Queen's Economics Department Working Paper No. 1144

The Curse of Irving Fisher (Professional Forecasters' Version)

Gregor W. Smith
Queen's University

James Yetman
University of Hong Kong

Department of Economics
Queen's University
94 University Avenue
Kingston, Ontario, Canada
K7L 3N6

11-2007

The Curse of Irving Fisher (Professional Forecasters' Version)

Gregor W. Smith and James Yetman†

November 2007

Abstract

Dynamic Euler equations restrict multivariate forecasts. Thus a range of links between macroeconomic variables can be studied by seeing whether they hold within the multivariate predictions of professional forecasters. We illustrate this novel way of testing theory by studying the links between forecasts of U.S. nominal interest rates, inflation, and real consumption growth since 1981. By using forecast data for *both* returns and macroeconomic fundamentals, we use the complete cross-section of forecasts, rather than the median. The *Survey of Professional Forecasters* yields a three-dimensional panel, across quarters, forecasters, and forecast horizons. This approach yields 14727 observations, much greater than the 107 time series observations. The resulting precision reveals a significant, negative relationship between consumption growth and interest rates.

JEL classification: E17, E21, E43

Keywords: forecast survey, asset pricing, Fisher effect

†Smith: Department of Economics, Queen's University; smithgw@econ.queensu.ca. Yetman: School of Economics and Finance, University of Hong Kong; jyetman@econ.hku.hk.
We thank the Social Sciences Research Council of Canada and the Bank of Canada research fellowship programme for support of this research. The opinions are the authors' alone and are not those of the Bank of Canada. Smith thanks the Department of Economics at UBC for hospitality while this research was undertaken. We thank Robert Dimand for helpful comments.

1. Introduction

Dynamic Euler equations restrict multivariate forecasts. The aim of this paper is to study an example of these restrictions applied to professional forecasts and so introduce a new way to test these key building blocks of dynamic economic models. Our application is to the CCAPM, both because forecast data are available for its variables and because the results can thus be benchmarked against many studies using historical data. To the extent that the results are another nail in the coffin of the CCAPM, we hope that the reader will focus on the interesting new nail rather than the familiar coffin.

Economists of course have previously used forecast survey data in estimating and testing asset-pricing models. For example, exchange-rate forecasts have been used in testing uncovered interest parity and measuring risk premia in the foreign exchange market. Analysts' forecasts of firm cash flows or other variables have been used to measure surprises that affect stock prices. But these studies generally study the link between the *median* forecast of a fundamental and an asset price or return. The median is adopted either because individual forecasts are not available (as in the MMS survey) or because some summary statistic must perforce be selected for use in a statistical model.

The main innovation of this paper is to use forecasts both for the fundamentals and for the asset returns. We use *only* forecast data. As a result, we can use the entire cross-section of individual forecasts and so add many observations to the statistical problem of estimating parameters and testing the model.

This approach raises two questions. With no realized data, are we still estimating the parameters of interest? Are there efficiency gains from this approach? We answer yes to both questions. The first answer simply uses the law of iterated expectations, where we take an Euler equation and project it on the forecasters' information set (actually, the forecasters do the projecting for us). The second answer follows from our empirical comparison of our approach with an application of traditional tests and estimates for the same series and time periods. In that comparison we find that our standard errors are more than ten times smaller than those of the traditional approach that uses only the realized data.

One hundred years ago, Irving Fisher (1907) introduced his famous, two-period diagram in *The Rate of Interest* (appendix to chapter VII, pp 374-394 and appendix to chapter VIII, pp 395-415) to describe a household's saving choice. Fisher's analysis linked the nominal interest rate to the inflation rate and to the growth rate of real consumption. Formalized as the Euler equation that links gross returns on assets to an intertemporal, marginal rate of substitution (IMRS), this relationship still is a component of many dynamic, economic models.

For the past twenty-five years, economists have studied this relationship extensively using data on consumption (or other variables that affect marginal utility) and asset returns. Cochrane (2001) provides a complete review of theory and evidence. The simplest versions based on CRRA utility often can be rejected in aggregate data. This is the curse of Irving Fisher. But research continues with this relationship underpinning predictions for all sorts of properties of saving and of returns.

Our study investigates whether professional forecasts reflect a version of the link between macroeconomic variables and interest rates. After all, forecasters are paid to filter information and to make accurate predictions. It is interesting to see whether their forecasts implicitly link returns with inflation or with the real side of the economy. If these links held in the data, then using them to link forecasts would improve accuracy and precision. And one might even imagine an evolutionary process in which forecasters that prosper are those whose forecasts reflect the structure of the economy, so that over time the forecasts of surviving forecasters tend to more closely mimic this structure.

Our application can be seen as a test of the consumption-based capital-asset-pricing model (CCAPM). Its over-identifying restrictions can be rejected in forecast data, just as often happens in realized data. But our main aim is to suggest a new way of testing any asset-pricing model. This method provides much greater precision by using the cross-sectional variation in information sets across forecasters. Thus it seems promising as a way to discriminate between models or to precisely parametrize them.

Section 2 explains the method proposed in the paper, and contrasts it to existing methods. Section 3 describes the data, drawn from the *Survey of Professional Forecasters*.

Section 4 then outlines a standard asset-pricing model that links multivariate forecasts. Sections 5 and 6 test for these links, first under a log-normal assumption and then using a non-parametric, rank test. Section 7 contrasts the findings with those from standard GMM estimation of the Euler equation using the historical data. Section 8 concludes.

2. The Method

The simplest way to describe the method we use is with an example. Our example is heuristic only in that it uses the simplest possible economic example (simpler than the one we use later in the application). But the example illustrates all of the econometric ideas. At the same time it allows us to set our approach in the context of existing research.

Let the index t count quarters from 1 to T. Suppose that a theory predicts a linear relationship between an interest rate, r_t, and the expectation of the next period's inflation rate, π_{t+1}. Define $\mathcal{F}_t$ as the information available in the market and reflected in bond returns. We use E_t as a shorthand for an expectation conditional on $\mathcal{F}_t$. Thus the relationship to be studied is:

$$r_t = d + b_\pi E_t \pi_{t+1}. \tag{1}$$

Suppose that the investigator wishes to estimate and test this relationship without fully specifying the law of motion for the inflation rate *i.e.* in a single-equation or limited-information context.

A traditional approach (which we shall call method 1) to this problem involves estimation by instrumental variables. The realized value π_{t+1} is substituted for the unobservable expectation, then projected on instruments z_t that lie in $\mathcal{F}_t$. The fitted value is then used in the estimating equation:

$$r_t = d + b_\pi E[\pi_{t+1}|z_t], \tag{2}$$

Then the estimator is two-stage least squares or more generally GMM/GIVE. McCallum (1976) and Pagan (1984) are classic references.

One practical difficulty with this method is that it may be challenging to find relevant instruments. When instruments are weak the two-stage least-squares estimator is biased towards OLS, its distribution is non-normal, and standard confidence intervals can be misleading. Dufour (2003) and Andrews and Stock (2005) survey and extend work on this syndrome. There are tests (and, to a lesser extent, estimators) that are robust to weak identification, and one can use them to form confidence intervals with good coverage properties. But naturally these intervals can still be wide when the instruments are weak.

