

## **Response letter to the review of the revised manuscript**

*Title: Interactive 3-D visual analysis of ERA 5 data: improving diagnostic indices for marine cold air outbreaks and polar lows*

Authors:

Meyer, M., Polkova, I., Modali, K. R., Schaffer, L., Baehr, J., Olbrich, S., Rautenhaus, M.

Dear Reviewers,

Dear Co-Editor,

we greatly appreciate the positive review of the revised manuscript. The suggestions of reviewers have helped further improving the manuscript. All comments have been addressed in this minor revision.

Our answers to each of the reviewer's comments (black font colour) are provided below in red font colour. The references to lines and paragraphs in our responses below refer to the revised manuscript without tracked changes. The revised manuscript with tracked changes shows all changes conducted as part of this minor revision (i.e., compared with the last submitted version of the manuscript).

Yours sincerely,

Marcel Meyer and Marc Rautenhaus, on behalf of all co-authors.

## Response to Reviewer #1 (RC1)

Thank you very much for reviewing the revised manuscript. We gratefully acknowledge the constructive and positive review, which has helped further improving the manuscript.

## RC1 - Introductory Comment:

*The authors put a lot of effort into the revision of the paper, whose presentation is now much improved, and they responded well to my earlier comments. The intention of the paper now comes out clearly and the methodology is very well explained. The paper provides an important step forward in terms of diagnostic indices for MCAOs and identifying regions favourable for PL development. And it is an excellent showcase for the potential of 3D interactive analysis in meteorology. Hence, I am convinced it provides a valuable contribution to WCD.*

*I have one main comment and a few minor comments, as detailed in the following [...].*

Response:

- We appreciate the positive evaluation and the additional suggestions for further improvements.

Action:

- We conducted a minor revision of the manuscript to address all comments. Details are given below.

RC1: *The collocation of the maximum MCAO depth and the occurrence of a PL is perhaps not too surprising given that PLs often require for their genesis some upper-level forcing in the form of a positive potential vorticity anomaly, as you outline in 3.2.3. Such upper-level positive potential vorticity anomalies are generally associated with an upward doming of the underlying isentropic surfaces (e.g., Hoskins et al. 1985) and, hence, also of the zero-isosurface  $m_{\theta_p}$ , leading to reduced stability (and lifting). This relationship is the likely reason why the new MCAO index performs well in predicting PL occurrence; via the upward doming of the upper boundary of the MCAO, it contains implicit information on upper-level forcing. The conventional MCAO index does not contain such information and instead it is purely a measure of the coldness of the CAO air relative to the sea surface. The coldness of the air is likely not the most critical parameter for PL development as long as upper-level forcing is absent. I think the authors should discuss this aspect.*

Response:

- Yes, we agree, this is a good complement for the discussion. One of our initial thoughts behind the new Polar Low Index was that it could potentially capture this interplay of lower-level and upper-level anomalies even better than the new MCAO index, but as results indicate, it appears the simpler new MCAO index does already contain the implicit information about the upper-level forcing, as it performs just as well and even a bit better than the new Polar Low index.

Action:

- We add a discussion about the implicit information about upper-level forcing contained in the new MCAO index to the revised manuscript (see line 542f, third paragraph in the Conclusion, revised manuscript).

RC1 - L50: MCOA -> MCAO

Response & Action:

- Thanks, corrected.

RC1 - L80: *Even though 3D depiction of atmospheric fields is not the standard, meteorologist are well aware of the potential of 3D visualization since a long time, see for example Figs. 6.12 and 6.14 in Uccellini (1990).*

Response & Action:

- Yes, agreed and added to the introduction (see line 78-79, Introduction, revised manuscript).

RC1 - L102: *No need to mention the 37 standard pressure levels on which ERA5 output is available since you are anyway using data on model levels.*

Response & Action:

- Agreed and deleted.

RC1 - Caption Table 1: *Briefly mention in the caption the meaning of the different symbols (e.g.,  $p_{tr}$ ,  $p_0$ ,  $p^*$  etc...)*

Response & Action:

- Yes, included.

RC1 - L173: *Maybe rephrase as "... if the lower-level instability extends all the way to the tropopause." or similar. I don't think an MCAO induces an instability, but the instability is rather a defining characteristic of an MCAO.*

Response & Action:

- Re-phrased, thanks.

RC1 - L195: *Fig. B1 suggests that the lysis point is selected randomly within a circle with a radius corresponding to the mean track distance around the genesis point. Is this correct? The way it is written now, one may think that several such increments are computed*

Response:

- No, that's not correct. The tracks are generated incrementally. In the first step, we generate a random genesis location. In the second step, we randomly generate the direction for the PL. Subsequently, we generate a set of randomized track-increments. The path-direction chosen randomly at the beginning dominates the direction of all subsequent track-increments, but we allow for very small random variations around the initially chosen direction at each increment. This leads to almost straight track paths but does allow for some small variations. We also define a small random component on the length of each track-increment, such that the overall track length of all random control events corresponds to the observed mean track length of all PL events in STARS, but there are some small variations amongst individual pseudo-events. We chose this setup to include some variability in the randomly defined control-events, as a simple way for approximating some of the variability in the observed tracks.

