

We really thank the anonymous Referee for the constructive and fruitful comments. Below we report our replies to the “Specific comments”.

Title

Title sounds too technical: is there really a need to add “(based on MESSy 2.53)”. As a suggestion you could further simplify the title to something like: Implementation of a new ice phase/ice cloud parameterization in the EMAC model.

According to the GMD rules of “Manuscript composition” for the authors, the title has to be “concise but informative, including model name and version number if a model description paper”. Therefore, “(based on MESSy 2.53)” in the title is a journal requirement and we cannot change it.

Abstract

Needs to be rearranged, right now is in my opinion a bit out of a logical order. I suggest:
1.) *mention that you implemented BN09 for both cirrus and mixed phase clouds*
2.) *Only now go in details of homog. vs heterog. nucleation, aerosols, etc.*

If possible, we would prefer to leave the order as it is. Our logic is:

- 1) Saying in one sentence what is the manuscript about (the first two lines in the Abstract).
- 2) Describing the parameterization in order to inform the reader about the capabilities of the “tool” that we will employ.
- 3) Answering the question: How do we use it? We can use it in both regimes.
- 4) Major results.

Some minor comments:

line 2: *realistically represent => quite a bold statement*

As BN09 takes into account processes which were previously neglected by EMAC, we changed the sentence to:

“A comprehensive ice nucleation parameterization has been implemented in the global chemistry-climate model EMAC to improve the representation of ice crystal number concentrations (ICNCs).”

line 4: *cold clouds => never defined it*

We wrote “cirrus clouds” instead of “cold clouds”.

Moreover, since the PREICE effect was actually added in this work (it was not considered by the original BN09 algorithm), this sentence was changed to:

“The parameterization of Barahona and Nenes (2009, hereafter BN09) allows the treatment of ice nucleation taking into account the competition for water vapour between homogeneous and heterogeneous nucleation in cirrus clouds.”

Rather, we mentioned the PREICE effect at line 7:

“BN09 has been modified in order to consider the pre-existing ice crystal effect and implemented to operate both in the cirrus and in the mixed-phase regimes.”

lines 4-6: *the sentence starting with “Furthermore” is hard to understand. Please rewrite!*

This sentence was changed as follows (see also Referee#1, P1L5):

“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.”

line 7: *Compared to the standard EMAC “results”... => ...*

We changed the word “results” with “parameterizations” (they are KL02 and LD06).

line 10: *...improves the model results... => too vague, be more concrete, which results?*

Such sentence was modified as written in our reply to Referee#1, P17L3-4:

“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.”

Introduction

1st paragraph *sounds like ice nucleation and droplet activation are the only two challenging processes in the representation of clouds. Is this true?*

At line 15 it is written that the representation of clouds is one of the major challenges in climate studies, and this holds generally for many processes which occur in clouds (e.g. cloud phase transitions, INP characteristics influencing ice nucleation, secondary ice production mechanisms, aerosol-clouds interactions). Then, we just mentioned the liquid droplet activation and we focused on the ice crystal formation because it is the main process considered in this work. For clarity, we changed the sentence to:

“Nevertheless, clouds remain one of the less understood components of the atmospheric system, and their representation in models (including processes like cloud droplet formation, ice nucleation, cloud phase transitions, secondary ice production, aerosol-cloud interactions) is one of the major challenges in climate studies (IPCC, 2013; Seinfeld et al., 2016).”

P2L15: *I think the word elusive isn’t used in a correct way here.*

We changed “most elusive” with “less understood”.

P2L4-13: *Wouldn’t it be more logic to start with mixed phase clouds? After all, they have a larger radiative impact on climate and (regionally) on cloud feedbacks than cirrus.*

Through the whole manuscript we firstly wrote about the cirrus regime and then the mixed-phase regime, e.g. in Sections 2.3.1, 3.2, 4.2 (with Figures 3, 4, 5). We simply decided to follow the order “from high altitudes to low altitudes” (or “from low temperatures to warmer temperatures”) like the CLOUD submodel does (see Figure 1). For consistency with the rest of the manuscript, we would prefer to keep this order also in the Introduction.

P2L4-13: *you never defined what a “mixed-phase cloud” is.*

Mixed-phase clouds are defined at lines 7-9, P2 (where we changed the temperature threshold to 238 K).

P2L4: *typically below -35° => why typically? depends on your definition, as there is no global definition of what a cirrus cloud is. So if you in your manuscript go for the -35° threshold just say that firmly.*

+ here you use Celsius, while throughout the whole text Kelvin. Be consistent. (I personally don’t see any advantage of using Kelvin over Celsius, but that’s purely a matter of personal taste)

Thanks for pointing this out. We removed the word “typically”.

We used Kelvin because it is the unit of the International System and it is used by the model. To be consistent through the whole manuscript we changed the temperature values from Celsius to Kelvin in the Introduction: line 4 and line 8, P1: 238 K.

P2L6: *missing references on the radiative role of cirrus - maybe Matus and L’Ecuyer 2017, Hong et al. 2016 for some recent satellite estimate of their radiative effects or even Kienast-Sjogren et al. 2016 for lidar-based estimates, Gasparini and Lohmann 2016 for GCM modelling-based estimates. The first two references could be cited in the context of mixed-phase CRE too.*

We cited Matus and L’Ecuyer 2017, Hong et al. 2016 for the cirrus warming effect. For the mixed-phase clouds, we added the sentence:

“Mixed-phase clouds generates a net cooling at TOA, although the estimates of their radiative effects are complicated by the coexistence of both ice and liquid cloud phases (Matus and L’Ecuyer, 2017).”

P2L10: *missing some references on mixed-phase being TD unstable, or similar.*

We added the following citations: “(e.g. Korolev et al., 2007; Korolev et al., 2017).”

