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Reply to Lovejoy and Varotsos - I

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Science is comprised of the creative process of formulating new hypotheses and the systematic attempt to refute these hypotheses by testing them against observation. For the latter it is not sufficient to demonstrate that observations are consistent with the hypothesis according to some prescribed test, one also must make sure that the observations are inconsistent with other plausible (null-) hypotheses. In the present context this is particularly clear, because the statement that a response is nonlinear in itself is a negation. The main proposition in the paper by Lovejoy and Varotsos (L&V) is that the response is *not linear*. Thus, the only valid way of testing this statement against the data is to demonstrate that the linearity hypothesis is rejected by the data.

In section 2.4 (Fig. 2) and section 3.3 (Fig. 4) of our comment (R&R-C) we demonstrate that a linear response is consistent with the data. The reply of Lovejoy and Varotsos (L&V-R) only deals with section 3 of R&R-C, so we shall restrict ourselves here to the question of linearity and intermittencies.

The issue of multifractal (clustered) intermittency versus non-Gaussian Lévy processes and their long-memory derivatives was discussed at depth in a paper we recently published in Earth System Dynamics, with Shaun Lovejoy as a very active referee.[1] We find it strange that L&V-R do not refer to this paper and the associated discussion.

Our test presented in Fig. 4 is deliberately extremely simple. It can be appreciated by anyone, without understanding of the analysis method. We have just employed exactly the same analysis method as L&V (using Lovejoy's computer routines) on the data from a very simple *linear* response model, a damped harmonic oscillator. The results of the analysis are indistinguishable from L&V's results from the same analysis of output from the ZC-model. This implies that the L&V results do not reject the linear response hypothesis.

> *L&V-R does not present any arguments against the validity of this test.*

Page 1, paragraph 2: L&V-R cite two papers that are supposed to demonstrate the nonlinearity of responses to "spiky" forcing in some climate models, and state that their contribution has been to quantify this. There are many other papers that find very weak nonlinearities (see the paper by Andrews et al. and references therein). However, we do not claim that the response to such forcing impulses is linear - it seems quite plausible that they are not. Our claim is that the analysis by L&V does not reject such a claim, and by no means represents a "quantification of the nonlinearity."

They also write "The L&V method... simply exploits the known fact that linear transformations of a time series can only make linear changes to the exponent scaling function $\xi(q)$, they cannot affect the nonlinear part that is associated with the intermittency." In section 3.1-3.2 of our comment (R&R-C) we show that this is true only if the following three conditions hold: (I) The linear transformation can be represented by a power-law response function. (II) The structure functions are power-laws for all q. (III) Internal variability is negligible. Moreover, in section 3.4 we demonstrate by an example (the damped, harmonic oscillator) that the scaling function changes radically under this linear transformation when it is computed from the trace-moment analysis.

Page 1, paragraph 3: Here L&V-R defends the trace moment analysis. The method is supposed to be effective particularly because "...it removes the linear qH term in the structure function so that the linear $K(q)$ part can be studied directly." We can't understand that subtraction of a straight line from a curved graph represents anything significant. The method is based on implicit assumptions that the underlying process *is* multifractal (that the structure functions are power-laws) with a distinct "outer scale" which defines the

[1]M. Rypdal and K. Rypdal, Late Quaternary temperature variability described as abrupt transitions on a 1/f noise background, Eart Syst. Dynam., 7, 281-293, 2016. doi: 10.5294/esd-7-281-2016. http://www.earth-syst-dynam.net/7/281/2016/esd-7-281-2016.pdf. Discussion: http://www.earth-syst-dynam.net/7/281/2016/esd-7-281-2016-discussion.html

scale range where the power-law scaling holds. The power-law exponents (the slope of each trace moment of order q in a log-log plot) is determined by fitting straight lines in the log-log plot under the constraint that they all converge at the same time scale (the outer scale). In many cases (L&V Fig. 6) these lines are poor fits to the actual trace moments, signifying that the time series are *not* multifractal. Our main problem with the trace moment method is that it is automatised to classify *any* non-Gaussian time series as multifractal. And by not devising error bars due to finite sample size, also time series that are realisations of monofractal processes will be classified as multifractals, since in single finite size realisations of the process there will always be deviations from the perfect scaling.

In this paragraph L&V-R also write: "Had R&R noted this fact (that removing the linear qH term allows $K(q)$ to be studied directly) they would not have bothered to develop the linear analysis (section 3.3) which is irrelevant to the trace moment analyses presented in L&V and to its conclusions."

The conclusions of L&V are true only if conditions (I-III) are valid, which we know the are not. How can this be irrelevant?

The phrase: "we would not have bothered to develop the linear analysis..." is disturbing. What does it signify? We are testing the linear response hypothesis by exploring its consequences and compare with data. Do L&V contend that this is an incorrect or irrelevant approach? What is the alternative?

