We really appreciate the positive evaluation of our manuscript and the nice words the Referee used for describing both our approach and our findings.
We thank the Referee for raising some points that can be helpful for improving the presentation and clarity of our findings.
Here we provide short replies (in italics, labelled by "A") to the Referee's comments (in normal font, labelled by "C") that will be addressed in a more detailed way in our final response and thoroughly considered in a revised version of our manuscript.

C1. 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.

A1. *We will be more precise when introducing some concepts and terms (as for example the notion of Hausdorff dimension or generally the fractal dimensions themselves).*

C2. 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.

A2. *We really appreciate this comment that allows us to make more clear some statements, trying to reduce some possible confusion.*
We are ready to clarify the sentence on properties met by real-world data since linearity and stationarity are usually not met. This is also strictly related to the use of adaptive methods that can be justified to overcome some limitations of fixed-basis methods such as linearity and stationarity assumption. Moreover, adaptive methods (as the MEMD) could be more suitable for reducing some mathematical assumptions and a priori constraints.
Concerning the quasi-Monte Carlo-based approach, it is used only to provide a more uniform set of direction vectors over which to compute the local mean of envelopes, and not to interpolate maxima and minima and/or to manipulate the data introducing a smoother dynamics. Moreover, the quasi-Monte Carlo method is also needed to avoid implicitly preferred directions that could be more dominant with respect to the others, which could introduce a source of errors in evaluating signal projections. We will add more details and corrections in a revised version of the manuscript to be more clear and to avoid confusion.

C3. 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.

A3. *We thank the Referee for this comment. D_0, D_1, and D_2 are strictly related to different properties of physical systems: (i) one purely geometric measure (D_0) providing us information on the coverage of the phase-space by the studied system's dynamics, (ii) one information measure (D_1) giving us a measure of the information gained on the phase-space with a given accuracy ε, and (iii) one measure of correlations, i.e., mutual dependence, between phase-space points (D_2). Since the collective behavior of a system is given by physical processes operating at different scales, it is straightforward to look how they contribute to the topology of the phase-space, not only singularly (as in panel (a) of Figs. 5-6) but especially when considering all processes occurring below a selected scale (as in panel (b) of Figs. 5-6) and by looking separately at the atmosphere and ocean (as in panel (c) of Figs. 5-6).*
In a revised version of our manuscript, we will provide more details on the fractal dimensions in terms of their physical meaning (at least for D_0, D_1, and D_2), together with a more detailed description of Figures 5-6.

C4. 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 procedures 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.

A4. *We thank the Referee for this suggestion. Our full system consists of 36 variables, thus we are working on a 36-D space. For visual purposes, we reduced our 36-D space to a 3-D subspace by looking at the behavior of the three selected dynamical variables (i.e., $T_{o,2}$, $\Psi_{o,2}$, and $\psi_{a,1}$) in a 3D plot. This allows us to investigate the 3D projection of the full system phase-space attractor, i.e., the set of values toward which our system tends to evolve.*
Concerning the calculation of the generalized fractal dimensions, we used the approach proposed by Hentschel and Procaccia (1983) consisting of partitioning the phase-space into hypercubes and then measuring the probability of finding a given hypercube filled by points and/or its generalization to a statistical order q (as also described at lines 40-52 of the submitted version). We will also be more precise in a revised version and include more details on the calculations of D_q.

C5. Finally, the reviewer appreciates the work of the authors. Still, it needs a bit more simplification and incorporating the issues mentioned above.

A5. *We really thank the Referee for his/her nice words on our manuscript and we will do our best to be more precise and more clear in the revised version of the manuscript.*