# Response to reviewer 2

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.

## 1 Major points

### Review comment 1

A major aspect of the study concerns the representation of the particle size distribution which is retrieved by two free parameters (different from the 2 moments of the atmospheric model GEM used to provide the test scenes) and the assumptions of the particle type. The difficulty of connecting atmospheric model output to single scattering properties (which is one of the fundamental assumptions) could be better explained. The motivation why the authors choose their approach and why they test certain settings need to be discussed in the beginning. Couldn't Tab. 1 and 4 be combined and better explained which is used for which purpose? Why is cloud ice the same and GEMsnow and GEMGraupel different in both?

### Author response:

We agree with the comment that the rather arbitrary choice of tested particles is a weak spot of the study. To improve this, the experiments will be repeated with a more principled selection of particles. The new selection is based on the particle properties described in Ekelund et al. (2019) and covers a broader range of mass-size relationships and scattering parameters. In particular, the GemSnow model has been removed from the selection of test particles because it does not cover small ice particle sizes.
We propose to make the following changes to the manuscript:

- Add a paragraph to the description of the GEM test scenes which explains the particle models that have been developed to match the assumptions of the GEM microphysics scheme and that are used to simulate observations.
- Add a paragraph to the description of the retrieval implementation that explains the difficulty of representing the complex mixture of different particles in the GEM model scenes with a single particle model as well as the new selection of particle models and habit mixes.
- Correct the error in the reported parameters of the mass-size relationship for the GEM Snow and GEM graupel.

However, because of these changes it will not be possible to combine Tab. 1 and Tab. 4, since the convey slightly different information.

## Reviewer comment 2

Although different parameterizations of the hydrometeor types are used to study their effects, vertical changes (development of sedimenting particles) are not considered. Similar polarization effects are not mentioned in the discussion on particleshape. Otherwise the paper nicely discusses the different aspects but in the end I ammissing a clear message on the outcome of the test (choice of particle types). What isrecommended for the future?

### Author response

The first statement made by the reviewer is not fully correct. Since the retrieval can reduce the concentration of particles and increase their size it can modify the ratio of small and large particles and thus represent the effects of sedimentation on the PSD.
Vertical changes in particle shape, i.e. transition from single crystals to aggregates, are represented indirectly through the particle size. The particle models used here are taken from standard habits from the ARTS SSDB described in Eriksson et al. (2018). Some of them combine pristine crystals at small particle sizes with aggregate shapes at larger sizes.
Polarization effects in the simulations were ignored for the simple reasons that the model scenes do not provide information on particle orientation or aspect ratios and that suitable scattering data for oriented particles has only recently been released (Brath et al., 2019).

For the revised version the sensors are assumed to point at nadir, which justifies neglecting polarization effects. Nonetheless, particle orientation can still have an effect on the observations. We will state clearly in the revised manuscript that polarization effects will have non-negligible impact on the observations of the MWI and ICI sensors.
We agree that in choice of the particle shape was not described well in the manuscript. To address this as well as to provide a clearer message on the outcome of our tests we propose the following changes for the revised manuscript:

- State clearly in Sect. 2.2 that for MWI and ICI polarization effects can not be neglected.
- Improve the description of the employed particle models in Sect. 2.3.
- Extend the discussion of the tested particle shapes to derive clearer recommendations for the future.

## Reviewer comment 3

Not only the two moments of the ice PSD but further variables are retrieved and their information content is nicely shown in Fig. 14. I am surprised that the information on moisture is so low although information along three water vapor lines is provided? This should at least in the upper atmosphere provide information? Is it due to the choice of relative humidity which mainly depends on temperature? I am also skeptical about the results of Fig. 16. Basically, there should be no liquid for temperatures colder than 40 deg C (freezing) but it even reference LWC goes up to 13 km? I would not support the statement on L568 – where is the evidence? Similar L527 – Liquid water estimation within mixed phase clouds is extremely difficult and if ICI and radar could really do that together this would be worth a separate paper. To better understand the information content, I suggest to plot the profiles of cumulative degrees of freedom for the different retrievals as this could help interprete where and how the synergy works.

### Author response

As can be seen in Fig. 8 from Eriksson et al. (2019), the information content on water vapor from ICI alone are at most 4 degrees of freedom for clear-sky scenarios. Since in the retrieval also the channels from MWI are included, the expected information content on water vapor should be somewhat higher. However, this is for clear-sky scenarios. In the presence of clouds, the information content will be significantly reduced.
Regarding the results of the retrieved cloud liquid water content (CLWC), Fig. 16 shows quite clearly an improvement, both in terms of CLWC and cloud liquid water path (CLWP), in the results of the combined retrieval compared to the passive-only retrieval. Yes, liquid clouds droplets are present at high altitudes in the first model scene but only below the 230 K isotherm. However, since this is the case only for the first scene, it does not seem relevant for the interpretation of Fig. 16.
To shed more light onto the information content regarding humidity and CLWC we will follow the referee's suggestion and replace the bar diagrams in the manuscript with a

figure (Fig. 1 shown below) which displays the cumulative DFS for all profiles in the test scenes.

