Responses to the second round of comments from the second reviewer.

We thank the referee for a second review.

Reviewer: "Regarding the PL detection and representation in ERA-5, the choice of the threshold values (tuned to have a good agreement with the Rojo list) remains a bit arbitrary. I do not understand what is meant by the authors when they mention that the results are QUALITATIVELY insensitive to the exact setting in the track matching an exclusion procedure."

Response: We agree that this formulation was a bit vague and therefore include a new paragraph at the end of the method section: "Our results are robust across multiple sensitivity tests in which subgroups of the PL track points were used: (i) PL tracks that match to a primary PL from the Rojo list, with a lifetime of at least 12 h, and a maximum life-time environmental near-surface wind speed exceeding 20 $\mathrm{m\,s^{-1}}$, (ii) PL tracks that match in at least 5 track points with the same PL from the Rojo list within a distance of less than 75 km, PL track points from (iii) initial, (iv) mature, and (v) lysis time steps."
Test (i) was presented in the first response. The remaining tests are provided in the following.

Reviewer: "I am also wondering whether considering all the tracks within a distance of less that 150km to a PL for at least one time step to be matches is conservative enough. Said differently, I am afraid that systems which are not PLs are included in the analysis."

Response: We provide another sensitivity test to show that the matching procedure does not influence the results. For this test we selected all PL tracks that match the same PL from the Rojo list for at least 5 different observations (from Rojo) within a radius of 75 km. 165 of the 370 PLs satisfy this matching criteria, which contain 7,630 of the original 12,695 time steps. Note, that several of the PLs from the Rojo list do not have 5 observations. The resultant SOM matrix (Fig. 1) is similar to the one from the manuscript. SOM nodes representing each of the shear categories discussed in the manuscript are recognised in this SOM matrix. Hence, the analysis described in the manuscript could be performed on basis of this subset of PL tracks.

Reviewer: "I do not agree with the explanation given to Reviewer 3 (Resp Line 116) that PLs in the Rojo list are seriously truncated because the observations which are uneven may miss the genesis and lysis periods (at least not to the point that the average duration should be 35,4 hours in ERA-5 while in Rojo et al, 2015 it is stated than most of the systems last less than 24 hours). This is a clearly a drawback in the representation in the reanalyses that should be mentioned (and this has an effect on the SOM analysis, see below)."

Response: In the previous response we discussed that a reanalysis-based climatology

