Extrapolators and Contrarians: Forecast Bias and Household Equity Trading*

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Abstract

We test whether forecast bias affects household equity trading in a representative sample of investors. Our experiment finds that, on average, subjects exhibit extrapolation bias – a tendency to excessively project recent experiences in the same direction. There is substantial heterogeneity, however, and a large minority instead exhibits contrarian bias. We combine the experimental results with administrative data on the subjects' stock trading. Forecast bias is related to the past returns of stocks investors select to purchase and sell. A one standard deviation increase in forecast bias is associated with purchasing stocks with 3.0 percentage points higher excess return over the prior year. People with higher forecast bias sell stocks with lower capital gains since purchase. Forecast bias is not associated with higher performance.

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

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Expectations play a key role in both behavioral and rational models of security selection decisions and the cross-section of returns.[1] There is evidence that investors do not update their expectations using Bayes rule as new information arrives. Instead, survey and experimental evidence show that investors exhibit forecast biases in their belief updating; people react excessively to the recent past, resulting in biased expectations.[2] The extant literature shows cross-sectional stock return patterns that are consistent with forecast biases,[3] however, other papers argue that such patterns are consistent with rational behavior.[4] In this paper, we show that elicited, individual-level measures of forecast bias are directly related to investors' equity trading decisions.

Directly measuring the empirical relation between investors' forecast biases and their security selection decisions is challenging for three reasons. First, individuals' biases are not directly observable. Second, an investor's decision to purchase a specific stock is a function of their expectations for that stock *relative* to the full cross-section of their expectations. Concisely capturing each investor's forecast biases across different securities is complicated by the sheer number of available securities. Third, combining measures of individual bias elicited in laboratory experiments with administrative trade-level data is rarely possible. This paper overcomes these challenges by combining results from an incentivized laboratory experiment with administrative data on our subjects' trading records.

We invite a representative sample of investors to participate in a laboratory experiment designed to elicit their forecast bias. Our experiment closely follows Afrouzi et al. (2023), and

[1] See Barberis (2018) and Adam and Nagel (2022) for reviews of this literature.

[2] See Greenwood and Shleifer (2014), Gennaioli, Ma, and Shleifer (2016), Adam and Nagel (2022), Barrero (2022), and Afrouzi et al. (2023).

[3] See De Bondt and Thaler (1985), Jegadeesh and Titman (1993), La Porta (1996), Bordalo, Gennaioli, La Porta, and Shleifer (2019), and Atmaz, Cassella, Gulen, and Ruan (2023).

[4] See Johnson (2002), Cochrane (2011, 2017), and Chen and Yang (2019).

asks subjects to forecast a stochastic process. The respondents receive a fixed show-up fee and are eligible to win monetary incentives 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 forecast the next realization. This continues for a total of 40 rounds. Our control over the forecasters' information set and the data-generating process allows us to measure an individual-level bias in belief formation, *Forecast Bias*.

Our estimates of forecast bias are consistent with those found in earlier lab experiments (e.g., Frydman and Nave, 2017; Afrouzi et al., 2023). On average, people exhibit extrapolation bias, implying an excessive belief that recent realizations predict future movements in the same direction (trend continuation). There is, however, substantial individual heterogeneity. Although a small majority exhibit extrapolation bias, a sizeable minority exhibit contrarian bias, implying an excessive belief that recent realizations predict future movements in the opposite direction (mean reversion).

We link the experimental results with 11 years of administrative trading, economic, and demographic data from before and after the experiment. This trade level data comprises all trades of every Danish resident from 2011-2021, including our subject pool, matched with detailed information on income, financial assets, housing assets, education, and other demographic variables. In total, our data include nearly 34,000 buy trades and 26,000 sell trades worth 3.9 billion Danish kroner (€507 million) for the 957 subjects in our experiment.[5]

The extant theoretical literature shows that forecast bias increases sensitivity to recent past returns, as investors overweight their importance in forming expectations about future

[5] Not every participant in the experiment trades in individual stocks during our sample period, explaining the smaller number of individuals in some analyses.

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 badly more attractive. Thus, theory predict 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. 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. Using excess returns and year-month fixed effects removes the effect of the overall market and shows that *Forecast Bias* relates to cross-sectional stock selection.

We show that the results are robust in multiple ways. First, we include numerous control variables that previous studies suggest could affect household trading choices, including wealth, income, age, education, 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 the diagnostic expectations function proposed by Gennaioli and Shleifer (2010), Bordalo, Coffman, Gennaioli and Shleifer (2016), and Bordalo, Gennaioli, La Porta, and Shleifer (2019). Third, we address selection concerns about whether our participants are representative of the general population using two instruments: a randomized show-up fee and travel time to the experimental site. The results are robust to controlling for sample selection using the Heckman selection model. Finally, the results are robust to measuring past returns over a wide range of different period lengths.

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 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 nearly identical if we restrict the sample to exclude investors with low financial literacy or to exclude investors who cannot correctly answer numerical problems.

We test 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, subjects with high (low) *Forecast Bias* tend to sell stocks with the lowest (highest) capital gains. A one standard deviation increase in *Forecast Bias* implies a 4.6 percentage point reduction in the probability that a subject sells their highest performing stock instead of their lowest.

Given that we know the true data-generating process in the experiment, we have a clear rational benchmark to compare the subjects' responses with, allowing us to confidently attribute deviations from that benchmark to bias. Outside the laboratory, there is evidence that past returns have some predictive power for the cross-section of returns (e.g., De Bondt and Thaler, 1985; Jegadeesh, 1990; Jegadeesh and Titman, 1993). Thus, it is possible that trading based on past returns could be rational, and this could be correlated with lab-measured forecasting biases. Empirically, however, we find no evidence that *Forecast Bias* is related to future returns at several horizons, suggesting that our results do not capture rational trading.

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. Prior studies use survey data and show that investors' stated expectations of aggregate market returns are related to past returns.[6] Several studies connect the relation between past returns and expectations of aggregate market returns with household portfolio choice decisions (e.g., see Vissing-Jorgensen, 2003; Malmendier and Nagel, 2011; Giglio, Maggiori, Stroebel, and Utkus, 2021; Beutel and Weber, 2022; Laudenbach, Weber, Weber, and Wohlfart, 2023).[7] Our work differs from the extant literature in two ways. First, we do not measure beliefs or expectations, but instead we elicit the underlying updating process governing belief formation to directly measure forecast bias. The advantage of this approach is that it allows us, using only a single bias parameter, to examine cross-sectional security selection decisions at different points in time, without requiring us to elicit return expectations for each stock at each point in time. Second, we explore the relation between forecast bias and individual stock selection, unlike the prior literature that focuses on overall flows into equity markets.

