Public response to second review egusphere-2024-3404

We thank the co-editor and the two reviewers for their constructive comments and suggestions. Please see our public responses below.

Response to the Co-Editor

Co-Editor Comment — Your manuscript has been scrutinized by three expert reviewers in the subject of cyclone clustering. (Unfortunately, one of the reviewers from the first round could not follow up in the second round, and thus I brought in a new reviewer in the second round.) All reviewers agree that your manuscript contains interesting results and valuable information. All three reviewers further agree, however, that the definition of cyclone clustering used in your manuscript is not consistent with a definition that the community has established over the previous couple of decades. Dacre and Pinot (2020) provide a review that seems to be well received by the community (based on citation metrics). All reviewers agree that your definition is consistent with the concept of cyclone families. I trust the expert reviewers that the distinction between cyclone clustering and cyclone families is important, and I agree with the reviewers that blurring an important distinction leads to confusion. Referee #3 provides a clear example of a scenario, in which cyclone families (and your perspective) are not congruent with clustering: "By requiring the tracks to follow similar paths, it excludes some of the serial clustering that would occur from cyclones coming from different directions (which seems plausible if a cyclone developed on a trailing cold front)."

Reply: Thank you for your considerate message and for overseeing the review process. We are glad to hear that our manuscript contains interesting results worth publishing and understand that some concerns remain, which we tried to address in this round of revisions. As also pointed out to the reviewers, our detections are not consistent with the definition of cyclone families by Bjerknes and Solberg (1922), as the latter specifically require that these cyclones form on the same front, like being connected by an umbilical cord. Given that we do not demand such a criteria, the proposed nomenclature would be misleading and we thus argue to refrain from such a renaming of our detected clusters.

We would also like to point out that previous papers that the reviewers refer to, e.g., Pinto et al. (2014) and Mailier et al. (2006), directly refer to the concept of cyclone families by Bjerknes and Solberg (1922). In fact, Mailier et al. (2006) even included the figure from Bjerknes and Solberg (1922) with the cyclone families in their paper. Hence, we cast significant doubt that these authors really meant something different compared to cyclone families when they introduced their terminology of cyclone clusters. However, as pointed out above and also in Dacre and Pinto (2020), cyclone families can probably be regarded as a subclass of cyclone clusters. Furthermore, Dacre and Pinto (2020) explicitly discuss different types of clustered cyclones. Nevertheless, as our algorithm is not specifically restricted to detect only these type of cyclone families, we argue that naming our detected clusters "cyclone families" is misleading and incorrect. Instead, we propose, motivated by Pinto et al. (2014) coining their detections "serial clustering of extratropical cyclones", to name our detections "spatio-temporal clustering of extratropical cyclones", given the space-time constraints on our detection algorithm.

Regarding your last point, our detection algorithm only demands a space-time proximity and we subsequently stratify our detected clusters into Bjerknes and stagnant type (see also response to reviewer

3). Hence, it is a priori not given that we exclude "serial clusters that would occur from cyclones coming from different directions". However, of course, given that our algorithm is different, we will not necessarily detect the same clusters. Vice versus, the other algorithms might miss spatio-temporally clustered cyclones detected by our scheme. In principle, this is neither an advantage or disadvantage, as the algorithms pursue different objectives.

Co-Editor Comment — Both reviewers of the second round suggest that the way forward for this manuscript is to refrain from the claim to identify cyclone clustering but rather to use the term cyclone families. I follow the clear (and strong) statement of reviewer #2 in that "the methodology the authors have developed is good in identifying exactly that, namely "cyclone families". So I would like to strongly suggest that the authors change the title of the manuscript and generally the wording used everywhere to "cyclone families". This would be a much more precise description of what the authors are actually analyzing with their methodology and would avoid confusion with many papers published about cyclone clustering over the last twenty years."

Reply: See our response to the comment above. We strongly argue against using this misleading nomenclature for our detections and instead introduced the term "spatio-temporal clustering of extratropical cyclones".

