

Dear Editor,

Please find there our responses to the reviewers' comments. Our responses are in blue.

Sincerely,

Zhipeng Qu

Anonymous Referee #2

Summary: The paper describes the creation of the test frames for testing the EarthCare retrieval algorithms. As such it provides substantial utility, which is justification for publication in principle. Publication in practice is to be decided based on its ability to fulfill its objective, which I judged based on the clarity and completeness of the description. Overall it passes with flying colors, as it provides a clear, concise, and compelling description of what is done and what is available. The authors are to be congratulated.

Thank you very much for your review and compliment! Please find below our response to each point.

I only have minor editorial comments or suggestions that the authors may want to consider for their final revision.

1. Line 26: Perhaps say “early to mid 2024 or perhaps later”.

Thank you for the suggestion. We change the phase to:

“which is scheduled for launch in early- to mid-2024”

2. Line 42: “Lacks this luxury” is a rather conversational way to make the point which might take readers a few passes to digest.

Agreed. We changed the phrase as:

“One could stop here and assess performance by comparing retrieved geophysical quantities to their simulated counterparts (cf. Mason et al. 2023), but obviously in the real mission it is impossible to conduct routine comparisons between retrievals and what is actually present.”

3. Line 63: I was a bit puzzled by the reference to the bin-resolved, as what a bin scheme can resolve is a non-parameteric distribution. Passing a parametric distribution to a bin scheme leads to a lack of resolution and seems simply a matter of practicality when interacting with the radiation, as such this strikes me as an unnecessary detail, elaboration, that is not necessary to understand the present paper.

Agreed. We made the following change:

“Bulk properties of atmospheric attenuators, such as 3D distributions of GEM’s cloud water contents (CWC), are used in conjunction with assumed aerosol/cloud size distributions in order for ECSIM to produce physically-consistent synthetic measurements for each of EarthCARE’s sensors.”

4. line 76: I don’t think I fully understood the rationale for not considering night scenes. The simplicity assumption would be that nocturnal situations don’t fundamentally sample a different meteorology, which might be true, but it should be stated, rather than simply focusing on the effect on the instruments.

Agreed. We changed the phrase as:

“With a simply assumption that night-time atmospheric conditions are not fundamentally different from day-time conditions, night retrievals can be approximated by neglecting MSI solar channels and solar back-ground for ATLID.”

5. Fig : I would have preferred a qualitative coloring of the frames, and a label of the colors

We made changes for Figure 1:

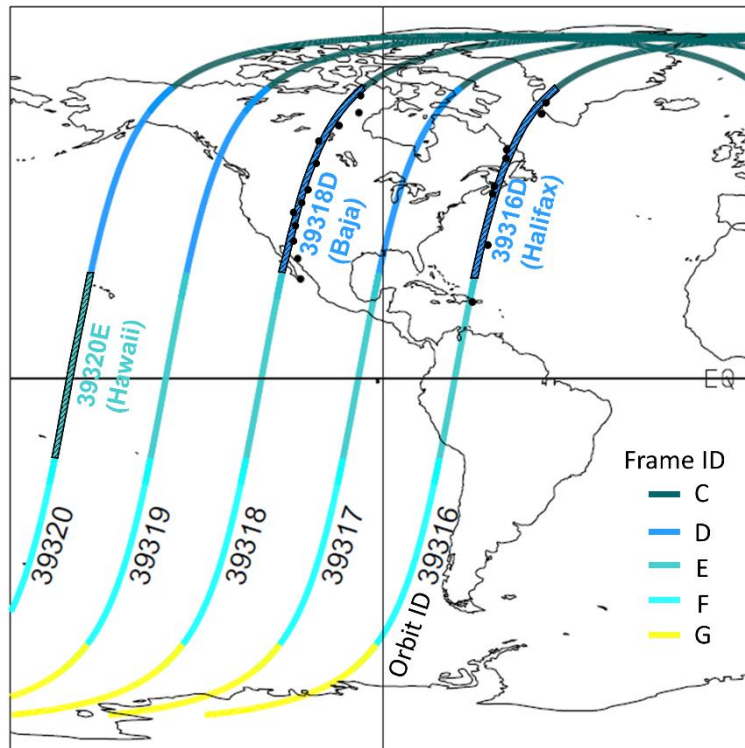


Figure 1: Examples of several successively numbered EarthCARE orbits as provided by ESA. Frames are colour-coded. The test frames are indicated by shaded areas. Frames 39316D, 39318D, and 39320E are referred to as “Halifax”, “Baja”, and “Hawaii”, respectively.

6. line 84: Why not use the ISO-8601 standard for date formatting.

Thank you for this suggestion. The format of the dates is changed to ISO-8601 standard.

7. line 103: I know the phrase non-hydrostatic primitive equations is used, but I find it confusing because I think of the hydrostatic assumption one of the things that make the primitive equations the primitive equations. I would prefer, the non-hydrostatic extension of the primitive equations.

