Answers to review https://doi.org/10.5194/amt-2023-170-EC1

Dear authors and reviewers,

Thank you to the reviewers for their first set of comments and to the authors for their revisions. I am going to send the manuscript out for a second review, and the reviewers/authors should take note of my comments below in the second reviews/revisions.

We thank the editor and reviewers for their comments. Below are the original comments in black with our responses in blue.

REVIEWER 1

1. Regarding the comment on the flowcharts in Fig. 2, readers of this paper are very likely to have the same questions as the reviewer, so the authors should update the text to add clarity and explanations. The reviewer is not asking why the B-NOM and B-SNG products are the way they are (indeed their design is out of your hands) but to explain the reasoning for BM-RAD to contain data from two different sources (B-NOM and B-SNG), and explain how you envision the two being used - perhaps different applications will require different sources?

Sentence added to clarify why it is important the Assessment Domain resolution for the radiative closure evaluation within EarthCARE: *"Among the different resolutions, the AD is especially important as the EarthCARE radiative computations products (Cole et al., 2023) will be evaluated on this domain."*

Reference added to the manuscript.

2. Further to my comment 1, the caption of Fig. 2 could be clearer, because it implies the flowchart is for the processing carried out in the production of B-NOM and B-SNG, when in fact it is the processing applied to B-NOM and B-SNG data to produce BM-RAD (which contains both outputs?)

Agreed and modified as suggested. Caption is now: *"Unfiltering flowchart for the BM-RAD product on the BBR grid resolutions (Full, Standard and Small) from the level-1 B-NOM (left panel) and the on the JSG grid resolutions (Assessment, JSG and JSG enhanced) from the level-1 B-SNG product (right panel)"*

3. Figure 3: please label all four panels as (a) to (d), and refer to them by letter in the caption.

Modified as suggested.

4. Regarding the comment on L219-220 of the original manuscript, I agree with the reviewer that the text is inconsistent with both the table and the conclusions. The new text in section 6 reads:

"The error metrics show that the MSI-based shortwave unfiltering does not perform significantly better than the stand-alone unfiltering approach. The gaining of including MSI information in the unfiltering process while improving results is not very relevant."

The conclusions state:

"Scene information from the MSI radiances (from M-RGR product), MSI cloud retrievals (from M-CLD processor), or snow products (from X-MET product) are useful to further reduce the unfiltering error."

These are inconsistent. Why not state in section 6 that a small improvement is detected but that the difference might not be significant in practice because of parallax effects (which you state in the conclusions)? The current use of the word "significant" in section 6 implies you are talking about formal statistical significance, but I don't think you have tested this. The word "relevant" also doesn't seem correct - surely any improvement of accuracy is "relevant"? Perhaps you mean "large" (as in "not very large")?

The text has been clarified following the recommendation of the editor. The text is now: *“The error metrics show that the MSI-based shortwave unfiltering provides in general a small improvement. The gaining of including MSI information in the unfiltering process while improving results might not be very large in practice because of parallax effects.”*

REVIEWER 2

5. Regarding the definition of \phi(\lambda), if you use it in the introduction, it must be defined in the introduction not in the section after. I suggest changing the text simply to "measured by a perfect instrument, i.e. one with a flat spectral response $\phi(\lambda)=1 (where λ is wavelength), ..."

Agreed and updated.

6. Regarding the comment on the original L148, I think the explanation that alpha does not depend on cloudiness is that clouds are not reflective in the longwave, which could be stated in the manuscript. Indeed, if you plug cloud single-scattering albedo and asymmetry factor into the equation for the reflectance of a cloud in the limit of infinite optical depth (e.g. Eq. 13.45 of Petty's book on atmospheric radiation) then you get a value typically in the range 0.02-0.1 in the thermal infrared. So there is much less contrast with the underlying surface than you get in the shortwave.

As pointed out by the editor, clouds are not reflective in the longwave. However, the solar contamination in the LW channel as discussed in this section is the result of the definition of the synthetic LW channel which has negative “sensitivity” in the SW region (in such a way that the LW for a solar spectrum of 5800 K is zero). This contamination, is therefore negative and proportional to the solar radiances and relatively independent of the spectral signature of the scene. This is the reason why the alpha factor does not depend on the cloudiness.

OTHER COMMENTS

7. Figs. 4 and 5: Yellow is a poor choice of colour as it appears quite faint - can you use a darker shade, e.g. orange?

Figs. 4 and 5. have been updated using a darker shade of yellow, as orange was already used for another surface type. Blue and green colors have also been modified to improve the plot.

8. L248 of revised manuscript: the reference to Fig. 7 appears as "Fig. ??"

Missing reference to the figure added.