Answer to Reviewer #2

We thank the reviewer for their feedback. Our answers are given in blue, all line numbers refer to the re-submitted manuscript. Based on many of the reviewer's suggestions, we thoroughly revised the manuscript and our analyses. More specifically, we

- added a sub-section on *Data Acquisition and Processing*
- modified the radar data processing. We now use the long-term tested radar processing discussed in detail in e.g. Küchler et al. (2017), Chellini et al. (2023), to process the radar's *Level-0* binary output. Processing includes the calculation of noise floor, de-aliasing, and a speckle filter.
- added uncertainty calculations to the water vapor retrieval following Roy et al. (2018) and Battaglia and Kollias (2019), and adjusted the ground-based analysis Section 4.
- performed analyses based on simulated radar measurements using the Passive and Active Microwave TRAnsfer model PAMTRA (Mech et al., 2019) to investigate the effects of differential attenuation and scattering on the water vapor retrieval performance.
- used SNR to filter measurements before performing the water vapor retrieval. A filter of SNR = 1dB is applied, using the same threshold as in Roy et al. (2020).
- added an inter-calibration bias correction to the airborne W-G radar measurements presented in Sec. 5.2.

Reviewer Comments

The paper presents novel measurements combining in situ and ground-based and airborne remote sensing instruments in the Arctic. The suite of instruments is extremely interesting including a W band and a DAR system. Data from few case studies are presented and documented. As it is the paper looks more like a presentation of a dataset (but in case this is not the right journal) and it does not match the expectations fostered in the reader by the title ("novel insights on Arctic water vapor, clouds and precipitation"). There is no real insight into cloud and precipitation, surely. The assessment of the performances of the retrieval of water vapor also needs more attention. The focus of the paper could be on this aspect (but then a more thorough analysis needs to be provided).

The scope of the presented manuscript is to (i) introduce the novel GRaWAC radar system with its technical details, (ii) investigate the potential for water vapor profiling and cloud microphysical analyses at an Arctic supersite, and (iii) illustrate first-ever Arctic air-borne measurements performed with a G-band radar system. Throughout the revisions, we highlighted the scope throughout the text, and provided more detail in the analysis of the retrieved water vapor profiles.

I highlight an initial list of major comments that certainly need to be addressed. They should provide a starting point for further investigations.

Major comments

1) Not clear how the retrieval is actually performed. Rephrase and improve. In Eq.7 you drop r_1 and r_2 (which is the basis of Eq.(6)). This generates confusion. So what is the relation *between r r_1 and r_2 (I suppose r=(r_1+r_2)/2)? In particular it is not clear how big is R, whose selection is critical for the retrieval! (if R is too small the signal will be small and difficult to be disantangled from noise). Explanation in the following paragraph is unclear and must appear up-front.*

R_1, r_2 and R are introduced in L 162 at the top of Sec. 2.1, and relate to one another through $R = r_2 - r_1$. We test the retrieval performance of different configurations of R in updated Fig. 4. LL 197 – 204 summarize how the retrieval is applied to the measurements.

2) Line 184: "We find that the bias to the reference is minimized for R = 200m and tavg = 60s, and we choose this setting for ground-based deployment, as illustrated in Sec. 4.". I think this statement should be discussed more in depth. I am surprised actually to see that there is one solution that fits it all. I would expect that more averaging may be needed at low water vapor conditions but that this could be relaxed in presence of larger moisture. This needs to be better investigated.

Indeed, we expect these settings to differ in conditions with higher moisture loading and different radar measurement settings like number of averaged spectra or measurement integration time. We revised the sensitivity analysis of the retrieval to the choice of R and $t_{avg,}$ now given in L 275 – 281 and new Fig. 4, including the following statement: *"Settings are further expected to depend on columnar water vapor loading (thus, dynamic range of DAR signal) and temporal variability of cloud conditions." (LL 280).*

3) Line 195: hopefully this value of K_w^2 has been used consistently in the calibration of the radar reflectivity.

Yes, the same value of K_w = 0.86 is used in the calculation of Ze. We added this detail to the new Section 2.3.

4) Line 198: in the computation of theta shouldn't the roll angle play a role as well?

We follow the calculation presented in Roy et al. (2022). For our application and general flight strategy, the roll angle is around 0°.

5) Line 207 " IWV retrieval is sensitive to the absolute calibration of the radar", I am not sure I agree with this. It is sensitive to the relative calibration between the two channels. If they are both biased the same results are unaffected, aren't they?. And isn't a way to cross calibrate the two radars at cloud top anyhow (like done in Tridon et al., Atmos. Meas. Tech., 13, 5065–5085, 2020 https://doi.org/10.5194/amt-13-5065-20203 and in your Sect 5.2) ? This is very relevant to be discussed also in relation to the discussion later on in Sect 5.1.

