Response to Reviewers

Manuscript titled: The composite development and structure of intense synoptic-scale Arctic cyclones

By: Alexander F. Vessey, Kevin I. Hodges, Len C. Shaffrey and Jonathan J. Day

Format of Response

The comments from each Reviewer are copied (in **GREY TEXT**) and are addressed in turn. Reponses are written in **RED TEXT**. Some comments are similar between the three anonymous reviewers. Where text has been edited in our manuscript, this added text may reflect the comments made by that reviewer, or the comments of a combination of reviewers.

Response to Anonymous Reviewer #1

Note to Anonymous Reviewer #1

The authors of this manuscript thank Reviewer #1 for taking time to read and review this manuscript. Please see the point-by-point responses below.

Response

A. 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.

We thank the reviewer for his/her comments on our research and for this positive review.

B. **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.

Yes, the reason for choosing to compare Arctic storms to winter (DJF) North Atlantic cyclones is, as the reviewer has said, because the conceptual models of extra-tropical cyclones (e.g., Norwegian Cyclone Model and Shapiro Keyser Model) are primarily based on the analysis of winter North Atlantic cyclones. This is somewhat stated on Lines 67-69, but additional text has been added here to make the reason more explicit (see added text in **BOLD**):

Lines [67 – 69]
"Winter extra-tropical cyclones occurring over the North-Atlantic Ocean are used as a reference to compare with intense winter and summer Arctic cyclones, as they have been investigated more extensively in previous research (e.g., Bjerknes, 1919, 1922; Shapiro and Keyser, 1990; Browning, 1997, 2004; Catto et al., 2010). **Conceptual models of extra-tropical cyclone structure and development are also primarily based on the analysis of winter extra-tropical cyclones. Therefore, comparing Arctic cyclones with winter North Atlantic Ocean cyclones allows for a more direct comparison to these conceptual models."**

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.

Thank you for raising this. Yes, Simmonds and Rudeva (2014) is a good reference in these places.

The reference for Simmonds and Rudeva (2014) has been added to Lines 46-47, where previous studies that have shown that the development of intense Arctic cyclones may be linked to interactions with tropopause polar vortices are described in the manuscript. The text here now reads (added text is shown in **BOLD**):

Lines [46 – 47]
"**Tanaka et al. (2012) and Tao et al. (2017) showed that in their Arctic cyclone case studies, a lower stratospheric positive potential vorticity anomaly (i.e., Tropopause Polar Vortices - TPVs) played a decisive role in each cyclone's development. Simmonds and Rudeva (2014) also showed that TPVs influenced the development of 54 out of 60 50 intense Arctic cyclones, with the sample size of 60 being comprised of the five most intense Arctic cyclones per calendar month from 1979-2009."**

In Figure 5 from Simmonds and Rudeva (2014), the spatial distribution of intense Arctic cyclones is also shown. Like Figure 2 from this manuscript, it is also shown that intense Arctic cyclones in winter typically occur over the Greenland, Norwegian and Barents Seas, but in summer, intense Arctic cyclones typically occur over the high Arctic Ocean. Simmonds and Rudeva (2014) is also a good reference when describing Figures 1 and 2, as they also show that Arctic cyclones are typically more intense and shorter-lived in winter than in summer.

This reference has been added to lines 140-141 when describing Figures 1 and 2, and to line 160 when describing Figure 2 (added text is shown in **BOLD)**:

Lines [140-141]
"The mean lifetime of the 100 most intense Arctic summer cyclones is however much greater than that of winter Arctic cyclones and North Atlantic Ocean extra-tropical cyclones (see Figure 1). **This is in agreement with Simmonds and Rudeva (2014), who show that the most intense Arctic cyclones are typically more intense in winter than in summer but have a longer lifetime in summer than in winter.**"

Lines [158-160]
"In contrast, the 100 most intense Arctic summer cyclones tend to have their maximum intensity over higher latitudes and over the Arctic Ocean that is north of the Eurasia coastline and the Bering Strait (see Figure 2). **This was similarly shown in Simmonds and Rudeva (2014).**"

Simmonds and Rudeva (2014) also show that the development of intense Arctic cyclones is linked to interactions with tropopause polar vortices. This reference has also been added in text to lines 371-372, where in this manuscript, it is also suggested that the development of intense summer Arctic cyclones may be linked to interactions with tropopause polar vortices. Added text is shown in **BOLD:**

Lines [371-372]
"Recent studies have suggested that some Arctic cyclones may have been strongly influenced by TPVs (e.g., Simmonds and Rudeva, 2014; Tao et al., 2017; Gray et al., 2021)"

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.

Yes, this is a good point. The text has been changed here to specify that the results from Vessey et al. (2020) are true when using the Hodges algorithm. Line 93-95 has been changed and now reads (added text is shown in **BOLD**):

In addition to using relative vorticity or mean SLP, cyclone frequency can also depend on the type of storm track filtering e.g., minimum lifetime of the storm tracks (1 day or 2 day etc.) and minimum migration distance that the storm travels over its lifetime e.g. storm must travel more than 1000 km (see Table 4 in Jung et al. 2012 – full reference below). We believe that it is worth adding this additional factor to cyclone frequency. Therefore, the above has been changed to:

> **Lines [93-94]**
> "Vessey et al. (2020) showed that **when using this storm tracking algorithm and identical filtering criteria**, tracking storms based on 850 hPa relative vorticity generally identifies more Arctic cyclones than those based on MSLP."

Jung, T., Miller, M. J., Palmer, T. N., Powers, P., Wedi, N., Achuthavarier, D., Adams, J. M., Altshuler, E. L., Cash, B. A., Kinter III, J. L., Marx, L., Stan, C. and Hodges, K. I. (2012) High-resolution global climate simulations with the ECMWF model in Project Athena: experimental design, model climate, and seasonal forecast skill. Journal of Climate, 25 (9). pp. 3155-3172.

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.

Yes, stating that last closed isobar of the summer cyclone composite fits within the 2200 x 2200 km domain, whereas, for the winter Arctic and North Atlantic Ocean cyclone composite, the last closed isobar does not, would be a better way to convey the size comparison of the cyclones. We also agree that stating the limit of the 10 m/s 850 hPa wind field contour also helps to show the difference in size between the cyclone composites.

The text here has been changed (to reference the last closed isobar) and now reads (added text is shown in **BOLD**):

Lines [166-170]

"~~The Arctic summer cyclone composite is also smaller than the Arctic winter and North Atlantic Ocean cyclone composites. This is shown by the 1000 hPa contour not exceeding a distance of 1100 km from the Arctic summer cyclone composite centre, but exceeding this threshold in the winter Arctic and North Atlantic Ocean extra-tropical cyclone composites (see Figure 3a, 3d, and 3g).~~ **The Arctic summer cyclone composite also appears to be smaller in size than the Arctic winter and North Atlantic Ocean cyclone composites. This is inferred by the last closed isobar not exceeding the 2200 x 2200 km domain in the Arctic summer cyclone composite (see Figure 3g). In contrast, the last closed isobar exceeds of the winter Arctic and North Atlantic Ocean cyclone composite the 2200 x 2200 km domain (see Figure 3a and 3d).**"

The text here has been changed (to reference the area within the 10 m/s 850 hPa wind field contour) and now reads (added text is shown in **BOLD**):

Line [180]

"**The area where the 850 hPa earth-relative wind speeds exceed 10 m s^{-1} is also much greater in the Arctic winter and North Atlantic Ocean extra-tropical cyclone composites than the Arctic summer cyclone composite (see Figure 3b, 3e and 3h).**"

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

Yes, Clancy et al. 2022 used two Arctic cyclone datasets based on the ERA-Interim reanalysis and the NCEP-NCAR reanalysis. In this manuscript obtain an Arctic cyclone dataset using the ERA-5 reanalysis. This has been added to the list of differences between the studies.

