Response to anonymous reviewer comments

Dear Reviewer,

We first thank you for your general positive evaluation of the manuscript. We agree that the points of concern that you raised add valuable discussion to the work. Below we address your comments in detail. We also note where we will make adjustments to the manuscript accordingly.

Many thanks,

Courtney Quinn, Terence O'Kane, and Vassili Kitsios

Main points:

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) [9]; Grudzien et al. (2018a,b) [4, 5] that are missing.

   We thank the reviewer for pointing out the additional relevant references and we will include them in accordance with the specific suggestions below.

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.

   In the way that we have implemented the variable dimension, one could compute the local KY measure first and then only compute the number of CLVs corresponding to that given value at each assimilation time step. (This is due to the fact that we compute dimension with the QR-factorization approach and the CLVs with a separate algorithm - clarification of this and further discussion has been added to the modified manuscript.) With respect to computational gain, there will be an additional cost associated with computing the unstable manifold regardless of the method. Whether or not this is realizable in high dimensional systems, or how the overall cost compares to increasing ensemble size in order to fully sample the variance are not questions we attempt to answer in this study. We rather focused on the applicability of assimilation in a variable unstable subspace for different situations of strongly coupled data assimilation (*i.e.* different types of observational subsets). We discuss the reviewer's comments regarding computational cost more within the specific points below.

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.

   We first preface our response with bringing to the reviewer's attention that there was a small error in the calculation of the CLVs for the results shown in the manuscript submission. We have since corrected this error and updated all of our experiments. The updated tables are included at the end of our responses below. Due to these corrected results, we now can conclude for the benchmark and atmosphere observations that all experiments are successful. The emphasis of the discussion is now regarding the variable CLV full RMSE being closest to the full rank experiments for each of the observation sets. These conclusions hold for a range of inflation values. We show in Table 1 below the results for the benchmark observations using full rank, 5 CLVs, and variable CLVs with inflation values of 1% (as in manuscript - revised values), 2%, 5% and 10%. As the lowest RMSEs occur for the 1% inflation cases, we conclude this is the optimal inflation value and leave these results in the manuscript. We have added a statement noting this to the beginning of the results section (Section 5 in the manuscript). The optimization was already done for the shadowed observations (Section 5.5), and the optimal inflation values are given in the table. All other experiments have an optimal inflation of 1%.

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.

   We agree with the reviewer that our language when discussing the utility of the adaptive scaling for the Kalman gain is too strong. We have changed the language to emphasize that the adaptive scaling works in this particular case, but needs to be tested on more models to form a a more general applicability. We also have added additional justification for the proposed scheme and relate it to the work of Miller *et al* [7]. More detailed discussion of our justification can be found in response to the specific points of the following section.

5. Because of the above points, there are too strong statements in the conclusion regarding the novelty and performance of the proposed method.

   Again, we agree with the reviewer that too strong of language is used in regards to some of the conclusions. We note that due to our corrected results we have already adjusted some of our conclusions accordingly. We will additionally adjust the specific statements mentioned in the following section.

Method	Observations [error variance]	⟨RMSE⟩ extratropical	⟨RMSE⟩ tropical	⟨RMSE⟩ ocean	⟨RMSE⟩ full	$\langle \dim_{KY} \rangle$
CLVs - 9 1% inflation	y_e, y_t, Y [1, 1, 25]	0.3142	0.1598	0.4948	0.4027	5.8928
CLVs - 5 1% inflation	y_e, y_t, Y [1, 1, 25]	0.3123	0.1843	0.5920	0.4550	5.8870
CLVs - variable 1% inflation	y_e, y_t, Y [1, 1, 25]	0.3215	0.1688	0.5346	0.4272	5.8863
CLVs - 9 2% inflation	y_e, y_t, Y [1, 1, 25]	0.3286	0.1893	0.5985	0.4625	5.8881
CLVs - 5 2% inflation	y_e, y_t, Y [1, 1, 25]	0.3274	0.1950	0.6083	0.4681	5.8871
CLVs - variable 2% inflation	y_e, y_t, Y [1, 1, 25]	0.3219	0.1916	0.5966	0.4594	5.8915
CLVs - 9 5% inflation	y_e, y_t, Y [1, 1, 25]	0.3564	0.2627	0.8858	0.6237	5.8832
CLVs - 5 5% inflation	y_e, y_t, Y [1, 1, 25]	0.3600	0.2665	0.8880	0.6264	5.8836
CLVs - variable 5% inflation	y_e, y_t, Y [1, 1, 25]	0.3552	0.2647	0.8747	0.6180	5.8848
CLVs - 9 10% inflation	y_e, y_t, Y [1, 1, 25]	0.4117	0.3719	1.3278	0.8799	5.8763
CLVs - 5 10% inflation	y_e, y_t, Y [1, 1, 25]	0.4187	0.3873	1.3168	0.8787	5.8824
CLVs - variable 10% inflation	y_e, y_t, Y [1, 1, 25]	0.4123	0.3731	1.3153	0.8736	5.8775

