Return Predictability, Expectations, and Investment: Experimental Evidence

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In an investment experiment, we show variations in information affect beliefs and decision-making 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. Subjects form extrapolative forecasts following a signal they perceive as useless, and their investment decisions underreact to their beliefs. If the same subjects perceive the signal as predictive, they 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. (*JEL* G11, G41, D84)

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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

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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 evidence on investors in naturally occurring markets. As we will 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 underutilize actual predictive variables in the data (Nagel and Xu, 2023), while extrapolating too strongly from past returns, a bias extensively documented in the macroeconomic and finance literatures (see, e.g., Afrouzi et al., 2023; Assenza et al., 2014; Beutel and Weber, 2022; Bordalo et al., 2020; Dominitz and Manski, 2011; Greenwood and Shleifer, 2014; Manski, 2018; Shiller, 2000). Third, there is widespread evidence of suboptimal 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 were 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 U.S. 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 U.S. equity index dividend-price ratios over equity returns at a 5-year horizon (see, e.g., Cochrane, 2009; Fama and French, 1988). 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 were 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: (a) to state whether they believe "Variable A" is useful, this round, to predict returns, (b) to give us their forecasts for the next-period "Index Return," and (c) 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 (Afrouzi et al., 2023; Landier, Ma, and Thesmar, 2019). 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 shows that extrapolative biases may not be robust to variations in information and 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 asked 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, that is, 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 overestimate the likelihood of both the upper- and lower-bound 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 analyze 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, for example, 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, for example, only investments, as often observed in the field. We show that such analyses lead to the erroneous interpretation that subjects always underreact 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.

Our paper relates to the existing literature as follows. 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 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 5, 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, for example, 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 reallocations but to differences in access to information and the resultant 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, Enke and Graeber (2023); Frydman and Jin (2022); Woodford (2020). Our findings complement these papers by showing that differences in the source of information can also change the model of decision-making, that is, 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 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. (2024) find that an information treatment where subjects are graphically shown there is no autocorrelation 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, that is, 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 the Enke et al. (2024) large-scale analysis of diminished sensitivities of decisions to information as a result of cognitive information-processing constraints. The Charles, Frydman, and Kilic (2024) framework differs considerably from our investment game and from real investors decisions, both in the actions subjects take and in the information treatments.[1] Their experimental paradigm allows them to measure the impact of complexity on cognitive uncertainty; ours allows us to mimic real investors' decisions when facing different predictive signals and to confront our results to the evidence from the field (e.g., Dahlquist and Ibert, 2024; Giglio et al., 2021a).

1. Experiment

1.1 Design

1.1.1 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 observed, 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), and a yellow dot, which marked the last realization of "Variable A." Subjects were explicitly told that "Variable A" helps predict returns in some rounds, but is useless in others; and that all rounds are independent. We provided subjects with examples of the displays with either predictive or unpredictive "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, for example, on the return process or on how "Variable A" can be used to predict returns, is given in the baseline treatment.

Subjects were 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."[2]

Feedback information is given at the end of each round in terms of whether or not "Variable A" was predictive, this round; what the next-period "Index Return" turned out to be; and how much subjects' investment portfolios made. The time-series display is updated to include the final "Index Return"

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

[2] Subjects provide their answers in the blank "boxes" at the beginning of each round. To keep previous answers from influencing the outcomes of the experiment, we do not have past answers or "by default" numbers, for example, a 50% risk investment, appear one round to the next.

realization, which is represented by a yellow dot, similar to "Variable A."[3] Subjects, endowed with a new 100 ECU, move on to the next round, regardless of the returns realized in previous rounds.

To mirror real investors' market risk, we simulate the "Index Return" time series to mimic the U.S. equity returns averaged over 5-year periods, which is 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 (Campbell and Shiller, 1988; Fama and French, 1988), that is, 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, that is, 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 comovements between "Index Return" and "Variable A," which differ across round types (predictable or not); the key to our experimental treatment.[4]

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 U.S. equity returns (CRSP data, period 1927-1998).[5] The predictive power of "Variable A" in process (2) is measured by $Corr(r_{t+1}^p, a_t) = 57\%$ and $\sigma_p^2 = 0.67\sigma^2$.[6]

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

3 The instruction sheet and examples of the feedback information subjects received can be found in Internet Appendix C.

4 "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 .497.

