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

Haoyang Liu† Christopher Palmer‡

December 2020

Abstract

Despite growing interest in expectation surveys, critics argue that survey responses are not reliable measures of the true expectations underlying financial decisions, and empirical work often finds only a weak correlation between investment and stated beliefs. In this paper, we document a systematic gap between an individual's own forecasted returns and her actual beliefs used in investment decisions. In particular, we show that perceived *past* home-price growth is a stronger predictor of 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. Many respondents report relying on past returns more than expected returns when making investment decisions, ruling out simple measurement-error explanations. To interpret these findings, we extend recent models of cognitive uncertainty to incorporate risk aversion. Finally, we present evidence consistent with financial risk inducing risk-averse investors to rely on "mental defaults," which in our context seems to include their recent experience of local housing markets rather than their stated expectations.

Keywords: belief formation, cognitive uncertainty, expectations surveys, housing-market momentum

*We thank our discussant Lawrence Jin, and Marianne Andries, John Campbell, Richard Crump, Francesco D'Acunto, Benjamin Enke, Andreas Fuster, Stefano Giglio, Ed Glaeser, Thomas Graeber, Jerry Hausman, Wei Jiang, Theresa Kuchler, Eben Lazarus, Jeffrey Levine, Yueran Ma, Stefan Nagel, Jonathan Parker, Felipe Severino, Jason Somerville, Steven A. Snell, David Sraer, David Thesmar, Adrien Verdelhan, James Vickery, Annette Vissing-Jørgensen, and seminar and workshop participants at CUHK Shenzen, the Federal Reserve MBS Analytical Forum, ITAM, MIT, Nankai SOF, NBER Summer Institute (Real Estate), PKU GSM, and SFS Cavalcade for helpful feedback and discussions. Tammy Lee, David Rubio, Dean Parker, and Claire Nelson provided outstanding research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the Federal Reserve Bank of New York or the Federal Reserve System.

†Federal Reserve Bank of New York; haoyang.liu@ny.frb.org.

‡MIT Sloan and NBER; cjpalmer@mit.edu.

1 Introduction

A significant body of recent work in behavioral economics and macrofinance seeks 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 sufficient comprehension of the questions or because their answers do not correspond to decision-relevant expectations (Cochrane (2011, 2017)). As a response, survey designers have proposed several techniques to reduce measurement error, for example, by using multiple framings to ask the same question, designing instruments for self-reported expectations, and eliciting both point estimates and expected distributions (Glaser et al. (2007); 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.[3] Moreover, even though survey respondents are usually willing to answer numerical expectations questions, it is unclear whether the representation of subjective beliefs elicited by expectations surveys is a fair characterization of the subjective probability function used in actual decision making.[4] Might the projection of actual beliefs onto survey forecasting exercises differ from the way actual beliefs are used in investment decision-making?

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

[1]See Manski (2004) and Manski (2018) for recent surveys.

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

[3]For examples, see Vissing-Jorgensen (2003); Dominitz and Manski (2011); Amromin and Sharpe (2014); Drerup et al. (2017); Giglio et al. (2019); Ameriks et al. (2020); Giglio et al. (2020); Liu and Sui (2020).

[4]For example, Manski (2018) describes this open question as follows. "There has, nevertheless, been awareness that the willingness and ability of respondents to report probabilistic expectations does not imply that persons regularly think probabilistically and use subjective probability distributions to make decisions. It has long been known that survey respondents are willing and able to respond to questions seeking point predictions of uncertain events or verbal assessments of likelihood. Yet persons need not use point predictions or verbal assessments of likelihood to make decisions."

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 Merton (1971) model of portfolio choice with a single risky asset with normally distributed future return r_{t+1}, the optimal share ϕ allocated to the risky asset is

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

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 therefore 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 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, past returns r_t would not *independently* enter this rational portfolio-choice rule. In contrast, our main empirical result can be summarized as finding that r_t affects ϕ_t 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.[5] Moreover, we find that perceived past returns matter more for investment 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 individual-specific risk aversion and a rich set of demographics, addressing potential collinearity between forecasted returns and subjective

[5] As we discuss below, this investment experiment allows us to abstract away from confounding demand shocks such as the effect of past returns on financial constraints.

past returns, instrumenting to account for measurement error in surveyed beliefs, 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). Even after flexibly controlling for several such 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 show that our results hold after instrumenting for elicited expected returns. Taken together, respondents' investment decision-making deviates from a fully rational framework wherein 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.[6] 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 the reported return forecasts they reported on the survey or their memory of past home-price growth more in their investment decisions.[7] This simple adjustment facilitates three additional findings.

First, a significant fraction (41%) of the population admits to relying more on their perception of past home-price growth than their surveyed return forecasts. This confirms that our findings about the importance of memory even conditional on expected returns are not an artifact of survey noise or omitted variables for a meaningful fraction of respondents. Second, risk-averse individuals and respondents without a college degree are more likely to rely on past returns in their decision-making. Third, by comparing our treatment group (those asked whether past or forecasted returns are more important to their decision-making) with the control group, we study whether the question itself nudges participants to rely more on forecasted returns. Instead of self-reflecting nudging respondents to lean against any cognitive bias in overemphasizing past returns, our treatment encourages people to rely *less* on their return forecasts in their decision-making, regardless of their response. This treatment effect suggests that increasing the salience of the uncertainty in the investment-

[6]This approach builds on a nascent survey 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)).

[7]The precise question framings are detailed in Section 3 and Figure 1.

decision process triggers many people to rely less on their stated beliefs.

To interpret these empirical findings, we turn to a growing literature on limited attention and cognitive uncertainty (Gabaix (2014); Khaw et al. (2018); Enke and Graeber (2019); Frydman and Jin (2019); Gabaix (2019)). 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 my forecast 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, such as her own perception of last period's return, 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 riskier. 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 5.2.3. While other forces such as ambiguity aversion (Fox and Tversky (1995)) are also consistent with some of our results, 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.

Prior LiteratureAndries et al. (2020)

Our paper makes several contributions to the literature measuring the role of beliefs about returns on investment decisions (Glaser et al. (2007); Armona et al. (2018); Giglio et al. (2019)). First, our results suggest that researchers could improve the measurement of the beliefs channel by directly controlling for factors that affect beliefs in addition to stated beliefs themselves. 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)).[8] Moreover, our findings that *perceived* past returns matter more than objectively measured past returns suggest that belief surveys, perhaps especially of retail investors, would further benefit from asking respondents about their perceptions of past returns.

We stress that our results do not argue against the usefulness of expectations surveys or reject the beliefs channel. Instead, we show that the magnitude of the beliefs channel could be much larger than previously estimated, consistent with the prior literature recognizing the noisiness of stated forecasts (Glaser et al. (2007); Armona et al. (2018); Giglio et al. (2019)). Our contribution is to show that the gap between actual and stated beliefs is not purely noise and instead has a systematic structure partially explainable by observable factors such as past returns experiences. Relatedly, we extend the extrapolative belief literature (Piazzesi and Schneider (2009); Greenwood and Shleifer (2014); Barberis et al. (2015); Glaeser and Nathanson (2017); Armona et al. (2018); Barrero (2020)) by showing that perceptions of past returns can directly influence behavior, finding evidence of implicit extrapolation even conditional on the explicit extrapolation that drives stated beliefs.

Second, our paper is directly related to work on limited attention and cognitive uncertainty in decision-making (Gabaix (2014); Drerup et al. (2017); Khaw et al. (2018); Enke and Graeber (2019); Frydman and Jin (2019); Gabaix (2019)). For example, Drerup et al. (2017) allow investors' decision processes to deviate from a rational investment-return model and instead 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).[9] 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 potential drivers of cognitive uncertainty, broadly consistent with the finding of Enke and Graeber (2019) that cognitive uncertainty is more acute in more complex environments.[10] By comparing the treatment group with the control

[8] 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.

[9] For practicality in their setting, Enke and Graeber (2019) use a common mental default for all agents, adding that they "acknowledge that the mental default in general likely depends on a multitude of factors." One useful feature of our data is to allow us to model mental defaults varying cross-sectionally.

[10] While our empirical evidence shows that risk averse investors are more likely to display cognitive uncertainty, as documented in Enke and Graeber (2019), risk aversion and cognitive uncertainty are two distinct

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 are consistent with the existence of cognitive uncertainty and suggest some of its important drivers. We further 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 by triggering a stress response in respondents.

Third, our results offer a potential solution to reconcile the strong evidence of personal experience as a belief driver that strongly affects behavior (Kaustia and Knüpfer (2008); Chiang et al. (2011); Malmendier and Nagel (2011, 2016); Malmendier et al. (2019); 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 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.

behavioral traits. 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.

Section 4 presents our empirical findings. Section 5 discusses various interpretations for our results, and Section 7 concludes.

2 Model

In this section, we provide a theoretical framework based on the nascent literature on cognitive imprecision (Gabaix (2014); Khaw et al. (2018); Enke and Graeber (2019); Frydman and Jin (2019); Gabaix (2019)) that can rationalize our empirical findings. While the stylized model presented here offers an intuitive framework to rationalize our empirical results, it is by no means the only model consistent with our findings.

As argued in Enke and Graeber (2019), people are often aware of their own cognitive limitations and shrink their posterior estimates of parameters 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" 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 approximately 3%. After shrinkage towards her default, the respondent might report 4% as her final answer.

In our context, we hypothesize that financial stakes such as monetary incentives trigger a stress response and 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, facing the real-world decision of buying an asset, investors upweight factors such as their perceived experiences as their lived experience feels more salient or safer to them than other information.[11]

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. 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

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

signal, denoted s. Both r_t and s are noisy forecasts for r_{t+1}, which we write as

$$r_t = r_{t+1} + \varepsilon_p \tag{2}$$

$$s = r_{t+1} + \varepsilon_s \tag{3}$$

because each factor contains a random deviation from future returns ε. While the error in past returns as a forecast $\varepsilon_p \sim \mathcal{N}(0, \sigma_p^2)$, respondents act as if the distribution of the error in the signal ε_s depends on the context of the particular survey question being asked. We index parameters used in survey forecast questions with e for expected and parameters used in investment actions with a. 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 $\mathcal{N}(0, \sigma_{s,a}^2)$ with $\sigma_{s,a} > \sigma_{s,e}$.[12] In a reduced-form way, the assumption $\sigma_{s,a} > \sigma_{s,e}$ captures that in forecasting returns, respondents focus on the level of s and to certain extent ignore the noisiness of s because a forecasting survey question is not a risky decision. By 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}.

We also note that 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 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 is through the endogenous attention framework of Gabaix (2014), which would lead respondents to have 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.

To simplify the exposition, in this section we assume that σ_p stays constant from the expectation to the investment-decision stage, while σ_s increases. In Appendix A, we show that the sufficient and necessary condition to generate a positive coefficient for β_t in equation (6) is that σ_s disproportionately increases relative to σ_p from the expectation to the decision

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

question. Let $r_{e,t+1}$ and $r_{a,t+1}$ denote a respondent's stated return forecast and the decision-relevant forecast used in investment decisions, respectively. We have

$$r_{e,t+1} = E[r_{t+1}|r_t, s, (r_d, \sigma, \sigma_p, \sigma_{s,e})] = c_e + \beta_{1,e} r_t + \beta_{2,e} s \tag{4}$$

$$r_{a,t+1} = E[r_{t+1}|r_t, s, (r_d, \sigma, \sigma_p, \sigma_{s,d})] = c_i + \beta_{1,a} r_t + \beta_{2,a} s, \tag{5}$$

where by Bayesian updating,

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

Because $\sigma_{s,a} > \sigma_{s,e}$, we have that $\beta_{1,e} < \beta_{1,a}$ and $\beta_{2,e} > \beta_{2,a}$. 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 and less on the signal s.

