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Nonlinear Processes in Geophysics
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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 #1

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This paper presents a detailed analysis of the principal bifurcations in a well known "toy" nonlinear model that has been widely used for the past 20 years as a vehicle for investigating mechanisms for the loss of predictability and onset of spatio-temporal chaos in the atmosphere and oceans. Although this model has been widely used in various contexts, its mathematical properties have not been extensively and systematically studied except in a few specific cases. So one of the distinctive contributions of this manuscript is to apply rigorous analysis tools to study and characterize the first and principal bifurcations of this model as a function of both the model dimensionality n and the forcing parameter F. The results show some clear systematic behavior as n and F are varied, with distinct differences depending on the sign of F. In particular, the present analysis sheds new light on the characteristics of waves that develop within

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Discussion paper

the Lorenz-96 model and their dependence on n and F. It is good to see the discussion go beyond simply applying numerical tools to obtain diagnostic information on solutions also obtained numerically, but also to examine the structure of the Jacobian, even in fairly high dimensions, to gain mathematical insight as to why certain bifurcations occur.

The paper is generally clear and well written with a reasonable and balanced survey of previous work using the Lorenz-96 model. My only slight concerns are (a) a quibble with the use of the term "standing wave" to describe the stationary wave-like structures found for F<0 and n = 4k+2, and (b) the authors could also make mention of other similar systems exhibiting double-hopf bifurcation to multiple traveling wave solutions (e.g. see Moroz & Holmes 1984. Double Hopf bifurcation and quasi-periodic flow in a model for baroclinic instability, J. Atmos. Sci., 41, 3147-3160). For (a), the term "standing wave" is usually interpreted in physics to mean a time-varying wave-like structure with fixed phase in space only - so can be decomposed into a superposition of two oppositely propagating traveling waves. But the structure described in the paper as "standing" seems actually to be a stationary wave which is effectively a traveling wave of nearly fixed amplitude Doppler-shifted to zero frequency. I would therefore urge the authors to use the term "stationary wave" for these structures in preference to "standing wave", despite precedents elsewhere for the latter (in my view misnomers).

Minor technical points and typos:

P.2 line 15 As well as self-referencing Sterk et al. (2010), you could also mention other earlier studies that also identified Hopf bifurcations associated with the onset of low frequency variability in atmospheres, oceans or laboratory experiments (e.g. Simonnet et al. 2003. Low-Frequency Variability in Shallow-Water Models of the Wind-Driven Ocean Circulation. Part II: Time-Dependent Solutions, JPO, 33, 729-752; Read et al. 1992. Quasi-periodic and chaotic flow regimes in a thermally driven, rotating fluid annulus, JFM, 238, 599-632) P.2 line 21 word missing after "which" - "was"? P.2 lines 24-27 commas recommended after "bifurcation", "that", "F>0", "n", "that" and "F<0", P.3

Fig. 1 Are the braided striations in the Hovmöller plots real or an artifact of the plotting? P.9 line 5 “associated with” not “to” P.22 line 20 Page numbers in reference Frank et al. 2014 seems to be incorrect - should be 1430027?

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