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Is Honesty Always the Best Policy ?

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Honesty may not be the best policy for professional forecasters if the pattern of forecasts reveals valuable information about the forecasters even before the outcome is realised. Rational forecasters will compromise between minimizing errors and mimicking prediction patterns typical of able forecasters. Simple models based on this argument imply that forecasts are biased in the direction of forecasts typical of able forecasters. Our models of strategic bias are rejected empirically as forecasts are biased in directions typical of forecasters with large mean squared forecast errors. This observation is consistent with behavioral explanations of forecast bias.

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I. Introduction

Can the apparent rejection of the Rational Expectations Hypothesis in survey forecast data be explained by Rational Cheating? The considerable number of studies that rejected the testable implications of the Rational Expectations Hypothesis using survey data [e.g. Pesando 1975; Carlson 1977; Pearce 1979, Figlewski and Wachtel 1981; Ito 1990] rely on the joint hypothesis that survey participants aim at minimizing squared forecast errors. However, as Scharfstein and Stein [1990] have argued, fully rational agents might choose to announce forecasts different from the conditional expected value of the variable to be predicted.

This paper introduces and tests the implications of advising games where advisors make many different forecasts of the same realization of the same variable. In this setting it can be possible to estimate an advisor's ability before the realization of the variable. Advisors who are concerned about their reputation when disclosing repeated forecasts to their clients will have other aims in addition to minimizing forecast errors. For example, they might not want to deviate too much from the previous announcements, or they might want to suggest that their new forecast contains significant new information. In which way advisors balance their joint objective to minimize forecasts errors and to look good before the outcome is observed depends on the specific model and the evaluation rule clients optimally employ in order to determine the ability of their advisors. For any specification, the models predict a rational bias in the forecast error.

Additionally, the cross-sectional implications of the models make it possible for us to distinguish between rational cheating as implied by the advising games and behavioral models of predictable forecast errors as advanced by the growing school of thought that incorporates psychological elements into decision making [e.g. Kahneman et. al 1982]. In our models advisors announce biased forecasts because it makes them look good. For example, if repeated forecasts of the same outcome are biased away from previous forecasts, it must be because the clients' perception is that advisors who change there forecasts by a large amount make good forecasts. Behavioral models do not have such implications.

The empirical results in this paper reject all of our advising games. For model specifications that would explain the observed forecast error bias, the cross-sectional implications are rejected, and for model specifications that have valid cross-sectional implications, the bias in the forecast error is unexplained. Thus, the data reject our general approach to rationalizing biased forecasts and not narrow assumptions of our models.

The paper proceeds as follows: Section two formalizes an example of the advising game and discusses testable implications. Section three presents empirical tests using a small panel data set of published predictions of short-term interest rates in the U.S.. Section four shows how different models of strategic bias are rejected by one of our two tests. Section five concludes.

II. Models of Advice

Rational agents may choose to report public forecasts different from their subjective mean predictions if honesty is not always the best policy. We assume that professional forecasters choose forecasts in order to convince clients that forecast errors are small. Clearly, this provides an incentive to report forecasts close to the forecaster's belief about the expected value of the variable forecast. However, it also may create incentives to provide a pattern of forecasts which imply small expected forecast errors before the outcome is observed. This means that Nash equilibrium forecasts could be biased . Rational clients, in turn, suspecting this, adjust advisors' forecasts for their own use. In Nash equilibrium, clients do not make systematic forecast errors. The efforts of professional forecasters to convince their clients that they have precise information might reduce the efficiency of communication, but it does not cause systematic confusion. If the actual signal is interpreted as part of the outcome, then as in the signalling literature, many equilibria are possible. In this model, a change of a stated forecast "means" that the forecasters' beliefs about the conditional mean of the forecast variable have changed by a greater amount.

To formalize this idea, consider the following general set-up. Let there be two agents in the following simple model of advice – an advisor and a client . The forecaster provides the client with two predictions of the value of a random variable y. The client uses these stated predictions to form his own forecast of the value of the variable. The client also attempts to determine the quality of the forecaster's information analyzing the stated forecasts and the realized value of the predicted variable. If the client concludes that the forecaster has poor information, he terminates the relationship and looks for a new forecaster. If the client is not convinced that the forecaster has poor information, the game is repeated. The forecaster attempts to convince the client that he has high quality information. For simplicity, assume a forecaster who has no other aim.

In our games, it is assumed that the i-th forecaster receives signal s_{1i} in period 1, makes a first forecast f_{1i} , then receives signal s_{2i} in period 2 and makes another forecast f_{2i}. Finally the outcome y is realized.

First, we consider a simple model of advice where we describe a Nash equilibrium in which forecasters play pure strategies and in which more able forecasters are more willing than less able forecasters to admit that they were wrong. In this first example of our game, it is assumed that the i-th forecaster receives signals:

$$(1) \qquad \begin{aligned} s_{1i} &= y + \sigma_i(\epsilon_i + \eta_i) \\ s_{2i} &= y + \sigma_i(\epsilon_i) \end{aligned}$$

The expected value of y conditional on s_{ti} is equal to s_{ti}, σ_i is a parameter which describes the quality of the signal and:

$$\epsilon_i \sim N(0,1)$$

$$\eta_i \sim \begin{cases} 1 & \text{with probability } \frac{1}{2} \\ -1 & \text{with probability } \frac{1}{2} \end{cases}$$

For still more simplicity assume that there are only two types of forecasters – some with $\sigma_i = 1$ and the rest with $\sigma_i = \sigma > 1$. To simplify notation, we suppress subscript i in this section.

