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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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This is a reply to reviewer #1

We agree with the reviewer that this discussion has digressed very far from the L&V paper and from our comment to it. But that is not our fault. Rather than sticking to the real issue, L&V started the discussion by publishing a very lengthy reply where the main message was that we were ignorant about the multifractal formalism. We had no other choice than responding to this attempt to discredit us as incompetent novices to the field. It is rather ironic that L&V in their last reply complain that we

are "too mathematical," whereas they are "physical." Our comment makes use only of elementary mathematics, we illustrate our points with simple demonstrations, and we mostly use the methods of and L&V and software downloaded from Lovejoy's web site.

The reviewer writes: "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."

How can there be a degree of linearity? The meaningful question is: what is the degree of nonlinearity. And we never disputed that the models are nonlinear. The issue is if this nonlinearity is strong enough to be detectable in global temperature by the methods employed by L&V. The proper way to test this is to find data and methods to reject the linearity hypothesis.

The reviewer writes: "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."

We find it very depressing that the reviewer, after all the exchanges on methodology in the discussion, can write that we conclude that the model is linear. This is wrong! We do not conclude that, neither in the comment, nor in the ensuing discussion. We have stressed again and again that linearity is a statement that cannot be verified (just like the statement that the photon has zero mass). This is not semantics, it is a fundamental principle in the philosophy of science. But linearity can be falsified by proper data and a proper test, and this is why the linearity hypothesis is a well-posed problem. Our whole point is that the two tests devised by L&V do not reject the linearity hypothesis. If nonlinearity cannot be detected by rejection of the linearity hypothesis in global temperature data, it gives credibility to linear modeling of the global temperature response. This is why it is important to clarify whether L&V's tests are correct or not.

In our Figure 2, we demonstrate that L&Vs test for subadditivity in the ZC-model is invalid. As a reviewer, one of us pointed out the unsatisfactory way L&V dealt with this issue in their Figure 3 of their paper, and this was still unsatisfactory in their published paper. The demonstration in Figure 2 of our comment was not made in the review process, and this comment is our only possibility of publishing it. Neither the discussion, nor the reviewer, have disputed the correctness of our Figure 2. Hence, if ESD will not publish this demonstration the journal sends the false message that the peer-review has established that Figure 3 of the L&V paper is correct, and that our Figure 2 is wrong. Alternatively, it sends the message that the journal will not accept comments on papers published by influential scientists.

The problems connected with the invalid implicit assumptions (I-III) in the L&V paper were raised by us in the reviews, but rejected by L&V with highly unsatisfactory arguments. We should probably have recommended rejection, but there was obviously a need for a more in-depth discussion of these points. As an alternative, the editor suggested to write a peer reviewed comment, which we did. In Sect. 3.2 of our comment we demonstrate theoretically that imperfect scaling in response function and structure functions may give rise to different estimated intermittency in forcing and response, even if the response is linear. The reviewer does not point out any error in this section.

Nevertheless, we stress in our comment that Sect. 3.2 is not essential for our conclusion. The essential thing is the demonstration that a linear response model with internal noise can reproduce the trace-moment results of L&V. The reviewer buys the argument of L&V that this is demonstrated only by one realization of this linear model, and ignores that we have made the code available for anyone to check that this is a statistically robust result. If the editor invites us to submit a revision, we will include the results of an ensemble run which proves this point.

The most important effect that produces the observed change of intermittency between forcing and response is probably the high internal variability. It is quite astonishing that this effect is not commented by the reviewer. The main reason for including the data from the NorESM model was to demonstrate the crucial effect of that internal variability.

How can the reviewer ignore that!

That L&V neglect internal variability was raised in the review, but it was rejected by L&V as unimportant by very obscure arguments. We have demonstrated in our comment (and we will include the NorESM results in a revised version) that it is very important. If the reviewer cannot prove otherwise, and still recommends that it is not worth publishing as a comment, the reviewer in fact recommends ESD to refrain publishing corrections to incorrect published results.

The reviewer ends his report by encouraging all the authors "to spend their efforts clarifying, quantifying the weakly nonlinear parts of the models and indeed of the real world!" That is an issue we are already working on with data from the NorESM model. However, those nonlinearities are difficult, and maybe impossible, to detect in the global temperature series. The likelihood of detection is much greater in regional or local temperature data, and in other climate variables. One cannot quantify nonlinearity if one is not able to detect it. And detection means rejection of the linearity hypothesis.

The reviewer seems to be of the opinion that it is unimportant whether the tests devised by L&V to detect nonlinearity are valid or not – since the models obviously are nonlinear anyway. Then one cannot avoid asking the crucial question; what was the point of publishing the L&V paper in the first place?

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