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Interactive comment on “Comment on "Scaling regimes and linear/nonlinear responses of last millennium climate to volcanic and solar forcing" by S. Lovejoy and C. Varotsos” *by* K. Rypdal and M. Rypdal

Anonymous Referee #1

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In his role as referee of “Scaling regimes and linear/nonlinear responses of last millennium climate to volcanic and solar forcings”, K. Rypdal already made many of the points that were then reiterated with M. Rypdal in this publication (see http://www.earth-syst-dynam-discuss.net/6/1815/2015/esdd-6-1815-2015-RR1.pdf). This new publication adds some detailed mathematics and some numerical analysis, but does little to illuminate the fundamental scientific debate about linearity/nonlinearity. Indeed, the entire debate that started with the original review of the Lovejoy Varotsos ESDD paper is not materially advanced by this new contribution.

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Discussion paper

Let us recall the interactions between L+V and K. Rypdal:

Oct. 23 2015, comments on the first draft of L+V

Dec. 15, 2015: L+V response.

Jan. 12, 2016, L+V revised ms submitted.

Jan. 21. 2016, K. Rypdal comments.

March 18, 2016: submission of the current paper “Comment on "Scaling regimes and linear/nonlinear responses of last millennium climate to volcanic and solar forcing" by S. Lovejoy and C. Varotsos”

Following this are no less than five exchanges:

SC1: ’On the importance and significance of Intermittency (Rebuttal of Section 3 of Rypdal and Rypdal 2016) by S. Lovejoy and C. Varotsos’, Costas Varotsos, 03 Apr 2016

AC1: ’Reply to C. Varotsos-1’, Kristoffer Rypdal, 07 Apr 2016

AC2: ’Results from the NorESM model’, Kristoffer Rypdal, 09 Apr 2016

SC2: ’Trained eye deceived by fractal clustering’, Costas Varotsos, 11 Apr 2016

SC3: ’Rebuttal of Section 2 of Rypdal and Rypdal 2016’, Costas Varotsos, 13 Apr 2016

AC3: ’Reply to "Trained eye..."’, Kristoffer Rypdal, 17 Apr 2016

AC4: ’Reply to "On testing the additivity..."’, Kristoffer Rypdal, 17 Apr 2016

SC4: ’Final comment on low frequency linearity, nonlinearity’, Costas Varotsos, 26 Apr 2016

SC5: ’Summary of Lovejoy and Varotsos rebuttal (SC1, SC2, and SC3) to Rypdal and Rypdal comment and replies (AC1, AC2, AC3, and AC4)’, Costas Varotsos, 26 Apr 2016

In the end, the exchanges have veered far from the original issues, sometimes into sterile squabbles about what are the appropriate definitions of notions such as “Levy process” or “multifractal” - or the analysis by R+R of an entirely different numerical model (NorESM) - the results of which while being relevant to a wider scientific discussion - are irrelevant to the conclusions of the L+V paper that was in fact under debate. This was not helped by the fact that in the exchanges, L+V did not insist strongly enough on focusing the discussion on the basics, allowing the debate to become too far flung.

As a reviewer, I found these public exchanges a bit astonishing: recall that the entire debate is about the degree of linearity of outputs of two indisputably nonlinear numerical models (the Z-C model and the NASA GISS E2R model). The models are by construction nonlinear and L+V give prima facie evidence that the nonlinearity is indeed evident in the both the high frequency response to strong, intermittent signals such as volcanic eruptions, and the low frequency response to combined solar and volcanic forcings. From a scientific point of view, there is little debate about the fact that the climate gives a nonlinear smoothing of volcanic forcings, and that at long enough time scales that the responses to climate forcings are also strongly nonlinear. Therefore both L+V and R+R have posed the question backwards: the problem is not to find under which circumstances the model is nonlinear but on the contrary to show under what ranges of amplitudes and of time scales that the nonlinearity is weak - and to quantify its weakness. Unfortunately the debate spiralled away into technical issues that were of little relevance to this central question. Beyond that, I can see the frustration on both sides. Take for example the dispute about the effect of a linear response to an intermittent (multifractal) forcing – this basic result is now nearly thirty years old and is not hard to understand. If the forcing is from a multifractal process over a wide enough range of scales (i.e. the structure function exponent of the forcing $\xi$for(q) is indeed concave when determined by averaging over an infinite ensemble of realizations), that the the exponent of the response of a linear system (again, if linear over a sufficiently wide range of scales), can at best differ by a linear term i.e. the response can at most be ξresp(q)= $\xi$for(q)+qH where the H is the exponent of the linear system Green's function. Clearly, if the Green's function of the linear system was not scaling, then the response would break the scaling in which case the exponent itself is no longer meaningful. Hence, if the forcing and the responses of an infinite ensemble over an wide enough range of scales show a nonlinear difference ξresp(q)$-\xi$for(q), then this can only be because the system is nonlinear.

