



Do survey expectations of stock returns reflect risk adjustments?[☆]



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ABSTRACT

To reconcile the disconnect between survey expectations of stock returns and rational expectations, researchers have hypothesized that survey participants may confound beliefs and preferences by (i) reporting risk-neutral forecasts of future returns; or (ii) reporting pessimistically-tilted forecasts reflecting ambiguity aversion or robustness concerns. We find that these hypotheses are strongly rejected by the data, albeit for different reasons: Inconsistent with hypothesis (i), survey return forecasts are reliably much higher than risk-free interest rates and survey expected excess returns are predictably time-varying. Inconsistent with (ii), agents are not always pessimistic about future returns, but often predictably optimistic and unconditionally unbiased.

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1. Introduction

Expectations play an important role in macroeconomics and asset pricing. The predominant modeling approach in these fields is to impose the assumption of rational expectations, which equates the subjective probability distribution perceived by the agents within the model with the objective probability distribution perceived by an outside observer equipped with a large sample of data generated by the model. The rational expectations approach is elegant and internally consistent, and it eliminates the need to empirically study the formation of subjective expectations—but the assumption about expectations underlying it could be false. Recognizing this, Manski (2004) calls on researchers to collect survey data on expectations. Measurement of expectations allows researchers to consider alternatives to rational expectations assumption in an empirically disciplined way.

A growing body of research in asset pricing follows this approach by examining survey data on investor stock market return expectations. This literature finds that the time-series dynamics of investor return expectations in surveys are in conflict

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with the predictions of influential rational expectations asset pricing theories. Models like (Campbell and Cochrane, 1999) generate volatile asset prices and predictable returns by making risk premia countercyclical. By virtue of the rational expectations assumption, the subjective beliefs of investors in these models agree with the objective distribution, and hence these theories predict that the representative investor perceives countercyclical expected returns. In contrast, the survey evidence in Vissing-Jorgensen (2003), Bacchetta et al. (2009), Greenwood and Shleifer (2014) and Adam et al. (2017) suggests that investor return expectations are procyclical: subjective expected returns are higher following high realized stock market returns and in times of high price-dividend ratios.

In these studies of return expectations, and more generally in much of the literature using survey measures of expectations, reviewed in Manski (2017) and Coibion et al. (2018a), researchers interpret the elicited survey expectations as a representation of subjective probability beliefs that are distinct from respondents' preferences. More specifically, the typical interpretation assumes that people do not confound the probability of a state of the world with the desirability of this state when they answer survey questions about subjective beliefs.

Whether this assumption of unconfoundedness in survey expectations is empirically correct is an open question. Manski (2017) reviews several examples from the literature in which individuals appear to overestimate the probability of extremely bad outcomes such as death or being a crime victim. These examples are suggestive of confounding of beliefs and preferences, but it is not clear whether similar confounding effects arise when individuals report expectations about asset returns and macroeconomic outcomes. If such confounding effects existed, this would have important consequences for modeling and use of survey data in finance and macroeconomics. In this paper, we provide empirical evidence on this question based on stock return survey expectations data.

At a generic level, the confounding hypothesis is not testable without taking a stand on a more specific model of preference-based distortions in reported expectations. We focus on two hypotheses that have attracted interest in the recent literature. They share the notion that survey expectations incorporate a risk-adjustment that gives more weight to states of high marginal utility. These states are, in the case of stock return expectations, states of low stock returns.¹ As a consequence, the risk-adjustment makes expectations, on average, pessimistically biased relative to unconfounded expectations. Stock return expectations are a good setting to test for the presence of such distortions because in this case it is clear in which direction a pessimism distortion would bias expectations. In the case of inflation expectations, for example, the focus of much work on macro expectations, it is less clear in which direction a risk-adjustment would bias expectations.

Our first set of tests focus on the hypothesis, proposed in Cochrane (2011, 2017), that survey participants report risk-neutral expectations. Under risk-neutral expectations, the outcomes are weighted by their physical probabilities multiplied by the marginal utility associated with the respective outcome (and rescaled so that the weights sum to one). Cochrane (2017) conjectures that since these "risk-neutral probabilities are a good sufficient statistic to make decisions" (p. 967), it is plausible that survey respondents report risk-neutral expectations.

Return expectations are an excellent setting for studying this hypothesis because asset pricing theory provides a sharp prediction: risk-neutral expectations of returns on traded assets should equal the risk-free rate. For instance, if a survey respondent is asked to state the expected rate of return on a diversified portfolio of stocks over the next 12 months, the risk-neutral expectations hypothesis predicts that the respondent should reply with the 12-month risk-free rate as their expected rate of return. This prediction holds under very general conditions. We can allow for differences in opinion, heterogeneous preferences, and biased subjective beliefs and we still obtain that risk-neutral expected returns equal the risk-free rate. In the absence of trading frictions, heterogeneous individuals should adjust the risk profile of their portfolio and their borrowing and lending such that their future time-discounted expected marginal utilities align with the current risk-free rate. As a consequence, their risk-neutral expected rates of return are all equal to the risk-free rate.

Our second set of tests looks for risk-adjustments that may be less extreme than under the risk-neutral expectations hypothesis. For example, risk aversion could influence survey expectations, but without pushing them all the way down to the risk-free rate. Relatedly, ambiguity aversion or model robustness concerns can be represented with a distorted probability measure that overweights bad outcomes (Hansen and Sargent, 2001). If survey respondents report their beliefs under this distorted probability measure, their expectations will be pessimistically biased relative to an unconfounded expectation. A number of researchers have recently used macroeconomic survey expectations under the assumption that they reflect such distorted probabilities (see, e.g., Baqaee, 2019; Bhandari et al., 2019; Szöke, 2019).²

The predictions of this pessimism hypothesis are not as sharp as those of the risk-neutral expectations hypothesis. To derive predictions, we need to make some assumptions about the unobserved unconfounded expectation. We take rational expectations as the unconfounded benchmark that we compare the observed survey expectations with in order to estimate whether there is a pessimism bias.

We use several different surveys of individual investors, professional investors, professional economists, and chief financial officers (CFOs) covering various sample periods from the 1980s until recently. The implication of the risk-neutral expectations hypothesis that expected returns equal the risk-free rate is strongly rejected. Unconditionally, survey expectations of stock market returns exceed the risk-free rate, as proxied by Treasury yields, by 1–5 percentage points depending

¹ This follows from the fact that there is a positive equity premium.

² The theory underlying these studies could also accommodate ambiguity- or fragility-loving agents, resulting in an optimistic bias. Our empirical work is not meant to test these theories, but rather to answer the question whether a pessimism distortions appears in return expectations.

on the survey and sample period, and the difference is highly statistically significant. Conditionally, the risk-neutral expectations hypothesis predicts that deviations of survey expected returns from the risk-free rate should be random measurement errors that are unrelated to cyclical variables. However, we find that for almost all surveys and forecast horizons, the stock market's price-dividend (P/D) ratio predicts the direction of the deviation. Specifically, for individual investors and CFOs, the deviation is procyclical: expected stock returns exceed the risk-free rate by more when the P/D ratio is high.

Overall, the empirical evidence strongly rejects the hypothesis that survey respondents report a risk-neutral expectation. This is a rejection in a very general sense, allowing for differences in opinion, heterogeneous preferences, and biased subjective beliefs. The rejection in our tests thus implies that there exists no internally consistent probability measure that can reconcile the observed survey data with the risk-neutral expectations hypothesis in a frictionless setting.

These results also hold up if we replace Treasury yields with variable mortgage rates as an alternative risk-free rate proxy. To the extent that there is a friction-induced wedge between borrowing and lending rates, a collateralized borrowing rate may be a better proxy for the risk-neutral expected return than a Treasury rate. However, using this alternative risk-free rate proxy does not materially change the results.

We then turn to our second set of tests where we empirically investigate whether survey expectations of returns are pessimistically biased relative to rational return expectations, for example due to ambiguity aversion or robustness-seeking. To test this prediction, we compare survey expected returns to realized rates of return. Unconditionally, we find that the expected stock market returns reported in surveys are approximately unbiased as forecasts of realized returns. Conditionally, taking into account predictable variation in the wedge between survey expected returns and realized returns, we find big deviations from the rational expectations benchmark. However, investors are roughly as many times optimistic as they are pessimistic. These findings are inconsistent with the idea that survey expectations have a pessimistic bias due to ambiguity aversion or robustness concerns.

For comparison, we also look at the properties of forecast errors for GDP, unemployment, interest rates and inflation from the Survey of Professional Forecasters. For GDP and unemployment, it is clear which tail of the distribution represents bad outcomes, while for the other variables this is more questionable. Similar to the return expectations data, we do not find evidence of a systematic pessimism bias. The absence of such a bias is thus not a unique feature of stock return expectations.

Our finding that survey expectations of returns are unconditionally approximately unbiased also speaks to a number of alternative theories of confounding of beliefs and preferences in addition to those that we have emphasized. For example, the approximate unbiasedness indicates that the individuals and professionals in our survey data do not forecast under an asymmetric loss function, e.g. as in [Capistrán and Timmermann \(2009\)](#). The evidence from survey expectations of returns is also inconsistent with the optimal expectations framework in [Brunnermeier and Parker \(2005\)](#) which would predict a bias towards optimism in stock return expectations.

In summary, we do not find evidence of marginal-utility weighting or a distortion towards overweighting of bad outcomes in survey expectations of returns. That said, we can of course only rule out specific hypotheses of how preferences could confound beliefs. If one could assign arbitrary time-varying weights to different states, one could always match any desired time-series properties of survey expectations. But it is not economically meaningful to assign arbitrary weights. If the weights are to represent the confounding effect of preferences, the implied preferences should be plausible in terms of the asset pricing predictions. For this reason, we have focused on a class of distortions that take the form of risk-adjustments, reflecting risk aversion or ambiguity aversion, that can be consistent with empirically observed risk premia and asset prices.

With these risk-adjustments towards overweighting bad outcomes, it appears impossible to simultaneously match asset price dynamics and the survey evidence under rational expectations. The fact that the (procyclical) empirical time-series dynamics of expected returns reported in surveys differ starkly from the (countercyclical) predictions of leading rational expectations models therefore cannot be explained away by positing that individuals report risk-adjusted expectations. To reconcile survey expectations with asset prices, some departure from rational expectations appears to be necessary, such as, for example, extrapolative expectations ([Barberis et al., 2015](#)), learning about underlying trends in price growth ([Adam et al., 2017; 2016](#)), or learning from life-time experience ([Collin-Dufresne et al., 2016; Nagel and Xu, 2019](#)), building on [Malmendier and Nagel \(2011, 2015\)](#).

