Everyday econometricians: Selection neglect and overoptimism when learning from others

Kai Barron, Steffen Huck, Philippe Jehiel

WORKING PAPER N° 2023 – 24

Everyday econometricians: Selection neglect and overoptimism when learning from others

Kai Barron
Steffen Huck
Philippe Jehiel

JEL Codes: C11, C90, D80, D83.
Keywords: Selection neglect, beliefs, overoptimism, survivorship bias, experiment.

Everyday econometricians: Selection neglect and overoptimism when learning from others*

Kai Barron[†], Steffen Huck[‡], Philippe Jehiel[§]

June 26, 2023

Abstract

This study explores selection neglect in an experimental investment game where individuals can learn from others' outcomes. Experiment 1 examines aggregate-level equilibrium behavior. We find strong evidence of selection neglect and corroborate several comparative static predictions of Jehiel's (2018) model, showing that the severity of the bias is aggravated by the sophistication of other individuals and moderated when information is more correlated across individuals. Experiment 2 focuses on individual decision-making, isolating the influence of beliefs from possible confounding factors. This allows us to classify individuals according to their degree of naivety and explore the limits of, and potential remedies for, selection neglect.

JEL Codes: C11, C90, D80, D83

Keywords: Selection neglect, beliefs, overoptimism, survivorship bias, experiment.

*The authors would like to thank Philipp Albert, Antonio Cabrales, Alexander Coutts, Daniel Friedman, Tilman Fries, Thomas Graeber, Simas Kucinskas, Andrew Schotter, Georg Weizsäcker, Florian Zimmermann, the participants at the Lisbon Game Theory Meeting and ECBE, ESA, MBEES and Nordic conferences, as well as numerous colleagues at the WZB for helpful comments and suggestions. Furthermore, we thank Markus Konrad for invaluable help with programming in o-Tree, and Nina Bonge for excellent support in running the experiments. Jehiel thanks the European Research Council (ERC) for funding (grant agreement No 742816). Barron gratefully acknowledges financial support from the German Science Foundation via CRC TRR 190 (project number 280092119).

[†]WZB Berlin: kai.barron@wzb.eu.

[‡]University College London and WZB Berlin: steffen.huck@wzb.eu.

[§]University College London and Paris School of Economics: jehiel@enpc.fr.

1 Introduction

Many important decisions are made infrequently over the course of a lifetime. These decisions permit little practice or experimentation and are costly to reverse. Such decisions include selecting a college, choosing a career path, making home-purchasing decisions, deciding whether to get married, and considering whether to have a child. A natural way to make these decisions is to look at the experiences of others who have made similar decisions in the past. However, this approach poses a fundamental challenge: The information about the outcomes associated with choosing one particular option in such choices is available exclusively for those individuals who chose that option. For instance, only those who decided to have children can tell us about how happy their lives are with children.

The econometrics literature on selection has demonstrated that the average outcomes of individuals who have made a specific decision can differ substantially from the expected outcome of the average individual in the entire population were she to make the same decision (see, for example, Heckman, 1979, 1990).[1] As a consequence, learning from the outcomes of others requires skillful interpretation of selected data sets. Neglecting this selection can lead to suboptimal decision making.

In this paper, we study two experiments that are designed to explore investment decisions in the presence of selected data. Using Jehiel's (2018) model as a guide, we design an investment game where individuals make a series of investment decisions. When making these decisions, they have access to a private signal and feedback about the outcomes of others who faced the same investment decision in the past. Mirroring real life, the available historical data set only contains the outcomes of others who chose to invest and is, thus, heavily shaped by selection. If individuals do not account for the selection, they will behave as if they are overoptimistic since the projects that are invested in tend to be better than the average project.

Our laboratory experiment ("Experiment 1") focuses on testing the model's equilibrium predictions and comparative statics. In this experiment, participants are given access to historical data collected from the outcomes of others' decisions. In addition, they are provided with a full description of the data-generating process such that, in principle, they could avoid the pitfalls of selection neglect. We find, however, that they draw heavily on the past data and fall into the trap of failing to account for the influence of selection. Consequently, they form biased beliefs, displaying overoptimism in their investment choices as predicted

[1]The literature in econometrics generally discusses this problem from the perspective of the econometrician conducting statistical analysis. However, we are all 'everyday econometricians' when we use the information that we receive in our daily lives to draw inferences and guide our decision-making. The objective of this paper is to explore the implications of selection neglect for the decision-making of the average person.

by Jehiel (2018).[2]

In addition, Experiment 1 demonstrates that the degree of overoptimism displayed varies systematically with the sophistication levels in the population and the heterogeneity in evaluating projects. We show that when evaluations are highly correlated (that is, when the current investor is able to recognize the features of a past project that induced investment by the past investor), the bias disappears. In contrast, when sophistication increases in the population (in our experiment: when one in three human players is replaced by a fully unbiased computer player), the bias is exacerbated for naive individuals. The sophisticated individuals exert a negative *information externality* on the naive players. All of these patterns in behavior are in line with the theoretical predictions generated by selection neglect.

Our online experiment ("Experiment 2") focuses on individual behavior in a one-shot environment. Here, the historical outcomes database is populated using a separate sample of individuals who participated in an earlier session of the experiment. This is akin to learning from the outcomes of a previous generation of decision-makers. Using a within-subject design, we compare investment choices where participants form beliefs about the probabilities of each outcome with near-identical lottery choices with known probabilities. Since the features and presentation of the investment and lottery tasks are identical, aside from the information provided about the probabilities of the outcomes, the only channel through which behavior may differ is, hence, via beliefs about these probabilities. Therefore, we can strip away possible confounds, such as risk preferences or auxiliary features of the choice architecture, and cleanly isolate the role of beliefs shaped by selection neglect.

Doing this allows us to achieve three objectives. First, it provides a robustness check for selection neglect in a simplified setting without strategic interactions. Second, Experiment 2 is designed to allow us to classify individual participants according to the degree of naivety that they display in neglecting selection. We achieve this by constructing two lottery tasks: (i) the *Bayesian lottery*, where the probabilities mirror the beliefs that a fully Bayesian individual would hold in the investment task, and (ii) the *Naive lottery*, where the probabilities mirror the beliefs of an individual who entirely neglects selection and naively extrapolates from the available data in the investment task. We can, therefore, compare the behavior of a given individual across the three tasks—if her behavior in the investment task is similar to that in the Bayesian lottery, then she is classified as Bayesian; as it moves closer to her behavior in the Naive lottery, she is classified as being more naive. Doing this allows us to place every individual on a *Bayesian-naive beliefs* spectrum. Third, the experiment allows us to explore the boundaries of naively extrapolating from selected data.

We document three main results in Experiment 2. First, we find strong evidence that the

[2]There has been ample evidence showing that entrepreneurs exhibit an overoptimism bias regarding the chances of their business succeeding (see, e.g., Cooper et al., 1988; Malmendier and Tate, 2005; Koellinger et al., 2007; Dawson et al., 2014). While such evidence is sometimes used to suggest that entrepreneurs may have a hard-wired form of overconfidence or that the bias is the result of a motivated cognition process, Jehiel (2018) shows that overoptimism can be the mere consequence of selection neglect.

average participant displays naivety, neglecting the influence of selection in the data they observe. This is a robust finding across all the scenarios that we consider, providing support for the key psychological mechanism in play in Experiment 1. Second, our individual classification exercise shows that an overwhelming majority of participants can be classified as being at least partially naive. Third, we show that the manifestation of naivety depends on the complexity of the underlying data-generating process (DGP)—as the DGP becomes less complex, individuals are less likely to naively extrapolate from the data they observe. However, even in our treatment with a less complex DGP, we still observe a substantial amount of naivety, suggesting that it is a persistent phenomenon that is likely to be influencing the formation of beliefs in many everyday scenarios where individuals learn from others.

The remainder of the paper is organized as follows. Section 2 summarizes the related literature, and Section 3 provides the theoretical framework. Section 4 presents Experiment 1, Section 5 discusses Experiment 2, and Section 6 concludes.

2 Related Literature

This paper contributes to the fast-growing literature studying biases in reasoning and belief formation in two ways. First, it provides a new perspective on a bias that has received considerable attention in the literature, namely *overconfidence*.[3] This literature examining overconfidence can be divided into two broad strands, (i) *"wired-in" cognitive mistakes* (see, e.g., Malmendier and Tate (2005); Koellinger et al. (2007); Puri and Robinson (2007); Moore and Healy (2008); Logg et al. (2018)), and, (ii) *motivated beliefs and reasoning* (see, e.g., Compte and Postlewaite (2004); Von Hippel and Trivers (2011); Eil and Rao (2011); Schwardmann and Van der Weele (2019); Zimmermann (2020); Möbius et al. (2022)). The current paper differs from both of these strands in that it demonstrates how a distinct cognitive statistical bias, selection neglect, can generate behavior that looks like overconfidence in certain situations.[4] Importantly, the theory provides a clear description of the contextual factors that should aggravate or mitigate the bias in beliefs. This is tested in the experiments.

Second, we contribute to the experimental literature studying boundedly rational information processing (Samuelson and Bazerman, 1985; Eyster and Weizsacker, 2010; Eyster

[3]The bias that we consider is distinct from overconfidence in that individuals do not form upward-biased beliefs about their own ability, however, it may often look like overconfidence. In our setting, individuals end up holding overoptimistic beliefs about the outcomes of their choices. This is similarly the case for overconfident individuals, who also form overoptimistic beliefs about the outcomes emanating from their actions. This overoptimism about the outcomes of actions (as opposed to generalized overoptimism) connects our work to the overconfidence literature.

[4]In this, we join a small body of work that studies how one cognitive bias may be a root cause that also manifests as another commonly observed bias. For example, Bénabou and Tirole (2002) consider a scenario in which an agent combats the imperfect willpower (present bias) of her future self by manufacturing an inflated self-confidence. Her overconfident beliefs serve a motivational role. In another contribution to this class of studies, Gödker et al. (2023) posit that the disposition effect may generate overconfidence. The authors present evidence in support of this hypothesis in an investment context.

et al., 2015; Martínez-Marquina et al., 2019; Guarino et al., 2021; Ngangoué and Weizsäcker, 2021; Barron and Fries, 2023). Several recent contributions to this literature study how individuals mistakenly draw inferences and form beliefs when observing statistical data with a specific structure. In this class of *statistical biases*, individuals typically make a systematic mistake in forming their mental representation of the DGP—i.e., their (subjective) mental model differs from the true statistical model of the world. For instance, Enke and Zimmermann (2019) examine belief formation by individuals who observe correlated signals. Their research reveals a tendency among individuals to mistakenly "double-count" the same information, a fallacy referred to as correlation neglect. Jin et al. (2021) examine whether individuals accurately interpret the "absence" of a signal, while Graeber (2023) explores the errors in belief formation that may arise from individuals' failure to account for the inherent noise in the information structure.[5]

The two experimental papers closest to ours—Enke (2020) and Esponda and Vespa (2018)—study how individuals draw inferences when they observe data that is only observed conditional on satisfying a specific criterion. Consistent with the findings of our study, both find that people frequently overlook the need to condition on the selection rule. Experimental participants tend to misinterpret the observed data as being representative of the unconditional distribution, thereby displaying *selection neglect*.[6] However, both Enke (2020) and Esponda and Vespa (2018) focus on distinctly different environments in comparison to our study. Enke (2020) investigates "echo chambers," while Esponda and Vespa (2018) examine a voting environment where hypothetical thinking is important. Unlike our study, neither explores the intricate relationship between selection neglect and overoptimism, mediated by learning from others' outcomes.

There has also been a recent interest in examining the implications of selection neglect in more applied work. For instance, Feld et al. (2022) study the implications of selection neglect for recruitment procedures within a profession typically dominated by men. The study theorizes that if the selection into the applicant pool is overlooked by employers, they may resort to generalized gender stereotypes, which, while possibly statistically accurate for the broader population, may not apply to their specific applicant pool. In another application to labor markets, Backhaus et al. (2023) study the implications of selection neglect for workers' wage expectations. They analyze how workers form beliefs regarding the wage penalty for part-time work using information about the average hourly wage of full-time

[5]In another noteworthy contribution to this literature, Araujo et al. (2021) study learning in a setting where optimal behavior requires conditioning on the expected past behavior of other participants. In their setting, taking into account others' choices is necessary to anticipate the worsening adverse selection over time. Araujo et al. (2021) find that a large fraction of participants neglect to condition properly, resulting in sub-optimal decisions.

[6]In earlier work, Koehler and Mercer (2009) study inference about mutual funds when companies selectively advertise only their best-performing funds. The authors provide evidence that both experts and novice investors fail to take into account this selective advertising, using the term *selection neglect* to describe the phenomenon.

employees and that of part-time employees. Both studies find evidence of selection neglect, with considerable implications for life-altering decisions.

From a theoretical standpoint, the experiment in this paper is analyzed through the lens of the theoretical framework presented in Jehiel (2018). This framework forms part of a growing body of work studying coarse reasoning (see Jehiel, 2005; Eyster and Rabin, 2005; Jehiel and Koessler, 2008; Esponda, 2008; Spiegler, 2011).[7] Applying some of these ideas to specific important domains, Brundage et al. (2022) explore the implications of selection neglect for the formation of political beliefs from non-representative samples, while Hübert et al. (2023) study the case of the police drawing inference from crime statistics, but neglecting to properly account for the fact that more crime will be detected in communities where policing is heavier.

3 Theoretical Framework

Drawing on Jehiel's (2018) model, we consider the investment problems of a population of risk-neutral agents where each investor i faces a new project and must decide whether to *invest* or *not invest*. Investors face projects individually, with each investor facing a different project. Projects are ex-ante identical, and the cost of investing in a project is $c > 0$. The return of a project, x, is a binary random variable. Either the project is successful, $x = \bar{x} > c$, or the project is unsuccessful, $x = \underline{x} = 0 < c$. The discussion of the theory mirrors the structure used in our experimental design.

When an investor faces a new project, she knows the cost of the project, but she doesn't know whether the project will be successful or unsuccessful. Prior to making the investment decision, she receives a private, informative signal about the project. One can think of this as the investor's private, informative *impression* about the project's *ex-ante* likely success.[8] (Note, we will use the terms "signal" and "impression" interchangeably.) Given this signal, the investor must decide whether to invest or not.

To account for the possibility that different agents may look at different characteristics of projects when forming their impressions, we model the statistical link between signals and success in the following way.

The success x of a project is fully determined by a triple (s_A, s_B, s_C) where s_A, s_B and s_C are the realizations of independent discrete uniform distributions on $\{1, \ldots 10\}$. Think of each s_X as the realization of a fair ten-sided dice. An agent facing a new project will see the

[7]It is important to note that static approaches to coarse reasoning, such as those developed in Eyster and Rabin (2005) and Jehiel and Koessler (2008), are insufficient to model steady states in which strategies are based only on data about implemented projects. Formalizing the idea that individuals extrapolate success rate data from implemented projects to all projects, regardless of their implementation status, can be achieved either by incorporating the concept of self-confirming equilibrium, as proposed by Esponda (2008), or by using an extensive-form game approach to coarse reasoning as presented in Jehiel (2005). For further details, see the CEPR working paper version of Jehiel (2018).

[8]We use the term impression as it may reflect the investor's (informative) gut feeling about a project.

realization of one of s_A, s_B or s_C, but not the realizations of the other two signals. So there are three possible types of agents: The A agents who observe the s_A signal; the B agents who observe the s_B signal, and the C agents who observe the s_C signal. One can think of the type of an agent, A, B or C, as expressions of the comparative advantage the agent has in assessing a particular dimension, A, B or C, of any given project.

In all cases, we assume that whether a project is successful or not is determined by (s_A, s_B, s_B) according to the rule

$$x = \begin{cases} 0 & \text{if } W < 22 \\ \bar{x} & \text{if } W \geq 22 \end{cases}$$

where $W = s_A + s_B + s_C$. That is, the project is successful if the total score over the three dimensions as measured by W is above the threshold of 22.

The central question of interest here is how the investor decides on her investment strategy, and in particular, how she forms a belief about the relationship between the signal she observes and the likely success of the project she faces. We study investor behavior under four different information regimes, comparing the behavior of our experimental participants to two benchmark agents: (i) the rational Bayesian decision maker, and (ii) the Naive Extrapolator defined in Jehiel (2018) who extrapolates from the observed database as if it were unbiased, thereby displaying neglect of the selection in the data.

The feedback environment

In our experiment, participants are informed about the statistical model linking signals to the return of the project. In addition, we also provide participants with information about the outcomes of past projects that other participants chose to invest in. Importantly, in our main treatments, participants only receive information about projects that were invested in (and therefore implemented), not about projects that were not implemented. Our simple theoretical framework assumes a corresponding information environment. Specifically, for all observed past projects, an agent of type A (respectively B, or C) observes the realization of the signal s_A (respectively s_B, or s_C) as well as the corresponding return. An agent who is receiving feedback about a past project is not informed about the signal that was received by the previous agent who made the decision to invest in the project.[9]

[9] One can think of this, for example, as an agent always looking at a project through the lens of her subjective personal impression. In line with the specialization story mentioned above, agents may differ in terms of their relative advantage in assessing different dimensions of a project, or in terms of how much they focus their attention on different dimensions of the project. Therefore, the agent who is learning from historical data may form an impression or receive a signal that is equally informative about the likely success of the project as the signal initially received by the agent who made the investment choices, but since the two agents focus on different dimensions of the project, their signals may differ. This is particularly likely to be the case when the situation is complex. In simpler contexts, impression formation is more likely to be homogeneous across different individuals.

Notice that feedback is irrelevant for **rational Bayesian decision makers** who can make the best decision based simply on their knowledge of the true statistical model (DGP), but it will affect the behavior of **Naive Extrapolators** who assess the link between their impression and the return by extrapolating from the historical data they observe. It turns out that, for Naive Extrapolators, in addition to the structure of the feedback being important, the composition of types in the population of investors also affects their behavior. We will consider four scenarios.

Scenario 1: Learning from others

Our first and main scenario is one in which a type A agent only observes projects that were invested in by type B or C agents. This is meant to represent situations in which investment decisions are complex, and the chance that two different investors consider the same dimension of the project (to form their impression or ex ante assessment) is small.

Scenario 2: Learning from others with correlated signals

Our second scenario considers the case in which all investors are of the same type. This can either represent situations in which only one dimension can be observed prior to the investment decision or situations in which investors are homogeneous in their way of forming a first opinion about projects. There are many possible reasons why impression formation may be more homogeneous in some contexts than in others. For example, one might expect impression formation to be more homogeneous in (i) simple contexts rather than complex contexts, (ii) when the individuals have a similar world view, rather than having vastly differing perspectives, or (iii) when one particular dimension of the project is made salient to all individuals.

Scenario 3: Learning from better-informed individuals

In the third scenario that we consider, investors of a given type (e.g. type A) receive feedback on projects previously handled by either (i) investors of another type (e.g. type B or C), or (ii) omniscient robot investors that only invest in projects that will be successful. This scenario is designed to mimic real-life circumstances where a novice investor may observe the performance of investments from a combination of both seasoned and less experienced investors, with the proportion between the two groups playing a crucial role.

Scenario 4: Learning from others when the counterfactual is observable

As a benchmark for comparison, we consider a scenario that typically does not transpire in real-world settings, namely that the outcomes of unchosen paths or counterfactual decisions are observable. In this scenario, agents receive feedback on all projects — those that were invested in, as well as those that weren't invested in. Here, a type A agent will receive feedback on all projects faced by type B or C agents. The choices made by these other agents are irrelevant for the feedback she receives, and consequently, there is no selection of feedback. Aside from serving as a comparative benchmark, this scenario captures a class of situations in which an individual makes a decision, but the outcome will be observed irrespective of the decision.[10]

Bayesian benchmark

Given the symmetry of the three types of signals, let us consider s_A as a representative signal. Given a full description of the DGP, a Bayesian can compute $P(\bar{x}|s_A = s)$, the Bayesian posterior probability of success conditional on $s_A = s$. The statistical model described above gives rise to $P(\bar{x}|s) = \frac{1}{200} \cdot (s^2 - s)$. Thus, given the assumed risk neutrality, a Bayesian investor follows the decision rule of investing whenever she observes a signal s satisfying $s \geq s^{Bayes}$ where s^{Bayes} is the smallest s satisfying

$$P(\bar{x}|s) > \frac{c}{\bar{x}} \Longleftrightarrow \frac{1}{200} \cdot (s^2 - s) > \frac{c}{\bar{x}}. \tag{1}$$

Using the parameters for c and $\bar{x}$ from the experiment, in all of our scenarios a Bayesian will invest according to the threshold strategy: $s^{Bayes} = 9$. This is calculated using $\bar{x} = 3.40$ and $c = 1$.[11]

Equilibrium with naive extrapolators

As an alternative to the Bayesian benchmark, we consider an investor who relies on the historical data and mistakenly assumes that the success proportions she observes are good estimates of the statistic she cares about. Such an investor extrapolates naively from her observed data. Specifically, we consider an investor of type A who assesses the potential of her current project by looking at the proportion $\widehat{p}$ of successful projects in her historical data that have the same realization of s_A as her current project. She invests if $\widehat{p} \cdot \overline{x} > c$ and does not invest otherwise. We refer to such investors as Naive Extrapolators because they fail to take into account the bias in the historical data that arises from only observing projects that were invested in.

Jehiel (2018) proposes an equilibrium that describes the steady-state generated by such a heuristic investment strategy. To depict the equilibrium, we consider the idealized situation

[10]This would, for example, include any decision that involves a bet on some observable event. For example, buying a house, which involves a bet on the performance of the housing market. One would later be able to observe whether it would have been a good financial decision, irrespective of the decision made.

[11]The Bayesian investor doesn't rely on the feedback (historical data) received from others to formulate her investment strategy, since she can use the description of the DGP to infer the Bayesian posteriors directly. Therefore, she follows the same threshold strategy in all four scenarios.

with a continuum of investors. Let $q_{inv}(s_A, s_B, s_C)$ denote the probability with which a project with characteristics (s_A, s_B, s_C) was invested in historically in the pool of projects observed by an investor of type A.

A Naive Extrapolator of type A observing signal $s_A = s^*$ would think that the probability of success of projects with this signal is:

$$\widehat{P}(\bar{x}|s^*; q_{inv}) = \frac{\sum_{s_B, s_C} 1_{s^*+s_B+s_C \geq 22} \cdot q_{inv}(s^*, s_B, s_C)}{\sum_{s_B, s_C} q_{inv}(s^*, s_B, s_C)} \tag{2}$$

where $1_{s^*+s_B+s_C \geq 22} = 1$ if $s^* + s_B + s_C \geq 22$ and 0 otherwise. This equation draws on the fact that all (s_B, s_C) are equally likely, and only those projects that were invested in are observed. Accordingly, a Naive Extrapolator will invest if $\widehat{P}(\bar{x}|s^*; q_{inv}) \cdot \overline{x} > c$ and not invest otherwise.

In equilibrium, the investment strategy of the various types of investors should give rise to q_{inv}, which leads to a fixed point formulation that depends on which scenario we are in. For each of the four scenarios, this yields a naive extrapolation equilibrium prediction for the threshold strategy followed (and corresponding propensity to invest) in that scenario. Table 1 summarizes these predictions. In Appendix A, we provide further details and intuition regarding the calculation of the equilibrium thresholds.

Table 1: Summary of theoretical predictions across scenarios

	Threshold	**Investment Propensity**
Naive Extrapolator		
Scen. 1: **Selected**	$s^{SN1} = 6$	0.5
Scen. 2: **Correlated**	$s^{SN2} = 9$	0.2
Scen. 3: **Externality**	$s^{SN3} = 5$ with $\mu = 0.8$ $s^{SN3} = 6$ with $1 - \mu = 0.2$	0.58
Scen. 4: **Control**	$s^{SN4} = 9$	0.2
Bayesian agent		
Scen. 1 - 4: **All Treatments**	$s^{Bayes} = 9$	0.2

Summary of hypotheses

The theoretical predictions for our two benchmark agents, the rational Bayesian and the Naive Extrapolator, are summarized in Table 1. The table provides the threshold strategies in each scenario, as well as the propensity to invest.[12] The latter provides a convenient outcome measure and is used in the main empirical analysis below.

Experiment 1 is designed to reflect these four scenarios, with each treatment representing one scenario. Table 1 provides clear guidance for our hypotheses in the experiment, since the Bayesian agent has the same propensity to invest in all four treatments, while the naive investor's propensity to invest can be ranked across the four scenarios. We have three main hypotheses, each of which tests one consequence of naive extrapolation against the alternative hypothesis that the average participant's behavior is approximately Bayesian:

Hypothesis A. 1. *Due to the influence of being exposed to selected data, participants will invest more in Scenario 1 than in Scenario 4.*

Hypothesis A. 2. *Due to the negative (information) externality exerted by highly informed decision makers, the propensity to invest in Scenario 3 will be higher than in Scenario 1.*[13]

Hypothesis A. 3. *The increased correlation in the signals in Scenario 2 implies that the influence of naive extrapolation will be ameliorated. The propensity to invest will be lower than in Scenario 1, and approximately equal to Scenario 4, even though participants in Scenario 2 only observe projects that are invested in.*

This summary of the theoretical predictions reveals that the influence of being a Naive Extrapolator is highly contingent on the scenario. In some scenarios, naive extrapolation does not lead to any bias and can yield optimal investment choices, while in others we have a clear theoretical prediction regarding how naive extrapolation will result in biased investment choices.

