Are Stated Expectations Actual Beliefs? New Evidence for the Beliefs Channel of Investment Demand*

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Abstract

Despite growing interest in expectation surveys, critics argue that survey responses are not reliable measures of the true expectations underlying financial decisions, and the empirical correlation between investment and stated beliefs is often weak. In this paper, we document a systematic gap between an individual's own forecasted home-price growth and her actual beliefs used in decision-making. In particular, we show that perceived *past* home-price growth is a stronger predictor of housing investment choices than a respondent's stated forecast of returns. Including perceived past returns as an additional factor improves the prediction of residential real estate investment decisions even after flexibly controlling for the forecasted distribution of future home-price growth. Despite this apparent extra reliance on past returns when making decisions, we find that stated expectations actually have lower forecast errors than past returns, ruling out simple measurement-error explanations. To interpret these findings, we extend recent models of cognitive uncertainty and mental defaults to incorporate risk aversion and present evidence suggesting that financial risk can induce risk-averse investors to rely on future-returns signals they deem less noisy.

Keywords: belief formation, cognitive uncertainty, expectations surveys, housing-market momentum, extrapolative beliefs, investment demand

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1 Introduction

A significant body of recent work in behavioral economics has sought to understand both how people form expectations and how these subjective assessments of the likelihood of future states (i.e., beliefs) affect actions (e.g., investment decisions).[1] As a result, several new expectation surveys have been developed to study the link between stated expectations and subsequent behavior.[2] However, critics of expectation surveys argue against their usefulness because respondents lack understanding of the questions or because their answers do not correspond to decision-relevant expectations (Cochrane (2017, 2011)). As a response, survey designers have proposed several techniques to reduce measurement error, for example, by asking the same questions in multiple framings (Glaser et al. (2007); Armona et al. (2018)), designing instruments for self-reported expectations (Armona et al. (2018); Giglio et al. (2019)), and eliciting both point estimates and expected distributions (Armona et al. (2018); Giglio et al. (2019)). However, even when researchers are able to both elicit beliefs and measure investment decisions for the same respondents, the empirical relationship between stated forecasts and actions is often weaker than predicted by theory (Giglio et al. (2019); Liu and Sui (2020); Giglio et al. (2020)).

In this paper, we show that the role of home-price beliefs in explaining investment behavior, which we refer to as the beliefs channel, is stronger when subjective *past* home-price growth is used as an additional predictor of behavior even conditional on stated expectations. Traditionally, researchers treat stated beliefs as a sufficient statistic for forecast-relevant data such as past price growth, implicitly assuming that past returns affect future investment only through the formation of the stated beliefs captured by survey. Such modeling of expectations and actions by first studying how expectations are formed and then how expectations affect actions permits a "divide-and-conquer" approach, where in the second step of modeling action prediction, the empiricist need not include any other variables in households' information set after controlling for their forecasts. In contrast, our results show that there is a direct empirical link from certain belief-formation factors to actions that bypasses stated beliefs.

To fix ideas mathematically, our findings can be illustrated as follows. In the classic single-asset model of portfolio choice with a single risky asset with normally distributed return r_{t+1}, the optimal share allocated to the risky asset is

$$\phi = \frac{E_t[r_{t+1}] - R_f}{\alpha \sigma_t^2},$$

[1] See Manski (2018) for a survey.

[2] See, for example, Giglio et al. (2019); Armona et al. (2018); Bailey et al. (2018); D'Acunto et al. (2018); Kosar et al. (2020); D'Acunto et al. (2019); Liu et al. (2020); Armantier et al. (2015).

where $E_t[r_{t+1}]$ is the expected return from t to $t+1$ conditional on all information available at time t, σ_t^2 is the conditional variance of r_{t+1}, α is the constant absolute risk-aversion parameter, and R_f is the risk-free rate. The risky-asset share depends on the distribution of returns used to form the expected return, and this expected return could depend on many factors. In a market with momentum, like the housing market, the prior period's return r_{t-1} could be one such factor used to predict $E_t[r_{t+1}]$. However, after conditioning on $E_t[r_{t+1}]$, σ_t^2 and α, r_{t-1} would not independently enter a rational portfolio-choice rule. In contrast, our main empirical result can be summarized as finding r_{t-1} affecting ϕ even after flexibly controlling for $E_t[r_{t+1}]$, measures of α, and the forecasted distribution of r_{t+1}.

Our analysis starts from the stylized investment experiment of Armona et al. (2018) run in the New York Federal Reserve Survey of Consumer Expectations, wherein respondents were asked to allocate a $1,000 investment between a 2% risk-free savings account and a housing fund with returns tracking local home price appreciation (HPA). In the same survey, Armona et al. (2018) also collected respondents' estimation of past returns (a subjective measure potentially differing from actual realized home-price growth), their forecasted home-price growth, and a rich set of demographics. We show that in this experiment, perceived past returns better predict investment behavior than do stated forecasted returns. Moreover, perceived past returns matter more than objective measures of past returns. Outside of this hypothetical experiment, perceived past home-price growth also improves prediction of intention to purchase a non-primary residence even after controlling for stated forecasted returns and the forecasted distribution of returns. We further verify that our results are robust to controlling for the rich set of demographics collected in Armona et al. (2018), addressing potential collinearity between forecasted returns and subjective past returns, and flexibly controlling for the forecasted distribution of returns to allow for any difference between risk-neutral and physical-risk beliefs.

Why do people rely on their memory of past returns when making investment decisions even conditional on how this memory affects their forecasts? We explore several explanations for our findings. We first address potential omitted variable bias from factors that are correlated with both beliefs and investment demand (e.g., risk aversion). We show that even after flexibly controlling for several additional factors that could be correlated with both investment demand and past returns, perceived past home-price growth still improves investment-decision prediction. Next, to address whether measurement error in survey responses can explain our findings, we consider the plausibility of a model wherein people base their investment decisions entirely on their memory of past returns but respond to the forecasted return question with this memory plus a random draw of noise. We show that empirically, forecasted home-price growth is a *better* forecast for future home prices than subjective past home-price growth in terms of mean squared forecast error. Whatever updates respondents make to subjective past returns when forming forecasted returns do have information content and are at least in this sense relatively rational beliefs. Taken together, it seems that respondents' decision process deviates from a fully rational framework where stated beliefs capture all decision-relevant information about future returns.

To motivate our preferred theoretical explanation for our findings, we collect additional data by asking respondents explicitly whether they rely more on past or expected returns when making decisions. This approach builds on a nascent survey-based literature in household finance which elicits both investors' decisions and asks them to self-examine the factors behind their choices (Liu et al. (2020); Chinco et al. (2019); Choi and Robertson (2020)). We rerun the investment experiment designed by Armona et al. (2018) in the 2020 wave of the same survey with one adjustment: before eliciting respondents' allocation of their $1,000 investments, we ask half of them (the treatment group) whether they consider their own return forecasts or their memory of past home-price growth more in their investment decisions.[3] This simple adjustment allows us to study three research questions and report the following results. First, in terms of summary statistics, we can test if there is a significant fraction of the population admits to relying more on their perception of past home-price growth than their own return forecasts. We find that 41% of respondents state that they base their decisions more on past returns than expected returns. This confirms that memory about the past indeed matters in decision-making on top of return forecasts for a meaningful fraction of households.

Second, we study the characteristics of respondents who claim to rely on past returns more than forecasted returns. We find that risk-averse individuals and respondents without a college degree are more likely to prefer past returns in their decision-making. However, even though college education is a strong predictor for choosing return forecasts over memory of the past, we show that reliance on past returns holds across education and income subgroups, suggesting that financial literacy is not a complete explanation for why investors overemphasize subjective past returns.

Third, by comparing our treatment group (those asked whether past or forecasted returns are more valuable to their decision-making) with the control group, we study whether the question itself nudges participants to rely more on forecasted returns. While we had hypothesized that this self-reflection could help correct any cognitive behavioral bias in overemphasizing past returns, we found that our treatment seems to encourage people to rely *less* on their return forecasts. This suggests that people consciously use memory of past returns to inform investment decisions rather than only passively through some subconscious

[3]Question framings are described in section 3 and Figure 1.

bias.

To interpret these empirical findings, we turn to a growing literature on limited attention and cognitive uncertainty (Enke and Graeber (2019); Gabaix (2014, 2019); Frydman and Jin (2019); Khaw et al. (2018)). We show that a model where financial risk induces risk-averse investors to rely on signals that they are more certain about is consistent with our evidence. Using an example similar to the one in Enke and Graeber (2019), when asked by a low-stakes survey question about subjective past and forecasted home-price growth, a risk-averse respondent might confidently reply 5% and 10%, respectively. However, when asked to make an investment decision, she might start to question her certainty of her own return forecast (e.g., "Is it really 10% as opposed to 7% or 13%?"). The risk-averse respondent may therefore shrink her stated forecast towards something that she is more certain about, an object referred to as the "mental default" in Enke and Graeber (2019). For example, imagine an agent observes signals on past home-price growth and future rent growth. While the optimal combination of both of them comprises a more accurate forecast of future returns than using past returns alone, the agent (mis)perceives the future rent-growth signal to be risky. Accordingly, when making an actual decision with higher stakes than a survey question about expected returns, the agent relies more heavily on past home-price growth instead of the combination of past returns and future rent growth. While other economic frameworks could also generate these findings, we provide direct evidence for certain factors playing a large role in stated forecasts and being shrunk in decision-making in section 6.4. We also note that our preferred cognitive uncertainty story is consistent with the strong correlation in the data between elicited risk aversion and a stated reliance on past returns.

Our paper makes the following contributions to the literature on beliefs. First, our results suggest that researchers could improve the measurement of the beliefs channel of decision-making by directly controlling for factors that affect beliefs in addition to stated beliefs themselves, contributing to the literature measuring the role of beliefs about returns on investment decisions (Armona et al. (2018); Giglio et al. (2019); Glaser et al. (2007)). At least in the housing market, such a factor appears to be perceived past returns, consistent with research emphasizing short-term price momentum in the housing market (Glaeser et al. (2014); Glaeser and Nathanson (2017); Armona et al. (2018); Guren (2018)).[4]

Second, our paper is directly related to work on limited attention and cognitive uncertainty in decision-making (Enke and Graeber (2019); Gabaix (2014, 2019); Frydman and Jin (2019); Khaw et al. (2018); Drerup et al. (2017)). For example, Drerup et al. (2017) allow investors' decision processes to deviate from a rational investment-return model and instead

[4]Whether our findings generalize to beliefs and investment decisions in other asset markets that do not feature price momentum is a useful avenue for future research.

follow some intuitive rule of thumb, with such departures from rationality potentially depending on an investor's financial sophistication. Building on this literature, Enke and Graeber (2019) propose that investors are often aware of their own cognitive noise (termed "cognitive uncertainty") and shrink their choices towards "mental defaults," or example, an even 50-50 split between risky and risk-free asset. Our work extends this literature by showing that recalled past returns serve as a plausible individual-specific mental default, generating between-investor variation in mental defaults contrasting with the mental default employed in Enke and Graeber (2019) that is assumed to be uniform across investors.[5] Our data, empirical setting, and model allow for an agent's mental default to vary across investors. Furthermore, by explicitly asking investors whether they rely more on forecasted returns or past returns and regressing their responses on a rich set of demographics, we find suggestive evidence that financial illiteracy and risk aversion are important drivers of cognitive uncertainty, broadly consistent with the finding of Enke and Graeber (2019) that cognitive uncertainty is more acute in more complex environments.[6] By comparing the treatment group with the control group in the 2020 experiment, we show that cognitive uncertainty seems to increase as we nudge investors towards self-reflection, i.e., cognitive uncertainty does not fade after more careful consideration. Taken together, our findings confirm the existence of cognitive uncertainty and suggest some of its important drivers. We show that investors' uncertainty about the same object can vary across survey questions, plausibly covarying with their attention to a given factor. In particular, financial risk could disproportionately increase subjective uncertainty for signals about which investors are relatively less certain.

