Reviewer II

The paper "Forward Modeling of Spaceborne Active Radar Observations" presents the implementation of a forward operator for spaceborne radar reflectivity observations within the CRTM model. The operator is evaluated for three types of spaceborne observing systems covering a broad frequency range (Ku, Ka, and W). The overall objective of the study is appealing: providing a spaceborne reflectivity simulator within an operational radiative transfer framework is a valuable step toward future data assimilation applications.

We sincerely thank the reviewer for taking the time to review our manuscript and for providing valuable comments.

The paper is well written and easy to follow. However, I think the order of the content in Section 2 could be re-arranged and extended with more details (see below). Besides, contrary to the approach used in other papers about the validation of spaceborne reflectivities, to avoid uncertainties in the prediction of hydrometeors using NWP models, the authors preferred to validate the simulator using hydrometeor profiles that have been retrieved using observations. I think it is a nice approach. However, details about the retrievals are not explained in the current manuscript and should be added in a future version of the manuscript.

We have revised Section II following the reviewer's suggestions and expanded the discussion of the retrieval products to provide additional clarity.

Abstract:

L4: "active radar module": I think "active" can be removed as a radar is always an active sensor. (same for the title)

Thank you for the suggestion. We have removed the word "active" accordingly.

Introduction:

L26 to L30: I recommend the authors to add the appropriate references on the current existing spaceborne missions. PMR onboard FY3G is not mentioned as a current spaceborne mission (see for instance: https://rmets.onlinelibrary.wiley.com/doi/10.1002/qj.4964). The authors could also add future planned spaceborne radar observing systems (e.g. WIVERN, INCUS).

We have revised the paragraph and added the additional current and planned spaceborne missions as suggested. Please see Lines 26-31 in the updated manuscript.

L36: The authors could also cite the work of Fielding et al. (2021).

This citation has now been discussed.

Section 2 :

« forward model in DA » here is a misleading title as the forward operator is then never applied to any NWP model in this paper, and there are no DA experiments in this paper. Therefore, I think the introduction of section 2 is out of the scope of the current paper (especially with the general equation of the 3DVar).

We believe that, since the ultimate goal of this work is the assimilation of radar observations, it makes sense to keep this introduction, especially given that the first reviewer specifically suggested relating our formulation to the data assimilation concepts.

"Radar forward model" would better fit into after the CRTM section as the radar solver is included in CRTM

We have re-arranged the section according to the reviewer's recommendation.

Section 2.1: it is not written how the authors account for the subgrid variability of hydrometeors, especially if the final goal is to apply this operator to global NWP models. Also, the authors don't mention the dielectric properties, which are very important to compute scattering properties.

Given that we have used the retrieved products for the assessment in this work, we did not account for beam filling. In our ongoing data assimilation work, we are computing a beam-filling index using the variability among the profiles within each model grid box. However, this effort is still in progress and is not included in the current manuscript.
We have, however, incorporated a description of how the dielectric properties were computed in the revised manuscript.

Section 3:

L115: it would be informative to add the radar frequencies of AOS

We have removed that short paragraph because the future of AOS is unclear and we are not really sure what frequencies will be selected for the AOS.

L122-124: I personally disagree with the authors. Global dataset of hydrometeors exists with NWP models, and have been used in many studies to validate radar forward operators (Di Michele et al. 2012; Fielding et al. 2021; Ikuta et al. 2016, David et al. 2025, etc. . .)

We have revised Section III and added further clarification to ensure that the reviewer's point is fully taken into account.

L123: The authors should explain the approximations made in the retrieval as this is crucial to explain the results of the sensitivity study to the PSD, and to the particle shapes (section 4). For example, is there any assumption on the PSD in the retrieval; which could then explain why the results are closer if the Thomson PSD is used in CRTM (Fig 4)?

We have revised the manuscript and provided additional information on the retrieval assumptions to clarify this point.

L134-136: In my opinion the sentence is a bit too long.

We have split the sentence into two for improved clarity, as follows:

> Specifically, we utilized the updated vBb/v1.1 version of the algorithm, which provides cloud masks and cloud type classifications. This version also improves detection near the ground by reducing surface clutter contamination compared to previous versions.

L173-176. The authors should add a reference when they argue that the graupel signal is similar than the one of ice, or to demonstrate this point. Indeed, this sentence is a bit counterintuitive as the properties of both species are different in the nature (mass, PSDs, dielectric properties).

