Reply to Jana Mentrok:

We thank Jana very much for providing the valuable comments and suggestions, especially on the accuracy of the description of the radiative transfer models and simulations. The replies are in blue for easier reading. Some of the comments/questions have been raised by the other two anonymous reviewers, so readers are referred to read the replies to the other two reviewers.

1 General Comments

My primary questions and concerns are with about the description of the radiative transfer modeling (section 2.3). Most of all, your description of RT4 seems off in several aspects. Several points you mention are not general features (or limitations) of RT4. They might be of the specific compilation and setup that you use. In its core RT4 is a scattering solver, it is in the strict sense not a radiative transfer model: it does not provide atmospheric or particle optical properties. Evans' PolRadTran package, through which RT4 is commonly retrieved (from Evans' webpage), provides further code for creating particle optical properties though. However, this is not an inclusive part of RT4 and should be distinguished from this, I strongly think.

Thank you very much for correcting my misunderstandings and providing these constructive comments. I have now revised the RTM description section (now Section 4.1, 1st paragraph) adaptively.

Furthermore, you imply that RT4 does only allow for a (single?) uniform ice layer (p7, l18:). This is wrong. The user might setup RT4 with as many layers as s/he wishes. Each layer is homogeneous, but using sufficiently many, thin layers, a non-uniform cloud can easily be modeled.

What we mean is that we only assume one single uniform ice cloud layer in this study for all the RT3 and RT4 simulations. The wording has been modified to clarify that: "The RT4 simulations we carried in this study assume a uniform ice cloud layer…".

Later on, in section 4.2, you also mention and apply RT3. Would be better to have that already covered in 2.3, too. In 4.2, p15, l12f: you state "RT3, which allows to simulate effects from randomly orientated ice crystals". You imply here that RT4 can not simulate randomly oriented particles. This is wrong. RT4 can handle azimuthally randomly oriented particles. And completely randomly oriented particles are evidently also random in azimuth, are just one special case of azimuthally randomly oriented particles.

I agree with you on the fact that azimuth randomness is assumed for both RT3 and RT4, so our description is not accurate and could be misleading. Now we introduce first of RT3 in the same section of RT4, in the 3rd paragraph of Section 4.1.

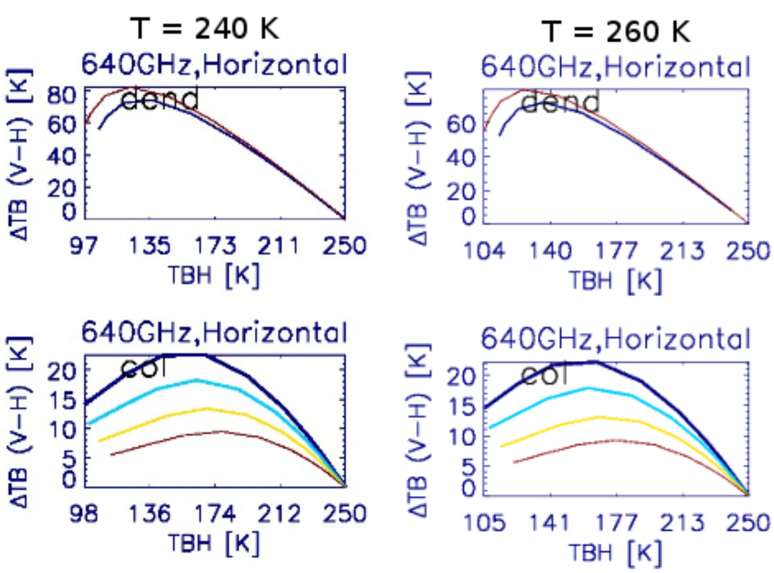
In 4.2 you also describe RT4 as "fully polarized" model. I think this is a somewhat misleading description. RT4 actually does only calculate two Stokes components. In a plane-parallel, horizonthally homogeneous atmosphere with azimuthally randomly oriented particles, the other two components are zero, though.

Yes, I agree with you. Although in Dr. Evan's PolRadTran description page, PolRadTran was introduced as a fully-polarized RTM (http://nit.colorado.edu/polrad.html), RT4 assumes azimuthally random orientation, so that only I & Q parameters need to be calculated layer by layer, but not for U & V. Your suggestion has now been incorporated in the $4^{th}$ paragraph of Section 4.1

On p7, l10f:, you state that Yang et al. (2013) scattering properties where used. According to the paper title this only provides properties up to wavelengths of 100um. Is the title misleading, or how did you prepare your scattering data?
You are correct. Although Yang et al. (2013) only provides calculation for the visible to the far-infrared spectrum, we still used their calculations here for the MW frequency. Dr. Evans provided us with this configuration, and our planned next step is to use ARTS for extensive study. As you know, ARTS has integrated the RT4 solver and Liu (2008)'s MW non-spherical database, so it would be a better choice. This defect has now explicitly mentioned in the main text.

