Weather and Climate Dynamics Discussions

[WCDD]

Interactive comment

Interactive comment on "Polar Lows – Moist Baroclinic Cyclones Developing in Four Different Vertical Wind Shear Environments" *by* Patrick Johannes Stoll et al.

Anonymous Referee #2

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In this study, a classification of polar lows (PLs), characterized by a large variety of cloud structures, large scale environment, and intensification mechanisms is proposed It is based on PLs detected in ERA-5 during the period 1999-2019, and makes use of Self-Organizing Maps (SOM). Such a method provides different patterns of variability which can be then connected to the vertical wind shear. Five diffrent configurations are found, of which 4 correspond to a strong shear. The orientation of the shear is found to be determinant for the dynamics of the system (which justifies previous classifications in forward and reverse shear PLs). In addition, it is found that there is no evidence for the existence of hurricane-like PLs that would intensify mainly by latent heat release. Spiraliform clouds would rather correspond to secluded cyclones.

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General comments : This is an original study on PLs for at least two reasons : so far the ability of ERA-5 to represent PLs has not been assessed, and the use of the SOM method fot displaying typical patterns in high-dimensional data, widely used by the scientific community, is certainly promising. The paper is clearly written. There are however a number of points that need to be clarified before the paper can be accepted for publication. Most of them pertain to the methodology, and have an influence on the interpretation of the results.

-Regarding the method :

a. PL detection and representation

1. How is the detection/tracking performed in ERA-5 ? In the Rojo list, there are primary PL tracks, as well as secondary PL ones (in this case, there is usually no threshhold on the associated surface wind speed, so that it is difficult to ensure that all are true PLs, according to the definition of Heinemann and Claud, 1987). Authors mention that they detect 243 of the 262 PL events of the list. In those 243 events, what is the proportion of primary PL which is detected ? This might surely affect the results (see below).

Heinemann, G., and C. Claud (1997), Report of a workshop on "Theoretical and observational studies of polar lows" of the European Geophysical Society Polar Lows Working Group, Bull. Am. Met. Soc., 78, 2643–2658.

2. A fair trajectory does not necessarily ensure that the PL is well represented, and in particular, surface wind speeds have been observed to be often under-estimated in previous reanalyses, even after downscaling (e.g. Laffineur et al, 2014). Could the authors comment on this?

3. Such methods usually detect more systems than in the reality. How many false positive PLs have been detected? Are they discarded ? In relation with the preceding point, the method of detection which has been selected is also questionable, Laffineur et al writing that "caution is required with use of the 850-hPa vorticity, which may be

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indicative of troughs but not necessarily closed mesocyclonic circulations".

4. Is there a difference in the representation/detection of PL between the Norwegian and the Barents Sea, as noted by Smirnova and Golubkin, 2017 ?

This part is absolutely fundamental, since it is the basis for the results which will be obtained subsequently.

b. Regarding the use of the SOM method, there is a point that must be better justified : It seems to me that PL for which the shear situation changes during their lifetime (what is their proportion ?) should be discarded (I suspect that this would drastically reduce the size of the samples, which might be problematic). Otherwise this certainly affects the results and should prevent from drawing general conclusions. Also, would the results be modified if only primary PLs were used ?

- Concerning the interpretation of the results, one conclusion would be that there is no hurricane-like development. It might be the case in ERA-5, but this does not ensure that this is true in reality. This conclusion is too strong (at least based on the results presented here). On one side, they may not be (all) represented, and on the other hand, since these cases are probably seldom, it may well be that the method tends to smooth them. Is this method appropriate for cases that occur only occasionally ?

Minor points :

- last line of page 2 : "without an a-priori determination of a variable used for the categorisation". I don't understand this point. To me, the SOM method is applied on a single variable which has been choosen -T anomaly at 850 hPa- , it is not the algorithm which determines the variable(s) to be considered.

- Rojo et al, 2019, JGR should be quoted. (see PANGAEA site).

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