5 $\mu = 6.07\%$, $\sigma = 9.02\%$; a_t follows an AR(1), with mean μ, persistence $\rho_a = 0.66$, and volatility $\sigma_a = 3.98\%$.

6 We describe our simulation method in Internet Appendix B.

1.1.2 Additional treatments and outcome variables. In addition to the three questions—(1) is "Variable A" informative or not, (2) next-period forecasts, and (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,[7] 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.

1.2 Implementation

In the baseline treatment, we let subjects play for 20 rounds. Ten rounds were simulated with i.i.d. process (1), and 10 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

[7] 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": $\kappa=\frac{1}{5}\frac{1-\rho_a^5}{1-\rho_a}=0.51$.

realization. In addition, they received their full portfolio ECU value from one randomly drawn round of the experiment.[8]

This compensation scheme was designed to incentivize subjects to provide truthful answers on whether or not they viewed “Variable A” as predictive, 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 (Internet 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;[9] the regression coefficients of r_t on a_{t-1} are close to 1 with R^2 close to $R^2 = .33$ of process (2) in the predictable rounds and around 0 (and not significant) in the i.i.d. rounds.[10]

Our experiment was implemented in four waves.

1.2.1 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 20 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 20 rounds where they were told when “Variable A” was useful and when it was not.[11]

The experiments took place in the University’s computer lab on an application we built using the oTree framework (Chen, Schonger, and Wickens,

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

[9] In two outlier i.i.d simulations, r_t has a small but significant *negative* loading on r_{t-1} (p-value = .04, and .06), though it did not appear to affect subjects’ answers.

[10] 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.

[11] Subjects played the same 20 rounds as in the baseline, but in a new randomized order.

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 Internet 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 10 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 10 additional rounds.[12] The third wave was conducted online because of strict COVID-19-related lockdowns. 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.[13]

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

1.2.2 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.[14] Because of the time and effort it takes to complete our experiment (avg. completion time = >1 hr 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

[12] We randomly selected five i.i.d rounds and five predictable rounds from the 20 rounds of the baseline treatment, in each additional treatment.

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

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

subjects' gender, age, income bracket, education and level of financial literacy (see Internet 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 fewer correct "Variable A" answers in the 20 rounds of the baseline treatment 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, that is, remaining subjects can still be incorrect 8 of 20 rounds, 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 of 20, as the TSE Master of finance students of the first three waves, denoting they likely exercised a similar amount of effort.[15]

Our rationale for excluding subjects with 11 or fewer 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 Internet Appendix Tables A.1 and A.2, show that online subjects with 11 or fewer correct "Variable A" answers do not use any available information to form their forecasts. That is, they do not extrapolate from past returns or use the "Variable A" signal; and their own forecasts have no influence on their investments.[16]

Statistics on the remaining Prolific subjects' demographics is provided in Internet 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 answered an average 2.4 of 3 questions about financial literacy, with more than half of subjects answering all three correctly.

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15 We have no reason to believe Master of Finance students have a comparative advantage over the rest of the population at "eyeballing" correlations.

16 Internet Appendix Tables A.1 and A.2 report our results for all online subjects, that is, for the third and fourth waves of the experiment.

2. 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 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. Below, we will present the main results we obtain for the baseline treatment. Descriptive statistics are in Table 1.[17]

2.1 "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 identified returns as predictable 82% of the time, and as unpredictable by "Variable A" 70% of the time (Table 1), which is significantly greater than 50%, if guesses were random (p-value < .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 (Ungeheuer and Weber, 2021; Wunderlich et al., 2011).[18]

Subject have a greater ability to identify information when it is useful rather than useless (82% > 70% with p-value < .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 those from previous studies showing 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). Overinterpreting the "Variable A" information as useful may also reflect optimism bias (here, a tendency to overestimate the signal as informative).

Taking into account how subjects interpret the information in "Variable A," we study their forecasts and risk decisions, in the rest of the paper, in rounds

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17 To be consistent, we also exclude TSE students with strictly fewer than 12 correct answers (8 of 120 students) from our pool of subjects.

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

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.

