nature portfolio

Peer Review File

Arctic cyclones have become more intense and longer-lived over the past seven decades

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

27th Mar 23

Dear Professor Zhang,

Your manuscript titled "Intensification of Arctic Cyclone Activity" has now been seen by 3 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:

[Link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Sylvia Sullivan, PhD
Editorial Board Member
Communications Earth & Environment

Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

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and uploaded as a Related Manuscript file type with the revised article.

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Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
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.

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All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

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Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Overall Comments
Recently, the Arctic has under gone an amplified warming relative to the rest of the Earth that has resulted in many changes to its weather and climate. This is considerable uncertainty if this so-called Arctic Amplification has resulted in changes in the nature of the large-scale weather systems known as extra-tropical cyclones that propagate into or form in the region. This paper updates and extends a previous analysis and provides convincing evidence that cyclone activity in the Arctic has increased since the middle of the 20th century. The paper is technically sound and, for the most part, well written. There are however some issues that need to be clarified before it is accepted for publication.
Major Comments
1) Debate about changes in cyclone activity in the Arctic.
A central result of this paper is that this new analysis helps reconcile a debate that has been ongoing (Line 92). The introduction covers this debate in a single sentence (Line 44) that references 4 papers. A more through discussion of this debate would improve the paper and would place this work in its proper context.
2)The Cyclone Activity Index
a) The authors use 3 different reanalysis product to compute their Energy-based Cyclone Activity Index. All three give similar results. This is a good thing but hardly surprising given the sea-level pressure is perhaps the most robust field in an atmospheric reanalysis in the mid and high latitudes. However, the Arctic is a data sparse region, especially as one goes back in time. Until recently, it has been difficult to provide an estimate of the uncertainty in a reanalysis field. The ERA5 does include a parallel, but lower resolution, ensemble version with 10 members that could be used to provide some confidence in the results.
b) The assessment of the statistical significance of a trend in a geophysical time series can be challenging because of the temporal autocorrelation (red noise) this is often a characteristic of these time series. This reduces the degrees of freedom and if not taken into account, it can bias the statistical significance. The authors reference Santer et al (2000) that discusses this issue but does not explicitly mention that they have taken the red noise characteristics of the time series.
c) There is also an interesting annual cycle in the CAI. The paper would be improved by a discussion of this cycle and its impact on the significance of the trend. If one looks at the time series of Arctic sea ice extent, then the trend in the full time series is smaller than that if one considers the trend in September sea ice extent and larger that that is one considers the trend in March sea ice extent. I'm wondering if something similar is occurring here. This would be of interest as the authors go onto to consider summer and winter behaviour.
d) The authors comment that the maximum in CAI in the early 1990s is also seen in the AO index. The reference is to the Thompson and Wallace (1998) paper. It does show an uptick in the AO in the early 1990s but, because of its age, its more recent behaviour. I believe that there are some more recent references but the inclusion of the AO index in Fig 1 would be useful.
e) Is there a temporal evolution in the trend? One characteristic of Arctic Amplification is that the warming accelerated in the early 2000s. This is also seen in other indices related to sea ice extent. Is the same true here? I understand that the spike in CAI activity in the early 1990s would make this a challenge but some sort of moving window trend analysis might provide some insight.
f) On lines 92-94, the trend in CAI is mapped into trends in central pressure and wind. How was this done?
3) Figure 2 is quite an interesting way to drill down on the results. I understand that these are anomalies but I'm not sure of the units for the duration anomalies. I assume that they are in days but this should be indicated. Furthermore, the authors claim that 'most prominent feature, superimposed on the low-frequency variability, was a long-term shift of the maximum count from weaker to stronger cyclones in both winter and summer'. This result is clearly influenced by the 1990 max in CAI. It is not clear that this behaviour has continued. Could the authors choose a couple of different central pressures, one high and one low, and repeat the trend analysis? This would provide some quantitative support for the claims made.

4) Eady Growth Rate
a) The authors use a well know diagnostic for baroclinicity, the Eady Growth Rate, in the interpretation of the reasons for the increase in cyclone activity. The EGR is defined in the Methods Section, however on line 189,the authors indicate that they used the 900hPa level in the definition of the EGR. The EGR requires two levels to define it. Did the authors use the surface to 900hPa? If so, I don't really think that this, as claimed by the authors, captures the lower tropospheric variability. If this is not the case, please be more specific as to the levels used.
b) The winter results show a recent increase in cyclone activity in the Barents Sea as well as in the central Arctic. This is consistent with some recent results (Moore et al., 2018; Ballinger et al., 2021; Moore et al., 2022) that show enhanced cyclone activity has resulted in a collapse of the winter Beaufort High during recent winters.
5) Breakpoint used in Figure 3
The authors divide the time period under consideration (1950-221) into two equal halves for the various fields shown in Figure 3. I wonder how much the 2nd period is influenced by the maximum in the 1990s. Are the results robust, if one chooses periods near the start and end of the record that exclude the 1990s maximum?
6) An impact of global warming is an intensification of the hydrological cycle in the Arctic (Bintanja & Selten, 2014; McCrystall et al., 2021). Do moist process play a role in the intensification of cyclone activity?
Minor Comments
1) The first line of the Abstract presupposes the results of the paper.
2) The wording is often hyperbolic and should be toned down. Examples: Lines 71-72 include the phrases 'extremely positive' and 'tremendous intensification'; Line 90 ' extends and solidifies' and Line 205 'conspicuously large'.
3)What is meant by 'jet stream waves' (Line 226)?

