Responses to Reviewers' Comments

The author would like to thank both reviewers very much for their useful comments which helped improve the manuscript.

Reviewer 1

- Section 2
  Q:
  In the model, if I correctly understand, the "h" term is only a parameter (not a function). If so, I think the model is invariant under a zonal shift. This neutral transformation would imply a second vanishing Lyapunov exponent in the spectrum. Please, clarify this point.
  A:
  This is a misunderstanding; the model has no zonal symmetry. The topography $h = h(\lambda, \mu)$ is a function of space and is actually the real topography of the earth expanded into spherical harmonics. Also the diabatic source terms $S_i = S_i(\lambda, \mu)$ are functions of space, fitted from reanalysis data. So any symmetry is broken in the model and this is actually crucial for getting a realistic mean state and variability. This is clarified in the revised manuscript.

- Section 6.1
  Q:
  Concerning the last sentence in Sec. 6.1. I note that the equivalence between Lyapunov exponent fluctuations measured from Gram-Schmidt vectors and from covariant vectors, was detected already in Figs. 5 and 7 of Ref. [1].
  A:
  This is acknowledged in the revised manuscript.

  Q:
  In fact, the large fluctuations observed at the edges of the spectrum are not really surprising, at the light of the previous results on the diffusion coefficients in (Kuptsov and Politi, 2011) and [1].
  Q:
  This is referenced in the revised manuscript.

- Section 6.2
  Q:
  It is absolutely necessary to include one formula defining the fraction of explained variance, in order to ensure the self-consistency of the text.
  A:
  Done.

- Section 6.3
  Q:
  It is not said which is the total length of the time series used.
  A:
  The length of the time series is 25000 days; this information is now given in the revised manuscript.

Q:
The value of τ_r is "hidden" in Sec. 3.
A:
The value of τ_r is now again given in the results section.

Q:
The three methods used to measure $D_{j,j}$ are not fully clear to me. I think the author should make a list with the three methods specifying which formulas are used in each one. And which parameters are used. Now the explanation is hidden in the caption of Fig. 5, and is hardly understandable.
After reading it several times ...
A:
This part of the manuscript has been substantially revised. The different methods for estimating the rate function and the elements of the diffusion matrix are now explained more clearly and in more detail (see revised manuscript).

- Minor comments:
  - Q:
    Two lines after Eq. (13), I would write scalar product instead of norm, because this is what matters for Gram-Schmidt orthogonalisation. I suppose the energy norm is trivially related to the scalar product used.
    A:
    Corrected.
  - Q:
    Vectors should be always typed in bold face, also for Greek letters.
    A:
    Corrected.
  - Q:
    Orthogonality of the eigenvectors, Eq. (18), is better written after Eq. (16).
    A:
    Done.
  - Q:
    The equation in the text preceding Eq. (18) is apparently lacking of $-\lambda$.
    A:
    Corrected.
  - Q:
    When introducing Eq. (27), it would be important to cite at least (Touchette, 2009) again and to mention this is the Gärtner–Ellis theorem (if I'm not wrong).
    A:
    Done.
  - Q:
    The "log" symbol is missing in Eq. (35).
    A:
    Corrected.
  - Q:
    Figures 4–7 should be introduced in the text, one by one.

A:
Done.

- Q:
  Last line of page 10. τ_c has not been defined.
  A:
  Corrected.
- Q:
  Page 12, line 5. I don't appreciate smaller deviations of the rate function in this case than for the zero exponent.
  A:
  For $\tau = 1$ day and $\tau = 2$ days, there is some non-Gaussianity visible for the zero exponent but not for the 200th exponent as is shown in Figure 6 in the revised manuscript.
- Q:
  In figures 5–8, I would use different colours for the lines in panel (b), at least for the coloured ones since they are not related to the same colours in panel (a).
  A:
  Done.
- Q:
  In the conclusions, it is mentioned that the most unstable exponents exhibit slower convergence to the large-deviation limit. Let me to point out that this is fully consistent with [1].
  A:
  This is referenced in the revised manuscript.
- Q:
  Labels (a), (b), etc. need to be included all the figures. This is critical in Fig. 9.
  A:
  Done.

Reviewer 2

- On section 1:
  Q:
  Line 8: based from what I understand: Could the covariance structure tell us something about how "close" or how "interactive" the various unstable and stable directions are? Could the covariance structure be related to the investigations of the inertial manifold using the angles between Lyapunov vectors. Maybe the work along the lines of Yang et al (2009) should be referenced here as a motivation.
  A:
  This is an interesting point but would clearly need the use of the covariant Lyapunov vectors. It is mentioned in the revised manuscript as a possible future research line and the work by Yang et al (2009) is referenced.

- On Section 2:
  Q:
  I think a short concise table listing all parameters of the model with their dimensional

and a dimensional values would be beneficial to introduce the model setup.
A:
Tables listing all the variables and parameters of the model have been included in the revised manuscript.

- On Section 3:
  Q:
  It should be noted that the mean of the finite time Lyapunov exponents are in fact average growth rates of linear perturbations of the system. But the finite time LEs are not directly the growth rate of those perturbation. In fact one can define backward, forward and covariant LEs. There is a good review paper on this by Kuptsov and Parlitz which explains this distinction. I think using the FTLEs of the Gram Schmidt algorithm is alright, but it should be better clarified what type of FTLEs they actually are.
  A:
  Some comments on this have been added to the manuscript and the paper by Kuptsov and Parlitz is cited. In the limit of large integration time τ, which is the focus of the present paper, the three types of FTLEs are actually equivalent and the backward FTLEs are easiest to calculate.

- On Section 4:
  Q:
  I think this section should motivate better why one should use EOFs and what would be potentially alternatives to this approach.
  A:
  A couple of comments have been added here (see revised manuscript).

- On Section 6:

  - Section 6.1:
    Q:
    Since the model is zonally symmetric there should be two zero exponents. Can you verify this and could you include this in this discussion?
    A:
    This is a misunderstanding; the model has no zonal symmetry. The topography $h = h(\lambda, \mu)$ is a function of space and is actually the real topography of the earth expanded into spherical harmonics. Also the diabatic source terms $S_i = S_i(\lambda, \mu)$ are functions of space, fitted from reanalysis data. So any symmetry is broken in the model and this is actually crucial for getting a realistic mean state and variability. This is clarified in the revised manuscript.

    Q:
    Figure 2: This result should be referenced with the findings about fluctuations of the LE for covariant, backward and forward exponents in Vannitsem, Lucarini (2016). I think when you study collective excitations this is an interesting different viewpoint.
    A:
    Done.

  - Section 6.2:
    Q:
    I think it would be helpful to present the matrix D as well as a surface plot and also

use the first EOF and second EOF to see what parts of the D matrix are actually reconstructed using the EOF method.
A:
I tried this but it turned out not to be really useful as the matrix D is strongly dominated by the diagonal and the leading EOFs do not explain that much variance, even in the diffusion limit. I have added a plot showing the correlation of selected FTLEs with all the other exponents (see Figure 5 in the revised manuscript).

- Section 7:
  Q:
  That no clear time scale separation is found is probably because QG equations are scale filtered quations. Similar results were found before in QG models (Vannitsem 1997, Schubert 2015, Vannitsem 2016).
  A:
  I agree and a comment along this line has been added.

  Q:
  Can the collective excitations be traced to any anomalous behaviour in the nonlinear background state x? I think that would an interesting addition but of course not necessary in order to do this study.
  A:
  A separate study is actually underway by the author linking FTLEs to underlying weather regimes in the model state.