Extrapolators and Contrarians: Forecast Bias and Household Stock Trading*

Steffen Andersen
Danmarks Nationalbank, Copenhagen Business School, and CEPR
san.fi@cbs.dk

Stephen G. Dimmock
National University of Singapore
dimmock@nus.edu.sg

Kasper Meisner Nielsen
Copenhagen Business School
kmn.fi@cbs.dk

Kim Peijnenburg
EDHEC Business School and CEPR
kim.peijnenburg@edhec.edu

February 2024

Abstract

We test whether forecast bias affects household stock trading by combining measures of bias elicited in laboratory experiments with administrative trade-level data. On average, subjects exhibit positive forecast bias (i.e., extrapolators), while a large minority exhibit negative forecast bias (i.e., contrarians). Forecast bias is positively associated with past excess returns of stocks that are purchased: Extrapolators (contrarians) purchase past winners (losers). Forecast bias is negatively associated with the capital gains of stocks that are sold. Furthermore, forecast bias explains investor heterogeneity in the relation between market returns and net flows to stocks. Overall, our study provides evidence of a common mechanism – forecast bias – that links past returns to trading decisions for purchases, sales, and net flows.

JEL Classifications: G5, G11, G41, D84, D81

Keywords: Extrapolation, Contrarian bias, Forecast bias, Expectations, Household finance, Experimental finance, Household trading

---

* We thank Claes Bäckman, Milo Bianchi, Pedro Bordalo, Patrick Coen, Zhi Da, Carey Deck, Andreas Fuster, Nicola Gennaioli, Tobin Hanspal, Lawrence Jin, Augustin Landier, Steffen Meyer, Gianpaolo Parise, Sugata Ray, Mirco Rubin, Mark Schneider, Michael Shin, Paolo Sodini, Theresa Spickers, David Thesmar, Raman Uppal, Zhiqiang Ye, Terry Zhang, and conference/seminar participants at Aarhus University, Academy of Behavioral Finance and Economics, Australia National University Summer Camp, Bank of England Workshop, Copenhagen Business School, CEPR Household Finance Conference, ESSEC Business School, Frankfurt School of Management, Helsinki Finance Summit, IESEG Business School, London Business School, Tilburg University, Toulouse School of Economics, University of Alabama, University of Geneva, University of Hawaii, University of Zurich, and the Workshop on Micro Data Meet Macro Models for helpful comments and suggestions. We also thank Akshaya Muthu and Phoon Ruei for excellent research assistance. Andersen and Peijnenburg gratefully acknowledge support from the Center for Big Data in Finance (Grant no. DNRF167). Andersen gratefully acknowledges support from the European Research Council through ERC grant "RDRECON" (ID:639383). Dimmock gratefully acknowledges financial support from the Singapore Ministry of Education research grant A-0003873-00-00. Nielsen thanks the Danish Finance Institute for financial support. The views expressed herein are those of the authors, and do not necessarily reflect the official views of Danmarks Nationalbank.

Expectations play a key role in both behavioral and rational models of investment decisions. A growing literature uses surveys to measure investors' expectations of stock market returns, and finds that recent past returns strongly affect investors, resulting in biased expectations. This literature also documents substantial across-investor heterogeneity in how past returns affect expectations.[1] Numerous studies further show that investors' expectations of market returns predict their allocations to risky asset classes.[2]

A natural question that follows from this literature on return expectations and allocations to asset classes is whether biases in investors' expectations affect their selection of individual securities within an asset class. To address this question, we elicit individual-level measures of forecast bias in a laboratory experiment and link it to administrative records of our subjects' stock trading decisions. We then test how forecast bias affects stock selection for purchases and sales decisions, as well as for net flows into stocks.

We invite a representative sample of investors to participate in a laboratory experiment designed to elicit their forecast biases. Our experiment closely follows Afrouzi et al. (2023), and asks subjects to forecast a stochastic process. The participants are eligible to win monetary prizes based on their forecast accuracy. At the beginning of the experiment, each subject observes 40 realizations of the process and is then asked to forecast the next realization. After making the forecast, the participant observes the next realization of the process and is asked to

[1] De Bondt (1993), Fisher and Statman (2000), Greenwood and Shleifer (2014), and Adam, Matveev, and Nagel (2021) show that investors' stock market return expectations are strongly related to past returns, and argue that investors' update their beliefs in a biased manner. Dominitz and Manski (2011), Heiss et al. (2022), von Gaudecker and Wogrolly (2022), and Atmaz, Cassella, Gulen, and Ruan (2023) document large heterogeneity across investors' in how past returns affect stock market expectations.

[2] Vissing-Jorgensen (2003), Dominitz and Manski (2007), Malmendier and Nagel (2011), Amromin and Sharpe (2014), Merkle and Weber (2014), Hoffman, Post, and Pennings (2015), Giglio, Maggiori, Stroebel, and Utkus (2021), Beutel and Weber (2022), and Laudenbach, Weber, Weber, and Wohlfart (2023) show that investors' stock market expectations predict asset allocation decisions.

forecast the next realization. This continues for a total of 40 rounds. Using these forecasts, we construct an individual-level measure of bias in belief formation, *Forecast Bias*.

Our aim is to elicit a general measure of our subjects' forecast bias and relate it to their individual stock trading decisions. Conceptually, variation in forecasts can arise from across-subject variation in information or from across-subject variation in information processing. Our laboratory experiment allows us to control and standardize the information available to the subject, and measure across-subject variation in how they process information. The advantage of measuring information processing, rather than information itself, is that it provides a single parameter that is widely applicable across different securities, time periods, and types of decisions.

Our estimates of forecast bias are consistent with those found in earlier studies (e.g., Dominitz and Manski, 2011; Afrouzi et al., 2023). On average, people exhibit extrapolation bias: forecasts are too high (low) following high (low) realizations. There is, however, substantial individual heterogeneity. Although a small majority exhibit extrapolation bias, a sizeable minority exhibit contrarian bias: forecasts are too low (high) following high (low) realizations.

We link the experimental results with 11 years of administrative register data on stock trading, income, wealth, and demographics from before and after the experiment. The trade-level data covers 2011-2021 and comprises all trades of every Danish resident, including our subject pool, matched with detailed information on income, financial assets, housing assets, education, and other demographic variables. In total, our sample includes around 29,000 purchases and 21,000 sales worth 3.4 billion Danish kroner (€442 million) for the 959 subjects in our experiment.

The combination of a laboratory experiment with administrative data offers several advantages. Our lab experiment allows us to cleanly measure the forecast bias parameter, while controlling the underlying data-generating process and the information available to the subject. In addition, we elicit control variables not available in the administrative data. The administrative data provide us with a representative sample of investors and with complete and accurate records of their trading and holdings.

Theory shows that forecast bias increases sensitivity to recent past returns when forming expectations of future returns (Barberis, Greenwood, Jin, and Shleifer, 2015, 2018). In particular, extrapolation bias makes stocks that recently performed well more attractive while contrarian bias makes stocks that recently performed poorly more attractive. Thus, theory predicts that higher extrapolation (contrarian) bias results in buying stocks with high (low) past returns. Following similar logic, higher extrapolation (contrarian) bias results in selling stocks with low (high) past returns.

Using our subject-specific measure, *Forecast Bias*, we show that individuals' biases are related to the past performance of the stocks they purchase; extrapolators tend to buy high performers and contrarians tend to buy poor performers. A one-standard deviation increase in *Forecast Bias* implies purchasing stocks with a 3.0 percentage point higher past one-year excess return relative to stocks purchased by other investors in the same period.

Next, we examine whether *Forecast Bias* affects sales decisions. The results show that *Forecast Bias* is negatively associated with the capital gains of stocks that are sold. Comparing across the stocks currently held in the portfolio, extrapolators tend to sell stocks with relatively low capital gains and contrarians tend to sell stocks with relatively high capital gains. A one standard deviation increase in *Forecast Bias* implies a 7.1 percentage point reduction in the probability that a subject sells their highest performing stock instead of their lowest.

We show that the relations between forecast bias and trading decisions are robust in multiple ways. First, we include numerous control variables that previous studies suggest could affect household trading decisions, including age, gender, marital status, education, income, wealth, risk aversion, trust, and financial literacy. We show that including these control variables has almost no effect on the coefficient estimate of *Forecast Bias*, suggesting its effect is unrelated to factors identified in prior studies. Second, we show that the results are robust to alternative forecast bias measures, including diagnostic expectations (Gennaioli and Shleifer, 2010; Bordalo, Coffman, Gennaioli and Shleifer, 2016; Bordalo, Gennaioli, La Porta, and Shleifer, 2019), sticky expectations (Woodford, 2003), extrapolative expectations (Metzler, 1941), and adaptive expectations (Cagan, 1956).

We further show that the relation between *Forecast Bias* and the past returns of purchased stocks is not driven by an inability to reason or by low financial sophistication. We restrict the sample to include only investors with at least four years of post-secondary education, and find results nearly identical to those in the main sample. Similarly, the results remain quantitively similar if we restrict the sample to exclude investors with low financial literacy or to exclude investors who cannot correctly answer numerical problems.

While our main set of results test how forecast bias relates to cross-sectional security selection decisions, we also examine how forecast bias relates to net flows to stocks. We find that investors with higher forecast bias increase (decrease) their allocations to stocks following positive (negative) market returns over the past year. We do not find that forecast bias affects the relation between investors' own excess past returns and their net flows to stocks.

When interpreting our findings, we implicitly treat *Forecast Bias* as a stable personal trait that governs how individuals process information to produce forecasts, and is thus distinct from the subjects' experiences and information. To test this, we create three categories of variables measuring stock market experiences: the investor's own portfolio returns, stock market crash experiences, and trading experience. We find no evidence that past experiences explain our subjects' forecast biases. Our finding indicates intra-person stability in forecast bias, consistent with Dominitz and Manski (2011) who find that the expectation formation process is an intra-personally stable trait. This finding is also consistent with our interpretation of *Forecast Bias* as capturing how subjects process past return information when they form expectations, and distinct from measures of the past returns a subject has experienced as in Kaustia and Knüpfer (2008) and Malmendier and Nagel (2011).

Next, we test whether forecast bias is related to our subjects' investment performance. On the one hand, given that *Forecast Bias* is a deviation from a clearly defined statistical benchmark, we might expect it to be associated with underperformance. On the other hand, there is empirical evidence that past returns have some cross-sectional predictive power (e.g., De Bondt and Thaler, 1985; Jegadeesh and Titman, 1993). Thus, it is possible that the relation between *Forecast Bias* and trading based on past returns could result in higher returns. Empirically, we find little evidence that *Forecast Bias* is related to investor performance.

Our main finding, that individual heterogeneity in forecast bias is significantly related to investors' security selection, is related to a growing literature on household stock market expectations.[3] Several studies examine how past returns or expectations of aggregate market returns relate to investors' allocations to risky asset classes (e.g., see Vissing-Jorgensen, 2003;

[3] Prior studies use survey data and show that investors' stated expectations of aggregate market returns are related to past returns. De Bondt (1993), Fisher and Statman (2000), Vissing-Jorgensen (2003), Malmendier and Nagel (2011), Amromin and Sharpe (2014), and Greenwood and Shleifer (2014) show a positive relation between past returns and the average expectation of market returns. Dominitz and Manski (2011), Heiss et al. (2022), von Gaudecker and Wogrolly (2022), and Laudenbach, Weber, Weber, and Wohlfart (2023) show there is significant, but stable, heterogeneity in how households incorporate past returns into expectations of overall market returns. Da, Huang, and Jin (2021) show that an individual stock's past return is related to expectations about its future return. Andries, Bianchi, Huynh, and Pouget (2022) study how signal precision affects forecast bias in an experimental asset market.

Malmendier and Nagel, 2011; Giglio, Maggiori, Stroebel, and Utkus, 2021).[4] Closest to our work is Laudenbach, Weber, Weber, and Wohlfart (2023) who show that beliefs about the historical autocorrelation of aggregate stock market returns relate to flows to the stock market during the COVID-19 crash. Our work differs from the prior literature in that we study how forecast bias relates to individual stock selection decisions.

Our study contributes to the literature on how past returns affect individual investor decisions. Prior studies show that different types of investment decisions are affected by different types of returns. The decision to purchase a stock is linked with that stock's past return (Grinblatt and Keloharju, 2000; Barber and Odean, 2008). The decision to sell a stock is linked to the investor's capital gain on that stock (Odean, 1998; Ben-David and Hirshleifer, 2012; Hartzmark, 2015). Decisions about net flows to stocks are linked to past market returns (Greenwood and Shleifer, 2014). Our study shows that a common mechanism – forecast bias – consistently affects these relations, and provides evidence unifying these results across different types of investment decisions.

