



EGUsphere, referee comment RC1
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Comment on egusphere-2022-351

Anonymous Referee #1

Referee comment on "The composite development and structure of intense synoptic-scale Arctic cyclones" by Alexander F. Vessey et al., EGU sphere,
<https://doi.org/10.5194/egusphere-2022-351-RC1>, 2022

Summary

The premise of this paper is obviously useful: Extreme synoptic-scale cyclones in the Arctic seem to develop differently than extreme synoptic-scale cyclones in the mid-latitudes, but that theory is based on only several case studies, not a comprehensive review of many storms. The implications the authors present in the paper (that prediction of, projection of, and impacts of these storms all depend on how they develop) make this worthwhile research. Honestly, I'm surprised it's taken so long for somebody to undertake a composite analysis like this. I am therefore very happy to see that the attempt here has a thorough and logical approach. The figures are clear and well-made. The methods used are ones that align well with the research questions. I have some comments, but everything can be addressed at the text level.

A. Content Comments

1. The authors focus on summer Arctic cyclones and how they are distinct from winter Arctic cyclones and winter North Atlantic cyclones. A logical question is: Why winter North Atlantic cyclones rather than summer North Atlantic cyclones? At first blush, the latter seems more logical to control for region.

I think I can infer the reason: The standard cyclone models we use are primarily based on the winter season, and the Norwegian Model is primarily developed from winter North Atlantic cyclones. Therefore, using the North Atlantic winter (rather than summer) is closest to giving a "standard model". However, I think it would be better to make this explicit in the text – probably around line 65-80 – but I'm not sold on that being the best place.

As an aside, if the authors delved more into “why” extreme Arctic summer cyclones are so different, I think it would be essential to include summer North Atlantic storms in order to control for both season and region. But the authors are writing a paper that outlines “what”, not “why”, so I think the format they chose is fine. Again, I think the logic just needs to be more explicit since it’s counter intuitive.

2. Lines 46-47, Figure 2, Lines 371-372: Simmonds and Rudeva (2014) is a good reference for a few things: The linkage with tropopause polar vortices for extreme cyclones, the location difference between winter/summer extreme storms, and the long lifetimes for extreme Arctic storms. Incorporating that paper for one/some of these reasons would be good.

3. Line 93-95: Vessey et al. (2020) showed that using relative vorticity (rather than mean SLP) leads to greater cyclone frequency using the Hodges algorithm, but they didn’t prove that it is true for other tracking algorithms in use. Therefore, it’s best to specify “with this algorithm”, or something similar.

4. Line 167 – 169: I don’t think using a common isobar is a good comparison of size. The 1000 hPa is not the edge of a cyclone, so it doesn’t have much meaning – especially when storms from different seasons/areas have different central pressures. It would be better to use a) the area with cyclonic relative vorticity or b) the area within the last-closed isobar (given some isobar interval). One of these might be able to be incorporated into the left-hand panels (although the current scale might be too large).

Alternatively, just with text changes, the convex 1008-hPa isobar in summer being entirely within the 2200 km by 2200 km box versus the (seemingly) convex 1016 hPa isobars in the winter cases extending beyond that window seems more compelling. The limit of the 10 m/s wind field would also be reasonable if focused on storm impacts.

5. Figure 3: Thank you for the very obvious direction arrows. That is helpful.

6. Lines 193-196: Clancy et al. (2022) also used different input data (ERA-Interim for one algorithm, NCP-NCAR reanalysis for another – the ERA5 data they use appears to be for non-cyclone tracking purposes), so the authors can add that to the list of differences

7. Lines 195-196: Using the top quartile v. the top 100 is difficult to compare with different periods and overall frequencies from the different algorithms. Reporting what percentage of storms fall into the “top 100” would be an improvement on the imprecise statement “many more”.

8. Figure 9: The “Summer Arctic Cyclones” left of the y-axis label is superfluous and likely better off being replaced with the distinguishing characteristic: north-south v. east-west.

9. Line 316-320: I disagree with the authors on the statement here that Aizawa and Tanaka (2016) did not consider baroclinic to barotropic transition. In their abstract, they state:

“The cyclone of 2012 is characterized by the structure change from the cold core to the warm core at the lower stratosphere, indicating a shift from the ordinary baroclinic cyclone to the typical Arctic cyclone.”

Later, they state:

“In the early stage [of the 2012 case] (Fig. 5), the vortex shows clearly the baroclinic structure.”

Therefore, they do discuss this transition for the 2012 case. Still, I think it’s fair to say that they do not make a transition from baroclinic to barotropic structures part of their overall conceptual model. In other words, I think a few tweaks to text are in order – in particular, pointing out that the 2012 case exemplifies this finding about transitions

10. Summary and Conclusions: I think bringing up the Clancy paper one more time here is important. Perhaps their different results indicate that the extreme storms differ from the average storms in the Arctic. But as the authors pointed out earlier, differences between the studies might also explain some of the discrepancy in results. There might be some “future work” statements around this, but at the very least a comment about the implications is worthwhile.

B. Proofreading Comments

- Throughout text and figure captions: The authors are using propagation-relative grids but they also often use north, south, east, or west to describe positions relative to cyclone centers. It’s not always clear whether these are earth-relative or map-relative (e.g., “north” = top of map). For clarity, the authors could a) always specify earth-relative or map-relative (i.e., propagation-relative) or b) describe the position as left,

right, ahead, or behind the cyclone (as is sometimes done already). Note: I say this all recognizing that because cyclones generally move west to east, map-north and earth-north are fairly close to each other. So it's a small thing.

- Line 15: Replace "National Snow and Ice Data Centre" with "National Snow and Ice Data Center". (The USA apologizes for the inconvenience of its disparate spelling.)
- Line 19-20: Since the Stroeve et al. (2007) is primarily about model validation, not model projections, I suggest using either Notz & SIMIP Community (2020) or Årthun et al. (2021) instead to make this point.
- Line 105: I believe "system centered" should be hyphenated as "system-centered".
- Line 107, 124: An apostrophe is needed to make a possessive, changing "cyclones" to "cyclone's".
- Line 124: The phrase "common direction" is unclear because previously, the terms "geographical orientation" and "oriented according to the cyclone's propagation direction" were used – but not "common direction". By default, then, I'd assume that "common" means "absolute", or, in this case "geographical. However, the authors then specify "propagation direction" in parentheses, showing my default assumption is incorrect. It might be clearer, then, to just say outright "An advantage of rotating each cyclone relative to propagation direction..."
- Line 215: Change "typically" to "typical".
- Line 253: A closing ")" is missing.
- Line 279: Replace "at the after" with "at and after".
- Line 305: Based on the figure concept, vertical velocity is inverted to be positive upwards, so this ought to say "negative vertical velocity".
- Line 306: Change "Only a weak" to "Only weak".

Full citations for references in the review that are not in from submitted manuscript

Årthun, M., I. H. Onarheim, J. Dörr, and T. Eldevik, 2021: The seasonal and regional transition to an ice-free Arctic. *Geophys Res Lett*, **48**, <https://doi.org/10.1029/2020gl090825>.

Notz, D., and S. Community, 2020: Arctic Sea Ice in CMIP6. *Geophysical Research Letters*, **47**, <https://doi.org/10.1029/2019gl086749>.

Simmonds, I., and I. Rudeva, 2014: A comparison of tracking methods for extreme cyclones in the Arctic basin. *Tellus A*, **66**, 25252, <https://doi.org/10.3402/tellusa.v66.25252>.