## Public respond to reviewers egusphere-2024-3404

The authors thank the editor and the reviewers for their constructive comments and suggestions. Please, see below our public responses.

## Response to the reviewers

## Reviewer 1

**Reviewer Comment 1.1** — This work introduces a new climatology of cyclone clustering using a novel detection and classification algorithm. The work and associated findings are interesting, and some new insights are provided, however I question the definition of clustering used here compared to some of the previous scientific literature. The authors use a fixed clustering threshold everywhere and largely identify clustering in the core of the storm track, where more cyclones are found. Whereas previous efforts have identified clustering as abnormal periods of high cyclone activity. Therefore, I would like to see more justification from the authors as to their methodological choices and explanation of novelties relative to prior studies, and how this work differs from a simple classification of the storm tracks. I recommend major revisions for this work, and detail my points, both major and minor, below.

**Reply**: Thank you for your constructive comments. It is correct that several previous definitions define cyclone clustering as abnormal periods of high cyclone activity, either through statistical measures or a running-mean occurrence threshold for cyclones in a geographically confined domain. However, at the same time, most of these efforts also refer back to the original work by Bjerknes and Solberg (1919) on cyclone families, which is based on the space-time proximity of cyclone tracks in a not a priori confined domain. Our detection algorithm tries to be as true as possible to this original definition of cyclone families, requiring a space-time proximity of cyclone tracks. Of course, the space-time proximity is also implicit in some of the other previously introduced cyclone cluster detection algorithms, though they either constrain the analysis to a pre-defined geographic location or use cyclone track densities for statistical assessments. The former is limited to a pre-defined domain, whereas the latter cannot be used to directly identify the cyclone tracks that are part of a specific cluster.
The motivation for our approach was to develop an algorithm that can be applied globally, while concomitantly retaining the information about the specific cyclone tracks are part of a respective cluster. With this information at hand, new questions about cyclone clustering can be addressed, such as: What are preferred regions for cyclone clustering? Do cyclones cluster more locally in time without necessarily moving much, or do they cluster in space and time, i.e., they move along similar paths? Are there differences in the characteristics of the different types of clusters? Are there structural differences in clustered and non-clustered cyclones? We believe that these are highly relevant questions to further characterise the occurrence of cyclone clusters and to assess dynamical differences between clustered and non-clustered cyclones. We made these points clearer in the revised introduction.

**Reviewer Comment 1.2** — My main concern surrounds the choice of the algorithm and justifications made by the authors. The method is to group cyclone travelling via a similar track or close in space/time, which their method does. However, this appears to by default just largely characterise the main storm tracks of the globe (Figs. 2,4). The standard view of clustering (e.g. Mailier et al., Pinto et al., Priestley et al.) characterises clustering as an abnormal rate of cyclone occurrences. Therefore, I would like to see more justification from the authors on their choice of thresholds for their detection method. If they are more strict, what events do they identify? Do signals become weaker as the frequency of events decreases, or are a different subset of events identified. Please clarify this and consider adding new results into the manuscript.

**Reply**: Thank you for your constructive comments on the justification of our method. It is correct that the cyclone clusters detected by our algorithm have a relatively high occurrence in the storm tracks region. This is not unexpected, as a higher occurrence of cyclones in general would also increase the likelihood of space-time proximity demanded by our algorithm. However, we also clearly show that only a fraction of maximum around 50-60 percent of cyclones are clustered in these storm track regions. This implies that not all cyclones must be characterised by a space-time proximity. Hence, despite the "normal-ness" of the storm track, the occurrence of cyclone clusters is not the norm in these regions either.

Furthermore, it is not quite correct to state that previous methods only characterised clustering as an abnormal rate of cyclone occurrences. The method of Priestley et al. (2016), for example, uses a threshold of having at least 4 cyclones in a 7-day running of cyclone occurrence to detect cyclone clusters, which gives similar results in the storm tracks region if applied globally (see top left panel Fig. 1), even when only using the more intense cyclones (see top right panel Fig. 1). When only retaining the most extreme cyclones, the signal shifts towards the end of the stormtrack (see lower panel Fig. 1). Hence, one could argue that the reason that cyclone clustering is abnormal in the Priestley et al. algorithm is due to the choice of a specific region as well as cyclone intensity. Also the Mailier et al. (2006) method does not directly characterise abnormal rates of cyclone occurrence, but rather the abnormal variability of cyclone occurrence.

Originally, Bjerknes and Solberg (2022) described cyclone families as the common evolution in the North-Atlantic storm track. However, even when adopting a cyclone clustering detection in their spirit, we find that only a fraction of cyclones actually clusters in the storm track region, with diminishing numbers towards the storm track exit, highlighting the abnormality of cyclone clustering in these regions. Most importantly, our diagnostic is motivated by enabling a dynamical perspective on cyclone clustering to investigate the mechanisms behind cyclone clustering, without a priori limiting the view to selected regions or the most intense cyclones. Hence, there was a need for a global detection scheme that applies a space-time proximity criteria to define cyclone clustering. It is this need that our detection algorithm replies to. We made this clearer in the revised version of the manuscript. We have also extended the discussion on the comparison with the other diagnostics (lines 164-169).

