Professional Forecasts of Interest Rates and Exchange Rates: Evidence from the Wall Street Journal's Panel of Economists

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Abstract

We use individual economists' 6-month-ahead forecasts of interest rates and exchange rates from the *Wall Street Journal*'s survey to test for forecast unbiasedness, accuracy, and heterogeneity. We find that a majority of economists produced unbiased forecasts but that none predicted directions of changes more accurately than chance. We find that the forecast accuracy of most of the economists is statistically indistinguishable from that of the random walk model when forecasting the Treasury bill rate but that the forecast accuracy is significantly worse for many of the forecasters for predictions of the Treasury bond rate and the exchange rate. Regressions involving deviations in economists' forecasts from forecast averages produced evidence of systematic heterogeneity across economists, including evidence that independent economists make more radical forecasts.

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Professional forecasters' predictions of macroeconomic variables are of widespread interest. Governments, businesses, and households purchase forecasts, presumably to help them form their own expectations and aid in economic decision-making.[1] Economic researchers increasingly use surveys of professional forecasters' predictions as proxies of otherwise unobservable expectations in studying asset price determination.[2] But compared with the effort put into making macroeconomic forecasts, the effort put into assessing forecast quality ex post is small (Fildes and Stekler (2002), p 462).

Ex post assessments of forecast quality are potentially valuable to forecasters and users of forecasts alike. The theory of rational expectations implies that, if professional forecasters understand fundamental economic processes, they will produce unbiased, identical forecasts given access to the same information and presented with similar incentives with respect to forecast accuracy. If ex post assessments show forecasters' predictions to be unbiased and statistically identical, they serve to increase confidence in the profession's knowledge of economic processes, researchers' use of forecasts to proxy economic expectations, and agents' use of forecasts to inform economic decision-making. But if assessments yield evidence of bias or heterogeneity, they call for a reexamination of assumptions about information access, incentives and, possibly, understanding of economic processes.

[1] For example, Carroll (2003) reports evidence that households use the reported forecasts of professional economists in forming their own expectations.

[2] For example, Anderson et al (2003) and the references cited by them, discuss researchers' use of professional economists' forecasts of macroeconomic variables to distinguish expected from unexpected macroeconomic announcements in studies of financial market reactions to economic news. Frankel and Froot (1987) and MacDonald (2000) observe that forecasts of interest rates and exchange rates potentially enable researchers to separate the confounding effects of expectations and time-varying risk premiums.

Of the studies that assess forecast quality from survey data, most focus on inflation, GDP and exchange rate forecasts and several cast doubt on the rationality of forecasters (MacDonald (2000)). For example Ito (1990), using survey data of individual economists' exchange rate forecasts, finds evidence of heterogeneous expectations, as do MacDonald and Marsh (1996), who use individual economists' exchange rate forecasts from a different survey. Lamont (2002) finds that the patterns of economists' forecasts of real GDP, the unemployment rate and the inflation rate are inconsistent with the single goal of forecast accuracy, suggesting strategic behavior. Laster *et al.* (1999) also finds evidence of strategic behavior by forecasters making real GDP forecasts from survey data which groups forecasters by industry rather than identifying them individually, which raises the issue of how carefully survey participants make their predictions when they are not identified. Compared with inflation, GDP and exchange rate forecasting, interest rate forecasting has received less attention.

To help address the comparative dearth of forecast assessments and to contribute to the debate on forecaster rationality we analyze interest rate and exchange rate forecasts from a highly visible but relatively little studied survey of forecasters, the *Wall Street Journal's* panel of economists. This survey is particularly well-suited to assessing forecast quality because the names and employers of the forecaster-economists are published along side their forecasts, which should give the economists strong incentives to think carefully about their forecasts. We focus on interest rate and exchange rate forecasts because their actual values are never subject to subsequent revision, unlike, say GDP, so there is no question about the actual values economists were predicting.[3] In addition, the *Wall Street Journal* surveys contain consistent data on interest rate and exchange rate forecasts for a longer period than on other variables. We proceed by testing whether economists' forecasts are unbiased, more accurate than naïve prediction rules,

[3] Keane and Runkle (1990) present evidence that using preliminary versus revised data can change the conclusions from unbiasedness tests.

and heterogeneous or indicative of strategic behavior by economists. We study the forecasts of individual economists as well as the survey means, allowing for the possibility that the interest rates and exchanges rates forecasted are non-stationary. We are unaware of previous papers that allow for non-stationarity in the actual data when applying tests of forecast unbiasedness to individual data. We are also unaware of previous papers using interest rate and exchange rate forecasts from the *Wall Street Journal* survey to study forecast unbiasedness, assess the statistical significance of forecast accuracy, or investigate forecast heterogeneity and possibly strategic behavior by economists.

To preview our results, we find that a majority of economists produce forecasts that are unbiased and that most produce forecasts that are less accurate than the forecasts generated by a random walk model. While efficient financial markets should make accurate forecasting of interest rates or exchange rates impossible, rational forecasters should not do significantly worse than a random walk model. We find that the economists' forecasts exhibit the same kind of heterogeneity found by Ito (1990) and MacDonald and Marsh (1996), using Japanese and European survey data, respectively. When we apply the models of Laster *et al.* (1999) and Lamont (2002) to our economists' forecasts we find evidence of strategic behavior similar to Laster *et al,* but contrary to Lamont's finding that economists make more extreme forecasts as they age, we find that more experienced economists make less radical forecasts.

The rest of the paper is organized as follows. Section 1 briefly reviews some of the past work on evaluating survey measures of expectations. Section 2 describes our data. Section 3 reports our empirical results and section 4 offers some conclusions.

1. Review of Past Work

Although researchers have put less effort towards assessing professional economists' forecasts than seems warranted, the existing research focuses on three issues.[4] The first is whether mean or median responses, usually referred to as consensus forecasts, give misleading inferences about the unbiasedness and rationality of individual forecasters. Figlewski and Wachtel (1981) report that pooling individuals' inflation forecasts from the Livingston survey produces stronger evidence of bias than using survey averages. Keane and Runkle (1990) find that individuals' inflation forecasts from the Survey of Professional Forecasters (SPF) are generally unbiased whereas Bonham and Cohen (2001) find many of the forecasters in the SPF to be biased and systematically heterogeneous so that pooling their forecasts is inappropriate.[5] The finding of bias in inflation expectations runs contrary to rational expectations, and might reflect heterogeneity of expectations. Whether the individual forecasts of interest rates and exchange rates of professional economists are similarly plagued by bias is a question addressed below.

A second issue of research focus is whether the standard tests of economists' forecast unbiasedness are rendered invalid by nonstationarity in the variables economists' forecast.[6] Liu and Maddala (1992) find that exchange rate forecasts from the Money Market Services (MMS) survey appear to be nonstationary but cointegrated with the actual data and thus, potentially unbiased; when they introduce a restricted cointegration test they find that the forecasts are indeed unbiased. In contrast, Aggarwal *et al.* (1995) and Schirm (2003) find that only about half

[4] Much of the work on evaluating survey measures of expectations focuses on inflation forecasts. See Croushore (1998) and Thomas (1999) for reviews of this work. MacDonald (2000) examines previous work on financial market expectations.

[5] Bonham and Cohen (2001) test whether the coefficients of the standard unbiasedness equation are the same across individuals and reject this hypothesis. Batchelor and Dua (1991) use individual forecast data from the Blue Chip Economic Indicators and find that most individuals are unbiased.

[6] The standard test is to regress the actual value being forecasted on the forecast and test that the intercept is zero and the slope is one.

the macroeconomic variables forecasted by economists in the MMS surveys appear unbiased after testing for nonstationarity and cointegration.[7] But Osterberg (2000), applying the Liu-Maddala techniques to more recent exchange rate forecasts in the MMS survey, finds that these forecasts are unbiased. The aforementioned tests, it should be noted, all use the median responses from the MMS surveys rather than forecasts of individual economists. To our knowledge the issue of variable non-stationarity and forecast unbiasedness has not been investigated using forecasts by individual economists.

A third issue of research focus concerns forecast heterogeneity and strategic behavior by forecasters as a potential source of such heterogeneity. Study of this issue has been furthered by the availability of data reporting forecasts by individuals. Ito (1990) and MacDonald and Marsh (1996) use individual data and report evidence supporting systematically heterogeneous expectations about exchange rate movements. The latter paper also finds that variations in the degree of heterogeneity can help explain the volume of trading in financial markets. Scharfstein and Stein (1990) and Erbeck and Waldmann (1996) argue that the incentive structure facing forecasters leads to "herding," that is, making forecasts that are close to the mean or "consensus" forecast. In contrast, Laster *et al.* (1999) and Lamont (2002) suggest that incentives could lead forecasters to make forecasts that are more extreme than their true expectations if forecasters are rewarded not only for being right but for being right when others are wrong. Laster *et al* (1999) find evidence consistent with strategic forecasting using forecasts of real GDP from the Blue Chip Economic Indicators, although their data are not ideal for testing their theory since

[7] These variables include the consumer price index, the producer price index, the M_1 money supply, personal income, durable goods, industrial production, retail sales, the index of leading indicators, housing starts, the trade balance, and unemployment.

individual forecasters are not identified, only the industry of their employment.[8] Lamont (2002) uses *Business Week's* annual set of economists' forecasts for real GDP growth, inflation, and unemployment to test whether forecasters make more radical predictions when they own their own firms, and hence may gain the most from publicity. He finds support for this hypothesis, as well as evidence that forecasters produce forecasts that deviate more from the mean forecast as they age. Perhaps due to the paucity of data on interest rate and exchange rate forecasts by individuals, the issue of heterogeneity in interest rate forecasts and strategic behavior in forecasting interest rates and exchange rates remains largely unstudied.

To investigate the rationality, accuracy, and heterogeneity for individual forecasters' interest rate and exchange rate forecasts we use data from the *Wall Street Journal's* bi-annual survey of economists. Several researchers have used these data previously, mainly to examine forecast accuracy. Kolb and Stekler (1996) examine the six-month-ahead interest rate forecasts from 1982 through January 1990 and find little evidence that forecasters, individually or on average, can predict the sign of interest rate changes. Greer reports similar evidence for predicting the direction of one-year changes for various variables for 1984-1997 (Greer (1999)) and for the long-term interest rate for 1984-1998 (Greer (2003)). Cho (1996) evaluates the six-month-ahead predictions of twenty-four forecasters who participated in all the surveys from December 1989 through June 1994. He finds that about 80 percent of the forecasters predicted the short-term interest rate more accurately than a random walk model but that very few predicted the long-term interest rate or the exchange rate better than a random walk model. Eisenbeis *et al.* (2002) uses the *Wall Street Journal* data from 1986 to 1999 to illustrate a new approach to ranking forecasters across variables that differ in volatility and cross-correlation.

[8] Pons-Novell (2003), using Livingston survey data on forecasts of the unemployment rate, found support for industry effects as in Laster *et al.* (1999) but not the age effect found by Lamont(2002) . The Livingston data, however, do not identify the individual respondents by name.

But to our knowledge, researchers have not previously used the *Wall Street Journal* data to test for unbiasedness of individual forecasts or to test for strategic forecasting by individual forecasters.

