-value	0.026		0.009	
LB: Inaction vs Revealed	0.333			
LNB: Inaction vs Revealed	0.529			
LB–LNB: Inaction vs Revealed	0.348			

(a) All 8 rounds

	Inaction ($n = 16$)		Revealed ($n = 15$)	
	LB	LNB	LB	LNB
Within-treatment p-value	0.157		0.046	
LB: Inaction vs Revealed	–			
LNB: Inaction vs Revealed	0.326			
LB–LNB: Inaction vs Revealed	0.326			

(b) Last 4 rounds

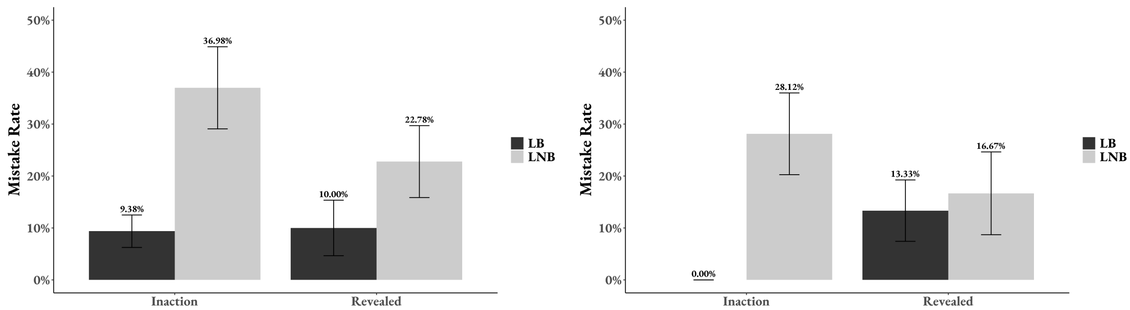
Notes: Each cell reports the p-value from a two-sided Wilcoxon signed-rank test for within-treatment comparisons (LB vs. LNB), and from a two-sided Wilcoxon–Mann–Whitney rank-sum test for between-treatment comparisons (Inaction vs. Revealed). Treatment-level sample sizes are reported in the column headers. Observations are aggregated at the matching-group level. In LB markets during the last four rounds, the easy-firm mistake rate is zero in both treatments; the p-value for the between-treatment comparison is therefore unavailable.

In summary, the market-level results show that LNB markets perform worse than LB markets in the Inaction treatment, indicating greater difficulty in learning from market inaction than from market action. This difficulty persists in the last four rounds, suggesting that repeated exposure alone does not eliminate it. Revealing acceptance and rejection decisions mitigates the LB–LNB gap in later rounds. Second, the individual-level results clarify the sources of market failure. Easy firms generally make few mistakes, although their mistakes remain a non-negligible source of failure in LNB markets. Hard firms account for most mistakes overall. We therefore next examine hard-firm behavior in greater detail.

5.3.3 Hard Firm Mistake rates

In this section, we focus on hard firms, whose matching decisions require inferring the state from easy firms' market activities. The correct decisions for hard firms therefore depend on whether easy firm behave as predicted. As shown in Section 5.3.2, easy firms sometimes make mistakes, especially in LNB markets. We therefore classify decisions of hard firms as correct only conditional on easy firms making predicted decisions: in LB markets, easy firms are matched as predicted, whereas in LNB markets, they remain unmatched.[7]

[7] In the experimental data, all easy-firm mistakes in LB markets are under-matching mistakes: easy firms remain unmatched when they are predicted to form a match. By contrast, all easy-firm mistakes in LNB markets are over-matching mistakes: easy firms form a match when they are predicted to remain unmatched.



(a) All 8 rounds

(b) Last 4 rounds

Figure 10: Mistake rates for Hard Firms

Table 15: Mistake rates for hard firms: pairwise tests across realized learning paths

	Inaction ($n = 16$)		Revealed ($n = 15$)	
	LB	LNB	LB	LNB
Within-treatment p-value	0.004		0.018	
LB: Inaction vs Revealed	0.628			
LNB: Inaction vs Revealed	0.159			
LB–LNB: Inaction vs Revealed	0.170			

(a) All 8 rounds

	Inaction ($n = 16$)		Revealed ($n = 15$)	
	LB	LNB	LB	LNB
Within-treatment p-value	0.005		0.564	
LB: Inaction vs Revealed	0.029			
LNB: Inaction vs Revealed	0.231			
LB–LNB: Inaction vs Revealed	0.021			

(b) Last 4 rounds

Notes: Each cell reports the p-value from a two-sided Wilcoxon signed-rank test for within-treatment comparisons (LB vs. LNB), and from a two-sided Wilcoxon–Mann–Whitney rank-sum test for between-treatment comparisons (Inaction vs. Revealed). Treatment-level sample sizes are reported in the column headers. Observations are aggregated at the matching-group level.

Figure 10 reports hard-firm mistake rates by treatment and learning pattern, and Table 15 reports the corresponding pairwise non-parametric tests. In the Inaction treatment, the hard-firm results align with the market-level findings. Across all eight rounds, hard firms make substantially more mistakes in LNB than in LB: 36.98% versus 9.38% ($p = 0.004$). This gap persists with experience. In the last four rounds, the mistake rate in LB falls to zero, whereas the rate in LNB remains 28.12% ($p = 0.005$). These results indicate that hard firms have greater difficulty learning from market inactivity than from market activity.