We also take the reviewer's point about a more complete discussion on the sensitivity of our algorithm to the chosen thresholds. We have assessed both the sensitivity in detecting the connected pairs of cyclones (first part of the algorithm) as well the the overlap thresholds (second part of the algorithm). First testing the sensitivity to the first part of the algorithm, we compared our 'standard' set of parameters (middle panel in top row in Figure 2) to a decrease (increase) in the distance threshold to 0.7 Rossby radius (1.3 Rossby radius), yielding a decrease (increase) to 10-14% (18-22%) in the main Atlantic storm tracks in winter (see Figure 2 top row left and right panels). Note that these frequencies are higher because we only detect connected pairs, so the overlap criterion has not to be satisfied, yet. Lowering and increasing the temporal proximity threshold yields similar results (lower row in Figure 2). Although these results show that the absolute numbers are sensitive to the chosen thresholds, we argue

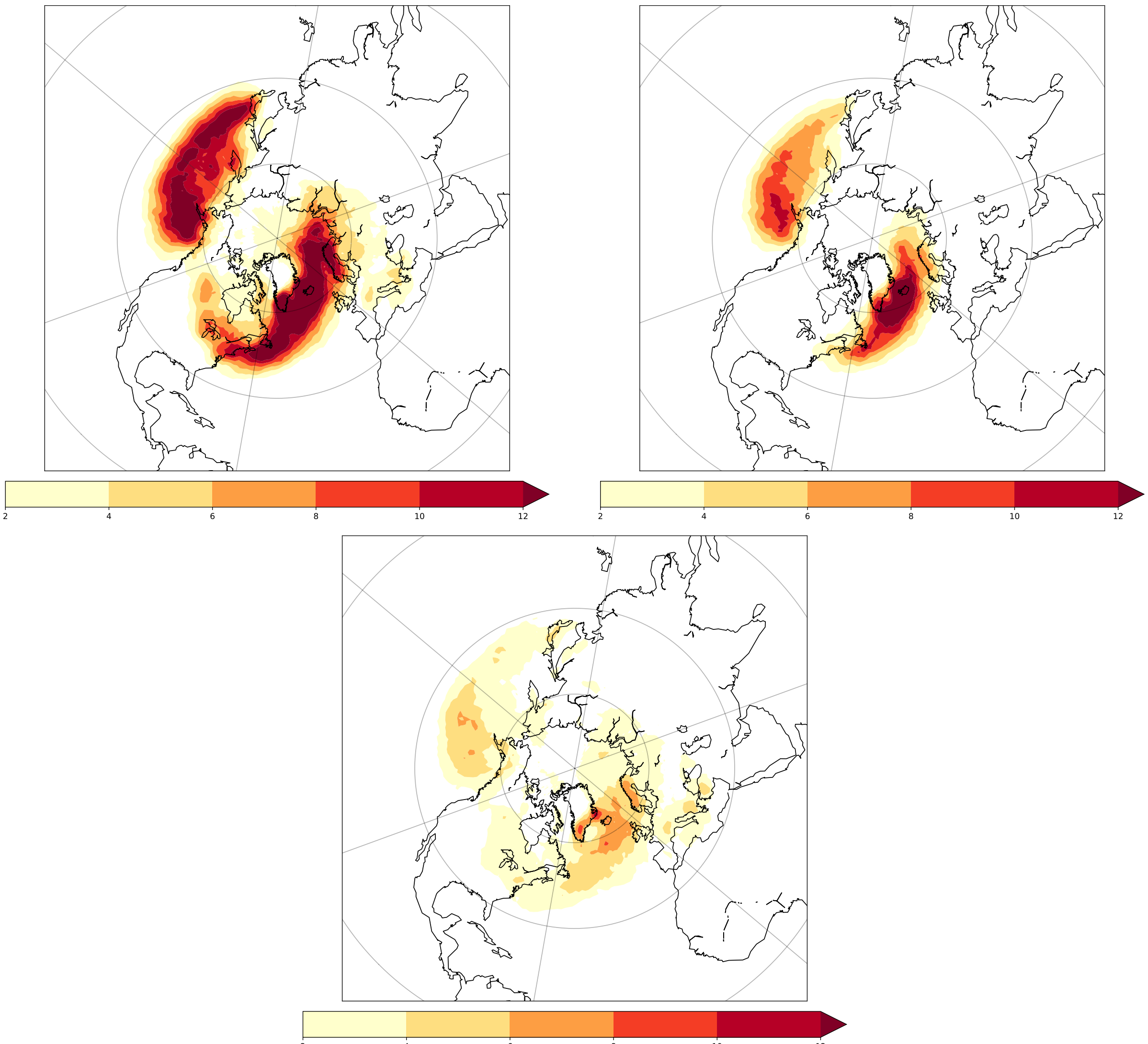


Figure 1: Cyclone clustering climatology for DJF in the Northern Hemisphere using a threshold of (upper left) at least 4 cyclones in a 7-day running mean in a local 700 km radius. Plotted in shading is the percentage of the time steps that this condition is satisfied. (upper right) The same as upper left, but only using intense cyclones (above a Laplacian of 2.0). (lower panel) The same as upper left, but for 3 cyclones in a 7-day running mean and only using the cyclones with an intensity above the local 5.0% pressure quantile during DJF.

