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 BRF-DT2 and results from this algorithm are compared with that using an earlier version (BRF-DT, see below) and from the MODIS C6 DT product.

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 BRF-DT and BRF-DT2, 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).

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 ASRVN-derived 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?

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 non-linearity 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.

As mentioned in my pre-review, 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.

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.

Detailed comments (page, line nr)

1,6 directional-directional > bidirectional

1, 9 data from the AERONET …

1,14 expected accuracy level > expected error envelope

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

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

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

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

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

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)

2, 19 although they aim at > for

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

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

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.

2, 35 which relationship?

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

3-28 The para starting here needs to be carefully re-written. 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.

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

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

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

5, 15-21 Fig 3 shows the clearly non-linear 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.

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.

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

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?

6,7 which angle?

6.11 is this a definition or a derivation? Please give references for this equation and / or defintion

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.

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?

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:

7.1-5 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

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 C6-DT or the new DT2? Please clarify.

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?

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!

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!

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?

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!

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??

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.

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 BRF-DT2 you account for directional effects, whereas BRF-DT does not. Hence, over vegetation a directional effects is expected (6, 3-7) more than

over non-vegetated surfaces. Yet, BRF-DT and DT2 give similar retrieval over vegetated and dissimilar over non-vegetated. Have I misunderstood something? Can you explain?

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.

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

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

1227 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?