Weather Clim. Dynam. Discuss., referee comment RC1
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Reviewer comment on wcd-2022-35

Anonymous Referee #1

Referee comment on "Cloud-radiative impact on the dynamics and predictability of an idealized extratropical cyclone" by Behrooz Keshtgar et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-35-RC1, 2022

This study investigates the impact of cloud radiative heating on the dynamics and predictability of extratropical cyclones using baroclinic life cycle simulations in a convection-permitting model. The simulations apply a new modeling technique which isolates the radiative impacts on the cyclone. The simulations show that radiative heating increases the kinetic energy of cyclones, most prominently at upper levels, through the intensification of latent heating from cloud microphysical processes and subsequent feedbacks on the large-scale flow.

Overall, this is a very well-written study and is appropriate for publication in *Weather and Climate Dynamics*. My one major criticism of this study is that the disagreement of the results with Schäfer and Voigt (2018) casts doubt on the usefulness of its conclusions, and in my opinion, warrants some sensitivity testing within the authors' model experiments. Otherwise, I only have minor revisions to suggest.

Major Revision:

The disagreement of the results with Schäfer and Voigt (2018) is concerning, especially because, in unpublished work, the authors have found that the model version affects the sign of the influence of cloud radiative heating on the intensity of the cyclones (lines 155-160). While the authors argue that this is a topic for another study, I think it's important for readers to know how sensitive the key conclusions of this study are to small changes in the configuration of the model. For example, if the authors re-ran their channel set-up with a different shallow convective or microphysics scheme, would they reach similar conclusions? A two-moment microphysics scheme may be more appropriate for the mixed-phase clouds that occur within extratropical cyclones. If switching out parameterization schemes is difficult to do within ICON, then perhaps the authors could re-run their channel set-up using the older version of the model used by Schäfer and Voigt (2018). At least this would quantify whether some of the differences in the conclusions between the two studies were caused by the differences in methodology (removing the

clear-sky radiative influence) rather than using a different model version. With the current state of the manuscript, it's very difficult to reconcile the differences between the two studies because of the different model versions and different methodologies used.

Minor Revisions:

Lines 46–49: Another relevant study to discuss is Grise et al. (2019), who examined the impact of cloud radiative effects on the extratropical storm tracks using the cloud locking procedure in a comprehensive global climate model. They reached a similar conclusion to Schäfer and Voigt (2018), that cloud radiative effects damp the intensity of extratropical cyclones.

Grise, K. M., Medeiros, B., Benedict, J. J., & Olson, J. G. (2019). Investigating the influence of cloud radiative effects on the extratropical storm tracks. *Geophysical Research Letters*, 46, 7700– 7707.

Lines 104–106: The plotting conventions are not entirely clear. If the channel width is 81 degrees latitude, what latitude is the middle of the channel? It seems like it should be 45 degrees since the Coriolis parameter is set at this latitude, but 45 degrees is not the midpoint on the y-axis in the figures.

Line 161: How do you define total precipitation rate and cloud cover? Averaged or integrated over what domain?

Line 237: Why isn't the boundary layer heating and cooling dipole from longwave CRH present in the cross sections in Fig. 6? Does this come from other sectors of the cyclone than the warm conveyor belt? If so, which ones?

Line 319 (Equation 6), figures in section 4, and Fig. 15: The sign convention here is really confusing, as the previous figures were CRH – REF, rather than REF – CRH. It took me a long time to figure out why the signs were opposite in Fig. 12f and Fig. 5h. Please use the same sign convention throughout the paper to avoid confusion.

Line 371: It's hard to see this based on Fig. 10b alone. The inset in Fig. 10b looks like the cloud radiative and latent heating contributions are roughly equal and opposite. It may be necessary to refer to Fig. 11 here.

Lines 398-400: The differences in vertical motion appear to be displaced eastward from

the differences in divergent flow. They are not co-located.

Figure 6: It would be helpful to show the approximate location of this cross section on Figure 1.

Figure 8: For completeness, why isn't the 4–6 km layer (where the positive PV tendency due to latent heating is largest) included on this figure?

Figure 13: It would be helpful to provide a different color bar for panel a.

Typos

Line 59: warm conveyor belts

Line 179: The reversal of the meridional PV gradient appears to occur in the western half of the domain.

Line 258: simulation