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 include the revised parts in the manuscript according to each reviewers' comments. We are hopeful that the revised manuscript will be acceptable for publication. The reviewers' comments are in bold, our answers are in normal font, and the revised parts are written in gray italics.

We have also slightly edited the manuscript for better readability. These changes are purely editorial nature and are thus not specifically highlighted.

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The disagreement of the results with Schäfer and Voigt (2018) is concerning, especially because, in unpublished work, the authors have found that the model version affects the sign of the influence of cloud radiative heating on the intensity of the cyclones (lines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 the University of Vienna (https://doi.org/10.25365/thesis.71895).

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. The results of these simulations are depicted in Figure 3 of the revised manuscript, showing that the sign of the cloud radiative impact is robust regardless of the microphysical parametrization.

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.

<u>Revised (L167: 180)</u>

"Our results are in contrast to the global simulations of Schäfer and Voigt (2018), who reported that CRH weakens idealized cyclones. Schäfer and Voigt (2018) also used the ICON atmosphere model and studied a cyclone growing from the same initial conditions. The disagreement between our finding of a strengthening CRH impact and the finding of Schäfer and Voigt (2018) of a weakening impact might seem discomforting at first sight, but in fact, it does point out the importance of model uncertainty in CRH. This is briefly described in the following. In a companion study, which was performed as a Master thesis advised by Aiko Voigt and Behrooz Keshtgar, Butz (2022) found that the result of Schäfer and Voigt (2018) is sensitive to the version of the ICON model. Butz (2022) found a weakening CRH impact in ICON version 2.1 (which is essentially the same version as used by Schäfer and Voigt, 2018), but a strengthening impact in ICON version 2.6 (which is the version used in the present study). Butz (2022) traced this difference to a difference in the simulation of low clouds, of which there are many in version 2.1 but fewer in version 2.6. The results of Butz (2022) imply that the CRH impact is not sensitive to whether a global or channel setup is used. Moreover, we have repeated our channel simulations with the two-moment microphysics scheme of Seifert and Beheng (2006) instead of the one-moment scheme and have found that CRH impact is independent of the microphysics scheme (Fig. 3). Thus, the CRH impact is robust with respect to the model domain and cloud microphysics, although it can be expected to be model dependent because of model uncertainty in the simulation of CRH."

<u>Revised (L493: 499)</u>

"Our results are in contrast to Schäfer and Voigt (2018), who found a weakening impact of CRH. As discussed in Sect. 3 and in Butz (2022), the disagreement arises from changes in low-level clouds between the ICON version used in our study and an earlier model version used by Schäfer and Voigt (2018). It, therefore, seems possible - and in fact not unlikely - that other models show a different sign and magnitude of the CRH impact, and it might also be that the CRH impact depends on the cyclone case. We hence do not intend to imply that CRH strengthens all extratropical cyclones. Instead, our

work highlights that CRH can have a considerable effect on extratropical cyclones, and that model uncertainty in CRH might be large enough to impact numerical forecasts at synoptic scales."

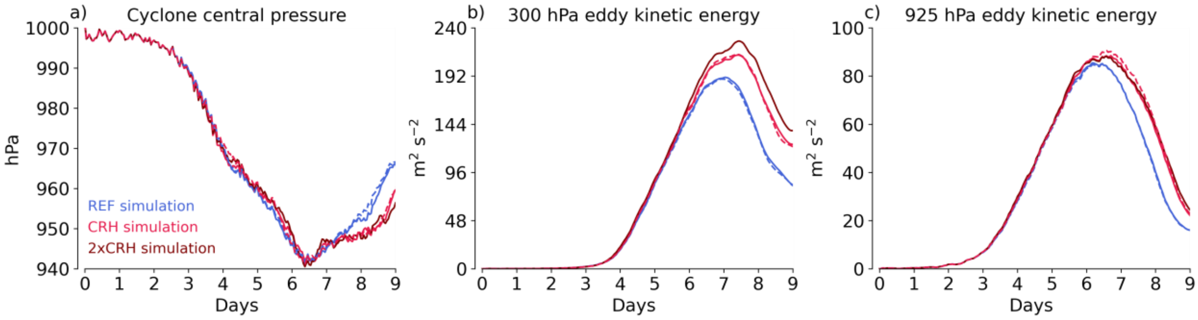

***Figure 3**. Evolution of (a) cyclone central pressure and eddy kinetic energy at (b) 300 hPa and (c) 925 hPa for simulations with no radiation (REF), cloud-radiative heating (CRH), and cloud-radiative heating increased by a factor of 2 (2xCRH). The dashed lines show additional simulations that use* the two-moment instead *of the one-moment microphysical scheme.*

