To: Editor, *Atmospheric Measurement Techniques (AMT)*

Re: Manuscript Number: egusphere-2022-886
Title: Simulation and sensitivity analysis for cloud and precipitation measurements via spaceborne millimeter wave radar
Author: Leilei Kou; Zhengjian Lin; Haiyang Gao; Shujun Liao; Piman Ding

Dear Editor,

Thanks for your attention and the reviewers' evaluation and comments on our manuscript (ID: egusphere-2022-886) once again. We appreciate editor and reviewers very much for the time and effort that they have put into reviewing the manuscript. The suggestions have enabled us to improve our work, as well as guide our research in the future. Based on the revised opinions, we have made targeted revisions to part of the article after discussion. We hope that the revised manuscript is now acceptable for publication in your journal.

* All the changed contents are highlighted in track change mode in the revised manuscript. More specific revisions against each point are explained as follows.

Thanks very much again for your help to our paper processing.

Best regards
Leilei Kou

Response to comments by reviewers 1#:

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.

Thank you very much for reviewing again as well as your valuable suggestions to improve the paper. We will benefit impressively from your suggestions about writing and technique details. According to the suggestions, I have made careful revisions to this paper. More specific revisions against each point are explained as follows.

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".

Response: Thanks very much for the valuable suggestion. This expression is inaccurate. We have omitted the "main" in the sentence. Besides, we have restructured the section 4, and the difference between improved and conventional setting was described in the subsection of "experiment design" to make it more clearly.

"4.1.2 Experiment design

Besides the comparison with the CloudSat observation data, the simulation results with improved and conventional setting were compared as well. For the stratiform case, the PSD parameters were assumed based on the empirical values of land stratiform precipitation clouds (Mason, 1971; Niu and He, 1995; Yin et al., 2011), in which the D_0 of cloud water was set to 0.01 mm, the D_0 of cloud ice was 0.02 mm, and μ was set as a constant of 1. As snow in stratiform clouds were mainly unrimed particles in middle and low latitudes (Yin et al., 2017), a mass-power relation representative m=0.0075$D^{2.05}$ of unrimed snow (Moisseev et al., 2017) was used in the simulation, where D was the volume equivalent diameter. During simulation with improved microphysical setting, a melting layer model with a width of 1 km was assumed below 0 ºC based on the statistical median of melting layer width in stratiform precipitation observed by radars (Liu et al., 2016; Wang et al., 2012), and the PSD parameters of the raindrops were calculated according to the melting model. For conventional setting, the melting model was not included, and the PSD parameters for raindrops were set as D_0 =1 mm, μ=3 based on the statistical average values of microphysical parameters of stratiform precipitation in eastern China (Chen et al., 2013; Wen et al., 2019)."

●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).

Response: Thanks very much for the suggestion. The corresponding sentences have been rewritten. "According to results from observation experiments reported in the literatures, the exponent *b* for snow varies from 1.4 to 2.8, and we derive the mean value of *b* close to 2.1 via averaging literature values of *b* from list of studies (Brandes et al., 2007; Heymsfield et al., 2010; Huang et al., 2019; Sy et al., 2020; Szyrmer and Zawadzki, 2010; Tiira et al., 2016; Wood et al., 2013). For graupel, the exponent *b* varies from 2.1 to 3, and a mean value of approximately 2.6 is derived from the studies in the literatures (Heymsfield et al., 2018; Mason et al., 2018; Von Lerber et al., 2017). Based on the range and mean value of *b* for the Gaussian distribution, we calculated the standard deviation (SD) to be 0.28 and 0.16 for snow and graupel, respectively."

The sentence in Figure 4 has been rewritten as well. "For *a* and *b* we took literature values from list of studies and calculated the mean, and the standard deviation of *b* for snow and graupel are calculated according to the range and average of Gaussian distribution."

●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).