Co-Editor Comment — Taking a "fresh look" at "established" science problems is an integral part of scientific rigor and advancement. Alternative approaches should not be dismissed lightly. The reviews, however, are convincing in arguing that the manuscript does not take a fresh look at cyclone clustering, clearly put by reviewer #3: "… it is important here to make it clear that this new study is not offering an algorithm that is better than others, rather it is detecting a specific subset of cyclones that are within the extratropical stormtracks. I think it should not be compared so closely to the previous studies as it is specifically choosing something different to look at." (my emphasis) To be accepted for publication, I would like the authors to follow the consensus recommendations of the reviewers and change terminology. The introduction and the conclusion sections are suitable places to discuss the relation between cyclone families and clustering, where such a discussion would need to acknowledge the definition of clustering as accepted by the community, e.g., as reviewed by Dacre and Pinot (2020).

Reply: We appreciate the sentiments of the co-editor, though, as pointed out above, we argue to refrain from a simple renaming with an incorrect term. We would also argue that we do take a fresh look at spatio-temporal clustering of extratropical cyclones and partially agree with reviewer 3 that our algorithm is not necessarily better, but that it provides a different approach, which can be applied globally as it does not require a predefined specific region, and unlike statistical approaches can be used to directly identify corresponding cyclone tracks. Based on the suggestions of the co-editor and the reviewers, we revised the manuscript accordingly, especially the introduction, methodology, and conclusion sections, to highlight the differences and context of our study. We hope that our revisions warrant further assessment of our study and hope that we were able to satisfy the co-editor and the reviewers.

Reviewer 2

Reviewer Comment 2.1 — The authors have revised their manuscript but have largely disregarded my comments regarding the approach and the choice of nomenclature, which is unfortunate. Thus, my main concerns remain (see my initial comments below), and in its current form I cannot recommend this article to be accepted for publication, but rather a rejection. Primarily, the nomenclature used and the discussion with the available literature are not adequate.

Nevertheless, I would like to suggest to the editor and the authors a way forward, so that the paper can be revised and hopefully published. I do think the manuscript contains valuable material, but it should be presented without contradicting the current nomenclature and available literature.

Reply: Thank you for carefully assessing our manuscript and for suggesting a way forward to get this manuscript published. Below, we provide a detailed response to your concerns.

Reviewer Comment 2.2 — There is a 2020' review paper on the topic of cyclone clustering (Dacre and Pinto, 2020) that I have suggested in my previous review and the authors have chosen not to consider it. I would like to insist that the authors consider the wide material included in this review, regarding the different types and metrics of clustering, followed by climatologies, considerations on different time scales, associated large-scale dynamics and a gap analysis (see also my comment on this in the initial review). I am sure the review article will be helpful to rework the introduction and rephrase aims of this study in terms of the available literature. Also the differences between "cyclone families" and "cyclone clustering" are discussed in this review paper.

Reply: We are aware of the review by Dacre and Pinto (2020) and apologise for the oversight to not cite it in our original submission. The methods summarised in the review, were, however, mostly covered in our original manuscript. To further address the reviewer's concern, we reworked the introduction, including citations of the review, and a clearer outline of the aim of our work in relation to existing definitions of cyclone clustering on different timescales. If the reviewer feels that certain aspects are still missing, we would appreciate a more direct pointer to these neglects.

Reviewer Comment 2.3 — The seminal work by Bjerkness and colleagues never mentions the word "cyclone clustering", but rather the concept of "cyclone families". I agree that the methodology the authors have developed is good in identifying exactly that, namely "cyclone families". So I would like to strongly suggest that the authors change the title of the manuscript and generally the wording used everywhere to "cyclone families". This would be a much more precise description of what the authors are actually analyzing with their methodology and would avoid confusion with many papers published about cyclone clustering over the last twenty years.

Reply: While we understand the reviewer's sentiment, such a nomenclature would neither be congruent with Bjerknes and Solberg (1922) nor with the aforementioned review paper by Dacre and Pinto (2020). For example, from Bjerknes and Solberg (1922) first sentence in the section on cyclone families: "In a series of cyclones formed on one and the same polar front, each cyclone usually follows a track lying south of that of the preceding cyclone." Hence, they clearly define cyclone families as a type of secondary cyclogenesis along the same frontal zone. We do not specifically demand such a condition in our detection and hence refer to the subcategory of out detection that is closest to that definition as Bjerknes type and not cyclone families.

