Author’s Response to Anonymous Referee #1’s Comments on “Exploring the Lyapunov instability properties of high-dimensional atmospheric and climate models”

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The authors thank the referee for their careful reading of the manuscript and for providing helpful recommendations and pertinent remarks. Please find our response to each of your questions or remarks below.

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.

Indeed, the graphs shown in the manuscript do not unambiguously prove the convergence of the rate functions. We have therefore moderated these statements as follows. Before, the relevant passage on page 16 read:

“We make the following observations. For all LEs the tendency for convergence of the rate function is visible. Also, the rate functions’ shape is approximately parabolic and the estimates of the rate functions converge to the asymptotic with a comparable speed regardless of the value of the corresponding LE. ”

This has been adapted as follows:

“We make the following observations. **The graphs suggest a convergence of the rate function for all LEs.** Also, the rate functions’ shape is approximately parabolic and the estimates of the rate functions **appear to** converge to the asymptotic with a comparable speed regardless of the value of the corresponding LE. ”

The following sentence on page 25, line 10:

“ Similarly to what was found in a previous analysis performed on a severely truncated version of MAOOAM (Vannitsem and Lucarini, 2016), we find that the time series of the FTLEs corresponding to the strongly damped mode are weakly correlated and one can construct the rate functions defining the large deviations laws; compare figures 19a-c) for the 351 st LE.”

has been corrected and now reads:

“Similarly to what was found in a previous analysis performed on a severely truncated version of MAOOAM (Vannitsem and Lucarini, 2016), we find that the time series of the FTLEs corresponding to the strongly damped modes are weakly correlated. **This would suggest that one can construct the rate functions defining the large deviations laws. The rate functions are shown in Fig. 20a-c) for the 351st LE. Their convergence properties are investigated in Appendix A2, and indicate that we have not yet converged to the central limit theorem even for these strongly damped modes.**

The sentence on page 27, line 7:

“ [. . .] one finds that all the FTLEs accurately obey large deviation laws [. . .]”

has been adapted and now reads:

“ [. . .] **the results suggest** that the FTLEs obey large deviation laws [. . .]”

Furthermore, the paragraph on page 27, lines 24-27:

“The analysis of the FTLEs of MAOOAM reveals some interesting insight into the dynamics. The FTLEs associated to the strongly dissipative modes obey large deviation laws, while those corresponding to the near-zero LEs do not. This behaviour is expected, and in agreement with what was found in Vannitsem and Lucarini (2016). Surprisingly, however, it is hard to find convergence for the FTLEs associated to the positive LEs. This may point to the presence of nontrivial ocean influence on the (mostly) atmospheric instabilities.”

now reads:

“The analysis of the FTLEs of MAOOAM reveals some interesting insight into the dynamics. Surprisingly, **it is hard to find convergence for the rate functions of the FTLEs, even for those associated to the positive LEs**. This may point to the presence of nontrivial ocean influence on the (mostly) atmospheric instabilities.”

Finally, the sentence from the abstract:

“In all considered configurations, it is possible to robustly define large deviations laws describing the statistics of the FTLEs corresponding to the strongly damped modes, while the opposite holds for near-zero LEs and, somewhat unexpectedly, also for the positive LEs.”

has been adapted and now reads:

“In all considered configurations, **we are not yet in the regime in which one can robustly define large deviations laws describing the statistics of the FTLEs**.”

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.

Smoothing or binning the data is an unavoidable step in order to evaluate the PDF. For this we have employed the kernel density estimation method of Scott (1979; see below) which is optimised to rapidly converge to the true underlying distribution.

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 10^4 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)).

As suggested, we have computed σ for a range of different averaging block lengths t_{ave} to verify the expected scaling behaviour. The results are shown in Fig. 1 for the PUMA model. Furthermore, we have compared the value of $\sigma^2 \cdot t_{ave}$ to the diffusion coefficient D. The results for PUMA are shown in Fig. 2, and suggest that the value of $\sigma^2 \cdot t_{ave}$ appear to converge. While the value of D fluctuates, it has the right order of magnitude.

The corresponding graphs for MAOOAM are shown in Figs. 3 and 4. Both the values of D and $\sigma^2 \cdot t_{ave}$ vary as a function of t_{ave}, indicating that much longer integration times are required than the 30 years used here to investigate the rate function. A discrepancy between D and $\sigma^2 \cdot t_{ave}$ is apparent for LE 100 in all experiments shown in Fig. 4. This can be explained by the extremely long decorrelation times for these near-zero LEs, due to the multiscale properties of the system.

These results have been added to the manuscript as an Appendix.

