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Atmospheric Measurement Techniques Discussions

Interactive comment on "Effect of surface BRDF of various land cover types on the geostationary observations of tropospheric NO₂" *by* K. Noguchi et al.

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We would like to thank the referee #3 for providing helpful comments for the original manuscript. We took into account these comments for the revised manuscript. We describe our responses to the referee's comments below.

Major comments:

1. With the purpose of obtaining an improved understanding of surface reflectance effects on tropospheric NO2 retrievals from a geostationary satellite instrument (GMAP-Asia, 10x10 km2 spatial resolution), the authors investigate the effect of BRDF on

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radiative transfer above specific land types. They build on existing MODIS products at very high spatial resolution to construct a BRDF-function. Then they use a very high-resolution land cover classification data from ALOS/AVNIR-2 to determine the most probable BRDF for a range of MODIS 1x1 km2 scenes in the wider Tokyo area. This is all sound and straightforward. But then Noguchi et al. proceed to investigate the effect of the BRDF-variability introduced by land cover differences on virtual (1x1 km2) NO2 tropospheric air mass factors, and report strong effects (differences up to a factor 2). While I think that these numbers are useful in themselves, they are probably an exaggeration of what may be expected in terms of AMF variability for a real GMAP-Asia (or TROPOMI) pixel that will cover a 100 km2 area with considerable land cover type heterogeneity. Therefore my main concern is that the AMF differences brought about by different land types at the scale of a relevant satellite pixel have not been reported yet, but it is easy for the authors to do so in a revised version of this study. To evaluate the effect of incomplete descriptions of surface reflectance, the authors also use the MODIS product to construct the Bidirectional Reflectance Factors, the Black Sky Albedo, and the White Sky Albedo, and they investigate the effects of these descriptions of surface reflectance, as well as aerosol scattering on the 1x1 km2 AMFs. The approach and findings appear useful, but, again, would gain in value when applied to the more relevant scale of a satellite pixel.

Reply: The reviewer has a good point, and we agree with the comment that realistic satellite measurements will usually see mixed surface types. In the present study, we focused on simple surface types to evaluate the range of possible effects. Although an exact treatment of a mixed BRDF is not straight forward, it would be a good assumption that the effect of the mixed BRDF on the AMF is just the average of the individual effects, weighted by the contribution of the different surface types included in the field of view (FOV) of the sensor. We added a discussion on this in the fourth paragraph of the Concluding remarks as follows:

“For real measurements, we also need to consider the problem of mixed surface types,

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since the field of view of the sensor of the GMAP-Asia project is 10km or less which would usually include several surface types simultaneously. The present study focused on simple surface types to evaluate the range of possible effects. Although an exact treatment of a mixed BRDF is not straight forward, it would be a good assumption that the effect of the mixed BRDF on the AMF is just the average of the individual effects, weighted by the contribution of the different surface types included in the field of view of the sensor. The effect of such mixed surface types on real measurements should be evaluated in future studies."

2. Abstract, line 5: 'the East Asia' should read 'East Asia' or 'eastern Asia'.

Reply: According to the comment, we removed "the" from the phrase.

3. P3445, L11-12: that surface parameters need to be known with high spatial resolution was discussed also in Zhou et al. [2009], and Boersma et al. [2011]. It would be appropriate to also cite these papers.

Reply: Following the suggestion, we now refer to those literatures in the second paragraph of Introduction.

4. P3445, L18: please be specific for what instrument (i.e. overpass time, viewing geometry) the findings of Zhou et al. [2010] hold.

Reply: We add the following description in the third paragraph of Introduction:

"Zhou et al. (2010) pointed out the importance of BRDF for tropospheric NO2 retrievals from OMI measurements, where viewing zenith angles on the ground surface are 0-70 deg and the overpass time is 13:45 at the equator,. . ."

5. P3445, L25: what is meant with 'OMI-based LER version 3'? This should be clarified.

Reply: It meant "Aura OMI Surface Reflectance Climatology Data Product-OMLER (V003)", which is obtained at the following URL: http://disc.sci.gsfc.nasa.gov/Aura/data-

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holdings/OMI/omler_v003.shtml

According to the comments, we corrected the sentence. We removed the word “version 3” since it is not essential in the context.

6. In section 2.1, it would be appropriate to discuss the quality of the MODIS albedo products. Have they been evaluated against other albedo datasets or against independent parameters?

Reply: For discussion, we added references for the validation of the MODIS albedo products in the third paragraph of Concluding remarks, where we had briefly described the validation for rice paddy in the original manuscript.

7. P3446; are there any differences in the construction of the BRDF between this work and the work by Zhou et al. [2010]?

Reply: There is no difference between them. We added the following description at the beginning of the first paragraph in section 2.1.

“Following the method proposed by Zhou et al. (2010),. . .”

8. P3447, L15-16: It’s unclear what this sentence tries to convey. I think the authors intend to say that the BRF, WSA, and BSA are all treated as LER values when the AMF and BAMFs are calculated with the radiative transfer. Please rephrase.

Reply: According to the comments, we added the following sentence in the third paragraph of Section 2.1.

“This means that the BRF, WSA, and BSA are all treated as LER values when the AMF and BAMFs are calculated with the RTM.”

9. P3448, L22-23: The details of the aerosol types assumed should be somewhere in this paper, not just by referring to an earlier paper.

Reply: According to the comments, we include a new table in the manuscript as Table

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3.

10. P3449, L25-26: How is the 100% urban or rice paddy result different from the 90%-95% threshold? Is it relevant?

Reply: We showed the results for 90%, 95% and 100% in Figure 1 of the supplement. The difference between the results for 90/95% and 100% is 10% or less for the urban type surface but larger for the rice paddy type. The main reason of the difference is the extremely small number of pixels for the case of 100%. Therefore, we rely on the results for 90/95% rather than the ones for 100%.

11. P3450, L10-12 and Figure 5: why does the deciduous forest type have the highest kernel values in winter? When trees lose their foliage in fall, I would expect the forest to appear darker as seen from space.

Reply: We agree and have to admit that we have no good explanation for this phenomenon at the present moment. It could be a snow contamination which is not detected by MODIS in deciduous forests, or an illumination of the trees from the side by the low sun which would make them appear brighter depending on relative azimuth. In future studies, we would like to examine this problem.

12. In section 3.3 and 3.4, I suggest to repeat that one constant NO2 profile has been assumed.

Reply: We add the following phrase in the first paragraph of section 3.3 as suggested.

“We assume the constant NO2 profile shown in Figure 4.”

We also add the following sentence in the first paragraph of section 3.4.

“Again, we assume the constant NO2 profile shown in Figure 4.”

13. P3452, L3: ‘whenever’?

Reply: Yes. We modified it according to the comment.

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14. P3452, L10: this sentence is not very clear. Since a vertical column is retrieved, an altitude-dependent error appears odd. I think the authors mean an error that depends on the assumed vertical distribution of NO2 (in the AMF calculation).

Reply: We change the sentence as follows:

“This suggests that using BRF, BSA or WSA instead of BRDF as an approximation of the surface albedo causes an error which depends on where the NO2 is located in the atmosphere, the difference becoming large if a high concentration of NO2 exists in the lowest layers.”

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 3443, 2014.