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Abstract
When faced with unfamiliar options, people often rely on the choices of others. We examine how this reliance affects search decisions through a laboratory experiment. Participants choose between two options whose value can only be discovered through costly sequential searches. Some search in isolation; others first observe the final choice, but not the search process, of a peer. This form of social information improves efficiency, yet behavior systematically departs from theory. When selecting which option to sample first, imitation of the peer's choice is frequent but not universal; moreover, participants often deviate from the optimal stopping rule, with both under and over-search observed. We introduce treatments that allow participants to choose whom to observe and access signals about the reliability of their peers: these interventions increase imitation and improve welfare. Our findings underscore the role of institutional design in facilitating social search, with implications for platform architecture and information diffusion.

JEL Classification: D8; C9; D1.

Keywords: Sequential Search; Social Information; Institutional Design.

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1 Introduction

Search is rarely conducted in isolation. The choices of others serve as a benchmark when individuals must compare options whose relative merits are initially unknown and information is costly to acquire. Since others may already have searched and evaluated viable options, their final choices may incorporate information gathered through previous evaluations. As a result, observing these choices shapes subsequent search and product discovery: first, it creates incentives for imitation, i.e., for the *exploitation* of social information; second, it lowers the expected value of continuing the search, reducing the appeal of independent *exploration*.

An extensive literature documents that when agents must learn about options they have little prior experience with, as typical in search markets, they rely heavily on the information in others' choices to shape their beliefs and decisions (see, e.g., Cai, Chen, and Fang, 2009; Moretti, 2011; Bailey, Johnston, Kuchler, Stroebel, and Wong, 2022; Mobius and Rosenblat, 2014). Moreover, recent analyses of online rating and ranking systems document that the actual impact of social information often depends on the quality of this information (see, e.g., Reimers and Waldfogel, 2021; Aguiar, Waldfogel, and Waldfogel, 2021). Hence, the reliability and credibility of information sources can play a crucial role in shaping how agents use social information when searching for new products. Yet, evidence on how information about others' choices can enhance search efficiency and product discovery is limited.

In this paper, we experimentally investigate how individuals use social information when choosing between two options of unknown quality, and searching for information about these options is costly. In our experiment, some individuals search in isolation; others have access to social information, that is, before engaging in their search, they observe the choice of a peer in the same context. Across treatments, we vary the information individuals have about the peer whose choice they observe and whether they can freely select which peer to follow. Our goal is threefold. First, we investigate how social information changes search behavior and product discovery. Second, we analyze how information on the reliability of observable peers influences how social information affects search behavior. Third, we vary how easily individuals can identify the most reliable peer to study how alternative mechanisms to evaluate the quality of information sources help reap the benefits of social information.

Our analysis provides insights into platform design, particularly concerning (i) the access to information on the reliability of the users whose experiences are shared and (ii) users' flexibility in determining their information source. Digital platforms routinely adopt scoring systems that promote users' past experiences and performance. For instance, Stack Overflow, GitHub, Duolingo, and expert forums such as specific subreddits—where users search for information about best practices—use mechanisms that make high-quality user behavior visible while allowing individuals to choose whose practices to emulate.[1] These examples suggest that purposeful design can balance guided social learning with personal

[1]Similar review and scoring systems are also used by Google Guides, Tripadvisor, and Yelp—where users access others' experiences with local businesses, places, services, and restaurants and search for information about them before making their choices—and to assess the reliability of online stores and vendors.

adaptability, crucially enhancing the effectiveness of social search. Our experiment aims to shed light on the impact of such mechanisms on agents' search and choice behavior.

To provide a theoretical framework guiding our experiment, we consider a simple variant of Weitzman (1979)'s classical sequential search model and solve it without and with social information. Without social information, an isolated agent must choose between two options whose qualities are i.i.d. draws. Before searching, the agent knows the quality distribution but does not observe the realized qualities of the two options. By sampling an option, however, the agent can perfectly learn its quality. After sampling the first option for free, the agent decides whether to discontinue the search or sample the second option at a cost. With social information, before engaging in sequential search (as described above), an agent observes the choice of a peer but knows neither the peer's search behavior nor the peer's search cost. The peer acted in isolation and faced the same quality realization as the agent; hence, the choice of the peer conveys some information about the realized qualities to the agent. We abstract from strategic considerations and payoff externalities.

Theory predicts that social information changes an agent's optimal search in the following ways. First, since the peer sampled both options with positive probability and then chose the better one, the option chosen by the peer is superior to the other option from the perspective of an agent with social information. Hence, while an isolated agent is indifferent about which option to sample first, an agent with social information finds it optimal to begin her search with the option chosen by the peer, which we refer to as *imitation*.

Second, the expected gain from the second search for an agent with social information is lower than for an isolated agent because the former discounts such gain by the probability that the peer has already sampled both options. Moreover, whereas the expected gain from the second search decreases in the quality of the first option sampled for isolated agents, it is non-monotone for agents with social information. The reason is that if the quality of the option chosen by the peer is low, the peer must have sampled both options; thus, the unchosen option's quality cannot be higher. As a result, the expected gain from the second search is nil, as when the quality of the option sampled at the first search is the highest possible.

Third, agents with social information are more likely to sample a higher-quality option at the first search and less likely to conduct the second costly search than isolated agents. Consequently, their expected ex-ante payoff exceeds that of isolated agents.

With our experiment, we first study whether agents search optimally without and with social information and how they trade off independent search (exploration) with the use of information in the choices of others (exploitation). Next, we examine how the perceived reliability of social information shapes this balance. Finally, we analyze whether reputational considerations or endogenous matching protocols can foster trust in information sources and improve the efficiency of social search.

The experiment has two parts. In Part A, which comprises 15 rounds, all participants search for information about the two options in isolation. This part allows participants to familiarize themselves with the decision environment (training). In Part B, which comprises

30 rounds, participants are randomly assigned the role of either First or Second Mover. First Movers search in isolation, while Second Movers have social information: before searching, they observe the choice of a First Mover. In Part B, each Second Mover is assigned to a fixed group of four First Movers. In each round, Second Movers observe the choice made by only one of these four First Movers; however, the identities of the First Movers remain the same across rounds, allowing Second Movers to evaluate their reliability from previous choices.

In the Benchmark treatment, we match Second Movers exogenously and randomly with First Movers in their group; Second Movers do not receive external signals about the performance of these four First Movers. According to our model, Second Movers should always begin by sampling the option chosen by their matched First Mover (imitation). In our data, imitation is frequent but not universal, occurring in around 70% of times, and the deviation from the theoretical prediction is statistically significant. The model also predicts that Second Movers, conditional on imitation, should sample the second option only if the first-sampled option has an intermediate quality and their search cost is low. Empirically, we observe that Second Movers sample the second option in 72% of these circumstances; this frequency drops to 20% when the model predicts that Second Movers should discontinue the search. Despite broad agreement, the departure from the theoretical predictions is again statistically significant, substantiating evidence of both under- and over-search. Finally, we quantify the welfare losses associated with observed behavior relative to the optimal behavior predicted by the model. We document that the primary source of inefficiency stems from the Second Movers' failure to imitate at the first search. Welfare losses due to Second Movers' suboptimal behavior at the second search are smaller in absolute and relative terms.

Motivated by these findings, we introduce experimental treatments to examine which mechanisms can mitigate the inefficiencies observed in social search. We manipulate the possibility for Second Movers to choose which First Mover to observe and the availability of information about First Movers' performance. More precisely, Second Movers are displayed a rating of the four First Movers based on their cumulative payoff in Part A of the experiment. These ratings enable Second Movers to make inferences about the reliability of individual First Movers. Our treatments draw inspiration from the experimental literature on platforms, which documents heterogeneous peer influence on behavior—even among individuals with minimal acquaintance (Uetake and Yang, 2020)—particularly when the peers' performance is disclosed (Lockwood, Jordan, and Kunda, 2002; Karlsson, Loewenstein, and Seppi, 2009). We complement previous work on how institutional design can optimize effort and conformity (see, e.g., Yildirim, Wei, Van den Bulte, and Lu, 2020) by focusing on how social information can enhance the efficiency of search and product discovery.

We implement a 2×2 between-subjects design and conduct the following treatments:

- NoSignal-Endo: First Movers' ratings in Part A are not communicated to Second Movers in Part B. Matching is endogenous: Second Movers can choose which First Mover from their group to observe.
- Signal-Exo: First Movers' ratings in Part A are communicated to Second Movers

in Part B. Matching is exogenous and random.

- Signal-Endo: First Movers' ratings in Part A are communicated to Second Movers in Part B. Matching is endogenous.

Our treatments (to which Second Movers are randomly assigned) manipulate features that may influence how agents assess the reliability of social information to identify which mechanisms can enhance the efficiency of social search. We compare Signal-Exo with Benchmark to examine how information about First Movers' past performance affects Second Movers' trade-off between independent search (exploration) and the exploitation of social information. We compare NoSignal-Endo with Benchmark to assess whether Second Movers are more likely to imitate First Movers whom they intentionally choose to observe. We compare Signal-Endo with Benchmark to investigate how these two mechanisms interact when agents can select who to follow based on peers' prior performance.

The estimated imitation frequency increases above 80% (on average) under endogenous matching. Although imitation rates remain below the theoretical prediction of full imitation, the positive difference with the Benchmark is significant at the 10% level. In contrast, when Second Movers receive a signal on the reliability of First Movers but cannot choose their match, the imitation frequency drops to approximately 50-60%, and the difference from the Benchmark is significant at the 1% level. The reason is that, in the Signal-Exo treatment, Second Movers may be matched with First Movers carrying a negative reliability signal, inducing Second Movers to trust First Movers even less than in the Benchmark. Unlike imitation propensity, treatment manipulations do not improve Second Movers' likelihood of following the theoretical predictions at the second search. Overall, by increasing imitation rates, endogenous matching significantly improves the welfare of Second Movers compared to First Movers, allowing them to benefit from social information.

Our findings suggest that combining access to information about peers' choices with endogenous network structure offers powerful opportunities for structuring social search. Platforms can support users entering unfamiliar domains by exposing them to the actions and outcomes of other peers and enabling them to exercise discretion in choosing whom to follow. For instance, Stack Overflow allows users to monitor the contributions of high-reputation participants and selectively adopt their practices. GitHub enables novice developers to follow experienced coders, observe detailed commit histories, and gradually emulate effective strategies. Duolingo leverages leaderboards and peer visibility to encourage social comparison while preserving user choice. Even in loosely moderated environments like expert subreddits (e.g., r/AskHistorians), users can observe which contributors consistently provide high-quality content and decide whose behavior to emulate. In these examples, purpose-oriented design helps new users navigate complex environments by learning from others while leaving space for adaptation and personal strategy. Our study shows that such an approach is essential to realize the informational benefits of social search.

Road Map. The following subsection discusses the related literature. In Section 2, we present the model and its theoretical predictions. In Section 3, we describe the experimental design. In Section 4, we present the testable hypothesis. In Section 5, we discuss the experimental findings on the plain effect of social information on search behavior, focusing on the Benchmark treatment. In Section 6, we consider treatment manipulations to understand which mechanisms can reduce the inefficiencies of social search. In Section 7, we conclude by discussing our findings. Additional details and robustness checks are in the appendices.

1.1 Related Literature

Given the nature of the decision problem we model, our work connects to the economic literature on multi-armed bandit problems (see, e.g., Bergemann and Valimaki, 2008; Hörner and Skrzypacz, 2017, for surveys). In these problems, agents repeatedly choose among different options (or bandit's arms) of initially unknown quality. At each point in time, they face a trade-off between selecting the best option given their current information set (exploitation) and incurring a cost to unveil information about one of the unexplored options to expand their information set, thus improving the quality of future decisions (exploration). Most experiments on bandit problems consider settings where agents face this trade-off in isolation, without access to information about others' choices. The results show that such isolated agents generally recognize the value of exploration and behave close to optimally, adjusting to changes in search conditions as predicted by theoretical models (Meyer and Shi, 1995; Caplin, Dean, and Martin, 2011; Lykopoulos, Voucharas, and Xefteris, 2022). Deviations from optimal search behavior—manifested as under- or over-exploration—can sometimes be traced to individual preferences, such as ambiguity aversion (Anderson, 2012), while recent evidence suggests that risk aversion explains little of the observed behavior, and that incorrect beliefs may play a more central role (Hudja and Woods, 2024).[2]

A few recent experiments study multi-armed bandit problems with multiple agents, focusing on strategic considerations. Building on the theoretical work of Bolton and Harris (1999) and Keller, Rady, and Cripps (2005), these papers consider settings where several homogeneous agents face simultaneously the same bandit problem. The best option is the same for everyone, but it is initially unknown. Hence, agents learn from others' publicly observable experimentation decisions and/or outcomes. In these environments, the bandit problem becomes a dynamic public-good dilemma, where the dynamically evolving information about the common state of the world is the public good. Similarly to the experimental studies investigating this complex information transmission dilemma (Boyce, Bruner, and McKee, 2016; Hoelzemann and Klein, 2021), we also focus on multi-armed bandits with multiple agents and a common state of the world. In contrast to these papers, however, in our setup, there are no payoff externalities, and agents are short-lived and move sequentially. Lifting strategic considerations allows us to focus on how informational externalities

[2]Laboratory experiments inspired by bandit-style problems have also been used to study how isolated agents respond to complexity (Banovetz and Oprea, 2023), as well as to assess the predictive performance of boundedly rational models of information acquisition (Gabaix, Laibson, Moloche, and Weinberg, 2006).

arising from observing others' choices affect individuals' exploitation-exploration trade-off and test whether social information crowds out incentives for independent exploration.

As we focus on how the information about the choices of others affects individual decision-making, our work is also related to a large experimental literature on social learning.[3] Since the seminal work by Anderson and Holt (1997), social learning experiments have mimicked situations in which many agents act sequentially to infer an underlying common state of the world. Each agent receives a private informative signal and observes, at no cost, the choices of all predecessors. Experimental findings show that agents often fail to aggregate the available information optimally. In contrast to what Bayesian updating would prescribe, they fall prey to behavioral biases, such as overconfidence, which leads them to overweight the informativeness of their private signals or their ability to interpret them (see, e.g., Nöth and Weber, 2003; Angrisani, Guarino, Jehiel, and Kitagawa, 2021; De Filippis, Guarino, Jehiel, and Kitagawa, 2022) or redundancy neglect, by which they fail to account for the informational content already embedded in others' actions (see, e.g., Eyster and Rabin, 2010; Eyster, Rabin, and Weizsacker, 2018; Enke and Zimmermann, 2019).[4]

Similarly to these experiments, we examine how social information affects individual decision-making absent payoff externalities to assess the presence and extent of behavioral inefficiencies. However, we depart from this literature in several respects. First, our agents do not face a decision environment where private and social information is free and exogenous, and where the core decision concerns how best to aggregate them. Instead, agents have limited social information—specifically, they observe only one predecessor—and must trade off the exploitation of this free social information against individual exploration via costly sequential search. Second, we explicitly focus on a context where the sequential interaction and information spillovers involve only two agents, rather than many. This shortens the chain of social information transmission and makes the redundancy problem irrelevant. Finally, while private signals in social learning experiments are typically noisy, in our setting, sequential search allows agents to perfectly learn the quality of the options they face, leaving no uncertainty about the information they acquire.

2 Theoretical Framework

Our model, which is a stylized version of those in Mueller-Frank and Pai (2016), Lomys (2025), and Lomys and Tarantino (2025), helps to understand how social information affects the sequential search of rational agents and guides our experimental investigation.

Agents and Options. Two agents, indexed by $n = 1, 2$, select sequentially a single option from the set $X := \{0, 1\}$. We denote by x an option in X and by $\neg x$ the option

[3] For surveys of the theoretical and experimental literature on social learning, see Bikhchandani, Hirshleifer, Tamuz, and Welch (2024) and Anderson and Holt (2008).

[4] See also Çelen and Kariv (2004) and Goeree, Palfrey, Rogers, and McKelvey (2007) for early evidence of deviations from Bayesian updating in social learning experiments. Although not explicitly framed in terms of overconfidence or redundancy neglect, their findings—under-weighting of social information and over-weighting of private signals, respectively—point to related behavioral patterns.

in X other than x. Agent n acts at time n, and the order of moves is exogenous.

Options' Qualities. Let q_x denote the quality of option x. Qualities q_0 and q_1 are independent draws from the uniform distribution on $Q := \{l, m, h\}$, where $0 < l < m < h$. The qualities of the two options are drawn once and for all at time 0. Agent n knows the quality distribution but does not observe the pair of realized qualities (q_0, q_1).

Sequential Search. Agent n collects information about the realized quality of the two options via a costly sequential search with recall:

1. Agent n decides which option to sample first, $s_n^1 \in X$. By sampling option s_n^1, Agent n perfectly learns its realized quality $q_{s_n^1}$.
2. Agent n decides whether to discontinue the search, $s_n^2 = d$, or to sample the remaining option, $s_n^2 = \neg s_n^1$, and perfectly learn its realized quality.
3. Agent n chooses an option $a_n \in S_n$, where S_n is the set of options she has sampled.[5]

The first search is free. The second search costs c_n. Search costs are independent across agents and drawn from the uniform distribution on $C := \{\underline{c}, \overline{c}\}$, where $0 < \underline{c} < \overline{c}$.

Social Information. Agent 1 acts in *isolation*: her search problem is as described above. Agent 2 has *social information*: before searching as described above, Agent 2 observes the option a_1 chosen by Agent 1; however, Agent 2 does not observe the search cost and sampling decisions of Agent 1 nor the quality of option a_1.

Payoffs. Agent n maximizes the difference between the quality of the option she chooses and the search cost she incurs: $q_{a_n} - c_n(|S_n| - 1)$.

2.1 Optimal Decisions of Agent 1

First Search. Since Agent 1 is isolated and qualities are i.i.d. across options, Agent 1 decides uniformly at random which option to sample first. Breaking indifferences uniformly at random captures that labels do not convey information about qualities or behavior.

Since qualities are i.i.d., we have

$$q_{s_1^1} = \begin{cases} l & \text{with probability } \frac{1}{3} \\ m & \text{with probability } \frac{1}{3} \\ h & \text{with probability } \frac{1}{3} \end{cases}. \tag{1}$$

Second Search. Agent 1 samples the second option, $s_1^2 = \neg s_1^1$, if and only if the expected gain from doing so is greater than her search cost, c_1. Given the quality of the option

[5] As standard in the sequential search literature, agents can only choose an option they sampled.

she sampled at the first search, $q_{s_1^1}$, Agent 1's expected gain from the second search is

$$V_1\left(q_{s_1^1}\right) \coloneqq \mathbb{E}\left[\max\left\{q - q_{s_1^1}, 0\right\}\right] = \begin{cases} \frac{m+h-2l}{3} & \text{if } q_{s_1^1} = l \\ \frac{h-m}{3} & \text{if } q_{s_1^1} = m \\ 0 & \text{if } q_{s_1^1} = h \end{cases}. \tag{2}$$

We assume $\underline{c} < \frac{h-m}{3} < \overline{c} < \frac{m+h-2l}{3}$. Hence, Agent 1's decision at the second search depends on the values of $q_{s_1^1}$ and c_1 and is as described by Table 1.

Table 1: Agent 1's Decision at the Second Search.

	$V_1\left(q_{s_1^1}\right)$	Agent 1's Decision	
		$c_1 = \underline{c}$	$c_1 = \overline{c}$
$q_{s_1^1} = l$	$\frac{m+h-2l}{3}$	Sample option $\neg s_1^1$	Sample option $\neg s_1^1$
$q_{s_1^1} = m$	$\frac{h-m}{3}$	Sample option $\neg s_1^1$	Discontinue search
$q_{s_1^1} = h$	0	Discontinue search	Discontinue search

<u>*Choice.*</u> Agent 1 chooses the best option she sampled, randomizing uniformly if indifferent.

<u>*Decision Tree.*</u> In Appendix A.1, we summarize Agent 1's decisions by representing the corresponding decision tree (see Figure A.1).

Theoretical Predictions for Agent 1. The analysis of Agent 1's optimal decisions leads to the following theoretical prediction about when an isolated agent discontinues the search.

Prediction 1. *At the second search, Agent* 1 *behaves as described in Table 1.*

Agent 1's first search and choice decisions are trivial and are not the focus of our experiment.

2.2 Optimal Decisions of Agent 2

<u>*First Search.*</u> Agent 2's belief about the qualities of the two options depends on Agent 1's choice, which is endogenously generated by Agent 1's optimal decisions. Two cases each occur with positive probability. First, if Agent 1 did not sample option $\neg a_1$, Agent 1's choice is uninformative about the quality of option $\neg a_1$. Second, if Agent 1 sampled option $\neg a_1$, Agent 1's choice reveals that option a_1 is superior to option $\neg a_1$, strictly so with positive probability. As we show formally in Appendix A.2.1 (see Result 1), these observations imply that Agent 2 finds it optimal to sample option a_1 first.

Using the decision tree in Figure A.1 to compute the probabilities, we have

$$q_{s_2^1} = q_{a_1} = \begin{cases} l & \text{with probability } \frac{1}{9} \\ m & \text{with probability } \frac{7}{18} \\ h & \text{with probability } \frac{1}{2} \end{cases} \tag{3}$$

In summary, social information affects the first search as follows:

- Whereas Agent 1 is indifferent about which option to sample first, Agent 2 is not and finds it strictly optimal to begin her search by the option chosen by Agent 1 at the end of her search process—a fact we will refer to as *imitation* in the following.
- Thanks to social information, Agent 2 samples a higher-quality option at the first search with greater probability than Agent 1 (compare equations (1) and (3)).

Second Search. Agent 2 samples the second option, $s_2^2 = \neg s_2^1$, if and only if the expected gain from doing so is greater than her search cost, c_2. The expected gain from the second search depends on the probability that Agent 1 did not sample option $\neg s_2^1$ given that she chose an option of quality $q_{s_2^1}$, denoted by $P_2(q_{s_2^1})$. With remaining probability, Agent 1 sampled option $\neg s_2^1$ but chose option s_2^1; if so, option s_2^1 is non-inferior by revealed preference. Thus, Agent 2's expected gain from the second search is

$$V_2\Big(q_{s_2^1}\Big) := P_2\Big(q_{s_2^1}\Big)\mathbb{E}\Big[\max\{q - q_{s_2^1}, 0\}\Big] = P_2\Big(q_{s_2^1}\Big)V_1\Big(q_{s_2^1}\Big). \tag{4}$$

Using Bayes rule and the decision tree in Figure A.1 to compute the probabilities, we have

$$P_2\Big(q_{s_2^1}\Big) := \mathbb{P}\Big(s_1^2 = d \mid q_{a_1} = q_{s_2^1}\Big) = \begin{cases} 0 & \text{if } q_{s_2^1} = l \\ \frac{3}{7} & \text{if } q_{s_2^1} = m \\ \frac{2}{3} & \text{if } q_{s_2^1} = h \end{cases}. \tag{5}$$

Equations (2), (4), and (5) imply that Agent 2's expected gain from the second search is

$$V_2\Big(q_{s_2^1}\Big) := \mathbb{P}\Big(s_1^2 = d \mid q_{a_1} = q_{s_2^1}\Big) = \begin{cases} 0 & \text{if } q_{s_2^1} = l \\ \frac{h-m}{7} & \text{if } q_{s_2^1} = m \\ 0 & \text{if } q_{s_2^1} = h \end{cases}. \tag{6}$$

We assume $\underline{c} < \frac{h-m}{7}$. Hence, Agent 2's decision at the second search depends on the values of $q_{s_2^1}$ and c_2 and is as described by Table 2.

