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J. M. Bernardo, J. O. Berger, A. P. Dawid and A. F. M. Smith (Eds.)

Reply to Discussion

Omar Aguilar, Gabriel Huerta, Raquel Prado & Mike West
ISDS, Duke University, Durham NC 27708-0251

We thank the discussants for their kind comments and questions, and the interesting questions raised. We address the contributions by named discussants.

DANIEL PEÑA

Professor Peña contributes some insightful discussion and provides useful additional references and connections to recent literature. We are particularly interested in following up his commentary on factor models and look forward to seeing his new work on the topic with Poncela.

A first set of key questions relate to interpretation of time series decompositions and the use of high-order AR (and TVAR) models when lower order ARMA (and TVARMA) models might be appropriate. A key reason for using high-order TVAR models in cases when TVARMA forms may be suggested is that it is simply more difficult to fit and use the latter classes of models. In applied work, little is lost with high-order AR and TVAR models that adequately represent the data structure, though fitting higher-order models requires adequate sample sizes and/or structured and informative priors. In a context where an MA component (or other non-AR structure) exists – as in the simulations run by Professor Peña – the decomposition methods applied to high-order AR models will naturally identify collections of high-frequency latent components that correspond to the MA components. This is theoretically predicted and we need to be aware of the implications: when such components arise as quasi-periodic ARMA(2,1) processes, as is typical, they do not have physical interpretation as meaningful "cycles" in the series, but simply represent model approximation and noise contributions. Such "noise" components will have very low moduli relative to those of physically meaningful and interpretable components. For example, we repeated Professor Peña's simulations by generating data from the ARMA process $(1 - .6B)y_t = (1 + .2B)a_t$ and then fitting AR(4) models to the synthetic data. Evaluated at approximate MLEs of the AR(4) parameters, most of cases lead to at least a pair of complex roots which do not correspond to any "real" quasi-periodic component in the data. In all such cases, however, the moduli of these complex roots are very low, less than 0.5 and typically in the range of 0.2 to 0.4. In applied work, interpretable quasi-periodic components are typically rather persistent, with moduli higher than 0.8. We discuss these and related issues in Prado and West (1997, section 2), where we explore the question of whether or not the residual noise structure apparent in various EEG series may be adequately described by TVMA components. We do this by fitting high-order TVAR models: conditioning on a selected number of interpretable lower-frequency/higher-modulus latent components, we can then simply "invert" the TVAR operator of the remaining components to infer TVMA structure. This can be informative in practice, in providing insights into underlying and more parsimonious structure. However, we do not see that it will typically provide additional advantages over the use of high-order TVAR models, so long as the resulting interpretation of identified latent components respects the substance of the application and bears in mind the predicted contributions of high-frequency noise components. Additional relevant commentary and practical insights into the issues of interpretation of component structure is best dealt with through case studies, and the discussants are referred to the deeper applied discussions in, for example, West, Prado and Krystal (1998).

A second key point relates to the question of dimension reduction in multivariate time series decompositions, and the connections with latent factor models as studied by Peña and others. As mentioned in the paper and discussion, the decomposition theory extends to general multivariate DLMs, and time-varying parameter vector autoregressions are a key special case. Our theoretical work on this topic is new, and as yet detailed only in the unpublished PhD thesis of the third author (Prado 1998, chapter 6; the thesis is available at the ISDS web site `http://www.stat.duke.edu`). We are excited about the potential for this theory to open up connections with factor models generally and to provide new ways to address the issues of model parametrisation, parameter redundancy and dimension reduction, though have, as yet, little to offer beyond optimisation! In addition to the cointegration and factor model connections mentioned by Professor Peña, note that the developments of scalar component analyses of Tiao and Tsay (1989) are also pertinent, as noted in our paper.

The final key point raised by Professor Peña has to do with dealing with nonstationarities in time series and in latent processes in multivariate models. This is certainly an interesting area. In the exchange rate study we are dealing with traditional returns, rather than actual rates, so that nonstationary components are unwarranted. More broadly, if trends are apparent or expected we typically prefer to model them via explicit components of an overall model, as a matter of general principle. This is exemplified in the treatment of the oxygen isotope series, where the model includes an explicit first-order polynomial trend; similar treatments are found in other related examples in some of our works in the references. This is in contrast to the view that nonstationarities are allowed through unit or explosive roots in ARMA or TVARMA models. That said, our work with priors on AR roots directly allows unit roots, and the general methodology of TVAR modelling does not restrict to stationarity at all, so that extensions of this work to multivariate contexts will allow nonstationarities. In this connection, we are eager to hear more about Professor Peña's approaches to model identification with nonstationary factors – the specification problem in factor models is a most pressing and challenging issue.

