The role of tropopause polar vortices in the intensification of summer Arctic cyclones by Gray, Hodges, Vautrey and Methven: Response to reviewers

The reviewers' comments are copied below in black with our point-by-point responses in blue.

Response to reviewer 1:

This submission undertakes a comprehensive analysis (based on 40 years of ERA5 reanalysis) of summer Arctic TPV influences on the intensification of near-surface cyclones. As the authors point out, there has been relatively little attention devoted this topic. The ideas, methods etc. used in the paper are described well and clearly, and the rationale and aims are succinctly expressed. In part, the investigation includes a careful stratification of the upper and low-level systems which serves to reveal much about the connections between these, and the accompanying analysis reveals some very interesting (and perhaps unexpected) results.

The submission is poised to make a significant contribution to the literature, but not in its present form. Before I would be able to recommend acceptance, there are a number of issues which need to be addressed.

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

Lines 29-31: In this connection with the AFZ very valuable to reference the works of

Crawford, A., and M. Serreze, 2015: A new look at the summer Arctic Frontal Zone. Journal of Climate, 28, 737-754, doi: 10.1175/jcli-d-14-00447.1.

Crawford, A. D., and M. C. Serreze, 2016: Does the summer Arctic frontal zone influence Arctic Ocean cyclone activity? Journal of Climate, 29, 4977-4993, doi: 10.1175/JCLI-D-15-0755.1.

Crawford, A. D., and M. C. Serreze, 2017: Projected changes in the Arctic Frontal Zone and summer Arctic cyclone activity in the CESM Large Ensemble. Journal of Climate, 30, 9847-9869, doi: 10.1175/JCLI-D-17-0296.1.

Thank you for highlighting these relevant papers. We now cite the second of these (Crawford and Serreze, 2016) in our paper in relation to our brief introduction to the Arctic frontal zone. Citation of the Crawford and Serreze (2017) paper would have required additionally citing several other papers that discuss the changes in projected summertime Arctic cyclone activity linked to the Arctic frontal zone. As our paper is not about either the impact of climate change on Arctic cyclones, or the impact of the Arctic frontal zone on cyclone activity, a detailed review of this literature would be excessive and disrupt the flow of this early part of the introduction.

Crawford and Serreze (2015) is also now cited in the last paragraph of section 3.2.

Line 37: 'Cavallo et al., 2009' should be Cavallo and Hakim, 2009'. (Similar error at lines 66, 104, 244, 261, 495, … and maybe elsewhere.)

Thank you for pointing this out. The error came from duplicate author names being included in the bibtex file and this has now been fixed.

Lines 41-65: A informative survey is presented here in connection with Arctic cyclone numbers and trends, and how results may depend on a range of techniques used. Beneficial here to cite analysis of Screen and coauthors, 2018: Polar climate change as manifest in atmospheric circulation. Current Clim. Change Reports, 4, 383-395, and Rudeva et al., 2015: Variability and trends of global atmospheric frontal activity and links with large-scale modes of variability. Jnl. Clim., 28, 3311-3330.

Thank you for highlighting these relevant papers. The Screen at al. (2018) paper is a review article that also cites several other papers in addition to the Rudeva and Simmonds (2015) paper on this topic. Hence, we have just cited the Screen et al. paper.

Lines 73-78: This text presents a nice clarification of many terms which, in this broad topic, have often been confused. Incidentally, in connection with 'radiative cooling re-building the PV reservoir' it would be helpful to reference the study of

Cavallo, S. M., and G. J. Hakim, 2012: Radiative impact on tropopause polar vortices over the Arctic. Monthly Weather Review, 140, 1683-1702, doi: 10.1175/MWR-D-11-00182.1.

Thank you for the suggestion. Cavallo and Hakim (2012) examine the importance of radiative cooling for the formation and characteristics of TPVs. Our sentence "Eventually, the larger-scale tropospheric polar vortex is re-established in autumn as a result of increasing net radiative cooling re-building the PV reservoir." refers to radiative cooling across the Arctic and is not specific to radiative cooling within TPVs. Hence, the addition of this citation is not appropriate here.

Line 93: Change 'Cavallo et al. (2010)' to 'Cavallo and Hakim (2010)' – similar change required at ll. 119, 177, 453, 552, …

Thanks for pointing this out. The error came from duplicate author names being included in the bibtex file and this has now been fixed.

Line 127: See also the detailed vorticity budget analysis of this storm by

Ryota Ishiyama, and Hiroshi L. Tanaka, 2021: Analysis of vorticity budget for a developing extraordinary Arctic cyclone in August 2016. Sola, doi: 10.2151/sola.2021-020.

Thank you for pointing out this newly published paper, a citation has been added to the text.

Line 146: I suggest starting a new paragraph with the 'In this study …'. This makes it clear that the introductory survey has finished, and the authors are now going on to detail what they plan to achieve in the paper.

We thank the reviewer for this suggestion. We prefer to not to break the paragraph here though as the sentence beginning "In this study we explore this research gap…" links directly to the preceding text in the paragraph which states what this research gap is. If we instead started a new paragraph with this sentence, we would need to restate the research gap.

Line 352-354: It is not clear whether we are looking at statistically significant differences between the months. This should be checked. If, e.g., an F test does not indicate significance perhaps this comment should be deleted, as we would just be looking at noise. If the differences are above the noise level, some (thermo)dynamic explanation or hypotheses should be offered. One thought that comes to mind is that by removing the '>= 2 days' constraint might mean radiative influences become more apparent, and result in result in a signal in midsummer.

For both the matched and unmatched cyclone sets the count distribution in August has a significantly different arithmetic mean to that in May at the 95% level according to a two-sided Welch's t-test (without assuming equal variance). However, sequential months are not always significantly different. This information has now been added to the paper.

For comparison the mean monthly numbers of TPVs with Arctic genesis and Arctic cyclones with maximum intensity in the Arctic have been added to Fig. 7. Recall that the constraints for both matched and unmatched cyclones include that the TPVs must have Arctic genesis and that the Arctic cyclones must have maximum intensity in the Arctic. Hence, these constraints have similarly been applied when comparing monthly variability of the matched and unmatched cyclones with that of TPVs and Arctic cyclones. There is little variability in the Arctic cyclone counts. In contrast, the TPV counts have a minimum in July and so follow an evolution similar to that for the matched cyclone counts. This suggests that the number of TPVs limits the number of matched cyclones in mid-summer with a consequent maximum in the number of unmatched cyclones then. Crawford and Serreze (2015) show that the strength of the Arctic Frontal Zone is maximum in July (see their Fig. 10) and so this may act to maintain Arctic cyclone numbers in mid-summer despite the reduction in TPVs. The above arguments have now been added to the paper. The July minimum in TPVs could be due to fewer TPVs being generated then because of the seasonal cycle in the tropospheric polar vortex or due to enhanced destruction of TPVs due to tropospheric latent heat release but further research would be necessary to investigate these hypotheses and so we do not include them in the paper.

