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.

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 your paper).

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.

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.

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.

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).

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.

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

8. L. 131, … *a sample of state vectors* x^n **…** (with explicit superscripts)

9. L. 186, … *with unique members …* , obscure. I understand *without overlap*.

10. L. 224, Eq. (5), mention that *S* is the number of 40-member ensembles (*S* = 25)

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.

12. L. 366, what are the *GC setups* that are supposed to be listed in Sec. 2.3.1 ? Be more explicit, or remove.

13. L. 397, … *derogates the error reduction*. That makes no sense to me. Do you mean … *increases the error*. ?

14. Ll. 464-465, *The shapes that we found included Gaussian, exponential or linear functions*. Well, you have shown none of that.

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).

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.

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.

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.