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Nonlinear Processes in Geophysics
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Interactive comment on "Nonlinear feedback in a six-dimensional Lorenz Model: impact of an additional heating term" *by* B.-W. Shen

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This paper has the merit of studying how changing the structure of feedbacks impact some of the most important properties of a minimal truncated set of equations describing convection.

While the paper has indeed merits, I would recommend the authors to improve the discussion on the physical relevance of their results and put them in a broader context of the published literature.

Some comments:

1) The authors should make clear that the problem was first studied by Salzman, who gave a very extensive treatment of the possibility of constructing reduced order models.

Lorenz then studied one of such models and got such an incredible result.

Also, in the following paper it is discussed that the L63 model is a member of a class of equivalence: Z.-M. Chen and W. G. Price, Chaos, Solitons Fractals 28, 571 2006_.

2) In presenting their models, the authors should explain more clearly that they have to be derived from the continuum equation by suitable truncation.

It is not clear to me why the authors choose such a truncation, where the first and third modes are used, while the second modes are left out, a bit arbitrarily, from my point of view. Could the authors explain the rationale for this?

In an earlier paper

V. Lucarini and K. Fraedrich, Symmetry breaking, mixing, instability, and low-frequency variability in a minimal Lorenz-like system, PRE 80, 026313 2009

we took a different route, constructing the truncation starting from below, including all the 1- and 2- modes, and adding the same two modes here indicated as M3 and M6. In this way, we also obtain a closed system of equations. Maybe the authors could consider looking into that paper, where it is explained that in order to achieve a complete thermodynamic consistency (see also C. Nicolis, Q. J. R. Meteorol. Soc. 125, 1859 1999 on the Thermodynamic Relevance of the L63 model) some modifications have to be implemented.

3) If possible, I would recommend the authors to discuss a bit the fact that the fractal dimension is similar for all their models when they are all in the chaotic regime. What is their take on this?

Also: the first Lyapunov exponent is almost identical in the 3D and one of the 6D model. Can they find a correspondence also for the other non-zero LE of the 3D model?

Some additional comments

When discussing the Lyapunov exponents, the authors might consider referring to G.

Benettin, L. Galgani, A. Giorgilli, and J. M. Strelcyn, Meccanica 15, 9 1980 as they first discussed the benefits of the GS method.

Also: the effect of mode truncation was extensively studied by Franceschini et al. V. Franceschini and C. Tebaldi, Meccanica 20, 207 1985 V. Franceschini, C. Giberti, and M. Nicolini, J. Stat. Phys. 50, 879 1988

Appendix: Attention: you are citing different definitions of fractal measure. They are not equivalent. See Ruelle 1989.

Ruelle, Chaotic Evolution and Strange Attractors, 1989

What does it mean that the second Lyapunov exponent is not zero (of course it has to)? Of course it cannot ever be exactly zero. It will converge only asymptotically to that value. The authors might consider adding error bars to their estimates.

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 475, 2015.