Lines 445-447: Role of stratospheric influence in the storm of September 2010 was analysed by Tao W, Zhang J, Zhang X (2017) The role of stratosphere vortex downward intrusion in a long-lasting late-summer Arctic storm. Quart. J. Roy. Meteor. Soc. 143:1953-1966

Thank you for suggesting this paper. The stratospheric intrusion discussed seems to fit the definition of a TPV in that it exists as a closed vortex in the middle troposphere and lower stratosphere (at 500 and 300 hPa, respectively, see their Fig. 3) when the low-pressure centre at the surface is weak and then “catches” the surface cyclone as both features intensify. However, the authors refer to this feature as the downward intrusion of the stratosphere vortex which, as we discuss in the introduction of our paper, is confusing terminology. In particular, the term “stratospheric vortex” is usually applied to the strong westerly polar stratospheric vortex which exists in winter, but this has not begun to spin up in September (stratospheric winds are easterly during summer with very little disturbance above the influence of the tropopause zone). Consequently, this paper is difficult to cite in relation to the stratospheric influence on Arctic cyclones. However, we have added here a sentence citing this paper as one that presents an Arctic cyclone with equivalent barotropic structure.

Lines 572-575: It would be worthy of note in the paper to mention the importance of this stratospheric influence on rapid development outside the Arctic, e.g., Kouroutzoglou et al., 2015: On the dynamics of a case study of explosive cyclogenesis in the Mediterranean. Meteor. Atmos. Phys., 127, 49-73.

The importance of stratospheric influence on the development of cyclones outside the Arctic (specifically extratropical cyclones) is already discussed in the second paragraph of section 3.4. The paper suggested by the reviewer focuses on cyclogenesis cases during winter over the Mediterranean. We are not specifically considering rapidly developing cyclones and, given the large number of case studies that have been performed on explosively developing cyclones, especially in winter, it does not seem appropriate to pick out an individual study for citation here.

Lines 597-600: A gremlim seems to have gotten into the author list for these two papers by Steve Cavallo and Greg Hakim.

Thank you for pointing this out. The error came from duplicate author names being included in the bibtex file and this has now been fixed.

Response to reviewer 2:

General comments:

This research investigated the relationship between Arctic cyclone (surface cyclone) and tropopause polar vortices (TPVs) using the same tracking algorithm. The authors analyzed the features of the track density and composited structure for Arctic cyclones by separating matched and unmatched cyclones. The results showed that most of the Arctic cyclones are far from the TPVs at their initiation, and about one-third of the Arctic cyclones developed associated with the TPVs. They also showed that while the genesis of the unmatched cyclones is along the Eurasia coastline, that of the matched cyclone generated over the Arctic Ocean, North America, and the Canadian Arctic Archipelago. The rearward title of relative vorticity for matched cyclones is less than that for unmatched cyclones. The topic is very interesting, and this study shows the relationship between Arctic cyclones and TPVs clearly for the first time. The findings in this study would promote the understanding of the cyclone development over the Arctic in summer. Therefore, the reviewer recommends for publication of this article in WCD after minor revisions.

We thank the reviewer for his/her interest in our research and this positive review.

Comments:

Lines 37: Tao et al (2017, QJRMS) also showed the importance of TPVs for the intensification of an Arctic cyclone.

Tao, W., Zhang, J., & Zhang, X. (2017). The role of stratosphere vortex downward intrusion in a long-lasting late-summer Arctic storm. Quarterly Journal of the Royal Meteorological Society, 143(705), 1953–1966. https://doi.org/10.1002/qj.3055

Reviewer 1 also suggested inclusion of this paper and it is now cited in the second paragraph of section 3.4. As stated in our response to reviewer 1, the authors of this paper refer to an intrusion of the stratospheric vortex rather than a TPV as having an important role in the development of their case study, although the feature shown seems to fit the definition of a TPV. As the authors do not refer to the feature as a TPV, it does not seem appropriate to include this paper in the list given here of papers that have presented a link between TPVs and Arctic cyclone genesis and intensification.

Line 215: Is the percentage of the cyclone associated with the TPVs within 2º is intermediate between 1º and 3º in Fig. 5? While the author showed the percentage of the cyclone associated with the TPV within 1º, 3º, and 5º at each stage (Fig. 5), the percentage of the matched cyclone (i.e., cyclones satisfied both criteria) is not shown in the results.

The caption of Figure 1 gives the total number of Arctic cyclones and TPVs identified across the 40 extended summer seasons. There is a total of 12155 Arctic cyclones tracked with lifetime of at least 1 day. Of these, 10636 have lifetimes of at least 2 days – this number has now been added to the caption of Fig. 5. The number of matched cyclones (341) is stated in the caption of figure 6. Hence only a small percentage of tracks (3.2%) meet the criteria that they are matched to an Arctic origin TPV because they are within 2^{o} of an Arctic origin TPV at the time of their maximum intensity and within 5^{o} of an Arctic origin TPV at the time of their maximum growth rate, exist for at least 2 days prior to their maximum intensity (note this means that their total lifetime will exceed 2 days) and meet their maximum intensity in the Arctic.

We have also added a set of bars to Fig. 5 to show the instantaneous matches within 2 degrees radius at the times of genesis, max growth rate and max intensity (and modified the text accordingly).

Line 279-281: Do the track density and cyclone features show similar results to the Figs. 1-3 in each month? Or these features have monthly variability?

We focus on examining the extended summer period as a whole as this improves the robustness of the results by providing more data than if considering the months separately. In response to the reviewer's question though, the genesis densities and track densities are shown for each month in Figs. 1 and 2 below for (all) Arctic cyclones and Arctic genesis TPVs. These panels can be compared to Fig. 1a and 1e in the paper for the extended-summer genesis density of (all) Arctic cyclone and Arctic genesis TPVs, respectively and to Fig. 2a and 2e in the paper for the extended-summer track density of (all) Arctic cyclone and Arctic genesis TPVs, respectively. As expected, the plots are less smooth when considering each of the five months individually compared with the extended-summer period. There is also some month-to-month variability. However, the basic features of the plots, as described in the paper are also present for each month. We have added to the paper that we have also examined the density maps for the individual months, but do not consider it worthwhile to also add Figs. 1 and 2 included here.