Table 2: Agent 2's Decision at the Second Search.

	$V_2\Big(q_{s_2^1}\Big)$	Agent 2's Decision	
		$c_2 = \underline{c}$	$c_2 = \overline{c}$
$q_{s_2^1} = l$	0	Discontinue search	Discontinue search
$q_{s_2^1} = m$	$\frac{h-m}{7}$	Sample option $\neg s_2^1$	Discontinue search
$q_{s_2^1} = h$	0	Discontinue search	Discontinue search

By equations (2) and (6), social information affects the second search as follows:

- For all $q \in Q$, we have $V_2(q) = P_2(q)V_1(q) \leq V_1(q)$ (because $P_2(q) \in [0, 1]$): given the quality of the option sampled at the first search, the expected gain from the second search (i.e., the value of searching) for Agent 2 is lower than for Agent 1. The value of searching measures an agent's incentive to explore. Hence, *ceteris paribus*, Agent

2's incentive to explore is lower than that of Agent 1. The reason is that Agent 2 can exploit her social information to make inferences about the qualities of the two options from Agent 1's choice; in contrast, Agent 1, who acts in isolation, cannot.

- Whereas the value of searching is decreasing in the quality of the first option sampled for Agent 1 ($V_1(l) > V_1(m) > V_1(h) = 0$), social information makes it non-monotone for Agent 2 ($V_2(l) = V_2(h) = 0 < \frac{h-m}{7} = V_2(m)$). This fact has two consequences.

  First, whereas Agent 1's incentives to search are maximal after sampling an option of quality l at the first search, they are minimal in the same circumstance for Agent 2. The reason is that Agent 2 infers from Agent 1's choice of an option of quality l that the latter must have sampled both options and that no better option is available. Second, since $V_2(m) > V_2(l)$, Agent 2's value of searching decreases as the quality of the option sampled at the first search decreases from m to l. As a result, Agent 2 samples the second option after sampling an option of quality m if $c_2 = \underline{c}$, whereas she always discontinues the search after sampling an option of quality l.

Choice. Agent 2 chooses the best option she sampled, randomizing uniformly if indifferent.

Thanks to social information, Agent 2: (i) samples a higher-quality option at the first search with a higher probability than Agent 1; (ii) conducts the second costly search with a lower probability than Agent 1. As a result, the expected ex-ante payoff of Agent 2 is greater than that of Agent 1 (i.e., the value of social information is positive).

Decision Tree. In Appendix A.1, we summarize Agent 1's decisions by representing the corresponding decision tree (see Figure A.2).

Theoretical Predictions for Agent 2. The analysis of Agent 2's optimal decisions leads to the following theoretical predictions about the sequential search of an agent with social information. We begin with the order of search.

Prediction 2. *Agent 2 samples option a_1 first.*

The next prediction is about when to discontinue the search.

Prediction 3. *At the second search, Agent 2 behaves as described in Table 2.*

The last prediction is about the value of social information.

Prediction 4. *The expected ex-ante payoff of Agent 2 is greater than that of Agent 1.*

Agent 2's choice decision is trivial and is not the focus of our experiment.

3 Experimental Design

The experimental design follows a two-prong approach with non-overlapping sets of participants. We name the two experimental protocols "Experiment I" (from Isolated) and "Experiment S" (from Social Information). Both experiments consist of two parts: Part

A and Part B. We refer to "Participants I" and "Participants S", depending on whether participants are assigned to Experiment I or Experiment S (between-subjects design). At the end of Part B, all participants complete a short questionnaire containing a few money-incentivized questions. Table 3 summarizes the experimental design.

Table 3: Summary of the Experimental Design.

	Experiment I	**Experiment S**
Part A	Participants I act in isolation (15 rounds)	Participants S act in isolation (15 rounds)
Part B	Participants I act in isolation (30 rounds)	Participants S have social information: options chosen by Participants I (30 rounds)

Part A. Part A lasts 15 rounds. The design is the same in Experiments I and S. Participants act in isolation as *First Movers*, corresponding to Agent 1 in the model. In each round, the following stages occur in sequence for each participant:

1. The participant observes their search cost.
2. The participant learns the realized quality of one of the two options selected uniformly at random. We *automate* the first search for simplicity. Since isolated agents are indifferent at the first search, sampling the first option uniformly at random is optimal.
3. The participant decides whether to discontinue the search or to pay the search cost to sample the second option and learn its realized quality.
4. If the participant discontinued the search, the first sampled option is implemented; if the participant sampled both options, the highest-quality option is implemented, randomizing uniformly if the options have the same quality. We *automate* the option choice to reduce the scope for errors and the cognitive load on participants, assuming that participants would (rationally) choose the highest-quality option they sampled.

Participants I and S are assigned to subgroups (for details on subgroup formation, see "Procedures" below). To control for learning, all participants within each subgroup face the same 15 realizations of pairs of options' qualities.

Part B. Part B lasts 30 rounds. The design varies between Experiments I and S. As in Part A, Participants I and S are assigned to subgroups. All participants within each subgroup face the same 30 realizations of pairs of options' qualities.

Part B: Experiment I. Participants I act in isolation as First Movers. Hence, Participants I in Part B face the same decision environment as in Part A for an additional 30 rounds.

Part B: Experiment S. Participants S have social information and act as *Second Movers*, corresponding to Agent 2 in the model. In each round, each Participant S is matched with a Participant I who has already participated in the experiment; the matching procedure varies across treatments and is described in Sections 3.2 and 6. In each round, the following stages occur in sequence for each Participant S:

1. Participant S observes their search cost and the option chosen by the matched Participant I in the same round. Participant S receives no information on the search cost and sampling decisions of the matched Participant I.
2. Participant S decides which option to sample first at no cost (in contrast to Part A, where we automated the first search).
3. Participant S decides whether to discontinue the search or to pay the search cost to sample the second option and learn its realized quality (as in Part A).
4. If Participant S discontinued the search, the first sampled option is implemented; if Participant S sampled both options, the highest-quality option is implemented, randomizing uniformly if the options have the same quality. As in Part A, we *automate* the option choice.

Participant S knows that the matched Participant I faced the same decision environment as Part A also in Part B: over the 30 rounds, the matched Participant I always only decided whether to discontinue the search or sample the second option after learning the quality of the first uniformly-at-random sampled option (implemented in an automated way).

Parametrization. Qualities q_0 and q_1 are i.i.d. draws from a uniform distribution on $Q \coloneqq \{l, m, h\} = \{5, 9, 19\}$. Search costs c_n are i.i.d. across participants and are drawn from a uniform distribution on $C \coloneqq \{\underline{c}, \overline{c}\} = \{1, 4\}$. This parametrization prevents participants from facing losses.[6] Moreover, it ensures that the predictions on First Movers' decisions in Table 1 hold for both risk-neutral and moderately risk-averse participants.[7]

Decision Environment. We design a neutral decision environment to avoid framing. In each round, participants see two closed colored boxes (Orange or Purple) on the screen, each containing a random number of coins. Each colored box corresponds to an option; the number of coins inside a box corresponds to the quality of that option. Participants can learn the quality (l vs. m vs. h) associated with a box only by opening the box.

Participants I play both Parts A and B in isolation. In each of the 45 rounds, they first learn about the quality of the first box, selected uniformly at random by the computer, and then decide whether to open the second box given their search cost.

Participants S play Part A like Participants I, whereas they have social information in Part B. The social information provided to Participants S consists of which colored box was kept as the payoff-relevant box by their matched Participant I in the same round.

[6] This also enables us to discard considerations related to participants' degree of loss aversion and to be able to always rely on the same CRRA utility function (which is not defined over losses).

[7] As in Holt and Laury (2002), we rely on a CRRA utility function $U(R) \coloneqq \frac{R^{1-\rho}}{(1-\rho)}$ and classify the degree of risk-aversion based on the value of ρ: $\rho \in [-0.15, 0.15]$ identifies risk-neutrality; $\rho \in [0.15, 0.68]$ identifies slight-to-moderate risk-aversion.

3.1 Procedures

The experiment involves $N = 100$ participants: we assign $N_I = 20$ participants to Experiment I and $N_S = 80$ participants to Experiment S (20 to the BENCHMARK treatment, described in Section 3.2, and 20 to each of the other three treatments, described in Section 6). Experiment I precedes Experiment S. Experiments I and S differ in the design of Part B for the availability of social information, which is accessible only to Participants S.

In Part B, we inform all Participants S about which colored box their matched Participant I kept as payoff-relevant in the same round. To control for learning, which may be affected by the realized sequence of boxes' qualities, we ensure Participants I and S face the same decision environment by holding such sequence fixed over rounds. At the same time, to avoid order effects or information spillovers across experimental sessions, we do not want all our participants to face the same sequence of boxes' qualities. Therefore, we split Participants I and S into 5 subgroups (I to V). For each subgroup, we draw a different sequence of boxes' qualities. We administer the same sequence to all participants in the same subgroup in the same order. Table 4 summarizes how we split participants into subgroups. Each subgroup counts $N_I/5 = 4$ Participants I and $N_S/5 = 16$ Participants S (4 per treatment). The matching of Participants S with Participants I in Part B occurs within subgroups; therefore, conditional on subgroup assignment, Participants S face the same sequence of boxes' qualities as their matched Participant I, in the same order.

Table 4: Experiments I and S: Grouping Structure and Matching Scheme.

Participants I

SG_{I}	SG_{II}	SG_{III}	SG_{IV}	SG_{V}	N_I
4	4	4	4	4	**20**

Participants S

SG_{I}	SG_{II}	SG_{III}	SG_{IV}	SG_{V}	N_S
16	16	16	16	16	**80**

When we provide Participants S with information about Participants I, we denote each of the 4 Participants I in a subgroup by one of the following 4 anonymous symbols: ♠, ♥, ♣, ♦. Labels are univocally assigned to Participants A and remain the same over rounds. Thus, Participants S can build and update their reputational assessments on the ability of Participants I over time based on experience from past rounds and matches.[8]

Payments. Participants receive a show-up fee of 5 Euros. The payment for Part A equals the payoff realized in one of the 15 rounds played, selected uniformly at random. The payment for Part B equals the sum of the payoffs realized in two of the 30 rounds played, each selected uniformly at random.

Participants can earn up to 19 Euros in the experiment. In expectation, Participants I and Participants S earn 12.56 and 13.64 Euros, respectively, if they behave as the model predicts. In addition, participants can earn a little extra money from their answers to the money-incentivized survey questions, up to a maximum of 1.33 Euros for Participants

[8]Participants are displayed a small table summarizing their history of play over previous rounds, which also includes the information on the identity and the payoff-relevant choice of their matched Participant I.

I and 3.33 Euros for Participants S. Overall, realized earnings in the experiment ranged between 8.75 and 20.5 Euros (including the show-up fee).[9]

Qualities and costs are presented in tokens. At the end of the experiment, payoffs in tokens are converted into earnings in Euros at the rate of 3 tokens per Euro.

Questionnaire. Participants answer the questionnaire before learning about their earnings. After collecting basic socio-demographic information (age, gender, and major), we elicit risk preferences using the streamlined version of the Preference Survey Module by Falk, Becker, Dohmen, Huffman, and Sunde (2023); this first part of the questionnaire is not incentivized. The second part of the questionnaire contains money-incentivized questions to measure numeracy and risk literacy; questions are taken from the Berlin test by Cokely, Schulz, Ghazal, and Garcia-Retamero (2012), and incentives follow a fixed-prize scheme (1 token per correct answer).

After the Berlin test, Participants S (but not Participants I) answer an additional short set of incentivized questions designed to elicit beliefs on their absolute and relative performance in the test, which we take as a measure of their (over)confidence. Incentives follow a fixed-prize scheme (2 tokens per correct answer).[10]

Upon completing the questionnaire, participants learn which rounds of Parts A and B have been randomly selected for payment and their respective earnings. They also receive feedback on their performance on the Berlin test and learn whether they have the right to extra earnings from the additional money-incentivized questions.

Implementation. We conducted the experimental sessions between December 2023 and January 2024 at the CESARE Laboratory for Experimental Economics at LUISS University. We recruited participants through ORSEE (Greiner, 2015) from the LUISS University students' participants pool.[11]

3.2 Benchmark Treatment

In Sections 4 and 5, we analyze the plain effect of social information on search behavior, focusing on the behavior of participants in the Benchmark treatment. In this treatment:

- Second Movers have no control over the matching procedure. At the beginning of each round, we match each Second Mover with one of the First Movers within the same subgroup through a standard random matching protocol. The probability of re-matching with the same Participant I in the next round is equal to 1/4.

[9]The average completion time was 31 minutes, with a minimum of 17 and a maximum of 51 minutes.

[10]To measure participants' (over)confidence, we ask them to rate: (i) their absolute performance in the Berlin test (number of correctly answered questions); (ii) their belief about the average performance of other nine randomly selected participants in the same test; (iii) their belief about their ranking with respect to the same other nine randomly selected participants. In Appendix C, we test whether these individual characteristics mediate the effect of social information. No significant effect emerges from the analysis.

[11]The study was approved by the Bocconi University Ethics Committee on July 27, 2023 (SA000643) and pre-registered on the American Economic Association's registry for randomized controlled trials on December 19, 2023 (#AEARCTR-0012629). The pre-registration is available at `https://doi.org/10.1257/rct.12629-1.0`. The experimental instructions are available in Appendix D.

- Second Movers receive no information on First Movers' performance: before searching, Second Movers only learn the identity of the matched First Mover through the associated anonymous symbol and which colored box the latter kept as payoff-relevant.

The differences between this and the other treatments are described in Section 6.

4 Testable Hypotheses

Predictions 1–4 in Section 2.1 give rise to the following testable hypotheses about the optimal search behavior of First and Second Movers.

Search without Social Information. We begin with isolated agents. According to Prediction 1, we expect First Movers to use the following policy at the second search.

Testable Hypothesis 1. *At the second search, First Movers sample the second option if and only if: (i) the quality of the first option sampled is low; or (ii) the quality of the first option sampled is medium, and the search cost is low.*

When the quality of the first option sampled is low, First Movers sample the remaining option regardless of their search cost; when the quality is medium, First Movers sample the remaining option if their search cost is low. Otherwise, First Movers discontinue the search because the expected gain from the second search is smaller than its cost. Testable Hypothesis 1 is robust in the sense that, under the parametrization of our experiment, First Movers are far from being indifferent between sampling the remaining option or discontinuing the search for any realization of the quality of the first option sampled and their search cost, even assuming a moderate degree of risk aversion (see footnote 7 in Section 3).

Search with Social Information. Turning to agents with social information, based on Prediction 2, we expect Second Movers to behave as follows at the first search.

Testable Hypothesis 2. *At the first search, Second Movers sample the option chosen by the matched First Mover.*

A Second Mover who trusts the rationality of her matched First Mover knows that, with positive probability, the latter has already sampled both options and chosen the best among them. Therefore, the option chosen by the matched First Mover is superior to the other option in the eyes of the Second Mover. As a result, the Second Mover finds it optimal to begin sampling from the option chosen by the First Mover (imitation).

As we show in Appendix A.2.1 (see Result 2), Testable Hypothesis 2 is remarkably robust: imitation is optimal for Second Movers regardless of the policy (optimal or not) adopted by the matched First Mover at the second search.

By Prediction 3, Second Movers should use the following policy at the second search.

Testable Hypothesis 3. *At the second search, Second Movers sample the second option if and only if the quality of the first option sampled is medium and the search cost is low.*

Social information reduces the value of searching for Second Movers and makes it non-monotone in the quality of the option sampled at the first search. Hence, Second Movers sample the second option less often than First Movers. In particular, if the quality of the first option sampled is low, whereas First Movers sample the second option regardless of their search cost, Second Movers never do so because the matched First Mover's choice already reveals that no better option is available (compare Testable Hypotheses 1 and 3).

Testable Hypothesis 3 is challenging for two reasons. The first reason is that, to be verified, the hypothesis requires that Second Movers believe that First Movers' behavior displays some degree of rationality. In particular, we show in Appendix A.2.2 that the hypothesis about what Second Movers should do after sampling an option of medium quality at the first search (conditional on imitation) is very robust: it holds even if First Movers' behavior markedly departs from the optimal search policy. However, the robustness of the hypothesis about what Second Movers should do after sampling an option of low quality at the first search (conditional on imitation) depends on Second Movers' search cost. When their search cost is high, Second Movers find it optimal to discontinue their search (as under Testable Hypothesis 3) even if First Movers' behavior markedly departs from the optimal search policy. In contrast, when their search cost is low, Second Movers find it optimal to discontinue their search (as under Testable Hypothesis 3) only if the matched First Mover samples the second option with sufficiently high probability after sampling an option of low quality (although not necessarily with probability one, as the optimal policy prescribes).

The second reason why Testable Hypothesis 3 is challenging is that the non-monotonicity of the Second Movers' value of searching in the quality of the first option sampled is a non-intuitive theoretical prediction. Indeed, this prediction contrasts with what happens for First Movers, for whom the value of searching increases as the quality of the first option sampled decreases. More broadly, in standard single-agent information acquisition problems, the value of acquiring more information decreases in the payoff that the agent has already secured. This property is in line with the prediction for First Movers but contrasts with the prediction for Second Movers in our experiment.

According to Prediction 4, we expect social information to affect payoffs as follows.

Testable Hypothesis 4. *Second Movers' expected payoff is higher than First Movers'.*

Social information benefits Second Movers. Table 5 reports the unconditional expected payoffs of First and Second Movers under optimal search (first row) and their expected payoffs conditional on the realized quality of the worst option (second to fourth row).[12] For each scenario, the second column reports the First Movers' expected payoff, the third column reports the Second Movers' expected payoff, and the last column reports their percentage difference. Under our parametrization, the overall payoff gain of Second Movers over First Movers is about 10%. Conditioning on the realized quality of the worst option,

[12] We compute expected payoffs assuming First and Second Movers search rationally and Second Movers expect that their matched First Movers searched rationally (hence, search rationally given this expectation).

payoff gains from social information are particularly sizeable (over 13%) when the quality of the worst option is low. Accordingly, in our experiment, we expect to observe the most significant differences in realized payoffs between First and Second Movers when the quality of the worst option is low (the most likely scenario, happening with probability 5/9).[13]

Table 5: Expected Payoffs from Optimal Search.

Quality of the Worst Option	**First Mover's Payoff**	**Second Mover's Payoff**	**% Difference 2nd Mover − 1st Mover**
Overall	12.56	13.64	9.85 %
l	10.6	12	13.2%
m	13.7	14.6	6.6%
h	19	19	0%

4.1 Decomposition of the Efficiency Loss

Our testable hypotheses rely on strong rationality assumptions. Deviations from rationality and Bayesian updating are widespread and well-documented in the experimental literature (see, e.g., El-Gamal and Grether, 1995; Alós-Ferrer and Garagnani, 2023; Enke and Graeber, 2023). In the setting of our experiment, three suboptimal behaviors may emerge:

- First Movers may over- or under-search. In addition to reducing the payoff of First Movers, this may also negatively affect the payoff of Second Movers. In particular, while over-search by First Movers can only benefit Second Movers, under-search diminishes the gains from the availability of social information.
- Second Movers may not begin sampling from the option chosen by the matched First Mover (lack of imitation), thus suboptimally exploiting their social information.
- Second Movers may over- or under-search.

To quantify the potential impact of these three deviations from rational behavior, we decompose the total loss of efficiency for Second Movers into three components. We measure the total loss of efficiency relative to the theoretical benchmark as

$$L_{\text{tot}} := \frac{P_0 - P_3}{P_0},$$

where: P_0 is the theoretical payoff (as predicted by the model) the Second Mover can achieve in a round through optimal first and second search, given the realizations of the search cost and the quality of the two options, conditional on observing the choice of a rational First Mover; P_3 is the Second Mover's realized payoff. We decompose L_{tot} as follows:

[13] Admittedly, predicted gains from social search are not particularly large. This is due to our choice of parametrization, which aimed at minimizing the cognitive load of the experimental participants and avoiding losses while maintaining non-trivial differences in search behavior between First and Second Movers. In a more natural setting, where both searches are costly, the payoff gains from social information become much more substantial while the predicted search behavior remains identical.

- The loss of efficiency due to the *First Mover's under-search* is

$$L_1 \coloneqq \frac{P_0 - P_1}{P_0},$$

  where P_1 is the payoff the Second Mover would obtain through optimal first and second search, given the option actually chosen by the matched First Mover.

- The loss of efficiency due to the *Second Mover's lack of imitation* is

$$L_2 \coloneqq \frac{P_0 - P_2}{P_0} - L_1 = \frac{P_1 - P_2}{P_0},$$

  where P_2 is the payoff the Second Mover would obtain through optimal second search, given the option actually chosen by the matched First Mover and the option the Second Mover sampled first. More specifically, we assume the following: (i) conditional on imitation at the first search, then the Second Mover discontinues the search unless the cost is low and the first option sampled has medium quality (see Table 2); (ii) conditional on no imitation at the first search, then the Second Mover neglects social information and behaves as an isolated agent (see Table 1).

- The residual loss of efficiency, due to the *Second Mover's over- or under-search*, is

$$L_3 \coloneqq L_{\text{tot}} - L_1 - L_2 = \frac{P_2 - P_3}{P_0}.$$

5 Empirical Analysis and Results

In this section, we discuss the experimental findings about Testable Hypotheses 1–4.

5.1 Search without Social Information

To begin with, we analyze the search behavior of isolated agents in our experiment. We use all data from Part B of Experiment I.[14] In this part of the experiment, Participants I act as First Movers, and their choices constitute the social information provided to Participants S in Part B of Experiment S (Second Movers) before they begin their search.

