Extrapolators and Contrarians: Forecast Bias and Individual Investor Stock Trading*

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Abstract

We test whether forecast bias affects individual investors' stock trading by combining bias measures from laboratory experiments with administrative trade-level data. On average, subjects exhibit positive forecast bias (extrapolators), while a large minority exhibit negative bias (contrarians). Forecast bias is positively associated with past excess returns of purchased stocks: Extrapolators (contrarians) purchase past winners (losers). Forecast bias is negatively associated with capital gains of sold stocks. Forecast bias explains investor heterogeneity in the relation between market returns and net flows to stocks. Our study shows that forecast bias links past returns to trading decisions for purchases, sales, and net flows.

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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 heterogeneity across investors in how past returns affect their expectations.[1] Numerous studies further show that investors' expectations of market returns predict their allocations to risky asset classes.[2]

A natural question arising from the 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 them to administrative records of our subjects' stock trading decisions. We then test how forecast bias affects stock selection for purchases and sales, 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 subjects are eligible to win monetary prizes based on their forecast accuracy. At the start, each subject observes 40 realizations of the process and is then asked to forecast the next realization. After making this initial forecast, they observe the next realization of the process and then forecast the next realization. This

[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 (2023), and Laudenbach, Weber, Weber, and Wohlfart (2024) show that investors' stock market expectations predict asset allocation decisions.

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 provided, allowing us to measure variation in how subjects process information. By focusing on measuring information processing, rather than information itself, we obtain a single parameter that is widely applicable across different securities, time periods, and decision types.

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 following high realizations and too low following 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 following high realizations and too high following 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.

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 subjects.

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 test how subjects' biases interact with past stock performance to affect stock selection decisions. In these tests, we focus solely on days when a subject trades, and for each of these investor-days we construct the set of individual stocks the investor could plausibly trade. Our key regression specification explains the actual stock traded using an interaction between the investor's *Forecast Bias* and that individual stock's past performance, while including investor-day fixed effects. This approach allows us to examine how *Forecast Bias* alters the effect of an individual stock's past performance on trading decisions, after removing all variation common to the investor on that day. The investor-day fixed effects controls for aggregate market performance and removes time-variant (and time-invariant) investor characteristics, such as past experiences, risk preferences, and wealth effects. By isolating the interaction between *Forecast Bias* and each individual stock's past return relative to other stocks available to trade, we can identify how a subject's forecast bias shapes their stock selection among the set of feasible investment opportunities.

First, we test whether *Forecast Bias* affects stock purchase decisions. The results show that individuals' biases are related to the past performance of the stocks they buy: extrapolators tend to buy stocks with high past returns and contrarians tend to buy stocks with poor past returns. Supplementary tests reveal that a one-standard deviation increase in *Forecast Bias* implies purchasing stocks with a 3.0 percentage point higher past one-year return compared to stocks bought by other investors in the same period.

Second, we test whether *Forecast Bias* affects stock sale 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 6.1 percentage point reduction in the odds that a subject sells a stock with a one standard deviation higher capital gain, relative to the baseline probability.

We show that the results for purchases and sales are robust across several alternative specifications. First, the results are similar when using alternative measures of forecast bias, including diagnostic expectations (Gennaioli and Shleifer, 2010; Bordalo, Coffman, Gennaioli and Shleifer, 2016; Bordalo, Gennaioli, La Porta, and Shleifer, 2019), sticky expectations (Woodford, 2003), and adaptive expectations (Cagan, 1956). Second, the results are robust when using alternative time-horizons for measuring past returns. Finally, to alleviate concerns about reverse causality, we show that the results are similar when analyzing only trades conducted after the subjects have participated in the experiment.

While our main set of results tests 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.

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.

We contribute to the literature on individual investors' stock market expectations[3] and, in particular, to those studies that focus on the role of expectations in allocations to risky asset classes (e.g., see Vissing-Jorgensen, 2003; Malmendier and Nagel, 2011; Giglio, Maggiori, Stroebel, and Utkus, 2021) and portfolio turnover (Liu, Peng, Xiong, and Xiong, 2022).[4] Our findings are also closely related to Laudenbach, Weber, Weber, and Wohlfart (2024), 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, as well as Beutel and Weber (2023), who use an information experiment to show that beliefs affect risky asset allocations. Our study differs from the prior literature in that we directly measure biases in forecasting in a controlled laboratory setting instead of directly measuring beliefs (and inferring biases from stated

[3] Prior studies use survey data and show a positive relation between past returns and investors' stated expectations of future returns on the aggregate market (De Bondt, 1993; Fisher and Statman, 2000; Vissing-Jorgensen, 2003; Malmendier and Nagel, 2011; Amromin and Sharpe, 2014; Greenwood and Shleifer, 2014) or investors' stated expectations of future returns on individual stocks (Da, Huang, and Jin, 2021). Across investors there is significant heterogeneity in how individual investors incorporate past returns into their expectations (Dominitz and Manski, 2011; von Gaudecker and Wogrolly, 2022; Laudenbach, Weber, Weber, and Wohlfart, 2024). Zhao (2020) tests how investors holdings of stocks that have exhibited trends relates to future trading activity.

[4] In the Online Appendix, Liu, Peng, Xiong, and Xiong (2022) examine how extrapolation beliefs relate to past returns of purchases, but their tests pool market timing and cross-sectional security selection decisions.

beliefs). This approach allows us to study individual stock selection decisions, instead of portfolio allocations to equities as an asset class, and we are the first to show direct evidence linking laboratory elicited biases in expectation formation to individual stock purchase and sale decisions.

Our study also contributes to the literature on how past returns affect individual investor decisions. Prior studies show that different past return measures affect different types of investment decisions. The decision to purchase a stock is related to that stock's past return (Grinblatt and Keloharju, 2000; Barber and Odean, 2008). The decision to sell a stock is related 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 related to past market returns (Greenwood and Shleifer, 2014). Our study contributes to this literature by showing that a single mechanism, forecast bias, affects how these different past return measures affect different types of investment decisions.

