

2nd round of reviews of “Simulation and sensitivity analysis for cloud and precipitation measurements via spaceborne millimeter wave radar” by Kou et al.

General comments

I want to thank the authors for their hard work in revising and improving the manuscript. In general, the revised manuscript shows vast improvements, especially in terms of readability, motivation and in clearing up previously ambiguous passages. I stand by considering the study as suitable for AMT. However there are still a few points that should be changed before publishing. Also, one explanation regarding the calculation of mass size parameters depending on rime fraction is puzzling to me. I therefore recommend another round of major revisions.

Specific comments

I still find the difference between “conventional” and “optimized” settings to be unclear in the text. Writing “the main difference...” gives the impression that there are other differences that are not mentioned / explained. If that is the case, please write down the other differences. If not, then omit the “main”.

It is still not clear to me what mass-size parameter literature values are averaged over. I still recommend including a sentence like, “For a and b we took literature values from -list of studies- and calculated the mean” and please go into detail, if you have left out any values that are listed in the literature (for example values for hail).

I don't understand the description of the variable density (variable mass size prefactor a) depending on rime mass fraction. As far as I'm aware the ELWP is the liquid water path along the trajectory of the rimed particle assuming a riming efficiency of 1. It is therefore not equal to the LWP and the equation in L510 (which is the definition of LWP not ELWP as far as I know) is incorrect. In Moissev et al. 2017, ELWP is about two times lower than LWP. If LWP=ELWP is assumed in the study, this must be discussed in more detail. I also don't understand how FR was calculated from ELWP, the formula seems to be missing. It is written that a linear increase of FR with ELWP is assumed and Moissev et al. 2017 is referenced. However, in Moissev Eq. 8 ELWP is proportional to $FR/(1-FR)$ which is not linear. Also the scatter plot in Fig 9 of Moissev et al. 2017 does not show a clear linear behavior between FR and LWP. I would therefore like to ask the authors to clear up these issues and discuss the calculation of the adjustment factor f in more detail. It is fine in my opinion to assume a linear relation between f and LWP, if this is what was done (I am not sure). But this decision must be discussed and possible errors resulting from that approach must be mentioned. In addition, the formula that was used to calculate f should be included (including numerical parameters).

Technical corrections (& minor comments)

L18 “optimal physical modeling” is misleading

L34 Not the best sentence, maybe: “Cloud radars are mainly operated spaceborne, airborne or ground-based.”

L39 “with a minimum detectable signal of about -30 dBZ”

L42 environment → environmental (?)

L50 highly important → important

L60 ~~satellite simulation~~

L128 was → were (or are)

L140 include references, where did you get the knowledge that 10 microns is typical?

L141 include Mie and Rayleigh citations

Eq 8 form looks a bit weird, maybe include spacing or dot between number and exp?
L200 I don't think mature is the right term. Also what do you mean by that?
L215 & L219 it's misleading that "prefactor a can vary considerably" and "relations vary slightly" are written
L229 why is 0.4 the typical value? Explanation on how that was derived is missing
L231 graupels → graupel particles
L240 unclear sentence
L258 uniform bin size set → I still think that the bin size should be mentioned somewhere earlier
L282 by the T-matrix
L284 I don't understand. Is the look-up table for backscattering cross-sections of individual particles?
L301 change with dominating microphysical processes (?)
L311 do you mean Rayleigh regime?
L335 leads to
L342 "most of the mass relations have the mean value of b close to..." → misleading / unclear; maybe better write: When averaging literature values of the exponent b from – list of studies – we derive $b = 2.1$ for particles classified as snow.
L346 see **L342** but for graupel
L358 represented as different models → represented by different distributions
L369 represents for the
L395 became apparent (?)
L413 Maybe better "We then selected two typical weather cases:" And maybe include a bit more explanation on why these were chosen as reviewer#2 has suggested.
L420 reference to appendix?
L425 the maximum total water content was at approximately 3 km with $\sim 0.9 \text{ g/m}^3$
L429 please include the resolution of ERA5 and MODIS (maybe in brackets?) here
L447 the empirical
L449 maybe include geographic region. In high latitudes (arctic) riming is observed also in stratiform clouds
L467 with attenuation → in the attenuated signal ?
L478 what does mostly mean? Either write xx% was within yy% or omit mostly and write the (higher) uncertainty that holds for all data
L503 include "typically" or "commonly" otherwise statement is incorrect
L546 nine sub modules → eight sub modules?
L557 significantly greater than ... what?
L570 is it % or rather percent points?