

Interactive comment on “Observing atmospheric formaldehyde (HCHO) from space: validation and intercomparison of six retrievals from four satellites (OMI, GOME2A, GOME2B, OMPS) with SEAC⁴RS aircraft observations over the Southeast US” by L. Zhu et al.

Anonymous Referee #3

Received and published: 20 April 2016

This manuscript presents an intercomparison exercise of tropospheric HCHO retrieved from satellites measurements using several products obtained from independent retrieval approaches. In order to validate the products, the authors use aircraft observations during the short term field deployment of SEAC⁴RS in the Southeast of the US. This is an important validation effort. In general, the paper is well written and within the scope of ACP. That said, I have important comments/suggestion that I recommend revisions before final publication.

Major comments:

- In the abstract and conclusion sections it is mentioned that the HCHO columns are biased low by 20-50%, which is a significant number. Then, it is concluded that HCHO from satellite provides a reliable proxy for isoprene emissions. How do the authors conclude that HCHO is reliable given the significant bias (plus any uncertainty associated to the assumptions of HCHO yields from isoprene conversion)? The only section with results shown is section 4, I recommend to insert a new section(s) where specific details are given regarding errors and how they affect the isoprene emission.

- The emissions of natural biogenic volatile organic compound have a temperature dependence, hence a seasonal variation that it is not studied in the present study. However, the statements along the manuscript are quite general regarding spatial and temporal distribution of HCHO. The focus of the study is only limited in the southeast of the US during less than two months. I suggest to state clearly that the results/conclusions shown are for this specific time/area.

- The aerosol optical depth (AOD) observed in the southeast of the US is significantly higher than other parts in north America. In addition, the temporal distribution of AOD over the southeast of the US has a maximum peak in the summer months (similar months as this study). However, it is not well documented in section 2 how aerosols are treated in the satellite retrievals. Also, current investigation in the southeast of the US associate aerosol aloft (e.g., Goldstein et al., 2008). I encourage the authors to explain in better detail the effect of aerosols in the retrieval and final uncertainty. How do aerosol aloft impact the retrieval of HCHO columns?

- The in-situ sensors are taken as the ground truth in this study, however there is quite a bit of manipulation in the conversion of mixing ratios to columns. It is not clear to me why the HCHO need to be normalized if the columns are compared with satellite retrievals. Please give a thorough description of why this is needed and why the "Day-to-day variability in HCHO columns can be fitted well to $\exp[0.11T]$ ". Is there

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any explanation of why CAMS and ISAF have differences of about 10% since they measure the same air mass?.

- Have the authors looked at the trace gas inhomogeneities captured by the in-situ observations and compare with results of satellite on a pixel-pixel resolution and ?. In other words, do the correlation improve if the air mass sampled is the same?

- In the abstract it is mentioned that “The GEOS-Chem chemical transport model provides a common intercomparison platform”. However, it is not clear how GEOS-Chem is used. Consider expanding the sentence explaining how this is achieved.

- Page 3, line 23: “Here and elsewhere, we use only satellite pixels with solar zenith angle less than 60, cloud fraction less than 0.3, and row anomalies (for OMI) screened”. In the same paragraph it is mentioned the detection limit of the satellites but it is not clear if you use only data above the detection limit for the analysis in this work.

- Page 4, line 16: There are several abbreviations that need to be defined, e.g., SAO, OMPS-PCA, etc. A table/appendix with abbreviations would be useful.

- Page 5, line 5-17: The conversion of mixing ratios to columns is achieved by assuming that HCHO is co-located with aerosols (identified with the mixing height from DIAL-HSRL), how valid is this assumption?. I suggest to explain also why an exponential decay with a scale height of 1.9 is used and how do the background column is found.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-162, 2016.

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