**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)" by Bacer, S., Sullivan, S. C., Karydis, V. A., Barahona, D., Krämer, M., Nenes, A., Tost, H., Tsimpidi, A. P., Lelieveld, J., and Pozzer**

Summary:

Bacer et al. have implemented the ice nucleation parameterization by Barahona and Nenes (2009; BN09) into the EMAC model in both the cirrus and the mixed-phase cloud regime. For cirrus clouds this new parameterization accounts for a competition of water vapor by homogeneous and heterogeneous nucleation as well as by pre-existing ice crystals. For heterogeneous nucleation in cirrus and mixed-phase clouds the ice nucleating parameterization by Phillips et al. (2013) is selected (other ice nucleating parameterizations are available in the model). Dust and soot are used as ice nucleating particles to compare BN09 to the previous ice nucleation parameterization in EMAC. The deposition of water vapor on pre-existing ice crystals as well as heterogeneous nucleation in BN09 lead for high altitude cirrus clouds leads to an decrease in ICNC (and IWC) and an increase for lower altitude cirrus clouds compared to the previous ice nucleation parameterization which accounts only for homogeneous nucleation in cirrus clouds. For mixed-phase clouds BN09 seems to lead to an increase in ICNC compared the previous ice nucleation parameterization. A comparison of in-cloud ICNC to aircraft observations shows a better simulation at T<205K with BN09 but an underestimation of ICNC in the temperature range 205-222 K. For warmer temperatures there is no difference in ICNC of the simulations using different ice nucleation parameterizations.

General comment:

The ice nucleation parameterization implemented in the EMAC model in this study accounts for previously not represented processes and is therefore a meaningful contribution to GMD. But some details of the model parameterizations, configuration and the results are not discussed in enough detail (see specific comments below). Also the shortwave and longwave cloud radiative effects of the default simulation are too strong compared to observations. Therefore I recommend a major revision of the manuscript.

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

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.

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.

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?
All figures showing zonal and annual means (Figs. 2-4,S1-3): These figures need to show some measure of significance.
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?
P11L1-2: What is the explanation for this decrease in IWC while ICNC increase? Are this changes significant?
P11L8-9 and 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.
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.
P16L1: Are some of the quantities in Table 2 tuned to agree with observed values (Mauritsen et al. 2012, Hourdin et al, 2016)?
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?
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.
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?
P17L26-28: Give references for WISP-94 and ICE-L. Why are these two datasets so different (the 25$^{th}$ to 75$^{th}$ percentile do not overlap)?
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.
P19L31-P20L2: As this is interstitial aerosol, at what relative humidity is the wet diameter of the sulfate aerosol in the Aitken mode computed?

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.
P2L7: The greenhouse effects dominates for cirrus clouds e.g. Chen et al. (2000).
P2L9: Mixed-phase clouds can also occur at colder temperatures, rephrase.
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.
P2L11-12: Provide here references such as McCoy et al. (2016).
P2L25: Explain what you mean here by "the overestimation of vertical velocity".
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.
P3L12: Do you mean numerical parcel model simulations?
P6L21-22: Is this reduction done only for cirrus clouds are also for mixed-phase clouds?
P6L33: Is the cloud parcel mentioned here explicitly computed in EMAC or do the equations (1-6) provide analytical solutions for the cloud parcel?
P9L10-14: Provide references for the anthropogenic aerosol emissions and describe how natural aerosols (e.g. dust) are treated.
P8L6: Is n_x the number of interstitial aerosol particles or is the number of aerosol particles in cloud droplets tracked? Please clarify.
P10L26-P11L1: Do you mean that IWC decreases where ICNC decreases?
P11L32-P13L2: The increase in IWC in equatorial regions at 200 hPa is about 5-10% (Fig. 2), I would not call this dramatic.
P13L21-23: It should be mentioned that observations of cloud droplet number concentration are uncertain (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).
P17L28-30: Please add the number of hours in mixed-phase clouds.
P17L30: When the measurements are for INP this needs to reflected in Fig. 5 itself (at least in the figure caption).
P18L5: Use INP parameterization instead of INP spectrum.
Caption of Fig. S4: Give references for the observational datasets.
Fig. S5 is a table not a figure.

References:

Bennartz, R. and Rausch, J.: Global and regional estimates of warm cloud droplet number concentration based on 13 years of AQUA-MODIS observations, Atmos. Chem. Phys., 17, 9815-9836, https://doi.org/10.5194/acp-17-9815-2017, 2017.
Chen, T., Rossow, W. B., and Zhang, Y.: Radiative Effects of Cloud-Type Variations, J. Climate, 13, 264–286, https://doi.org/10.1175/1520-0442(2000)013<0264:REOCTV>2.0.CO;2, 2000.
Gryspeerdt, E., Quaas, J., Goren, T., Klocke, D., and Brueck, M.: Technical note: An automated cirrus classification, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-723, in review, 2017.
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McCoy, D. T., I. Tan, D. L. Hartmann, M. D. Zelinka, and T. Storelvmo (2016), On the relationships among cloud cover, mixed-phase partitioning, and planetary albedo in GCMs, J. Adv. Model. Earth Syst., 8, 650–668, doi:10.1002/2015MS000589.