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. In the paper with edits highlighted, major changes to the paper (mainly larger blocks of text) are highlighted with red font.

Response to reviewer 1:

[first paragraph omitted here as identical to previous review]
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. I appreciate the authors' thoughtful responses and for incorporating some of my previous points into the manuscript, and think it has improved in the latest version. Some of the major issues, however, have not been adequately addressed, which I describe in detail below. I think consideration of these remaining issues are important in making this manuscript complete and the application of the methodology more strongly convincing.

We appreciate the reviewer taking the time to provide a second review of our article and respond to their comments below.

1) The assumption that Arctic cyclones evolve from a tilted structure to less tilted structure may apply for midlatitudes, but not necessarily the Arctic. While there is no question it applies for baroclinic waves, the Arctic may be a mix of waves, vortices, or both, but should theoretically be dominated by vortices (e.g., Hakim 2000). Unfortunately, Tao et al. (2017) did not specifically consider this aspect in their analysis. Given these differences, and the fact that the authors developed and applied an automatic algorithm to summarize a large sample, it would be highly beneficial if the authors provide a proof of concept example through a case study in their manuscript.

We do not assume that Arctic cyclones evolve from a tilted structure to a less tilted structure. The structural evolution is a result from the analysis presented in Section 3.4. In the previous revision we added additional text to the Methods section to acknowledge that the TPV features could be waves or vortices. That text has been reordered to reflect the changes to the methods described below and can now be found in lines 251-254.

I offer an example of a long-lived TPV and Arctic cyclones in real-time at the writing of this review (31 August) in the following figures. This also would clarify my (or another reader's) possible confusion in applying these methods. I can trace the TPV present just north of Alaska around 77_N 140_W back to at least 9 August using readily available online plots (source provided below), in which the TPV was present in nearly the same location. Thus, it has at least a 23 day lifetime and no clear lysis in sight, making the likely lifetime on the order of at least one month. The previous methodologies by Hakim and Canavan (2005), Cavallo and Hakim (2009), and Szapiro and Cavallo (2018) would likely have all identified this feature as a TPV given its fraction of time spent in the Arctic.

Figure 1 highlights a short 5-day period within this timeframe, in which this single TPV interacted with two Arctic cyclones. Both Arctic cyclones were clearly integrated with the TPV during the later stages of their respective lifecycles. However, it is clear that AC2 would

not be associated with the TPV since at its genesis time it was more than 2 degrees away from the TPV (Figure 1c). Both ACs are within 5 degrees of the TPV during around the times of maximum AC growth rates (Figure 1a,b,d). To add to the complexity, it is possible that cyclones similar to AC2 formed from asymmetries on the main TPV that were associated with smaller-scale shortwaves moving around the main TPV and over the Arctic Frontal Zone (See Figure 2 for an example of the subsequent AC forming). This makes me uneasy about how statements,
such as the one on lines 357-360, lead the reader to think that the genesis of an Arctic cyclone had nothing to do with a TPV, when it can not be ruled out that it was associated with part of the mesoscale structure of a large TPV that happened to be centered further away than a 2 degree (or even a 5 degree) distance threshold.

The reviewer here states that our method would not associate this particular cyclone (AC2) with the TPV "since at its genesis it was more than 2 degrees away from the TPV". As described in our Methods section (lines 211-224), when determining cyclones that have a **sustained association** with TPVs we match according to the distance between them at the time of maximum growth rate and time of maximum intensity of the cyclone. We do not consider the distance between the TPV and cyclone at the cyclone genesis time. Hence, the reviewer is incorrect to assume our method would be incapable of associating the cyclone and TPV in the case (s)he presents in their review.

The lines referred to by the reviewer describe results for **instantaneous** matches between cyclones and TPVs. In light of changes described below to the radii used for these matches they now state "While TPVs are near to some Arctic cyclones at their genesis time, TPVs are more likely to be near to Arctic cyclones when the Arctic cyclones are intensifying rapidly. For the maximum 10^{o} overlap radius considered, 37% of Arctic cyclones are matched with a TPV at the time of maximum Arctic cyclone growth rate compared to 30% at the time of Arctic cyclone genesis and this difference is proportionally bigger for 2 and 5^{o} overlap radius
Hence, we clearly state the criteria used for our statements. In response to the reviewer's comment though we have added a note here that "(note though that TPVs may influence cyclones from greater distances dependent on their structure (see discussion in Sect.2.2))".