References

Ballinger, T. J., Walsh, J. E., Bhatt, U. S., Bieniek, P. A., Tschudi, M. A., Brettschneider, B., et al. (2021). Unusual west Arctic storm activity during winter 2020: Another collapse of the Beaufort High? Geophysical Research Letters, 48(13), e2021GL092518.
Bintanja, R., & Selten, F. (2014). Future increases in Arctic precipitation linked to local evaporation and sea-ice retreat. Nature, 509(7501), 479-482.
McCrystall, M. R., Stroeve, J., Serreze, M., Forbes, B. C., & Screen, J. A. (2021). New climate models reveal faster and larger increases in Arctic precipitation than previously projected. Nature communications, 12(1), 6765.
Moore, G., Schweiger, A., Zhang, J., & Steele, M. (2018). Collapse of the 2017 winter Beaufort High: A response to thinning sea ice? Geophysical Research Letters, 45(6), 2860-2869.
Moore, G., Steele, M., Schweiger, A. J., Zhang, J., & Laidre, K. L. (2022). Thick and old sea ice in the Beaufort Sea during summer 2020/21 was associated with enhanced transport. Communications Earth & Environment, 3(1), 198.

Review by Kent Moore

Reviewer #2 (Remarks to the Author):

Review for Intensification of Arctic Cyclone Activity, Zhang et al. 2023

Paper Summary

This paper uses an improved cyclone tracking algorithm and calculated a new cyclone activity metric (Energy based Cyclone Activity Index EnCAI) on ERA5, NCEP-NCAR, and JRA-55 reanalysis datasets from 1950-2021 to examine trends and changes in Arctic cyclone frequency, duration, and intensity. Spatial analyses are used to show the relationship to larger scale atmospheric processes.

The paper is well written and presents a novel contribution to the field with an increased climatology temporal range, an updated cyclone tracking algorithm, and new insights into the larger scale driving mechanisms for cyclone activity. I recommend publication with suggested changes as detailed below.

Major Comments

Cyclone Database
– It is surprising that the results from the reanalyses of such vastly different resolutions produces comparable cyclone results in terms of frequency and intensity. While Figure 1 does show the EnCAI for the three reanalyses, additional figures could be added to the supplementary materials to demonstrate spatial range/variation of cyclone activity between the datasets.
– As the authors note, the community has a number of methods and algorithms. This study still uses only one of those algorithms. The authors should discuss any limitations of using one algorithm and how results compare to results from other tracking algorithms. The authors could include caveats on how the results and interpretations of this paper may vary with using a different algorithm.

The large increase in the 1980s
– While figure 1 shows a jump in the trend in the late 1980s, values do decrease again in the 1990s. Additional statistical analyses could be used to more robustly demonstrate that this constitutes a breakpoint or step change in the time series. This would improve the reasoning for most of the analysis being conducted in terms of contrasting the multi decadal averages of 1950-1985 and 1985-2021.
– The authors should consider discussing whether an increase in available satellite observations around 1980 that are assimilated into data assimilation models to generate reanalysis datasets could influence the trends seen in this long-term dataset.

Driving mechanisms section
– Overall, it would be good to be clearer about whether the analysis presented in this paper is able to demonstrate causation versus correlation and how. The language in the results and conclusions section should be adjusted to reflect this.
– The paper could consider whether decadal atmospheric patterns may affecting the decadal analyses e.g. the Arctic Oscillation?
– Lines 300-312: this discussion is very compelling. However, how these distinct cyclone phases and their specific drivers are determined from the analysis presented is not clear. Another figure or diagram would be needed to support this discussion.
– Analysis of statistical significance of the differences in atmospheric conditions may be warranted and bolster the scientific discussion here.

Minor Comments

• Line 32: it could be helpful to define what is meant by “strong cyclones” in this context.
• Lines 94-98: a discussion on how these cyclone trends are affected by the large-scale trend in SLP over the polar regions would be useful here.
• Line 160: suggest removing “well” before “consistent”.
• Consider adding analysis of cyclone genesis location to support sections like 175-181.
• Line 212: an example of the major comment about correlation v causation. Does the analysis demonstrate causation or correlation here? It appears to be correlation and the sentence should be adjusted accordingly.
• Conclusions: could discuss how these findings relate to future climate projections.

• Line 406-407: would be useful to elaborate more on the choice of 350km as the radius threshold. How was that determined?
• Figure 2: I might be misunderstanding, but what is the reasoning for calculating the anomalies relative to the climatological values from 1980-2009 and not the full length of the record?
• Figure 2: it is curious that there are no anomalies for c) for stronger cyclones. Is this because there are few strong summer cyclones? If so, the authors should note this.
• Figure 2: it was hard to discern from the figure (or any others) the discussion that the strong cyclones are also able to propagate further poleward into the Arctic. An additional figure may be warranted examining genesis and lysis location or something similar.
• Figure 4: these plots are very small with a lot of information. I suggest changing the orientation to portrait, so that each subplot can be larger.

Reviewer #3 (Remarks to the Author):

This paper uses a cyclone tracking scheme to track Arctic cyclones in reanalysis products, regardless of where they generate. The authors determine a tendency towards a greater number of cyclones, as well as longer lasting cyclones, and an increase in stronger cyclones over the Arctic in the last 7 decades. Physical processes, such as changes in baroclinicity, tropospheric, and stratospheric processes are proposed as the cause of the trend.

This paper is mostly fine, and the figures are of high quality, but needs refinement on the communication of the methods and some of the ideas and conclusions presented. Some general issues, with non-exhaustive specific examples are:

It might be helpful to break up the results section into further subsections and look at each of baroclinicity, tropospheric processes, and stratospheric processes separately, for clarity. I'd like to see a clearer explanation for why each of these lines of research was followed.

