The Generation of EarthCARE L1 Test Data sets Using Atmospheric Model Data Sets

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Special issue: EarthCARE Level 2 algorithms and data products

Reply to Anonymous Referee #1

We thank the reviewer for the comments. All the specific technical corrections mentioned have been addressed, including a re-write of the conclusions.

Reply to Specific Comments

Comment 1

- It is not clear how is considered the specific attenuation (Eq. 21) of hydrometeors in the simulated CPR radar reflectivity. In other words, is (and how) attenuation considered in the simulations of L1 radar data (reflectivity and Doppler velocity) of this work? If not, authors should discuss a little bit about this assumption on L1 radar data.

The specific attenuation of hydrometeors is considered in the forward simulated CPR radar reflectivity. This is clearly demonstrated in Fig. 13 where panel (c) shows the observed CPR radar reflectivity with weaker radar reflectivity values in the lower part of the convection and the dimming or even disappearing of the surface radar echo due to the accumulated hydrometeor attenuation effects. We apologize for not making this clear in the manuscript, thus, the reviewer's question is well justified. In the revised manuscript, in section 2.3.1 the last three paragraphs are modified and a new equation (25) in introduced to clarify that we use the attenuated CPR radar reflectivity in the radar instrument model.

Comment 2

- the formulas in this document describing how are simulated the CPR reflectivity and the (apparent) Doppler velocity do not take account of multiple scattering (contrary to the Monte Carlo lidar simulator described in detail in this paper). However, on Fig.13c (and Fig. 28) showing the (observed) CPR raw radar reflectivity factor, and as written by authors line 453, "the fainted CPR echoes that fill the surface echo gap around 4100 km are due to multiple scattering". I don't understand: do the L1 radar data simulated in this work take multiple scattering into account? If yes, how? It must be explained and described in the paper. If not, authors should discuss about this assumption on L1 radar data (reflectivity and Doppler velocity) of this work. The reviewer is correct. We do present CPR forward simulations with multiple scattering, but we do not provide any information on how these CPR multiple scattering calculations have been performed. Hence, we are adding a short description (2.3.2) of the multiple scattering forward calculations.

Comment 3

- Line 394 : "The procedure was...while retaining a suitable degree of accuracy". This technique to speed up the calculation generates errors on the radiative fluxes, certainly depending on the position (middle, cloud edge). What are the orders of magnitude of these errors?

In order to answer this question, a run for a segment of the Halifax scene for which each pixel is explicitly calculated was conducted. The analysis of these results-vs-the "fast" method have been added to the manuscript. See the text around line 430.

Comment 4

- One of the objectives of this work is the production of "realistic" synthetic L1 data. I know that 3D radiative transfer (RT) is very expensive in computing time. The authors preferred to calculate TR in 1D for reasons of calculation time. What is the position of the authors on the "reality" of L1 data in that case? Can they give/discuss a little bit the order of magnitude of the error on the radiative fluxes because 3D RT effects are neglected?

The reviewer is correct in implying that 3D effects, particularly, for radiances, can be large and depend strongly on the cloud distribution and solar viewing conditions. An acknowledgment of this fact and a discussion has been added to the paper. Note also that 3D-v-1D issues, mainly from the EarthCARE radiative assessment point-of-view, are discussed in a companion paper https://amt.copernicus.org/preprints/amt-2022-304/amt-2022-304.pdf.

It should also be noted that, apart from being much more achievable with respect to computational demands, that 1D calculations are more consistent with the (still-state-of-the-art) 1D passive imager retrievals that will be applied to the simulated data. This consistency is important from a technical algorithm verification (e.g. the algorithm and associated code works as designed) even if the 1D simulations are less realistic when compared to 3D MC simulations!

See revised text around line 445.

Comment 5

- The conclusion is a little hasty and does not include the important contents of the document. I think it is important to rewrite it.

We agree with the reviewer, the conclusion has been re-written. See Section 4.

Technical Corrections

Line 2 : Since "high spectral resolution" is specified for lidar, therefore "Doppler" should be specified

for the radar.

Done

Line 21 : Please define Z e and IWC.

Done

Line 72 : typing error in Eq.1

Fixed

Line 51 : the title of the section 2.1.1 is not adapted. Maybe "Hydrometeors microphysical

properties" ?

