Response to Reviews 12/10/2021

Color Coding:
Black: Reviewer
Red: Authors

We thank this Reviewer for very helpful comments.

Comment: There is some displacement in the line numbering of the Reviewers and both the submitted and the ATM versions. This made it sometimes difficult to identify the location of the Reviewers comments.

Reviewer # 1

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

21 October 2021

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

Reviewer 1

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 (e.g. 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.

Authors

The comments of this Reviewer are well taken. We were also concerned about the same issues. We have done numerous experiments to understand the sources of differences between the theoretical and CERES ADMs to convince ourselves that the synthesis of the two is sound, even if the two approaches are not identical. In **Figure 1** the patterns of bi-directional correction differences for desert under clear-sky from MODTRAN simulations and CERES observations are illustrated. Largest difference occurs for higher VZAs. While inaccuracies in the specific surface spectral reflectance used in the simulations may contribute to the differences, our experiments show that they are most likely due to differences in sampling frequency of observations at high VZAs. A hybrid approach is applied that hopefully is compensating for the uneven-sampling in the two methods.

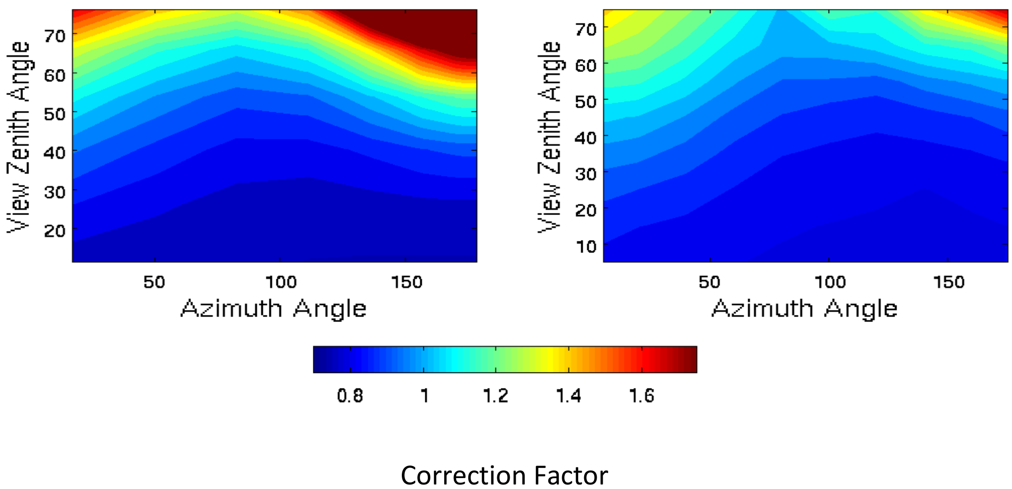


Figure 1. *Bi-directional correction factors at SZA 63.2° over desert for clear-sky Left: Simulations; Right: CERES observations (Bright Desert)*

Before undertaking the simulations, we had to develop a method to reconcile different scene types and angular binning of the CERES and simulated ADMs and a weighting function to combine the two data sources. CERES-TRMM clear-sky ADM classification by surface types does not fully match the IGBP surface classification. In the simulations, the 12 IGBP surface classifications are used. For clear sky, there are 8 surface types in CERES ADMs. An effort was made to combine the corresponding CERES ADMs and simulated ADMs based on IGBP scene classifications to generate new synthesized ADMs for 12 IGBP surface types. The cloud classification in CERES ADMs is based on Cloud Optical Depth (COD) and cloud phase (water cloud, ice cloud) over ocean, low-mod tree/shrub, mod-high tree/shrub, desert, and snow/ice.

For clear sky, the synthesized ADMs are generated from a combination of simulated and CERES bi-directional correction factors based on IGBP surface classifications for each angular bin by weighting, as presented in the manuscript. For example, CERES Low-Mod Tree/Shrub ADMs are grouped from observations of the following three IGBP surface scenes: Savannas, Grassland, and Crops/Mosaic (Loeb et al., 2003). The difference in the bi-directional correction factors between the combined and CERES ADMs for Savannas is shown in **Figure 2.** At lower viewing zenith angles the percentage of differences is mostly within +/- 10% but the differences are much larger at higher viewing zenith angles.

