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Atmospheric Measurement Techniques Discussions

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.

Zachary Fasnacht et al.

zachary.fasnacht@ssaihq.com

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We greatly appreciate and thank the reviewers for their efforts related to this manuscript. They have provided important comments which have lead to several improvements in the paper. All comments from reviewers have been addressed below. *Reviewers' comments/questions below are denoted with italics*, responses are in plain text, **and additions to the manuscript are in bold.**

Responses to Reviewer 1:

Section 3.1: The analysis is based on data for a single month (January). It is evident form Fig. 9 that there 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.

In response to the reviewer's first comments, we have included July 2006 results in the analysis for Section 3.1 and additionally added a table to show the statistics when MODIS GCF = 0.0. The reviewer makes a good comment about this quote which has been revised to note that the Raman based ECF leads to a better comparison than the MODIS GCF.

Pg 9 lines 3-5, we made a few changes to discuss the July 2006 data:

"As shown in Table 2, the GLER and the OMI-derived LER compare best **in January** at 388 nm where R^2 is 0.76 and the bias is 0.002 **with the Raman ECF (using the MODIS GCF $\mathbf{R^2}$ is 0.60 and the bias is 0.007).**"

Pg 10 lines 1-6 were changed to account for the addition of the MODIS GCF in the comparison tables and the quote the reviewer noted above was changed

"**Overall the comparison is better using the Raman ECF cloud screen than when using the MODIS GCF. This is expected given that there is a 15 minute window between the** Aqua and Aura **overpass times in 2006 (becomes 7 minutes in 2009)**

leading to some change in cloud cover. It is also worth noting since OMI has a wider swath than MODIS, cloud retrievals are not available **from MODIS** for pixels on the edge of the OMI swath (these pixels are not shown in Fig. 1). **For these reasons the Raman ECF will be used for cloud screening in the rest of the paper.**"

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.

To make this paragraph more clear we have removed this statement as it has no impact on the results.

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.

The reviewer was correct that we were showing only a limited sample so we have now included all orbits for April 10, 2006 over the Pacific Ocean. The original figure showed spatial dependence in AOD which caused a crosstrack dependence of AOD with higher AOD on the west side which is close to Asia. By adding the orbits for the rest of the day over the Pacific Ocean, this issue has been resolved (see Fig 8). This analysis is simply a small case study that shows the possible impact of aerosols. A more rigorous evaluation is needed to determine the exact quantitative impact of aerosols but is outside the scope of this paper and will be investigated further in the future.

We have reworded text in section 3.3 to make it more clear that this is simply a case study to show possible aerosol effects on the GLER results.

Pg 14 lines 6-8 the following word choice changes were made:

"Figure 5 shows the **MERRA-2 AOD and the LER change for orbit 9229 where AOD ranges** from around 0.05 in the South Pacific gyre to larger than 0.4 in the northern Pacific."

Pg 15 line 6-7, we updated the 466nm slope and RMSE for additional orbits in aerosol analysis

"The combination of these changes improves the slope from **1.16** before considering aerosols **at 466nm** to **1.0** after aerosols are introduced."

Pg 15 lines 10-11, following sentence added to note the LER difference across the swath: "Figure 8 shows that aerosols increase GLER generally by 0.01-0.02, with the largest increase at large forward scattering angles."

Pg 15, line 12, stylistic change was made to remove "on the whole"

Pg 15, line 16-17, stylistic change was made to change "little to no change" to "**small decrease in LER"**

Pg 16, lines 3-8, some text was reworded to emphasize that this is simply a case study

"**In this case study,** we note that an AOD of 0.1-0.15 **increased the LER by** as much as **0.01-0.02 at 466nm, with the largest increase being** in the forward scattering direction. At 354nm, however, similar AOD values **slightly decrease LER in the backward scatter** direction, but can increase LER by as much as 0.01 in the forward scattering direction. **While this analysis was for only a specific case study, we note that the aerosol contribution likely accounts for some of the difference between GLER and the OMI-derived LER.**"

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

We have added the following text to section 2.6, pg 7, lines 14-20:

"**Beginning in mid-2007, OMI experienced an anomaly known as the "row anomaly" that has affected the L1b radiance data. There have been several impacts from the row anomaly including decreased radiances due to possible blockage, increased signal due sunlight being reflected into the instrument, a wavelength shift due to a change in the slit function, and earthshine radiances from outside the FOV that are reflected into the nadir port. The row anomaly is further explained in Schenkeveld et al. (2017). For this reason, after 2007 we focus only on rows 1-21, which are not affected by the row anomaly.**"

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.

The reviewer raises an important point about other possible causes for the trend in the LER difference. We do note, however, that if the cause of the drift was the wind speed, the largest change would be at the longer wavelengths where the direct reflectance is more important and wind speed uncertainty is the greatest as shown in section 3.5.

Additionally, reviewer 2 has requested the inclusion of sun-glint angle, wind speed, and chlorophyll in Fig. 9. The change as noted in 2011 in the wind speeds is quite small and there is no apparent drift after this point. From 2016-2018, both wind speed and chlorophyll appear quite consistent, but the GLER-OMI difference still appears to drift through these 3 years.