In the second period, the optimal forecast of y is s_{2i}. There is no reason why the able forecaster would not frankly state his new prediction. The less able forecasters, however, will not state this prediction. If they did, the absolute value of their change in predictions, $(f_{2i} - f_{1i}) = (s_{2i} - s_{1i}) = -\sigma_i\eta_i$ would be equal to $\sigma > 1$. The client would know that the forecaster received poor signals, since an able forecaster would never change a prediction by more than 1 in either direction. The less able forecasters rationally choose to adjust their predictions up 1 if $s_{2i} > s_{1i}$ and down 1 if $s_{2i} < s_{1i}$. Observing only the stated predictions f_{1i} and f_{2i}, the client has no way of distinguishing between able and less able forecasters. When y is revealed, the client has some information on the quality of advice, but not enough to catch incompetence with certainty. This makes the dishonest strategy optimal [Ehrbeck and Waldmann 1994, Waldmann 1995].

We have considered only the Nash equilibrium in which rational forecasters are frank, arguing that this is focal. Since we consider a game of costless communication, there are a huge number of Nash equilibria (consider English and German for example). In fact, we can generate a new Nash equilibrium by applying any one-to-one R^2 - to - R^2 function to (f_{1i}, f_{2i}) as given in e.g. the Nash equilibrium described above. In Nash equilibrium, the client will know this function and will be able to invert it. This causes no change in the information transmitted or payoffs for either player. This makes it unlikely that any refinement of Nash equilibrium will eliminate this multiplicity (which occurs in all models considered in this paper). The predictions of our model and our empirical results might be invalidated because we do not know which of this huge number of Nash equilibria is being played. The agents in our game might be speaking a different language than we imagine, for example, by describing interest rates in basis points instead of in percent. Pre-play communication, such as the specific definitions of terms provided by the publisher of the data used in our empirical section, does not eliminate these equilibria. However, we are not concerned by such multiplicities, because they seem to require the almost telepathic properties of Nash equilibrium. The relationship between professional forecasters and clients is impersonal and often temporary. We see no reason why we alone should be ignorant of the Nash equilibrium being played or why our ignorance would be dispelled if we ceased to theorize and became clients of professional forecasters ourselves.

We also consider to be unreasonable Nash equilibria in which sophisticated forecasters announce forecasts such that clients could not deduce their signals from their forecasts. For example, if all forecasters gave arbitrary constant or random forecasts unrelated to their signal, agents would ignore the forecasts when attempting to learn about the ability of forecasters and would have no reason to attempt to learn anyway. Given our assumptions this would give a large class of Nash equilibria. Clearly the example mentioned above is unreasonable as clients would have no reason to pay for arbitrary forecasts in the first place. It may be that there are more plausible Nash equilibria of this type, but it seems unlikely that they would survive refinement, as able forecasters act in a way which is counter to their clients' interests and which makes it more difficult for their clients to assure themselves of the able forecaster's ability.

This leaves the extremely strong and simple assumptions about the distribution of η_i as a weak point of our analysis.

Below, we relax the assumptions about the distribution of η_i. To keep the notation simple we assume that η_i and ϵ_i have the same distribution. It is possible to find Nash equilibria in which forecasters play pure strategies for a broad class of assumptions about this distribution. For other assumptions, no such equilibrium exists.

Here we assume that η_i and ϵ_i are distributed with a density function described as

$$\eta_i \sim \frac{1}{\sigma_i} h(\frac{\eta_i}{\sigma_i}) \qquad (2)$$
$$\epsilon_i \sim \frac{1}{\sigma_i} h(\frac{\epsilon_i}{\sigma_i}).$$

where σ_i is as before 1 or $\sigma > 1$. We make fairly strong assumptions about h both to ensure tractability and to guarantee the existence of a Nash equilibrium in which forecasters play pure strategies. We assume that h has bounded support and so without loss of generality assume that

$$h(x) = 0 \quad if \quad |x| > 1. \qquad (3)$$

We assume that h is symmetric, continuous, and strictly concave and that for all x

$$\frac{|h'(x)|}{\sigma} < (h(1))^2. \qquad (4)$$

To describe clients aims more precisely, we assume that if the posterior odds ratio that the forecaster is able is less than p_{min}, then the client terminates the relationship and looks for a new forecaster, and that if the posterior odds ratio is exactly p_{min}, the client is indifferent between keeping the current forecaster and looking for a new one. Clients are assumed to observe only the forecasts which they purchase and outcomes, so they choose a new forecaster at random. Given the behavior of clients there are, in principal, three sorts of second period forecasts. Those which imply a posterior odds ratio less than p_{min} and loss of a client which will not occur in Nash equilibrium, those which imply a posterior odds ratio of more than p_{min} which will occur, and those which imply a posterior odds ratio of exactly p_{min} which will occur with positive probability. In Nash equilibrium a broad range of forecasts imply a posterior odds ratio of exactly p_{min} which makes it possible for us to consider clients' mixed strategies in which the probability of terminating the relationship is a freely chosen function of the change in forecast. This gives us a continuum of degrees of freedom and makes it possible to find a Nash equilibrium.

The forecasters are assumed to have infinite time preference and so to care only about whether the client terminates the relationship before paying for the next forecast. As above we only consider Nash equilibria in which able forecasters are frank. First the optimal strategy of the less able forecaster (i) is of the form given by,

(5)
$$\begin{aligned} f_{1i} &= s_{1i} \\ f_{2i} &= f_{1i} + g(s_{2i} - s_{1i}), \end{aligned}$$

for some function g. This is clearly true because of the definitions of s_{1i} and s_{2i} and the symmetry of the distributions of η and ϵ.

As noted above the analysis of the less able forecasters' strategies depends on the resulting clients' posterior odds ratio. If the change in signal ($s_{2i} - s_{1i} = -\eta_i$) is small, the forecaster can be honest about this change without worrying about losing the client. In this case, the forecaster's only concern is that a second period forecast error greater than 1 in absolute value will imply loss of the client. Therefore, the forecaster announces $f_{2i} = s_{2i}$, the forecast which minimizes this risk. For larger η_i, the forecaster will announce a forecast such that the posterior odds ratio is exactly p_{min}. Nash equilibrium g must be such that this occurs for a variety of values of η_i and resulting values of $f_{2i} - f_{1i} = g(s_{2i} - s_{1i})$. This leaves us free to choose the probability that the client terminates the relationship as a function of $f_{2i} - f_{1i}$ in order to make the function g optimal for the forecaster. Finally as is shown in Waldmann [1995] inequality (4) implies that it is indeed optimal for able advisor (j) to be frank and announce $f_{2j} = s_{2j}$ given the clients' strategy. This means that we have found a Nash equilibrium of the game.

