

Interactive comment on “**Comparison of ice particle characteristics simulated by the Community Atmosphere Model (CAM5) with in-situ observations**” by T. Eidhammer et al.

**Anonymous Referee #2**

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*We thank the reviewer for the constructive comments and suggestions for improving this manuscript. Our responses to this review are in italics below.*

In this study, ice particle characteristics from two field observations (one is located over the mid-latitude and dominated by in-situ cirrus, while the other is located over tropics and dominated by anvil cirrus) are compared with those simulated by NCAR CAM5. Detailed ice particle properties, such as slope parameter, high moments and massweighted fall speed, are compared between simulations and observations. The model sensitivity to DCS (the critical size for autoconversion of cloud ice to snow) is further examined. The results presented here are interesting, and can help to guide the further improvement in the ice cloud microphysics in climate models. The manuscript is also well written, and I therefore recommend its publication with some further clarification.

I think the manuscript will benefit from some further discussions on what might cause the overestimation in the slope parameter and underestimation in high moments. I appreciate the sensitivity test with DCS documented in the manuscript, but as the authors showed that changes in DCS helps little to improve the slope parameter and high moments.

*To explain why the modeled high moments are underestimated we mainly have to speculate at this time. It could come from too few large particles. For example the aggregate efficiency is rather low (0.1), compared to some estimates at warmer temperatures (near freezing, in conditions with a quasi-liquid layer), or in the dendritic growth regime near -13 to -15 (Pruppacher and Klett 1997). As pointed out by another reviewer, this could explain some of the higher number concentration at higher temperatures as well. We have included some new text regarding this issue.*

Specific comments:

Section 2.1, aircraft measurements: it may be worth to discuss why data from some more recent field campaigns, such as SPARTICUS (also taken place over the SGP), is not included in this study. Some of these more recent field campaigns have done a better job on addressing the shattering effects, and may have observations that lasted longer.

*We are certainly open to using other campaign data to extend this research, and thank the reviewer for bringing other possibilities to our attention. We chose these particular field campaigns due to the general good quality of the data, our experience in working with them, and the presence of the CVI on the aircraft which helps constrain our estimate of integrated mass (and mass-weighted fall velocity as a result).*

Page 7644, lines 11-14: I understand the tuning of convective microphysics over ocean and land, but it is still not clear to me why this would lead to choose the ocean grids only. Will the results over land grid be quite different from what are presented in this study?

*The results do not change much. Including the moments over land in the moment calculations for TC4 show that they decreased by up to a factor of 1.25 for temperatures < -50C.*

Page 7646, line 17: I think it would be also highly interesting to see the value of  $\lambda$  and  $N_0$  used in the microphysics and radiation calculation, the one determined before all loss terms. Those  $\lambda$  and  $N_0$  diagnosed from the  $q$  and  $N$  output ensures the consistency with the model output, but those are not what really used in the microphysics and radiation calculation.

*After looking into this item again we need to clarify a few points. First, the  $\lambda$  and  $N_0$  for cloud ice calculated offline from the output  $q$  and  $N$  are consistent with the  $\lambda$  and  $N_0$  directly output (this output is actually after all microphysical source/sink terms are applied). Further, this output is consistent with the ice parameters used for the radiation (i.e., after the microphysical source/sink terms are applied).*

*For the  $\lambda$  and  $N_0$  for snow (which we found to be different from the direct output and offline calculations based using  $q$  and  $N$ , in contrast to cloud ice), the reason for this difference is that the output  $\lambda$  and  $N_0$  are from vertical level interfaces, while output snow mixing ratio and number concentration are interpolated to the vertical level mid-points. However, for radiation the parameters for snow are calculated from the interpolated  $q$  and  $N$  at the level mid-point, and hence are consistent with our off-line calculations. Again, this is after microphysical source/sink terms are applied so that the offline  $\lambda$  and  $N_0$  are consistent with what is used in the radiation.*

*Thus, the values of  $\lambda$  and  $N_0$  for both cloud ice and snow that we calculate offline are consistent with what is going into radiation calculations. We have removed the text in the manuscript on this to avoid confusion.*

Page 7651, lines 23-24: the last sentence (“A smaller  $B$  . . .”) is not clear to me and needs some clarification.

*This sentence has been improved: “For the ARM-IOP case, Heymsfield et al. (2013) found the  $B$  coefficient to be -0.0292, which is comparable with our model results.”*

Page 7652, line 4:  $\lambda$  is fairly constant for cloud ice. I think  $\lambda$  generally decreases with increasing temperature for cloud ice, as  $q_i$  increases with temperature. The fairly-constant lines in Figure 3 is mainly because a log-scale was used.

*We agree that  $\lambda$  for cloud ice decreases with increasing temperature. We have removed cloud ice in the sentence referred to here.*

Section 3.1.2, moments: For the 0th moment, it is worth to discuss that though it represents the number concentration, it is the number concentration of particles larger than a certain particle size cut ( $D_{min}$ , 75  $\mu m$  in the paper). Predicted ice crystal number concentration  $N$  from the model without this size cut can be substantially higher. It is also worth to discuss the implication for comparing modeled and observed ice crystal number concentrations.

