Review of "Implementation of a comprehensive ice crystal formation parameterization for cirrus and mixed-phase clouds into the EMAC model (based on MESSy 2.53)"

This study describes the implementation of a new ice nucleation scheme for both mixed-phase and cirrus clouds. In particular, its cirrus part includes the previously missing heterogeneous freezing on ice nucleating particles and the effect of vapour deposition on pre-existing ice.

I can imagine the manuscript is a result of hard modelling work and I believe it deserves to be published, but only after (most of) the comments are addressed. I find the manuscript's results OK, however the authors very often only superficially describe the plotted results without explaining their causes. As the manscript presents the implementation of previously developed schemes I would really expect the authors to make the additional step forward and try to better understand their results. Moreover, I would suggest to better structure some of the introductory parts of the text.

I added many specific comments. Their main point is, however, not to demotivate the authors, but to try to help them substantially improve the quality of the manuscript.

A summary of major points

- Please try to understand your results in a larger detail and provide more information when needed!
- How is convection simulated in your model. How do you deal with detrainment from deep convection? Never mentioned in the paper, despite we also clearly see some responses of convection to the modifications in ice nucleation schemes.
- The introduction should be written in a more coherent way, in particular the mixed-phase part. See detailed comments.
- Keep in mind you are comparing two schemes which should already by construction give you different results for cirrus clouds as one simulates only homogeneous freezing, while the other includes also heterogeneous freezing and pre-existing ice effect.
- considering that you don't write only about cirrus, but also about mixed-phase conditions, you could very briefly mention how other relevant processes work in your model, e.g. Wegener-Bergeron-Findeisen process, secondary ice formation, autoconversion, etc.
- I would find it valuable to also briefly mention how your results compare to results from studies using the same/similar ice nucleation scheme in CAM5.

- You should write a stronger conclusion, which can be relevant also to people other than MESSY model users. I also don't think the authors made a strong enough point to convince MESSY model users who don't focus on clouds to use the new ice nucleation scheme.

Specific comments
(a mixture of a few major and many minor 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

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.

Some minor comments:
line 2: *realistically* represent => quite a bold statement
line 4: cold clouds => never defined it
lines 4-6: the sentence starting with "Furthermore" is hard to understand. Please rewrite!
line 7: Compared to the standard EMAC *results*… => …
line 10: …improves the model results…. => too vague, be more concrete, which results?

Introduction

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

line 15: *…clouds remain one of the most elusive components of the atmospheric system…*
I think the word elusive isn't used in a correct way here

page 2

- 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.
- you never defined what a "mixed-phase cloud" is

-

line 4: *typically below -35°C* => 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°C 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)

line 6: 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.
line 10: missing some references on mixed-phase being TD unstable, or similar

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.

line 12: *The fraction of cloud ice has a profound impact on the cloud forcing in global climate models*: 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)

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, due to the overestimation of vertical velocity this is under debate (Cziczo et al., 2013; Barahona and Nenes, 2011; Barahona et al., 2017).

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.

Lines 26-30:
The following (very long) sentence should appear earlier in text as it defines the two ice crystal formation regimes.
"Overall, two different regimes for ice crystal formation are distinguished: the mixed-phase regime at subfreezing temperatures between 238 K and 273 K, where ice crystals form exclusively by heterogeneous nucleation and alter the phase composition of the mixed-phase clouds, and the cirrus regime at colder temperatures (T < 238 K), where ice crystals originate via heterogeneous and/or homogeneous nucleation to form cirrus clouds. "

end of page 2, beginning of page 3: 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?

<u>page 3</u>

line 10: 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."

line 18: please explain what INP spectra mean. I assume that's simply a parameterization of het ice nucleation?

line 29: *"…has been compared with the results generated via the standard model configuration"*
What kind of scheme does your standard version of the model use?

Model description and set-up of simulations

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)?

page 4

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

page 6

This cannot be a separate paragraph:
"Finally, the influence of the pre-existing ice particles is not taken into account. The only precaution adopted by the CLOUD submodel is the reduction of the number of aerosol particles available for ice nucleation by the existing ice particle number."

