Answer to Reviewer #1

We thank the reviewer for their very helpful and constructive feedback which improved our manuscript fundamentally! 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.

Overall impression

This paper describes a new radar system "GRaWAC". This a G-band radar, of which there are only 3 or 4 worldwide, so that in itself makes the instrument relatively novel. The system is developed for Arctic field observations, and can be operated on the ground or on an aircraft, with examples of both shown here. Observations at two closely spaced frequencies near the 183 GHz water vapour absorption line allow retrieval of water vapour profiles in a similar manner to the technique previously demonstrated by Roy, Cooper, et al with the VIPR system at JPL, but this time in Arctic environments, which bring their own challenges (i.e. not much vapour!) and examples of such high-latitude retrievals are shown. The paper is in scope for AMT since it describes a new instrument which is of significant interest to the community and illustrates the kind of measurements it is capable of making. So I would like to see this paper published. However, I do think there are problems with the manuscript in it's current form which make it less impactful and convincing than it could be. So my suggestion would be major revision, to give the authors time to reflect on these issues and refine what is presented (assuming they agree with the comments below)

Primary comments

The paper does 3 things:

1. It describes the radar itself, with a moderate amount of technical detail

2. It describes the methodology for water vapour retrieval

3. It applies that methodology to illustrative cases, one measured from the ground at Svalbard, and others measured from an aircraft.

Item 2 has been established in previous papers elsewhere, and the authors are largely reviewing/reworking this technique here. Items 1 and 3 are new information to the literature, and are therefore the really important bits, and I would like to see a greater depth to the discussion of both of these elements, but in particular to the analysis of the observations.

For item 1 (the radar instrument part):

• Currently there is no description of the data acquisition and processing. How is reflectivity computed? Are the samples used to compute Doppler spectra, from which reflectivity is derived? Or incoherent averaging of received power? Is the background noise level subtracted? Any further filtering? These details are relevant for the water vapour retrieval application because to do profiling the dual frequency ratio (and therefore each reflectivity) needs to be measured to very high precision.

We thank the reviewer for this important remark and decided to add a new Subsection 2.3 “Data Acquisition and Processing” to fully address the raised remarks.

• Are other parameters measured / stored? I see Doppler velocity later on. What about spectrum width, or Doppler spectra, or I&Q samples? Useful to know the full capabilities.

During data acquisition, the full Doppler spectrum is stored at 167.3 GHz and 174.7 GHz. Integrating over the spectrum and using coherent averaging, Ze, as well as additional moments are computed (and stored) at 167.3 GHz like mean Doppler velocity, spectral width and skewness. I&Q samples can be stored optionally. We discuss the full details in the new Sec. 2.3.

• The radar is FMCW which can result in “range sidelobes”. Have you any information about the magnitude of these artefacts in GRaWAC, or any analysis of what kind of situations it is significant in? It looks like perhaps there is a hint of this in Figure 9b (horizontal line of low reflectivity at around 1200m range) and Figure 12a (thin line of low reflectivity at around 3000m, mirroring the echo from the surface)

We apply a Slepian type 2 FFT windowing technique to suppress range sidelobes to 47dB. The sidelobes become visible at an SNR>47dB. This is only rarely the case for rain events, but can be relevant for ground reflections and will be visible as ground return mirroring in measurements performed from aircraft.

• You note that there is an issue with the beams not overlapping at short range, and that this loss is calculated with some assumptions. Can you explain in more detail how this is corrected and the likely uncertainties? Also, do you worry at all about near-field effects?

For the correction of beam overlap, the beams are assumed to be Gaussian shaped and calculations are based on Sekelsky and Clothiaux (2002). More details can also be found in Rose, 2022. We added these references to the manuscript (L 104).

• Some practical information about operation of such an instrument in Arctic environments, and from aircraft would be useful, even if it is brief. I appreciate some of this may be similar to the approaches used previously for the W-band RPG instrument which is very successful, but worth mentioning, and noting anything that needed to be changed or tuned for operation at these higher frequencies.