The text here has been changed and now reads (added text is shown in **BOLD**):

Lines [192-196]

"This contrasts with results from Clancy et al. (2022), who show that the structure of Arctic cyclones in every season is comparable to those in the mid-latitudes. However, Clancy et al. (2022) used a different cyclone compositing methodology, storm tracking algorithm **that is applied to different reanalysis datasets (ERA-Interim and NCEP/NCAR reanalyses rather than ERA-5) and metric of intensity to obtain the most intense cyclones**."

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".

We agree that the term "many more" is imprecise. Although it is difficult to quantify the exact number of cyclones per season that contribute to the Arctic cyclone composites in Clancy et al. (2022), as it is not stated in that paper.

In Figure 1 from Clancy et al. (2022), Arctic cyclone counts per season and year are shown in a time-series, after selecting the upper quartile of Arctic cyclones from all Arctic cyclone tracks by intensity. For summer (JJA), between ~250 and ~380 Arctic cyclones each year between 1985-2016 are included within the "top quartile" sample. Assuming 300 summer Arctic cyclones per year would indicate that 9,600 cyclones (300 storms per year x 32 years) contribute to the composites from Clancy et al. (2022). This is much greater than the 100 cyclones used in this manuscript.

Therefore, the term "many more" has been changed to "**thousands more**". Using such a great number of storms to contribute to their composites may also likely smooth out the unique axi-symmetric structure of Arctic summer cyclones that is identified in this manuscript. This may be a further reason for the difference in the composites between our study and Clancy et al. (2022). The text here has been changed and now reads (added text is shown in **BOLD**):

> **Lines [192-196]**
> "This contrasts with results from Clancy et al. (2022), who show that the structure of Arctic cyclones in every season is comparable to those in the mid-latitudes. However, Clancy et al. (2022) used a different cyclone compositing methodology, storm tracking algorithm that is applied to different reanalysis datasets (ERA-Interim and NCEP/NCAR reanalyses rather than ERA-5) and metric of intensity to obtain the most intense cyclones. Clancy et al. (2022) also did not filter Arctic cyclones to retain the 100 most intense cyclones per season, instead calculating the cyclone composite as an average of **thousands more** cyclones per season**, which may smooth out this unique axi-symmetric structure of Arctic summer cyclone composites (see Figure 3)**."

(Differences between this study and Clancy et al. (2022) is further expanded upon in Section 4: Summary and Conclusions, in response to the 10th comment from Review #1).

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.

Yes, we agree. The y-axis label "Summer Arctic Cyclones" is unnecessary, as this detail is mentioned in the figure caption. It has been removed.

We have kept the "distinguishing characteristic: north-south v. east-west" along the x-axis. But following the first comment from Reviewer #1 in <u>Section B. Proofreading Comments</u>, these have been replaced by the propagation-relative terminology (e.g., ahead-behind, right-left).

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

The authors agree with the reviewer here. The wording of this section has been changed to reflect the above points.

This paragraph, which was between Lines 316-320 in the original manuscript, has been edited and now reads (added text is shown in **BOLD**):

Lines [316-320]
"~~An aspect that is not considered by Aizawa and Tanaka (2016), but is highlighted in this study, is that intense Arctic summer cyclones appear to typically undergo a structural transition during their life-cycle. The development in the composite structure of intense Arctic summer cyclones shows that they undergo a transition from having a baroclinic structure to an axi-symmetric cold core structure throughout the troposphere around the time of maximum intensity. After this transition, the structure of intense Arctic summer cyclones is somewhat similar to the Aizawa and Tanaka (2016) model.~~ **An aspect that is highlighted in this study, is that intense Arctic summer cyclones appear to typically undergo a structural transition during their lifecycle. The development in the composite structure of intense Arctic summer cyclones shows that they undergo a transition from having a baroclinic structure to an axi-symmetric cold core structure throughout the troposphere around the time of maximum intensity. After this transition, the structure of intense Arctic summer cyclones is somewhat similar to the Aizawa and Tanaka (2016) model. Aizawa and Tanaka (2016) showed this structural transition when analysing 'The Great Arctic Cyclone of 2012' and for another Arctic summer cyclone in June 2008. The composite Arctic summer cyclone shown in this study suggests that this transition has likely occurred in other past intense Arctic summer cyclones and that it may be typical of intense Arctic summer cyclones."**

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.

Yes, referring to the results of Clancy et al. (2022) is worthwhile here, to indicate that the composite structure of Arctic cyclones can differ between studies, and that further work could help to verify the results from this study and Clancy et al. (2022). This has been addressed in additional text in the Summary and Conclusion section.

The penultimate paragraph in this section [Lines 358-364] has been edited and now reads (new text in **BOLD**):

> "The composite structural development of Arctic summer cyclones is found to be different to that of winter North Atlantic Ocean extra-tropical cyclones and winter Arctic cyclones. This is also different to the structural development of extra-tropical cyclones described in the Norwegian Cyclone Model (Bjerknes, 1919, 1922) and Shapiro-Keyser Model (Shapiro and Keyser, 1990). The Arctic summer cyclone composite also shows differences to the Arctic Cyclone Model proposed by Aizawa and Tanaka (2016). This raises questions such as whether this unique structural transition of Arctic summer cyclones is captured in climate model projections and weather forecasting models, and how might the frequency of these Arctic summer cyclones change in response to climate change?
>
> **The results found in this study contrast with those in Clancy et al. (2022), who show that the structure of Arctic cyclones in all seasons is similar to mid-latitude cyclones. However, there are numerous differences in the methods between this study and Clancy et al. (2022). In this study, cyclone composites were calculated across the 100 most intense cyclones per season, but in Clancy et al. (2022), thousands more cyclones were included when producing cyclone composites. It is possible that this unique structural development of Arctic summer cyclones is only present in the most intense Arctic summer cyclones, and why it was shown in this study and not in Clancy et al. (2022). In addition, Clancy et al. (2022) use a different cyclone compositing method, storm tracking algorithm applied to different reanalysis datasets, and a different intensity metric to determine the most intense cyclones that contribute to their composites. These differences in method may also contribute to the differences with the results presented in this study. Future work may seek to test the sensitivity of sample size and composite method on determining the structure of intense Arctic cyclones."**

C. Proofreading Comments

1. 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.

Yes, we see how this could be confusing for the reader. We think it is best to go with option b) that the reviewer suggests here.

