Trained eye deceived by fractal clustering

Reply to the reply

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Summary

Our original paper [*Lovejoy and Varotsos, 2016*] (hereafter L+V) quantified something rather straightforward and – we thought – uncontroversial, the fact that the response of the atmosphere to volcanic forcings is nonlinear. The basic fact that a linear transfer function (Green's function) can only make a linear modification to the structure function exponent $\xi(q)$ has been known for some time and is not even contested by [*Rypdal and Rypdal, 2016*] (henceforth R+R).

In order to demonstrate that a transfer function is nonlinear, it therefore suffices to compare the exponent $\xi_v(q)$ of the forcing and the $\xi_r(q)$ of the response, and check if the difference is linear in q. *Within the limitations of the available data*, this was done in L+V. The quantification of the intermittency was done via estimates of the parameters C_1, α of both the forcing and the response. *The key limitation of the analysis was the existence of a single time series for each, and these were over finite ranges of time scales.* In contrast, the mathematics applies to an ensemble average with the exponents $\xi(q)$ estimated over a wide range of scales. The two key assumptions required to apply the method are therefore a) that the samples analyzed are indeed representative of the underlying processes and b) that the range of scales over which the exponents were estimated is adequate for the purpose. These are the true limitations of our analysis and conclusions. R+R's hypotheses I-III (re-iterated in this comment, R+Rr, "r" for "response" [Earth Syst. Dynam. Discuss., doi:10.5194/esd-2016-10-AC1, 2016]), are thus irrelevant as indicated in our response (L+Vr, [Earth Syst. Dynam. Discuss., doi:10.5194/esd-2016-10-SC1, 2016]).

The conclusions of L+V therefore stand without modification. However, the new comments of R+R clarify some of their misunderstandings about clustering, shuffling, multifractality and trace moments so that this provides a useful occasion for discussing a few additional points. We hope that these clarifications will be of interest to the broader community.

The untrained eye works well

The key misunderstanding comes out quite clearly in R+Rr when they boast that their trained eye can distinguish multifractal clustering from a purported Levy lack of clustering: "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" (R+Rr, p1). This echoes the comment in R+R: "If the original signal were a multifractal, the result should be quite different after shuffling."

Before proceeding, we therefore need a word about the meaning of "clustering" in a scaling context. Although it is a little vague, scaling clustering is essentially the same thing as "sparseness" and the sparseness of a set of points is most fundamentally characterized by its fractal codimension (there are subexponential factors that refine the characterization – most famously the "lacunarity" – but these are second order corrections to the dominant power laws determined by the codimension). The codimension c is the difference between the dimension of the embedding space and the (fractal) dimension of the set. For any given resolution l, the fraction of the space occupied by the set is $\approx l^{-c}$ (note that for any set, $c \geq 0$). *Any set of points with a nonzero codimension will thus be sparse and will appear to be clustered.* The problem of clustering is a purely *fractal set* problem, not a multifractal (measure density/field) problem. From the above quote, one can surmise that the authors believe that Levy processes do not involve sparse fractal sets whereas multifractal processes do. We investigate this below.

