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Interactive Comment

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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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 mass-weighted 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.

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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.

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.

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?

Page 7646, line 17: I think it would be also highly interesting to see the value of lamta and N0 used in the microphysics and radiation calculation, the one determined before all loss terms. Those lamta and N0 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.

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

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

Section 3.1.2, moments: For the 0th moment, it is worth to discuss that though it

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represents the number concentration, it is the number concentration of particles larger than a certain particle size cut (Dmin, 75 um 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.

Page 7654, line 4: Please clarify how the competition between homogeneous and heterogeneous nucleation does not happen readily in convective clouds in CAM5

Page 7655, Figure 7: blue lines. In the regime where cloud ice dominates, why does smaller lamta (blue lines) predict even lower Vm than the original one (red lines)?

Page 7656, Figure 7: comparing green lines with red lines. At lower temperature, it is not clear to me why Vm has little change if both ai and as increase by 50%.

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 DCS=80um, and Dmin=75um, most of particles examined here are located as snow category. If we choose Dmin=0, the DCS-moment relationship may be different.

Page 7659, lines 22-23: how are the zonal-mean effective radii calculated?

Page 7659, line 29: why is there a slight increase of snow water path with increasing DCS in Figure 13 c)?

Page 7660, lines 17-18: It may be worth to comment why liquid water path in the mid-latitudes increases with decreasing DCS? (I guess this is due to Bergeron-Findeisen process).

Page 7661, line 21: you mean we see a lower crystal concentration?

Technical corrections:

Page 7642, line 12: remove "," Page 7642, line 20: remove "," Page 7645, line 16: "while mass and number concentrations are proportional to the 0th and 3rd moments" →

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“while number and mass concentrations are proportional to the 0th and 3rd moments, respectively”?

Page 7652, line 17: “N0” ->”N”?

Page 7652, line 27: “-4” → “-40”

Page 7659, line 6: Zhang et al. (2013) → Zhao et al. (2013)

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 7637, 2014.

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