

Responses to the comments of Reviewer 2

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.

The manuscript by Hoque et al. shows MAX-DOAS measurements of NO₂ and HCHO at three sites in Asia, namely Phimai (Thailand), Pantnagar (India) and China (Japan). The MAX-DOAS measurements are compared with the global chemistry model CHASER simulated concentration in the near-surface layer as well as the profiles. An attempt was made to use the ratio of Formaldehyde and NO₂ concentrations to derive ozone production sensitivity.

While I have mentioned some critical concerns about the significance of this study with respect to the Journal in my short review before the discussion phase, I provide my elaborate review here. Most likely, the short review prior to the discussion phase is not available in the interactive discussion; I append that here and expect it to be addressed.

Response: We thank the reviewer for the comments. The comments during the short review has been addressed herein.

Broadly the paper covers two separate aspects, namely MAX-DOAS measurements and comparisons with the global model. On the one hand, there are some shortcomings in both aspects of this study; I also find it difficult to motivate the readers, why such a comparison should be made in 2022, and what do we expect to learn from it. For a comprehensive evaluation of the global model, a global dataset (e.g. NDACC) should be used, which are also recently employed to evaluate TROPOMI data products (e.g. (De Smedt et al., 2021; Lerot et al., 2021; Verhoelst et al., 2021)). If the study is focused on south-east Asia, why a regional model with a better spatial resolution is not used? Several previous studies have used high resolution (few km), global models, for comparison with MAX-DOAS measurements and emphasised the need to even go for higher spatial resolution (sub km). This study, on the other hand, presents the model results at 2.8° resolution in the base case and 1.4° in the improved resolution case, which in my opinion, is too coarse for comparison with MAX-DOAS measurements.

Response: We thank the reviewer for the comments. Here are our responses:

(1) It is true that, similar studies have been reported in literature using different global and regional models. However, similar type of comparison is still important to evaluate many existing models. Because, simulation from different models differ depending on various model parameters and mechanism.

(2) Firstly, our research group focuses on various global scale study using the CHASER model. Research themes include global-scale chemistry, satellite comparison, and data assimilation. Thus, evaluation of regional/local simulations is important to assess model uncertainties and limitations, as conducted in earlier studies. Secondly, the choice of the regions is directly related to our MAX-DOAS observation facility, i.e., the A-SKY network. We agree with the reviewer that a regional model would be a better option. However, our research objectives are defined based on our existing modeling and observation facilities.

(3) We agree with the reviewer that; global models should be evaluated with global datasets. We have included the evaluation of global NO₂ and HCHO simulations with OMI observations in the revised manuscript.

(4) Earlier studies have indeed emphasized on higher model resolutions. High resolution models are preferable, however, not widely available/affordable due to technical limitation and expenses. Moreover, horizontal model resolution is not the sole the reason for discrepancies in comparison studies involving simulations. For example, the global mean bias (MB) in the CHASER NO₂ simulations at 2.8° and 0.56° compared to OMI were respectively, -0.25 and -0.24 x 10¹⁵ molecules cm⁻² (Sekiya et al 2018). Over SE-Asia the MB and root-mean square error at 2.8 and 0.56 were ~0.54 and 0.61 x 10¹⁵ molecules cm⁻² (Sekiya et al 2018), respectively. Moreover, model resolution impact on comparison result varies spatially (William et al 2017). Most of the global studies involving CHASER is conducted at 2.8 resolution, which is our standard model settings. Our objective is to evaluate the standard model simulations with the A-SKY observations. The impact of the model resolution on the results has been assessed in section 3.3.4.

Concerning the drawbacks related to MAX-DOAS retrievals, I find the vertical grid resolution (1km) too coarse, which limits the usability and interpretation of such data for air pollution-related studies. There are some technical issues related to the measurements as well, but those should be discussed in a detailed review if the editor deems the manuscript suitable for discussion in ACPD.

Response: We thank the reviewer for the comments. We have assumed an exponential profile and considering the fact that vertical resolution depends on the aerosol loading, we think our retrieval settings are reasonable. Using such settings, our aerosol and trace gas concentrations has shown good agreement with surface concentrations (Damiani et al 2021; Irie et al 2008, 2015,2012) and satellite observations (Irie et al 2012). Moreover, many air-pollutions related studies have been conducted using the similar settings (Irie et al 2016, 2019, 2021, Hoque et al 2018a, 2018b). Additional technical issues have been addressed in the detailed review responses.