The 'Driving Mechanisms' section also seems to include quite a lot of what seems to me is better suited to either the introduction, (e.g. recent work on trends in SPV, the SAV vs. the SPV, strong summer cyclone development, lines 237-239, 244-245, 248-249, etc) or the discussion section (e.g. the more speculative parts on the warming hole's cause, the strong cyclones of 2012 and 2016, QG theory, etc.) And in fact, many of these things are repeated in the 'Summary & Discussion', without any additional discussion.

Some general but minor comments:

Incorrect use of past tense throughout the paper.
Line 135: defined -> define
Line 146 existed -> probably mean something more like 'is found'
Line 147 was highly consistent - > is highly consistent
Etc. The results should be spoken about in a present tense, even if the analysis is covering a past period, because the results are current…I hope that makes sense.
Vague wording in reference to physical processes/characteristics
Line 161 ' leading large-scale atmospheric modes' -- which?
Line 143: what essential spatial structures?
Some grammatical problems with sentence structure, such as missing commas, em-dash, etc. This issue seems to get somewhat more glaring through the manuscript, might just need a more thorough edit.
Line 30-31 missing commas or em-dash around 'and long-term changes in'
Line 136-137 same as above for 'or being generated within'

Some more specific comments, mostly on the intro and first part of results where I didn't have general comments:

Line 20-22, this last sentence seems to be slightly misrepresenting ideas about reduced baroclinicity and cyclones, as, to my understanding, this is meant to apply to mid-latitude cyclones, not necessarily Arctic ones.

Lines 41-46: First sentence is about observations, which is not necessarily clear.

Line 53-54: Quite a vague sentence if the purpose is to make convincing argument of the usefulness of the manuscript.

Line 59: improved compared to what?

Line 73-74: if EnCAI is re-iterating the AO timeseries, why do we need EnCAI?

Line 86: is there a typo here? The error on the trend clearly makes the trend non-significant.

Lines 92-96: This is a very helpful way of putting the results, I like this section. Does this come from EnCAI? I realize EnCAI is defined in the methods but it would be helpful to understand where these metrics are coming from.

Lines 104-114: I find this part a bit difficult to follow. It could use some rewriting for clarity. Is this all to say that the shape of the SLP distribution changes, while the cyclone count distribution shifts?

Line 118-119: is this true in general, i.e., can it be cited, or is it a result of the paper?

Line 125-128: What is meant by less integrated nature of a metric, after saying something about a full spectrum of cyclone intensity?

Line 155-160: I don't really understand these sentences. Things like 'One maximum increase', 'shift of the high frequency', and then what the difference between cyclone occurrence and cyclone track …

Line 176-180: Can this be shown in a more explicit way using cyclone tracks from the tracking algorithm?

Line 181-182: their propagation outside the Arctic, I assume?

Line 221-222: It's not that it can't explain it, it's that it's in contradiction.

A few very minor comments

Line 32 decade -> decades

Line 141, elsewhere" well covers -> cover

Line 228 At the same time -> I think this should start a new paragraph

Responses to Reviewers

We thank all three reviewers for their constructive and detailed evaluations on our manuscript. We have carefully addressed and incorporated the comments and suggestions, which, we believe, have improved our manuscript in both content and presentation. Below are our one-to-one responses to the review comments and suggestions (the comments and suggestions in black and the responses in blue):

Responses to Reviewer #1:

Major Comments

1) Debate about changes in cyclone activity in the Arctic.

A central result of this paper is that this new analysis helps reconcile a debate that has been ongoing (Line 92). The introduction covers this debate in a single sentence (Line 44) that references 4 papers. A more through discussion of this debate would improve the paper and would place this work in its proper context.

In response to this suggestion, we have added two more papers [Wickström et al. (2019) and Valkonen et al (2021)] to the list of recent studies reporting trends of Arctic cyclones. These studies are cited in Lines 65, 172 and 244. The citations in Line 65 are the studies using reanalysis datasets. The inconsistency can be further substantiated by the citations using climate model output, as noted with references in Lines 65-67. To make this clearer, we have added "using reanalysis datasets" after "… recent studies" in Line 64 of the revised manuscript. We have also added a new reference to the IPCC (2021), which highlights the lack of model agreement in projected future changes in Arctic cyclone frequency. The IPCC reference is cited in Line 699.

2) The Cyclone Activity Index

a) The authors use 3 different reanalysis product to compute their Energy-based Cyclone Activity Index. All three give similar results. This is a good thing but hardly surprising given the sea-level pressure is perhaps the most robust field in an atmospheric reanalysis in the mid and high latitudes. However, the Arctic is a data sparse region, especially as one goes back in time. Until recently, it has been difficult to provide an estimate of the uncertainty in a reanalysis field. The ERA5 does include a parallel, but lower resolution, ensemble version with 10 members that could be used to provide some confidence in the results.

We agree with the reviewer about the difficulty to estimate uncertainties in the reanalysis datasets for the data-sparse Arctic region. We believe the consistency of the EnCAIs between the three reanalysis datasets can be attributed to two factors: (1) cyclones are an integrated element of, and conditioned by, the large-scale atmospheric circulation. Therefore, the model-data assimilation systems for generating the reanalysis datasets, which are able to replicate the large-scale atmospheric circulation patterns and constrained by the observations outside the Arctic, can realistically capture the cyclone development in the Arctic; (2) the observations within the Arctic, although limited (such as the drifting ice stations back to the mid-19th century), provided further constraints locally. We have added new text (Lines 107-113) to make these points in the manuscript.