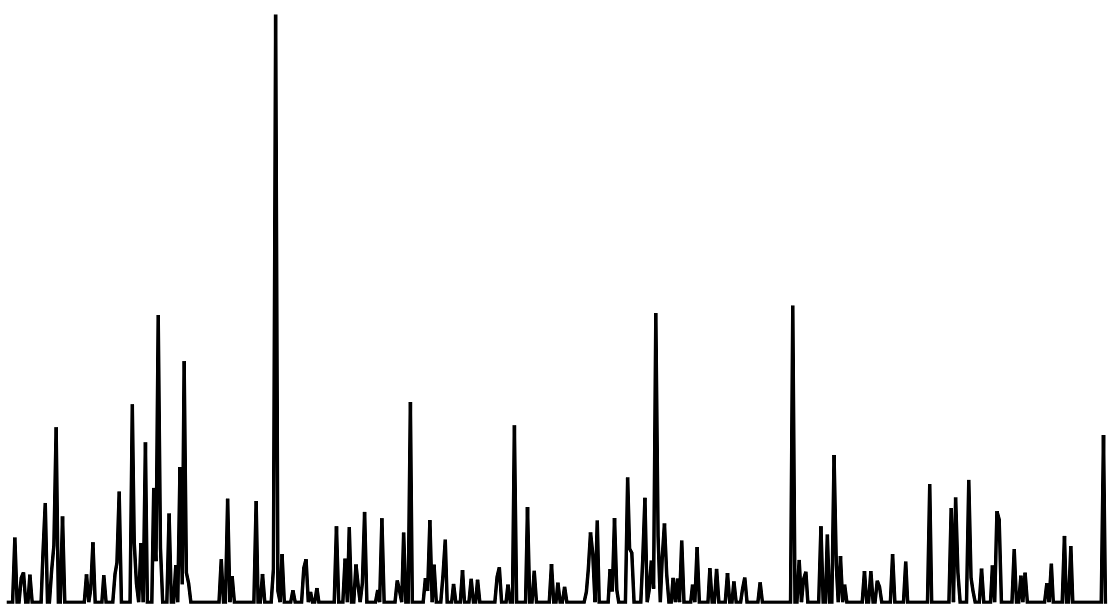

Fig. 1: A realization of a volcanic eruption model based on a Levy distribution of statistically indepedent eruption amplitudes (vertical, time runs from left to right). At time step an independent random Levy variable is chosen, those below a small threshold are set to zero corresponding to small undetected volcanic eruptions. Here a Levy distribution with exponent q_D =1.5 was used, the resulting process above is multifractal with (for large γ) codimension function $c(\gamma) \approx \gamma q_D$. Even the untrained eye can see the clustering that appears without any externally caused correlations.

Consider fig. 1 that shows a realization a model of (absolute) volcanic forcings (V) using a Levy process. Recall that Levy processes have long power law tails on their probability distributions (i.e. $\Pr(V > s) \approx s^{-q_D}$ for s>>1 where "*Pr*" indicates "probability"), in this example we used an exponent $q_D = \alpha = 1.5$ (note that for Levy processes of index α, the power law exponent q_D is equal to α and is restricted to $\alpha = q_D < 2$ whereas for multifractal processes, there is no such limitation, q_D can take on any positive value). Using the material from ch. 5 of [*Lovejoy and Schertzer, 2013*] (see eq. 5.54) it is not hard to show that the codimension function $c(\gamma)$ characterizing the extreme part of the above Levy process has the form: $c(\gamma) \approx \gamma q_D$. (Recall that $c(\gamma) \approx -\log Pr(\gamma)/\log\lambda$ where the singularity $\gamma = \log V_\lambda/\log\lambda$ and where V_λ is the amplitude of the "spikes" at the resolution l=L/λ where L is the largest scale in the simulation and $\lambda \geq 1$ is the scale ratio).

The codimension function thus quantifies how the larger values have larger γ's and larger codimensions, it thus quantifies their sparseness, the fact that the higher values are more clustered than the lower values which necessarily have smaller codimensions (c is a convex and increasing function). This quantifies the clustering of the Levy volcano model at all levels of volcanic activity.

Since clustering is a fractal not a multifractal problem, let us reduce the problem to its simplest expression by considering fractal sets generated by random walks ("flights") based on the above simulation. These are the "Levy flights", beautifully illustrated in [*Mandelbrot, 1982*]. The Levy flight based on fig. 1 is shown in fig. 2. Levy flights are generated by taking independent random Levy variables, and then using them as the basis for a two dimensional random walk (the angles between each step are taken as uniformly random variables). Once again, is the clustering in fig. 2 real or is it just a figment of our imagination? Do we see it only because our eyes are not properly trained?