Damiani et al, (2021), [Variabilities in PM2.5 and Black Carbon Surface Concentrations Reproduced by Aerosol Optical Properties Estimated by In-Situ Data, Ground Based Remote Sensing and Modeling](https://www.mdpi.com/2072-4292/13/16/3163) , <https://www.mdpi.com/2072-4292/13/16/3163>

Detailed review:

Introduction:

1. The authors motivate the readers about the current study in a way that MAX-DOAS measurements of near-surface concentrations and profiles are used to evaluate a global model CHASER (lines78-98). A study with such motivation is more suited for GMD (model evaluation papers). At least in the introduction, I could not find motivation for understating the atmospheric chemistry of the region of interest.

Response: We thank the reviewer for the comments. The following changes are included in response to this comment.

L62 – 73: The observational sites examined for the present study have different atmospheric characteristics. Thailand is strongly affected by pollution because of rapid economic development and urbanization. Moreover, biomass burning in Southeast Asia is a significant source of O₃ precursors, contributing up to 30% of the total concentrations during the peak burning season (Amnuaylorajen et al., 2020; Khodmanee et al. 2021). Because of rapid industrialization, India the second most populous country in the world, is witnessing an increasing O₃ trend along with NO₂ and HCHO concentrations in all major cities (Mahajan et al; 2015; Lu et al, 2018;). The Indo-Gangetic Plain (IGP), which covers ~21% of the Indian subcontinent land area is hotspots of severe air pollution (Giles et al; 2005, Biswas et al; 2019). In contrast, surface O₃ concentrations have shown an increasing trend in Japan, despite decreasing NO_x and VOC concentrations related to emission control measures after 2000 (Irie et al., 2021). Therefore, observational and modeling studies must be conducted to improve our quantitative understanding of the O₃-NO_x-VOC relation in these regions.

2. Line 62 – I think it is more accurate to replace “radiation” with “radiance”.

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

3. Lines 68-78: In my opinion, MAX-DOAS is, a powerful independent technique for monitoring atmospheric constituents, and I would mention it first before stating that it is complementary to in situ and satellite measurements. Observation, dataset and methods1. In my opinion, the climate classification for Pantnagar should be done in a different way. The current classification does not consider summer as a separate season and is rather partly combined in spring and summer monsoon. The months Apr-June are extreme summer months in the Indo-Gangetic plain, with daytime temperatures above 40°C and an average temperature above 30 °C.

Response: Firstly, we have made appropriate changes to texts. Secondly, we thank the reviewer for the comment. The climate classification for the IGP region is based on many earlier studies in the region (i.e., Hoque et al, 2018b, Mahajan et al (2015) etc.). Such classification was adopted for consistency with the literature studies. Including summer as a separate season will not change the conclusions substantially.

Observation, dataset and methods:

4. Figure1: As the study focuses on the evaluation of the model over the south and east Asian region, I would recommend restricting the map boundaries to only relevant regions. The color codes show the surface volume mixing ratios (VMR) and not concentrations. The color bar legend should be corrected accordingly.

Response: Because, CHASER is a global model and global evaluation of the simulations has been included in the revised manuscript, we prefer not to change Figure 1. However, the color bar legends have been changed to “volume mixing ratio”.

lines 139-141: Campaign is used two times in the same sentence.

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

5. MAX-DOAS system: What is the spectral range of the spectrometer used in these measurements. I am keen to know why the higher wavelength window of 460-490nm was chosen for NO₂ retrieval. The instrument used for this study participated in the CINDI and CINDI-2 campaign, and there the fit interval used for NO₂ retrieval was 425-490nm or 411-445nm.

Response: The spectral range of the spectrometer is 310 – 515 nm . While the CINDI (1 and 2) semi-blind intercomparisons used the window 425–490 nm, the present study uses 460- 490 nm for much faster retrievals by the DOAS fitting used in JM2. Also, the difference between representative wavelengths for NO₂ and O₄ can be very small, minimizing the wavelength-dependence of AMF information.