We appreciate the suggestion using the ten ERA5 ensemble members to do additional analysis. However, these ensembles are not designed to create additional realizations of the ERA5 reanalysis dataset. They are designed to estimate background errors (B matrix, see Ref. 63: Hersbach et al., 2020) that are required and used in the 4D-Var data assimilation procedure. As described in Hersbach et al. (2020), the ensembles are provided for estimating relative, random "synoptic" uncertainties, rather than systematic uncertainties. It is suggested that the ensembles should be mainly used for evaluating the representation of the synoptic situation at a particular time; the ensemble is not appropriate for use in climate analysis including long-term trends. We have had discussions with the ERA5 lead author, who has double-confirmed this. Given this information, we prefer not to include these ten ensembles in the paper.

Hersbach, H. et al. The ERA5 global reanalysis. *Q. J. R. Meteorol. Soc.* **146**, 1999-2049 (2020).

b) The assessment of the statistical significance of a trend in a geophysical time series can be challenging because of the temporal autocorrelation (red noise) this is often a characteristic of these time series. This reduces the degrees of freedom and if not taken into account, it can bias the statistical significance. The authors reference Santer et al (2000) that discusses this issue but does not explicitly mention that they have taken the red noise characteristics of the time series.

We agree with the reviewer about the challenge of autocorrelated time series. We have taken red noise into account following the approach of Santer et al. (2000) by calculating autocorrelations and effective sample sizes. Then we conducted a t-test using the modified standard error of the regression coefficient. In response to the reviewer's comment, we have added a more explicit description of this in Lines 145-146 of the revised manuscript.

c) There is also an interesting annual cycle in the CAI. The paper would be improved by a discussion of this cycle and its impact on the significance of the trend. If one looks at the time series of Arctic sea ice extent, then the trend in the full time series is smaller than that if one considers the trend in September sea ice extent and larger that that is one considers the trend in March sea ice extent. I'm wondering if something similar is occurring here. This would be of interest as the authors go onto to consider summer and winter behaviour.

We appreciate the comment and agree with the reviewer about the seasonality. At the reviewer's suggestion, we have added a figure in Supplementary Information showing time series of September and March EnCIAs (Fig. S2), with related discussions in Lines 177-181 of the revised manuscript. The trend of EnCAI in September is larger than in March, similar to the changes in the sea ice extent. Considering the nature of this study and the length limit of this manuscript, we have chosen to focus on the overall characteristics of Arctic cyclone activity in this manuscript and will conduct a more detailed seasonality analysis in a follow-up study as mentioned in the manuscript.

d) The authors comment that the maximum in CAI in the early 1990s is also seen in the AO index. The reference is to the Thompson and Wallace (1998) paper. It does show an uptick in the AO in the early 1990s but, because of its age, its more recent behaviour. I believe that there are some more recent references but the inclusion of the AO index in Fig 1 would be useful.

We accepted reviewer's suggestions. We have added Ref. 40 and an associated brief description in Line 140 of the revised manuscript, which, together with Refs. 38 and 39, shows recent behavior of the AO/NAO. In the interest of Figure 1's conciseness and readability, we added a new figure, Fig. S1, in the Supplementary Information showing the AO index and the averaged EnCAI from the three reanalysis datasets. We have also added a citation of this figure in the main text Lines 128-129 of the revised manuscript.

e) Is there a temporal evolution in the trend? One characteristic of Arctic Amplification is that the warming accelerated in the early 2000s. This is also seen in other indices related to sea ice extent. Is the same true here? I understand that the spike in CAI activity in the early 1990s would make this a challenge but some sort of moving window trend analysis might provide some insight.

This is an interesting question. According to our analysis, the CAI and the Arctic amplification are not linearly associated. This is similar for other parameters/indices describing atmospheric dynamics. This is because changes in atmospheric dynamic systems, such as cyclones and the AO/NAO, are not only expressed by their intensities but also by their spatial structures. For example, the AO/NAO index did not further increase when the Arctic amplification started to be more prominent since the early 2000s. Instead, its spatial structure was shifted, showing a meridionally transformed circulation pattern in the early 2000s (e.g., Ref. 39). In addition, the Arctic amplification and cyclones can interact with each other, which is a highly challenging topic in need of investigations.

f) On lines 92-94, the trend in CAI is mapped into trends in central pressure and wind. How was this done?

The calculation of EnCAI uses both cyclone central pressure and geostrophic wind. All of these terms were calculated at the same time.

3) Figure 2 is quite an interesting way to drill down on the results. I understand that these are anomalies but I'm not sure of the units for the duration anomalies. I assume that they are in days but this should be indicated. Furthermore, the authors claim that 'most prominent feature, superimposed on the low-frequency variability, was a long-term shift of the maximum count from weaker to stronger cyclones in both winter and summer'. This result is clearly influenced by the 1990 max in CAI. It is not clear that this behaviour has continued. Could the authors choose a couple of different central pressures, one high and one low, and repeat the trend analysis? This would provide some quantitative support for the claims made.

In response to the reviewer's comment, we added the unit (hours) of the duration in Line 214 to make the description clearer. Meanwhile, we selected SLPs at 1005 hPa and 980hPa for both winter and summer and calculated the trends of the cyclone counts exceeding these intensity thresholds. We confirmed that there is a decreasing trend at 1005 hPa and an increasing trend at 980 hPa. These trends can also be seen in Figure 2a and c.