Presumably R+R's difficulty is that they mistakenly associate clustering with deliberate, "externally imposed" statistical dependencies and the Levy flights have none, therefore, their trained eye will not see clustering in a Levy volcanic process. This is probably why they put so much emphasis in shuffling experiments in which the values are kept but their order is randomized in order to destroy any correlations (see their fig. 1). For reference, fig. 2 also shows the same Levy flight but based on a shuffled series. As expected, the clustering is not affected since the clustering is not due to statistical dependencies between successive jumps. R+R failed to realize that the Levy process in fig. 1 is indeed multifractal with nontrivial c(γ): citing a paper by Mandelbrot does not alter this fact! Obviously a fractal set with the same fractal dimension as the Levy flight – and hence with the same degree of clustering at all scales - could have been produced by a "beta model" cascade process (see section 5.1 of [*Lovejoy and Schertzer, 2013*]) which on the contrary does indeed use the long range statistical dependencies implicit in the cascade model to obtain the fractal set (for reference, in 2-D space, the codimension is 2-α, the fractal dimension is α). The point is that the clustering is quantified by the codimension and does not depend on the method of generation.

The above clearly shows that contrary to R+R, clustering *need not* be a consequence of statistical dependencies between consecutive values of the process: the statistical properties depend on *both* the probability distribution of the amplitudes of the spikes *and* any statistical dependencies between them. (Note: Levy processes have divergent autocorrelations so that the term "statistical dependencies" is more precise in this context than R+R's term "correlations").

Now, we can examine the *converse* situation. Rather than using a Levy based multifractal let us take a cascade - based multifractal simulation (fig. 3) with parameters estimated by Haar fluctuations and trace moments i.e. designed to make it close to the data - of the type as shown in [*Lovejoy, 2014*] and reproduced in L+Vr. Since R+R had no trouble noticing the clustering in that cascade-based case, we merely note our agreement with them. However, we can now do the converse experiment: we can take this cascade-based multifractal simulation and shuffle it. According to R+R's reasoning, if we shuffle a cascade-based multifractal, then we should eliminate the clustering. However, fig. 3 shows that this is not at all the case (can the reader detect the original unshuffled simulation?). An untrained eye is adequate to easily notice the clustering and the fact that it persists even with the shuffling. For the record, we also show fig. 4 which is a walk directly generated by the [*Gao et al., 2008*] volcanic series.

So what's going on? The fact is that the clustering of a set depends on the codimension of the set, the clustering of a multifractal process depends on the codimension function $c(\gamma)$. If two processes have similar codimension functions then they will have similar statistics including similar clustering, in this case the Levy multifractal and the cascade multifractal did indeed have similar $c(\gamma)$'s (see fig. 5: the $\xi(q)$'s are nearly identical and consequently, so are the $c(\gamma)$'s; as mentionned above, we concede that there are sub-exponential "corrections" to the dominate power law $c(\gamma)$ behaviour, but these will not alter our conclusions). Indeed, this is the fundamental meaning of the thermodynamic formalism of multifractals that was developped in the 1980's and 1990's (see e.g. Box 5.1 of [*Lovejoy and Schertzer, 2013*]): just as thermodynamics is a well defined theory irrespective of any microtheory (e.g. statistical mechanics), the same is true for multifractal processes. This means that if the multifractal exponents – the multifractal analogues of the thermodynamic potentials - are known, then the process is well defined, independent of any specific stochastic generating process, the exponent functions $c(\gamma)$ or $\xi(q)$ are sufficient.

Note that the structure of multifractal cascade processes is indeed built up using a mechanism that has long range statistical dependencies so that in general, shuffling *does* make a difference (see e.g. the shuffling experiments in ch. 3 of [*Lovejoy and Schertzer, 2013*]). However, as one moves to sets with higher and higher codimensions the statistical dependencies become less and less important – and as we see in the Levy process model – they may be totally absent.

In conclusion, the exchange of comments with R+R has brought to the fore the fact that - as is often the case in stochastic processes – that different models may exist that have similar or even identical statistical properties. In the present case, the statistics for the Levy-based multifractals and the cascade-based multifractals are sufficiently close that the data (the brown curve in fig. 5) that they are likely to be inadequate to distinguish them, i.e. to decide which is more realistic.

