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Atmospheric Measurement Techniques Discussions

Interactive comment on "Accounting for surface reflectance anisotropy in satellite retrievals of tropospheric NO_2" *by* Y. Zhou et al.

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First of all, we want to thank this reviewer for the thoughtful and constructive comments. We have prepared a manuscript with minor revisions and additions which will be submitted shortly.

In the following reviewer comments will be in italics, our response in normal typeface.

1. Response to general comments

The study does not comment on oceanic surfaces. What are the general problems over ocean causing the large oceanic regions without any values? Why are there, in spite of that, values existing over large parts of the North Sea? How should oceanic

pixels be treated in future, to the authors' suggestion? Many sources (Cities) are close to the coast, and thus the NO_2 retrieval has to be accurate also over oceans!

Author response: The referee is right that this study is focusing on NO_2 over land due to the fact (P1977 L22-29) that MODIS BRDF/albedo standard products MOD43B are provided by the MODIS Land Team, which cover all land and coastal areas and shallow water regions (within 5 km of land and less than 50 meters deep). Therefore, in spite of the missing values over large oceanic regions, values exist over parts of the North Sea and the coastal areas where the water is less than 50 meters deep. For oceanic pixels, we mentioned in the paper (P1986 L1-5) that more complex models with non-linear parameters are often used for more accurate modeling of the BRDF over water surfaces. We suggest to use the LER data sets for oceanic pixels as long as no operational BRDF products are available for water surfaces. In fact, in the meantime we have processed all OMI data using the Kleipool et al. (2008) LER data set for all oceanic pixels where no MODIS BRDF data are available to make sure no relevant NO2 information (e.g. from ship emissions) is lost over the oceans.

One result of this study is that the dependency of NO_2 VCD_{trop} on the surface reflectance dataset (TOMS/GOME versus MODIS) has a much larger effect than the anisotropy, and an update of the DOMNO product is under way using the Kleipool albedo dataset. It would thus be quite interesting to include the Kleipool LER in this study, e.g. in Fig. 11.

Author response: We agree that it would be interesting to include the Kleipool et al. (2008) LER data set which was introduced in the DOMINO product on 17 February 2009. However, for the period 2006 and 2007 covered in this study the Kleipool LER data set has not been applied to the DOMINO product yet and it would involve a substantial amount of work to reprocess the data with the Kleipool LER climatology in the same way as we have done for the GOME/TOMS LER data set (including a recomputation of cloud parameters). Nevertheless, we mapped the Kleipool OMI LER at 440nm onto the OMI pixels selected in the paper, and added a new figure (new Fig. 11

in the revised manuscript) to show the comparison between the reflectance data sets including BRF, black-sky albedo, TOMS/GOME LER and OMI LER. A more detailed analysis of the impact on NO_2 VTCs will be done in a future study.

2. Response to specific comments

P1975 L1: Koelemeijer accounts for inter-annual variability (monthly means).

Author response: In the meteorological and climate community the word interannual is used to describe changes from year-to-year, not seasonal (intra-anual) variations. The Koelemeijer data set is a monthly climatology and thus only describes seasonal but no interannual variations.

P1976 L20: Is it possible to be more specific w.r.t. the "larger" uncertainties?

Author response: We tried to find more specific information on uncertainties in spectral albedo, but unfortunately validation is generally done for broadband albedo and not for the spectral range we are interested. The cited publication of Vermote and Kotchenova (2008) provides only very rough hints that the uncertainty is "larger" at 470 nm but no quantitative values. We had also been in contact with the author before submitting our manuscript but had not received any more specific information.

P1977 L2-3: I assume the angles are defined at the ground?

Author response: Yes, the angles are defined at the ground as shown in Fig. 1. We added ", which are defined at the ground (see Fig. 1)." in the revised manuscript.

P1977 L24: What are the aerosol assumptions for the MODIS retrieval and how far might aerosols affect both MODIS albedo and NO_2 retrieval?

Author response: The MODIS Collection 5 albedo/BRDF retrieval algorithm includes an internal aerosol inversion as described in Vermote et al. (2008) to correct for the presence of aerosols. Aerosols are clearly also an important factor affecting NO_2 retrievals and the way this problem should best be dealt with is still an open issue. However,

we don't think it would be adequate to discuss this topic in the framework of this study. The way DOMINO (and our retrieval) is dealing with aerosols has been described in previous studies (e.g. Boersma et al. 2004).

