General comments:

The manuscript by Chan et al. presents a comparison work for satellite-based and ground-based NO_2 and HCHO measured in Munich. The work also evaluated the horizontal distributions of NO_2 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 NO_2 profiles as a priori, the author recomputed OMI and TROPOMI NO_2 VCDs. This quantified influence of a priori NO_2 profiles in 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 O_4 scaling factor is still an interesting open question to the DOAS community. I am not challenging the validity of the O_4 scaling factor in this work (i.e., should or should not use O_4 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.

P7 L23-24. Please provide a quantitative description of the small effect of the radiative transfer simulation of O_4.

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, NO_2/HCHO map over Munich and surroundings might show more details of distribution features, i.e., whether there are any NO_2/HCHO hotspots near the MAX-DOAS site.

P11 L1-3 and L11-12. Without a good local map (masked with TROPOMI NO_2/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 NO_2/HCHO (annual mean) masked over a map similar to Figure 1 (should be larger than Fig. 1, e.g., 50 km × 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.

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 5^{th} to 10^{th} 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.

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?

P13 L17-18. Please provide the calculated aerosol extinction to NO_2 ratios.

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

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

P15 L2-4. Is the ε_{surf} have any horizontal distribution pattern? For example, for the 180 degrees measurements, do we have larger ε_{surf} than other directions (similar to the higher signal of NO_2 and HCHO from this azimuth angle)? For example, in Fig. 7b, do you have better/worse correlations for some directions?

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 NO_2 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 NO_2 and in-situ NO_2, and vice versa. Any comments?

P18 L14-15. It is very nice to see the improvement from TROPOMI NO_2 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 NO_2 signal. For North America, an hourly regional air quality forecasting model is used to re-calculate 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?

Technical corrections:

P4 L9: Move the definition of DSCD to here.

P4 L9: Move the full name of O_4 (oxygen collision complex) to here.

P8 L10: Define ΔSCD_{ij}, $\Delta SCD_{zenithj}$, and Δz_j.

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

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