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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Lyapunov analysis of multiscale dynamics: The slow manifold of the two-scale Lorenz '96 model" *by* Mallory Carlu et al.

Anonymous Referee #2

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Summary

This manuscript explores the Lyapunov spectrum of the two-scale Lorenz '96 model, composed of K "slow" variables coupled to $K \times J$ "fast" variables. The main result is that the covariant Lyapunov vectors (CLVs) corresponding to Lyapunov exponents (LEs) close to zero have large average projections on the slow variables in a total energy norm. The authors claim that these CLVs constitute a tangent space "slow manifold," although the authors use this term rather loosely and do not, for example, show that this collection of CLVs is the tangent space of a proper invariant manifold. In the absence of such a demonstration, the term "slow tangent space" might be more appropriate. It seems natural that slowly growing/decaying CLVs should project onto

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the slow variables, although the authors give a useful characterization the properties of this collection of slow CLVs. There are more CLVs in the slow tangent space than there are slow variables, so this tangent space necessarily includes some projection onto the fast variables which nevertheless does not induce a fast time scale. When the the slow and fast variables are decoupled, the Lyapunov spectrum of the slow variables "covers" the slow tangent space in the sense that the largest and smallest LEs of the slow variables roughly span the range of LEs of the slow tangent space. This result holds when the nonlinear systems are coupled but the tangent spaces remain uncoupled.

These results are interesting, although it's not clear whether they are particular to the L96 system or are generic to multi-scale systems. Such a determination would be a subject for further research. The present manuscript would be strengthened by a more careful characterization of the slow tangent space—in particular, whether it is actually associated with a slow manifold as the authors claim. A few of the analyses seem poorly motivated or incomplete, see major comments below. In my opinion, this paper would be publishable if these issues are addressed.

Major Comments

1. **Partition of the CLVs:** The CLVs in the slow tangent space have strong projections on the slow variables, but figure 5 seems to indicate a different fundamental partition of the CLVs: CLVs with a projection PDF peaked at zero (black and green curves in inset) and those whose probability of no projection onto the slow variables is *zero* (red curves in inset). I suspect that the red curves could be given a large projection onto the slow variables by a suitable rescaling of the projection norm, but the others (peaked at zero projection) could not. This is more of an observation than a critical comment, but I would be interested in the authors' thoughts on this matter.

2. **Finite-time LEs:** It is not clear what is gained by the use of the finite time LEs. They are used to show that the PDFs of the growth rates of disparate CLVs are similar. This conclusion could have been drawn from the instantaneous growth rates of the CLVs. The case for finite-time LEs is further weakened by the authors' use of a finite time window that they consider asymptotic after stating that the finite-time LEs asymptotically converge to the LEs (compare lines 22 and 24–25 on page 18). This may just be poor phasing, but it is confusing and makes the finite-time LE results difficult to interpret.

3. **Near-tangencies:** The authors show that two (out of hundreds) of the CLVs exhibit frequency near-tangencies, but for this to be a convincing explanation for the smooth edges of the slow tangent space the authors need to show that this behavior is generic; that is, that most if not all of the CLVs near the slow tangent space have frequent near-tangencies.

4. **Finite-size LEs:** The authors produce a table showing which LE corresponds to the second plateau of the finite-size LEs, but it's not clear why there should be any direct correspondence between the growth rate of a nonlinear disturbance and the LEs, which are the average growth rates of infinitesimal disturbances. They note that others have observed a similar second plateau and conjectured that this represents the growth rate of a linear instability involving a reduced set of variables. Even if this is true, its not clear why the growth rate of an instability involving a reduced set of variables should be equal to an LE from the original system. This idea needs further explanation in the context of the present paper.

   The claim that the LEs corresponding to the second FSLE plateau provides a "convincing" estimate of the leftmost boundary of the "central band" of the slow tangent space (lines 9–10 on page 22) does not seem to follow from figure 8b. The vertical lines indicated in this figure are all clearly on the left side of the central band, but they don't appear to align with any obvious boundary.

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Minor Comments

1. Note that Norwood et al. (J. Phys. A, 2013, doi:10.1088/1751-8113/46/25/254021) also studied CLVs in a multi-scale system, found "slow" CLVs that coupled onto both slow and fast variables, and noted that finite amplitude disturbances could be used to distinguish between the slow and fast systems. However, their model did not have an extensive Lyapunov spectrum, so they did not study the scaling properties of what they called "coupled" CLVs.
2. **Symmetry of the Lyapunov spectrum in the conservative case:** The authors argue that the Lyapunov spectrum is symmetric when forcing and dissipation are turned off, but the results presented don't necessarily back up this claim. Figure 2 simply plots the reflected Lyapunov spectrum on top of the original, which only shows that spectrum is symmetric to within the width of the curve. It would be more compelling to show the absolute difference between the reflected and original spectra on a semi-log scale with an appropriate estimate of the precision of the numerically computed LEs.
3. Lines 16–18 on page 1: "Additionally, usual arguments based on scale analysis, where only a limited set of scales are deemed important, usually fail because of the presence of, possibly slow, upward or downward cascade of energy and information." This sentence does not make sense; it is not clear what is trying to be said here.
4. Footnote 5: It is not clear why simply shifting the index by $1/2$ would improve the convergence of the spectrum.
5. Some minor grammatical errors:
    (a) Line 16 of page 1: "is stiff" instead of "immediately results as stiff".
    (b) Line 12 of page 2: "For some time" instead of "Since some time".

(c) Line 18 of page 2: “and” should be inserted before “(iii)”.

(d) Line 29 of page 3: “main result” instead of “main results”.

(e) Line 4 of page 4: “synoptic” instead of “synpoptic”.

(f) Line 15 of page 5: “perhaps” instead of “pherhaps”.

(g) Line 1 of page 22: “obviously” instead of “obviousy”.

(h) Improper parenthesis around references: page 3, line 11; page 4, line 15; page 7, lines 28 & 29; page 21, line 5; page 22, line 3; and page 23, lines 18 & 19.

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