Response to minor review/comments - *egusphere-2022-434*:

Guidance on how to improve vertical covariance localization based on a 1000-member ensemble

by Tobias Necker, David Hinger, Philipp Johannes Griewank, Takemasa Miyoshi, and Martin Weissmann

We would like to thank the editor and reviewer for their additional comments. We appreciate the time invested during the revision process. Below we respond to the comments and further explain how we addressed them in the revised manuscript. The **original queries are bold**, and our responses are normal text. We explain all changes and provided line numbers (e.g., L1 = Line 1) that refer to the newly submitted pdf latex diff document.

RC3
Suggestions for revision by Submitted on 11 Nov 2022
Anonymous referee #1

Discussions about the symmetric, positive semi-definite (SPSD) feature for the localization matrix are added. Several potential ways to guarantee a SPSD localization matrix are mentioned. As the authors mentioned in the text that EOL can play the role of inflation or deflation if needed. It would be helpful to add a little discussion about the impact of inflation on the SPSD localization matrix.
Indeed, the impact of inflation (of variances) on SPSD would be an interesting topic to study but appears beyond the scope of our paper.
Please note that the EOL does not play the role of “inflation,” as it does not inflate or deflate variances. By definition, the EOL corrects sampling errors in correlations. Values larger or smaller than one on the diagonal of the localization matrix (which would impact variances) do not originate from the EOL but could be provoked by SPSD methods and, thus, should be fixed.
We removed or replaced the word “inflation” where required to avoid confusion for readers.
(L234; L400-401)

EC3
As you may have seen, one referee has submitted a comment on the new revised version of the paper. Although she has not let her name explicitly known this time, the referee is Lili Lei, who was referee 1 of the first version of your paper (she has agreed that I let again her name known to you).

She only asks you to comment on the link between inflation and the SPSD character of localization matrices. Please respond to that comment.
Please see our response to the reviewer's comment above.

As Editor, I also have a number of comments that you will find below (the line numbers are those of file egusphere-2022-434-ATC2.pdf, which contains explicitly the modifications you have made on the previous version).

You have now studied in some detail the SPSD character of the localized matrices you have obtained. I have first a few remarks on this point.
1. The need for localized matrices to be SPSD should be mentioned, and at least briefly discussed, in your introduction (and mentioned in particular in the paragraph ll. 85-91 where you present the general structure of your paper).
We now mention the need for SPSD localization in the introduction twice. (L77-78; L90-91)

2. You have used the nearest correlation matrix (NCM) algorithm defined by Higham (2002), which identifies the SPSD matrix that is nearest to a given symmetric matrix. It could be useful to mention by which measure the distance between two matrices is defined in that algorithm.
The distance is measured using weighted Frobenius norms. We added a comment to Sect. 3.4. in the manuscript. (L430-431)

3. LL. 435-436, … the SPSD localization performs only marginally worse than the EOL localization. Obscure. From what I understand, there are no two localization methods to be compared. You have implemented the NMC algorithm on the results of EOL localization, and the result is a slight degradation of the error against the 1000-member ensemble.
We rephrased the sentence to avoid confusion. (L437-438)

4. Ll. 498-499. Applying an NCM algorithm for achieving positive semi-definiteness resulted in only very minor changes of the EOL that hardly affect the error reduction. Since that seems to be your main conclusion concerning the SPSD character of the localization matrix, I think it should be mentioned in the abstract. I add that, since EOL is intended at minimizing an error, it is not surprising that NCM increases that error. A more significant test (possibly for a future work) would be of course to consider the impact on the analysis which follows the localization.
Indeed, it would be very important to investigate the impact on the analysis and forecast, which we hope to examine soon using an OSSE setup.
We added a short comment to the abstract and now mention the error reduction. A longer comment was not possible due to the word limit for the abstract (200 words). (L12)

And for other comments
5. Ll. 45-46 (and elsewhere) …. a Gaussian-shaped tapering function …. The words Gaussian-shaped, where I understand you actually mean unimodal (a unique maximum, with possibly symmetry about that maximum) is not very appropriate (in particular, the functions you consider become zero at finite distance).
We replaced Gaussian-shaped with Gaspari-Cohn, to be precise. (L44-45)

6. L. 156, … Gaspari-Cohn function (GC; Eq. 4.10, Gaspari and Cohn, 1999) … It could be good to give the explicit expression of the function that you are referring to here.
We think it is not necessary to add the explicit expression as the function is referenced. (L45)

7. L. 116. Mention the year (2016, according to the caption of several Figures)
We added the year. (L117)

8. L. 131, … a sample of state vectors xn … (with explicit superscripts)
We added the superscript. (L132)

9. L. 186, … with unique members … , obscure. I understand without overlap.
We rephrased the sentence to avoid confusion and relaced “unique” with “without repetition.” (L187)

10. L. 224, Eq. (5), mention that S is the number of 40-member ensembles (S = 25)
We now include the definition of S. (L227)

11. L. 271, The variability between day to night also appears to be small. That does not seem to be visible from what you show. Insert the words (not shown) or something similar. The same remark applies to other places, for instance to l. 357.
We added “(not shown)” as suggested. (L270; L357)

12. L. 366, what are the GC setups that are supposed to be listed in Sec. 2.3.1 ? Be more explicit, or remove.
We slightly adapted section 3.3.3 to avoid confusion, and are now more explicit. (L365-371)

13. L. 397, … derogates the error reduction. That makes no sense to me. Do you mean … increases the error. ?
We replaced “derogates” with “leads to less” error reduction. (L397)

14. Ll. 464-465, The shapes that we found included Gaussian, exponential or linear functions. Well, you have shown none of that.
We agree that this comment was not precise and removed it. (L467-468)

15. Fig. 7. The standard deviations on the All curves cannot be seen (whether on the screen or on a print-out). If standard deviations are so small that they cannot be distinguished from the (rather thick) curves themselves, say it (the same remark may apply to other figures, please check that aspect carefully).
We checked all figures and added a comment to the caption of Fig 7 that refers to the small standard deviation. (see Fig7)

16. L. 448, Our analysis includes three prognostic variables (humidity, temperature, and wind). Well, you have so far spoken of four variables, including the two wind components.
We changed three to four and now mention the “horizontal wind components”. (L450-451)

Please revise your paper according to Lili Lei’s comment, as well as to mine. In case you disagree with a particular comment, or decide not to follow a particular suggestion, please state precisely your reasons for that.

I thank you for having thought of Nonlinear Processes in Geophysics for submitting your paper, and look forward to receiving a further revised version.
We addressed all comments and hope that the manuscript in its present form can be accepted for publication in NPG. Thank you!

REFERENCE
Higham, N. J.: Computing the nearest correlation matrix—a problem from finance, IMA Journal of Numerical Analysis, 22, 329–343, https://doi.org/10.1093/imanum/22.3.329, 2002.