Atmos. Meas. Tech. Discuss., 8, C3144–C3151, 2015
www.atmos-meas-tech-discuss.net/8/C3144/2015/

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.

B. T. Johnson et al.

jbenjam@gmail.com

Received and published: 25 September 2015

We appreciate the patience of the referees and editors during the review of this article. Due to a recent job change of the primary author, it has been a challenge to find enough time to respond to your much appreciated comments.

Throughout this response, I'll refer to Referee #1: as [R1] and #2 as [R2]. Quoted text is taken straight from the referee's comments, and my response follows.

Let's start by addressing the Major Comments first. Both reviewers had similar major comments:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

[R1] "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 = aDˆb (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. "

[R2] "Area for further discussion: How do these snowflakes compare to the traditional mass-dimension relationship of m= a Db? It is stated in the paper (Fig. 5 is what I am thinking of specifically) that an increase of 50 times the effective radius leads to a mass increase of 125,000. This, however, is effective radius and not the maximum dimension as I believe this is what the mass-dimension relationship above uses. Has the value of b in these snowflakes been examined? If so, what is it and how does it fit into previous field study relationships? I believe this discussion would strengthen the article."

We agree that the effect of not explicitly considering the mass-dimension relationship resulted in an unintended mass-dimension relationship when scaling particles equally in all directions. For compact / nearly spherical particles, this would result in an exponent of roughly 3, as the referee pointed out. When I started this research, my intention was to make a comparison between the mass of individual particles and the associated scattering and extinction properties throughout the onset of melting. For individual particles, the only way that makes sense to do this is to scale the mass while preserving the shape, so that you can eliminate the effect of variations in shape from a change in mass. The only way to preserve overall shape is to scale all dimensions equally. The referee is correct in that it may not result in PSD-integrated simulations that are consistent with observations of brightness temperatures or radar reflectivities originating from actual melting snowflakes, but it does allow a researcher to explicitly compare the single particle scattering properties (figures 3 through 10).

We also agree that in order to fix this, it would require months of additional calculations.

A detailed comment in the manuscript has been inserted to reflect this comment as it applies to calculations using the PSD-integrated quantities (Figures 11 through 15). Also added a reference to Leinonen, J., & Moisseev, D. (2015). What do tripleâĂŘfrequency radar signatures reveal about aggregate snowflakes?. Journal of Geophysical Research: Atmospheres, 120(1), 229-239.

Minor Comments:

[Note: any referee comments that have to do with simple grammar, punctuation, word choice, etc. have been corrected without additional comment here, unless there's a disagreement.]

[R1 and R2] "Line 7 - 9: “The onset of melting is generally believed: : :”.This sentence does make logical sense, but citation of literature here would make the argument stronger."

Added 2 references: Willis, P. T., & Heymsfield, A. J. (1989). Structure of the melting layer in mesoscale convective system stratiform precipitation. Journal of the Atmospheric Sciences, 46(13), 2008-2025. They discuss the onset of melting and the coincidence with the brightband; and most directly responsive is the article by Bohren, C. F., & Battan, L. J. (1982). Radar backscattering of microwaves by spongy ice spheres. Journal of the Atmospheric Sciences, 39(11), 2623-2628. The authors made direct measurements and simulation of a normalized radar backscattering cross-section for spongy ice spheres with various liquid water contents. Most notable is that they show a a rapid change in the backscattering cross-section between 0 and about 7 percent liquid water volume fraction.

[R1]: "Page 5624: Lines 10-13: Why does the scattering contribution decrease with melting?" and "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)."

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

I think it's completely consistent with the dark band observations or "step" observations seen in W-band reflectivites. In fact, if W-band back-scattering behaved similarly to Ku and Ka (and S and X, etc.), then you would have expect to see a similar style of bright-band response as is observed at those channels. However, it is a bit beyond the scope of this paper to speculate on the dark band (or even bright-bands) beyond some speculative / hand-waving arguments.

[R2]: "Page 5624 Lines 17 – 19: I think this is an interesting "accidental" finding. I would like to see more on this topic in the future."

[Specifically this is referring to the notion that at 94 GHz, we may be seeing the influence of finer-scale structure on the extinction properties of the needle aggregate.] Yes, it's a very interesting topic, I'd be happy to collaborate with anyone willing to undertake such a project, since I won't be getting around to it within the next few years.

