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MACROECONOMIC FORECASTS AND MICROECONOMIC FORECASTERS IN THE SURVEY OF PROFESSIONAL FORECASTORS

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Abstract

Do professional forecasters distort their reported forecasts in a way that compromises accuracy? New research in the theory of forecasting suggests such a possibility. In a recent paper, Owen Lamont finds that forecasters in the *Business Week* survey make more radical forecasts as they gain experience. In this paper, I use forecasts from the Federal Reserve Bank of Philadelphia's *Survey of Professional Forecasters* to test the robustness of Lamont's results. My results contradict Lamont's. However, careful examination of a methodological difference in the two surveys suggests a more general theory of forecasting that accounts for both sets of results.

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I. Introduction

Traditional discussions of the theory of forecasting assume that economic forecasters attempt to minimize their forecast errors. Implicit in this assumption is the idea that consumers desire accurate forecasts. Thus, a market-based explanation of forecaster behavior suggests that professional forecasters respond by using their training, expertise, and experience to formulate forecasts that meet their clients' desires for accuracy. Those who produce accurate forecasts command a higher salary than those who produce relatively inaccurate forecasts. Indeed, a large body of literature devoted to testing whether forecasts are "rational" assumes that forecast errors are costly and, thus, that forecasters are motivated solely by the accuracy of their forecasts.

Recently, some research economists have begun to question this view. The common theme of this new literature is that forecasters face economic incentives to distort their forecasts relative to their true expectations. Professional forecasters are thought to report to their clients not their best, most-informed estimates but, rather, estimates that have been adjusted in a way that compromises accuracy. These new theories are driven by the possibility that professional forecasters' compensation may not depend on forecast accuracy alone and carry the implication that forecasters will act strategically with respect to one another in formulating and adjusting their final forecasts. The new view casts professional forecasters in a particularly bad light.

Rather than being objective, fact-finding social scientists, the forecasters are thought to be willing to compromise scientific principles in formulating their forecasts.

Lamont (1995) argues that reputational incentives encourage a forecaster to report to his clients forecasts that are extreme and presents evidence to suggest that such incentives vary over a forecaster's lifetime. Lamont finds that forecasters report more radical--and less accurate--forecasts as they age. Laster, Bennett, and Geoum (1997) argue that some forecasters derive an economic benefit from the publicity their forecasts generate. According to their theory, the bigger the publicity payoff is, the larger the incentive is for a forecaster to adjust his forecast relative to his true expectation. Laster et al. find empirical support in favor of their publicity theory: self-employed forecasters and forecasters employed in the securities industry--two sectors that arguably have big payoffs to publicity--report forecasts that deviate more from consensus than those of forecasters employed in other sectors.[1] Additional work in this area includes Ito (1990), who finds that foreign exchange rate forecasters adjust their forecasts to reflect "wishful expectations" and Ehrbeck and Waldmann (1996), who present a model of strategic behavior in which forecasters fail to optimally adjust their forecasts in order to send a signal about their ability.

These new theories carry important consequences for forecast consumers and academic researchers. For example, an important conclusion of the Laster et al. study is that individual forecasts may be biased. Thus, consumers who rely upon an economic forecast for planning

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[1]Well-known professional forecasters A. Gary Shilling, Stephen Roach (chief economist at Morgan, Stanley & Co.), and Richard Rippe (chief economist at Prudential Securities) have questioned the results of this study in a recent *Wall Street Journal* article [”Some Economic Forecasts May Be Biased,” *Wall Street Journal*, March 25, 1997, p. A2].

purposes may be making sub-optimal decisions if they rely solely on the forecast of one individual. Similarly, academic researchers who test various theories of economic expectations should note that the forecasts obtained from professional forecasters may not reflect true expectations.

This paper investigates the robustness of Lamont's (1995) empirical results. Lamont tested his theory with annual forecasts obtained from the *Business Week* forecast survey. The *Business Week* survey is just one of many possible forecast surveys that Lamont could have used to test his theory. Many alternatives exist. One such alternative is the Federal Reserve Bank of Philadelphia's *Survey of Professional Forecasters*. This survey is conducted quarterly and includes professional forecasts for a wide range of economic variables--including the variables that Lamont used in his study--over the period 1968 to present. I use these data and follow Lamont's empirical methodology closely in an attempt to verify whether the reputational factors found to plague the *Business Week* survey also exist in the *Survey of Professional Forecasters*. The results provide little evidence to support Lamont's theory. However, a reinterpretation of these results based on a methodological difference in the two surveys suggests a more general theory that encompasses both of our findings.

The plan of the paper is as follows. Section II provides an overview of Lamont's theory of forecasting and reviews his empirical results. Section III presents the new results obtained with forecast data from the *Survey of Professional Forecasters* and proposes an explanation that encompasses both sets of results. Section IV concludes.

II. Previous Results

Lamont's Reputation Theory of Forecasting

Lamont's (1995) theory of forecasting argues that a professional forecaster may face a compensation scheme that rewards him for reporting forecasts that differ from his true beliefs. Such a forecaster is thought to be compensated on the basis of his reputation, which depends on his forecast accuracy and on the difference between his forecast and the prevailing consensus (or average) forecast. The latter determinant is crucial because it provides a forecaster with the incentive to act strategically in reporting his forecast.[2] In Lamont's model, a forecaster acts strategically by adjusting his forecast relative to the consensus in a direction that affects his reputation favorably. Because forecasters make such strategic adjustments, their forecasts do not reflect their beliefs. The forecasts may also be subject to more error than would occur in the absence of the strategic adjustment. Thus, unlike the traditional theory of forecasting, where only accuracy matters, Lamont's theory argues that each forecaster behaves strategically and is willing to sacrifice some accuracy to maximize his compensation.