Our work also connects to an earlier literature that documents forecast biases for a range of variables, including forecasts of stock market analysts ([DeBondt and Thaler \(1990\)](#)), professional forecasters ([Giordani and Söderlind \(2006\)](#)), households ([Carroll, 2003](#)) and firms ([Coibion et al., 2018b](#)). The literature has also recently started to look into the connection between return expectations and portfolio choice ([Ameriks et al., 2019; Giglio et al., 2019](#)). What distinguishes our analysis is that we seek to understand whether marginal-utility weighting generates biases in return forecasts.

The remainder of the paper is structured as follows. In [Section 2](#), we derive testable implications of the risk-neutral expectations and pessimistic expectations hypotheses. After describing the data sources in [Section 3](#), we present the main empirical results in [Section 4](#). [Section 5](#) investigates how unconditional and conditional biases in investors' return expectations depend on investor experience and wealth. This section also considers the biases in forecasts of GDP and unemployment. [Section 6](#) concludes.

2. Hypotheses about survey expectations

Let R_{t+1} denote a one-period return on a stock market index realized over the period t to $t + 1$ and let R_t^f denote the one-period return offered by a risk-free asset over the same period. As a benchmark, we consider an investor i who can freely

trade in both instruments. The effects of trading constraints will be discussed below. The investor's first-order conditions imply that the returns must satisfy

$$1 = E_t^{\mathcal{P}^i} [M_{t+1}^i R_{t+1}] \quad (1)$$

$$1 = E_t^{\mathcal{P}^i} [M_{t+1}^i] R_t^f, \quad (2)$$

where M_{t+1}^i is the agents' one-period stochastic discount factor (SDF) from t to $t+1$ and $E_t^{\mathcal{P}^i}$ is an expectations operator that is based on some (potentially subjective) probability measure \mathcal{P}^i . Specific asset pricing theories give rise to specific forms of M^i or make specific assumptions about \mathcal{P}^i , but we shall not be concerned with this here: the testable implications derived below will rely exclusively on Eqs. (1) and (2) being satisfied for some M^i and some probability measure \mathcal{P}^i . In fact, we can even allow M^i and \mathcal{P}^i to differ across investors.

Eqs. (1) and (2) assume that agents can (at the margin) freely trade in the stock and in the bond market. This is in line with the assumptions made in a wide range of representative agent asset pricing models, such as those in the tradition of Campbell and Cochrane (1999) or Bansal and Yaron (2004). Limited participation models, such as the one considered by Guvenen (2009), postulate that some agents trade only in the bond market but have no access to the stock market, while others have access to both markets. For agents without access to the stock market, Eq. (1) will not necessarily be satisfied. For agents with access to both markets, Eqs. (1) and (2) both hold. To prevent our empirical results from being tainted by limited stock market access, we shall consider below mainly survey sources for which we know that survey respondents do have stock market access. While this should alleviate most concerns, we cannot rule out that portfolio adjustment costs may still cause some deviations from Eqs. (1) and (2), although these deviations should arguably be small.

2.1. Risk-neutral expectations

Let $\mathcal{E}_t^i[\cdot]$ denote the expectations of individual i measured in a survey. The risk-neutral expectations hypothesis postulates that survey expectations are risk-neutral forecasts, i.e.,

$$\mathcal{E}_t^i[R_{t+1}] = E_t^{\mathcal{P}^i} \left[\frac{M_{t+1}^i}{E_t^{\mathcal{P}^i} [M_{t+1}^i]} R_{t+1} \right] + \varepsilon_t^i, \quad (3)$$

where the term pre-multiplying $R_{t,t+1}$ inside the expectations operator is a Radon–Nikodym term that transforms the “physical” probability of future states, which enter the computation of the expectation $E_t^{\mathcal{P}^i}[\cdot]$, into a “risk-neutral” or “marginal-utility weighted” probability.³

The subjective expectation $E_t^{\mathcal{P}^i}[\cdot]$ can differ from the objective expectation, which we denote $E_t[\cdot]$. The measurement error ε_t^i captures the fact that we empirically measure expectations with noise.

Note that the risk-neutral hypothesis formulated in Cochrane (2011, 2017) is stronger than what we stated in Eq. (3), as he additionally postulates that \mathcal{P}^i is a “rational” or “objective” probability measure. This additional constraint, however, turns out not to be relevant for the arguments that follow, prompting us to proceed with the more general case in which agents are allowed to have objective or subjective beliefs.

Eq. (3) implies that future returns that materialize in states in which marginal utility and thus the SDF is high (low) are treated by agents as if they are more (less) likely than under the objective measure and thus lead to an upward (downward) “distortion” of the expected returns relative to plain return expectations. Under the risk-neutral expectations hypothesis, survey expectations can thus look “distorted,” if one wrongly interprets them as a forecast under real-world probabilities. Therefore, the risk-neutral expectations hypothesis could potentially help reconcile rational expectations asset pricing theories with the survey evidence.

Eq. (3), together with Eqs. (1) and (2), implies

$$\mathcal{E}_t^i[R_{t+1}] = R_t^f + \varepsilon_t^i, \quad (4)$$

which shows that under the risk-neutral expectations hypothesis survey expectations of future stock returns must equal—up to a measurement error—the risk-free interest rate. This is intuitive, as under the risk-neutral measure, all subjectively expected returns are identical and equal to the risk-free interest rate. This implication of the risk-neutral return hypothesis

³ Let s_t denote the state in t and $p^i(s_{t+1}|s_t)$ the physical probability (implied by \mathcal{P}^i) of transitioning from s_t to state s_{t+1} in $t+1$. The risk-neutral probability $n^i(s_{t+1}|s_t)$ of reaching state s_{t+1} given state s_t is then

$$n^i(s_{t+1}|s_t) \equiv p^i(s_{t+1}|s_t) \frac{M^i(s_{t+1}|s_t)}{E_t^{\mathcal{P}^i} [M^i(s_{t+1}|s_t)]},$$

so that under the risk-neutral expectations hypothesis, we get

$$\mathcal{E}_t^i[R_{t+1}] = E_t^{n^i} [R_{t+1}] + \varepsilon_t^i,$$

where $E_t^{n^i}[\cdot]$ is the expectations operator that integrates over states using the probabilities $n^i(s_{t+1}|s_t)$.

can be tested empirically. Specifically, since it is possible to interpret returns as nominal returns and the SDF as a nominal discount factor, one can test Eq. (4) directly using nominal return expectations from surveys and nominal risk-free interest rates. A failure of Eq. (4) to hold will thereby imply that there exists no (objective or subjective) probability measure \mathcal{P}^i that is consistent with the risk-neutral return hypothesis.

Let $\mathcal{E}_t[R_{t+1}]$ denote the mean (or median) of a cross-section of survey return forecasts $\{\mathcal{E}_t^i[R_{t+1}]\}$. Since Eq. (4) holds for every investor, it also holds for the mean (median) of these survey return expectations; i.e.,

$$\mathcal{E}_t[R_{t+1}] - R_t^f = \varepsilon_t, \tag{5}$$

where ε_t is the cross-sectional mean (median) of the individual measurement errors ε_t^i . We assume $E[\varepsilon_t] = 0$, but we allow ε_t to be autocorrelated over time.

Unconditional test. We can then examine the risk-neutral expectations hypothesis by estimating a in

$$\mathcal{E}_t[R_{t+1}] - R_t^f = a + \varepsilon_t. \tag{RN-U} \tag{6}$$

and testing the null hypothesis $H_0 : a = 0$.

Conditional test. In addition to this prediction about unconditional means, the risk-neutral expectations hypothesis also implies a strong prediction about the time-series dynamics: Since the wedge $\mathcal{E}_t[R_{t+1}] - R_t^f$ should be zero except for measurement error, it should not be correlated with any covariates that are uncorrelated with the measurement error. For example, macroeconomic variables or standard market return predictors in asset pricing should not correlate with the wedge. On the other hand, this wedge would be strongly correlated with such variables under interesting alternative hypotheses. For example, if respondents report rational expectations without risk adjustment, the wedge in reported expectations should be equal to the conditional market risk premium (absent measurement error). Any covariates x_t that are correlated with wedge $\mathcal{E}_t[R_{t+1}] - R_t^f$ should predict $R_{t+1} - R_t^f$ with the same sign. We therefore consider the specification

$$\mathcal{E}_t[R_{t+1}] - R_t^f = a_0 + a_1'x_t + \varepsilon_t. \tag{RN-C} \tag{7}$$

where the vector x_t includes predictor variables that could, for example, capture variation in $E_t[R_{t+1}] - R_t^f$. The risk-neutral expectations hypothesis implies $H_0 : a_0 = 0 \wedge a_1 = 0$.

2.2. Pessimistic expectations

A less extreme hypothesis than the risk-neutral expectations hypothesis is one in which investor expectations are pessimistically biased, but not all the way down to the risk-free rate. For example, investors that are averse to ambiguity or that are seeking robustness make decisions as if they hold pessimistically biased expectations about asset returns compared with rational expectations. The SDF in these models can be represented as $M_{t+1}^i = A_{t+1}^i Q_{t+1}^i$, where A_{t+1}^i is a conventional marginal utility-based SDF and Q_{t+1}^i , with $E_t[Q_{t+1}^i] = 1$, can be viewed as a belief distortion that overweights bad states of the world.

Whether expectations reported in surveys reflect these belief distortions is an open question. Baqaee (2019), Bhandari et al. (2019) and Szöke (2019), for example, assume so. However, this need not be the case. Even if investor choices and asset prices can be accurately characterized by viewing Q_{t+1}^i as a belief distortion, this does not imply that when investors are asked to state their expectations in a survey, they report the expectations distorted by Q_{t+1}^i . Whether they do so is an empirical question that we investigate here. Resolving this issue is important for interpretation of survey measures of expectations and also for the empirical measurement of ambiguity aversion.

To derive testable predictions, we now require somewhat stronger assumptions than in the case of the risk-neutral expectations hypothesis. Here we assume that expectations unconfounded by preferences would be rational expectations. The belief distortion Q_{t+1}^i then implies

$$\mathcal{E}_t^i[R_{t+1}] = E_t[Q_{t+1}^i R_{t+1}] + \varepsilon_t^i. \tag{8}$$

Since R is the return on a risky asset that systematically offers higher payoffs in good states, its payoff is negatively correlated with Q . Therefore,

$$\mathcal{E}_t^i[R_{t+1}] = E_t[R_{t+1}] + cov(Q_{t+1}^i, R_{t+1}) + \varepsilon_t^i < E_t[R_{t+1}] + \varepsilon_t^i \tag{9}$$

i.e., the belief distortion leads to a pessimistic bias in expectations.⁴

Since (9) holds for every individual, it also holds for the mean (or median)

$$\mathcal{E}_t[R_{t+1}] < E_t[R_{t+1}] + \varepsilon_t \tag{10}$$

where ε_t is the cross-sectional mean (median) of the individual measurement errors ε_t^i .