Our application in this paper is to the CCAPM, where the weak-instrument problem arises because consumption growth and inflation are difficult to forecast. Neeley, Roy, and Whiteman (2001), Stock and Wright (2000), and Yogo (2004) all show that weak instruments are a problem for this specific combination of estimation method and asset-pricing equation.

An alternative, widely-used approach (which we shall call method 2) uses forecast survey data. Suppose the investigator has a panel of forecasts reported in a survey, by J forecasters indexed by j. The information set of forecaster j is denoted $\mathcal{F}_{jt}$. Researchers most often use the median forecast, here denoted $\overline{E}_{jt}\pi_{t+1}$, and substitute it in the theory (1) to give the estimating equation:

$$r_t = d + b_\pi \overline{E}_{jt}\pi_{t+1}. \tag{3}$$

Some researchers assume this median is error-laden and so they instrument it, too. A wide range of interesting studies have used this method, either because only the median is available or because some statistic from the cross-section must be chosen. In the latter case the median also can be compared to or augmented with other statistics.

Our goal is not to criticize the use of the median but rather to explore whether more information can be used. Nevertheless, researchers who test for unbiasedness or accuracy argue that one should avoid the median, for it does not reflect any specific information set to which unbiasedness should apply. Figlewski and Wachtel (1983), Keane and Runkle (1990), and Thomas (1999) develop this argument. The median of many forecasts is not the forecast given any information set. The same argument applies here. Estimation using the sample versions of equations like these is based on the the law of iterated expectations and the idea that the sample mean forecast error converges to the population error, zero, as T grows. But these are properties of rational individual forecasts, and not necessarily of the median forecast.

One can justify using an individual forecast $E_{jt}\pi_{t+1}$ rather than the unobservable $E_t\pi_{t+1}$ by assuming plausibly that $\mathcal{F}_{jt} \subset \mathcal{F}_t$ and so appealing to the law of iterated expectations. Thus

$$r_t = d + b_\pi E_{jt}\pi_{t+1} + b_\pi \eta_t, \tag{4}$$

with the residual

$$\eta_t = E_t\pi_{t+1} - E_{jt}\pi_{t+1}$$

thus being uncorrelated with the regressor. Estimating the J equations (4) as a system is known in the rationality-testing literature as pooling. In such a panel there is an equation for each forecaster, but with the same dependent variable. However, Zarnowitz (1985) and Bonham and Cohen (2001) have argued that the least-squares estimator of b_π is inconsistent, due to the common dependent variable in the cross-section. Their argument referred to tests of unbiasedness but also applies here, even though the dependent variable is the return rather than the realized inflation rate. It is intuitive that – with a common b_π – forecasters with high values of $E_{jt}\pi_{t+1}$ will have low values of the residual. This cross-sectional dependence makes ordinary least squares inconsistent. One can avoid this inconsistency by estimating (4) for each individual forecaster with a forecaster-specific $b_{\pi j}$ and comparing the results. But this does not provide an overall estimate or test.

Another possibility is to include several different forecasts, say from forecasters 1 and 2, in the statistical model, like this:

$$r_t = d + b_\pi[gE_{1t}\pi_{t+1} + (1-g)E_{2t}\pi_{t+1}] \tag{5}$$

and to estimate the weight g at the same time as $\{d, b_\pi\}$. This is pooling in the classic sense of Bates and Granger (1969). Gottfries and Persson (1988) provide the theoretical underpinning for this method, using the recursive projection formula, and Smith (2007) provides examples. However, one cannot include all the J forecasts in one regression without exhausting degrees of freedom, unless J is much smaller than T. Overall, then, information on the cross-section cannot be exploited completely in method 2.

In method 3, our approach in this paper, we project *both* sides of the theory (1) on $\mathcal{F}_{jt}$ (or rather professional forecasters do) to give:

$$E_{jt} r_t = d + b_\pi E_{jt} \pi_{t+1}, \tag{6}$$

because we have aligned forecasts made by each forecaster for both variables at the same time and for the same time. We then estimate the J equations (6) with the panel of forecasts. If there are no missing observations then this has $J \times T$ observations. The pooled slope b_π is common to all equations. We let the professional forecasters find instruments and do the forecasting. Thus if there is relevant, cross-sectional variation in how they do this, then b_π can be estimated consistently and with greater precision than in methods 1 or 2. Method 3 may be particularly helpful when J is large relative to T, when there are regime changes so that T is limited, or when there is relatively little time-series variation in π_t, say during successful episodes of inflation-targeting.

Another feature of forecast surveys further enlarges the number of observations. The surveys typically include forecasts made for the same variables at different horizons. If the number of horizons is H then the sample size potentially is $H \times J \times T$ in method 3 as opposed to T in method 1.

In fairness, we may be overstating this contrast by comparing $H \times J \times T$ to T, for two reasons. First, panels of forecasts usually are unbalanced; there are numerous missing observations. Second, we could repeat method 1 with different sets of instruments and with different horizons, thus raising the number of effective observations in that approach. But finding different sets of valid instruments for Euler equations has not always been easy. Moreover, when we use forecast survey data we have two added advantages. The forecasts are real-time data, measured at the time for which they apply and so using no information announced thereafter. And using these forecasts involves no generated regressor problem. They are the expectations of (some) agents, not our estimates of those expectations. We do not know what parameters or instruments the forecasters used but we do not need to know since we have their forecasts.

Our study is not directly related to work that investigates the accuracy of multivariate or real-time forecasts (such as Bauer, Eisenbeis, Waggoner, and Zha (2003) or Croushore

(2006)), the diffusion of information into forecasts (such as Carroll (2003) or Bauer, Eisenbeis, Waggoner, and Zha (2006), or the disagreement among forecasters (such as Mankiw, Reis, and Wolfers (2005)). But that research certainly shows that there is heterogeneity among forecasters, which is the characteristic that makes method 3 of interest.

3. SPF Data

In the application, the source for the panel data is the *Survey of Professional Forecasters* (SPF) conducted by the Federal Reserve Bank of Philadelphia (`www.philadelphiafed.org/econ/spf/`). The data are quarterly, and run from 1981:1 to 2007:3. Quarters, indexed by t, run from 1 to $T = 107$.

Forecast horizons also are quarterly. Forecasts are reported for the previous quarter, the current quarter, and the following four quarters. Horizons are indexed by h, which counts from 0 (applicable to the previous quarter) to $H = 5$.

The survey uses a cross-section of forecasters, indexed by j which runs from 1 to J. We include all forecasters who make predictions for at least two observations on all three variables that we study, a criterion that gives $J = 171$. No forecaster made predictions for all 107 observations. The maximum number of observations predicted was 94 and the average was 15. Given the missing observations the number of jt combinations is 2984. Finally, we include only horizons for which there are predictions for all three variables. Most observations do include predictions for all 5 horizons, so the total number of observations, or hjt combinations, is 14727. This total is 140 times greater than the number of quarterly time series observations. Figure 1 shows the histogram of forecasts per forecaster.

