

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

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Abstract

Recent work on expectations suggests that professional forecasters may have incentives that lead them to make more extreme forecasts than they would make were accuracy the only criterion. We use the interest rate and exchange rate forecasts from the *Wall Street Journal's* panel of economists to investigate this issue. We examine the unbiasedness and forecast accuracy of individual forecasters, finding that several forecasters produce biased forecasts and most forecasters cannot out-predict a random walk model. Our tests show evidence of systematic heterogeneity across forecasters and are consistent with independent forecasters making more radical predictions than forecasters from financial institutions.

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I. Introduction

Forecast surveys of professional economists are of widespread interest because they potentially quantify unobservable economic expectations, which play a central role in economic and financial theory and behavior. Financial market researchers use forecasts of macroeconomic variables for a variety of purposes, such as distinguishing expected from unexpected macroeconomic announcements in studies of financial market reactions to economic news.¹ Forecasts of interest rates and exchange rates potentially enable researchers to separate the confounding effects of expectations and time-varying risk premiums and so advance the theories of interest-rate and exchange-rate determination.² But while the value of having observable data on economic expectations is clearly great, many economists remain skeptical that forecast survey data adequately measure unobservable expectations.

At issue is whether survey respondents' forecasts represent unbiased expectations based on informed opinion or whether the forecasts are biased or strategic because respondents face other incentives. The theory of rational expectations holds that economists having access to the same information about the macro economy will produce identical forecasts of macroeconomic variables, assuming they have the same loss function with respect to forecast errors.³ But Ito (1990) and MacDonald and Marsh (1996) report evidence of significant, systematic heterogeneity across respondents. In addition, Laster *et al.* (1999) and Lamont (2002) find that some forecasters appear to make forecasts that are more extreme than their true expectations, consistent with theories of strategic forecasting behavior.

¹ See, for example, Anderson et al (2003) and the references therein.

² See Frankel and Froot (1987) and MacDonald (2000).

³ For a discussion of how different loss functions affect forecasters under rationality, see Elliott, Komunjer, and Timmermann (2003)

This paper investigates the individual forecasts of knowledgeable, accountable survey respondents to determine whether their forecasts are unbiased, whether they exhibit systematic heterogeneity, and whether they indicate strategic behavior. To this end we analyze interest rate and exchange rate forecasts of the *Wall Street Journal's* panel of economists, a highly visible but relatively little studied survey. This survey is particularly well-suited for our purposes because it is taken on a regular basis and because the economists surveyed have their names and employers published along side their forecasts in the *Wall Street Journal*, presumably giving them incentives to think carefully about their forecasts. We choose to study the economists' interest rate and exchange rate forecasts because forecasts of these variables appear in the largest number of surveys and because the actual values of the interest rates and exchange rate are never subject to subsequent revision, unlike, say, GDP, so there is no question about the actual values the economists were predicting. We proceed by studying the forecasts of individual economists in the survey as well as the survey means, allowing for the possibility that the variables being forecasted are nonstationary. We are unaware of previous papers that allow for nonstationarity in the actual data when applying tests of forecast unbiasedness to individual data.

To preview our results, we find that several individual economists' forecasts appear biased 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 be able to do as well as a random walk. We find that the economists' forecasts exhibit the same kind of heterogeneity found by Ito (1990) and MacDonald and Marsh (1996). 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 fail to corroborate Lamont's finding that economists make more extreme forecasts as they age.

The rest of the paper is organized as follows. Section II briefly reviews some of the past work on evaluating survey measures of expectations. Section III describes our data. Section IV reports our empirical results. Section V offers some conclusions.

II. Review of Past Work

Though the vast literature analyzing survey data on expectations of economic variables has examined a variety of issues, three concerns have dominated recent work.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 individual responses to the Livingston survey produces stronger evidence of bias in the expected inflation series than tests using survey averages. Keane and Runkle (1990) find that individual data from the Survey of Professional Forecasters (SPF) are generally unbiased but Bonham and Cohen (2001) find that the individual SPF forecasts should not be pooled and that many of the individual forecasts are biased.⁵ Researchers who use individual rather than pooled data can test for systematic heterogeneity across respondents. 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.

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.

A second issue is whether nonstationarity of the variables being forecasted renders invalid the standard unbiasedness test.⁶ Liu and Maddala (1992) find that Money Market Services (MMS) data on exchange rate forecasts appear nonstationary but are cointegrated with the actual data. They introduce a restricted cointegration test and find that the forecasts are unbiased. Aggarwal *et al.* (1995) and Schirm (2003) find that only about half the variables forecasted by the MMS surveys appear unbiased after testing for nonstationarity and cointegration. But Osterberg (2000), applying the Liu-Maddala techniques to more recent MMS data, finds that the MMS exchange rate forecasts are unbiased. These tests all use the median responses from the MMS surveys.

A third issue is whether survey respondents report their true expectation or whether they behave strategically. Scharfstein and Stein (1990) and Erbeck and Waldmann (1996) argue that the incentive structure facing respondents leads to “herding,” that is, making forecasts that are close to the mean or “consensus” forecast. Alternatively, Laster *et al.* (1999) and Lamont (2002) suggest that incentives lead respondents to make forecasts potentially at variance with their true expectations because respondents are rewarded not only for being right but for being right when others are wrong. Such forecasts are potentially more extreme than the forecasts resulting from incentives that reward accuracy alone. 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 individual forecasters are not identified, only the industry of their employment.⁷ Lamont (2002) uses *Business Week’s* annual set of economists’ forecasts for real GDP growth, inflation, and unemployment to test whether forecasters who own

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.

⁷ 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.

their own firms, and hence may gain the most from publicity, make more radical predictions. He finds evidence supporting this hypothesis as well as evidence that forecasters produce forecasts that deviate more from the mean forecast as they age.

After describing the survey data used in this paper, we employ these data to investigate the issues just discussed. We investigate aspects of the rationality of individual forecasters through tests of unbiasedness, taking into account the possible nonstationarity of the data, and through tests of predictive accuracy. We then test for systematic heterogeneity and strategic behavior.

III. Description of the 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.⁸ 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 in the July survey they are asked for their forecasts for the last business day of December.⁹ The

⁸ 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).

⁹ Respondents have often been asked for 12-month ahead forecasts but these are not available for the entire period.

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.¹⁰

Several researchers have used the *Wall Street Journal* data, 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. To our knowledge, however, 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.

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 longer 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

¹⁰ 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.