We added some information in the introductory paragraph of Section 2 (L91). We additionally added the following sentence to Sec. 5 (L 361-363): *"For aircraft operation, the stand and blowers are removed from the radars before integration in the bellypod. No additional modifications are necessary compared to ground-based deployment in either radar."*

• You say in a couple of places that an extra frequency would help disentangle other differential effects (scattering etc), but that dual-frequency is

We are unsure how to address this comment.

For item 3 (the case studies)

• I think the paper would be much more impactful if the water vapour profiling was analysed and interpreted in greater depth. At the moment, we have estimates of ρv based on the difference between dual frequency ratio samples spaced 200m apart. These are then compared to radiosonde profiles. While the overall magnitudes of ρv are broadly comparable, the structure of the vapour profile is not particularly well correlated between the two measurements, and this did not give me great confidence in the results. I therefore found it confusing to read in several places that the profiles agree "remarkably well". So there are a few things to consider here:

We thank the reviewer for the very constructive and helpful feedback which helped to improve our understanding of the retrieval and its analyses a lot. While revising the manuscript, we edited Sec. 4 (ground-based analysis) fundamentally.

– First, the authors need to put error bars on their ρv profiles. This should not be difficult. You can predict the precision of a reflectivity measurement from the number of chirps, the spectrum width, and the signal to noise ratio (some care required in how this latter quantity is defined). If spectrum width is not available, a "representative" value could be chosen. The point here is that at the moment you can't assess whether the sonde and radar profiles are comparable, because you don't have uncertainties on either of them. So the extent to which agreement is good or bad, and how surprising or remarkable that is, can't be assessed. Once this is done you can go back through and have some more meaningful discussion / evaluation of the comparison and structures obtained.

We added uncertainties to the retrieved water vapor profiles based on Roy et al. (2018) and Battaglia and Kollias (2019), as well as Hogan et al. (2005) to estimate the precision of Ze. We added the description of error calculation to Sec. 3.1 (ll 189-196). As spectral width is not saved for the 174.7 GHz channel (see new Sec. 2.3), we assume the same Ze precision as in the 167.3 GHz channel in the uncertainty calculation. Based on the uncertainty calculations and analyses, we fundamentally revised Section 4.1.

– Related to this, there is currently a lot of emphasis in the paper about differential scattering and presence of liquid attenuation influencing the results. And in the discussion you say that these effects lead to "omnipresent" deviations (which you imply to be errors) of up to 2 grams per cubic metre, which seems quite large - this is pretty much the full magnitude of the signal you're trying to retrieve in some cases. It's not clear to me that this differential scattering & liquid attenuation is really a serious effect? How big is differential scattering likely to be in different scenarios - can you show some calculations with PAMTRA? 167 vs 175 GHz is less than 5% difference in frequency, so I would (perhaps naively) expect the difference in reflectivity samples to be quite modest. And for liquid attenuation - again you can evaluate this - but I find it hard to imagine that there is a big differential effect here, and of course is only restricted to narrow regions of the cloud where significant LWC exists. But without quantifying these things, I don't think you can interpret differences between retrieval and sonde as being caused by those effects.

Based on the reviewer's comment, we analyzed differential liquid attenuation and scattering, respectively, for the analyzed case study based on simulations.