Agreed. The change was made in the manuscript.

8. Fig 5: For domains 2 and 3, I inferred that they are implemented 13 times, for each of the instances of domain 4, but if this could be said more explicitly it would avoid confusion arising from Fig. 5 which shows just one instance.

Thanks for this suggestion. The phrase is now changed to:

“The downscaling transitional domains at Δx of 2.5 km and 1 km adapt themselves to the locations of the $\Delta x=0.25$ km domains (both domains at Δx of 2.5 km and 1 km are repeated 13 times). A common $\Delta x=10$ km domain was used for all 13 segments.”

9. Fig 6 - wouldn't a binary color scale be more appropriate for what I infer to be a binary mask.

The “water-land mask” and “ice fraction” variable are actually continuous values between 0 and 1 (e.g. 50% of grid is land), although most of cases are either 0 or 1. Given this we prefer to keep the continued color map. However, the variable name “water-land mask” is confusing and we have changed it to “water-land fraction”. We also changed the description in the caption.

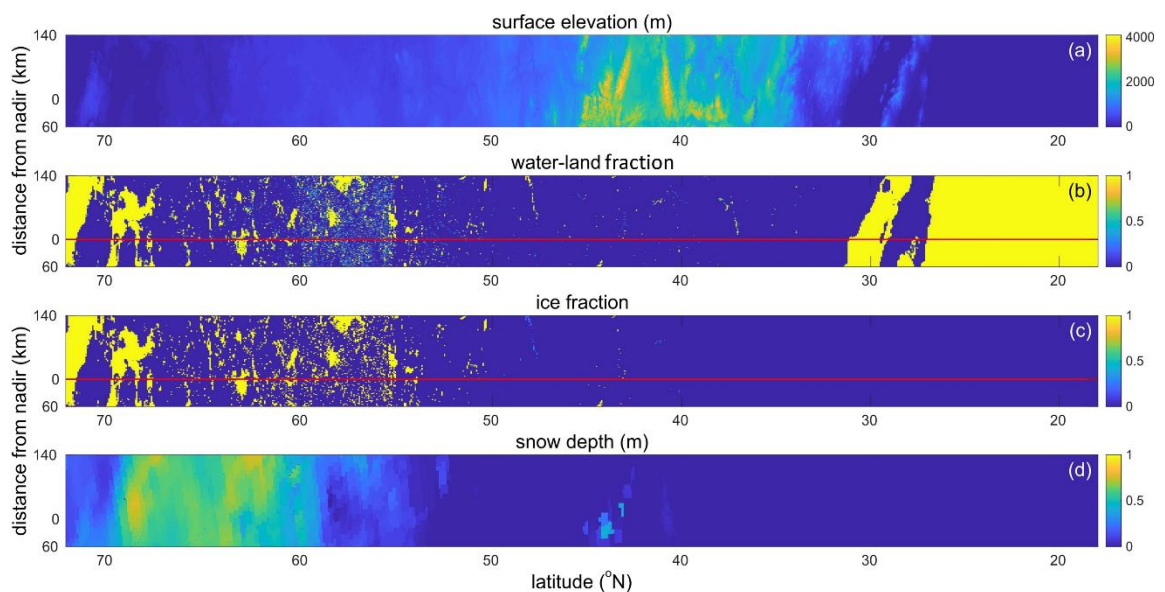


Figure 6: All panels are for the Baja frame (see Figure 1 and Figure 3) and each panel's title is self explanatory. For (b) and (c), blue (fraction of 0) corresponds to 100% land and yellow (fraction of 1) to either 100 % water or 100% ice.

10. line 134: April is not that late in spring, so I was surprised by how little snow there was in the Rockies, making me wonder if this was a bias, or just a false expectation on my part.

From Figure 6d, we can still find some areas with snow depth of ~30 cm near 44°N. The snow information is based on NWP model outputs using a global surface analysis (relatively low resolution), hence there might be inaccuracies. However, since the primary purpose of the test frames is for end-to-end simulation, we, and other algorithm developers, considered inaccuracies and uncertainties such as these to be acceptable.

11. line 203: I thought the 'quite good' was a bit of an overstatement. I guess it depends on one's expectations, and raises the question as to whether the qualitative judgments that are made in these sections are appropriate.

We replaced the phrase with:

“Despite these discrepancies, Figure 13 shows that in the vicinity of where the satellite tracks intersect, vertical realizations of clouds from both GEM simulations and CloudSat retrievals indicate smooth mid-level low density clouds, although those clouds from GEM is more extensive. The altitudes of GEM's clouds over the Rooky Mountains are also in fair agreement with CloudSat's. Unlike the *Halifax frame*, the magnitudes of modelled and “observed” IWPs agree quite nicely, in general.”

With regard to the qualitative judgement in the manuscript, we added PDF plots in Figs. 8, 9, 11, 12, 14. More discussions are also added in the revised manuscript. Please refer to the answers to reviewer #1 for more details.