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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long term MAX-DOAS measurements of NO_2, HCHO and aerosols and evaluation of corresponding satellite data products over Mohali in the Indo-Gangetic plain" *by* Vinod Kumar et al.

Anonymous Referee #2

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Review for 'Long term MAX-DOAS measurements of NO2, HCHO and aerosols and evaluation of corresponding satellite data products over Mohali in the Indo-Gangetic plain'

The paper titled 'Long term MAX-DOAS measurements of NO2, HCHO and aerosols and evaluation of corresponding satellite data products over Mohali in the Indo-Gangetic plain' reports long term MAX-DOAS observations of AOD, NO2 and HCHO from Mohali, a suburban site in the Indo-Gangetic plain covering the period of January 2013 – June 2017. The MAPA algorithm is used to retrieve vertical profile and

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Discussion paper

vertical column densities (VCD) and the results are discussed in detail. Seasonal and annual trends along with diurnal variation of vertical profiles are discussed and inter-comparison of the MAX-DOAS measured AOD, NO2 and HCHO with satellite observations are reported. Finally, they have compared surface volume mixing ratios of NO2 and HCHO with in situ observations. Whilst the study is strong on analysis, with the methodology explained in detail, the paper is weak on new results. At present, the manuscript as it stands is more appropriate for a methods journal, for example Atmospheric Measurement Techniques. No novel results are presented in terms of improving our understanding of the chemistry or physics of the region, or of the two chemical compounds and their impacts. The main conclusions of the paper are twofold – presenting an inter-comparison with satellite products, which is a methodology based conclusion and second that ozone production is sensitive to NOx and VOCs in winter, but more to NOx during the other seasons – not a novel result considering the past publications globally and in this region. Even the methods section is not new considering it has been developed in the past by some of the co-authors and has already been used in different parts of the world.

Hence, I would reject the current paper and encourage submission in a methods journal or ask the authors to focus more on the observations rather than the observation methodology to decipher novel results. At present the manuscript, although replete with instrument and retrieval details, does not include significant results on atmospheric chemistry.

Specific comments:

Measurement technique:

1. Why did the detector temperature have to be adjusted for different seasons? If the temperature was not stabilized for different seasons, would the diurnal temperature change not lead to the same issue? Was the DC and offset measured for different temperatures and removed from the spectra according to the ambient temperature?

(line 151-152)

2. ‘During the period of measurement, the horizon in the viewing direction was determined by a residential building with a height of about 40m at a distance of 3 km.’ - This part is not clear. Does it mean the line of sight has an obstacle within 3 km? Is this why a 1 degree elevation angle was not used? (line 164-165)

3. What was the calibration process for the instrument? Did the authors perform any spectral calibration?

4. ‘Based on the measured radiances at 360 nm, colour index (ratio of measured radiances at 330 and 390 nm) and measured O4 airmass factors (O4 SCD/ O4 VCD), we can classify the sky conditions into the following seven categories:’ The difference between the upper and lower wavelengths for the classification is small. Why have the authors have not used radiances from ends of the measured window? (Line 204-205)

5. The sky conditions defined by colour indexes (CI) were not supported by any other supporting information. Were there any other methods like visual inspection, or comparison with sky images done for validation? Without validation how can the CI be used for cloud classification – especially since it is based only on radiances? What were the thresholds used for the classification – what determines a cloud hole as compared to broken clouds? Although the authors have cited some past work, that is from different parts of the world, with very different aerosol loading and SZAs – this information is missing from appendix B. (Line 206-207)

6. What was the threshold for RMS used for filtering the QDOAS analysis? Was there a reason for using this filter?

7. ‘This value was derived as the mean of the Ångström exponent (AE) between 470-550 nm measured by MODIS for the measurement period, where we do not observe a strong intra-annual variation (Fig. D3)’ The calculation of the AE value from MODIS poses two concerns: The wavelength range is not same and satellite instrument view-

ing geometry is different than ground based observation. The overpass times of the satellite will also determine what AE is measured, which will change drastically in places with high aerosol loading, or in seasons of biomass burning etc. Why not to use any ground based AOD observations if available – if not, what is the sensitivity of MAPA to the AE values used? (Line 246-247)

8. Were there any radiosonde or BL height measurements available? This would add to the discussion on the differences in the in situ and MAX-DOAS profiles and also to the seasonal variation.

Chemistry:

9. 'Fig. 11 shows the afternoon time (12:30-14:30) monthly mean HCHO/NO2 ratio calculated using the MAX-DOAS observations. We observe that in winter months, mean daytime HCHO/NO2 ratios between 1 and 2 are observed, which represents sensitivity towards both NOx and VOCs.'- The HCHO/NO2 ratio was calculated only for 12:30-12:30 hrs. What was the ratio during rest of the day – emissions would show a diurnal profile, affecting this ratio. (line 650-655)

10. 'However, these are comparable to previous in situ NO2 VMR measured for a period of more than one year at urban and suburban locations of India (e.g. Mohali 8.9 ppb, Pune ∼9.5ppb and Kanpur 5.7ppb)(Gaur et al., 2014;Kumar et al., 2016).'What about other locations from India such as Debaje and Kakade (2009) and Beig et al. 2007. There are many other observations of NO2 and HCHO from urban and suburban regions in India. Please update the ciations.

11. The explanation for higher mixing rations in the MAX-DOAS as compared to in situ observations is not satisfactory. The fact that both the species how higher values for the MAX-DOAS are indicative of an instrumental bias, rather than source regions as speculated in the paper. If the authors are convinced that the power plant, or VOC degradation at higher altitudes contributes to this effect, it can be checked using air mass back trajectories.

12. Figures D6 and D7 need to be discussed in more details in terms of the chemistry leading to large diurnal differences between the in situ and MAX-DOAS observations.

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