Author’s Response to S.G. Penny’s Comments on “Exploring the Lyapunov instability properties of high-dimensional atmospheric and climate models”

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The authors thank S.G. Penny for the thorough reading of the manuscript, the supportive comments and the constructive remarks. The manuscript has benefited a lot from your helpful input.

Below is a point-by-point list of modifications that have been applied, based on your report.

General points:

In general, this is a very nicely written paper. The introduction is accessible and informative. The results are interesting and I believe will inspire a number of new research directions.

I’d suggest that the authors perform a forced atmosphere-only and a forced ocean-only experiment with the MAOOAM system to compare how typical forced atmosphere or ocean models compare to coupled systems in terms of the Lyapunov spectrum. Or if it already exists, point to a previous work by the authors that has done this comparison. This would be helpful for guidance to the operational centres currently making decisions about what is gained from transitioning from separate component forecast systems to a fully coupled forecast system.

As a general comment, the authors should strive to cite the original works for various concepts rather than a reference text or review paper.

To the editor: Regarding the journal’s typesetting decisions, please place the figures closer to where they are mentioned in the text.

Technical points:

Page 2

L 9-10:

“but also the errors that are present either in the model parametrizations, known as model errors,”

It would be more accurate to say this is known as ’model parameterization error’, since ’model error’ includes systematic misrepresentation of the system dynamics.

Indeed, the term model error is too general here. We have removed the inaccurate part of the sentence, which now reads: "This sensitivity property affects not only the dynamics of errors in the initial conditions but also the errors that are present either in the model parametrizations or in the boundary conditions [...]."

L 11:

Missing period at the end of the line.

Corrected.

L 30:

This paragraph should start with a "However,"

We agree; this has been added to the paragraph.

L 32:

" for atmospheric instabilities, and most notably convective"

Remove the word 'and'

The superfluous 'and' has been removed.

L 34:

"The oceanic circulation, by contrast, is mostly mechanically driven by atmospheric winds"

This is true on shorter timescales, but you should also acknowledge buoyancy forcing and their effects on the thermohaline circulation. This is also an important aspect of the ocean circulation and occurs over much longer timescales.

Indeed, at longer timescales, buoyancy forcing is an important aspect on the ocean circulation that must be mentioned. The mechanisms of forcing of the deep oceanic circulation are also related to the winds, that favour the mixing. Other important factors are tides and wave breaking. There are various views on this by oceanographers.

The sentence has been adapted from the original version:

"The oceanic circulation, by contrast, is mostly mechanically driven by atmospheric winds [...]."

and now reads:

"The **surface** oceanic circulation, by contrast, is mostly mechanically driven by atmospheric winds [...]. **On even longer timescales, buoyancy fluxes are an important driver for the deep ocean's thermohaline circulation.**"

Page 3:

L 26:

"corresponding to positive [and neutral] Lyapunov exponents"

The sentence has been adapted as suggested. Indeed, the null exponents also form a part of the unstable subspace as used by Trevisan et al. (2010).

Page 4:

L 27 -28

In general, I don't like the use of the term 'this paper' in technical writing. First, it is incorrect - this is an online journal so this work will primarily be consumed as an electronic file. Second, it feels as if it is organized for the benefit of the writer, rather than the reader. Perhaps instead you could give the reader more context as to what they are about to read.

Instead of:

"1.3 This paper In this paper we wish…"

try,

"1.3 Programmatic Goals

We wish to provide some first steps…"

Thank you for pointing this out. We have implemented the changes you have suggested.

Page 5:

L 1-2:

"In the present manuscript, we explore for the first time the Lyapunov spectra of a primitive-equation model, PUMA, and of the intermediate-order coupled ocean- atmosphere system, MAOOAM."

I believe the Lyapunov spectra of MAOOAM was already studied to some degree in Vannitsem and Lucarini (2016), the wording here makes it sound like the authors are claiming it is explored here for the first time. Perhaps reword, for example:

"In the present manuscript, we explore the Lyapunov spectra of the intermediate-order coupled ocean-atmosphere system MAOOAM, and for the first time, of the primitive-equation model PUMA."

The Lyapunov spectra of MAOOAM (more precisely of its predecessor, VDDG; see Vannitsem et al. 2015) have indeed been studied before, but only for the low-order model configuration of 36 variables. It is, however, the first Lyapunov analysis of MAOOAM model configurations with hundreds of variables. We have changed the sentence as follows to clarify this:

"In the present manuscript, we explore for the first time the Lyapunov spectra of **the** primitive-equation model PUMA, and of **intermediate-order configurations of** the coupled ocean-atmosphere system, MAOOAM."