After describing our data, we employ them to investigate the dominating issues in the recent work on expectations of economic variables: unbiasedness of individuals' forecasts, the implications of nonstationarity of the data for the accuracy of unbiasedness tests, and systematic heterogeneity of forecasts, possibly as a result of strategic behavior. In addition, we go beyond past researchers' use of the *Wall Street Journal* data by examining the statistical significance of the surveyed economists' forecast accuracy.

2. The *Wall Street Journal* survey data

Since 1981 the *Wall Street Journal* has published forecasts of several economic variables by a set of economists at the beginning and at the mid-point of each year. The economists are identified both by name and by employer. The survey is dominated by economists employed by banks and securities firms but it also includes representatives from non-financial industries, consulting and forecasting companies, universities and professional associations.[9] The initial survey presented economists' forecasts of the prime rate. In January 1982 the survey introduced forecasts of the Treasury bill and Treasury bond interest rates. Additional forecasts have been added including the CPI inflation rate, real GDP growth, and the dollar-yen exchange rate, among others. In the January survey economists are asked for their forecasts of the Treasury bill rate, Treasury bond rate, and the dollar-yen exchange rates for the last business day of June, and

[9] For respondents that appeared in at least six surveys from January 1982 through July 2002, the employer mix is as follows: banks (30 individuals and 394 observations), econometric modelers (5 and 108), independent forecasters (26 and 325), industrial corporations (5 and 41), securities firms (39 and 626), and others (10 and 154).

in the July survey they are asked for their forecasts for the last business day of December.[10] The surveys are published in the first week of January and July, along with commentary on the forecasts and, more recently, discussion of the accuracy of the last set of forecasts.[11]

In this paper we examine the six-month-ahead forecasts of the Treasury bill and Treasury bond rates that began in 1982 along with the six-month-ahead forecasts of the dollar-yen exchange rate that began in 1989. Our sample ends with the July 2002 survey. This long time period allows larger sample sizes for individual forecasters and a larger number of participants. We choose the interest rate and exchange rate variables both because they appear on the largest number of surveys and because the actual data are not revised so there is no question of what variable the forecasters were predicting.[12]

Table 1 reports the means and standard deviations of the survey responses along with the range, and number of respondents. The number of respondents varies over time: only twelve economists participated in the January 1982 survey compared with fifty-five in the July 2002 survey. There is also considerable turnover in the respondents themselves. Table 1 also reports the actual values for the Treasury bill rate, the Treasury bond rate, and the yen-dollar exchange rate on the last business day of June and December.

For several tests we restrict the sample to the set of respondents that made at least twenty forecasts. Table 2 reports the names, participation dates, and professional affiliations of these respondents from 1982 through 2002.

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[10] Respondents have often been asked for 12-month ahead forecasts but these are not available for the entire period.
[11] The selection of survey respondents does not depend on their past performance. The *Journal* tries to get broad representation but also wants to include the chief economists from major financial institutions. We thank Jon Hilsenrath of the *Wall Street Journal* for this information.

[12] There was a change in the definition of the three-month Treasury bill rate from the discount yield to the bond-equivalent yield starting with the July 1989 survey. The long-term bond rate refers to the thirty-year bond until the July 2001 survey when it was changed to the ten-year rate. All data are available from the authors on request.

Figures 1-3 show the dispersion in the forecast errors, defined as actual minus predicted, of the Treasury bill rate, the Treasury bond rate, and the yen-dollar exchange rate. The figures are similar in showing a considerable spread in forecasts. The assumption that agents form unique rational expectations using the same model and same information is clearly not supported by the data. Figure 1 indicates that the errors in predicting the Treasury bill rate are largely of one sign for about half the surveys, suggesting that while expectations vary across individuals a common source exists for at least some of the error. Figures 2 and 3 provide stronger support for this interpretation, where an even higher proportion of the survey errors are of the same sign for the long-term bond rate and the exchange rate. The correlation coefficient for the two interest rate forecast errors is .66, indicating that most of the forecast errors are from unpredicted shifts in the yield curve rather than unpredicted changes in its slope. There is little evidence of correlation in the errors for interest rates and the exchange rate.[13]

3. Evaluating the survey data

3.1. Tests of unbiasedness

A major issue in the literature on economic expectations is unbiasedness, which is a requirement for rationality when a forecaster's loss function is symmetric about the forecast error. Denoting the forecast of a variable made at time (t-1) for time t as ${}_{t-1}F_t$ and the actual value of the variable as A_t, the usual test involves estimating

$$A_t = \alpha + \beta\ {}_{t-1}F_t + \varepsilon_t \qquad [1]$$

[13] For the forecast errors in the figures, the correlation between the Treasury bill forecast errors and the exchange rate errors is .02 and the correlation between the Treasury bond forecast errors and the exchange rate errors is –.07.

where ε_t is a random error term. A forecast series is unbiased if the joint hypothesis that α=0 and β=1 cannot be rejected.[14]

As is well-known estimating [1] may produce misleading inferences when A and F are nonstationary and not cointegrated since the error term will also be nonstationary, resulting in the spurious regression problem noted by Granger and Newbold (1974). If the actual series is nonstationary, an unbiased forecast must also be nonstationary and the two series must be cointegrated with a cointegrating vector of zero and one. Liu and Maddala (1992) suggest a restricted cointegration test when A and F are I(1): impose the restrictions α=0 and β=1 and use the data to compute forecast errors; if the forecast errors are stationary, the restrictions are supported and the forecasts are unbiased since the cointegrating vector is unique with only two series.[15] We perform the Liu-Maddala test below after first establishing whether A and F are I(1).

To establish that the As – the daily Treasury bill, Treasury bond and exchange rate data sampled at six-month intervals, the data frequency that matches our forecast series -- are I(1), we perform unit root tests. Using levels data we cannot reject the hypothesis of a unit root for any of the three series, but using first-differenced data we can reject the unit root hypothesis for all three. Thus all three actual series appear to be I(1).[16]

To establish that the Fs -- the Treasury bill, Treasury bond and yen-dollar exchange rate forecast series of the thirty-three economists listed in Table 2 who responded to at least 20 surveys -- are I(1), we perform 99 unit root tests (three forecast series for each of the thirty-three

[14] Rationality tests often include a test that ε_t is not autocorrelated and may also include other information available at time (t-1) on the right hand side of equation [1]. Rationality requires that all such variables have zero coefficients.
[15] Papers employing this restricted cointegration test include Hakkio and Rush (1989) and Osterberg (2000).

[16] The ADF statistics using 1 lag for the levels of the Treasury Bill rate, Treasury bond rate, and yen-dollar exchange rate are -.867, -.970, and -2.396 respectively, indicating that each series has at least one unit root. The ADF statistics for the first differences are -4.950, -6.143, and -3.612 indicating that all series are I(1). Rose (1988) and Rapach and Weber (2004) also find that the nominal interest rate has a unit root while Baillie and Bollerslev (1989) report similar findings for nominal exchange rates.

economists). The t statistics for augmented Dickey-Fuller (ADF) unit root tests performed on levels and first differences for individual forecasters are reported in the second column of Tables 3-5. Starred values indicate rejection of the unit root hypothesis at the 0.01, 0.05 or 0.10 levels of significance. Of the 99 forecast series, 71 appear to be I(1) using the 10% significance level or better.

To complete the Liu-Maddala test we impose the restriction that α=0 and β=1 on [1], use the As and Fs to compute the forecast errors, and perform ADF tests to determine whether the forecast errors are I(0). The third columns in Tables 3-5 report ADF t statistics for the case of a zero intercept since the null hypothesis is that the residuals have an expected value of zero. Box-Ljung Q statistics to test for serial correlation in the residuals appear beneath the t statistics. Of the 99 forecast error series all but four are I(0) at the 10% level or better and only four show evidence of serially correlated errors.

To pass the Liu-Maddala test the Fs must be I(1) and the forecast residuals must be I(0). Nearly 60 % of the Treasury bill rate forecasts reported in Table 3 meet both criteria.[17] In addition, over three-quarters of the Treasury bond rate forecast series in Table 4 and two-thirds of the exchange rate forecast series in Table 5 meet both criteria.[18] Altogether, two-thirds (67) of the 99 forecast series pass the Liu-Maddala test of unbiasedness. Moreover, the three series of mean survey responses pass the Liu-Maddala test, as indicated in the last row of each table.

While the results of the Liu-Maddala tests are encouraging to proponents of forecaster rationality, Lopes (2000) provides evidence that the power of their restricted cointegration test

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[17] About one-third of the forecast series appear to be I(0) despite the Treasury bill rate series being I(1). First differences of four other forecast series appear to be nonstationary even though the first difference of the Treasury bill rate series is stationary; the forecast errors in these four cases do appear stationary, however. For some individuals there are gaps, usually just one, in the forecast series. While Shin and Sarker (1993) find that occasional missing values do not change the asymptotic distribution of the standard Dickey-Fuller tests, our samples are small so that the results with a gap remain suspect.

[18] Of the eleven exchange rate forecast series that failed, three had ten or fewer observations.

may be low, as is usual with unit root tests. He uses Monte Carlo techniques to show that a more powerful test of unbiasedness in finite samples is a simple t-test for the hypothesis that a forecast series' mean forecast error is zero. Accordingly, we also report the mean forecast error and its t-statistic in column 4 of each table. We fail to reject at the 10% level the null hypothesis of unbiasedness for 73% of the Treasury bill forecast series, 67% of the Treasury bond forecast series, and 88% of the exchange rate forecast series.[19] Of the forecast series with test statistics that reject the null, all of the Treasury bill rate and exchange rate forecast series and about two-thirds of the Treasury bond rate forecast series err on the high side. Biased forecasts by some forecasters did not serve to impart bias to the survey mean forecasts, however: the average forecast errors of the survey mean forecasts were statistically indistinguishable from zero, implying unbiasedness.

In summary, about two-thirds of the forecast series appear to be statistically unbiased, as do all three series of mean survey responses. Economists whose forecasts appeared to be biased usually overestimated the 6-month-ahead level of the Treasury bill, Treasury bond or yen-dollar exchange rate, with overestimation occurring more frequently in predicting interest rates than exchange rates. Based on the t-tests for unbiasedness at the 10% level, about 60 % of the survey economists were statistically unbiased in their predictions of the Treasury bill, Treasury bonds and exchange rate; about 10% made biased forecasts of one of the three rates; and the remaining 30% made biased forecasts of two series. No economist made biased predictions of all three rates.[20]

[19] At the less stringent 5% level, 80%, 73% and 91%, respectively, of the Treasury bill, Treasury bond, and exchange rate series fail to reject the null of unbiasedness.

[20] If the less stringent 5% level is used to judge unbiasedness, 67% of the survey forecasters were statistically unbiased in their predictions of all three rates; about 6% made biased forecasts of one of the rates; and the remaining 27% made biased forecasts of two rates.

3.2 Measures of predictive ability

While unbiasedness is a requirement for rationality of forecasters with symmetric loss functions, predictive ability is a hallmark of forecasters who "know the true model" determining macroeconomic variables. We take two approaches to measuring predictive accuracy: first, we assess forecasters' success at predicting the direction of interest rate and exchange rate changes;[21] second, we compare forecasters' accuracy to the accuracy of a traditional benchmark, the random walk model without drift, and test whether the accuracy metrics are statistically different. Although previous researchers have employed the *Wall Street Journal* survey to assess predictive accuracy using one approach or the other (but not both), they reach contradictory conclusions.[22] Moreover, we are unaware of any previously published research using the *Wall Street Journal* survey that tests for statistical differences in the accuracy of individual economists' forecasts versus forecasts of the random walk model.

