WORKING PAPERS

N° 1561

August 2024

"Return Predictability, Expectations, and Investment: Experimental Evidence"

Marianne Andries, Milo Bianchi, Karen K. Huynh, Sébastien Pouget

Toulouse School of Economics

Return Predictability, Expectations, and Investment: Experimental Evidence *

Marianne Andries[†] Milo Bianchi[‡] Karen K. Huynh[§] Sébastien Pouget[¶]

August 2024

forthcoming in *The Review of Financial Studies*

Abstract

In an investment experiment, we show variations in information affect belief and decision behaviors within the information-beliefs-decisions chain. Subjects observe the time series of a risky asset and a signal that, in random rounds, helps predict returns. When they perceive the signal as useless, subjects form extrapolative forecasts, and their investment decisions under-react to their beliefs. When they perceive the signal as predictive, the same subjects rationally use it in their forecasts, they no longer extrapolate, and they rely significantly more on their forecasts when making risk allocations. Analyzing investments without observing forecasts and information sets leads to erroneous interpretations.

Keywords: Return Predictability, Expectations, Long-Term Investment, Extrapolation, Model Uncertainty.

JEL codes: G11, G41, D84.

*For helpful comments and discussions, we would like to thank Elena Asparouhova, Tiziana Assenza, Matthieu Bouvard, Thomas Chaney, Cary Frydman, Xavier Gabaix, Fabian Gamm, Valentin Haddad, David Hirshleifer, Emir Kamenica, Yaron Levi, Sophie Moinas, Pierre-Olivier Weil, and seminar participants at TSE, Collegio Carlo Alberto, Sciences Po, MIT Sloan, Oxford, USC, UCLA, Paris-December Meeting 2020, Macro-Finance Society 2021, WE_ARE Seminar Series (2023), WAPFIN NYU Stern (2023); as well as Stefano Giglio and two anonymous referees. We acknowledge funding from the TSE Sustainable Finance Center and from ANR (ANR-17-EURE-0010 grant).

[†]University of Southern California. E-mail: andries@usc.edu

[‡]Toulouse School of Economics, TSM, and IUF, University of Toulouse Capitole, Toulouse, France. E-mail: milo.bianchi@tse-fr.eu

[§]Amundi. E-mail: karine.huynh-ext@amundi.com

[¶]Toulouse School of Economics and TSM, University of Toulouse Capitole, Toulouse, France. E-mail: sebastien.pouget@tse-fr.eu

1 Introduction

How do investors form their expectations about risk and return? How do these expectations affect their investment decisions? While the first question, and how information affects beliefs, has been extensively studied, it's only recently that the research has focused on the "beliefs to decisions" channel. The empirical finance literature documents a puzzling fact: investors adjust their portfolios too little in response to changes in their own beliefs, compared to the classical Merton-Samuelson investment model (see Giglio et al., 2021a,b). We propose an investment experiment with information treatments that allows us to better understand the mechanisms underlying this puzzle. We find that variations in the information subjects observe affect not just their forecasts and investments, but also how they *form* their beliefs and how they *use* their beliefs in their investment decisions. In the baseline, subjects have extrapolative forecasts and make risk decisions similar to those observed in Giglio et al. (2021a); and our results replicate the low investment sensitivity to forecasts puzzle they document. However, when given more information, the *same* subjects change their forecast model – they no longer extrapolate; and their risk decisions respond more elastically to their own beliefs, closer to the classical Merton-Samuelson model, and to the behavior of large asset managers (see Dahlquist and Ibert, 2024). These within-individual variations in forecast and investment behaviors operate in all subject subgroups sorted on observable individual characteristics, indicating they likely extend to real investors.

Relying on the experimental methodology is key for us to analyze the information-beliefs-decisions chain. First, it gives us full control over which information agents have access to, on their prior beliefs, on their portfolio constraints and on the risks they face. Varying these inputs across information treatments allows us to distinguish agent-specific from information-specific behaviors. Second, it enables us to collect the data, within and across subjects, on both beliefs (forecasts) and decisions; a crucial distinction from most empirical evidence on investors in naturally-occurring markets. As we show below, this is key to understand investors' behaviors: analyzing our subjects' investments without the belief data leads to erroneous interpretations.

Our experiment replicates, as much as possible, the risks and information accessible to investors making decisions in the field. Moreover, our choice of design is motivated by several considerations and observations from field data. First, predictive information is publicly available to market participants, hence possibly affecting their time-varying beliefs and risk allocations. Second, the evidence shows investors' forecasts deviate from the rational expectation model: they under-utilize actual predictive variables in the data (Nagel and Xu, 2023), while extrapolating too strongly from past returns – a bias documented extensively in the macroeconomic and finance literature (see e.g., Shiller, 2000; Dominitz and Manski, 2011; Greenwood and Shleifer, 2014; Assenza et al., 2014; Manski, 2018; Bordalo et al., 2020; Beutel and Weber, 2022; Afrouzi et al., 2023). Third, there is widespread evidence of sub-optimal investment decisions, be it due to inertia (see e.g., Brunnermeier and Nagel, 2008; Calvet, Campbell, and Sodini, 2009), or to behavioral biases (e.g., the disposition effect, Odean, 1998). Mimicking, in our experiment, investors' information and risk opportunities may thus prove fruitful to better understand the mechanisms via which agents depart from rational beliefs and optimal decisions, with clear implications for households' portfolio choices and wealth.

Our experimental design emulates the canonical case of an investor who, first, gathers information to forecast asset returns; and, second, makes portfolio decisions. We vary the information investors receive and study how it affects each of these two steps and, most importantly, their potential interactions. More precisely, our experiment proceeds as follows.

Subjects are shown time-series displays of two variables, labeled "Index Return" and "Variable A", over several rounds, each corresponding to new, independent, simulations. "Index Return" is simulated, in all rounds, from the same process designed to reproduce the US equity index 5-year returns in its mean and volatility, and with zero time series persistence. "Variable A" also has the same unconditional distribution in all rounds; but it is simulated to predict "Index Return" differently across rounds. In some rounds, it is useful to predict returns, and, to mimic signals available to real market investors, we let "Variable A" have the same persistence and the same predictability power over "Index Return" as the US equity index dividend-price ratios over equity returns at a 5-year horizon (see e.g., Fama and French, 1988; Cochrane, 2009). In these rounds, "Variable A" and "Index Return" are correlated variables. In the other rounds, "Variable A" is uncorrelated to "Index Return" and useless to predict returns.

To best study the role of information in our experiment, we impose a high level of ex-ante uncertainty. Subjects are just told that: 1) "Variable A" helps predict "Index Return" in some rounds, though we do not specify which ones nor what is their likelihood (we let subjects infer from the time-series display whether "Variable A" seems predictive of "Index Return", each round), 2) all rounds are independent, and 3) the average "Index Return" value is 6.07%. Points 1) and 2) discipline which information subjects may use each round and how; point 3) pins down the unbiased average "Index Return" forecast.

Each round, subjects are incentivized: i) to state whether they believe "Variable A" is useful, this round, to predict returns, ii) to give us their forecasts for the next-period "Index Return", and iii) to invest an endowment, that we renew each round, between the risky "Index Return" and a riskless cash asset. At the end of each round, we provide them feedback on all three tasks.

We find that whether or not subjects perceive "Variable A" as useful greatly affects their forecast and investment behaviors. When they view "Variable A" as useless, subjects have extrapolative forecasts: they use the last realization of "Index Return" to make their next-period predictions. This finding matches the evidence in the macroeconomic and finance literature (see above) qualitatively and quantitatively: our subjects have extrapolative biases of the same magnitude as in previous experimental work (Landier, Ma, and Thesmar, 2019; Afrouzi et al., 2023). When they view "Variable A" as predictive, the *same* subjects no longer extrapolate. They use "Variable A" exclusively to make their "Index Return" next-period forecasts; and their beliefs vary with "Variable A", in these rounds, consistently with a model of rational expectations under partial information.

This first set of results establishes that our information treatment generates two distinct information-to-beliefs processes; switching from one to the other occurs within subjects and depends solely on the perceived source of information. This finding not only shows that extrapolative biases may not be robust to variations in information, it also invites us to analyze whether these within-subject variations may, in turn, induce variations in beliefs-to-investments behaviors, keeping preferences constant and within a fully controlled risk and information framework.

We find subjects vary their investments one round to the next in line with their own forecasts; however, the magnitude of the pass-through from beliefs to investments differs across round types – perceived as predictable by "Variable A" or not. Investment decisions are more than twice as sensitive to variations in forecasts coming from "Variable A" in rounds where it is perceived as predictive, than they are to extrapolative forecasts in rounds where it is perceived as useless.

To interpret subjects' investments, we confront them to the classical portfolio choice model (Merton, 1969), which provides tight predictions about the average ratios of investments to beliefs *across* round types; and about the elasticities of investments to beliefs *within* round types. We show, first, that subjects increase their average investments when they perceive "Variable A" as informative strictly as predicted by the classical model under unbiased perceptions of the relative conditional variances across round types. In an extension to our baseline experiment, we ask subjects to provide 80% confidence intervals around their forecasts, and we confirm they have unbiased average risk assessments in both round types. However, we find, second, the sensitivity of investments to forecasts is too low compared to the classical framework, in both round types; it is four times too low for extrapolative forecasts.

Our results on average investments and on investment elasticities can be reconciled by a modified Merton model whereby subjects display cognitive uncertainty (Enke and Graeber, 2023) when forming their "effective" beliefs – i.e., the beliefs they use to make decisions. Instead of moving one-for-one with forecasts, beliefs update partially around their average level, depending on how uncertain subjects are about their interpretation of information; where beliefs "stickiness" is determined by a cognitive uncertainty parameter which fully captures how subjects decisions depart from the classical framework. Our estimates of this parameter quantify the greater cognitive uncertainty about extrapolative forecasts than those informed by "Variable A". In our framework, subjects *know*, as explicitly told, that "Variable A" is predictive in some rounds; they *think* extrapolation may help predict returns. The difference is reflected in how they use their own forecasts to make their risk decisions, and our experimental estimates of cognitive uncertainty.

We extend our analysis in several directions. First, we elicit subjects' perceptions of "extreme" returns – probabilities that next-period returns exceed the +15% upper bound, that they fall below the -3% lower bound. We find they over-estimate the likelihood of both the upper and the lower bounds low probability events, and display a preference of skewness: they increase (decrease) their investments when they perceive upper (lower) bound probabilities as higher, independent from their forecasts. Second, we analyse and reject that heterogeneity in subjects' characteristics substantially change the pattern of forecast and investment behaviors; even though our cognitive uncertainty estimates vary across subgroups, e.g., subjects with higher education have lower uncertainty. Third, in additional information treatments, we vary how easily interpretable the "Variable A" signal is to form forecasts; our results confirm the cognitive uncertainty interpretation.

Next, we verify whether the separate information-beliefs-decisions paths we document for each round type could be identified using subsets of our experimental data, e.g., only investments, as often observed in the field. We show that such analyses lead to the erroneous interpretation that subjects always under-react to information, and have close to no extrapolative biases. Finally, we discuss the external validity of our findings and their implications for individual investors' optimal decisions, as well as for the dynamics of investors' demand and equilibrium asset prices.

After a review of the literature, we present our experimental design in Section 2. In Section 3, we describe the main results of our experiment; and in Section 4 how to interpret them. Section 5 provides additional results and robustness checks. In Section 6, we discuss the implications of our results. Section 7 concludes. Additional results are provided in the Online Appendix.[1]

Related literature.

Giglio et al. (2021a) elicit market forecasts from a large pool of Vanguard investors, and analyze their portfolio positions. They find that investors' beliefs, which are extrapolative, have limited impact on their risk taking decisions. This finding is replicated in Giglio et al. (2021b), who study how investors' expectations about stock returns varied during the COVID-19 crash, and how they adjusted their portfolios over that period. In contrast, Dahlquist and Ibert (2024) study

[1] The Online Appendix is available at https://sites.google.com/site/marianneandries/

professional asset managers and find they have counter-cyclical expectations, in line with the dividend-price ratio predictability of Fama and French (1988); and these forecast variations affect their risk decisions, with a higher pass-through than in Giglio et al. (2021a).

In Section 6, we show our results across round types match both sets of evidence, qualitatively *and* quantitatively, even though they are obtained within subjects in a controlled environment that excludes well-known sources of inertia, e.g., inattention, transaction costs and anchoring on prior decisions. This suggests that the differences between Giglio et al. (2021a) and Dahlquist and Ibert (2024) may not be due to differences in their investors' preferences or exogenous constraints to dynamic portfolio re-allocations but to differences in access to information and the resulting confidence – or cognitive uncertainty – in one's own forecasts.

That not just the quantity of information but also the "type" of information received affects our subjects' model of belief formation is consistent with various works in the literature, such as Gabaix (2019) on sparsity, Bordalo, Gennaioli, and Shleifer (2012) on salience; as well as with experimental evidence on information processing, Woodford (2020); Frydman and Jin (2022); Enke and Graeber (2023). Our findings complement these papers by showing that differences in the source of information can also change the model of decision making, i.e., the pass-through from beliefs to investments.

Liu and Palmer (2021) compare surveys of beliefs on real estate markets to investment choices into a housing fund, from experimental data, and find that they load on different sources of information. Though these results differ from ours – our subjects do not use information other than in their forecasts to make their investment decisions – they confirm the standard information-to-beliefs-to-decisions chain needs to be revisited. Barberis and Jin (2023) propose a theoretical framework doing so, whereby actions follow an experience-based model-free approach while beliefs are model-based and extrapolative. These assumptions are tailored to fit the empirical evidence on investors' surveys of beliefs – which are extrapolative on average – and portfolios – which appear influenced by investors' own life experience (Malmendier and Nagel, 2011). They cannot, however, explain our experimental results.

Finally, our results are closely related to two recent experimental works. Beutel and Weber

(2022) conduct a randomized information field experiment on a representative sample of German households to whom they ask their forecasts and what risk investments they would hypothetically choose if given wealth to invest. Similarly to us, they find that subjects tend to excessively extrapolate from past returns. They also show that different investors display different mental models when forming expectations, which complements our result that different forecast models coexist *within* investors when facing different information treatments. Our finding that providing useful information can induce beliefs closer to rational expectations is distinct from Beutel and Weber (2022); it highlights the importance of the way in which information is presented (Ungeheuer and Weber, 2021), with or without graphical displays. In an experiment on German stockholders, Laudenbach et al. (2023) find that an information treatment where subjects are graphically shown there is no auto-correlation in returns makes their beliefs closer to rational expectations.

Another result distinct from Beutel and Weber (2022) is that our subjects' investment choices are closer to the classical Merton model when their beliefs are based on the predictive signal we provide, indicating cognitive uncertainty varies depending on the source of information. This relates to the experiment in Charles, Frydman, and Kilic (2024), who adapt Enke and Graeber (2023) to study how the certainty equivalents of risky lotteries vary with beliefs, for subjects who face tasks of different cognitive "complexity" (see, e.g., Woodford (2020)): they either receive informative signals to update their payoff distributions, or are explicitly told what the distribution is. The authors find that subjects with the complex task, i.e., who have to interpret the information they receive, have a weaker transmission between their stated payoff distributions and their certainty equivalents. This result on the weak transmission between belief distributions and certainty equivalents complement ours on the low sensitivity of investments to forecasts; and also obtains in Enke et al. (2024)'s large scale analysis of diminished sensitivities of decisions to information as a result of cognitive information-processing constraints. Charles, Frydman, and Kilic (2024)'s framework differs considerably from our investment game and from real investors decisions, both in the actions subjects take and in the information treatments.[2] Their experimental paradigm allows them to measure the impact of complexity on cognitive uncertainty; ours allows us to mimic real investors'

[2]In addition, their experiment does not allow to observe variations in decisions' sensitivity to beliefs within subjects.

decisions when facing different predictive signals and to confront our results to the evidence from the field (e.g., Giglio et al., 2021a; Dahlquist and Ibert, 2024).

2 Experiment

2.1 Design

2.1.1 Baseline treatment

Our experiment is designed to mimic the market risk real investors face and to allow us to study how their beliefs and portfolio decisions vary with the information they receive.

Subjects observe, in successive independent rounds, graphic displays of the past realizations of an "Index Return" – in bold red; and of a "Variable A" – in dotted blue; where a yellow dot marks the last realization of "Variable A". Subjects are explicitly told that "Variable A" helps predict returns in some rounds, but is useless in others; and that all rounds are independent. We provide subjects with examples of the displays with either predictive or un-predictive "Variable A" at the beginning of the experiment, as shown in Figure 1. Subjects are also given the average value of the "Index Return". No other information, e.g. on the return process or on how "Variable A" can be used to predict returns, is given in the baseline treatment.

Subjects are asked, each round: 1) whether or not they believe, looking at the time series display, that "Variable A" is useful to predict returns; 2) what their forecasts are for the next-period "Index Return"; and 3) how much they want to invest, out of a 100 ECU (Experimental Currency Unit) endowment we renew each round, in the risky "Index Return".[3]

Feedback information is given at the end of each round: whether "Variable A" was predictive, or not, this round; what the next-period "Index Return" turned out to be; how much subjects' investment portfolios made. The time series display is updated to add the final "Index Return" realization – with a yellow dot, similar to that of "Variable A".[4] Subjects then move on to the next round, endowed with a new 100 ECU, irrespective of the returns realized in previous rounds.

[3] Subjects provide their answers in "boxes" that are made blank at the beginning of each round: past answers do not appear one round to the next, and neither do "by default" numbers, e.g., a 50% risk investment, so as not to influence the outcomes of the experiment.

[4] The instruction sheet and examples of the feedback information subjects receive can be found in Appendix C.

To mirror real investors' market risk, we simulate the "Index Return" time series to mimic the US equity returns averaged over 5-year periods – a realistic buy-and-hold investment horizon, given the low trade frequencies often observed in the data (Alvarez, Guiso, and Lippi, 2012; Sicherman et al., 2016). To mirror real investors' financial market information environment, we simulate "Variable A", in rounds where it is predictive, to mimic the predictive power of dividend-price ratios for the following 5-year returns (Fama and French, 1988; Campbell and Shiller, 1988) – i.e., predictive signals real investors can readily obtain when making their portfolio decisions.

Across *all* rounds, the "Index Return" time series is simulated to have the same average return, the same average volatility, and, crucially, no serial autocorrelation in returns i.e., no predictable persistence. Similarly, "Variable A" is simulated to have the same average value, the same average volatility, and the same persistence, across all rounds. Visually, the time series variations look exactly similar across rounds, *except* for the co-movements between "Index Return" and "Variable A" which differ across round types (predictable or not); the key to our experimental treatment.[5]

In rounds where "Variable A" is not informative, the process r_t of "Index Return" is simulated according to the random walk:

$$r_{t+1} = \mu + \epsilon_{t+1}, \tag{1}$$

where $\{\epsilon_t\}$ are i.i.d. normally distributed shocks $\epsilon_t \sim \mathcal{N}(0, \sigma^2)$.

In rounds where "Variable A" is informative, the predictable process r_t^p of "Index Return" is simulated according to:

$$r_{t+1}^p = a_t + \epsilon_{t+1}^p, \tag{2}$$

where a_t is the realization at time t of the "Variable A" and $\{\epsilon_t^p\}$ are i.i.d. normally distributed shocks $\epsilon_t^p \sim \mathcal{N}(0, \sigma_p^2)$. We use the parameters of the return-dividend yield VAR model estimated by Cochrane (2009) on US equity returns (CRSP data, period 1927-1998).[6] The predictive power of "Variable A" in process (2) is measured by $Corr(r_{t+1}^p, a_t) = 57\%$ and $\sigma_p^2 = 0.67\sigma^2$.[7]

[5] "Index Return" and "Variable A" unconditional distributions are statistically indistinguishable between rounds. Kolmogorov–Smirnov tests for distributions on arbitrary pairs of the displayed simulated returns drawn from the two types of rounds have an average p-value equal to 0.497.

[6] $\mu = 6.07\%$, $\sigma = 9.02\%$; a_t follows an AR(1), with mean μ, persistence $\rho_a = 0.66$, and volatility $\sigma_a = 3.98\%$.

[7] We describe our simulation method in Appendix B.

Throughout, we refer to process (1) as the "i.i.d." case, to process (2) as the "predictable" case.

2.1.2 Additional treatments and outcome variables

In addition to the three questions – 1) is "Variable A" informative or not, 2) next-period forecasts, 3) next period investments, we also elicited subjects' perceptions of risk. We proceeded in two ways to do so. In one experiment, we asked subjects to provide 80% confidence intervals around their own forecasts, each round. In another, we asked them to answer these two questions about next-period returns: "What is the probability that the index return is higher than 15%?" and "What is the probability that the index return is lower than -3%?". The advantage of the first approach is that it allows us to verify if subjects have the correct perception of the index returns volatility; the advantage of the second approach, which follows Giglio et al. (2021a), is that it allows us to determine whether subjects overestimate the risks of low probability events.

We also experimented on information treatments other than the baseline where we varied how easily interpretable the "Variable A" signal is. In one experiment, we asked subjects to provide their forecasts and investments over the following cumulative five periods. In contrast to the one period forecast, for which it is necessary and *sufficient* to identify a_t as the best forecast for r_{t+1} when "Variable A" is predictive, the long-horizon average forecast requires to also estimate the dynamics of the "Variable A" process, for which no information is explicitly given in the experiment. The rational forecast rule for 5-period average returns appears considerably more difficult to evaluate from the time series displays we provide,[8] so this treatment corresponds to making information less accessible than the baseline.

In two other experiments, we made, instead, "Variable A" easier to interpret. In the first, we asked subjects to play the investment game in rounds where they were explicitly told when "Variable A" was useful and when it was not, before they had to make their next-period forecasts and investments. In the second, we revealed to subjects the simulation processes (1) and (2) before they played the investment game, but not which rounds "Variable A" was predictive or not.

[8] A fully informed rational forecaster would derive, under the simulations of processes (1) and (2): $\mathbb{E}_t\left(\overline{r}_{t+1,t+5}\,|_{\text{i.i.d.}}\right) = \mu$ and $\mathbb{E}_t\left(\overline{r}_{t+1,t+5}\,|_{\text{predictable}}\right) = \kappa a_t + (1-\kappa)\mu$, where $\overline{r}_{t+1,t+5}$ is the average return over five periods starting at $t+1$; a_t is the realization of "Variable A" at time t; and $\kappa < 1$ depends on ρ_a, the persistence of "Variable A": $\kappa = \frac{1}{5}\frac{1-\rho_a^5}{1-\rho_a} = 0.51$.

2.2 Implementation

In the baseline treatment, we let subjects play for twenty rounds. Ten rounds were simulated with i.i.d. process (1), and ten with predictable process (2). The order of the graphs was randomized across subjects. They were not told that "Variable A" was useful in precisely half the rounds.