Our study also contributes to the literature on the security selection decisions of individual investors. Prior studies show numerous behavioral biases affect security selection.[5] More directly related to our work is Kaustia and Knüpfer (2008) who show that personally experienced IPO returns affect future demand for IPO shares through reinforcement learning, which suggests beliefs affect IPO purchase decisions. We add to the literature on behavioral

[4] Liu, Peng, Xiong, and Xiong (2022) test whether extrapolation beliefs are related to portfolio turnover, and do not find a relation. In the online appendix, Liu, Peng, Xiong, Xiong (2022) examine the relation between extrapolation beliefs are related to past returns, but their tests pool market timing and cross-sectional security selection decisions.

[5] The behavioral factors include attention grabbing events (Barber and Odean, 2008), social networks (Ivković and Weisbenner, 2007; Hvide and Östberg, 2015; Knüpfer, Rantapuska, and Sarvimäki, 2023), lottery and gambling preferences (Kumar, 2009), IQ (Grinblatt, Keloharju, and Linnainmaa, 2012), psychological distance (Bhamra, Uppal, and Walden, 2022), rank effects (Hartzmark, 2015), and dividend-paying preferences (Hartzmark and Solomon, 2019), among other factors.

biases in individual security trading by showing that forecast bias in belief formation is important for explaining the heterogeneity in stock purchase, sale, and net flow decisions among individual investors.

Our study informs work in asset pricing on extrapolation and contrarian biases. An extensive literature in asset pricing establishes stylized facts about stock returns and posits that these can be attributed to forecast bias.[6] Our study complements these studies by showing a direct empirical relation between individual-level elicited biases in expectation formation and the individual's stock selection decisions.

1. Eliciting Individuals' Forecast Bias

We conduct a laboratory experiment to measure our subjects' forecast bias. The experiment is designed to capture biases in how the subjects process information when forming expectations. This differs from much of the related literature, which uses survey measures of subjects' expectations of stock market returns to study investor decisions at the asset class level (e.g., see, among others, Vissing-Jorgensen, 2003; Greenwood and Shleifer, 2014; Giglio, Maggiori, Stroebel, and Utkus, 2021). Our approach allows us to study investor decisions at a more granular level – security selection within an asset class – because it does not necessitate measuring a time-series of each investor's expectation for every stock (a prohibitively difficult task). Instead, by combining a single measure of forecast bias with past stock returns, we can examine investor decisions over time for a vast number of individual securities.

[6] For instance, the literature documents short-term momentum (Jegadeesh and Titman, 1993) and long-term reversal (De Bondt and Thaler, 1985; Lakonishok, Shleifer, and Vishny, 1994), which the authors attribute to investors' forecast biases. Similarly, several models explain cross-sectional return patterns by assuming investors suffer from forecast biases (e.g., Barberis, Shleifer, and Vishny, 1998; Hong and Stein, 1999; Bordalo, Gennaioli, La Porta, and Shleifer, 2019; Cassella, Chen, Gulen, and Petkova, 2022; Atmaz, Cassella, Gulen, and Ruan, 2023). See Barberis (2018) and Adam and Nagel (2022) for reviews of the literature on expectations and asset pricing.

1.1 The Elicitation Procedure

We develop an experimental module that includes a task to elicit individuals' forecast bias.[7] The task closely follows Afrouzi et al. (2023). In the forecast task, the subjects observe past values of an investment and then make forecasts about the future value.

The underlying data-generating function for the value of the investment is a first-order autoregressive (AR(1)) process with the first value set to 100:

$$x_{t+1} = 100 + 0.5 \cdot (x_t - 100) + \varepsilon_t \,. \tag{1}$$

The AR(1) coefficient is set to 0.5, the mean to 100, and the error term is drawn from a normal distribution with a standard deviation of 25.[8] Afrouzi et al. (2023) validate this method with a series of experiments, and show that forecast biases are similar with different parameter choices for the mean and standard deviation of the process, and across subject pools with different levels of sophistication (MIT students versus MTurk participants). In related work, Landier, Ma, and Thesmar (2019) show that forecast biases do not depend on the labelling of the process: results are similar when subjects are asked to forecast a stable random process, GDP growth, CPI, stock returns, or house price growth.

In our forecast task, the data-generating process, an AR(1) with a coefficient of 0.5, is not calibrated to stock prices and the forecasting horizon is generic, as we want to capture a general measure of forecast bias. Our goal is to test if interpersonal variation in forecast bias relates to trading decisions. Our tests thus assume that the interpersonal ranking between subjects' forecast bias is stable across different autocorrelations – to the extent this assumption is incorrect it will reduce the power of our tests and bias against finding significant results.

[7] The complete instructions of the forecasting experiment are in Online Appendix A.

[8] Afrouzi et al. (2023) experiment with different values of the AR(1) coefficient in the range from 0 to 1 with 0.2 increments and find that overreaction is stronger for less persistent processes. We choose a single value for the AR(1) coefficient to ensure all responses are comparable.

To begin, the subjects see 40 past realizations, and submit one- and two-period-ahead forecasts. Figure 1 provides a screenshot of the forecasting task.[9] The top panel shows the first 40 realizations as well as two "**x**'s", one blue and one orange, to indicate forecasts one period and two periods ahead, respectively. The subjects submit their forecasts for the next two periods by sliding the "**x**'s" up or down to their desired value and clicking "Make forecast." Once the subject clicks "Make forecast," they observe the next realization, and are asked to make two new forecasts, as seen in the bottom panel of Figure 1. This step is repeated until each subject has submitted 40 rounds of forecasts. On average, the subjects take 9 minutes and 47 seconds to make the 40 rounds of forecasts, equivalent to four forecasts per minute, with only 27 (8) out of 959 subjects taking less than five (more than 20) minutes.

To incentivize the subjects, in addition to the show-up fee, each subject has a 10% chance of being eligible to receive an incentive payment based on the accuracy of their forecasts. Each subject rolls a 10-sided dice to determine if they are eligible for the incentive payment, and if so, they roll a 4-sided and a 10-sided dice to randomly determine which of their 40 forecasts is selected to calculate their forecast accuracy. To ensure incentive compatibility, we follow Hossain and Okui (2013) by letting the forecast accuracy affect the probability of winning a prize and not the amount of the prize. Thus, for the selected forecast, the subject's probability of winning a prize is determined as: $100 - 5 \times \left|forecast_{i,t} - realization_{i,t}\right|$. If the forecast differs from the realized value by more than 20 in absolute terms, the probability of winning the prize is set to zero. The subject then rolls two 10-sided dice, and if the value from the roll is smaller than the winning probability, the subject receives 2,000 DKK (approximately €260).[10] Based on this procedure, 17 subjects received a prize from the forecasting task.

[9] The actual experiment is conducted in Danish. Figure 1 presents an English translation of an experiment screen shot. Throughout this study, we report English translations of experiment questions and instructions.

[10] In January 2023, one kroner is worth U.S. $0.14 and €0.13.

1.2 Measures of Forecast Bias

Using the forecasts elicited from the experiment, we construct our main measure of forecast bias at the individual level. We follow Afrouzi et al. (2023) and estimate the forecast bias that is implied by each subject's predictions using the following regression:

$$F_{i,t}(x_{i,t+1}) - E_{i,t}(x_{i,t+1}) = a_i + b_i \cdot (x_{i,t} - \bar{x}) + \varepsilon_{i,t} \tag{2}$$

where $F_{i,t}(x_{i,t+1})$ indicates subject i's forecast of next period's realization $x_{i,t+1}$ and $E_{i,t}(x_{i,t+1})$ is the rational forecast. Thus, the left-hand side of equation (2) is the subject's forecast error. The parameter b_i measures forecast bias.[11] A value of $b_i > 0$ indicates extrapolation bias: forecasts are too high (low) following high (low) realizations. A value of $b_i < 0$ indicates contrarian bias: forecasts are too low (high) following high (low) realizations.

In the experiment, each subject observes a unique, randomly determined series of realizations. By random chance, some subjects observe a time-series that appears to have higher or lower persistence than 0.5, a mean different from 100, or a standard deviation of the error term different from 25. Accordingly, we construct two alternative measures of forecast bias that account for the unique path of realizations observed by each subject. *Forecast Bias Residual* is the residual from regressing *Forecast Bias* on the standard deviation of realizations and person-specific empirical persistence parameter in the full set of 80 realizations. A second alternative measure, *Forecast Bias Limited Information*, incorporates that the rational forecast changes after each realization. At any given point in time, the within-sample least-squares estimate of the rational forecast given all prior realizations of the process is given by:

$$\tilde{E}_{i,t}(x_{i,t+1}) = \bar{x}_{i,(0,t)} + \hat{\vartheta}_{i,(0,t)} \cdot [x_{i,t} - \bar{x}_{i,(0,t)}] \tag{3}$$

[11] Due to the small sample of forecasts, the OLS estimator of the persistence parameter of the AR(1) process is biased. The Kendall approximation that corrects for this bias is $b_i + \frac{1+3\vartheta}{T}$, which implies a bias of 0.06 for an AR(1) parameter of $\vartheta = 0.5$ and 40 forecasts. Our *Forecast Bias* measure thus underestimates the tendency to extrapolate. Note that the bias is consistent across subjects, and so does not affect our cross-sectional tests.

where $\bar{x}_{i,(0,t)}$ is the mean of the process from period 0 through t and $\hat{\vartheta}_{i,(0,t)}$ is the within-sample AR(1) parameter estimate using all realizations from period 0 through t. We then estimate the limited information forecast bias as:

$$F_{i,t}(x_{i,t+1}) - \tilde{E}_{i,t}(x_{i,t+1}) = a_i + b_i \cdot (x_{i,t} - \bar{x}_{i,(0,t)}) + \varepsilon_{i,t} \tag{4}$$

where $b_i > 0$ indicates extrapolation bias and $b_i < 0$ indicates contrarian bias.

Our third alternative measure is *Forecast Bias Rank*, which is the rank transformation of *Forecast Bias*. Zero indicates the lowest level of *Forecast Bias* and one the highest. This measure ensures that our results are not driven by outliers.

We consider four additional alternative measures of forecast bias. Two define forecast bias relative to the forward-looking rational benchmark: *Diagnostic Expectations* (Bordalo, Gennaioli, and Shleifer, 2018; Bordalo, Gennaioli, La Porta, and Shleifer, 2019) and *Sticky Expectations* (Woodford, 2003). We multiply *Sticky Expectations* by -1 so that it is directionally consistent with the other measures. The final two measures are backward-looking and do not incorporate features of the true data-generating process: *Extrapolative Expectations* (Metzler, 1941) and *Adaptive Expectations* (Cagan, 1956).[12]

2. Data and Variables

Access to the data used in this study is provided by Statistics Denmark, the government agency with central authority for Danish statistics. We use the research infrastructure at Statistics Denmark to recruit subjects based on administrative register data and to conduct our laboratory experiment, as described in detail later in this section. Statistics Denmark provides demographic, economic, and financial data, including stock holdings as well as trading records

[12] See Section IV.B of Afrouzi et al. (2023) for the exact specifications.

reported by banks and brokerage firms to the Danish Tax Authorities. The administrative registers are comprehensive and cover the entire Danish population.

2.1 Sample Selection and Lab Experiment

The starting point of our analysis is to recruit subjects for our experimental tasks. Statistics Denmark recruits the subjects using the following criteria provided by the authors. The initial population includes all of the 5,806,081 individuals residing in Denmark as of January 1, 2019. We then restrict the pool of eligible subjects in four steps. First, we exclude all individuals younger than 30 or older than 60, to remove students and retirees. Second, we exclude all individuals who do not reside within a 45-minute drive of the Statistics Denmark office in Copenhagen where the experiments are conducted. Third, we exclude individuals who are not homeowners for at least two years between 2014 to 2018. Finally, we exclude individuals who do not own at least 10,000 DKK in risky assets (stocks and mutual funds) in at least three of the years between 2014 and 2018.[13] After applying these criteria, the pool of eligible subjects contains 75,847 individuals. From the pool of eligible subjects, Statistics Denmark randomly invite 24,821 individuals to participate in our study.

Following Andersen, Hanspal, Martínez-Correa, and Nielsen (2021), we induce exogenous variation in the likelihood that subjects accept the invitation by randomly offering half of the subjects a 10% chance of winning a show-up fee of 1,000 DKK (approximately €130), while the other half has a 10% chance of winning 2,000 DKK (approximately €260). In robustness tests, we use this exogenous variation in incentives as an instrument to address potential concerns about sample selection bias.

[13] We exclude own company stock from this measure of risky assets.

In total, 959 subjects accept the invitation and participate in the experiment (3.9% participation rate). Online Appendix Table D.1 compares experiment participants and non-participants in terms of their demographic and economic characteristics, as well as their trading behaviors. Although some of the variables are significantly different, the magnitudes of the differences are small. Participants are slightly older (50.5 versus 49.5 years old), more educated (16.5 versus 16.0 years of education), more likely to be male (69% versus 56%), and less likely to be married (64% versus 69%) or have children (81% versus 86%). The differences are not significant for financial assets, or housing wealth. Furthermore, participants and non-participants have no consistent difference in trading behavior based on past returns, but participants do trade more frequently. Robustness tests discussed in Section 3.1 address potential selection bias and show it is unlikely to affect our results.

