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

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General comments:

The authors propose combining two apparently contradictory analysis techniques to the outputs of a low (36) dimensional dynamical ocean - atmosphere model. The first, makes a nontrivial decomposition of the 36 dimensional signal into series with well-defined time scales, the second analyses the phase spaces assuming the existence of scale invariant properties. The justification and interpretation of this is opaque.

While the authors question the utility of conventional analysis techniques, at least the latter can be interpreted in straightforward manners. The interpretation of their results is nontrivial.

Detailed comments:

The notation is not easy to follow. Please explain the curly bracket notation used throughout:

$$\{\mathbf{s}(t)\}|_{t\in T} = \{s_1(t), s_2(t), \ldots, s_k(t)\}$$

On the left, a bold symbol "**s**" is used which is standard for indicating a vector. Why do the authors (apparently needlessly) add curly brackets and then an explicit restriction as a subscript?

Further, there is the bizarre looking symbol $(D_2^{\sum_j})$, that is also not adequately explained.

When discussing the mathematical properties of the usual decompositions ("completeness, convergence, linearity, and stationarity") it is stated that "these conditions are not usually met when real-world geophysical data are analyzed". This is confusing since the mathematical properties of Fourier or other decompositions are valid irrespective of any application. I think the authors meant to question the appropriateness of such decompositions for their specific application? However, this is a mathematical question that cannot be answered without reference to a specific assumed mathematical framework. In the paper the authors do not analyze empirical data at all but rather model outputs. Contrary to real empirical series, their series are therefore taken from a well-defined mathematical framework given by dynamical systems theory. Please explain why standard decompositions are not adequate for studying such model outputs and why there is a need for them to be replaced by decompositions with quite nontrivial interpretations and properties.

Also in the Methods section, it is stated that the authors "put forward a novel approach based on combining two different data analysis methods for Multivariate Empirical Mode Decomposition and generalized fractal dimensions". What is confusing is that while the MEMD analyzes time series in real space, in their application, the generalized fractal dimensions analysis is carried out in a quite different space - the phase space of each series. The result is that for each time series with characteristic time scale τ, that the corresponding phase spaces are assumed to be scaling. In other words, while there are essentially no scaling properties in real space, it is assumed that there will be nontrivial scaling properties in the corresponding phase space. The approach is presumably justified if the characterizing these scaling properties via generalized fractal dimensions will help understand the system. At this point one wonders whether the conventional Fourier spectrum of each τ scale series might have been easier to interpret, to understand. All this needs explanation, clarification.

In particular, when the generalized fractal dimensions are estimated, the authors need to show that there are indeed some phase space scaling properties. Using mathematical definitions such as eqs. 1-3 - where the small scale limits are taken - has only a formal validity when the definitions are applied to numerical model outputs, especially when the latter has been subjected to cubic spline interpolation which makes the small scales artificially smooth. In practice, one needs to display scaling behaviour over at least an order of magnitude or so in scale in order for any fractal dimension estimates to be convincing. The authors must therefore display some of their scaling plots - not just logarithmic slopes that have already been interpreted in terms of dimensions.

In this regard, I could also add that figs. 9 and 11 are almost certainly largely spurious. This is because typically for moments of order $q \approx > 3$-4, the moments are completely dominated by a single hypercube (a "second order multifractal phase transition") so that for larger q, the values will depend sensitively on the exact details of the input series. Similarly for $q<0$ most if not all the values will likely be spurious essentially due to the statistics of the very sparsely populated regions of phase space (the very low probability regions, see e.g. the discussion in ch. 5 of [*Lovejoy and Schertzer*, 2013]). In other words over most of the range of moments given in the figure ($-20<q<20$), the dimensions are likely to be spurious .

Finally, the interpretation of the key figures 5-8 is not at all obvious. Calling these characterizations "topological, geometric" is unhelpful and/or misleading since they are actually statistical exponents without any straightforward relationship to the phenomenon under study.

The authors could note that whereas a white noise signal would give a correlation dimension equal to the dimension of the phase space itself (it is space filling), that a Brownian motion in a space $d \geq 2$ has a constant dimension = 2.

Reference:

Lovejoy, S., and Schertzer, D., *The Weather and Climate: Emergent Laws and Multifractal Cascades*, 496 pp., Cambridge University Press, 2013.