P2L11-13: *The section on mixed-phase is in a poor state. It deserves at least 1-2 sentences more, giving reference for the listed processes/facts (e.g. that mixed-phase, if we define it just by temperature threshold, is probably responsible for most/a large share of precipitation, which is different from the cloud top phase classification of Mulmenstadt et al. 2015).*

Are mixed phase responsible for lightning and storms?

Aren’t convective clouds treated by a different scheme in your model?

What do you mean by “strong storms”. That’s all written in a to ambiguous way for a scientific paper.

We added the following information regarding mixed-phase clouds (see also Referee#1, P2L10-11).

“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).”

Convective clouds are treated separately by the CONVECT submodel, but this information is given in Section 2.1.

As written in the reply to Referee#1 (P2L10-11), we removed the sentence mentioning “strong storm” in favour of the information written before.

P2L12: *there’s tons of references on that, why didn’t the authors include any? (e.g. Tan et al., 2016 , Science, studies looking more specifically into the Southern Ocean like Vergara-Temprado et al, 2018, PNAS and many more)*

We added the citations: McCoy et al., 2016; Tan et al., 2016; Vergara-Temprado et al., 2018.

P2L23-26: *I don’t think the upper 2 statements are totally correct, possibly due to a too condensed information. Early observational studies of cirrus clouds were affected by the problem of ice crystal shattering, which implied several times too large ice crystal number*

concentrations. Such numbers were hard to explain other than with homogeneous nucleation, and were also replicated by model studies.

Moreover, we have also numerous modelling studies (ok, Barahona et al. being one of them) showing that heterogenous nucleation might play a role in cirrus, for example: Sullivan et al., 2016, Storelvmo and Herger 2014, Penner et al. 2015, Gasparini and Lohmann 2016.

I don't think there is a universal agreement on the overestimation of vertical velocities by GCMs. A study by Joos et al. 2008 and Kärcher and Ström 2003 show a good agreement between vertical velocity observations and model updrafts. The updrafts were based on the large scale updraft and a TKE based term, which was in Joos et al. 2008 over mountains replaced by gravity waves. As Joos et al. use the same (I guess) dynamical core than the described model, we can imagine that the TKE based updrafts could be in line with observations.

And Cziczo et al., 2013 also isn't talking about updraft overestimation, despite being cited for it.

We extended these lines to better explain this issue (see also Referee#1, P2L25).

“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).”

We did not cite Kärcher and Ström (2003) and Joos et al. (2008) because the fact that simulated vertical velocity (given by the sum of large-scale vertical velocity and subgrid-scale TKE component) is in good agreement with observations does not mean that it is not underestimated (Fig. 9 in Kärcher and Ström (2003) shows that the modeled vertical velocity is underestimated, although it has definitely improved with respect to the previous representation, i.e. large-scale vertical velocity). We thought it was more appropriate to cite these two references in Section 2.2 (line 18, P6):

“Other studies, e.g. Kärcher and Ström (2003) and Joos et al. (2008), showed that w is in good agreement with vertical velocity observations.”

P2L26-30: *The following (very long) sentence should appear earlier in text as it defines the two ice crystal formation regimes.*

We split the sentence into smaller sentences and we also exchanged their order (because we have always firstly written about cirrus clouds and then about mixed-phase clouds):

“Overall, two different regimes for ice crystal formation are distinguished. The cirrus regime at cold temperatures ($T < 238$ K), where ice crystals originate via heterogeneous and homogeneous nucleation to form cirrus clouds. The mixed-phase regime at subfreezing temperatures between 238 K and 273 K, where ice crystals form exclusively via heterogeneous nucleation and alter the phase composition of the mixed-phase clouds.”

However, we preferred to leave these lines in the same position because they mention the ice nucleation mechanisms (homogeneous and heterogeneous) which are described

before.

P2L31-35: *I am missing a description of freezing in mixed-phase clouds? Why do you always refer only to cirrus, if you implemented freezing also at mixed-phase conditions?*

Ice nucleation in mixed-phase clouds is described at lines 18-23, P2. At the end of page 2 and beginning of page 3, we describe the competition for water vapour between homogeneous and heterogeneous nucleation (in the cirrus regime) and the PREICE effect, i.e. the two processes which will be considered by BN09. This part has been slightly changed after the request by the Referee#1 (please, see point P2L35-P3L1).

P3L10: *Can you find some evidence/reference for the following sentence: "Including sophisticated schemes in general circulation models (GCMs) allows for a more realistic description of the variability of cloud properties and cloud radiative effects, improving the model climate predictions"*

We cited: Lohmann and Feichter (2005) and Barahona et al. (2014).

P3L18: *please explain what INP spectra mean. I assume that's simply a parameterization of het ice nucleation?*

Exactly. We replaced the words "spectrum" and "spectra" with "parameterization(s)" (as requested by the Referee#1).

P3L29: *What kind of scheme does your standard version of the model use?*

The standard configuration of EMAC and its schemes are described in Sections 2.1. and 2.2.

Model description and set-up of simulations

P4: *Convection plays a large role in global high cloud distributions and their properties.*

You should include some more information on how the CONVECT submodel interacts with the microphysics and cloud cover. I add some questions which could be addressed:

- *How does the convective detrainment works?*
- *How do you compute/parameterize the size of ice crystals that are detrained from convective clouds?*
- *How does the scheme decide whether you detrain liquid or water (or even vapour)?*

We added the following information (see also Referee#1, P4):

"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."

P4L25: *You previously defined ice crystal number concentration as ICNC. Here, you define it again as N_i . Please, be consistent!*

We replaced “ N_i ” with “ICNC”. Moreover, we replaced “ N_l ” with “CDNC” and we removed “ q_w ”, “ q_i ”, “ q_l ” as they are never used later in the text.

