Review of "Top of the Atmosphere Reflected Shortwave Radiative Fluxes from GOES-R" by Pinker et al.

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Overview

The manuscript prepared by Pinker et al describes the conversion of radiances from the ABI instrument on GOES-R to SW radiative fluxes. First, a spectral regression is applied to convert narrow band radiances to broadband radiances. Second, angular distribution models are applied to convert the broadband radiances to radiative fluxes. The derived radiative fluxes are compared to those from the CERES FLASHFlux product. Possible reasons for discrepancies are discussed.

This work addresses an important and interesting topic, and I believe that SW radiative fluxes from GOES have the potential to be of great value to the scientific community. However, I have several major concerns as outlined below. In summary, there are significant gaps in the description of the methods that need addressing, and the reasons for differences with CERES data would benefit from some additional analysis. After addressing these concerns, I believe the work would be a good fit for publication in *Atmospheric Measurement Techniques*.

Major comments

L99: In order to apply equation 3, there is an assumption that ADMs from observations and simulations for a given scene type belong to the same population. I am not convinced this is the case. If the CERES anisotropic factors and the simulated anisotropic factors are substantially different (eg. due to neglected processes in the simulations such as 3D radiative effects), the weighted average anisotropic factor from equation 3 might end up somewhere in the middle, not representing either. I suggest discussing this caveat, or addressing this issue with a figure showing that the underlying radiances for a challenging scene type largely overlap between the simulations and CERES.

L103: How is it possible to know "m", ie. the number of CERES observations associated with the anisotropic factor for each angular bin? If I understand correctly, the authors are using the existing CERES ADMs derived from the CERES instrument on TRMM (Loeb et al., 2003), combined with their simulations. These CERES ADMs provide anisotropic factors but, to my knowledge, they do not provide the number of observations that were used to derive the anisotropic factors in each angular bin.

L109: What is the "tool" that was developed to select 100 profiles from the original database of 15704? How does it ensure a variety of conditions are represented? Details are needed, otherwise there is no way that the results can be reproduced.

L164-171: Some key information is missing relating to how clouds are included in the simulations. The following should be included in Table 3:

- What are the cloud altitude/pressure boundaries for the 3 cloud types considered?

- What is the phase of each cloud type? I assume cirrus is ice, stratocumulus is liquid. Altostratus is a mixture? What ice optical properties are used in the simulations?
- Are the 3 cloud types always simulated in isolation, or does the set of simulations include combinations ie. multi-layer cloud?
- Is there any attempt to consider cloud fraction?

L264-267: The differences shown in Fig 9c and 9d occur after applying a NTB conversion and then ADMs. The authors claim that the reason for the differences could be the temporal offset between CERES and GOES. I am not convinced. The observations are co-located to within 5min. Not many cloud regimes are drastically changing within 5min at the CERES footprint scale. I expect the uncertainty due to the NTB conversion and ADMs is much larger. For the NTB conversion in cloudy scenes, one possible reason is that the ABI bands do not provide sufficient spectral coverage. Figure 1b in Gristey et al., JClim, 2019 (https://doi.org/10.1175/JCLI-D-18-0815.1) shows SW spectral reflectance variations for different cloud types. Comparing with the ABI bands, I suspect some spectral variations associated with cloud variability are missed. For ADMs in cloudy scenes, the cloud properties must be retrieved for the selection of the correct ADM. Misclassification of cloud properties will therefore result in flux differences. Even if the correct scene type is selected, ADMs have an uncertainty due to within-scene variability and within-angular bin variability. I suggest including discussion of these possible reasons.

L271: This section does not mention the time range/case studies of observations used from GOES-16 and GEOS-17. Some cases are listed in Table 7, but this table is not referenced anywhere in the text. It is not clear if these cases studies encompass all of the data used in the study. Again, this is essential information for anyone interested to reproduce the results.

L293-294: There seems to be an inconsistency here. The previous paragraph states that FLASHFlux was used because the GOES data was only available for about a week, and FLASHFlux is available within that timeframe. Fair enough. But then it is stated that GOES data is now available in the CLASS archive going back to 2017. So, there is no longer a valid reason to perform comparisons against the (less accurate) FLASHFlux data. Is there any reason that the authors cannot perform their analysis using the GOES data from the CLASS archive against the primary CERES L2 SSF product? Maybe I am missing something.

L296: A major step missing from the paper is how the scene properties are determined for the ABI observations. I expected to see details in this section. My understanding is that both the regression coefficients for the NTB conversion and the ADMs are a function of scene type. I see that a fixed surface type is assumed but how are the changing atmospheric properties accounted for when converting the ABI narrow band radiances to broadband fluxes?

L298: I find it strange that the authors decided to perform their comparisons at the ABI spatial resolution by applying a bi-linear interpolation to the CERES data. It would make more sense to aggregate the ABI data and perform comparisons at the CERES footprint scale. By performing comparisons at the coarser of the two scales, non-linearity due to interpolation is not an issue.

L317: There is no reference to Fig 11, 12 or 13 in the text. These figures are key to the findings of the study and should be referred to throughout the results section.

L358-365: I do not necessarily disagree with these comments on possible differences in the surface spectral reflectance, but they are purely speculative and insubstantial. Can any supporting analysis be added? For example, MODIS provides a surface spectral reflectance co-located with CERES on both Aqua and Terra, albeit at a coarse spectral resolution. The observed MODIS surface reflectance could be compared with MODTRAN values, even just for a handful of case studies, to quantify any differences.