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.[5] Our study complements these studies by showing a direct empirical relation between individual-level elicited biases in expectation formation and individuals' stock selection decisions.

Our paper demonstrates the relevance of combining lab experiments with administrative data on financial decisions to study belief formation and its relation to real-world choices. The

[5] 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; Cassella and Gulen, 2018; Bordalo, Gennaioli, La Porta, and Shleifer, 2019; Cassella, Chen, Gulen, and Petkova, 2022; Atmaz, Cassella, Gulen, and Ruan, 2023; Jin and Peng, 2023). See Barberis (2018) and Adam and Nagel (2022) for reviews of the literature on expectations and asset pricing.

controlled environment of a laboratory setting enables clean measurement of relevant parameters, while the connection to actual financial decisions ensures external validity. By integrating experimental and administrative field data, we gain a deeper understanding of the heterogeneity in belief formation and its effect on important financial 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., 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.

1.1 The Elicitation Procedure

We develop an experimental module that includes a task to elicit individuals' forecast bias.[6] 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:

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

[6] See Online Appendix A for the complete instructions of the forecasting experiment.

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.[7] 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). Landier, Ma, and Thesmar (2019) show that forecast biases are unaffected both when subjects are informed about the underlying process and when different labels are attached to the process. For a fixed stochastic process, subjects exhibit similar biases regardless of whether the process is labelled a "stable random process" or given an economic context (GDP growth, CPI, stock returns, or house price growth). Similarly, Frydman and Nave (2016) use a within-person design to show that subjects who exhibit extrapolative biases in a stock market experiment also exhibit similar biases in perceptual tasks, and argue that extrapolative beliefs stem from low-level perceptual processes instead of deliberative analytical judgement.

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 whether 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] 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. Subjects are not informed about the true data-generating process.[8] 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 and prevent risk aversion from affecting forecasts, 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: $100 - 5 \times |forecast_{i,t} - realization_{i,t}|$. If the forecast differs from the realized value by more than

[8] Afrouzi et al. (2023) show that informing a sample of MIT engineering undergrads that that the underlying data generating process is an AR(1) does not significantly alter their elicited biases, suggesting uncertainty about the true data generating process is unlikely to drive the observed forecast bias.

[9] The actual experiment is conducted in Danish. The caption "Værdin over tid" translates to "value over time." Online Appendix A contains an English translation of the complete instructions of the forecasting experiment.

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.

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

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[10] At the time of our experimental sessions, one kroner was worth between U.S. \$0.15-\$0.16 and €0.13.

[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.

and investor-specific empirical persistence parameter in the full set of 80 realizations. A second alternative measure, *Forecast Bias Limited Information*, incorporates that the subjects do not know the true data generating process, but can estimate it using all prior realizations of the process. At any given point in time, *t*, the within-sample least-squares estimate of the rational forecast is:

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

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 observed by subject *i* from period 0 through *t*. We then estimate the limited information forecast bias as:

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

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.

Our main measure does not require us to assume a specific model of expectation formation. As a robustness test, 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). 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]

[12] The specifications of the alternative measures of forecast bias are the same as in Afrouzi et al. (2023), except we multiply Sticky Expectations by -1 so that it is directionally consistent with the other measures. See Online Appendix B for the exact specifications.

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 later in this section. Statistics Denmark provides demographic, economic, and financial data, including stock holdings as well as trading records 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 Recruitment 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 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 invites 24,821 individuals to participate in our study.

[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 C.1 compares experiment participants and nonparticipants 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. For trading, sample participants trade slightly more and purchase stocks with slightly lower returns relative to the nonparticipants.

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 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

[14] The average *Forecast Bias* is not significantly different pre- and post-Covid.

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 (2024).

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 highly correlated (see Online Appendix Table C.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 uses 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.

Online Appendix Table C.3 shows summary statistics of the relation between *Forecast Bias* and individual characteristics. We define contrarians as subjects with *Forecast Bias* smaller than -0.5, unbiased as subjects with *Forecast Bias* between -0.5 and 0.5, and extrapolators as subjects with *Forecast Bias* larger than 0.5. Some individual characteristics are statistically different, but the magnitudes of the differences are small. Overall, *Forecast Bias* has little relation with economic and demographic characteristics, preferences, and proxies of cognitive abilities.[15] The latter is particularly interesting because *Forecast Bias* measures errors relative to a statistically optimal benchmark. Yet proxies for the cognitive abilities of contrarians and extrapolators are generally similar to those of unbiased subjects.

Conceptually, 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., forecasts that are consistently too high following high realizations but also too low following low realizations).

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.[16] Danish tax law requires third parties to report information on income, wealth, and trading directly to SKAT. For example, banks and

[15] Online Appendix Table C.3 shows statistically significant differences in proxies for quantitative reasoning ability for different levels of *Forecast Bias*, but the magnitudes of the differences are small.

[16] For example, Grinblatt and Keloharju (2000), Kaustia and Knüpfer (2008), and Knüpfer, Rantapuska, and Sarvimäki (2017, 2023) 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.

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, including both domestic and international stocks.[17] 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%) participants in the experiment purchase at least one stock between 2011 and 2021 and a total of 583 of the 959 (61%) individuals sell at least one stock between 2011 and 2021. Panel B of Table 1 summarizes the purchases and sales. On average, subjects make 44 purchases and 33 sales, for a total of 50,298 unique trades.[18] The distribution of trading activity is highly skewed, with the 52 most active traders making about half of total trades. To ensure that our results are not driven by a few extremely active investors, our empirical specifications weight subjects equally. The average purchase has a value of 59,293 Danish kroner (€7,708) and the average sale is 82,819 kroner (€10,766). 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.

