Response to reviewer #2

We thank the reviewer for his/her evaluation of our paper and useful comments that helped improve the manuscript. We appreciate reviewer's time and effort in reviewing the manuscript. Below are our responses to each comment. All reviewer's comments are in the standard font while the responses are in the italic font.

On behalf of the authors,

Alexander Vasilkov

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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.

*We added a reference to the paper by Kuhlmann et al. in the introduction. Other references were already given. We also added the following in the introduction:*

*"Russel et al. (2011) studied the effect of using different surface albedo products on the NO2 columns and found that the impact of the surface albedo can be up to ±40% for land."*

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 effects 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.

*We do not think that our results including the statement of up to 50% change in NO2 vertical columns are misleading because we clearly state in the abstract and manuscript that the comparisons of retrievals with geometry-dependent LERs are carried out versus those with climatological LERs. Our goal is to document a new global product that will be publically available and could be easily used in the existing operational satellite trace-gas and cloud algorithms. The existing operational algorithms make use of climatological LER products. A question arises how big differences could occur if the climatological LER product would be replaced with the geometry-dependent LER product. We try to answer this practical question in the paper. That is why we are comparing the retrievals based on the geometry-dependent LER with the retrievals based on the geometry-independent climatological LER. The reviewer is right when saying that the differences may be dominated by the large differences between MODIS and OMI-based reflectance data sets. But our main goal is to provide practical results of the comparisons useful for decision-making of developers of the operational algorithms. We think that theoretical results of considering BRDF effects versus not considering such effects have been sufficiently described by Zhou et al. (2010). We would like to note that Lin et al. (2014) used a similar approach in evaluating their NO2 retrievals with full BRDF treatment: they compared their new product with the DOMINO-2 product which makes use of climatological surface LER.*

*To clarify this issue we added the following text in the introduction:*

*"The main goal of this paper is to document a new global surface reflectivity product that will be publicly available and could be easily used within several existing operational satellite trace-gas and cloud algorithms. We implement the geometry-dependent LERs based on a MODIS BRDF product and use these LERs within OMI cloud and NO2 algorithms. Henceforth, when we refer to geometry-dependent LERs, this refers to a MODIS-based data set. We compare the cloud and NO2 retrievals based on the geometry-dependent LER with the retrievals based on the climatological LER derived from TOMS and OMI measurements. Henceforth, climatological LERs refer to products derived from OMI and TOMS. The differences between those retrievals include both BRDF effects and possible biases between the MODIS and other instrument (OMI and TOMS) reflectance data sets. The existing operational algorithms make use of climatological LER products. By comparing the products retrieved with the geometry-dependent LER with those retrieved with the climatological LER, we address a practical question of how large the differences in various satellite products would be if the climatological LERs are replaced with the geometry-dependent LERs."*

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.

*We agree. Indeed, the geometry-dependent LER approach provides an exact match of TOA radiances with the full BRDF approach but not the photon path lengths. This simplification can lead to some biases in the calculation of AMFs and thus to biases in the retrieved NO2 vertical columns. Zhou et al. (2010) have estimated the biases. They compared the box AMFs calculated with the full BRDF with that calculated with black-sky albedo and white-sky albedo. According to their Fig. 3 and corresponding text on page 1190, "The effect of surface treatments is most strongly felt near the surface, where the box AMFs differ by up to 10% in this example". A similar order of the difference is found in comparisons of the NO2 vertical columns in Fig. 10 where "Relative differences are smaller than 12% for most of the domain" (see page 1195 of the paper). We consider those differences to be notable but not significant. However, we carried out calculations of NO2 AMF with full BRDF treatment and compared it with that calculated with the corresponding geometry-dependent LER. Differences in AMFs due to different treatment of the surface appear to be small. We added a figure that shows the comparisons.*

*We added at the beginning of Section 6 the following:*

"*The geometry-dependent LER approach provides an exact match of TOA radiances with the full BRDF approach but not the photon path lengths. This simplification can lead to some biases in the calculation of AMFs and thus to biases in the retrieved NO2 vertical columns. Zhou et al. (2010) have estimated the biases. They compared the box AMFs and NO2 vertical columns calculated with the full BRDF with that calculated with black-sky albedo and white-sky albedo. According to their data, maximum differences in the box AMFs are up to 10% at the surface and differences in the NO2 vertical columns are smaller than 12%. We carried out calculations of NO2 scattering weights and AMFs with full BRDF treatment and compared them with that calculated with the corresponding geometry-dependent LER. Fig. 11a shows an example of the* *altitude dependence of scattering weights calculated with the full BRDF treatment and the geometry-dependent LER. It can be seen that the difference between the scattering weights is small. An AMF difference for this case is 5.6%. Fig. 11b shows a scatter plot of the full BRDF AMFs versus the geometry-dependent LER AMFs calculated for OMI measurements over the eastern US for orbit 12414 of 14 Nov. 2006. Differences in AMFs due to different treatment of the surface are within ± 6% (at 95% confidence interval) and always less than 10%"*.