Response: I am sorry for the mistake. Thanks very much for pointing out. The adjust factor *f* was assumed to increase linearly with LWP. We have revised the relevant content and added more details. "The adjust factor *f* is obtained from f=1/(1-FR) where FR is the ratio of the rime mass to the snowflake mass. According to Moisseev et al (2017), FR can be expressed as a function of the effective liquid water path (ELWP), $ELWP \approx 4\alpha_u / \pi \cdot FR/(1-FR)$, given that the rime mass is determined by the mass of swept supercooled liquid droplets. Considering the connection between ELWP and liquid water path LWP (according to Moisseev et al (2017), ELWP is approximately half of LWP), we assumed that the adjust factor f increased linearly with LWP, and the relation between f and LWP was derived to be $f \approx 0.5\pi LWP/\alpha_u + 1$. The assumption ignores possible changes in particle mass linked to the presence of different crystal habits, and the exponent b in the mass-size relation remains constant. Large uncertainty may occur in the cases where majority of precipitation occurs in the form of crystals."

Technical corrections (& minor comments)

L18 "optimal physical modeling" is misleading

Response: Thanks for your suggestion. The term of "optimal physical modeling" has been revised as "improved physical modeling".

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

Response: Thanks very much for your suggestion. The sentence has been revised as "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)

Response: Sorry for the poor writing. Thanks for your suggestions. The corresponding expressions and sentences have been modified in the revised manuscript.

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

Response: Thanks for pointing out. The references of Mason (1971) and Miles et al (2000) have been added.

L141 include Mie and Rayleigh citations

Response: Thanks for your suggestion. Another reference of Bohren and Huffman (1983) has been added. "their scattering characteristics can usually be calculated via Mie theory (Bohren and Huffman, 1983) or Rayleigh approximation (Zhang, 2017) based on the sphere assumption."

Bohren, C.F., and Huffman, D.R.: Absorption and scattering of light by small particles, New York: Wiley, 1983.

Eq 8 form looks a bit weird, maybe inlcude spacing or dot between number and exp?

Response: Thanks for the suggestion. We have rewritten Eq.8.

$$D_{\max} = \begin{cases} 11\exp\left(0.069T\right) & stratiform \\ 21\exp\left(0.070T\right) & convective \end{cases}$$

L200 I don't think mature is the right term. Also what do you mean by that?

Response: Thanks for your suggestion. The term of "mature" has been revised as "large".

L215 & L219 it's misleading that "prefactor a can vary considerably" and "relations vary slightly" are written.

Response: Sorry for the unclear description. Thanks for pointing out. "relations vary slightly" refers to the mass-power relations in different literatures vary slightly. The prefactor *a* can vary considerably, and the prefactor *a* varies between 0.005 and 0.014 cgs units. In different literatures, the prefactor *a* almost all varies in this range.

The sentence has been rewritten. "In different studies, the statistical results of mass-size relations vary slightly (Brandes et al., 2007; Mason et al., 2018; Tiira et al., 2016; Wood et al., 2015), with the primary difference being the diameter expression for the maximum dimension diameter, D_m, median volume diameter, D_0, or volume equivalent diameter, D."

L229 why is 0.4 the typical value? Explanation on how that was derived is missing.

Response: Thanks for your suggestion. The sentence has been rewritten. "The density is generally between 0.2 and 0.9 g/cm^3, with the typical value of approximately 0.4 g/cm^3 from the statistical results in observation experiments (Heymsfield et al., 2018; Ryzhkov and Zrnic, 2019)."

L231 graupels → graupel particles

Response: Thanks for the suggestion. The term has been revised.

L240 unclear sentence

Response: Thanks for pointing out. The sentence has been revised. "Similar mass relations can be found for graupel, and its exponent *b* is larger than that for snow."

L258 uniform bin size set → I still think that the bin size should be mentioned somewhere earlier

Response: Thanks for your suggestion. The information about the bin size has been added. "Uniform bin sizes are set for hydrometeors, for example, *dD* is 0.01 mm for cloud water. "

L282 by the T-matrix

Response: Thanks for pointing out. The mm^2 following the T-matrix has been removed.

L284 I don't understand. Is the look-up table for backscattering cross-sections of individual particles?

Response: Yes, the backscattering cross-sections for particles of different diameters are stored in the look-up table.

L301 change with dominating microphysical processes (?)

Response: Yes. The term of "microphysical process" has been revised as "dominating microphysical processes".

L311 do you mean Rayleigh regime?

Response: Yes, the linear growth stage in the Mie scattering region means the Rayleigh regime.