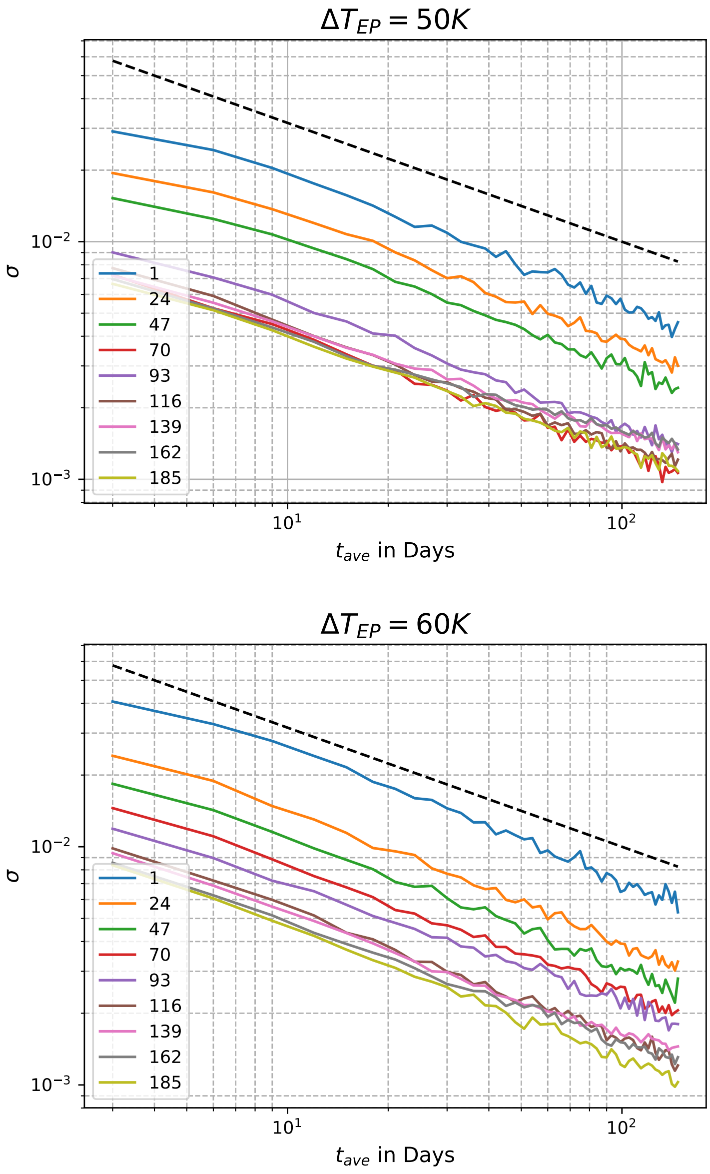

Figure 1. Standard deviation σ as a function of the block averaging length t_{ave} for different Lyapunov exponents of the PUMA model. The Lyapunov index is shown in the legend. The top panel shows the results for a temperature gradient ΔT_{EP} of 50K, the bottom panel for 60K. The black dashed line corresponds to $t_{ave}^{-\frac{1}{2}}$ scaling.

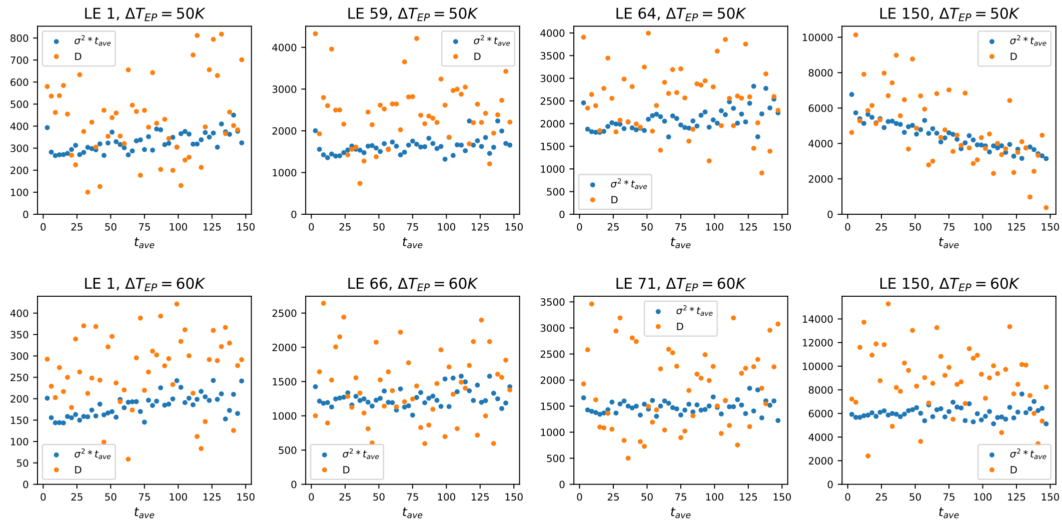

Figure 2. The metric $\sigma^2 \cdot t_{ave}$ versus the diffusion coefficient D, derived from the inverse of the second derivative at the minimum of the rate function, as a function of the block averaging length t_{ave} for different Lyapunov exponents of the PUMA model. The Lyapunov index is shown in the title. The top panels show the results for a temperature gradient of 50K, the bottom panels for 60K.