4 Experiment 1

4.1 Design and Procedures

The objective of the experimental design is to construct an environment in which we can directly evaluate the influence of observing selected historical outcome data on investment behavior. The ideal experiment would also allow us to test how behavior changes when: (i) varying the sophistication of some investors, and (ii) varying the correlation between the signals observed across investors.

To achieve these objectives, we consider an experimental environment in which a project's success is determined by the realization of three dice, and private information is modeled

[12]Since each of the ten signals is equally probable, the investment propensity follows directly from the threshold strategy.

[13]Note, in order to focus directly on the influence of the externality exerted by exogenously increasing the investment quality of a subset of individuals, this hypothesis is referring to the average investment propensity, when *excluding the omniscient robot investors*. This allows for a direct comparison with the investors in Scenario 1, as any change in investment must operate through the investment outcomes they are observing.

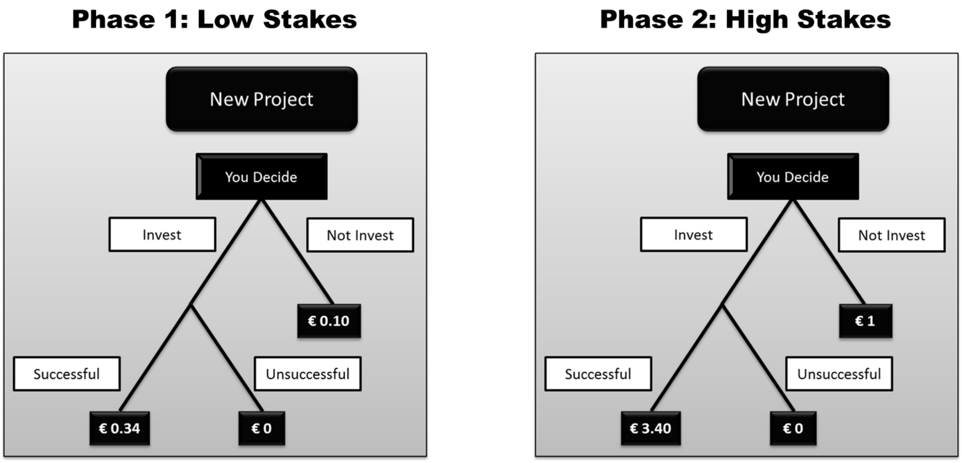
as the observation of one of these dice. We now provide details of the experimental design, echoing the main features of the model presented in Section 3.[14]

A Single Investment Decision

When an investor faces a new project, she makes a binary choice: to *invest* or *not invest*. She alone has the opportunity to invest in that particular project — i.e., it is not a group activity. The success of a project is determined by rolling three fair ten-sided dice, with the sides taking values 1 to 10. A project is *successful* ($x = \bar{x}$) when the sum of these three dice is weakly greater than 22, and *unsuccessful* ($x = 0$) otherwise.

Before making the investment decision, the participant observes the value of one of the three dice (Dice a, Dice b, or Dice c).[15] The observed dice value is informative about the likely success of the project, with higher dice values implying higher success probabilities. In the experiment, the three dice are described as representing attributes of the project.

Figure 1: Overview of the possible outcomes of an investment decision



Costs and Benefits of an Investment

Every participant faces 20 rounds of investment decisions. In the first 10 rounds (Low Stakes), choosing to invest has a cost of €0.10, a *successful* investment pays out €0.34, while an *unsuccessful* investment pays out €0. In rounds 11 to 20 (High Stakes), choosing to invest has a cost of €1, a *successful* investment pays out €3.40, while an *unsuccessful* investment pays out €0. In each round, participants are endowed with enough money to cover the cost of making an investment in that round. This ensures that they cannot earn a negative payoff. The possible outcomes of an investment decision are summarized in Figure 1 (which was also displayed to participants in the instructions for the experiment). The rationale for the Low Stakes phase is that it provides a period during which participants can

[14]The instructions for the SELECTED treatment are included in the Appendices.
[15]This dice value is described to participants as being an *attribute* of the project. This *attribute* assumes the role of the *private impression*, s_i, described in the theoretical section above.

learn about how the investment game works, while also building a large database of past investment outcomes to be used for feedback. Notice that for risk-neutral agents, moving between the two incentive regimes (High stakes vs Low stakes) would not influence their investment choices since all values are simply multiplied by 10.

Number of Investment Decisions

An important objective of the experimental design is allowing participants the opportunity to learn from the decisions of others while avoiding confounding issues that may arise from learning from small samples. There is tension between generating a large database of investment decisions and avoiding participant fatigue from making too many repetitive decisions. We resolve this issue using the strategy method (Selten, 1967) in the following way.

In each of the 20 rounds, a participant must decide for each of the ten possible attribute values whether she would like to invest or not, defining her *investment plan* for that round. The computer then randomly generates 50 projects (i.e. 50 sets of three dice rolls) for that specific participant in that round. Each of the 50 projects is then assessed using her *investment plan*, and investments are made when prescribed by her *plan*. In each round, one project is randomly chosen to be relevant for payment.

What information can participants use to guide their investment decisions?

A key object of interest in the investment decision is the participant's belief about the probability that a project will be successful, conditional on each possible attribute value (i.e. her belief about $P(\bar{x}|s_A)$). Participants in the experiment have access to two sources of information to guide their assessment of this probability:

1. A full description of the data generating process (**DGP**),

2. Information from past investments made by others (**"Learning from others"**).

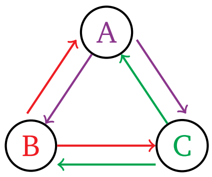
Using information about the data generating process (DGP)

In the experiment, participants were provided with a detailed description of the DGP.[16] For the Bayesian benchmark agent, this information about the DGP is sufficient to calculate $P(\bar{x}|s_A)$, and hence the optimal investment strategy. Therefore, information received about others' outcomes is completely superfluous once she is informed about the DGP.

[16]The experimental instructions carefully described how a project is comprised of three fair ten-sided dice rolls (Dice a, Dice b, and Dice c) and that if the sum was weakly greater than 22, the project would be *successful*.

Learning from Others

In contrast to the Bayesian agent, the Naive Extrapolator relies on the data she receives about the outcomes of investments made by others. This information is generated in the following way. Participants are randomly assigned to groups of 3 participants. They stay in this group of 3 throughout all twenty rounds of the investment game. Each participant in the group is given a player label, namely Group Member A, Group Member B, or Group Member C. Within each group, each player is able to observe the outcomes of past investments made by the two other group members (but not the outcomes of their own investments).



Updating the Personal Database

Over the course of the experiment, participants receive a large quantity of data regarding the outcomes of past projects (information about up to 2000 projects in total).[17] In order to assist them, the information each participant receives is organized into an infographic containing the information in their *personal database*. Figure 2 provides an example of an infographic that a Group Member B might see. All projects the individual receives feedback about are collected into buckets according to the relevant attribute (in this case, attribute b). For each attribute value from 1 to 10, the infographic reports the number of projects in that bucket and the fraction of those projects that were successful.

Participants provide their *investment plans* in each round by clicking one of the two options, "invest" or "don't invest", at the bottom of every attribute value column (see Figure 2).

Treatments: the feedback scenarios

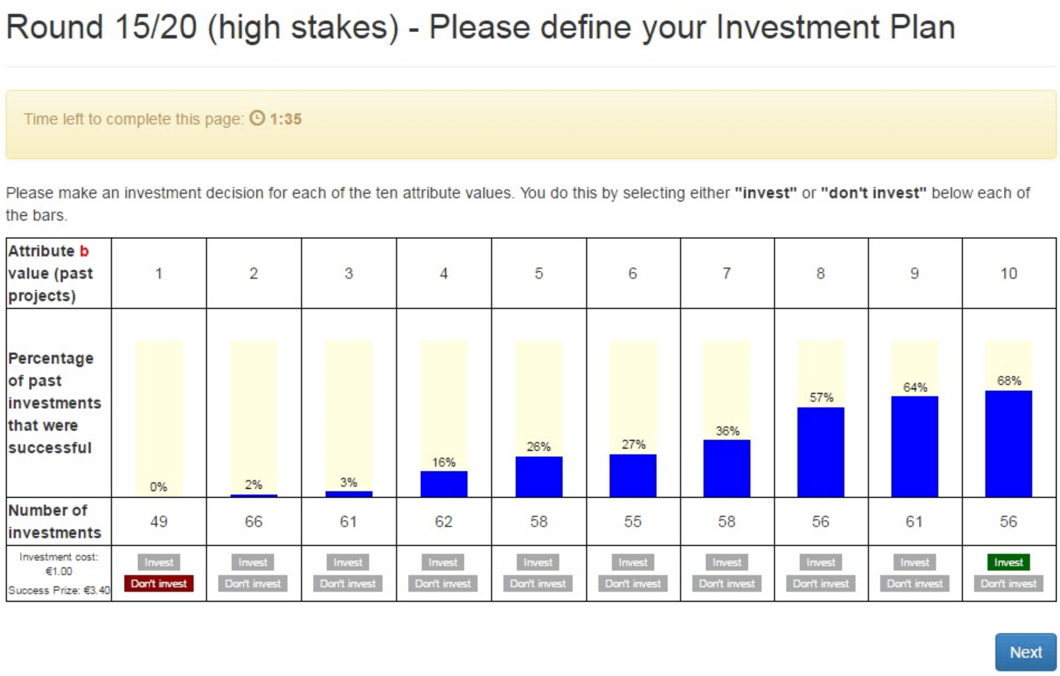
We consider four treatment conditions that differ only in the structure of the feedback. Each treatment replicates one of the four information scenarios described in the theory section above. The SELECTED treatment is our central treatment. In each of the other three treatments, one aspect of the structure of the feedback is altered slightly. In all four treatments, participants face identical incentives and are provided with a full description of the DGP.

[17] At the end of every round, each participant's database of observed past projects was updated by adding all projects that: (i) were part of the fifty projects faced by each of the other group members (one hundred in total); and (ii) were invested in by that group member. In the first round, all group members start with an empty database. In every subsequent round, all projects that satisfy these two criteria are added to the participant's personal database. This implies that the number of observations in the database grows over time.

Scenario 1: Selected Treatment

The Selected treatment allows us to examine the investment behavior of participants in feedback scenario 1, where they are provided with access to the outcomes of all projects invested in by their group members in earlier rounds. For example, type A group members observe the investments of the type B and C individuals in their group. Individuals always observe the attribute corresponding to their type (e.g., attribute a for a type A group member).

Figure 2: Example database observed by a B participant (data from decisions by A and C)



Scenario 2: Correlated Treatment

The Correlated treatment is identical to the Selected treatment, with the exception that all three group members are of the same type and therefore observe perfectly correlated project attributes. This implies that when a participant receives feedback from her two group members, she now observes the same project attribute as her group member.

Scenario 3: Externality Treatment

The Externality treatment provides a test of the theoretical prediction that Naive Extrapolators can be negatively affected by the presence of more informed investors. We introduce this variation experimentally by replacing one member of every group with a computer player who has perfect information about all three dice rolls and, therefore, only invests in successful projects. Other than this, the Externality treatment is identical to the Selected treatment. The decision rule that the computer player follows is: *invest if and only if the project will be successful*.[18] This rule is known to participants.[19] With one of the three players only investing in successful projects, the selection effect of feedback becomes stronger, implying that Naive Extrapolators should overinvest even more than in the SELECTED treatment.

Scenario 4: CONTROL TREATMENT

To accurately measure the effect of being exposed to selected feedback, it is important to observe investment behavior when the feedback is not selected. Our CONTROL treatment provides participants with feedback from *all* projects faced by their group members, irrespective of whether they invested or not. It is otherwise identical to the SELECTED treatment.

Implementation of the Experiment

With the aim of ensuring that participants had a clear understanding of the decision problem they faced, at the beginning of the experiment, participants received detailed instructions. Once all participants had carefully read through the instructions, they were required to complete a comprehensive set of control questions in order to ensure that all participants had fully absorbed and understood the instructions. Only after correctly answering all seven control questions did participants move on to the investment game.[20] Additionally, the pattern of behavior described in the results section below suggests that participants understood the decision problem that they faced (e.g., (i) the vast majority of the participants followed a threshold strategy in each of the last 5 rounds, and (ii) very few participants invested for low attribute values.)

[18]It is worth briefly noting our rationale for this choice of decision rule. One natural alternative would have been for the computer to behave like a fully Bayesian agent with imperfect information, namely investing for an attribute value of 9 and 10, and not otherwise. There are several reasons why we did not adopt this approach. First, by describing this alternative rule, we would have needed to tell participants the optimal decision rule without providing an explanation for why the computer follows this rule. This would make following a threshold decision rule salient, as well as anchoring participants to the specific Bayesian threshold rule. Therefore, if they followed this rule, we would not know if they had recognized the effect of selection, or were simply anchored to the computer's decision rule. Second, the computer decision rule that we use has the attractive features that it: (i) is easy to understand, (ii) has a more pronounced effect on the selection of projects, and (iii) makes it salient to participants that the computer is acting in an optimal way and selecting only successful projects. This should help participants to recognize selection and would work against finding an effect of the treatment.

[19]In particular, participants are told: "Unlike the two human players who observe only one attribute value, the Robot Player observes all three attribute values. Therefore, the Robot Player always knows when a project it is considering will be successful in advance. The Robot Player therefore always invests if the project will be successful, and never invests in unsuccessful projects."

[20]For example, question 2 of the control questions aimed to ensure that participants were aware that there was no role for experimentation: "Do you observe the outcome of projects that *you* invested in before the end of the experiment?" If participants tried to submit their answers and there was an error, the computer alerted them to the error, and they had to try again until they answered all questions correctly. Ninety-one percent of participants proceeded after making zero or one error.

Experiment 1 was conducted at the WZB-TU laboratory in Berlin between December 2016 and January 2018, with two sessions of 24 participants for each treatment group. This implies a total of 8 sessions, with 192 participants (48 in each treatment group).[21] Participants were solicited through an online database using ORSEE (Greiner, 2015) and the experiment was run using the experimental software, o-Tree (Chen et al., 2016).

After the 20 rounds of the investment game, there was also a risk elicitation task, and we obtained some demographic and other non-incentivized measures.[22] Sessions lasted up to 90 minutes. Average earnings from the investment game were €17.8 (including a €5 show-up fee).

4.2 Results

In Section 3, we provided the theoretical predictions for two benchmark agent types: (i) a fully rational Bayesian agent, and (ii) a naive extrapolator. The predicted behavior of these two benchmark agents differs starkly under the four feedback scenarios discussed, generating distinct comparative static predictions for each of the two types of agent. Since Experiment 1 is designed such that each treatment mirrors one of these four information scenarios, it allows us to directly evaluate the comparative static predictions of the theory by comparing investment behavior between treatments.

Recall that the Bayesian agent makes the same investment choices in all four information scenarios (i.e. Correlated = Control = Selected = Externality), while the propensity to invest of the Naive Extrapolator can be ranked by treatment as follows: Correlated = Control < Selected < Externality. The overarching question we wish to study in Experiment 1 is whether the observed pattern of behavior more closely reflects the predicted behavior of the rational Bayesian agent or the Naive Extrapolator.

Figure 3 provides a first look at investment behavior between treatments. Counting each individual as a single observation, the figure reports the average propensity to invest in the last five rounds.[23] It is striking that the pattern of investment behavior between treatment conditions strongly reflects the pattern predicted by the Naive Extrapolator model, with the

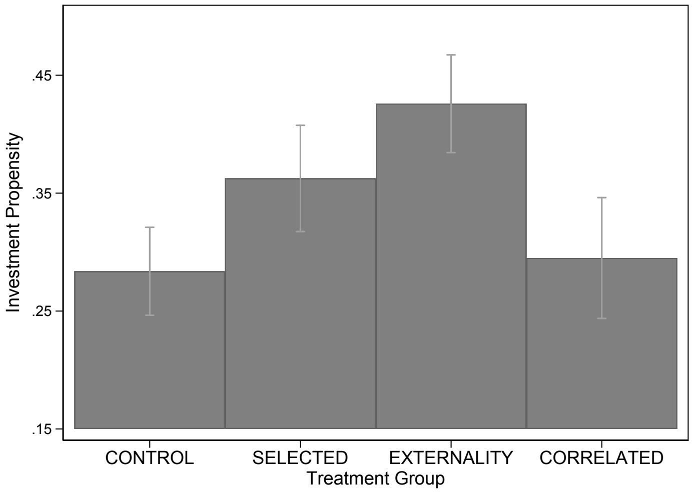
[21]In addition, we collected data for a further 2 sessions of Experiment 1 in which we explore behavior in the scenario where participants are not informed about the true underlying DGP. This treatment is discussed in Section 4.3.5 below.

[22]Additionally, after both the investment game and the risk elicitation task, participants completed a single round public goods game that was completely unrelated to this study. The instructions for the public goods game were only handed out after the investment game was completed. This implies that while completing the investment game, participants only knew that they would play a second game that was completely unrelated to the investment game. This information was identical across treatment groups.

[23]We focus on the later rounds in which: (i) the stakes for investing were higher, (ii) participants have familiarized themselves with the task, and (iii) they have accumulated a large database. The first ten rounds were intended to allow participants to learn about how the game worked, and also to fill their databases with a sufficient number of observations to reduce the influence of inference from small samples. Figure 4 shows how the propensity to invest evolved over the twenty rounds in each treatment. The figure shows that the investment propensity appeared to have stabilized by the last five rounds.

investment propensity ranked across treatments as follows: Correlated ≈ Control < Selected < Externality. This evidence suggests that naive extrapolation is present amongst the participants in our experiment and results in overinvestment.[24] Below, we provide further evidence in support of this assertion.

Figure 3: Propensity to invest in the last five rounds, by treatment



Do we observe evidence of naive extrapolation?

Here, we test the fundamental assumption underlying the theoretical framework, namely that there are individuals who fail to fully account for the way in which the data they observe has been selected. We test for the presence of naive extrapolation by comparing investment behavior in the Selected and Control treatments. This is a test of Hypothesis 1.

Since we used the strategy method to elicit participants' desire to invest for each attribute value in each round, we can directly obtain each participant's investment propensity by counting the number of attribute values for which she wished to invest within a given round.[25] In Figure 4, we report the average investment fraction across rounds by participants in each of the four treatment conditions. We are predominantly interested in the later rounds where participants have finished learning how the game works, stake sizes are more

[24]To check for statistically significant differences between the investment propensities in these four treatments, we conduct t-tests for each of the six possible binary comparisons. We find that these tests are completely in line with the ranking: Correlated ≈ Control < Selected < Externality in the sense that there is no statistically significant difference between Correlated and Control and each of the other five tests is significant at the 5% level (specifically, the largest p-value is p=0.024 for the comparison of Selected and Correlated). However, importantly, these t-tests do not account for any potential correlational structure that might emanate from individuals within groups learning from one another. For this reason, in Tables 2, 3 and 8 we also report the results from regressions that analyze each of these six treatment comparisons, clustering the errors at the interaction group level. We view these regression coefficients as providing more robust estimates of the statistical differences between the treatments. In general, the results remain the same.

[25]For example, if a participant chose to invest for all attribute values between 6 and 10 in a round, then her propensity to invest was 0.5. If, instead, she behaved as the Bayesian agent would and invested only for attributes 9 and 10, then her investment propensity would be 0.2 in that round.

meaningful (i.e. ten times larger), and investment behavior has stabilized (see footnote 23 for a more detailed discussion of the reasons). The figure shows that over time investment behavior in the different treatments diverges.

Figure 4: Propensity to invest across rounds, by treatment

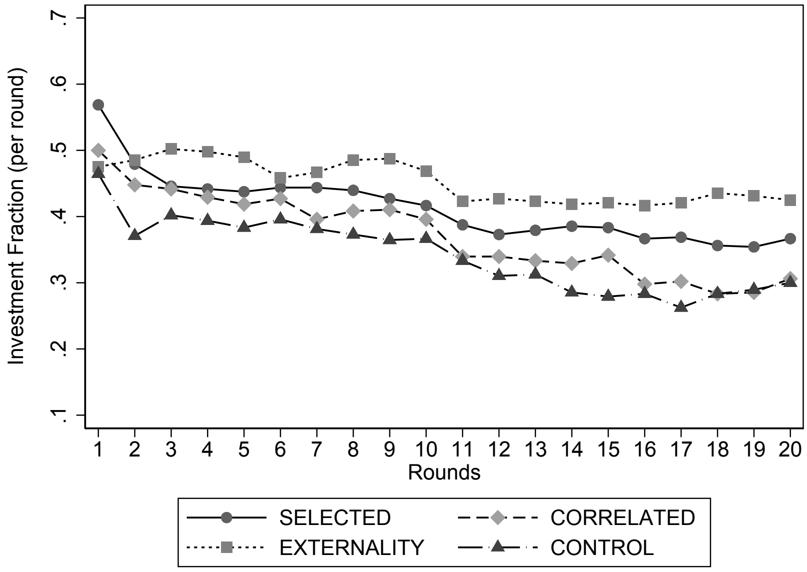


Table 2 tests whether there are statistically significant differences in the propensity to invest between treatments. Column (1a) reports treatment differences in the propensity to invest over all twenty rounds, while (1b) restricts attention to the last five rounds. As a robustness check, in column (2) we consider the restricted sample of individuals who followed a threshold strategy in each of the last five rounds.[26] We adopt the conservative approach of including each individual as a single observation. The results across the three regressions are fairly consistent, with participants investing between 27% and 34% of the time in the Control treatment and increasing their investment propensity by 7-8 pp in Selected. This provides our first result.

Result A. 1. *(Hypothesis A.1) Individuals who are given access to a selected subset of past outcomes generated by others mistakenly increase their propensity to invest.*

[26]For example, an individual is removed from the restricted sample if she invested for an attribute value of 6, but not for an attribute value of 7, in one of the last five rounds.

Table 2: Propensity to invest by treatment

	Full Sample		Restricted Sample
	All	R16-20	R16-20
	(1a)	(1b)	(2)
SELECTED	0.07***	0.08***	0.07*
	(0.03)	(0.03)	(0.04)
EXTERNALITY	0.11***	0.14***	0.15***
	(0.03)	(0.03)	(0.03)
CORRELATED	0.03	0.01	-0.03
	(0.03)	(0.03)	(0.04)
Constant	0.34***	0.28***	0.27***
	(0.02)	(0.02)	(0.02)
Observations	192	192	148
Adjusted R^2	0.070	0.113	0.178

Notes: (i) OLS regressions include one observation per individual, (ii) The dependent variable is an individual's average investment propensity, either over all rounds, or over rounds 16-20, (iii) The comparison treatment in the regressions is the CONTROL treatment, (iv) Standard errors are clustered at the interaction group level. This means that in the SELECTED and CORRELATED treatments there are three individuals per cluster, in the EXTERNALITY treatment there are two individuals per cluster, and in the CONTROL treatment each individual is a cluster, since their feedback is not influenced by their group members' choices. The standard errors are reported in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Do more informed individuals exert a negative externality on others?

We study this question by comparing SELECTED with EXTERNALITY. The information externality of introducing the omniscient computer players is illustrated in Figure 17 in the Appendices, which portrays the average historical database observed in round 20. For every attribute value, there is an upward shift in the fraction of observed past projects that were successful when comparing the EXTERNALITY to the SELECTED treatment.

Table 3 tests whether this information externality translates into a shift in investment behavior. Indeed, the regression estimates suggest that the propensity to invest is 6 to 8 pp higher in the later rounds in EXTERNALITY in comparison to SELECTED.[27] This increase in investment is particularly noteworthy in light of the fact that participants are already overinvesting in the SELECTED treatment. This is reflected by the results in Table 2 which

[27]We do not observe a significant difference in investment when we pool all twenty rounds. Examining Figure 4 shows that during the first ten periods, while participants face low incentives and are still learning and accumulating data, the gap between the two treatments is smaller than in the later periods when the stakes are higher.

show that investment is 11 - 15pp higher in EXTERNALITY relative to CONTROL, representing a 30 - 50% increase.

Result A. 2. *(Hypothesis A.2) When the decision making of one individual is improved, this exerts a negative externality on those learning from her outcomes.*

Table 3: Propensity to invest in SELECTED and EXTERNALITY

	Full Sample		Restricted Sample
	All	R16-20	R16-20
	(1a)	(1b)	(2a)
EXTERNALITY	0.04	0.06^{**}	0.08^{**}
	(0.03)	(0.03)	(0.03)
Constant	0.41^{***}	0.36^{***}	0.34^{***}
	(0.02)	(0.02)	(0.03)
Observations	96	96	73
Adjusted R^2	0.010	0.034	0.052

Notes: (i) OLS regressions include one observation per individual, (ii) The dependent variable is an individual's average investment propensity, either over all rounds, or over rounds 16-20. (iii) The comparison treatment in the regression is the SELECTED treatment, (iv) Standard errors are clustered at the interaction group level. This means that in the SELECTED treatment, there are three individuals per cluster, and in the EXTERNALITY treatment there are two individuals per cluster. The standard errors are reported in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Do correlated signals offset the influence of naive extrapolation?

The discussion above has illustrated the harmful effects naive extrapolation can exert on decision making. These effects are driven by the failure to pay attention to the private information guiding other decision makers' choices, implying a selected subset of outcomes is observed. However, it is important to notice that the influence of naive extrapolation varies in a systematic way according to the characteristics of the decision making context. In particular, in situations where different individuals focus on similar dimensions of the decision problem, naive extrapolation should play a smaller role. Our CORRELATED treatment allows us to test whether increasing the correlation in the signals received by different individuals about a given project causes the harmful influence of naive extrapolation to dissipate.

