Cloud-radiative impact on the dynamics and predictability of an idealized extratropical cyclone - Response to Reviewers

Behrooz Keshtgar, Aiko Voigt, Corinna Hoose, Michael Riemer, and Bernhard Mayer

We thank the reviewers for their evaluations, questions, and suggestions to improve our manuscript. Below, we respond to each of the reviewers' comments and describe how we plan to adapt the manuscript. We are confident that we can address the reviewers' comments in the revised manuscript, and we are hopeful that the revised manuscript will be acceptable for publication. The reviewers' comments are in bold, our answers are in normal font.

Reviewer 1

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The disagreement of the results with Schäfer and Voigt (2018) is concerning, especially because, in unpublished work, the authors have found that the model version affects the sign of the influence of cloud radiative heating on the intensity of the cyclones (lines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.

We agree with the concern regarding the sensitivity of the results to changes in the model setup. However, we are also convinced that the sensitivity does not question the results of our paper because of two arguments. These are described in detail in the following.

First, to study the sensitivity and to better understand the cloud radiative impact on cyclones, AV and BK were supervising a Master thesis that was done in parallel to the work described in our paper (Butz, 2022). The thesis is published at the library of University of Vienna (https://phaidra.univie.ac.at/open/o:1539084).

In the thesis, the same global model setup for the simulation of idealized baroclinic life cycles was used as in Schäfer and Voigt (2018), and the cloud radiative impact was compared between two model versions, ICON 2.1 (which is essentially the same model version as that used by Schäfer and Voigt (2018)) and ICON 2.6 (which is the model version used in our manuscript). These simulations showed that the model versions simulate similar cyclones when radiation is not taken into account, but that enabling cloud-radiation interaction leads to the strengthening of the cyclone in ICON 2.6 and weakening in ICON 2.1. Further analysis described in the Master thesis showed that the version dependence of the cloud-radiative impact is due to a bug within the physics-dynamics coupling of the turbulence scheme (Zängl and Schäfer (2021)) in ICON 2.1 and ICON 2.0 used by Schäfer and Voigt (2018): in this model version, the surface latent heat flux was too high, creating an artificially moist boundary layer and a much higher low-level cloud cover compared to ICON 2.6. The thesis also showed that the high low-level cloud cover in ICON 2.1 is responsible for the weakening of the cyclone, whereas the cyclone strengthens in the global setup of ICON 2.6. The cyclone strengthening in the global setup is in line with the cyclone strengthening that we find in the channel setup of ICON 2.6. We intend to publish the detailed results of the master thesis in a separate article soon.

Second, motivated by the reviewer's comment, we have repeated our 2.5 km channel simulations using a two-moment microphysical scheme instead of a one-moment scheme. These simulations are depicted in Figure 1 showing that the sign of the cloud radiative impact is robust regardless of the microphysical parametrization. We will add these simulations to the revised manuscript.