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.

*Thanks. We added this reference.*

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

*Our approach is a particular implementation choice that differs from others published in the literature. We added "This is an implementation choice that is designed to minimize potential errors due to cross talk between* $O_3$ *,* $NO_2$ *and* $O_2$*-*$O_2$ *cross sections during the fitting procedure."*

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

*Done.*

P7, L211: "over over" -> "over"

*Corrected.*

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.

*Eq. 6 is correct , but the definition of T not straight-forward. We changed the definition of T to clarify that it is in units of radiance, not unitless. To clarify this we changed the definition of T to the following:*

*"T is the total (direct + diffuse) solar irradiance reaching the surface converted to the ideal Lambertian-reflected radiance by diving by pi and multiplied by the transmittance of the reflected radiation in the direction of a satellite instrument."*

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.

*We agree. However, to keep the traceability of manuscript changes for all reviewers we decided to preserve the original manuscript structure for now*.

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

*Correct, added "over land" to make this distinction.*

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.

*We agree that more discussion of the angular dependence of geometry-dependent LER over the ocean is needed. We added in Section 4 the following:*

*"The total ocean reflectance comprises of three components: direct and diffuse solar light reflected from the ocean surface and water-leaving light. A fraction of each component strongly depends on geometry. Reflection of direct solar light dominates in the sun glint area. At the edges of the swath the relative contribution of reflected diffuse light increases because the sky radiance increases to the horizons and the reflection angle increases thus the Fresnel reflection increases. The higher values of LER nearer to the eastern part of the swath than at the western part are mostly due to sky light reflected from the ocean surface. An angular distribution of the sky radiance is not symmetric in the plane of satellite observations because the sun is in the western part of the swath. The sky radiance is higher in the eastern part of the swath and it is reflected at higher angles than the light from the western part. Additionally, the higher reflection angle results in higher Fresnel reflection in the eastern part of the swath. This is confirmed by our calculations of the view angle dependence of the reflected light only, i.e. no water-leaving radiance included."*

P8, L259: "for same" -> "for the same"

*Done.*

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?

*We agree. We replaced the last sentence in Section 5.2 by the following paragraph:*

*"The effect of replacing the climatological surface LER by the geometry-dependent LER is much more pronounced for the O2-O2 OCP retrievals than for the RRS retrievals. This can be explained by two physical factors. Firstly, the Rayleigh optical depth of the atmosphere in the UV (the spectral window of the RRS cloud algorithm is 345 - 354 nm) is much higher than in the visible (the wavelength of the O2-O2 OCP retrieval is 477 nm). Higher scattering in the UV leads to a larger fraction of diffuse light illuminating the surface thus decreasing BRDF effects. In the visible, the smoothing effect of Rayleigh scattering is less than in the UV thus resulting in larger BRDF effects. Secondly, sensitivities of the OCP, derived from RRS and O2-O2, to surface reflectivity are different for the RRS and O2-O2 algorithms. The light path of direct sunlight reflected by the surface does not contribute to the RRS signal, because there is no Raman scattering involved. But this direct light path does contribute to O2-O2 absorption. That is why the RRS algorithm is generally less sensitive to the surface and to its BRDF for low cloud fractions. For high surface reflectivity, the reflected direct solar light significantly contributes to TOA radiance therefore causes the OCP differences related to the absence of RRS in direct solar light and the presence of O2-O2 absorption in direct solar light. However, for low surface reflectivity, this mechanism becomes less significant because a fraction of the reflected direct solar light in the TOA radiance is smaller."*

*We also added the following:*

*"Lin et al. (2014) compared ECFs and OCPs derived from O2-O2 absorption using the OMI operational algorithm and their own algorithm that makes use of SCDs from the operational algorithm and a set of ancillary parameters that include MODIS BRDF. Their scatter plots of the operational ECF and OCP retrievals versus the new retrievals with BRDF shown in their Fig. 2 are qualitatively similar to ours."*

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

*Data in Fig. 5 are for ECF less than 0.25 only (the most interesting range of ECF for trace-gas retrieval). The difference in ECF does converge to zero at high ECF. To clarify this, we added a scatter plot of ECF with climatological LER versus ECF with BRDF for the entire range of ECF. The scatter plot shows that the difference in ECF diminishes with increasing ECF. We also added to the text:*

*"Figure 5a is a scatter plot of ECF retrieved with the geometry-dependent ECF versus ECF retrieved with climatological LER for the entire range of ECF. It shows that the scatter of data around the 1:1 line diminishes with increasing ECF, i.e. the difference in ECFs decreases with increasing ECF as expected."*

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.

*Corrected. The 1:1 lines are added and mentioned in the captions.*

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

*Corrected.*

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).

*Agree. We clarified here:*

*“An effect of replacing the climatological LER with geometry-dependent LER …”*

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

*Agree. We changed this statement:*

*Replacing the climatological LER with geometry-dependent LER reduces the surface LER …”*