Authors Response

List of all relevant changes:

All references to pages, lines, figures and equations are from the published ACPD manuscript:
http://www.atmos-chem-phys-discuss.net/14/7637/2014/acpd-14-7637-2014-discussion.html

Below is a list of all relevant changes made in the manuscript:

We included all technical corrections suggested by the reviewers

Title has changed to: *Comparison of ice cloud properties simulated by the Community Atmosphere Model (CAM5) with in-situ observations*

Page 7643, after line 4: We included this paragraph:
During the TC4 campaign, a 2D-S (Stereo) probe was also flown on the NASA DC8 aircraft (Toon et al., 2010). This probe has a lower size detection limit and better resolution compared to the CIP. Heymsfield et al. (2014) used volume extinction coefficients (σ) to compare 2D-S and CIP+PIP observations against a diode laser hygrometer (DLH) probe, and found that σ from CIP+PIP compared well, while the 2D-S σ were about 50% higher than the DLH σ. They suggested that the reason for the overestimation of 2D-S σ was due to occasional small particles from shattering that were not removed during the post processing procedures. We therefore only use the CIP + PIP observations here.

Page 7643, line 14: We included this paragraph:
Images from the two-dimensional probes were analyzed using D_{max}, where D_{max} 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).

Page 7644, line 14: We included this sentence:
However, including grid boxes over land has a minimal impact and does not change our conclusions (not shown).

Page 7645, line 20: We now denote moments when integrated from zero with a "*", to differentiate with moments integrated from D_{min}.

Page 7646, line 10. We included a new equation, describing lambda in terms of moments (new equation 6).

Page 7646, lines 13-17: We removed sentence and replaced with:

Note that the size distribution parameters and moments are derived from the q and N after they are updated from the microphysical processes, consistent with the quantities used for the radiation calculations.

Page 7647, 11: We included sentence:
Furthermore, this is done regardless of the snow mass mixing ratio, which could in fact be zero.

Page 7647: We included the description of how to determine the intercept parameter from observations, as the estimation of the slope parameter is linked to the intercept parameter. The text from line 18 now reads:
"The λ and N0 derived from observations were calculated by linear fit in log-linear space to the measured size distributions. The fits were performed using a principal component analysis to minimize the error normal to the fit line. Only size spectra that provided at least 5 size bins with non-zero concentration were considered in order to maintain a reasonable fit. This threshold was generally met in this study when a measurable size distribution existed from 75 μm to at least 275 μm in length. When larger particles were present up to 30 bins were included in the fits. The potential fitting errors, and resulting λ and N0 errors, depend on the number of bins used for the fit, the number of particles measured in each size bin, and the accuracy of the instruments in a particular size range. These conditions are most favorable in broad size distributions with low λ. Due to probe inaccuracies (Strapp et al., 2001) and smaller sample volume for small particles, the errors will be larger for high λ."

Page 7648, after line 12: Added this paragraph, with new equation:
The measured moments ($M_{obs,k}$) are calculated using

$$M_{obs,k} = \sum_{D_{\min}}^{D_{\max}} N(D) D^k \ . \qquad (9)$$

Only integer moments were computed, and physical quantities may not correspond to the same moment for both the observations and model (for example, ice water content is proportional to M_3 in the model following the assumption of spherical particles but is closer to M_2 in the observations). 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. Since the measured moments are in a pure form, the observed and modeled moments can be compared directly.

Page 7649, line 21: Added these sentences:
The projected area is measured directly with the CIP (25 μm resolution) in TC4 and the 2DC (30 μm resolution) in the ARM-IOP project. Mass is computed from the power-law relationship m = 0.00528D2.1 given in Heymsfield et al. (2010), which when integrated gave generally good agreement with the total mass measured by the CVI.

Page 7650, line 13: We included this sentence:

Although D_{cs} is a size parameter for conversion of cloud ice to snow, not all particles larger than D_{cs} are classified as snow since the cloud ice distribution is complete (meaning that it extends from zero to infinity with significant concentrations larger than D_{cs}).

Page 7651, line 23-24: We changed the sentence to:
For the ARM-IOP case, Heymsfield et al. (2014) found the B coefficient to be -0.0292, which is comparable with our model results.

Page 7652, lines 3 and 4. We changed sentence to:
Figure 3 shows that when the modeled λ is calculated individually for snow and cloud ice, λ for snow is fairly constant over all temperatures.

