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Interactive comment

Interactive comment on "Microphysical Properties of Three Types of Snow Clouds: Implication to Satellite Snowfall Retrievals" *by* Hwayoung Jeoung et al.

Stefan Kneifel

skneifel@meteo.uni-koeln.de

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I have a couple of questions and comments regarding the manuscript under discussion submitted by Jeoung et al. entitled "Microphysical Properties of Three Types of Snow Clouds: Implication to Satellite Snowfall Retrievals".

I find this an interesting study, which uses ground-based remote sensing observations of snowfall to investigate its implications for satellite retrievals. However, I think some important corrections and processing steps are not described well or even taken into account as far as I could see:

1) Impact of snow scattering on the passive 89 GHz channel

The authors mention that the brightness temperature (TB) at the 89 GHz channel is mainly driven by the atmospheric gas emission, liquid water emission and scattering of surface radiation back to the radiometer. I completely agree, but I am missing how you corrected for the TB enhancement due to the snow above. In my own simulations and measurements (see Kneifel et al., JGR, 2011), I found that the TB at 90 GHz can be enhanced by 5 K for an SWP of only 200 g/m^3 (assuming the Liu, 2008 sector snowflakes). Not correcting for snow scattering is therefore likely causing a bias in your LWP estimate which increases with SWP. I think this issue needs to be discussed.

I was also wondering why you don't forward simulate the ground-based passive 89 GHz TBs based on your retrieved snow profiles. You only simulate GMI but you could do the same for the ground-based looking-upward perspective and provide a consistency check in that way.

2) Correction for radar attenuation

Maybe I missed it but I couldn't find how you corrected for radar attenuation due to gases, liquid water and snow? Water vapour and oxygen can easily add up to 2-3 dB two-way attenuation at W-Band. Even worse is super-cooled liquid water whose absorption properties also depend on the absorption model used and are roughly causing 7 dB (two-way) for an LWP of 1kg/m^2. Also snow and ice can contribute substantially to attenuation at W-Band (see for example Tridon et al., AMT, 2020). All these contributions to W-Band attenuation will affect (bias) your SWP and SR estimates and I think have to correct for it.

3) Radar calibration

Recent studies showed that radar calibration is an issue for many cloud radars (for example Kollias et al., AMT, 2019). How was the W-Band radar calibrated? Using rainfall as suggested by the manufacturer (Myagkov et al., AMTD, 2020)? Also, how has the passive channel been calibrated? Maybe a reference to Küchler et al., JTECH, 2017 which describes this radar-radiometer design would also be useful for the reader.

4) Liquid water retrieval

What liquid water refractive index model did you use for your LWP retrieval (m in Eq. 4)? For super-cooled temperatures the refractive index models deviate substantially, especially the very old models (see for example the comparisons in Kneifel et al., JAMC, 2014).

5) Cloud Top Estimate

I find your approach with spectrum width interesting. However, I have several questions related to Fig. 2 were your "prove" that your method works. 1) Why is there a spectrum width different from zero in the upper regions where there is no cloud? If you properly remove the noise level, spectrum width should be zero, or? 2) Why are there horizontal bands of enhanced spectrum width and how do they influence other radar moments? 3) Why don't you use SNR in order to judge whether your Ze measurement is reliable? 4) Wouldn't your approach become much more stable if you combine it with a Ze threshold? 5) The radar adjusts it's output power in case of strong precipitation. This will likely affect your sensitivity profile and hence ability to identify the "true" cloud top. It might not be so relevant for your classification but it might be worth mentioning.

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