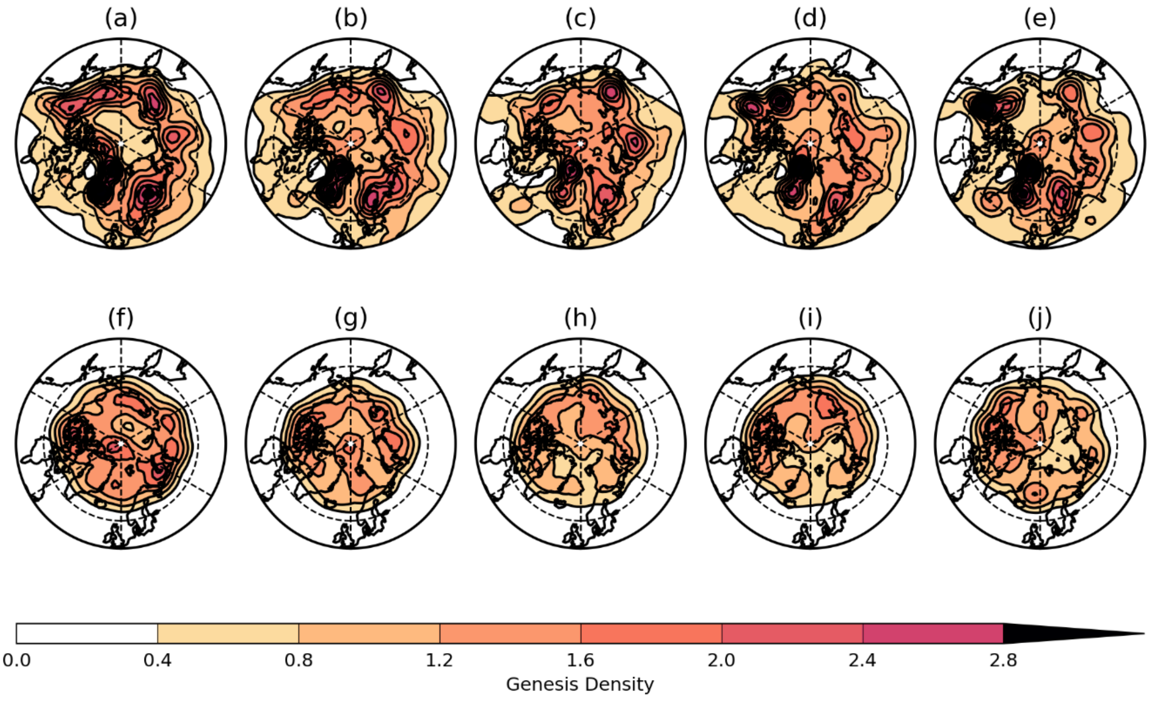

Figure 1. Genesis density for (top row) all Arctic cyclones and (bottom row) Arctic genesis TPVs for (a,f) May, (b.g) June, (c,h) July, (d,i) August, (e,j) September 1979-2018.

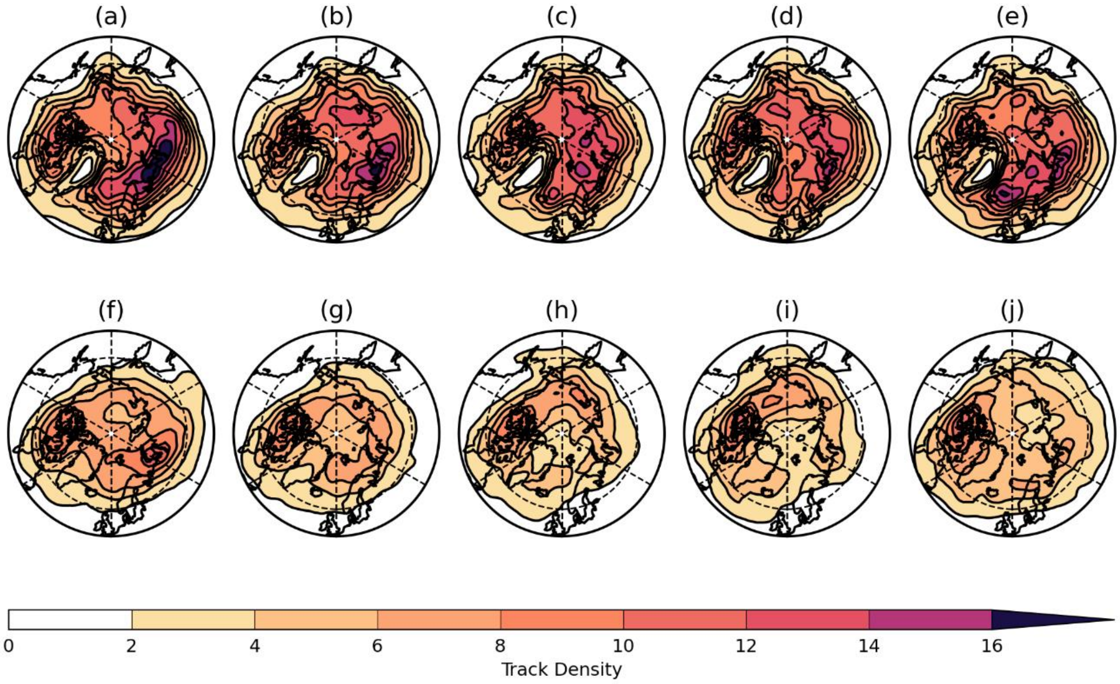

Figure 2. As for figure 1 but for track density.

Lines 285: Does the timing of the maximum relative vorticity match with that of the minimum SLP? Or they have some lag?

All cyclones are tracked using spatially filtered 850-hPa relative vorticity as described in section 2.1. An associated MSLP pressure minima is sought for each track point – a description of the method used to do this has now been added to section 2.1 of the paper. Thus, while the cyclone centre MSLP value at the time of maximum relative vorticity is almost always known (as almost all cyclones have an associated MSLP minima when they are intense), the timing of the tracked cyclone's lowest MSLP value along the track may not always be defined. Having said that there is usually a reasonably close timing match between the time of the MSLP pressure minimum and relative vorticity maximum along a cyclone track. For example, Fig. 4 in Bengtsson et al. (2009, https://doi.org/10.1175/2008JCLI2678.1) shows that for lifecycle composites of intense winter extratropical cyclones the MSLP minimum lags the 850-hPa relative vorticity maximum by about 2 hours. We also calculated a similar lifecycle composite plot for our matched and unmatched cyclones (Fig. 3 here) and this shows negligible difference between the timing of the MSLP minimum and relative vorticity (and wind speed) maxima. We have added a note on this timing consistency to the paper, but chose not to also add this figure as we do not consider it adds sufficient useful additional information.

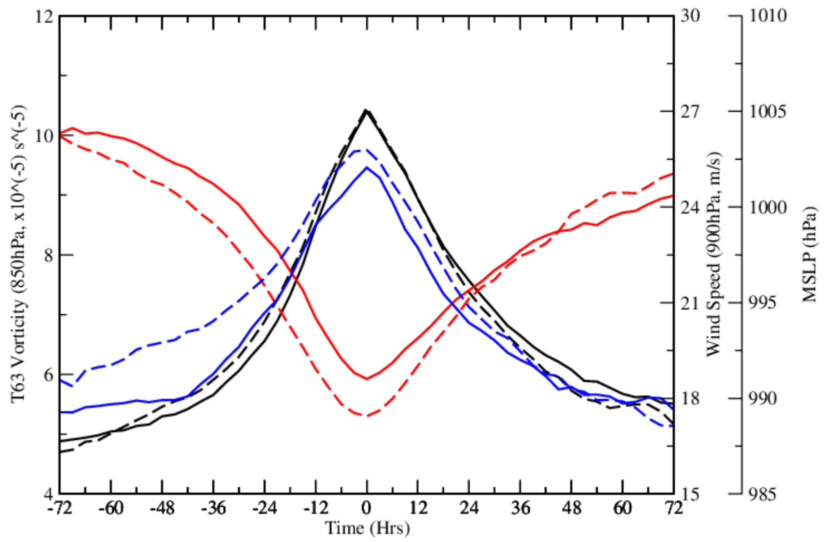

Figure 3: Lifecycle composite for 200 most intense matched (solid) and unmatched (dashed) Arctic cyclones showing filtered 850-hPa relative vorticity (black), 900-hPa windspeed (blue) and MSLP (red).

