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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.

Anonymous Referee #1

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General comments In this paper, Chan et al. presented the long-term MAX-DOAS measurements of NO2 and HCHO profiles in Nanjing. The data are used to validate OMI NO2 and HCHO products, discuss the effects of a-priori profiles on OMI retrievals, analyze effects of regional transports, and effects of pollution control measures during the Youth Olympic Games. In general the scientific topic is meaningful, and the MAX-DOAS data quality is well proved. However the authors need to give more deep discussions in many parts to firmly prove the conclusions. Generally I have three major concerns below:

1) Regarding the comparisons with the OMI data in section 3.2, the authors should also

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show the comparisons of a-priori profile shapes of OMI NO2 retrievals with the NO2 profiles measured by MAX-DOAS for the discussion on the effect of a-priori profiles. In addition, the authors also need to discuss the a-priori effect for HCHO even in the case that a good agreement is found. It is very important to see if good agreements of HCHO profile shapes can been also seen between MAX-DOAS and OMI a-priori.

The underestimation of OMI NO2 VCDs is up to 50% compared to MAX-DOAS data shown in Fig. 2. However the previous study in Wuxi, see Wang et al. 2017, shows the underestimation is ∼20%. One major difference is that the NO2 product is from NASA in your study, but from DOMINO v2 in Wang et al., 2017. Are there big differences of both OMI NO2 data sets? Why are there big differences? In order to answer the questions, the author needs to do comparisons also with the DOMINO v2 product. Meanwhile DOMINO v2 is an official product which is well known and widely used. In addition the author demonstrates that "Measurements with such large spatial coverage are probably difficult to capture the spatial gradient of NO2 and resulted in an underestimation over pollution hot spots due to the averaging of large OMI footprint. This effect is especially significant over Nanjing, as it is a local pollution hot spot surrounded by rather clean areas". If it is true, the NO2 measured by the MAX-DOAS is dominated by local emission. However the discussion on regional pollution transport in section 3.4, the author concludes that "the air quality of Nanjing is significantly influenced by the air pollution transportation, especially during winter.". The two elaborations are contradictive. Therefore the authors need to carefully discuss the reason of the underestimations of the OMI NO2 data.

2) In section 3.4, the authors used the reconstructed maps to quantitatively validate the satellite maps. Therefore a speculated life time is used to scale MAX-DOAS VCD in the reconstruction of maps. However are the quantitative comparisons reasonable? Because the authors assume that all the pollutants measured by the MAX-DOAS instrument are from emissions in an area corresponding to the starting location of a trajectory. However emissions in different grids along the trajectory route should be

mixed up and contribute to the pollutants measured by the MAX-DOAS in reality. The emissions from different distances should be scaled differently. But do we know the proportions of the different emissions? I think the reasonable comparisons of the reconstructed maps with the satellite maps are the relative distributions, but not the absolute values.

The reconstructed maps should depend on the selected backward time of trajectories. The author should show the maps with different trajectory backward time and compare them with the satellite maps in order to see which time is reasonable. And the suitable backward time depends on actual lifetime. Since the lifetime effect is already implied if different backward time is tested, the scaling with lifetime might be not needed and do not give any meaningful results. In addition, transports of pollutants can occur during night time and day time. Lifetime only matters for transports during day time. Night time transports can reach a far distance and contribute to concentrations of pollutants during day time. This is another reason why lifetime should not be applied.

The backward propagation method has been applied to long lifetime pollutants and also trace gases measured from MAX-DOAS in previous studies. Some references should be cited in the paper. Meanwhile the sentence “We developed a new technique to assemble the source contribution map using backward trajectory analysis” in the abstract might be inappropriate.

3) In section 3.5, the author compared the pollutants during the Youth Olympic Games with those before and after the event in order to characterize the effect of pollution control measures. Since pollution transports can impact Nanjing as the author demonstrates in section 3.4, the difference of transport conditions in the three periods should also be discussed. Meanwhile the author simply elaborates “As the meteorological conditions are very similar during the three periods”. I think the author has to show wind fields, trajectories, precipitations, and temperatures in the three periods in order to convince the readers. Near-surface concentrations of the pollutants should be also derived from the MAX-DOAS profile inversion. Since near-surface concentrations

should be mainly dominated by the local emission, but VCDs (AODs) contain contributions of pollutant transports. Therefore it is also meaningful to include the comparisons of the surface concentrations as VCDs shown in Fig.9.