Third, our results offer a potential solution to reconcile the strong evidence of personal experience as a belief driver that strongly affects behavior (Malmendier and Nagel (2016, 2011); Malmendier et al. (2019); Chiang et al. (2011); Kaustia and Knüpfer (2008); Nagel and Xu (2019)) and the somewhat weak empirical link between self-reported expectations and behavior found in recent papers. This puzzle begins with the growing literature on the "experience effect," anchored by evidence in Malmendier and Nagel (2011) that investors with lifetime experience of low real stock-market returns simultaneously have low stock-return expectations and low equity shares. Although this evidence is consistent with the experience effect working through the beliefs channel, recent work matching individual-level expectations data with trading records often finds only a modest empirical relationship

[5]Mostly out of necessity, the authors argue that "While we acknowledge that the mental default in general likely depends on a multitude of factors, we assume that in unfamiliar environments this default is influenced by an ignorance prior, which assigns equal probability mass to all states of the world ex ante."

[6]Broadly speaking, our paper is also consistent with the finding of Frydman and Jin (2019) that risk taking is more sensitive to more frequently occurring stimuli. In our context, subjective past experience is more salient to investors than their forecasts, which have yet to occur.

between stated beliefs and investment actions. For example, using administrative stock trading data with expectation surveys, Giglio et al. (2019) and Giglio et al. (2020) show that belief changes do not predict when trading occurs and explain the direction and magnitude of trades conditional on trading less than textbook models would imply. Similarly, Liu and Sui (2020) find that proxies of expectations for Bitcoin returns have minimal explanatory power for when investors trade but do predict some degree of trade directionality conditional on transacting. Our paper shows that the somewhat weak empirical link between stated beliefs and behavior could be caused by a wedge between decision-relevant expectation and stated forecasts. Instead of using what they state they believe on surveys when they make investment decisions, investors could base their actions on their subjective past experience, which could help explain strong experience effects contrasted with the weak predictability of stated beliefs.

The reminder of the paper is organized as follows. Section 2 presents a theoretical model adapting notions of cognitive uncertainty to our setting and allowing for a role of risk aversion. Section 3 describes the survey data used in our study and presents summary statistics. Sections 4 and 5 present nts descriptive evidence and our regression evidence, respectively. Section 6 discusses different interpretations for our results. Section 7 concludes.

2 Model

In this section, we provide a theoretical framework based on the nascent literature on cognitive imprecision (Enke and Graeber (2019); Gabaix (2014, 2019); Frydman and Jin (2019); Khaw et al. (2018)) that can rationalize our empirical findings. As argued in Enke and Graeber (2019), people are often aware of their own cognitive limitations and shrink their answers or behaviors towards a default value. Consider a GDP expectation survey as an example. Based on all available information, a respondent's best guess for next year's GDP growth could be 5%, termed the "signal" in Enke and Graeber (2019) because it incorporates signals the respondent has received. However, because the respondent is uncertain about this answer, she might shrink it towards a "mental default." One possible mental default is the average GDP growth in the postwar period of 3.2%. After shrinkage, the respondent might report 4% as her final answer.

In our context, we hypothesize that financial stakes such as monetary incentives induce risk-averse agents to rely more on signals about which they are more certain. Because there is no personal wealth on the line when answering a survey question about forecasted returns, respondents use all information available to them (e.g., 5% in the GDP example above). However, in the investment experiment and the real-world decision of buying an investment property, investors upweight to their perceived experiences as these experiences feel more salient or safe to them than other information.[7]

Let r_{t+1} denote the future return respondents are asked to forecast and assume agents believe $r_{t+1} \sim \mathcal{N}(\mu_d, \sigma^2)$, where, as in Enke and Graeber (2019), μ_d stands for the mental default of r_{t+1}. Agents form their forecasts using two pieces of relevant data. The first is their perception of past home-price growth, denoted as r_{t-1}. The second is a home-price forecast based on forecasts for variables related to home prices, including, for example, forecasts of rent, inflation, GDP, and local unemployment. We call the second piece of information the signal, denoted s. Both r_{t-1} and s are noisy forecasts for r_{t+1}, formalized as

$$r_{t-1} = r_{t+1} + \varepsilon_p \tag{1}$$

$$s = r_{t+1} + \varepsilon_s. \tag{2}$$

While $\varepsilon_p \sim \mathcal{N}(0, \sigma_p^2)$, respondents act as if the distribution of ε_s depends on the context of the particular survey question being asked. When asked to forecast returns, respondents treat the distribution of ε_s as $\mathcal{N}(0, \sigma_{s,e}^2)$, and when asked about investment choices, respondents act as if the distribution of ε_s is $\mathcal{N}(0, \sigma_{s,i}^2)$, with $\sigma_{s,i} > \sigma_{s,e}$.[8] In a reduced-form way, the assumption $\sigma_{s,i} > \sigma_{s,e}$ captures that in forecasting returns, respondents focus on the level of s and to certain extent ignore the noisiness of s. In contrast, when making an investment decision with monetary incentives, risk-averse respondents more fully attend to the uncertainty in s and the resulting uncertainty in their forecast of r_{t+1}.

An alternative motivation for this assumption is that the investment question is more complex than the return forecast question, with additional factors to consider such as risk bearing capacity. Enke and Graeber (2019) find that investors also display more cognitive uncertainty when facing more complex choices. This added complexity could affect the perceived uncertainty in s more than in r_{t-1} because past experience is salient to investors and relatively unaffected by question framing. Another rationalization that generates a disproportionate increase in perceived uncertainty in s relative to r_{t-1} is through the endogenous attention framework of Gabaix (2014) that would lead to respondents having different loss functions in answering the expectation and the investment questions. We view these explanations as conceptually similar to our risk-based explanation. Whether driven by risk, complexity, or sparsity, the end result is that because of differential stakes when reporting forecasts versus making consequential financial decisions, agents may weight factors differently in each domain.

[7] See, e.g., Malmendier and Nagel (2011) for support for this personal-experience channel.

[8] The true distribution of ε_s is allowed to be different from $\mathcal{N}(0, \sigma_{s,e}^2)$ and $\mathcal{N}(0, \sigma_{s,i}^2)$. Our results are independent of any deviations between the perceived distribution of ε_s and the true distribution.

Let r_e and r_i denote a respondent's reported return forecast and the decision-relevant forecast. We have

$$r_e = E[r_{t+1}|r_{t-1}, s, (r_d, \sigma, \sigma_p, \sigma_{s,e})] = c_e + \beta_{1,e} r_{t-1} + \beta_{2,e} s \tag{3}$$

$$r_i = E[r_{t+1}|r_{t-1}, s, (r_d, \sigma, \sigma_p, \sigma_{s,d})] = c_i + \beta_{1,i} r_{t-1} + \beta_{2,i} s, \tag{4}$$

where by Bayesian updating

$$\begin{aligned}
\beta_{1,e} &= \frac{\sigma_{s,e}^2(\mu_d^2 + \sigma^2)}{(\sigma_{s,e}^2 + \sigma_p^2)(\mu_d^2 + \sigma^2) + \sigma_p^2 \sigma_{s,e}^2} \\
\beta_{2,e} &= \frac{\sigma_p^2(\mu_d^2 + \sigma^2)}{(\sigma_{s,e}^2 + \sigma_p^2)(\mu_d^2 + \sigma^2) + \sigma_p^2 \sigma_{s,e}^2} \\
\beta_{1,i} &= \frac{\sigma_{s,i}^2(\mu_d^2 + \sigma^2)}{(\sigma_{s,i}^2 + \sigma_p^2)(\mu_d^2 + \sigma^2) + \sigma_p^2 \sigma_{s,i}^2} \\
\beta_{2,i} &= \frac{\sigma_p^2(\mu_d^2 + \sigma^2)}{(\sigma_{s,i}^2 + \sigma_p^2)(\mu_d^2 + \sigma^2) + \sigma_p^2 \sigma_{s,i}^2}.
\end{aligned}$$

Because $\sigma_{s,i} > \sigma_{s,e}$, we have that $\beta_{1,e} < \beta_{1,i}$ and $\beta_{2,e} > \beta_{2,i}$. Intuitively, if respondents perceive their signal s to be noisier in the investment-decisions domain than the forecasting-returns domain, they will rely more on their past experience r_{t-1} and less on the signal s.

Our experiment asks respondents to allocate a fixed amount investment between a housing fund and a risk-free savings account. To map the decision-relevant return forecast r_i to the share invested in a housing fund, we return to the standard single risky asset model with constant absolute risk aversion used in the introduction, with the housing share ϕ given by

$$\phi = \frac{r_i - R_f}{\alpha \sigma_i^2},$$

where R_f is the risk-free rate, α is the absolute risk aversion parameter, and σ_i^2 is the conditional variance of r_i after considering r_{t-1} and s. Taking a linear approximation of ϕ around the average value of r_i,α, and σ_i^2, and letting γ_α and γ_σ denote the partial derivatives of ϕ over α and σ_i^2, we have

$$\begin{aligned}
\phi &\approx \tilde{c} + r_i + \gamma_\alpha \alpha + \gamma_\sigma \sigma_i^2 \\
&= c_1 + \beta_{1,i} r_{t-1} + \beta_{2,i} s + \gamma_\alpha \alpha + \gamma_\sigma \sigma_i^2 \\
&= c_2 + \left(\beta_{1,i} - \beta_{1,e}\frac{\beta_{2,i}}{\beta_{2,e}}\right) r_{t-1} + \frac{\beta_{2,i}}{\beta_{2,e}} r_e + \gamma_\alpha \alpha + \gamma_\sigma \sigma_i^2.
\end{aligned}$$

By $\beta_{1,e} < \beta_{1,i}$ and $\beta_{2,e} > \beta_{2,i}$, the coefficient on subjective past experience r_{t-1} is positive even conditional on the stated forecast r_e, consistent with our empirical findings below. Further, if we assume that all respondents share the same $\sigma_{s,e}$ but that more risk averse and less sophisticated respondents have larger $\sigma_{s,i}$, we have that these respondents rely more on their subjective past experience and less on return forecasts than other respondents (consistent, for example, with the empirical results of Appendix Table A9). Such heterogeneity can be motivated with more risk-averse respondents being as confident about their signal s as other respondents in answering the return forecast question but recognizing more uncertainty in s than other respondents in answering the investment-decision question.

3 Data and Summary Statistics

Our data come from the Federal Reserve Bank of New York's Survey of Consumer Expectations (SCE). The SCE is an internet-based survey of a rotating panel of approximately 1,200 household heads from across the US. The survey elicits expectations about a variety of economic variables, such as inflation, stock market returns, GDP growth, and the unemployment rate. Respondents participate in the panel for up to twelve months, with a roughly equal number rotating in and out of the panel in each month. For a detailed overview of the SCE, see Armantier et al. (2017).

The data that we use are mainly from the housing module of the SCE, which we will often refer to as the housing survey. The housing module is an annual survey fielded in February every year since 2014 to the active panel members in the SCE. The housing module has multiple blocks of questions, collecting perceived past home-price growth, housing choice and mortgage credit history, expectations of future home-price growth and credit conditions.