The reasoning above implies that g is monotonically increasing and differentiable and that its derivative is less than or equal to 1. For η close to zero the less able forecasters are frank. For larger η the less able forecasters are rationally stubborn as is formally proven in Waldmann [1995]. Together these observations imply that the expected value of the regression coefficient of $(f_2 - y)$ on $(f_2 - f_1)$ is negative. This is our first testable prediction.

Second, the variance of $(f_{2i} - f_{1i})$ is greater for less able forecasters than for able forecasters. This follows from the fact that less able forecasters balance their desire to make small changes in forecasts like able forecasters and their desire to make small forecast errors like more able forecasters. Clearly expected squared forecast errors are greater for less able forecasters. They would be greater even if less able forecasters minimized mean squared forecast errors, and less able forecasters do not minimize mean squared forecast errors in Nash equilibrium. Therefore, across forecasters, mean squared changes in forecasts are positively correlated with mean squared forecast errors. This is our cross sectional prediction.

Thus the model gives two predictions – each of the less able forecasters changes his forecast too little to minimize expected squared errors, yet when different forecasters are compared those with larger expected squared changes in forecasts still have larger expected squared forecast errors. The reason for these two predictions is very simple. Less able forecasters balance their desire to have small changes in forecasts like able forecasters and their desire to have small forecast errors like able forecasters.

It is easy to modify advising games to eliminate the prediction that forecast errors are negatively correlated with the change in forecasts. It is extremely difficult, however, to eliminate this prediction without reversing the prediction that large mean squared changes in forecast are correlated with large mean squared forecast errors. In the remainder, this section presents two modified models which imply different predictions about the rational bias in forecasts. Each implies that forecasts are biased in the direction which creates a pattern typical of able forecasters, which, in turn, implies a cross sectional prediction.

The second specification in this section implies over-adjustment of the publicly announced forecasts from first- to second period signals. As before, the forecaster receives two signals. However, in this model, the quality of the first signal is identical for all forecasters, and the quality of the second signal varies across forecasters. This can be viewed as better interpretation of previous information in later periods. The client in this model attempts to learn about the improvement in the quality of the forecaster's signals. For example assume that ϵ and η are independent, that the sum of ϵ and η has the same distribution for all forecasters and η has a higher variance for more able forecasters. In Nash equilibrium forecasters will not make a second forecast equal to the expected value of y given their signals. Consider what clients would do if forecasters were frank. In contrast to the previous model a large difference between the new and the old announcement is a good sign, since more able forecasters have a larger variance of η. Therefore if clients believe forecasters are frank, forecasters will be rationally jumpy and will change their forecasts too much. This implies an expected positive coefficient in the regression of $(f_2 - y)$ on $(f_2 - f_1)$. This occurs because a large change in the forecast is a good sign – a sign of high quality second period information. For this to be true, it must also be true that forecasters who change their forecast by a large amount have small second period forecast errors.

For our third specification we relax the strong assumption that clients observe only the forecasts of the forecaster whom they employ. The model of rational boasting presented below would imply that forecasters choose to under-utilise the information contained in the average of past predictions. The motive is again that each forecaster tries to make clients believe in superior forecaster's information than is warranted.

If forecasters know the average of first period forecasts ($\bar{f}_1$), then for normally distributed disturbances the expected squared error minimizing forecast will be a weighted average of the current signal, the first period signal and a weighted average of first period forecasts [Ehrbeck 1993]. For a simple example, the average of first period forecasts ($\bar{f}_1$) might be the optimal combination of all publicly available first period data. In this special case, the expected squared error minimizing forecast (f^*_{2i}) is a weighted average of the average of first period forecasts and the second period signal. The weight on the second signal increases in the quality of the second period signal. In a simple example in which both $(y - s_{2i})$ and $(y - \bar{f}_1)$ are normally distributed, lower variance of $(y - s_{2i})$ implies higher variance of $(f^*_{2i} - \bar{f}_1)$. This suggests that it may be rational for forecasters to overstate the difference between their second period estimate of y and $\bar{f}_1$. That is, forecasters may rationally put a higher weight on s_{2i} than would minimize expected squared forecast errors. This version of rational cheating has two implications : first that $(f_{2i} - y)$ is positively correlated with $(f_{2i} - \bar{f}_1)$ and second that high mean squared $(f_{2i} - \bar{f}_1)$ is correlated with low mean squared forecast errors.

The logic of rational cheating is always the same for each of the models we consider – less able forecasters balance their desire to have small forecast errors with their desire to have a pattern of repeated forecasts typical of able forecasters. This implies a fairly strong prediction. In each case we predict that forecasts are biased in a direction which creates a pattern typical of able forecasters. This general prediction makes our models of rational cheating distinguishable from behavioral models of predictable forecast errors [Andreasson 1987; 1990; Andreasson and Kraus 1990; Case and Shiller 1988; De Bondt and Thaler 1990; De Long et al. 1990; Einhorn and Hogarth 1978; Frankel and Froot 1988; Grether 1980; Kahneman et al. 1982]. If the bias in forecasts were due to less than full rationality, one can easily obtain the opposite prediction. If forecasters have a behavioral bias and some have a larger bias than others, one would expect (other things equal) that the forecasts with the larger bias would have larger mean squared forecast errors. This is the opposite of the pattern we predict and enables us to test all of our models, and indeed our general approach, against the alternative behavioral models.