ROBERT KOHN

Professor Kohn raises several issues. His first concern is the treatment of AR models and our priors on root parameters. He asks about the practical benefits of prior modelling focussed on the roots as primary parameters, rather than the partial autocorrelations or standard linear AR parameters. To some extent the choice of prior focus is a matter of personal taste and experience. Having developed significant applied experience in use and interpretation of decompositions, the former approach is simply natural for some of us as the latent components, and their defining parameters, *are* the AR model. Once one thinks most readily about a model class in terms of a specific parametrisation, that is likely the preferred parametrisation for prior modelling. In considering a "new" application in the natural or biomedical sciences, for example, we think about the likely forms of behaviour evidenced in the data – often a small number of key, dominant components together with several higher frequency components (see also the comments in reply to Professor Peña above) – and then prior specification is natural on the root parameters. By comparison, translating this substantive and qualitative prior information into priors on the partial autocorrelations or linear AR parameters is complicated and much less direct. From a more mechanical viewpoint, dealing with the issue of unit moduli on either real or complex roots is a benefit, and we simply disagree with Professor Kohn that spectral methods might be used instead. In fact, one of our main motivations originally was to introduce priors allowing unit roots in order to properly account for potential spikes in spectra, partly in response to the literature reporting difficulties in identifying close spectral peaks and spikes using AR models (the problem lies in standard methods of fitting, not in the models per se, as discussed in Huerta and West, 1997b). As favoured by the discussant and his collaborators, one might consider partially nonparametric Bayesian methods in the spectral domain, though combining such models with time domain trends, dynamic regressions and other components is rather unappealing and evidently quite challenging.

Embedded in Professor Kohn's discussion is the comment that the latent component processes delivered by the AR and TVAR decompositions are correlated, as they are "driven" by individual innovations that are multiples of that in the overall TVAR model. This issue was also raised by Professor Neil Shephard in the floor discussion at the conference. The issue of interpretability of components in light of such dependence does not overly worry us – see the discussions in applications in West, Prado and Krystal (1998), for example. Nevertheless, there are cases in which extended models with component-specific innovations might be desired. Some synthesis of the "individual component" models of West (1995,1996) and higher-order AR/TVAR models therefore seem to be of interest, and are likely worth exploring.

A further point raised concerns time-varying parameter models and the idea of building random walk, or other evolution models for the root parameters directly, rather than using the "standard" evolution models on the linear autoregressive parameters. This is an interesting idea, and one that has been anticipated in recent conversations among the authors. Whether or not this provides any real advantage or additional mileage in practice remains to be seen. Such developments will be technically quite complicated relative to the standard analysis framework, and appear infeasible without MCMC methods and the associated computational paraphernalia. Prior to embarking on this it seems desirable to first gain a deeper understanding of the nature of the time evolution *implied* for the root parameters based on the usual, analytically tractable random walk models, and perhaps others, for the usual "linear" AR/TVAR parameters.

The final point raised by Professor Kohn is a general exhortation to routinely explore model fit and adequacy. Of course we concur and, though we do not typically derive much comfort from single numerical measures of goodness-of-fit, our applied work does routinely involve ranges of standard, and not-so-standard, diagnostics. Some of the discussion of residual analyses, and issues raised in developing posterior residual analyses in an MCMC context, are neatly exemplified in Huerta and West (1997b), and, in a rather non-standard multivariate time series model, in Aguilar and West (1998b) and West and Aguilar (1997), for example. Additional material related to diagnostics and model assessment in time series appears in Harrison and West (1991) and West and Harrison (1997, chapters 10 and 11).

SIDDHARTHA CHIB AND NEIL SHEPHARD

Professors Chib and Shephard question the use of explicit unit roots in AR/TVAR models. The question is related to the econometric debate over the last decade and more concerning "trend stationarity" vs "difference stationarity" of time series, and the practical issues of interpretation of model components as the roots move from the stationary to nonstationary regions. As mentioned above in reply to Professor Peña, we generally prefer to model trends in terms of explicit model components ("in the Harrison tradition") as exemplified in the oxygen isotope analysis of the paper and elsewhere (e.g., in other examples in West 1995, 1996). To some extent, the prior masses on unity for *real* roots represent theoretical completion and perhaps undetected polynomial trends but usually are of much lesser practical interest than the prior masses on unity for the moduli of complex roots. As mentioned in reply to Professor Kohn, a key initial motivation for these new structured priors was our interest in addressing and resolving the problems encounter in AR spectral estimation in problems with close peaks or spikes in spectra. As fully discussed in Huerta and West (1997b), allowing unit moduli for complex roots overcomes supposed problems in spectral estimation with autoregressive models using standard methods of estimation and in Bayesian analyses under standard priors.

CLAUS DETHLEFSEN AND SOREN LUNDBYE-CHRISTENSEN

Professors Dethlefsen and Lundbye-Christensen raise questions about hyperparameter estimation in dynamic models, and about model assessment and diagnostics. On the latter point, the authors comments suggest a lack of awareness of a large body of existing methods for model checking, assessment and testing – using both standard Bayesian methods and additional techniques tailored to dynamic models. See the discussion and references from Professor Kohn above, as well as our reply to Professor Kohn. For complex, multivariate, non-Gaussian models in longitudinal studies, in particular, the discussants' ideas relate to techniques illustrated in Aguilar and West (1998b) and West and Aguilar (1997).

On hyperparameter estimation, we see little need for EM type approaches as MCMC methods for hyperparameter estimation in various classes of DLMs have been around for some time now. Inference on variance components and parameters in system evolution matrices were among the first such problems to be dealt with – see West (1995, from an early technical report in 1993, 1996, 1997a and b). Much of this builds on the foundational work of authors including Carlin, Polson and Stoffer (1992), Carter and Kohn (1994), Fruhwirth-Schnatter (1994), and de Jong and Shephard (1995), on MCMC methods in various dynamic models, and recent years have seen the growth of a large and still burgeoning literature. As a result, and now for some time, combined inference on evolving state variables and model hyperparameters has been standard in many contexts; see West and Harrison (1997, chapter 16) for further discussion and references. In the context of sequential analysis, however, there are very challenging problems of sequentially updating inferences on combined state variables and parameters, as mentioned in the concluding discussion to the paper.

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