Reviewer 1:

Summary: In this study, the authors attempt to consistently simulate radiometer and radar measurements of ice particles using in situ aircraft data. Based on the in situ particle imagery, aggregates of columns and dendrites are generated at different levels of the atmosphere and used in the radiative transfer simulations, along with the particle size distributions and derived mass-size relations. The simulated brightness temperatures and polarization differences are roughly in agreement with the corresponding aircraft measurements. Simulations with the addition of oriented dendrites provided better correspondence between the simulated and observed polarization differences.

Overall, this is a very interesting study that I believe makes progress in more consistently simulating physical and radiometric properties of ice precipitation. However, there are a number of specific points in the manuscript (outlined below) that should be clarified and potentially expanded upon before it is accepted for publication.

Specific comments:

Line 104: Please add the power for the 95 GHz radar.

OK, I have added that the power of the 95GHz radar is 1.8kW.

Line 155: Clarify whether this preferential alignment includes some canting/wobbling.

As there is no information to constrain the choice of a particular canting angle, we do not make any such assumption in the particle generation process. The particles have a random orientation upon generation. They are reoriented based on the maximum moment of inertia, such that the maximum distribution of mass is in the horizontal plane. We have now noted this in section 3.1.

Line 156: Are 3D effects like multiple scattering important in capturing the polarization differences? Please discuss or add some references here.
The simulations include multiple scattering. We believe this tends to decrease the polarisation differences (see e.g. Brath et al 2020).
The impact of neglecting the 3D heterogeneous atmosphere (i.e. assuming 1D) on polarisation differences has not been widely studied. Barlakas and Eriksson (2020) look at errors caused by ignoring 3D effects, but polarisation is neglected in that study, so only the first Stokes component (I) was simulated. We have updated the text in the manuscript with a brief summary of that study.

Lines 172-173: Shouldn’t there be a transition between layers with predominately aggregates of columns and predominately aggregates of dendrites? Please add some brief discussion about whether this transition zone may or may not be important in the radiometer signal.

We acknowledge that our particle choice is a simplified representation. It is an imperfect choice, however a “better” representation was not obvious from the imagery. Since we are considering a radiometer viewing the whole cloud from above, we don’t think that including a layer with a mixture of aggregate shapes would have a major impact. There are other uncertainties which are likely to be more significant, for example the fact that the in-situ and remote sensing measurements were not made at the same time (ISMAR measurements were made at 10-10:20 UTC and the in-situ cloud measurements were taken between 10:37 and 11 UTC), meaning there could be a change in microphysics within that time anyway.

Line 179: Please add some more details about the orientation assumptions of the particles within the aggregation model.

OK, have added that the monomers and initial aggregates are random, prior to being reoriented.

Lines 213-215: It is unclear to me why the a and b parameters are being adjusted independently, with the other one being fixed. Isn't there a set of unique a and b pairs that that give a certain IWC, subject to the PSD? Please address more thoroughly in the text why this method of determining the m-D coefficients is constrained in this way.

One could map out the a and b parameter space and work out the best fit. Our goal was not to derive unique a and b values, but to construct scattering models that could fit the radar reflectivity measurements independently from the ISMAR measurements, and then see if those same scattering models could match the ISMAR measurements by using the available limited in-situ information. By comparing simulated Z to measured Z in Fig. 4, we show that we have chosen values of a and b that are realistic.

Line 226: Please clarify the distribution being referred to here.

Ok, have specified it's Ze.

Line 250: Please add some more details about the resolution of scattering calculations (i.e., the number of dipoles) and how many orientations were used.

OK, have done this.

Line 276: Wouldn't these sizes be underestimates of the true maximum dimensions given that they are derived from 2D images? Please clarify.

We have added our thoughts on this to section 2.2.

Lines 309-310: How much does the aspect ratio of the aggregate impact the IMA simulations if the individual monomers are not interacting? Are the polarimetric signature more dependent on the orientations of the individual monomers? Please discuss this point briefly.

This is an interesting question, and we thank the reviewer for bringing it to our attention. In McCusker et al. (2020) it is shown that the IMA can successfully reproduce polarimetric parameters such as ZDR (up to 200GHz). Since IMA only includes interactions within individual monomers, this implies that the monomer shapes and distribution of monomer orientations within the aggregate determine the polarisation properties, rather than the shape of the "envelope" around the aggregate. Thus, we have changed our interpretation that incorrectly simulated V-H is due to the aspect ratio of the aggregates to a suggestion that it may be due to the orientation or aspect ratio of the monomer crystals within the aggregate:

1- If V-H is underestimated, the monomers should perhaps be oriented before aggregation or pivoted on attachment, which both result in flatter and more dense particles (Schrom et al 22), or the aspect ratio of the monomers needs to be smaller (thinner dendritic monomers).

2- If V-H is overestimated, the distribution of monomers should be more isotropic, or the aspect ratio of the individual monomers may be inaccurate.

Line 317: Based on Fig. 6b, it appears that the brightness temperatures of the simulation are on the low end of the distribution of observed brightness temperatures. There should be some additional clarification that the deepest precipitation region of the cloud where the brightness temperatures are lowest is the focus in this section.

The deepest precipitation region is not intentionally our focus in this section. We are comparing one simulated value (simulated using a single microphysical profile of in-situ measurements) to a time-series of ISMAR measurements. We are just pointing out that for the value of V that we simulate, there tends to be more measured V-H values that are larger than the value close to 3 that we simulate (i.e. our simulated V-H seems too small).

Line 347: Please explain why this aspect ratio was chosen.

We are exploring the sensitivity of our simulations to adding dendritic monomers to some region of the cloud, and we attempt to get an idea about whether this is the type of thing that could bring the simulations more in-line with the observations. To that end, the size and aspect ratio of the dendrites are chosen arbitrarily, but we are not saying that this is exactly what's happening in the cloud.

Lines 347-350: Why is this portion of the atmosphere replaced by dendrites? Wouldn't dendrites be more likely above the region containing mostly aggregates? Where is the dendritic growth zone in the profile? Please address these points in this section.

As pointed out in the manuscript, there are a variety of particles present in the imagery, as one would expect in a heterogeneous cloud. However, as in Fig 3, single dendrites were clearly imaged in L7 (2-3km) by the CIP100 probe, which is why we changed particles in that layer.

We acknowledge that in the dropsonde data, the DGZ between -20 to -10C is higher in the cloud between about 3-5 km (i.e. L6 and L5). It is possible that the dendrites imaged in L7 were formed higher in the cloud where T is approximately -15, but take time to grow to larger sizes, so only become obvious in the imagery when they have fallen to lower levels.

Line 380: The phrase "has a bell shape" should be replaced by something more quantitative.

We appreciate your comment, but we are actually doing a qualitative test here, not a quantitative one. We want to avoid cases where total scattering is being truncated as a result of not having large enough particles in the model. To do that, we ensure that the N(D)sigma(D) distribution has a clear peak and tails, rather than the distribution being truncated. We have clarified the text in the manuscript.

Line 431: Please describe how aspect ratio was calculated for these particles.

More detail on aspect ratio definitions has been added to section 3.1.

Line 468: Are these particles truly column aggregates or could they be a mixture of irregular ice particles? Please discuss.

We have changed our conclusions slightly, as per previous comments. However, we have noted in the discussion that the availability of more detailed imagery would be beneficial to better constrain particle shapes, along with having in-situ and remote sensing measurements obtained at the same time.