2.2 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 setup is tailored to analyze what information they use to form their forecasts: past returns, that is, extrapolative forecasts (see the literature review), or other available signals. Accordingly, to analyze forecasts, we run the following regression:

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

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. Table 2 presents the results.

Subjects use both the "Variable A" signal a_t and the past return r_t to form their forecasts (columns 1 and 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 = .71).

A one-percentage-point increase in r_t increases next-period forecasts by 0.18 percentage point in rounds perceived as unpredictable by "Variable A"; a one-percentage point increase in a_t increases next-period forecasts by 0.37 percentage point 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 percentage point in rounds perceived as predictable and 10.7 percentage point otherwise (Table 1), a significant difference (p-value < .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 versus using extrapolation otherwise—is true both between and within subjects.

2.3 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 affect their risk-taking. An increase of one percentage point 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 percentage point 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 whether forecasts are "informed" or not by the extrapolative signal r_t: the difference between 1.43 ECU and 1.56 ECU in columns 3 and 4, Table 4, is not significant (p-value = .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 percentage point change in "informed forecasts" versus 1.85 ECU for "noisy forecasts" (columns 1 and 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 percentage point) in predictable rounds.[19]

2.4 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. Table 5 provides results.

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

[19] 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).

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 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 also 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.

3. Mechanisms

3.1 Interpretation of forecast model

The results of Section 2.2 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., Hirshleifer and Teoh, 2003; Hong, Stein, and Yu, 2007; Kruschke and Johansen, 1999). Using a limited subset of signals, as may be optimal under rational inattention, helps also explain mutual fund managers' decisions (Kacperczyk, van Nieuwerburgh, and Veldkamp, 2016; van Nieuwerburgh and Veldkamp, 2010).

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,"[20] 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:

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

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 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 $\{\bar{\mu}, \lambda_u, \lambda_p, \pi_u, \pi_p\}$.[21]

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 setup.

Second, we set π_u, π_p equal to the true posterior probabilities we observe in the data. That is, we assume that subjects neither 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.

[20] Such extrapolative beliefs can be derived from various psychological mechanisms, including the law of small numbers (as in Bianchi and Jehiel, 2015; 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).

[21] The model is described in detail in Internet Appendix D.1.

Third, we assume subjects have the same extrapolative bias previously observed in the literature when applying the 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 Afrouzi et al. (2023); Landier, Ma, and Thesmar (2019) in an experimental setting comparable to our i.i.d rounds.

Fourth, and finally, we assume subjects rationally update their beliefs from the prior $\bar{\mu} = \mu$ when applying the 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 $\lambda_p = \frac{\pi_p^2 + (1-\pi_u)^2}{\pi_p + (1-\pi_u)}$;[21] such that the model is *fully specified* by setting the 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.

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 $\{\alpha_{i,1}^m, \alpha_{i,2}^m, \beta_{i,1}^m, \beta_{i,2}^m, \delta_{i,1}^m, \delta_{i,2}^m\}$ of Equation (11). We confront their average values and confidence intervals to the corresponding regression coefficients, derived in our data, and we control 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.[22]

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, $\alpha_1^m + \alpha_2^m$ in rounds where "Variable A" is perceived as predictive and α_1^m otherwise (Equation (11)), cannot be rejected in our data.[23] Because those derive from the anchoring on μ, the true unconditional expectation, this result shows that, on average, our subjects do not have an optimism or pessimism bias in their forecasts,

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22 Our test of the model in Table 7 would not reject the alternative $\mathbb{E}_t^u(r_{t+1}) = \lambda_u r_t + (1-\lambda_u)\mu + \widetilde{\mathbb{E}}_t^u(r_{t+1})$ and $\mathbb{E}_t^p(r_{t+1}) = \lambda_p a_t + (1-\lambda_p)\mu + \widetilde{\mathbb{E}}_t^p(r_{t+1})$, as long as $\widetilde{\mathbb{E}}_t^u(r_{t+1})$ and $\widetilde{\mathbb{E}}_t^p(r_{t+1})$ use information orthogonal to a_t and r_t and have a mean of zero. Such models are discussed in Section 4.3.

23 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 (Internet Appendix Table C.2).

whether "Variable A" is perceived predictive or not, contrasting with previous investors' evidence Dominitz and Manski (2007); Giglio et al. (2021a); Hurd and Rohwedder (2012). This result does not exclude that another form of optimism bias may be at play in subjects' overinterpreting "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.