In our first approach to predictive accuracy we use standard techniques to assess economists' accuracy in predicting the direction of change in the Treasury bill rate, Treasury bond rate, and yen-dollar exchange rate over 6-month intervals. The results appear in columns five and six of Tables 3-5. Column 5 reports the fraction of correctly-predicted changes along with the p-value for Fisher's exact test of the hypothesis that predicted and actual changes were independent. Column 6 reports the standard χ^2 statistic and the Pesaran-Timmerman (1992) test

[21] Leitch and Tanner (1991) argue that the direction of change is more closely related to profits than say the mean square error for interest rate predictions.
[22] Kolb and Stekler (1996) and Greer (1999, 2003) present tests of directional change whereas Cho (1996) compares economists' forecast errors against the forecast errors made by the naïve model of no change. Kolb and Stekler and Greer find that little evidence that economists can predict the direction of change, whereas Cho finds that eighty percent of the economists outperformed the naive model when forecasting the Treasury bill rate.

statistic, also with a χ^2 distribution with 1 degree of freedom, of the same independence hypothesis.[23]

The directional accuracy tests suggest that the surveyed economists provide no useful information.[24] In forecasting the Treasury bill rate about two-thirds of economists predicted the direction of change correctly more than half the time, but for no economist was the percentage of correctly predicted directions significantly greater than expected by chance; moreover for a few, the percentage was significantly lower. In predicting the Treasury bond rate, only about one-third of economists forecasted directional change correctly more than half the time; nevertheless, few predicted directional change less accurately than chance. The surveyed economists were more successful in predicting directional change in the yen-dollar exchange rate: about 80 predicted correctly more than half the time; nevertheless none predicted correctly more often than would be expected by chance. Finally, the survey means successfully predicted the direction of Treasury bill rate and exchange-rate changes about as accurately as chance, but predicted the direction of Treasury bond rate changes significantly more poorly than chance. Thus, when set the task of predicting the direction of interest rate and exchange rate changes, the surveyed economists acquit themselves modestly, at best.

In our second approach to predictive accuracy, we compare the accuracy of the surveyed economists' predictions to the accuracy of a model predicting that interest rates and exchange rates follow a random walk without drift. Specifically, we computed the ratio of the mean square errors (MSEs) of each economist's forecast series to the MSEs of forecast series covering the

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[23] For each forecaster we constructed a contingency table with the number of times the forecaster predicted a decline and there was a decline, the number of times the forecaster predicted an increase and there was an increase, the number of times the forecaster incorrectly predicted a decrease, and the number of times the forecaster incorrectly predicted an increase.

[24] We also performed the test of Cumby and Modest (1987), suggested by Stekler and Petrei (2003), in which the actual change is regressed on a binary variable taking the value of one if the forecaster predicted an increase and zero otherwise. These tests, not reported, also indicated that the respondents were unable to provide useful information on the direction of change.

same dates but using as forecasts the six-month-earlier actual values (that is, actuals on the last business day in December and June, respectively, to forecast values for the last business day in June and December, respectively; these actuals are usually published along side the forecasts in the *Wall Street Journal)*. The question becomes whether individual economists can outperform the random walk model by achieving a ratio less than one. In addition to analyzing this ratio we follow the recommendation of Fildes and Stekler (2002) and test for statistically significant differences between individuals' forecasts and random walk forecasts of no change using the modified Diebold-Mariano (1995) test statistic proposed by Harvey *et al.* (1997). Specifically, this statistic tests whether the mean difference between the squared forecast errors of the economist and of the random walk model is significantly different from zero; this statistic has a t-distribution under the null hypothesis that the mean is zero. We report our results in Tables 3-5. The next-to-the last column reports the number of forecasts made by each economist together with the sum of the squared forecast errors. The last column reports the ratio of each economist's MSE to the MSE from a random walk model and the Diebold-Marino statistic in parentheses.

The statistical evidence indicates that economists generally fail to beat and tend to be statistically less accurate than the random walk model. Although in predicting the Treasury bill rate eight of thirty-three economists achieve a MSE ratio less than one, the Diebold-Marino statistics indicate that no economist forecasts significantly better than the random walk model (i.e. a t-statistic that is significantly less than zero) and five do significantly worse at the 10% level. In predicting the Treasury bond rate, no economist achieved a MSE ratio less than one; moreover, about two-thirds of economists predicted significantly worse than a random walk model, judging by the Diebold-Marino statistics (i.e., a t-statistic significantly greater than zero). Accuracy in predicting the yen-dollar exchange was little better: no economist achieved a MSE ratio less than one, and half predicted significantly worse than a random walk model, judged by the Diebold-Marino statistics. Economists' poor predictive ability is reflected in the survey mean predictions. Although survey mean predictions of the Treasury bill rate achieve a MSE ratio less than one, the survey mean predictions do not differ statistically from the random walk predictions. Survey mean predictions of neither the Treasury bond rate nor the yen-dollar exchange rate achieved MSE ratios less than one, and although the mean predictions of the Treasury bond rate did not differ statistically from the random walk predictions, the mean exchange-rate predictions were significantly worse than the random walk predictions.

Taken all together, the evidence on predictive ability suggests that agents who use forecasts and prize accuracy would have suffered less disappointment by assuming that interest rates and exchange rates stay at their last observed levels rather than by relying on forecasts from the *Wall Street Journal* survey. The dismal predictive accuracy of many of the economists leads us to ask whether the forecasts are systematically heterogeneous, possibly because some economists face incentives to forecast large interest rate and exchange rate changes.

3.3. Tests of systematic heterogeneity of forecasts

Professional economists who are rational, who know the "true model," and who, in addition, have access to the same macroeconomic information relevant to forecasting interest rates and exchange rates – as a priori reasoning suggests is probably the case – should produce homogenous (identical) forecasts. In this section we examine whether forecasts of the economists in the *Wall Street Journal* survey are homogeneous or systematically heterogeneous.

To test for homogeneity in forecasts we follow Ito (1990), who posits a fixed-effects model. Ito models the forecast for time t of the j^{th} economist, $f_{j,t}$, as being a function of common information, I_t, an individual effect represented by an individual-specific dummy variable, g_j, and a random error term, $u_{j,t}$:

$$f_{j,t} = f(I_t) + g_j + u_{j,t} \, . \qquad [2]$$

Ito assumes further that $f(I_t)$ contains a constant so that the average of the g_js may be set to zero. Averaging equation [2] across all economists and then subtracting the average from [2] yields:

$$f_{j,t} - f_{AVE,t} = g_j + (u_{j,t} - u_{AVE,t}) \, . \qquad [3]$$

Homogeneity of forecasts can be tested by estimating [3] on forecast data for individual economists and testing that the estimated values of g_j are identical across economists.[25] [26]

Table 6 presents the results from estimating [3] using the Treasury bill rate, Treasury bond rate and the yen-dollar exchange rate forecasts of the economists in the *Wall Street Journal* survey and testing for forecast homogeneity. Like Ito (1990) we estimate [3] twice, first letting the g_js represent dummy variables for individual economists and again letting the g_js represent dummy variables for the economists' sector of employment. Panels A and B, respectively, report results from the two estimations. We report results for two sub-samples of economists, one including all economists having at least six survey responses (Panel 1) and another including all economists having at least twenty responses (Panel 2), the same economists whose forecasts were examined in sections 3.1 and 3.2.[27]

The evidence in Table 6 overwhelmingly rejects the hypothesis of homogeneous forecasts. In Panel A, F tests reject the null hypothesis of identical g_j estimates for all economists at the 0.01 level for all the data sets, indicating the presence of significant individual effects. In

[25] An essentially identical approach is to regress the individual forecasts on a set of time dummies as well as a set of individual dummies and test for individual effects.

[26] Ito uses [3] to test for heterogeneity in exchange rate forecasts made by Japanese economists. He finds that the data reject the hypothesis of homogeneous forecasts both when the g_js are individual dummy variables and when the g_js represent the industry of the economist's employment. Ito also finds that economists employed in export industries have a depreciation bias whereas those employed in the import business have an appreciation bias, a pattern he terms the "wishful thinking" effect. MacDonald and Marsh (1996) also find evidence of heterogeneity across exchange rate forecasters from a large survey of European economists. In addition they report that the dispersion of forecasts is positively related to the volume of foreign exchange trading. MacDonald and Marsh report that the European economists are generally less accurate than a random walk for 3-month predictions but that a substantial number of economists beat a random walk when making 12-month forecasts.

[27] These are unbalanced panels since participants change over time.

Panel B, coefficient estimates of five employment sectors appear (top number, standard errors beneath) along with F tests of the null hypothesis that the estimated coefficients are identical (reported in the last row). The data soundly reject the null for all data sets. The coefficient estimates indicate that, compared with other economists, independent forecasters made significantly lower forecasts of the Treasury bill and Treasury bond rate and significantly higher forecasts of the yen-dollar exchange rate. Economists employed by securities firms also made comparatively low forecasts of the Treasury bond rate, but not as low as economists employed by independent firms. Economists affiliated with banks produced forecasts statistically indistinguishable from the consensus, as did economists employed by econometric modeling firms, except for yen-dollar exchange rate forecasts made by Panel 2, which were statistically lower.

In summary, the evidence from the *Wall Street Journal* survey suggests that the economists' forecasts are indeed systematically heterogeneous. This finding leads us to investigate whether individual forecasters behave strategically in making their forecasts.

3.4. Tests of strategic forecasting

Laster *et al.* (1999) and Lamont (2002) suggest that the incentive structure facing professional economists potentially motivates them to supply heterogeneous forecasts. Specifically, they argue that if economists are rewarded both for forecast accuracy and for "standing out from the crowd," economists may announce more extreme predictions than if they were rewarded for forecast accuracy alone.[28] To investigate this possibility we estimate a model combining elements of Lamont (2002) and Laster *et al.* (1999):

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[28] Lamont (2002) models forecasters' payoff function as follows:

$$w_j = R(|f_j - a|, |f_j - f_{c(-j)}|)$$

$$|f_j - f_{c(-j)}|_t = \beta_0 + \beta_1\ AGE_{j,t} + \beta_2\ AGE_{j,t}{*}\ MODEL_{j,t} + \beta_3\ AVEDEV(\text{-}j)_t + \beta_4\ OWN_{j,t} + \sum \gamma_i\ D_{i,t} + \varepsilon_{j,t} \qquad [4]$$

Following Lamont our dependent variable – a measure of "standing out from the crowd" – is the absolute value of the difference between an individual economist's time t forecast and the average time t forecast omitting that economist's forecast. AGE is the number of years an economist had participated in the *Wall Street Journal* survey at the time of survey t while the interaction term AGE*MODEL allows the effect of an economist's age to differ if the economist is employed by an econometric modeling firm.[29] AGE is included to control for changing incentive structures: incentives might encourage young forecasters to make extreme forecasts so as to gain publicity while encouraging older forecasters to make less extreme forecasts so as to protect the reputations; alternatively, incentives might encourage young forecasters to make less extreme forecasts so as to hide their inexperience while encouraging seasoned, secure forecasters to make more radical forecasts. AVEDEV(-j) is the average absolute deviation of the forecasts from the mean, omitting the j^{th} economist; this latter variable controls for variations in the spread of the forecasts over time. The dummy variable, OWN, equals one if an economist is employed at a firm that bears his name. Finally, following Laster *et al.*, we add dummy variables for the industry employing the j^{th} economist at the time of survey t, the D_{jt}s. Our industries include banks, securities firms, finance departments of corporations, econometric modelers, and economists employed by independent firms not bearing the economists' names, similar to Laster

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where w_j is the payoff to forecaster the j^{th} forecaster, $|f_j - a|$ is the absolute value of the j^{th} forecaster's forecast from the actual value, and $|f_j - f_{c(-j)}|$ is the absolute value of the j^{th} forecaster's forecast from the consensus forecast, omitting the j^{th} forecaster's forecast. Lamont assumes the partial derivative of R with respect to the first argument, R_1, is negative: inaccurate forecasts reduce a forecaster's payoff. But he allows that the partial derivative of R with respect to the second argument, R_2, is an empirical question.

