Response at AMTD stage

November 4, 2016

We are very grateful to referee #2 for the careful reading of our manuscript and for providing constructive comments which helped to improve the manuscript. This document includes all the referee's comments as well as our replies to every one of them (revised text are given as red color). ================================

General comments

Review of Wu et al., "Improved MODIS dark target AOD algorithm over land: angular effect correction", submitted for publication in amt (amt-2016-185)

The MS presents an improvement of the AOD retrieval using MODIS observations over dark targets, as compared to the MODIS DT product, by taking into account angular surface reflection effects to separate surface and atmospheric contributions to the reflectance measured at the top of the atmosphere. Improvements are especially for either small or large scattering angles. Using ASRVN data for three years, the authors derive a relationship for the spectral surface BRF. They also discuss the use of surface information through the NDVI evaluated for wavelengths in the short wave infrared (SWIR). They call their new algorithm BRFDT2 and results from this algorithm are compared with that using an earlier version (BRFDT, see below) and from the MODIS C6 DT product.

1. The work presented here is an improvement over that described in an article submitted to by the same authors to IEEE, 2016, which is frequently referenced. However, in view of the current unavailability of Wu et al. (2016) it is hard to judge what the differences and improvements are between the algorithm versions BRFDT and BRFDT2, apart from examples presented in the current MS. Hence the submission of the current MS seems pre-mature as it not possible to assess the information from WU et al. (IEEE 2016). Furthermore, as the IEEE MS is still under review and it's outcome is uncertain, the relevant information should be briefly summarized in the current MS submitted to AMT. For instance, on p. 1, line 4, the authors refer to a "relationship that applied in the previous study"; although I do not recommend that this relationship is given in the abstract, is should be discussed in the main part of the MS. This information is not provided in sufficient detail in, e.g. the discussion at the end of Sect. 1 (p3).

Reply: Thanks for the reviewer pointing out the confusing description in MS. We have modified the text in abstract, section 1 and 3 in revised manuscript to make it clear and understandable without seeing IEEE MS. By the way, we already resubmitted the previous study (Wu et al., 2016) to AMT on Nov 4, 2016. We hope that readers can get a fully understanding of our work.

2. The derivation of the BRF relationships builds on the data provided from the ASRVN; hence the methodology applied to provide ASRVN data should be described and references should be given. ASRVN is part of AERONET and uses both MODIS reflectances and AERONET data to develop their surface BRF data base (Wang et al., 2009). Hence the authors should explain why the AOD retrieved using the ASRVNderived BFR relations can be validated using AERONET AOD data:

could the relatively small improvement of the DT2 results be a consequence of the method used? Likewise, using the MODIS BRDF product (4, 23) implies that an atmospheric correction has been applied to produce this product. How is that accounted for?

Reply: Thanks for the reviewer's comments.

- The introduction of the ASRVN data is given in section 2.2 in revised manuscript, including the methodology and accuracy of the data.
- We state that the relatively small improvement of the DT2 results is mainly attributed to the large variability of BRF ratios in the algorithm (see discussions in section 4.4 in revised manuscript), other than the ASRVN BRF data.
- MODIS BRDF product is introduced in section 2.1 in revised manuscript, including the atmosphere correction and accuracy of the product.

3. Furthermore, the BRF relationships are derived as linear fits to scatterplots which clearly show that these relations are NOT linear (Fig 3), or with data points which are averages with very large standard deviations. I would expect a discussion of the effects of both the nonlinearity and the large uncertainty in the BRF data used in these fits on the retrieval results. The comparison between MODIS and AERONET AOD in Fig 12, almost all within EE, is too good considering that Fig 11 shows that for DT2 75% is within EE.

Reply: Thanks for the reviewer's comments and suggestions.