We have changed the following according to this advice:

- Figure caption for Figure 4
- North/South labels on Figure 5 – changed to Left/Right
- North/South labels on Figure 8 – changed to Left/Right
- Figure caption for Figure 8
- North/South labels on Figure 9 – changed to Left/Right
- East/West labels on Figure 9 – changed to Ahead/Behind
- Figure caption for Figure 9

2. 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 15: "National Snow and Ice Data Centre" has been replaced with "National Snow and Ice Data Center"

3. 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 19-20: Stroeve et al. (2007) has been replaced with both Notz & SIMIP Community (2020) or Årthun et al. (2021)

4. Line 105: I believe "system centered" should be hyphenated as "system-centered".

Line 105: Yes, I agree - "system centered" has been replaced by "system-centered"

5. Line 107, 124: An apostrophe is needed to make a possessive, changing "cyclones" to "cyclone's".

Line 107, 124: "cyclones" has been replaced by "cyclone's"

6. 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 paratheses, 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 124: Yes, I understand the confusion here. "Propagation direction" is meant here, so the phrase "common direction" has been deleted and it is said outright "An advantage of rotating each cyclone relative to propagation direction…"

7. Line 215: Change "typically" to "typical".

Line 215: "typically" has been replaced by "typical"

8. Line 253: A closing ")" is missing.

Line 253: A closing ")" has been added.

9. Line 279: Replace "at the after" with "at and after".

Line 279: "at the after" has been replaced by "at and after"

10. Line 305: Based on the figure concept, vertical velocity is inverted to be positive upwards, so this ought to say "negative vertical velocity".

Line 279: Yes - "positive vertical velocity" has been replaced by "negative vertical velocity"

11. Line 306: Change "Only a weak" to "Only weak".

Line 306: "Only a weak" has been replaced by "Only weak"

Response to Anonymous Reviewer #2

Note to Anonymous Reviewer #2

The authors of this manuscript thank the Reviewer #2 for their time for reading and reviewing this manuscript.

Response

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 upper level processes.

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

We thank the reviewer for his/her comments on our research and highlighting its novelty.

1. Line 15: National Snow and Ice Data Center

Thank you for pointing this out, "Centre" has been changed to "Center".

2. 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.

Yes, thank you for pointing this out. Li et al (2021) emphasises the decreasing trends in Arctic Sea ice extent from 1979-2020. It is a useful reference to add here. It has been added in the text here.

3. 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.

Yes, we agree that these references are valuable additions here, as they highlight the hazardous conditions that Arctic cyclones can cause. They have been added here.

4. 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.

Yes, we're happy to expand upon the choice of cyclone tracking parameter.

When conducting our analysis, we found that the point of maximum filtered 850 hPa relative vorticity generally coincides with the point of minimum mean sea level pressure. This is shown in the Figure 1 below.

Figure 1 shows the difference in timestep (i.e., 6-hours) between the point of maximum filtered 850 hPa relative vorticity of each cyclone and the point of minimum mean sea level pressure. For each class of cyclone analysed in this study, these points match (or very nearly match i.e., within 1 timestep) and the modal difference is 0.

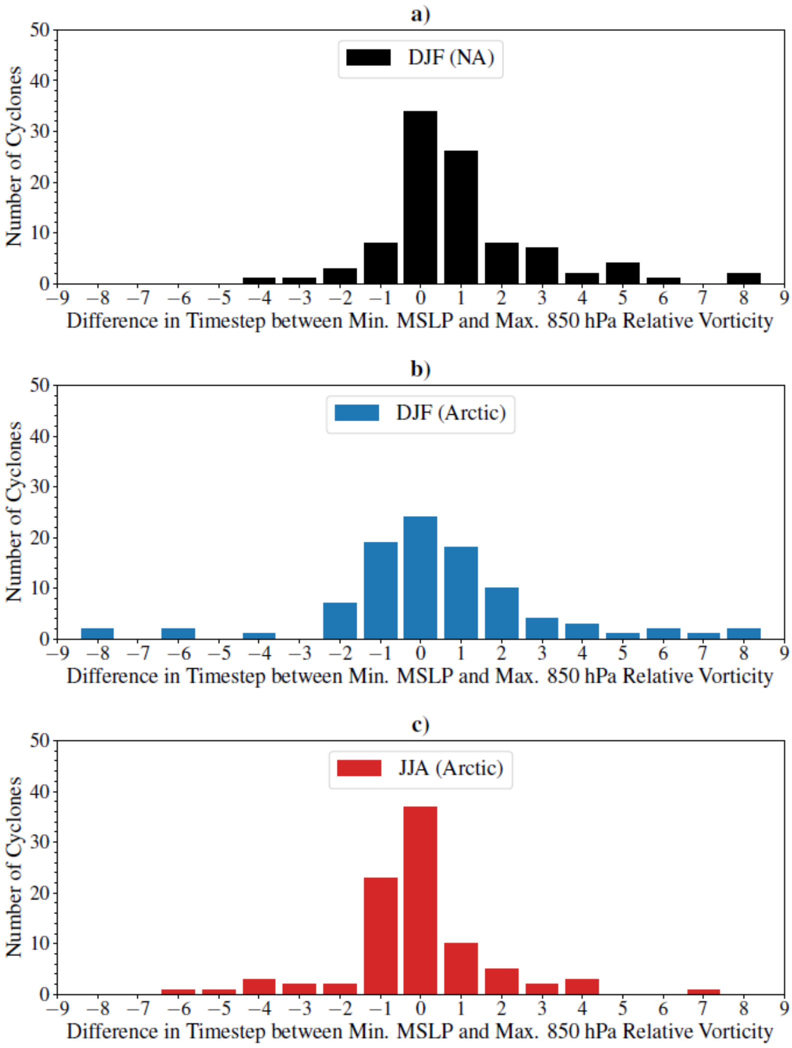

Figure 1. *The difference in timestep (i.e., 6-hours) between each cyclone's point of maximum 850 hPa relative vorticity and point of minimum mean sea level pressure, which contribute to the a) winter North Atlantic Ocean cyclone composite, b) winter Arctic cyclone composite, and c) summer Arctic cyclone composite.*

As part of the original study, composites of the 100 most intense cyclones per class were produced when filtering storms according to both maximum 850 hPa relative vorticity and minimum mean sea level pressure. The sensitivity of the results depending on this choice was minimal, as Arctic summer cyclones were shown by both methods to have a different axi-symmetric composite structure and development to winter Arctic and North Atlantic Ocean storms. This is likely because the lifetime positioning of each cyclone when computing the composites is generally the same between using the point of maximum 850 hPa relative vorticity or the point of minimum mean sea level pressure, as they general occur at the same timestep (see Figure 1).

Given that this choice does not have a material impact on the overall results of this analysis, the choice to use the point of minimum mean sea level pressure was made because in our experience, cyclones are typically associate by a wider audience as being most intense when their mean sea level pressure is lowest. Furthermore, the full resolution mean sea level pressure is preferred as it is unfiltered by the storm tacking algorithm and could be replicated in other studies that do not use the same storm tracking algorithm.