Furthermore, our stagnant type would certainly be in conflict with the original definition of Bjerknes and Solberg (1922). Hence, cyclone families is not a correct and suitable nomenclature in the context of our detection. Regarding Dacre and Pinto (2020), they point out that there are different reasons why cyclones cluster in time, one of them being the original definition of cyclone families, though there are also others. According to that definition, cyclone families would be a sub-category of clustered cyclones. We agree with this sentiment and hence introduced our two different types in our detection. However, just referring to all of our detections as cyclone families would certainly be incorrect and we can thus not follow the reviewer's suggestion.

Given the strong feelings of the reviewer about the nomenclature, we however propose an alternative name for our detections as "spatio-temporal clustering of extratropical cyclones" and changed the name in the title and several locations in the manuscript accordingly. This nomenclature is motivated by Pinto et al. (2014), who refer to "serial clustering of extratropical cyclones" with respect to their detections, but our nomenclature also clearly distinguishes our detection from their's.

Reviewer Comment 2.4 — Please note that Pinto et al. (2014) have not only considered secondary cyclogenesis in the trailing cold fronts (upstream cyclone development) as a possible reason for cyclone clustering, but rather also of the role of the eddy driven jet stream, Rossby wave-breaking, and in particular the possibility of downstream cyclone developments (their figure 8). Moreover, clustering can also happen by chance. This is also discussed in the review paper from Dacre and Pinto, 2020). Thus, the paragraph starting in line 45 should be updated.

Reply: In relation to Pinto et al. (2014), our paragraph states that "clustering is often associated with secondary cyclogenesis", which implies that there are also other mechanisms, not only secondary cyclogenesis. However, to further clarify this point, we now also mention the potential role of the eddy driven jet, Rossby wave breaking, and the possibility of downstream development.

Reviewer Comment 2.5 — Comments from the first formal review: 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.

Reply: We thank the reviewer for reiterating these concerns. Regarding "the main result is simply the well-known storm tracks": This is not correct, as we sub-select cyclones based on our methodology.

Hence, our results do not "simply" reproduce previous Eulerian or Lagrangian measures of the storm track. In fact, it is a result of our methodology that a significant fraction of cyclones in the storm track are in fact spatio-temporally clustered. In addition, our methodology also highlights that it is mainly the Bjerknes type of clustered cyclones that occur in the storm tracks region, not the stagnant type. Moreover, with our algorithm we are able to distinguish between different groups/clusters of subsequent cyclones. These kind of distinctions are not possible without our detection and categorisation.

Regarding the definition of Mailier et al. (2006) on "overdispersion" and that the "period of time significantly differs from the expected value", one should be allowed to ask what kind of clustered cyclones will be missed with such a definition. Or in other words, what is the main interest? Detecting something unusual, or detecting the occurrence of cyclones in a certain spatio-temporal vicinity? Thought experiment: Let there be a region where cyclones cluster continuously according to the definition of Pinto et al. (2014). Such a region would not be identified as unusual by Mailier et al. (2006) and no clusters would thus be detected. Hence, these two definitions would be in conflict in identifying preferred regions for cyclone clustering.

We would also like to draw the reviewer's attention to Figure 1 in the supplement provided in the first round of reviews, where we adopted the Pinto et al. (2014) method to the entire Northern Hemisphere. It is clearly evident that when applying the Pinto et al. (2014) method at locations in the storm tracks, one will also pick up a cyclone cluster signal there, whereas Mailier et al. (2006) do not pick up clustered cyclones in this region according to their definition (e.g., their figure 5). This also indicates that even the already existing algorithms detecting cyclone clustering are quite different, both in their definition as well as in the regions where cyclone clustering is detected.

We are not sure if the statement that cyclone occurrence in storm tracks is "random" is unambiguous to the reader, even though we understand what the reviewer means to imply in the given context.

