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Comment on acp-2022-25

Anonymous Referee #1

Referee comment on "Multi-axis differential optical absorption spectroscopy (MAX-DOAS) observations of formaldehyde and nitrogen dioxide at three sites in Asia and comparison with the global chemistry transport model CHASER" by Hossain Mohammed Syedul Hoque et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-25-RC1, 2022

In this paper, formaldehyde (HCHO) and nitrogen dioxide (NO2) vertical profiles are retrieved from MAX-DOAS observations at three sites in Asia from January 2017 through December 2018. The three sites are Phimai in Thailand, Pantnagar in India, and Chiba in Japan. They correspond to rural, semi-urban, and urban conditions, respectively. The NO2 and HCHO concentrations in the 0-4km altitude range show consistent seasonal variations throughout the investigated period, which are interpreted in terms of dry and wet seasons and, in the case of Phimai and Pantnagar, biomass burning episodes. The HCHO to NO2 concentration ratios together with MAX-DOAS ozone retrieval results are also used to infer the ozone sensitivity to NOx and VOCs at the three sites. It is found that reasonable estimates of transition regions between the NOX-limited and VOC-limited ozone production regimes can be derived when the NO2-HCHO chemical feedback is accounted for.

In the second part of the study, the MAX-DOAS observations of NO2 and HCHO are used to assess the CHASER global CTM at the three sites. CHASER shows reasonably good performances in reproducing the abundances of both trace gases in Phimai and Pantnagar but not in Chiba. Comparison results are interpreted in terms of model resolution, emission inventories, and contributions of the different emission sources.

This study fits with the scope of ACP. However, there are a lot of aspects of the work that should be further clarified and/or discussed prior to final publication. Those aspects are detailed below. Moreover, as already raised during the quick review, the overall presentation quality is questionable, largely due to the poor English language used throughout the manuscript but also to repeated errors in the axes and title labellings of several figures. This should be improved in the revised version of the paper.

Important specific comments:

*Lines 225-235: The VCD retrieval is based on several assumptions that are poorly discussed and justified. For instance, did you check that the dependence of the Abox profiles on the trace gas concentration profiles is indeed minimal? Did you test other a priori VCD values? How valid is assuming an Angstrom exponent value of 1.00?

*Lines 234-236: You should describe how these averaging kernels are calculated. Looking at Figure 3, HCHO and NO2 VCD averaging kernels seem to be close to unity but it is not the case for f1 averaging kernels, and especially f2 and f3 averaging kernels which are close zero. Does it mean that you can basically retrieve only the VCDs from your measurements and that for f1, f2, and f3 the retrieval essentially reproduces the a priori? Also, are similar averaging kernels obtained for the two other stations? These points should be further discussed in the revised manuscript.

*Line 284: Anthropogenic emissions used in the CHASER model were based on the HTAP_v2.2 2008 inventory. Why didn't you use more recent inventories like the REAS v3 one (see https://acp.copernicus.org/articles/20/12761/2020/acp-20-12761-2020.pdf)? How can it affect the results and conclusions of your study, especially for Pantnagar and Chiba?

*Figure 9: How do you explain such a large effect when model profiles are smoothed with the MAX-DOAS averaging kernels, especially in the altitude range (0-2km) where the MAX-DOAS retrievals have a maximum of sensitivity.

*Figure 10 and related discussion: What would be also interesting to show are model profiles at both 2.8°x2.8° and 1.4°x1.4° resolution smoothed by the MAX-DOAS AVK. I think only this comparison allows to discuss quantitatively the effect of the model resolution on the CHASER/MAX-DOAS agreement. Since the 2.8°x2.8° and 1.4°x1.4° model profiles have a significantly different shape, we can expect a different impact when those profiles are smoothed with the AVKs.

*Section 3.2.3: Given the coarse horizontal resolution of the CHASER model (2.8°x2.8°), how valid is the assessment of NO2 and HCHO from this model for the Pantnagar station which is located in a region (Himalayan foothills) with highly varying topography? I would suggest to remove Pantnagar from the model evaluation since the topography is not properly taken into account in your analysis.

Minor comments:

*Line 125: You should indicate here which types of industries are located in the Pantnagar region.

*Lines 151-152: The use of the 70°EL instead of the 90°EL for the reference spectra should be better justified. How the use of 70°EL (instead of 90°EL) can minimize variations in the measured signals. Also what do you mean by 'variations in the measured signals'?

