Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2018-73-AC1, 2018

Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of snowflake microphysical properties from multi-frequency radar observations" *by* Jussi Leinonen et al.

Jussi Leinonen et al.

jussi.s.leinonen@jpl.nasa.gov

Received and published: 16 July 2018

Dear Reviewer,

Thank you for your comments on the manuscript. Please find our responses to the comments below each question below. The original questions are included in *italics* for clarity. Except where mentioned otherwise, the descriptions of changes refer to the revised version, which will be submitted along with the final response.

0) The returned manuscript contains mark-up throughout the article. While some (much?) of it may not be easily readable, it may yet help to improve details here and there.

While some of the comments were indeed a little hard to read, we went through all

the notes and made modifications accordingly, especially for the helpful grammatical corrections that the reviewer suggested. Most of the more substantial comments in the returned manuscript seem to correspond to the reviewer comments below, so we address them under each comment.

1) Page 2, Lines 13-14: Do two-frequency measurements constrain snowflake sizes when multiple populations of ice hydrometeors are present and one species dominates the reflectivity? Recent polarimetric results show that it is not uncommon for a large ice hydrometeor species to dominate reflectivity while a second smaller ice hydrometeor species dominates KDP. So the word "constrained" in these two lines is a weak constraint.

The reviewer is correct that having multiple hydrometeor species present would complicate the interpretation of the dual-frequency signal. But even then, a high dual-frequency ratio would indicate the presence of large snowflakes (because snowflakes much larger than the wavelength are required for such a signal to occur), even if they do not dominate the ice content. In this sense, we believe "constrain" is an appropriate term.

2) Page 4, Line 5: What does "cumulative" mean in "cumulative attenuation"? It was not clear what population of samples contributed to the R-M-S calculation mentioned on this line.

This is the total attenuation (dB), as opposed to the specific attenuation (dB per unit distance). All bins in the case of Sect. 5.2. were used to calculate the RMS value. We changed "cumulative" to "total" and specified which bins were used for the RMS.

3) Page 5, Line 17: The comment "normalized cross sections are constant in the small-particle limit" is not strictly true. They can vary by about a factor of two depending upon the particle shape, even though the particles are small compared to the wavelength.

We do not see variations as large as a factor of two in this region within our scattering dataset. Regardless, this feature of the normalization is not critical for the functioning of the algorithm so we have just changed this sentence to say “roughly” constant.

4) Page 5, Lines 17-18: "The samples used for the averaging are weighted using a Gaussian function of the distance from the bin center". Are the samples used in the averaging all associated with a single bin? If so, why does averaging of the samples within a bin require a weighting function? Is it because the bin widths are large and what is wanted is some sort of average representative of the bin center? Finally, what is the width of the Gaussian function? More information needs to be provided about this step so that others can reproduce it based on what is written.

The need for the Gaussian weighting arises because we start with a dataset of snowflakes that are distributed somewhat randomly around the (D, m) space. When we try to grid these values, there are inevitably some bins in which there are, by random chance, only a few snowflakes, causing sampling noise. Using a Gaussian weighting that also utilizes points outside the bin (but weighted lower than those inside the bin) helps smooth out this noise. This leads to more stable calculations of reflectivity.

There is now a bit more explanation on this at this point in the paper.

5) Page 5, Line 23: "thus should" is weak. It either does or does not. Which is it?

Answering this question definitively would likely take an entire new study. Since the part of this sentence beginning with “thus should” is not critical to this paragraph, we have removed it in order to reduce speculation.

6) Page 5, Line 28: How many "logarithmically spaced integration points" do you use?

1024 points. This is now stated here.

7) Page 6, Line 9: "we appeared to us" -> "we found"

Changed as suggested.

8) Page 6, Lines 28-33: Is the discussion here about errors overly optimistic? Assum-

ing a mass-dimension relationship can lead to errors. In the case of this study do the two databases it uses have mass-dimension relationships built into them. If they do, what is the consequence of this in terms of retrieval results and errors based on a retrieval tht uses the same/different mass-dimension relationships than those used in the databases? Why would you expect "their effect should be similar on each collocated radar frequency" to be true? Nothing much been mentioned about particle orientation in the manuscript but we generally do not know much about it and the effect of having orientations wrong would not be expected to lead to similar errors at different frequencies. In summary, perhaps the structural errors in the retrieval need to be made perfectly clear. Because they are really difficult to deal with and no ideas seem to be out there to deal with them using available datasets, simply making them clear has to be sufficient at this point in time.

