Response to reviewer 2 comments

Bruno et al. show in their manuscript unusual strong HCN emissions due to peatland fire emissions during El-Nino periods. They compare IASI HCN to TOMCAT simulations and suggest a scaled HCN emission for September 2015. Further, they compare the extreme year 2015 with other El-Nino years, and connect HCN emissions with soil moisture and calculate enhancement ratios for HCN.

The manuscript is well within the scope of ACP, however it needs major improvements. In particular the objective and the structure of this manuscript us unclear to me, and statements in the conclusions are not well supported. In addition, the new IASI HCN data product used in this manuscript is currently unpublished and undocumented, which makes it difficult for me to evaluate this manuscript.

General comments:

- The reference "Moore et al., in preparation" is, according to the ACP submission guidelines (https://www.atmospheric-chemistry-and-physics.net/submission.html) allowed to use in the review stage of the submission, but referees need to have access to this unpublished work. This is not the case at the moment for me as referee, so the ACP guidelines are not met here, even though Copernicus staff pointed out that issue (see MS overview). Please provide the Moore et al. reference as asset. In particular, I need more information about the statements made about retrieval sensitivities in different atmospheric layers, which are essential to understand major findings of the current study.
We apologise for the confusion caused over this issue. On reflection, we have decided to take an alternative approach. The reference to Moore et al. has now been removed from the manuscript. Instead, we address the reviewer concerns by the inclusion of a new section validating the new HCN and updated CO products against the well-established NDACC network of ground-based FTIR instruments. These ground-based data have been used extensively by other IASI data producers (i.e. Kerzenmacher et al., 2012; Duflot et al, 2013, 2015). Due to the nature of the study and for brevity, we choose to include data from the Reunion Island station in the Indian Ocean. This is the closest site to the Indonesia region, but is also influenced by seasonal burning from African Savanna fires which allows us to critically test the CO and HCN across elevated and background scenes. We include total column averaging kernels from NDACC and the IASI ULIRS products in the paper to demonstrate and explain the vertical sensitivity similarities and differences. We also apply the IASI averaging kernels to the NDACC data to remove smoothing error so we can perform a fair "apples-to-apples" comparison with IASI.

Kerzenmacher, T., Dils, B., Kumps, N., Blumenstock, T., Clerbaux, C., Coheur, P.-F., Demoulin, P., García, O., George, M., Griffith, D. W. T., Hase, F., Hadji-Lazaro, J., Hurtmans, D., Jones, N., Mahieu, E., Notholt, J., Paton-Walsh, C., Raffalski, U., Ridder, T., Schneider, M., Servais, C., and De Mazière, M.: Validation of IASI FORLI carbon monoxide retrievals using FTIR data from NDACC, Atmos. Meas. Tech., 5, 2751–2761, https://doi.org/10.5194/amt-5-2751-2012, 2012.

Duflot, V., Hurtmans, D., Clarisse, L., R'honi, Y., Vigouroux, C., De Mazière, M., Mahieu, E., Servais, C., Clerbaux, C., and Coheur, P.-F.: Measurements of hydrogen cyanide (HCN) and acetylene (C2H2) from the Infrared Atmospheric Sounding Interferometer (IASI), Atmos. Meas. Tech., 6, 917–925, https://doi.org/10.5194/amt-6-917-2013, 2013.

Duflot, V., Wespes, C., Clarisse, L., Hurtmans, D., Ngadi, Y., Jones, N., Paton-Walsh, C., Hadji-Lazaro, J., Vigouroux, C., De Mazière, M., Metzger, J.-M., Mahieu, E., Servais, C., Hase, F., Schneider, M., Clerbaux, C., and Coheur, P.-F.: Acetylene (C2H2) and hydrogen cyanide (HCN) from IASI satellite observations: global distributions, validation, and comparison with model, Atmos. Chem. Phys., 15, 10509–10527, https://doi.org/10.5194/acp-15-10509-2015, 2015.

- The structure of the manuscript lacks consistency. In some sections (e.g. 3.1.1), the paragraphs of this section are barely connected to each other. In section 3, many sub-sections start again with details of the data product, which should have been introduced in section 2. Further, figures are sometimes only marginally introduced and barely discussed. Also, the order of appearance in the text is not the same as the numbering of the figures and the reader needs to jump from one figure to another quite often.

To improve the consistency and flow of the manuscript, a paragraph from Section 3.1.2 has been moved to Section 2.1.3. In addition, the numbering of the figures has been corrected so that it now matches the order of their discussion in the text.