As compensation for participating in the experiment, subjects received 5 ECU for every correct answer regarding whether "Variable A" was predictive and 10 ECU for every "precise" forecast in a $(-1\%, +1\%)$ interval of the return realization. In addition, they received their full portfolio ECU value from one randomly drawn round of the experiment.[9]

This compensation scheme was designed to incentivize subjects to provide truthful answers on whether they viewed "Variable A" as predictive or not, and on their best forecasts; and to encourage them to carefully optimize their risk investments. Because the likelihood of "winning" a precise forecast was low – under processes (1) and (2), the realized next-period returns have 11% chance of being in the $(-1\%, +1\%)$ interval around the fully informed rational conditional expectation, on average – the risk that subjects might choose to "hedge" between their forecast answers and their investment decisions was small. Finally, because the portfolio compensation derived from a single round randomly chosen at the end of the experiment, the scope for wealth affecting risk taking decisions differently across rounds is limited.

To verify the simulated data correctly represents either the i.i.d. process (1) or the predictable process (2), we regressed the returns $\{r_t\}$ in each simulation on the predictive variable $\{a_{t-1}\}$ and on the previous realized returns $\{r_{t-1}\}$. The results (Online Appendix Table C.1), are consistent with our simulation strategy: the regression coefficients of r_t on r_{t-1} are close to 0 in all rounds;[10] the regression coefficients of r_t on a_{t-1} are close to 1 with R^2 close to $R^2 = 0.33$ of process (2) in the predictable rounds and around 0 (and not significant) in the i.i.d. rounds.[11]

Our experiment was implemented in four waves.

[9] When we elicited both short and long-horizon investments, we randomly selected either one for compensation.

[10] In two outlier i.i.d simulations, r_t has a small but significant *negative* loading on r_{t-1} (p-value = 0.04, and 0.06), though it did not appear to affect subjects' answers.

[11] Even though $Corr(r_t, a_t) = 0$ under both processes (1) and (2), the 20 final draws for "Index Return" and the 20 final draws for "Variable A" are statistically correlated, with correlation -25%, in our simulated data. For this reason, we often present results obtained when regressing on the last realized r_t and a_t separately, rather than simultaneously, in the rest of the paper.

2.2.1 Master of Finance students

In the first wave of our experiment implementation (January 2019), we recruited 58 participants, students in the Master of Finance at the University of Toulouse Capitole / Toulouse School of Economics (TSE). In addition to the baseline treatment, we asked subjects their forecasts and investments for the full five-periods ahead, over the same twenty rounds of the game.

We recruited 36 students from the same Master in the second wave (January 2020). We asked subjects to provide 80% confidence intervals around their own forecasts, and, after they finished the baseline treatment, to play for another twenty rounds where they were told when "Variable A" was useful and when it was not.[12]

The experiments took place in the University's computer lab on an application we built using the Otree framework (Chen, Schonger, and Wickens, 2016). After logging in, subjects saw detailed instructions, including a description of the tasks and of the payment rules, as well as one example of a predictable round display and one example of an i.i.d. round display (see Appendix C). They could ask questions at any time during the session. All questions were asked and answered privately.

We conducted a third wave in March 2021 with 26 subjects from the same Master's program. After they finished the baseline treatment, subjects played another ten rounds where they were told when "Variable A" was useful and when it was not; then, we revealed the simulation processes (1) and (2), and subjects played for ten additional rounds.[13] The third wave was conducted online, due to strict COVID-related lock-downs. Subjects were invited to join a zoom session that allowed them to interact with the experimenter during the experiment. They accessed the same application as in the previous two waves, and were told they could ask questions via private message on zoom.[14]

In addition to the answers we obtained directly from subjects in the first three waves of the experiment, we also collected their grades in the Master of Finance program, and their gender.

Subjects received as compensation for participating in the experiment a Euro amount equal to

[12] Subjects played the same twenty rounds as the baseline, in a new randomized order.

[13] We randomly selected five i.i.d rounds and five predictable rounds from the twenty rounds of the baseline treatment, in each additional treatment.

[14] In the lab, many subjects asked that we explain the 80% confidence intervals. Absent such clarification, online subjects appeared to mis-understand the question, with, e.g., constant 10% and 90% returns thresholds throughout, despite variations in forecasts, so we do not account for their answers on confidence intervals.

their total ECU payoff, divided by 20, resulting in an average payment of 12 Euros.

2.2.2 Online subjects, Prolific

TSE students may have, as just starting a Master of Finance, more financial knowledge than the average population (albeit not necessarily than real investors in financial markets overall).

In the fourth wave of the experiment, we extended our subject pool and recruited subjects from Prolific, an online survey and experiment platform.[15] Because of the time and effort it takes to complete our experiment – the average time of completion is greater than one hour in the first three waves, it was both difficult and costly to attract online subjects. We recruited 94 subjects from Prolific, over several weekends in June and July 2023. They played only the baseline treatment, but were also asked their upper bound and lower bound probability perceptions (probability of next-period returns above 15% or below -3%) each round.

Subjects accessed the same application as in the first three waves. We added several attention checks over the experiment, standard to online subject pools. If subjects failed the attention checks, they were removed from the experiment and received no compensation. In addition to the answers collected in the experiment, we added survey questions to gather information on subjects' gender, age, income bracket, education and level of financial literacy (see Appendix C).

As compensation for participating in the experiment, subjects received a dollar amount corresponding to their total ECU payoff divided by 10, subject to a minimum participation fee of $5, as imposed by Prolific compensation rules.

In contrast to the first three waves of the experiment, the participation fee provided an incentive for some subjects to sign up and exercise no effort in the investment game. The time spent on the experiment, a standard measure of effort in the lab, does not allow us to identify such subjects, as we could not control what other activities subjects may have been involved in while playing the investment game online. We opted for another, indirect, measure of effort: we imposed a threshold on the number of correct answers when identifying "Variable A" as predictive or not, such that any subject with 11 or less correct "Variable A" answers in the 20 rounds of the baseline treatment

[15] https://www.prolific.com/

was removed from our pool. This threshold, which removed 37 Prolific subjects, was determined *before* we analyzed subjects' forecasts and investments. We chose it because: 1) despite being low, i.e., remaining subjects can still be incorrect 8 rounds out of 20, it excludes with a 75% chance subjects who would choose purely random "Variable A" answers; 2) the remaining Prolific subjects have the same average number of correct "Variable A" answers, 15 out of 20, as the TSE Master of finance students of the first three waves, denoting they likely exercised a similar amount of effort.[16]

Our rationale for excluding subjects with 11 or less correct "Variable A" answers, determined *before* we analyzed their forecasts and investments, is that they are "playing" the game randomly, so their answers are uninformative to our analysis. *After* analyzing their forecasts and investments, we find compelling evidence supporting this assumption. Results, reported in Online Appendix Tables A.1 and A.2, show that online subjects with 11 or less correct "Variable A" answers do not use any available information to form their forecasts, i.e., they do not extrapolate from past returns or use the "Variable A" signal; and their own forecasts have no influence on their investments.[17]

Statistics on the remaining Prolific subjects' demographics is provided in Online Appendix Table A.3. Our subjects are evenly split in gender (46% identify as female); the median age is 38, with the youngest being 19 years old; 70% have some college education; 35% earn less than $50,000 per year and 19% earn more than $110,000 per year; finally they correctly answer an average 2.4 out of 3 questions on financial literacy, with more than half of subjects answering all three correctly.

3 Main results

Given our time series simulation methodology, the forecast for next-period "Index Return", at any time t, of a fully informed rational subject playing our experiment would be the constant μ in the i.i.d. case and the time-varying a_t, whose last realization is saliently displayed, in the predictable case. Under classical investment models, the risk taking decisions of the same fully

[16] We have no reason to believe Master of Finance students have a comparative advantage at "eyeballing" correlations than the rest of the population.

[17] Online Appendix Tables A.1 and A.2 report our results for all online subjects, i.e., for the third and fourth waves of the experiment.

informed rational subject would move in step with her forecasts in the predictable rounds (and be constant in the i.i.d. rounds), with a higher average risk investment in predictable rounds where the next-period "Index Return" conditional variance is lower than in i.i.d rounds. The subjects in our baseline treatment, however, play the investment game each round without knowing how it is simulated. We analyze how it affects their forecasts and investments, pooling the four waves of implementation; as well as subjects' reported risk assessments. We present below the main results we obtain for the baseline treatment. Descriptive statistics are in Table 1.[18]

3.1 "Variable A" information

To study how subjects' forecasts and decisions vary with the information they receive, we start by analyzing their ability to identify when "Variable A" is useful or not, and thus to separate i.i.d. versus predictable round.

Subjects correctly identify returns as predictable 82% of the time, and as unpredictable by "Variable A" 70% of the time (Table 1); significantly greater than 50%, if guesses were random (p-value < 0.01). The examples provided in Figure 1 show the difference between the correlated and uncorrelated rounds is far from visually obvious; making this first result notable. It speaks to people's ability to visually infer simple correlations, consistent with existing work in neuroscience and experimental finance (Wunderlich et al., 2011; Ungeheuer and Weber, 2021).[19]

Subject have a greater ability to identify information when it is useful rather than useless (82% $>$ 70% with p-value < 0.01). As a result, subjects perceive "Variable A" information to be predictive in 56% of rounds, as opposed to the true 50%. This finding is in line with previous studies that have shown that people have an innate desire to perceive patterns, and find it harder to identify randomness and the absence of correlations (Chapman, 1967; Tversky and Kahneman, 1973; Whitson and Galinsky, 2008). It may also reflect an optimism bias in over-interpreting the "Variable A" information as useful.

Taking into account how subjects interpret the information in "Variable A", we study their

[18] To be consistent, we also exclude from our pool of subjects TSE students with strictly less than 12 correct answers (8 students out of 120).

[19] Ungeheuer and Weber (2021) show correlated tail-events are harder to correctly assess.

forecasts and risk decisions, in the rest of the paper, in rounds they perceive as predictable versus rounds they perceive as unpredictable by "Variable A", which allows us to analyze how investors vary their beliefs and decisions according to their subjective information set.

3.2 Forecasts

Our experiment is designed to mimic real investors' market risk in an information environment where they always observe past market returns; as well as a signal that, in some rounds, mimics a real returns predictor (the price-dividend ratio) in the data. Our set-up is tailored to analyze what information they use to form their forecasts: past returns, i.e., extrapolative forecasts (see the literature review), or other available signals. Accordingly, to analyze forecasts, we run the following regression:

$$F_{i,k} = \alpha_1 + \alpha_2 \text{Predict}_{i,k} + \beta_1 a_{t,k} + \beta_2 a_{t,k} \times \text{Predict}_{i,k} + \delta_1 r_{t,k} + \delta_2 r_{t,k} \times \text{Predict}_{i,k} + \epsilon_{i,k}, \quad (3)$$

where $F_{i,k}$ is the forecast of subject i for next-period returns in round k; $\text{Predict}_{i,k}$ is a dummy taking value 1 if subject i perceives "Variable A" as useful to predict returns in round k; $a_{t,k}$ and $r_{t,k}$ are the last realizations of "Variable A" and "Index Return" in round k. The results are presented in Table 2.

Subjects use both the "Variable A" signal a_t and the past return r_t to form their forecasts (columns (1)-(2), Table 2). However, they use the "Variable A" signal *only* when they perceive it as useful (columns (3)-(5)): the loading on $a_t \times$ Predict is significant at the 1% threshold, the loading on a_t alone is not significantly different from zero. Subjects extrapolate from the past return *only* when they perceive other information ("Variable A") as useless (columns (6)-(8)): the loading on r_t alone is significant at the 1% threshold, the loading on r_t when Predict = 1 is not significantly different from zero (p-value = 0.71).

A one percentage point (p.p.) increase in r_t increases next-period forecasts by 0.18 p.p. in rounds perceived as unpredictable by "Variable A"; a one p.p. increase in a_t increases next-period forecasts by 0.37 p.p. in rounds perceived as predictable, controlling for individual and round fixed effects. Subjects' ability to exploit the information provided in predictable rounds and vary their beliefs accordingly translates into greater forecast accuracy: the distance between forecasts and next-period returns realizations is 7.7 p.p. in rounds perceived as predictable and 10.7 p.p. otherwise (Table 1), a significant difference (p-value < 0.01).

These results obtain with or without controlling for individual and round fixed effects. The forecast pattern – using "Variable A" only in rounds where it is perceived as predictive vs. using extrapolation otherwise – is true both between and within subjects.

3.3 Investments

Our experiment is designed to mimic real investors' market risk, to study how their decisions vary with the information they observe, and the forecasts they make. Accordingly, to analyze investment decisions, we run the following regression:

$$\theta_{i,k} = \alpha_1 + \alpha_2 \text{Predict}_{i,k} + \beta_1 F_{i,k} + \beta_2 F_{i,k} \times \text{Predict}_{i,k} + \epsilon_{i,k}, \tag{4}$$

where $\theta_{i,k}$ is subject i's investment into the risky fund (out of her 100 ECU endowment) in round k; $F_{i,k}$ is subject i's forecast of next period return, and $\text{Predict}_{i,k}$ is the "perceived predictable" dummy, as above. The results are reported in Table 3.

Subjects' stated beliefs about expected returns have an impact on their risk taking. An increase of one p.p. in forecasts translates into up to 1.67 ECU greater investments, significant at the 1% threshold (columns (1)-(3), Table 3). Subjects rely on their own forecasts more when they perceive returns as predictable by "Variable A": the loading on $F_{i,k} \times \text{Predict}_{i,k}$ is positive and significant (columns (4)-(6)). Controlling for individual and round fixed effects, an increase of one p.p. in the next-period return forecast results in an additional 1.38 ECU investment in rounds where "Variable A" is perceived as useless versus an additional $1.38 + 0.48 = 1.86$ ECU in rounds it is perceived as informative, a 35% greater pass-through from forecasts to investments.

These results obtain with or without controlling for individual and round fixed effects; they are true both between and within subjects. Those with significantly higher average forecasts have significantly greater risk investments; any given subject has a significantly higher risk investment in rounds where her next-period return forecast is above her own average; and both effects are amplified in rounds when "Variable A" is perceived as informative.

We extend the analysis of regression (4) to quantify the impact of information on portfolio decisions within the information-beliefs-decisions chain. As seen in Table 2, $\{a_t, r_t\}$ signals explain only some of subjects' forecast variations: the regression R^2s do not exceed 18% (with individual and round fixed effects). To isolate how investments are affected by forecasts directly attributable to $\{a_t\}$ signals, when "Variable A" is perceived as predictive, and to $\{r_t\}$ signals, when "Variable A" is perceived as useless, we use the two-stage least square specification:

$$\theta_{i,k} = \tilde{\alpha} + \tilde{\beta}\widetilde{F}_{i,k} + \tilde{\epsilon}_{i,k}, \tag{5}$$

where $\widetilde{F}_{i,k}$ is derived from the first-stage regressions

$$\begin{cases} F_{i,k} = \alpha_u + \underbrace{\beta_u r_{t,k}}_{\widetilde{F}_{i,k}} + \epsilon_{u,i,k} & |_{A \text{ perceived useless}} \\ F_{i,k} = \alpha_p + \underbrace{\beta_p a_{t,k}}_{\widetilde{F}_{i,k}} + \epsilon_{p,i,k} & |_{A \text{ perceived predictive}} \end{cases}, \tag{6}$$

and $\theta_{i,k}, F_{i,k}, a_{t,k}, r_{t,k}$ are as above. $\widetilde{F}_{i,k}$ corresponds to the "informed forecasts" of subject i in round k as opposed to the "noisy forecast" $F_{i,k}$. The results are reported in Table 4.

When "Variable A" is viewed as useless, the pass-through from forecasts to investments is unchanged wether forecasts are "informed" or not by the extrapolative signal r_t: the difference between 1.43 ECU and 1.56 ECU in columns (3)-(4), Table 4, is not significant (p-value = 0.78). When "Variable A" is perceived as predictive, the pass-through is close to double for forecasts "informed" by a_t: 3.19 ECU per p.p. change in "informed forecasts" versus 1.85 ECU for "noisy forecasts" (columns (1)-(2)).

That regressions (4) and (5) differ significantly only in rounds perceived as predictable by "Variable A" is a key result: it is the first to indicate that subjects use the information in "Variable A", which is truly predictive in some rounds, differently from the extrapolative information in "Index Return", which is actually useless throughout our experiment.

The greater pass-through from forecasts to investments, and from informative signals to investments, in rounds where "Variable A" is viewed as useful, has significant return implications for our subjects. Market timing their investments according to the signal a_t, when it is perceived as useful, increases their portfolios' expected returns by 7% (0.2 p.p.) in predictable rounds.[20]

3.4 Risk assessments

Subjects provide three separate measures of risk: their 80% confidence intervals (CI) around their forecasts, their probability estimates that next-period return will exceed +15%, and their probability estimates that next-period return will fall below -3%. We study how these risk assessments interact with the next-period forecasts and whether they affect investment decisions.

We find subjects vary their reported confidence intervals independently from their forecasts (-2% correlation in both round types), consistent with first and second moment estimates of normal distributions. To analyze the impact of variations in CI on investments, we run the regression:

$$\theta_{i,k} = \alpha_1 + \alpha_2 F_{i,k} + \beta_1 HighCI_{i,k} + \beta_2 F_{i,k} \times HighCI_{i,k} + \epsilon_{i,k}, \tag{7}$$

where $\theta_{i,k}$ and $F_{i,k}$ are subject i's investment and forecast in round k and $HighCI_{i,k}$ is a dummy variable equal to 1 if subject i's CI in round k is above her median CI for rounds of same type, perceived as predictable or not by "Variable A", as k. Results are provided in Table 5.

The loading on $F_{i,k}$ is significant and positive throughout; the loadings on $HighCI_{i,k}$ and on $F_{i,k} \times HighCI_{i,k}$ are overall not significantly different from zero: variations in confidence intervals, a measure of subjects' risk perceptions, have no significant impact on their investment decisions.

Turning to the upper and lower bound probability assessments, we find, first, that subjects vary

[20]From the results of regressions (3) and (5), $\theta_{i,k} = \tilde{\alpha} + \tilde{\beta}\beta_p a_t + \tilde{\epsilon}_{i,k}$, where $\tilde{\beta} \times \beta_p = 3.19 \times 0.37 = 1.18$ in rounds where "Variable A" is perceived as useful. Expected portfolio returns $R_{p,t+1} = \theta_t R_{t+1}$ are thus increased by $\tilde{\beta}\beta_p\sigma^2(a_t)$ (+ small positive Jensen terms) when returns are determined by simulating process (2).

them in line with their forecasts, with correlation 39% (−42%) for the probability that next-period returns exceed +15% (fall below -3%), consistent with a perceived distribution of risk centered on forecasts, and with the evidence in Giglio et al. (2021a). Second, to analyze the impact of variations in upper and lower bound probabilities on investments, we run the regressions:

$$\begin{cases} \theta_{i,k} & = \alpha_1^H + \alpha_2^H F_{i,k} + \beta_1^H HighProbHigh_{i,k} + \beta_2^H F_{i,k} \times HighProbHigh + \epsilon_{i,k}^H, \\ \theta_{i,k} & = \alpha_1^L + \alpha_2^L F_{i,k} + \beta_1^L HighProbLow_{i,k} + \beta_2^L F_{i,k} \times HighProbLow + \epsilon_{i,k}^L, \end{cases} \tag{8}$$

where $\theta_{i,k}$ and $F_{i,k}$ are as above, and $HighProbHigh_{i,k}$ ($HighProbLow_{i,k}$) is a dummy variable equal to 1 if subject i's upper bound probability (lower bound probability) in round k is above her median probability for rounds of same type, perceived as predictable or not by "Variable A", as k. Results are provided in Table 6.

The loading on $F_{i,k}$ is positive and significant overall; the loading on $HighProbHigh_{i,k}$ is significant and positive, the loading on $HighProbLow_{i,k}$ is significant and negative; the loadings on $F_{i,k} \times HighProbHigh_{i,k}$ and $F_{i,k} \times HighProbLow_{i,k}$ are mostly not significant; in both types of rounds (perceived as predictable by "Variable A" or not), with and without individual and round fixed-effects: subjects use their forecasts and, *independently*, their upper and lower bound probabilities to make their investment decisions. The coefficients in Table 6 are not only significant but large in magnitude: subjects invest up to 10.5 additional ECU (up to 14.3 fewer ECU) when they perceive a greater than median chance that next-period returns are above +15% (below -3%), in Panel C.

4 Mechanisms

4.1 Interpretation – forecast model

The results of Section 3.2 suggest that, as a forecast rule, subjects choose to use, each round, only one signal, which varies depending on "Variable A" being informative or not. This matches previous evidence in the literature on the propensity to rely on one variable at a time when making forecasts (e.g., Kruschke and Johansen, 1999; Hirshleifer and Teoh, 2003; Hong, Stein, and Yu, 2007). Using a limited subset of signals, as may be optimal under rational inattention, helps also explain mutual fund managers' decisions (Van Nieuwerburgh and Veldkamp, 2010; Kacperczyk, Van Nieuwerburgh, and Veldkamp, 2016).

Accordingly, we assume that, when "Variable A" is useless, subjects apply expectation model $\mathbb{E}^u(r_{t+1})$, which loads positively on r_t, the last realization of "Index Return";[21] whereas when "Variable A" is predictive, they apply expectation model $\mathbb{E}^p(r_{t+1})$, which loads positively on a_t, the last realization of "Variable A", such that:

$$\begin{cases} \mathbb{E}_t^u(r_{t+1}) & = \lambda_u r_t + (1-\lambda_u)\bar{\mu} \\ \mathbb{E}_t^p(r_{t+1}) & = \lambda_p a_t + (1-\lambda_p)\bar{\mu} \end{cases}, \tag{9}$$

where $\bar{\mu} = \mathbb{E}(r_t) = \mathbb{E}(a_t)$ under subjects' subjective expectations.

To decide when to apply model $\mathbb{E}^u(r_{t+1})$ or model $\mathbb{E}^p(r_{t+1})$, subjects assess, each round, whether "Variable A" is predictive or not. However, they know their assessments may be wrong, which we assume they take into account, such that their forecasts follow:

$$\begin{cases} \mathbb{E}_t\left(r_{t+1} \,|_{A \text{ perceived useless}}\right) & = \pi_u \mathbb{E}_t^u(r_{t+1}) + (1-\pi_u)\mathbb{E}_t^p(r_{t+1}) \\ \mathbb{E}_t\left(r_{t+1} \,|_{A \text{ perceived predictive}}\right) & = \pi_p \mathbb{E}_t^p(r_{t+1}) + (1-\pi_p)\mathbb{E}_t^u(r_{t+1}) \end{cases}, \tag{10}$$

where π_u and π_p correspond to the probabilities that a given subject assigns to the fact that "Variable A" is truly useless or predictive, conditional on the fact that she perceives it as such.