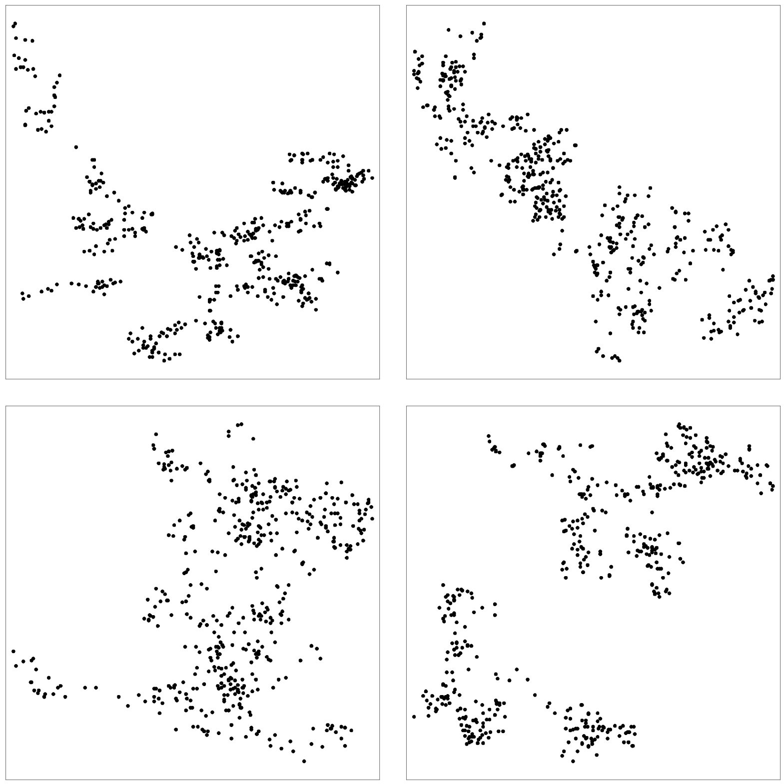

Fig. 2: A Levy flight with α = 1.5 using the simulation shown in fig. 1. One of these is the original, the others are shuffled. As expected, shuffling does not affect the clustering.

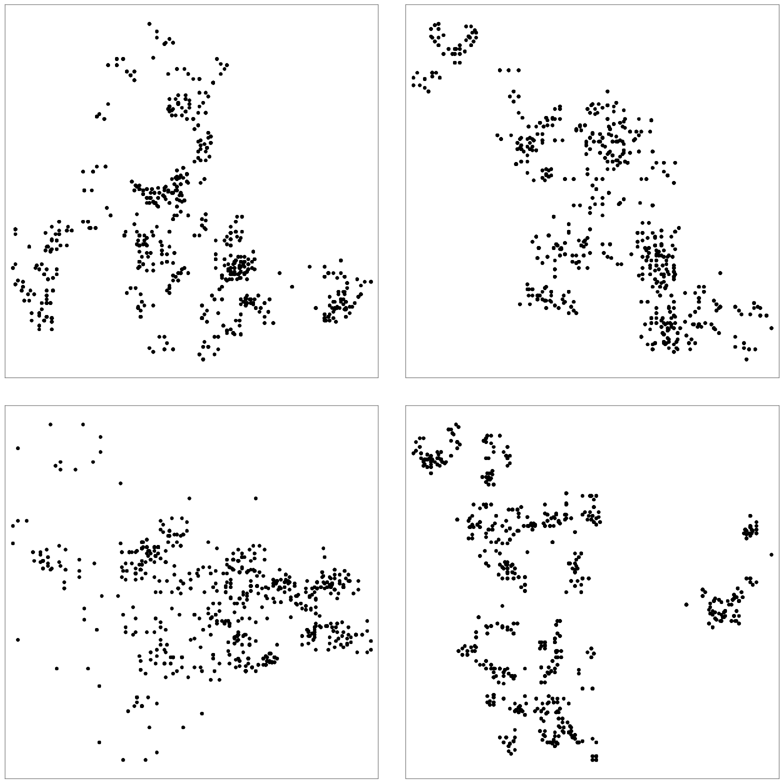

Fig. 3: Can the trained eye spot the fake cascade based multifractal simulation? The original simulation is from [*Lovejoy*, 2014]. In order to bring out the fractal clustering, this was used as the basis of a "flight" (random walk) representation. The successive volcanic spikes were used as the lengths of vectors and each vector was successively rotated by 45^o (i.e. in a deterministic fashion so as not to introduce extra elements of randomness, although this is not important). The other three walks/flights were produced from the same simulation by randomly shuffling (randomizing) the order of the amplitudes before the walk was produced (in order to destroy any correlations). Notice that in spite of the shuffling, that the flights are all highly clustered. Can the reader distinguish the original simulation from the shuffled fakes? The clustering is determined by the fractal codimension and the construction mechanism (correlated or not) is irrelevant.