Figures 3 and 4 suggest that by the later rounds, the propensity to invest in the CORRELATED treatment converges to the same level as in the CONTROL treatment. This is in line with the theoretical predictions. Table 2 supports this conclusion, showing that there are no significant differences in investment between the CORRELATED and CONTROL treatments.

Furthermore, the point estimate of the difference between treatments is small, ranging from 3pp to -3pp across the three specifications.

Result A. 3. *(Hypothesis A.3) An increase in the correlation between signals received by different individuals ameliorates the influence of naive extrapolation when learning from others.*

4.3 Discussion of Experiment 1

So far, the results from Experiment 1 show how observing a selected historical database of outcomes can bias individuals' decisions towards overoptimism.

4.3.1 Heterogeneity and risk attitudes

Our data reveal that the chosen investment strategies are heterogeneous among participants. Assuming agents are only concerned with material payoffs, the observed propensity to invest in Control and Correlated (where naive extrapolation is not expected to bias choices) is higher than the investment propensity of a risk-neutral Bayesian agent. A natural step for explaining the observed heterogeneity in investment choices would be to relax the risk neutrality assumption we impose in our theoretical analysis. However, reconciling our observed data with the theory in this way would imply concluding that the average participant displays some form of risk-loving behavior. But risk loving is not consistent with what we observed in the risk elicitation task data that we have collected for our pool of participants.[28]

An alternative explanation is to consider that there is an additional taste for investment beyond the material payoffs and that this additional taste is heterogeneous among agents. However, the primary object of interest of our experiment is not this within-treatment heterogeneity; rather, we wish to test whether the between-treatment differences in investment behavior reflect the pattern predicted by a model of naive extrapolation. We are predominantly interested in the shift in investment rates between treatments rather than the absolute levels. Under the assumption that the taste for investment is distributed similarly amongst participants across our treatment groups, the statistical differences we observe in investment rates across treatments allow us to safely reject the hypothesis that all participants are forming beliefs in a Bayesian fashion. In order for the heterogeneity in the taste for investment to explain the treatment differences we observe, one would need to argue that this taste for investment interacts with our treatments in precisely the way predicted by a model of naive extrapolation. We view this as improbable—in our view, the treatment differences we observe are driven by shifts in beliefs rather than shifts in preferences. We explore this further in Experiment 2, providing additional supporting evidence for this assertion.

[28]Fewer than 15% of participants display risk-loving behavior in our risk elicitation task.

4.3.2 Inference from selected data

The existing literature studying belief formation from selected data is small, which is somewhat surprising given the proliferation of selected data in daily life. However, recent work has demonstrated how neglect of selected data can generate striking deviations from rational behavior in contexts very different from those considered in the present paper. In an interesting experiment that builds on the theoretical work in Esponda (2008) and Esponda and Pouzo (2017), Esponda and Vespa (2018) consider a committee decision problem where the participant and two computer players decide jointly whether to invest in a risky project. The computers follow a noisy decision rule governed by an unknown parameter. When the computer's decisions are correlated with the true state of the world, the participant observes a selected sample of past projects. Their paper shares with the current paper its consideration of the neglect of selected data. However, it differs in terms of the strategic setting, presence of an experimentation motive (through learning from own experience), and requirement of hypothetical reasoning. In line with the results of this paper, Esponda and Vespa (2018) find evidence of neglect of endogenous selection.

In work complementary to the current paper, Enke (2020) studies whether individuals are able to take into account missing information. The paper presents a careful and insightful experimental investigation into the existence of selection neglect in the context of individuals selecting themselves into news networks that reflect their own views (sometimes referred to as "echo chambers"). Enke (2020) shows that selection neglect could lead to the polarization of beliefs and provides evidence on the cognitive mechanism underlying the bias. While exploring a similar theme, the focus on how selection neglect in "echo chambers" can lead to the polarization of views is different from the current paper's focus on a micro-foundation of overconfidence. Interestingly, while Enke (2020) demonstrates that selection neglect can lead to the polarization of views, we demonstrate that in a large class of situations, the same bias can also result in all individuals holding beliefs that are biased in the same direction. This is indicative of the importance of the context in determining the predicted behavior of selection neglect agents.

4.3.3 Overconfidence

Taken together, the nascent evidence discussed above suggests that selection neglect plays a significant role in influencing behavior across a range of contexts. However, an important lesson from this literature is that, depending on the context, selection neglect can manifest itself as distinctive behaviors. In the contexts considered in the current paper, selection neglect manifests as behavior akin to that generated by overoptimism or overconfidence provided that signals across different investors are not perfectly correlated. When they are, the apparent overconfidence disappears.

This distinction highlights that the overoptimism bias that we observe is neither the mere consequence of some general belief distortion nor simply due to the fact that the available information is biased towards more profitable projects, as this would be true whether or not signals are correlated. Instead, the overoptimism that we observe is affected by whether participants look at the same aspect of a project or at different aspects when they make their judgments. Our study, therefore, suggests that overoptimism should be expected to be more prevalent in complex investment problems than in simple ones. Similarly, on the population level, overoptimism may be more common in heterogeneous societies with competing education and value systems than in more uniform societies where people are trained in similar ways.

4.3.4 Survivorship bias and reference group neglect

Two concepts related to the ideas discussed in this paper are *survivorship bias* and *reference group neglect*. Survivorship bias typically refers to situations in which individuals draw inferences from a distribution of outcomes that is truncated to the left, normally because the observations in the left tail have "failed" in some sense. For example, Denrell (2003) studies how false beliefs can result from an undersampling of failed ventures, and Brown et al. (1992) demonstrate that survivorship can induce an apparent persistence in returns in the performance of mutual funds. Since the concept of selection neglect shares some common features with survivorship bias, one might conjecture that individuals prone to selection neglect might also be prone to biased inference when observing the outcomes of a survival process. While this may be the case, a key difference is that the selection we study is generated by a specific channel, namely that individuals select into particular life decisions on the basis of private information and may try to learn from observing the outcomes of others who have made similar decisions. This form of selection is more subtle than in a survival process (which involves the survival of particular types) and results in particular equilibrium predictions when individuals who neglect selection are simultaneously learning from one another (e.g., exacerbation of the bias when learning from more informed or more rational individuals and amelioration of the bias when signals are highly correlated).

Reference group neglect is a concept discussed by Camerer and Lovallo (1999) in their seminal experimental study of whether excess entry and business failure can be explained by overconfidence. In particular, in their experiment excess entry is exacerbated when participants self-select into participating in the experiment, presumably on the basis of believing that they are highly skilled in the relevant area. The authors attribute this exacerbation of the bias to the fact that participants fail to adjust their entry decision to incorporate the self-selection of their competitors. Two key differences between reference group neglect and the selection neglect considered in this paper are: (i) that Camerer and Lovallo (1999) consider a process where individuals, rather than projects are selected, which drives the pattern of predicted behavior across different information structures in our paper;[29] (ii) the current paper focuses on situations where individuals learn from the outcomes of others, which is different in spirit from the neglect of self-selection considered by Camerer and Lovallo (1999).

4.3.5 Experimenter demand concerns

One concern that can be raised about Experiment 1 is that the experimental design may lead participants towards behaving as if they display selection neglect, even though they might not fall victim to selection neglect in other real-world contexts. While it is always important to take experimenter demand concerns seriously, there are several mitigating factors that lend support to the conclusion that our results are driven by participants displaying selection neglect. In particular, in order to make it easier for participants to detect the selection process, we explicitly provided participants with full information regarding the simple data-generating process. In most real-world contexts, such as learning about the expected earnings one will receive when following a certain career path, one has much less information about the true data-generating process, and the influence of selection is more hidden. In our experiment, participants, therefore, had two avenues available to them to adjust their behavior away from the naive extrapolation benchmark: (i) they could rely solely on their knowledge of the data-generating process, or (ii) if they recognized the presence of selection, they could use the information in the data set, but exercise caution by deflating the observed success probabilities. In both these cases, even if participants find the task challenging, it is not ex-ante clear that we would expect to see overinvestment (as predicted by naive extrapolation). Rather, if they recognize that the data they observe is selected, and consequently find the investment decision more complex, a cautious investment approach would be natural, which could result in participants becoming overly cautious and investing too little.

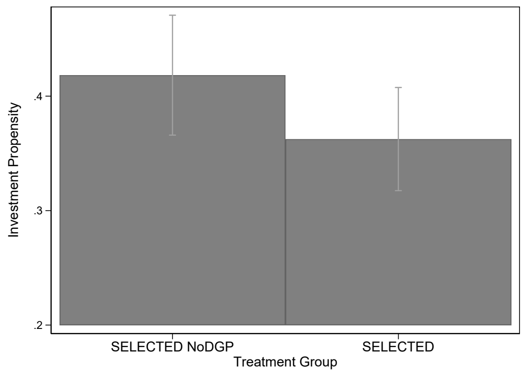
In order to assess the influence of providing participants with explicit knowledge of the data-generating process, we additionally ran a treatment that is identical to Selected, with the exception that participants were not provided with a description of the data-generating process (i.e. information about the statistical relationship between signals and project success). Instead, they had to rely purely on the data received from others and summarized in their *personal database* for this. The comparison of the investment rate in this SelectedNoDGP treatment and the Selected treatment provides us with a measure of how the provision of the DGP information affects investment rates.

Figure 5 shows that when participants are not provided with information about the DGP, their rate of investment is even higher than in Selected, thereby suggesting that either

[29]Interestingly, when one considers the neglect of self-selection of people, there are also situations in which the predictions of this neglect can generate pessimism rather than optimism (e.g. when learning from the outcomes associated with a harmful decision, such as smoking or drug usage).

channel (i) or (ii) mentioned above is partially alleviating the higher degree of overinvestment observed when participants can only rely on feedback received from others' decisions. However, as already noted this provision of information about the DGP is insufficient to fully overcome the bias generated by observing selected data, as observed by the enduring presence of overinvestment in SELECTED and EXTERNALITY.

Figure 5: Influence of DGP knowledge on investment rates (in the last five rounds)



Taken together, this evidence suggests that in contexts where participants have easy access to selected data, while the provision of additional unbiased information about the data generating process may help to reduce the degree of the bias, the bias due to selection neglect may persist. This speaks to many real world scenarios in which individuals can draw on multiple sources of information in order to draw inference. For example, an individual deciding whether to buy a particular product on Amazon is presented with easy access to reviews by previous buyers of that product. These reviews clearly represent a selected sample due to multiple layers of selection.[30] While the potential buyer might easily be able to search the internet for less biased assessments of the product's quality (e.g. independent product evaluations), our results here suggest that on average the biased product reviews will influence individuals beliefs about the quality of the product due to being easily accessible. In a world where data and reviews are proliferating, the fact that the beliefs and actions of the average individual are influenced by the data that is put in front of them, even when it is systematically biased, seems important. Naively extrapolating from the data one sees can lead to substantial errors.

5 Experiment 2

Experiment 1 was designed to study aggregate-level equilibrium behavior when individuals learn from one another. Specifically, it provides a test of a set of comparative statics for steady-state equilibrium behavior predicted by the model in Jehiel (2018). Experiment 1

[30] e.g. selection into buying the product, selection into writing the review, etc.

is, however, not well suited to examine individual behavior as it is difficult to cleanly distinguish the influence of preferences from beliefs at the individual level. Specifically, after playing the investment game in Experiment 1 for a few rounds, the variation in the success frequencies observed in an individual's database rapidly decreases.[31] This means that, at the individual level, it is difficult to distinguish a selection neglect subject who is very risk averse from an individual with Bayesian beliefs who is risk neutral or has a taste for investing. Generally, it would require making strong assumptions to separately identify the role of preferences and beliefs for a specific individual.[32] At the aggregate level, Experiment 1 is able to achieve its desired objectives, since it is reasonable to assume that preferences (e.g., risk, taste for investment) are evenly distributed across treatments, which allows us to attribute differences in behavior to changes in beliefs.

To expand upon the insights generated by Experiment 1, we subsequently designed Experiment 2, which involves a simplified one-shot individual decision-making version of the investment game where participants learn from a "previous generation" of decision-makers. Experiment 2 has three objectives. First, it aims to test whether the key cognitive bias that underpins the results in Experiment 1, namely the failure to take into account the selection of data, is observed to a similar degree when the setting is simplified. In doing this, it provides a form of replication and stress test of the key cognitive mechanism. Second, Experiment 2 is designed to experimentally control for the role of preferences at the individual level, thus, permitting an individual-level classification of individuals according to their beliefs on a *Bayesian–naive beliefs* spectrum. This is achieved by adding two tasks to the experiment that directly measure how individuals would invest (i) if they held perfectly Bayesian beliefs, and (ii) if they held perfectly naive beliefs. Third, it can shed light on the psychological mechanisms generating selection neglect. To achieve this, we use the measurement of an individual's degree of naivety as an outcome and systematically vary the choice environment to examine whether particular features of the environment exacerbate or ameliorate the manifestation of naivety. Specifically, we can ask whether neglecting selection is a general cognitive mistake that individuals make across many contextual environments or whether specific features of the decision environment help individuals to recognize the selection and to account for it in their decision-making. The main empirical results associated with each of these three objectives are discussed in Sections 5.2.2, 5.2.3, and 5.2.4 respectively.

[31]The implication of this is that, after a few rounds, there is not much variation within an individual in the naive probability of success. Since there is also no variation in the Bayesian probability of success, it becomes difficult to identify from choices which of these probabilities a particular individual holds in mind without making strong assumptions about their preferences. At the aggregate level, we have between-subject exogenous variation in the naive probabilities, which is the primary source of identification that we use in Experiment 1.

[32]Such an exercise would rely almost entirely on variation from the first few rounds of the game. This seems undesirable as these rounds paid low stakes, and participants likely used the initial rounds to experiment and learn about the game, knowing that the high-stakes rounds were still to come.

5.1 Experimental Design

Experiment 2 involves a simplified version of Experiment 1. Once again, participants in the experiment decide whether to *invest* or *not invest* in a new project after observing a noisy signal about the likelihood that the project will succeed.

5.1.1 The 3-Dice PastData treatment

The 3-Dice PastData treatment is the core treatment of Experiment 2. We use it for our main individual-level classification analysis and it also serves as the benchmark treatment for the majority of the treatment comparisons that we consider. Each of the other treatments in Experiment 2 involves a variation in one particular feature of the decision architecture relative to the 3-Dice PastData treatment. We, therefore, now provide a full description of this 3-Dice PastData treatment. In Section 5.1.3 below, we explain how each of the other treatments differs.

A project's success is again determined by the realization of three dice (now referred to as the **green**, **red** or **purple** dice), and private information is modeled as the observation of one of these dice. One key difference is that, in Experiment 2, the participant only plays a single round of the investment game—i.e., it is a one-shot task. This serves to simplify and shorten the experiment substantially as well as reducing any experimenter demand effects that participants may feel due to the repeated nature of Experiment 1. This one-shot investment game has the same incentive structure as the high-stakes rounds in Experiment 1 (with the exception that payoffs are now denominated in pounds rather than euros).

Participants are fully informed about the features of the data-generating process (i.e., they are told that the success of the project is determined by rolling three fair ten-sided dice). In addition, participants are provided with access to the outcomes of projects *that previous participants chose to invest in* (i.e., not the outcomes of projects that previous participants chose not to invest in). This data from previous participants is collected in another treatment, the 3-Dice NoPastData treatment, which is identical to the 3-Dice PastData treatment, with the exception that participants do not have access to any past data when they make their investment decisions.[33]

As in Experiment 1, these past projects are summarized according to the dice value that is relevant for the participant who is currently making the investment decision. Importantly, the dice value of the decision-making participant differs from the dice value observed by the individuals who made the past investment decisions (i.e., who they are learning from). Participants are informed about this. This data from past participants, therefore, comprises

[33]For each of the 100 participants who participate in the 3-Dice NoPastData treatment, the computer will randomly generate 50 projects. It will then use the decisions indicated by their strategy method investment choices in the investment game in Part A to evaluate whether they invest or do not invest in each of the 50 projects. For each of the 100 participants, the computer will collect together the projects that they invested in. For further details, see Appendix D.1.

a selected database of investment outcomes.

5.1.2 Parts of the Experiment

The experiment consists of four parts: Parts A, B, C and D. Part A always comes first. Part B and C are randomly ordered. Part D always comes last. Participants were told that one of Parts A, B and C would be randomly chosen to be relevant for calculating their payment for the experiment.

In Part A, participants complete the one-shot **investment game task**. As in Experiment 1, we use the strategy method to elicit their investment choice for every possible realization of the dice between 1 and 10.

In Part B, the **Bayesian lottery (BL)**, participants make a set of investment decisions that are nearly identical to those in Part A, with one key exception. In Part B, we endow participants with the probabilistic beliefs that they would hold if they were to perfectly calculate the Bayesian probabilities of success in Part A. Specifically, the differences between Part B and Part A are as follows. First, the outcome of the investment decision in Part B does not depend on 3 dice being rolled. Rather, participants are told that there will be a single dice rolled, a **brown** dice. Second, each value of this **brown** dice between 1 and 10 is associated with a project that has a **known probability** of success (i.e., each dice value between 1 and 10 is associated with an objective lottery and participants are told the probability of the lottery). So, essentially, the **brown** dice determines which lottery they will face. We again use the strategy method to elicit participants' investment choices for every realization of the **brown** dice between 1 and 10—i.e., whether they prefer the lottery with known probabilities or the fixed monetary payment of £1. Third, a crucial feature of Part B is that we (the experimenters) choose the objective probabilities of each of the 10 lotteries to coincide with the Bayesian probabilities from Part A. This means that for an individual who is able to calculate the Bayesian probabilities of the project succeeding in Part A, the investment decision in Part A and Part B will be identical.

Part C, the **Naive lottery (NL)**, is almost exactly the same as Part B, with two differences. First, the dice in Part C is labelled as a **blue** dice. Second, there are different probabilities for each of the ten objective lotteries—in Part C, the probabilities are chosen to coincide with the beliefs that a fully naive extrapolator would form in Part A.

In Part D, we collect some basic demographic data and ask participants a few free text questions about how they made their decisions in Parts A, B and C.

5.1.3 Treatment Conditions

Experiment 2 consists of nine treatment conditions. In this section, we outline the core features of these treatments. A more detailed description is provided in Appendix D.1, with

Table 11 summarizing the key features of the treatments. The treatments can be divided into four sets.

First, the two treatments which have already been described above, namely the 3-Dice NoPastData and 3-Dice PastData treatments, are the central treatments in Experiment 2. They fulfill multiple objectives within our study: (i) they serve to provide the primary robustness check for Experiment 1 by testing for naivety in a simplified context, (ii) they facilitate our individual-level analysis, and (iii) they establish a benchmark for comparison for the remaining treatments in Experiment 2. Each of the other seven treatments is constructed by varying one particular element of the decision-making context relative to one of these two benchmark treatments.

Second, the 3-Dice17 NoPastData and 3-Dice17 PastData treatments are identical to the 3-Dice NoPastData and 3-Dice PastData treatments, with the exception that the threshold for a project to be successful is reduced from 22 to 17. This means that the Bayesian probabilities of success are shifted upwards—conditional on any particular observed dice roll realization, a project is more likely to be successful with the lower threshold. These treatments provide an additional robustness check by testing whether our main results persist when all of the probabilities of success are inflated.

Third, 2-Dice NoPastData and 2-Dice PastData treatments allow us to probe the boundaries of naive extrapolation. In these two treatments, the complexity of the DGP is reduced by using two dice instead of three to define a project. In these treatments, a project is successful if the sum of the two dice is at least 16.

Fourth, in the 3-Dice PartialDGP, 3-Dice CUE and 3-Dice ExtraInfo treatments, we examine whether adding or removing particular pieces of information about the DGP influences the manifestation of naivety. Each of the treatments is identical to the 3-Dice PastData treatment, with the exception that one feature is adjusted. In the **3-Dice PartialDGP**, participants are only provided with a partial description of the data-generating process. This treatment allows us to investigate the influence of an investor not fully knowing the true underlying model that is generating the data. The **3-Dice Cue** explores the limits of naive extrapolation. In this treatment, participants read a series of leading questions before making their investment choice. Since these questions contain no information, this treatment allows us to ask whether a "cue" of this type can induce the investor to view the problem through a different lens, helping them to see the logical error present when engaging in naive extrapolation. Finally, in the **3-Dice ExtraInfo** treatment, participants are provided with a second table that summarizes the information about the number of projects that past investors chose to invest in. This allows us to evaluate whether the provision of this additional information, which highlights the fact that not all projects are observed, helps to alleviate naivety.

5.1.4 Procedures

Experiment 2 was programmed in Qualtrics and participants were recruited via the online platform, Prolific. The data collection for the first seven treatments was conducted between 29 November 2022 and 12 December 2022.[34] Thereafter, inspired by helpful comments received from the referees and editor reviewing our paper, we added two further treatments, 3-Dice17 No PastData and 3-Dice17 PastData, which were implemented on 20 April 2023 and 21 April 2023, respectively. In total, we collected 1000 observations in Experiment 2, approximately 100 individuals in each of the treatments aside from the core 3-Dice PastData treatment, for which we collected 200 observations. The median time taken to participate in the experiment was approximately 10 minutes, and the average participant received a payment of £3.35.

5.2 Empirical Analysis

5.2.1 Preliminaries and Definitions

In Part A, both rational agents and naive extrapolators should follow a threshold strategy since, for both types of agents, the perceived probability of a project succeeding is increasing monotonically in the value of the dice that they observe. In Parts B and C, the objective (known) probability of success is increasing in the dice value, so individuals should also follow a threshold strategy if they prefer a higher probability of winning.[35]

We can, therefore, define a set of variables that contain a count of the number of dice values between 1 and 10 that an individual invests for in each of Parts A, B, and C, respectively:

- $\tilde{n}_i$ is a count of the dice values for which individual i invests in Part A.
- n_i^{BL} is a count of the dice values for which individual i invests in the Bayesian Lottery.
- n_i^{NL} is a count of the dice values for which individual i invests in the Naive Lottery.

We can now also define an index that captures whether the individual's investment behavior in Part A is closer to their investment behavior when we induce in their mind the objective Bayesian probabilities as beliefs (i.e. their Part B investment behavior) or whether

[34] As pre-registered (AEARCTR-0010536), we aimed to collect 200 observations in the benchmark 3-Dice PastData treatment, and 100 observations in each of the other six treatments, resulting in 800 participants in total for these first seven treatments. The data from the two No PastData treatments was collected first between 29 November 2022 and 2 December 2022; thereafter, the data collection for the other five treatments was carried out on 12 December 2022. In each of these two waves of data collection, after consenting to participate in the experiment, participants were randomly assigned to a treatment.

[35] In Experiment 2, over 93% of participants followed a threshold strategy in each of Parts A, B, and C of the experiment.

it is closer to their investment behavior when we induce in their mind the beliefs of a fully naive extrapolator (i.e., their Part C investment behavior). Define:

$$\mu_i^n = \frac{n_i^{NL} - \tilde{n}_i}{n_i^{NL} - n_i^{BL}} \tag{3}$$

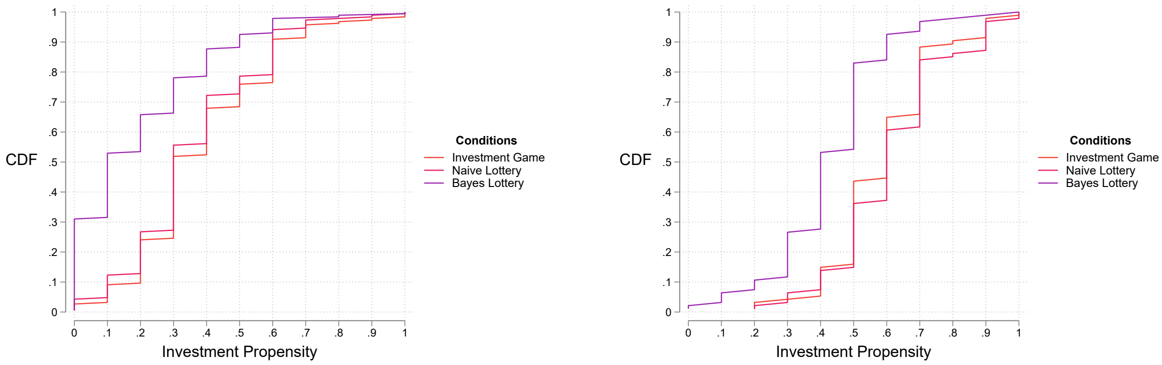
The interpretation of this parameter is as follows. If $\mu_i^n = 1$ then the individual's investment behavior in Part A is the same as in the Bayesian Lottery, implying that in Part A they are behaving as if they hold in mind the correct Bayesian beliefs as the probabilities associated with investing for each dice value. If $\mu_i^n = 0$ then the individual's investment behavior in Part A is the same as in the Naive Lottery, implying that in Part A the individual is behaving as if they hold in mind the observed empirical frequencies as the probabilities of success for each dice value. For each individual, we will therefore obtain a μ_i^n value that denotes how close their Part A investment behavior is to Bayesian versus Naive Extrapolation. Since we hold constant many other features of the choice environment that might influence investment choices via preferences (e.g., risk preferences, framing, midpoint anchoring, taste for investment), we can attribute differences in investment behavior between Part A, B and C of the experiment to differences in beliefs about the probability of success for each dice value.