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.¹¹

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.

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 appears to be rejected by the data. Figure 1 indicates that for about half the surveys the errors in predicting the Treasury bill rate are largely of one sign, suggesting that while expectations vary across individuals a common source exists for at least some of the error. There is more support for this interpretation in Figures 2 and 3 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

¹¹ 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.

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.¹²

IV. Evaluating the Survey Data

A. Tests of Unbiasedness

Most evaluations of expectations and forecasts begin with tests for 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

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

where ε_t is a random error term. A forecast series is unbiased if the joint hypothesis that $\alpha=0$ and $\beta=1$ cannot be rejected.¹³

It is well-known that 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 also has to 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 $\alpha=0$ and $\beta=1$ 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

¹² 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.

¹³ 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.

series.¹⁴ We perform the Liu-Maddala test below after first establishing whether A and F are I(1).

To establish that the actual series are I(1) we perform unit root tests on daily T-bill, T-bond and exchange rate data sampled at six-month intervals, the data frequency that matches our forecast series. 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).¹⁵

To establish that the forecast series are I(1) we perform unit root tests on the Treasury-bill, Treasury-bond and yen-dollar exchange rate forecast series of the thirty-three individuals listed in Table 2 who responded to at least 20 surveys. 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, that is three sets of forecasts each for thirty-three respondents, 71 appear to be I(1) using the 10% significance level or better.

To complete the Liu-Maddala test we impose the restriction that $\alpha=0$ and $\beta=1$ on [1], use the actual and forecast series to compute the forecast errors, and perform ADF tests to determine if 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.

¹⁴ Papers employing this restricted cointegration test include Hakkio and Rush (1989) and Osterberg (2000).

¹⁵ 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.

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 forecast series 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. 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.¹⁶ Pass rates on the Liu-Maddala test are greater for the Treasury-bond and yen-dollar exchange rate series: 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. Of the eleven exchange rate forecast series that failed, three had ten or fewer observations. In summary, two-thirds (67) of the 99 forecast series pass the Liu-Maddala test of unbiasedness. In addition, the three series of mean survey responses pass the Liu-Maddala test, as indicated in the last row of each table.

Lopes(2000) provides evidence that the power of the restricted cointegration test may be low, as is generally the case with unit root tests. He provides Monte Carlo evidence that a simple t-test for the hypothesis that the average forecast error is zero has more power in finite samples. Accordingly we also report, in column 4 of each table, the mean forecast error and its t-statistic. Using this test we would reject unbiasedness of the Treasury bill forecasts for 7 of the 33 forecasters at the 5% level and 9 at the 10% level. All the forecasters exhibiting statistically significant mean errors for the Treasury bill rate made predictions that were too high on average. The t test rejects unbiasedness for 9 (5%) and 11 (10%) of the sets of forecasts of the Treasury

¹⁶ 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.

bond rate. About two-thirds of these forecasters also erred on the high side. For the dollar-yen exchange rate, only 3 (5%) and 4(10%) of the mean forecast errors were significantly different from zero and all erred on the high side. For the survey mean forecasts, the t-tests indicate that the mean forecast errors were not significantly different from zero.

In summary, the interest rate forecasts exhibit more cases of bias, usually in the upward direction, than do the exchange rate forecasts. Based on the t tests for unbiasedness, which may be more powerful for our sample sizes, 19 or about 60 percent of the forecasters passed the unbiasedness tests for all three series, 4 failed for one series, and 10 failed for two series. No one failed for all three series.¹⁷

B. Measures of predictive ability

Users of forecasts are undoubtedly concerned with predictive ability as well as unbiasedness so in this section we present two approaches to measuring predictive accuracy.¹⁸ One measure is whether forecasters can predict the direction of change correctly.¹⁹ As noted above, previous studies of the *Wall Street Journal* survey data find little evidence that respondents can beat a coin flip, with the exception of Cho (1996) who found that eighty percent of the respondents did better for the Treasury bill rate. Columns five and six of Tables 3-5 provide evidence on the ability of survey respondents to predict correctly the direction of six-month changes. Column 5 reports the fraction of predicted changes that were correct along with the p-value of the hypothesis test that predicted and actual changes were independent, using Fisher's exact test. Column 6 reports the standard χ^2 statistic and the Pesaran-Timmerman

¹⁷ If a more stringent 5 % level is used rather than 10%, two fail the test once and nine fail the test twice.

¹⁸ See Carroll (2003) for evidence that households use the reported forecasts of professional economists in forming their own expectations.

¹⁹ Leitch and Tanner (1991) argue that the direction of change is more closely related to profits from trading Treasury-bill futures contracts than, say, the mean square error for interest rate predictions.

(1992) test statistic, also with a χ^2 distribution with 1 degree of freedom, for the test of independence of predicted and actual changes.²⁰

For the Treasury bill rate about two-thirds of the forecasters predicted the direction of change correctly more than half the time, but no forecaster got a significantly higher fraction of correct directions than would be expected by chance. A few got a significantly lower fraction. For the Treasury bond rate, about two-thirds forecasted the direction of change correctly less than half the time but few got a significantly lower fraction than would be expected by chance. For the exchange rate, about 80 percent of the forecasters predicted the direction of change correctly more than half the time but, again, no one got a higher fraction than would be expected by chance. For the survey means, the predicted change of direction was not significantly different from a coin flip for the Treasury bill rate and the exchange rate but was significantly worse for the Treasury bond rate. The basic message from these results is that the survey respondents do not provide useful information on the direction of change.²¹

Our second approach to judging predictive power is to compare their forecasts to the traditional benchmark of the accuracy of a naïve model of no change, a model which assumes that the variables forecasted follow a random walk without drift. To evaluate the forecasters against this benchmark we compute the ratio of the mean square errors (MSEs) of each forecaster's forecast series to the MSEs of forecast series covering the same dates but using as forecasts the six-month-earlier actual values, that is, the actuals on the last business day in December to forecast values for the last business day in June, and actuals on the last business day

²⁰ 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.

²¹ 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.

of June to forecast values for the last business day in December. These actuals are usually published along side the forecasts in the *Wall Street Journal*. Using the random walk model as the benchmark, the question becomes whether individual forecasters can outperform the random walk model.