We use three samples throughout the paper. Our analysis starts with the 2015 sample. One unique advantage of the 2015 sample is that it includes the investment experiment designed by Armona et al. (2018). Respondents are asked how they would allocate a $1,000 investment between a 2% risk-free savings account and a housing fund that tracks home-price appreciation in their local zip code. To provide real-world stakes, respondents were promised a random chance at receiving the actual returns of their investment. Usefully for our purposes, this experiment is not subject to any real-world constraints in housing-related behavior. For example, some borrowers might want to invest in housing but do not believe they qualify for a mortgage or have sufficient cash on hand. By abstracting away factors like this, the hypothetical investment question offers a constraint-free measure for investment choices. While we use the housing share in allocation of $1,000 as our primary measure of investment behavior, we also examine other housing-related behaviors, including the probability of buying a non-primary residence. Working with a published data set also enables us to use the same sample and start from the same specifications as in Armona et al. (2018) for comparability and transparency.

The second sample that we use is a combined sample based on the 2015-2020 housing surveys with six years of data. Although the $1,000 investment question was not asked from 2016-2019, we use data from these later years to show that our other results hold in other years. Our final sample is the 2020 housing survey. In addition to repeating the investment experiment of the 2015 data, we add to the 2020 survey the additional feature of asking half of the respondents whether they base their investment decisions more on past returns or expected returns. Figure 1 reports this survey question page, with treatment and control questions n panels A and B, respectively.

3.1 Survey Questions

This section provides more details on how the relevant survey questions are framed.

Framing of Past and Future Home Price Changes Respondents are asked about home price changes in their zip code over the last 12 months and the last 5 years and how they expect home prices to change in their zip code over the next 12 months and 5 years. These questions are framed in three alternative formats with each respondent randomly shown one of the three formats.[9] In all multivariate specifications, we control for indicators of which format was used for a given respondent.

1. Questions asked in terms of the levels of house prices: For example, past one-year home-price change perceptions were elicited as follows: "*You indicated that you estimate the current value of a typical home in your zip code to be [X] dollars. Now, think about how the value of such a home has changed over time. (By value, we mean how much that typical home would approximately sell for.) What do you think the value of such a home was one year ago?*"

2. Questions asked in terms of percentage changes: For example, past one-year home-price change perceptions were elicited as follows: "*Now, think about how the value of such a home has changed over time. Over the past 12 months, how has the value of such a home changed? (By value, we mean how much that typical home would approximately sell for.) [increased/decreased]*" followed by *"By about what percent do you think the value of such a home has [increased/decreased] over the past 12 months? Please give your best guess.*"

[9]Having multiple framings is motivated by Glaser et al. (2007), who find that framing affects how survey respondents report expected stock returns. See Armona et al. (2018) for further discussion.

3. Questions asked in the same way as in the percentage-change frame above except that the changes were in terms of dollar amounts: For example, past one-year home-price change perceptions were framed as "*By about what dollar amount do you think the value of such a home has [increased/decreased] over the past 12 months? Please give your best guess.*"

Expected Housing Choice and Investment Decision Survey respondents are also asked about their anticipated housing-related behavior.

1. Investment in a housing fund: *"Consider a situation where you have to decide how to invest $1,000 for one year. You can choose between two possible investments. The first is a fund that invests in your local housing market and pays an annual return equal to the growth in home prices in your area. The second is a savings account that pays 2% of interest per year. What proportion of the $1,000 would you invest in the housing market fund and the savings account?"*

2. Probability of buying a primary residence: "*And if you were to move to a different primary residence over the next 3 years, what is the percent chance that you [or your spouse/partner] would buy (as opposed to rent) your new home?*"

3. Reasons for renting the next primary residence: "*Which of the following are reasons you would rent and not purchase a home if you were to move over the next three years?*" Respondents are offered 12 options to choose from and can also specify other unmentioned reasons. Of particular interest to us are the first three reasons: "*I don't make enough money,*" *"I don't have enough money saved up, or I have too much debt,*" and "*My credit is not good enough."*

4. Probability of buying an investment property: "*What is the percent chance that over the next 3 years you [or your spouse/partner] will buy a home that you would NOT use as your primary residence (meaning you would use it as a vacation home, or as an investment property, etc.)?*"

5. Evaluating housing in their zip code as an investment: "*If someone had a large sum of money that they wanted to invest, would you say that relative to other possible financial investments, buying property in your zip code today is*:" with options including "*A very good investment,*" "*A somewhat good investment,*" "*Neither good nor bad as an investment,*" "*A somewhat bad investment,*" and "*A very bad investment.*"

3.2 Summary Statistics

Table 1 reports summary statistics for the 2015-2020 sample. We discuss summary statistics for the 2020 sample in greater detail by treatment and control in section 6.3. Table 1 shows that the average age in our sample is 51 years old. Homeowners comprise 76% of respondents, 29% have household income higher than $100,000, and 57% are college educated. Respondents were asked a series of five questions based on Lipkus et al. (2001) and Lusardi (2008) that provide an individual-specific measure of numeracy. We code the number of correct answers (ranging from 0 to 5) as a covariate. There is strong correlation between the numeracy score and education or income, consistent with Lusardi (2008). For example, 53% of the college graduates in our sample answered all 5 questions correctly, compared with 30% among respondents without a college degree. Similarly, 59% of households with income over $100,000 answered scored 5 out of 5, compared with 37% among other households. Later in the paper, we use the numeracy score, college education, and income as proxies for financial literacy to explore potential drivers for cognitive uncertainty.

We note that as an online survey, the SCE oversamples college-educated and high-income households. In general, we expect any bounded rationality identified in the SCE sample to be stronger in the overall population. Using a SCE-ACS weight to calculate nationally representative statistics, we verify that our results are largely unchanged or stronger after weighting the observations. For example, for the self-reflection question in 2020, 48% of our weighted respondents report that they base their decisions on past returns, higher than the 41% number before weighting.

On average, households perceive that local home-price growth over the past 12 months was around 4% and expect an average of 4% local home-price growth over the next 12 months. Both perceived past HPA and HPA forecasts show substantial heterogeneity, with standard deviations of 6% and 5%, respectively. There are also differences between perceived and objectively measured past experiences, which we term the perception gap. The average absolute perception gap is 5 percentage points, indicating that on average, people's perception of last year's local returns is five percentage points away from objectively measured average local returns. Both the actual experience and the perception gap affect investors' choices with similar coefficients after controlling for the forecasted distribution of future returns.

Our primary outcome variable is the average share of $1,000 invested in the housing fund and averages 54% and 61% in 2015 and 2020 respectively, both with standard deviations over 30%. For real-world outcomes, the average self-reported probability of moving in the next three years is 30%. Among those who reported an over 5% moving probability, 67% expect to buy their next primary residence. Around 9% of respondents expect to buy an investment property within the next 3 years.

4 Descriptive Evidence

To illustrate our core findings, we first present graphical evidence on the relationships between investment actions and forecasted and perceived past home-price growth. Figure 2 shows binned scatter plots of shares invested in the housing fund out of a $1,000 investment versus home-price growth, both forecasted returns (left-hand graph) and perceived past returns (right-hand graph) along with 95% confidence intervals. We note several takeaways from panel A. First, the fitted lines in the two graphs have about the same slopes, meaning that the bivariate coefficients from regressing the housing share onto forecasted and perceived past HPAs are about the same.[10] Second, bin means in the forecasted HPA graph are much further away from the fitted line than bin means in the perceived past HPA graph on the right, implying that the statistical relationship between investment and forecasted HPA is weaker than the one between investment and the perceived past HPA. Similarly, the confidence intervals for the forecasted HPA are much wider than the ones for the perceived HPA. These observations about Figure 2 are surprising for two reasons. First, we generally expect an investor's forecasted return to be a summary of all past information relevant to expected returns used in her decision-making (with the caveat of not controlling for the distribution of expected returns, which we will address in our regression evidence). It is therefore striking to see that the perceived past HPA is a stronger empirical predictor for investment than forecasted HPA.

Second, using the logic of instrumental variables, the similar magnitudes of the relationship between investment and stated beliefs and perceived past returns is prima facia evidence that perceived past returns independently affect beliefs. Recall that the reduced-form relationship between an outcome and an instrument that affects that outcome only through an intermediating covariate is equal to the causal effect of that covariate on the outcome times the first-stage coefficient of the covariate on the instrument. Armona et al. (2018) find that for every 1 percentage point higher perceived past HPA, forecasted HPA goes up by around 0.2 percentage points. If perceived past HPA affects behaviors only through the expected HPA, we would expect the reduced-form coefficient on perceived past HPA to be roughly 0.2 times the coefficient on the expected HPA (again with the caveat of not controlling for other confounds, which we will address in section 5). This discrepancy between estimated magnitudes and magnitudes that would be expected if perceived past returns affected outcomes only through stated expectations is further evidence of a difference between stated and decision-relevant beliefs.

Figure 3 presents graphical evidence in a different way. Here in each plot we control for the

[10]The bivariate coefficients for the forecasted and the perceived past HPAs are 1.03 and 1.19, respectively.

other home-price growth variable linearly. For example, in the perceived HPA graph on the right-hand side, we control for the forecasted HPA. Comparing Figure 2 with Figure 3, we can see that after controlling for the perceived past HPA, the relationship between the forecasted HPA and investment is significantly attenuated, whereas the coefficient on perceived past HPA is almost unchanged after controlling for the forecasted HPA. In contrast, the traditional approach of assuming that belief-relevant data affects actions only through beliefs would predict that conditional on forecasted HPA, perceived HPA would lose predictive power for actions and not vice versa.

5 Home-Price Beliefs and Behavior

Before presenting our main results on how forecasted and perceived past home-price growth predict investment behavior, Table 2 studies the relationship between the perceived past and the forecasted home-price growth.[11] These results demonstrate that perceived past home-price growth is an important factor considered by investors in their stated beliefs, making it a plausible mental default for return forecast in investment decisions. Column 1 of Table 2 regresses the expected home-price growth on the perceived past home-price growth in a bivariate regression. Columns 2 to 4 add individual controls and forecasted fundamentals, both separately and together. Across all specifications, there is a strong relationship between the perceived past and the forecasted home home-price growth, showing that respondents incorporate past returns into their return forecasts. Every one percentage point higher perceived past home-price growth is associated with 22 basis points higher forecasted home-price growth, controlling for forecasted fundamentals and individual controls.

5.1 Perceived Past Home-Price Growth and Behavior

To estimate the relationship between perceived returns, stated beliefs, and investment decisions, our main regression model is

$$Y_i = \alpha + \beta_1 HPA_{i,t-1} + \beta_2 HPA_{i,t+1} + X_i'\phi + \varepsilon_i, \tag{5}$$

where $HPA_{i,t-1}$ and $HPA_{i,t+1}$ are respondent i's perception of home-price growth over the last 12 months and her expected home-price growth over the next 12 months, respectively, and Y_i is an investment outcome of interest. In our baseline specifications, we consider the share of a \$1,000 investment allocated to a housing derivative tracking local home-price

[11] See related specifications estimated on a similar sample in Armona et al. (2018).

growth. Additional specifications consider the stated probability of buying a primary or a non-primary residence in the next three years. The vector X_i is a particularly rich set of demographic controls relative to the prior literature on beliefs and contains binary indicators for owning a home, numeracy, ethnicity, gender, marital status, education, labor force status, census region, a quadratic in age, and logs of household income, home equity, liquid savings, and personal debt.