Minor revision:

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 included the findings of Grise et al. (2019) and also Li et al. (2015) in the introduction section of the revised manuscript. Using COOKIE simulations with transparent clouds, Li et al. (2015) showed that atmospheric cloud radiative effects in the midlatitude increase eddy kinetic energy.

Revised (L41: 44)

"… Climate model studies showed that cloud radiative heating and cooling (hereafter CRH) increase the eddy kinetic energy in the midlatitudes (Li et al., 2015). However, using a different climate modeling technique, Grise et al. (2019) showed that the coupling of CRH with the circulation damps the intensity of extratropical storm tracks…"

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.

Thanks for the comment. 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 clarified this in the revised manuscript.

Revised (L106: 108)

"…The geographical latitude center of the grid is set to 45 degrees north and the cyclone is initialized at 45 degrees north. Thus, the grid extends latitudinally from 4.5 to 85.5 degrees north. However, in all figures, the range of the latitudes is chosen so that only the necessary information is shown…"

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., from 10 to 80 degrees north and -25.5 degrees west to 25.5 degrees east. We have added this remark at the end of section 2.2 since it applies to all spatial averages in the analyses.

Revised (L149: 151)

"…When we calculate spatial averages over the entire simulation domain, we exclude the northern and southern boundaries and perform the calculation from 10 to 80 degreess north and -25.5 degrees west to 25.5 degrees east."

Revised (L181: 183)

"… Fig. 4 shows the evolution of spatially averaged total precipitation rate and cloud cover for the three simulations and the differences with respect to the REF simulation. The total precipitation rate is derived from hourly accumulated precipitation and includes precipitation in all forms (rain, snowfall, etc.) …"

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 increased 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 altitude) 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 pointing this out. We revised the manuscript so that now the same convention (CRH-REF) is used throughout the manuscript. This includes the adaptation of the text in section 4.1, Eq. 6, and Fig. 12.

Revised (L314)

"Here, we apply the framework by considering the CRH simulation as the reference analysis, and the REF simulation without radiation as the forecast."

Revised (L341)

$\Delta PV = PV_{CRH} - PV_{REF}.$

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. The revised manuscript now mentions that CRH and total latent heating control the diabatic impact until day 5 based on Fig. 10 b, and the remark that differences are initially due to cloud radiative heating is now explained in the context of Fig. 11.

Revised (L395:396)

"… shows that CRH and total latent heating control the diabatic impact until day 5."

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

We agree that the word "co-located" is misleading. We have edited the sentence and have added more detail in the revised manuscript.

Revised (L422:428)

"…This is demonstrated for day 5.5, for which differences from the near-tropopause divergent flow and differences in vertical motion are located east and southeast of the cyclone center in the warm conveyor belt (Fig. 12 c and d). The differences in vertical motion are located slightly eastward of the differences in divergent flow. This 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..."

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

The cross-section location is now shown 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?

Thanks for the comment. We now include the vertical profile of total latent heating (cloud microphysical heating plus heating from saturation adjustment) and its PV tendency in Fig. 7 since the result shown in Fig. 8 is based on total latent heating. Diabatic PV tendencies are similar between 4-6 km and 6-8 km, thus in our separation 4-8 km layer represents the mid-levels. Please also note that the new analysis shows the diabatic PV tendencies calculated based on three vector components (Eq. 3) and in the new Fig. 8, the evolution of PV tendencies is based on the mass-weighted vertical averages. We have revised the text with the new figures accordingly. However, our findings are the same. Section 3.3 is revised following the comments of reviewer 2.

Revised (L259:260)

"Another heating rate that is required to be considered for the total latent heating comes from the saturation adjustment scheme. In Fig. 7 b, the total latent heating from the sum of cloud microphysics and saturation adjustment is shown …"

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

Thanks for the suggestion. 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. In the new figure, results are based on the mass-weighted vertical average.