III. Testing Rational Stubbornness

To test the first implication of our first two advising games, data is necessary in which identified forecasters predict several times the value of some economic variable for the same target period. The North Holland *Economic Forecast* data is one such source. In this monthly newsletter, forecasts from a panel of experts of key economic variables for industrialized countries are published. The prediction variable used for this work is the forecast of the annualized discount rate on new issues of 91-day US-Treasury Bills, based on weekly auction average rates. This variable has been chosen because the panel for the U.S. is the richest and because interest rate forecasts predict a quoted price which reduces definitional ambiguities that arise when predicting, e.g. national income data. Each month the panel of experts submits predictions of the average interest rate for the quarters of the calendar year. The forecast data have consequently been split in three, small homogeneous panels of first month, second month, and third month forecasts respectively. For the empirical test, only forecasts of those panel participants who reported at least 15 times over the sample period from January 1985 to June 1990 have been included. The cross-section dimension of the data is $N = 23$. The times-series dimension is $T = 22$. The average number of non-missing observations per participant is between 18 and 19 for each forecast horizon. The realization data needed for the error calculations come from the *Federal Reserve Bulletin.* Quarterly averages of discount rates are calculated as the simple average of the monthly data which are quoted on an annualized discount basis. For the regression analysis, the data have been stacked across agents per period and along time:

$$\mathbf{Y} = \mathbf{X}\boldsymbol{\beta} + \mathbf{u} \tag{6}$$

where $\mathbf{Y}$ is the $TN \times 1$ -stack of second period forecast errors, $\mathbf{X}$ is the $TN \times 2$ -stack of constant terms and changes in predictions from the first period to the second period, $\boldsymbol{\beta}$ is the 2×1 vector of regression coefficients, and $\mathbf{u}$ is the $TN \times 1$ -stack of disturbance terms. Keane and Runkle [1990] have noted that this regression can be used to test the joint hypothesis of rational expectations and a quadratic loss function.

When running such regressions, care needs to be taken for possible correlation between forecast errors because forecasters are likely to be surprised by the same aggregate shocks as Chamberlain [1984] and Keane and Runkle [1990] have emphasized. Ignoring this potential correlation would lead to incorrectly low standard errors and thus spurious rejection of the null hypothesis.
More precisely, for our first set of estimated standard errors, we assume:

$$E(u_{i,t}^2) = \sigma_i^2 \qquad \text{for all t = 1,...,T ; i = 1,...,N} \tag{7}$$

$$E(u_{i,t}u_{j,s}) = \begin{cases} \rho\sigma_i\sigma_j & \text{for all t = s and i} \neq \text{j} \\ 0 & \text{otherwise} \end{cases} \tag{8}$$

This specification allows for heteroskedasticity of the disturbances across units and for non-zero contemporaneous correlation between the disturbances in different units, but excludes (time) serial correlation. The common correlation coefficient ρ reflects the assumption of an aggregate surprise.

The resulting $TN \times TN$-matrix of disturbances is block- diagonal:

$$\Omega = \begin{pmatrix} \Psi & 0 & \cdots & \cdots & 0 \\ 0 & \Psi & 0 & \cdots & \vdots \\ \vdots & 0 & \ddots & \ddots & \vdots \\ \vdots & \cdots & \ddots & \ddots & 0 \\ 0 & \cdots & \cdots & 0 & \Psi \end{pmatrix} \tag{9}$$

where

$$\Psi = \begin{pmatrix} \sigma_1^2 & \rho\sigma_1\sigma_2 & \cdots & \rho\sigma_1\sigma_N \\ \rho\sigma_2\sigma_1 & \sigma_2^2 & \cdots & \vdots \\ \vdots & \ddots & \ddots & \vdots \\ \rho\sigma_n\sigma_1 & \cdots & \cdots & \sigma_N^2 \end{pmatrix}$$

Individual OLS regressions can be run for each forecaster separately to obtain residual series $\mathbf{e}_i$. These residuals series are used to estimate the elements of Ω as follows:

$$\hat{\sigma}_i = \sqrt{\frac{\mathbf{e'}_i\mathbf{e}_i}{(T_i - 2)}} \tag{10}$$

and

$$\hat{\rho} = \frac{\sum_{i \neq j} \hat{\rho_{ij}}(T_{ij} - 1)}{\sum_{i \neq j}(T_{ij} - 1)}, \tag{11}$$

where T_i is the number of periods with observations for forecaster i, T_{ij} is the number of periods with observations for both forecasters i and j, and $\hat{\rho_{ij}}$ is the sample correlation coefficient for any pair of forecasters (i, j).

Using $\hat{\Omega}$, we can run the regression of the stacked data in equation (6) to obtain an estimate of $\boldsymbol{\beta}$ and correct the estimated standard errors of the coefficient estimates as

$$(\mathbf{X'X})^{-1}\mathbf{X'}\hat{\Omega}\mathbf{X}(\mathbf{X'X})^{-1} \tag{12}$$

This approach gives efficient estimates of $Vcov_{\hat{\beta}}$ under the strong assumptions described above. Below, we calculate five different estimates of $Vcov_{\hat{\beta}}$ based on weaker assumptions. We report this variety of estimates because we aim to calculate both t-like statistics biased towards zero and t-like statistics biased away from zero.

In each case, the variance covariance matrix of $\boldsymbol{\beta}$ is estimated as follows.

$$(13) \qquad V cov_{\hat{\beta}} = (\mathbf{X}'\mathbf{X})^{-1} \left(\sum_t \left[(\sum_i \mathbf{X}_{it}\hat{u}_{it})'(\sum_i \mathbf{X}_{it}\hat{u}_{it}) \right] \right) (\mathbf{X}'\mathbf{X})^{-1}$$

where $\mathbf{X}_{it}$ is the observation for forecaster i in period t , (1, change in forecast) if the change in forecast is available and (0,0) if no change in forecast is available; $\hat{u}$ is an estimate of the disturbances which we hope gives reasonable estimates of variances and covariances. Each of the five sets of t-like statistics calculated with equation (13) are based on different estimates of $\hat{u}$. For our second set of estimated t-like statistics we relax the assumption of time invariant individual specific heteroskedasticity and constant equal correlation of forecast errors across individuals. These t-like statistics are calculated with equation (13) for estimated disturbances calculated according to equation (14) using residuals from individual regressions, $\mathbf{e}_i$, and correcting for the number of degrees of freedom lost in estimation.