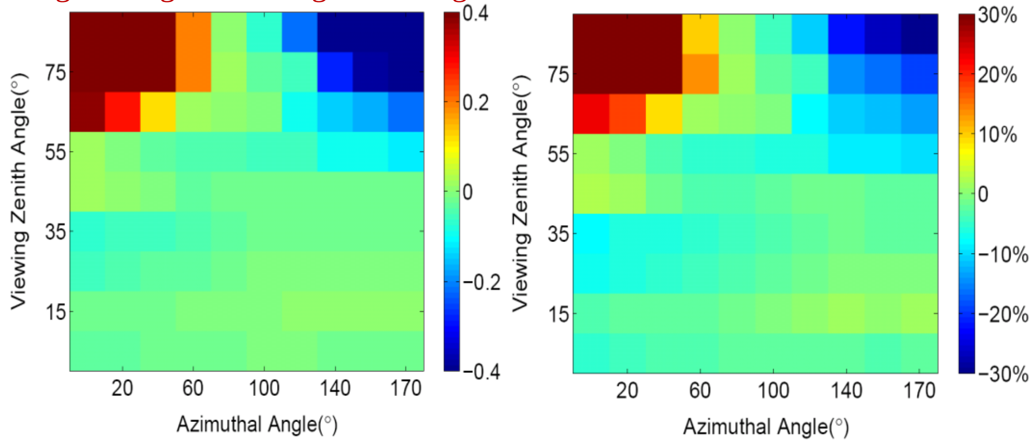


Figure 2. *Distribution patterns of the difference of the bi-directional correction factor between combined ADMs and CERES ADMs for Savannas over clear sky at Solar Zenith Angle of 70-80°:*
Left: Difference (Combined ADMs – CERES ADMs)
Right: Percentage of Difference (Difference/CERES ADMs)

At an early stage of this work when ABI observations were not yet available, we have tested the approach with SEVIRI observations. The following Table (Niu and Pinker, 2011) illustrates that using the hybrid approach results in better agreement with CERES compared to what was achievable with CERS ADMs alone.

Table 7. Evaluations of July 2004 monthly mean TOA upward SW flux estimates as driven with SEVIRI observations when using CERES ADMs or synthesized ADMs, against CERES observations (SRBAVG product).

Statistical results	BIAS ($W\ m^{-2}$) CERES ADMs	BIAS ($W\ m^{-2}$) Synthesized ADMs	RMSE ($W\ m^{-2}$) CERES ADMs	RMSE ($W\ m^{-2}$) Synthesized ADMs
Clear sky (7801 samples)	8.7	4.6	7.1	6.5
All sky (8128 samples)	−3.1	−2.7	8.2	6.3

As mentioned in our manuscript, we have originally prepared two papers. The first one summarized the early results with proxy observations like SEVIRI, GERB, MODIS etc. where some of these issues are explained in detail. Due to concern that the early material may not be any more of interest to the readers, we have focused in the second paper on ABI using the latest versions of GOES-16 and 17 data.

Reviewer 1

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.

Authors

Please see response to previous comment.

Reviewer 1

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.

Authors

An effort was made to have representation of different climatic regions and covering all seasons equally. This selection depends on the availability of observations, namely, if less soundings are available in certain region, that region will be under-represented.

Reviewer 1

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 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?

Authors

The cloud model is the MODTRAN built-in one. The table below gives information from the MODTRAN manual. All clouds are assumed single layer type. Cloud fraction is considered in the flux calculation step. For N2B and ADM conversion, each pixel or field-of-view is assumed to be either clear or total cloudy.

Properties of the MODTRAN Cumulus and Stratus Type Model Clouds.

ICLD	Cloud Type	Thickness (km)	Base (km)	.55µm Ext. (km^{-1})	Column Amt. ($km\ gm / m^3$)
1	Cumulus	2.34	0.66	92.6	1.6640
2	Altostratus	0.60	2.40	128.1	0.3450
3	Stratus	0.67	0.33	56.9	0.2010
4	Stratus/Stratocumulus	1.34	0.66	38.7	0.2165
5	Nimbostratus	0.50	0.16	92.0	0.3460

Reviewer 1

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

Et us discuss(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.