Table 1: Summary metrics of DA experiments using right-transform matrix (ETKF) and benchmark observations (y_e, y_t, Y). The angle brackets $\langle \cdot \rangle$ denote average over analysis steps. Compare to results in [12]. Parameters: analysis window 0.08, 10 ensemble members.

Specific suggestions:

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?

   We have changed the statement to read "to prescribe" instead, as this was our intended connotation.

2. p.2, l.2: "implying very large ensemble sizes are needed" → "implying that very large ensemble sizes are needed

   We have amended the sentence.

3. p.3, l.6: What are "local CLVs"? I know local LEs, but not local CLVs.

This was a typo. We were referring to the CLVs calculated at a given time. We have removed the term “local”.

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.

   We can agree that the statements are superfluous. We will save the discussion of these two topics for when they arise in the study.

5. p.4, l.18: “uncentering parameters”: please explain what this means.

   The terminology “uncentering” was originally used in the Peña and Kalnay study [10] and refers to the uncentering of the unstable zero equilibrium in the classical Lorenz model. Dynamically, the k_1 and k_2 values used in the manuscript allow for the second unstable asymptotic Lyapunov exponent (for $k_1 = k_2 = 0$: $\lambda_1 = 0.91, \lambda_2 = 0, \lambda_i < 0$ for $i > 2$).

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.

   That is precisely what we meant. We will change the language as suggested.

7. p.6, l.16: Can you be sure/prove that the last digits of 5.9473 are relevant?

   We do not claim that the last digits are relevant, they are included only as the Lyapunov exponents were given up to four digits.

8. p.7, l.2: “approach their corresponding asymptotic values” → “approach their corresponding LEs asymptotic values”

   We have changed the sentence to read “approach the asymptotic Lyapunov exponent 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.

   We have added some additional discussion around the concept of local dimension used in our manuscript as suggested.

10. p.7, l.4: “We see the local dimension”→ “We see that the local dimension”

    We have amended the sentence.

11. p.7, l.13: "the cocycle of": Please explain what a cocycle is. How is it connected to (1)?

    In relation to system (1) in the manuscript, a cocycle is the forward and backward mapping of solutions under the tangent dynamics. The cocycle is then given as $\mathcal{A}(x(t), \tau) = \mathrm{e}^{\tau Jf(x(t))}$ where Jf denotes the Jacobian of f, the right-hand-side of system (1). We have clarified this in the manuscript and have restructured the sentence such that *a prior* knowledge of the term cocycle is not assumed.

12. p.8, l.7: "local time-varying Kaplan-Yorke dimension.": what is its interpretation? This is key to your paper.

    We have added a more descriptive explanation of the local Kaplan-Yorke dimension here, particularly relating to our implementation and its relation to the CLV behaviour.

13. p.8, l.16-19: Palatella and Trevisan (2015) [9] could be mentioned here.

    We have added the reference.

14. p.9, l.4: "Suppose there exists" → "Suppose that there exists"

    We have amended the sentence.

15. p.9, Fig.5, legend: replace the "v" symbol by "or" for the sake of clarity.

    We have changed the legend accordingly.

16. p.9, l.9-11: The errors are also supposed uncorrelated in time (white).

