Dear Alan Geer,

Your comments are repeated here (grey italic text), followed by our response (black standard font).

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 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.

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It was very nice to find out that you were one of the referees, as parts of our work clearly are inspired by your recent article. For the same reason, we are especially happy to see that you find our results interesting and useful. We also take the opportunity to thank warmly for the detailed review and the useful feedback.

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.

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To be honest, we did not consider this option when writing the manuscript.

Both for this point and point b below, a critical aspect is the relative relevance of the particles selected by Liu and Hong. Is the “database mean” representative for the mean atmospheric habit mix? Probably not. It seems that we both suspect that by averaging the database properties, too much weight would be put on solid particles (such as plates and columns), at least for somewhat larger particles. Your comment above indicates this, and in Sec 5.3.3. we did sort out such particles with Dmax > 1 mm. That is, some kind of weighting is required before averaging the DDA properties, but our feeling is that we still know far too little about true habit mixes to make a correct weighting. For this reason it feels safer to leave this for the future. Accordingly, to progress, we need much better knowledge on true habit mixes, for all relevant conditions, ranging from tropical deep convection to different forms of high-latitude snowfall.

Another aspect is that, even though our study builds upon your recent article, we have probably very different motivations. Our goal is to improve the basic knowledge, to set the stage for future studies using DDA data, while you have a much more practical and urgent goal, to improve the global forecasts of ECMWF. We were pleased to find that there exists a DDA shape that shows intermediate properties over a large range of frequencies and size parameters. This gives a clear and simple alternative to SPA, and this finding was enough for us inside this study.

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 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.

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We did consider something in this direction. Of course, to go beyond a qualitative discussion would be to prefer, but we decided to avoid defining a quantitative measure, basically for the same reasons as above.

Our approach avoids assumptions on true ice masses and particle size distributions, but on the other hand the target for a goodness measure is less well defined. In fact, we have no clear reference to use for defining a quantitative measure. As argued above, to use the mean of the DDA databases as a reference could in fact be misleading and assuming some other exact weighting between the particle shapes feels premature. Here the advantage of your approach emerges, as you are using real observations, there is a clear target to match. These aspects should hopefully be clear by comments in our manuscript. For example, in Sec 6 (Summary and conclusion) we wrote:

“The critical part in our approach is the judgement how representative the different DDA particles are with respect to the mean conditions in the atmosphere. Due to the lack of reference data, we selected to not push the analysis too far at this point and discussed only in general terms which particle shapes show overall average properties. It is of course possible to use the same methodology to, e.g., select a representative shape separately for ``cloud ice'' and ``snow'', or targeting different cloud types.”

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.

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First of all, we wanted to have as similar set-ups for Fig. 5 and 9 as possible, to keep the associated text parts as short as possible. We think that our choice makes good sense for Fig. 5, the interpretation of these tests would be much more complicated if the cloud was

broken and/or embedded in gas absorption. With respect to Fig. 9, yes, this is merely an example. But an important one, at least for us with an interest in observations at frequencies above 200 GHz (ICI and ISMAR). Here it can be added that for more complicated cases (i.e. including horizontal structures) there are additional possible complications, such as that there could be a relationship between "beam filling" and the asymmetry parameter.
In any case, our ambition was just to demonstrate that the patterns seen in the plots of scattering efficiency largely remains after doing radiative transfer calculations, as this is maybe not totally clear for all readers.

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.

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We agree fully with the ambition to aim for finding optical properties that are consistent over the full range of relevant microwave frequencies. As mentioned in our Introduction, we selected to put focus on the higher end of this frequency range, as this part is less studied and is so far our main interest. But we are glad that there is an interest in our results also for lower frequencies. As a solution, we have now prepared a supplement. The main content of the supplement is Fig. 7 repeated for six additional frequencies (10, 36, 50, 166, 340 and 664 GHz, to complement 90 and 874 GHz already in Fig. 7 of the main text). We selected to duplicate Fig. 7 as we think it is given the most detailed information (for passive observations).

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 much smaller. The choice is justified later in the paper but it would be good to breifly 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.

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Good feedback. Yes, we are here treating air fraction as a pure tuning parameter, while it also can be seen as a physical quantity, and this can cause some confusion. We have now added some comments in start of Sec. 5.3 and in Sec. 6, to hopefully avoid misunderstandings.

