

Report #1

Submitted by referee #3

General Comments

The editor asked me to check whether the reviewer comments were properly addressed, and when reading them I understand why. The response letter is poorly organized and difficult to follow. In the future, copy the comments into a separate document and insert your answers directly after each comment. This way it is easy to see which question you are addressing.

Because of this, I am reading the manuscript as if it were submitted for review for the first time. I am focusing on the statistical part of the manuscript, as I presume the editor selected me as a reviewer due to my expertise in the statistical analysis of atmospheric data.

The authors would like to apologize for the extra time that was necessary in reviewing our poorly formatted initial response letter. We ensure that the current document is formatted with our responses inserted directly after each reviewer comment.

Specific comments on the manuscript:

Lines 5-6: stating varimax as more advanced method than EOF is a bit of overkill. Varimax is just an additional step in the analysis to make the components clearer and more interpretable

The authors agree that varimax is simply a step to consider in the analysis rather than a separate method, and we have adjusted our language to reflect this observation.

Line 107: the text jumps from EOF to PCA without acknowledging that they actually are the same computational method with just a bit different applications and interpretation. Now this information comes only >220 lines later

We thank the reviewer for this comment, as we had not realized there was a jump in our manuscript before relaying the necessary information. We have corrected this by ensuring we present the relevant discussion before jumping from EOF to PCA.

Lines 308-309: be careful when stating SVD and eigenvalue decomposition equivalent as the computation differs (Golub and Van Loan, 1996) and they may yield somewhat different outcomes (e.g. Isokääntä et al. 2020).

We thank the reviewer for this comment, as it is an important point that should not be overlooked. We especially appreciate that the reviewer has mentioned references to

help us to easily address this concern. We respond by adding comments with references to clarify that the two methods may yield different results.

Lines 392-393: Generalizing assumption that climate data would be gaussian and not skewed would need really strong references. Thus, I recommend softer or case specific wording

We clarify here that climate data can only be considered Gaussian in specific circumstances. For example, mean annual temperature records that do not show an abrupt increase in warming or cooling may be considered Gaussian. We support this with an included citation:

Skelton, A., Kirchner, N., & Kockum, I. (2020). Skewness of temperature data implies an abrupt change in the climate system between 1985 and 1991. *Geophysical Research Letters*, 47, e2020GL089794. <https://doi.org/10.1029/2020GL089794>

Lines 438-449: Why six components? For PCA there are multiple different methods for helping to find optimal number of components (see e.g. Isokääntä et al. Sect 3.3 and references therein). Thus, instead of speculating with redundant/noise containing components, you could see what these tests say. Naturally, they do not give absolute truth, as in PCA the final number of components is always a result of subjective, expert inference.

We thank the reviewer for bringing up this point and for including references we could use for further analysis. We add a discussion to our results to clarify our reasoning for displaying the first six components.

Figure 4: adding line for cumulative variance explained would be helpful

The authors agree that adding cumulative variance would be helpful, and as such, we add new lines to Fig. 4 to show the cumulative variance for both varimax and unrotated modes. We use this line to better show how much variance we lose by choosing to perform SSA analysis on our unrotated modes as opposed to our varimax modes. We also alter the color scheme to ensure that each plot is easy to read.

Figure 5.: I may not understand the dataset well enough, but I would assume that the seasonality is quite different when comparing northern and southern latitudes. Thus I do not understand the choice to show the overall effect. Would three panels give more information (north, mid, south)? And when I scroll down, the seasonal variation shows in fig 6, justifying my point.

We would like to thank the reviewer for this comment as we agree that showing the effect of the decomposition for multiple latitude regions would be more helpful. We

have added 3 additional panels that show the effect of the decomposition for Northern Extratropics, Southern Extratropics, and Tropical regions. A New discussion has been added to address these new panels in Fig. 5, as well as a new discussion that connects Fig. 5 to the EOF patterns in Fig. 6.

Discussion/Conclusions/Abstract: recommendation to use cyclostationary EOF comes from nowhere as you have not used it in your analyses. Why are you recommending something you are not proving to be valid? If you write it like this, it raises a question that if you consider it as superior method why did you no use it in your analyses?

Thank you for this comment, as it brings up an important oversight of language. We have removed our recommendation from the abstract and changed the language in our discussion/conclusions so that it is not a formal recommendation, and instead, we simply discuss possible exploration of the method in the future.

In the future, I recommend on putting some effort on organizing the manuscript such that the figures would be closer to text they are referred. E.g here, fig 5 is mentioned on page 16 but the figure is on page 18, Fig 6 is mentioned on top of page 17 but the figure is on page 20. Not to mention fig 8, mentioned on page 19 while the figure is on page 22 and one needs to scroll over two other figures to see it.

The authors would like to apologize for the figures being scattered through the text and difficult to follow. We have since reorganized them, so they are closer to the text referring to them.

References

Golub, G. H. and Van Loan, C. F.: Matrix Computations, 3 ed., The Johns Hopkins University Press, Baltimore, 694 pp., 1996.

Isokääntä, S., Kari, E., Buchholz, A., Hao, L., Schobesberger, S., Virtanen, A., and Mikkonen, S.: Comparison of dimension reduction techniques in the analysis of mass spectrometry data, Atmos. Meas. Tech., 13, 2995–3022, <https://doi.org/10.5194/amt-13-2995-2020>

Report #2

Submitted by referee #1

The manuscript has improved significantly and may be suitable for publication after minor technical changes. The authors are requested to edit the manuscript to improve its structure, flow, clarity, brevity, and overall presentation.

The authors have done their best to address this comment in several ways, including the following:

1. What used to be our methods Sect. 2.3 has now been moved to become results Section 3.1 because the plots of our dataset are part of our results and should be included as such.
2. We removed our section in Appendix A1 as we felt it was not relevant to our main discussion. We also removed our equation describing skewness in Sect. 2.4.3, as we felt it was not necessary.
3. We reorganized our section 2.3.2 so that our text on limitations of EOFs is better organized to stay with the topic that it is describing.
4. Some of our section titles have been relabeled to be more descriptive
5. We added a summary at the beginning of Sect. 3 to give the reader an easy-to-follow structure for what our main results are.
6. As requested by referee #3, we reorganized the placement of figures so that they occur nearer to the text in which they are referenced.
7. We made changes to the structure and text in our conclusions to help improve their flow and clarity.
8. We added more references to figure numbers in our conclusions to make our results easier to track.