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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.

**y. zhou**

yipin.zhou@empa.ch

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First of all, we want to thank this reviewer for the constructive comments. We followed them all as described in detail below.

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

*(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.*

Author response: We agree that the discrepancy between results in this study and those reported in Schaub et al. (2007) need to be better explained. The same concern was raised by reviewer 1 and we therefore invite you to read our response to the questions of referee 1 concerning P789, 13-20 and P793, 1-3. A discussion will also be added in the revised manuscript concerning Fig. 13 in Schaub et al. (2007) which does not correctly illustrate the effect of a changing surface pressure on the box AMF profile and which is indeed not consistent with Fig. 7 of this work (please see our corresponding response to referee 1).

*(2) The main problem of this study is that only the pressure is adjusted to high spatial resolution while the NO2 profiles (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.*

Author response: Yes, we are well aware of this problem and the main goal of our current work is to improve this situation step by step for a limited study domain such as central Europe. We probably did not state sufficiently clearly that this work is only the first step in this process (although it was mentioned in the introduction). We are currently working on improvements to the ground reflectance (see our contribution to the recent EGU conference on the use of MODIS data, contribution ID: EGU2009-10160) and, in a following step, will replace $NO_2$ a priori profiles by output from a regional scale air quality forecast model. However, even when data sets at higher spatial resolution will become available in the future, problems with too low resolution input data and resolution mismatches will clearly remain, though probably less severe, in particular for the operational products with global coverage. We agree that improving the resolution of only one of the input data sets while keeping the others at low resolution may generate inconsistencies. In our opinion, the topography is one factor that can and should be done accurately even if the $NO_2$ a priori profiles are at a lower resolution (which is and will remain true in the near future for all operational products). Like this we can at least assure a correct simulation of box airmass factors (assuming albedo is correct) such that errors with the computation of the total air mass factor (Equation 1) only remain with the $NO_2$ a priori profile. It is clear that higher resolution surface reflectance and a priori profile data sets (and aerosol information) must follow in a next step to really achieve a significant improvement for regional scale retrievals, and their improvement will be more relevant than the topography issue. We admit that this has not been pointed out sufficiently well in our manuscript. In the revised manuscript we will enhance the introduction and discussion sections accordingly.

*(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 $NO_2$) 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.*

Author response: We agree that improvements are needed and the same point was raised by referee 1. Please have a look at our response to the first question about section 2.1 (Introduction) from reviewer 1. Title and introduction part of the article will be modified. Some details like how exactly the a priori profiles are changed are indeed specific to the DOMINO product. It should be noted, however, that the basic concepts (e.g. preserving mixing ratios for rescaling the a priori profile) can easily be transferred to other coordinate systems and the main results, the influence of a shift in the topography on the box AMF profile, on the total AMF and on the vertical $NO_2$ columns are not specific to DOMINO. The GOME-2 technical document http://o3msaf.fmi.fi/docs/vs/2005/o3safvsgome-2NO2finalreport061126.pdf for exam-



ple draws very similar conclusions with respect to the modification of the box AMF profiles.

*(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 $NO_2$ in the revised retrieval over low altitude sites. Please explain why you have taken this decision.*

Author response: This is a good point. Compressing or expanding a vertical column of air conserves mixing ratios rather than subcolumns. This physical fact is the main motivation for our choice. However, we found that preserving mixing ratios or subcolumns makes almost no difference for the results. This is understandable as it is only the shape of the a priori profile that matters but not the absolute values. For the case presented in Figure 8b, for instance, the $NO_2$ VTC changes by 8% when shifting the profile down and conserving mixing ratios (see table 1), and it changes by almost the same amount (8.03%) when conserving subcolumns. It is not correct that our choice amplifies the effect compared to that followed by Schaub, but rather the opposite. This will be mentioned in the revised manuscript.