Based on the arguments presented above, we disagree that our results "are clearly in disagreement with many studies published on this topic". In fact, as indicated above, our results resemble a hemispheric implementation of the Pinto et al. (2014) method. As pointed out, our method does not "simply" define the storm track, also because storm tracks are not defined as an area "where cyclones tend to follow each other", but as areas with regular storm occurrence, with no demand on their propagation relative to each other.

Regarding the suggestion of introducing an intensity threshold to avoid a potential dominance "by non-synoptic-relevant systems", we would like to ask the reviewer to further clarify the definition of "non-synoptic-relevant-systems", as our cyclone detection scheme already ensures that all detected cyclones are synoptically developing systems, with restrictions on a minimum lifetime and minimum strength (implicitly given through the threshold for the Laplacian of MSLP). In fact, it is only by not limiting cyclones to a certain higher intensity threshold that one can clearly distinguish the different characteristics of cyclones, especially in the light of clustered and non-clustered cyclones.

Overall, we thus do not agree with the sentiments of the reviewer, but acknowledge that certain aspects of our motivation and implication of our results could have been presented in a clearer form. We thus reworked certain aspects of our manuscript and have hopefully adequately addressed these issues.

Reviewer Comment 2.6 — 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).

Reply: We hope that the reworked version of our manuscript is worthy the reviewer's time, as we would appreciate more detailed comments on the overall manuscript.

Reviewer 3

Reviewer Comment 3.1 — In this study the authors have developed a novel algorithm to detect cyclones that fall into a category that they refer to as cyclone families. In other words, cyclones that occur close to each other in space-time. The paper is mostly well-written and there are some interesting results. However, I think it needs to be made clearer that this paper is not representing a new and better way to identify cyclone clustering, as the authors seem to suggest. Rather it is a method to identify a specific subset of cyclones – those that follow each other or have similar tracks. The paper mentions other studies that have considered cyclone clustering from a statistical point of view, or a regional point of view. Both of these previous types of studies consider that clustering is the grouping together of cyclones in time and space that is unusual given the mean frequency of cyclones. The reason for this is the potential impacts of these clustering events. In the present study, it is not clear to me whether the large fraction of clustered cyclones in the main stormtracks has a similar impact.

So, I recommend some clarification that this algorithm is specifically looking for a subset of cyclones that fall into the "cyclone families" group (although since it does not consider secondary cyclogenesis specifically, it is a somewhat expanded group).

Reply: We thank the reviewer for these useful comments. We explicitly did not pursue an impact-based definition of cyclone clustering, but wanted to rather have a more dynamic meteorology approach to this phenomenon. Based on the reviewer's comments, we tried to further clarify this motivation and its context to previous work on cyclone clustering in our introduction. We also agree with the reviewer that our detected clusters are not strictly cyclone families, as we do not demand that they form on the same frontal zone, as introduced by Bjerknes and Solberg (1922).

Reviewer Comment 3.2 — Perhaps the title could be changed to say "cyclone families" instead of "cyclone clustering".

Reply: The other reviewer made a similar suggestion, though, as pointed out above, our detections do not strictly classify as cyclone families accordingly to Bjerknes and Solberg (1922). Hence, motivated by Pinto et al. (2014), who refer to their detections as "serial clustering of extratropical cyclones", we propose a new nomenclature motivated by our detection algorithm as "spatio-temporal clustering of extratropical cyclones". This naming also clearly distinguishes our detection from previous detection algorithms.

Reviewer Comment 3.3 — Introduction: the review of the literature as it is presented seems to suggest that the current study is less restrictive than the dispersion diagnostic or the regional studies. However, I would argue that this new method is quite restrictive in a different way. By requiring the tracks to follow similar paths, it excludes some of the serial clustering that would occur from cyclones coming from different directions (which seems plausible if a cyclone developed on a trailing cold front). Perhaps the introduction could be slightly reframed to discuss what has gone before and to highlight that this is a new and different way of looking at cyclone families, rather than a better way to identify clustering.

