We really thank the anonymous Referee for the very constructive comments. Below we report our replies.

SPECIFIC COMMENTS

P4: *Give more details about the model. How are convective clouds treated? The two-moment cloud microphysics scheme in ECHAM also handles the freezing of the detrained condensate of convective clouds.*

As the Referee#2 has raised the same issue, the following information about convection has been added in Section 2.1.
"The CONVECT submodel contains multiple convection parameterizations (Tost et al., 2006). In this work the scheme of Tiedtke (1989) has been used. Convective cloud microphysics is highly simplified and neither explicit aerosol activation into liquid droplets nor aerosol effects in the ice formation processes are taken into account, i.e. convective microphysics is solely based on temperature and updraft strength. Detrainment from convection is treated by taking updraft (and downdraft) concentrations of water vapour and cloud condensate and the corresponding massflux detrainment rates into account. These are merged including turbulent detrainment (i.e. exchange of mass through the cloud edges) and organised detrainment (i.e. organized outflow at cloud top). The detrained water vapour is added to the large-scale water vapour field, while the detrained cloud condensate is directly used as a source term for cloud condensate by the large-scale cloud scheme (i.e. the CLOUD submodel), which considers the detrained condensate either liquid or ice depending on the temperature (if $T < 238$ K the phase is ice) and the updraft velocity. The size and numbers of the detrained condensate are not taken into account explicitly."

P4: *Also give details how dust is computed. If dust emissions are computed online they could be quite variable between simulations.*

Offline dust emissions from the AEROCOM data set were used in all simulations. We added this information in Section 2.4 (lines 9-13, P9), rather than at P4.
"All simulations have been run for 6 years (1 year as spin-up time plus 5 years for the analysis) using emissions starting from the year 2000 (GFEDv3.1 from van der Werf et al., 2010 for biomass burning and CMIP5-RCP4.5 from Clarke et al., 2007 for anthropogenic emissions). As in Pozzer et al. (2012), dust is offline prescribed using monthly emission files based on the AEROCOM data set (Dentener et al., 2006). Also volcanic and secondary organic aerosol emissions are based on AEROCOM, while GFEDv3.1 and CMIP5-RCP4.5 have been used to simulate emissions of black carbon and organic aerosols, respectively. Finally, aerosol climatologies have been used for the interactions with radiation (Tanre et al., 1984) and heterogeneous chemistry (Aquila et al., 2011). Prescribed climatologies of sea surface temperatures (SST) and sea-ice concentrations (SIC) from AMIP (30 years: 1980-2009) have been used as boundary conditions."

P4: *Describe how clouds and aerosol-particles interact. Droplet formation is mentioned later but should already be mentioned here. Which of the aerosol modes/species are used in the activation parameterization?*

We added the following information in Section 2.1, P4 (and we reduced lines 17-19, P9).
"Cloud droplet formation is parameterized by the "unified activation framework"

(UAF) (Kumar et al., 2011; Karydis et al., 2011). It is an advanced physically based parameterization which merges two theories: κ-Köhler theory (KT) (Petters and Kreidenweis, 2007), which governs the activation of soluble aerosols, and Frenkel-Halsey-Hill adsorption activation theory (FHH-AT) (Kumar et al., 2009), which describes the droplet activation due to water adsorption onto insoluble aerosols (e.g., mineral dust). Aerosol modes that consist of only soluble material follow the KT, and the required effective hygroscopicity (κ) is calculated based on the chemical composition of the mode as described by the ISORROPIA thermodynamic equilibrium model (Fountoukis and Nenes, 2007). Aerosol modes that consist of an insoluble core with soluble coating follow the UAF scheme, which takes into account the effects of adsorption and absorption on the cloud condensation nuclei (CCN) activity of the mixed aerosol. More details about the UAF scheme and its implementation in the EMAC model can be found in Karydis et al. (2017)."

P7L29-P8L1: *Soot particles are considered as ice nucleating particles (INP) for cirrus clouds ($T<238$ K). Whether soot particles initiate freezing at these cold temperatures and at supersaturations below the threshold for homogeneous nucleation is controversial (Kanji et al., 2017). The motivation and impact for choosing soot particles as INP for cirrus clouds need to be discussed.*

We chose DU and BC as INPs for a technical reason explained probably too poorly at lines 25-27, P9. Phillips et al. (2013, P13) can consider the contribution of four species DU, BC, BIO, and soluble OC to immersion/condensation and deposition nucleation modes. However, the default configuration of EMAC (i.e. simulation KL+LD) takes into account only the contribution of DU and BC to immersion nucleation in the mixed-phase regime via LD06. Therefore, we decided to include only these species (DU and BC) for the computations of P13.
We modified the sentence at line 1, P8: "Dust and soot, the aerosol species considered in this work for the reasons explained in Section 2.4, ... ".
In Section 2.4, we made the last paragraph preciser: "The P13 parameterization is used to simulate deposition and immersion/condensation nucleation whenever BN09 is called (for the reasons explained in Subsection 2.3). Since LD06 takes into account only dust and soot for immersion nucleation, we set the same aerosol species as contributions for P13 and turned off the biological and organic contributions."