[17] In our sample of trades, 56.6% of purchases and 59.7% of sales between 2011 and 2021 are of Danish stocks.

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

For each security in our regression samples, we calculate returns using daily data for the year ending the day before a purchase or sale. We report averages including returns of both the traded stocks as well as the stocks included in the consideration set of individuals but ultimately not traded. The average lagged annual return in our purchase sample is 26.2%, but returns are positively skewed and the quasi-median is 8.9% (for comparison, the average lagged annual return for the Danish stock market is 20.0% for our sample). The average lagged annual return in our sales sample is 29.3%, and the quasi-median is 13.5%. We also calculate the capital gain since purchase for the stocks in our sample.[19] The average capital gain is 23.1% and the quasi-median is 3.4%.

3. Forecast Bias and Stock Trading Decisions

3.1 Forecast Bias and Stock Purchases

We hypothesize that *Forecast Bias* affects how investors react to stock performance. Specifically, extrapolators purchase stocks that recently performed well and contrarians purchase stocks that recently performed poorly. We test how *Forecast Bias* and past returns interact to affect which stock is purchased by the investor. We examine purchase decisions at the investor-stock-day level, and limit our sample to include only days in which the investor makes at least one purchase (Section 4 considers the issue of whether to trade). These tests require data on both the stocks the investor purchases and those they do not.

Individual investors have limited time and attention and are unlikely to evaluate all of the thousands of available stocks. Instead, each investor will focus on a smaller set of stocks that have captured their attention via news coverage, financial advice, or conversations with friends and family. This set of stocks forms the investor's consideration set. Ideally, for each

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[19] 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.

purchase we would know this consideration set. However, as the consideration set remains unobserved, we construct a proxy set that includes the purchased stock and all stocks meeting the following requirements: (1) The stock is traded at least once by one of our subjects during the sample period. (2) At least one investor in Denmark (considering all investors in the country, not just those in our sample) must have purchased the stock during the month. This creates a comprehensive proxy consideration set, containing all plausibly considered stocks, but is likely overinclusive containing far more stocks than the investor actually considers: for an average investor-purchase day, there are 1.2 purchased stocks and 1,362.9 stocks in the proxy consideration set.

Using this sample, we estimate the following linear probability model:

$$Purchase_{i,j,t} = \beta_1 \cdot ForecastBias_i \cdot Perform_{j,t} + \beta_2 \cdot Perform_{j,t} + \boldsymbol{\delta X_{i,j,t}} + \theta_{i,t} + \varepsilon_{i,j,t} \quad (5)$$

where $Purchase_{i,j,t}$ is an indicator variable equal to 100 if subject i buys stock j on date t. $Perform_{j,t}$ is the lagged annual return of stock j as of the end of the prior trading day, winsorized at the 1st and 99th percentiles to ensure the results are not driven by outliers. $\boldsymbol{X_{i,j,t}}$ is a matrix of control variables and $\theta_{i,t}$ is an investor-day fixed effect. The unit of observation is investor-stock-day. Because trading activity is highly skewed, we estimate weighted-least square regressions such that each subject receives equal weight. The standard errors reported below the coefficients are clustered at the investor level.

Including the investor-day fixed effect results in a specification that closely matches the investor's decision. Conditioning on the investor's decision to trade that day, the fixed effect removes all variation common to that investor and the market on that day. This leaves only the variation across stocks and the investor's reaction to that variation. The investor-day fixed effect removes several potential confounding issues with the interpretation. First, it removes the overall market performance, and thus the regressions examine the likelihood an investor chooses to purchase a stock given its performance relative to other stocks available for purchase at that point in time. Thus, we examine the relation between forecast bias and security selection, without confounding this relation with market timing. Second, it removes the direct effect of any investor characteristic at that point in time, such as wealth, risk-aversion, or past experiences.

We include several variables to control for the likelihood of individuals to purchase certain stocks, irrespective of past return. $Held\ before_{i,j,t}$ and $Current\ holding_{i,j,t}$ are indicator variables equal to one if subject *i* has ever held stock *j* and if subject *i* currently holds stock *j*, respectively. In our sample, 55.3% of purchases are of stocks that the investor either currently holds or has previously held, and 41.9% are of stocks currently held. $Portfolio\ weight_{i,j,t}$ is the weight of a current holding in the portfolio (and zero for stocks not currently held). $Stock\ purchase\ fraction_{j,m-1}$ is constructed from the full sample of all Danish investors. It is defined as the percentage of all investor-stock purchases in the preceding month, *m-1*, that were in stock *j*. This variable measures the popularity of stock *j* among Danish investors.

As discussed earlier, we hypothesize that the coefficient on the interaction term, $ForecastBias_i \cdot Perform_{j,t}$, will be positive. Extrapolators, with a positive forecast bias, will buy high performers, and contrarians, with a negative *Forecast Bias*, will buy poor performers. Consistent with the predictions of theory, the coefficient in column (1) of Table 2 is positive and significant.[20]

[20] Our main specification uses lagged one-year returns as the performance measure. 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 on past returns and individual investors' decisions (e.g., see Barber and Odean, 2002; Laudenbach, Weber, Weber, and Wohlfart, 2024). As a robustness test, we evaluate lagged returns over three, six, and 36-month time-periods. The results in Online Appendix Table C.4 show that the coefficient on the interaction term is significant for the six and 36-month periods.

Column (2) of Table 2 reports coefficients from a conditional logit regression that conditions out investor-day effects. Aside from the different estimation method, the specification is the same as in column (1). The result implies that subjects with a one standard deviation higher level of forecast bias have 5.3% higher odds of buying a stock with a one standard deviation higher past return, relative to the baseline probability.

