Dear Editor,

first of all, we would like to express our sincere thanks for your overall positive evaluation of our manuscript and for your particular thoughts on our approach and your encouragement in staying rigorous and formal to keep the scientific level up as high as we can. With this letter, we are submitting a revised version of our manuscript entitled “Multiscale fractal dimension analysis of a reduced order model of coupled ocean-atmosphere dynamics”. We carefully considered and addressed all minor comments and suggestions raised by Referee #2 to improve our manuscript.

We are confident that the revised version allows us to present our results in a more detailed and appropriate way. In the following, we provide a point-by-point reply (in italics) to all comments (in normal font) of Referee #2.

Sincerely,
Tommaso Alberti, Reik Donner, and Stéphane Vannitsem

Referee #2

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).

We thank the Reviewer for his/her positive evaluation of our revised manuscript. We think that this general comment could really open a wide discussion on the topic of data analysis in general. While we agree that sophisticated algorithms could lead to additional difficulties in interpreting the associated results, it is also surely true that this also applies to less sophisticated ones. Indeed, we could easily question on the reliability of describing a plethora of natural phenomena, which are nonlinear and/or non-stationary (as also the Reviewer stated in their minor comments below), by using stationary methods or via fixed-basis decomposition methods that could not be representative of the phenomenon under study. We think that completely answering or solving this debate is really difficult and likely not possible within just one paper. However, we considered the Reviewer’s suggestions to improve the clarity of our manuscript.

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?

We thank the Reviewer for this important point that surely needs to be further investigated in future work to obtain a complete understanding of real space scaling properties. However, our main aim is not to propose novel dynamical system models to explain energy/heat transfer or any other specific kind of physical process in the atmosphere-ocean coupled system. We rather propose and perform an investigation of the role of the different temporal scales in determining some of the key features of the studied model and how they can be reconciled with reanalysis data. The model we used here has been developed starting from the quasi-geostrophic equations describing the interaction between a two-layer atmosphere and a one-layer ocean over an infinitely deep quiescent ocean layer to investigate the ocean-atmosphere coupled dynamics. We are aware that different mechanisms could be responsible for developing low-frequency variability and, for this reason, we have slightly modified the sentence on line 26 accordingly, also introducing the suggested additional recent bibliography.

Minor comments:

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

We agree and modified this statement accordingly.

2. Line 50 and several other places: the scaling exponents Dq characterized the statistics of the phase space scaling; calling them "geometric" is anachronistic (from Mandelbrot) and misleading. Elsewhere Dq 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 Dq.

We agree and modified this statement accordingly.

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

However, the Dq are exponents characterizing the rate at which the sparseness (D0), the information (D1), the correlations (D2) *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.

The Reviewer is right since the term "scale" as used here refers once to the phase space and then to the real space. We revised the corresponding paragraph accordingly.

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.

We thank the Reviewer for this comment. Unfortunately it is not possible to perform such an analysis at this point, since the used model does not involve a sufficiently wide range of spatial scales. However, in addition to Franzke et al. (2020) we also refer to Lovejoy and Schertzer (2013) in our revised manuscript.

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.

We thank the Reviewer for this comment. We agree that nonlinearity and non-stationarity are properties of phenomena manifesting into signals. We modified the text accordingly.

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

We changed this accordingly.

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].

We changed this accordingly.

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-D1 where d is the dimension of the phase space).

We thank the Reviewer for this comment. We used the multifractal width since it is a simple and relatively easy to interpret quantitative concept to evidence what is also reported in Figs. 7-8, i.e., the different values of D_q for different q. We prefer to avoid introducing yet another measure like the co-dimension of the mean for the benefit of the reader and for the sake of simplicity. We however added a few details on this aspect in the corresponding part of the manuscript.

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.

We thank the Reviewer for this comment. We added a few details on this aspect in the conclusion part of the manuscript.

References:

Del Rio Amador, L., and Lovejoy, S., Using regional scaling for temperature forecasts with the Stochastic Seasonal to Interannual Prediction System (StocSIPS), *Clim. Dyn.*, *in press* doi: doi: 10.21203/rs.3.rs-326161/v1, 2021a.

Del Rio Amador, L., and Lovejoy, S., Long-range Forecasting as a Past Value Problem: Untangling Correlations and Causality with scaling, *Geophys. Res. Lett.*, *under review*, 2021b.

Lovejoy, S., The Half-order Energy Balance Equation, Part 1: The homogeneous HEBE and long memories, *Earth Syst .Dyn.* , *(in press)* doi: https://doi.org/10.5194/esd-2020-12, 2021.

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

Lovejoy, S., Procyk, R., Hébert, R., and del Rio Amador, L., The Fractional Energy Balance Equation, *Quart. J. Roy. Met. Soc.* , 1–25 doi: https://doi.org/10.1002/qj.4005, 2021.