The experiment was conducted in-person, in sessions of around 15 subjects, which took place at Statistics Denmark in Copenhagen. We conducted two sessions per day on 21 of the days between February 5, 2020 and March 11, 2020, at which time the experiment was suspended to comply with Covid protocols. The experiment was later resumed with an additional 12 days of two sessions per day between November 9 and 26, 2020.[14]

2.2 Measures of Forecast Bias

Panel A of Table 1 summarizes the measures of forecast bias for our subjects. We report means and quasi-medians defined as the average value for the 45th through 55th percentiles. We report quasi-medians instead of medians, because our data agreement with Statistics Denmark prohibits reporting any statistics that are not based on at least 10 observations. All forecast bias measures except for *Forecast Bias Rank* are re-scaled to have a standard deviation of one to

[14] The average *Forecast Bias* is not significantly different pre- and post-Covid. Furthermore, we include a post-Covid indicator as a control in our analyses.

facilitate the interpretation of our regression results. Negative values of forecast bias imply contrarian bias relative to the data generating process: forecasts are too high (low) following low (high) realizations. Positive values imply extrapolation bias: forecasts are too high (low) following high (low) realizations. On average, subjects are extrapolators. The mean (quasi-median) of the *Forecast Bias* parameter is 0.14 (0.28), which is significantly greater than the benchmark of zero (p-value < 0.0001).

The histogram in Figure 2 shows the distribution of *Forecast Bias*. Observations at zero indicate no bias, while observations to the left and right of zero indicate progressively greater contrarian bias and extrapolation bias, respectively. The figure shows that a small majority exhibit extrapolation bias, but there is substantial heterogeneity and a large minority exhibit contrarian bias. The finding of heterogeneity in forecast bias that includes both extrapolators and contrarians is consistent with prior empirical studies such as Dominitz and Manski (2011), Heiss et al. (2022), von Gaudecker and Wogrolly (2022), and Laudenbach, Weber, Weber, and Wohlfart (2023).

Panel A of Table 1 also reports summary statistics of the alternative measures of forecast bias: *Forecast Bias Residual*, *Forecast Bias Limited Info*, *Forecast Bias Rank, Diagnostic Expectations*, *Sticky Expectations, Extrapolative Expectations*, and *Adaptative Expectations*. All measures are all highly correlated (see Online Appendix Table D.2). In Section 3.3, we show that our results are robust to using these alternative forecast bias measures.

As a simple check of reliability, we calculate two alternative measures identical to *Forecast Bias* except the first is calculated using only the odd numbered periods and the second uses only the even numbered forecasts (thus we have two measures per subject, each based on only 20 non-overlapping observations). The Cronbach (1951) alpha between these two variables is 0.89, which is substantially higher than the standard cutoff of 0.7 suggesting the subjects' responses have high internal consistency.

2.3 Trading and Portfolio Data

We combine data from several administrative registers made available to us through Statistics Denmark. Data on income, wealth, and investments come from the official records of the Danish Tax and Customs Administration (SKAT) for the years 2011 to 2021, and are comparable to the data from other Nordic countries.[15] Danish tax law requires third parties to report information on income, wealth, and trading directly to SKAT. For example, banks and brokerages report investment holdings and trades at the individual level. Thus, our trading data are reported directly from administrative sources and are not self-reported by individuals. The data contain information on individuals' stock holdings by ISIN number at the end of the year as well as daily records of all stock transactions. We supplement this information with demographics from the Civil Registration System and educational records from the Ministry of Education. We match the data at the individual level using the civil registration number (CPR), which is the Danish equivalent of the social security number in the United States.

A total of 680 of the 959 (71 percent) participants in the experiment purchase at least one stock during 2011 through 2021 and a total of 583 of the 959 (61 percent) individuals sell at least one stock during 2011 through 2021. Panel B of Table 1 summarizes the purchases and sales. We first average across trades for an individual and then report averages across individuals. On average, subjects make 43.0 purchases and 36.1 sales, for a total of 50,298 unique trades.[16] The distribution of trading activity is highly skewed, with the 52 most active

[15] For example, Grinblatt and Keloharju (2000), Kaustia and Knüpfer (2008), and Knüpfer, Rantapuska, and Sarvimäki (2017) study data from Finland; Hvide and Östberg (2015) and Fagereng, Guiso, Malacrino, and Pistaferri (2020) study data from Norway; and Calvet, Campbell, and Sodini (2007, 2009) study data from Sweden.

[16] We aggregate trades in the same stock within a day to get unique person-stock-day purchases. Our sales variable includes both partial sales and full divestment of a position.

traders making about half of total trades. The average purchase has a value of 42,525 Danish kroner (€5,528) and the average sale is 60,417 kroner (€7,854). In aggregate, the value of trades in our sample is slightly greater than 3.4 billion kroner (€442 million).

We supplement the administrative data with return data from Refinitiv and Compustat Global. The Refinitiv data are matched using ISIN codes. The Compustat data are matched using the GVKEY to ISIN mapping files provided by Capital IQ. For benchmark returns, we use the WRDS World Index for Denmark.

For each security, we calculate its excess returns as the simple difference between the actual return and the benchmark return over the prior year, as Ben-David, Li, Rossi, and Song (2022) find a simple market adjustment best explains investors' cross-sectional allocations. We calculate returns using daily data for the year ending the day before a purchase or sale. The average lagged annual excess return for purchases is 2.6%, but excess returns are positively skewed and the quasi-median is -3.3% (for comparison, the average lagged annual return for the Danish stock market is 18.6% for our sample). The average lagged annual excess return for sales is 11.6%, and the quasi-median is 5.5%. We also calculate the capital gain since purchase for the stocks in our sample.[17] The average capital gain is 31.9% and the quasi-median is 13.5%.

2.4 Control Variables

In the analysis we include control variables for age, gender, marital status, parental status, years of education, income, housing assets, and financial assets. Following Dimmock, Kouwenberg, Mitchell, and Peijnenburg (2016, 2021), we supplement these with additional variables from our experiment, which measure risk aversion, financial literacy, numeracy,

[17] For individuals who make multiple purchases of stocks over time, we use the weighted average capital gain per share. For partial sales when the subject has purchased shares in multiple tranches at different prices, we assume the subject sells from each tranche on a pro rata basis.

optimism, overconfidence, and trust.[18] We also include a *Post-Covid experiment* indicator variable for subjects whose session occurred in November after the Covid protocols were relaxed. See Appendix Table A1 for variable definitions. Panel C of Table 1 reports summary statistics for the control variables.

The forecast bias elicitation experiment is designed so that risk aversion should not affect forecasts (see Hossain and Okui, 2013). Nevertheless, our experiment includes a task to measure risk aversion. Following Wakker, Erev, and Weber (1994), Andersen, Harrison, Lau, and Rutström (2008), and Andersen, Hanspal, Martinez-Correa, and Nielsen (2021), the subjects choose between 40 lottery pairs. Subjects choose between two lotteries, where one lottery is safer (see Online Appendix B). The measure of risk aversion is the fraction of lottery pairs in which the subject chose the safer option.

Prior studies show that financial literacy is strongly associated with financial decisions (Lusardi and Mitchell, 2007, 2011, 2014; van Rooij, Lusardi, and Alessie, 2011). To ensure that forecast bias is not a proxy for low financial literacy, we include the number of correct responses to four financial literacy questions from Lusardi and Mitchell (2007). Numeracy is the number of correct responses to three numerical problems from the Health and Retirement Survey, and serves as a measure of the subjects' quantitative abilities.

Prior studies find that optimism and overconfidence relate to financial decisions (Puri and Robinson, 2007; Grinblatt and Keloharju, 2009). In our setting, optimism and overconfidence could cause subjects to have biased expectations about the mean of the stochastic process. But optimism and overconfidence are unlikely to affect our measure, because *Forecast Bias* does not capture a persistent upward or downward bias. Rather, our measure captures the directional response in forecasts to recent realizations of values (e.g.,

[18] Online Appendix B provides the exact wording of these questions.

forecasts that are consistently too high following high realizations but also too low following low realizations). Nevertheless, we control for optimism and overconfidence in our regressions. For overconfidence, we follow Moore and Healy (2008), and for optimism, we follow Puri and Robinson (2007). Finally, we control for trust as Guiso, Sapienza, and Zingales (2008) report a relation between trust and portfolio choice.

3. Forecast Bias and Stock Selection

3.1 Forecast Bias and Stock Purchases

We hypothesize that *Forecast Bias* affects how investors react to stock performance. In particular, extrapolators purchase stocks that recently performed well and contrarians purchase stocks that performed poorly. But investors must be aware of a stock's performance for it to affect trading, and evidence shows that investor attention is limited (e.g., Barber and Odean, 2008). In our data, we do not observe whether and when an investor becomes aware of the performance of a particular stock. Accordingly, in our baseline specification we include observations conditional on a purchase occurring, as this indicates that the investor has paid attention to that specific stock (Section 4 considers the issue of whether to trade). Further, we analyze the lagged annual returns of stock purchases; a period long enough to capture information events even for inattentive investors.

We estimate the following specification:

$$Prior\ annual\ excess\ return_{i,j,t} = \beta_1 \cdot Forecast\ Bias_i + \boldsymbol{\delta X_{i,t}} + \theta_t + \varepsilon_{i,t} \qquad (5)$$

where $Prior\ annual\ excess\ return_{i,j,t}$ is the lagged annual excess return of stock *j* purchased on date *t* by subject *i*, winsorized at the 1st and 99th percentiles to ensure the results are not driven by outliers. The unit of observation is person-stock-day. As discussed earlier, *Forecast Bias* is re-scaled to have a standard deviation of one, and thus the estimated coefficients directly show the economic magnitudes of a one-standard deviation change in *Forecast Bias*. $\boldsymbol{X_{i,t}}$ is a matrix of control variables and θ_t is a year-month fixed effect, included to remove potential confounding variation (e.g., market volatility, recent attention-grabbing events, the state of the economy, etc.). Because trading activity is highly skewed we estimate weighted regressions, such that each subject receives equal weight. Standard errors, clustered by subject, are reported in parentheses below the coefficient estimates.

The dependent variable is the lagged return over the prior year less the return on the Danish stock market. By removing the overall market performance and including year-month fixed effects,[19] the regressions compare relative performance across stocks and not trading in response to overall market movements. Thus, we examine the relation between forecast bias and security selection, without confounding this relation with market timing.

Table 2 reports regression results that test the relation between *Forecast Bias* and the past performance of purchased stocks. Column (1) does not include control variables, column (2) includes demographic and economic control variables, and column (3) also includes controls for financial literacy, numeracy, optimism, overconfidence, trust, and risk aversion. All columns include year-month fixed effects. Throughout this study, we use variations of the regression in column (3) as our baseline specification.

As discussed earlier, we expect a positive relation between *Forecast Bias* and lagged stock returns. Contrarians, with a negative *Forecast Bias*, will buy poor performers, while extrapolators, with a positive *Forecast Bias*, will buy high performers. Consistent with the predictions of theory, in all three columns the coefficient on *Forecast Bias* is positive and significant. The coefficient estimate in column (3) implies that a one standard deviation change in *Forecast Bias* is associated with buying stocks that had 3.0 percentage points higher excess

[19] The lagged returns are calculated over the prior year ending the day before the purchase, and thus there is some variation in the benchmark return even within year-month due to trading on different days within the month.

returns over the past year. The coefficients are quite stable as we add control variables, suggesting that *Forecast Bias* is largely independent of economic and demographic factors such as wealth, education, financial literacy, etc.[20]

The prior tests focus on lagged one-year return periods. The past year is a natural evaluation period as brokerages and financial media often report returns over the past year, and this is a commonly used period in the literature examining how past returns affect individual investors' decisions (e.g., see Barber and Odean, 2002; Kumar, 2009; Laudenbach, Weber, Weber, and Wohlfart, 2022). As a robustness test, we evaluate lagged returns over alternative time-periods, ranging from one week to three year time horizons. The results in Online Appendix Table D.3, show that the coefficient on *Forecast Bias* is positive and statistically significant for all but the one week and one month horizons.

Our sample includes only those subjects who accepted our invitation to participate in the experiment. To address sample selection concerns, we re-estimate our main specification using a Heckman model. The instruments are the randomized financial incentive to participate in the experiment and the subject's commuting time to the experimental site.[21] The coefficient on *Forecast Bias* in the Heckman selection model is almost identical to that in the main specification, alleviating sample selection bias concerns (details on the analysis are in Online Appendix C and the results are in Online Appendix Table C.1).

[20] Online Appendix Table D.4 shows there is little correlation between *Forecast Bias* and economic and demographic characteristics. Furthermore, Online Appendix Table D.5 shows the results are robust in specifications using person-month-level observations.

