Nonlin. Processes Geophys. Discuss.,
https://doi.org/10.5194/npg-2017-76-RC1, 2018
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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.

Anonymous Referee #1

Received and published: 5 February 2018

This manuscript presents a Lyapunov analysis of two models: PUMA (purely atmospheric) and MAOOAM (atmosphere-ocean coupled). The purpose of this is to investigate the impact of different configurations (resolution, dissipation, and atmosphere-ocean coupling) on the instabilities of the system. This is an interesting path of research since, ultimately, it will permit to understand better the goodness of a certain model for forecasting certain processes operating at particular spatio-temporal scales.

While I consider the topic of this work is worth of being published, and the manuscript reasonably well written, I have one important criticism on the methodological procedure that prevents me from recommending its publication.

An important part of the manuscript is devoted to the rate function of the finite-time Lyapunov exponent (FTLE) distribution. As claimed in the abstract

1) For the PUMA model: "The convergence rate of the rate function(al) for the large deviation law of the FTLEs is fast for all exponents".

2) For the MAOOAM: "[...] it is possible to robustly define large deviation laws describing the statistics of the FTLEs corresponding to the strongly damped modes, [...]"

My main criticism is the meaningfulness of the rate function analysis considering the data used and their quality. I itemize next my concerns (a-c):

a) In all cases the claimed convergence is far from apparent with the naked eye. In my view the rate functions vary consistently as parameter tave changes, but I don't see a true convergence of the data as such. I find very questionable sentences like "For all LEs the tendency for convergence of the rate function is visible" in page 16, line 3; and "...FTLES accurately obey large deviation laws ..." in page 27. To judge the convergence from the shift of a whole curve as a parameter changes is really problematic.

b) The data analysis resorted to a strong smoothing of the data obtained from short time series, cf. the histograms. It is difficult to evaluate the errors accumulated thereby.

c) From a theoretical point of view, I have doubts that the rate function can be detected with the time intervals over which the FTLEs are computed ("tave"). I'm afraid that the values of "tave" used are simply too small to reach the asymptotic rate function, even if the time series were infinitely long. From a theoretical perspective the choice of a reference time "T", based on the time interval in which autocorrelations of the FTLE decay below 1/e, presents certain problems. The rate function is calculated for times only up to 28*T, what may be too small to detect the rate function (independently of the amount of data). I say this because in many spatio-temporal chaotic systems the "renewal time" of the Lyapunov vector operates at a time scale T_x much larger than T.

In such a case, in order to detect the rate function one must take time intervals (tave) larger than T_x (> > T), see Pazo et al (2013). For instance, in Laffarge et al (2013), with 40 coupled maps the time interval (tave) used to measure the rate function is 10ˆ4 iterations.

The only way I see to demonstrate something unambiguously with the numerical data at hand is to check the convergence of a particular quantity (instead of a curve). Following Pazo et al (2013), and as double-check, I suggest to measure the variance of the FTLE for different "tave" values. Multiplying by tave, it should be possible to verify if the data level off at a certain value D in the range of tave values considered. (The diffusion coefficient D is the inverse of the second derivative of the rate function at its minimum, see e.g. (Kuptsov and Politi, 2011)).

Minor comments:

1. Eq. (19), please mention that Mˆ* is the adjoint of M. Note also that M has a wrong font type in Eq. (19).

2. Letter M is used for the resolvent matrix and for the integer (tave= T*M). I would avoid this duplicity.

3. The concept backward Lyapunov exponent appears in page 10, line 22, without much explanation. Note that Lyapunov exponents obtained from Eq. (19) are actually "forward Lyapunov exponents". The mirror definition of Osedelets theorem with M Mˆ*, instead of Mˆ* M, yields backward LEs. I point to table 1 in Pazo et al (2010) and to (Ershov & Potapov, 1998. On the concept of stationary Lyapunov basis. Physica D 118(3-4), 167–198.) for the formal link between Osedelets theorem and Bennetin's algorithm.

4. Page 11. The relationship of the KY dimension with the fractal dimension was confusing for me as written now (probably due to the intention of making it simple for the unfamiliar reader). The KY dimension is an estimation of the information dimension,

usually denoted D_1. D_1 is known to be (equal or) smaller than the capacity or box-counting dimension (D_0), which I guess is what the authors refer to by "fractal dimension", following the paper by Frederickson et al (1983). My taste is that nowadays one can talk of D_KY as estimation of D_1, and perhaps to mention the information dimension bounds the capacity/fractal dimension (for the unfamiliar reader on these questions).

5. Page 11, when introducing the FTLEs the authors refer to Haller (2000). I have nothing against, but I think it is more appropriate H. Fujisaka, Prog. Theor. Phys. 70, 1264 (1983)

6. Page 15, line 1, I think the use of "much smaller" is exaggerating the difference between the spectra. Using "smaller" is enough.

7. Page 15, line 4, when mentioning the Tibaldi-Molteni index, why is not the paper of Tibaldi and Molteni (1990) cited instead?

8. It should be said somewhere that Fig. 1 shows (only) the 200 largest LEs.

9. In Fig. 1, it looks like the Lyapunov index starts at 0, instead of 1. I guess the lines have to be displaced 1 unit rightwards.

10. Page 15, line 9, "faster" -> "fast" (?)

11. In Fig. 2 it is not said which kernel function is used, or how the bandwidth was optimized.

12. Figs. 4,5 the x-tic labels overlap.

13. Figs. 11-14. I'm curious how the Lyapunov spectra look like when the x-coordinate (the Lyapunov index) is rescaled by the number of degrees of freedom. Is there some overlapping of the data for the most negative LEs?

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