- We have recalculated the BRF ratio by using a second-order polynomial fit. The effect of the large uncertainty in the BRF ratio is discussed in section 4.4 in revised manuscript, shown as bellow:

  We also note that the BRF ratios still have a large uncertainty even by accounting for the scattering angle, which may cause errors of the AOD retrieval. The standard deviation for 0.644/2.12 ratio are about ±0.1 and even larger for 0.466/2.12 ratio (about ±0.12). To check the effects of BRF ratios uncertainty on the AOD retrieval, we performed a sensitivity test by given typical vegetation area where $R_{dd,2.12}$ is around 0.15 calculated by BRDF kernel code *brdf_forward* (the code website given in acknowledgments). In this test, we added one standard deviation on the BRF ratios (e.g., $R_{0.466/2.12} \pm 0.12$,$R_{0.644/2.12} \pm 0.1$). We found that the ratios uncertainty can cause > 0.054 ($> 22\%$) errors in the AOD retrieval under $\tau \leq 0.25$. Nevertheless, the errors become small as increasing aerosol loading. For example, when $\tau = 0.5$, the error of the retrieval is 0.025 (5%).
- It is true that almost all MODIS AOD are within EE. We can see the 1-σ interval gets smaller in the new algorithm than C6_DT. This would help to improve the MODIS AOD accuracy, where EE would be stricter. In addition, the angular bias in BRF_DT has been mostly corrected by the new algorithm. In summary, the new algorithm has better retrievals over BRF_DT and C6_DT.

4. As mentioned in my prereview, the manuscript seems to be written in a hurry. It would benefit from a careful reading of the text to better explain what is discussed and improve the readability. Although part of my initial suggestions have been followed, similar improvements could be made throughout the MS. Also I suggest proofreading by a native English speaker.

Reply: Thanks for the reviewer's suggestion.

5. As a final comment, I wonder why nobody from the MODIS team is included as a co-author. Clearly the work would have benefitted from a cooperation and ensure that the results, if appropriate, would be included in the MODIS DT algorithm.

Reply: Thanks for the reviewer's comments and suggestions. This basically agrees with the points by reviewer # 1. We recall our reply to reviewer # 1 here:

We have specified the accuracy of our code with a complete comparison to the MODIS algorithm, and we are confident that it gives exactly the same result. However, it was completely recoded, in another programming language, so we can use it to add the changes to it and do experiments. In this way, it will also be clear that co-authorship of the MODIS DT team is not necessary but we still acknowledge the help.

Specific comments

Detailed comments (page, line nr)

1. 1, 6 directional-directional > bidirectional

Reply: Corrected.

2. 1, 9 data from the AERONET ...

Reply: Corrected.

3. 1, 14 expected accuracy level > expected error envelope

Reply: Done.

4. 1, 16 please refer to text books for the correct definition of aerosol, e.g. Seinfeld and Pandis, 1998, p. 97

Reply: Thanks for the reviewer's suggestion. We have removed the incorrect definition of aerosol.

5. 2, 4-6 AOD is the columnintegrated extinction, and thus does not describe the properties of individual particles such as absorption and scattering

Reply: Corrected.

6. 2,6 what is inverted? The model? Or is the model used to invert aerosol properties from the measured signal?

Reply: Corrected as follows:

Many AOD products have been produced or retrieved by interpreting and inverting the radiative transfer process with the input of satellite measurements.

7. 2, 10-11 awkward formulation of relatively high and relatively low: suggest to remove these terms

Reply: Done.

8. 2, 12 reference to an article in 2008 seems inappropriate: much has been improved in these 8 years!

Reply: We note that the MODIS algorithm has been improved from Collection 5.1 to 6, including dark target and deep blue algorithm. However, the uncertainty level over land still limits within $\pm(0.05 + 15\%)$, resulting large uncertainty (±0.03) in the global mean AOD (Levy et al., 2013) that can not meet the need of the precision ($0.01 \sim 0.02$) in the estimate of direct aerosol forcing (McComiskey et al., 2008). Therefore, we state that reference to an article in 2008 is still available to evaluate the current product.

9. 2,15 "For this reason": I don't understand, from the way you describe this, why two different algorithms were developed (actually there are more than only DT and DB)

Reply: We have removed "For this reason" in revised manuscript. Since DT and DB are the MODIS operational algorithms, they are more representative for the algorithm development than other versions. Thus, we focus on these two versions for the discussion.

10. 2, 19 although they aim at > for

Reply: Corrected.

11. 2, 21 to combine the effect: is that true? Please reformulate

Reply: We have revised the corresponding text as follows.

MODIS DT land AOD algorithm makes use of the presence of a dark surface in two visible channels 0.47 and 0.66 μm and the approximate transparency of the atmosphere at a relatively long wavelength 2.12 μm, **to obtain an accurate estimation of the atmosphere scattering**.