    We have clarified the assumption of temporally uncorrelated errors in the description of the Kalman filter method.

17. p.9, Eqs.(8,9): Missing punctuation.

    We have added punctuation to the noted equations.

18. p.10, l.5: "There is difficulty"→"There is a difficulty"

    We have amended the sentence.

19. p.10, l.7: "assumption of linearity": this is confusing here since you have not introduced the extended Kalman filter yet.

    We have removed the statement in which this phrase appears as it is not critical to the reader.

20. p.10, l.21: Even though quoting Bishop et al. (2001) [1] is certainly adequate, a reference to Hunt et al. (2007) [6] is also missing as it is equally relevant.

    We have added the reference as suggested.

21. p.10, Eq.(14) is wrong, is it? It should be

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

    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.

    We thank the reviewer for catching this typo. We did in fact mean to define the matrix as above, as the left multiplication with its transpose is included in Eq (15). We have also changed the notation from $\mathbf{E}$ to $\mathbf{S}$ as suggested.

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) [6]).

    We have changed the text to specify that we are following the definition of the Kalman filter as given in Bishop et al [1].

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) [2]; Raanes et al. (2019) [11].

    We have included the additional references here where we discuss the sampling errors due to nonlinearity.

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?

    This is badly phrased. We meant "asymptotic Lyapunov exponents", and therefore the rank of the error covariance matrix is kept fixed. We have updated the manuscript accordingly.

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?

    We always compute all 9 CLVs in this case, and then only use the number corresponding to the experiment set-up in constructing the covariance matrix. In this toy model we were not as concerned with optimizing computation time. However, it is easily implemented to only compute a subset of the CLVs either specified as constant or variable based on the calculated dimension for the assimilation step. (Note, the dimension is calculated from the FTLEs computed using the QR method so this is a separate process to the CLV calculation.) We discuss more about computation time in response to the final comment (44) below.

26. p.12, l.19: "regardless of observation set" → "regardless of the observation set"

    We have amended the sentence.

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.

    We have changed "analysis window" to "assimilation window" throughout the manuscript (Δt conflicts with our integration time step notation).

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

    The different observation sets relate to the exploration of strongly coupled data assimilation (strong CDA), namely the use of cross-covariances to update unobserved states. We were interested in whether the use of the reduced space method (either fixed or variable) is applicable in different observation scenarios, including the cases where some subsystems are left unobserved or where there are temporally correlated errors. We have emphasized this in the introduction to the results section (Section 5) and in Section 5.1.

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.

    We have revised the tone of the sentence which now reads: "In many applications the true variance ..."

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.

    We have amended the sentence to read: "we decrease the assimilation window and assume perfect observations."

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.

    We take this as a comment. To clarify, as mentioned in our response to point 25, one only needs to compute the number of CLVs corresponding to the local Kaplan-Yorke dimension for a given assimilation step. In our experiments this varies between 0 and 8 for different assimilation steps. We discuss practical implementation in response to the reviewer's final point below.

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

    We have changed the range of both figures (16 and 17 in previous manuscript) to [450-550] in accordance with the discussion around the dynamical properties.

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.

    This point was addressed in the main comments section above. With our correction to the CLV calculation, we no longer have such a discrepancy between experiments. The corrected table can be found in the following section. We have additionally done an optimization on inflation and find that the 1% is in fact optimal for these experiments.

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

    We have changed the sentence to read: "we are interested to see if we can avoid collapse (*i.e.* the loss of variability) in the ensemble mean of the extratropical attractor".

35. p.21, l.1: "in ability" → "in the ability"

    We have amended the sentence.

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.