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.

Thanks.

Line 258: simulation

Thanks for spotting these errors, all are corrected 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 described here and the revised parts in the manuscript are included.

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 the University of Vienna (https://doi.org/10.25365/thesis.71895).

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 3 of the revised manuscript, showing that the sign of the cloud radiative impact is robust regardless of the microphysical parametrization.

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.

Revised (L167: 180)

"Our results are in contrast to the global simulations of Schäfer and Voigt (2018), who reported that CRH weakens idealized cyclones. Schäfer and Voigt (2018) also used the ICON atmosphere model and studied a cyclone growing from the same initial conditions. The disagreement between our finding of a strengthening CRH impact and the finding of Schäfer and Voigt (2018) of a weakening impact might seem discomforting at first sight, but in fact, it does point out the importance of model uncertainty in CRH. This is briefly described in the following. In a companion study, which was performed as a Master thesis advised by Aiko Voigt and Behrooz Keshtgar, Butz (2022) found that the result of Schäfer and Voigt (2018) is sensitive to the version of the ICON model. Butz (2022) found a weakening CRH impact in ICON version 2.1 (which is essentially the same version as used by Schäfer and Voigt, 2018), but a strengthening impact in ICON version 2.6 (which is the version used in the present study). Butz (2022) traced this difference to a difference in the simulation of low clouds, of which there are many in version 2.1 but fewer in version 2.6. The results of Butz (2022) imply that the CRH impact is not sensitive to whether a global or channel setup is used. Moreover, we have repeated our channel simulations with the two-moment microphysics scheme of Seifert and Beheng (2006) instead of the one-moment scheme and have found that CRH impact is independent of the microphysics scheme (Fig. 3). Thus, the CRH impact is robust with respect to the model domain and cloud microphysics, although it can be expected to be model dependent because of model uncertainty in the simulation of CRH."

Revised (L493: 499)

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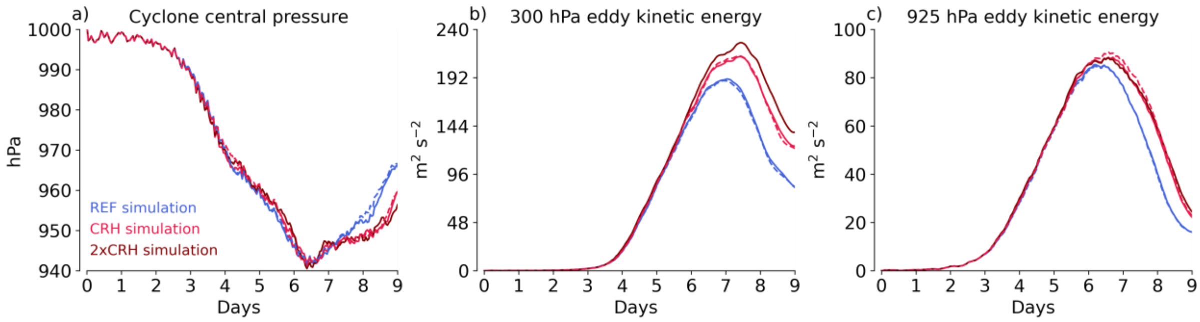

***Figure 3**. Evolution of (a) cyclone central pressure and eddy kinetic energy at (b) 300 hPa and (c) 925 hPa for simulations with no radiation (REF), cloud-radiative heating (CRH), and cloud-radiative heating increased by a factor of 2 (2xCRH). The dashed lines show additional simulations that use the two-moment instead of the one-moment microphysical scheme.*

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 heating occur. We have revised Sect. 2.2 to clarify the simulation setup.

Revised (L124: 142)

"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 between the simulations with all-sky and clear-sky radiation. However, when radiation is included in the baroclinic life cycle simulations, Schäfer and Voigt (2018) found a strong atmospheric cooling in the first days. This initial cooling also occurs in our model setup (Fig. 2 a). 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 and cooling (CRH) or changes in the atmospheric background.

