Atmos. Meas. Tech. Discuss., 8, C1693–C1697, 2015
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Atmospheric Measurement Techniques
Discussions

Interactive comment on "The microwave properties of simulated melting precipitation particles: sensitivity to initial melting" *by* B. T. Johnson et al.

Anonymous Referee #1

Received and published: 25 June 2015

This paper is a continuation work on the forward modeling of radar and radiometer signatures of icy hydrometeors. Over the last 5 years or so, many papers have been published on using detailed 3D models to tackle this problem, but until now, most such studies have avoided the problem of snowflake melting because of the added complexity introduced by the melting process. As such, this study is the next logical step in this line of research, and potentially an important contribution to bringing realistic shape models available for the entire vertical column, thus improving radar and radiometer retrievals.

The paper is well written and easy to follow. However, I found a few points that might

limit the validity and applicability of the research. Thus, I believe that major revision is necessary before this paper should be published.

Major comment:

1. The authors simulate snowflakes of different sizes by scaling the dipole size relative to the effective radius, as described on page 5622, lines 8-10. This causes the exponent b in the mass-dimensional relationship $m = aD^b$ (where m is the mass and D is the diameter) to take the value of $b = 3$. In reality, aggregate snowflakes have approximately $b = 2$. This causes the mass scaling, and consequently the (back)scattering cross section scaling, to be incorrect. For example, Leinonen and Moisseev (2015; J. Geophys. Res. Atmos., 120, 229–239, doi:10.1002/2014JD022072) found significantly different results for their $b = 2.1$ aggregates compared to those that were produced by the Petty and Huang (2010) aggregates with $b = 3.0$. This is particularly disappointing because this issue could have been rectified relatively easily before the DDA runs were made, especially given that the authors report that the melting algorithm is quite computationally efficient. Now, with considerable time invested into the DDA calculations, it probably cannot be fixed within the scope of this paper. This problem should be recognized in the paper and its effects on the results examined, especially on the results integrated over size distributions (as these are the most likely to be affected by the incorrect scaling).

Minor comments:

Page 5617:

Lines 7-9: "The onset of melting is generally believed..." I agree, but a reference should be provided.

Page 5619:

Lines 22-23: Perhaps I have missed it, but how were the dry aggregate models generated?

Page 5620:

Line 9: "A stochastic control factor" - Does this mean that some of the qualifying dipoles are randomly selected?

Line 12: Why is such a coating expected to occur (at least on a macroscopic scale)? Please provide a reference.

Lines 19-20: While ice structure collapse seems to be simulated at least implicitly by SPMM, breakup is not simulated at all due to the center-of-mass seeking behavior.

Page 5621:

Lines 1-2: How does the execution time scale with the number of dipoles?

Line 11: It would be more appropriate to say "DDSCAT 7.1 implementation of the DDA method".

Line 19: This reads like Warren and Brandt (2008) is the reference to the DDA validity criterion. Furthermore, the $|m|kd < 0.5$ threshold is not for "satisfactory convergence" but rather a criterion that ensures that we satisfy the fundamental assumption of DDA: that the electric field is approximately constant within each dipole. This should be clarified and a reference to the validity criterion provided.

Lines 24-25: I'm not sure how $|m|$ increases when the amount of liquid water increases. The number of dipoles with larger $|m|$ increases, yes, but how do you calculate $|m|$ for the purposes of the convergence threshold? To my knowledge, the maximum $|m|$ found within the particle should be used to ensure the validity of DDA.

Page 5622:

Line 21: Typo, "implentation" -> "implementation"

Lines 19-24: Interesting that the execution time becomes so much longer with water introduced. Is this mainly because of an increase in the number of DDA iterations

required for convergence?

Page 5623:

Line 2: "two-frequency groups" -> "two frequency groups"

Page 5624:

Lines 10-13: Why does the scattering contribution decrease with melting?

Line 13: Interesting to see this and I wonder if this would change the interpretation of the "dark band" sometimes seen at the W-band (e.g. Kollias and Albrecht, 2005, Geophys. Res. Lett. 32, L24818, doi:10.1029/2005GL024074).

Page 5627:

Line 11: The denominator should be $(m_w^2 + \mathbf{2})$

Line 12: 0.93 is a good value for Ku- and Ka-bands, but is somewhat off for W-band (for example CloudSat uses 0.75).

Lines 19-23: Are all snowflakes in the size distribution assumed to have the same melted fraction? In reality, I would expect that smaller snowflakes melt faster than larger ones.

Page 5628:

Line 1: The IWC and the reflectivity depend on both N_0 and D_0. What value of N_0 was used? I could not find it in the paper.

Line 6: $D_0 = 0.55$ cm is very large considering that the largest snowflake you have is only 2500 um (0.25 cm). This causes significant truncation in the IWC and the reflectivity, as they are both disproportionately dependent on the large snowflakes. I made a couple of quick calculations assuming D^3 dependence for IWC and D^6 for reflectivity (this worst case scenario would probably be most realistic at the Ku-band). Compared to an untruncated exponential size distribution, your IWC is smaller by a factor of 0.44 and your reflectivity is smaller by a factor of 0.35 (-4.5 dB).

Line 13: Such heavy snowfall would almost certainly result in aggregates much larger than 2500 um.

Lines 17-21: If you are simply scaling the snowflakes as mentioned in my major comment, you should get a D^6 rather than D^4 diameter dependence in the Rayleigh regime, so I'm not sure if you can draw many conclusions about this. Furthermore, I might be mistaken, but sphere-based bright band models manage, to my knowledge, produce a bright band without the enhanced aggregation.

Line 23: "features are" -> "feature is"

Figure 1: It is hard to see what is happening to the details of the snowflake as it melts. Perhaps you could provide zoomed-in views of a few of the images?

Figure 3: In sub-figures c and d, the colors of the bottom 15 or so points are practically indistinguishable. I suggest tuning the color scheme to improve the readability.

Figure 15: I wonder if the implicit PSD truncation (see my comment for page 5628, Line 6) at high D_0 affects the results shown in this figure. It certainly seems that the TB saturates at high D_0 values; this may be because the PSD is not changing much in practice because of the truncation.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 5615, 2015.