We next examine whether revealing acceptance and rejection decisions improves hard-firms behavior. Across all eight rounds, the hard-firm mistake rate in LB is similar in Revealed and Inaction: 10.00% versus 9.38%. In LNB, however, the mistake rates fall from 36.98% in Inaction to 22.78% in Revealed, although this difference is not significant in the non-parametric test ($p = 0.159$). The difference-in-differences regression in Table 16 nevertheless provides evidence that Revealed improves performance in LNB relative to Inaction ($p = 0.044$), while the LB–LNB gap in Revealed is only marginally significant ($p = 0.089$).[8]

The effect becomes more nuanced in the last four rounds. Under Revealed, hard-firm mistake rates are 13.33% in LB and 16.67% in LNB, with no significant difference between the two learning patterns. Relative to Inaction, Revealed significantly reduces the LB–LNB gap ($p = 0.021$ in the non-parametric test). However, between-treatment comparisons suggest that this reduction is driven not by improved performance in LNB, but by significantly worse performance in LB under Revealed. The difference-in-differences regression in Table 16 confirms this pattern for the last four rounds. One possible explanation is that the additional information becomes distracting in LB markets, where it provides little additional useful information.

Overall, hard firms exhibit persistent difficulty learning in LNB markets relative to LB markets in the Inaction treatment. Revealing acceptance and rejection decisions mitigates this relative difficulty. Across all eight rounds, the narrower gap is driven primarily by fewer mistakes in LNB under Revealed. In the last four rounds, however, it is also driven by more mistakes in LB under Revealed.

Result 3 (Experiment 3)**.** *In the Inaction treatment, market-level correctness is significantly lower in LNB than in LB markets. The Revealed treatment mitigates this LB-LNB gap in the last four rounds. Hard firms account for most market-level mistakes and exhibit the same pattern. These results support Hypothesis 3.*

These findings yield two main insights. First, participants have persistent difficulty inferring the state from market inaction. This difficulty appears at both the market and individual level. Its persistence in the last four rounds suggests that repeated exposure alone does not eliminate the problem.

Second, revealing acceptance and rejection decisions mitigates the LB-LNB difficulty gap at both the market and individual levels. At the individual level, the LB-LNB gap in hard-firm mistake rates almost disappears in the Revealed treatment, as indicated by the regression

[8] The non-parametric within-treatment comparison of the LB-LNB gap in the Revealed treatment is statistically significant across all eight rounds ($p = 0.018$).

results. The channel underlying this disappearing gap is mixed: it is driven primarily by fewer mistakes in LNB, but also by more mistakes in LB. Taken together, the results suggest that greater transparency changes how participants learn from market activity, although additional information may not be uniformly beneficial across market types.

Table 16: Difference-in-Differences Estimates of Hard-Firm Mistake Rates

	All 8 Rounds	Last 4 Rounds
β_0: Intercept	0.231^{**}	0.001
	(0.091)	(0.040)
β_1: LNB	0.289^{***}	0.300^{***}
	(0.072)	(0.080)
β_2: Revealed	0.005	0.136^{**}
	(0.062)	(0.060)
β_3: LNB × Revealed	-0.190^{*}	-0.284^{***}
	(0.094)	(0.098)
Round fixed effects	Yes	Yes
R^2	0.126	0.102
Num. obs.	230	118
N Clusters	31	31
$H_0 : \beta_1 + \beta_3 = 0$	$p = 0.089$	$p = 0.764$
$H_0 : \beta_2 + \beta_3 = 0$	$p = 0.044$	$p = 0.159$

Notes: The dependent variable is Hard-Firm Mistake Rates. The estimated model is

$$Y_{gr} = \beta_0 + \beta_1 \,\text{LNB}_{gr} + \beta_2 \,\text{Revealed}_g + \beta_3 \,(\text{LNB}_{gr} \times \text{Revealed}_g) + \gamma_r + \varepsilon_{gr},$$

where g indexes matching groups and r indexes rounds. The omitted category is an LB market in the Inaction treatment. Round fixed effects γ_r are included in all specifications. Standard errors, clustered at the matching-group level, are reported in parentheses. The test $H_0 : \beta_1 + \beta_3 = 0$ evaluates whether an LB–LNB gap remains in the Revealed treatment. The test $H_0 : \beta_2 + \beta_3 = 0$ evaluates the treatment effect of Revealed within LNB markets.

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

6 Conclusion

In this paper, we studied belief formation and decision making in economic environments where individuals learn not only from actions, but also from inaction. Across a series of laboratory experiments featuring an individual state-guessing task, a non-cooperative colored-hats game, and a cooperative incomplete-information matching market, we documented a systematic pattern: participants deviated more from optimal belief formation and decision making when information was conveyed through inaction. This pattern is consistent with underreaction to inaction, whether represented by the absence of a signal, an opponent's failure to act, or market inactivity. We further showed that making inaction more salient improves learning, although not always fully. In the first two settings, inaction was represented as a distinct action; in the matching market, otherwise hidden acceptance and rejection decisions were revealed. These interventions helped participants better extract the informational content of inaction. Overall, our findings point to a robust bias in belief updating: individuals underreact to inaction, leading to suboptimal decisions across a range of economic environments. This bias can be mitigated when decision makers are reminded of the contingencies under which an action could have occurred but did not.

Our findings suggest that individuals may fail to incorporate information conveyed by inaction through two related channels. First, inaction may be less salient than action, and therefore less likely to trigger belief updating. Second, individuals may fail to recognize the states or contingencies that would have generated an action, making it difficult to infer from the absence of that action. This interpretation connects to recent work on contingent reasoning, as in Martínez-Marquina, Niederle and Vespa (2019). In our proposed mechanism, these two channels operate jointly: making the relevant contingencies clearer also makes inaction more salient. Future work could disentangle these two sources of underreaction to inaction through more targeted experimental designs, thereby clarifying how difficulties in contingent reasoning vary across different forms of information.

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Appendix A Parameters

A.1 State-Guessing: Parameters and Theoretical Predictions

One-signal state-guessing with prior Pr(Red) = 3/5 and Pr(Blue) = 2/5:

Stage one	State	Red		Blue	
	Signal	r	$\varnothing$	r	$\varnothing$
$(\Pr(\text{Red} \mid \cdot), \Pr(\text{Blue} \mid \cdot))$		$(\frac{3}{4}, \frac{1}{4})$	$(\frac{3}{7}, \frac{4}{7})$	$(\frac{3}{4}, \frac{1}{4})$	$(\frac{3}{7}, \frac{4}{7})$
Stage-two best response		Red	**Blue**	Red	**Blue**

Table A.1: State-guessing game with one signal.

