General comments:
This study evaluates how often pre-existing tropopause-level features are present for Arctic cyclone development. Specifically, the 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 computing tracks of TPVs and Arctic cyclones from ERA5 based on certain flow metrics and distance thresholds. Using these methods, the authors find 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 maximum growth rate and intensity, respectively. This offers an interesting and somewhat surprising result, because the alternative explanation that is offered is that Pettersen type A cyclogenesis must be more common.

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.

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.

   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° away from the TPV (Figure 1c). Both ACs are within 5° 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 $2°$ (or even a $5°$) distance threshold.

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.

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 promimity (Figure 3b) until the two TPVs merge (Figure 3c) and TPV2 becomes vertically aligned with the Arctic cyclone (Figure 3d).

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)

Note that all analysis plots are available from http://arctic.som.ou.edu/tburg/models/. Archive mode can be found by clicking the settings tab toward the upper right corner.

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. 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 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.

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).

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.

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

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

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.

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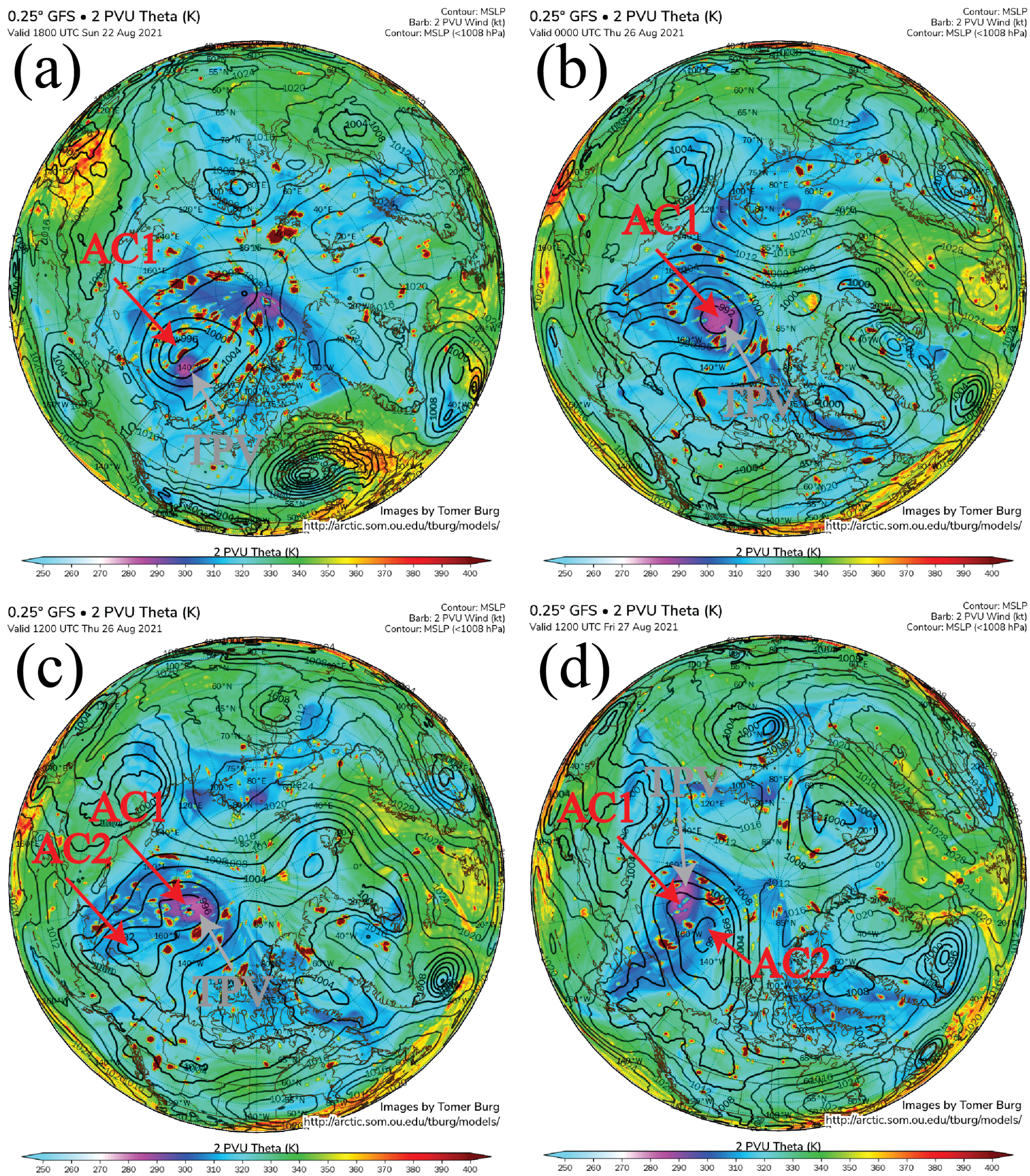