An important question to ask is: What types of compensation schemes encourage forecasters to behave in this manner? Lamont discusses several possibilities. One possibility is that a forecaster's compensation depends on the publicity his forecast generates for his employer. A forecast that deviates from the prevailing consensus may generate more attention and press coverage than one that merely restates the consensus. To the extent that his employer values the

[2]Lamont's theorizing is a bit informal, so some important issues are left unaddressed. One issue is whether Lamont's model possesses an equilibrium. Laster et al. (1997) develop a model that is similar in spirit to Lamont's and show that a unique equilibrium distribution of forecasts exists in their model.

publicity, a forecaster faces an incentive to distinguish himself from his rivals by reporting a forecast that is far from the consensus opinion--even if doing so would compromise accuracy.[3] Other compensation schemes provide an incentive for a forecaster to adjust his forecast toward the consensus. For example, an employer's uncertainty about a forecaster's ability may cause a forecaster to adjust toward the consensus if the employer imposes a penalty for an incorrect forecast that is less when other forecasters are also wrong.

A common feature of the compensation schemes Lamont considers is that they all suggest a forecaster will consider the accuracy of his forecast and the difference between his forecast and the consensus. Thus, Lamont supposes that forecasters are compensated according to

$$w_j = R(|y - f_j|,|f_j - f_c|) \tag{1}$$

where w_j is the wage received by the jth forecaster, f_j is his reported forecast, f_c is the consensus forecast, y is the actual value of the variable being forecast, and |.| indicates absolute value. The forecast, f_j, incorporates the forecaster's "strategic adjustments" to his true expectation. Thus, f_j need not coincide with the forecaster's true expectation. Equation (1) indicates that wages are based on forecast accuracy (measured by the absolute value of the forecast error) and on the deviation of the forecast from consensus ($|f_j - f_c|$). Lamont assumes that compensation depends positively on accuracy. However, the relationship between compensation and the deviation from consensus may be positive or negative. If compensation depends on publicity, for example, a

[3] In this paper, the consensus forecast is defined as an average taken over forecasters. Zarnowitz and Lambros (1987) contains a critical discussion of this definition of consensus.

forecaster faces an incentive to differentiate himself from others. Thus, an increase in $|f_j - f_c|$ leads to higher wages. In contrast, an inexperienced forecaster, concerned that a large forecast error may damage his reputation, may perceive that his wage is related negatively to $|f_j - f_c|$. Finally, it is instructive to compare Lamont's theory of forecasting with the more traditional theory. When wages are unrelated to $|f_j - f_c|$, the forecaster has no incentive to act strategically and thus reports his true expectation, which can be represented by e_j. Thus, in the absence of strategic considerations, $f_j = e_j$, and wages depend only on the forecast error, $|y - e_j|$.

Do *Business Week* Forecasters Distort Their Forecasts?

Lamont's empirical tests are based on the premise that reputational incentives vary over a forecaster's professional lifetime. These incentives may become weaker or stronger as a forecaster gains experience, suggesting that the extent to which a forecaster considers the consensus in reporting his forecast is likely to change over time. For example, an inexperienced forecaster may perceive benefits to "playing it safe" and adjust his forecast toward the consensus, as described above. Over time, as uncertainty about this forecaster's ability diminishes, the incentive to adjust subsequent forecasts toward the consensus may also diminish, suggesting that $|f_j - f_c|$ varies positively with experience. A forecaster's deviation, $|f_j - f_c|$, may also vary negatively with experience. Here, a forecaster who has already established a (good) reputation may desire to protect that reputation by becoming more conservative over time. Such a forecaster faces an increasing incentive to adjust his forecast toward the consensus as he gains experience.

Using annual data from the *Business Week* forecast survey, Lamont constructs an unbalanced panel data set of forecasts made over the period 1971 to 1992 for annual real GNP growth, the annual unemployment rate, and the annual CPI inflation rate. The forecasts are for the years 1972 to 1993. The data are used to estimate the following cross-section/time-series regression model, specified to test the hypothesis that reputational incentives vary over a forecaster's lifetime:

$$|f_{j,t} - f_{c(-j),t}| = \alpha_j + \beta * AGE_{j,t} + \delta * AGE_{j,t} * MODEL_{j,t} + \gamma * AVGDEV_{(-j),t} + e_{j,t} \qquad (2)$$

where j indexes the forecaster and t the date being forecast; $f_{j,t}$ is the forecast made by the jth forecaster for period t; $f_{c(-j),t}$ is the corresponding consensus forecast, constructed as a cross-section average, excluding the jth forecaster; $AGE_{j,t}$ is a proxy for experience, constructed as a forecaster-specific time trend that begins at zero with the first non-missing observation for the jth forecaster and increments by one each period thereafter; $MODEL_{j,t}$ is a zero/one dummy variable that takes a value of one for forecasters that *Business Week* classifies as an "econometric model" and a value of zero for forecasters classified as "economist"; $AVGDEV_{(-j),t}$ is a proxy to control for the effect of aggregate uncertainty (i.e., macroeconomic shocks), constructed as a cross-section average of

$|f_{j,t} - f_{c(-j),t}|$, excluding the jth forecaster; and α_j is an individual-specific, time-invariant intercept to be estimated, β, δ, γ are slope parameters to be estimated, and e_{jt} is an error term with zero expected value and constant variance (across forecasters and time). Note that the "consensus" forecast at date t, denoted by $f_{c(-j),t}$, differs from forecaster to forecaster. For example, the consensus forecast considered by forecaster i is defined as the average forecast of all forecasters excluding i (i.e., $f_{c(-i),t}$). The variable $AVGDEV_{(-j),t}$ is defined in a similar manner.