Action:

- We adapted the phrasing (see line 194ff, revised manuscript) and replaced one of the examples in Fig. B1 with another example from our analysis for clarification.

RC1: *Suggest to remove "more realistic insights". I don't think one way or the other of depicting meteorological data is more realistic, but the 3D approach is certainly useful for an interactive exploration.*

Response:

- We agree, it is difficult to say what is more realistic in general and have removed this part of the sentence.

RC1 - L289: *delete "in"*

Response:

- Thanks, corrected.

References:

Hoskins, B. J., M. E. McIntyre, and A. W. Robertson, 1985: On the use and significance of isentropic potential vorticity maps. *Quart. J. Roy. Meteor. Soc.*, 111, 877–946, doi:10.1002/qj.49711147002.

Uccellini, L. W., 1990: Processes contributing to the rapid development of extratropical cyclones. *Extratropical Cyclones. The Erik Palmén Memorial Volume*, C. W. Newton, and E. O. Holopainen, Eds., American Meteorological Society, Boston, USA, 81–105.

Response:

- Both references are included in the revised manuscript.

Response to Reviewer #2 (RC2)

Thank you very much for the positive and thorough review. We have addressed all comments in the revised manuscript.

RC2 - Line 40: *"sea surface potential temperature over the ocean": it seems redundant to have sea surface and ocean here. Suggestion: potential temperature calculated using the sea surface temperature.*

Response & Action:

- We adapted the phrasing to avoid appearing redundant (see line 40, revised manuscript).

RC2 - Line 102 (and Appendix A): “137 vertical model levels, 37 pressure levels”. I am confused here: what are you actually using in Met.3D, model levels or pressure levels? Has the vertical interpolation onto pressure level to be performed before using Met.3D or does Met.3D interactively perform the vertical interpolation? Moreover, in Appendix A, only horizontal remapping is mentioned. Therefore, I suggest to explicitly write about the vertical interpolation somewhere, either in Appendix A if the vertical interpolation is performed before using Met.3D or in Sect. 2.1 if the vertical interpolation is performed in Met.3D. The authors can also maybe explicitly write the 37 pressure levels available as well.

Response:

- For all our key analyses we are using data on model levels. During initial testing and for the example in Fig. D1-b in the Appendix, we additionally used data on pressure levels. Met.3D can process both, data on pressure and on model levels, and the vertical interpolation is done on-the-fly.

Action:

- We removed the pressure levels from the text (see line 102f, revised manuscript) to avoid misunderstandings and added to Sec. 2.1 (line 107, revised manuscript) a note about internal on-the-fly vertical interpolation as part of Met.3D.

RC2 - Line 105: I do not understand why a polar stereographic projection is required before using Met.3D (what does it do to the grid of the data?). Isn't it Met.3D itself that performs the projection at the plotting stage? If yes, I suggest a sentence like “Met.3D displays fields using a polar stereographic projection.” or similar. Moreover, can the grid step in degrees be written here (for example 0.5°x0.5°)?

Response:

- Met.3D performs the projection of simple 2D line-geometries (such as country borders) at the plotting stage, but it does not perform the projection of the 3-D data variables at the plotting stage. That's because this would slow down the 3D visualization algorithms (e.g., for computing 3D iso-surfaces) too much for interactive visual analysis. Therefore, the projection is done in a pre-processing step using the climate data operators.

Action:

- We added the grid resolution to the text (see lines 105f, revised manuscript).

RC2 - Line 120: 3-D features -> the 3-D structure

Response & Action:

- Yes, adapted phrasing (see line 121, revised manuscript).

RC2 - Line 129: “on a large geographical domain (all longitudes; northern latitudes in the interval 25-90°” -> over the Northern Hemisphere (north of 25°N)

Response & Action:

- Yes, thanks, good point; we re-phrased (see line 130f, revised manuscript).

RC2- Lines 129-130: *I suggest to remove “grid-dimension in lat-lon height: 261x1441x137”.*

Response & Action:

- Yes, deleted.

RC2 - Line 132: *I suggest to also remove “on a smaller grid (440x440x137)”. The first “440” is not smaller than the “261” of line 130 so that is confusing. I suggest to replace “on a smaller grid....Nordic Seas” with “on a smaller domain covering the Barents and Nordic Seas”. The polar stereographic aspect is already mentioned in Sect. 2.1.*

Response & Action:

- Yes, thanks, adapted to simplify the sentence (see line 131ff revised manuscript).