Changed to "Hydrometeor microphysical properties"

Line 65 : the title of the section 2.1.2 is not adapted. Maybe "VIS-UV-IR optical properties of

hydrometeors"?

Changed to "VIS-UV-IR hydrometer optical properties"

Line 90 : the title of the section 2.1.3 is not adapted. "Maybe "Micro wave optical properties of

hydrometeors "

hanged to "Microwave scattering properties"

Line 99: the title of the section 2.1.4 is not adapted. Maybe "Aerosols microphysical and optical

properties "?

Changed to "Aerosol microphysical and optical properties"

Line 100 : please give the definition of HETEAC.

Done: See line 106

Line 104 : the title of the section 2.15 in not adapted. Maybe "Gases transmission"?

Title of section 2.15 has been changed to :``Optical Gaseous absorption"

Line 109 : the tilte of the section 2.1.6 is not adapted. Maybe "Radiative properties of surface" ?

Title of 2.1.6 has been changed to "Shor-wave Surface radiative properties"

Line 110 : if possible, please change list level and the title "SW" by something as "Short wavelength

properties"

Done.

Line 113 : please give the definition of MOM.

Done: See line 118.

Line 114 : pleas give the definition of "Iso-Vol-Geo-Kernel"

Done: See line 120.

Line 119 : if possible, please change list level and the title "LW" by something as "Long wavelength

properties"

Done

Line 123 : section 2.1.1 is empty. Maybe delete this section ?

Fixed

Line 165 : Point is missing.

Fixed

Line 181 : the title is not necessary. The section dealing with Rayleigh-Brillouin scattering can be

included in the section "Spectral Broadening and Multiple scattering".

The suggestion has been followed.

Line 201 : give the unity of λ

Done

Line 219 dans 220 : "co-polar" is used two times. Please correct.

Fixed

Line 227 : the title of Table 3 is not valid. It is not the CRP, but ATLID ? Moreover, "End-of-life Copolar-Mie transmission" appears 3 times in the parameter column. Please correct.

Fixed

Line 227 : "as well as the effects of the background filter etc. Poisson shot…" These 2 last sentences are not clear. Moreover, please give the definition of ACCD.

Done: See text around line 240

Line 253 : φ or δ ?

Fixed

Line 265 : "FOV" and not "fov". Moreover, it is written that the "telescope fov is 5e-3 mrad". I don't understand this value.

Fixed.

Line 273 : The problem of noise seems to be partially addressed already (line 227). Nevertheless, it is

discussed again here without explaining how is calculated the effects of dark current and ACCD

readout noise. Authors should regroup this topic here and should explain more.

The discussion has been expanded. See text around line 285.

Line 288 : "in Section 2". Please correct.

Fixed.

Line 295 : typing error in Eq. 20. Please correct.

Fixed.

Line 295 : Eq. 20 , 21 , 22 : maybe give references for this equations ?

These equations are rather basic text book equations. No reference is thought necessary.

Line 301 :Eq. 23 : are there no typing errors with i, j, and h?

Fixed.

Line 306 : nadir "one-way" ?

"nadir" is correct.

Line 306 : please define X-MET.

Done: See text around line 333.

Line 322 : Table 4 : altitude is given 2 times, please define &3dB and IFOV

Done

Line 323 "show in Fig 6a"

Done

Line 331 "shown in Fig 6b"

Done

Line 380 : define "tau" or replace it by τ

Done

Line 386 : this sentence is not clear

The text has been re-written

Line 289 : typing errors in the formula (parenthesis problem)

Fixed.

Line 398 and 406 : Table 5 and 6: the wavelengths of the channels are not strictly the same as that

indicated in Chang et al. 2019: Why?. Moreover, please define u1 and u 2.

See revised text around line 460.

Line 400 : MSI ?

MSI has been defined. See line 416

Line 415 : Please give information of this formula (especially the definition of A), because Eisinger et

al. (2022) and Velazquez-Blazquez et al. (2022) are not submitted yet.

See text around line 475

Line 421 : please give the definition of « psf-weighting ».

Done

Line 503 : maybe add "as seen in Fig.13" or something like that.