Having said this, we are happy to expand upon this within the text. The figure presented above provides rationale for this choice, and has been added to Supplementary Material, and is reference to in the text. Therefore, additional text has been added here (Lines 111-112), to make the reason for this choice more explicit (see added text in **BOLD**):

> **[Lines 111-116]**
> **"**Firstly, the 100 most intense winter North Atlantic Ocean extra-tropical cyclones and Arctic winter and summer cyclones with the lowest **full resolution** MSLP minima between 1979 and 2020 in ERA5 are identified (storm selection). **Figure S1 in the supplementary material shows the difference in timestep between the point of maximum T42 filtered 850 hPa relative vorticity and point of full resolution minimum MSLP of each cyclone that contributes to each cyclone composite. For each class of cyclone, the point of maximum intensity per atmospheric variable in each cyclone's lifecycle generally coincide, and the difference in timestep is shown to be mostly -1, 0, or 1. Therefore the choice here to filter to the 100 most intense cyclones by minimum full resolution MSLP rather than maximum T42 filtered 850 hPa relative vorticity does not significantly change the composites computed, as the lifetime positioning timestep of each cyclone generally coincide.**
>
> To be characterised as an Arctic cyclone, cyclones must have 60% of their track points in the Arctic (north of 65°N) and their maximum intensity (i.e., minimum MSLP) in the Arctic. This is to ensure that intense Arctic cyclones that spend most of their lifetime in the Arctic are selected. A similar procedure was followed to identify the 100 most intense cyclones

that occurred over the North Atlantic Ocean region (between -53 to 20°E and 30 - 65°N) from 1979 - 2020.**”**

5. 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.

Yes, we are happy to expand upon this in the manuscript. This has been made more explicit by adding the following text here (Lines 120-121) (see added text in **BOLD**):

[Lines 120-121]
“This grid can then be rotated so that each cyclone is orientated according to the it's propagation direction, or the geographical orientation of each cyclone is kept before an average is made over all cyclones (storm rotation). **The cyclone's propagation direction is determined using a pair of consecutive track points for the points before, at and after each track point, and then these vectors are averaged to smooth the direction (Catto et al., 2010).”**

6. 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?

Yes, we agree this should be more explicit. Therefore, we have added the following text here (Lines 132-133) (see added text in **BOLD**):

[Lines 132-133]
“The development of each composite is also determined at timesteps up to 48 hours before and up to 192 hours after the time of maximum intensity at 48-hour intervals. **Only cyclones that survive up 192 hours after the time of maximum intensity contribute to the cyclone composite at these lifetime positions. Each cyclone is orientated according to its propagation direction at that lifetime position point before averaging and computing the composite over all cyclones**. Temperature anomalies are calculated by subtracting the temperature value at each spatial point from the mean temperature across the horizontal 20° longitude-latitude domain at each vertical level.”

7. 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.

Yes, this is a good additional point to make here for why we should compute the cyclone composites according to the cyclone's propagation direction. This reason and reference has been added to this section (Line 125) with some additional text (see added text in **BOLD**):

> **[Lines 125-128]**
> "This can help to determine features such as conveyor belts within the cyclone composite (Catto et al., 2010). **The occurrence of these conveyor belts has also been shown to coincide with the occurrence of extreme weather e.g., high wind speeds (Browning, 2004; Martínez-Alvarado et al., 2014) and high precipitation (Catto et al., 2015). Thus, a further advantage of rotating each cyclone relative to its propagation direction before averaging, is that it may uncover details of the occurrence of these conveyor belts and extreme weather within each class of cyclone.** In this study, cyclone composites are produced by rotating each cyclone to a common direction of propagation before averaging, so that it can be determined whether conveyor belts, which have been shown to occur in typical extra-tropical cyclones (e.g., Browning, 1997), also occur in Arctic cyclones.

8. 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.

Yes, we see that there could be confusion here. We are trying to point to the reason for why the maximum 850 hPa earth-relative winds occurs in the southern region of the composite and near the composite centre (as shown in Figure 3b, 3e and 3h). Earth-relative winds are the combination of the cyclones system-relative winds and the cyclones propagation velocity. In the southern region of the composite cyclone, this is where the system-relative winds direction and the cyclones propagation velocity are the same, and therefore the 850 hPa earth-relative winds are highest in this southern region.

This sentence has been reworded and now reads (see added text in **BOLD**):

> **[Lines 178-180]**
> **"**In each cyclone composite, the maximum 850 hPa earth-relative wind speeds occur in the southern region of the composite and near the composite (see Figure 3b, 3e and 3h). ~~In this region, the system-relative winds would be enhanced by the cyclonic (anti-clockwise) propagation velocity of the cyclone.~~ **Earth-relative winds are the combination of the cyclones system-relative winds and the cyclones propagation velocity. The cyclone composite 850 hPa earth-relative winds are highest in this region, because the cyclonic (anti-clockwise) system-relative winds and the cyclones propagation direction are in the same direction. Therefore the 850 hPa system-relative winds are enhanced by the propagation velocity, thus resulting in the highest earth-relative 850 hPa in the southern region of the cyclone composites.**

9. 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.

Clancy et al. 2022 use a different measure (the local Laplacian of SLP and (ii) the maximum SLP within a closed contour) to filter their cyclones tracks to obtain the most intense cyclones. We agree with the reviewer that this should be highlighted in our manuscript. This additional difference between our study and Clancy et al. (2022) has been added to Lines 192-196. The text here has been changed and now reads (added text is shown in **BOLD**):

[Lines 192-196]
"This contrasts with results from Clancy et al. (2022), who show that the structure of Arctic cyclones in every season is comparable to those in the mid-latitudes. **However, Clancy et al. (2022), used a different cyclone compositing methodology, storm tracking algorithm that is applied to different reanalysis datasets (ERA-Interim and NCEP/NCAR reanalyses rather than ERA-5) and metric of intensity to obtain the most intense cyclones. Clancy et al. (2022) also did not filter Arctic cyclones to retain the 100 most intense cyclones per season, instead calculating the cyclone composite as an average of thousands more cyclones per season, which may smooth out this unique axi-symmetric structure of Arctic summer cyclone composites (see Figure 3)."**

In response to Comment #4, we now show that for each class of cyclone, the points of maximum relative vorticity and points of minimum mean sea level pressure generally coincide at the same timestep (see Figure 1 above). Therefore, using relative vorticity as the measure of intensity would not significantly alter the results presented in our manuscript.

We also believe the composites may be sensitive to the sample size of cyclones included to compute the composite, the reanalysis data used for the atmospheric variables around the cyclone, the storm tracking algorithm used to identify cyclones, and the storm compositing method used to compute the cyclone composites.

We agree that there should be more discussion on why the results from this study and that from Clancy et al. (2022) may differ. This was also highlighted by Reviewer #1 (see their Comment #10). Therefore, we have added a new paragraph of discussion in Section 4:

Summary and Conclusions, to highlight why cyclone composites may differ between studies due to the methods chosen. The penultimate paragraph in this section [Lines 358-364] has been edited and now reads (new text in **BOLD**):

[Lines 358-364]
"The composite structural development of Arctic summer cyclones is found to be different to that of winter North Atlantic Ocean extra-tropical cyclones and winter Arctic cyclones. This is also different to the structural development of extra-tropical cyclones described in the Norwegian Cyclone Model (Bjerknes, 1919, 1922) and Shapiro-Keyser Model (Shapiro and Keyser, 1990). The Arctic summer cyclone composite also shows differences to the Arctic Cyclone Model proposed by Aizawa and Tanaka (2016). This raises questions such as whether this unique structural transition of Arctic summer cyclones is captured in climate model projections and weather forecasting models, and how might the frequency of these Arctic summer cyclones change in response to climate change?