P6L20-21: *This cannot be a separate paragraph.
What do you mean with “the only precaution”?*

We merged these lines to the previous paragraph.

We changed “precaution” with “expedient”. We mean that this is the unique, approximate way of the CLOUD submodel to take into account the pre-existing ice crystals.

P6L23-25: *The text between points 2.3 and 2.3.1 is repeating the information already given before. Please remove it.*

Done.

P6L26-29: *You already provided the same information on page 3. Please try to avoid repetition!*

We deleted lines 26-27, P3 but we left lines 16-18, P3 because we consider this the minimum information to introduce BN09. Here, line 26, P6 gives new information, while the next sentence was slightly changed:

“It explicitly considers the competition for water vapour between homogeneous and heterogeneous nucleation in the cirrus regime, the influence of chemically-heterogeneous, polydisperse aerosols acting as INPs, and allows to use different heterogeneous nucleation parameterizations.”

P7L26: *Not sure that you can assume that P13 agrees better with observations in every model (thinking that vertical velocities might be different than in CAM)*

Naturally, we cannot assume that P13 agrees better with observations in every model, but the sensitivity studies suggest that generally P13 performs better than the other INP parameterizations listed at lines 19-20, P7. Barahona et al. (2010), who compared various INP parameterizations available in BN09 using another global model, the Global Modeling Initiative (GMI), found that PDA08 (which is the previous version of P13) better agrees with observations.

Implementation

P8L21: *Did you define what “M modes” are?*

We defined “M” at line 21: M can be K (Aitken), A (accumulation) or C (coarse). We modified the sentence to make it clearer:

“...the diameters D_M are not distinguished among aerosol species but only among the modes (Aitken (K), accumulation (A), coarse (C), i.e. $M = K, A, C$) which the species belong to.”

P8L24-25: *Could you describe that a bit better as it is not a standard procedure in GCMs?*

This procedure is actually performed by the BN09 parameterization. We explained it better and we moved these lines to the end of Section 2.3.1.

“In order to account for sub-grid variabilities, the output variables of BN09 which depend on the vertical velocity ($f(w)$) are weighted over a Gaussian updraft velocity

distribution by numerically calculating the integral (Morales and Nenes, 2010; Sullivan et al. 2016):

$$\overline{f(w)} = \frac{\int_0^\infty f(w')P(w')dw'}{\int_0^\infty P(w')dw'} \quad (1)$$

where $P(w')$ is the Gaussian probability density function of sub-grid vertical velocities (w') with mean 0.1 cm s^{-1} and standard deviation equal to w_{sub} .”

P9L3-4: *I think that doesn't fit in the model description part of the paper but in the results.*

We moved this sentence in Section 3 (before Section 3.1), and we added the new sentence: “In this Section we investigate the changes and the effects obtained by using BN09 in the different regimes.”

Model results

P10L3-4: *Not sure about that. I think your Figure 3a makes me think it is not INPs but mountains that contribute most to the larger ICNC in the northern hemisphere.*

Looking at the maps (Figures 3 and S2) and according to the literature cited at line 11, indeed both factors (big mountain chains and more INPs in the NH) contribute to higher ICNCs in the NH. We rephrased the sentence:

“..., while they are much higher over the mid-latitudes in the Northern Hemisphere (NH) because of larger INP concentrations and the influence of big mountain chains, e.g. Rocky Mountains and the Himalayas (Figure 2a).”

Figures: *Please indicate which areas are significantly different from the “DEF” case in Figures 2 and 3 by applying an appropriate statistical significance test! Same for plots S1, S2, S3.*

Add +/- 1 or 2 st. deviation shading to the lines plotted in S4.

You could also tentatively try to plot the 25th and 75th percentile range in Figure 5, maybe only for 1 setup due to clarity (BN+BN, I would suggest).

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.

We plotted the error bars for +/- one standard deviation (only for the simulation BN+BN) in Figure S4.

We plotted 5th-95th and 25th-75th percentiles of BN+BN in the comparison with flight measurements (see Figure 1 of this document).

Table 2: *Please also add standard deviations to your Table 2 for a better feeling of the magnitude of changes due to changing microphysics!*

Done. We attributed to each annual global mean the (temporal and spatial) standard deviation.

Figures: *Do you show in-cloud or all-sky ICNC and IWC values on your figures? Mention it somewhere in text!*

We used in-cloud ICNCs only in Figure 5 (as specified in the caption). We added the information “(grid-averaged)” in the captions of the other Figures.

P10L6-7: *Moreover, KL02 simulate only homogeneous nucleation, while BN09 simulate also heterogeneous nucleation at cirrus conditions. Therefore, you should point out somewhere that you are not really making an apples-to-apples comparison.*

Actually, here we compared BNhom (not BN09) and KL02, but we explained it better.

“As ice crystals are formed almost exclusively via homogeneous nucleation here (not shown) and BNhom and KL02 produce the same order of magnitude of ICNCs (Barahona and Nenes, 2008), the negative bias is likely due to the PREICE effect predicted by BN09.”

P10L8-9: *How was that done before in the REF case? Did you use only large-scale updraft? Do you consider a Gaussian distribution of vertical velocities (Sullivan et al., 2016) also in mixed-phase conditions?*

As answered to Referee#1, 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), thus, we removed such sentence.

KL02, like BN09, uses $w_{sub} = 0.7 * \sqrt{TKE}$, not the large-scale updraft.

BN09 uses a Gaussian distribution of vertical velocities (in the cirrus regime in BN+LD, in the mixed-phase regime in KL+BN, in both regimes in BN+BN).