We begin with the housing fund share as the outcome variable. Table 3 examines whether perceived past home-price growth improves action prediction after controlling for an individual's forecasted home-price growth. Columns 1 to 3 regress the housing fund investment share on expected and perceived past returns, both separately and together. The bivariate regression results in columns 1 and 2 report coefficients for the past and future returns with similar magnitudes, although the coefficient on the perceived past return is more precisely estimated than the bivariate coefficient on the expected return. The R^2 for the perceived past HPA in column 2 is larger than the one for the expected HPA in column 1, suggesting that at least in the investment-experiment sample, perceived past HPA can explain more variation in the outcome variable than can expected HPA. In column 3, when we include both return variables in one specification, perceived past returns still have statistically significant predictive power for the housing investment allocation.

Whether these bivariate results demonstrate that stated beliefs are not a sufficient statistic for actual beliefs depends on whether perceived past returns are simply correlated with other non-belief factors that influence investment demand. As a first step to assess the potential role of omitted variables, columns 4 to 6 add the same demographic controls as in Armona et al. (2018). Of particular interest, these controls include a dummy for above-median self-reported risk aversion, helping us address potential endogeneity from high past returns causally increasing risk tolerance (Malmendier and Nagel (2011); Meeuwis (2019)).[12] In column 6, which includes both expected and perceived-past HPA and the full set of demographic controls, perceived past HPA still has a both statistically and economically significant effect on investment decisions. A one percentage point higher perceived past HPA is associated with 83 basis points higher share allocated to a local housing fund in contrast to the traditional approach assuming that past information affects decisions only through expectations and omitting past information in action-prediction regressions.

Another consideration when interpreting Table 3 is that we do not control for the expected distributions of return. For example, it could be that investors believe that past home-

[12]While separating higher risk tolerance from higher expected returns with survey evidence is always challenging (cf. Malmendier and Nagel (2011)), later in Tables 5 and A8, we account for risk aversion in more flexible ways.

price growth is a strong predictor for *downside* risk even conditional on the expected mean. Without controlling for downside risk, the statistically significant coefficient for the perceived past home-price growth could be driven by investors basing their decisions on downside risk. To address this, Table 4 includes a number of controls for the forecasted distribution of returns. In the survey, we ask respondents about their belief probabilities of home prices going up by more than 10%, up between 0% and 10%, down by less than 5%, and down by more than 5%. In column 1, we add the probability of a decline in home prices, which Armona et al. (2018) show is a strong predictor for decisions, to the specification in column 6 of Table 3. In column 2, we further add the other two self-reported probabilities. In column 3 and 4, we add a quadratic and cubic, respectively in each return-range probability. Across all these specifications, the relationship between perceived past home-price growth and investment decision remains statistically significant. Comparing column 3 with column 4, we also observe that adding incremental flexibility of a cubic in the forecasted distribution moments adds very little to the adjusted R^2 and almost does not change the coefficient on perceived past home-price growth, suggesting that our specification of the distribution of returns is sufficiently flexible. One might argue that we only measure the forecasted distribution of returns through four coarse bins, which limits our power. For example, we only ask respondents about the probability of home prices going down by more than 5% but perhaps what affects their decision-making is their belief probabilities of home prices going down by more than 10%. While our sample sizes prevent us from being fully nonparametric about the expected distribution of returns, our results are also robust to restricting our sample to those who placed zero probability on a home-price decline larger than 5%.

Collinearity between forecasted home-price growth and subjectively measured past home-price growth could also make it challenging to interpret the coefficients separately for these two return measures, although a priori, such collinearity should bias us against finding evidence that past returns matter even conditional on stated forecasts. To address this, in columns 5 and 6 of Table 4, we include one return variable linearly in our specification while controlling for the other return variable flexibly through bin fixed effects. For example, in column 5, we first divide our observations into 50 equally sized bins according to their perceived past HPA. We then control for fixed effects for these bins and also control for the expected HPA linearly. Similarly, in column 6, we control for bin fixed effects for the expected HPA and report a linear coefficient for the perceived past HPA. Bin fixed effects allow us to control for one factor relatively nonparametrically and thereby absorb any correlation between perceived past returns and forecasted returns.[13] Column 6 shows that subjective

[13]Note that because survey responses bunch around round number like "0%", "5%", or "10%", the actual number of bins tends to be smaller than the specified target number of equally sized bins. This is because,

past home-price growth remains an important predictor for investment behavior even after controlling for the forecasted home-price growth in a flexible way. Appendix Table A2 verifies that this result is robust to different numbers of bins for the returns variables.

We conduct several other robustness tests to probe the validity of our finding that while respondents incorporate past returns into their return forecasts, they increase their emphasis on past returns when actually making decisions. For example, our online survey oversamples high-income and educated households. To verify that our results hold in the general population, we weight observations using ACS-SCE sampling weights and show qualitatively similar results in Appendix Table A1. The hypothetical investment experiment that we study so far is from the baseline stage in Armona et al. (2018), where respondents were not incentivized. In Appendix Table A3, we show that our results hold for the smaller subsample whose investment decisions were incentivized with the possibility of receiving the realized gross return of their composite housing and savings fund with their chosen weights.[14] Also, the results of Bordalo et al. (2020) raise the possibility that past returns are correlated with beliefs about future fundamentals, a potentially important component of investment demand distinct from beliefs about future housing returns. We address this concern in Appendix Table A4, which shows that our results are also robust to controlling for forecasted fundamentals. Finally, Appendix Table A5 verifies that the perceived past HPA has added predicting power for investment decisions on top of actual past HPA. In column 2, where both the perceived past and the actual past are controlled, past perception has a statistically significant coefficient.

In all specifications with both forecasted and perceived past home-price growth in Tables 3 and 4, the forecasted return has a coefficient that is not statistically different from zero. However, we do not view this as evidence that expectations do not matter in investment decisions. First, our sample size is relatively small, and this limits our statistical power. Second, in most columns, even though the coefficient for the forecasted home-price growth is not statistically significant, it still has a positive and the expected sign. Third, the coefficients for the *distribution* of returns are often significant. As demonstrated in Armona et al. (2018), downside risk is a stronger predictor for certain housing-related behaviors than the forecasted return on housing, consistent with the broader downside risk asset pricing literature (Lettau et al. (2014); Farhi and Gabaix (2016)). Finally, we can reject the null that the coefficients for both the level and the distribution of the forecasted return are jointly zero, as expected

for example, 12.3% of the respondents answered “0%” as their forecasted home-price growth and these respondents are always put in the same bin, independent of the number of bins that specified. We report both the number of specified bins and actual bins.

[14]The Appendix Table A3 sample corresponds to the control group in Armona et al. (2018). We choose this sample because they did not receive any information between and baseline stage and the incentivized stage, whereas the two treatment groups in Armona et al. (2018) received factual information on past home prices before the incentivized stage. See Armona et al. (2018) for further details on experimental design.

if multicollinearity is the cause of individually statistically insignificant coefficients.

Taking stock, in all specifications, past HPA improves the prediction of investment decisions even conditional on stated beliefs. Moreover, this finding is robust to flexible specifications and explanations based on collinearity. This is consistent with the empirically weak predictive power of stated beliefs to explain investment actions relative to theoretical benchmarks (see Giglio et al. (2019); Liu and Sui (2020); Giglio et al. (2020)). Still, our main point of emphasis is not to reject the beliefs channel but to demonstrate that allowing subjective past home-price growth to capture some of the gap between decision-relevant and stated expectations strengthens the empirical connection between beliefs and investment. In the remainder of this section, we test for cross-sectional heterogeneity in the emphasis of past returns in decision making and verify our results hold with other measures of housing investment.

5.2 Heterogeneity

We investigate heterogeneity across different subgroups in our sample to test potential explanations for our findings. We divide our sample into homeowners and renters, age above 50 and below 50, male and female, college graduates and not, those with household income above and below $75,000, and those with a high and low numeracy scores.[15] Appendix Table A6 reports the results of estimating (5) for each subsample. Across most subgroups, even after controlling for the forecasted distribution of future returns and demographics, perceived past home-price growth strongly predicts investment choices.[16] One important exception is renters, for whom the only return-related variable with a statistically significant coefficient is the downside risk. One potential explanation is that renters are averse to downside risk in home prices and therefore avoid buying a home. The results show that conditioning on downside risk, renters do not consider either the perceived past or forecasted returns. That our results hold among both the college educated and non-college educated and among both those with household income higher and lower than $75,000 suggests that it is unlikely that our results are entirely due to lack of financial literacy or misunderstanding the question.[17]

[15]Given that our surveys are answered by household heads, we note that male and female household heads could have different characteristics than average males and females in the general population.

[16]The results of Appendix Table A6 are further robust to controlling for a cubic in the probabilities that make up the forecasted distribution of returns.

[17]Our results also hold for households with high numeracy scores. For the low numeracy subsample, the coefficients have the right signs, although not statistically significant, potentially because of the much smaller sample size.

5.3 Other Housing-Related Behaviors

To examine robustness to alternative measures of investment beyond the investment experiment, we extend our analysis to housing-related behaviors including the probability of buying a non-primary residence (including both investment and vacation homes) within the next three years, the probability of buying the next primary residence conditional on moving within the next three years, viewing housing as a good investment. These variables are collected in all years between 2015 and 2020, and, unlike the housing-fund investment experiment, are subject to real-world constraints. For example, borrowers who would like to invest in housing might not qualify for a mortgage or be interested in moving. Accordingly, we a priori expect the relationship between returns, forecasted or subjective historical and behavior to be weaker than in the investment experiment, similar to the findings of Armona et al. (2018).

Appendix Table A7 reports regression estimates using alternative investment action outcomes. Columns 1 and 2 show that there is a strong correlation between perceived past home-price growth and the probability of buying a non-primary home. For buying a primary residence, column 4 shows that the coefficients for neither year-ahead or subjective year-past home-price growth are statistically significant after controlling for demographics and the expected distributions of future returns. Again, this result could be in part due to constraints and confounds. For example, places with the highest past home-price growth tend to be high cost areas, creating added challenge for households to become homeowners, even if they do believe home prices will continue to rise. Columns 5 and 6 show that both forecasted and subjective past home-price growth are strong predictors of viewing housing as a good investment. Taken together, controlling for past returns improves the ability of belief factors to predict real-world investment outcomes beyond in the investment experiment.

6 Interpretation

In this section, we explore interpretations of the empirical findings in Section 5. First, we rule out alternative explanations, including omitted variable bias (section 6.1) and measurement error in stated home-price expectations (section 6.2). While there are surely omitted variables and measurement errors in stated beliefs, we show that they are unlikely to fully explain our results. To explore other potential drivers, we ask half of the respondents (treatment group) in the 2020 survey whether they value subjective past returns more or return forecasts more in decision-making and report results in section 6.3. Among other results, we show that lack of financial sophistication (proxied by non-college graduates) and risk aversion are both strong predictors for choosing perceived past HPA over forecasted HPA. Also, the treatment group rely less on their return forecasts than the control group do, consistent with an explanation based on cognitive uncertainty. In Section 4, we present direct evidence for rent forecast as a "shrunk factor" (denoted as s in equations (1) to (4)) in the cognitive-uncertainty framework of section 2 .

