Reply to the discussion of "Non-Gaussian OU based models and some of their uses in financial economics," read to the Royal Statistical Society on 18th October, 2000. To be published in Journal of the Royal Statistical Society, B, 63, in 2001.

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We would like to thank all the contributors to the discussion of our paper. A number of the comments have certainly advanced our understanding of OU processes and stochastic volatility. We have structured our reply by topic, going through alternative models, inference, Lévy processes, option pricing and other issues.

Alternative models A number of discussants have pointed clearly to alternative models which share features, such as second order properties, with our OU based volatility models. We mentioned in our paper some diffusion based alternatives and these are highlighted in the comments by Valentine Genon-Catalot and Catherine Larédo; Eric Renault; Nour Meddahi. These diffusion alternatives are generally non-linear processes with Gaussian increments, with the non-linearity forcing the process to be positive. Our approach is to advocate linear processes with non-Gaussian increments for volatility. Although diffusions have many advantages, only in the CIR case (to our knowledge) is it possible to easily analytically study the cumulant functional of $x^*(t), \sigma^{2*}(t)|\sigma^2(0)$. This is the vital issue in option pricing theory. We think our models open up a new class of analytic option pricing models. This is studied, following our initial work, by Nicolato and Venardos (2000) and Tompkins and Hubalek (2000).

Eric Renault points out the work of Andersen on discrete time autoregressive volatility models. It is clear we should have referenced this important and related work. Of course moving to continuous time does change the model structure very considerably as time aggregation means discrete time increments to integrated volatility do not have an autoregressive structure (although instantaneous volatility does). This point is made forcefully in the work by Meddahi and Renault quoted above. Professor Renault worries that our OU based model does not allow the conditional variance of volatility to be proportional to the conditional mean. This fear is shared by Nour Meddahi. However, Figure 6 shows this is actually the case when one conditions on returns, rather than on the unobserved instantaneous volatility.

Peter Brockwell and Richard Davis make an interesting contribution, introducing ARMA type Lévy based continuous time volatility models. They give conditions on the volatility process so that it is positive. We look forward to thinking about this process in detail. In a sense their comment has answered one of the queries of Maurice Priestley. The other point that Professor Priestley makes is that we should compare the fit of our model to alternative non-linear diffusion based models. This is surely right, although statistical fit is only one criteria for use. Another, equally important one, is that of tractability.

Sir David Cox makes an important point, that we are using a parameter driven model (Cox (1981)) and so are not really explaining volatility in terms of past data. Instead he suggests an observation driven model, derived via a Taylor expansion from a general non-linear autoregression. The resulting model is ARCH like. Such models are indeed appealing, although the properties of observation driven models are often hard to discern. Further, they are often difficult to manipulate when it comes to option pricing theory.

Frank Diebold makes some interesting comments about the marginal distribution of increments to integrated volatility. He argues that his work on realised volatility suggests it is close to lognormal. The lognormal (LN) distribution is self-decomposable (Bondesson (1992, p. 30 and pp. 59-60); see also Thorin (1977)) and so we could setup a LN-OU process. LN-OU processes have substantially heavier tails than IG-OU processes, which has some attractions in the context of equity data. We are currently working out the detailed implications of the LN-OU and hope to report on it in the future. Finally, while IG-OU processes do not temporally aggregate to being inverse Gaussian, calculations suggest the disagreement is mild (see Barndorff-Nielsen and Shephard (2001a)). We do not yet know if this is true for LN-OU processes.

Clive Granger points out that the non-normality in our models is built out of a normal distribution. This is true, but the flexibility that is achieved with normal variance/mean mixtures (or put another way, with subordination of Brownian motion with drift) is extraordinary — allowing us to deal with, for example, the double exponential distribution favoured by Professor Granger in some of his recent writing. We agree that our linkage with trade-by-trade dynamics is primitive and much work needs to be carried out in this context. Finally, we share his concern about the role of hypothesis testing based upon hugh datasets.

Benoit Mandelbrot dismisses our models as being extremely complicated. We will leave it to the reader to decide if our linear volatility models are more complicated than Professor Mandelbrot's favoured multifractal processes.