3.2 Interpretation of risk assessments

Before we turn to the analysis of subjects' investments, and the results of Section 2.3, we study and interpret their risk assessments, described in Section 2.4.

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 subjects 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:

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

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 3.1, 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 $\mathbb{E}(Var^p(r_{t+1})) = \sigma_p^2$, 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 20 rounds of the experiment are small (0.18 percentage point standard deviation). Similarly, the correlation between "Variable A" and "Index Return" is stable within predictable rounds (Internet 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; that is, subjects neither overestimate nor underestimate on average their ability to correctly detect when "Variable A" is predictive.

We note that the assumptions we make for parameters $\{\mathbb{E}\left(Var_t^u(r_{t+1})\right), \mathbb{E}(Var^p(r_{t+1})), \pi_u, \pi_p\}$ 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 will 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= .38 for rounds where "Variable A" is perceived as useless and p-value= .84 in rounds where it is perceived as predictive.[24] The average reported CI is 20.7 percentage point across all rounds, almost exactly equal to the true 21.0 percentage point 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 percentage point, is significantly below the true 23.1 percentage point in process (1) (p-value = .02); the reported CI in rounds perceived as predictable by "Variable A," 20.4 percentage point, is significantly above the true 18.9 percentage point in process (2) (p-value = .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.[25] 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 percentage point (1.7 percentage point) above that implied by the model; in rounds where "Variable A" is perceived as useful, they are 9.0 percentage point (4.9 percentage point) above. Such misperceptions can arise under the cognitive uncertainty model of Enke and Graeber (2023), as shown in Enke et al. (2024). As we will discuss below, subjects also display cognitive uncertainty behaviors in their risk decisions, consistent with the interpretation above.

3.3 Interpretation of investment model

To interpret subjects' investments, we take the classical Merton-Samuelson portfolio choice model with normally distributed returns (Merton, 1969) as

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

[25] 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 < .01 and < .01 for the lower bounds, and p-values = .55 and .04 for the upper bounds, in rounds where "Variable A" is perceived as useless or as predictive respectively.

the baseline, and discuss which, if any, extensions 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.

3.3.1 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 2; such that the relative average forecast-to-investment ratios across round types are determined, for each subject, by her relative perceived variances:

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

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

Motivated by our analysis of Section 3.2, 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 $\mathbb{E}(Var^p(r_{t+1})) = \sigma_p^2$; and her average forecast-to-investment ratios across round types. We find that Equation (14) cannot be rejected, at conventional levels (p-value= .59).[26]

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, for example, 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.[27]

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), with unbiased risk assessments). We find a median $\gamma = 24$.[28] 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).[29]

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

[27] Such models are discussed in Internet Appendix D.2.

[28] Specifically, the median γ is 21 and 26, in rounds where "Variable A" is perceived as useless and as predictive, respectively. A few subjects have "extreme" investment decisions, meaning they always invest 0 ECU or 100 ECU. Hence, to account for these outliers, we report the median rather than the average.

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

3.3.2 Elasticity of investments. From the Merton-Samuelson model (Equation (13)), we derive:

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

that is, 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 3.1. Given these assumptions, the Merton-Samuelson model implies:

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

where $\overline{\overline{\mathbb{E}}}$ 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 a mean value of 6.01; the elasticity of investments to "informed forecasts" has a mean value of 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 a mean value of 6.35; the elasticity of investments to "informed forecasts" has a mean value of 3.19 (column 1, Table 4), about twice lower. Subjects' investments are inelastic—they underreact 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 3.1). 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:

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

where $\overline{\overline{\mathbb{E}}}$ 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 $\xi|_{A\ perceived\ useless}=0.74$ and $\xi|_{A\ perceived\ predictive}=0.50$ on average.

Taking the "informed forecasts" $\{\widetilde{F}_{i,k}\}$ (and the model of Section 3.1) as Bayesian updates given signals $\{r_t, a_t\}$, Equation (17) follows Enke and Graeber (2023). However, our finding that cognitive uncertainty affects investments decisions, but not forecasts, even when incentivized, and the resultant internal inconsistency between reported expectations and actions, is new to their analysis;[30] as is the evidence that extrapolative beliefs generate higher cognitive uncertainty.

