

Earth Syst. Dynam. Discuss., referee comment RC1
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Comment on esd-2020-96

Anonymous Referee #1

Referee comment on "Multiscale fractal dimension analysis of a reduced order model of coupled ocean–atmosphere dynamics" by Tommaso Alberti et al., Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2020-96-RC1>, 2021

The work by Alberti et al. is a very intensive and information paper. It shows how to model Atmosphere and ocean dynamics within the scope of ESD. The authors extended the concept of multiscale generalized fractal dimensions employing Multivariate Empirical Mode Decomposition to analyze multiscale and multivariate behavior of the ocean–atmosphere coupled dynamics. Although the concept is not new to the scientific community, it is interesting to know how such a process is applicable for elucidating atmospheric behavior. The one important thing is that they tried to give more credits to the relevant works as much as possible.

The paper is well written with proper usage of English and scientific jargon. However, for the general audience, some of the terminologies need to be explained simpler. For example, the readers may not necessarily need to know about the Hausdorff dimension. Although they are making some valid assumptions in the methodology, some statements are a bit confusing. For instance, the authors mentioned that mathematical properties of completeness, convergence, linearity, and stationarity are usually not met when real-world geophysical data are analyzed. But it is not clear the reason behind this and what makes the use of adaptive methods. How is the complexity of data suitable for such methods?. Likewise, while the shifting process needs careful implementation for multivariate techniques, Mandic (2010) proposed an alternative way to cubic spline interpolation in each direction with a quasi-Monte Carlo-based approach. But the reviewer does not fully agree with it as such interpolation may lose the data's intrinsic properties since this approach produces smoother dynamics that do not exist in the data.

The authors tried to interpret most of the results efficiently. However, some of the interpretation is very unclear and hard to follow. For example, the authors did not mention what is the physical meaning behind the correlation dimension. It is just a kind of statistics of the data. Without understanding the physical meaning, it is not clear why it is a function of time. Another issue is that some of the figures are not interpreted well. e.g., the description of Figures 5 and 6 are not marched. They are not clear, as seen in the figures. For the general audience, they are confusing. Even though the multiscale correlation dimension for each MIMF decreases with an increasing timescale, as seen in

panel (a), the other two panels are not well elaborated.

The authors cleverly described the experiments. To reproduce the work, one needs to understand all the mathematical formulas. In the scientific method, some time calculation and mathematical expression do not match as most of the calculation procedure follow fundamental statistical programming. It needs a concise explanation of calculating all these quantities like system attractors, phase space, and correlation dimensions. The description of these quantities introduced in the manuscript is very dubious and complex to replicate.

The reviewer is thankful for providing data sets. But it becomes worthy if it includes an explanation of how to reanalyze these data sets. Finally, the reviewer appreciates the work of the authors. Still, it needs a bit more simplification and incorporating the issues mentioned above.