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Nonlinear Processes in Geophysics
Discussions

Interactive comment on "Application of local attractor dimension to reduced space strongly coupled data assimilation for chaotic multiscale systems" *by* Courtney Quinn et al.

Anonymous Referee #2

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This a very interesting paper. It is extremely well written, with very few typos and it is a pleasure to read it. Nonetheless, the study and the manuscript have in my opinion a few flaws that need amending.

1. Although the bibliography is rather dense (and better done than usual), there are still a few (very) relevant references such as Palatella and Trevisan (2015); Grudzien et al. (2018a,b) that are missing.
2. Even though the number of CLVs incorporated in the DA algorithm depends on time, you still need to compute a number of CLVs corresponding to the maximum

local KY, do you? So that the gain is computationally limited. If I am wrong, please explain.

3. In at least a few experiments, you need to use optimally tuned inflation (section 5.3 for instance), since it is already known that the lack of span of the unstable modes can be compensated with by a stronger multiplicative inflation. Otherwise several of your claims are undermined.
4. The new inflation scheme is not justified enough. Beware that it has been tested in a very specific case and does not warrant generality.
5. Because of the above points, there are too strong statements in the conclusion regarding the novelty and performance of the proposed method.

I believe that the paper only requires minor revisions before acceptance. But the above points should be seriously addressed and perhaps a few experiments re-run with multiple inflation values.

You will find below a list of minor suggestions or related to the points above, which could help improve the manuscript:

1. p.1, l.5: "to determine" is a bit ambiguous as it could also need "to infer" which would be a bold statement. I guess you meant "to prescribe", right?
2. p.2, l.2: "implying very large ensemble sizes are needed" $\longrightarrow$ "implying that very large ensemble sizes are needed"
3. p.3, l.6: What are "local CLVs"? I know local LEs, but not local CLVs.
4. p.3: Even though the beginning of the paper is very good and enjoyable, reading "We also examine the role of correlated versus random observational errors.",

"along with a novel scheme for adaptive Kalman gain inflation." at the end of the introduction is odd as these two subjects do not seem directly connected to the main objective of the paper, and they seem, at this stage of the reading, unnecessary.

5. p.4, l.18: "uncentering parameters": please explain what this means.
6. p.4, l.27: "We are interested in analysing both the local and global dynamics of system (1).": I guess you mean the short-term and asymptotics dynamics – your words lack accuracy here.
7. p.6, l.16: Can you be sure/prove that the last digits of 5.9473 are relevant?
8. p.7, l.2: "approach their corresponding asymptotic values" $\longrightarrow$ "approach their corresponding LEs asymptotic values"
9. p.7, l.3: You have to discuss/justify more the concept of local KY dimension. This is the main idea of your paper.
10. p.7, l.4: "We see the local dimension" $\longrightarrow$ "We see that the local dimension"
11. p.7, l.13: "the cocycle of": Please explain what a cocycle is. How is it connected to (1)?
12. p.8, l.7: "local time-varying Kaplan-Yorke dimension.": what is its interpretation? This is key to your paper.
13. p.8, l.16-19: Palatella and Trevisan (2015) could be mentioned here.
14. p.9, l.4: "Suppose there exists" $\longrightarrow$ "Suppose that there exists"
15. p.9, Fig.5, legend: replace the “v” symbol by “or” for the sake of clarity.
16. p.9, l.9-11: The errors are also supposed uncorrelated in time (white).

17. p.9, Eqs.(8,9): Missing punctuation.
18. p.10, l.5: "There is difficulty" $\longrightarrow$ "There is a difficulty"
19. p.10, l.7: "assumption of linearity": this is confusing here since you have not introduced the extended Kalman filter yet.
20. p.10, l.21: Even though quoting Bishop et al. (2001) is certainly adequate, a reference to Hunt et al. (2007) is also missing as it is equally relevant.
21. p.10, Eq.(14) is wrong, is it? It should be

$$\mathbf{E} = \mathbf{R}^{-1/2}\mathbf{H}\mathbf{X}^{\mathrm{f}}. \tag{1}$$

Also it is not recommended to use $\mathbf{E}$ as it is usually used for the full ensemble matrix. Authors often use $\mathbf{S}$ instead.