3.3.3 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 3.2) 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).[31] 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, for example, Barberis and Huang (2008), based on probability distortions that overestimate tail events (Kahneman and Tversky, 1979), also consistent with our subjects' reported beliefs (Section 3.2), while an aversion for negatively skewed wealth profiles

[30] In the experiment of Enke and Graeber (2023), both belief updating and decisions exhibit cognitive uncertainty. Charles, Frydman, and Kilic (2024) find 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.

[31] 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, that is, for ξ_i in Equation (17).

is at the core of the "rare event" literature in asset pricing (e.g., Barro, 2006; Gabaix, 2008).[32]

4. Additional Results

4.1 Variations across subjects

The results in Section 2 are equally valid across and within subjects, suggesting *all* subjects follow similar behaviors; a key finding. We explore what heterogeneity remains in our data.

4.1.1 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: for example, 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 latter may be due to anchoring biases, rather than optimistic versus pessimistic personalities. Our data confirm this interpretation: only 8 (1) 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. In fact, 55 (34) of 169 subjects have prudent (high) risk investments—below the average for a given round—80% of the time. Given their homogeneous forecasts, these results suggest important variations in risk appetites across subjects.

4.1.2 Prolific versus Master of finance subjects. As discussed in Section 1, Prolific subjects are recruited online from a representative pool of the U.S. 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

[32] 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).

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 3.3, 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)).[33]

4.1.3 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.[34] 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 Internet Appendix Tables A.4 to A.12.[35]

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 3.3, 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 our results, using the methodology of Section 3.3 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

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

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

35 Some of the individual characteristics we analyze comove. For example, higher education is 43% correlated with higher income. Internet Appendix Table A.13 provides the correlation matrix.

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 in which magnitudes vary significantly, they do not offset the forecast and investment patterns of Section 2: 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.[36]

4.2 Additional treatments

4.2.1 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 1). 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.

To analyze how this greater "information uncertainty" affects subjects' decisions, we follow an analysis similar to Section 2 and report our results in Internet 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.[37] The sensitivity of investments to forecasts remains positive and

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[36] The additional pass-through from forecasts to investments in rounds perceived as predictable by "Variable A" is not significant within the Prolific subjects subgroups. This is also reflected in the ξ's for rounds perceived as predictable by "Variable A" not being systematically lower for all subgroups, for example, $\xi=0.72$ 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 because of the small sample size (28 subjects per subgroup), since we observe that Prolific subjects otherwise have the same behaviors as TSE students (Table 8).

[37] 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 optimism biases at the long horizon.

significant, but, (1) it is lower than for next-period investments, as evidenced by a change in beliefs of one percentage point 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 = .11).

4.2.2 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 2 and report our results in Internet 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 percentage point change in forecasts (Internet Appendix Table A.15).[38]

Revealing processes (1) and (2) changes the forecast and investment results considerably (Internet 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 noninformative 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) in the baseline treatment.

Taken together, the results we obtain in all three additional treatment are strongly supportive of the model interpretation of Section 3: the more easily interpretable the information in “Variable A,” the more it enters forecasts;[39] the more uncertain subjects are about the information they use to form their beliefs, the less their own forecast variations affect their risk decisions.

4.3 Robustness

We extend the empirical analysis of Section 2 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 Internet 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

[38] 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).

[39] Accordingly, we would expect higher loadings for the forecast results in Internet Appendix Table A.15, in rounds revealed as predictable by “Variable A.” However, these results are estimated with large standard errors, because of the small sample size.

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, that is, $\{a_{t-1,k}, a_{t-2,k}, \dots\}$ 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 vs. column 3 in Internet Appendix Table A.18).[40] We find no evidence that subjects use information from previous rounds, to form their forecasts and choose their investments (Internet 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 percentage point (Internet Appendix Table A.20).

Finally, we analyze and reject that the signals $\{a_t, r_t\}$ may directly contribute to investment variations, that is, affect investment decisions other than via their impact on forecasts: though investments load on a_t (significant in some specifications), it explains less than 0.5% of investment "noise," that is, variations unexplained by forecasts (Internet Appendix Table A.21).