L335 leads to

Response: Thanks for the suggestion. The term of "lead to" has been revised as "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

Response: Thanks very much for the valuable suggestion. The relevant sentences have been revised. "According to results from observation experiments reported in the literatures, the exponent *b* for snow varies from 1.4 to 2.8, and we derived the mean value of *b* close to 2.1 via averaging literature values of *b* from list of studies (Brandes et al., 2007; Heymsfield et al., 2010; Huang et al., 2019; Sy et al., 2020; Szyrmer and Zawadzki, 2010; Tiira et al., 2016; Wood et al., 2013). For graupel, the exponent *b* varies from 2.1 to 3, and a mean value of approximately 2.6 was derived from the studies in the literatures (Heymsfield et al., 2018; Mason et al., 2018; Von Lerber et al., 2017)."

L358 represented as different models → represented by different distributions

L369 represents for the

Response: Thanks for the suggestion. The relevant terms have been revised.

L395 became apparent (?)

Response: Yes. The sentence has been revised. "For raindrops, the backscattering difference became apparent after the equivalent diameter was 2 mm."

L413 Maybe better "We then selected two typical weather cases: …." And maybe include a bit more explanation on why these were chose as reviewer#2 has suggested.

Response: Thanks very much for your valuable suggestion. More explanation has been added. "The cases were selected by combining historical CloudSat data and typical weather processes observed on the ground."

L420 reference to appendix?

Response: Thanks very much for your valuable suggestion. We have added this sentence. "More details about model setup can refer to Appendix A."

L425 the maximum total water content was at approximately 3 km with ~0.9 g/m^3.

Response: Thank you. The sentence has been revised.

L429 please include the resolution of ERA5 and MODIS (maybe in brackets?) here

Response: Thanks very much for your suggestion. The information has been added. "Considering the resolution of ERA5 data (0.25°) and the MODIS scanning track (2330 km), the outermost grid in the WRF simulation data was used for comparison."

L447 the empirical

Response: The term has been "the empirical".

L449 maybe include geographic region. In high latitudes (arctic) riming is observed also in stratiform clouds

Response: Thanks for your suggestion. The geographic has been added in the sentence. "As snow in stratiform clouds were mainly unrimed particles in middle and low latitudes (Yin et al., 2017)."

L467 with attenuation → in the attenuated signal ?

Response: Yes, "with attenuation" refers to 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

Response: Thanks very much for your valuable suggestion. The term of mostly has been omitted and the sentence has been rewritten. "The relative error ($|Z_{sim} - Z_{obs}| / Z_{obs}$, where Z_{sim} represents the simulated reflectivity and Z_{obs} represents the observations, the units of Z_{sim} and Z_{obs} are converted to mm^6/m^3) at each height was within 20 %."

L503 include "typically" or "commonly" otherwise statement is incorrect.

Response: Thanks for the suggestion. The term of "commonly" has been added in the sentence. "Unlike stratiform clouds, a large percentage of heavily aggregated and/or rimed snow commonly exist in convective clouds (Yin et al., 2017)."

L546 nine sub modules → eight sub modules?

Response: Thanks for pointing out. "nine sub modules" has been revised as "eight sub modules".

L557 significantly greater than … what?

Response: Thanks for pointing out. The sentence has been rewritten. "Using the exponential PSD with a power-law mass spectrum for snow and graupel, we found that the effects of prefactor *a* on radar reflectivity were significant."

L570 is it % or rather percent points?

Response: It should be percent points. The sentence has been rewritten. "The average relative errors in radar reflectivity profile between the simulation and CloudSat data were within 20 %, which improved by 20–80 percent points compared with the conventional setting."

Special thanks to the reviewer for his/her good comments.

Response to comments by reviewers 2#:

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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.

We appreciate you very much for the time and effort that you have put into reviewing the manuscript. Special thanks to you for the valuable suggestions about issue 1 and issue 2. According to the comments, I have made careful revisions to this paper. More specific revisions against each point are addressed in the notes below.

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2. Main issues

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Issue 1: Particle shape sensitivities

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.

