Review of wcd-2022-60

'The role of boundary layer processes in summer-time Arctic cyclones"

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Paper in review in Weather and Climate Dynamics Discussion

1 General Comment

This study analyses the role of boundary layer processes for two strong Arctic cyclone case studies that track over sea ice. Both cyclone differ with respect to their structure, i.e., a warm-core and a cold-core Arctic cyclone are selected. A potential vorticity (PV) framework for PV tendencies in the boundary layer is applied, which accounts for friction (Ekmann friction and baroclinic PV generation) and sensible heat fluxes (vertical and horizontal gradients) in the boundary layer. Application of the PV framework facilitates the quantification of the effect of boundary layer processes on Arctic cyclone development. The applied framework is described well in the Methodology, and minor shortcomings of the method are discussed throughout the manuscript (e.g., PV fluxes into or out of the control volume, neglect of diabatic heating). Instantaneous PV tendencies and boundary layer averaged PV tendencies for both cyclones show that both cyclones are characterized by frictional Ekmann pumping and downward sensible heat fluxes, while frictional baroclinic PV modification differs between both cyclones and is positive for the warm-core and negative for the cold-core cyclone, respectively. In both cyclones it cools the lower troposphere, i.e. enhances the cold-core and decreases the warm-core anomaly. Although both cyclones differ with respect to their structure at maximum intensity, they both develop a persistent cold-core structure, consistent with Ekmann pumping, frictional baroclinic PV generation and downward sensible heat fluxes over sea ice, which may explain the longevity of cold-core Arctic cyclones.

The manuscript is clearly structured, comprehensive and includes well-defined research questions and the respective conclusions. It is also very well-written. The length of the manuscript is justified as it includes very comprehensive explanations of processes that act in the boundary layer. The figures could be slightly improved (see detailed comments below).

I recommend the publication of this manuscript but I have several questions that should be addressed prior to publication.

1 Introduction
The introduction is relatively long (four pages), but I find it well-written and comprehensive, and the general concepts are well explained. Thus, the length of the introduction is justified in my opinion.

2 Key questions
The introduction leads towards the clearly stated key questions. I appreciate the clear formulation of the goals of the study. Yet, in my perspective, question 3 is not a key topic addressed in this study and should be re-

moved from the key questions. Throughout Results and Discussion, the structures from cyclones A and B are compared to what is known about mid-latitude cyclones from literature, however, the methodology is not explicitly applied to a mid-latitude cyclone in this study - which is what I would expect from question 3. Nevertheless, I find the references to mid-latitude cyclones throughout the manuscript useful.

3 Boundary layer height definition
How is the boundary layer top determined? The framework hinges on the determination of the boundary layer top. Could the authors please elaborate on how this was determined? Do the BL tops in both cyclones show a similar height, and would differences in the BL heights influence the diagnostics?

4 Presentation of figures
Generally, the figures are clearly presented. However, as the core focus lies on the comparison of cyclone A and B, I would appreciate if the authors could consistently place the corresponding figures next to each other for better comparison. I would also suggest to plot the cyclone phase-space (Figs. 2b and 4b) directly next to each other. Are cyclone phase-spaces in the IFS similar as in ERA5 (which is shown only)? Could this additionally be shown in the figure? The authors mention a lot of dates in the manuscript. Instead of referring to the date (e.g., caption Figs. 2 and 4), it would be easier for the reader if the authors would consistently refer to time of max. growth rate (pink) and max. intensity (blue). These time markers could also be potentially highlighted in a legend in the figure. Moreover, Figures 8 and 9 were better to compare if placed directly next to each other, e.g., as 2x2 panel figure. The different y-axis scaling may also be misleading at first glance. I find it difficult to identify the PV structures and respective differences in Figs. 10 and 11, and would suggest to adjust the colorbar. I appreciate the clear schematic shown in Figure 12. It may be helpful for the reader to additionally mention the respective effects on baroclinic PV modification in the figure (either in the caption or indicated in the schematic). It may also be helpful to already refer to Fig. 12 in l. 240f and l. 367.