12. 2, 25 limited MODIS observations: this suggests that based on the current work more observations can be used to increase the DOF and thus better invert the aerosol properties, but in this MS this is not demonstrated

Reply: It is theoretically true that the increase of the data of frequency can better invert the aerosol properties by accumulating the measurements over several days. However, this will bring another problem that the algorithm would be less stable and more complex due to the cloud occurrence and rapid change of the surface during this period. Therefore, we did not demonstrate this in the manuscript.

13. 2, 30-35 I believe that in the current versions of DB more sophisticated surface correction techniques are used than those based on the minimum reflectance techniques: why are these discussed if obsolete and no longer used?. Especially since in the current study only dark targets are considered.

Reply: Thanks for the reviewer's suggestion. We have removed the corresponding text.

14. 2, 35 which relationship?

Reply: We have made it clear as follows:

Utilizing the observed spectral AOD and water vapor from ground AERONET sites, Levy et al. (2007) explicitly performed the atmospheric correction on MODIS measurements to derive **the spectral relationship** of the surface reflectance between visible wavelengths 0.466 and 0.644 μm versus short wave Infrared 2.12 μm (VISvsSWIR). The derived relationship is used to constrain the surface reflectance in the AOD retrieval, where the factor of vegetation amount and the scattering angle is also considered in the relationship (Levy et al., 2007, 2013).

15. 3,21-22 Thus .. surface: please clarify what you mean and reformulate.

Reply: We have revised the corresponding text as follows:

High quality AOD retrieval requires accurately estimating the contribution of anisotropic or non-Lambertian surface from the TOA radiance. The TOA radiance by coupling with surface BRDF effects is originally proposed by Li et al. (1996), and further improved and validated by Qin et al. (2001) (see equation 1). It has a high accuracy (0.7% on average) over several surface cover types (Qin et al., 2001), and has been applied to BRF_DT AOD algorithm (Wu et al., 2016). Here, we briefly introduce the TOA radiance by considering non-Lambertian surface, and the retrieval algorithm.

16. 3-28 The para starting here needs to be carefully rewritten. For instance, the sentence: 'scattered or reflected back into space and forward transmitted into the atmosphere' contains so many inconsistencies that it does not make any sense. Likewise 'partly scattered and partly attenuated' suggest that scattering and attenuation of the light beam are independent processes; etc. Note that these are examples, there is more wrong with this para.

Reply: We agree with the reviewer's comments. We have revised the paragraph as follows:

A sketch diagram is shown for the TOA radiance received by the satellite sensor. In this figure, we have a parallel solar beam F_0 with a zenith angle θ_s as the incident radiation into the atmosphere. This radiation is scattered and absorbed by the atmosphere when reaching to the bottom boundary of the atmosphere. Part of the scattered radiation can get into the view of the sensor, called "path reflected radiance" ρ^a. Some other parts of the scattered radiation keep forward propagation, called downward radiance including hemispherical and directional transmitted radiance $t_{dd}(i)$ and $t_{dh}(i)$, and interact with the underlying surface. Regarding the underlying surface as a non-lambertian, the anisotropic reflection of the surface is simply described as 4 elements: hemispherical-directional ① (R_{hd}), bihemispherical ② (R_{hh}), bidirectional ③ (R_{dd}) and directional-hemispherical reflection/reflectance ④ (R_{dh}), respectively, see Figure 2. The downward radiation undergoes complicated reflections between the surface and the atmosphere.

17. 4, eq 1 is given by WU et al. 2016 but we don't know whether this MS will actually be published.

Reply: We have added some additional references (e.g., Li et al., 1996; Qin et al., 2001) for eq 1 (also see reply 15).

18. 4, 28 is the BRF actually retrieved or provided from a parameterization?

Reply: BRF is retrieved with the spectral BRF ratio of VIS/SWIR.

19. 5, 6 what is 'the observed one'? see also my comment above: I believe that these are MODIS TOA reflectances?

Reply: We have replaced it with "MODIS measurements".

20. 5, 15-21 Fig 3 shows the clearly nonlinear relation between BRF in the VIS and that in SWIR: clearly there are different relations for low and high BRF(SWIR) and hence a linear fit is not justified. What do the authors want to show with Fig 3? I see no explanation of any of this. The large scatter in the data points at low BRF(SWIR) shows the large uncertainty in a VIS/SWIR ratio. Further use of this data set after selection for dark surfaces reduces the correlations and leads the authors to conclude that surface brightness has a strong impact on the spectral surface BRF relationship. I see no illustration of that, nor a justification for this conclusion.