Lamont's reputation theory--with time-varying incentives--suggests a forecaster's deviation from consensus ($|f_{j,t} - f_{c(-j),t}|$) ought to vary as he gains experience. Thus, a statistical test of the reputation hypothesis is constructed by estimating equation (2) and testing the estimated value of β for statistical significance. Rejection of the null hypothesis that β equals zero is evidence in favor of time-varying reputational incentives.

Business Week classified the participants in its survey as "economists" and "econometric models," and Lamont incorporates this distinction into his empirical work. Forecasters classified as "economist" are identified by the name of the economist reporting the forecast. Forecasters classified as "econometric model" are identified by the name of the model (i.e., Data Resources; Wharton, EFA, University of Pa.) or by the name of the firm or person responsible for the model (i.e., the Fair Model, Princeton Univ.; Townsend-Greenspan). Lamont argues that only humans--that is, forecasters classified as "economist"-- are subject to reputational incentives. The inclusion in (2) of the dummy variable MODEL permits the experience proxy AGE to have a differential effect on forecasts generated by an "econometric model."

Table 1.A shows Lamont's baseline results. The table reports Lamont's parameter estimates (t-statistics in parentheses) of the coefficients attached to AGE (β), AGE*MODEL (δ), and AVGDEV (γ), and the number of observations (column headed NT) for the three forecasts analyzed: annual real GNP growth, the annual unemployment rate, and the annual CPI inflation rate. Both fixed and random effects estimates are reported. Fixed effects estimates are obtained

by creating forecaster-specific dummy variables (i.e, the α_j) and applying OLS, under the assumption that the errors ($e_{j,t}$) are mean zero with constant variance over time and across forecasters. Random effects estimates are obtained by incorporating α_j into the error term and applying generalized least squares (GLS), under the assumption that the random effects, α_j, are uncorrelated across individuals. In constructing Table 1.A, I have divided Lamont's estimates of α and δ by 100 so that the estimated values of these parameters give the effect in percentage points of a one-year increase in experience.[4]

Table 1.A shows that statistically significant reputational effects exist in the *Business Week* survey for real GNP and unemployment rate forecasts. On average, forecasters move 0.018 to 0.022 percentage points away from the consensus forecast for real GNP growth per year of experience gained. The corresponding point estimate for the unemployment rate equation is roughly 0.015 percentage points per year. The coefficients on the dummy variable interaction term, AGE*MODEL, are negatively signed and partly offset the (positive) values of the estimated parameters on AGE, suggesting that only forecasts produced by "economists" are significantly related to experience. Finally, the parameter estimates on the proxy for aggregate uncertainty, AVGDEV, are positive, large, and statistically significant. This means that each individual's forecast deviates more from the consensus the larger is the deviation of others' forecasts from consensus.

[4]Lamont expressed his dependent variable, $|f_{j,t} - f_{c(-j),t}|$, in basis points. So in his paper, α and δ give the effect of a unit increase in experience in basis points, rather than in percentage points.

One problem with equation (2) is that it implies the size of a forecaster's optimal strategic adjustment, as represented by $|f_{j,t} - f_{c(-j),t}|$, is invariant to the magnitude of $f_{j,t}$. A more reasonable assumption is that the size of the adjustment should depend on the size of the variable being adjusted. Lamont recognizes this problem and suggests two solutions. Both solutions involve transforming the dependent variable in equation (2) such that the size of the forecast adjustment rises with the magnitude $f_{j,t}$. Lamont's first solution is to divide the dependent variable in equation (2) by the consensus forecast, $f_{c(-j),t}$. With this transformation, the absolute deviation from consensus is expressed as a percent of the consensus, $(|f_{j,t} - f_{c(-j),t}|/ f_{c(-j),t})*100.0$. Since the consensus forecast for real GNP growth is not always positive over the sample period, this adjustment is not reasonable for this forecast. Lamont's second solution is to divide the dependent variable in equation (2) by AVGDEV to yield a new dependent variable given by $(|f_{j,t} - f_{c(-j),t}|/AVGDEV_{(-j),t})*100.0$. Lamont's reported results, displayed here in Tables 1.B and 1.C, confirm his earlier findings: the coefficients attached to AGE are generally positive and significant, suggesting that a forecaster's deviation from the consensus rises as he gains experience.[5]

Lamont concludes that the statistical evidence supports his reputation theory of forecasting. His results suggest that as *Business Week* forecasters gain experience, they report more radical forecasts--that is, forecasts that differ from the consensus by increasing amounts.

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[5]Lamont does not report the fixed effects estimates associated with the transformed dependent variables. He also does not report an estimate for the coefficient on the aggregate variability proxy. It is not clear whether the proxy was included in the regressions.

The next section investigates the robustness of these results with a panel of forecasts obtained from the Federal Reserve Bank of Philadelphia's *Survey of Professional Forecasters*.