We have added further discussion of the Miller *et al* [7] study to motivate our proposed modified Kalman gain. Through the study of data assimilation schemes on the Lorenz '63 model, they find that the forecast error covariance is often underestimated in highly nonlinear systems, particularly when the model is in a region of phase space subject to transitions. This leads to the Kalman gain being underestimated. The authors account for this by including the third and fourth moments of anomalies within the Kalman gain calculation, however this is done for the Extended Kalman Filter (EKF) method (see eq. 4.3 of [7]). O'Kane and Frederiksen [8] also derive a higher order gain for a closure-based statistical dynamical Kalman filter applied in spectral space (see eq. 27 of [8]). Here we use the notion that increased spread in a subsystem represents the inability of ensemble members to track the same transitions in phase space. In such a situation the forecast error covariance is likely to be underestimated (presumably due to emerging importance of higher moments), therefore we scale the forecast error covariance within the Kalman gain calculation by a factor that represents a measure of overall spread in the system. This then provides an adaptive scaling of the Kalman gain at every assimilation time step based on the background performance of the system (large spread implies an increase in Kalman gain, small spread implies a decrease). The scaling can equivalently be written as an observation error variance scaling, and its overall behaviour is a balancing between the forecast error covariance and the observation error variance within the calculation of the Kalman gain. We have expanded upon this discussion within the manuscript.

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

    Our introduction of the scaled Kalman gain was written poorly here. We actually do not modify the forecast error covariance matrix as worded in the original manuscript. We add an additional scaling term to the forecast error covariance matrix only within the calculation of the Kalman gain. The scaling factor ($||\mathbf{P}^f||$) itself is a measure of the anomalies squared, therefore making the quantity used in the Kalman gain a quartic measure of anomalies. In a crude way one could consider this an incorporation of higher moments into the Kalman gain, however since this is not a true estimate of the fourth moment, we used the spread-based argument for its motivation (as given above).

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.

We have changed the language to refer to the β-factor approach as deflation.

39. p.27, l.1: "there is remarkable" → "there is a remarkable"

    We amended the sentence.

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.

    We agree with the reviewer that our statement was too strongly worded. We have amended the sentence to read: "We have explored the varying rank of the error covariance matrix related to the transient growth in the stable modes of the system, and in particular the applicability of this varying rank on different configurations of strong CDA."

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.

    That is correct, we meant the connotation "to specify". We have revised the sentence accordingly.

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.

    We have toned down the language around the discussion of these results after the correction to the calculation. We no longer claim the 5 CLV case performs "much" worse, as all methods now perform comparably. We point out, however, that the 6 CLV and variable CLV both show a reduction in the full RMSE which can be related to significant linear growth in more than one stable mode. We also have included the reference [5] within our discussion of inflation in Section 4.1, and the reference [4] in discussing the impact of transient growth of asymptotically stable modes on model errors in the introduction.

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.

Instead of removing, we have amended the statement to one which reflects our original sentiment. We wanted to suggest that the method be tested on other systems of weak coupling and then perhaps a more general statement can be made under deeper analysis. We have changed the statement to read "The adaptive scaling introduced here should be tested on additional systems with weak coupling in order to assess its general applicability, although care may need to be taken in the choice of the norm."

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.

    The reviewer is correct in that we do not address computational cost throughout the paper. This was mainly intentional, as we did not aim to optimize computational performance, but rather explore a method that allows for the reduction of phase space and/or better representation of errors in that reduced subspace. Whether or not there can be a computational cost reduction by using this method over spanning the space needed to fully sample the variance is a question we do not attempt to answer as it will depend on the system one wishes to assimilate. In accordance with the reviewer's request, we have added a footnote within the introduction at the point where we discuss assimilation in the unstable subspace (AUS). The footnote acknowledges the additional cost that comes with computing the unstable subspace which may or may not be less than the cost of sampling the full model variance, and it also states that we will leave the exploration of numerical efficiency in high dimensional systems for future study.

    We additionally note that the projection onto CLVs is an example of one specific projection that works for the variable local dimension, but it is not the only one. One could also project onto the backwards Lyapunov vectors (BLVs) which are computationally less expensive to compute and attain similar results. Table 2 below includes the results of variable dimension and projection onto BLVs. We have added a discussion of this alternate projection into the manuscript, as well as the additional statistics of the variable BLV experiment. However, we focus on the CLVs in this study as the *a posteriori* analysis of CLV alignment can provide more information of the time-varying phase space behaviour. (The BLVs are orthogonal by construction.) The revised manuscript has an additional discussion of CLV alignment, variable dimension, and specifically the relationship to ensemble data assimilation which justifies the utility of CLVs.

