Dear Editor,

We have prepared a detailed response below. In bold, our responses to the reviewers comments, and in bold italicized, the additions in the manuscript. We hope that this new version is suitable for publication as NPG letters.

On behalf of the authors

Stephane Vannitsem

Editor's comment

Two referees have now sent their evaluations of the Letter, which are accessible on the NPG site. A third referee (hereafter referee 3) has also submitted a review, but not in a format that makes it directly accessible on the site. I have just submitted it in the format of an editor

comment.

Apologies are due to the authors. They submitted their paper as an NPG Letter on June 20 last. NPG Letters are meant to report particularly important results and major advances in a concise and engaging style. Rules require them to be handled with priority. However, for various reasons, including withdrawal by a first editor, it is only now that two reviews have been submitted in the required format. It may also be that the somewhat specific nature of NPG Letters had not been mentioned clearly enough to the solicited referees.

Referees 1 and 2 are both specialists of tropical meteorology and predictability. They recommend acceptance of the paper, subject to revisions. Referee 3 is a specialist of dynamical systems, but not (at least from what I know) of atmospheric dynamics. Although he/she does not make explicit recommendations, his/her report amounts to at least major revisions.

All three referees ask for a more complete study, including additional computations or numerical results. Referees 2 and 3 acknowedge that it is in a sense a preliminary work.

Actually, the authors explicitly state their intention to perform additional future work (which is normal if a Letter is meant to be a preliminary but important announcement of further studies).

But the referees nevertheless consider the Letter cannot be published in its present form.

The Interactive Discussion of the paper, during which any member of the scientific community can submit comments, is open until 03 Oct. The authors will then be given a deadline to send their responses to the referees and possible other contributors. Given the above,

I suggest, at their preference, two ways of action for the authors.

1. Either they resubmit their paper as an NPG Letter, possibly stressing more strongly that the letter is only submitted in anticipation of a more complete paper. The authors should take into account the comments and suggestions of all three referees, and give a point-by-point response to all of these comments and suggestions. In case they disagree with a particular comment or suggestion, or decide not to follow it, they should state precisely their reasons for that. Following the referees' suggestions may not be compatible with the format required for NPG Letters (such as for instance the requirement for fewer than 2,500 words in the main text). The authors will then have to choose and explain their choice. This revised version will. be sent for further review to referees 1 and 2. I also mention that a paper submitted as an NPG Letter may be accepted, but only as an ordinary article, and not as a Letter..

2. Or the authors withdraw their present paper, and submit a new, more complete, article (not an NPG Letter). The reviewing process will then start from scratch. Such a more complete article should include in my opinion comparisons with observations and possibly results produced by more comprehensive numerical models.

We thank the editor for clarifying the aims and constraints of NPG Letters. We will revise and resubmit as a Letter, preserving the central result while clarifying assumptions, physical scales, predictability claims, and limitations. We are not presenting this as a comprehensive model of tropical weather. Requested observational comparisons and coupled-dynamics studies fall outside the scope of this Letter but are certainly worth performing in the future as we planned.

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.

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.

Reviewer 3

The predictability of the atmospheric tropical belt is investigated by exploring the emergence of chaos in reduced-order model off the vorticity equation under the weak temperature gradient hypothesis. It is shown that the emergence of chaos depends on the aspect ratio (the length scale of the forcing), the forcing amplitude and its symmetry properties. There is an unusual increase in the number of positive Lyapunov exponents that should be the core of the paper; unfortunately, this is postponed for future works.

I am left with a taste of "so, what"? The authors have an interesting model, come with interesting questions but postpone the core of the work for future works… So, let see what they will do with this. They did not explain sufficiently the key aspect (for instance, when n = 20, what is the corresponding length scale?, the role of the dissipation rate r, the implication of the two components of the forcing, etc.). Consequently, this manuscript appears as a preproject… This should be matured to become a paper to quote…

Thank you very much for your feedback. Indeed the letter's intention is to propose a new research avenue to evaluate the predictability of the tropical atmosphere. To our knowledge, the tropical atmosphere has been studied for decades essentially with the vision that pure linear waves are the essential ingredients of the dynamics. Although we recognize that the source of predictability of processes in that region has always been related to the interaction with the surface, in the current program, we hypothesize that the ocean is not the only player. So we propose a systematic strategy for the analysis of this problem based on a hierarchy of models that can then be systematically explored using the dynamical systems theory. We further stressed that in the Introduction and conclusion.