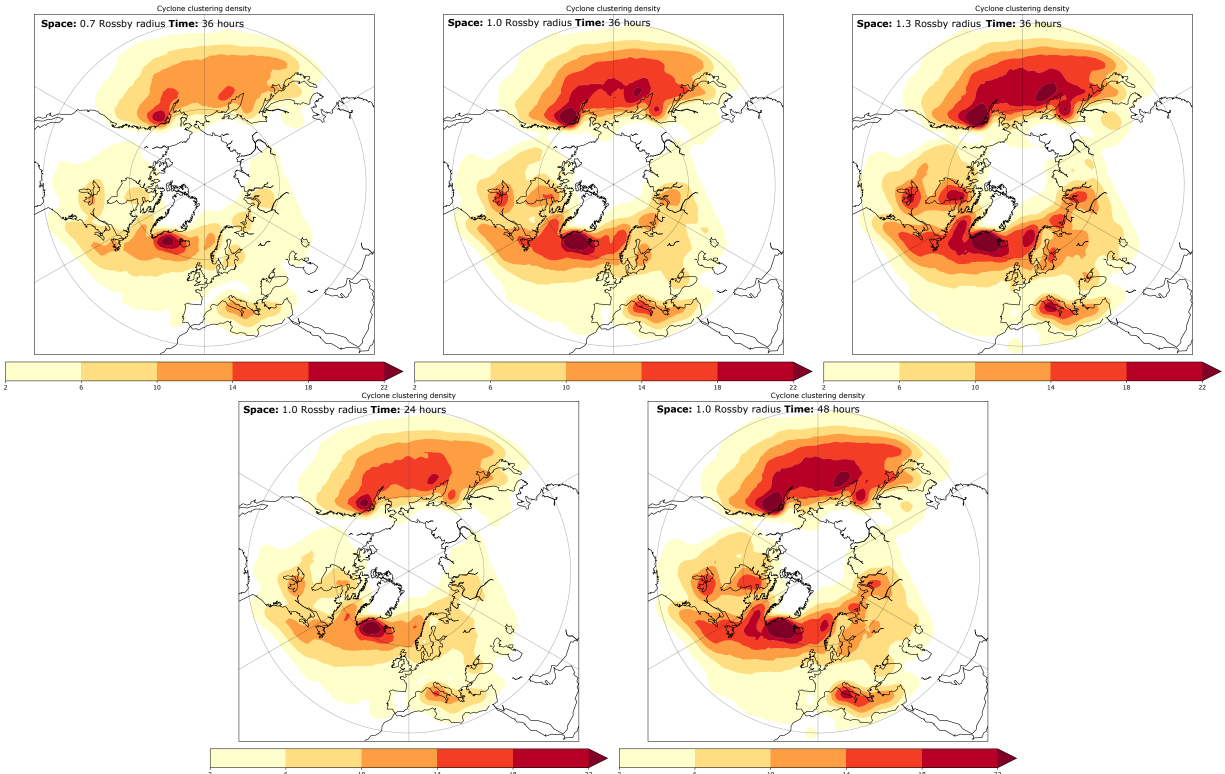


Figure 2: Clustering Density sensitivity (Proximity) of the frequency of connected pairs. Plotted is the frequency that a connected cyclone is found within a 1000 $km^2$ surrounding, independent if the overlap threshold is satisfied. Space-proximity thresholds used in each plot are indicated in the top left of each panel.

that both the space-proximity (Rossby radius of deformation) and the time-proximity (median recurrence of cyclones in the storm tracks) are physically well motivated.
Testing different overlap thresholds also yields a sensitivity, albeit less than for the proximity thresholds (see Figure 3). As discussed in the manuscript, these thresholds are less obvious to motivate physically. Physically one could argue that the overlap length should be longer than the Rossby radius of deformation, however how much longer is a somewhat subjective choice. As described above, we did not want to preselect intense clustering events, therefore we decided to choose overlap thresholds on the less restrictive side, as this allows for the possibility to differentiate and investigate different types of clusters, though this is beyond this study.
Our relatively high fractional clustering densities are partly due to including all clusters regardless of the length of the cluster. For example, when demanding a cluster length of least 4 cyclones, instead of 2, yields significantly reduced frequencies (Figure 4). Moreover, the frequencies at the storm track exit (up to 10%) are slightly lower compared to Priestley et al. (2017).
We included a more thorough discussion of the sensitivities in the revised version of the manuscript (lines 124-127)

**Reviewer Comment 1.3** — L17/18 – rephrase to "is often quantified to be associated with European weather extremes"

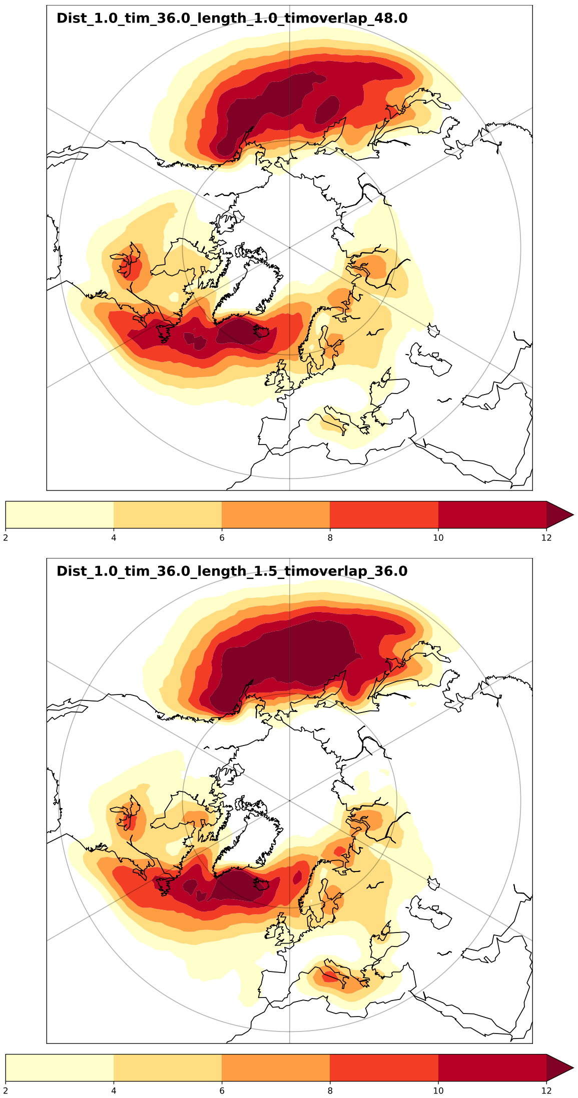


Figure 3: Clustering Density sensitivity (Overlap). Space-proximity and overlap thresholds used in each plot are indicated in the top left of each panel.