The last two columns of Tables 3-5 report our results. The next-to-the last column reports the number of forecasts made by each forecaster together with the sum of the squared forecast errors. The final column reports the ratio of each forecaster's MSE to the MSE from a random walk model for the period that the forecaster participated in the survey. Ratios less than one indicate forecasters who are more accurate than a random walk. Judging by this metric, eight of the thirty-three forecasters beat a random walk for the Treasury-Bill rate, but no forecaster beat the random walk model for either the Treasury-Bond rate or the yen-dollar exchange rate. The ratios for the survey mean predictions indicate that the mean forecast had a smaller MSE than a random walk model for the Treasury bill rate but not for the Treasury bond rate or the exchange rate.

To test whether there is a statistically significant difference between an individual's forecast and the random walk prediction of no change, we computed the modified Diebold-Mariano (1995) test statistic proposed by Harvey *et al.* (1997). This statistic tests whether the mean of the difference between the squared forecast errors of the survey respondent and the squared forecast errors of the random walk model is significantly different from zero. The statistic, reported in parentheses in the last column, has a t-distribution under the null hypothesis that the mean is zero. For the Treasury bill rate forecasts, no respondent does significantly better than a random walk model (i.e. a t-statistic that is significantly less than zero) and five do significantly worse at the 10% level. For the Treasury bond rate forecasts, about two-thirds of the respondents do significantly worse than a random walk model while about half do

significantly worse for the exchange rate forecasts. The mean square errors of the survey mean forecasts for the interest rates are not significantly different from those from a random walk but the mean square error for the survey mean forecast of the exchange rate is significantly higher than that of a random walk.

Taken together, the measures of forecast accuracy suggest that investors would usually have been wise to assume the interest rates and exchange rate would stay at their last observed levels rather than use the survey forecasts. Moreover the large forecast errors are consistent with behavioral models in which forecasters have incentives to forecast large changes.

C. Tests of systematic heterogeneity of forecasts

As Ito (1990) and Keane and Runkle (1990) note, forecasters who form expectations rationally using the same information should produce homogenous (identical) forecasts. A priori reasoning suggests that professional economic forecasters almost surely have access to the same macroeconomic information relevant to forecasting interest rates and exchange rates. Thus, should we observe these forecasters to make heterogeneous forecasts, we could reasonably attribute the heterogeneity to differences in the models they use or to differences in the incentives respondents face in making forecasts.²² In this section we examine whether the forecasts of the economists in the *Wall Street Journal* survey are homogeneous or systematically heterogeneous and in the next section examine whether the economists face different incentives.

Ito (1990) tests for heterogeneity of exchange rate forecasts made by Japanese economists using a fixed-effects model. He assumes that economists' forecasts are a function of common information, I_t , an individual effect represented by an individual-specific dummy variable, g_j , and a random error term representing, say, measurement error:

²² Heterogeneous forecasts could also be due to forecaster irrationality. As mentioned above, Elliott *et al.* (2003) discuss how different loss functions would produce heterogeneity.

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

It further assumes that $f(I_t)$ contains a constant so that the average of the g_j s may be set to zero.

Averaging equation [2] across all economists and then subtracting the average from [2] yields

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

It tests for heterogeneity by estimating [3] and testing that the estimated values of g_j are identical across economists.²³ He finds that the data reject this hypothesis. When Ito groups the economists by industry of employment and re-estimates [3] with employment effects rather than individual effects he finds that the data again reject the hypothesis. In particular he finds that economists employed in export industries have a depreciation bias whereas economists in the import business have an appreciation bias, which he terms the “wishful thinking” effect.²⁴

Following Ito we estimate [3] using two sub-samples of the Treasury-bill rate, Treasury-bond rate and the yen-dollar exchange rate forecasts from the respondents to the *Wall Street Journal* survey. The first sub-sample, which we call Panel 1, includes all economists with at least six survey responses; the second, Panel 2, includes all economists with at least twenty responses, the group whose forecasts were examined in sections A and B.²⁵ We first estimate [3] with dummy variables for individual economists; then we re-estimate [3] using employment dummy variables in place of individual dummies.

Table 6 presents the results of our tests for heterogeneous forecasts. The top part of the table presents F tests of the null hypothesis that the g_j estimates for individual economists are

²³ 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.

²⁴ 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.

²⁵ These are unbalanced panels since participants change over time.

identical. The F tests reject the null hypothesis at the 0.01 level for all the data sets, indicating the presence of significant individual effects.

The bottom part of Table 6 reports estimates of [3] using employment dummy variables in place of individual dummies. Coefficient estimates appear as the top number in each cell, standard errors as the bottom number. The coefficient estimates indicate that, compared with other economists, independent forecasters make 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 make lower forecasts of the Treasury-bond rate, but not as low as those employed by independent firms. The forecasts of economists affiliated with banks do not deviate significantly from the consensus, nor do the forecasts of economists employed by econometric modeling firms except for yen-dollar exchange rate forecasts, which are lower for Panel 2. The last row of Table 6 reports F tests of the null hypothesis that g_j estimates for the employment dummies are identical. The data soundly reject the null hypothesis. Thus the evidence from the *Wall Street Journal* survey suggests that the forecasters' forecasts are indeed systematically heterogeneous. This finding leads us to investigate whether individual forecasters behave strategically in making their forecasts.

D. Tests of strategic forecasting

Laster *et al.* (1999) and Lamont (2002) suggest that the incentive structure facing professional forecasters potentially motivates them to supply heterogeneous forecasts. Specifically if forecasters are rewarded both for forecast accuracy and for standing out from the crowd, forecasters may announce predictions more extreme than their conditional expectations. We examine this possibility using the interest rate and exchange rate forecasts of the economists

in the *Wall Street Journal* surveys. As noted above, these data are well-suited for tests of incentives because the names of the individuals and their firms accompany the forecasts.

To investigate the effects of incentive structure on forecasts Lamont (2002) models forecasters' payoff function as follows:

$$w_j = R(|f_j - a|, |f_j - f_{c(j)}|) \quad [4]$$

where w_j = payoff to forecaster j

f_j = forecast of forecaster j

a = actual value

$f_{c(j)}$ = mean forecast of panel other than forecaster j

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 , could be positive, negative, or zero. If R_2 is zero forecasters will make forecasts that represent their conditional expectations but if R_2 is non-zero forecasters face incentives to behave strategically. If $R_2 > 0$ a forecaster is rewarded for differentiating his forecast from those of other forecasters, behavior which Lamont calls "scattering". But if $R_2 < 0$ the forecaster is rewarded for making a forecast similar to the forecasts of others, behavior which Lamont calls "herding". Lamont also suggests that if R_2 is indeed non-zero forecasters' incentives to scatter or herd might change with age. For example, incentive structures might encourage young forecasters to make extreme forecasts so as to gain publicity but encourage older forecasters to herd so as to protect the reputations they have established. Alternatively incentives might encourage young, unestablished forecasters to herd so as to hide their inexperience but encourage seasoned, secure forecasters to make more radical forecasts. Lamont concludes that the sign of R_2 is an empirical question.

