Response to reviewer 3

We thank the referee for the time he/she has put on reading our manuscript and providing feedback.
Based on the combined comments of the referees, we have decided to implement these general changes:

- We will switch to an airborne measurement set-up and the introduction section will be modified accordingly
- The text in the result section will be shortened significantly
- Redundant results for scene 2 will be placed in an appendix
- The selection of tested retrieval habits will be revised/changed

Below we respond to the main questions raised by the referee, and outline how we will revise the manuscript.

Major comments

Reviewer comment 1

The paper is presented as an application for ICI in combination with a Cloudsat like configuration but it is not clear to me what geometry of observations the authors are thinking about. They state "As mentioned above, the same incidence angle as for the passive radiometers is assumed also for the radar. In practice, this could be achieved by remapping the radar observations to the lines of sights of the passive beam". Are they thinking about a scanning W-band radar? or at a off-nadir pointing radar? If the former is true then they should discuss what is a realistic technological solution (and what are the consequences in terms of sensitivity) and the authors should refer to state of the art scanning W-band radar concepts (there is none at the moment!); if the latter is true they should discuss what are the consequences of such a selection (e.g. forground clutter) and they need to convince me that what we could gain from such a configuration compensate from the loss of information introduced by pointing in such a slanted direction. There should be a certain degree of "realism" in what we are trying to simulate, especially if this was part of an ESA study.

Author response:

The reviewer raises a very relevant point with his comment. To address this, we changed our simulation setup to simulate perfectly co-located observations at nadir. Realistic modeling of a space-borne viewing geometry (at least in a variational retrieval) is currently not feasible due to the computational complexity. We still deem this sufficient for the scope of the study, i.e. studying the fundamental synergies between active and passive observations. In addition to this, we follow the recommendation made in the second comment and will pitch the application more towards air borne observations.

Reviewer comment 2

"the beams of all three sensors are modeled as perfectly coincident pencil beams". Again this is quite an assumption. Non uniform beam filling will play a key factor. This is one of the many simplifications (no polarization, no multiple scattering,1D, ...) that needs to be clearly listed at the beginning of Sect.2.2.1 (some appear only at page 27). For this reason I would actually pitch more towards an airborne configuration where these simplification indeed can be realistically assumed or of a radar with a radiometric mode (where you can actually match footprints). Otherwise the (not massive) gain of having a radar-radiometer combination that you show later on can be completely washed out by the errors introduced to these assumptions. I imagine that you may also have airborne data where to test how realistic your forward model is.

Author response

As mentioned above, we will follow the reviewer's suggestion to pitch the application of the combined retrievals more towards combined retrievals. We will also make these limitations more clear in Sect. 2.2.1 and discuss their implications more thoroughly.

Reviewer comment 3

Fig 2: these PSDs look very weird to me. Why do they have the plateau at small sizes? y-axis units are obviously wrong unless you are renormalizing by some mass (but it is not explained).

Author response

The reviewer is of course right, the units on the axis of the plots were indeed wrong and will be corrected in the revised manuscript. Otherwise, the PSDs correspond to the modified-gamma functions that are assumed in the Milbrandt and Yau (2005) microphysics scheme.

Reviewer comment 4

Fig 3: sorry I do not follow what is this (what is the y-axis?), and why this plot is meaningful.

Author response

We will remove this plot from the revised version of the manuscript.

Reviewer comment 5

Eq.6: Clearly with values lower than 230 K it does not make any sense (negative RH, or large than1.1???)

Author response

We would like to thank the author to point out this inconsistency, as there are indeed two mistakes in Eq. 6. The right equation should be

$$\phi(t) = \begin{cases} 0.7, & 270 \text{ K} < t \\ 0.7 + 0.01 \cdot (t - 270), & 220 < t \leq 270 \text{ K} \\ 0.2, & t < 220 \end{cases} . \tag{1}$$

This will of course be corrected in the updated version of the manuscript.