Done

Reply to Anonymous Referee #2

Reply to the Major Comments

In summary, the reviewer has concerns that "...that the discussion suffers from weak qualitative comments." and that the short presentation "...weakens the way the ECSIM's potential is shown". The reviewer also presents two interesting examples where more quantitative information would be valuable. If this were a stand-alone paper, then the reviewer's concerns would indeed be well justified! However, this paper is part of a special issue and should be evaluated in this context. The main goal of this paper is to document of means by which the simulated L1 data used by the large majority of the 15 or so other papers within the special issue were generated

(https://amt.copernicus.org/articles/special_issue1156.html). These papers cover the full range of EarthCARE algorithms and instruments and contain many new and detailed results. It was a deliberate decision to keep the simulation paper concise and general, while the e.g. the various algorithm papers would go into specific depth.

However, guided by the reviewer's concerns, we have attempted make the goal and context of the paper much clearer and have made stronger explicit links to specific results presented within the other special issue papers. We, however, have refrained from the addition of any substantial amount of new material.

Major comments:

- P24L436-438: 'It can be seen that, as expected, the lidar penetration into the clouds is

Limited.....

See text around line 500.

- P25L450-457: 'The strong 94-GHz attenuation by hydrometeors results to missed detections near the surface.....

See text around line 522

Regarding the CPR Doppler velocities comment, I expected a quantitative assessment of the conditions that will make CPR Doppler velocities valid. For....

See text around line 522

Minor comments:

- P3L57: consider explaining the acronym ECCC

Done

- Section 2.1 focuses on 'scene constituents'. However, 2.1.2 is named VIS-UV-IR which is not a constituent but is about hydrometeors-radiation interaction in the VIS-UV-IR spectra range. Then 2.13 is named radar although section 3 is already named radar....

The Section and Subsection titles have been re-worked.

- Table 1: Add "in GEM" to the figure label: "Cloud and precipitation microphysical parameters in GEM".

Done

- P4L79-85: Which is the consequence of the lack of a backscatter peak of the aggregated solidcolumns phase functions? ...

The text was unclear and lead to confusion. In fact the backscatter peak is taken into account in the simulations. See the text around line 90.

- P13L243: "polarization elements" act perfectly in polarization state. I would say this a strong limitation of the simulator. Which are the consequences of this assumption. This could be added to the limitation table. Polarizing effects on the lidar depolarization products have been deeply studied and its influence is not negligible. This should be assessed.

Again, our original text was unclear and led to confusion. See text around line 305. We strongly agree that polarization calibration and cross-talk correction is important. ATLID specific studies have (and continue) to be carried out (some at the non-open industrial level). However, this paper is not the place to for a detailed discussion.

See text around line 310

- P13L253 Are the stars '*' needed in the equation?

Fixed

- P14L273: Could the authors either shed light on the way dark current noise and ACCD readout noise are simulated or provide references to.

More detail has been given. See text around line 290.

- P34L481: The assessment of the Doppler velocity is qualitative but not quantitative.

See the specific CPR related papers within this special issue for details

Typos:

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- P4L70: extinction absorption -> extinction, absorption
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Fixed

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- P4L78: observation( -> observation (
```

Fixed

- P5L102: ad-Hoc -> ad hoc.

Fixed

- P10L189: Tis -> T is

Fixed

- P10L204: symbol μ is not in the equation

Fixed

- P16L308:]is ->] is

Fixed

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- P23L424: Level-11 -> Level-L1 ?
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Fixed

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- P24L444: in 13 -> in Figure 13
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Fixed

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- P24L447: ( 12 -> (12
```

Fixed

- P26L461 and P26L467: Figure 16 is mentioned before Fig. 15. Consider exchange order of figures.

Fixed. Text has been revised accordingly so that the Fig 15 is mentioned first.

- Figure 13: Consider exchange order of axis. It would be easier to compare the model and observed reflectivity is the axis are more near. The same for the Doppler velocity.

Not done. Switching between the "model truth" and "observed" may be confusing for some readers.

- Figure18: scene -> scene.

Fixed

- P34L482: in 22 -> in Figure 22.

Fixed

- P34L484: 23. -> 23).

Fixed

- Figure 21: echo -> echo.

Fixed