Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-14-RC2, 2019

Atmospheric Measurement Techniques Discussions

Interactive comment on "Evaluation of differential absorption radars in the 183 GHz band for profiling water vapour in ice clouds" *by* A. Battaglia and P. Kollias

Anonymous Referee #2

Received and published: 26 March 2019

This paper develops an idea from previous studies to evaluate the potential benefit of a differential absorption radar system near 183GHz to measure water vapour in ice-phase clouds. This is an important application, and the technology and idea are new and emerging so it is timely to do this kind of study.

What this paper does well is delivery of the "headline" results that: retrieval of humidity in ice cloud is possible; that the precision of RHice is at a level where it is microphysically useful; that looking down from space gives quite different sensitivities and error characteristics than looking up from the ground.

What the paper does not do so well are:

(i) placing this work in the context of what has been done before. Previous papers are reviewed briefly, but what is not so clear is the exact point of departure of this paper vs the previous studies. In particular I would like to see the current paper clearly distinguished from the previous papers by Roy et al and Millan et al, because these both consider water vapour retrieval in the presence of cloud and precipitation, and the Millan study includes ice which is also your focus. It needs to be clear (a) what aspects of the retrieval methodology are new, and (b) what aspects of the way the performance of the retrieval are analysed are new

(ii) clearly explaining the sensitivity of the results to assumptions made, or the generality of the conclusions drawn. For example a particular ice particle scattering model is assumed, and some linear fit between dB of attenuation (per unit IWC, per unit path length) vs frequency is made A+Bf. What is not explained are - the justification for that scattering model; the change in the results if you'd made a different choice; whether the numerical values of A and B matter, or just the fact that the behaviour is linear.

I would like to see this paper published, but I would also like to see it revised significantly and expanded to address some of these points.

Some specific comments:

In the abstract you mention an airborne system in the same breath as a spaceborne system, and in the paper you concentrate on the satellite option. But these two setups are actually quite dissimilar. A satellite moves at about 7km/s relative to the earth's surface, which means even relatively few pulses lead to quite large pixels. A research aircraft flies at speeds of about 100m/s - seventy times slower. So in principle you could do a lot more averaging of pulses, which improves your number of independent samples. It would be interesting to consider a separate "airborne" scenario in addition to your ground and space views.

Page 2 line 25. "...coupled with that of temperature" - I agree with the other reviewer that the impact of not knowing T perfectly (e.g. estimating from reanalysis/forecast model with 1 or 2K typical uncertainty) needs to be discussed somewhere.

Page 2 line 28. You start talking about supercooled LWC here. Is this factor included in the retrieval? or does that 0.5dB differential not matter relative to the size of signal you are estimating?

Page 3 line 2 "could help us understand how the ice crystal grow significantly enhance water mass fluxes due to sedimentation" - this sentence needs rewording

Page 3 line 5, and figure 1. You have constructed some sort of Magono-Lee style diagram in the figure here, but this is not a particularly accurate representation of our current state of knowledge. Specifically, at temperatures below -20C or so the crystals are almost always polycrystalline, which may be in the form of multiple plates, or bullet rosettes (the column polycrystal form). This was recognised a long time ago (e.g. Aufm Kampe et al 1951 J. Meteorol.) and has been reiterated by e.g. Bailey and Hallett (2009 JAS). Minor points - one of your images, which I suspect is meant to show a column, actually shows a capped column. This happens when a column is transported (by convection, or sedimentation) to a temperature favouring plate growth. This touches on a problem for your idea of using T,RHice to constrain the crystal types in the cloud - in a deep ice cloud like your case study in figure 3, particles are growing and falling through temperature changes of 10s of K across their lifetime. The habit diagram in figure 1 is for isothermal growth in the lab. Connecting the two is not simple. Indeed in your retrieval you actually assume the particles are aggregates... which do not even appear on your habit diagram...

Figure 1: A simple, but useful exercise to add here, would be to give some indication of how the growth rates of the crystals depend on RHice and T. You could show indicative calculations for a few temperatures and crystal size of a few hundred microns. Then you could perturb the calculation by your expected uncertainties (q +/- 3%) and see how the growth rate is affected. Ultimately the uncertainties will be most important for RHice close to 100%, and the more sub- or super-saturated you are, the less significant

they will become.

Page 4 line 14. Multiple scattering neglected because footprint is small and SSA is low. Can you foresee any scenario where these would become significant? I'm surprised CloudSat suffers from multiple scattering at 94GHz, yet this G-band system would not, especially if there is a substantial optical depth to the cloud

Page 5 line 7 and Figure 2. This calculation needs explanation. What are we assuming here and why? Various Dm values are shown. What size distribution is assumed here? The caption tells us this is from Leinonen and Szyrmer's study, but with "LWC=0" - so I infer no riming. It would be better to explain these things directly. Then later on, you say you are using SSRGA for the ice scattering. This seems to be a contradiction? Are you somehow using both? Or SSRGA tuned to L&S's aggregates?

Figure 2: I was quite surprised both at the amount of attenuation here, and its frequency dependence. What physical effect is driving this - absorption or scattering? Is there any sensitivity of this to the scattering model assumed?

Figure 4 - show ground based simulated Z's here as well, to match figure 6

Page 11. It could be useful to explore the trade off between long vs short pulses. Long pulses = better SNR which seems to be a critical parameter. But shorter pulses = more range gates = better fit to attenuation profile (and implicitly more independent pulses in total)?

Page 12 ground based system assumed to be at 270K level. Why? Are the data from everywhere in globe, or a particular latitude band? I would expect the performance trade offs you touch on to change a lot between the tropics and polar regions

The discussion of figure 8 is very brief. Please expand and carefully explain what you want the reader to take from this, and how the plot supports that argument

Minor points

I'm not a spectroscopy person, but the use of "left" and "right" in relation to the wings of the absorption line seemed odd. I guess you are referring to higher and lower frequencies? or is the diagram in your head in wavelength (in which case, it's the opposite way around!)

Page 9 line 8 tidy up brackets] -> [

Figure 5. When I printed this half the figure disappeared. I'm not sure if this reflects an underlying issue with the figure file. I could view it OK on the screen however. Note that in the lower panel, the legend for the 94GHz profile is wrong (cyan line rather than black x)

Figure 8 caption - dashed region -> shaded region

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-14, 2019.