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1. Summary
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This is a review of the revised version of Kou et al., "Simulation and sensitivity analysis for cloud and precipitation measurements via spaceborne millimeter wave radar". The authors evaluate the sensitivity of a forward model for radar reflectivity to its microphysical input variables. The forward model includes cloud ice and water, melting mixed-phase precipitation, snow, graupel and rain. They then perform comparisons of reflectivities that are forward-modeled for two WRF simulations (one stratiform and one convective event) against CloudSat observations of the same events.

Although the authors have provided sufficient responses to most of my original concerns, there are still two substantial issues that have not been addressed adequately.

Issue 1 could be addressed by deferring the particle shape and orientation part of this study to a future, more complete study. Issue 2 could be addressed by following the revisions I've suggested below.

I believe that addressing these issues and that by addressing the remaining comments on this revision of the paper, the paper will be suitable for publication.

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2. Main issues
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Issue 1: Particle shape sensitivities
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The authors responded to my original comment by performing DDA simulations of the scattering properties of their chosen particle shapes (sphere, spheroid, cylinder). The point of my comment wasn't that DDA needed to be applied to these shapes. Instead the point was that more realistic shape variations are needed and that DDA is the method usually used to calculate scattering properties for more realistic shapes. The use of realistic shapes and DDA (or perhaps Raleigh-Gans) to calculate scattering properties is the current standard for evaluating the shape sensitivity for millimeter-wavelength radar reflectivity in snow. I think that the authors cannot claim to be assessing shape sensitivity accurately when using only spheres, spheroids and cylinders.

Issue 2: "Conventional" versus "improved"
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After going through this version of the paper thoroughly, I still find it difficult to discern what are the conventional and improved assumptions for the two test cases. This needs to be stated more clearly. Part of the problem is that there is no clear layout of the experimental design (this would usually be included in a methods or objectives section, but section 2.1 is the closest we have to this).

I would suggest:

a) Add a paragraph just after the first paragraph in section 4. The new paragraph should describe the authors' intentions to test the forward model simulations using both conventional and improved parameter settings and briefly describe in general terms the objective of using the conventional and improved settings.

b) Add a section just before section 4.1.2 that contains an outline of what is being tested for the stratiform case. Describe what parameters are changed between the conventional and improved radar simulations for this case and the scientific justification for those parameter changes. Then proceed to describe the radar reflectivity simulation results.

c) Do the same thing in section 4.2 for the convective case. Also, structure section 4.2 similar to the way section 4.1 is structured: A subsection for the WRF simulation description, a subsection for the experiment design (describing the conventional and improved parameter settings), and a subsection for the results.

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3. Responses to prior comments
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These notes provide my assessment of the authors' responses to my original comments (egusphere-2022-886-author_response-version1.pdf)

Prior comments, overall
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1. Thanks for providing these additional details. They are sufficient for explaining the perturbations in b.

2. This revision addresses my original comment, thanks. There are some additional comments that apply to these revisions, please see the specific comments section that follows.

3. Thanks, this additional text resolves my comment.

Prior comments, WRF model simulations
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1. Thanks for this response and the details provided in the new Appendix A. This addresses my concern, but please also see the specific comments section that follows.

Prior comments, particle shape and orientation
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1. This doesn't really address the point of my original comment. The meaning of the original comment is that using soft spheres, spheroids and cylinders doesn't give a realistic representation of how scattering properties for snow particles vary with shape at 94 GHz. This is true regardless of whether the spherical/spheroidal particles' scattering properties are calculated using DDA or T-matrix theory.

See for example, Figure 12 and the related discussion in Wood et al. (2015).

In order to accurately assess sensitivities of radar reflectivity to particle shape variations, more realistic partical shape variations must be used. And in order to evaluate the scattering properties of more realistic particle shapes, a technique such as DDA must be used.

The authors comment:

"We mainly considered the difference between sphere and spheroid with with different orientations in this study. In future research, we will consider the influence of more particle shapes on radar reflectivity."

I think this is not sufficient to support the authors claims of evaluating particle shape and orientation effects in this study.

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4. Specific comments from review of version 2

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Note that the ATC document and the version2 paper are not consistent in their revisions. For example, L21 of the ATC uses the phrase "brightness band" while the corresponding line in the version 2 paper (also L21) uses the term "bright band". L39 in the ATC gives CloudSat minimum detectable signal of -30 dBZ, while L38 of the version2 paper gives -29 dBZ.

Comments and the line numbers used here refer to the revised version2 paper.

L11: Should be "improve" rather than "improving".

L22-23: Relative error in the vertical profile of what variable?