In our setting, our experiment asks respondents to allocate a fixed investment amount between a housing fund and a risk-free savings account. To map the investment-decision-relevant return forecast $r_{a,t+1}$ to the share invested in a housing fund, we again return to the standard Merton (1971) single risky asset model with constant absolute risk aversion used in the introduction, with the housing share ϕ_t given by

$$\phi_t = \frac{r_{a,t+1} - R_f}{\alpha \sigma_{a,t+1}^2},$$

where R_f is the risk-free rate, α is the absolute risk aversion parameter, and $\sigma_{a,t+1}^2$ is the conditional variance of $r_{a,t+1}$.[13] Taking a linear approximation of ϕ around the average value of r_a, α, and σ_a^2, and letting γ_α and γ_σ denote the partial derivatives of ϕ over α and σ_a^2, we

[13] The variance $\sigma_{a,t+1}^2$ is conditional on all information available to a given investor, including $r_{a,t+1}$ and s.

have

$$
\begin{aligned}
\phi_t &\approx \tilde{c} + r_{a,t+1} + \gamma_\alpha \alpha + \gamma_\sigma \sigma^2_{a,t+1} \\
&= c_1 + \beta_{1,a} r_t + \beta_{2,a} s + \gamma_\alpha \alpha + \gamma_\sigma \sigma^2_{a,t+1} \\
&= c_2 + \left(\beta_{1,a} - \beta_{1,e} \frac{\beta_{2,a}}{\beta_{2,e}}\right) r_t + \frac{\beta_{2,a}}{\beta_{2,e}} r_{e,t+1} + \gamma_\alpha \alpha + \gamma_\sigma \sigma^2_{a,t+1}. \qquad (6)
\end{aligned}
$$

Equation (6) motivates the regression specifications we use in our empirical section below and provides a framework to think about what statistical role we might expect r_t to play in explaining decisions when agents have cognitive uncertainty. In a standard model, conditional on expected returns $r_{e,t+1}$, there would be no role for r_t in (6) because r_t is simply a linear factor in $r_{e,t+1}$ with the same weight in (4) and (5) because $r_{e,t+1} = r_{a,t+1}$.[14] However, under cognitive uncertainty, we have the result that $\beta_{1,e} < \beta_{1,a}$ and $\beta_{2,e} > \beta_{2,a}$, such that the coefficient on subjective past experience r_t in (6) is positive. Consistent with this prediction, our empirical findings below provide evidence that perceived past returns have independent predicting power for investment decisions even after conditional on stated forecasts.[15]

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, an annual survey fielded in February every year since 2014 to the active panel members in the SCE that we will often refer to as the housing survey. 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.

[14] To relax the assumption that r_t enters $r_{e,t+1}$ linearly, many of our empirical specifications will control for flexible functions of $r_{e,t+1}$ and its forecasted distribution.

[15] Further, if we assume that all respondents share the same $\sigma_{s,e}$ but that more risk-averse and less sophisticated respondents have larger $\sigma_{s,a}$, 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 Section 5.3). 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 facing more uncertainty in s than other respondents in answering the investment-decision question.

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.[16] Usefully for our purposes, this experiment is not subject to any real-world constraints on 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 such demand factors, the hypothetical investment question offers a measure of investment choices unlikely to be affected by typical demand factors. We primarily use the housing share in the allocation of $1,000 as our primary measure of investment behavior, but we also examine other housing-related behaviors, including the probability of buying a non-primary residence.[17]

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 details 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

[16] To provide real-world stakes, a subset of respondents were promised a random chance of receiving the actual gross return of their investment. The survey instrument informed the randomly selected incentivized respondents that two out of 1,000 respondents would receive the gross return of their constructed derivative after one year. While belief survey research shows that incentivizing attentive responses improves elicitation accuracy (Carson et al. (2014)), a literature on survey responsiveness finds that people are often insensitive to the odds of receiving a reward and are more responsive to a small chance at a large reward rather than a certain small reward (e.g., Porter and Whitcomb (2003)).

[17] 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.

shown one of the three formats.[18] 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.*"
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 Decisions 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

[18] 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.

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 5.3. 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 Home-Price Beliefs and Behavior

Before presenting our main results on how forecasted and perceived past home-price growth predict investment behavior, Table 2 investigating how stated beliefs are formed by estimating the relationship between a respondent's perceived past and her forecast of future local housing-price returns. Consistent with Armona et al. (2018) and Glaeser and Nathanson (2017), 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. We also note that the R^2 in column 4 is 0.26, suggesting that even with our detailed set of demographic covariates and controls for each individual's forecast of fundamentals, much of the variation in home-price beliefs is idiosyncratic and driven by unobservables.

4.1 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 bivariate fitted lines in the two graphs have about the same slopes, meaning that the unconditional relationships between investment and a percentage point change in forecasted or perceived past HPA are about the same.[19] 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 contrast with the intuition that an investor's forecasted return is 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). Instead, Figure 2 provides preliminary evidence that perceived past HPA is a stronger empirical predictor for investment than forecasted HPA.

Figure 3 presents this graphical evidence differently, in each plot controlling for the other home-price growth variable linearly. For example, in the perceived HPA graph in the right-hand panel, 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.

4.2 Perceived Past Home-Price Growth and Investment

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

$$Y_{i,t} = \alpha + \beta_1 \hat{r}_{i,t} + \beta_2 \hat{E}_t[r_{i,t+1}] + X_{i,t}'\phi + \varepsilon_{i,t}, \tag{7}$$

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

where $\hat{r}_{i,t}$ and $\hat{E}_t[r_{i,t+1}]$ are respondent i's perception of home-price appreciation (HPA) over the past 12 months and her stated expected HPA over the next 12 months, respectively, and Y_{it} 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 growth. Additional specifications consider the stated probability of buying a primary or a non-primary residence in the next three years. The vector X_{it} is a 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 ϕ_{it} 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 returns in column 2 is larger than the one for the expected returns 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)), conceptually similar to a correlation between α and $\hat{r}_{i,t}$.[20] In column 6, which includes both expected and perceived-past HPA and the full set of demographic controls, perceived past HPA still has a statistically significant effect on investment decisions. We find that a one percentage point higher perceived past HPA is associated with 83 basis points higher share

[20]While separating higher risk tolerance from higher expected returns with survey evidence is always challenging (cf. Malmendier and Nagel (2011)), Tables 5 and A10 below account for risk aversion in more flexible ways.

allocated to a local housing fund. To benchmark this magnitude, recall from Table 1 that a one standard deviation increase in perceived past returns (5.4 percentage points in the 2015 sample) is associated with a 4.5 percentage point higher share allocated to the local housing fund. In particular, this effect size is the same order of magnitude as the differences reported by Table 3 in risky-asset allocations across gender and homeowner status. Again, this finding contrasts with the traditional approach, which assumes that past returns affect decisions only through expectations and that belief factors can be omitted from action-prediction regressions conditional on stated beliefs.

Another consideration when interpreting Table 3 is the lack of controls for the expected distributions of return. For example, it could be that investors believe that past home-price growth is a strong predictor for *downside* risk even conditional on the expected mean. Without controlling for downside risk, the statistically significant coefficient on 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. Inspired by Engelberg et al. (2009), the SCF asks 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 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.[21] Column 6 shows that subjective 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 A1 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 A2. We also note that the hypothetical investment experiment studied in our main results 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.[22] Further, 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 returns have added predictive power for investment decisions even conditional on actual past returns. In column 2, where both the perceived past and the actual past returns are included

[21]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, 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.

[22]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.

as controls, perceived past returns are still significantly significant predictors of investment.

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 if multicollinearity is the cause of individually statistically insignificant coefficients.

Taking stock, in all specifications, controlling for perceived past returns improve 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, 2020); Liu and Sui (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.

4.3 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, college graduates and not, those with household income above and below $75,000, ages above 50 and below 50, males and females, those with a high and low numeracy scores, and those who did and did not check a housing website in the past year.[23] Appendix Tables A6 and A7 report the results of estimating (7) for each subsample.

[23]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. The question framing for recently checking a housing website is "Over the past 12 months, how often have you consulted websites or other sources that give you information on the estimated current value of your property or properties in your area?" with answers "Never", "1-2 times", "3-4 times", or "5 times or more".

Across most subgroups, even after controlling for the forecasted distribution of future returns and demographics, perceived past home-price growth strongly predicts investment choices.[24] An 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 sensitive to downside risk in home prices and therefore avoid buying a home. Conditional on the stated belief distribution of future returns, renters do not appear to consider either the perceived past or forecasted returns.

Another source of heterogeneity that we seek to explore with the heterogeneity results is financial literacy. As suggested by the framework in Section 2, it is possible that many investors lack the required expertise to make an informed home price forecast. Facing financial risk, they trust their subjective experience as a more reliable signal than their own stated forecasts for investment decisions. It could be that financially sophisticated investors can make an relatively informed forecast and use it as a basis for financial choices. We use several proxies for financial literacy, including income, education, age, numeracy score, and whether respondents checked housing websites or other sources for their homes' estimated values. We find mixed evidence for the effect of financial sophistication. 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 our findings are relatively common across income and education groups.[25] On the other hand, the combination of columns 1, 2, 7, and 8 of Appendix Table A7 suggests that our results are primarily driven by younger investors and those who do not actively follow the housing market.[26]

While our main focus is on the coefficients for the perceived past returns, we also note that the coefficients for the stated forecasts in columns 7 and 8 of Appendix Table A7 show that investors actively following the housing market display a much stronger reliance on their expected returns than do the other investors. This is consistent with the heterogeneity results in Giglio et al. (2019), who find a much stronger relationship between stated forecasts and actions for investors who pay more attention to their accounts, measured by frequency of logging in. Here, our contribution is that even the inattentive investors could also demonstrate a strong expectation effect, if we properly measure their decision-relevant expected return by incorporating the perceived past returns. This supports the explanation that lack of knowledge about the housing market could induce investors to rely on their subjective

[24]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.

[25]Our results also hold for households with high numeracy scores. Because only a small sample of low-scoring respondents on the numeracy test, the coefficients for the low-scoring subsample have the right signs but are very imprecise.

[26]See also Agarwal et al. (2009), who find a higher incidence of financial mistakes among the young and old relative to the middle-aged.

past and shrink the role of stated forecasts, broadly consistent with the cognitive uncertainty framework in Section 2.

4.4 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 A8 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.

5 Interpretation

In this section, we adjudicate among possible interpretations of the empirical findings in Section 4. First, we consider the possibility that our results are affected by omitted variable bias (Section 5.1) or measurement error in stated home-price expectations (Section 5.2). 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 5.3. 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. Furthermore, the treatment group relies less its return forecasts than the control group does, consistent with an explanation based on cognitive uncertainty. In Section 4.1, we present direct evidence for rent forecast as a "shrunk factor" (denoted s in equations (2) to (5)) in the cognitive-uncertainty framework of Section 2.

5.1 Omitted Variable Bias and Risk Aversion

The Merton (1971) framework for risky-asset demand in equation (1) above motivates our exploration of factors that should affect investment demand and that could be conditionally correlated with past returns r_t. Specifically, we check whether beliefs about the distribution of expected returns (σ_t^2), risk aversion (α), measurement error in surveyed expectations $\hat{E}_t[r_{t+1}]$, and multicollinearity between $\hat{E}_t[r_{t+1}]$ and r_t could be driving our results. Other potential omitted variables include demand shocks that depend on the outcome variable of interest. For example, when the dependent variable is the probability of buying a primary residence, omitted variables include preferences over home ownership, the relative quality of owner-occupied and rental housing in a respondent's local area, the likelihood of moving regions, mortgage-credit availability, etc. When the outcome variable is share invested in the housing fund, the environment is much simpler, motivating its use for our purposes. Presumably, an investor's decision about such a derivative investment is a function of only the forecasted distribution of the expected return distribution and risk aversion, especially when the initial investment capital is provided by the survey administrators.

For the forecasted distribution of home-price growth, we control for the subjective probabilities of future returns falling into four ranges—and polynomials of those bin probabilities—in Table 4. As discussed in Section 4.2 above, flexible return-distribution controls add some explanatory power and reduce the magnitude of the coefficient on past returns in predicting investment. However, past returns are still a significant predictor of investment demand even conditional on the distribution of expected returns.

While Tables 2-4 include some risk-aversion controls, 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 on a risk tolerance metric, measured using a 1-10 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 percentage points, suggesting that our risk tolerance variable is a particularly 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. Conditional on this measure of risk tolerance, 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. We also note that the R^2 increases by only 0.009 from columns 2 to 3 as we move from a linear control for the risk tolerance score to more nonparametric indicators for each value of the risk tolerance score. This small marginal impact of additional flexibility suggests that finer measures of risk appetites are unlikely to reverse our main results.

