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Atmospheric Measurement Techniques
Discussions

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

W. Su et al.

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Dear Dr. Domenech,

Thanks for your comments. Please see our response below (in italics).

The manuscript describes the methodology employed to derive the current angular distribution models (ADMs) for the CERES instruments, i.e. the radiance-to-flux conversion algorithms for CERES thermal and solar measurements. This paper is necessary to document the new CERES product Ed4SSF, which improves the retrieval of cloud parameters and TOA flux with respect to previous versions. The enhancement of the

cloud retrieval algorithms is briefly mentioned here, since the manuscript's main objective is to update the algorithms discussed in Loeb et al. (2005) and Kato and Loeb (2005).

In my opinion, the manuscript is of a great interest for the ERB community. This paper, once published, will be cited in all new papers using CERES derived fluxes. It would be desirable, however, to evaluate this paper together with its second part. There is no results section in this paper, it just shows the proposed methodology. Only the "validation" part will totally justify the selection of the new methods. I would rather prefer to split the current manuscript in two parts: one describing the SW algorithms and the second to discuss the LW approach, but including the corresponding evaluation of results. In any case, the current manuscript is already significantly long. Thus, the current distribution of papers could be the right choice.

We are working on the validation portion of the paper, and we hope to submit it soon. We will keep you updated on the progress.

In general, I would recommend to publish the manuscript. However I would like to raise some general comments.

Section 7 is very interesting. The comparison of the new developments against the previous results from Loeb et al. (2005), which are currently the reference models for empirical flux retrieval, shows a significant improvement for certain regions. This part should be enlarged and emphasized. The summary of the conclusions does not provide any extra details, however a better discussion of the importance of the flux results improvement and its implications regarding the ERB would be very welcome.

Section 7 discusses the effect of changes in scene identification and ADMs on TOA fluxes, it intends to show the TOA flux differences rather than improvement caused by changes in scene identification and ADMs. The improvements that have been made in quantifying the Earth radiation budget will be discussed more in the validation paper.

I would also like to see more information on the "new" scene ID. According to Fig. 18, the impact in the retrieved flux due to changes in the cloud algorithms is larger than the flux differences caused by the use of new ADMs. However, there is only information concerning changes in the cloud algorithms in the introduction.

We briefly described the "new" cloud algorithm in the Introduction section, as a detailed discussion of the new cloud algorithm warranst a separate paper (which will be led by dr. Minnis). Also a glimpse of the cloud fraction difference can also be seen in Fig. 19. The impact of cloud algorithm change on flux might be larger than that from ADM change on a regional basis, but on a global mean basis, they are equivalent. For example, the global mean SW flux decreased by 0.3 Wm^{-2} due to cloud change but increased by 0.6 Wm^{-2} due to ADM change. The large changes noted in the left panels of Fig. 18 result mainly from sample number differences between Ed2SSF and Ed4SSF for reasons we discussed in the paper (1st paragraph on page 8847).

Figures 2 and 7 show clearly the differences between the two flux datasets (Ed2 and Ed4) under certain regions. I consider interesting to see the results for the rest of scene types.

Figures 2 and 7 show the RMS errors between normalized measured radiances and normalized ADM predicted radiances (equation 4). We used RMS error (equation 4) to assess the anisotropy characterization for all scene types. All ADMs described in this paper produced smaller RMS error than those described by Loeb et al. (2005) because of improvement in ADMs and/or scene identification. Due to the length of the paper, we can't include RMS error figures for every scene type, but the RMS error figures over most of the scene types are available on the CERES science team meeting web page.

It is not clear to me how the sea ice and fresh snow presence is detected. There are two different sources to detect the fresh snow and sea ice: a combination of NSIDC and NESDIS data, and MODIS radiance's thresholds (cloud mask snow/ice fraction). But, then, I do not see those parameters being use when reading sections 4.3 and 4.5.

Snow/ice maps from NSIDC and NESDIS are described in the Observation section, and cloud mask snow/ice fraction is also briefly described there. A detailed description of the cloud mask snow/ice fraction was provided in Minnis et al (2008), and we added this reference. We used cloud mask snow/ice fraction in sections 4.3 (page 8833) and 4.5 (page 8839) to determine the snow fraction and sea ice fraction.