Page 6:

L 1:

For consistency, I suggest to change the order of the listed prognostic variables to match the order presented in the equations 1,2,3,4 below.

The sentence has been adapted to match the order of the equations: "The prognostic equations as written in PUMA's code have four prognostic fields, the relative vorticity η, the divergence D, **the logarithm of the surface pressure** $\ln p_s$ **and the temperature** T."

Page 8:

Table 1: Typo: "surface pressure pressure"

This has been corrected.

Page 10:

L 1:

"in a synthetic form"

What is synthetic about this? Perhaps you could just say,

"as a dynamical system"

The sentence has been adapted as suggested.

L 25:

"2. Every time step, the model propagator is computed from the tangent linear model. This is the matrix that quantifies the transition from one model state into that one time step later."

This could be worded more clearly. Please clarify the definitions of the resolvent matrix, model propagator, and tangent linear model, and make sure to use the terms consistently for the remainder of the text.

Indeed, this was worded ambiguously. We have adapted the text as follows. On P 10 L 13, have added the words in bold: "[...] the matrix $\mathbf{M}$ is referred to as the resolvent matrix **or propagator**."

Furthermore, we have clarified step 2 of the algorithm as follows:

"2. At every time step t_i, a matrix $\mathbf{P}_i$ that represents the linear propagator from t_{i-1} to t_i is computed using the tangent linear model along the model state trajectory. $\mathbf{P}_i$ is the equivalent of the matrix $\mathbf{M}$ for a finite time difference $t_i - t_{i-1}$. We take into account the numerical integration scheme when computing $\mathbf{P}_i$, by evaluating the model Jacobian at all intermediate points of the scheme. We have implemented the second- and fourth-order Runge-Kutta schemes, which require two and four evaluations of the Jacobian per time step, respectively."

"3. The model is integrated forward in time, and the propagators are accumulated (multiplied) into a matrix P"

It seems the more general procedure would be to integrate the linear propagator (e.g. using a geometric integrator / Magnus Expansion), but that this 'accumulation' via multiplication serves as an approximation. Perhaps you could be more precise about this statement.

We have changed the description of step 3 as follows:

"3. As the model is integrated forward from time t_i to t_{i+b}, the corresponding linear propagator $\mathbf{P}_{i,i+b}$ is approximated by multiplying the b matrices, $\mathbf{P}_{i,i+b} = \mathbf{P}_{i+b} \dots \mathbf{P}_{i+1}$. In the experiments that follow, we have chosen $b = 1$."

Page 11:

L 5-6:

It seems odd to me that you cite a different author than Kaplan and Yorke for the Kaplan-Yorke dimension.

Kaplan, J. L. and Yorke, J. A. In Functional Differential Equations and Approximations of Fixed Points: Proceedings, Bonn, July 1978 (Ed. H.-O. Peitgen and H.-O. Walther). Berlin: Springer-Verlag, p. 204, 1979.

Thank you for pointing this out. This reference has been corrected.

L 18: "Finite-time Lyapunov exponents (FTLEs)" You have already used this acronym before defining it here.

We have moved the definition to the first occurrence of the acronym.

" (e.g. Haller, 2000)" Perhaps you should instead cite one of the originators of the idea of FTLEs, e.g. Abarbanel, H. D. I., R. Brown, and M. B. Kennel, "Variation of Lyapunov Exponents on a Strange Attractor,"Journal of Nonlinear Science,1, 175–199 (1991).

Indeed. We have replaced the reference to Haller by the references suggested by you and the other referee:

- H. Fujisaka, Progress of Theoretical Physics 70, 1264 (1983)
- H.D. Abarbanel et al., Journal of Nonlinear Science 1, 175–199 (1991)

L 22:

"If a dynamical system is an Axiom A system or –invoking the chaotic hypothesis – one of a certain type of non Axiom A systems, these fluctuations for a finite, but large M may be described (based on (Schalge et al., 2013; Pazó et al., 2013; Laffargue et al., 2013)) by a large deviation law (Kifer, 1990; Touchette, 2009)." This sentence is a bit clumsy. Perhaps you could reword or break into two sentences to make it easier to read.

Indeed, this sentence is hard to read. We have rewritten it as follows:

"As discussed in (Schalge et al., 2013; Pazó et al., 2013; Laffargue et al., 2013), some dynamical systems have the property that for a finite, but large M, the fluctuations of their FTLEs can be described by a large deviation law (Kifer, 1990; Touchette, 2009). This is the case for Axiom A systems, and invoking the chaotic hypothesis, extends to certain types of non Axiom A systems."