Another alternative explanation based on risk aversion 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 (see Chetty et al. (2017); Meeuwis (2019)). However, our risk-tolerance variable is measured contemporaneous with the investment decision, i.e., *after* any effect of past returns on current risk aversion has been realized. We also note that the median gain in home value over the past year in our sample is around \$6,000, relatively small compared with a median annual income of \$75,000 and unlikely to cause a large shift in risk aversion. Finally, in Appendix Table A10, we interact past home-price growth with measures of the importance of home equity in an individual's portfolio. These measures include leverage in her primary residence, home value as a share of net assets, and home values divided by income. Intuitively, the wealth effect of past returns should be stronger for households with higher leverage or a more expensive home relative to their income. Across all specifications, none of the interaction terms has a statistically significant coefficient, suggesting past returns lowering risk aversion is not an alternative explanation for our empirical findings.

5.2 Measurement Error in Home Price Expectations

Could our results in Section 4 stem from noise in survey responses? Such an explanation finds plausibility, for example, in the bunching of returns forecasts around 0%, 5%, 10%, etc. Similarly, the common finding across expectations surveys that different question framings on returns generate systematically different responses (Glaser et al. (2007); Armona et al. (2018); Glaser et al. (2019)) suggests a degree of instability and noise in stated beliefs. While such survey errors are likely present in both perceived past and forecasted returns, if stated forecasts are particularly noisy, this could induce downward bias in the expected return coefficient and an upward bias in the past experience coefficient as the latter would be correlated with the signal in the former.

However, as we detail in this subsection, several pieces of evidence are inconsistent with a measurement-error interpretation. Foremost, our results that past returns matter even conditional on the expected returns are robust to instrumenting for the expected returns. To formalize the null hypothesis that measurement error explains our results and the bias correction instrumenting affords under the null, we first present an econometric framework with supporting simulations in Section 5.2.1 followed by empirical results in Section 5.2.2. Second, in Section 5.3, we will show that 41% of the investors admit relying more on the past return over the expected return. If an investor knows her expected return and the expected return is only imperfectly observed by the econometrician due to measurement error, we would still expect all investors to report that their decisions are based on their observable-to-them true expected returns. Instead, we find that a sizable fraction of the population is backward-looking and aware of it.

To contextualize this exercise, Table 6 presents summary statistics for the forecast errors of various forecasts. In both the 2015 and 2015-2019 samples in panels I and II, $\hat{E}_t[r_{i,t+1}]$ has smaller forecast errors than $\hat{r}_{i,t}$ by mean absolute error and mean square error, suggesting that the adjustments that our survey respondents make on $\hat{r}_{i,t}$ to form forecasts are on average rational. However, to illustrate the likelihood that stated beliefs contain noise (or irrational discretion), we compare the forecast-error performance of stated expected returns against predicted future returns. We predict future returns $\widehat{HPA}_{i,t+1}$ from a linear regression of realized returns on past returns and combinations of demographics and forecasted fundamentals. Table 6 shows that conditional on any combination of belief factors, predicted future returns $\widehat{HPA}_{i,t+1}$ have smaller forecast errors than $\hat{E}_t[r_{i,t+1}]$, consistent with stated beliefs being elicited with noise.

5.2.1 Econometric Framework for a Measurement Error Interpretation

To formalize the measurement error explanation of our results, we consider whether the following data generating process (DGP) could generate our results.

$$\begin{aligned} Y_{i,t} &= \beta_1 E_t^*[r_{i,t+1}] + \varepsilon_{i,t} \\ E_t^*[r_{i,t+1}] &= \pi_1 \hat{r}_{i,t} + Z_{i,t}' \pi_2 + v_{i,t} \\ \hat{E}_t[r_{i,t+1}] &= E_t^*[r_{i,t+1}] + \eta_{i,t}, \end{aligned} \tag{8}$$

where the outcome variable $Y_{i,t}$ is a linear function of the true forecast $E_t^*[r_{i,t+1}]$ plus some independent unobserved heterogeneity.[27] Consistent with classical assumptions about expectation formation, investors form expectations $E_t^*[r_{i,t+1}]$ as a function of their perceived past home price growth $\hat{r}_{i,t}$ and other belief factors, including a systematic component $Z_{i,t}$ (expected economic conditions, expected rent growth, etc.) and a discretionary adjustment $v_{i,t}$.[28] In this data-generating process, investors base their decisions on their actual forecasts $E_t^*[r_{i,t+1}]$, but the econometrician observes only a noisy measure $\hat{E}_t[r_{i,t+1}]$ of actual beliefs that contains measurement error $\eta_{i,t}$. The measurement error concern is that because forecasted returns are imprecisely reported, when we regress actions on $\hat{r}_{i,t}$ and $\hat{E}_t[r_{i,t+1}]$, we could still estimate a positive coefficient on $\hat{r}_{i,t}$ even though investors do follow a two-step procedure of first formulating $E_t^*[r_{i,t+1}]$ and then basing investment decisions on it.

Our alternative DGP is

$$\begin{aligned}
Y_{i,t} &= \beta_1 E_t^*[r_{i,t+1}] + \beta_2 \hat{r}_{i,t} + Z_{i,t}'\beta_3 + \varepsilon_{i,t} \\
&= (\beta_1\pi_1 + \beta_2)\hat{r}_{i,t} + Z_{i,t}'(\beta_1\pi_2 + \beta_3) + \beta_1 v_{i,t} + \varepsilon_{i,t} \\
E_t^*[r_{i,t+1}] &= \pi_1 \hat{r}_{i,t} + Z_{i,t}'\pi_2 + v_{i,t} \\
\hat{E}_t[r_{i,t+1}] &= E_t^*[r_{i,t+1}] + \eta_{i,t},
\end{aligned} \tag{9}$$

where we acknowledge measurement error η in stated beliefs but also allow the possibility that subjective past experience $\hat{r}_{i,t}$ and $Z_{i,t}$ have independent effects on actions, such that the null hypothesis in (8) corresponds to $\beta_2 = \beta_3 = 0$. Equivalently, an investor could weight factors differently in the action stage than in the forecast stage, for example, overweighting their own past experience in investment decisions relative to the forecast-stating domain.[29]

The following simulation illustrates that the DGP under the null hypothesis can generate a positive coefficient on $\hat{r}_{i,t}$ if we only control for $\hat{r}_{i,t}$ and $\hat{E}_t[r_{i,t+1}]$ together. We parameterize the model in 9 according to the null hypothesis of no independent effect of past returns on investment with $\beta_1 = \sqrt{2}$, $\beta_2 = \beta_3 = 0$, $\pi_1 = \pi_2 = 1$, and $\hat{r}_{i,t}, Z_{i,t}, \varepsilon_{i,t}, v_{i,t}$ are independently, identically distributed $\mathcal{N}(0,1)$. We then vary the standard deviation σ_η of the measurement error $\eta_{i,t} \sim \mathcal{N}(0, \sigma_\eta^2)$ to test how measurement error affects the corresponding regression

[27] To simplify the exposition, we abstract away from risk aversion in this version of the decision rule.

[28] Table 2 shows that forecasted fundamentals can explain a meaingful fraction of the variation in stated forecasts.

[29] As argued in Section 2, one motivation for overweighting and underweighting at the decision stage is the investor's confidence in $\hat{r}_{i,t}$, relative to her confidence in $Z_{i,t}$. If an investor is more confidence in $\hat{r}_{i,t}$ than in $Z_{i,t}$, she is likely to overweight $\hat{r}_{i,t}$. Building on an exogenous information treatment designed by Armona et al. (2018) to provide respondents with actual past home price growth, Appendix Table 10 shows that when investors are informed of actual past returns, they rely more on both the past home price growth and their updated forecasts.

coefficients. Panel I of Figure 4 shows that the estimated coefficient on $\hat{r}_{i,t}$ increases in the variance of the measurement error. In other words, despite the data being generated with the true coefficient β_2 on past returns being zero, measurement error in expected returns and the positive correlation between past returns and the signal in stated returns (from $\pi_1 > 0$) upward biases OLS estimates of the role of past returns in investment. This result highlights the potential for measurement error in stated returns to spuriously generate a non-zero estimated role for past returns in the second-stage statistical model of investment decisions.

However, we next show that if we instrument for $\hat{E}_t[r_{i,t+1}]$, this fixes the bias in the second-stage coefficient on $\hat{r}_{i,t}$. If past returns in fact have no effect on investment because they are fully incorporated into expectations, instrumental-variables estimates of β_2 should be statistically close to zero. To see this, consider the second-stage equation, where we regress investment decisions on predicted expected returns and perceived past returns

$$Y_{i,t} = \beta_1 \widehat{E_t[r_{i,t+1}]} + \beta_2 \hat{r}_{i,t} + \varepsilon_{i,t}, \tag{10}$$

where the predicted values are a function of both the included ($\hat{r}$) and excluded (Z) exogenous variables $\widehat{E_t[r_{i,t+1}]} = \hat{\pi}_1 \hat{r}_{i,t} + Z'_{i,t}\hat{\pi}_2$ Under the null hypothesis, where both $\hat{r}_{i,t}$ and $Z_{i,t}$ only affect $Y_{i,t}$ through $E^*_t[r_{i,t+1}]$, instrumenting would provide an unbiased estimate of β_2 such that the average 2SLS estimate of $\hat{\beta}_2$ would be 0. We note that under the null, other belief factors $Z_{i,t}$ meet the requirements for a valid instrument for $\hat{E}_t[r_{i,t+1}]$. Accordingly, by the unbiasedness of 2SLS, the expected second-stage estimated coefficient $\hat{\beta}_2$ on $\hat{r}_{i,t}$ equals the true second-stage coefficient for $\hat{r}_{i,t}$, which is 0. In Appendix B, we formally derive $E[\hat{\beta}_2] = 0$. Panel II of Figure 4 illustrates this finding, showing that in simulations the estimated coefficient $\hat{\beta}_2$ on $\hat{r}_{i,t}$ is consistently close to 0 regardless of the size of the measurement error σ_η. This simulation result is also robust to adding measurement error in $Z_{i,t}$, alleviating potential concern that other factors considered in the stated forecasts being measured with noise could also affect the coefficient on $\hat{r}_{i,t}$.

5.2.2 Addressing Measurement Error through Instrumental Variables

Motivated by the derivations and simulations in Section 5.2.1, we present results that instrument for $\hat{E}_t[r_{i,t+1}]$ with forecasted fundamentals $Z_{i,t}$. The logic behind these tests is that if the measurement-error null hypothesis were the reason for our estimated effects of past returns, we could address the resulting bias through instrumenting and the 2SLS estimate of β_2 would be 0. Moreover, under the null hypothesis of measurement error in stated beliefs and no independent role for belief factors, any forecasted fundamental would satisfy the exclusion restriction and be a valid instrument for stated beliefs. However, we demonstrate that our core findings are robust to instrumenting, leading us to reject the measurement-error hypothesis as the model generating our results.

Out of many potential belief factors that could be considered elements of $Z_{i,t}$, we focus on inflation forecasts and rent forecasts as instruments because belief-formation regressions suggests that survey respondents incorporate these views into their home price forecasts. Table 2 demonstrates that individual expectations of inflation and rent growth are incorporated into individual expected returns, satisfying the 2SLS relevance condition (see Appendix Table A11 for the exact first stage used in our 2SLS estimation). As an alternative to using forecasts of fundamentals as instruments for expected housing returns, we follow Lewbel (1997) and construct an additional instrument based on higher-order moments of the potentially mismeasured variable $(\hat{E}_t[r_{i,t+1}] - \overline{\hat{E}_t[r_{i,t+1}]})^2$. Appendix C outlines the assumptions required by this approach, and Appendix Table A11 demonstrates the strong first stage for higher-order moments of stated returns.

Table 7 presents both OLS and IV estimates of (10) without and with individual-level controls in columns 1-3 and 4-6, respectively. Across all columns, $\hat{r}_{i,t}$ has statistically significant coefficients in both OLS specifications and when we instrument for $\hat{E}_t[r_{i,t+1}]$, inconsistent with the prediction of the null hypothesis that both $\hat{r}_{i,t}$ and $Z_{i,t}$ affect $Y_{i,t}$ only through stated forecasts $\hat{E}_t[r_{i,t+1}]$. At the same time, Table 7 offers additional evidence against the null hypothesis. Under the null hypothesis, forecasted fundamentals are excludable and valid instruments under the null hypothesis. However, instrumenting for stated returns does not reverse any measurement-error induced attenuation bias in Table 7. Contrary to what would be expected under the null hypothesis, instrumenting reduces the magnitude of expected returns. While inconsistent with the null hypothesis of no independent effect of belief factors on investment conditional on stated beliefs, we argue below that these results are consistent with expectations surveys eliciting different beliefs from the beliefs used in decision making.