P8: *equations (4-5): Is the number of existing ice crystals subtracted from $N_{i,het}$ or are soot and dust particles removed from the interstitial aerosol after heterogeneous nucleation? If not the INP could "freeze" several times leading to unrealistically high ICNC.*

The number of new ice crystals formed heterogeneously is not subtracted to the interstitial aerosol, however, the reduction of aerosols is taken into account by the SCAV submodel, which simulates the nucleation scavenging. This means that the number of aerosols available for ice nucleation is "updated" by SCAV and there is no risk of counting several times the same particles as INP.

P8L17: *Is the dry diameter of sulfate in the Aitken soluble mode used or the dry diameter of the Aitken soluble mode?*
How is the dry diameter of sulfate in the Aitken soluble mode computed?
How sensitive is BN09 to this choice of INP diameter?

Thanks for pointing it out. As aerosols are internally mixed, the diameters are only computed for different aerosol modes (not for different aerosol species), thus,

we actually used the dry diameter of the Aitken soluble mode (not the diameter of sulfate). We corrected lines 17, P8 and 1, P20.
The dry diameter of the Aitken soluble mode is computed by the aerosol model GMXe. It is then used by BN09 to compute the diameter of cloud droplets using an approximation (linearly dependent on the dry diameter of the Aitken soluble mode) derived from the equilibrium calculations proposed by Lewis et al. 2008.
Based on few tests using the BN09 offline, the dependency to the diameter is very weak.

All figures *showing zonal and annual means (Figs. 2-4,S1-3): These figures need to show some measure of significance.*

We estimated the statistical significance using the Welch's t-test and we marked the areas with 95% level of significance in all plots which show relative percentage changes. All figures were modified accordingly.

P10L7-10: *Why is TKE higher at lower altitude in BN+LD?*
Could the changes in the mixed-phase regime in Fig. 2b,f be due to increased sedimentation of larger ice crystals from cirrus clouds?

The sentence regarding TKE does not explain why there is a positive bias in the mixed-phase regime in the comparison of BN+LD with KL+LD (i.e. Figure 2b), therefore we removed such sentence.
As noticed by the Referee, BN09 produces larger ice crystals which sediment faster from cirrus clouds, thus, lines 8-10 were modified as follows.
"Interestingly, ICNCs at lower altitudes are also influenced by the ice nucleation parameterization used in the cirrus regime. In fact, there is an increase of ICNCs in the mixed-phase regime probably due to a faster sedimentation of the larger ice crystal produced by BN09 in cirrus clouds, especially in the NH where there are larger sources of efficient ice-nucleating mineral dust."

P11L1-2: *What is the explanation for this decrease in IWC while ICNC increase? Are this changes significant?*

The new plots (with significance levels) show that, in the mixed-phase regime, the IWC decrease and the ICNC increase overlap and are significant only in a small area, at high latitude and around 700 hPa. A possible reason could be attributed to the different ice crystal sizes in this area.
Lines 1-4, P11 were changed: "On the other hand, IWC in KL+BN slightly reduces (up to 20%) in the mixed-phase regime in areas where ICNC increases, especially in the NH at high latitudes (Figure 2g). This could be due to the the different sizes of ice crystals, however, the areas with significance are rather small."

P11L8-9, L16-17: *The reason for the high ICNC concentrations in the Himalaya region and Antarctica (e.g. Fig. 2 or Fig. S4) is not discussed. Due to the coarse resolution of the simulations, the topography may not be resolved well. Using a high resolution topography dataset, Gryspeerdt et al. (2017) identify cirrus clouds over Antarctica as primarily synoptic cirrus clouds not primarily orographic cirrus clouds.*

Naturally, the resolution of the topography is quite coarse, about 300 km x 300 km at the equator, and both the Himalayan region and Antarctica are represented as wide and very high plateaus (see Figure 1 of this document). The high values of ICNCs over the Himalayan region (and Andes) at 200 hPa are related to the high values of the turbulent contribution to the vertical velocity (w_{sub}, Figure 2-left of this document). On the other hand, the high values of ICNCs over Antarctica at 600 hPa can be related to the high values of both w_{sub} (Figure 2-right) and ice supersaturation in this area. We discussed it at lines 9-8 and 16-17, P11.