6. lines 148-149 Why would you want to minimize the variations in measured signals for various off-axis measurements. According to the DOAS principle, reference measurements should be taken at a 90° elevation angle to account for stratospheric contribution in the dSCDs. If the 90° measurements could not be taken due to any physical restrictions, this should be stated accordingly.

Response: Firstly, we used a spectrometer with a fixed integration time throughout the day. The intensity of the spectra usually depends on the elevation angle (EL) within a 15-min interval (time for a complete scan for all EL). Thus, the variation range of intensities measured at all ELs can be large, which occasionally leads to intensity saturation at the reference angle. To avoid such phenomenon, the refence measurement were conducted at 70° instead of 90°. All the ELs were considered in the differential air mass factor

calculation to retrieve the vertical profiles. Thus, the choice of reference EL (70° or 90°) is not a critical issue. Secondly, “variation in the measured signals” has been replaced with “to avoid saturation of intensity”. The changes/addition in the revised manuscript is as follows:

L163-167: The sequences of the ELs at all the sites were repeated every 15 min. The reference spectra are recorded at EL of 70° instead of 90° to avoid saturation of intensity. Because all the ELs were considered in the box air mass factor (A_{box}) calculation to retrieve the vertical profile, the choice of reference EL (70° or 90°) is not an important issue for this study.

7. How would the additional off-axis measurements at elevation angle $> 10^\circ$ reduce the systematic errors in the fitting results. In my opinion, measurements at some elevation angles (e.g. 15° and 30°), provide important information regarding the trace gas and aerosol profiles during inversion and should not be skipped if possible. Moreover, later in this study, the authors analyse profiles at high altitudes ($> 2\text{Km}$), and measurements at high elevation angles are necessary for the accuracy of such retrieval. Even the surface layer used in this study has a thickness of 1km, and measurements at high elevation angles are crucial for this layer as well.

Response: Restricting ELs < 10 , has been adopted after the detailed study of Irie et al., 2015, utilizing MAX-DOAS, Sky radiometer, LIDAR and CRDS observations, in a view on requirement of a correction factors for the O_4 absorptions. Adopting a single correction factor (f_{O_4}) of 1.25 for all of the elevation angles led to systematic overestimation of near-surface aerosol extinction coefficients. When the ELs were limited to $\leq 10^\circ$ and an EL- dependent correction factor was used, the agreement between MAX-DOAS AEC profiles and other instruments were improved. With these modifications, the possible effects of temperature-dependent O_4 absorption cross section and uncertainty in DOAS fit on an aerosol profile retrieval are expected to minimize.

We also agree with the reviewer’s opinion on the importance of higher ELs. Our current measurement and retrieval settings enhances the capability for observing the planetary boundary layer (PBL) as a result of the loss of sensitivity to extinction at high altitudes, where clouds are usually more dominant than aerosols. Thus, with our current setting realistic profiles can be derived, with lower accuracy in the higher altitudes.

Lines 170-171 and Figure 2: How does the DOAS fit for O_4 look like in the two wavelengths window used in this study. An intercomparison of O_4 dSCDs retrieval from the two fit windows should also be shown (at least in the appendix).

Response: The DOAS fit in the two-wavelength window is provided in the Supplementary information (Figure S1).

8. Line 187. It was difficult for me to visualise what the profile shapes look like for different values of F. It would be nice to have example plots showing the profile shapes for some values of F (similar to that shown in Fig 1 of Beirle et al., 2019 for h and s)

Response: Aerosol extinction profile with different F values has been shown in the supplementary information (Fig.S2)

9. It will be more accurate to save that VMRs are “calculated” using the partial VCDs rather than “converted”. Though in this study, the height of the box is fixed, in general, it would be better to also mention that this conversion also considers the height of the box.

Response: We have replaced “converted with calculated”. We did not assume fixed height but an exponential profile.

Why the heights of the boxes are chosen to be so wide at 1km. Several studies (e.g. Kumar et al 2020) indicate a strong gradient in NO₂ profiles in the lowest 1km. As the MAX-DOAS measurements in this study are used to evaluation of near surface VMRs of trace gases from the global model, higher vertical resolution in the profile retrieval should be more relevant.