*(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 $O_2$-$O_2$ retrieval? How accurate are the results? How consistent is the rescaling of the TM4 profile with the assumptions made in the cloud retrieval?).*

Author response: Ok, some more details about the OMI cloud retrieval algorithm will be added. Note that a high-resolution topography data base is already used for the cloud retrieval. By switching the trace gas retrieval to GTOPO the consistency with the cloud retrieval is therefore significantly improved. The following lines will be added:

"The retrieval method for OMI cloud parameters uses the top-of-atmosphere reflectance as a measure to determine cloud fraction, and the depth of the $O_2$-$O_2$ band as a measure to determine cloud pressure (Acarreta et al., 2004)."

"In the OMI cloud retrieval algorithm the topography is represented by the ETOPO digital elevation data set which, at the scale of individual pixels, is almost identical to GTOPO30 used here. By adapting the GTOPO30 data set in the $NO_2$ retrieval the consistency with the cloud retrieval is thus significantly improved."

A discussion on cloud pressure accuracy and its possible impact on this study will also be added. Please refer to our response to the question about P794 from reviewer 1.

*(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.*

Author response: The uncertainty in this correction is indeed a major limitation for the validation of satellite $NO_2$ columns with in situ $NO_x$ measurements and the same problem had been encountered in many previous studies. Long-term monitoring data with selective $NO_2$ instruments are virtually unavailable so far (Empa is currently trying to improve this situation with 3 stations permanently equipped with both photolytic and molybdenum converters). In general the problem becomes larger the more distant a station is located from ($NO_x$) emission sources. Since the Po Valley is a rather strongly polluted area the corrections are generally smaller than those applied previously for the Swiss Plateau e.g. by Schaub et al. (2006) but still considerable. In winter, the overestimation of $NO_2$ by molybdenum converters is smallest and therefore the results are more reliable in this season.

Our main conclusion that the OMI $NO_2$ VTCs tend to underestimate $NO_2$ in winter (as written in the conclusions) and that this problem is reduced by the better treatment of the topography is thus little affected by this uncertainty. However, considering that the uncertainty in vertical $NO_2$ columns derived from the in situ measurements is generally



larger than the differences between the original and new retrieval, we have generally refrained from drawing firm conclusions regarding any other improvements of the retrieval.

*(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.*

Author response: Agreed, this needs to be discussed. The larger ground based column obtained with the lower (correct) surface is a simple consequence of the increased column of air. The difference thus scales with the difference between $p_{\mathrm{eff}}$ and $p_{\mathrm{TM4}}$. Note that this effect is different from the effect on the AMF calculation where choosing to conserve mixing ratios or subcolumns makes no real difference. The following lines will be added at P798, 5:

"Due to our choice of preserving mixing ratios in the rescaled $NO_2$ profile, the $\mathrm{VTC_G}$ calculated with $p_{\mathrm{TM4}}$ are somewhat smaller than those calculated with the higher effective surface pressure $p_{\mathrm{eff}}$. These differences are of a similar order but generally smaller than the differences between the satellite $NO_2$ VTC obtained for $p_{\mathrm{TM4}}$ and $p_{\mathrm{eff}}$, which are a consequence of the differences in the box AMFs near the surface rather and not of the conservation of mixing ratios."

*(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.*

Author response: Thanks for this suggestion. We double checked our results and found no error. This counterintuitive result is an effect of using median values rather than means.

*(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.*

Author response: As stated above, we do not intend to draw firm conclusions regarding improvements of the retrieval based on this validation. Furthermore, the focus of the paper is on the method itself instead of the validation. In order to avoid making this paper even longer, we therefore decided not to add one more table in the validation section. We intent to publish a more detailed validation analysis once we have refined the other parameters albedo and a-priori profiles as well.

*(10) One point that is also not addressed in the paper is the effect of temperature dependence of the $NO_2$ 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?*

Author response: This is a good point. We follow the same approach to correct the temperature dependence of the $NO_2$ cross-section in the box AMF calculation (Boersma et al., 2004). A linear relationship between the logarithm of the surface pressure and the effective absorption temperature is assumed. For a lower GTOPO altitude the effective temperature is correspondingly larger. Note, however, that this modification of the temperature profile has a negligible effect on the AMF (Boersma et al., 2004). The temperature correction term will be described explicitly in the revised manuscript.

*(11) Typo: page 787, cm2 should read cm3*

Author response: Changed

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