Specific comments:

Subsection 4.1.1. How sensitive is the broad-band radiance field to changes in the aerosol burden/type? Which is the flux difference using the theoretical adjustment from Loeb et al. (2005) and actual AOD?

The broad-band radiance field tends to be more isotropic as aerosol loading increases and/or aerosols become more absorptive. The new ADMs that account for AOD tend to increase fluxes near polluted coastal regions compared to the fluxes calculated using the theoretical adjustment, and the global annual mean difference is less than 1 Wm^{-2}.

Subsection 4.2.1. Have you tried to use a non-linear model like RPV instead of a kernel-based BRF model?

Yes, we did and found that the RossLi model works better.

Subsection 4.2.2. Which is the reduction in relative RMS errors compared to Loeb et al. (2005) due to the new ADM?

The RMS error distributions using the ADMs from Loeb et al. (2005) and the ADMs described in this paper are provided in Fig. 1 for April 2002 using Terra observations. The RMS error is reduced from 10.5% to 9.8%.

Section 4.3. Why do you treat differently the fresh snow over clear sky and the fresh snow of clear parts in cloudy scenes?

This is because we only have CERES broadband radiance measurements over the *whole CERES footprints and cannot invert fluxes separately for the clear-portion of the CERES footprints and the cloudy-portion of the CERES footprints.*

Section 5.2. In case of mixed surfaces you use the predominant surface type. However, over ocean, land and desert the anisotropy is calculated by averaging among the different surface types. Is there any reason not to do it here?

We chose this strategy because there are significant differences in anisotropy among ocean, land, and desert (see Fig. 2 left), whereas the difference in anisotropy among fresh snow, sea ice, and permanent snow are negligible (see Fig. 2 right) for viewing zenith angles less than 70 degrees (which is the limit of CERES viewing zenith angle in cross track mode).

Technical corrections:

There is a typo in page 8819, 1st sentence.

Thanks, we will fix it in the new version.

Figure 11 and 15. Could you please include the legends?

We added legend to Figure 15. For Figure 11, we explain what each color represents in the caption and feel it is rather cumbersome to add a descriptive legend to the figure.

Figure 18 and 19. Please define the error metric employed.

We have added this to the caption.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 8817, 2014.

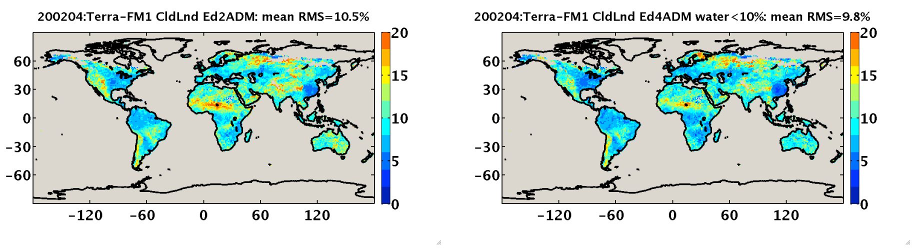

Fig. 1.

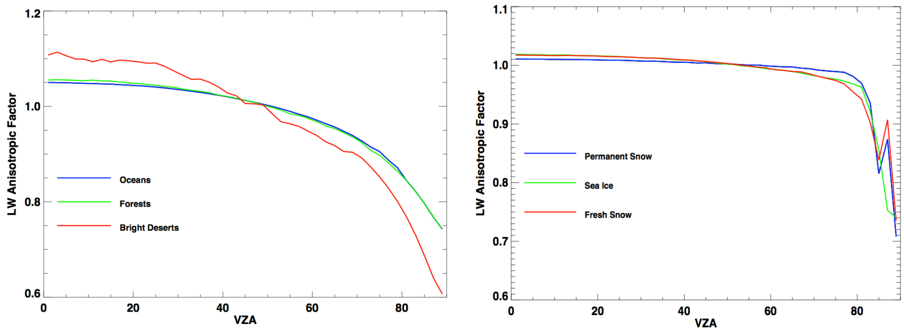
1.2
1.0
0.8
0.6
0
20
40
60
80
VZA
LW Anisotropic Factor
Oceans
Forests
Bright Deserts
1.1
1.0
0.9
0.8
0.7
0.6
0
20
40
60
80
VZA
LW Anisotropic Factor
Permanent Snow
Sea Ice
Fresh Snow

Fig. 2.