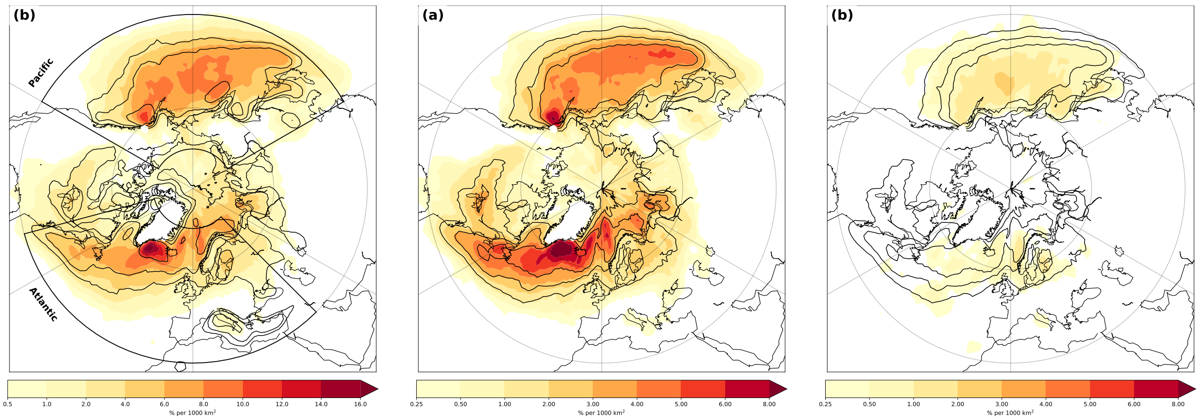


Figure 4: Clustering Density for cyclone clusters of at least length 4 (Nr of storms), for (left) all clustered cyclones (middle) clustered cyclones of the Bjerknes type and (right) clustered cyclones of the Stagnant type.

**Reply**: We find the formulation "quantified to be associated" a bit strange and prefer to keep our original formulation. In the past, cyclone clusters have been associated with extremes, so we cannot see what "quantified to be associated with" adds to this statement.

**Reviewer Comment 1.4** — L23 – not all references discussed in L22 are related to statistical quantification of clustering

**Reply**: You are right that that Priestley et al (2017) is not directly related to the dispersion statistic, we have adjusted the text accordingly.

**Reviewer Comment 1.5** — L27-29 – I find some of your discussion of overdispersive hard to follow here. The sentence "In contrast, a region is overdispersive when cyclones occur less regularly compared to a Poisson process." Is to me incorrect. Overdispersive is the deviation from a poisson process. Perhaps stated as when the rate of cyclones is variable compared to a Poisson process?

**Reply**: It is correct that over-dispersion is defined as the presence of greater variability (statistical dispersion) than expected based on a statistical model. However, under-dispersion also indicates deviation from a Poisson process. Our usage of the wording "regular" stems from the way Mailier et al used this term when referring to their results of over- and under-dispersion. Given the potential confusion about these statements, we clarified these points in the revised version of the manuscript by using the term "variable" as suggested by the reviewer.

**Reviewer Comment 1.6** — L32 "generally small and have large uncertainties"

**Reply**: We rephrased accordingly.

**Reviewer Comment 1.7** — L33 – how is it a problem to define clustering in a relative sense?

**Reply**: It is not a problem in itself, but using a relative measure has the disadvantages outlined in the sentence directly following in line 34. We will try to connect these sentences better to streamline the logic. In addition, we made it clearer that our aim is to be able to directly identify the specific cyclones that are part of a cluster, which allows for a more dynamical analysis and comparison of clustered cyclones.

**Reviewer Comment 1.8** — L41 – I would argue that these studies do not use an "impact-based definition", but instead the clustering method introduced by Pinto et al. (2014) classifies storms into clusters that then happen to cause impacts.

**Reply**: We referred to them as impact-based definitions, because these clustering detections pre-select only the most intense cyclones and a priori focus on a specific geographic region. However, we see the reviewer's point and rephrased accordingly.

**Reviewer Comment 1.9** — L48/49 – this is incorrect. The algorithm does not a priori assume clustering is due to secondary cyclogenesis. Just that secondary cyclogenesis often contributes to clustering.

**Reply**: Thank you for this clarification. Our argument was that the algorithm cannot detect clustered cyclones that form due to other mechanisms than secondary cyclogenesis. We have adjusted the text accordingly.

**Reviewer Comment 1.10** — L78 – why are you using ERA-Interim and not ERA5. Interim is now very outdated and limited in time.

**Reply**: Our analysis was already performed before a more complete ERA5 dataset was available. Preliminary testing indicates that our results are not sensitive to the choice of the reanalysis. We commented on that in the revised version of the manuscript.

**Reviewer Comment 1.11** — L89 – 'meters'

**Reply**: We corrected accordingly.

**Reviewer Comment 1.12** — L104 – I am confused as to your overlapping criteria. On L94 you mention a 36 hour threshold, which I believe is the time difference for a cyclone to be within 1.5 Rossby radius, then what is the 2 days relating to? Must they be within 36 hours/1.5RR for 2 days of each cyclones lifecycle? This whole section is quite hard to follow so I suggest editing to improve readability.

**Reply**: Thank you for pointing out this confusion. We agree that the description of the methodology would benefit from further clarification. We further clarified the steps in our clustering algorithm and have adjusted the text accordingly.