Reviewer comment 6

Line 210; this means that the vertical resolution changes with the surface temperature, really weird choice.

Author response

We agree with the reviewer that the chosen retrieval grids may not be optimal. We will change them to fixed-resolution grids for the revised manuscript.

Reviewer comment 7

fIG4 : not clear to me why the scattering depression is not increasing at higher frequencies. I would expect that the optical thickness would drastically increase increasing frequency. Is this due to very large asymmetry parameters then? But this is not what I do see in Fig.5 (though Fig4 is of course a very idealized case) If this is the case then results will be very dependent on particle habits (which may introduce additional uncertainties in the retrieval)

Author response

It is correct that extinction increases rapidly with frequency, but the final scattering depression depends also on other factors. One consideration is the background absorption due to gases. A higher gas absorption decreases the effect of scattering, and this effect generally increases with frequency. It is correct that also the asymmetry parameter needs

to be considered, which increases with frequency. A higher asymmetry parameter gives a lower depression for a given cloud optical depth, see Fig. 5 of Eriksson et al. (2015).
It can be hard to judge the scattering depression in a figure like Fig. 5, as the clear-sky values differ between the channels. In the version found below, extracted scattering depressions are shown in the second panel. For high-clouds with moderate cloud optical depth, the scattering depression increases monotonously with frequency, while in the most dense cloud region (around lat 2.7) this is not the case for the reasons discussed above.

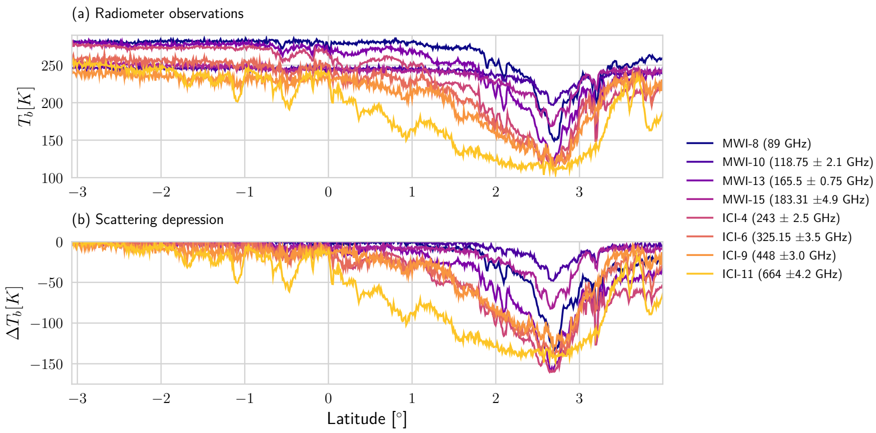

Figure 1: Simulated brightness temperatures (Panel (a)) and cloud signal depressions computed for selected channels of the MWI and ICI radiometers for the first test scene.

Reviewer comment 8

8) Line 275: not clear what you mean, in Tab.4 there are 6.

Author response

What was meant here is that different ice shapes are tested for the single frozen hydrometeor species which is used in the retrieval. Tab. 4 lists the different shape models that were investigated.
Since the section describing the selection of particle models will be rewritten, this sentence will be reformulated to make it clearer.

Reviewer comment 9

9) “extends below the sensitivity limit of the passive-only observations around 10-5 kg m-3” : very sloppy sentence. Passive mi-crowave radiometer are sensitive to integrated contents!

Author response

As response to a comment from another reviewer the corresponding paragraph will be rewritten and this sentence will be removed.

Reviewer comment 10

Fig 6d: this retrieval looks really weird. Where are all the stripes coming from? Certainly this does not look like acloud, or? What kind of constraint have you imposed on the cloud top?

Author response

It is true that the passive only retrieval does not perform well in terms of the vertical structure of IWC. The reason for this is that the passive observations alone do not provide much information on the vertical distribution of ice. To correct for this, further regularization would be necessary which is not applied here in order to keep the comparison to the other retrieval methods fair. All of this is discussed in the discussion section of the manuscript.