Our paper also contributes to the literature on the security selection decisions of individual investors. Prior studies show numerous factors that affect security selection, including attention grabbing events (Barber and Odean, 2008), social networks (Ivković and Weisbenner, 2007; Hvide and Östberg, 2015; Knüpfer, Rantapuska, and Sarvimäki, 2022), lottery and gambling preferences (Kumar, 2009), IQ (Grinblatt, Keloharju, and Linnainmaa, 2012), ambiguity aversion (Dimmock, Kouwenberg, Mitchell, and Peijnenburg, 2016), probability weighting preferences (Dimmock, Kouwenberg, Mitchell, and Peijnenburg, 2021), and geographical and cultural distance (Bhamra, Uppal, and Walden, 2022), among other

[6] Vissing-Jorgensen (2003), Malmendier and Nagel (2011), 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), Laudenbach, Weber, Weber, and Wohlfart (2023), and von Gaudecker and Wogrolly (2022) 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.

[7] Liu, Peng, Xiong, and Xiong (2022) test whether extrapolation beliefs are related to portfolio turnover, and do not find a relation.

factors. We add to this literature by showing that forecast bias is important in explaining the heterogeneity in stock selection among individual investors.

Our paper 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. For instance, the literature documents short-term momentum (Jegadeesh and Titman, 1992) and long-term reversal (De Bondt and Thaler, 1985), and authors suggest that these patterns arise due to investors' forecast biases (e.g., Lakonishok, Shleifer, and Vishny, 1994; La Porta, Lakonishok, Shleifer, and Vishny, 1997). 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; Da, Huang, and Jin, 2021). Our paper complements these studies, by showing a direct empirical relation between individual-level elicited biases in expectation formation and the individual's stock selection decisions. To this end, our study provides individual-level evidence of trading activities that are consistent with these models of cross-sectional patterns in stock returns.

1. Eliciting Individuals' Forecast Bias

1.1 The Elicitation Procedure

We develop an experimental module that includes a task to elicit individuals' forecast bias.[8] The task closely follows Afrouzi et al. (2023), in which subjects observe past values of an asset and then make forecasts about the future value of that asset. A key feature of the

[8] The module also includes separate experiments to measure risk aversion, attention costs, as well as answers to a series of questions to measure control variables for financial literacy, numeracy, trust, optimism, and overconfidence. See Online Appendix B for the complete list of all questions.

experimental design is that subjects observe past values, but do not know the underlying data-generating process.

For our experiment, the underlying data-generating process is as follows. The first observation of the value of the asset is set to 100 and the subsequent observations are generated with a first-order autoregressive (AR(1)) process:

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

The AR(1) coefficient is set to 0.5 and the error term is drawn from a normal distribution with a standard deviation of 25.[9]

To begin, the subjects see 40 past realizations of the value of the asset, and submit one- and two-period-ahead forecasts. Once subjects submit their forecasts, they observe the next realization of the value of the asset, and are asked to submit new one- and two-period-ahead forecasts. This continues for 40 rounds.

Panel A of Figure 1 provides a screenshot of the initial forecasting task.[10] Panel A shows the first 40 realizations and their values as well as two "**x**'s", one blue and one orange, to indicate forecasts one 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 of the value, and are asked to make two new forecasts, as seen in Panel B of Figure 1.

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 957 subjects taking less than five (more than 20) minutes.

[9] 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 AR(1) coefficient to ensure all responses are comparable.

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

To incentivize the subjects, each subject has a 10% chance of being eligible to receive an incentive payment. 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 that choices are incentive compatible, 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 20, 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 greater than or equal to the winning probability, the subject received 2,000 DKK (approximately €260).[11] Based on this procedure, 18 subjects received a prize from the forecasting task.

1.2 Measures of Forecast Bias

Using the forecasts elicited from the experiment described above, we construct several measures of forecast bias at the individual level. As the basis for our main measure, we follow Afrouzi et al. (2023, equation 4.1) and estimate each subject's implied AR(1) parameter using the following regression:

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

where $F_{i,t}(x_{i.t+1})$ indicates the forecast of next period's realization, $x_{i.t+1}$, made by subject *i* at period *t*. Given the data generating process, $b_i > 0.5$ indicates extrapolation bias and $b_i < 0.5$

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

indicates contrarian bias.[12] We define our main measure as: $Forecast\ Bias\ = \frac{b_i - 0.5}{\sigma}$. That is, for ease of interpretation, we subtract 0.5 from the parameter estimate and divide by its standard deviation of 0.32; thus, a value of zero is the rational benchmark, negative values indicate contrarian bias, positive values extrapolation bias, and a value of -1 or 1 indicates the parameter estimate is one standard deviation from the rational benchmark.

Each subject sees 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 person-specific empirical persistence parameter and standard deviation of the error term based on the full set of 80 realizations. A second measure, *Forecast Bias Limited Information*, incorporates that the rational forecast changes after each realization. We compare each forecast to the within-sample least-squares estimate of the forecast given all prior realizations of the process. At any given point in time, the limited information rational forecast, $E_t(x_{i.t+1})$, is given by:

$$E_t(x_{i.t+1}) = \bar{x}_{i,(0,t)} + \hat{b}_{i,(0,t)}\left[x_{i,t} - \bar{x}_{i,(0,t)}\right] \tag{3}$$

where $\bar{x}_{i,(0,t)}$ is the mean of the process from period 0 through *t* and $\hat{b}_{i,(0,t)}$ 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:

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

[12] 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 $b_i + \frac{1+3b_i}{T}$, which is 0.56 for an OLS estimate of b_i of 0.5 and 40 forecasts. Our *Forecast Bias* measure thus underestimates the tendency to extrapolate.

where b_i greater than zero indicates extrapolation bias and less than zero indicates contrarian bias.

As a fourth measure of forecast bias, we estimate a parameter using the diagnostic expectations model of Bordalo, Gennaioli, and Shleifer (2018) and Bordalo, Gennaioli, La Porta, and Shleifer (2019).[13] Specifically, we estimate:

$$F_{i,t}(x_{i.t+1}) - E_t(x_{i.t+1}) = b_i \cdot [E_t(x_{i.t+1}) - E_{t-1}(x_{i.t+1})] + \varepsilon_{i,t} \tag{5}$$

where $F_{i,t}(x_{i.t+1})$ indicates the forecast of next period's realization, $x_{i.t+1}$, made by subject *i* at period t and $E_t(x_{i.t+1})$ is the rational expectation of $x_{i.t+1}$ at period *t*. Thus, the equation regresses subject *i*'s forecast error relative to the rational expectation on the change in the rational expectation in the prior period. A coefficient estimate of $b_i > 0$ indicates extrapolation bias and $b_i < 0$ indicates contrarian bias.

2. Data and Variables

Access to the data used in this study is provided by Statistics Denmark, a government agency with central authority on Danish statistics. We use the research infrastructure at Statistics Denmark to recruit subjects on the basis of administrative register data and conduct our laboratory experiment, as described in more detail later in this section. Statistics Denmark provide economic, financial, and demographic data, including holdings of stocks and mutual funds 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 as detailed in the sections below.

