Response to reviewer #3

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

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.

Our approach is not identical to the approach already evaluated by Zhou et al, 2010 and Noguchi et al., 2014. Firstly, we have developed a product that could be easily used in the existing operational satellite trace-gas and cloud algorithms based on the MLER concept. The use of this product in the existing algorithms does not require extensive computational efforts. The "BRF approach" cannot be easily used in the existing operational satellite trace-gas and cloud algorithm. Moreover, it requires extensive radiative transfer (RT) computations that prevent from the use of a vector RT code which is necessary. Next, our product is global: it is applicable to the ocean unlike Zhou et al, 2010 and Noguchi et al., 2014.

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.

We carried out calculations of NO2 AMF with full BRDF treatment and compared it with that calculated with the corresponding geometry-dependent LER. We added a figure that shows the comparisons. We also 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%".

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.

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 we are aimed to obtain 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).

To clarify this issue we have made the following additions to 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."

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.

We think that this is a pure theoretical issue, which was sufficiently investigated by Zhou et al. (2010). They have estimated possible NO2 differences due to not accounting for full BRDF. 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 mentioned their study in Section 6 (see our answer to the previous comment). We would like to note that both black-sky albedo and white-sky albedo derived from MODIS do not adequately describe the real surface albedo that depends on an exact angular distribution of the sky radiance. That is why we consider such comparisons to be purely theoretical.

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.

The reviewer is absolutely right that the role of aerosols is quite important and the aerosols can reduce the BRDF effects by increasing the diffuse solar light at the surface. We think that their role was carefully studied in several papers, e.g. Lin et al. (2014 & 2015). That is why we intentionally limited our discussion of the aerosol effects. We briefly discussed the aerosol effects in Introduction. We also stated in Section 2.3.1 that our cloud algorithms implicitly accounts for non-absorbing aerosols, treating them as clouds and this increases effective cloud fraction. The aerosol effect is thought to be significant in trace gas algorithms because the aerosol affects

AMFs. We mentioned in Conclusions that we plan to explicitly include aerosols in the NO2 algorithm in the future work.

We added at the end of Section 3 the following:

"It should be noted that aerosols are not included in the computation of the geometry-dependent LER. Scattering by aerosols in the atmosphere reduces the BRDF effects (Noguchi et al., 2014). Therefore, the use of the geometry-dependent LER may result in overestimation of the BRDF effects. While non-absorbing aerosols are implicitly accounted for in the cloud algorithms (see Section 2.3.1), the aerosols directly affect the Air Mass Factor (AMF), thus trace gas retrievals."

and the following in Section 2.3.1:

"However, the increase of cloud fraction due to the presence of aerosols cannot correctly reproduce an increase of diffuse solar light at the surface caused by aerosol scattering. This may introduce some error in calculation of the clear-sky subpixel radiance because the BRDF effect depends on a ratio of diffuse to direct solar light."

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.

To present our results, we selected OMI orbit 12414 because it contains land and ocean areas in approximately equal proportions. Data in Fig. 13 are for orbits 12391 and 12414. We agree that more data from different conditions and seasons need to be evaluated to make the numbers more representative. To look at BRDF variations with region and viewing geometry we process OMI data for the entire day of Nov 14, 2006. To evaluate BRDF variations with season we processed OMI data for one more day in summer (July 14, 2006). We added a figure that shows the ECF and OCP retrievals from the O2-O2 cloud algorithm for those two days. We also added the following text in Section 5:

"To make the numbers characterizing the ECF and OCP differences be more representative, we processed OMI data for two days of November 14 and July 14, 2006. Figure 10 shows the ECF and OCP differences as a function of ECF for those two days. The ECF differences calculated for the entire day of Nov 14, 2006 (Fig.10c) are quite close to those calculated for a single orbit 12414 of that day (Fig.8b). The OCP differences over land calculated for the entire day

(Fig.10d) are slightly lower than those calculated for orbit 12414 of that day (Fig. 9b) while the OCP differences over ocean for the entire day are quite close to those calculated for one orbit. The ECF and OCP differences are similar for different seasons. A small increase of the OCP differences in November may not be statistically significant. The data in Fig. 10 indicate that the ECF and OCP differences obtained for OMI orbit 12414 are globally representative. "

We also calculated the tropospheric NO2 AMFs for two days: Nov 14 and Jul 14, 2006. A global analysis of the AMF differences due to replacing the climatological LER with the geometry-dependent LER shows that the AMF differences for OMI orbit 12414 are quite representative for both days. A figure below shows a global map of the trop AMF and the AMF differences for Nov 14, 2006. We decided not to include the figure in the manuscript but added at the end of Section 6 the following:

"To make the numbers characterizing the AMF differences be more representative, we calculated the tropospheric NO2 AMFs using the geometry-dependent LER and compared them with those calculated with the climatological LER for two days: November 14 and July 14, 2006. The AMF differences arising from both replacing the climatological LER with the geometry-dependent LER and changing the cloud parameters exhibit strong spatial variations with smaller effects over the ocean, unpolluted, or cloudy areas similar to Fig. 13. A global analysis of the AMF differences shows that the AMF differences for OMI orbit 12414 are consistent with those for both days."