Two-signal state-guessing with prior Pr(Red) = 2/5 and Pr(Blue) = 3/5:

Stage one	State	Red			Blue		
	Signals	rr	$r\varnothing$	$\varnothing\varnothing$	rr	$r\varnothing$	$\varnothing\varnothing$
$(\Pr(\text{Red} \mid \cdot),\ \Pr(\text{Blue} \mid \cdot))$		$(\frac{4}{7}, \frac{3}{7})$	$(\frac{2}{5}, \frac{3}{5})$	$(\frac{2}{9}, \frac{7}{9})$	$(\frac{4}{7}, \frac{3}{7})$	$(\frac{2}{5}, \frac{3}{5})$	$(\frac{2}{9}, \frac{7}{9})$
Stage-two best response		Red	**Blue**	Blue	Red	**Blue**	Blue

Table A.2: State-guessing game with two signals.

In the one-signal game, the prior is skewed towards Red. Correctly guessing the boldfaced **Blue** indicates that learning from inaction occurs. In the two-signal game, rr and $\varnothing\varnothing$ are arguably strong signals. For the pivotal situation $r\varnothing$, correctly guessing the boldfaced **Blue** (note that the prior is skewed towards Blue) indicates that $\varnothing$ is treated as salient as r, i.e., learning from inaction occurs.

A.2 Colored Hats: Parameters and Theoretical Predictions

	Player 1	Player 2
State	Red	Blue
Announcement	at least one Red[9]	
Period-one actions	claim red	(inaction)
Period-two actions	N.A.	claim blue

Table A.3: Learning from action when the state is (Red, Blue).

	Player 1	Player 2
State	Red	Red
Announcement	at least one Red	
Period-one actions	(inaction)	(inaction)
Period-two actions	claim red	claim red

Table A.4: Learning from inaction when the state is (Red, Red).

	Player 1	Player 2
State	Blue	Red
Announcement	at least one Blue	
Period-one actions	claim blue	(inaction)
Period-two actions	N.A.	claim red

Table A.5: Learning from action when the state is (Blue, Red).

	Player 1	Player 2
State	Blue	Blue
Announcement	at least one Blue	
Period-one actions	(inaction)	(inaction)
Period-two actions	claim blue	claim blue

Table A.6: Learning from inaction when the state is (Blue, Blue).

[9] Announcing "at least one blue" leads to symmetric actions (and inaction). In experiment, the announcement is randomly drawn from the two possibilities. The same remark applies to the announcement in Table A.5.

A.3 Matching Markets: Parameters and Theoretical Predictions

The predicted matches are indicated by boldfaced and underscored matching values.

	j_1		j_2	
i_1	**4**	**5**	-7	-1
	-2	2	-3	-8
i_2	1	-6	**3**	**4**
	-3	4	2	-7

(a) Learning from action

	j_1		j_2	
i_1	-3	1	-9	-1
	3	2	-2	-8
i_2	-1	-5	**4**	**3**
	-2	4	2	-7

(b) Learning from inaction

Figure A.1: Matching Parameter 1; each player's endowment is 10.

	j_1		j_2	
i_1	**1**	**7**	-5	1
	-7	5	-3	-2
i_2	-2	-9	**4**	**2**
	1	-6	1	-4

(a) Learning from action

	j_1		j_2	
i_1	-1	4	-5	5
	7	5	-3	-2
i_2	-2	-9	**2**	**1**
	-4	-6	1	-4

(b) Learning from inaction

Figure A.2: Matching Parameter 2; each player's endowment is 10.

	j_1		j_2	
i_1	**8**	**3**	-2	5
	-4	7	-4	1
i_2	-7	-6	**1**	**2**
	-5	-8	7	-5

(a) Learning from action

	j_1		j_2	
i_1	-8	5	-2	-5
	6	7	-4	1
i_2	-7	-6	**1**	**1**
	5	-8	7	-3

(b) Learning from inaction

Figure A.3: Matching Parameter 3; each player's endowment is 10.

	j_1		j_2	
i_1	**7**	**5**	-1	-7
	-3	6	-5	1
i_2	2	-9	**3**	**2**
	-2	-7	1	-5

(a) Learning from action

	j_1		j_2	
i_1	-7	9	-2	-3
	3	6	-1	-3
i_2	-6	4	**2**	**5**
	-2	7	1	-9

(b) Learning from inaction

Figure A.4: Matching Parameter 4; each player's endowment is 10.

Appendix B Instructions and Screenshots (Translated)

B.1 Experiment 1

B.1.1 Instructions

[Welcome]

Welcome to this experiment! Please read the following instructions carefully.

This experiment will last approximately 40 minutes. During the experiment, please remain silent and do not communicate with other participants in any way. If you have any questions, please raise your hand, and an experimenter will assist you individually.

Each participant will sit alone at a computer terminal, and all decisions will be made on the computer screen. The experiment is anonymous: neither the experimenters nor the other participants will know which participant is seated at which station, nor will they be able to identify whether a particular decision was made by you or by someone else in the room.

This is an individual decision-making experiment. Your earnings depend only on your own decisions and are independent of the decisions of other participants.

Because you arrived on time, you have received a participation fee of 20 yuan. You will earn additional income during the experiment based on your decisions. At the end of the experiment, your total earnings will be paid to you privately.

Throughout the experiment, all earnings will be denominated in "yuan" (RMB).

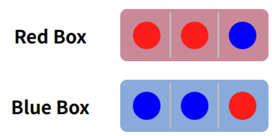
[Box-Guessing Task]

In today's experiment, you will complete a series of tasks that are entirely based on your own decisions. The setting is as follows.

There are 100 boxes in front of you. Each box is either a "Red Box" or a "Blue Box".