P10L10: *First you talk about cirrus, than mixed-phase, now cirrus again, I guess. That's confusing for the reader, which expects this sentence to refer to mixed-phase clouds. Please reorder or clarify better!*

We reordered and rephrased lines 6-13 as follows (including also what we replied to Referee#1, P10L7-10).

“As ice crystals are formed almost exclusively via homogeneous nucleation here (not shown) and BNhom and KL02 produce the same order of magnitude of ICNCs (Barahona and Nenes, 2008), the negative bias is likely due to the PREICE effect predicted by BN09. Indeed, it has been demonstrated that homogeneous nucleation dominates in the upper troposphere in the tropics and in the SH (Haag et al., 2003; Liu et al., 2012; Barahona et al., 2017), while heterogeneous nucleation is important in the NH (Cziczo et al., 2009), where cirrus clouds are formed from a combination of homogeneous and heterogeneous processes. 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. Overall ...”

P10L17: *missing citation(s) at the end of the following sentence.*

This assertion refers to our results. Probably the expression “using the various ice schemes in the mixed-phase regime” is confusing. We modified it:

“Overall, the ICNC deviations in the mixed-phase regime obtained using the two different parameterizations are smaller (mostly within $\pm 20\%$) than in the cirrus regime.”

P10L18-20: *please rephrase (cirrus don't occur throughout the year? where, why not?...)*

We are sorry, the word “whole” is missing: “Since cirrus clouds do not occur throughout the whole year, ...”.

P10: *Ice nucleation in mixed-phase may not be the main source of IWC and ICNC between 0 and -38° C. Could you estimate that from your model and comment on that? Possible processes that might not be negligible are for instance sedimentation of ICs from cirrus or detrainment of IC from convection.*

The Referee is right but, unfortunately, we do not have these tendencies (sedimentation and detrainment) stored as output. Thus, we cannot quantify their contributions but we mentioned these sources for the mixed-phase regime in the revised manuscript.

P10L22: *BN+LD case shows some differences with respect to DEF also in the mixed-phase regime (see Fig 2, Fig 3 f, also fig S1).*

- *Why is that when the mixed-phase freezing is the same? What other sources of ice exist in mixed phase?*

- *Can there be some radiative/dynamical/microphysical responses of mixed-phase to difference in cirrus scheme?*

- *There seems to be a response in convection in the tropics. Is this really the case? What caused it? Did the atmospheric stability change? Please comment!*

Previously, in P10L10 we mentioned the sedimentation of larger ice crystals from cirrus clouds as a possible cause for the increment of ICNCs at lower altitudes in BN+LD with respect to DEF. Naturally, changes of cloud phase (ice and supercooled liquid) in mixed-phase clouds influence radiative fluxes and dynamics, because of the “self-maintaining feedback pathway between liquid water, radiation, and turbulence” (Morrison et al., 2011), and there can be a response in convection. Indeed, the net cooling found in Table 2 (NCRE) can decrease the static stability and enhance updrafts, which in turn affects ICNCs. However, we did not investigate these aspects (linked, among the other things, to convective clouds which are treated by an independent submodel) because they are not the focus of this study. This paper wants to describe the implementation of BN09 and analyse the products of BN09, without expanding to other atmospheric effects which will be studied in a future scientific paper (while this remains a paper about model developments).

P10L23: *I don't understand what you mean with this sentence? If you look at upper troposphere, the opposite is true. While I agree with the following statement for regions between -30 and 0° C.*

The sentence was not properly correct. We changed lines 23-25 as follows.

“IWC pattern (Figure 2e) qualitatively follows the ICNC distribution. It is quite symmetrical between the two hemispheres except at high latitudes in the NH, where IWC is slightly higher because of the higher values of ICNC. Particularly, IWC exhibits three local maxima: two over the mid-latitudes in both hemispheres and one in the tropics, associated to storm tracks and deep convections, respectively (Li et al., 2012). These features are in agreement with satellite observations, e.g. Waliser et al. (2009), Li et al. (2012).”

P10L23: *I don't understand the connection with the first part of the sentence. I see you have 3 peaks of IWC which come out of your model, which is good, as the observations agree with it (please consult/refer e.g. to: Li et al., 2012).*

What atmospheric features do the 3 peaks correspond to?

Please, see our previous answer (P10L23).

Global distribution

P11L7: *You never mention why you decided for 200 and 600 hPa levels.*

We indicated in parenthesis that the two levels are representative for the cirrus regime and the mixed-phase regime (we wrote this also in the caption of Figures 3 and 4). We specified it better at lines 7-8, P11: “Figure 3 shows the global distributions of ICNC annual means at two different altitudes: 200 hPa (where temperatures vary between 200 K and 220 K) to represent the cirrus regime and 600 hPa (where temperatures are approximately between 240 K and 260 K) to represent the mixed-phase regime.”

P11L8: *What do you mean by precipitation patterns? Can you also mention why does this happen, and why ICNC peaks also over mountains.*

Does the ICNC global distribution compare well with recent observations by Sourdeval et al., 2018 and Gyrsppeerdt et al., 2018?

Thanks for the interesting references. We changed the sentence (also according to the Referee#1’s comment P11L8-9) as follows.

“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 Gyrsppeerdt 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).”

P11L11-12: *Again, I would like to see more explanations and not only description of figures. Why are India and Indonesia different from the rest of the world?*

The new ice crystals produced BN09 (i.e. the output which is then passed to the CLOUD submodel) are almost exclusively formed via homogeneous nucleation at 200 hPa, therefore the reduction is actually due to the PREICE effect. We rephrased all the lines 11-13, P11.