We study forecasts for three variables (listed with their SPF codes in brackets): π, the CPI inflation rate, quarter-to-quarter, seasonally adjusted, at annual rates, in percentage points (`cpi`); x, the growth rate of real personal consumption expenditures, quarter-to-quarter, annualized, in percentage points (calculated from the level forecasts `rconsum`); and r, the quarterly average 3-month treasury bill rate in percentage points (`tbill`). We work with this definition of r so that the maturity coincides with the frequency of data. An alternate measure of inflation uses the deflator for personal consumption expenditure;

but that series begins only in 2007. Alternate bond yields in the survey are the AAA corporate bond yield and the yield-to-maturity on a 10-year treasury bond; but according to theory these are less directly tied to the quarterly inflation and consumption growth forecasts than is the T-bill return.

For a typical variable, say r, the forecast of the value at time t by forecaster j, h quarters in advance is denoted $\mathrm{E}_{jt-h} r_t$. The standard Fisher effect relates the nominal interest rate, r_t, to the inflation rate over the ensuing time period, π_{t+1}. Thus if such an effect holds in forecasts then it would link $\mathrm{E}_{jt-h} r_t$ to $\mathrm{E}_{jt-h} \pi_{t+1}$ for example. Before examining those links empirically, we first derive them from a version of the CCAPM, which thus includes consumption growth x_{t+1} in this relationship too.

4. Asset Pricing

Not every hypothesized link between economic variables can be tested using macroeconomic forecasts. For example, one would not try to test a decision rule in forecasts, for its coefficients would not necessarily coincide with those in the reduced-form solution. But Euler equations linking endogenous variables can be used for estimation and testing in this way. They apply whatever the structure of the rest of an economic model, and can be tested in multi-step forecasts because of the law of iterated expectations.

We try to interpret the links between the three forecasts using the CCAPM, because of the wealth of existing evidence and because r is a market interest rate rather than the policy interest rate (the federal funds rate). Thus one could not use forecasts of this interest rate to try to uncover forecasts of a policy rule, for example.

We first extend the notation of section 2 to formally describe the information known by each forecaster. Suppose that $\{r_t, x_t, \pi_t\}$ are adapted to each of J filtrations $F_j = \{\mathcal{F}_{jt} : t \in [0, \infty)\}$ where $\mathcal{F}_{jt}$ is a non-decreasing sequence of sub-tribes on a probability space $(\Omega_j, \mathcal{F}_j, P_j)$. Thus each forecaster observes current and past values of these three variables, total information accrues over time, and forecasters may have different information sets, in that $\mathcal{F}_{jt}$ is not simply generated from these three variables but may reflect other sources of information. For example, some forecasters may look at many disaggregated series before making their forecasts, while others may use large statistical models. In addition, the expectation that determines the market interest rate is based on an information set, denoted $\mathcal{F}_t$, that is larger than that of any forecaster: $\mathcal{F}_{jt} \subset \mathcal{F}_t \, \forall \, j$.

Denote the nominal return on a riskless, discount bond issued at time t and maturing at time $t+1$ by r_t. Suppose that investors have CRRA utility in real consumption c_t. The discount factor is β and the coefficient of relative risk aversion is α. These parameters are constants that describe market participants, so they do not depend on j. Denote the growth rate of consumption x_t and the growth rate of prices π_t. Then the three variables are linked by the Euler equation:

$$\mathrm{E}\left[\frac{\beta(1+r_t)}{(1+x_{t+1})^{\alpha}(1+\pi_{t+1})}|\mathcal{F}_t\right] = 1, \tag{7}$$

where the subscripts reflect the fact that the nominal interest rate is known at the beginning of the time period. By the law of iterated expectations, the predictions of forecaster j thus satisfy:

$$\mathrm{E}\left[\frac{\beta(1+r_t)}{(1+x_{t+1})^{\alpha}(1+\pi_{t+1})}|\mathcal{F}_{jt}\right] = 1. \tag{8}$$

For simplicity, from now on we denote a conditional expectation by E_{jt}. Notice that this restriction (8) across forecasts for several variables by a given forecaster does not imply that forecasters make identical forecasts.

Since the filtrations are non-decreasing over time, the law of iterated expectations again applies, so that if we consider forecasts of this same combination of variables that are made in earlier time periods (at longer horizons), then:

$$\mathrm{E}_{jt-h}\left[\frac{\beta(1+r_t)}{(1+x_{t+1})^{\alpha}(1+\pi_{t+1})}\right] = 1, \tag{9}$$

for $h \geq 0$. When $h = 0$ the theory connects actual interest rates with one-step-ahead forecasts of inflation and consumption growth. For longer horizons (forecasts made at earlier dates) all three variables are forecasted.

Recall that the data consist of $H \times J \times T$ observations on the forecasts $\{\mathrm{E}_{jt-h}r_t$, $\mathrm{E}_{jt-h}x_{t+1}, \mathrm{E}_{jt-h}\pi_{t+1}\}$. Because of Jensen's inequality we cannot immediately match these up with the theory (9). But under additional assumptions we can use the data to estimate parameters and test this relationship. First, we can make a distributional assumption that makes the asset-pricing restrictions (9) directly testable using forecast data. Second, we test a necessary condition for this necessary condition, in the form of a non-parametric test based on ranks. The next two sections outline these approaches in turn.

5. Log-Normality

The distributional assumption is that the logarithm of the composite random variable in the asset-pricing model is normally distributed. This assumption has a history of constructive use in asset-pricing, including contributions by Hansen and Singleton (1983), Campbell (1986), and Campbell and Cochrane (1999). Our specific application uses *conditional, joint* log-normality. So suppose that the composite variable:

$$\frac{(1+r_t)}{(1+x_{t+1})^{\alpha}(1+\pi_{t+1})} \tag{10}$$

conditional on $\mathcal{F}_{jt-h}$ is log normal with mean μ_{jt-h} and variance σ^2_{hj}. Combining this distribution with the pricing equation (9) gives:

$$\exp[\mu_{jt-h} + 0.5\sigma^2_{hj}] = \frac{1}{\beta}, \tag{11}$$

from the properties of the log-normal density, so that

$$\mu_{jt-h} = -\ln\beta - 0.5\sigma^2_{hj}. \tag{12}$$

Finally, we use the property that for small x, $\ln(1+x) \approx x$. This approximation worsens at high interest rates. The *SPF* data include forecasts for the high inflation rates and high interest rates of the early 1980s, so we also study shorter samples that begin in 1984 or 1990. With this approximation, the conditional mean is:

$$\begin{aligned}\mu_{jt-h} &\equiv \mathrm{E}_{jt-h}\ln(1+r_t) - \mathrm{E}_{jt-h}\ln(1+\pi_{t+1}) - \alpha\mathrm{E}_{jt-h}\ln(1+x_{t+1}) \\ &\approx \mathrm{E}_{jt-h}r_t - \mathrm{E}_{jt-h}\pi_{t+1} - \alpha\mathrm{E}_{jt-h}x_{t+1}.\end{aligned} \tag{13}$$

Combining (12) and (13) gives:

$$\mathrm{E}_{jt-h}r_t = \mathrm{E}_{jt-h}\pi_{t+1} + \alpha\mathrm{E}_{jt-h}x_{t+1} - \ln\beta - 0.5\sigma^2_{hj}. \tag{14}$$

This expression uses the forecasts for the three variables we actually have, and applies for any horizon.