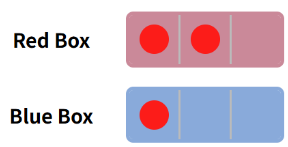
Baseline treatment:

Regardless of whether the box is a "Red Box" or a "Blue Box", each box has three doors. Behind each door there is one ball. A "Red Box" contains two red balls and one blue ball. A "Blue Box" contains one red ball and two blue balls. As illustrated below, in a "Red Box", there are red balls behind two of its doors and a blue ball behind the remaining door. In a "Blue Box", there is a red ball behind one door and blue balls behind the other two doors.



Inaction treatment and Revealed Treatment:

Regardless of whether the box is a "Red Box" or a "Blue Box", each box has three doors. Behind each door there may be a red ball, or there may be no ball. A "Red Box" contains two red balls, while a "Blue Box" contains one red ball. As illustrated below, in a "Red Box", there is a red ball behind two of the doors and no ball behind the remaining door. In a "Blue Box", there is a red ball behind one door and no ball behind the other two doors.



Each task proceeds through the following four steps.

Step 1: Learn the Proportion of Box Types

You will be informed of the proportions of Red Boxes and Blue Boxes among the 100 boxes. This proportion may vary across tasks.

For example, there may be 40 Red Boxes and 60 Blue Boxes among the 100 boxes.

Step 2: Computer Randomly Draws One Box

The computer randomly selects one box from the 100 boxes. Every box has an equal chance of being selected. You do not know whether the selected box is a Red Box or a Blue Box.

For example, if there are 40 Red Boxes and 60 Blue Boxes, then the probability that the computer selects a Red Box is 40%, while the probability that it selects a Blue Box is 60%.

Baseline treatment:

Step 3: Computer Randomly Opens Box Doors

After the box is selected, the computer is given one or more opportunities to open a door. The number of door openings may vary across tasks. Before each door opening, the computer randomly reshuffles the positions of the three balls inside the selected box and then randomly opens one door. As a result, the computer may observe either a red ball or a blue ball:

- If the selected box is a Red Box, the probability of observing a red ball is $\frac{2}{3}$ and the probability of observing a blue ball is $\frac{1}{3}$.
- If the selected box is a Blue Box, the probability of observing a red ball is $\frac{1}{3}$ and the probability of observing a blue ball is $\frac{2}{3}$.

After completing all door openings, the computer will summarize the information it has observed (the numbers of red-ball observations and blue-ball observations) and report this information to you.

Inaction treatment:

Step 3: Computer Randomly Opens Box Doors

After the box is selected, the computer is given one or more opportunities to open a door. The number of door openings may vary across tasks. Before each door opening, the computer randomly reshuffles the positions of the red balls inside the selected box and then randomly opens one door. As a result, the computer may observe a red ball:

- If the selected box is a Red Box, the probability of observing a red ball is $\frac{2}{3}$.
- If the selected box is a Blue Box, the probability of observing a red ball is $\frac{1}{3}$.

After completing all door openings, the computer will summarize the information it has observed (the numbers of red-ball observations) and report this information to you.

Revealed treatment:

Step 3: Computer Randomly Opens Box Doors

After the box is selected, the computer is given one or more opportunities to open a door. The number of door openings may vary across tasks. Before each door opening, the computer randomly reshuffles the positions of the red balls inside the selected box and then randomly opens one door. As a result, the computer may either observe a red ball or observe no ball:

- If the selected box is a Red Box, the probability of observing a red ball is $\frac{2}{3}$ and the probability of observing no ball is $\frac{1}{3}$.
- If the selected box is a Blue Box, the probability of observing a red ball is $\frac{1}{3}$ and the probability of observing no ball is $\frac{2}{3}$.

After completing all door openings, the computer will summarize the information it has observed (the numbers of red-ball observations and no-ball observations) and report this information to you.

Step 4: Guess the Type of the Box

You must then guess whether the box selected in Step 2 was a Red Box or a Blue Box.

You may choose either: “Red Box" or “Blue Box".

If your guess is correct, you earn 10 yuan for that task. If your guess is incorrect, you earn 0 yuan for that task.

If you are uncertain which type of box was selected, you should choose the box type that you believe is more likely. For example, if you believe the box is more likely to be a Red Box than a Blue Box, then you should choose Red Box.

After Step 4 is completed, the task ends and you proceed to the next task.

[Number of Tasks and Total Earnings]

You will complete a total of 15 tasks.

The procedures are identical across all tasks. The only possible differences are:

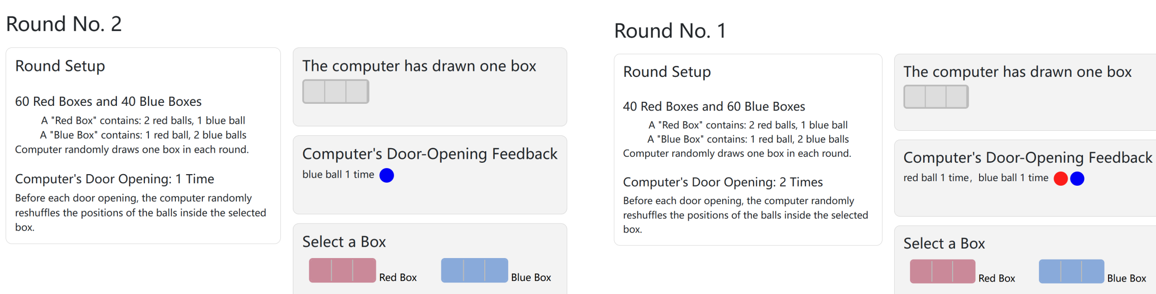
1. The proportion of Red Boxes and Blue Boxes in Step 1 may differ across tasks. Please pay attention to this information at the beginning of each task.
2. The number of door openings in Step 3 may differ across tasks. In some tasks, the computer opens only one door. In other tasks, the computer opens two doors. Before each door opening, the positions of the balls inside the selected box are reshuffled randomly.

After each task ends, the system automatically proceeds to the next task. You will not learn whether your guess was correct until all 15 tasks have been completed.