III. Reputational Effects in the *Survey of Professional Forecasters*

The Federal Reserve Bank of Philadelphia has been conducting the *Survey of Professional Forecasters* since the second quarter of 1990. The quarterly survey began in 1968 and was originally conducted by the National Bureau of Economic Research and the American Statistical Association. Forecasts are available for a large assortment of macroeconomic variables, including real GNP (GDP) and its components, interest rates, the unemployment rate, two price index series, and various other business indicators. Each survey includes quarterly forecasts for the current and following four quarters as well as annual forecasts for the current and following year. The survey has been used by academic researchers to test various theories in economics and appears to meet the basic requirements necessary to replicate Lamont's empirical methodology.[6]

There are two differences between the *Survey of Professional Forecasters* and the *Business Week* survey. First, no information in the former is available on the characteristics of the forecasters. Several recent analyses of strategic forecasting argue for the importance of controlling for forecaster-specific traits, such as industry affiliation [e.g., Ito (1990) and Laster (1997) et al.], in testing for strategic behavior. Indeed, as discussed above, Lamont controls for the distinction between "econometric model" and "economist." Second, the identity and

[6]Recent examples are Keane and Runkle (1990), Bonham and Cohen (1995) and Croushore (1996).

company affiliation of participants in the *Survey of Professional Forecasters* are confidential. The names and company affiliations of *Business Week* survey participants are published along with their forecasts. Each of these distinctions is discussed in further detail below.

Data

I follow Lamont closely in constructing an unbalanced panel of forecasts from the *Survey of Professional Forecasters* suitable for testing his reputation hypothesis. I start by gathering data from fourth-quarter surveys only. These surveys are conducted over the first two weeks of November and thus correspond closely to the timing of the *Business Week* surveys, the results of which generally appear in December. Second, following Lamont, I choose only forecasters who have made three or more annual forecasts for real GNP over the period 1971 to 1992. The final data set consists of one-year-ahead forecasts for real GNP growth, the annual unemployment rate, and the annual inflation rate (measured by the GNP implicit deflator). The first time-series observation is a forecast, made in November 1971, for annual real GNP growth, the annual unemployment rate, and inflation in 1972. The time-series dimension of the panel is identical to that used by Lamont.

Some data transformations were required to match Lamont's data set. A forecast for the level of real GNP is defined as the forecast for the level of nominal GNP divided by the forecast of the level of the GNP implicit deflator.[7] Real GNP growth and inflation are defined on a

[7] Early surveys included forecasts only for nominal GNP and the implicit deflator. Thus, I construct a forecast for the level of real GNP by taking the ratio of the two throughout the entire sample--even though real GNP forecasts are available later in the sample. Since forecasts for the CPI are not available until 1981Q3, I use the implicit deflator in place of the CPI.

fourth-quarter-over-fourth-quarter basis, using the two fourth-quarter forecasts of the corresponding levels that are available in a fourth-quarter survey. The annual year-ahead unemployment-rate forecast is constructed as an arithmetic average of the quarterly forecasts. All data are expressed in percentage points.

Table 2 provides some interesting statistics on the variables in the data set. The table shows that after using Lamont's greater-than-or-equal-to-three criterion, I retain 104 forecasters and roughly 700 observations per variable--both about the same as Lamont reported. On average, there are about seven observations per forecaster (compared with 5.5 in Lamont's data set); the minimum number of time series observations per forecaster is 3 (Min T_j) and the maximum 17 to 18 (Max T_j). Finally, the average absolute deviation of the forecast from consensus over the period 1971 to 1992 is 1.01%, 0.26%, and 0.73% for real GNP growth, the unemployment rate, and the inflation rate. Lamont reported average absolute deviations of 0.73%, 0.32%, and 0.51%. Thus, forecasts for real GNP growth and inflation in the *Survey of Professional Forecasters* appear a bit more diffuse than those in Lamont's data set.[8] On the whole, my data set appears to conform quite well to that used in Lamont's study. Figures 1 to 3 plot the individual and mean forecasts for each date in the sample.

Empirical Results Using the *Survey of Professional Forecasters*

[8]Some of the discrepancy might reflect a difference in the way that Lamont and I compute growth rates. Lamont appears to have computed his growth rates on an annual-average-over-annual-average basis while I use the fourth-quarter-over-fourth-quarter computation. The latter computation was a bit more convenient and is unlikely to affect the results.

This section presents the estimation results obtained with forecasts from the *Survey of Professional Forecasters*. As mentioned above, I am unable to distinguish between forecasts generated by "economists" and those generated by "econometric models." Therefore, I estimate the following variant of equation (2),

$$|f_{j,t} - f_{c(-j),t}| = \alpha_j + \beta * AGE_{j,t} + \gamma * AVGDEV_{(-j),t} + e_{j,t} \qquad (3)$$

which differs from (2) by excluding the dummy variable interaction term, $AGE_{j,t}*MODEL_{j,t}$. Table 3.A presents fixed and random effects estimates (t-statistics in parentheses) for β and γ obtained by estimating equation (3). On the whole, the results do not provide much support for Lamont's theory.

Like Lamont, I find that the aggregate uncertainty proxy, AVGDEV, has a large, positive, and statistically significant effect on a forecaster's absolute deviation from consensus. The point estimates of the effect are about the same as those reported by Lamont. The most important variable for testing Lamont's theory is AGE because the theory asserts that a forecaster's deviation from consensus ought to vary with his experience. As Table 3.A shows, the experience proxy, AGE, has a negative but, in many cases, a statistically insignificant effect on a forecaster's absolute deviation from consensus. The only exceptions concerning statistical significance are the fixed effects estimate in the real GNP growth equation (-0.0185, with a t-statistic of -1.75) and the corresponding fixed and random effects estimates in the inflation equation (-0.0156, with a t-statistic of -1.96, and -0.0137, with a t-statistic of -1.91). However, these point estimates are only borderline significant, and when I reestimate the fixed effects models using heteroskedasticity-consistent standard errors (not shown), the estimated t-statistics are insignificant (-1.54, for real GNP growth and -1.23, for inflation).[9] Thus, with the exception of the random effects parameter estimates of the inflation equation, I find little evidence to support the hypothesis that forecasters in the *Survey of Professional Forecasters* face time-varying incentives to act strategically in formulating their forecasts.