Notice that we could have used the approximation $\ln(1+x) \approx x$ directly on the forecasted, combination of variables (9) and then applied the expectations operator to reach a linear relationship much like the result (13). But using log-normality before the approximation yields valuable information in the form of the term σ^2_{hj}, which provides an economic interpretation for heterogeneity in the intercept terms. To capture this, we use d_{hj} to denote a set of 0-1 dummy variables (*i.e.* fixed effects) that may vary with each of the subscripts: the horizon and forecaster. The estimating equations then are:

$$\mathrm{E}_{jt-h} r_t = d_{hj} + b_\pi \mathrm{E}_{jt-h} \pi_{t+1} + b_x \mathrm{E}_{jt-h} x_{t+1}. \tag{15}$$

We find estimates $\{\hat{b}_\pi, \hat{b}_x\}$ and also estimates $\hat{b}_x$ with $b_\pi = 1$ imposed.

Of course the coefficients $\{b_x, b_\pi, d_{hj}\}$ are estimates of the parameters connecting the three forecasts. But a stronger statement can be made about them: they are consistent estimates of the underlying economic parameters, and in particular $\hat{b}_x$ is an estimate of the utility parameter α. According to the theory, the parameter on the inflation forecast is $b_\pi = 1$. To see this, note that the estimating equations are based on a projection (8) of the asset-pricing model (7) onto the forecasters' information, just as in standard GMM/IVE estimation. As long as forecasters have rational expectations, then, the Euler-equation residuals are orthogonal to the regressors. Thus least-squares is consistent, and efficient given the information set.

Our method involves a two-step estimator, but it does not involve a generated regressor problem that requires us to correct the standard errors. In the traditional two-step approach, method 1 of section 2, the econometrician first constructs $E[\pi_{t+1}|z_t]$, say, then substitutes this generated regressor into the original equation (1). The OLS formula understates the standard errors because it neglects the sampling uncertainty associated with the fact that the econometrician has estimates of the parameters in the first step, rather than known values. Pagan (1984) described how to do correct inference. In our case, step one is conducted by the professional forecasters. Their expectations then are reported to the Federal Reserve Bank of Philadelphia; we do not need to estimate any parameters associated with them. Thus the OLS standard errors are correct.

The theory (14) shows that we cannot identify the discount factor β, for there are $H \times J + 1$ constants but one less estimated intercept term. But we can allow the intercept to depend on the horizon h and forecaster j, as the theoretical derivation suggests. We thus use fixed effects d_{hj}, an approach which pools the time-series observations, but does not place restrictions on the intercepts across forecasters or horizons. This setup allows for forecast uncertainty to rise with the horizon and to vary with the forecaster, just as the variance terms in the theory are indexed by hj.

We also investigate more restrictive fixed effects. When we use d_h and d_j separately (reducing the number of fixed effects from $H \times J$ to $H + J$) the constant term varies by forecaster but follows the same pattern over horizons for each forecaster. When we use only d_h there is no forecaster-specific term. When we use only d_j the intercepts do not vary with the horizon. Our most restrictive estimation uses d, an intercept that is common across all dimensions of the panel.

Estimation is by generalized least squares, which is necessary because the panel is unbalanced. Roughly speaking, the weight on forecasting entity j is given by the square root of the number of observations it forecasts. As a result, minimizing the sum of squared residuals receives a larger weight the larger the number of forecasts, as is appropriate given the reduced sampling uncertainty in that case. Standard errors are robust to heteroskedasticity and autocorrelation.

We observed that the data contain some hard-to-believe observations for the early 1980s. For example, there are some reports of last quarter's interest rate that are different by many basis points from the actual interest rate, and some multi-horizon forecasts of quarterly inflation that are quite different from the same forecaster's prediction for annual inflation. To ensure that the results are not driven by these observations, we also estimate over sub-samples defined as follows. First, we delete observations in which r_{0jt} differs from the mode over j for that observation and horizon by more than 1 percentage point or π_{1jt} differs from the mode over j by more than 3 percentage points. Second, we replace r_{0jt}

with the actual, quarterly average T-bill rate from the previous quarter. Third, we study samples that begin in 1984 and in 1990.

We would wish to test the underlying assumption of log normality. According to the theory, the log forecast errors (constructed using the realized data and the forecasts) are normal with mean zero and variance σ_{hj}^2. That means that the complete set of these forecast errors is distributed as a mixture of normals with the same mean but different variances. As a result, they need *not* be normally distributed. Sure enough, when we undertake an omnibus, multivariate test we find that the density has fat tails. According to the theory, for a given horizon h and forecaster j the density *is* normal, but the typical number of observations in bins sorted by hj is about 15. Even if we used graphical methods to assess the normality of hundreds of densities, with so few observations per density the power of such tests would be low. Consequently, we proceed to report findings from the estimation (and some other tests) but then also look at non-parametric results in the next section that do not rely on this distributional assumption.

Table 1 contains results, based on the entire sample of 14727 observations. The findings based on sub-samples (omitting outliers, using actual r_{0t}, and studying sub-periods of time) are very similar and so are not reported. There also is no evidence of horizon-specific fixed-effects, d_h, that are common across forecasters. As is clear from the R^2 values in table 1, the hypothesis that the statistical model includes only forecaster-fixed-effects, d_j, cannot be rejected; the p-value is 0.27. This variation over forecasters is consistent with the log-normal model, according to which it captures σ_{hj}. But restricting the intercept further, to d, can be rejected; the p-value is 0.00.

Finding no role for d_h is surprising when viewed through the lens of the log-normal model. There, the intercept includes σ_{hj}^2, which we would expect to rise with the horizon h. But it is less surprising if one simply thinks about the patterns in the three forecasts. Finding a role for d_h would mean that the difference between an interest-rate forecast, on the one hand, and inflation and consumption-growth forecasts, on the other, varied systematically with the horizon when averaged over the time periods. It would also mean that this pattern was systematic over forecasters. From this perspective, it would instead be surprising to find $\hat{d}_h$ to be significantly different from zero.

Focusing on the key, middle line of table 1 then, for the model including d_j, there are three main economic findings. First, variation in forecasts of inflation and consumption-growth explains 62 percent of the variation over time and horizons in the interest-rate forecasts. Thus, there is clearly a multivariate link between these forecasts. And both regressors are significant; the p-values are 0.00 and 0.01.

Second, there is a positive coefficient $\hat{b}_\pi$, but we can readily reject the hypothesis that $b_\pi = 1$. (Moreover, imposing this restriction does not change $\hat{b}_x$ significantly.) Interest-rate forecasts do not seem to respond 1:1 to changes in inflation forecasts.

Third, there is a *negative* coefficient $\hat{b}_x$ and we can readily reject the hypothesis that $b_x = 0$ with a one-tailed test. This is the curse of Irving Fisher (professional forecasters' version). This method detects a significant, time-varying real interest rate, but it is not related to consumption growth the way theory predicts.