Authors

Thank you for pointing this out. We have now included these comments in the manuscript. We have stated:

As discussed in Gristey et al. (2019) there are SW spectral reflectance variations for different cloud types. Possibly, for ABI bands some spectral variations associated with cloud variability are missed. It is important to have the correct cloud properties to be able to select correct ADM. Misclassification of cloud properties will therefore result in flux differences. They also argue that ADMs have an uncertainty due to within-scene variability and within-angular bin variability leading to additional flux differences.

Reviewer 1

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

Authors

These are all the cases that have been re-done against the updated CERES data. Numerous other cases were compared with the FLASHFlux data at an earlier stage. We now reference Table 7 in the text and we add information on the time range in this table.

Reviewer 1

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.

Authors

No. This Reviewer is not missing anything. Indeed, there was some confusion regarding what was used in the latest version of comparison as presented in Table 7. We apologize for the mistake we made in labeling the product. We have been involved in using the FLASHFlux data in preliminary evaluation for such a long time (due to the latency of data availability) that we labeled it as Flash Flux and not CERES. We have now prepared a full data base of what was used so the reader can have a hold of the data used. Information will be provided how to access this database.

Reviewer 1

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?

Authors

We use the IGBP classification under clear conditions. Table 2 describes the surface classification for IGBP 18 types, and their reduction to 12 IGBP types as used in this study to match the CERES types. This is also discussed in the text. For cloudy conditions we select the N/B and ADMs according to cloud classification and optical depth.

Reviewer 1

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.

Authors

For the re-mapping, we adopted the ESMF re-gridding package. The detailed information can be found at:

http://earthsystemmodeling.org/regrid/
For an ideal situation, the ABI high-resolution TOA SW fluxes should be mapped into the CERES footprint for validation as suggested by the Reviewer. However, there are reasons that make it difficult to do so. For example, the case 12/26/2019 UTC 19. There could be more than 18000 pixels in a single swath of the SSF if constrained to the region of U.S. Different pixels have different times. Neglecting the seconds, there are still more than 30 mins differences (this changes case by case) between the first pixel and the one at the end and this brings up a time matching issue. But if remapping the SSF to ABI, we can set up a unique time for ABI (ABI is at 5 min intervals) and then constrain the region and the time range of SSF.
Both remapping the ABI to SSF and re-mapping SSF to the ABI bring up spatial matching errors as recognized by the scientific community. In Figure 10, we show the SSF before re-gridding (Figs 10 (a) & (b)) and after re-gridding (Figs. 10 (c) and (d)). As seen, the fluxes after re-mapping CERES SSF to the ABI resolution resemble well the original CERES. A case of reverse mapping is shown in the Appendix and indeed as the Reviewer suggested, it reduces the edge effects. Another consideration is the computational efficiency of re-mapping the curvilinear tripolar grid to unconstructed grid. For large arrays, it is more efficient to remap the unconstructed grid to the curvilinear tripolar grid.

Reviewer 1

317: 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.

Authors

References have been added to the text.

Reviewer 1

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.

Authors

We have removed now section 5.1.2 since it caused some concerns.

Reviewer 1

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.

Authors
The GOES data come in 5 min granule but do not provide a time stamp for each pixel.

Minor comments

Reviewer 1
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.

Authors
Is removed now.

Reviewer 1
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.

Authors
Thank you. We have now referenced this paper later in the manuscript.

Reviewer 1
L81: Down arrow in the text is out of place and should be removed.

Authors
Cannot find this arrow.

Reviewer 1
L129: Are the “surface variables” also part of the SeeBor dataset, or added by the authors? Please clarify when the dataset is first introduced.

Authors
The surface variables are part of SeeBor but we did not use them. We used information from MODIS.

Reviewer 1
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).

Authors
Spectral albedo.

Reviewer 1
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.

Authors
This is in reference to the temperature profiles. There is a positive bias at lower altitudes and negative bias above 1 mb. Was added to text.

Reviewer 1
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.

Authors
Done.

Reviewer 1
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".

Authors
Done.

Reviewer 1
L146: In addition to aerosol scattering, what about the role of absorption? The 6 aerosol types considered presumably have different single scatter albedo.

Authors
Build in the MODTRAN model. They represent aerosols with different single scatter albedos.

