Gong & Wu: Microphysical Properties of Frozen Particles Inferred from Global Precipitation Measurement (GPM) Microwave Imager (GMI) Polarimetric Measurements

JM

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This is a very important paper with some very interesting findings. The demostration of polarization differences in measurements alone is enlightning. The explanation of the pattern from a simple theoretical setup is quite impressive. I have a couple of questions, comments and concerns, though.

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.

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.

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. 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.

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?

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?

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?

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.

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.

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.

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?

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

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

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?

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?

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

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?

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.

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

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

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 property aspects affect PD statistics, too.

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

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