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

We have corrected the font and added the following sentence after Eq. (19):

"where $\mathbf{M}^*$ is the adjoint of $\mathbf{M}$."

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

We have replaced the letter M by the lowercase letter m.

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 M^*, instead of M^* 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.

Indeed, the algorithm we have used produces the backward Lyapunov exponents. The order of the matrices in Eq. (19) has been corrected and now reads $\mathbf{M}\mathbf{M}^*$. We have also adapted the sentence:

"The *Lyapunov exponents* are then defined as the natural logarithm of the eigenvalues of Λ_{x_0}."

This sentence now reads:

"The **backward** *Lyapunov exponents* **(Ershov and Potapov, 1998; Pazó et al., 2010)** are then defined as the natural logarithm of the eigenvalues of Λ_{x_0}."

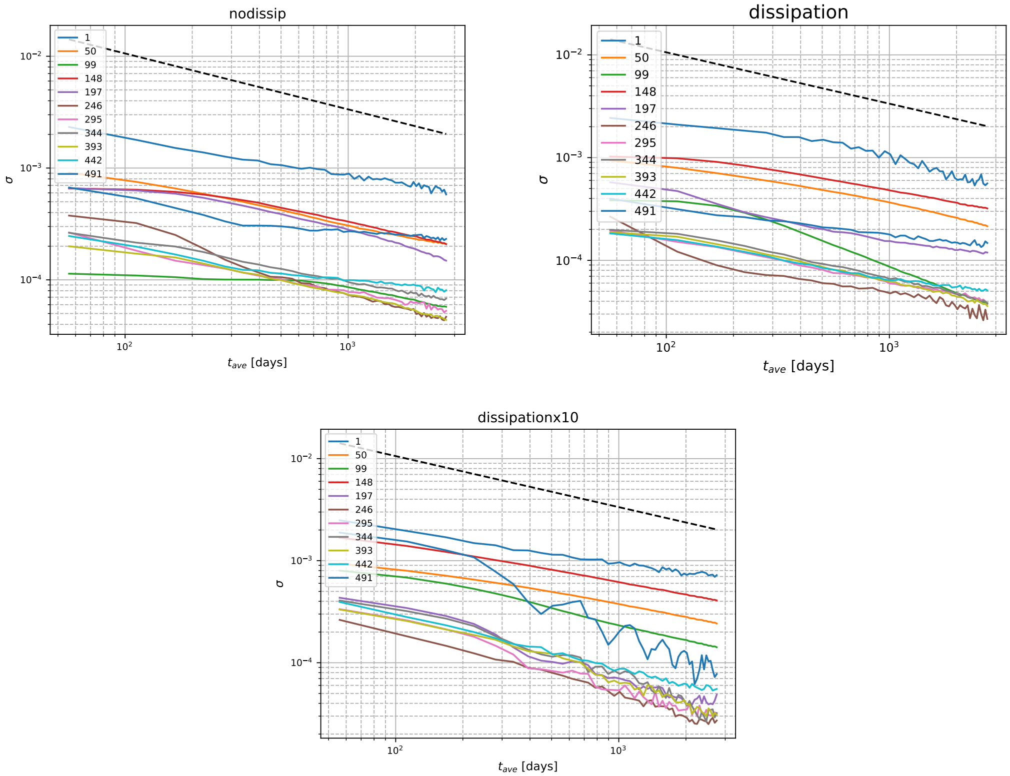

Figure 3. Standard deviation σ as a function of the block averaging length t_{ave} for different Lyapunov exponents of the 9x9 configuration of the MAOOAM model. The Lyapunov index is shown in the legend. The top left panel shows the results for the experiment without scale-dependent dissipation, the top right panel corresponds to the reference value for dissipation and the lower panel shows the enhanced dissipation results. The black dashed line corresponds to $t_{ave}^{-\frac{1}{2}}$ scaling.

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).