As explained in Sect. 2.2, the databases do not prescribe a specific mass-dimension relationship. Rather, they cover a two-dimensional area of the (D, m) space. Thus we can freely choose the mass-dimensional relationship; this is the reason we can retrieve α rather than assuming it. Some error does result from our choice to fix β, but this is mentioned as an error source in this paragraph.

It is true that the assumption about orientation is another potential error source. We have added this to the second sentence of the paragraph.

The effects of error sources should be similar (even if not completely equal) to all reflectivities because the radar cross sections at different wavelengths respond in a similar way to the error sources. For example, if the particle size increases, the radar cross section will generally increase at all wavelengths. Thus the effect of the error source on the DWR will be smaller than on the individual reflectivities. This is now discussed in more detail in the paragraph.

9) Page 7, Lines 7-14: What is your method for choosing a retrieved state vector x based on measurement y? Is it the maximum in E[x|y] given y? Perhaps this implicit *but it needs to stated directly.*

E[x|y] is itself the retrieved state vector. It is the conditional expectation of x given y, and thus the average over all possible values of x that might occur for a given y. It is noteworthy that this retrieval approach is different from usual approaches that maximize the likelihood: Those methods find the *mode* of a conditional distribution, while ours finds the *mean*. This is worth pointing out and has been added at the end of this paragraph.

10) Page 7, Lines 18-20: 10000 points scattered across a three-dimensional space is not that dense of a set of points. Are you sure that your sampling is sufficient? Where do the means and standard deviations of x_i come from on Line 19? Are they from your a priori data or some other place? Not sure from what is written.

As we point out in the last sentence of this paragraph, we evaluated this by trying a finer discretization (and consequently a larger number of points) and did not see a meaningful change beyond the 10000 points.

The mean and standard deviation are those of p(x), the Bayesian prior. This is now mentioned here. Thanks for pointing out that this information was missing.

11) Page 8, Line 17: Some might argue that a Voronoi based density is a much more meaningful "representative" density than a superscribing sphere. What might be a better word than "representative" here?

The reviewer is right that “representative” may not be the best word here. We changed this to “bulk density for the snowflake ensemble”, which makes it clearer that this is a value that applies to the entire ensemble than single snowflakes. It is mentioned below the equation for ρ_{bulk} that more complicated definitions could be used and that the main motivation for this definition is its simplicity.

12) Page 9, Lines 26-27: Most aircraft in situ probes currently retrieve particle sizes based on a single two-dimensional image of them, which can lead to large biases

depending upon the particle shapes and orientations as they fly through the probe sample volume. I would image there are similar issues with the PIP measurements discussed on the next page.

Please refer to our response to your comment 18; we think this issue is better addressed in that context.

13) Page 10, Line 7: How was the value of the "linear scaling factor" set?

The linear scaling factor is derived as the best fit to PIP images. The factor is needed because PIP outputs the disk-equivalent diameter, i.e. the diameter of a circle with an area equal to the projected area of the particle. This is smaller than the maximum diameter. By analyzing PIP images for a given case, we can derive a conversion factor between the two diameters.

14) Page 10, Line 17: Is assuming that the "a priori distribution is multivariate normal" a good assumption? What is your justification for this assumption?

The amount of data in the a priori datasets is nowhere near enough to rigorously determine the correctness of this assumption. However, there are a few reasons to assume the Gaussian distribution:

1) Given no information otherwise, the Gaussian distribution is the most natural choice in the maximum-entropy sense, as pointed out by e.g. Jaynes (2003).

2) Microphysical quantities often have lognormal distributions globally, and hence their logarithms have Gaussian distributions (recall that our state vector uses logarithmic quantities).

3) An inspection of the a priori datasets revealed fairly symmetric distributions that seemed to be well approximated by Gaussians, although we did not rigorously validate this with statistical tests.

Thus the multivariate Gaussian distribution is a natural choice given the lack of evidence to the contrary. We moved this discussion to the last paragraph of this section and expanded it.

15) Page 11, Lines 22-24: "which identifies a change in the hydrometeor type at roughly the same altitude where a rapid in increase in D_m is observed". What altitude is this? I do not see it clearly comparing Fig. 2a with Fig. 2d. Fig. 2e I mostly contains blue with purple and green speckle towards the top in the orange box region and no single altitude with a change jumps out.