Response: We did not assume fixed height but an exponential profile. Considering this point and the fact that vertical resolution depends on the amount of aerosol loading, our retrieval settings are reasonable. Surface aerosol and NO₂ concentrations retrieved with such settings has shown good agreement with other observations and regional model (Damiani et al 2021, Irie et al, 2008). Our HCHO retrievals has been used to validate the TROPOMI observations (De Smedt et al 2021). Therefore, we think our retrieval settings for the model evaluation is reasonable.

10. Lines 222-223: In lines 170-171, the authors state that significant O₄ absorption in 460-490nm was used to retrieve the O₄ ΔSCD. Then why an aerosol retrieval in the same wavelength window is not performed? Rather an Angstrom exponent was used to retrieve the AOD at 470nm.

Response: We thank the reviewer for the comments. Aerosol at 476 nm are retrieved from the 460 – 490 nm. AOD at 470 nm is calculated using the Angstrom exponent for the NO₂ retrieval

11. Lines 223-223: What is the basis of the assumption of Angstrom exponent = 1. How does the choice of Angstrom exponent affect the retrieval?

Response: Angstrom exponent of 1 is assumed to reflect the strong wavelength - dependence of the AOD values. The choice of the Angstrom values had non- substantial impact on the retrieval. Uncertainty related to the Angstrom value was smaller than the uncertainties in the A_{box} profiles. The following texts are added in the revised manuscript.

L 254 – 257: The choice of the Angstrom exponent value can induce uncertainty in the retrieved VCDs. However, such uncertainty was found to be non-significant compared to that of A_{box} profiles. Uncertainty in the A_{box} profiles are assumed to as high as 30 to 50%. Such values are derived empirically from comparison with sky radiometer and LIDAR observations (i.e., Irie et al., 2008b).

12. Line 261: Please cite the latest version of CHASER and mention the model version number.

Response: The latest version of the model is CHASER 4.0. Appropriated changes in the has been included in the manuscript.

13. Lines 262-264: What is the name of the chemical mechanism used for CHASER simulation?

Response: The chemical mechanism of CHASER is mainly adopted from the Master Chemical Mechanism (MCM). The information has been added in the revised manuscript.

L304 – 305 :The chemical mechanism is largely based on the master chemical mechanism (MCM, <http://mcm.york.ac.uk>)(Jenkin et al., 2015).

14. Please provide specific details of biomass burning emissions. Which product of ECMEF (might be GFAS?)

Response:

L321- 322: CHASER simulations. Anthropogenic emissions were based on the HTAP_v2.2 for 2008. Biomass burning and soils emissions from the ECMWF/MAC (Global Fire Assimilation System (GFAS)) reanalysis were used

15. Lines 278-285: Please provide an estimate of NO_x and VOCs emissions from different sectors in the regions of interest. This is important to understand and confirm the important emissions sectors speculated in the subsequent sessions.

Response: The following texts were included in response to this comment

L332-342: NO_x emissions in India were estimated as 14 Tg/yr in 2016, almost two-fold increase since 2005 (~8 Tg/yr), with the energy and transportation sector being the largest contributor (Sadavarte et al 2014). Indian anthropogenic non-methane VOCs (NMVOCs) emissions in 2010 were estimated ~ 10 Tg/yr, with respective contributions of 60, 16, and 12% from residential, solvents, and the transport sector(Sharma et al 2015). In Japan, vehicular exhausts (14 - 25%), gasoline vapor (9 - 16%), liquefied natural gas (7 - 10%), and liquefied petroleum gas (49 - 71%) contribute to the total VOC concentrations (Morino et al., 2011), with annual NMVOC emission of ~2 Tg (Kannari et al., 2007). Annual NO_x emissions in Japan and Thailand in 2000 was estimated as ~2000 and 591 kt/yr, with the largest contribution from transport-oil use, followed by the energy and industrial sector (Ohara et al., 2007). Annual anthropogenic VOC emissions in Thailand are approximately 0.9 Tg, with 43, 38, and 20% contributed, respectively, from industrial, residential and transportation sectors (Woo et al; 2020).

16. Line 286: It would be nice to already mention here, what is the purpose of multiple CHASER simulations?

Response: The following texts has been included in response to this comment.

L343- 344: Multiple CHASER simulations with different settings used for sensitivity studies are presented in Table 3.