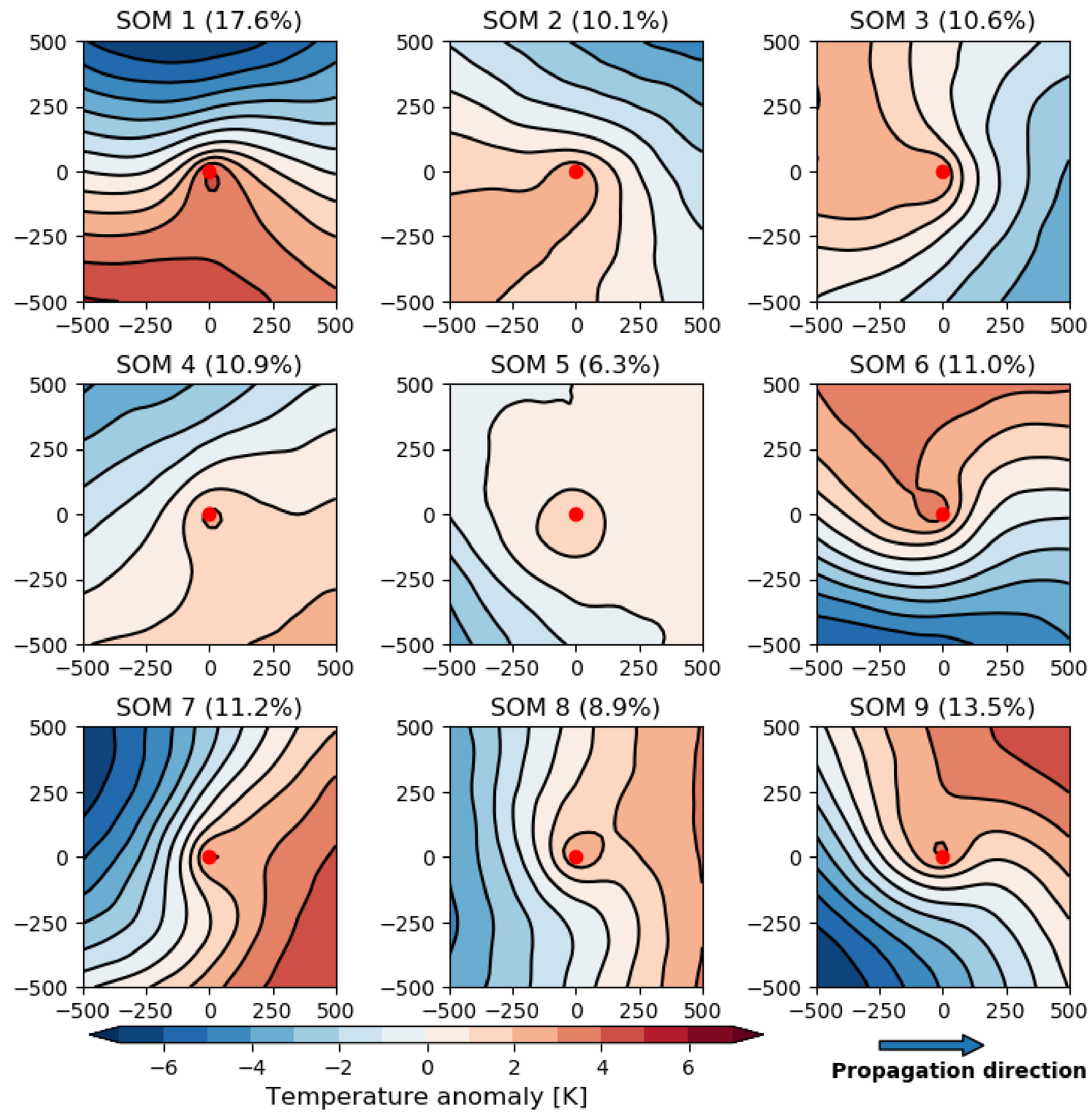


Figure 1: As Figure 3 of the Supplement, but based on the subset of 165 PLs with 7,630 time steps that satisfy additional criteria of strict matching.

is likely biased towards longer live times, as longer living PLs are more likely well represented in the reanalysis. We also hypothesised that observational climatologies are likely underestimating the lifetime as a satellite image of the genesis and lysis time may not exist due to occasional time gaps of multiple hours between satellite images that were used for the derivation of the climatology. We now provide some more evidence: The Rojo list for example includes two PLs that have only one time step (nr. 243 and 111.b). Hence it is obvious that the full lifetime of these two PLs is not captured in the observational climatology. Further, multiple PLs in the Rojo list have less then 4 time steps. Additionally, Rojo et al. [2015] state: ”In nine PL cases (out of 190 total), the formation or dissipation phases could not be observed because the temporal spacing of images did not allow tracking of such rapidly evolving systems for their entire lifespan. Despite the fact that these tracks

are probably abbreviated, we still consider them in this study."
We added an extra paragraph to the manuscript: "The resulting mean lifetime of ~35 h (13,221 hourly time steps divided by 374 PLs) is slightly longer than that observed by Rojo et al. [2015], who found that two thirds of the PLs detected from satellite images lasted for less than 24 h. This difference may be explained by biases in both the model and the observational dataset. Our reanalysis-based dataset is likely biased towards longer lifetimes, as longer-living and larger PLs are better reproduced by the reanalysis. In contrast, satellite-based datasets, such as Rojo et al. [2015], are likely underestimating the lifetime based on the first (last) satellite image from which a PL may be observed after (before) the PL genesis (lysis) for two reasons: (i) Sometimes the time gap between two consecutive satellite images is multiple hours [Rojo et al., 2015], (ii) the PL is not identified due to other disturbing cloud structures [e.g. Furevik et al., 2015]."