“The relative changes clearly show that BN09 used in the cirrus regime (Figure 3b, d) reduces ICNC (up to 60%) worldwide with respect to the default experiment, and the ICNC annual global mean drops to $137 L^{-1}$ (i.e. more than 30%). Such a reduction occurs mostly because of the PREICE effect, being the ice crystals mainly of homogeneous origin at this altitude. However, there are positive biases along the ITCZ and over the TWP area. As the concentrations of new ice crystals produced by BN09 are not particularly remarkable in these regions (not shown), deep convection is likely to play a role. Indeed, there is a certain response of the convective activity to the choice of the ice nucleation scheme used in the cirrus regime.”

P11L13: *Is this only your speculation or do you have any evidence for it? Please show them!*

Please, see our previous answer (P11L11-12).

P11L16-17: *Why, please explain it!*

As answered to Referee#1 (P11L8-9, L16-17), we wrote:

“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).”

P11L18-19: *I agree, that is very interesting, and therefore would be nice to understand what caused it!*

We added some comments in the manuscript at lines 7-10, P10 because this can be seen already in Figure 2. Thus, we changed lines 18-20, P11 to:

“Figure 3f confirms what already noticed in Figure 2b, that is the ice nucleation scheme used in the cirrus regime affects the ICNC at the mixed-phase regime altitudes predicting higher ICNCs especially in the NH.”

P11L29-31: *What about the Intertropical Convergence Zone and peak of tropical convection in the Pacific warm pool area? Maritime aerosol cannot play a large role in a dynamically-driven detrained clouds.*

Moreover, you also did not include marine aerosols in the model, so I don't understand why you mentioned them.

Why don't you look at your particle radius and verify if the model is giving reasonable values in the tropical Pacific?

The Referee is right. We did not consider marine aerosols as potential INPs. We removed the sentences at lines 29-31.

Although BN09 and KL02 produce different sizes of newly formed ice crystals, both schemes present the highest radius values over the TWP (at 200 hPa), as can be seen in Figure 2 of this document, and this impacts on the IWC of Figure 4 (left). Lines 28-31 were changed as follows.

“Nevertheless, two interesting features appear. First, the high IWC values ($> 10 \text{ mg kg}^{-1}$) over the TWP at 200 hPa, where ICNCs are not particularly high. This is probably caused by the bigger radii of the newly formed ice crystals simulated in this area, both by KL02 and BN09. Second ...”

P11L29: *I guess 200 hPa is close to the level of maximum detrainment from deep convective clouds. It is therefore important to look at what size you assume for detrained ICs (I assume you use a 1-moment version of convective microphysics, so there needs to be more assumption to couple it to the stratiform microphysics).*

Moreover, one of your coauthors showed how that the vertical velocities are quite high in the mentioned area (Barahona et al., 2017). I guess part of this is due to the prevailing large-scale ascent motion (quite noticeable in Joos et al., 2008), while indeed a lot of it has to be connected to deep convection, and, in GCM modeling world, to TKE values. Please explore that in larger detail!

We added some information about the CONVECT submodel and how it interacts with the CLOUD submodel in the revised manuscript as written at point P4. The sizes of detrained condensate particles are not computed explicitly and we cannot quantify them (as mentioned also at point P10).

The Referee is right, Barahona et al. (2017) in Fig. 4 show high standard deviations in vertical velocity over ITCZ and TWP, however, our w_{sub} (Figure 3-left of this document) is generally smaller worldwide. Thus, we cannot easily attribute the high values of IWC over the TWP area to TKE and so to deep convection.

P13L1: *Why is this the case? It would be extremely interesting to understand that, as this region plays a large role in global energy balance. Did you change the model tuning in between? Can this happen due to changes in convection, which somehow responds to a different cirrus scheme?*

On page 15 you even give a hint for that: “When BN09 is used in the cirrus regime, Ptot grows by 4% especially because of the increase of the convective precipitation contribution (the large scale precipitation of all simulations remain almost constant)”

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

Model comparisons and observations

P13L4-9: *This text doesn't fit into the results section, please move it to model description!*

Done.

P13L24: *Please prove that a change of 7% is large by showing the variability (maybe add in table).*

It must be stressed that 7% changes are based on global annual means. Although the difference is not statistically significant, it is still remarkable to have such a change on a global scale. We agree with the Referee that the sentence was not correctly formulated and we changed it removing the text “is quite sensitive to the ice scheme used”. Following the Referee’s suggestion, we added the variability (one standard deviation) of the calculated fields in Table 2.

P13L28: *...that applied ECHAM => that used ECHAM-HAM*

Done.

P15L15-19: *You say that BN09 makes larger IC, but large scale precipitation doesn't change. That's surprising. Why?*

These lines of the text refer to annual global means, so the sentence “the large scale precipitation of all simulations remain almost constant” is actually not appropriate because there are regional and local differences (as shown in Figure 4 of this document). This sentence has been removed.

P15L21-23: *so all that hard work for nothing? Or what should I get from that?*

We are not sure about what the Referee means here. With this sentence we want to point out that the biggest differences among the four simulations occur between the simulations which use different ice nucleation parameterizations in the cirrus regime, i.e. KL+LD and KL+BN are clearly different from BN+LD and BN+BN. This is what we refer to also at line 15, P16.

P16L21-23: *Radiation changes for quite a bit, and this is probably a more important parameter for climate compared with ICNC, IWP, etc. I would more strongly point SW, LW, and NET CRE anomalies, maybe even show a lon x lat plot of them (with significance on it).*

We included the global distributions of SCRE, LCRE, and NCRE (with levels of significance) in the supplement file and we changed lines 4-8, P15.