[R1 and R2]: "Page 5627: Line 11: Kw = (mw2-1)/(mw2+1) should be Kw = (mw2-1)/(mw2+2).and Kw = 0.93 is typically used for water at 263K for low frequencies, but might not apply to higher frequencies. For example, Liao et al., 2010 (doi:10.1109/TGRS.2008.916079) states Kw = 0.93 typically applies to frequencies lower than 10 GHz and they use a value of 0.698 for 94 GHz (other literature is somewhat different). "

I fixed the typo (fortunately my code is correct): I've modified the text to reflect my intent, but I'll agree and have noted that using a constant value of 0.93 is a "defect". My intent was to control as many variables as possible, but still produce something that looks like radar reflectivity, with the goal being to show that any differences in computed quantities could be directly ascribed to changes in the physical property of interest (frequency, D0, melt fraction), rather than being conflated with a change in dielectric constant. In fact, the whole k-factor would need to be integrated itself over the size distribution, since melting fraction (and consequently the effective dielectric constant of that set of particles) would be changing with changing mass and melt frac-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

tion of individual particles... It's an excellent point that probably never gets properly considered, thank you.

[R1]: "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."

Yes, we made this unrealistic assumption again, for computational convenience. I've added a caveat in the text to reflect this. The logic behind this choice was to sacrifice some realism for the ability to explore melt fraction vs. reflectivity (e.g., figure 11), otherwise the melt-fraction with either need to be for D0 only, or somehow otherwise averaged across all sizes. An alternative way of looking at this dilemma is "what's the melt fraction of an ensemble of melting snowflakes of various sizes and masses?" There may be smarter ways to do this, open to discussion.

[R1]: "Page 5628: Line 1: The IWC and the reflectivity depend on both N0 and D0. What value of N0 was used? I could not find it in the paper."

The relationship we used was from Sekhon and Srivastava, 1970. N0 and D0 both change, depending on the precipitation rate. So there's no fixed value of N0. I added the following relationships to the text, but there simply wasn't enough room to explore the change in N0 and do it justice. D0 = 0.14 Rˆ{0.45} [cm], and N0 = 2.5x10ˆ3 Rˆ{-0.94} [mmˆ{-1} mˆ{-3}] when R is given in mm hrˆ{-1}, liquid equivalent.

[R1]: "Page 5628: Line 6: D0 = 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."

We agree that this is a defect (a silly oversight on my part). I've added comments to the text and figure captions to reflect the fact that the truncation error may be severe enough to impact these calculations. However, an exploration of the truncation effect would be too time consuming to explore here. Unfortunately, our computational resources do not allow for computations of melting particles above 2500 microns effective diameter, so an evaluation of the impact is limited. An alternative DDA method that uses sparse matrices might be a solution that could be explored in the future.

[R1]: "Page 5628: Line 13: Such heavy snowfall would almost certainly result in aggregates much larger than 2500 um."

Yes. Addressed by the above comment. I've made modifications throughout the paper that reflect the probable truncation error here.

[R1]: "Page 5628: Lines 17-21: If you are simply scaling the snowflakes as mentioned in my major comment, you should get a D6 rather than D4 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."

Yes, in the Rayleigh regions, true – in the Mie regions it's more like Dˆ4. At Ku band, you're fairly well into the Rayleigh region for light snowfall, but as you progress into heavier snowfall, it transitions into a more Mie-like region. Spherical models do, indeed produce brightbands, but often for the wrong reasons, and the frequency dependence is wrong too. I'll state, without proof, that it's "impossible" to properly simulate the bright-band at Ku, Ka, and W simultaneously using low-density melting spheres that are not frequency tuned. You might get one right, but the others will be way off. This is all based on my experience with trying to simulate Wakasa Bay data, which had coincident Ku, Ka, and W band observations. No amount of fiddling with density, melt fraction, etc. could result in simultaneous matches of reflectivities for all three frequencies. Not to mention even if you get somewhat close, your TB calculations are way off because you're using spheres (the backscattering ratio to the single scattering albedo is not realistic). I can't think of a good reference off hand that shows this. I recall that Leinonen and Kulie have shown some triple frequency comparisons in prior publications, but I cannot recall whether they were able to fit to real observations using the same physical properties. Open to discussion on this point as well.

[R1]: "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?"

That’s sort of the point. I wanted to show that the shapes aren’t changing in a significant way over the onset of melting (the way I’ve modeled it, anyway). I might have to make that an "ancillary material" item, if that’s possible at AMT. I can provide the shape files and some high resolution images (these were rendered using POVRay).

