Review of «Cloud-radiative impact on the dynamics and predictability of an idealized extratropical cyclone" by B. Keshtgar, A. Voigt, C. Hoose, M. Riemer and B. Mayer

In this study the authors investigate the effect cloud radiative effects (CRE) on the intensification of an idealized extratropical cyclone. The impact of radiation on mid-latitude dynamics is not fully understood. Therefore this paper is a valuable contribution to the scientific literature. The authors find that the influence of the CRE is not caused by a direct modification of potential vorticity in the cyclone but by modification of the latent heat release by cloud microphysical processes which in turn change the divergent wind especially at tropopause levels. The influence on the lower troposphere is much smaller. The paper is clearly written, and the results and conclusions are mostly supported by the figures.

The paper is suitable for publication after minor revisions which are mainly related to the figures which I sometimes found hard to understand. Another concern is that the authors find the opposite impact (intensification of the cyclone by CRE) of CRE on an extratropical cyclone as the study by Schäfer and Voigt (2018) and it is not fully clear how sensitive the presented results are to differences in the model setup. Some more discussion or testing on that would be helpful for the reader.

Detailed comments:

Chapter 2.2 Simulation design: I have some difficulties do understand your simulation design. First you say that in Schäfer and Voigt (2018) two simulations have been performed whereas in one simulation, the radiation is switched off completely. This changes the initial conditions under which the cyclones form in the channel and therefore the effect of CRE on cyclone dynamics cannot easily be investigated with this setup. In contrast, in your setup you perform one simulation where radiation is completely switched off (REF) and one simulation where radiation is switched on but the dynamical core only sees the cloud-related part but no clear-sky part. So is it correct that before clouds are forming in your simulation it should be (almost) equal to REF? Or is the REF simulation with clear-sky radiation and clouds set to zero and the CRH simulations include the CRE ? I am also then confused by your sentence that you need to call the radiation scheme twice? Can you maybe rewrite this paragraph and add more information? I would appreciate that a lot.

L. 65 ff: Research questions: I find the first research question quite broad and unspecific. I think it would be nicer to formulate a clearer question. What is "strongly"? In terms of what? Maybe you want to include something about "cyclone intensification and/or cyclones eddy kinetic energy"

Fig. 5: e-h) You say that you have a substantial PV difference at day 7,8, which is true. Do you also know if the isentropic PV gradient across the tropopause is changing and if/how the wind speed is influenced at tropopause levels?
Fig. 5: i-l) You say that in the lower levels the changes are small. They are small in terms of horizontal extent but the amplitude is also considerable. I think that the signal along the cold front is interesting and I would be curious to see where it comes from. Is it PV production below cloud base heating? Or something else? Stronger PV production due to

increased latent heating? Or both? A stronger PV anomaly along the cold front could also lead to an increased northeastward low-level wind in the cyclones warm sector ahead of the cold front which could change the moisture supply to the WCB. Do you have any idea if this anomaly might also be of interest and have an impact?

L 193: You say that the PV differences indicate a deeper tropopause fold. I don't know where you can see that. Could you please clarify this?

L 210 ff, Eq.3: As in your later analysis you also show the turbulent PV tendencies, wouldn't it make sense here to write the full PV tendency equation including also the momentum tendencies? Especially because you show them in Fig. 8. Additionally, you say that the assumption that the vertical gradient is dominant is typically justified. However I'm not so sure if this is the case in your high resolution 2.5 km simulation and especially not in the PBL and tropopause region. Can you comment on this? And can you clarify how exactly you calculated the PV-tendencies? Did you use the vertical approximation or did you calculate it based on all three components?

Fig. 7: where does the very strong cooling in the lowest model levels in the cloud microphysics come from? Is it rain evaporation?

Fig. 9: In Fig. 9a there are very strong negative tendencies along the 2 pvu line from the CRH simulation, however I can't see where they come from Figures 9 b,c, and d. Can you comment on that?

L 366: You say that the sum of the cyan (diagnosed) and black (difference potential enstrophy) match well. However isn't the diagnosed contribution (cyan) line twice as large as the black one between days 6.5 and 9 or even has an opposite sign at the end? Can you maybe comment more detailed on that ?

Fig. 10: I am confused when comparing Figures 10 a and b. The dark blue line showing the diabatic contribution in Fig. 10a should also be visible in Fig. 10b, e.g. equal the red (or black?) line in Fig. 10b? Can you please clarify and also add to the figure caption what exactly is shown? What exactly is the black (total) line and the red (total latent heating) in Fig. 10b? And is one of these lines also visible in Fig. 10a? Also add that the values in the Fig. 10b are one order of magnitude smaller (10-6 in 10a vs. 10-7 in 10b).

L 397: you say that the differences from the near-tropopause divergent flow are co-located with differences in the vertical motion (Fig. 12c,d). However the difference in the divergence is shifted to the west compared to the differences in the omega field. Please describe more careful and/or explain why they are not co-located.

Fig. 12: What exactly is shown here? Is it the difference REF-CRH or CRH-REF? And what isentropic/pressure level. Please add missing information to the figure caption. And how does it compare to Fig. 5 which shows the opposite changes? Please clarify.

Fig. 15: You say in L 436 that CRH leads to more latent heat release (Fig. 15b). I cannot see that? Can you explain in more detail what you mean here and how your statement is supported by your figure?

Additional corrections:

L.. 53: …convective heating (Fovell et al., 2016), *and* Ruppert et al. (2020) ….

L. 66: how strongly *does* cloud-radiative heating

L. 158: This shows that model differences…. → This shows that differences in the representation of clouds and their radiative heating in models can ….

L 176: …strong PV *gradients* that *separate*

L. 191: Higher PV east of the trough center (blue colors) and lower PV at the tip of the ridge (red colors) ….

L 200: resulting in higher intensity and delayed intensity peak time. What intensity are you referring to? Can you be more precise?

L 210, Eq.3: vectors in bold or with arrow above

L 227: …due to the evaporation of ~~the r~~ain and snow melting.

L229: …as shown in Fig. 6 e,f, g.

Fig. 6: I would prefer to have the units K/h and pvu/h instead of per second.

Fig. 8: pvu /h instead of pvu/s ?

L 246: PV tendency. *But*, at lower….

L 248: … by *the* longwave CRH …

L 255: …heating rates from turbulence, convection….
I assume that the convective heating rate here only assumes heating from the parameterization of shallow convection because deep convection is resolved? Could you please clarify?

L 257: the part starting from: …"and are shown for the CRH simulation in the first row and their differences with the REF simulation in the second row" does not belong in the text but in the figure caption.

L 338: …leads to the best results. “Best” in terms of what? Please rephrase.

L 363: …controls the near tropopause PV gradient: Does it control the PV gradient and/or the PV shape/distribution at upper levels?

L 366:….spatially integrated tendencies, shown in Fi. 10.

L 372: …the diabatic impact *between days 4.5 and 5.*