Testable Hypothesis 1. According to Testable Hypothesis 1, First Movers should sample the second option in two scenarios: (i) if the quality of the option they sampled at the first search is low ($q_{s_1^1} = 5$), or (ii) if the quality of the option they sampled at the first search is medium and their search cost is low ($q_{s_1^1} = 9$ and $c_1 = 1$). Table 6 reports the observed and estimated frequency with which First Movers sample the second option, conditional on the quality of the option they sampled first, distinguishing whether their search cost is low or high. We obtain estimated search frequencies with a Linear Probability Model in which we regress search occurrences on a series of binary indicators, each corresponding to a possible search scenario (see Appendix B, Table B.1 for the full

[14]Our sample consists of 600 observations from 20 Participants I.

regression results). A search scenario corresponds to a combination of the First Movers' search cost and the quality of the first option sampled.

Table 6: First Movers' Second Search Frequency.

Scenario	Prediction	Observed	Estimated	S.E.	P-value
Low search cost: $c_1 = 1$					
$q_{s_1^1} = 5$	1	0.958	0.952	0.042	0.314
$q_{s_1^1} = 9$	1	0.750	**0.766**	0.036	0.003
$q_{s_1^1} = 19$	0	0.068	0.057	0.032	0.153
High search cost: $c_1 = 4$					
$q_{s_1^1} = 5$	1	0.710	**0.713**	0.046	0.003
$q_{s_1^1} = 9$	0	0.223	**0.234**	0.045	0.006
$q_{s_1^1} = 19$	0	0.022	0.010	0.018	0.591

Notes. We consider second search decisions by First Movers in Part B of Experiment I. Estimates, standard errors, and p-values are from the regression in Appendix B, Table B.1. P-values refer to equality tests between theoretical and estimated frequencies (statistically significant differences in bold).

Empirical results broadly conform to the model predictions, especially when the search cost is low (top panel). In this case, First Movers' search behavior perfectly follows the theoretical predictions when the quality of the first option sampled is low or high. When the quality is medium, most First Movers behave as predicted (roughly 75%); yet, the difference from the theoretical prediction is statistically significant, with evidence of under-search.

When the search cost is high (bottom panel), First Movers' behavior departs more markedly from the model predictions, conforming to the prediction only when the quality of the first option sampled is high. When the quality of the first option sampled is low or medium, although the empirical evidence goes in the expected direction, the difference between estimated and predicted frequencies is statistically significant. In particular, First Movers under-search when the quality is low and over-search when the quality is medium.

Besides reducing the payoff of First Movers, discrepancies between the First Movers' behavior in the experiment and optimal search also affect the quality of Second Movers' social information. Using the decomposition of the efficiency loss in Section 4.1, we analyze how First Movers' suboptimal search affects Second Movers' payoffs in the following sections.

5.2 Search with Social Information

Next, we analyze the search behavior of agents with social information in our experiment. We use all data from Part B of Experiment S collected under the Benchmark treatment.[15] In this part of the experiment, Participants S act as Second Movers, and their social information comes from the choices of the First Movers in Part B of Experiment I.

Testable Hypothesis 2. According to Testable Hypothesis 2, Second Movers should sample at the first search the option chosen by their matched First Mover (imitation).

[15] Our sample consists of 600 observations from 20 Participants S assigned to the Benchmark treatment.

Table 7 reports the observed and estimated frequency of imitation among Second Movers, conditional on the option chosen by their matched First Movers. We obtain estimated imitation frequencies with a Linear Probability Model in which we regress the Second Movers' decision to sample a given option at the first search on a binary indicator equal to 1 if the option chosen by the matched First Mover is the same as the first option sampled by the Second Mover (see Appendix B, Table B.2, columns 1–2 for the full regression results).

Table 7: Second Movers' Imitation Frequency – BENCHMARK.

Option Chosen by First Mover	Prediction	Observed	Estimated	S.E.	P-value
a_1 = purple	1	0.746	**0.737**	0.011	0.000
a_1 = orange	1	0.694	**0.687**	0.009	0.000

Notes. We consider first search decisions by Second Movers assigned to the BENCHMARK treatment in Part B of Experiment S. Estimates, standard errors, and p-values are from the regression in Appendix B, Table B.2, columns 1 and 2. P-values refer to equality tests between the theoretical and estimated frequencies (statistically significant differences are in bold).

Although frequent, imitation is not ubiquitous, and the difference from the theoretical prediction is highly significant. This discrepancy emerges despite imitation being a remarkably robust theoretical prediction, as discussed in Section 4. As we will show at the end of this section, under-imitation represents the primary source of efficiency loss for Second Movers. The treatment manipulations we introduce in Section 6 aim to examine which interventions can reduce the under-imitation we observe in the BENCHMARK treatment.

Testable Hypothesis 3. According to Testable Hypothesis 3, Second Movers should sample the second option if and only if the quality of the first option sampled is medium and the search cost is low ($q_{s_2^1} = 9$ and $c_2 = 1$). Table 8 reports the observed and estimated frequency with which Second Movers sample the second option. We obtain estimated search frequencies with a Linear Probability Model in which we regress search occurrences on a binary indicator equal to 1 in the only scenario in which Second Movers should sample the second option (see Appendix B, Table B.2 for the full regression results).

Table 8: Second Movers' Second Search Frequency – BENCHMARK.

Scenario	Prediction	Observed	Estimated	S.E.	P-value
$q_{s_2^1} = 9$ and $c_2 = 1$	1	0.736	**0.722**	0.043	0.003
$q_{s_2^1} \neq 9$ or $c_2 = 4$	0	0.195	**0.199**	0.011	0.000

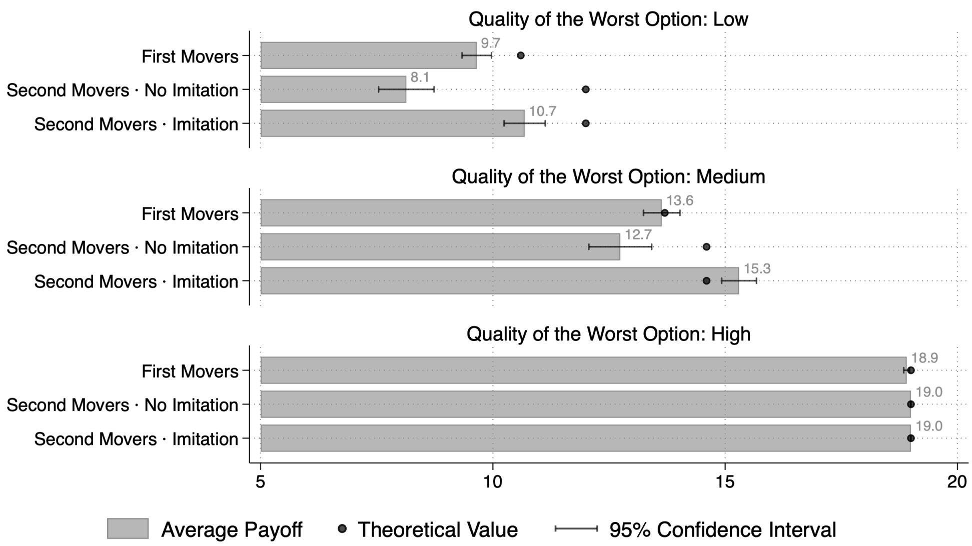
Notes. Considering the BENCHMARK treatment in Part B of Experiment S, we focus on the restricted sample of second search decisions by Second Movers who imitate at the first search. Estimates, standard errors, and p-values are obtained from the regression in Appendix B, Table B.2, column 3. P-values refer to equality tests between the theoretical and estimated frequencies (statistically significant differences in bold). We report in Appendix B, Table B.2, column 4 the regression results with the unrestricted sample of all Second Movers assigned to the BENCHMARK treatment in Part B of Experiment S.

The empirical evidence goes in the expected direction. When the quality of the first option sampled is medium, and the search cost is low, Second Movers conduct the second costly search more than 70% of the time. This probability decreases below 20% in all other scenarios combined. However, the difference between estimated and predicted frequencies is statistically significant: Second Movers under-search in the only scenario in which the model predicts they should sample the second option and over-search otherwise.

To unpack what happens in the latter scenarios, we regress search occurrences on a series of binary indicators corresponding to the possible combinations of search cost and the quality of the first option sampled in which the model predicts that Second Movers should not sample the second option (see Appendix B, Table B.2, columns 5–6 for the full regression results). We find consistent evidence of over-search across all scenarios except when the quality of the first option sampled is high. The most prominent tendency to over-search emerges when the quality of the first option sampled is low and the Second Mover's search cost is also low. As we discuss in Section 4, this tendency may emerge as an optimal response to Second Movers' expectation that their matched First Movers may discontinue the search with some positive probability after sampling a low-quality option first.

Testable Hypothesis 4. According to Testable Hypothesis 4, we expect the payoff of Second Movers to be greater than that of First Movers when the realized quality of at least one of the two options is low or medium. Figure 1 shows that this occurs only conditional on imitation at the first search. When, instead, Second Movers begin their search from the option not chosen by the matched First Mover, their payoff is substantially lower than predicted and even lower than the payoff of First Movers when the realized quality of the worst option is low. In general, the average payoff of Second Movers is 12.35 and that of First Movers is 11.73, compared to a predicted value of 13.64 and 12.56, respectively (see Table 5).

Figure 1: Average Payoff of First and Second Movers – Benchmark.



Notes. We compute average payoffs at the individual level for First and Second Movers (assigned to the Benchmark treatment), considering all rounds in Part B of Experiment I and S, respectively.

Table 9: Payoff Effects, First and Second Movers – Benchmark.

	(1)	(2)	(3)
Second Movers	0.623		
	(0.382)		
Second Movers · No Imitation		-1.364*	
		(0.515)	
Second Movers · Imitation		1.337**	
		(0.328)	
Worst L			-9.257***
			(0.577)
Worst M			-5.273***
			(0.802)
Second Movers · Worst L · No Imitation			-1.515**
			(0.413)
Second Movers · Worst L · Imitation			1.031*
			(0.479)
Second Movers · Worst M · No Imitation			-0.893
			(1.066)
Second Movers · Worst M · Imitation			1.664***
			(0.269)
Second Movers · Worst H · No Imitation			0.092
			(0.058)
Second Movers · Worst H · Imitation			0.092
			(0.058)
Constant	11.728***	11.728***	18.908***
	(0.386)	(0.389)	(0.058)
Observations	40	56	153
R2-overall	0.061	0.366	0.856

Notes. Ordinary Least Squares. We consider First Movers and Second Movers assigned to the Benchmark treatment in Part B of Experiments I and Experiment S, respectively. The dependent variable is a continuous variable measuring the participant's average round payoff: for each model specification, individual averages are obtained within the context of interest; Column 1: the average payoff accounts for all rounds played by each individual (an observation is a participant); Column 2: for each Second Mover, we compute, separately, the average payoff realized under rounds in which the Second Mover did and did not imitate; Column 3: for Second Movers, we further break down and compute, separately, the average payoff realized conditioning also on the quality of worst option (an observation is a participant in a given condition). *Second Movers* is a binary indicator equal to 1 for the participants who played as Second Movers. *Imitation* is a binary indicator equal to 1 if the option sampled first by the Second Mover in that round is the same as the option chosen by the matched First Mover. *Worst L* (*M*, *H*) is a binary indicator equal to 1 if the realized quality of the worst option faced by the participant in that round was low (medium, or high). Standard errors, clustered at the matching subgroup level, are in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

We conclude with the payoff analysis in Table 9. In all columns, the dependent variable is the participants' average round payoff.[16] In column 1, the constant corresponds to the

[16]Table B.3 in the Appendix shows an alternative version of Table 9, which uses one observation per individual per round. Results are qualitatively the same.

overall average round payoff of First Movers, and the estimated coefficient for the binary indicator "Second Mover" captures the overall difference between the average round payoff of First and Second Movers. According to model predictions, this coefficient should be positive and statistically significant. Consistent with the graphical evidence in Figure 1, the coefficient is positive but not statistically significant: without controlling for imitation, social information does not significantly increase Second Movers' payoffs.

In column 2, we analyze the difference between First and Second Movers' payoffs, conditioning on whether the Second Movers imitated. Being Second Movers yields a payoff gain of the predicted magnitude only when Second Movers imitate. In column 3, we further analyze payoff differences conditioning on both imitation and options' quality: we interact the indicator variable that captures imitation with two other binary indicators identifying pairs where the realized quality of the worst option is low or medium, in line with Figure 1. In this case, the constant corresponds to the average payoff First Movers obtain when the realized quality of both options is high. In addition to confirming that Second Movers' payoff is significantly higher than that of First Movers only when Second Movers imitate, this regression establishes that not imitating First Movers' choices can even reduce Second Movers' payoff below that of First Movers when the realized quality of the worst option is low.

Figure 1 and Table 9 clearly show that the payoff of Second Movers is particularly low (compared to the theoretical predictions) when they do not imitate, and at least one of the two options has a low or medium quality. To analyze the source of this efficiency loss in more detail, we follow the decomposition introduced in Section 4.1 (see Appendix B, Table B.6 for details), focusing on the cases in which the payoff of Second Movers is lower than predicted, which accounts for 29.0% of the observations. The overall efficiency loss for this subsample is 38.5%, and the main source of efficiency loss, accounting for almost half of the total loss, is the lack of imitation (17.4%), followed by search mistakes by Second Movers, who engage in both over- or under-search (12.8%), and by First Movers' under-search (8.3%).

6 Experimental Treatments

Motivated by the loss of efficiency observed in the Benchmark treatment compared to the predicted value of social information, we explore possible remedies that help increase the efficiency of social search by reducing under-imitation. Specifically, we introduce three treatment manipulations in Experiment S designed to help Second Movers more easily identify and/or select the most reliable First Mover; next, we examine the impact of these manipulations on participants' behavior and overall efficiency. The goal is to offer insights into which interventions, or market design, can help increase the efficiency of social search by inducing agents to trust and rely more on their social information.[17]

[17] Alternatively, we could implement interventions seeking to reduce First Movers' under-search or Second Movers' under- or over-search. We deliberately do not pursue this direction because educational interventions or selection mechanisms that seek to mitigate the presence of irrational agents in a society (or platform) are, at best, more complex and time-intensive to implement than initiatives that promote reputa-

Across treatments, the design of Part A remains identical, while we vary the design of Part B over two dimensions, as summarized in Table 10:

- *Signal vs. No Signal.* Under the "Signal" condition, Second Movers receive a signal on the past performance of the 4 First Movers in their subgroup, whose composition remains fixed throughout the session. The signal consists of the ranking of First Movers within their subgroup, depending on the cumulative payoff First Movers earned over the 15 rounds in Part A. Under the "No Signal" condition, Second Movers receive no information on the performance of the First Movers in their subgroup.
- *Exogenous vs. Endogenous Matching.* Under the "Exogenous Matching" condition, Second Movers have no control over the matching procedure: at the beginning of each round, their matched First Mover is selected through a standard random matching protocol within each subgroup. Under the "Endogenous Matching" condition, Second Movers choose, in each round, with which First Movers to match within their subgroup before making any other choice.

Table 10: Experiment S, Part B – Treatment Conditions.

	Exogenous Matching	**Endogenous Matching**
No Signal	Benchmark	NoSignal-Endo
Signal	Signal-Exo	Signal-Endo

We implement our treatments through a *2-by-2 between-subjects factorial design* and equally divide $N_B = 80$ participants in the 4 treatment conditions ($N_B^T := N_B/4 = 20$).[18]

Imitation across Treatments. To study the impact of our treatments on Second Movers' propensity to imitate their matched First Movers, we run the panel regression

$$\begin{aligned} I(s^2_{1,it} = x) = \beta_0 &+ \beta_1 \cdot I(a_{1,it} = x) \\ &+ \beta_{1,\,\textsc{NoSignal-Endo}} \cdot [I(a_{1,it} = x) \cdot I(T_i = \textsc{NoSignal-Endo})] \\ &+ \beta_{1,\,\textsc{Signal-Exo}} \cdot [I(a_{1,it} = x) \cdot I(T_i = \textsc{Signal-Exo})] \\ &+ \beta_{1,\,\textsc{Signal-Endo}} \cdot [I(a_{1,it} = x) \cdot I(T_i = \textsc{Signal-Endo})] \\ &+ \eta_i + \varepsilon_{it}. \end{aligned} \tag{7}$$

The dependent variable is a binary indicator equal to 1 if the participant samples option $x \in \{orange, purple\}$ at the first search. The explanatory variable $I(a_1 = x)$ is a binary indicator equal to 1 if the option chosen by the matched First Mover is the same as the first option sampled by the Second Mover. The interactions between the indicator $I(a_1 = x)$ and the treatment indicators allow us to test whether the imitation propensity in other treatments differs from the Benchmark. We also estimate the imitation frequency by option type for each treatment condition separately and compare it with the theoretical predictions (invariant across treatments).

tional assessments. Therefore, we believe our approach yields broader and more immediate policy insights.

[18]This sample size allows us to reach a minimum detectable effect (MDE) equal to 1 standard deviation.

The results are summarized in Table 11, where we report observed and estimated frequencies of imitation by treatment, conditional on the option chosen by the matched First Movers (see Appendix B, Table B.4 for the full regression results). The results show that the extent to which Second Movers trust the social information coming from First Movers changes significantly and crucially depends on whether Second Movers can choose the First Mover to match with.

Table 11: Second Movers' Imitation Frequency – Treatment Differences.

Option Chosen by First Mover	Prediction	Observed	Estimated	S.E.	P-value
BENCHMARK					
a_1 = purple	1	0.746	**0.737**	0.011	0.000
a_1 = orange	1	0.694	**0.687**	0.009	0.000
NOSIGNAL-ENDO					
a_1 = purple	1	0.844	**0.826***	0.051	0.028
a_1 = orange	1	0.864	**0.850***	0.040	0.020
SIGNAL-EXO					
a_1 = purple	1	0.600	**0.608*****	0.029	0.000
a_1 = orange	1	0.518	**0.524*****	0.022	0.000
SIGNAL-ENDO					
a_1 = purple	1	0.845	**0.842***	0.045	0.024
a_1 = orange	1	0.795	**0.792***	0.035	0.004

Notes. We consider first search decisions by Second Movers in Part B of Experiment S. Estimates, standard errors, and p-values are from the regression in Appendix B, Table B.4, columns 2-5. P-values refer to equality tests between the theoretical and estimated frequencies (statistically significant differences in bold). Symbols ***, **. and * denote a significant difference with respect to the BENCHMARK treatment in the pooled regression (Table B.4, column 1) at 1%, 5%, and 10%.

The impact of social information is significantly stronger under endogenous matching. The estimated frequency of imitation increases above 80% in the NOSIGNAL-ENDO and SIGNAL-ENDO treatments, lying above the imitation frequency in the BENCHMARK. Interestingly, when matching is endogenous, the availability of signals on First Movers' previous performance does not play a significant role, suggesting that *internal* reputational assessments, built on Second Movers' past (voluntary) matching experiences, outweigh *external* signals based on First Movers' past performance.

In contrast, under exogenous matching, receiving an *external* signal on the reliability of their First Mover substantially affects their imitation propensity. More specifically, when signals are available, the estimated frequency of imitation drops to approximately 60% (or even below), and the difference from the BENCHMARK is strongly significant. Under exogenous matching, observing the First Mover's choice only marginally impacts the Second Movers' first sampling decisions. We explain this last finding by observing that in this treatment (SIGNAL-EXO), Second Movers may learn they have been matched with First Movers who carry a negative signal, and this may induce the former to trust the latter even less than in the Benchmark, where Second Movers observe no signal.

Second Search across Treatments. In Table 12, we report the average search frequency for all treatments, including the Benchmark. Treatment differences are estimated using the same empirical approach as in equation (7), with two main differences: the dependent variable is now a binary indicator equal to 1 if Second Movers conduct the second costly search, and the main explanatory variable is a binary indicator equal to 1 in the only scenario in which the second search is expected ($q_{s_2^1} = 9$ and $c_2 = 1$). As for the analysis of Second Movers' second search decisions in the Benchmark (Table 8 and Table B.2 Appendix B), we focus on the restricted sample of second search decisions by Second Movers who follow a rational (imitative) policy at the first search, as our theoretical prediction on the evolution of the search process only holds in these cases.

Table 12: Second Movers' Second Search Frequency – Treatment Differences.

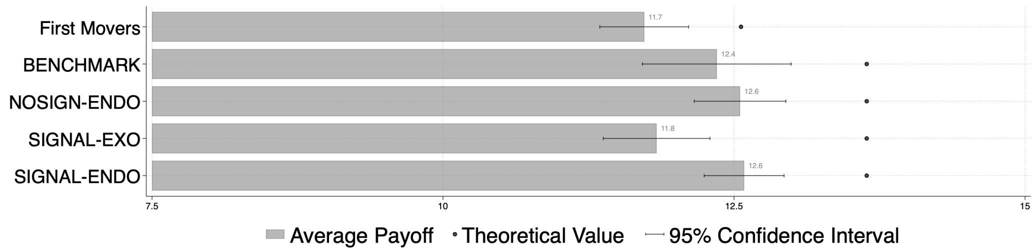
Scenario	Prediction	Observed	Estimated	S.E.	P-value
Benchmark					
$q_{s_2^1} = 9$ and $c_2 = 1$	1	0.736	**0.722**	0.043	0.003
$q_{s_2^1} \neq 9$ or $c_2 = 4$	0	0.195	**0.199**	0.011	0.000
NoSignal-Endo					
$q_{s_2^1} = 9$ and $c_2 = 1$	1	0.635	**0.642**	0.094	0.019
$q_{s_2^1} \neq 9$ or $c_2 = 4$	0	0.249	**0.248**	0.022	0.000
Signal-Exo					
$q_{s_2^1} = 9$ and $c_2 = 1$	1	0.618	**0.611**	0.048	0.001
$q_{s_2^1} \neq 9$ or $c_2 = 4$	0	0.254	**0.256**	0.012	0.000
Signal-Endo					
$q_{s_2^1} = 9$ and $c_2 = 1$	1	0.699	**0.694**	0.062	0.008
$q_{s_2^1} \neq 9$ or $c_2 = 4$	0	0.202	**0.203**	0.014	0.000

Notes. We consider the restricted sample of second search decisions in Part B of Experiment S by Second Movers who imitate at the first search. Estimates, standard errors, and p-values are from the within-treatment regressions in Appendix B, Table B.5, columns 2-5. P-values refer to equality tests between the theoretical and estimated frequencies (statistically significant differences in bold). Symbols ***, **, and * denote a significant difference with respect to the Benchmark treatment in the pooled regression (Table B.5, column 1), at 1%, 5%, and 10%.