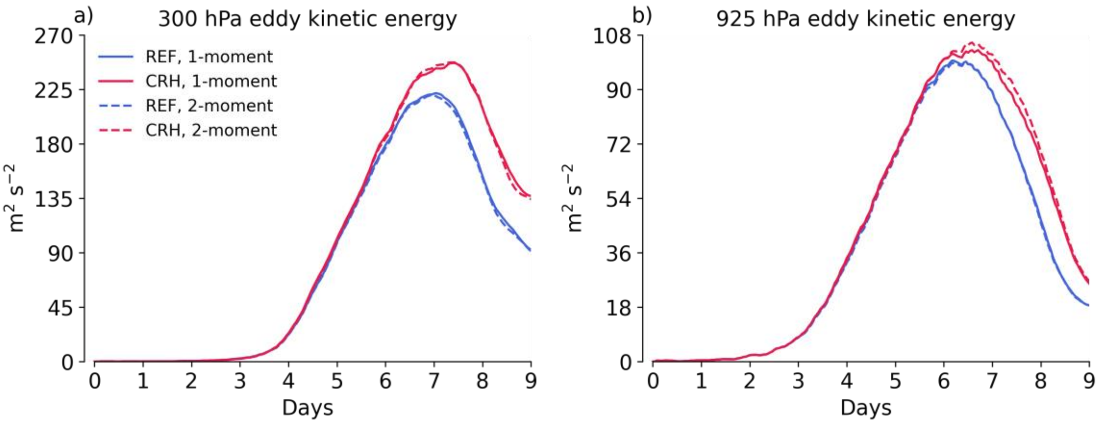

Figure 1: Evolution of the eddy kinetic energy at (a) 300 hPa and (b) 925 hPa for simulations with no radiation (REF) and with only cloud-radiative heating (CRH). The dashed lines are for simulations with 2-moment microphysics.

Finally, we would like to note that the cloud radiative impact on extratropical cyclones' dynamics and predictability remains poorly understood and has received very little attention. In light of the dependence of the cloud radiative impact on the ICON model version, it seems possible - and in fact not unlikely - that other models might show a different sign and magnitude of the cloud radiative impact, and that the cloud radiative impact might be case dependent. We thus do not intend to imply that cloud radiative effects always strengthen all extratropical cyclones. Instead, our paper highlights that cloud radiative effects can have a considerable effect on extratropical cyclones and that this impact can be understood from the cloud radiative modulation of latent heating and known impacts of latent heating on extratropical dynamics. Thus, our paper highlights the importance of correctly simulating cloud radiative effects in numerical weather prediction and climate models.

We plan to revise the paper to clarify the above points.

<u>Minor revision:</u>

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.

Thanks for pointing us to the work of Grise et al. (2019). We will include their finding in our revised manuscript. We will further include the study of Li et al. (2015) (https://doi.org/10.1175/JCLI-D-14-00825.1). Using COOKIE simulations with transparent clouds, Li et al. (2015) showed that atmospheric cloud radiative effects in the midlatitude increase eddy kinetic energy.

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.

During the grid generation, the geographical latitude center of the grid was set to 45 degrees and extends 81 degrees in the latitudinal direction. Thus, the grid extends from 4.5 to 85.5 degrees North. In all the map plots the range of the y-axis was chosen so that necessary information was plotted. Indeed 45 is the center and the cyclone is initialized at 45 degrees north. We will clarify this in the revised manuscript.

As a reminder, the model simulation is solely based on the planar channel Cartesian coordinates with a uniform Coriolis parameter set at 45 degrees north. Nevertheless, the geographical latitude and longitude coordinates were assigned to grid cells during the grid generation for visualization and remapping purposes.

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

The total precipitation rate is derived based on the time derivative of hourly accumulated precipitation diagnosed from the model output and includes precipitation in all forms (rain, snowfall, etc.). Both total precipitation rate and cloud cover are averaged spatially over the entire simulation domain excluding northern and southern boundaries i.e., longitude: [-25.5:25.5] and latitude: [10:70]. We will add this information in the revised manuscript.

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?

The cloud radiative cooling at the top of the boundary layer (2 km) and warming from below is also visible in the cross-section in Fig. 6 b. For better visualization, we will increase the latitudinal extent in Fig. 6 in the revised manuscript. Also, the dipole of longwave cloud radiative cooling and heating in the boundary layer (0-2 km) is mostly present behind the warm conveyor belt, south and southwest of the cyclone center where shallow stratocumulus clouds are located.

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.

Thanks for this comment. We will use the same convention (CRH-REF) throughout the paper. This will include an adaptation of the text in section 4.1 and in particular Eq. 6.

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.

Thanks for the comment. We will revise the text accordingly and better clarify that differences are initially due to cloud radiative heating.

Lines 398-400: The differences in vertical motion appear to be displaced eastward from the differences in divergent flow. They are not co-located.