[13] Diagnostic expectations are based on the representativeness heuristic by introduced by Kahneman and Tversky (1972, 1973).

2.1 Sample Selection and Lab Experiment

The starting point of our analysis is to recruit subjects for our experimental tasks. Statistics Denmark recruited 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 potential pool of individuals eligible to participate in the study 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)[14] in at least three of the years between 2014 and 2018. After applying these criteria, the pool of eligible subjects consists of 75,846 individuals. From the pool of eligible subjects, Statistics Denmark randomly invited 24,821 individuals to participate in our study.

Following the procedure in 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 the show-up fee of 1,000 DKK, while the other half has a 10% chance of winning 2,000 DKK. In robustness tests, we use this exogenous variation in incentives as an instrument to address potential concerns about sample selection bias.

In total, 959 subjects accepted the invitation and participated in the experiment (a 3.9% participation rate). The experiment was conducted in-person, in sessions of around 15 subjects, which took place at Statistics Denmark in Copenhagen. We conducted two session per day on

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

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.[15]

2.2 Measures of Forecast Bias

Panel A of Table 1 summarizes the measures of forecast bias for the subjects in the experiment. Negative values of forecast bias implies that subjects underreact to the data generating process, while positive values imply that subjects overreact. On average, subjects over-extrapolate. Our data agreement with Statistics Denmark prohibits reporting any statistics that are not based on at least 10 observations, and so in place of medians we report the average value for the 45th through 55th percentiles. The mean (quasi-median) of the *Forecast Bias* parameter is 0.04 (0.09), which is significantly greater than the rational benchmark of zero (p-value < 0.0001). Similarly, the mean (quasi-median) of the diagnostic expectations parameter is 0.19 (0.26), significantly greater than the rational benchmark of zero (p-value < 0.0001). The finding that the average subject over-extrapolates is consistent with the findings of Afrouzi et al. (2023).

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 under- or overreaction, respectively. We refer to subjects that underreact as exhibiting contrarian bias, and subjects that overreact as exhibiting extrapolation bias, respectively. The figure shows that a small majority exhibit extrapolation bias, but there is substantial heterogeneity with a large minority exhibiting contrarian bias, implying an excessive belief that recent realizations predict future movements in the opposite direction (mean reversion).

[15] 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.

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 comes from the official records of the Danish Tax and Customs Administration (SKAT) for the years 2011 to 2021.[16] 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 and mutual fund holdings by ISIN number at the end of the year as well as daily records of all security 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) individuals purchase at least one stock during 2011 through 2021 and a total of 749 of the 959 (78 percent) individuals sells at least one stock during 2011 through 2021. Panel B of Table 1 summarizes the purchases and sales of individual stocks in the sample. We first average across trades for an individual, and then report averages across individuals. On average, subjects make 49.5 purchases and 34.7 sales, for a total of 59,682 unique trades.[17] The distribution of purchase activity is highly skewed, with the 50 most active traders making 49.9% of total purchases. The average purchase costs 41,239 Danish kroner (€5,361) and the average sale is for 53,703 kroner (€6,981). In aggregate, the value of purchases and sales in our sample is slightly greater than 3.9 billion kroner (€507 million).

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[16] Data on individuals' stock trades is first available for 2011 (before 2011, only annual snapshots of stock holdings are available).

[17] We aggregate trades in the same stock within a day to get unique person-stock-day purchases.

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 12-month period, as Ben-David, Li, Rossi, and Song (2022) find a simple market adjustment best explains investors' cross-sectional allocations. The average lagged 12-month excess return for purchases is 2.4%, but excess returns are positively skewed and the quasi-median is -3.4% (for comparison, the average lagged 12-month return for the Danish stock market is 18.6% for our sample). The average lagged 12-month excess return for sales is 1.7%, and the quasi-median is -0.9%. We also calculate the capital gain since purchase for the stocks in our sample. The average capital gain is 35.0% and the quasi-median is 18.8%.

2.4 Control Variables

In the analysis we include control variables for subjects' age, gender, marital status, parental status, years of education, income, housing assets, and financial assets. We supplement these with additional variables from our experiment, which included questions to measure risk aversion, financial literacy, numeracy, 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. These variables mitigate against potential omitted variable bias from factors that could be correlated with forecast bias.

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

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. Figure 3 shows an example of one lottery pair, where Option A is safer but has a lower expected return than Option 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 simply 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 (HRS), 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 an upward or downward bias in their forecasts, but this is unlikely to affect our measures of forecast bias, as our measure does not capture a persistent upward or downward bias. Rather, our measure captures persistent under- or over-reaction to recent realizations of values (e.g., forecasts that are consistently too high following high realizations but also too low following low realizations). Nevertheless, we control for the optimism measure and overconfidence in our regressions. For overconfidence, we construct a control based on Moore and Healy (2008). In particular, after both the financial literacy and the numeracy questions, we ask respondents how many questions they answered correctly. The difference between estimated correct answers and actual correct answers provides us with an overconfidence control. We follow Puri and Robinson (2007) to construct a control for optimism and include a question assessing individuals' subjective life expectancies. The measure for optimism is based on the comparison between subjective and objective life expectancies (where the latter are derived from age/gender population mortality tables). Finally, we control for trust using the question "Generally speaking, would you say that most people can be trusted or that you have to be very careful when dealing with people?" from the World Values Survey, as Guiso, Sapienza, and Zingales (2008) report a relation between trust and portfolio choice.

3. Forecast Bias, Past Returns, and Stock Purchases

3.1 Forecast Bias and Stock Purchases based on Past Returns

Theory predicts that extrapolators purchase stocks that performed well in the past while contrarians purchase stocks that performed poorly. Testing this hypothesis by regressing the purchase decision on *Forecast Bias* interacted with past returns is infeasible due to the large number of different stocks available for purchase.[19] Instead, we test whether forecast bias relates to the past excess return of the stocks, conditional on purchase. In particular, we estimate the following specification:

$$Prior\ 12\ month\ excess\ return_{i,t} = \beta_1 \cdot Forecast\ Bias_i + \boldsymbol{\delta X_{i,t}} + \theta_t + \varepsilon_{i,t} \qquad (6)$$

where $Prior\ 12\ month\ excess\ return_{i,t}$ is the lagged 12-month return of the purchased shares less the return on the Danish stock market over the same period, $\boldsymbol{X_{i,t}}$ is a matrix of control variables, and θ_t is a year-month fixed effect. By removing the overall market performance over the prior year from the stock return, the regressions effectively compare relative performance across stocks and not trading in response to overall market movements. The

[19] See Grinblatt and Keloharju (2001, Section III) for a detailed discussion of this issue.

lagged return is winsorized at the 1st and 99th percentiles to ensure our results are not driven by outliers. The unit of observation is person-month; if an individual trades multiple times in the month the dependent variable is the equal-weighted average excess return. We include year-month fixed effects 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.

For ease of interpretation, *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*. Standard errors, clustered by subject, are reported in parentheses below the coefficient estimates.