“Looking at the percentage changes and the global distributions in the supplement file (Figure S4) it is evident that the cloud radiative effects are sensitive to the

ice nucleation scheme used for cirrus clouds. Indeed, SCRE increases more than 5% with BN09 because of the less efficient scattering of shortwave radiation by fewer and larger crystals. More importantly, LWCR decreases up to 15% in BN+LD because cirrus clouds, at the same, can trap less longwave radiation in the Earth-atmosphere system. As a result, NCRE diminishes with statistical significance over some areas in the tropics and high latitudes, and the cooling effect is enhanced.”

P16L11: *Mention that you are talking about median values as means can be very different!*

In the caption of Figure 5 there are all the specifics about the plots (kind of statistics: median, kind of variable: in-cloud ICNC, spatial coverage, vertical coverage).

P17L3-4: *Isn't that interesting, considering that BN09 should give comparable results to KL02 for homogeneous freezing, while BN09 has also PREICE and heterogeneous freezing effects included. So one would rather expect just the opposite, BN09 to be lower than KL02. Why do we see the opposite?*

Are the results the same when comparing means instead of medians? Or do the vertical velocities calculated by the base model change for some reason between KL02 and BN09 schemes?

Unfortunately, Figure 5 in the manuscript is affected by an error made during the post-processing and has been replaced by Figure 1 of this document (see also Referee#1, P17L3-4). 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 (see also Referee#1, P17L3-4).

“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 75th 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 25th percentile (although remaining within the 5th 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 overestimate 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 competitive 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 means are higher than the medians (Figure 5-left of this document), but the results are similar: ICNCs simulated using BN09 in the cirrus regimes are lower than ICNCs simulated using KL02, and the latter ones overestimate the observations. Vertical velocity does not change between KL02 and BN09, a part for the PREICE correction in BN09.

P17L4: *The comparison with aircraft data doesn't show BN09 as superior to the less realistic KL02, but rather the opposite. In particular, as there is only a small fraction of cirrus that reside at temperatures below 200 K (-73° C) in the area where most of the measurements come from (extratropics). Can you comment on that?*

Please, see our previous answer (P17L3-4).

P17L3-4: *Did you make sure you are comparing apples-to-apples? For instance, GCMs normally simulate cirrus in the winter polar stratosphere, which might be responsible for parts a non-negligible fraction of the distribution. Better to remove them from the analysis.*

Also, did you normalize the model output based on the latitude not to give a too large meaning to the (numerous) polar gridpoints?

Actually, we did not simulate polar stratospheric clouds (moreover, the high latitudes are basically excluded from the analysis as the latitudinal coverage is 25S – 75N).

We did not normalized the model output. We did it in Figure 5-right of this document, taking into account the volumes of the different grid boxes, and we can observe that the results change only slightly. As the observations are not homogeneously distributed any weighted result would have the same flow of the original plot.

P17L3-4: *It would be interesting to look at a plot of vertical velocity in function of temperature, if you believe that to be (part of) the reason for differences between KL02 and BN09.*

Most of this section changed accordingly to the new Figure. Please, see our previous answer P17L3-4.

We did not plot the vertical velocity in function of temperature because now the ICNCs simulated by BN+LD and BN+BN are lower than KL+LD and KL+BN, as expected.

P17L15-18: *You show a large underestimation only below 200 K, while between 200 and 210 K both schemes seem to be comparably bad (hint on problems with vertical velocities???)*

Most of this section changed accordingly to the new Figure. Please, see our previous answer P17L3-4.

P17L18: *Yes, but only at the very cold temperatures, which correspond only to a small fraction of cirrus and aren't the most relevant in terms of radiative (and in general climatic) impacts.*

In the new manuscript also this sentence was changed.

P17L20-21: *As far as I recall, CAM modeling community undertook some efforts to decrease the overestimation of cold cirrus ICNC. Within various realizations of ECHAM, as it seems like, we have just the opposite problem. Too few ICNC at coldest cirrus conditions. It is not intuitive at all that such problems are alleviated by implementing a scheme, which should on average decrease the ICNC. I would love to read a discussion on this point in the corrected manuscript.*

Please, see our previous answer P17L3-4.

P17L25-26: *This now sounds different to discussions from section 3.2 (Figure 3, results for 600 hPa). Why?*

Rather than Section 3.2, this is different (the Referee is right) to what discussed about Figure 2c in Section 3.1. P13 actually produces less new ice crystals than LD06. The differences in Figure 5 (right) are small because of the “effect of smoothing” derived by averaging along the latitude, as we showed in our reply to Referee#1 (point P17L25-26). We deleted the assertion “... P13 and LD06 produce similar ICNC ...”

P17L27-28: *Please add references for WISP-94 and ICE-L campaigns.*

Done.

P17L30: *I think there's some datasets out there that extend to mixed phase temperatures. Look for instance into Heymsfield et al., 2013: Ice Cloud Particle Size Distributions and Pressure-Dependent Terminal Velocities from In Situ Observations at Temperatures from -8 to -86°C.*

Thanks for the reference. We added the following sentence at the end of the paragraph.

“Finally, ICNCs in Figure 5 (right) are in good agreement with the results of Heymsfield et al. (2013), also based on flight campaigns. They found that ICNCs decrease as temperature increases and are within the range 5-50 L⁻¹ in the mixed-phase regime. Besides the flight measurements, the recent ICNC estimates from lidar-radar satellite retrievals must be mentioned, e.g. Sourdeval et al. 2018 and Gryspeerd et al. 2018. In particular, Gryspeerd et al. 2018 analysed the behaviour of ICNCs within clouds as a function of temperature. Differently from Figure 5 (left), they showed that there is a weak temperature dependence of ICNC, which increases with decreasing temperature. On the other hand, similarly to Figure 5, they found a small increase of ICNC around 265–270 K and, interestingly, a small peak at about 233 K due to orographic and frontal regimes, which could explain our higher modeled ICNCs between 230 K and 240 K.”