[29] Lamont found that this variable was important and that the effect of age was not significant for forecasts from econometric models.

et al. The hypothesis that economists behave strategically is supported by statistically significant coefficients on AGE, AGE*MODEL, OWN, and the D_{jt}s, as well as by statistical differences among the estimated coefficients of the D_{jt}s.

Table 7 presents estimates of [4] using the Treasury bill rate, Treasury bond rate and the yen-dollar exchange rate forecasts of the economists in the *Wall Street Journal* survey. As in the previous section we report estimates for two sub-samples of economists, one including all economists having at least six survey responses (Panel 1) and another including all economists having at least twenty responses (Panel 2), the same economists whose forecasts were examined in sections 3.1 and 3.2.

The Table 7 estimates show overwhelming evidence of strategic behavior by economists in the form of statistically significant estimated coefficients of AGE, OWN and several of the D_{jt}s, as well as statistical differences among the D_{jt}s. The estimated coefficients of AGE are negative and usually statistically significant, implying that economists make less extreme forecasts the longer they are surveyed.[30] This age effect holds for all economists including those employed by econometric modeling firms, since the estimated coefficient of AGE*MODEL never achieves significance. Though pervasive, the estimated age effects are small in absolute terms: compared with a first-time respondent, an economist in the survey for 10 years (20 surveys) is about 4 basis points closer to the mean interest rate forecast and a little less than one yen closer to the mean exchange rate forecast. Larger in absolute terms is the effect of employment by a forecasting firm bearing one's name: forecasts of such economists deviate more from the mean forecasts than forecasts of other economists by amounts ranging from 13 to

[30] As noted above, the *Wall Street Journal* does not systematically drop forecasters with poor records so a negative coefficient should not be due to a survivorship bias. It is possible, however, that people who make extreme and inaccurate forecasts drop out to avoid negative publicity. We also estimated a model with age and AVEDEV(-j) as explanatory variables for each of the individuals listed in Table 2. Age was statistically significant at the .10 level for only about one-third of the panel and was negative in most cases. No individual had significantly positive coefficients on age for all three variables being forecasted.

22 basis points for the interest rates and 1.7 yen, on average, for the exchange rate. The name effect appears to drive economists' strategic behavior rather than independence per se: only in forecasting the Treasury bond rate did economists employed by independent firms named for others make forecasts statistically more extreme than the consensus, and even then the effect was absolutely small. Surveyed economists employed by banks appeared to make less extreme forecasts than other economists, judging from the consistently negative and statistically significant estimated coefficients of Banks. Economists employed by securities firms, corporations and econometric modeling firms also tended to make less extreme forecasts, judging from the generally negative although inconsistently significant estimated coefficients of their respective dummy variables. When the hypothesis that economists' forecasts deviated equally from the consensus regardless of employment is tested, F statistics soundly and universally reject the hypothesis. Because it seems unlikely that economists in different industries had differential access to the macroeconomic data needed to make interest rate and exchange rate forecasts, we conclude that incentive structures encourage economists employed in different industries to supply heterogeneous forecasts, with economists from firms bearing their own names being more likely to make extreme forecasts because they gain the most from being right when others are wrong.[31]

3.5 Discussion of results

We believe that the results presented in sections 3.1 – 3.4 present a consistent story. Our findings from section 3.1 – that 30% of economists produced biased forecasts, generally in the upward direction – and from section 3.2 – that economists generally failed to forecast as

[31] We also estimated equation [4] allowing for individual fixed effects or individual random effects. These models gave similar estimates for the effects of AGE and AVEDEV but wiped out the statistical significance of the industry effects. Since individuals change industries occasionally in our sample, as indicated in Table 2, the industry differences appear to be captured by the individual effects.

accurately as the random walk model and sometime forecasted less accurately – is consistent with the heterogeneity of forecasts we found in section 3.3. When we tested for evidence of strategic behavior by economists in section 3.4 by using a synthesis of the Lamont (2002) and Laster *et al.* (1999), we obtained some results similar to theirs. Like Lamont and Laster *et al.* we found that economists from independent firms tend to make more extreme forecasts and, like Lamont, we found that economists whose firms bear their names make forecasts that consistently deviate more from the survey mean than other economists. But whereas Lamont found evidence that economists make more extreme forecasts the longer they are surveyed, we found the opposite to be true: the estimated coefficients of AGE are consistently negative and usually statistically significant.

Although our results on strategic behavior bear some similarities to Lamont and Laster *et al.*'s, we believe it is important to note the advantages of the *Wall Street Journal* survey data on interest rates and exchange rates for testing strategic behavior compared with *Business Week* survey data used by Lamont and the Blue Chip Economic Indicators data used by Laster *et al.* Although the *Business Week* survey publishes forecasts of economists by name, Lamont studied economists' forecasts of real GDP growth, inflation and unemployment, all of which are subject to revision, which raises the issue of which values economists were forecasting. Laster *et al.* also study economists' forecasts of real GDP growth, so the caveats that apply to Lamont apply to Laster *et al.* as well. In addition, the Blue Chip Indicators data Laster *et al.* use groups forecasters by industry rather than identifying them individually; hence the incentives to forecast strategically are not as strong.

Our finding that the *Wall Street Journal's* panel of economists cannot predict changes in interest rates and exchange rates more accurately than a random walk model is not surprising, given the efficiency of financial markets. What is perhaps surprising is that so many of the panel forecast significantly worse than the random walk model. The explanation of these results we favor is that many of the economists face incentives that reward the exceptionally right guess but do not equally penalize the exceptionally wrong guess. An alternative explanation is that even if the economists know the random walk model to be more accurate over time, this leaves them with no story to spin about their forecasts. Always telling customers that you predict no change in interest rates or exchange rates may simply be too truthful to keep one employed.

4. Conclusions

While widespread public interest in forecasts of macroeconomic variables has led professional economists to put considerable effort in generating forecasts, less effort has gone into assessing the quality of these forecasts. The theory of rational expectations implies that professional economists' forecasts should be unbiased and identical given access to the same information and similar incentives with respect to predictive accuracy. Previous studies employing survey data of professional economists' forecasts to assess forecast quality have tended to lack comprehensiveness, suffer from data problems, or produce inconclusive results.

This paper has sought to help fill the void by using semi-annual survey data from the *Wall Street Journal*'s panel of economists to study interest rate and exchange rate forecasts of individual economists. We found that while about 60% of the surveyed economists produced unbiased estimates, virtually all failed to make 6-month ahead forecasts of the Treasury bill rate, Treasury bond rate and yen-dollar exchange rate that beat a naïve random walk model for accuracy, and many made forecasts significantly less accurate than the random walk model. When we tested for homogeneity of interest rate and exchange rate forecasts, we found them to be systematic heterogeneous. In particular, we found that independent economic forecasters (those not employed by banks, security firms, corporations' finance departments, or econometric model firms) made significantly lower forecasts of the Treasury bill rate and Treasury bond rate and significantly higher forecasts of the yen-dollar exchange rate. Evidence of systematically heterogeneous forecasts led us to consider whether economists faced economic incentives to produce heterogeneous forecasts. When we estimated an incentives model combining elements of models estimated by Lamont (2002) and Laster *et al.* (1999), we found evidence that economists who would be expected to gain the most from favorable publicity – those employed by firms named for them – make more extreme forecasts, whereas economists employed by other institutions tend to make more conservative, less extreme forecasts. We found no evidence that economists become more radical with age. If anything, experienced economists appear to preserve their reputations by deviating less from the consensus forecast than inexperienced economists.

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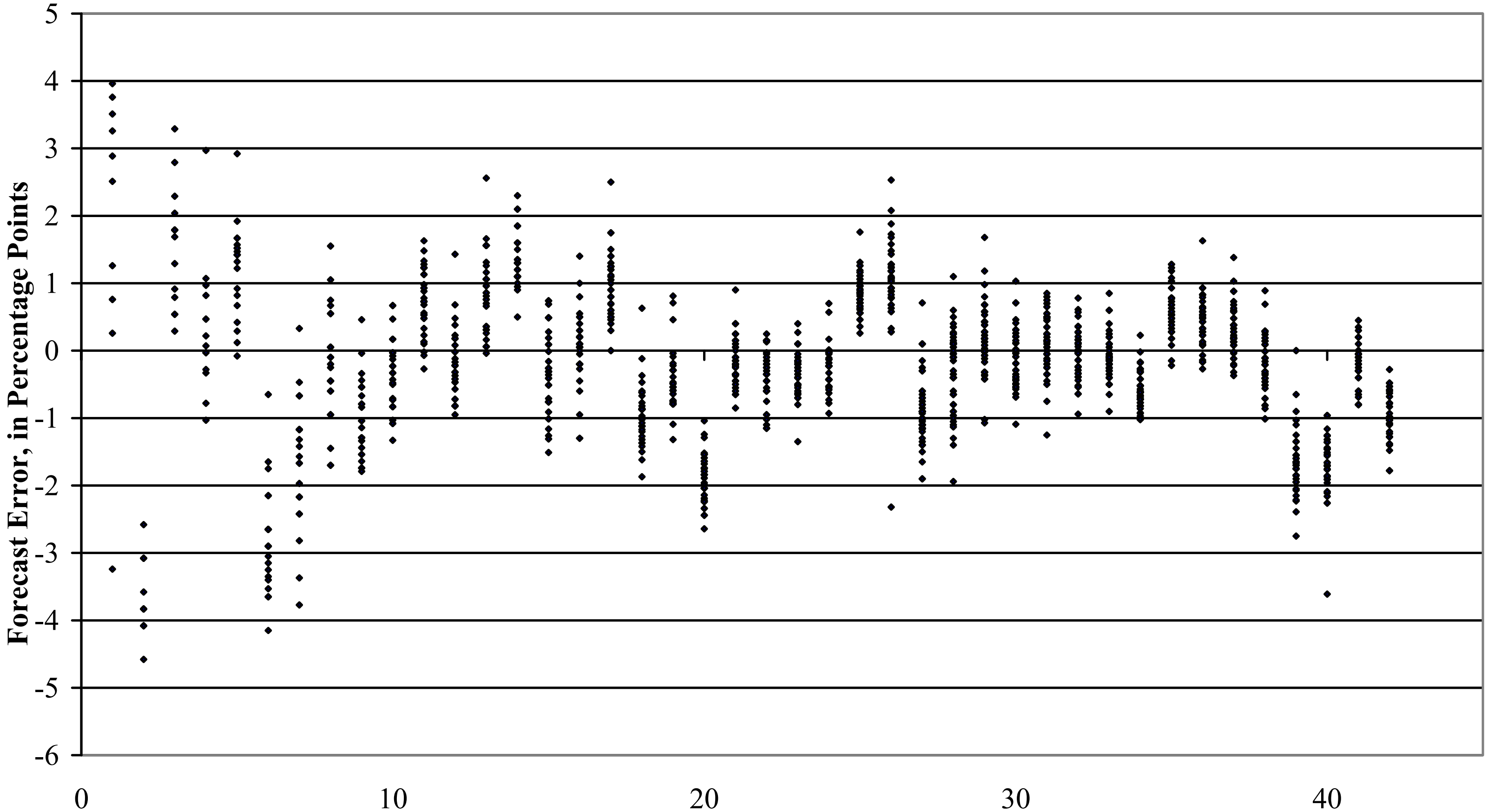
Figure 1