4) Eady Growth Rate

a) The authors use a well know diagnostic for baroclinicity, the Eady Growth Rate, in the interpretation of the reasons for the increase in cyclone activity. The EGR is defined in the Methods

Section, however on line 189,the authors indicate that they used the 900hPa level in the definition of the EGR. The EGR requires two levels to define it. Did the authors use the surface to 900hPa? If so, I don't really think that this, as claimed by the authors, captures the lower tropospheric variability. If this is not the case, please be more specific as to the levels used.

We used three vertical levels at 925, 900, and 875 hPa to calculate the EGR. At the reviewer's comment, we added a sentence in Lines 757-758 of the revised manuscript to make this clearer.

b) The winter results show a recent increase in cyclone activity in the Barents Sea as well as in the central Arctic. This is consistent with some recent results (Moore et al., 2018; Ballinger et al., 2021; Moore et al., 2022) that show enhanced cyclone activity has resulted in a collapse of the winter Beaufort High during recent winters.

We have added the suggested papers and associated discussion in Lines 300-302.

5) Breakpoint used in Figure 3
The authors divide the time period under consideration (1950-221) into two equal halves for the various fields shown in Figure 3. I wonder how much the 2nd period is influenced by the maximum in the 1990s. Are the results robust, if one chooses periods near the start and end of the record that exclude the 1990s maximum?

We did not intend to split the time period into two equal halves. The split of the time period is based on Fig. 2, which shows a prominent increase in both strong cyclone counts and durations around the mid 1980s for both winter and summer. According to Fig. 2, for strong cyclones deeper than 990 hPa, there are positive anomalies during recent decades and negative anomalies in during earlier decades, even when the spike in the early 1990s is excluded. To make the definition of the two periods clearer, we have revised the sentence in Lines 249-250.

6) An impact of global warming is an intensification of the hydrological cycle in the Arctic (Bintanja & Selten, 2014; McCrystall et al., 2021). Do moist process play a role in the intensification of cyclone activity?

We believe the moist process would intensify the cyclone activity through latent heat release. So, following the reviewer's comment, we have added a discussion about this in Lines 651-656.

Minor Comments
1) The first line of the Abstract presupposes the results of the paper.

This indicated cases, not a long-term trend. However, we have revised the abstract so that this presupposition is no longer present.

2) The wording is often hyperbolic and should be toned down. Examples: Lines 71-72 include the phrases 'extremely positive' and 'tremendous intensification'; Line 90 ' extends and solidifies' and Line 205 'conspicuously large'.

Following the suggestion, we have removed some of the hyperbolic adverbs and done some additional rewording to tone down the statement.

3) What is meant by 'jet stream waves' (Line 226)?

They are planetary Rossby waves propagating along the jet streams. We have added an explanation in Lines 420-421 of the revised manuscript to make this clearer.

Responses to Reviewer #2

Major Comments

Cyclone Database
– It is surprising that the results from the reanalyses of such vastly different resolutions produces comparable cyclone results in terms of frequency and intensity. While Figure 1 does show the EnCAI for the three reanalyses, additional figures could be added to the supplementary materials to demonstrate spatial range/variation of cyclone activity between the datasets.

In response to this suggestion and the minor comment below about cyclogenesis locations, we have added a new figure (Fig. S3) demonstrating the spatial range/variation of the tracks of strong cyclones entering the Arctic (north of 60°N) in the three reanalysis datasets. From the figure, all three reanalysis datasets consistently capture the fundamental characteristics of the cyclogenesis and cyclolysis locations, propagation trajectories, and changes between the two multidecadal periods used in this study. One noticeable difference is that the higher resolution dataset captures more cyclones. This is more obvious in the summer plots.

– As the authors note, the community has a number of methods and algorithms. This study still uses only one of those algorithms. The authors should discuss any limitations of using one algorithm and how results compare to results from other tracking algorithms. The authors could include caveats on how the results and interpretations of this paper may vary with using a different algorithm.

We appreciate the reviewer's comments and have added a paragraph in Lines 665-678 including new citations of the papers by Simmonds and Rudeva (2014) and Crawford and Serreze (2016). In this paragraph, we discussed potential uncertainties caused by the algorithms, especially when using higher resolution datasets. We propose a comparison between our newly improved algorithm with other algorithms as a follow-up study.

The large increase in the 1980s
– While figure 1 shows a jump in the trend in the late 1980s, values do decrease again in the 1990s. Additional statistical analyses could be used to more robustly demonstrate that this constitutes a breakpoint or step change in the time series. This would improve the reasoning for most of the analysis being conducted in terms of contrasting the multi decadal averages of 1950-1985 and 1985-2021.

In response to the reviewer's comment, we examined the trends from 1950-1988 and from 1996-2021, excluding the spike from the late 1980s and the early 1990s. The EnCAI trends for the two periods are 0.16 ± 0.83 and 0.30 ± 0.78 per decade, respectively. The trend during the more recent period is almost double that during the earlier period. These results indicate that the spike does not influence the long-term trend from 1950-2021. This can also be confirmed by the time series in Fig. 1, showing mean EnCAI values above +0.5 after 2010 and below -0.5 before 1960. We have added a discussion on this in Lines 164-169 of the revised manuscript.

In addition, the definition of the two multidecadal periods for spatial structures and mechanisms analysis is not based on the long-term trend. We split the two periods based on Fig. 2, which shows a prominent increase in strong cyclone counts and durations around the mid-1980s. To make this clearer, we have reworded the sentences in Line 249-250.

– The authors should consider discussing whether an increase in available satellite observations around 1980 that are assimilated into data assimilation models to generate reanalysis datasets could influence the trends seen in this long-term dataset.

