Interactive comment on "Fluctuations of finite-time Lyapunov exponents in an intermediate-complexity atmospheric model: a multivariate and large-deviation perspective" by Frank Kwasniok

The manuscript has been improved with respect to the previous version. The results are interesting and new. However, the presentation is still messy at several points. In my opinion, the manuscript could be publishable after substantial changes. I provide the details below.

A. Large deviation theory

The presentation of the large deviation theory is a mess, specially considering the presentation of the block averaging method. Is not enough to say that for large enough τ FTLEs are virtually uncorrelated?

I think it is much simpler to write a large deviation formula for the j-th exponent

$$P(\Lambda_j^{(\tau)} = z) \sim e^{-\tau I_j(z)}$$

with rate function

$$I_j(z) = \sup_\theta [\theta z - \gamma_j(\theta)]$$

and

$$\gamma_j(\theta) = \lim_{\tau \to \infty} \frac{1}{\tau} \ln \left\langle e^{\tau \theta \Lambda_j^{(\tau)}} \right\rangle$$

That's all. Equations (35) and (36) in the text are recovered defining $\theta' = \tau\theta$, but they are much more difficult to understand. After all the calculations in the manuscript, Eq. (36) presents a rate function that apparently depends on τ; while it is claimed it doesn't in the previous sentence (!). (This also applies to Eq. (46).)

B. Inertial manifold

At the end of Sec. 6.2 the new paragraph on the inertial manifold is not correct. It points to a dimensionality of the inertial manifold (≈ 125) much smaller than the dimensionality of the attractor obtained by the Kaplan-Yorke formula. This is not possible: the dimension of the attractor is necessarily smaller than the dimension of the inertial manifold.

C. Correlation length

1. The formula for the correlation length in Eq. (57) is puzzling. Where it comes from? what hypotheses have been assumed?

2. The text suggests that the crossover τ for convergence to the large-deviation regime is related to the correlation time of the FTLE. I disagree with this interpretation for two reasons. First, the correlation lengths observed are not much different in comparison with the crossover τ's. Second, in spatially extended systems it is typical that correlation times are insensitive to the system size, while crossover τ scales with the system size as L^z, $z > 0$. This indicates a different operate in each case (Pazó et al, 2013).

D. Legendre transform method

I encourage the author to emphasize in the conclusions that the Legendre transform approach appears to be superior to the probability density approach in order to find the rate function (assumed the conditions of the Gärtner-Ellis theorem hold). Moreover the Legendre transform method yields diffusion coefficients fully consistent their direct measurement from the data.

E. Figures

1. In Figs. 2(c) and 2(d) the factor $\tau^{1/2}$ should appear in the y-axis label.
2. I have a suggestion, which can be followed or not: In Figs. 3 (a) and 3(b), wouldn't it be clearer to set log scale for the y-axis?
3. Figure 4 is more clear writing $y_j^{(\tau)}$ in the y-axis label.
4. Contrary to what is claimed in the author's response, the labels '(a), '(b)', etc. are not included in the revised ms.

F. Typos

1. Page 17, line 8, $n \to L/n$.
2. Page 21, line 4, to $\to$ too.