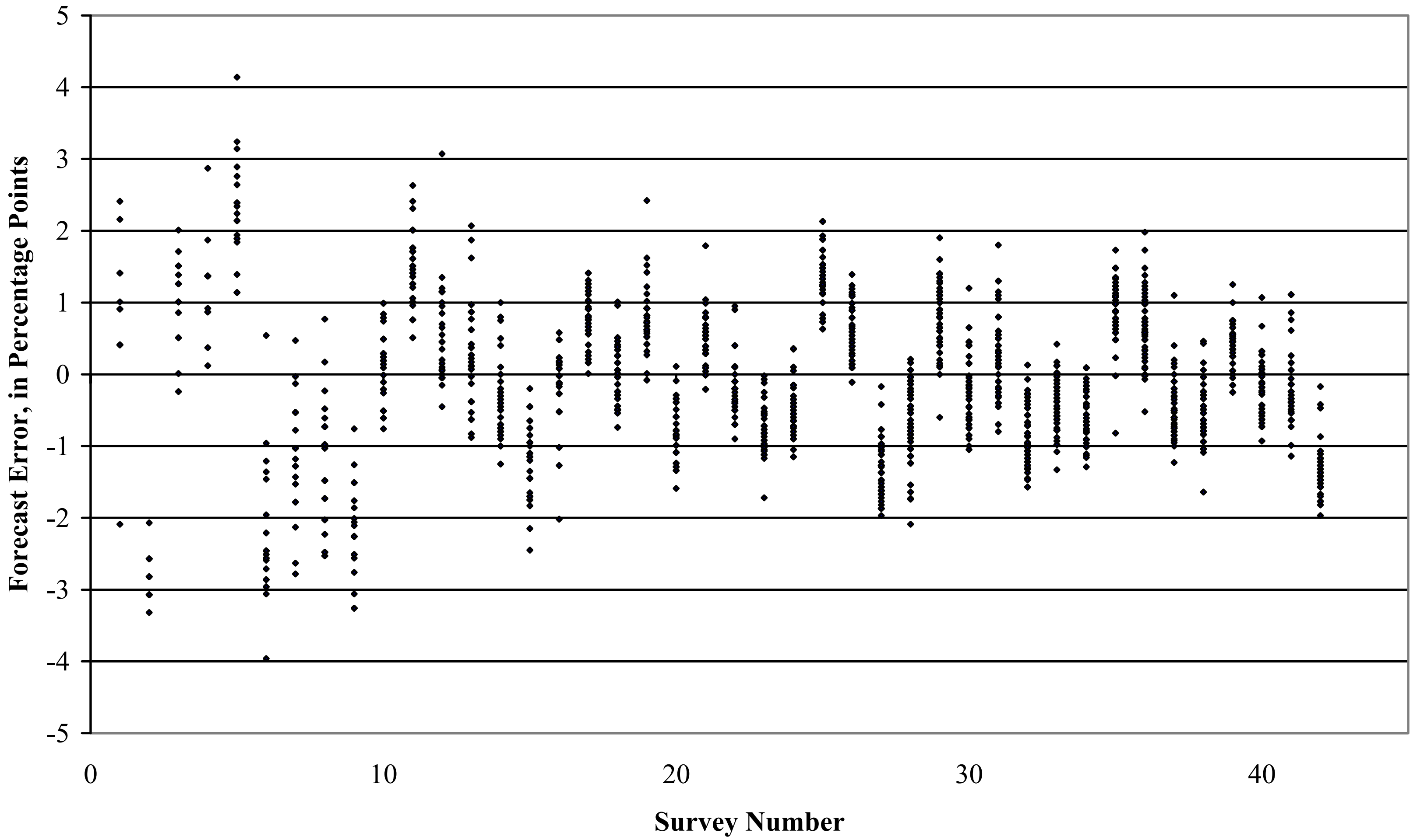
Forecast Errors of the Treasury Bill Rate



Note: Forecast errors are measured as the actual rate minus forecasters' predictions on the survey date, six months earlier. Forecast errors are shown for the 42 surveys beginning with January 1982 and ending with July 2002.

Figure 2

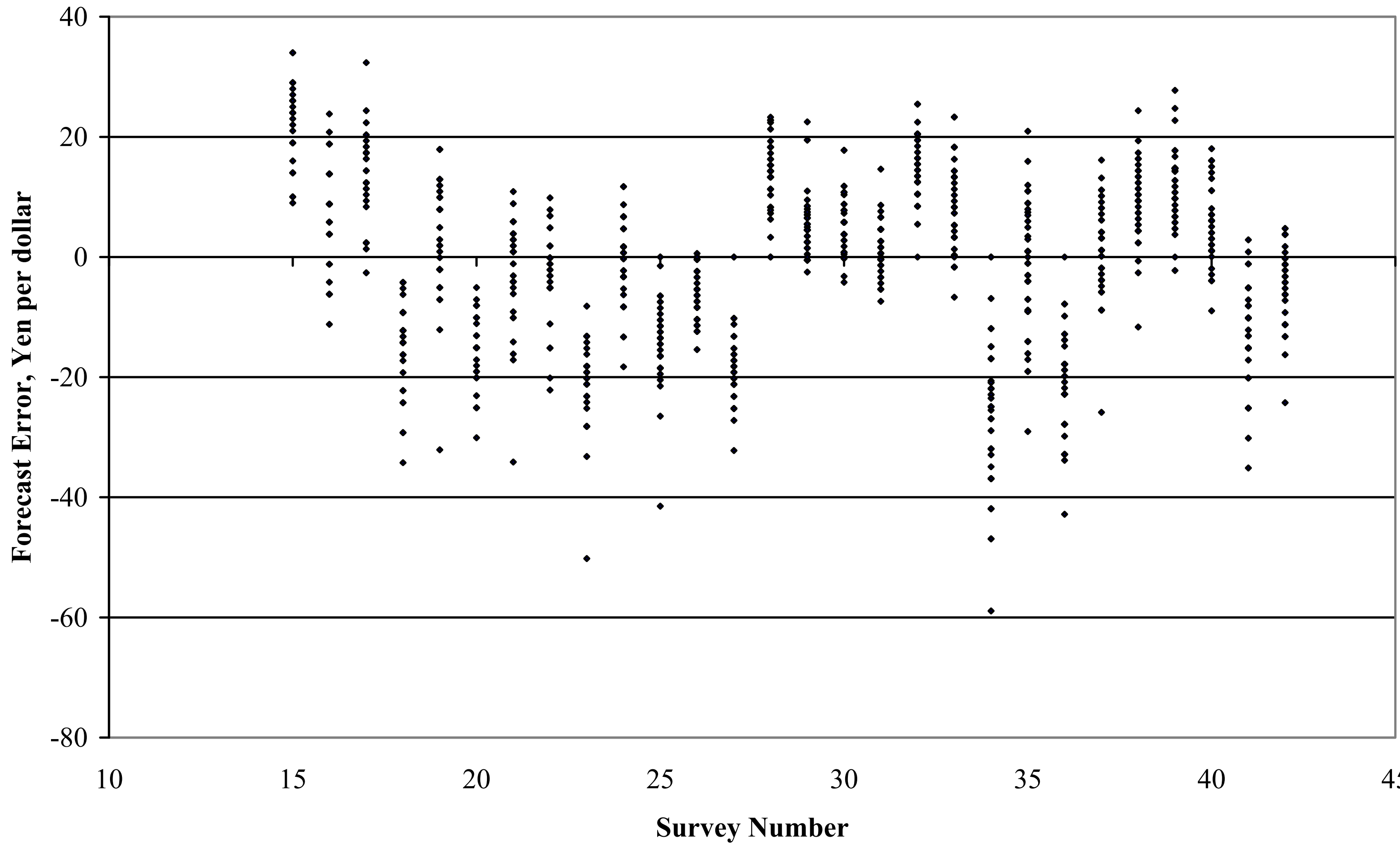
Forecast Errors for theTreasury Bond Rate



See notes to Figure 1.

Figure 3

Forecast Errors for the Yen-Dollar Exchange Rate



Note: Forecasts of the yen-dollar exchange rate were added to the *Wall Street Journal* survey in January 1989. Forecast errors are shown for the 28 surveys from January 1989 to July 2002, which correspond to survey numbers 15-24 in our sample.

Table 1
Summary Statistics for Survey Forecasts

Survey Date	Treasury bill Rate			Treasury bond Rate			Yen-Dollar Rate		
year_mo	Mean S.D.	Range N	Actual	Mean S.D.	Range N	Actual	Mean S.D.	Range N	Actual
1982_01	11.06	8.8-16		13.05	11.5-16				
	2.05	12	12.76	1.13	12	13.91			
1982_07	11.61	10.5-12.5		13.27	12.5-13.75				
	.54	14	7.92	.35	14	10.43			
1983_01	7.37	5.5-9.625		10.11	9-11.625				
	.94	17	8.79	.71	17	11.01			
1983_07	8.60	6-10		10.59	9-11.75				
	.89	17	8.97	.60	17	11.87			
1984_01	8.72	7-10		11.39	9.5-12.5				
	.64	24	9.92	.68		13.64			
1984_07	10.62	8.5-12		13.75	11-14.75				
	.76	24	7.85	.85	24	11.54			
1985_01	8.56	6.5-10.6		11.60	10-13.25				
	.98	24	6.83	.80	24	10.47			
1985_07	7.31	5.5-8.75		10.51	8.5-11.8				
	.82	25	7.05	.83	25	9.27			
1986_01	6.96	5.5-7.75		9.45	8-10.5				
	.58	25	5.96	.63	25	7.24			
1986_07	6.02	5-7		7.41	6.5-8.25				
	.51	30	5.67	.51	30	7.49			
1987_01	4.98	4.1- 6		7.05	5.88-8				
	.48	35	5.73	.53	35	8.51			
1987_07	5.91	4.25-6.63		8.45	5.88-9.4				
	.50	35	5.68	.66	35	8.95			
1988_01	5.70	4-6.6		8.65	6.8-9.75				
	.58	36	6.56	.71	36	8.87			
1988_07	6.78	5.8-7.6		9.36	8-10.25				
	.39	32	8.1	.56	32	9			
1989_01	8.29	7.25-9.5		9.25	8.25-10.5		121.37	110-135	
	.60	38	7.99	.49	38	8.05	6.15	38	144
1989_07	7.76	6.4-9.1		8.12	7.4-10		136.53	120-135	
	.52	38	7.8	.48	38	7.98	8.47	38	143.8
1990_01	7.03	5.5-8		7.62	7-8.4		137.78	120-155	
	.48	40	8	.35	40	8.41	6.81	40	152.35
1990_07	7.56	6-8.5		8.16	7.25-9		149.78	140-170	
	.43	40	6.63	.40	40	8.26	7.14	40	135.75
1991_01	6.14	4.9-7.03		7.65	6-8.5		133.65	120-170	
	.42	40	5.71	.46	40	8.42	9.69	40	137.9
1991_07	5.84	5-6.6		8.22	7.3-9		140.78	130-155	
	.35	40	3.96	.38	40	7.41	5.61	40	124.9
1992_01	3.80	2.75-4.5		7.30	6-8		127.64	115-160	
	.34	42	3.65	.37	42	7.79	8.07	42	125.87
1992_07	3.54	2.9-4.3		7.61	6.45-8.3		127.33	115-147	
	.39	42	3.15	.38	42	7.4	7.07	42	124.85
1993_01	3.41	2.7-4.45		7.44	6.7-8.4		127.70	115-157	
	.32	44	3.1	.33	44	6.68	7.07	44	106.8