$$(14) \qquad \hat{u}_{it} = e_{it}\sqrt{\frac{T_i}{T_i - 2}}$$

This might appear to imply a large gain in robustness. However, the resulting standard errors are biased down in small samples such as ours (recall the variance-covariance matrix is calculated with residuals from separate regressions for each individual). The resulting t-like statistics are reported in parentheses in the second row of reported t-like statistics in each table. For our third estimated variance covariance matrix of $\boldsymbol{\beta}$ we use raw forecast errors. Under the null, the disturbances to our regression equation are equal to the forecast errors. If $\hat{u}_{it}$ in equation (13) is estimated with the forecast error of forecaster i in period t, the resulting estimate of the variance covariance matrix of beta is unbiased under the null of rational expectations and a quadratic loss function. If, on the other hand, forecast errors are predictable, the resulting estimate will be biased up by a positive definite matrix. If forecast errors are independent across time, the resulting test has almost no power against the alternative that they are correlated with the change in forecast. The resulting t-like statistics are reported in parentheses in the third row of reported t-like statistics in each table. They provide extremely robust tests with extremely low power. The fourth set of t-like statistics is calculated using equation (13) with residuals obtained from individual regressions excluding the time period in question. In other words, for each forecaster i and for each period t we estimate an individual regression excluding data from period t and use the resulting parameter estimates to calculate a residual for period t. The resulting estimated variance covariance matrix is biased up by a positive definite matrix. Thus, if the fourth and second set of estimated standard errors are similar, one can be fairly confident that neither bias is large. These t-like statistics are reported in parentheses as the fourth set of t-like statistics in each table. Finally, the fifth set of t-like statistics is calculated with equation (13) using residuals from pooled regressions excluding the time period in question. In other words, for each period t we estimate a pooled regression excluding data from period t and use the resulting parameter estimates to calculate residuals for period t. Again, the resulting estimated variance covariance matrix is biased up by a positive definite matrix. These t-like statistics are reported in parentheses as the fifth set of t-like statistics in each table.

The results of the three regressions using the changes in forecasts from the first month to the third month, the first month to the second, and the second to the third as regressors are summarized in Table 1. Recall the prediction from our first model that forecast errors are negatively correlated with changes in forecasts. In all three regressions, the estimated coefficients have the wrong sign. All of the t-like statistics for the regression coefficients of the forecast error on the change from the first to the third month and on the change from the second to the third month are significant. For the regression coefficient on the change in forecast from the first to the second period, t- like statistics calculated with residuals from individual regressions including the time period in question are significant. Forecasters in this particular sample do not choose to place too much weight on old forecasts as predicted in Waldmann [1991],[1995] and our model presented in section 2 above. To the contrary, the forecasters in this panel put too much weight on their new information or, at least, change their forecast too much. Correcting for that bias would improve their forecast. In particular, the improvement in the forecasts made in the third month of the quarter is unambiguously statistically significant. Whether one considers improvements to the forecasts made in the second month of the quarter to be significant depends on whether one is willing to assume that the variance of forecast errors is constant and that the correlation of forecast errors across different pairs of forecasters is identical. It is clear that both the Rational Expectations Hypothesis with a quadratic loss function and the model proposed above are strongly rejected by our data assuming that our interpretation of the variable to be forecasted is accepted by survey participants and their clients.

The results reported above are valid only under the assumption that our interpretation of the phrase "**Treasury bill rate, three months, percent:** Discount rate on new issues of 91-day Treasury bills. Based on the weekly auction average rates" in *Economic Forecasts* is unambiguously correct. Recall that forecasts of the yield of 91-day Treasury bills was chosen to minimize the ambiguity in variable definitions. Unfortunately, the definition of the quarterly average has an element of ambiguity. It might mean the average of three values of the monthly series published in the *Federal Reserve Bulletin* as we assume. Alternatively, it could be the average over the 13 weekly auctions which take place in the quarter. Fortunately, it is possible to use the data in *Economic Forecasts* to see whether the survey participants agree about the definition of the variable which they are forecasting. *Economic Forecasts* uses a standard format for surveys of different variables and in each case asks for "forecasts" of the variables for the previous quarter. In the case of the yield on 91-day Treasury bill this seems an easy task since all the relevant data on realizations is publicly available. However, even in the third month of the following quarter the back-casts are not all identical to our interpretation of the outcome nor are they all the same. Assuming that this reflects sincere disagreement about the definition of the variable to be forecasted (and not a final attempt to convince extremely naive clients that the forecaster has not made a mistake) this introduces ambiguity in our notion of forecast errors. We can use the back- casts to test the hypothesis that forecasters announce optimal forecasts of their own, later-period back- casts. To avoid loosing too many data points we use the average of all available back-casts for each forecaster as our back-cast variable. Nonetheless, some forecasters announce no back casts at all for some quarters so the number of observations are somewhat reduced. As reported in Table 2, the results with back-casts are almost identical to the results with outcomes. Forecasters do not minimize the expected squared difference between their forecasts and their own back-casts. They also do not report biased predictions of the sort predicted by our model. Instead of putting too much weight on their previous forecasts they change their forecasts too much. The results with back-casts reported in Table 2 are strikingly similar to the results with outcomes reported in Table 1. This strongly suggests that the rejection of rational expectations with a quadratic loss function and the rejection of our model are not due to ambiguity in definition of the variable to be forecasted.