Reply: Thanks for the reviewer pointing out the confusing place in the manuscript. Combining the next comment by the reviewer, we have replaced the full dataset with the one filtered with the dark surface (shown in Figure 3). We have also removed the conclusion in revised manuscript that surface brightness has a strong impact on the spectral surface BRF relationship. The corresponding text has been revised as follows:

To ensure pixels over a dark surface, the BRF dataset were filtered with $R_{dd,0.466} < 0.06$, $R_{dd,0.644} < 0.15$ and $R_{dd,2.12} < 0.25$. The data after the filtration are expected to match the dark surface in the DT algorithm in which the measurements were selected after masking the pixels of cloudy, water and snow/ice over the 20×20 pixels box (500 m resolution) with the measured 2.12 μm reflectance ranging from 0.01 and 0.25, and removing brightest 50% and darkest 20% of the measured 0.66 μm reflectance (Remer et al., 2005; Levy et al., 2007, 2013).

21. 5, 25-29 I do not understand the motivation given in lines 21-25 for the choice to investigate the relationships for dark surface only: I suggest that the authors state upfront that they are interested only in DT improvement and then delete these sentences here. In that case they don't have to repeat the same information in the beginning and at the end of this para (lines 17-29) and can start from the onset with dark surface only. Show Fig 3 for dark surface only and no speculation on whether it is surface brightness that causes the scattering in figure 3, or other factors as sated on line 28-29.

Reply: Thanks for the reviewer's suggestion, we have added the description of dark surface before analysis (see reply 20).

22. 5, 30 here start discussions on the VISvsSWIR surface BRF: what is meant with that? A relation as shown in Figure 3? What from such relation, which parameters, is discussed in terms of other parameters, especially considering the fact that we have not seen VIS vs SWIR for dark surfaces? Fig 4 shows VIS and SWIR versus scattering angle, or the ratio VIS/SWIR, all of which are not a relation VISvsSWIR. Please find some more accurate terms to describe what you are actually doing. For instance, 6,3 write "the dependence": dependence of what on what? Please find a suitable term for VISvsSWIR and use that throughout the MS

Reply: Thanks for the reviewer pointing out the confusing place in manuscript. We have added descriptions about dark surface, where "the relationship" is corrected as "the ratio" for VIS/SWIR. The descriptions and plots of Figure 3 and 4 in has been modified in revised manuscript. The modified sentence started at "the dependence" is given as follows:

It was shown that BRF ratios of VIS/SWIR can present an angular dependence over vegetated area (e.g., Gatebe et al., 2001). This is because vegetations (e.g., plant canopies) are not randomly oriented (Rondeaux and Vanderbilt, 1993), which could lead to the difference of the ratios with different angles.

23. 6, 2 clear sky is not the same as AOD<0.2, I cannot check the statement given in a paper that is not available and last but not least, isn't this paper on the differences between Lambertian and non-Lambertian?

Reply: We agree with the reviewer that we should emphasize the difference between Lambertian and non-Lambertian. The corresponding text has been removed in revised manuscript.

24. 6, 7 which angle?

Reply: We have removed the sentence with the angle (indicated the scattering angle).

25. 6, 11 is this a definition or a derivation? Please give references for this equation and / or definition

Reply: We have added references (e.g., Levy et al., 2007; Hsu et al., 2013) for this equation.

26. 6, 13 I see VIS, SWIR and VIS/SWIR vs angle in Fig 4, but no relationships vs angle! Furthermore the caption of Figure 4 is not consistent with what is presented.

Reply: We agree with the reviewer. We have changed the caption of Figure 4 as follows:

The dependence of the spectral BRF on the scattering angle.

27. 6,18-22 please reformulate. I guess that you mean that at 0.466 micrometer the absorption is larger at small scattering angles than at large scattering angles? And vice versa at the 2 longer wavelengths, although I don't see from the figure that the absorption at these 2 wavelengths is similar (as you state): the SWIR BRF is clearly higher, but may be for other reasons than absorption? And thus 0.466 / SWIR has a stronger dependence on scattering angle the other ratio?