Under the model of Equations (9) and (10), forecasts follow:

$$\begin{aligned} F_{i,k} = \alpha_1^m + \alpha_2^m \text{Predict}_{i,k} + \beta_1^m a_{t,k} + \beta_2^m a_{t,k} \times \text{Predict}_{i,k} \\ + \delta_1^m r_{t,k} + \delta_2^m r_{t,k} \times \text{Predict}_{i,k}, \end{aligned} \tag{11}$$

where $F_{i,k}$ is the forecast of subject i for next-period returns in round k; $\text{Predict}_{i,k}$ is a dummy

[21] Such extrapolative beliefs can be derived from various psychological mechanisms, including the law of small numbers (as in Bianchi and Jehiel (2015) and Jin and Peng (2024)) and diagnostic expectations (as in Bordalo et al. (2019)), or simply from a lack of knowledge of the underlying price process (Adam, Marcet, and Beutel (2017), Gabaix (2019)).

equal to 1 if subject i perceives "Variable A" as useful to predict returns in round k; $a_{t,k}$ and $r_{t,k}$ are the last realizations of "Variable A" and "Index Return" in round k; and the coefficients $\{\alpha_1^m, \alpha_2^m, \beta_1^m, \beta_2^m, \delta_1^m, \delta_2^m\}$ are determined by the parameters $\{\bar{\mu}, \lambda_u, \lambda_p, \pi_u, \pi_p\}$.[22]

To choose $\{\bar{\mu}, \lambda_u, \lambda_p, \pi_u, \pi_p\}$, we make the following assumptions. First, we assume subjects are unbiased in their average forecasts: $\bar{\mu} = \mu = 6.07\%$ the true unconditional returns expectation, which we explicitly provide to them in the experiment set-up.

Second, we set π_u, π_p equal to the true posterior probabilities we observe in the data, i.e., we assume that subjects do not overestimate nor underestimate their ability to correctly detect when "Variable A" is predictive. This assumption is motivated by the fact that subjects receive feedback each round on their ability to identify "Variable A" as predictive.

Third, we assume subjects have same extrapolative bias as previously observed in the literature when they apply model $\mathbb{E}_t^u(r_{t+1}) = \lambda_u r_t + (1-\lambda_u)\bar{\mu}$: we set $\lambda_u = 0.32$, as estimated by Landier, Ma, and Thesmar (2019); Afrouzi et al. (2023) in an experimental setting comparable to our i.i.d rounds.

Finally, fourth, we assume subjects update their beliefs rationally from prior $\bar{\mu} = \mu$ when they apply model $\mathbb{E}_t^p(r_{t+1}) = \lambda_p a_t + (1-\lambda_p)\bar{\mu}$. Subjects are not told $\mathbb{E}_t(r_{t+1}) = a_t$ in predictable rounds, corresponding to $\lambda_p = 1$, but, in the graphical displays they are provided each round, they observe 40-period time series of the loadings of $\{r_{t+1}\}$ on $\{a_t\}$. Our assumption is that they do not over- or underestimate on average the value of those loadings; while taking into account their risk of mistakes when identifying "Variable A" as predictive. This fourth assumption yields $\lambda_p = \frac{\pi_p^2 + (1-\pi_u)^2}{\pi_p + (1-\pi_u)}$;[22] such that the model is *fully specified* by setting parameters $\{\bar{\mu}, \lambda_u, \pi_u, \pi_p\}$.

Equations (9) and (10), and our assumptions for $\{\bar{\mu}, \lambda_u, \lambda_p, \pi_u, \pi_p\}$, correspond to a model where subjects have an imperfect ability to detect predictability and imperfect knowledge of the return processes, but 1) are *sophisticated* in being aware of these limitations; 2) are *rational* in estimating their probabilities of being right or wrong about "Variable A"; 3) are unbiased in their average forecasts; 4) are unbiased, on average, in assessing the loading of $\{r_{t+1}\}$ on $\{a_t\}$ in the simulated graphs; and 5) have the standard "extrapolative" bias in rounds without information.

[22]The model is described in details in Appendix D.1.

To test the model in our experimental data, we measure the posterior probabilities $\{\pi_{u,i}, \pi_{p,i}\}$, for each subject i; which determines, given $\bar{\mu} = 6.07\%, \lambda_u = 0.32$, the forecast coefficients $\{\alpha^m_{i,1}, \alpha^m_{i,2}, \beta^m_{i,1}, \beta^m_{i,2}, \delta^m_{i,1}, \delta^m_{i,2}\}$ of Equation (11). We confront their average values and confidence intervals to the corresponding regression coefficients, derived in our data, controlling for subject and round fixed effects. The results are provided in Table 7. We find that the model's predicted intercepts and loadings on the last realized values of "Index Return" and "Variable A", r_t and a_t, across rounds, cannot be rejected, at conventional levels.[23]

The dual expectation model of Equations (9) and (10) is consistent not only with the forecast variations we observe, one round to the next, as captured by the loadings on a_t and r_t, but also with the average forecast levels across round types: the model-implied intercepts, $\alpha^m_1 + \alpha^m_2$ in rounds where "Variable A" is perceived as predictive and α^m_1 otherwise (Equation (11)), cannot be rejected in our data.[24] Because those derive from the anchoring on μ, the true unconditional expectation, this result shows that, on average, our subjects do not have an optimistic or pessimistic bias in their forecasts, whether "Variable A" is perceived predictive or not, contrasting with previous investors' evidence Dominitz and Manski (2007); Hurd and Rohwedder (2012); Giglio et al. (2021a). This result does not exclude that another form of optimism bias may be at play in subjects' over-interpreting "Variable A" as predictive in 56% of rounds instead of the true 50%.

Finally, we note that the model-induced variations in beliefs correspond to the "informed forecasts" $\{\widetilde{F}_{i,k}\}$, in regression (6); other variations in $\{F_{i,k}\}$ are noise according to our model.

4.2 Interpretation – risk assessments

Before we turn to the analysis of subjects' investments, and the results of Section 3.3, we study and interpret their risk assessments, described in Section 3.4.

Under our normally distributed simulation processes, next-period return risk are fully captured by variance estimates. Similar to the forecast model of Equations (9) and (10), we assume that sub-

[23]Our test of the model in Table 7 would not reject the alternative $\mathbb{E}^u_t(r_{t+1}) = \lambda_u r_t + (1-\lambda_u)\mu + \widetilde{\mathbb{E}}^u_t(r_{t+1})$ and $\mathbb{E}^p_t(r_{t+1}) = \lambda_p a_t + (1-\lambda_p)\mu + \widetilde{\mathbb{E}}^p_t(r_{t+1})$, as long as $\widetilde{\mathbb{E}}^u_t(r_{t+1})$ and $\widetilde{\mathbb{E}}^p_t(r_{t+1})$ use information orthogonal to a_t and r_t and have mean 0. Such models are discussed in Section 5.3.

[24]The low 5.1% average forecast in rounds where "Variable A" is perceived as useless (Table 1) is due to subjects' extrapolating from r_t, which has a low average realization of 1.5% in i.i.d. rounds (Online Appendix Table C.2).

jects have variance model $Var_t^u(r_{t+1})$ when "Variable A" is useless, and variance model $Var_t^p(r_{t+1})$ when "Variable A" is predictive; such that, when taking into account their risk of mistakes when assessing if "Variable A" is informative, their reported variances follows:

$$\begin{cases} Var_t\left(r_{t+1}\left|_{A \text{ perceived useless}}\right.\right) & = \pi_u Var_t^u(r_{t+1}) + (1-\pi_u) Var_t^p(r_{t+1}) \\ Var_t\left(r_{t+1}\left|_{A \text{ perceived predictive}}\right.\right) & = \pi_p Var_t^p(r_{t+1}) + (1-\pi_p) Var_t^u(r_{t+1}) \end{cases}, \tag{12}$$

where π_u and π_p correspond to the probabilities that a given subject assigns to the fact that "Variable A" is truly useless or predictive, conditional on the fact that she perceives it as such.

In line with the assumptions for forecast model parameters $\{\bar{\mu}, \lambda_u, \lambda_p, \pi_u, \pi_p\}$ in Section 4.1, we assume, first, that subjects are unbiased in their average variance estimates: $\mathbb{E}\left(Var_t^u(r_{t+1})\right) = \sigma^2$ and $\mathbb{E}\left(Var^p(r_{t+1})\right) = \sigma_p^2$, the true next period variances from processes (1) and (2). Even though subjects are not explicitly provided with variance statistics, we do not view this assumption as unreasonable: variance sample estimates converge quickly with sample size such that the variations in the 40-period long "Index returns" realized volatilities across the twenty rounds of the experiment are small (0.18 p.p. standard deviation). Similarly, the correlation between "Variable A" and "Index returns" is stable within predictable rounds (Online Appendix Table C.1). Subjects thus "eyeball" the same information each round on both the unconditional and the conditional risk they face, making the assumption that they have unbiased average estimates credible. Second, we assume that subjects are unbiased in assessing the shape of the distribution, such that they perceive risk as normally distributed. Third, as before, we let π_u, π_p be equal to the true posterior probabilities in the data, i.e., subjects do not overestimate nor underestimate on average their ability to correctly detect when "Variable A" is predictive.

We note that the assumptions we make for parameters $\{\mathbb{E}\left(Var_t^u(r_{t+1})\right), \mathbb{E}\left(Var^p(r_{t+1})\right), \pi_u, \pi_p\}$ in the model of Equation (12) are meant to capture the average reported confidence intervals in our data but not their variations within round types. The average risk perceptions are the correct statistics to interpret average investments, as we show below.

From the posterior probabilities $\{\pi_{u,i}, \pi_{p,i}\}$ for each subject i in our experimental data, we derive 80% confidence intervals from the model-implied average variances, and confront them to subjects' average reported CI in each round type. The model cannot be rejected, with p-value= 0.38 for rounds where "Variable A" is perceived as useless and p-value= 0.84 in rounds where it is perceived as predictive.[25] The average reported CI is 20.7 p.p. across all rounds, almost exactly equal to the true 21.0 p.p. in our simulated processes (1) and (2). In addition, the evidence rejects risk assessment models that do not fall strictly between the unconditional and conditional variances of processes (1) and (2): subjects' reported CI in rounds perceived as unpredictable by "Variable A", 21.1 p.p., is significantly below the true 23.1 p.p. in process (1) (p-value = 0.02); the reported CI in rounds perceived as predictable by "Variable A", 20.4 p.p., is significantly above the true 18.9 p.p. in process (2) (p-value = 0.05).

We turn next to the upper and lower bound risk assessments. We derive for each subject i and round-type the probabilities that next period returns exceed +15% or fall below -3% implied by the variance model of Equation (12) with unbiased average estimates and the assumption of normal distributions; and confront them to those they report in the experiment. The model is rejected at the 5% level.[26] Subjects perceive fatter tails than the normal distribution, especially on the downside: in rounds perceived as unpredictable by "Variable A", the average stated lower-bound (upper-bound) probability is 10.9 p.p. (1.7 p.p.) above that implied by the model; in rounds where "Variable A" is perceived as useful, they are 9.0 p.p. (4.9 p.p.) above. Such misperceptions can arise under the cognitive uncertainty model of Enke and Graeber (2023), as shown in Enke et al. (2024). As we discuss below, subjects also display cognitive uncertainty behaviors in their risk decisions; consistent with the interpretation above.

4.3 Interpretation – investment model

To interpret subjects' investments, we take the classical Merton-Samuelson portfolio choice model with normally distributed returns (Merton, 1969) as the baseline, and discuss which, if any, exten-

[25]Testing is done by confronting, individually, for each subject and round type, their average confidence intervals to the model implied ones.

[26]The model is tested using for each subject i and round-type, their average reported upper bound and lower bound probabilities and comparing them to those implied by the variance model, given their average forecasts. We obtain p-values < 0.01 and < 0.01 for the lower-bounds, and p-values $= 0.55$ and 0.04 for the upper-bounds, in rounds where "Variable A" is perceived as useless or as predictive respectively.

sions are necessary to explain the evidence in our experimental data. An agent with power utility and risk aversion γ_i has optimal risk investment

$$\theta_i = \frac{1}{\gamma_i} \frac{\mathbb{E}_i(r)}{\sigma_i^2(r)}, \tag{13}$$

given her expectation $\mathbb{E}_i(r)$ and estimated variance $\sigma_i^2(r)$ of normally distributed excess return r.

Average investments.

From Equation (13), and substituting forecasts for expectations, we obtain $\gamma_i \sigma_{i,k}^2 = \frac{F_{i,k}}{\theta_{i,k}}$, for any round k and subject i, using the notations of Section 3; such that the relative average forecast-to-investment ratios across round types are determined, for each subject, by her relative perceived variances:

$$\frac{\overline{\mathbb{E}}\big(\frac{F}{\theta} \mid_{A \text{ perceived useless}}\big)}{\overline{\mathbb{E}}\big(\frac{F}{\theta} \mid_{A \text{ perceived predictive}}\big)} = \frac{\overline{\mathbb{E}}\left(Var\left(r_{t+1} \mid_{A \text{ perceived useless}}\right)\right)}{\overline{\mathbb{E}}\left(Var\left(r_{t+1} \mid_{A \text{ perceived predictive}}\right)\right)}, \tag{14}$$

where $\overline{\mathbb{E}}$ denotes sample averages.

Motivated by our analysis of Section 4.2, we derive for each subject i her variance expectations under the model of Equation (12), using her probability of mistakes when identifying "Variable A" as useful and assuming unbiased estimates $\mathbb{E}\left(Var_t^u(r_{t+1})\right) = \sigma^2$ and $\mathbb{E}\left(Var^p(r_{t+1})\right) = \sigma_p^2$; and her average forecast-to-investment ratios across round types. We find that Equation (14) cannot be rejected, at conventional levels (p-value= 0.59).[27]

Subjects' average investments follow the Merton-Samuelson model with normally distributed unbiased risk assessments; consistent with the 80% confidence intervals they report. We note that this finding excludes possible model extensions, e.g., assuming greater ambiguity in rounds without "Variable A" information, where the difference in the perceived risk across round types is significantly greater than for the true variances σ^2 and σ_p^2.[28]

We can infer from Equation (13) each subject i's implicit risk aversion γ_i, from her average investments (relative to forecasts) and average perceived variance (according to Equation (12),

[27] We removed two subjects with average forecasts-to-investments ≈ 0 when "Variable A" is perceived predictive.

[28] Such models are discussed in Appendix D.2.

with unbiased risk assessments). We find a median $\gamma = 24$.[29] This measure is high with respect to estimates in the experimental literature (γ estimates in the lab are mostly below 10), but consistent with previous evidence in asset pricing (e.g. Hansen, Heaton, and Li, 2008; Malloy, Moskowitz, and Vissing-Jørgensen, 2009).[30]

Elasticity of investments.

From the Merton-Samuelson model (Equation (13)), we derive:

$$d\theta_i = \frac{1}{\gamma_i} d\left(\frac{\mathbb{E}_i(r)}{\sigma_i^2(r)}\right), \tag{15}$$

i.e., variations in investments are explained by variations in expectation $\mathbb{E}_i(r)$ and variance $\sigma_i^2(r)$.

To take Equation (15) to our experimental data, we observe, first, that variations within round types in reported confidence intervals have no bearing on investments (Table 5). Accordingly, we assume subjects' variance beliefs $\sigma_i^2(r)$ are constant within round types, for each subject i. Second, we assume variations in expectation $\mathbb{E}_i(r)$ are captured by variations in $\widetilde{F}_{i,k}$, consistent with the belief model of Section 4.1. Given these assumptions, the Merton-Samuelson model implies:

$$\begin{cases} \frac{d\theta}{d\widetilde{F}} \,|_{A \text{ perceived useless}} & = \overline{\mathbb{E}}\left(\frac{\theta}{F} \,|_{A \text{ perceived useless}}\right) \\ \frac{d\theta}{d\widetilde{F}} \,|_{A \text{ perceived predictive}} & = \overline{\mathbb{E}}\left(\frac{\theta}{F} \,|_{A \text{ perceived predictive}}\right) \end{cases}, \tag{16}$$

where $\overline{\mathbb{E}}$ denotes sample averages.

Under Equation (16), the elasticity of investments to "informed forecasts" is equal to the average investment-to-forecast. We find this equality is rejected in the data. In rounds where "Variable A" is perceived as useless, the average investment-to-forecast has mean value 6.01; the elasticity of investments to "informed forecasts" has mean value 1.56 (column(3), Table 4), almost four times lower. In rounds where "Variable A" is perceived as predictive, the average investment-to-forecast has mean value 6.35; the elasticity of investments to "informed forecasts" has mean value 3.19

[29] Specifically, the median γ is 21 and 26, in rounds where "Variable A" is perceived as useless and as predictive respectively. A few outlier subjects have "extreme" investment decisions – they always invest 0 ECU or 100 ECU, hence our reporting the median rather than the average.

[30] We note the 42.6% average equity share in Table 1 is lower but comparable to the 67.5% in Giglio et al. (2021a).

(column(1), Table 4), about twice lower. Subjects' investments are inelastic – they under-react to their own forecasts – compared to the Merton-Samuelson model.

To reconcile this result with our previous finding that average investments across round types align with the classical model under unbiased risk perceptions, we posit that variations in next-period "effective" beliefs $\mathbb{E}_i(r)$ in Equation (15) are not those subjects report in their "informed forecasts" (and the model of Section 4.1). In line with the cognitive uncertainty model of Enke and Graeber (2023), we assume instead that subjects anchor on their average forecasts, such that variations in "effective" beliefs, that determine their investment decisions, are given by:

$$\mathbb{E}_{i,k}(r) = \xi_i \overline{\mathbb{E}}\left(F_i\right) + \left(1 - \xi_i\right) \widetilde{F}_{i,k}, \tag{17}$$

where $\overline{\mathbb{E}}$ denotes sample averages, and $\xi_i > 0$ represents the cognitive uncertainty distortion.

Since the belief model of Equation (17) does not distort average forecasts, it does not invalidate the matching, as shown above, of the average investment-to-forecast ratios to the Merton-Samuelson portfolio choice model; while now allowing the classical model to also accommodates investment variations, if we let the average "cognitive uncertainty" distortion be higher in rounds where forecasts are extrapolative than in rounds where they are informed by "Variable A". Specifically, we derive $\xi\mid_{A\ perceived\ useless} = 0.74$ and $\xi\mid_{A\ perceived\ predictive} = 0.50$ on average.

Taking the "informed forecasts" $\{\widetilde{F}_{i,k}\}$ (and the model of Section 4.1) as Bayesian updates given signals $\{r_t, a_t\}$, Equation (17) follows Enke and Graeber (2023). However, our finding that cognitive uncertainty impacts investments decisions but not forecasts, even when incentivized, and the resulting internal inconsistency between reported expectations and actions, is new to their analysis;[31] as is the evidence that extrapolative beliefs generate higher cognitive uncertainty.

[31] In the experiment of Enke and Graeber (2023), both belief updates and decisions exhibit cognitive uncertainty. Charles, Frydman, and Kilic (2024) find an internal inconsistency between subjects' certainty equivalents of risky lotteries and the probabilities they assign to each of the lottery payoffs, however they do not elicit their average expectations.

Preference for skewness.

Our analysis, so far, does not account for the evidence that reported probabilities of "extreme" returns (above +15% / below -3%) 1) are *not* consistent with normally distributed risk assessments (Section 4.2); and 2) induce variations in investments (Table 6).

We interpret these results as indicating a preference for skewness, independent from decisions related to the first and second moments in the risk distribution, such that they do not invalidate our model interpretation of subjects' average investments and elasticity of investments to forecasts. Our reasoning is based on the following two observations. First, subjects' reported upper and lower bound probabilities do not influence how changes in forecasts (first moment) affect investments (Table 6).[32] Second, if their reported upper and lower bound probabilities were indicative of subjects' perceived variances (second moment), higher estimates on either sides would indicate higher risk; they would *both* lower investments contrary to the evidence in Table 6.

The results of Table 6 show, instead, that subjects find positively skewed returns, with higher probability of "extreme" high payoff, attractive, while they find, at the same time, negatively skewed returns unappealing. Such preference for positively skewed "lottery stocks" is modeled in, e.g., Barberis and Huang (2008), based on probability distortions that overestimate tail events (Kahneman and Tversky, 1979), also consistent with our subjects' reported beliefs (Section 4.2); while an aversion for negatively skewed wealth profiles is at the core of the "rare event" literature in asset pricing (e.g. Barro, 2006; Gabaix, 2008).[33]

5 Additional results

5.1 Variations across subjects

The results in Section 3 are equally valid across and within subjects, suggesting *all* subjects follow similar behaviors; a key finding. We explore what heterogeneity remains in our data.

[32]This result also excludes that the reported upper and lower bound probabilities may proxy for how cognitive certain or uncertain subjects are about their own forecasts, i.e. for ξ_i in Equation (17).

[33]The classical Merton-Samuelson model with power utility and normal distributions of risk (Equation (13)) can be extended to allow for the pricing of higher moments in non-normal distributions (Martin, 2013).

Individual fixed effects.

We find limited heterogeneity in subjects' average forecasts, only 13% of which are explained by individual fixed effects. This result contrasts with survey evidence: e.g., Giglio et al. (2021a) find up to 60% of variations in beliefs are explained by individual fixed effects.

One important difference is that real investors vary their forecasts over time given new data points on the *same* time series of market returns, whereas each of the rounds our subjects play corresponds to a *completely new* time series simulation of "Index Return". The homogeneous average forecasts we observe in our experiment compared to the belief persistence in survey data suggest the later may be due to anchoring biases, rather than optimistic versus pessimistic personalities. Our data confirms this interpretation: only 8 (1) out of 169 subjects have pessimistic (optimistic) forecasts – below (above) the reported average for a given round – 80% of the time.

Subjects' average risk investments display greater heterogeneity: 43% of all ECU risk positions are explained by individual fixed effects. 55 (34) out of 169 subjects have prudent (high) risk investments – above the average for a given round – 80% of the time. Given their homogeneous forecasts, these results suggest important variations in risk appetites across subjects.

Prolific versus Master of finance subjects.

As discussed in Section 2, Prolific subjects are recruited online from a representative pool of the US population, and likely differ in their understanding of financial markets from TSE Master of finance students. We analyze if these differences are reflected in forecasts and risk decisions across the two groups, controlling for individual and round fixed effects, as reported in Table 8.

We find Prolific subjects have same behaviors as the Master of Finance subjects: they all use the information in "Variable A" only when they view it as useful, and extrapolate from past returns otherwise; they all invest according to their own forecasts, with greater loadings in rounds perceived as predictable. They use the signals $\{a_t, r_t\}$ with same magnitude to form their forecasts across rounds types. Risk investments' greater loading on forecasts in rounds perceived as predictable by "Variable A" is not statistically different across the two subject groups. The only significant difference we observe is that Prolific subjects use their own forecasts less when making their risk decisions, in both types of rounds. A one percentage point increase in forecasts leads to 2.03 higher ECU investments on average for TSE Master's students, and to 1.17 higher ECU investments on average for Prolific subjects. Interpreted through the lens of the investment model of Section 4.3, this result indicates Prolific subjects are less confident in the forecasts they form from the signals $\{a_t, r_t\}$, reflected in a higher average cognitive uncertainty parameter ξ (Equation (17)).[34]

Individual characteristics.

