

Response to reviewer 1 comments

By providing a detailed description and evaluation of the new IASI retrieval, the authors addressed my main concern regarding the initial version of the manuscript. In addition, the restructuring has significantly improved the quality of the manuscript. Having read the authors' responses, I still believe that the manuscript would benefit greatly from TOMCAT simulations being performed at a much finer resolution. However, I understand that this resolution was chosen due to the authors' previous work.

My main concern with the revised manuscript is that the evaluation of the newly presented satellite product for CO and HCN is only performed at one observation station. This station is located in a remote region. In the context of the manuscript, however, choosing this station is valid due to the impact of the Indonesian fires. However, confidence in the satellite product would be strengthened if the authors also evaluated their product at other observation locations, particularly given that additional NDACC stations provide FTIR datasets. Many peatland fires occur at higher latitudes. While this is not the particular focus of the study but potentially important for follow-up studies: how well does the new satellite product perform at higher latitudes? How well does it perform in regions influenced by anthropogenic activities? I strongly recommend performing the same evaluation and validation for all other available NDACC stations that provide CO and HCN data. At a minimum, this should be added to the supplementary material.

We thank the reviewer for this thoughtful comment and agree that validation at additional NDACC stations would provide valuable information on the broader performance of the new satellite products. However, as noted by the reviewer, the primary objective of this present manuscript is to investigate the capability of the CO and HCN products to capture the emissions and transport associated with the 2015 Indonesian peatland fires. For this specific application, the Maïdo station is particularly well suited because it was directly impacted by the resulting biomass-burning plumes and provides a relevant independent dataset for evaluation. This is even recognised by the reviewer, "In the context of the manuscript, however, choosing this station is valid due to the impact of the Indonesian fires."

A comprehensive validation across all available NDACC stations would considerably expand the scope of the manuscript, shifting its focus from a case study of the Indonesian peatland fire event to a global product validation effort. Such an analysis requires a detailed assessment of the product performance under a wide range of atmospheric and emission conditions and is beyond the objectives of the present work. We also note that peatland fires occurring in other regions may differ substantially from Indonesian peatland fires in terms of fuel characteristics, combustion processes, and emission profiles. Therefore, validation at higher-latitude sites would address scientific questions that are distinct from those investigated here.

We agree that this is an important topic, just not one that should be presented as supplementary material or tacked onto a paper with a different focus. A dedicated study to evaluate the performance of the new products at multiple NDACC stations spanning different latitudes and atmospheric environments is currently in preparation. This will

involve many additional co-authors, and will provide a robust global comparison along the lines of that discussed in the preceding paragraph.

Minor comments:

- Lines 5 and 65: Please introduce the FTIR abbreviation properly. Currently, the abbreviation is introduced in line 78.

We thank the reviewer for pointing this out. We have now introduced the FTIR (Fourier Transform Infrared) abbreviation at its first occurrence in the manuscript (Lines 5 and 65), ensuring that the abbreviation is properly defined before subsequent use.

- Line 76: Avoid the double introduction of chemical compounds and abbreviations.

We thank the reviewer for pointing this out. We have removed the redundant introduction of the chemical compounds and their abbreviations in the revised manuscript, improving the clarity and readability of the text.

- Figure 5: To what extent is the outcome of the comparison of the averaged total column sensitive to the averaging region? On what basis did you define the region?

The region was defined to cover Indonesia and, at the same time, capture the wildfire emission plumes. Significantly extending or reducing the size of the domain would either include areas largely unaffected by the emissions or exclude portions of the plume, thereby reducing the representativeness of the spatially averaged total column. To clarify this point, we have revised the description of the averaging box in Line 355 of the manuscript.

- Line 678: Please provide a proper link to the NDACC database.

We thank the reviewer for pointing this out. The link to the NDACC Maïdo data was inadvertently omitted from the data availability section. We have now added the correct link to the NDACC data platform in the revised manuscript.

- Figure 8: What does HCN204 stand for? It is not defined in the caption or the manuscript text.

We thank the reviewer for pointing this out. "HCN204" was an unintended label originating from an older internal naming convention used during figure preparation. We have corrected this error in the revised manuscript and updated the figure accordingly to avoid any confusion.

- Figure 13: Please improve the quality of the figure.

We thank the reviewer for this comment. We will carefully assess the quality and readability of Figure 13 during the final preparation of the manuscript and make any necessary adjustments to the layout and formatting prior to final submission.