[21] The Greater Copenhagen area comprises a blend of prosperous and less privileged neighborhoods. Consequently, commuting time to the experimental site is largely uncorrelated with the socioeconomic status of a neighborhood. We further note that our regressions control for individual characteristics that are related to an individuals' socioeconomic status and, hence, investment behavior.

3.2 *Forecast Bias and Stock Sales*

Conceptually, the relation between *Forecast Bias* and stock sale decisions mirrors that of purchase decisions; contrarians should prefer to retain low performers and sell high performers, while extrapolators should prefer the opposite. There is, however, an important distinction between sales and purchases. While purchases can be selected from the entire universe of stocks, sales are selected from the more limited set of existing stock holdings.[22] The mean (quasi-median) number of individual stocks held at the time of a sale is 11.5 (8.5), and so it is easy for investors to compare performance across all holdings. Thus, for sales, we can clearly identify the comparison group of stocks that are not sold. This allows us to estimate the following linear probability model:[23]

$$Sale_{i,j,t} = \beta_1 \cdot Perf_{i,j,t} \cdot ForecastBias_i + \beta_2 \cdot Perf_{i,j,t} + \boldsymbol{\delta X_{i,j,t}} + \theta_{i,t} + \theta_l + \varepsilon_{i,j,t} \qquad (6)$$

where $Sale_{i,j,t}$ is an indicator variable equal to 100 if subject i sells stock j on date t, $Perf_{i,j,t}$ is a measure of the performance of that stock (e.g., capital gain since purchase)[24] as of the end of the prior trading day, $\boldsymbol{X_{i,j,t}}$ is a matrix of control variables, $\theta_{i,t}$ is a person-day fixed effect, and θ_l is a fixed effect for the length of the holding in months.[25] We limit the sample to days when the subject sells at least one stock (sales days), as Chang, Solomon, and Westerfield (2016) argue non-trading may be caused by inattention and not deliberate choice. The unit of observation is person-stock-day.

---

[22] Although short sales can be made from the entire universe of stocks, shorting is extremely rare for individual investors (e.g., Barber and Odean, 2008).

[23] Online Appendix Table D.6 shows the results are similar for a conditional logit model.

[24] Because we require holding-level capital gains, the sample for the sales regressions begins in 2013 as we cannot observe purchase prices before 2011. For positions that were initiated prior to January 1, 2011, we calculate capital gains using the price as of the end of December 2010.

[25] Columns (1), (3), and (5) include the stocks' lagged returns over the past year as an independent variable and this causes the loss of observations whose returns are not in the return databases.

The person-day fixed effect specification closely matches the subjects' decision. We limit the comparison to the set of stocks the investor could potentially sell on that particular day, and examine the relative likelihood that an investor chooses to sell a stock given that stock's performance *relative* to the investor's other stock holdings. Further, the person-day fixed effects remove individual specific effects that drive the decision to trade on that day.

We include a length of holding fixed effect, as prior studies show a strong relation between holding length and the probability of selling (e.g., Ben-David and Hirshleifer, 2012). We include controls for the stock's idiosyncratic standard deviation, beta, and its weight in the subject's portfolio. The standard errors reported below the coefficients are clustered by person.

Table 3 reports regression results for sales, and the columns consider several different performance measures. In column (1), the performance measure is the lagged annual excess return. This specification follows that in the purchase regressions in the prior subsection. In this specification, the coefficient on the interaction term between performance and *Forecast Bias* is not significant.

In columns (2) through (5), the performance measure is capital gain since purchase. *Forecast Bias* measures how investors incorporate past return information into expectations. For sales decisions, investors have information that is not available for purchases – their capital gain on a particular stock. A large literature shows that capital gains are strongly related to sales decisions, with much of this literature focused on preference based explanations such as the disposition effect (e.g., Shefrin and Statman, 1985; Odean, 1998) or realization utility (e.g., Barberis and Xiong, 2012). In contrast, our study of *Forecast Bias* relates to a smaller subset of the stock sales literature that focuses on how belief updating affects the relation between capital gains and sales decisions (e.g., Ben-David and Hirshleifer, 2012).[26] To ensure that our results are distinct from the disposition effect, we follow Ben-David and Hirshleifer (2012) and include an indicator variable for capital gains greater than zero as a control.

In column (2), the coefficient on the interaction term between *Forecast Bias* and capital gain is negative and significant. Comparing across positions held in their portfolio, extrapolators are more likely to sell stocks with lower capital gains and contrarians are more likely to sell stocks with higher capital gains. This result is consistent with Ben-David and Hirshleifer (2012) who argue the relation between capital gains and sales decisions is driven by belief updating. We show that *Forecast Bias*, which captures the belief updating process, affects the direction in which capital gains affect sales decisions. Our results are also consistent with the V-shaped relation between capital gains and sales decisions found by Ben-David and Hirshleifer (2012) – both positive and negative capital gains can increase sales propensity, depending upon whether the investor is an extrapolator or a contrarian. The coefficient on the indicator variable for positive capital gains is positive and significant, consistent with a disposition effect, however, the effect of *Forecast Bias* is robust to this control.

Column (3) further controls for the disposition effect by including the square and cube of capital gain, as well as lagged annual excess returns. The higher-order terms control for the possibility that the relation between capital gains and sales propensity is non-linear. After including these terms, the interaction term between *Forecast Bias* and capital gains is nearly unchanged, suggesting that the relation between *Forecast Bias* and sales decisions is distinct from the disposition effect.

[26] Ben-David and Hirshleifer (2012) discuss *cross-sectional* beliefs and the disposition effect. This is distinct from Andersen, Hanspal, Martínez-Correa, and Nielsen (2021) who study beliefs about the overall market and the disposition effect. Our use of person-day fixed effects removes the effect of beliefs about the overall market.

In columns (4) and (5), we replace capital gain with the within-portfolio ranking of each security's capital gain (e.g., 0 indicates the stock with the lowest capital gain, 0.5 the median, etc.). Hartzmark (2015) shows that the relative rank of a stock's capital gain within an investor's portfolio strongly affects sales decisions. Following similar logic in our setting, conditional on an investor deciding to make a sale (e.g., to fund the purchase of a new stock) it is natural for the investor to compare across holdings and choose a stock for which they are relatively pessimistic. Extrapolators will tend to be pessimistic about stocks with lower capital gains while contrarians will be pessimistic about stocks with higher capital gains.

Column (4) shows that the coefficient on the interaction term between *Forecast Bias* and capital gain rank is negative and significant. The coefficient estimates imply that a one standard deviation increase in *Forecast Bias* results in a 6.3 percentage point reduction in the probability that a subject sells their highest performer instead of their lowest performer.

In column (5), we add numerous additional control variables. Hartzmark (2015) shows there is a U-shaped relation between the capital gain rank and sales propensity, and that people are more likely to sell stocks that are among the best or worst performers within their portfolio. Accordingly, we add a non-linear term for capital gains, $(CapGain\ rank\ -\ 0.5)^2$, defined as the square of the capital gain rank minus 0.5, as well as indicator variables for the stocks with the lowest and highest capital gain in the portfolio, and the lagged annual excess return. The results show that, while we find strong support for the non-linear relation reported in Hartzmark (2015), the interaction term between *Forecast Bias* and capital gain rank remains negative and significant even with the inclusion of the additional control variables.

3.3 Alternative Measures of Forecast Bias

Table 4 shows results with alternative measures of *Forecast Bias*. Except for the alternative measures, the specifications for the purchases analyses in Panel A are identical to that in column (3) of Table 2 and the specifications for the sales analyses in Panel B are identical to that in column (5) of Table 3, respectively.

Columns (1) and (2) show results for alternative measures of *Forecast Bias* based on the person-specific realized random process. In the elicitation experiment, the subjects observe time-series of realizations generated using the same underlying parameters. However, because each subject observes a unique time-series, by random chance some subjects observe time-series that appear to differ from the true process. To address this issue, the alternative measure in column (1) is the residual from regressing *Forecast Bias* on the empirically observed persistence and standard deviation of the 80 realizations. The alternative measure in column (2) employs a subject-specific rational benchmark that is updated every round of the elicitation procedure using the realizations the subject has observed until that point in the experiment. Section 1.2 contains details on both alternative measures. For both purchases and sales, the results are significant and similar to those in the main specification.

Column (3) shows results using the rank transformation of *Forecast Bias* as the independent variable, to ensure the results are not driven by outliers. The results are similar to those in the main specification.

Columns (4) through (7) shows results for four alternative measure of forecasting bias: *Diagnostic Expectations*, *Sticky Expectations, Extrapolative Expectations*, and *Adaptive Expectations*. The results are similar to those found using the *Forecast Bias* measure, except *Sticky Expectations* and *Extrapolative Expectations* are not significant in the purchase analysis.

3.4 Quantitative Reasoning Ability, Financial Sophistication, and Forecast Bias

The *Forecast Bias* variable measures errors relative to a statistically optimal benchmark. A potential concern is whether it reflects deficiencies in quantitative reasoning ability, which could also affect stock selection decisions.[27] We argue that such a concern is unlikely to drive our results, because *Forecast Bias* is not monotonically related to subjects' deviations from rationality; low values indicate an excessive contrarianism while high values indicate excessive extrapolation. Thus, people in the tails of *Forecast Bias* may have relatively poor quantitative reasoning, but this is not sufficient to create a monotonic relation with past stock returns of stocks traded. Our findings require that individuals who are consistently contrarians during the experiment are also contrarian investors and individuals who are consistently extrapolators during the experiment are also extrapolators as investors That is, our findings require directionally consistent deviations from rationality in both domains. Nevertheless, to ensure that our results are not driven by subjects with limited cognitive skills or low financial sophistication, we examine subsamples based on proxies for sophistication.

The purchase results are in Panel A of Table 5 and the sale results are in Panel B. Aside from limiting the sample, the specifications for the purchase analyses are identical to that in column (3) of Table 2 and for the sales analyses are identical to that in column (5) of Table 3, respectively. Columns (1) through (3) limit the sample to include, respectively, only subjects with at least 16 years of formal education, who got all four financial literacy questions correct, or who got all three numeracy questions correct. The coefficients on *Forecast Bias* are significant in all specifications, and the magnitudes are similar to those in the full sample.

4. *Forecast Bias*, Aggregate Stock Market Returns, and Net Flows into Stocks

The primary focus of this study is to test how forecast bias affects individuals' *cross-sectional* stock selection decision conditional on trading. In this section, we step back from stock selection and instead test how forecast bias relates to net flows to stocks. This aligns with

[27] Online Appendix Table D.4 shows statistically significant differences in proxies for quantitative reasoning ability for different levels of *Forecast bias* but the absolute differences are small.

much of the literature that examines surveys of investors' expectations, and which focus on the *time-series* of beliefs about stock market index returns (see for example Amromin and Sharpe, 2014; Greenwood and Shleifer, 2014; Adam, Matveev, and Nagel, 2021; and Giglio, Maggiori, Stroebel, and Utkus, 2021). Most closely related to our study, Laudenbach, Weber, Weber, and Wohlfart (2023) use survey measures of investor-level beliefs about historical stock index autocorrelations to explain investors' net flows into stocks. We expand the extant literature by testing whether bias in information processing affects net flows to stocks. Specifically, we examine how forecast bias interacts with past market index returns and with each investor's own excess returns to affect net flows.

Column (1) of Table 6 reports regression results in which the dependent variable is net flows into stocks. The unit of observation is investor-month, and the sample includes all months in which an investor owns stocks, even if the investor does not trade (i.e., the sample does not condition on trading). The dependent variable, net flows, is defined as the difference between the value of stock purchases and sales in a month, divided by the value of stocks owned at the beginning of the month (multiplied by 100). We regress this variable on *Forecast Bias* interacted with lagged market and own-portfolio excess returns:

$$\begin{aligned} Net\ Flows_{i,t} = {} & \beta_1 \cdot MarketRet_t \cdot ForecastBias_i \\ & + \beta_2 \cdot ExcessRet_{i,t} \cdot ForecastBias_i \\ & + \beta_3 \cdot MarketRet_t + \beta_4 \cdot ExcessRet_{i,t} + \boldsymbol{\delta X_{i,t}} + \theta_i + \theta_t + \varepsilon_{i,t} \end{aligned} \tag{7}$$

where $MarketRet_t$ is the return on the Danish stock market index over the prior 12 months and $ExcessRet_{i,t}$ is return on investor *i*'s stock portfolio in excess of the market index over the prior 12 months.[28] The specification includes individual fixed effects, which subsume the direct

[28] Because we require the past 12 months of the subjects' investment returns, these tests are estimated over the period 2012-2021, with the 2011 data used only to calculate the subjects' lagged investment returns.

effect of *Forecast Bias* and control for the investors' savings rates, general trading tendencies, etc. The specification also includes year-month fixed effects, which control for overall market conditions, the state of the economy, etc.