- The objective of the paper is not clear to me. The first part reads as a model study showing that emissions should be improved for 2015 to better match IASI observations. Then enhancement ratios are calculated, and further years are investigated. In the end, again only 2015 is investigated regarding details of the fire processes and plume dynamics. Most of these activities are treated rather standalone, I am missing the connecting element in the manuscript.
The entire structure of the manuscript has been substantially revised in order

to address the referee's comments and to include a dedicated validation section for the IASI retrievals against independent measurements.

The revised manuscript is now organized as follows:

- **Introduction and description of the datasets used in the study;**
- **Validation of the CO and HCN retrievals against NDACC measurements;**
- **Analysis of the 2015 HCN emissions using satellite observations and comparison with TOMCAT model simulations performed with different emission setups;**
- **Estimation of new emission ratios (ER) and emission factors (EF) from satellite observations, together with an evaluation of the HCN correlation during the September–December 2015 period;**
- **Extension of the analysis to other El Niño years (2019 and 2023), including an assessment of possible hydrological drivers responsible for the observed differences;**
- **Analysis of the 2015 HCN emissions using satellite observations and comparison with TOMCAT model simulations performed with different**
- **Application of the new FINNpeatSM emission database to investigate the temporal relationship between burn depth and wildfire emissions.**

- The conclusions contain statements that are not covered by the findings discussed earlier in the manuscript. Please prepare all statements in the main part of the manuscript based on the presented data.

The conclusion now supports the changes made to the structure of the manuscript described in the previous comment.

- Given the rather small scales that can occur if fires are active and plumes are transported, I am wondering if the TOMCAT T42 horizontal resolution is good enough for this task. Also it is not clear to me why the older and coarser ERA-Interim reanalysis was chosen for nudging instead of the supreme ERA5 data set. What about vertical transport in the reanalysis, in particular in the tropics. Further, vertical transport of biomass burning plumes due to pyroconvection may occur - how was that handeled by the model?

The model horizontal resolution is 2.8° × 2.8°, which indeed is coarse. At this resolution the higher resolution of ERA5 over ERA-Interim is not relevant. Note that the model does not use the reanalysis vertical velocities directly. The model uses the reanalysis divergence, averaged onto the model horizontal grid, to diagnose the large-scale vertical advection. The model also has sub-grid scale treatments of convection and boundary layer mixing. There is no treatment of pyroconvection.

- The manuscript contains 17 figures of different quality. The authors should carefully think about which figures are really necessary to support their findings and move all other figures to a supplement. Figures used in the manuscript should have similar font sizes (same as font sizes of the text) and line widths. Figure 15 could be a nice example for the figure style (in my opinion).

All figures have been revised to ensure consistency in font sizes and line widths and to achieve the best possible fit within the page dimensions.

Specific comments:

- Abstract: Please define all used abbreviations and acronyms (e.g. CO, TOMCAT, GFED, FINNpeatSM).

All abbreviations and acronyms have been properly defined and edited throughout the entire manuscript.

- L76: "high spectral resolution and sampling": This is relative regarding the background of the reader. Some may have ground-based FTS systems in mind, others radiometers. Please briefly mention the typical IASI spectral resolution/sampling and also the spectral range employed by IASI.

We have included at statement clarifying the full IASI spectral range of 645-2760cm-1 a spectral resolution of 0.5cm-1 and a spectral sampling of 0.25 cm-1.

- L95: "For HCN, the a priori profile was constructed from INTEX-B aircraft information ...": I do not understand how the retrieval is using aircraft measurements prior 2009 for the retrievals. I guess the data was further processed before using it in the retrievals. Again, it is difficult to understand the quality of the data used in this study without the knowledge of Moore et al.

The a priori is used in the optimal estimation retrieval to provide a starting point for the retrieval and so the year of measurement is irrelevant here. Intex-B measurements were chosen as they provide measurements at a number of tropospheric levels through pollution events over North America (i.e. plume cut-throughs). As it was unlikely that these measurements would be truely representative of fires from different regions of the world, we included a loose constraint on the a priori profile of 300% uncertainty to reduce the constraint of using a single a priori profile.

- L120: Why is the information about the different latency products relevant for this section? For a study published years after the events discussed, I would always assume that final data is used instead of near-real-time data. In my opinion it is sufficient to mention that IMERG Final Run data was used together with the reference describing more details of different data products.

The text has been edited as suggested to improve clarity and relevance.

- L140: In this chapter, SWI is introduced (but the acronym is not defined here, only later in Figure 4). For a typical reader of ACP, it would be good to connect SWI to GWL, which are occasionally used together in the article.

The SWI subsection has been edited to properly define the acronym and to clarify its connection to GWL where relevant.