Lamont examines individual economists' forecasts of real GDP growth, the unemployment rate, and CPI inflation from annual *Business Week* surveys published between 1971 and 1992. Regressing $|f_j - f_{c(-j)}|$ on the age of forecasters and a dummy for self-employment, Lamont finds that individuals deviate more from the mean forecasts the longer the individuals are in the survey, consistent with incentives to scatter that grow with age, and when they own their own forecasting firms, consistent with incentives to scatter when seeking publicity. Lamont also finds that individuals' forecast accuracy deteriorates with age although this effect disappears after controlling for the behavior of self-employed forecasters.

Lamont's findings have been partly corroborated by other researchers working with different data sets. Laster *et al.* (1999) apply Lamont's basic approach to forecasts of real GDP growth from 1976 to 1996 from the Blue Chip Economic Indicators, a survey that groups forecasters by industry rather than identifying them individually. Like Lamont, Laster *et al.* find that forecasters from independent forecasting firms make more extreme forecasts, suggesting that they behave strategically.²⁶ Pons-Novell (2003) also found differences in absolute deviations from the consensus across industry-affiliations using the Livingston forecasts of the unemployment rate, but did not find that forecasters make more radical forecasts as they age.

We re-examine the strategic behavior hypothesis by estimating a model combining the elements of Lamont and Laster *et al.* using the data from the *Wall Street Journal* surveys:

$$|f_j - f_{c(-j)}|_t = \beta_0 + \beta_1 \text{AGE}_{j,t} + \beta_2 \text{AGE}_{j,t} * \text{MODEL}_{j,t} + \beta_3 \text{AVEDEV}(-j)_t + \beta_4 \text{OWN}_{j,t} + \sum \gamma_i D_{i,t} + \varepsilon_{j,t} \quad [5]$$

The dependent variable is the absolute value of the difference between an individual's forecast and the average forecast omitting that individual's forecast. AGE is the number of years a fore-

²⁶ Laster *et al.* (1999) find no evidence of systematic bias across industry groups, unlike Ito (1990)

caster had appeared in the *Wall Street Journal* survey at the time of survey t and $AVEDEV(-j)$ is the average absolute deviation of the forecasts from the mean, omitting forecaster j . This latter variable controls for variations in the spread of the forecasts over time. Following Lamont we include a dummy variable, OWN , that equals one if a forecaster is employed at a firm that bears his name as well as an interaction term $AGE*MODEL$ that allows the effect of forecaster age to differ if the forecaster is employed by an econometric modeling firm.²⁷ Finally we add dummy variables for the industry of the firm where individual i works at the time of survey t , the D_{itS} . We use the same industries as Laster *et al.*: banks, securities firms, independent forecasters, nonfinancial corporations, econometric modelers, and all others.

Table 7 reports estimates of [5] for the forecasts of the Treasury-bill, Treasury-bond and yen-dollar exchange rates for respondents with at least six forecasts (Panel 1) and at least twenty forecasts (Panel 2). Our estimates show clear evidence of strategic behavior by forecasters but generally contravene Lamont's evidence of more extreme forecasts the longer forecasters remain in the survey. Indeed, we find the opposite to be true: forecasters' absolute deviations from the mean tend to decline the longer they are included in the survey, as evidenced by coefficients on AGE that are negative and usually statistically significant.²⁸ The estimated effects are, however, small. Someone in the survey for 10 years (20 surveys) would be expected to be about 4 basis points closer to the mean interest rate forecast than a first-time respondent. The analogous effect for the exchange rate is a little less than one yen. Thus, there is no evidence of these forecasters

²⁷ Lamont found that this variable was important and that the effect of age was not significant for forecasts from econometric models.

²⁸ 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.

becoming more radical with age.²⁹ We also find no difference in the AGE effect for forecasters employed by econometric modeling firms.

The estimated coefficients on the industry dummies, reported in Table 7, are similar to those reported by Lamont and Laster *et al.* As Lamont found, forecasters whose firms bear their names consistently deviate more from the survey mean forecasts than other forecasters. The estimated difference ranges from 13 to 22 basis points for the interest rates and averages 1.7 yen for the exchange rate. There is less evidence that economists affiliated with independent firms that do not bear their names make more radical forecasts. While the estimated coefficients on the dummy for employment at independent firms are positive, they are substantially smaller than those on the OWN dummy and are statistically significant only for the Treasury-bond rate forecasts. Similar to the results of Laster *et al.*, economists affiliated with commercial banks deviated less from the mean forecast. Econometric modelers and corporate economists also tended to deviate less but the estimated effects are not always significant. F statistics indicate significant differences in forecast deviations across the industries. Since it seems unlikely that forecasters in different industries have differential access to the macroeconomic data needed to make interest rate and exchange rate forecasts, we conclude that incentive structures encourage forecasters employed in different industries to supply heterogeneous forecasts, with forecasters from firms bearing their names being more likely to make extreme forecasts because they gain the most from being right when others are wrong.³⁰

²⁹ 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.

³⁰ We also estimated equation [5] 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.

V. Conclusions

Expectations play integral roles in both economic theories and real life. While researchers usually assume that expectations are rational, little direct evidence exists to justify this assumption. Past studies investigating survey data on the expectations of professional forecasters have been inconclusive. This paper uses the semi-annual survey data from the *Wall Street Journal's* panel of economists and finds that individual interest rate and exchange rate forecasts frequently violate the unbiasedness assumption of rational expectations and are generally less accurate than random walk models. The forecasts also exhibit systematic heterogeneity across forecasters.

While rationality is usually defined as minimizing the variance of forecast errors, Lamont (2002) and Laster *et al.* (1999) suggest that forecasters may have rational reasons for making more extreme forecasts. We find evidence that forecasters that would be expected to gain the most from favorable publicity, namely those from firms named for the forecaster, make more extreme forecasts whereas forecasters from commercial banks, who would be expected to be more conservative, make less extreme forecasts. We find no evidence that forecasters become more radical with age. If anything, experienced forecasters appear to preserve their reputations by deviating less from the consensus forecast than inexperienced forecasters.