We removed the unclear sentence (was L 207) from the manuscript. We refer the reviewer to comment 12) below.

6) Line 235, not sure why a level of 1 dB is selected, probably better to use 0 dB? Also I would plot negative SNR as well

We chose this threshold based on the threshold presented in Roy et al. (2020).

7) Figure 2: maybe it is more significative to show relative errors in panel c) (and the radiosounding profile). Quality of figure must be improved (add grid).

We removed this Figure from the revised manuscript, and instead added a more thorough analysis and discussion of the water vapor profile uncertainty and sensitivity to R and t_{avg}. New Fig. 4 now shows the relative errors of the retrieved water vapor profile as function of R and t_{avg}, and Fig. 5 includes errorbars on the retrieved water vapor profiles.

8) Figure 3 and following. There are too many panels(mostly redundant). Replace panel d with b). Delete panel c) you can include the SNR as contour lines in panel a) (Just identify the levels that are of interest for the discussion).

We believe that it is important to show Z_e at 174.7 GHz to illustrate attenuation effects between the two frequencies. In order to reduce panels, we decided to move Ze precision and DAR signal to new Fig. 3 where there are more relevant to the discussion in the text.

9) Figure 5: I am really struggling in understanding what is the DWR plotted in the central panel. It seems it is not corresponding to the two reflectivites in the left panels.

We changed Fig. 5 substantially during the revisions. New Fig. 5 now illustrates the DAR DFR profile which corresponds to the difference of reflectivity Ze (167) – Ze (174) in dB units.

10) "Profiles of DAR and RS agree remarkably well, with deviations of less than 0.5 gm−3", I am not sure I agree with this statement. A deviation of 0.5 g/m^3 is a huge error (e.g. 50% for 1 g/m^3).

We removed this statement from the revised manuscript, and added a thorough analysis and discussion of the uncertainty of the water vapor profile retrieval to Sec. 4.1.

11) Figure 7: it is interesting to see that the instrument can record Doppler spectra but this should be used to derive some microphysical properties. The figure as it is is not really adding much. Basically all the interesting work (e.g. with multiwavelenght spectra) is left as future work. I would probably skip the figure.

Fig. 7 illustrates GRaWAC's spectral capabilities, and, for the first time, adds recorded Doppler spectra at 167.3 GHz to the literature. We agree with the reviewer that the analysis of multi-frequency spectra will provide lots of interesting microphysical details, but leave this for future work due to the main scope of the manuscript.

12) Sect 5.1: Fig10: it is unclear to me what are the initial few dbs of the differential reflectivity at the top of the profile and the decrease a little bit lower. What explanation can be provided? If it is volume mistmatch then you should avoid plotting this part of the profile. It seems that there is a retrieval of water vapour at such heights (I may be mistaken, that's why you need to put grids in plots!). If so, how is that possible? In general retrieval errors seem substantial to me.

In the presented case, negative DFR values occur mostly at cloud top, and around cloud *edges as illustrated in grey features in Fig. 1 (d) where we expect that super-cooled liquid layers or small ice hydrometeors prevail. These small hydrometeors would lead to Rayleigh scattering in both W and G-band frequencies, thus, a DFR W-G of around 0 (see e.g., Chellini et al., 2022).* In order to investigate a potential intercalibration offset between MiRAC W-band and GRaWAC G-band radar in RF05, we determine the DFR distribution of the three range bins adjacent to cloud top at each time stamp between 10:30 and 11:00 UTC. This time frame was chosen as flight altitude remained constant along the leg of the flight pattern. Fig. 2 illustrates that the resulting DFR distribution is well centered around 0 dB as expected when inter-calibration is good. Negative DFR values could occur due to volume mismatch resulting from different opening angles and the imperfect match of vertical and temporal resolution of both radar systems.