5. 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 2.2); 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 2.3). We will discuss below, first, how crucial the role of the experimental framework is, that is, whether these sets of results could be deduced from real investors' data, and, second, the implications of the mechanisms we document for equilibrium outcomes.

5.1 Understanding data evidence

Most empirical databases on real investors provide only their portfolio allocations (see, e.g. Andries, Bonelli, and Sraer, 2024; Gabaix et al., 2024), not the information they use, and which beliefs they have. Even in the rare cases in which investors' forecasts are observed, as in Giglio et al. (2021a),

[40] 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 vs. column 6 in Internet Appendix Table A.18).

what market information determines said forecasts is unknown. With similar data on our subjects' market decisions, would we be able to understand their behaviors? That is, 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.[41] Let's assume, first, we only know that subjects can observe past returns data. To study their extrapolative bias, similar to, for example, 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 Internet Appendix Table A.22, columns 1–3. We find $\delta=0.12$ ECU per percentage point 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 4.1). 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).[42] We would conclude that our subjects' extrapolative biases are weak and much lower than previous estimates in the literature.

Next, let's assume 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 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 Internet Appendix Table A.22, columns 4–6. We find $\beta=0.86$ ECU per percentage point 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

[41] The assumption that we can observe subjects' investments over the 20 independent rounds of the investment game is already quite strong and not easily comparable to real investors' data.

[42] 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, with points 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.

regression (18) to 47% compared to the 43% obtained with individual fixed effects only.[43] 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).[44] 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 observe subjects' forecasts, but not what information they find useful. That is, 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 and 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 Afrouzi et al. (2023); Landier, Ma, and Thesmar (2019); 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 vs. 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.

5.2 Market implications

As described above, correctly interpreting investment decisions in our experiment—that is, 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 are not readily accessible when analyzing real investors, and directly testing the belief and investment models of Section 3 on their portfolio decisions may not be feasible. This raises the two questions that we will discuss below: (1) How much should we believe

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

[44] The same "back-of-the-envelope" exercise as footnote [44] 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.

our experimental results reflect real investors' decision process? (2) Do the mechanisms we document matter, that is, 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 4). 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* (Internet Appendix Tables A.4 to A.11). Given that all our subject groups behave similarly, we are inclined to believe real investors would too.

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 (column 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. That is, 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").[45] 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

[45] 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, for example, because of regime shifts.

subjects on how to use "Variable A," when useful, increases both investments and market timing further, with up to 41% higher portfolio returns.[46] 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 (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.

6. Conclusion

We have designed 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

[46] 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 percentage point greater returns in predictable rounds, a 13% increase, as calculated under the method of footnote 46.

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 underreact 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

"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)	(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	.15	.15	.02	.15	.16	.03	.16	.16	.18
Individual FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	Yes
Round FE	Yes	Yes	No	No	Yes	No	No	Yes	Yes

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 parentheses. *p <.1; **p <.05; ***p <.01.

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	.13	.55	.58	.14	.56	.59
Individual FE	No	Yes	Yes	No	Yes	Yes
Round FE	No	No	Yes	No	No	Yes

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 parentheses. $^*p<.1$; $^{**}p<.05$; $^{***}p<.01$.

Table 4
Investment and "informed forecasts"

Dep variable	Investment			
	(1) 2SLS	(2) OLS	(3) 2SLS	(4) OLS
Forecast	3.19***	1.85***	1.56***	1.43***
	(0.67)	(0.10)	(0.29)	(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

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 parentheses (computing standard errors clustered at the individual-round level would yield a singular covariance matrix). $^*p$ <.1; $^{**}p$ <.05; $^{***}p$ <.01.

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	.25	.63	.68	.08	.66	.68
Sample	Predict=1			Predict=0		
Individual FE	No	Yes	Yes	No	Yes	Yes
Round FE	No	No	Yes	No	No	Yes

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 parentheses. These results were obtained during wave two of the experiment implementation (TSE lab, January 2020). *p <.1; **p <.05; ***p <.01.

