Atmos. Meas. Tech. Discuss., 2, C23–C28, 2009
www.atmos-meas-tech-discuss.net/2/C23/2009/

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 #1

Received and published: 13 April 2009

The paper by Zhou et al. describes an approach to reduce topography-related errors of tropospheric NO2 from OMI satellite measurements over the Alpine region, and includes comparisons with ground-based in situ observations. This paper is a follow-up to the study described in Schaub et al. 2007. Although the paper is clearly written and presents some interesting results, there remain important questions (see below) that should be resolved. There are inconsistencies between this study and the results described in Schaub et al. 2007 that are not clearly explained. Therefore, I can not recommend it for publication in AMT in its current stage. The paper may be acceptable after important revisions.

Specific Comments

1 Introduction

A general overview of the treatment of topography in DOAS retrievals is missing in the introduction. The impact of the topography on the air-mass factor is a well known issue in DOAS retrieval of trace-gas columns from satellite measurements. To my knowledge, most state-of-the-art retrieval algorithms take the surface height into account for the calculation of the AMF. A very similar approach to reduce topography-related errors as presented here, using effective pixel-average terrain heights based on GTOPO data, has been used for the operational trace-gas column retrieval algorithms for GOME and SCIAMACHY since many years (see for instance: earth.esa.int/pub/ESA_DOC/GOME/ATBD.pdf, 2004).

P784 22-29: Here it is mentioned that the DOMINO algorithm uses the same coarse resolution data set to obtain the a priori NO2 profiles and surface pressure to ensure consistency. Does this mean that the accuracy in the retrieved NO2 VTC is in general better using this "DOMINO" method? Otherwise, what would be the point of using this "DOMINO" method to ensure consistency, as compared to the use of effective pixel-average terrain heights based on GTOPO data, in terms of the NO2 VTC accuracy?

2. Data and methods

P785, P786 1-12 Three different OMI NO2 products are mentioned, but they are not clearly described. In Boersma et al., 2007, the near-real time NO2 algorithm is called DOMINO, but in this paper, DOMINO seems to stand for an improved off-line algorithm. This must be clarified. Why don't you start with a short description of the "standard" OMI near real time product (with reference), then introduce the improvements in the (DOMINO ??) off-line product? Also, it must be clearly described here if the treatment of the surface pressure in the AMF calculation is the only difference between the algorithm used in this study and the (DOMINO ??) off-line algorithm.

P786, 9-11 Why is the surface albedo data-set now based on Koelemeijer et al, 2003? As described by Boersma et al., 2004, the combined TOMS/GOME surface albedo data-set was used in the “standard” OMI near real time product, because this albedo data-set was considered an improvement upon the Koelemeijer et al, 2003 climatology.

P787, 8-17 The detailed description of the layer definitions in the TM4 model using hybrid level coefficients is not very informative. Please give more information on the number of layers in the lower, middle and higher troposphere.

P788, 21-22 What is exactly plotted in Figure 2? As explained in the text, h_eff is calculated for every OMI pixel, i.e. it has the irregular OMI spatial resolution. Is the h_eff used for Fig 2 an average for a certain period?

P789, 9-12 It would be interesting to known the effect of the additional surface pressure levels in the AMF-LUT on the calculated AMF. I expect that this effect will be rather small.

P789, 13-20 This error in the calculation of the AMF in the DOMINO product, as described here, seems to point to a major problem. Besides possible topography-related errors, this error in the DOMINO retrieval algorithm results in additional errors in the calculated NO2 VTC. How large is this additional error in the NO2 VTC for the polluted Swiss Plateau and Po Valley regions? And how can this error in the DOMINO product explain the much larger topography-related errors (30-40% percent for polluted conditions) reported in Schaub et al., 2007? In the conclusions, the authors write that the error in the calculation of the AMF in the DOMINO product results in too low box AMFs close to the ground (this is correct) and therefore in a too large sensitivity to surface pressure changes. However, looking at Fig 7 and 8, it is not obvious to me how this effect can fully explain the discrepancies between the results reported in Schaub et al., 2007 and this study? Furthermore, I would not describe the elimination of this problem in future versions of the DOMINO product as an “additional improvement”.