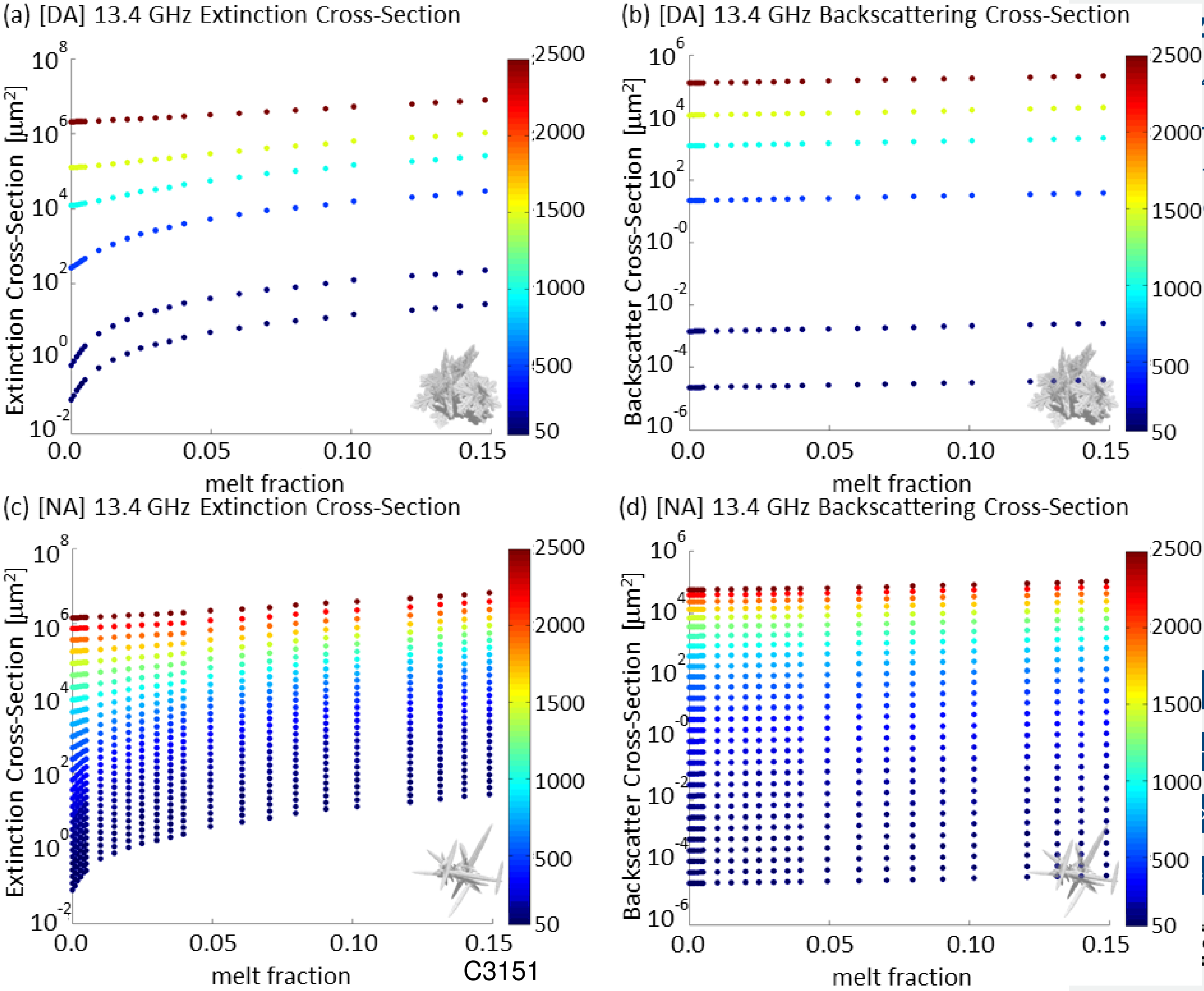
[R1]: "Figure 3: In sub-figures c and d, the colors of the bottom 15 or so points are practically indistinguishable. I suggest tuning the color scheme to improve the readability."

The default Matlab colorbar strikes again. I lost access to the scripts that I used to generate this figure after I switched jobs, so I used GIMP to change the lowest range of blues to a purple color. This provides some additional contrast. (See attached image).

[R1]: "Figure 15: I wonder if the implicit PSD truncation (see my comment for page 5628, Line 6) at high D0 affects the results shown in this figure. It certainly seems that the TB saturates at high D0 values; this may be because the PSD is not changing much in practice because of the truncation."

Yes, the previous comments address this issue. I simply went too far with my range of D0 values without considering the truncation effect.

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



Screen / Esc
-friendly Version
tive Discussion
ussion Paper