The mismatch in the location of differences in the vertical motion and the differences in the near-tropopause divergent flow is likely related to the westward tilt during the cyclone intensification phase (day 5.5). The vertical velocity differences are plotted on the isobaric surface at 500 hPa whereas the divergent tendency is plotted at 326 k isentrope near the tropopause. Thus, the vertical velocity differences will be advected upward to the near-tropopause level west of their position in the mid-troposphere. We agree that the word “co-located” is misleading and we will edit the sentence in the revised manuscript accordingly.

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

We will show the cross-section location in Fig. 1 in the revised manuscript. Thanks.

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?

We agree that this layer should be also included. We will edit the figure and include the 4-6 km layer in the revised manuscript.

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

Thanks, however, we believe that keeping the same color bar and the number of contours in panel a helps to compare the differences with other panels more easily.

Typos

Line 59: warm conveyor belts

Thanks.

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

of the domain.

Line 258: simulation

Thanks for spotting these errors, all will be revised in the manuscript.

Reviewer 2

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.

We thank the reviewer for their thorough evaluation of our manuscript. Reviewer 1 also raised a concern regarding the sign of the cloud radiative impact. Our answer to both concerns is written here as well as in our text for reviewer 1.

We agree with the concern regarding the sensitivity of the results to changes in the model setup. However, we are also convinced that the sensitivity does not question the results of our paper because of two arguments. These are described in detail in the following.

First, to study the sensitivity and to better understand the cloud radiative impact on cyclones, AV and BK were supervising a Master thesis that was done in parallel to the work described in our paper (Butz, 2022). The thesis is published at the library of University of Vienna (https://phaidra.univie.ac.at/open/o:1539084).

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Second, motivated by the reviewer's comment, we have repeated our 2.5 km channel simulations using a two-moment microphysical scheme instead of a one-moment scheme. These simulations are depicted in Figure 1 showing that the sign of the cloud radiative impact is robust regardless of the microphysical parametrization. We will add these simulations to the revised manuscript.

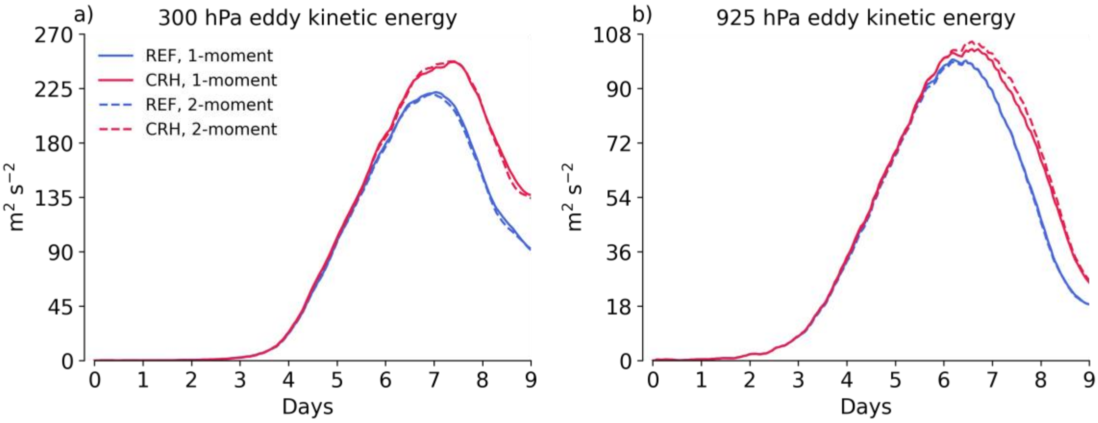

Figure 1: Evolution of the eddy kinetic energy at (a) 300 hPa and (b) 925 hPa for simulations with no radiation (REF) and cloud-radiative heating (CRH). The dashed lines are for simulations with 2-moment microphysics.