3 Results and discussion

P790, 1-10 Did the authors use additional snow cover data to make sure that partly snow covered OMI pixels were excluded in the calculation of the monthly mean maps in Fig 3 and Fig 4? The surface albedo climatology used in the retrieval algorithm does not provide information on the actual snow cover, and partly snow covered OMI measurements could be included in the 50% cloud radiance threshold case.

P791, 24-25 Please include an explanation why the box AMF near the ground increases when the surface pressure is decreased (Fig 7). This explanation is missing in section 3.2.

P792-P793 As described by the authors, the topography-related error for cloud free pixels is relatively small (<=8%) compared to the partly cloudy case. However, there is a relatively long discussion on this error in Section 3.2. I suggest to shorten this section (or combine with section 3.3) by removing the detailed analysis for different retrieval parameters (Fig. 9 and Table 1). A short explanation of the effect for an averaged summer profile (4%) and winter profile (8%) should be sufficient.

P793, 1-3 The results from this study are not consistent with the results reported in Schaub et al., 2007. The topography-related error for cloud free pixels reported in Schaub et al., 2007 is 3 to 5 times larger than the one reported in this study. Is this only due the error in the calculation of the AMF in the DOMINO product, as mentioned in Section 2.2? Here it should also be noted that Fig 13 in Schaub et al. 2007 is not correct: it gives a wrong impression of the topography-related effect (in contrast to figures 7 and 8 in this paper, which are correct). Since this paper presents a follow-up study to Schaub et al. 2007, the problems with this previous study should be clearly described.

P794, 2-4 Why has a fixed cloud fraction of 15% been selected? It would be interesting to know how the effect on the AMF changes for this particular pixel when varying the cloud radiance fraction from 0% to 50%.

P794 The retrieval error due to an inaccurate surface pressure as described in section

3.3, should be related to the error in the retrieved OMI cloud parameters, especially the error in the cloud top pressure. Figure 12 shows that for a cloud top located inside the polluted boundary layer (at 900 hPa), the retrieval error due to an inaccurate surface pressure is large (∼30%), even if the error in the surface pressure is only 20 hPa! However, the uncertainty in the OMI cloud-top pressure is probably larger than 20 hPa, especially for small cloud fractions (0-20%). How large is the uncertainty in the OMI cloud-top pressure for small cloud fractions? How does the uncertainty in the OMI cloud-top pressure effects the error analyses presented in this section? This issue should be discussed as well in this section.

4. Validation

P796, 1-7 It is not clear to me if molybdenum converters are sensitive to NOx (NO2+NO) or to NO2 only (besides the sensitivity to other odd nitrogen species such as PAN, HNO3 and organic nitrates)

P798,8 Are additional actual snow cover data used to exclude (partly) snow covered OMI measurements, or is the selection only done via the cloud fraction and the surface albedo climatology used in the OMI retrieval algorithm?

P798,15-17 It is understandable that very high correlation coefficients (> 0.7-0.8) are not to be expected when combining OMI NO2 VTC and in-situ measurements. However, I do not understand why measurements with correlation coefficients of 0.3-0.5 are already considered “well correlated”.

P800, 8-10 Please also explain why the slope and r2 for Motta are improved with p_eff, while the corresponding monthly mean comparison is not improved.

General remark.

Neither this paper nor Straub et al., 2007 give information about possible errors related to the other important surface parameter: the surface albedo. Why is there no discussion on possible surface albedo related errors for this region in this paper? How large

is the impact of surface albedo uncertainties on the NO2 VTC for this region, compared to the topography related errors?

Minor Comments

P783,9-13 Please add the satellite platforms of the instruments mentioned here.

P783,17 It is better to write “the gradually improving spatial resolution”

P784, 3-5 Aerosol have a large impact on the AMF as well, and therefore aerosol properties are also key input parameters for the AMF calculation.

P784, 5-6 Is it really a new type of error that was identified by Boersma et al, 2007? To my knowledge, these types of errors were already discussed for the TOMS instruments. Therefore, it is better to write: “Boersma et al. 2007 described the type of errors . . .”

P788, 7-8 The cloud fraction (f_cl) and the cloud top pressure (p_c) are retrieved, not the cloud top height.

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