5.2.2 Is the average participant naive?

In Experiment 1, we presented evidence that the average participant displayed some degree of naivety, implying that they deviated from behaving like a Bayesian. In Experiment 2, we aim to replicate this finding in a far simpler setting. While the 3-Dice PastData treatment retains the core elements of Experiment 1, we have introduced several important adjustments. First, participants only make a single investment decision. Second, we reduced the salience of the information about past projects by representing it in a simple table rather than graphically (thereby reducing potential experimenter demand effects). Third, we simplified the instructions substantially, transformed the task into an individual decision-making task rather than a dynamic interactive game, and added additional understanding checks to ensure that participants fully understood the setting. Therefore, testing for the presence of naivety in this simplified setting serves as a good robustness check for the core mechanism discussed in Experiment 1.

To evaluate whether the average individual is naive, we test whether $\mu^n < 1$ in the 3-Dice PastData treatment. We find that, on average, $\mu^n_{3D.PD} = -0.09$, which is significantly below the Bayesian benchmark of $\mu^n = 1$ ($p < 0.01$) and close to the fully naive benchmark of $\mu^n = 0$. This suggests that the average individual is engaging in investment behavior in the investment game in Part A, which is more similar to that in the Naive Lottery than in the Bayesian Lottery. Figure 6a illustrates this visually by showing that the CDF of the propensity to invest in the investment game is much closer to that in the Naive Lottery than the Bayesian Lottery.

Figure 6: CDFs of propensity to invest (investment game vs Bayes vs Naive)



(a) 3-DICE PASTDATA (b) 3-DICE17 PASTDATA

As an additional robustness check, in Figure 6b we document a similar pattern of behavior in the 3-DICE17 PASTDATA treatment where the threshold for the success of a project is reduced to 17 instead of 22.[36] This shows that naivety persists in a scenario where the probability of success associated with every dice number is shifted upwards. This is important since it provides further evidence that our results are not driven by another bias that "looks like" naivety under the parameterization that we consider in most of our treatments. For example, it helps to alleviate the concern that some fraction of individuals are simply uncertain about what to do in the investment game and therefore invest in half of their investment opportunities, thereby distorting average investment propensities away from the Bayesian benchmark and towards 0.5.[37]

Result B. 1. *(Hypothesis B.1) The average participant in the 3-DICE PASTDATA treatment is (at least) partially naive. Observed behavior in the Investment Game is more similar to that in the Naive Lottery than in the Bayesian Lottery.*

5.2.3 Individual-level classification of participants

The design of Experiment 2 allows a clean and simple individual-level classification of participants. Investment behavior in the investment game (Part A) can be directly compared with investment behavior in the Bayesian Lottery (Part B) and Naive Lottery (Part C). The only difference between the three scenarios is the individual's beliefs about the probabilities of

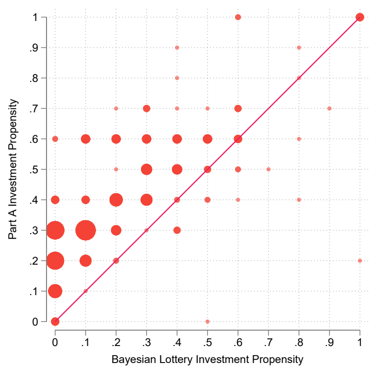
[36] In 3-DICE17 PASTDATA, we find that $\mu^n_{3D17.PD} = 0.26$, which is also significantly lower than the Bayesian benchmark of $\mu^n = 1$ ($p < 0.01$). This is the case for all six PASTDATA treatments that we consider in Experiment 2, indicating a robust presence of (at least) partial naivety in the average individual.

[37] In 3-DICE17 PASTDATA, a risk-neutral Bayesian would invest for more than half of their investment opportunities. This is in contrast to our other treatments in which a risk-neutral Bayesian would invest for fewer than half of their investment opportunities. This implies that a bias in investment "towards the center" would lead to different predictions (relative to naivety) in the 3-DICE17 PASTDATA and 3-DICE PASTDATA treatments.

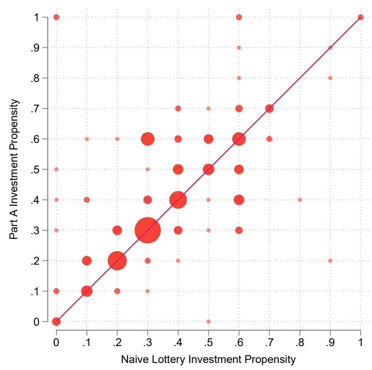
success associated with each dice value. This means that simply comparing an individual's investment propensity in the different parts of the experiment reveals whether their beliefs in the investment game are closer to the Bayesian probabilities or the fully Naive benchmark probabilities.

Figure 7 plots the propensity to invest in the 3-DICE PASTDATA investment game relative to the Bayesian lottery (Panel 7a) and relative to the Naive lottery (Panel 7b). The figure shows that the majority (83%) of participants are above the diagonal in Panel 7a, implying that they invest more in the investment game than in the Bayesian lottery. In contrast, in Panel 7b, the observations appear to be distributed much closer to the diagonal, with 54% lying on the diagonal. This suggests that in the investment game individuals typically hold beliefs that are closer to the naive probabilities than the Bayesian probabilities.

Figure 7: Individual investment propensities (Part A vs B/C; 3D PASTDATA)



(a) **Bayesian lottery comparison**

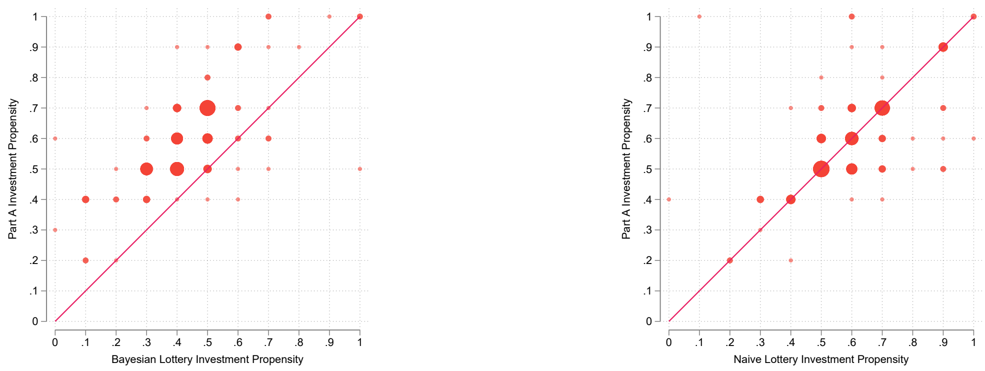


(b) **Naive lottery comparison**

To check whether this pattern of behavior is robust to variation in the features of the choice environment, Figure 8 presents the results for the same exercise for the 3-DICE17 PASTDATA treatment.[38] As expected, lowering the threshold for a successful project to 17 in the investment game leads to an increase in the investment propensity in all three parts of the experiment (since the Bayesian and Naive probabilities are also increased as a direct result of the threshold reduction). Nevertheless, the basic pattern is replicated: the majority (82%) of observations in Panel 8a lie above the 45-degree line, while those in Panel 8b appear to be distributed around the 45-degree line, with 53% lying on the diagonal.

[38]We chose to examine this threshold reduction because it introduces a meaningful change in the decision context by moving the risk-neutral Bayesian benchmark from investing in fewer than half of the available investment opportunities to investing in more than half of the available investment opportunities. Appendix D.4 provides a discussion of how reducing the threshold for success leads to behavior in the investment game that is closer to that in the Bayesian lottery in the absence of past data (i.e., in the 3-DICE17 NOPASTDATA treatment relative to the 3-DICE NOPASTDATA). We added the 3-DICE17 NOPASTDATA to examine precisely this empirical regularity of our previous treatments, namely over-investment in the absence of past data. Given this observed difference between the two treatments *without past data*, the regularity in the pattern of behavior observed when past data is added provides further support for our core findings.

Figure 8: Individual investment propensities (Part A vs B/C; 3D17 PastData)



(a) **Bayesian lottery comparison** (b) **Naive lottery comparison**

While these figures provide a useful visual illustration of the overall pattern of investment behavior across the three parts of the experiment using pairwise comparisons between Part A and Part B/C, in order to properly classify individuals, we want to combine their investment choices from all three parts. Table 4 reports the results from such a classification exercise. Column (1) reports the classification results for the 3-Dice PastData treatment, while column (2) reports those for the 3-Dice17 PastData treatment. The results are remarkably similar, with the majority (approximately 80%) of the individuals in each treatment classified as either partially naive, exactly naive, or extremely naive.[39]

Table 4: Classification of individuals into discrete types (percentages).

	3-Dice PD (1)	3-Dice17 PD (2)
Less than Bayes ($\tilde{n}_i < n_i^{BL} < n_i^{NL}$)	3.5	5.0
Exactly Bayes ($\tilde{n}_i = n_i^{BL} < n_i^{NL}$)	3.0	6.0
Bayes = Naive = Invest ($\tilde{n}_i = n_i^{BL} = n_i^{NL}$)	5.9	4.0
Partially Naive ($n_i^{BL} < \tilde{n}_i < n_i^{NL}$)	8.4	13.0
Exactly Naive ($n_i^{BL} < \tilde{n}_i = n_i^{NL}$)	47.0	48.0
Extremely Naive ($n_i^{BL} < n_i^{NL} < \tilde{n}_i$)	24.8	18.0
Violates Stochastic Dominance ($n_i^{BL} > n_i^{NL}$)	7.4	6.0
Observations (N)	202	100

Notes: (i) The table provides a classification of individuals into discrete types according to their investment behavior in Part A, B and C of Experiment 2, (ii) The columns report the distribution of types in percentage points, (iii) In the Appendices, we replicate this classification exercise for the 2-Dice PastData treatment and for the four 3-Dice22 PastData treatments, pooled together (see Table 20). The results are very similar, with the exception that there is less naivety in 2-Dice PastData, (iv) It is worth noting that the classification in this table is finer than in Figure 7 above. This is why we see 47% classified as "Exactly Naive" here, while 54% lie on the diagonal in Figure 7. For example, an individual who is on the diagonal in Figure 7 may violate stochastic dominance ($n_i^{BL} > n_i^{NL} = \tilde{n}_i$) or invest the same in the investment game as in the Bayesian lottery ($\tilde{n}_i = n_i^{BL} = n_i^{NL}$).

[39]In the Appendices, we replicate this classification exercise for the 2-Dice PastData treatment and for the four 3-Dice22 PastData treatments, pooled together (see Table 20). The results are very similar, with the exception that there is less naivety in 2-Dice PastData.

This evidence suggests that our aggregate-level results are not driven by a small fraction of individuals that display naivety. The majority of individuals display at least some degree of naivety, resulting in upward-biased beliefs and over-investment in the investment game.

5.2.4 Potential mechanisms

In addition to providing a robustness check for the presence of naivety and facilitating the individual analysis, Experiment 2 was also designed to shed light on the contextual factors that might amplify or mitigate the biasing effect that observing selected data can have on belief formation. In a series of treatments, we explore how the manifestation of naivety about selected data is influenced by (i) the complexity of the DGP, and (ii) the features of the information that the decision-maker can draw on when forming a mental representation of the DGP. Appendix D.2 provides a detailed description of the pre-registered hypotheses and Appendix D.3 describes the associated results. This analysis yields two findings regarding the drivers of naivety.

First, comparing the 3-Dice PastData and 2-Dice PastData treatments shows that reducing the complexity of the DGP reduces the prevalence of naivety. This is indicated by the shift in the average μ^n from -0.09 to 0.27, implying a statistically significant difference of 0.36 ($p < 0.01$).[40]

Result B. 2. *Participants become less naive as the complexity of the data generating process is reduced—the average participant in the 2-Dice PastData treatment is closer to the behavior of a Bayesian agent than the average participant in the 3-Dice PastData treatment.*

Second, we find that naivety is robust to varying the precise information that decision-makers have access to when they are forming a mental representation of the DGP. By holding the DGP constant across treatment conditions, we examine whether adding or removing particular pieces of information serves to exacerbate or ameliorate naivety. We find that when the DGP is non-trivial (as is the case in our 3-dice treatments), manipulating the difficulty of drawing inference directly from the DGP information does not affect investment behavior. Likewise, providing participants with cues designed to prompt them to think about the problem more thoroughly fails to diminish naivety. These observations suggest that participants encounter cognitive limitations when constructing a mental model that maps the DGP to the (subjective) success probabilities. These results suggest that naivety about selection is a robust phenomenon that is resistant to mitigation, particularly in complex scenarios where extracting the relevant Bayesian probabilities is challenging. Unfortunately, such circumstances arise commonly in real-world settings.

[40]While we do observe evidence that reducing the complexity of the data-generating process results in a significant reduction in naivety, it is important to also note that the μ^n observed in the 2-Dice PastData treatment is still far away from the Bayesian benchmark of $\mu^n = 1$, indicating that there remains a large amount of naive investment behavior even in this setting with a simpler DGP.

Result B. 3. *When the DGP is complex, participants do not become less naive when provided with additional information or cues about the DGP. Instead, the majority of individuals appear to be willing to follow past data naively.*

6 Conclusion

The information revolution of recent decades has led to an explosion of easily available, large data sets. In particular, we are increasingly able to observe both objective and subjective outcomes (e.g. online reviews) of other people's choices relating to a wide range of decisions.[41] This information can help us to make better decisions by learning from the mistakes and successes of others. However, it comes with the challenge of drawing accurate inferences from the data we observe. In particular, in many cases, the outcome of interest is only observed for a specific subset of individuals: those who chose the option about which we want to learn. It is, therefore, imperative that we understand how individuals learn from such selected data and whether they are aware of the mistakes that naive extrapolation can generate.

This paper presents the results from two experiments—a laboratory experiment focusing on the equilibrium and comparative static predictions of Jehiel (2018) and an online experiment zooming in on individual behavior. In Experiment 1, we find that behavior is largely in line with the model's predictions. Investment behavior is compatible with selection neglect, and the strength of the bias varies with others' information and sophistication as predicted by the model.

In Experiment 2 on Prolific, we implement a simplified non-strategic version of the investment problem from Experiment 1. By including near-identical lottery tasks with known probabilities, we are able to cleanly isolate the role of biased beliefs. This provides a robustness check for the central psychological mechanism in Experiment 1—the presence of naive extrapolation. In addition, Experiment 2 allows us to classify participants according to the degree of bias they display. The majority of participants display substantial levels of selection neglect. Examining the potential moderators of this bias, we find that the manifestation of naive extrapolation varies with the complexity of the data-generating process. However, even in the simplest version of our investment problem selection neglect shapes the behavior of the vast majority of participants.

The strength and robustness of our findings may appear to be reasons for concern. Many of the most important decisions we make in our lives (our college choices, career choices, deciding whether and where to buy a house, whether to switch jobs, get married, get divorced, or have children) involve large forks in the road, leading to different paths through life. The information about these paths is necessarily selected since only a subset of individ-

[41]Note, online reviews are only observed for the subset of individuals who chose (i) to buy the product, or participate in the activity, and then (ii) to write a review.

uals select into a given path. If individuals systematically neglect to take into account this selection when interpreting the data they have access to, this may bias their beliefs about key life-changing decisions.

This raises the question of whether and how the bias can be assuaged. Unfortunately, our data suggest this is a challenging task, particularly in situations where the data generating process is non-trivial. While reducing the complexity of a problem appears to dampen the bias, we have not found an intervention that eliminates it. Moreover, many of the infrequent life decisions mentioned above are complex in nature.

It seems that while we are all "everyday econometricians" in our daily lives, like econometricians of the past, from time to time, we fall into the trap of neglecting the influence of selection in the data we observe, causing biased inference and suboptimal decision making. This highlights a need for improved data literacy in the general population and appropriate adjustments to educational curricula.

References

Araujo, F., S. Wang, and A. Wilson (2021). The times they are a-changing: Dynamic adverse selection in the laboratory. *American Economic Journal: Microeconomics 13*(4), 1–22.

Backhaus, T., C. Schäper, and A. Schrenker (2023). Causal misperceptions of the part-time pay gap. *Labour Economics*, 102396.

Barron, K. and T. Fries (2023). Narrative persuasion. *CESifo Working Paper No. 10206*.

Bénabou, R. and J. Tirole (2002). Self-confidence and personal motivation. *Quarterly Journal of Economics 117*(3), 871–915.

Brown, S. J., W. Goetzmann, R. G. Ibbotson, and S. A. Ross (1992). Survivorship bias in performance studies. *Review of Financial Studies 5*(4), 553–580.

Brundage, M., A. T. Little, and S. S. You (2022). Selection neglect and political beliefs. *OSF Preprints*.

Camerer, C. and D. Lovallo (1999). Overconfidence and excess entry: An experimental approach. *American Economic Review 89*(1), 306–318.

Chen, D. L., M. Schonger, and C. Wickens (2016). oTree: An open-source platform for laboratory, online, and field experiments. *Journal of Behavioral and Experimental Finance 9*, 88–97.

Compte, O. and A. Postlewaite (2004). Confidence-enhanced performance. *American Economic Review 94*(5), 1536–1557.

Cooper, A. C., C. Y. Woo, and W. C. Dunkelberg (1988). Entrepreneurs' perceived chances for success. *Journal of Business Venturing 3*(2), 97–108.

Dawson, C., D. de Meza, A. Henley, and G. R. Arabsheibani (2014). Entrepreneurship: cause and consequence of financial optimism. *Journal of Economics & Management Strategy 23*(4), 717–742.

Denrell, J. (2003). Vicarious learning, undersampling of failure, and the myths of management. *Organization Science 14*(3), 227–243.

Eil, D. and J. Rao (2011). The good news-bad news effect: Asymmetric processing of objective information about yourself. *American Economic Journal: Microeconomics 3*(2), 114–138.

Enke, B. (2020). What you see is all there is. *Quarterly Journal of Economics 135*(3), 1363–1398.

Enke, B. and T. Graeber (2023). Cognitive uncertainty. *Quarterly Journal of Economics (forthcoming)*.

Enke, B. and F. Zimmermann (2019). Correlation neglect in belief formation. *Review of Economic Studies 86*(1), 313–332.

Esponda, I. (2008). Behavioral equilibrium in economies with adverse selection. *American Economic Review 98*(4), 1269–1291.

Esponda, I. and D. Pouzo (2017). Conditional retrospective voting in large elections. *American Economic Journal: Microeconomics 9*(2), 54–75.

Esponda, I. and E. Vespa (2018). Endogenous sample selection: A laboratory study. *Quantitative Economics 9*(1), 183–216.

Eyster, E. and M. Rabin (2005). Cursed equilibrium. *Econometrica 73*(5), 1623–1672.

Eyster, E., M. Rabin, and G. Weizsacker (2015). An experiment on social mislearning. *Working Paper*.

Eyster, E. and G. Weizsacker (2010). Correlation neglect in financial decision-making. *Working Paper*.

Feld, J., E. Ip, A. Leibbrandt, and J. Vecci (2022). Identifying and overcoming gender barriers in tech: A field experiment on inaccurate statistical discrimination. *CESifo Working Paper 9970*.

Gödker, K., T. Odean, and P. Smeets (2023). Disposed to be overconfident. *CESifo Working Paper No. 10357*.

Graeber, T. (2023). Inattentive inference. *Journal of the European Economic Association 21*(2), 560–592.

Greiner, B. (2015). Subject pool recruitment procedures: organizing experiments with orsee. *Journal of the Economic Science Association 1*(1), 114–125.

Guarino, A., M. Angrisani, P. Jehiel, and T. Kitagawa (2021). Information redundancy neglect versus overconfidence: A social learning experiment. *American Economic Journal: Microeconomics 13*(3), 163–197.

Heckman, J. (1990). Varieties of selection bias. *American Economic Review 80*(2), 313–318.

Heckman, J. J. (1979). Sample selection bias as a specification error. *Econometrica 47*(1), 153–161.

Hübert, R., A. T. Little, et al. (2023). A behavioral theory of discrimination in policing. *Mimeo*.

Jehiel, P. (2005). Analogy-based expectation equilibrium. *Journal of Economic Theory 123*(2), 81–104.

Jehiel, P. (2018). Investment strategy and selection bias: An equilibrium perspective on overoptimism. *American Economic Review 108*(6), 1582–1597.

Jehiel, P. and F. Koessler (2008). Revisiting games of incomplete information with analogy-based expectations. *Games and Economic Behavior 62*(2), 533–557.

Jin, G. Z., M. Luca, and D. Martin (2021). Is no news (perceived as) bad news? an experimental investigation of information disclosure. *American Economic Journal: Microeconomics 13*(2), 141–73.

Koehler, J. J. and M. Mercer (2009). Selection neglect in mutual fund advertisements. *Management Science 55*(7), 1107–1121.

Koellinger, P., M. Minniti, and C. Schade (2007). 'I think I can, I think I can': Overconfidence and entrepreneurial behavior. *Journal of Economic Psychology 28*(4), 502–527.

Logg, J. M., U. Haran, and D. A. Moore (2018). Is overconfidence a motivated bias? experimental evidence. *Journal of Experimental Psychology: General 147*(10), 1445.

Malmendier, U. and G. Tate (2005). Does overconfidence affect corporate investment? CEO overconfidence measures revisited. *European Financial Management 11*(5), 649–659.

Martínez-Marquina, A., M. Niederle, and E. Vespa (2019). Failures in contingent reasoning: The role of uncertainty. *American Economic Review 109*(10), 3437–74.

Möbius, M. M., M. Niederle, P. Niehaus, and T. S. Rosenblat (2022). Managing self-confidence: Theory and experimental evidence. *Management Science 68*(11), 7793–7817.

Moore, D. A. and P. J. Healy (2008). The trouble with overconfidence. *Psychological review 115*(2), 502.

Ngangoué, M. K. and G. Weizsäcker (2021). Learning from unrealized versus realized prices. *American Economic Journal: Microeconomics 13*(2), 174–201.

Puri, M. and D. T. Robinson (2007). Optimism and economic choice. *Journal of Financial Economics 86*(1), 71–99.

Samuelson, W. F. and M. H. Bazerman (1985). Negotiation under the winner's curse. *Research in Experimental Economics 3*, 105–38.

Schwardmann, P. and J. Van der Weele (2019). Deception and self-deception. *Nature Human Behaviour 3*(10), 1055–1061.

Selten, R. (1967). Die strategiemethode zur erforschung des eingeschränkt rationalen verhaltens im rahmen eines oligopolexperiments. In H. Sauermann (Ed.), *Beiträge zur experimentellen Wirtschaftsforschung*, pp. 136–168. Tübingen: Mohr.

Spiegler, R. (2011). *Bounded rationality and industrial organization*. Oxford University Press.

Von Hippel, W. and R. Trivers (2011). Reflections on self-deception. *Behavioral and Brain Sciences 34*(1), 41–56.

Zimmermann, F. (2020). The dynamics of motivated beliefs. *American Economic Review 110*(2), 337–61.

ONLINE APPENDICES

Appendix A: Theory — some further details

A.1 The naive extrapolation equilibrium in each scenario

Scenario 1: Learning from others

In this case, a symmetric pure strategy equilibrium would require that an investor of type X invests if she gets a signal $s_X \geq s^{SN1}$. Thus $q_{inv}^{SN1}(s^*, s_B, s_C) = 1$ if $s_B, s_C \geq s^{SN1}$, $q_{inv}^{SN1}(s^*, s_B, s_C) = 0.5$ if $s_B \geq s^{SN1} > s_C$ or $s_C \geq s^{SN1} > s_B$ and $q_{inv}^{SN1}(s^*, s_B, s_C) = 0$ otherwise where s^{SN1} should be such that $\widehat{P}(\bar{x}|s^{SN1}; q_{inv}^{SN1}) \cdot \overline{x} > c$ and $\widehat{P}(\bar{x}|s^{SN1} - 1; q_{inv}^{SN1}) \cdot \overline{x} < c$ if $s^{SN1} > 1$.

Given the symmetry of the problem, it is readily verified that $\widehat{P}(\bar{x}|s_A; q_{inv}^{SN1})$ simplifies into $P(\bar{x}|s_A; s_B \geq s^{SN1})$ which is clearly larger than $P(\bar{x}|s_A)$ (because the extra conditioning on $s_B \geq s^{SN1}$ shifts the probability of success upwards). This in turn implies that there is more investment in the equilibrium with naive investors in scenario 1 (in comparison to the Bayesian benchmark). We have $s^{SN1} = 6$ when $\bar{x} = 3.40$ and $c = 1$.

Scenario 2: Learning from others with correlated signals

When all investors are of type A, if agents follow the threshold strategy to invest if s_A is no smaller than s^{SN2}, we would have $q_{inv}^{SN2}(s^*, s_B, s_C) = 1$ if $s^* \geq s^{SN2}$ and 0 otherwise. This implies that for all $s_A \geq s^{SN2}$, $\widehat{P}(\bar{x}|s_A; q_{inv}^{SN2}) = P(\bar{x}|s_A)$ and thus it cannot be that $s^{SN2} < s^{Bayes}$ (given that $P(\bar{x}|s_A)\bar{x} < c$ for any $s_A < s^{Bayes}$). When signals are perfectly correlated among investors, there cannot be overinvestment in equilibrium, and imposing some exogenous trembling would force $s^{SN2} = s^{Bayes}$.