6.1 Omitted Variable Bias and Risk Aversion

Potential omitted variables depend on the outcome variable of interest. For example, when the dependent variable of interest is the probability of buying a primary residence, omitted variables include preference for home ownership, the relative quality of owner-occupied and rental housing in a respondent's local area, the likelihood of moving regions, etc. When the outcome variable is share invested in the housing fund, the environment is much simpler, motivating our use of this hypothetical investment question. Presumably, an investor's decision about such a derivative investment is a function of only the forecasted distribution of return distribution and risk aversion. For the forecasted distribution of home-price growth, we control for the subjective probabilities of future returns falling into four ranges and the polynomials of them in Table 4. Table 5 further explores the role of risk aversion in explaining our results. Column 1 reports estimates from a bivariate regression of the housing investment share onto risk tolerance, measured on a 1 to 10 scale. The coefficient is both economically and statistically significant. Moving the risk tolerance from 1 to 10 increases the housing share by as much as 30%, suggesting that our risk tolerance variable is a reasonably meaningful measure of risk appetites. In columns 2 and 3, respectively, we add the risk tolerance measure to our baseline specification linearly and by controlling for indicators of each potential value from 1 to 10. Even after controlling for risk tolerance in these two different ways, there is still a strong correlation between the perceived past home-price growth and the housing investment share, suggesting that our results cannot be fully explained by risk aversion as an omitted variable.

Another story related to risk aversion that we address is through the wealth channel. Large past home-price growth increases households' net worth, and could reduce their absolute risk aversion parameter, if for example we model households having constant relative risk aversion or decreasing relative risk aversion as found in Meeuwis (2019). To address this story, we first note that the average gain in home value in our sample is around \$6,000, relatively small compared with a median annual income of \$75,000, unlikely to cause a large shift in risk aversion. Also, in Appendix Table A8, we interact past home-price growth with measures for importance of home equity in a household's portfolio. These measures include leverage in their primary residence, home values divided by their net assets, and home values divided by their income. The idea is that for two households with the same demographics (including income) and the same perceived past home-price growth, home price appreciation for the household with a higher home value is more likely to affect their investment decision through higher net worth and the associated lower risk aversion than for the other household. Across all specifications, none of the coefficients for the interaction terms are statistically significant, alleviating concerns for lower risk aversion caused by rising wealth.

6.2 Measurement Error in Home Price Expectations

Could our results in section 5 stem from noise in survey responses? Such an explanation finds plausibility in the fact that the forecasted home-price growth exhibits bunching around "0%", "5%", "10%", etc. Perhaps investors base their decisions entirely on the perceived past home-price growth, and for the expected return question, they report perceived past plus random noise to round to a cognitively accessible round number. Several pieces of evidence are inconsistent with this interpretation. Foremost, we show that the stated beliefs are actually *less* noisy with lower forecast errors such that although they are less predictive of investment actions, stated beliefs are a more accurate forecast than perceived past home-price growth. In other words, far from being simply a noised-up measure of true individual expectations, self-reported home price expectations do have information content relative to the single factor of perceived past returns.

Panel A of Table 6 reports summary statistics for forecast errors. The forecasted returns have larger mean error than the subjective past returns but have smaller mean absolute error and mean squared error than the subjective past returns. Based on mean absolute error and mean squared error, the forecasted return better predicts the future than the subjective past, at least marginally. However, one could argue that both the forecasted and subjective past home-price growth must be noisy, and the difference between their mean absolute errors shown in the second row of panel A is modest. Also, mean absolute and squared errors could be influenced by outliers. To address these, panel B of Table 6 studies the information content in forecasted returns in a different way. We first restrict our sample to the 86% respondents who reported a forecasted return different from their perception of past returns. Next, we examine whether the updating from the subjective past to the forecasted return is at least in the right direction. For example, looking at the first row of panel B, out of the 2,746 respondents reporting a higher forecast than their subjective past, 56% of them indeed experienced a higher future return than their subjective past. While little more than half of these optimists update in the right direction, this result is much stronger in the downward updating group, where 80% update in the correct direction.[18]

6.3 Direct Survey Measures of Decision Factors

For a more direct measure of decision-making factors, we ask half of the 2020 respondents whether they rely more on their own forecasted returns or past home-price growth when making investment decisions. The question framing is discussed in Section 3 and illustrated in Figure 1. We ask this question of a randomly selected subset of respondents because answering the question itself could change respondents' behavior, and we are interested in testing whether any induced self-reflection affects subsequent answers to investment questions. We report summary statistics for the 2020 responses separately for the control group, the treatment group, and within the treatment group separately for those answering that they rely more on future returns or past returns. We also study the characteristics of respondents choosing future returns over past returns to adjudicate theories for why some respondents weight past returns so heavily when making investment decisions even conditional on forecasted returns. Finally, we compare the investment decisions of the treatment and control groups to explore whether the self-reflecting question itself affects decision-making.

Table 7 presents summary statistics for the 2020 sample. The first two columns present average characteristics for the treatment group and the control group separately.[19] The two groups have similar characteristics, as expected given random assignment. Our treatment group does have slightly higher subjective past HPA and expected HPA than the control group.[20] The next two columns show summary statistics for those who consider future or past HPA as the more important return variable underlying their investment decisions. First, 41% of respondents (167 out of 408) from the treatment group report that they rely on past returns more than future returns in decision-making. This confirms our earlier empirical finding that, at least for a substantial share of our sample, realized returns do drive investors' decisions independent of their effect on return forecasts. Second, respondents selecting past or future returns have very different observable characteristics. Compared with those stating they rely on past returns, respondents who rely more on future returns are more optimistic about both the past and future of their local housing market. Respondents in the forward-looking group are also more likely to be college graduates and are more risk seeking, contributing to their significantly higher average investment in the housing fund (69% versus

[18]These results are also robust to winsorizing the sample with various cutoffs.

[19]We drop 8 observations from treatment-group respondents that left the question about decision-making factors blank.

[20]We fail to reject that the means of the observables in Table 7 are equal between treatment and control (p-values of 0.14 to 1).

52%).[21]

To explore whether our results extend to other asset markets, we also ask respondents a similar question choosing between expected future and past stock returns in the context of investing in a stock fund. The last row of Table 7 reports summary statistics on this question. On average, 37% of the respondents report relying more on past stock returns when making stock-market investment decisions, on par with the 41% that rely on past returns for housing-market decisions. There is also a strong correlation between choosing future returns for the housing question and the stock question. Among respondents selecting future return for the housing question, 80% of them also choose future returns for the stock-market investment question, whereas only 37% of those relying on past returns for housing report relying on future returns for stocks.

We next explore correlates of responses to past versus future return questions to explain why past returns affect investment choices even conditional on stated beliefs. First, consistent with our model of cognitive uncertainty in Section 2, risk tolerance is a strong predictor of relying on expected returns over past returns. Moving from the most to the least risk averse households, the probability of choosing future returns increases by 29% and 32% for the housing and stock-market questions, respectively. Second, households with low financial literacy or sophistication could find it cognitively challenging to formulate beliefs about future home prices and instead rationally rely on their subjective experiences. We use three variables as proxies for financial literacy: income, college-education, and numeracy.

Appendix Table A9 reports estimates from regressing the dummy of choosing forecasted returns over subjective past returns as the outcome variable on demographics. Besides the three proxies for financial literacy, we only report covariates with statistically significant coefficients for at least one of the housing and stock questions. College education is a strong predictor for choosing forecasted returns over subjective past returns. Although the coefficients for numeracy and income are statistically insignificant, the importance of the education coefficient suggests that financial literacy may one reason why investors seem to overemphasize perceived past returns. However, as discussed in Section 5.2, our results hold for both high- and low-income households and among college-educated and non-college educated respondents, suggesting that financial illiteracy is unlikely to be a complete explanation for investors overweighting past returns.

While we hypothesized that this self-reflection could help correct any cognitive behavioral bias in overemphasizing past returns, we found that our treatment seems to encourage people to rely *less* on their return forecasts, as shown by the interaction terms in columns 3 and 4 of

[21] Appendix Table A9 presents multivariate regression evidence comparing the more forward looking with the more backward looking investors.

Table 8. This suggests that people consciously and deliberately use memory of past returns to inform investment decisions rather than only passively through some subconscious bias.

We also study whether people's reported reliance on future versus past returns is consistent with their actual decisions rule. In other words, do those reporting that they rely on expected returns (past return) indeed base their investment decisions on their return forecast (perceived past return)? Table A10 reports the results. From the p-values for the expected return, we can see that neither the backward-looking or the forward-looking group rely on the expected return in a statistically significant way. For the perceived past return, the backward-looking respondents indeed rely on them, consistent with their self-reported behavior. The forward-looking group also display dependence on past returns in some specifications. We also note that the forward-looking group on average invest much more in the housing fund than the backward-looking group.

6.4 Direct Evidence for Shrunk Factors in Cognitive Uncertainty

The cognitive uncertainty model in section 2 assumes existence of a signal s that an investor relies on in forming return forecast, but down weights in an investment decision. This is demonstrated by $\beta_{2,e} > \beta_{2,i}$ in equations (3) and (4). In this section, we show that forecasted rent growth is such a factor. Column 1 in Table 9 regresses home-price growth forecast on perceived past home-price growth, rent growth, and demographic controls. Rent growth is an important factor considered in home-price growth even conditional on other factors. A one percentage point higher rent growth is associated with a 0.11% higher expected home-price growth. Column 2 regresses the share invested in a housing fund on perceived past home-price growth, rent forecast, and other controls. It shows that despite rent growth's importance in home price forecast, it is omitted in investment decisions. Column 3 shows that the result is robust to controlling for the level and the distribution of home price forecast.

7 Conclusion

In this paper, we document that stated beliefs are not sufficient statistics summarizing all decision-relevant information used in expectation formation. In particular, controlling for subjective past experience improves action prediction even after controlling for forecasted returns. These results have important empirical and theoretical implications. Empirically, our results suggest that researchers could improve the measurement of the beliefs underlying investment choices by eliciting perceptions of past returns. Theoretically, our findings advance the growing literature of cognitive uncertainty by providing novel supporting evidence and showing that risk aversion together with financial incentives could increase cognitive uncertainty.

There are several avenues for future research. One is to test whether investors' reliance on past returns extends beyond the housing market to other assets. We present preliminary evidence that in the stock market, investors find return forecasts more valuable than in the housing market but we lack conclusive evidence due to data limitations. We leave research for the stock market and other asset classes for future works. A second direction is to provide additional causal evidence for the channel that we hypothesize by isolating whether exogenous variation in skin in the game induces investors to weight subjective experiences more than their own forecasted returns.

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Figure 1: Investment Questions in the 2020 Survey

Panel A: Treatment Group

Consider a situation where you have to decide how to invest $1,000 for one year. You can choose between two possible investments.

The first is a fund that invests in your local housing market, and pays an annual return equal to the growth in home prices in your area. The second is a savings account that pays **2%** of interest per year.

Which factor do you consider more when making this investment decision?

- Expected return on the local housing market over the *next* 12 months
- Realized return on the local housing market over the *past* 12 months

What proportion of the $1,000 would you invest in:

*(Please note: The numbers need to add up to **100**.)*

The housing market fund		%
The savings account		%
TOTAL	0	

Panel B: Control Group

Consider a situation where you have to decide how to invest $1,000 for one year. You can choose between two possible investments.

The first is a fund that invests in your local housing market, and pays an annual return equal to the growth in home prices in your area. The second is a savings account that pays **2%** of interest per year.

