Atmos. Meas. Tech. Discuss., 7, C267–C269, 2014
www.atmos-meas-tech-discuss.net/7/C267/2014/

Atmospheric Measurement Techniques
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Discussions

Interactive comment on "Improved scattering radiative transfer for frozen hydrometeors at microwave frequencies" *by* A. J. Geer and F. Baordo

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Received and published: 25 March 2014

The referee's comments are included in italics.

1. I noted that the sector snowflake, your model of choice, is the extreme (or near extreme) case in many of the plots in Figs. 2-3 (e.g. it has the lowest asymmetry parameter at all frequencies). Thus I wonder if your search for the optimal model is constrained by the available models, and that one would ideally use something outside the search range.

The sector snowflake is near the extreme end of the available models, but I don't think

the search range needs to be extended in the direction of lower scattering. At higher frequencies the dendrite, not the sector, is the least scattering shape. This can be seen in Fig. 7b, where negative skewness indicates insufficient scattering compared to observations. At 92 GHz and above, the dendrite snowflake gives higher negative skewness than the sector snowflake, but the sector agrees better with observations. Only around 50 GHz is the sector snowflake the least scattering shape. However, both the sector snowflake and the dendrite provide quite good results at 50 GHz, probably even scattering too little, as indicated in Fig. 5. Figure 3 is consitent with all of this: only at 50 GHz does the sector snowflake have lower SSA and extinction than the dendrite snowflake. Hence in this sense, the search was not constrained.

However, it is certainly true in a wider sense that the search was constrained by the available models. For example there is a big gap between the columns and plates (broadly high SSA and extinction) and the rosettes and snowflakes (broadly lower SSA and extinction). Had the optimal particle fallen into that gap we would have had to blend the properties of two shapes. Further, as you point out, there are other shapes available, like aggregates, that could provide further options for improving the scattering.

2. Your proposed method for histogram comparison bears close resemblance to the concept of Kullback-Leibler divergence, so I would be hesitant to call it a new statistical measure.

As far as I can see, the Kullback-Leibler (K-L) divergence is $\sum_i p_i log\frac{p_i}{q_i}$ whereas the proposed statistic is essentially $\sum_i log|\frac{p_i}{q_i}|$. Hence, because of the pre-multiplied p_i, the K-L divergence would not give so much weight to the bins we are most interested in, i.e. those with small populations, which is the problem with other typical statistical tests I looked at. However, it is a fair point that the 'new' statistic is unlikely to be completely novel and further it lacks the robust theoretical underpinnings of something like the K-L divergence, so it is better not to draw attention to it as a 'new statistic'. It is just a tool that seems to work for the current problem.

3. On the subject of the mass-size relations (equation 6): firstly, the exponent b need not necessarily be 3 for spheres if one uses a size-dependent density, as is quite commonly done (e.g. the snowflake model of Matrosov (2007)). Secondly, the b for your crystals (around 1.5) seems to be unrealistic for larger snowflakes, where aggregation is the dominant growth mechanism and both theory experiments indicate that b should be roughly 2 (e.g. Mitchell (1996), Westbrook (2004)) and can be even higher for rimed snow. One could speculate that neglecting aggregation is one reason why your approach performs worse for large snow - especially as you do not consider aggregates as potential models.

The first point could be included in any revised manuscript. The second point, on aggregates, I would hope to leave for future work, but it is again a point worth making in any revised manuscript.

4. Concerning the consistency of Mie spheres: there have also recently been attempts to fix the inconsistency by using spheroidal models for snowflakes. However, the results of Leinonen et al. (2012) indicate that spheroids cannot be made consistent with physical shapes at different frequencies, either (at least in the backscattering direction).

I had not looked at this work before, but a mention of non-spheroidal models (and the fact they do not much improve on the Mie sphere) would be well worth including in the introduction to any revised manuscript.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 1749, 2014.