We group subjects according to observable individual characteristics. We analyze if gender, risk appetite (as measured by the average risk taking over the experiment), and "understanding" of information (as measured by the number of correct "Variable A" answers over the experiment) affect their behaviors. For TSE students, we consider their average grades in the Master's program, and, for those who played the experiment in the lab, if they were fast or slow in completing the tasks.[35] For Prolific subjects, we analyze their age, annual income, education, and financial literacy. The forecasts and investments of subject groups sorted on their individual characteristics are provided in Online Appendix Tables A.4 to A.12.[36]

Heterogeneous investment decisions are observed in several cases: women and wealthier subjects use their own forecasts significantly less when making their risk decisions, in all rounds, whereas those with greater financial literacy (Prolific subjects) or higher grades (TSE subjects) use their own forecasts significantly more, in all rounds. Subjects who are slower when playing the experiment in the lab use their forecasts significantly less in rounds where they perceive "Variable A" as useful, when choosing their risk investments.

Taken together, and interpreted through the lens of the investment model of Section 4.3, these results suggest differences across these groups in self-confidence about the forecasts they form from the signals $\{a_t, r_t\}$. To quantify these differences, we measure how the average cognitive uncertainty ξ (Equation (17)) varies with observable individual characteristics, across round types. We report

[34] On average, across all rounds, $\xi = 0.64$ for Prolific subjects versus $\xi = 0.56$ for TSE students.

[35] We do not analyze fast or slow answers in the online implementations as we cannot control whether subjects may sometimes be distracted, pause and stop playing the experiment for any length of time.

[36] Some of the individual characteristics we analyze co-move, e.g., higher education is 43% correlated with higher income. The correlation matrix is provided in Online Appendix Table A.13.

our results, using the methodology of Section 4.3 based on the elasticity to "informed forecasts", in Table 9. We find that subjects with greater financial literacy (Prolific subjects) have lower cognitive uncertainty ξ's in rounds where "Variable A" is perceived as useful, while those with higher grades (TSE subjects) and women have lower cognitive uncertainty in their extrapolative beliefs. The more educated and the wealthier (Prolific subjects), as well as those who play the game faster (TSE subjects), have lower cognitive uncertainty in both round types.

Subjects display considerably less heterogeneity in their forecasts. Women extrapolate from r_t more in rounds where they perceive "Variable A" as useful; those who invest more (greater risk appetite) use "Variable A" more in rounds where they view it as useless. All other differences are insignificant at the 5% threshold.

Three main results emerge: 1) there is some heterogeneity in investments' loadings on forecasts; 2) there is limited heterogeneity in forecasts' loadings on $\{a_t, r_t\}$; and 3) even in the few cases where magnitudes vary significantly, they do not offset the forecast and investment patterns of Section 3: notwithstanding their individual characteristics, all subjects use the information in "Variable A" only when they view it as useful, and extrapolate from past returns otherwise; they all invest according to their own forecasts, with greater loadings in rounds perceived as predictable.[37]

5.2 Additional treatments

Increasing information uncertainty: long horizon forecasts and investments.

A fully informed rational agent would forecast the average return over five periods starting at $t+1$ as $\mathbb{E}_t\left(\overline{r}_{t+1,t+5}\,|_{\text{i.i.d.}}\right) = \mu$ and $\mathbb{E}_t\left(\overline{r}_{t+1,t+5}\,|_{\text{predictable}}\right) = \kappa a_t + (1-\kappa)\mu$, where a_t is the realization of "Variable A" at time t, and $\kappa < 1$ depends on the persistence of "Variable A" (see Section 2). The rational forecast rule for 5-period average returns thus requires not only to identify a_t as the best forecast for r_{t+1} when "Variable A" is predictive but also the dynamics of the "Variable A" process; making it considerably more difficult to evaluate from the time series displays we provide.

[37] The additional pass-through from forecasts to investments in rounds perceived as predictable by "Variable A" is not significant within the Prolific subjects sub-groups. This is also reflected in the ξ's for rounds perceived as predictable by "Variable A" not being systematically lower for all subgroups, e.g. $\xi = 0.72$ on average for rounds where "Variable A" is perceived as predictive and $\xi = 0.64$ on average for rounds where "Variable A" is perceived as useless for the High Income / Low Income subgroups. This is likely due to the small sample size (28 subjects per sub-group), since we observe that Prolific subjects otherwise have the same behaviors as TSE students (Table 8).

To analyze how this greater "information uncertainty" affects subjects' decisions, we follow an analysis similar to Section 3 and report our results in Online Appendix Table A.14. Subjects no longer use the information in "Variable A", even when they view it as predictive for next-period returns; they extrapolate from past returns in *all* rounds, with and without subjects' fixed effects, with lower, but still significant, loadings on r_t than for next-period returns.[38] The sensitivity of investments to forecasts remains positive and significant, but, 1) it is lower than for next-period investments – a change in beliefs of one p.p. results in an average 0.74 ECU change in investment; and 2) the pass-through from forecasts to investments is not significantly higher in rounds where "Variable A" is perceived as predictive. This is also reflected in the average long-term investments (Table 1), which are not significantly different across round types (p-value = 0.11).

Reducing information uncertainty.

In the remaining two additional treatments, information was made easier to interpret, either because subjects were told when "Variable A" was useful to predict returns, or because they were told about processes (1) and (2). To analyze how lower "information uncertainty" affects subjects' decisions, we follow an analysis similar to Section 3 and report our results in Online Appendix Tables A.15 and A.16.

Revealing when "Variable A" is predictable does not change subjects' forecasts, relative to the baseline; but it increases significantly the pass-through from forecasts to investments in rounds revealed as predictable by "Variable A", to 2.94 ECU per p.p. change in forecasts (Online Appendix Table A.15).[39]

Revealing processes (1) and (2) changes the forecast and investment results considerably (Online Appendix Table A.16): the loadings on a_t in rounds perceived as predictable by "Variable A" increase to 0.60; the loading on r_t in rounds perceived as non-informative collapses to -0.01; the influence of forecast variations on investments is greater in *all* rounds, with a 3.10 ECU average pass-through, compared to 1.40 ECU for the same subjects (TSE Master's students, third wave)

[38] We also find subjects have higher average forecasts for 5-period average returns than for the next period, consistent with the evidence in Cassella et al. (2021) that investors have optimistic biases at the long-horizon.

[39] The 2.11 ECU pass-through in rounds revealed as unpredictable by "Variable A" is not statistically different from the baseline treatment for the same subject pool (TSE Master's students, second wave and third wave).

in the baseline treatment.

Taken together, the results we obtain in all three additional treatment are strongly supportive of the model interpretation of Section 4: the more easily interpretable the information in "Variable A", the more it enters forecasts;[40] the more uncertain subjects are about the information they use to form their beliefs, the less their own forecast variations affect their risk decisions.

5.3 Robustness

We extend the empirical analysis of Section 3 in several directions. First, we verify whether the forecast and investment patterns may emerge gradually and differ between early and late rounds. Results are reported in Online Appendix Table A.17. We find the overall forecast pattern is qualitatively the same throughout, though the loading on the "Variable A" signal, when it is viewed as predictive, is significantly higher in later rounds of the experiment. We find no evidence that investments' loadings on beliefs differ between early and late rounds.

Second, we verify if subjects use other realizations of "Variable A" and "Index Return" in round k, i.e., $\{a_{t-1,k}, a_{t-2,k}, \dots\}$ and $\{r_{t-1,k}, r_{t-2,k}, \dots\}$, as well as forecasts decisions and realizations of "Variable A" and "Index Return" in rounds prior to k. The past realizations within the same round of "Variable A", when it is perceived as predictable, help explain forecasts: the regression R^2 (adjusted R^2) using $\{a_t, a_{t-1}, a_{t-2}, \dots\}$ increases to 22% (14%) compared to 16% (7%) when only a_t is used, controlling for individual and round fixed effects (column (1) versus column (3) in Online Appendix Table A.18).[41] We find no evidence that subjects use information from previous rounds, to form their forecasts and choose their investments (Online Appendix Table A.19). There is limited evidence of anchoring, though a high forecast in the previous round lowers investments in the next by 0.24 ECU per p.p. (Online Appendix Table A.20).

Finally, we analyze and reject that the signals $\{a_t, r_t\}$ may directly contribute to investment variations, i.e., affect investment decisions other than via their impact on forecasts: though in-

[40] Accordingly, we would expect higher loadings for the forecast results in Online Appendix Table A.15, in rounds revealed as predictable by "Variable A". However, these results are estimated with large standard errors, due to the small sample size.

[41] Forecasts in rounds not perceived as predictable by "Variable A" also load significantly on the past returns realizations $\{r_{t,k}, r_{t-1,k}, r_{t-2,k}, \dots\}$, however the regression R^2 is unchanged (column (4) versus column (6) in Online Appendix Table A.18).

vestments load on a_t (significant in some specifications), it explains less than 0.5% of investment "noise", i.e., variations unexplained by forecasts (Online Appendix Table A.21).

6 Discussion

Our experimental framework allows us to observe separately 1) the information subjects have, 2) how they perceive the signals they receive, 3) how it affects their forecasts, and 4) how it affects their investment decisions. From our observations, we document the following set of "rules": subjects have extrapolative forecasts "by default" unless they receive a signal they believe to be predictive, in which case they use it exclusively (Section 3.2); the pass-through from their forecasts to their investments is low, but less so when forecasts are informed by an external signal ("Variable A") perceived as predictive (Section 3.3). We discuss below, first, how crucial the role of the experimental framework is, i.e., whether these sets of results could be deduced from real investors' data; and second, the implications of the mechanisms we document for equilibrium outcomes.

6.1 Understanding data evidence

Most empirical databases on real investors provide only their portfolio allocations (see e.g., Gabaix et al. (2024); Andries, Bonelli, and Sraer (2024) for recent examples), not the information they use, and which beliefs they have. Even in the rare cases where investors' forecasts are observed, as in Giglio et al. (2021a), what market information determines said forecasts is unknown. With similar data on our subjects' market decisions, would we be able to understand their behaviors? I.e., beyond allowing us to observe decisions within subjects when exposed to different information in a controlled environment, how crucial was our experimental framework to understand the mechanisms we document? To answer this question, we conduct the following thought experiment: with subsets of our experimental data, which inferences would we make?

Suppose that we just have access to subjects' investment decisions.[42] Let's assume, first, we

[42]The assumption that we can observe subjects' investments over the twenty independent rounds of the investment game is already quite strong and not easily comparable to real investors' data.

only know that subjects can observe past returns data. To study their extrapolative bias, similar to e.g. Benartzi (2001); Berk and Green (2004) who document how asset demands respond to their past returns, we would analyze:

$$\theta_{i,k} = \alpha + \delta r_{t,k} + \epsilon_{i,k}, \tag{18}$$

where $\theta_{i,k}$ is subject i's investment into the risky fund (out of her 100 ECU endowment) in round k, and $r_{t,k}$ is the last realization of "Index Return" in round k. The results of regression (18) are in Online Appendix Table A.22, columns (1)-(3). We find $\delta = 0.12$ ECU per p.p. change in realized returns r_t, not significant at the 10% threshold when controlling for individual and round fixed effects; the R^2 (adjusted R^2) of regression (18) is 46% (43%), almost unchanged from the 43% we obtain with individual fixed-effects only (see Section 5.1). Moreover, a "back of the envelope" analysis relating the estimated $\delta = 0.12$ ECU to average investments would suggest that the pass-through from r_t to beliefs is an order of magnitude smaller than reported in Table 2 (for rounds where subjects do extrapolate).[43] We would conclude that our subjects' extrapolative biases are weak and much lower than previous estimates in the literature.

Let's assume, next, we now know that subjects observe a signal ("Variable A") that can be predictive. Similar to Dahlquist and Ibert (2024) who analyze if asset managers use price-earning ratios to make their decisions, we would run:

$$\theta_{i,k} = \alpha + \beta a_{t,k} + \epsilon_{i,k}, \tag{19}$$

where $\theta_{i,k}$ is as above, and $a_{t,k}$ is the last realization of "Variable A" in round k. The results of regression (19) are in Online Appendix Table A.22, columns (4)-(6). We find $\beta = 0.86$ ECU per p.p. change in the signal a_t, significant at the 1% threshold, controlling for individual and round fixed effects. However, adding "Variable A" information only improves the R^2 of regression (18) to

[43] To obtain this result 1) we compare $\delta = 0.12$ to the 43 ECU average investment (Table 1), 2) we assume subjects' average forecast is the true 6.07%, and 3) we assume their risk allocations vary one for one with beliefs as in the classical model, 2) and 3) being the default assumptions in the absence of forecast data. This gives an extrapolative pass-through from r_t to beliefs of $\frac{0.12}{43} \times 6.07 = 0.02$, as compared to the 0.18 extrapolative bias of Table 2.

47% compared to the 43% obtained with individual fixed-effects only.[44] Here too, we would infer that the pass-through from a_t to beliefs is significantly lower than the one reported in Table 2 (in rounds where subjects perceive "Variable A" as predictive).[45] We would conclude that predictive information has only a small impact on subjects' decisions.

Without observing their beliefs, the analysis of regressions (18) and (19) would lead us to conclude the information subjects have access to has a limited influence on their behaviors; in contradiction with the evidence in our data (see, e.g., the 65% R^2's in Table 6).

Finally, suppose that we do observe subjects' forecasts, but not what information they find useful, i.e., we do not know which rounds subjects perceive as predictive by "Variable A". Similar to Giglio et al. (2021a), we would verify how investments vary with forecasts, and how forecasts vary with available information (a_t and r_t in our framework), corresponding to columns (1)-(2) in Table 2 and to columns (1)-(3) in Table 3.

Observing forecasts helps explain investments variations, with $R^2 = 58\%$ (column (3), Table 3); but the interpretation of the mechanisms underpinning subjects' forecasts and decisions remains incorrect. We would infer subjects *always* extrapolate but with low extrapolative bias, a 0.10 loading on r_t (column (1), Table 2), one third the 0.32 estimate in Landier, Ma, and Thesmar (2019); Afrouzi et al. (2023); and *always* use "Variable A" information, but less than they should rationally do so. We would overestimate how much subjects use their extrapolative forecasts (1.67 ECU pass-through instead of 1.38 ECU, columns (3) versus (6) in Table 3), and underestimate how much "Variable A" information affects their risk decisions (1.67 ECU pass-trough instead of the 3.19 ECU pass-through using "informed forecasts" in Table 4). We conclude: knowing how subjects interpret the information they receive is crucial to understand the mechanisms that determine their beliefs and belief-to-investment decisions.

[44] Regressing investments on $r_{t,k}$ and $a_{t,k}$ simultaneously does not improve the R^2 either (column (7) in Online Appendix Table A.22).

[45] The same "back of the envelope" exercise as footnote [43] would lead us to a pass-through of $\frac{0.86}{43} \times 6.07 = 0.12$, as compared to the 0.38 pass-through in Table 2.

6.2 Market implications

As described above, correctly interpreting investment decisions in our experiment – i.e., that subjects respond more or less elastically to their own forecasts depending on what information they find useful – requires observing the information subjects have access to, their forecasts, *and* how they perceive said information. Such observational data is not readily accessible when analyzing real investors, and directly testing the belief and investment models of Section 4 on their portfolio decisions may not be feasible. This raises the two questions, which we discuss below: 1) how much should we believe our experimental results reflect real investors' decision process?, and 2) do the mechanisms we document matter, i.e., what are their implications?

We argue our experimental results are likely representative of real investors' behaviors based on the following observations. First, all subject groups in our experiment follow the same information-forecast-investment process (Section 5). Subjects recruited online on Prolific behave similarly to TSE Master of Finance students (Table 8). Individual characteristics observable in real investors – gender, financial literacy, income, education, age, risk appetite – affect the magnitudes of the pass-throughs but not the mechanisms per se (Online Appendix Tables A.4 to A.11). Given that all our subject groups behave similarly, we are inclined to believe real investors would also do so.

Second, analyses of real investors that most closely resemble our experimental framework suggest our results are consistent with evidence in the data. Giglio et al. (2021a) study individual investors' forecasts and decisions but do not observe what information they use. They find a pass-through from forecasts to investments of 1.18, controlling for fixed-effects, compared to 1.38 in our experimental data in rounds where subjects do not use "Variable A" information, and 1.67 across all rounds (Table 3); they find forecasts load significantly on past returns but with a low extrapolation bias of 0.06, similar to the low 0.10 average bias we obtain (columns (2) in Table 2); and they find, as we do, that forecasts are strongly correlated with perceived probabilities of returns' lower bounds. Dahlquist and Ibert (2024), in contrast, study asset managers who observe price-dividend ratio information, similar to the "Variable A" signals in our experiment. Consistent with our results for rounds where "Variable A" is perceived as useful, Dahlquist and Ibert (2024) find forecasts load significantly on price-dividend ratios but not on past returns, i.e., they do not find any extrapolative bias; they find a pass-through from forecasts to investments of 2.05, comparable to 1.86 in our experimental data (for rounds perceived as predictable by "Variable A").[46] The results in Giglio et al. (2021a) and Dahlquist and Ibert (2024), who study investors' forecasts and risk decisions in information environments that closely resemble those of our experimental framework, are strongly suggestive the mechanisms we document operate in the data.

If investors follow the forecasts and investments mechanisms we document, such that they *all* respond to information similarly – the behaviors we observe are true across and within subjects, one of our key results – the real implications may be important. First, for investors' wealth accumulation: in our baseline treatment, observing a dividend-price ratio type signal they perceive as useful increases subjects' portfolio returns by 31%, via greater average investments (70% of the increase) and better market timing (30% of the increase). Educating subjects on how to use "Variable A", when useful, increases both investments and market timing further, with up to 41% higher portfolio returns.[47] These results make clear the role financial intermediaries can play, not as portfolio advisors but as information providers (see also Andries and Haddad, 2020; Bender et al., 2022), and their potentially large impact on investors' wealth. That advisors can generate greater market participation is consistent with the evidence in Linnainmaa et al. (2020). Schoar and Sun (2024) show, in an experiment, that educating investors can lead them to adopt market timing strategies. Our results also speak to the importance of the way in which information is provided. Ungeheuer and Weber (2021) show that subjects tend to perceive correlations when presented in graphical terms, but not when they are described in words. This may explain the difference between our results and those of Beutel and Weber (2022), who find that subjects' forecasts are not sensitive to information on the current price-earning ratio.

Second, we document 1) a limited pass-through from forecasts to risk positions overall, and 2) a lower pass-through when forecasts are extrapolative. Our results may explain why Chaudhry

[46] We note that asset managers in the field, who use price-dividend ratios as predictive signals, face the additional uncertainty, compared to our experimental framework, that they are not certain past predictors will stay informative in the future, due to, e.g. regime shifts.

[47] In the information treatment where we reveal to subjects the simulation of processes (1) and (2), investments are 28% higher in rounds perceived as predictable by "Variable A" (Table 1), while market timing generate 0.35 p.p. greater returns in predictable rounds, a 13% increase, as calculated under the method of footnote [20].

(2022) finds the equilibrium price impact of variations in analyst-reported expected returns is orders of magnitude smaller than implied by standard portfolio choice models. They also suggest we need to proceed with caution when making inferences for equilibrium outcomes from survey evidence of extrapolative beliefs (see e.g., Barberis et al., 2015, 2018; Maxted, 2024), similar to Enke, Graeber, and Oprea (2023) who show the interplay between behavioral biases and confidence is key to analyze their aggregate impact. Our findings speak further to the interactions between information and the dynamics of asset demand, with potentially large effects on asset prices (see Gabaix and Koijen, 2021). Charles, Frydman, and Kilic (2024) suggest adapting the model of Haddad, Huebner, and Loualiche (2021), who show passive investing lowers stocks' demand elasticities, to study the impact of investors' cognitive uncertainty, and more specifically, given our sets of results, the effect of different information environments on equilibrium price dynamics. We view such estimation as an interesting avenue for future research.

7 Conclusion

We design an experiment that allows us to analyze how investors form their beliefs about returns, and choose their risk allocations, depending on the information they receive.

While we find important dispersion in forecasts and risk allocations each round, *all* subjects behave according to the following two rules. First, when they are provided with a relatively simple predictive signal, subjects utilize the relevant information to form rational forecasts. When no such useful information is given, subjects default to extrapolative expectations, with magnitudes similar to those documented in previous studies.

Second, even though subjects use their forecasts to choose their investments, they under-react to the stated beliefs compared to the classical portfolio choice model. The pass-through from forecasts to decisions differs across information treatments: investments are twice as sensitive to forecasts informed by the predictive signal we provide than to subjects' own extrapolative expectations.

Tables and Figures

Table 1: Descriptive Statistics

Variable	Obs.	Mean	Median	Std. Dev.	Min	Max
$Pr(A\ perceived\ predictive \mid\ predictable)$	169	0.82	0.80	0.14	0.30	1
$Pr(A\ perceived\ useless \mid\ i.i.d)$	169	0.70	0.70	0.20	0.20	1
Predict	169	0.56	0.55	0.14	0.20	0.90
Forecast (in %)	3,380	5.9	6	8.0	-30	100
Forecast Distance (in %)	3,380	9.0	7.2	8.0	0.0	93.8
Invest (in ECU)	3,380	42.6	35	36.0	0	100
5-year Forecast (in %)	1,080	6.7	6	7.7	-15	100
5-year Invest (in ECU)	1,080	52.4	50	33.4	0	100
		Predict=1				
Forecast (in %)	1,888	6.5	7	7.6	-30	70
Confidence Interval (in %)	393	20.4	20	14.5	1	88
Upper prob. (in %)	660	22.3	15	23.0	0	100
Lower prob. (in %)	660	20.3	10	22.9	0	100
Forecast Distance (in %)	1,888	7.7	6.2	6.5	0.0	78.0
Invest (in ECU)	1,888	46.4	40	36.3	0	100
5-year Forecast (in %)	566	7.4	6	8.9	-15	100
5-year Invest (in ECU)	566	53.9	50	33.9	0	100
		Predict=0				
Forecast (in %)	1,492	5.1	5	8.4	-20	100
Confidence Interval (in %)	287	21.1	20	14.1	1	82
Upper prob. (in %)	480	19.4	10	21.1	0	100
Lower prob. (in %)	480	28.3	20	25.1	0	100
Forecast Distance (in %)	1,492	10.7	8.5	9.4	0.1	93.8
Invest (in ECU)	1,492	37.9	25	35.0	0	100
5-year Forecast (in %)	514	6.1	6	6.0	-14.5	80
5-year Invest (in ECU)	514	50.7	50	32.8	0	100
		"Variable A" is revealed predictive				
Forecast (in %)	460	6.3	7	6.4	-15	28
Invest (in ECU)	460	54.1	50	38.4	0	100
		"Variable A" is revealed not predictive				
Forecast (in %)	460	6.0	6	7.2	-15	30
Invest (in ECU)	460	49.1	50	38.5	0	100
		"Model revealed" treatment - Predict = 1				
Forecast (in %)	144	5.8	6.3	5.6	-15	20
Invest (in ECU)	144	55.0	50	37.6	0	100
		"Model revealed" treatment - Predict = 0				
Forecast (in %)	94	4.7	5	5.7	-16	17
Invest (in ECU)	94	43.0	30	39.9	0	100

NOTE: "Predict" is a dummy equal to one if the subject perceives "Variable A" is useful to predict returns. "Predict=1" and "Predict=0" results correspond to rounds perceived as predictable or not by "Variable A" in the baseline treatment across all waves.