Specific Comments:

1) P4 L3: A reference should be given for QDOAS. Please clarify which of the two wavelength ranges is used for NO2 and HCHO?

2) P4 L4: Please clarify the reference spectrum is the zenith measurement in individual elevation scan or around noon time?

3) Table 1: Do you determine the DOAS fit settings based on sensitivity studies (which are not shown) or previous studies? If you determined them based on previous studies, some references should be given. In addition, do you apply the wavelength dependent Ring suggested by Wagner et al., 2009? If not, please discuss why the additional Ring is not needed in your analysis.

Wagner, T., Beirle, S., and Deutschmann, T.: Three-dimensional simulation of the Ring effect in observations of scattered sun light using Monte Carlo radiative transfer models, Atmos. Meas. Tech., 2, 113-124, 2009.

4) Section 2.1.2: examples of DOAS fits should be shown, especially for HCHO, in order to convince the quality of HCHO analysis.

5) P4 L17-19: How do you filter the data under continuous clouds when the variability of O4 dSCDs are not large?

6) P5 L15: Since O4 VCD can systematically vary during a year due to variations of temperature and pressure, as Wagner et al. (2018 AMT) demonstrated, the phenomenon can explain the scaling factor in many places. How do you consider the variation of temperatures in the retrievals of aerosols? If you don't consider it, a discussion on the uncertainties due to the effect has to be given.

7) P6 L11: How do you determine the single scattering albedo and asymmetry parameters, and also Ångström coefficien? The parameters can significantly change in the long-term measurements, uncertainty estimations of aerosol results due to the parameters should be given in the paper.

8) P6 L16-18: How do you determine the wavelengths of the AMF simulations of O4, NO2, and HCHO?

9) P6 L20: How do you deal with the NO2 above 3km? The considerable amount of NO2 at high altitudes might also impact retrievals of NO2 below 3km.

10) Section 2.1.3: Figures of comparisons of measured dSCDs and modeled dSCDs for profile retrievals should be shown in the manuscript or supplement to show the convergence of the profile retrievals.

11) Section 2.3: The overpass time of OMI should be given.

12) P8 L13-14: Can the constraint of a-priori profile contribute to the underestimations? In order to show this, comparisons of measured dSCDs and modeled dSCDs are needed.

13) P8 L28: As I know, there are not domestic heating systems in Nanjing since it is in the south of Huai River.

14) P10, L1: The underestimation of OMI NO2 compared to MAX-DOAS is not consistent with Wang et al., 2017. The underestimation here is much stronger.

15) P13, L6: Oxidation rate of VOCs to HCHO is also stronger in summer than in winter. The variations of oxidation rate can also contribute to seasonal pattern of HCHO. And secondary sources of HCHO are significant. The seasonal pattern of HCHO might be due to contributions of biogenic emissions of precursor VOCs. The sentence should be modified.

16) P13, Figure 5: The color scale of subfigure (a) should be changed to allow see-

ing the gradient more clearly. As I elaborated in General comment (2), the relative distribution is much more important than the absolute values.

17) P13, L16: Since you calculate the trajectories in each altitude grids of MAX-DOAS profiles, how do you combine the trajectories with the profiles? Do you assign partial columns in each vertical grid to different grid points in the map along trajectories at individual altitudes? The procedure need to be clarified.

18) P14, L5-6 how do you determine the lifetime and backward time? The question is corresponding to the general comment (2).

19) P14, L12: As I demonstrate in comment (2), the quantitative comparisons with OMI data are not reasonable.

20) P14, L19: Since HCHO is dominated by the secondary formations from VOCs, which have a long lifetime, therefore VOCs might be transported to a far distance and contribute to local HCHO concentrations. Therefore transport effects on HCHO might be even larger if the transports are from far distance. The backward time of trajectories of 6 hour might be not long enough in the reconstruction of HCHO maps. Following my general comment (2), I suggest you to generate the maps with different backward time of trajectories and compare the relative distributions with OMI maps.

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