Other comments

Abstract

"This result contrasts with the idea of a predictable tropical atmosphere, and opens important questions on the effective origin of predictability in the Tropics." This sentence opposes two features that appear contradictory. Please, clarify.

Thank you for pointing that out. We modified the sentence as

"This result contrasts with the idea of a predictable standalone tropical atmosphere and opens important questions on the effective source of predictability in the Tropics through interactions with the other components of the climate system."

Introduction

The introduction is a little bit short… I don't understand why the problem of the atmospheric predictability can be resumed in knowing "whether the tropical atmosphere is essentially slaved to its boundary conditions." Indeed, it is now well-known that very simple systems are not long-term predictable. Please, explain. I am rather surprised to read that the unpredictability results from "the uncertainty at the interface between the ocean and the atmosphere". What is exhibited, particularly by Lorenz, is that the unpredictability is inherent from the dynamics itself and does not result from uncertainties, and this is what quantify the Lyapunov exponents… Please, clarify.

We understand your difficulty in capturing this point. In fact the ocean is very slow and therefore the properties of the dynamics of the atmosphere will slowly change as a function of the imposed boundaries. So authors usually focus on the slow boundary dynamics that are shaping the systems' attractor, while the sensitivity to initial conditions seems to play a secondary role. In other words, the intrinsic size of the atmospheric attractor would be rather small as compared with the imposed change to this attractor due to external forcing. This is precisely an aspect that we will address in the future, using a step by step approach with first the analysis of a pure atmospheric model with no changes of the surface boundary forcing. We have slightly changed the second paragraph of the Introduction accordingly.

According to the authors, the question is therefore to know "whether the tropical atmosphere is essentially slaved to its boundary conditions". It is therefore surprising to use simplified equations for investigating this. How are preserved the boundary conditions in the simplified model here investigated? This is a key question to address the problem posed by the authors but it seems to be forgotten from the beginning… So, what? In the other hand, how inserting stochastic forcing can help in answering this problem? Please explain.

In the current analysis, the impact of the boundary conditions is not taken into account as the forcing is frozen to a constant value. Those boundaries will affect the forcing and in future works, the intention is precisely to add new processes allowing for modifications of the divergence field.

Section 2.3

If I well understand this manuscript, the authors use an integration of the model over 30 s to assess Lyapunov exponents leading a horizon prediction of 1 day… It seems strange to me…

Thank you for pointing that out. We integrated the model for each experiment (each parameter value) for 100,000 time units, or 33,333 days. The time step, on the other

hand, is fixed to 0.001 time unit, so about 30 seconds. We have clarified that in the manuscript in section 2.3.

Section 3.1

How is selected n = 20? What is its influence on the results?

The aspect ratio n is chosen in order to cover the whole tropical region over 40,000 km. We have added a sentence in the first paragraph of Section 3.1.

Figure 1 shows that the Lyapunov dimension becomes greater than 3 very quickly. It would be interesting to investigate how is the transition from periodic to chaotic. It appears that the hyperchaos arises also very quickly after the first chaotic solution, a feature quite expected with large dimensional model.

Thank you very much for this comment. Indeed, we are planning to investigate in detail the bifurcation diagram of this system in order to put on firm grounds the transitions between the different regimes. This is commented on in the conclusion.

