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Nonlinear Processes
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Discussions
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Interactive comment on "Wave propagation in the Lorenz-96 model" *by* Dirk L. Van Kekem and Alef E. Sterk

Anonymous Referee #2

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General comments:

This work analyses the dynamical behaviour, more precisely the bifurcation structure, in the Lorenz96 system as the forcing parameter is varied. For very small forcing, the Lorenz96 system exhibits a stationary state, and the authors focus mainly on the emergence of waves (i.e. periodic solutions) through Hopf (and related) bifurcations from this stationary state. The emerging waves are analysed in terms of their dependence on the dimension of the system. Both the cases of positive and negative forcing are considered and shown to exhibit different behaviour.

Specific comments:

The paper is well written and clearly structured. As the Lorenz96 system is widely used in the atmospheric physics community due to its similarity (in certain aspects) with the climate system, the research question is relevant and timely. I have three major concerns with this manuscript though.

1. The authors fail to draw a connection between their results and the physical phenomena to which the Lorenz96 system pertains, namely atmospheric circulation. A mathematical analysis of the bifurcations in this system would be fine for a mathematics journal, but I believe the general readership of NPG to be ultimately interested in the implications for the physical world. Just to mention a few specific things: the authors should provide a more comprehensive description of the Lorenz96 model and its geophysical interpretation. What is the precise meaning of the model variables; what does the forcing represent physically; (very important!) what is the physical interpretation of *negative* forcing, if any? The properties of the emerging waves should be translated into the physical world, too. What do the results at the end of section 3.1. imply for atmospheric waves; does a larger dimension n mean a finer latitude grid or a bigger planet; what is the interpretation of the limiting wave period?

2. The paper seems to provide very little beyond a previous preprint of the authors (arxiv-preprint 1704.05442). In fact, only the part of the manuscript considering the case of negative forcing seems to contain unpublished material. Given that the authors' results are much less comprehensive in this situation and that this case, so far at least, lacks justification, the question arises whether the manuscript provides enough new material to justify publication in NPG. The authors should provide clear guidance regarding what is new in this manuscript and what is not, and justify why the new material should be of interest to the general NPG readership.

3. The paragraph starting pg.15,l.12 is pretty unclear. It seems to be important though for a major conclusion of this manuscript. The main problem seems to me that you do not define the Neimark–Sacker bifurcation. In fact, the standard definition of the Neimark–Sacker bifurcation pertains to discrete time systems, only. Please clarify. Further, the definition of the "multi–stability lobe" is unclear, and what is the evidence that the two mentioned curves indeed bound such a region?

Technical corrections:

There are some, albeit few and minor, technical issues:

1. Please decide whether you want to write "Figure (No)" or abbreviate as 'Fig. (No)". Same with "Equation".
2. pg.2,l.21, replace "which restricted to" with "which considers only" or similar.
3. pg.2,l.21, replace ", and thereby we" with "; these two manuscripts together" or similar.
4. I think the appropriate technical term is "Jacobi matrix".
5. pg.4,l.2 mention that "equilibrium solution" and "steady flow" mean time–independent solutions.
6. pg.4,l.8 should be "genericity conditions".
7. pg.4,l.9 you never actually define what a Hopf bifurcation is.

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