⁴ To the extent that ambiguity aversion is responsible for much of the equity premium, then $M_{t+1}^i \approx Q_{t+1}^i / R_{f,t}$. If so, we are back to the risk-neutral expectations hypothesis, where $\mathcal{E}_t^i[R_{t+1}] \approx R_t^f$.

Unconditional test. Based on (10), we can then examine the pessimistic expectations hypothesis by estimating the average pessimism bias $b \equiv E[\mathcal{E}_t(R_{t+1}) - E_t(R_{t+1})]$ in

$$\mathcal{E}_t[R_{t+1}] - E_t[R_{t+1}] = b + e_t, \quad (11)$$

where $e_t = \mathcal{E}_t(R_{t+1}) - E_t(R_{t+1}) - b$ is a composite residual with $E[e_t] = 0$ that contains the measurement error ε_t as well as the time-varying part of the beliefs wedge that is not due to ε_t . We allow e_t to be serially correlated. The inequality (10) implies $H_0: b < 0$.

However, $E_t[R_{t+1}]$ is unobservable. One approach is to substitute in $R_{t+1} = E_t[R_{t+1}] + \eta_{t+1}$, which yields

$$\mathcal{E}_t[R_{t+1}] - R_{t+1} = b + e_t - \eta_{t+1} \quad (\text{PE-U1}) \quad (12)$$

A potential problem with this approach is that average realized returns can be substantially different from rational conditional expected returns over extended periods of time. Since some of our survey series are quite short, this could be a serious problem. For example, the 1990s were a period in which return predictions from forecasting regressions based on the dividend yield (which indicated low expected returns) differed substantially from (high) average realized returns. If survey expectations are on average below realized returns during this period, this may not be an indication of pessimistic beliefs but instead reflect sampling error. Put differently, since even somewhat precise estimation of expected returns requires very long sample periods, just replacing $E_t[R_{t+1}]$ with realized returns could be very inefficient.

An alternative and likely more efficient approach is to substitute the expected returns $E_t[R_{t+1}]$ in (11) by the fitted value, $\hat{E}_t[R_{t+1}]$, from the predictive regression

$$R_{t+1} = k_0 + k'_1 z_t + u_t, \quad (13)$$

where z_t includes commonly used return predictors—e.g., the dividend yield—and u_t is a potentially serially correlated residual. The fitted value is also a noisy estimate of the conditional expected returns, but it should be substantially more precise than the average realized return over a relatively short time. In particular, to yield more precise estimates, the first-stage regression to generate $\hat{E}_t[R_{t+1}]$ could be run on a sample that is much longer than the time series of survey expectations. Such a longer time series helps with reducing small-sample biases in the predictive regression.

For this approach to be valid in terms of consistency, z_t does not necessarily have to be in the information set of survey respondents, as this would not bias the estimate of the unconditional mean wedge b .⁵ Using this approach, we estimate the coefficient b in

$$\mathcal{E}_t[R_{t+1}] - \hat{E}_t[R_{t+1}] = b + e_t + \omega_t, \quad (\text{PE-U2}) \quad (14)$$

where $\omega_t \equiv E_t[R_{t+1}] - \hat{E}_t[R_{t+1}]$, and test $H_0: b < 0$. Since $\hat{E}_t[R_{t+1}]$ is a generated variable, we need to adjust the standard errors accordingly. The asymptotic distribution is provided in Online Appendix C.

Conditional test. Pessimism due to ambiguity aversion would not only imply pessimism relative to rational expectations on average, but also conditionally, period by period. As already indicated in (11), the expectations wedge may be time-varying. Since the inequality (10) holds in every state, it also holds if we condition on a vector of covariates x_t , with $E[\varepsilon_t|x_t] = 0$, so that

$$E\{\mathcal{E}_t[R_{t+1}] - E_t[R_{t+1}]|x_t\} < 0. \quad (15)$$

Assuming that this conditional expectation is approximately linear in x_t , we have

$$\mathcal{E}_t[R_{t+1}] - E_t[R_{t+1}] = b_0 + b'_1 x_t + e_t, \quad (16)$$

where $e_t = \mathcal{E}_t[R_{t+1}] - E_t[R_{t+1}] - E\{\mathcal{E}_t[R_{t+1}] - E_t[R_{t+1}]|x_t\}$ and so $E[e_t|x_t] = 0$.⁶ The pessimism hypothesis implies that $b_0 + b'_1 x_t < 0$. For this linear model to be consistent with this inequality, x_t needs to be suitably bounded. The rational expectations alternative implies $b_0 = 0 \wedge b_1 = 0$.

We can again follow two approaches to deal with the unobservability of $E_t[R_{t+1}]$ in (16). The first approach is to substitute in $R_{t+1} = E_t[R_{t+1}] + \eta_{t+1}$, which yields

$$\mathcal{E}_t[R_{t+1}] - R_{t+1} = b_0 + b'_1 x_t + e_t - \eta_{t+1} \quad (\text{PE-C1}) \quad (17)$$

For $E[\eta_{t+1}|x_t] = 0$ to hold, we require that x_t is in the information set of survey respondents. As before, this approach suffers from the fact that R_{t+1} is an extremely noisy proxy for $E_t[R_{t+1}]$.

The second approach substitutes the fitted value, $\hat{E}_t[R_{t+1}]$, from the predictive regression (13), which yields

$$\mathcal{E}_t[R_{t+1}] - \hat{E}_t[R_{t+1}] = b_0 + b'_1 x_t + e_t + \omega_t \quad (\text{PE-C2}), \quad (18)$$

where $\omega_t = E_t[R_{t+1}] - \hat{E}_t[R_{t+1}]$, as before in the unconditional case above. To have $E[\omega_t|x_t] = 0$, we need x_t to be in the information set of survey respondents and be included in z_t . We show in Online Appendix C how to obtain the asymptotic distribution of the estimator in this case.

⁵ However, if z_t is not in the information set, then statistical power is lost, as any deviation of $\hat{E}_t[R_{t+1}]$ from $E_t[R_{t+1}]$ adds noise.

⁶ At this point, we do not need to assume that x_t is in the time- t information set of survey respondents. If it's not, it simply means we cannot replace $E[E_t[R_{t+1}]|x_t]$ by $E[R_{t+1}|x_t]$. However, for implementation of the estimation in terms of observables below, we will need the assumption that x_t is in the survey respondents' information set.

Table 1

Summary Statistics of Survey Data The table shows summary statistics for the survey datasets we use in this study. We aggregate the individual survey responses in terms of means or medians within monthly or quarterly time periods as shown in the second column. The last two columns show the time series mean and standard deviation of the aggregated mean or median percentage expected return series.

| Survey Source | Aggreg. | Periods | Forecast Horizon | Sample | Avg. # Obs. per Period | Aggreg. | $E[R]$ Mean | $E[R]$ S.D. |
|----------------------|-----------|---------------------------|------------------|--------|------------------------|---------|-------------|-------------|
| CFO | quarter | 2000q3 - 2016q1 | 1yr | | 390 | mean | 5.72 | 1.56 |
| UBS own | month | 1999m2 - 2007m10 | 1yr | all | 702 | mean | 10.31 | 2.68 |
| | | | 1yr | > 100k | 310 | median | 8.28 | 2.31 |
| UBS market | month | 1999m2 - 2003m4 | 1yr | all | 706 | mean | 10.16 | 2.71 |
| | | | | > 100k | 311 | median | 8.55 | 2.29 |
| UBS extended | month | 1972m8 - 2016m2 (w/ gaps) | 1yr | n/a | | mean | 10.76 | 3.16 |
| | | | | | | median | 8.73 | 2.76 |
| Shiller individual | quarter | 1999m1 - 2015m8 | 3m | | 75 | mean | 10.47 | 3.27 |
| | | | | | | median | 8.85 | 2.69 |
| | | | 6m | 77 | mean | 9.46 | 2.23 | |
| | | | | | median | 0.89 | 1.30 | |
| | | | | | mean | 1.34 | 1.23 | |
| 1yr | 81 | mean | 2.13 | 1.60 | | | | |
| | | median | 2.70 | 1.28 | | | | |
| 10yr | 76 | mean | 5.09 | 2.98 | | | | |
| | | median | 5.67 | 1.97 | | | | |
| Shiller professional | quarter | 1999m1 - 2015m8 | 3m | | 60 | mean | 37.05 | 24.19 |
| | | | | | | median | 22.55 | 27.07 |
| | | | 6m | 63 | mean | 0.57 | 1.41 | |
| | | | | | median | 1.37 | 1.25 | |
| | | | 1yr | 69 | mean | 2.00 | 1.92 | |
| median | 3.71 | 1.34 | | | | | | |
| 10yr | 65 | mean | 5.12 | 3.08 | | | | |
| | | median | 7.36 | 2.87 | | | | |
| Livingston | half-year | 1952m6 - 2017m12 | 6-12m | n/a | | mean | 70.74 | 25.84 |
| | | | | | | median | 56.85 | 20.18 |
| | | | | | | mean | 2.75 | 2.86 |

3. Data

We use data on stock market return expectations from several different datasets, which, to the best of our knowledge, cover all available quantitative data on U.S. stock market return expectations of individuals who are not professional forecasters.

These surveys provide respondents' point expectations of stock market returns or stock price changes. A potential alternative would be to work with surveys that elicit quantiles of respondents' subjective distributions. Manski (2017) highlights advantages of probabilistic expectations data, and he surveys research based on such data. For the specific purpose of our paper, however, point expectations are preferable. The risk-neutral expectations hypothesis makes sharp predictions about point expectations of asset returns, but we would not be able to derive testable predictions about quantiles of subjective asset return distributions without additional auxiliary assumptions about the functional form of marginal utility.

3.1. Survey data sources

Table 1 provides summary statistics for the different survey datasets. The first dataset is the Duke CFO Global Business Outlook, a quarterly survey conducted by Duke University's Fuqua School of Business and CFO magazine. As Table 1 shows, the sample contains about 400 observations per quarter. Respondents in the survey provide the rate of return they expect on the S&P 500 index over the next year. We obtain the median and mean responses from this survey.

The second dataset is the UBS/Gallup survey.⁷ The survey is based on a nationally representative sample. But to participate in the survey, respondents need to hold stocks, bonds, or mutual funds of a combined value of at least \$10,000. We use data from February 1999 onwards, when the survey was conducted on a regular monthly basis until 2007, with about 700 observations per month. We also observe whether the respondent household holds more than \$100,000 in stocks, bonds, or mutual funds. As Table 1 shows, this subsample of wealthy households accounts for somewhat less than half of the sample. We use data from two survey questions about expected returns. The first expectations question asks about the return that

⁷ The archive is available at <http://ropercenter.cornell.edu/ubs-index-investor-optimism/>.

the respondent expects from an investment in the stock market during the next 12 months. This question is available until April 2003. The second expectations question asks for the return that the respondents expect on their own portfolio. This question was in the survey until October 2007.