"ICNCs in the cirrus regime (Figure 3a) show areas with high values over land and in correspondence with mountainous regions, e.g. the Rocky Mountains, Andes, and Tibetan Plateau with ICNCs $>$ 500 L^{-1}. Such pattern is strongly related to the turbulent contribution of the vertical velocity w_{sub} and in agreement with Gryspeerdt et al. (2017), who detected in these areas mostly orographic cirrus clouds. Figure 3a also shows higher ICNCs around the edge of the Antarctic ice sheet and over those regions which experience a strong convective activity, i.e. the Inter Tropical Convergence Zone (ITCZ) and the Tropical Warm Pool (TWP), as observed in Sourdeval et al. (2018)."

"At 600 hPa, ICNCs increase towards high latitudes, in particular over Greenland (up to 2000 L^{-1}) and Antarctica (mostly $>$ 2000 L^{-1}) (Figure 3e). It must be said that, due to the very low temperatures in the the latter region, even at 600 hPa the conditions are typical of the cirrus regime, and the high ICNCs can be related to the high values of both w_{sub} and ice supersaturation. Gryspeerdt et al. (2017) found that cirrus clouds over Antarctica have primarily synoptic origin. However, differently from Figure 3e, observations do not present such a high peak of ICNC over Antarctica (Gryspeerdt et al. 2018; Sourdeval et al., 2018)."

P15L1-2: *These high values of SCRE and LCRE in the default simulation are surprising. As can be seen from your Table 2, the observed values of both, SCRE and LCRE are lower. The default simulation needs to be retuned to better match the observed values. Is this simulation in radiative balance at the top of the atmosphere? If not the comparison of CRE of the different simulations to observations is not very meaningful. Add the net radiative balance at the top of the atmosphere to Table 2.*

Actually, the default simulation is not in radiative balance at TOA (the imbalance is about 4.5 W/m^2). Nevertheless, we preferred to keep the physical parameters constant in all simulations, so to show all the differences arising when using the BN09 algorithm, also from the radiative point of view. In addition, the simulations using BN09 in the cirrus regime are well balanced, and in line with the work of Roeckner et al. (2004, Table 1). We agree that the comparison of CRE with observations can be less meaningful, but we preferred to show it for completeness.

As suggested, we added in Table 2 the radiative fluxes of SW, LW, and the net imbalance at TOA.

P16L1: *Are some of the quantities in Table 2 tuned to agree with observed values (Mauritsen et al. 2012, Hourdin et al, 2016)?*

None of the quantities in Table 2 has been tuned.

P16L5: *It is mentioned previously in the text that homogeneous nucleation dominates in the tropics and in the SH, whereas heterogeneous nucleation is important in the NH. Would it be possible to split the observations and the analysis in this section into the tropics and the NH extratropics?*

Unfortunately, we cannot split the observational data set into tropics and midlatitudes because this analysis is a work in progress by Krämer et al. (paper in preparation).

P17L3-4: *What is the reason for the better performance of BN09 compared to KL02 at low temperatures? I would assume that both schemes compute homogeneous nucleation at these low temperatures and that the vertical velocities are similar.*

Unfortunately, Figure 5 is affected by an error made during the post-processing and has been replaced by Figure 3 of this document. The results in the mixed-phase regime remain basically unchanged (right plot). In the cirrus regime (left plot), the simulations KL+LD and KL+BN undergo big differences at temperatures below 225 K, and the strong underestimation at very cold temperatures is not evident anymore. The simulations BN+LD and BN+BN show only slight changes which make them a bit closer to the observations (in the intervals 185-190 K and 202-226 K). We are sorry for the mistake. Now, at very cold temperatures ICNCs simulated using BN09 in the cirrus regime are lower than the ICNCs computed by KL+LD and KL+BN, as expected.

The text in Section 4.2 has been modified accordingly to the new Figure. Moreover, we mentioned some comparisons with other modeling studies as the Referee#2 suggested.

"Again, the simulations can be grouped in two sets according to the ice nucleation scheme used in the cirrus regime, i.e. KL+LD/KL+BN and BN+LD/BN+BN, because of their similarities. For most of the temperature range, the simulations which use KL02 in the cirrus regime overestimate the observed ICNCs (although they mostly remain below the 75^{th} percentile). The overestimation of ICNCs is common to other modeling studies (e.g. Wang and Penner, 2010, Liu et al., 2012, and Shi et al., 2015) and especially in cold cirrus clouds (for $T < 205$ K). On the other hand, the simulations which use BN09 in the cirrus regime are very close to the observations at temperatures below 200 K and between 220 K and 230 K, while they underestimate ICNCs between 200 K and 220 K. In this temperature range the simulations can exceed the observed 25^{th} percentile (although remaining within the 5^{th} percentile). In comparison with the other two simulations, BN+LD and BN+BN always predict lower ICNCs at temperatures below 230 K, as expected because of the competition and PREICE effects. Finally, all four simulations overestimates ICNCs by one order of magnitude in the temperature range $230 - 240$ K.