- L155: So the fire emissions are resolved much better than the meteorology driving the model (GRED: 0.25° vs ERA-I 0.75°)?

Yes, but the meteorology is in any case averaged down to the horizontal resolution of the model (2.8° × 2.8°).

- Figure 1: The color bar used is made for highlighting positive and negative differences around the center of the color bar. Here, however, absolute columns are shown and for columns around 1.5e16 molecules/cm2, it is difficult to discriminate this concentration from missing values. I suggest to change to a uniform color bar like viridis or turbo.

The figure caption has been corrected to specify that the map on the left-hand side shows 1 November 2015, and the color table has been changed to improve the visualization of absolute column values.

- Figure 1 right panel: Please explain why the regridded monthly mean data reveals these abrupt changes in total column at +- 20° latitude. Is this an instrument, data processing or plotting artifact?

Thank you for highlighting this issue. The apparent abrupt changes at ±20° latitude in the right panel of Figure 1 were not physically meaningful, but instead resulted from an error in how the regridded monthly mean data were handled during processing. This issue has now been identified and corrected, and the updated figure no longer exhibits these artificial discontinuities.

- Figure 2: Please explain abbreviations like "T_HCN" in the figure caption.
The figure caption has been edited to include explanations for "T_HCN."

- L210/Figure 3: Please mark these longitudes of the extent of Indonesia in the Plots in Figure 3.

We have added two lines to the plots to represent the approximate meridional extent of the Kalimantan and Sumatra regions.

- L215: At which altitudes is IASI sensitivity greatest? And I guess the last sentence of this paragraph refers again to Figure 2 as well as the first sentence of the following paragraph?

The text has been edited to clarify that IASI HCN sensitivity is greatest in the upper troposphere, between 8 and 12 km, and the relevant sentences have been revised for clarity.

- L227: In my opinion, this and the following paragraphs should be moved into Section 2.1.2

The relevant paragraphs have been moved to Section 2.1.3 as suggested.

- L246/Figure 5: The text says Fig. 5 shows all HCN emissions by the model, the figure caption says it only shows GFED emissions. What is really shown in Fig. 5? Further, it would be interesting to see which emission source is responsible for the suggested overestimation of HCN emissions in September. So it could be interesting to show the combined emissions together with GFED emissions only.

The original caption of Figure 5 already states "Monthly average HCN 2015 emissions (molecules cm^{-2} s^{-1}) based on GFED v4.1" and does not imply that only GFED emissions are shown. GFED is indeed the main component of HCN emissions in the region, while other sources do not significantly contribute to the September peak. The caption has been slightly edited for clarity.

- Figure 5 caption: "(molecules cm−2 s+−1)" The "+-1" looks strange here. Please use a more common way to indicate uncertainties (in case this was meant by the "+-1")

The "+-1" in the figure caption was a typo and not related to uncertainty. It has been corrected to read "(molecules cm^{-2} s^{-1})" using the proper formatting.

- Figure 6: Is there a good reason why Figure 1 shows data from 1 November 2015, while Figure 6 shows data from 2 November 2015? Further, I think a better comparison would be to grid the model data on the measurement geolocations (and apply the averaging kernel) instead of regridding the measurement data on the regular model horizontal grid. The comment about the color bar for Figure 1 also applies for this Figure.

The date difference between Figures 1 and 6 was a typo; both figures show data from 2 November 2015 and we apologize for the confusion. The colour table has been changed in both figures to improve visualization. Regarding the comparison, the model grid is coarser than the IASI measurements; therefore, regridding the measurements onto the model grid remains the preferred approach.

- Equation (2): What is meant by "smoke" here? Is this really only for smoke conditions (i.e. with large aerosol concentrations)? In that case, IASI would not be able to

measure these air masses and would be not suitable for this kind of study. Maybe the authors should instead refer to "polluted air" or "plume"?
The term "smoke" in Equation (2) has been replaced with "plume" as suggested.

- L283: What does a low correlation factor indicate here?
A low correlation factor in this instance implies cases where the CO and HCN values are unrelated, most likely to occur where the HCN in undetectable (i.e. measurement signal below the IASI detection limit). The 0.3 level was chosen to filter out these noisy data and ensure that the correlation between HCN and CO is strong enough to derive a reliable emission ratio (Whitburn et al., 2016).

Whitburn, S., M. Van Damme, L. Clarisse, S. Turquety, C. Clerbaux, and P.-F. Coheur (2016), Doubling of annual ammonia emissions from the peat fires in Indonesia during the 2015 El Niño, Geophys. Res. Lett., 43, 11,007–11,014, doi:10.1002/2016GL070620.

