This book is homage to Sir Clive W.J. Granger and Robert F. Engle III for their giant contributions to econometrics, and at the same time congratulating them for receiving the 2003 Prize in Economics in Honour of Alfred Nobel. A group of mostly younger authors have contributed studies that in one way or another continue the work of the laureates.

The reader first meets an impressive list of the contributions of the two laureates in a formal dedication. Following the editors’ introduction, the laureates present their short accounts of how they perceive the creative processes they have been involved in. A quote from Engle’s confession: “There is nothing in our chosen career that is as exhilarating as having a good idea. But a very close second is seeing someone develop a wonderful new application from your idea”.

Nine out of the 13 essays in this book are about modelling financial time series. They seem to be crowding out macro-economic data as objects of econometric research. The reasons are obvious:

(i) You can get almost arbitrarily many observations, whereas with macrodata, a few hundred observations is a rare luxury, and
(ii) The data are exact; no revisions are needed.

The long financial series are particularly useful for testing non-linear models using methods that do not require second-order stationarity, in short methods that have their roots in Granger’s and Engle’s works.

The first essay is by well-known authors: T.G. Andersen, T. Bollerslev, F.X. Diebold and G. Wu. The subject is the beta coefficient, measuring the risk of an individual equity as compared to the general stock market risk. Beta is simply the regression coefficient between the price of the asset and the market index. A debate has been going on for decades whether these betas are constant or varying over time. The authors eloquently show that although individual variances and covariances are highly persistent, the betas of some major company shares are not, because of a nonlinear fractional cointegration between individual equity and the market. Consequently, the betas are best modelled as stationary time series. The question is how surprising this result is. Major companies should have a major impact on the index.

Given the mass of data of the stock market, the most elusive goal of econometricians is to find a model that would forecast stock prices; if known by the public the gain would be eliminated through arbitrage. Still, people continue to try to test their models against random walk forecasts, only to find that the latter are hard to beat. One of the most fascinating essays in this book is by Y. Bao and T.-H. Lee. They test various nonlinear models on Standard&Poor’s 500 Index (S&P500) and come up with an astonishing result. Forecasting the entire distribution one period ahead, they are able to beat the naïve forecast only for the right tail of the distribution. However, it is hard to see how you could make money on this knowledge and the authors do not claim that you could. But this is a new result and it is elegantly presented using the Kullback-Leibler Information Criterion, testing simultaneously both model specification and parameter estimation errors.
The next article: *Flexible seasonal time series models*, by Z. Cai and R. Chen, presents a new method to model seasonal time series. The model is a locally linear factorisation of trend and season with a sliding window and a kernel smoother. The authors show that the estimates are consistent. The method is illustrated using simulation and applying it on two real time series. The results are compared with a model by Burman and Shumway (1998). However, with no proper testing of forecast accuracy it does not become clear to the reader how this model would be superior to other ways of modelling and forecasting seasonal time series. The most common methods, naïve forecasts and Holt-Winters are not included, not to speak of seasonal ARIMA. Consulting *Forecasting Principles* on the IIF website would have been of great help, or indeed past volumes of *IJF* where many seasonal forecasting studies have been published. The purpose of this paper becomes even more obscure because the authors’ conclusions are missing.

If you want to update your knowledge of ARFIMA (Autoregressive Fractionally Integrated Moving Average) models, the next survey article by N. Hang and W. Palma is what you should read. Since Granger and Joyeux (1980) introduced the model, the maximum likelihood (ML) estimation of its parameters has intrigued many researchers. Methods such as exact ML based on the Cholesky decomposition of the covariance matrix tend to be complex and even inefficient in small samples. A faster procedure is the Levinson-Durbin algorithm. To both the estimation of autocorrelations is critical, and ways to do this are presented. Casting ARFIMA models into state-space form lead to exact ML estimates. Estimating the moving average part of time series models has always been the trickier part. Many “quasi” ML methods have been presented where a long AR polynomial approximates the MA side. In simulation experiments the authors show that all the ML methods, exact and quasi, are about equally accurate. All have a small downward bias in the degree of differencing. The authors survey the literature of incomplete, or otherwise irregular and heteroscedastic time series with long memory. The paper’s 35 pages cover a large chunk of the literature. The only reference I miss is the seminal paper by Geweke and Hudak (1983) where the error minimisation is done in the frequency domain.