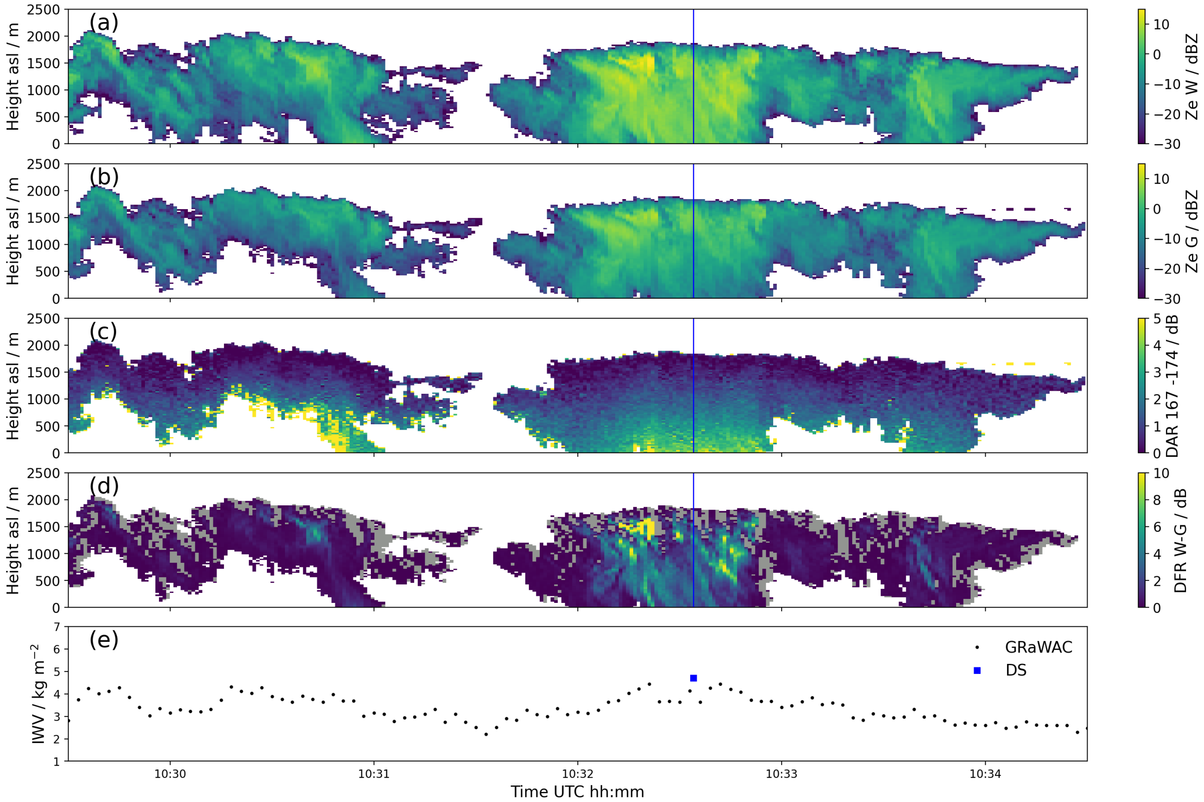

Figure 1 Time-range contour of (a) MiRAC and (b) GRaWAC Ze, (c) GRaWAC DAR, (d) W-G DFR, and (e) dropsonde (blue) and GRaWAC (black) IWV during RF05, 18.02.24. Time of dropsonde launch is marked in blue

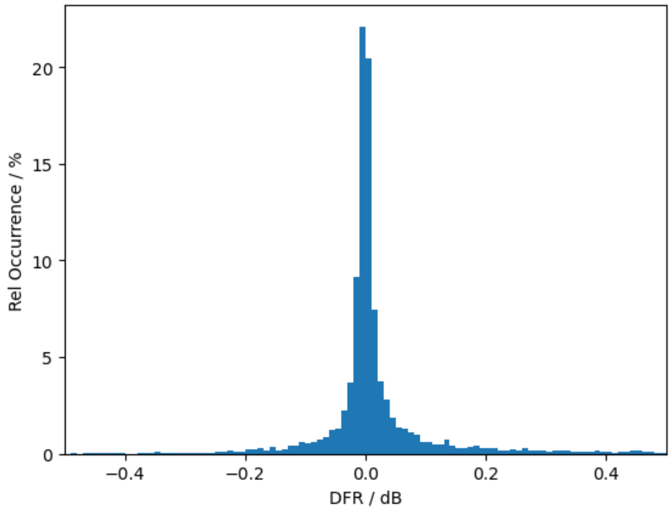

Figure 2 Distribution of W-G DFR around cloud top between 10:30 and 11 UTC during RF05.

Even though a potential intercalibration bias seems small to negligible in this particular case, we decided to remove panel (d) from the updated Fig. 10 as Sec. 5.1 focuses on the water vapor potential which is derived from stand-alone GRaWAC observations.

Conditions are different during RF06 analyzed in Sec. 5.2 where a thin ice-containing cloud layer is used to inter-calibration the two radar systems. Using this approach, we here find a mean intercalibration offset of -4.2 dB between W- and G-band radar. As the observed conditions do not allow for an inter-calibration from the ground return in clear-sky cases as proposed by Li et al. (2005), further investigation is necessary to quantify the bias using latest campaign data from the COMPEX field study. We added a thorough discussion on the intercalibration bias to the updated manuscript, LL 434-442.

13) Sect. 5.2: again not much quantitative insight in what is going on. Is this differential signal W-G due to differential scattering or extinction? Some clue from the sigma0 values? Can we size particles from DWR?

We thoroughly revised Sec. 5.2 following the reviewers' remarks around the inter-calibration of the systems, and added more information on the raised points to the analysis text.

Minor comments:

Line 260: for the retrieval at its minimal condition (rephrase)
Deleted in the revised manuscript.

Line 336: "at the time of launch" ==> at the time of the dropsonde launch
Done.

References

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