Overall, the simulations BN+LD and BN+BN agree particularly well with the measurements at temperatures lower than 200 K but underestimate the ICNCs within the interval $200 - 220$ K, due to an overestimation of the competitive nucleation and PREICE effects. Barahona et al. (2010) showed that the competitive nucleation effect is small using P13. Also, Liu et al. (2012) found that BN09 (using the parameterization of Phillips et al., 2008 for heterogeneous nucleation) and BNhom produced very similar results in the cirrus regime, suggesting that the competive nucleation effect was small because of the low ICNCs formed heterogeneously. Thus, we can deduce that the PREICE effect is the one which is likely overestimated in our simulations. Interestingly, modeled ICNCs do not show any particular trend, like also Kuebbeler et al. (2014) who used ECHAM-HAM. Differently, other studies found that ICNCs are inversely proportional with temperature, e.g. Liu et al. (2012) and Shi et al. (2015) with CAM5, indifferently if they used the ice nucleation scheme of Liu and Penner (2005) or BN09, and Barahona et al. (2010) with GEOS-5 and BN09. Such distinct behaviours are likely derived from the wide model variability in reproducing subgrid-scale processes, like vertical velocity, which play a role in ice nucleation. We reiterate that ICNC is highly dependent on the vertical velocity which is usually poorly represented in terms of spatial and temporal variability (Barahona et al., 2017). ”

The lines 9-11, P1 (Abstract) changed to: “Overall, ICNCs agree well with the observations, especially in cold cirrus clouds (at temperatures below 205 K), although they are underestimated between 200 K and 220 K. As BN09 takes into account processes which were previously neglected by the standard version of the model, it is recommended for future EMAC simulations.”

The lines 2-4, P19 (Conclusions) changed to: “Overall, all modeled results agree well with global observations and the literature data. The comparison made with flight measurements has pointed out that ICNCs are overestimated by KL02 in the cirrus regime. BN09 agrees well with the observations in cold cirrus clouds, however, the PREICE effect is likely overestimated causing the underestimation of ICNCs between 200 K and 220 K.”

P17L25-26: *In Fig. 2b for example an increase in ICNC in the mixed-phase regime is shown when using BN09 in the cirrus regime. How does this agree with the similarity of the ICNCs of the different simulations in the mixed-phase regime compared to the aircraft measurements?*

It must be said that the “similarities” in the mixed-phase regime shown in Figure 5 (right) can be actually equal to absolute differences of 200 1/L for temperatures below 250 K (please note the log scale on the vertical axis). In fact, if we consider, for example, the ICNC values at T=238 K in Figure 3-right of this document, we can observe that the simulations BN+LD (green) and BN+BN (red) are higher than DEF (blue) by almost 200 1/L. ICNCs shown in Figure 3-right are nothing else that the mean computed along the latitudes of the ICNCs shown in Figure 4-left in this document (to be precise, Figure 3 actually shows the medians, which are a bit smaller than the means). The absolute differences between DEF and BN+KL are shown in Figure 4-center. If we average the differences along the latitude (Figure 4-right), we find that the differences at T=238 K are about 150 1/L. Thus, the differences shown in Figure 2 (of the manuscript, left column) are strongly smoothed by averaging along the latitudinal dimension, reducing the differences in the mixed-phase regime of Figure 3-right.
The reason why the simulations are different from the observations is explained at lines 28-29, P17, why the two data sets of observations are different is explained in the next point.

P17L26-28: *Give references for WISP-94 and ICE-L.*
Why are these two datasets so different (the 25th to 75th percentile do not overlap)?

We added the references of both projects.
For the project WISP-94 an optical array probe for airborne measurements was used, while the data of project ICE-L come from the Continuous Flow Diffusion Chamber (more information can be found in the “Supporting information” of DeMott et al. 2010). Thus, the differences between the two data sets are due to the employment of different instruments and to the ice particle shattering which affects the probe measurements (producing a positive bias).

P19L5-7: *I agree with your point (1) and (3) but the general better performance of BN09 compared to the default parameterizations is not conclusively shown. While BN09 performs better at $T < 205K$, there will be fewer and optically thinner clouds at these low temperatures than at the temperature range 205-222K where BN09 agrees less* *well with aircraft observed in-cloud ICNC than the default parameterizations. In my opinion the additional processes computed by BN09 outweigh this drawback and BN09 should be used in future EMAC simulations but a generally better performance cannot be asserted.*

The Referee is right. A better performance of BN09 could not and cannot (with the new Figure 3) be established. We modified lines 5-9, P19 as follows.
"As BN09 takes into account additional processes which were previously neglected by the standard version of the model, without consuming extra computational resources, we recommend to apply this ice nucleation scheme in future EMAC simulations. We also suggest to select P13 among the INP parameterizations available in BN09, since it incorporates the ice-nucleating ability of different aerosol species (dust, soot, bioaerosols, and soluble organics) and simulates both deposition and immersion/condensation nucleation."