Finally, we would like to note that the cloud radiative impact on extratropical cyclones' dynamics and predictability remains poorly understood and has received very little attention. In light of the dependence of the cloud radiative impact on the ICON model version, it seems possible - and in fact not unlikely - that other models might show a different sign and magnitude of the cloud radiative impact, and that the cloud radiative impact might be case dependent. We thus do not intend to imply that cloud radiative effects always strengthen all extratropical cyclones. Instead, our paper highlights that cloud radiative effects can have a considerable effect on extratropical cyclones and that this impact can be understood from the cloud radiative modulation of latent heating and known impacts of latent heating on extratropical dynamics. Thus, our paper highlights the importance of correctly simulating cloud radiative effects in numerical weather prediction and climate models.

We plan to revise the paper to clarify the above points.

Detailed comments:

Chapter 2.2 Simulation design: I have some difficulties to 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.

Yes, the reviewer is right that the REF and CRH simulations are the same until clouds form and hence cloud-radiative effects occur. In Schäfer and Voigt (2018) three simulations were performed with different radiative configurations: 1) No radiation, 2) all-sky radiation that includes the radiative contributions from clouds, and 3) clear-sky radiation in which clouds are set to zero in the radiation transfer calculation. The cloud-radiative impact was then estimated as the difference in the evolution of eddy kinetic energy between simulations with all-sky and clear-sky radiation. The problem with the Schäfer and Voigt (2018) approach is that due to the strong clear sky radiative cooling, the atmospheric background state changes, and it is not clear whether the radiative impact on the cyclone is solely due to cloud radiative heating or changes in the atmospheric background.

In our study, we therefore use a new approach that avoids this problem. Our new approach uses two simulations: 1) No radiation (REF) as in Schäfer and Voigt (2018), and 2) cloud radiation only (CRH). In the CRH simulation, only the radiative heating from atmospheric cloud-radiative effects, which is defined as the all-sky minus the clear-sky radiative heating (Eq. 1 in our manuscript), is passed to the dynamical core. The approach has the important advantage that it isolates the cloud-radiative impact and eliminates the change in the atmospheric background state due to clear-sky radiative cooling. Thus, the REF and CRH simulations are the same before clouds start to form. This can be seen by comparing Figs. 4 b and 11 a, for example.

To calculate cloud radiative heating in the model during the simulation two calls to the radiation scheme are needed: one call in which the scheme calculates all-sky radiative heating rate including clouds, and one call in which clouds are set to zero, providing the clear-sky radiative heating rate.

We will revise Sect. 2.2 to clarify the simulation setup.

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"

Thanks for the comment. The research question will be changed to "How strongly does cloud-radiative heating affect an idealized extratropical cyclone?" In this study, we have looked at the impact of cloud radiative heating rate on the simulated cyclone in terms of eddy kinetic energy, cyclone central pressure, precipitation rate, cloud cover, potential vorticity, and potential vorticity tendency all in a quantitative manner. Thus, our first research question does not merely refer to 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?

We have not specifically studied the changes in the potential vorticity gradient across the tropopause. Although we agree that it could be interesting to study whether cloud radiative effects might change the tropopause sharpness or vertical wind shear, Haualand and Spengler (2021) recently showed that the baroclinic development is less sensitive to changes in potential vorticity gradient, wind stratification across tropopause or tropopause altitude than the impact of diabatic processes on tropopause structure. Thus, here we focus on the impact of cloud radiative heating on baroclinic growth in terms of near-tropopause potential vorticity difference in the trough and the ridge and their relation to the cyclone maximum intensity.

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?

Thanks for the question. We did not investigate in detail the source of the potential vorticity differences at lower levels because on average the cloud radiative heating has a relatively smaller impact on the evolution of near-surface eddy kinetic energy and cyclone central pressure during the baroclinic growth (Section 3.1). Thus, the low-level wind speed changes are also small in comparison to the changes at the upper levels. Although the low-level positive potential vorticity differences and hence the increased wind speed could increase the moisture supply to the warm conveyor belt, changes in the latent heating will be projected to the near-tropopause potential vorticity. Additionally, it is also shown in section 3.2 that the upper-level potential vorticity differences contribute most to cyclone intensification during the rapid growth phase. Yet we agree that understanding these differences could be important for the correct simulation of complex mesoscale potential vorticity structures, and their impact on near-surface wind speed. But for the cyclone simulated in our study, low-level PV differences on average are not strong enough to change the cyclone's near-surface eddy kinetic energy.