Unlike the results about imitation, our treatments do not affect the probability that Second Movers behave as the theory predicts. There are no treatments under which Second Movers' behavior at the second search is statistically different from that under the Benchmark treatment. The non-monotonic prediction of Second Movers' reaction to the quality of the option sampled at the first search is counterintuitive and relies on strong assumptions about their ability to perform sophisticated Bayesian reasoning. Since we do not manipulate this aspect across treatments, this may contribute to explaining the lack of a treatment effect in this behavioral dimension.

Payoff Effects across Treatments. Lastly, we analyze how treatment manipulations affect the payoff of Second Movers. Figure 2 shows the average payoffs realized by Second Movers in each treatment and the average payoffs of First Movers (as a reference). The positive effect of social information is maximal when Second Movers are allowed to choose their source of social information (NoSignal-Endo and Signal-Endo).

Figure 2: Average Payoff of First and Second Movers – Treatment Differences.



Notes. We compute average payoffs considering the payoffs earned by First and Second Movers over all rounds in Part B of Experiment I and S, respectively.

Table 13: Payoff Effects – Treatment Differences.

	Overall	Quality of the Worst Option		
		Low	Medium	High
Second Movers · Benchmark	0.623	0.502	0.770	0.092
	(0.385)	(0.358)	(0.590)	(0.058)
Second Movers · NoSignal-Endo	0.823**	0.941**	0.564	-0.225
	(0.269)	(0.311)	(0.467)	(0.329)
Second Movers · Signal-Exo	0.107	0.353	-0.655	-0.108
	(0.289)	(0.241)	(0.631)	(0.169)
Second Movers · Signal-Endo	0.858**	1.111***	0.315	0.092
	(0.190)	(0.188)	(0.388)	(0.058)
Constant	11.728***	9.652***	13.635***	18.908***
	(0.389)	(0.583)	(0.799)	(0.058)
Observations	100	100	100	100
R2	0.104	0.070	0.079	0.050

Notes. Ordinary Least Squares. An observation is a participant. The dependent variable is a continuous variable measuring the participant's average round payoff. Column 1: the average payoff accounts for all rounds played in Part B (of Experiment I and S, respectively, for First and Second Movers); Columns 2-4: the average payoff is obtained considering only rounds in which the realized quality of at least one of the two options is low or medium, respectively. $Second\ Movers$ is a binary indicator equal to 1 for the participants who played as Second Movers. Standard errors, clustered at the matching subgroup level, are in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

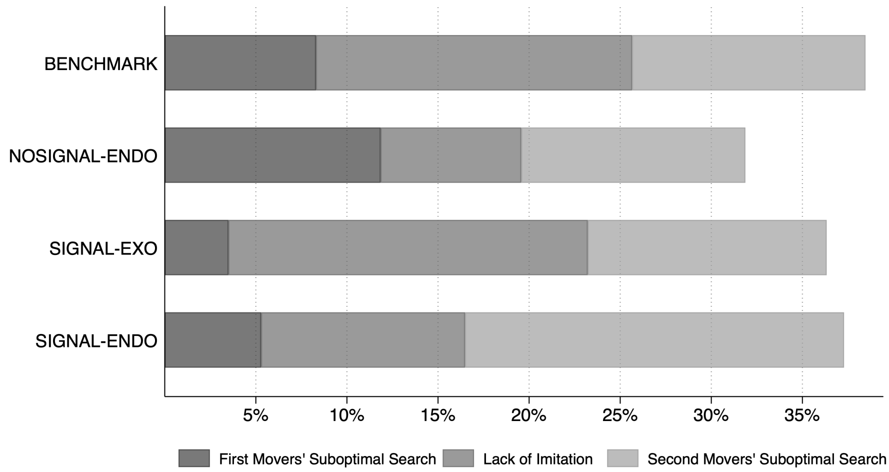
Table 13 confirms that Second Movers earn significantly more than First Movers only under endogenous matching, where the observed payoff increase (~ 1) approximates the theoretical prediction in Table 5. The results are quantitatively and statistically stronger when we restrict the analysis of payoff differences to scenarios where the realized quality of at least one of the two options is low or medium, where the model predicts that most payoff gains from social information should materialize. In light of the results emerging from

Tables 11 and 12, this positive payoff effect under endogenous matching is entirely driven by the increase in the propensity of Second Movers to imitate their matched First Mover.

We conclude the analysis of the effects of our treatment manipulations on payoffs with the decomposition of the efficiency loss in Section 4.1 (see Appendix B, Table B.6 for details). The share of cases in which the Second Movers' payoff is lower than predicted is substantially higher under the SIGNAL-EXO treatment (38.5%) and lower under the endogenous matching treatments (26.8% and 24.3% without and with signal, respectively), and so is the associated loss, compared to the BENCHMARK treatment (29.0%).

To analyze the sources of inefficiency across treatments, we zoom in on cases in which the Second Movers' payoff is lower than predicted and summarize our results in Figure 3. When matching is exogenous, and signals on First Movers' previous performance are available, the primary source of inefficiency remains Second Movers' under-imitation, which contributes to the overall efficiency loss even more than in the BENCHMARK treatment. In contrast, under endogenous matching, Second Movers' under-imitation is no longer the primary source of inefficiency: irrespective of whether signals on the information source are provided, most of the efficiency loss is due to Second Movers' suboptimal search.

Figure 3: Decomposition of Second Movers' Efficiency Loss – Treatment Differences.



Notes. For each treatment, using all data from the First and Second Movers' decisions in Part B of Experiments I and S, we quantify the sources of efficiency loss using the decomposition in Section 4.1 (see Appendix B, Table B.6 for details).

7 Conclusion

Inspired by the overwhelming evidence that agents rely on the choices of others to shape their behavior and beliefs, we conduct a lab experiment to investigate how individuals use social information when searching for information about alternative options whose quality is initially unknown. To provide a theoretical framework for our experiment, we develop a simple variant of Weitzman (1979)'s classical sequential search model in which we allow some agents (Second Movers) to observe the final choice of a peer who searched in isolation

(First Mover) before beginning their search. The theoretical analysis shows that social information has non-trivial implications for optimal search, compared to optimal search in isolation. First, social information directs agents to begin their search with the option chosen by the peer they observe, referred to as imitation. Second, social information decreases the expected gain from continuing the search and makes it non-monotone in the quality of the first option sampled. Finally, social information increases the expected payoff.

Our experiment demonstrates that, although the theoretical benchmark relies on complex Bayesian reasoning, participants' behavior broadly aligns with the model predictions. This congruence occurs despite notable deviations, suggesting that even boundedly rational agents can extract substantial value from structured social environments. The most significant deviation we document is the limited imitation propensity: Second Movers frequently fail to begin their search from the option chosen by the First Movers they observe, in contrast to what the optimal search policy prescribes. Because of this deviation, Second Movers forgo most of the informational gains embedded in the observed behavior.

This under-utilization of social information emerges as the principal source of inefficiency. Interestingly, this result is related to novel experimental evidence showing that individuals are less sensitive to information discovered by others, as opposed to discovering the exact same information by themselves (Conlon, Mani, Rao, Ridley, and Schilbach, 2025). In line with these findings, we document that inefficiencies from under-reaction to social information are mitigated when individuals more actively engage in the social information acquisition process, i.e., they can choose from whom to acquire information. Indeed, under endogenous matching, the rate of imitation rises substantially, and so does Second Movers' welfare. These mechanisms are not limited to the laboratory: they mirror features present in online platforms that rank and score contributors, such as Stack Overflow, GitHub, Duolingo, or specific subreddits. These systems allow users to filter or prioritize information from reliable sources, thus navigating uncertainty more effectively.

Our findings have normative implications. Rather than attempting to directly eliminate behavioral deviations, institutional design and careful platform architecture can productively harness simple heuristics, such as imitation, by channeling them toward high-quality social signals. In this light, promoting selective imitation through curated exposure can offer a powerful alternative to attempting to instill optimal decision-making.

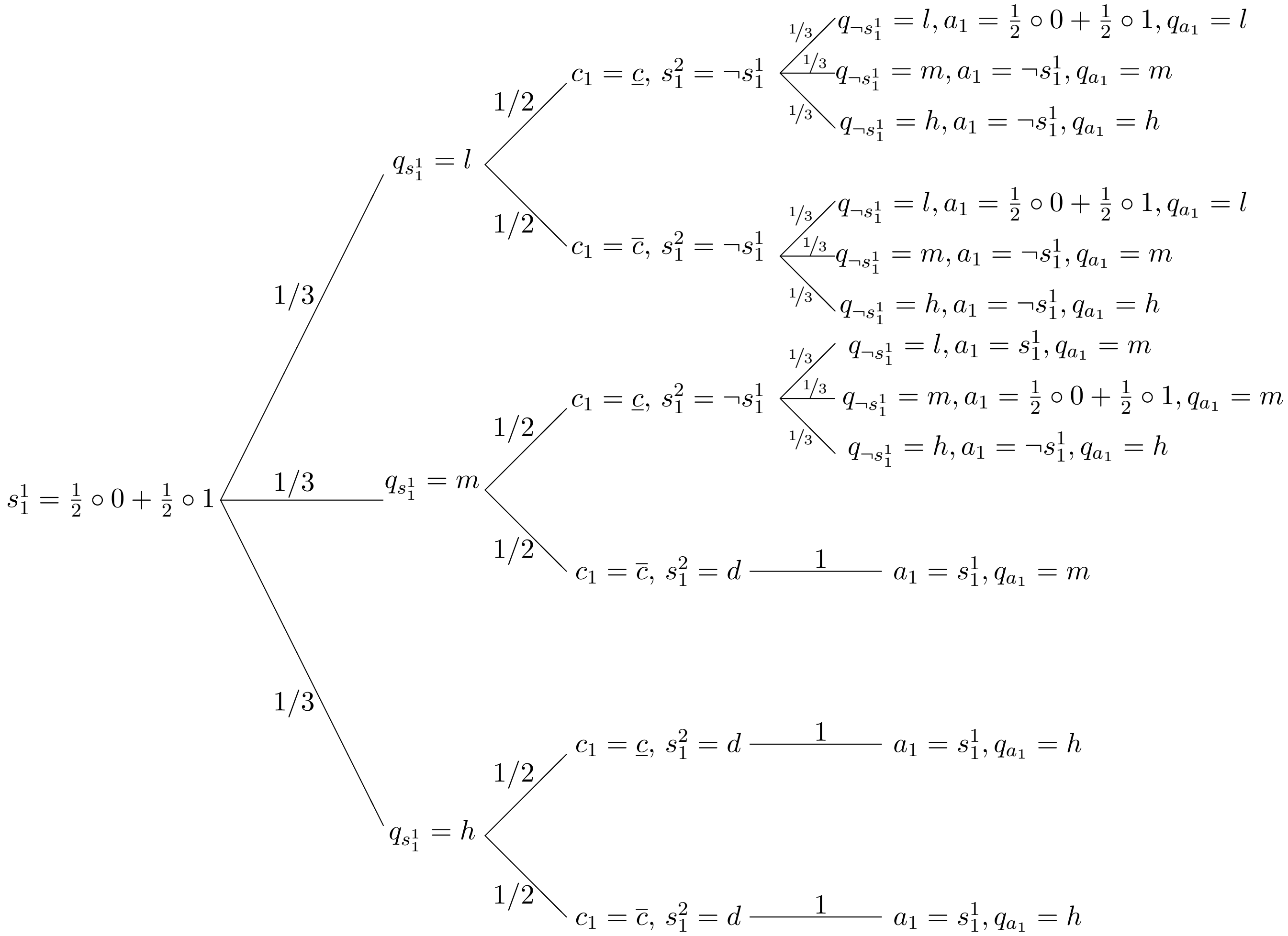
To conclude, we acknowledge that our evidence is "in vitro": it reveals what is possible under controlled conditions. The crucial question of external validity remains open. Establishing whether similar mechanisms operate outside the laboratory—where incentives, noise, and complexity differ—requires well-designed field experiments. Our analysis is a natural stepping stone for future research aimed at testing how robustly social information can guide search and product discovery in real-world environments.

A Optimal Decisions

A.1 Decision Trees

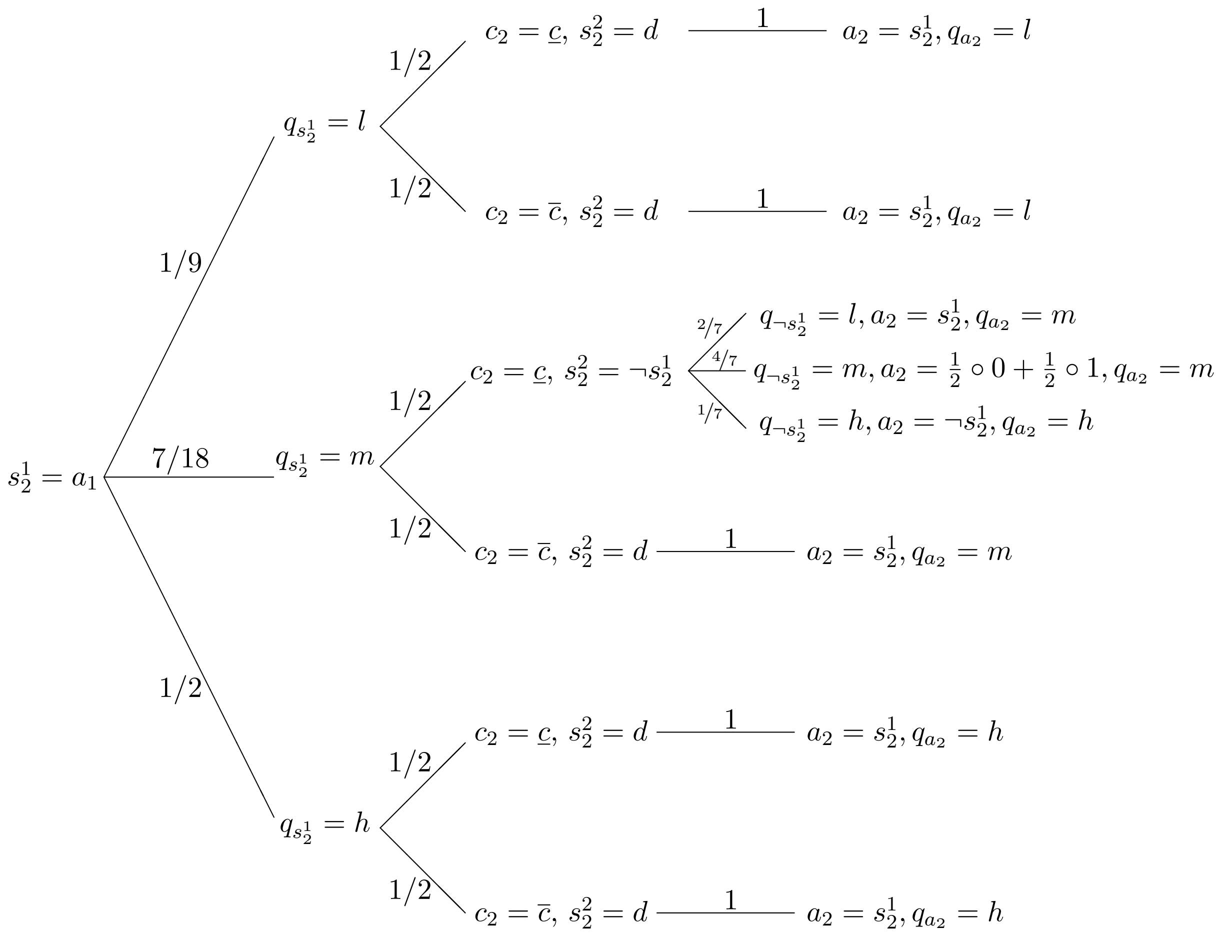
In the following, $\sum_x \xi(x) \circ x$ denotes the mixture assigning probability $\xi(x)$ to option x.

Figure A.1: Decision Tree for the Search Problem of Agent 1.



Notes. At the first search, Agent 1 decides uniformly at random which option to sample and observes its quality. At the second search, if her search cost is less than the expected gain from an additional search, Agent 1 samples the second option and observes its quality; otherwise, she discontinues the search. Finally, Agent 1 chooses the best among the options she sampled, randomizing uniformly if indifferent. At the end of each terminal node, we report the quality of the option that Agent 1 chooses.

Figure A.2: Decision Tree for the Search Problem of Agent 2.



Notes. At the first search, Agent 2 samples the option chosen by Agent 1 and observes its quality. At the second search, if her search cost is less than the expected gain from an additional search, Agent 2 samples the second option and observes its quality; otherwise, she discontinues the search. The expected gain from the second search is discounted by the probability that Agent 1 did not sample the other option. Finally, Agent 2 chooses the best among the options she sampled, randomizing uniformly if indifferent. At the end of each terminal node, we report the quality of the option that Agent 2 chooses.

A.2 Omitted Proofs

A.2.1 First Search

In this appendix, we proceed in steps to prove the following two results. The first result establishes the optimality of Agent 2's policy at the first search described in Section 2.2.

Result 1: Optimality of Imitation. Suppose Agent 1 follows the optimal first search, second search, and choice policy characterized in Section 2.1. Then, Agent 2 finds it optimal to sample option a_1 at the first search.

The second result shows that the theoretical prediction on imitation for Second Movers is valid regardless of what First Movers do at the second search, as discussed in Section 4.

Result 2: Robustness of Imitation. Suppose Agent 1 follows the optimal first search and choice policy characterized in Section 2.1. Then, Agent 2 finds it optimal to sample option a_1 at the first search regardless of the policy adopted by Agent 1 at the second search.

Step 1: Value of the Search Problem of Agent 2. To begin with, we characterize the value of Agent 2's search problem.

Step 1.(a). Suppose that:

- Agent 1 decides uniformly at random which option to sample first (as under the optimal policy characterized in Section 2.1).
- Agent 1 follows some policy $\sigma\colon C\times Q \to [0,1]$ at the second search (not necessarily the optimal policy). Here, $\sigma(c,q)$ is the probability that Agent 1 with search cost c samples the second option when the option she sampled at the first search has quality q.
- At the end of her search, Agent 1 chooses the best option she sampled, randomizing uniformly if indifferent (as under the optimal policy characterized in Section 2.1).

For all $q \in \{l, m, h\}$, let σ_q be the probability under policy σ that Agent 1 samples the second option when the option she sampled at the first search has quality q. Since search costs are drawn from the uniform distribution on $\{\underline{c}, \overline{c}\}$, we have

$$\sigma_q = \frac{1}{2}\sigma(\underline{c}, q) + \frac{1}{2}\sigma(\overline{c}, q). \tag{A.1}$$

Step 1.(b). Suppose Agent 2 sampled option x first.

- If $q_x = l$, the expected gain from the second search for Agent 2 is
$$V_\sigma(q_x = l) \coloneqq \mathbb{P}_\sigma(q_{\neg x} = m \mid q_x = l)(m - l) + \mathbb{P}_\sigma(q_{\neg x} = h \mid q_x = l)(h - l), \tag{A.2}$$
where $\mathbb{P}_\sigma$ denotes Agent 2's belief on $U \times U$ conditional on Agent 1's choice and Agent 1's search and choice policies as described in Step 1.(a) (hence, the dependence on σ). Agent 2 samples option $\neg x$ if and only if $c_2 < V_\sigma(q_x = l)$.
- If $q_x = m$, the expected gain from the second search for Agent 2 is
$$V_\sigma(q_x = m) \coloneqq \mathbb{P}_\sigma(q_{\neg x} = h \mid q_x = m)(h - m). \tag{A.3}$$
Agent 2 samples option $\neg x$ if and only if $c_2 < V_\sigma(q_x = m)$.
- If $q_x = h$, the expected gain from the second search for Agent 2 is 0, and Agent 2 discontinues the search.

Step 1.(c). By Step 1.(b), the value of the search problem of Agent 2 with search cost c_2 who samples option x at the first search is

$$\begin{aligned} V_\sigma(x, c_2) \coloneqq\ & \mathbb{E}_\sigma(q_x) \\ & + [V_\sigma(q_x = l) - c_2]\mathbb{P}_\sigma(q_x = l)\mathbb{1}\{c_2 < V_\sigma(q_x = l)\} \\ & + [V_\sigma(q_x = m) - c_2]\mathbb{P}_\sigma(q_x = m)\mathbb{1}\{c_2 < V_\sigma(q_x = m)\}. \end{aligned} \tag{A.4}$$

Step 1.(d). Suppose $x = a_1$.

- First, observe that

$$\mathbb{P}_\sigma(q_{a_1} = l) = \frac{3 - 2\sigma_l}{9}, \tag{A.5}$$

$$\mathbb{P}_\sigma(q_{a_1} = m) = \frac{3 + \sigma_l - \sigma_m}{9}, \tag{A.6}$$

$$\mathbb{P}_\sigma(q_{a_1} = h) = \frac{3 + \sigma_l + \sigma_m}{9}. \tag{A.7}$$

From equations (A.5)–(A.7), we have

$$\mathbb{E}_\sigma(q_{a_1}) = \frac{(3 - 2\sigma_l)l + (3 + \sigma_l - \sigma_m)m + (3 + \sigma_l + \sigma_m)h}{9}. \tag{A.8}$$

- Next, observe that

$$\mathbb{P}_\sigma(q_{\neg a_1} = m, q_{a_1} = l) = \frac{1 - \sigma_l}{9}, \tag{A.9}$$

$$\mathbb{P}_\sigma(q_{\neg a_1} = h, q_{a_1} = l) = \frac{1 - \sigma_l}{9}. \tag{A.10}$$

From equations (A.2), (A.5), (A.9), and (A.10), we have

$$\begin{aligned} V_\sigma(q_{a_1} = l) &= \frac{\mathbb{P}_\sigma(q_{\neg a_1} = m, q_{a_1} = l)}{\mathbb{P}_\sigma(q_{a_1} = l)}(m - l) + \frac{\mathbb{P}_\sigma(q_{\neg a_1} = h, q_{a_1} = l)}{\mathbb{P}_\sigma(q_{a_1} = l)}(h - l) \\ &= \frac{(1 - \sigma_l)(h + m - 2l)}{3 - 2\sigma_l}. \end{aligned} \tag{A.11}$$

- Finally, observe that

$$\mathbb{P}_\sigma(q_{\neg a_1} = h, q_{a_1} = m) = \frac{1 - \sigma_m}{9}. \tag{A.12}$$

From equations (A.3), (A.6), and (A.12), we have

$$\begin{aligned} V_\sigma(q_{a_1} = m) &= \frac{\mathbb{P}_\sigma(q_{\neg a_1} = h, q_{a_1} = m)}{\mathbb{P}_\sigma(q_{a_1} = m)}(h - m) \\ &= \frac{(1 - \sigma_m)(h - m)}{3 + \sigma_l - \sigma_m}. \end{aligned} \tag{A.13}$$

Step 1.(e). Suppose $x = \neg a_1$.

