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Atmospheric Measurement Techniques Discussions

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

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Response to reviewer #1

We thank reviewer #1 for the time to carefully reading the manuscript and providing useful comments. We understand that these comments are positive on the scientific content of the manuscript while appropriate revisions and 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 refer to the marked-up version of the manuscript.

General comments:

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Discussion paper

The manuscript by Chan et al. presents a comparison work for satellite-based and ground-based NO2 and HCHO measured in Munich. The work also evaluated the horizontal distributions of NO2 and HCHO measured with different azimuth angles. The comparison process is accurate and comprehensive. Some of the findings are important and valuable to the research community. For example, using MAX-DOAS NO2 profiles as a priori, the author recomputed OMI and TROPOMI NO2 VCDs. This quantified influence of a priori NO2profilesin the satellite retrieval is interesting (i.e., the low-spatial-resolution a priori in original satellite data vs. MAX-DOAS derived a priori). The manuscript is well-written and should be published after addressing the following comments.

Specific comments:

P5 L12 to P6 L2. I think the O4 scaling factor is still an interesting open question to the DOAS community. I am not challenging the validity of the O4 scaling factor in this work (i.e., should or should not use O4 scaling), but I feel the author's description is a bit misleading. I.e., one should at least mention those works (including Spinei et al., 2015; Wagner et al., 2019) that did not find it necessary to apply a scaling factor to bring model simulations and measurements into an agreement.

Response: We followed the reviewer's comment and added the references and descriptions to studies which do not require any correction to bring observation and simulation together (page 4, line 12-13).

P7 L23-24. Please provide a quantitative description of the small effect of the radiative transfer simulation of O4.

Response: We have added the quantitative value for the surface albedo effect on the simulation of O4 DSCDs (page 7, line 29 to page 8, line 2).

P10 L18. I think for this research work, a localized pixel-averaging map from TROPOMI is more useful than the map over Germany. For example, NO2/HCHO map over Munich

and surroundings might show more details of distribution features, i.e., whether there areany NO2/HCHO hotspots near the MAX-DOAS site.

Response: In addition to the spatial distribution maps of NO2 and HCHO over Germany, we have also supplemented zoomed in maps of Munich and its surrounding areas. Major hotspots, e.g., power plant and airports, also marked on the maps (see figure 3).

P11 L1-3 and L11-12. Without a good local map (masked with TROPOMI NO2/HCHO), it is difficult for the reader to understand where are these emission sources (or hot spots), relative to the observation site. One should consider plot TROPOMI NO2/HCHO (annual mean) masked over a map similar to Figure 1(should be larger than Fig. 1, e.g., 50 km $\times$50 km). Also, proper labels (larger) for the discussed sources should be included, i.e., it is impossible to find where is the "English Garden", or "natural gas power plant" on Figure 1.

Response: See the response above. In addition, a city map is also included (figure 3e).

P12 L1-2. Since the y-axis for the four panels in Fig. 4 is very different, I am not sure the argument here is valid, i.e., the HCHO peak in the south and south-west during summer is less pronounced. The absolute values from these two directions are about twice the corresponding values in the winter. Anyway, my point is the background level HCHO is different from winter to summer. Thus, to reveal the spatial distribution changes, one may needs to remove the background signal (e.g., mean HCHO or 5th to 10th percentile HCHO for each season). Also, given the very large error bars (1 std of HCHO), even after removing the background signal, I am not sure we can say the spatial variations from winter to summer is statistically significant.

Response: The original idea of using separated plots for summer and winter time data is to show there is a big difference of the background value between summer and winter. We now followed the reviewer's comment and show normalized plots for

measurements at different azimuth angles by dividing the mean value.

Regarding to the comment of large error bars, the error bars are the 1σ standard deviation which represents the natural variation of the measurements, e.g., diurnal variation. These variations will not decrease even if we average large amounts of data, while the errors of the measurement values are very small as it is an average of a large number of data. As the errors are too small to be visible in the plots, therefore, we decided to show the 1σ standard deviation instead. We have further clarified this point in the manuscript (page 12, line 1-5).

P13 L6-8. I fully agree with the author that the biogenic emission from plants contributed to most of the signals shown in Fig. 5. But, is this possible to further separate the sources by divide the data into summer and winter periods? I guess in the winter HCHO dataset, one may see a better day of week variability. Any comments?