Scenario 3: Learning from better-informed individuals

In this case investors of a type A face in their feedback projects that were handled either by naive investors of type B or C, or, by omniscient investors that would invest only if the project is successful (the latter are implemented through machines). Letting λ denote the proportion of non-omniscient investors, and letting s^{SN3} denote their equilibrium threshold, one would have $q_{inv}^{SN3}(s^*, s_B, s_C) = \lambda q^C(s^*, s_B, s_C) + (1-\lambda) q^R(s^*, s_B, s_C)$ where $q^C(s^*, s_B, s_C) = 1$ if $s_B, s_C \geq s^{SN3}$, $q^C(s^*, s_B, s_C) = 0.5$ if $s_B \geq s^{SN3} > s_C$ or $s_C \geq s^{SN3} > s_B$ and $q^C(s^*, s_B, s_C) = 0$ otherwise; $q^R(s^*, s_B, s_C) = 1$ if $s^* + s_B + s_C \geq W$ and 0 otherwise, and s^{SN3} should be such that $\widehat{P}(\bar{x}|s^{SN3}; q_{inv}^{SN3}) \cdot \overline{x} > c$ and $\widehat{P}(\bar{x}|s^{SN3} - 1; q_{inv}^{SN3}) \cdot \overline{x} < c$ if $s^{SN3} > 1$.

It can be shown that when there are more fully informed omniscient investors around (when λ is smaller), the overinvestment bias increases, i.e., s^{SN3} gets smaller. When $\lambda = 1/2$,

$\bar{x} = 3.40$ and $c = 1$ (as in our experiment), we have a symmetric equilibrium in mixed strategies, where players mix between playing $s^{SN3} = 5$ with probability $\mu = 0.8$ and playing $s^{SN3} = 6$ with probability $1 - \mu = 0.2$.[42]

Scenario 4: Learning from others when also observing the counterfactual

Here, type A investors observe feedback about all projects faced by type B or C investors, irrespective of whether they invested or not. This implies that while Naive Extrapolators still form their belief about the mapping from signals to success probabilities, $\widehat{P}(\cdot|\cdot)$, according to equation 2, they now observe the outcomes of past projects with probability one. Therefore, q_{inv} should be replaced by $q = 1$ in equation 2, and Naive Extrapolators form a belief $\widehat{P}(\bar{x}|s^*; q = 1)$, which in expectation is equal to the Bayesian posterior, $P(\bar{x}|s^*)$. Consequently, $s^{SN4} = s^{Bayes}$.

[42]Note, λ refers to the fraction of fully informed omniscient investors amongst those who are generating the feedback. This is why $\lambda = 1/2$ is relevant in relation to our experimental design, and not $\lambda = 1/3$.

A.2.1 Calculating the equilibrium in scenario 1

As noted above, without loss of generality, due to the symmetry of the game, we can consider the perspective of a type A agent. Making use of the two observations that: (i) $P(\bar{x}|s_A; s_B \geq s^{SN1}) = \sum_{s_B} P(\bar{x}|s_A; s_B)$, and (ii) $P(\bar{x}|s_A; s_B) = \frac{s_A+s_B-11}{10}$ if $s_A+s_B \geq 12$ and 0 otherwise, equation 2 can be expanded using the following analytical expression:

$$
\begin{aligned}
\widehat{P}(\bar{x}|s_A; q_{inv}^{SN1}) &= P(\bar{x}|s_A; s_B \geq s^{SN1}) \\
&= \frac{(\widetilde{s^{SN1}} - 11) \cdot (2s_A + \widetilde{s^{SN1}} - 12)}{20 \cdot (s^{SN1} - 11)}
\end{aligned}
\tag{4}
$$

where

$$
\widetilde{s^{SN1}} := \begin{cases} s^{SN1} & \text{if } s_A + s^{SN1} \geq 12 \\ 12 - s_A & \text{if } s_A + s^{SN1} < 12 \end{cases}
$$

This allows us to construct a table consisting of the expected fraction of successful projects in a type A agent's feedback, conditional on signal s_A and the threshold strategy being used by the other agents, s^{SN1} (see Table 5 below). Therefore, each column of the table reports the success fractions that a type A agent would observe in her feedback if all other players were following a specific strategy s^{SN1}, as the number of projects in her feedback gets large.

To illustrate this, let us consider how the feedback of a type A agent changes, depending on the strategy followed by the agents generating her feedback. The right-most column shows the success fractions observed if the feedback is generated by players who only invest after receiving a signal of 10. For a type A agent with this type of feedback, on average 50% of the projects that she observes with $s_A = 6$ are successful projects. In contrast, the left-most column shows the success fractions observed if the feedback is generated by players who invest for a signal of 1 or higher (i.e. they always invest). For a type A agent with this type of feedback, on average 15% of the projects that she observes with $s_A = 6$ are successful projects. The success fractions in this left-most column coincide with the Bayesian success probabilities (since there is no selection in the feedback).

Table 5: Expected success fractions for observed projects

	$s^{SN1} =$									
	1	2	3	4	5	6	7	8	9	10
$s_A =$										
1	0	0	0	0	0	0	0	0	0	0
2	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.03	0.05	0.1
3	0.03	0.03	0.04	0.04	0.05	0.06	0.08	0.1	0.15	0.2
4	0.06	0.07	0.08	0.09	0.1	0.12	0.15	0.2	0.25	0.3
5	0.1	0.11	0.13	0.14	0.17	0.2	0.25	0.3	0.35	0.4
6	0.15	0.17	0.19	0.21	0.25	**0.3**	0.35	0.4	0.45	0.5
7	0.21	0.23	0.26	0.3	0.35	0.4	0.45	0.5	0.55	0.6
8	0.28	0.31	0.35	0.4	0.45	0.5	0.55	0.6	0.65	0.7
9	**0.36**	0.4	0.45	0.5	0.55	0.6	0.65	0.7	0.75	0.8
10	0.45	0.5	0.55	0.6	0.65	0.7	0.75	0.8	0.85	0.9

Bayesian agents therefore use the probabilities in the left-most column and invest for all signals s_A such that $P(\bar{x}|s_A) > \frac{c}{x} \approx 0.294$. Clearly, this inequality is only satisfied for $s_A = 9$ and $s_A = 10$, implying that the risk neutral Bayesian agent will follow the threshold strategy $s^{Bayes} = 9$.

To find the symmetric equilibrium investment strategy for naive investors, recall from the main text that the symmetric pure strategy equilibrium is given by s^{SN1} should be such that $\widehat{P}(\bar{x}|s^{SN1}; q_{inv}^{SN1}) > \frac{c}{x}$ and $\widehat{P}(\bar{x}|s^{SN1} - 1; q_{inv}^{SN1}) < \frac{c}{x}$ if $s^{SN1} > 1$. Since one can read the beliefs of a naive investor, $\widehat{P}(\bar{x}|s_A; q_{inv}^{SN1}) = P(\bar{x}|s_A; s_B \geq s^{SN1})$, directly off Table 5, one can see that the symmetric equilibrium in pure strategies is given by $s^{SN1} = 6$. The logic is as follows: given that $\frac{c}{x} = \frac{1}{3.4} \approx 0.294$, moving down the $s^{SN1} = 6$ column in Table 5 shows that the Type A individual who holds these beliefs will invest for $s_A = 6$, but not for $s_A = 5$.

The information contained in Table 5 can be depicted graphically as in Figure 9. This visualisation of the data provides a second illustration of the equilibrium threshold strategies followed by the Bayesian and naive investor. In Figure 9, the Bayesian investor's beliefs are represented by the lowest (blue) curve.[43] The ratio $\frac{c}{x} \approx 0.294$ is drawn as a horizontal (light blue) line. The Bayesian agent invests for signals which yield a belief about the probability of success that exceeds the ratio $\frac{c}{x}$ (i.e. when the dark blue curve is above the light blue horizontal line). Clearly this is the case for signals $s_A = 9$ and $s_A = 10$.

For the naive investor, each of the curves in Figure 9 depict her beliefs as a function of the

[43]This lowest (blue) curve depicts the information contained in the left-most column of Table 5.

threshold strategy being followed by the individuals generating her feedback. For example, the top-most curve reflects the naive investors beliefs when all other agents are following a threshold strategy, where they only invest after a signal of 10, but not for lower signals. Looking at the $s^{SN1} = 6$ curve shows that the individual will invest for $s_A = 6$, but not for $s_A = 5$ (since the first belief is above the horizontal line, while the second is below).

Figure 9: Selection Neglect Agent's Perceived Success Probabilities

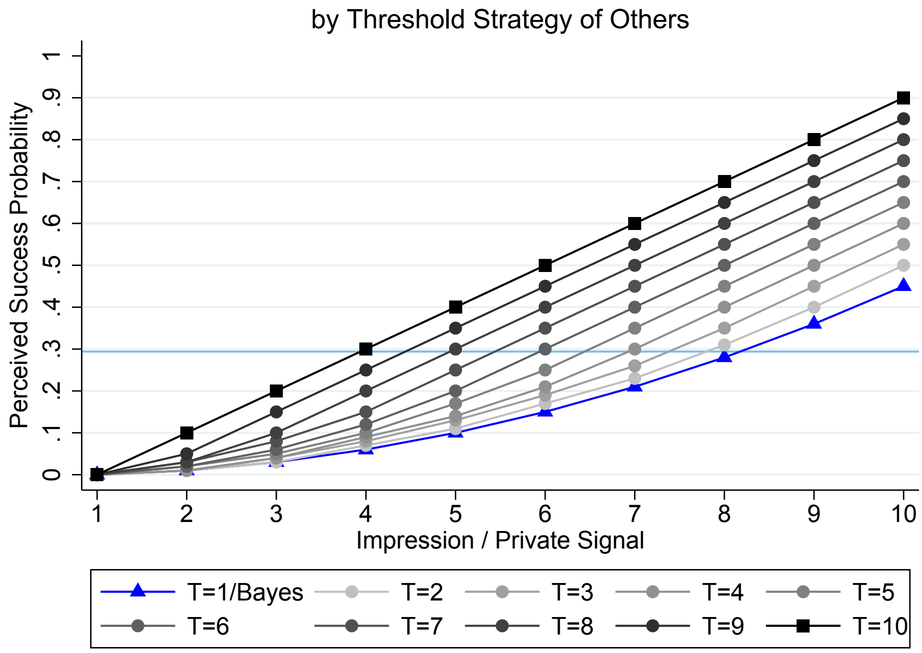


Figure 9 also provides an illustration of how individuals can exert an externality on others through the feedback generated. As the threshold strategy followed by other agents moves upwards, the naive agent's beliefs become increasingly distorted. This leads to poorer decision making by the naive agent. For example, if other agents are following the rational threshold strategy, $s^{Bayes} = 9$, the naive agent will hold highly distorted beliefs (i.e., refer to the distance between the T=1/ Bayes curve and the T=9 curve).

A.2.2 Calculating the equilibrium in scenario 2

As discussed in the main text above, in scenario 2, for all $s_A \geq s^{SN2}$, $\widehat{P}(\bar{x}|s_A; q_{inv}^{SN2}) = P(\bar{x}|s_A)$. The feedback received when there are perfectly correlated signals symmetric play is shown in Table 6. However, allowing for some trembling would imply that every column of the table is equivalent to the left-most column which contains the Bayesian success probabilities, conditional on s_A. Therefore, in this scenario risk neutral naive investor chooses $s^{SN2} = s^{Bayes}$.

Table 6: Expected success fractions for observed projects

	$s^{SN2} =$									
	1	2	3	4	5	6	7	8	9	10
$s_A =$										
1	0									
2	0.01	0.01								
3	0.03	0.03	0.03							
4	0.06	0.06	0.06	0.06						
5	0.1	0.1	0.1	0.1	0.1					
6	0.15	0.15	0.15	0.15	0.15	0.15				
7	0.21	0.21	0.21	0.21	0.21	0.21	0.21			
8	0.28	0.28	0.28	0.28	0.28	0.28	0.28	0.28		
9	**0.36**	0.36	0.36	0.36	0.36	0.36	0.36	0.36	**0.36**	
10	0.45	0.45	0.45	0.45	0.45	0.45	0.45	0.45	0.45	0.45

A.2.3 Calculating the equilibrium in scenario 3

Recognizing that the omniscient players invest iff a project is successful gives $\mathbb{E}_{s_B,s_C}[q^R(s_A,s_B,s_C)] = P(\bar{x}|s_A)$, which implies that:

$$\begin{aligned}
\widehat{P}(\bar{x}|s_A;q_{inv}^{SN3}) &= \frac{\sum_{s_B}[(1-\lambda)\cdot P(\bar{x}|s_A;s_B)\cdot 1_{s_B\geq s^{SN3}}+\lambda\cdot P(\bar{x}|s_A)]}{\sum_{s_B}[(1-\lambda)\cdot 1_{s_B\geq s^{SN3}}+\lambda\cdot P(\bar{x}|s_A)]} \\
&= \frac{(1-\lambda)\cdot \widehat{P}(\bar{x}|s_A;q_{inv}^{SN1}=q_{inv}^{SN3})\cdot(11-s^{SN3})+\lambda\cdot P(\bar{x}|s_A)\cdot 10}{(1-\lambda)\cdot(11-s^{SN3})+\lambda\cdot P(\bar{x}|s_A)\cdot 10} \\
&= \frac{(1-\lambda)[(11-\widetilde{s^{SN3}})\cdot(s_A-11)+\frac{1}{2}(10\cdot 11-\widetilde{s^{SN3}}(\widetilde{s^{SN3}}-1))]+\lambda\cdot\frac{s_A^2-s_A}{2}}{(1-\lambda)\cdot 10\cdot(11-s^{SN3})+\lambda\cdot\frac{s_A^2-s_A}{2}}
\end{aligned} \tag{5}$$

where

$$\widetilde{s^{SN3}} := \begin{cases} s^{SN3} & \text{if } s_A+s^{SN3}\geq 12 \\ 12-s_A & \text{if } s_A+s^{SN3}<12 \end{cases}$$

Using the parameterization relevant for our experimental design (i.e. $\lambda=\frac{1}{2}$) yields the following table of beliefs of a type A naive investor as a function of the threshold followed by other non-omniscient players, s^{SN3}, and her own signal, s_A.

Table 7: Expected success fractions for observed projects

	$s^{SN3} =$									
	1	2	3	4	5	6	7	8	9	10
$s_A =$										
1	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
2	0.02	0.02	0.02	0.03	0.03	0.04	0.05	0.06	0.10	0.18
3	0.06	0.06	0.07	0.08	0.10	0.11	0.14	0.18	0.26	0.38
4	0.11	0.13	0.14	0.16	0.18	0.21	0.26	0.33	0.42	0.56
5	0.18	0.20	0.22	0.25	**0.29**	**0.33**	0.40	0.48	0.57	0.70
6	0.26	0.29	0.32	0.35	0.40	0.46	0.53	0.60	0.69	0.80
7	0.35	0.38	0.42	0.46	0.52	0.58	0.64	0.71	0.78	0.87
8	0.44	0.47	0.52	0.57	0.63	0.68	0.74	0.79	0.85	0.92
9	0.53	0.57	0.62	0.67	0.72	0.77	0.82	0.86	0.91	0.96
10	0.62	0.67	0.71	0.76	0.80	0.84	0.88	0.92	0.95	0.98

For the Bayesian agent, the information in Table 7 is irrelevant. Instead she relies on her knowledge of the DGP and, as above, uses the probabilities $P(\bar{x}|s_A)$ to guide her decision making (the left-most column of Table 5 contains these probabilities). She therefore again follows the threshold strategy $s^{Bayes} = 9$.

There is no pure strategy symmetric equilibrium for risk neutral naive investors. Rather, there is exists a symmetric equilibrium in mixed strategies in which players mix between $s^{SN3} = 5$ and $s^{SN3} = 6$, such that all players are indifferent between these two strategies.[44] This condition is satisfied when players choose $s^{SN3} = 5$ with probability $\mu = 0.8$.

[44]This is the case when agents are indifferent between investing and not investing after the signal $s_A = 5$ (i.e. when the perceived probability of success is 0.294118). The equilibrium mixture probability is not obtained by simply weighting the two relevant probabilities reported in Table 7, but rather by calculating the equilibrium, allowing for mixing.

A.2.4 Learning dynamics

The above construction can also be used to analyze the learning dynamics (in a similar fashion as what is considered in the online Appendix of Jehiel (2018)).

If we consider the dynamics in which at every round t, players use the feedback generated in round $t-1$ (as considered in Jehiel (2018), then one gets a dynamics of thresholds given by (assuming there is a continuum of investment data in each round) s^{SN1k} where $s^{SN1(k+1)}$ is defined so that for

- $s_A = s^{SN1(k+1)}$, $\widehat{P}((\overline{x} \mid s_A, s_B \geq s^{SN1k}) > \frac{c}{\overline{x}}$ and
- $s_A = s^{SN1(k+1)} - 1$, $\widehat{P}((\overline{x} \mid s_A, s_B \geq s^{SN1k}) < \frac{c}{\overline{x}}$.

This defines a sequence of thresholds parameterized by the first threshold s^{SN11}.

One can check that no matter what s^{SN11} is chosen, this sequence converges to the equilibrium threshold $s^{SN1} = 6$ in just a few rounds. For example, starting from $s^{SN11} = 10$, we get (from inspecting Table 4 or Figure 7), $s^{SN12} = 4, s^{SN13} = 7, s^{SN1k} = 6$ for $k \geq 4$. Starting from $s^{SN11} = 1$, we get $s^{SN12} = 9, s^{SN13} = 5, s^{SN14} = 7, s^{SN1k} = 6$ for $k \geq 5$.

In our experiment, the feedback did not consist only of the projects implemented in the last round but of all projects implemented in earlier rounds. This would lead to modify the dynamics of thresholds s^{SN1k} as follows.[45]

Define

$$\widehat{P}_t(\overline{x} \mid s_A, s^{SN1k}, k < t) = \frac{\sum_{k<t} \widehat{P}((\overline{x} \mid s_A, s_B \geq s^{SN1k}) P(s_B \geq s^{SN1k})}{\sum_{k<t} P(s_B \geq s^{SN1k})}$$

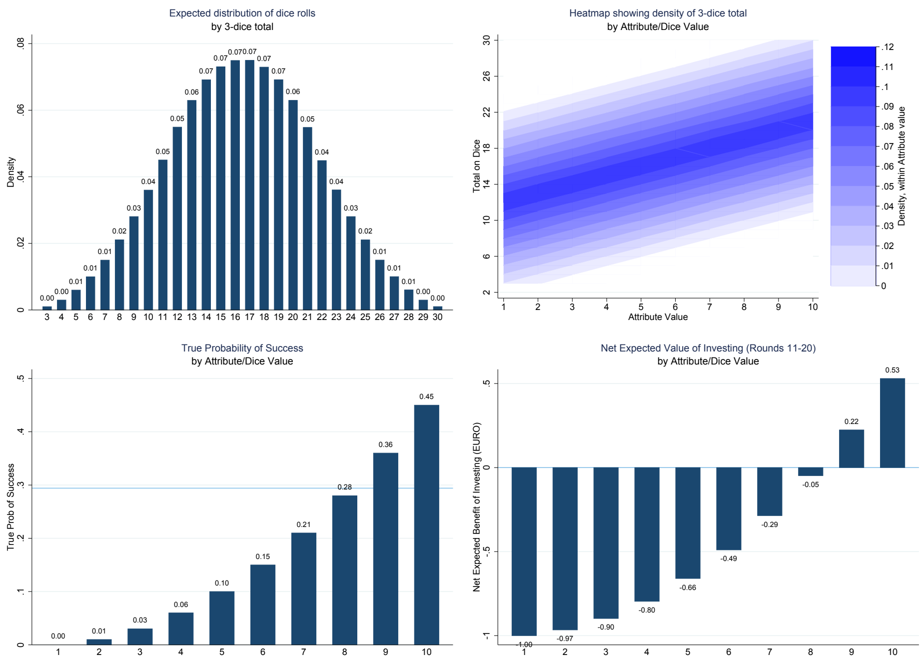
This induces a a value of s^{SN1t} defined so that for $s_A = s^{SN1t}$, $\widehat{P}_t(\overline{x} \mid s_A, s^{SN1k}, k < t) > \frac{c}{\overline{x}}$ and for $s_A = s^{SN1t} - 1$, $\widehat{P}_t(\overline{x} \mid s_A, s^{SN1k}, k < t) < \frac{c}{\overline{x}}$.

One can check again that such a modified dynamics would lead to quick convergence no matter how s^{SN11} is chosen. For example, with $s^{SN11} = 10$, we would have $s^{SN12} = 4, s^{SN1k} = 6$ for $k \geq 3$ (with a convergence one round before it occurs in the previous dynamics).

[45]The various rounds should be weighted by the corresponding mass of implemented projects, hence the terms $P(s_B \geq s^{SN1k})$.

Appendix B: The data generating process (DGP)

Figure 10: Overview of the DGP and Decision Problem



The data generating process is summarized in Figure 10.[46] The top left panel shows the unconditional distribution over the total when rolling three fair ten-sided dice. The top-right panel uses a heatmap to show the conditional distributions of the 3-dice totals, conditional on observing one of the dice values. Since the investor does not really care about the the full distribution of 3-dice totals, but rather cares about how this distribution maps onto the binary success / failure variable, the bottom-left panel displays the probability of success after observing each dice roll from 1 to 10. Given the parameters that we chose in our experiment in the last 10 rounds (i.e. the cost of investing, €1, and value of a successful investment, €3.40), the bottom-right panel then translates this into the net expected value of investing after observing each dice value from 1 to 10. This last panel illustrates that for the rational, risk neutral investor, it is only attractive to invest after observing a value of 9 or 10, although a value of 8 is marginal. A risk averse investor should be even more cautious about investing.

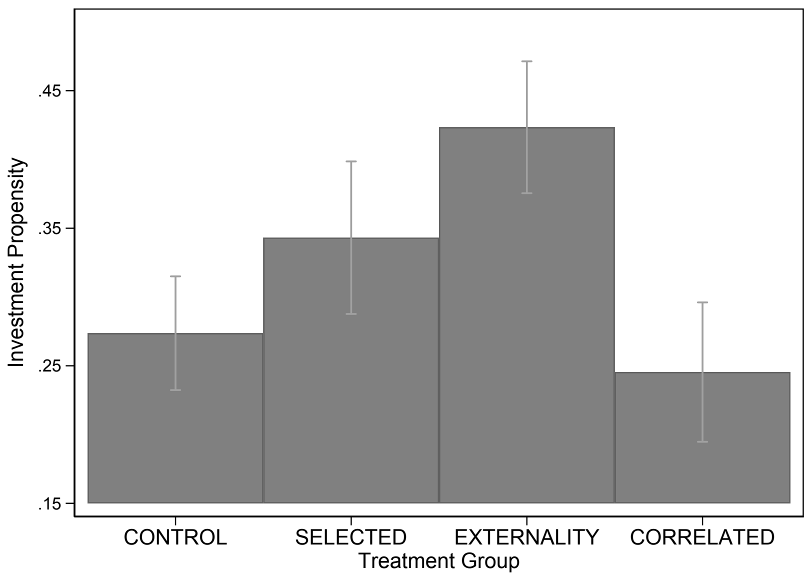
46The figures in the top two panels are generated by simulating 10 million projects and are therefore (fairly precise) approximations of the true distribution. The bottom two panels reflect the precise distribution.

Appendix C: Additional results for Experiment 1

Appendix C.1: Robustness checks

As a robustness check to our main results from Experiment 1[47], we also replicate our analysis for the subset of individuals who are "well behaved" in the sense that they follow a threshold strategy in each of the last five rounds. Restricting attention to this subsample rules out individuals who deviated from following a threshold strategy at least once (e.g. within a given round, invested for an attribute value of s, but did not invest for s' where $s < s'$). This restriction is quite conservative as applies not only to individuals who, (i) made a mistake, or (ii) lacked understanding, but also to those who (iii) understood fully, but simply believed that a lower attribute had a higher probability of leading to a successful project in at least one instance. It is therefore reassuring that the vast majority of participants (i.e. 77%) satisfy this restriction. We refer to this subsample as the "Restricted Sample", both in the regression tables in the main text, and in the figures below. Restricting attention to this subsample does not change any of the results presented above. An additional benefit of focusing on "well behaved" participants, who follow a threshold strategy, is that we can also present some results relating to the threshold strategy chosen. However, since an individual's threshold strategy is mechanically related to her investment propensity, these results do not yield novel insights into behavior.

Figure 11: Investment fraction in last five rounds, by treatment (Restricted Sample)



[47]The main results don't exclude any participants.