The results found in this study contrast with those in Clancy et al. (2022), who show that the structure of Arctic cyclones in all seasons is similar to mid-latitude cyclones. However, there are numerous differences in the methods between this study and Clancy et al. (2022). In this study, cyclone composites were calculated across the 100 most intense cyclones per season, but in Clancy et al. (2022), thousands more cyclones were included when producing cyclone composites. It is possible that this unique structural development of Arctic summer cyclones is only present in the most intense Arctic summer cyclones, and why it was shown in this study and not in Clancy et al. (2022). In addition, Clancy et al. (2022) use a different cyclone compositing method, storm tracking algorithm applied to different reanalysis datasets, and a different intensity metric to determine the most intense cyclones that contribute to their composites. These differences in method may also contribute to the differences with the results presented in this study. Future work may seek to test the sensitivity of sample size and composite method on determining the structure of intense Arctic cyclones."

10. 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.

Yes, for +8 day composites, only the systems that survive to 8 days are considered.

Text has been added in the methodology section to make this more clear. Additional text has been added to Lines 133-134 to make this more explicit (see added text in **BOLD**):

[Lines 133-134]
"The development of each composite is also determined at time-steps up to 48 hours before and up to 192 hours after the time of maximum intensity at 48-hour intervals. **Only**

cyclones that survive up 192 hours after the time of maximum intensity contribute to the cyclone composite at these lifetime positions."

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

Yes, we agree with this point. "From" has been changed to "After".

12. 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 be 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.

Gray et al. (2021) found that the role of Tropopause Polar Vortices in the development of Arctic cyclones is not as clear as the reviewer suggests. We believe that TPVs being a key aspect in the development of intense Arctic cyclones is currently more of an open question.

We agree that Tanaka, Yamagami and Takahashi, (2012) and Aizawa and Tanaka (2016) did show evidence to suggest that Tropopause Polar Vortices (TPVs) may influence the development of intense Arctic cyclones. However, these studies based their conclusions on only three (Tanaka et al. 2012) and two (Aizawa and Tanaka 2016) past Arctic cyclone case studies. We have edited the text in the Introduction between Lines 46-47 to add the reference of Tanaka, Yamagami and Takahashi, (2012), which also show Arctic cyclone case studies that were found to have a unique barotropic structure to the discussion (see added text in **bold):**

[Lines 45-46]
"This unique structure is also identified in ~~two~~**five** other past Arctic cyclones that occurred in **August 2006, August 2007, and June 2008 (Tanaka et al. 2012),** June 2008 (Aizawa et al., 2014; Aizawa and Tanaka, 2016), and September 2010 (Tao et al., 2017)."

Simmonds and Rudeva (2014) showed that Tropopause Polar Vortices (TPVs) influenced the development of 54 out of 60 most intense Arctic cyclones, with this sample size of 60 being comprised of the five most intense (by central pressure) cyclones per calendar month. However, the sample size from Simmonds and Rudeva (2014) per season in winter (DJF) and summer (JJA) is still very small and only 15 cyclones. (Following comments from Reviewer #1 - see their Comment #2, we also now include Simmonds and Rudeva (2014) as a reference in our manuscript). We have added text to the Introduction Section, that references Simmonds and Rudeva (2014) and explicitly mentions that TPVs may play a role in the development of intense Arctic cyclones. This added text is as follows (see added text in **bold**):

[Lines 46-49]

"Tanaka et al. (2012) and Tao et al. (2017) showed that in their Arctic cyclone case studies, a lower stratospheric positive potential vorticity anomaly (i.e., Tropopause Polar Vortices - TPVs) played a decisive role in each cyclone's development. Simmonds and Rudeva (2014) also showed that TPVs influenced the development of 54 out of 60 intense Arctic cyclones, with the sample size of 60 being comprised of the five most intense Arctic cyclones per calendar month from 1979-2009. However, a more systematic study that examines a greater sample of Arctic cyclones is required to show this unique cyclone structure in generality."

In a more recent paper by Gray et al. (2021) (full reference given below), the role of TPVs in development of Arctic cyclones was analysed across a much larger sample of cyclones than in Tanaka et al. (2012), Rudeva et al. (2014) and Aizawa and Tanaka (2016). Gray et al. (2021) showed that "only about one-third of Arctic cyclones have their genesis or intensify while a TPV of Arctic origin is (instantaneously) within about twice the Rossby radius of the cyclone centre" [see their Figure 6 (full citation of Gray et al. (2021) is given below)]. This reference is already referred to in our manuscript, see Lines 365-374.

TPVs were not the subject of this analysis; therefore, we can only comment on whether there is evidence to suggest that TPVs, which occur at the tropopause level, may play a role in the development of intense Arctic cyclones. Figure 7 in our manuscript does suggest that these unique axi-symmetric Arctic summer cyclones may be more strongly influenced by the stratosphere than winter Arctic and North Atlantic Ocean cyclones (see lines 272-281 in our manuscript). In this section, we have added the following text (see text in **bold**), to make this point more explicit:

[Lines 272-281]

"The temperature anomaly in the stratosphere is much more defined and larger (8.0 - 9.0 ◦C) in the Arctic summer cyclone composite at and after the time of maximum intensity (see Figure 8h and 8i). In contrast, the maximum temperature anomaly in the stratosphere after the time of maximum intensity in the winter North Atlantic Ocean extra-tropical cyclone and Arctic cyclone composites is 5.0 - 6.0 ◦C (see Figure 8). Furthermore, the altitude of the tropopause is much lower in the Arctic summer cyclone composite at and after the time of maximum intensity than the winter North Atlantic Ocean extra-tropical cyclone and Arctic cyclone composites (see Figure 8). The level of tropopause is shown to be approximately 500 hPa at the centre of Arctic summer cyclone composite at and after the time of maximum intensity. In contrast, the level of tropopause is approximately 300 hPa at the centre of the winter North Atlantic Ocean extra-tropical cyclone and Arctic cyclone composites at and after the time of maximum intensity. **These differences suggest that the development of intense Arctic summer cyclones may be more influenced by the stratosphere than intense winter Arctic and North Atlantic Ocean cyclones.**"

Gray, S. L., Hodges, K. I., Vautrey, J. L., and Methven, J.: The role of tropopause polar vortices in the intensification of summer Arctic cyclones, Weather and Climate Dynamics, pp. 1–31, 2021.

13. 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.

Yes, our confusion has come from the conceptual model (Figure 7) from Aizawa and Tanaka (2016). The conceptual model gives the impression, from its rectangular shape, that the area of Arctic cyclones is the same at upper levels as at the surface. The ambiguous statement, which does not reference the upper or lower level in the abstract from Aizawa and Tanaka (2016) "The horizontal scale of the Arctic cyclone reaches 5000 km in diameter which is one of the largest cyclones found on the Earth" also misled us.

Yes, it is stated by Aizawa and Tanaka (2016) in the Conclusions that *"The horizontal scale of the Arctic cyclone circulation reaches 5000 km in diameter at upper level"*. The reference to '4,400 km' in our manuscript is to the maximum area of Figures 3.