Page 7653, line 20: We included these sentences:
The modeled M0 show a slightly smaller decrease with increasing temperature compared to the observations. The aggregation efficiency specified in the model 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 -15o C (Pruppacher and Klett 1997). This could result in a smaller decrease in M0 with temperature. However, the ice nucleation rate in CAM could also be a source of the large modeled M0 values. It is not possible based on current observational data to isolate the cause of this bias.

Page 7653, line 25: We added these sentences:
An underestimation of the higher moments by the model indicates that the concentration of large particles is too low. This could be due to uncertainties in several microphysical processes and parameters including the rather low aggregation efficiency or too slow diffusional growth.

Page 7654, lines 3 to 6: We removed sentence and replaced this sentence on line 8:
The source of the ice crystal number concentration of the detrained condensate comes from an assumed particle radius (25 μm for deep convection and 50 μm for shallow convection) and therefore the model does not explicitly calculate ice nucleation from the detrained ice.

Page 7654, line 16 to 18: Sentence replaced by:
For the moments, we have only considered particles larger than 75 μm. For comparison Figs. 4 and 5 also show the moments for the ARM-IOP and TC4 cases from the model when integrating the moments from either 0 μm or 75 μm. Clearly the lower moments increase when including all sizes, while the higher moments are not as sensitive to inclusion of small sizes in the integration.

Page 7645: We introduced a new discussion about modeled and measured ice water content:

The moment comparison gives an illustration of the behavior of the modeled and observed size distributions. However, this comparison does not reveal differences in ice (+snow) water content (IWC) since IWC in the model is proportional to M¬3 (assumed spherical shape) while the observed IWC is proportional closer to M2. Therefore we also show a comparison of the IWC (Fig. 6). The observed IWC from ARM-IOP is rather insensitive to temperature, while the modeled IWC has a sharp increase with temperature, with smaller than observed values at low temperatures and larger values at relatively high temperatures. For the TC4 IWC, the model and observation have a similar temperature trend but the modeled IWC is slightly lower than the observed IWC.

Figures 4 and 5: Added modeled moments when integrating from Dmin = 0 μm to infinity.

Added a new Figure 6, comparing modeled and observed ice water content.

Figure 7: Fixed an error of the blue curve (lambda = lambda/2)

Point by point response to Reviewer 1:
We thank the reviewer for the constructive comments and suggestions for improving this paper. Our responses to this review are in italics below.

In this study, the authors evaluated ice microphysics in CAM5 using aircraft observations. Several parameters about ice size distribution were compared with two different field campaigns. The sensitivity of the ice-snow autoconversion was also evaluated. These detail evaluations are useful for improving cloud microphysics scheme in CAM5.

Specific comments:
Page 7647, the method for calculating the slope parameter from observations has been introduced. Please also introduce how to calculate the intercept parameter from observations.

The fitting method described in the manuscript returns both the slope and intercept parameters simultaneously, since these are the two parameters required to describe the best-fit exponential function. Some text has been added for clarification.

Page 7648, the authors only consider ice sizes larger than 75 micron, both in the observations and in the model, to be consistent. As far as I know, CAM5 model results show that cirrus clouds at low temperature are dominant by ice particles with size less than 75 micron. Please discuss this issue in detail. It is better to show the fraction of ice particles with size greater than 75 micron based on model output mass concentration and number density.

We have overlaid the modeled moments when integrating from D_{min} = 0 μm in figures 4 and 5 for comparison and included a short discussion in the paper.

Point by point response to Reviewer 2:

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 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.
After looking into this item again we need to clarify a few points. First, the lambda and N0 for cloud ice calculated offline from the output q and N are consistent with the lambda and N0 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 N0 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 N0 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 N0 are consistent with what is used in the radiation.

Thus, the values of lambda and N0 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: 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.
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 (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.
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 μm) and therefor 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 lamta (blue lines) predict even lower Vm 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 λ is not changed. This test does not work for cases when λ 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 Vm has little change if both ai and as increase by 50%.
In the area where the Vm does not change much (very low temperatures), Vm is mainly affected by cloud ice. In this area, the lambda value increases slightly (up to a factor of 1.2) with increasing ai. Thus, this increase in lambda reduces the expected impact on Vm with increasing ai (Vm 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 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.
We note that even for a case with D_{cs}=80 μm, there is still a large amount of ice larger than 80 μm. 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 Dmin = 0, instead to Dmin = 75 μm. This figure clearly shows that the lower moments are heavily influenced by which Dmin 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 Dmin = 0 and Dmin = 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 Dcs. This means that if we only consider Bergeron processes we should expect more liquid at higher Dcs, 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*

Point by point response to David L. Mitchell:
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 Fs ≥ 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 ~ 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.
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]