The third dataset is a series constructed by Nagel and Xu (2019) that uses additional surveys to extend the UBS/Gallup survey forward and backward in time. We use the data from 1987m6 onwards, when observations are available monthly without gaps. In this series, the missing market return expectation in the UBS/Gallup survey from 2003 to 2007 is imputed from the own portfolio return expectation as the fitted value from a regression of expected market returns on own portfolio expectations in the part of the sample where both are available. This series further includes mean one-year return expectations from Ameriks et al. (2019) (one survey in 2014). The series is then extended using data from surveys that do not have percentage return expectations, but coarser measures of investor beliefs. This is done by regressing the available return expectations on the average reported probability of a rise in the stock market in the Michigan Survey of Consumers (available from 2002 to 2016) and the fitted value is used to extend the percentage expectations series.⁸ This extended series is then regressed on a measure of the proportion of respondents expecting a rise in the stock market in surveys conducted by the Conference Board (1987–2016, monthly) and the fitted value used to extend the expected return series.

The fourth and fifth datasets are surveys from Robert Shiller and the Investor Behavior Project at Yale University.⁹ The surveys are based on two samples: wealthy individual investors and institutional investors. Each individual response includes the day on which the survey was completed. The dataset starts in January 1999, and we use data until August 2015. The average number of responses per quarter is 75 (60) for the individual (institutional) investor dataset. Survey respondents are asked to forecast the percentage change in the Dow Jones Industrial Index over various horizons (3 months, 6 months, 1 year and 10 years).

The final data set is from the Livingston Survey, which is run by the Federal Reserve Bank of Philadelphia. Differently from previously listed surveys, respondents are economists from industry, government, banking, and academia. The survey is conducted twice a year (with releases in June and December) and starts in June 1946. We use the data until December 2017.¹⁰ Survey participants forecast the level of the S&P500 six and twelve months ahead. Since the exact date at which respondents complete the survey is unknown, one cannot use the actual S&P500 level at the survey completion date to compute expected capital gains. We therefore use the six- and twelve-months ahead index level mean forecasts to compute a forward expected capital gain.

3.2. Matching with returns data

In the tests of the risk-neutral expectations hypothesis, we compare survey expectations to risk-free rates over a matched maturity. For maturities from three to six months, our baseline tests use daily U.S. Treasury Bill yields, obtained from the FRED database at the Federal Reserve Bank of St. Louis. For maturities from one to 10 years, we use daily zero-coupon yields from Gürkaynak et al. (2007).¹¹ Using the six-month and the one-year yields we compute the six-twelve months forward rate of return. We also explore alternative specifications in which we use the one-year adjustable mortgage rate from the FRED database as a proxy for individuals' collateralized borrowing rates. Since this series was discontinued at the end of 2015, the empirical results using mortgage interest rate have 2015 as their sample end. We convert all yields into effective yields over the relevant maturity.

For each survey source and survey forecast horizon we use the risk-free interest of a corresponding maturity prevailing at the reported survey date. For the CFO survey, we know a reference date, which is a day very close to when the survey was sent (by fax). The survey administrators request a response within a few days from this reference date. We match the survey responses with the risk-free rate on the reference date. For the UBS/Gallup survey, we know the two- to three-week period in which the survey took place and we use the average daily yield during this period. The extended UBS/Gallup series is monthly, and we match survey expectations with the yield at the end of the month preceding the survey month. For the Shiller datasets, for which we observe the response date of each individual survey response, we match each survey response with the interest rate prevailing on the day of the survey. Since the Shiller surveys ask about price growth on the Dow Jones Industrial Average (DJIA), but the test of the risk-neutral expectations hypothesis requires a total return, we adjust the price growth series by adding the DJIA dividend yield at the end of the month preceding the survey date, adjusted for the relevant forecasting horizon under the assumption that the dividend yield stays constant. The Livingston survey is sent to participants in May and November after the consumer price index release for the previous month. We match survey expectations with the average daily forward risk-free rates during the second half of these months. As with the Shiller survey, we augment the forward expected capital gains series with the appropriately adjusted S&P500 dividend yield at the end of the month preceding the distribution of the Livingston survey.

⁸ This fitting procedure eliminates any level biases specific to the Michigan and Conference board series and imposes on them the level bias present in the UBS series. The qualitative data in the Conference Board series and the probability responses in the Michigan series do not allow us to identify the level bias in mean return forecasts from these surveys.

⁹ The surveys are available at <http://som.yale.edu/faculty-research/our-centers/international-center-finance/data>.

¹⁰ Historical data is available at <https://www.philadelphiafed.org/research-and-data/real-time-center/livingston-survey/historical-data>.

¹¹ The periodically updated data are available at <http://www.federalreserve.gov/pubs/feds/2006>.

In tests of the pessimism hypothesis, we compare survey expectations with returns or price growth of stock market indices. In the case of the CFO survey, we match the survey expectations with the total return on the S&P 500 index over the one-year period starting from the reference date. For the UBS/Gallup survey, we use the return of the same series over the one-year period starting from the first day of the interview period. For the monthly extended UBS/Gallup series, we use a one-year total return on the S&P 500 index from the end of the month prior to the survey month. For the Shiller surveys, we use price growth on the DJIA realized over the relevant horizon starting from the date of the individual response. For the Livingston survey we use the realized price growth of the S&P 500 index over the considered six months period.

Having computed the wedges between survey expectations and returns, we then aggregate the data to time series by computing means or medians within months or quarters, as shown in the second and seventh column of Table 1.

3.3. Matching with conditioning variables

Several test rely on the use of conditioning variables (RN-C, PE-C1, PE-C2). Our baseline results use the P/D ratio, which is a commonly used stock return predictor. We use the S&P 500 P/D ratio for the CFO, UBS and Livingston surveys, the Center for Research in Security Prices (CRSP) value-weighted index P/D ratio for the UBS extended series and the PE-C2 test with the Shiller survey, and the DJIA P/D ratio for the PE-C1 test with the Shiller surveys. In terms of timing, we use the P/D ratio measured at the end of the last month preceding the survey reference date for the CFO survey, at the end of the last month preceding the first day of the interview period for the UBS survey, at the end of the last month preceding the date of the individual response for the Shiller surveys, and at the end of the last month preceding the distribution of the Livingston survey. In robustness checks, we also consider the consumption wealth ratio (cay) of Lettau and Ludvigson (2001) and the 10-year minus 3-month (10y-3m) Treasury term spread as predictors.¹²

Some of our tests (PE-U2 and PE-C2) use an estimate of objective conditional expected returns $\hat{E}_t[R_{t+1}]$, which we construct from a predictive regression. We use monthly returns or price growth and the S&P 500 P/D ratio for the CFO survey as well as the Livingston survey and the CRSP value-weighted index P/D ratio for all the other tests (or the other predictor variables in robustness checks). When we construct the fitted value $\hat{E}_t[R_{t+1}]$, we do so using a predictor that is timed relative to the survey date in the same way as explained above, with the exception of the Shiller survey, where we use the predictor at the end of the quarter preceding the interview quarter. We construct multi-period return forecasts by using multi-period realized returns on the left-hand side of Eq. (13).

4. Empirical results: return expectations

4.1. Risk-neutral expectations hypothesis

Table 2 reports results for the RN-U test based on Eq. (6) using Treasury rates as risk-free rates. This test looks at the most basic implication of the risk-neutral expectations hypothesis: Are subjective expected returns on average equal to the risk-free rate over the forecast horizon? As the table shows, the answer is a clear no. For surveys from all sources and horizons except the Shiller individual investor survey using medians over a 10-year horizon, the subjective expected returns elicited in the surveys exceed risk-free rates by several percentage points. As the t -statistics and p -values show, we can reject the risk-neutral expectations hypothesis at extremely high levels of significance. There is also a remarkable degree of consistency across different types of survey respondents. Subjective expected returns exceed risk-free rates, contradicting the risk-neutral expectations hypothesis, for business practitioners (CFOs), professional investors, wealthy individuals, and individual investors.

The risk-neutral expectations hypothesis not only implies that the unconditional average subjective expected return is equal to the risk-free rate, but also that this equality holds conditionally, state by state. The RN-C tests based on (7) reported in Table 3 shed light on this conditional version of the risk-neutral expectations hypothesis. We use Treasury rates as risk-free rates and the P/D ratio as the regressor x_t that could drive time variation in subjective expected excess returns under the alternative hypothesis. We obtain small-sample bias-adjusted coefficient estimates and simulate F -statistics under the null hypothesis of risk-neutral expectations, as described in Online Appendix D.

The main test of the risk-neutral expectations hypothesis is the test of the joint hypothesis $a_0 = 0$ and $a_1 = 0$. As the results in Table 3 show, this hypothesis is rejected at the 5% level for all but 5 of the 28 survey series. Like the unconditional tests in Table 2, the conditional tests here indicate that there is a substantial wedge between the subjective expectations of returns and risk-free rates.

To what extent does this wedge vary with the P/D ratio? The bias-adjusted point estimates of a_1 point to an interesting difference between individual and professional investors. For almost all survey series with individual investor respondents and CFOs, either the estimates of a_1 indicate a statistically significant positive relationship of subjective expected excess returns to the P/D ratio, or the estimates are not significantly different from zero. In contrast, the subjective expected excess returns of professional investors in the Shiller survey are, with one exception (mean series at 10-year horizon), negatively

¹² The cay variable is available quarterly starting in 1952. The historical data is available at <https://sites.google.com/view/martinlettau/data>. The term spread series is from the FRED database at the Federal Reserve Bank of St. Louis and starts in 1982. We aggregate the daily interest rate data by computing monthly averages.