For regimes associated with a Lyapunov dimension greater than 9, how many Lyapunov exponents are null? This is indeed puzzling and this should be checked and explained since all the manuscript is devoted to Lyapunov exponents without further analysis. This cannot be postponed for future works because this could come from a mistake in computing Lyapunov exponents. Otherwise, what would be the interest of such a work? I had a look on the video, it seems rather surprising that these not so turbulent dynamics lead to so large Lyapunov dimension… A clear link could be made between these videos and the figures of the manuscript… This would help the reader…

There is only one zero Lyapunov exponent. Please see below the Lyapunov spectrum obtained with an integration of 100,000 time units for parameter χ_2=0.32, n=0.20 (corresponding to a Lyapunov dimension larger than 9). The Lyapunov spectrum has a quite classical shape, except that the number of positive exponents is large. We do not see any mistake in the computation of Lyapunov exponents.

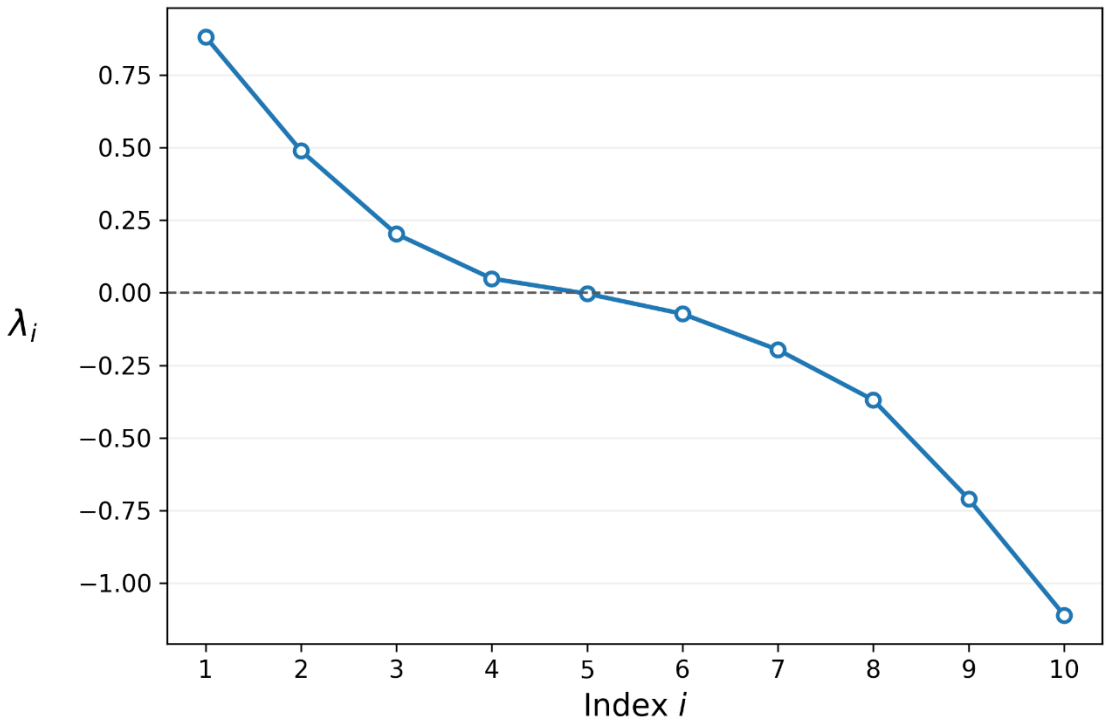


Figure 1: Lyapunov spectrum for χ_2=0.32 and χ_5=0., r'=0.0028 and n=0.20. Four exponents are positive, one zero and the last five are negative. This result is obtained after an integration of 100,000 time units corresponding to 33,333 days.

Concerning the videos, the behavior becomes more erratic when we increase the values of the forcing amplitude. The discussion is slightly modified (in view of the size limit of the letter) in Section 3.1.

When only Xi_2 or Xi_5 is reported, does it means that the second Xi_i is null ? Please, clarify.

Thank you. This has been done in the figure captions.

Since there is no state portrait, it is rather difficult to figure out which type of (hyper) chaos is actually observed. The influence of symmetric and asymmetric forcing is therefore difficult to assess. The Lyapunov exponents being a poor marker for the type of chaos actually observed.

Thank you for the remark. This will be addressed in the future by making bifurcation diagrams. The current letter was focusing on the predictability of those systems.