

Interactive comment on “On the microwave optical properties of randomly oriented ice hydrometeors” by P. Eriksson et al.

A.J. Geer (Referee)

alan.geer@ecmwf.int

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This is a very interesting study comparing different models for the optical properties of frozen hydrometeors. One highlight for me was the demonstration of two different ways of comparing single-particle optical properties: as a function of mass, or as a function of the maximum dimension. I have been convinced by this study that mass makes more sense in many ways, not just for comparison purposes but also potentially for integrating bulk optical properties across size distributions. Another highlight was the argument in support of the Liu (2008) sector-snowflake as a representative particle shape, though as explained below it would be nice to make the argument more quantitative. I do not think it would not be difficult to do this. Overall, this work will be very

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useful many people trying to understand how best to model the effects of cloud ice and snow. It has certainly given me a lot to think about for the future.

All references can be found in the bibliography of the paper under review.

General points

1. Approximation by a single proxy shape: The approach of finding a proxy particle with "average" single-scattering properties looks to be a good alternative to what we did in Geer and Baordo (2014) which is to try to find a shape with the best bulk scattering properties (which of course included assumptions on the particle size distribution and the correctness of the ECMWF forecasts). The new approach uses an ensemble of physically-plausible discrete dipole (DDA) shapes as the reference. There are a few comments around this:

a) Why not use the the ensemble properties themselves, rather than trying to find one proxy particle? For example, Kulie et al. (2010) included an ensemble-based shape in their experiments. In our study I did initially include an ensemble shape but it gave far too much scattering, being driven by the many solid, high-density particles in the Liu database (i.e. the plates and columns) so it was left out of the final paper for clarity. We could have tuned the weights of the ensemble members (in theory, if not so easily in practice) but it was much simpler to select a single proxy shape out of the Liu (2008) database.

b) There is only a visual comparison between the proxy shape and the reference shapes, and a quantitative comparison would be much more robust and would offer the reader a better summary of the results. It would be really good to find a metric to measure the discrepancy between the proxy shape and the reference - one possibility is something like the norm of log ratios we used in Geer and Baordo (2014). A perfect summary of the new results would be a figure with frequency on the x axis and the error metric on the y-axis (or rather four plots, measuring error in absorption, scattering, backscatter and asymmetry separately). The plot could show some of the soft particle

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approximation (SPA) shapes and also the possible DDA proxies. Obviously the exact choice of how to derive this metric can be debated (i.e. for which size parameters do we think it is most important to get a good fit?). However, an error metric based on linear x-bins in size parameter (mass based), logarithmic errors in absorption, scattering and backscatter, and linear errors in asymmetry would exactly represent of the visual comparisons this study is already making when looking at plots like Fig. 3.

c) The study also presents radiative transfer simulations as an alternative way of measuring the discrepancy between the reference and the proxy particles. These are definitely very helpful and they help provide the broad summary of the results that I am asking for in the previous comment. However, I do wonder if the choice of a relatively thin, high ice-cloud is representative enough of all situations (indeed, the approach is intended to avoid multiple scattering). The error metric outlined above could give an alternative viewpoint and help support Fig. 9.

d) It would have been nice to see consideration of lower microwave frequencies. The biggest problem we were trying to solve in Geer and Baordo (2014) was the enormous excess of scattering generated by our soft particles at around 30-50 GHz. It would be great to see at least one plot panel somewhere covering those kind of frequencies. Ideally we want to be able to use a proxy particle to represent snow and ice all the way down to 6 GHz for microwave imagers and all the way up to perhaps 1 THz for future sub-millimetre applications, so it is important that the scattering properties are realistic across this whole range.

2. Dielectric mixing rules: It is good that different mixing approaches have been explored to give the SPA the best chance of success. I also fully support the intention that the paper should not be distracted too much by mixing rules that hopefully will become increasingly irrelevant for practical applications (section 2.2, last paragraph). However, when first reading the paper I was wondering why the air fractions chosen in the various early examples were often around 0.4-0.6, which is a zone that emphasises the differences between mixing rules. With air fractions closer to 0 or 1, the spread is

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much smaller. The choice is justified later in the paper but it would be good to briefly explain these choices where the SPA is first used. As mentioned on P12898 L25, Geer and Baordo (2014) used air fractions of approximately 0.9. This was a physical-based approach (thought it is true, not directly based on observations) intended to make the soft sphere represent large particles like snow-flakes and aggregates, which are very low density if modelled as a sphere with the same maximum dimension as the particle. The text refers to 0.9 as a "high" value and it would be good to see some clarification of that. For example, in section 5.3.2, such a value appears to give good results at 90 GHz. I think it is probably just a difference in philosophy: in the paper under review, air fraction is acknowledged to be a tuning parameter, rather than a physically-based quantity.