+What do you mean with "*the only precaution*"?

lines 23-25: The text between points **2.3** and **2.3.1** is repeating the information already given before. Please remove it.

lines 26-29: You already provided the same information on page 3. Please try to avoid repetition!

page 7

line 26: 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)

2.3.2 Implementation

page 8

line 21: Did you define what *"M modes"* are?

line 24-25: *"They are weighted over a Gaussian updraft velocity distribution, with mean 0.1 cm s^{-1} and standard deviation equal to w_{sub}, in order to account for the sub-grid variability (Sullivan et al., 2016) "*
Could you describe that a bit better as it is not a standard procedure in GCMs?

page 9

lines 3-4: "*Overall, BN09 is a scheme more realistic than KL02 and LD06 which improves the ice nucleation in EMAC by taking into account processes which were previously neglected (e.g. water vapour competition, influence of polydisperse aerosols, PREICE effect).*"

I think that doesn't fit in the model description part of the paper but in the results.

3 Model results

page 10

lines 3-4: 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.

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).

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

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

lines 6-7: "*This is likely due to the PREICE effect predicted by BN09, as it has been shown that BNhom and KL02 produce the same order of magnitude of ICNC (Barahona and Nenes, 2008).* "

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.

Lines 7-9: *On the other hand, ICNCs increase at lower altitudes and especially in the NH. This is due to higher TKE at lower altitudes, which impacts the updraft velocity and increases heterogeneous nucleation contribution.*

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?

line 10 and further: "*Indeed…*"

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!

Lines 16-17: missing citation(s) at the end of the following sentence: *"Overall, the ICNC differences obtained using the various ice schemes in the mixed-phase regime are smaller (mostly within ±20%) than in the cirrus regime."*

Lines 18-20: please rephrase (cirrus don't occur throughout the year? where, why not?...)

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.

line 22: 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!

[hint: by adding significance you might get by for some of the patterns by simply pointing out some differences aren't significant]

line 23: "*IWC decreases with increasing temperature, where ICNC is lower (Krämer et al., 2016),*

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.

…and we find three areas with higher values over the mid-latitudes in both hemispheres and the tropics (Figure 2e)."

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?

line 25: *"…IWC is slightly higher because of the higher values of ICNC."*

Does this always hold true? Add some supporting references at this point.

page 11

<u>general comment on section 3.1</u>

I often miss a more detailed discussion of why some of the changes occur, what caused them? For a better process understanding I would recommend to add also zonally averaged figures of IC radii, RH (or RH_{ice}), temperature, for example also cloud droplet number concentration (helps for mixed-phase), and maybe cloud cover.

3.2 Global distributions

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

line 7: "*ICNCs in the cirrus regime mostly follow the precipitation pattern*"
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 Gyrspeerdt et al., 2018?

"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, except over Indian and Indonesian areas"
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?

Line 13: *"Such a reduction occurs mostly because of the PREICE effect in the SH and the competition in the NH."*
Is this only your speculation or do you have any evidence for it? Please show them!

Lines 16-17: *"At 600 hPa, ICNC increases towards high latitudes, in particular over Greenland (up to 2000 L^{-1}) and Antarctica (mostly > 2000 L^{-1})."*
Why, please explain it!

Line 17: *"Interestingly, the ice nucleation scheme used in the cirrus regime affects the ICNC at the mixed-phase regime altitudes"*
I agree, that is very interesting, and therefore would be nice to understand what caused it!

Line 29-32:
"Maritime updraft velocities are weaker, and recent work has shown that there are 30 important oceanic sources of INP (e.g. DeMott et al., 2016). These effects may combine to produce few large crystals in this Southern Pacific region. "
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?

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!

<u>page 13</u>

line 1:
"…but using BN09 in the cirrus regime dramatically increases IWC in equatorial regions at 200 hPa."

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, P_{tot} grows by 4% especially because of <u>the increase of the convective precipitation contribution</u> (the large scale precipitation of all simulations remain almost constant)"

4 Model comparisons and observations

Lines 4-9: This text doesn't fit into the results section, please move it to model description!