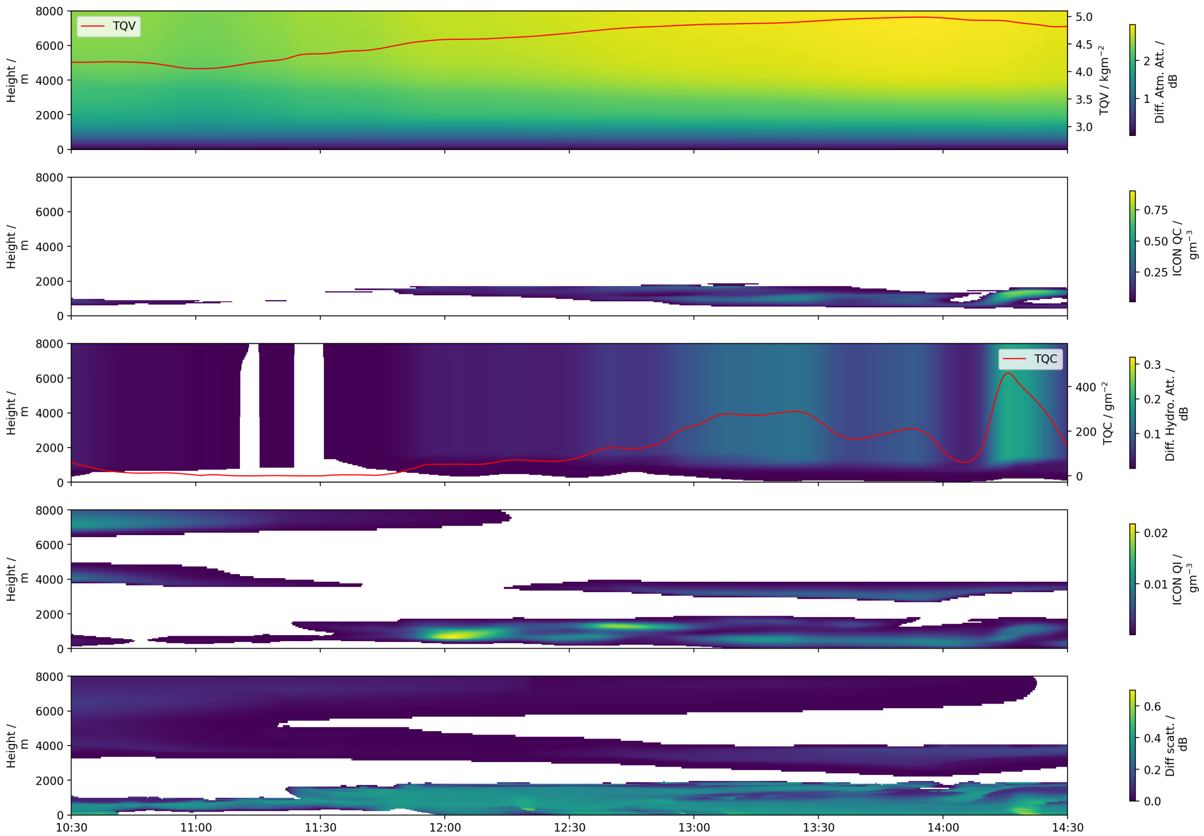

Figure 1 Time-Height contour of simulated (a) differential 2-way atmospheric attenuation 167-174 with ICON TQV (red), (b) ICON QC field, (c) differential 2-way liquid attenuation with ICON TQC (red), (d) ICON QI field, and (e) differential Ze 167-174 for Feb. 20th, 2025.

We used 3D-column meteogram output from continuously available ICON simulations (with 2-moment microphysics scheme, Kiszler et al., 2023) and forward-simulated radar Ze and hydrometeor as well as atmospheric attenuation using the Passive and Active Microwave TRAnsfer model PAMTRA (Mech et al., 2019; Ori et al 2020).

The simulations illustrated in Fig. 1 show that differential scattering is generally higher (up to 0.7 dB) than differential liquid attenuation (reaching values up to 0.3 dB with *TQC*=500gm^{-2}) in the analysed case. While differential liquid attenuation in this case occurs just below cloud top of the low-level mixed-phase cloud deck, differential scattering is dominant below 1 km where precipitation occurs. Even though observed cloud conditions of course might differ from the modeled scenario, the presented simulations give an idea of the relative impact of differential liquid attenuation versus hydrometeor scattering effects. We revised the analysis presented in Sec. 4.1 thoroughly, including the new Fig. 6 discussing differential scattering and attenuation effects on the water vapor retrieval performance.

