Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2016-133-RC3, 2016

Atmospheric Measurement Techniques
Discussions

Interactive comment on "Accounting for the effects of surface BRDF on satellite cloud and trace-gas retrievals: A new approach based on geometry-dependent Lambertian-equivalent reflectivity applied to OMI algorithms" *by* A. Vasilkov et al.

Anonymous Referee #3

Received and published: 13 June 2016

In their manuscript "Accounting for the effects of surface BRDF on satellite cloud and trace-gas retrievals: A new approach based on geometry-dependent Lambertian-equivalent reflectivity applied to OMI algorithms", A. Vasilkov et al. report on an approach to include surface BRDF effects in OMI NO2 and cloud retrievals. The algorithm is based on the use of MODIS BRDF values for land and a simplified model of surface reflection for the ocean which are then used as geometry dependent LER input parameter for existing lookup-tables of air mass factors and intensities. The algorithm

is applied to real OMI data and the results are compared to those obtained with the standard OMI LER climatology.

The topic of the manuscript is interesting and relevant for UV/vis satellite retrievals of atmospheric parameters where BRDF effects are currently mostly ignored. The approach suggested by the authors is attractive as it would require only small changes to current retrieval schemes and would not much increase computational requirements. The paper is to my knowledge also the first to investigate the effect of BRDF on OMI cloud parameters. The manuscript is well written, clearly structured and contains adequate illustration of the results in figures.

Unfortunately, there are several important shortcomings in the study as outlined below, and I can therefore not recommend the current version of the manuscript for publication in AMT. In my opinion, major revisions are needed before it can be considered again for publication.

Major comments:

1. The most important problem with the manuscript is that the "new approach based on geometry-dependent Lambertian-equivalent reflectivity" is – at least as far as I understand – not new but identical to the approach already evaluated by Zhou et al, 2010 and Noguchi et al., 2014 who named it "BRF-approach". Both studies show that this approach is not properly accounting for BRDF effects, which is not surprising as it replaces the direct surface reflectance term with the appropriate value but leads to a wrong source function for the diffuse radiation field. It therefore has a tendency to overestimate BRDF effects.

   In their manuscript, the authors need to discuss previous evaluations of this approach and compare the results of their approximation with those from calculations using the full BRDF treatment. Without such a comparison, it is not clear what the uncertainty of their approximation is.

2. The second problem of the manuscript is that comparisons are made to calculations using OMI LER which is based on a different approach applied to a different data set than the MODIS surface product used in their new algorithm. Therefore, no clear separation of BRDF effects and the effects of other differences between the two products can be made which is an important limitation of the study.

   In my opinion, the authors need to add a comparison to a data product using MODIS surface reflectance but without accounting for BRDF effects in order to be able to quantify BRDF effects. The current comparison is also interesting for users as it indicates how large changes in the OMI products would be, but this is a different question.

3. The role of aerosols is only touched upon in the manuscript, but could be quite important in different parts of the algorithm: in the determination of BRDF parameters in the MODIS product, in the effect of aerosols on cloud parameters when using the new BRDF and in the importance of BRDF on the results. As aerosols increase scattering they will reduce the importance of BRDF effects (see for example the discussion in Noguchi et al., 2014). In the way the algorithm is set up currently (Rayleigh atmosphere), BRDF effects will be overestimated leading to errors in the cloud parameters and air mass factors.

   The effect of aerosols in the different parts of the algorithm has to be discussed and if possible, the uncertainty introduced by overestimation of BRDF effects be quantified.

4. The current manuscript mainly discusses measurements from one single OMI orbit from November 2006 and is therefore based on a very limited data set. Additional data points are shown in Fig. 13 but it is not clear to me from which orbits they are taken. I'm convinced that the effect of BRDF varies with region, season, and viewing geometry, and this needs to be evaluated if one aims at giving meaningful numbers for the uncertainty introduced by ignoring BRDF effects. Also, the approximation made when using geometry dependent LER may introduce different uncertainties depending on geometry and surface type.

   In my opinion, significantly more different situations need to be evaluated in more detail to make the numbers derived for the BRDF effects on OMI products meaningful.

Minor comments

- The authors use their own O2-O2 cloud algorithm, presumably because this gives them full control of the settings. They state that very good correlation is found for ECF > 0.2 but this of course is not the range of ECFs later discussed. In that sense the difference to current OMI products may be also influenced by the differences between the two implementations of the O2-O2 algorithm.
- Neglecting oceanic foam may be necessary but will lead to an overestimation of BRDF effects over oceans.
- The authors use a vector RTM. It is however not clear to me from the manuscript how polarisation is treated at the surface – can you please provide some details here.
- When introducing BRDF in the cloud product, wouldn't it make sense to also include an approximate treatment of angular dependencies of the reflection from clouds?
- It might be trivial but can BRDF parameters safely be averaged over all MODIS pixels within one OMI scene? Is this a linear problem?
- Is equation 9 used for the figures? If so, isn't that creating a bias in the analysis?
- Which data is shown in Figure 13?

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-133, 2016.