Response to reviewer 2 comments

Bruno et al. have considerably improved their manuscript according to the points mentioned by my previous review. Structure and quality of figures have been improved throughout the manuscript. In particular one of the main points, the vague description of the retrieval, has been well addressed in the revised manuscript. Still, I have a few points, which I mention below.

(Line numbers refer to the track-change document.)

Remaining points:

- Overall: Some answers to points of my previous report are very helpful for me, but the content of these answers has not been added to the manuscript. In case the authors think that my questions were unlikely to arise for other readers, this may be justified - otherwise I recommend adding these information to the manuscript.

We thank the reviewer for this valuable remark. In the revised manuscript, we have attempted to incorporate as many of the points raised during the review process as possible, particularly those that we felt would improve the clarity and completeness of the paper. For some comments, however, we decided not to include additional text because the relevant information is already available elsewhere in the manuscript or can be found in the referenced literature. In these cases, we sought to avoid unnecessary repetition and to maintain a concise and focused presentation.

- Overall: The number of figures even increased in the revised manuscript. The authors should carefully consider to move some of these figures to a supplement, in particular those figures which explain details, but not the key scientific message of this paper. Examples for these figures one could move to a supplement are the averaging kernels (Figures 1-2) or the time series of the ONI (Figure 11).

We thank the referee for this suggestion. We carefully considered moving these figures to the Supplementary Material. However, we believe that the averaging kernel figures should remain in the main text because this study presents the first scientific application of the IASI HCN retrieval product. In such cases, it is common practice to provide the retrieval characteristics and vertical sensitivity in the main manuscript, allowing readers to assess the quality and information content of the dataset used throughout the analysis.

We also prefer to retain the ONI time series in the main text. This figure provides important context for the comparison between the strong 2015/16 El Niño event and the more recent 2019 and 2023 events discussed in the paper. Keeping the ONI record readily visible allows readers to directly assess the relative strength and temporal evolution of these events without having to refer to supplementary material.

- The description of the ULIRS retrieval for CO and HCN is now very helpful. However, I suggest to include a table (e.g. to a supplement) with all important information (spectral region, used spectroscopy, interfering species (fitted or not), a priori, typical degrees of freedom) for CO and HCN at one point.

We thank the referee for this helpful suggestion. Accordingly, we have added Table S1 in the Supplementary Material, which summarize the main information on the ULIRS retrievals of CO and HCN.

- The authors answered my question about the TOMCAT resolution and vertical transport. This answer was very helpful for me, but I wonder why they chose not to include these information into section 2.1.4. (just one example for the first comment)

We thank the reviewer for this remark. Following this suggestion, we have included the relevant information from our previous discussion in Section 2.1.4 of the revised manuscript.

- L140: In the text, the authors changed the uncertainty of the HCN constraint from 300% to 100%. In the answer to my question, still 300% are mentioned. I am wondering, which number is correct. In case it really changed from 300% to 100%: Was that just a typo or did you reprocess the data with a different constraint?

We reduced the a priori constraint from 300% to 100%. The a priori used for the HCN was based on aircraft measurements within biomass burning plumes, so already including HCN values relevant to studying fire events. The reduction of a priori covariance restraint increased the number of converged retrievals, without significantly changing the overall quality of residual fit.

- L258: The answer to my question about the relation of horizontal resolutions of ERA-I and GFED was helpful, but I still think it should be mentioned in the text that the higher resolved GFED data is degraded to the lower TOMCAT resolution. (another example for the first comment).

We thank the reviewer for this suggestion. We believe that the treatment of the input datasets at the model resolution is already described in the model description section, where the horizontal resolution of TOMCAT and the processing of the input fields are specified.

- L315: The formulation "excellent overall agreement" seems a little odd for me.

We thank the reviewer for this suggestion. The text has been revised accordingly.

- Figure 8: What does "HCN204" in the title of the plots mean? Please define in the caption.

We thank the reviewer for pointing this out. "HCN204" was an unintended label originating from an older internal naming convention used during figure preparation. We have corrected this error in the revised manuscript and updated the figure accordingly to avoid any confusion.

- Equation (2) seems to be not present in the revised manuscript (technical issue?).

We thank the reviewer for noticing this issue. The omission of Equation (2) was due to a technical error in the LaTeX source during the manuscript preparation process. This error has now been corrected, and Equation (2) has been restored in the revised manuscript.

- The authors have nicely provided a complete list of links where to get the data from they used for this study. Only for the NDACC Maïdo data, the link seems to be missing "(...)".

We thank the reviewer for pointing this out. The link to the NDACC Maïdo data was inadvertently omitted from the data availability section. We have now added the correct link to the NDACC data platform in the revised manuscript.