P17L30-32: *That isn't true for the warmer of the mixed-phase clouds. Figure 11 of the referenced Kanji et al., 2017 paper schematically illustrates that ICNC can also be higher than INP numbers due to secondary ice processes.*

We rephrased the sentence.

“It should be also noted that the measurements actually concern INPs. When the INP number is not high enough to deplete the ambient supersaturation, INP concentrations and ICNCs can correspond, however, it is well known that the two concentrations show discrepancies with increasing temperature because of the secondary ice formation (Kanji et al. 2017).”

Conclusions

P18L15: *I would expect you to give a reason for the observed changes or lack of them after the line 15.*

We provided the reasons at lines 5-8, P15. They are not new discoveries so we thought not to repeat them here in the Conclusions.

P19L1-2: *This is not obvious from the data you show. Please, try to prove it in a quantitative way with the help of some appropriate statistical methods, or rephrase the conclusions!*

The sentence changed (see also Referee#1, P17L3-4):

“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.”

P19L5-9: *Those are relatively weak conclusive words. Could you find some stronger statement on top of being able to include more processes in the model? Please think in the direction of why should anyone not using EMAC care about your manuscript (or consider citing it).*

At line 13, P18, we added the sentence:

“We found that changing the ice nucleation scheme in the cirrus regime generates larger differences of ICNC and IWC than changing parameterization in the mixed-phase regime, that is the simulations using the same parameterization in the cirrus regime (e.g. BN+LD and BN+BN) are easily discernible from the others (LD+KL and LD+BN). Interestingly, we also observed a certain dependence of ICNC and IWC in the mixed-phase regime on the parameterization used for cirrus clouds.”

As pointed out by the Referee, it is expected that the EMAC Community will be interested in this paper more than others, however, this work will be useful for future model comparisons with focus on ICNC estimates.

The last paragraph was changed as follows (see also Referee#1, P17L3-4 and P19L5-7).

“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.

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. By using the configuration BN+BN, the EMAC model becomes one of the few GCMs which take into account in a detailed manner the complexity of ice nucleation. Finally, this work offers further material for future GCM comparisons with focus on ICNC estimates and for future modeling evaluations against flight measurements and lidar-radar satellite retrievals.”

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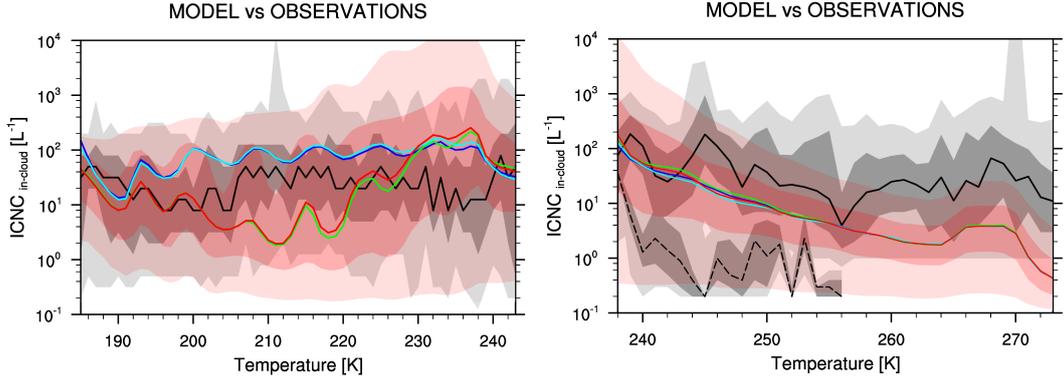


Figure 1: New Figure 5. Modeled in-cloud ICNC and flight measurements versus temperature. Lines are medians of KL+LD (blue), BN+LD (green), KL+BN (light blue), BN+BN (red), and observations (black). Shaded areas indicate 5th-95th and 25th-75th percentiles of observations and BN+BN.

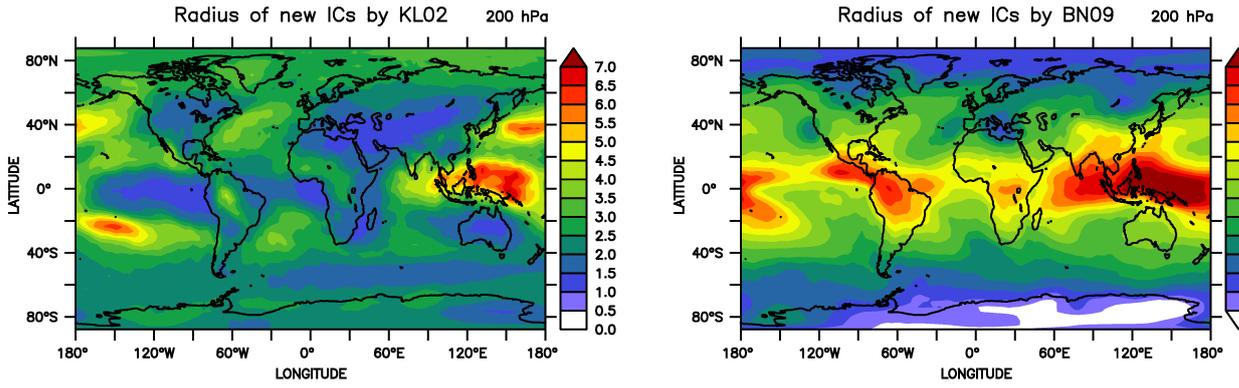


Figure 2: Radius (μm) of new ice crystals at 200 hPa.

