

Weather Clim. Dynam. Discuss., referee comment RC3
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Comment on wcd-2021-30

Anonymous Referee #3

Referee comment on "The role of tropopause polar vortices in the intensification of summer Arctic cyclones" by Suzanne L. Gray et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-30-RC3>, 2021

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:

- 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.

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.

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.

Specific comments:

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

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.

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.

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).

- 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

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

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