This case I highlight is not an outlier case. It could be one TPV and several Arctic cyclones as shown in Figure 1, or several TPVs with one Arctic cyclone as shown in Figure 3. Either way, there is evidence that there are both waves and vortices playing a considerable role, complicating the well-known conceptual picture of baroclinic lifecycles in midlatitudes. I wonder if it is possible that the conclusion that 35 and 38 percent of Arctic cyclones are in close proximity to a TPV at maximum growth rate and intensity, respectively, and/or 10% have a nearby TPV at their genesis time, are misleadingly low due to cases like this. I appreciate that the authors have added some instances in the text pointing out that there is not requirement for sustained associations, but those statements are quite subtle. I would like the authors to address how this case would be analyzed using their current methods, and how or whether it could impact their statistics and conclusions, and to what degree.

The figures quoted by the reviewer are those in our Fig. 6 (Fig. 5 in previous submission) and are for instantaneous matches between cyclone and TPV features at the relevant times. It is possible to have a cyclone to match with more than one TPV and vice versa. For the sustained matches (when the same cyclone and TPV must match over an extended period), the cyclones are matched to the TPVs so you can get more than one cyclone matching a TPV if the TPV is long enough; this provides the best sample of cyclones. This information has been added to the paper in section 2.2.

With regard to the cross sections composites, I understand that all cyclones are oriented in the same direction relative to their motion (around line 249), and I do not have any issue with whether this impacts the matching. I also understand how the maxima vorticity at each level is projected onto the cyclone motion direction. But I am not clear on how these methods take into account \satellite TPVs" that to break off of a lobe from the main vortex or that happens to be nearby be contributing to the baroclinic growth or maintenance of a single Arctic cyclone. Given that I can find a case currently, it can not be ruled out that this happens quite commonly. Figure 3 highlights an example where one Arctic cyclone intensifies and/or is prolonged by multiple TPVs. In Figure 3a, an Arctic cyclone is intensifying from TPV1. The Arctic cyclone intensifies more rapidly when 2 TPVs are located in close proximity (Figure 3b) until the two TPVs merge (Figure 3c) and TPV2 becomes vertically aligned with the Arctic cyclone (Figure 3d).

The composites are generated using cross-sections are centred on the cyclones and so will include all upper-tropospheric features (and any TPVs) in the vicinity of those cyclones at the reference time used for compositing.

In summary, I would like the authors to please, at least, address from this case:
(a) which Arctic Cyclone and TPV pairs would be matched,
(b) which Arctic Cyclone and TPV pairs would be unmatched,
(c) how the instantaneous cross sections that are used for making the composites would be oriented. (note that latitude/longitude contours are labeled on the figures)

We thank the reviewer for pointing out this interesting very recent case to us and for using it to highlight his or her questions and concerns about our methodology. We considered applying our approach to this case, but the date is outside the range of data used in the results of the paper and also the ERA5 data were not yet available when we looked on the Copernicus website for the field of potential temperature on the 2 PVU surface which we need to identify the TPV (although the data is now available for vorticity). Hence, it is not possible to look at this case for this paper. To demonstrate our method, we also consider it more appropriate to use a case within the period we have examined (so 1979-2018). In recognition of the reviewer's many questions about our methodology we have made two important changes to the paper:

(a) We have added to the paper a new figure, Fig. 1, to illustrate the application of our criteria for a sample case. We choose a sample case that is already documented in the published literature: one related to the extreme 2016 Arctic cyclone.
(b) We have relaxed our matching criteria so that matched cyclones are now required to be within 10^{o} and 5^{o} of a TPV at the times of maximum growth rate and maximum intensity, respectively (increased from 5^{o} and 2^{o}, respectively). The criteria for unmatched cyclones have been correspondingly changed such that these are greater than 10^{o} from a TPV at both the times of maximum growth rate and maximum intensity. These criteria changes were made to address the reviewer's concerns that our original smaller matching radii may be too restrictive if TPVs and cyclones have structures that are closer to baroclinic waves than point vortices. We have also corrected a minor bug in the matching code used to select cases for the composites which has reduced the numbers of matching cases for given matching radii; consequently, the number of instantaneous matches at the time of maximum growth rate for the new 10^{o} matching radius is similar to that found previously for the 5^{o} matching radius. In practice though the changes in numbers and matching criteria have made little difference to the conclusions drawn from the figures in which the characteristics of the matched and unmatched cyclones are compared as shown in the revised paper.

Finally, in response to point (c) at the end of this reviewer's comment we are confused as, contrary to what is stated by the reviewer, we do not label longitude and latitude on our composite figures (Figs. 9 and 10).

2) My point about the spectral filtering cutoff at T63 is that the authors should at the very

least acknowledge differences from the Cavallo and Hakim (2010) method (i.e., that the TPV identification does not exactly follow their method in this sense). For example, Cavallo and Hakim (2010) use a numerical model with 30-km grid spacing, and as the authors point out in the response, the equivalent spectral resolution is about 60 km in this study. Otherwise in Cavallo and Hakim (2010), it does not appear that there is any filtering, other than the requirement that the TPVs that are selected in the statistical analysis must have a sufficiently large amplitude with respect to the spatial/background pattern. I do not know whether this will cause differences in the sample of TPVs that are identified, however, such differences in the identification method should at the very least be stated to point out that it is possible
for this to cause differences.