P1978 L21: Please discuss how far the channel3 albedo is applicable for the NO_2 fit wavelength range, and what systematic errors this inconsistency may cause.

Author response: We have checked albedo differences between 470nm (center of MODIS channel 3) and 440nm (NO_2 retrieval) reported by Kleipool et al. (2008). According to their Fig. 15, the spectral dependence of the LER within this wavelength range is very small for most of the land types. Obvious differences exist only over water (the average LER decreases from about 0.058 at 440nm to 0.05 at 470nm) and bare land (the average LER increases from about 0.135 at 440nm to 0.15 at 470nm). A discussion of this point has been added in the revised manuscript.

P1979, L1: "8 days . . .16 days": I do not understand this setup. Which 16-day observation period? From MODIS or from OMI?

Author response: The way the MODIS products are constructed is indeed somewhat confusing. The MOD43B products are produced every 8 days based on observations from a longer 16-days period (P1978 L23-24). There is thus some temporal overlap between MODIS observations contributing to neighbouring 8-daily data sets. The following sentence (P1978 L26-P1979 L1) probably added to the confusion. It has been changed to: "The 8-day MODIS datasets were then applied to all OMI observations from days 5 to 12 within the corresponding 16-day MODIS observation interval.

P1979, L21: "each OMI pixel": also clouded ones? Is there a cloud-threshold applied for the comparison of NO_2 VCD_{trop}.

Author response: Yes, for the comparison of NO_2 VCD_{trop} in Section 4 a threshold for the cloud radiance fraction of 50% was applied as described on P1989, L8-9. We think it would not be appropriate to mention this already at this point.

P1980, L24: "Kleipool et al. . . .used in DOMINO": This reads as if DOMINO already involves the Kleipool albedo. Is this really the case?

Author response: Thanks for this good point. Yes, the Kleipool LER has been introduced in DOMINO on February 17, 2009 but the earlier data have not yet been reprocessed (the re-processing will be part of the next product version). P1974 L28-29 has been modified as follows: "high resolution OMI observations has become available and has been introduced in the operational Dutch OMI NO_2 (DOMINO) product on February 17, 2009 (Boersma et al., 2009a)" in the revised manuscript.

P1981, Eq 4: Please define all quantities and subscripts ("cr") directly after the Equation.

Author response: We add the following definitions on P1981 L22: "p_{c} is the cloud pressure, p_{eff} the surface pressure, f_{cl} the OMI effective cloud fraction, and I_{cl} and I_{cr} are the radiances for cloudy and clear scenes, respectively.".

P1982 first paragraph: Which albedo dataset was used within the original OMI cloud algorithm? How large are the deviations from your updated algorithm? Is there a recommendation to re-run the OMI cloud product with a new albedo database?

Author response: For consistency the OMI cloud algorithm uses the same albedo data sets as the NO2 algorithm, i.e. TOMS/GOME (Koelemeijer) before 17 February 2009 and OMI (Kleipool) thereafter. This information will be added in the revised version. Furthermore, a discussion about differences in cloud fraction and cloud pressure retrieved with our MODIS data set as compared to TOMS/GOME will be added in Sect. 4.2. As described on P1980, L28, given the significant differences between the different albedo data sets we think it is mandatory to re-run the OMI cloud product with any new albedo database as was done in our study.

P1987 Section 3.3: I needed some time to understand Fig. 8, as there should not be an angular dependency of VCD_{trop} on angles, if the retrieval correctly accounts for

angular dependencies (BRDF)! Then I noticed that Fig. 8 results from the assumption of a constant, i.e. angular independent, SCD. This is a rather strange point of view. I thus recommend to discuss the sensitivity of AMFs (instead of VCD_{trop}) on surface reflectance in chapter 3.3. I assume that this shift in focus would not mean much effort, and will not alter the conclusions, but would be more appropriate and less confusing than assuming an angular-independent constant SCD. By the way, I recommend to define and use a symbol/abbreviation for VCD_{trop}, instead of just writing about "NO_2" or "NO_2 retrieval".