The results in column (1) show a positive and significant coefficient on the interaction between *Forecast Bias* and lagged market index returns. Extrapolators' net flows increase when the market does well and contrarians' net flows increase when the market does poorly. The interaction between *Forecast Bias* and investors' own lagged excess returns on their stock portfolios is not significant. The results for these two coefficients show what type of returns interact with forecast bias – the returns of the underlying asset class are important but the investor's own excess returns are not. This provides evidence that forecast bias is distinct from overconfidence – people may increase allocations to stocks following high excess returns – but any effect from excess returns does not interact with forecast bias.

Although the coefficient on the interaction term between *Forecast Bias* and lagged market index returns is significant, the implied economic magnitude is small. The coefficient estimate implies that, following a lagged market index return one standard deviation above the mean, a one standard deviation increase in *Forecast Bias* is associated with a net flow into stocks of 12 basis points. The small economic magnitude found in this unconditional regression is consistent with the literature. Giglio, Maggiori, Stroebel, and Utkus (2021) find that beliefs have little explanatory power for the timing of trades, but that conditional on trade occurring, beliefs explain the direction and magnitude of trade. Accordingly, we separate the decision to trade (columns (2), (3), and (4)) from the action taken conditional upon trading (column (5)).

Columns (2), (3), and (4) of Table 6 examine the decision of *when* to actively adjust the amount allocated to stocks, ignoring the size of the adjustment. In column (2), the dependent variable is *Trade Month*, an indicator equal to 100 for months when the absolute value of the investor's net flow is greater than 1%.[29] In column (3), the dependent variable is *Buy Month*, an indicator equal to 100 for months when the investor's net flow is greater than 1%. In column (4), the dependent variable is *Sell Month*, an indicator equal to 100 for months when the investor's net flow is less than -1%. None of the interaction terms in these columns are significant; *Forecast Bias* lacks the ability to predict when investors will trade.[30] This is similar to Giglio, Maggiori, Stroebel, and Utkus (2021), who find that changes in beliefs have little ability to predict when investors will trade.

Column (5) turns to the relation between net flows and the interaction between *Forecast Bias* and lagged returns *conditional* upon trading in that month. This regression is identical to that in column (1), except we restrict the sample to include only months in which the investor's absolute net flow is greater than 1%. The results are directionally similar to the unconditional results, but the implied economic magnitude is 7.2 times larger. The set of results in Table 6 shows that the relation between *Forecast Bias* and past returns is driven by the intensive margin of trading – actions taken conditional upon trading – and not by the decision to trade.

Taken together, our results on stock purchases, sales, and net flows provide evidence of a single underlying mechanism that unites the relation between past returns and investor trading decisions. Prior studies find that different types of investment choices are affected by different types of returns. The decision to purchase a stock is linked to that stock's historical returns (Grinblatt and Keloharju, 2000; Barber and Odean, 2008); the decision to sell a stock is linked to the investor's capital gains on that stock (Odean, 1998; Ben-David and Hirshleifer, 2012; Hartzmark, 2015); and decisions about net flows to stocks are linked with past market returns

---

[29] We define *Trade month* as absolute net flows greater than 1% because portfolio rebalancing might lead to small absolute net flows if the values of sales and purchases are not identical. We do not want to classify portfolio rebalancing as an active decision about net flows.

[30] Online Appendix Table D.7 shows the results are similar for a conditional logit model.

(Greenwood and Shleifer, 2014). Our study shows that a common mechanism – forecast bias – consistently affects these relations, and thus provides evidence unifying these results across different types of investment decisions.

5. Do Investment Experiences Affect Forecast Bias?

When interpreting the prior results, we implicitly treat forecast bias as a stable personal trait, consistent with Dominitz and Manski (2011) who find that individuals forecast biases are intra-personally stable over time. Conceptually, we interpret forecast bias as a parameter that governs how individuals process information to produce forecasts. In the lab experiment, we provide all subjects with information in an identical fashion, and follow Dominitz and Manski (2011), Heiss et al. (2022), and von Gaudecker and Wogrolly (2022) in attributing variation in forecasting behavior to variation in how individuals process information. Outside the lab, subjects' information sets will vary based on what they observe, including their personal investment experiences. Indeed, the literature finds that personal experience affects individuals' return expectations and risk taking (e.g., Vissing-Jorgensen, 2003; Malmendier and Nagel, 2011; Andersen, Hanspal and Nielsen, 2021) and Bordalo et al. (2023) present a model of how individuals' biases can change over time. Our operative assumption is that *Forecast Bias* captures a temporally stable personal characteristic – how individuals process information and form expectations – and not the direct effect of information on expectations. Thus, our measure should capture a component of expectation formation that is conceptually distinct from personal experiences.

To examine whether forecast bias is an individual trait that is distinct from personal experiences, we test whether investors' pre-experiment stock market experiences predict their forecast bias. We generate three categories of investor experience variables.

The first category of investor experience variables is motivated by studies showing that personally experienced returns affect trading decisions (e.g., Kaustia and Knüpfer, 2008). In the context of our study, a subject who experiences, for example, positive returns after buying stocks with recent negative returns might learn to be a contrarian. We classify contrarian buys as purchases of stocks that had negative excess returns over the past 12 months. We then measure the ratio of successful contrarian buys (defined as having a positive excess return over the subsequent 12 months) to unsuccessful contrarian buys. We construct similar ratios for contrarian sales, extrapolator buys, and extrapolator sales for five different horizons: one week, one month, three months, six months, and 12 months. We also include the average annual market return in the years a person held equity.

The second category of investor experience variables measures whether a person held equity during a stock market crash, motivated by prior studies showing long lasting effects of economic crises on individual risk taking (Knüpfer, Rantapuska, Sarvimäki, 2017; Andersen, Hanspal and Nielsen, 2021). Specifically, we create indicator variables for people who held equity during the 2000-2002 dot-com crash and the 2007-2009 financial crisis.

The third category of investor experience variables measures general trading experiences: the number of buys, number of sells, indicators for never buying, never selling, and the number of years a person held equity since 1997. In total, we generate 23 investor experience variables (Online Appendix Table D.8 reports summary statistics).

In Table 7 we regress *Forecast Bias* on the investor experience variables defined above and the full set of control variables. Only one of the 23 investor experience variables is significantly related to *Forecast Bias* (but is the opposite sign to that implied by experience effects). A likelihood ratio test fails to reject that the investor experience variables are jointly insignificant (p-value = 0.653). Similarly, likelihood ratio tests fail to reject that each of the three categories of investor experience variables are insignificant (p-values of 0.549, 0.383, and 0.947, respectively). Overall, these results are inconsistent with the idea that forecast bias is determined by investor experience.

6. Forecast Bias and Investment Performance

The prior sections show that *Forecast Bias* is related to past stock returns and trading decisions. Although our laboratory elicitation procedure ensures that *Forecast Bias* is a *bias* – a deviation from a clearly defined statistically optimal benchmark – the literature shows that past returns have some predictive power for future returns (e.g., De Bondt and Thaler, 1985; Jegadeesh and Titman, 1993). Thus, trading based on past returns could be a rational trading strategy. Accordingly, in this section, we test the relation between *Forecast Bias* and investment performance.

Using the end-of-year stock holdings and information about trades within the year, we impute monthly holdings for each subject and construct their value-weighted monthly portfolio returns less the risk-free rate. We sort the subjects into three portfolios based on their *Forecast Bias* parameter and aggregate across investors to construct a time-series of returns. We then estimate a CAPM regression and report the results in Table 8. The standard errors reported in parentheses are calculated using the Newey-West correction with three lags. In Panel A investors are equal weighted and in Panel B they are value weighted.

Only one of the six alpha estimates reported in Table 8 is significant and none of them are positive. Taken together, the results in Table 8 do not support the idea that *Forecast Bias* captures a propensity for rational momentum or reversal trading.

7. Conclusion

Our study is the first to show a relation between individual-level measures of forecast bias and cross-sectional stock trading decisions. We elicit a measure of forecast bias using a laboratory experiment for a sample of investors in Denmark. On average, individuals exhibit extrapolation bias, though there is substantial heterogeneity. We link our measure of forecast bias to administrative register data on stock trades from 2011-2021 and examine how it affects individuals' stock selection decisions.

We find that forecast bias is positively related to the past excess returns of stocks purchased by individual investors: extrapolators (contrarians) tend to purchase stocks with high (low) past annual excess returns. Turning to sales decisions, we find that forecast bias is negatively related to investors' capital gains since purchase of stocks that are sold. We find that investors with higher forecast bias increase (decrease) their allocations to stocks following positive (negative) annual market returns. Overall, our results show that heterogeneity in forecast bias – errors in how investors incorporate past returns into expectations – explains across-investor variation in how past returns affect investors' decisions about trading individual stocks and net flows to stocks.

References

Adam, K., D. Matveev, and S. Nagel. 2021. Do survey expectations of stock returns reflect risk adjustments? *Journal of Monetary Economics* 117, 723-740.

Adam, K., and S. Nagel. 2022. Expectations data in asset pricing. In *Handbook of Economic Expectations*, R. Bachman, G. Topa, and W. van der Klaauw Editors, Elsevier.

Afrouzi, H., S.Y. Kwon, A. Landier, Y. Ma, and D. Thesmar. 2023. Overreaction in expectations: Evidence and theory. *Quarterly Journal of Economics* 138, 1713-1764.

Amromin, G., and S.A. Sharpe. 2014. From the horse's mouth: Economic conditions and investor expectations of risk and return. *Management Science* 60, 845-866.

Andersen, S., T. Hanspal, and K.M. Nielsen. 2021. Once bitten, twice shy. The power of personal experiences in risk taking. *Journal of Financial Economics* 132, 97-117.

Andersen, S., T. Hanspal, J. Martínez-Correa, and K.M. Nielsen. 2021. Beliefs and the disposition effect. Working paper, Copenhagen Business School.

Andersen, S., G. W. Harrison, M. I. Lau, and E. E. Rutström. 2008. Eliciting risk and time preferences. *Econometrica* 76, 583–618.

Andries, M., M. Bianchi, K. Huynh, and S. Pouget. 2022. Return predictability, expectations, and investment: Experimental evidence. Working paper, Toulouse School of Economics.

Atmaz, A., S. Cassella, H. Gulen, and F. Ruan. 2023. Contrarians, extrapolators, and stock market momentum and reversal. *Management Science*, forthcoming.

Barber, B.M., and T. Odean. 2002. Online investors: Do the slow die first? *Review of Financial Studies* 15, 455-487.

Barber, B.M., and T. Odean. 2008, All that glitters: The effect of attention and news on the buying behavior of individual and institutional investors. *Review of Financial Studies* 21, 786-818.

Barberis, N.C. 2018. Psychology-based models of asset prices and trading volume. In *Handbook of Behavioral Economics*, D. Bernheim, S. DellaVigna, and D. Laibson Editors, Elsevier.

Barberis, N., R. Greenwood, L. Jin, and A. Shleifer. 2015. X-CAPM: An extrapolative capital asset pricing model. *Journal of Financial Economics* 115, 1-24.

Barberis, N., R. Greenwood, L. Jin, and A. Shleifer. 2018. Extrapolation and bubbles. *Journal of Financial Economics* 129, 203-227.

Barberis, N., and W. Xiong. 2012. Realization utility. *Journal of Financial Economics* 104, 251-271.

Barberis, N., A. Shleifer, and R. Vishny. 1998. A model of investor sentiment. *Journal of Financial Economics* 49, 307-343.

Ben-David, I., and D. Hirshleifer. 2012. Are investors really reluctant to realize their losses? Trading responses to past returns and the disposition effect. *Review of Financial Studies* 25, 2485-2532.

Ben-David, I., J. Li, A. Rossi, and Y. Song. 2022. What do mutual fund investors really care about? *Review of Financial Studies* 35, 1723-1774.

Beutel, J., and M. Weber. 2022. Beliefs and portfolios: Causal evidence. Working paper, University of Chicago.

Bhamra, H.S., R. Uppal, and J. Walden. 2022. Psychological distance and subjective beliefs. Working paper, Imperial College.

Bordalo, P., K. Coffman, N. Gennaioli, and A. Shleifer. 2016. Stereotypes. *Quarterly Journal of Economics* 131, 1753-1794.

Bordalo, P., J. Conlon, N. Gennaioli, S. Kwon, and A. Shleifer. 2023. How people use statistics. Working paper, Oxford University.

Bordalo, P., N. Gennaioli, R. La Porta, and A. Shleifer. 2019. Diagnostic expectations and stock returns. *Journal of Finance* 74, 2839-2874.

Bordalo, P., N. Gennaioli, and A. Shleifer. 2018. Diagnostic expectations and credit cycles. *Journal of Finance* 73, 199-227.