We have now explicitly noted that our filtering approach is different to that used by Cavallo and Hakin (2010). Also, now the revised matching radii for the composites are greater relative to the resolution implied by the spectral filtering of the data, so any separation of features greater than the criteria must be very well resolved.

More ideally, however, would be to either use the TPV tracks described Szapiro and Cavallo (2018) or perform a sensitivity test with it, since this would be more consistent with Cavallo and Hakim (2010). Szapiro and Cavallo (2018) compare their method's climatology with the earlier TPV climatology (not to mention one of the authors is the same as the earlier TPV climatology). To me, this seems like a much stronger justification.

The Szapiro and Cavallo (2018) paper published in the journal Geoscientific Model Development introduces a new software package for tracking TPVs. Hence, it is appropriate that the climatology results from the new method are compared there to the previously published climatology of Cavallo and Hakim (2010). We also compare our findings for the climatology to those of Cavallo and Hakim (2010) and other works. Please see section 3.1 of our paper – indeed this first results section was included in our paper largely to demonstrate that our climatology of both TPVs and Arctic cyclones was consistent with that of other studies while also showing, for the first time to our knowledge, a climatology generated using the ERA5 dataset. It is beyond the scope of our study to additionally use the tracks generated using the approach of Szapiro and Cavallo (2018) although, as stated in our earlier response this may be an interesting comparison to perform in a future study.

The authors pointed out in their response to the review that there are fewer citations for the Szapiro and Cavallo (2018) algorithm than for the TRACK algorithm. This is completely irrelevant and certainly not a reasonable justification. While TRACK has been used a lot, it has been used for other purposes and this is the first time with TPVs. Just because TRACK has been used or cited more in comparison does not imply anything about the consistency of the methods in identifying and/or tracking TPVs as defined in the previous literature.

As stated in the Methods section, the identification of TPV feature points is based on the approach used by Cavallo and Hakim (2010). We appreciate that the use of TRACK in many other studies for tracking cyclone features does not automatically make it suitable for tracking the identified TPVs. However, once features have been identified many papers have demonstrated the ability of TRACK to connect the features to form realistic tracks. TRACK has been developed as a general method with a lot of flexibility rather than being designed for a specific application.

Other specific comments:
1) On line 218, TPVs are further referred to as "Arctic origin" TPVs, which slightly differs from the traditional TPV definition that do not have an origin requirement but do require 60% of their lifetimes in the Arctic are north of 65_N (e.g., Hakim and Canavan 2005; Cavallo and

Hakim 2009). If this is indeed correct, then all conclusions that refer to matches between TPVs and Arctic cyclones need to have “Arctic origin" inserted before “TPV" to emphasize the slight distinction (including in the abstract).

It is correct that we require TPVs to have Arctic origin when matching them to Arctic cyclones as described in the Methodology. As can be seen in the bottom row of Fig. 2 (was Fig. 1), relatively few TPVs have genesis outside the Arctic but, as the identified features are not consistent with the generally accepted definition of TPVs (e.g. see the AMS Glossary: https://glossary.ametsoc.org/wiki/Polar_vortex), we wanted to exclude them. The clarifier “Arctic origin” has been added to the Abstract and Conclusions sections.

2) Lines 71-79: Starting with “Note that TPVs are distinct...", the remainder of the paragraph seems to have no support cited from the literature. While this may be the view of the authors, unless there is documented evidence or evidence is presented, it should not be stated.

We have added a citation to a paper by Waugh et al. entitled “What Is the Polar Vortex and How Does It Influence Weather?” to explain the larger-scale stratospheric and tropospheric vortices. It is also discussed in the AMS Glossary (see above).

3) Lines 80-81: Sentence starting with “Cyclonic TPVs..." is supported in the literature by Cavallo and Hakim (2010).

Citation added.

4) Lines 105-107: Bray et al. (2021) and Lillo et al. (2021) both offer evidence of long-lived cyclonic TPVs in the literature.

Thank you for pointing out these very recently published papers. A citation to Lillo et al. (2021) has been added as another example of very long-lived cyclonic TPVs. We have also taken the opportunity to add citations to two other very recently published papers: Capute and Torn (2021) and Lukovich et al. (2021).

5) Lines 541-542: In order to state the statement beginning... “However, this link has been explored...", it needs to be shown that this methodology works as intended. A great way would be through a case study (I highlighted an example above). If the methodology for the intense and long-lived case can clearly demonstrate the intended design, then it would make for a more convincing argument that the algorithm worked for a larger sample that contains less intense or long-lived cases. This is similar to my first major comment above.

See response to first major comment above.