Review comments for "The first microwave and submillimetre closure study using particle models of oriented ice hydrometeors to simulate polarimetric measurements of ice clouds" by McCusker et al.

This manuscript provides a closure study on cross-instrument consistency of the observed ice microphysics from in-situ and inferred ice microphysics from remote sensing measurements using suborbital campaign collected data. The main science goal is to understand to what degree the V-H polarization difference (PD) signal from sub-mm channels, in particular, 243 and 664 GHz from the ISMAR instrument, is induced by oriented ice particles that are realistically observed by in-situ cloud probes and Ka band radars. With the fully polarized simulation realized by ARTS and its scattering database using the observed particle shapes, the authors found the 243 GHz PD and TB can be largely reproduced in different cloud regimes, but the largest PD signals have to involve a few percent (10-50% of the bottom layer) of dendrite monomers. Given the identical set-up, the simulated 664 GHz PDs however are too large and TBv is too warm. The authors speculated several possible reasons to explain such a discrepancy (too noisy; ice might not be dominantly horizontally oriented; particle habit incorrect, etc.)

Overall this is a very nice and informative paper that I strongly encourage publication on AMT. The experiment design was thoughtfully crafted to make sure to use as much as collocated data as possible, and the level of details paid toward the execution and documentation are highly appreciated. The major conclusions are solidly supported by evidence.

Before publication, I think some minor issues can be fixed or improved. I'll explicitly say "optional" if the additional work is not necessary to complete this paper, but would be otherwise "nice to have" to help enhance the science impact of this paper.

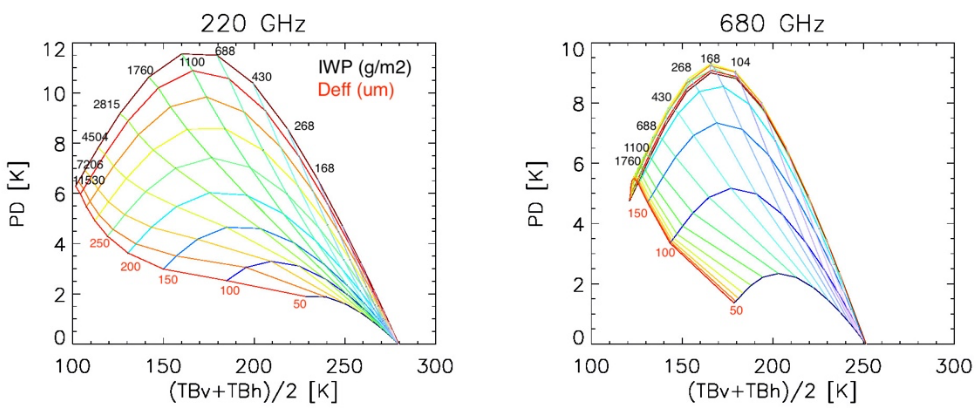
Major suggestions:

1. The falling speed (i.e., terminal velocity) should be different for dendrite monomer vs. dendrite aggregates. You have W-band Doppler radar but the vertical velocity data were never used. Can you check to validate your finding using the Doppler velocity?
2. I scrutinized your Fig. 7d and found for TBv in the range of 245 to 235 K, there are two groups of PDs. The larger PD group seems to correspond to latitude = 58 to 58.5 deg and latitude = 57.5 – 57.2 deg. You have ground precipitation radar (Fig. 1) that you can check against, e.g., Kdp, Zdr, for orientation signal, as well as the connection to surface precipitation type and intensity. It's very interesting that we can see the cloud touches ground in these two latitude bands (i.e., precipitation) that I don't know if can help you infer more connections between oriented dendrite (as opposed to dendrite aggregates) and surface precipitation properties.
3. For interpretation of the 664 GHz PD discrepancy, I think other than instrument noise issue (that I had an impression was fixed for 664 GHz for some other flights?), the observed TB-PD relationship is much more scattered compared to the tight 243 GHz relationship. For smaller particles up in Layer 1-3, they tend to be less impacted by the aerodynamics but more by temperature and humidity. This is supported by findings using CALIPSO lidar, which sees

much fewer oriented ice than microwave sensors (e.g., Noel and Chepfer, 2010; Zhou et al., 2012).

References:
Zhou et al. (2012): https://doi.org/10.1175/JAMC-D-11-0265.1
Noel and Chepfer (2010): https://doi.org/10.1029/2009JD012365

4. The scan pattern of the in-situ cloud probes can be better elaborated. For example, you can overlay the flight level of the aircraft carrying the cloud probes. If the particles are collected at different altitudes and cloud regimes throughout this leg, what lead you to think the columnar and dendritic aggregates are representative?
5. For Fig. 10 and related text on 664 GHz discrepancy, I'd suggest you carry out a 100% random orientation simulation for columnar aggregates, but 100% horizontal orientation for dendrite aggregates below, as a reference to your possible TBv and PD range. That would be helpful to support some of your arguments.
6. I have some doubts regarding using 100 um as the cut-off. Although I agree with you that they shouldn't contribute much to the PD signal, 243 and especially 664 GHz are still sensitive to particles smaller than 100 um. See one of my simulations (not exactly the same frequency but close) below also using TC4 and 100% oriented columnar aggregates. The possibility on 664 GHz discrepancy induced by this cut-off should be discussed in the context.



Minor suggestions:
Section 2.2, paragraph 1: some summary on in-situ instrument limitations and retrieval uncertainties needed.

Line 208-209: are the Nevzorov probe sensitive to the same size range of ice particles compared to your Ka and/or W band radards?

[optional] IMA as an approximation to DDA: Since you have to use DDA to simulate dendrite monomer scattering, there's a discomfort feeling of inconsistency here. Although you cited your previous paper on demonstrating that IMA is a good approximation, is it for the same particle shape and size range and frequency? It's better to add a baseline comparison for DDA result compared to IMA for a simplified setting here.
Line 252: explain the meaning of "k".

Line 372-375: This paragraph needs clarification. Why don't you use direct trajectory as the timeseries instead of using the latitude bin? I understand the original timestamp will lead to too many noise and non-robust signal, but your design is only valid when assuming the clouds don't change within a given latitude bin. Does this flight scans back and forth on the same trajectory which is perpendicular to latitudes? This was never explained very well and maybe it should be explained in more detail when you introduce Fig. 1.

Line 512: this is contradictory to your statement in Line 425.