At the end of the experiment, the computer will randomly select 3 of the 15 tasks. Your earnings from those 3 selected tasks will count toward your experiment payment.

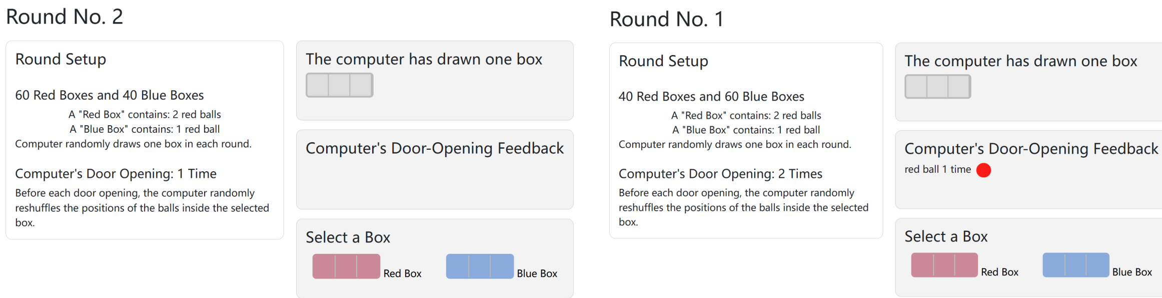
Your total payment = participation fee (20 yuan) + the sum of the final payoffs from the 3 randomly selected rounds.

B.1.2 Screenshots



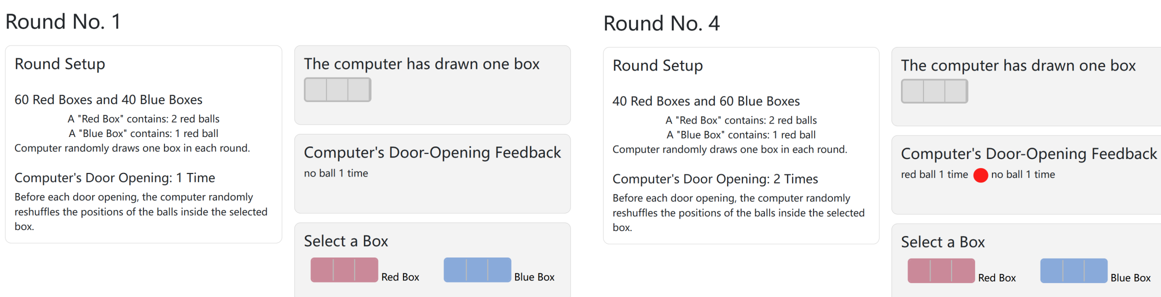
(a) One-signal Game (b) Two-signal Game

Figure A.5: Screenshots from Experiment 1 (Baseline)



(a) One-signal Game (b) Two-signal Game

Figure A.6: Screenshots from Experiment 1 (Inaction Treatment)



(a) One-signal Game (b) Two-signal Game

Figure A.7: Screenshots from Experiment 1 (Revealed Treatment)

B.2 Experiment 2

B.2.1 Instructions

[Welcome]

Welcome to this experiment! Please read the following introduction carefully.

This experiment will last approximately 40 minutes. During the experiment, please remain silent and do not communicate with others in any way. If you have any questions, please raise your hand, and an experimenter will assist you individually.

Each participant will sit alone at a computer terminal, and all decisions will be made on the computer screen. The experiment is anonymous: neither the experimenters nor the other participants will know which participant is seated at which station, nor will they be able to link any decision to you or to anyone else in the room.

Because you arrived on time, you have received a participation fee of 15 yuan. You will earn additional income during the experiment, depending on your own decisions and the decisions of other participants. At the end of the experiment, your total earnings will be paid to you privately.

Throughout the experiment, all earnings will be denominated in "yuan" (RMB).

[The Hat-Guessing Task]

In this experiment, you will be randomly paired with 1 other participant in the room to form a 2-person group. The experiment consists of 8 rounds of a "hat-guessing game" You will complete all 8 rounds with the same partner.

At the beginning of each round, you receive an initial payoff of 12 yuan. Your final payoff for the round depends on the outcome of the hat-guessing task.

[Task Procedure]

Each round of the hat-guessing task proceeds through the following steps.

Step 1: The Computer Randomly Assigns Hats

- The computer randomly assigns one hat to you and one hat to your partner. Each hat may be either Red or Blue, with equal probability; that is, each color occurs with probability 50%.
- The colors of your hat and your partner's hat are independently determined. In other words, the color of one hat does not affect the color of the other hat.
- Neither you nor your partner can see the color of your own hat. However, each of you can see the color of the other person's hat.

Step 2: The Computer Announces Public Information

The computer announces one piece of public information about the hat colors in your 2-person group. Both you and your partner observe this information, and the announced information is always true.

The announcement is one of the following:

- "At least one hat is Red."
- "At least one hat is Blue."

Step 3: Guessing the Color of Your Own Hat

You and your partner must determine the color of your own hats. You may need multiple opportunities—that is, multiple periods—to make a decision.

Inaction treatment:

<u>**Available Choices:**</u> In each period, you may choose "Red" or "Blue" as the color of your own hat. If you cannot determine your hat color, you may make no choice.

When making a choice, neither you nor your partner can observe the other participant's current-period choice. In other words, you make your choices simultaneously in each period.

If you choose "Red" or "Blue" in a given period—that is, if you make a guess about the color of your hat—your task for the round ends, and you do not need to make any further choices. You cannot revise your guess or choose a different color in a later period. **However, if you have not chosen either "Red" or "Blue," you proceed to the next period and may make a choice later.**

The round ends once both you and your partner have chosen the colors of your own hats.

[Task Payoffs]

A correct guess generates a positive payoff, while an incorrect guess generates a negative payoff. The payoff also depends on the period in which you make your guess.

The payoff table is shown below. The computer screen will display this table in every round.