**Reviewer Comment 1.13** — L114 – 'yields all cyclone clusters' – what does this mean?

**Reply**: This means that we 'obtain all cyclone clusters'. We rephrased for better readability.

**Reviewer Comment 1.14** — L115-120 – for analysis I understand that you only take the part of the track that contributes to that part of a cluster in the analysis. Does this mean that in the track densities and intensity calculations, you only use fractional parts of the tracks? Please clarify this? If later on you search for the most intense storm in a cluster, does this mean you are not using all the information of each track to do this analysis?

**Reply**: Thank you for pointing out this potential point of confusion. We indeed only use the fractional parts of the tracks for the track density analysis. For cyclone intensity, however, we use the entire track. We see that this is slightly inconsistent and rectified this in the revised version of the manuscript by consistently showing track densities also using the full tracks (see revised Figures 2 to 5 in the main manuscript).

**Reviewer Comment 1.15** — L121 – stagnant clusters do not travel far, but in your schematic of figure 1 track 2 does travel a long way. If you are taking just the end part of the track at 'stagnant' you can't really say that it has not travelled far in my opinion.

**Reply**: Thank you for pointing this out. The reviewer's interpretation is correct. While the clustered part of the cyclone tracks is usually geographically very confined, individual tracks of these cyclones can cover a larger geographic region. We clarified this in the revised version of the manuscript.

**Reviewer Comment 1.16** — L129 – the statement on only using the connected parts of tracks in clustered cyclones, does this impact your findings?

**Reply**: As discussed above, when using the whole track, we obtain a qualitatively similar distribution of cyclone cluster density for the Northern Hemisphere winter season. The main difference is that the absolute density is higher when considering the entire track. Based also on the other comments of the reviewer, we decided to change figures 2-5 in the main manuscript using the whole track.

**Reviewer Comment 1.17** — L139 – how 'similar'? Please give some more quantitative information to this statement

**Reply**: We have quantified this statement in more detail, though do not believe that a more quantitative comparison is necessary beyond stating that they are qualitatively similar in the manuscript. Below the main results of Figure 7 and 8 from the main manuscript are shown, using the mean intensity instead of the maximum intensity of a cyclone in a cluster. These still show that clustered cyclones are stronger than non-clustered, moreover this is especially the case for Bjerknes type clusters.

**Reviewer Comment 1.18** — L143 and figure 2 – I don't understand the units here or how to interpret them. Is this the fraction of total cyclones, and then Fig. 2c,f is the fraction of clustered compared to clustered+solo? Please explain these units and the interpretation of the figure more clearly in the caption and the text.

**Reply**: Thank you for pointing out this potential confusion. The cyclone clustering density is defined in the same way as in Neu et al (2013), displaying the percentage of cyclone occurrence per time step and area of 1000 $km^2$. Fractions are indeed with respect to total cyclone counts. We have clarified this in the caption accordingly.

**Reviewer Comment 1.19** — L147-149 – these irregularities are surely the interesting part, as your method largely detects regular activity. Can you detect such irregularities using this method?

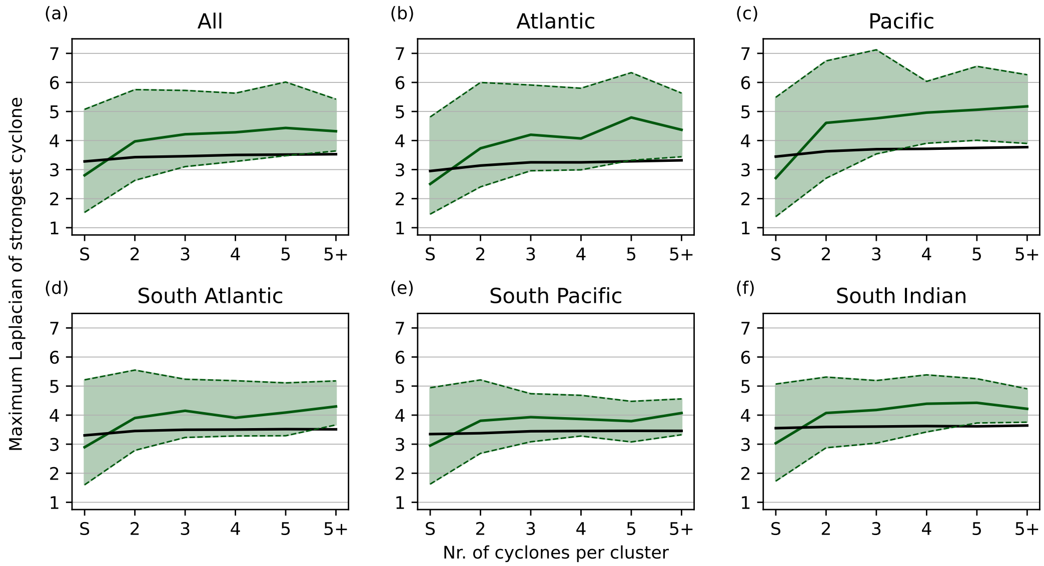


Figure 5: As figure 7 in the main manuscript, but using the mean intensity within a cluster instead of maximum intensity within a cluster. Cyclone intensity as function of cluster length, i.e. the number of storms in a cluster. The bin denoted with "S" indicates the strength of solo (non-clustered) cyclones. Green solid line indicates median values and variability between the 10 and 90 % quantiles is indicated by shading. The black line indicates expected value from randomly chosen clusters.

**Reply**: While we appreciate the interest of the reviewer in this irregular part alluded to in previous publications, we would argue that this interest is subjective. Here, we identify cyclone clusters based on a space-time proximity, without a priori demanding a statistical focus on irregularities. We detect cyclone clusters both in regions where they occur as irregularities as well as in regions where they are more common. Being able to distinguish these regions is valuable, as it allows for the further characterisation of cyclone clusters as well as focus on their dynamical differences.

