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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 #1

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1 General response and main points

I find the results presented to be novel and interesting, but I feel that there are a number of points that warrant improvement before the manuscript can be considered for publication. To summarize my understanding of the main result:

> the authors have demonstrated that in the two layer Lorenz 96 system, with respect to a scaling law in the coupling strength, the number of fast variables and relative amplitudes therein, there is a consistently identifiable, wide spectral band (close to the neutral spectrum) where the covariant Lyapunov vectors will project strongly onto the slow spatial variables. The au-

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> thors conclude that there is a slow manifold structure that extends beyond the slow variables into the layer of fast variables based upon a resonance between the fast and slow variables, corresponding to similar Lyapunov spectrum when viewed in the decoupled, singular limit.

While these results are interesting and highly relevant, I feel that the conclusions and the overall analysis requires greater detail and justification.

It appears to me that there are two conceptually different notions of “slow coherent structures” that the authors wish to find a correspondence between. Firstly, there is the slow coherent structure of the X_k variables which evolve on the slow time-scale in the two layer Lorenz model. On the other hand, there is the slow structure defined by the Lyapunov exponents that are close to zero, such that the corresponding covariant Lyapunov vectors exhibit weakly exponential, or sub-exponential, growth and decay. What has been established is one direction of implication: (i) we may consistently find that it is *only* a weakly unstable and stable spectral interval around zero where the associated covariant Lyapunov vectors project strongly onto the slow layer X_k. However, the authors have not established the other line of correspondence. Specifically, it is yet to be shown: (ii) what is the overall distribution of projection coefficients onto all spatial modes for the “close-to-neutral” spectral band. Particularly, restricting to the covariant vectors that correspond to a small threshold on the asymptotic rate of exponential growth or decay (absolute value of the Lyapunov exponent), it is of interest to find which spatial components the covariant Lyapunov vectors project most consistently onto.

An example of what would help establish the other direction of correspondence would be to show the distribution of the projection coefficients for the covariant Lyapunov vectors corresponding to a radius around the zero exponent. Particularly, it is of interest to know how strongly peaked this distribution is over the slow components X_k when the spectral radius is small, as compared to when we extend the radius outwards. This would identify which spatial components, in a multiscale system, we consistently

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find weakly-exponential and sub-exponential growth and decay of perturbations most frequently. The two pieces of information (i) and (ii) above would, in my opinion, give a much more complete picture together of the correspondence between these two slow structures.

A separate but important note is that the authors have not justified the use of the terminology slow “manifold”. The actual manifold structure has not been sufficiently explained or supported for the understanding of the reader. To my knowledge, the existence of unstable and stable manifolds (whose tangent spaces are the sum of the unstable and stable Oseledec spaces respectively) extends to trajectories of partially hyperbolic dynamical systems, but a general construction for a center manifold doesn’t extend from fixed point theory to this setting. If there is indeed a manifold structure, this point requires significant elaboration. I am satisfied with describing the above result as a “coherent structure”, but the results and conclusions should be justified if using terminology with precise mathematical meaning in their statement. That said, I am in favor of the authors re-submitting when they have clarified the above points and addressed a number of minor points in the following section.

2 Minor Points

1. The authors frequently refer to the slow manifold in the tangent space. The global manifold structure associated to the collection of all point-wise tangent spaces (and points in the underlying base-manifold) is the tangent bundle. The meaning of the slow manifold in the tangent space should be clarified.
2. Page 4, line 17: there appears to be a typo or grammatical error in “a chaotic dynamics”.
3. Page 4, line 21-22: the parameter b controls the relative amplitude of the Y variables, but this is unclear from the sentence.

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4. Page 4, equation (2), third line: there is a typo in the sub-indices where the right hand side should have the sub-indices switched.