– Related to the vertical structure the ρv retrieval: the precision of the profile depends on the precision of the reflectivity estimates, which in turn gets better as you average more chirps. Have you done this experiment? Particularly with the ground based measurements - why not average more, and get a more precise profile which you can have high confidence in? Then you can really compare in detail with the sonde.

During ground-based operation, we had GRaWAC measure at fixed integration times throughout deployment. Averaging the output Ze and DAR signals, respectively, to longer t_{avg} within the water vapor retrieval (see comment below) does not impact the uncertainty after a certain saturation value. We will repeat this experiment in future ground-based deployments by varying the measurement chirp settings (rather than purely varying the water vapor retrieval averaging time).

– You could also discuss more clearly about the impact of different vertical resolutions for the retrieval. I didn't feel that choosing this spacing because it minimised the "bias" between radar retrieved profile and radiosonde profile was necessarily a good reason. There is a trade-off here - coarser resolution has a bigger difference in DFR between the two samples, which is easier to measure = more precise estimate. But coarser resolution means loss of detail where there are sharp vertical features to resolve (e.g. airmass boundaries, inversions). It would be good to discuss this aspect more fully, and show what the retrievals look like for different choices, rather than just a residual (taking into account also the next comment - i.e. the radiosonde is not the truth!)

We added the description of the error calculations and its dependencies to Sec. 3.1 (see comment above). We replaced previous Fig. 2 with a new Fig. 5, analyzing the sensitivity of the retrieved water vapor profile and its relative uncertainty to t_{avg} and *R* parameters.

– I think it's important to acknowledge when comparing sonde to radar that the radar is a true vertical profile, integrated over some well-defined time/length scale; the radiosonde profile is collected over tens of minutes, during which time the sonde drifts by 10s or even 100s of kilometres (you can probably estimate what this scale is for your cases from the wind profile). So in that sense the radar is "better", or at least better defined in what it represents;

and also it means that any comparison between the two is confounded by the fact that they are sampling different parts of the atmosphere - this needs some discussion.

As shown in Figs. 1 and 2 in trajectories and wind speed profiles, the three radiosondes launched on Feb. 20, 2025, drifted in-land over the complex Svalbard orography. With wind speeds increasing up to 25 ms^{-1} at 5000m, it took the sondes approximately 20 minutes to reach 5000m, corresponding to approximately 13 km distance from AWIPEV station. As strong winds from the West dominate the free troposphere, deviations between GRaWAC point measurements and radiosonde profiles are expected to be small due to the column vs drift measurements. Given the wind profiles, we expect differences in the boundary layer due to the sampling of different air masses between GRaWAC and radiosonde observations. We added a discussion sentence to L 283 – 287: "Biases between radar-retrieved and radiosonde profiles can additionally arise due to the fact that fixed-column measurements by GRaWAC are compared to drifting in-situ balloon measurements. For the case shown, the radiosonde had drifted 13 km in-land at 5000 m altitude, due to prevailing strong west winds. Even though conditions over Kongsfjorden are not always horizontally stratified, we assume biases due to the co-location to be smaller than the expected bias due to the retrieval approach."