In response to this suggestion, we have added a discussion in Lines 114-123. The EnCAI time series does not show a discontinuation or unusual variation around 1980. Rather, the EnCAI fluctuations are well within the range of one standard deviation of its long-term variability. Also, different reanalysis datasets assimilate different amounts of satellite observations using different approaches. We cannot see systematic errors in the EnCAI between the reanalysis datasets. This can be attributed to the factors discussed in Lines 107-113: (1) the cyclones in the Arctic are an integrated element of the large-scale atmospheric circulation, which are supported by the physical process analysis in the "Driving Mechanisms" section, and the model-data assimilation system for generating reanalysis datasets are able to replicate the large-scale atmospheric circulation; and (2) the in-situ SLP observations in the subarctic and lower latitudes, plus the additional in-situ observations within the Arctic (such as the drifting ice stations) have provided good constraints to the reanalysis dataset. We therefore contend that assimilation of satellite observations around 1980 may not have fundamental influence on the SLP and, in turn the EnCAI trend.

Driving mechanisms section

– Overall, it would be good to be clearer about whether the analysis presented in this paper is able to demonstrate causation versus correlation and how. The language in the results and conclusions section should be adjusted to reflect this.

The analysis presented in this section is about causation, not correlation. This is because the baroclinic instability, the tropospheric jet streams and waves, the stratospheric vortex are the fundamental mechanisms driving cyclone genesis, development, and propagations based on the cyclone dynamics theory and research. For example, cyclones consume baroclinic energy; they do not create baroclinic energy. To make this clearer, we revised the sentences in Lines 333-335 and in Line 591 of the revised manuscript to state explicitly and clearly what we analyzed.

– The paper could consider whether decadal atmospheric patterns may affecting the decadal analyses e.g. the Arctic Oscillation?

We have followed the suggestion and have added a brief discussion in Lines 405-410 and a figure (Fig. S1) in Supplementary Information about the relationship between the AO and EnCAI. The differences of the 500 hPa GHT between the two multidecadal periods shows a decrease over the Labrador Sea and the southern tip of Greenland, but an increase over the North Atlantic and Mediterranean Sea. This pattern has strong projection on the AO. Therefore, the AO and the EnCAI are highly correlated (correlation coefficient = 0.71) though the AO has a much smaller increasing trend, further suggesting two-way interactions between the large-scale circulation variability and the synoptic scale cyclones as discussed in Lines 126-140.

– Lines 300-312: this discussion is very compelling. However, how these distinct cyclone phases and their specific drivers are determined from the analysis presented is not clear. Another figure or diagram would be needed to support this discussion.

We have accepted the suggestion and added a schematic diagram (Fig. 5) to summarize and illustrate the changes in the intense summer cyclone structure and driving mechanisms more clearly.

– Analysis of statistical significance of the differences in atmospheric conditions may be warranted and bolster the scientific discussion here.

We have accepted the suggestion and added statistical significance test for the differences of the cyclone frequencies in both winter and summer (Fig. 3d and f). Figure 3's caption contains the information on the significance test.

Minor Comments

• Line 32: it could be helpful to define what is meant by "strong cyclones" in this context.

We have added examples of the strong cyclones in Lines 41-42 to make the sentence clearer. The definition of strong cyclones used in this study is described in Lines 251-252, which states that the criterion is a cyclone central SLP deeper than 990 hPa.

• Lines 94-98: a discussion on how these cyclone trends are affected by the large-scale trend in SLP over the polar regions would be useful here.

Following the comment above about impacts of decadal atmospheric patterns, such as the AO, we have added a discussion in Lines 405-410 in the main text and Fig. S1 in Supplementary Information. The AO represents a large-scale SLP variations in the Arctic. Its long-term trend is much smaller than the EnCAI's trend (Fig. S1). This is because, although cyclones cumulatively contribute to the large-scale SLP (e.g., monthly) on a daily basis, the large-scale SLP variability and changes are also influenced by anticyclones and other SLP disturbance. We therefore believe the large-scale SLP trend within the Arctic does not directly influence the EnCAI trend.

• Line 160: suggest removing "well" before "consistent".

We accepted the suggestion; “well” has been deleted.

• Consider adding analysis of cyclone genesis location to support sections like 175-181.

Following this suggestion, we have added a sentence in Lines 328-329 and new Fig. S3 in Supplementary Information. Fig. S3 explicitly shows the tracks (including the cyclogenesis and cyclolysis locations) of strong cyclones with central SLP deeper than 970 hPa in winter and 985 in summer after entering the Arctic (north of 60°N). It is apparent that the strong summer cyclones reaching the central Arctic (north of 80°N) are predominantly generated within the Arctic, in contrast to the tracks of strong winter cyclones, most of which come from the midlatitudes and subarctic (Fig. S3a-f).

• Line 212: an example of the major comment about correlation v causation. Does the analysis demonstrate causation or correlation here? It appears to be correlation and the sentence should be adjusted accordingly.

Please see our response to the same comment above.

• Conclusions: could discuss how these findings relate to future climate projections.

At the reviewer’s suggestion, we have added a discussion about future projection in Lines 689-699, together with a reference to the IPCC (2021). This discussion is in addition to the references provided earlier (Line 67) to studies of projected changes in Arctic cyclones.

• Line 406-407: would be useful to elaborate more on the choice of 350km as the radius threshold. How was that determined?

At the reviewer’s suggestion, we have added the information of spatial resolutions of NCEP-NCAR reanalysis and ERA5 in Lines 726-727. We choose a radius of 350 km to ensure that there are enough grid points in the lower resolution NCEP-NCAR reanalysis dataset (2.5° x 2.5°) to calculate mean SLP gradients. For example, the largest distance of two grid points at 60°N is about 307.735 km. In this case, a radius of 350 km includes 9 grid points to be used to calculate SLP gradient.

