

## ***Interactive comment on “A geometry-dependent surface Lambertian-equivalent reflectivity product for UV/Vis retrievals: Part II. Evaluation over open ocean” by Zachary Fasnacht et al.***

### **Anonymous Referee #1**

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#### General comments

The paper presents a Lambertian-equivalent ocean surface reflectivity product for UV and visible wavelengths. The product is based on modeling of the outgoing radiation over Case 1 water scenes and takes into account satellite viewing geometry and ocean surface roughness. The product is a significant improvement over climatological Lambertian reflectivity datasets used in many cloud, trace gas, and aerosol algorithms. A significant part of the paper is devoted to the evaluation of the product's performance in comparison with surface reflectance derived from the OMI instrument. The paper is well structured and the presentation is mostly clear. There are some issues with

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## Specific comments

Section 3.1: The analysis is based on data for a single month (January). It is evident from Fig. 9 that there is a large seasonal variability in the lambertian-equivalent ocean surface reflectivity which is due to the changing viewing geometry as well as changes in the input parameters. It is unclear if the numbers you quote in the section are applicable to other seasons or to the whole product. I suggest either adding data for June or redoing the analysis for a yearly (sub)sample. This is especially pertinent to any use of this product as a replacement for climatological datasets.

p. 10, l.1: Quote: “the cloud screening methods produce similar results with only small differences that do not impact the overall evaluation.” To support the above statement I suggest adding a third table showing statistics for the left column of Figure 1.

top of the p. 11: I do not see “two main regions” in Fig.2 . It is not clear what two “distributions” the authors refer to as there is not clustering in the data. The range of 0.2-0.4 mentioned in the text appear to be arbitrary.

p. 15 Fig 8 and its analysis in the text: Figure 8 attempts to analyze the influence of aerosols based on the data from a single random orbit with a specific dependence of AOD on VZA. This analysis is obviously statistically insignificant and thus meaningless. A physical quantity like AOD should not depend on the observational geometry. Any such dependence is an indication of either a problem with the data or a lack of statistical power of the dataset. I suggest either removing this figure or redoing the analysis based on a better sample.

p.16, caption for Fig.9 and p.17 l. 8: Readers should not be expected to be familiar with the ONI row anomaly. Some discussion and explanation of why a specific row was used is needed

p.17, l. 14: Quote: “ in Fig. 10 there is a small downward trend in the difference between GLER and OMI-derived LER of at most 0.005 LER. This may be related to the

downward drift in the OMI measurements” While it may be correct, the authors do not present enough evidence to support the statement and do not consider other possibilities. Trends in the auxiliary may be responsible. The authors used wind datasets from two different instruments with the switch occurring in the middle of the data series. How do the two datasets compare and could the switch affect the trend? In order to support their statement the author could adjust the calculations for the downward drift in the OMI measurements and see if they can reproduce the trend.

p.18 l.22 Wind speed and chlorophyll are two independent variables. Please describe how they were jointly perturbed to produce the results in Table 5.

#### Technical corrections

p.2, l. 5: due to p.2, l. 12: “angle” is missing after viewing p.8 Section 3.1 tables and graphs: The correspondence between tables and plots is not clear. Please state that Table 1 provides statistics for right column plots of Fig.1 in the caption. Same for Table 2 p.9, Fig. 1 caption’s last sentence : Clarify that the left and right columns are for two cloud screening methods. p.20, l. 17: “combination of things” does not sound good; effects or factors?

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