FIG. 1. 2 PVU tropopause potential temperature (colors) and mean sea level pressure (contours) from GFS analyses valid (a) 18 UTC August 22, (b) 00 UTC August 26, (c) 12 UTC August 26, and (d) 12 UTC August 27, 2021. The TPV in discussion is annotated in gray, while the Arctic Cyclones (ACs) in discussion are annotated in red. I disclose I am not Tomer Burg and I saved these images from his web page and thank him for this useful product available at http://arctic.som.ou.edu/tburg/products/realtime/models/. All data are from the GFS model.

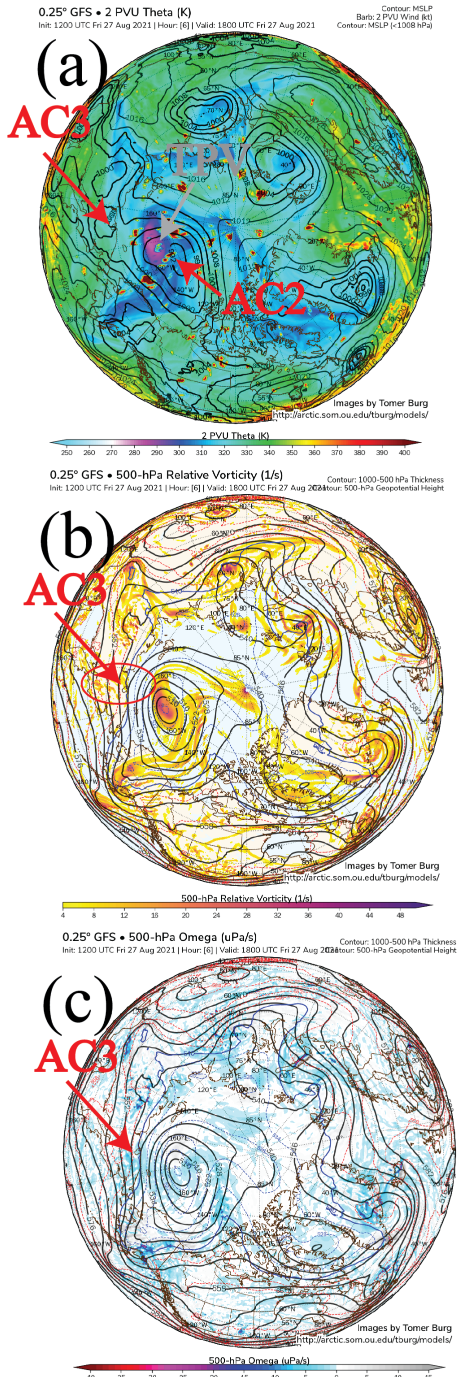

Fig. 2. (a) 2 PVU tropopause potential temperature (colors) and mean sea level pressure (contours), (b) 500 hPa relative vorticity (colors), heights (thick black contours), and 1000:500 hPa thickness (dashed color contours), and (c) omega (colors), 500 hPa heights (black solid contours) and 1000:500 hPa thickness (dashed contours) from GFS 6-h forecasts valid 18 UTC August 27. The TPV in discussion is annotated in gray, while the Arctic Cyclones (ACs) in discussion are annotated in red. A shortwave moving around the TPV is circled in red in panel (b). I disclose I am not Tomer Burg and I saved these images from his web page and thank him for this useful product available at http://arctic.som.ou.edu/tburg/products/realtime/models/. All data are from the GFS model.