5.2.3 Reduced Form and Evidence for Shrunk Factors

The cognitive uncertainty model in Section 2 assumes existence of a signal s that an investor relies on when forming return forecasts but down-weights in investment decisions. Mathematically, this behavior would imply $\beta_{2,e} > \beta_{2,i}$ in equations (4) and (5) and $\frac{\beta_2}{\beta_3} > \frac{\pi_1}{\pi_2}$ in equation (9). In this section, using the reduced-form version of regressions in Section 5.2.2, we show that forecasted rent growth and inflation forecast are such factors.[30] Column 1 in Table 8 repeats column 5 of Table 2 for reference by regressing home-price growth forecast on perceived past home-price growth, forecasted rent growth, inflation forecast and

[30] See Kindermann et al. (2019) for how rent growth affects home price forecasts.

demographic controls. Both forecasted rent growth and inflation forecast are statistically significant factors considered in home-price growth even conditional on other factors. A one percentage point higher rent growth is associated with a 0.11 percentage point higher expected home-price growth, and a one percentage point higher inflation forecast is associated with a 0.13 percentage point higher expected home-price growth.

Column 2 regresses the share invested in a housing fund on perceived past home-price growth, forecasted rent growth, forecasted inflation, and our usual individual controls. Despite rent growth's and inflation forecast's importance in home price forecast, they are ignored (if not down-weighted) in investment decisions, inconsistent with their impacting investment only through beliefs. Column 3 reinforces this point by conditioning on flexibly specified return distribution forecasts that should capture the effect of beliefs on investment if stated beliefs are a sufficient statistic for the beliefs used in decision making. Instead, conditional on past beliefs, past returns are up-weighted and forecasted rent growth and forecasted inflation are down-weighted. While the negative coefficients on forecasted rent growth and inflation are statistically insignificant, their magnitudes are relatively large and insensitive to our controls for beliefs about the distribution of future returns.

Overall, the pattern of results in Table 8 are inconsistent with with stated beliefs fully capturing the beliefs channel of investment demand. They are, however, consistent with a model of decision making such as cognitive uncertainty wherein investors form different beliefs depending on the nature of the decision domain, relying more (less) on belief factors they are more (less) confident in when making financial decisions.

5.3 Direct Survey Measures of Decision Factors

For a more direct measure of decision-making factors, we ask half of the 2020 survey respondents whether they rely more on their survey-reported returns forecasts 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 (the treatment group) to be able to test whether the self-reflection required to answer the question changes investment behavior. 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 their stated expected returns or past returns. We also study the characteristics of respondents choosing their stated expected 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 9 presents summary statistics for the 2020 sample. The first two columns present average characteristics for the treatment group and the control group separately.[31] The two groups have similar characteristics, as expected given random assignment.[32] The next two columns show summary statistics for those who consider their stated expected returns or past returns as the more important consideration underlying their investment decisions. First, 41% of respondents from the treatment group report that they rely on past returns more than their survey-stated expectations 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 expected returns. Second, respondents relying on past or stated expected returns have significantly different observable characteristics. Compared with those stating they rely on past returns, respondents who rely more on stated expected 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 higher average housing investment in the housing derivative (69% versus 52%).

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 9 reports summary statistics on this question. On average, 37% of 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 stated expected returns for the housing question and the stock question. Among respondents selecting stated expected return for the housing question, 80% of them also choose stated expected returns for the stock-market investment question, whereas only 37% of those relying on past returns for housing report relying on stated expected returns for stocks.

We next explore correlates of responses to past versus stated expected return questions to explain why past returns affect investment choices on average even conditional on stated beliefs. Appendix Table A12 reports estimates from regressing an indicator for choosing forecasted returns over subjective past returns as the outcome variable on demographics. Besides three proxies for financial literacy (income, college-education, and numeracy), we only report covariates with statistically significant coefficients for at least one of the housing and stock questions. First, consistent with our model of cognitive uncertainty in Section 2,

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

[32]We fail to reject that the means of the observables in Table 9 are equal between treatment and control (p-values of 0.14 to 1).

risk tolerance is a strong predictor of relying on stated expected returns over past returns.[33] Moving from the most to the least risk averse households, the probability of choosing stated expected 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. Consistent with this possibility, we find that college education is a strong predictor for choosing stated forecasted returns over past returns.

We hypothesized that this self-reflection could nudge respondents to lean against any cognitive behavioral bias leading them to overemphasize past returns. Instead, we found that our treatment seems to encourage people to rely *less* on their return forecasts, as shown by the Forecasted Returns $\times$ Treated interaction terms in Table 10. This suggests that some people consciously and deliberately use their memory of past returns to inform investment decisions even conditional on their surveyed expectations rather than only passively through some subconscious bias.

Finally, we study whether people's reported reliance on future versus past returns is consistent with their actual investment decision rule. In other words, do those reporting that they rely on stated expected returns indeed base their investment decisions on their return forecast? Appendix Table A13 reports these results. Neither the backward-looking or the forward-looking group rely on the expected return in a statistically significant way. Backward-looking respondents indeed rely on perceived past returns, consistent with their self-reported decision factors. The forward-looking group also displays dependence on past returns in some specifications. We also note that the forward-looking group on average invests much more in the housing fund than does the backward-looking group.

6 Measuring the Beliefs Channel

A central research question in the expectation literature is to measure the magnitude of the beliefs channel (e.g., Giglio et al. (2019); Armona et al. (2018)). Our results suggest that because stated beliefs are not necessarily decision-relevant beliefs, the coefficients on stated beliefs are only incomplete measures of the role of beliefs in investment demand. To use our estimates to quantify the size of the beliefs channel, we analyze the alternative model that allows investors to base their investment decisions on different expectations than the ones they stated on the survey. In a slight simplification of (9), we let

[33] As shown by Enke and Graeber (2019), cognitive uncertainty is distinct from risk aversion. Our results suggest that risk averse investors could simultaneously display more risk aversion and cognitive uncertainty as two distinct features. For more details, see Enke and Graeber (2019).

$$
\begin{aligned}
Y_{i,t} &= \beta_0 + \beta_1 \hat{E}_t[r_{i,t+1}] + \beta_2 \hat{r}_{i,t} + \varepsilon_{i,t} \\
\hat{E}_t[r_{i,t+1}] &= \pi_0 + \pi_1 \hat{r}_{i,t} + v_{i,t}.
\end{aligned} \tag{11}
$$

Here, the OLS estimate of β_1 in regression $Y_{i,t}$ on $\hat{E}_t[r_{i,t+1}]$ without controlling for $\hat{r}_{i,t}$ is

$$
\begin{aligned}
\hat{\beta}_1^{OLS} &= \frac{Cov(\hat{E}_t[r_{i,t+1}], Y_{i,t})}{Var(\hat{E}_t[r_{i,t+1}])} \\
&= \beta_1 + \frac{\beta_2 \pi_1 Var(\hat{r}_{i,t})}{\pi_1^2 Var(\hat{r}_{i,t}) + \sigma_v^2}.
\end{aligned}
$$

The IV estimate for β_1 with $\hat{r}_{i,t}$ as an instrument is

$$
\hat{\beta}_1^{IV} = \frac{(\beta_2 \pi_1 + \beta_1 \pi_1^2) Var(\hat{r}_{i,t})}{\pi_1^2 Var(\hat{r}_{i,t})} = \beta_1 + \frac{\beta_2}{\pi_1}.
$$

There are several implications from the above derivations. First, under the alternative DGP in (11), investors do make their decisions based on expected returns, but the decision-relevant return, denoted $\hat{E}_D[r_{i,t+1}]$, is $C(\beta_2 \hat{r}_{i,t} + \beta_1 \hat{E}_t[r_{i,t+1}])$, where C is an arbitrary positive constant. In other words, there is ambiguity in the magnitude of the beliefs channel driven by how we choose the scaling constant C. Our preferred scaling for $\hat{E}_D[r_{i,t+1}]$ is

$$
\hat{E}_D[r_{i,t+1}] = \frac{\beta_2 \hat{r}_{i,t} + \beta_1 \hat{E}_t[r_{i,t+1}]}{\beta_2 + \beta_1},
$$

such that $\hat{E}_D[r_{i,t+1}]$ is a convex combination of $\hat{r}_{i,t}$ and $\hat{E}_t[r_{i,t+1}]$. Under this scaling, the effect of decision-relevant beliefs $\hat{E}_D[r_{i,t+1}]$ on investment is $\beta_1 + \beta_2$. With $\beta_1 > 0$, this framework allows us to estimate a larger total size for the beliefs channel than the coefficient on $\hat{E}_t[r_{i,t+1}]$ in a specification controlling for $\hat{r}_{i,t}$ as in (11).

Second, the OLS coefficient on $\hat{E}_t[r_{i,t+1}]$ without controlling for $\hat{r}_{i,t}$ could be larger or smaller than $\beta_1 + \beta_2$. The sufficient and necessary condition for the OLS coefficient being smaller than $\beta_1 + \beta_2$ is

$$
\pi_1(1 - \pi_1) Var(\hat{r}_{i,t}) < \sigma_v^2.
$$

Intuitively, if there is a large deviation between $\hat{r}_{i,t}$ and $\hat{E}_t[r_{i,t+1}]$ caused by a small π_1 or a large $\frac{\sigma_v^2}{Var(\hat{r}_{i,t})}$, the OLS coefficient on $\hat{E}_t[r_{i,t+1}]$ will understate the belief channel. Finally, we note that the IV estimate $\hat{\beta}_1^{IV} = \beta_1 + \frac{\beta_2}{\pi_1}$ is larger than $\beta_1 + \beta_2$ as long as investors extrapolate less than one for one, or $\pi_1 < 1$.[34] In situations where researchers can differentiate belief

[34] This in part explains the gap between the OLS and IV estimates of the beliefs channel found by Armona

factors from demand shocks, including the belief factors directly in the demand equation will therefore enable better characterizations of the importance of beliefs in demand.

7 Conclusion

In this paper, we document that stated beliefs are not sufficient statistics summarizing all decision-relevant information used in decision-making. In particular, controlling for subjective past experience improves investment 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 belief factors such as 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 trigger an increase cognitive uncertainty.

There are several extensions we leave for future research. One is to test whether investors' extra reliance on past returns extends beyond the housing market to other assets. While we present preliminary evidence that in the stock market, investors find return forecasts more valuable than in the housing market, we lack conclusive evidence due to data limitations. We leave research for the stock market and other asset classes for future works. A second avenue is to provide additional causal evidence for the channel that we hypothesize by isolating whether it is financial risk that induces investors to weight subjective experiences more than their own forecasted returns. A third direction further tests cognitive uncertainty. Our work takes a first pass at documenting cross-sectional variation in a plausible mental default driven by past experience. Future work could explore how individual-level mental defaults are formed.

et al. (2018).

References

Agarwal, Sumit, John C Driscoll, Xavier Gabaix, and David Laibson, "The age of reason: Financial decisions over the life cycle and implications for regulation," *Brookings Papers on Economic Activity*, 2009, *2009* (2), 51–117.

Ameriks, John, Gábor Kézdi, Minjoon Lee, and Matthew D Shapiro, "Heterogeneity in expectations, risk tolerance, and household stock shares: The attenuation puzzle," *Journal of Business & Economic Statistics*, 2020, *38* (3), 633–646.

Amromin, Gene and Steven A Sharpe, "From the horse's mouth: Economic conditions and investor expectations of risk and return," *Management Science*, 2014, *60* (4), 845–866.

Andries, Marianne, Milo Bianchi, Karen K Huynh, and Sébastien Pouget, "Return Predictability, Expectations, and Investment: Experimental Evidence," October 2020. Working Paper.

Armantier, Olivier, Giorgio Topa, Wilbert Van der Klaauw, and Basit Zafar, "An overview of the survey of consumer expectations," *Economic Policy Review*, 2017, (23-2), 51–72.

_ , Wändi Bruine de Bruin, Giorgio Topa, Wilbert Van Der Klaauw, and Basit Zafar, "Inflation expectations and behavior: Do survey respondents act on their beliefs?," *International Economic Review*, 2015, *56* (2), 505–536.

Armona, Luis, Andreas Fuster, and Basit Zafar, "Home price expectations and behaviour: Evidence from a randomized information experiment," *The Review of Economic Studies*, 2018, *86* (4), 1371–1410.