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 paper, 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% higher excess 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.

3.2 Alternative Measures of Forecast Bias

Table 3 shows results with alternative measures of *Forecast Bias*. Aside from these alternative measures, the specification is identical to that in Column (3) of Table 2. 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 generated using the same underlying parameters. However, because each subject observes a unique time-series, by random chance some subjects will observe time-series that appear to differ from the true process. To address this issue, the alternative measure used in column (1) is the residual from regressing *Forecast Bias* on the empirically observed persistence and standard deviation in the 80 realizations. Column (2) employs an alternative measure based on a comparison with a subject-specific rational benchmark that is updated every round of the elicitation procedures using the realizations the subject has observed until that point in the experiment. Section 1.2 contains details on both alternative measures. The results using these alternative measures are significant and similar to those in the main specification.

Column (3) shows results where we use 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.

Column (4) shows results for an alternative measure of forecasting bias, *Diagnostic Expectations*, based on the model of Bordalo, Gennaioli, and Shleifer (2018) and Bordalo, Gennaioli, La Porta, and Shleifer (2019). The results are similar to those found using the *Forecast Bias* measure.

3.3 *Sample Selection*

Although we invite a random sample of Danish investors for our experimental sessions, we observe *Forecast Bias* only for those investors who choose to participate. A potential concern is that the decision to participate might depend on unobserved individual traits that correlate with *Forecast Bias* and trading choices. To address these concerns, we re-estimate our main results using a Heckman sample selection model that exploits exogenous variation in our sampling procedure.

To induce exogenous variation in the financial incentive to participate in the experimental sessions, we randomize the potential show up fee (either 1,000 DKK (135 euros) or 2,000 DKK (270 euros) with equal probability). Further, we randomly select individuals living within the Greater Copenhagen area, which allows us to use the subject's commuting time to the experimental site in minutes as an instrument for opportunity costs. We also note that variation in the show up fee is by construction unrelated to trading decisions and that the commuting time to the site of the experimental sessions does not predict trading behavior.[20] Table 4 presents the results.

Column (1) of Table 4 shows the coefficients from the first stage of the Heckman selection model, where the dependent variable is an indicator for participation in one of the experimental sessions. The indicator takes the value of one for the 680 subjects who trade individual stocks during the sample period out of 24,532 invited subjects.[21] The coefficient on the expected show-up fee is positive and statistically significant at the 10% level. Increasing the potential show-up fee increases the probability of participation by 0.31 percentage points

[20] The Greater Copenhagen area is an amalgam of affluent and less affluent neighborhoods. As a result, commuting time to the experimental site does not predict 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.

[21] Participants who do not trade individual stocks are excluded from this analysis.

(an 11.3% increase relative to the mean participation rate of 2.8%). The coefficient on the travel time to the experimental site is negative and statistically significant at the 5% level. The effect of travel time on participation is economically significant: the marginal effect evaluated at the mean implies that living 10 minutes travel time further away from the experimental site reduces the probability of participation by 0.32 percentage points (an 11.7% decrease relative to the mean participation rate).

Columns (2) of Table 4 reports results from the second stage regression where the dependent variable is the lagged 12-month excess return of the purchased shares. The estimated coefficient on *Forecast Bias* is 3.028, which is close to the estimated coefficient of 3.024 in our baseline specification (column (3) of Table 2). Given that point estimates on the variables of interest from the Heckman selection model are almost identical to the corresponding coefficients in our baseline specification, we conclude that our results are robust to controlling for sample selection bias.

3.4 Quantitative Reasoning Ability, Financial Sophistication, and Forecast Bias

Although *Forecast Bias* measures errors relative to a rational benchmark, it is unlikely that its relation with stock purchase decisions simply captures a common correlation with poor numerical ability or low financial sophistication. *Forecast Bias* is non-linear in subjects' forecast errors; low values indicate an excessive belief in mean reversion while high values indicate an excessive belief in trend continuation. 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. Our findings require that individuals who are consistently contrarian in their forecasts during the experiment are also contrarian investors and individuals who are consistently extrapolators during the experiment are also extrapolators as investors (i.e., our findings require directionally consistent deviations from rationality in both domains).

Nevertheless, to ensure our results are not driven by subjects with limited cognitive skills or low financial sophistication, we examine subsamples based on proxies for sophistication.

The specifications in Table 4 are identical to that in column (3) of Table 2, except that we limit the sample. Columns (1) to (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 coefficient on *Forecast Bias* is positive and significant in all three columns, and the magnitudes are similar to that in the full sample.

4. Alternative Time Horizons and Future Returns

4.1 Alternative Time Horizons

The prior tests focus on lagged 12-month return periods. This is a commonly used return period in the literature for examining how past returns affect individual investor decisions (e.g., see Barber and Odean, 2002; Kumar, 2009; Laudenbach, Weber, Weber, and Wohlfart, 2023, among others). The past year is a natural evaluation period as brokerages and financial media often report returns over the past year and 52-week highs. As a robustness test, we evaluate lagged returns over alternative time-periods. Other than using alternative time periods these results, reported in Table 5, follow the baseline specification.

The six columns in Table 5 report results where the dependent variables are the returns over the prior week, month, three months, six months, 12 months, and 36 months, respectively. Note that we do not annualize the returns and so the coefficient point estimates are not directly comparable across specifications. For all but the 1-week and 1-month time horizons, the coefficient on *Forecast Bias* is positive and statistically significant (albeit at only the 10% level for the three month returns).

4.2 Forecast Bias and Future Returns

Throughout the paper we interpret *Forecast Bias* as a *bias*, implying that it captures systematic errors in forecasting. The prior sections show that *Forecast Bias* is related to past stock returns, however, the literature shows that past stock returns have some predictive power for future returns (e.g., Jegadeesh, 1990; Jegadeesh and Titman, 1993). For *Forecast Bias* to capture erroneous behavior, it should not be related to future outperformance. Accordingly, in this section, we test the relation between *Forecast Bias* and future excess returns of purchased stocks. Aside from using future excess returns as the dependent variable, the regressions follow our baseline specification.

The six columns in Table 6 report results where the dependent variables are the returns beginning the day after purchase and continuing for one week, one month, three months, six months, 12 months, and 36 months, respectively. None of the *Forecast Bias* coefficients are significant. Combined with the results in Table 5, we find that *Forecast Bias* is related to the past excess returns of securities selected for purchase, but is not related to excess returns after purchase. Thus, it is unlikely that *Forecast Bias* captures a propensity for rational momentum or reversal trading.

5. 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]

[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).

The mean (quasi-median) number of individual stocks held at the time of a sale is 8.2 (5), and thus 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 in turn allows for a fixed effect specification that closely matches the subjects' decision.

Table 8 reports regression results for sales. We limit the sample to days when the subject sells at least one stock (sales days). The unit of observation is person-stock-day. We estimate regressions of the type:

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

where $Sale_{i,j,t}$ is an indicator variable equal to one 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) 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.