By using the 10 ms^{-1} earth-relative wind speed at the 850 hPa level and the area within the last closed isobar as a guide of size of the cyclones at near the surface, it is shown that the summer Arctic cyclones are smaller near the surface than the winter Arctic and North Atlantic Ocean composite cyclones (see Figure 3 in the manuscript). This suggests that the statement made by Aizawa and Tanaka (2016), that "The horizontal scale of the Arctic cyclone reaches 5000 km in diameter which is one of the largest cyclones found on the Earth", which is based on two Arctic summer cyclones, is not supported by our analysis of Arctic cyclones near the surface.

The text has been changed in Section 3.7 to reflect the review this and the reviewers comments (see added text in **BOLD**):

[Lines 293-296]
"This conceptual model highlights six main features of Arctic summer cyclones, which include: a warm core in the lower stratosphere and cold core in the troposphere, a deep tropopause fold descending down to 500 hPa over the cyclone centre, a secondary circulation in the troposphere, a downdraft in the lower stratosphere~~,~~ **and** a deep cyclonic circulation up into the stratosphere. ~~and a horizontal extent of approximately 5,000 km.~~ **Aizawa and Tanaka (2016) also describe Arctic cyclones as being one of the largest cyclones found on the Earth, with a horizontal diameter of 5,000 km at upper levels (i.e., in the stratosphere).**"

[Lines 301-302]
"~~In contrast to the Aizawa and Tanaka (2016), the general size of the Arctic summer cyclone composite appears to be much less than 4,400 km (see Figure 3), as shown by the extent of the highest closed MSLP contour (1008 hPa).~~ **Despite these similarities, the Arctic summer cyclone composites does also show differences to the Aizawa and Tanaka**

(2016) model. The general size of the Arctic summer cyclone composite appears to be smaller near the surface than the winter Arctic and North Atlantic Ocean cyclone composites (see Figure 3). This is implied by the area within the last-closed isobar (see Figure 3a, 3d and 3g), and the area within the 10 m s^{-1} earth-relative wind speed contour of each cyclone composite (see Figure 3b, 3e and 3h). Although Aizawa and Tanaka (2016) use a different metric to infer cyclone size, the Arctic summer cyclone composite is smaller in size than the winter Arctic and North Atlantic Ocean cyclone composites near the surface."

14. Lines 309-310: No needing 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).

Yes, we agree that there is repetition here. This statement *"… which was based on the analysis of just two past Arctic cyclone case studies"* on lines 309-310 has been deleted.

15. 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.

We agree that a timeseries of when the cyclones within the top 100 cyclones per class would be a valuable addition.

Such a timeseries has been created and is shown below in Figure 2 below. Overall, there has been no visible trend in the frequency of intense winter North Atlantic cyclones (Figure 2a), winter Arctic cyclones (Figure 2b), or summer Arctic cyclones (Figure 2c), when considering the occurrence of the 100 most intense cyclones per class.

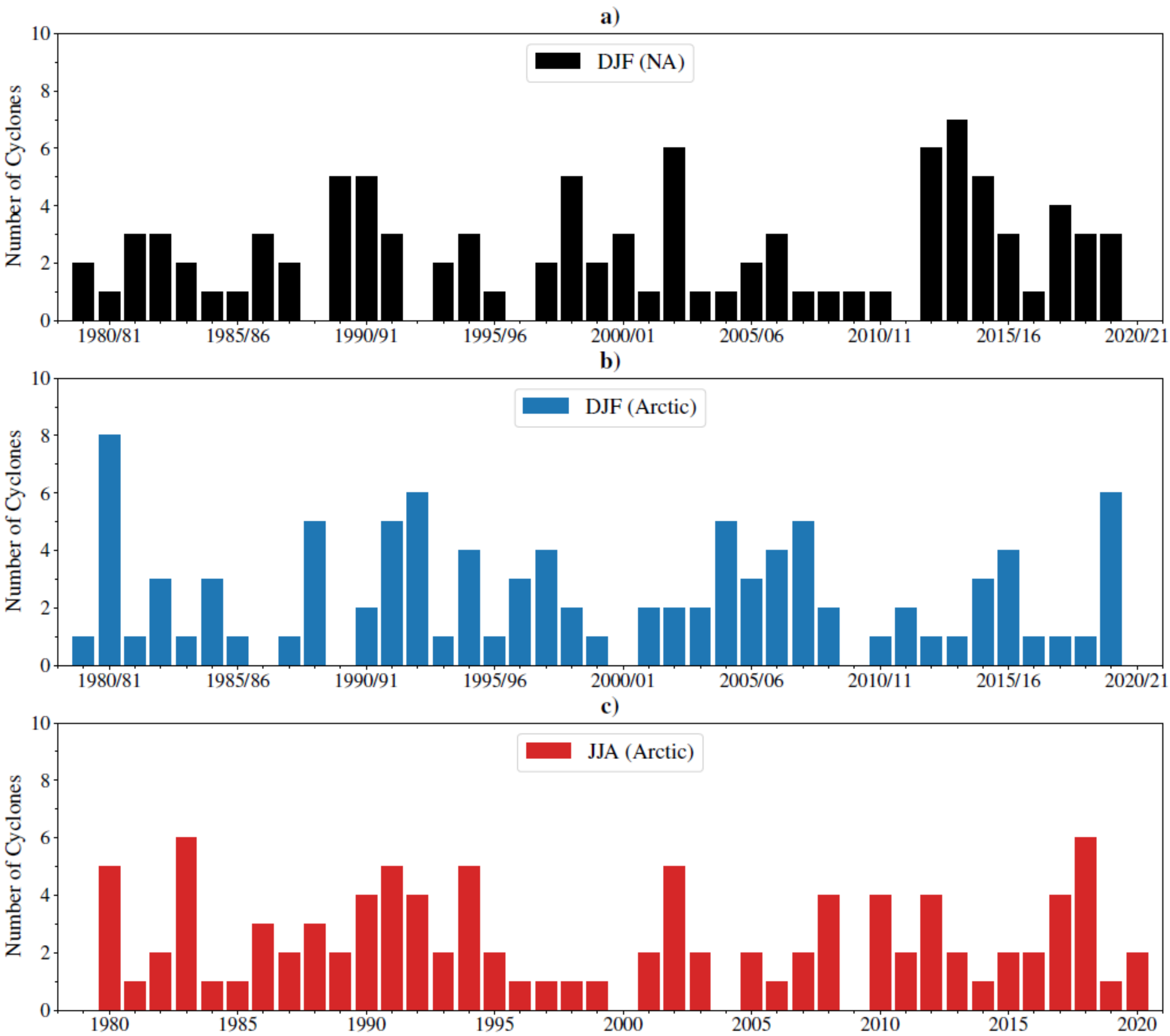

Figure 2. *Timeseries showing the occurrence per year of each cyclone in the sample of the 100 most intense a) winter (DJF) North Atlantic (NA) Ocean cyclones, b) winter Arctic cyclones, and c) summer Arctic cyclones.*

The authors agree that this information may distract the reader's attention from the main theme. Therefore, we think it is best to put this figure in Supplementary Material. Text has been added in our manuscript to point attention to this information. Additional text has been added to Lines 158-160 to make this more explicit (see added text in **BOLD**):

> **[Lines 158-160]**
> "In contrast, the 100 most intense Arctic summer cyclones tend to have their maximum intensity over higher latitudes and over the Arctic Ocean that is north of the Eurasia coastline and the Bering Strait (see Figure 2).
>
> **Over the past few decades, the Arctic has experienced the greatest change in mean surface temperature than any other region on Earth (Lenssen et al., 2019; GISTEMP Team, 2021). This raises the question, has the occurrence of intense Arctic cyclones changed over the last few decades? Figure S2 in the supplementary material shows a timeseries by year of the occurrence of each cyclone within the sample of the 100 most**

intense per cyclones class. Overall, there is no visible trend in the frequency of intense winter North Atlantic cyclones (see Figure S2a in supplementary material), winter Arctic cyclones (Figure S2b in supplementary material), or summer Arctic cyclones (Figure S2c in supplementary material), when considering the occurrence of the 100 most intense cyclones per class."