Bailey, Michael, Ruiqing Cao, Theresa Kuchler, and Johannes Stroebel, "The economic effects of social networks: Evidence from the housing market," *Journal of Political Economy*, 2018, *126* (6), 2224–2276.

Barberis, Nicholas, Robin Greenwood, Lawrence Jin, and Andrei Shleifer, "X-CAPM: An extrapolative capital asset pricing model," *Journal of Financial Economics*, 2015, *115* (1), 1–24.

Barrero, Jose Maria, "The micro and macro of managerial beliefs," 2020. SSRN Working Paper No. 3196761.

Bordalo, Petro, Nicola Gennaioli, Rafael La Porta, and Andrei Shleifer, "Expectations of Fundamentals and Stock Market Puzzles," May 2020. Working Paper.

Cao, H Henry, Tan Wang, and Harold H Zhang, "Model uncertainty, limited market participation, and asset prices," *The Review of Financial Studies*, 2005, *18* (4), 1219–1251.

Carson, Richard T., Theodore Groves, and John A. List, "Consequentiality: A Theoretical and Experimental Exploration of a Single Binary Choice," *Journal of the Association of Environmental and Resource Economists*, 2014, *1* (1/2), 171–207.

Chetty, Raj, László Sándor, and Adam Szeidl, "The effect of housing on portfolio choice," *The Journal of Finance*, 2017, *72* (3), 1171–1212.

Chiang, Yao-Min, David Hirshleifer, Yiming Qian, and Ann E Sherman, "Do investors learn from experience? Evidence from frequent IPO investors," *The Review of Financial Studies*, 2011, *24* (5), 1560–1589.

Chinco, Alex, Samuel M Hartzmark, and Abigail B Sussman, "Risk-Factor Irrelevance," 2019. SSRN Working Paper.

Choi, James J. and Adrianna Z. Robertson, "What Matters to Individual Investors? Evidence from the Horse's Mouth," *The Journal of Finance*, 2020, *Forthcoming.*

Cochrane, John H, "Presidential address: Discount rates," *The Journal of Finance*, 2011, *66* (4), 1047–1108.

_ , "Macro-finance," *Review of Finance*, 2017, *21* (3), 945–985.

D'Acunto, Francesco, Daniel Hoang, and Michael Weber, "Unconventional fiscal policy," in "AEA Papers and Proceedings," Vol. 108 2018, pp. 519–23.

_ **, Ulrike Malmendier, Juan Ospina, and Michael Weber**, "Exposure to daily price changes and inflation expectations," 2019. NBER Working Paper No. 26237.

Dominitz, Jeff and Charles F. Manski, "Using Expectations Data to Study Subjective Income Expectations," *Journal of the American Statistical Association*, 1997, *92* (439), 855–867.

_ **and Charles F Manski**, "Measuring and interpreting expectations of equity returns," *Journal of Applied Econometrics*, 2011, *26* (3), 352–370.

Dow, James and Sérgio Ribeiro da Costa Werlang, "Uncertainty aversion, risk aversion, and the optimal choice of portfolio," *Econometrica*, 1992, pp. 197–204.

Drerup, Tilman, Benjamin Enke, and Hans-Martin Von Gaudecker, "The precision of subjective data and the explanatory power of economic models," *Journal of Econometrics*, 2017, *200* (2), 378–389.

Engelberg, Joseph, Charles F. Manski, and Jared Williams, "Comparing the Point Predictions and Subjective Probability Distributions of Professional Forecasters," *Journal of Business & Economic Statistics*, 2009, *27* (1), 30–41.

Enke, Benjamin and Thomas Graeber, "Cognitive uncertainty," 2019. NBER Working Paper No. 26518.

Farhi, Emmanuel and Xavier Gabaix, "Rare disasters and exchange rates," *The Quarterly Journal of Economics*, 2016, *131* (1), 1–52.

Fox, Craig R and Amos Tversky, "Ambiguity aversion and comparative ignorance," *The Quarterly Journal of Economics*, 1995, *110* (3), 585–603.

Frydman, Cary and Lawrence J Jin, "Efficient coding and risky choice," 2019. SSRN Working Paper No. 3270773.

Gabaix, Xavier, "A sparsity-based model of bounded rationality," *The Quarterly Journal of Economics*, 2014, *129* (4), 1661–1710.

_, "Behavioral inattention," in "Handbook of Behavioral Economics: Applications and Foundations 1," Vol. 2, Elsevier, 2019, pp. 261–343.

Gagnon-Bartsch, Tristan, Matthew Rabin, and Joshua Schwartzstein, *Channeled attention and stable errors*, Harvard Business School, 2018.

Giglio, Stefano, Matteo Maggiori, Johannes Stroebel, and Stephen Utkus, "Five Facts About Beliefs and Portfolios," April 2019. NBER Working Paper No. 25744.

_, _, _, and _, "Inside the Mind of a Stock Market Crash," May 2020. NBER Working Paper No. 27272.

Gilboa, I and D Schmeidler, "Maxmin Expected Utility Theory with Non-Unique Prior," *Journal of Mathematical Economics*, 1989, pp. 141–153.

Glaeser, Edward L and Charles G Nathanson, "An extrapolative model of house price dynamics," *Journal of Financial Economics*, 2017, *126* (1), 147–170.

_, Joseph Gyourko, Eduardo Morales, and Charles G Nathanson, "Housing dynamics: An urban approach," *Journal of Urban Economics*, 2014, *81*, 45–56.

Glaser, Markus, Thomas Langer, Jens Reynders, and Martin Weber, "Framing effects in stock market forecasts: The difference between asking for prices and asking for returns," *Review of Finance*, 2007, *11* (2), 325–357.

_, Zwetelina Iliewa, and Martin Weber, "Thinking about prices versus thinking about returns in financial markets," *The Journal of Finance*, 2019, *74* (6), 2997–3039.

Greenwood, Robin and Andrei Shleifer, "Expectations of returns and expected returns," *The Review of Financial Studies*, 2014, *27* (3), 714–746.

Guren, Adam M, "House price momentum and strategic complementarity," *Journal of Political Economy*, 2018, *126* (3), 1172–1218.

Hendren, Nathaniel, "Private information and insurance rejections," *Econometrica*, 2013, *81* (5), 1713–1762.

_, "Knowledge of future job loss and implications for unemployment insurance," *American Economic Review*, 2017, *107* (7), 1778–1823.

Hurd, Michael D, "Subjective probabilities in household surveys," *Annual Review of Economics*, 2009, *1* (1), 543–562.

Kaustia, Markku and Samuli Knüpfer, "Do investors overweight personal experience? Evidence from IPO subscriptions," *The Journal of Finance*, 2008, *63* (6), 2679–2702.

Kézdi, Gábor and Robert J Willis, "Household stock market beliefs and learning," Technical Report, National Bureau of Economic Research 2011.

Khaw, Mel Win, Ziang Li, and Michael Woodford, "Cognitive imprecision and small-stakes risk aversion," 2018. NBER Working Paper No. 24978.

Kindermann, Fabian, Julia Le Blanc, Monika Piazzesi, and Martin Schneider, "Learning about Housing Cost-Survey Evidence from German House Price Booms," Technical Report, mimeo, Stanford University 2019.

Kosar, Gizem, Tyler Ransom, and Wilbert der Klaauw, "Understanding migration aversion using elicited counterfactual choice probabilities," 2020.

Lettau, Martin, Matteo Maggiori, and Michael Weber, "Conditional risk premia in currency markets and other asset classes," *Journal of Financial Economics*, 2014, *114* (2), 197–225.

Lewbel, Arthur, "Constructing instruments for regressions with measurement error when no additional data are available, with an application to patents and R&D," *Econometrica*, 1997, *65* (5), 1201–1213.

Lipkus, Isaac M, Greg Samsa, and Barbara K Rimer, "General performance on a numeracy scale among highly educated samples," *Medical Decision Making*, 2001, *21* (1), 37–44.

Liu, Hongqi and Pengfei Sui, "Social Interactions in the Market: A Live Belief Contagion Test," 2020.

_ , Cameron Peng, A. Wei Xiong, and Wei Xiong, "Resolving the Excessive Trading Puzzle: An Intergrated Approach Based on Surveys and Transactions," 2020.

Lusardi, Annamaria, "Financial literacy: an essential tool for informed consumer choice?," 2008. NBER Working Paper No. 14084.

Malmendier, Ulrike and Stefan Nagel, "Depression babies: do macroeconomic experiences affect risk taking?," *The Quarterly Journal of Economics*, 2011, *126* (1), 373–416.

_ and _ , "Learning from inflation experiences," *The Quarterly Journal of Economics*, 2016, *131* (1), 53–87.

_ , Demian Pouzo, and Victoria Vanasco, "Investor experiences and financial market dynamics," *Journal of Financial Economics*, 2019.

Manski, Charles F., "Measuring expectations," *Econometrica*, 2004, *72* (5), 1329–1376.

_ , "Survey Measurement of Probabilistic Macroeconomic Expectations: Progress and Promise," *NBER Macroeconomics Annual*, 2018, *32*, 411–471.

— **and Francesca Molinari**, "Rounding Probabilistic Expectations in Surveys," *Journal of Business & Economic Statistics*, 2010, *28* (2), 219–231.

Meeuwis, Maarten, "Wealth fluctuations and risk preferences: Evidence from us investor portfolios," 2019. Working Paper.

Merton, Robert C, "Optimum consumption and portfolio rules in a continuous-time model," *Journal of Economic Theory*, 1971, *3* (4), 373–413.

Nagel, Stefan and Zhengyang Xu, "Asset pricing with fading memory," 2019. NBER Working Paper No. 26255.

Piazzesi, Monika and Martin Schneider, "Momentum traders in the housing market: Survey evidence and a search model," *American Economic Review*, 2009, *99* (2), 406–11.

Porter, Stephen R. and Michael E. Whitcomb, "The Impact of Lottery Incentives on Student Survey Response Rates," *Research in Higher Education*, 2003, *44* (4), 389–407.

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

Figure 1: Investment Questions in the 2020 Survey

I. 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	

II. 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.

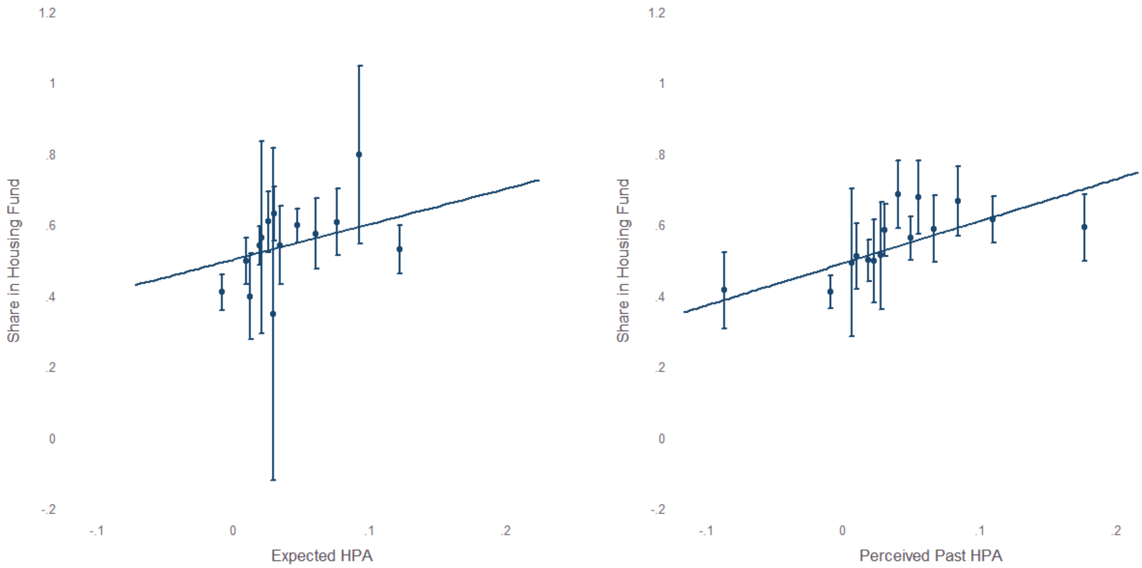
What proportion of the $1,000 would you invest in:

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

The housing market fund		%
The savings account		%
TOTAL	0	

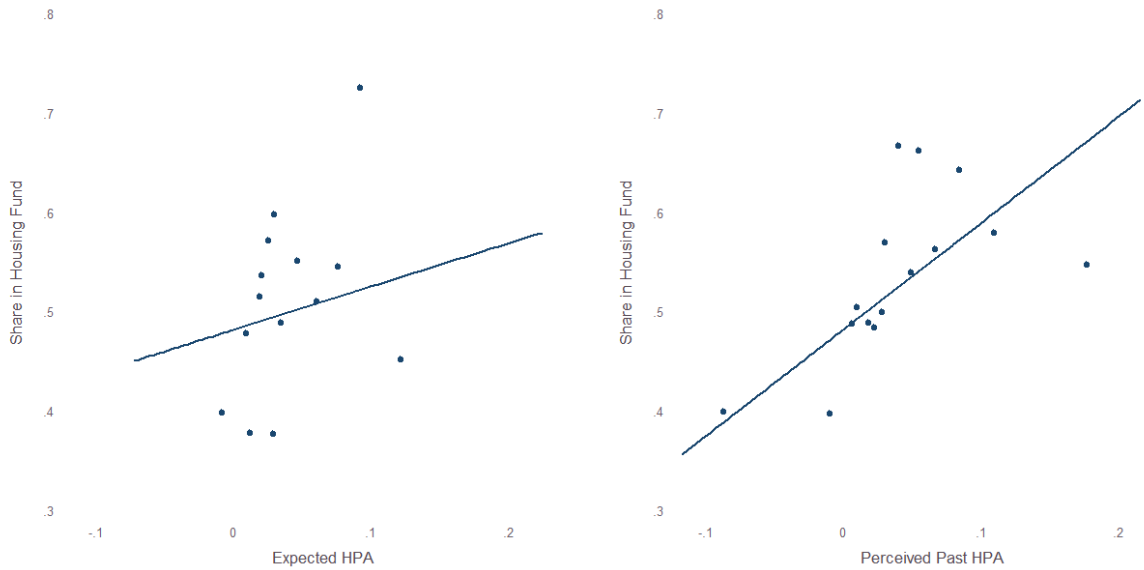
Notes: Figure shows the investment experiment in the 2020 survey. Half of the respondents receive questions shown in the top panel. The other half receive questions shown in the bottom panel.

Figure 2: Housing Fund Shares versus Forecasted Returns and Perceived Past Returns



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.

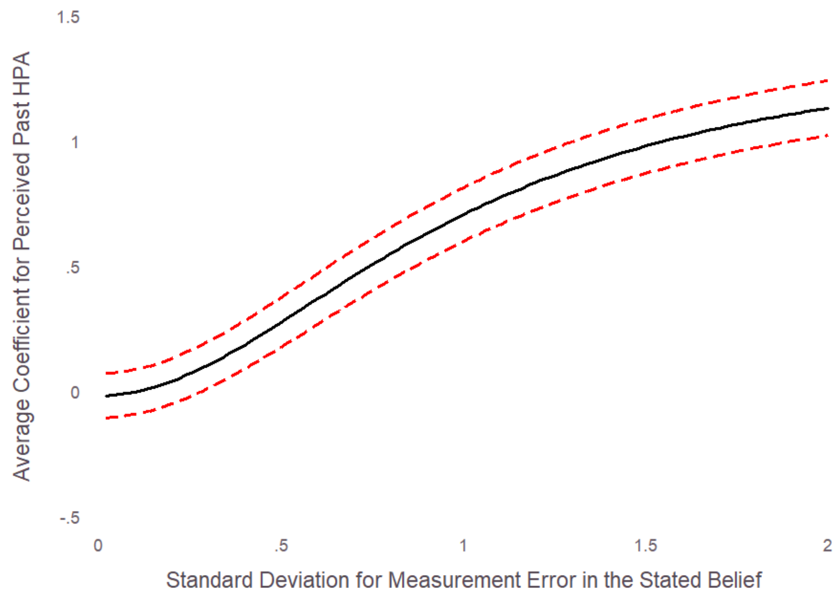
Figure 3: Binned Scatter Plots Controlling for the Other Covariate Linearly



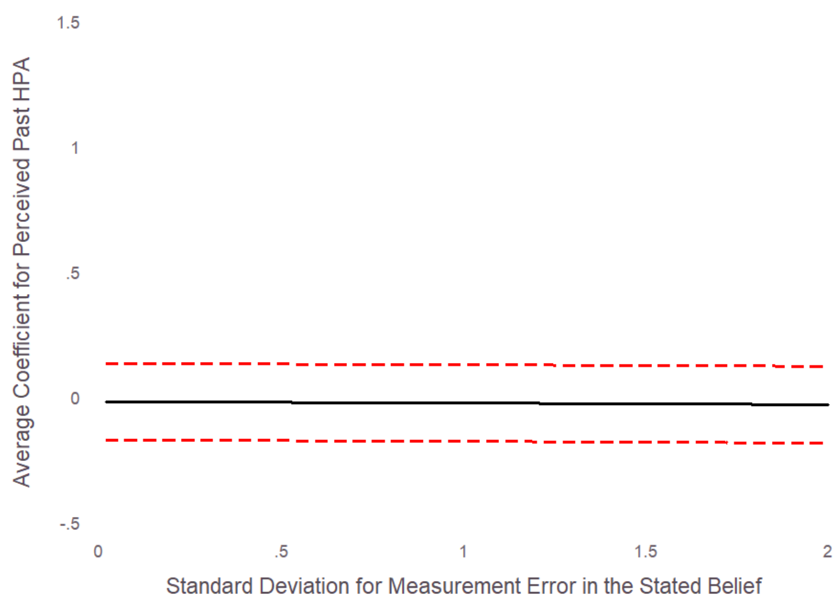
Notes: Figure presents binned scatter plots for the share of an \$1,000 investment in the housing fund versus the expected home-price growth and the perceived past home-price growth. N = 1,012.

Figure 4: Simulated Past Returns Coefficients Under Measurement Error

I. Simulated OLS Coefficient on Past Returns

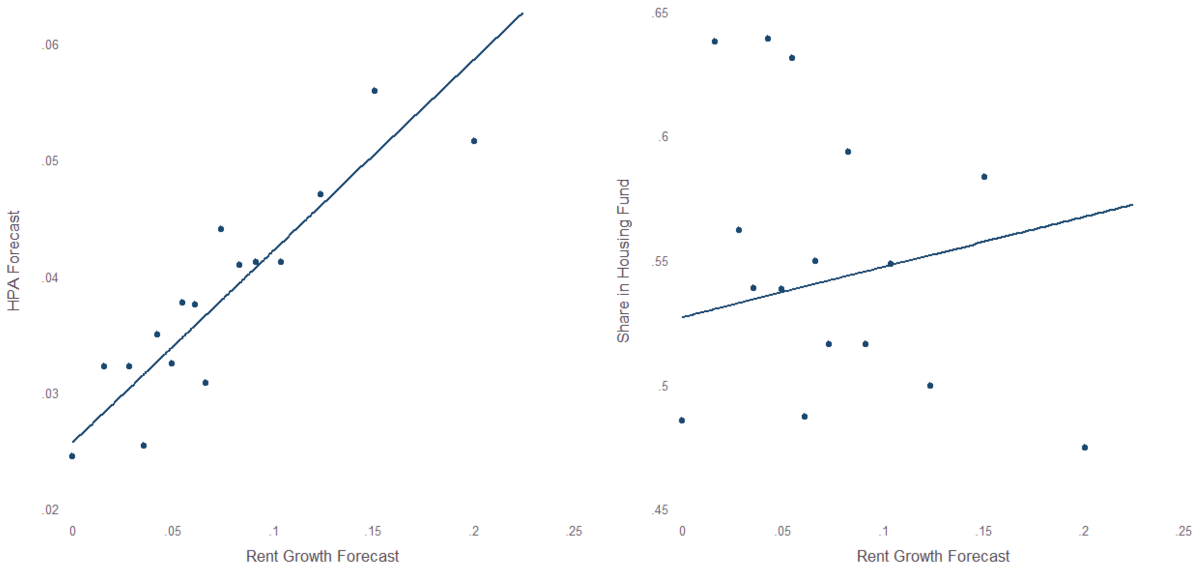


II. Simulated 2SLS Coefficient on Past Returns



Notes: Panels I and II, respectively, plot average OLS and 2SLS coefficients on perceived past returns from a regression of simulated investment decisions on expected returns and past returns where expected returns are measured with error. The 2SLS estimator in panel II instruments for stated expected returns with expected rent growth. The data generating process is specified in (9) with $\beta_1 = \sqrt{2}$, $\beta_2 = \beta_3 = 0$, and $r_{i,t}, Z_{i,t+1}, \varepsilon_{i,t}, v_{i,t} \sim \mathcal{N}(0,1)$. The measurement error $\eta_{i,t}$ in the stated forecasts is normally distributed, with varying standard deviation shown in the horizontal axis. Black lines plot average coefficients from 100 simulations of 1,000 observations each. Dashed red lines plot average confidence intervals.

Figure 5: Expected Returns and Housing Fund Share versus Forecasted Rent Growth



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.
I. Individual Characteristics			
Age (years)	5,836	51.2	15.3
Male Indicator	5,835	0.54	0.50
Minority Indicator	5,828	0.16	0.37
Married Indicator	5,836	0.65	0.48
Homeowner Indicator	5,816	0.76	0.42
College Graduate Indicator	5,835	0.57	0.50
1(Household Income $\geq$ \$100K)	5,779	0.29	0.45
1(Liquid Savings $\geq$ \$75K)	5,481	0.66	0.47
Numeracy Score	5,836	4.06	1.04
Risk Loving	5,825	4.41	2.20
II. Beliefs and Investment Actions			
Forecasted HPA in the Next 12 months	5,869	0.04	0.05
Perceived HPA in the Past 12 months	5,866	0.04	0.06
Confidence in Recalled Price Change	5,865	3.21	0.90
Actual HPA in the Past 12 months	5,785	0.05	0.04
|Perception Gap|	5,785	0.05	0.05
Share Invested in a Housing Fund (2015)	1,012	0.54	0.34
Share Invested in a Housing Fund (2020)	808	0.61	0.32
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
III. Fundamental Forecasts			
1yr Inflation Expectation	5,827	3.40	4.17
1yr Mortgage Rate Change Expectation	5,827	0.34	0.66
1yr Rent Growth Expectation	5,827	4.15	6.41
1yr Economic Condition Expectation	5,826	3.24	0.81
1yr Stock Market Growth Expectation	901	7.15	6.94

Notes: Table reports means, standard deviations, and counts of individual responses used in the empirical analysis. Numeracy is coded between 1 and 5, based on the number of correct answers to 5 questions testing numerical literacy. Risk loving is coded from 1 (risk averse) to 10 (risk loving). Confidence level of past home-price growth estimate is coded from 1 (not all confident) to 5 (very confident). 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. 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. Likelihood of buying a primary residence is asked to respondents who report an over-5% probability of moving within 3 years.