Table 1, continued

Survey Date	Treasury bill Rate			Treasury bond Rate			Yen-Dollar Rate		
year_mo	Mean S.D.	Range N	Actual	Mean S.D.	Range N	Actual	Mean S.D.	Range N	Actual
1993_07	3.34 .31	2.37-4 44	3.07	6.84 .35	5.99-7.5 44	6.35	112.16 6.44	100-130 44	111.7
1994_01	3.40 .28	2.5-4 51	4.26	6.26 .38	5.5-7 51	7.63	113.10 5.90	100-140 49	98.51
1994_07	4.67 .60	3.15-8 58	5.68	7.30 .39	6.5-8.1 58	7.89	106.85 3.69	99-115 52	99.6
1995_01	6.50 .49	4.89-7.5 59	5.6	7.94 .38	6.8-8.6 59	6.63	104.09 4.00	95-117 57	84.78
1995_07	5.44 .56	4-7.04 62	5.1	6.61 .52	5.75-8.05 62	5.96	89.23 4.24	80-100 60	103.28
1996_01	4.98 .45	3.5-6.25 64	5.18	6.03 .44	5-7.5 64	6.9	104.71 4.56	87-112 62	109.48
1996_07	5.31 .40	4.18-6.3 58	5.21	6.86 .47	5.45-7.7 58	6.65	109.99 4.25	98-120 56	115.77
1997_01	5.16 .41	4.4-6.5 57	5.25	6.52 .52	5-7.6 57	6.8	113.45 4.15	100-122 55	114.61
1997_07	5.41 .35	4.58-6.3 55	5.36	6.79 .40	5.8-7.5 55	5.93	114.89 4.66	105-125 54	130.45
1998_01	5.18 .30	4.25-6 56	5.1	6.02 .37	5.2-6.95 56	5.62	130.41 7.03	115-145 54	138.29
1998_07	5.08 .25	4.25-5.5 55	4.48	5.72 .36	5-6.38 55	5.09	141.28 10.38	120-172 53	113.08
1999_01	4.20 .33	3.5-5 54	4.78	5.05 .44	4.25-6.8 54	5.98	122.77 9.93	100-150 52	120.94
1999_07	4.89 .34	3.7-5.6 54	5.33	5.83 .48	4.5-7 54	6.48	124.75 7.19	110-145 53	102.16
2000_01	5.58 .35	4.5-6.25 53	5.88	6.38 .40	4.8-7.13 53	5.9	105.32 7.20	90-132 53	106.14
2000_07	6.11 .41	5-6.9 53	5.89	6.01 .39	5-7.1 53	5.46	105.34 5.94	90-126 53	114.35
2001_01	5.36 .38	4.3-6.4 52	3.65	5.35 .31	4.5-6 54	5.75	113.21 5.39	97-127 53	124.73
2001_07	3.39 .42	2.7-5.35 54	1.74	5.28 .40	4-6 54	5.07	126.48 6.18	113-140 54	131.04
2002_01	1.89 .32	1.25-2.5 55	1.7	5.06 .51	3.75-6 55	4.86	132.76 7.34	117-115 55	119.85
2002_07	2.19 .33	1.5-3 54	1.22	5.21 .36	4-6.25 55	3.83	123.58 6.53	110-143 55	118.75

Note: Survey respondents are asked early in January and July for their forecasts for the last business day of July and December, respectively. The mean, standard deviation (S.D.) and range of the forecasts in each survey are shown. The number of respondents (N) varies across surveys. The actual values of the variables forecasted are shown in the "Actual" column.

Table 2
Participants Responding To At Least Twenty Surveys

Person	Firm	start	end	gaps	missing dates
David Berson	Fannie Mae	199001	200207	0	
Paul Boltz	T. Rowe Price	198401	199801	0	
Philip Braverman		198401	199901	0	
	Briggs Schaedle	198401	198807		
	Irving Securities	198901	198907		
	DKB Securities	199001	199901		
Dewey Daane	Vanderbilt Univ.	198807	200207	0	
Robert Dederick	Northern Trust	198607	199607	0	
Gail Fosler	Conference Board	199101	200207	0	
Maury Harris		198607	200207	0	
	Paine Webber Inc.	198607	200007		
	UBS Warburg	200107	200207		
Richard Hoey		198401	199401	1	199107
	A.G. Becker	198401	198407		
	Drexel Burnham	198501	199101		
	Dreyfus Corp.	199201	199401		
Stuart G. Hoffman	PNC Bank, Fin Serv	198801	200207	1	199401
William Hummer		199301	200207	0	
	Wayne Hummer	199301	199707		
	Hummer Invest.	199807	200207		
Edward Hyman		198301	200207	1	198901
	C.J. Lawrence	198301	199107		
	ISI Group	199201	200207		
Saul Hymans	Univ. of Michigan	198607	200207	0	for yen:199407 199607 199807 199901
David Jones	Aubrey G. Lanston	198201	199301	0	
Irwin Kellner	ManuHan-Chem-Chase	198201	199701	1	198407
Carol Leisenring	CoreStates Finl.	198707	199801	0	
Alan Lerner		198201	199307	1	198401
	Bankers Trust	198201	199207		
	Lerner Consulting	199301	199301		
Mickey Levy		198507	200207	0	
	Fidelity Bank	198507	199107		
	CRT Govt. Securities	199201	199307		
	NationsBank Cap. Mk	199401	199807		
	Bank of America	199901	200207		
Arnold Moskowitz		198401	200007	1	198807
	Dean Witter	198401	199107		
	Moskowitz Capital	199201	200007		
John Mueller	LBMC	199107	200207	2	199401 199507
Elliott Platt	Donaldson Lufkin(DLJ)	198807	200001	1	199207
Maria Ramirez		199207	200207	1	199401
	Ramirez Inc.	199207	199307		
	MF Ramirez	199407	200107		
	MFR	200201	200207		
Donald Ratajczak		198701	200101	0	
	Georgia State Univ.	198701	200001		
	Morgan Keegan	200007	200101		
David Resler		198407	200207	0	
	First Chicago	198407	198701?		
	Nomura Securities I	198707	200207		
Alan Reynolds		198607	200001	1	199501
	Polyconomics	198607	199107		
	Hudson Institute	199201	200001		
Richard Rippe		199001	200207	0	
	Dean Witter	199001	199107		
	Prudential Securities	199201	200207		

Table 2, continued
Participants Responding To At Least Twenty Surveys

Person	Firm	start	end	gaps	missing dates
Norman Robertson		198201	199601	1	199407
	Mellon Bank	198207	199207		
	Carnegie Mellon	199301	199601		
A. Gary Shilling	Shilling & Co.	198201	200207	4	198307 198401 198901 198907
Alan Sinai		198201	200207		198807 199707
	Data resources	198207	198307		
	Lehman Bros Shearson	198401	198801		
	The Boston Co.(Lehman)	198901	199207		
	Economic Advisors Inc (Lehman)	199301	199307		
	Lehman Brothers	199401	199701		
	WEFA Group	199801	199801		
	(Primark) Decision Economic	199807	200207		
James Smith		198701	200207	2	198807 199401
	UT-Austin	198701	198801		
	Univ. of N.C.	198901	199901		
	Natl Assn of Realtors	199907	200001		
	Univ. of N.C.	200007	200207		
Donald Straszheim		198607	200207	11	198807 199707-200201
	Merril Lynch	198607	199701		
	Strszheim Global Advisors	200207	200207		
Raymond Worseck	A.G. Edwards	198901	199901	0	
David Wyss		198401	200207	4	198807 199407(yen) 200001-200101
	Data Resources	198401	199907		
	Standard & Poor's (McGraw-Hill)	200107	200207		
Edward Yardeni		198607	200007	1	198807
	Prudential Bache	198607	199107		
	C.J. Lawrence	199201	199507		
	Deutsche Bank	199601	200007		

Table 3
Unbiasedness and Accuracy of Treasury Bill Rate Forecasts

Individual	Liu-Maddala Restricted CointegrationTest of Unbiasedness ADF(forecast) ADF(Δforecast)	 ADF(error) Q(4)	Mean Forecast Error and t-test for Unbiasedness	Fraction of Correct Directions (p-value for independence test)[a]	χ^2 and Pesaran-Timmerman Tests of Independence[b]	Accuracy Σ (A-F)^2 n	 MSE Ratio to Random Walk (Modified DM statistic)[c]
David	-3.149**	-2.426**	-.351	.577	.735	17.488	.877
Berson	-3.030**	4.260	(-2.369)**	(.453)	.765	26	(-.754)
Paul	-2.720*	-2.901***	-.460	.517	.348	39.928	1.929
Boltz	-2.833*	.541	(-2.257)**	(.694)	.361	29	(1.810)*
Phillip	-3.768***	-4.680***	.203	.483	1.178	37.695	1.780
Braverman	-3.931***	1.696	(1.027)	(.368)	1.217	31	(1.225)
Dewey	-2.289	-2.775***	-.382	.517	.348	21.981	.984
Daane	-3.632**	2.200	(-2.584)**	(.694)	.361	29	(-.066)
Robert	-1.559	-2.758***	-.084	.524	.029	13.270	1.008
Dederick	-2.984**	2.752	(-.477)	(1.000)	.031	21	(.039)
Gail	-3.171**	-3.313***	-.514	.542	.697	25.241	1.402
Fosler	-4.061***	6.633	(-2.776)**	(.653)	.728	24	(1.370)
Maury	-1.571	-3.185***	-.092	.545	.308	22.264	.958
Harris	-3.275**	2.009	(-.639)	(.728)	.318	33	(-.211)
Richard	-1.660	-2.290**	-.425	.350	.848	25.598	1.674
Hoey	-2.334	3.560	(-1.765)*	(.613)	.892	20	(1.698)
Stuart G.	-1.954	-3.245***	-.164	.621	1.830	20.978	.966
Hoffman	-3.870***	.842	(-1.043)	(.264)	1.896	29	(-.160)
William	-2.047	-1.819*	-.380	.600	1.250	14.282	1.038
Hummer	-2.516	2.019	(-2.190)**	(.582)	1.316	20	(.220)
Edward	-1.784	-4.399***	.289	.564	.416	47.690	1.515
Hyman	-4.026***	6.248	(1.672)	(.706)	.427	39	1.076
Saul	-2.545	-2.828***	-.196	.455	.203	28.911	1.245
Hymans	-3.900***	8.681	(-1.210)	(.733)	.209	33	(2.010)*
David	-1.701	-2.770***	-.316	.391	1.245	67.325	1.533
Jones	-4.117***	4.205	(-.882)	(.400)	1.301	23	(1.052)
Irwin	-3.635**	-4.828***	-.102	.333	3.274*	51.619	1.190
Kellner	-4.854***	1.172	(-.421)	(.141)	3.387*	30	(1.480)
Carol	-1.669	-2.430**	.025	.455	.188	12.913	.982
Leisenring	-3.114**	3.773	(.147)	(1.000)	.197	22	(-.081)
Alan	-1.765	-3.887***	-.583	.652	1.806	51.187	1.188
Lerner	-5.333***	6.775	(-1.990)*	(.221)	1.888	23	(.505)
Mickey	-2.409	-3.810***	-.152	.514	.000	28.724	1.175
Levy	-4.476***	3.691	(-.991)	(1.000)	.000	35	(.888)