5. The covariant Lyapunov vectors are described as an intrinsic quantity for the system, but this is not totally accurate. The Oseledec spaces are an intrinsic quantity, but any choice of covariant vectors is only unique up to non-zero scalar when there is non-degenerate spectrum. When there is non-trivial multiplicity of exponents, there is no unique choice even up to non-zero scalars, as any choice of vectors subordinate to the Oseledec splitting can be described as covariant. This should be clarified for the reader.

6. Figure 1: this figure should be significantly re-worked as it is difficult to interpret the results.

   (a) The piece of the spectrum chosen for visualization in Fig 1.b is not spectrum of interest. The centrality of the modes closest to zero is what is of greatest significance for the analysis of slow modes, as described in Section 1. Likewise, this is important for the analysis of the numerics, as the neutral modes converge more slowly to their asymptotic limits. Particularly, the non-zero Lyapunov exponents that are close to zero become hard to distinguish from actual zero exponents numerically. It is extremely important for the analysis to understand how many zero exponents possibly exist, *even if some may be spurious* because their extremely weak exponential behavior is itself of interest.

   (b) Qualitatively, I would especially like to see how wide or narrow the "close-to-neutral" spectral band becomes when varying the parameters K and J.

   (c) None of the circles, squares or triangles are clearly distinguishable in the figure. If we are to make any analysis based upon the correspondence between different values of K and their markers, we need another, further zoomed in scale.

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(d) Pesin's formula only holds for an SRB measure, while in general the sum of the positive Lyapunov exponents holds as an upper bound to the KS entropy. If there exists an SRB measure for this system, it should be clarified. If the plot for H_{KS} corresponds only to an upper bound, this should be clarified.

7. Page 10, lines 3-5: the KY-dimension has only been shown equal to the information dimension of the attractor in limited cases. If it is not known that this is equal to the information dimension of the attractor, it should be clarified that the KY dimension is an approximation for the information dimension.

8. Page 10, lines 10-11: the meaning of the thermodynamic limit for the spectrum should be clarified for the reader.

9. Page 10, lines 10-15: the meaning of the statements about convergence are unclear. It appears that in the visualized part of the spectrum of Fig 1.b that there could be a limiting mode when increasing the dimension of each sector's fast layer J. However, in other portions of the spectrum visualized in 1.a, the evolution doesn't seem to be monotonic in J (particularly in the leading and trailing exponents). Likewise, assuming that there is an asymptotic mode for the spectrum in large J, what is the meaning of convergence in K? What quantities are being compared at numerical precision? This needs clarification.

10. Figure 2.b: this plot doesn't add any new information. It would suffice to say that the Lypunov spectrum is symmetric with respect to time reversal of the dynamical system. It would again be more interesting to visualize the "close-to-neutral" spectrum in the conservative system.

11. Page 11, lines 27-28, Page 12 lines 1-9: Introducing this change of scale by γ is confusing because the only change of scale that has been introduced thus far is with respect to the parameter b. This change of scale would generally change the dynamical system itself and thus the associated covariant vectors (and potentially

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the spectrum). If this is not a discussion of changing the dynamical system itself, but rather simply the projection of the covariant vector into the fast variables, the meaning of the scale factor of γ is totally unclear. As is elaborated by the authors, multiplying the fast variables by γ and re-normalizing by the standard Euclidean distance has the effect of magnifying or de-magnifying the the projection into fast variables, and therefore *changes the span of the associated CLV*. This is mathematically unsound. If this discussion is related to the family of norms in equation (29), it should be moved into that section and the meaning should be clarified.

12. Page 12, footnote 4: the meaning of this footnote is unclear, as is where the verification of “a notable part of the energy lying in the slow variables” has taken place in the text. Please clarify this.

13. Page 12, footnote 5: the meaning of the re-indexing is unclear. Why does indexing change the convergence of the spectrum? Once again, it also needs to be clarified what is the meaning of the convergence of the spectrum within a J mode as K is increased.