To eliminate this problem, we develop and apply a new modeling approach that isolates the impact of CRH in a clean and easy-to-interpret manner. Our new approach requires two simulations: one simulation with no radiation as in Schäfer and Voigt (2018), and one simulation with only CRH. In the latter simulation, only the radiative heating from clouds, defined as the all-sky minus the clear-sky radiative heating is passed to the model's dynamical core. In terms of the thermodynamic equation, our approach is described by Eq. 1

where J represents the heating rates from other diabatic processes. In terms of model implementation, our approach requires two calls to the radiation scheme: one call in which the scheme calculates the all-sky radiative heating rate including clouds, and one call in which clouds are set to zero, providing the clear-sky radiative heating rate. CRH is then calculated accordingly and passed to the dynamical core instead of the all-sky or clear-sky radiative heating rates. Our approach, thus, removes the initial radiative adjustment, and the cyclone forms in the same background state independent of whether CRH is active or not. This is shown in Fig. 2 b and c."

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. We have changed the research question to "How strongly does CRH affect an idealized extratropical cyclone?" In this study, we study the impact of cloud radiative heating on the simulated cyclone in terms of eddy kinetic energy, cyclone central pressure, precipitation rate, cloud cover, potential vorticity, and potential vorticity tendency. 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 heating 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, and wind stratification across tropopause or tropopause altitude. Instead, the baroclinic development is more sensitive to 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 small impact on the evolution of near-surface eddy kinetic energy and cyclone central pressure during the baroclinic growth (Section 3.1). 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 latent heating will be projected to the near-tropopause potential vorticity. We show in section 3.2 that the upper-level potential vorticity differences contribute most to cyclone intensification during the rapid growth phase. 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 positive difference in the net diabatic potential vorticity between days 6.5 to 8.5 is due to the enhanced potential vorticity tendency by total latent heating. Negative potential vorticity tendency by cloud radiative heating has a smaller impact on the net diabatic potential vorticity. Also, our analysis based on spatial averages (similar to the analysis in Fig. 8) shows that the evolution of differences in PV tendency due to the advection is small in the boundary layer.

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 have revised the text in the manuscript that the higher amplitude could "imply" a stronger tropopause fold since this is not explicitly shown in the figure. Thanks for the comment.

Revised (L214: 216)

"…Positive PV differences west of the trough center (red colors) and negative PV differences on the poleward side of the ridge (blue colors in Fig. 5 g and h) imply a deeper tropopause fold and stronger ridge for the baroclinic wave with CRH, although 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?

Thanks for the comment. In the old manuscript, the diabatic potential vorticity (PV) tendencies in section 3 were 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, which our analysis shows that is indeed the case. However, for completeness, we have revised Sect. 3.3 to include all diabatic PV tendencies (similar to Sect. 4) and PV tendencies are calculated based on 3 vector components. In the new Fig. 8, the evolution of PV tendencies is based on the mass-weighted vertical average. We have revised Sec. 3.3 based on the new figures accordingly. Overall, our main findings have not changed.

Revised (L232: 240)

"

$$\frac{D}{Dt}PV = \frac{1}{\rho}\left(\boldsymbol{\eta} \cdot \nabla\dot{\theta}\right) + \frac{1}{\rho}(\nabla \times \boldsymbol{F}) \qquad (3)$$

where D/Dt denotes the material derivative, ρ is the density, $\boldsymbol{\eta}$ *is the absolute vorticity vector,* $\dot{\theta}$ *is the diabatic heating tendency, and* **F** *is the frictional force. The first and the second terms on the r.h.s. of Eq. 3 represent the PV modification due to diabatic heating tendencies and nonconservative momentum. For PV modification due to diabatic heating tendencies, one can assume that the vertical gradient of* $\dot{\theta}$ *dominates the PV tendency, which is typically the case. Thus, the main effect of diabatic heating is an increase of PV below the maximum of the heating and the reduction of PV above it. However, for our analysis, we derive the diabatic PV tendencies based on the three vector components of Eq. 3 and on model levels to benefit from the high vertical resolution. Our analysis includes all diabatic heating tendencies of the ICON model as well as the nonconservative momentum due to the parameterization of turbulence, shallow convection and non-orographic gravity waves."*

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 whether the strong cooling is due to rain evaporation or some other process. 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 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 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 have mentioned this in the revised manuscript.