RC2 - Line 136: *I do not understand “at which air aloft is considered for calculation of the MCAO index”. I suggest to remove as the rest of the sentence is clear without this part.*

Response:

- This was a reminder/repetition of the meaning of the characteristic pressure level, as the level at which potential temperature is taken for the calculation of conventional indices.

Action:

- We deleted this part of the sentence to avoid repetition and address the comment (see line 136-137, revised manuscript).

RC2 - Line 164: *“without lower-level instability”: does it mean that the lowest-level potential temperature is larger than the potential skin temperature? If yes, maybe write it (with a formula).*

Response:

- We compare potential air temperature with potential skin temperature for a set of vertical levels, and if potential skin temperature is smaller than potential air-temperature for all vertical levels, including the bottom ones, than we define this as “no lower-level instability”.

Action:

- We clarified the phrasing in the revised manuscript (see line 164, revised manuscript).

RC2 - Line 171: *remove “(in hPa)” as no units are written except in Table 1.*

Response & Action:

- Yes, adapted phrasing.

RC2 - Lines 214-216: *if the hours with  $m_p=0$  are not considered in the temporal average, then I suggest to remove “and setting the index to zero for all other hours” and instead write that the index is equal to 0 if all time steps have  $m_p=0$ . Or is there something I have misunderstood?*

Response:

- You are correct, thanks, the phrasing contained a kind of duplication.

Action:

- We have adapted the phrasing (see line 216ff, revised manuscript).

RC2 - Lines 319, 394, and caption of Fig. 6: *I like the additional composites performed by the authors that I think really show the differences between the conventional and new MCAO indices. However, I do not think the term “long-term average” describes what is calculated. From my understanding, what is done here is a composite of the maps of the indices averaged over the polar lows duration.*

Response:

- For each single event (PL and pseudo-event), we average the index values, as described in the text. For the composite analysis we compute the average of index values during all events (PL and pseudo-events, respectively). We agree that the term “long-term average” could be interpreted as considering index values for all days of the time-period, which would be misleading.

Action:

- We rephrased the caption to Fig. 6 and the text around line 394.

RC2 - Section 3.3.2: *would it make sense to switch the 3rd (“The number...” ) and 4th (“To assess...” ) paragraphs and maybe merge the new 4th paragraph with the 5th (“Results...” )? I think it would read better.*

Response:

- Yes, agreed.

Action:

- Re-structuring of parts of Section 3.3.2.

RC2 - Line 398: *to help the reader, maybe point at the area between Iceland and Norway where there are more yellow colours in Fig. 6c than in Fig. 6d -> (e.g., compare the values in the area between Iceland and Norway in Fig. 6c,d).*

Response:

- Yes, good point, thanks.

Action:

- We adapted the text (see line 398, revised manuscript).

RC2 - Line 421: *does a true positive rate of 1 imply that the index always correctly detect a PL (100% match)? If yes, could it be mentioned somewhere (main body or caption of Fig. 7) to more easily understand the ROC curves?*

Response:

- Yes, a true positive rate of 1 means that all PLs would be detected.

Action:

- We mention perfect true positive rate in the text (see line 424, revised manuscript).

Technical comments:

RC2 - Line 50: *MCOA -> MCAO*

Response: Thanks, corrected.

RC2 - Line 65: *“that maximizes the link to observed PLs” -> that maximizes this link.*

Response: Yes, thanks, adapted.

RC2 - 212: *shouldn't it be  $t^*$ start and  $t^*$ stop in the parenthesis (with the stars)?*

Response: Yes, thanks, corrected.

RC2 - Line 213: *end -> lysis*

Response: Ok, adapted.

RC2 - Lines 219, 302: *upper level -> upper-level*

Response: Yes, adapted.

RC2 - Line 239: *Movies 1-2 -> Movies 1, 2*

Response: Ok, adapted.

RC2 - Line 289: *remove one “in”*

Response: Thanks, corrected.

RC2 - Line 321: *remove comma after “areas”*

Response: Thanks, corrected.

RC2 - Line 338: *Fig. 5-h -> Fig. 5h*

Response: Thanks, corrected.

RC2 - Caption of Fig. 5: *“(>=95-th percentile)” is correct for the MCAO indices (panels c and f) but shouldn't it be the 5th percentile for the polar low index (panel i)?*

Response: Yes, it is, thanks. We have corrected the text in the caption accordingly.

RC2 - Line 404: *remove comma after “Sect. 2.4)”*

Response: Ok, adapted.

RC2 - Line 406: *Fig. 6-e -> Fig. 6e*

Response: Yes, adapted.

RC2 - Eq. 6: *columns (:) after ifs are not needed*

Response: Ok, simplified.

RC2 - Line 433: *Fig. 7 -> Fig. 7a*

Response: Yes, specified here and for 7b as well.

RC2 - Line 463: *add a full stop (.) at the end of the sentence after “Table 2)”*

Response: Thanks, corrected

RC2 - Line 576: *proof -> prove*

Response: Thanks, corrected.