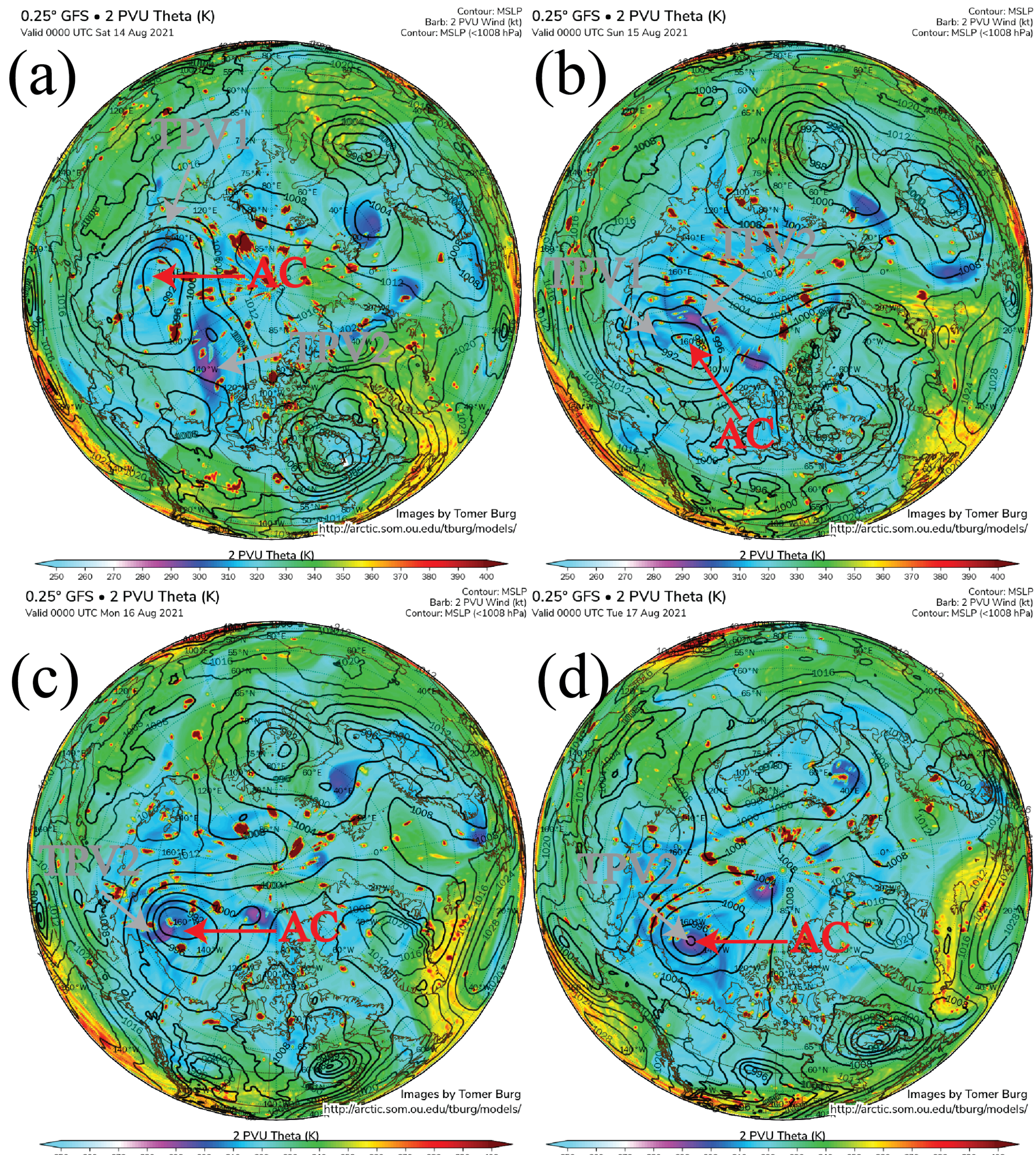

Fig. 3. 2 PVU tropopause potential temperature (colors) and mean sea level pressure (contours) from GFS analyses valid 00 UTC (a) August 14, (b) August 15, (c) August 16, and (d) August 17, 2021. The TPV in discussion is annotated in gray, while the Arctic Cyclones (ACs) in discussion are annotated in red. I disclose I am not Tomer Burg and I saved these images from his web page and thank him for this useful product available at http://arctic.som.ou.edu/tburg/products/realtime/models/. All data are from the GFS model.