Table 2

Unconditional Test of the Risk-Neutral Expectations Hypothesis This table presents tests of the RN-U hypothesis. The column labeled a reports the mean of the subjective expected return (in terms of percent) in excess of the risk-free rate (based on Treasury securities) over the relevant horizon. The t -statistics and p -values are based on a Newey–West estimator with 2 lags for bi-annual data, 4 lags for quarterly data and 12 lags for monthly data.

| Survey source | | | a | t -statistic | p -value for $H_0 : a = 0$ | |
|--------------------|----------------------|--------|--------|----------------|------------------------------|--------|
| CFO | | mean | 3.89 | 9.47 | 0.0000 | |
| | | median | 3.55 | 8.43 | 0.0000 | |
| UBS own | all | mean | 6.55 | 12.53 | 0.0000 | |
| | | median | 4.52 | 9.99 | 0.0000 | |
| | > 100k | mean | 6.40 | 12.36 | 0.0000 | |
| | | median | 4.79 | 10.82 | 0.0000 | |
| UBS market | all | mean | 6.64 | 13.31 | 0.0000 | |
| | | median | 4.61 | 14.13 | 0.0000 | |
| | > 100k | mean | 6.36 | 12.29 | 0.0000 | |
| | | median | 4.74 | 15.80 | 0.0000 | |
| UBS extended | | | 5.80 | 20.10 | 0.0000 | |
| Shiller individual | 3m | mean | 1.00 | 4.71 | 0.0000 | |
| | | median | 1.45 | 6.73 | 0.0000 | |
| | 6m | mean | 2.29 | 7.98 | 0.0000 | |
| | | median | 2.86 | 11.22 | 0.0000 | |
| | 1yr | mean | 5.02 | 9.26 | 0.0000 | |
| | | median | 5.81 | 13.14 | 0.0000 | |
| | 10yr | mean | 8.90 | 2.34 | 0.0194 | |
| | | median | −5.33 | −1.19 | 0.2341 | |
| | Shiller professional | 3m | mean | 0.68 | 2.28 | 0.0223 |
| | | | median | 1.48 | 5.19 | 0.0000 |
| | | 6m | mean | 2.16 | 3.82 | 0.0001 |
| | | | median | 3.86 | 8.94 | 0.0000 |
| 1yr | | mean | 5.24 | 5.23 | 0.0000 | |
| | | median | 7.43 | 8.41 | 0.0000 | |
| 10yr | mean | 42.47 | 10.79 | 0.0000 | | |
| | median | 28.88 | 7.88 | 0.0000 | | |
| Livingston | 6-12m | mean | 1.97 | 4.93 | 0.0000 | |

related to the P/D ratio at the 5% significance level. For all individuals, CFOs, and professionals, however, the joint hypothesis $a_0 = 0$ and $a_1 = 0$ is overwhelmingly rejected.

The statistically weak relationship between subjective expectations and the P/D ratio in [Table 3](#) contrasts with the much stronger relationship documented in [Greenwood and Shleifer \(2014\)](#). The key difference is that here we examine subjective expected excess returns while Greenwood and Shleifer use expectations of total returns as dependent variable. During our sample period, the P/D ratio and Treasury rates are positively correlated and, hence, subtracting the risk-free rate from the subjective expected return weakens the positive relationship with the P/D ratio; see also the analysis in [Adam et al. \(2017\)](#), which presents regression estimates for both excess returns and plain returns.

However, for the UBS extended series, where we have the longest time series, and hence more statistical power than for the shorter series, we can still reject the hypothesis $a_1 = 0$ with a high level of statistical confidence. [Fig. 1](#) shows the fitted values of subjective expected returns in excess of Treasury rates based on the bias-adjusted point estimates of a_0 and a_1 along with two-standard-error bands from the RN-C test regression (7) for the UBS extended sample. The figure shows that the estimated subjective conditional expected excess return is, in conflict with the predictions of the risk-neutral expectations hypothesis, far above zero throughout the whole sample. The lower boundary of the two-standard-error bands never include zero anywhere. Since the survey expectations in a given period likely contain substantial measurement error, the projection in [Fig. 1](#) might provide a better description of the time-series dynamics of the true subjective expectations than the actual belief wedge $\varepsilon_t[R_{t+1}] - R_t^f$. But as [Fig. 1](#) shows, even the actual belief wedge never dips below zero.

The results we have presented so far could potentially be rationalized under the risk-neutral return hypothesis if individuals are borrowing constrained and face very high shadow interest rates, which could cause their risk-neutral expectations of stock returns to be substantially higher than Treasury rates. However, the fact that the RN-U and RN-C tests reject the risk-neutral expectations hypothesis for the subsample of wealthy investors in the UBS survey, the Shiller individual investor survey (which is also based on a sample of wealthy individuals), CFOs, professional investors in the Shiller survey, and for professional forecasters in the Livingston survey, cast doubt on this alternative explanation. First, for these samples of wealthy investors and professionals, the borrowing constraints story does not appear plausible—especially if it requires shadow rates that are four or more percentage points above Treasury rates. Second, the point estimates of a for the CFO, UBS wealthy, and Shiller individual survey at the one-year horizon in [Table 2](#) are very similar to the a estimated from the

Table 3

Conditional Test of the Risk-Neutral Expectations Hypothesis This table presents tests of the RN-C hypothesis in which we regress subjective expected returns in excess of Treasury rates on the lagged P/D ratio. The columns labeled a_0 and a_1 report the intercept and slope coefficients, respectively, from these regressions. We report the a_1 estimates multiplied by a factor of 1000. They are bias-adjusted for small samples, as described in Online Appendix D. The last two columns report Monte Carlo p -values obtained by simulating F -statistics under the null hypothesis, as described in Online Appendix D.

| Survey Source | | a_0 | a_1 $\cdot 10^3$ | p -value for $H_0 : a_0 = a_1 = 0$ | p -value for $H_0 : a_1 = 0$ | | |
|--------------------|----------------------|--------|-----------------------|---|-----------------------------------|--------|--------|
| CFO | mean | -1.39 | 8.66 | 0.0044 | 0.2307 | | |
| | median | 0.08 | 5.48 | 0.3937 | 0.2628 | | |
| UBS own | all | mean | 1.09 | 6.57 | 0.0000 | 0.0589 | |
| | | median | -2.65 | 8.92 | 0.1860 | 0.0057 | |
| | > 100k | mean | 0.02 | 7.86 | 0.0000 | 0.0302 | |
| | | median | -2.03 | 8.48 | 0.0106 | 0.0160 | |
| UBS market | all | mean | -0.52 | 7.96 | 0.0000 | 0.0059 | |
| | | median | -0.91 | 6.17 | 0.0000 | 0.0221 | |
| | > 100k | mean | -1.72 | 9.00 | 0.0000 | 0.0148 | |
| | | median | -0.36 | 5.66 | 0.0000 | 0.2243 | |
| UBS extended | | mean | 2.24 | 5.79 | 0.0000 | 0.0002 | |
| | | median | 0.03 | 1.95 | 0.0029 | 0.4759 | |
| Shiller individual | 3m | mean | 0.04 | 2.66 | 0.0000 | 0.0852 | |
| | | median | 2.61 | -0.47 | 0.0000 | 0.8115 | |
| | 6m | mean | 3.66 | -1.43 | 0.0000 | 0.5086 | |
| | | median | 10.46 | -9.77 | 0.0000 | 0.0141 | |
| | 1yr | mean | 9.18 | -6.02 | 0.0001 | 0.5184 | |
| | | median | 34.51 | -50.86 | 0.9759 | 0.4601 | |
| | 10yr | mean | -1.58 | -11.40 | 0.8236 | 0.8359 | |
| | | median | 4.26 | -6.45 | 0.0005 | 0.0068 | |
| | Shiller professional | 3m | mean | 4.99 | -6.34 | 0.0000 | 0.0015 |
| | | | median | 9.48 | -13.20 | 0.0000 | 0.0001 |
| 6m | | mean | 9.58 | -10.19 | 0.0000 | 0.0005 | |
| | | median | 19.51 | -25.89 | 0.0000 | 0.0052 | |
| 1yr | | mean | 20.39 | -23.54 | 0.0000 | 0.0016 | |
| | | median | 83.88 | -76.17 | 0.0000 | 0.0844 | |
| 10yr | | mean | 76.47 | -87.97 | 0.0000 | 0.0200 | |
| | | median | 8.07 | -12.23 | 0.1309 | 0.0844 | |
| Livingston | | 6-12m | mean | 8.07 | -12.23 | 0.1309 | 0.0844 |

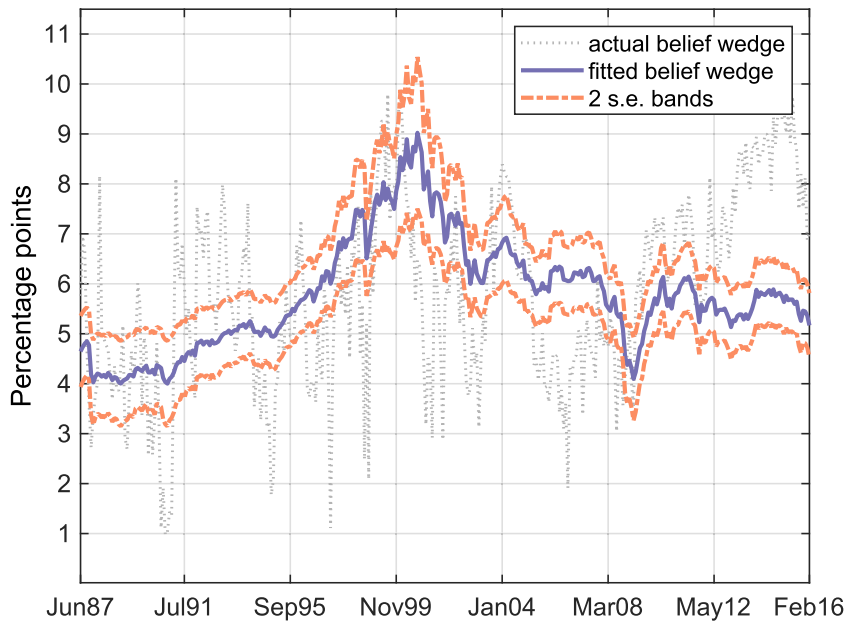


Fig. 1. Fitted values from regression (7) of subjective expected returns in excess of Treasury rates on the P/D ratio in the UBS extended sample.