P19L31-P20L2: *As this is interstitial aerosol, at what relative humidity is the wet diameter of the sulfate aerosol in the Aitken mode computed?*

As mentioned before (see point P8L17), we used the dry diameter of the Aitken soluble mode.
The wet diameter is calculated by the aerosol model GMXe based on the relative humidity computed online by the model.

TECHNICAL CORRECTIONS

P1L5: *Only one of the multiple ice nucleating particle spectra is applied in one simulation. Rephrase the sentence as it reads now as if the multiple ice nucleating particle spectra are applied simultaneously.*
Also INP spectra should be replaced by INP parameterization.

We changed the sentence: "Furthermore, the influence of chemically-heterogeneous, polydisperse aerosols is considered by applying one of the multiple ice nucleating particle parameterizations which are included in BN09 to compute the heterogeneously formed ice crystals."
We changed the words "spectrum" and "spectra" with "parameterization(s)" in the whole manuscript.

P2L7: *The greenhouse effects dominates for cirrus clouds e.g. Chen et al. (2000).*

We added such information: "... they scatter solar radiation back into the space (albedo effect) and absorb and re-emit longwave terrestrial radiation (greenhouse effect). Differently from other types of clouds, cirrus clouds produce a net warming at the top of the atmosphere (TOA) (e.g. Chen et al. 2000, Hong et al., 2016, Matus and L'Ecuyer, 2017)."

P2L9: *Mixed-phase clouds can also occur at colder temperatures, rephrase.*

We used the temperature threshold $-35°C$ (e.g. Lohmann et al. 2009), expressed in Kelvin as requested by the Referee#2.
We changed the sentence to: "In addition, mixed-phase clouds consist of both supercooled liquid cloud droplets and ice crystals and appear at subfreezing temperatures above 238 K."

P2L10-11: *This is only true when deep convective clouds are included in the term mixed-phase clouds, but deep convective clouds are often named separately. As you here include deep convective clouds, this should be mentioned explicitly.*

We preferred to remove such sentence as it was outside the issues treated in this paper. Rather, we included some short information about precipitation and cloud electrification here and about secondary ice production at line 31, P2.
"As ice crystals can grow quickly to precipitation-sized particles, precipitation is mainly formed in mixed-phase clouds, while precipitation from cirrus clouds does not usually reach the surface (Lohmann 2017). The mixed phase is also important for cloud electrification and intracloud lightning, which occur through the in-cloud charge separation via a transition from supercooled raindrops to graupel over the mixed-phase temperature range (Korolev et al. 2017)."
"The cirrus regime ... The mixed-phase regime ... In the latter regime, besides primary nucleation, another mechanism which controls ICNCs is the secondary ice production, i.e. the production of new ice crystals via the multiplication of pre-existing ice particles without the action of INPs."

P2L11-12: *Provide here references such as McCoy et al. (2016).*

Done.

P2L25: *Explain what you mean here by "the overestimation of vertical velocity".*

We added some new lines to explain it better.
"Based on modeling studies, homogeneous nucleation has been considered the dominant process for cirrus formation (e.g. Haag et al., 2003; Gettelman et al., 2012) because the concentration of liquid droplets is higher than that of INPs in the upper troposphere. However, some field measurements found a predominance of heterogeneous nucleation and lower ice crystal number concentrations (ICNCs) than produced by homogeneous nucleation (e.g. Cziczo et al., 2013; Jensen et al., 2013). What process is dominant is still under debate, although recent studies suggested the overestimation of the vertical velocity as possible cause of the discrepancy between modeled results and observations (e.g. Barahona and Nenes, 2011; Zhou et al., 2016; Barahona et al., 2017)."

P2L35-P3L1: *This sentence is true for mixed-phase clouds while the sentences before and afterwards concern cirrus clouds. This is confusing, rephrase or move this sentence.*

The PREICE effect concerns both the mixed-phase and the cirrus regimes and does not include the "condensation onto pre-existing cloud droplets". We removed that part of the sentence and we reformulated lines 32-35, P2 and 1-2, P3 as follows.
"This competition between homogeneous and heterogeneous nucleation for water vapour drastically affects the ICNC in the cirrus regime, even at low INP concentrations (Kärcher and Lohmann, 2003; Spichtinger and Cziczo, 2010). On the other hand, both in the cirrus regime and in the mixed-phase regime, water vapour can also be reduced by depositional growth onto pre-existing ice crystals and ice crystals carried into the cloud via convective detrainment and advective transport, thus, inhibiting ice nucleation. The impact of pre-existing ice crystals (PREICE) can be especially important in cirrus clouds, when..."