L390-400: Again, the text here relating to the temporal offset between GOES and CERES is speculative and would be much better served by some supporting analysis. I suggest including a scatter plot using the same data in Fig. 10. The x-axis would be the temporal offset (ranging from 0 to 5 min) and the y-axis would be the difference between GEOS and CERES. Data points could be colored by scene type. If the temporal offset is an important issue, expect to see a clear positive gradient.

Minor comments

L38-44: The first paragraph of the introduction does not really serve a purpose. It is irrelevant for the analysis and does not add much to the manuscript in my opinion. It could be removed.

L51-52: There is a recent review paper on shortwave ADMs that could be cited here: Gristey et al., 2021, https://doi.org/10.3390/rs13132640.

L81: Down arrow in the text is out of place and should be removed.

L129: Are the "surface variables" also part of the SeeBor dataset, or added by the authors? Please clarify when the dataset is first introduced.

L130: Is the surface albedo a single broadband value? If so, how is this combined with the spectral surface albedo used in MODTRAN (discussed later).

L132: There is a positive bias in what variable? At what altitude? Please be more specific. Fig. 4 shows 3 variables. The sign of the temperature bias depends on altitude; the water vapor bias is positive only at lower altitudes; the ozone bias is positive only at higher altitudes.

L135: This section does not mention that the surface type is fixed in time. Implications are discussed later, but it should be stated clearly here since this is where the dataset is first introduced and it is an important aspect of the work.

L146: Under clear-sky, scattering by aerosol is important, but probably not multiple scattering. Most aerosol loadings are dominated by single scattering. Suggest removing "multiple".

L146: In addition to aerosol scattering, what about the role of absorption? The 6 aerosol types considered presumably have different single scatter albedo.

L157: Please provide an explanation of where the number 288,000 comes from. I calculated 6 aerosol types x 12 surface types x 100 profiles = 7200 simulations for clear-sky.

L162: How are the variations at 4 different wind speeds accounted for. The 100 profiles do not include wind speed information. I also assume this is surface wind speed but please clarify.

L176: How is the number of Gaussian quadrature points determined? A sentence or two explaining the use of Gaussian quadrature would help the reader here.

L182: “azimuth angle” should be “relative azimuth angle”, I think.

L184: “ignoring spherical geometry” – what does this mean?

L226: 8-stream is used as the baseline/truth in Fig 7, but I do not see any evidence that 8-stream is itself sufficient. If the number of streams was further increased to eg. 16 or 32, would there be any benefit?

L231: Yes, the results for Scaled Isaacs are better than Isaacs, but how to quantify that they are “satisfactory”? I noticed that they are typically much worse than 4-stream DISORT in Fig 7b.

L260: Switching between wavelength and wavenumber is confusing for the reader. Since SW radiation is usually expressed in wavelength, and most of the plots in this study are in wavelength, I strongly suggest converting any instances of wavenumber throughout this manuscript to wavelength for consistency.

L264: “Figure 9” -> “**Fig. 9**” for consistency.

L292: “2019however” – needs fixing.

L301: “must be less than ±5 min”. Is this threshold based on any analysis? What is special about 5 min?

L312: At the footprint scale of which instrument, CERES or ABI?

L321-324: This text is a repeat of the previous section and is not needed again here.

L354: Where is the section 5 heading? It jumps straight from section 4.1 to section 5.1. Are there other subsections from section 4 that are missing?

L377: “the calculated broad-band reflectance was around 0.45” – was this for cloud free scenes only?

L379: Agreed that the filter function for channel 6 (Fig 14) could be problematic. But what impact does this have on the total NTB conversion? What is the weight associated with channel 6?

Table 1: The first part of the caption is not necessary.

Table 1: ABI band 3 is NIR, not VIS.

Table 1, column 2: “Central wavelength” would be better than “Channel”. Need to include units.

Table 1, column 3: Are these spectral band widths associated with a threshold percent drop off in response?

Table 2: Could be more reader friendly. I suggest ordering the first column so that the groups in the second column are next to each other.

Table 4: “Azimuth angle” -> “Relative azimuth angle”.

Table 6: Not referenced anywhere in the text. I do not think it serves a purpose. Suggest removing it.

Table 7: Not referenced anywhere in the text. List of dates and statistics are useful. I suggest keeping the table but making reference to it in the data/results sections.

Fig 1, box 2: “watervapor” -> “water vapor”.

Fig 1: Remove arrow leaving bottom box.

Fig 2: Remove floating arrow leaving the left of the first box.

Fig 3: End of caption is missing.

Fig 3: Top of figure seems to be cut off.

Fig 4: Suggest removing “(logarithmic scale)” from the caption. The error bars are plotted on the same (linear) scale.

Fig 6: Are these nadir radiances at TOA? What is the scene type? Need to include this information in the caption.

Fig 7: Wavelength is increasing from right to left, opposite to the previous figure. For consistency, I suggest reproducing this figure with the wavelength increasing from left to right.

Fig. 8: This figure is in wavenumber but others are in wavelength. For consistency, I suggest reproducing this figure in wavelength. Wavenumber could always be included as a second axis along the top of the plot.

Fig 10: Labels need correcting in the caption. (e) is missing, (d) is in the wrong place.