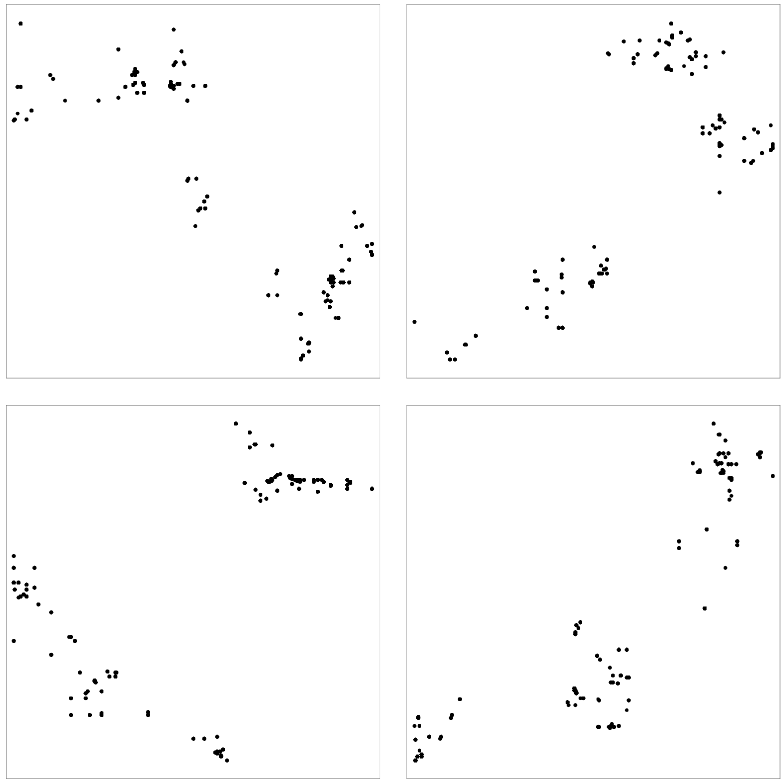

Fig. 4: The random walk/flight representation of the [*Gao et al., 2008*] volcanic reconstruction. One of these is the original, the others were shuffled.