Reply: We realized that the discrepancy of the absorption at different wavelength is hard to explain for the result in Figure 4. We have revised the corresponding text as follows:

Figure 4 A - C present the dependence of the surface BRF on the scattering angle at three wavelengths (0.466, 0.644 and 2.12 μm), respectively. With the increase of the scattering angle, the BRF of 0.644 and 2.12 μm present a fairly flat trend with their low correlation coefficients R (< 0.45). While this is not the case for the 0.466 μm BRF data, which showing a significant increased-trend (R = 0.75) with the scattering angle. This suggests that the angular shape of the surface BRF tends to be more significant at shorter wavelength. This is comparable with the finding in Wang et al. (2010).

Figure 4 D and E show the dependence of BRF ratios on the scattering angle for 0.466/2.12 and 0.644/2.12, respectively. We note that the ratio of 0.644/2.12 is nearly nonsensitive to the scattering angle with a small slope (0.00027) of the regression. This is mainly due to their similar (nearly flat) trend with the scattering angle. To account for the non-linearity of the 0.466/2.12 ratio with the scattering angle, a second-order polynomial fit was applied. In addition, data with last bin (large scattering angle) were neglected in the fitting process since large errors of the BRF present in this case as mentioned above.

28. 6,24 NO, this was not discussed, actually you concluded on 6.29 that there may be more reasons. But good to look at surface type anyway. However, this really needs to be described better! Why are you using here 1.24 and 2.12 micrometer? How is NDVI evaluated? Who measured the reflectances at these wavelengths? Did you use the same dark surface data set (for different wavelengths)? What is shown in Figure 5? What is observed by MODIS? Why do you apply linear regression while clearly the dependence of the observations on scattering angle is not linear, esp. for the blue data points which are independent of angle for angles smaller than 130. Hence the regression line is not correct and using that in any application leads to incorrect results. What do you mean with the statements on 6,32-33: nonlinear transformation (isn't NDVI the ratio of sums and differences?), neutralizes the dependence but does not change due to their dependence? Since I see no illustration I have no clue what you mean.
And that goes on on p.7:

Reply: Thanks for the reviewer's comments. We have checked the variability of the BRF ratios with surface types and added descriptions for that in revised manuscript. The definition of NDVI is added in revised manuscript where MODIS 1.24 and 2.12 μm observation used that were filtered with the same dark surface dataset (for different wavelengths). To clearly see the directional effect of $NDVI_{SWIR}$, a plot of $NDVI$ as function of the scattering angle is given in Figure 5B. The nonlinearity and uncertainty is more clear in this figure.

The modified text is given as follows:

To check the variability of the BRF ratios with surface cover type or seasons, the MODIS Land Cover Type/Dynamics products (MCD12C data during the year of 2006) were used for this analysis. The BRF data with urban and non-urban types were regrouped and further separated from Summer and Winter (Summer: June-July-August, Winter: December-January-February). For urban sites, the ratios of VIS/SWIR (0.466/2.12: ~ 0.3, 0.644/2.12: > 0.6) are generally higher than non-urban sites (0.466/2.12: 0.23 - 0.28, 0.644/2.12: 0.58 - 0.59), but present less seasonal variability.

The variability of the BRF ratios due to the change of surface properties might become small using vegetation index $NDVI_{SWIR}$ which is defined as (Levy et al., 2007; Hsu et al., 2013):

$$NDVI_{SWIR} = (\rho_{1.24}^{obs} - \rho_{2.12}^{obs})/(\rho_{1.24}^{obs} + \rho_{2.12}^{obs}) \quad (1)$$

Where $\rho_{1.24}^{obs}$ and $\rho_{2.12}^{obs}$ indicate MODIS observed reflectance at 1.24 and 2.12 μm, respectively. However, we found that the BRF ratios were hardly separated by this index. This is mainly due to the disturbance of the directional effect in $NDVI_{SWIR}$.