R+R attack this result both mathematically and numerically. Mathematically, they impute a number of assumptions in particular the scaling of the transfer function. However, this assumption is not needed as an extra assumption since the statement "ξresp(q)$-\xi$for(q) is nonlinear" is only meaningful if the forcing and response – and hence the transfer function – are indeed scaling (L+V only reject the need for extra mathematical assumptions). The (nontrivial) problem is therefore to gauge how confident we can be that the empirically/numerically estimated exponents ξresp(q) and $\xi$for(q) are indeed representative of a process (i.e. of an infinite ensemble), and this over a wide enough range of time scales. This is the true weakness of L+V's claims. Their results are from over barely a factor of 100 in scale and from a single realization of the volcanic forcing – and are thus unsatisfactorily limited (as L+V more or less admit). Here, the numerical results of R+R concerning linear oscillators may be of some relevance. What R+R have shown is that for a single realization, over a range of $\approx$ 100 in scale that one can concoct a linear process that comes surprisingly close to mimicking the response of the ZC model. But what does this prove? On the one hand – within the constraints of the available data - L+V give prima facie evidence that the system is strongly nonlinear, on the other hand R+R show that – due to these constraints – that L+V's conclusions might have been produced by an appropriately concocted linear system. The trouble is that one knows that - by construction - the numerical model in question is in fact nonlinear, and the mere fact that a linear model can be concocted that reproduces the results over a narrow range of scales and over a single realization in no way forces us to concludes that the model is instead linear!

My impression is that L+V conceded too little – e.g. they essentially ignored the linear oscillator result as being irrelevant – whereas R+R concluded too much – that this result somehow forces us to accept that the model that we know to be nonlinear is in fact linear. In this example and others, the difference between L+V and R+R often appears to be one of scientific methodology.

My conclusion is that the literature debate that started in the ESDD version of the L+V paper has ended up being more of a shouting match than a constructive exchange. Some interesting points have emerged - the most fundamental being that the model outputs that L+V analysed were only marginally adequate for their purpose – either to show the subaddivity at long scales or the nonlinearly volcanic response. But these points were more or less already conceded in the L+V ESD paper if they had been aware of a more appropriate, more convincing suite of model outputs, they surely would have used them. The extra comments in R+R mostly underlined the need to delve further - and I think that L+V would probably concur: the main point of their original paper was to pose the question "what are approximate conditions for linearity?" and to suggest methodologies for dealing with it.

Aside from this, a few interesting points did emerge – not so much from the from R+R paper itself - but from the ensuing exchange. I'm thinking in particular of R+R's new analysis of the NorESM model or the interesting finding by L+V that the Levy process model with independent spikes yields both realistic clustering (in spite of having no temporal dependencies), and that it is quantitatively close to both a cascade based model and to the real volcanic data. But these are best the subject of proper peer reviewed publications, not ESDD commentaries.

My overall recommendation is therefore that the new R+R paper does not add sufficiently to the already significant exchange on the original L+V paper. The question that they – and L+V - pose is posed backwards and the new comments in R+R add little to the questions already discussed in the ESDD debate on the original L+V paper. Instead, I encourage all the authors to spend their efforts clarifying, quantifying the weakly nonlinear parts of the models and indeed of the real world!

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