*We have overlaid the modeled moments when integrating from 0 micron in figures 4 and 5.*

Page 7654, line 4: Please clarify how the competition between homogeneous and heterogeneous nucleation does not happen readily in convective clouds in CAM5 *This statement was wrong since the microphysical scheme is for stratiform clouds and does not include convective clouds. The higher ice crystal concentration in TC4 than in the ARM-IOP case is more likely from detrained condensate. We have removed the sentence referred to above and included this text instead:*

*“Note that although the observations and model results for TC4 considered here are of stratiform cloud types (anvil cirrus), detrainment plays an important role. The source of the ice crystal number concentration of the detrained condensate comes from an assumed particle radius (25  $\mu m$ ) and therefore the model does not explicitly calculate ice nucleation from the detrained ice.”*

Page 7655, Figure 7: blue lines. In the regime where cloud ice dominates, why does smaller  $\lambda$  (blue lines) predict even lower  $V_m$  than the original one (red lines)? *Thank you for pointing this out. It turns out during the data processing and plotting, a test had been included that works for the regular cases where  $\lambda$  is not changed. This test does not work for cases when  $\lambda$  is changed. The test in the script is removed and now  $V_m$  is always larger when  $\lambda = \lambda / 2$ .*

Page 7656, Figure 7: comparing green lines with red lines. At lower temperature, it is not clear to me why  $V_m$  has little change if both  $a_i$  and  $a_s$  increase by 50%. *In the area where the  $V_m$  does not change much (very low temperatures),  $V_m$  is mainly affected by cloud ice. In this area, the  $\lambda$  value increases slightly (up to a factor of 1.2) with increasing  $a_i$ . Thus, this increase in  $\lambda$  reduces the expected impact on  $V_m$  with increasing  $a_i$  ( $V_m$  is dependent on  $1/\lambda$ , see Eq. 11 in revised manuscript).*

Section 3.2: It may be worth to discuss how the cut-off size used for calculating moments may affect how the DCS-moment relationship. For example, with  $D_{CS}=80\mu m$ , and  $D_{min}=75\mu m$ , most of particles examined here are located as snow category. If we choose  $D_{min}=0$ , the DCS-moment relationship may be different. *We note that even for a case with  $D_{cs}=80 \mu m$ , there is still a large amount of ice larger than 80  $\mu m$ .  $D_{cs}$  is a size parameter for conversion of cloud ice to snow, but it does not mean that all particles larger than  $D_{cs}$  are classified as snow. Both the cloud ice and snow distributions are complete, meaning they extend from zero to infinity with*

*significant concentrations larger than  $D_{cs}$  for cloud ice and smaller than  $D_{cs}$  for snow. This concept is now clarified in the paper.*

*As stated earlier, we have included an additional figure showing how the moments look like when we use  $D_{min} = 0$ , instead to  $D_{min} = 75 \mu\text{m}$ . This figure clearly shows that the lower moments are heavily influenced by which  $D_{min}$  we use, while the higher moments are less influenced. For simplicity, we did not include every different  $D_{cs}$  cases, since we cannot really compare the entire size distribution against observations for the lower moments anyway, which are the cases that are mostly affected. We did, however, find that the relationship between the moments for different  $D_{cs}$  values was consistent for  $D_{min} = 0$  and  $D_{min} = 75$  microns, and hence this does not change our conclusions.*

Page 7659, lines 22-23: how are the zonal-mean effective radii calculated?  
*The zonal-mean effective radii are calculated only for gridboxes that contain ice, snow or liquid. Further, the radii are the in-cloud values and not the grid-box mean value.*

Page 7659, line 29: why is there a slight increase of snow water path with increasing DCS in Figure 13 c)?  
*We tried before publishing this paper to look into this, but found it difficult to sort out which processes that cause this increase in snow water path because of the complexity of nonlinear interactions between the various microphysics processes.*

Page 7660, lines 17-18: It may be worth to comment why liquid water path in the midlatitudes increases with decreasing DCS? (I guess this is due to Bergeron-Findeisen process).  
*Bergeron processes (total for both snow and ice) decreases with increasing  $D_{cs}$ . This means that if we only consider Bergeron processes we should expect more liquid at higher  $D_{cs}$ , opposite to what we seen in figure 14b. Thus, it appears that the Bergeron process cannot explain this result.*

Page 7661, line 21: you mean we see a lower crystal concentration?  
*You are correct. Actually, we removed that part of the sentence as we have already stated that the concentration is lower in the sentence above.*

Technical corrections:

Page 7642, line 12: remove “,” *Done*

Page 7642, line 20: remove “,” *Done*

Page 7645, line 16: “while mass and number concentrations are proportional to the 0th and 3rd moments” ! “while number and mass concentrations are proportional to the 0th and 3rd moments, respectively”? *Done*

Page 7652, line 17: “N0” ->“N”? *Correct, this is a mistake introduced in the typesetting phase. This is now corrected.*

Page 7652, line 27: “-4” ! “-40” *Correct, this is a mistake introduced in the typesetting phase. This is now corrected.*

Page 7659, line 6: Zhang et al. (2013) ! Zhao et al. (2013) *Done*