P3L12: *Do you mean numerical parcel model simulations?*

Yes, we corrected the expression.

P6L21-22: *Is this reduction done only for cirrus clouds are also for mixed-phase clouds?*

It is actually done only for cirrus clouds. We modified the sentence to: “The only expedient adopted by the CLOUD submodel is to reduce the number of aerosol particles available for ice nucleation by the existing ice particle number in the cirrus regime.”

P6L33: *Is the cloud parcel mentioned here explicitly computed in EMAC or do the equations (1-6) provide analytical solutions for the cloud parcel?*

We used here the expression “cloud parcel” just to explain what happens when INPs overcome a certain threshold. There are no explicit computations of cloud parcels. To avoid misunderstandings, we simply deleted the part “that develops in the cloud parcel”.

P9L10-14: *Provide references for the anthropogenic aerosol emissions and describe how natural aerosols (e.g. dust) are treated.*

We improved the description of aerosol emissions between lines 9-13, P9 as written previously at point P4.

P8L6: *Is n_x the number of interstitial aerosol particles or is the number of aerosol particles in cloud droplets tracked? Please clarify.*

According to Phillips et al. 2008 and 2013, n_x is the number of aerosol particles “including interstitial IN and IN immersed in cloud liquid”. However, with the implementation of BN09 in EMAC, P13 uses only interstitial aerosols. We clarified this in Sections 2.3.1 and 2.3.2:
“..., n_x is the number concentration of aerosol particles (interstitial and INP immersed in cloud droplets) of species X, ...” at line 7, P8.
“... of interstitial aerosol of species X (which can be ...” at line 19, P8.
Indeed, in immersion/condensation ice nucleation parameterizations it is usually assumed that each INP corresponds to exactly one cloud droplet which freezes when the INP reaches its characteristic freezing temperature, as discussed in Paukert et al. 2017.

P10L26-P11L1: *Do you mean that IWC decreases where ICNC decreases?*

Yes, we meant that, but actually the sentence is unclear.
We rephrased it: “The relative changes in Figure 2f show a pattern very similar to Figure 2b, therefore, IWC decreases where ICNC reduces (and vice versa) when BN09 is used in the cirrus regime.”

P11L32-P13L2: *The increase in IWC in equatorial regions at 200 hPa is about 5-10% (Fig. 2), I would not call this dramatic.*

We deleted this part, as IWC does not increase “dramatically” but increases where also ICNCs increase.

P13L21-23: *It should be mentioned that observations of cloud droplet number concentration are uncertain (Bennartz and Rausch, 2017).*

Done. Now, the sentence is:
“Vertically integrated cloud droplet number concentration ($CDNC_{burden}$) is not influenced by the choice of the ice nucleation scheme. Its values are comparable with previous modeling studies (e.g. Lohmann et al., 2007; Hoose et al., 2008; Salzmann

et al., 2010; Wang and Penner, 2010; Kuebbeler et al., 2014; Shi et al., 2015) and observations, although satellite observations are still affected by strong uncertainties (Bennartz and Rausch, 2017)."

P13L27-29: *The underestimation of IWP was found in previous studies using ECHAM-HAM. The IWP in ECHAM is not underestimated see e.g. Mauritsen et al. (2012).*

Thanks, we changed "ECHAM" with "ECHAM-HAM".

P17L28-30: *Please add the number of hours in mixed-phase clouds.*

We added this information: "The modeled ICNCs are in rather good agreement with two data sets of flight measurements taken from the projects Winter Icing Storms Project (WISP-94) and Ice in Clouds Experiment-Layer Clouds (ICE-L), which consider about 99 and 46 flight hours, respectively."

P17L30: *When the measurements are for INP this needs to reflected in Fig. 5 itself (at least in the figure caption).*

We added such information in the caption of Figure 5.

P18L5: *Use INP parameterization instead of INP spectrum.*

We changed the words "spectrum" and "spectra" with "parameterization(s)" in the whole manuscript.

Caption Fig. S4: *Give references for the observational datasets.*

Done.

Fig. S5 *is a table not a figure.*

Thanks, we corrected it.

