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Atmospheric Measurement Techniques Discussions

Interactive comment on "An improved tropospheric NO_2 retrieval for satellite observations in the vicinity of mountainous terrain" *by* Y. Zhou et al.

Anonymous Referee #2

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In their paper "An improved tropospheric NO2 retrieval for satellite observations in the vicinity of mountainous terrain", Zhou et al. report on a correction procedure applied to the DOMINO OMI tropospheric NO2 product to improve the representation of surface pressure in the retrieval. They show that replacing the coarse TM4 based pressure fields by high spatial resolution fields in the Alpine region leads to significant increases in the retrieved tropospheric NO2 in the Swiss plateau and the Po-valley, two regions where the coarse surface pressure is lower than the real pressure. They present some sensitivity studies showing that the effect is larger for cloudy pixels and in winter and compare the updated retrievals with surface measurements.

The paper is clearly structured and well written. It reports on an interesting issue in satellite remote sensing of tropospheric composition and fits well into the AMTD scope. However, I do have some questions and reservations to the methods and results presented as listed below which have to be addressed properly before the paper can be accepted for publication in AMT.

1) The topic discussed in this paper is not new but rather an update of previous work by the same group. However, the results shown now indicate a clearly smaller effect, and the reasons for this need to be discussed. Two possible explanations are already given in the text (a bug in the DOMINO AMF retrieval and the small number of profiles discussed in Schaub et al.) but also there seems to be a difference in the application of the block airmass factors in Fig. 13 of Schaub et al. and Fig. 7 of this work.

2) The main problem of this study is, that only the pressure is adjusted to high spatial resolution while the NO2 profile (as well as the surface albedo) remain at low spatial resolution. This leads to inconsistencies when applying the correction for topography as the average NO2 profile for the region is shifted in altitude but not adapted for other effects such as emissions (Po-valley vs. Alps), local meteorology (quite important in the Po valley) or temperature (and thus BL height). Therefore, any improvement one might find by refining topography could be coincidental as the effects might be overruled once more appropriate meteorological and emission data are used. I'm sure that the authors are aware of this problem but it is never stated in the paper and needs to be discussed.

3) Both in the title and throughout the paper, it sometimes is not clear that the discussion is really about one specific product (DOMINO OMI NO2) and not satellite retrievals in general. While the underlying problem (if you don't use the right topography you won't get the right airmass factor) is valid for all retrievals, the details are very specific to the implementation of DOMINO. This needs to be made clear in the title and the text.

4) In contrast to the work in Schaub et al., the authors have decided to conserve the mixing ratios and not the sub-columns in their profiles. While I can see some reasons

for that, it is interesting to point out that this choice will amplify the effect as moving the same mixing ratio down increases the weight of the lowest layers which will increase the NO2 in the revised retrieval over low altitude sites. Please explain why you have taken this decision.

5) In the part on the effect on cloudy pixels, some discussion is needed on how the pressure in the cloud retrieval is determined (what is the surface data base used in the O2-O2 retrieval? How accurate are the results? How consistent is the rescaling of the TM4 profile with the assumptions made in the cloud retrieval?).

6) The molybdenum correction applied to the in-situ observations is very large and has a distinct seasonality. In fact, the changes from this correction appear to be much more important than those from the pressure correction on the satellite data, and I wonder how large the uncertainty of this is.

7) When comparing in-situ and satellite columns, two different in-situ columns are used for the standard and the new retrieval. The differences are quite large, certainly of the order of the changes in the satellite data. This is mainly the effect of the choice to scale mixing ratios, not concentrations (see my comment above). This needs to be discussed in the paper.

8) In Fig. 14., something is odd in spring. While for all other seasons the new columns are larger than the original ones, this is not the case in spring for the individual groups. However, it is the case for the 'all stations' panel. Please check for plotting mistakes.

9) I'm missing a table showing the change in average column and correlation coefficient per season and station group with the two retrievals. Please add to demonstrate the improvement of your updated retrieval.

10) One point that is also not addressed in the paper is the effect of temperature dependence of the NO2 cross-section which to my knowledge in the DOMINO product is corrected with the airmass factor but is not mentioned here. What exactly happened

with this correction in the retrievals shown here?

Typo: page 787, cm2 should read cm3

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 781, 2009.