



EGUsphere, referee comment RC2  
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## **Comment on egusphere-2022-351**

Anonymous Referee #2

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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-RC2>, 2022

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In recent times the Arctic region, and its tendencies, have attracted much interest in a wide range of respects. This submission uses a composite approach to explore the nature of extreme Arctic-basin storms, how they differ between summer and winter, and how their revealed behavior differs from their midlatitude counterparts. Also revealed is the complex roles played by low-level baroclinicity and upperlevel processes.

The work builds on earlier research by these and other authors. It presents some new insights, but does require significant revision.

Line 15: National Snow and Ice Data Center

Lines 15-17: Also to reference here recent Arctic sea ice analysis of Li et al (2021) Trends and variability in polar sea ice, global atmospheric circulations and baroclinicity. Ann. NY

Acad. Sci. 1504: 167-186 doi: 10.1111/nyas.14673.

Lines 51-54: Topical and relevant papers of Takuji Waseda, Takehiko Nose, Tsubasa Kodaira, Kaushik Sasmal and Adrean Webb, 2021: Climatic trends of extreme wave events caused by Arctic cyclones in the western Arctic Ocean. *Polar Science*, **27**, 100625, doi: 10.1016/j.polar.2020.100625, and Vernon A. Squire, 2020: Ocean wave interactions with sea ice: A reappraisal. *Annual Review of Fluid Mechanics*, 52, 37-60, doi: 10.1146/annurev-fluid-010719-060301 valuable additions here.

Lines 111-112: The algorithm used here identifies cyclonic systems based on 850 hPa relative vorticity, rather than mean sea level pressure which is often used in other studies. The former parameter does have a number of advantages. However, I was puzzled as to why the central value of MSP rather than the (filtered) vorticity was used to identify the 100 most intense cases. Some words should be devoted to the rationale behind this change of focus. (A few lines below (at line 131) the authors state that when cyclones' MSLP reach their minima is 'when they are at their most hazardous'. One would have thought this would occur for the 850 hPa rel. vort. reaching its extreme.

Lines 120-121: Please to be explicit as to the determine of the propagation direction. I am assuming that the propagation 'vector' is taken to the displacement of the MSLP minimum. Similarly, I would guess that the direction here is determined by an (unbiased) centred difference of the central location at + 6hr minus -6hr. Important to spell out.

Similar issue for composites prior to, and after, the most intense time (lines 132-133, ...). Are these directions determined from the 12 hr location differences centred on the relevant time, or is same propagation value used for all of these?

Lines 124-125: A good point is made here in connection with retaining the integrity/identity of conveyor belts when performing the required rotations. These features are known to be intimately involved with various extremes associated with cyclones and fronts. Emphasise this very important message by also referencing the article of Jennifer L. Catto, Erica Madonna, et al., 2015: Global relationship between fronts and warm conveyor belts and the impact on extreme precipitation. *Journal of Climate*, **28**, 8411-8429, doi: 10.1175/JCLI-D-15-0171.1.

Lines 179-180: Perhaps reword this sentence. 'In this region, the system-relative winds would be enhanced by the cyclonic (anti-clockwise) propagation velocity of the cyclone' could be read as saying the system-relative (as distinct from 'earth-relative') winds would be STRONGER, whereas I am sure the authors want to say the exact opposite.

Lines 192-196: While they had a larger testing set (lowest quartile of all Arctic cyclones), these comments perhaps do not fully convey the procedure and results obtained by Robin Clancy et al. As distinct from the measure used here they made use of two measures of intensity, namely (i) the local Laplacian of SLP and (ii) the maximum SLP within a closed contour around a cyclone minus the minimum SLP within this contour. The former is closely related to the geostrophic relative vorticity, while the latter is very similar to the cyclone 'depth' (make reference in paper to Murray et al. (1995), Responses of climate and cyclones to reductions in Arctic winter sea ice, *J. Geophys. Res.*, *100*, 4791-4806, doi: 10.1029/94JC02206). (I had made some comments earlier on whether the definition of 'intensity' in the present paper is really appropriate.) As such, the Clancy method may be seen as more appropriate to identifying extreme systems and could, notwithstanding the different sample sizes, be seen as one of the reasons for the differing conclusions reached here.