Fig. 4: Including the NAO index would be better for understanding Line 304-317.

The NAO index has now been added to Fig. 4. and the caption and paper text modified accordingly.

Lines 334-336: Why the number of unmatched cyclones is similar to matched cyclones? Isn't a cyclone classified as an unmatched cyclone when it does not satisfy the criteria of a matched cyclone?

The result that the number of unmatched cyclones is similar to that of matched cyclones is mainly just a fortunate coincidence. The criteria used to define the matched and unmatched cyclones are given in the second paragraph of section 2.2. As explained here, matched cyclones are defined as cyclones that are within 2° of an Arctic origin TPV at the time of their maximum intensity and within 5° of an Arctic origin TPV at the time of their maximum growth rate. Unmatched cyclones are defined as cyclones that are further than 5° from a TPV at both their time of their maximum growth rate and time of their maximum intensity. Hence, some cyclones are classified as neither matched nor unmatched (those that are further than 5° from a TPV at their time of their maximum growth rate but between 2 and 5° from the TPV at the time of their maximum intensity). As both matched and unmatched cyclones also must reach their maximum intensity in the Arctic and exist for at least two days prior to their time of maximum growth rate the number in both sets is only a small fraction of the total number to tracked cyclones. The impact of the separate criteria on the cyclone numbers are now given in the text in the second paragraph of section 3.2.

Lines 340-341: Is there any relationship between the location of the local maximum of genesis density for the matched cyclone and surface condition (e.g., cyclone generated over the marginal ice zone in Inoue and Hori (2011, SOLA))?

Inoue, J., & Hori, M. E. (2011). Arctic cyclogenesis at the marginal ice zone: A contributory mechanism for the temperature amplification? Geophysical Research Letters, 38(May), 1–6. https://doi.org/10.1029/2011GL047696

The relationship between cyclogenesis and the surface condition, in terms of ice fraction, is an interesting question, as is the inverse relationship of the impact of cyclones on ice fraction. However, we have not attempted to match the cyclogenesis locations of individual cyclones to the surface condition at the time – this would be beyond the scope of our study with its focus on the relationship of cyclones to TPVs. As we point out in the text, the main genesis regions occur near Greenland (likely linked to orographic processes) and along the Arctic coastline, where the Arctic frontal zone exists in summer (e.g., compare our Fig. 1a showing the genesis density for all cyclones with Fig. 1a in Day and Hodges (2018, https://doi.org/10.1029/2018GL077587) showing the location of the Arctic frontal zone marked by a magenta box). We also note in the text the presence of an additional localised genesis region over the North pole. This occurs for the matched cyclones, but not for the unmatched cyclones. Hence, this genesis region is assumed to be a result of interaction of cyclones with TPVs, rather than due to surface conditions.

Lines 407-409 and 450-452: Do these sentences indicate that the PV at the middle-troposphere in the matched cyclones is due to the frictional processes? If so, is the middle-tropospheric PV for each cyclone influenced by the surface condition (over the land, ocean, and sea ice)?

Several papers have been published in which the diabatic and frictional generation of potential vorticity in extratropical cyclones has been attributed to different processes. For example, Chagnon et al. (2013, https://doi.org/10.1002/qj.2037) present vertical cross sections through an extratropical cyclone case study that show contributions of longwave radiation, boundary layer heating, large-scale cloud and convection to PV generation (their Fig. 9). Although frictional processes mainly generate PV in the atmospheric boundary layer, the PV can be advected out of the boundary layer and thus contribute to the PV in the middle troposphere. However, diabatic generation from cloud and convective processes, as well as longwave radiation, is also important in this region. It is certainly plausible that the PV in the middle troposphere is influenced by the surface condition through friction and the importance of frictional processes for Arctic cyclone development (and their contribution to PV) is currently being investigated as part of an ongoing research project in which two of the authors are involved (https://research.reading.ac.uk/arctic-summertime-cyclones/overview-arctic-cyclones/). An additional sentence has been added to the paper text noting methods that could be used to attribute mid-tropospheric PV generation to individual model processes.

Response to reviewer 3:

General comments:

This study evaluates how often pre-existing tropopause-level features are present for Arctic cyclone development. Specifically, the authors seek to quantify the frequency that the tropopause-level disturbance called tropopause polar vortices (TPVs) are linked to Arctic cyclones during several stages of the cyclone's lifecycle. The analysis is performed by the authors computing tracks of TPVs and Arctic cyclones from ERA5 based on certain flow metrics and distance thresholds. Using these methods, the authors find a rather surprising result that at most, 10 percent of Arctic cyclones have a nearby TPV at genesis, while 35 and 38 percent of Arctic cyclones are in close proximity to a TPV at the maximum growth rate and intensity, respectively. This is surprising because the alternative explanation that is offered is that Pettersen type A cyclogenesis must be more common, where the tropopause-level anomaly is generated as a result of baroclinic interactions with low levels.

The study is novel in that the relation of TPVs and Arctic cyclones has never before been systematically quantified as has similarly been established in midlatitudes between tropopause-level features and surface cyclone development. Although I point out some serious flaws below, I do think that this preliminary work warrants further analysis and ultimately could successfully answer the scientific question to make a positive and notable contribution after major revisions. In particular, my most primary concerns at this stage are:

We thank the reviewer for taking the time to review our paper and for her/his recognition of the novelty of our work. We address her/his concerns below.

1) The main argument presented for type A cyclogenesis as I read it is that TPVs are not evident near the unmatched Arctic Cyclones since relative vorticity does not increase with height. By thermal wind balance, this implies that there should not be a minimum potential temperature anomaly at all levels (up to the tropopause). However, the cross-sections for each case in the composites are all taken in the same (along-track) direction. Unlike baroclinic waves, where there is usually a westward tilt with height, in the case of TPVs, TPVs could lie in any direction from the center of the surface cyclone, and may not be large enough to span even half a quadrant. Dynamically, this would still create favorable baroclinic conditions needed for an Arctic Cyclone to form or intensify due to a TPV. One additional complication for using the along-track direction is that there may be multiple TPVs influencing the surface cyclone over the course of its lifecycle, and hence a surface cyclone's trajectory may not be straightforward as it is in midlatitudes where there is typically one relatively large upshear PV anomaly. In addition, it is further used as evidence that in the unmatched cases, there must be a diabatic and/or frictional process to create upper-level PV in conservative flow (lines 406- 409), but the reasoning does not account for additional features that may be (conservatively) advected from elsewhere to influence the nearby flow. For example, there can be multiple TPVs rotating around a larger-scale feature, or TPVs moving toward the cyclone around the flow of lower-latitude Rossby waves or wave breaks. Thus, cross-section direction is likely unique for each individual case, and important characteristics may be inadvertently missed by fixing cross-sections to along-track direction. While it would be helpful for the authors to state the details of their calculation instead of referring to Bengtsson et al. (2009), I am somewhat familiar with their method. Nonetheless, the tilt method is similar to that of the cross-sections by projecting vorticity in a certain direction and thus would suffer from the same shortcomings as the cross-sections.

