Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-735-AC1, 2017

Atmospheric Chemistry and Physics
Open Access
Discussions

Interactive comment on "Validation of OMI, GOME-2A and GOME-2B tropospheric NO_2, SO_2 and HCHO products using MAX-DOAS observations from 2011 to 2014 in Wuxi, China" *by* Yang Wang et al.

Yang Wang et al.

y.wang@mpic.de

Received and published: 15 February 2017

First of all we want to thank this reviewer for the positive assessment of our manuscript and the constructive and helpful suggestions!

General comments: In this paper, Wang et al. presented MAX-DOAS retrievals of NO2, SO2, and HCHO over Wuxi, a city within the heavily polluted Yangtze River Delta region in eastern China. They compared the MAX-DOAS retrievals with various OMI and GOME-2A/2B products. They also investigated the effects of a priori profiles and aerosols on satellite retrievals. The paper presents an interesting study that should be

of interest to satellite trace gas retrieval community, especially the section discussing the effects of vertical profiles and aerosols on retrieval biases. The paper is generally well organized (given the multiple species/products/topics covered) and figures are mostly clear, although some improvement in writing would help. That said, I don't feel that the paper is quite ready for publication in its current form. It is very long with a lot of long, complicated sentences that are not easy to understand. I also feel that some of the 21 multi-panel figures are not completely necessary and can be removed or moved to the supple-mental material. Overall, I'd recommend that the authors try to make the paper more concise and focus more on the key points.

Author reply: Many thanks for the positive assessment! We made four important modifications to the paper. Firstly we moved section 3.1 about the coincidence criteria into supplement as section 1. Secondly we moved the section 2.1.2 about the cloud effects on MAX-DOAS observations into the supplement as section 2. Thirdly we rewrote the discussion on the aerosol effects in section 3.5 of the revised version (see general comment b from Reviewer 2). Fourthly we rewrote the discussion about the influence of the eCF on the shape factor effects on the AMF (section 3.2 of the revised version). This modification is following the specific comment #6.

Specific Comments:

1. The introduction part may be a bit too long and can be shortened.

Author reply: We shortened the introduction by rewriting paragraphs 6 and 7 of the introduction section.

2. The authors used the entire section 3.1 and several figures (Fig. 2-6) in the main text to introduce how temporal/spatial averaging is done to match MAX-DOAS data with satellite data for the comparison. To me, such a lengthy discussion would be justified if the data averaging time and/or spatial averaging radius could be used for other validation/comparison studies. But I doubt that would be the case, given the location of the site and the inhomogeneous surface properties and trace gas loading over the

area (Fig. 1). I feel that this section is probably best included in the supplementary material.

Author reply: Thanks for the suggestion! We followed your suggestion to move the entire section 3.1 into the supplement. And we added a new paragraph in the beginning of section 3 to describe the main conclusions about the coincident criteria.

3. How was eCF calculated? And how was daily mean satellite VCDs calculated? Did the authors consider the size of the each satellite ground footprint?

Author reply: The effective cloud fraction (eCF) is defined as in Stammes et al. (2008). We added this reference to section 2.2 of the revised version. We directly extract eCF data from the published operational products. We clarified the calculation of the daily and bi-monthly mean satellite VCDs in the beginning of section 3 as "Here it needs to be clarified that the daily and bi-monthly averaged satellite data are the averaged values of all satellite pixels located in the coincidence area around the measurement site (see below). The averaged MAX-DOAS data are the averaged values for all measurements within 2 hours around the satellite overpass time". Note that for the selection of the satellite data we don't explicitly use the size of the satellite ground footprint. Instead we use the distance of the center of the pixel to the measurement station. However, we chose distances according to the size of the footprint sizes and the expected gradients of the trace gases. Note that we also exclude the outermost pixels of the OMI swath (i.e. pixel numbers 1–5 and 56–60) as described at the end of section 2.2.

4. Section 3.2: the NASA SO2 product essentially uses the same AMF for all pixels (regardless of viewing geometry and other conditions) that may lead to additional errors and affect its correlation with MAX-DOAS retrievals.

Author reply: Thanks for pointing out this important information! We agree that this simplified assumption might be part of the reason for the worse correlation of the NASA OMI product with MAX-DOAS compared to the BIRA product. We added this information in section 2.2 of the revised manuscript as "A fixed surface albedo (0.05), surface

pressure (1013.25 hPa), solar zenith angle (30°) and viewing zenith angle (0°) as well as a fixed climatological SO2 profile over the summertime eastern U.S. are assumed in the PCA retrieval (Krotkov et al., 2008).". It needs to be noted that the significantly worse R2 for the OMI NASA product compared to the OMI BIRA product could partly be attributed to the assumed fixed measurement condition (and thus the fixed AMF) in the NASA PCA retrievals. However the similar slopes and MRDs between the two OMI products indicate that the simplification of the NASA PCA retrieval only slightly contributes to the systematic bias of the averaged values."