References

- Aquila, V., et al.: MADE-in: a new aerosol microphysics submodel for global simulation of insoluble particles and their mixing state, Geosci. Model Dev., 4, 325–355, 2011.
- DeMott, P.J., Prenni, A.J., Liu, X., Kreidenweis, S.M., Petters, M.D., Twohy, C.H., Richardson, M.S., Eidhammer, T., and Rogers, D.C.: Predicting global atmospheric ice nuclei distributions and their impacts on climate, PNAS, 107, 25, 11217–11222, 2010.
- Fountoukis, C., and Nenes, A.: ISORROPIA II: a computationally efficient thermodynamic equilibrium model for K^+-Ca^{2+}-Mg^{2+}-NH_4^+-Na^+-SO_4^{2-}-NO_3^--Cl^--H_2O aerosols, Atmos. Chem. Phys., 7, 4639–4659,2007.
- Hong, Y., Liu, G., and Li, J.-L. F.: Assessing the Radiative Effects of Global Ice Clouds Based on CloudSat and CALIPSO Measurements, Journal of Climate, 29, 7651–7674, 2016.
- Karydis, V.A., Kumar, P., Barahona, D., Sokolik, I.N., and Nenes, A.: On the effect of dust particles on global cloud condensation nuclei and cloud droplet number, J. Geophys. Res.-Atmos., 116, 2011.
- Kumar, P., Sokolik, I.N., and Nenes, A.: Parameterization of cloud droplet formation for global and regional models: including adsorption activation from insoluble CCN, Atmos. Chem. Phys., 9, 251–2532, 2009.
- Kumar, P., Sokolik, I. N., and Nenes, A.: Cloud condensation nuclei activity and droplet activation kinetics of wet processed regional dust samples and minerals, Atmos. Chem. Phys., 11, 8661–8676, 2011.

- Lewis, E.R.: An examination of Köhler theory resulting in an accurate expression for the equilibrium radius ratio of a hygroscopic aerosol particle valid up to and including relative humidity 100%, J. Geophysical Res., 113, D03205, 2008.

- Lohmann, U., and Hoose, C.:Sensitivity studies of different aerosol indirect effects in mixed-phase clouds, Atmos. Chem. Phys., 9, 8917–8934, 2009.

- Matus, A.V., and L'Ecuyer, T.S.: The role of cloud phase in Earth's radiation budget, Geophys. Res. Atmos., 122, 2559–2578, 2017.

- Paukert, M., Hoose, C., and Simmel, M.: Redistribution of ice nuclei between cloud and rain droplets: Parameterization and application to deep convective clouds, J. Adv. Model. Earth Syst., 9, 514–535, 2017.

- Petters, M. D., and Kreidenweis, S. M.: A single parameter representation of hygroscopic growth and cloud condensation nucleus activity, Atmos. Chem. Phys., 7, 1961–1971, 2007.

- Roeckner, E., Brokopf, R., Esch, M., Giorgetta, M., Hagemann, S., Kornblueh, L., Manzini, E., Schlese, U., and Schulzweida, U.: The atmospheric general circulation model ECHAM5, Part II, Report No. 354, Max-Planck-Institut für Meteorologie, 2004.

- Tanre, D., Geleyn, J.-F., and Slingo, J.M.: First results of the introduction of an advanced aerosol-radiation interaction in the ECMWF low resolution global model, in: Aerosols and their climatic effects, edited by: Gerber, H. and Deepak, A., A. Deepak Pub., 133–177, 1984.

- Tost, H., Jöckel, P., and Lelieveld, J.: Influence of different convection parameterisations in a GCM, Atmos. Chem. Phys., 6, 5475–5493, 2006.

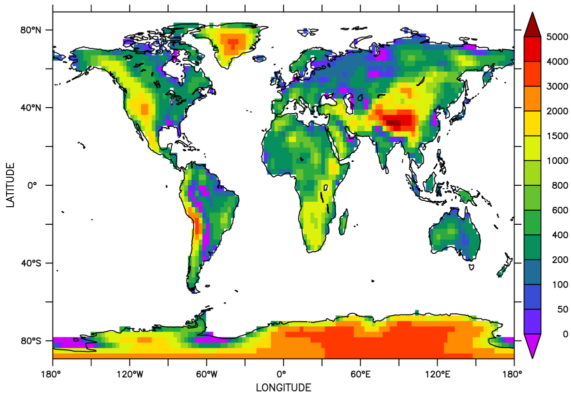

Figure 1: Topography (m) of T42 horizontal resolution derived from surface geopotential.

