Review of wcd-2022-60

"The role of boundary layer processes in summer-time Arctic cyclones"
by
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<u>Recommendation:</u> Major revisions

<u>General Comments:</u>

The Manuscript investigates the development of Arctic cyclones and in particular the role of non-conservative processes in the boundary layer on the overall system. The authors chose an integral PV diagnostic to analyze the development as well as standard synoptic analysis. The manuscript is well written, and the figures are of good quality. While the topic is of general interest, with an increased focus on Arctic cyclones in the community, the chosen diagnostic would benefit from being put in a more physical context. Also, the specific choice of the diagnostic for the question at hand should be argued for in more detail, as other studies employed less complex analysis tools to address similar problems. Also, the potential added value of the integrated PV should be made clearer, given its rather complex nature compared to other tools, see also comments below.

One of the key findings appears to be the role of the low-level temperature structure in the cyclone development. This finding is rather interesting and appears to also explain some of the open questions about Arctic cyclones, e.g., their longevity. However, given the number of other items discussed in this manuscript as well as the complex and manifold nature of the presented material, this key finding gets a bit "lost" in the overall structure of the manuscript. Furthermore, the diagnostic tools needed to discuss this finding might not demand the complexity of the diagnostic tools chosen by the authors. It might be worthwhile to focus more on this specific aspect of the low-level temperature structure while toning down some of the other more descriptive parts addressing the development of the cyclones.

The introduction is rather long and might benefit from some rearrangements. Some aspects could be moved into other sections, for example some paragraphs in the introduction already address the methodology of the work. Related to the previous comment. If the manuscript would focus more on a specific aspect and finding, it would potentially also allow to have the introduction being more focused in that direction.

The authors should further motivate their choice of a PV framework over, e.g., an energy framework. Furthermore, investigating the impact on PV in the boundary layer is rather questionable, as the usual balance assumption breaks down in these regions that would be needed to make inferences about the implications of the changes in PV. The authors should clarify why PV should be a good and variable choice for such highly unbalanced and turbulent environments, as the main inferences about the implied development are associated to balanced PV thinking. Relatedly, in lines 426 and following, the authors state that "it is difficult to say how the BL PV tendencies contribute to the tropospheric depth-integrated circulation evolution". Given this difficulty, what is the actual benefit of this rather complex framework when trying to assess the influence of surface processes on the overall cyclone development?

The definition of the boundary layer height, upon which most of the PV-related arguments rely, is not further clarified and just given as h. How is it determined? Furthermore, the tendency of the boundary layer height is also used in the diagnostic without having clarified how this quantity is derived. The authors should specify the definition of the boundary layer height and how its tendency was calculated. Related, it would be of interest how much this boundary layer height varies within the cyclone and over the course of its lifecycle, as this has implications on the relative contributions in the PV budget.

Also, as the main interest appears to be in the horizontal circulation, i.e., the vertical component of the vorticity, it is not clear why the full three-dimensional version of the PV calculation is used. In fact, the authors also argue for the relevance of the horizontal temperature gradients, thereby including the horizontal vorticity vector component projecting onto the tilted isentropes. However, this part of the PV does not contribute to the horizontal circulation. The more tilted the isentropes, the less the PV is equivalent to the circulation one would associate with a cyclone. The authors should thus further clarify the role of these components in their argumentation. Transporting PV alone, especially if a large fraction is associated with steep isentropic slopes, cannot be directly related to changes in the horizontal circulation in a cyclone. To relate that type of PV to this kind of circulation, one would need to have significant tilting of the isentropes, to make them flatter again. It is likely that this process is involved in what is described as boundary layer ventilation in lines 116-118.

Explaining Ekman pumping in a balanced steady-state and PV-conserving framework is also a bit misleading. While the presented reasoning is self-consistent, the reduced stability in the boundary layer could have also been due to sensible heating from the surface or wind-induced mixing of the stratification. In that case, one could have attained a reduction in PV in the boundary layer without any inferences and implications about the layers aloft, i.e., no increase in stability aloft and implications for circulation, under the assumption of conserved PV. The authors should further substantiate their arguments, especially as the term Ekman pumping is used several times in the results and discussion.

