International Journal of Forecasting 4 (1988) 325–330
North-Holland

THE FUTURE OF FORECASTING

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Abstract: This paper discusses our aims in organizing this special issue. We believe that a synthesis of the field is needed to resolve the many conflicts between theory and empirical evidence and between advocates of the various forecasting methods. One step toward synthesis is to agree on the major problems in the field, the goal of this issue. Another step is to establish standards for both practice and research and some proposals are made to this end. We also give some personal opinions on the most important research opportunities in the field.

Keywords: Forecasting–general, judgment, time series, econometrics

1. Aims of the special issue

We believe that there is substantial confusion on the part of practitioners interested in using research literature to select and apply forecasting methods. Advocates of the various forecasting methods disagree about the most appropriate method for any situation. They also disagree about the details of how particular models should be applied and evaluated. Our hope is that a special issue of the International Journal of Forecasting focused on major problems in the field will lead to synthesis in research and make forecasting a more useful tool for planning and decision-making.

We had two specific aims in organizing this special issue: (1) to identify the major problems, both theoretical and practical, facing forecasters and (2) to propose research agendas to help understand and solve those problems. A wide range of papers meeting these aims were selected for the issue. Several authors give critical evaluations of the major forecasting methods. Fischhoff deals with problems in judgmental forecasting; a set of related papers by Ord, Cogger, and Chatfield evaluate problems in time series forecasting; and McNees and Ashley evaluate economic and econometric forecasting. Huss reviews scenario analysis, in our opinion an important method in its own right because it features a direct link between planning and forecasting. Applied forecasting is evaluated in two critical areas. Mahajan and Wind evaluate forecasting for new products, while Taylor evaluates forecasting of market prices. Finally, a group of papers by Belsley, Armstrong, and Makridakis takes a global view of forecasting and offers some general principles to guide future research.,

The plan of this paper is as follows. The next section discusses the need for synthesis in forecasting research. The following section argues that standards are needed for both practice and research in forecasting. We propose a set of standards for carrying out empirical research. We also

offer our own data bases for this empirical research to anyone interested. Finally, some personal opinions are offered on the most important research opportunities in the field.

2. The need for synthesis in forecasting

Prior to 1980, research in forecasting emphasized theoretical development of quantitative methods. Kalman (1960), Klein (1962), Brown (1959, 1963), and Box and Jenkins (1970) laid the foundations of quantitative forecasting and researchers too numerous to mention here greatly expanded the field. This period of theoretical development led to many expectations about forecasting performance. Some of the more important expectations were:

(1) The better the fit (ex post) of the forecasting model, the better the accuracy (ex ante) should be.

(2) Models selected for optimal one-step-ahead prediction should perform just as well at longer leadtimes.

(3) Models with adaptive parameters should perform better than models with fixed parameters.

(4) Greater complexity in model-building should lead to greater accuracy.

(5) Econometric models should necessarily be more accurate at forecasting than time series models.

Since 1980, a large body of empirical research has been produced which challenges these theoretical expectations. Although most forecasting theory in both econometrics and time series is based on identifying a model which yields a good ex post fit, empirical evidence shows that good fit often does not lead to good forecasting ex ante. More seriously, there is considerable evidence that the quality of fit is not even correlated closely with forecast accuracy ex ante. One-step-ahead accuracy also seems to have little relationship with accuracy at longer leadtimes. Models with adaptive parameters have not done better than models with fixed parameters; just the opposite result may be true. Complexity in model-building does not necessarily improve accuracy. Simple forecasting methods have performed well compared to far more sophisticated alternatives. For summaries of the empirical evidence on these points, see Fildes (1985), Makridakis et al. (1982), Makridakis (1986) and Armstrong (1984, 1986).

The empirical evidence on econometric vs. time series forecasting is more difficult to interpret. For example, Armstrong's (1984) review concluded that econometric models were no better than univariate time series models in short-term applications. Another review by Fildes (1985) disputes this conclusion, contending that Armstrong relied on studies that did not conform to good modeling practice.

The reasons for these conflicts are controversial as discussed in the commentary on the M-competition edited by Armstrong and Lusk (1984). Many empiricists argue that theory is bound by unrealistic assumptions, while many theorists argue that empirical studies have been poorly designed. Whatever the reasons for the conflicts, they cannot be ignored because they impede the development of forecasting as a scientific discipline. In our opinion, the challenge for the future is to consolidate the field of forecasting. We should synthesize past achievements and seek to develop a comprehensive theoretical basis for selecting and applying forecasting methods in practice.

3. The need for standards in forecasting

How can synthesis be achieved? First, we can reach some agreement on the major problems in the field, the goal of this special issue. Second, we can establish standards for practice in forecasting. We propose that the International Institute of Forecasters (IIF) create a committee on practice to develop such standards. One important problem that this committee should study is the implementation of forecasting models in computer software. The lack of standards today means that one can get vastly different results from the same forecasting model implemented by different programmers. Surely sufficient research is available by now to reach agreement on the technical details of how forecasting models should be operated.