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Figure 1

Forecast Errors for Treasury Bill Rate

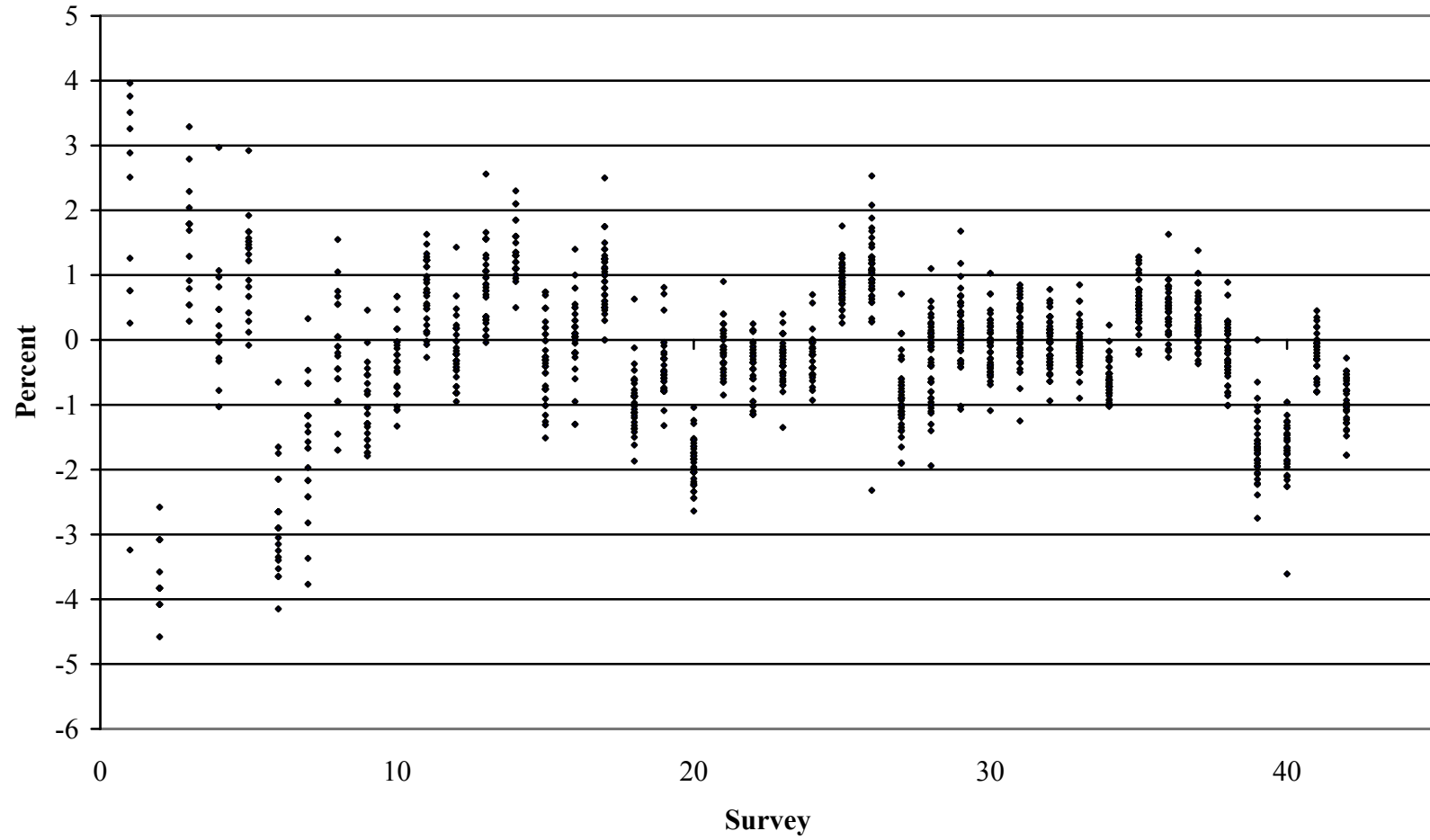


Figure 2

Forecast Errors for Treasury Bond Rate

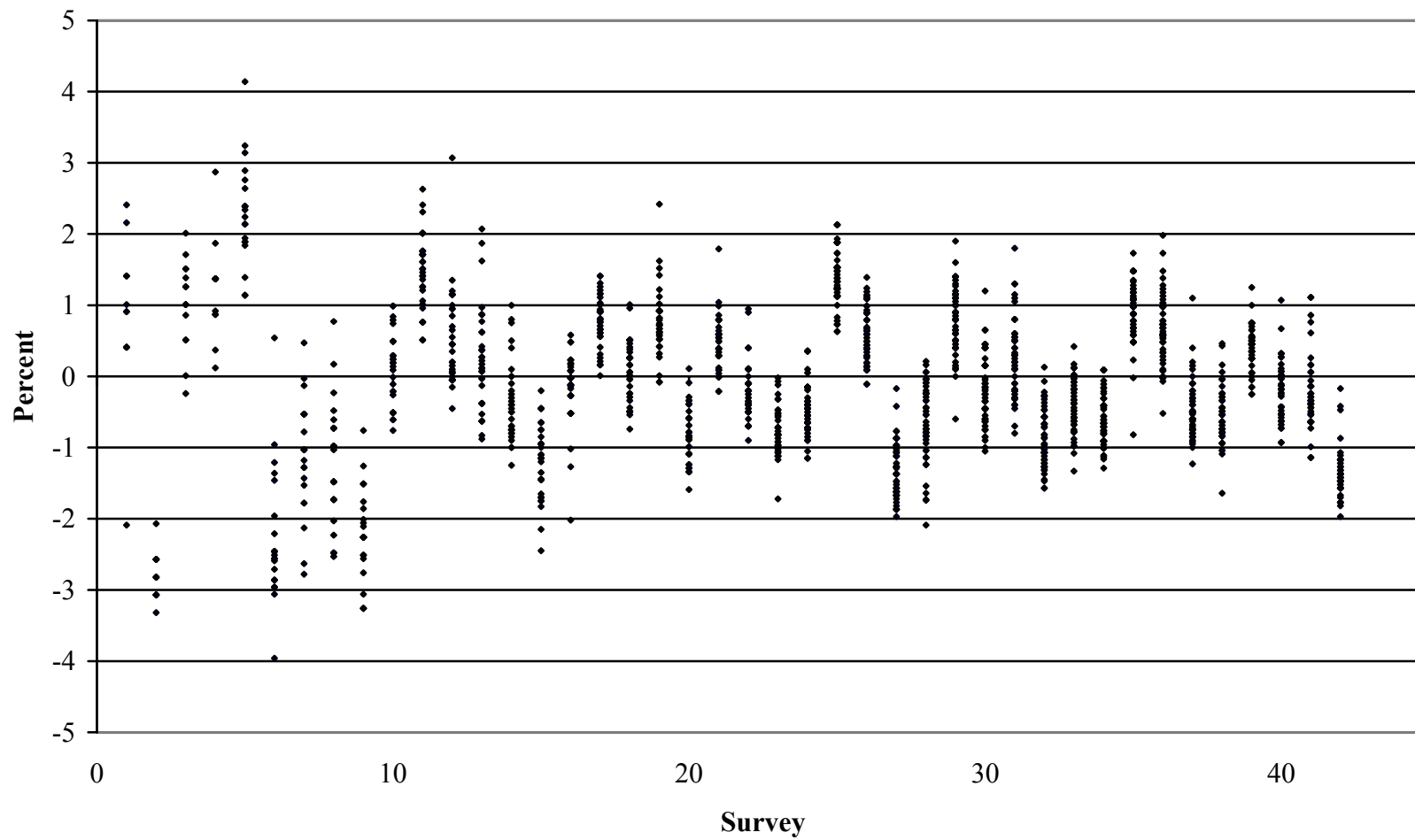


Figure 3

Forecast Errors for Yen-Dollar Exchange Rate

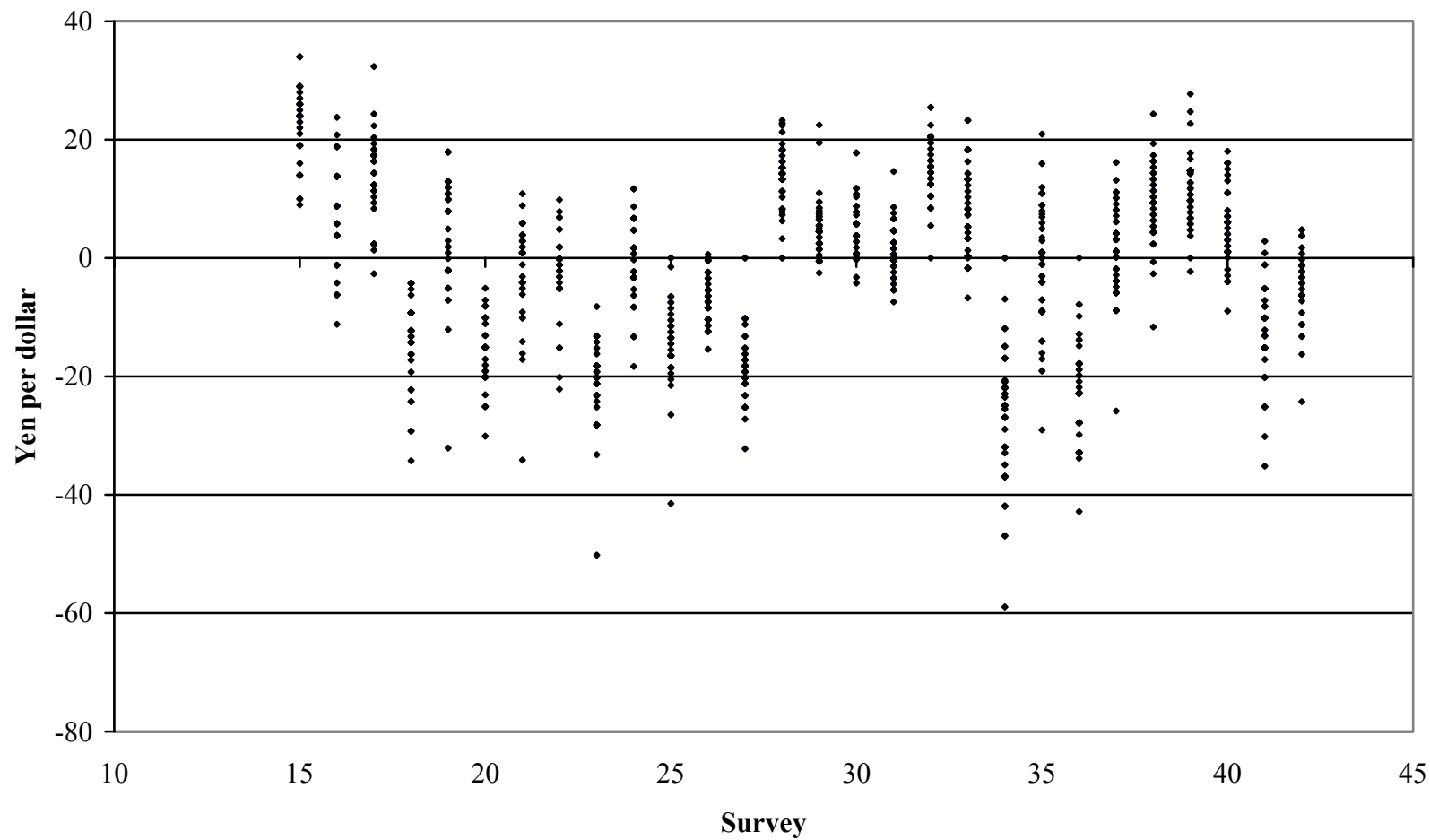


Table 1
Summary Statistics for Survey Forecasts

Survey Date	Treasury-Bill Rate			Treasury-Bond Rate			Yen-Dollar Rate		
	Mean S.D.	Range n	Actual	Mean S.D.	Range n	Actual	Mean S.D.	Range N	Actual
198201	11.06 2.05	8.8-16 12	12.76	13.05 1.13	11.5-16 12	13.91			
198207	11.61 .54	10.5-12.5 14	7.92	13.27 .35	12.5-13.75 14	10.43			
198301	7.37 .94	5.5-9.625 17	8.79	10.11 .71	9-11.625 17	11.01			
198307	8.60 .89	6-10 17	8.97	10.59 .60	9-11.75 17	11.87			
198401	8.72 .64	7-10 24	9.92	11.39 .68	9.5-12.5	13.64			
198407	10.62 .76	8.5-12 24	7.85	13.75 .85	11-14.75 24	11.54			
198501	8.56 .98	6.5-10.6 24	6.83	11.60 .80	10-13.25 24	10.47			
198507	7.31 .82	5.5-8.75 25	7.05	10.51 .83	8.5-11.8 25	9.27			
198601	6.96 .58	5.5-7.75 25	5.96	9.45 .63	8-10.5 25	7.24			
198607	6.02 .51	5-7 30	5.67	7.41 .51	6.5-8.25 30	7.49			
198701	4.98 .48	4.1- 6 35	5.73	7.05 .53	5.88-8 35	8.51			
198707	5.91 .50	4.25-6.63 35	5.68	8.45 .66	5.88-9.4 35	8.95			
198801	5.70 .58	4-6.6 36	6.56	8.65 .71	6.8-9.75 36	8.87			
198807	6.78 .39	5.8-7.6 32	8.1	9.36 .56	8-10.25 32	9			
198901	8.29 .60	7.25-9.5 38	7.99	9.25 .49	8.25-10.5 38	8.05	121.37 6.15	110-135 38	144
198907	7.76 .52	6.4-9.1 38	7.8	8.12 .48	7.4-10 38	7.98	136.53 8.47	120-135 38	143.8
199001	7.03 .48	5.5-8 40	8	7.62 .35	7-8.4 40	8.41	137.78 6.81	120-155 40	152.35
199007	7.56 .43	6-8.5 40	6.63	8.16 .40	7.25-9 40	8.26	149.78 7.14	140-170 40	135.75
199101	6.14 .42	4.9-7.03 40	5.71	7.65 .46	6-8.5 40	8.42	133.65 9.69	120-170 40	137.9
199107	5.84 .35	5-6.6 40	3.96	8.22 .38	7.3-9 40	7.41	140.78 5.61	130-155 40	124.9
199201	3.80 .34	2.75-4.5 42	3.65	7.30 .37	6-8 42	7.79	127.64 8.07	115-160 42	125.87
199207	3.54 .39	2.9-4.3 42	3.15	7.61 .38	6.45-8.3 42	7.4	127.33 7.07	115-147 42	124.85
199301	3.41 .32	2.7-4.45 44	3.1	7.44 .33	6.7-8.4 44	6.68	127.70 7.07	115-157 44	106.8

199307	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
199401	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
199407	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
199501	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
199507	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
199601	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
199607	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
199701	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