- First, observe that

$$\mathbb{P}_\sigma(q_{\neg a_1} = l) = \frac{3 + 2\sigma_l}{9}, \tag{A.14}$$

$$\mathbb{P}_\sigma(q_{\neg a_1} = m) = \frac{3 - \sigma_l + \sigma_m}{9}, \tag{A.15}$$

$$\mathbb{P}_\sigma(q_{\neg a_1} = h) = \frac{3 - \sigma_l - \sigma_m}{9}. \tag{A.16}$$

From equations (A.14)–(A.16), we have

$$\mathbb{E}_\sigma(q_{\neg a_1}) = \frac{(3 + 2\sigma_l)l + (3 - \sigma_l + \sigma_m)m + (3 - \sigma_l - \sigma_m)h}{9}. \tag{A.17}$$

- Next, observe that

$$\mathbb{P}_\sigma(q_{a_1} = m, q_{\neg a_1} = l) = \frac{1+\sigma_l}{9}, \tag{A.18}$$

$$\mathbb{P}_\sigma(q_{a_1} = h, q_{\neg a_1} = l) = \frac{1+\sigma_l}{9}. \tag{A.19}$$

From equations (A.2), (A.14), (A.18), and (A.19), we have

$$\begin{aligned} V_\sigma(q_{\neg a_1} = l) &= \frac{\mathbb{P}_\sigma(q_{a_1} = m, q_{\neg a_1} = l)}{\mathbb{P}_\sigma(q_{\neg a_1} = l)}(m-l) + \frac{\mathbb{P}_\sigma(q_{a_1} = h, q_{\neg a_1} = l)}{\mathbb{P}_\sigma(q_{\neg a_1} = l)}(h-l) \\ &= \frac{(1+\sigma_l)(h+m-2l)}{3+2\sigma_l}. \end{aligned} \tag{A.20}$$

- Finally, observe that

$$\mathbb{P}_\sigma(q_{a_1} = h, q_{\neg a_1} = m) = \frac{1+\sigma_m}{9}. \tag{A.21}$$

From equations (A.3), (A.15), and (A.21), we have

$$\begin{aligned} V_\sigma(q_{\neg a_1} = m) &= \frac{\mathbb{P}_\sigma(q_{a_1} = h, q_{\neg a_1} = m)}{\mathbb{P}_\sigma(\neg q_{a_1} = m)}(h-m) \\ &= \frac{(1+\sigma_m)(h-m)}{3-\sigma_l+\sigma_m}. \end{aligned} \tag{A.22}$$

Step 1.(f). Summing up, from definition (A.4) and equations (A.5), (A.6), (A.8), (A.11), and (A.13), we have that the value of the search problem of Agent 2 with search cost c_2 who samples option a_1 at the first search is

$$\begin{aligned} V_\sigma(a_1, c_2) = &\frac{(3-2\sigma_l)l + (3+\sigma_l-\sigma_m)m + (3+\sigma_l+\sigma_m)h}{9} \\ &+ \left[\frac{(1-\sigma_l)(h+m-2l)}{3-2\sigma_l} - c_2\right]\frac{3-2\sigma_l}{9}\mathbb{1}\left\{c_2 < \frac{(1-\sigma_l)(h+m-2l)}{3-2\sigma_l}\right\} \\ &+ \left[\frac{(1-\sigma_m)(h-m)}{3+\sigma_l-\sigma_m} - c_2\right]\frac{3+\sigma_l-\sigma_m}{9}\mathbb{1}\left\{c_2 < \frac{(1-\sigma_m)(h-m)}{3+\sigma_l-\sigma_m}\right\}. \end{aligned}$$

Similarly, from definition (A.4) and equations (A.14), (A.15), (A.17), (A.20), and (A.22), we have that the value of the search problem of Agent 2 with search cost c_2 who samples option $\neg a_1$ at the first search is

$$\begin{aligned} V_\sigma(\neg a_1, c_2) = &\frac{(3+2\sigma_l)l + (3-\sigma_l+\sigma_m)m + (3-\sigma_l-\sigma_m)h}{9} \\ &+ \left[\frac{(1+\sigma_l)(h+m-2l)}{3+2\sigma_l} - c_2\right]\frac{3+2\sigma_l}{9}\mathbb{1}\left\{c_2 < \frac{(1+\sigma_l)(h+m-2l)}{3+2\sigma_l}\right\} \\ &+ \left[\frac{(1+\sigma_m)(h-m)}{3-\sigma_l+\sigma_m} - c_2\right]\frac{3-\sigma_l+\sigma_m}{9}\mathbb{1}\left\{c_2 < \frac{(1+\sigma_m)(h-m)}{3-\sigma_l+\sigma_m}\right\}. \end{aligned}$$

Step 2: Proof of Result 1. Suppose Agent 1 follows the optimal search and choice policy characterized in Section 2.1. Under this policy, we have

$$\sigma(\underline{c}, l) = \sigma(\bar{c}, l) = \sigma(\underline{c}, m) = 1 \qquad \text{and} \qquad \sigma(\bar{c}, m) = \sigma(\underline{c}, h) = \sigma(\bar{c}, h) = 0. \tag{A.23}$$

Equations (A.1) and (A.23) imply

$$\sigma_l = 1, \qquad \sigma_m = \frac{1}{2}, \qquad \sigma_h = 0. \tag{A.24}$$

In turn, equations (A.11), (A.13), (A.20), (A.22), and (A.24) imply

$$V_\sigma(q_{a_1} = l) = 0, \qquad V_\sigma(q_{a_1} = m) = \frac{h-m}{7}, \tag{A.25}$$

and

$$V_\sigma(q_{\neg a_1} = l) = \frac{2(h+m-2l)}{5}, \qquad V_\sigma(q_{\neg a_1} = m) = \frac{3(h-m)}{5}. \tag{A.26}$$

Recall that we assume

$$0 < l < m < h, \quad 0 < \underline{c} < \bar{c}, \quad \underline{c} < \frac{h-m}{3} < \bar{c} < \frac{m+h-2l}{3}, \quad \underline{c} < \frac{h-m}{7}. \tag{A.27}$$

Equalities and inequalities (A.25)–(A.27) imply that only two cases are possible:

- Case 1:

$$V_\sigma(q_{a_1} = l) < \underline{c} < V_\sigma(q_{a_1} = m) < V_\sigma(q_{\neg a_1} = m) < \bar{c} < V_\sigma(q_{\neg a_1} = l). \tag{A.28}$$

- Case 2:

$$V_\sigma(q_{a_1} = l) < \underline{c} < V_\sigma(q_{a_1} = m) < \bar{c} \leq \min\{V_\sigma(q_{\neg a_1} = l), V_\sigma(q_{\neg a_1} = m)\}. \tag{A.29}$$

We now show that, in each case, we have $V_\sigma(a_1, c_2) > V_\sigma(\neg a_1, c_2)$ for all $c_2 \in \{\underline{c}, \bar{c}\}$, from which the desired result follows.

Case 1. Suppose condition (A.28) holds.

- If $c_2 = \underline{c}$, we have

$$\begin{aligned}
&V_\sigma(a_1, \underline{c}) - V_\sigma(\neg a_1, \underline{c}) \\
&\quad = \mathbb{E}_\sigma(q_{a_1}) + [V_\sigma(q_{a_1} = m) - \underline{c}]\mathbb{P}_\sigma(q_{a_1} = m) \\
&\quad - \mathbb{E}_\sigma(q_{\neg a_1}) - [V_\sigma(q_{a_1} = l) - \underline{c}]\mathbb{P}_\sigma(q_{\neg a_1} = l) - [V_\sigma(q_{\neg a_1} = l) - \underline{c}]\mathbb{P}_\sigma(q_{\neg a_1} = m) \\
&\quad = \frac{2l+7m+9h}{18} + \left[\frac{h-m}{7} - \underline{c}\right]\frac{7}{18} \\
&\quad - \frac{10l+5m+3h}{18} - \left[\frac{2(h+m-2l)}{5} - \underline{c}\right]\frac{5}{9} - \left[\frac{3(h-m)}{5} - \underline{c}\right]\frac{5}{18} \\
&\quad = \frac{4\underline{c}}{9} \\
&\quad > 0,
\end{aligned}$$

where: the first equality holds by definition (A.4) and condition (A.28); the second equality holds by equalities (A.6), (A.8), (A.13), (A.14), (A.15), (A.17), (A.20), (A.22), and (A.24); the inequality holds because $\underline{c} > 0$.

- If $c_2 = \bar{c}$, we have

$$
\begin{aligned}
&V_\sigma(a_1, \bar{c}) - V_\sigma(\neg a_1, \bar{c}) \\
&\qquad = \mathbb{E}_\sigma(q_{a_1}) - \mathbb{E}_\sigma(q_{\neg a_1}) - [V_\sigma(q_{\neg a_1} = l) - \bar{c}]\mathbb{P}_\sigma(q_{\neg a_1} = l) \\
&\qquad = \frac{2l + 7m + 9h}{18} - \frac{10l + 5m + 3h}{18} - \left[\frac{2(h + m - 2l)}{5} - \bar{c}\right]\frac{5}{9} \\
&\qquad = \frac{h - m + 5\bar{c}}{9} \\
&\qquad > 0,
\end{aligned}
$$

where: the first equality holds by definition (A.4) and condition (A.28); the second equality holds by equalities (A.8), (A.14), (A.15), (A.17), (A.20), and (A.24); the inequality holds because $h > m$ and $\underline{c} > 0$.

Case 2. Suppose condition (A.29) holds.

- If $c_2 = \underline{c}$, we have

$$
\begin{aligned}
&V_\sigma(a_1, \underline{c}) - V_\sigma(\neg a_1, \underline{c}) \\
&\qquad = \mathbb{E}_\sigma(q_{a_1}) + [V_\sigma(q_{a_1} = m) - \underline{c}]\mathbb{P}_\sigma(q_{a_1} = m) \\
&\qquad - \mathbb{E}_\sigma(q_{\neg a_1}) - [V_\sigma(q_{a_1} = l) - \underline{c}]\mathbb{P}_\sigma(q_{\neg a_1} = l) - [V_\sigma(q_{\neg a_1} = l) - \underline{c}]\mathbb{P}_\sigma(q_{\neg a_1} = m) \\
&\qquad = \frac{2l + 7m + 9h}{18} + \left[\frac{h - m}{7} - \underline{c}\right]\frac{7}{18} \\
&\qquad - \frac{10l + 5m + 3h}{18} - \left[\frac{2(h + m - 2l)}{5} - \underline{c}\right]\frac{5}{9} - \left[\frac{3(h - m)}{5} - \underline{c}\right]\frac{5}{18} \\
&\qquad = \frac{4\underline{c}}{9} \\
&\qquad > 0,
\end{aligned}
$$

where: the first equality holds by definition (A.4) and condition (A.29); the second equality holds by equalities (A.6), (A.8), (A.13), (A.14), (A.15), (A.17), (A.20), (A.22), and (A.24); the inequality holds because $\underline{c} > 0$.

- If $c_2 = \bar{c}$, we have

$$
\begin{aligned}
&V_\sigma(a_1, \bar{c}) - V_\sigma(\neg a_1, \bar{c}) \\
&\qquad = \mathbb{E}_\sigma(q_{a_1}) - \mathbb{E}_\sigma(q_{\neg a_1}) \\
&\qquad = -[V_\sigma(q_{\neg a_1} = l) - \bar{c}]\mathbb{P}_\sigma(q_{\neg a_1} = l) - [V_\sigma(q_{\neg a_1} = m) - \bar{c}]\mathbb{P}_\sigma(q_{\neg a_1} = m) \\
&\qquad = \frac{2l + 7m + 9h}{18} - \frac{10l + 5m + 3h}{18} \\
&\qquad - \left[\frac{2(h + m - 2l)}{5} - \bar{c}\right]\frac{5}{9} - \left[\frac{3(h - m)}{5} - \bar{c}\right]\frac{5}{18} \\
&\qquad = \frac{m - h + 15\bar{c}}{18}
\end{aligned}
$$

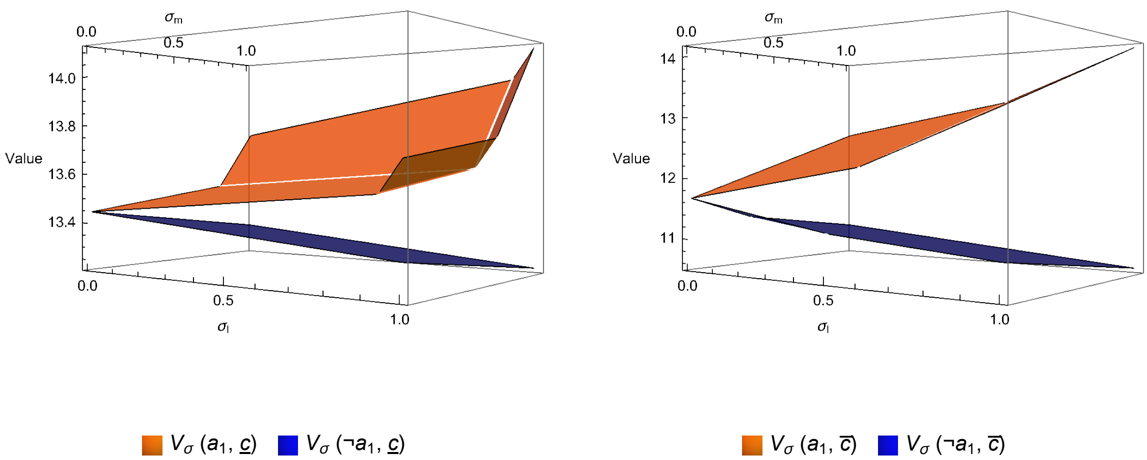
$> 0,$

where: first equality holds by definition (A.4) and condition (A.29); the second equality holds by equalities (A.8), (A.14), (A.15), (A.17), (A.20), (A.22), and (A.24); the inequality follows from the assumption $\bar{c} > \frac{h-m}{3}$, which implies $m-h+15\bar{c} > 0$. ■

Step 3: Proof of Result 2. Consider the parametrization of our experiment: $q_0, q_1 \in \{l, m, h\} = \{5, 9, 19\}$ and $c_2 \in \{\underline{c}, \bar{c}\} = \{1, 4\}$. To obtain the desired result, we have to show that $V_\sigma(a_1, c_2) > V_\sigma(\neg a_1, c_2)$ for all $c_2 \in \{\underline{c}, \bar{c}\}$ and $(\sigma_l, \sigma_m) \in [0,1] \times [0,1] \setminus \{(0,0)\}$. If $(\sigma_l, \sigma_m) = (0,0)$, then Agent 1 does not sample the second option when the quality of the first option sampled is low or medium; as a result, Agent 1's choice is uninformative about the relative quality of the two options and Agent 2's social information has no value. In this case, Agent 2's search problem becomes identical to that of Agent 1, and Agent 2 is indifferent about which option to sample first; that is, $V_\sigma(a_1, c_2) = V_\sigma(\neg a_1, c_2)$ for all $c_2 \in \{\underline{c}, \bar{c}\}$.

To avoid uninsightful calculations, we establish the desired result numerically. Figure A.3 represents the value of Agent 2's search problem as a function of (σ_l, σ_m). In the left (resp., right) panel, we represent $V_\sigma(a_1, c_2)$ and $V_\sigma(\neg a_1, c_2)$ for $c_2 = \underline{c}$ (resp., $c_2 = \bar{c}$). For each c_2, the orange plane represents $V_\sigma(a_1, c_2)$, and the blue plane represents $V_\sigma(\neg a_1, c_2)$.

Figure A.3: Value of the Search Problem of Agent 2.



Notes. We represent the value of Agent 2's search problem as a function of (σ_l, σ_m). In the left (resp., right) panel, we represent $V_\sigma(a_1, c_2)$ and $V_\sigma(\neg a_1, c_2)$ for $c_2 = \underline{c}$ (resp., $c_2 = \bar{c}$). For each c_2, the orange plane represents $V_\sigma(a_1, c_2)$, and the blue plane represents $V_\sigma(\neg a_1, c_2)$.

In both panels of the figure, the orange plane always lies above the blue plane, except when $(\sigma_l, \sigma_m) = (0,0)$, meaning that $V_\sigma(a_1, c_2) > V_\sigma(\neg a_1, c_2)$ for all $c_2 \in \{\underline{c}, \bar{c}\}$ and $(\sigma_l, \sigma_m) \in [0,1] \times [0,1] \setminus \{(0,0)\}$. It follows that Agent 2 finds it (strictly) optimal to sample option a_1 at the first search, regardless of the policy adopted by Agent 1 at the second search (i.e., regardless of σ_l and σ_m), provided that $(\sigma_l, \sigma_m) \neq (0,0)$, as we wanted to show.

A.2.2 Second Search

In this appendix, we analyze the extent to which the theoretical predictions in Section 2.2 about Agent 2's behavior at the second search are robust to Agent 1's deviations from optimal behavior. We do so conditional on imitation, i.e., assuming that Agent 2 samples option a_1 at the first search (as prescribed by Agent 2's optimal policy). We consider the parametrization of our experiment: $q_0, q_1 \in \{l, m, h\} = \{5, 9, 19\}$ and $c_2 \in \{\underline{c}, \overline{c}\} = \{1, 4\}$. Again, to avoid uninsightful calculations, we establish the desired result numerically.

Figure A.4. Suppose the option chosen by Agent 1 has a low quality: $q_{a_1} = l$. In Figure A.4, the blue curve represents $V_\sigma(q_{a_1} = l)$ in equation (A.11) (i.e., the expected gain from the second search for Agent 2 conditional on imitation at the first search when $q_{a_1} = l$) as a function of σ_l (i.e., the probability, optimal or not, that Agent 1 samples the second option when the option she sampled at the first search has quality l). The horizontal green lines correspond to Agent 2's possible search costs ($c_2 = \underline{c}$ and $c_2 = \overline{c}$). The vertical gray line corresponds to $\sigma_l^{\text{obs}} = 0.845$, the observed frequency with which First Movers in Part B of Experiment I sample the second option when the option they sampled at the first search has quality l.[19]

Given a value of σ_l, if Agent 2's search cost is greater than or equal to $V_\sigma(q_{a_1} = l)$ (i.e., it lies on or above the blue curve), Agent 2 finds it optimal to discontinue the search; if Agent 2's search cost is smaller than $V_\sigma(q_{a_1} = l)$ (i.e., it lies below the blue curve), Agent 2 finds it optimal to sample the second option.

Figure A.4 allows us to draw the following conclusions.

- Projecting the intersection of each green line with the blue curve on the horizontal axis returns the minimal value of σ_l for which the theoretical prediction in Section 2.2 about Agent 2's optimal policy as a function of her search cost c_2 remains valid.

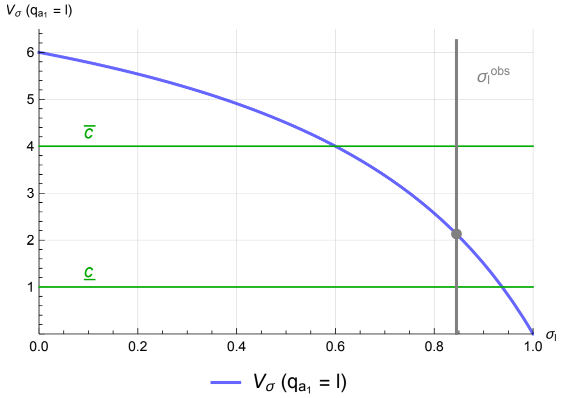
  As the figure shows, when $c_2 = \overline{c}$, Agent 2 finds it optimal to discontinue the search even if Agent 1 adopts a largely suboptimal policy at the second search—namely, as long as $\sigma_l \geq 0.6$, as opposed to $\sigma_l = 1$ under the optimal policy. Hence, this theoretical prediction is very robust to Agent 1's deviations from the optimal policy.

  When $c_2 = \underline{c}$, instead, Agent 2 finds it optimal to discontinue the search only if $\sigma_l \geq 0.9375$, which is pretty close to σ_l under the optimal policy. Hence, this theoretical prediction is more fragile to Agent 1's deviations from the optimal policy.

- Once we consider the observed frequency σ_l^{obs}, Second Movers should discontinue their search if $c_2 = \overline{c}$ (as under the theoretical prediction) but should sample the second option if $c_2 = \underline{c}$ (in contrast to the theoretical prediction).

[19] Computing σ_l^{obs} with data from Part A of Experiment I would yield analogous results: $\sigma_l^{\text{obs}} = 0.859$.

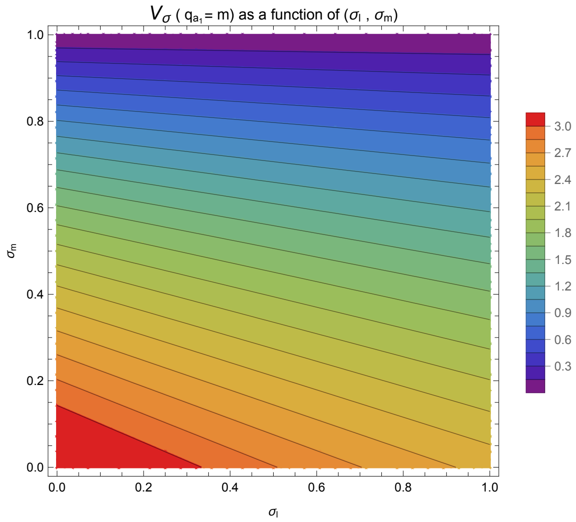
Figure A.4: Expected Gain from the Second Search for Agent 2 if $q_{a_1} = l$.



Notes. The blue curve represents $V_\sigma(q_{a_1} = l)$ in equation (A.11) (i.e., the expected gain from the second search for Agent 2 conditional on imitation at the first search when $q_{a_1} = l$) as a function of σ_l. The horizontal green lines correspond to Agent 2's possible search costs ($c_2 = \underline{c}$ and $c_2 = \overline{c}$). The vertical gray line corresponds to the observed frequency σ_l^{obs}.

Figure A.5. Suppose the option chosen by Agent 1 has a medium quality: $q_{a_1} = m$. In Figure A.5, we report the value of $V_\sigma(q_{a_1} = m)$ in equation (A.13) (i.e., the expected gain from the second search for Agent 2 conditional on imitation at the first search when $q_{a_1} = m$) as a function of σ_l (i.e., the probability, optimal or not, that Agent 1 samples the second option when the option she sampled at the first search has quality l) and σ_m (i.e., the probability, optimal or not, that Agent 1 samples the second option when the option she sampled at the first search has quality m). Different colors correspond to different values of $V_\sigma(q_{a_1} = m)$. Intuitively, for fixed σ_l (resp., σ_m), $V_\sigma(q_{a_1} = m)$ decreases in σ_m (resp., σ_l).