DDA is known to be slow in calculating scattering properties compared to other methods like Mie-theory and TMatrix-method. How do you use it to "speed up" your calculations?
Sorry, we meant to say "the FFT method was used to speed up the DDA calculation". It has been corrected now in the main text.

Your statement of scattering properties being only weakly dependent on temperature seems in contradiction with Tang et al. (2016) (where Wu is a co-author). Could you provide some more information what refractive index model you used, and how big the "minor" differences are?
Which Tang et al. (2016) paper were you referring to? I asked Dr. Wu and he didn't remember he co-authored a paper with Tang.
As for the refractive index model, the reference is:
Ray, P., 1972, Broadband Complex Refractive Indices of Ice and Water, Appl. Opt. 11, 1836-1844




One comparison of assuming different ice particle temperature (240K vs. 260K) is shown above. Only where the PDs peak on the TB-axis has changed by several Ks, but the peak amplitude of PDs barely change.

Does your statement "Frozen particle obey a Gamma size distribution" refer to frozen particles in general (then, I'd like to see that referenced) or to RT4 (see my general concerns above) or to your setup of the RT model in this study? Please be clear on this. I'd also like to see a reference or further details for the optimization procedure.

It's for RT4 and CRM simulations, and in general for many ice particle parameter retrievals, e.g., for CloudSat standard product, etc.. Evans et al. (2012) is cited because he also assumed a Gamma distribution for his TC4 retrieval.

Apart from the RT modeling, your way of using aspect ratio needs more discussion and evidence. You first define aspect ratio as ratio of the H- and V- optical property components, which i think, is fine and could be seen just as an unfortunate terminology (as aspect ratio is commonly used for describing the geometric particle properties). However, later on you directly compare your aspect ratios with geometric aspect ratios (refering to Davis et al (2005), which in contrast to your statement find 1.2 as the best fitting AR, not 1.3) without ever discussing (or proving) whether they can be seen as equivalent.

This discrepancy has also been brought up by the other two reviewers. Please refer to my replies to their related questions. In the revised manuscript, we modified significantly the 2nd last paragraph of Section 4.1 and the last paragraph of Section 4.2 to reflect the change.

I find your simple theoretical study very enlightning and impressive. I wonder, though, why at other places in the paper (p15, l9ff:, p17, l19ff:) you desparately try to find further explanations for the bell-curve when the simple study already explains such behaviour, ie more complicated explanations are not necessary.

Thanks for your recognition of our exploration based on the highly-simplified, conceptual model. We spent extensive efforts on trying to seek better "fitting" to the observations originate from three considerations. First of all, the conceptual model has many assumptions, e.g., one ice cloud layer, homogeneous, and there is no information about size, habit whatsoever. Furthermore, we have to assume that tau is proportional to f^4 in order to find the AR value that determines the peak PD amplitude and where it occurs on the TB axis. Apparently more sophisticated RTMs and inputs (different ice water content profile, particle size, habit, background water vapor, liquid below, etc.) are necessary to make the situation closer to the real world. Secondly and most importantly, we put every effort to seek whether we can get a deeper understanding of what factors contribute to the PD-TB relationship, and whether we can even retrieve some ice microphysical parameters (e.g., aspect ratio, effective diameter) in the future given the observational constraints. Last but not the least, RTMs and simulation settings used in this paper will serve as the touchstone for us to determine whether we can trust and extend these RTMs to a broader spectrum (e.g., higher-frequency MW beyond 640 GHz and IR) in order to find the best configuration of channels to yield the most abundant and/or most accurate ice microphysical retrieval products. This last point is closely related to our recently funded project of developing a new polarization instrument in the IR spectrum. Now we include some of the aforementioned motivations to justify our purpose of conducting extensive RTM simulations in this work (see the paragraph right below Fig. 8 after we finish discussing the conceptual model). We don't plan to include the third point as the project has not begun yet.

2 Specific comments

p5, l26ff: You discuss a distinct branch with linear PD-TB at warm TB, later you talk about "the surface branch". I assume the further one is what you mean by surface branch, but could you make that clear?