Cagan, P. 1956. The monetary dynamics of hyperinflation. In *Studies in the Quantity Theory of Money*, M. Friedman Editor, University of Chicago Press.

Calvet, L.E., J.Y. Campbell, and P. Sodini. 2007. Down or out: Assessing the welfare costs of household investment mistakes. *Journal of Political Economy* 115, 707-747.

Calvet, L.E., J.Y. Campbell, and P. Sodini. 2009. Fight or flight? Portfolio rebalancing by individual investors. *Quarterly Journal of Economics* 124, 301-348.

Cassella, S., Z. Chen, H. Gulen, and R. Petkova. 2022. Extrapolators at the gate: Market-wide misvaluation and the value premium. Working paper, Tilburg University.

Chang, T.Y., D.H. Solomon, and M.M. Westerfield. 2016. Looking for someone to blame: Delegation, cognitive dissonance, and the disposition effect. *Journal of Finance* 71, 267-302.

Cronbach, L. 1951. Coefficient alpha and the internal structure of tests. *Psychometrika* 16, 297-334.

Da, Z., X. Huang, and L. Jin. 2021. Extrapolative beliefs in the cross-section: What can we learn from crowds? *Journal of Financial Economics* 140, 175-196.

De Bondt, W.F. 1993. Betting on trends: Intuitive forecasts of financial risk and return. *International Journal of Forecasting* 9, 355-371.

De Bondt, W.F. and R. Thaler. 1985. Does the stock market overreact? *Journal of Finance* 40, 793-805.

Dimmock, S.G., R. Kouwenberg, O.S. Mitchell, and K. Peijnenburg. 2016. Ambiguity aversion and household portfolio choice puzzles: Empirical evidence. *Journal of Financial Economics* 119, 559-577.

Dimmock, S.G., R. Kouwenberg, O.S. Mitchell, and K. Peijnenburg. 2021. Household portfolio underdiversification and probability weighting: Evidence from the Field. *Review of Financial Studies* 34, 4524-4563.

Dominitz, J., and C.F. Manski. 2007. Expected equity returns and portfolio choice: Evidence from the Health and Retirement Study. *Journal of the European Economic Association* 52, 369-379.

Dominitz, J., and C.F. Manski. 2011. Measuring and interpreting expectations of equity returns. *Journal of Applied Econometrics* 26, 352-370.

Fagereng, A., L. Guiso, D. Malacrino, and L. Pistaferri. 2020. Heterogeneity and persistence in returns to wealth. *Econometrica* 88, 115-170.

Fisher, K.L., and M. Statman. 2000. Investor sentiment and stock returns. *Financial Analysts Journal* 56, 16-23.

Gennaioli, N. and A. Shleifer. 2010. What comes to mind. *Quarterly Journal of Economics* 125, 1399-1433.

Giglio, S., M. Maggiori, J. Stroebel, and S. Utkus. 2021. Five facts about beliefs and portfolios. *American Economic Review* 111, 1481-1522.

Greenwood, R., and A. Shleifer. 2014. Expectations of returns and expected returns. *Review of Financial Studies* 27, 714-746.

Grinblatt, M., and M. Keloharju. 2000. The investment behavior and performance of various investor types: A study of Finland's unique data set. *Journal of Financial Economics* 55, 43-67.

Grinblatt, M., and M. Keloharju. 2009. Sensation seeking, overconfidence, and trading activity. *Journal of Finance* 64, 549-578.

Grinblatt, M., M. Keloharju, and J.T. Linnainmaa. 2012. IQ, trading behavior, and performance. *Journal of Financial Economics* 104, 339-362.

Guiso, L., P. Sapienza, and L. Zingales. 2008. Trusting the stock market. *Journal of Finance* 63, 2557-2600.

Hartzmark, S.M. 2015. The worst, the best, ignoring all the rest: The rank effect and trading behavior. *Review of Financial Studies* 28, 1024-1059.

Hartzmark, S.M. and D.H. Solomon. 2019. The dividend disconnect. *Journal of Finance* 74, 2153-2199.

Heiss, F., M. Hurd, M. van Rooij, T. Rossman, and J. Winter. 2022. Dynamics and heterogeneity of subjective stock market expectations. *Journal of Econometrics* 231, 213-231.

Hoffman, A.O.I., T. Post, and J.M.E. Pennings. 2015. How investor perceptions drive actual trading and risk-taking behavior. *Journal of Behavioral Finance* 16, 94-103.

Hong, H., and J.C. Stein. 1999. A unified theory of underreaction, momentum trading, and overreaction in asset markets. *Journal of Finance* 54, 2143-2184.

Hossain, T., and R. Okui. 2013. The binarized scoring rule. *Review of Economic Studies* 80, 984-1001.

Hvide, H., and P. Östberg. 2015. Social interaction at work. *Journal of Financial Economics* 117, 628-652.

Ivković, Z., and S.J. Weisbenner. 2007. Information diffusion effects in individual investors' common stock purchases: Covet thy neighbors' investment choices. *Review of Financial Studies* 20, 1327-1357.

Jegadeesh, N., and S. Titman. 1993. Returns to buying winners and selling losers: Implications for stock market efficiency. *Journal of Finance* 48, 65-91.

Kaustia, M., S. Knüpfer. 2008. Do investors overweight personal experience? Evidence from IPO subscriptions. *Journal of Finance* 63, 2679-2702.

Knüpfer, S., E.H. Rantapuska, and M. Sarvimäki. 2017. Formative experiences and portfolio choice: Evidence from the Finnish Great Depression. *Journal of Finance* 72, 133-166.

Knüpfer, S., E.H. Rantapuska, and M. Sarvimäki. 2023. Social interaction in the family: Evidence from investors' security holdings. *Review of Finance* 27, 1297-1327.

Kumar, A., 2009. Who gambles in the stock market? *Journal of Finance* 64, 1889-1933.

Lakonishok, J., A. Shleifer, and R. Vishny. 1994. Contrarian investment, extrapolation, and risk. *Journal of Finance* 49, 1541-1578.

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

Laudenbach, C., A. Weber, R. Weber, and J. Wohlfart. 2023. Beliefs about the stock market and investment choices: Evidence from a field experiment. Working paper, Goethe University.

Liu, H., C. Peng, W.A. Xiong, and W. Xiong. 2022. Taming the bias zoo. *Journal of Financial Economics* 143, 716-741.

Lusardi, A., and O.S. Mitchell. 2007. Baby boomer retirement security: The roles of planning, financial literacy, and housing wealth. *Journal of Monetary Economics* 54, 205-224.

Lusardi, A., and O.S. Mitchell. 2011. Financial literacy around the world: An overview. *Journal of Pension Economics and Finance* 10, 497-508.

Lusardi, A., and O.S. Mitchell. 2014. The economic importance of financial literacy: Theory and evidence. *Journal of Economic Literature* 52, 5-44.

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

Merkle, C., and M. Weber. 2014. Do investors put their money where their mouth is? Stock market expectations and investing behavior? *Journal of Banking and Finance* 46, 372-386.

Metzler, L.A. 1941. The nature and stability of inventory cycles. *Review of Economics and Statistics* 23, 113-129.

Moore, D.A., and P.J. Healy. 2008. The trouble with overconfidence. *Psychological Review* 115, 502-517.

Odean, T. 1998. Are investors reluctant to realize their losses? *Journal of Finance* 53, 1775-1798.

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

Shefrin, H, and M. Statman. 1985. The disposition to sell winners too early and ride losers too long: Theory and evidence. *Journal of Finance* 40, 777-790.

van Rooij, M., A. Lusardi, and R. Alessie. 2011. Financial literacy and stock market participation. *Journal of Financial Economics* 101, 449-472.

Vissing-Jorgensen, A. 2003. Perspectives on behavioral finance: Does "irrationality" disappear with wealth? Evidence from expectations and actions. *NBER Macroeconomics Annual* 18, 139-194.

von Gaudecker, H-M., and A. Wogrolly. 2022. Heterogeneity in households' stock market beliefs: Levels, dynamics, and epistemic uncertainty. *Journal of Econometrics* 231, 232-247.

Wakker, P., I. Erev, and E.U. Weber. 1994. Comonotonic independence: The critical test between classical and rank-dependent utility theories. *Journal of Risk and Uncertainty* 9, 195-230.

Woodford, M. 2003. Imperfect common knowledge and the effects of monetary policy. In *Knowledge, Information, and Expectations in Modern Macroeconomics*, P. Aghion, R. Frydman, J.E. Stiglitz, and M. Woodford Editors, Princeton University Press.

Figure 1: Elicitation of forecast bias

This figure shows an example of the forecasting task. The upper panel, shows an example of the first round of the prediction task. The subject observes 40 past realizations of the process (green dots with numbers showing exact values). The subject is asked to make forecasts for the next two rounds by sliding the blue and orange "**x**" up and down, and then clicking the "Make forecast" button. The next realization of the process is then revealed, as seen in the lower panel, and the subject is asked to make two new predictions. This process continues for a total of 40 rounds.

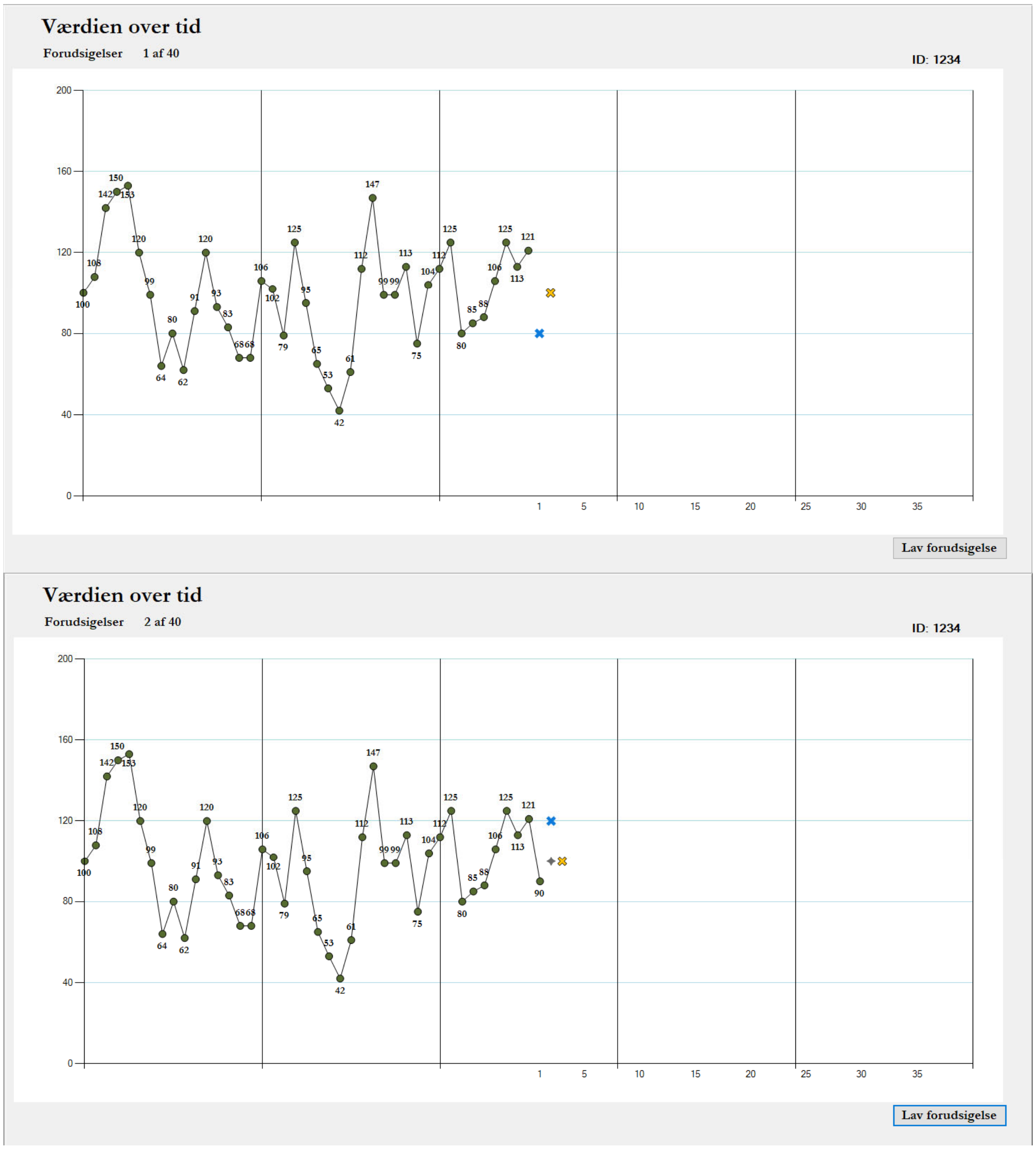

Figure 2: Histogram of *Forecast Bias*

This figure shows a histogram of the distribution of *Forecast Bias*. A value of zero implies no bias, a value greater than zero implies extrapolation bias (i.e., forecast is biased in the direction of recent realizations), and a value below zero implies contrarian bias (i.e., forecast is biased in the opposite direction of recent realizations). We truncate the tails to avoid reporting bins with fewer than five observations, in accordance with our data agreement with Statistics Denmark.