We agree with the reviewer that TPVs could lie in any direction from the centre of the surface cyclone and may interact to form or intensify the cyclone (or to contribute to its decay). However, the reviewer is incorrect that we require the TPVs to lie in a specific direction relative to the cyclone centre as part of the matching algorithm. The reviewer has misunderstood our method of matching cyclones to TPVs (and equivalently determining cyclones that are not matched to TPVs). We do **not** require that the associated TPV lies in the along-track direction to be matched to a cyclone. As described in the second paragraph of the methods section 2.2., we identify matched (and unmatched) cyclones according to whether a TPV lies within (or not) specified radii from the cyclone centre at the times of maximum growth rate and maximum intensity. We do not specify directions, just radii. The methods section (line 213 of the original submission) states "Note though that the TPVs are not constrained to be located such that the direction of the tilt between a TPV and the associated surface Arctic cyclone is conducive to baroclinic growth (i.e., rearwards relative to the movement of the cyclone)". Similarly, for unmatched cyclones there will be no TPV within the specified radii in any direction from the cyclone centre.

When evaluating the composite cyclones (composites of cyclones either matched or unmatched to a TPV) we do show plots in the direction defined by the track of the surface cyclone. The TPV (if present) is not required to lie along this section for any individual cyclone within the composite, but it is a result emerging from the data that the tropopause disturbance is large to the left of the surface cyclone in the "along-track" direction; as stated in the text, although the relative vorticity maximum tilts mainly to the "west" of the nominally west-east section with height, it also extends to the north for both the matched and unmatched composites. A plot demonstrating this northwards extension is shown in Fig. 4 here.

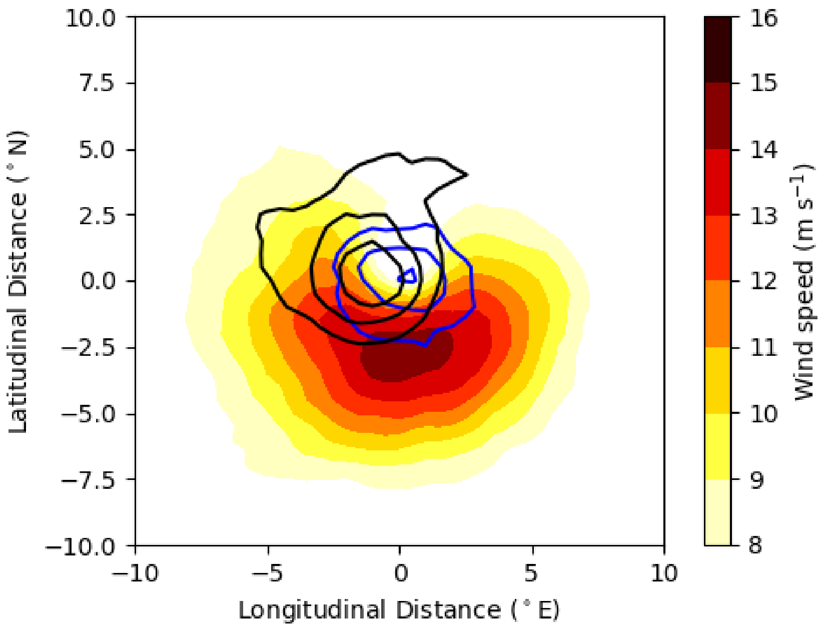

Figure 4. Composite matched cyclone at the time of maximum intensity showing 900-hPa wind speed (colours), and relative vorticity at 900 hPa (blue contours) and 400 hPa (black contours). Relative vorticity contours begin at $4x10^{-5}$ s^{-1} with $4x10^{-5}$ s^{-1} interval. The cyclone motion is to the right.

The composites were produced by considering the track direction of each individual cyclone at the required compositing time (e.g., the time of the maximum growth rate of that specific cyclone) and rotating each cyclone so that the nominal track direction is eastwards before compositing. So, the fact that any cyclone may follow a convoluted path is not an issue. Indeed, in the situation where the

TPV and low-level cyclone are co-rotating due to their interaction, the TPV is still expected to be to the left (or "west") of the surface cyclone in the "along-track coordinate" defined in this way (a feature of a tilted, rotating vortex column). We have not explicitly considered the effects of several TPVs interacting with a surface cyclone, but instead focused on the TPV that is matched at the time of maximum growth rate for the surface cyclone. The distance criterion is sufficiently small that only one distinct TPV will be matched at any instant.

We apologise that the above aspects of the method were not clear to the reviewer. We have added additional text to the methods section to make these aspects clearer.

The reviewer also states that we propose "in the unmatched cases, there must be a diabatic and/or frictional process to create upper-level PV in conservative flow (lines 406- 409)". This is incorrect. The lines referred to by the reviewer state "In both the matched and unmatched composites, there is a secondary maximum in PV near the ground and additionally for the matched composite, enhanced PV in the mid-troposphere. These features indicate the influence of diabatic and frictional processes since the development of a new maximum could not occur in conservative flow." Hence, we are clearly referring to PV generation near the surface and in the mid-troposphere, not at upper levels. The tropopause-level disturbance is expected to grow primarily through adiabatic advection of PV by the flow induced by the low-level cyclone, even in the unmatched cases.

Finally, the reviewer states that "it would be helpful for the authors to state the details of their calculation instead of referring to Bengtsson et al. (2009)" when referring to our calculation of tilt. The method of calculating the tilt (shown in Fig. 10) was fully described in the third paragraph of the methods section 2.2 in the original submission. A cross-reference to this description is now given where the results of the tilt calculation are first shown and discussed in section 3.3.

2) The choice of matching cyclones within up to 5 degrees (or 555 km) is too restrictive. A reason is given on lines 210-214, but even when just considering the Coriolis parameter, alone, the Rossby radius of deformation varies quite substantially between the pole and 65N. Cavallo and Hakim (2010) find that TPVs radii can range up to around 1000 km, and Aizawa and Tanaka (2016) find that Arctic Cyclones can have radii up to ~2500 km. This puts a maximum spatial scale for which one could expect from observed cases in the peer-reviewed literature that the possible influence between an upper-level and lower-level PV anomaly to be somewhere in the 1000-2500 km range. I appreciate the sensitivity test to the chosen threshold at the beginning of Section 3.2, but there should also be tests done at larger threshold distances. Perhaps a sensitivity test could be done by varying the chosen threshold and performing Monte-Carlo tests between Arctic Cyclones and a random drawing of TPVs. See the details in Lillo et al. (2021) (Figure 17) for a similar test between cold air outbreak locations and TPVs.

