More comments on: "Late Quaternary temperature variability described as abrupt transitions on a 1/f noise background"
Martin Rypdal and Kristoffer Rypdal

General comments:

The key disagreement concerns the issue of multifractality and my previous critique was completely focused on this – it was not a "diversion… from the actual issues" as the authors' now claim in their response. In the original paper numerous statements were made that were only true for monofractal processes and this fact was never acknowledged. To make things worse, the (now numerous) claims of multifractality in the climate were not at all acknowledged so that one could easily get the impression that the authors were living in the early 1960's before even the general existence of intermittency corrections in scaling processes were well known. With the discovery of multifractals in the 1980's, the archaic terminology "1/f noise" has been almost universally abandonned due it's unnecessary vagueness so that it's presence in the title of the paper was not encouraging.

The key point of the paper was the scaling analysis of ice cores. This analysis assumed *a priori* that the evident "bursts" were not symptoms of intermittency – as others had claimed - but rather that it was simply a symptom of a fundamentally nonintermittent process that was punctuated by a qualitatively different process. Since the authors were convinced that the correct framework was monofractal (quasi-Gaussian) the bursts could only be considered outliers, subjectively culled so that and each burst or jump-free section could be analysed separately. Unsurprisingly, these series selected for lack of symptoms of intermittency were indeed less multifractal.

But irrespective of the true nature of the scaling, it was frustrating that the issues were never discussed nor the existence of alternative (scaling) hypotheses ever mentioned. In the new version, at least the alternative hypothesis on DO events is acknowledged, but it is rapidly dismissed by spurious handwaving arguments from lines 45-55. They are variants of the speculation (hope?) that "Surely a multifractal model will give rise to bursts, but nothing that looks like DO events" (cited from the response).

In spite of the numerous precise comments calling for systematic monofractal/quasi-Guassian qualifiers on numerous statements, the authors have maintained most of their monofractal approach (dressed up in "1/f" jargon so that the monfractal restriction isn't obvious), and they are apparently confident that their new figure 4 demonstrates once and for all that all the claims of multifractality in the climate are wrong.

Before turning to the key analysis in section 2.4, let me first enumerate several reasons why – notwithstanding the claim in their response that they are familiar with mulitfractals – that we may lack confidence in their analyses. Consider the following oddities:

1) The wavelet coefficient $W(t,\Delta t)$ (eq.2) is called the "wavelet fluctuation function". This is bound to lead to confusion since it is has a different normalization than the usual fluctuations defined by

wavelets (such as differences, anomalies or Haar fluctuations). The usual fluctuations are $W(t,\Delta t)\ \Delta t^{-1/2}$, see below.

Of course one can define things any way one wants, but this is certainly confusing.

2) The authors themselves may have been confused on this point since they claim that $< W(t,\Delta t)^q>$ is a q^{th} order structure function whereas the q^{th} order structure function used throughout the turbulence and multifractal literature is different, it is: $\Delta t^{-q/2}< W(t,\Delta t)^q>$. Therefore the usual structure function exponent, in the usual notation is $\xi(q) = \tau(q) - q/2$ (this uses the nonstandard $\tau(q)$ defined by the authors in line 190, see the next point).

3) The above $\tau(q)$ is however also highly confusing, because it is the same notation that is invariably used in the dimension formalism of multifractals for the "partition function exponent", i.e. $\tau(q)$ usually means something totally different. For comparison, the general relation between the authors' $\tau_a(q)$ and the usual $\tau_u(q)$ is:

$$\tau_u(q) = q(d-1/2+H)+\tau_a(q)-1$$

here, d is the dimension of space (for time series, d=1) and H is the fluctuation exponent (i.e. $\xi(1)$).

4) In the authors' response (and echoed in the new version of the paper) one finds the incredible statement: "The reviewer seems to forget that multifractality is not a generalization of second-order statistics. On the contrary, multifractality is infinitely more restrictive, since it requires that structure functions of all orders are power-laws, while scaling in second-order statistics only requires that the second-order structure function is a power law."

Let us look at this more closely. In as far as it goes, the first sentence is correct: multifractality does not seek to generalize second order statistics, ordinary q^{th} order statistical moments are enough for that. But what if we restrict ourselves to scaling processes (i.e. those that possess a scale symmetry under zooms)? In this case, the generalization of second order statistics of the fluctuations is the q^{th} order structure function (generalized from the more usual second order one), and for scaling processes, this is characterized by the exponent $\xi(q)$.