Including the person-day fixed effect means the regression compares the relative likelihood that a person chooses to sell a stock given that stock's characteristics relative to the person's other stock holdings. We include fixed effect for the length of the holding, as prior studies show a strong relation between holding length and the probability of selling (e.g., Ivković, Poterba, and Weisbenner, 2005; Ben-David and Hirshleifer, 2012; Hartzmark, 2015). We include controls for the idiosyncratic standard deviation of the stock and its weight in the subjects' portfolio. The standard errors reported below the coefficients are clustered by person.

The columns in Table 8 consider several different performance measures. In column (1), the performance measure is the lagged 12-month excess return as in the purchase regressions. In this specification, the coefficient on the interaction term between performance and *Forecast Bias* is not significant.

Unlike with purchases, for each potential sale decision individuals can observe their capital gain on that particular security. A large literature shows that unrealized capital gains are strongly related to sales decisions,[23] and thus individuals may use capital gains when forming expectations of returns. Indeed, Ben-David and Hirshleifer (2012) argue that belief updating is the primary driver of the relation between capital gains and sales decisions.[24] Accordingly, in the remaining columns of Table 8 we examine measures of capital gains since purchase.[25]

In column (2), the coefficient on the interaction term between *Forecast Bias* and capital gains is negative and significant. Contrarians are less likely to sell their worst performing stocks and extrapolators are less likely to sell their best performers. This result is consistent with the idea that capital gains cause belief updating, and explains the V-shaped relation between capital gains and sales propensity found by Ben-David and Hirshleifer (2012). Both gains and losses increase sales propensity because of the heterogeneity in *Forecast Bias*.

Column (3) extends the model by including the square and cube of capital gains, an indicator variable for positive capital gains, and lagged 12-month returns. The higher-order terms control for the possibility that the relation between capital gains and sales propensity is non-linear and the indicator variable controls for the possibility of a discontinuity in sales propensity around zero. 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 unaffected by controlling for effects documented in prior studies.

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[23] See, for example, Odean (1998), Ben-David and Hirshleifer (2012), and Hartzmark (2015).

[24] Ben-David and Hirshleifer (2012) discuss *cross-sectional* beliefs and the disposition effect. This is distinct from studies such as 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.

[25] 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.

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, 1 the highest, etc.). Hartzmark (2015) shows investors are more likely to sell the most extreme winning and extreme losing positions in their portfolios. Hartzmark argues his findings are consistent with belief updating – investors with limited attention are more likely to reexamine their beliefs following substantial gains or losses. We extend this argument by testing whether heterogeneity in *Forecast Bias* affects the direction of belief updating.

Column (4) shows that the coefficient on the interaction term between *Forecast Bias* and capital gains rank is negative and significant. The coefficient estimates imply that a one standard deviation increase in *Forecast Bias* results in a 4.6 percentage point reduction in the probability that a subject sells their highest performer instead of their lowest performer (a 19.2% change relative to the mean probability of a sale).

In column (5), we add numerous additional control variables. Hartzmark (2015) shows there is a U-shaped relation between the capital gains rank and sale propensity and that people are more likely to sell their best and their worst performer. Accordingly, we add a non-linear term for capital gains, $(CapGain\,rank\ -\ 0.5)^2$, defined as the capital gains rank minus 0.5 squared, as well as indicator variables for the stocks with the lowest and highest capital gain in the portfolio, and the lagged 12-month excess return. We also include interactions of *Forecast Bias* with $(CapGain\,rank\ -\ 0.5)^2$. The inclusion of this additional interaction term is important, as explanations for the rank effect based on concepts like salience or limited attention predict a V-shaped relation between within-portfolio ranking and sales propensity, but do not make any prediction for the interaction term with *Forecast Bias*. The results show that the interaction term between *Forecast Bias* and capital gains rank remains negative and significant even with the inclusion of the additional control variables, and that the interaction term between Forecast Bias and $(CapGain\ rank\ -\ 0.5)^2$ is not significant.

6. Conclusion

Our study is the first to empirically link individual-level forecast bias to household equity trading decisions. We measure forecast bias using a large-scale laboratory experiment in a representative sample of investors in Denmark. On average, individuals exhibit extrapolation bias, though there is substantial individual heterogeneity. A small majority of our sample are extrapolators and a large minority are contrarians. We link our experimental measure to administrative trade-level data from 2011-2021. As predicted by theory, forecast bias relates to stock purchases based on past returns. Individuals that exhibit extrapolation (contrarian) bias pick stocks that have performed well (badly) relative to the market in the past 12 months. Furthermore, forecast bias has a negative relation with the capital gains of stocks sold: Extrapolators (contrarians) tend to sell those stocks that have performed poorly (well) since purchase relative to their other portfolio holdings. Forecast bias is not related to future returns, highlighting that these choices are based on errors in forecasting and are not a rational trading strategy.

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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 yellow "**x**" up and down. Once the subject settles on their forecast, they click the "Make forecast" button and observe the next realization of the process, as seen in the lower panel. They are then asked to make two new predictions. This process continues for a total of 40 rounds.