Arnold Moskowitz	-2.800* -4.842***	-3.934** 3.671	-.078 (-.425)	.333 (.072)*	4.332** 4.468**	36.167 33	1.863 (1.512)
John Mueller	-2.937* -3.442**	-2.221** 3.907	-.310 (-1.512)	.238 (.030)**	5.743** 6.030**	26.525 21	1.711 (.996)
Elliott Platt	-2.725* -3.202**	-3.248*** 2.597	.077 (.461)	.522 (1.000)	.034 .035	14.410 23	1.092 (.379)
Maria Ramirez	-2.117 -2.585	-1.692* 1.803	-.374 (-2.678)**	.600 (.319)	1.684 1.772	10.209 20	.810 (-.593)
Donald Ratajczak	-2.023 -3.382**	-3.022*** .705	-.135 (-.939)	.586 (.462)	.909 .941	17.279 29	.897 (-.506)
David Resler	-2.485 -4.057***	-4.401*** 3.540	-.099 (-.629)	.514 (1.000)	.036 .037	33.284 37	1.117 (.658)
Alan Reynolds	-1.331 -2.891*	-1.995** 7.928	.104 (.569)	.519 (1.000)	.030 .031	23.776 27	1.662 (1.711)*
Richard Rippe	-3.192** -3.667**	-2.583** 1.481	-.349 (-2.185)**	.577 (.428)	1.009 1.049	19.738 26	.990 (-.051)
Norman Robertson	-2.562 -4.123***	-3.836*** 3.265	-.207 (-.841)	.571 (.701)	.289 .300	47.190 28	1.034 (.133)
A. Gary Shilling	-3.126** -5.300***	-3.388*** 2.056	.338 (1.446)	.553 (1.000)	.080 .082	80.992 38	1.428 (1.110)
Alan Sinai	-2.086 -4.320***	-4.063*** 5.303	-.278 (-1.459)	.525 (1.000)	.102 .105	59.551 40	1.075 (.292)
James Smith	-2.660 -3.588**	-2.577** 9.800*	.202 (.882)	.467 (.358)	1.701 1.760	46.689 30	2.415 (2.560)**
Donald Straszheim	-1.035 -1.936	-2.347** 2.171	-.076 (-.465)	.524 (1.000)	.002 .002	12.906 22	1.171 (.169)
Raymond Worseck	-2.049 -2.828*	-2.390** 1.238	-.291 (-1.619)	.524 (.656)	.404 .424	15.336 21	1.464 (1.657)
David Wyss	-2.208 -3.958***	-4.242*** 2.417	-.210 (-1.301)	.559 (.728)	.215 .222	30.722 34	1.336 (1.180)
Edward Yardeni	-1.928 -3.110**	-2.626*** .868	.254 (1.626)	.393 (.102)	4.044* 4.194*	20.197 28	1.690 (2.339)**
Survey Mean	-2.647 -4.950***	-4.309*** 1.709	-.223 (-1.318)	.524 (1.000)	.096 .098	51.444 42	.891 (-557)

Notes:
***, **, * signify statistical significance at the .01, .05, and .10 levels
[a] The number in parentheses is the significance level of the test for independence of predicted and actual changes using the Fisher exact test.
[b] These are Chi-square statistics for the test of independence of predicted and actual changes, see Pesaren and Timmerman (1992)
[c] The modified DM test is the modification of the Diebold-Mariano (1995) test of differences in squared forecast errors given in Harvey *et al* (1997).

Table 4
Unbiasedness and Accuracy of Treasury bond Rate Forecasts

Individual	Liu-Maddala Restricted Cointegration Test of Unbiasedness ADF(forecast) ADF(Δforecast)	 ADR(error) Q(4)	Mean Forecast Error and t-test for Unbiasedness	Fraction of Correct Directions (p-value for independence)	χ^2 and Pesaran-Timmerman Tests of Independence	Forecast Accuracy Σ (A-F)^2 n	 MSE Ratio to Random Walk (Modified DM statistic)
David	-1.424	-4.789***	-.163	.269	5.110**	15.612	1.388
Berson	-5.626***	8.454	(-1.074)	(.043)**	5.310**	26	(2.963)***
Paul	-3.171**	-2.857***	-.455	.414	.232	40.280	1.664
Boltz	-3.529**	2.837	(-2.216)**	(.669)	.240	29	(2.199)**
Phillip	-5.037***	-3.891***	.269	.581	.057	42.084	1.664
Braverman	-4.235***	1.226	(1.298)	(1.000)	.059	31	(1.377)
Dewey	-2.382	-4.107***	-.490	.310	2.653	25.412	2.088
Daane	-6.463***	4.773	(-3.254)***	(.164)	2.748	29	(2.431)**
Robert	-1.894	-4.993***	-.046	.409	.833	13.946	1.533
Dederick	-4.943***	4.133	(-.254)	(.659)	1.458	21	(2.216)**
Gail	-1.312	-2.392**	-.590	.500	.825	22.078	1.999
Fosler	-4.553***	7.005	(-3.742)***	(.615)	.861	24	(2.187)**
Maury	-1.191	-5.221***	.095	.545	.021	19.213	1.426
Harris	-4.870***	8.784	(.713)	(1.000)	.021	33	(1.668)
Richard	-2.140	-2.602**	-.443	.300	3.039*	41.128	2.135
Hoey	-2.535	11.496**	(-1.414)	(.160)	3.199*	20	(2.274)**
Stuart G.	-1.695	-4.168***	-.183	.345	3.131*	13.755	1.304
Hoffman	-5.522***	4.667	(-1.462)	(.128)	4.137**	29	(1.942)*
William	-1.631	-3.236***	-.387	.300	1.832	12.605	1.300
Hummer	-4.453***	10.435*	(-2.434)**	(.290)	1.928	20	(1.354)
Edward	-1.501	-4.109***	.501	.538	.030	59.230	2.123
Hyman	-5.486***	7.866	(2.743)***	(1.000)	.031	39	(1.801)*
Saul	-1.402	-5.403***	-.186	.455	.122	20.005	1.486
Hymans	-5.948***	12.111**	(-1.390)	(1.000)	.520	33	(2.073)*
David	-2.074	-3.124***	-.276	.478	.048	39.840	1.252
Jones	-3.742**	2.073	(-1.006)	(1.000)	.050	23	(.967)
Irwin	-2.579	-4.899***	-.159	.433	2.143	38.332	1.190
Kellner	-7.460***	7.124	(-.767)	(.272)	2.217	30	(.676)
Carol	-1.522	-5.804***	-.010	.591	.282	10.413	1.175
Leisenring	-6.388***	8.473	(-.067)	(.655)	.002	22	(.941)
Alan	-2.183	-3.882***	-.523	.652	1.806	43.875	1.525
Lerner	-4.813***	4.164	(-1.921)*	(.685)	.320	23	(2.129)**
Mickey	-2.581	-6.895***	-.088	.514	.008	28.397	1.471
Levy	-7.662***	5.468	(-.571)	(1.000)	.150	35	(2.153)**

Arnold Moskowitz	-2.831* -6.454***	-5.387*** 5.660	.012 (.055)	.424 (.278)	1.636 1.688	45.956 33	1.764 (1.706)*
John Mueller	-1.397 -4.429***	-1.842* 7.100	-.362 (-2.035)*	.381 (.361)	1.527 1.604	16.028 21	1.796 (2.154)**
Elliott Platt	-2.569 -4.903***	-4.729*** 4.268	.069 (.385)	.435 (.680)	.434 .454	16.210 23	1.593 (2.221)**
Maria Ramirez	-1.435 -5.654***	-2.077** 4.222	-.456 (-3.708)***	.350 (1.000)	.019 .020	9.906 20	1.206 (.949)
Donald Ratajczak	-1.152 -4.745***	-5.111*** 5.544	-.092 (-.634)	.310 (.067)*	3.948** 5.798**	17.389 29	1.469 (2.948)***
David Resler	-3.229** -4.704***	-4.442*** 3.581	.018 (.105)	.541 (.687)	.315 1.016	37.129 37	1.510 (2.558)**
Alan Reynolds	-1.482 -3.878***	-2.964*** 2.142	.204 (1.229)	.407 (.420)	1.187 1.232	20.397 27	2.031 (2.778)**
Richard Rippe	-1.196 -6.679***	-3.391*** 3.371	-.137 (-.911)	.308 (.105)	3.718** 3.867**	15.103 26	1.343 (1.472)
Norman Robertson	-2.248 -4.483***	-4.526*** 3.287	-.201 (-.828)	.286 (.030)**	5.320** 5.517**	45.725 28	1.254 (2.124)**
A. Gary Shilling	-2.636* -5.943***	-3.083*** 2.280	.534 (2.754)***	.553 (1.000)	.011 .011	63.702 38	1.761 (2.111)**
Alan Sinai	-2.275 -5.397***	-5.222*** 4.684	-.027 (-.146)	.500 (.730)	.234 .240	51.929 40	1.293 (1.299)
James Smith	-1.391 -5.143***	-4.429*** 3.802	.604 (3.431)***	.600 (1.000)	.599 .620	37.865 30	3.222 (2.228)**
Donald Straszheim	-1.120 -4.352***	-4.463*** 5.540	.004 (.021)	.476 (1.000)	.043 .046	15.843 22	1.560 (2.291)**
Raymond Worseck	-.587 -4.222***	-3.240*** 2.295	-.177 (-.972)	.429 (.659)	.531 1.458	14. 601 21	1.503 (1.803)*
David Wyss	-3.683** -4.514***	-4.753*** 3.412	-.137 (-.831)	.294 (.032)**	6.103** 6.287**	31.063 34	1.147 (.906)
Edward Yardeni	-1.152 -5.295***	-3.493*** 7.406	.575 (3.896)***	.536 (1.000)	.778 .807	25.757 28	2.182 (2.346)**
Mean	-2.459 -5.832***	-5.570*** 7.109	-.135 (-.832)	.333 (.024)**	6.133** 6.283**	46.418 42	1.132 (1.072)