Reviewer 1
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.

Authors
288,000 is wrong. It comes from 6 aerosol types x 100 profiles x 480 angles. However, the 480 angles do not include edge angles, that is, 0^o for solar zenith angle, 0^o and 180^o for relative azimuth angle and 180^o for satellite viewing angle. If including these edge angles, the number of MODTRAN simulations for each surface type is 462,000.

Reviewer 1
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.

Authors
We do not use this option. Text modified.

Reviewer 1
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.

Authors
The Gaussian angle cosines are equally spaced. The angles were previously selected for flux computations.

Reviewer 1
L182: "azimuth angle" should be "relative azimuth angle", I think.

Authors
Corrected to relative azimuth angle.

Reviewer 1
L184: "ignoring spherical geometry" – what does this mean?

Authors
The satellite zenith angle at the surface and at the TOA would be little different if considering the earth spherical curvature. For simplicity, we assume plane parallel geometry when converting satellite zenith angle from TOA value to surface value.

Reviewer 1
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?

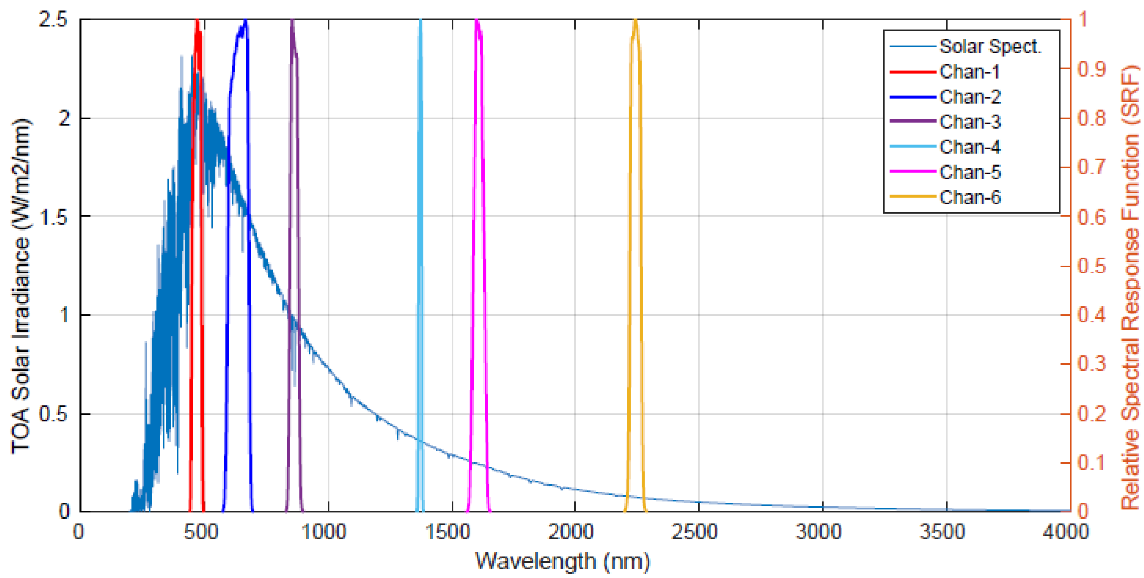
Authors
We used a scaled two stream RT solver to speed up the simulation. It is basically a two-stream scheme that is calibrated with 8-stream solution at a few spectral points. The accuracy benefit obtained from a higher number of streams may be totally lost in between the anchor points where a two-stream scheme is used. Also, we are mainly focusing on spectral conversion from narrow to board band, the impact of number of streams may be not that significant.

Reviewer 1
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.

Authors
As illustrated in the Figure. We did not keep all the data so difficult now to get numerical values.

Reviewer 1
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.

Authors
We have replotted Figure 8 in wavelength.



Reviewer 1
L264: "Figure 9" -> "Fig. 9" for consistency.

Authors
Corrected.

Reviewer 1
L292: "2019however" – needs fixing.

Authors
Done.

Reviewer 1

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

Authors

The basic reason is that the GOES-R data (for the CONUS region) are provided in granules of 5 min interval. We set up a unique time of ABI and then constrain the region and the time range of CERES SSF.

Reviewer 1

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

Authors

CERES.