With respect to the source of the potential vorticity differences, Fig. 8 h of our manuscript shows that at lower levels (around 925 hPa) the net positive difference in the diabatic potential vorticity is due to the enhanced potential vorticity tendency by total latent heating. Potential vorticity tendency by cloud radiative heating has a relatively smaller impact on the net diabatic potential vorticity. Thus, part of the potential vorticity difference at lower levels is due to enhanced latent heat release. The remaining differences are due to changes in advective potential vorticity tendencies. Please have a look at Figure 2 and the explanation in our answer to the reviewer's next comment (L 210 ff).

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?

In Fig. 5 panels g and h, the positive potential vorticity differences west of the trough (red colors) and negative values in the ridge (blue colors) show that the cyclone with cloud radiative heating has a higher wave amplitude. We will revise the text in the manuscript that the higher amplitude could "imply" a stronger tropopause fold, but this is not explicitly shown in the figure.

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?

The diabatic potential vorticity (PV) tendencies in section 3 are calculated on model levels based on only the physical temperature tendencies with the assumption that the vertical gradient of temperature tendencies dominates the PV tendency (Eq. 3 in the manuscript). In section 4, however, the full 3-dimensional PV tendency equation on isentropic levels including the momentum tendencies is studied (Eq. 4 in the manuscript). We made this separation to first demonstrate the clear relation between the vertical dipole of cloud radiative cooling and heating and PV.

The assumption for Eq. 3 in the manuscript can be justified for diabatic processes due to the higher variability of heating rates in the vertical direction, although we agree that this is not the case for advective PV tendencies. To demonstrate this, we have investigated the differences in the local PV tendency between simulations with and without cloud radiative heating with contributions from the diabatic and advective PV tendencies (expansion of Eq. 3 in the manuscript):

1. $\frac{\partial}{\partial t} PV = \frac{1}{\rho}\left(\eta \cdot \nabla\dot{\theta}\right) - \left(\boldsymbol{U} \cdot \nabla PV\right)$

Here, absolute vorticity (η), wind ($\boldsymbol{U}$), and gradient operator (∇) include all 3 vector components. Figure 2 shows the difference PV tendencies (CRH -REF, Figure 3 a) and the contribution from diabatic processes (Figure 3 b, c and d) and advection (Figure 3 e) at a single model level around 925 hPa at day 7. The third row shows the contributions from the vertical component.

As shown also in the manuscript, the inclusion of cloud radiative heating has the strongest impact on the diabatic PV tendencies from latent heating and to a lesser degree on turbulence which have opposite signs and compensate each other (Figure 3, b, and c). The majority of differences in PV tendency (Figure 3 a) are due to the advective tendencies along the warm conveyor belt (cf. Figure 3 a and e).