L 29: Make the definition of I() on its own line and given an equation number.

We have changed this as suggested.

Page 13: I'm not sure that I understand the table caption: "Common parameter values for the different model configurations of MAOOAM." There is only one value given for each parameter. Do you mean, "Model parameter values that are identical across all MAOOAM configurations used in this study"?

Indeed, this is what we meant. We have changed the table caption accordingly.

Page 16:

L 6: "consequence of the non-existing clear-cut time-scale separation" Please find another way to say this.

We have adapted the original sentence:

"We interpret these results as another consequence of the non-existing clear-cut time-scale separation in a purely atmospheric model like PUMA."

as follows:

"**We interpret these results to stem from the lack of a** clear-cut time-scale separation in a purely atmospheric model like PUMA."

It would be nice if you could elaborate somewhere how you define the 'timescale' and units of the Lyapunov exponents, how you expect that to influence the prediction range, and explicitly how you expect these scales to map to different spatial scale instabilities. It seems to be mentioned in passing in a few places, but it would be helpful to summarize in one place before going into the results.

Thank you for this remark. We have added the following paragraph, just after the definition of the Lyapunov exponents:

"If one or more LEs are positive, small errors on the initial conditions of the system grow exponentially and the system is chaotic. In that case, the time horizon of the system's predictability is proportional to the inverse of the largest Lyapunov exponent, $\frac{1}{\lambda_1}$. As this predictability horizon is expressed in days for operational forecasting, we also express the exponents λ_i in units day^{-1}. To translate the spectrum of LEs into spatial scales of the instabilities in an unambiguous way, the CLVs must also be determined. However, if there is scale-dependent dissipation, the largest negative LEs are likely to be associated with the smallest, most dissipative scales."

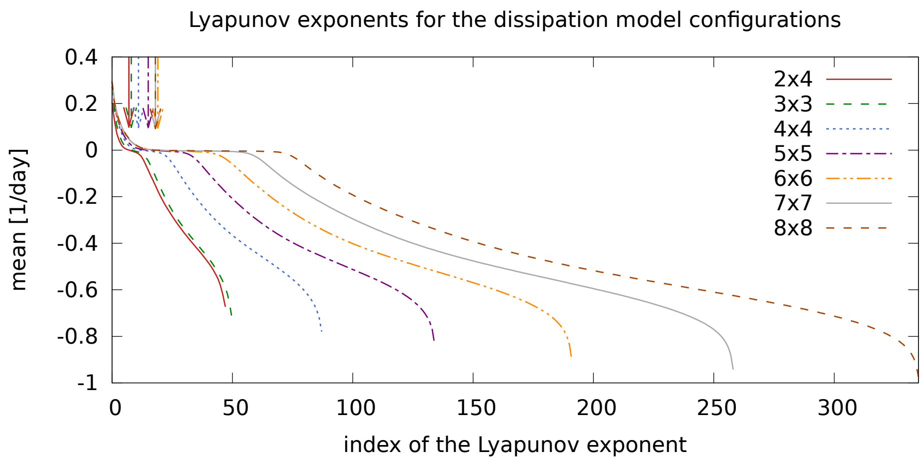

Figure 1. Lyapunov spectra of MAOOAM without ocean dynamics or in "slab ocean" mode, for model configurations from atm. 2x4, oc. 2x4 (red full line) up to atm. 8x8, oc. 8x8 (brown dashed line). Lyapunov exponents are ranked in decreasing order, and the index of the smallest positive Lyapunov exponent is indicated with a downward-pointing arrow for each model configuration.

Page 18:

"The highly populated central manifold of MAOOAM is in stark contrast with the few near-zero LEs in PUMA. Being a purely atmospheric model, PUMA's Lyapunov spectrum does not exhibit the large time-scale separation present in MAOOAM. Indeed, the spectrum of PUMA bears more resemblance to that of the QG two-layer model of Schubert (2015)."

I'm curious if the authors have run their MAOOAM model in a forced-atmosphere and forced-ocean mode and computed LEs in order to demonstrate that the central manifold is largely eliminated without active coupling?

The Lyapunov spectra of forced-atmosphere models such as those of Charney and Straus (1970) and Marshall and Molteni (1993) have been computed, for example, in: Vannitsem, "Predictability of large-scale atmospheric motions: Lyapunov exponents and error dynamics", Chaos 27, 032101 (2017). In this case, the spectrum does not display the large set of near-zero exponents.