5 Role of F_{BG}
The authors emphasize the different impact of F_{BG} on both cyclones A and B which are characterized by a low-level dominant warm-core and upper-level dominant cold-core structure. In the maps in Figs. 6,7 the differences are present near the cyclone centers, however, I find it difficult to justify the emphasized "large" differences based on the time evolution of the cyclone-area integrated budgets, as both time series (Figs. 8 and 9) show an initial positive F_{BG} tendency, and a subsequent negative F_{BG} tendency - albeit with slightly different timing relative to max. intensity and differing magnitude. I wonder if the large positive values are related to processes over land (see l. 372f, and Figs. 6,7)? Were grid points over land removed from the cyclone-area integrated budgets? I wonder if this would change the evolution of PV budgets, as the values over land are substantially larger than over sea ice, which is the key focus of this study (see also next comment). Yet, I appreciate the in-depth theoretical

consideration and schematic of differences in F_{BG} between cold-core and warm-core cyclones (Discussion and Fig. 12).

6 Cyclone depth-integrated circulation budget
Figure 6 shows that very large PV tendencies occur over land and coincide with the 750-km cyclone mask. Are PV tendencies over land removed from the PV tendency budget over the cyclone area show in Figs. 8-9? If a larger fraction of the cyclone mask tracks over land at certain time steps, one could imagine that the PV tendencies over land dominate the averaged PV tendencies. I wonder if the large PV tendencies for cyclone A in the early phase are related to its initial tracking over land? Are large PV tendencies over land relevant or irrelevant for the cyclone evolution if they are somewhat spatially decoupled from the cyclone center? Moreover, I find it difficult to compare the evolution of PV tendencies between both cyclones (Figs. 10 and 11). I also wonder if there is a systematic relation between the surface type and the magnitude of PV tendencies? The spatial and temporal variability in PV tendencies within each cyclone are very large, such that I find it difficult to identify the systematic differences between both systems. If the PV tendencies and cross-sections are averaged over several hours (i.e., 6-12 hours after time of max. growth rate), are some of the large-variability features smoothed out and a clearer signal emerges?

7 Discussion
The discussion summarizes the results and theoretical concepts, and explains how F_{BG} acts differently in warm-core and cold-core cyclones and is associated with a cooling in both cases. I appreciate the detailed discussion which helps to put the results into context.

2 Specific comments and technical corrections

1. l. 89-90: Could the authors please elaborate on the concept of Ekman pumping in cyclones in the introduction, in particular the convergence at BL top and associated vortex squashing?

2. l. 216, 565: After introduction of BL as abbreviation for boundary layer, please consistently use "BL" instead of "boundary layer".

3. l. 306: "This can also be seen spatially in Fig. 2a." This sentence can be removed if Fig. 2a is referenced in the sentence before. The same applies to l. 326 "The cyclone tracks are presented in Fig. 4." .

4. l. 336ff: Could the authors please elaborate on the respective implications of the different temperature variations?

5. l. 342: Typo, please add "the" to "In cross-section".

6. l. 403: I appreciate that the authors mention the y-scale difference between Figs. 8 and 9, but I think that this difference makes it difficult to directly compare the individual tendencies (see also general comments 4 and 6).

7. l. 406f: "The time series of F_{BG} for Cyclone B is considerably different to that of Cyclone A at and after maximum intensity, indicating that this is an important term to understand." From comparison of both time series, I find it difficult to conclude that F_{BG} is most different, as many tendencies appear to differ substantially. Similar to the previous comment, it may help to use the same y-axis scaling for both figures. Following up on general comments 5 and 6, I wonder if there is substantial influence from interaction with land surfaces.

8. l. 463: Typo, "to to".

9. l. 464: The authors mention the larger tendencies over ocean (and previously over land), and from Figs. 10 and 11 it appears that the surface type may have an influence on the boundary layer tendencies. Do the authors find a systematic difference, and if so, how does it influence the results?

10. l. 464-465: Did the authors check if the cyclone track is not co-located with the SLP minimum?

11. l. 524: Could the authors please elaborate on the term "equivalent barotropic"?

12. Figs. 2 and 4: Please merge both tracks in one figure or combine in one large 2x2 panel figure for direct comparison of tracks A and B.

13. Figs 3a,b and 5a,b: I find the figures too small, and the colorbar for Fig. 3b is not ideally adjusted to the region of interest.

14. F_{BG}: In the figure legends, the F_{BG} term is referred to as F_H, and the F_{EK} term as F_V. I would ask the authors to make the labeling consistent.