Review of "Using Two-Stream Theory to Capture Fluctuations of Satellite-Perceived TOA SW Radiances Reflected from Clouds over Ocean" by Tornow et al.

16 June 2020

General comments

This study applies a semi-physical model to predict the hemispheric radiance distribution of reflected solar radiation by clouds over ocean. When comparing the predicted radiance fields against those from empirically based CERES ADMs, the authors find smaller residuals against CERES observations at many solar-viewing geometries. The value of accounting for cloud effective radius and above cloud water vapor in the new method is highlighted.

The paper is nicely written and of interest to the community. I find it remarkable that a 2-stream cloud albedo and simple representation of radiative transfer can compete with the state-of-the-art CERES ADMs. I do, however, believe that the manuscript would benefit from some more transparency in the relative shortfalls of the new method, and several clarifications. After addressing these minor comments outlined below, I recommend prompt publication in AMT.

Minor comments

L13: I find these statistics alone are a bit misleading. After first reading these numbers, I was then slightly disappointed when reading the text and figures to see that some geometries are much worse, and improvements are often much more marginal. Instead of just stating "up to" values, I believe a more honest representation of the results in the abstract should also mention how frequently improvements are seen and/or typical improvements.

L22-23: Is it correct that EarthCARE will use observation based fluxes in the closure assessment? My understanding is that EarthCARE will use observed radiances for this purpose.

L33: CERES ADMs are developed from years of observations, not months. This is actually mentioned later in the manuscript.

L35-36: ERBE only defined 2 scene types containing cloud over ocean. There was also clear sky ocean (technically containing cloud cover up to 5%), and an overcast scene that did not separate surface types. I assume these are the 4 scene types the authors refer to here, but it is probably worth making this distinction.

Eq. 1: Best to define "g" explicitly since it is defined later as the asymmetry parameter. Is there a unit inconsistency in these equations?

L92: Why cut off SZA specifically at 82 deg?

Fig 3: Can you comment on the asymmetry either side of the sun-glint? "Coakley-Chylek refl. surface" gives smaller residuals at viewing zenith angles plotted to the left of the sunglint, but generally worse or comparable to the right. The opposite is true for Fig 6a.

Fig 4: The meaning of the sign of the change should be noted in the caption. I worked out that negative change means the Log-Linear is better, but I had to read the text to get that.

L265-267: Similar to my second comment above about statistics in the abstract, I think these summary sentences over-clam the results somewhat. The proposed log-linear model *sometimes* outperformed the existing sigmoidal approach, but there were also many geometries when it did relatively badly. That should be acknowledged as part of these summary sentences.

Grammatical corrections

L13-14: “radiance residuals”->”radiance residuals calculated against CERES observations”. It is worth mentioning in the abstract that they are residuals against observations. This may not be obvious to a reader who just picks up the abstract.

L49: Given the importance of water vapor above cloud, I recommend “role of single scattering”->”role of solar absorption and single scattering”.

L56: “semi-statistical”->”semi-physical”. Better to use consistent language throughout.

L80: “(“Note for cloud layer”)”. I do not understand the meaning of this.

L86: “those” -> “whose”