This is a review 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. They find in particular that including radar attenuation in the forward model gives improved results over a forward model without attenuation.

Overall, this seems to be a concise and well-executed study. The conclusion that including attenuation in the forward model is necessary for reproducing W-band reflectivities in precipitation that includes melting and liquid phases is not surprising. Perhaps more surprising is that such good agreement was achieved in the comparisons of reflectivities between the WRF simulations and the CloudSat observations. It appears that the model fields were selected from the exact groundtrack and the exact time of the CloudSat overpass. It's unusual, I think, for model features, particularly precipitation, to be so well-located with the observations.

I think the study is a useful contribution to the precipitation retrieval literature. My overall comments relate to how the sensitivity perturbations were defined and how the WRF simulations were configured. Most significant is whether the assessment of uncertainties due to particle shape and orientation are sufficient. I'd like to see this addressed in revision. My specific comments are more extensive and touch mainly on unclear language and missing details. Because they are extensive, I am calling the necessary revisions "major".

I think that with revision, the paper can be acceptable for publication. For this current revision, the scientific significance is good, the scientific quality is fair but this is difficult to judge due to the presentation. The presentation quality is fair.

Overall comments:
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Perturbations to PSD parameters:
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No explanation of how the assumed perturbations in parameters were determined, or what sources were used to justify the assumptions. E. g., Line 284 "According to the range in b, the standard deviation (SD) as assumed to be 0.5 and 0.3 for snow and graupel".

Overall, the explanations of the sensitivity analysis falls short, particularly as related to PSD perturbations. For example, when "a" is increased, does the reflectivity increase because of the resulting change in the scattering properties of individual particles, or is it because the ice water content increased? When "a" was increased, was N_w decreased so that the ice water content was unchanged? The same sort of concern applies to the evaluation of sensitivities for other PSD parameters.

I suggest also looking at Wood and L'Ecuyer, 2021, AMT. How do your sensitivity results compare to their conclusions about sources of uncertainty in retrieved snowfall?

WRF model simulations:
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Details of how the model simulations were performed are lacking. Sufficient details should be provided to reproduce the simulations. In particular, information about the microphysical parameterizations should be provided, but also other details such as nested domain sizes, positions, time steps, vertical gridding should be included. This information could be provided in an appendix.

Particle shape and orientation:
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The authors rely on a set of "soft" (mixture of ice and air) particle shapes to evaluated sensitivities to particle shape and orientation. The shapes are spheres and spheroids for snow and rain, and cylinders are additionally included for cloud ice. The T-matrix method is used to calculate scattering properties. It is very unlikely that soft ice spheres and spheroids provide adequate results for evaluating sensitivities to particle shape and orientation for snow or cloud ice at W-band. More realistic variations in particle shapes (e. g., Wood et al., 2015, JAMC) using the discrete dipole approximation for scattering properties can give backscatter cross-sections that vary by a couple of orders of magnitude at larger particle sizes. This seems inconsistent with the results in Figure 6. I request that the authors look at more realistic backscattering cross-sections, particularly for snow, and reexamine their conclusions. These backscattering properties are readily available, from either the Liu database or OpenSSP described in my specific comments beolow, for example.

Specific comments:
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L 36-37: I’m not sure why you would say that the CPR is the "most typical spaceborne radar". There are and have been several other spaceborne radars, none of which are cloud radars like the CPR, but rather precipitation radars.

L 41-42: I also don’t understand here why you would say "comprehensive view" and "fully detecting clouds and associated precipitation". There are numerous limitations in terms of spatio-temporal sampling and in measurement capabilities that make the CPR observations incomplete.

L 43-45: How does initiating research demonstrate detection capability?

L 53-54: Note that "GPM" is the acronym for the project. The relevant instrument is the "Dual-frequency Precipitation Radar", "DPR".

L 56-57: The seriousness of the effects of particle shape and orientation depend very much on radar wavelength. The effects on Ka- and Ku-band radars like the DPR are much less than those on the W-band CPR.

L 57-58: It is not clear what is meant by "density of mixed particles" here. Does "density" mean the particle concentration, or the actual bulk density of individual particles? Does "mixed particles" mean "mixed-phase particles"? And how does this "density of mixed particles" impact PSD? Where are your citations for these statements?

L 65: What does "optimization physical parameter settings" mean?

L 73-246: There is a significant omission of citations to relevant reference material throughout this section. Please examine this section and add citations to appropriate references to support the assumptions you have made.

L 77-78: What makes the cases you selected "typical"? Were the cases really selected by going through the historical CloudSat data? Were there any other criteria? Why did you choose the particular cases presented in sections 4.1 and 4.2?

L 79-80: How were the WRF simulation results verified by observation data? The validation of the model results probably deserves a section of its own.

L 77-85: This is a very cursory description of the methodology for the simulations. It is missing many relevant details about the setup of the model. What microphysics parameterization was used?

L 97-98: Because contact freezing is essentially instantaneous, I think graupel are usually considered to be ice-air mixtures unless they fall below the freezing level and begin melting.