Author response: Thanks for this good point. Chapter 3.3 and Fig. 8 have been modified in the revised manuscript now discussing the sensitivity of AMFs instead of VCD_{trop}. Since VTCs (vertical tropospheric columns) have been defined in the paper, we replace "NO_2" or "NO_2 retrieval" by "VTCs" or "NO_2 VTCs" in most cases unless "retrieval" refers to the retrieval algorithm in the paper.

P1990 first paragraph: Why are the spatial patterns for Nov (d and f) that different (in sign)?

Author response: Fig. 9 can help understand the large difference in sign between Fig. 10d and Fig. 10f. The blue lines (winter cases) in Fig. 9a are mostly positive (using the BRF reflectance as LER results in mostly higher NO_2 than using full BRDF) while in Fig. 9b they are mostly negative (using black albedo results in lower NO_2 than using full BRDF), which correspond to Fig. 10d and Fig. 10f, respectively. Thus, in winter the MODIS black albedo (which is a hemispherically integrated quantity) is significantly higher than the BRF values for most of the range of viewing zenith angles covered by OMI (and therefore NO_2 retrieved with black albedo is lower). In winter the BRDF has a large angular dependence with very high values at high viewing zenith angles (see Fig.5d) which appear to strongly contribute to the black albedo. It should be noted that MODIS itself only covers a limited viewing zenith angle range and therefore the extrapolation of the BRDF to high angles as needed for the computation of black albedo is uncertain, and in winter at high latitudes (i.e. high solar zenith angles) may

actually be unrealistic. This is another advantage of using the BRDF directly.

Section 4.2: An alternative approach would be an analysis of the swath-dependency of the operational DOMINO product. The advantage would be that a longer time-period and also other regions could be investigated without additional input. I would assume that taking 5 years of OMI data would allow statements on the mean swath dependency with sufficient statistics, i.e. eliminating the sampling issue. Of course, the other difficulties (local time, cloud interference) would remain.

Author response: A longer period of data would clearly be valuable and provide better statistics. However, we prefer not to mix our analysis with yet another data set (DOMINO) which is different from our GOME/TOMS LER data set in several aspects as described in Zhou et al. (2009). So far, our own processing of the OMI data using GOME/TOMS LER is only available for 2006 and 2007 and, due to the stronger BRDF effects in November, we chose to focus on the November data of the two years. We leave a more extensive analysis to a future evaluation of our data set.

Table 1: 2 times "land"

Author response: The typo has been corrected.

Figs. 3 and 7: This study focuses on the troposphere. I thus recommend to show altitude in km as y-axis and focus on the lowermost (5 km) troposphere, replacing or at least complementing the current pressure-plots. Esp. for Fig. 3, the differences in the BL would show up much clearer.

Author response: We don't agree on this point. Selecting pressure as (linear) vertical axis better emphasizes the tropospheric part of the profile than a plot versus altitude and that is the reason why we selected this way of presenting the data. For example, in our figure the range between the surface (1000 hPa) and the middle of the troposphere (500 hPa, or approx. 5 km) spans about 50% of the y-axis.

Figs. 5 right: as the dependencies on VZA are smooth, I would prefer to have lines

instead of symbols.

Author response: Since the figure appears to us to be sufficiently clear using symbols we refrain from changing it to lines.

Figs. 6 and 9: The calculation of NO_2 BRF involves averaging over the occuring range of angles. The large negative relative deviations at low VZA would be thus considerably closer to 0 if the outermost pixels would be skipped. I.e. the high VZA does not only show extremely high deviation, it also lifts the mean! In other words, the observed deviation for low VZA depends on the chosen range of VZA. This should be mentioned somewhere.

Author response: No, the NO_2 BRF does not require any averaging over angles and cutting to a smaller VZA range wouldn't change the lines in figures 6 and 9 in any way. In particular, it wouldn't move the lines up or down. Black albedo, on the other hand is a hemispherically integrated quantity. The problem with this quantity was already pointed out in our answer to *"P1990 first paragraph: Why are the spatial patterns for Nov (d and f) that different (in sign)?"* above.

Figs. 11: What would the difference Kleipool vs. BRDF look like? I recommend to have another figure showing the maps of the different albedo datasets (BRF, bs, TOMS/GOME, Kleipool) and their deviations.

Author response: A new Fig. 11 and a more detailed discussion of the different albedo data sets and their impact on the NO_2 retrieval have been added in the revised manuscript.

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