The reviewer argues that matching cyclones to TPVs within up to 5° is too restrictive. As noted by the reviewer, we provide an argument for our choice in the paper based on the Rossby deformation radius, $L_R=NH/f$. As stated there, the Rossby deformation radius in the Arctic is estimated to be at most 500 km (for a tropopause at 7 km (H) and Coriolis parameter (f) of about 1.5 x 10^{-4} s^{-1}; we assumed a static stability value (N) of $10^{-2} s^{-1}$). The reviewer states that the 5° radius is too small because (i) the Rossby deformation radius "varies quite substantially between the pole and 65°N" as a consequence of the change in Coriolis parameter, and (ii) the horizontal scales of both TPVs and Arctic cyclones can exceed 5°.

In response to point (i), the Coriolis parameter varies between 1.46 and 1.32 x10^{-4} s^{-1} between the pole and 65°N due to its dependence on sin(latitude) and so the Rossby radius varies between about

479 and 530 km for the given *N* and *H*. Hence, the reviewer is incorrect that the variation in Coriolis parameter across the Arctic region markedly affects the Rossby radius. *N* is not observed to vary substantially on large-scales. Therefore, the primary variation in the scaling is associated with the assumed depth scale of the motions, *H,* and 7km is on the larger side.

In response to point (ii), the separation radius at which two features can interact baroclinically depends on the shape of the structures interacting and the nature of interaction. At one limit a point PV anomaly on tropopause (or ground) would have an influence on velocity which falls off with e-folding distance, L_R (and with Rossby depth scale fL_R/N). So, this is our justification for 500 km (approximately L_R). A vortex patch with radius smaller than L_R actually has a similar influence on the flow around it – so again 500 km is appropriate, although on the smaller end. At the other extreme if the disturbances at both levels were sinusoidal waves, then the horizontal scale for interaction would be between L_R and 1/k (where k is the wavenumber) which could be a lot larger. Similarly, the depth scale could be greater. So, the reviewer is partly right that if Arctic cyclones and TPVs were all larger scale (and part of baroclinic waves) then range of interaction could be much greater than L_R. By using a matching criterion of separation < L_R we are being conservative in that within this distance we would expect interaction no matter what the shape of the disturbances. But interaction is possible for disturbances separated by distances greater than this (if they have the right shape).

Our aim in defining the criteria for matched and unmatched cyclones is to distinguish between cyclones that are very likely and very unlikely, respectively, to interact with a TPV (recall that most cyclones are not categorised as either matched, or unmatched with a TPV). While our matching radii criteria are such that we may miss some cyclones that interact with a TPV, the matched set is unlikely to include cyclones that are not interacting with a TPV. An important point which we only learn after the analysis in the paper (although it is expected for baroclinic interactions and has been shown previously in case studies of cyclones interacting with TPVs) is that in the matched (and unmatched) cases the upper and lower features end up stacked on above the other at maximum intensity, although there is up-shear tilt during maximum growth. Hence by requiring unmatched cyclones to be further than 5^o away from a TPV at the times of both their maximum growth rate and maximum intensity our unmatched set is unlikely to include cyclones that are interacting with a TPV.

A summary of the above justification has now been added to the methods section of the text (section 2.2).

3) I do also have some concerns that this analysis could very likely be filtering out a large portion of TPVs. The spectral filtering of T5-T63 would remove features equal to or less than about 400 km. This is near the mean radius of TPVs (Cavallo and Hakim 2010) and therefore, given the ERA5 resolution of 30 km, would be eliminating perhaps as many as half of all TPVs. See in particular Figure 2a in Cavallo and Hakim (2010). Also compare with Figure 6a in Hakim and Canavan (2005). Note the differences in radii, where Hakim and Canavan (2005) may also suffer from the effects of spectral filtering while Cavallo and Hakim (2010) use a regularly-spaced grid and conversely does not suffer the same effect. The spectral filtering here may also be removing more features toward the pole, and this is evident in Figure 2a,b, where there is a very dampened summer Arctic cyclone maximum near the pole in contrast to Serreze and Barrett (2008). I suggest using the track data that is described in Szapiro and Cavallo (2018), which uses a watershed segmentation approach to identify minima. They also compare directly with Hakim and Canavan (2005), and hence this would keep results most consistent with previous results. While those results do not use ERA5, I would think it is quite likely these data with ERA5 are freely available if requested. This would be far more advantageous than just using the same tracking algorithm for both surface cyclones and TPVs.

The reviewer states that we could “very likely be filtering out a large portion of TPVs” by our spectral filtering. She/he justifies this statement by saying that our T5-T63 filtering would remove features smaller than about 400 km. In fact, the smallest resolved scale in spectral transform model depends on the shape of the feature. Lander and Hoskins (1997, doi:10.1175/1520-0493(1997)125<0292:BSAPIA>2.0.CO;2) suggest that $\pi a/N$ (where a is the radius of the earth and N is the total wavenumber) is an approximate estimate for circular features (it would be smaller for wave-like features). So T63 is equivalent to a *smallest resolved scale* of about 320 km (a little smaller than the scale mentioned by the reviewer). It does not matter where you are on the sphere since the total wavenumber is an isotropic measure of scale – so this applies near the pole or equator. This resolution refers to resolving one feature from another (i.e., distinguishing them, like peaks in optics) – not the appropriate grid spacing on a spectral transform grid which would be much smaller. In terms of equivalence to a grid-point model, the usual guidance there is minimum of 5-6 grid points needed to partially resolve a feature – so this spectral resolution is equivalent to a grid-point model with spacing of about 60 km. Cavallo and Hakim (2010) find that the vast majority of TPVs (after filtering which was designed to isolate well-resolved vortices) have radii exceeding 200 km, and so diameters exceeding 400 km. Hence, these features would be represented with the T63 upper limit of filtering used in our study.

It is also worth noting that the spectral filtering does not mean that it is a sharp cut-off for the scale of the features represented at 320 km (such that smaller features are absent); they will tend to be smoothed which is the point to remove the small-scale noise that results in multiple centres. Centres can be unambiguously distinguished if their separation is greater than 320 km. In addition, removing the planetary scales (by the lower limit of filtering, T5) also allows shallow systems to be more easily identified.

A summary of the above arguments has now been added to the paper in section 2.1 where the spectral filtering is described.

Finally on this point of filtering scale, as already noted in the paper we performed some preliminary case study analysis in which different spectral filtering ranges were compared (not shown). This analysis showed that the chosen filtering retained features of interest while removing smaller mesoscale features, such as fronts, and smaller TPV features. This preliminary work was included in the MSc dissertation of our co-author Jonathan Vautrey (available from the University of Reading) and the relevant plots from this dissertation are included here (Fig. 5 here). These show, for this specific time, little difference between applying T5-T63 and T5-T100 filtering for the identification of TPVs. However, the T5-T63 filtering removed some more of the smaller-scale features (e.g., fronts from the 850 hPa relative vorticity field) and so yielded a more manageable, and hence preferable, dataset for tracking.