	Correct Guess	Incorrect Guess
Period 1	11	-12
Period 2	7	-8
Period 3	4	-5
Period 4	2	-3
Period 5	1	-2
Period 6	0	0
...	0	0

For example, if you guess your hat color in Period 1 and your guess is correct, your payoff is 11 yuan. If you guess your hat color in Period 2 and your guess is incorrect, your payoff is -8 yuan.

As the number of periods increases, the absolute values of the payoffs associated with correct and incorrect guesses decrease. In Period 6 and all subsequent periods, the payoff from any guess is 0, regardless of whether the guess is correct or incorrect. Therefore, if the task reaches Period 6, the computer automatically ends the round.

[Choice History]

At the end of each period, the computer screen displays the history of choices made by you and your partner in previous periods. The recorded choices are “Red” and “Blue.” You can observe your partner’s previous choices, and your partner can observe your previous choices.

Suppose that in Period 1, you make no choice and your partner chooses “Red.” The task proceeds to Period 2. At that point, you see a table like the one below, which contains the payoff table and the history of previous choices.

	Correct	Incorrect	Your Choice	Partner's Choice
Period 1	11	-12		Red
Period 2	7	-8		
Period 3	4	-5		
Period 4	2	-3		
Period 5	1	-2		
Period 6	0	0		
...	0	0		

Choice History (Example)

Revealed treatment:

<u>Available Choices:</u> In each period, you may choose "Red," "Blue," or "Cannot tell."

When making a choice, neither you nor your partner can observe the other participant's current-period choice. In other words, you make your choices simultaneously in each period.

If you choose "Red" or "Blue" in a given period—that is, if you make a guess about the color of your hat—your task for the round ends, and you do not need to make any further choices. You cannot revise your guess or choose a different color in a later period. However, if you choose "Cannot tell," you proceed to the next period and may make a choice later.

The round ends once both you and your partner have chosen the colors of your own hats.

[Task Payoffs]

A correct guess generates a positive payoff, while an incorrect guess generates a negative payoff. The payoff also depends on the period in which you make your guess.

The payoff table is shown below. The computer screen will display this table in every round.

	Correct Guess	Incorrect Guess	Cannot Tell
Period 1	11	-12	Continue
Period 2	7	-8	Continue
Period 3	4	-5	Continue
Period 4	2	-3	Continue
Period 5	1	-2	Continue
Period 6	0	0	0
...	0	0	0

As the number of periods increases, the absolute values of the payoffs associated with correct and incorrect guesses decrease. In Period 6 and all subsequent periods, the payoff from any choice is 0, regardless of whether the choice is a correct guess, an incorrect guess, or "Cannot tell." Therefore, if the task reaches Period 6, the computer automatically ends the round.

[Choice History]

At the end of each period, the computer screen displays the history of choices made by you and your partner in previous periods. The recorded choices are "Red," "Blue," and "Cannot tell." You can observe your partner's previous choices, and your partner can observe your previous choices.

Suppose that in Period 1, you choose "Cannot tell" and your partner chooses "Red." The task proceeds to Period 2. At that point, you see a table like the one below, which contains the payoff table and the history of previous choices.

	Correct	Incorrect	Cannot Tell	Your Choice	Partner's Choice
Period 1	11	-12	Continue	Cannot Tell	Red
Period 2	7	-8	Continue		
Period 3	4	-5	Continue		
Period 4	2	-3	Continue		
Period 5	1	-2	Continue		
Period 6	0	0	0		
...	0	0	0		

Choice History (Example)

[Summary: When Does a Round End?]

- In any period, if you guess the color of your own hat—that is, if you choose "Red" or "Blue"—your task for the round ends.
- If at least one participant chooses "Cannot tell," the task proceeds to the next period.
- If both you and your partner have made guesses, the round ends.
- If the task reaches Period 6, the computer automatically ends the round.

Your final payoff for the round is calculated as follows:

$$\text{Final payoff for the round} = \text{initial payoff of 12 yuan} + \text{task payoff.}$$

[Number of Rounds and Total Earnings]

You will complete a total of 8 rounds of the hat-guessing task. The procedures are identical across all rounds.

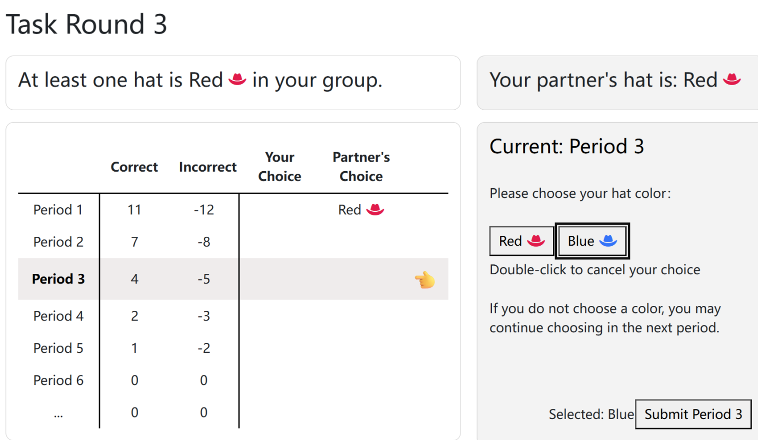
The rounds are independent. At the beginning of each new round, the computer randomly assigns a new hat color to each participant. The assigned colors are unrelated to the hat colors in previous rounds.

After each round ends, the system automatically proceeds to the next round. You will not learn whether your guess in a given round was correct until all 8 rounds have ended.