To test the second (cross sectional) prediction of our model we compare the magnitude of changes in forecasts to the magnitude of forecast errors. To do this we first calculate for each forecaster the mean squared change in forecast from e.g. the second to the third month. Then we calculate the mean squared forecast error for each forecaster. Then we regress the mean squared forecast error on the mean squared change. This is a regression across the 23 forecasters. Clearly the disturbances in this regression are not normally distributed so we also report the rank correlation coefficient of the mean squared change in forecast and the mean squared forecast error. Recall that the model of rational stubbornness implies that small changes in forecasts are correlated with small forecast errors, but that Tables 1 and 2 already demonstrate that it is false. On the other hand the results reported in Tables 1 and 2 can only be rationalized if there is an incentive to change forecasts by a large amount. This means that our basic approach to explaining biased forecasts by rational cheating requires large changes in forecasts to be correlated with small forecast errors.

In fact mean squared changes in forecasts are strongly positively correlated with mean squared forecast errors as is shown in Table 3.

We can rationalize the results reported in Tables 1 and 2 as it is easy to find models which give the opposite of the prediction of rational stubbornness, but we have not been able to obtain a fully rational model in which forecasters change forecasts too much and in which the forecasters who make large changes in forecasts have larger mean squared forecast errors. Such forecasters damage their reputation twice, by making poor forecasts and by producing a pattern of forecasts typical of less able forecasters. It appears extremely difficult to reconcile such conduct with full rationality.

IV. Rational Boasting

The results presented above can be interpreted as strengthening the rejection of the Rational Expectations Hypothesis. The failure to reconcile systematic forecast errors with an optimizing model supports the view the rejection is meaningful and does not reflect a misinterpretation of the optimization problem which rational forecasters attempt to solve. Alternatively, the empirical results can be used to try to specify different advising games which rationalize the observed behavior. It is easy to modify the advising game to eliminate the false prediction that forecast errors are negatively correlated with the change in forecasts. It is extremely difficult, however, to eliminate this prediction without reversing the correct prediction that large mean squared changes in forecast are correlated with large mean squared forecast errors. In the remainder, this article presents two modified models which avoid one but not both false predictions. The fact that we have not been able to find a model of rational cheating which is not rejected by the data strengthens the rejection of our approach to rationalizing biased forecasts and so strengthens the evidence against the rational expectations hypothesis.

The first specification in this section implies over-adjustment of the publicly announced forecasts from first- to second period signals. As before, in this modified model the forecaster receives two signals. However, in this model, the quality of the first signal is identical for all forecasters, and the quality of the second signal varies across forecasters. This can be viewed as better interpretation of previous information in later periods. The client in this model attempts to learn about the improvement in the quality of the forecaster's signals. For example assume that ϵ and η are independent, that the sum of ϵ and η has the same distribution for all forecasters and η has a higher variance for more able forecasters. In Nash equilibrium forecasters will not make a second forecast equal to the expected value of y given their signals. Consider what clients would do if forecasters were frank. In contrast to the previous model able forecasters a large difference between the new and the old announcement is a good sign, since more able forecasters have a larger variance of η. Therefore if clients believe forecasters are frank, forecasters will be rationally jumpy and will change their forecasts too much (proof available on request). This implies an expected sign of the coefficient in the regression of $(f_2 - y)$ on $(f_2 - f_1)$ is compatible with the empirical findings summarized in Tables 1 and 2. However, this occurs because a large change in the forecast is a good sign – a sign of high quality second period information. For this to be true, it must be true that forecasters who change their forecast by a large amount have small second period forecast errors. This is contradicted by the results reported in Table 3.

We now consider relaxing the strong assumption that clients observe only the forecasts of the forecaster whom they employ. The model of rational boasting presented below would imply that forecasters snub the information contained in the average of past predictions. The motive is again that each forecaster tries to make clients believe in superior forecaster's information than is warranted. This game is different from the games considered above because we assume that clients know the average of month old forecasts. In the earlier models we assumed that clients never observe the forecasts of forecasters other than the one they employ and that forecasters never learn each others' forecasts. In fact, the existence of our data set shows that old forecasts are publicly available.

If forecasters know the average of one month old forecasts, then for normally distributed disturbances the expected squared error minimizing forecast will be a weighted average of the current signal, the month old signal and the average of month old forecasts. For a simple example the average of month old forecasts ($\bar{f}_1$) might be the optimal combination of all publicly available month old data. In this special case, the expected squared error minimizing forecast (f_2^*) is a weighted average of the average of month old forecasts and the second period signal. The weight on the second signal increases in the quality of the second period signal. For a simple example in which both $(y - s_{i2})$ and $(y - \bar{f}_1)$ are normally distributed, lower variance of $(y - s_{i2})$ implies higher variance of $(f_2^* - \bar{f}_1)$. This suggests that it may be rational for forecasters to overstate the difference between their second period estimate of y and $\bar{f}_1$. That is, forecasters may rationally put a higher weight on s_{i2} than would minimize expected squared forecast errors.

This version of rational cheating has two implications : first that $(y - f_{i2})$ is positively correlated with $(f_{i2} - \bar{f}_1)$ and second that high mean squared $(f_{i2} - \bar{f}_1)$ is correlated with low mean squared forecast errors.

The expected positive sign of the coefficient in the regression of $(f_{i2} - y)$ on $(f_{i2} - \bar{f}_1)$ explains the finding reported in Ehrbeck [1992] which shows that forecasters choose to ignore the information contained in average of past predictions. Table 4 reports the empirical results. The five sets of t-like statistics in Table 4 are calculated in the same way as in the previous section.

The first prediction of the model of rational boasting is strongly confirmed. However, it is important to note that these empirical results were calculated before the model was developed to explain them. Thus, the model of rational boasting is an ex-post rationalization which explains the results. The model was not used to make a prediction which was then confirmed. Again, we re- estimate the equations using back-casts in place of outcomes. As is shown in Table 5 the results are very similar.

In spite of the strong confirmation of the first prediction of the model of rational boasting, the model is false. The second prediction – that large mean squared $(f_{i2} - \bar{f}_1)$ is correlated with small mean squared forecast errors is equally strongly rejected as is shown in Table 6.