199707	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
199801	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
199807	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
199901	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
199907	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
200001	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
200007	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
200101	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
200107	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
200201	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
200207	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

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. Securitie	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
	Polycconomics	198607	199107		
	Hudson Institute	199201	200001		
Richard Rippe		199001	200207	0	
	Dean Witter	199001	199107		
	Prudential Securiti	199201	200207		
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		
	Straszheim 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 Cointegration Test of Unbiasedness		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	
	ADF(Δforecast)	ADF(error) Q(4)				$\Sigma (A-F)^2$ n	MSE Ratio to Random Walk (Modified DM statistic) ^c
David Berson	-3.149** -3.030**	-2.426** 4.260	-.351 (-2.369)**	.577 (.453)	.735 .765	17.488 26	.877 (-.754)
Paul Boltz	-2.720* -2.833*	-2.901*** .541	-.460 (-2.257)**	.517 (.694)	.348 .361	39.928 29	1.929 (1.810)*
Phillip Braverman	-3.768*** -3.931***	-4.680*** 1.696	.203 (1.027)	.483 (.368)	1.178 1.217	37.695 31	1.780 (1.225)
Dewey Daane	-2.289 -3.632**	-2.775*** 2.200	-.382 (-2.584)**	.517 (.694)	.348 .361	21.981 29	.984 (-.066)
Robert Dederick	-1.559 -2.984**	-2.758*** 2.752	-.084 (-.477)	.524 (1.000)	.029 .031	13.270 21	1.008 (.039)
Gail Fosler	-3.171** -4.061***	-3.313*** 6.633	-.514 (-2.776)**	.542 (.653)	.697 .728	25.241 24	1.402 (1.370)
Maury Harris	-1.571 -3.275**	-3.185*** 2.009	-.092 (-.639)	.545 (.728)	.308 .318	22.264 33	.958 (-.211)
Richard Hoey	-1.660 -2.334	-2.290** 3.560	-.425 (-1.765)*	.350 (.613)	.848 .892	25.598 20	1.674 (1.698)
Stuart G. Hoffman	-1.954 -3.870***	-3.245*** .842	-.164 (-1.043)	.621 (.264)	1.830 1.896	20.978 29	.966 (-.160)
William Hummer	-2.047 -2.516	-1.819* 2.019	-.380 (-2.190)**	.600 (.582)	1.250 1.316	14.282 20	1.038 (.220)
Edward Hyman	-1.784 -4.026***	-4.399*** 6.248	.289 (1.672)	.564 (.706)	.416 .427	47.690 39	1.515 1.076
Saul Hymans	-2.545 -3.900***	-2.828*** 8.681	-.196 (-1.210)	.455 (.733)	.203 .209	28.911 33	1.245 (2.010)*
David Jones	-1.701 -4.117***	-2.770*** 4.205	-.316 (-.882)	.391 (.400)	1.245 1.301	67.325 23	1.533 (1.052)