Revised (L386: 387)

"…Note that the sum of diabatic, rotational and divergent tendency does not add up to the difference potential enstrophy shown in Fig. 9 a because of the residual term RES (see Eq. 8)."

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 (green 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. (2019)). The missing negative tendency from numerical diffusion is likely also responsible for negative difference potential enstrophy during the cyclone decay phase. We have added more details about this in the revised manuscript.

Revised (L368: 372)

"… It is reasonable to assume that this is due to a sink of difference potential enstrophy from model diffusion, discussed in detail in Baumgart et al. (2019). Their results indicated that numerical diffusion leads to a negative contribution that can be as large as the contribution from the advective tendencies. The contribution from numerical diffusion, however, cannot be quantified from our model output and means that one should in fact not expect a perfectly closed budget. he generation of PV anomalies by a numerical model's dynamical core has been also demonstrated in, e.g., Saffin et al. (2016). "

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).

Thanks for the comment. In the revised manuscript, we have made the color of total diabatic tendency consistent in both panels. The cyan line in Fig. 10 a is the contribution from the total diabatic tendency and is the same as the cyan line in Fig. 10 b. The contributions from individual diabatic processes to the total diabatic tendency (cyan line in Fig. 10 b) are shown in different colors.

Revised (Fig 10)

*"**Figure 10.** Evolution of the spatially averaged difference potential enstrophy tendency and contributions from individual processes diagnosed from the r.h.s. of Eq. 8. The analysis is performed*

around the tropopause on the 326 K isentrope. Panel (b) further decomposes the total diabatic (cyan line) contribution into the contributions from individual diabatic processes. Note the different scales in panels."

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.

We agree that the word "co-located" is misleading. We have edited the sentence and have added more detail in the revised manuscript.

<u>Revised (L422:428)</u>

"…This is demonstrated for day 5.5, for which differences from the near-tropopause divergent flow and differences in vertical motion are located east and southeast of the cyclone center in the warm conveyor belt (Fig. 12 c and d). The differences in vertical motion are located slightly eastward of the differences in divergent flow. This 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..."

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.

In the old manuscript, the fields in the right column were the differences between the REF and CRH simulation (REF-CRH). However, we revised the manuscript so that now the same convention (CRH-REF) is used throughout the manuscript. This includes the adaptation of the text in section 4.1, Eq. 6, and Fig. 12.

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?

Thanks for the comment. We show in Fig. 4 and Fig. 7 that cloud radiative heating increases the precipitation rate and total latent heat. 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. We have edited the text to clarify this remark.

<u>Revised (L465:470)</u>

"Letting CRH interact with the cyclone until day 4 leads to more latent heat release. This is shown by the increase in the different divergent wind tendencies, which represent an indirect impact of latent heat release near the tropopause. Compared to the simulation starting at day 3, different divergent wind tendencies are enhanced for the simulation starting at day 4 (Fig. 15 b). With amplified divergent wind tendencies, differences in the rotational flow also increase and change the near tropopause PV during the cyclone mature stage between days 6 to 7.5. This effect becomes stronger if CRH is active until days 5 and 6 (Fig. 15 c and d)."

<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 has been 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, this is 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.

Revised(L221:222)

"… Thus, including CRH helps to reinforce this impact, resulting in higher eddy kinetic energy and delayed peak time."

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

Corrected.

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

Thanks.

L229: …as shown in Fig. 6 e,f, g.

Thanks, all are revised in the manuscript.

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

Thanks for the suggestion. However, 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.

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

As mentioned above we used the same unit convention throughout the paper since it facilitates the comparison with the Baumgart studies.

L 246: PV tendency. But, at lower….

Thanks.

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: L66, L282.

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, but we kept this line since we think it helps the reader to follow the arguments.

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 now clarify this in the revised manuscript.

Revised(L359:360)

"… The tests showed that the budget is better closed when we compute Eq. 8 on the 1 degrees x 1 degrees grid."

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 differences near the tropopause. We have changed the manuscript accordingly. Thanks for this remark.

Revised (L385:386)

"… controls the PV differences near the tropopause …"

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

Thanks.

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

Thanks.

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