Figure A.5: Expected Gain from the Second Search for Agent 2 if $q_{a_1} = m$.



Notes. The figure reports the value of $V_\sigma(q_{a_1} = m)$ in equation (A.13) (i.e., the expected gain from the second search for Agent 2 conditional on imitation at the first search when $q_{a_1} = m$) as a function of (σ_l, σ_m). Different colors correspond to different values of $V_\sigma(q_{a_1} = m)$.

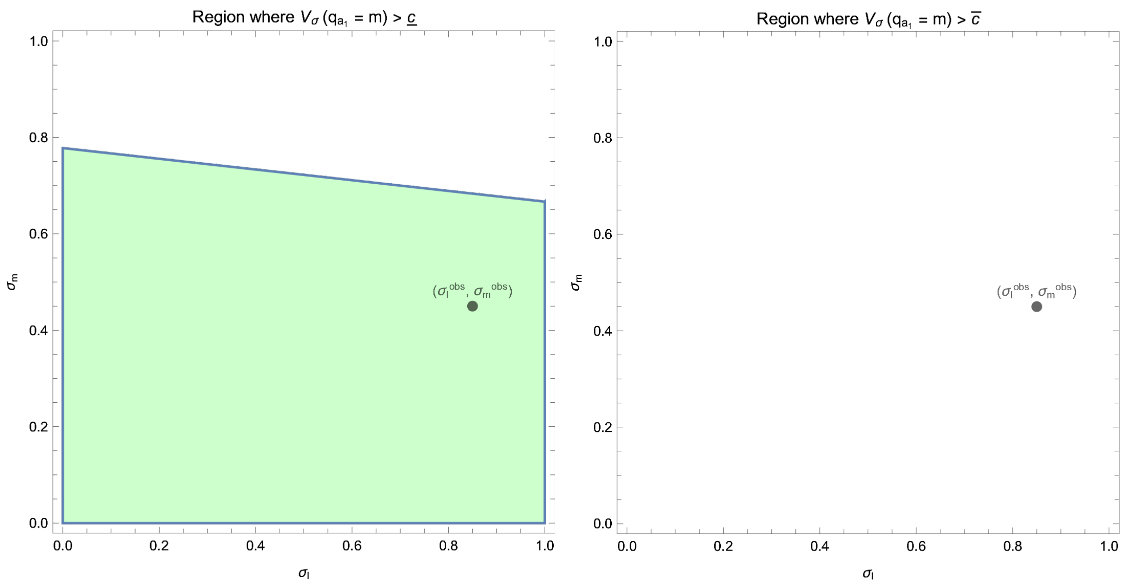
Figure A.6. Suppose the option chosen by Agent 1 has a medium quality: $q_{a_1} = m$. In both panels of Figure A.6, the green area (empty in the right panel) represents the region of the (σ_l, σ_m)-space in which $V_\sigma(q_{a_1} = m) > c_2$; the left (resp., right) panel corresponds to $c_2 = \underline{c}$ (resp., $c_2 = \overline{c}$). In both panels, the gray dot corresponds to $(\sigma_l^{\text{obs}}, \sigma_m^{\text{obs}}) = (0.845, 0.455)$, the observed frequencies with which First Movers in Part B of Experiment I sample the second option when the option they sampled at the first search has quality l and m.[20]

Figure A.6 allows us to draw the following conclusions.

- Agent 2 with $c_2 = \underline{c}$ finds it optimal to sample the second option when $q_{a_1} = m$ for a large combination of (σ_l, σ_m), including at the observed empirical frequencies $(\sigma_l^{\text{obs}}, \sigma_m^{\text{obs}})$, and not only when $(\sigma_l, \sigma_m) = (1, 1/2)$ as under the optimal policy. Hence, the corresponding theoretical prediction in Section 2.2 is very robust to Agent 1's deviations from the optimal policy.

- Agent 2 with $c_2 = \overline{c}$ always finds it optimal to discontinue the search when $q_{a_1} = m$, including at the observed empirical frequencies $(\sigma_l^{\text{obs}}, \sigma_m^{\text{obs}})$, and not only under the optimal policy $(\sigma_l, \sigma_m) = (1, 1/2)$. Hence, the corresponding theoretical prediction in Section 2.2 is extremely robust to Agent 1's deviations from the optimal policy.

Figure A.6: Optimality of Second Search for Agent 2.



Notes. In both panels, the green area (empty in the right panel) represents the region of the (σ_l, σ_m)-space in which $V_\sigma(q_{a_1} = m) > c_2$; the left panel corresponds to $c_2 = \underline{c}$, and the right panel corresponds to $c_2 = \overline{c}$. In both panels, the gray dot corresponds to the observed frequencies $(\sigma_l^{\text{obs}}, \sigma_m^{\text{obs}})$.

[20]Computing $(\sigma_l^{\text{obs}}, \sigma_m^{\text{obs}})$ with data from Part A of Experiment I would yield analogous results: $(\sigma_l^{\text{obs}}, \sigma_m^{\text{obs}}) = (0.859, 0.469)$.

B Additional Tables

Table B.1: First Movers' Second Search Frequency.

	All Rounds	R1–15	R16–30
	(1)	(2)	(3)
$I(q_{s_1^1} = 5,\, c_1 = 1)$	0.941***	0.958***	0.909***
	(0.039)	(0.037)	(0.037)
$I(q_{s_1^1} = 5,\, c_1 = 4)$	0.702***	0.719***	0.678***
	(0.057)	(0.070)	(0.065)
$I(q_{s_1^1} = 9,\, c_1 = 1)$	0.755***	0.819***	0.721***
	(0.034)	(0.024)	(0.041)
$I(q_{s_1^1} = 9,\, c_1 = 4)$	0.224**	0.294**	0.162***
	(0.058)	(0.070)	(0.026)
$I(q_{s_1^1} = 19,\, c_1 = 1)$	0.046	0.024	0.073
	(0.031)	(0.025)	(0.054)
Constant	0.010	0.034	-0.009
	(0.018)	(0.021)	(0.010)
Observations	600	300	300
R2-overall	0.539	0.580	0.514
σ_u	0.135	0.140	0.153
σ_e	0.319	0.306	0.324
ρ	0.153	0.173	0.182

Notes. Linear Probability Model with individual fixed effects. An observation is a First Mover in a round of Part B of Experiment I. Columns 1–3: the dependent variable is a binary indicator equal to 1 if the participant samples the second option; the explanatory variables $I(q_{s_2^1} = q, c_2 = c)$ are binary indicators equal to 1 if the option sampled by the First Mover at the first search is of quality $q \in \{5, 9, 19\}$ and the search cost is equal to $c \in \{1, 4\}$. Standard errors, clustered at the matching subgroup level, are in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B.2: Second Movers' First and Second Search Decisions – BENCHMARK.

	First Search		Second Search			
	(1)	(2)	(3)	(4)	(5)	(6)
$I(a_1 = purple)$	0.424***					
	(0.020)					
$I(a_1 = orange)$		0.424***				
		(0.020)				
$I(q_{s_2^1} = 9,\, c_2 = 1)$			0.523***	0.442***		
			(0.054)	(0.071)		
$I(q_{s_2^1} = 5,\, c_2 = 1)$					0.232**	0.276**
					(0.060)	(0.073)
$I(q_{s_2^1} = 5,\, c_2 = 4)$					-0.200**	-0.181
					(0.061)	(0.089)
$I(q_{s_2^1} = 9,\, c_2 = 4)$					-0.612***	-0.625***
					(0.086)	(0.052)
$I(q_{s_2^1} = 19,\, c_2 = 1)$					-0.710***	-0.710***
					(0.071)	(0.077)
$I(q_{s_2^1} = 19,\, c_2 = 4)$					-0.717***	-0.710***
					(0.085)	(0.090)
Constant	0.313***	0.263***	0.199***	0.276***	0.727***	0.712***
	(0.009)	(0.011)	(0.011)	(0.014)	(0.046)	(0.052)
Observations	600	600	430	600	430	600
R2-overall	0.190	0.190	0.222	0.131	0.597	0.590
σ_u	0.099	0.099	0.114	0.098	0.102	0.106
σ_e	0.447	0.447	0.400	0.445	0.284	0.296
ρ	0.047	0.047	0.075	0.047	0.114	0.114

Notes. Linear Probability Model with individual fixed effects. An observation is a Second Mover assigned to the BENCHMARK treatment in a round of Part B of Experiment S. Columns 1 and 2: the dependent variable is a binary indicator equal to 1 if the participant samples option $x \in \{orange, purple\}$ at the first search; $I(a_1 = x)$ is a binary indicator equal to 1 if the option chosen by the matched First Mover is the same as the first option sampled by the Second Mover; $I(q_{s_2^1} = q, c_2 = c)$ are binary indicators equal to 1 if the option sampled by the Second Mover at the first search is of quality $q \in \{5, 9, 19\}$ and the search cost is equal to $c \in \{1, 4\}$. Columns 3–6: the dependent variable is a binary indicator equal to 1 if the participant samples the second option. Columns 3 and 5 report results obtained on the restricted sample of second search decisions by only the Second Movers who follow a rational (imitative) policy at the first search. Columns 4 and 6 report results obtained on the full sample. Standard errors, clustered at the matching subgroup level, are in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B.3: Payoff Effects, First and Second Movers – BENCHMARK.

	(1)	(2)	(3)
Second Mover	0.623		
	(0.378)		
Second Mover × Imitation: NO		-1.328	
		(0.695)	
Second Mover × Imitation: YES		1.395**	
		(0.347)	
Second Mover × Lowest: L × Imitation: NO			-1.382**
			(0.387)
Second Mover × Lowest: L × Imitation: YES			1.270**
			(0.434)
Second Mover × Lowest: M × Imitation: NO			-0.849
			(1.292)
Second Mover × Lowest: M × Imitation: YES			1.696***
			(0.360)
Second Mover × Lowest: H × Imitation: NO			0.115
			(0.062)
Second Mover × Lowest: H × Imitation: YES			0.115
			(0.062)
Constant	11.728***	11.728***	18.885***
	(0.382)	(0.382)	(0.062)
Observations	1200	1200	1200
R2-overall	0.003	0.022	0.231

Notes. (Pooled) Ordinary Least Squares. An observation is a participant in a round. We consider all First Movers in Part B of Experiment I and Second Movers assigned to the Benchmark treatment in Part B of Experiment S. Columns 1–3: the dependent variable is a continuous variable measuring the participant's round payoff. $Second\ Movers$ is a binary indicator equal to 1 for the participants who played as Second Movers. *Imitation* is a binary indicator equal to 1 if the option sampled first by the Second Mover in that round is the same as the option chosen by the matched First Mover. $Worst\ L$, $Worst\ M$, and $Worst\ H$ are binary indicators equal to 1 if the realized quality of the worst option faced by the First and Second Mover in that round was low, medium, or high, respectively. Standard errors, clustered at the matching subgroup level, are in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B.4: Second Movers' Imitation Frequency – Treatment Differences.

	Pooled (1)	Benchmark (NoSignal-Exo) (2)	NoSignal-Endo (3)	Signal-Exo (4)	Signal-Endo (5)
	Dep. variable: $I(s_1^2 = purple)$				
$I(a_1 = purple)$	0.424***	0.424***	0.676***	0.132*	0.634***
	(0.020)	(0.020)	(0.091)	(0.052)	(0.080)
$I(a_1 = purple) \cdot$ NoSignal-Endo	0.252*				
	(0.101)				
$I(a_1 = purple) \cdot$ Signal-Exo	-0.292***				
	(0.051)				
$I(a_1 = purple) \cdot$ Signal-Endo	0.210*				
	(0.076)				
Constant	0.287***	0.313***	0.150**	0.476***	0.208***
	(0.012)	(0.009)	(0.040)	(0.022)	(0.035)
Observations	2400	600	600	600	600
R2-overall	0.222	0.190	0.498	0.014	0.403
	Dep. variable: $I(s_1^2 = orange)$				
$I(a_1 = orange)$	0.424***	0.424***	0.676***	0.132*	0.634***
	(0.020)	(0.020)	(0.091)	(0.052)	(0.080)
$I(a_1 = orange) \cdot$ NoSignal-Endo	0.252*				
	(0.101)				
$I(a_1 = orange) \cdot$ Signal-Exo	-0.292***				
	(0.051)				
$I(a_1 = orange) \cdot$ Signal-Endo	0.210*				
	(0.076)				
Constant	0.247***	0.263***	0.174**	0.392***	0.158**
	(0.015)	(0.011)	(0.051)	(0.029)	(0.045)
Observations	2400	600	600	600	600
R2-overall	0.248	0.190	0.498	0.014	0.403

Notes. Linear Probability Model with individual fixed effects. An observation is a Second Mover in a round of Part B of Experiment S. Observations are pooled across all treatments in Column 1, where the baseline condition corresponds to the Benchmark treatment, and analyzed separately for each treatment in Columns 2–5. Columns 1–5: the dependent variable is a binary indicator equal to 1 if the participant samples option $x \in \{orange, purple\}$ at the first search; $I(a_1 = x)$ is a binary indicator equal to 1 if the option chosen by the matched First Mover is the same as the first option sampled by the Second Mover. Standard errors, clustered at the matching subgroup level, are in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B.5: Second Movers' Second Search Frequency – Treatment Differences.

	Pooled	Benchmark (NoSignal-Exo)	NoSignal-Endo	Signal-Exo	Signal-Endo
	(1)	(2)	(3)	(4)	(5)
$I(q_{s_2^1} = 9,\ c_2 = 1)$	0.523***	0.523***	0.394**	0.355***	0.491***
	(0.054)	(0.054)	(0.116)	(0.060)	(0.076)
$I(q_{s_2^1} = 9,\ c_2 = 1) \cdot$ NoSignal-Endo	-0.129				
	(0.118)				
$I(q_{s_2^1} = 9,\ c_2 = 1) \cdot$ Signal-Exo	-0.168				
	(0.105)				
$I(q_{s_2^1} = 9,\ c_2 = 1) \cdot$ Signal-Endo	-0.031				
	(0.094)				
Constant	0.225***	0.199***	0.248***	0.256***	0.203***
	(0.011)	(0.011)	(0.022)	(0.012)	(0.014)
Observations	1765	430	513	332	490
R2-overall	0.150	0.222	0.104	0.098	0.183

Notes. Linear Probability Model with individual fixed effects. An observation is a Second Mover in a round of Part B of Experiment S. Observations are pooled across all treatments in Column 1, where the baseline condition corresponds to the Benchmark treatment, and analyzed separately for each treatment in Columns 2–5. Columns 1–5: the dependent variable is a binary indicator equal to 1 if the participant samples the second option; $I(q_{s_2^1} = q, c_2 = c)$ are binary indicators equal to 1 if the option sampled by the Second Mover at the first search is of quality $q \in \{5, 9, 19\}$ and the search cost is equal to $c \in \{1, 4\}$. We report results obtained on the restricted sample of second search decisions by only the Second Movers who follow a rational (imitative) policy at the first search. Standard errors, clustered at the matching subgroup level, are in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B.6: Decomposition of Second Movers' Efficiency Loss.

	Full Sample			
	Predicted Payoff	Actual Payoff	Share of Second Movers Actual < Predicted	Efficiency Loss
Benchmark	13.4	12.4	29.0%	8.6%
NoSignal-Endo	13.4	12.6	26.8%	6.4%
Signal-Exo	13.4	11.8	38.5%	11.9%
Signal-Endo	13.4	12.6	24.3%	7.4%
	Restricted Sample			
	Efficiency Loss	Lack of Imitation	First Movers Non-Optimal Search	Second Movers Non-Optimal Search
Benchmark	38.5%	17.4%	8.3%	12.8%
NoSignal-Endo	31.9%	7.7%	11.9%	12.3%
Signal-Exo	36.3%	19.7%	3.5%	13.1%
Signal-Endo	37.3%	11.2%	5.3%	20.8%

Notes. Top panel: an observation is a Second Mover in a round of Part B of Experiment S. Bottom panel: the sample consists of only the Second Movers whose realized payoffs are lower than predicted.

C Individual Characteristics

As pre-registered, we test whether some individual characteristics mediate the effect of social information. In particular, we are interested in evaluating the impact of:

- Statistical numeracy and risk literacy, which we measure through participants' scores in the Berlin test administered at the end of all experimental tasks to all participants.
- Overconfidence, which we measure—only for participants assigned to Experiment S—as the distance between participants' self-assessment of their performance, elicited at the end of the Berlin test, and their actual performance.[21]

Table C.1, which reports the analysis run on the decisions taken by all Second Movers in Part B of Experiment S (pooled across treatments), shows that neither of these individual characteristics plays a critical role. Regression results show that a marginally significant effect of the expected sign is detected only at the first search: higher overconfidence correlated with a lower imitation propensity.

As robustness, we expand the model to also account for risk preferences, which we assess for all participants through the streamlined version of the Preference Survey Module by Falk et al. (2023), in which participants face 5 non-incentivized interdependent choices between a lottery and a safe payment (quantitative risk measure). The results, reported in Table C.2, are unaffected.

[21]For robustness, we repeat the analysis relying on an alternative measure of overconfidence, equal to the distance between participants' belief about their performance and their belief about the average performance of other participants in the Berlin test. The results remain qualitatively unchanged (see Table C.3).

Table C.1: Individual Characteristics

	First Search		Second Search		Payoff	
	(1)	(2)	(3)	(4)	(5)	(6)
$I(a_1 = purple)$	0.544***					
	(0.058)					
$I(a_1 = purple) \cdot Literacy$	-0.016					
	(0.033)					
$I(a_1 = purple) \cdot OverConf$	-0.078*					
	(0.030)					
$I(a_1 = orange)$		0.544***				
		(0.058)				
$I(a_1 = orange) \cdot Literacy$		-0.016				
		(0.033)				
$I(a_1 = orange) \cdot OverConf$		-0.078*				
		(0.030)				
$I(q_{s_2^1} = 9,\, c_2 = 1)$			0.389***	0.373**		
			(0.081)	(0.111)		
$I(q_{s_2^1} = 9,\, c_2 = 1) \cdot Literacy$			0.016	0.006		
			(0.011)	(0.015)		
$I(q_{s_2^1} = 9,\, c_2 = 1) \cdot OverConf$			0.038	0.009		
			(0.042)	(0.059)		
Second Mover					0.882*	
					(0.365)	
Second Mover · *Literacy*					-0.060	-0.012
					(0.117)	(0.119)
Second Mover · *OverConf*					-0.272	-0.172
					(0.143)	(0.162)
Second Mover · BENCHMARK						0.715
						(0.384)
Second Mover · NOSIGNAL-ENDO						0.889*
						(0.372)
Second Mover · NOSIGNAL-EXO						0.324
						(0.404)
Second Mover · SIGNAL-ENDO						0.932**
						(0.317)
Constant	0.287***	0.244***	0.225***	0.289***	11.728***	11.728***
	(0.008)	(0.012)	(0.011)	(0.014)	(0.387)	(0.393)
Observations	2400	2400	1765	2400	100	100
R2-overall (R2)	0.222	0.230	0.151	0.102	0.090	0.122

Notes. Columns 1–4: Linear Probability Model with individual fixed effects; an observation is a Second Mover in a round of Part B of Experiment S. Columns 5–6: Ordinary Least Squares; an observation is a participant. Columns 1–2: the dependent variable is a binary indicator equal to 1 if the participant samples option $x \in \{orange, purple\}$ at the first search. Columns 3–4: the dependent variable is a binary indicator equal to 1 if the participant samples the second option after a rational first search (3) or in the entire sample (4). Columns 5–6: the dependent variable is a continuous variable measuring the participant's average round payoff over all rounds of Part B of Experiment S. *Literacy* and $OverConf$ are, respectively, Second Movers' score in the Berlin Test, and the difference between their expected (self-reported) and actual performance in the test. Standard errors, clustered at the matching subgroup level, are in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table C.2: Individual Characteristics – Including Risk Preferences

	First Search		Second Search		Payoff	
	(1)	(2)	(3)	(4)	(5)	(6)
$I(a_1 = purple)$	0.630**					
	(0.148)					
$I(a_1 = purple) \cdot Literacy$	-0.015					
	(0.034)					
$I(a_1 = purple) \cdot OverConf$	-0.078*					
	(0.031)					
$I(a_1 = purple) \cdot Risk$	-0.006					
	(0.011)					
$I(a_1 = orange)$		0.630**				
		(0.148)				
$I(a_1 = orange) \cdot Literacy$		-0.015				
		(0.034)				
$I(a_1 = orange) \cdot OverConf$		-0.078*				
		(0.031)				
$I(a_1 = orange) \cdot Risk$		-0.006				
		(0.011)				
$I(q_{s_2^1} = 9,\, c_2 = 1)$			0.343**	0.376*		
			(0.097)	(0.149)		
$I(q_{s_2^1} = 9,\, c_2 = 1) \cdot Literacy$			0.016	0.006		
			(0.012)	(0.016)		
$I(q_{s_2^1} = 9,\, c_2 = 1) \cdot OverConf$			0.038	0.009		
			(0.045)	(0.058)		
$I(q_{s_2^1} = 9,\, c_2 = 1) \cdot Risk$			0.003	-0.000		
			(0.008)	(0.008)		
Second Movers					0.967*	
					(0.410)	
Second Movers · *Literacy*					-0.059	-0.011
					(0.119)	(0.117)
Second Movers · *OverConf*					-0.273	-0.172
					(0.142)	(0.162)
Second Movers · *Risk*					-0.006	-0.004
					(0.012)	(0.015)
Second Movers · Benchmark						0.767
						(0.438)
Second Movers · NoSignal-Endo						0.946*
						(0.435)
Second Movers · Signal-Exo						0.379
						(0.529)
Second Movers · Signal-Endo						0.982*
						(0.411)
Constant	0.287***	0.244***	0.225***	0.289***	11.728***	11.728***
	(0.008)	(0.011)	(0.011)	(0.014)	(0.389)	(0.396)
Observations	2400	2400	1765	2400	100	100
R2-overall (R2)	0.221	0.230	0.150	0.102	0.091	0.122

Notes. Ibidem. *Risk* is a (survey-based) measure of Second Movers' willingness to take risks.

