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

Anonymous Referee #4

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The paper of P. Stoll and co-authors "Polar lows – moist baroclinic cyclones developing in four different vertical wind shear environments" investigates the polar lows configurations characterized by the vertical wind shear. The study is dedicated to a very sharp theme in mesoscale meteorology and climatology. A wide variety of modern methods and products are used. While studying the baroclinic polar low development, the authors claim that a hurricane-like development is not presented in the STARS dataset according to the ERA-5 data.

The manuscript presents new and valuable scientific results, well written and illustrated. At the same time, several critical issues might significantly impact the results. I list

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major and minor comments below and suggest a major revision of the manuscript.

Major comment #1

Lines 138-139 – One may say that if PL radii vary from 150-600 km, in the case of small PL, you take a too large area around the vortex, which is not associated with its core. In the opposite situation, if the PL radius is 600 km, you cut the periphery, which is known to be the area of the maximum wind speed and turbulent fluxes. Depending on the addressed question, the area of 250 km around the PL center is too small (if you look at the largescale environment) or large (if you look at the processes inside the vortex itself). This might have a significant effect on the resulted composites. I would instead suggest using the estimate of PL radii presented in Rojo et al., 2015, and a buffer zone of a fixed size to cover all PL in the same way. To provide the PL on the even grid, you can normalize them before the interpolation, like somewhat presented in Rudeva and Gulev, 2011 (Rudeva, I., and S.K. Gulev Composite analysis of the North Atlantic extratropical cyclones in NCEP/NCAR reanalysis. Mon. Wea. Rev., 139, 2011, 1419-1436.)

Lines 155-157 – The same as for the previous comment. If you consider the mean wind vector in the area with a 500 km diameter around the PL center, in the case of large PLs (even 450 and 600 km in horizontal), you don't catch the environmental mean flow at all. My suggestion is to reconsider the area of the PL, at least as in the previous comment.

Figure 8 – you never describe how you compute the composites since the simple mean for even a regular grid is not enough – all PLs have different sizes. Did you do the normalization by the PL diameter? Please, see Tilinina et al., 2018, for the description of what composites are.

Major comment #2

Lines 226-227 Isn't it evident that baroclinic instability-induced vortices become ther-

mally homogeneous at the lysis stage? Wouldn't it be more correct to exclude lysis stages from the analysis of "archetypal meteorological conditions during the PL development" (line 62)?

Figure 2: 1. The evolution-transition blue arrows are messy and make the figure unreadable. 2. Again, the figure accounts for the same mesocyclones at different stages, which is quite confusing. I would rather plot the percentage of timesteps presented by each lifecycle stage in a particular node than the absolute value.

Minor comments

Line 33 – Talking about the hybrid nature of polar lows, I would suggest citing Terpstra et al., 2014.

Line 79 – It is better to say "ranges from 30 minutes up to 12 hours".

Line 90 – The tropopause is usually located at 300 hPa.

Lines 90-91 – 1. As far as I know, polar lows are not presented a lot poleward of 80ËŽN, so the problem of longitude convergence might not be a large one in the considered area. 2. How did you do the coarsening of data in a longitudinal direction? Did you calculate the mean of each two longitudinal points? Please, clarify it in the text.

Line 117 – How did you associate the automatically detected by the vorticity field PLs with that detected in satellite data from the Rojo list? Please, clarify it in the text.

Line 135 – why don't you just extend the ERA-5 boundaries chosen for the analysis to cover the four excluded mesocyclones? Furthermore, how did you estimate the influence of this elimination?

Line 165 – from where the 60 rm radius filter came from? Does it have any physical meaning? Please, provide an argumentation for this choice in the text of the manuscript. Why not use the Savitzky-Golay filter only?

Line 170 – "of a time step" instead of "of an time step."

Lines 192-193 – There is a more common practice to distinguish between different stages of the lifecycle of any cyclonic phenomena – normalizing the number of timesteps and letting 0-0.2 to be the genesis stage, 0.2 – 0.8 to mature stage, and 0.8 to 1 to lysis (see Simmonds, 2000; Rudeva and Gulev, 2007 for example). How can you prove your choice of the mature stage definition?

Lines 196-197 – The statements done on these lines need to be proven by some citation or description on why some signal may be simply removed from the analysis like these transition states of the system.

Line 208 – remove "is evident." Line 218 – How was the medium-level cloud cover considered?

Line 256 – I would not call the pattern depicted in fig. 4 "a spectrum." Try to recall it.

Line 266 – "most likely intensify through the baroclinic instability."

Line 281 – I would rephrase this sentence as follows: "The strong shear is more common for time steps in the first half of the PL lifetime" since your version is a bit confusing: it seems like time steps are more common, while strong shear is more common.

Lines 282-283 – the same as for the previous comment: PL time steps are not "occurring" in the philosophical sense – it may be characterized by one or another shear condition.

Lines 281-287 – please rephrase the whole paragraph. Especially at lines 284-285, where you say that time steps are intensifying. I am sure that you meant that the PLs are intensifying at these time steps.

Lines 288-289 – Did you investigate the static stability parameter separately for that 30% of PLs within the weak-shear category, which are observed at the intensification stage? This may affect your main conclusion that the "convective" PLs are not presented in the STARS dataset.

Line 312 – This statement is not fair enough. One would not expect the WISHE mechanism to be responsible for the intensification of PLs at the decaying stage (70% of time steps in this category); Secondly, it is known that the WISHE mechanism is effective at the mature stage of baroclinic MCs. For the "convective" PLs to create conditions for WISHE effect activation, it needs that the strong cyclonic circulation occurs.

Line 317 – Note that CAPE is the trigger mechanism that plays a larger role in the first half of the PL lifecycle, while the CISK is the process that occurs more in the second half (Emanuel and Rotunno, 1989), and it is better not to mix them up.

Figure 6 caption – remove "within."

Lines 336-344 – Saying "cold-air outbreak around Svalbard" (line 338) for forward-shear synoptic-scale condition and further "cold-air outbreak to the west of Svalbard" (line 341) for reverse-shear PLs, you confuse the reader because these descriptions are partly overlapping. Looking at fig.7, one may notice that the direction of the cold-air outbreak and isotherms inclination is very different for two of these cases. Thus, I would pay more attention to the text on these differences than say that both types of shear forms, particularly under the same conditions. For example, reverse-shear conditions are linked to the cold-air outbreaks over the Norwegian Sea, having the north-east direction of the mean flow, while forward-shear conditions are more about the CAO in the Barents Sea, with the mean flow directed west-north-westward.

Line 345 – "occur south of Svalbard" instead of "occur to the south of Svalbard."

Line 344 In addition to lines 336-344 – Here, left-shear conditions are typical in cold-air outbreaks with northward mean flow. This is its main difference from what we see in fig.7 for forward-shear.

Lines 348-352 – The weak-shear category is still looking not enough investigated. Fig.7e shows very clearly two types of patterns of PLs occurrence under this category. One of those is naturally the lysis near the east coast of Greenland. However, the

second, which is less numerous, should represent that 30% of generated PLs correspond to the upper-level PV anomaly-induced development of the PLs (see R&T2003, chapter 4.4, or Bracegirdle and Gray, 2007).

Line 380 – need to add a citation for the CISK mechanism realization, such as Charney and Eliassen, 1964; Rasmussen 1979; Businger and Reed, 1984; or any other that you prefer.

Figure 9 – (b) instead of (b, c)

Line 399 – check the citation.

Line 405 – “which is actually” instead of “is actually.”

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