Authors' reply to Referee #2

We thank the anonymous Referee #2 for the helpful comments. Below, we provide our replies to each comment; number of lines and sections refer to the old manuscript.

Major Comments

(1) The model result uncertainty could be very large from a few aspects.
(1.1) The model grid spacing is very coarse (300 km) and the output time frequency is very sparse (every 5 hours). Many times, the cloud lifetime can be even less than 10 hours, then the sampling cannot be representative with every 5-hour time frequency. I'd suggest look at the sensitivity to model resolution (such as 100 km) and output time frequency (hourly) to meet the goal of quantification.

We agree with the observations raised by the Referee. It would be interesting to perform sensitivity runs and investigate the influence of spatial and temporal resolutions on the tendencies. However, running new simulations at various resolutions with hourly output frequency would require much time, and new analysis should be performed. This is not the objective of this paper and could be addressed as an independent study. We mentioned at the end of the Conclusions that this can be an interesting future study.

(1.2) Need to do ensemble runs for quantification.

Also in this case, running ensemble experiments would require much time. Nevertheless, in the revised manuscript, the simulations were run for five years (instead of one year) so the analysis of the tendencies is now more robust.

(1.3) Need to discuss that the results might be changed with different models or different physical parameterizations such as cumulus or microphysics parameterizations.

In this regard, we added some new lines at the end of the Subsection 4.4.1, where we already discussed the sensitivity of the results to microphysics parameterization changes, and also in the Conclusions.

(2) For the sink of ice crystal, sublimation should be considered.

Unfortunately, as replied to Referee #1 point *II)*, the sublimation term is combined with SEDI; the separation between sublimation and sedimentation is not straightforward, and we cannot estimate the sublimation impact individually.

(3) Result section: I feel a little surprised that the authors started the discussion of results for the source and sink of ice directly. It would be nice to understand the overall model performances in simulating radiation, clouds and precipitation first. Then get to the analysis of ice crystal number concentrations and its budget.

We added a new section (4 "*Model results and evaluation of ICNC*") in the revised manuscript to evaluate the model ICNC against satellite ICNC retrievals before starting with the analysis of the tendencies.
The understanding of the overall model performance in simulating radiation, clouds and precipitation goes beyond the scope of this paper. The EMAC model is continuously de-

veloped, tested, and evaluated (against observations and other model results). The EMAC model and all its improvements are well documented in papers of the Special Issue "The Modular Earth Submodel System" of Copernicus and in the MESSy Consortium Website (https://www.messy-interface.org). Section 2.1 provides the standard description of EMAC; L94-95 cites some of the studies which deal with the model performance in simulating different physical quantities (e.g. aerosol burdens, cloud cover, radiation, cloud radiative effects...).

(4) Since one of the purposes of the study is to test the sensitivity to two other nucleation parameterizations, then some description about the two default and two tested schemes is needed, particularly about how different they are in terms of representing ice formation such as temperature dependent, supersaturation dependent, and aerosol dependent.
If aerosol dependent, then what aerosols are considered? Why did you replace the immersion freezing scheme with a contact freezing scheme? Shouldn't they be considered together?

The differences between the ice nucleation schemes in cirrus regime and mixed-phase regime are detailed in Bacer et al. 2018 (in Sections 2.2., 2.3.1, and Figure 1). We added some information regarding the schemes and also the reference. We specified at L138 that the parameterizations for heterogeneous nucleation are aerosol dependent. The ice nucleation parameterizations working in the mixed-phase regime are listed at L135-138: immersion freezing is not replaced with contact freezing; contact nucleation is always considered via LD06; immersion nucleation can be simulated either via LD06 or P13 (which also simulates deposition nucleation). We made L135-138 clearer.

Minor Comments

1. *Calling everything below -35 deg C as "cirrus clouds" is not accurate. I would suggest change to "pure ice clouds".*
   According to the definitions provided, for example, by Krämer et al. 2016 and Heymsfield et al. 2017, and the terminology used in most of the literature, we consider "cirrus clouds" (i.e. clouds purely composed of ice crystals) equivalent to "pure ice clouds", and we would like to keep this terminology in the manuscript.

2. *For the convective detrainment, does the model treat the detrainment at the levels with T> -35 deg C? If not, is there a reason? Theoretically convective detrainment of droplet and ice can occur from middle to top troposphere.*
   Convective detrainment can occur also at $T > -35°$C: the cloud condensate at $T < -35°$C is considered in the ice phase (and it is a source of ICs), while the cloud condensate at $T > -35°$C is considered in the liquid phase (and it is a source of cloud droplets). This is explained at L121-122.

3. *Line 210-215, does FREE include the droplet freezing in convective parameterization?*
   FREE does not include ice crystals formed in convective pararmeterizations. FREE is an independent term defined in the CLOUD submodel (convection is simulated by another submodel, CONVECT), and it includes the ICs formed from liquid water droplets that are transported in regions where temperature is $< -35°$C, as written at L163.

4. *Section 4.2, how to reconcile that DETR is much larger than NCIR in zonal mean (Fig. 2) but smaller than it in global spatial distribution (Fig. 1)?*
   We are not sure what the Referee means here, as both Figure 1 and Figure 2 show that DETR is generally higher than NCIR, so the Figures are in agreement.

5. *Line 284-286, I am confused by this sentence. Earlier it is said LD06 is a contact freezing scheme which is for heterogenous freezing. Here you said LD06 parameterizes only homogeneous nucleation. Also P13 should be an immersion freezing scheme which should be much more efficient than the contact freezing LD06, but the results in section 4.4.1 did not even mention the differences they can make.*
   We thank the Referee for noticing that there is indeed an inconsistency at L285; we replaced *"LD06"* with *"KL02"*.
   Since NCIR and NMIX are defined as the rates of new ICs in the cirrus regime and new ICs in the mixed-phase regime, it is not possible to discern the contributions from contact and immersion freezing. However, during some previous tests, we found that immersion nucleation simulated with LD06 produces more ICs than immersion-condensation and deposition nucleation using P13. This is in agreement with Phillips et al. 2008, who compared their empirical parameterization (which is the previous version of P13) with other parameterizations including LD06.

References

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[3] Krämer, M., Rolf, C., Luebke, A., Afchine, A., Spelten, N., Costa, A., Meyer, J., Zöger, M., Smith, J., Herman, R. L., Buchholz, B., Ebert, V., Baumgardner, D., Borrmann, S., Klingebiel, M., and Avallone, L.: A microphysics guide to cirrus clouds Part 1: Cirrus types, Atmospheric Chemistry and Physics, 16, 3463–3483, 2016.

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