More comments on "Multiscale fractal dimension analysis of a reduced order model of coupled ocean-atmosphere dynamics"

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General comments:

The manuscript has been improved, in particular, it is more readable, easier to follow although it is still very technical.

The authors are not alone in this tendency to develop more and more sophisticated algorithms that yield results further and further removed from the original physical problem. These methods are not wrong, the problem is more with the interpretation of the results. Recall that even the (old) Fourier technique was sufficiently difficult to interpret that it led to the "missing quadrillion" in atmospheric variability that was only recently discovered (2015) and that is still widely ignored! Therefore my point for discussion (below) is somewhat optional (I think it could potentially better situate the authors' technique), but not essential for publication In other words, if the authors can respond to the minor comments below, then the paper could be published).

Discussion point:

My main issues are still associated with the rather indirect and difficult to interpret method that is introduced. For example, a key empirical feature of macroweather temperatures is their temporal scaling over wide ranges (typically ≈ 1 month up to decades and longer) that involves long range system memory. It has recently been shown that such memories arise as classical consequences of the classical heat equation when the correct radiative-conductive boundary conditions are used [*Lovejoy et al.*, 2021], [*Lovejoy*, 2021]. Both the empirical finding itself (that can be used for example for monthly, seasonal forecasting, land and ocean, [*Del Rio Amador and Lovejoy*, 2021a; *Del Rio Amador and Lovejoy*, 2021b]) and the rather general (heat storage) mechanism (that applies to both land and ocean), bring into question the strong assertion (line 26) that "low-frequency variability (LFV) is strictly related to the ocean.".

Rather than investigating the scaling in a rather abstract phase space constructed with a complex sifting procedure, shouldn't we first attempt to understand the rather fundamental real space scaling that has still not been satisfactorily explained by dynamical systems theory?

Minor comments:

1 Line 38: box-counting was proposed in the 1950's, not by Ott 2002.

2. Line 50 and several other places: the scaling exponents D_q characterized the statistics of the phase space scaling; calling them "geometric" is anachronistic (from Mandelbrot) and misleading. Elsewhere D_q is even attributed "topological properties" even though the phase space is considered to be a set of isolated points (i.e. with topological dimension zero – or after interpolation, topological dimension=1). It is the phase space density of points whose density statistics are characterized by D_q.

3. Line 50: there is a conceptual slippage. It is stated (blue):

with $\Theta(\cdots)$ being the Heaviside function. More specifically, D_0 is a purely geometric measure providing us information on the coverage of the phase-space by the studied system's dynamics, D_1 is an information measure giving us a measure of the information gained on the phase-space with a given accuracy, while D_2 is a measure of correlations, i.e., mutual dependence.

However, the D_q are exponents characterizing the rate at which the sparseness (D_0), the information (D_1), the correlations (D_2) *change with scale* – i.e. NOT the values at any given scale. There is then confusion because the next line: "without exploring how these properties evolve at different scales" refers now to scales in real space rather in phase space.

4. Line 110, one discusses scale invariant features over a wide range of scales and then refers to a recent review (Franzke et al 2020). On the one hand, it would be of interest to see if the model has realistic real space scaling

properties, and the slightly older monograph [*Lovejoy and Schertzer*, 2013] covers far more relevant material since it includes spatial scaling (the main source of temporal scaling) as well as the shorter (weather) time scales covered by the authors' model.

5. Line 123: The authors mention: "nonlinearity and non-stationarity properties of signals". We should be clear that signals are simply signals, they are neither nonlinear nor nonstationary. The latter are properties of processes or of models or of infinite ensembles – i.e. of theoretical constructs. In other words, the pertinence (or otherwise) of MEMD must be justified (or not) by the theoretical framework from which the signal is assumed to issue. Therefore the argument should be based on the characteristics of the 36 component dynamical system that is assumed to be a good model of the real world system.

6. Eq. 11, the original exponent (q) was correct!

7. Line 369: the effect of sample size and its implications for spurious scaling may be due either to first order multifractal phase transitions (from the probability tail as indicated here), or from second order phase transitions (see ch. 5, section 5.3, [*Lovejoy and Schertzer*, 2013].

8. Line 380: The "multifractal width" is in fact an ad hoc way of quantifying multifractality. It is not optimal since it is generally not a characteristic of the process, since it is sensitive to the sample size (this is due to multifractal phase transitions either the first order transitions mentioned on line 369 or to second order transitions c.f. above reference). That is why a better alternative is simply to use the co-dimension of the mean (= $d-D_1$ where d is the dimension of the phase space).

9. Although there is much discussion about scaling properties in phase space, there is no mention of the fundamentally important scaling properties in real space.. It would be valuable if the authors could discuss how their results help us understand (or not), this basic feature of temperature and other fields.

References:

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