Forecast Bias is designed to capture how people process information to form expectations. Although we do not know exactly what information our subjects process (e.g., past returns, media coverage, etc.), lagged 12-month stock returns should convey the general direction of this information. Our results are consistent with this interpretation: extrapolators (contrarians) purchase stocks whose returns reveal recent positive (negative) information. Moreover, any alternative interpretation must account for the monotonic relationship between our measure and the past returns of purchases, as well as for the U-shaped relation between *Forecast Bias* and forecast error. *Forecast Bias* quantifies *directional* errors from the rational benchmark: low values indicate excessive contrarianism while high values indicate excessive extrapolation. Thus, people with both high and low values of *Forecast Bias* deviate from rationality, but the absolute magnitude of deviations from rationality cannot explain our results. Instead, our findings require that individuals who are consistently contrarians (extrapolators) during the experiment are also contrarians (extrapolators) as investors. That is, our findings require directionally consistent deviations from rationality in both the experimental and trading domains.

Table 3 reports an alternative specification examining the relation between *Forecast Bias* and stock purchase decisions. In addition to providing a robustness test of the baseline specification, this new specification allows us to quantify the economic magnitude of the relation in returns rather than probabilities. Specifically, we limit the sample to include only investor-stock-day purchases, use the stock's excess return as the dependent variable, and estimate the following specification:

$$Prior\ annual\ excess\ return_{i,j,t} = \beta_1 \cdot Forecast\ Bias_i + \boldsymbol{\delta X_{i,t}} + \theta_m + \varepsilon_{i,t} \quad (6)$$

where $Prior\ annual\ excess\ return_{i,j,t}$ is the lagged annual excess return of stock j purchased on day t by subject i, winsorized at the 1st and 99th percentiles, $\boldsymbol{X_{i,t}}$ is a matrix of control variables, and θ_m is a month fixed effect.[21]

Column (1) of Table 3 does not include the control variables while column (2) does. In both columns the coefficient on *Forecast Bias* is positive and significant. This is consistent with the results from the baseline specification – higher forecast bias is associated with purchasing stocks with higher past returns. The coefficient estimate in column (2) implies that a one standard deviation increase in *Forecast Bias* is associated with buying stocks that had 3.0 percentage points higher excess returns over the past year.

3.2 Forecast Bias and Stock Sales

Conceptually, the relation between *Forecast Bias* and stock sales mirrors that of purchases: Contrarians prefer to sell high performers and retain low performers, while extrapolators 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 almost exclusively selected from the more limited set of existing holdings.[22] The mean (quasi-median) number of individual stocks held at the time of a sale is 11.5 (8.5). This enables

[21] This specification does not include an investor or investor-day fixed effect, as it would subsume the coefficient of interest. Accordingly, we include a much larger set of control variables than in equation (5). The control variables largely follow Dimmock, Kouwenberg, Mitchell, and Peijnenburg (2016, 2021) and include: age, male, married, children indicator, education, financial assets, income, housing assets, post-Covid experiment indicator, risk aversion, financial literacy, numeracy, optimism, overconfidence, and trust. See Appendix Table A1 for definitions of the control variables.

[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).

investors to easily compare performance across all of their holdings, providing a clearly defined consideration set. Thus, for the sale regressions we limit the sample to include only stocks that are held by the investor as of the end of the previous day.

For sales, we estimate a linear probability model similar to that in Eq. (5) for purchases:

$$Sale_{i,j,t} = \beta_1 \cdot ForecastBias_i \cdot Perform_{i,j,t} + \beta_2 \cdot Perform_{i,j,t} + \boldsymbol{\delta X_{i,j,t}} + \theta_{i,t} + \theta_l + \varepsilon_{i,j,t} \quad (7)$$

where $Sale_{i,j,t}$ is an indicator variable equal to 100 if subject *i* sells stock *j* on date *t* and $Perform_{i,j,t}$ is a measure of investor *i*'s performance on stock *j* as of the end of the prior trading day (e.g., capital gain since purchase), winsorized at the 1st and 99th percentiles to ensure the results are not driven by outliers.[23] $\boldsymbol{X_{i,j,t}}$ is a matrix of control variables, $\theta_{i,t}$ is an investor-day fixed effect and θ_l is a fixed effect for the length of the holding in months. We limit the sample to include only days when the subject sells at least one stock. The unit of observation is investor-stock-day. Because trading activity is highly skewed, we estimate weighted regressions such that each subject receives equal weight. The standard errors reported below the coefficients are clustered at the investor level.

Including the investor-day fixed effect means the specification examines the likelihood that an investor chooses to sell a stock *relative* to that of other stocks held in the portfolio. We include a length of holding period fixed effect as prior studies show a strong relation between length of holding and sales (e.g., see Ben-David and Hirshleifer, 2012; Hartzmark, 2015). We include two control variables. $Portfolio\ weight_{i,j,t}$ is the weight of stock *j* in investor i's portfolio. $Stock\ sales\ fraction_{j,m-1}$ is constructed from the full sample of all Danish

[23] 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.

investors, and is defined as the percentage of all investor-stock sell trades in the preceding month, *m-1*, that were in stock *j*.[24]

Table 4 reports regression results for sales and considers two different performance measures.[25] In column (1), following the specification in the purchase regressions, the performance measure is the lagged annual return. In this specification, the coefficient on the interaction term between performance and *Forecast Bias* is not significant.

In columns (2) and (3), the performance measure is capital gain since purchase. Column (2) reports coefficients from a linear probability model and column (3) reports coefficients from a conditional logit regression. *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 – and a large literature shows that capital gains are strongly related to sales decisions (e.g., Odean, 1998 Ben-David and Hirshleifer, 2012; Hartzmark, 2015). Further, capital gains are salient and typically easy to observe in brokerage accounts, making it likely investors pay attention to them and incorporate them into their forecasting process (Frydman and Rangel, 2014; Frydman and Wang, 2020).