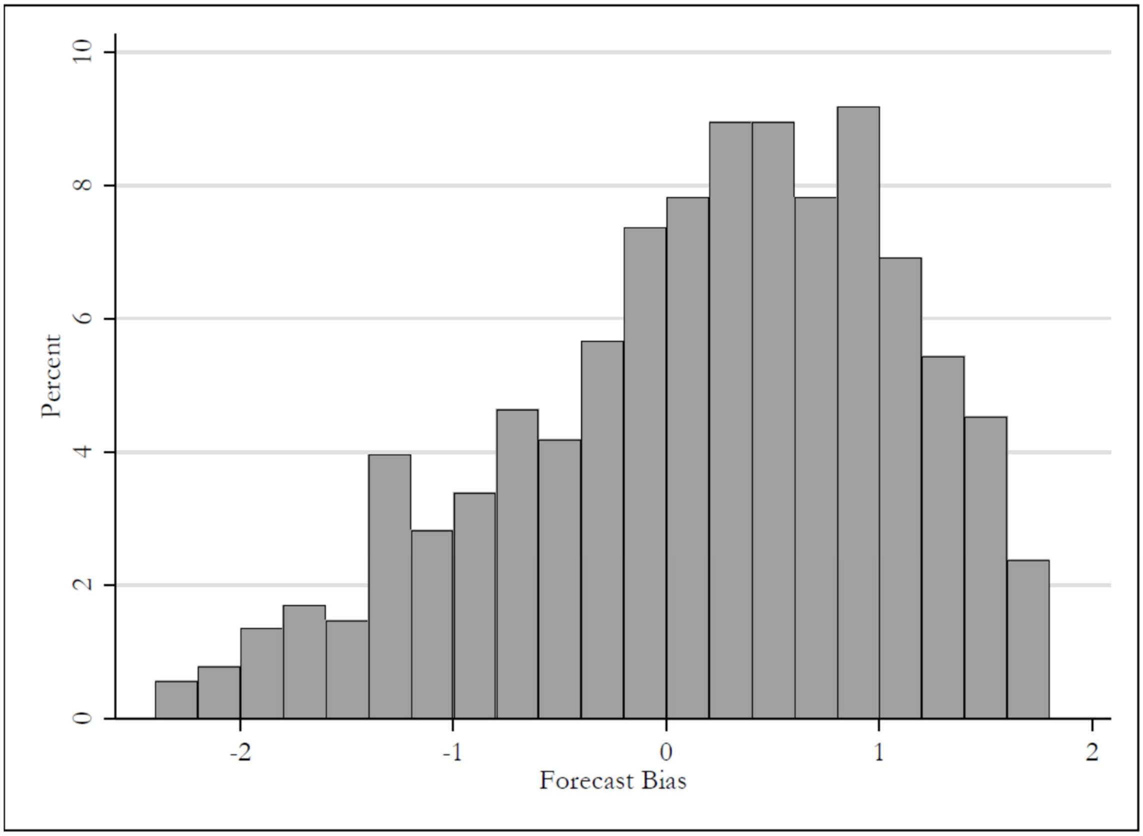

Table 1: Summary statistics

This table reports summary statistics. Appendix Table A1 defines all variables. For each variable, we report the mean and standard deviation. The column "quasi-median" reports the average value of the variable for subjects between the 45th and 55th percentile (this is done because our data agreement prohibits reporting non-aggregated values). Panels A, B, and C report summary statistics for the forecast bias measures, trading and stock characteristics, and control variables, respectively. The summary statistics for the control variables are regarding the year the experiment is conducted, 2020. All forecast bias measures in Panel A, except *Forecast Bias Rank*, are rescaled to have a standard deviation of one.

Panel A: Forecast Bias measures

	Mean	Std. dev.	Quasi-median
Forecast Bias	0.14	1.00	0.28
Forecast Bias Residual	0.00	1.00	0.14
Forecast Bias Limited Info	0.16	1.00	0.26
Forecast Bias Rank	0.50	0.29	0.50
Diagnostic Expectations	0.27	1.00	0.37
Sticky Expectations	-0.53	1.00	-0.33
Extrapolative Expectations	-0.70	1.00	-0.71
Adaptive Expectations	2.17	1.00	2.27

Panel B: Trading and stock characteristics

	Mean	Std. Dev.	Quasi-median
Number of buys	43.04	98.53	14.25
Value of buy	42,525	82,941	20,773
Prior annual excess ret. (buys)	2.6%	37.0%	-3.3%
Number of sales	36.07	82.77	11.41
Value of sale	59,877	100,758	32,204
Prior annual excess ret. (sales)	11.6%	38.4%	5.6%
Capital gain since purchase	31.3%	78.5%	13.5%
Net flows	0.21	1.97	0.02
Trade month	14.85%	18.22%	7.66%
Buy month	8.23%	11.15%	3.54%
Sell month	6.62%	8.63%	3.34%
Conditional net flows	-1.10	16.57	1.73

Panel C: Control variables			
	Mean	Std. dev.	Quasi-median
Age	50.56	7.87	52.52
Male	0.69	0.46	1
Married	0.64	0.48	1
Children	0.81	0.39	1
Education	16.45	2.20	16.98
Financial assets (000's)	2,432.8	16,112.57	855.07
Income (000's)	771.00	635.61	646.26
Housing assets (000's)	1,920.04	1,916.59	1633.44
Post-Covid experiment	0.34	0.47	0
Risk aversion	0.49	0.16	0.49
Financial literacy	3.40	0.80	4
Numeracy	2.83	0.43	3
Optimism	4.59	7.96	5
Overconfidence	0.19	0.91	0
Trust	4.23	1.55	5

Table 2: Forecast bias and stock purchases

This table reports the coefficients of OLS regressions in which the dependent variable is the lagged annual excess return of the stock purchased. Excess return is the stock's return less the value-weighted Danish stock market return and is winsorized at the 1st and 99th percentiles. The key independent variable is *Forecast Bias*, which is adjusted to have a standard deviation of one. The unit of observation is trade-level over the period 2011-2021, and the observations are weighted such that each person has equal weight in the regressions. Column (1) does not include any controls. Column (2) and (3) include age, male, married, children indicator, education, financial assets, income, housing assets, and post-Covid experiment indicator. Column (3) also includes risk aversion, financial literacy, numeracy, optimism, overconfidence, and trust. All columns include year-month fixed effects. Appendix Table A1 defines the variables. Standard errors are clustered at the individual-level and appear in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)
Forecast Bias	2.484**	2.583**	2.974**
	(1.253)	(1.270)	(1.258)
Age		-7.760	-8.698
		(8.263)	(8.328)
Male		1.187	2.390
		(3.020)	(2.987)
Married		6.435**	6.709**
		(2.749)	(2.753)
Children		-4.492	-4.524
		(3.110)	(3.102)
Education		-1.059	-0.936
		(0.660)	(0.702)
Financial assets		0.588	0.834
		(0.972)	(0.967)
Income		0.204	0.111
		(0.845)	(0.838)
Housing assets		-0.018	0.062
		(0.276)	(0.285)
Post-Covid experiment		2.069	2.126
		(2.665)	(2.687)
Risk aversion			-8.581
			(7.211)
Financial literacy			-1.769
			(2.282)
Numeracy			-5.731
			(4.328)
Optimism			0.113
			(0.165)
Overconfidence			-0.655
			(2.261)
Trust			0.442
			(0.742)
Year-Month Fixed Effect	Yes	Yes	Yes
N	29,268	29,268	29,268

Table 3: Forecast bias and stock sales

This table reports the coefficients of OLS regressions in which the dependent variable equals 100 if the stock is sold and zero otherwise. The unit of observation is person-stock-day, and the sample includes all stock holdings on days in which the person makes at least one sale. The observations are weighted such that each person has equal weight in the regressions. The sample period is 2013-2021. The key independent variables are *Forecast Bias x Performance measure*, where the performance measure is *Lagged annual excess return* in column (1), *Capital gain* in columns (2) and (3), and *Capital gain rank* in columns (4) and (5). Excess return is the stock's return less the value-weighted Danish stock market return and is winsorized at the 1st and 99th percentiles. *Capital gain* is the percentage change in price since purchase and is winsorized at the 1st and 99th percentiles. *Capital gain rank* is the within person-day rank of *Capital gain*. *Forecast Bias* is adjusted to have a standard deviation of one. All columns include controls for idiosyncratic risk and the stock's portfolio weight as well as person-by-day and monthly holding length fixed effects. Columns (3) and (5) include a control for lagged annual excess returns. Standard errors are clustered at the individual-level and appear in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Performance measure	Lag. excess return	Capital gain		Capital gain rank	
	(1)	(2)	(3)	(4)	(5)
Forecast Bias × Perform.	-0.457	-0.921**	-0.839**	-6.278***	-7.108***
	(0.738)	(0.410)	(0.401)	(2.186)	(2.173)
Performance measure	3.973***	-1.375***	-12.568***	-5.923*	-7.102*
	(0.885)	(0.463)	(2.290)	(3.426)	(3.807)
I(Capital gain > 0)		4.598***	9.902***	6.396***	6.430***
		(1.348)	(1.697)	(2.072)	(2.252)
Capital gain squared			5.072***		
			(0.929)		
Capital gain cubed			-0.454***		
			(0.090)		
$(CapGain\ rank - 0.5)^2$					36.797***
					(6.762)
Capital gain highest					0.068
					(2.442)
Capital gain lowest					1.012
					(2.605)
Person-Day Fixed Effect	Yes	Yes	Yes	Yes	Yes
Length of Holding F.E.	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes
N	163,832	181,889	162,730	181,889	162,730

Table 4: Alternative *Forecast Bias* measures

This table reports estimates for alternative forecast bias measures for purchases (Panel A) and sales (Panel B). The specification in Panel A is the same as in column (3) of Table 2. The specification in Panel B is the same as in column (5) of Table 3. In column (1), *Forecast Bias Residual* is generated using a person-specific estimated persistence parameter and standard deviation of the error term based on the 80 realizations of the stochastic process (see Section 1.2 for details). In column (2), *Forecast Bias Limited Information* is based on a subject-specific rational benchmark that is updated every round of the elicitation procedure using the realizations that subject has observed until that point in the experiment (see Section 1.2 for details). In column (3), *Forecast Bias Rank* is the rank transformation of *Forecast Bias*. In column (4), *Diagnostic Expectations* is estimated using the diagnostic expectations function of Bordalo, Gennaioli, and Shleifer (2018, eq. 3). In column (5), *Sticky Expectations* is estimated using the sticky expectations function (Afrouzi et al., 2023, eq. 8) and is multiplied by -1 to be directionally consistent with the other measures. In column (6), *Extrapolative Expectations* is estimated using the extrapolative expectations function (Afrouzi et al., 2023, eq. 6). In column (7), *Adaptive Expectations* is estimated using the adaptive expectations function (Afrouzi et al., 2023, eq. 5). Appendix Table A1 defines the variables. Standard errors are clustered at the individual-level and appear in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Purchases							
Forecast bias measure	Residual	Limited Info	Rank	Diagnostic	Sticky	Extrapolative	Adaptive
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Forecast Bias	3.284**	2.969**	9.395**	2.351*	2.150	1.362	2.466*
	(1.276)	(1.313)	(4.515)	(1.296)	(1.357)	(1.406)	(1.334)
Year-Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	29,268	29,268	29,268	29,268	29,268	29,268	29,268

Panel B: Sales							
Forecast bias measure	Residual	Limited Info	Rank	Diagnostic	Sticky	Extrapolative	Adaptive
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Forecast Bias × Capital gain rank	-6.909***	-6.806***	-22.382***	-6.505***	-4.761**	-4.494**	-5.645***
	(2.064)	(2.187)	(7.425)	(2.069)	(2.175)	(2.109)	(2.115)
Capital gain rank	-9.390**	-7.255*	1.952	-6.314	-11.045***	-11.477***	4.250
	(3.796)	(3.813)	(5.076)	(3.860)	(3.914)	(3.980)	(6.135)
I(Capital gain > 0)	6.435***	6.424***	6.356***	6.368***	6.323***	6.321***	6.358***
	(2.247)	(2.243)	(2.255)	(2.253)	(2.257)	(2.256)	(2.255)
$(CapGain\ rank\ -\ 0.5)^2$	36.657***	36.599***	36.719***	36.732***	36.472***	36.494***	36.583***
	(6.757)	(6.753)	(6.761)	(6.758)	(6.763)	(6.750)	(6.758)
Capital gain highest	0.096	0.157	0.135	0.103	0.208	0.189	0.185
	(2.440)	(2.438)	(2.441)	(2.442)	(2.456)	(2.444)	(2.449)
Capital gain lowest	1.040	0.980	0.936	0.956	0.874	0.890	0.893
	(2.604)	(2.602)	(2.604)	(2.601)	(2.601)	(2.599)	(2.599)
Person-Day Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Length of Holding F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	162,730	162,730	162,730	162,730	162,730	162,730	162,730

