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TIME SERIES MODELS FOR EXCHANGE RATE AND AGRICULTURAL PRICE FORECASTS

Discussion

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This is a well written paper that provides a good introduction to a number of topics in time series analysis including the comparatively recent concept of cointegration, which I believe will find increasing application in econometric analysis and in forecasting. The application of the methods to the problem of forecasting agricultural prices and the exploration of a potential role for exchange rates in such a forecasting model set a high standard for applied work.

In these comments I will attempt to put this work into a context that I hope will provide insight into what I see as both its strengths and weaknesses and also highlight its relevance for a group of economists whose primary area of interest lies in problems of decision making in a risky environment. I take as given that in order to make informed decisions one needs to have a concept of the likelihood of possible outcomes of any actions taken and that the proper way to make such a conceptualization is through a probability model. Ideally this means a complete multivariate probability distribution, conditional on all control variables. Furthermore, one role for economists is the specification of such a model. Given the documented biases in subjective probability assessments I think this is a particularly important role.

Unfortunately, the ideal probability representation is unattainable and we are left with the question of how to proceed in the development of a useful one. The paradigm developed by Hodges provides a useful framework for addressing this issue. Hodges uses a three-fold classification of uncertainty: structural uncertainty, uncertainty conditional on structure (risk), and technical uncertainty. The first of these concerns the uncertainty involved in the choice of a particular model. For example the use of a particular set of variables in a linear regression model with i.i.d. normal errors involves uncertainty about whether the conditional mean is indeed linear in the variables, about whether the variables are exogenous, about the normality and i.i.d. assumptions and indeed about whether the conditional mean is even well defined for the problem at hand. In short, there are considerable uncertainties about model specification. Risk, the second category, is something that statisticians and econometricians know quite a bit about. If the assumptions about the structure are correct then it becomes essentially a mathematical or numerical exercise to derive the characteristics of the uncertainty. Technical uncertainty is a bit more

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nebulous, involving the uncertainties that arise through the inability to make exact computations or through having to rely on approximate numerical methods for results.

Hodges points out that any empirical modeling problem involves making trade-offs among these three types of uncertainty. He also argues that, in reporting results, analysts have a tendency to focus on risk, which is comparatively easy to quantify, and that this effectively biases policy decisions towards those that are optimal in a world of greater rather than less certainty.

It is arguable that this is at least implicitly recognized by those writers on strategic decision-making that Vern Eidman discusses when they reject forecasting models and suggest "scenario" analysis as a replacement. While I doubt this was what they had in mind, I think a useful way to think about the use of scenarios, especially if one is willing to attach probability weights to them, is as a choice between structural and both conditional and technical uncertainty. Forecasting models are often very restrictive in terms of the structure they impose but the statistical properties of these models are generally reasonably well-understood, particularly for large samples, and the models are designed to be computationally accurate. Scenario analysis, on the other hand, can be viewed as the selection of discrete points in a very complex probability space and results that are derived using such a model are therefore akin those obtained from numerical integration. The technical uncertainties that arise, therefore, may be great if the set of scenarios is not rich enough. Furthermore it is difficult to know how to use data to improve such models since by nature they deal with random events associated with changing structures.

Bradshaw and Orden have provided a careful study which explores systematically the pros and cons of alternate structural assumptions within a particular class of forecasting models, namely univariate and bivariate autoregression models. The authors have provided a very readable review of these models and discussed the rather recently developed cointegration concept. The authors argue that careful attention to the time series properties exhibited by the data can lead to improved forecasting models. Towards this end they postulate that a number of alternative structures might be able to characterize the price and exchange rate behavior that they examine. These include models expressed in the levels of the variables and in their first differences, for both univariate and bivariate models. They also examine bivariate models that contain a cointegration relationship.

These alternative models are explored in two ways. First, through the use of exploratory data analysis, especially through the examination of auto and partial auto-correlation functions. Second, through diagnostic testing and model selection criteria. These include unit roots tests for integration and cointegration, model selection criteria such as the Schwartz Criteria, and out-of-sample forecasting root mean squared errors. On the basis of their careful and fairly comprehensive

analysis the authors are able to select a model that seems to best capture the behavior exhibited by the data and they come to the interesting conclusion that the inclusion of exchange rates does not improve performance in models for wheat, corn and soybean prices.

I think this study provides a good example of careful econometric modeling. The authors have postulated a class of alternative models and systematically explored which of these models best represents the phenomena that they examine. My main reservation about the paper, however, concerns what I feel to be a lack of sufficient justification for their choice of the alternative structures they examine, though I recognize that one study cannot hope to be comprehensive. A number of questions came to mind on reading the paper. For example, what leads the authors to conclude that an equilibrium relationship should exist between agricultural prices and the exchange rate, and that therefore cointegration is worth examining? Have they adequately examined the possibility of seasonality in prices, particularly when seasonality is a stylized fact in these markets? Why is homoskedasticity be imposed? Is the assumption that the model structure is constant over the sample period justified, particularly given the changes in monetary policy?

The choice of what class of alternative structures is examined depends on a judgment of what structures are likely and must be informed by an understanding of what drives the system being modeled, in this case, grain markets. Answers to these questions require, a least implicitly, some model of the markets examined. Such a model serves to eliminate a vast number of possible structures and allows the analyst to focus on those structures that stand the best chance of yielding useful results. In general, the choice of what class of alternative structures is examined depends on a judgement of what structures are likely and must be informed by an understanding of what drives the system being examined. This is true even when the goal of the modeling exercise is forecasting and is critical if results are to be given economic interpretation. Conversely, the choice of the structures examined reflects the prior information that the analyst is willing to impose.

The lack of what, for want of a better term, I will call structural considerations is also reflected in the distinction the authors make between time series methods and "structural" econometrics. I think this is an unfortunate distinction that is largely the result of the independent historical development of the disciplines of time series analysis and econometrics. It has had the result that the two fields are often viewed as providing alternative, rather than complementary, modeling approaches. It is my conviction that any analyst working with time oriented models of economic phenomena should draw on knowledge of both economic structure and the properties of time series models. In any case, the authors claim that multivariate models represent some sort of middle ground between univariate time series methods and structural models is misleading, especially in that both multivariate and dynamic structural models can be "reduced" to univariate models and vice versa.

Finally, I would like to point out that forecasting models that provide only point estimates are not nearly as useful as those that provide a complete predictive probability distribution. At a minimum a forecaster has the responsibility to provide some assessment of the degree of confidence that can be placed on a point forecast. More usefully a probability representation, even if it is one the arbitrarily assumes normality (or lognormality) and provides mean and variance estimates, would be an improvement over simple point forecasts. This is especially true for analysts and decision makers who must assess the risks faced in their choices among alternative strategies.

References

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