Reply: Thank for your raising these concerns. We tried to put our detection more into context in our revised introduction and hope to have addressed these issues. We agree that our method has its own restrictions, though in a very different way compared to previous detections. It is, however, not correct that our detection would a priori exclude some serial clustering that might occur in a specific region, as these clusters might be picked up by our stagnant type, which does not demand that cyclones travel along a similar path. We tried to clarify these aspects further in our revised version of the manuscript.

Reviewer Comment 3.4 — Lines 52-57: This paper does not seem particularly relevant to the current study. It is only considering that tracks might have similar characteristics, but does not consider any time constraint.

Reply: The reviewer has a point, but given that this study uses the tracks to identify clusters with similar tracks, we feel that it is relevant to our approach, even though it does not specifically have a time constraint. Hence, we would like to keep this discussion.

Reviewer Comment 3.5 — Line 75: "cyclone mechanism" should perhaps be "cyclone development mechanism". However, I don't think the paper does look at this, so mentioning the latent heating here is misleading.

Reply: We changed the manuscript to "cyclone development mechanism". Thanks for pointing this out. Regarding the latter point, we still think it is relevant for the introduction, as one of our main motivations to develop our algorithm was to enable dynamical investigations into different evolution characteristics for individual clusters. We tried to further clarify this aspect in our introduction.

Reviewer Comment 3.6 — Figure 1: The panels need labels as given in the caption, and the caption contains unnecessary "in" on the second line.

Reply: We added the labels, and we removed the unnecessary "in" from the caption.

Reviewer Comment 3.7 — Line 98: "Conceptionally" should be "conceptually".

Reply: Changed in the revised manuscript.

Reviewer Comment 3.8 — Line 145: Wherever the "Laplacian" is mentioned, it should really be the Laplacian of mean sea level pressure. This is important for the figures and captions also.

Reply: Thank you for pointing this out. We made according changes in the manuscript, and revised figures 7-11.

Reviewer Comment 3.9 — Line 168: The cyclone clustering is not abnormal. The frequency of clustering is highest in the end of the stormtracks. Please change the wording here, and check the bibliography/referencing in this line.

Reply: We fixed both the wording and the reference.

Reviewer Comment 3.10 — Line 194: I think the figures of 35-40% quoted here are incorrect, looking at Figure 2f, where the values look much higher than this.

Reply: Thank you for pointing out this discrepancy. We corrected the values to be in accordance with the figure. Please not that the original numbers referred to the fraction calculated only using the connected part of the track (red dots in Figure 1).

Reviewer Comment 3.11 — Line 226: I find the term "length of cluster" to be confusing since it actually refers to the number of tracks within a cluster. Surely size of cluster would be more appropriate?

Reply: We agree with this comment and have changed the manuscript accordingly.

Reviewer Comment 3.12 — Line 247: "stronger" should be "larger".

Reply: Fixed.

Reviewer Comment 3.13 — Line 258: "between" should be "of".

Reply: Fixed.

Reviewer Comment 3.14 — Figure 7: The figures need units of the Laplacian of MSLP.

Reply: We have added the units of the Laplacian of the mean sea level pressure to the figure labels for both Figure 7 and 8.

Reviewer Comment 3.15 — Figure 9: The labels on these panels are too small to read on an A4 page.

Reply: We have increased the label sizes of the panels.

Reviewer Comment 3.16 — Line 267: Check the parentheses for the referencing.

Reply: Fixed.

Reviewer Comment 3.17 — Conclusions: Again, I think it is important here to make it clear that this new study is not offering an algorithm that is better than others, rather it is detecting a specific subset of cyclones that are within the extratropical stormtracks. I think it should not be compared so closely to the previous studies as it is specifically choosing something different to look at. I also think that since this method does not use a front identification method, it cannot distinguish secondary cyclogenesis as it is defined in the literature. Therefore, the conclusions need some work to demonstrate the novelty of this method and the implications for future work.

Reply: The reviewer raises valid concerns about references to secondary cyclogenesis, which we do not explicitly detect. It is also correct that we introduce a different approach. However, while it might be different, we feel that our study needs to be put in context to other studies that attempt to perform related analyses, even though the motivation and specifications of the algorithms differ significantly. We tried to reconcile the concerns of the reviewer by revising parts of the introduction, methodology, and conclusions accordingly.