*Line 196: You should give here the AEC value at 100km you used, as well as the scaling height of your exponentially decreasing a priori profile.

*Lines 201-203: The parameterization of Irie et al. (2008a) does not provide information on the vertical resolution and measurement sensitivity. Then it is said that 'The retrievals and simulations conducted by other groups for similar geometries (i.e., Frieß et al., 2006) are used to overcome such limitations'. I don't understand this latter sentence. Do you mean that you used previous studies based on the optimal estimation method to estimate the vertical resolution and sensitivity of your own parameterized retrieval? Could you please clarify?

*Lines 206-208: You should describe in a table the settings (pressure and temperature profiles, wavelength, surface albedo, etc) you used for the calculation of your box air mass factors LUT.

*Lines 244-245: For the estimation of the systematic errors, uncertainties of 30% and 50% on the retrieved AOD are assumed. Where these uncertainty values come from?

*Lines 246-247: Did you try to estimate the presence of an EL bias e.g. by performing horizon scans on a regular basis?

*Lines 252-254: The criteria used for the cloud screening should be justified. How do you determine them?

*Lines 289-290: Where these emission values come from? References or justification are needed here.

*Lines 397-398: In Figure 5, only the O3 concentrations for SZA < 50° are used to minimize stratospheric effects. Does it mean that only HCHO and NO2 data corresponding to SZA lower than 50° have been selected for these plots? If not, this means that HCHO and NO2 retrieval results does not timely coincide with the O3 concentrations. This point should be clarified.

*Line 398: It is stated that the JM2 O3 product showed good agreement with ozonesonde measurements. Has such verification been done at the three stations involved in the present study? Also, the Irie et al. (2021) reference is missing in the list.

*Figure 7(a): Even if they both correspond to high O3 concentration conditions, I am surprised to see that the Rfn vertical profiles at Phimai and Pantnagar have both the same shape. Could you comment on this point? Also, why the Rfn vertical profiles from the CHASER model are not included in Figures 7(a) and (b)?

*Section 3.2.1: I think it would be useful to show the seasonally-averaged MAX-DOAS AVK corresponding to the climate classifications of each site in the Supplement. This would support the discussion here.

*Figure 8: given the very large error bars on the MAX-DOAS vertical profiles, I think it is important to say that the CHASER with AK – MAX-DOAS differences are not statistically significant.

*Section 3.2.3: Why no CHASER versus MAX-DOAS profile comparisons are shown for NO2 and HCHO for Pantnagar? This is not consistent to what is presented at the Phimai and Chiba stations.

*Line 744: Is it 1.1° or 1.4°?

Technical corrections:

*Line 24: 'variation' -> 'variations'

*Line 29: 'good performances reproducing' -> 'good performances in reproducing'

*Line 48; 'the lifetime' -> 'the lifetime of HCHO'

*Line 78: 'satellite retrieval' -> 'satellite data retrievals'

*Lines 97-98: 'in three atmospheric environments' -> 'in three different atmospheric

environments'.

*Figure 1, page 6: I would use 'concentration' instead of 'concentrations' in the legend of the color bar.

*Line 144: 'campaign' -> 'campaigns'

*Line 147: 'consist' -> 'consists'

*Line 164: 'following equation.' -> 'following equation:'

*Lines 174-175: 'cross section data' -> 'cross section data sources'

*Line 181: 'using the optimal estimation method (Irie et al., 2008a; Rogers, 2000)' -> 'using the approach developed by Irie at al. (2008a) which is based on the optimal estimation method (Rogers, 2000).'

*Line 182: 'In this approach, the measurement vector y….are defined as'

*Line 188: 'window' -> 'windows'

*Line 192: 'compromise' -> 'includes'

*Figures 5 and 6: It is not clear to me why the y-axis scales of the three plots are not the same in both figures. Please comment. Also, to my opinion, only the transition lines should change between figures 5 and 6, so one unique figure including the three transition lines should be fine.

*Line 458: 'clarify' -> 'support'

*Page 554: 'imitate' -> 'reproduce'

*Figure 9: ‘HCHO’ should be changed to ‘NO2’ in the x-axis label of all plots.

*Figure 10(b): I guess the blue and green curves should be inverted (green curve should be in blue and the blue curve in green).

*Figure 11: the same x-axis scale should be used in the four plots.

*Line 822: ‘Biogenic’ -> ‘biogenic’

*Legend of Figure 14(b): ‘no anthrpogenic’ -> ‘no anthropogenic’