Table 2: The Effect of Perceived Past Returns on Belief Formation

Dependent Variable: Forecasted Returns	(1)	(2)	(3)	(4)	(5)
Perceived Past Returns	0.28***	0.27***	0.25***	0.24***	0.21***
	(0.015)	(0.016)	(0.015)	(0.016)	(0.030)
Forecasted Rent Growth			0.16***	0.16***	0.11***
			(0.012)	(0.012)	(0.034)
Forecasted Inflation			0.06***	0.05***	0.13***
			(0.016)	(0.017)	(0.036)
Individual Controls		X		X	X
Fundamentals			X	X	X
Sample		2015-2020			2015
Observations	5,816	5,389	5,816	5,389	1,012
R-Squared	0.120	0.151	0.166	0.200	0.263

Notes: Dependent variable is surveyed expected house price appreciation over the next year. Perceived past returns are respondent's estimate of home-price appreciation in their zip code over the past year. 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, 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 a 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. The samples used in columns 1-4 and 5 are survey years 2015-2020 and 2015 alone, respectively. 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)
Forecasted Returns	1.00***		0.44	0.81***		0.41
	(0.29)		(0.31)	(0.29)		(0.30)
Perceived Past Returns		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)
Male				7.82***	7.64***	7.77***
				(2.23)	(2.22)	(2.23)
Homeowner				-4.76	-5.87*	-5.53*
				(3.12)	(3.10)	(3.11)
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 a 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)
Forecasted Returns	0.20	0.17	0.13	0.13	0.29	
	(0.30)	(0.31)	(0.31)	(0.31)	(0.32)	
Perceived Past Returns	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: The Role of Risk Aversion in Investment Decisions

Dependent Variable: Housing Fund Share	(1)	(2)	(3)
Risk Tolerance (1-10)	3.38***	3.18***	
	(0.49)	(0.94)	
Forecasted Returns		0.12	0.09
		(0.31)	(0.31)
Perceived Past Returns		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 Cubic		X	X
Individual Controls		X	X
Observations	1,012	1,012	1,012
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: Forecast Errors Summary Statistics

	Panel A: 2015 Sample		
	Mean Error	Mean Absolute Error	Mean Squared Error
Raw HPA Forecast	1.47%	4.28%	0.0032
Perceived Past HPA	1.14%	4.80%	0.0040
$\widehat{HPA}_{i,t+1}$ ($\hat{r}_{i,t}$)	1.48%	3.42%	0.0021
$\widehat{HPA}_{i,t+1}$ ($\hat{r}_{i,t}$, demographics)	1.48%	3.32%	0.0020
$\widehat{HPA}_{i,t+1}$ ($\hat{r}_{i,t}$, fundamentals)	1.48%	3.55%	0.0022
$\widehat{HPA}_{i,t+1}$ ($\hat{r}_{i,t}$, fundamentals, demographics)	1.48%	3.46%	0.0021

	Panel B: 2015-2019 Sample		
	Mean Error	Mean Absolute Error	Mean Squared Error
Raw HPA Forecast	1.33%	4.32%	0.0033
Perceived Past HPA	0.59%	4.78%	0.0041
$\widehat{HPA}_{i,t+1}$ ($\hat{r}_{i,t}$)	1.33%	3.01%	0.0016
$\widehat{HPA}_{i,t+1}$ ($\hat{r}_{i,t}$, demographics)	1.33%	3.00%	0.0015
$\widehat{HPA}_{i,t+1}$ ($\hat{r}_{i,t}$, fundamentals)	1.33%	3.11%	0.0017
$\widehat{HPA}_{i,t+1}$ ($\hat{r}_{i,t}$, fundamentals, demographics)	1.33%	3.10%	0.0016

Notes: Table reports summary statistics for forecast errors using the raw home-price appreciation forecast, perceived past home-price growth, and predicted future home-price appreciation. Predicted values are estimated from a linear regression of realized returns on past returns and the combination of other factors indicated. Demographics 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 a 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. The samples in Panels A and B are 2015 and 2015-2019, respectively.

Table 7: Instrumental Variables Estimates of Investment Decisions

Dependent Variable: Housing Fund Share (on a 0-100 scale)						
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	2SLS	2SLS	OLS	2SLS	2SLS
Forecasted Returns	0.44	-2.40	-1.24*	0.41	-0.86	-0.93
	(0.31)	(1.46)	(0.70)	(0.30)	(1.40)	(0.62)
Perceived Past Returns	1.07***	1.82***	1.51***	0.83***	1.15***	1.17***
	(0.22)	(0.44)	(0.28)	(0.22)	(0.42)	(0.26)
Individual Controls				X	X	X
Instruments		E(Rent) E(Inflation)	Lewbel		E(Rent) E(Inflation)	Lewbel
First Stage F-stat		23.94	466.4		21.66	419.7
Observations	1,012	1,012	1,012	1,012	1,012	1,012

Notes: Table reports OLS and IV estimates of investment decisions on the predicted values of home price forecasts and the perceived past home price growth. The instruments in columns 2 and 5 are forecasted rent growth and forecasted inflation. The Lewbel instrument used in columns 3 and 6 is $(\hat{E}_t[r_{i,t+1}] - \overline{\hat{E}_t[r_{i,t+1}]})^2$ as explained in Appendix C. First-stage coefficients are reported in Appendix Table A11.

Table 8: Evidence for Shrunk Belief Factors

Dependent Variable:	Expected HPA	Housing fund share	
	(1)	(2)	(3)
Forecasted Returns			0.26
			(0.32)
Perceived Past Returns	0.21***	0.81***	0.57**
	(0.030)	(0.21)	(0.22)
Forecasted Rent Growth	0.11***	-0.21	-0.22
	(0.034)	(0.26)	(0.27)
Forecasted Rate of Inflation	0.13***	-0.32	-0.34
	(0.036)	(0.27)	(0.28)
Probabilities Cubic			X
Individual Controls	X	X	X
Observations	1,012	1,012	1,012
R-Squared	0.263	0.174	0.190

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 9: Summary Statistics for 2020 Survey Experiment

	Control	Treatment	Equal Means (p-value)	Within Treatment: Select $\hat{E}_t[r_{i,t+1}]$	Within Treatment: Select $\hat{r}_{i,t}$
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 HPA 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 $\hat{E}_t[r_{i,t+1}]$ 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 zip code level home price appreciation between March 2019 and February 2020 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 $\hat{E}_t[r_{i,t+1}]$ for stocks is an indicator for whether a respondent reported that she relies more on her own stated expected 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 10: Effect of Self-Reflection on Investment Decisions

Dependent Variable: Housing Fund Share (on a 0-100 scale)				
	(1)	(2)	(3)	(4)
Forecasted Returns	1.46***	1.39**	1.21**	1.17**
	(0.56)	(0.55)	(0.59)	(0.60)
Perceived Past Returns	0.98***	0.82**	0.96***	0.80**
	(0.37)	(0.38)	(0.36)	(0.37)
Forecasted Returns × Treated	-1.47**	-1.40**	-1.35*	-1.30*
	(0.71)	(0.68)	(0.74)	(0.72)
Perceived Past Returns × Treated	0.49	0.57	0.38	0.44
	(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 Fore. Ret. = 0 for Treated	0.90	0.99	0.76	0.77
p-value for Past Ret. = 0 for Treated	0.0001	0.0002	0.0004	0.0009
Distribution of Forecasted 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.

Appendix

A Sufficient and Necessary Condition for an Independent Effect of Past Experience

In this Appendix, we derive the sufficient and necessary condition for an independent effect of past experience in Section 2. We relax the assumption in Section 2 that σ_p is constant between the expectation to the investment decision stages. That is, we assume that $\sigma_{p,i} > \sigma_{p,e}$ and $\sigma_{s,i} > \sigma_{s,e}$. The model and coefficients are as follows

$$\begin{aligned}\phi &\approx \tilde{c} + r_i + \gamma_\alpha \alpha + \gamma_\sigma \sigma_i^2 \\ &= c_1 + \beta_{1,i} r_t + \beta_{2,i} s + \gamma_\alpha \alpha + \gamma_\sigma \sigma_i^2 \\ &= c_2 + \left(\beta_{1,i} - \beta_{1,e}\frac{\beta_{2,i}}{\beta_{2,e}}\right) r_t + \frac{\beta_{2,i}}{\beta_{2,e}} r_e + \gamma_\alpha \alpha + \gamma_\sigma \sigma_i^2. \end{aligned} \tag{12}$$

$$\beta_{1,e} = \frac{\sigma_{s,e}^2(\mu_d^2 + \sigma^2)}{(\sigma_{s,e}^2 + \sigma_{p,e}^2)(\mu_d^2 + \sigma^2) + \sigma_{p,e}^2 \sigma_{s,e}^2}$$

$$\beta_{2,e} = \frac{\sigma_{p,e}^2(\mu_d^2 + \sigma^2)}{(\sigma_{s,e}^2 + \sigma_{p,e}^2)(\mu_d^2 + \sigma^2) + \sigma_{p,e}^2 \sigma_{s,e}^2}$$

$$\beta_{1,i} = \frac{\sigma_{s,i}^2(\mu_d^2 + \sigma^2)}{(\sigma_{s,i}^2 + \sigma_{p,i}^2)(\mu_d^2 + \sigma^2) + \sigma_{p,i}^2 \sigma_{s,i}^2}$$

$$\beta_{2,i} = \frac{\sigma_{p,i}^2(\mu_d^2 + \sigma^2)}{(\sigma_{s,i}^2 + \sigma_{p,i}^2)(\mu_d^2 + \sigma^2) + \sigma_{p,i}^2 \sigma_{s,i}^2}$$

The coefficient for r_t in equation (12), $\beta_{1,i} - \beta_{1,e}\frac{\beta_{2,i}}{\beta_{2,e}}$ can be simplified as

$$\beta_{1,i} - \beta_{1,e}\frac{\beta_{2,i}}{\beta_{2,e}} = (\mu_d^2 + \sigma^2)\frac{\sigma_{s,i}^2 \sigma_{p,e}^2 - \sigma_{s,e}^2 \sigma_{p,i}^2}{\sigma_{p,e}^2[(\sigma_{s,i}^2 + \sigma_{p,i}^2)(\mu_d^2 + \sigma^2) + \sigma_{p,i}^2 \sigma_{s,i}^2]} \tag{13}$$

Finally, we derive a necessary and sufficient condition for $\beta_{1,i} - \beta_{1,e}\frac{\beta_{2,i}}{\beta_{2,e}}$ to be positive. The denominator and the factor $(\mu_d^2 + \sigma^2)$ in equation (13) are both positive so it suffices to check the sign of $\sigma_{s,i}^2 \sigma_{p,e}^2 - \sigma_{s,e}^2 \sigma_{p,i}^2$. The positivity of $\sigma_{s,i}^2 \sigma_{p,e}^2 - \sigma_{s,e}^2 \sigma_{p,i}^2$ is equivalent to $\frac{\sigma_{s,i}^2}{\sigma_{s,e}^2} > \frac{\sigma_{p,i}^2}{\sigma_{p,e}^2}$. Thus, the coefficient for r_t in the investment stage is positive if and only if σ_s increases relatively more than σ_p between the expectation and investment decision stages.

B Derivation for the Second Stage Coefficient for $\hat{r}_{i,t}$

In this Appendix, we formally derive $\hat{\beta}_2$ in (10). To simplify our derivations, we assume that the empirical variance-covariance matrix is the same as the population one. We have

$$\hat{\beta}_2 = \frac{Cov(\widetilde{r}_{i,t}, Y_{i,t})}{Var(\widetilde{r}_{i,t})}, \tag{14}$$

where $\widetilde{r}_{i,t}$ is the residual from regressing $\hat{r}_{i,t}$ on the other covariate, $\hat{E}_t[r_{i,t+1}]$. For $\hat{E}_t[r_{i,t+1}]$, by the assumption that the empirical variance-covariance matrix is the same as the population matrix, we have $\hat{\pi}_1 = \pi_1$ and $\hat{\pi}_2 = \pi_2$. Therefore

$$\widetilde{r}_{i,t} = \hat{r}_{i,t} - \phi(\pi_1 \hat{r}_{i,t} + \pi_2 Z_{i,t}), \tag{15}$$

where ϕ is the coefficient from regressing $\widehat{E_t[r_{i,t+1}]}$ on $\hat{r}_{i,t}$, i.e.,

$$\begin{aligned}
\phi &= \frac{Cov(\widehat{E_t[r_{i,t+1}]}, \hat{r}_{i,t})}{Var(\widehat{E_t[r_{i,t+1}]})} \\
&= \frac{Cov(\pi_1 \hat{r}_{i,t} + \pi_2 Z_{i,t}, \hat{r}_{i,t})}{Var(\pi_1 \hat{r}_{i,t} + \pi_2 Z_{i,t})} \\
&= \frac{\pi_1 Var(\hat{r}_{i,t}) + \pi_2 Cov(\hat{r}_{i,t}, Z_{i,t})}{\pi_1^2 Var(\hat{r}_{i,t}) + 2\pi_1\pi_2 Cov(\hat{r}_{i,t}, Z_{i,t}) + \pi_2^2 Var(Z_{i,t})}
\end{aligned} \tag{16}$$

Plugging (15) and (16) into (14), we have

$$\begin{aligned}
\hat{\beta}_2 &= \frac{Cov(\widetilde{r}_{i,t}, Y_i)}{Var(\widetilde{r}_{i,t})} \\
&= \frac{\beta_1(\pi_1 Var(\hat{r}_{i,t}) + \pi_2 Cov(Z_{i,t}, \hat{r}_{i,t}))}{Var(\hat{r}_{i,t} - \phi(\pi_1 \hat{r}_{i,t} + \pi_2 Z_{i,t}))} - \frac{\beta_1(\pi_1 Var(\hat{r}_{i,t}) + \pi_2 Cov(\hat{r}_{i,t}, Z_{i,t}))}{Var(\hat{r}_{i,t} - \phi(\pi_1 \hat{r}_{i,t} + \pi_2 Z_{i,t}))} \\
&= 0.
\end{aligned}$$

Thus, the second-stage coefficient for $\hat{r}_{i,t}$ is 0.