• Figure 2: I might be misunderstanding, but what is the reasoning for calculating the anomalies relative to the climatological values from 1980-2009 and not the full length of the record?

A 30-year period is usually recommended as a baseline of climatology in climate studies. We choose a period of 1980 - 2009 to represent this period, covering both relatively colder and warmer periods of the Arctic climate. The choice of the baseline does not influence the identified variability and trends.

• Figure 2: it is curious that there are no anomalies for c) for stronger cyclones. Is this because there are few strong summer cyclones? If so, the authors should note this.

Yes, there are not many cyclones deeper than 975 hPa. We have added a note in the figure caption.

• Figure 2: it was hard to discern from the figure (or any others) the discussion that the strong cyclones are also able to propagate further poleward into the Arctic. An additional figure may be warranted examining genesis and lysis location or something similar.

Following the comments here and above, we have added a new figure, Fig. S3, in the Supplementary Information to show the strong cyclone propagation trajectories, including the cyclogenesis and cyclolysis locations.

• Figure 4: these plots are very small with a lot of information. I suggest changing the orientation to portrait, so that each subplot can be larger.

We accepted the suggestion and have replotted Fig. 4.

Responses to Reviewer #3

This paper is mostly fine, and the figures are of high quality, but needs refinement on the communication of the methods and some of the ideas and conclusions presented. Some general issues, with non-exhaustive specific examples are:

It might be helpful to break up the results section into further subsections and look at each of baroclinicity, tropospheric processes, and stratospheric processes separately, for clarity. I'd like to see a clearer explanation for why each of these lines of research was followed.

We double-checked the journal's format requirements, which state that more than two levels of subheadings are not allowed.

Nevertheless, we attempted a "virtual" subdivision of the "Driving Mechanisms" section as suggested by the reviewer. However, we found that the subsections were intertwined and that some topics did not fit cleanly under a single "subheading". For example, S-SAV (Synoptic lower Stratosphere Arctic Vortex) is named the "tropopause polar vortex" in some studies (Lines 552-553). Does that still put it into the Stratosphere category? Also, the stratospheric and tropospheric processes and the near surface baroclinic instability work closely to drive changes in Arctic cyclones. After trying several unsatisfying subdivisions, we found it to be cleaner and more reader-friendly to move some of the previous "Results" material into the "Summary and Discussion" section, as described in the response to the following comment.

The 'Driving Mechanisms' section also seems to include quite a lot of what seems to me is better suited to either the introduction, (e.g. recent work on trends in SPV, the SAV vs. the SPV, strong summer cyclone development, lines 237-239, 244-245, 248-249, etc) or the discussion section (e.g. the more speculative parts on the warming hole's cause, the strong cyclones of 2012 and 2016,

QG theory, etc.) And in fact, many of these things are repeated in the 'Summary & Discussion', without any additional discussion.

We appreciate reviewer's suggestions and have implemented some restructuring of the text. The analysis and discussions in Lines 237-239 and 244-245 are the new results from this study, not a citation of other studies, so we would like to keep them in the original locations. Following the suggestion, we have moved various portions of the text in the revised manuscript to the section entitled "Summary and Discussion". For example, Lines 625-628 include discussions about the North Atlantic "warming hole". We have also removed the discussions about the strong cyclones of 2012 and 2016 out of the Results section and into Lines 41-42 of the Introduction and Lines 606-608 of "Summary and Discussion". The QG theory is cited in connection with the findings in Fig. 4f. We would like to retain it in the Results section because it provides an explanation for the GHT increase (Lines 485-487).

Some general but minor comments:
Incorrect use of past tense throughout the paper.
Line 135: defined -> define

Corrected.

Line 146 existed -> probably mean something more like 'is found'

Corrected.

Line 147 was highly consistent - > is highly consistent
Etc. The results should be spoken about in a present tense, even if the analysis is covering a past period, because the results are current…I hope that makes sense.

We have gone through the Results section and have revised the tense, except in a few instances where the present tense sounded awkward.

Vague wording in reference to physical processes/characteristics
Line 161 'leading large-scale atmospheric modes' -- which?

It is the first EOF mode, conventionally the AO. But with the poleward shift of the polar center of action, the zonally oriented AO is transitioned to a meridionally aligned ARP. We have added a sentence to make this clearer in Lines 298-299.

Line 143: what essential spatial structures?

It is the structure of cyclone frequency. We have added this information in Lines 258-259.

Some grammatical problems with sentence structure, such as missing commas, em-dash, etc. This issue seems to get somewhat more glaring through the manuscript, might just need a more thorough edit.

Line 30-31 missing commas or em-dash around ‘and long-term changes in’

Reworded sentence so that commas or dash not needed.

Line 136-137 same as above for ‘or being generated within’

Commas inserted.

Some more specific comments, mostly on the intro and first part of results where I didn’t have general comments:
Line 20-22, this last sentence seems to be slightly misrepresenting ideas about reduced baroclinicity and cyclones, as, to my understanding, this is meant to apply to mid-latitude cyclones, not necessarily Arctic ones.

The baroclinicity has increased not only within the Arctic, but also in large areas of the midlatitudes, seeing Fig. 4a. To make this clearer, we have modified the sentence in Lines 22-30 of the revised manuscript.

Lines 41-46: First sentence is about observations, which is not necessarily clear.

In response to the reviewer’s comments, we have revised the sentence to “… Results from these studies show sensitivities to …” in Line 64.

Line 53-54: Quite a vague sentence if the purpose is to make convincing argument of the usefulness of the manuscript.

