Authors' reply

December 22, 2020

Dear Editor,

we thank you for your comments. Below, we reply to your comments point-by-point. At the end of the document, we list other changes made in the last version of the manuscript.

Minor revisions

1. *How do your findings relate to models with higher spatial resolution? Clearly, running simulations at higher resolution is beyond the scope of this revision but would you expect that observed numerical tendencies also occur at a higher resolution scale?*
   We think that most of our results would be qualitatively similar at higher resolution. The numerical tendencies could become even more relevant at higher resolution, as local gradients may become steeper such that discrepancies requiring numerical tendencies will be maintained or even increased. A stronger impact may result from a change in vertical resolution as the microphysics is more susceptible to changes in vertical motion and associated thermodynamics.

2. *It is not clear why sublimation as a sink cannot be treated independently from sedimentation. Maybe add a sentence on this matter to make this clearer.*
   The point is that sublimation of ICs is not explicitly treated in CLOUD. If cloud ice sublimates, the ice mass is reduced to zero; consequently, the ice number concentration is also reduced to zero by a numerical tendency. In case of incomplete sublimation, all ICs shrink, and ICNC does not change. In the end, it is not correct saying that sublimation is considered in the sedimentation term as it could be part of a numerical tendency. We removed this part at L167 and wrote "as it is not explicitly treated."

3. *Conclusions: It should be pointed out that sublimation is omitted in the analysis, given that sublimation is a substantial sink of upper tropospheric ice.*
   We modified L420: "The loss processes of ice crystals are melting (MELT), self-collection (SELF), aggregation (AGGR), and accretion (ACCR); sublimation is excluded from the analysis of this study."

4. *Abstract: Are the ICNC in good agreement with satellite observations? Is this statement accurate? Figures 1 and 2 do not corroborate this clearly. Maybe use same colormap for Figs. 1 and 2 to make it easier for the reader to interpret both figures.*
   We modified L8-9 in the Abstract to make it more precise: "We found that model ICNCs are in overall agreement with satellite observations in terms of spatial distribution, although the values are overestimated, especially around high mountains."
   We agree with the Editor that using the same color bar would facilitate, in principle, the comparison. However, we found that this was not the case for the comparison between DARDAR and EMAC as the DARDAR plots lost clarity in their patterns using a discrete color bar and increasing the maximum value (to match the one of the EMAC color bar). Therefore, we believe that Figs 1 and 2 are more representative with the current color bars.

5. *End of Section 4: It would be valuable to mention here that the NOfree simulation compares well with observations. That would also give an additional argument for examining NOfree in section 5.*
   We added a new sentence at L252: "Interestingly, the ICNC zonal mean computed with the NOfree

simulation (not shown) is closer to the observations with respect to the REF simulation, suggesting that the FREE tendency contributes to the overestimation of ICNC."

6. *Page 13, line 296: DETR is independent of updrafts? Why, then, is DETR higher over land?*
DETR depends on the detrained mass flux, which depends on the convective mass flux or pressure velocity and ultimately vertical velocity. These vertical velocities are higher over land because the thermal structure is favorable for generation of larger buoyancies [Del Genio et al. 2007]. In EMAC, DETR is computed on the basis of the cloud condensate parameterized by the CONVECT submodel, which simulates convective clouds and does depend on updrafts as written at L123: "Convective cloud microphysics in EMAC is solely based on temperature and updraft strength...".

7. *Page 16, line 310-312: It does not look like NCIR is the dominant source of ICs between 350 and 200 hPa. Based on Fig. 6, it seems to dominate only above approximately 200 hPa level.*
We agree that the sentence was not correct; we made the sentence more specific: "We clearly see that ice nucleation in the cirrus regime (NCIR) is the dominant source of ICs at pressures lower than 200 hPa between the tropics and lower than 350 hPa at high latitudes."

8. *Page 20, line 397: Did you mean homogeneous freezing level instead of melting level?*
Yes, thank you for noticing this; we corrected the sentence.

9. *Page 20, lines 406-407: Does the ice water content change between FUT and REF simulations?*
The global means of ice water path (IWP) in REF, $(12.71 \pm 0.27)\mathrm{g/m^2}$, and in FUT $(12.13 \pm 0.25)\mathrm{g/m^2}$, are very close; the zonal mean of ice water content (IWC) in FUT (not shown) increases in the upper troposphere from the homogeneous freezing level. Since the entire paper focuses on ICNC, we did not include this information in the manuscript and we deleted the sentence at L410-412.

10. *Page 21, line 434-435: "consists in" reads a bit awkward. Also, please elaborate on the statement "FREE should not depend only on CDNC". On what does it depend?*
Thank you for noticing this; we replaced it with "consists of".
At L433, we meant that FREE could depend also on updraft velocity like in Muench and Lohmann (2020), as we wrote at L430. We made this paragraph clearer in the manuscript.

11. *Page 21, line 457: Did you refer to the homogeneous "freezing level"?*
Yes. We changed "freezing level" to "homogeneous freezing level" also in other parts of the manuscript to be clearer.

12. *Page 21, line 458: You can omit "vertical".*
We removed "vertical".

Further changes

- [L330-335] "It must be stressed that the IC sources and sinks of Figures 6 and 7 cannot be expected to balance for *the following* reasons. First, the tendencies of physical processes are not computed in this study, i.e. transport due to advection, turbulence, and convection and sedimentation (R_{transp} and R_{sedi} in equation (1), respectively). In particular, R_{transp} is not computed in the CLOUD submodel but derives from various submodels in EMAC, e.g. CVTRANS (Tost et al., 2010) and E5VDIFF (Roeckner et al., 2004). Second, *sublimation is a missing sink in this study. Finally,* numerical tendencies also affect ICNC at each model time step and play a significant role in the ICNC budget (as discussed in Subsection 5.1)."

- [Section 4] We specified that the model results in Figs. 1 and 2 refer to the REF simulation.

- [L468] "The *tendencies of transport, sedimentation, and sublimation* could also be included to close the ICNC budget."