Figure 12: Propensity to invest across rounds, by treatment (Restricted Sample)

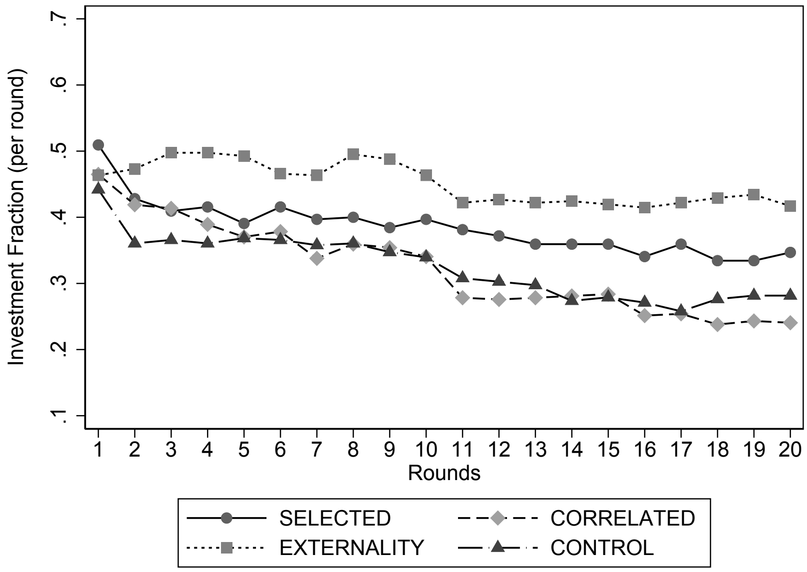
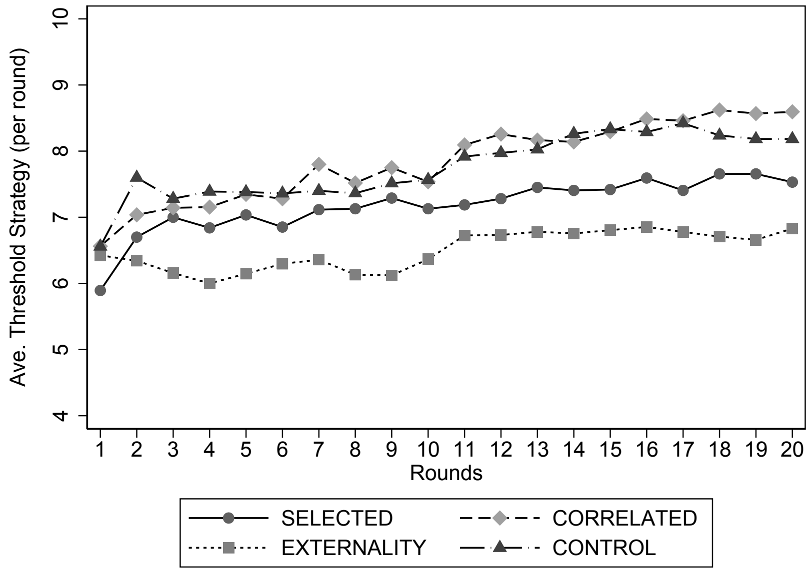


Figure 11 and 12 reproduce figures 3 and 4 respectively, for the restricted sample. The observed pattern of behavior is very similar in the restricted sample to that observed in the full sample. This is supported by the regression results reported in tables 2 and 3, showing similar results for the restricted and full sample. Together, these results suggest that our treatment effects are not driven by individuals who failed to understand the game, or chose their strategy randomly.

Figure 13 shows the average threshold strategy followed across the twenty rounds for the restricted sample. We only present this figure for the restricted sample, as one needs to make further assumptions to assign a threshold to individuals who don't follow a proper threshold strategy. However, in our experimental design, the threshold strategy is the mechanical inverse of the investment propensity for individuals who follow a threshold strategy. Figure 13 uses the average of all individuals in the restricted sample for the last five rounds, but in the first fifteen rounds, additionally, for each round, excludes individuals who didn't follow a threshold strategy in that particular round. This explains why the figure 13 is not a perfect reflected image of figure 12 for the first fifteen rounds.

Figure 13: Average threshold strategy across rounds, by treatment (Restricted Sample)



The manifestation of the treatment effect

The evidence presented in the main text for Experiment 1 focuses on differences in the mean propensity to invest across all signals / attribute values that an individual observes before choosing to invest in a project. To complement this, one may look at the full investment strategies across treatments. For Naive Extrapolators, investment threshold should be shifted downwards in the SELECTED and EXTERNALITY treatments — implying that the treatment effect should be concentrated at intermediate attribute values (e.g. an investor who would invest for all attribute values of 8 or higher in the CONTROL treatment, might, in the SELECTED treatment, instead invest for all attribute values 7 or higher, and in the EXTERNALITY treatment, invest for all attribute values 6 or higher).[48]

[48]That is, in all treatments, low attribute values should be below an individual's threshold, and high attribute values be above their threshold. Therefore, individuals should never invest for these low attribute values, and always invest for high attribute values. A shift downwards in their threshold would increase the propensity to invest at intermediate attribute values.

Figure 14: Investment by attribute value, between treatments (R16-20)

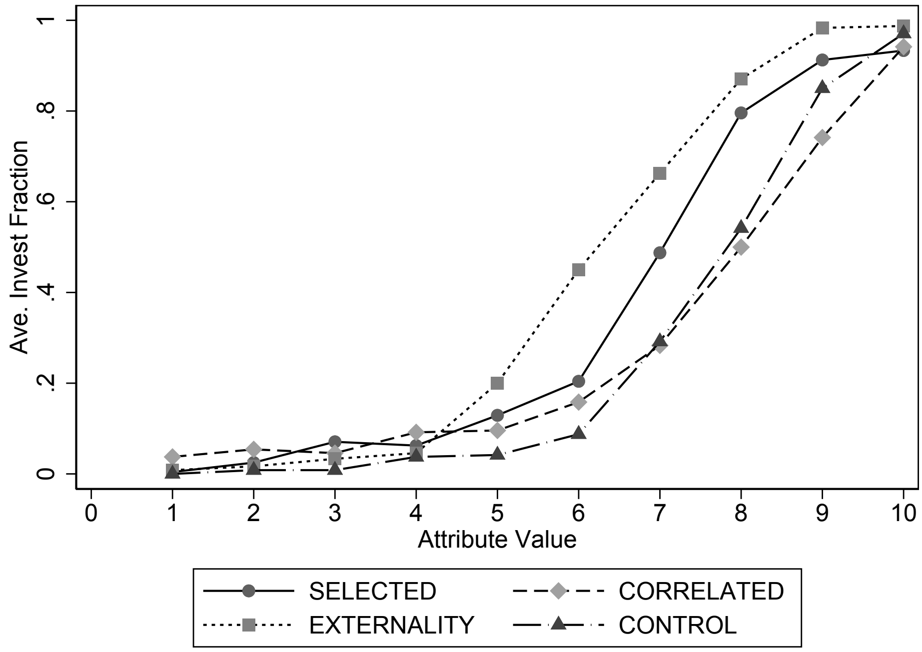


Figure 14 shows that our data is consistent with this, with the treatment differences arising at intermediate attribute values. The figure displays the average participant's propensity to invest in the last five rounds, conditional on each attribute value. Participants appear to have understood the task well as we observe very low investment rates for attributes 1 to 4. Similarly, we observe very high investment rates for attributes 9 and 10. The differences in investment between treatments occurs predominantly between attributes 6 and 8.

Figure 15 shows the average propensity to invest for each attribute value for the restricted sample. It is very similar to figure 14, with the exception that individuals in the restricted sample seem even better at not investing for attribute values 1 to 4 (as one might expect). Figure 16 provides some evidence on heterogeneity in the threshold strategy that participants followed in each of the treatments, but reporting the distribution of thresholds observed in round 20. In the figure, participants are grouped according to the attribute value, s^*, such that they invested for all $s \geq s^*$ in round 20. Therefore, participants with a threshold of $s^* = 11$ did not invest for any attribute value. The figure shows that the treatment shifted the threshold distribution to the left in the SELECTED and EXTERNALITY treatments, relative to the CONTROL and CORRELATED treatments.

Figure 15: Investment by attribute value, between treatments (R16-20, Rest. sample)

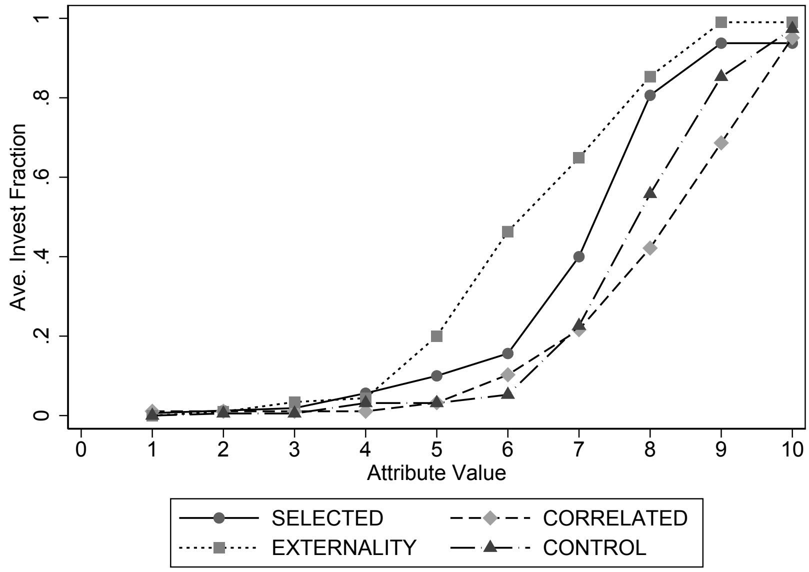
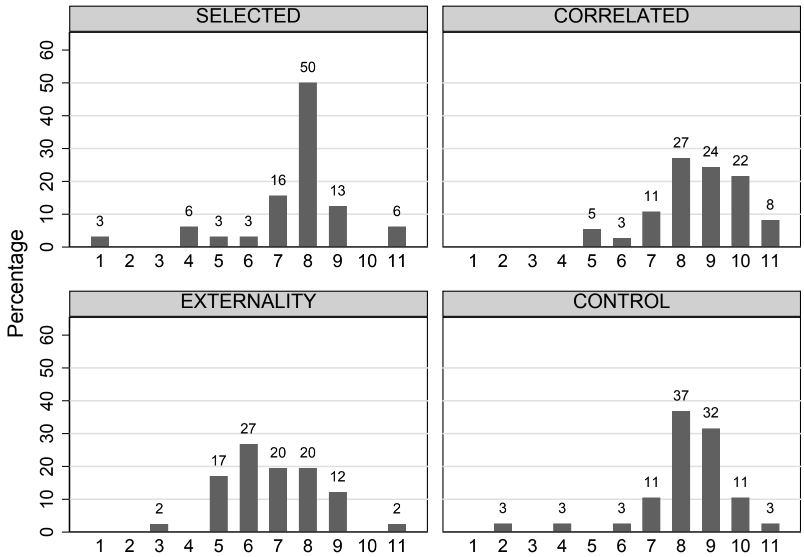


Figure 16: Distribution of threshold strategies in round 20 (Restricted sample)



Appendix C.2: Additional tables and figures from Experiment 1

Table 8: Propensity to invest in SELECTED and EXTERNALITY relative to CORRELATED

	Full Sample		Restricted Sample
	All	R16-20	R16-20
	(1a)	(1b)	(2a)
SELECTED	0.04	0.07*	0.10**
	(0.03)	(0.04)	(0.04)
EXTERNALITY	0.08***	0.13***	0.18***
	(0.03)	(0.03)	(0.04)
Constant	0.37***	0.30***	0.25***
	(0.02)	(0.03)	(0.03)
Observations	144	144	110
Adjusted R^2	0.039	0.091	0.184

Notes: (i) OLS regressions include one observation per individual, (ii) The dependent variable is an individual's average investment propensity, either over all rounds, or over rounds 16-20. (iii) The comparison treatment in the regression is the CORRELATED treatment, (iv) Standard errors are clustered at the interaction group level. This means that in the SELECTED and CORRELATED treatments, there are three individuals per cluster, and in the EXTERNALITY treatment there are two individuals per cluster. The standard errors are reported in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Propensity to invest by treatment (observations for each round)

	Full Sample		Full Sample		Restricted Sample
	All	All	R16-20	R16-20	R16-20
	(1a)	(1b)	(2a)	(2b)	(2c)
SELECTED	0.07***	0.07***	0.08***	0.08***	0.07**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
EXTERNALITY	0.11***	0.11***	0.14***	0.14***	0.15***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
CORRELATED	0.03	0.03	0.01	0.01	-0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Constant	0.34***	0.45***	0.28***	0.28***	0.27***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Round FEs		Y		Y	Y
Observations	3840	3840	960	960	740
Adjusted R^2	0.052	0.115	0.106	0.103	0.167

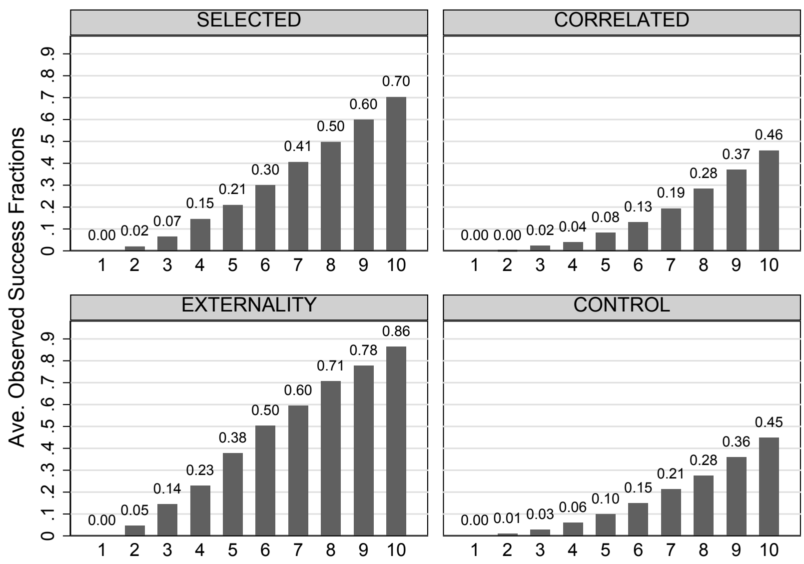
Notes: (i) OLS regressions include one observation per individual per round, (ii) The dependent variable is an individual's investment propensity in that round, (iii) In columns (1b), (2b) and (2c) we include round fixed effects (FEs). (iv) In column (2c), we restrict the sample to only those individuals who followed a pure threshold strategy in each of the last five rounds, (v) Standard errors are clustered at the interaction group level. This means that in the SELECTED and CORRELATED treatments all decisions by the relevant group of 3 individuals are included in a cluster, in the EXTERNALITY treatment there are two individuals per cluster, and in the CONTROL treatment each individual is a cluster, since their feedback is not influenced by their group members' choices. The standard errors are reported in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Propensity to invest in SELECTED and EXTERNALITY (observations for each round)

	Full Sample		Full Sample		Restricted Sample
	All	All	R16-20	R16-20	R16-20
	(1a)	(1b)	(2a)	(2b)	(2c)
EXTERNALITY	0.04	0.04	0.06**	0.06**	0.08**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Constant	0.41***	0.50***	0.36***	0.36***	0.34***
	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
Round FEs		Y		Y	Y
Observations	1920	1920	480	480	365
Adjusted R^2	0.013	0.053	0.036	0.028	0.046

Notes: (i) OLS regressions include one observation per individual per round, (ii) The dependent variable is an individual's investment propensity in that round, (iii) In columns (1b), (2b) and (2c) we include round fixed effects (FEs). (iv) In column (2c), we restrict the sample to only those individuals who followed a pure threshold strategy in each of the last five rounds, (v) Standard errors are clustered at the interaction group level. This means that in the SELECTED treatment, all decisions by the relevant group of 3 individuals are included in a cluster, while in the EXTERNALITY treatment there are two individuals per cluster. The standard errors are reported in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 17: Average database observed by participants in round 20 (Experiment 1)



Appendix D: Additional material from Experiment 2

Appendix D.1: Description of treatments in Experiment 2

Experiment 2 consists of nine treatment conditions. They can be divided into four sets of treatments:

*The **3-Dice NoPastData** and **3-Dice PastData** treatments.*

The **3-Dice NoPastData** serves to populate the databases for other treatments. In this treatment, participants do not have access to any past data and must base their investment decisions in Part A solely on the information that they have about the data generating process. Additionally, the treatment will allow us to test whether additional (superfluous) information can impair decision quality.[49]

The **3-Dice PastData** treatment is identical to the 3-Dice NoPastData treatment, with the following two exceptions. First, the participants in the 3-Dice NoPastData treatment base their investment decisions on the realization of the **red** dice, while those in the 3-Dice PastData treatment base their investment decisions on the realization of the **purple** dice.[50] Second, in Part A of 3-Dice PastData, when participants make their investment decision they are provided with information about the outcomes of the projects that participants in the 3-Dice NoPastData treatment chose to invest in. Specifically, for each of the 100 participants who participate in the 3-Dice NoPastData treatment, the computer will randomly generate 50 projects. It will then use the decisions indicated by their strategy method investment choice in Part A to evaluate whether they invest or do not invest in each of the 50 projects. For each of the 100 participants, the computer will collect together the projects that they invested in.[51] It will then organize these projects into a database according to the **purple** dice value of the project. Investors in the 3-Dice PastData treatment will be able to

[49]Note, one implication of participants not having access to past data from other participants in Part A of the 3-Dice NoPastData treatment is that in Part C, the objective probabilities corresponding to the Part A beliefs of a "naive extrapolator" are not well defined (since the naive extrapolator has no data to extrapolate from in Part A). Therefore, to keep this treatment as similar as possible to the other treatments, we populate the Part C probabilities in the following way: We will use the beliefs that a "naive extrapolator" would hold in expectation if they were observing the past projects from individuals who all followed the decision rule: invest iff the project has a dice value of 6 or greater. These probabilities correspond to those reported in the fifth column from the right in Table 5 (i.e., the column headed "6").

[50]This is purely a difference in labelling since the projects are symmetric in the three dice. However, the reason that we use the different dice colors in the two treatments is because we wish to make it clear to participants that when they observe past projects that others invested in, the dice that the past investors observed is different to the one that they themselves observe.

[51]Specifically, participants are told that they will observe the outcomes of projects invested in by 100 past participants. They are told that " ... the computer has generated 50 projects for each of these 100 individuals. This implies that the computer generated a total of 5000 projects. For each of these 5000 projects, the computer rolled three dice," and "If it was invested in, then it is added to the database of past projects that you can observe. If it was not invested in, then you do not observe the outcome of that project and it is not added to the database."

observe the percent of these past projects that were successful, conditional on each **purple** dice value between 1 and 10.

*The **2-Dice NoPastData** and **2-Dice PastData** treatments.*

The second pair of treatments test the boundaries of naive extrapolation. Specifically, here we reduce the number of dice that define a project from 3 to 2, which reduces the complexity of the data generating process. The objective is to investigate whether individuals are more prone to extrapolate naively from past data when they face a decision problem that depends on a complex data generating process in comparison to when it is relatively simpler.

The **2-Dice NoPastData** treatment is the same as the 3-Dice NoPastData treatment, with the exception that a project in Part A is defined by rolling two ten-sided dice instead of three. In the 2-dice treatments, participants are told that a project is successful if the sum of the two dice is at least 16.

The **2-Dice PastData** treatment is the same as the 3-Dice PastData treatment, with the exception that the past data that participants now observe is collected from the 2-Dice NoPastData treatment. The probabilities in Part B and Part C are adjusted accordingly to fit the 2-dice case.[52]

*The **3-Dice PartialDGP**, **3-Dice Cue** and **3-Dice ExtraInfo** treatments.*

The third set of treatments aim to further explore the underlying mechanisms generating naive extrapolation. Each of the three treatments is very similar to the 3-Dice PastData, but one particular feature of the decision environment is varied.

The **3-Dice PartialDGP** is identical to the 3-Dice PastData treatment, with the exception that participants are not provided with a complete description of the data generating process. Here, while participants are told that the sum of the three ten-sided dice defines the success of the project, they are not told the exact threshold for success. Specifically, they are told that there is some value x and if the sum of the three dice is equal to at least x then the project is successful. Therefore, they are provided with sufficient information to know that success is monotonically increasing in the value of any individual dice realization (in particular, the one that they observe). This treatment allows us to investigate the influence of an investor not fully knowing the true underlying model that is generating the data.

The **3-Dice Cue** explores the limits of naive extrapolation. It is the same as the 3-Dice PastData treatment, except the participants read a series of leading questions before making their investment choice. The questions themselves do not reveal any additional information, but rather guide the participant through a series of logical steps that they may have followed themselves just by thinking hard about the problem (without requiring additional

[52]As in the 3-dice treatments, the probabilities in Part B correspond to the Bayesian probabilities of success for a project in Part A. The probabilities in Part C are also be calculated in the same way as for the 3-dice treatments.

knowledge). We can then ask whether this specific type of "cue" or "nudge" can induce the investor to view the problem through a different lens, helping them to see the logical error present when engaging in naive extrapolation.

The **3-Dice ExtraInfo** treatment is the same as the 3-Dice PastData treatment, except that participants are provided with a second table that summarizes the information about the number of projects that past investors chose to invest in. Similarly to the success percentages, these are organized according to the values of the **purple** dice. Therefore, the table shows the number of past investments that were invested in, conditional on each **purple** dice value between 1 and 10.

*The **3-Dice17 NoPastData** and **3-Dice17 PastData** treatments.*

The final two treatments in Experiment 2, the **3-Dice17 NoPastData** and **3-Dice17 PastData** treatments, are very similar to the core 3-Dice NoPastData and 3-Dice PastData treatments. The only difference is that the threshold for a project to be successful is reduced from 22 to 17. This means that the Bayesian probabilities of success are shifted upwards—conditional on any specific observed dice roll realization, a project is more likely to be successful with the lower threshold. These treatments aim to achieve two objectives. First, they provide an additional robustness check for our main result by assessing whether individuals still display selection neglect when the (Bayesian and Naive) probabilities associated with each dice roll are substantially higher. Under these two treatments, a risk-neutral Bayesian would invest for more than half of the possible dice values (in our other treatments, a risk-neutral Bayesian would invest for fewer than half the dice values). This implies that if there is a pull towards the middle of the choice list (e.g., a presentation effect) in these two treatments, this bias would now result in under-investment (in contrast to in our other treatments, where it would result in over-investment).

Second, in our other treatments, we observe over-investment even in the absence of additional information about past investments of others (i.e., when there is only information about the DGP).[53] These additional treatments with a lower success threshold allow us to shed light on why this is the case. Specifically, one potential explanation for the over-investment in the 3-Dice NoPastData treatment is that some individuals find inferring the probabilities of success from a description of the data-generating process to be a complex task. This may result in cognitive uncertainty, which leads to estimates that are distorted towards the cognitive default (see, e.g., Enke and Graeber, 2023). In the absence of information about past investments from others, this cognitive uncertainty may induce participants to shift their beliefs about success from the Bayesian probabilities implied by the

[53]As noted in the main text, these two treatments, 3-Dice17 NoPastData and
textsc3-Dice17 PastData, were not part of the initial seven pre-registered treatments in Experiment 2. We added these final two treatments in response to helpful suggestions that we received from the editor and referees. Therefore, we were already aware of the over-investment (relative to Bayes) in the 3-Dice NoPastData treatment when designing the 3-Dice17 NoPastData and
textsc3-Dice17 PastData treatments.

DGP towards 50% (which is a plausible candidate for the cognitive default). If this is the case, then by shifting the success threshold in these new treatments, the Bayesian probabilities are shifted upwards such that these Bayesian probabilities are closer to the cognitive default. This should reduce over-investment in the absence of past investment information from others. Crucially, this will then allow us to assess whether receiving this additional past information from others' investments leads to an increase in investment away from Bayes' and the cognitive default, making investors worse off.

Table 11 provides a summary of the key features of the treatments.

Table 11: Summary of the treatments in Experiment 2

Treatment Name	**Treatment Variation**	**No. of Dice**	**Data on Past Projects?**	**Which Past Projects?**	**Part B**	**Part C**
3-Dice NoPastData		3	No		Bayes	NaiveSim
3-Dice PastData	Add past project data	3	Yes	3-Dice NoPastData	Bayes	Naive
2-Dice NoPastData	2 dice instead of 3	2	No		Bayes	NaiveSim
2-Dice PastData	Add past project data	2	Yes	2-Dice NoPastData	Bayes	Naive
3-Dice PartialDGP	Partial info about DGP (success $\geq x$)	3	Yes	3-Dice NoPastData	Bayes	Naive
3-Dice Cue	Socratic method	3	Yes	3-Dice NoPastData	Bayes	Naive
3-Dice ExtraInfo	Add info. on no. of "invested in" projects	3	Yes	3-Dice NoPastData	Bayes	Naive
3-Dice17 NoPastData	Uses a success threshold of 17	3	No		Bayes	NaiveSim
3-Dice17 PastData	Add past project data	3	Yes	3-Dice17 NoPastData	Bayes	Naive

Notes: (i) "Bayes" refers to the correct Bayesian probabilities associated with the relevant **Part A** investment decision, (ii) When the participants have access to past project data in **Part A**, then "Naive" refers to using probabilities in **Part C** that are equal to the empirical success fractions observed in the past data in **Part A**, (iii) When the participants do not have access to past project data in **Part A**, then "NaiveSim" refers to simulated empirical success fractions that would be obtained (in expectation) if the participant were observing the past projects that a risk neutral Bayesian subject had invested in.

Appendix D.2: Pre-registered hypotheses for Experiment 2

The following hypotheses were pre-registered in the AEA Registry (AEARCTR-0010536) after the completion of Experiment 1, but prior to the data collection for Experiment 2. The description of these hypotheses relies on the definitions that are developed in Section 5.2.1 of the main text.

D.2.1 Is the average participant partially naive?

In Experiment 1, we presented evidence that the average participant displayed some degree of selection neglect, implying that this average participant deviated from Bayesian behavior and was classified as being partially naive. In Experiment 2, we aim to replicate this finding in a far simpler setting. While Experiment 2 retains the core elements of Experiment 1, we have introduced several important adjustments relative to Experiment 1. First, participants only make a single investment decision. Second, we have reduced the salience of the information about past projects by representing it in a simple table rather than graphically (thereby reducing potential experimenter demand effects). Third, we have substantially simplified the instructions, tranformed the task into an individual decision-making task rather than a dynamic interactive game, and added additional understanding checks to ensure that participants fully understand the setting. Therefore, if we replicate the finding in this setting, it would provide strong support that it is a robust result.