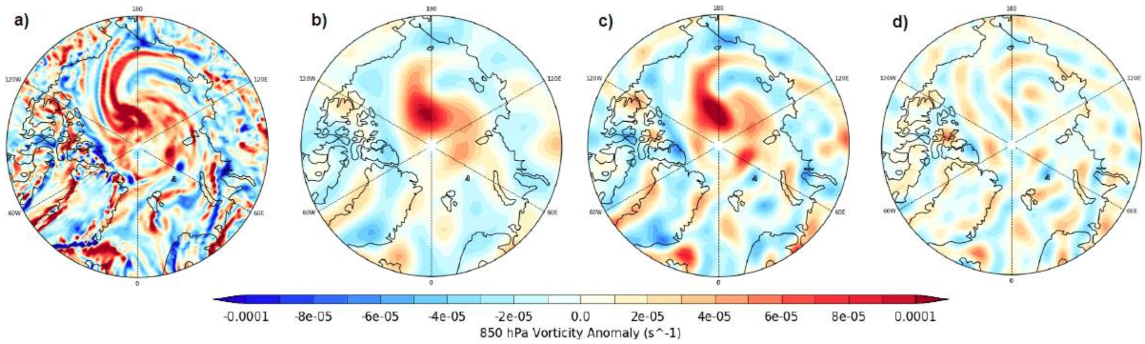

Figure 3.1: *850-hPa relative vorticity anomaly at 00 UTC 16/08/2016 for various filtering criteria: a) unfiltered, b) T5-63, c) T5-100, d) T40-100. Maps orientated with 0° longitude at bottom.*

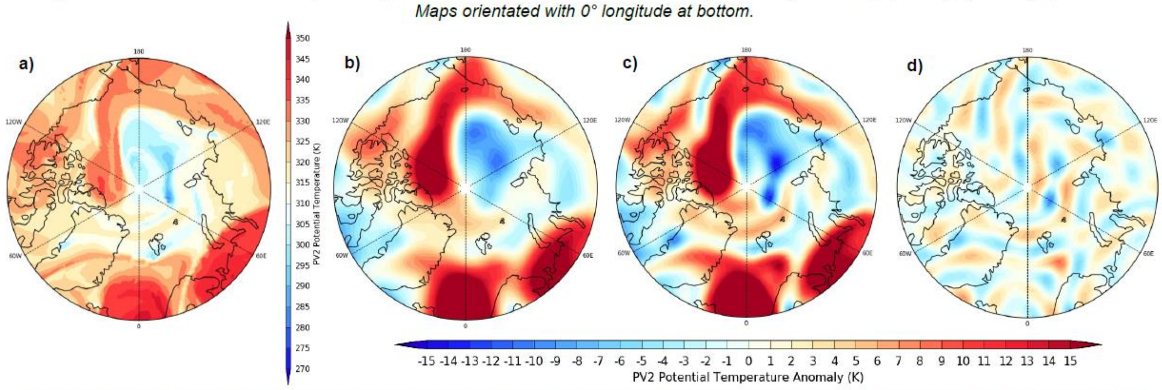

Figure 3.2: *PV2 potential temperature (anomaly) at 00 UTC 16/08/2016 for various filtering criteria: a) unfiltered, b) T5-63, c) T5-100, d) T40-100. Maps orientated with 0° longitude at bottom.*

Figure 5. Figures from the MSc dissertation written by co-author Vautrey. See individual captions for details.

The reviewer also speculates that the spectral filtering may be removing more features toward the pole based on the lack of a maximum in Arctic cyclone track density near the pole which contrasts with Serreze and Barrett (2008). In fact, the spectral filter we use is isotropic and so it is not true that it removes more features towards the pole. When comparing with the Serreze and Barrett (2008) paper we note that their Fig. 1 shows cyclone counts whereas track density shown in our paper. As noted in Serreze and Barrett (2008), slow-moving cyclones could be counted twice or more in a given grid cell. This is especially likely given the coarse resolution of the data used, the NCEP reanalysis with a 2.5x2.5^{o} grid. Slow-moving cyclones are more likely to occur near the pole, away from the polar jet stream. Hence, track density plots and cyclone count plots are not directly comparable, and it is not surprising that the maximum in cyclone counts found by Serreze and Barrett (2008) is not apparent in our track density plots. For a better comparison with Fig. 1 of Serreze and Barrett (2008), Fig. 6 below shows an equivalent plot to Fig. 2(a) in our paper, but for feature density rather than track density and with a bounding latitude of 60^{o}N (instead of 50^{o}N).

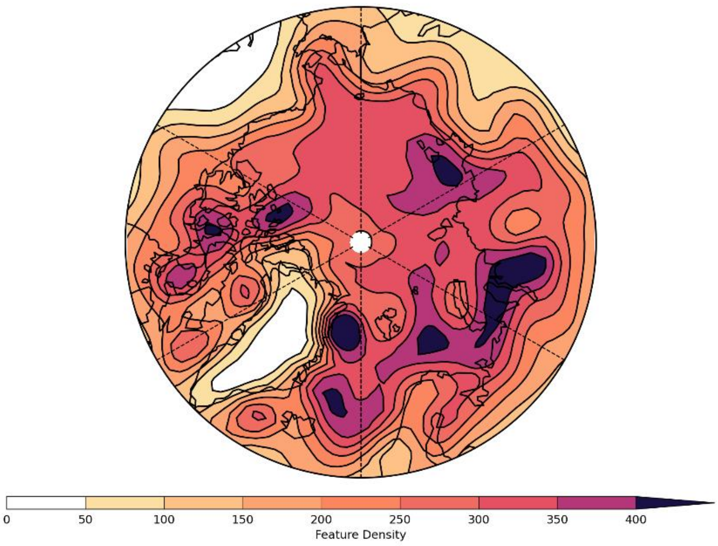

Figure 6. Feature density for (all) tracked Arctic cyclones (minimum lifetime of 1 day) for the extended summer seasons 1979-2018. Units are number per unit area per season where the unit area is equivalent to a five degree spherical cap (~10^6 km^2).

This figure shows a local maximum between about 120-150° East that is somewhat similar to the maximum in cyclone counts found over the central Arctic Ocean in Serreze and Barrett (2008) though displaced to the south and west. Note though that our results are for the extended summer (May-September) whereas Serreze and Barrett (2008) considered July-August and so our signal may be diluted and/or shifted. Also, as stated in our methods section, we filter out less mobile systems. Finally, on this point, we note that Serreze and Barrett (2008) identify features using MSLP whereas we use 850-hPa relative vorticity – as noted by Vessey et al. (2020) there can be differences between features identified and tracked using these two fields. In summary, there is not a problem with our filtering approach near the pole and the differences between our results and those of Serreze and Barrett (2008) are readily explained.

Lastly, the reviewer suggests that we repeat our work using a different identification and tracking algorithm for TPVs, that of Szapiro and Cavallo (2018). While we agree that it might be interesting to compare our findings with that obtained using a different algorithm, we make the following points to justify retaining our current approach:

(i) The potential benefits of repeating our work with this different algorithm are not obvious, given that we refute above the suggestion from the reviewer that our spectral filtering is removing the TPVs we want to identify and track, and so the significant work that would be required is not justified.