In keeping with my objective of replicating Lamont's empirical methodology as closely as possible, I also estimated two additional versions of equation (3). First, for the unemployment rate and inflation forecasts, I reestimated equation (3) by replacing the dependent variable, $|f_{j,t} - f_{c(-j),t}|$, with the variable $|f_{j,t} - f_{c(-j),t}|/f_{c(-j),t}$ *100.0 so that the absolute deviation from consensus is expressed as a percent of the consensus. Second, for all three variables, I expressed the dependent variable as a percent of AVGDEV by constructing the variable $|f_{j,t} - f_{c(-j),t}|/\text{AVGDEV}_{(-j),t}$ *100.0. Lamont found that these transformations bolstered the evidence in favor of reputational effects in the *Business Week* survey.

In contrast, my results are mixed. When the dependent variables are expressed as a percent of the consensus (Table 3.B), AGE exerts a <u>negative</u> and statistically significant effect on a forecaster's absolute percent deviation from consensus for the inflation forecast, regardless of the estimation method (fixed or random effects) and whether the heteroskedasticity correction is imposed (not shown). When the dependent variables are expressed as a percent of AVGDEV (Table 3.C), AGE has a negative and (borderline) statistically significant effect on the inflation

[9]These estimates are obtained in TSP, version 4.3, by using the ROBUST option in the PANEL command.

forecasts regardless of the estimation method, but the effect becomes insignificant when the fixed effects standard error is adjusted for the presence of heteroskedasticity. In no case does AGE exert a statistically significant effect on the real GNP growth and unemployment rate forecasts.

A summary of the preceding results is as follows. First, I find no evidence that real GNP growth and unemployment rate forecasts in the *Survey of Professional Forecasters* are affected by time-varying reputational incentives. Second, I do find some limited evidence in favor of a relationship between a forecaster's experience and his inflation-forecasts' deviations from consensus. However, in contrast with Lamont's findings, I find a negative relationship: forecasters in the *Survey of Professional Forecasters* become more conservative, not more radical, in reporting their inflation forecasts as they gain experience. Estimates of this effect, as reported in Table 3.A, suggest that forecasters, on average, adjust their inflation forecasts toward the consensus by 0.014 to 0.016 percentage points per year of additional experience. These results are subject to a caveat concerning the omission from equation (3) of the dummy variable interaction term, $AGE_{j,t}*MODEL_{j,t}$. As noted above, the *Survey of Professional Forecasters* lacks the information required to create the dummy variable MODEL. The effect of this omission, however, is unclear. From a theoretical perspective, it is not at all clear that *Business Week's* classification of forecasters as "economists" and "econometric models" is economically relevant. Econometric models--such as the well-known DRI model, which is included in Lamont's *Business Week* sample as an "econometric model"--do not generate forecasts without the aid of an economist. Indeed, McNees (1989) notes that forecasts from various econometric models may differ for a variety of reasons, including differences in forecasters' assumptions about future values of exogenous variables and about future monetary and fiscal policies.

Econometric model forecasts may always be manipulated--perhaps, to reflect reputational considerations--by changing the paths of the variables that are exogenous to the models.[10] From this perspective, it is not obvious that “econometric model” forecasts are less susceptible than “economist” forecasts to reputational incentives. A second reason for thinking that the omission of Lamont’s dummy variable interaction term may not be distorting my results is that Lamont found that variable to be insignificant in many cases, indicating that, in those cases, AGE does not have a statistically significant differential effect on “econometric model” forecasts.

A Robustness Check: The Case of the Missing Observations

A troubling feature of the *Survey of Professional Forecasters’* data set is the presence of “unusually long” periods of time in which some of the forecasters do not participate in the surveys. For instance, it is not uncommon for a forecaster to miss 20 consecutive quarterly surveys and then reappear as a participant, sometimes for only a few additional surveys before dropping out entirely. Figure 4 documents this phenomenon by plotting the real GNP growth forecasts of the 41st through 60th forecasters included in my sample. In each panel, I plot all real GNP growth forecasts available for a given forecaster over the entire sample period. The figure shows several individuals with prolonged periods of missing observations. For instance, forecaster #66 participated in fourth-quarter surveys in 1971 and 1973, missed all subsequent fourth-quarter surveys through 1980, and then reappeared in 1981. Similar participation records characterize forecasters #67, #68, #69, #72, and #75.

[10]McNees (1989) writes that forecasts may even be adjusted for “nefarious” reasons “...to induce the forecast user to adopt a certain course of action."

As is the case in most surveys, some participants eventually drop out of the *Survey of Professional Forecasters*. Occasionally, new participants are added. The spotty participation records noted in Figure 4 strongly suggest a problem in the way that the NBER assigned identification numbers to new participants. Specifically, the records suggest that the NBER may have assigned the same identification to multiple individuals--a possibility bolstered by the Philadelphia Fed's experience in conducting the survey since 1990, which indicates that individuals who miss several consecutive quarterly surveys tend not to reenter the survey at a later date. In this study, the improper assignment of individual identifiers is likely to lead to bias in the estimated coefficient on the experience variable and, thus, may explain why I fail to confirm Lamont's finding of a positive and significant relationship between experience (AGE) and the unemployment rate and real GNP growth forecasts. In extreme cases, the bias may be so severe that it reverses the sign of the coefficient estimate.