Figure 5 shows the dependence of the MODIS observations $\rho_{1.24}^{obs}$ and $\rho_{2.12}^{obs}$ and $NDVI_{SWIR}$ on the scattering angle. These observations were also filtered with the same dark surface dataset. The medians of $\rho_{1.24}^{m}$ (R=0.89) are much more dependent on the scattering angle than $\rho_{2.12}^{m}$ (R=0.47). As result, we can see that the medians of $NDVI_{SWIR}$ (R=0.874) also gives a significant increased-trend with the increase of the scattering angle. This suggests that the $NDVI_{SWIR}$ can differ by 0.2 with different scattering angles. The difference of 0.2 in $NDVI_{SWIR}$ would lead to the bias by > 0.012 ($\geq 5\%$) in the AOD retrieval when $\tau \leq 0.25$ using C6_DT relationship for example.

29. 7.15 do you take median values or use the regression? Since you have not given the eq for NDVI, can you show this calculation and how NDVI depends on scattering angle? And illustrate what the effect is on DT AOD retrieval? Now it is all speculation

Reply: Yes, we take median values for the regression. The calculation and how $NDVI_{SWIR}$ depends on scattering angle is given in reply 28, as well as the effect on the DT AOD retrieval (last paragraph in reply 28).

30. 7.6-10 this para is very vague and I don't understand the effect of NDVI. Following the linear relationships in Fig 5 should not be advised by lack of linearity and large uncertainty, as said above. Further, do you mean here to remove NDVI form C6DT or the new DT2? Please clarify.

Reply: We removed the NDVI from the new DT2. The revised text is shown in section 4.4 (revised manuscript), as follows:

To clarify the effect of $NDVI_{SWIR}$ omission in the AOD retrieval, we compared the uncertainty level of the retrievals between BRF_DT and BRF_DT2 over the global land area. Meanwhile, to avoid the angular effects in the results, the retrievals were sorted in bins of the scattering angle. It was found that these algorithms give a similar ($< 1\%$) uncertainty level in the retrieval (more details see section 5.2.2). This demonstrates that the removal of $NDVI_{SWIR}$ would not cause too much error in the new algorithm retrieval.

31. 7.13 just above you said the surface type, parameterized through NDVI, can be omitted form the equation, which contradicts the statement of a strong dependence on surface type What is it?

Reply: We have modified the corresponding text as follows:

Results of three years ASRVN BRF dataset over around 100 AERONET sites show that the BRF ratio of VIS/SWIR has a dependence on the scattering angle and surface type. Due to the disturbance of the directionality by 1.24 and 2.12 μm observations, the $NDVI_{SWIR}$ cannot be well used to refine the BRF ratio. Thus, $NDVI_{SWIR}$ will not be taken into account in the BRF parameterization.

32. 7.15-25 I am confused! what do you mean with the overestimation: it is what you have shown in Fig 4d, or is something wrong with that? If so, what? Can we trust the observations or not? Then, you chose a scattering angle smaller or larger than 115 deg: WHY 115? What is this choice based on? I don't see it in any figure. Motivate! Then we see eq 4, which is Figure 4e: hardly any dependence on scattering angle. But then: eq 5! Where for teta<115 where you take the fit to the whole scattering angle range ???? And teta > 115 ?? where does that come from? Can you explain that, where is it illustrated? And then another surprise on line 24: I can imagine that vegetation causes an angular dependence, and black soil and dead vegetation less, but have such data sets been separated that you can make this conclusion? It seems to me that you did not do this and thus the relation applies to an average over ALL dark surfaces as defined by your selection criteria!

Reply: We admit that the divided linear regression at 115° is reluctant for the BRF ratio. We recalculate the ratio using a second-order polynomial fit method over all the scattering angle (see Figure 4 in revised manuscript). The weak dependence on scattering angle for ratio 0.644/2.12 is due to the weak BRF feature at long wavelength (e.g., 0.644 and 2.12 μm).

We did not separated the dark surface dataset as black soil or vegetations. The inappropriate descriptions has been removed in revised manuscript.

33. 7, 27 You have derived your relations for low aerosol loading (5, 10 low aerosol loading: AOD<0.2), so would you expect that they apply over areas with high aerosol loading which do not satisfy your initial assumptions? In particular your China case? This puts in doubt all your comparisons for high AOD!

Reply: It is true that the new relations would not take effect on areas with high aerosol loading since they are only for the BRF (directional radiation) other than diffused radiation (e.g., R_{hd} or R_{hh}). We have removed the descriptions about China case in revised manuscript.

34. 8, 13 Omission of NDVI does not affect the retrieval: what is this conclusion based on? It is only omitted in DT2, can there be other factors?