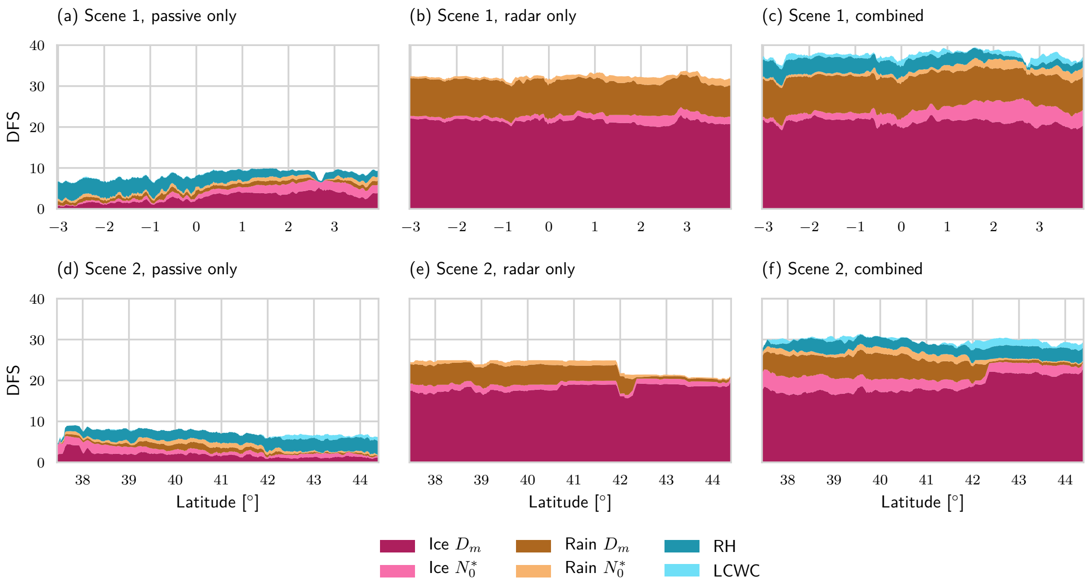


Figure 1: Degrees of freedom for signal for all retrieval configurations and both test scenes obtained with the Large Plate Aggregate model. The colored areas in each plot represent the contribution to the cumulative degrees of freedom from each retrieval quantity. Results for the first and second test scene are displayed in the first and second row, respectively. The first, second and third panel in each row show the results for the passive-only, radar-only and the combined retrieval.

## Reviewer comment 4

The manuscript is rather lengthy making it difficult for the reader to extract the major-points. I strongly suggest a) to move part of the analysis into an appendix (, b) remove double statement (see minor comments, also the LWC plot) and c) to remove figurecaption like information (for example L92 or "filled contours") from the text. The text must make sense without looking at the figure. Figure only support the statements made in the text. Lengthy descriptions such as "The plot shows.." need to be avoided.

### Author response

We will follow all the referee's recommendations to make the manuscript more concise.

## 2 Minor comments

### Reviewer comment 1

L39: Is sensitivity really the right word? Range resolution is the main advantage –signal-to noise range depends on distance and hydrometeor distribution,

#### Author response

Following the reviewer's suggestion the sentence will be rewritten.

### Reviewer comment 2

L48: MWI will also cover new spectral channels, e.g. 118 GHz

#### Author response

We will incorporate this information into the introduction.

### Reviewer comment 3

L62: "high-resolution" is always relative for a model. I would recommend avoiding this term and use Cloud resolving Model (CRM).

#### Author response

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

### Reviewer comment 4

L68: After you mention GPM (with scanning radar) it might be good to say that you are only looking at a nadir pointing radar (curtain). The swath center came bit as a surprise.

#### Author response

We will incorporate this information in the introduction as suggested.