C Justification of Lewbel Instrument

For detailed derivations of the Lewbel instrument, we refer to Lewbel (1997). Conceptually, this strategy uses higher-order moments of a mismeasured variable as its instrument since it is correlated with the signal but conditionally uncorrelated with the noise. This appendix presents a concise summary of this approach. Consider the regression model in which a variable Z is measured with uncertainty.

$$
\begin{aligned}
Y_i &= \alpha + \beta_W^T W_i + \beta_Z Z_i + \epsilon_i \\
Z_i &= \gamma + \Lambda X_i + \upsilon_i
\end{aligned}
$$

Construct an instrument q for Z of the following form with appropriate residuals defined below, and lowercase variables representing deviation from the mean:

$$
\begin{aligned}
q_i &= (Z_i - \overline{Z})^2 = z_i^2 = (x_i + \upsilon_i)^2 \\
Z_i &= \alpha_1 + \beta_{W,1}^T W_i + \tilde{Z}_i \\
X_i &= \alpha_2 + \beta_{W,2}^T W_i + \tilde{X}_i \\
\tilde{Z}_i &= \tilde{X}_i + \upsilon_i
\end{aligned}
$$

There are a set of four assumptions that need to be made for this instrument to be of use

1. $E((1, W^T, X)^T(1, W^T, X))$ exists and is nonsingular
2. $E(\epsilon) = E(\upsilon) = 0$ and $E(\upsilon^3) = 0$
3. $E(x^\psi \upsilon^\lambda \epsilon^k) = E(x^\psi)E(\upsilon^\lambda)E(\epsilon^k)$ for $\psi, \lambda \in \{0, 1, 2\}, k \in \{0, 1\}$
4. $E(x^2 \tilde{X}) \neq 0$

The instrument will lead to a consistent two staged least square estimate if $E(q\epsilon) = E(q\upsilon) = 0$ and $E(q\tilde{Z}) \neq 0$. The first results in $E((x_i + \upsilon_i)^2 \epsilon_i) = E((x_i + \upsilon_i)^2)E(\epsilon_i)$ by assumption 3. The second results in $E((x_i + \upsilon_i)^2 \upsilon_i) = E(\upsilon_i x_i^2 + 2x_i \upsilon_i^2 + \upsilon_i^3) = 0$ by assumption 2 and $E(x_i) = 0$ by definition. The third results in $E((x_i + \upsilon_i)^2(\tilde{X}_i + \upsilon_i))$ which satisfies its inequality similar to the previous two due to assumptions 3 and 4. Therefore, the instrument leads to a consistent estimate.

The instrument has the additional requirement that the measurement error is symmetrically distributed, which is plausible in our context because all else being equal, stated forecasts tend to be equally likely to be lower or higher than their true conception.

Table A1: Addressing Collinearity Between Expected and Perceived Past HPA

Dependent Variable: Housing Fund Share (on a 0-100 scale)						
	(1)	(2)	(3)	(4)	(5)	(6)
Forecasted Returns	0.21	0.28	0.36			
	(0.31)	(0.32)	(0.32)			
Perceived Past Returns				0.62***	0.62***	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
Bin Probabilities Cubic	X	X	X	X	X	X
Demographics	X	X	X	X	X	X
Observations	1,012	1,010	1,006	1,012	1,012	1,008
R-Squared	0.171	0.207	0.233	0.162	0.177	0.195

Notes: Columns 1-3 divide our observations into 10, 100, and 200 equally sized bins according to their perceived past HPA and control for bin fixed effects. Columns 4-6 similarly control for bin fixed effects for forecasted returns in a similar way. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table A2: Investment Decision Factors Using Representative Weights

Dependent Variable: Housing Fund Share (on a 0-100 scale)						
	(1)	(2)	(3)	(4)	(5)	(6)
Forecasted Returns	0.54		-0.013	0.46		0.10
	(0.35)		(0.36)	(0.30)		(0.32)
Perceived Past Returns		1.05***	1.05***		0.78***	0.76***
		(0.24)	(0.25)		(0.23)	(0.25)
Confident in Past Returns				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 A3: Investment Decision Factors for Incentivized Subsample

Dependent Variable: Housing Fund Share (Incentivized Stage)			
	(1)	(2)	(3)
Forecasted Returns	0.15	0.28	0.12
	(0.60)	(0.60)	(0.59)
Perceived Past Returns	0.91**	0.91**	0.85**
	(0.38)	(0.39)	(0.39)
Actual Past Returns			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)
Forecasted Returns	0.50*	0.26
	(0.30)	(0.32)
Perceived Past Returns	0.70***	0.57**
	(0.22)	(0.22)
Forecasted Fundamentals	X	X
Probabilities Cubic		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: Housing Fund Share		
	(1)	(2)
Forecasted Returns	0.335	0.113
	(0.301)	(0.312)
Actual Past Returns	0.318	0.253
	(0.202)	(0.203)
Perceived Past Returns		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 Cubic	X	X
Individual Controls	X	X
Observations	1,012	1,012
R-Squared	0.144	0.151

Notes: Variable units are in percentage points (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, Economic Characteristics

Dependent Variable: Housing Fund Share (on a 0-100 scale)						
	Owner	Renter	College	Non-Coll	Inc>$75K	Inc≤$75K
	(1)	(2)	(3)	(4)	(5)	(6)
Forecasted Returns	0.31	-0.49	0.38	-0.037	0.33	0.053
	(0.39)	(0.56)	(0.52)	(0.40)	(0.59)	(0.37)
Perceived Past Returns	0.95***	0.14	0.87**	0.75***	0.71*	0.68**
	(0.27)	(0.43)	(0.37)	(0.29)	(0.41)	(0.28)
Pr(HPA$<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

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: Heterogeneity in Investment Decision-Making, Other Characteristics

Dependent Variable: Housing Fund Share (on a 0-100 scale)	Age$\geq$50	Age$<$50	Male	Female	High Numeracy	Low Numeracy	Checked Website	Didn't Check
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forecasted Ret.	0.47	0.16	0.20	-0.13	0.41	0.19	0.72*	-0.59
	(0.46)	(0.44)	(0.49)	(0.38)	(0.41)	(0.53)	(0.40)	(0.44)
Perceived Past Ret.	0.13	1.11***	0.77**	0.76**	0.87***	0.58	0.33	1.25***
	(0.32)	(0.31)	(0.31)	(0.31)	(0.28)	(0.37)	(0.31)	(0.34)
Pr(HPA$<0\%$)	-0.063	-0.13	-0.17**	-0.045	-0.14**	0.074	-0.099	-0.086
	(0.072)	(0.076)	(0.078)	(0.071)	(0.064)	(0.10)	(0.071)	(0.078)
Pr(HPA$<-5\%$)	-0.13	-0.040	-0.032	-0.12	-0.0019	-0.34**	-0.20	0.041
	(0.13)	(0.15)	(0.15)	(0.12)	(0.13)	(0.14)	(0.14)	(0.14)
Pr(HPA$>10\%$)	0.039	0.034	0.0068	0.057	-0.097	0.22**	0.012	0.091
	(0.12)	(0.095)	(0.11)	(0.093)	(0.090)	(0.11)	(0.091)	(0.12)
Individual Controls	X	X	X	X	X	X	X	X
Observations	478	534	551	461	746	266	628	384
R-Squared	0.202	0.143	0.149	0.147	0.150	0.259	0.140	0.150

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 A8: 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)
Forecasted Returns	0.12***	0.19**	-0.53**	-0.24	0.20***	0.12***
	(0.029)	(0.052)	(0.18)	(0.15)	(0.044)	(0.030)
Perceived Past Returns	0.092*	0.073*	0.11	0.046	0.20***	0.15***
	(0.041)	(0.030)	(0.15)	(0.097)	(0.013)	(0.016)
Pr(HPA next year $< 0\%$)		0.0051		-0.033		-0.027***
		(0.011)		(0.035)		(0.0032)
Pr(HPA next year $< -5\%$)		0.068***		-0.064		-0.015
		(0.014)		(0.038)		(0.015)
Pr(HPA next year $> 10\%$)		-0.022		-0.061		0.0030
		(0.015)		(0.069)		(0.0091)
Owns Home		2.12**		22.7***		0.12
		(0.74)		(0.57)		(0.52)
Confident in past price projections		2.09***		3.37**		1.62***
		(0.40)		(0.98)		(0.29)
Above-median Risk Aversion		-5.45***		-2.59**		-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 A9: Effect of Information Treatment on Decision-Making

Dependent Variable:	Baseline Stage Housing Fund Share		Treatment Stage Housing Fund Share	
	(1)	(2)	(3)	(4)
Baseline Forecasted Returns	0.86	0.43		
	(0.53)	(0.52)		
Baseline Perceived Past Returns	0.58	0.41		
	(0.39)	(0.39)		
Treatment Stage Forecasted Returns			1.24**	1.05**
			(0.49)	(0.50)
Actual HPA			1.40***	1.46***
			(0.41)	(0.42)
Individual Controls	X	X	X	X
Baseline Probabilities		X		X
Observations	340	340	340	340
R-Squared	0.174	0.188	0.218	0.226

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 A10: 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)
Forecasted Returns	0.32	0.31	0.37	0.34	0.32	0.31
	(0.39)	(0.39)	(0.43)	(0.43)	(0.39)	(0.39)
Perceived Past Returns	0.98***	0.94***	0.91***	0.85***	1.29***	1.21***
	(0.28)	(0.28)	(0.31)	(0.31)	(0.39)	(0.39)
Perceived Past Returns	-0.014	-0.023				
× (Home Value/Equity)	(0.019)	(0.019)				
Perceived Past Returns			0.002	-0.005		
× (Home Value/Net Assets)			(0.062)	(0.058)		
Perceived Past Returns					-0.052	-0.051
× (Home Value/Income)					(0.063)	(0.063)
Risk Tolerance (1-10)	2.37***		2.38***		2.18***	
	(0.64)		(0.68)		(0.63)	
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	0.154	0.168	0.135	0.149	0.166	0.177

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 A11: First Stage Estimates of Expected Returns

Dependent Variable: 1-year HPA Expectation				
	(1)	(2)	(3)	(4)
Perceived Past Returns	0.24***	0.21***	0.21***	0.16***
	(0.03)	(0.02)	(0.03)	(0.02)
Forecasted Rent Growth	0.11***		0.11***	
	(0.03)		(0.03)	
Forecasted Inflation	0.11***		0.13***	
	(0.03)		(0.03)	
Lewbel Instrument		0.06***		0.06***
		(0.00)		(0.00)
Individual Controls			X	X
F-stat	23.94	466.8	17.77	420
Observations	1,012	1,012	1,012	1,012

Notes: The Lewbel instrument is $(\hat{E}_t[r_{i,t+1}] - \overline{\hat{E}_t[r_{i,t+1}]})^2$ as explained in Appendix C. For definitions of individual controls, see notes to Table 7. Robust standard errors in parentheses. Significant at *p<0.10, **p<0.05, ***p<0.01.

Table A12: Characteristics of Respondents Choosing Forecasted Returns Over Subjective Past Returns

Dependent Variable: Choose Forecasted Return Over Past Return		
	Housing Question	Stock Question
	(1)	(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 A13: 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)
Forecasted Returns	1.46***	1.39**	1.21**	1.17**
	(0.56)	(0.55)	(0.60)	(0.59)
Perceived Past Returns	0.98***	0.84**	0.96***	0.82**
	(0.37)	(0.37)	(0.37)	(0.37)
Forecasted Returns	-1.45**	-1.44**	-1.31	-1.27
× Considering Future	(0.74)	(0.73)	(0.80)	(0.78)
Perceived Past Returns	-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)
Forecasted Returns	-1.69*	-1.55*	-1.61*	-1.47*
× Considering Past	(0.90)	(0.87)	(0.90)	(0.85)
Perceived Past Returns	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 Fore. Ret. for Forward Looking = 0	0.9827	0.9148	0.8496	0.8462
p-value for Fore. Ret. for Backward Looking = 0	0.7446	0.8090	0.5483	0.6233
p-value for Past Ret. for Forward Looking = 0	0.0661	0.0260	0.1253	0.0480
p-value for Past Ret. for Backward Looking = 0	0.0004	0.0054	0.0004	0.0069
Controls for Distribution of Forecasted 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.