**Reviewer Comment 1.20** — L151 I would argue from Figure 2a that solo cyclones are not just on the exit of the storm track, but mainly where cyclones just have more infrequent occurrence. Consider rephrasing.

**Reply**: The reviewer has a point. We have rephrased accordingly.

**Reviewer Comment 1.21** — L156 – you are comparing different things here. In Priestley et al. (2020) this percentage is of family cyclones in total, these do not have to contribute to clusters as in this analysis here.

**Reply**: Thank you for pointing this out. We still think the comparison is appropriate, as we also do not distinguish between primary and secondary cyclones and just clusters (or families) in total.

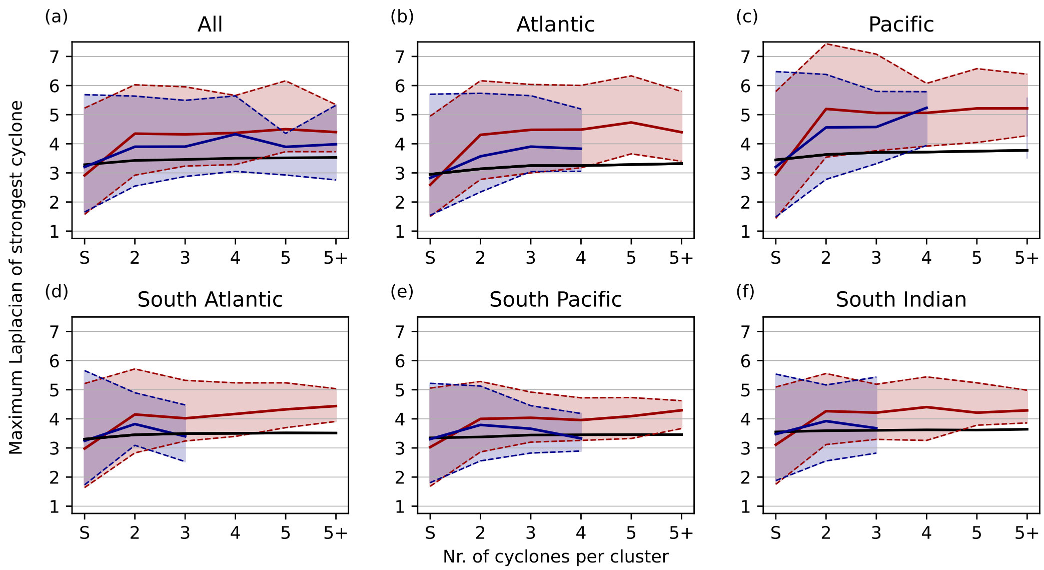


Figure 6: As Figure 5 but for Bjerknes type (red solid line and shading, for mean and 10 to 90 % quantiles) and stagnant type (blue shading). Black line indicates expected value from randomly chosen clusters.

**Reviewer Comment 1.22** — Figure 2 – I would suggest making the upper limit of your colourbar higher as it is hard to detect some of the maxima within your figures due to the colour saturation. This is especially the case in b/f.

**Reply**: Thank you for the suggestion. We will adjust the figure accordingly.

**Reviewer Comment 1.23** — L197/198 – some reference editing is needed here, should be in brackets.

**Reply**: Thank you. We corrected accordingly.

**Reviewer Comment 1.24** — L209/210 – would you not expect the strongest cyclone in a cluster to be stronger than random most of the time anyway, as you are preselecting a strong cyclone?

**Reply**: In contrast to previous cyclone cluster detection algorithms, we do not pre-select strong cyclones, i.e., we do not employ an intensity threshold for cyclones considered in our clustering algorithm. Hence, in contrast to previous studies, we can perform this distinction and present it in our manuscript.

**Reviewer Comment 1.25** — L225/226 – to clarify this, for this analysis you calculate the 90th percentile at all locations and this is how often a cyclone has intensity exceeding this value?

**Reply**: Yes, the interpretation of the reviewer is correct. We have clarified this in a revised version of the manuscript accordingly.

**Reviewer Comment 1.26** — Figure 8 – caption should be blue shading for your stagnant clusters

**Reply**: Thank you. We fixed this typo accordingly.

**Reviewer Comment 1.27** — Figure 10 – I don't fully understand what you are using to generate this information. Is the length of overlap for how long the cyclones overlap or from the point of genesis? The same with Time of overlap, is this for two connected storms, or just the length of the cluster? Please make the text associated with this figure clearer as to how this is interpreted and generated.

**Reply**: Thank you for pointing out this confusion. These overlaps are defined in the methods section, though the manuscript obviously would benefit from referring the reader back to the methods section when discussing these results to avoid confusion. We have clarified accordingly.

**Reviewer Comment 1.28** — Figure 11 – are these results the same if you use something like MSLP or cyclone size? Theories on clusters are that the final storm is more intense and larger and so would be good to document alongside this result.

**Reply**: We did in fact perform this analysis also using MSLP and the results are very similar (see Figure 7. We now refer to these results in the revised version of the manuscript and have added the figure to the supplement. Regarding the size of cyclones. There is no generally accepted definition of the size of a cyclone, as it could be based on vorticity, MSLP, or other intensity or structure measures. We hence refrain from including such a discussion. We do not find evidence that the final storm is more intense.