This point of philosophy may also resolve the possible discrepancy (mentioned on P12898 L22) between our conclusions blaming Mie theory for strong forward scattering and the new conclusions suggesting that our air fraction was too high. It is fair to criticise our conclusion in Geer and Baordo (2014) and having read the new work I agree it would be better to attribute that strong forward scattering to the use of a low-density the Mie soft sphere, rather than to Mie theory in general. However, the low density soft sphere comes out of our attempted physical approach to assigning density, as opposed to using it as a tuning factor.

3. The importance of bulk scattering properties: this is very important to the discussion at the end of section 6 (comparing to the Geer and Baordo results) and is a major part of section 7. On P12898 L19, it is remarked that the sector snowflake in our bulk scattering results has a surprisingly low asymmetry compared to the single-scattering results presented in the study under review. I also find this surprising and if I have time in the future I would like to check it more carefully. However, one possibility is that our size distribution is emphasising the low size parameters or the very high size parameters, where the sector snowflake does have the lowest asymmetry. This leads into the really important difference between the new approach and the Geer and Baordo approach.

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The choice of particle is very important for bulk optical properties not just because it controls the single scattering properties but also because it drives the size distribution, thus preferentially selecting scattering properties from different size ranges. Figure 12 of the paper under review illustrates this nicely. My main comment here is that I found section 7 a bit hard to follow and it could perhaps do with a little extra clarification and justification:

a) I wasn't quite sure how the a and b coefficients were being used in the field (2007) size distribution because of potentially confusing statements on P12900 - P12901, e.g. "we applied fixed a and b values for all particles" followed by "the following calculation steps (including rescaling of the PSD) used the actual particle masses from the DDA database".

b) I am unsure of the conclusion (P12902 L17) that the different spread in top and bottom panels of figure 12 comes from the scattering cross-section being more closely linked to effective diameter than the maximum dimension. This may be true but I would still not find this the clearest explanation of the results. To me Fig. 12 says that if you use the MH97 distribution for all particles, then you get less spread between bulk scattering properties. If you allow the particle size distribution to be driven by the choice of shape, then you can get much greater differences in bulk scattering because, with different particles, the mass is placed in very different effective size bins. This is a very interesting part of the paper but I am still trying to fully digest it and really understand it.

c) I suspect the advantage of maximum dimension as the basis for an integration from single to bulk scattering properties (not in the results of this paper, but more generally) is more subtle and likely to be that, as it makes for a more compact distribution of scattering properties, there may be fewer numerical integration issues. For example, it is important to have an appropriate choice of integration bin size, so that all the integration doesn't disappear into one bin or even outside the integration range for certain combinations of water path and shape. Using maximum dimension may help

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here.

4. In the conclusions: "If new databases are created, the limitations of present databases in temperature, particle size, and frequencies should be avoided". This is a really important point and I think it could be taken further. It would be good to come up with some really specific recommendations (in this paper, or in some appropriate venue) that could be followed by people generating new DDA databases. My wish list might include:

- a) Frequencies from 1 GHz to 1 THz to cover all known and future sensors.
- b) Scattering properties should be generated over the full size range for all particles (regardless of physical plausibility).
- c) It is important to include aggregates alongside traditional "solid" shapes. I also wonder about including graupel and hail too.

The choice of oriented or random particles, and how to design size bins (mass or maximum dimension?) are also important questions for future research. It is perhaps computer power and the available research effort that imposes limitations on these databases.

5. Just a point for discussion, but I wonder why there is any continuing support for the solid sphere or SPA in our community. Thinking pragmatically, current and future DDA databases are publicly available and easy enough (even easier) to use than the Mie approach. The main justification for a Mie approach is perhaps this: we worry that fixing on a particular DDA shape will be limiting: it will generate optical properties that are valid only in very specific situations, not generally. In contrast a soft sphere is supposed to be more representative of the ensemble of particle variability across the globe. I think that the new work is a particularly good illustration of why this philosophy is wrong.