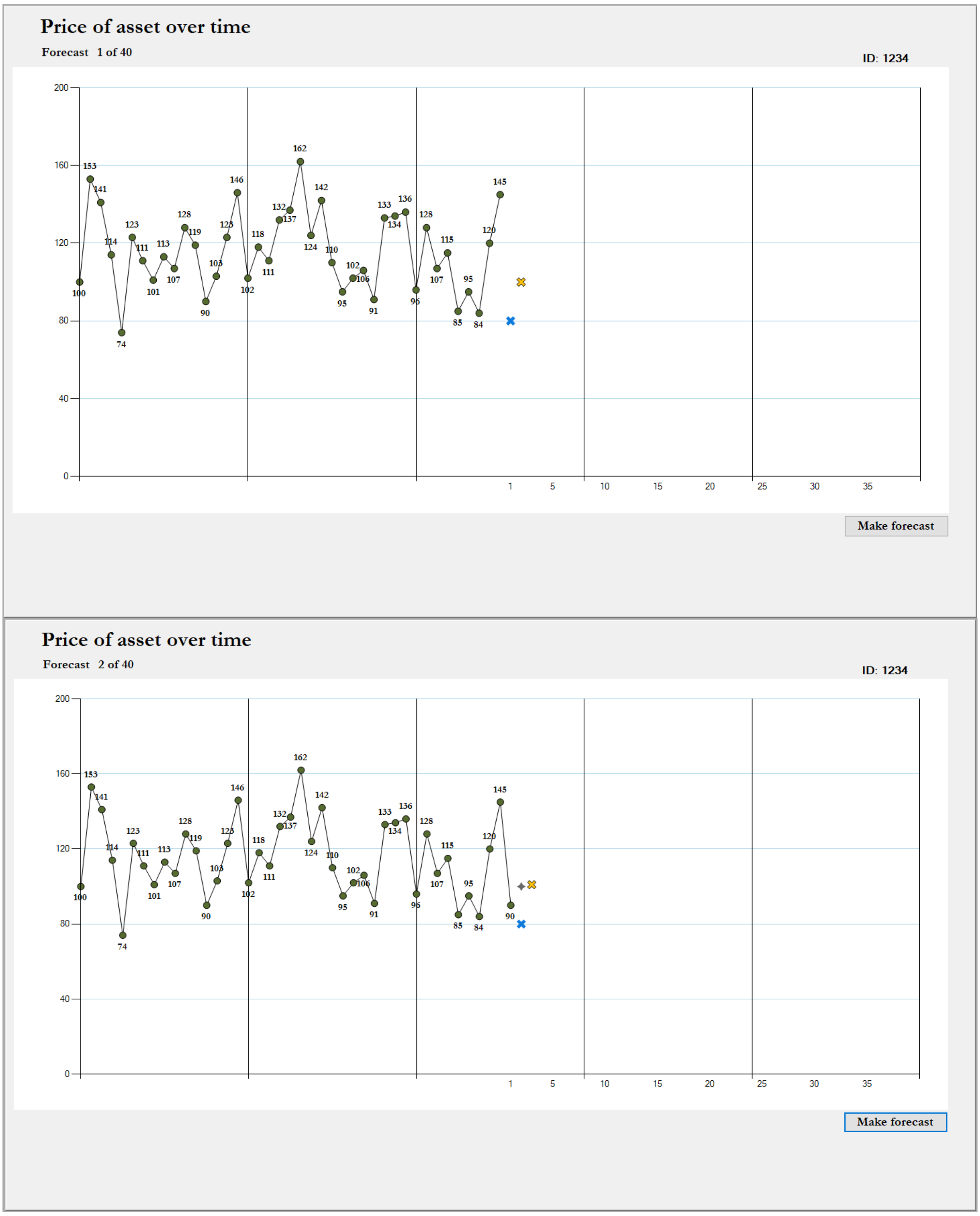

Figure 2: Histogram of *Forecast Bias*

This figure shows a histogram of *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).

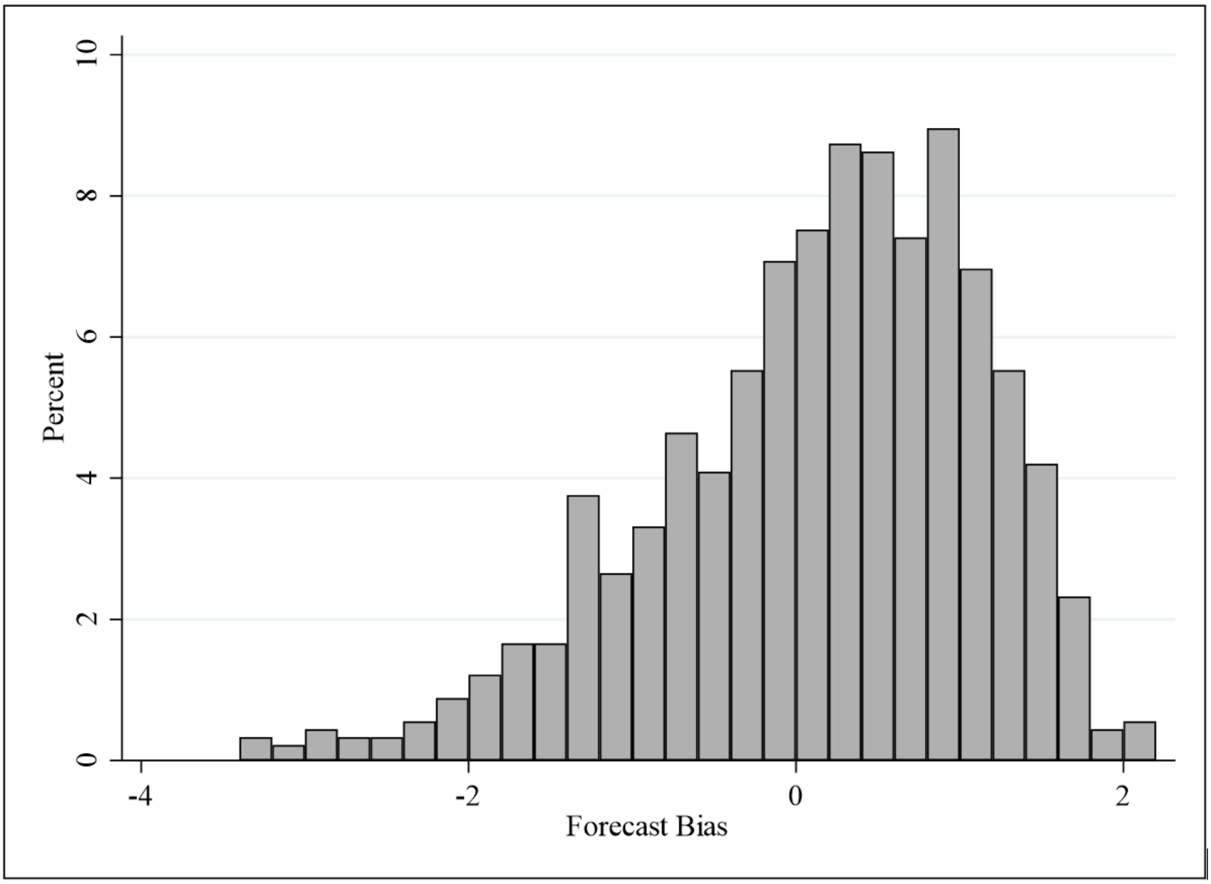

Figure 3: Elicitation of risk aversion

This figure shows an example of the decision tasks used to elicit risk aversion. For each decision, the subject is asked to choose between two lotteries, with the winning amounts and probabilities varied across tasks. In the example below, Option A is less risky but has a lower expected return than Option B.