Page 1, last paragraph, and page 2, paragraphs 1 and 2: There are many formulations here that make no sense to us, and therefore is hard to comment on. What is "a linear analysis"? Trace moment analysis is neither linear or non-linear. Linearity is a property of the response model which is subject to testing. And what do L&V mean by "dominant statistics"? We cannot relate to these phrases unless the authors are more explicit about their meaning - if there is any.

What we can read out of these paragraphs, however, is that L&V define the class of multifractals to encompass essentially all stationary stochastic processes (monofractals constitute a subclass). The class of strictly multifractal processes (which excludes monofractals) then includes all stationary non-Gaussian processes. We recommend the seminal paper by Mandelbrot , Fischer, and Calvet[2] as a reference for the definition of multifractal stochastic processes.

> *The genealogies of multifractals (the multiplicative cascades) and of Lévy processes (non-Gaussian, uncorrelated noise) are fundamentally different, and so are the typical dynamical mechanisms. To treat them as one single class deprives us of important tools of understanding the underlying mechanisms governing a process.*

L&V-R show in their Fig. 1a three multifractal constructions and one volcanic signal, suggesting that it is impossible by inspection to distinguish the latter from the others. Well, we immediately did. Multifractals and Lévy processes are both intermittent, in the sense that they are leptokurtic (heavy-tailed PDFs) on the short time scales and converge to Gaussian on the long time scales. But the multifractal intermittency is clustered, which the Lévy process is not. If you're training your eye, it is usually easy to distinguish a Lévy process from a multiplicative cascade. If you believe there is no essential difference, you probably will not see it. The important matter, however, is not what you can distinguish by your eye, but what you can disclose by analysis. By construction the structure functions of the multiplicative cascades are perfect power laws (straight lines in a log-log plot), while this will not be the case for the volcanic signal (see Fig. 3c in R&R-C). If a scaling function like $K(q)$ is constructed from the indiscriminating trace moment analysis all signals will appear as multifractal.

If one generates realisations of multifractal and Lévy processes, there will always be realisations that look very similar. In order to see the difference, one will have to do statistics. If L&V will send us the routine they have used to generate the multifractals, with the parameters used to generate the plots in Fig. 1a, we shall do this statistics and demonstrate the difference.

L&V seem to believe that they escape from a logical problem by extending the multifractal class to encompass processes with non-power law structure functions, but by this they only create a new one. For this wide class of processes it is not true that "linear transformations of a time series can only make linear changes to the exponent scaling function $\xi(q)$," no matter how much they insist on the opposite. This was proven theoretically in R&R-C section 3.3 and by the linear oscillator example in section 3.4. L&V's repeated disregard of these results does not make them less true.

> *If they maintain their assertion they have to show that our analysis in section 3.4 is wrong.*[3]

Page 2, paragraphs 3 and 4, and L&V-R Fig. 1b: Fig. 1b is obvious. It follows from the non-Gaussianity of the signals. We agree that they are intermittent (bursty) , and the volcanic signal is more intermittent than the response. But they are not multifractal. They could both arise from Lévy processes. The lower intermittency of the response signal are due to two different effects: (A) The memory in the response smears out the volcanic spikes. This is a linear effect. In Fig. 1e in this

[2] B. Mandelbrot, A. Fischer, and L. Calvet, A Multifractal Model of Asset Returns, Cowles Foundation Discussion Paper # 1164, September 15, 1997, http://users.math.yale.edu/ bbm3/web_pdfs/Cowles1164.pdf

[3] A Mathematica notebook which contains the computations leading to R&R-C Fig. 4 can be downloaded from https://dl.dropboxusercontent.com/u/12007133/Commentanalysis.nb

document we demonstrate that a transformation of the volcanic signal by a long-memory response kernel leads to a lower slope shown in L&V-R Fig. 1b. (B) Internal variability (noise) will also contribute to a flattening. In Fig. 1f we show the effect on the slope of adding a stochastic forcing in the response model, producing an internal variability. Hence L&V-R Fig. 1b can be explained perfectly from a linear response model.

We totally agree "that there is a huge difference in intermittency between R&R Fig. 4b and 4c." But that is our whole point!

> *That huge difference in intermittency between forcing and response is produced by a linear response model, proving that L&V's assertion that "linear transformations of a time series can only make linear changes to the exponent scaling function" is wrong. It is wrong because conditions I-III described in R&R-C section 3 are not satisfied.*

