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Comment

Interactive comment on “Critical surface albedo and its implications to aerosol remote sensing” by F. C. Seidel and C. Popp

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Replies to Referee 1

We are very grateful for the thoughtful reviewer comments, which helped to considerably improve the manuscript.

Note Referee 1 has based his comments on our initially submitted manuscript before typesetting and refers therefore to a different line numbering convention. Nevertheless, our responses are based on the published discussion version and we provide the corresponding page and line numbers in our published discussion manuscript.

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General Comments

Comment 1 [...] *“I wish the authors had dealt with anisotropic surfaces instead of Lambertian targets. However they stated clearly the scope of the paper to address only isotropic scenes.”* [...]

Reply We appreciate the reviewer’s comment on the use of non-Lambertian surfaces and we will add a sentence in the last paragraph of the introduction summarizing the following reasoning. We have performed a purely theoretical analysis for a range of discrete spectral surface reflectance, or hemispherical-directional reflectance factor (HDRF) values. We think that taking into account non-Lambertian surfaces would complicate the interpretation of our results without much benefit. In addition, we are disinclined to introduce ‘real’ surface types, such as grass, water, vegetation, etc., because our results would be contingent on the inherent assumptions of the surface type’s BRDF. Instead, we believe that the AOD retrieval sensitivity as a function of continuous surface reflectance values (or albedo) provides more value to the reader. One can choose the reflectance value from any given BRDF at the corresponding scattering angle and find the AOD retrieval sensitivity for that reflectance value from our results. Technically, the BRDF would only change the spectral surface albedo a_λ in Eq. 5 from a scalar number to a vector containing a surface phase function as function of the scattering angle between the direction of the direct irradiance (sun’s position) and the viewing geometry.

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Specific comments

Comment 2 *“The abstract: the authors mention the retrieval of aerosols over bright surfaces such as clouds, snow and ice. Surprisingly, they do not mention bright desert scenes when the retrieval of aerosols above such targets is problematic. The authors address this issue at the end of Section 3.2.1 but it should be also mentioned somehow in the abstract.”*

Reply We have not mentioned particular surface types in the abstract to remain as concise as possible. We do not mention desert surfaces because we are emphasising that bright surfaces feature increased AOD retrieval sensitivities compared to e.g. desert regions, which are intermediate between dark and bright targets and closer to the critical surface albedo (CSA). Please see also our reply to the similar comment 11 of Reviewer 3.

Comment 3 *“Lines 120-130: the authors set one of the scopes of their work by considering only Lambertian surfaces. Did they look at cases of non-Lambertian surfaces? Are they intending to do so? In order to apply some of the results of this work to surface albedo and aerosol retrieval it seems to me that surface anisotropy should be considered.”*

Note This comment refers to page 7729 and lines 9 to 16 in the discussion manuscript.

Reply We have performed a purely theoretical study and believe that the use of non-Lambertian surfaces would not increase the value, but complicate the interpretation of our results. Please refer for more details to our answer on comment 1 above.

Comment 4 *“Lines 151-157: the authors say that the continental, urban and maritime aerosol models are taken from d’Almeida et al., whereas the desert and stratospheric aerosols are from Dubovik et al., and Russell et al. However the caption of Table 2 says that the desert aerosol type is from d’Almeida et al., whereas the biomass burning one is from Dubovik et al. Can the author clarify? What kind of desert particles do they consider (spherical or no-spherical [or spheroid])?”*

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Note This comment refers to page 7730 and lines 11 to 19 in the discussion manuscript.

Reply We apologize for that confusion. We correct the corresponding sentence on lines 13 and 14 on page 7730 by: “The background desert aerosol model was adopted from d’Almeida et al. (1991), the stratospheric volcanic aerosol model from Russell et al. (1996) and the biomass burning aerosol model from Dubovik et al. (2002).” The caption of Table 2 is already correct.

Comment 5 *“Again, in Section 2.2, it would be interesting to cover the case of non-Lambertian desert surfaces, with larger AOD values than presented.”*

Reply Please refer to our replies on comments 1 and 3 about the integration of non-Lambertian surfaces. Regarding AOD, we neglect large values with $\tau^{aer} > 1.0$ because they are rather atypical in nature and we would like to stay away from potential approximation errors in aerosol multiple scattering of the radiative transfer model. References to relevant literature are given in our manuscript to justify the former statement on AOD climatology.

Comment 6 *“Lines 200-205: the authors define the condition for retrieving the critical surface albedo, using the derivative of the fifth order polynomial that approximates the TOA reflectance. To be completely exact it should be added that the derivative should be zero for any value of the AOD. Otherwise, it is a minimum or a maximum.”*

Note This comment refers to page 7731 and lines 16 to 21 in the discussion manuscript.

