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Earth System Dynamics Discussions

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

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R&R's summary of the discussion

The discussion has followed two almost separate paths:

Path1: SC1->AC1->SC2->AC3->SC5->RC1->AC5

Path2: AC2->SC3->AC4->SC4->RC1->AC5->SC6->AC6

Path 1 was initiated by L&V who wanted to continue a discussion about "intermittency in general and volcanic intermittency in particular," which was started in the review

process of our discussion paper (now published in ESD) "Late Quaternary temperature variability described as abrupt transitions on a 1/f noise background," http://www.earth-syst-dynam.net/7/281/2016/

The discussion along this path is only weakly connected to the issue of nonlinearity in the global temperature response, and is quite technical. Of relevance to the L&V paper and our comment to it are two conclusions from our side:

(i) Internal variability can explain the entire difference between the intermittency estimated from the forcing signal and the ensuing temperature signal. This was demonstrated through our harmonic oscillator example in the comment, and through the structure function analysis of the NorESM data demonstrated in Figure 1 in AC3.

(ii) Lack of scaling in the response function, and in the structure functions, will lead to "spurious multifractality" and give rise to different intermittency estimates for forcing and response, even if the response is linear. The intermittency estimates will depend on the scale interval used for fitting a straight line to structure functions/trace moments in log-log plots.

Both (i) and (ii) can potentially explain the estimated difference in intermittency observed by L&V, but the NorESM analysis shows that (i) is sufficient. It is also a phenomenon that is intuitively very easy to understand; even if the intermittency in the forcing and response is the same, addition of a strong internal noise to the response will reduce its intermittency. Neither L&V, nor Reviewer #1, have presented any compelling evidence for the assertion that this difference is caused by nonlinearity in the response. Their claim that the demonstration made by means of the harmonic oscillator response is the result of a cherry-picked realization is easily rebutted. In a revision of our comment we will show this through a Monte Carlo study, and we will also demonstrate the effect by using a linear power-law Greens function rather than the harmonic-oscillator Greens function.

Path 2 started with our demonstration that our Figure 2 in the comment, which dealt with the alleged subadditivity in the ZC-model, could be reproduced also in the NorESM model. And it also showed the high power on all scales in the internal variability as compared to the forced variability. L&V never contested these results, but in SC3, Varotsos, Sarlis, and Lovejoy (VSL) presented a "rebuttal" of another test we made in Section 2.3 of our comment article. Here they used the first 195 yr of the volcanic forced temperature time series to estimate the Haar fluctuation curve of the internal noise. The linearity hypothesis would have been rejected if this curve could be shown to be different from another estimated curve. The crucial issue here is whether or not the difference between these two estimated curves is larger than the statistical uncertainty of the estimates. We demonstrated in Figure 1 in AC4 that this uncertainty is considerably larger than this difference, and hence that the result is statistically insignificant and does not reject the linearity hypothesis.

In SC4, and reiterated in Point 1 in SC6, Varotsos, Lovejoy and Sarlis claim that the two curves are different and that we have not addressed this result. They simply ignore that we have demonstrated that the result is statistically insignificant. The discussion in SC6 demonstrates again that L&V are utterly confused about how to deal with statistical uncertainty in hypothesis testing. They don't understand that in all these tests of subadditivity, the test is to check if we can reject the statement that two Haar fluctuation curves are equal. This can be done if the difference between the curves is larger than the finite sample size uncertainty of the estimated curves. In all the tests under discussion this difference is smaller than the uncertainty, and the rejection of linearity fails.

In SC4 they manage to write that our presentation of results from the NorESM model is "an indirect admission that our original paper was correct." This absurd statement is another example which demonstrates that L&V dismiss basic principles of scientific methodology. So let us reiterate: a well-posed scientific hypothesis cannot be verified, only falsified. Linearity in the response is such a well-posed hypothesis. That the tests devised by L&V have failed to falsify linearity, does not mean that one should not stop

searching for new data and new tests that could falsify it. The larger the arsenal of data and tests which fail to falsify linearity, the greater our confidence in the hypothesis. It is therefore highly appropriate and relevant in a comment to bring in other relevant data and test methods.

The comments of Reviewer #1 (RC1) did not address any of the issues discussed in Path 2. The reviewer recognizes that there seems to be a disagreement on scientific methodology between us and L&V, but expresses the view that such a disagreement somehow does not belong in a public discussion and does not deserve to appear in a peer reviewed comment. We disagree with that view, and hope it is not the editorial policy of ESD. Discussion about scientific method and how papers should be written in a concrete context should be a central part of the open review process adopted by Copernicus journals.

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