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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.

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General Comments:

This paper compares ice cloud properties predicted by CAM5 with those observed during two field campaigns. Such comparisons are greatly needed to improve climate models and this work is commendable. The paper is well organized and generally well written. However, there are some issues that were not clear to this reviewer, and although possibly clear to others, the authors are requested to consider the following comments.

Major Comments:

1) After reading this paper, the title does not appear to capture the paper's contents (i.e. there is little about ice particle characteristics in the paper). Perhaps the paper could be retitled something like "Comparison of ice cloud properties simulated by the Community Atmosphere Model (CAM5) with in-situ observations".

2) Section 2.1: Please state what type of methodology was used in processing the microphysical data. For example, Appendix A in Lawson (2011, AMT) describes various methods for processing the data and determining the dimensions of ice particles. While Appendix A is not applicable to the data used here, DMT must have employed some data processing protocol, and this will determine how the ice particle dimension was measured.

3) As acknowledged by the authors, the Cloud Imaging Probe or CIP (used in this study) is vulnerable to the sampling problem of ice particle shattering. During TC4, the 2DS probe was also flown, and the 2DS appears to be less vulnerable to the shattering problem (e.g. Lawson 2011, AMT). Were the CIP and 2DS ever flown together on flight missions, and if so, can they be intercompared over their common size-range? Favorable comparisons would engender greater confidence that this comparison between CAM5 microphysical predictions and CIP measurements was meaningful.

4) Page 7647, lines 8-11: Is the assumption Fs $\geq$ Fi always valid, even for relatively young cirrus and TTL cirrus? For example, the cirrus literature (e.g. Lawson et al. 2006, JAS) and our own research show that for T < -53°C approximately, cirrus ice particle size distributions (PSDs) often do not extend beyond $\sim$ 250 microns in particle length, indicating virtually all ice particles can be classified as cloud ice (in which case Fs = 0). Please comment on how such conditions are addressed in Eqn. 6 and Eqn. 10.

5) Page 7648, lines 3-12: In this section it is not clear how the measured PSD moments are calculated for comparison with the CAM5 predicted moments. For example, for M2

in CAM5, k = 2, but in actual ice clouds, 1 < k < 2. Similarly, for M3 in CAM5, k = 3, but for aggregated snowfall k $\approx$ 2. How are these facts considered and accounted for in the comparison of moments between CAM5 and natural PSD? Please provide equations showing how the moments were calculated from the measurements.

6) Page 7648, lines 8-10: The λ predicted from (5) may be greater than the λ obtained from a natural ice cloud having the same N and q if only particles having D > 75 microns are considered in the natural ice cloud (whereas all sizes are considered in (5)). This is because the concentration of smaller ice crystals (D < 75 μm) is generally "super-exponential" (i.e. anomalously high) in natural ice clouds (see for example Cotton et al. 2012, Q. J. Royal Met. Soc.). Thus λ from (5) will generally not be consistent with the λ fitted to observations where only particles having D > 75 μm are considered. However, based on relationships provided in Mitchell (1991, JAS), the error should be on the order of 16% to 30% for μ ranging from -0.6 to -1.0.

7) Page 7649, Eq. 9: When I derived Eq. 9, I got the same result as shown in this paper except that the denominator was 6λb Γ(4,Dmin) (i.e. no "x" is present). Is this "x" in the denominator of Eq. 9 a mistake? If not, please define "x". Also, is the calculation of Vm in CAM5 based on Eq. 9?

8) Page 7649, lines 5-9: Based on my research there appears to be a lack of support regarding the value of "κ" for determining ice fall speeds. While Foote and du Toit (1969, JAS) found $\kappa \approx$ 0.4 for rain drops, I found no other studies that determine a value for Đž. The authors cite Heymsfield et al. (2007) but this paper states that κ is usually given as 0.4 (Rutledge & Hobbs 1984) or 0.5 (Liu et al. 1983). When I read these papers, the Liu 1983 paper did not give justification for the value assigned to κ. The authors of this current paper state that κ = 0.54, which is not supported in the literature as far as I can determine. The authors must have a reason for using this value, but this reason needs to be clearly stated with evidence supporting its use. This would really "clear the air" on this issue, since so many papers cite H2007 to justify their use of κ.

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9) Page 7649, lines 17-21: This paragraph addresses the calculation of Vm based on in situ measurements, but does not provide sufficient information on how this was done. For example, the Heymsfield-Westbrook scheme requires knowledge of ice particle projected area and mass; how were these determined from the measurements? While area is measured directly by the CIP at a pixel resolution of 25 μm (this resolution should be mentioned under “Aircraft measurements”), it is not clear how ice particle mass was obtained. Please also show the formula used to calculate Vm from the in situ data.

10) Page 7653, lines 21-28: I think this paragraph refers to Fig. 4 but it is not clear; please mention Fig. 4 if that is correct.

11) Page 7654, lines 1-15: For M0 in Fig. 5, the model overestimate of M0 increases with increasing temperature. Could this be anecdotal evidence that the aggregation process in CAM5 is under-active?

12) Page 7656, lines 25-26: There is some empirical evidence regarding the value of Dcs in Cotton et al. (2012, QJRMS), where they attempt to deduce Dcs from aircraft in situ data.

13) Page 7657, lines 18-27: While no single value of Dcs is a silver bullet, Fig. 9 does suggest that a variable Dcs may improve agreement with measurements, with small Dcs at cold temperatures. A number of papers show the temperature dependence of PSD and the two PSD modes corresponding to cloud ice and snow. These papers suggest that Dcs should be a function of temperature; see, for example, Field (1999 JAS, 2000 QJRMS). Please make Dcs a function of temperature such that agreement with measurements is optimized, and show the resulting Dcs parameterization. Please also show comparisons between measured and modeled PSD moments & Vm as a function of temperature.

14) Page 7658, lines 13-20: Same comment as in (13).

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Minor Comments:

1) Page 7647, lines 1-3: “snow and cloud ice” should be “cloud ice and snow”

2) Page 7647, line 23: “diameter” => “length”? Note that the concept of diameter does not apply to non-spherical ice particles.

3) Page 7648, line 8: For clarity, after “Note that”, please add “in model calculations”.

4) Page 7652, line 17: No => N ?

5) Page 7659, line 29: decrease => increase?

6) Fig. 4.; no y-axis units are shown for M3

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C1613/2014/acpd-14-C1613-2014-supplement.pdf

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