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. 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".

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This text has now been rewritten and somewhat restructured, for better clarity.

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.

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We spent considerable time discussing exactly this question, and this is a reason to why we included results for F07 with fixed a and b (dashed lines in Figs, 12 and 13). Our point here is that a high spread is obtained also when the dmax-based PSD is not driven by shape.
In any case, the statement "the scattering cross-section is more closely linked to d_e than to d_max" is based on comparing Figs. 3 and 10. The reference to these figures was not clear before, and a comment has now been added. To be clear, our relatively lengthy discussion around Fig. 12 aims at showing that we are using comparable PSDs, that we have not selected a case that is favouring the de-based PSD. The new comment should hopefully remind the reader about the conclusion from comparing Figs. 3 and 10.

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 here.

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Yes, we agree. There are additional considerations, such as this one. We also remind about the last comment we make in Sec. 7, we need to obtain better information on the variability of PSDs (not just mean ones). Is a de- or dmax-based showing the smallest day-to-day variability?

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.

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We did consider adding recommendations on a future DDA database, but in the end decided to not do so, the main points should be clear from the results. We agree fully with the three points listed, and more can be added. For example, the present limitation of complete random orientation must be avoided. Horizontally aligned particles should be covered, but it should also be possible to derive results for arbitrary orientation distributions (probably with the constraint of no preferred azimuthal orientation). Anyhow, most important is to create a database that easily can be extended, such as allowing to add lower temperatures, if later found needed. That is, a complete discussion would be quite lengthy.

It can here be mentioned that we ourselves are planning to present a new DDA database, that should meet the demands mentioned above. The first practical steps towards the database have just been taken.

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.*

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This sounds as reasonable analysis of why SPA is still being used. But it should be remembered that a clear demonstration of that a single DDA shape gives equal or better general performance has been lacking. The first clear demonstration was provided by you and Bardo, just recently! For this reason we are very happy if you think that our study provides additional support for focusing on advancing the usage of DDA data.

Minor points

C45861 - 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.

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Yes, poor logic. Two comments added to make clear that this paragraph deals with ice particle properties.

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

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We find the introduction already quite long and a complete review of “ice modelling” is out of scope. We have tried to focus on studies around SPA and attempts to test the realism of different particle types from the Hong and Liu databases (i.e. particularly your own study and Kulie et al [2010]). However, the series of papers by Gail Skofronick-Jackson and co-authors is related and clearly important work, but we forgot that Liu was used in these papers. We have now added a short reference to a recent paper by those authors, to indicate the importance of their work. Thanks for the hint about the poster.

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.

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Commented added, making clear that the corresponding impact on particle absorption is exemplified in Sec 3.4.

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.

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We have now double-checked that the particle types included cover the full range of the databases, and added a sentence in the text to clarify this. We could not include all particle types as that would strongly clutter the figures. There is no objective reason for the selection of particle types to include, beside the fact that they cover the range as now stated.

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

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The sentence removed, as anyhow just distracting information.

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. 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?

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Good to get feedback on these issues, a good reminder that the colour perception differs between persons. The Nowell data are now throughout plotted in magenta, and have an unique symbol (pentagram).

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

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We interpret this as a comment to point 2 above.

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 faction of 0.4 was chosen.

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More details are now given. To match this, a slight change in Sec 5.3.3 was required. There is no clear motivation for the exact value of 0.4, but it is now motivated why solid particles not can be used.

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.

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Yes correct, Figure 6 clearly lacked labelling. Now included.

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"?

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That sentence was mainly added to indicate the switch of topic, from g to absorption. It is probably possible to find comments in the literature indicating an underestimation of the role of absorption (we have at least obtained such comments as part of the review of earlier manuscripts), but that issue is not in any way critical to motivate this part of the figure.

Accordingly, the sentence of concern was rewritten to avoid an unnecessary discussion.

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?
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Yes, which we had investigated, but missed to make clear in the text. Now fixed.

12 - P12895, L27 - what is "DOIT module"?
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Fixed as part of changes around point 8.

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 in any global sense) I might have put "to remove the less physically plausible particles". Just a mater of taste - up to you.
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A good advice, suggestion followed.

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?)
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A small comment is added to be more clear that it is the Hong particle that is discussed.

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?
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All changed as suggested.

Kind regards,

Patrick Eriksson and co-authors