Table 2: Forecast and Predictability

Dep Variable	Forecast								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
a(t)	0.24***		0.06	0.01	0.01				0.15**
	(0.05)		(0.07)	(0.06)	(0.06)				(0.06)
a(t) × Predict			0.28***	0.36***	0.37***				0.30***
			(0.10)	(0.08)	(0.08)				(0.07)
r(t)		0.10				0.18***	0.18***	0.18***	0.19***
		(.)				(0.03)	(0.03)	(0.03)	(0.03)
r(t) × Predict						-0.17***	-0.17***	-0.17***	-0.12***
						(0.04)	(0.04)	(0.04)	(0.04)
Predict			-0.47	-0.90*	-0.95*	1.71***	1.71***	1.73***	-0.42
			(0.53)	(0.47)	(0.47)	(0.34)	(0.40)	(0.40)	(0.50)
N	3,380	3,380	3,380	3,380	3,380	3,380	3,380	3,380	3,380
R^2	0.15	0.15	0.02	0.15	0.16	0.03	0.16	0.16	0.18
Individual FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	Yes
Round FE	Yes	Yes	No	No	Yes	No	No	Yes	Yes

NOTE: This table reports the results of OLS regressions. The dependent variable is the forecast of next period returns in percentage points. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. a(t) denotes the last realization of "Variable A". r(t) denotes the last realization of "Index Return". Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denote significance at 10%, 5% and 1% level, respectively.

Table 3: Investment and Forecasts

Dep Variable	Investment					
	(1)	(2)	(3)	(4)	(5)	(6)
Forecast	1.60***	1.67***	1.67***	1.27***	1.36***	1.38***
	(0.25)	(0.18)	(0.19)	(0.24)	(0.19)	(0.20)
Forecast × Predict				0.58***	0.52***	0.48***
				(0.08)	(0.14)	(0.13)
Predict				3.12**	4.07***	4.33***
				(1.34)	(0.91)	(0.91)
N	3,380	3,380	3,380	3,380	3,380	3,380
R^2	0.13	0.55	0.58	0.14	0.56	0.59
Individual FE	No	Yes	Yes	No	Yes	Yes
Round FE	No	No	Yes	No	No	Yes

NOTE: This table reports the results of OLS regressions. The dependent variable is the endowment invested in the risky asset, in ECU. "Forecast" is the forecast of next period returns in percentage points. "Predict" is a dummy equal to one if the subject declares that "Variable A" is useful to predict returns. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denote significance at 10%, 5% and 1% level, respectively.

Table 4: Investment and "informed forecasts"

Dep Variable	Investment			
	(1) 2SLS	(2) OLS	(3) 2SLS	(4) OLS
Forecast	3.19*** (0.67)	1.85*** (0.10)	1.56*** (0.29)	1.43*** (0.14)
N	1,888	1,888	1,492	1,492
Sample	Predict=1		Predict=0	
Instrument	a(t)		r(t)	
Individual FE	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes

NOTE: This table reports the results of the 2SLS regressions (5), and the OLS regression of Equation (4). The dependent variable is the endowment invested in the risky asset, in ECU. "Forecast" is the forecast of next period returns in percentage points. "Predict" is a dummy equal to one if the subject declares that "Variable A" is useful to predict returns. In the 2SLS columns, "Forecast" is instrumented by a_t, the last realization of "Variable A", when "Predict=1", and by r_t, the last realization of "Index Return", when "Predict=0". Clustered standard errors, at the round level, are in parenthesis (computing standard errors clustered at the individual-round level would yield a singular covariance matrix). *, ** and *** denotes significance at 10%, 5% and 1% level, respectively.

Table 5: Investment and Confidence Intervals

Dep Variable	Investment					
	(1)	(2)	(3)	(4)	(5)	(6)
Forecast	2.81***	2.42***	2.33***	2.46***	2.19***	2.13***
	(0.40)	(0.42)	(0.42)	(0.40)	(0.60)	(0.61)
High CI	1.54	1.14	-0.79	6.72	2.84	1.95
	(2.50)	(2.82)	(2.34)	(4.96)	(2.92)	(3.52)
Forecast × High CI	-0.26	-0.10	-0.08	-1.65***	-0.97	-0.85
	(0.42)	(0.48)	(0.44)	(0.28)	(0.57)	(0.57)
N	393	393	393	287	287	287
R^2	0.25	0.63	0.68	0.08	0.66	0.68
Sample	Predict=1			Predict=0		
Individual FE	No	Yes	Yes	No	Yes	Yes
Round FE	No	No	Yes	No	No	Yes

NOTE: This table reports the results of OLS regressions. The dependent variable is the fraction of the endowment invested in the risky asset, in percentage points. "Forecast" is the forecast of next period returns in percentage points. "High CI" is a dummy equal to one in rounds where the reported confidence interval is above or equal the subject's median value for the same round type – perceived as predictable or not by "Variable A". "Predict" is a dummy equal to one if the subject declares that "Variable A" is useful to predict returns. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denote significance at 10%, 5% and 1% level, respectively. These results were obtained during wave two of the experiment implementation (TSE lab, January 2020).

Table 6: Investment and Upper/Lower Bound probability

Dep Variable	Investment					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Forecast	1.37***	1.52***	1.54***	0.86**	1.28**	1.18**
	(0.36)	(0.34)	(0.31)	(0.34)	(0.48)	(0.44)
HighProbHigh	9.83**	9.62**	10.14***	5.93	10.84***	10.41**
	(4.45)	(3.37)	(3.17)	(3.59)	(3.74)	(3.76)
Forecast × HighProbHigh	-0.51	-0.61**	-0.67***	0.11	-0.49	-0.31
	(0.36)	(0.23)	(0.18)	(0.27)	(0.42)	(0.37)
N	660	660	660	480	480	480
R^2	0.09	0.58	0.64	0.10	0.61	0.64
Sample		Predict=1			Predict=0	
Panel B						
Forecast	0.43	0.61*	0.58	0.63*	0.86***	0.86***
	(0.39)	(0.32)	(0.40)	(0.35)	(0.26)	(0.25)
HighProbLow	-15.18***	-15.45***	-15.44***	-7.40*	-7.69**	-8.06**
	(4.41)	(4.09)	(4.35)	(3.83)	(3.01)	(3.02)
Forecast × HighProbLow	0.91*	0.74*	0.77*	0.51	0.03	0.11
	(0.45)	(0.35)	(0.40)	(0.37)	(0.27)	(0.26)
N	660	660	660	480	480	480
R^2	0.09	0.59	0.64	0.10	0.61	0.64
Sample		Predict=1			Predict=0	
Panel C						
Forecast	0.53	0.87	0.88	0.44	1.14**	0.93*
	(0.60)	(0.54)	(0.56)	(0.43)	(0.52)	(0.47)
HighProbHigh	8.04*	8.21**	8.74**	5.51	10.47***	10.02**
	(4.48)	(3.60)	(3.27)	(3.60)	(3.62)	(3.62)
HighProbLow	-14.27**	-13.86***	-13.73***	-7.25*	-6.76**	-7.65**
	(5.12)	(4.12)	(4.34)	(3.68)	(2.95)	(2.97)
Forecast × HighProbHigh	-0.23	-0.43	-0.48*	0.17	-0.46	-0.24
	(0.41)	(0.32)	(0.27)	(0.30)	(0.41)	(0.35)
Forecast × HighProbLow	0.81	0.56	0.57	0.52	-0.04	0.10
	(0.51)	(0.41)	(0.44)	(0.38)	(0.26)	(0.26)
N	660	660	660	480	480	480
R^2	0.10	0.60	0.65	0.10	0.62	0.65
Sample		Predict=1			Predict=0	
Individual FE	No	Yes	Yes	No	Yes	Yes
Round FE	No	No	Yes	No	No	Yes

NOTE: This table reports the results of OLS regressions. The dependent variable is the fraction of the endowment invested in the risky asset, in percentage points. "Forecast" is the forecast of next period returns in percentage points. "HighProbHigh" is a dummy equal to one in rounds where the reported upper bound probability (probability that next period return is above 15%) is above or equal the subject's median value for the same round type – perceived as predictable or not by "Variable A". "HighProbLow" is a dummy equal to one in rounds where the reported lower bound probability (probability that next period return is below -3%) is above or equal the subject's median value for the same round type – perceived as predictable or not by "Variable A". "Predict" is a dummy equal to one if the subject declares that "Variable A" is useful to predict returns. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denote significance at 10%, 5% and 1% level, respectively. These results were obtained during wave four of the experiment implementation (Prolific online, July 2023).

Table 7: Forecast Model

	Model	Data	Difference	p-value
	(1)	(2)	(3)	(4)
α_1	3.79	3.83	-0.04	0.911
	$[3.50, 4.08]$	$[3.18, 4.48]$	$[-0.75, 0.67]$	
α_2	-1.36	-0.42	-0.94	0.067
	$[-1.56, -1.15]$	$[-1.40, 0.56]$	$[-1.94, 0.07]$	
β_1	0.11	0.15	-0.03	0.599
		$[0.03, 0.26]$	$[-0.14, 0.08]$	
β_2	0.41	0.30	0.11	0.108
		$[0.17, 0.43]$	$[-0.02, 0.24]$	
δ_1	0.26	0.19	0.07	0.103
	$[0.21, 0.31]$	$[0.13, 0.26]$	$[-0.01, 0.15]$	
δ_2	-0.18	-0.12	-0.06	0.129
	$[-0.22, -0.15]$	$[-0.19, -0.05]$	$[-0.14, 0.02]$	

NOTE: In column (1), we report the average predicted values according to the forecast model of Section 4.1. The confidence intervals are obtained by plugging the upper-bound and lower-bound values of the extrapolation coefficient estimated by Landier, Ma, and Thesmar (2019); Afrouzi et al. (2023), $\lambda_u = 0.32$ with S.E. 0.03, into the forecast model of Section 4.1. In column (1) we make the conservative assumption that the probabilities of mistakes when identifying "Variable A" are estimated without errors, for each subject. In column (2), we report the estimates of the OLS regression (3): $F_{i,k} = \alpha_1 + \alpha_2 \text{Predict}_{i,k} + \beta_1 a_{t,k} + \beta_2 a_{t,k} \times \text{Predict}_{i,k} + \delta_1 r_{t,k} + \delta_2 r_{t,k} \times \text{Predict}_{i,k} + \epsilon_{i,k}$, estimated with round and subject fixed effects. The confidence intervals are obtained using standard errors two-way clustered by round and subject. The 95% confidence interval in column (3) are estimated using standard errors that are computed as $\sqrt{\sigma_m^2 + \sigma_d^2}$ where σ_m^2 and σ_d^2 are the standard errors as in column (1) and (2), respectively. In column (4), we report the p-values of the t-tests that the difference in column (3) is equal to zero.

Table 8: Forecast and Investment, TSE students versus Prolific subjects

Dep Variable	Forecast		Investment	
	(1)	(2)	(3)	(4)
a(t)	-0.03			
	(0.07)			
a(t) × Prolific	0.14			
	(0.13)			
a(t) × Predict	0.42***			
	(0.09)			
a(t) × Predict × Prolific	-0.16			
	(0.18)			
r(t)		0.21***		
		(0.04)		
r(t) × Prolific		-0.09		
		(0.08)		
r(t) × Predict		-0.20***		
		(0.05)		
r(t) × Predict × Prolific		0.07		
		(0.10)		
Forecast			2.03***	1.69***
			(0.17)	(0.22)
Forecast × Prolific			-0.86**	-0.68*
			(0.30)	(0.33)
Forecast × Predict				0.54**
				(0.20)
Forecast × Predict × Prolific				-0.26
				(0.30)
Predict	-1.12*	1.92***		3.47**
	(0.57)	(0.40)		(1.30)
Predict × Prolific	0.56	-0.56		2.76
	(1.16)	(0.78)		(2.74)
N	3,380	3,380	3,380	3,380
R^2	0.16	0.16	0.59	0.60
Individual FE	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. In columns (1)-(2), the dependent variable is the next-period forecast of returns, in percentage points. In columns (3)-(4), the dependent variable is the ECU next-period investment in the risky asset. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. "Prolific" is a dummy equal to one if the subject was recruited via the online Prolific platform. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denote significance at 10%, 5% and 1% level, respectively.

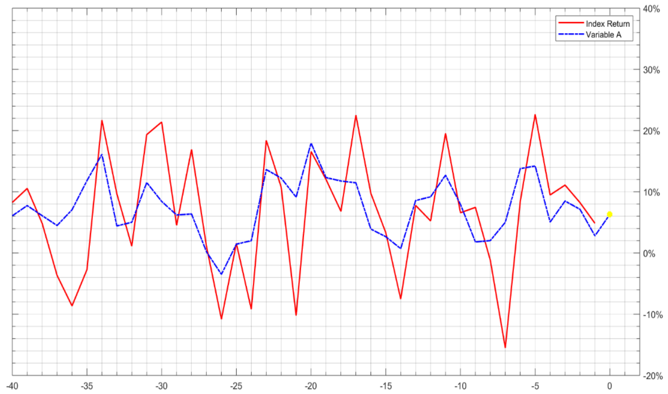
Table 9: Average ξ accross sub-samples

Sub-sample	A perceived useful	A perceived useless
High Investments	0.58	0.69
Low Investments	0.37	0.78
Fast	0.36	0.58
Slow	0.56	0.81
High Ability	0.47	0.75
Low Ability	0.52	0.74
Female	0.50	0.60
Male	0.49	0.88
High Grades	0.51	0.68
Low Grades	0.45	0.87
Young	0.58	0.64
Old	0.47	0.75
High Education	0.63	0.59
Low Education	0.99	0.72
High Income	0.65	0.58
Low Income	0.79	0.69
High Fin. Literacy	0.48	0.71
Low Fin. Literacy	0.83	0.44

NOTE: This table reports the average of ξ estimated on sub-samples of subjects, using the elasticity to "informed forecasts" $\{\widetilde{F}_{i,k}\}$ in both round types. "High θ" is a dummy equal to one if the subject takes larger risk investments, on average, than the median; "Fast" is a dummy equal to one if the subject is faster, on average, than the median seconds in answering each round's questions; "High Ability" is a dummy equal to one if the subject is better than the median in identifying when "Variable A" is useful or not; "High Grades" is a dummy equal to one if the subject has average grades above her/his cohort's median in TSE Master's program; "Female" is a dummy equal to one if the subject is a woman. The variables "Young", "High Education", "High Income" and "High Fin. Literacy" only apply to Prolific subjects. "Young" is a dummy equal to one if the subject's age is less than the median of all Prolific subjects. "High Education" is a dummy equal to one if the subject has a 4-year college degree. "High Income" is dummy equal to one if the subject has annual income above $50,000. "High Fin. Literacy" is a dummy equal to one if the subjects answer correctly three financial literacy questions.

Examples

Below are examples of what you may see during the experiment:



The red line represents the past *returns of the index* from period -40 to period -1.

The blue dashed line is the past realizations of *Variable A* , from period -40 to 0. Today's value of *Variable A* is indicated by the yellow dot. In certain rounds, this yellow dot can be useful for predicting *the index returns* . We are at date 0, today.

In the graph above, *Variable A* is *useful* to predict *the index returns.*

Here is another example of the graphs that you may see. In this second graph, *Variable A* is *not useful* in predicting *the index returns.*

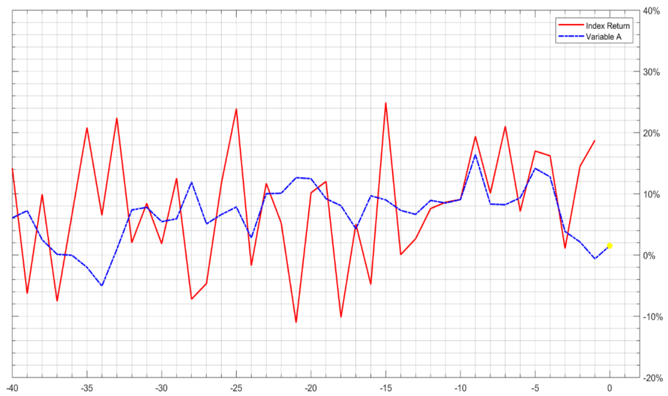


Figure 1: **Example page:** This page is provided to subjects before they start playing the investment game and provides examples of the two types of rounds – "Variable A" predictive or not.

References

Adam, K., A. Marcet, and J. Beutel. 2017. Stock price booms and expected capital gains. *American Economic Review* 107:2352–408.

Afrouzi, H., S. Y. Kwon, A. Landier, Y. Ma, and D. Thesmar. 2023. Overreaction in expectations: Evidence and theory. *The Quarterly Journal of Economics* 138:1713–64.

Alvarez, F., L. Guiso, and F. Lippi. 2012. Durable consumption and asset management with transaction and observation costs. *American Economic Review* 102:2272–300.

Andries, M., M. Bonelli, and D. A. Sraer. 2024. Financial advisors and investors' bias .

Andries, M., and V. Haddad. 2020. Information aversion. *Journal of Political Economy* 128:1901–39.

Assenza, T., T. Bao, C. Hommes, D. Massaro, et al. 2014. Experiments on expectations in macroeconomics and finance. *Experiments in Macroeconomics* 17:11–70.

Barberis, N., R. Greenwood, L. Jin, and A. Shleifer. 2015. X-capm: An extrapolative capital asset pricing model. *Journal of financial economics* 115:1–24.

———. 2018. Extrapolation and bubbles. *Journal of Financial Economics* 129:203–27.

Barberis, N., and M. Huang. 2008. Stocks as lotteries: The implications of probability weighting for security prices. *American Economic Review* 98:2066–100.

Barberis, N. C., and L. J. Jin. 2023. Model-free and model-based learning as joint drivers of investor behavior. Working Paper, National Bureau of Economic Research.

Barro, R. J. 2006. Rare disasters and asset markets in the twentieth century. *The Quarterly Journal of Economics* 121:823–66.

Benartzi, S. 2001. Excessive extrapolation and the allocation of 401 (k) accounts to company stock. *The Journal of Finance* 56:1747–64.

Bender, S., J. J. Choi, D. Dyson, and A. Z. Robertson. 2022. Millionaires speak: What drives their personal investment decisions? *Journal of Financial Economics* 146:305–30.

Berk, J. B., and R. C. Green. 2004. Mutual fund flows and performance in rational markets. *Journal of political economy* 112:1269–95.

Beutel, J., and M. Weber. 2022. Beliefs and portfolios: Causal evidence. *Chicago Booth Research Paper* .

Bianchi, M., and P. Jehiel. 2015. Financial reporting and market efficiency with extrapolative investors. *Journal of Economic Theory* 157:842–78.

Bordalo, P., N. Gennaioli, Y. Ma, and A. Shleifer. 2020. Overreaction in macroeconomic expectations. *American Economic Review* 110:2748–82.

Bordalo, P., N. Gennaioli, R. L. Porta, and A. Shleifer. 2019. Diagnostic expectations and stock returns. *The Journal of Finance* 74:2839–74.

Bordalo, P., N. Gennaioli, and A. Shleifer. 2012. Salience theory of choice under risk. *The Quarterly Journal of Economics* 127:1243–85.

Brunnermeier, M. K., and S. Nagel. 2008. Do wealth fluctuations generate time-varying risk aversion? micro-evidence on individuals. *American Economic Review* 98:713–36.

Calvet, L. E., J. Y. Campbell, and P. Sodini. 2009. Fight or flight? portfolio rebalancing by individual investors. *The Quarterly Journal of Economics* 124:301–48.

Campbell, J. Y., and R. J. Shiller. 1988. Stock prices, earnings, and expected dividends. *Journal of Finance* 43:661–76.

Cassella, S., B. Golez, H. Gulen, and P. Kelly. 2021. Horizon bias in expectations formation .

Chapman, L. J. 1967. Illusory correlation in observational report. *Journal of Verbal Learning and Verbal Behavior* 6:151–5.

Charles, C., C. Frydman, and M. Kilic. 2024. Insensitive investors. *The Journal of Finance* 79:2473–503.

Chaudhry, A. 2022. *Do subjective growth expectations matter for asset prices?*

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.

Chen, H., N. Ju, and J. Miao. 2014. Dynamic asset allocation with ambiguous return predictability. *Review of Economic Dynamics* 17:799–823.

Cochrane, J. H. 2009. *Asset pricing: Revised edition.* Princeton university press.

Dahlquist, M., and M. Ibert. 2024. Equity return expectations and portfolios: Evidence from large asset managers. *The Review of Financial Studies* 37:1887–928.

Dominitz, J., and C. F. Manski. 2007. Expected equity returns and portfolio choice: Evidence from the health and retirement study. *Journal of the European Economic Association* 5:369–79.

———. 2011. Measuring and interpreting expectations of equity returns. *Journal of Applied Econometrics* 26:352–70.

Ellsberg, D. 1961. Risk, ambiguity, and the savage axioms. *The Quarterly Journal of Economics* 643–69.

Enke, B., and T. Graeber. 2023. Cognitive uncertainty. *The Quarterly Journal of Economics* 138:2021–67.

Enke, B., T. Graeber, and R. Oprea. 2023. Confidence, self-selection, and bias in the aggregate. *American Economic Review* 113:1933–66.

Enke, B., T. Graeber, R. Oprea, and J. Yang. 2024. Behavioral attenuation .

Fama, E. F., and K. R. French. 1988. Dividend yields and expected stock returns. *Journal of Financial Economics* 22:3–25.

Frydman, C., and L. J. Jin. 2022. Efficient coding and risky choice. *The Quarterly Journal of Economics* 137:161–213.

Gabaix, X. 2008. Variable rare disasters: A tractable theory of ten puzzles in macro-finance. *American Economic Review* 98:64–7.

———. 2019. Behavioral inattention. In *Handbook of Behavioral Economics: Applications and Foundations 1*, vol. 2, 261–343. Elsevier.

Gabaix, X., and R. S. Koijen. 2021. In search of the origins of financial fluctuations: The inelastic markets hypothesis. Working Paper, National Bureau of Economic Research.

Gabaix, X., R. S. Koijen, F. Mainardi, S. Oh, and M. Yogo. 2024. Asset demand of us households .

Giglio, S., M. Maggiori, J. Stroebel, and S. Utkus. 2021a. Five facts about beliefs and portfolios. *American Economic Review* 111:1481–522.

———. 2021b. The joint dynamics of investor beliefs and trading during the covid-19 crash. *Proceedings of the National Academy of Sciences* 118:e2010316118–.

Greenwood, R., and A. Shleifer. 2014. Expectations of returns and expected returns. *Review of Financial Studies* 27:714–46.

Haddad, V., P. Huebner, and E. Loualiche. 2021. How competitive is the stock market? theory, evidence from portfolios, and implications for the rise of passive investing .

Hansen, L. P., J. C. Heaton, and N. Li. 2008. Consumption Strikes Back? Measuring Long-Run Risk. *Journal of Political Economy* 116:260–302.

Hirshleifer, D., and S. H. Teoh. 2003. Limited attention, information disclosure, and financial reporting. *Journal of Accounting and Economics* 36:337–86.

Hong, H., J. C. Stein, and J. Yu. 2007. Simple forecasts and paradigm shifts. *The Journal of Finance* 62:1207–42.

Hurd, M. D., and S. Rohwedder. 2012. Stock price expectations and stock trading. Working Paper, National Bureau of Economic Research.