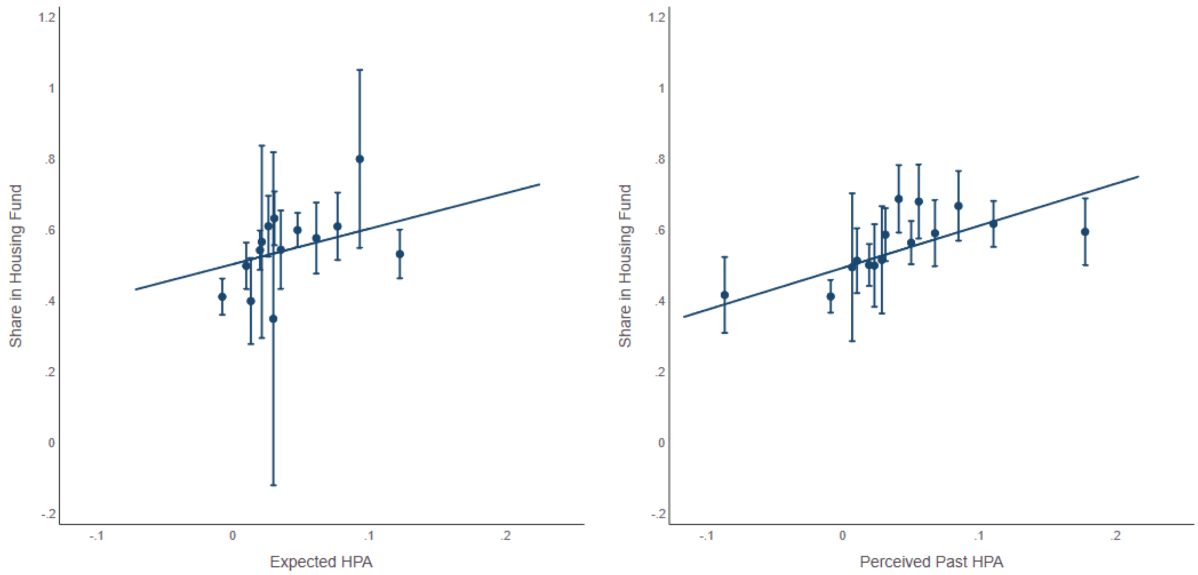
What proportion of the $1,000 would you invest in:

*(Please note: The numbers need to add up to **100**.)*

The housing market fund		%
The savings account		%
TOTAL	0	

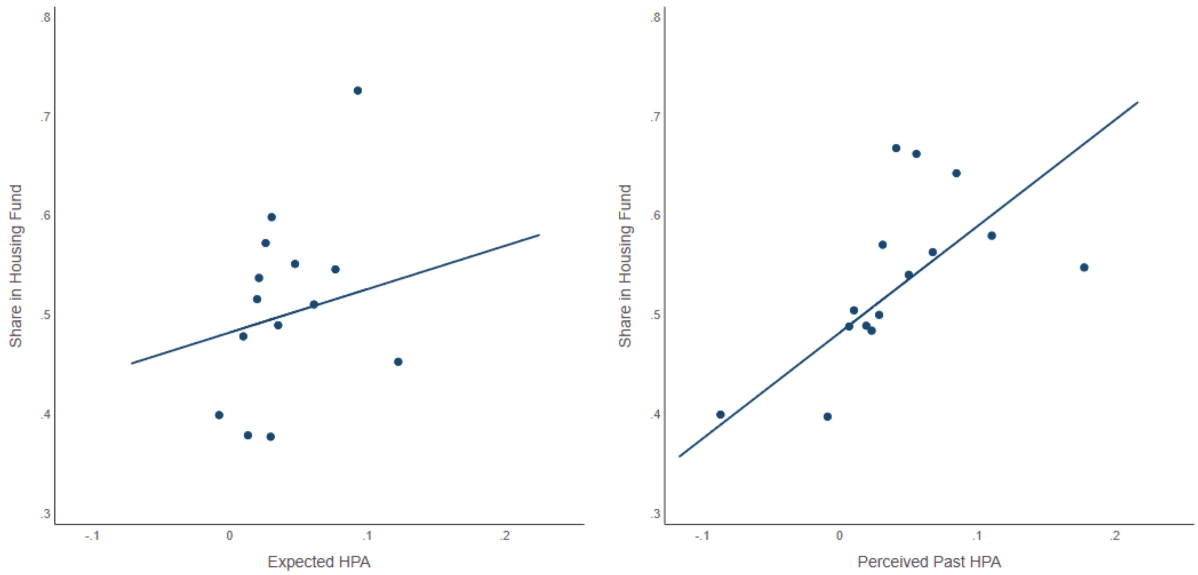
Notes: Figure shows the investment experiment in the 2020 survey. Half of the respondents receive questions shown in the top panel. The other half receive questions shown in the bottom panel.

Figure 2: Binned Scatter Plots of Share in Housing Fund versus Perceived Past and Expected HPAs



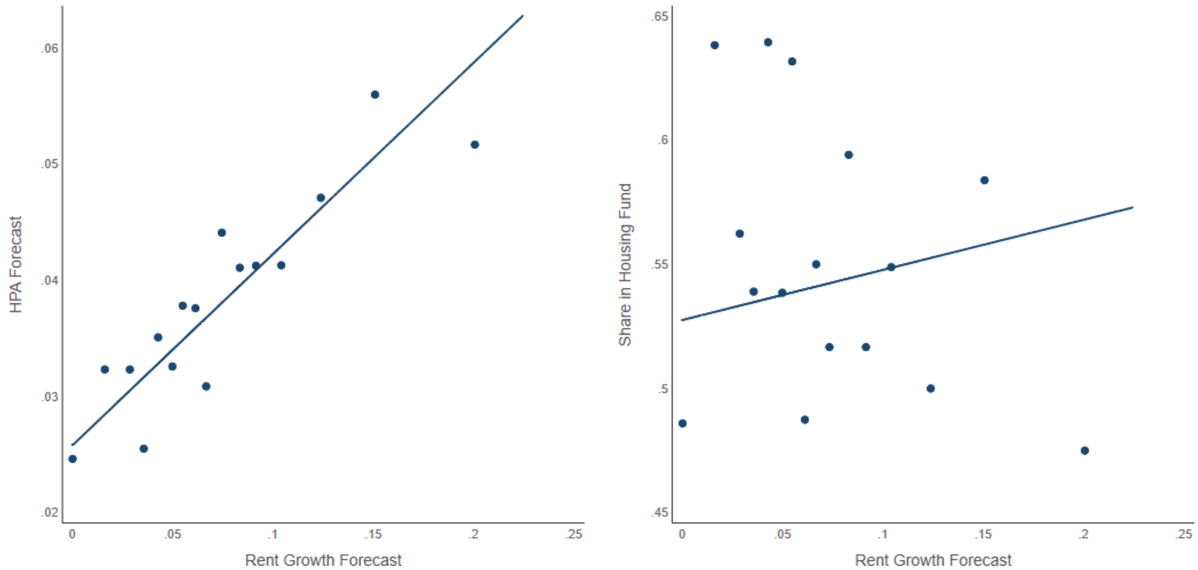
Notes: Figure presents binned scatter plots for the share of an $1,000 investment in the housing fund versus the expected home-price growth and the perceived past home-price growth. N = 1,012.

Figure 3: Binned Scatter Plots Controlling for the Other Covariate Linearly



Notes: Figure presents binned scatter plots for the share of an $1,000 investment in the housing fund versus the expected home-price growth and the perceived past home-price growth. N = 1,012.

Figure 4: Binned Scatter Plots of HPA Forecast and Share in Housing Fund versus Rent Forecast



Notes: Figure presents binned scatter plots for HPA forecast and the share of an $1,000 investment in the housing fund versus rent forecast. N = 1,012.

Table 1: Summary Statistics: 2015-2020 Sample

	Response Count	Mean	Std. Dev.
Confidence in Recalled Price Change	5,865	3.21	0.90
Age (years)	5,836	51.2	15.3
Homeowner Indicator	5,816	0.76	0.42
1(Household Income $\geq$ \$100K)	5,779	0.29	0.45
1(Liquid Savings $\geq$ \$75K)	5,481	0.66	0.47
Risk Loving	5,875	1.79	0.75
Married Indicator	5,836	0.65	0.48
Minority Indicator	5,828	0.16	0.37
Male Indicator	5,835	0.54	0.50
Bachelor's Degree or More Indicator	5,835	0.57	0.50
Numeracy Score	5,836	4.06	1.04
Perceived HPA in the Past 12 months	5,866	0.04	0.06
Expected HPA in the Next 12 months	5,869	0.04	0.05
$\vert$Perception Gap$\vert$	5,793	0.05	0.05
Probability of Moving within 3 years	5,862	0.30	0.34
Probability of Buying a Primary Residence	3,858	0.67	0.33
Probability of Buying an Investment Property	5,861	0.09	0.18
Share Invested in a Housing Fund (2015)	1012	0.54	0.34
Share Invested in a Housing Fund (2020)	808	0.61	0.32

Notes: Table reports means, standard deviations, and counts of individual responses for variables used in the empirical analysis. Confidence level of past home-price growth estimate is coded from 1 (not all confident) to 5 (very confident). Risk loving is coded from 1 (risk averse) to 3 (risk loving). Numeracy is coded between 1 and 5, based on the number of correct answers to 5 questions testing numerical literacy. Perception Gap is the absolute value of the difference between a respondent's perception of last year's home-price growth in their zip code and zip-code-level returns estimated from CoreLogic's repeat-sales index. Likelihood of buying a primary residence is asked to respondents who report an over 5% probability of moving within 3 years. Share invested in a housing fund is asked in both 2015 and 2020 and represents the share of a hypothetical $1,000 investment allocated by the respondent to an index of local housing market returns instead of a savings account with a 2% annual yield.

Table 2: Perceived Past Returns and Expected Returns

Dependent Variable: 1-year HP Expectation	(1)	(2)	(3)	(4)
1-year Perceived HPA	0.26***	0.26***	0.23***	0.22***
	(0.029)	(0.031)	(0.027)	(0.029)
Individual Controls		X		X
Fundamentals			X	X
Observations	1,012	1,012	1,012	1,012
R-Squared	0.139	0.165	0.240	0.260

Notes: One percentage point is denoted as 1. Individual controls include binary indicators for owning a home, numeracy, ethnicity, gender, marital status, education, labor force status, census region, age, age^2, and logs of household income, equity in home, liquid savings, personal debt, a dummy for consulting websites about home prices in the past 12 months, and a dummy for receiving questions in a percentage-change framing instead of a level framing, as discussed in section 3.1, a dummy that equals 1 if respondent reports being confident in their recall of past home price changes (i.e. answers 4 or more on a 1-5 scale, where 5 is very confident), a dummy that equals 1 if respondent reports a 4 or less (on 1-10 scale) to question about willingness to take risks in financial matters, where 10 is very willing. Fundamentals include measures of respondent expectations of general inflation, mortgage rate changes, rent inflation, future economic conditions, and future credit availability. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table 3: Effects of Forecasted and Past Returns on Investment

Dependent Variable: Housing fund share (on a 0-100 scale)						
	(1)	(2)	(3)	(4)	(5)	(6)
1-year HP Expectation	1.00***		0.44	0.81***		0.41
	(0.29)		(0.31)	(0.29)		(0.30)
1-year Perceived HPA		1.18***	1.07***		0.93***	0.83***
		(0.20)	(0.22)		(0.21)	(0.22)
Confident in recalled HPA				4.28*	4.01*	3.94*
				(2.38)	(2.38)	(2.38)
Above-median risk aversion				-7.23***	-7.12***	-7.08***
				(2.13)	(2.12)	(2.12)
Individual Controls				X	X	X
Observations	1,012	1,012	1,012	1,012	1,012	1,012
R-Squared	0.012	0.034	0.036	0.116	0.127	0.129