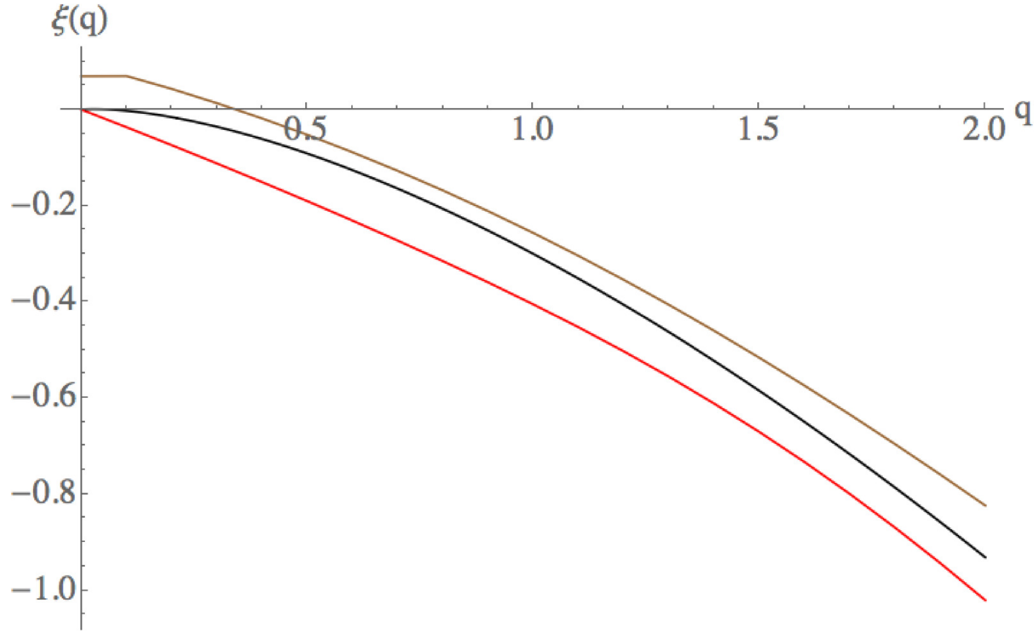

Fig. 5: A comparison of the ξ(q) from the multifractal cascade volcanic simulation (black; realizations are shown in L+V and analysed in fig. 3 above, universal multifractal parameters $\alpha = 1.5$, $C_1 = 0.2$, $H = -0.3$) and from 8 realizations of the Levy process model (purple, fig. 1 and analysed in fig. 2), with $q_D = 1.5$ (note that in this case, q_D is the same as the Levy α for the Levy process: in the universal multifractal cascade model, the Levy α refers to the generator i.e. the logarithm of the process). The brown curve is the empirical ξ(*q*) for the [*Gao et al., 2008*] volcanic reconstruction. The cascade based universal multifractal is a little closer to the data than the Levy based multifractal but the two are close. In the Levy process model there is a first order multifractal phase transition at $q = q_D = 1.5$ (many, realizations will however be needed to clearly show it, for the cascade based multifractal the transition is at about $q_D = 10$, see section 5.3.2 of [*Lovejoy and Schertzer, 2013*]).

Minor comments

1) <u>The statistical test argument.</u> R+R complain that we did not properly reply to this argument, but it has two flaws. The first is that since linear behaviour is a special case of the more general nonlinear behaviour, it is impossible *in principle* to prove linearity from data or from numerics! At best one can bound the degree of nonlinearity and show that it is smaller than some limit. This is analogous to the problem of photon mass: the statement that a photon is exactly massless is a purely theoretical statement, experiments can only put a lower bound on its value. Similarly, the statement that a process is exactly monofractal i.e. scaling but *not* multifractal is also a purely theoretical statement. Empirically, one can only puts

bounds on the nonlinear part of the exponent functions, put bounds on the degree of multifractality. In many of their papers, R+R do not seem to appreciate this, essentially equating scaling with the monofractal special case.

The second problem is that R+R's statistical test involves a classical error of logic. The failed logic is perhaps most clear if we transpose the problem. Let us paraphrase the debate that would have ensued if L+V had proposed that the speed of light is finite whereas R+R had argued that it is on the contrary infinite (in the following it is only the logic that is important!).

The transposed paraphrase would be:

> The statement that a photon has a finite velocity in itself is a negation. The main proposition in the paper by *Lovejoy and Varotsos* (L&V) is that the speed of light is finite, not infinite. Thus, the only valid way of testing this statement against the data is to demonstrate that the infinite speed 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 an infinite speed is consistent with the data.

From this, R+R would have had no trouble accepting the hypothesis that the speed of light is indeed infinite, and claiming that their method has established it as fact.

In the present case, R+R's test failed to reject linearity, so that they effectively argue that we must accept it. This is a classical error in the logic of statistical hypothesis testing. It particularly bizarre since there is a body of evidence from numerous scientists showing that the atmospheric responses to volcanic eruptions are indeed nonlinear.

(For reference, the above paraphrase can be compared with the original from R+Rr:

> *"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.")*

<u>2) Linear oscillators:</u> We also failed to pay much attention to R+R's reproduction of the (low intermittency) volcanic response using a linear oscillator. R+Rr bring this up again, but we are mystified: our claim was simply that the model *transfer function* (i.e. the part of the model that transforms volcanic forcings into atmospheric responses) cannot be linear. It has nothing to do with whether or not it would be possible to model the GCM output series in some totally different way using a linear model. The irrelevance of this can be appreciated by considering that a single realization of a process – a single series - can trivially be generated by an appropriate Fourier filter of a realization of white noise (just take the ratio of the

Fourier Transforms as the filter!), hence for single series, one can always find a "linear model" (in this case a filter).