In both columns, the coefficient on the interaction term between *Forecast Bias* and capital gain is negative and significant.[26] 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. The result in column (3) implies that subjects

[24] The purchase regressions also include $Held\ before_{i,j,t}$ and $Current\ holding_{i,j,t}$ as controls. We do not include these variables in the sale regressions because the sample includes only stocks the investor currently holds.

[25] Column (1) includes the stocks' lagged returns over the past year as an independent variable and this causes the loss of observations for which we do not observe a full year of returns.

[26] The coefficients on the performance measures, lagged annual returns and capital gains, are difficult to interpret as the relation with sales is non-linear and non-monotonic. In particular, investors are both more likely to sell their highest performers as well as their lowest performers (Ben-David and Hirshleifer, 2012; Hartzmark 2015). Online Appendix Table C.5 shows that our results are robust to including additional terms capturing the non-linear and non-monotonic relation between performance and sales propensity, as well as additional variables included in prior studies examining the disposition effect.

with a one standard deviation higher level of forecast bias have a 6.1% lower probability of selling a stock with a one standard deviation higher capital gain, relative to the baseline probability.

Our findings complement the literature on the relation between capital gains and investor sales decisions. Much of this literature focuses on the disposition effect: the finding that, on average, investors are more likely to sell winners than losers. The leading explanations for this pattern are preference-based, such as the realization utility model of Barberis and Xiong (2012). In contrast, our study does not attempt to explain the disposition effect and instead focuses on beliefs[27] about returns rather than preferences. Our results are consistent with Ben-David and Hirshleifer (2012), who argue that capital gains affect beliefs about future returns, which in turn affect sales decisions. We contribute to the literature on beliefs and stock sales, by showing that *Forecast Bias*, which captures the belief updating process, affects the direction in which capital gains affect sales decisions. Note that our findings on forecast bias do not contradict the importance of preferences in determining sales decisions. It is entirely possible that preferences, all else equal, drive people to hold losers and sell winners. However, preferences are the not only factor affecting sales decisions; other factors, such as return expectations, also matter. In particular, we show that forecast bias interacts with past stock performance to affect both purchase and sale decisions.

3.3 Alternative Measures of Forecast Bias

Table 5 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

[27] 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. Liao, Peng, and Zhu (2022) show that the interaction between extrapolative beliefs and the disposition effect can explain time-series patterns in aggregate stock market returns and trading volume. Our use of investor-day fixed effects removes the effect of beliefs about the aggregate market.

that in column (1) of Table 2 and the specifications for the sales analyses in Panel B are identical to that in column (2) of Table 4.

Columns (1) and (2) in Table 5 show results for alternative measures of *Forecast Bias* based on the investor-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 coefficients are similar to those in the main specification, albeit for sales one is statistically insignificant.

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 *Extrapolative Expectations* is not significant in the sales analysis.

3.4 Forecast Bias and Post-Experiment Trading

Our prior tests relate *Forecast Bias* to purchase and sales decisions made both before and after the experiment. This raises a potential concern of reverse causality; for example, if learning from pre-experiment trading influenced the subjects' responses in the elicitation procedure. Even before examining the data, a learning story seems unlikely, as tests in Section 5 show no relation between *Forecast Bias* and investment performance. Nevertheless, as a robustness test, Table 6 reports results in which we estimate our main analyses on the subset of trades made after the lab experiments. Except for the change in sample, the regressions for purchases in column (1) and sales in column (2) are identical to the baseline specifications. In both columns, the results are similar to the baseline results.

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 much of the literature that examines surveys of investors' expectations, and focuses on the *time-series* of beliefs about stock market index returns (e.g., Amromin and Sharpe, 2014; Greenwood and Shleifer, 2014; Adam, Matveev, and Nagel, 2021; Giglio, Maggiori, Stroebel, and Utkus, 2021). Most closely related to our study, Laudenbach, Weber, Weber, and Wohlfart (2024) 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 7 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:

$$\begin{aligned} Net\ Flows_{i,t} = {} & \beta_1 \cdot MarketRet_t \cdot ForecastBias_i \\ & + \beta_2 \cdot ExcessRet_{i,t} \cdot ForecastBias_i \\ & + \beta_3 \cdot ExcessRet_{i,t} + \boldsymbol{\delta X_{i,t}} + \theta_i + \theta_t + \varepsilon_{i,t} \end{aligned} \tag{8}$$

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 investor fixed effects, which subsume the direct effect of *Forecast Bias* and control for the investors' savings rates, general trading tendencies, etc. The control variables, $\boldsymbol{X_{i,t}}$, include the value of beginning of month stock holdings, financial assets, housing assets, income, education, children, and marital status. The specification also includes year-month fixed effects, which control for overall market returns, 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 or biased self-attribution – people may increase allocations to stocks following

[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.

high excess returns – but any effect from the investor's own 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 of trade.[29] 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 7 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%.[30] 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.[31] 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.

[29] Andries, Bianchi, Huynh, and Pouget (2022) show that signal precision affects forecast bias and the magnitude of the passthrough to investment decisions in an experimental asset market.

[30] 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.

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

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 7 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 affects 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 (Greenwood and Shleifer, 2014). Consistent with the literature, we find that lagged individual stock returns, capital gains, and overall market returns affect stock purchases, stock sales, and net flows into the market, respectively. Furthermore, our results show that forecast bias is a mechanism through which these different types of returns influence the corresponding investment decisions.

5. 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. However, successfully implementing such strategies requires careful timing of both purchases and sales, which may be difficult for individual investors to execute successfully. 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.

6. 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. Beyond security selection decisions, 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.

Our results demonstrate widespread, yet heterogeneous, biases in investors' forecasts, which significantly affect their stock trading decisions. These findings have broader implications for other domains where forecasting is important, such as the effects of inflation expectations on consumption or interest rate expectations on mortgage choices. Heterogeneity in forecast bias can provide a potential rationale for the observed heterogeneity in beliefs and responses, particularly in domains where seemingly similar individuals form different beliefs and react differently to the same data.