(It isn't clear, but the authors seem to think that there exist processes that have perfect second order scaling, but for which no other moments are scaling?! If so, please give an example of such a process, or explain how it might even be possible!).

Now recall that the usual nonintermittent Gaussian and quasi-Gaussian processes - precisely those discussed by the authors (that they term "self-similar", line 192) - have a perfectly linear $\xi(q)$

function. However, it is not hard to show that for any scaling function, $\xi(q)$ need only be convex. Since a straight line is a special case of a convex function, it is on the contrary *their* assumptions that are "infinitely more restrictive" than the assumption of multifractality! This is precisely why multifractals processes are the general scaling processes. This generality has been universally recognized ever since its discovery!

Look at it another way: as we see below, the entire section 2.4 falls down precisely because it is based on the unsubstantiated claim that an empirical set of points can be proven to be exactly linear, i.e. that one can reject the hypothesis that on the contrary that they lie on a concave curve. Unfortunately, all that science and statistics can do is to put empirical bounds on the degree of concavity, i.e. on the degree of multifractality!

Section 2.4: the empirical analyses

Given the number of independent authors and papers that have claimed multifractality – and given the authors nonstandard definitions and notations - it would be surprising if they could invalidate this now substantial body of work. This scepticism is bolstered because rather than actually looking at the evidence upon which the claims of multifractality have been made – and the data analysis techniques that were used to obtain them - the authors work from scratch using their modified structure function.

I myself was intrigued by the authors results, so I took a closer look. If the reader follows the zoom into the key part of fig. 4, the problem will become clear: the authors' results turn out to be fully compatible with the existing multifractal claims, but the method that they used was simply too blunt to detect it!

Indeed this zoom sequence is sufficiently clear that if the authors do ultimately publish figure 4, I will not hesitate to use it as a pedagogical device to teach the "do's" and "don'ts" of multifractal analysis!

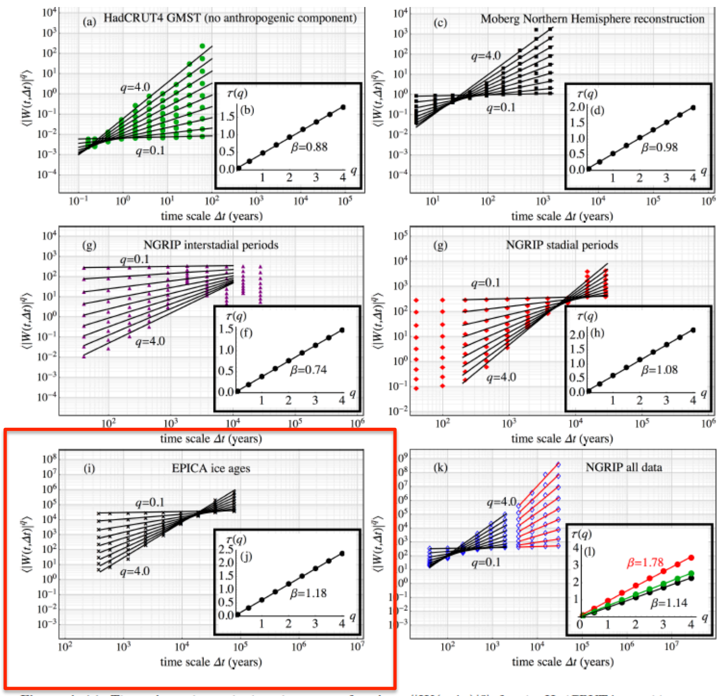

Figure 4. (a): The estimated wavelet-based structure functions $\langle |W(t,\Delta t)|^q \rangle$ for the HadCRUT4 monthly global mean surface temperature where the anthropogenic component has been removed using a linear-response model. The lines show the fitted power-law functions $c_q \Delta t^{\tau(q)}$. The q-values are $q = 0.1, 1.0, 1.5, \ldots, 4.0$. (b) The scaling function $\tau(q)$ obtained from the fitted power-laws in (a). The line is a linear fit to the estimated

Fig. 1a: This is the first of a zoom sequence into the authors' new figure 4 purporting to demonstrate empirically that climate data are not multifractal. The red box indicates the area of the zoom: the analysis of the EPICA temperature proxy data (we will not bother to repeat such zooms on the other subfigures).