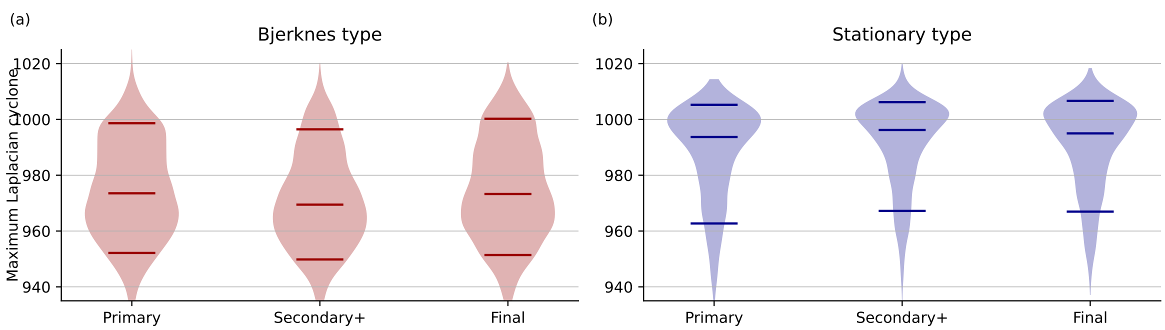


Figure 7: As Figure 11 in the main manuscript, but using the pressure instead of the (normalised) Laplacian as an intensity measure. Violin plots for the minimum pressure of cyclones for all clusters (left), Bjerknes type (middle), and stagnant type (right) for the first (Primary), all secondary, and final cyclones in each cluster. Medians and 10% and 90% quantiles are indicated by horizontal lines.

# Reviewer 2

**Reviewer Comment 2.1** — The aim of this paper is to provide a global climatology of cyclone clustering based on Reanalysis data. With this aim, the authors apply a cyclone cluster detection approach to cyclone track data derived with the Murray and Simmonds tracking method. I am a bit puzzled about the results, because the main result is simply the well-known storm tracks, which can be derived easily from both eulerian and lagrangian methods (e.g. Hoskins and Hodges, 2002). Following the definition of Mailier et al (2006, also their Fig. 6), clustering is a synonym of overdispersion, meaning that there are periods of time where the number of occurrences within a defined period of time significantly differs from the expected value. Indeed, the areas where cyclone clustering is identified over the North Atlantic Ocean correspond to the areas on the flanks and downstream of the main storm track, and not the main storm track itself (see review by Dacre and Pinto, 2020, in particular their Figure 2). Indeed, over the core of main storm track, cyclone occurrences tends to be random, and at the its entrance underdispersed (see also their Fig. 2). Thus, the results included in this manuscript are clearly in disagreement with many studies published on this topic. So, while I understand the idea of the authors, the methodology simply identifies areas where "cyclones tend to follow each other", which are per definition the storm tracks themselves. I would also recommend to consider a minimum intensity and lifetime for the cyclones (lines 85ff), as otherwise the whole picture may be dominated by non-synoptic-relevant systems. Given the very fundamental question marks regarding the methodology, which I do not think it is adequate to answer the research questions and the objectives posed, I have refrained to make detailed comments on the text. Based on the above, I unfortunately cannot recommend this manuscript for publication. However, I do agree that a strongly re-worked manuscript would be an important contribution to this field of work, given the lack of studies outside of the North Atlantic Basin on cyclone clustering (see research gaps in Dacre and Pinto, 2020).

Dacre and Pinto, 2020, https://doi.org/10.1038/s41612-020-00152-9
Hoskins and Hodges, 2002, https://doi.org/10.1175/1520-0469(2002)059¡1041:NPOTNH¿2.0.CO;2
Mailier et al. 2006, https://doi.org/10.1175/MWR3160.1

**Reply**: Thank you for your detailed critical feedback. Below you find our response to the specific issues that you raised.

"the main result is simply the well-known storm tracks, which can be derived easily from both eulerian and lagrangian methods"

While it is correct that the stormtrack can be determined from both Eulerian and Lagrangian methods, our algorithm does not detect the stormtrack in its entirety, because we focus on clustered cyclones only, thereby only detecting a fraction of all cyclones within the stormtrack region. Furthermore, it is clearly evident from our findings that the fraction of clustered cyclones varies within the stormtrack. If our algorithm would simply detect the storm track, the fraction of clustered cyclones should just be the same everywhere. Given that this is not so, we disagree with the statement of the reviewer.

"definition of Mailier et al (2006, also their Fig. 6), clustering is a synonym of overdispersion" ... "over the core of main storm track, cyclone occurrences tends to be random, and at the its entrance underdispersed (see also their Fig. 2). Thus, the results included in this manuscript are clearly in disagreement with many studies published on this topic."

The usage of a statistical measure to classify cyclone clusters has its own disadvantages, some of them outlined in the manuscript, such as that one cannot identify the specific cyclones that are part of a cluster, though we will refrain from going into the detailed disadvantages of the statistical method and rather focus on the benefits of our method. Our results are not necessarily in disagreement with Mailier et al (2006), especially given that Mailier et al. (2006) motivated their research by referring to the cyclone families introduce by Bjerknes and Solberg (1922).

Moreover, there are other definitions of cyclone clustering. The method of Priestley et al. (2016), for example, uses a threshold of having at least 4 cyclones in a 7-day running of cyclone occurrence to detect cyclone clusters, which gives similar results in the storm tracks region if applied globally (see top left panel Fig. 8), even when only using the more intense cyclones (see top right panel Fig. 8). When only retaining the most extreme cyclones, the signal shifts towards the end of the stormtrack (see lower panel Fig. 8).

"the methodology simply identifies areas where "cyclones tend to follow each other", which are per definition the storm tracks themselves."

This is not correct and immediately obvious from Figure 2 in our paper, which shows fractions of clustered storms. See also comment on the first statement further above. While most clusters occur in the storm track regions, the very definition of the storm track is not that all cyclones have to be equally clustered in space-time proximity. The common Lagrangian definition of the storm track relies purely on the occurrence of cyclones, without an a priori limitation on their space-time priximity when they are detected.