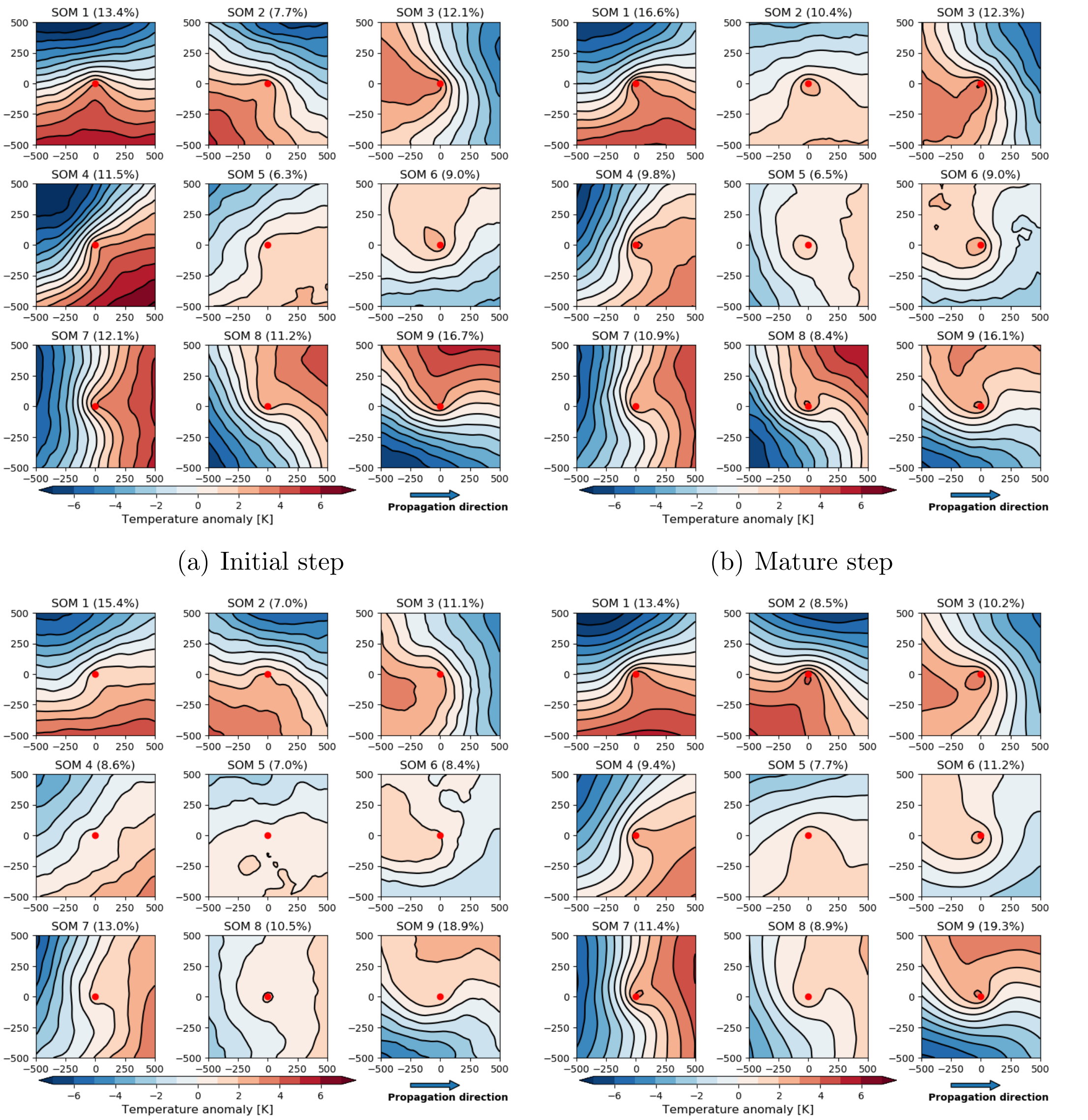
Reviewer:"Regarding the use of the SOM method, I have 2 main points: - can the choice of a 3x3 nodes array be better justified?"

Response: We formulated in the manuscript L.192ff: "We find an array of 3×3 nodes to be most suitable, reducing 12,695 PL-centred fields to 3×3 archetypal nodes. Larger arrays mainly display additional details of minor interest (Supplementary Fig. 2), whereas smaller arrays merge nodes that contained relevant individual information." Note, that the 4×5 array from Supplement Figure 2 shows basically the same structures as the 3×3 and we could derive the shear categories from the 4×5 array in a similar way as described in the manuscript. Hence the size of the array has no influence on the results.

Reviewer: "- would it make sense to run the method separately for genesis, mature and lysis phases to better isolate the mechanisms (I agree with Reviewer 4 that it is not totally correct to speak of development for the lysis phase)? Also this would counterbalance (at least to some extent) the fact that PLs with a longer lifetime have a larger effect on the analysis."