Table 6
Investment and upper/lower-bound probabilities

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	.09	.58	.64	.10	.61	.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	.09	.59	.64	.10	.61	.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	.10	.60	.65	.10	.62	.65
Sample		Predict=1			Predict=0	
Individual FE	No	Yes	Yes	No	Yes	Yes
Round FE	No	No	Yes	No	No	Yes

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 parentheses. These results were obtained during wave four of the experiment implementation (Prolific online, July 2023). $^{*}p$ <.1; $^{**}p$ <.05; $^{***}p$ <.01.

Table 7
Forecast model

	Model	Data	Difference	p-value
	(1)	(2)	(3)	(4)
α_1	3.79	3.83	-0.04	.911
	$[3.50, 4.08]$	$[3.18, 4.48]$	$[-0.75, 0.67]$	
α_2	-1.36	-0.42	-0.94	.067
	$[-1.56, -1.15]$	$[-1.40, 0.56]$	$[-1.94, 0.07]$	
β_1	0.11	0.15	-0.03	.599
		$[0.03, 0.26]$	$[-0.14, 0.08]$	
β_2	0.41	0.30	0.11	.108
		$[0.17, 0.43]$	$[-0.02, 0.24]$	
δ_1	0.26	0.19	0.07	.103
	$[0.21, 0.31]$	$[0.13, 0.26]$	$[-0.01, 0.15]$	
δ_2	-0.18	-0.12	-0.06	.129
	$[-0.22, -0.15]$	$[-0.19, -0.05]$	$[-0.14, 0.02]$	

In column 1, we report the average predicted values according to the forecast model of Section 3.1. The confidence intervals are obtained by plugging the upper-bound and lower-bound values of the extrapolation coefficient estimated by Afrouzi et al. (2023); Landier, Ma, and Thesmar (2019), $\lambda_u = 0.32$ with S.E. 0.03, into the forecast model of Section 3.1. 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 columns 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	.16	.16	.59	.60
Individual FE	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes

This table reports the results of OLS regressions. In columns 1 and 2, the dependent variable is the next-period forecast of returns, in percentage points. In columns 3 and 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 parentheses. *p <.1; **p <.05; ***p <.01.

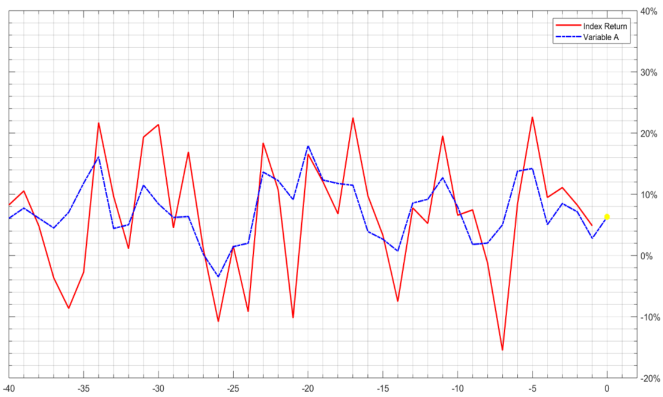
Table 9
Average ξ across subsamples

Subsample	"Variable A" perceived useful	"Variable 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
All	0.50	0.74

This table reports the average of ξ estimated on 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 three financial literacy questions correctly.

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*.

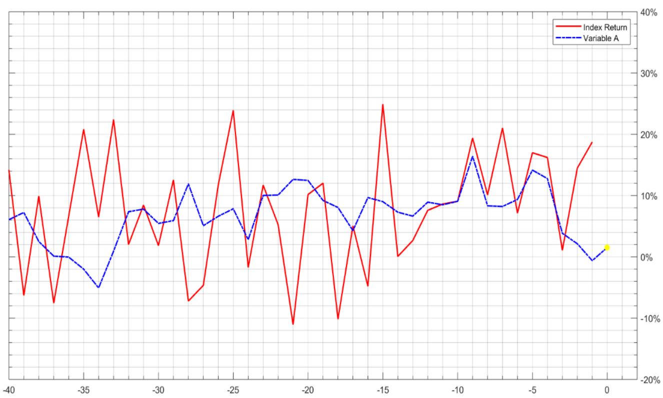


Figure 1
Example page: This page was provided to subjects before they started playing the investment game and provides examples of the two types of rounds, namely, "Variable A" predictive or not.

Code Availability: The replication code is available in the Harvard Dataverse: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/VYPKDJ.

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