Reviewer and Editor Responses: “How Does Cloud-Radiative Heating over the North Atlantic Change with Grid Spacing, Convective Parameterization, and Microphysics Scheme?”

Reviewer 1

This study explores the sensitivity of model representation of atmospheric cloud radiative heating profiles over the North Atlantic to changes in grid resolution, atmospheric convection (explicit or parameterized), and microphysics scheme within the ICON model. While grid resolution is only found to play a small role, cloud radiative heating profiles are highly sensitive to the model representation of convection and microphysics. In particular, the role of cloud ice mass mixing ratio appears to play a critical role.

This manuscript provides a good discussion of the factors governing atmospheric cloud radiative heating profiles at midlatitudes in the ICON model. Technically, the manuscript is sound and just needs some minor corrections/clarifications (detailed below). However, my general impression in reading this paper is that I’m not sure GMD is really the right journal for this work. The manuscript is using the model output data set from a previous study (Senf et al. 2020) and really not describing fundamentally new methods, rather than just extending the authors’ previous work from the tropics to the midlatitudes. I’ll leave it to the editor to decide whether GMD is the appropriate venue for this work.
We thank the reviewer for their time and effort in evaluating our work and for their feedback. We also recognize the reservation about GMD as the appropriate journal. Our initial submission was to ACPD, and the editor requested transfer to GMD, so we also defer to the editor’s judgment in this case.

Lines 9-10: Isn’t this point (coupling of microphysics and convection schemes) just a hypothesis provided at the end of the paper (Line 352-360)? If so, it doesn’t belong in the abstract as a statement of certainty. I don’t see any formal evidence presented to support this conjecture.
Yes, you’re correct. We’ve replaced this text in the abstract with “*the broadening of the vertical velocity distribution with explicit convection*.” Figure 13 and Equations 4-13 do present concrete evidence for this point.

Thank you very much for highlighting this point, as it encouraged us to dig more into the coupling of microphysics and convection schemes, which we had misrepresented in Sec. 3.4.2. In fact, not even the condensate mass mixing ratios from the grid-scale microphysics are seen in the convective buoyancy formulation. We have corrected this text and now emphasize the separation of convective and grid-scale microphysics schemes in the final paragraph of Sec. 3.4.2.

Lines 23-25: Lu et al. (2007) do not discuss cloud-radiative impacts, and models do not agree on whether the presence of cloud radiative effects drive a poleward circulation shift (see discussion in Voigt et al. 2020 review). For example, Li et al. (2015) do not find a poleward expansion of the circulation due to the presence of cloud radiative effects, and they actually show that cloud-radiative effects decrease the static stability in the tropics.

Thank you for pointing out that this reference was not directly applicable. We remove this sentence and instead cite Li et al. 2015 and Voigt et al. 2020 for the idea that upper-tropospheric radiative heating in the tropics versus cooling in the midlatitudes promotes baroclinicity and static stability.

Line 32: The intensification of ENSO due to cloud radiative effects is again a model dependent result. Middlemas et al. (2019) found a differing effect on ENSO.
Thank you for noting that the language in this paragraph is too definitive. We add a reference to Middlemas et al. 2019 and clarify that "anomalies in cloud-radiative effects can intensify *or mute* the amplitude of ENSO *depending on model framework*."

Lines 136-137: More detail probably needs to be provided here to explain this conclusion, as the numbers in Table S1 do in fact look quite sensitive to the particular thresholds used.
Yes, there is a subtlety here. After describing the three sets of percentile thresholds (60-60-25, 62-67-30, and 65-70-35), we clarify that "*the cloud fractions associated with these percentile thresholds change by up to an order of magnitude; cloud fraction is generally larger than these threshold values when a cloud forms, so that the occurrence probability of cloud classes is mostly insensitive to which thresholds are used (Fig. S1).*"

Lines 146-155: It also seems important to note/discuss here that the altitude of the lower and upper tropospheric cooling peaks differs fairly significantly by model.
We have added a sentence to this paragraph to say that "*The altitudes of cloud-radiative cooling maxima also vary by about 80 hPa between the models in both the lower and upper troposphere.*"

Lines 169-170: Also convective heating rates appear to be important in this layer.
Yes, after the components mentioned (clear-sky LW cooling, dynamic heating, clear-sky SW heating, microphysical heating, and cloudy LW cooling), the convective heating would be next most important. We add a final sentence that "*the three smallest components of the budget are convective heating, shortwave cloud-radiative heating, and turbulent heating at these altitudes.*"

Lines 172-173: Also, the cooling peak appears to be slightly higher in altitude in the one-moment scheme.
Indeed. The goal with Figure 4 is primarily to indicate that LW cloud-radiative heating contributes non-negligibly to the heating budget both for the one- and two-moment schemes, so for simplicity's sake, we do not note this point in the text.