Results and discussion:

18. Lines 301 and 306 (and also at several sections of the manuscript): Figure 4 shows volume mixing ratios (not concentrations).

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

19. Figure 4: Please use the same y-axis scale for all the subplots. Also, in lines 302 and 348, it is important to mention that the standard deviations (or error bars) show the variability (not to be confused with measurement uncertainty).

Response: The y-axis scale of the HCHO plots are revised. A similar y-scale for the NO₂ plots cannot be used because the Chiba NO₂ levels are too high compared to Phimai and Pantnagar. Thus, only the NO₂ y-scale for Phimai and Pantnagar has been revised. Appropriate corrections have been included regarding the standard deviations.

20. Lines 326-328: How do these mixing ratios and the seasonality compare with the other studies reported in the Indo-Gangetic plain or other sites in India) (e.g. Biswas and Mahajan 2021, AAQR, Kumar et al 2020, ACP).

Response: The following texts were added

L391-399: The peak HCHO mixing ratio at Pantnagar is almost twice that of in Pune city (~ 3 ppbv) (Biswas and Mahajan, 2021), a site in the IGP region. The HCHO seasonality at the two sites are found to be dissimilar, because of differences in the VOC sources, however, lower mixing ratios during the monsoon is consistent. From another site in the IGP region (i.e., Mohali), Kumar et al., (2020) reported lowest HCHO VCDs during March 2014 and 2015, attributing them to lower biogenic and anthropogenic VOC emissions. At Pantnagar, the lowest HCHO mixing ratios are observed during the monsoon. The rainfall events in the IGP region shows strong annual variability (Fukushima et al. 2019). Discrepancies between the sites might be related to the rainfall pattern.

21. Section 3.1.3.1: In my understanding, the HCHO and NO₂ indicator ratios (R_{FN}) indicator proposed by Martin et al., 2004 and Duncan et al., 2010 are based on the tropospheric vertical column densities (VCDs) and NOT concentrations. As the authors work with the MAX-DOAS system in this study, why they have chosen to calculate the ratio based on concentration and not the VCDs?

Response: Our MAXDOAS system is optimized for retrieving aerosol and trace gas information in the planetary boundary layer rather than across the entire tropospheric column. Therefore, the surface HCHO and NO₂ concentrations are used to calculate the ratios. Secondly, the effectiveness of the column-based R_{FN} values are under discussion because of altitude-dependence of HCHO and NO₂. Thirdly, Souri et al. (2020)'s approach is based on surface R_{FN} values, which has been adopted in this study.

22. Lines 390-391 and Figure 5: What is the person correlation coefficient of the scatter plots shown here. I wonder, how robust are the calculations drawn based on slopes of the scatter plot if the correlation is poor.

Response: The Pearson correlation coefficient is not critical in such case. Because, the slope indicates the transition region from VOC-limited to NO_x limited, and is dependent on the NO₂-HCHO chemical feedback.

23. Figure 5: Please show a similar plot color-coded according to solar radiation (radiance at a selected wavelength). This would enable the authors to evaluate the contribution of chemistry in ozone production independent of available solar radiation.

Response: We thank the reviewer for the comments. Our discussion was primarily focused on the calculation of the transition line between the NO_x and VOC-limited regions. Discussion on in-depth chemistry of ozone would be beyond the scope of our discussion. Thus, we did not include the suggested figure and related discussion in the manuscript. We will include such figures and discussion in a more detailed study.

24. How do the R_{FN} values compare to previous studies (based on model, satellite and MAX-DOAS observations) in India (or Indo-Gangetic plain)?

Response: The following texts has been included in response to the comment

L499 – 510: At Pantnagar, high O_3 occurrences lie below (42%) and above (57%) the transition line, indicating that O_3 production is sensitive to both HCHO and NO_2 which contradicts results reported by Biswas et al. (2019). Based on satellite and ground-based observations, the study estimated the R_{FN} values at a site in the IGP as > 4 and >2 respectively, and regarded the O_3 regime as NO_x -limited. Mahajan et al (2015) reported R_{FN} values of less than 1 over the IGP region signifying as a VOC-limited region. Pantnagar is a sub-urban site situated beside a busy road. Therefore, effects of anthropogenic emissions are expected year-round, especially with pyrogenic emissions during the spring and post-monsoon period. O_3 sensitivity to both NO_x and VOCs in the north-west IGP region has also been reported by Kumar and Sinha (2021). Therefore, the balance between the VOC and NO_x -limited region in the IGP is reasonable. The mean and minimum R_{FN} value along the transition line are, respectively, 5.59 and 6.09. The minimum value (i.e., 5.59) is higher than Phimai (3.26), suggesting higher VOC levels at Pantnagar, consistent with the observations.

