Reviewer 1

Comments to the Authors

Review of "Emergence of chaos in the tropical atmosphere: Study of the weak temperature gradient system" by Vannitsem and Demaeyer, submitted to Nonlinear Process in Geophysics.

The study revisits the interesting issue on the emergence of chaos over the tropical atmosphere using the in the reduced-order model versions of the vorticity equation. By calculating the Lyapunov exponents and Kolmogorov-Sinaï entropy in the different magnitudes of n, $\chi 2$, and $\chi 5$, the emergence of chaos is analyzed. The results show that the predictability of the tropical atmosphere is low, which contrasts with traditional conclusion.

The paper is interesting and well organized. In addition, the authors have conducted the systematic analysis of the emergence of chaos under different scenarios in this study. However, some revisions are needed. Detailed comments are below.

Thank you very much for your feedback on our manuscript. As it is a letter and as we would like to keep it concise, we do not plan to make new extensive numerical experiments, but we acknowledge in the conclusion the interest of using tools related to the NLLV in future studies to clarify the impact of saturation of the error.

1. The work mainly focuses on the divergence forcing factor, and draw a conclusion that the predictability of the tropical atmosphere is not high. I think it is somewhat hasty for the conclusion. Firstly, the chaos is highly sensitive to the initial conditions. If both the initial conditions and the boundary fields are taken into account, the conclusion might be different. Secondly, the relative effects of initial conditions and boundary fields on predictability also generally vary with the timescales. At the synoptic or shorter timescales, the initial conditions dominate the predictability, while at longer timescales, the influence of boundary fields increases. So in this study, what is the timescale for the reduced-order model? Finally, the tropical atmospheric predictability is subject to modulation by the tropical ocean. The reduced-order model versions of the vorticity equation employed in the study is a purely atmosphere governing equation, with no ocean coupling? If so, the conclusion may be not solid. Overall, I recommend the authors added more explanations and clarifications in the manuscript, which will make your conclusion more rigorous.

We do agree with the reviewer. Many processes that can modify the predictability limit of the atmosphere are not incorporated, in particular the interaction with the ocean. The focus here is to have a baseline for comparison that will be further

studied with the inclusion of other processes in the future. To stress this, we added in the manuscript the following sentences in the Introduction:

"The current analysis is a first step of a systematic program of development of a hierarchy of models incorporating key processes of the tropical dynamics, specifically focusing on the role of humidity and the interaction with the ocean."

2. In the introduction, lines 21-24, the authors pointed out some works related to the nonlinearities. The Conditional Nonlinear Optimal Perturbation (CNOP) and nonlinear local Lyapunov exponent (NLLE) methods also consider the nonlinearities of dynamical systems, and have been widely applied to the tropical atmosphere. Two references are shown in the following are recommended to be listed.

   Reference:

   Duan W, Mu M, Wang B., 2004: Conditional nonlinear optimal perturbations as the optimal precursors for El Nino–Southern Oscillation events. Journal of Geophysical Research: Atmospheres, 109(D23).

   Ding, Ruiqiang, Jianping Li, and Kyung-Ja Ha., 2008: Trends and interdecadal changes of weather predictability during 1950s–1990s. Journal of Geophysical Research: Atmospheres 113(D24).

Thank you for pointing to these references. We have added a paragraph in the conclusion underlying the usefulness of those tools for future studies of the predictability limits of the tropical atmosphere:

"These steps will be taken up in a near future. Further investigations could also consider nonlinear predictability diagnostics, such as conditional nonlinear optimal perturbations (CNOP) \citep{Duan2004} and nonlinear local or finite-time Lyapunov exponents \citep{Ding2007,Ding2008}, to complement the asymptotic Lyapunov analysis presented here."

3. The authors pointes out the multiple variables in the reduced-order model versions of the vorticity equation, such as the n, $\chi 2$, and $\chi 5$. However, what are they practical physical implications? Explaining their specific physical implications would improve readability for the more readers.

Thank you for pointing out to this. The parameter n defines the aspect ration n==2Ly/Lx, with Ly=4000 km; χ_2, and χ_5 denote symmetric and antisymmetric coefficients of the prescribed velocity potential in the spectral expansion (see Appendix A). They are not separate measured meteorological variables. We clarified this in Section 2.3:

"Note that the forcing presented in the previous section is also projected on the different modes of the models, See Appendix~\ref{sec:appA} for more details about

the forcing χ decomposition. For the model version with 10 modes, two projections survive with one symmetric and one antisymmetric components, denoted χ_2, and χ_5, respectively. A similar procedure allows for getting the forcing for the 36-variable system."

4. For the symmetric component $\chi 2$ with reasonable values of forcing amplitude, the dominant Lyapunov exponent is low, suggesting a relative high predictability. However, for the asymmetric component $\chi 5$, the result is contrary. And both symmetric and anti-symmetric components of the forcing are present, the results are similar to the asymmetric forcing. So if in the real tropical atmosphere, which the practical variable corresponding to the component is more dominant? In addition, from the forcing term χ equation (11), the forcing is varying, not the constant. In this study, the component $\chi 2$ and $\chi 5$ are both constant. Do they capture the actual forcing variability of the tropical atmosphere? please clarify.

This is an interesting point. The symmetric and antisymmetric characters of the observed forcing depend on the period of the year. When the ITCZ is moving above or below the equator, the antisymmetric part dominates. This is illustrated in the paper of Trenberth et al (2000) already cited in the manuscript. So we suspect that in Summer and Winter, the instability properties should be stronger than in the other seasons. To clarify this aspect, we wrote in Section 2.2:

"The asymmetric part is reflecting the displacement of the heat source in Winter and Summer above or below the equator, following the ITCZ displacement."

5. From the calculation of the Lyapunov exponent, it does not take account of the nonlinearity. If using the nonlinear local Lyapunov exponent (NLLE), would the results be different? Please add some discussions in the sections 3 and 4. Some references on the NLLE method are showing in the following.

   Reference:

   Ding R, Li J., 2007: Nonlinear finite-time Lyapunov exponent and predictability. Physics Letters A, 2007, 364(5): 396-400.

   Li X, Ding R, Li J., 2020: Quantitative study of the relative effects of initial condition and model uncertainties on local predictability in a nonlinear dynamical system. Chaos, Solitons & Fractals, 139: 110094.

   Li X, Zhang X, Ding R, et al., 2026: Investigating the predictability limit and forecast error dynamics of summer air temperatures over East Asia using the ensemble forecast dataset from ECMWF. Atmospheric Research, 108834.

We have added a sentence in the conclusion as follows:

Further investigations could also consider nonlinear predictability diagnostics, such as conditional nonlinear optimal perturbations (CNOP) \citep{Duan2004} and nonlinear local or finite-time Lyapunov exponents \citep{Ding2007,Ding2008, Li2026}, to complement the asymptotic Lyapunov analysis presented here.