It is possible to understand this pattern using a behavioral model of bias. For example, forecasters might put too little weight on the lagged average because they are sincerely overconfident and not because they are strategically boasting. There is considerable evidence for subjective over-confidence in the absence of strategic motives for boasting [e.g. Einhorn and Hogarth 1978]. If the degree of subjective over confidence varies across forecasters, then other things equal (or uncorrelated) one would expect the more overconfident forecasters to announce forecasts further from the lagged average and further from the truth.

V. Conclusion

The tests of rationality in survey data reported in the literature can be criticized on the ground that professional forecasters' true motives actually invalidate the auxiliary hypothesis of a quadratic loss function. We have therefore develop and test the implications of several models in which fully rational forecasters choose not to announce the conditional expected value of the forecasted variable in order to make clients over-estimate the quality of their advice. To arrive at testable predictions, simple structures of an advising game have been studied. In our first model, professional forecasters are concerned about their reputation and when repeatedly announcing forecasts of a variable for the same target, they under-adjust old predictions in the light of new information. This model leads to the predictions that forecast errors are negatively correlated with changes in forecasts - an implication rejected by the data. Subsequent modelling attempts rationalize this empirically observed behavior by introducing a model of rational jumpiness. Here, professional forecasters over-adjust in the light of new information when making forecasts in order to make clients over-estimate the quality of advice that they are receiving. The prediction of this model is a positive correlation between forecast errors and changes in forecasts. However, this model falsely implies that forecasters with large changes in forecasts have small forecast errors. The opposite pattern is found in the data. A related model of rational boasting could explain why professional forecasters might choose to ignore the information contained in the average of past forecasts and why forecast errors might be correlated with the difference between the forecast and the lagged average forecast. But again, this model implies a false prediction – in this case that forecasters with forecasts far from the lagged average have low forecast errors. Again, this is the opposite of the pattern found in the data.

The empirical results in this paper reject our advising games. For model specifications that would explain the observed forecast error bias, the cross-sectional implications are rejected, and for model specifications that have valid cross-sectional implications, the bias in the forecast error is unexplained. Thus, the data reject our general approach to rationalizing biased forecasts and not narrow assumptions of our models and thus reinforces evidence against the rational expectations hypothesis. In contrast, the patterns we describe are consistent with behavioral models of bias. It is possible that our inability to reconcile the rational expectations hypothesis with the stylized facts is the result of assumptions made for tractability. In particular, we assume that clients choose forecasters at random and only decide when to abandon their current forecaster. However, the overwhelming failure of several attempts to reconcile the rational expectations hypothesis with the data casts doubt on its validity.

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Table 1

Test of Rational Stubbornness

Dependent Variable: Forecast-Outcome

	Regression	Constant	Change in forecast	
1.	Change 3rd-1st	-0.006	0.127	N = 384
	month forecast	$(-0.560)^a$	$(4.654)^a$	$R^2 = 0.08$
		$(-0.588)^2$	$(4.835)^2$	
		$(-0.649)^c$	$(2.563)^c$	
		$(-0.533)^d$	$(3.657)^d$	
		$(-0.647)^e$	$(2.856)^e$	
2.	Change 2nd-1st	-0.022	0.230	N = 396
	month forecast	$(-1.050)^a$	$(4.845)^a$	$R^2 = 0.09$
		$(-1.090)^2$	$(2.750)^2$	
		$(-1.176)^c$	$(1.165)^c$	
		$(-1.024)^d$	$(1.261)^d$	
		$(-0.889)^e$	$(1.164)^e$	
3.	Change 3rd-2nd	-0.004	0.236	N = 385
	month forecast	$(-0.427)^a$	$(6.160)^a$	$R^2 = 0.15$
		$(-0.436)^2$	$(5.514)^2$	
		$(-0.437)^c$	$(2.827)^c$	
		$(-0.389)^d$	$(2.080)^d$	
		$(-0.443)^e$	$(3.118)^e$	

OLS regressions with corrected standard errors
In parentheses t-like statistics for $H_0 : \boldsymbol{\beta} = \mathbf{0}$

Sources and definitions of variables as described in text. N is the total number of observations used in the regression.

a) calculated assuming time invariant variances and covariances of disturbances. Each pair of individuals' disturbances assumed to have the same correlation.

b) calculated without imposing restrictions on the variance covariance matrix of disturbances, except disturbances at different times are assumed to be uncorrelated. Disturbances estimated from residuals of individual regressions corrected for the number of degrees of freedom lost in estimation.

c) Like b) except that disturbances are assumed to be equal to forecast errors.

d) Like b) except that disturbances are estimated with forecast error minus fitted value using coefficients from an individual regression excluding the period in question.

e) Like b) except that disturbances are estimated with forecast error minus fitted value using coefficients from a pooled regression excluding the period in question.

Table 2

Test of Rational Stubbornness

Dependent Variable: Forecast-Backcast

	Regression	Constant	Change in forecast	
1.	Change 3rd-1st	-0.007	0.125	N = 363
	month forecast	$(-0.845)^a$	$(5.029)^a$	$R^2 = 0.08$
		$(-0.820)^2$	$(4.597)^2$	
		$(-0.958)^c$	$(2.715)^c$	
		$(-0.717)^d$	$(3.349)^d$	
		$(-0.910)^e$	$(3.157)^e$	
2.	Change 2nd-1st	-0.028	0.233	N = 374
	month forecast	$(-1.392)^a$	$(5.031)^a$	$R^2 = 0.11$
		$(-1.456)^2$	$(3.111)^2$	
		$(-1.482)^c$	$(1.206)^c$	
		$(-1.315)^d$	$(1.383)^d$	
		$(-1.146)^e$	$(1.235)^e$	
3.	Change 3rd-2nd	-0.005	0.254	N = 366
	month forecast	$(-0.566)^a$	$(7.323)^a$	$R^2 = 0.20$
		$(-0.572)^2$	$(7.487)^2$	
		$(-0.576)^c$	$(2.836)^c$	
		$(-0.497)^d$	$(3.112)^d$	
		$(-0.575)^e$	$(4.213)^e$	

OLS regressions with corrected standard errors

In parentheses t-like statistics for $H_0: \boldsymbol{\beta} = \mathbf{0}$

a,b,c,d,e) See notes to table 1.