"I would also recommend to consider a minimum intensity and lifetime for the cyclones (lines 85ff), as otherwise the whole picture may be dominated by non-synoptic-relevant systems."

Our cyclone detection and tracking by design disregards non-synoptic systems. Otherwise, more or less all commonly accepted cyclone detection tracking schemes would suffer from such a deficiency. These commonly used schemes already have some a priori thresholds on some kind of measure for cyclone intensity. However, this limit is usually set such that one can also detect the genesis stage of cyclones, not just the most intense part of a cyclone track. Hence, the reviewer is mistaken about non-synoptic-relevant systems. Regarding the pre-selecting of only the more and most intense cyclones. This would significantly skew the results and would not allow for a more comprehensive comparison between clustered and non-clustered cyclones in general. In addition, it could lead to a misleading interpretation given the skewness of the distribution. We do already set a minimum lifetime of 24 hours, as we write in lines 84-85.

Overall, we thus cannot see that the criticism by the reviewer can be upheld. Hence, we cannot see the issues raised as fundamental and we believe that they were already clear in the original version of the manuscript, though certain aspects raised by this reviewer have also been further clarified due to changes in the manuscript in response to the other reviewer.

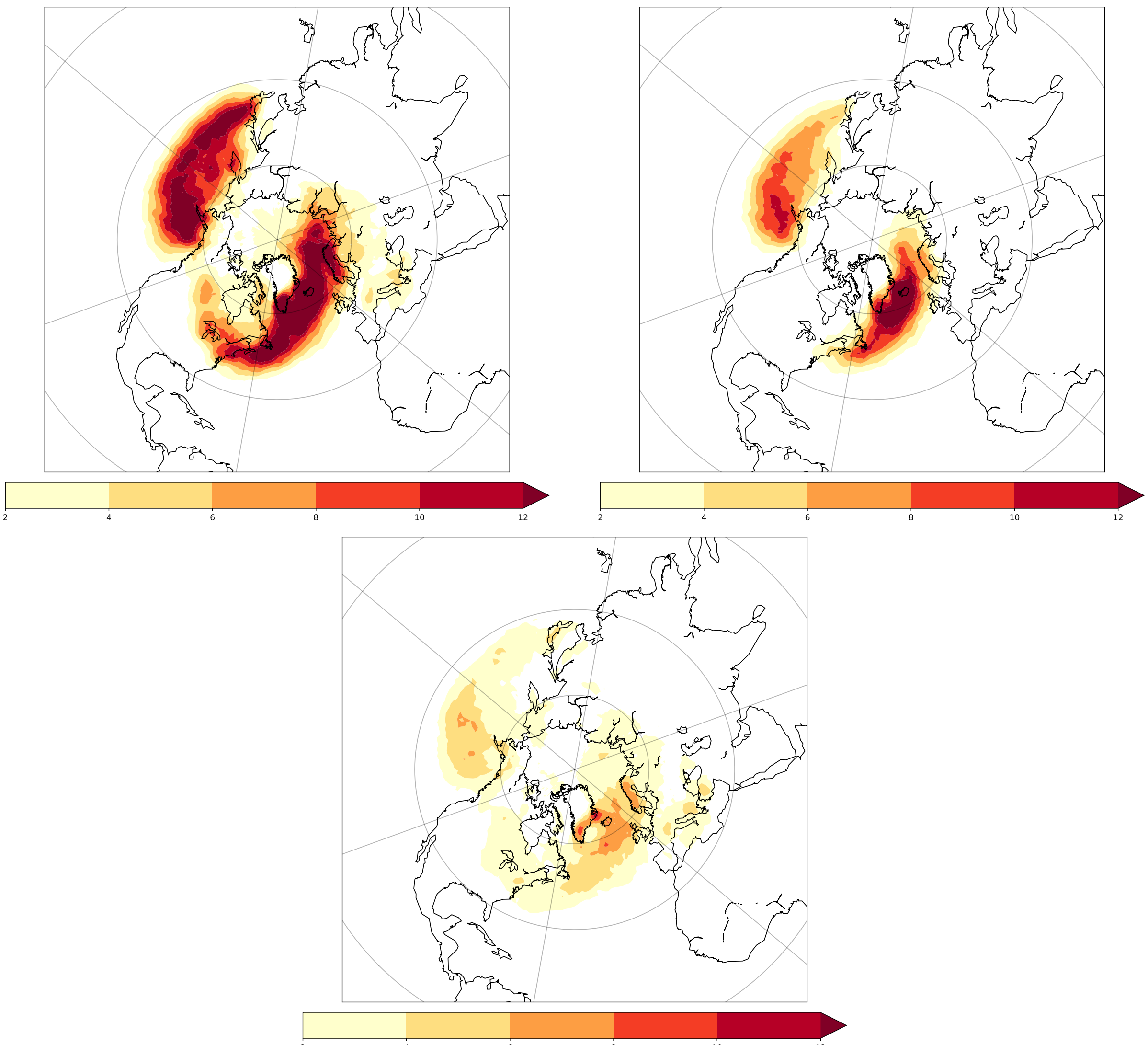


Figure 8: Cyclone clustering climatology for DJF in the Northern Hemisphere using a threshold of (upper left) at least 4 cyclones in a 7-day running mean in a local 700 km radius. Plotted in shading is the percentage of the time steps that this condition is satisfied. (upper right) The same as upper left, but only using intense cyclones (above a Laplacian of 2.0). (lower panel) The same as upper left, but for 3 cyclones in a 7-day running mean and only using the cyclones with an intensity above the local 5.0% pressure DJF quantile.