### Reviewer comment 5

L70: There has been quite some literature about combining active and passive MWusing a Bayesian framework which should be acknowledged, e.g. Grecu, M., & Olson,W. S. (2006), Johnson et al. (2012) , Munchak, S. J., & Kummerow

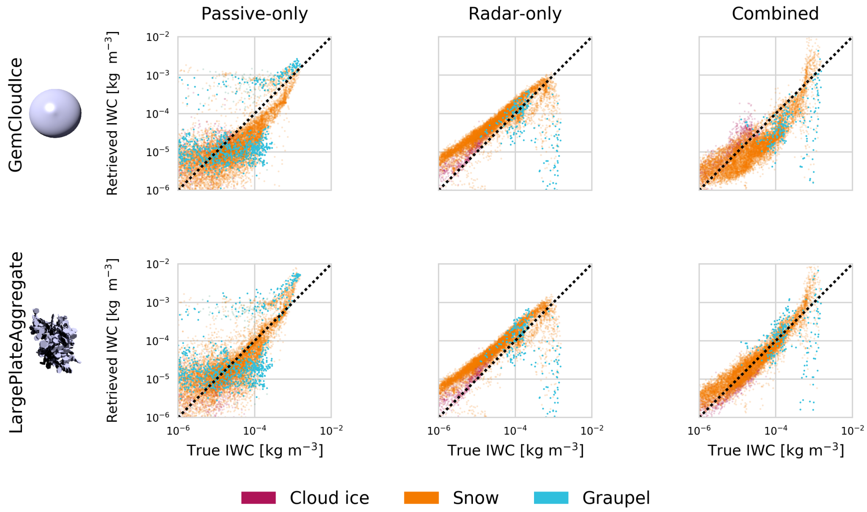


Figure 2: Scatter plots for the second test scene showing the retrieved IWC plotted against the IWC in the GEM model scene for the passive-only, radar-only and combined retrieval.

### Author response

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

## Reviewer comment 6

L84: Test scenes have a grid resolution of 1 km horizontally. As this is not the true model resolution I would have recommended to coarse sample the model data (maybe every 5th data point) and include more diverse profiles instead. This might be especially interesting for the scatter plots.

### Author response

The point raised by the reviewer here is certainly correct. However, the decision to restrict simulations to two test scenes was motivated primarily by the computational costs of performing the retrievals. The scatter plot in Fig. 10 (shown in Fig. 2 below), which was unfortunately missing from the manuscript , shows that the emergent structures are consistent for both test scenes. This indicates that the scenes cover sufficient profile variability to be statistically representative.

### Reviewer comment 7

Motivation lacking: "To perform RT simulations for each GEM profile the PSD needs to be diagnosed from the prognostic GEM variables, i.e. N and m.."

#### Author response

Since also reviewer 1 requested changes in the corresponding paragraph, it will be rewritten taking into account the reviewer's suggestion.

### Reviewer comment 8

L92:"prognoses" means forward in time - you mean diagnosed, calculated,determined..

#### Author response

The word will be replaced by derived in the revised version of the manuscript.

### Reviewer comment 9

L98: I find the term "horizontal and vertical scaling" strange – why not saying PSD shape is similar but scaling in respect to diameter and number density. At least definethe term clearly the first time that you use it or define a short for it.

#### Author response

This issue was also mentioned by reviewer 1 and will be addressed in the revised version of the manuscript.

### Reviewer comment 10

L103: model test – be careful also at other instances that "model" can mean too manythings. Here I would say GEM test scenes.

#### Author response

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

### Reviewer comment 11

L119: Need to clearly say that polarization effects are neglected though these can beseveral Kelvin, e.g. Xie et al., 2015. You ignore this effect but even consider noise reduction.

#### Author response

See response to general comment 2.

## Reviewer comment 12

L157-159: needs to be better motived, references?

### Author response

We will provide a better motivation of the use of the $\chi^2$ statistic in the revised manuscript.

## Reviewer comment 13

L172: I doubt that the model has constant vertical resolution. It will be better close to the surface and worse aloft. This should be mentioned than GEM is introduced.

### Author response

As suggested by the reviewer, this will be mentioned when the GEM test scenes are introduced. Moreover, a sketch will be added to the manuscript which displays the GEM model grid and the grids of all retrieval quantities for the retrievals.

## Reviewer comment 14

L 174: for all hydrometeor species of the model? It would be helpful to first introduce all retrieval quantities – I was missing a motivation for the paragraph around L195. How do you define the freezing level (and later the troposphere)? How do they vary in both test scenes? The model also likely has supercooled liquid water above the freezing layer – how is this treated?

### Author response

As mentioned above, a figure will be added to the manuscript that displays all retrieval variables as well as the freezing level and the tropopause. Moreover, we will add an explanation of how the freezing level and tropopause are defined in the manuscript.
For the simulated observations, supercooled liquid is treated in just the same way as liquid water below the freezing level. As described in the paragraph around L. 211, cloud liquid water in the retrieval is treated as purely absorbing and simulated using a parametrized absorption model. Moreover, it is restricted to temperatures of 230 K and up.