Response: Thanks very much for your valuable suggestion. Yes, the discussion of shape sensitivity is not sufficient. In this study, we mainly discuss the shape sensitivity of spheres and spheroids, which is not sufficient to evaluate the effects of particle shapes on backscattering properties. In the future research, we will consider more realistic variations in particle shapes to evaluate sensitivity of the scattering properties to hydrometeor shapes more comprehensively.

We have added relevant information in the revised manuscript. "Here we mainly discuss the backscattering difference between spheres and spheroids. In future research, we will consider more realistic variations in particle shapes to evaluate sensitivity of the scattering properties to hydrometeor shapes more comprehensively."

Issue 2: "Conventional" versus "improved"

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.

Response: Thank you very much for the valuable suggestion. The section 4 has been restructured. An example of the stratiform case is presented below. The structure of the convective case is similar to this.

"**4 Simulation results for typical cases**

Based on the sensitivity analysis of typical cloud physical parameters, we simulated the radar reflectivity of typical cloud scenes by assuming appropriate physical parameters for different hydrometeors and cloud precipitation types with the hydrometeor mixture ratio from the WRF as input. The simulation results were compared with CloudSat observation data.

Two typical weather cases of a cold front stratiform cloud and a deep convective process were shown, which were simulated with improved setting accounting for the particle shapes, melting modeling, and mass-power relations for snow and graupel. The cases were selected by combining historical CloudSat data and typical weather processes observed on the ground. For comparison, the results with conventional simulation were also shown.

4.1 Stratiform case

4.1.1 WRF scenario simulation

4.1.2 Experiment design

For comparison with CloudSat data, the two-dimensional (2-D) hydrometeor profile from the WRF model on the track matching CloudSat was selected as the input for the radar reflectivity simulation. The WRF data at 04:30 AM was selected. Owing to the uneven output height layer of the WRF, data for the WRF simulation results were interpolated in the vertical direction. The vertical grid of the interpolated data was 240 m, corresponding to the CloudSat CPR data.

Figure 9a–e shows the latitude-height cross-section of the hydrometeors in the stratiform case simulated by the WRF for cloud water, cloud ice, snow, rain, and the total hydrometeors. The vertical extent of snow is widely distributed, ranging from 3 to 10 km. Rain is mainly below 3 km, with water contents between 0.1 and 0.2 g/m^3. At approximately 0 ºC, the water content for cloud water, snow, and rain were large, which led to a high total water content, with a maximum of 0.57 g/m^3.

Besides the comparison with the CloudSat observation data, the simulation results with improved and conventional setting were compared as well. For the stratiform case, the PSD parameters were assumed based on the empirical values of land stratiform precipitation clouds (Mason, 1971; Niu and He, 1995; Yin et al., 2011), in which the D_0 of cloud water was set to 0.01 mm, the D_0 of cloud ice was 0.02 mm, and μ was set as a constant of 1. As snow in stratiform clouds were mainly unrimed particles in middle and low latitudes (Yin et al., 2017), a mass-power relation representative m=0.0075$D^{2.05}$ of unrimed snow (Moisseev et al., 2017) was used in the simulation, where D was the volume equivalent diameter. During simulation with improved microphysical setting, a melting layer with a width of 1 km was assumed below 0 ºC based on the statistical median of melting layer width in stratiform precipitation observed by radars (Liu et al., 2016; Wang et al., 2012), and the PSD parameters of the raindrops were calculated according to the melting model. For conventional setting, the melting model was not included, and the PSD parameters for raindrops were set as D_0 =1 mm, μ=3 based on the statistical average values of microphysical parameters of stratiform precipitation in eastern China (Chen et al., 2013; Wen et al., 2019).

4.1.3 Radar reflectivity simulation 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

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

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

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.

Response: We appreciate you very much for the time and effort that you have put into reviewing the manuscript. The suggestions have enabled us to improve our work, as well as guide our research in the future.

Thanks for your valuable suggestion about evaluating particle shape and orientation. In this study, we mainly consider the shapes of spheres and spheroids, which is not sufficient to evaluate the effects of particle shapes on backscattering properties. In future research, we will consider more realistic variations in particle shapes to evaluate sensitivity of the scattering properties to hydrometeor shapes more comprehensively.