We have clarified this point by referring to the results of Moradi et al. (2023), whose Figure 8 shows that assigning the same water content to ice or graupel leads to only small differences in reflectivity, with differences approaching 5 dBZ only in strongly convective regions.

> Moradi et al. (2023) demonstrate that fully swapping the ice water content between ice clouds and graupel can introduce differences of up to 5 dBZ in the heavily convective regions of tropical cyclones, although in most areas the differences are substantially smaller.

Section 4:

The authors should explain why the sensitivity study is not performed at the three frequencies (for the PSDs as well as on the shapes).

This was limited by our scattering database used in CRTM. We have included the following explanation in the text:

> However, the CRTM cloud scattering database includes only spherical particles for rain; therefore, we have limited this sensitivity study to the W-band, as DPR frequencies are primarily sensitive to rain.

L193: Another point is that there is usually a spatial and temporal mismatch between NWP models and radar observations, which makes difficult to disentangle errors in the forward operator from spatial and temporal mismatch (see for instance section 4 in Borderies et al. 2018)

We have edited this paragraph as follows to be consistent with the previous statement we made in Section III:

> As noted earlier, there are several sources that can be used to validate a forward model. For this study, we have chosen retrieval products to avoid temporal and spatial mismatches with the observations. While we acknowledge that some dependence on radiative transfer exists in these retrievals, CRTM is not used during the retrieval process. Therefore, we consider these retrieval products a reliable source for validating the RT model.

L197-200: the authors should add the end of the period of study.

We believe the current text already indicates the study period by specifying that it covers the first week of May.

In the text, the units of the comparisons of Table 1 should be in dB, and not dBZ (a difference between two dBZ is in dB).

We have corrected the text and updated the corresponding figure to use the appropriate unit, i.e., dB.

Legend of Table 1: what are the shapes used for the simulations?

We used sector snowflakes to represent snow particles. This information has been added in a table and the text is revised to better reflect this.

L221: Is there any reference for the retrieval?

We have added a few new references for the retrievals.

L222: The authors should explain why the simulations do not account for rain effect

We have explained the reason for excluding the rain water content from the simulations as follows:

> The rain backscatter was excluded because no reliable rain water content was available that was collocated with the CloudSat CPR observations. Because the CPR 94-GHz radar is primarily sensitive to frozen hydrometeors, excluding rain water content while not ideal, is unlikely to have a substantial impact on the results.

L260: the authors should explain why the simulations do not account for frozen hydrometeors. It is confusing for the reader.

This has been clarified in the revised version as follows:

> The retrieval database used to prepare the input water content profiles required by CRTM did not include water content for frozen hydrometeors such as snow and ice. As a result, these simulations do not account for frozen hydrometeors, which may contribute additional reflectivity in certain cases and could explain some of the residual discrepancies. However, because DPR frequencies are primarily sensitive to rain and liquid cloud, excluding frozen hydrometeor water content, while potentially affecting specific situations such as heavy snow, is unlikely to impact the overall results.

L266:"w-band"-> "W-band"

Thank you! This has been corrected to "W-band."

L267: in that case, the authors should say that they use the corrected reflectivity in the observations.

It is expected that the results for attenuation-corrected reflectivity and attenuated reflectivity would be similar, because the shape of snow particles has little impact on attenuation, which is primarily sensitive to rain, liquid cloud, and species such as water vapor.

I think that this result about the larger differences for smaller content is very interesting. Is there any paper in the literature to support this result, or any physical argument to support these findings? Is this larger difference at smaller contents only due to the shape, or also to the mass-diameter relationships which is associated to each shape? I would recommend the authors to add some more explanations about this point.

We believe this is largely due to the logarithmic scale used in computing reflectivity in dBZ. When these differences are converted to physical units, such as $\mathrm{mm}^6/\mathrm{m}^3$, the absolute differences become much larger at higher reflectivity values. For instance, at -30 dBZ with a 4 dBZ difference, the difference in linear units is approximately 1.51×10^{-3} $\mathrm{mm}^6/\mathrm{m}^3$, whereas at 30 dBZ with a 1 dBZ difference, it is approximately 2.59×10^2 $\mathrm{mm}^6/\mathrm{m}^3$. The text has also be updated to reflect this.

L290: In my opinion the first perspective would be to test this forward operator on NWP model fields (at least before estimating any observation errors for DA).

Thank you for your suggestion. This work is already underway, and the new forward operator is currently being tested within both the NASA and NOAA NWP systems.