Attached we have included Fig. 1 of the response in which we took Fig. 9 and deseasonalized the data in order to better see the trends. As seen in Fig. 10, the trend in the LER difference at 354nm is only 0.005 and negligible at 466nm. Based on Fig. 1 of the response, at 354nm it appears that while GLER seems consistent, OMI LER does decrease starting around 2011/2012. We do note, there are some year to year changes in LER possibly due to phenomenon such as El Nino/La Nina, but such changes are captured both by GLER and OMI-derived LER.

The general consensus is that the OMI drift is 1-1.5% radiance through the first 10 years of the OMI mission as noted by Schenkeveld et al. 2017. We see a drift of about 0.005 in LER at 354nm, which corresponds to around a 1% drift through the OMI mission. The OMI team is currently working to assess the drift and will be applying a correction to the radiance data in the next version of L1b processing.

Pg 16 lines 23-26 were changed to be more precise about the current accepted rate of the OMI drift:

"We note that in Fig. 10 there is a small downward trend in the difference between GLER and OMI-derived LER of at most 0.005 in LER. **At 354nm a change of 0.005 LER corresponds to approximately 1% in TOA radiance which is close to the 1-1.5% TOA radiance degradation noted by Schenkeveld et al. 2017.**"

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.

We re-calculated GLER by perturbing the wind speed and chlorophyll in either direction by their assumed uncertainty. In the case of the combined perturbations, we simply perturbed each input in 4 possible directions (CHL high & WSP high; CHL low & WSP low; CHL high & WSP low; CHL low & WHP high) and then simply averaged the magnitude of the difference between the original LER and the adjusted LER. Additionally, we determined the maximum LER difference from all possible scenarios.

Pg 19 Lines 14-19 were changed (updated text in bold) to address this concern:

"**To determine the combined effect, we additionally calculated GLER perturbing both the wind speed and chlorophyll for the four possible combinations.** Table 6 shows the mean difference **from the combined sensitivity analysis** in GLER is similar to that obtained **by only perturbing the chlorophyll**. The maximum difference from the combined sensitivity test, however, is similar to that of the wind speed perturbation. This is because while the wind speed has a significant impact on sun glint, only a small fraction of OMI pixels are impacted by glint."

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?

We greatly appreciate the reviewer noting these corrections and have made the corrections in the manuscript.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2019-260/amt-2019-260-AC1-supplement.pdf

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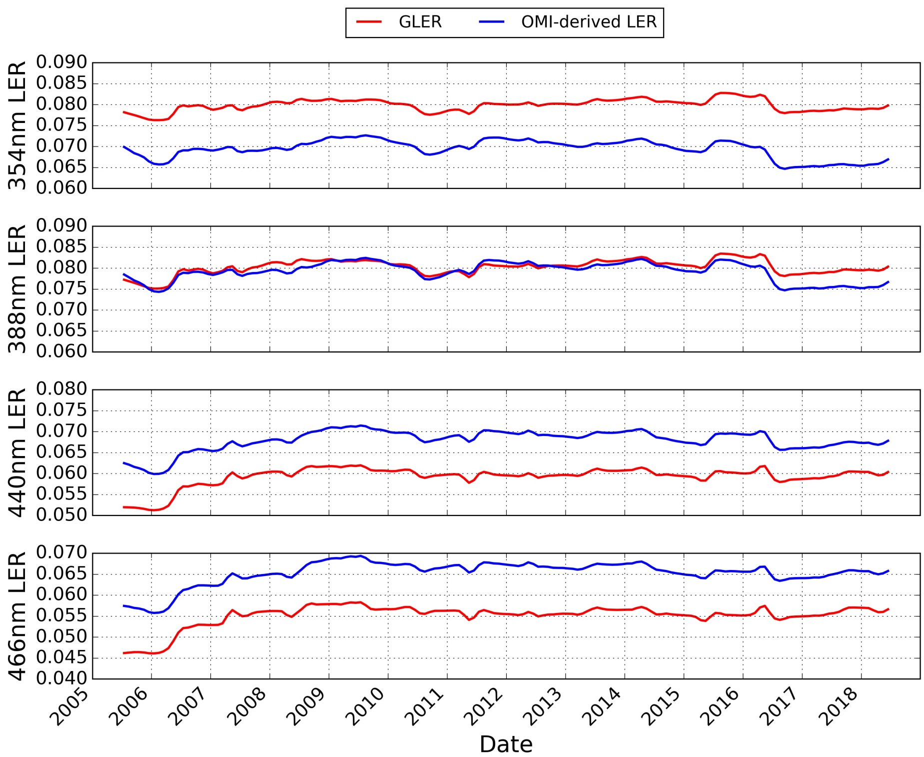

Fig. 1. De-seasonalized trend of GLER and OMI LER corresponding to Fig. 9 of the original manuscript. All data was screened the same as Fig. 9.