Hypothesis B. 1. ***(Naivety of the average individual)*** *The average participant in the 3-Dice PastData treatment is partially naive.*

We test this hypothesis by evaluating whether the $\mu^{n}_{3D.PD} < 1$ for the average participant in the 3-Dice PastData treatment.

D.2.2 Does complexity of the DGP influence the prevalence of naive extrapolation?

In Part A of Experiment 2 (and in Experiment 1), participants typically have two sources of information that they can draw on in order to infer the probabilities that an investment will succeed conditional on each possible dice value. First, they can draw on their knowledge of the data generating process. Second, they can draw on the empirical data that they observe from past participants. A key question here is how individuals decide which source of information to rely on. One possibility is that some participants will follow the data irrespective of other contextual features of the choice environment. However, an alternative possibility is that it depends on the difficulty (costliness) of processing the different sources of information. An important dimension here is the complexity of the data generating process. In scenarios where it is relatively simpler for individuals to process the information that they have about the data generating process, they may be more inclined to take it into account.

In our 2-Dice PastData treatment we reduce the complexity of the data generating process by reducing the number of dice that define a project from three to two. We then ask whether this reduction in complexity induces the average participant in the 2-Dice PastData treatment to move closer to using Bayesian probabilities in their Part A investment decisions in comparison to participants in the 3-Dice PastData treatment.

Hypothesis B. 2. ***(Complexity of the DGP)*** *Participants become more naive as the complexity of the data generating process increases—the average participant in the 2-Dice PastData treatment is closer to Bayesian than the average participant in the 3-Dice PastData treatment.*

We test this hypothesis by assessing whether $\mu^n_{2D.PD} > \mu^n_{3D.PD}$.

D.2.3 Does a reduction in DGP information exacerbate naive extrapolation?

In the real world, it is often the case that individuals do not have full information about the true underlying data generating process and have to draw on partial DGP information along with past data. It is plausible that in such scenarios individuals will rely more on the other data source at their disposal, namely the past data from previous participants. We therefore ask whether participants in our 3-Dice PartialDGP treatment are more inclined to the follow the past data than those in the 3-Dice PastData treatment.

Hypothesis B. 3. ***(Partial information about the DGP)*** *Participants become more naive when there is a reduction in the availability of information about the data generating process—the average participant in the 3-Dice PartialDGP treatment is further from Bayesian than the average participant in the 3-Dice PastData treatment.*

We will test this hypothesis by testing whether $\mu^n_{3D.PDGP} < \mu^n_{3D.PD}$.

D.2.4 Can cues shift the way individuals think about the problem?

Some forms of cognitive mistakes may emanate from individuals approaching a particular problem in the wrong way—i.e., they may *represent* the problem in the wrong way.[54] Many statistical biases take this form. Here, we are focused on selection neglect, which involves individuals treating a conditional distribution as if it representative of the unconditional distribution that they are interested in. The next hypothesis explores whether simply asking individuals a series of leading questions can induce them to approach the problem differently.[55] Importantly, the questions themselves contain no additional information, but simply aim to shift how an individual thinks about the problem.

[54]For example, imagine a school student that has learnt two algorithms for solving particular types of algebra problems—each algorithm is appropriate for solving one class of problems, but these classes are mutually exclusive. When faced with a new problem, he focuses on certain features of the problem and tries to assess which is the appropriate algorithm to use. If he represents the features of the problem incorrectly, he may adopt the wrong algorithm.

[55]This is reminiscent of the approach taken by Socrates in Plato's "Meno".

Hypothesis B. 4. ***(Socratic Method)*** *Participants become less naive when they are prompted (through means of a series of questions) to think about the investment problem in a different way—the average participant in the 3-Dice Cue treatment is closer to Bayesian than the average participant in the 3-Dice PastData treatment.*

We will test this hypothesis by assessing whether $\mu^n_{3D.CUE} > \mu^n_{3D.PD}$.

D.2.5 Does additional information about the past data help alleviate naivety?

As noted above, a fully rational Bayesian agent does not need to draw on the past data at all in this experiment (except in the 3-Dice PartialDGP treatment), since they know the data generating process. However, if individuals do draw on the past data, a question of interest is whether having additional information about this past data that they are observing influences how they process it. Specifically, in the 3-Dice ExtraInfo treatment, in addition to receiving information about the percentage of past projects that were successful for each dice value, participants also learn about the number of projects that were invested in for each value of the purple dice. Under the assumption that all the past participants followed the same threshold strategy, this information would be sufficient to reveal what that threshold was.

We ask whether being provided access to this additional information about the past projects helps participants to reduce their naive extrapolation. This could operate in different ways. First, participants could draw on the additional information directly and try to learn from the threshold followed by past participants (however, since the past participants had less information than them, this doesn't seem like an attractive strategy). Second, the additional information could help to alert them to the selection present in the data and therefore induce them to think more carefully about the decision problem.

Hypothesis B. 5. ***(Additional information about the past data)*** *Participants become less naive when they are provided with additional information about the past data—the average participant in the 3-Dice ExtraInfo treatment is closer to Bayesian than the average participant in the 3-Dice PastData treatment.*

We will test this hypothesis by assessing whether $\mu^n_{3D.EI} > \mu^n_{3D.PD}$.

D.2.6 Is being exposed to past data harmful?

Here we ask whether participants are better off with less information. Specifically, we compare a scenario in which a participant is provided with access to past data to a scenario in which she is not provided with access to past data, i.e. 3-Dice PastData and 3-Dice NoPastData. Since Part C of these two treatments are not comparable (in 3-Dice NoPastData, the Naive Lottery probabilities are not well defined because the participants don't observe past data), we cannot use μ^n as our metric for comparison. However, we can construct a simpler measure that just uses Part A and Part B and reflects the distance between the Part A investment decision and the individual's behavior when they hold Bayesian beliefs. Specifically, define $\sigma_i^n = 1 - \frac{\tilde{n}_i - n_i^b}{10}$. Again, the closer this metric is to 1, the closer is investment behavior in Part A and Part B, implying that the individual's Part A behavior is closer to Bayesian (in the sense of holding Bayesian beliefs).

Hypothesis B. 6. ***(Harmful information)*** *Participants become less Bayesian when they are exposed to past data—the average participant in the 3-DICE NOPASTDATA treatment is closer to Bayesian than the average participant in the 3-DICE PASTDATA treatment.*

We test this hypothesis by assessing whether $\sigma^n_{3D.NPD} > \sigma^n_{3D.PD}$.

Appendix D.3: Results for pre-registered hypotheses for Experiment 2

Table 12 summarizes the results from all six of the hypotheses discussed above.[56] The left-most column shows that we observe a substantial amount of naivety in the 3-Dice PastData treatment. The average participant has a μ^n that is less than 0, which is significantly below the Bayesian benchmark of $\mu^n = 1$ ($p < 0.01$) and close to the naive benchmark of $\mu^n = 0$. This suggests that on average individuals are engaging in investment behavior in the investment game in Part A that is more similar to that in the Naive Lottery than in the Bayesian Lottery. This conclusion is consistent with the picture that emerges from a visual inspection of the cdf of investment behavior in the Investment Game (Part A) in comparison to the Bayesian Lottery (Part B) and the Naive Lottery (Part C). The left-hand panel of Figure 6 depicts the cdfs for the 3-Dice PastData treatment. The figure suggest that at the aggregate level, behavior in the Investment Game more closely resembles that in the Naive Lottery task in comparison to that in the Bayesian Lottery task. Taken together, these results provide strong support for the finding from Experiment 1 that individuals tend to make investment choices in the Investment Game as if they hold naive probabilities as their beliefs, rather than Bayesian probabilities. Since we find this result in a new participant pool with a simplified version of the game and a within-individual benchmark for Bayesian and Naive beliefs, this lends support to the conclusion that naive extrapolation from data is a robust finding.

Result B. 1. *(Hypothesis B.1) The average participant in the 3-Dice PastData treatment is (at least) partially naive. Observed behavior in the Investment Game is more similar to that in the Naive Lottery than that in the Bayesian Lottery.*

Table 12: Hypothesis tests from Experiment 2

	Hyp B.1	Hyp B.2	Hyp B.3	Hyp B.4	Hyp B.5	Hyp B.6
Alternative Hypothesis (H_a):	$\mu^n_{3D.PD} < 1$	$\mu^n_{3D.PD} < \mu^n_{2D.PD}$	$\mu^n_{3D.PD} > \mu^n_{3D.PDGP}$	$\mu^n_{3D.PD} < \mu^n_{3D.CUE}$	$\mu^n_{3D.PD} < \mu^n_{3D.EI}$	$\sigma^n_{3D.PD} < \sigma^n_{3D.NPD}$
Mean (3D.PD)	-0.09	-0.09	-0.09	-0.09	-0.09	0.83
Std err. (3D.PD)	0.07	0.07	0.07	0.07	0.07	0.01
Mean (Comparison Group)	1.00	0.27	0.14	-0.11	-0.02	0.82
Std err. (Comparison Group)		0.06	0.12	0.07	0.11	0.02
Diff.	1.09	0.36	0.23	-0.03	0.07	-0.01
T-Statistic	14.61	3.15	1.68	-0.23	0.50	-0.60
p-value	<0.01	<0.01	0.95	0.59	0.31	0.73
N	202	303	299	301	303	302
H_0 Rejected	Y	Y	N	N	N	N

Notes: (i) The table contains the tests of the pre-registered hypotheses from Experiment 2, (ii) The column headers denote the alternative hypothesis (H_a), where the null hypothesis (H_0) always involves a test that the difference equals 0, (iii) Hypothesis 1 involves a one-sample test and Hypothesis 6 involves a different outcome variable.).

[56] Table 14 in the Appendices replicates this analysis for the restricted subset of individuals who did not violate stochastic dominance by investing more often in the Bayesian Lottery than in the Naive Lottery. The results remain very similar.

The second column of Table 12 shows that individuals in the 2-Dice PastData moved closer to behaving as if they held Bayesian beliefs in the Investment Game in comparison to individuals in the 3-Dice PastData treatment. This is indicated by the shift in the average μ^n from -0.09 to 0.27, implying a statistically significant difference of 0.36 ($p < 0.01$). While we do observe evidence that reducing the complexity of the data generating process results in a significant reduction in naivety, it is important to also note that the μ^n observed in the 2-Dice PastData treatment is still far away from the Bayesian benchmark of $\mu^n = 1$, indicating that there remains a large amount of naive investment behavior even in this setting with a simpler DGP.

Result B. 2. *(Hypothesis B.2) Participants become less naive as the complexity of the data generating process is reduced—the average participant in the 2-Dice PastData treatment is closer to Bayesian than the average participant in the 3-Dice PastData treatment.*

Columns 3, 4, 5, and 6 of Table 12 show that we fail to reject the null hypothesis for each of the remaining four hypotheses. This suggests that in our setting, none of these four channels influence the propensity of individuals to invest in the Investment Game. These results suggest several insights. First, individuals over-invest in our setting even without access to past data. This can be seen in the right-most column of Table 12 (Hyp. B.6), which shows that participants in the 3-Dice NoPastData treatment are not closer to Bayesian than those in the 3-Dice PastData treatment.[57] This indicates that participants are not very good at inferring the Bayesian probabilities from their knowledge of the data-generating process in this context, which is also a feature of many real-world scenarios where individuals may try to learn from observing past outcomes of others. Second, the treatments that try to vary the difficulty of drawing inference directly from the 3-dice DGP information did not shift investment behavior (Hyp B.3, B.4, and B.5). This indicates that in contrast to the reduction of complexity present when moving to the simpler 2-dice setting, the average participant appears to have a cognitive constraint that leads them to form a noisy mental model of the mapping from the DGP to the success probabilities in the 3-dice setting. Third, importantly, even with this cognitive constraint in accurately extracting Bayesian probabilities from the DGP information, individuals who are not naive should still move closer to acting like a Bayesian when they receive information about the success frequencies of projects that were invested in. Upon seeing the past data, if an individual recognizes and appreciates the selection process that generates the success frequencies that she observes, she should discount the observed success frequencies and therefore invest less in the Investment Game than in the Naive Lottery. Taken together, these results suggest that naivety is quite a robust phenomenon that is difficult to alleviate in scenarios where the true DGP is complex, and it is non-trivial to extract the relevant Bayesian probabilities from knowledge about the DGP.

[57]It can also be seen in Table 16, which summarizes the propensity to invest in the Investment Game in the two NoPastData treatments as well as the two corresponding PastData treatments.

Unfortunately, this is the case for many real-world scenarios of interest.

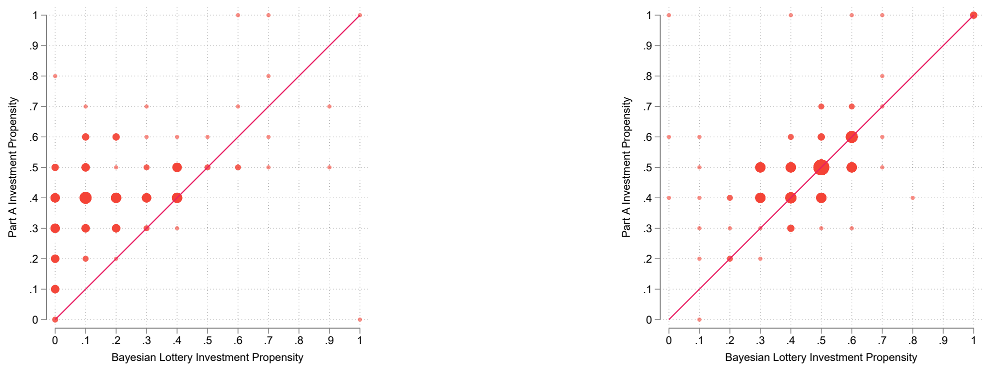
Result B. 3. *(Hypotheses B.3, B.4, B.5, B.6) When the DGP is complex, participants do not become less naive when provided with additional information or cues about the DGP, nor do they become more naive when there is a partial reduction in the availability of information about the DGP. Instead, the majority of individuals appear to be willing to follow the past data naively.*

Appendix D.4: Examining overinvestment in the absence of past data

One of the features of the decision context used in the majority of our treatments is that a risk-neutral Bayesian should invest in fewer than 50% of the investment opportunities that she faces. This means that biases other than the naivety that we are studying here, such as a tendency to shade towards a cognitive default of investing in 50% of the available investment opportunities, could lead to over-investment (Enke and Graeber, 2023). As discussed in the main text, this is not an issue for the aggregate-level between-treatment comparisons in Experiment 1 and Experiment 2 (since the decision context is held constant across treatments).

For the individual-level analyses in Experiment 2, a simple bias toward investing in 50% of the available investment opportunities would also not present an issue for comparing Part A, B, and C and, therefore, would not influence our main results or classification exercises (since, there again, the decision context is held constant across parts). However, if such a bias is related to the complexity of the decision context, then it is plausible that it could exert a stronger influence on investment decisions in Part A relative to Part B and C. For example, this might occur if cognitive uncertainty is larger in Part A relative to Part B and C. This does not seem implausible. Consider an individual in the 3-Dice NoPastData treatment. In Part A, this individual only receives a description of the data-generating process (DGP) and must infer the probability of success associated with each of the dice values. Since this may be a challenging exercise for at least a subset of participants, they may shade their assessments toward holding beliefs that the probability of success is closer to 50%. Since all of the Bayesian probabilities are below 50%, this shading towards 50% could inflate the propensity to invest. Provided this occurs more in Part A (where cognitive uncertainty is plausibly higher) than in Part B (where cognitive uncertainty is plausibly lower), this mechanism could lead to over-investment in Part A.

Figure 18: Individual investment propensities (comparing two 3D NoPastData treatments)



(a) **3D NoPastData** (b) **3D17 NoPastData**

Consistent with this idea, we do see over-investment (relative to Bayes) even in the absence of past data. For example, in Figure 4, we see over-investment in the first round of Experiment 1. Similarly, in Figure 18a, we see that there is more investment in the investment game than in the Bayesian lottery for the majority of participants in the 3-Dice NoPastData treatment. This raises the following question: Could (part of) the overinvestment observed in the PastData treatments be driven by a mechanism other than naively learning from the past data? We argue that even the naivety that we detect in our individual-level analysis is robust to this concern for the following two reasons.

First, even if individuals form success probability beliefs that are inflated towards 50% when they only receive a description of the DGP (as in the 3-Dice NoPastData treatment), there is still scope for learning from the past data and adjusting their belief downward if they are not naive and factor in the presence of selection. Consider an individual who, upon receiving a description of the DGP, forms the belief that the probability of success after observing a dice value of 7 is 50%. Now, she observes selected past data with an empirical success frequency of 40% for a dice value of 7. If the individual takes into account the positive selection in the data, she should adjust her belief downwards to end up well below 40%. Even if she observed an empirical success frequency of 50%, she should adjust her belief downwards if she realizes that the data is positively selected.[58]

Second, to directly examine this question, we added two additional treatments with a lower success threshold, 3-Dice17 NoPastData and 3-Dice17 PastData. The rationale for adding these two treatments is that they shift the Bayesian probabilities of success upwards. This implies that a bias that distorts beliefs towards 50% will no longer result in overinvestment relative to Bayes. Figure 18b shows that the lower success threshold is effective in shifting beliefs closer to the Bayesian benchmark in the absence of past data (i.e., in 3-Dice17 NoPastData). Therefore, since we do observe individuals overinvesting when past data is added in the 3-Dice17 PastData treatment (see Figure 8a), this indicates that naivety is causing individuals to shift their beliefs and invest too much.

This can also be seen clearly in Table 13. Columns (*a) and (*b) show the distributions of individual investment behavior with and without past data by comparing choices in the investment game and the Bayesian lottery. The first two columns (1*) report the results for a project success threshold of 22, while the last two columns (2*) do so for a success threshold of 17. In column (1a), we see that without past data, consistent with the discussion above, 78% of individuals invest more in the investment game than in the Bayesian lottery. When past data is added in column (1b), 82% of individuals invest more in the investment game. Importantly, the vast majority of these individuals are investing precisely the same amount in

[58]An analogous hypothetical example is the following. Suppose that there is a university competition where students from a particular class all complete a quiz. Thereafter, each student chooses whether to submit their quiz and enter the competition or not. A spectator is asked to estimate the average quiz score of all students in the class—she guesses that the average is 70 points out of 100. Now, suppose the spectator is told that the average score of those who entered the competition is 70 points and is asked whether she would like to adjust her guess. Even though the information coincides with the spectator's guess, if she takes into consideration the likely positive selection into the competition, she should lower her guess.

the investment game and Naive lottery in 3-Dice PastData (see Figure 7b). Therefore, even though the fraction of individuals overinvesting is similar with and without past data when the success threshold is 22, the fact that the majority of individuals invest *exactly* the same amount in the investment game and Naive lottery when they have past data suggests that they do believe that empirical frequencies observed in the past data are the true probabilities.

Nevertheless, these results do not cleanly parse the naivety explanation from the above-mentioned cognitive uncertainty explanation. Columns (2a) and (2b), however, do successfully achieve this. Column (2a) shows that with the lower threshold for success, 42% of individuals invest more in the investment game than in the Bayesian lottery without past data. When past data is added in column (2b), the classification is nearly identical to that in column (1b)—now, 82% of individuals invest more in the investment game. Furthermore, Figure 8b shows that the majority of them are behaving in exactly the same way in the investment game and the Naive lottery, which suggests that they hold the same beliefs in both. This shows that naivety is playing an important role in driving the results that we observe.[59]

Table 13: Classification of individuals relative to Bayes.

	3-Dice		**3-Dice17**	
	NoPastData	**PastData**	**NoPastData**	**PastData**
	(1a)	(1b)	(2a)	(2b)
Less than Bayes ($\tilde{n}_i < n_i^{BL}$)	8	7	22	7
Exactly Bayes ($\tilde{n}_i = n_i^{BL}$)	14	11	36	11
More than Bayes ($n_i^{BL} < \tilde{n}_i$)	78	82	42	82
Observations (N)	100	202	100	100

Notes: (i) The table provides a classification of individuals into discrete types according to their investment behavior in Part A, B and C of Experiment 2, (ii) Each of the columns reports the distribution of types within a particular treatment in percentage points, (iii) The first two columns report the distributions for the 3-Dice NoPastData and 3-Dice PastData treatments, while the last two columns report the same information for the 3-Dice17 NoPastData and 3-Dice17 PastData treatments, (iv) When comparing this table to Table 4, it is worth taking into consideration the following. First, since there is no naive benchmark in the NoPastData treatments, this table only compares the choices in the investment game to the Bayesian benchmark, resulting in only three types. This allows a comparison between the NoPastData and PastData treatments, which is not possible using the more fine-grained classification employed in Table 4. Second, this simplified classification pools together several categories from Table 4 and categories such as "Violates Stochastic Dominance" are not identified here. This means that the category labels do not have exactly the same meaning between tables—for example, "Exactly Bayes" here is not identical to the category labelled "Exactly Bayes" in Table 4. The precise definitions are denoted by the comparisons in parentheses next to the category labels.

[59]It is important to note that we are not claiming that cognitive uncertainty is playing no role. Rather, it does seem to be an important factor for understanding behavior in the NoPastData treatments. However, the results here illustrate that it is not explaining the coincidence of behavior between the Naive lottery and the investment game in the PastData treatments. In other words, the results robustly show that naivety is playing an important role in inflating beliefs and generating overinvestment.

Appendix D.5: Additional tables and figures from Experiment 2

Table 14: Hypothesis tests from Experiment 2 (Restricted Sample)

	Hyp B.1	Hyp B.2	Hyp B.3	Hyp B.4	Hyp B.5	Hyp B.6
Alternative Hypothesis (H_a):	$\mu^n_{3D.PD} < 1$	$\mu^n_{3D.PD} < \mu^n_{2D.PD}$	$\mu^n_{3D.PD} > \mu^n_{3D.PDGP}$	$\mu^n_{3D.PD} < \mu^n_{3D.CUE}$	$\mu^n_{3D.PD} < \mu^n_{3D.EI}$	$\sigma^n_{3D.PD} < \sigma^n_{3D.NPD}$
Mean (3D.PD)	-0.13	-0.13	-0.13	-0.13	-0.13	0.81
Std err. (3D.PD)	0.06	0.06	0.06	0.06	0.06	0.01
Mean (Comparison Group)	1.00	0.20	0.08	-0.16	-0.07	0.79
Std err. (Comparison Group)		0.06	0.11	0.07	0.11	0.02
Diff.	1.13	0.33	0.21	-0.03	0.06	-0.02
T-Statistic	17.77	3.37	1.76	-0.29	0.48	-1.20
p-value	<0.01	<0.01	0.96	0.61	0.31	0.88
N	187	280	279	283	283	281
H_0 Rejected	Y	Y	N	N	N	N

Notes: (i) The table contains the tests of the pre-registered hypotheses from Experiment 2, (ii) The column headers denote the alternative hypothesis (H_a), where the null hypothesis (H_0) always involves a test that the difference equals 0, (iii) Hypothesis 1 involves a one-sample test and Hypothesis 6 involves a different outcome variable.), (iv) The sample is restricted to the set of individuals who did not violate stochastic dominance by investing more often in the Bayesian Lottery (Part B) than the Naive Lottery (Part C).

Table 15: Average investment propensity across 3-Dice22 PD treatments (Investment Game)

	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
	3DicePastData		**3DiceExtraInfo**		**3DicePartialDGP**		**3DiceCUE**		**3DPD:3DEI**		**3DPD:3DPDGP**		**3DPD:3DCUE**	
	mean	sd	mean	sd	mean	sd	mean	sd	diff.	t-stat.	diff.	t-stat.	diff.	t-stat.
Investment Propensity	0.401	(0.211)	0.395	(0.219)	0.434	(0.221)	0.387	(0.192)	-0.006	(-0.225)	0.033	(1.229)	-0.014	(-0.580)
Observations	202		101		97		99		303		299		301	

Notes: (i) The first eight columns [labelled (1) to (4)] report the mean and standard deviation of participants' propensity to invest across the ten possible dice values in the investment game (Part A), (ii) The final six columns [labelled (5) to (7)] report the mean comparison of each of the treatments considered in (2) to (4) against the 3DicePastData treatment in (1); t-statistics are reported in parentheses, (iii) Symbols: * for $p < 0.1$, ** for $p < 0.05$, *** for $p < 0.01$.

Table 16: Average investment propensity across 3-Dice22 and 2-Dice Baseline Treatments

	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
	3D PastData		**3D NoPastData**		**2D PastData**		**2D NoPastData**		**3DPD:3DNPD**		**3DNPD:2DNPD**		**3DPD:2DPD**		**2DPD:2DNPD**	
	mean	sd	mean	sd	mean	sd	mean	sd	diff.	t-stat.	diff.	t-stat.	diff.	t-stat.	diff.	t-stat.
Investment Propensity	0.401	(0.211)	0.425	(0.190)	0.351	(0.152)	0.333	(0.203)	0.024	(0.995)	0.092***	(3.310)	-0.050**	(-2.337)	0.018	(0.731)
Observations	202		100		101		100		302		200		303		201	

Notes: (i) The first eight columns [labelled (1) to (4)] report the mean and standard deviation of participants' propensity to invest across the ten possible dice values in the investment game (Part A), (ii) The final eight columns [labelled (5) to (8)] report mean comparisons of the treatments considered, with the relevant comparison described in the column header; t-statistics are reported in parentheses, (iii) Symbols: * for $p < 0.1$, ** for $p < 0.05$, *** for $p < 0.01$.