5, Section 3.2: Fig. 8, 10, 13, these figures may be replaced with a table.

Author reply: Since figures represent the most direct way to show the dependence of the consistency on the eCF, we prefer to keep these figures.

6. Section 3.3: Fig. 14 – one would expect that the AMF calculated from TM4 profile would be on average smaller than AMF calculated using MAX-DOAS profile? Note that Fig. 14e and Fig. 15e show the same sign in AMF difference. The TM4 shape in Fig. 14a shows larger weight than MAX-DOAS in the lowest part of the profile, the IMAGES profile in Fig 15a, on the other hand, shows smaller weight than the MAX-DOAS profile in the in lowest layers.

Author reply: Great thanks for pointing out this problem. The reviewer is correct with his/her description of the effect of the shape factor on the AMF. However because of the missing information on the shape factor above 4 km (which was not explicitly mentioned in the original manuscript) the SF effect on the AMF was not correctly explained in the original manuscript. In the revised version (in section 3.2) we firstly clarified how we treat the concentration of TGs above 4km as "It needs to be noted that only the profiles below 4km can be reliably drawn from MAX-DOAS observations. Thus the profile_M between 4km and the tropopause (a fixed value of 16 km is used in this study) are derived from the corresponding CTM profiles of the individual satellite data sets. Therefore the SF_M is derived from the combined profile_M using Eq.3.". We also modified the explanation about the different effects on the AMF under different cloud conditions. For details, please see the revised manuscript. In addition, in part 4 of section 3.2, we also point out that the lack of information about the profiles above 4km from MAX-DOAS observations is a potential error source in the analysis of SF effects on satellite AMF calculations.

7. Fig. 18, the authors may want to point out that GOME-2/OMI ratio for NO2 may be much more meaningful than that for SO2, given the overall smaller retrieval uncertainty.

Author reply: Thanks for pointing out this issue! We modified the description in section 3.4 in the revised version as "For NO2, the RatioSat for both GOME-2 instruments show good agreement. Good agreement is also found for the seasonal variation with the MAX-DOAS results, but the absolute values differ. The systematic difference of RatioSat and RatioM-D can be attributed to the known overestimation of the GOME-2 A/B tropospheric VCD compared to the MAX-DOAS results (see Fig. 12a). This finding also indicates that using GOME-2 and OMI data can lead to wrong conclusions about the diurnal cycles of NO2. Also for the other trace gases we investigated the ratios between the different data sets. However, because of the larger uncertainties compared to NO2, the conclusions for SO2 and HCHO should be treated with care. For SO2, although RatioSat shows several deviations from RatioM-D., RatioM-D and RatioSat are consistent on average and close to unity during a whole year indicating similar SO2 VCDs around the overpass times of GOME-2 and OMI. For HCHO, on average good agreement between RatioSat and RatioM-D is found for GOME-2A and GOME-2B (except some outliers of RatioSat). Interestingly, both RatioSat and RatioM-D are below unity indicating lower HCHO VCDs in the morning than in the afternoon. ".

8. Fig. 19: did the authors use MAX-DOAS profiles to correct for retrievals to isolate aerosols as a source of error in satellite retrievals?

Author reply: Yes, the OMI data used in Fig. 19 (Fig. 14 in the revised version) are the

modified VCD using MAX-DOAS profiles. Thanks for pointing out the missing information. We clarified it in section 3.5 as "It needs to be noted that the OMI VCDs used in Fig. 14 are the modified values using the SFs derived from MAX-DOAS observations in order to isolate the aerosol effects.".

9. Fig. 20 and section 3.6: can the authors specify the aerosol optical properties and size distribution assumed in the RTM calculations? Particularly, for the UV wavelengths especially 319 nm?

Author reply: We modified the description of aerosol properties used in the RTM simulations in section 3.5 of the revised version as "The aerosol optical properties (single scattering albedo of 0.9, asymmetry parameter of 0.72, and Angstroem parameter of 0.85) are taken from the AERONET observations at the nearby Taihu station (Holben et al. 1998, 2001)." .

10. Section 4: instead of simply repeating the results already presented in the paper, the authors may consider condensing this part or provide some more in-depth discussion.

Author reply: We made some modification in conclusion part to improve the discussion.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-735, 2016.