We also believe that the IIF should develop ethical standards for forecasting practice. Although fortune telling is illegal in many states and countries, we see no difference between fortune telling and the stock market forecasts which appear regularly in the business press. We doubt that many investors are aware that stock prices follow a random walk process. We also doubt that many investors are aware of the track records of the forecasters on which they rely. Although it would certainly be a difficult standard to enforce, we believe that forecasters should reveal their track records to the public. The IIF might also study the possibility of publicizing the track records of forecasters who habitually mislead the public. We concede that there are legal problems with this idea but we feel that something should be done to establish professional standards of ethics in forecasting.

A third step toward synthesis in forecasting is to establish standards for research. Armstrong's paper in this issue proposes such standards. We agree with Armstrong and the next section supplements his proposals with more specific guidelines for empirical research.

4. Standards for empirical research

We predict that the trend toward empirical research in forecasting will continue. Unfortunately, much of the empirical research that has been done to date is difficult to interpret. It is particularly frustrating that it has not been possible to replicate some important studies in the field. The following guidelines should make empirical research more useful as a basis for both theoretical development and practice:

(1) Hypothesis testing: Empirical studies should be designed to test prespecified hypotheses. To prevent bias and advocacy, multiple hypotheses should be established as well.

(2) Real data: Empirical testing must be done with real data. Furthermore, that data should be collected in a random fashion when possible.

(3) Replicability of results: Unless the data are made available to other researchers and details of the forecasting methods are known, empirical studies have little value.

(4) Post-sample testing: Empirical evaluations of forecast accuracy should be done on hold-out samples of data that were not used to develop the forecasting model.

(5) Dynamic testing: In most empirical studies, all forecasts have been made from one point in time. In practice, forecasting is usually a continuous process, with a new set of forecasts made each time period. When enough data are available, dynamic testing should be done. Forecasts should be made from successive time origins to judge the stability of model performance.

(6) Multiple accuracy measures: It is not necessarily true that different forecasting methods do equally well under different loss functions. Various accuracy measures (MAPE, MSE, medians, ranks, etc.) should be used.

(7) Benchmark comparisons: Forecasts should be compared to naive methods or other simple benchmarks to ensure that the data are worth forecasting in the first place. As Ashley recommends, time series methods can be used to benchmark econometric models.

(8) Costs and effort required: Criteria other than accuracy are usually important in choosing a forecasting method. Estimates of the cost and effort required to learn, apply, and monitor forecasting methods should be made.

5. Data bases for forecasting research

There are many data bases of economic and financial information in the public domain. However, micro-level or company data cannot easily be found. More company data is urgently needed for forecasting research. In the meantime, we offer the data bases listed below to interested researchers. All data are available on floppy disks formatted for the IBM PC. To obtain the inventory data at the cost of duplication and mailing, write to Gardner. To obtain the M, M2, or M3 competition data under the same terms, write Makridakis.

Inventory data: This is a collection of 160 time series of inventory demands for service parts. One hundred series are composed of quarterly demands contributed by an automotive parts distributor in the United States. Another such distributor contributed 60 series, each based on 13 accounting periods per year. These companies use exponential smoothing systems to process the series.

M-Competition data: Either the sample of 111 series or the 1,001 series are available. See Makridakis et al. (1982) for a description of the series.

M2-Competition data: This is a collection of 100 time series. Forecasts are required for 30 series. The other series are independent variables such as prices, advertising, and GNP, for use in explaining and predicting the 30 series. The series are accompanied by a short description of the four companies who contributed data for the M2-competition.

M3-Competition data: This is a collection of 150 monthly time series of sales data from five companies. Like the inventory data, automatic forecasting systems are used in practice to process all of these series.

6. Conclusion: Research opportunities in forecasting

What does the future hold for the field of forecasting? Based on the number of tough research problems raised in this special issue, it may seem difficult to be optimistic about future progress in the field. However, we are optimistic because so many of these problems represent lucrative research opportunities.

Perhaps the most important research opportunity in this issue is advanced by Fischhoff: there is a considerable research literature on judgment in psychology and related fields which has yet to be

exploited by forecasters. Since judgment influences every forecast (see the papers by Fischhoff and Belsley), we predict that researchers will respond to this opportunity.

In new product forecasting, an area with a significant impact on business profitability, relatively little empirical research has been done to date. Many new-product models are available, although not much is known about their comparative performance. We predict that Mahajan and Wind's call for an M-competition for new product models will lead to valuable research. Meta-analysis is another type of research that has yet to be exploited in new product forecasting and we expect to see papers in this area as well.

In forecasting market prices, it seems clear that nothing can be done with daily stock prices. However, Taylor argues that forecasting may prove valuable in other markets. Certainly the potential returns in other markets are worth pursuing from the investor's viewpoint, the reason why we predict that work in this area will continue.

In the general area of quantitative forecasting, the authors in this issue propose too many research opportunities to review here. Perhaps the most important opportunity is discussed in the paper by Ord: to continue to promote synthesis between econometrics and time series analysis through work on multiple time series methods. We believe that this work is important because it should lead to a better understanding of the relative accuracy of econometric and time series methods. This understanding can only lead to improvements in both types of forecasting. Little is known about the accuracy of multiple time series methods and we predict that useful empirical work in this area is forthcoming.

In conclusion, this special issue is certainly not exhaustive of the problems and research opportunities in forecasting. We welcome comments from readers as well as formal submissions to the IJF on likely directions for forecasting research.

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