Reviewer 1

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

Authors

We could not find the line number but we have modified the text so perhaps it is gone.

Reviewer 1

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?

Authors

We have modified the section numbering so this problem is gone.

Reviewer 1

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

Authors

Yes.

Reviewer 1

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?

Authors

The solar irradiance in the spectral interval assigned to channel 6 is 87 w/m2. Included in Figure 8 discussion.

Reviewer 1

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

Authors

Removed.

Reviewer 1

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

Authors

Corrected.

Reviewer 1

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

Authors

Done.

Reviewer 1

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

Authors

It is explained in "https://www.goes-r.gov/spacesegment/ABI-tech-summary.html", "*TABLE I. Summary of the wavelength, resolution, and sample use and heritage instrument(s) of the ABI bands. The minimum and maximum wavelength range represent the full width at half maximum (FWHM or 50%) points. [The Instantaneous Geometric Field of View (IGFOV).]*"

Reviewer 1

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.

Authors

Done.

Table 2. Surface classification description for IGBP 18 types, IGBP 12 types, CERES clear sky 6 types, and NTB cloudy sky 4 types

<table>
<tr><th>IGBP (18 types)</th><th>IGBP (12 types)</th><th>CERES clear-sky (6 types)</th><th>NTB cloudy-sky (4 types)</th></tr>
<tr><td>Evergreen Needleleaf</td><td rowspan="2">Needleleaf Forest</td><td rowspan="7">Mod-High Tree/Shrub</td><td rowspan="13">Land</td></tr>
<tr><td>Deciduous Needleleaf</td></tr>
<tr><td>Evergreen Broadleaf</td><td rowspan="2">Broadleaf Forest</td></tr>
<tr><td>Deciduous Broadleaf</td></tr>
<tr><td>Mixed Forest</td><td>Mixed Forest</td></tr>
<tr><td>Closed Shrublands</td><td>Closed Shrub</td></tr>
<tr><td>Woody Savannas</td><td>Woody Savannas</td></tr>
<tr><td>Savannas</td><td>Savannas</td><td rowspan="6">Low-Mod Tree/Shrub</td></tr>
<tr><td>Grasslands</td><td rowspan="3">Grasslands</td></tr>
<tr><td>Permanent Wetlands</td></tr>
<tr><td>Tundra</td></tr>
<tr><td>Croplands</td><td>Croplands</td></tr>
<tr><td>Open Shrublands</td><td>Open Shrub</td></tr>
<tr><td>Urban and Built-up</td><td>Open Shrub</td><td>Dark Desert</td><td rowspan="2">Desert</td></tr>
<tr><td>Bare Soil and Rocks</td><td>Barren and Desert</td><td>Bright Desert</td></tr>
<tr><td>Snow and Ice</td><td>Snow and Ice</td><td>Snow and Ice</td><td>Snow and Ice</td></tr>
<tr><td>Water Bodies</td><td>Ocean</td><td>Ocean</td><td>Water</td></tr>
</table>

Reviewer 1

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

Authors

Relative azimuth angle”.

Reviewer 1

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

Authors

We reference it now.

Reviewer 1

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.

Authors

Done.

Reviewer 1

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

Authors
Thanks. Done.

Reviewer 1
Fig 1: Remove arrow leaving bottom box.

Authors
Done.

Reviewer 1
Fig 2: Remove floating arrow leaving the left of the first box.

Authors
We did not see it in our version.

Reviewer 1
Fig 3: End of caption is missing.

Authors
The arrow is indeed there in paper_2_GOES_R_ 05_27_2021_revised_08_05_2021.docx.
It is not in the latest version that will be submitted with the responses, so it is good.

Reviewer 1
Fig 3: Top of figure seems to be cut off.

Authors
Fixed now.

Reviewer 1
Fig 4: Suggest removing "(logarithmic scale)" from the caption. The error bars are plotted on the same (linear) scale.

Authors
Done.

Reviewer 1
Fig 6: Are these nadir radiances at TOA? What is the scene type? Need to include this information in the caption.

Authors
It is nadir.

Reviewer 1

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.

Authors

We opted to leave it as is.

Reviewer 1

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.

Authors

It was re-plotted.

Reviewer 1

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

Authors

Done.