Table 5: Subsample analysis based on proxies for financial sophistication

This table reports estimates for subsamples of financially sophisticated investors for purchases (Panel A) and sales (Panel B). The specification in Panel A is the same as in column (3) in Table 2. The specification in Panel B is the same as in column (5) in Table 3. Column (1) includes only respondents with 16 or more years of education; column (2) includes only respondents that answer all four financial literacy questions correctly; and column (3) only includes respondents that answer all three numeracy questions correctly. Appendix Table A1 defines the variables. Standard errors are clustered at the individual-level and appear in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Purchases

	Highly educated subsample	High financial literacy subsample	High numeracy subsample
	(1)	(2)	(3)
Forecast Bias	3.655**	3.629**	2.471**
	(1.464)	(1.523)	(1.255)
Year-Month Fixed Effect	Yes	Yes	Yes
Control variables	Yes	Yes	Yes
N	19,310	18,479	24,572

Panel B: Sales

	Highly educated subsample	High financial literacy subsample	High numeracy subsample
	(1)	(2)	(3)
Forecast Bias x Capital gain rank	-5.796**	-6.259**	-8.039***
	(2.431)	(2.547)	(2.344)
Capital gains rank	-8.736*	-4.764	-3.361
	(4.642)	(4.545)	(3.856)
I(Capital gain > 0)	7.921***	2.855	5.872**
	(2.606)	(2.629)	(2.306)
Forecast Bias x (Capital gain rank – 0.5)^2	19.188**	19.181**	14.951**
	(8.112)	(9.008)	(7.458)
(Capital gain rank – 0.5)^2	36.872***	35.367***	32.034***
	(8.003)	(7.817)	(7.280)
Capital gain highest	-0.763	2.320	-2.221
	(2.839)	(2.935)	(2.546)
Capital gain lowest	-1.524	-3.751	2.023
	(3.040)	(3.088)	(2.745)
Person-Day Fixed Effect	Yes	Yes	Yes
Length of Holding F.E.	Yes	Yes	Yes
Control variables	Yes	Yes	Yes
N	119,042	111,609	142,542

Table 6: Net flows, trading, and conditional net flows

This table reports the results of OLS regressions of monthly net flows, trading activity, and conditional net flows during the period 2012-2021. In columns (1) and (5), the dependent variable is net flows, which is defined as the value of purchases less the value of sales divided by beginning of month portfolio value, and is winsorized at the 1st and 99th percentiles. In columns (2), (3), and (4), the dependent variables are indicators equal to 100 if, respectively, the absolute value of the subject's net flows is greater than 1%, the value of net flows is greater than 1%, and value of net flows is less than -1%. In columns (1) through (4), the sample includes all person-months in which the subject owns individual stocks. In column (5), the sample includes only person-months in which the absolute value of the subject's net flow is greater than 1%. In all columns, each observation is weighted by the inverse of the number of months the individual appears in the sample. Lag market return is the return on the Danish stock market index over the prior year. Lag excess return is the subject's stock return over the prior year less the lag market return. All columns include controls for financial assets, housing assets, income, education, children, and marital status, as well as individual and year-month fixed effects. Appendix Table A1 defines the variables. Standard errors are clustered at the individual-level and appear in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Net flows (1)	Trade month (2)	Buy month (3)	Sell month (4)	Conditional net flows (5)
Forecast Bias × Lag market return	0.854**	-0.739	-0.341	-0.398	6.121***
	(0.346)	(1.057)	(0.782)	(0.705)	(2.151)
Forecast Bias × Lag excess return	0.099	0.547	0.173	0.374	0.457
	(0.194)	(0.594)	(0.432)	(0.409)	(0.900)
Lag excess return	0.080	3.035***	1.656***	1.379***	-0.955
	(0.237)	(0.696)	(0.544)	(0.455)	(1.124)
Individual Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year-Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes
N	87,140	87,140	87,140	87,140	11,947

Table 7: Forecast bias and investor experiences

This table reports the coefficients of OLS regressions in which the dependent variable is *Forecast Bias*. The independent variables measure investor's experiences in the stock market. Appendix Table A1 defines the variables. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Forecast Bias	
	Coef.	Std.err.
Ratio contrarian buys success over fails (one week)	-0.017	(0.052)
Ratio contrarian buys success over fails (one month)	-0.032	(0.058)
Ratio contrarian buys success over fails (three months)	-0.059	(0.079)
Ratio contrarian buys success over fails (six months)	0.062	(0.092)
Ratio contrarian buys success over fails (12 months)	-0.030	(0.081)
Ratio extrapolator buys success over fails (one week)	0.066	(0.063)
Ratio extrapolator buys success over fails (one month)	0.008	(0.061)
Ratio extrapolator buys success over fails (three months)	0.032	(0.067)
Ratio extrapolator buys success over fails (six months)	-0.045	(0.055)
Ratio extrapolator buys success over fails (12 months)	-0.036	(0.049)
Ratio contrarian sells success over fails (one week)	-0.048	(0.047)
Ratio contrarian sells success over fails (one month)	0.134**	(0.054)
Ratio contrarian sells success over fails (three months)	-0.037	(0.048)
Ratio contrarian sells success over fails (six months)	0.012	(0.047)
Ratio contrarian sells success over fails (12 months)	-0.037	(0.023)
Average annual market return in stock holding years	0.586	(2.458)
Experienced 2001 crash	0.050	(0.155)
Experienced 2008 crash	0.018	(0.162)
Number of buys	-0.004	(0.003)
Number of sells	0.004	(0.003)
Never buy indicator	-0.059	(0.115)
Never sell indicator	-0.208	(0.132)
Years of trading experience (since 1997)	-0.009	(0.016)
Controls	Yes	
N	736	

Table 8: Forecast bias and investment performance

This table reports the coefficients of regressions that examine the investment performance of our subjects' individual stock holdings during the sample period 2011-2021. For each subject, we create the time-series of their monthly value weighted stock returns less the risk-free rate. Subjects are divided into three groups based on the value of their *Forecast Bias*. In both panels, we form a value weighted portfolio for each investor. Panel A reports results in which the investors are equal weighted and Panel B reports results in which the investors are value weighted. The coefficients are from a CAPM regression in which the independent variable is the return on the Danish stock market index less the risk-free rate. Standard errors are calculated using the Newey-West correction with three lags and appear in parentheses. The symbol * denotes significance at the 10% level.

Panel A: Equal weighted investors

	Forecast Bias ≤ -0.5	-0.5 < *Forecast Bias* ≤ 0.5	*Forecast Bias* > 0.5
	(1)	(2)	(3)
α	-0.003*	-0.003	-0.003
	(0.002)	(0.002)	(0.002)
β_M	0.974***	0.952***	0.972***
	(0.049)	(0.055)	(0.057)
N	132	132	132

Panel B: Value weighted investors

	Forecast Bias ≤ -0.5	-0.5 < *Forecast Bias* ≤ 0.5	*Forecast Bias* > 0.5
	(1)	(2)	(3)
α	-0.001	-0.002	-0.003**
	(0.005)	(0.002)	(0.001)
β_M	0.953***	0.946***	0.940***
	(0.117)	(0.043)	(0.040)
N	132	132	132

Appendix Table A1: Variable definitions

Variable name	Definition
Forecast Bias	The forecast bias parameter estimated as in equation (2). For ease of interpretation, we divide the parameter estimate it by its standard deviation.
Forecast Bias Residual	The residual from regressing *Forecast Bias* on the person-specific empirical persistence parameter and standard deviation of the error term based on the full set of 80 realizations. For ease of interpretation, we divide the parameter estimate by its standard deviation.
Forecast Bias Limited Information	The forecast bias limited information parameter estimated as in equations (3) and (4). For ease of interpretation, we divide the parameter estimate by its standard deviation.
Forecast Bias Rank	Rank transformation of *Forecast Bias*, where zero indicates the lowest level of *Forecast Bias* and one indicates the highest.
Diagnostic Expectations	The diagnostic expectations parameter estimated as in equation (3) in Bordalo, Gennaioli, and Shleifer (2018). For ease of interpretation, we divide the parameter estimate by its standard deviation.
Sticky Expectations	The sticky expectations parameter estimated as in equation (8) in Afrouzi et al. (2023). For ease of interpretation, we divide the parameter estimate by its standard deviation. We multiply the parameter by -1 so that it is directionally consistent with the other forecast bias measures.
Extrapolative Expectations	The extrapolative expectations parameter estimated as in equation (6) in Afrouzi et al. (2023). For ease of interpretation, we divide the parameter estimate by its standard deviation.
Adaptive Expectations	The adaptive expectations parameter estimated as in equation (5) in Afrouzi et al. (2023). For ease of interpretation, we divide the parameter estimate by its standard deviation.
Prior annual return	The return on the purchased stock over the prior year ending the day before purchase
Prior annual market return	The return on the value-weighted Danish stock market over the prior year ending the day before purchase
Prior annual excess return	The difference between the prior annual return of the stock and the prior annual market return
Capital gain	The percentage change in the value of the position relative to its purchase price
Capital gain rank	The within person-day rank variable of *Capital gain*, where 0 indicates the lowest capital gain stock and 1 the highest.
Capital gain highest	Indicator variable equal to one for the subject's stockholding with the highest capital gain on that day

Capital gain lowest	Indicator variable equal to one for the subject's stockholding with the lowest capital gain on that day
Net flows	Value of stock purchases in a month less the value of stock sales divided by the beginning of month value of stocks owned (multiplied by 100)
Trade month	Indicator variable equal to 100 in months in which the absolute value of the investor's net flow into stocks is greater than 1% of the beginning of month portfolio value
Buy month	Indicator variable equal to 100 in months in which the value of the investor's net flow into stocks is greater than 1% of the beginning of month portfolio value
Sell month	Indicator variable equal to 100 in months in which the value of the investor's net flow into stocks is less than -1% of the beginning of month portfolio value
Ratio contrarian buys success over fails (X week/months)	Ratio of the number of contrarian buys that generated positive excess returns over the subsequent X week/months divided by the number of contrarian buys that generated negative excess returns over the subsequent X week/months. A contrarian buy is defined as a stock purchase that had negative excess returns over the past X week/months. These variables are generated using data from 2011 until the date of the experiment.
Ratio extrapolator buys success over fails (X week/months)	Ratio of the number of extrapolator buys that generated positive excess returns over the subsequent X week/months divided by the number of extrapolator buys that generated negative excess returns over the subsequent X week/months. An extrapolator buy is defined as a stock purchase that had positive excess returns over the past X week/months. These variables are generated using data from 2011 until the date of the experiment.
Ratio contrarian sells success over fails (X week/months)	Ratio of the number of contrarian sells that generated negative excess returns over the subsequent X week/months divided by the number of contrarian sells that generated positive excess returns over the subsequent X week/months. A contrarian sell is defined as a stock sale that had positive excess returns over the past X week/months. These variables are generated using data from 2011 until the date of the experiment.
Number of buys	Number of buy trades from 2011 until the date of the experiment
Number of sells	Number of sell trades from 2011 until the date of the experiment
Never buy indicator	Indicator variable equal to 1 if the subject never bought a stock between 2011 and the date of the experiment.
Never sell indicator	Indicator variable equal to 1 if the subject never sold a stock between 2011 and the date of the experiment.
Years of trading experience (since 1997)	Number of years the subject holds stocks from 1997-2020

Experienced 2001 crash	Indicator variable equal to 1 if the subject held equity between 2000 – 2002
Experienced 2008 crash	Indicator variable equal to 1 if the subject held equity between 2007 – 2009
Income	The natural logarithm of the sum of labor income, social transfers, pension income, income from investments, and other personal income, reported in Danish kroner (DKK)
Financial assets	The natural logarithm of the sum of stocks, bonds, and deposit accounts (DKK)
Housing assets	The natural logarithm of the value of the subjects' home (DKK)
Age	The natural logarithm of age in years
Education	Years of formal education
Male	Indicator for male
Married	Indicator if subject is currently married
Children	Indicator for whether the subject has children
Risk aversion	Fraction of paired lottery choice questions for which the subject chose the safer option
Financial literacy	Number of the four financial literacy questions answered correctly
Numeracy	Number of the three numeracy questions answered correctly
Optimism	Subjects' stated life expectancy less objective life expectancy from actuarial tables
Overconfidence	The sum of financial literacy and numeracy questions the subject believes they answered correctly less the number they actually answered correctly
Trust	Likert scale where zero indicates "Most people can be trusted" and six indicate "One has to be very careful with other people"
Post-Covid experiment	Indicator for subjects whose experimental session was in November 2020