Line 180, typo: Change "Then" to "The"
Thank you, done.

Lines 184-185: Also, a large heating peak develops at lower altitudes, which is not present in the simulations with the deep convective parameterization.
Yes, this is worthy of mention, thank you. We state that "*the explicit representation of convection also produces prominent heating below 9 km, not present in the other two-moment simulations*."

Lines 221-227: Good to double check the percentage values quoted in this paragraph. They appear to match what is shown in Fig. S3, not Fig. 7.

Many thanks for catching this. We showed the occurrence boxplots from the one-moment microphysics (Fig. S3) in an earlier draft and had not updated the values to reflect the two-moment boxplots.

Lines 230-232: Can you provide a physical explanation for why the isolated high clouds warm and the deeper clouds cool?

Yes, certainly. We have added the following: "*Isolated high clouds absorb more outgoing longwave radiation (OLR) than clear sky, whereas deep clouds absorb this OLR in the liquid cloud at lower altitudes and reemit it at colder temperatures from their cloud tops.*"

Line 243 (and hereafter): The term "higher grid spacing" could be confusing and could imply coarser resolution to some readers. I would either say "higher resolution" or "finer grid spacing".

Yes, this is a good point. We have changed instances of *higher grid spacing* to *finer grid spacing*.

Line 269: It doesn't look like a factor of four. At best, it looks like a factor of two.

Thank you for catching this. The max range of ice crystal number concentrations (2 L^{-1} up to 8 L^{-1}) was mixed up with the max range of snow mass mixing ratio.

Line 271: The relative increase actually appears stronger in the thin cloud layers.

You are correct that the relative increase of q_s is larger for the *High* and especially *High-Low* classes than either the *High-x-Middle* or *High-x-Middle-x-Low* classes. However, omission of q_s from the CRH calculations is more influential for the deeper clouds with larger-magnitude q_s, since longwave absorption would be in proportion to absolute condensate mass. We clarify that the monotonic increase in q_s has "*largest-magnitude changes from deep clouds*."

Line 317, 325: This citation structure is confusing. Initially, I was looking for Fig. 10a and Table 2 in this paper. Please clarify that this figure and table are in the Sullivan et al. (2022) paper, and not this paper.

BibTeX is persnickety with citation formats like this. We have done the following: *(e.g. Sullivan et al., 2022,* ***their*** *Figure 10a)* and *(e.g. Sullivan et al., 2022,* ***their*** *Table 2)*.

Line 328: I think you need to elaborate more on why you choose "supersaturation generated by vertical velocity" as one of your cloud controlling factors. The other two are obvious from the above equations, but this one is less obvious.

Thank you for point out this need for clarification. We note that "*T and q_v appear explicitly in Eqs. 9-13, while the influence of w is felt indirectly by setting saturation with respect to ice (RH_{ice} or S_{ice} in Eqs. 11 and 13). The strength of w relative to v_{Ts} also determines whether ice crystals sediment.*"

Line 384, typo: boundary

Thank you, done.

Figure 1 caption: North Africa, as well

Yes, thank you for catching this oversight.

Code and data availability: available is misspelled.
Thank you, corrected.

References:

Li, Y., Thompson, D. W. J., & Bony, S. (2015). The influence of atmospheric cloud radiative effects on the large-scale atmospheric circulation. *J. Clim.*, **8**, pp. 7263–7278. https://doi.org/10.1175/JCLI-D-14-00825.1

Middlemas, E. A., Clement, A. C., Medeiros, B., & Kirtman, B. (2019). Cloud radiative feedbacks and El Niño–southern oscillation. *J. Clim.*, **32**(15), pp. 4661–4680. https://doi.org/10.1175/JCLI-D-18-0842.1