Table 4

Unconditional Tests of the Pessimism Hypothesis This table presents unconditional tests of the pessimism (PE-U) hypothesis. The columns labeled PE-U1 report the mean of the subjective expected return in excess of the realized return over the forecast horizon along with the associated *t*-statistic and *p*-value based on a Newey-West estimator with 2 lags for bi-annual data, 4 lags for quarterly data, and 12 lags for monthly data. The columns labeled PE-U2 report the mean of the subjective expected return in excess of the fitted value from a regression of the relevant return or price growth for each survey on the lagged P/D ratio, with regression parameters estimated with data from 1926 to 2017. The *t*-statistics and *p*-values in this case are computed based on the asymptotic approximation outlined in Online Appendix C, including a Newey-West estimator of the covariance matrix using 2 lags for bi-annual data, 4 lags for quarterly data, and 12 lags for monthly data.

| Survey Source | | | PE-U1 | | | PE-U2 | | | |
|------------------------------------|----------------------|--------|----------|-----------------|-----------------------------------|----------|-----------------|-----------------------------------|--------|
| | | | <i>b</i> | <i>t</i> -stat. | <i>p</i> -val. $H_0: b \leq 0$ | <i>b</i> | <i>t</i> -stat. | <i>p</i> -val. $H_0: b \leq 0$ | |
| CFO | | mean | −1.61 | −0.43 | 0.6663 | −0.60 | −0.37 | 0.6425 | |
| | | median | −3.69 | −1.05 | 0.8526 | −1.70 | −1.11 | 0.8663 | |
| UBS own | all | mean | 7.59 | 1.62 | 0.0526 | 7.09 | 2.79 | 0.0026 | |
| | | median | 5.56 | 1.21 | 0.1141 | 5.06 | 2.06 | 0.0199 | |
| | > 100k | mean | 7.44 | 1.59 | 0.0558 | 6.94 | 2.73 | 0.0031 | |
| | | median | 5.83 | 1.27 | 0.1025 | 5.33 | 2.17 | 0.0148 | |
| UBS market | all | mean | 13.97 | 2.07 | 0.0193 | 9.84 | 3.17 | 0.0008 | |
| | | median | 11.94 | 1.79 | 0.0366 | 7.81 | 2.64 | 0.0042 | |
| | > 100k | mean | 13.69 | 2.01 | 0.0222 | 9.55 | 3.06 | 0.0011 | |
| | | median | 12.07 | 1.81 | 0.0351 | 7.94 | 2.70 | 0.0035 | |
| UBS extended Shiller individual | 3m | mean | −1.86 | −0.72 | 0.7636 | 2.10 | 1.06 | 0.1454 | |
| | | median | −0.55 | −0.68 | 0.7513 | −0.51 | −1.01 | 0.8428 | |
| | 6m | mean | −0.31 | −0.38 | 0.6497 | −0.07 | −0.14 | 0.5563 | |
| | | median | −0.01 | −0.01 | 0.5030 | −0.61 | −0.69 | 0.7548 | |
| | 1yr | mean | 0.41 | 0.25 | 0.4030 | −0.04 | −0.05 | 0.5192 | |
| | | median | 0.52 | 0.17 | 0.4323 | 0.28 | 0.18 | 0.4290 | |
| | 10yr | mean | 1.11 | 0.36 | 0.3578 | 0.87 | 0.57 | 0.2841 | |
| | | median | 11.53 | 0.50 | 0.3088 | 8.26 | 0.48 | 0.3158 | |
| | Shiller professional | 3m | mean | −2.01 | −0.08 | 0.5326 | −6.73 | −0.36 | 0.6389 |
| | | | median | −0.59 | −0.62 | 0.7334 | −0.83 | −1.69 | 0.9542 |
| | | 6m | mean | 0.05 | 0.05 | 0.4794 | −0.04 | −0.08 | 0.5301 |
| | | | median | −0.08 | −0.05 | 0.5181 | −0.74 | −0.86 | 0.8044 |
| 1yr | | mean | 1.12 | 0.65 | 0.2587 | 0.96 | 1.17 | 0.1216 | |
| | | median | 0.58 | 0.20 | 0.4218 | 0.31 | 0.22 | 0.4118 | |
| 10yr | mean | 2.60 | 0.89 | 0.1864 | 2.56 | 1.86 | 0.0318 | | |
| | median | 43.18 | 2.27 | 0.0115 | 40.35 | 2.77 | 0.0028 | | |
| Livingston | 6–12m | mean | 25.81 | 1.43 | 0.0761 | 25.61 | 1.93 | 0.0268 | |
| | | median | −1.26 | −1.16 | 0.8773 | −0.58 | −0.72 | 0.7654 | |

full UBS sample, which suggests that the on-average less-wealthy individuals in the full UBS sample are not systematically different from wealthy investors and professionals in terms of their subjective expected return.

A more subtle friction-based explanation could be that there is a wedge between borrowing and lending rates. If the borrowing margin is relevant for many households, the Treasury rates that we have used so far may not be the relevant interest rates in households' Euler Eq. (2). Instead, borrowing rates may be more relevant. For this reason, we re-run the RN-U and RN-C tests with subjective expected excess returns calculated relative to one-year adjustable mortgage rates. We use a collateralized borrowing rate rather than an unsecured borrowing rate to avoid contamination by a substantial credit spread.

Results are presented in Table I in Online Appendix A. It shows that there isn't much difference from the earlier tests with excess returns relative to Treasury rates. For the unconditional test, we still reject the risk-neutral expectations hypothesis for all (one-year horizon) series, just as we did in Table 2. The point estimates for α are slightly smaller, but all of them are still substantially greater than zero by about one to five percentage points. Similarly, for the conditional test of the hypothesis $\alpha_0 = \alpha_1 = 0$, we reject the null at a 5% level for all but one of the survey expectations series. Overall, taking into account the potential effects of differences in borrowing and lending rates doesn't help much to rescue the risk-neutral expectations hypothesis.

4.2. Pessimism hypothesis

The results so far suggest that subjective expectations of returns exceed risk-free rates by substantial amounts, which is inconsistent with the risk-neutral expectations hypothesis. However, this still leaves the possibility that their expectations are pessimistically biased relative to objective expectations of returns under the real-world probability measure. The alternative is that survey respondents simply do what the survey asks them to do: provide the expected return under their

Table 5

Conditional Tests of the Pessimism Hypothesis This table presents conditional tests of the pessimism (PE-C) hypothesis where we regress on the lagged P/D ratio the subjective expected returns in excess of realized returns (PE-C1) or the subjective expected return in excess of the fitted value from a regression of realized returns on the lagged P/D ratio (PE-C2), where the latter regression is estimated over the period 1926–2017. Based on these regression estimates, we determine, at every point in time, whether we can reject the pessimism hypothesis (weakly negative predicted subjective excess return) or the optimism hypothesis (weakly positive predicted subjective excess return) at a 5% level. In the PE-C1 case, the regressions are bias-adjusted as described in Online Appendix D. In the PE-C2 case, the t -statistics for this test are computed based on the asymptotic approximation outlined in Online Appendix C, including a Newey-West estimator of the covariance matrix using 2 lags for bi-annual data, 4 lags for quarterly data, and 12 lags for monthly data.

| | | | PE-C1 | | PE-C2 | |
|-----------------------------|--------|--------|--|-----------------|--|-----------------|
| | | | Reject Pessimism (share of observations) | Reject Optimism | Reject Pessimism (share of observations) | Reject Optimism |
| CFO | | mean | 0.1212 | 0.1667 | 0.0968 | 0.2258 |
| | | median | 0.0645 | 0.1774 | 0.0172 | 0.3621 |
| UBS own | all | mean | 0.3504 | 0.0256 | 0.3714 | 0.0000 |
| | | median | 0.3419 | 0.0256 | 0.2190 | 0.0000 |
| | > 100k | mean | 0.3504 | 0.0256 | 0.3429 | 0.0000 |
| | | median | 0.3504 | 0.0256 | 0.2286 | 0.0000 |
| UBS market | all | mean | 0.8889 | 0.0000 | 0.5686 | 0.0000 |
| | | median | 0.5714 | 0.0000 | 0.2745 | 0.0000 |
| | > 100k | mean | 0.4444 | 0.0000 | 0.5294 | 0.0000 |
| | | median | 0.4762 | 0.031 | 0.2941 | 0.0000 |
| UBS extended Shiller indiv. | 3m | mean | 0.1563 | 0.3125 | 0.1335 | 0.1278 |
| | | median | 0.1692 | 0.5538 | 0.0000 | 0.2769 |
| | 6m | mean | 0.1846 | 0.4154 | 0.0000 | 0.1231 |
| | | median | 0.2154 | 0.2154 | 0.0000 | 0.2615 |
| | 1yr | mean | 0.2308 | 0.1692 | 0.0000 | 0.1231 |
| | | median | 0.1846 | 0.2154 | 0.0462 | 0.1538 |
| | 10yr | mean | 0.0154 | 0.7692 | 0.1077 | 0.0923 |
| | | median | 0.1077 | 0.1077 | 0.2286 | 0.4857 |
| Shiller prof. | 3m | mean | 0.7077 | 0.0000 | 0.0857 | 0.4286 |
| | | median | 0.1231 | 0.4000 | 0.0000 | 0.0769 |
| | 6m | mean | 0.1538 | 0.0000 | 0.0000 | 0.0000 |
| | | median | 0.1846 | 0.2308 | 0.0000 | 0.0000 |
| | 1yr | mean | 0.1385 | 0.0923 | 0.0000 | 0.0000 |
| | | median | 0.1846 | 0.0308 | 0.0000 | 0.0000 |
| | 10yr | mean | 0.1846 | 0.0615 | 0.0000 | 0.0000 |
| | | median | 0.2615 | 0.3077 | 0.4571 | 0.0000 |
| Livingston | 6-12m | mean | 0.1846 | 0.6615 | 0.3143 | 0.0000 |
| | | median | 0.0391 | 0.5547 | 0.0703 | 0.4141 |

perceived real-world probability measure. In this section, we report results from tests of this pessimistic expectations hypothesis.

Table 4 reports the results from unconditional tests. The set of columns labeled PE-U1 presents results from estimating Eq. (12). A positive (negative) estimate for the coefficient b indicates that the subjective return expectation exceeds on average (falls on average short of) the realized returns. The table also reports p -values for a one-sided test of the weak pessimism hypothesis $b \leq 0$. It shows that one cannot reject weak pessimism for more than two-thirds of the subjective expectations series. However, the weak pessimism hypothesis includes $b = 0$ and only about one-third of the t -statistics turn out to be negative. Moreover, none of the t -statistics would allow the alternative null hypothesis of unconditional optimism to be rejected. If anything, there is thus a tendency towards unconditional optimism rather than pessimism. Overall, there is little evidence of deviations from unconditional unbiasedness of return expectations. In particular, the mean bias is also not significantly different from zero for the UBS extended series for which we have the longest time series and (among the one-year horizon series) the smallest standard error and highest statistical power. Subjective expected returns thus appear to be, on average, close to unbiased, which is inconsistent with a substantial pessimism bias.