25. Line 472: I was wondering if the boundary layer height directly from the model simulations or reanalysis data products (e.g. ERA5) can be used and more suitable.

Response: The PBL heights from CHASER and reanalysis could be used, however, we have not tested the suitability of the datasets. For the current study, we followed the methodology suggested by Jin et al., (2017).

26. Section 3.1.3.2: It is difficult for me to understand the need to calculate the factor “F” (column to surface conversion factor, equation 9) in the context of this study. Authors use and discuss “F” to get column integrated values (i.e. concentration). However, the MAX-DOAS retrieval also provided the vertical column densities, which is a much simpler approach.

Response: As mentioned earlier, our system is optimized for retrieving aerosol and trace gas information in the planetary boundary layer rather than across the entire tropospheric column. Thus, the retrieved VCDs will mostly reflect the near surface information. Thus, the model values were used to calculate the F value.

27. Lines 501-503 It might be true that there is no relevant literature in the south and south-east Asia presenting “F” values. But there is sufficient literature discussing both the surface concentrations and the vertical column densities, from which “F” can be derived.

Response: We agree with the reviewer’s opinion and removed the sentence.

28. Lines 511-512: Averaging kernels are highly sensitive to atmospheric conditions, and hence these should be applied to individual profiles, and the averaging should be performed rather than using an averaged averaging kernel for a season.

Response: We have tested both ways – applying (1) AKs of individual retrieval, and (2) seasonal averaged AKs to the model profiles and found the results to be similar. Moreover, Franco et al (2015) applied seasonally averaged AKs to the model output. We have included the following text corresponding to this comment.

Applying individual AKs to the model outputs yielded similar results. The seasonally averaged AKs for both sites are shown in Fig S4.

29. Lines 515-516: How and between which parameters are the R values calculated? Are the R-Values calculated using individual measurements, daily average or seasonal mean?

Response: The R-value indicates the correlation between the observed and simulated HCHO seasonality. The text has been revised.

30. Figure 8: can the authors explain why the application of averaging kernels significantly decreases the column in Phimai, but results in an increase in Chiba?

Response: CHASER estimates at both sites are different, i.e., higher and lower compared to observations in Phimai and Chiba, respectively. Thus, the smoothing yields different results. Moreover, such differences can also potentially be related to the a priori values used for smoothing. The a priori values are obtained from the retrieved SCD and VCD values. The differences in the atmospheric conditions at both sites will be reflected in the a priori values, and thus leading to different smoothing results.

31. Lines 533, 550, 590, 652, 653, 655: The MAX-DOAS profile retrievals are performed at a vertical resolution of 1Km, and hence it is not appropriate to quantitatively evaluate the model profiles at intermediate layers (e.g. 0.5 km or 200m).

Response: The evaluation at the intermediate layers has been removed from the discussion.

32. Line 546: Please provide appropriate reference justifying the model overestimation of biogenic emissions.

Response: The texts has been edited in response to this comment

L771-773: Consequently, it is likely that the biogenic emissions for this region in the model are overestimated. The Southeast Asian isoprene emissions in CHASER is 128 Tgyr⁻¹, higher than the CMAS-GLO-BIO (Sindelarova et al., 2022) inventory (78 Tgyr⁻¹).

Lines 554-558: If biogenic emissions are overestimated in the model (as mentioned before), I would expect a higher increase in simulated HCHO than observed between January and August.

Response: We have edited the earlier sentence. The biogenic emissions for the Southeast Asian region are overestimated in CHASER. Because the observed HCHO columns are enhanced during the dry season, the difference between the observations and simulations are low.

33. I am surprised to learn that emissions due to wintertime heating is not included in the anthropogenic emission inventory. From the EDGAR website (https://edgar.jrc.ec.europa.eu/dataset_htap_v2#p1), it seems that the sectors “htap_6 Residential” and “htap_3 Energy” include the wintertime heating emissions.