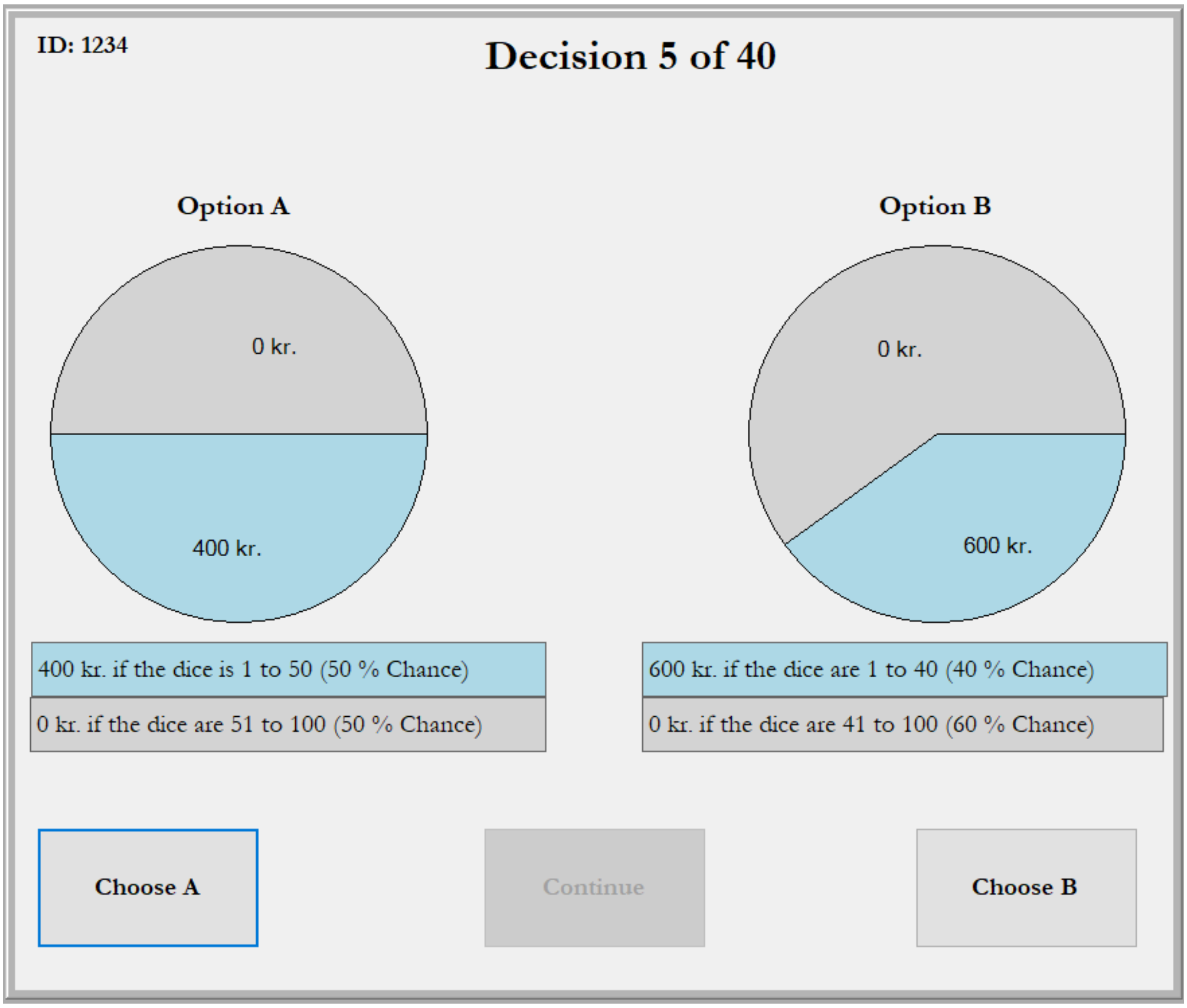

Table 1: Summary statistics

This table reports summary statistics. Appendix Table A1 defines all variables. For each variable, we report the mean and standard deviation. The column "Median (decile)" reports the average value of the variable for subjects between the 45^{th} and 55^{th} percentile (this is done because our data agreement prohibits reporting non-aggregated values for any variable). Monetary amounts are reported in thousands of Danish kroner. Panels A, B, and C report summary statistics for the forecast bias measures, trading and stock characteristics, and control variables, respectively. The number of respondents in this table is N = 904 and the summary statistics for the control variables are regarding the year the experiment is conducted, 2020.

Panel A: Summary statistics: Forecast Bias measures			
	Mean	Std. Dev.	Median (decile)
Forecast Bias	0.04	1.00	0.09
Diagnostic	0.19	0.70	0.26

Panel B: Summary statistics: Trading and stock characteristics			
	Mean	Std. Dev.	Median (decile)
Number of buys	49.50	109.85	15.5
Value of buy	41,239	80,795	20,454
Prior 12-month excess ret. (buys)	2.4%	36.7%	-3.4%
Number of sales	34.69	90.59	8.8
Value of sale	53,703	90,830	29,168
Prior 12-month excess ret. (sales)	1.7%	19.5%	-0.9%
Capital gain since purchase	35.0%	59.0%	18.8%

Panel C: Summary statistics: Control variables			
	Mean	Std. Dev.	Median (decile)
Income (000's)	749.19	567.57	635.65
Financial assets (000's)	2,063.03	13,801.94	720.93
Housing assets (000's)	1,922.30	1,939.33	1,604.12
Age	50.56	7.70	51.53
Education	16.45	2.20	16.67
Male	0.69	0.46	1
Married	0.64	0.48	1
Children	0.81	0.39	1
Risk aversion	0.49	0.16	0.48
Financial literacy	3.40	0.80	4
Numeracy	2.83	0.43	3
Optimism	4.59	7.96	4.46
Overconfidence	0.19	0.91	0
Trust	4.23	1.55	5
Post-Covid experiment	0.34	0.47	0

Table 2: Forecast bias and purchases based on past excess returns

This table reports the coefficients of OLS regressions in which the dependent variable is the average lagged 12-month excess return of the stocks purchased by the subject within a month during the sample period 2011-2021. Excess return is calculated as the return in excess of 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 person-month, and each observation is weighted by the number of months an individual made purchases. Column (1) does not include any controls. Column (2) includes age, male, married, children indicator, post-Covid experiment indicator, income, housing assets, financial assets, and education. Column (3) includes the same controls as in column (2) plus controls for financial literacy, numeracy, optimism, overconfidence, trust, and risk aversion. 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	2.539**	2.594**	3.024**
	(1.287)	(1.308)	(1.302)
Age		-7.840	-9.151
		(8.619)	(8.671)
Male		0.797	2.324
		(3.037)	(3.049)
Married		6.862**	7.230**
		(2.814)	(2.821)
Children		-2.939	-2.975
		(3.146)	(3.111)
Post-Covid experiment		0.294	0.208
		(2.622)	(2.636)
Income		0.0599	0.144
		(0.301)	(0.312)
Housing assets		-0.0260	0.284
		(0.968)	(0.972)
Financial assets		-0.826	-0.951
		(1.073)	(1.057)
Education		-0.849	-0.696
		(0.619)	(0.641)
Financial literacy			-3.087
			(2.326)
Numeracy			-5.167
			(4.424)
Optimism			0.120
			(0.167)
Overconfidence			-0.988
			(2.347)
Trust			0.693
			(0.752)
Risk aversion			-8.945
			(7.740)
N	10,851	10,851	10,851

Table 3: Robustness tests: Alternative *Forecast Bias* measures and specifications

This table reports the coefficients of OLS regressions in which the dependent variable is the average lagged 12-month excess return of the stocks purchased by the subject within a month during the sample period 2011-2021. Excess return is calculated as the return in excess of the value-weighted Danish stock market return and is winsorized at the 1st and 99th percentiles. 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). All columns include the full set of control variables 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.