Inference Gareth Roberts and Omiros Papasiliopoulos productively focused on the Γ-OU volatility case, reparameterising the model into jump times and jump sizes. This approach is also independently introduced by Sylvia Fruhwirth-Schnatter. All three of these researchers then design MCMC algorithms to sample parameters, jump sizes and times given the returns. This can, of course, be carried out in a number of ways, with varying degrees of effectiveness. The discussion studies carefully a number of approaches. This is clearly an important and productive technique which is, in principle, extendable to the superposition and multivariate cases. Further, the method works with any OU process which has a BDLP with an integrable Lévy density, for such BDLPs all correspond to compound Poisson processes. This is a wide class of processes. However, it does not include cases, such as the IG-OU, which do not have integrable Lévy density, which means the BDLP has an infinite number of jumps in any finite interval of time, and so some adaption of the above procedure would be needed.

Professors Griffin and Steel implement an MCMC algorithm via the series representation in the Γ-OU case. We found this very interesting and would hope they would report their results more extensively elsewhere. The comment of Mike Pitt and Stephen Walker was innovative. They suggested a simulation based approach to estimating the likelihood function for the SV model in the Γ-OU case. This is based on a smooth particle filter which Mike Pitt has been developing. At the moment we do not understand how this approach can be used in cases where the density of $\sigma_n^2, z(n\lambda\Delta)|\sigma_{n-1}^2, z((n-1)\lambda\Delta)$ is unknown (which is the case typically). We hope that Pitt and Walker will report this at some length elsewhere. Certainly their comments greatly interested us.

Petros Dellaportas, Emma McCoy and David Stephens have been studying long memory models by the superposition of discrete time AR(1) models. These can then be handled by MCMC algorithms. This approach to long memory is certainly worthy of study. They asked us about the utility of the continuous time modelling. This does raise the mathematical difficulty of working in this area, but the choice of Δ is basically in the hands of the econometrician nowadays as prices are mostly recorded in continuous time. Hence basing the analysis in continuous time seems suitable. Further, one of our wishes is to carry out option pricing off these models, which is most easily achieved via continuous time.

Both the above discussants and Enrique Sentana and Frank Critchley asked us about the identification of the superposition of OU processes. It is helpful in thinking about this issue to work with the $IG(\delta,\gamma)$-OU case, with

$$\sigma^2(t)=\sum_{j=1}^{m}\sigma_j^2(t), \qquad \text{where} \qquad \sigma_j^2(t)\sim IG(\delta w_j,\gamma)\text{-OU},$$

where the weights $\{w_j\}$ are strictly positive and sum to one, while the corresponding damping values are $\{\lambda_j\}$. In order to gain statistical identification it is necessary to order either the weights or the damping factors. Under such a setup the mean, variance and autocorrelation function identifies all the parameters in the model and hence this model can be estimated from data. It is this structure we have recently been using in Barndorff-Nielsen and Shephard (2000) to estimate these model in practice.

Valentine Genon-Catalot and Catherine Larédo express their disappointment that we did not manage to estimate these models off non-second order information. We share their concern and hope that progress can be made in this area. Our recent work on realised volatility is aimed at improving matters, but there is clearly still much to be carried out.

Enrique Sentana makes a series of points about the statistical basis of our estimation methods. They are well taken and clearly some more work needs to be made in this direction. We have formalised some of these ideas in Barndorff-Nielsen and Shephard (2000). Certainly indirect inference methods may be useful in this context, particularly as GARCH or QARCH based models seem such obvious auxiliary models in this context.

Bent Jesper Christensen asks us about our leverage model, where he argues for a more traditional log-volatility model with changes in the log-price appearing in the volatility process. Although this model has much merit, it does remove the linear structure of the process and so it becomes much less mathematically tractable. Although Professor Christensen is of course correct about the causal story he tells, in terms of observables the two models can produce very similar effects.

David Hobson asks if we can introduce a leverage effect which allows us to maintain the property that log-prices have continuous sample paths. This would clearly be desirable from a mathematical finance viewpoint. The issues are clearest when $z(t)$ is a compound Poisson process and $\mu=\beta=0$. Then our model has

$$x^*(t)=\int_0^t\sigma(s)\mathrm{d}w(s)+\rho\sum_{j=1}^{N(t)}z_j.$$

We may 'smooth' this by modifying to

$$x^*(t)=\int_0^t\sigma(s)\mathrm{d}w(s)+\rho\sum_{j=1}^{N_t}z_jh(t-\tau_j)$$

where τ_j is the j-th arrival time of the Poisson process $N(t)$ and h is a nonnegative continuous function such that $h(s)=0$ for $s\le 0$, $h(s)>0$ for $s>0$ and $h(s)\to 1$ for $s\to\infty$. That is, we have a shot noise type behaviour.