Minor points

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1 - Introduction, P12875, L17: "main error sources are associated with the microphysical state of the particles" - this is certainly true in some regimes but not, for example, for cloud liquid water absorption. The text could be modified a little to avoid implying this, or to give examples of the regimes where these errors are most significant.

2 - Introduction, P12876, L10 onwards: Two paragraphs describe the current state of ice modelling in typical applications. It would be nice, though far from essential, to include some additional current projects in the discussion. I am thinking particularly of the GPROF retrievals for the GPM mission, or similar high-profile snow and ice cloud retrievals. One interesting poster by Gail Skofronick-Jackson and coauthors is here, indicating the use of the Liu database for GPM:

http://www.isac.cnr.it/~ipwg/meetings/tsukuba-2014/posters/P1-5_Jackson.pdf

3 - Section 2.1. On first reading this section I was not convinced of the importance of the imaginary part of the refractive index. At the end of the section there is a strong recommendation against using Warren (1984). However, on first reading, and given the evidence presented to that point, I did not find that particularly convincing: yes, the imaginary part varies, but how does that affect absorption or brightness temperature? Actually figure 3 is extremely convincing on this point (the high absorption generated by Hong (2009) particles, which are based on the deprecated refractive index model.) It may be worth supporting this recommendation with a preview of the results in later sections.

4 - Section 3.1, P12885, L4: Some Liu (2008) shapes are ignored. The text could explain the basis for selecting shapes and assure the reader that the remaining shapes are representative of the full range of optical properties available in the database.

5 - Section 3.3. does not introduce beta, theta and phi

6 - P12886, L16: Figure 4 is introduced with "All three aggregate types in the Nowell database are plotted with the same symbol". One Hong shape shares a star as well.

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On checking very closely, and in the right light, I can see one is blue and one is green, but initially I thought the colours were the same and this caused more than a little confusion. Is it possible to separate these colours a little more?

7 - P12887, L8: "This can be discerned in Fig. 3".

8 - P12889, L13-14 and Fig. 5: A number of extra details of the satellite simulations need describing here, or at least there should be a reference to a description somewhere in the paper: a) the model that's been used; b) whether the surface is visible (in which case, where this is land or sea, and how the surface emissivity is computed). c) How the "cloud induced change" is defined. I am expecting that positive on the y axis corresponds to a decrease in brightness temperature compared to the clear-sky case, but this needs to be defined; d) Why the air fraction of 0.4 was chosen.

9 - Figure 6: Given that all the axis labels are the same, this figure really needs some additional labelling of the subplots. Ideally all subplots in the manuscript need some kind of label (a letter or a more complete title) but here it is vital.

10 - P12890, L9 "There is some uncertainty regarding the importance of absorption for passive measurements". This discussion might need a bit more motivation as it confused me. It seems very obvious to me that absorption is important. When and why would anybody not bother to model absorption? Or is the question more about the scientific interpretation of brightness temperature changes in terms of scattering or absorption? Is it possible to give references that back up the assertion of "uncertainty"?

11 - P12892, L10 - conclusion that Maxwell-Garnett air in ice is the best mixing rule. The evidence seems to be based on comparisons at 183 GHz. Is this statement justified across the frequency range?

12 - P12895, L27 - what is "DOIT module"?

13 - P12896, L3 - "to just keep the most realistic particles". Given the particle selection is a judgement call, and not driven directly by observations (obviously, very hard to do

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in any global sense) I might have put "to remove the less physically plausible particles". Just a matter of taste - up to you.

14 - P12898, L12 - "This particle type is throughout below the fitting line at 874 GHz" For some reason I was confused by the text at this point and at first I thought this was referring to the Liu sector. After re-reading it is clear you mean the Hong particle, as the Liu database does not go up to 874 GHz. However, the text could perhaps be reworded to more clearly signal that it is now considering the qualities of the Hong particles (perhaps by starting a new paragraph?)

Typos and grammar

P12889, L2: "where not Rayleigh conditions apply" -> "where Rayleigh conditions do not apply"

P12889, L20: radiance -> brightness temperature

P12890, L2: "changing g with" -> "changing g by"?

P12890, L21: "consequence of that the" -> "consequence of the behaviour that"?

P12892, L27: "to prefer" -> "preferable"

P12893, L7: "on the same time" -> "at the same time"

P12903, L22: "Novel" or Nowell?

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 12873, 2014.

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