Thanks for pointing out that this wasn't clear in the text. This is supposed to refer to the transition just over 5 km altitude between roughly 20 and 45 km on the distance scale. Above this altitude, retrieved D_m is small and NPOL indicates a mixture of green (vertical ice) and blue (aggregates). Below that altitude, D_m increases substantially and NPOL begins to indicate mostly blue (aggregates). We have changed the text accordingly; this point was clarified and the discussion of this feature was split into its own paragraph.

16) Page 13, Fig. 2: The "NPOL" is hard to see in some of the figures. Perhaps arrows can be used to demarcate the location of the NPOL along the x-axis?

This is a good suggestion and makes the plot clearer. We changed Fig. 3 so that the location of NPOL is denoted by an arrow. This change is explained in the caption.

On the other hand, in Fig. 2 the "NPOL" text was included by mistake - as one can see from Fig. 1a, the measurement period used in this case does not actually reach NPOL. Hence, any indication of NPOL on Fig. 2 was removed.

17) The words "good", "well", "seems", "should", "can", "appear to", "somewhat" are not informative. Replace them with quantitative statements or rewrite the sentences containing them in a more definitive, precise way. See Page 15, Lines 4, 28, 29, 30, 31, 32 Page 17, Lines 3, 20, 21, 26, 29 Page 18, Lines 1, 4, 11 Page 19, Lines 3, 16, 17 Page 20, Lines 2, 30

(The page and line numbers below refer to the submitted version in order to make it clear which reviewer comment they correspond to.)

Page 15 line 4: the qualifiers were removed as they were unnecessary.

Page 15 lines 28-32 and page 17 line 3, we rewrote the sentences and added RMS error, bias and correlation values to quantify these statements.

Page 17 lines 20-21: This sentence was reworded to make clear that this is a visual comparison.

Page 17 line 26: Removed "appear to".

Page 17 line 29: Removed "somewhat".

Page 18 lines 1, 4, 11; Page 19 lines 16-17; Page 20 line 2: Unlike above, where numbers to back up the interpretation were missing (and were added in the revision) these qualitative statements are already quantified by the numbers given in Figs. 5 and 6. Therefore we believe that a discussion offering our qualitative interpretation of the quantitative data is appropriate here.

Page 19, Line 3: Similar to above, this statement is quantified by the plots found in the supplement.

Page 20 line 30: We are not sure what the issue here is exactly, but in any case we reworded the sentence here in response to comment 21.

18) Page 15, Line 14: Particle imagers might be able to measure accurately the maximum dimension of a two-dimensional projection of a particle but this does not mean that particle imagers retrieve the maximum dimensions of the particles accurately. All depends upon the particle shape and orientation.

We modified this sentence and the text around it to better acknowledge this source of error.

19) Page 16, Fig. 4: Squares would be better than rectangles in the subpanels.

The aspect ratios have been changed to square.

20) Page 17, Line 27: "The multi-frequency retrievals... permit considerably more variation". Is this an advantageous feature of these retrievals if there is no improvement in correlation or any other objective measure of skill?

The variability is evidence that the algorithm has confidence, in the probabilistic sense, to estimate α as something other than the prior value. Given the poor quality of the in situ estimates of α, we think this is worthwhile information in itself. We expanded this discussion a bit.

21) Page 20, Lines 27-29: The sentence contained in these lines is really problematic written in current tense. "Graupel occurs in relatively rare events" would mean that this is always true and everywhere. Do you mean this only for your datasets? If so, this would read much better as "Graupel occurred in relatively rare events". Most of the paper was written in present tense, including places describing things done, occurring, ..., in the past. The paper was sufficiently well-written that this use of present tense everywhere was not too distracting.

This was meant to say that graupel is a relatively rare occurrence *globally*, considering all cases of icy precipitation, and thus being able to detect graupel would not necessarily affect the statistics of retrieval accuracy that much. We reworded the sentences slightly to make this clearer.

22) Page 21, Line 18: "The high cross-correlations found in this study". What are correlations are being referred to here and where did they occur in the manuscript. Not clear.

Most noticeably, the correlations are fairly high in the prior correlation matrix of Eq. 17. We added a reference to that equation here.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2018-73, 2018.