## Reviewer comment 15

L 198: Vertical resolution of retrieval grid: Why 4 points? The freezing layer must be very different for both cased. Maybe a sketch would be helpful as later on lines 230 the different vertical resolutions for other variables is discussed?

#### Author response

We have revised the retrieval implementation and now use fixed retrieval grids with a resolution of 2 km for the $N_0^*$ parameters. Reducing the resolution of the retrieval grids for the $N_0^*$ parameters was found to aid the convergence of the retrieval.
The freezing level does indeed vary between the two scenes. The freezing levels of both scenes will be added to Fig. 1.

### 2.1 Reviewer comment 16

L281: How do I know that Large Plate is the best performing model? Which parameter,plot, table does show that?

#### Author response

This section will be revised to make it clear which results show that the Large Plate Aggregate yields the best performance.

### Reviewer comment 17

L283-L307: Can be shortened significantly

#### Author response

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

### Reviewer comment 18

L332: There can I see that? Give figure?

#### Author response

A reference to the figure will be added in the revised version of the manuscript.

### Reviewer comment 19

L325: The two paragraph here give similar information -> streamline

#### Author response

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

### Reviewer comment 20

L333-344: I would put this to the appendix

### Author response

We will follow the reviewers advice and put the analysis of the second test scene into the appendix.

## Reviewer comment 21

L444: Here it needs to be made clearer how this goes beyond what GPM is doing.

### Author response

To clarify how our work goes beyond what GPM a paragraph detailing this will be added to the discussion.

## Reviewer comment 22

L495: "does not say much about the general validity of the assumption". Here you should dig in a bit more. What is the role of a priori and covariances?

### Author response

Following the suggestion of the reviewer we will extend the discussion of the a priori assumptions.

## Reviewer comment 23

L560: Rethink the bullet structure. 2. Is not an independent result. For each result refer to the part of the manuscript where you can clearly see that. Especially result 3 should be detailed – how do ICI channel advance the currently available data?

### Author response

The bullet points will be remove in the revised manuscript and replaced by a text which presents the conclusion in a logically coherent way.

## Reviewer comment 24

Fig. 3: Is it really worth having the slightly different size distribution shapes for frozen and liquid? Isn't there a stronger difference between different frozen hydrometeors

### Author response

This is certainly true but in most clouds ice and rain can be distinguished fairly well a priori, which simplifies treating them as different species using different PSD shapes. Distinguishing between different frozen hydrometeors is difficult to do a priori and using multiple species of hydrometeors in the retrieval was found to cause ambiguities which the retrieval is not able to resolve.

## Reviewer comment 25

Figures 7 and 8: I'm not sure why these are separate figures – it seems like allpanels could fit on one page.

### Author response

Figures 7 and 8 will be combined into a single figure in the revised manuscript.

## Reviewer comment 26

Fig. 4 and also in text: "cloud signal" say that this is dTB.

### Author response

Following the reviewers recommendation, the passive cloud signal will be referred to in the text as $\Delta T_B$ and the radar signal as $\text{dBZ}_{\text{max}}$.

## Reviewer comment 27

Fig. 5: Can you add freezing layer height?

### Author response

We propose to add the freezing layer height to Fig. 1, since this will allow showing the freezing level height for both scenes, which addresses minor comment 15 as well.

## Reviewer comment 28

Fig. 6: It would be nice to see the absolute values of IWP somewhere. Maybe you could add another time series with IWP as the sum of the different components such that the reader can see where the different categories (cloud, graupel, snow and hail) contribute most?

### Author response

To address the reviewers request we will add absolute IWP of the reference scene and its decomposition into different hydrometeor classes to Fig. 6.

## Reviewer comment 29

Fig. 7: I think it is retrieved vs. truth. The word following is not really exact. Why not put 7 and 8 together?

### Author response

Fig. 7 and 8 will be merged and the caption will be corrected.

### Reviewer comment 30

Fig. 9: Could go to the appendix

#### 2.1.1 Author response

Fig. 9 will be moved to the appendix.

### Reviewer comment 31

Fig. 10 I only see the caption???

#### Author response

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

### Reviewer comment 32

Tab. 1. Assumed particle model information for each hydrometeor class given by GEM-model. In fact it could be good to combine it

#### Author response

In order to give a better overview of the particle models that are used in the retrieval Tab. 4 will be extended in the revised version of the manuscript. This, however, would make merging Tab. 1 and 4 slightly confusing so we decided against the reviewers recommendation.

### Reviewer comment 33

Tab.3 : I would recommend to add a first column with a spelled out name

#### Author response

We will add the column to the revised version of the manuscript.

## Grammar, typos and reformulations

The authors would like to thank the reviewer for the additional comments, all of which will be incorporated into the revised manuscript.

## References

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