Regarding the effects of surface heat fluxes, there have been recent idealized studies addressing the effects of surface fluxes on cyclone development. The authors are encouraged to put their reasoning and findings in context with these studies, which are based on both PV and energy arguments (e.g., Haualand and Spengler, 2020; Bui and Spengler, 2021). Furthermore, the authors neglect the diabatic effects of latent heating in the free troposphere. The authors should further justify this neglection, especially as latent heating often plays a significant role in cyclone development. Even though Arctic environments often feature less absolute humidity, the effects on polar cyclones can still be significant and dominant (e.g., Terpstra et al., 2015).

Regarding the depth-integrated circulation, it appears that the authors implicitly assume that the isentropes are quasi-horizontal, which contrasts with the previous emphasis of circulation projecting on rather tilted isentropic surfaces. In the extreme case of almost vertically oriented isentropes, the approximation in (12) is misleading. If the authors are mainly interested in the circulation associated with the horizontal wind components in conditions of rather flat isentropes, the previously argued relevance of the other components in the PV should be further explained and put in context. Furthermore, density multiplied by PV is just vorticity projected onto the isentropic surface. So, the integral over an area, which would need to be level with the isentrope, would correspond to the circulation. Once isentropes feature a significant tilt, this equivalence is not exact anymore. Especially in the boundary layer, isentropes can be significantly tilted and it becomes questionable what this measure really represents. The authors should further comment on the implications of rather tilted isentropic surfaces for their diagnostic, which would become especially relevant in the boundary layer.

<u>Specific Comments:</u> (Reference to line numbers in the manuscript)

L41: Statement on previous work in that sentence needs a reference.

L61-81: This paragraph is rather long and a bit difficult to parse for the reader. Consider splitting by topics. It appears the first half focuses on TPVs, whereas the latter half makes inferences about lower and upper vorticity structures as well as warm and cold core lows.

L82-89: The paragraph first emphasizes the role of surface fluxes, but then only comments further on frictional aspects, where surface sensible and latent heat fluxes have been discussed more recently. In addition to many case studies addressing sensitivities to surface fluxes, Haualand and Spengler (2020) introduced the concept of direct and indirect effects of surface sensible and latent heat fluxes, see also Bui and Spengler (2021). It is not clear why the authors mainly emphasize the momentum fluxes.

L91: It is correct that several studies have used PV, but at least equally many have also used other measure, especially an energy framework related to the Lorenz energy cycle. The authors should provide further arguments for favoring their choice over, e.g., an energy framework.

L101: How can the authors infer that isentropes must have risen from balanced arguments? While the presented reasoning is self-consistent, the reduced stability could have also been due to sensible heating from the surface or wind-induced mixing of the stratification. In that case, one could have attained a reduction in PV in the boundary layer without any inferences and implications about the layers aloft.

L102: Why should PV be conserved in such a dynamic environment in or near the BL? And why should these layers have responded in the first place, see comment above.

L108: The association is less to horizontal temperature gradients, but to the fact that the isentropes have a significantly enough tilt so that the horizontal vorticity vector components sufficiently project onto them. Of course, there is an equivalence between the temperature gradient and the tilt of the isentropic surfaces, which will also depend on the stratification. The authors could further clarify their reasoning.

L170: How can the authors ensure that 925 hPa is above the boundary layer?

L259: How do the authors justify the assumption that there are no non-conservative processes in the free troposphere? Given that they are interested in cyclone development, diabatic processes associated to latent heat release are expected to occur in the free troposphere.

L268: See comment above about neglecting non-conservative processes in the free troposphere.

L417: The role of latent heating in the free troposphere is largely ignored in this manuscript. The authors should further justify this neglection and try to quantify it.