Jin, L. J., and C. Peng. 2024. The law of small numbers in financial markets: Theory and evidence .

Kacperczyk, M., S. Van Nieuwerburgh, and L. Veldkamp. 2016. A rational theory of mutual funds' attention allocation. *Econometrica* 84:571–626.

Kahneman, D., and A. Tversky. 1979. Prospect theory: An analysis of decision under risk. *Econometrica* 47:363–91.

Kruschke, J. K., and M. K. Johansen. 1999. A model of probabilistic category learning. *Journal of Experimental Psychology: Learning, Memory, and Cognition* 25:1083–.

Landier, A., Y. Ma, and D. Thesmar. 2019. Biases in expectations: Experimental evidence .

Laudenbach, C., A. Weber, R. Weber, and J. Wohlfart. 2023. Beliefs about the stock market and investment choice: Evidence from a survey and a field experiment .

Linnainmaa, J., B. Melzer, A. Previtero, and S. Foerster. 2020. Investor protections and stock market participation: an evaluation of financial advisor oversight. Working Paper.

Liu, H., and C. Palmer. 2021. Are stated expectations actual beliefs? new evidence for the beliefs channel of investment demand. Working Paper, National Bureau of Economic Research.

Malloy, C. J., T. J. Moskowitz, and A. Vissing-Jørgensen. 2009. Long-run stockholder consumption risk and asset returns. *The Journal of Finance* 64:2427–79.

Malmendier, U., and S. Nagel. 2011. Depression babies: Do macroeconomic experiences affect risk taking? *The Quarterly Journal of Economics* 126:373–416.

Manski, C. F. 2018. Survey measurement of probabilistic macroeconomic expectations: progress and promise. *NBER Macroeconomics Annual* 32:411–71.

Martin, I. W. 2013. Consumption-based asset pricing with higher cumulants. *Review of Economic Studies* 80:745–73.

Maxted, P. 2024. A macro-finance model with sentiment. *Review of Economic Studies* 91:438–75.

Merton, R. C. 1969. Lifetime portfolio selection under uncertainty: The continuous-time case. *The review of Economics and Statistics* 247–57.

Nagel, S., and Z. Xu. 2023. Dynamics of subjective risk premia. *Journal of Financial Economics* 150.

Odean, T. 1998. Are investors reluctant to realize their losses? *The Journal of finance* 53:1775–98.

Schoar, A., and Y. Sun. 2024. Financial narratives and investor beliefs: Experimental evidence on active vs. passive advice .

Shiller, R. J. 2000. Measuring bubble expectations and investor confidence. *Journal of Psychology and Financial Markets* 1:49–60.

Sicherman, N., G. Loewenstein, D. J. Seppi, and S. P. Utkus. 2016. Financial attention. *The Review of Financial Studies* 29:863–97.

Tversky, A., and D. Kahneman. 1973. Availability: A heuristic for judging frequency and probability. *Cognitive psychology* 5:207–32.

Ungeheuer, M., and M. Weber. 2021. The perception of dependence, investment decisions, and stock prices. *Journal of Finance* 76:797–844.

Van Nieuwerburgh, S., and L. Veldkamp. 2010. Information acquisition and under-diversification. *The Review of Economic Studies* 77:779–805.

Whitson, J. A., and A. D. Galinsky. 2008. Lacking control increases illusory pattern perception. *Science* 322:115–7.

Woodford, M. 2020. Modeling imprecision in perception, valuation, and choice. *Annual Review of Economics* 12:579–601.

Wunderlich, K., M. Symmonds, P. Bossaerts, and R. J. Dolan. 2011. Hedging your bets by learning reward correlations in the human brain. *Neuron* 71:1141–52.

ONLINE APPENDIX

Appendix A Additional Results

Table A.1: Descriptive Statistics: Online subjects (Prolific + TSE) with number of correct answers ≤ 11

Variable	Obs.	Mean	Median	Std. Dev.	Min	Max
$Pr(A\ perceived\ predictive \mid\ predictable)$	39	0.76	0.80	0.19	0.40	1
$Pr(A\ perceived\ useless \mid\ i.i.d)$	39	0.25	0.20	0.19	0	0.7
Predict	39	0.76	0.75	0.18	0.35	1
Forecast (in %)	780	13.2	9	15.8	-25	100
Forecast Distance (in %)	780	13.0	8.6	14.2	0.0	95.3
Invest (in ECU)	780	40.2	33.5	33.0	0	100
			Predict=1			
Forecast (in %)	590	13.5	9	15.8	-25	100
Upper prob. (in %)	568	27.6	20	26.5	0	100
Lower prob. (in %)	568	22.0	12	23.1	0	100
Forecast Distance (in %)	590	12.9	8.5	14.3	0.1	95.3
Invest (in ECU)	590	43.2	40	33.0	0	100
			Predict=0			
Forecast (in %)	190	12.4	9.5	15.8	-15	100
Upper prob. (in %)	172	19.4	10	20.9	0	90
Lower prob. (in %)	172	24.6	12	27.2	0	100
Forecast Distance (in %)	190	13.3	9.0	14.1	0.0	87.5
Invest (in ECU)	190	30.6	20	31.2	0	100

NOTE: "Predict" is a dummy equal to one if the subject perceives "Variable A" is useful to predict returns. ""Variable A" is revealed predictive" and "Variable A" is revealed not predictive" correspond to treatments where subjects are told explicitly if "Variable A" is useful or not.

Table A.2: Forecast and Investments: Online subjects (Prolific + TSE) with number of correct answers ≤ 11

Dep Variable	Forecast						Investments		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
a(t)	0.65	0.64	0.69						
	(0.49)	(0.42)	(0.41)						
a(t) × Predict	-0.54	-0.51	-0.59						
	(0.51)	(0.44)	(0.43)						
r(t)				0.12	0.08	0.08			
				(0.10)	(0.09)	(0.09)			
r(t) × Predict				-0.05	-0.00	-0.00			
				(0.10)	(0.12)	(0.11)			
Forecast							-0.05	0.31	0.33
							(0.13)	(0.24)	(0.24)
Forecast × Predict							0.15	0.13	0.15
							(0.15)	(0.11)	(0.14)
Predict	4.19	3.86	4.51*	1.18	0.83	1.01	10.71*	4.96	4.33
	(3.67)	(2.55)	(2.55)	(2.28)	(0.78)	(1.06)	(5.69)	(3.11)	(3.03)
N	780	780	780	780	780	780	780	780	780
R^2	0.01	0.63	0.64	0.00	0.63	0.64	0.03	0.55	0.57
Individual FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Round FE	No	No	Yes	No	No	Yes	No	No	Yes

NOTE: This table reports the results of OLS regressions. The dependent variable is the forecast of next period returns in percentage points, and the endowment invested in the risky asset in ECU respectivement. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. a(t) denotes the last realization of "Variable A". r(t) denotes the last realization of "Index Return". "Forecast" is the forecast of next period returns in percentage points. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denote significance at 10%, 5% and 1% level, respectively.

Table A.3: Demographic characteristics of Prolific subjects

Variable	Obs.	Mean	Median	Std. Dev.	Min	Max
Age	56	39.1	38	11.9	19	71
FinLit (# of correct answer)	57	2.4	3	0.9	0	3
Female	26/56					
Education						
High School Degree/GED or less	16/56					
Two or four-year college degree	28/56					
Master's degree or above	12/56					
Annual income						
Less than \$50,000	20/57					
From \$50,000 to \$110,000	26/57					
Above \$110,000	11/57					

Table A.4: Forecast and Investment, Gender

Dep Variable	Forecast				Investment	
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	0.44***	0.03				
	(0.07)	(0.10)				
a(t) × Female	-0.13*	-0.07				
	(0.07)	(0.14)				
r(t)			-0.06	0.14***		
			(0.04)	(0.04)		
r(t) × Female			0.12**	0.05		
			(0.05)	(0.06)		
Forecast					2.07***	1.81***
					(0.18)	(0.22)
Forecast × Female					-0.71**	-0.67*
					(0.30)	(0.33)
Forecast × Predict						0.34*
						(0.19)
Forecast × Predict × Female						0.11
						(0.28)
Predict						5.12**
						(1.90)
Predict × Female						-1.21
						(2.75)
N	1,865	1,475	1,865	1,475	3,340	3,340
R^2	0.16	0.27	0.14	0.30	0.58	0.59
Sample	Predict = 1	Predict = 0	Predict = 1	Predict = 0	All	All
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. In columns (1)-(4), the dependent variable is the next-period forecast of returns, in percentage points. In columns (5)-(6), the dependent variable is the ECU next-period investment in the risky asset. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. "Female" is a dummy equal to one if the subject is a woman. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively.

Table A.5: Forecast and Investment, High versus Low Risk Investment

Dep Variable	Forecast				Investment	
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	0.38***	-0.11				
	(0.07)	(0.07)				
a(t) × High Investments	-0.02	0.27**				
	(0.09)	(0.12)				
r(t)			-0.01	0.20***		
			(0.04)	(0.05)		
r(t) × High Investments			0.02	-0.06		
			(0.06)	(0.07)		
Forecast					1.74***	1.41***
					(0.18)	(0.19)
Forecast × High Investments					-0.13	-0.07
					(0.36)	(0.36)
Forecast × Predict						0.48**
						(0.20)
Forecast × Predict × High Investments						0.01
						(0.28)
Predict						5.03***
						(1.03)
Predict × High Investments						-1.65
						(1.94)
N	1,888	1,492	1,888	1,492	3,380	3,380
R^2	0.16	0.27	0.13	0.30	0.58	0.59
Sample	Predict = 1	Predict = 0	Predict = 1	Predict = 0	All	All
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. In columns (1)-(4), the dependent variable is the next-period forecast of returns, in percentage points. In columns (5)-(6), the dependent variable is the ECU next-period investment in the risky asset. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. "High θ" is a dummy equal to one if the subject takes larger or equal risk investments, on average, than the median, for the same wave and the same round-type, in both types of rounds. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively.

Table A.6: Forecast and Investment, High versus Low Ability

Dep Variable	Forecast				Investment	
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	0.32***	0.02				
	(0.08)	(0.13)				
a(t) × High Ability	0.10	-0.02				
	(0.10)	(0.15)				
r(t)			0.04	0.17**		
			(0.04)	(0.06)		
r(t) × High Ability			-0.09*	0.01		
			(0.05)	(0.07)		
Forecast					1.47***	1.17***
					(0.26)	(0.26)
Forecast × High Ability					0.45	0.47
					(0.30)	(0.32)
Forecast × Predict						0.53**
						(0.19)
Forecast × Predict × High Ability						-0.10
						(0.27)
Predict						2.63*
						(1.36)
Predict × High Ability						2.89
						(1.70)
N	1,888	1,492	1,888	1,492	3,380	3,380
R^2	0.16	0.27	0.14	0.30	0.58	0.59
Sample	Predict = 1	Predict = 0	Predict = 1	Predict = 0	All	All
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. In columns (1)-(4), the dependent variable is the next-period forecast of returns, in percentage points. In columns (5)-(6), the dependent variable is the ECU next-period investment in the risky asset. “Predict” is a dummy equal to one if the subject declares “Variable A” is useful to predict returns. “High Ability” is a dummy equal to one if the subject is better or equal to the median, in the same wave, in identifying when “Variable A” is useful or not, as measured by $Pr(A\ \textit{perceived predictive} \mid \textit{predictable}) + Pr(A\ \textit{perceived useless} \mid\ i.i.d)$. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively.

Table A.7: Forecast and Investment, Age

Dep Variable	Forecast				Investment	
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	0.38***	0.33				
	(0.09)	(0.20)				
a(t) × Young	-0.06	-0.39				
	(0.12)	(0.28)				
r(t)			-0.06	-0.02		
			(0.07)	(0.10)		
r(t) × Young			0.10	0.26*		
			(0.09)	(0.12)		
Forecast					1.09**	1.05**
					(0.46)	(0.45)
Forecast × Young					0.13	-0.10
					(0.56)	(0.57)
Forecast × Predict						0.00
						(0.08)
Forecast × Predict × Young						0.51
						(0.32)
Predict						9.04***
						(2.36)
Predict × Young						-4.85*
						(2.42)
N	660	480	660	480	1,140	1,140
R^2	0.20	0.38	0.18	0.40	0.59	0.60
Sample	Predict = 1	Predict = 0	Predict = 1	Predict = 0	All	All
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. In columns (1)-(4), the dependent variable is the next-period forecast of returns, in percentage points. In columns (5)-(6), the dependent variable is the ECU next-period investment in the risky asset. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. "Young" is a dummy equal to one if the subject is younger or of same age as the median of 38 years old. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively. These results were obtained during wave four of the experiment implementation (Prolific online, July 2023).

Table A.8: Forecast and Investment, Income

Dep Variable	Forecast				Investment	
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	0.32***	0.06				
	(0.11)	(0.23)				
a(t) × High Income	0.05	0.05				
	(0.06)	(0.25)				
r(t)			0.02	0.15		
			(0.07)	(0.11)		
r(t) × High Income			-0.05	-0.06		
			(0.09)	(0.15)		
Forecast					1.80***	1.79***
					(0.39)	(0.48)
Forecast × High Income					-0.87*	-0.98*
					(0.47)	(0.52)
Forecast × Predict						0.01
						(0.49)
Forecast × Predict × High Income						0.17
						(0.58)
Predict						3.88
						(3.44)
Predict × High Income						5.11
						(4.97)
N	649	471	649	471	1,120	1,120
R^2	0.20	0.38	0.18	0.39	0.59	0.61
Sample	Predict = 1	Predict = 0	Predict = 1	Predict = 0	All	All
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. In columns (1)-(4), the dependent variable is the next-period forecast of returns, in percentage points. In columns (5)-(6), the dependent variable is the ECU next-period investment in the risky asset. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. "High Income" is a dummy equal to one if the subject has income above or equal to $50,000 per year. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively. These results were obtained during wave four of the experiment implementation (Prolific online, July 2023).

Table A.9: Forecast and Investment, Education

Dep Variable	Forecast				Investment	
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	0.23	0.23				
	(0.16)	(0.18)				
a(t) × High Education	0.20	-0.20				
	(0.12)	(0.21)				
r(t)			-0.02	0.16		
			(0.08)	(0.10)		
r(t) × High Education			0.02	-0.09		
			(0.10)	(0.14)		
Forecast					1.52***	1.37***
					(0.28)	(0.42)
Forecast × High Education					-0.55	-0.49
					(0.42)	(0.47)
Forecast × Predict						0.18
						(0.41)
Forecast × Predict × High Education						0.01
						(0.47)
Predict						5.39
						(3.29)
Predict × High Education						2.17
						(4.85)
N	649	471	649	471	1,120	1,120
R^2	0.20	0.38	0.18	0.39	0.59	0.60
Sample	Predict = 1	Predict = 0	Predict = 1	Predict = 0	All	All
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. In columns (1)-(4), the dependent variable is the next-period forecast of returns, in percentage points. In columns (5)-(6), the dependent variable is the ECU next-period investment in the risky asset. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. "High Education" is a dummy equal to one if the subject has a 4-year college degree or above. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively. These results were obtained during wave four of the experiment implementation (Prolific online, July 2023).

Table A.10: Forecast and Investment, High Financial Literacy

Dep Variable	Forecast				Investment	
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	0.40**	0.03				
	(0.16)	(0.32)				
a(t) × High Fin. Literacy	-0.08	0.13				
	(0.17)	(0.38)				
r(t)			0.07	0.08		
			(0.08)	(0.14)		
r(t) × High Fin. Literacy			-0.13	0.05		
			(0.10)	(0.16)		
Forecast					0.61**	0.57**
					(0.25)	(0.22)
Forecast × High Fin. Literacy					1.25***	1.20***
					(0.38)	(0.40)
Forecast × Predict						0.15
						(0.16)
Forecast × Predict × High Fin. Literacy						-0.09
						(0.31)
Predict						8.19***
						(2.62)
Predict × High Fin. Literacy						-2.99
						(4.09)
N	660	480	660	480	1,140	1,140
R^2	0.20	0.38	0.18	0.39	0.61	0.62
Sample	Predict = 1	Predict = 0	Predict = 1	Predict = 0	All	All
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. In columns (1)-(4), the dependent variable is the next-period forecast of returns, in percentage points. In columns (5)-(6), the dependent variable is the ECU next-period investment in the risky asset. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. "High Fin. Literacy" is a dummy equal to one if the subject has correct answers on all three financial literacy questions. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively. These results were obtained during wave four of the experiment implementation (Prolific online, July 2023).

Table A.11: Forecast and Investment, Grades

Dep Variable	Forecast				Investment	
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	0.38***	-0.05				
	(0.09)	(0.11)				
a(t) × High Grades	-0.05	-0.03				
	(0.15)	(0.14)				
r(t)			0.02	0.20***		
			(0.05)	(0.07)		
r(t) × High Grades			0.01	-0.01		
			(0.07)	(0.09)		
Forecast					1.69***	1.32***
					(0.26)	(0.32)
Forecast × High Grades					0.99**	1.13**
					(0.35)	(0.48)
Forecast × Predict						0.67
						(0.43)
Forecast × Predict × High Grades						-0.33
						(0.52)
Predict						2.72
						(2.67)
Predict × High Grades						1.95
						(3.02)
N	959	801	959	801	1,760	1,760
R^2	0.15	0.16	0.13	0.22	0.60	0.61
Sample	Predict = 1	Predict = 0	Predict = 1	Predict = 0	All	All
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. In columns (1)-(4), the dependent variable is the next-period forecast of returns, in percentage points. In columns (5)-(6), the dependent variable is the ECU next-period investment in the risky asset. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. "High Grades" is a dummy equal to one if the subject has average grades at or above her/his cohort's median in TSE Master's program. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively. Because grading was affected by the COVID period, these results are specific to TSE students in the lab implementation (TSE lab, January 2019, 2020).

Table A.12: Forecast and Investment, Fast versus Slow

Dep Variable	Forecast				Investment	
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	0.46***	-0.08				
	(0.10)	(0.12)				
a(t) × Slow	-0.20	0.03				
	(0.16)	(0.16)				
r(t)			0.01	0.23***		
			(0.03)	(0.05)		
r(t) × Slow			0.03	-0.05		
			(0.06)	(0.07)		
Forecast					2.38***	1.70***
					(0.32)	(0.41)
Forecast × Slow					-0.44	0.18
					(0.38)	(0.45)
Forecast × Predict						1.20**
						(0.44)
Forecast × Predict × Slow						-1.12**
						(0.53)
Predict						0.21
						(2.51)
Predict × Slow						5.17
						(3.16)
N	959	801	959	801	1,760	1,760
R^2	0.15	0.16	0.13	0.22	0.59	0.60
Sample	Predict = 1	Predict = 0	Predict = 1	Predict = 0	All	All
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. In columns (1)-(4), the dependent variable is the next-period forecast of returns, in percentage points. In columns (5)-(6), the dependent variable is the ECU next-period investment in the risky asset. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. "Slow" is a dummy equal to one if the subject is as slow or slower, on average, than the median seconds in answering each round's questions. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively. Because the time spent on the experiment may be affected by external constraints for online subjects, we report these results solely for the lab implementation (TSE lab, January 2019, 2020).

Table A.13: Subjects Characteristics – Correlation Matrix

	High Investments	Fast	High Ability	Female	High Grades	Young	High Educ.	High Income	High Fin. Lit.
High Investments	1.00								
Fast	0.04	1.00							
High Ability	-0.05	-0.07	1.00						
Female	0.13	0.06	0.10	1.00					
High Grades	0.15	-0.32	0.09	0.05	1.00				
Young	-0.08		-0.09	-0.06		1.00			
High Educ.	0.04		-0.07	-0.06		-0.25	1.00		
High Income	-0.14		0.04	-0.24		-0.01	0.43	1.00	
High Fin. Lit.	0.14		0.12	-0.35		-0.08	0.18	0.27	1.00

NOTE: This table reports the correlations between the characteristics dummies: "High θ" is a dummy equal to one if the subject takes larger risk investments, on average, than the median; "Fast" is a dummy equal to one if the subject is faster, on average, than the median of 61 seconds in answering each round's questions, which we apply only to the TSE lab implementations; "High Ability" is a dummy equal to one if the subject is better than the median in identifying when "Variable A" is useful or not; "High Grades" is a dummy equal to one if the subject has average grades above her/his cohort's median in TSE Master's program; "Female" is a dummy equal to one if the subject is a woman. The variables "Young", "High educ", "High income" and "High FinLit" only apply to Prolific subjects. "Young" is a dummy equal to one if the subject's age is less than the median of all Prolific subjects. "High Educ." is a dummy equal to one if the subject has a 4-year college degree. "High Income" is dummy equal to one if the subject has annual income above $50,000. "High Fin. Lit." is a dummy equal to one if the subjects answer correctly three financial literacy questions.

Table A.14: Forecast and Investment, Long Horizon

Dep Variable	Forecast (5)				Investment (5)	
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	-0.07	-0.08				
	(0.10)	(0.09)				
a(t) × Predict	0.05	0.09				
	(0.11)	(0.09)				
r(t)			0.07**	0.08***		
			(0.03)	(0.03)		
r(t) × Predict			-0.03	-0.06		
			(0.05)	(0.05)		
Forecast (5)					0.74**	1.39**
					(0.27)	(0.50)
Forecast (5) × Predict						-0.90
						(0.61)
Predict	1.03	0.81*	1.33**	1.48**		7.20
	(0.62)	(0.46)	(0.58)	(0.53)		(4.48)
N	1,080	1,080	1,080	1,080	1,080	1,080
R^2	0.01	0.15	0.01	0.16	0.51	0.52
Individual FE	No	Yes	No	Yes	Yes	Yes
Round FE	No	Yes	No	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. In columns (1)-(4), the dependent variable is the forecast of the average returns over the next five periods, in percentage points. In columns (5)-(6), the dependent variable is the ECU investment in the risky asset for the next five periods. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. a(t) denotes the last realization of "Variable A". r(t) denotes the last realization of "Index Return". Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively. These results were obtained during wave one of the experiment implementation (TSE lab, January 2019).

Table A.15: Forecast and Investment, Revealed Predictability

Dep Variable	Forecast				Investment	
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	0.11	0.10				
	(0.10)	(0.11)				
a(t) × R.Predictive	0.24**	0.24**				
	(0.10)	(0.10)				
r(t)			0.21***	0.22***		
			(0.04)	(0.04)		
r(t) × R.Predictive			-0.33***	-0.38***		
			(0.06)	(0.05)		
Forecast					2.49***	2.11***
					(0.27)	(0.29)
Forecast × R.Predictive						0.83**
						(0.33)
R.Predictive	-1.19	-1.25	1.34**	1.49**		-0.32
	(1.07)	(0.95)	(0.58)	(0.53)		(3.23)
N	920	920	920	920	920	920
R^2	0.01	0.16	0.06	0.21	0.62	0.63
Individual FE	No	Yes	No	Yes	Yes	Yes
Round FE	No	Yes	No	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. In columns (1)-(4), the dependent variable is the next-period forecast of returns, in percentage points. In columns (5)-(6), the dependent variable is the ECU next-period investment in the risky asset. "R.Predictive", for "revealed predictive", is a dummy equal to one when subjects are told, before they form their forecasts and investments, that "Variable A" is useful to predict returns. a(t) denotes the last realization of "Variable A". r(t) denotes the last realization of "Index Return". Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively. These results were obtained during wave two and three of the experiment implementation (TSE lab, January 2020 and TSE online, 2021).

