Weather Clim. Dynam. Discuss., referee comment RC2
https://doi.org/10.5194/wcd-2022-60-RC2, 2023

Comment on wcd-2022-60

Anonymous Referee #2

Referee comment on "The role of boundary layer processes in summer-time Arctic cyclones" by Hannah L. Croad et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-60-RC2, 2023

This paper discussed the mechanisms by which the atmospheric boundary layer affects the evolution of Arctic cyclones. This is an interesting topic, which to my knowledge has not been previously studied in a similar manner, i.e. from a potential vorticity perspective. The paper is well written and the results are interesting. I have no significant concerns, just a list of minor comments or questions which the authors may wish to consider.

Comments:

L111-112 - it might be worth mentioning Boutle et al. (2007, QJ) here, as that was really the first paper to discuss both the Ekman and baroclinic mechanisms acting within the same simulation (Adamson et al. mentioned both, but don't really discuss the Ekman mechanism at all, focussing on the baroclinic mechanism as the spin-down process). I suggest this because there are several places where I think the similarities/analogies to mid-latitude cyclones are slightly stronger than stated, but not all mid-latitude cyclones look like those of Adamson et al. which is the main basis for comparison. In particular:

- L568-9 - this distinction reminds me of the Type A vs Type B classification of mid-latitude systems of Petterssen & Smebye (1971), with Type A being the "true baroclinic" systems, similar to your Cyclone A and that explored by Adamson et al., whilst Type B is "upper level dominated", similar to your Cyclone B and that explored by Boutle et al. (2007 & 2015).
- In particular, the experiment of Boutle et al. (2007) which uses a spatially uniform sea-surface temperature then becomes conceptually even more similar to your Cyclone B, where the sea-ice is providing the quasi-uniform surface temperature which limits the low level baroclinicity.
- This is relevant for the baroclinic PV generation, where Boutle et al. (2007)'s Fig 3b (compared with your Fig 7b) shows a region of negative PV generation via this mechanism in the cyclone's warm sector. It's not explored, but I strongly suspect its

creation is via the same processes you describe for Cyclone B.

- It is also relevant for the surface heat flux PV generation, which their Fig 1b (compared with your Fig 7c) shows can provide a dominant term in the PV budget when the surface fluxes are strong (& negative) enough without much spatial variation of the surface temperature.
- L345-346 - it does have some similarity to that presented in Fig 2a of Boutle et al. (2015), i.e. the tropopause PV structure almost reaching down to the boundary layer and interacting with the BL generated PV.

I should be clear that I don't think any of this detracts from the work that you've done, but just helps with some of the comparisons to mid-latitude systems.

Section 2.5 - it's nice to present this to the reader, but it's not really new - just a repetition of what is already presented in other cited papers. You could possibly move it to an appendix to shorten the main paper and get to your own results quicker?

L259 - is there no latent heat release happening at mid-levels within these systems? It would be good to clarify whether it is indeed small, or whether you are just ignoring it for simplicity.

Table 1 - do you also only have 6 hourly data from the IFS (you state that you only have that from ERA5)? As it might be worth mentioning if so, as it probably exaggerates the differences between the two for max growth rate/intensity etc - I would hope the difference might be less if you had more frequent data, but 6 hours is the minimum difference you can 'resolve'. The statement on L300 also might not be true if you had more frequent data.

Figs 6 & 7 - not sure if it's worth pointing out somewhere that in Fig 6, the Ekman generation is almost exactly cancelled by the heat-flux generation, leaving the baroclinic generation as the only significant contributor to the total. This is another significant difference from Fig 7, where there is definitely some Ekman generation left in the residual

Figs 8 & 9 - the labels here don't match the text - I think you're using Fv rather than Fek and Fh rather than Fbg?

L410 - there must be a mistake here, as you're comparing Fbg against Fv, which I think are the same - do you mean Sv or Fek?

Reference:

Boutle, I.A., Beare, R.J., Belcher, S.E. and Plant, R.S. (2007). A note on boundary-layer friction in baroclinic cyclones. Q. J. R. Meteorol. Soc., 133, 2137-2141, doi:10.1002/qj.179

Petterssen S, Smebye SJ. 1971. On the development of extratropical cyclones. Quarterly Journal of the Royal Meteorological Society 97: 457–482