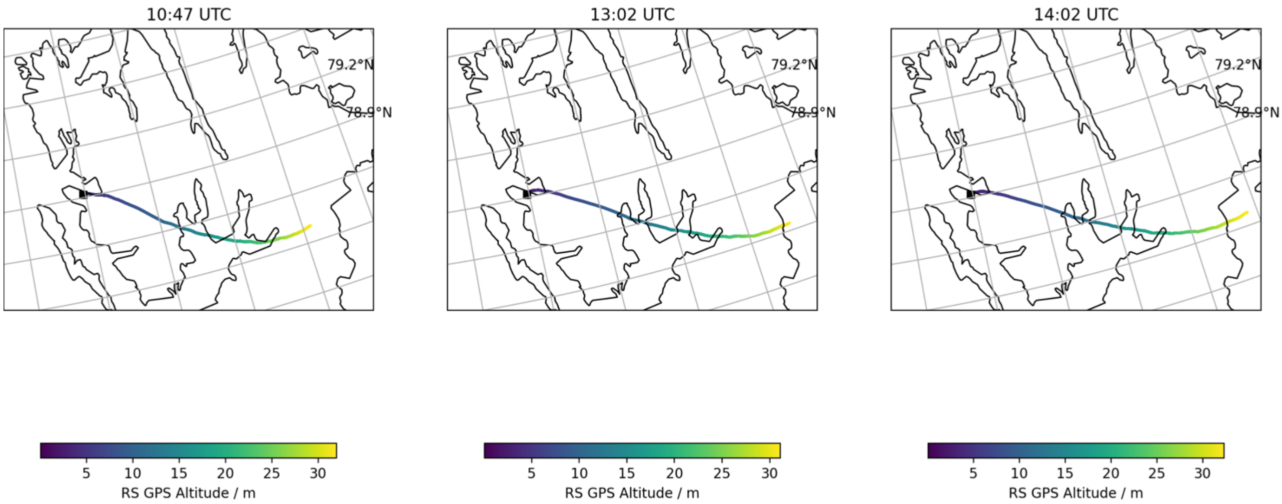

Figure 2 Trajectories of the three radiosondes discussed in Sec. 4.1, color-coded by GPS Altitude of the sounding.

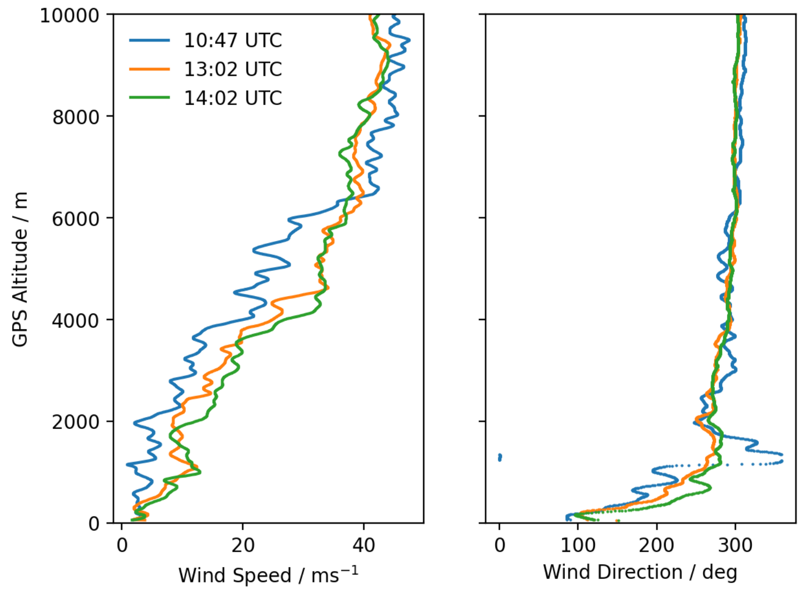

Figure 3 Profiles of wind (a) speed and (b) direction of three radiosondes launched on Feb. 20, 2025 at 10:47 (blue), 13:02 (orange), 14:02 (green) UTC at AWIPEV station, Svalbard.

Secondary comments

1. Line 96-99 I think there is some repetition here about the bistatic antennas, please have another look

We agree with the reviewer and removed the repetitive information from the respective paragraph.

2. Your block diagram mentions frequency multiplication and amplification. I guess the multiplication is ×24? Can you include that somewhere? Optionally, you could break down the multiplication and amplification stages.

We modified the block diagram (Fig. 1b) to clarify the amplification and multiplication stages.

