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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Performance assessment of a triple-frequency spaceborne cloud–precipitation radar concept using a global cloud-resolving model" *by* J. Leinonen et al.

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We thank reviewer #2 for the helpful comments. Please find our point-by-point responses below each comment in italics.

This is a nice manuscript that will instigate constructive conversations within the spaceborne radar community about future mission capabilities. The manuscript is well-written and organized in a logical fashion. I recommend publication after some minor issues are resolved.

Point to consider:

1. The authors justify the ice model choices used for each frozen hydrometeor category, and their choices are logical given the plethora of possible models that could be used. Scattering characteristics between different ice models can be significant, however, and there might be significant sensitivity to the results presented based on the microphysical model assumptions. Can the authors provide an example of how results may differ if a different snow or ice model is used for some of the ice and snow categories? For instance, the NICAM simulated reflectivity PDF shown in Fig. 2 might appreciably change with different scattering models. The rosette model is used for cloud ice, and this choice is justified by previous results from Geer and Baordo (2014). But I'm not sure about the snow category. What happens to the reflectivity PDF if the snow category is changed to a different type of aggregate or dendrite model? Maybe there isn't much sensitivity in the reflectivity PDF on a global basis.

The snow model is indeed probably the largest uncertainty in the radar forward model. However, evaluating it is complicated due to the lack of availability of scattering data. The Nowell et al. (2013) dataset only contains aggregates made of bullet rosettes. Although rosettes of two different sizes are included, they probably cannot be expected to be different enough to conduct a proper sensitivity analysis. The dataset published by the lead author in Leinonen and Moisseev (2015) contains many types of aggregates, but the published data only contains backscattering cross sections, so it cannot be used to run the forward model used in this paper, which also requires extinction and absorption cross sections. While those could be derived from the raw dataset, that would require additional quality control and verification that would, in our opinion, be too extensive to cover in this, already lengthy, paper. Furthermore, the Leinonen and Moisseev (2015) study provided some evidence that the choice of crystal type is not critical to aggregate models.

That said, some insight into the sensitivity can be gained by comparing the results for the current model to those given by an alternative, spheroid-based snow scattering model. In the attached figures, we have provided the dB difference in reflectivity for the *vertical cross section of the tropical cyclone case.*

In these figures, negative bias (blue) indicates that the spheroid model produces lower reflectivities than the aggregate model. The spheroid model uses the mass-dimensional relaitionship for "aggregates of side planes, bullets and columns" by Mitchell, D. L., R. Zhang, and R. L. Pitter (1990), Mass-dimensional relationships for ice particles and the influence of riming on snowfall rates, J. Appl. Meteorol., 29(2), 153–163, doi:10.1175/1520-450(1990)029<0153:MDRFIP>2.0.CO;2. It can be seen that the difference between these models is considerable. Moreover, even the Ku-band exhibits a significant difference, meaning that much of the bias is due to the difference in mass-dimensional relationships rather than the different morphologies. It is not immediately clear, though, how much this reflects on the difference between backscattering cross sections of different aggregate types.

2. Fig. 2: Approximately how many CloudSat samples were used to create the PDF? The authors provide the time period used to construct the PDF, but a comparison between the total number of global observations between CloudSat and NICAM would be interesting to know.

The total number of valid reflectivity measurements used is approximately 10^8*, this is now stated in the first paragraph of section 3 (along with the equivalent numbers for GPM).*

3. Figure 3a: How is this image generated to produce clouds of differing brightness? I know this is a qualitative image to provide the reader with a better context of the modeled global cloud distribution. It is a very nice visual addition to the manuscript. Is the sun assumed to illuminate all sectors of the globe at a constant solar zenith angle with brightness differences due to cloud microphysical differences? Was an idealized VIS RT simulation performed to obtain this image? A descriptive sentence to explain how this image was generated would be beneficial.

A short description of how this image was generated was added to the first paragraph

of Sect. 5.1.

4. Is there any reason not to use current CloudSat and GPM radar characteristics to demonstrate possible improvements in cloud and precipitation detectability using the radar configurations used in this study?

We have added a paragraph at the end of Sect. 5.1. that briefly describes the improvements in detection over GPM and CloudSat.

5. Figs. 4, 7, 9, 11, 13, 15: It is difficult to read the red wording at the bottom of each panel.

Among other improvements to the bar plots, the legibility of this text has been improved.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 4137, 2015.

