

Interactive comment on “MAX-DOAS measurements of tropospheric NO₂ and HCHO in Nanjing and the comparison to OMI observations” by Ka Lok Chan et al.

Anonymous Referee #2

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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 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

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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. 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. 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. 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. 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. 6. Regarding to the assessments of air quality during Youth Olympic, it would be better to show some meteorological parameters during the 3 periods. Technical comments: 1. Page 8, line 27: 'pNO₂' should be 'NO₂' 2. Page 14, line 12: 'This agree well with the fact' should be 'This agrees well with the fact' 3. There might still be other typos and errors in the manuscript. Please check the entire manuscript carefully.

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