3. There is some discussion about calibration of the radar receiver chain by passively viewing black-body targets. What about the transmitter chain?

We added the following information to the manuscript (L130): “*Transmitter power is measured at the horn flange and corrected for horn, antenna and subreflector-blockage losses.*“

4. In Table 1, units are indicated in the form “wavelength / mm”. Personally I find this notation ambiguous, and would prefer to see the units in brackets, or a separate column of the table. Also note that the units for the dimension are cubic metres but the dimensions provided are centimetres. Also it would be good to replace “gain” with “antenna gain”

Done.

5. Line 47 I would move the definition of optical depth slightly earlier, because you need it to understand equation (1)

Done

6. Line 165 - what is "variable hydrometeor occurrence" and why is it important in this context?

We clarified the corresponding sentence which now reads (L182): *"A differential scattering bias is introduced in the case of large, non-Rayleigh scattering hydrometeors, or when the hydrometeor population varies strongly in range, such as near cloud boundaries or in areas of phase change."*

7. Line 200 "argumentation" - change to "argument" (I think!)

Done.

8. Does equation 10 rely on the ground echo being independent of frequency?

The ground echo (Normalized Radar Cross Section) sigma_0 depends on the frequency as discussed in Roy et al., 2022.

9. Figure 3: can you add more axis labels on the height axis, and consider a background grid to help the reader locate the features you discuss in the text. You might also consider some annotation to help make it easier (e.g. to find the "mixed-phase cloud deck" on line 225 {what altitude?}, or the "thin cloud layer" on line 235)

We revised this Figure thoroughly, including a reduction of panels to improve readability. This Figure now corresponds to Fig. 2.

10. Also figure 3, can you refine the colour scales to bring out more detail. SNR range is 0-40dB but I can't see any features above ~20, and it's hard to see what's going on. Reflectivities seem to be all >-30, so no need to go all the way down to -50 on the current scale. For DAR (a key measurement for your paper's focus) the only features in the yellow colour are ones which are affected by very low SNR, which I would suggest you filter out from this panel, and then tighten the scale down to 0-10dB or something like that (based on the 1D profiles in Figure 5)

Done. This Figure now corresponds to Fig. 2.

11. Line 240 should "Saturation" be "Supersaturation"?

Done.

12. Line 268 - you talk about Boundary Layer moistening - where is the Boundary Layer in this case (and how diagnosed?)

We identify the boundary layer from the radiosonde profiles as the bottom of the temperature inversion, aligning roughly with cloud top. We added the following sentence to the manuscript (L250): *"Radiosonde temperature profiles reveal a temperature inversion just above cloud top, indicating the moist layer separating from the free troposphere above."*

13. Figure 4 - useful to add more minor ticks/axis labels on x-axes of these panels and perhaps grid lines

We removed this Figure from the manuscript.

14. Line 290 and thereabouts: you interpret these spectra in terms of different peaks - how did you diagnose what hydrometeors are dominating each peak?

We based our analysis on findings presented in Shupe et al. (2004), and additionally investigated the hydrometeor distribution in ICON simulations (see comment above) as no in-situ reference observations are available. In the simulation, the mixed-phase nature of the investigated cloud is clearly represented with a super-cooled liquid cloud layer prevailing just below cloud top where specific humidity drops. *QC* increases from t_0 to t_2. Ice is distributed throughout the entire cloud layer at all time steps with *QI* peaking at 700m. *QI* is highest at t_1, and drops to half the amount at t_2.