Table 17: Average investment propensity in 3-Dice17 PastData and 3-Dice17 NoPastData

	(1)		(2)		(3)	
	3D17 PastData		**3D17 NoPastData**		**3D17PD:3D17NPD**	
	mean	sd	mean	sd	diff.	t-stat.
Investment Propensity	0.602	(0.175)	0.507	(0.184)	-0.095***	(-3.741)
Observations	100		100		200	

Notes: (i) The first four columns [labelled (1) and (2)] report the mean and standard deviation of participants' propensity to invest across the ten possible dice values in the investment game (Part A), (ii) The final two columns [labelled (3)] report the comparison of the means the two treatments considered; t-statistics are reported in parentheses, (iii) Symbols: * for $p < 0.1$, ** for $p < 0.05$, *** for $p < 0.01$.

Table 18: Comparison of investment game and lottery games

	Investment Game	Bayes Lottery	Naive Lottery	Diff. (Bayes:Invest)	Diff. (Naive:Invest)
3-Dice 22 Past Data (Pooled)	0.40	0.22	0.37	0.18***	0.03***
	(0.21)	(0.24)	(0.21)	(22.70)	(4.46)
N	499	499	499		
2-Dice Past Data	0.35	0.22	0.35	0.13***	0.00
	(0.15)	(0.18)	(0.18)	(9.80)	(0.28)
N	101	101	101		
3-Dice 17 Past Data	0.60	0.45	0.60	0.15***	0.00
	(0.17)	(0.19)	(0.19)	(10.41)	(0.12)
N	100	100	100		

Notes: (i) This table contains comparisons of the rate of investment in Part A, B and C of Experiment 2, (ii) The 3-Dice 22 sample consists of individuals in the four 3-Dice22 treatments where participants observed past data, pooled together, (iii) The statistics in parentheses below the group means denote standard deviations, while those below differences denote t-statistics, (iv) Symbols: * for $p < 0.1$, ** for $p < 0.05$, *** for $p < 0.01$.

Table 19: Comparison of investment game and lottery games (Restricted Sample)

	Investment Game	Bayes Lottery	Naive Lottery	Diff. (Bayes:Invest)	Diff. (Naive:Invest)
3-Dice 22 Past Data (Pooled)	0.39	0.20	0.37	0.19***	0.02***
	(0.21)	(0.22)	(0.20)	(25.33)	(3.52)
N	471	471	471		
2-Dice Past Data	0.34	0.20	0.35	0.14***	-0.01
	(0.15)	(0.17)	(0.18)	(10.21)	(-1.03)
N	93	93	93		
3-Dice 17 Past Data (Pooled)	0.59	0.43	0.61	0.16***	-0.02
	(0.16)	(0.17)	(0.17)	(11.93)	(-1.55)
N	94	94	94		

Notes: (i) This table contains comparisons of the rate of investment in Part A, B and C of Experiment 2, (ii) The 3-Dice 22 sample consists of individuals in the four 3-Dice22 treatments where there was past data, pooled together, (iii) The statistics in parentheses below the group means denote standard deviations, while those below differences denote t-statistics, (iv) The sample restriction removes individuals who violated stochastic dominance between Part B and Part C, (iv) Symbols: * for $p < 0.1$, ** for $p < 0.05$, *** for $p < 0.01$.

Table 20: Classification of individuals into discrete types (percentages).

	All 4 PD22 Treatments	**3-Dice PD**	**2-Dice PD**
	(1)	(2)	(3)
Less than Bayes ($\tilde{n}_i < n_i^{BL} < n_i^{NL}$)	3.6	3.5	5.9
Exactly Bayes ($\tilde{n}_i = n_i^{BL} < n_i^{NL}$)	3.8	3.0	5.9
Bayes = Naive = Invest ($\tilde{n}_i = n_i^{BL} = n_i^{NL}$)	6.8	5.9	9.9
Partially Naive ($n_i^{BL} < \tilde{n}_i < n_i^{NL}$)	6.6	8.4	12.9
Exactly Naive ($n_i^{BL} < \tilde{n}_i = n_i^{NL}$)	46.5	47.0	37.6
Extremely Naive ($n_i^{BL} < n_i^{NL} < \tilde{n}_i$)	27.1	24.8	19.8
Violates Stochastic Dominance ($n_i^{BL} > n_i^{NL}$)	5.6	7.4	7.9
Observations (N)	499	202	101

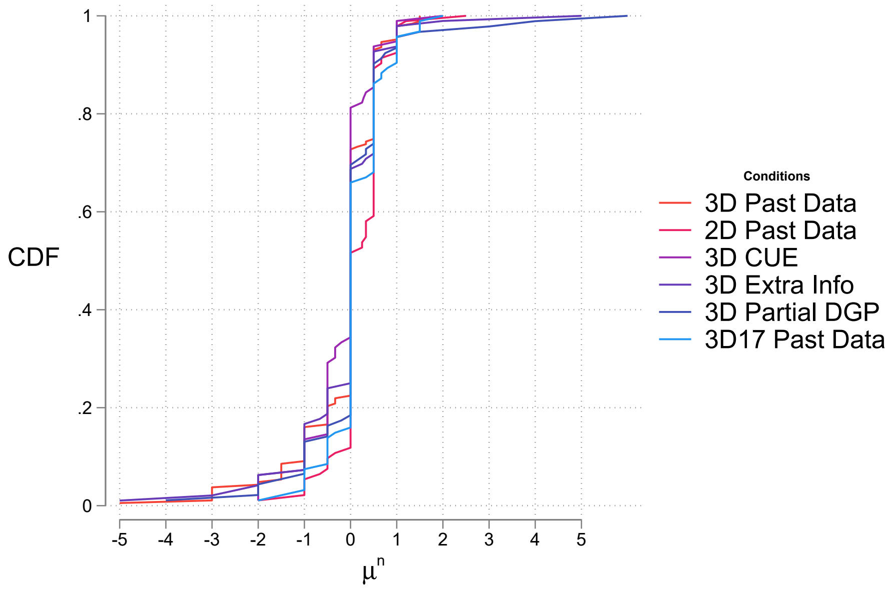
Notes: (i) The table provides a classification of individuals into discrete types according to their investment behavior in Part A, B and C of Experiment 2, (ii) The columns report the distribution of types in percentage points, (iii) Column (1) pools together all individuals in the 4 PastData treatments with a threshold of 22 (i.e., 3-Dice PastData, 3-Dice PartialDGP, 3-Dice CUE, and 3-Dice ExtraInfo).

Table 21: Correlates of μ^n

	3-Dice PastData Only	All 4 PD22 Treatments
	(1)	(2)
Female = 1	-0.349***	-0.223**
	(0.129)	(0.087)
At Least Some College = 1	-0.014	-0.053
	(0.174)	(0.110)
Age	0.079**	0.041**
	(0.032)	(0.021)
Age^2	-0.001**	-0.000*
	(0.000)	(0.000)
Zero Understanding Mistakes = 1	0.061	-0.001
	(0.134)	(0.090)
Time Spent on Part A Decision Screen (seconds)	0.003*	0.001
	(0.002)	(0.001)
Constant	-1.795***	-0.918**
	(0.689)	(0.453)
Treatment FEs	N	Y
Observations	186	468
Adjusted R^2	0.047	0.014

Notes: (i) OLS regressions include one observation per individual, (ii) The dependent variable is μ^n, (iii) The variable "At Least Some College" is a binary variable that takes a value of one if the individual reported an education level higher than high school, (iv) The regressions exclude individuals who violated stochastic dominance by investing more often in Bayesian Lottery than in the Naive Lottery (15 individuals or 7% in column 1 and 28 individuals or 6% in column 2), as well as individuals who chose not to report their gender as male or female (1 individual in column 1 and 3 individuals in column 2), (v) Standard errors are reported in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 19: CDF of μ^n across treatments (Restricted sample)



Appendix E: Instructions for the SELECTED treatment

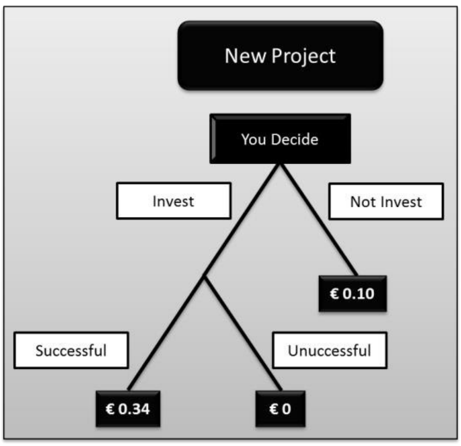
In Game I, you are going to have the opportunity to make a series of investment decisions. For each decision, you will be presented with a "project". Every project will turn out to either be a *successful project* or an *unsuccessful project*. Each project will have three "attributes". These attributes will be related to whether the project will be successful or not. You will be able to observe one of the three attributes for every project you face. (This will always be the same attribute).

In order to help you learn about whether a project you face will be a *successful project* or an *unsuccessful project,* you will be placed in a group with two other participants and you will be able to learn from the success or failure of the past investments of your group members. In particular, you will be able to observe whether projects similar to the one that you are currently considering were successful or not. Projects are "similar" when the attribute you observe is exactly the same between the projects. (We will provide you with more details on the exact information you will receive below).

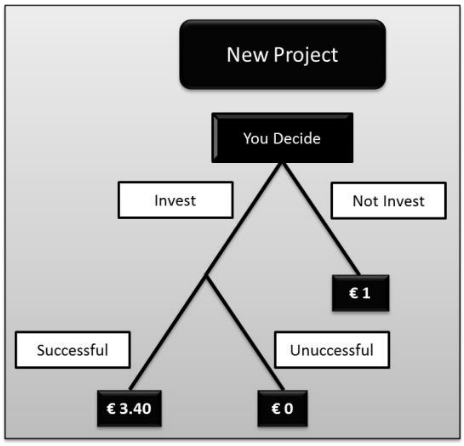
There will be two phases in the Investment Game. In Phase 1 (Low Stakes), each time you face a new project, you will be given an amount of money, €0.10. This is exactly the amount that it costs to make the investment. You can then choose whether to INVEST this €0.10; or NOT INVEST and keep the €0.10 to be paid at the end of the experiment. If you INVEST and the project is *successful*, you will be paid a high prize, which has value €0.34. If you INVEST and the project is *unsuccessful*, you will receive nothing and you will lose the €0.10 that you invested.

Phase 2 (High Stakes) is exactly the same, except all the amounts are multiplied by ten. This explanation is summarized in the following diagram.

Phase 1: Low Stakes



Phase 2: High Stakes



In **Phase 1 (Low Stakes)**, you will face low cost, low prize investment opportunities. In this phase, the cost of investment will be **€0.10** and the prize when the investment is successful will be **€0.34**. This phase will allow you to learn how the investment game works, and also to learn about which projects are likely to be *successful projects* and which projects are likely to be *unsuccessful projects*.

In **Phase 2 (High Stakes)**, the game is exactly the same. In particular, the chances that a project will be *successful* or *unsuccessful* are exactly the same as in Phase 1. The only difference is that the cost of investment will now be **€1.00** and the prize when the investment is successful will be **€3.40**.

Phase 1 (Low Stakes) investment costs and returns

Decision	Successful Project	Unsuccessful Project
Invest	Receive €0.34	Receive €0
Do Not Invest	Receive €0.10	

Phase 2 (High Stakes) investment costs and returns

Decision	Successful Project	Unsuccessful Project
Invest	Receive €3.40	Receive €0
Do Not Invest	Receive €1.00	

Learning about the project:

In order to help you learn about whether a project will be *successful* or *unsuccessful,* you will be able to observe whether similar projects that were invested in by your group members in the past were successful. The text below provides you with details explaining: (i) what is meant by "similar" projects; and (ii) which past projects you will observe.

Who is in your group?

As mentioned above, you will be randomly assigned into a group with two other participants in this experiment (three in total, including yourself – called Group Members **A**, **B** and **C**). You will stay in this group for both Phase 1 and Phase 2 of the experiment.

Which past projects will you observe?

In order to learn about which projects are likely to be successful, you will be able to observe the success of projects that were invested in by the other members of your group. You will also observe one of the attributes of these past projects.

What are project attributes?

Every project has three attributes, called **a**, **b** and **c**. For every project, each of these three attributes takes an integer[1] value between 1 and 10. The attribute values are determined by the computer rolling three ten-sided dice – i.e. Attribute **a** is equal to the number shown on the purple dice, called Dice **a**; Attribute **b** is equal to the number shown on the red dice, called Dice **b**; and Attribute **c** is equal to the number shown on the green dice, Dice **c**. All three dice are fair dice (i.e. each dice has an equal chance of showing every number between 1 and 10).

[1] An integer is a whole number, so each attribute takes one of the following ten values: 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10.

Who observes which attributes?

Each of the three group members (including you) will observe only one of the three attributes – and each group member will observe a different attribute from the other two.

More specifically, Group Member **A** will always observe Attribute **a**, Group Member **B** will always observe Attribute **b**; and Group Member **C** will always observe Attribute **c**.

When is a project successful?

The success of a project is determined by adding up the three attribute values (i.e. adding up the numbers on the three dice). If this total is equal to 22 or more (i.e. 22 to 30) then the project is *successful*; if the total is equal to 21 or less (i.e. 3 to 21) then the project is *unsuccessful*.

Example: What information does Group Member C observe?

*Information about current project that Group Member **C** is considering*

If, for example, you are Group Member **C**, then when you are considering a new project, you will always be able to observe Attribute **c** for this new project before you decide whether to invest in it.

*Information about past projects that Group Members **A** and **B** invested in*

As Group Member **C**, you would also have access to information about the Attribute **c** value of all projects that other members of your group invested in in the past. You will also be told whether these past projects were successful or not. In other words, you will know what proportion of the projects that other members of your group invested in with a particular Attribute **c** value were successful.

An example

Let’s consider a concrete example to clarify this. Consider the following hypothetical project:

Attribute a	Attribute b	Attribute c	Successful / Unsuccessful
10	5	7	**Successful**

The information above is a complete summary of the project - it contains all three attribute values and the information about its success.

Recall that the three attribute values are determined by throwing three fair ten-sided dice (Dice **a**, Dice **b**, and Dice **c**). Furthermore, notice that the project is “successful” because adding up the three attribute values = 10 + 5 + 7 = 22, which is between 22 and 30. Of course, as explained above, a participant in the experiment cannot observe such a complete description of projects.

Rather, assume that Group Member **C** is the one that has the opportunity to invest in this project. When she is deciding whether or not to invest, she will only observe the value of Attribute **c**:

Attribute a	Attribute b	Attribute c	Successful / Unsuccessful
		7	

In addition, Group Member **C** will also observe whether other past projects that (i) had a value of 7 for Attribute **c** *and* (ii) that either **A** or **B** invested in in the past; were successful or not. (You will see more detailed information about this below.)

*Now, what information will be revealed after **C** makes her decision?*

If she **decides <u>not</u> to invest**, then nobody, neither **A**, **B** nor **C** will receive any further information about this project.

If she **decides to invest**, then **after** the investment has been made, Group Member **A** will observe the following data about the project:

Attribute a	Attribute b	Attribute c	Successful / Unsuccessful
10			Successful

while Group Member **B** will observe:

Attribute a	Attribute b	Attribute c	Successful / Unsuccessful
	5		Successful

Notice that, while Group Members **A** and **B** observe whether the project that Group Member **C** invested in is successful or not, **C** herself will only find out at the end of the experiment. Group Members will never receive immediate feedback on the success of their own projects.

Summary

The following summarises some of the key information from above:

- The game is completely symmetric for the three players (i.e. there is no difference between being assigned to be Group Member **A**, **B** or **C** at the start of the game. The only difference between players is the information they receive about the other two group members' past investments).
- Each of the three participants in your group will observe the value of one of the three attributes for: (i) all the projects they face, as well as (ii) all projects that other members of their group have previously invested in. Each will observe a different attribute.
- You will only observe the outcome of projects that your group members invested in.
- You won't observe the outcome of the projects that you invest in until the end of the experiment.
- The relationship between an attribute value and the chances of the corresponding project's success will stay the same throughout the experiment.
- The information on past projects accumulated in Phase 1 is carried over to Phase 2.

Collecting and summarising the information about past projects

Since the other members of your group may invest in many projects, the computer will organize the information about these past projects for you.

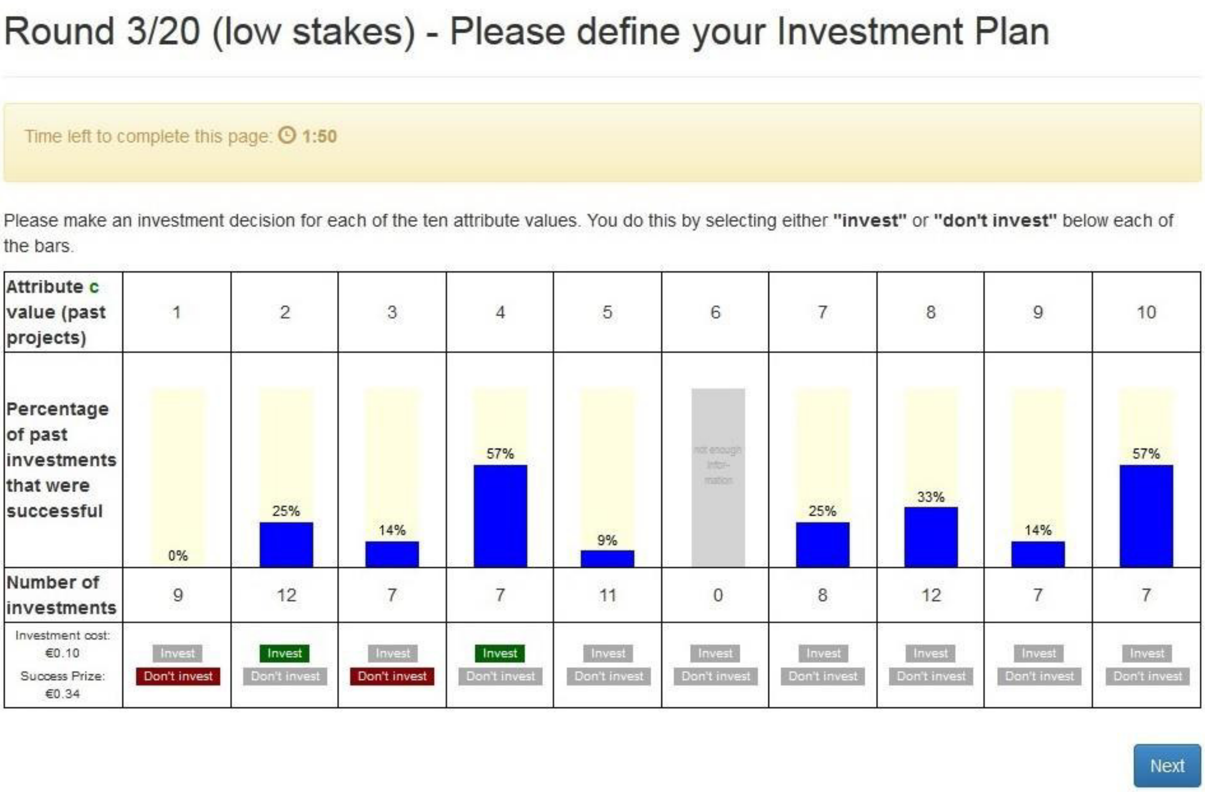
Instead of showing you the individual data from each of the projects that other members of your group have invested in, the information for all past projects invested in by other members of your group will be summarized in the following way. If you are Group Member **C**, then the computer will collect together all of these projects which have the same Attribute **c** value and tell you:

(i) The number of times other members of your group (**A** and **B**) invested in a project with that value of Attribute **c**; and

(ii) The proportion of projects invested in by other members of your group (**A** and **B**) with the same Attribute **c** value that were successful.

Specifically, Group Member **C** will see a screen that looks similar to the following – except with different information on success rates (other participants will see a similar screen, corresponding to their own attribute):

Figure 1: The Information Screen of Group Member C about Past Projects



Note: In the example in Figure 1, Group Member **A** and **B** have invested in 7 projects with an Attribute **c** value of 4. Of these 7 projects, 57% were successful. Group Member **C** sees this information.

Timeline for decision making

The text above has described the information that you will receive when making a decision about investing in a **single new project**. However, in the experiment, there will be 10 rounds of investment decisions in Phase 1 and 10 rounds of investment decisions in Phase 2. This makes 20 rounds of investment decisions in total. In each of these rounds of investment decision, you will be asked to state whether you would like to INVEST or NOT INVEST in **50 projects**.

In each round, the way you will do this is by telling the computer whether you would like to INVEST or NOT INVEST in projects with each of the 10 possible Attribute values (i.e. you make ten decisions and are providing the computer with an "Investment Plan" for how to act on your behalf – see the decision buttons in the bottom row of Figure 1). In each round, the computer will then see 50 randomly selected new projects, and it will follow your "Investment Plan" to decide whether to INVEST or NOT INVEST in each of these projects on your behalf. So, you make 10 decisions, and the computer uses these decisions to decide whether to INVEST or NOT INVEST in 50 projects on your behalf.

Providing the computer with an "Investment* Plan*" for investing on your behalf

More specifically, we will ask you to report whether you would like to INVEST or NOT INVEST in **projects with each possible Attribute value between 1 and 10**. Therefore, during each round of decisions, we will ask you to make 10 investment decisions[2] – one for each possible Attribute value between 1 and 10. The other members of your group will also report whether they would like to INVEST or NOT INVEST for each Attribute value between 1 and 10 for the Attribute they observe (Attribute **a** for Group Member **A**; **b** for **B**; **c** for **C**).

Computer acts according to your "Investment Plan"

In each round of decisions, after every member of the group has made their 10 decisions, each group member will face 50 randomly selected new projects. Given the choices that you have made, the computer will look at the relevant Attribute value of each of these 50 projects you face and then INVEST or NOT INVEST, according to the "Investment Plan" you gave it (i.e. if you draw a project which has an Attribute value of 6 and you said that you would like to invest in projects with an Attribute value of 6, then the computer will invest in this project on your behalf). You can change your "Investment Plan" for the next round of decisions.

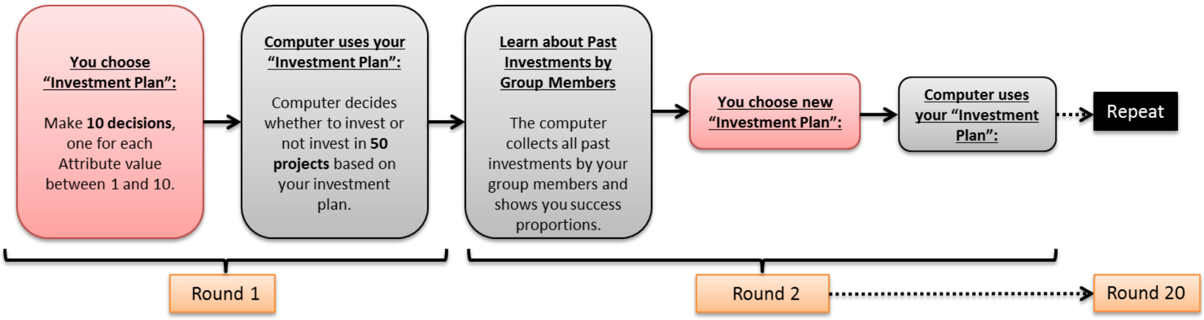
The "Investment Plan": An example:

If this is a little bit complicated, it might be simpler to think about the "Investment Plan" that you give the computer in the following way. If you are Group Member **C**, and you select INVEST for an Attribute **c** value equal to 6, then you are telling the computer: "In this round, every time you see a project with an Attribute **c** value of 6, please choose INVEST on my behalf". The computer will then go through 50 randomly chosen new projects and act on your behalf as you have instructed it.

[2] i.e. we will ask if you would like to INVEST or NOT INVEST when the Attribute value is 1; when the Attribute value is 2; when the Attribute value is 3; …; when the Attribute value is 10.

Updating the information you observe about past projects:

Once these 50 project decisions have been made, your database of information regarding the past success rates of projects invested in by other Group Members will be updated with their new investments, and you will be given the opportunity to revise your "Investment Plan" which will determine your investment strategy for the next 50 projects.



How your payment will be calculated

As described above, in every round of investment decisions, you will make 10 decisions. These 10 investment decisions will provide the computer with an "Investment Plan" for how to act on your behalf when faced with the next 50 randomly drawn new projects. **In every round, one of these 50 projects will be randomly selected to be the one that affects your payment in the experiment.** For this project selected for payment, the computer will act as you have instructed it to and either INVEST or NOT INVEST. If your "Investment Plan" prescribes that the computer invest for this particular project then the contribution to your payment will depend on whether the project is *successful* or *unsuccessful*, as described above.

- Therefore, the **maximum** you can earn in Game I is: 10 * €0.34 + 10 * €3.40 = **€37.40** if you always invest and every project you invest in is successful.
- The **minimum** you can earn in Game I is **€0** if you always invest and every project you invest in is unsuccessful.
- If you never invest in Game I, you would earn: 10 * €0.10 + 10 * €1 = **€11**.

You will not learn about the outcomes of your own investments during the experiment. At the end of the experiment, you will be informed about the investment decisions that you made that are relevant for your payment. In each of the two phases, for the ten projects chosen to contribute to your payment, you will learn: the number of projects that you chose to invest in, the number that were successful, and how they contributed to your final payment.

We will now proceed to Game I. Before we do, if you have any questions at this moment, please raise your hand. The experimenter will come to you.