L51-53: To be correct, QuickBeam doesn't compare modeled clouds to observations, it is a radar simulator package. It is up to the users to make the comparisons. Also, QuickBeam is capable of simulating radar reflectivities for radars other than CloudSat. Finally, to say that QuickBeam does not simulate mixed-phase melting particles is entirely incorrect. See section 4 of the Haynes et al. paper you have referenced.

L57: No citation for WRF-SBM.

L60-61: I am not sure what a "cloud data simulator" is, please clarify. If this is referring to cloud radar simulators, the statement is not correct. QuickBeam, as an example, uses scattering properties obtained from discrete dipole simulations of realistic snow particle shapes from the Liu (2004). It does not use an "equivalent spherical shape" for snow particles.

L82-85: Technically, all of these steps are not part of the "forward modeling". The "forward model" consists only of the component that takes in the simulated cloud and precipitation fields from WRF and outputs the simulated reflectivity profiles. The activities listed here actually compose the entire research method.

L83: Should be "Weather Research and Forecasting (WRF) model". Also, no citation is provided for the model.

L92: Be a bit more specific here. Which "real observation data"?

L96: Should be "refractive index", not "reflective index".

L99: Need citation for Liebe model.

L143: I'm not sure what is meant by "direction of raindrop particles". Please clarify.

L162: Can you provide a citation that supports this statement? I don't recall ever seeing an exponential distribution used for cloud ice.

L177-178: This statement explicitly contradicts the actual findings in Nowell et al, 2013. Nowell et al. find that "the backscatter cross section is not well duplicated by the soft or solid spherical/spheroid approximations" in comparison to DDA results for realistic particles. This quote from Nowell et al. applies to particles with size parameters larger than "x ~ 0.75", which is true for most snowflakes at 94 GHz.

This is the root of my concern raised in my original comments about the need for using more realistic shape and scattering models for snow particles.

L204-205: I'm not sure how the comment on graupel altitudes is relevant to this work.

L289: Is this equation reference correct? None of these variables appear in equation 9.

L294: Same comment as above for L289.

L300-325: This is a long paragraph and covers several different topics. Perhaps split it into two or three shorter ones.

L302: I still object to this use of 'dB'. Using the units 'dB' for this quantity is equivalent to using the units 'mm' for a variable that is measured in meters. It is misleading, confusing, and shouldn't be done in a professional publication.

L306-308: It is not clear how this statement about changes in N0 through natural aggregation processes is relevant to the sensitivity study.

L308: What is "among them" referring to? This isn't clear.

L357-358: I don't think a comparison of reflectivities calculated using sphere and spheroid shapes will adequately evaluate the sensitivitiy of radar reflectivity to snow particle shape. The actual uncertainty at 94 GHz is much larger than 1.6 dB. See for example, Wood et al. (2015) for an evaluation of different aggregate shape assumptions.

L376: Do you mean "mixing ratio"?

L390-392: Citations needed for ERA5 and MODIS products.

L399-400: I don't think it is possible to unequivocally state that the cloud scenario simulation results are valid based solely on evaluations of cloud fraction and cloud top temperature.

L414: Are these mass-power parameters the "improved microphysical parameter settings" referenced at L432-433? If so, it would be good to point out here that these are "improved" parameters since they are selected to be consistent with the stratiform conditions specific to this case.

L433-435: OK, here is a statement about what "conventional" means. Apparently, "improved" includes the melting layer model. Are the PSD parameters given here (D0=1mm, mu=3) for the conventional or improved settings? This statement isn't clear, and it's also not clear what is the basis for selecting the "improved" settings.

L442: "the PSD parameters for raindrops were based on the assumed value". This isn't clear because both the "conventional" and "improved" simulations use "assumed" PSD parameter values.

L447-448: For both the "conventional" and "improved" cases, aren't there constraints on the mass-power relation?

L478: Again, it is unclear what is meant by "conventional" and "improved" settings, but then it is somewhat explained in the following lines, but not clearly.

L510-512: It is still not clear to me how the mass-diameter relationship affects the shape of the PSD.

L513: Should be "significant" rather than "significantly".

L513-514: Revise this to "Variation in a may result in reflectivity uncertainty of approximately 45% for snow and 30% for graupel, mainly due to changes in the particle scattering properties."

L515-516: Again, I think the approach used to estimate uncertainties due to particle shape significantly underestimates this uncertainty.

L536: This is the first mention of "multiband measurements". What is meant by this, and why is it introduced for the first time here?

L537-538: Similar comment here as above. It is not clear what is meant by "increasing the polarization function" and how the results of this study support this statement.