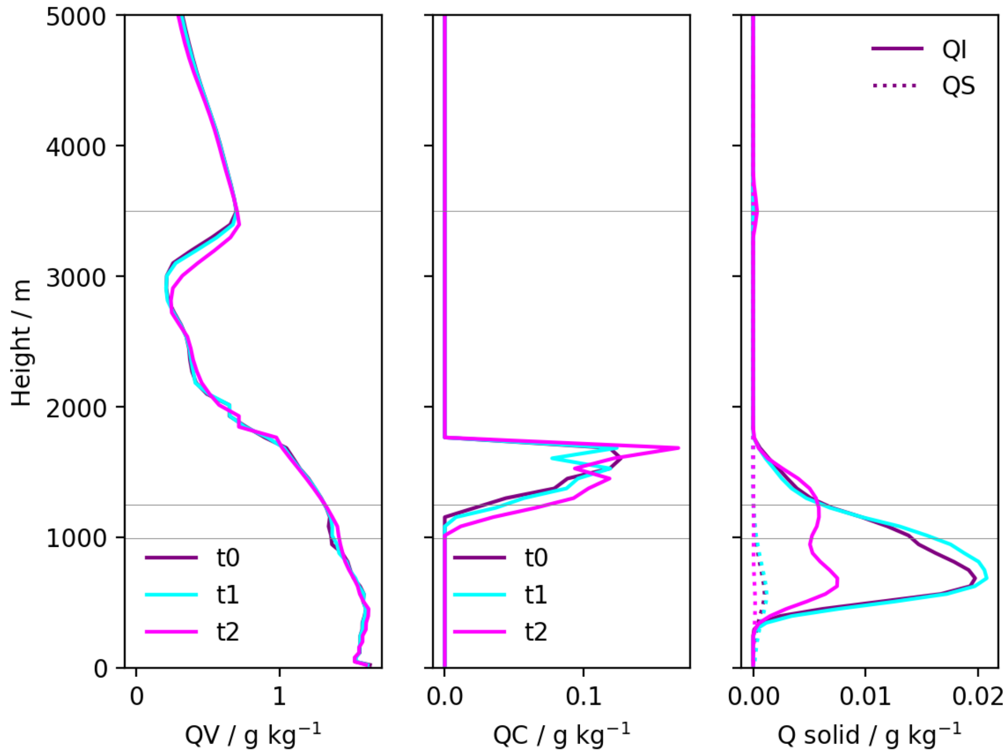

Figure 4 Profiles of ICON-simulated (a) QV, (b) QC, (c) QI (solid) and QS (dash) for t_0 11:59 UTC (purple), t_1 12:04 UTC (cyan), t_2 12:20 UTC (magenta) on Feb 20, 2025.

Based on these results, we revised the corresponding text in the manuscript which now reads (LL 341 - 348): "*The upper cloud layer is dominated by downward motion around -0.7 ms^{-1}. Due to the temperature at this height (≈ 250 K in sounding profile at 13:02 UTC) and the recorded negative motion, we relate this peak to the presence of ice hydrometeors. The upper layer of the low-level mixed-phase cloud deck is more complex to analyze. At 11:59 UTC, we hypothesize that the peak centered at 0.0 ms^{-1} at both 1250 and 990 m corresponds to super-cooled liquid hydrometeors. A peak forming at 1250 m centers at -0.6 ms^{-1} at 12:04 UTC, and further broadens at 990 m, presumably relating to the presence of ice hydrometeors. Spectra* *recorded at 12:20 UTC clearly illustrate the mixed-phase nature of the cloud deck as a bi-modal shape of the spectrum emerges (e.g., Shupe et al., 2004).”*

15. Figure 12 can you change the range axis to a height axis, based on the altitude of the aircraft? I found this figure difficult to interpret.

As flight altitude changes throughout the depicted time line, we prefer illustrating the measurements in range coordinates. Given the added air-borne intercalibration discussion (see reviewer #2), we modified Fig. 12 correspondingly.

References

Battaglia, A. and Kollias, P.: Evaluation of differential absorption radars in the 183GHz band for profiling water vapour in ice clouds, Atmospheric Measurement Techniques, 12, 3335–3349, https://doi.org/10.5194/amt-12-3335-2019, 2019.