The authors need to provide a more complete analysis and discussion on the apparent discrepancies on this central point.

Lines 207-209: It would be helpful and insightful to express these omega 'speeds' in terms of approximate m/s (or cm/s) making use of hydrostatic equation etc.

Line 256-260: It is not clear to me how the summer composites at 96, 144 and 192 hrs (i.e., 4, 6, and 8 days) in Figure 7 were constructed. From Figure 1b 22 (6+16) of the 100 intense ACs last 6 days or less. So, e.g., for the +8 day composites (Figure 7c) were only the systems that survive to 8 days considered, or was the atmospheric structure somehow incorporated into the composites. Either way, there will be some bias in those later lags. The procedure followed should be made clear here.

Also, at line 256 'From' is potential ambiguous. Would suggest replace with 'After'.

Lines 290-320: This section should be more carefully written to reflect the different development roles of low-level baroclinity and upper level support. A key aspect of intense Arctic cyclones is known to be the presence of Tropopause Polar Vortices (TPVs). These points were made very strongly by Tanaka, Yamagami and Takahashi, (2012) and Aizawa and Tanaka (2016). Make reference also here to study of Rudeva et al., 2014 ('A comparison of tracking methods for extreme cyclones in the Arctic basin. *Tellus*, 66A, 25252, doi: 10.3402/tellusa.v66.25252) who showed that, in all months, the vast majority of extreme Arctic cyclones were associated with a TPV. TPVs as such are mentioned in the paper, but only in the last few lines. This important structure and concept must be introduced much earlier, and more clearly integrated in to the analysis.

Lines 301-302: I'm not sure where the '4,400 km' figure for the surface system came from – it certainly was not mentioned in the 2016 paper of Takuro Aizawa. They suggest, for their 2008 case, a diameter of 3000 km (their Fig. 2 and Page 193), based on the radial and tangential 10 m wind. Their 2012 storm had a similar diameter (their Figure 3a). (Perhaps the present authors are confusing that Aizawa and Tanaka refer to diameter of up to 5000 km at the UPPER LEVEL (an enlargement that would be expected from the dynamics). Please write this, and the conclusions from it, more carefully.

Lines 309-310: No need to include the statement '... which was based on the analysis of just two past Arctic cyclone case studies'. This has already been made clear a few lines above (lines 290-291).

Lines 322-372, Section 4: The focus of the paper has been on the composites of the 100 winter and summer cases. The authors have not presented the dates of these individual events; this is quite appropriate in that that level of detail might needlessly distract the reader's attention from the main theme. Having said that, it would be of interest to know whether there have been any trends in the frequency of these events. Maybe a timeseries (by year) would be of interest. Or more simply, the technique I saw used by Rudeva I. and co-authors (2014) A comparison of tracking methods for extreme cyclones in the Arctic basin. *Tellus* 66A, 25252, could be used to establish statistical significance or otherwise of any shift in the 'centroid' of yearly occurrence of extremes.

Lines 416-417: Please note this paper has passed the 'the 'Discussion' phase, and has now been published. Details are ...

Gray, S.L., Hodges, K.I., Vautrey, J.L. and Methven, J., 2021: The role of tropopause polar vortices in the intensification of summer Arctic cyclones. *Weather and Climate Dynamics*, 2, 1303–1324, doi: 10.5194/wcd-2-1303-2021.

Line 470-471: Please append correct details to reference:

Waseda, T., Webb, A., Sato, K., Inoue, J., Kohout, A., Penrose, B. and Penrose, S. 2018. 'Correlated increase of high ocean waves and winds in the ice-free waters of the Arctic Ocean', *Sci. Rep.* 8, 4489, doi: 10.1038/s41598-018-22500-9.