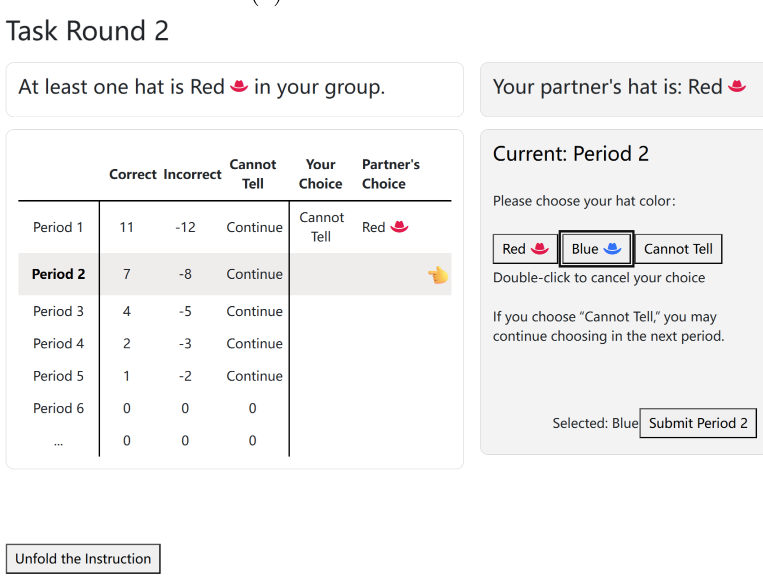
At the end of the experiment, the computer randomly selects 2 of the 8 rounds for payment. For each selected round: your final payoff for the selected round = initial payoff of 12 yuan + task payoff.

Your total payment = participation fee (15 yuan) + the sum of the final payoffs from the 2 randomly selected rounds.

B.2.2 Screenshots



(a) Inaction Treatment



(b) Revealed Treatment

Figure A.8: Screenshots from Experiment 2

B.3 Experiment 3

B.3.1 Instructions

[Welcome]

Welcome to this experiment! Please read the following introduction carefully.

This experiment will last approximately 45 minutes. During the experiment, please remain silent and do not communicate with others in any way. If you have any questions, please raise your hand, and an experimenter will assist you individually.

Before the experiment begins, you will be randomly assigned to a group consisting of 4 participants. This group will remain fixed throughout the entire experiment. Each participant will sit alone at a computer terminal, and all decisions will be made on the computer screen. The experiment is anonymous: neither the experimenters nor the other participants will know which participant is seated at which station, nor will they be able to link any decision to you or to anyone else in the room.

Because you arrived on time, you have received a participation fee of 15 yuan. You will earn additional income during the experiment, depending on your own decisions and the decisions of other participants. At the end of the experiment, your total earnings will be paid to you privately.

Throughout the experiment, all earnings will be denominated in "yuan" (RMB).

[Roles and Grouping]

Within the fixed group of 4 participants, you will be randomly assigned to one of two roles: 2 participants will be "Fruits" and 2 participants will be "Colors." These roles remain fixed throughout the entire experiment—once assigned, you will always be either a Fruit or a Color.

The experiment consists of 8 rounds, each corresponding to a different "matching game." In each round, 4 participants in the group will complete one matching game together.

At the beginning of every round, you receive an initial payoff of 10 yuan.

[Matching Game and Matching Payoffs]

Each matching game involves four participants: Lemon, Mango, Yellow, and Green. If your role is “Fruit,” you will be randomly assigned to be either Lemon or Mango each round. If your role is “Color,” you will be randomly assigned to be either Yellow or Green each round.

In each round, Fruits and Colors may form “matches,” and each Fruit can match with at most one Color, while each Color can match with at most one Fruit. For example, Lemon may match with Yellow, or with Green, or Lemon may remain unmatched. Two Fruits cannot match with each other, and two Colors cannot match with each other.

“Weather” affects the payoffs from matching. Each round’s weather may be Sunny or Rainy, with equal probability (50% each). The weather is determined independently each round by the computer. At the beginning of each round, the two Fruits will learn the weather, while the two Colors will not.

The payoffs for each possible Fruit–Color match are shown in a “Matching Payoff Table.” In the example below, you can find the payoff for any Fruit–Color pair at the corresponding cell of the table. Each cell contains three rows: the first row lists the payoffs under Sunny weather, and the second row under under Rainy. Each row contains two numbers: the first number is the Fruit’s payoff, and the second number is the Color’s payoff. For example:

- If Lemon matches with Yellow and the weather is Sunny, Lemon earns 3 yuan and Yellow earns 1 yuan.
- If Lemon matches with Green and the weather is Rainy, Mango earns –3 yuan and Yellow earns –4 yuan.

	Yellow		Green		
Lemon	3	1	-6	-7	Sunny
	2	4	-8	-9	Rainy
Mango	-1	-5	6	7	Sunny
	-3	-4	8	9	Rainy

Figure A.9: Matching Payoff Table (Example)

If a participant does not match, the matching payoff is 0 yuan. For instance, in a 4-person group, if Lemon matches with Yellow, while Mango and Green remain unmatched, and the weather is Sunny, then Lemon earns 3 yuan, Yellow earns 1 yuan, and Mango and Green each earn 0 yuan.

[Matching Process and Final Payoffs]

In the matching game, the four participants may freely engage in three types of actions: sending invitations, accepting/rejecting invitations, and breaking a temporary match. Details are as follows.

[Sending Invitations]

Each participant may send a matching invitation to any participant of the opposite role (i.e., Fruits may invite any Color, and Colors may invite any Fruit). The invited participant immediately receives a notification showing the inviter's identity; the other participants do not observe this information.

Before receiving a response (acceptance or rejection), the inviter cannot send another invitation, but may receive invitations from others and choose whether to accept or reject them.

[Accepting or Rejecting Invitations]

Upon receiving an invitation, the invited participant has 15 seconds to respond:

- If the invited participant rejects the invitation, or does not respond within 15 seconds (which counts as a rejection), the invitation expires.
- If the invited participant accepts within 15 seconds, the two participants form a temporary match.
- During these 15 seconds, the invited participant may still send invitations to others.

[Temporary Matches, Final Matches, and Final Payoffs]

If an invitation is sent and accepted, a temporary match is formed. If the round has not ended, all matches are temporary. This means:

- Participants in a temporary match may still send new invitations and may accept or reject incoming invitations.
- If a participant forms a new temporary match, any previous temporary match is automatically dissolved.
- Either participant in a temporary match may unilaterally break the match at any time; the other party simply receives a notification.