Thus, to check the robustness of my results, I adopt the following "correction" procedure. First, I check the quarterly participation record of each of the 104 forecasters in my panel. Second, I impose an "eight or more" missing-observations criterion to identify cases in which the NBER may have assigned the same identification number to two different individuals. After examining the quarterly participation record of each forecaster, I identify those who have an occurrence (i.e., a "gap") of eight or more consecutive missing observations. Three cases are possible. In case #1, three or more observations are available both before and after the gap. I assign the observations before the gap to the original forecaster; the observations after the gap are assigned to a newly created forecaster. In case #2, less than three observations are available before and after the gap. Here, I eliminate all observations from further consideration in order to maintain consistency with Lamont's three or more selection criterion. In case #3, more than three observations are available on one side of the gap and less than three are available on the other side. In this case, I eliminate the latter from further consideration. The remaining observations are assigned to the original forecaster.

After implementing this procedure, I find five occurrences of case #1, three occurrences of case #2, and 15 occurrences of case #3. Roughly 170 observations--slightly less than 25 percent of the total--are affected by the correction. Table 4 provides some summary statistics on the "corrected" data. For comparison, the table is formatted in the same manner as Table 2. Table 4 shows that there are 106 forecasters in the corrected data set, two more than in the original data set. The two additional forecasters emerge as a consequence of five occurrences of case #1 and three occurrences of case #2 in the original data set. About 35 fewer forecasts are available for each of the variables due to multiple occurrences of cases #2 and #3. The average forecast and average ABSDEV are about the same as those in the original data set. With fewer total observations and more forecasters, the average number of observations per forecaster falls in the corrected data set (by about one-half an observation).

Table 5.A shows the parameter estimates (t-statistics in parentheses) obtained by estimating equation (3) on the corrected data set. The table provides no evidence in favor of a relationship between experience (AGE) and a forecaster's deviation from consensus. In all cases, the t-statistics are extremely low. Notably, there now appears to be no relationship between experience and the inflation forecasts. As with the previous results, the present results suggest a large, positive, and significant effect of AVGDEV on a forecaster's deviation from consensus. The magnitude of the effect is about the same as that obtained with the original data set. The

fixed effects results are unchanged when the OLS standard errors are adjusted for the presence of heteroskedasticity, and the qualitative nature of the results is also unchanged when equation (3) is estimated with the dependent variables expressed as a percent of the consensus (Table 5.B) or as a percent of AVGDEV (Table 5.C).

The results obtained with the corrected data set are striking because they provide much stronger evidence against Lamont's theory than that provided with the "uncorrected" data. One problem with the corrected data set is that the rule used to reallocate its observations among forecasters is arbitrary, suggesting that the corrected data may be subject to some of the same problems that appear to plague the uncorrected data. To guard against this possibility, I constructed a third data set by eliminating from the panel all forecasters who have an occurrence of eight or more consecutive missing observations. Thus, rather than reallocating the observations of "problem forecasters," I simply exclude these observations from the panel. I continue to use my eight or more criterion to identify "problem forecasters." Since this criterion is stringent, the advantage of this data set is that it contains a panel that is almost certainly purged of individual-identifier problems. But there is also a cost because I lose about 25 percent of the total observations and about the same percentage of forecasters.

Tables 6 and 7 provide summary statistics and the estimation results The parameter estimates reported in Table 7 are very similar to those in Table 5.A, and there are no new findings to report. Based on either the corrected data set or on the data set that eliminates "problem forecasters," I find no evidence in favor of a relationship between a forecaster's experience and the deviation of his forecast from the consensus.

Overall, then, this study finds little evidence in favor of Lamont's reputation theory of forecasting.

Discussion of the Results: A Difference in Publicity Motive?

An important question to ask is: Why do my results differ from Lamont's? One possibility is that our results are sensitive to minor differences in empirical methodology. As noted above, Lamont and I use alternative methods to compute the growth rates of real GNP and the price level. We also use alternative measures of the price level. To the extent that both groups of forecasters engage in strategic behavior, it is hard to imagine that the degree to which such behavior is reflected in the data hinges on these issues. Thus, data issues are deemed unlikely to account for our radically different conclusions. In this regard, it is important to note that there are no differences in our definition of the annual unemployment rate, and I found no evidence to suggest the unemployment rate forecasts incorporate strategic behavior.

Another possibility is that my results are biased because I do not control for the distinction between forecasts made by "economists" and those made by "econometric models." However, the case for the importance of this distinction seems weak on both theoretical and empirical grounds, as discussed above.

A more likely explanation lies in the possibility that there are behavioral differences between the two groups of forecasters related to the degree of anonymity provided by the two surveys. Forecasters in the *Survey of Professional Forecasters* are anonymous and thus expect to receive no publicity from their participation, while *Business Week* forecasters, whose names and company affiliations are published with their forecasts, can, in contrast, expect a great deal of publicity from their participation. Thus, it is reasonable to expect that the two surveys attract a different class of volunteer forecasters. The *Business Week* survey may attract precisely those forecasters who face a publicity incentive and who behave in a manner consistent with Lamont's theory. Participants in the *Survey of Professional Forecasters* may volunteer their time for reasons unrelated to a publicity motive--perhaps out of a sense of professional responsibility to the NBER and to the Federal Reserve System--and thus face different incentives than the *Business Week* forecasters.

Although this explanation seems capable of accounting for the difference in conclusions that Lamont and I reach, it does have some limitations. First, it ignores Lamont's non-publicity sources of strategic forecasting, and it is not clear that his results are driven solely by a publicity motive. To the extent that other motives are more important drivers of Lamont's results, something else is responsible for our different conclusions. Second, the explanation assumes that different individuals comprise the surveys. Unfortunately, The Philadelphia Fed is not able to attach names to the participants in the early surveys. Thus, it is not possible to determine the degree to which the two surveys share forecasters. Substantial overlap in participants would invalidate the proposed explanation for our different conclusions.