We have performed some additional simulations without ocean dynamics, the results of which are shown in Fig. 1. The structure of the spectrum is similar to the one presented in the manuscript, and is associated with the exchange of energy between the ocean (a thermal bath) and the atmosphere.

No ocean-only experiments have been done with MAOOAM because it would imply that (i) temperature will no longer play a role in the dynamics (since it is just a passive scalar) and (ii) one must impose some specific wind stress forcing. This question was investigated using the low-order coupled ocean-atmosphere model OA-QG-WS v2, a predecessor of MAOOAM, in Vannitsem, "Stochastic modelling and predictability: analysis of a low-order coupled ocean–atmosphere model", Phil. Trans. R. Soc. A 372.2018, 20130282 (2014). There, the ocean system was found to converge towards a constant value when a constant surface forcing, consistent with the fully coupled dynamics, is applied.

L 9:

“The additional positive and near-zero exponents that are introduced at these scales nevertheless indicate that the added resolution still resolves some scales that are important for the description of the dynamics. ”

This implies that the number of positive LEs should asymptote as the resolution reaches a level to capture all relevant scales. Is this the expectation?

Yes, this is expected when the resolution suffices to capture all relevant dynamics.

Page 19:

L 4:

“for the models [that] do not include”

Fixed.

Page 20:

L 8:

“The experiments [that] take this”

Fixed.

Page 25:

L 3-8:

I think this is an incredibly important passage, and should be investigated further to guide the development of coupled atmos/ocean systems.

This is indeed an important and counterintuitive result, especially since forced-ocean models appear to be quite stable under a constant forcing. We have added Fig. 2 to the manuscript, and have added the sentence:

“Indeed, the quantity D_{KY}/N, which approximates the relative fraction of the attractor’s dimension, increases for increasing ocean resolution, but decreases for increasing atmosphere resolution, as illustrated in Fig. [2]. ”

Page 28:

“The source code to compute the Lyapunov exponents is available upon request to the corresponding author.”

Please either include it as part of the supplemental material or make is available, for example, as part of the package: http://github.com/Climdyn/MAOOAM

We have created a branch in the MAOOAM git repository and tagged the version **v1.3.1-lyapunov** that was used to perform the simulations in this manuscript. This version is archived on Zenodo (https://doi.org/10.5281/zenodo.1198650).

The code availability section concerning MAOOAM has been adjusted to:

“The source code for the latest version of MAOOAM is available at http://github.com/Climdyn/MAOOAM. The version of MAOOAM that was used to compute the Lyapunov exponents is archived at https://doi.org/10.5281/zenodo.1198650.”

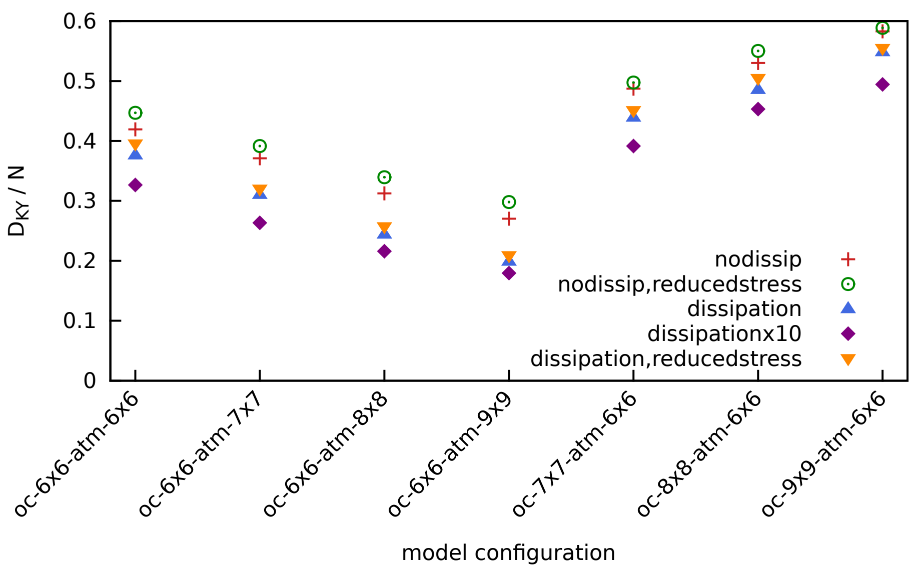

Figure 2. Kaplan-Yorke or Lyapunov dimension D_{KY} of MAOOAM divided by the total number of dimensions N, as a function of the model configuration.