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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 #2

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Surface reflectance is an important parameter in satellite trace gas retrievals in the UV/VIS range. This study proposes an approach that is able to account for surface BRDF effects while requiring only minimal changes to existing retrieval algorithms.

This is a very welcome contribution to this topic. The publication is generally well written and clear and the methods are sound. Particularly novel and valuable aspects of the manuscript are an approach for considering ocean BRDF in cloud and trace gas retrievals and a comprehensive analysis of BRDF effects on cloud parameters.

Despite these positive aspects, the study has serious deficiencies as detailed below that need to be addressed before it can be considered for publication. This will require a major revision.

Major issues:

There have been several previous studies (e.g. Russel et al., 2011; McLinden et al., 2014; Kuhlmann et al., 2015) comparing NO2 columns retrieved with MODIS-based surface reflectance/albedo with NO2 columns retrieved using climatological Lambertian Equivalent Reflectances (LER) such as the Kleipool et al. (2008) data set.

As properly referenced, there have also been several previous studies on the effect of surface reflectance anisotropy (BRDF effects) on NO2 retrievals (Zhou et al., 2010; Noguchi et al., 2014; Lin et al., 2014, 2015).

The novelty of this study as compared to these earlier ones is the treatment of the surface reflectance as a geometry-dependent LER to account for BRDF effects. To be a valuable contribution to the existing body of literature, the publication needs to demonstrate the advantages and limitations of this approach, but it falls short in doing so for two reasons:

1) Instead of analyzing the effects of geometry-dependent versus geometry-independent LER it only compares results based on MODIS reflectance products with results based on OMI-LER from Kleipool et al. (2008). These results are only little influenced by the geometry-dependence of surface reflectance but are dominated by the large differences between MODIS and OMI-based reflectance data sets, an aspect that has been addressed at length in previous studies. For the same reason, the conclusion in the abstract (and conclusions section) that geometry-dependent LERs can change NO2 vertical columns by up to 50% is very misleading as it gives the wrong impression that this is a direct consequence of the geometry-dependence.

In contrast to the present study, Zhou et al. (2010) differentiated between the ef-

fects of switching from an OMI climatological LER to a MODIS reflectance product from the effects of considering BRDF effects versus not considering such effects. They concluded that considering geometry-dependent versus geometry-independent reflectance changes NO2 by mostly well below 20% and that these changes are smaller than those induced by switching from the OMI-based LER of Kleipool et al. (2008) to MODIS.

2) The proposed approach of using a geometry-dependent LER instead of a full BRDF treatment is very interesting since it has the potential to simplify the retrieval (e.g. existing look-up-table based retrievals could still be used) while accounting for the influence of surface anisotropic reflectance at least to first order.

However, this is still a simplification with respect to a full BRDF treatment since only the TOA radiance is reproduced but the paths of photons reaching the TOA are not exactly the same as in the case of a full BRDF treatment with likely consequences on the vertical sensitivity profile (box AMF profile). The study fails to demonstrate the implications of such a simplification. The effects of such simplifications were addressed by Zhou et al. (2010) which compared a full BRDF treatment with a treatment taking either the MODIS albedo as LER or taking the BRF value for the given illumination and viewing geometry as LER. In both cases, differences from a full BRDF treatment were significant (see their Fig. 10). A similar analysis is needed for the approach proposed here in order to demonstrate both the advantages of a geometry-dependent LER as compared to a geometry-independent LER (Fig. 1 provides some hints) and the limitations with respect to a full BRDF treatment.

Minor points:

Page 2, Line 24: I suggest to include the MERIS based albedo data set of Popp et al. (2011) which is used in the latest FRESCO cloud algorithm and will be considered also for future TROPOMI products.

P5, L152-155: Please explain why O3 and NO2 slant columns are taken from indepen-

dent OMI algorithms.

P6, L159: It would probably be useful to make clear that you are referring to vertical column densities of O2-O2.

P7, L211: “over over” -> “over”

P7, Equation (6): This equation appears incomplete as the rightmost term only multiplies two unit-less numbers (reflectance times transmittance) but does not represent a radiance.

P8ff: The manuscript structure would probably become clearer by introducing a new section “4 Results and Discussion” and making the present sections 4-6 subsections of this.

P8, L244-245: This is only true over land, not over the ocean.

P8, L254ff and Figure 2: The MODIS-based BRDF reflectance patterns over the ocean need to be better explained. There are two areas of high reflectance, one in the upper right hand part of the figure and another one off the west coast of South America. Given the overpass time of OMI around 1 PM, I assume that only the latter is due to specular reflectance around the glint spot. The high values near the eastern boarder of the swath must be due to the Morel parameterization of diffuse light which depends on chlorophyll content. I am surprised that these values are in a similar range as those near the glint spot and that the pattern doesn’t resemble the distribution of chlorophyll in the Atlantic.

P8, L259: “for same” -> “for the same”

Section 5: The discussion of the effects on cloud parameters is very short, especially for the O2-O2 algorithm. How do the results compare with the findings of Lin et al. 2015?

Figure 5: How do you explain the fact that the difference in ECF does not converge to

zero at high ECF?

Figures 5 and 8: The x- and y-axis scales in the left-hand figures should be identical and the 1:1 line should be displayed as reference.

P10, L319: “for unpolluted NO2 mixing ratios” -> “for unpolluted NO2 mixing ratio profiles” (since the profile shape matters, not the absolute NO2 values).

P10, L320: I don’t agree with this statement. What is shown here is only to a minor extent a “BRDF effect” (see my major concerns above).

P11, L331: Same issue: It is not correct to state that “BRDF reduces ..”.

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