Given this third finding, one might ask whether any individual forecaster produced forecasts consistent with the theory. We next allow for slope values b_{xj} and $b_{\pi j}$ that are specific to forecaster j. The estimating equations are:

$$\mathrm{E}_{jt-h} r_t = d_j + b_{\pi j} \mathrm{E}_{jt-h} \pi_{t+1} + b_{xj} \mathrm{E}_{jt-h} x_{t+1}. \tag{16}$$

When we test the pooling restrictions that $b_{xj} = b_x$ and $b_{\pi j} = b_\pi$ they are rejected with a p-value of 0.00 and the R^2 value rises from 0.618 to 0.671. Obviously then, there is statistically significant heterogeneity in these slopes.

Figure 2 plots the pairs of point estimates, $\{\hat{b}_{xj}, \hat{b}_{\pi j}\}$ for $j = 1, \ldots, 171$. Figure 2 shows that a majority of forecasters have small, negative values of the coefficient $\hat{b}_{xj}$, on the consumption growth forecast. That means that – although there are a handful of forecasters with $b_{xj} < -1$ – those outliers are not driving the result from table 1 that the pooled value is negative. As for the coefficient $b_{\pi j}$ on the inflation forecast, figure 2 shows that there is a surprising variability in that value across forecasters. For a number of forecasters, the value is not only well below 1 but even less than zero.

Using these point estimates and robust standard errors, we next calculated the t-statistics for two hypotheses; the first an upper, one-tailed test that $b_{xj} = 0$ and second a two-tailed test that $b_{\pi j} = 1$. The t-statistic for each forecaster uses degrees of freedom that reflect the number of observations and horizons predicted by that forecaster. In the first test, a low p-value, denoted p_{xj}, is evidence of a positive value for b_{xj}. Thus a low value of $1 - p_{xj}$ is evidence against the economic model. In the second test, a low p-value, denoted $p_{\pi j}$ is evidence of a $b_{\pi j}$ different than 1, and so is evidence against the economic model.

Figure 3 plots the pairs $\{1 - p_{xj}, p_{\pi j}\}$ for each of the 171 forecasters. It is scaled to be a square with the same units on both axes so that the findings on the two hypotheses can be directly compared. (Note, though, that these are not joint tests of the two hypotheses.) The figure can be read this way: if there were strong support for the economic model than many points would appear towards the north-east corner of the graph. But in fact relatively few points can be seen there. This is the disaggregated version of the curse of Irving Fisher.

Some forecasters might believe in a tax-adjusted version of the Fisher effect of inflation. When nominal interest income is taxable for all investors, the interest rate must move more than 1:1 with the inflation rate for real interest rates to be unaffected by inflation. With a broader definition of the Fisher effect to include some forecasters with $\hat{b}_{\pi j} > 1$, the proportion of forecasters whose predictions follow this link would rise. But figure 1 shows that $\hat{b}_{\pi j} > 1$ for relatively few forecasters, so the change would not be great.

Finally, we also calculated LM tests for residual autocorrelation over time. The p-values are low, which means that the time-series properties of interest rate forecasts, from quarter to quarter, do not align completely with those of inflation and consumption-growth forecasts. The persistence in residuals means that the errors from the implicit Euler equation contain a predictable component. That is further evidence against this dynamic Euler equation holding in forecast data. We also 'corrected' for first-order autocorrelation by estimating an ARMA(1,1) model with a common-factor restriction. In this statistical model, the value of b_π fell slightly, while b_x remained precisely estimated and negative.

6. Rank Regression

The utility function, distribution, and approximation in the previous example give rise to a linear relationship among the three forecasts. But any concave, increasing utility function in current consumption will give rise to a monotonic relationship, in which $\mathrm{E}_{jt-h}r_t$ is positively related to $\mathrm{E}_{jt-h}\pi_{t+1}$ and $\mathrm{E}_{jt-h}x_{t+1}$. We use rank regression to test this general implication of the theory, because a monotone relationship implies a linear relationship in ranks. Of course if the log-normal, CRRA version is an accurate guide then statistical efficiency will be lower when we switch to ranks. But with 14727 observations, allowing for weaker assumptions seems completely feasible.

The possibility of reporting errors in $\mathrm{E}_{jt-h}\pi_{t+1}$ and $\mathrm{E}_{jt-h}x_{t+1}$ provides a second rationale for this approach. If there are measurement errors on the right-hand side of the estimating equations, then the ordinary-least-squares estimator will be inconsistent. And the traditional, downward bias from measurement error might explain some of the findings of the previous section, such as estimates $\hat{b}_\pi$ that are less than 1. But measurement errors that are not large enough to change rankings will not lead to inconsistency in rank regression. Rank regression also is less sensitive to any oddball, outlier forecasts.

Let $R_{hjt}\colon \mathbf{R} \to \mathbf{N}^*$ be the function that maps forecasts into ranks. This pools all forecasters, time periods, and horizons in calculating ranks. Let d_{hj} again denote a set of $J \times H$ 0-1 dummy variables. Then consider the linear regression:

$$R_{hjt}(\mathrm{E}_{jt-h}r_t) = d_{hj} + \rho_\pi R_{hjt}(\mathrm{E}_{jt-h}\pi_{t+1}) + \rho_x R_{hjt}(\mathrm{E}_{jt-h}x_{t+1}). \tag{17}$$

Rank regression cannot identify b_π and b_x; instead the regression parameters are partial rank correlation coefficients, because all three rank series have equal variance. They thus can be used to test for any monotone association, but cannot estimate the scale of such an effect.

Ranks are found using the `egen rank` instruction in `STATA`. Ties are assigned the average of the two adjacent ranks. We again studied a range of fixed effects or intercepts. Table 2 shows rank regression results for three types of intercepts: d_{hj}, d_j, and d. As in the parametric case in table 1, F-tests would tell us to restrict these to d_j but no further.

And we experimented with ranking over only t or only h and j, for example. These results were very similar and so are not shown.

We find that $\hat{\rho}_\pi = 0.337$, so there is a positive association between inflation forecasts and interest-rate forecasts. And $\hat{\rho}_x = -.076$, so there again is a negative association between interest-rate forecasts and consumption-growth forecasts, controlling for the inflation forecast. Both effects are precisely estimated, thanks to the large sample size. This is the most general or robust version of the curse. In addition, the residuals from the rank regression were positively autocorrelated, as in the previous section.

7. Standard Estimation and Testing

We have argued that restricting multivariate forecasts provides a natural way to estimate parameters and test for their constancy across forecasters. So it seems natural to compare our empirical findings with those from the standard approach (method 1) that uses only the historical, realized data.

To do this, we measured interest rates, consumption, and prices the same way they are defined in the SPF. Consumption is real personal consumption expenditures, quarterly, seasonally adjusted: `pcecc96` from `FRED`. The interest rate is the the 3-month T-bill rate, on the secondary market, averaged from monthly data: `tb3ms`. The price level is the CPI (all items) seasonally adjusted, averaged from monthly data: `cpiaucsl`. Growth rates x and π are quarter-to-quarter. Estimation is from 1981:1 to 2007:3, just as in the forecast data.