Notes: One percentage point is denoted as 1. Confident in recalled HPA is a dummy that equals 1 if respondent reports being confident in their recall of past home price changes (i.e. answers 4 or more on a 1-5 scale, where 5 is very confident). Above-median risk aversion is a dummy that equals 1 if respondent reports a 4 or less (on 1-10 scale) to question about willingness to take risks in financial matters, where 10 is very willing. Individual controls are controlled in columns 4 to 6. For definitions of these controls, see notes to Table 2. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table 4: Robustness of Investment Effects to Distributional Controls

Dependent Variable: Housing fund share (on a 0-100 scale)	(1)	(2)	(3)	(4)	(5)	(6)
1-year HP Expectation	0.20	0.17	0.13	0.13	0.29	
	(0.30)	(0.31)	(0.31)	(0.31)	(0.32)	
1-year Perceived HPA	0.75***	0.74***	0.66***	0.66***		0.61***
	(0.22)	(0.22)	(0.22)	(0.22)		(0.22)
Pr(HPA next year $< 0\%$)	-0.12***	-0.095*	-0.019	0.013	-0.047	0.063
	(0.042)	(0.052)	(0.16)	(0.31)	(0.31)	(0.32)
Pr(HPA next year $< -5\%$)		-0.081	-0.54*	-0.53	-0.36	-0.61
		(0.098)	(0.31)	(0.53)	(0.52)	(0.54)
Pr(HPA next year $> 10\%$)		0.027	0.53***	0.46	0.42	0.39
		(0.071)	(0.16)	(0.31)	(0.31)	(0.31)
Confident in recalled HPA	3.58	3.74	4.22*	4.24*	3.64	4.45*
	(2.36)	(2.37)	(2.38)	(2.39)	(2.41)	(2.44)
Above-median risk aversion	-7.20***	-7.14***	-7.42***	-7.45***	-7.75***	-6.61***
	(2.10)	(2.11)	(2.11)	(2.11)	(2.11)	(2.17)
Probabilities Squared			X	X	X	X
Probabilities Cubed				X	X	X
Bin FEs for Perceived Past HPA					X	
Bin FEs for Expected HPA						X
Individual Controls	X	X	X	X	X	X
Observations	1,012	1,012	1,012	1,012	1,011	1,012
R-Squared	0.137	0.138	0.150	0.150	0.196	0.171

Notes: One percentage point is denoted as 1. Pr(Decrease in HP next year) is the probability (on a 0-100 scale) that respondent assigns to year-ahead home prices decreasing. For definitions of individual controls, see notes to Table 2. In column 5, we first divide our observations into 50 equally sized bins according to their perceived past HPA, and then control for fixed effects for these bins. In column 6, we control for bin fixed effects for expected HPA in a similar way. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table 5: Role of Risk Aversion

Dependent Variable: Share Invested in a Housing Fund			
	(1)	(2)	(3)
Risk Tolerance (1-10)	3.38***	3.18***	
	(0.49)	(0.94)	
1-year HP Expectation		0.12	0.09
		(0.31)	(0.31)
1-year Perceived Past HPA		0.64***	0.58***
		(0.22)	(0.22)
Confident in recalled HPA		3.80	4.01*
		(2.39)	(2.39)
Above-median risk aversion		4.08	
		(3.76)	
Risk Tolerance Score FEs			X
Probabilities Squared		X	X
Probabilities Cubed		X	X
Individual Controls		X	X
Observations	1012	1012	1012
R-Squared	0.048	0.160	0.169

Notes: One percentage point is denoted as 1. For definitions of individual controls, see notes to Table 2. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table 6: Information Content in Forecasted Returns

	Panel A: Forecast Errors	
	Future Realized - Year-ahead Forecast	Future Realized - Subjective Past
Mean Error	1.32%	0.65%
Mean Absolute Error	4.39%	4.99%
Mean Squared Error	0.0035	0.0042
Number of Observations	4,882	4,882

	Panel B: Updating Directions	
	Future $<$ Subjective Past	Future $>$ Subjective Past
Forecast $<$ Subjective Past	56.1%	43.9%
Forecast $>$ Subjective Past	20.1%	79.9%

Notes: Panel A reports summary statistics for forecast errors using the perceived past home-price growth and expected home-price growth. Panel B reports summary statistics for updating directions between subjective past home-price growth and forecasted home-price growth. In panel B, observations with 1-year home price forecast the same as the perceived past 12-month home-price growth are excluded.

Table 7: Summary Statistics: 2020 Experiment

	Control	Treatment	Equal Means (p-value)	Within Treatment Select r_{t+1}	Within Treatment Select r_{t-1}
Number of Observations	404	404		239	165
Share Invested in Housing Fund	59.8%	61.4%	0.47	68.2%	51.4%
1-year Actual HPA	3.32%	3.32%	0.99	34.4%	3.14%
1-year Perceived HPA	4.12%	4.39%	0.43	4.96%	3.56%
1-year HP Expectation	3.34%	3.54%	0.47	4.01%	2.85%
With a College Degree	59.1%	62.4%	0.35	66.5%	56.3%
Confidence in Perceived HPA	3.22	3.19	0.62	3.30	3.04
|Perception Gap|	4.04%	3.75%	0.28	3.74%	3.76%
Age (years)	52.6	51.0	0.14	51.3	50.6
Homeowner Indicator	78.0%	78.0%	1.00	75.7%	81.2%
1(Household Income $\geq$ \$100K)	31.0%	32.7%	0.60	33.5%	31.5%
Household Income	88,434.4	92,970.3	0.31	96,391.2	88,015.2
1(Liquid Savings $\geq$ \$75K)	69.8%	71.8%	0.54	69.4%	75.2%
Risk Tolerance (1-10)	4.64	4.60	0.78	4.92	4.15
Choosing r_{t+1} for Stocks		62.4%		79.9%	37.0%

Notes: Table reports summary statistics for the 2020 sample. Share invested in housing fund is the share of \$1,000 invested in a fund with an annual return equal to the growth in home prices in the respondent's local area, with the rest of the \$1,000 invested in a savings account that pays 2% per year.1-year Actual HPA is the zipcode level home price appreciation between March 2019 and Februrary provided by Zillow. When the zip-code level home price index is unavailable, we use the county level home price index, and when the county level home price index ix unavailable, we use the state level home price index. Confidence in perceived HPA is a self-reported confidence level about the respondent's reported past HPA, on a 1-5 scale. Perception gap is defined as the difference between perceived past HPA and the actual past HPA. Choosing r_{t+1} for stocks is an indicator for whether a respondent reported that she relies more on her own forecasted returns than past returns when making decisions about investing in the stock market. The third column reports p-values for a t-test of whether the treatment and control means in that row are equal.

Table 8: Experiment in 2020 for Home Price Expectations

Dependent Variable: Housing fund share (on a 0-100 scale)				
	(1)	(2)	(3)	(4)
Expected HPA in the Next 12 months	1.46***	1.39**	1.21**	1.17**
	(0.56)	(0.55)	(0.59)	(0.60)
Perceived HPA in the Past 12 months	0.98***	0.82**	0.96***	0.80**
	(0.37)	(0.38)	(0.36)	(0.37)
Expected HPA in the Next Year	-1.47**	-1.40**	-1.35*	-1.30*
* Treated	(0.71)	(0.68)	(0.74)	(0.72)
Perceived HPA in the Past Year	0.49	0.57	0.38	0.44
* Treated	(0.52)	(0.53)	(0.52)	(0.52)
Treated	4.36	4.08	4.71	6.13
	(3.18)	(3.15)	(4.76)	(4.67)
p-value for Expected HPA = 0 for Treated	0.9888	0.9874	0.7560	0.7659
p-value for Perceived HPA = 0 for Treated	0.0001	0.0002	0.0004	0.0009
Distribution of Expected Return			X	X
Individual Controls		X		X
Observations	808	808	808	808
R-Squared	0.069	0.166	0.083	0.178

Notes: One percentage point is denoted as 1. Treated is a dummy for the treatment group, who receive one extra question on whether they consider past return or future return more in investment decisions before making investment choices. For definitions of individual controls, see notes to Table 2. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table 9: Rent Growth As a Shrunk Factor

Dependent Variable:	Expected HPA	Housing fund share	
	(1)	(2)	(3)
Expected HPA in the Next 12 months			0.043
			(0.32)
Perceived HPA in the Past 12 months	0.19***	0.71***	0.54**
	(0.032)	(0.21)	(0.23)
Expected Rent Growth	0.11**	-0.36	-0.33
	(0.042)	(0.28)	(0.29)
Probabilities			X
Probabilities Squared			X
Probabilities Cubed			X
Individual Controls	X	X	X
Observations	1,012	1,012	1,012
R-Squared	0.287	0.226	0.236

Notes: For definitions of individual controls, see notes to Table 2. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

A Appendix Tables

Table A1: Perception, Expectation, and Investment (Weighted)

Dependent Variable: Housing fund share (on a 0-100 scale)	(1)	(2)	(3)	(4)	(5)	(6)
1-year HP Expectation	0.54		-0.013	0.46		0.10
	(0.35)		(0.36)	(0.30)		(0.32)
1-year Perceived HPA		1.05***	1.05***		0.78***	0.76***
		(0.24)	(0.25)		(0.23)	(0.25)
Confident in recalled HPA				3.18	2.85	2.82
				(2.71)	(2.70)	(2.71)
Above-median risk aversion				-8.50***	-8.80***	-8.73***
				(2.51)	(2.49)	(2.48)
Individual Controls				X	X	X
Observations	1,012	1,012	1,012	1,012	1,012	1,012
R-Squared	0.004	0.030	0.030	0.142	0.154	0.154

Notes: Observations are weighted by SCE-ACS weights. One percentage point is denoted as 1. Individual controls are controlled in columns 4 to 6. For definitions of these controls, see notes to Table 2. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table A2: Address Collinearity Between Expected and Perceived Past HPA

Dependent Variable: Housing fund share (on a 0-100 scale)	(1)	(2)	(3)	(4)	(5)	(6)
Baseline 1-year HP Expectation	0.21	0.28	0.36			
	(0.31)	(0.32)	(0.32)			
Baseline 1-year Perceived HPA				0.62***	0.63***	0.64***
				(0.23)	(0.23)	(0.23)
Bin FEs for Perceived HPA	X	X	X			
Bin FEs for Expected HPA				X	X	X
Number of Bins Specified	10	100	200	10	100	200
Number of Actual Bins	9	46	71	9	44	64
Probabilities Squared	X	X	X	X	X	X
Probabilities Cubed	X	X	X	X	X	X
Demographics	X	X	X	X	X	X
Observations	1012	1010	1006	1012	1012	1008
R-Squared	0.171	0.207	0.233	0.162	0.177	0.195

Notes: In Columns (1) to (3), we first divide our observations into 10, 100, and 200 equally sized bins according to their perceived past HPA, and then control for fixed effects for these bins. In Columns (4) to (6), we control for bin fixed effects for expected HPA in a similar way. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table A3: Perception, Expectation, and Investment (Incentivized Stage)

Dependent Variable: Housing Fund Share (Incentivized Stage)	(1)	(2)	(3)
1 year HP Exp (Incentivized Stage)	0.15	0.28	0.12
	(0.60)	(0.60)	(0.59)
1 year Perceived Past HPA	0.91**	0.91**	0.85**
	(0.38)	(0.39)	(0.39)
1 year Actual Past HPA			0.78**
			(0.34)
Individual Controls	X	X	X
HPA Dist Forecast (Baseline Stage)		X	X
Observations	330	330	330
R-Squared	0.159	0.162	0.177