R&R's "erroneous structure function analyses": This last part of L&V-C is yet another indication that L&V haven't read our comment properly. On lines 310-312 (Eq. (23)) we write explicitly that we compute the structure function from the cumulative summed forcing time series $Y(t) = \sum_{t'=0}^{t} F(t')$, not from $F(t)$ itself. In time series analysis this is the standard approach when structure functions are computed from noise processes ($H < 0$). As L&V-R Fig. 2a shows, the structure function of the forcing time series $F(t)$ itself is flat and reveal no other information than that the fluctuations are not growing with scale. Working on the cumulative sum (the "profile") is also the standard procedure in the DFA-analysis, which L&V-R employ in Fig. 2b. The structure function defined in Eq. (23) can be written $\hat{S}_q(\Delta t) = \Delta t^q \langle |\Delta F(t,\Delta t)|^q \rangle$, where $\Delta F(t,\Delta t)$ is the the moving average of $F(t)$ over a window Δt. This is essentially the same as the Haar structure function of Lovejoy and Schertzer multiplied by Δt^q. Hence for a scaling noise for which $\langle |\Delta F(\Delta t,t)|^q \rangle \sim \Delta t^{qH}$ the correspondence between our structure function and the Haar structure function is $\hat{S}_q(\Delta t) \sim \Delta t^q S_q^{\mathrm{Haar}} \sim \Delta t^{(1+H)q}$. What L&V-R plot in their Fig. 2b is $(S_2^{\mathrm{Haar}})^{1/2} \sim (\Delta t)^{-1} (\hat{S}_2(\Delta t))^{1/2}$. From our Fig. 3c, which L&V-R claim is wrong, we find that $\hat{S}_2(\Delta t) \sim \Delta t^1$ (the dashed line in the figure). From the equation above this yields $(S_2^{\mathrm{Haar}})^{1/2} \sim \Delta t^{-1/2}$, in good agreement with L&V Fig. 2b, and the spectrum shown in their Fig. 3 is of course also consistent with this.

As mentioned above, working on the cumulative sum is standard in time series analysis, and was explicitly stated in R&R-C. Hence it does not give L&V much credit to overlook it. What is even more disturbing is that structure-function analysis of the signal itself and the cumulative sum was discussed in a response to Shaun Lovejoy in connection with another discussion paper in ESDD where Lovejoy is a referee.[4] Lovejoy complains that we do not cite thirty years old paper of his. What about reading and citing our contributions in ongoing public discussions?

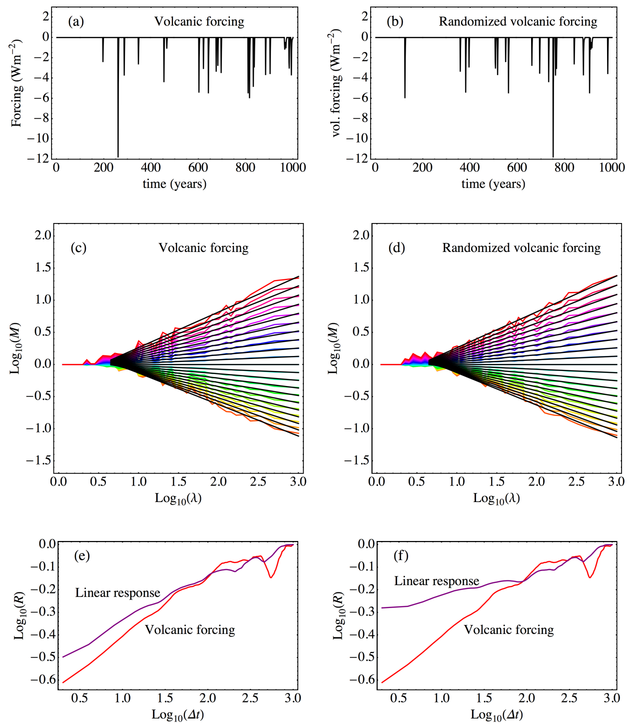

Figure 1. (a): The volcanic signal used in the ZC-simulations. (b): A synthetic signal obtained from the volcanic signal in (a) by re-distributing the volcanic spikes randomly in time. (c) Trace-moment analysis of the volcanic signal used in the ZC-simulations. (d): Trace-moment analysis of the randomized volcanic signal in (b). (e): Shows the ration $R = \langle \Delta F(\Delta t) \rangle / \sqrt{\langle \Delta F(\Delta t)^2 \rangle}$ for the volcanic signal used in the ZC-simulations (red) and for a linear-response to this signal (purple). The linear-response model has a power-law Green's function determined by the exponent $\beta = 0.8$. (f): As in (e), but with a Gaussian white noise added to the forcing. The noise has a standard deviation $\sigma = 0.3\,\mathrm{Wm}^{-2}$.

As a final and explicit demonstration that trace moment analysis cannot determine whether the volcanic signal is a multifractal clustered time series or a non-Gaussian Lévy process we have made this analysis in Fig. 1a-d in this document. Fig. 1a shows the volcanic signal used in the ZC-simulations, and in Fig. 1b the same set of volcanic spikes with the time of the spikes chosen at random. Figs. 1c and 1d show that the corresponding trace moments are indistinguishable. This demonstrates that:

> *Automatised trace moment analysis only detects non-Gaussianity, not multifractal clustering.*

[4] T. Nilsen, K. Rypdal, and H.-B. Fredriksen, Are there multiple scaling regimes in Holocene temperature records, Earth. Syst. Dynam. Discuss., 6, 1201, 2015, http://www.earth-syst-dynam-discuss.net/esd-2015-32/ , see AC C610, 'response to Lovejoy'.