	(1)	(2)	(3)	(4)
Forecast Bias Residual	3.104**			
	(1.260)			
Forecast Bias Limited Information		2.862**		
		(1.306)		
Forecast Bias Rank			9.313**	
			(4.511)	
Diagnostic Expectations				2.167*
				(1.291)
Control variables	Yes	Yes	Yes	Yes
N	10,851	10,851	10,851	10,851

Table 4: Controlling for sample selection bias

This table reports the coefficients of a Heckman selection regression controlling for sample selection bias. In the first stage, the dependent variable takes a value of one if an individual shows up for the experiment. The selection equation includes the exogenous variables *Show-up fee* and *Travel time*. *Show-up fee* is the randomized show-up fee in the invitation letter of either 1,000 or 2,000 DKK. *Travel time* is the minutes of commuting time from the individual's neighborhood to the location of the experiment. In the second stage, the dependent variable is the average lagged 12-month excess return of the stocks purchased by the subject within a month during the sample period 2011-2021. Excess return is calculated as the return in excess of 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 person-month, and each observation is weighted by the number of months an individual made purchases. The first stage regression contains age, male, married, children indicator, post-Covid experiment indicator, income, housing assets, financial assets, and education. The second stage regression includes the full set of control variables 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.

Dependent variable	First stage: Show up (1)	Second stage: Excess return (2)
Show-up fee value (DKK)	0.058*	
	(0.034)	
Travel time to site (minutes)	-0.006**	
	(0.002)	
Forecast Bias		3.028**
		(1.292)
Control variables	Yes	Yes
N	34,713	10,851

Table 5: Subsample analysis based on proxies for financial sophistication

This table reports the coefficients of OLS regressions in which the dependent variable is the average lagged 12-month excess return of the stocks purchased by the subject within a month during the sample period 2011-2021. Excess return is calculated as the return in excess of 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 person-month, and each observation is weighted by the number of months an individual made purchases. Column (1) includes only respondents with more than 16 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. All columns include the full set of control variables 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.

	(1) Highly educated subsample	(2) High financial literacy subsample	(3) High numeracy subsample
Forecast Bias	3.913**	3.734**	2.576**
	(1.536)	(1.516)	(1.292)
Control variables	Yes	Yes	Yes
N	7,312	6,892	9,392

Table 6: Forecast bias and purchases based on past excess returns: Different horizons

This table reports the coefficients of OLS regressions in which the dependent variables are average lagged 12-month excess return of the stocks purchased by the subject within a month during the sample period 2011-2021. Excess returns are calculated as the return in excess of the value-weighted Danish stock market return and are winsorized at the 1st and 99th percentiles. In columns (1) through (6), the dependent variables are the lagged returns over the prior week, month, 3-months, 6-months, 12-months, and 36 months respectively. The key independent variable is *Forecast Bias*, which is adjusted to have a standard deviation of one. The unit of observation is person-month, and each observation is weighted by the number of months an individual made purchases. All columns include the full set of control variables 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.

	(1)	(2)	(3)	(4)	(5)	(6)
Excess return horizon	Prior 1-week	Prior 1-month	Prior 3-months	Prior 6-months	Prior 1-year	Prior 3-year
Forecast Bias	0.193	0.323	0.918*	1.494*	3.024**	8.031***
	(0.163)	(0.295)	(0.475)	(0.767)	(1.302)	(3.032)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
N	11,038	11,020	10,983	10,937	10,851	10,421

Table 7: Forecast bias and future excess returns

This table reports the coefficients of OLS regressions in which the dependent variables are average post-purchase excess returns of the stocks purchased by the subjects within a month during the sample period 2011-2021. Excess return is calculated as the return in excess of the value-weighted Danish stock market return and is winsorized at the 1st and 99th percentiles. In columns (1) through (6), the dependent variables are the post-purchase returns over the next week, month, 3-months, 6-months, 12-months, and 36 months, respectively. The key independent variable is *Forecast Bias*, which is adjusted to have a standard deviation of one. The unit of observation is person-month, and each observation is weighted by the number of months an individual made purchases. All columns include the full set of control variables 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.

Excess return horizon	(1) Future 1-week	(2) Future 1-month	(3) Future 3-months	(4) Future 6-months	(5) Future 1-year	(6) Future 3-year
Forecast Bias	0.021	-0.048	-0.245	-0.101	0.377	3.023
	(0.106)	(0.160)	(0.302)	(0.435)	(0.734)	(2.842)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
N	11,311	11,291	11,269	10,998	10,118	6,744

Table 8: Forecast bias and sales based on capital gains

This table reports the coefficients of OLS regressions in which the dependent variable equals one if the stock is sold and zero if the stock is held in the portfolio but not sold on a day when a stock is sold by the individual. The unit of observation is at the stock level, and each observation is weighted by the number of sales made by the individual. The sample period is 2013-2021. The key independent variables are *Forecast Bias x Performance measure*, where the performance measure is *Lagged 12-month excess return* in column (1), *Capital gain* in columns (2) and (3), and *Capital gain rank* in columns (4) and (5). Excess return is calculated as the return in excess of 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 variable 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 12-month 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	Lagged Excess Return	Capital Gain		Capital Gain Rank	
	(1)	(2)	(3)	(4)	(5)
Forecast Bias × Perform.	-0.003	-0.010**	-0.009**	-0.064***	-0.074***
	(0.010)	(0.004)	(0.004)	(0.023)	(0.023)
Performance measure	0.054***	-0.006	-0.145***	0.018	0.015
	(0.011)	(0.005)	(0.024)	(0.023)	(0.021)
Capital gain squared			0.059***		
			(0.01)		
Capital gain cubed			-0.005***		
			(0.001)		
I(Capital gain > 0)			0.107***		
			(0.018)		
Forecast Bias × $(CapGain\,rank - 0.5)^2$					0.115
					(0.073)
$(CapGain\,rank - 0.5)^2$					0.303***
					(0.065)
Capital gain highest					-0.005
					(0.025)
Capital gain lowest					0.022
					(0.027)
Control variables	Yes	Yes	Yes	Yes	Yes
N	155,302	172,095	154,308	172,095	154,308

Appendix Table 1: Variable definitions

Variable name	Definition
Forecast Bias	The forecast bias parameter estimated as in equation (2) of the paper. For ease of interpretation, we subtract 0.5 from the parameter estimate and divide 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 subtract 0.5 from the parameter estimate and divide it by its standard deviation.
Forecast Bias Limited Information	The forecast bias limited information parameter estimated as in equations (3) and (4) of the paper. For ease of interpretation, divide the parameter estimate by its standard deviation.
Diagnostic	The diagnostic expectations parameter estimated as in equation (5) of the paper. For ease of interpretation, we divide the parameter estimate by its standard deviation.
Prior 12-month return	The return on the purchased stock over the 12-month period ending the day prior to purchase
Prior 12-month market return	The return on the value-weighted Danish stock market over the 12-month period ending the day prior to purchase
Prior 12-month excess return	The difference between the prior 12-month return of the stock and the prior-12 month 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
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