We first estimated the sample version of:

$$\mathrm{E}\left[\frac{\beta(1+r_t)}{(1+x_{t+1})^\alpha(1+\pi_{t+1})}|z_t\right] = 1, \tag{18}$$

using instruments z_t, by iterated GMM. Table 1 contains the parameter estimates and their standard errors, along with the J-test statistic and its p-value. The rows of the table adopt various instrument sets and hence varying degrees of over-identification.

There are three main findings in table 1. First, we find positive, plausible, and precisely estimated discount factors $\hat{\beta}$, especially as we add instruments. Second, the J-test rejects the over-identifying restrictions of the CCAPM with p-values of 0.00. Third, the coefficient of relative risk aversion, α, is not estimated with precision. The sign varies with the instrument set, a sensitivity that is one of the hallmarks of weak identification.

In the last line of table 3 we use π_{t+1} and x_{t+1} as instruments, as if these can be perfectly forecast, so that the estimator is non-linear least squares. Naturally this estimator is biased because the realized, future values are correlated with the forecast error, but the precision of the least-squares estimator gives an upper bound on that of the GMM estimator. Yet even in this case $\hat{\alpha}$ is only slightly greater than its standard error.

While table 3 summarizes results of the typical approach to estimation and testing, our estimation with forecast data used the linear version suggested by the log-normal assumption. So, for comparison, we also estimate the linear model in historical data. The estimating equations are the sample versions of:

$$\mathrm{E}\big[\ln(1+r_t) - d - \ln(1+\pi_{t+1}) - b_x \ln(1+x_{t+1})|z_t\big] = 0. \tag{19}$$

We restrict the coefficient on inflation to be 1, as in table 3, and we use the same sets of instruments as we used there. Imposing this restriction makes identification easier and also gives us the best chance of finding a small standard error for $\hat{b}_x$.

The results are similar to those from table 3. The J-test rejects the CCAPM restrictions. Notably, with no instrumental variables estimator can we find a value for $\hat{b}_x$ that is greater than its standard error. Only when we use the inconsistent OLS estimator (and continue to impose $b_\pi = 1$) do we estimate b_x with any precision. And that precision is much less than we found in the forecast data of table 1.

These results again reflect the weak instrument problem, the difficulty of forecasting consumption growth and inflation with these current and lagged macroeconomic variables. In the linear model instrumental-variables estimation with the same number of instruments as regressors is the same as two-stage least squares. The first stage regressions of $\ln(1+\pi_{t+1})$ and $\ln(1+x_{t+1})$ on z_t have $\overline{R}^2$ values that range from 0.15 to 0.28, depending on the set of instruments. This imprecision is inherited in the large standard errors attached to $\hat{b}_x$.

The references on the CCAPM in section 2 provide tests that are robust to weak identification. They show that we do not need the forecast data in order to reject the over-identifying restrictions of the CCAPM for this time period and data. However, it is noteworthy that both approaches give the same conclusion about the theory. We might not pursue our interest in learning about Euler equations from forecast data if they suggested resounding support for the CCAPM that was not found in the historical data alone.

What then do we gain from using the forecast data? The answer is the much greater precision of estimates. The standard errors on $\hat{b}_x$ in table 1 are more than ten times smaller than those in table 4. We hope that this precision may be useful in other applications, to decisively distinguish between competing theories or to narrow confidence intervals for parameters in models that are not rejected.

8. Conclusion

Dynamic Euler equations automatically restrict multivariate forecasts. We have tested an example of these restrictions on the multivariate (and multi-horizon) predictions of professional forecasters. If such economic links are important, one would expect professional forecasters to incorporate them in their predictions. And economists have long used surveys of professional forecasters to test other features of macroeconomic models, such as the unbiasedness of statistical forecasting that is attributed to the market participants who inhabit those models. We find that interest-rate forecasts (a) move less than one-to-one with inflation forecasts and (b) are negatively related to forecasts of real consumption growth. These findings make for an indirect rejection of the specific asset-pricing model, by showing that forecasters do not follow its restrictions. But the use of forecast survey data provides much greater precision than does traditional estimation with the historical, realized data.

While we have applied this method to the CCAPM, it certainly can be used to study other asset-pricing models, given the wealth of data in the SPF and other surveys. Linear factor models of the stochastic discount factor seem to be natural candidates for study. But directly applying the law of iterated expectations requires that the factors themselves be linear in macroeconomic flow or price variables that are in the SPF, such as GDP growth or aggregate investment.

References

Andrews, Donald W. K. and James H. Stock (2005) Inference with weak instruments. Cowles Foundation discussion paper 1530.

Bates, J.M. and Clive W. Granger (1969) The combination of forecasts. *Operational Research Quarterly* 20, 451-468.

Bauer, Andy, Robert A. Eisenbies, Daniel F. Waggoner, and Tao Zha (2003) Forecast evaluation with cross-sectional data: The Blue Chip surveys. *Federal Reserve Bank of Atlanta Economic Review*, second quarter, 17-31

Bauer, Andy, Robert A. Eisenbies, Daniel F. Waggoner, and Tao Zha (2006) Transparency, expectations, and forecasts. Federal Reserve Bank of Atlanta Working Paper 2006-3.

Bonham, Carl and Richard H. Cohen (2001) To aggregate, pool, or neither: Testing the rational expectations hypothesis using survey data. *Journal of Business and Economic Statistics* 19, 278-291.

Campbell, John Y. (1986) Bond and stock returns in a simple exchange model. *Quarterly Journal of Economics* 101, 785-803.

Campbell, John Y. and John H. Cochrane (1999) By force of habit: A consumption-based explanation of aggregate stock market behavior. *Journal of Political Economy* 107, 205-251.

Carroll, Christopher (2003) Macroeconomic expectations of households and professional forecasters. *Quarterly Journal of Economics* 118, 269-298.

Cochrane, John H. (2001) *Asset Pricing.* Princeton University Press.

Croushore, Dean (2006) An evaluation of inflation forecasts from surveys using real-time data. Working Paper No. 06-19, Research Department, Federal Reserve Bank of Philadelphia.

Dufour, Jean-Marie (2003). Identification, weak instruments, and statistical inference in econometrics. *Canadian Journal of Economics* 36, 767-808.

Figlewski, Stephen and Paul Wachtel (1983) Rational expectations, informational efficiency, and tests using survey data: A reply. *Review of Economics and Statistics* 65, 529-531.

Fisher, Irving (1907) *The Rate of Interest.* New York: Macmillan.

Gottfries, Nils and Persson, Torsten (1988) Empirical examinations of the information sets of economic agents. *Quarterly Journal of Economics* 103, 251-259.

Hansen, Lars Peter and Kenneth J. Singleton (1983) Stochastic consumption, risk aversion, and the temporal behavior of asset returns. *Journal of Political Economy* 91, 249-65.

Keane, Michael P. and David E.Runkle (1990) Testing the rationality of price forecasts: New evidence from panel data. *American Economic Review* 80, 714-735.

Mankiw, N. Gregory, Ricardo Reis, and Justin Wolfers (2004) Disagreement about inflation expectations. *NBER Macroeconomics Annual 2003* 209-248.

McCallum, Bennett T. (1976) Rational expectations and the estimation of econometric models: An alternative procedure. *International Economic Review* 17, 484-90.