We imagine that it is possible to find a computationally efficient projection for high dimensional systems that incorporates the variable dimension aspect. Our main goal in this manuscript though was to introduce the implementation of time-varying dimension in this toy model with different observational sets to understand in what configurations one might expect successful results. It would be of interest for practical applications if future studies could explore numerically efficient projections that span the time-varying dimension and therefore utilise the ideas of AUS in high dimensional systems.

Minor corrections to DA experiments

After submission we discovered a small error in the orthogonalisation step of the CLV calculation within the DA experiments. In correcting we found that using the CLV without orthogonalisation to the backwards subspaces (Algorithm 2.1 in [3]) was more appropriate due to the lack of an accurate forward model within the DA implementation. This has minor impact on the results of the experiments, however we include the tables with the updated statistics below which we will also update in the new manuscript.

Method	Observations [error variance]	$\langle$RMSE$\rangle$ extratropical	$\langle$RMSE$\rangle$ tropical	$\langle$RMSE$\rangle$ ocean	$\langle$RMSE$\rangle$ full	$\langle \dim_{KY} \rangle$
CLVs - 9 (full rank)	y_e, y_t, Y [1, 1, 25]	0.3142	0.1598	0.4948	0.4027	5.8928
CLVs - 5 (unstable/neutral subspace + 1)	y_e, y_t, Y [1, 1, 25]	0.3123	0.1843	0.5920	0.4550	5.8870
CLVs - 6 (global dimension)	y_e, y_t, Y [1, 1, 25]	0.3156	0.1674	0.5513	0.4310	5.8892
CLVs - variable (local dimension)	y_e, y_t, Y [1, 1, 25]	0.3215	0.1688	0.5346	0.4272	5.8863
BLVs - variable (local dimension)	y_e, y_t, Y [1, 1, 25]	0.3149	0.1658	0.5122	0.4141	5.8895

Table 2: Summary metrics of DA experiments using right-transform matrix (ETKF) and benchmark observations (y_e, y_t, Y). The angle brackets $\langle\cdot\rangle$ denote average over analysis steps. Compare to results in [12]. Parameters: assimilation window 0.08, inflation factor 1%, 10 ensemble members.

Method	Observations [error variance]	$\langle$RMSE$\rangle$ extratropical	$\langle$RMSE$\rangle$ tropical	$\langle$RMSE$\rangle$ ocean	$\langle$RMSE$\rangle$ full	$\langle \dim_{KY} \rangle$
CLVs - 9	y_e, z_e, y_t, z_t $[1, 1, 1, 1]$	0.1734	0.1332	0.5782	0.4515	5.8672
CLVs - 6	y_e, z_e, y_t, z_t $[1, 1, 1, 1]$	0.1715	0.1386	0.5807	0.4524	5.8718
CLVs - variable	y_e, z_e, y_t, z_t $[1, 1, 1, 1]$	0.1697	0.1383	0.5659	0.4433	5.8681

Table 3: Summary metrics of DA experiments using right-transform matrix (ETKF) and atmosphere observations (y_e, z_e, y_t, z_t). Parameters: assimilation window 0.08, inflation factor 1%, 10 ensemble members.

Method	Observations [error variance]	$\langle$RMSE$\rangle$ extratropical	$\langle$RMSE$\rangle$ tropical	$\langle$RMSE$\rangle$ ocean	$\langle$RMSE$\rangle$ full	$\langle \dim_{KY} \rangle$
CLVs - 9	y_t, z_t, Y, Z $[1, 1, 25, 25]$	7.0467	0.1571	0.3675	4.7182	4.5449
CLVs - 9	y_t, z_t, Y, Z $[0.01, 0.01, 25, 25]$	5.5203	0.0480	0.1764	3.6860	5.4052
CLVs - 9	y_t, z_t, Y, Z $[1, 1, 0.25, 0.25]$	7.1308	0.1386	0.0873	4.7566	4.4959
CLVs - 9	y_t, z_t, Y, Z $[0.01, 0.01, 0.25, 0.25]$	5.1697	0.0431	0.0822	3.42478	5.4899

Table 4: Summary metrics of DA experiments using right-transform matrix (ETKF) and ENSO observations (y_t, z_t, Y, Z). Parameters: assimilation window 0.08, inflation factor 1%, 10 ensemble members.