(ii) The identification and tracking algorithm we use (TRACK) has been used successfully in many published papers to track synoptic-scale cyclones both in the extratropics and Arctic e.g. see Vessey et al. (2020, , https://doi.org/10.1007/s00382-020-05142-4) for Arctic cyclones. Also, our identification of TPVs follows that of Cavallo and Hakim (2010).

(iii) There is the benefit of consistency in using the same tracking algorithm for both Arctic cyclones and TPVs.

(iv) The TRACK algorithm has a long history of use by many authors going back to its first description in Hodges (1994); this paper has 308 citations according to the Web of Science. Consequently, it is a well-recognised and understood algorithm by researchers interested in tracking weather features. Kevin Hodges is one of our co-authors, ensuring the correct usage and interpretation of the results. In contrast, the Szapiro and Cavallo (2018) algorithm is obviously much more recent (with only 2 citations to date) and we have not previously used this algorithm.

(v) All identification and tracking algorithms have their advantages and limitations, as demonstrated for example by the IMILAST tracking intercomparison project (Neu et al., 2013, https://doi.org/10.1175/BAMS-D-11-00154.1). Different algorithms are based on different assumptions made in the design of the identification and tracking. TRACK has been designed to be flexible enough to be applied to many different types of systems with the smallest set of criteria so that it is as objective as possible. Introducing additional criteria such as whether systems interact or merge makes things much more subjective so in TRACK this is left to post-tracking analysis if required. However, the spectral filtering helps with system merging to some extent. The watershed segmentation approach proposed by Szapiro and Cavallo (2018) may have problems when the system is very shallow or is an inflexion. Removing the large-scale background, as we do, can help. There is also an issue that this approach can result in over-segmentation in noisy data and then additional criteria need to be introduced to merge unless the data is pre-filtered/smoothed.

Specific comments:

1) Lines 168,192: What is the rationale for requiring systems to be mobile?

The rationale for requiring systems to be mobile is to avoid selecting features that are e.g., tied to orographic features. This requirement is commonly applied when tracking synoptic-scale systems e.g., see Vessey et al. (2020, https://doi.org/10.1007/s00382-020-05142-4) which compares Arctic cyclones from four reanalysis systems.

2) Line 184: Are theta anomalies identified with respect to a (climatological) time mean? If so, tracking minima in theta anomalies will also potentially remove some TPVs. For example, in certain regions of the Arctic where TPVs are very common/frequent, a relatively weak (but dynamically significant) TPV for that location may not necessarily be accompanied by a negative anomaly.

No, we use the planetary wave filtering (removing power for $N < 5$) which is purely spatial.

3) This approach takes the view that there can only be one TPV associated with an Arctic Cyclone. For example, reducing the matching radius from 5 to 2 degrees between maximum growth and maximum intensity. But why can there not be more than one TPV associated with an Arctic Cyclone? Perhaps one TPV could be responsible for an initial growth cycle of an Arctic Cyclone until it becomes vertically tilted, but then another nearby TPV could then “take over” to contribute to either further growth or sustained longevity. It also seems that as an Arctic Cyclone matures and obtains a larger radius, that the scale of potential influence from other nearby TPVs could also increase considerably due to the cyclone’s larger scale.

We do not preclude the possibility that more than one TPV interacts with a cyclone when considering the instantaneous matches shown in our Fig. 5. However, when producing the sets of matched and unmatched cyclones for compositing we conservatively require the Arctic cyclone to be matched to the same TPV at its maximum intensity and maximum growth rate. As the reviewer states, this means that we would miss cyclones that interact with a different TPV at their maximum intensity time and maximum growth rate time (though as far as we are aware there is no data on how often this occurs). As stated in one of our previous responses, our aim is to produce sets of cyclones that we are confident comprise cyclones either matched or not matched with a TPV, rather than to consider every cyclone that may have had an interaction with a TPV during its lifetime; the majority of tracked cyclones are not in either set because they fail to meet the criteria we set. The clarification that we are excluding the possibility that a cyclone interacts with a different TPV at its time of maximum intensity and growth rate has been added to the methods section of the text (section 2.2).

4) In comparing the Figure 2 results to Cavallo and Hakim (2009) around line 261, it seems notable to point out that Figure 2d shows fewer TPVs over the Northern Siberian coast /Arctic Frontal Zone area; In particular, compare with Figure 1 of Cavallo and Hakim (2010). This may be important to note given the results found later in this study that there are a relatively high number of unmatched TPVs and Arctic cyclones near the Arctic Frontal Zone (around line 340).

Both Fig. 1 of Cavallo and Hakim (2010) (which shows a map of the occurrence of intensifying TPVs) and Fig. 1 of Cavallo and Hakim (2019) (which shows maps of the occurrence of genesis, lysis, growth and decay of TPVs) show fewer TPVs over the Northern Siberian coast /Arctic Frontal Zone area than over the Canadian Arctic Archipelago. These maps are consistent with our findings (Fig. 2d) for track density. We are slightly confused as to the point the reviewer is making here but have modified our text to point out the consistency more strongly between our findings and those of Cavallo and Hakim (2009, 2010).

Lines 276-282: The long-lived TPV described in Szapiro and Cavallo (2018) started out relatively small in July 2006 and grew upscale until September. This is contrary to the speculation here. Perhaps the explanation is that the TPVs continue to grow radiatively until an external factor can influence it, such as the jet stream, which is weak in the summer as described in Cavallo and Hakim (2012). This is also consistent with the finding here that TPVs of Arctic genesis have longer

This comment from the reviewer is incomplete and consequently difficult to respond to. However, there is also not enough detail in the Szapiro and Cavallo paper on the long-lived system they mention to comment on this and how much this system is considered long lived because of their methodology. Note that we do not speculate on the size or lifetime of TPVs in our paper other than requiring them to have a minimum lifetime. We agree that it is plausible that TPVs can intensify radiatively as was shown for a case study by Cavallo and Hakim (2009). For that case study, latent heating was found to act to destroy the TPV. Note that to the extent that longwave radiative cooling behaves like Newtonian relaxation, the cooling by itself cannot result in upscale growth since the process is linear.

Technical corrections:

It is confusing that it states that minimum lifetimes are required to be 1 day for TPVs on line 192. Is it not 2 days that is often referred to later for both TPVs and Arctic Cyclones?

No, this is correct in the paper. Initially when identifying TPV and Arctic cyclone features a minimum lifetime of 1 day is used. The resulting features are used in the analysis of the climatological features of the TPVs and Arctic cyclones (section 3.1). The minimum lifetime is increased to 2 days for the later results sections, as is clearly stated.

Lines 264-265: There should be a reference to Figure 2(f) at the end of this sentence as it refers to TPVs that form outside the

The reference to Fig. 2(f) has been added (note that this comment from the reviewer was incomplete but that does not affect the interpretation).

Line 372: Insert "(ξ)" after Relative vorticity since this notation is not defined

Relative vorticity is defined as ξ on line 58 and so has not been redefined here.