L 100-101: Is there a reasons to use Maxwell-Garnett rather than something like a three-component Bruggeman model (e.g., Haynes et al. 2009, JGR Atmosphere). Also, note that it is "Garnett" rather than "Garnet".

L 109: I think this formula is correct only if mu=0. See, e.g., Chase et al. (2020, Atmosphere), equations 7 and 9.

L 114: More correctly, R_gas is the specific gas constant. If you are using the R_gas for dry air and T is the air temperature, this formula is not correct.

L 127-129: Was there a reason for using the normalized gamma distribution rather than making the more common assumption of a negative exponential distribution?

L 131-133: Cloud ice particle habit also depends on the amount of supersaturation in the environment where the particle forms and grows. The more common term for "collision and merging" is "aggregation".

L 133-134: Is the Liu database relevant to this work? Was it used in some way? The next sentence states that T-matrix calculations were used, not the Liu database scattering properties.

L 136: I don't find this Hogan et al. citation in the bibliography. How was D defined for these ice particles? Is it an equivolume ice diameter?

L 137: The Fu, 1996 reference cited here is not in the bibliography. Please include it. Were these circular cylinders or hexagonal cylinders? Can you also provide a reference that describes the T-matrix method or code that was applied?

L 139: I'm not sure what point this sentence is making. Perhaps try to state it more clearly. To me, it seems the distribution of orientations is an inherent part of the "falling behavior".

L 142: I don't believe that cloud ice size distributions are considered similar to those of raindrops. Do you have references that suggest an exponential distribution is appropriate for cloud ice?

L 144: Same comment as I made above regarding L 109.

L 156: The term "aggregation" is more typically used, rather than "conglomeration".

L 157-159: Be cautious about using the terms "typically" or "normally", here and in other places in the paper. Is it reasonable to say that some value is typical or normal when only one or two supporting citations are provided?

L 162-168: How is D defined for the snowflakes? The long axis of the assumed spheroid or the equivolume spherical ice diameter?

L 171: The correct name for the second citation is "von Lerber et al."

L 177-178: OK, this describes the "D" for the mass and density relations, but it still isn't clear what diameter was used.

L 188: What is "mass water fraction"? Most of the mass of a graupel particle is due to water (in the form of ice) so the mass fraction of that water will almost always be near 1.0 since the mass of air in the graupel particle is very small.

L 200-203: Are you also ignoring aggregation and collision-coalescence?

L 209-211: But the exponent "b" changes as the particle melts and the shape of the particle melts, does it not? In the end, when the particle is fully melted, and nearly spherical, the value of "b" should be near 3. Can you justify using b=2.1 over the full range of particle melting?

L221: Should the left hand side of equation 19 be "N_w(D_w)"?

L 235: Usually the term "extinction cross-section" is used.

L 239-241: And how were the attenuation and the two-way path integrated attenuation addressed when combining different types of hydrometeors?

L 247-333: This is a general comment for the sensitivity section. Wood and L'Ecuyer (2021, AMT) looked at W-band retrieval uncertainty sources. How do your results compare with theirs?

L 258: It's probably more correct to say that the particles are small "compared to the radar wavelength".

L 259-261: Check grammar/sentence structure.

L 259-264: Were these sensitivities calculated by perturbing D_0 while simultaneously keeping W constant? Or did W increase as D_0 was increased?

L 265: Please check this equation reference. I think it is not correct.

L 278: This isn't the correct equation to convert N_0 to dB(N_0). "dB" indicates "decibel" (i.e., "deci" "Bel", or one-tenth of a Bel). dB(N_0) should be 10*log10(N_0).

L 280-282: This states "may result in an uncertainty of approximately 45% and 30% for snow and graupel", but it doesn't say what property of the snow and graupel this uncertainty applies to. Please clarify.

L 296-299: Please recheck your values for D_0. 20 mm and 30 mm seems extremely large for liquid cloud droplets. Either there is a typographic error here, or an error in the calculation of D_0, I think.

L 297-299: I don't think there is much gained by including the results from the Gamma(D_0=30) case. Clearly, if two PSDs for liquid water droplets are nearly the same, the simulated reflectivities will be nearly the same. The significant point here is that, given the same water content, different assumptions about the shape of the PSD can have a strong effect on the simulated reflectivity.

L 300: It's probably more correct to say the "reflectivity change" was 4.5 dB.

L 314-334: I think it's questionable whether these different shapes of soft (mixtures of ice and air) particle shapes give a good representation of the sensitivity of reflectivity to particle shape and orientation. Methods such as the discrete dipole approximation are accepted as giving much more realistic values for backscattering by ice and snow particles. I think it would be appropriate to look at other sources of DDA backscattering properties (e.g. the Liu database mentioned earlier, or OpenSSP) to see if your results are consistent with DDA results.

L 345-346: What was the vertical grid spacing? Was the spacing uniform or stretched (with layers getting generally thicker with height)? What data were used for initial and boundary conditions? What time-stepping was used? What microphysical parameterizations were used?