References

[1] Craig H Bishop, Brian J Etherton, and Sharanya J Majumdar. Adaptive sampling with the ensemble transform kalman filter. part i: Theoretical aspects. *Monthly weather review*, 129(3):420–436, 2001.

[2] Marc Bocquet, Patrick N Raanes, and Alexis Hannart. Expanding the validity of the ensemble kalman filter without the intrinsic need for inflation. *Nonlinear Processes in Geophysics*, 22(6):645–662, 2015.

[3] Gary Froyland, Thorsten Hüls, Gary P Morriss, and Thomas M Watson. Computing covariant lyapunov vectors, oseledets vectors, and dichotomy projectors: A comparative numerical study. *Physica D: Nonlinear Phenomena*, 247(1):18–39, 2013.

[4] Colin Grudzien, Alberto Carrassi, and Marc Bocquet. Asymptotic forecast uncertainty and the unstable subspace in the presence of additive model

Method	Observations [error variance]	⟨RMSE⟩ extratropical	⟨RMSE⟩ tropical	⟨RMSE⟩ ocean	⟨RMSE⟩ full	$\langle \dim_{KY} \rangle$
CLVs - 9 3% inflation	$\tilde{y}_e, \tilde{y}_t, \tilde{Y}$ $[1, 1, 25]$	0.5200	0.1635	0.4255	0.4552	5.9576
CLVs - variable 3% inflation	$\tilde{y}_e, \tilde{y}_t, \tilde{Y}$ $[1, 1, 25]$	0.4848	0.1568	0.4095	0.4263	5.9371
CLVs - 9 3% inflation	$\tilde{y}_e, \tilde{z}_e, \tilde{y}_t, \tilde{z}_t$ $[1, 1, 1, 1]$	0.4258	0.1342	0.4300	0.4687	5.9237
CLVs - variable 3% inflation	$\tilde{y}_e, \tilde{z}_e, \tilde{y}_t, \tilde{z}_t$ $[1, 1, 1, 1]$	0.4337	0.1373	0.4027	0.4649	5.9191
CLVs - 9 4% inflation	$\tilde{y}_t, \tilde{z}_t, \tilde{Y}, \tilde{Z}$ $[1, 1, 25, 25]$	6.8613	0.1431	0.3025	4.5961	4.5309
CLVs - variable 4% inflation	$\tilde{y}_t, \tilde{z}_t, \tilde{Y}, \tilde{Z}$ $[1, 1, 25, 25]$	6.8722	0.1463	0.3370	4.6054	4.5541

Table 5: Summary metrics of DA experiments using right-transform matrix (ETKF) and shadowed trajectory as observations. We set the observation error covariances to the standard values as in [12]. Parameters: assimilation window 0.08, 11 ensemble members, inflation as noted in table.

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Method	Observations [error variance]	$\langle$RMSE$\rangle$ extratropical	$\langle$RMSE$\rangle$ tropical	$\langle$RMSE$\rangle$ ocean	$\langle$RMSE$\rangle$ full	$\langle \mathrm{dim}_{KY} \rangle$
CLVs - 9	x_e, y_e, z_e [1, 1, 1]	0.0640	8.4752	36.3662	21.7108	4.1332
CLVs - var	x_e, y_e, z_e [1, 1, 1]	0.0670	9.4872	40.4894	24.1528	4.1152
CLVs - 9 adaptive gain	x_e, y_e, z_e [1, 1, 1]	0.0032	0.7241	3.5757	2.1504	5.9991
CLVs - var adaptive gain	x_e, y_e, z_e [1, 1, 1]	0.0034	0.8350	4.0632	2.4501	5.8888

Table 6: Summary metrics of DA experiments using left-transform matrix (ESRF) and the full extratropical subsystem as observations (x_e, y_e, z_e). We use perfect observations (no random error added to the control run) with the observation error covariances set to the standard values as in [12]. Parameters: assimilation window 0.02, inflation factor 1%, 10 ensemble members.

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