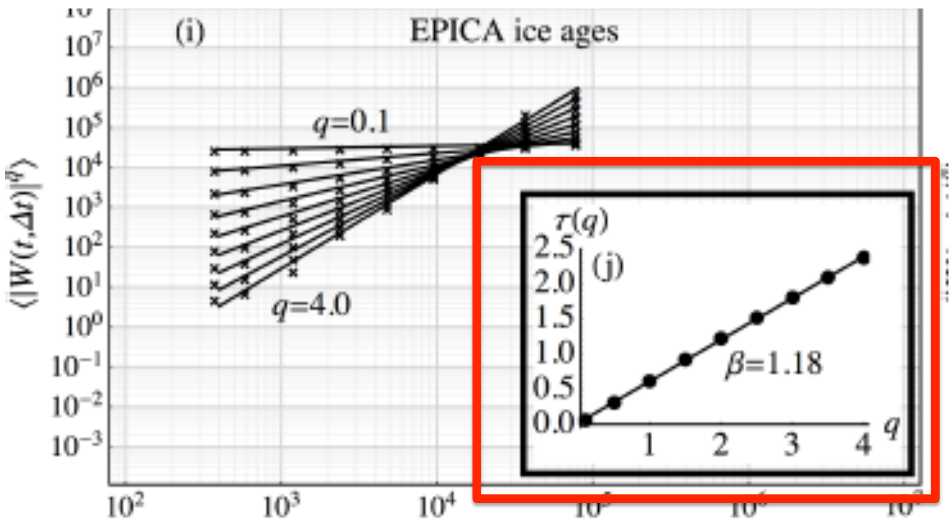

Fig. 1b: After this first zoom, we see the critical exponent scaling function graph. Although the notation, $\tau(q)$ is apparently borrowed from the dimension multifractal formalism, this is *not* the usual partition function exponent (see the text for the complex relation between the two). Nor – in spite of the claim - is it a usual structure function exponent (denoted $\xi(q)$ in the turbulence and multifractal literature). The standard structure function exponent is related to the authors' exponent via: $\tau(q) = \xi(q)+q/2$.

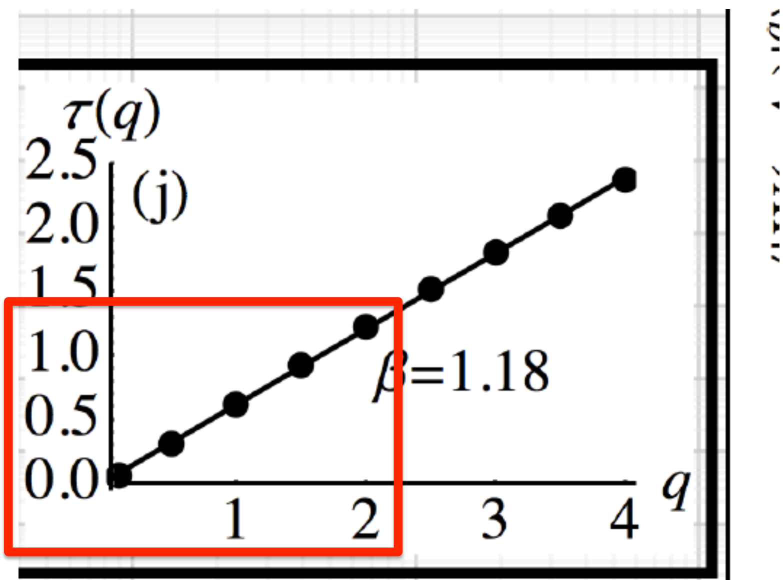

Fig. 1c: Finally, in our second zooming into the authors' τ(q) function, we begin to (barely) discern the problem. First, we avoid the higher *q* values since the scaling can be spurious due to multifractal phase transitions, so that the range 0<*q*<2 is (usually) the most reliable (the phase transitions are consequences of the high order moments being dominated by single extreme empirical values). Second, the multifractality, manifests itself in a (slight) concavity of the points. A symptom of this – barely discernable at this low magnification and with the massive circles used in the graph – is the failure of the regression line to pass exactly through the origin.