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4. Specific comments from review of version

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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 -30dBZ, while L38 of the version2 paper gives -29 dBZ.

Response: Thanks very much for pointing out. This may be related to the manuscript version. We will check the manuscript carefully before submitting.

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

Response: Thank you very much. The term has been revised.

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

Response: Thanks for pointing out. The sentence has been rewritten. "The average relative error of radar reflectivity in the vertical profile was within 20 %."

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.

Response: Sorry for the inaccurate statement. The sentence has been rewritten. "QuickBeam is a user-friendly radar simulation package that converts modeled clouds to the equivalent radar reflectivities measured by a wide range of meteorological radars."

L57: No citation for WRF-SBM.

Response: Thanks for pointing out. The citation has been added.

"Iguchi, T., Matsui, T., Shi, J.J., Tao, W.-K., Khain, A.P., Hou, A., Cifelli, R., Heymsfield, A., and Tokay, A.: Numerical analysis using WRF-SBM for the cloud microphysical structures in the C3VP field campaign: impacts of supercooled droplets and resultant riming on snow microphysics, J. Geophys. Res., 117, D23206, doi: 10.1029/2012JD018101."

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.

Response: Sorry for the inaccurate statement. Thanks very much for pointing out. The sentence about the traditional cloud radar simulator has been removed.

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.

Response: Thanks for pointing out. This includes not only forward modeling, but also sensitivity analysis and results comparison. The sentence has been rewritten. "The framework of forward modeling and simulation for spaceborne millimeter radar was composed of eight sub modules."

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

Response: Thanks for pointing out. "Weather Research and Forecasting (WRF)" has been revised as "Weather Research and Forecasting (WRF) model". The reference of Skamarock et al (2019) has been cited.

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

Response: Thanks very much for the suggestion. The sentence has been rewritten. "The WRF simulation results were then validated by using the real satellite and ground observation data such as ground-based radar data."

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

Response: Sorry for the mistake. Thanks for pointing out. The term has been revised.

L99: Need citation for Liebe model.

Response: Thanks for the suggestion. The reference of Liebe (1981) has been added.

"Liebe, H.J.: Modeling attenuation and phase of radio waves in air at frequencies below 1000 GHz, Radio Sci., 16, 1183-1199, doi: 10.1029/RS016i006p01183, 1981."

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

Response: Sorry for the unclear statement. The term of "direction of raindrop particles" has been revised as "particle orientation".

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

Response: The reference of Ryzhkov and Zrnic (2019) has been provided.

Ryzhkov, A. V., and Zrnic, D. S.: Radar polarimetry for weather observations, Cham, Switzerland: Springer Press, 2019.

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.

Response: Thanks very much for the good comment. This citation is really inaccurate. The citation of Nowell et al (2013) in this sentence has been removed.

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

Response: Yes, this sentence makes no sense. Thanks for pointing out. This sentence has been removed.

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

L294: Same comment as above for L289.

Response: Thanks for pointing out. This should be equation (6)-(9), to be exact. The sentences have been rewritten. "For cloud ice, D_0 is calculated from Eqs. (6)-(9) given W and T; μ is the only parameter that needs to be assumed." "Based on Eqs. (6)-(9), D_0 varied from 0.1–0.5 mm at –60 ºC and 0.2–0.8 mm at –20 ºC when W ranged from 0 to 0.5 g/m^3."

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

Response: Thanks very much for the valuable suggestion. This paragraph has been split into 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.

Response: The dBN_0 has been omitted. The $\log_{10}(N_0)$ has been used in the revised manuscript. Correspondingly, the dBN_0 in Figure 4 has been modified to $\log_{10}(N_0)$.

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.

Response: Sorry for the unclear statement. The two sentences are meaningless to the sensitivity analysis. These have been removed. "An increase in a leads to an obvious increase in the corresponding particle scattering properties, and then causes the reflectivity change. Using an average mass-power relation assumption, the variation in a as a result of the degree of aggregation and riming, and particle shapes may result in the reflectivity uncertainty of approximately 45 % and 30 % for snow and graupel, respectively."

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.

Response: Thanks very much for the comment. The comparison of reflectivities using spheres and spheroids is not sufficient to evaluate the shape effects for snow. In future research, we will consider more realistic variations in snow shapes to evaluate sensitivity of the scattering properties to particle shapes more comprehensively.