One concern with the PE-U1 tests could be that realized returns are just too noisy to provide much statistical power when we compare subjective expected returns with realized returns, especially given the relatively short sample for which survey expectations are available. This issue is addressed in the set of columns labeled PE-U2 in Table 4, which compares the subjective return expectations with fitted values from a regression of realized returns on the lagged dividend yield estimated over the period 1926–2017. The reported estimates in Table 4 show that almost all point estimates of b move closer to zero. The absolute value of the t -statistics, however, are often bigger than with the PE-U1 test because the standard errors of

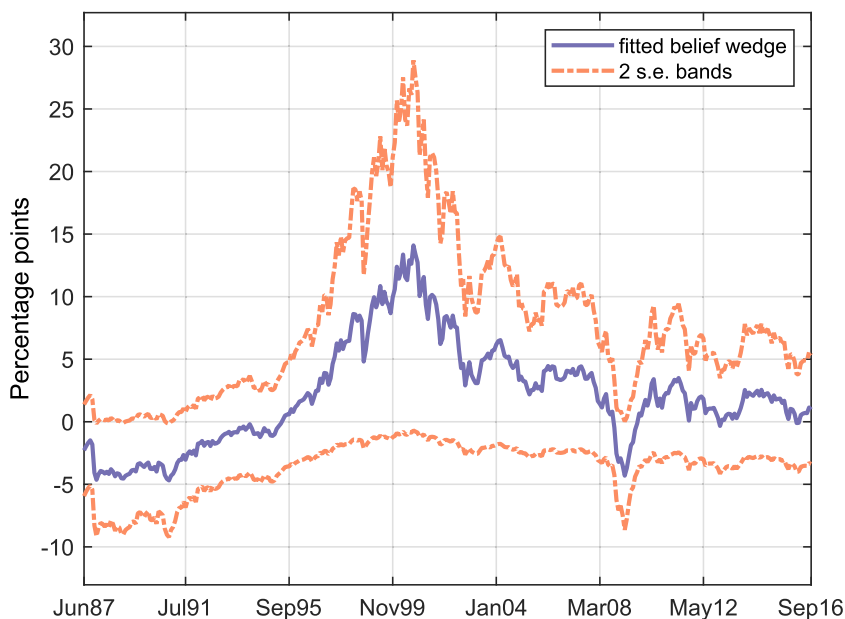


Fig. 2. Fitted values from regression (18) of subjective expected returns in excess of the estimated objective expected returns on the P/D ratio in the UBS extended sample. The objective expected returns are estimated from regression (13) of realized returns on P/D ratio over a longer sample from 1926 to 2017.

the estimation become smaller, consistent with an increase in statistical power.¹³ Overall, the picture remains mixed. Some estimates of b are larger than zero, while some are smaller, but again with the tendency towards optimism being slightly more prevalent: we can reject the weak pessimism hypothesis at the 5% level for 11 of the 28 series, while the weak optimism hypothesis is rejected in only one instance. By and large, this test again suggests that subjective expectations are in unconditional terms not far from being unbiased.

Table 5 presents conditional tests of the pessimism hypothesis. Using the estimated coefficients from Eqs. (17) and (18), we check, at each point in time for all observed price-dividend and expectations pairs whether one can reject, at the 5% level, conditional pessimism (thus implying optimistic expectations) or reject conditional optimism (thus implying conditional pessimism). The table reports the share of observations for which we reject conditional pessimism or conditional optimism. For the remaining share of observations—i.e., one minus the reported share of rejections of optimism and pessimism—no definite conclusion can be reached at the considered significance level.

The PE-C1 version of the test in Table 5, which is based on Eq. (17), looks at the predicted wedge between subjective expected returns and subsequently realized returns; the PE-C2 version of the test, which is based on Eq. (18), calculates the wedge as the difference between the subjective expected return and the fitted value from a predictive regression of realized returns, where the predictive regression uses the lagged P/D ratio and is estimated over the period 1926–2017.

The first notable result from Table 5 is that many observations can be classified as neither optimistic nor pessimistic. From a purely statistical point of view, subjective expected returns are thus often in the vicinity of objective expected returns. Nevertheless, for a substantial share of observations, we can reject return pessimism. This is especially true for the UBS surveys. For other surveys, such as the Shiller individual surveys, the share of rejections of optimism and pessimism are roughly balanced (PE-C1) or tilted in favor of rejecting optimism (PE-C2). Overall, whether tests more often reject optimism or pessimism appears to depend on the survey source. Since surveys cover different sample periods, the direction of rejections might depend on the sample period. This conjecture is supported by Fig. 2, which uses the UBS extended sample and reports the fitted values of subjective expected returns in excess of the estimated objective expected returns on the P/D ratio, along with two-standard-error bands from the PE-C2 test regression (18). The deviations of subjective expected returns from estimated objective expected returns is procyclical. In boom times, like the late 1990s, investors tend to be too optimistic. Following crashes, like in early 2009, investors tend to be too pessimistic. The UBS/Gallup survey includes observations from the late 1990s, but no observations from the financial crisis and its aftermath. In contrast, the Shiller surveys include observations from the financial crisis and subsequent years. This partly explains why these other surveys on average have a lower share of optimistic observations than the UBS/Gallup survey.

¹³ This increase in statistical power occurs despite the fact that the standard errors are adjusted for the estimation uncertainty coming from the first-stage predictive regression.

Table 6

Tests of the Pessimism Hypothesis: High vs. Low Experience This table presents unconditional test (PE-U1 and PE-U2) and conditional test (PE-C1 and PE-C2) of the pessimism hypothesis for investors with different stock market experience using data from the UBS survey. Investors with experience above or equal to the median number of years are classified as high experience, the remaining as low experience. The remaining details of the tests are as described in the notes of Tables 4 and 5.

| Survey Source | | | Unconditional | | | Conditional | | |
|---------------|------------|-----------|---------------|-----------------|------------------------------------|--|--------------------|--------|
| | | | <i>b</i> | <i>t</i> -stat. | <i>p</i> -value $H_0: b \leq 0$ | Reject Pessimism (share of observations) | Reject Optimism | |
| UBS own | all | high exp. | PE-U1 7.04 | 1.52 | 0.0647 | PE-C1 0.4017 | 0.0085 | |
| | | low exp. | 8.25 | 1.75 | 0.0401 | 0.4615 | 0.0000 | |
| | > 100k | high exp. | 6.97 | 1.51 | 0.0659 | 0.4103 | 0.0085 | |
| | | low exp. | 8.06 | 1.71 | 0.0440 | 0.4359 | 0.0085 | |
| | UBS market | all | high exp. | 13.34 | 1.97 | 0.0244 | 0.4921 | 0.0000 |
| | | low exp. | 14.73 | 2.18 | 0.0146 | 0.5714 | 0.0000 | |
| > 100k | high exp. | 13.16 | 1.94 | 0.0261 | 0.6190 | 0.0000 | | |
| | low exp. | 14.31 | 2.09 | 0.0184 | 0.7619 | 0.0000 | | |
| UBS own | all | high exp. | PE-U2 6.54 | 2.62 | 0.0044 | PE-C2 0.3429 | 0.0000 | |
| | | low exp. | 7.75 | 3.01 | 0.0013 | 0.3714 | 0.0000 | |
| | > 100k | high exp. | 6.47 | 2.60 | 0.0046 | 0.3333 | 0.0000 | |
| | | low exp. | 7.56 | 2.92 | 0.0017 | 0.3714 | 0.0000 | |
| | UBS market | all | high exp. | 9.21 | 3.01 | 0.0013 | 0.5490 | 0.0000 |
| | | low exp. | 10.60 | 3.34 | 0.0004 | 0.5686 | 0.0000 | |
| > 100k | high exp. | 9.03 | 2.98 | 0.0015 | 0.4706 | 0.0000 | | |
| | low exp. | 10.18 | 3.13 | 0.0009 | 0.4706 | 0.0000 | | |

Table II in Online Appendix B reports conditional results when we include also the consumption wealth ratio (*cay*) of Lettau and Ludvigson (2001) or the 10-year minus 3-month term spread (10y-3m), or both variables as additional predictor variables. The overall picture emerging from the table is the same as the one obtained when using only the P/D ratio as predictor variable. The most notable change is the fact that the UBS market series now delivers considerably fewer rejections of pessimism when *cay* is included as a predictor variable. This, however, only reflects the fact that this survey series is particularly short to start with and that the *cay* series is available only on a quarterly frequency, so that we now use only one forecast observation per quarter. Apart from this issue, the result in Online Appendix B shows that the conclusions are robust to using alternative approaches for constructing rational return predictions. This partly reflects the fact that the R-square value of the predictive first-stage regression (13) often does not increase much when adding the additional conditioning variables.

Overall, our results show that while subjective expectations are, in unconditional terms, close to unbiased, there is a substantial time-varying conditional bias. This conditional bias flips the sign and is often an optimism bias, inconsistent with the pessimistic expectations hypothesis.

4.3. Heterogeneity by experience and wealth

Our tests so far have focused mostly on measures of central tendency in forecasts. If marginal-utility weighting in expectations is concentrated among a subset of the population, tests based on central tendencies of forecasts may miss this fact. To separate true expectations heterogeneity from noise in survey responses, we need to condition on individual respondent characteristics. Unfortunately, the availability of such data is limited. Only in the UBS data we have individual-level responses for percentage return expectations and a small number of respondent characteristics.

For the UBS survey, we have already shown earlier that there are no substantial differences in the properties of return expectations when we group respondents by wealth. Here we break down the sample further by investment experience. Specifically, we consider two investor groups, one with years of stock market experience above or equal to the median and one with years of stock market experience below.

Table 6 repeats the unconditional pessimism test from Table 4 for the mean expectations from the UBS survey. As discussed before, the UBS survey spans a time period during which return expectations were generally high relative to the subsequently realized outcomes (in PE-U1 tests) and predictive regression predictions (in PE-U2 tests). This is reflected in the generally positive *b* estimates in Table 6. Comparing estimates across groups, there is only little heterogeneity: investors with little stock market experience report somewhat more optimistic expectations than more experienced investors, but the difference in point estimates of around one percentage point is quite small. Focusing on the most pessimistic group based on these two dimensions of heterogeneity would do little to change our earlier conclusions about the absence of risk-adjustments in reported return expectations.