It is hard for me to write about the next essay: *Boosting-based frameworks in financial modeling: application to symbolic volatility forecasting*, by V.V. Gavrischchaka. The reason is simple, not to say embarrassing: I did not understand its message. Mainly, it seems to address the question when an ensemble of machine-learning (black box) models is better than a single model, what is usually called “combining” or “pooling” in the vocabulary the of readers of this journal. In an empirical experiment models are compared by calculating how well they are able to classify data points as either high or low volatility regimes. Here a combination of machine-learning models are claimed to have a hit rate 240% higher that the best GARCH(1,1). New terms, which may be well known to experts in this field, are used with very short explanations. Except for a handful to classical articles, the references are probably unknown to the readers of *IJF*, and many would be hard to find, not being journal publications or books. Finally, the style is poor with many grammatical errors. No forecasting competition in accordance with IIF principles is performed, so we do not know if this complicated technique improves on simpler ones. If these data-heavy methods are to enter into what we know as the forecasting sphere, first we will all have to adopt a common language.

R. Hillebrand has contributed a highly interesting essay: *Overlaying time scales in financial volatility data*. Many authors have found that financial data contain both slow and fast mean-reverting. The author tries to capture this dual feature using generalisations of GARCH models that allow for different time scales. One way to estimate model coefficients is by a sliding window. None of the models tested improves on a GARCH(1,1). The elusive short term correlation is easily observable from wavelet charts. Considerable improvement in forecast accuracy is achieved with Muller’s heterogeneous ARCH or ‘HARCH’ model, where long term fluctuations are used to forecast short term volatility. All forecast comparisons are
Based on observations outside the estimation period, but differences in accuracy are not tested statistically.

*Evaluating the ‘Fed Model’ of stock price valuation: an out-of-sample forecasting perspective* is the title of the next essay written by D.W. Jansen and Z. Wang. The model known by this name is based on a cointegration relationship between the earnings yield of the S&P500 and 10 year US government bond yields. The relationship finds support in economic theory. The authors ask themselves if the model can beat the best univariate model in forecasting accuracy. The answer is in the affirmative, and it is shown that a nonlinear ESTAR (Exponential Smooth Transition Autoregressive) model is even better than the linear model. But only on longer forecast horizons. Reliable forecast diagnostics are used to achieve these interesting results.

In the paper: *Structural change as an alternative to long memory in financial time series*, T.L. Lai and H. Xing discuss the relationship between (spurious) persistence and structural breaks, a theme that has interested many authors. They suggest modelling structural breaks using a hidden Markov chain model estimated recursively on the data, demonstrating how the parameters (regression and volatility) have changed over time. No forecasting comparisons are presented to show how this method compares with other methods. The list of references is extensive, but I missed David Hendry’s work, e.g. the books Clements and Hendry (1999 a,b) (both reviewed in IJF 2000 and 2001, respectively) on breaking and cobreaking, close both to the authors’ theme and to the work of the laureates.

*Time series mean level and stochastic volatility modelling by smooth transition autoregressions: a Bayesian approach*, by H.F. Lopes and E. Salazar contains two contributions: introducing LSTAR (logistic smooth transition AR) modelling of the volatility, and applying MCMC (Markov Chain Monte Carlo) methods to derive inference from such models. The level of the series is also modelled as LSTAR. A simulation study reveals promising properties of the models. They are also tested on Canadian Lynx and the S&P500 data, but no empirical comparisons are made with other models.

In the 90s I lectured econometrics to Ph. D students in economics at the Stockholm School of Economics. I would emphasize the need to be sceptical about empirical estimates of elegant theoretical models. In an exercise they were asked to test the so called ‘Taylor rule’ relating interest rates to inflation. They would find that:

(i) If both interest rates and inflation are to be regarded as I(1), then the Taylor rule could at most be regarded as a long-term equilibrium.