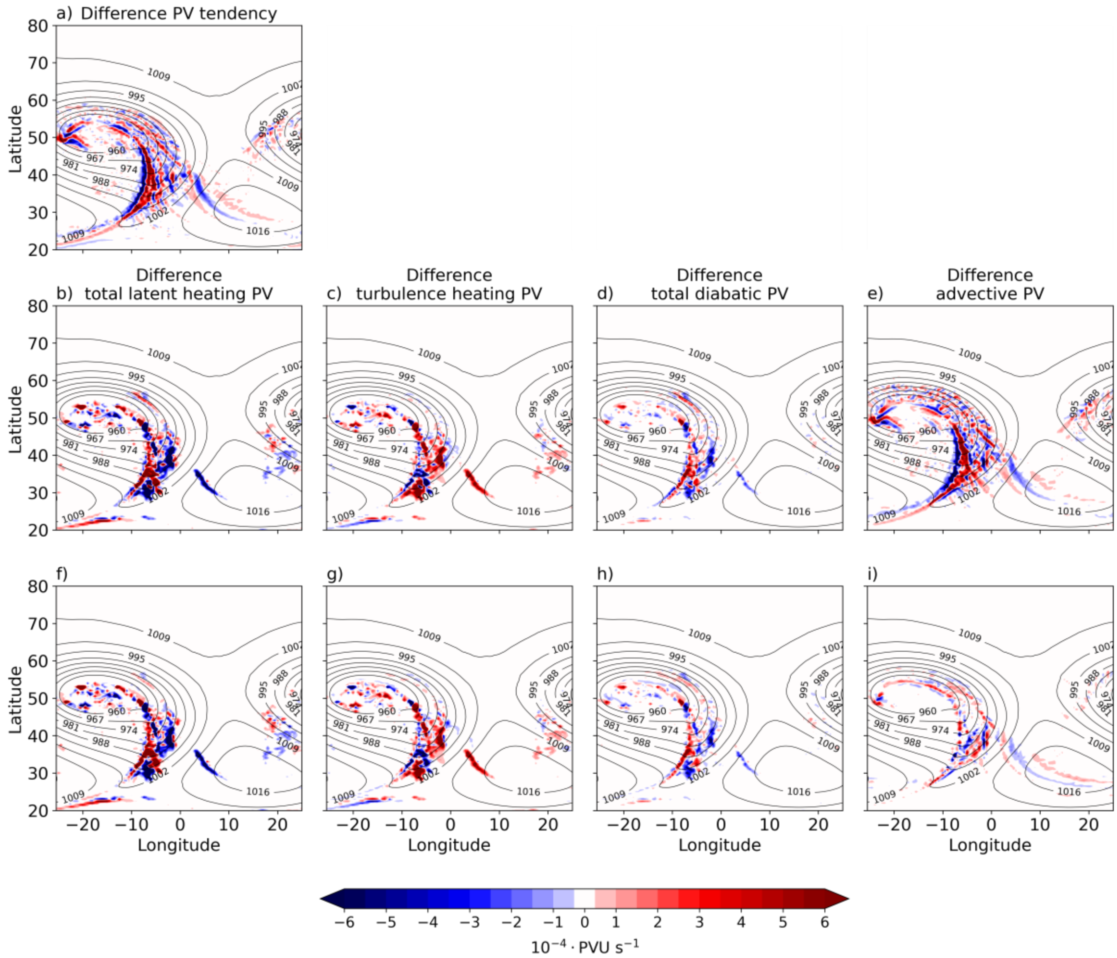

Figure 3: Spatial distribution of (a) difference potential vorticity tendency (CRH -REF) and the contribution from (b) total latent heating, (c) turbulence, (d) total diabatic and (e) advective tendencies based on 3 vector components of equation 1. The third row shows the contribution from the vertical component.

Comparing diabatic PV tendencies derived based on 3 vector components and with only the vertical component, we find that diabatic PV tendencies are dominated by the vertical gradient of diabatic heating rates, whereas the vertical component of the advective tendency is smaller than the horizontal part (cf. Figure 3 e and h). We will add these considerations and more clarification in the revised manuscript.

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

Unfortunately, we do not have the output for the individual process rates from the microphysics scheme in the model. Thus, we cannot say for sure whether the strong cooling is due to rain evaporation or something else. However, previous studies showed that the cooling near the surface is due to the evaporation of rain and snow melting both alongside the warm conveyor belt and the warm front (e.g. Crezee et al. (2017); Joos and Wernli (2012)).

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?