Chellini, G., Gierens, R., Ebell, K., Kiszler, T., Krobot, P., Myagkov, A., Schemann, V., and Kneifel, S.: Low-level mixed-phase clouds at the high Arctic site of Ny-Ålesund: a comprehensive long-term dataset of remote sensing observations, Earth System Science Data, 15, 5427–5448, https://doi.org/10.5194/essd-15-5427-2023, 2023.

Kiszler, T., Ebell, K., and Schemann, V.: A Performance Baseline for the Representation of Clouds and Humidity in Cloud-Resolving ICON-LEM Simulations in the Arctic, Journal of Advances in Modeling Earth Systems, 15, e2022MS003299, https://doi.org/10.1029/2022MS003299, 2023.

Küchler, N., Kneifel, S., Löhnert, U., Kollias, P., Czekala, H., and Rose, T.: A W-Band Radar–Radiometer System for Accurate and Continuous Monitoring of Clouds and Precipitation, Journal of Atmospheric and Oceanic Technology, 34, 2375–2392, https://doi.org/10.1175/JTECH-D-17-0019.1, 2017.

Mech, M., Maahn, M., Kneifel, S., Ori, D., Orlandi, E., Kollias, P., Schemann, V., and Crewell, S.: PAMTRA 1.0: the Passive and Active Microwave radiative TRAnsfer tool for simulating radiometer and radar measurements of the cloudy atmosphere, Geoscientific Model Development, 13, 4229–4251, https://doi.org/10.5194/gmd-13-4229-2020, 2020.

Ori, D., Schemann, V., Karrer, M., Dias Neto, J., von Terzi, L., Seifert, A., and Kneifel, S.: Evaluation of ice particle growth in ICON using statistics of multi-frequency Doppler cloud radar observations, Quarterly Journal of the Royal Meteorological Society, 146, 3830–3849, https://doi.org/10.1002/qj.3875, 2020.

Rose, Th., 2022: RPG-FMCW-94 Cloud Radar (Operation and Software Manual), https://www.radiometer-physics.de/download/PDF/Cloud%20Radar/RPG-FMCW-Operation%20and%20SW_Manual.pdf *last access: 11.05.2026*

Roy, R. J., Lebsock, M., Millán, L., Dengler, R., Rodriguez Monje, R., Siles, J. V., and Cooper, K. B.: Boundary-layer water vapor profiling using differential absorption radar, Atmospheric

Measurement Techniques, 11, 6511–6523, https://doi.org/10.5194/amt-11-6511-2018, 2018.

Roy, R. J., Lebsock, M., Millán, L., and Cooper, K. B.: Validation of a G-Band Differential Absorption Cloud Radar for Humidity Remote Sensing, J. Atmos. Oceanic Technol., 37, 1085–1102, https://doi.org/10.1175/JTECH-D-19-0122.1, 2020.

Roy, R. J., Cooper, K. B., Lebsock, M., Siles, J. V., Millán, L., Dengler, R., Monje, R. R., Durden, S. L., Cannon, F., and Wilson, A.: First Airborne Measurements With a G-Band Differential Absorption Radar, IEEE Transactions on Geoscience and Remote Sensing, 60, 1–15, https://doi.org/10.1109/TGRS.2021.3134670, 2022.

Sekelsky, S. M. and Clothiaux, E. E.: Parallax Errors and Corrections for Dual-Antenna Millimeter-Wave Cloud Radars, Journal of Atmospheric and Oceanic Technology, 19, 478–485, https://doi.org/10.1175/1520-0426(2002)019<0478:PEACFD>2.0.CO;2, 2002.

Shupe, M. D., Kollias, P., Matrosov, S. Y., and Schneider, T. L.: Deriving Mixed-Phase Cloud Properties from Doppler Radar Spectra, Journal of Atmospheric and Oceanic Technology, 21, 660–670, https://doi.org/10.1175/1520-0426(2004)021<0660:DMCPFD>2.0.CO;2, 2004.