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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.

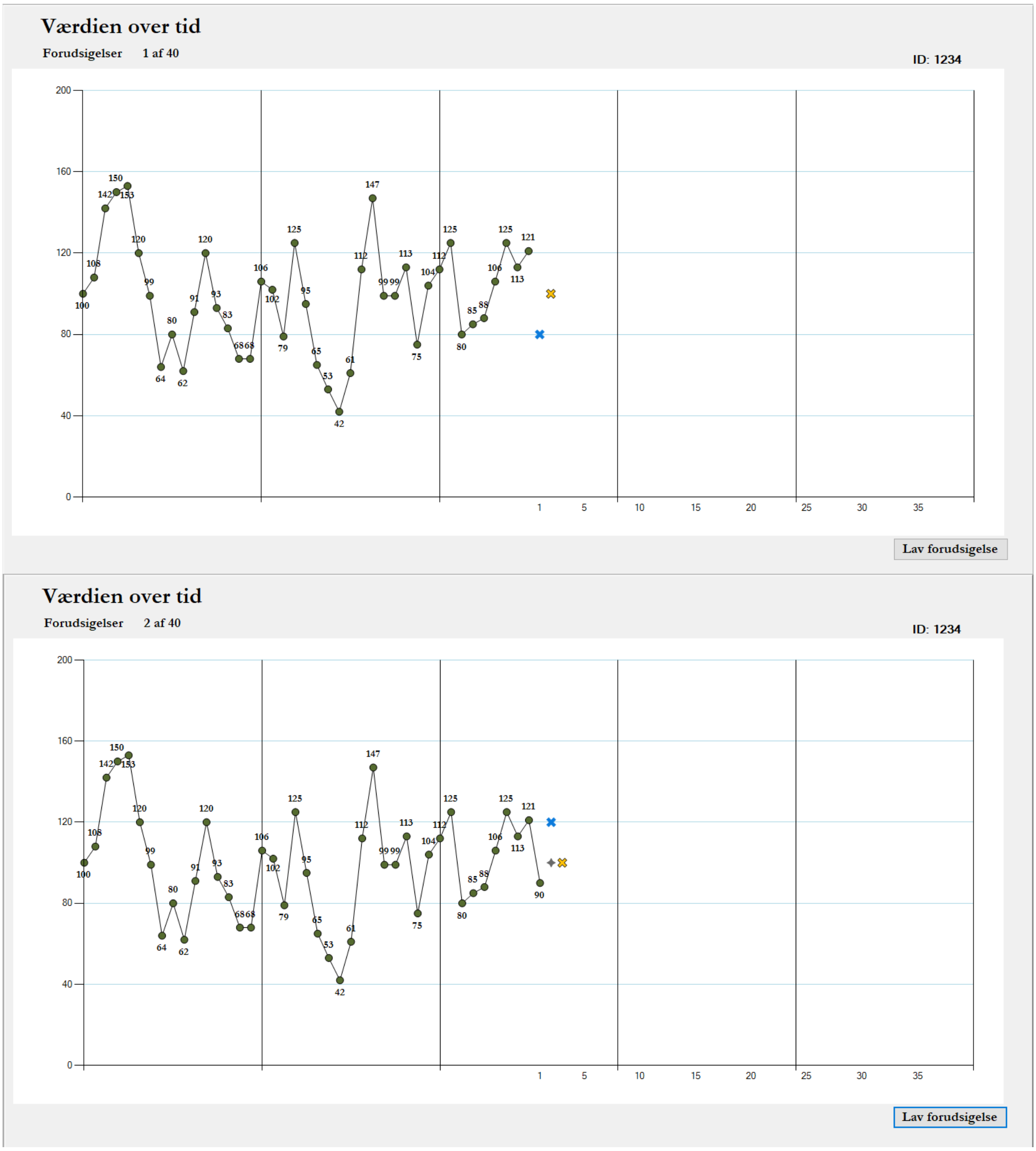

Figure 2: Histogram of *Forecast Bias*

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.