The following references that are included in the additional text above have also been added to the reference list.

GISTEMP Team, 2021: GISS Surface Temperature Analysis (GISTEMP), version 4. Accessed 17 Jun 2021, https://data.giss.nasa.gov/gistemp/.

Lenssen, N. J., G. A. Schmidt, J. E. Hansen, M. J. Menne, A. Persin, R. Ruedy, and D. Zyss, 2019: Improvements in the GISTEMP uncertainty model. J. Geophys. Res.: Atmos., 124 (12), 6307–6326.

16. 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.

Yes, thank you for pointing this out. This reference has been updated.

17. 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.

Yes, thank you for pointing this out. This reference has been updated.

Response to Anonymous Reviewer #3

Note to Anonymous Reviewer #3

We thank Reviewer #3 for their time for reading and reviewing this manuscript. We address her/his concerns below.

Response

Summary:
Overall very interesting and well-written paper. The paper studies the climatology of intense Arctic cyclones, something that has previously been done mostly based on case studies. The authors utilize a system-centered composition analysis following the cyclones through their lifetime allowing for complete and easily comparable climatologies to be made. The paper extends our knowledge on summer Arctic cyclones and makes the important point that Arctic cyclones can be structurally different depending on the season. Some minor corrections could be made into the text to enhance readability and clearing out some of the method choices made.

We thank the reviewer for taking the time to review our paper and for his/her comments on the importance of our research.

Comments:

1. Line 69: Any newer references on this topic? Maybe Varino et al. (2018) or Wickström et al. (2017)?

Yes, we agree that these more recent references are a valuable addition here. We have added them to Line 69.

2. Line 99: How are the identified cyclones grouped as cyclone tracks? The authors mention the nearest neighbor approach, but I assume there has to be some sort of limit, as to how far connected cyclone instances can exist from one another in order to be considered part of the same cyclone?

Yes, we agree that this detail is missing from the methods section. The limit to which feature points (i.e., cyclone point) can be connected as the same cyclone is dependent on maximum speeds of cyclones. If a feature point lies outside of the maximum distance that one cyclone can travel in one timestep, that feature point is considered as a separate cyclone. Hodges (1999) outlines these limits as displacement factors, which are outlined in their Table 1.

To make this more explicit in our manuscript, we have added the following text to lines 98-99 (additional text shown in **BOLD**):

[Lines 98-99]
"Features **within the maximum displacement factor (5° for the regions north of 30°N)** are then grouped into cyclone tracks using a nearest-neighbour approach, which links feature points in consecutive time-steps **(Hodges 1999)**."

3. Line 111: Why only North Atlantic winter storms were compared, why not compare winter storms to winter storms and summer storms to summer storms?

The reason for choosing to compare Arctic storms to winter (DJF) North Atlantic cyclones is because the conceptual models of extra-tropical cyclones (i.e., the Norwegian Cyclone Model and Shapiro Keyser Model) are primarily based on the analysis of winter North Atlantic cyclones. By comparing Arctic storms to winter (DJF) North Atlantic cyclones, we can therefore compare Arctic cyclones against a "standard model" of extra-tropical cyclones.

A similar question was asked by Reviewer #1 (see their Comment #1). In response to Reviewer #1, we made this more explicit in our manuscript by adding text to Lines 67-69. This additional text is given below, with the added text given in **BOLD**:

[Lines 67-69]
"Winter extra-tropical cyclones occurring over the North-Atlantic Ocean are used as a reference to compare with intense winter and summer Arctic cyclones, as they have been investigated more extensively in previous research (e.g., Bjerknes, 1919, 1922; Shapiro and Keyser, 1990; Browning, 1997, 2004; Catto et al., 2010, **Varino et al., 2019; Wickström et al., 2020**). **Conceptual models of extra-tropical cyclone structure and development are also primarily based on the analysis of winter extra-tropical cyclones. Therefore, comparing Arctic cyclones with winter North Atlantic Ocean cyclones allows for a more direct comparison to these conceptual models.**"

4. Line 155: Adding another reference would be good.

We agree with the reviewer here.

Simmonds et al. (2008) show similar results to Vessey et al. (2020). They also show (see their Figure 5) that, in ERA-40, Arctic cyclone system density [similar to cyclone track density] is typically highest in winter over the Greenland, Norwegian and Barents Seas, and over the Canadian Archipelago, whereas in summer, Arctic cyclone track density is typically highest over the coastline of Eurasia and the Arctic Ocean.

Therefore, Simmonds et al. (2008) has been added as a reference on Line 155.

Here is the full reference for Simmonds et al. (2008):

Simmonds, I., Burke, C., and Keay, K.: Arctic climate change as manifest in cyclone behavior, J. Clim., 21, 5777–5796, 2008.

5. Line 173-180: The discussion on 850hPa winds is interesting, but it made me curious how different the results would be if you looked at wind speed closer to ground (and maybe over land vs. ocean). A paper by Valkonen et al. 2021 showed that over different sea ice regimes the cyclone wind speeds were different and one would assume even larger differences between land and sea surface. Any comments?

Yes, this is a good point. We show the horizontal wind speed structure of the three classes of cyclone at the 850 hPa level (see Figure 3b, 3e and 3h), which is not at the surface level.

We have now created similar plots to Figure 3b, 3e and 3h, but plotting the 10-metre winds instead of winds at the 850 hPa level (see Figure 1 below). Figure 1 shows the earth-relative 10-metre wind speed composites for winter (DJF) and summer (JJA) Arctic storms. The main difference between the 10-metre earth-relative wind speed and the 850 hPa wind speeds is the magnitude of the winds. The maximum 850 hPa earth-relative wind speeds are 32.3, 24.5 and 20.9 m s^{-1} for the winter North Atlantic Ocean cyclone, winter Arctic cyclone and summer Arctic cyclone composites respectively (see Figure 3 in manuscript). But for the 10-metre wind speeds, the maximum is 21.2, 16.2 and 12.3 m s^{-1} for the winter North Atlantic Ocean cyclone, winter Arctic cyclone and summer Arctic cyclone composites respectively (see Figure 1 below).