The proposed explanation does have some appeal, however. Perhaps most important, it is consistent with the idea that microeconomic forces, other than forecast accuracy, affect forecasters' behavior, including the choice of surveys in which to participate. Thus, the explanation endogenizes the survey-participation choice and suggests a more general theory of forecasting that encompasses not only how forecasters in a particular survey behave but also the choice of surveys in which to participate. This more general theory has strong implications that could be tested by assembling a multi-dimension panel data set that incorporates many different forecast surveys. The proposed explanation also points to the importance of considering carefully the forecast survey to use in testing the new theories of forecasting.

IV. Summary and Conclusions

Recent theories of forecasting suggest that professional forecasters may face an economic incentive to distort their reported forecasts in a way that compromises the accuracy and information content of the forecasts. As a consequence, reported forecasts may not reflect true expectations. This paper uses data from the Philadelphia Fed's *Survey of Professional Forecasters* to test Owen Lamont's reputation theory of forecasting. Lamont tested his theory on data from the *Business Week* forecast survey and found that forecasters tend to report forecasts that deviate more from the consensus, and, thus, that are more radical, as they gain experience.

Using data from the *Survey of Professional Forecasters*, I replicate Lamont's empirical methodology closely and find no evidence in favor of a relationship between a forecaster's experience and the deviation of that forecaster's reported forecast from the consensus. However, a careful examination of an important difference between the surveys related to the anonymity of the participants suggests that the two surveys may draw a different class of forecasters. This may account for my failure to confirm Lamont's findings and points to the importance of choosing carefully the forecast survey with which to test the new theories of forecasting. A more general theory of forecasting that endogenizes the survey-participation choice is proposed.

Table 1.A
Lamont's Estimation Results
***Business Week* Forecast Survey**
Dependent Variable:$|f_{j,t} - f_{c(-j),t}|$ in percent

Forecast Tested	**β (AGE)**	**δ (AGE*MODEL)**	**γ (AVGDEV)**	**NT**
Real GNP Growth (%)				
Fixed Effects	0.0180	-0.0091	0.77	728
	(2.44)	(0.620)	(7.54)	
Random Effects	0.0220	-0.0156	0.77	728
	(3.30)	(1.32)	(7.87)	
Annual Unemployment Rate (%)				
Fixed Effects	0.0148	-0.0228	0.67	700
	(4.28)	(3.54)	(4.53)	
Random Effects	0.0145	-0.0190	0.63	700
	(4.66)	(3.35)	(4.83)	
Annual CPI Inflation (%)				
Fixed Effects	0.0053	-0.0097	0.65	700
	(1.21)	(1.08)	(4.51)	
Random Effects	0.0059	-0.0117	0.65	700
	(1.46)	(1.58)	(4.74)	

Source: Lamont (1995), Table 2.

Table 1.B
Lamont's Estimation Results
***Business Week* Forecast Survey**
Dependent Variable:$|f_{j,t} - f_{c(-j),t}| / f_{c(-j),t}$ in percent

Forecast Tested	**β (AGE)**	**δ (AGE*MODEL)**	**γ (AVGDEV)**	**NT**
Annual Unemployment Rate (%)				
Random Effects	0.18	-0.17	N.A.	700
	(4.31)	(-2.39)		
Annual CPI Inflation (%)				
Random Effects	0.36	-0.19	N.A.	700
	(3.82)	(1.12)		

Source: Lamont (1995), Table 3.

Table 1.C
Lamont's Estimation Results
***Business Week* Forecast Survey**
Dependent Variable:$|f_{j,t} - f_{c(-j),t}|$/ $AVGDEV_{(-j),t}$ in percent

Forecast Tested	**β (AGE)**	**δ (AGE*MODEL)**	**γ (AVGDEV)**	**NT**
Real GNP Growth (%)				
Random Effects	2.71	-2.28	N.A.	728
	(2.82)	(-1.35)		
Annual Unemployment Rate (%)				
Random Effects	3.52	-7.18	N.A.	700
	(3.56)	(-4.04)		
Annual CPI Inflation (%)				
Random Effects	1.22	-2.89	N.A.	700
	(1.50)	(1.96)		

Source: Lamont (1995), Table 3.

Table 2
Forecast Summary Statistics
Survey of Professional Forecasters

	Real GNP Growth	Unemployment Rate	Inflation Rate
Number of Forecasters	104	104	104
Number of Observations	688	701	694
Average Forecast (%)	3.08	6.83	5.25
Average ABSDEV (%)	1.01	0.26	0.73
Average Number of Observations per Forecaster	6.6	6.7	6.7
Min T_j	3	3	3
Max T_j	17	18	17

Table 3.A
Estimation Results
Survey of Professional Forecasters
Dependent Variable:$|f_{j,t} - f_{c(-j),t}|$ in percent

Forecast Tested	**β (AGE)**	**γ (AVGDEV)**	**NT**
Real GNP Growth (%)			
Fixed Effects	-0.0185	0.65	688
	(-1.75)	(4.65)	
Random Effects	-0.0111	0.62	688
	(-1.19)	(4.62)	
Annual Unemployment Rate (%)			
Fixed Effects	-0.0003	0.83	701
	(-0.15)	(9.09)	
Random Effects	-0.0008	0.84	701
	(-0.47)	(9.47)	
Annual PGNP Inflation (%)			
Fixed Effects	-0.0156	0.67	694
	(-1.96)	(4.08)	
Random Effects	-0.0137	0.57	694
	(-1.91)	(3.70)	