Thank you for pointing out this ambiguity. The original version read:

"[. . .] D_{KY}, which provides (a lower bound on) the fractal dimension of the attractor, and is defined as (Frederickson et al., 1983): [. . .]"

We have adapted this following your suggestion:

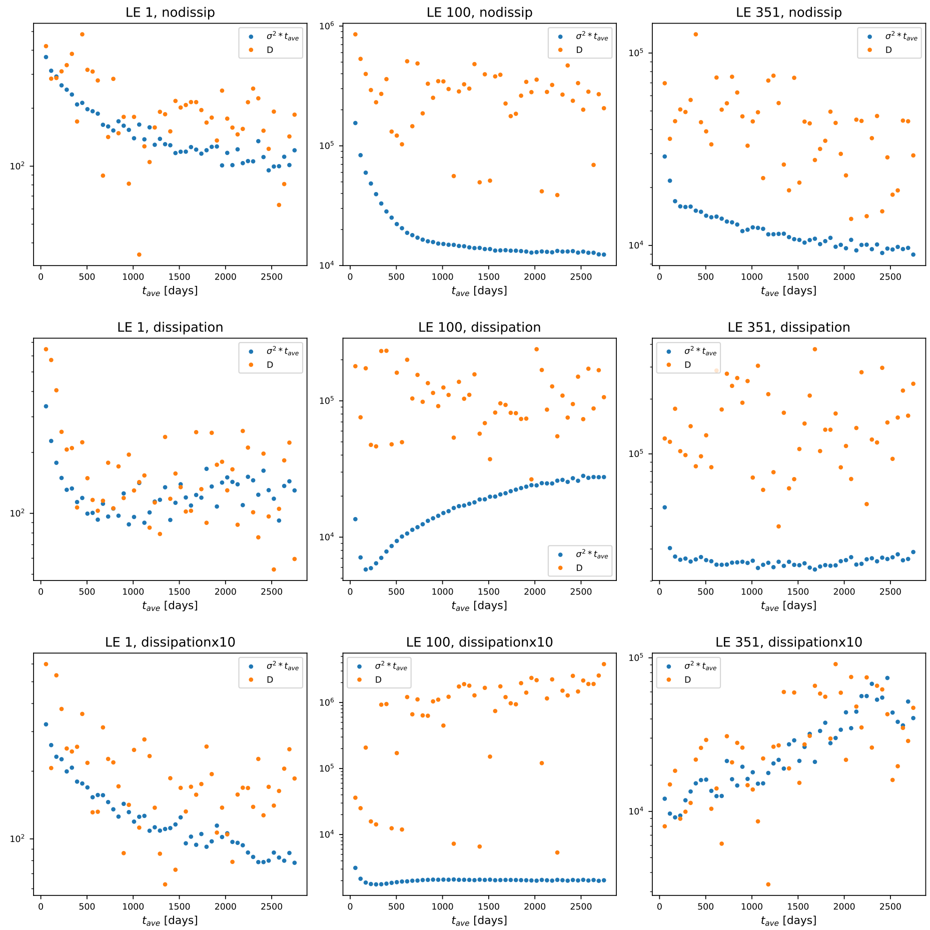

Figure 4. The metric $\sigma^2 \cdot t_{ave}$ versus the diffusion coefficient D, derived from the inverse of the second derivative at the minimum of the rate function, as a function of the block averaging length t_{ave} for different Lyapunov exponents of the 9x9 configuration of the MAOOAM model. The Lyapunov index is shown in the title. The top panels show the results for the experiment without scale-dependent dissipation, the centre panels correspond to the reference value for dissipation and the lower panels show the results for an enhanced dissipation coefficient.

“[...] D_{KY}, which **is an estimation of the information dimension D_1. D_1 is known to be less than or equal to the capacity or box-counting dimension D_0 , also referred to as the fractal dimension (Frederickson et al., 1983).** D_{KY} is defined as **(Kaplan and Yorke, 1979)**: [...]”

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)

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)

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

Indeed, we have corrected the wording accordingly.

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

Thank you for pointing this out. The reference has been adapted accordingly.

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

We have adapted the sentence by adding the text in boldface:

“Figure 1 shows **the 200 largest LEs of** the two different Lyapunov spectra obtained in our experiments with PUMA.”

The caption of Fig. 1 has been adapted accordingly:

“The **200 largest LEs of the** Lyapunov spectra of PUMA for the two different setups with $\Delta T_{EP} = 50K$ and $60K$.”

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.

We have recreated the graph with the index starting at 1 instead of 0.

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

Indeed, “faster decaying LE 150” is now replaced by “**fast-decaying** LE 150”.