Lévy processes Nick Bingham makes a series of interesting points about Lévy processes. His work with Rudiger Kiesel certainly sounds interesting and we look forward to reading it. Multivariate modelling is challenging and stimulating. His point about quadratic variation is of course true, however we have recently been studying a finite sample version of it in Barndorff-Nielsen and Shephard (2000). The motivation for it is in dealing with intra-day data.

Like Professor Bingham, Professors Benth, Karlsen and Reikvam make very interesting points about multivariate models. Our paper has only scratched the surface on this topic. We know from informal discussions with Professor Benth that he has been thinking about portfolio theory in the context of our models, where the investor is faced with transaction costs. We look forward to being able to read about this work when it is completed. Professors Christensen, Lawrence and Sentana's comments accords with our view that this is a vital topic.

Jan Rosiński's new result on series expansion is highly interesting to us for it removes the requirement to compute the inverse tail mass of the Lévy measure for many problems. In particular it covers the IG case. We have been using this result in Barndorff-Nielsen and Shephard (2001b).

Ken-iti Sato makes a number of points of historical worth, while his new result on selfdecomposability and subordination of Brownian motion with drift and work extending subordination to the multivariate case are of particular importance.

Option pricing Elisa Nicolato and Manos Venardos briefly discuss their work on option pricing for our SV models. This shows that the linear structure of the model means that analytic option pricing results can be found for a wide class of distributions. In particular their result on the leverage case is particularly welcome. This relates also to Robert Tompkins who discusses various estimation methods for these models via option data. This may allow us to have a better understanding of the choice of equivalent martingale measure (EMM).

Stewart Hodges' wide ranging discussion puts our work in context, and we thank him for this. His comments about our choice of EMM is of course correct. We hope that we will eventually be able to understand the choice of EMM within the context of the choice of utility function. Work along these lines is being carried out by Professor Benth and coauthors at University of Oslo. We think this type of research is really important. Finally, Professor Hodges makes some interesting links with the implied process models which have recently been used in the finance literature. It is surely the case that we need stronger links to that approach.

Mark Davis discusses various areas where the option pricing theory based on our model could be used. He argues that these models have their largest potential in value at risk type calculations. This may well be true, although we have yet to really study these fields in any depth. However, his wise words are surely helpful in guiding us.

Howell Tong and Hailiang Yang emphasise the importance of the Esscher transformation for option pricing. This is a very convenient tool. However, from an economic viewpoint its choice seems somewhat arbitrary. As we mentioned above, theory based on utility functions would seem a rather sounder object. We hope such methods will be developed for our models.

Other issues Stephen Taylor asks about the intra-day seasonal component of volatility. His points are, of course, correct and more sophisticated modelling would allow the various effects he discussed to be taken into account. It is clear that the paper by Taylor and Xu (1997) is of importance in this field.

Frank Critchley asks a number of questions about the estimation of our models. In particular he desires a more formal cross-validation approach to breaking the dataset into pieces. Our hope in carrying this out in a simple way was to see if the model was reasonably stable over time. At the moment our main effort is to think about design effective estimation methods, while we hope we will be able to return to issues of outliers and inliers later.

Jens Ledet Jensen wonders if the use of hidden Markov models (HMMs) may not give a simple model structure for these types of problems. In some senses this is true, however in terms of the properties of integrated volatility our models are quite simple compared to HMMs. It is certainly the case that a slowly moving trend model of the type he suggests may give a good description of this type of data, however mean reversion in volatility is now a standard assumption following a number of years of rigourous empirical testing.

Chris Jones asks us why our volatility models are not of the type

$$\mathrm{d}x^*(t) = \{\mu + \beta\sigma(t)\}\,\mathrm{d}t + \sigma(t)\mathrm{d}w(t).$$

It is certainly the case that economic theory does not tell us that the risk premium (which relates the mean to the variance) should be of the form we use $\mu + \beta\sigma^2(t)$, rather than the one he favours. Our choice was based on mathematical tractability and, more importantly, on the fact that our model structure can alternatively be viewed as being obtained by subordinating Brownian motion with drift by a generalised subordinator — integrated volatility.

It is a great pleasure that Professor Lawrence made a comment to our paper, as it gives us the opportunity to correct an oversight in not quoting his important research on autoregressive models with non-negative errors. This is clearly related to our continuous time work. His paper Lawrence and Lewis (1985) is a good starting point to read about this work.

Anthony Ledford discusses the extremal behaviour of returns for our SV models. This is an important topic, but it is clear that the tail index of returns y_n is immediately inherited from the tail index of $\sigma^2(t)$. This is one of the advantages of these types of models over discrete time ARCH type models where these issues are much more involved.

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