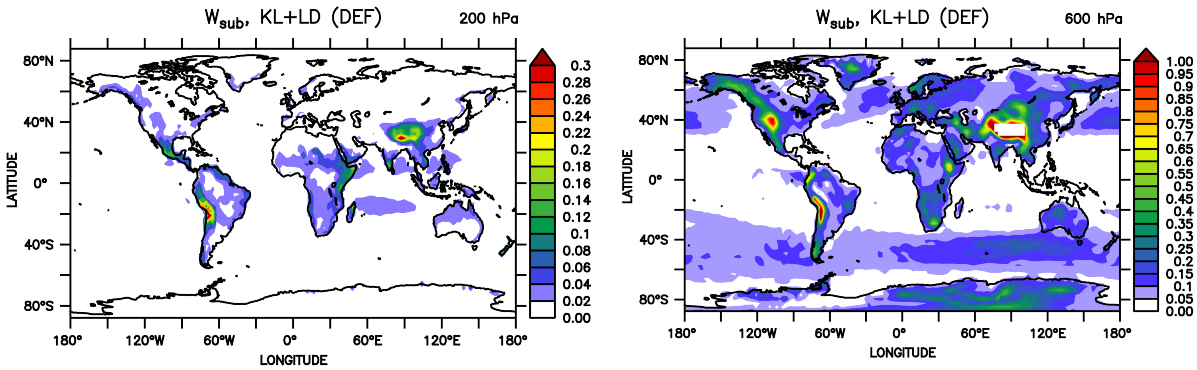

Figure 2: Annual means of $w_{sub} = 0.7\sqrt{TKE}$ (m/s) at 200 hPa and 600 hPa for the default simulation.

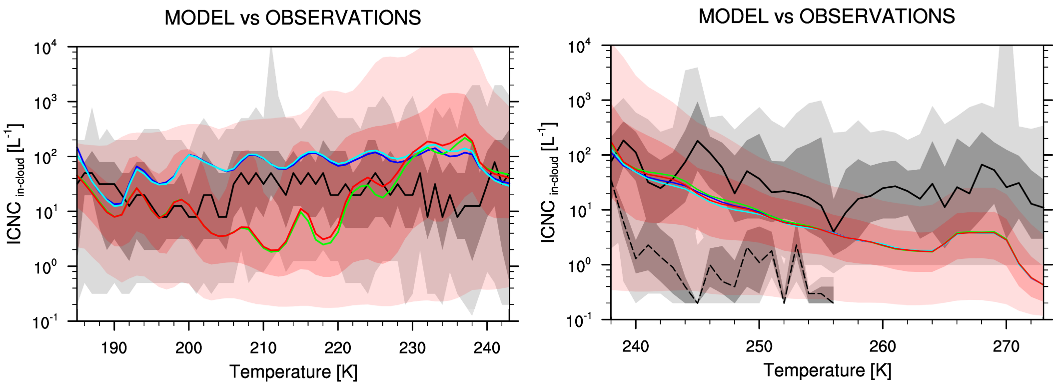

Figure 3: New Figure 5. Modeled in-cloud ICNC and flight measurements versus temperature. Lines are medians of KL+LD (blue), BN+LD (green), KL+BN (light blue), BN+BN (red), and observations (black). Shaded areas indicate 5th-95th and 25th-75th percentiles of observations and BN+BN.

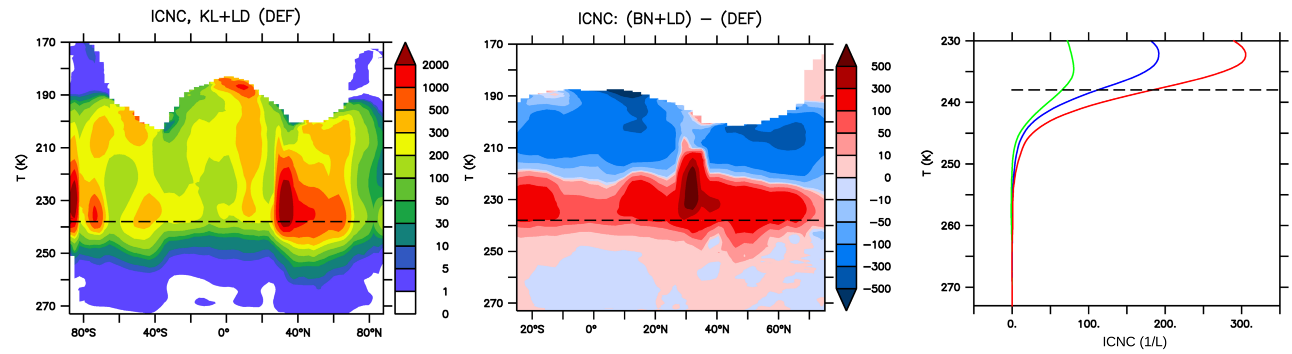

Figure 4: (To be noted: vertical axes are temperature!) (Left) Annual zonal means of ICNC (1/L) of the default simulation. (Center) Absolute differences of ICNC annual zonal means between BN+LD and DEF, within 25S and 75N. (Right) Average along the latitude of: BN+LD - DEF (central figure, blue), KL+BN - DEF (green), BN+BN - DEF (red).