L 347: It's probably more correct to say "interior domains" rather than "internal layers".

L 359-360: I'm not sure it's accurate to say that WRF "accurately simulated the cloud system" based only on comparisons of cloud fraction and cloud top temperature.

L 362-363: I'm not sure that choosing to use the WRF results along the CloudSat track is an effective way to do comparisons between models and satellite observations. One of the frequent errors in models is features like clouds and precipitation may not be located precisely in the location of interest at a particular time. As an example, modeled fronts and their associated precipitation may propagate more slowly or more rapidly than the observed preciptation. Perhaps a better approach would be to statistically compare the properties of the modeled versus the observed clouds and precipitation, using model results from the area under *and near* the CloudSat ground track.

L 367-369: Regarding "Snow is widely distributed...", can you provide more details? Are you talking about the horizontal extent, the vertical extent, or something else. Maybe something like "The vertical extent of snow is widely distributed...". Same comment with respect to the next statement, which is about rain. "Rich" is not a clear description. Do you just mean to say the the total water contents for cloud water, snow and rain were large?

L 372: The Yin et al. (2017) work cited here does not appear in the bibliography.

L 375-376: See my comment regarding the Figure 9 caption (L 724). It's probably more clear to refer to these as "unattenuated reflectivities", "attenuation" (is this one-way or two-way?), and "attenuated reflectivities".

L 379-381: Suggest using "unattenuated reflectivity" and "attenuated reflectivity". Also rather than the "end of the melting region", use "below the melting layer" or "below the melting level." Also suggest using "with attenuation" and "without attenuation" rather than "after attenuation" and "before attenuation". "Before" and "after" can have misleading implications when talking about a radar beam propagating downward through the atmosphere. Finally, there is a well-known reference to this behavior in W-band radar observations from space. See Sassen et al. (2007, Geophysical Research Letters).

L 382-383: Did you demonstrate this, or did you mean to cite existing work? How large does this diameter need to be, and how is this relevant to the bright band discussion? If I look at figures 6 and 7, the backscatter cross-sections for the larger particles do not appear to be stable.

L 396: Usually just "bright band".

L 399: Rather than using different names for this feature ("brightness band", "bright band", "strong echo band"), please choose one name and use it consistently. Also, when you say the reflectivity was stronger, what are you comparing to?

L 400-401: How did you calculate this relative error? Relative errors shouldn't be calculated using "dB" values (i.e. (dB_test - dB_true)/dB_true. The values should be converted back to linear units (e.g., mm^6 m^-3), then the relative error calculated.

L 407-408: See my earlier comment concerning the modeling of the stratiform case. Additional details about the model configuration would be interesting to see. What was used for convective parameterization?

L 416: See earlier comment concerning "rich" and "widely distributed". Also, I would suggest that when discussing results that involve vertical profiles of data, don't use the terms "high" and "low" to describe data values. Instead, use "large" and "small".

L 419-420: Snow and graupel are not mixed-phase particles unless they are melting. What is the meaning of "components of snow and graupel were complex"?

L 421: See my earlier comment about the missing Yin et al. (2017) reference.

L 425-428: Does this mean that the assumed rime mass fraction, and therefore the adjustment factor "f" was uniform with height for each simulated profile (since liquid water path is a column variable)? Also, are you saying that you treated the rime mass fraction as liquid water for the purpose of refractive index calculations?

L 429: But you adjusted the PSD so that the water contents in the simulated profile matched the water contents output by the model, yes?

L 451-452: See my earlier comment about computing relative error with reflectivities.

L 463: You mean the sensitivity of reflectivity to D_0? When describing sensitivities, try to express them as "the sensitivity of x to changes in y" so that the meaning is clear.

L 463-467: You mean the sensitivity of reflectivity to D_0? When describing sensitivities, try to express them as "the sensitivity of x to changes in Y" so that the meaning is clear. Yes, by imposing the empirical constraints on the PSD, the PSD itself has few degrees of freedom compared to a PSD with independent variations in parameters, so the PSD is less variable. Is this an unexpected result? Finally see my earlier comment about computing relative changes in reflectivity - be sure these percentages are calculated correctly.

L 467-468: How does the particle density affect the PSD? In general the particle density (as defined by the coefficient "a" and exponent "b" of the mass power law) are considered independent of the PSD paramemters.

L 469-472: But is this sensitivity due to the increase in "a" changing the scattering properties of particles, or is it because the increase in "a" increases the water content for the population of the particles? These two effects need to be separated, otherwise the influence of the change in "a" is overestimated. Also, see my earlier comment about computing fractional sensitivities for reflectivity.

L 480-481: Relative errors in what? Also, see my earlier comment about computing fractional errors in reflectivity.

L 724: It's not clear what is meant by "before", "during", and "after" attenuation. I'm guessing that panel (f) shows the unattenuated reflectivities, panel (g) shows the attenuation (is this one-way or two-way?), and panel (h) shows attenuated reflectivities. Is that correct?