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Interactive Comment

Interactive comment on "Next-generation angular distribution models for top-of-atmosphere radiative flux calculation from the CERES instruments: validation" *by* W. Su et al.

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General comments:

The manuscript describes the validation activities performed to assess the accuracy of the CERES angular distribution models (ADMs) described in Su et al., 2010. This paper is of paramount importance to the scientific community in order to understand the uncertainties and limitations of the CERES flux new product Ed4SSF. This manuscript follows the studies developed in Loeb et al., 2003, 2006, 2007 and explores new approaches such as assessment of parallax effects or scene misidentification errors.

In my opinion, the manuscript is interesting and well written. This paper, once published, should be cited in all new papers using CERES derived fluxes, and justifies the publication of the previous Su et al., 2010 paper; which shows the new proposed methodology for the CERES ADM construction.

In general, I would recommend to publish the manuscript. However, I would like to see the authors response to some critical points. In particular, I am not very convinced about the methodology selected to assess the scene ID errors.

Please find below my comments.

——————— Specific comments: ———————

Section 2.1

In my opinion, the methodology behind the direct integration is not sufficiently explained.

I know that the method has already been discussed in Loeb et al. 2007 and presented in Loeb et al., 2003 (for the non sun-synchronous TRMM satellite), but I still think that a brief mathematical description is necessary.

The general idea is clear, but the details of the method are confusing. For instance, it is not explicitly stated in the text that the ADM predicted radiance for every angular bin within the study region is obtained using the DI-derived flux and the CERES ADM (anisotropic factor) corresponding to the observed scene. More details would be welcome.

According to Fig. 1, it seems that the results show a positive TOA SW flux bias over the ITCZ in January and a negative bias over the ITCZ in July (not so clear though). It would be nice if the authors could discuss it in the text.

Section 2.2

Both RMS and bias flux errors obtained with the Ed.4 LW ADMs are higher than the errors obtained using the previous Ed. 3. That's weird since the Ed. 4 ADM is an improved version of Ed. 3.

How do the authors justify this error increase? In particular considering that the LW flux consistency check reports a reduction of 2-3 Wm-2 over the errors obtain with Ed. 3 (Loeb et al., 2007). These test quantify the viewing geometry dependence of the ADMs. Thus, could it be due to a worse scene ID (scene misidentification) for some new identified regions?

Page 4495, Line 21: I do not understand the meaning of that sentence.

Section 3

Why do not the authors use the same approach of section 4 to determine the ADM error in the study with MODIS measurements? You could employ Eq. 7 to deduct the error derived from the nb2bb conversion from the total error.

Section 3.1

I was just wondering, which average flux values do you use to obtain the "absolute" RMS errors?

Page 4498, Line 8: Since the authors are employing CERES AT data for your consistency check they could use CALIPSO data to discriminate thin clouds from aerosols with high AOD.

Section 3.4

I appreciate the effort of the authors to provide an estimate of the CERES flux uncertainties. That information is highly demanded by the scientific community. However, I have doubts in the approach described in this section.

You assume that your methodology to obtain the "scaling factor" is independent on the employed ADM. But results of the consistency check obtained from the idealized ADM (which I assume is obtained from the ratio between RT radiances and corresponding

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fluxes) is likely giving pretty different relative RMS errors than the Ed.4 ADM.

In addition, how do the authors compute the theoretical fluxes from the 3D RT models? How do they define the simulated scenes? 3D models usually provide a TOA flux (e.g. 100 km) obtained from limb-to-limb radiance observations; which is different from a ADM-derived flux. ADM-derived fluxes are dependent on the observed scene within the simulation domain, however modeled fluxes are obtained for the whole simulated domain.

It would be nice if you could describe better the methodology.

Section 4

In order to check the dependence of the ADMs on the instrument measurements, it would be nice to compare the ADM errors obtain in the MSIR consistency check against the results obtained from MODIS.

Since the CERES-MISR-MODIS data is available, the authors could reproduce the study of section 3 using only the MODIS data corresponding to the cases studied here.

During the TOA SW flux consistency study, which footprint size do you use to create the CERES-like MISR database? CERES 20x20 nadir spatial resolution?

Section 4.2

I would like to point out that the RLRA is not always appropriate for collocation of multi-angular broadband radiances. Semi-transparent cirrus or broken clouds are difficult to handle with the RLRA method, and usually the most significant radiatively layer of clouds is not at the top of the cloud.

Has been addressed the parallax due to clouds in the optical path of oblique observations collocated at the RLRA determined for a nadir cloud?

Section 5

I think the use of C3M-enhanced data to improve the scene ID will result in the introduction of biases in the retrieval of ADM-derived fluxes unless the flux inversion is performed for ADMs constructed with that scene ID definition.

The C3M-enhanced product uses radar and lidar A-train measurements to "improve" the cloud detection. If you define your scene ID based on this new retrieval you should "re-train" your ADM models to be consistent with the new scene definition.

The Ed. 4 models rely on the cloud detection provided by MODIS (Minnis et al., 2010). Clouds that are, by definition, not captured by the CERES-MODIS cloud-mask are part of the intra-scene variability inherent to the clear-sky ADMs. Even if you are able to detect them with an improved scene ID the cloudy ADM cannot provide a better estimation of the flux because no ADM has been produced to model the angular behavior of such clouds. The same can be applied to the rest of cloud retrievals that determine the anisotropic factor selection of cloudy scenes.

Thus, I would not say that biases in the cloud retrievals determination of MODIS, i.e. limitations of imager-based cloud retrievals, result in misidentification errors in CERES derived fluxes. Those limitations are part of the CERES approach. I consider misidentification errors scenes that are occasionally not well determined, but MODIS, in general, identifies them correctly. Those are the scene ID errors that affect the selection of anisotropic factors for flux conversion.

I my opinion the authors comment in page 4512, line 5 the most relevant study to perform on this topic, that is to assess the error caused by misclassification of scenes during the ADM construction.

—————————- Technical corrections: —————————-

Page 4503, Line 8: Table number is missing.

Figures 6-9: These plots are difficult to read. The idea of the "thin" and "thick" bars is terrible, please try to use another way to represent them.

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