Reply: We have added a new description about this. Since we recalculated the BRF ratio by using a second-order polynomial fit, the results lightly differ from the previous one. In addition, we rescaled AOD color bar (from -0.06 to 0.06) in Figure 6 as suggested in **comment 36**. The new descriptions is given as follows:

In Figure 6 D (areas south of 45°S), the difference of the AOD retrievals of both BRF_DT2 - C6_DT and BRF_DT2 - BRF_DT do not vary with $NDVI_{SWIR}$. This demonstrates again that the omission of $NDVI_{SWIR}$ in the parameterization does not significantly affect the retrieval.

35. 8, 17: BRF algorithms are spatially smoother than C6: why does that demonstrate that the BRF retrieval are less affected by the underlying surface? NDVI is also smooth!

Reply: Thanks for the reviewer pointing out the confusing place in the manuscript. The difference between BRF and C6_DT algorithms is caused by whether the surface anisotropic reflection is considered, other than NDVI since it presents spatially smooth.

The revised text is given as follows:

In the "yellow ellipse" area shown in Figure 6, we can see that BRF_DT and BRF_DT2 AOD are spatially smoother than C6_DT. This to some degree demonstrates that BRF_DT and BRF_DT2 AOD are much less affected by the anisotropic reflection of the underlying surface.

36. 8, 20 With this colour scale for the difference plots, differences of 0.08 and 0.8 are not visible, and they are very small anyway; so how can you claim that there is a strong dependence on scattering angle. May be illustrate in a different way??

Reply: Thanks for the reviewer's suggestion. We have rescaled the color bar to make it clear. The revised text is shown as follows:

For the whole land area shown in Figure 6, the difference of BRF_DT2 - BRF_DT AOD presents a significant dependence on the scattering angle, where a positive difference of 50% (0.06) was found at a small scattering angle ($\Theta \leq 130°$), and a equal negative difference at a large scattering angle ($\Theta \geq 150°$), while less dependence was found for the difference of BRF_DT2 - C6_DT AOD.

37. 8, 30 This para discusses cases with high aerosol loading, i.e. beyond the validity and thus applicability of your relations! Hence the conclusions are not valid and should be removed. Apart from that, the differences in the various lines are not always visible.

Reply: Thanks for the reviewer's suggestion. We have removed the related parts.

38. 9, 1-8: OK, this is valid, but this significant difference does not always occur, as you indicate in the next line. However, I am not sure I understand your explanation: for BRFDT2 you account for directional effects, whereas BRFDT does not. Hence, over vegetation a directional effects is expected (6, 37) more than over nonvegetated surfaces. Yet, BRFDT and DT2 give similar retrieval over vegetated and dissimilar over nonvegetated. Have I misunderstood something? Can you explain?

Reply: Thanks for the reviewer pointing out the confusing place in manuscript. It is true that BRF_DT and DT2 give similar retrieval over vegetated and dissimilar over non-vegetated. This is probably because $NDVI_{SWIR}$ is saturated over the area with abundant vegetation, thus showing less directionality than less vegetated area (e.g., sparse vegetation and bare soil).

39. 9.15 Negative AOD results occur because of the expected error which can be negative, and hence occur for small AOD and not for larger AOD.

Reply: We agree that negative AOD results occur because of the expected error which can be negative, and hence occur for small AOD and not for larger AOD. We have removed the corresponding text in revised manuscript.

40. 10, 4 Too much tolerance? What do you mean? This is based on the retrieval technique, and is a theoretical EE.

Reply: We realized that "Too much tolerance" are inappropriate descriptions for EE. We have removed the corresponding text in revised manuscript.

41. 10, 10 Why 8 over China and 20 in the other areas? As mentioned above, the assumptions made here do not apply over China and results are not meaningful and therefore should not be presented.

Reply: Thanks for the reviewer's suggestion. We have removed the related parts.

42. 12, 27 I do not understand why an effective vegetation index is lacking, it is omitted in DT2 as you explained above. And I do not understand the comment referring to DB while this MS deals with DT Figure 12: at least 3 typos and I wonder why the EE varies with scattering angle?

Reply: Thanks for the reviewer's suggestion. We have removed this paragraph in revised manuscript.

We look forward to hearing from you regarding our new submission. We would be glad to respond to any further questions and comments that you may have. Thank you all very much.

References

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