Response: We performed the SOM analysis for the genesis, mature, and lysis time steps, respectively, and the results are similar to the analysis based on all time steps as utilised in the manuscript (Fig. 2). Main difference: The horizontal temperature gradients are largest at the initial and smallest at the lysis time steps. This result is included in the manuscript in Section 3.3. Further, the SOM patterns based on individual time steps during each PL (Fig. 2a-c) are locally more variable than the SOM patterns based on all time steps ((Fig. 2d), as the SOM method is based on 370 instead of 12,665 time steps.
This analysis shows again that the obtained results are robust for variations in the method. Finally, the intention here is to show the progression over different (SOM) states during the PL life time, which requires that the SOM is constructed based on all time steps.

Reviewer: "How do the results of this study relate to those which consider weather



(a) Initial step (b) Mature step

(c) Lysis step (d) all steps

Figure 2: As Figure 3 of the Supplement, but based on the (a) initial, (b) mature and (c) lysis time step of each of the 370 polar lows. For comparison (d) shows Figure 3 of the Supplement.

regimes to describe the synoptic conditions prevailing in PL cases? In my opinion, this is something that is missing and should be discussed in the paper."

Response:
Contextualising our work with previous work on the association of PL development with weather regimes could indeed be an interesting addition. We thus added the following paragraphs to the manuscript at the end of section 3.5:
"Multiple studies have investigated PL development associated with different weather regimes [e.g. Blechschmidt, 2008, Claud et al., 2007, Mallet et al., 2013, Rojo et al., 2015]. Comparing the typical PL propagation direction and synoptic-scale compos-

ite maps associated with the different weather regimes [e.g. Fig. 12 and 13 of Rojo et al., 2015]) and shear conditions (Fig. 7), it is apparent that forward-shear conditions somewhat resemble Scandinavian Blocking (SB), reverse shear the negative phase of the North Atlantic Oscillation (NAO-), left shear the NAO+, whereas right and weak-shear situations are difficult to associate with a specific weather regime. However, composite maps of wind at 850 hPa for the Atlantic Ridge, NAO+, and NAO- featuring PLs [Rojo et al., 2015, Fig. 13a-c] are quite similar for the Norwegian and Barents Sea. Hence, the association of specific weather regimes with different shear conditions has to be considered with caution.
Furthermore, the synoptic situation for the weather regimes differ in the area of PL formation depending on whether or not PLs form [Mallet et al., 2013, Fig. 10]. For example, Mallet et al. [2013] found a pattern anti-correlation of -0.4 between the normal SB pattern and the SB pattern when PLs occur. Thus, weather regimes mainly indicate whether the synoptic situation might be generally conducive for PL development, whereas the shear categories successfully identify synoptic conditions leading to different types of PL development (Fig. 7)."

Reviewer: "What is the degree of confidence which can be put in ERA-5 precipitation fields in case of PLs? Using ERA-5 precipitation to assess the relative importance of convective processes in weak/strong shear classes is really questionable."

Response: We agree that precipitation fields of ERA-5 have to be considered with some caution. However, the humidity profiles for the PL studied in Stoll et al. [2020] were of comparable quality for the ECMWF HRES model, which is quite similar to ERA-5, and a high-resolution convection-permitting model, which indicates that ERA-5 may represent moist processes reasonably well.
We present results from ERA-5 in Section 3.3 and in the conclusion we are clear about this: "Generally our analysis **based on ERA-5** provides no evidence for the occurrence of hurricane-like intensification of PLs predominantly by convective processes within an environment of low vertical shear." If the reader does not trust ERA-5, we propose the idea to test the hypothesis with a high resolution data set (see last point of this response).
Thus, the statement that convective processes appear less important for the weak than for the strong shear class makes sense within the ERA-5 model world. We see two main processes that may contribute to convection: surface sensible heat fluxes and latent heating by condensation, where the precipitation is an appropriate measure for the latter. ERA-5 captures both strong and weak-shear situations, so also weak-shear situations are produced. Both processes are, in the ERA-5 model world, of lower strength for weak than for strong shear situations.

Reviewer: "Last sentence: CARA should be described (at least in a general way), or omitted. Is it possible to add a reference?"

Response: We include a short description of CARA and improved the reference. Last sentence of the manuscript: "To further clarify this hypothesis, studies using high-resolution datasets, such as the European regional atmospheric reanalysis

CARA with a model grid-spacing of 2.5 km [Copernicus, 2020], could be used to investigate the life-cycle of PLs."

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Copernicus Arctic Regional Reanalysis Service: https://climate.copernicus.eu/copernicus-arctic-regional-reanalysis-service, last access: 2020-06-11