What we claim is something different: that a reasonable physical model of the forcing *process* and response *process* – i.e. of an ensemble whose typical realizations are embodied in the forcing and response time series –cannot be connected by a linear filter.

3) Misunderstanding trace moment analysis: R+R make the extraordinary claim that *"trace moment analysis only detects non-Gaussianity, not multifractal clustering"*. This statement is remarkable in its pretensions: the authors think that their trivial numerical shuffling experiment has overturned (over!) thirty years of developments in multifractals and multifractal analysis! Their misunderstanding is related to the fundamental misunderstanding discussed above, it is based on the erroneous idea that only cascade-based multifractals show clustering whereas Levy-based multifractals do not. The point is that clustering is determined by codimensions and codimensions are determined by the trace moments (after Legendre transformation, see e.g. ch. 5 of [*Lovejoy and Schertzer, 2013*]; note that trace moments themselves are essentially stochastic generalizations of the partition function method applied to the deterministic multifractals that appear in the phase spaces of deterministic chaotic systems). The subtle interplay between probability distributions and statistical interdependencies are indeed well captured by trace moment analysis, explaining why it is a powerful technique. In this case, fig. 5 shows that the statistics of the two models are indeed very close and that both are close to the data (and could presumably be made much closer if the parameters were appropriately adjusted for this purpose).

In any case, returning to the use of trace moments in L+V they determine the essential nonlinear part – and hence intermittent part - of the $\xi(q)$ function. That is the way that it was used in L+V and it is sufficient for the claim in L+V that there is a nonlinear volcanic response.

4) In R+Rr, it is pointed out that the q^{th} order structure $S_q(\Delta t)$ and its exponent $\xi(q)$ that R+R (plotted in fig. 3 of R+R) are not in actual fact the fluctuations of the actual series themselves but rather refer to the fluctuations in the integral (running sum) of the series. This led L+Vr to point out that there was an error. R+R would have avoided misinterpretations by abstaining from using the turbulence notation S_q and $\xi(q)$ in a nonstandard manner. Alternatively, since R+R's integration increases the *H* exponent of the series by unity, they could have provided the relation between their notation and the turbulence notation: $S_q(\Delta t) = \Delta t^{-q}(S_q(\Delta t))_{R+R}$ and $\xi(q) = \xi(q)_{R+R} - q$ (the subscript R+R indicates R+R's functions). The notation S_q and $\xi(q)$ come from turbulence theory and should not be altered without at least informing the reader of the nonstandard usage.

In any case, as it is clear from L+Vr (and R+Rr show no doubt on this) there is no "curved scaling function" to be "incorrectly interpreted by L&V" (compare R+R line 322 and Figs. 2 and 3 of L+Vr).

Conclusion

The statistical structure of multifractals is the result of a subtle interplay between probability distributions and statistical dependencies. On the basis of a numerical randomization experiment, that destroys the statistical dependencies, R+R argue that thirty years of developments on multifractal processes and analysis is wrong, pointing to the example of a Levy process. Yet, the Levy process example is simply an extreme case where the multifractality is only due to the probability distributions and it does nothing to discredit multifractal theory and analysis. On the contrary, we show that the very similar cascade based multifractal volcanism model has very similar statistics and very similar properties under shuffling – that the statistical dependencies are quite weak especially for the stronger events. By showing that quite different multifractal production mechanisms can lead to very similar statistics, R+R's example nicely vindicates the thermodynamic multifractal formalism: mulitfractal processes are well defined irrespective of any detailed underlying model.

But this digression into multifractal theory has taken us very far from the issues raised in L+V that R+R purport to criticize. We therefore reiterate that the key limitations of L+V's conclusions are the twin assumptions: a) that the finite range of scales over which exponents are estimated is adequate and b) that the single available realizations of the forcing and response processes are representative of the ensemble process. We admit that without more data and simulations that our conclusions are thereby circumscribed, yet these reasons are quite different from those claimed by R+R, and there are no compelling arguments to doubt our conclusions about the nonlinear response of atmospheric models to volcanic forcings. In future, numerical climate simulations using theoretically generated forcings could in principle overcome this limitation and give a more definitive conclusion.

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