Following the reviewer’s comment, we have revised the sentences in Lines 74-76 of the revised manuscript to make our argument clearer: “While possible data biases may cause uncertainties in the identification of the changes in Arctic cyclone activity, the robustness of the results can be enhanced if they are supported by solid physical mechanisms.”.

Line 59: improved compared to what?

The improved algorithm is described in section Methods. We added “(see Methods section)” in Line 81 to indicate this.

Line 73-74: if EnCAI is re-iterating the AO timeseries, why do we need EnCAI?

The EnCAI and the AO represent different physics. Considering that cyclones occur at daily basis and the AO is defined based on monthly SLP data, the coincidence or correlation between the EnCAI and the AO suggests that cyclone activity is a major contributor to the AO variability.

Line 86: is there a typo here? The error on the trend clearly makes the trend non-significant.

The second value "±0.93" indicates one standard deviation of variability relative to the trend. It is not the error of the trend. At the reviewer's comment, we added an explanation in the sentence in Lines 142-143 to make this clearer.

Lines 92-96: This is a very helpful way of putting the results, I like this section. Does this come from EnCAI? I realize EnCAI is defined in the methods but it would be helpful to understand where these metrics are coming from.

In the calculation of the EnCAI, we first calculated cyclone central SLP and geostrophic windspeed. These two parameters are used to calculate the first and second terms of EnCAI, on the right hand side of Eq. (1) in Methods.

Lines 104-114: I find this part a bit difficult to follow. It could use some rewriting for clarity. Is this all to say that the shape of the SLP distribution changes, while the cyclone count distribution shifts?

We have followed the suggestion and revised the text in Lines 207-208. We now state that the maximum value of cyclone count shifts from the weaker cyclone categories to the stronger cyclone categories with time in Fig. 2a and c.

Line 118-119: is this true in general, i.e., can it be cited, or is it a result of the paper?

This is true in general, but there is no reference to cite. We therefore removed the sentence "In general, strong cyclones tend to have longer duration".

Line 125-128: What is meant by less integrated nature of a metric, after saying something about a full spectrum of cyclone intensity?

Considering the reviewer's comment, we have revised the sentence in Lines 240-241 to "These studies also did not consider the different temporal variations of these metrics for different cyclone intensities". These metrics are defined as individual parameters of cyclone activity, such as cyclone count or duration. However, in our study, the description of the entire cyclone activity index integrates information covering the count, duration, and intensity.

Line 155-160: I don't really understand these sentences. Things like 'One maximum increase', 'shift of the high frequency', and then what the difference between cyclone occurrence and cyclone track …

Based on the reviewer's comment, we have revised the sentence in Lines 290-292 of the revised manuscript. "One maximum increase" is "One maximum value of the increase in the frequency of strong winter cyclones"; "an eastward shift of the high frequency" indicates the area with high frequency values shifted eastward from the earlier period to the later period.

Line 176-180: Can this be shown in a more explicit way using cyclone tracks from the tracking algorithm?

At the reviewer's suggestion, we have added a new Fig. S3 in the Supplementary Information, explicitly showing the cyclogenesis and cyclolysis locations and propagation trajectories.

Line 181-182: their propagation outside the Arctic, I assume?

Yes, the intensity outside the Arctic does not influence the definition of strong cyclone here.

Line 221-222: It's not that it can't explain it, it's that it's in contradiction.

Following the reviewer's comment, we have revised the sentence in Lines 400-401 to "By itself, the decreased EGR over these area does not support the overlying increased cyclone frequency". The increased cyclone frequency can come from other genesis locations steered by the large-scale atmospheric circulation dynamics as analyzed in the following paragraphs.

A few very minor comments
Line 32 decade -> decades

Corrected.

Line 141, elsewhere" well covers -> cover

Removed "well".

Line 228 At the same time -> I think this should start a new paragraph

As suggested, have split the paragraph.

15th Aug 23

Dear Professor Zhang,

Your manuscript titled "Intensification of Arctic Cyclone Activity" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Editorial Board Member
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REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

The authors have taken in to consideration all of the reviewer comments and made substantive changes to methods, figures, analyses, and text. The manuscript is improved and suitable for publication.

For figure S3, I still think that it may be useful to have a third column of figures which shows starting (genesis) and ending (lysis) locations in differing marker styles and colors without the tracks. This will help ease of viewing / interpretation and bolster the findings written in the text. Alternatively, perhaps calculating and listing an average of the genesis and lysis latitudes to compare and contrast the seasons and time frames and referencing them in the text will serve this purpose.

Reviewer #3 (Remarks to the Author):

The authors have done a thorough job of responding to and incorporating the many comments of all the reviewers and I am satisfied that the improved manuscript is fit to be published in its current form.

Responses to Reviewer #2

We thank Reviewer #2 for his/her suggestion. We have made revisions following the suggestion. Below is our response (the comments and suggestions in black and the responses in blue):

The authors have taken into consideration all of the reviewer comments and made substantive changes to methods, figures, analyses, and text. The manuscript is improved and suitable for publication.

For figure S3, I still think that it may be useful to have a third column of figures which shows starting (genesis) and ending (lysis) locations in differing marker styles and colors without the tracks. This will help ease of viewing / interpretation and bolster the findings written in the text. Alternatively, perhaps calculating and listing an average of the genesis and lysis latitudes to compare and contrast the seasons and time frames and referencing them in the text will serve this purpose.

We have accepted the reviewer's suggestion and added the cyclogenesis and cyclolysis locations (the circles in red and blue, respectively) in Supplementary Fig. S3. We have also added a brief description about this in Line 224-225 of the main text.