4.1 Annual global means

line 24: Please prove that a change of 7% is large by showing the variability (maybe add in table).

line 28: *…that applied ECHAM* => that used ECHAM-HAM

lines 15-19: You say that BN09 makes larger IC, but large scale precipitation doesn't change. That's surprising. Why?

line 21: "*The annual zonal mean profiles show clearly that the simulations using the same ice nucleation scheme in the cirrus regime are very close to each other, i.e. KL+LD and KL+BN, and BN+LD and BN+BN (as already visible in Table 2).* "

- ⇨ so all that hard work for nothing? Or what should I get from that?

<u>page 16</u>

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).

4.2 Comparison with aircraft measurements

line 11: Mention that you are talking about median values as means can be very different!

page 17

lines 3-4: "*From Figure 5 (left) we deduce that KL02 produces too low ICNCs in cold cirrus clouds (for T < 205 K) as well, while BN09 works better at such low temperatures*"

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?

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?

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?

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.

Lines 15-18: *"On the contrary, the simulations which consider only homogeneous nucleation in the cirrus regime show a large underestimation (even below the 5^{th} percentile) at temperatures lower than 210 K, however, they are always within the observed 25^{th} – 75^{th} percentiles at higher temperatures.*

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???).

ECHAM5 has strongly underestimated ICNC at low temperatures thus far (Kuebbeler et al., 2014), " 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.

"The implementation of BN09 has helped to alleviate this dramatic underestimation of cold cirrus ICNC (in agreement with Barahona et al., 2017). "

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.

line 27: *"The simulations do not show any significant difference among each other, meaning that the parameterizations P13 and LD06 produce similar ICNC via pure heterogeneous nucleation."*

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

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

line 30: 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.

line 31-32: "*At mixed-phase conditions, the INP number is usually not so high that supersaturation is depleted before all particles have nucleated, so INP concentrations and ICNCs should generally correspond."*

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.

Conclusions

<u>page 18</u>

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

<u>page 19</u>

line 1-2: "*The comparison made with flight measurements has demonstrated that ICNCs are more realistically simulated when BN09 is used in the cirrus regime.*"

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!

lines 5-9: 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).

References

(in the order in which they appear in comments to the manuscript)

Matus and L'Ecuyer, 2017: The role of cloud phase in Earth's radiation budget

Hong et al., 2016: Assessing the radiative effects of global ice clouds based on CloudSat and CALIPSO measurements

Kienast-Sjögren et al., 2016: Radiative properties of mid-latitude cirrus clouds derived by automatic evaluation of lidare measurements

Gasparini and Lohmann et al., 2016: Why cirrus cloud seeding cannot substantially cool the planet

Mülmenstädt et al., 2015: Frequency of occurrence of rain from liquid-,mixed-, and ice-phase clouds derived from A-Train satellite retrievals.

Tan et al., 2016: Observational constraints on mixed-phase clouds imply higher climate sensitivity

Vergara-Temprado et al., 2018: Strong control of Southern Ocean cloud reflectivity by ice-nucleating particles

Sullivan et al., 2016: Understanding cirrus ice crystal number variability for different het. ice nucleation spectra

Storelvmo and Herger, 2014: Cirrus cloud susceptibility to the injection of ice nuclei in the upper troposphere

Penner et al., 2015: Can cirrus cloud seeding be used for geoengineering?

Joos et al., 2008: Orographic cirrus in the global climate model ECHAM5

Kärcher and Ström, 2003: The roles of dynamical variability and aerosols in cirrus cloud formation

Cziczo et al., 2013: Clarifying the dominant sources and mechanisms of cirrus cloud formation

Li et al., 2012: An observationally based evaluation of cloud ice water in CMIP3 and CMIP5 GCMs and contemporary reanalyses using contemporary satellite data, look for CIWP

Sourdeval et al., 2018: Ice crystal number concentration estimates from lidar-radar satellite remote sensing. Part 1: Method and evaluation

Gyrspeerdt et al., 2018: Ice crystal number concentration estimates from lidar-radar satellite retrievals . Part 2 : Controls on the ice crystal number concentration

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