L376: Do you mean "mixing ratio"?

Response: Yes, it is mixing ratio. The term of "mixture ratio" has been revised as "mixing ratio".

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

Response: Thanks for the suggestion. The citations for ERA5 and MODIS products have been added.

"Menzel, W.P., Frey, R.A., Zhang, H., Wylie, D.P., Moeller, C.C., Holz, R.E., Maddux, B., Baum, B.A., Strabala, K.I., and Gumley, L.E.: MODIS global cloud-top pressure and amount estimation: algorithm description and results, J. Appl. Meteorol. Climatol., 47, 1175-1198, doi: 10.1175/2007JAMC1705.1, 2008.

Hersbach, H., Bell, B., Berrisford, P., Hirahara, S., Horányi, A., Moñoz-Sabater, J., Nicolas, J., Peubey, C., Radu, R., Schepers, D., Simmons, A., Soci, C., Abdalla, S., Abellan, X., Balsamo, G., Bechtold, P., Biavati, G., Bidlot, J., Bonavita, M., Chiara, G.D., Dahlgren, P., Dee, D., Diamantakis, M., Dragani, R., Flemming, J., Forbes, R., Fuentes, M., Geer, A., Haimberger, L., Healy, S., Hogan, R.J., Hólm, E., Janisková, M., Keeley, S., Laloyaux, P., Lopez, P., Lupu, C., Radnoti, G., Rosnay, P., Rozum, I., Vamborg, F., Villaume, S., Thépaut, J-N.: The ERA5 global reanalysis, Q. J. Roy. Meteor. Soc., 146, 1999-2049, doi: 10.1002/qj.3803, 2020."

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.

Response: Thanks for the suggestion. This sentence has been removed.

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.

Response: Sorry for the unclear statement. About improved and conventional microphysical parameter settings, we have added a subsection for the experiment design. Detailed description can be found in the next comment of "L433-435".

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.

Response: Sorry for the unclear statement. Thanks for pointing out. We have added a subsection of the experiment design to describe the conventional and improved parameter settings.

"Besides the comparison with the CloudSat observation data, the simulation results with improved and conventional setting were compared as well. For the stratiform case, the PSD parameters were assumed based on the empirical values of land stratiform precipitation clouds (Mason, 1971; Niu and He, 1995; Yin et al., 2011), in which the D_0 of cloud water was set to 0.01 mm, the D_0 of cloud ice was 0.02 mm, and μ was set as a constant of 1. As snow in stratiform clouds were mainly unrimed particles in middle and low latitudes (Yin et al., 2017), a mass-power relation representative $m=0.0075D^{2.05}$ of unrimed snow (Moisseev et al., 2017) was used in the simulation, where D was the volume equivalent diameter. During simulation with improved microphysical setting, a melting layer with a width of 1 km was assumed below 0 ºC based on the statistical median of melting layer width in stratiform precipitation observed by radars (Liu et al., 2016; Wang et al., 2012), and the PSD parameters of the raindrops were calculated according to the melting model. For conventional setting, the melting model was not included, and the PSD parameters for raindrops were set as D_0 =1 mm, μ=3 based on the statistical average values of microphysical parameters of stratiform precipitation in eastern China (Chen et al., 2013; Wen et al., 2019).

Chen, B.J., Yang, J., and Pu, J.P.: Statistical characteristics of raindrop size distribution in the Meiyu season observed in Eastern China, J. Meteorol. Soc. Jpn, 91, 215-227, doi: 10.2151/jmsj.2013-208, 2013.

Liu, L.P., Zhou, M.: Characteristics of bright band and automatic detection algorithm with vertical pointed Ka band cloud radar, Plateau Meteor., 35, 734-744, doi: 10.7522/j.issn.1000-0534.2014.00160, 2016.

Wang, D.W., Liu, L.P., Zhong, L.Z., Wei, Y.Q., and Wang, X.B.: Analysis of the characters of melting layer of cloud radar data and its identification, Meteor. Mon., 38, 712-721, doi: CNKI:SUN:QXXX.0.2012-06-010, 2012.

