Response to
ACP Review by David L. Mitchell
Title: Comparison of ice particle characteristics simulated by the Community Atmosphere Model (CAM5) with in-situ observations Author(s): T. Eidhammer et al.
MS No.: acp-2013-1025 MS Type: Research Article

We thank David Mitchell for the constructive comments and suggestions for improving this paper. Our responses to this review are in italics below.

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.

Specific 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".
Agreed, title has been changed.

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.
We included this sentence highlighting the methodology:

"Furthermore, images from the two-dimensional probes were measured by Dmax, where Dmax is the diameter of the smallest circle that completely encloses the projected image. Area ratio, given by the area of the imaged particle divided by the area of the smallest enclosing circle, was used to filter poorly imaged particles from the analysis following the criteria given in Field et al. (2006). "

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.
For TC4, comparisons are done in: Heymsfield, A. J., D. Winker, M. Avery, M. Vaughan, G. Diskin, M. Deng, V. Mitev, and R. Matthey, 2014: Relationships between Ice Water Content and Volume Extinction Coefficient from In Situ Observations for Temperatures from 0° to -86°C: Implications for Spaceborne Lidar Retrievals. J. Appl. Meteor. Clim., ***53****, 479–505.*

In that paper, a combination of CIP and PIP, and 2DS estimated extinction coefficients (σ) are compared against extinction observations by DLH (diode laser hygrometer probe). The combination of CIP and PIP compared better with the DLH probe than the 2DS. During TC4, 2DS still had shattering issues (no special front tips yet on the instrument). Heymsfield et al. 2014 suggests that for the TC4 data, that occasional small particles produced by shattering that entered the field of view of the probe were not identified or removed using interarrival times but contribute significantly to σ for reasons related to depth-of-field considerations. We included a paragraph dealing with this issue in the text.

4) Page 7647, lines 8-11: Is the assumption $F_s \geq F_i$ 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 ~ 250 microns in particle length, indicating virtually all ice particles can be classified as cloud ice (in which case $F_s = 0$). Please comment on how such conditions are addressed in Eqn. 6 and Eqn. 10.
In the model, the fraction of snow is calculated by a simple maximum overlap assumption of the cloud fraction above. This is done regardless of the snow mass mixing ratio (which could in fact be zero). Thus, the snow fraction will always be greater than or equal to the cloud ice fraction, by design of the parameterization. However, even though the snow fraction may be large, the snow mass mixing ratio could be very small. We have clarified this in the text.

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 ≈ 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.
The measured moments are in pure analytic form. Only integer moments were computed, and they do not have to be explicitly tied to natural properties such as IWC. The idea is that each moment weights a certain portion of the size distribution differently (low moments for small particles, and high moments for large ones), to allow a simple comparison with the modeled distributions. The formula used to compute the measured moments is as follows:

$$M(p) = \sum_{D_{\min}}^{D_{\max}} N(D) D^p$$

Where the p is the moment to be computed, D is particle diameter in the range of interest, and N(D) is the number concentration of particles at each D.

We have included some text to clarify the calculations of moments from the observations.

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.
The model assumes exponential PSDs by design of the parameterization, and we wish to calculate λ from the observations in a way that is consistent with this assumption for an apples-to-apples comparison. Furthermore, the λ derived from observations were calculated by linear fit in log-linear space to the measured size distributions. Thus the λ from the model and observations should be consistent.
Otherwise, differences in λ calculated by incorporating sub-exponential or super-exponential effects on the observed λ will convolve biases in the mean size (for particles > 75 microns, inversely proportional to λ for exponential PSD) with the assumption of the PSD shape in the model.

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?
A mistake occurred during the typesetting phase. The x should not have been included, and was not included in our initial submission.

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 κ ≈ 0.4 for rain drops, I found no other studies that determine a value for κ. 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.
In Heymsfield et al., (2007) (Heymsfield, A. J., Bansemer, A., and Twohy, C. H.: Refinements to ice particle mass dimensional and terminal velocity relationships for ice clouds. Part I: Temperature dependence. J. Atmos. Sci., 64, 1047–1067, 2007) it is explicitly stated that κ = 0.54. It is even mentioned in the abstract.

Thus, at the time of running the model, I think it is justified to use 0.54, since we have a concrete reference for this value. Our intent for this publication was also to run the model in the default state, in the same way it is distributed to users, which assumes k = 0.54. There are potentially some issues with applying this value of k to small ice particles in the distribution, but investigation of this issue is beyond the scope of this paper.

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.
Yes, fall velocity is computed using the Best/Reynolds number approach described in Heymsfield and Westbrook (JAS 2010). Projected area is measured directly with the CIP in TC-4 (25 μm resolution) and the 2D-C (30 μm resolution) in the ARM project. Mass is computed from the power-law relationship given in Heymsfield et al (JAS 2010), $m=0.00528D^{2.1}$, which when integrated gave generally good agreement with the total mass measured by the CVI. We have included some new text in the manuscript with more detail to address this point. However, the formula for calculating V_m includes several equations and would take too much text in the manuscript. We therefore have chosen to refer to the relevant published papers instead.

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.
We made it clear in the text that we are still discussing Fig. 4.

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?
It is possible that an under-estimate could cause this bias, but there are other possibilities too, such as ice nucleation rate. It is not possible based on current observational data to know what is the specific cause of this bias. We do note that the aggregation 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). We have added

some text about this issue in the paper.

12) Page 7656, lines 25-26: There is some empirical evidence regarding the value of Dcs in Cotton et al. (2013, QJRMS), where they attempt to deduce Dcs from aircraft in situ data.
The autoconversion of ice to snow does not just represent the aggregation process but also represents growth from small to large ice by vapor diffusion, and potentially riming. Thus, it might be difficult to say much about "empirical" values for D_{cs} because it is not a physical parameter.

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.
As stated above, D_{cs} is not a physical parameter. Further, the intent of this paper is to test existing parameters but not to extend and improve the parameterization. This will be a focus of future work, by improving how ice and snow is dealt with in the model. We therefore kindly reject the suggestion of developing a parameterization for the D_{cs} parameter in this work.

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

Technical Comments:
1) Page 7647, lines 1-3: "snow and cloud ice" should be "cloud ice and snow"
This is corrected.
2) Page 7647, line 23: "diameter" => "length"? Note that the concept of diameter does not apply to non-spherical ice particles.
We changed diameter to length.
3) Page 7648, line 8: For clarity, after "Note that", please add "in model calculations".
It is added.
4) Page 7652, line 17: No => N ?
This is corrected. An error was introduced during the typesetting phase.
5) Page 7659, line 29: decrease => increase?
This is corrected
6) Fig. 4.; no y-axis units are shown for M3
M3 is unitless. We changed the unit bracket to [unitless]