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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Exploring the Lyapunov instability properties of high-dimensional atmospheric and climate models" *by* Lesley De Cruz et al.

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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

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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 centers 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.

L 11:

Missing period at the end of the line.

L 30:

This paragraph should start with a "However,"

L 32:

" for atmospheric instabilities, and most notably convective"

Remove the word 'and'

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.

Page 3:

L 26:

"corresponding to positive [and neutral] Lyapunov exponents"

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..."

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-

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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."

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.

Page 8:

Table 1: Typo: "surface pressure pressure"

Page 10:

L 1:

"in a synthetic form"

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

"as a dynamical system"

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 ma-

trix, model propagator, and tangent linear model, and make sure to use the terms consistently for the remainder of the text.

"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.

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.

L 18:

"Finite-time Lyapunov exponents (FTLEs)"

You have already used this acronym before defining it here.

" (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).

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.

L 29:

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

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"?

Page 16:

L 6:

"consequence of the non-existing clear-cut time-scale separation"

Please find another way to say this.

—

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.

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?

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?

Page 19:

L 4:

"for the models [that] do not include"

Page 20:

L 8:

"The experiments [that] take this"

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.

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

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