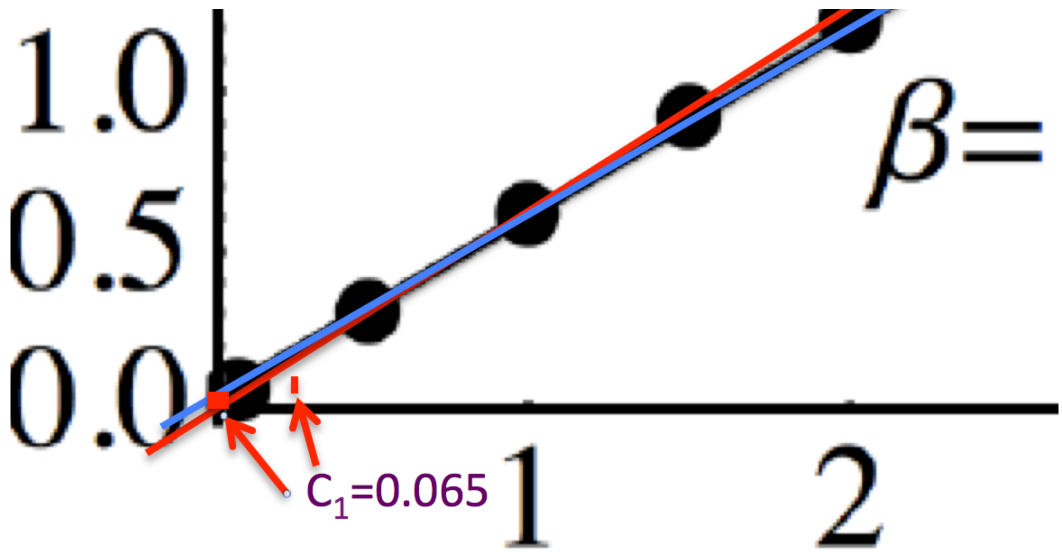

Fig. 1d: The third and final final zoom. In order to quantify the multifractality, one can use a graphical construction based on the equations $\tau(1) = \xi(1) + 1/2 = H + 1/2$ and $\tau'(1) = \xi'(1) + 1/2 = H - C_1 + 1/2$ (the authors' $\tau(q)$). C_1 is the codimension of the mean that characterizes the intermittency near the mean ($q = 1$). These equations show that the tangent to the $\tau(q)$ curve at $q = 1$ (indicated in blue) should cut the $\tau(q)$ axis (vertical) at a distance C_1 above the horizontal axis (the red line – not the authors' black one - is the exact monofractal line defined by the $\tau(q=1)$). According to a series of published estimates of C_1 over this range of time scales (starting with [*Schmitt et al.*, 1995]), the value $C_1 \approx 0.065$ for ice core temperature proxies is a good estimate (from table 11.7 of the review [*Lovejoy and Schertzer*, 2013]). This literature value is shown in the figure as the short red segments: they are almost exactly the same length as those found from the difference in the red and blue curves from the authors' data!

However, the value $C_1 \approx 0.065$ is actually indicative of strong intermittency: for example at atmospheric turbulent scales (seconds, minutes, hours), C_1 was found to be in the range 0.075 to 0.087 (table 8.1 of [*Lovejoy and Schertzer*, 2013]), so that the intermittency characteristics of the EPICA series at these scales are nearly as strong as those of turbulence! (Of course the reason that they can have strong effects is that they operate over wide ranges of scales). Interestingly, the H estimated from the authors' data is roughly $\beta/2 - C_1 \approx 0.59 - 0.065 \approx 0.52$ which is fairly close to the turbulent value 0.41 (table 8.1 of [*Lovejoy and Schertzer*, 2013]). I could recall that it is also close to the early climate estimate of 0.4 [*Lovejoy and Schertzer*, 1986].

In conclusion, there is not necessarily any contradiction in the authors' basic analyses ($\tau(q)$) and those in the literature. The problem lies in their treatment of $\tau(q)$ which was simply inadequate to accurately quantify the concavity (a fuller characterization requires the determination of the second order derivative of $\tau(q)$ at $q=1$, or the use of universal multifractals). Their rejection of multifractality is thus quite groundless.

References:

Lovejoy, S., and D. Schertzer (1986), Scale invariance in climatological temperatures and the local spectral plateau, *Annales Geophysicae*, *4B*, 401-410.

Lovejoy, S., and D. Schertzer (2013), *The Weather and Climate: Emergent Laws and Multifractal Cascades*, 496 pp., Cambridge University Press, Cambridge.

Schmitt, F., S. Lovejoy, and D. Schertzer (1995), Multifractal analysis of the Greenland Ice-core project climate data., *Geophys. Res. Lett*, *22*, 1689-1692.