Reviewer 2

The authors use a vorticity model to address the question of whether synoptic-scale tropical dynamics are chaotic or not, under the rather stringent assumptions of weak temperature gradient and time-independent wave-form divergence forcing. The model is then considered in a tropical strip domain with solid walls running parallel to the equator and Galerkin truncated based Fourier mode expansion. The results suggest that chaotic behaviour emerges under reasonably moderate forcing based on Lyapunov exponent analysis. The results are shown to be sensitive to a few key parameters such as the aspect ratio of the wave form, the meridional symmetry, and the amplitude of the forcing. As such, the work is quite original and interesting and deserves attention. However, the work is a bit too theoretical and does not make connections with the real tropical atmosphere; for instance, besides the divergence amplitude which is vaguely compared to values reported in literature, the choice of parameters is rarely justified by observations. It is, however, understandable as this is an exploratory paper and the authors promised to consider more realistic settings in the near future. The more serious shortcoming is the WTG approximation and the neglect of divergence modes that are known to dominate tropical dynamics and are strongly coupled with the Rossby modes. The most obvious example is the Madden-Julian oscillation which is both divergence and vorticity driven. Other examples are convectively coupled Kelvin waves and mixed Rossby gravity waves that are known to be strongly coupled to convection and interact with smaller scale moist gravity waves and meso-scale convective systems. The work could also be put in the more general context of MJO predictability studies, and seasonal to sub-seasonal predictions of tropical weather and climate variability. There is an abundant literature on these topics from the practical point of view and very little theoretical work. The questions of the existence-or-not of predictability limits and predictability sources for the MJO and tropical dynamics in general are still outstanding, and I have a feeling that this kind of work could help in the debate. This, in particular, could guide the choice and provide the necessary justifications for the parameter values used in the analysis. The paper could benefit from some discussion in the conclusion section and in the model setup on how the model and the presented results could be used or expanded to address these practical questions.

Thank you very much for your positive evaluation of our work and general consideration on the importance of providing a better perspective to the work.

Specific comments:

1. Line 30: The assumption of constant divergence source amounts to neglecting the large-scale organization of convection and assuming that Rossby wave dynamics are separate from diabatically driven large-scale waves such as

convectively coupled Kelvin waves and the MJO. This comment is not intended as a critic of the present work but instead to put a side note to say that in fact, convection organization itself has a predictability limit, and even the forced Rossby waves were predictable, it won't guarantee the predictability of tropical dynamics as a whole.

That is a very good point. We acknowledge this aspect. The current work constitutes a baseline for future investigations in which the complexity of the system is increased, by incorporating humidity, convective forcing, large-scale coupling between the ocean and the atmosphere, and interaction with the stratosphere. This program will evaluate the contributions of each component in a step-by-step basis, yet keeping the system as simple as possible. The purpose being to also keep a fully nonlinear approach to apprehend the nature of the variability through bifurcation analyses. We have stressed that by modifying the introduction and conclusion as follows:

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"

In the conclusion: "Overall, this analysis constitutes the baseline of a detailed program in which the emergence of chaos will be analyzed in a hierarchy of models of increasing complexity, evaluating in such a way the key processes controlling the predictability of the tropical atmosphere."

2. Eqn 12 and Line 80: Instead of imposing a doubly periodic divergence term, the authors could try something that has a Gaussian shape in the meridional direction (that can be either symmetric or asymmetric as in Gill 1980) to remain in the spirit of tropical dynamics.

Thank you for the useful suggestion. The present sine/cosine profiles were selected to isolate equatorial parity under a compact Galerkin basis. A Gaussian profile would require another forcing projection and experiment that we could address in the future. We added this in the list of future analysis in the conclusion.

3. Section 3.1: Please say what n is here to remind the reader.

Thank you for pointing that out. It is now done in Section 2.2.

4. Line 110: The question remains as to whether this multiplicative nature will become a stabilizer rather than a chaos amplifier when divergence modes are allowed to vary and interact with the Rossby waves, as it is presumably the case for tropical dynamics.

Thank you for the interesting comment. We keep that in mind for future analysis.

5. Line 114: Why are they unrealistic? A sudden drop in phase speed is expected beyond the forcing wavenumber, which may lead to a large group speed!?

We agree that visual speed alone is insufficient to diagnose realism and phase/group speeds differ. We remove the categorical claim, report only rapid simulated propagation, and do not attempt quantitative speed attribution. We revised the text accordingly:

"For large values of the forcing (beyond 0.2) the solutions show wild behaviors as suggested by the very fast propagation of large-scale envelopes."

6. Line 115: By "there is no solution anymore", you mean that the numerical code blows up? Clearly, the dissipation parameter r has a say in this. How does its value impact the results?

You are right. The nature and amplitude of the dissipation parameter play an important role in this behavior. We stressed that in Section 3.1:

"This last behavior depends on the type of dissipation used and the amplitude of the friction parameter. This aspect will be explored in detail in the future."

7. Figure 2: Why is there a sudden drop in all the instability measures at the 0.08 mark and the oscillatory behaviour and sudden pickup just before 0.1?

The nature of the solutions are changing here. A periodic window appears before the rapid increase of the Lyapunov exponents after 0.09. Such a phenomenon is typical in dynamical systems analysis as for instance in the bifurcation diagram of the Lorenz system: A succession of chaotic and periodic windows appears related to the way the underlying phase space structure is organized. It is per se an important subject of analysis, much beyond the scope of the current letter. The related physical question on the origin of this change of dynamical properties is also an important topic that will be explored in the future.

8. Figure 4: Increasing n seems to have an overall stabilizing effect, although there are some outliers such as the case n=2.5 with the anti-symmetric forcing, which is by itself intriguing as it is unstable at small forcing and stable when the forcing is large. I wonder if the authors have an explanation or whether the analysis is actually insufficient for a full understanding of the model's behaviour.

Thank you very much for pointing that out. It is also true for n=2 under the same forcing. This is indeed a very counterintuitive result, suggesting that at small scales (typically scales smaller than a few thousand kilometers) the stability properties are reversed with a stabilization when the forcing is increased. It is important to keep in mind that the forcing here is at the same scale as the process itself. At first sight this suggests that the solutions are essentially slaved to (constrained by) the forcing. A

bifurcation analysis should allow us to disentangle that. It will also be explored in the future.

9. Line 150: How do the higher resolution results compare to the low resolution ones presented in the previous sections? Why not do the analysis with this higher order projection?

It is clear that higher dimensional models should present a higher fidelity with reality. The reason for not doing everything with the higher order model is the potential numerical cost of doing all the experiments here, with a systematic exploration of a quite large range of parameters. The interest of the lower order model is that it is also amenable for a detailed bifurcation analysis that is planned for a future study. The higher order model can also be tackled in the same way but with a computational cost which is much higher. Finally, having a hierarchy of models provides a set of nice testbeds for explorations.