The spatial distribution in the 10-metre wind field is similar to that of the 850 hPa wind speed field, with the maximum 10-metre wind speed generally occurring in the southern region of the composite and near the composite centre (see Figure 3 in manuscript). Though, both the winter Arctic cyclone and summer Arctic cyclone composites are perhaps more axi-symmetric than the winter North Atlantic Ocean cyclone composite.

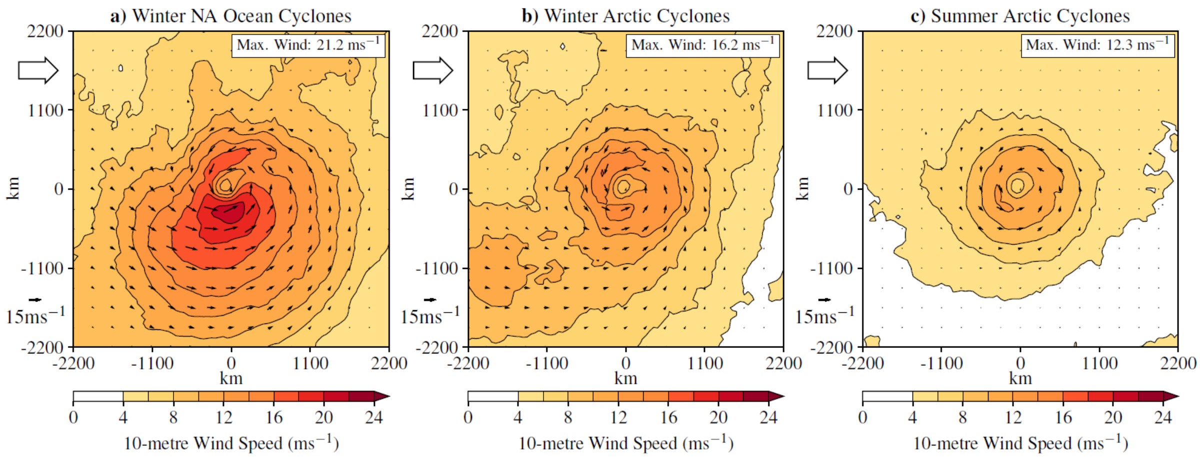

Figure 1. *Horizontal 10-metre earth-relative wind speed (m s^{-1}) composite structure at the time of maximum intensity (minimum MSLP), of* ***a)*** *winter (DJF) North Atlantic (NA) Ocean cyclones,* ***b)*** *winter Arctic cyclones and* ***c)*** *summer (JJA) Arctic cyclones. The large arrow indicates the direction of storm propagation.*

It is not possible to determine differences in the wind field over between land, sea and sea ice using our composite methodology. As the 100 most intense cyclones are averaged together, this definition in each cyclone of the localised differences due to varying surface friction over different land surfaces is smoothed out by the averaging composite method (see

Figure 1 above). Yes, we would expect wind speeds to be faster over the sea surface, due to reduced surface friction, but using our method we are unable to comment on this in our composites.

As human activity occurs at the surface, it would be important to clarify that the differences shown between the 850 hPa earth-relative winds (shown in Figure 3 in the manuscript) translate into differences in the 10-metre earth-relative winds. We believe that Figure 1 above has a place in our manuscript. As it adds information to Figure 3, we believe it is best placed in the Supplementary Material. Reference to this new figure in the Supplementary Material has been added to the text between Lines 173-180. This additional text is given below, with the added text given in **BOLD**:

> **[Lines 173-176]**
> "The maximum 850 hPa earth-relative wind speeds are weaker in the Arctic summer cyclone composite (20.9 m s^{-1}) than the Arctic winter cyclone composite (24.5 m s^{-1}) and the North Atlantic Ocean winter extra-tropical cyclone composite (32.3 m s^{-1}) (see Figure 3b, 3e and 3h). **These differences in magnitude between the cyclone composites in the 850 hPa earth-relative wind speeds, which would occur at an altitude of approximately 1 km above the surface, are also shown when analysing the composite 10-metre earth-relative wind speeds per cyclone class (see Figure S3 in the Supplementary Material). Overall, the North Atlantic Ocean winter extra-tropical cyclone composite has the strongest 10-metre wind speeds, and the Arctic summer cyclone composite has the weakest 10-metre wind speeds.** This is likely a consequence of the weaker pressure gradients within the Arctic summer cyclone composite."

6. Line 219-222: Please rephrase this sentence, it is very long and a bit difficult to understand.

Thank you for pointing this out. We agree that Line 219-222 is a very long and complicated sentence. Please see our changes below, with deleted text shown with a ~~strikethrough~~ and additional text in **BOLD**:

> **[Lines 219-222]**
> "By analysing the transect **outlined in Figure 4,** ~~4° degrees ahead of cyclone centre and perpendicular to the cyclones direction of propagation, that intersects the maximum in vertical velocity in each composite and where the WCB is indicated to (see Figure 4),~~ the WCB and CCB appear to be present in both the winter Arctic and North Atlantic Ocean cyclone composites (see Figure 5a and 5b)."

7. Line 301-302: As the size of the cyclones in this paper is compared to that of the Aizawa and Tanaka (2016) cyclones, it would be important to know if the size of the cyclone was determined in the same way in both papers. If it was not, it would be good to give an explanation as to why the last closed isobar was chosen as the area of the cyclone in this paper.

Aizawa and Tanaka (2016) infer the size of cyclones by the radius of the cyclonic circulation, which is denoted in their Figure 2 as the negative (inflowing) radial wind at the 10-metre level. Aizawa and Tanaka (2016) state "The inflow region is 1500 km in radius corresponding to the region of the cyclonic circulation".

In our manuscript, we do not show 10-metre system-relative wind speeds and therefore cannot comment on the radius of the system-relative inflowing wind at 10-metres. Therefore, to comment on cyclone size, we need to use a different proxy for the size of the composite cyclone. In this manuscript, following comments by Reviewer #1 (see their comment #4), we infer the size of the cyclone composites by using the area within the last closed isobar (see Figure 3), the area within the 10 ms^{-1} earth-relative wind speed contour (see Figure 3). The 10 ms^{-1}.

We agree with the reviewer that this difference in inferring the size of the cyclones should be more explicit in our manuscript. We have added text here to make it clearer that we use a different metric of cyclone size. This section has been changed following comments from Reviewer #2, but we have added the following text in **BOLD** to this section, to address these comments from Reviewer #3.

> **[Lines 301-302]**
> "The general size of the Arctic summer cyclone composite appears to be smaller at the 850 hPa level (i.e., near the surface) than the winter Arctic and North Atlantic Ocean cyclone composites (see Figure 3). This is implied by the area within the last-closed isobar (see Figure 3a, 3d and 3g), and the area within the 10 m s^{-1} earth-relative wind speed contour of each cyclone composite (see Figure 3b, 3e and 3h). **Although Aizawa and Tanaka (2016) use a different metric to infer cyclone size, the Arctic summer cyclone composite is smaller in size than the winter Arctic and North Atlantic Ocean cyclone composites near the surface. In this study, cyclone size is inferred by the area within the last closed isobar and the area where the 850 hPa earth-relative wind speeds exceed 10 m s−1 (see Figure 3 and Section 3.3).**"

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