Inaction treatment: The "Matching Payoff Table" is displayed to everyone, and the currently active temporary matches within the 4-person group are shown by a highlighted indicator at the corresponding cell—this information is public.

Revealed treatment: The "Matching Payoff Table" is displayed to everyone, and the currently active temporary matches within the 4-person group are shown by a highlighted indicator at the corresponding cell; meanwhile, the group's history activities are shown below the table. All of this information is publicly observable.

At the end of the round, all temporary matches become final matches:

- Participants in a final match receive: Final payoff = initial 10 yuan + matching payoff
- Participants without a match receive: Final payoff = initial 10 yuan

[When Does a Round End?]

At the beginning of each round, a 60-second public countdown starts (visible in the upper-left corner of the screen):

- If the public matching indicators remain unchanged for 60 consecutive seconds—meaning no temporary match is formed and no existing temporary match is broken—then the round ends.
- Whenever any change occurs (a match is formed or broken), the 60-second countdown restarts.

Additionally, once a round has lasted at least 60 seconds, a button labeled "Agree to Proceed to Next Round" becomes available. If all four participants in the group press this button, the round ends immediately without waiting for the countdown to expire. Note: Even after pressing the "Agree" button, you may continue all matching actions until the round ends, and you may also cancel your agreement.

[Number of Rounds and Total Earnings]

You will play 8 matching games in total. All procedures are identical across rounds; the only difference is the Matching Payoff Table, which updates each round.

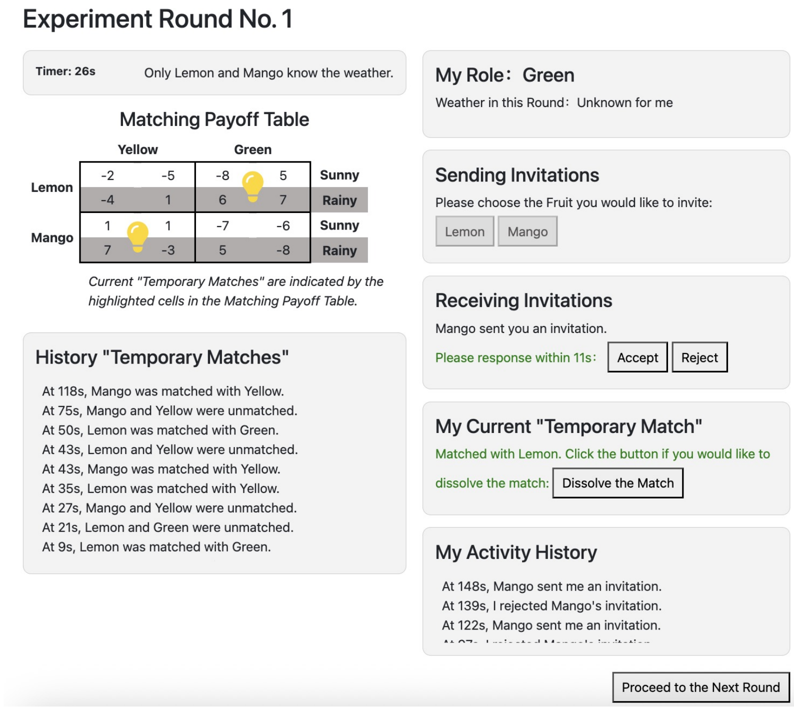
Before the 8 official rounds, there will be one practice round. The practice round is designed to help you become familiar with the procedure, and the earnings from this practice round do not count toward your final payment.

After each round ends, the system automatically proceeds to the next round. In the new round, you will not be able to view any payoff tables or outcomes from previous rounds.

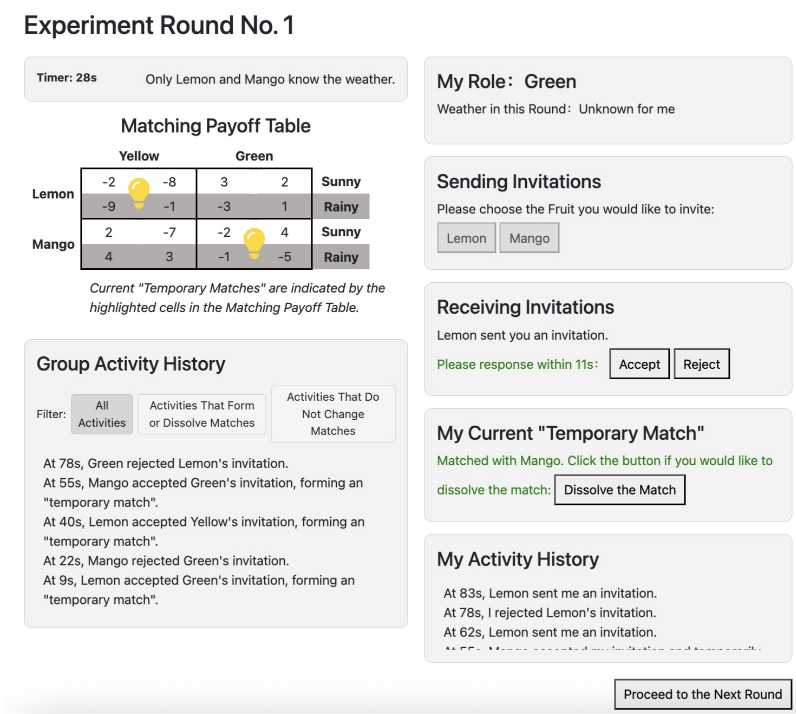
At the end of the experiment, the computer will randomly select 4 out of the 8 rounds, and your earnings from those 4 rounds will count toward your experiment payment.

Your total payment = participation fee (15 yuan) + the sum of the final payoffs from the 4 randomly selected rounds.

B.3.2 Screenshots



(a) Inaction Treatment



(b) Revealed Treatment

Figure A.10: Screenshots from Experiment 3