We've changed the wording to "there is a distinct branch in Fig. 2a, showing a strong linear PD-$TB_v$ relationship at the warm or clear-sky TBs that corresponds to the surface polarization signals.".

p6, l5ff: "It is non-trivial to determine the magnitude of PD" – why is that? Or what do you actually mean by "magnitude of PD"? Is PD not simply the difference of the V- and H-channel measurements?

What we mean is the absolute value of PD.

p6, l6f: "oceanic PDs are larger at 89GHz" – what does the comparison ("larger") refer to? larger than land PDs? larger than at 166GHz?

We now reworded the sentences as "For example, as we can see from Fig. 1c, PDs are larger at 89 GHz over the ocean because of higher V-pol emissivity on calm water surfaces (acting like a mirror) than on windy surfaces, whereas land surfaces generally have little polarization due to surface roughness."

p6, l8: For me it is not obvious from figures 1&2 that surface emissivity is frequency dependent. It is very likely, but how is that seen in the figures? Could you elaborate on that? And also be more specific how that (freq. dependency of surface emissivity) affects the analysis of PD with respect to frozen hydrometeor microphysics?

After careful thinking, we remove this sentence. It is not directly derivable that surface emissivity is frequency dependent from Fig. 1 & 2.

p6, l10: You seem to imply that negative PD and/or clearsky measurements are stronger affected by noise than others. Why would they? Or do I just misread this statement?

There is a sentence immediately after this line that points to the Appendix B. We used the negative PD to estimate channel noise level.

p11, l5: Please provide a reference for the TC4 campaign.

The references (Evans et al., 2005; 2012) have been cited when we first mention the TC4 campaign and CoSSIR measurements in the last paragraph of Section 2.1.

p11, l7ff: "in optically thick cloud of TB V = 150K, which are also associated with large negative PD values" – to me Fig.5 rather looks like large negative values are all over the place, maybe a general offset for some measurements. Are these large negatives from a similar measurement time or region?

Yes, there is a sentence immediately after the one you mentioned here explains the reason:

“These cases are found in the July 19th and August 6th flight legs but not in July 17th flight leg. Data qualities are considered much noisier in the former two flights than the latter one, but we still keep to show the original data from all three flight legs in Fig. 5 as the peak of PD-TB relationship alters little by including the noisier data (Frank Evans, personal communication).”

p11, l8f: “Data qualities are considered much noisier” – are they noisier or not? in my understanding that shouldn’t be up to “consideration”, but is a verifiable fact. I’d find it interesting to see the 3 days separately. Also, what is the general atmospheric situation for each of them? The cloud types observed? A reference would be good.

We used all 3 days of data mainly because the bell curve features stand robust for all 3 jet legs, although we also have to admit that it seems that the post-calibration does not work perfectly so some shift of the PD – TB relationship toward the negative PD is discernable, and it’s hard to kick out those “bad data”, as the majority still remains to have “good” quality. The 3 days of 640 GHz CoSSIR measurements have been used by Dr. Evans for simultaneous retrieval of multiple cloud ice parameters (Evans et al., 2012, ACP, Fig. 7-12), so the data quality should be good enough to be included here.

p11, l20f: “The bulk volume scattering coefficients can differ between the V- and H-polarization” – only those? what about extinction and absorption?

For the high-frequency MW channels, ice scattering dominates the extinction over the absorption, so the statement is reasonable here.

p14, l10f: Please provide references for the pre-dominant habit statements.

A reference has been included:

Libbrecht, K. G. (2005). The physics of snow crystals. Reports on Progress in Physics, 68 (4), 855-895. doi:10.1088/0034-4885/68/4/R03

p14, l13: “which is indicative of stronger water vapor attenuation at 640 GHz” – could you elaborate how you come to that conclusion? to me this seems fairly far-fetched considering that so many cloud microphysics and cloud optical prop- erty aspects affect PD statistics, too.

We agree with you that this statement is more or less at large. We now added a new paragraph at the end of Section 4.2 to include all possible explanations that make sense to us and that possible to account partially to the observed PD – TB relationship at three distinct frequencies.

p16, l25f: Please provide references for the different degree of orientation depending on precipitation type.

A reference to Bob Houze’s ‘Cloud Dynamics’ book (Section 6.2) has been added to the citation list. Thanks.

p17, l31: How do you get to the 30% error estimate? this has not been discussed in the paper, has it?

Please check my reply to Reviewer#1's first question. The 30% error estimate was never intended to be a focus of this paper, so we just briefly mentioned at the end of the manuscript to raise up attention of some serious impacts that omitting PD could cause.