Table 3.B
Estimation Results
Survey of Professional Forecasters
Dependent Variable:$|f_{j,t} - f_{c(-j),t}| / f_{c(-j),t}$ in percent

Forecast Tested	β (AGE)	γ (AVGDEV)	NT
Annual Unemployment Rate (%)			
Fixed Effects	-0.0166	N.A.	701
	(-0.54)		
Random Effects	-0.0225	N.A.	701
	(-0.89)		
Annual PGNP Inflation (%)			
Fixed Effects	-0.4573	N.A.	694
	(-2.80)		
Random Effects	-0.3940	N.A.	694
	(-2.63)		

Table 3.C
Estimation Results
Survey of Professional Forecasters
Dependent Variable:$|f_{j,t} - f_{c(-j),t}|$/ $AVGDEV_{(-j),t}$ in percent

Forecast Tested	**β (AGE)**	**γ (AVGDEV)**	**NT**
Real GNP Growth (%)			
Fixed Effects	-1.4104 (-1.27)	N.A.	688
Random Effects	-0.8534 (-0.86)	N.A.	688
Annual Unemployment Rate (%)			
Fixed Effects	-0.3682 (-0.46)	N.A.	701
Random Effects	-0.2856 (-0.43)	N.A.	701
Annual PGNP Inflation (%)			
Fixed Effects	-1.8269 (-1.67)	N.A.	694
Random Effects	-1.7226 (-1.71)	N.A.	694

Table 4
Forecast Summary Statistics
Corrected Data
Survey of Professional Forecasters

	Real GNP Growth	**Unemployment Rate**	**Inflation Rate**
Number of Forecasters	106	106	106
Number of Observations	654	664	659
Average Forecast (%)	3.05	6.84	5.24
Average ABSDEV (%)	0.98	0.26	0.70
Average Number of Observations per Forecaster	6.2	6.3	6.2
Min T_j	3	3	3
Max T_j	17	18	17

Table 5.A
Estimation Results
Corrected Data
Survey of Professional Forecasters

Dependent Variable:$|f_{j,t} - f_{c(-j),t}|$ **in percent**

Forecast Tested	**β (AGE)**	**γ (AVGDEV)**	**NT**
Real GNP Growth (%)			
Fixed Effects	-0.0038	0.64	654
	(-0.29)	(4.61)	
Random Effects	0.0070	0.61	654
	(0.61)	(4.60)	
Annual Unemployment Rate (%)			
Fixed Effects	0.0006	0.84	664
	(0.24)	(8.92)	
Random Effects	0.0001	0.83	664
	(0.04)	(9.14)	
Annual PGNP Inflation (%)			
Fixed Effects	0.0016	0.59	659
	(0.20)	(4.23)	
Random Effects	0.0013	0.56	659
	(0.17)	(4.17)	

Table 5.B
Estimation Results
Corrected Data
Survey of Professional Forecasters
Dependent Variable: $|f_{j,t} - f_{c(-j),t}| / f_{c(-j),t}$ **in percent**

Forecast Tested	**β (AGE)**	**γ (AVGDEV)**	**NT**
Annual Unemployment Rate (%)			
Fixed Effects	0.0071	N.A.	664
	(0.18)		
Random Effects	0.0020	N.A.	664
	(0.06)		
Annual PGNP Inflation (%)			
Fixed Effects	-0.0786	N.A.	659
	(-0.48)		
Random Effects	-0.1013	N.A.	659
	(-0.66)		

Table 5.C
Estimation Results
Corrected Data
Survey of Professional Forecasters
Dependent Variable:$|f_{j,t} - f_{c(-j),t}|$/ $AVGDEV_{(-j),t}$ in percent

Forecast Tested	**β (AGE)**	**γ (AVGDEV)**	**NT**
Real GNP Growth (%)			
Fixed Effects	0.1332 (0.10)	N.A.	654
Random Effects	0.9932 (0.81)	N.A.	654
Annual Unemployment Rate (%)			
Fixed Effects	-0.0764 (-0.08)	N.A.	664
Random Effects	-0.0438 (-0.05)	N.A.	664
Annual PGNP Inflation (%)			
Fixed Effects	0.0594 (0.05)	N.A.	659
Random Effects	-0.0910 (-0.08)	N.A.	659

Table 6
Forecast Summary Statistics
Problem Forecasters Dropped
Survey of Professional Forecasters

	Real GNP Growth	Unemployment Rate	Inflation Rate
Number of Forecasters	81	81	81
Number of Observations	518	528	522
Average Forecast (%)	2.97	6.83	5.32
Average ABSDEV (%)	1.03	0.26	0.75
Average Number of Observations per Forecaster	6.4	6.5	6.4
Min T_j	3	3	3
Max T_j	17	18	17

Table 7
Estimation Results
Problem Forecasters Dropped
Survey of Professional Forecasters

Dependent Variable:$|f_{j,t} - f_{c(-j),t}|$ in percent

Forecast Tested	**β (AGE)**	**γ (AVGDEV)**	**NT**
Real GNP Growth (%)			
Fixed Effects	-0.0072	0.57	518
	(-0.49)	(3.78)	
Random Effects	0.0039	0.54	518
	(0.30)	(3.75)	
Annual Unemployment Rate (%)			
Fixed Effects	-0.0005	0.78	528
	(-0.18)	(7.66)	
Random Effects	-0.0004	0.79	528
	(-0.17)	(8.02)	
Annual PGNP Inflation (%)			
Fixed Effects	-0.0041	0.60	522
	(-0.45)	(4.28)	
Random Effects	-0.0052	0.58	522
	(-0.62)	(4.44)	

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