Response to reviewer and editorial comments

Response to reviewer comments

We thank the referee for the helpful comments. We have revised the manuscript accordingly. The responses (blue fonts) are provided after stating the reviewer comments. Figure, Table, and line numbers correspond to the revised manuscript. The highlighted text are corresponding changes in the revised manuscript.

Major comments:

The authors stated in their reply to Reviewer #1 that they removed Pantnagar from the model evaluation. However, looking at Sections 3.3.3 and 3.3.4, it seems not to be the case. A clarification is needed here.

Response: We thank the reviewer for the comment. For the Pantnagar site, the comparison (profiles and partial columns) between the observations and the simulations close to the measurement site have been removed. In section 3.3.3, HCHO observation are compared qualitatively with the simulations in the IGP region. In section 3.3.4, simulations at two different spatial resolutions for the site are compared. Overall, the point to point comparison for the Pantnagar site has been discarded.

A discussion on the impact of using more recent inventories in the CHASER simulations on the comparison results with OMI is missing and should be added (see also related specific comment below).

Response: We thank the reviewer for the comments. Yes, surely, including such comparison studies will add more merit to the discussions. However, as mentioned earlier, currently we are unable to implement updated inventories in our simulations due to some technical limitations, which we are working to fix. To address this aspect, we have included the following text in the revised manuscript.

L576- 579 It should be noted that simulations based on old NO_x emission inventory will likely affect the model-satellite comparison results. However, the current study has not assessed such impact due to technical issues related to using an updated emission inventory. This issue will be addressed in a separate study.

Specific comments:

Figure 1, page 6: May be replace ‘during June in 2018’ by ‘in June 2018’ in the title?

Response: Appropriate corrections has been included in the revised manuscript.

Page 11, line 213: not clear what you did here. Did you fix your k value at 100km to the mean SAGE III extinction coefficient in the 15-40km altitude range? A clarification is needed here.

Response: Yes. According to Irie et al. (2008, 2011, 2015), such assumptions has negligible impact on the retrieval due to lower sensitivity of the MAX-DOAS observations above 2 km.

Page 11, line 213: may be replace ‘non-substantial’ by ‘negligible’?

Response: Appropriate corrections has been included in the revised manuscript.

Figure 3, page 13: Do the error bars on the averaging kernels correspond to the standard deviation? This information should be included in the legend of the figure.

Response: The caption of figure 3 has been revised

Page 14, lines 283-284: not clear to me how the cloud screening approach works. More particularly, how can you retrieve information on clouds based on the HCHO and NO2 dSCD residuals? This point should be clarified.

Response: It is known that clouds can bias the retrieved concentrations. While the discrimination between clouds and aerosols is still very challenging, the following data screenings were made to minimize the influence of clouds. First, we filtered output from the retrieval only for retrieved AOD less than 3, the largest value in the LUTs. This excludes large optical depth cases, most of which should be due to optically thick clouds. Further data screening was made using the root-mean squares of residuals of the O_4, NO_2, and HCHO dSCDS. Larger residuals likely occur when constructing a profile is too simple to represent the true profile, particularly with a steep vertical gradient of extinction due to clouds. Also, rapid changes in optical depth within 30 min that corresponds to the full scanning time can lead to larger residuals. The threshold values were determined statistically corresponding to the mode plus one sigma (1σ) in the logarithmic histogram of relative residuals. The following sentences has been included in the revised manuscript.

L282-285: Larger residuals likely occur due to two reasons: (1) when the constructed profile is too simple to represent the true profile, particularly with a steep vertical gradient of

extinction due to clouds, and (2) rapid changes in optical depth within 30 min (time for one complete scan) (Irie et al, 2011).

Page 17, line 253: please remove 'For analysis,' at the beginning of the sentence.

Response: Appropriate corrections has been included in the revised manuscript.

Page 18, line 365: I would replace 'signifying' by 'indicating'.

Response: Appropriate corrections has been included in the revised manuscript.

Page 22, line 461: I would add the following sentence (or something similar): 'This criterion on the SZA is also applied for the selection of the NO2 and HCHO concentrations.

Response: Appropriate corrections has been included in the revised manuscript as follows:

L463: This criterion on the SZA is also applied for the selection of the NO_2 and HCHO concentrations.

Page 22, line 462: In order to avoid confusion, you should mention that the good agreement between the JM2 O3 product and ozonesondes was obtained in a previous study and has not been checked here. I would rephrase the sentence as 'Although not checked here, the JM2 O3 product showed good agreement with ozonesonde measurements in a previous study (Irie et al., 2021).'

Response: Appropriate corrections has been included in the revised manuscript.
L464-465: Although not checked here, the JM2 O_3 product showed good agreement with ozonesonde measurements in Tsukuba (Irie et al., 2021).

Figure 5 (c) and (f), page 23: in order to better distinguish the data points, you could use a y-axis upper limit of about 12 ppbv instead of 20 ppbv?

Response: Figure 5(c) and (f) have been revised.

Page 23, line 489: 'Schroder' -> 'Schroeder'

Response: Appropriate corrections has been included in the revised manuscript.

