The importance of Intermittency and Multifractality in the climate:

comments on "Late quaternary temperature variability described as abrupt transitions on a 1/*f* noise background" (M. Rypdal and K. Rypdal)

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Back in the 1960's, before the revolution in scaling associated with fractals and multifractals, little was known about scaling processes and there were few scaling models. Back then, the origin of scaling as a scale invariance symmetry principle was not fully recognized and the vague term "1/f noise" was commonly used to characterize virtually all scaling processes irrespective of their actual scaling exponents. Today the situation is totally different: since 1983 it has been clear that the general scaling framework is multifractals which involve a infinite hierarchy of exponents. This fact is reflected in a now massive literature not only in statistical physics and turbulence, but also in nonlinear geophysics. If the only problem with this paper was the antiquated jargon, this could be easily remedied. However, the 1960's jargon faithfully reflects the authors' outdated theoretical framework which is devoid of any reference to the huge advances in the subject over the last thirty or forty years.

Although it is never mentioned, their paper's entire framework is quasi-Gaussian and this explains the authors' first two sentences:

> "The temporal variations in Earth's surface temperature are well described as *scaling* on an extended range of time scales. In this parsimonious characterisation, a single parameter specifies how the fluctuation levels on the different time scales are related to each other."

Since general scaling processes are characterized by convex exponent *functions*, on the contrary they generally require an *infinite number* of parameters. The reduction to a single exponent is not just "parsimonious", but it is also highly simplistic. There is indeed a whole literature about how to reduce this infinite hierarchy to a finite and hence manageable number. In my opinion – but there there are others - the most promising is to exploit a kind of multiplicative version of the central limit theorem that leads to universal multifractals [*Schertzer and Lovejoy*, 1987] (at last count this paper had received 961 citations) and this leads to a reduction to three parameters (but not one!). Indeed, since the exponent function is convex, it is obvious that at least three parameters are needed (see [*Schertzer et al*., 2013] for further discussion of this precise point).

The quasi-Gaussian framework is again implicitly invoked in the third sentence where the spectrum is related to the fluctuation exponent H by the equation $\beta = 2H\text{-}1$: here the (multifractal) "intermittency correction" is missing. It is interestingly to recall that the first models of intermittency corrections were developed in the turbulence literature by [*Kolmogorov*, 1962], [*Yaglom*, 1966] and [*Mandelbrot*, 1974], they were the precursors to the discovery of the general multifractal framework in the 1980's: the authors' intermittency-free theoretical framework is thus pre-1970!

But the third sentence belies another theoretical slippage: it uses the exponent H in a nonstandard way. In the turbulence literature, H directly characterizes the fluctuations, but in the authors' usage, it instead applies it to the integral of the series i.e. it characterizes the *fluctuations in the integral*, not in fluctuations the process itself, it is therefore larger by 1 than the standard fluctuation exponent H. Calling this H_I, we have the authors' $H = H_I$ whereas the turbulent H for the process is H_p, we have $H_I = H_p+1$. With the authors' definition, the celebrated Kolmogorov law would be a 4/3 rather than a 1/3 law, the Corrsin-Obhukhov law would also be a 4/3 law and the Bolgiano-Obhukhov law would be an 8/5 rather than a 3/5 law. Presumably, the only reason for this confusing usage is the popularity of the Detrended Fluctuation Analysis technique - that works by analysing the integral of a series rather than the series itself - which has needlessly propagated the confusion.

A slightly different problem arises in equation 1. In fact - even in the authors' restrictive monofractal (quasi-Gaussian) framework - there are two problems. First at best, the relation could hold for a quasi-Gaussian process with mean zero i.e. if the variance was replaced by the standard deviation. If the authors' do this, then their definition is the same as the "Aggregated Standard Deviation (ASD) technique [*Koutsoyiannis and Montanari*, 2007], later re-baptised the "Climatactogram" and criticized in [*Lovejoy and Schertzer*, 2013] ch. 5 and in [*Lovejoy et al.*, 2013]. With this modification, the definition would still fail for any quasi-Gaussian process with $\beta>1$. Using the usual fluctuation exponent H_p, this is actually quite obvious because (still neglecting intermittency corrections), we have $\beta = 1+2H_p$ so that $\beta>1$ implies $H_p>0$ so that fluctuation grow with scale. Obviously, however the standard deviation of a temperature averaged over a length Δt (eq. 1 when the variance is corrected to read "standard deviation") can only *decrease* with averaging scale, so that there is a clear contradiction.

The situation can be easily cleared up using wavelets, see [*Lovejoy and Schertzer*, 2012]: when $\beta>1$ ($H_p>0$), the wavelet corresponding to the authors' definition (called the "tendency fluctuation in [*Lovejoy and Schertzer*, 2012], although a better term is "anomaly fluctuation") has inadequate low frequency spectral resolution and the corresponding fluctuations end up being spuriously dominated by the lowest frequencies present in the sample, they no longer reflect the scaling of the system at frequency Δt^{-1}.

Many of the above comments were recently made in comments on a recent ESD publication by the same authors [*Lovejoy*, 2015] so that it is frustrating that the authors still do not even mention the last 30- 40 years of developments in scaling. However, this would be a minor issue if it didn't impinge directly and quite fundamentally on their results. Specifically, by adopting the quasi-Gaussian framework, events such as Dansgaard-Oeschger events must indeed be considered as outliers and analysed in the subjective manner proposed here where extreme shifts are essentially removed from the analysis by subjectively cutting the data into more homogenous segments. (Incidentally, the authors' fig. 2b nicely shows that this subjective cutting out of sections simply breaks the scaling; the black squares are not convincingly linear as the authors imply, take a close look!). However – as explicitly suggested in [*Lovejoy and Schertzer*, 2013] ch. 5 – and reiterated with considerable detail in comments on the authors' previous publication [*Lovejoy*, 2015] - if we drop the restrictive monofractal hypothesis and allow the process to be multifractal then the Dansgaard-Oeschger events may be on the contrary be *expected* as necessary manifestations of the intermittency (multifractality)!

(Multifractality is in accord with several papers going back to [*Schmitt et al.*, 1995], but especially in accord with the long tails displayed in temperature probability distributions (going back to [*Lovejoy and Schertzer*, 1986]) including those of GRIP paleotemperatures that are shown in [*Lovejoy and Schertzer*, 2013] and reproduced in [*Lovejoy*, 2015]). As indicated, the authors are necessarily aware of these results due the explicit discussion of them in [*Lovejoy*, 2015] (unfortunately, the legend of the corresponding fig. 2a in [*Lovejoy*, 2015] didn't mention that the power law tails on the GRIP probability distributions were only present when *non* interpolated series were used, see [*Lovejoy and Schertzer*, 2013] ch. 5 for the full details).

The authors may have scientific disagreements with various points raised above, but I simply don't understand why they systematically refuse to even acknowledge the existence of intermittency or the now thirty year old multifractal framework and with it, the results of many other workers with their claims of the pertinence of intermittency for climate and paleoclimate analyses. Science simply cannot progress in this way.

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