Responses to Comments

We sincerely thank Editor Eriksson, and an anonymous reviewer for the constructive and thoughtful comments.

Comment are in *blue italic lettering*, responses in black.

Editor Comments

Sec 2.1: To avoid confusion, clarify already here (or just here) that the simulations and results assume both instruments to be nadir looking and have a very high horizontal resolution (as you don't cover footprint inhomogeneities). As it is now, you present two instruments, but simulate something else.

Thanks for your suggestion. The assumptions on the active and passive instruments have been added in Sec. 2.1, as shown in lines 84-88. The discussions on this aspect in other paragraphs have been deleted.

Line 75 + Table 1: The term "desired noise characteristics" is vague. Are these values target, breakthrough or something else more quantitative? Who has set these values? Are they +-1 std dev, or something else?

The noise characteristics are disclosed by the "AtmOS Microwave Radiometer Instrumentation RFI.PDF" document on the https://aos.gsfc.nasa.gov/gallery.htm website. They show the desired and target radiometric resolution, and the former is used in this study. The noise quantity for the 310GHz is not given, and it is assumed to be 1.5K here. More details are added in the manuscript, as shown in Lines 76-78 and the caption of Table 1.

Sec 3: Add references to be more clear about what you have taken from the literature and what you have added yourself. I would suggest adding references on at least lines 144, 158 and 188.

The references have been added, as shown in Lines 143, 148, 157, 162, and 185.

Fig 3: In panel titles it says "Ghz". Should be "GHz".

The error in Fig. 3 has been corrected.

Lines 170-178: Here I want to clarify that applying OEM in ARTS is not restricted to the PSD of De la Noe et al. For example, MGD can be applied in multiple ways. And as far as I see, your prior assumptions could also be applied inside ARTS. More generally, I don't think your argumentation for not doing OEM is fully correct. I would say that what you bring up on lines 179-180 is the critical aspect. That the assumptions behind OEM are not valid for retrieval of hydrometeor properties.

The statements for the reason have been modified, and I only use the points given in Pfreundschuh et al., 2020, as shown in Lines 175-178.

Line 344: I don't see why this fact would lead to a stronger constrain for ice inside the altitude region.

This sentence has been deleted, as shown in the discussions around Lines 340.

Line 348: Here it becomes clear that the retrieval database contains dry profiles. Maybe I miss something, but do you impose any constrain between the presence of hydrometeors and relative humidity? Or can there be a high IWC where the relative humidity is very low?

That is a good idea to use the correlations between the IWC and relative humidity. We do not use this information in this study. Since the correlations are right inside the precalculated retrieval database, it is definitely worth investigating. Thanks for giving this hint.

Line 368: Are the results in Fig 8 consistent with the DOFs obtained in Eriksson et al (2020)?

I double-checked the program and use the Typhon package to calculate the IWV now. Even though, the discrepancies can still be seen for the dry atmosphere (IWV<42 kg m-2) with low IWC(IWC<100 g m-2), where Fig. 8 shows higher DoFs in this area than the results in Eriksson et al (2020). I think there are two possibilities to explain the disagreement. First, we estimate the DoF using the atmospheric profiles that only contain ice cloud profiles, and liquid hydrometer species are excluded. Second, the random atmosphere/cloud cases in each IWP-IWV bin here are likely to be denser than that in your database since the figure in Eriksson et al (2020) spreads a much broader IWP and IWV range. That implies that more microphysics variabilities may exist in each bin here and therefore, more diversity in the simulated BT is possible. We have added some statements saying that the DoF estimation is likely to be different if we use more complicated atmospheric profiles, as shown in Lines 363-365 and the caption of Fig. 8.

Line 369: I would suggest calling the section: 5 Results and discussion

The title of this subsection has been modified, as shown in line 366.

Figures 11-13 take a lot of space. Are both Fig 11 and 13 really needed? If yes, place them after each other as they are so similar.

Fig. 13 has been deleted, and the arguments have been combined into the discussion for Fig. 11.

Fig 12: As Referee #1 pointed out, you don't describe how you define your retrieval uncertainty. Accordingly, what to expect in Fig 12 is not clear. Most importantly, does your retrieval uncertainty include what Rodgers denotes as smoothing error? If no, then I don't see the point in Fig 12 because then you don't know what to expect. You can get very high values in Fig 12 and all is OK (if the total error is dominated by the smoothing term). If yes, then I don't agree with your analysis. Assuming Gaussian errors, there should be a few points above 3 (ie outside of +-3 std dev). Why do you expect that most values should be around 1? On a linear scale, the distribution should peak at 0. Further, I don't think you need all panels here, just some examples should suffice.

Sorry for not illustrating it clearly. All uncertainties estimations are calculated based on the ensemble approach to finding the standard deviation using the Bayesian MCI. For the radiometer-only retrievals, the last ensemble in the EnPE implementation is used. For the radar-only and synergistic retrievals, the random cases from the Cholesky decomposition are used. The difference is that the random cases have the same weights in the radar-only retrievals, but they are weighted according to the BT disagreements for the combined retrievals. Since the covariance matrix in Eq. (4) is derived by combining the prior Gaussian PDF and the conditional Gaussian PDF, the smoothing error describing the prior uncertainty should be included. I agree that the analysis of delta error is not appropriate. This figure has been deleted until more deep investigations about how to use the uncertainty as a diagnostic parameter are conducted in future work.

Line 464: If you think that the selection of the initial state is critical, you have not solved the minimisation problem properly.

I agree with this point. Thanks for pointing it out. This sentence has been deleted, as shown in Lines 432-437.

Lines 504-505: It says "a tropical transect" on both lines. The same, or different ones?

The sentence has been corrected. As shown in Line 475-476.

There are also a number of language issues, but I expect that those problems are handled in the copy-editing.

The manuscript has been polished and many grammar mistakes are corrected. Please check it again.

As a final remark, I can mention that our new article on joint passive and active retrievals https://amt.copernicus.org/preprints/amt-2021-306 got accepted one week ago and should come out in AMT relatively soon. Contact me if you want to see the final manuscript version already now.

I have read this paper and it is fantastic. Thanks for sharing this work. Studying in Prof. Buehler's group is one of the best experiences I have ever had in my life. Hopefully I could have the opportunity to work with the ARTS group again in the future. All the best wishes for the ARTS community.

Reviewer 1 Comments

Considering my comment on p. 17, l. 400 (version from August) What is meant with "retrieval uncertainty" and how do you estimate it?

Author response:

The retrieval uncertainty is created by different retrieval algorithms associated with the retrieved quantities, and we indicate in lines 420-421.

The authors explained what the retrieval uncertainty but they still did not explained how do they estimate it.

Sorry for the poor illustrations of the retrieval uncertainties. More explanations are given in the response to the editor's comments above. Please check it.

Figure 3 (p.8): "Integrated water content for ice and snow particles for the selected latitudinal transect and the corresponding brightness temperature..." Caption is inconsitent with figure.

The caption of Fig. 3 has been corrected.

Figure 14 (p. 26): The unit for the PDF (y-axis) still seems to be missing.

The unit for the PDF has been added.