Notes: One percentage point is denoted as 1.For definitions of individual controls, see notes to Table 2. Before the incentivized stage, the treatment group was provided information on the actual past 1-year HPA. Then we elicit again their expected home-price growth and allocation of $1,000 between a synthetic housing fund and a 2% savings account. For the control group at the incentivized stage, we elicit their expected home-price growth and allocation of $1,000 without providing any information. Note that their home price expectation and asset allocation could still change from the baseline stage because in between, they are asked many housing related questions. Answering these questions themselves might help respondents reflect on their home price expectations. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table A4: Controlling for Forecasted Fundamentals

Dependent Variable: Housing fund share (on a 0-100 scale)	(1)	(2)
1-year HP Expectation	0.50*	0.26
	(0.30)	(0.32)
1-year Perceived HPA	0.70***	0.57**
	(0.22)	(0.22)
Forecasted Fundamentals	X	X
Probabilities		X
Probabilities Squared		X
Probabilities Cubed		X
Individual Controls	X	X
Observations	1,012	1,012
R-Squared	0.176	0.188

Notes: One percentage point is denoted as 1. For definitions of individual controls, see notes to Table 2. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table A5: Actual versus Subjective Past Home Price Growth

Dependent Variable: Share Invested in a Housing Fund	(1)	(2)
1-year HP Expectation	0.335	0.113
	(0.301)	(0.312)
1-year Actual Past HPA	0.318	0.253
	(0.202)	(0.203)
1-year Perceived Past HPA		0.624***
		(0.225)
Confident in recalled HPA	4.518*	4.233*
	(2.378)	(2.389)
Above-median risk aversion	-7.523***	-7.360***
	(2.129)	(2.120)
Probabilities	X	X
Probabilities Squared	X	X
Probabilities Cubed	X	X
Individual Controls	X	X
Observations	1,012	1,012
R-Squared	0.144	0.151

Notes: One percentage point is denoted as 1.For definitions of individual controls, see notes to Table 2 Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table A6: Heterogeneity in Investment Decision-Making

Dependent Variable: Share Invested in a Housing Fund

Panel A: Economic Characteristics

	Owner	Renter	College	Non-Coll	Inc>$75K	Inc≤$75K
	(1)	(2)	(7)	(8)	(9)	(10)
1-year HP Exp	0.31	-0.49	0.38	-0.037	0.33	0.053
	(0.39)	(0.56)	(0.52)	(0.40)	(0.59)	(0.37)
1-year Perc HPA	0.95***	0.14	0.87**	0.75***	0.71*	0.68**
	(0.27)	(0.43)	(0.37)	(0.29)	(0.41)	(0.28)
Pr(HPA$<0\%$)	-0.012	-0.32***	-0.13*	-0.072	-0.030	-0.13**
	(0.062)	(0.082)	(0.073)	(0.076)	(0.088)	(0.067)
Pr(HPA$<-5\%$)	-0.15	0.21	-0.13	-0.032	-0.23	-0.019
	(0.12)	(0.20)	(0.15)	(0.13)	(0.21)	(0.11)
Pr(HPA$>10\%$)	-0.045	0.22*	-0.12	0.14	0.17	-0.0092
	(0.085)	(0.12)	(0.11)	(0.093)	(0.12)	(0.087)
Individual Controls	X	X	X	X	X	X
Observations	750	262	563	449	399	613
R-Squared	0.145	0.239	0.171	0.165	0.182	0.135

Panel B: Other Characteristics

	Age<50	Age≥50	Male	Female	High Numeracy	Low Numeracy
1-year HP Exp	0.47	0.16	0.20	-0.13	(11)	0.19
	(0.46)	(0.44)	(0.49)	(0.38)	0.41	(0.53)
1-year Perc HPA	0.13	1.11***	0.77**	0.76**	(0.41)	0.58
	(0.32)	(0.31)	(0.31)	(0.31)	0.87***	(0.37)
Pr(HPA$<0\%$)	-0.063	-0.13	-0.17**	-0.045	(0.28)	0.074
	(0.072)	(0.076)	(0.078)	(0.071)	-0.14**	(0.10)
Pr(HPA$<-5\%$)	-0.13	-0.040	-0.032	-0.12	(0.064)	-0.34**
	(0.13)	(0.15)	(0.15)	(0.12)	-0.0019	(0.14)
Pr(HPA$>10\%$)	0.039	0.034	0.0068	0.057	(0.13)	0.22**
	(0.12)	(0.095)	(0.11)	(0.093)	-0.097	(0.11)
Individual Controls	X	X	X	X	(0.090)	X
Observations	478	534	551	461	X	266
R-Squared	0.202	0.143	0.149	0.147	746	0.259

Notes: For definitions of individual controls, see notes to Table 2. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table A7: Other Housing-Related Behaviors: 2015-2020 Data

Dependent Variable:	Pr(Buy non-primary home)		Pr(Buy home)		Viewing Housing Good Investment	
	(1)	(2)	(3)	(4)	(5)	(6)
Expected HPA in the Next 12 months	0.12***	0.19**	-0.53**	-0.24	0.20***	0.12***
	(0.029)	(0.052)	(0.18)	(0.14)	(0.044)	(0.030)
Perceived HPA in the Past 12 months	0.092*	0.074*	0.11	0.050	0.20***	0.15***
	(0.041)	(0.030)	(0.15)	(0.098)	(0.013)	(0.016)
Pr(HPA next year $<$ 0%)		0.0052		-0.033		-0.027***
		(0.011)		(0.035)		(0.0032)
Pr(HPA next year $< -5\%$)		0.068***		-0.065		-0.014
		(0.014)		(0.038)		(0.015)
Pr(HPA next year $>$ 10%)		-0.022		-0.060		0.0025
		(0.015)		(0.070)		(0.0091)
Owns Home		2.13**		22.8***		0.11
		(0.74)		(0.57)		(0.52)
Confident in past price projections		2.09***		3.40**		1.61***
		(0.39)		(0.96)		(0.29)
Above-median risk aversion		-5.45***		-2.57**		-0.83*
		(0.38)		(0.68)		(0.35)
Individual Controls		X		X		X
Observations	5,375	5,375	3,575	3,575	5,387	5,387
R-Squared	0.002	0.089	0.005	0.253	0.033	0.087
Subsample	All	All	Pr(Move) $\geq$ 5%	Pr(Move) $\geq$ 5%	All	All

Notes: One percentage point is denoted as 1. Viewing housing good investment is a discrete variable for view of housing as an investment on a 10, 20, 30, 40, 50 scale, with 50 being a very good investment. For definitions of individual controls, see notes to Table 2. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table A8: Interacting HPA with Share of Housing in Wealth or Income

Dependent Variable: Housing fund share (on a 0-100 scale)	(1)	(2)	(3)	(4)	(5)	(6)
Baseline 1-year HP Expectation	0.36	0.35	0.42	0.40	0.38	0.37
	(0.39)	(0.39)	(0.43)	(0.43)	(0.39)	(0.39)
Baseline 1-year Perceived HPA	1.01***	0.98***	0.93***	0.87***	1.32***	1.25***
	(0.28)	(0.27)	(0.31)	(0.31)	(0.38)	(0.38)
Perceived Past 1-year HPA	-0.012	-0.021				
*(Home Value/Equity)	(0.019)	(0.019)				
Perceived Past 1-year HPA			0.010	0.0048		
*(Home Value/Net Assets)			(0.064)	(0.061)		
Perceived Past 1-year HPA					-0.052	-0.050
*(Home Value/Income)					(0.063)	(0.062)
Risk Tolerance (1-10)	2.53***		2.51***		2.37***	
	(0.62)		(0.66)		(0.61)	
Probabilities	X	X	X	X	X	X
Individual Controls	X	X	X	X	X	X
Risk Aversion FEs		X		X		X
Observations	711	711	624	624	718	718
R-squared	0.146	0.161	0.126	0.139	0.154	0.166

Notes: In columns 1 to 2, the sample is restricted to homeowners with a positive home equity. In columns 3 and 4, net assets is defined as home equity plus liquid assets and minus personal debt. The sample is restricted to households with positive assets. To reduce the effects of outliers, respondents with (Home Value/Equity), (Home Value/Net Assets), and (Home Value/Income) in the top and bottom 1% of the distribution for those variables are dropped. The results for the full sample including the outliers are similar to results for the trimmed sample. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table A9: Characteristics of Respondents Choosing Forecasted Returns Over Subjective Past Returns

Choose Forecasted Return Over Subjective Past Return		
	Housing Question (1)	Stock Question (2)
College Graduate	0.11**	0.11**
	(0.055)	(0.056)
Low Numeracy	-0.038	0.092
	(0.060)	(0.060)
Log(Income)	-0.015	0.032
	(0.044)	(0.049)
Married	0.11*	0.025
	(0.061)	(0.060)
Risk Tolerance (1-10)	0.033***	0.036***
	(0.012)	(0.013)
Log(Personal Debt)	-0.025**	0.0050
	(0.012)	(0.012)
Male	0.040	0.098*
	(0.050)	(0.050)
Individual Controls	X	X
Observations	404	404
R-Squared	0.099	0.089

Notes: One percentage point is denoted as 1. For definitions of individual controls, see notes to Table 2. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table A10: Experiment in 2020 for Home Price Expectations: Forward Looking versus Backward Looking

Dependent Variable: Housing fund share (on a 0-100 scale)				
	(1)	(2)	(3)	(4)
Expected HPA in the Next 12 months	1.46***	1.39**	1.21**	1.17**
	(0.56)	(0.55)	(0.60)	(0.59)
Perceived HPA in the Past 12 months	0.98***	0.84**	0.96***	0.82**
	(0.37)	(0.37)	(0.37)	(0.37)
Expected HPA in the Next Year	-1.45**	-1.44**	-1.31	-1.27
* Considering Future	(0.74)	(0.73)	(0.80)	(0.78)
Perceived HPA in the Past Year	-0.16	0.16	-0.21	0.12
* Considering Future	(0.58)	(0.58)	(0.61)	(0.60)
Considering Future Returns	13.5***	10.9***	11.5**	11.5**
	(3.67)	(3.67)	(5.28)	(5.15)
Expected HPA in the Next Year	-1.69*	-1.55*	-1.61*	-1.47*
* Considering Past	(0.90)	(0.87)	(0.90)	(0.85)
Perceived HPA in the Past Year	1.09	0.89	0.93	0.70
* Considering Past	(0.69)	(0.73)	(0.64)	(0.67)
Considering Past Returns	-5.81	-3.38	-3.90	-0.86
	(3.85)	(3.98)	(6.02)	(6.04)
p-value for Expected HPA for Forward Looking = 0	0.9827	0.9148	0.8496	0.8462
p-value for Expected HPA for Backward Looking = 0	0.7446	0.8090	0.5483	0.6233
p-value for Perceived HPA for Forward Looking = 0	0.0661	0.0260	0.1253	0.0480
p-value for Perceived HPA for Backward Looking = 0	0.0004	0.0054	0.0004	0.0069
Distribution of Expected Return			X	X
Individual Controls		X		X
Observations	808	808	808	808
R-Squared	0.098	0.182	0.114	0.196

Notes: One percentage point is denoted as 1. Considering future is a dummy that is equal to 1 for respondents who are in the treatment group and report that they consider future returns more important than past returns in their investment decisions. Considering past is a dummy that is equal to 1 for respondents who are in the control group and report that they consider past returns more important than future returns in their investment decisions. For definitions of individual controls, see notes to Table 3. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.