Response: We followed the reviewer's comment and separated the day of week analysis into winter and summer periods. For NO2, a more significant weekend reduction can be observed in summer, which is due to shorter atmospheric lifetime and less accumulation from weekdays. For HCHO, the weekly pattern is much less pronounced during winter (no weekend reduction can be observed). In winter, HCHO levels observed on Sunday are even slightly higher than that of the weekday average. The anthropogenic sources of HCHO in the troposphere include the oxidation of various long lifetime VOCs, such as, methane. Their lifetimes are even longer in winter and therefore result in a less significant weekly pattern. This information is included in the revised manuscript (page 13, line 13-30).

P13 L17-18. Please provide the calculated aerosol extinction to NO2 ratios.

Response: We have supplemented the aerosol extinction to NO2 ratios for both summer and winter in the manuscript (page 14, line 7 to page 15, line 1).

P14 L2. Please provide the calculated aerosol extinction to HCHO ratios.

Response: We have supplemented the aerosol extinction to HCHO ratios for both summer and winter in the manuscript (page 15, line 3-4).

P14 L9. Which model is used in the comparison? Please clarify.

Response: The model refers to the multiple linear regression model. We have revised the sentence to avoid confusion (page 15, line 10).

P15 L2-4. Is the εsurf have any horizontal distribution pattern? For example, for the 180 degrees measurements, do we have larger εsurf than other directions (similar to the higher signal of NO2 and HCHO from this azimuth angle)? For example, in Fig. 7b, do you have better/worse correlations for some directions?

Response: As the in-situ monitor station is located northwest of the MAX-DOAS measurement site, MAX-DOAS measurements of aerosol extinction at surface layer with azimuth angle of 315° agree the best with the in-situ data with a correlation coefficient of 0.82. The result indicates the strong spatial variation of aerosols in Munich and a single in-situ monitor is not representative for the general pollution condition in the city. We have supplemented a corresponding statement in the manuscript (page 16, line 19-22).

P15 L22-29. I agree with the author that the sampling height could be one of the major reasons for this large systematic difference (50 %). If the author's hypothesis is correct, i.e., the difference is due to NO2 vertical dispersion, one may see the systematic differences in different atmospheric conditions. For example, data collected around warm local noon (better vertical mixing) should show better agreement between MAX-DOAS surface NO2 and in-situ NO2, and vice versa. Any comments?

Response: Following the reviewer's comment, we have separated the measurements into few categories by meteorological factors, such as, temperature and wind speed, for analysis. However, we do not see any significant improvement of the agreement between the MAX-DOAS and in-situ measurements. In addition, we have linearly ex-

trapolated the MAX-DOAS measurements to near street level (15m a.g.l.) using the lowest two layers of the NO2 vertical profile retrieval. The extrapolated near street level NO2 concentrations are on average only ∼10% higher. However, the discrepancy between MAX-DOAS and in-situ measurements remains quite large. The result indicates a stronger enhancement of NO2 level at near street level compared to the upper part of the mixing layer. The vertical mixing of pollutants in an urban environment is rather complicated. The atmospheric processes are especially complicated in the lowest several tens of meters where pollutants emitted from tail pipes are dispersed to the ambient environment. These processes are strongly dependent on many factors, such as, the urban street configurations, emission characteristics and meteorological factors. Higher spatio-temporal resolution measurements and a proper CFD model are required to better investigate the pollution dispersion effect in urban environment. However, this topic is beyond the scope of this study. A more detailed description and explanation is included in the manuscript (page 16, line 34 to page 17, line 3).

P18 L14-15. It is very nice to see the improvement from TROPOMI NO2 when using MAX-DOAS derived profile as a priori. TM-5 is too coarse and high-spatial-resolution a priori is needed to capture enhanced local NO2 signal. For North America, an hourly regional air quality forecasting model is used to recalculate TROPOMI AMF (Griffin et al., 2019). For Europe, hourly CAMS regional model profiles available at 0.1° resolution will be used in future TROPOMI data (e.g., Zhao et al., 2020). In general, I think these results found in current work look good. But, can the author give some comments on why there is an overestimate from the “OMI corr” point for February 2017?

Response: We have supplemented the references to the recent relevant studies (page 20, line 13-14). For the OMI measurement on Feb 2017 exceeding the MAX-DOAS value, this is mainly because there are only three valid OMI measurements during the month due to cloudiness and row anomaly issue while the MAX-DOAS has 20 valid measurements in Feb 2017. We have supplemented this explanation in the manuscript (page 19, line 14-15).

Technical corrections:

P4 L9: Move the definition of DSCD to here.

Response: Done.

P4 L9: Move the full name of O4 (oxygen collision complex) to here.

Response: Done.

P8 L10: Define ΔSCDij, ΔSCD zenithj, and Δzj. Figs.

Response: Done.

7b, 7d, and 8b. If these are colour coded density plots, please include proper colour bars.

Response: Done.

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