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

The kernel function used was the one by Scott, D., "On optimal and data-based histograms", Biometrika 66 (3): 605–610, doi:10.1093/biomet/66.3.605 (1979). We have added this reference and adapted the sentence:

“The top panels of these figures show the approximation of the respective distributions obtained via kernel smoothing of the distribution of the block-averaged LEs. ”

It now reads:

“The top panels of these figures show the approximation of the respective distributions obtained via **kernel density estimation** (Scott, 1979) of the distribution of the block-averaged LEs. ”

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

Thank you for pointing this out, this has been corrected.

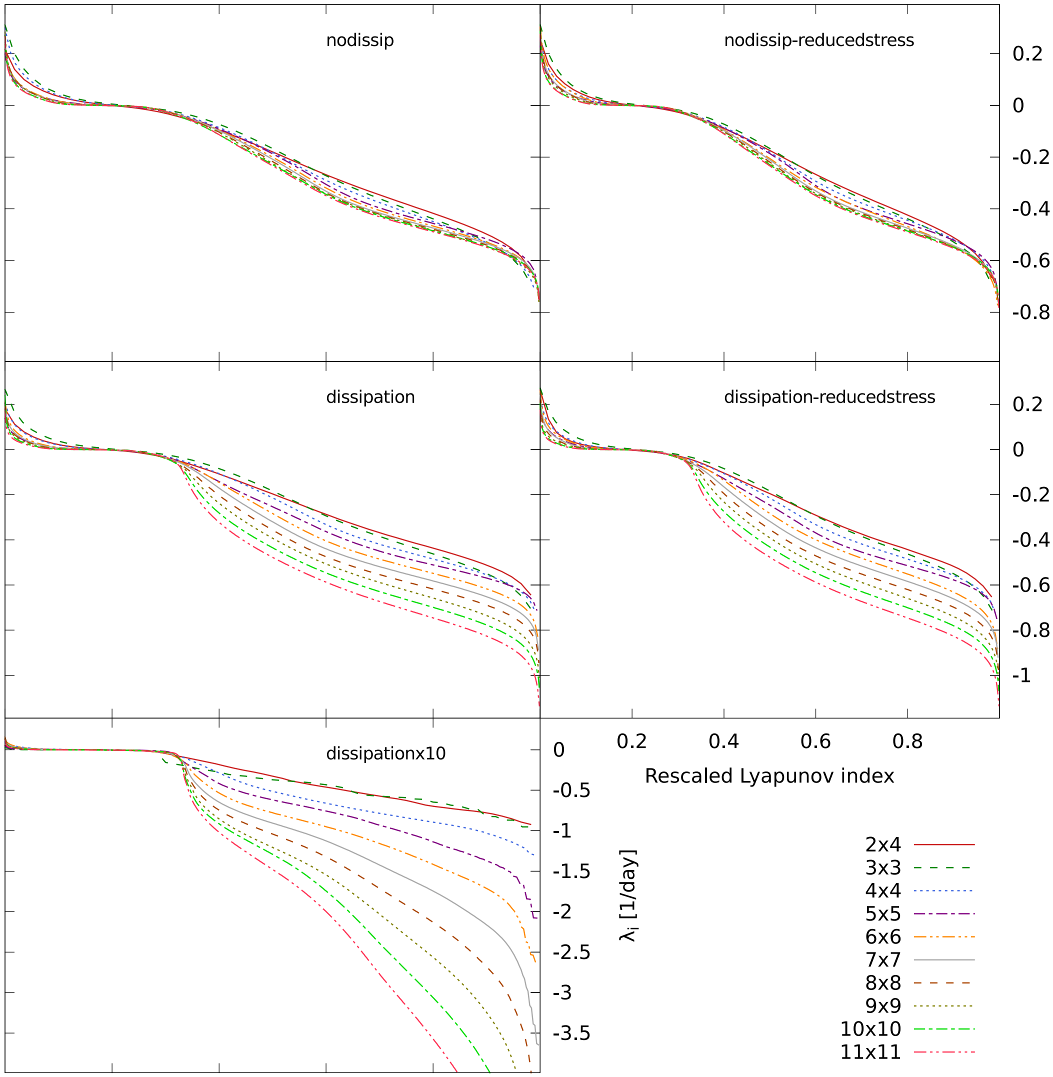

Figure 5. Lyapunov spectra for the different MAOOAM experiments as a function of the *rescaled* Lyapunov index i/N.

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?

The Lyapunov spectra for MAOOAM are plotted as a function of the rescaled Lyapunov index λ_i/N in Fig. 5. The different regions do seem to overlap. The values of the most negative LEs only overlap in the experiments with *scale-independent* dissipation ("nodissip").