(ii) Since the Fed is supposed to react to inflationary chocks and inflation is supposed to be controlled by the interest rate, there must be a two-way relationship between the two variables.

(iii) A third variable, “potential output” is included in the “rule”. It is hard to establish if there is an empirical analogue to this theoretical concept, and different estimates lead to different inference.

I am happy to see that P. Siklos and M.E. Wohar bring all these question marks up to debate in an article that surveys the literature on the Taylor rule, which, as the authors note, is poor in time series analyses studies. *Estimating Taylor-type rules: an unbalanced regression?* is the longest article in the book and what we end up with is that there seems to be a cointegration relationship between interest rates and inflation, that inflation forecasts should be included in the relationship as instrumental variables, that the way ‘potential output’ is constructed has an impact on the relationship, and that the Greenspan era marked a more aggressive anti-inflationary policy than his predecessor’s. In-sample forecasts are shown to substantiate these claims, but no proper out-of-sample tests are provided. Adding forecasts
to the rule is an interesting contribution. But it also adds to the confusion around the ‘rule’. It is well-known that there is little information in two-year-ahead inflation forecasts. But central banks declare that they have a two year perspective when setting interest rates. So it is a mystery that since the 80s the anti-inflationary policies have worked so well. Whereas the ‘Fed’s model’ above has no official sanction, the ‘Taylor rule’ is next to official in central bank lingo.

Bayesian inference on mixture-of-experts for estimation of stochastic volatility, by A. Villigran and G. Huerta addresses the question of combining models (here called ‘experts’) in a dynamic way, using a Bayesian MCMC algorithm. This is a very technical study, which produces somewhat technical results, such as the posterior probability that a combination of ARCH and GARCH models would have explained the returns of a stock market index during a particular period. Forecasting properties of the models, or model combinations are not presented, nor an interpretation of why a certain combination of models would yield the best (or worst) results for a certain period.

Granger made his name with an article, published in 1958, modelling sunspot data. His model is now tested on newer data by G. Yoon, who finds that the model still explains 80% of the sunspot variation. In a brief rejoinder, Granger is critical about his juvenile effort to explain a time series with no theory behind the model. He suspects that the 11 year cycle, found in the data, could be due to different activity intensity in different parts of the sun turned towards earth, as the sun rotates in 11 year cycles. But just imagine: estimating models with a desk top calculator and getting something out of it that is still interesting!

The last paper in the book is titled: A new class of tail-dependent time-series models and its applications in financial time series, and is written by Z. Zhang. The autocorrelation of a time series cannot guide the analyser to an adequate model if the distribution of observations has fat tails and/or behaves differently for positive and negative values. Here (lag-\(k\)) tail-dependence is first defined as a probability limit (observations approaching their supremum) for observations at \(k\) periods apart, both observations being larger than a given value. A Gamma test is suggested for such dependence, which would typically occur after jumps in returns on shares. Max-stable models are combined with Markov switching. The method is applied on the S&P500 time series, which is first made homoscedastic by dividing by the variance estimates of a GARCH(1,1). Tail dependence is found in the series.

Being a Festschrift, this is not a textbook. It is more like a special issue of an academic journal. As such it is worth reading, especially if you are acquainted with nonlinear modelling. But the reader should not expect a text that is easy to digest. It is quite technical, I would say, sometimes too technical. As an economist, I would like to get some explanation, or even speculation of why some nonlinear models seem to work with financial time series, i.e. what kind of behaviour does it reflect among the people who produce the figures? Another criticism is that editors should offer more help to authors whose first language is not English. And typos should have disappeared with automatic language control.

Also, I sincerely hope that authors, writing about themes close to forecasting, would pay more attention to IIF’s principles. It is interesting to learn about new ideas that seem to work on real data, but how do we know if the suggested method is better than some current methods, i.e. are the latter in fact encompassed by the new model, to use David Hendry’s concept? This journal and IIF still have an important mission.

Lars-Erik Öller