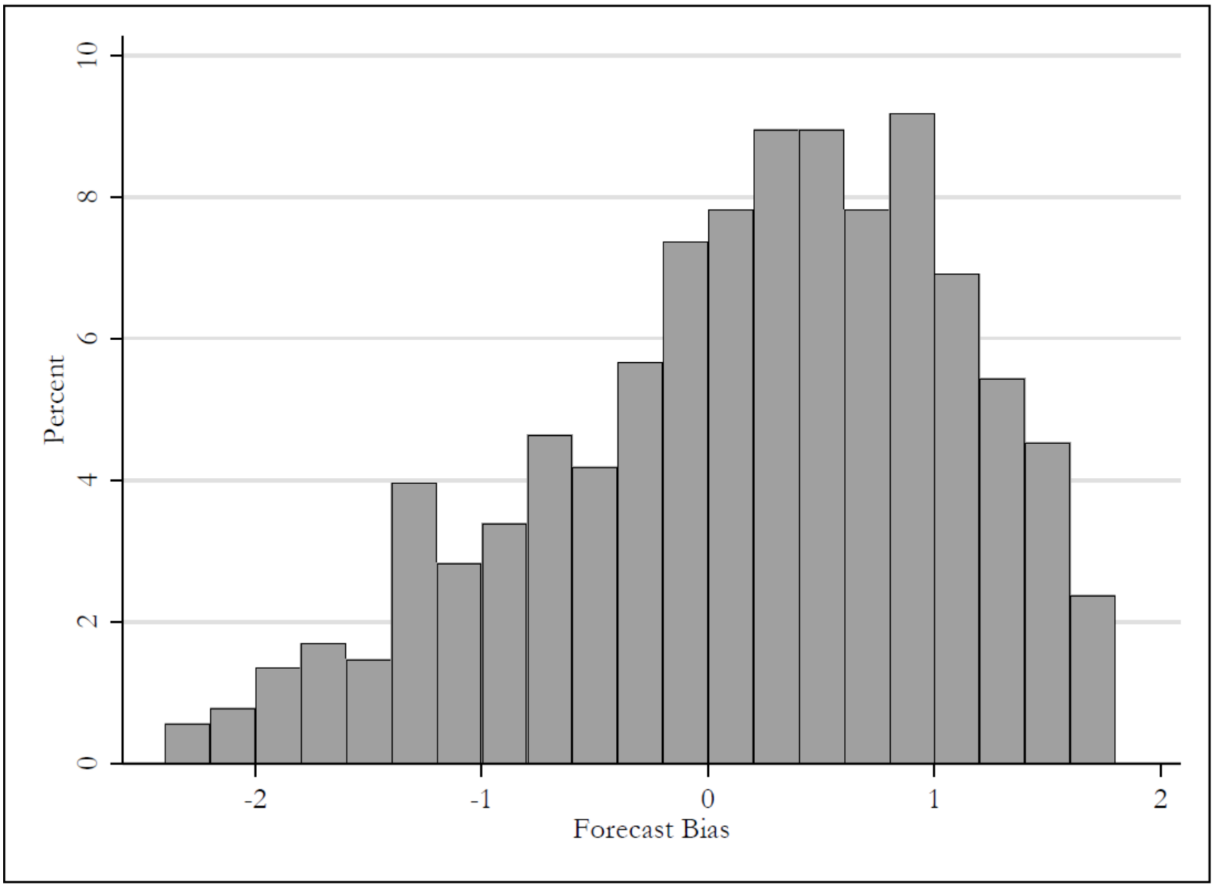

Table 1: Summary statistics

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 individual characteristics, respectively. The summary statistics for the individual characteristics are for 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	43.57	99.85	14.18
Value of buy	59,293	134,950	25,315
Prior annual ret. (buys)	26.20%	84.57%	8.94%
Prior annual excess ret. (buys)	6.41%	82.85%	-9.52%
Number of sales	33.10	75.92	9.94
Value of sale	82,819	624,121	32,815
Prior annual ret. (sales)	29.27%	78.83%	13.47%
Capital gain since purchase	23.09%	99.46%	3.37%
Held before (%)	1.81%	13.34%	0%
Current holding (%)	0.21%	4.53%	0%
Portfolio weight (buys)	0.02%	0.90%	0%
Portfolio weight (sales)	8.67%	13.32%	4.14%
Stock purchase fraction (%)	0.06%	0.41%	0.01%
Stock sales fraction (%)	1.07%	1.83%	0.27%
Net flows	0.28	10.63	0
Trade month	13.82%	34.51%	0%
Buy month	7.73%	26.70%	0%
Sell month	6.09%	23.91%	0%
Conditional net flows	2.15	28.37	2.14

Panel C: Individual characteristics			
	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 results of regressions of the relation between stock purchases and *Forecast Bias*. Column (1) reports the coefficients of a weighted-least squares regression in which the dependent variable equals 100 if the stock is purchased and zero otherwise, and includes an investor-day fixed effect. Column (2) reports the coefficients of a conditional logit regression in which the dependent variable equals 1 if the stock is purchased and zero otherwise, and conditions out investor-day effects. The key independent variable is *Forecast Bias x Performance measure*, where the performance measure is *Lagged annual return*. The return is winsorized at the 1st and 99th percentiles. The unit of observation is investor-stock-day over the period 2011-2021. In both columns, the observations are weighted such that each investor has equal weight in the regressions. *Forecast Bias* is adjusted to have a standard deviation of one. Both columns include an indicator for a stock previously held by the subject, an indicator for a stock currently owned by the subject, the portfolio weight of a current holding (zero for stocks not currently held), and the fraction of all investor-stock purchases for the full Danish population in the past month that were in the stock. 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.

	Weighted-least squares	Conditional logit
	(1)	(2)
Forecast Bias × Perform.	0.004***	0.061**
	(0.001)	(0.026)
Performance measure	-0.008***	-0.019
	(0.002)	(0.026)
Investor-Day Fixed Effect	Yes	Yes
Control variables	Yes	Yes
N	33,675,400	33,675,400

Table 3: Forecast bias and stock purchases – Alternative specification

This table reports results of weighted-least squares 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*. The unit of observation is trade-level over the period 2011-2021. The observations are weighted such that each investor has equal weight in the regressions. *Forecast Bias* is adjusted to have a standard deviation of one. Column (1) does not include control variables. Column (2) includes controls for age, male, married, children indicator, education, financial assets, income, housing assets, post-Covid experiment indicator, risk aversion, financial literacy, numeracy, optimism, overconfidence, and trust. Both specifications 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)
Forecast Bias	2.484**	2.974**
	(1.25)	(1.26)
Control variables	No	Yes
Year-Month Fixed Effect	Yes	Yes
N	29,268	29,268

Table 4: Forecast bias and stock sales

This table reports results of regressions of the relation between stock sales and *Forecast Bias*. Columns (1) and (2) report the coefficients of a weighted-least squares regression in which the dependent variable equals 100 if the stock is sold and zero otherwise, and includes an investor-day fixed effect. Column (3) reports the coefficients of a conditional logit regression in which the dependent variable equals 1 if the stock is sold and zero otherwise, and conditions out investor-day effects. The key independent variables are *Forecast Bias x Performance measure*, where the performance measure is *Lagged annual return* in column (1) and *Capital gain* in columns (2) and (3). Returns and capital gains are winsorized at the 1st and 99th percentiles. The unit of observation is investor-stock-day over the period 2013-2021. In both columns, the observations are weighted such that each investor has equal weight in the regressions. *Forecast Bias* is adjusted to have a standard deviation of one. All columns include monthly holding length fixed effects, as well as controls for the portfolio weight of a current holding, and the fraction of all investor-stock sales for the full Danish population in the past month that were in the stock. 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.