Table C.3: Individual Characteristics – Alternative Measure of Overconfidence

	First Search		Second Search		Payoff	
	(1)	(2)	(3)	(4)	(5)	(6)
$I(a_1 = purple)$	0.385**					
	(0.102)					
$I(a_1 = purple) \cdot Literacy$	0.042					
	(0.058)					
$I(a_1 = purple) \cdot OverConf$	-0.054					
	(0.046)					
$I(a_1 = orange)$		0.385**				
		(0.102)				
$I(a_1 = orange) \cdot Literacy$		0.042				
		(0.058)				
$I(a_1 = orange) \cdot OverConf$		-0.054				
		(0.046)				
$I(q_{s_2^1} = 9,\, c_2 = 1)$			0.391**	0.346**		
			(0.094)	(0.113)		
$I(q_{s_2^1} = 9,\, c_2 = 1) \cdot Literacy$			0.030	0.027		
			(0.039)	(0.034)		
$I(q_{s_2^1} = 9,\, c_2 = 1) \cdot OverConf$			-0.063	-0.053		
			(0.041)	(0.040)		
Second Movers					0.344	
					(0.551)	
Second Movers · *Literacy*					0.130	0.097
					(0.170)	(0.166)
Second Movers · *OverConf*					-0.166	-0.072
					(0.121)	(0.128)
Second Movers · Benchmark						0.406
						(0.639)
Second Movers · NoSignal-Endo						0.619
						(0.543)
Second Movers · Signal-Exo						-0.069
						(0.566)
Second Movers · Signal-Endo						0.613
						(0.479)
Constant	0.287***	0.244***	0.225***	0.289***	11.728***	11.728***
	(0.010)	(0.015)	(0.011)	(0.014)	(0.387)	(0.393)
Observations	2400	2400	1765	2400	100	100
R2-overall (R2)	0.222	0.228	0.150	0.104	0.066	0.111

Notes. Columns 1–4: Linear Probability Model with individual fixed effects; an observation is a Second Mover in a round of Part B of Experiment S. Columns 5–6: Ordinary Least Squares; an observation is a participant. Columns 1–2: the dependent variable is a binary indicator equal to 1 if the participant samples option $x \in \{orange, purple\}$ at the first search. Columns 3–4: the dependent variable is a binary indicator equal to 1 if the participant samples the second option after a rational first search (3) or in the entire sample (4). Columns 5–6: the dependent variable is a continuous variable measuring the participant's average round payoff over all rounds of Part B of Experiment S. *Literacy* and *OverConf* are, respectively, Second Movers' score in the Berlin Test, and the difference between Second Movers' expected (self-reported) performance and their expectation of others' average performance in the test. Standard errors, clustered at the matching subgroup level, are in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

D Instructions

We translated the following instructions from Italian into English. Sample screenshots of the main decision stage screens, as visualized by the participants, are in Appendix D.3.

D.1 Experiment I

Experiment I: Introduction

Welcome!

This is an experiment on economic decision-making.

For your participation, you will earn a **minimum of 5 Euros**. You may earn extra money, up to a **maximum of 25 Euros**. The final amount of your earnings depends on your decisions in the experiment and on chance. All payments will be made at the end of the experiment via PayPal.

The study will last approximately 30 minutes.

Thank you for your participation!

Today's study consists of three parts:

- We begin by describing the decisions you will make in **Part A**. Next, you must complete a brief comprehension quiz to verify your full understanding of the instructions.
- At the end of Part A, you will receive further instructions about the next decisions you will make and proceed to **Part B**. Again, you must complete a brief comprehension quiz to verify your full understanding of the instructions.
- Finally, you will be asked to complete a **questionnaire**. The questionnaire also contains incentivized questions, which can allow you to earn extra money.

In all three parts of the study, **you will have the opportunity to earn some tokens**. At the end of the study, tokens will be converted to Euros at the rate **3 tokens = 1 Euro**.

You will be paid only if you complete all parts of the study.

Click on the "Next" button to start.

Experiment I: Part A

Part A: Instructions

In the first part of today's study, you will participate in **15 rounds of decision-making**. In each round, you will be asked to make a single decision.

Scenarios

There are 2 "boxes", labeled **"Purple box"** and **"Orange box"**.
Each box contains a certain number of tokens, which determine the value of the box.
Each box may contain 5, 9, or 19 tokens, with the same probability.

Therefore, there are 9 possible scenarios:

Scenario	Purple box	Orange box
1	**5 Tokens**	**5 Tokens**
2	**5 Tokens**	**9 Tokens**
3	**5 Tokens**	**19 Tokens**
4	**9 Tokens**	**5 Tokens**
5	**9 Tokens**	**9 Tokens**
6	**9 Tokens**	**19 Tokens**
7	**19 Tokens**	**5 Tokens**
8	**19 Tokens**	**9 Tokens**
9	**19 Tokens**	**19 Tokens**

At the beginning of each round, the computer will randomly draw one of these scenarios, at random. The 9 scenarios are all equally likely.

Part A: Instructions

Your decision

At the end of each round, you can only keep one of the two boxes: your payoff depends on its content.

At the beginning of each round, both boxes are closed. The only way to learn the number of tokens contained in each box, hence its value, is to open the box.

You will be shown, at no cost, the content of one of the two boxes, randomly selected by the computer. Each box has the same probability of being opened first.

After checking the content of the first box, you will **DECIDE WHETHER TO OPEN THE SECOND BOX.**
Opening the second box has a cost, which can be: **LOW (1 token)** or **HIGH (4 tokens)**.
At the beginning of each round, the computer will randomly determine whether your cost is low or high, with the same probability, as through a fair coin flip. Before choosing whether to open the second box, you will find out if your cost is low or high.

Hence, you will decide whether you prefer:

- **To pay the cost to OPEN the second box and learn about its content**.

  → In this case, you will keep the box containing the greatest number of tokens. Should the two boxes contain the same number of tokens, the computer will randomly select one for you to keep (each box has the same probability of being selected).

- **NOT to pay the cost, GIVING UP THE CHANCE TO OPEN the second box and learn about its content**.

  → In this case, you will keep the first (and only) opened box.

Your **PAYOFF** from the round will be equal to the **number of TOKENS contained in the box you KEEP**, minus any cost you may have paid to open the second box.

Part A: Instructions

Recap of previous rounds

From the second round, at the bottom of your screen, you will see a table summarizing the outcomes of the previous rounds. For each past round, you will be reminded of:

1. The color and the content of the first box;
2. Your cost;
3. The color and content of the second box (only if you decided to open it!);
4. Your payoff from the round.

Earnings: Part A

At the end of today's study, **1 of the 15 rounds** you played in Part A will be **randomly selected**. Each round has the same chance of being selected (1/15).

Your **EARNINGS from Part A will be equal to your PAYOFF in the randomly selected round**. Since you do not know which round will be selected for payment, you will want to do your best in each round!

Part A: Comprehension Questions

Before we begin this study, we ask that you answer some questions to verify your understanding of the instructions.
Please feel free to click the "Back" button to review the instructions if necessary!
Upon completing the quiz, we will provide you with the list of correct answers, and you will proceed to Part A.

1. ***How many decision rounds will you play?***
2. ***Will you be paid for:***
   - *All decision rounds (conversion rate: 3 tokens = 1 Euro)?*
   - *One randomly selected round (conversion rate: 3 tokens = 1 Euro)?*
3. ***At the beginning of each round, both the Purple and the Orange boxes are closed. What is the cost of opening the first box, in tokens?***
4. ***Who/What determines which of the two boxes is opened first?***
   - *I can freely choose which box I want to open first.*
   - *The computer randomly determines which box to open first.*
5. ***What is the cost of opening the second box?***
   - *Opening the second box is costless.*
   - *Opening the second box costs either 1 or 4 tokens; the computer randomly determines (with equal probability) the cost at the beginning of each round.*
   - *Opening the second box costs either 1 or 4 tokens; the computer randomly determines (with equal probability) the cost at the beginning of Part A, and the cost remains the same throughout all rounds.*
6. ***Each of the two boxes, the Purple and the Orange box, can either contain 5, 9, or 19 tokens, with the same probability. Hence, 9 equally likely scenarios are possible. Which of the following statements is correct?***
   - *At the beginning of each round, the computer randomly draws one of the 9 scenarios. The same scenario will have a lower probability of being redrawn in subsequent rounds.*
   - *At the beginning of Part A, the computer randomly draws one of the 9 scenarios, which stays the same throughout the rounds.*
   - *At the beginning of each round, the computer randomly draws one of the 9 scenarios. The same scenario will have the same probability of being redrawn in subsequent rounds.*

Part A: Play

Experiment I: Part B

Part B: Instructions

In the second part of today's study, you will participate in **additional 30 rounds of decision-making**. In each round, you will be asked to make a single decision.

The scenarios and rules of the game are the same as in Part A. Before we get started, we will show you a summary of the instructions.

Scenarios

There are 2 "boxes", labeled **"Purple box"** and **"Orange box"**.
Each box contains a certain number of tokens, which determine the value of the box.
Each box may contain 5, 9, or 19 tokens, with the same probability: hence, as before, there are 9 possible and equally likely scenarios.

As before, at the end of each round, you can only keep one of the two boxes: your payoff depends on its content.

Part B: Instructions

Your decision

At the beginning of each round, both boxes are closed. The only way to learn the number of tokens contained in each box, hence its value, is to open the box.

You will be shown, at no cost, the content of one of the two boxes, randomly selected by the computer. Each box has the same probability of being opened first.

After checking the content of the first box, you will **DECIDE WHETHER TO OPEN THE SECOND BOX.**
Opening the second box has a cost, which can be: **LOW (1 token)** or **HIGH (4 tokens)**.
At the beginning of each round, the computer will randomly determine whether your cost is low or high, with the same probability, as through a fair coin flip. Before choosing whether to open the second box, you will find out if your cost is low or high.

Hence, you will decide whether you prefer:

- **To pay the cost to OPEN the second box and learn about its content**.

  → In this case, you will keep the box containing the greatest number of tokens. Should the two boxes contain the same number of tokens, the computer will randomly select one for you to keep (each box has the same probability of being selected).

- **NOT to pay the cost, GIVING UP THE CHANCE TO OPEN the second box and learn about its content**.

  → In this case, you will keep the first (and only) opened box.

Your **PAYOFF** from the round will be equal to the **number of TOKENS contained in the box you KEEP**, minus any cost you may have paid to open the second box.

Part B: Instructions

Recap of previous rounds

From the second round, at the bottom of your screen, you will see a table summarizing the outcomes of the previous rounds. For each past round, you will be reminded of:

1. The color and the content of the first box;
2. Your cost;
3. The color and content of the second box (only if you decided to open it!);
4. Your payoff from the round.

Earnings: Part B

At the end of today's study, **2 of the 30 rounds** you played in Part B will be **randomly selected**. Each round has the same chance of being selected.

Your **EARNINGS from Part B will be equal to the SUM OF your PAYOFFS in the randomly selected rounds**. Since you do not know which rounds will be selected for payment, you will want to do your best in each round!

Part B: Comprehension Questions

Before we move on to the second part of the study, we ask that you answer some questions to verify your understanding of the instructions.
Please feel free to click the "Back" button to review the instructions if necessary!
Upon completing the quiz, we will provide you with the list of correct answers, and you will proceed to Part B.

1. ***How many decision rounds will you play?***

2. ***Will you be paid for:***
    - *All decision rounds (conversion rate: 3 tokens = 1 Euro)?*
    - *Two randomly selected rounds (conversion rate: 3 tokens = 1 Euro)?*

Part B: Play

Experiment I: Questionnaire

Questionnaire

To complete the final part of this study, we ask you to respond to a short questionnaire.

The questionnaire also contains some incentivized questions. These questions involve additional monetary rewards that can allow you to earn extra money. We will flag you in red when you are shown one of the incentivized questions.

As in Parts A and B, your earnings (if any) from incentivized questions will be expressed in tokens. Recall that, at the end of the study, tokens will be converted to Euros at the rate **3 tokens = 1 Euro**.

Questionnaire

D.2 Experiment S

Experiment S: Introduction

Welcome!

This is an experiment on economic decision-making.

For your participation, you will earn a **minimum of 5 Euros**. You may earn extra money, up to a **maximum of 25 Euros**. The final amount of your earnings depends on your decisions in the experiment and on chance. All payments will be made at the end of the experiment via PayPal.

The study will last approximately 30 minutes.

Thank you for your participation!

Today's study consists of three parts:

- We begin by describing the decisions you will make in **Part A**. Next, you must complete a brief comprehension quiz to verify your full understanding of the instructions.
- At the end of Part A, you will receive further instructions about the next decisions you will make and proceed to **Part B**. Again, you must complete a brief comprehension quiz to verify your full understanding of the instructions.
- Finally, you will be asked to complete a **questionnaire**. The questionnaire also contains incentivized questions, which can allow you to earn extra money.

In all three parts of the study, **you will have the opportunity to earn some tokens**. At the end of the study, tokens will be converted to Euros at the rate **3 tokens = 1 Euro**.

You will be paid only if you complete all parts of the study.

Click on the "Next" button to start.

Experiment S: Part A

Part A: Instructions

In the first part of today's study, you will participate in **15 rounds of decision-making**. In each round, you will be asked to make a single decision.

Scenarios

There are 2 "boxes", labeled **"Purple box"** and **"Orange box"**.
Each box contains a certain number of tokens, which determine the value of the box.
Each box may contain 5, 9, or 19 tokens, with the same probability.

Therefore, there are 9 possible scenarios:

Scenario	Purple box	Orange box
1	5 Tokens	5 Tokens
2	5 Tokens	9 Tokens
3	5 Tokens	19 Tokens
4	9 Tokens	5 Tokens
5	9 Tokens	9 Tokens
6	9 Tokens	19 Tokens
7	19 Tokens	5 Tokens
8	19 Tokens	9 Tokens
9	19 Tokens	19 Tokens

At the beginning of each round, the computer will randomly draw one of these scenarios, at random. The 9 scenarios are all equally likely.

Part A: Instructions

Your decision

At the end of each round, you can only keep one of the two boxes: your payoff depends on its content.

At the beginning of each round, both boxes are closed. The only way to learn the number of tokens contained in each box, hence its value, is to open the box.

You will be shown, at no cost, the content of one of the two boxes, randomly selected by the computer. Each box has the same probability of being opened first.

After checking the content of the first box, you will **DECIDE WHETHER TO OPEN THE SECOND BOX.**

Opening the second box has a cost, which can be: **LOW (1 token)** or **HIGH (4 tokens)**.

At the beginning of each round, the computer will randomly determine whether your cost is low or high, with the same probability, as through a fair coin flip. Before choosing whether to open the second box, you will find out if your cost is low or high.

Hence, you will decide whether you prefer:

- **To pay the cost to OPEN the second box and learn about its content**.

  → In this case, you will keep the box containing the greatest number of tokens. Should the two boxes contain the same number of tokens, the computer will randomly select one for you to keep (each box has the same probability of being selected).

- **NOT to pay the cost, GIVING UP THE CHANCE TO OPEN the second box and learn about its content**.

  → In this case, you will keep the first (and only) opened box.

Your **PAYOFF** from the round will be equal to the **number of TOKENS contained in the box you KEEP**, minus any cost you may have paid to open the second box.

Part A: Instructions

Recap of previous rounds

From the second round, at the bottom of your screen, you will see a table summarizing the outcomes of the previous rounds. For each past round, you will be reminded of:

1. The color and the content of the first box;
2. Your cost;
3. The color and content of the second box (only if you decided to open it!);
4. Your payoff from the round.

Earnings: Part A

At the end of today's study, **1 of the 15 rounds** you played in Part A will be **randomly selected**. Each round has the same chance of being selected (1/15).

Your **EARNINGS from Part A will be equal to your PAYOFF in the randomly selected round**. Since you do not know which round will be selected for payment, you will want to do your best in each round!

Part A: Comprehension Questions

Before we begin this study, we ask that you answer some questions to verify your understanding of the instructions.
Please feel free to click the "Back" button to review the instructions if necessary!
Upon completing the quiz, we will provide you with the list of correct answers, and you will proceed to Part A.

1. ***How many decision rounds will you play?***
2. ***Will you be paid for:***
    - *All decision rounds (conversion rate: 3 tokens = 1 Euro)?*
    - *One randomly selected round (conversion rate: 3 tokens = 1 Euro)?*
3. ***At the beginning of each round, both the Purple and the Orange boxes are closed. What is the cost of opening the first box, in tokens?***
4. ***Who/What determines which of the two boxes is opened first?***
    - *I can freely choose which box I want to open first.*
    - *The computer randomly determines which box to open first.*
5. ***What is the cost of opening the second box?***
    - *Opening the second box is costless.*
    - *Opening the second box costs either 1 or 4 tokens; the computer randomly determines (with equal probability) the cost at the beginning of each round.*
    - *Opening the second box costs either 1 or 4 tokens; the computer randomly determines (with equal probability) the cost at the beginning of Part A, and the cost remains the same throughout all rounds.*
6. ***Each of the two boxes, the Purple and the Orange box, can either contain 5, 9, or 19 tokens, with the same probability. Hence, 9 equally likely scenarios are possible. Which of the following statements is correct?***
    - *At the beginning of each round, the computer randomly draws one of the 9 scenarios. The same scenario will have a lower probability of being redrawn in subsequent rounds.*
    - *At the beginning of Part A, the computer randomly draws one of the 9 scenarios, which stays the same throughout the rounds.*
    - *At the beginning of each round, the computer randomly draws one of the 9 scenarios. The same scenario will have the same probability of being redrawn in subsequent rounds.*

Part A: Play

Experiment S: Part B

Part B: Instructions

T1 & T3 (Exogenous Matching Treatments)

In the second part of today's study, you will participate in **additional 30 rounds of decision-making**. Unlike Part A, in each round of this part of the study, you will be asked to make two decisions.

T2 & T4 (Endogenous Matching Treatments)

In the second part of today's session, you will participate in **additional 30 rounds of decision-making**. Unlike Part A, in each round of this part of the study, you will be asked to make three decisions.

Part B: Instructions

Scenarios

There are 2 "boxes", labeled **"Purple box"** and **"Orange box"**.
Each box contains a certain number of tokens, which determine the value of the box.
Each box may contain 5, 9, or 19 tokens, with the same probability: hence, as before, there are 9 possible and equally likely scenarios.

Unlike Part A, before making your decisions, we will provide you with some **additional information about the decisions made by another player**.

Part B: Instructions

Information

Before you, **4 other players (♠ ♣ ♥ ♦) participated in this same study and had in front of them the very same sequence of scenarios as you.**
These 4 players always played according to the rules of Part A, also in Part B: at the

beginning of each round, the computer randomly selected which box to open first, at no cost, with each box having the same probability of being selected.

T1 (Exogenous Matching & No Signal)

At the beginning of each new round, **you will be randomly matched with one of these 4 players (♠ ♣ ♥ ♦), and you will learn which box (Purple vs. Orange) was kept by this player**.
The labels assigned to the 4 players are constant over rounds: you can identify when you are matched again with the same player. The probability of being re-matched with the same player in the next round is 1 out of 4, hence 25%.

T2 (Endogenous Matching & No Signal)

At the beginning of each new round, you will have the **chance to choose which of 4 players (♠ ♣ ♥ ♦) you want to match with, and you will learn which box (Purple vs. Orange) was kept by this player**.
The labels assigned to the 4 players are constant over rounds: you can choose whether to match again with the same player over rounds or not.

T3 (Exogenous Matching & Signal)

At the beginning of each new round, **you will be randomly matched with one of these 4 players (♠ ♣ ♥ ♦), and you will learn:**

- **Which box (Purple vs. Orange) was kept by this player;**
- **The ranking of each player, based on the cumulative payoffs in Part A.**

The labels assigned to the 4 players are constant over rounds: you can identify when you are matched again with the same player. The probability of being re-matched with the same player in the next round is 1 out of 4, hence 25%.

T4 (Endogenous Matching & Signal)

At the beginning of each new round, you will have the **chance to choose which of 4 players (♠ ♣ ♥ ♦) you want to match with, and you will learn:**

- **Which box (Purple vs. Orange) was kept by this player;**
- **The ranking of each player, based on the cumulative payoffs in Part A.**

The labels assigned to the 4 players are constant over rounds: you can choose whether to match again with the same player over rounds or not.

Although the content of each box you will have in front of you is the same as the content that the other 4 players (♠ ♣ ♥ ♦) had in front of them, **the cost of opening the second box is randomly drawn for each player at the beginning of each round**. Hence, you will learn which box your matched player kept, but you will not know:

- What the cost of the matched player to open the second box was;
- Whether the matched player opened the second box.

Part B: Instructions

Your decisions

As before, at the end of each round, you can only keep one of the two boxes: your payoff depends on its content.

T1 & T3 (Exogenous Matching Treatments)

Decision 1

Unlike Part A, you can now **CHOOSE WHICH BOX (Purple vs. Orange) TO OPEN FIRST**.
Opening the first box is costless.

Decision 2

After checking the content of the first box, you will **DECIDE WHETHER TO OPEN THE SECOND BOX.**
Opening the second box has a cost, which can be: **LOW (1 token)** or **HIGH (4 tokens)**.
At the beginning of each round, the computer will randomly determine whether your cost is low or high, with the same probability. Recall that your cost may be different from the one faced by your matched player (who played before you)!

Hence, you will decide:

1. **WHICH BOX TO OPEN FIRST, at no cost**;
2. **WHETHER TO PAY THE COST TO OPEN THE SECOND BOX**.

As in Part A, if you decide:

- **To pay the cost to OPEN the second box** and learn about its content,
  → You will keep the box containing the greatest number of tokens. Should the two boxes contain the same number of tokens, the computer will randomly select one for you to keep (each box has the same probability of being selected).
- **NOT to pay the cost, GIVING UP THE CHANCE TO OPEN the second box and learn about its content,**
  → You will keep the first (and only) opened box.

Your **PAYOFF** from the round will be equal to the **number of TOKENS contained in the box you KEEP**, minus any cost you may have paid to open the second box.

T2 & T4 (Endogenous Matching Treatments)

Decision 1
You will be asked to choose **WHICH OF THE 4 PLAYERS (♠ ♣ ♥ ♦) YOU WANT TO MATCH WITH**.
Accordingly, you will learn which box your matched player kept in the same round.

Decision 2
Unlike Part A, you can now **CHOOSE WHICH BOX (Purple vs. Orange) TO OPEN FIRST**.
Opening the first box is costless.

Decision 3
After checking the content of the first box, you will **DECIDE WHETHER TO OPEN THE SECOND BOX.**
Opening the second box has a cost, which can be: **LOW (1 token)** or **HIGH (4 tokens)**.
At the beginning of each round, the computer will randomly determine whether your cost is low or high, with the same probability. Recall that your cost may be different from the one faced by your matched player (who played before you)!

Hence, you will decide:

1. **WHICH PLAYER YOU WANT TO MATCH WITH**;
2. **WHICH BOX TO OPEN FIRST, at no cost**;
3. **WHETHER TO PAY THE COST TO OPEN THE SECOND BOX**.