Reply We apologize for our sloppy description on how we determine the CSA in this subsection. We will split this paragraph and put our method into a separate subsection called “Calculation of the critical surface albedo” following the current subsection 2.3. In addition, we will rewrite this paragraph completely to make it more clear and add an $= 0$ to Eq. 6.

Comment 7 *“Line 221-226: can the authors explain that part better and provide more*

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comments in the text to justify the changes in the CSA with AOD. They state earlier in the same section that the CSA is obtained when Equation 6 is zero. It should be true for any value of the AOD. Therefore I do not understand this result.”

Note This comment refers to page 7732 and lines 5 to 8 in the discussion manuscript.

Reply We will rewrite this part to improve the explanation of the CSA dependence of AOD. Although, the TOA reflectance does not depend on AOD at the CSA, the CSA itself depends slightly on AOD. Fig. 2b) shows in the zoom window nicely that the TOA reflectances does not cross at the exactly same surface albedo or CSA. With our strict definition of CSA, where two different AOD lead to twice the same TOA reflectance ($R(\tau_1) = R(\tau_2)$) at one specific surface albedo, we can show that ($R(\tau_2) = R(\tau_3)$) is also true but at another surface albedo. Thus:

$$a_{[R(\tau_1)=R(\tau_2)]} \neq a_{[R(\tau_2)=R(\tau_3)]}.$$

Nevertheless, the other reviewer makes the point that we use a strict and idealised definition of the CSA while in reality there is a range of surface. We will include and discuss this point in the discussions and in the methods part of our final paper.

Comment 8 *“Lines 231-235: please show on Fig. 5 the regression line with the slope, offset and the residuals. Avoid comments like “in general, the points are aligned and use in the text the results of the regression instead.”*

Note This comment refers to page 7732 and lines 12 to 14 in the discussion manuscript.

Reply Fig. 5 does not show a correlation analysis of two independent variables. We plot TOA reflectance values as a function of CSA. The combination of parameters going into Eq. 1 lead to the fact that the sum of the surface and atmospheric reflectances are close to the surface albedo. Imaging a spherical albedo of 0.06, a up- and downward transmittance of 0.97 each and the TOA reflectance value will be very close to the surface albedo for all albedo values from zero to unity. The atmospheric reflectance just adds a constant to that. A change in the values will also change the TOA reflectance

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as a function of surface albedo and must not be parallel to the 1:1 line any more. Thus, it does not make sense in our perspective to show a regression plot in Fig. 5.

Comment 9 *“Lines 261-262: “The CSA is almost spectrally neutral for absorbing SSA and vice-versa”. Please avoid such a formulation.”*

Note This comment refers to page 7733 and lines 9 to 10 in the discussion manuscript.

Reply We will improve that sentence. In particular, we will use the more common term “spectrally invariant”.

Comment 10 *“Lines 289-291: the minimum for the CSA corresponds also to the minimum in the aerosol phase functions (around 120 degrees)...”*

Note This comment refers to page 7734 and lines 3 to 4 in the discussion manuscript.

Reply We will improve that sentence together with the one before and after to make our point on the dependence of the CSA on the phase function more clear.

Comment 11 *“Table 2: the caption should be clarified as indicated previously, as there seems to be a discrepancy with the text of the paper (origins of the aerosol classes).”*

Reply The caption of Table 2 is correct. We will correct the corresponding text according to reply 4.

Comment 12 *“Figure 4: the caption should recall that the derivative is plotted at 550nm, as mentioned in the text.”*

Reply We will add the wavelength to the caption.

Comment 13 *“Figure 6 - Panel (b): is there a particular reason why a scattering angle of 104 degrees was taken for panel, instead of more rounded values like 100 or 110 degrees? Same comment for Figure 8.”*

Reply We have chosen common values for the viewing and solar geometries while their combination (see Eq. 4) may lead to somewhat unusual scattering angles.

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Technical comments

Comment 14 *“Line 77: “the CSA is also of relevance””*

Note This comment refers to page 7727 and lines 28 to 29 in the discussion manuscript.

Reply We will improve this sentence.

Comment 15 *“Line 492: reference Popp et al. The year of publication is missing.”*

Note This comment refers to page 7739 and lines 14 to 17 in the discussion manuscript.

Reply We have corrected that issue already during the typesetting.

Comment 16 *“Figure 2: the title of the Y-axis for panel (b) is half hidden. Please correct.”*

Reply We have corrected that issue already during the typesetting.

Comment 17 *“Figure 6 - Panel (f): what “A179 degrees” mean for the scattering angle? Looking at Figure 8, I guess it is typo.”*

Reply Yes, we will correct this typo.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 7725, 2011.

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