

# ***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***

**K. Rypdal and M. Rypdal**

kristoffer.rypdal@uit.no

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This comment by Varotsos and Sarlis suffers from severe logical weaknesses.

Reply to point 1: We reproduce the result that Varotsos and Sarlis (V&S) refer to in Fig. 1 of our response named AC4, and demonstrate by a Monte Carlo simulation that the difference between the two curves are statistically insignificant. We did not discuss this in our original comment, because we used the entire 1000 yr record to construct a signal for the internal variability, and got very small difference between the curves. LSV's use of the first 195 yr of volcanic forcing response as a measure of internal variability

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is of course legitimate, but it comes with a penalty of greater statistical uncertainty.

Reply to point 2: V&S misses the point that the finite-sample size uncertainty for a 1000 yr time series is much smaller than the uncertainty for the 195 yr series. But V&S' main problem is that they continue to be utterly confused about the logic of the tests (and so does Reviewer #1). All our tests are devised to REJECT the linearity hypothesis. It is quite obvious that the curves corresponding to the red and brown bullets in Fig. 1d of our comment are so close that they are within the confidence limits that would result from the same kind of Monte Carlo simulations that we did in AC4. This means that the test does not reject linearity.

Reply to point 3: We agree that the main effect that leads to the difference of intermittency between forcing and response in our harmonic oscillator example probably is caused by the internal variability. But so what? If this is the case, the test demonstrates that internal variability explains the trace-moment results of L&V in a linear response model. This is an important and highly relevant observation.

V&S argue that they have falsified the linearity hypothesis in points 2 and 3 above, and therefore our demonstration concerning the trace moments is irrelevant. As discussed above, we don't accept that the linearity hypothesis was falsified in points 2 and 3, but let us for the sake of argument assume that it were true, and let us look at the original L&V-paper. They first devised a test of subadditivity which they contended rejected linearity. Then they devised the trace moment analysis as an alternative test. According to the logic of V&S, the trace moment test of L&V should then be irrelevant. So why publish it?

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