Sources and definitions of variables as described in text.

Backcast is the average of the individual's available responses under the heading **"Treasury bill rate, three months, percent:** Discount rate on new issues of 91-day Treasury bills. Based on the weekly auction average rates" for the previous quarter.

N is the total number of observations used in the regression.

Table 3

Cross Sectional Test of Rational Stubbornness

Dependent Variable: Mean Squared Forecast Error

	Regression	Constant	Mean sq Change in f'cast	
1.	3rd month error	-0.012	0.321	$R^2 = 0.573$
	on dif 3rd-1st	(-1.695)	(5.305)	
	Rank Correlation = 0.658			
2.	2nd month error	-0.003	0.633	$R^2 = 0.660$
	on dif 2nd-1st	(-0.206)	(6.383)	
	Rank Correlation = 0.131			
3.	3rd month error	0.014	0.158	$R^2 = 0.382$
	on dif 3rd-2nd	(2.729.)	(3.601)	
	Rank Correlation = 0.818			

OLS Regressions and Rank Correlation Coefficients
In parentheses t-like statistics for $H_0: \boldsymbol{\beta} = \mathbf{0}$

Sources and definitions of variables as described in text.
Regressions across 23 forecasters.
Rank correlation from a separate regression using ranks.

Table 4

Test of Rational Boasting

Dependent Variable: Forecast-Outcome

	Regression	Constant	Forecast - lagged average	
1.	3rd month f'cast -	-0.017	0.386	N = 419
	1st month	$(-1.292)^a$	$(8.718)^a$	$R^2 = 0.41$
	average f'cast	$(-1.110)^b$	$(6.630)^b$	
		$(-1.711)^c$	$(3.598)^c$	
		$(-1.027)^d$	$(5.268)^d$	
		$(-0.843)^e$	$(4.494)^e$	
2.	2nd Month f'cast -	-0.035	0.609	N = 435
	1st month	$(-1.365)^a$	$(11.137)^a$	$R^2 = 0.47$
	Average f'cast	$(-1.265)^b$	$(7.181)^b$	
		$(-1.674)^c$	$(2.271)^c$	
		$(-1.169)^d$	$(4.330)^d$	
		$(-0.943)^e$	$(3.454)^e$	
3.	3rd Month f'cast -	-0.012	0.787	N = 419
	2nd Month	$(-1.061)^a$	$(20.616)^a$	$R^2 = 0.77$
	Average f'cast	$(-0.989)^b$	$(20.765)^b$	
		$(-1.224)^c$	$(3.716)^c$	
		$(-0.947)^d$	$(11.721)^d$	
		$(-0.794)^e$	$(14.063)^e$	

OLS regressions with corrected standard errors

In parentheses t-like statistics for $H_0 : \boldsymbol{\beta} = \mathbf{0}$

a,b,c,d,e) See notes to table 1.

Sources and definitions of variables as described in text.

N is the total number of observations used in the regression.

Table 5

Test of Rational Boasting

Dependent Variable: Forecast-Backcast

	Regression	Constant	Forecast - lagged average	
1.	3rd Month f'cast -	-0.020	0.357	N = 397
	1st Month	$(-1.734)^a$	$(9.469)^a$	$R^2 = 0.37$
	Average f'cast	$(-1.379)^b$	$(6.214)^b$	
		$(-2.263)^c$	$(3.653)^c$	
		$(-1.238)^d$	$(4.926)^d$	
		$(-1.015)^e$	$(4.212)^e$	
2.	2nd Month f'cast -	-0.040	0.557	N = 411
	1st Month	$(-1.652)^a$	$(10.417)^a$	$R^2 = 0.43$
	Average f'cast	$(-1.518)^b$	$(6.477)^b$	
		$(-1.864)^c$	$(2.127)^c$	
		$(-1.379)^d$	$(4.065)^d$	
		$(-1.088)^e$	$(3.034)^e$	
3.	3rd Month f'cast -	-0.014	0.729	N = 397
	2nd Month	$(-1.481)^a$	$(22.027)^a$	$R^2 = 0.73$
	Average f'cast	$(-1.369)^b$	$(23.558)^b$	
		$(-1.698)^c$	$(3.702)^c$	
		$(-1.301)^d$	$(11.485)^d$	
		$(-1.063)^e$	$(15.039)^e$	

OLS regressions with corrected standard errors
In parentheses t-like statistics for $H_0 : \boldsymbol{\beta} = \mathbf{0}$

a,b,c,d,e) See notes to table 1.
Sources and definitions of variables as described in text.
Backcast is the average of the individual's available responses under the heading "**Treasury bill rate, three months, percent:** Discount rate on new issues of 91-day Treasury bills. Based on the weekly auction average rates" for the previous quarter.
N is the total number of observations used in the regression.

Table 6

Cross Sectional Test of Rational Boasting

Dependent Variable: Mean Squared Forecast Error

	Regression	Constant	Mean sq f'cast - lagged avg.	
1.	3rd month error	-0.013	0.538	$R^2 = 0.621$
	on dif 3rd-1st avg	(-1.939)	(5.869)	
	Rank Correlation = 0.715			
2.	2nd month error	-0.012	0.971	$R^2 = 0.899$
	on dif 2nd-1st avg	(-1.724)	(13.654)	
	Rank Correlation = 0.552			
3.	3rd month error	-0.004	0.905	$R^2 = 0.899$
	on dif 3rd-2nd avg	$(-1.557.)$	(13.648)	
	Rank Correlation = 0.942			

OLS Regressions and Rank Correlation Coefficients
In parentheses t-like statistics for $H_0:\ \boldsymbol{\beta} = \mathbf{0}$

Sources and definitions of variables as described in text.
Regressions across 23 forecasters.
Rank correlation from a separate regression using ranks.

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