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Atmospheric Chemistry and Physics Discussions

Interactive comment on "MAX-DOAS measurements of tropospheric NO_2 and HCHO in Nanjing and the comparison to OMI observations" *by* Ka Lok Chan et al.

Ka Lok Chan et al.

ka.chan@dlr.de

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We thank reviewer #2 for the useful comments. These comments are helpful for improving our manuscript. We understand that the comments on the scientific content of the manuscript in general are positive, however, several clarifications are necessary. We have addressed the reviewer's comments on a point to point basis as below for consideration. All page and line numbers are refer to the marked-up version of the manuscript.

The manuscript entitled 'MAX-DOAS measurements of tropospheric NO2 and HCHO in Nanjing and the comparison to OMI observations' by Chan et al. presented a long term

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Discussion paper

MAX-DOAS observations of atmospheric nitrogen dioxide (NO2) and formaldehyde (HCHO) in Nanjing. The MAX-DOAS measurements were validated by comparing to sun-photometer observations. The authors then used the MAX-DOAS data for the validation of the NASA's OMI NO2 and HCHO products. OMI observations in general show a good agreement with the ground based observations. A discussion of a priori profile on the satellite retrieval is also presented. The MAX-DOAS data is also used for the investigation of regional transportation of pollutants and for the assessment of air quality during the Youth Olympic Game in Nanjing. The study is in general well written and scientifically interesting for the community. Therefore, I recommend publishing the manuscript after addressed the following minor comments.

Minor comments:

1. Although the agreement between OMI and MAX-DOAS HCHO observations is already very good, it is still interesting to see the effect of MAX-DOAS profiles being used for OMI HCHO VCDs retrieval. I understand that there is a large fraction of HCHO above the MAX-DOAS retrieval height compared to NO2, using the MAX-DOAS profile would result in a larger OMI HCHO columns. This is also relevant for other MAX-DOAS satellite comparison studies.

Response: Following the reviewer's comment, we have added the OMI HCHO VCDs retrieved with MAX-DOAS measurements as a priori for comparison. The results are indicated in Figure 5. A more detailed discussion is added to the manuscript (page 14, line 19-26).

2. The authors mentioned on page 11 that it is difficult to quantify the effect of spatial inhomogeneity of NO2 on the satellite data comparison due to lack of high spatial resolution data. I am wonder if the MAX-DOAS is still measuring? If yes, then it would be useful to compare the latest measurement to TROPOMI observations. As TROPOMI provides much higher spatial resolution data, these datasets are very useful for the quantification spatial gradient effect on satellite to ground based measurement

comparison.

Response: Unfortunately, the MAX-DOAS measurements only cover a period from April 2013 to March 2017 while TROPOMI was launched in October 2017. The MAX-DOAS is under maintenance after March 2017 till now. Therefore, there is no overlap between the MAX-DOAS and TROPOMI data. We will try to the compare the MAX-DOAS data to TROPOMI observations when the MAX-DOAS is back online.

3. The reconstruction of spatial distribution NO2 and HCHO from MAX-DOAS measurements using trajectories simulations are particularly interesting. However, the description of the method is a bit too brief. The author should include a more detail description.

Response: We have supplemented a more detailed description of the spatial reconstruction procedure (page 16, line 2-8).

4. In addition, do the authors try other lifetime weight factors in this study? Are the weighting factors determined by fitting to satellite data or the authors just select a realistic one? This can be relevant for other similar studies.

Response: The assumed lifetime is only used for the calculation of the weighting factor and the weighting factor is only useful when multiple trajectories overlapping with each other within a single grid point. We have tried different lifetime and backward time to reconstruct the spatial distribution map of NO2 and HCHO. As expected maps created with shorter backward time correlates better with OMI observations, but then the spatial coverage are very limited. In order to get a balance between having better spatial coverage and the reliability of the reconstructed pollution maps, these number are used in this study. This information is now added in the manuscript (page 17, line 7-8, page 18, line 1).

5. Putting Figure 5 and Figure 7 a and b on the same page (or same figure) would be much easier for the readers to see the agreement between the reconstructed maps

and satellite observations. Same comment applies to Figure 6 and Figure 7 c and d.

Response: We followed the reviewer's comment and combined Figure 5 and Figure 7 a and b as a single figure. Same procedure is also applied to Figure 6 and Figure 7 c and d.

6. Regarding to the assessments of air quality during Youth Olympic, it would be better to show some meteorological parameters during the 3 periods.

Response: We have added the meteorological measurements such as temperature, wind speed and wind direction to support the discussion. The meteorological data are shown in Figure 10. Description of the meteorological data can be found in section 2.3.

Technical comments:

1. Page 8, line 27: 'pNO2' should be 'NO2'

Response: We have corrected the typo.

2. Page14, line 12: 'This agree well with the fact' should be 'This agrees well with the fact'

Response: We have corrected the grammatical mistake.

3. There might still be other typos and errors in the manuscript. Please check the entire manuscript carefully.

Response: We have proofread the manuscript carefully to avoid any typo and error.

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