Irwin Kellner	-3.635** -4.854***	-4.828*** 1.172	-.102 (-.421)	.333 (.141)	3.274* 3.387*	51.619 30	1.190 (1.480)
Carol Leisenring	-1.669 -3.114**	-2.430** 3.773	.025 (.147)	.455 (1.000)	.188 .197	12.913 22	.982 (-.081)
Alan Lerner	-1.765 -5.333***	-3.887** 6.775	-.583 (-1.990)*	.652 (.221)	1.806 1.888	51.187 23	1.188 (.505)
Mickey Levy	-2.409 -4.476***	-3.810*** 3.691	-.152 (-.991)	.514 (1.000)	.000 .000	28.724 35	1.175 (.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 Debold-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		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(Δ forecast)	ADR(error) Q(4)				$\Sigma (A-F)^2$ n	MSE Ratio to Random Walk (Modified DM statistic)
David Berson	-1.424 -5.626***	-4.789*** 8.454	-.163 (-1.074)	.269 (.043)**	5.110** 5.310**	15.612 26	1.388 (2.963)***
Paul Boltz	-3.171** -3.529**	-2.857*** 2.837	-.455 (-2.216)**	.414 (.669)	.232 .240	40.280 29	1.664 (2.199)**
Phillip Braverman	-5.037*** -4.235***	-3.891*** 1.226	.269 (1.298)	.581 (1.000)	.057 .059	42.084 31	1.664 (1.377)
Dewey Daane	-2.382 -6.463***	-4.107*** 4.773	-.490 (-3.254)***	.310 (.164)	2.653 2.748	25.412 29	2.088 (2.431)**
Robert Dederick	-1.894 -4.943***	-4.993*** 4.133	-.046 (-.254)	.409 (.659)	.833 1.458	13.946 21	1.533 (2.216)**
Gail Fosler	-1.312 -4.553***	-2.392** 7.005	-.590 (-3.742)***	.500 (.615)	.825 .861	22.078 24	1.999 (2.187)**
Maury Harris	-1.191 -4.870***	-5.221*** 8.784	.095 (.713)	.545 (1.000)	.021 .021	19.213 33	1.426 (1.668)
Richard Hoey	-2.140 -2.535	-2.602** 11.496**	-.443 (-1.414)	.300 (.160)	3.039* 3.199*	41.128 20	2.135 (2.274)**
Stuart G. Hoffman	-1.695 -5.522***	-4.168*** 4.667	-.183 (-1.462)	.345 (.128)	3.131* 4.137**	13.755 29	1.304 (1.942)*
William Hummer	-1.631 -4.453***	-3.236*** 10.435*	-.387 (-2.434)**	.300 (.290)	1.832 1.928	12.605 20	1.300 (1.354)
Edward Hyman	-1.501 -5.486***	-4.109*** 7.866	.501 (2.743)***	.538 (1.000)	.030 .031	59.230 39	2.123 (1.801)*
Saul Hymans	-1.402 -5.948***	-5.403*** 12.111**	-.186 (-1.390)	.455 (1.000)	.122 .520	20.005 33	1.486 (2.073)*
David Jones	-2.074 -3.742**	-3.124*** 2.073	-.276 (-1.006)	.478 (1.000)	.048 .050	39.840 23	1.252 (.967)
Irwin Kellner	-2.579 -7.460***	-4.899*** 7.124	-.159 (-.767)	.433 (.272)	2.143 2.217	38.332 30	1.190 (.676)
Carol Leisenring	-1.522 -6.388***	-5.804*** 8.473	-.010 (-.067)	.591 (.655)	.282 .002	10.413 22	1.175 (.941)
Alan Lerner	-2.183 -4.813***	-3.882*** 4.164	-.523 (-1.921)*	.652 (.685)	1.806 .320	43.875 23	1.525 (2.129)**
Mickey Levy	-2.581 -7.662***	-6.895*** 5.468	-.088 (-.571)	.514 (1.000)	.008 .150	28.397 35	1.471 (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 for Table 3