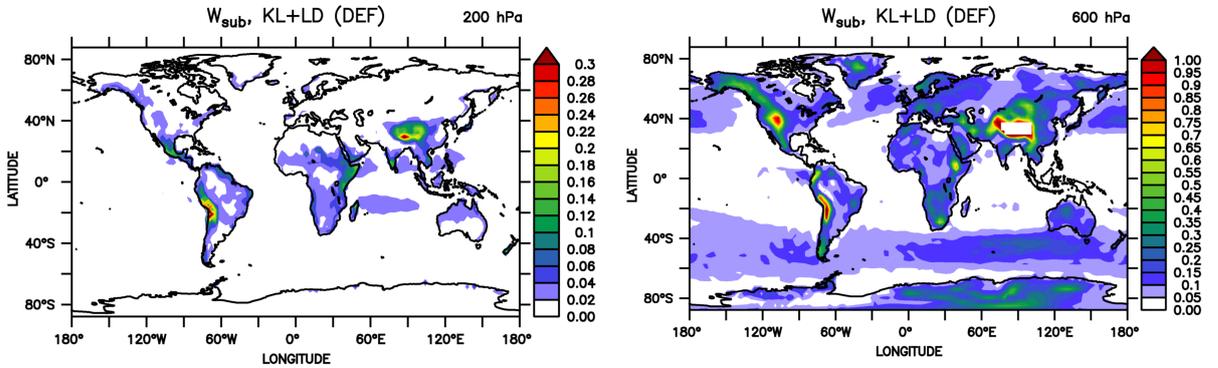


Figure 3: Annual means of $w_{sub} = 0.7\sqrt{TKE}$ (m/s) at 200 hPa and 600 hPa for the default simulation.

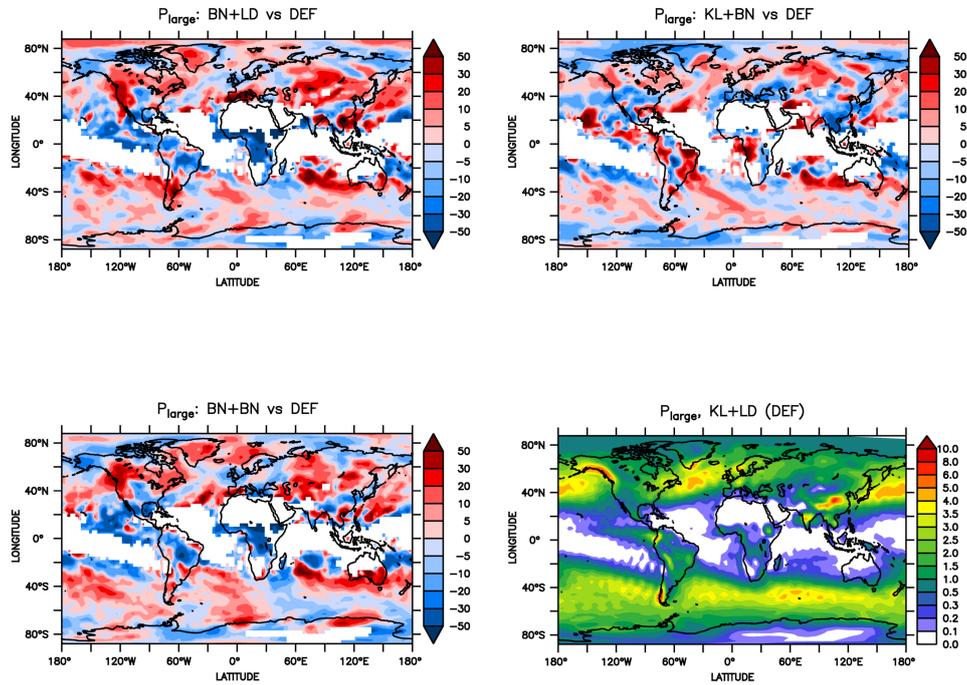


Figure 4: Annual means of large-scale precipitation (mm/day) for the default simulation (bottom, right) and the relative changes.

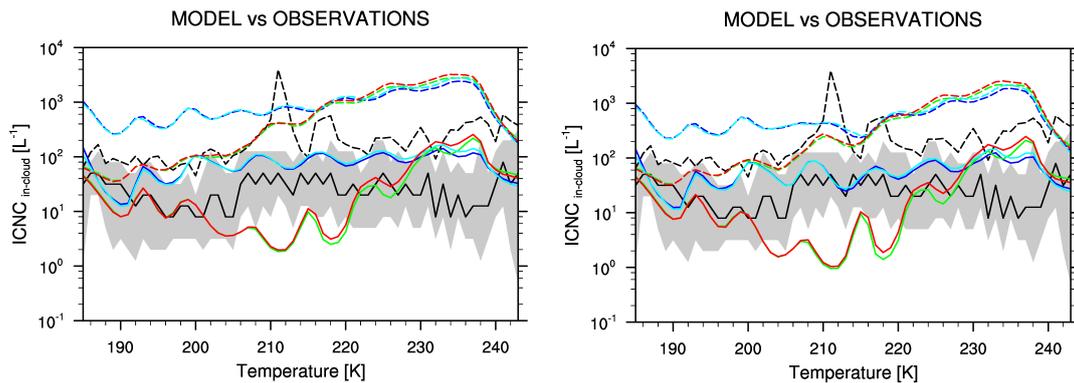


Figure 5: As Figure 5 (left). Modeled in-cloud ICNC and flight measurements versus temperature. Lines are medians, dashed lines are means of KL+LD (blue), BN+LD (green), KL+BN (light blue), BN+BN (red), and observations (black). Left: not weighted statistics. Right: weighted statistics.