Notes: See notes to Table 3

Table 5
Unbiasedness and Accuracy of Yen-Dollar Exchange Rate Forecasts

Individual	Liu-Maddala Restricted Cointegration Test of Unbiasedness		Mean Forecast Error and t-test for Unbiasedness	Fraction of Correct Directions (p-value for independence)	χ^2 and Pesaran-Timmerman Tests of Independence	Forecast Accuracy	
	ADF(forecast) ADF(Δforecast)	ADF(error) Q(4)				Σ (A-F)^2 n	MSE Ratio to Random Walk (Modified DM statistic)
David	-2.504	-2.721***	-3.118	.385	2.275	5175.980	1.518
Berson	-3.589**	1.681	(-1.133)	(.217)	2.366	26	(2.452)**
Paul	-1.122	-2.120**	2.563	.474	.003	3301.963	1.397
Boltz	-2.735*	4.258	(.841)	(1.000)	.003	19	(1.930)*
Phillip	-2.007	-2.847***	-.204	.667	2.291	3404.713	1.113
Braverman	-3.097**	1.481	(-.072)	(.198)	2.405	21	(.381)
Dewey	-2.105	-3.209***	2.873	.393	1.011	6518.140	1.729
Daane	-3.535**	3.265	(.996)	(.441)	1.048	28	(2.012)*
Robert	-.791	-2.185**	1.146	.563	.152	3109.605	1.518
Dederick	-2.042	3.752	(.320)	(1.000)	.163	16	(1.921)*
Gail	-3.116**	-2.699**	2.701	.542	.697	4957.834	1.621
Fosler	-3.357**	3.660	(.918)	(.653)	.728	24	(1.828)*
Maury	-1.917	-2.695**	-2.724	.571	.324	5034.540	1.336
Harris	-3.212**	3.536	(-1.078)	(.698)	.336	28	(1.642)
Richard	-1.370	-1.984**	4.253	.500	.000	2685.864	2.170
Hoey	-2.073	3.865	(.786)	(1.000)	.000	10	(2.201)**
Stuart G.	-1.874	-2.980***	-1.251	.444	.759	4941.500	1.374
Hoffman	-2.827*	3.403	(-.474)	(.448)	.788	27	(2.028)*
William	-1.755	-2.432**	.240	.550	.135	3451.686	1.197
Hummer	-2.847*	2.423	(.080)	(1.000)	.142	20	(1.400)
Edward	-2.179	-2.260**	-5.529	.543	.675	5159.600	1.513
Hyman	-3.404**	2.403	(-2.225)**	(.569)	.701	27	(2.025)*
Saul	-1.982	-2.291**	1.873	.458	.084	3194.330	1.055
Hymans	-2.312	3.291	(.789)	(1.000)	.088	25	(.593)
David	-.792	-1.722*	.136	.444	.225	1648.664	1.364
Jones	-1.962	2.238	(.028)	(1.000)	.253	9	(2.071)*
Irwin	-1.135	-2.831***	3.762	.647	2.082	2955.657	1.442
Kellner	-3.155**	3.259	(1.191)	(.294)	2.212	17	(1.056)
Carol	-1.138	-1.947*	-.385	.526	.003	2809.424	1.190
Leisenring	-1.606	4.245	(-.134)	(1.000)	.003	19	(.904)
Alan	-1.537	-.814	-7.008	.500	.476	2839.654	2.301
Lerner	-2.670*	2.892	(-1.372)	(1.000)	.529	10	(2.358)**
Mickey	-1.842	-2.598**	-3.438	.607	.778	4672.100	1.239
Levy	-3.257**	4.886	(-1.435)	(.560)	.867	28	(1.350)

Arnold Moskowitz	-1.373 -2.827*	-2.315** 2.750	-2.802 (-.960)	.583 (.673)	.243 .358	4893.624 24	1.399 (1.635)
John Mueller	-2.405 -2.739*	-2.550** 3.444	2.911 (1.063)	.524 (.659)	.311 .327	3329.745 21	1.311 (.826)
Elliott Platt	-1.764 -3.366**	-2.376** 3.983	-1.493 (-.495)	.636 (.384)	1.352 1.416	4245.175 22	1.239 (1.331)
Maria Ramirez	-2.369 -2.784*	-2.648** 6.150	-2.993 (-.920)	.500 (1.000)	.159 .167	4202.448 20	1.550 (1.908)*
Donald Ratajczak	-1.683 -3.186**	-3.075*** 3.363	2.600 (.927)	.400 (.653)	.329 .343	4886.268 25	1.357 (1.716)*
David Resler	-1.673 -3.116**	-2.991*** 4.052	-1.367 (-.580)	.536 (1.000)	.050 .052	4245.559 28	1.126 (1.132)
Alan Reynolds	-1.309 -2.814*	-2.296** 2.255	-.762 (-.279)	.591 (.666)	.627 .657	3470.269 22	1.082 (.466)
Richard Rippe	-2.688* -3.759***	-2.942*** 1.791	.305 (.118)	.577 (.453)	.735 .765	4343.981 26	1.275 (1.621)
Norman Robertson	-.327 -2.730*	-2.072** 2.063	-.216 (-.058)	.571 (1.000)	.286 .308	2517.032 14	1.254 (1.109)
A. Gary Shilling	-2.298 -3.653**	-1.483 2.917	-13.233 (-3.983)***	.538 (1.000)	.763 .793	11728.621 26	3.441 (3.582)***
Alan Sinai	-2.613 -3.434**	-2.506** 3.374	-1.653 (-.554)	.519 (1.000)	.008 .008	6320.800 27	1.796 (1.654)
James Smith	-1.800 -4.013***	-1.616 3.248	-11.881 (-4.713)***	.630 (.407)	1.511 1.569	9506.039 27	2.644 (2.294)**
Donald Straszheim	-1.093 -3.058**	-3.770*** 4.067	1.350 (.476)	.588 (.620)	.701 .745	2237.738 18	1.092 (.293)
Raymond Worseck	-1.305 -3.308**	-1.530 6.685	-3.109 (-1.003)	.571 (.673)	.269 .283	4235.650 21	1.385 (1.297)
David Wyss	-2.522 -3.551**	-2.805*** 2.847	.080 (.024)	.542 (1.000)	.168 .175	6049.966 24	1.693 (3.278)***
Edward Yardeni	-1.578 -2.717*	-2.302** 2.356	-4.860 (-1.810)*	.667 (.163)	3.055 3.187	4546.241 24	1.300 (1.360)
Mean	-1.941 -3.147**	-2.838*** 3.596	-1.529 (-.645)	.464 (.687)	.491 .509	4594.172 28	1.219 (2.114)**
Notes: See notes to Table 3							

Table 6
Tests of Heterogeneity of Forecasts Across Survey Respondents

Dependent variable: Deviation of an individual's time t forecast from the mean time t forecast

Data set	Panel 1[3]			Panel 2[4]		
Number of forecasters	93	93	79	33	33	33
Number of forecasts	1650	1650	1280	924	924	722
Forecast variable	T-Bill rate	T-Bond rate	Yen/$ rate	T-Bills rate	T-Bonds Rate	Yen/$ Rate
Panel A: Models with Individual Dummy Variables						
Tests for individual effects[1]	4.09***	8.63***	6.76***	5.96***	15.38***	12.23***
Panel B: Models with Employment Dummy Variables						
Banks	-.009 (.039)	-.025 (.038)	.837 (.594)	-.013 (.056)	-.041 (.053)	.343 (.784)
Security firms	-.044 (.036)	-.145*** (.035)	.423 (.540)	-.054 (.049)	-.136*** (.046)	-.175 (.656)
Independent Forecasters	-.158*** (.044)	-.262*** (.043)	1.653** (.653)	-.240*** (.062)	-.350*** (.059)	2.618*** (.824)
Corporate forecasters	-.033 (.083)	-.090 (.080)	1.874 (1.214)	na	Na	na
Econometric models	-.047 (.064)	-.107 (.062)	-1.483 (.974)	.014 (.077)	-.062 (.074)	-2.552** (1.113)
Constant	.047 (.031)	.108 (.030)	-.582 (-1.28)	.015 (.041)	.069 (.039)	-.454 (.529)
F test for differences across employers[2]	3.46***	10.91***	2.93**	4.95***	10.58***	5.92***

, * represent statistical significance at the .05 and .01 levels

[1] This F statistic tests that the coefficients for all individuals are the same.

[2] This F statistic tests that the coefficients for all employer types are the same.

[3] Panel 1 includes all economists having at least 6 forecasts.

[4] Panel 2 includes all economists having at least 20 forecasts.

Table 7
OLS Estimates of Incentives Model

Dependent variable: Absolute value of the deviation of an economist's time t forecast from the time t forecast mean excluding that economist

Data set	Panel 1			Panel 2		
Number of forecasters	93	93	79	33	33	33
Number of forecasts	1650	1650	1280	924	924	722
Forecast variable	T-Bill	T-Bond	Yen/$	T-Bill	T-Bond	Yen/$
AGE	-.0018* (.0011)	-.0021** (.0010)	-.0428*** (.0149)	-.0022 (.0015)	-.0029** (.0014)	-.0435** (.0206)
AGE*MODEL	.0002 (.0045)	-.0041 (.0042)	.0214 (.0720)	.0040 (.0054)	-.0011 (.0049)	-.0165 (.0956)
AVEDEV	.8436*** (.0512)	.6983*** (.0765)	.8610*** (.0793)	1.0475*** (.0830)	.9218*** (.1148)	.6490*** (.1108)
OWN	.1697*** (.0382)	.1298*** (.0364)	1.7425*** (.5638)	.2185*** (.0514)	.2042*** (.0470)	1.6198** (.6782)
Independent but not OWN	.0527 (.0333)	.0710** (.0318)	.2293 (.4760)	.0370 (.0505)	.1095** (.0462)	.1236 (.6422)
Banks	-.0742*** (.0269)	-.0944*** (.0257)	-.9469*** (.3983)	-.1388*** (.0396)	-.1574*** (.0362)	-1.9637*** (.5339)
Securities firms	-.0254 (.0248)	.0115 (.0236)	-.3453 (.3616)	-.0844** (.0344)	-.0495 (.0316)	-1.7803*** (.4485)
Corporate forecasters	-.1133** (.0572)	-.0966* (.0539)	-.7845 (.8384)			
Econometric Models	-.1476** (.0334)	-.0974 (.0698)	-1.1935 (1.3083)	-.2706*** (.0962)	-.2020** (.0875)	-1.1726 (1.9129)
Constant	.0979*** (.0334)	.1492*** (.0397)	1.5665*** (.5343)	.0836* (.0502)	.1319** (.0573)	3.4837*** (.7448)
F test for differences across industries	9.20***	10.53***	4.40***	11.82***	14.38***	8.51***
R^2	.185	.097	.101	.218	.150	.100
*, **, and *** represent statistical significance at the .10, .05, and .01 levels						