14. Page 13, Figure 3.b: this is an interesting figure, but key information is lost based on the scale. It would be helpful for the reader to see the width of the spectral band around zero corresponding to the observed boundaries of the coherent structure in the top panel. Particularly, this is important to compare the scale of weakly-exponential and sub-exponential growth and decay that corresponds to these strong projections. It will be particularly interesting to observe, as it appears in the figure, where the boundary of the coherent structure extends beyond corresponding weakly-exponential growth and decay in the spectrum into strongly expansive or dissipative behaviour. This could be presented in an additional figure.

15. Page 16, lines 13-15: numerical verification is completely unnecessary as the

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attractor, in the uncoupled limit, is decomposable into the two disjoint subsystems — any invariant ergodic measure on the attractor must also be decomposable into two ergodic invariant measures without any shared support. Oseledec's theorem, the splitting of the tangent space and the associated exponents, is stated in terms of an ergodic, invariant measure. Therefore the exponents and the splitting can be computed on each component independently.

16. Page 16, lines 19-24: this approximation of the decoupled dynamics in the fully coupled system should be made explicit. As this is a novel approximation introduced by the authors, it should be made absolutely clear what terms are being neglected in the computation of the approximately decoupled Jacobian. It would be useful for the reader to show how these terms scale with the coupling parameter. Likewise, the method of computing the Lyapunov exponents for each sub-system via this approximation should be made explicit.

17. Page 17, Figure 6.b: due to the scale in the reconstructed spectrum, a visual inspection is not informative of the similarity or dissimilarity of the actual spectrum and that which is produced via the decoupled approximation.

18. Page 18, lines 3-4: The claim that tangent-space coupling proves to be nearly irrelevant for the estimate of the LEs has not been justified quantitatively, only visually. It would be extremely helpful to study the RMSE of the distance of the true, fully coupled spectrum, from the approximate spectrum reconstructed from the decoupled approximation — this could be produced similarly to what is done in equation (24) and Fig. 6.c with the true, fully decoupled spectrum and the approximate spectrum. Claiming the approximate isolation of elements of the spectrum to either the fast or slow variables is unsupported without a quantitative measure. Particularly, the authors should explore, as a function of the coupling h, at what point the approximation will fail to recover an adequate reconstruction of the full spectrum. Other benchmarks of interest include: (i) how well do the

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approximated exponents recover the upper bound on the KS entropy, via the sum of the positive approximated exponents versus the true ones, and (ii) how well do the approximate exponents reproduce the KY dimension computed via the true exponents.

19. Page 18, lines 25-26: it has not been explicitly specified how to associate an exponent, or the associated covariant vector, from the full spectrum with that of the approximate spectrum computed from the block Jacobian. As this is a novel approximation, it is important that this is not open to interpretation.

20. Page 19, line 1: "proof" has a precise meaning mathematically, and mathematical proof has not been evidenced in this case.

21. Page 20, lines 2-4, Figure 6.d, Table 1: This measure should again be made quantitatively. The visualization and the table are useful, but there needs to be more analysis. In particular, it would be useful to know, relative to the coupling strength: (i) what is the exact spectral interval associated to the coherent structure when studied via the exact spectrum; (ii) what is the spectral interval produced via the approximate spectrum; (ii) how many total exponents lie within each of the intervals in (i) and (ii) above; and (iv) what are the distributions of exponents in each interval studied in (iii) above.

22. Section 5: I am unable to interpret the results until the definitions and methodology are made more clear. Equation (28) has no clear definition from the quantity in equation (27) and is the primary issue for interpreting the section. What are we studying when we vary δ? The FSLE defined in the equation $\Lambda(\delta_n) = \frac{\log(\sigma)}{\langle \tau(\delta_n) \rangle}$ is defined via three parameters: n, σ and δ_0. What is being varied in equation (28)? Likewise, what is δ in the horizontal axis of Figure 8.a? I will be interested in reviewing this section carefully when this is made more clear.

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