Reviewer comment 11

"In general, the radar-only results exhibit only very weak dependency on the particle model, making the results for different particle shapes virtually indistinguishable." Again another dangerous sentence. We know (unfortunately) that this is not true (otherwise our ice problems would be sorted). Here my guess is that you have not properly explored the backscattering variability (particularly looking at the different degree of riming). It is notclear to me whether there is enough variability in your ARTS database, I guess you are more focused at ice particles (including aggregates) but you are not considering really rimed particles. Regions where graupel is present should be avoided from the discussion of the radar-only retrieval for the simple reason that in those regions attenuation correction and multiple scattering effects make the problem very tricky. I guess that the radiometer as well is in serious trouble when entering those areas. Again, I would not start tackling regions the observation system is not tailored for.

Author response

As mentioned above, we will revise the particle habits used in the retrieval, but we expect that particle shape will continue to have a smaller impact on our radar-only retrieval. What the results shown in scatter plot in Fig. 7 and 8 indicate is that the uncertainty which can be attributed to the particle size distribution (PSD) is larger than that introduced by the assumed particle shape. However, it is difficult in general to draw a clear line between particle shape and PSD. This is especially true if particle size is described by $D\text{max}$, and the PSD is defined accordingly. In this case, IWC of a given PSD will depend on the particle's effective density, and e.g. degree of riming becomes

critical. Accordingly, to what extent retrieval errors are due to shape or PSD, depend partly on definitions.
The ARTS single scattering database does include several types of rimed particles. Two of them are the GEM Graupel and GEM Hail models which are used in the simulation of the synthetic observations. For the retrieval, however, it is true that we did not include rimed particles in the tested particle models but this will be changed for the revised version of the manuscript.
Both the forward simulations and the retrieval handle attenuation consistently. We therefore think it is worth considering even regions where graupel is present as this allows us to assess the uncertainties caused by not having a realistic representation of rimed particles in the retrieval.
It is certainly correct that for space-borne observations multiple scattering needs to be considered and this will add complexity to the retrieval. Here, however, we can avoid this extra complexity as we use simulated observations which do not include multiple scattering.

Reviewer comment 12

Fig.10 is missing!!!

Author response

Fig. 10 was unfortunately missing from the manuscript. The figure will be included in the appendix of the revised version together with the analysis of the second test scene.

Reviewer comment 13

"Since the calculation of the AVK involves the forward model Jacobian, this effectmust be related to the non-linearity of the forward model" well I would avoid such veryspeculative statements.

Author response

Following the suggestion of the reviewer, the sentence will be removed from the manuscript.

Reviewer comment 14

You need to be very careful how you present the results in Fig. 14. The conclusions that I can draw is the following: a CloudSat like radar is pro-viding much more information than the ICI+MWI radiometers when characterizing ice particles (really the radiometer is providing some additional water vapour information). As a result we should invest in the former and not the latter. While I may agree with the previous statement and strongly support a CloudSat-like radar on an operational mission my feeling is that you are pitching your radiometer system at the wrong kind of scenes (I already see an improvement going from the first to the second scene). I would have selected completely different scenes (including high latitude clouds with mixed phase). It is to me an overkill to try to retrieve D_M of rain for these scenes from your PMW radiometer suite of sensor. If you have any skill in warm rain you should properly prove it