As in Part A, if you decide:

- **To pay the cost to OPEN the second box** and learn about its content,
  → You will keep the box containing the greatest number of tokens. Should the two boxes contain the same number of tokens, the computer will randomly select one for you to keep (each box has the same probability of being selected).
- **NOT to pay the cost, GIVING UP THE CHANCE TO OPEN the second box and learn about its content,**
  → You will keep the first (and only) opened box.

Your **PAYOFF** from the round will be equal to the **number of TOKENS contained in the box you KEEP**, minus any cost you may have paid to open the second box.

Part B: Instructions

Recap of previous rounds

From the second round, at the bottom of your screen, you will see a table summarizing the outcomes of the previous rounds. For each past round, you will be reminded of:

1. The color and the content of the first box;
2. Your cost;
3. The color and content of the second box (only if you decided to open it!);
4. Your payoff from the round;
5. The symbol associated with your matched player and the color of the box she kept.

Earnings: Part B

At the end of today's study, **2 of the 30 rounds** you played in Part B will be **randomly selected**. Each round has the same chance of being selected.

Your **EARNINGS from Part B will be equal to the SUM OF your PAYOFFS in the randomly selected rounds**. Since you do not know which rounds will be selected for payment, you will want to do your best in each round!

Part B: Comprehension Questions

Before we move on to the second part of the study, we ask that you answer some questions to verify your understanding of the instructions.
Please feel free to click the "Back" button to review the instructions if necessary!
Upon completing the quiz, we will provide you with the list of correct answers, and you will proceed to Part B.

1. ***How many decision rounds will you play?***
2. ***Will you be paid for:***
    - *All decision rounds (conversion rate: 3 tokens = 1 Euro)?*
    - *One randomly selected round (conversion rate: 3 tokens = 1 Euro)?*
    - *Two randomly selected rounds (conversion rate: 3 tokens = 1 Euro)?*
3. ***In each round, how many decisions will you take?***
    - *Only one decision.*
    - *Two decisions.*
    - *Three decisions.*

4. ***Which decision(s) will you make?***
   - *Which player to match with.*
   - *Which box to be opened first (at no cost).*
   - *Whether to open the second box (paying the cost).*
   - *All three decisions listed above.*
   - *Which player to match with and which box to open first.*
   - *Which player to match with and whether to open the second box,*
   - *Which box to open first and whether to open the second box.*
5. ***At the beginning of each round, you will be matched with one of the 4 players who already played, and you will learn which box this player kept in the same round:***
   - *Facing boxes with the same content as yours and the same cost (to open the second box) as you.*
   - *Facing boxes with the same content as yours but a different cost to open the second box.*
   - *Facing boxes with the same content as yours but not necessarily the same cost to open the second box.*

Part B: Play

Experiment S: Questionnaire

Questionnaire

o complete the final part of this study, we ask you to respond to a short questionnaire.

The questionnaire also contains some incentivized questions. These questions involve additional monetary rewards that can allow you to earn extra money. We will flag you in red when you are shown one of the incentivized questions.

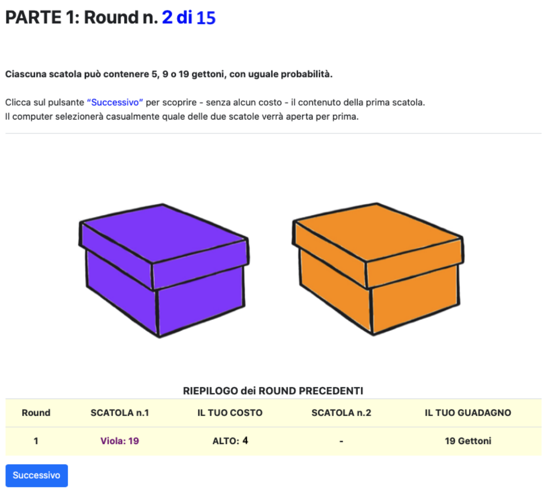
As in Parts A and B, your earnings (if any) from incentivized questions will be expressed in tokens. Recall that, at the end of the study, tokens will be converted to Euros at the rate **3 tokens = 1 Euro**.

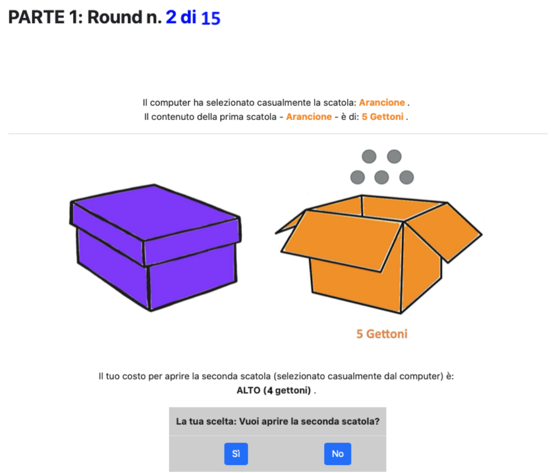
Questionnaire

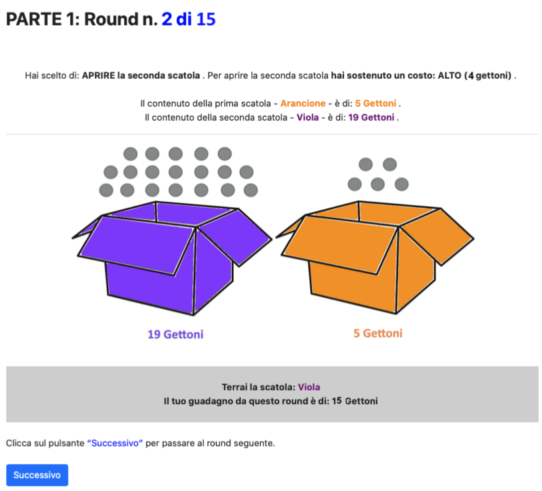
D.3 Screenshots

The experiment was programmed on oTree (Chen, Schonger, and Wickens, 2016). As an illustrative example, we report the screenshots of the main decision stage screens visualized by participants. Screenshots are from the original version of the software (in Italian).

D.3.1 Decision-Making in Isolation

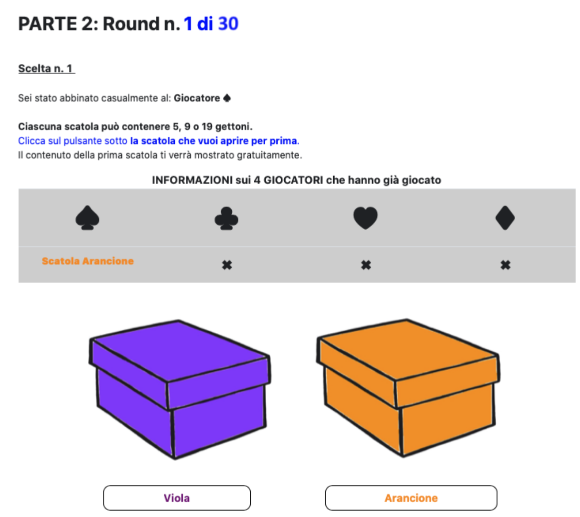


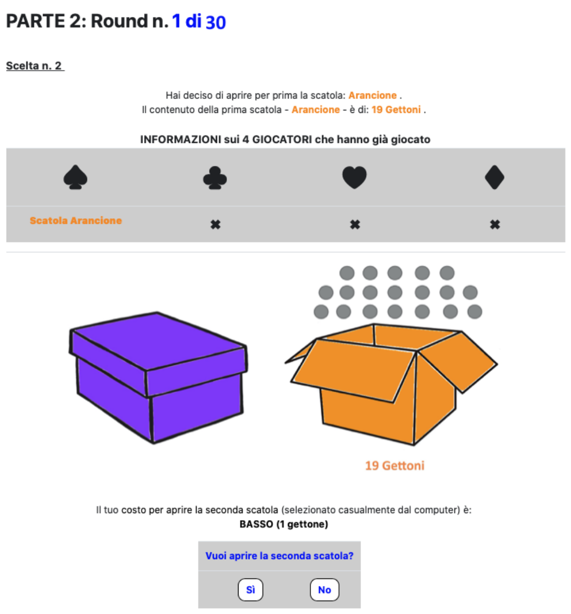


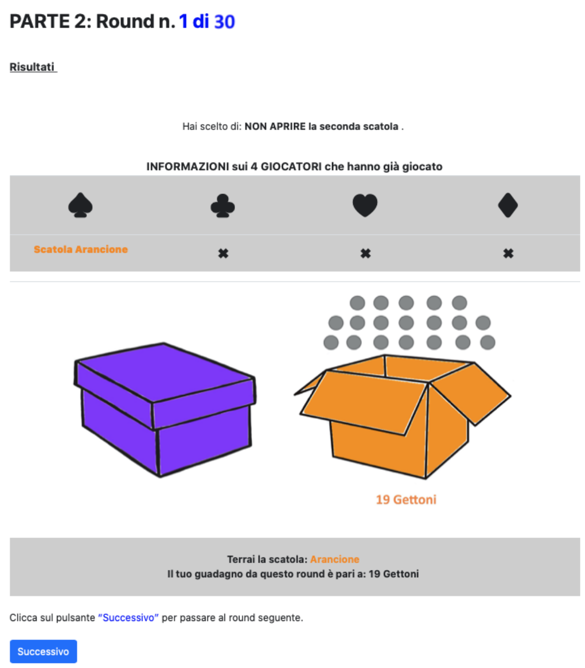


D.3.2 Decision-Making with Social Information

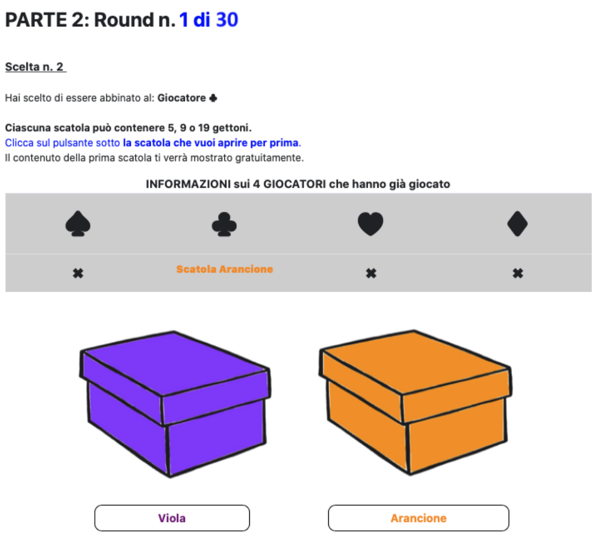
Benchmark (No Signal, Exogenous Matching)

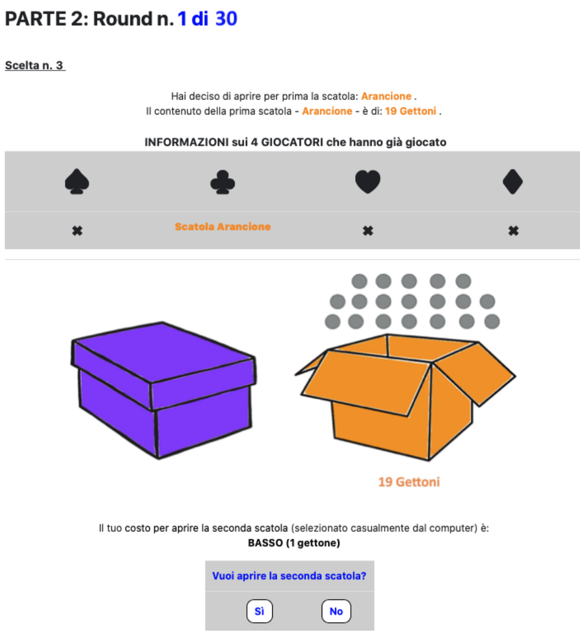


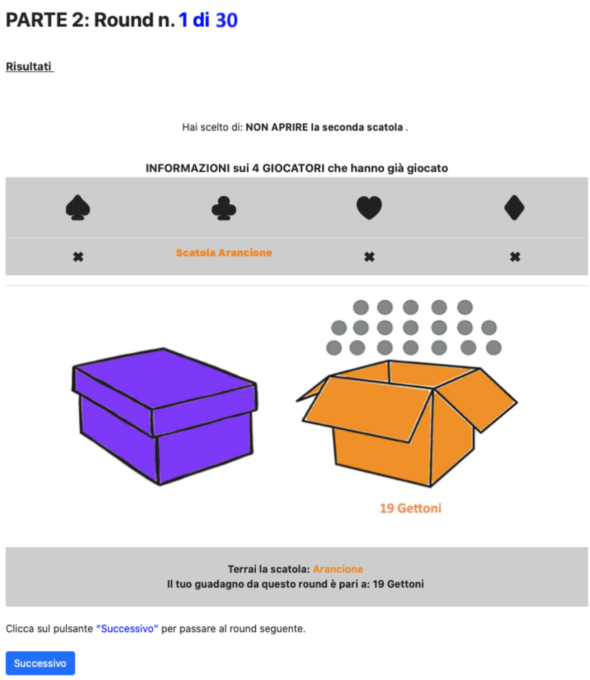




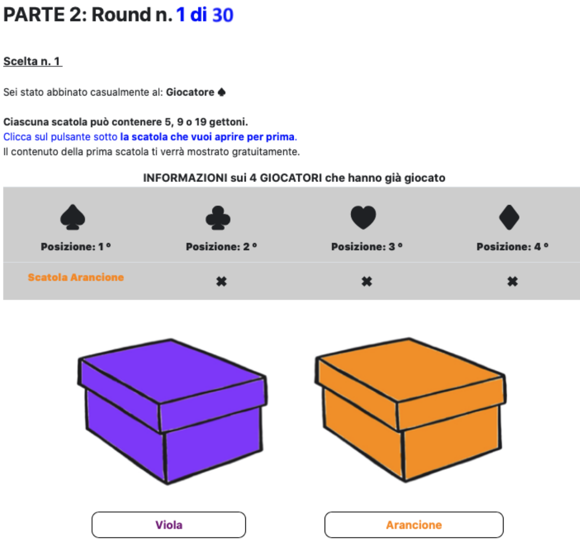

PARTE 2: Round n. 1 di 30
Scelta n. 1
Clicca sul pulsante "Scopri" sotto al simbolo del giocatore a cui vuoi essere abbinato.
Scoprirai così quale scatola ha tenuto quel giocatore in questo round.
INFORMAZIONI sui 4 GIOCATORI che hanno già giocato
"Scopri"
"Scopri"
"Scopri"
"Scopri"

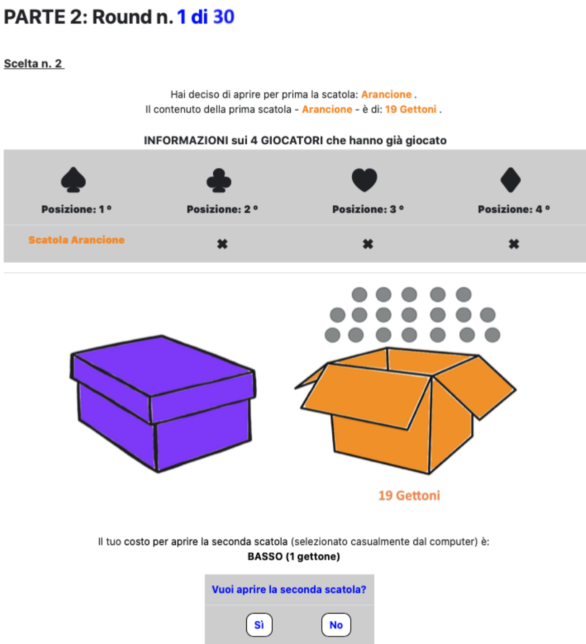

PARTE 2: Round n. 1 di 30
Scelta n. 2
Hai scelto di essere abbinato al: Giocatore ♣
Ciascuna scatola può contenere 5, 9 o 19 gettoni.
Clicca sul pulsante sotto la scatola che vuoi aprire per prima.
Il contenuto della prima scatola ti verrà mostrato gratuitamente.
INFORMAZIONI sui 4 GIOCATORI che hanno già giocato
Scatola Arancione
Viola
Arancione

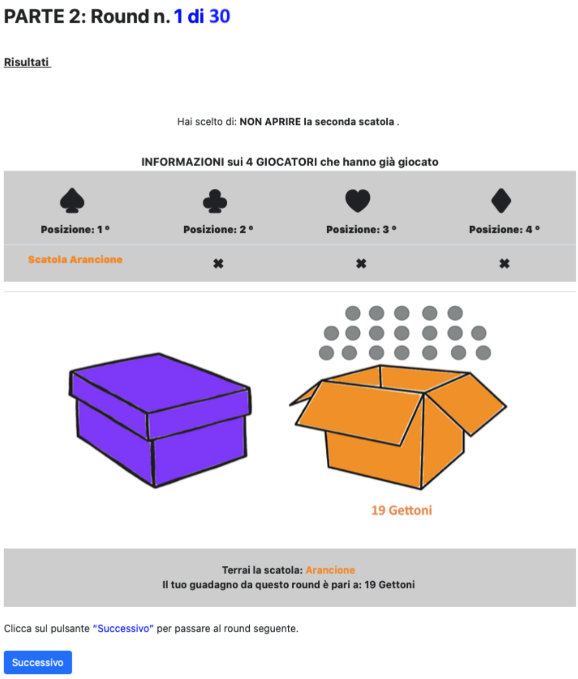

PARTE 2: Round n. 1 di 30
Scelta n. 3
Hai deciso di aprire per prima la scatola: Arancione .
Il contenuto della prima scatola - Arancione - è di: 19 Gettoni .
INFORMAZIONI sui 4 GIOCATORI che hanno già giocato
Scatola Arancione
19 Gettoni
Il tuo costo per aprire la seconda scatola (selezionato casualmente dal computer) è:
BASSO (1 gettone)
Vuoi aprire la seconda scatola?
Sì
No


PARTE 2: Round n. 1 di 30
Risultati
Hai scelto di: NON APRIRE la seconda scatola .
INFORMAZIONI sui 4 GIOCATORI che hanno già giocato
Scatola Arancione
19 Gettoni
Terrai la scatola: Arancione
Il tuo guadagno da questo round è pari a: 19 Gettoni
Clicca sul pulsante "Successivo" per passare al round seguente.
Successivo

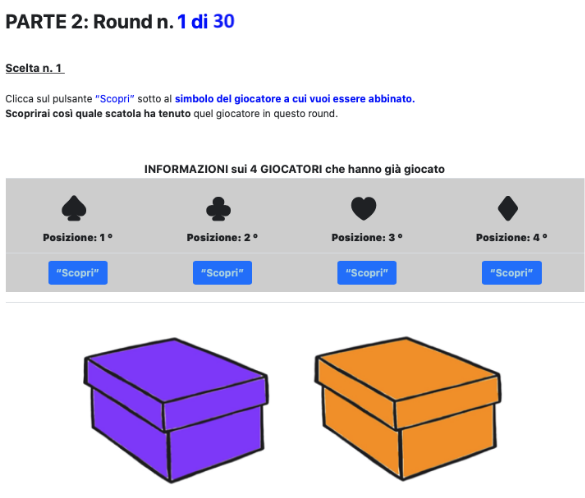
Sig-Exo (Signal, Exogenous Matching)

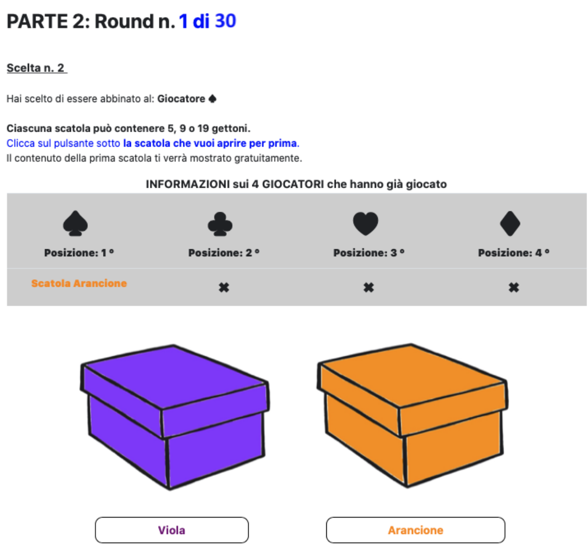


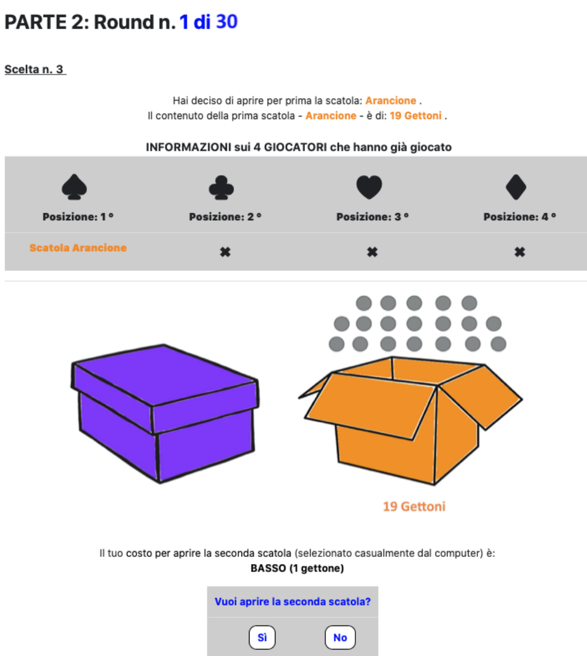


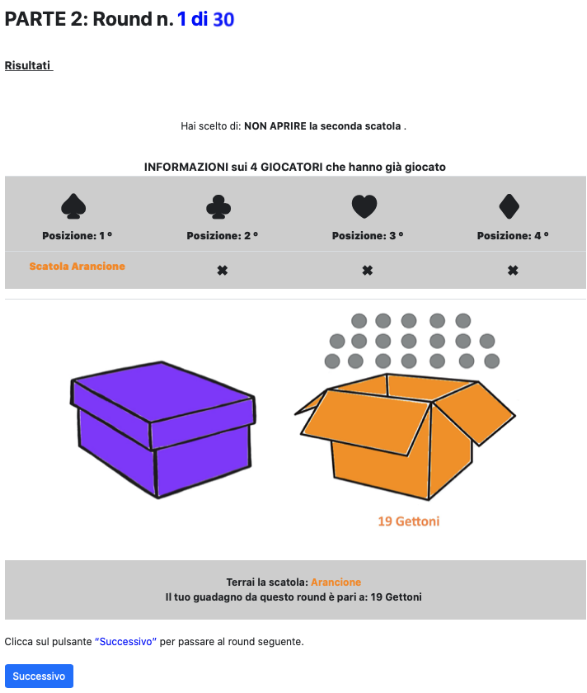


Sig-Endo (Signal, Endogenous Matching)









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