Neeley, Christopher J., Amlan Roy, and Charles H. Whiteman (2001) Risk aversion versus intertemporal substitution: A case study of identification failure in the intertemporal consumption capital asset pricing model. *Journal of Business and Economic Statistics* 19, 395-403.

Pagan, Adrian (1984) Econometric issues in the analysis of regressions with generated regressors. *International Economic Review* 25, 221-247.

Smith, Gregor W. (2007) Pooling forecasts in linear rational expectations models. Queen's Economics Department working paper 1129.

Stock, James H. and Jonathan H. Wright (2000) GMM with weak instruments. *Econometrica* 68, 1055-1096.

Thomas, Lloyd B., Jr. (1999) Survey measures of expected US inflation. *Journal of Economic Perspectives* 13 (Autumn) 125-144.

Yogo, Motohiro (2004) Estimating the elasticity of intertemporal substitution when instruments are weak. *Review of Economic Studies* 86, 797-810.

Zarnowitz, Victor (1985) Rational expectations and macroeconomic forecasts. *Journal of Business and Economic Statistics* 3, 293-311.

Figure 1: Observations per Forecaster

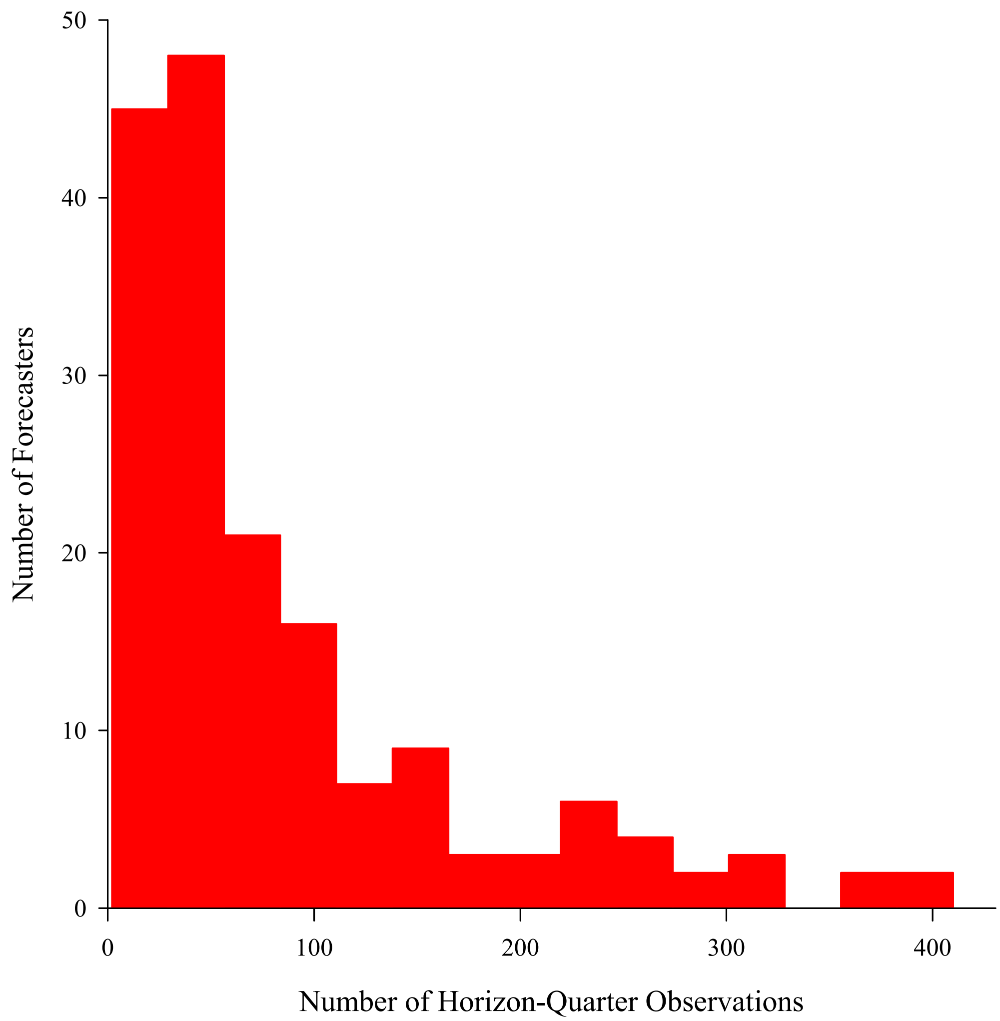


Table 1: Linear Forecast Regression

$$\mathrm{E}_{jt-h} r_t = d_{hj} + b_\pi \mathrm{E}_{jt-h} \pi_{t+1} + b_x \mathrm{E}_{jt-h} x_{t+1}$$

Ints.	$\hat{b}_\pi$ (se)	$\hat{b}_x$ (se)	R^2
d_{hj}	0.774 (0.020)	-0.026 (0.013)	0.628
d_h, d_j	0.742 (0.019)	-0.030 (0.012)	0.618
d_j	0.743 (0.019)	-0.030 (0.012)	0.618
d_h	1.178 (0.016)	-0.002 (0.014)	0.451
d	1.178 (0.016)	-0.003 (0.014)	0.451

Notes: $\{j,t,h\}$ index forecaster, time period, and horizon. Ints. is the set of intercepts. There are 14727 observations for 1981:1 -2007:3.