Wen, L., Zhao, K., Yang, Z.L., Chen, H.N., Huang, H., Chen, G., and Yang, Z.W.: Microphysics of stratiform and convective precipitation during Meiyu season in eastern China, J. Geophys. Res., 125, e2020JD032677, doi: 10.1029/2020JD032677, 2020.”

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.

Response: Sorry for the inaccurate statement. Thanks very much for pointing out. The PSD parameters of the raindrops in improved setting were calculated according to the melting model. For conventional setting, the melting model was not included, and the PSD parameters for raindrops were set as D_0 =1 mm, μ=3 based on the statistical average values of stratiform precipitation in eastern China (Chen et al., 2013; Wen et al., 2019).

This sentence has been rewritten. “Without the melting model, the PSD parameters for raindrops were based on the assumed fixed value.”

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

Response: Thanks very much for pointing out. The mass-power relation was used for both "conventional" and "improved" cases. The sentence of “in other words, simulations with mass-power constrained PSD of snow and the melting model are more similar to the observations” has been removed.

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.

Response: Thanks for pointing out. We have restructured the section 4, and the difference between improved and conventional setting was described in the subsection of “experiment design”.

“4.2.2 Experiment design

In the convective case, snow and graupel were abundant. Unlike stratiform clouds, a large percentage of heavily aggregated and/or rimed snow commonly exist in convective clouds (Yin et al., 2017); therefore, rimed particles were assumed for convective clouds modeling. Considering the effect of riming, a varying mass-power relationship was assumed in the simulation with improved setting. As the prefactor *a* in the mass-power relations increases with the riming degree (Mason et al., 2018; Moisseev et al., 2017; Ryzhkov and Zrnic, 2019), an adjustment factor *f* was considered in the simulation process, i.e., $a=a_u f$, where a_u is the density prefactor for unrimed snow. *f* is obtained from $f=1/(1-FR)$ where FR is the ratio of the rime mass to the snowflake mass. According to Moisseev et al (2017), FR can be expressed as a function of the effective liquid water path (ELWP), $ELWP \approx 4\alpha_u / \pi \cdot FR/(1-FR)$, given that the rime mass is determined by the mass of swept supercooled liquid droplets. Considering the connection between ELWP and liquid water path LWP (according to Moisseev et al (2017), ELWP is approximately half of LWP), we assumed that the adjust factor *f* increased linearly with LWP, and the relation between *f* and LWP was derived to be $f \approx 0.5\pi LWP/\alpha_u + 1$. The assumption ignores possible changes in particle mass linked to the presence of different crystal habits, and the exponent *b* in the mass-size relation remains constant. Large uncertainty may occur in the cases where majority of precipitation occurs in the form of crystals. The exponent *b* for snow was assumed to be the mean value of 2.1 based on the sensitivity analysis. Then, the corresponding scattering properties and PSD for snow and graupel were calculated according to the mass-power relations.

The effect of riming was not considered in the conventional simulation. In the simulation with the conventional microphysical setting, a mass-power relation of $m=0.0075D^{2.05}$ of unrimed snow (Moisseev et al., 2017) was used for simulation of snow particles, and a constant density of 0.4 g/cm^3 was assumed for graupel particles."

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

Response: Sorry for unclear statement. The statement about the effect on the shape of the PSD in this sentence has been removed. "The mass-diameter relationships for snow and graupel differ substantially for different particle habit types. Using the exponential PSD with a power-law mass spectrum for snow and graupel, we found that the effects of prefactor *a* on radar reflectivity were significant."

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

Response: Thanks for the suggestion. The term of "significantly" has been revised as "significant".

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."

Response: Thank you very much for the valuable suggestion. The sentence has been revised.

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

Response: Thanks for your good comment. This study mainly considers the shapes of sphere and spheroid. More realistic shape variations will be considered in the future research. The sentence has been rewritten. "The assumption of sphere and spheroid could lead to an average reflectivity difference of approximately 4–14 %."

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

Response: This is about the application research prospect. This may not be appropriate at the end of this sentence. The sentence about the "multiband measurements" has been removed.

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.

Response: Thanks for the comment. This sentence has been removed.

Special thanks to the reviewer for his/her good comments.