As we will also explain more in our answer to the reviewer's next comment (L 336), there are other factors that contribute to the evolution of difference potential enstrophy tendency that are not shown in panels b, c, and d of Fig. 9, but they are present in panel a. For instance, during the maximum intensity, a considerable source for the negative difference potential enstrophy is due to the model diffusion by the dynamical core which is part of the RES term in Eq. 8. We will add this remark in the revised manuscript.

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?

The sum of the diagnostics (cyan line) and the difference potential enstrophy tendency (black line) do not perfectly match due to the reasons mentioned in section 4.1. However, the sum captures the evolution of difference potential enstrophy reasonably well. The higher values for the sum between days 6.5 to 8 are due to the fact that numerical diffusion from the dynamical core is not taken into account. Unfortunately, we cannot quantify the contribution of numerical diffusion from the available model output. Previous work, however, showed that numerical diffusion leads to a negative contribution and that the contribution can be as large as the contribution from the advective tendencies (Baumgart et al. (2018, 2019)). The missing negative tendency from numerical diffusion is likely also responsible for negative difference potential enstrophy during the cyclone decay phase. We will add more details about this in the revised manuscript.

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).

The dark blue line in Fig. 10 a is the contribution from the total diabatic tendency and is the same as the black line in Fig. 10 b. The contributions from individual diabatic processes to the total diabatic tendency (black line in Fig. 10 b) are shown in different colors. As a reminder, we have summed the heating rate from microphysics and saturation adjustment and named it total latent heating in the manuscript. For Fig. 10 we will use better colors and clarify the meaning of the lines in the caption.

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.

The mismatch in the location of differences in the vertical motion and the differences in the near-tropopause divergent flow is likely related to the westward tilt during the cyclone intensification phase (day 5.5). The vertical velocity differences are plotted on the isobaric surface at 500 hPa whereas the divergent tendency is plotted at 326 K isentrope near the tropopause. Thus, the vertical velocity differences will be advected upward to the near-tropopause level west of their position in the mid-troposphere. We agree that the word "co-located" is misleading and we will edit the sentence in the revised manuscript accordingly.

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.

The fields in the right column are the differences between the REF and CRH simulation (REF-CRH). Indeed, and based on a related comment by reviewer 1, we will update the figure so that it shows the difference "CRH-REF" and is consistent with the first part of the manuscript.

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?

The first statement is based on Figs. 4 and 7 where we demonstrate that cloud radiative heating increases the latent heat release. We will add the reference to the figures. However, the increase in latent heat release can be also interpreted by the increase in the different divergent wind tendencies between days 5 to 7.5 (red lines in Fig 15). Since the upper-tropospheric divergent flow represents an indirect impact of latent heat release near the tropopause, the amplification of divergent tendency is a sign of amplified latent heat release.

<u>**Additional corrections:**</u>

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

Thanks.

L. 66: how strongly does cloud-radiative heating

Thanks.

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

Thanks, this will be corrected.

L 176: …strong PV gradients that separate

Thanks.

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

Thanks, all will be revised in the manuscript.

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

The intensity and peak time refer to eddy kinetic energy shown in Fig. 3. Will clarify this in the manuscript.

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

We will correct this.

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

Thanks.

L229: …as shown in Fig. 6 e,f, g.

Thanks, all will be revised the in the manuscript.

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?

We used the same unit convention throughout the paper. Also, Baumgart et al. (2018, 2019) used PVU/s. Thus, we decided to keep PVU/s as this facilitates the comparison with the Baumgart studies.

L 246: PV tendency. But, at lower….

L 248: … by the longwave CRH …

Thanks.

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?

Yes, the heating rate from convection is only due to parametrized shallow convection. This has been indicated in the manuscript: Lines 263-264. But we will add a corresponding remark in the revised manuscript in L255 for clarification.

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.

Thanks, we will edit the text accordingly.

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

Here "best" refers to the budget closure for Eq. 8. We will clarify this in the revised manuscript.

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

The correct term is PV distribution near the tropopause. We will edit it in the revised manuscript. Thanks for this remark.

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

Thanks.

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

Thanks.

References

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