Author response

Our interest in this study is neither arguing for one nor the other observation system. The question that we want to address is whether combined observations have extra value compared to separate observations. Such combined observations could be achieved by performing joint flights with the aircraft carrying the ISMAR sub-millimeter radiometer and another one carrying a radar, by flying a cloud radar in constellation to Metop-SG, or by adding a sub-millimeter radiometer to the platform carrying some future cloud radar. We consider it out of the scope of this study to judge the cost effectiveness of either of these solutions.
As the referee clearly favours radars, we would like to balance this by mentioning that passive instruments have an additional strength in their much higher areal coverage. The swath of ICI and MWI is about three orders of magnitude broader than that of CloudSat and EarthCARE.
Although a cloud radar certainly provides more information on frozen hydrometeors than ICI, our results clearly show that also radar observations alone are insufficient to accurately determine the microphysical properties of ice hydrometeors (Fig. 4, 7). The passive adds information on the microphysics of the clouds to the radar (note the significant increase in information content on N_0^* in Fig. 14) which helps to reduce retrieval uncertainties (Fig. 11). Although it is not clear whether these improvements carry over to space-borne observations, our results clearly show this as a synergy between the passive and active observations (esp. Fig. 4, 11, 14).
The cloud scenes used in the manuscript were selected with the aim of providing a representative sampling of the type of clouds present in the two model scenes that were available for the study. We did not want to cherry pick scenes were the retrieval works well to provide a more realistic assessment of the retrieval.
Rain must be handled in the retrieval due to its effect on the passive radiances. However, we never claim that we have any skill in retrieving warm rain and so we do not agree that we are required to prove to have it.

Reviewer comment 15

LWP and Fig.16. I have a serious problem here. The cloud I see on the right is a liquid cloud. So how it is possible that your radiometer is doing sobadly in the LWP retrieval and why the combined is so much better? I guess this must go back to understanding surface emissivity and integrated water vapour (maybe somecomments there should be made to explain what kind of surface/IWP we are dealingwith). You have not included radar path integrated attenuation in your retrieval (like istypically done in radar retrievals) but this could of course help in this case.

Author response

The cloud in the right of the scene is a mixed-phase cloud. There are several explanations for why the retrieval does not work well here: First of all, our observations setup does not make use of the channels around 23 GHz, which are typically used for retrieving LWP. And also here the performance of the passive-only retrieval suffers from the lack of a priori information on the vertical position of the cloud. Since liquid water at higher altitudes has a stronger impact on the observations, the retrieval puts too little cloud water too high in the atmosphere because of its inability to locate it properly. This is discussed in Sect. 4.2.3 of the manuscript.

Reviewer comment 16

I do not think that for OE to work The forward model must be linear as stated at line 544.

Author response

The OEM can of course be applied to non-linear problems but a complication that arises is that it can get stuck in secondary minima. The sentence will be corrected in the revised version of the manuscript.

Reviewer comment 17

17)Sect.4 and 5: a lot of waffling here (e.g. the three bullet conclusion, you need to bemuch more quantitative and linked to what you have proved; the three statements aresomething I could have formulated on my own without making any simulation). Again the conclusions must be related to the cloud regime you are considering (and cannot be valid for all!)

Author response

One of the main advantages that we see in the combined retrieval is that it actually works for a wide range of different cloud regimes. If the cloud regime was known a priori, good results can probably be achieved using only a radar and suitable a priori assumptions. In general, however, this is not the case, which leads to the uncertainties that we currently have in the observational record for IWP and IWC.
For the revised manuscript, we will rewrite the conclusion and parts of the discussion to make it more concise and the point mentioned above more clear.

1 Minor comments

Reviewer comment 1

I would avoid the use of “ice mass density” and use “ice water content”

Author response

The proposed changes will be adopted in the revised version of the manuscript.

Reviewer comment 2

Table 2: it would be good to see footprints as well

Author response

Since in the revised manuscript an airborne viewing geometry will be considered the footprint sizes of MWI and ICI are not relevant anymore.

Reviewer comment 3

Line 130: dBZ are the wrong units for a std of a reflectivity!

Author response

We are unsure what the reviewer is referring to here since quantifying uncertainty in the radar observations in dBZ seems to fairly common. This is for example how it is handled in the DARDAR cloud (Delanoë and Hogan, 2010) product as well as in the study by Jiang et al. (2019).

