Atmos. Meas. Tech. Discuss., 3, C806–C809, 2010
www.atmos-meas-tech-discuss.net/3/C806/2010/

Atmospheric Measurement Techniques Discussions

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

Anonymous Referee #2

Received and published: 28 June 2010

The study "Accounting for surface reflectance anisotropy in satellite retrievals of tropospheric NO2" by Zhou et al. investigates, to my knowledge for the first time in literature, the impact of surface reflectance anisotropy, which is usually neglected in NO2 retrieval schemes. It is a well-written and solid done sensitivity study which fits well in the scope of AMT and should be published after some minor revisions and additions as listed below.

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 NO2 retrieval has to be accurate also over oceans!

- One result of this study is that the dependency of NO2 VCDtrop on the surface reflectance dataset (TOMS/GOME versus MODIS) has a much larger effect than the anisotropy, and an update of the DOMINO 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.

Specific comments:

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

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

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

P1977 L24: What are the aerosol assumptions for the MODIS retrieval, and how far might aerosols affect both MODIS albedo and NO2 retrieval?

P1978 L21: Please discuss how far the channel3 albedo is applicable for the NO2 fit wavelength range, and what systematic errors this inconsistency may cause.

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

P1979 L21: "each OMI pixel": also clouded ones? Is there a cloud-threshold applied for the comparison of NO2 VCDtrop below?

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

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

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?

P1987 Section 3.3: I needed some time to understand Fig. 8, as there should not be an angular dependency of VCDtrop 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 VCDtrop) 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 VCDtrop, instead of just writing about “NO2” or “NO2 retrieval”.

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

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 statisitcs, i.e. eliminating the sampling issue. Of course, the other difficulties (local time, cloud interference) would remain.

Table 1: 2 times “land”

Figs. 3 and 7: This study focusses 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.

Fig. 5 right: as the dependencies on VZA are smooth, I would prefer to have lines instead of symbols.

Figs. 6 and 9: The calculation of NO2 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.

Fig. 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.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 1971, 2010.