	Weighted-least squares		Conditional logit
Performance measure	Lag. excess return	Capital gain	Capital gain
	(1)	(2)	(3)
Forecast Bias × Perform.	-0.432	-0.893**	-0.063**
	(0.695)	(0.401)	(0.027)
Performance measure	3.759**	-0.427	-0.012
	(0.884)	(0.448)	(0.026)
Investor-Day Fixed Effect	Yes	Yes	Yes
Length of Holding Fixed Effect	Yes	Yes	Yes
Control variables	Yes	Yes	Yes
N	162,508	176,451	165,947

Table 5: Alternative *Forecast Bias* measures

This table reports the coefficients of weighted-least squares regressions for alternative forecast bias measures for purchases (Panel A) and sales (Panel B). The specification in Panel A is the same as in column (1) of Table 2. The specification in Panel B is the same as in column (2) of Table 4. In column (1), *Forecast Bias Residual* is generated using an investor-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 (Online Appendix B, eq. 1). In column (5), *Sticky Expectations* is estimated using the sticky expectations function (Online Appendix B, eq. 2) 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 (Online Appendix B, eq. 3). In column (7), *Adaptive Expectations* is estimated using the adaptive expectations function (Online Appendix B, eq. 4). 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 × Lagged return	0.004***	0.012**	0.014**	0.005**	0.009*	0.012*	0.013**
	(0.001)	(0.005)	(0.005)	(0.002)	(0.005)	(0.007)	(0.005)
Investor-Day Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	33,675,400	33,675,400	33,675,400	33,675,400	33,675,400	33,675,400	33,675,400

Panel B: Sales							
Forecast bias measure	Residual (1)	Limited Info (2)	Rank (3)	Diagnostic (4)	Sticky (5)	Extrapolative (6)	Adaptive (7)
Forecast Bias × Capital gain	-0.703* (0.382)	-0.558 (0.388)	-3.241** (1.415)	-0.957** (0.420)	-0.733* (0.439)	-0.679 (0.442)	-0.835* (0.433)
Investor-Day Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Length of Holding Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	176,451	176,451	176,451	176,451	176,451	176,451	176,451

Table 6: Forecast bias and trading post-experiment

This table reports the coefficients of weighted-least squares regressions using only trading observations post-experiment for purchases (column 1) and sales (column 2). The specification for purchases in column (1) is the same as in column (1) of Table 2. The specification for sales in column (2) is the same as in column (2) of Table 4. In both columns, the sample includes only trade-days occurring after the subject participated in the laboratory experiment. 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.

	Purchases	Sales
	(1)	(2)
Forecast Bias × Perform.	0.003**	-1.046**
	(0.001)	(0.482)
Performance measure	-0.007***	-0.396
	(0.002)	(0.587)
Investor-Day Fixed Effect	Yes	Yes
Length of Holding Fixed Effect	No	Yes
Control variables	Yes	Yes
N	12,416,384	72,771

Table 7: Net flows, trading, and conditional net flows

This table reports the results of weighted-least squares 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 the value of net flows is less than -1%. In columns (1) through (4), the sample includes all investor-months in which the subject owns individual stocks. In column (5), the sample includes only investor-months in which the absolute value of the subject's net flow is greater than 1%. In all columns, the observations are weighted such that each investor has equal weight in the regressions. 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 the value of beginning of month stock holdings, 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	Trade month	Buy month	Sell month	Conditional net flows
	(1)	(2)	(3)	(4)	(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)
Investor 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 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' portfolios are equal-weighted and Panel B reports results in which the investors' portfolios 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 by its standard deviation.
Forecast Bias Residual	The residual from regressing *Forecast Bias* on the investor-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 equation (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 (1) in Online Appendix B. For ease of interpretation, we divide the parameter estimate by its standard deviation.
Sticky Expectations	The sticky expectations parameter estimated as in equation (2) in Online Appendix B. 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 (3) in Online Appendix B. For ease of interpretation, we divide the parameter estimate by its standard deviation.
Adaptive Expectations	The adaptive expectations parameter estimated as in equation (4) in Online Appendix B. For ease of interpretation, we divide the parameter estimate by its standard deviation.
Lagged annual return	The return on the purchased stock over the prior year ending the day before purchase
Lagged annual market return	The return on the value-weighted Danish stock market over the prior year ending the day before purchase
Lagged annual excess return	The difference between the lagged annual return of the stock and the lagged annual market return
Capital gain	The percentage change in the value of the position relative to its purchase price
Held before	Indicator if the subject held the stock previously
Current holding	Indicator if the subject holds the stock currently
Portfolio weight	A stock's weight in the portfolio
Stocks purchase fraction	Prior month purchases of a stock as a fraction of prior month total stock sales in Denmark (multiplied by 100)
Stocks sales fraction	Prior month sales of stock as a fraction of prior month total stock sales in Denmark (multiplied by 100)
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
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 (details in Online Appendix B)
Financial literacy	Number of the four financial literacy questions answered correctly (details in Online Appendix B)
Numeracy	Number of the three numeracy questions answered correctly
Optimism	Subjects' stated life expectancy less objective life expectancy from actuarial tables (details in Online Appendix B)
Overconfidence	The sum of financial literacy and numeracy questions the subject believes they answered correctly less the number they actually answered correctly (details in Online Appendix B)
Trust	Likert scale where zero indicates "Most people can be trusted" and six indicate "One has to be very careful with other people" (details in Online Appendix B)
Post-Covid experiment	Indicator for subjects whose experimental session was in November 2020