Reviewer comment 4

Line 180: "The remaining shape of each PSD is described by the shape parameters alpha and beta, not to be confused with the parameters of themass-size relationship shown in Tab. 1."; very confusing. Why are you using the same letters????

Author response

We used the same letters to be consistent with the definition and used in Delanoë et al. (2014) and Cazenave et al. (2018). However, since the explicit values of the α and β parameters are probably of little interest for the average reader, we will simply refer to Cazenave et al. (2018) and not name the parameters explicitly.

Reviewer comment 5

Line 193: wrong units

Author response

This will be corrected in the revised version of the manuscript.

Reviewer comment 6

Line 199: English

Author response

This will be corrected in the revised version of the manuscript.

Author response 7

Line 35 page 2 (not really limited,this is a wide range!!)

Author response

The corresponding sentence will be reformulated in the revised manuscript.

Reviewer comment 8

Line 54 page 2. maybe it is worth mentioning all the heritage coming from radar-radiometer retrievals with W-band (Ka and Ku-band) radars with PMW radiometers.

Author response

Following the suggestion of the reviewer, a paragraph that mentions previous work on synergistic retrievals using radar and passive radiometers at lower microwave frequencies will be added to the introduction.

Reviewer comment 9

Line 229: “troposphere” is too generic Line

Author response

The use of the word *troposphere* and should have been *tropopause*. This will be corrected in the revised version of the manuscript.

Reviewer comment 10

Fig 4 caption: you need to include how thick is the layer.

Author response

This will be included in the revised version of the manuscript.

Author response 11

250: rho is not defined

Reviewer comment

ρ will be defined in the revised version of the manuscript.

Reviewer comment 12

Line 4: 272.5????

Author response

This mistake will be corrected in the revised version of the manuscript.

Reviewer comment 13

Fig 4 caption: you need to include how thick is the layer.

Author response

The layer thickness will be added to the figure caption in the revised version of the manuscript.

References

Cazenave, Q., Ceccaldi, M., Delanoë, J., Pelon, J., Groß, S., and Heymsfield, A.: Evolution of DARDAR-CLOUD ice cloud cloud retrieval: new parameters and impacts on the retrieved microphysical properties, Atmos. Meas. Tech. Discuss., 2018, 1–24, https://doi.org/10.5194/amt-2018-397, URL https://www.atmos-meas-tech-discuss.net/amt-2018-397/, 2018.

Delanoë, J. and Hogan, R.: DARDAR-CLOUD, URL www.icare.univ-lille1.fr//projects_data/dardar/docs/varcloud-algorithm_description-v1.0.pdf, 2010.

Delanoë, J., Heymsfield, A. Protat, A., Bansemer, A., and Hogan, R.: Normalized particle size distribution for remote sensing application, J. Geophys. Res.-Atmos., 119, 4204–4227, https://doi.org/10.1002/2013JD020700, 2014.

Eriksson, P., Jamali, M., Mendrok, J., and Buehler, S. A.: On the microwave optical properties of randomly oriented ice hydrometeors, Atmos. Meas. Tech., 8, 1913–1933, https://doi.org/10.5194/amt-8-1913-2015, 2015.

Jiang, J. H., Yue, Q., Su, H., Kangaslahti, P., Lebsock, M., Reising, S., Schoeberl, M., Wu, L., and Herman, R. L.: Simulation of Remote Sensing of Clouds and Humidity From Space Using a Combined Platform of Radar and Multifrequency Microwave Radiometers, Earth Space. Sci., 6, 1234–1243, https://doi.org/10.1029/2019EA000580, URL https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019EA000580, 2019.

Milbrandt, J. and Yau, M.: A multimoment bulk microphysics parameterization. Part II: A proposed three-moment closure and scheme description, J. Atmos. Sci., 62, 3065–3081, https://doi.org/10.1175/JAS3534.1, 2005.