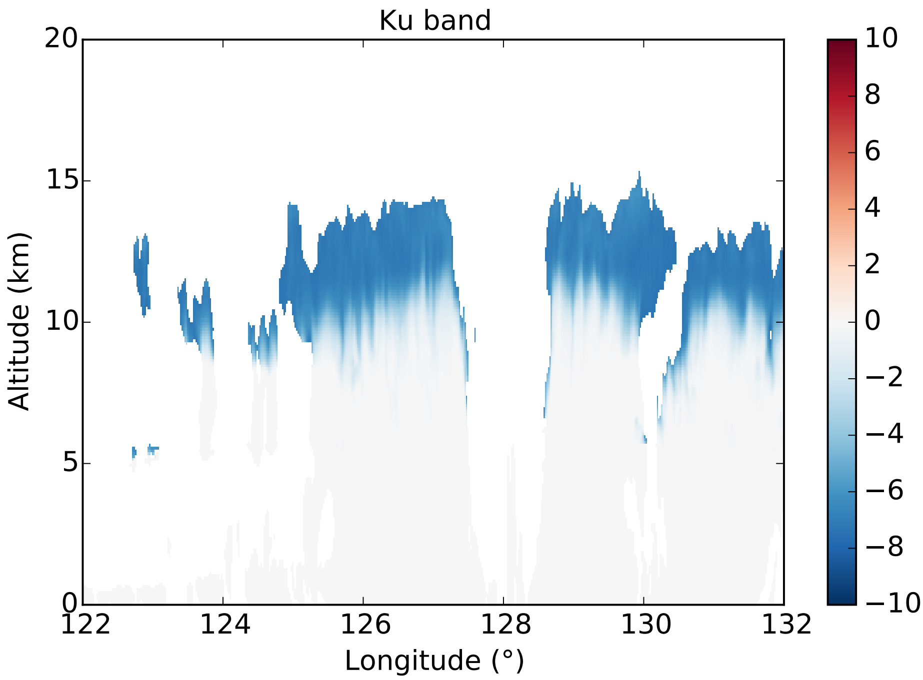

Fig. 1. Ku-band reflectivity difference between detailed rosette aggregate vs. spheroidal snow models.

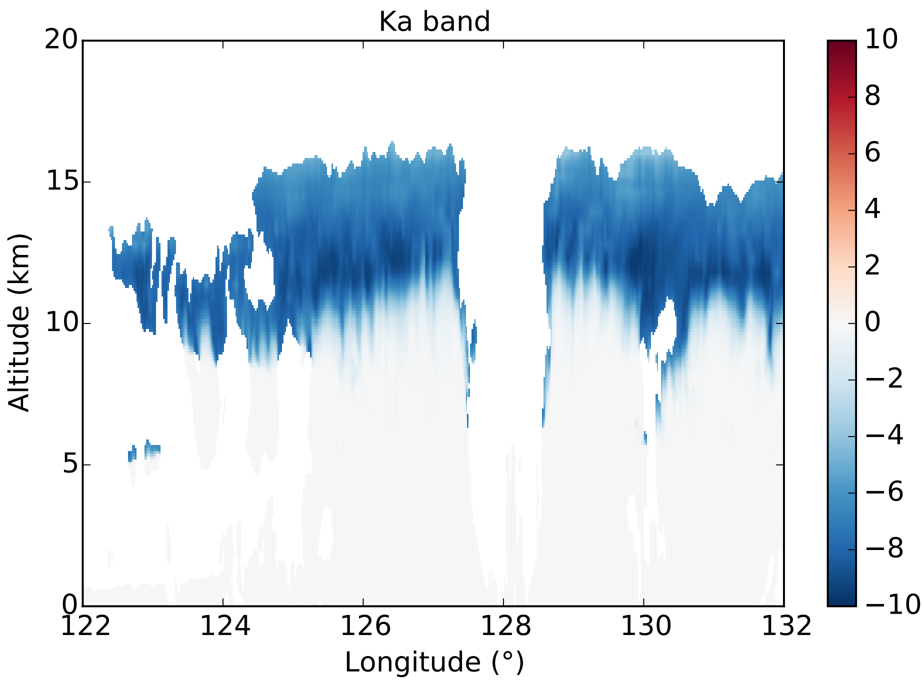

Fig. 2. As Fig. 1, but for the Ka-band.

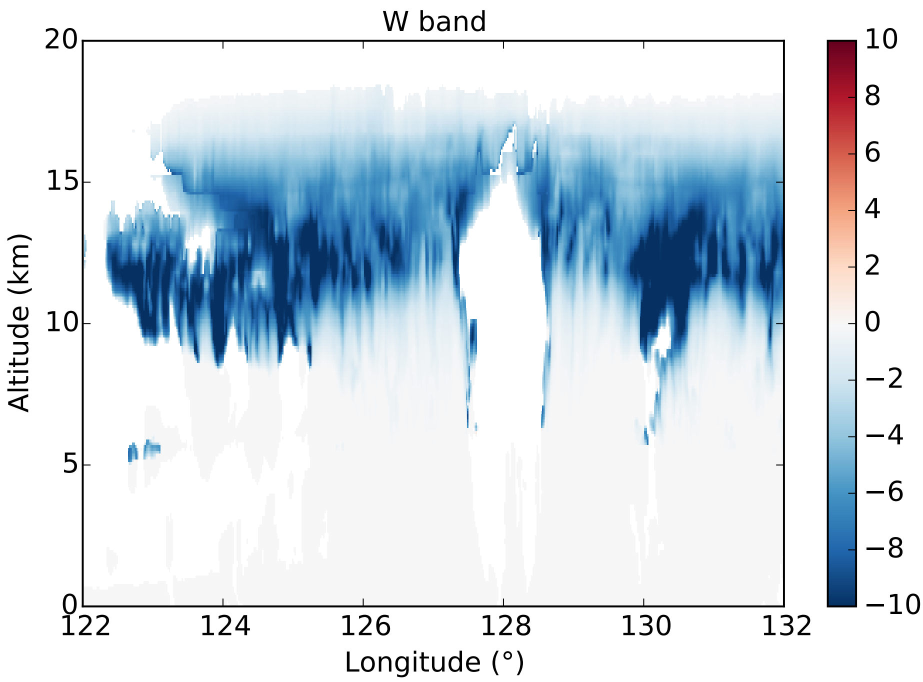

Fig. 3. As Fig. 1, but for the W-band.