Table 7

Tests of the Pessimism Hypothesis: Macroeconomic Expectations The table reports unconditional (PE-U1) and conditional (PE-C1) tests of the pessimism hypothesis using expectations of macroeconomic variables from the Survey of Professional Forecasters. The column labeled PE-U1 reports the mean of the subjective expectation in excess of the realized outcome, the t -statistic for the stated null hypothesis, and the p -value based on a Newey-West estimator with 4 lags. For the conditional test (PE-C1), we regress the subjective expected inflation rate in excess of the realized outcome on the lagged output gap, and the subjective expected real GDP growth, unemployment rate, and T-bill interest rate in excess of the respective realized outcomes on the lagged 10y-3m term spread. The regressions are bias-adjusted as described in Online Appendix D. Based on the regression estimates, we determine, at every point in time, whether we can reject (at the 5% level) the pessimism hypothesis (weakly positive predicted subjective excess unemployment rate or weakly negative subjective excess prediction for other variables) or the optimism hypothesis (weakly negative predicted subjective excess unemployment rate or weakly positive subjective excess prediction for other variables). The table reports the share of observations for which pessimism/optimism can be rejected.

| | | PE-U1 | | | PE-C1 | |
|---------------------|-----|-------|------------|-----------------------------------|--|---|
| | | b | t -stat. | p -val. $H_0:$ $b \leq 0$ | Reject Pessimism (share of observations) | Reject Optimism (share of observations) |
| Real GDP growth | t | -0.44 | -2.70 | 0.9965 | 0.0000 | 0.6014 |
| | t+1 | -0.15 | -0.67 | 0.7501 | 0.0000 | 0.0000 |
| | t+2 | 0.07 | 0.25 | 0.4021 | 0.0000 | 0.0000 |
| | t+3 | 0.31 | 1.09 | 0.1384 | 0.0000 | 0.0000 |
| | t+4 | 0.44 | 1.39 | 0.0816 | 0.0000 | 0.0000 |
| Unemployment rate | t | 0.03 | 2.29 | 0.9891 | 0.0000 | 0.4832 |
| | t+1 | 0.05 | 1.28 | 0.9004 | 0.0000 | 0.3108 |
| | t+2 | 0.03 | 0.47 | 0.6793 | 0.0000 | 0.2789 |
| | t+3 | -0.02 | -0.20 | 0.4203 | 0.0000 | 0.0000 |
| | t+4 | -0.05 | -0.39 | 0.3486 | 0.0000 | 0.0000 |
| 3-month T-bill rate | t | 0.05 | 3.33 | 0.0004 | 0.7152 | 0.0000 |
| | t+1 | 0.16 | 2.90 | 0.0019 | 0.5933 | 0.0000 |
| | t+2 | 0.32 | 2.95 | 0.0016 | 0.5369 | 0.0000 |
| | t+3 | 0.50 | 3.09 | 0.0010 | 0.4595 | 0.0000 |
| | t+4 | 0.67 | 3.24 | 0.0006 | 0.0000 | 0.0000 |
| CPI inflation | t | 0.03 | 0.40 | 0.3435 | 0.0000 | 0.0000 |
| | t+1 | 0.18 | 1.14 | 0.1276 | 0.0000 | 0.0000 |
| | t+2 | 0.28 | 1.56 | 0.0600 | 0.0000 | 0.0000 |
| | t+3 | 0.35 | 1.89 | 0.0295 | 0.2980 | 0.0000 |
| | t+4 | 0.43 | 2.18 | 0.0146 | 0.5000 | 0.0000 |
| GDP inflation | t | -0.02 | -0.19 | 0.5753 | 0.0000 | 0.0000 |
| | t+1 | -0.04 | -0.27 | 0.6079 | 0.1961 | 0.1127 |
| | t+2 | -0.02 | -0.08 | 0.5322 | 0.2611 | 0.1330 |
| | t+3 | -0.00 | -0.01 | 0.5024 | 0.2129 | 0.1485 |
| | t+4 | 0.07 | 0.26 | 0.3963 | 0.0000 | 0.0000 |

Table 6 presents also further evidence consistent with this interpretation. It repeats the conditional optimism and pessimism tests (PE-C1, PE-C2) from Table 5 for different experience groups. One can reject conditional pessimism slightly more frequently for the less experienced investor group, but the differences between groups are again quite small.

5. Empirical results: macroeconomic expectations

As final part of our analysis, we check whether the properties we found for return expectations are also present in other types of economic expectations. Since we do not observe other types of expectations in the investor return expectations data sets, we focus on forecasts of macroeconomic variables in different data, the Survey of Professional Forecasters (SPF). This means that we cannot perform within-individual comparisons of different types of forecasts, but we can at least check whether the macroeconomic forecasts exhibit the same lack of risk-adjustment that we see in the return expectations data. Since these are professional forecasts, this also gives us another cross-comparison of individuals versus professionals.

We focus on forecasts of GDP, unemployment, the 3-month T-bill rate and inflation. Since CPI inflation forecasts are available only from 1981 onwards, we consider also forecasts of the GDP deflator, which are available from 1968 onwards, thus cover the run-up of inflation rates during the 1970's as well as the subsequent reversal.

To test for the presence of risk-adjustments, we need to take a stand on how the values of the different variables are associated with "good times" and "bad times", i.e., with "low" and "high" marginal utility of consumption. This is relatively straightforward for GDP and unemployment: marginal-utility weighting induces underestimation of future GDP growth and

overestimation of future unemployment. For nominal interest rates and inflation, one also obtain underestimation, provided that inflation and interest rates are higher during times of economic expansions, as seems reasonable to assume.¹⁴

The SPF is a quarterly survey that starts in 1968:Q4. We use the data until the fourth quarter of 2019. The survey is sent to forecasters after the first release of GDP for the previous quarter. The forecasters provide quarterly forecasts for five quarters ($t, t + 1, \dots, t + 4$, where t is the survey quarter). GDP projections are available from 1968:Q4 and are transformed into quarter-over-quarter growth rates using the median forecast and expressed in annualized terms. Civilian unemployment projections are for the quarterly average of the underlying monthly unemployment rate and expressed in percentage points. They are also available from 1968:Q4. Interest rate forecasts are available only from 1981:Q3 and are for the quarterly average level of the daily observations of the 3-month Treasury bill rate, expressed in annualized percentage points. The CPI forecasts are for the quarter-over-quarter change in the quarterly-average index level. Forecasts are based on the median response of survey participants, expressed in annualized percentage points and available from 1981:Q3. The GDP price index forecasts are in levels. These forecasts are transformed into quarter-over-quarter growth rates, using the median forecast, and expressed in annualized percentage points. The GDP price index forecasts are available from 1968:Q4. For our analysis, we use the SPF forecast error files, with realizations of the forecasted variables based on the latest vintage data.¹⁵

Table 7 reports the outcomes of the unconditional pessimism test (PE-U1). It shows that there is evidence for unconditional pessimism both in the current quarter forecast of GDP growth and the current quarter forecast of the unemployment rate. The evidence is statistically significant at around the 1% level, but the magnitude is quite small. The point estimate implies that the GDP growth forecast is on average underestimated by about 0.1 p.p. in terms of the (non-annualized) quarter-on-quarter growth rate. The unemployment rate is overestimated by a miniscule amount (0.03 p.p.). Moreover, going to longer forecasting horizons, the bias switches sign. For quarter $t + 4$, the bias is roughly the same magnitude, but opposite sign as for quarter t , albeit not statistically significant at conventional levels because standard errors are larger at longer horizons.¹⁶ For the T-bill rate, we consistently reject pessimism, with the optimism bias increasing with the forecast horizon. We suspect, however, this finding to be an artefact of the sample period, which starts in 1981 and thus only covers the period in which interest rates have displayed a sustained downward trend. This is partly confirmed by the fact that the CPI inflation series, which covers the same sample period as the T-bill series, similarly displays a statistically significant optimism bias, which is also increasing with the forecast horizon. For the longer sample period covered by the GDP deflator, we do not find statistically significant forecast biases. Overall, these findings are broadly similar to our earlier return expectations results in that we do not find clear evidence for a systematic pessimism bias.

Table 7 reports also outcomes obtained from the conditional pessimism test (PE-C1). For GDP, unemployment and the T-bill rate, we use the term spread between the 10-year and 3-month treasury yield as conditioning variable, following Rudebusch and Williams (2009).¹⁷ For CPI inflation and the GDP deflator we use the lagged output gap.¹⁸ While we can reject for some observations optimism about GDP and unemployment for short prediction horizons, we can often reject pessimism for the T-bill and CPI inflation forecasts. For the forecasts of the GDP deflator, we can reject pessimism slightly more often than optimism. The finding that we can reject conditional optimism in some periods and reject conditional pessimism in other periods is also in line with what we found for return expectations. Overall, systematic risk-adjustments seem to be largely absent not only in return expectations of individuals, but also in macro forecasts of professionals.

6. Conclusion

Our empirical findings show that subjective stock return expectations from a number of different surveys are not consistent with the idea that survey respondents report expectations under a risk-neutral probability measure. We show that both the unconditional and conditional properties of subjective return expectations are inconsistent with the risk-neutral expectations hypothesis, which predicts that subjective expected returns are equal to a maturity-matched risk-free rate. Allowing for differences in borrowing and lending rates or restricting the sample to individuals who are unlikely to be borrowing constrained does not change this basic conclusion.

More generally, we don't find evidence that individuals report risk-adjusted expectations that are pessimistically distorted relative to the empirical distribution of stock returns. Unconditionally, average subjective expected returns are close to average realized returns without a significant bias. Conditionally, there are substantial deviations of subjective expected returns

¹⁴ When inflation is instead varying due to pure nominal shocks, the situation is more difficult: households with positive net nominal positions will suffer a wealth loss from positive inflation surprises, while households with a negative nominal position, e.g., mortgage holders, experience a wealth gain. The pessimism hypothesis then implies that the former households overestimate inflation, while the latter underestimate it. Testing this implication is, however, not feasible using SPF forecasts.

¹⁵ The Federal Reserve Bank of Philadelphia provides data files containing forecast errors by matching the median forecasts with the appropriate realizations. The data is available at <https://www.philadelphiafed.org/research-and-data/real-time-center/survey-of-professional-forecasters/data-files/error-statistics>.

¹⁶ Giordani and Söderlind (2006) find evidence for pessimism in SPF GDP forecasts at horizons beyond quarter t and they speculate that this may be a consequence of several years of unexpectedly good GDP growth in the 1990s. Our results are consistent with this interpretation.

¹⁷ We condition on averages of the daily spread data during the quarter preceding the survey quarters.

¹⁸ The output gap is computed as the relative difference between actual and potential levels of real GDP estimated by the U.S. Bureau of Economic Analysis and the U.S. Congressional Budget Office, respectively. The quarterly output gap series is available in the FRED database at the Federal Reserve Bank of St. Louis (<https://fred.stlouisfed.org/graph?g=f1cZ>).

from the objective expected returns generated by empirical predictability regressions, but these deviations are optimistic in some periods and pessimistic in others, and they cancel out on average.

This predictable time variation in subjective expectations error around an unconditional mean of approximately zero is the most interesting property of these aggregated return expectations series and an important topic for future research. For example, learning from experience (Malmendier and Nagel, 2011; 2015), return extrapolation (Barberis et al., 2015), or learning from price growth (Adam et al., 2017; 2016) could contribute to these time-varying subjective expectations errors. An exploration whether the conditional biases in stock return expectations covary with forecast errors for other variables could also help to shed more light on the source of these errors. First steps in this direction, albeit for forecast variables other than returns, have been undertaken in Bhandari et al. (2019) and Kamdar (2018).

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:[10.1016/j.jmoneco.2020.04.010](https://doi.org/10.1016/j.jmoneco.2020.04.010).

CRedit authorship contribution statement

Klaus Adam: Writing - original draft. **Dmitry Matveev:** Writing - original draft. **Stefan Nagel:** Writing - original draft.

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