22. p.11, l.7: "The Kalman gain K is defined through equation (9a)": No. Not in the classical ETKF (see Hunt et al. (2007)).
23. p.11, l.15-20: In this context, sampling errors are actually due to nonlinearity, as it was explained and proven by Bocquet et al. (2015); Raanes et al. (2019).
24. p.11, l.25-27: "This differs to past approaches where the subspace was determined in terms of the long time averaged (invariant) unstable and neutral CLVs (Trevisan and Uboldi, 2004; Carrassi et al., 2008; Trevisan and Palatella, 2011).": This statement is misleading. You just mean that the number of retained CLVs is kept fixed. Did you?
25. p.12, l.7-8: "We compute CLVs at the assimilation step using Algorithm 1.": The number of CLVs is fixed over the full time span of the algorithm, is it?
26. p.12, l.19: "regardless of observation set" $\longrightarrow$ "regardless of the observation set"

27. p.12, l.23: The term "Analysis window" is unfortunate as it usually refers to the time range over which asynchronous observations are assimilated in 4D-Var or with an ensemble smoother. I guess you mean the time interval between updates. You could denote it Δt for instance. Please change it throughout the manuscript.

28. p.13, section 5.1: Please explain better what changing the observation set has to do with the main goal of the manuscript.

29. p.13, l.9-10: "We argue here that in reality, the true variance of the observation error can be spatially dependent and errors are often correlated in time.": this is a bit too much, since there are quite a few DA papers dealing with at least spatially correlated errors.

30. p.13, l.27: "we decrease the analysis window and do not perturb the control run at all when taking the observations.": You mean that the synthetic observations are not perturbed, do you? The sentence seems a bit twisted.

31. p.15, l.15: "Finally we analyse our novel reduced subspace method which uses a variable number of CLVs based on the local Kaplan-Yorke dimension.": yes, but I guess you need to compute a number of CLVs corresponding to the maximum local KY dimension, so that even though it is theoretically interesting, it is, in practice, of limited interest.

32. p.17, Fig. 7 (a-c): please plot over a smaller range, typically [500-600] as in Fig. 16.

33. p.18, Table 3, and discussion around: This experiment does not account for what is actually known in the literature. You should have made an experiment with the 6 CLVs but with optimally tuned inflation, or you could have used the finite-size EnKF (Bocquet et al., 2015, and references therein). It is by now well known that the gap between the second and the third experiment might be compensated by optimally tuned inflation.

34. p.19, l.16-18: "but we are interested if we can preserve": I don't really understand the phrase.

35. p.21, l.1: "in ability" $\longrightarrow$ "in the ability"

36. p.25, l.5-10: In this paragraph, you argue but you don't give a strong rationale for the inflation scheme you propose. You need a stronger case to convince the reader. All the more since the inflation scheme is tested with a toy model in a very specific configuration.

37. p.25, Eq.(25): The modified $\mathbf{P}^{\mathrm{f}}$ does no have the good engineering dimensional (cube in the anomalies instead of square). What do you make of this?

38. p.25, l.23-24: It seems like the β-factor approach is implementing deflation. Is it so? If yes, please use the term deflation instead of inflation, which is customary.

39. p.27, l.1: "there is remarkable" $\longrightarrow$ "there is a remarkable"

40. p.27, l.7-8: "We have demonstrated the varying rank of the error covariance matrix related to the transient growth in the stable modes of the system.": true but this was already emphasised in the literature, so that your implicit statement of novelty should be tuned down here.

41. p.27, l.16-17: "to determine the rank": unclear and confusing; I believe you mean "to specify the appropriate rank"; you don't discover the rank, you set it.

42. p.27, l.22-24: "In particular, we found that spanning the space comprised of the asymptotic unstable, neutral, and first weakly stable mode (5 CLVs in this case) performed much worse than using either dimension measure (asymptotic and local).": I don't think so. This is one weak point of your study. It is known that in this case, the inflation must be adjusted to account for the error upscaling from the the region of the spectrum (Grudzien et al., 2018a,b). I don't believe that

you have tuned the inflation, have you? If true, your statement appears to be too strong.

43. p.28, l.19-20: "The adaptive scaling introduced here can be applied to general systems with weak coupling, although care may need to be taken in the choice of the norm.": No, you have not proven anything like that. Please remove the statement.

44. p.28, l.33-34: "Future work should also consider the numerical cost of CLV calculation and methods to increase efficiency for high dimensional systems": At the very end, you raise what experts familiar with AUS have in mind reading your paper: what you propose is certainly interesting and of theoretical interest but of lesser practical value since (i) one needs to compute the CLVs alongside (ii) with the variable CLV context, you need to compute the maximum number of CLVs. You should mentioned this point way earlier in the paper, unless I am mistaken.

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