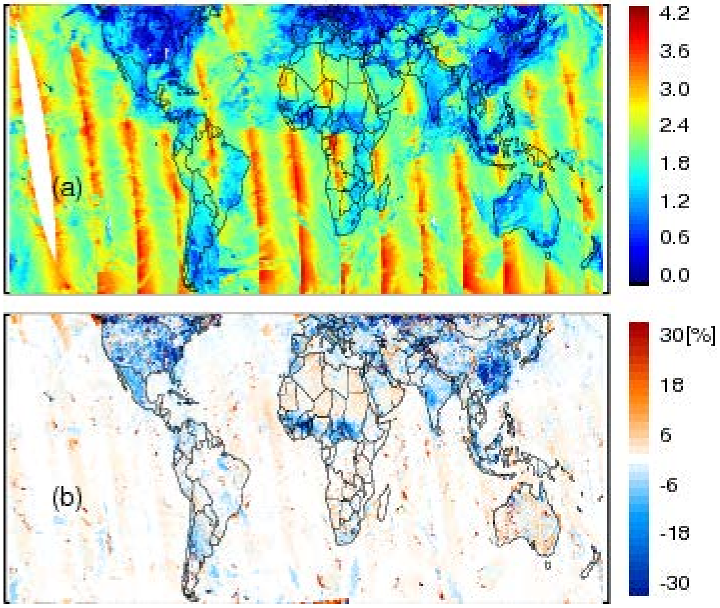

Figure. (a) AMF calculated with the geometry-dependent LER; (b) AMF differences.

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.

The reviewer is right; we use our own O2-O2 cloud algorithm to get full control of the settings. We briefly mention comparisons of our algorithm with the operational O2-O2 algorithm just to provide some information about verification of our own algorithm. We do not use the operational O2-O2 algorithm products in our comparisons. That is why our comparisons are not 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.

We added the following in Section 3:

"We neglect contributions from oceanic foam that can be significant for high wind speeds."

and Conclusions:

" We plan to improve the oceanic model of BRDF by including a variable wind speed and oceanic foam with areal fraction that depends on the wind speed in our computations."

- 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.

We added to Section 2.1 the following:

"We account for polarization at the ocean surface using a full Fresnel reflection matrix as suggested by Mishchenko and Travis (1997)."

Mishchenko, M. I. and Travis, L. D.: Satellite retrieval of aerosol properties over the ocean using polarization as well as intensity of reflected sunlight, J. Geophys. Res., 102, 16989–17013, doi:10.1029/96JD02425, 1997.

- 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?

All our algorithms are based on the MLER approach, i.e. clouds are treated as an opaque Lambertian surface. That is why we do not consider 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?

To address to this question we added at the end of Section 3 the following:

*"Averaging the BRDF coefficients over an OMI pixel may not be equivalent to averaging the high resolution surface LER over the OMI pixel. We carried out a numerical experiment of calculations of TOA radiances using the high resolution BRDF coefficients and OMI geometries for the US Washington-Baltimore corridor area (Fig. 1). The TOA radiances were converted into LERs using Eq. 6 and then the LERs were averaged over OMI pixels. The resulting LERs were compared with that calculated from the standard procedure of averaging the BRDF coefficients first. We found that the mean LER difference was equal to 0.75*10^-5 with the standard deviation of 4.2*10^-4 which is quite acceptable for our purposes."*

- Is equation 9 used for the figures? If so, isn't that creating a bias in the analysis?

Eq. 9 is not used for the figures. Eq. 9 is mostly intended to illustrate the effect of changing surface reflectance on AMF in cloudy conditions. We clarify this in the manuscript by adding the following:

"It should be noted that we derived Eq. 9 and 10 to qualitatively illustrate the effect of changing surface reflectance on AMF in cloudy conditions. The equations are not used to produce data in the figures. The data in the figures of Section 6 are obtained numerically using Eq. 8."

- Which data is shown in Figure 13?

Orbit 12414 of 14 Nov 2006 for data over America and orbit 12391 of 13 Nov 2006 for data over China. We added this information to the figure caption.