Page 27, lines 572-574: Given the fact that the comparison OMI versus CHASER is done at a global scale, it is not clear to me why only few days with OMI observations are remaining in July and December after filtering.

Response: We thank the reviewer for the comments. We selected the coincident dates between the simulation and daily observations for every month based on fixed data filtering criteria. Unfortunately, the NO_2 data selection results yielded very few coincident days in July and December, thus discarded from the comparison. The word" coincident" has been added to avoid confusion as follows:

L574-575 The month of July and December were discarded from the NO_2 comparison because few coincident days (only five days) were available after filtering.

Page 27, introductory paragraph on the comparison with OMI (lines 568-574): at the end of this paragraph, I would add a disclaimer about the fact that the comparison results are likely affected by the use of rather old emission inventories in the model simulations. I would then add a Section 3.2.3 with a discussion on the impact of using more recent inventories on the OMI versus CHASER comparison results (a bit like the authors did in Section 3.3.4 for the comparisons at the MAX-DOAS sites).

Response: We thank the reviewer for the comments. Yes, surely, including such comparison studies will add more merit to the discussions. However, as mentioned earlier, currently we are unable to implement updated inventories in our simulations due to some technical limitations, which we are working to fix. To address this aspect, we have included now the following text in the revised manuscript.

L576- 579 It should be noted that simulations based on old NO_x emission inventory will likely affect the model-satellite comparison results. However, the current study has not assessed such impact due to technical issues related to using an updated emission inventory. This issue will be addressed in a separate study.

Page 27, line 582: 'The spatial representativeness between…' -> 'The difference in spatial representativeness between…'; 'observation' -> 'observations'; 'one potential reasons' -> 'one potential reason'

Response: Appropriate corrections has been included in the revised manuscript.

Page 27, line 583: I don't understand why the word 'however' is used here. The fact that the CHASER simulations at 1.1° improve the MBE and RMSE is a further indication that the difference in spatial representativeness between the model and observations is one potential reason for the observed negative bias.

Response: The word “however” has been removed.

Page 28, line 592: the second ‘although’ in the sentence should be removed.

Response: Appropriate corrections has been included in the revised manuscript.

Figure 7, page 29: Why the CHASER NO2 and HCHO maps are not shown in the figure?

Response: Model simulations have been included in Figure 7.

Page 32, line 656: It should be ‘Figure 9’ instead of ‘Figure 7’.

Response: Appropriate corrections has been included in the revised manuscript.

Page 38, lines 795-796: I would start the sentence as follows (or something similar): ‘Overall, given the large uncertainty on the MAX-DOAS profiles (see Fig. 10), the differences ….’

Response: The sentence has been revised.

Page 41, line 848: Referring to Fig. S5 is not correct (it corresponds to the discussion on the correlation between HCHO concentration in the 0-2km altitude range and temperature). So the figure on the impact of the MAX-DOAS a priori profile on the smoothing of the CHASER NO2 profile at Chiba seems to be missing. When you will add this figure, please correct the figures numbering in the Supplement and in the main text accordingly.

Response: We thank the reviewer for the comment. We have added the figure (Fig S7) in the supplement and respective figure numbers in the manuscript have been revised.

Page 45, line 910: ‘observatios’ -> ‘observations’
Response: Appropriate corrections has been included in the revised manuscript.

Page 54, line 1094: I would give the literature reference (Duncan et al., 2010) associated to the standard transition region approach.
Response: The reference has been included in the conclusion.

Page 54, line 1099: ‘clarified’ -> ‘further indicate’
Response: Appropriate correction has been included in the revised manuscript.

Page 54, line 1102: I would replace ‘agreed well’ by ‘agreed reasonably well’.

Response: Appropriate correction has been included in the revised manuscript.

Page 56, Acknowledgements: Personally, I would also thank the OMI HCHO and NO2 data providers.

Response: The data providers have been acknowledged

Response to Editorial Comments

* please check the suggestions made by the reviewer carefully, and implement them

Response: We have addressed all the reviewer comments within our knowledge and capability.

* please check the last abstract of your summary as it needs some language editing

Response: The abstract has been revised.

* in Figure 2, the label of the y-axis says "differential OD" while the quantity shown appears to be absolute OD

Response: We think the y-axis caption is correct. The spectra are plotted as the differential optical density from the reference spectrum. Similar type of figure has been used in our previous works also (i.e., Irie et al., 2011, Hoque et al, 2018)

* in the caption of Figure 3, please indicate what the error bars represent

Response: The caption of Figure 3 has been revised.

* in Figure 4, a bit more vertical space between the sub-figures would improve readability

Response: Figure 4 has been revised

* in Figure 7, the use of colour schemes appears arbitrary. I would suggest to use the colour scheme from (b) for both difference plots and to make sure that it is centred on 0 making yellow the value shown for 0

Response: Figure 7 has been revised

* in Figures 8, 9, 14, 15 and 16, I think it would be good to always include the 0 in the y-axis to facilitate comparisons

Response: Figure 8,9,14,15, and 16 have been revised.