Table A.16: Forecast and Investment, Revealed Model

Dep Variable	Forecast				Investment	
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	-0.04	-0.04				
	(0.15)	(0.19)				
a(t) × Predict	0.62***	0.64**				
	(0.18)	(0.23)				
r(t)			0.01	-0.01		
			(0.09)	(0.11)		
r(t) × Predict			-0.06	-0.04		
			(0.10)	(0.11)		
Forecast					3.10***	2.59***
					(0.34)	(0.79)
Forecast × Predict						0.71
						(0.70)
Predict	-3.03**	-2.50	1.20	1.94**		1.95
	(1.06)	(1.55)	(0.72)	(0.83)		(5.21)
N	238	238	238	238	240	238
R^2	0.10	0.28	0.01	0.19	0.72	0.73
Individual FE	No	Yes	No	Yes	Yes	Yes
Round FE	No	Yes	No	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions for our third wave of experiment (March 2021). In columns (1)-(4), the dependent variable is the next-period forecast of returns, in percentage points. In columns (5)-(6), the dependent variable is the ECU next-period investment in the risky asset. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. a(t) denotes the last realization of "Variable A". r(t) denotes the last realization of "Index Return". Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively. These results were obtained during wave three of the experiment implementation (TSE online, March 2021).

Table A.17: Forecast and Investment, Learning

Dep Variable	Forecast		Investment	
	(1)	(2)	(3)	(4)
a(t)	0.00			
	(0.08)			
a(t) × Late Rounds	0.02			
	(0.08)			
a(t) × Predict	0.27***			
	(0.08)			
a(t) × Predict × Late Rounds	0.18**			
	(0.08)			
r(t)		0.21***		
		(0.05)		
r(t) × Late Rounds		-0.05		
		(0.05)		
r(t) × Predict		-0.18***		
		(0.06)		
r(t) × Predict × Late Rounds		0.00		
		(0.06)		
Forecast			1.50***	1.25***
			(0.18)	(0.24)
Forecast × Late Rounds			0.33	0.24
			(0.29)	(0.35)
Forecast × Predict				0.42*
				(0.23)
Forecast × Predict × Late Rounds				0.15
				(0.26)
Predict	-0.94*	1.74***		4.15***
	(0.48)	(0.40)		(0.87)
Late Rounds	-0.58	0.35	6.20***	6.11***
	(0.61)	(0.32)	(1.98)	(1.86)
N	3,380	3,380	3,380	3,380
R^2	0.15	0.16	0.57	0.58
Individual FE	Yes	Yes	Yes	Yes
Round FE	No	No	No	No

NOTE: This table reports the results of OLS regressions. In columns (1)-(3), the dependent variable is the next-period forecast of returns, in percentage points. In columns (4)-(5), the dependent variable is the ECU next-period investment in the risky asset. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. "Late Rounds" is a dummy equal to one for rounds 11-20, the second half of the baseline treatment. a(t) denotes the last realization of "Variable A". r(t) denotes the last realization of "Index Return". Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively.

Table A.18: Forecasts – Time Series Information other than $\{a_t, r_t\}$

Dep Variable	Forecast					
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	0.37***	0.39***	0.44***			
	(0.06)	(0.09)	(0.08)			
a(t-1)		-0.42***	-0.41***			
		(0.06)	(0.05)			
a(t-2)		-0.21***	-0.19**			
		(0.07)	(0.07)			
$\overline{a}$		17.94	11.11			
		(24.28)	(24.97)			
r(t)				0.17***	0.18***	0.18***
				(0.03)	(0.03)	(0.04)
r(t-1)					0.08*	0.09**
					(0.04)	(0.04)
r(t-2)					0.03	0.03
					(0.02)	(0.02)
$\overline{r}$					40.63***	41.35***
					(9.81)	(11.81)
N	1,888	1,888	1,888	1,492	1,492	1,492
R^2	0.16	0.09	0.22	0.30	0.05	0.31
Adj. R^2	0.07	0.09	0.14	0.20	0.05	0.21
Sample		Predict=1			Predict=0	
Individual & Round FE	Yes	No	Yes	Yes	No	Yes

NOTE: The dependent variable is the next-period forecast of returns, in percentage points. Columns (1)-(2) are restricted to rounds perceived as predictable by "Variable A". Columns (3)-(4) are restricted to rounds perceived as unpredictable by "Variable A". a_t, a_{t-1}, a_{t-2} and r_t, r_{t-1}, r_{t-2} are the last three realizations of "Variable A" and "Index Return" in the current round; $\overline{a}$ and $\overline{r}$ are their average values in the current round's full time series. "Predict" is a dummy equal to one if the subject declares that "Variable A" is useful to predict returns. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively.

Table A.19: Forecast, Past Rounds Returns

Dep Variable	Forecast						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$a_1(t)$	-0.00		-0.00				0.03
	(0.06)		(0.06)				(0.06)
$a_1(t)$ × Predict	-0.05		-0.06				-0.01
	(0.08)		(0.08)				(0.09)
$r_1(t+1)$		0.04*	0.04*				0.02
		(0.02)	(0.02)				(0.03)
$r_1(t+1)$ × Predict		-0.04	-0.04				-0.04
		(0.03)	(0.03)				(0.03)
$\overline{a_1(t)}$				-0.19		-0.19	-0.23
				(0.18)		(0.17)	(0.18)
$\overline{a_1(t)}$ × Predict				-0.32		-0.31	-0.31
				(0.20)		(0.20)	(0.23)
$\overline{r_1(t+1)}$					0.14***	0.14***	0.11*
					(0.04)	(0.04)	(0.06)
$\overline{r_1(t+1)}$ × Predict					-0.07	-0.09	-0.05
					(0.06)	(0.07)	(0.08)
Predict	1.70**	1.66***	2.01***	3.31**	1.85***	3.81**	3.82**
	(0.61)	(0.40)	(0.64)	(1.32)	(0.52)	(1.52)	(1.51)
Round number	0.04	0.04	0.04	0.04	0.03	0.04	0.04
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
N	3,211	3,211	3,211	3,211	3,211	3,211	3,211
R^2	0.15	0.15	0.15	0.15	0.15	0.15	0.15
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	No	No	No	No	No	No	No

NOTE: This table reports the results of OLS regressions. The dependent variable is the forecast of next period returns in percentage points in any given round $k > 1$. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns in round k. $a_{-1}(t)$ and $r_{-1}(t+1)$ denote the final realization of "Variable A" and of "Index Returns" in the previous round $k-1$. $\overline{a_-(t)}$ and $\overline{r_-(t+1)}$ denote the average of all final realizations of "Variable A" and of "Index Returns" in rounds 1 to $k-1$. The "Round number" variable is added to detect possible trends. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denote significance at 10%, 5% and 1% level, respectively.

Table A.20: Forecast and Investment, Anchoring

Dep Variable	Forecast				Investment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Past Forecast	0.14	-0.01	0.13	0.00			-0.83***	-0.24***
	(0.15)	(0.08)	(0.12)	(0.07)			(0.17)	(0.07)
Past Forecast × Predict	-0.06	-0.05	-0.05	-0.05			0.04	-0.01
	(0.11)	(0.07)	(0.09)	(0.06)			(0.18)	(0.10)
Past Error			0.17*	0.10*			0.34**	0.17*
			(0.09)	(0.05)			(0.14)	(0.08)
Past Error × Predict			-0.13*	-0.10**			-0.08	-0.17
			(0.07)	(0.04)			(0.19)	(0.12)
Past Investment			-0.02	-0.02**	0.44***	0.02	0.51***	0.05
			(0.01)	(0.01)	(0.04)	(0.04)	(0.04)	(0.04)
Past Investment × Predict			0.01	0.01	0.06*	0.03	0.07**	0.04
			(0.01)	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)
Past Profit			0.05	0.03			-0.05	-0.08
			(0.05)	(0.05)			(0.18)	(0.18)
Past Profit × Predict			-0.03	-0.02			-0.14	-0.14
			(0.06)	(0.06)			(0.24)	(0.23)
Predict	1.80**	1.67***	2.29**	2.11***	6.77***	8.20***	6.81**	9.58***
	(0.71)	(0.53)	(0.87)	(0.68)	(2.04)	(1.64)	(2.73)	(2.26)
Round number	0.04*	0.04	0.04*	0.05	0.38***	0.80***	0.33***	0.78***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)	(0.11)	(0.04)	(0.10)
N	3,211	3,211	3,211	3,211	3,211	3,211	3,211	3,211
R^2	0.02	0.15	0.04	0.16	0.24	0.48	0.27	0.48
Individual FE	No	Yes	No	Yes	No	Yes	No	Yes
Round FE	No	No	No	No	No	No	No	No

NOTE: This table reports the results of OLS regressions. In columns (1)-(4), the dependent variable is the next-period forecast of returns, in percentage points. In columns (5)-(8), the dependent variable is the ECU next-period investment in the risky asset. "Predict" is a dummy equal to one if the subject declares "Variable A" is useful to predict returns. "Past Forecast", "Past Error", "Past Investment" and "Past Profit" are, respectively, the next-period forecast of returns, the error between the realized next-period return and the forecast, the ECU next-period investment in the risky asset, and the ECU profit made on the risk investment in the preceding round. The "Round number" variable is added to detect possible trends. Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively.

Table A.21: Information and Investment – Outside Forecasts

Dep Variable	Investment "Noise"					
	(1)	(2)	(3)	(4)	(5)	(6)
a(t)	0.43***	0.40*	0.39	0.31**	0.34**	0.40**
	(0.15)	(0.22)	(0.23)	(0.14)	(0.14)	(0.16)
r(t)	-0.05	-0.07	-0.12	0.08	0.04	0.06
	(0.09)	(0.08)	(0.08)	(0.09)	(0.08)	(0.05)
N	1,888	1,888	1,888	1,492	1,492	1,492
R^2	0.00	0.00	0.01	0.00	0.00	0.00
Adj. R^2	0.00	-0.09	-0.10	-0.00	-0.13	-0.14
Sample	Predict=1			Predict=0		
Individual FE	No	Yes	Yes	No	Yes	Yes
Round FE	No	No	Yes	No	No	Yes

NOTE: This table reports the results of OLS regressions. The dependent variable is the residual of the regression of ECU next-period investment in the risky asset on subjects' stated forecasts. a_t is the last realization of "Variable A" and r_t the last realization of "Index return". Columns (1)-(3) are restricted to rounds perceived as predictable by "Variable A". Columns (4)-(6) are restricted to rounds perceived as unpredictable by "Variable A". Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively.

Table A.22: Investment and Information

Dep Variable	Investment							Forecast	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
r(t)	0.13	0.13*	0.12				0.24***		0.17***
	(0.08)	(0.07)	(0.07)				(0.04)		(0.03)
a(t)				0.86***	0.86***	0.88***	1.06***	0.37***	
				(0.13)	(0.16)	(0.16)	(0.18)	(0.06)	
N	3,380	3,380	3,380	3,380	3,380	3,380	3,380	1,888	1,492
R^2	0.00	0.43	0.46	0.01	0.44	0.47	0.47	0.16	0.30
Adj. R^2	0.00	0.40	0.43	0.01	0.41	0.43	0.44	0.07	0.20
Sample	All	All	All	All	All	All	All	Predict = 1	Predict = 0
Individual FE	No	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Round FE	No	No	Yes	No	No	Yes	Yes	Yes	Yes

NOTE: The dependent variable is the ECU next-period investment in the risky asset in column (1) - (7) and the stated forecast in column (8) and (9). r_t is the last realization of "Index Return". a_t is the last realization of "Variable A". Two-way clustered standard errors (round and individual levels) are in parenthesis. *, ** and *** denote significance at 10%, 5% and 1% level, respectively.

Appendix B Return Process

Case with Predictable Returns.

We simulate predictable annual returns according to the VAR process:

$$r^p_{1,t+1} = \alpha x_{1,t} + \varepsilon_{1,t+1}, \tag{B.1}$$
$$x_{1,t+1} = \beta x_{1,t} + \delta_{1,t+1},$$

where $r^p_{1,t}$ is the demeaned annual excess log return and $x_{1,t}$ is a state variable, estimated from the demeaned annual log dividend yield. The two shocks ε_1 and δ_1 follow normal distributions with mean 0 and standard deviation $\sigma(\varepsilon_1)$ and $\sigma(\delta_1)$ respectively, and have correlation $\rho_{\varepsilon,\delta}$. We use the estimated parameters from Cochrane (2009) on US equity (CRSP, 1927-1998): $\alpha = 0.16$, $\beta = 0.92$, $\sigma(\delta_1) = 15.2\%$, $\sigma(\varepsilon_1) = 19.2\%$, $\rho_{\varepsilon,\delta} = -0.72$.

The returns in the predictable process (2) displayed to subjects in the experiment correspond to a compounded 5-year average of returns simulated from annual process (B.1) above. For any simulated series from process (B.1) of length $5 \times T$: $\{x_{1,1}, x_{1,2}...x_{1,5\times T}\}$ and $\{r^p_{1,2}, r^p_{1,3}...r^p_{1,5\times T+1}\}$, we extract the returns $\{r^p_2, r^p_3, ..., r^p_{T+1}\}$ where $r^p_2 = \mu + \frac{r^p_{1,2}+r^p_{1,3}+r^p_{1,4}+r^p_{1,5}+r^p_{1,6}}{5}$; $r^p_3 = \mu + \frac{r^p_{1,7}+r^p_{1,8}+r^p_{1,9}+r^p_{1,10}+r^p_{1,11}}{5}$;...; $r^p_{T+1} = \mu + \frac{r^p_{1,5T-4}+r^p_{1,5T-2}+r^p_{1,5T-1}+r^p_{1,5T}+r^p_{1,5T+1}}{5}$, where $\mu = 6.07\%$ (again from Cochrane (2009)). Iterating from $r^p_{1,t+1}$, we obtain

$$\begin{aligned} r^p_{t+1} &= \underbrace{\mu + \frac{1}{5}\alpha\frac{1-\beta^5}{1-\beta}x_{1,t}}_{\text{expected return } a_t} \\ &\quad \underbrace{+\frac{1}{5}\left[\alpha\frac{1-\beta^{5-1}}{1-\beta}\delta_{1,t+1} + \alpha\frac{1-\beta^{5-2}}{1-\beta}\delta_{1,t+2} + .. + \alpha\delta_{1,t+5-1} + \sum_{i=1}^{5}\varepsilon_{1,t+1}\right]}_{\text{shock } \epsilon^p_{t+1}}, \end{aligned}$$

corresponding to the predictable returns process (2).

From a simulated series from process (B.1): $\{r^p_{1,2}, r^p_{1,3}...r^p_{1,5\times T+1}\}$ and $\{x_{1,1}, x_{1,2}...x_{1,5\times T}\}$, we also extract the conditional expectations $\{a_1, a_2...a_T\}$ for the predictable returns $\{r^p_2, r^p_3...r^p_{T+1}\}$ where $a_1 = \mu + \frac{1}{5}\alpha\frac{1-\beta^5}{1-\beta}x_{1,1}$; $a_2 = \mu + \frac{1}{5}\alpha\frac{1-\beta^5}{1-\beta}x_{1,6}$;...; $a_T = \mu + \frac{1}{5}\alpha\frac{1-\beta^5}{1-\beta}x_{1,5T-4}$, where

$\mu = 6.07\%$ as above. The predictive variable a thus constructed is such that $(a - \mu)$ follows an AR(1) process with persistence β^5.

Case with i.i.d. returns.

We simulate i.i.d. annual returns according to process:

$$r_{1,t+1} = \mu + e_{1,t+1}, \tag{B.2}$$

where $\mu = 6.07\%$ as in (B.1) and $e_1 \sim N(0, \sigma^2(e_1))$. We set $\sigma(e_1) = 20.18\%$ so that the unconditional variance is the same as for $r^p_{1,t+1}$ in (B.1). The returns in i.i.d. process (1), displayed to subjects in the experiment, correspond to a compounded 5-year average of returns simulated from annual process (B.2).

Conditional Variance of Returns.

Let $r_{N,t}$ be the N-year demeaned average return in the i.i.d. case

$$r_{N,t} = \frac{r_{1,t} + r_{1,t+1} + ... + r_{1,t+N}}{N}.$$

The conditional variance (equal to the unconditional variance) of $Nr_{N,t}$ is

$$Var_t(Nr_{N,t+1}) = N\sigma^2(e_1). \tag{B.3}$$

Let $r^p_{N,t}$ be the N-year demeaned average return in the predictable case:

$$r^p_{N,t} = \frac{r^p_{1,t} + r^p_{1,t+1} + ... + r^p_{1,t+N}}{N},$$

such that:

$$Nr^p_{N,t+1} = \underbrace{\alpha\frac{1-\beta^N}{1-\beta}x_{1,t}}_{\text{expected return } Nx_{N,t}} + \underbrace{(\alpha\sum_{i=1}^{N-1}\frac{1-\beta^i}{1-\beta}\delta_{1,t+i} + \sum_{i=1}^{N}\varepsilon_{1,t+i})}_{\text{shock } N\varepsilon^P_{t+1}},$$

with conditional variance:

$$
\begin{aligned}
Var_t(Nr^p_{N,t+1}) &= N\sigma^2(\varepsilon_1) + \alpha^2\sigma^2(\delta_1)\sum_{i=1}^{N-1}(\frac{1-\beta^i}{1-\beta})^2 \\
&\quad +2\alpha\rho_{\varepsilon,\delta}\sigma(\varepsilon_1)\sigma(\delta_1)\sum_{i=1}^{N-1}\frac{1-\beta^i}{1-\beta}.
\end{aligned}
$$

Given our estimated parameters, the negative term in $\rho_{e,\delta}$ dominates the positive term in α^2, so that $Var_t(r^p_{N,t+1}) < Var_t(r_{N,t+1})$, for N sufficiently low. For our experiment, we are interested in $N = 5$ for the one-period returns and $N = 25$ for the five-period averages, for which we have $Var_t(r^p_{5,t+1}) = 0.67Var_t(r_{5,t+1})$; $Var_t(r^p_{25,t+1}) = 0.61Var_t(r_{25,t+1})$.

Appendix C Experimental Protocol

Appendix C.1 Baseline treatment

The experiment starts with the instruction page (Figure C.1), then an example page (Figure 1), followed by 20 rounds of Question Page / Result Page (Figures C.2 and C.3).[48] Each round corresponds to a new simulation of returns, 10 rounds for the i.i.d. process (1) and 10 rounds for the predictable process (2). Subjects "play" the 20 rounds in a randomized order.

For the predictable rounds, we obtain the simulated returns of process (2) via a simulation of length 225 of the VAR process (B.1), averaged over 5-year periods to obtain 45 points for the expected return process r^p_{t+1} and 45 points for the conditional expectations a_t. We repeat this procedure to get 1,000 simulations, among which we choose the 10 simulations that have a statistical correlation between the simulated returns r^p_{t+1} and the conditional expectations a_t closest to 0.57, the theoretical correlation between the returns process and the predictive variable a.

For the i.i.d. rounds, we obtain the simulated returns of process (1) via a simulation of length 225 of the annual i.i.d. process (B.2), averaged over 5-year periods to obtain 45 points for the expected return process r_{t+1}. In addition, and independently, we add a simulation of length 225 of the state variable $x_{1,t}$ from VAR process (B.1) to obtain 45 points with same distribution as the variable a_t in the predictable rounds. We repeat this procedure to get 1,000 simulations, among which we choose the 10 simulations that have a statistical correlation between the simulated returns r_{t+1} and the variable a_t closest to 0, the theoretical correlation between the returns process and the variable a in the i.i.d. case.

We verify for each of the 20 rounds displayed to our subjects, the statistical regressions of the returns r_t on the variable a_{t-1}, and on past returns r_{t-1}. The results are displayed in Online Appendix Table C.1. In all rounds, the graph displayed in the Question page shows the first 40 points for the returns r_t, from $t = -40$ to $t = -1$ in red, and the first 41 points for variable a_{t-1}, from $t = -40$ to $t = 0$ in blue (shifted so that r_t and a_{t-1} are one above the other); with a_{-1},

[48] Figure C.1, C.2 and C.3 correspond to the first wave of our experiment implementation, in the TSE Lab (January 2019).

the best predictor for next-period returns r_0 displayed as a fat yellow dot at $t = 0$. Descriptive statistics for the 20 rounds are provided in Online Appendix Table C.2.

Appendix C.2 Additional questions/treatments

The instruction page in Figure C.1, as well as the Question Page / Result Page in Figures C.2 and C.3 add to the baseline treatment the solicitation of 5-period ahead forecasts and investments. In another implementation of our experiment, we asked subjects to provide, instead, their 80% confidence intervals via the two questions of Figure C.4. In yet another implementation, we asked subjects to provide their upper and lower bound probabilities via the two questions of Figure C.5.

In a separate treatment, subjects were asked to play another 20 rounds after they had completed the baseline treatment, where we revealed in the first 10 rounds that "Variable A" was predictive and in the last 10 rounds that it was useless to preduct returns. We used exactly the same 10 predictive and 10 i.i.d rounds as in the baseline treatment, each set in a new randomized order, to ensure subjects' answers can be compared across treatments.

Finally, in another treatment, subjects were asked to play another 10 rounds, after they had completed the baseline treatment. Before they had to choose their forecasts and investments in the new treatment, we revealed the simulation processes (1) and (2). The 10 rounds simulations were chosen randomly from the 20 rounds of the baseline treatment, 5 from i.i.d simulations, 5 from predictable simulations. The order of the 10 rounds was random across subjects.

Appendix C.3 Prolific: demographics, individual characteristics

For the online implementation of our experiment, we recruited subjects from Prolific.

To make sure these subjects understood and were paying attention to the experiment, they were asked two comprehension and two attention questions, standard to online experiments on such platforms (Figure C.6).

At the end of the experiment, we asked subjects to answer demographics questions on their gender, age, income, and education. In addition, we asked three questions related to their financial literacy. The Prolific survey questions, including financial literacy, are provided in Figure C.7.

Table C.1: Regression Coefficients of r_t on a_{t-1} and r_{t-1}.