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

Individual	Liu-Maddala 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(Δ forecast)				$\Sigma (A-F)^2$	MSE Ratio to Random Walk (Modified DM statistic)
	ADF(error)	Q(4)				n	
David Berson	-2.504 -3.589**	-2.721*** 1.681	-3.118 (-1.133)	.385 (.217)	2.275 2.366	5175.980 26	1.518 (2.452)**
Paul Boltz	-1.122 -2.735*	-2.120** 4.258	2.563 (.841)	.474 (1.000)	.003 .003	3301.963 19	1.397 (1.930)*
Phillip Braverman	-2.007 -3.097**	-2.847*** 1.481	-.204 (-.072)	.667 (.198)	2.291 2.405	3404.713 21	1.113 (.381)
Dewey Daane	-2.105 -3.535**	-3.209*** 3.265	2.873 (.996)	.393 (.441)	1.011 1.048	6518.140 28	1.729 (2.012)*
Robert Dederick	-.791 -2.042	-2.185** 3.752	1.146 (.320)	.563 (1.000)	.152 .163	3109.605 16	1.518 (1.921)*
Gail Fosler	-3.116** -3.357**	-2.699** 3.660	2.701 (.918)	.542 (.653)	.697 .728	4957.834 24	1.621 (1.828)*
Maury Harris	-1.917 -3.212**	-2.695** 3.536	-2.724 (-1.078)	.571 (.698)	.324 .336	5034.540 28	1.336 (1.642)
Richard Hoey	-1.370 -2.073	-1.984** 3.865	4.253 (.786)	.500 (1.000)	.000 .000	2685.864 10	2.170 (2.201)**
Stuart G. Hoffman	-1.874 -2.827*	-2.980*** 3.403	-1.251 (-.474)	.444 (.448)	.759 .788	4941.500 27	1.374 (2.028)*
William Hummer	-1.755 -2.847*	-2.432** 2.423	.240 (.080)	.550 (1.000)	.135 .142	3451.686 20	1.197 (1.400)
Edward Hyman	-2.179 -3.404**	-2.260** 2.403	-5.529 (-2.225)**	.543 (.569)	.675 .701	5159.600 27	1.513 (2.025)*
Saul Hymans	-1.982 -2.312	-2.291** 3.291	1.873 (.789)	.458 (1.000)	.084 .088	3194.330 25	1.055 (.593)
David Jones	-.792 -1.962	-1.722* 2.238	.136 (.028)	.444 (1.000)	.225 .253	1648.664 9	1.364 (2.071)*
Irwin Kellner	-1.135 -3.155**	-2.831*** 3.259	3.762 (1.191)	.647 (.294)	2.082 2.212	2955.657 17	1.442 (1.056)
Carol Leisenring	-1.138 -1.606	-1.947* 4.245	-.385 (-.134)	.526 (1.000)	.003 .003	2809.424 19	1.190 (.904)
Alan Lerner	-1.537 -2.670*	-.814 2.892	-7.008 (-1.372)	.500 (1.000)	.476 .529	2839.654 10	2.301 (2.358)**
Mickey Levy	-1.842 -3.257**	-2.598** 4.886	-3.438 (-1.435)	.607 (.560)	.778 .867	4672.100 28	1.239 (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 for Table 3

Table 6
 Tests of Heterogeneity of Forecasts Across Survey Respondents
 Dependent variable: Deviation of forecast from mean forecast

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 rate	T-Bond rate	Yen/\$ rate	T-Bills rate	T-Bonds rate	Yen/\$ rate
F Tests for individual effects ¹	4.09***	8.63***	6.76***	5.96***	15.38***	12.23***
Tests for employer effects						
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.

Table 7
 OLS Estimates of Incentives Model, Equation [5]
 Dependent variable is the absolute value of the deviation of each forecast
 from the mean forecast from that survey

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