Response: We thank the reviewer for pin pointing the issue. We have revised the sentences.

34. Sector 3.2.3 could be merged with 3.2.1 and 3.2.2.

Response: We prefer to keep the subsections for better readability

35. Lines 678-680: It is not clear for me, why observations above 1.8km are compared with the model. Both MAX-DOAS profile retrievals and model simulation are performed above the ground level.

Response: The comparison between the model and measurements at the Pantnagar site has been discarded.

36. Lines 685-686: This brings me to the previous comment. Why in the first place, measurements are restricted to elevation angles less than 10°.

Response: The discussion related to this comment has been discarded. The use of ELs < 10 has been explained in the earlier comments

37. Lines 687-691: What is included in the whole IGP. Please show it on a map. What are the limitations of comparing the measurements at a point (representative of a few Km) to the entire IGP?.

Response: Instead of a map, we have added the following texts:

L905-913: The IGP is the most fertile region in South Asia, which accounts for approximately 50% of the total agricultural production of India and is one of the significant contributing regions to the global greening based on leaf area index (Sarmah et al., 2021). Moreover, IGP is one of the regional HCHO hotspots in India (Chutia et al., 2019). The observed HCHO seasonality at Pantnagar is consistent with that reported by Mahajan et al. (2015) for the entire IGP region. Consequently, comparison with the HCHO retrievals in Pantnagar can assess the model capability in the IGP region. The spatial representativeness is a limitation for comparison between a point measurement and regional simulations. Thus, the results are interpreted qualitatively.

38. Figure 11: Please use the same y-axis range for subplots of HCHO and NO₂.

Response: The discussion related to comparison in Pantnagar has been discarded in the revised manuscript.

39. Line 711-712: If the biogenic emissions are overestimated, how come the simulated isoprene concentrations are reasonable?

Response: We have revised the earlier sentences that mentioned the “biogenic emission in the model is overestimated” to “biogenic emissions for Southeast Asia in the model is overestimated”. The Indian isoprene emissions in CHASER is 15 Tg/yr, comparable to the MEGAN estimate of 12 Tg/yr. (Guenther et al; 2006). CHASER simulated isoprene concentrations in the IGP region are also comparable with the observations. Thus, the simulated isoprene concentrations in the IGP region is deemed reasonable.

40. Figure 12 and line 735: In my opinion, it will be better to show the time series at the three stations rather than the zonal mean if the inferences are made with respect to observation at the three sites.

Response: We have revised figure 12, which is fig.14 in the revised manuscript and the related discussions following the comment in section 3.3.4.

41. Lines 738:740: 10% is the average, and based on this, one cannot infer that the comparison result will improve by at least 10%.

Response: We thank the reviewer for pinpointing the issue. We have removed the sentence.

42. Line 748: How did the authors estimate that the impact of model resolution is 20%.

Response: The impact was estimated based on the differences between the two simulations. However, the sentence has been removed in the revised manuscript.

43. Line 755: What stops the authors from using an updated emission inventory if those are already available.

Response: We thank the reviewer or the comments. The information has been taken from a study in the literature (Miyazaki et al., (2020)). Because of some technical issues, we haven't yet updated the emission inventories, which we are currently trying to fix. Uncertainty related to the old emission inventory are discussed in the light of comparison with the OMI data in section 3.3.4

44. Line 782: How are the biogenic emissions optimized?

Response: We used multiple simulations with varying biogenic emissions. The biogenic emissions were changed until the wet season estimates coincides with the observations.

45. Lines 789-791: Please provide an estimate of NO_x emissions from different sectors based on the emission inventory used for the simulations.

Response: We have added the information in section 2.3. The following texts are included:

L328 – 331: Global NO_x emissions of 43.80 TgNyr⁻¹ are used in the simulations, considering industries (23.10 TgNyr⁻¹), biomass burning (9.65 TgNyr⁻¹), soil (5.50 TgNyr⁻¹), lightning (5 TgNyr⁻¹), and aircrafts (0.55 TgNyr⁻¹) as significant sources. Global isoprene emissions from vegetation were set to 400 TgCyr⁻¹.