Figure 2: Forecaster-Specific Slopes

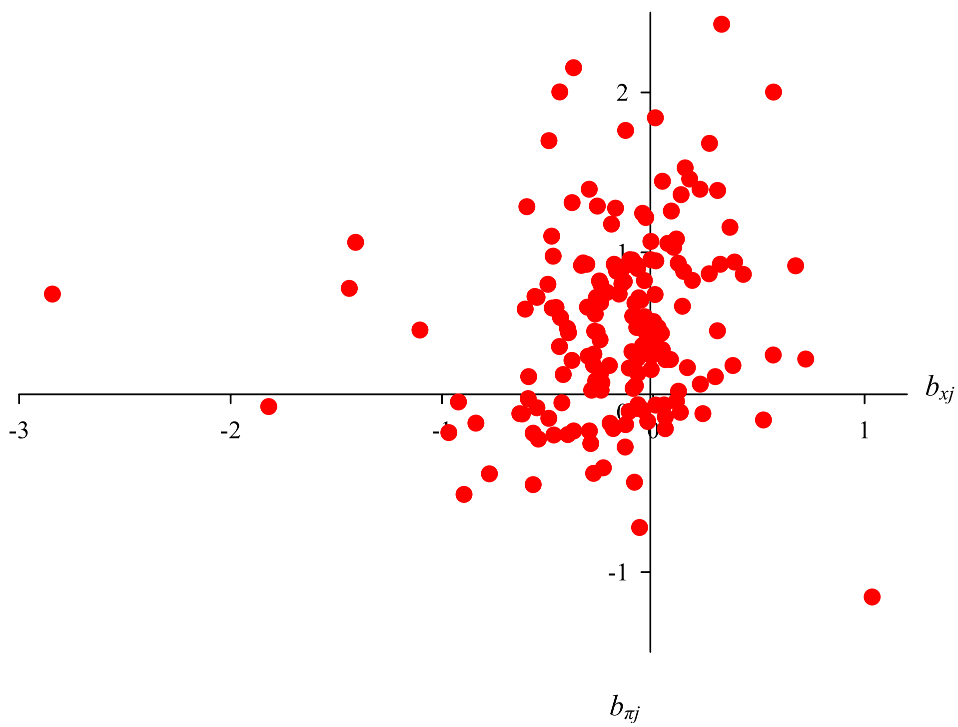
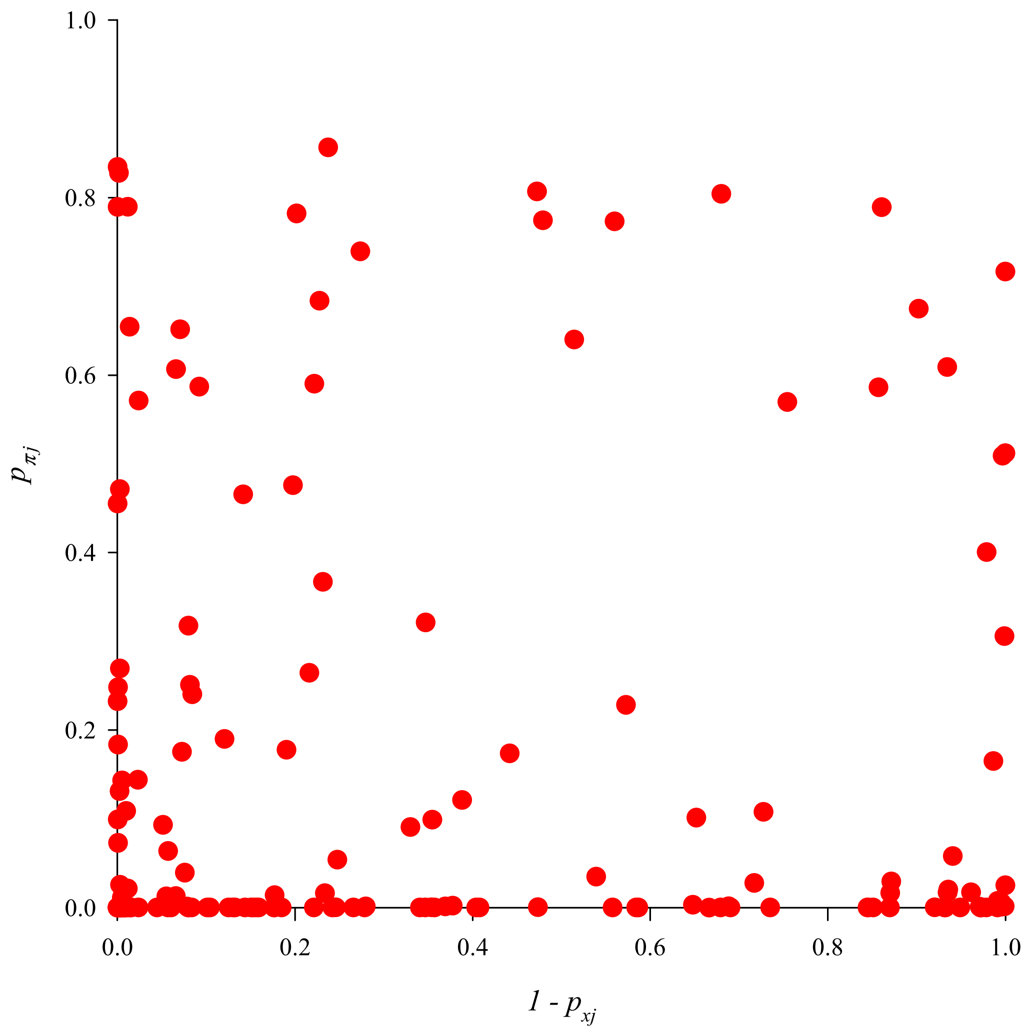


Figure 3: *p*-values



Notes: p_{xj} is the p-value for the one-tailed t-test of H_0: $b_{xj} = 0$;
$p_{\pi j}$ is the p-value for the two-tailed t-test of H_0: $b_{\pi j} = 1$.

Table 2: Rank Forecast Regression

$$R_{hjt}(\mathrm{E}_{jt-h}r_t) = d_{hj} + \rho_\pi R_{hjt}(\mathrm{E}_{jt-h}\pi_{t+1}) + \rho_x R_{hjt}(\mathrm{E}_{jt-h}x_{t+1})$$

Ints.	ρ_π (se)	ρ_x (se)	R^2
d_{hj}	0.355 (0.009)	-0.077 (0.007)	0.531
d_j	0.337 (0.009)	-0.076 (0.007)	0.521
d	0.571 (0.007)	-0.043 (0.007)	0.333

Notes: $\{j,t,h\}$ index forecaster, time period, and horizon. Ints. is the set of intercepts. There are 14727 observations for 1981:1 -2007:3.

Table 3: Standard GMM Evidence

$$\mathrm{E}\left[\frac{\beta(1+r_t)}{(1+x_{t+1})^{\alpha}(1+\pi_{t+1})}|z_t\right]=1$$

z_t	$\hat{\alpha}$ (se)	$\hat{\beta}$ (se)	$J(df)$ (p)
ι, r_{t-1}	-6.49 (3.61)	0.795 (0.088)	
ι, r_{t-1}, π_t	-0.23 (0.27)	0.970 (0.009)	63.4(1) (0.00)
ι, r_{t-1}, π_t, x_t	-0.22 (0.27)	0.971 (0.009)	64.2(2) (0.00)
ι, $r_{t-1}, \ldots r_{t-3}$ $\pi_t, \ldots \pi_{t-2}$, $x_t, \ldots, x_{t-2}$	0.07 (0.19)	0.980 (0.007)	75.4(8) (0.00)
ι, x_{t+1}, π_{t+1} (NLLS)	0.14 (0.11)	0.983 (0.004)	

Notes: Estimation uses 107 quarterly observations from 1981:1 to 2007:3.

Table 4: Linear GMM Evidence

$$\mathrm{E}\Big[\ln(1+r_t) - d - \ln(1+\pi_{t+1}) - b_x \ln(1+x_{t+1}) | z_t\Big] = 0$$

z_t	$\hat{b}_x$ (se)	$J(df)$ (p)
ι, $\ln(1+r_{t-1})$	-10.6 (12.2)	
ι, $\ln(1+r_{t-1})$, $\ln(1+\pi_t)$	-0.01 (0.26)	67.9(1) (0.00)
ι, $\ln(1+r_{t-1})$, $\ln(1+\pi_t)$, $\ln(1+x_t)$	0.01 (0.26)	68.4(2) (0.00)
ι, $\ln(1+r_{t-1}), \ldots \ln(1+r_{t-3})$ $\ln(1+\pi_t), \ldots \ln(1+\pi_{t-2})$, $\ln(1+x_t), \ldots, \ln(1+x_{t-2})$	0.18 (0.19)	75.4(8) (0.00)
ι, $\ln(1+\pi_{t+1})$, $\ln(1+x_{t+1})$ (OLS)	-0.86 (0.37)	

Notes: Estimation uses 107 quarterly observations from 1981:1 to 2007:3.