Graph no.	Predictable	a(t-1)	p-value	R-squared	r(t-1)	p-value	R-squared
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	No	0.07	0.79	0	-0.13	0.45	0.02
2	No	-0.05	0.88	0	-0.01	0.96	0.00
3	No	0.09	0.78	0	0.16	0.34	0.02
4	No	-0.02	0.95	0	0.02	0.89	0.00
5	No	-0.27	0.4	0.02	0.13	0.44	0.02
6	No	-0.12	0.58	0.01	0.58	0.6	0.01
7	No	-0.02	0.94	0	-0.1	0.52	0.01
8	No	-0.05	0.91	0	-0.3	0.06	0.09
9	No	0.01	0.96	0	-0.34	0.04	0.11
10	No	-0.01	0.98	0	-0.04	0.81	0.00
11	Yes	1.17	0	0.34	0.21	0.21	0.04
12	Yes	1.53	0	0.38	-0.07	0.67	0.01
13	Yes	1.19	0	0.38	0	0.99	0.00
14	Yes	1	0	0.36	0.03	0.87	0.00
15	Yes	0.96	0	0.33	0.07	0.64	0.01
16	Yes	0.99	0	0.32	0.04	0.79	0.00
17	Yes	1.11	0	0.4	0	0.99	0.00
18	Yes	1.09	0	0.35	0.14	0.4	0.02
19	Yes	1.06	0	0.35	-0.11	0.5	0.01
20	Yes	0.85	0	0.32	-0.14	0.39	0.02

NOTE: This table reports the results of OLS regressions. The dependent variable is the returns r_t either for the i.i.d process (1) or the predictable process (2). Columns (3), (4) and (5) report the coefficient, p-value and R^2 of the regression on a_{t-1}. Column (6), (7) and (8) report the coefficient, p-value and R^2 of the regression on r_{t-1}.

Table C.2: Descriptive Statistics

Variable	Obs.	Mean	Median	Std. Dev.	Min	Max
a(t)	20	6.04	5.48	3.32	2.06	12.17
r(t)	20	3.16	2.95	8.90	-11.79	19.04
r(t+1)	20	6.62	5.94	8.45	-7.75	30.92
"Variable A" predictive						
a(t)	10	5.99	5.36	3.40	2.11	11.88
r(t)	10	4.82	3.82	8.15	-11.79	16.76
r(t+1)	10	4.93	3.78	4.13	-0.19	11.34
"Variable A" useless						
a(t)	10	6.08	5.70	3.42	2.06	12.17
r(t)	10	1.49	-2.44	9.73	-10.51	19.04
r(t+1)	10	8.31	6.72	11.29	-7.75	30.92

NOTE: This table reports the statistics for the last realizations of "Variable A" and of "Index Return", $a(t)$ and $r(t)$, that subjects observe, each round, in the "Question page".

Instruction

At the beginning of each round, you will be shown a graph of the past realizations of *the returns of an index*. You will also see the past realizations of a second variable (*Variable A*) in the same graph. In some rounds, *Variable A* is useful for predicting *the index returns*. In other rounds, the two variables are independent and *Variable A* cannot be used to predict *the index returns* .

Your task:

For each round, you will be endowed with 100 ECUs. Your task in each round includes 3 parts:

- Decide *whether variable A is useful* to forecast the index returns .
- Make forecasts on *the index returns* at different horizons .
- Choose how much of the 100 ECU you own to invest in the index. You will have to make two choices. One choice refers to an investment over one period, the other to an investment over five periods.

There are 20 rounds in this experiment. Every round is independent.

In all rounds, the average value of *returns* is 6.07%.

After each round, you will be shown information related to the realization of *the index returns* and whether *Variable A* was useful or not to make forecasts on *the index returns* . You will also be informed about the precision of your forecasts and about the total wealth you earn in that round.

How payoff is computed?

Your final payoff comprises of three parts:

(1) *Usefulness of variable A:* You will receive 5 ECU for every correct answer.

(2) *Forecast:* You will receive 10 ECU for every precise forecast. A forecast is considered precise if it lies between -1% and +1% of the realization.

(3) *Investment:*

Your final wealth in a given round is computed both for the one-period and the five-period horizon.
It is computed as: The value of your investment in *the index* over one period or five periods; plus the ECUs you did not invest, which stay unchanged.
At the end of the experiment, we will randomly choose one round and an investment horizon in order to compute the final payoff.

Your final payoff in ECU is the sum of payoff **(1)** and **(2)** for the *entire 20 rounds* and payoff **(3)** of *one randomly chosen round and horizon.*

Your final payoff in EUR is the final payoff in ECU divided by 20. This final payoff will be paid to you in cash at a future class.

If you have questions, please raise your hand and we will come to assist you.

Next

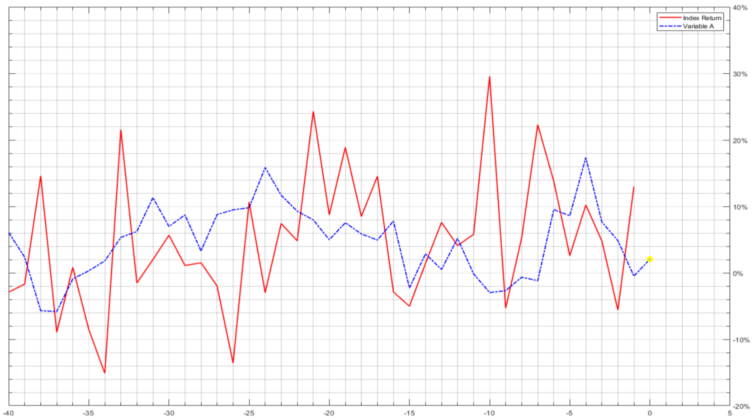
Figure C.1: **Instruction page:** This page is provided to subjects before they start playing the investment game and provides instructions.

See General Instruction

See Examples

Round 1: Forecasting and Investing

Below is the realization of *the index returns* and *Variable A* for the last 40 periods. You are at date 0, today.



You are endowed with 100 ECUs.

What is your forecast of *the index return* over the next period? — Your forecast (in percentage):

If your investment is for 1 period, how many of your 100 ECU do you want to invest in *the stock index?* — Investment amount (in ECU):

What is your forecast of the average 1-period returns over the next 5 periods? — Your forecast (in percentage):

If your investment is for 5 periods, how many of your 100 ECU do you want to invest in *the index?* — Investment amount (in ECU):

In this graph, do you think *Variable A* (blue line) is useful to predict *the index returns* (red line)? — Yes / No

Next

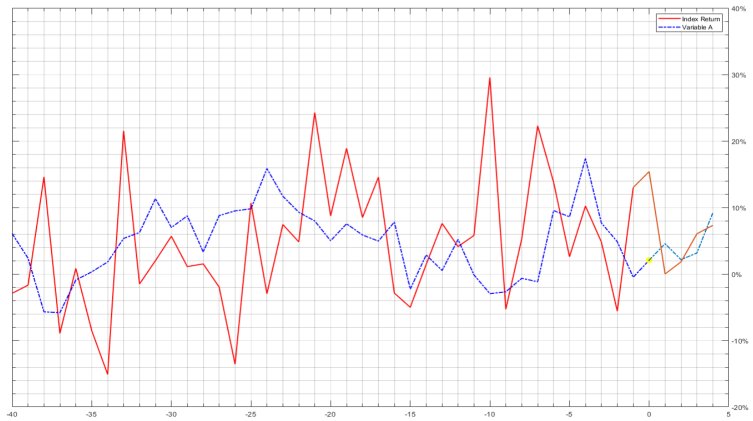
Figure C.2: **Question page:** Example of the question page where subjects write their answers on.

See General Instruction

See Examples

Round 1: Realization

The graph below shows the realizations of *the index returns* for the next 5 periods.
In this round, *Variable A* was **not useful** to predict *the index returns* .



Forecasting and investment result

HORIZON: 1 PERIOD

Description	**Index Return (Next period)**	**Forecast result**	**Value before realization**	**Value after realization**
Investment in *the index*	15.39 %	imprecise	50	58.32
Total Wealth	---	---	100	108.32

HORIZON: 5 PERIODS

Description	**Index Return (average over 5 periods)**	**Forecast result**	**Value before realization**	**Value after realization**
Investment in *the index*	6.11 %	precise	50	67.86
Total Wealth	---	---	100	117.86

Click the "Next Button" to go to the next round.

Next

Figure C.3: **Answer page:** Example of the answer page where subjects are told the realization of “Index returns”, and how well they did this round.

What is your forecast of *the index return* over the next period? %

There is **1 in 10** chance that the actual index return of the next period will be **below** : %

There is **1 in 10** chance that the actual index return of the next period will be **above** : %

Figure C.4: **Confidence Intervals:** We elicit subjects' confidence intervals via the questions above.

What is the probability that the index return is higher than 15%? %

What is the probability that the index return is lower than -3%? %

Figure C.5: **Upper and Lower Bound probabilities:** We elicit subjects' upper and lower Bound probabilities via the questions above.

Question 1: When is your market forecast rewarded as precise?

- ◯ When it's equal to the realized market return
- ◯ When it's less than 0.5% away from the realized market return
- ◯ When it's less than 1% away from the realized market return

Question 2: Your investment choices matter because:

- ◯ You will be paid only if you have an average positive return on your investment account
- ◯ You will receive your investment profit in all rounds in addition to your participation fee
- ◯ You will receive your investment profit in one randomly chosen round in addition to your participation fee

Attention Question

When asked for favorite shape, you must select "Triangle".

Based on the text you read above, what is the shape that you have been asked to select?

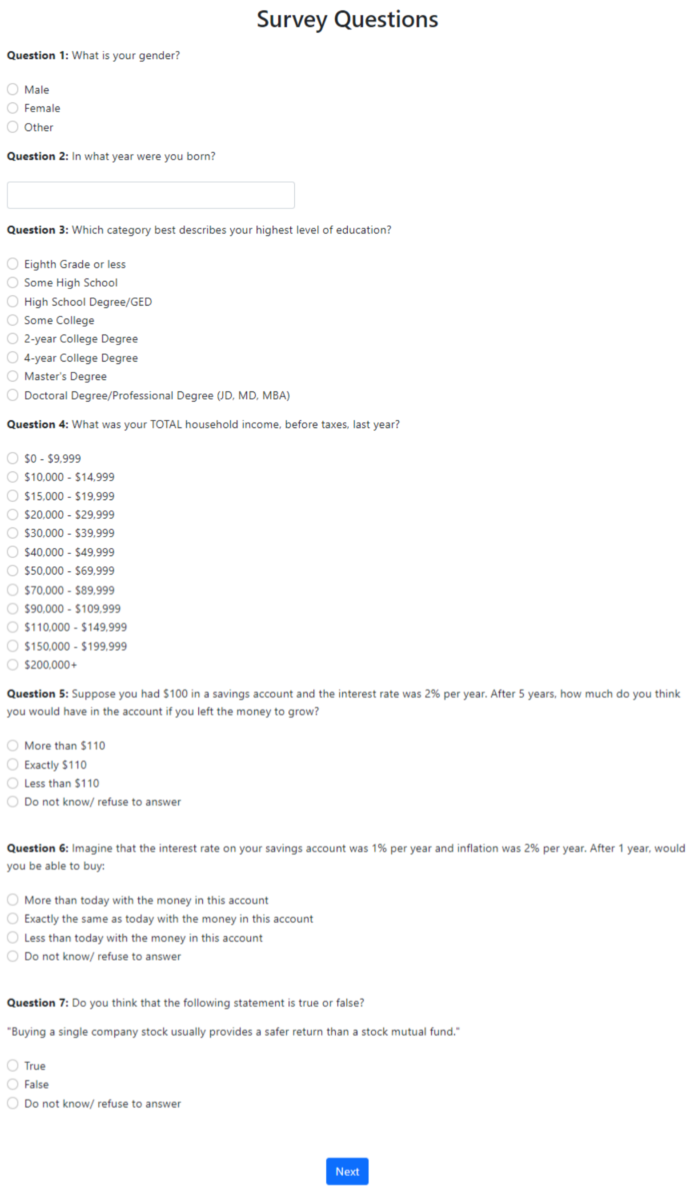
- ◯ Circle
- ◯ Square
- ◯ Rectangle
- ◯ Triangle
- ◯ Hexagon
- ◯ Oval

Attention Question

Please select "very often" to show that you pay attention to this question.

- ◯ Never
- ◯ Occasionally
- ◯ Often
- ◯ Very often
- ◯ Always

Figure C.6: **Comprehension and attention:** We verify subjects' comprehension and attention to the game via the questions above.



Survey Questions

Question 1: What is your gender?

- Male
- Female
- Other

Question 2: In what year were you born?

Question 3: Which category best describes your highest level of education?

- Eighth Grade or less
- Some High School
- High School Degree/GED
- Some College
- 2-year College Degree
- 4-year College Degree
- Master's Degree
- Doctoral Degree/Professional Degree (JD, MD, MBA)

Question 4: What was your TOTAL household income, before taxes, last year?

- $0 - $9,999
- $10,000 - $14,999
- $15,000 - $19,999
- $20,000 - $29,999
- $30,000 - $39,999
- $40,000 - $49,999
- $50,000 - $69,999
- $70,000 - $89,999
- $90,000 - $109,999
- $110,000 - $149,999
- $150,000 - $199,999
- $200,000+

Question 5: Suppose you had $100 in a savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow?

- More than $110
- Exactly $110
- Less than $110
- Do not know/ refuse to answer

Question 6: Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, would you be able to buy:

- More than today with the money in this account
- Exactly the same as today with the money in this account
- Less than today with the money in this account
- Do not know/ refuse to answer

Question 7: Do you think that the following statement is true or false?

"Buying a single company stock usually provides a safer return than a stock mutual fund."

- True
- False
- Do not know/ refuse to answer

Next

Figure C.7: **Survey page:** The questions above allow us to obtain demographics information from online subjects.

Appendix D Models

Appendix D.1 Forecast Model

In the model described in Section 4.1, subjects want to use expectation model $\mathbb{E}^u(r_{t+1})$ when "Variable A" is useless, and expectation model $\mathbb{E}^p(r_{t+1})$ when "Variable A" is predictive, s.t.:

$$\begin{cases} \mathbb{E}_t^u(r_{t+1}) & = \lambda_u r_t + (1-\lambda_u)\bar{\mu} \\ \mathbb{E}_t^p(r_{t+1}) & = \lambda_p a_t + (1-\lambda_p)\bar{\mu} \end{cases},$$

where r_t is the last realization of "Index Return", a_t is the last realization of "Variable A", and $\bar{\mu} = \mathbb{E}\left(r_{t+1}\right)$ is the unconditional subjective expectation.

Because subjects take their risks of mistake when identifying "Variable A" as useful or not, their forecasts follow:

$$\begin{cases} \mathbb{E}\left(r_{t+1}\mid_{A\ perceived\ useless}\right) & = \pi_u \mathbb{E}^u(r_{t+1}) + (1-\pi_u)\mathbb{E}^p(r_{t+1}) \\ \mathbb{E}\left(r_{t+1}\mid_{A\ perceived\ predictive}\right) & = \pi_p \mathbb{E}^p(r_{t+1}) + (1-\pi_p)\mathbb{E}^u(r_{t+1}) \end{cases},$$

where the weights π_u and π_p correspond to the probabilities that a given subject assigns to the fact that "Variable A" is indeed useless or predictive, conditional on the fact that she perceives it as such.

Given these assumptions, forecasts are given by:

$$\begin{aligned} F_{i,k} = {} & \alpha_1^m + \alpha_2^m \text{Predict}_{i,k} + \beta_1^m a_{t,k} + \beta_2^m a_{t,k} \times \text{Predict}_{i,k} \\ & + \delta_1^m r_{t,k} + \delta_2^m r_{t,k} \times \text{Predict}_{i,k}, \end{aligned}$$

where $F_{i,k}$ is the forecast of subject i for next-period returns in round k; $\text{Predict}_{i,k}$ is a dummy taking value 1 if subject i perceives "Variable A" as useful to predict returns in round k and taking value 0 otherwise; $a_{t,k}$ and $r_{t,k}$ are the last realizations of "Variable A" and "Index Return" in round

k. The coefficients $\{\alpha_1^m, \alpha_2^m, \beta_1^m, \beta_2^m, \delta_1^m, \delta_2^m\}$ are fully determined by parameters $\{\bar{\mu}, \lambda_u, \lambda_p, \pi_u, \pi_p\}$:

$$\begin{cases} \alpha_1^m & = \left(\pi_u \left(1-\lambda_u\right) + \left(1-\pi_u\right)\left(1-\lambda_p\right)\right)\bar{\mu} \\ \alpha_1^m + \alpha_2^m & = \left(\pi_p \left(1-\lambda_p\right) + \left(1-\pi_p\right)\left(1-\lambda_u\right)\right)\bar{\mu} \\ \beta_1^m & = \left(1-\pi_u\right)\lambda_p \\ \beta_1^m + \beta_2^m & = \pi_p \lambda_p \\ \delta_1^m & = \pi_u \lambda_u \\ \delta_1^m + \delta_2^m & = \left(1-\pi_p\right)\lambda_u \end{cases}$$

As described in Section 4.1, we set $\bar{\mu} = \mu = 6.07\%$ the true statistical average and $\lambda_u = 0.32$ as in Landier, Ma, and Thesmar (2019); Afrouzi et al. (2023). We assume that subjects do not overestimate nor underestimate on average their ability to correctly detect whether or not "Variable A" is predictive: we set π_u, π_p as the true posterior probabilities

$$\pi_p = \Pr\left(\textit{predictable} \mid A \textit{ perceived predictive}\right)$$

$$\pi_u = \Pr\left(i.i.d \mid A \textit{ perceived useless}\right),$$

which we observe in the data for each individual subject.

To set λ_p, we assume that subjects have no systematic bias, i.e., they do not overestimate nor underestimate on average the value of the loadings of $\{r_{t+1}\}$ on $\{a_t\}$, and take into account their risk of mistakes in identifying "Variable A" as predictive.

Let λ_p^p and λ_p^u be the estimated loadings of $\{r_{t+1}\}$ on $\{a_t\}$ in rounds perceived as predictable and as useless, respectively. The unbiased estimates of λ_p^p and λ_p^u are:

$$\begin{cases} \lambda_p^p & = \frac{\bar{\pi}_p \times 1 + (1-\bar{\pi}_u)\times 0}{\bar{\pi}_p + (1-\bar{\pi}_u)} \\ \lambda_p^u & = \frac{\bar{\pi}_u \times 0 + (1-\bar{\pi}_p)\times 1}{\bar{\pi}_u + (1-\bar{\pi}_p)} \end{cases}$$

where $\bar{\pi}_p = Pr(A \textit{ perceived predictive} \mid \textit{predictable})$ is the true fraction of predictable graphs perceived as such and $\bar{\pi}_u = Pr(A \text{ perceived useless} \mid i.i.d)$ is the true fraction of i.i.d. graphs perceived as such, i.e., $\pi_p = \frac{\bar{\pi}_p}{\bar{\pi}_p + (1-\bar{\pi}_u)}$ and $\pi_u = \frac{\bar{\pi}_u}{\bar{\pi}_u + (1-\bar{\pi}_p)}$.

Taking into account their probability of mistakes in identifying "Variable A" as predictive corresponds to setting parameter λ_p to:

$$\lambda_p = \frac{\pi_p \lambda_p^p + (1-\pi_u)\lambda_p^u}{\pi_p + (1-\pi_u)},$$

such that we obtain:

$$\lambda_p = \frac{\pi_p^2 + (1-\pi_u)^2}{\pi_p + (1-\pi_u)}.$$

The forecast model of Section 4.1 is *entirely* specified by setting parameters $\{\mu, \lambda_u, \pi_u, \pi_p\}$.

Appendix D.2 Investment Model

In Section 3.3, we show that subjects rely on their own forecasts differently across rounds, with a more limited "trust" accorded to extrapolative belief variations. We verify whether formalizing such a notion may be achieved via ambiguity averse agents, as in the classic Ellsberg Paradox (Ellsberg, 1961). Extending the classical Merton-Samuelson model of Equation (13) to allow for ambiguous returns predictability, in the one-period static case of our experiment, i) lowers the average risk investment, for a given level of return volatility; and ii) leads to a lower pass-through to investment from positive predictive signals than from negative ones, the well-known "worst case scenario" over-weighting specific to such models.[49] Both the decrease in the average risk taking and the asymmetry in the impact of "good" versus "bad" signals are amplified by greater ambiguity.

In our estimates, however, we do not find evidence of a systematically higher pass-through from forecasts to investment decisions when subjects receive "bad" versus "good" predictive signals, in either round type (Online Appendix Tables D.1 and D.2). Moreover, as shown in Section 4.3, our subjects' average investments are consistent with the classical Merton-Samuelson model: they do

[49] See, e.g., the theoretical results of Chen, Ju, and Miao (2014) who derive optimal risk taking decisions under ambiguous returns predictability in a dynamic framework.

not reflect potential differences in model uncertainty across round types.

Models in which our subjects would view their own forecasts as noisier, and hence riskier, in rounds without a predictive "Variable A" can be rejected for the same reason. If greater noise risk were perceived when subjects extrapolate from past returns, it would depress average portfolio investments in these rounds, and the difference in risk taking across round types would no longer match the return variances unbiased estimates of Equation (12), under $\mathbb{E}\left(Var_t^u(r_{t+1})\right) = \sigma^2$ and $\mathbb{E}\left(Var^p(r_{t+1})\right) = \sigma_p^2$.

Finally, though measurement errors play an important role, as evidenced by the differential impact of the "instrumented forecasts" of Equation 5 relative to the outright forecasts of Equation (4) in rounds where "Variable A" is perceived as useful, they cannot explain why forecasts "instrumented" by "Variable A" signals, in rounds where they are perceived as predictive, are treated differently from forecasts "instrumented" by extrapolation elsewhere, our core investment result.

Table D.1: Ambiguity aversion – Asymmetry test I

Dep Variable	Investment			
	(1)	(2)	(3)	(4)
Forecast	1.62***	2.00***	1.41***	1.43***
	(0.19)	(0.15)	(0.22)	(0.23)
N	1,018	866	447	1,029
R^2	0.70	0.65	0.75	0.65
Sample	Predict = 1 Below = 0	Predict = 1 Below = 1	Predict = 0 Below = 0	Predict = 0 Below = 1
Subject FE	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. The dependent variable is the ECU next-period investment in the risky asset. "Forecast" is the forecast of next period returns in percentage points. "Predict" is a dummy equal to one if the subject declares that "Variable A" is useful to predict returns. "Below" in the column (1), (2) take value of 1 if a_t is equal or below the true mean 6.07% and 0 otherwise. "Below" in the column (3), (4) take value of 1 if r_t is equal or below the true mean 6.07% and 0 otherwise. Clustered standard errors (round level) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively.

Table D.2: Ambiguity aversion – Asymmetry test II

Dep Variable	Investment			
	(1)	(2)	(3)	(4)
Forecast	1.49***	1.94***	1.48***	1.29***
	(0.28)	(0.13)	(0.15)	(0.22)
N	743	1,142	603	874
R^2	0.72	0.63	0.73	0.66
Sample	Predict = 1 Below = 0	Predict = 1 Below = 1	Predict = 0 Below = 0	Predict = 0 Below = 1
Subject FE	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes

NOTE: This table reports the results of OLS regressions. The dependent variable is the ECU next-period investment in the risky asset. "Forecast" is the forecast of next period returns in percentage points. "Predict" is a dummy equal to one if the subject declares that "Variable A" is useful to predict returns. "Below" in the column (1), (2) take value of 1 if a_t is equal or below the average realization of "Variable A" in the same round and 0 otherwise. "Below" in the column (3), (4) take value of 1 if r_t is equal or below the average realization of "Index Return" in the same round and 0 otherwise. Clustered standard errors (round level) are in parenthesis. *, ** and *** denotes significance at 10%, 5% and 1% level, respectively.