- Table 1: I am not sure if I understand this table correctly: The first four columns are cited from the authors which are mentioned in column 1? And the columns with EF_HCN are derived using findings from this study and equation (3)? In this case the "author" column is kind of misleading for the EF_HCN columns. Further, I am not sure why there are EF_HCN results for so many different correlation coefficients presented. Again: What does the correlation coefficient tell me here?

The table and corresponding text have been updated to make the work and significance clearer to the reader. The values of EF_{CO} vary a lot in the literature, even when burning peat from the same region of the world. This variability is likely due to differences in peat water content of just local peat composition variation. To be characterise a range of scenarios for our estimation of EF_{HCN} we need to consider this EF_{CO} variability. To add a further level of complexity, we have demonstrated that the relationship between IASI-measured CO and HCN varies across the late 2015 burning period (the linear correlation coefficient). In the table we show that by considering this relationship also, we can see a distinct difference in the calculated EF_{HCN} between a fairly low r value (0.5 here taken as meaning low) and the higher r values (r>0.8). The differences can be up to 50% and are not usually considered in other studies which use satellite data to derive EF.

- L319: Since MLS is mentioned here: Since MLS measured HCN and CO vertically resolved in 2015 - wouldn't it be useful to include this data into this study?
The reference to MLS from Field et al. (2016) was used to highlight that the air parcels from the Indonesia fires were being uplifted during the

September/October 2015 period. This is relevant as we know that the HCN sensitivity peaks between 8 and 12 km, so when higher HCN values are uplifted to these altitudes the measured concentration of HCN will increase. We highlight that this corresponds to the highest HCN:CO correlation.

- L321: "Before mid-October, we likely underestimate the HCN amounts as plumes are closer to the surface.": Does that mean that the original emissions (blue curve in Fig. 5) could be correct and only the poor sensitivity of IASI underestimates the true amounts of HCN here?

The IASI averaged total column is compared with the TOMCAT model outputs smoothed using the IASI averaging kernels, so it takes into account the lower IASI sensitivity close to the surface. Despite that it still shows a significant difference during September-early October period. We have added the following sentence: "This alone cannot explain the large discrepancies between the IASI measurements and the TOMCAT simulations observed in Figure 3, since the TOMCAT outputs have already been smoothed using the IASI averaging kernels to ensure comparable sensitivity."

- L325: Please give a reference for the de-facto standard.

The text has been rephrased, and a reference to NOAA pages has been added.

- L327: Figures 9 and 11 are mentioned earlier than Figure 8. Please adjust the order of figures.

The order of figure mentions has been corrected so that former Figures 8, 9, and 11 are cited sequentially in the text.

- L343: Why is the HCN burden introduced? How was the burden calculated? What does the burden give as additional information compared to the columns shown before? Figure 10 again shows the CO columns in the same style as the HCN burdens in Figure 8, so there is a chance for misinterpretation.

Tracking the HCN burden (and indeed the CO burden) tells us three critical things about the observations: tracks regional wildfire activity, marks the specific burning such as peat fires, how the emissions or atmospheric load is changing over time. There are caveats in that particularly for HCN the sensitivity to HCN in the IASI measurements is in the upper troposphere and lower stratosphere, so there is a reliance on vertical transport. This also related back to the earlier point why we put emphasis on the correlation coefficient between HCN and CO in an air parcel. Both the HCN and CO burdens are merged into the same figure panel for clarity and easy comparison for the reader. The burden calculation is used in many IASI studies (i.e. R'honi et al., 2013; Pope et al. ,2021). We add a reference to R'honi et al. in the paper.

$TM_X = M_X C_X S/Na$

TM_x = burden (Gg or Tg)
M_x= molar mass of HCN or CO in g mol^{-1}
C_x = mean columns in molec cm^{-2}
Na = Avogadro Number: 6.0221415e23
S = surface area (in cm^2)

R'Honi, Y., Clarisse, L., Clerbaux, C., Hurtmans, D., Duflot, V., Turquety, S., Ngadi, Y., and Coheur, P.-F.: Exceptional emissions of NH_3 and HCOOH in the 2010 Russian wildfires, Atmos. Chem. Phys., 13, 4171–4181, https://doi.org/10.5194/acp-13-4171-2013, 2013.

Pope, R. J., Kerridge, B. J., Siddans, R., Latter, B. G., Chipperfield, M. P., Arnold, S. R., et al. (2021). Large enhancements in Southern Hemisphere satellite-observed trace gases due to the 2019/2020 Australian wildfires. Journal of Geophysical Research: Atmospheres, 126, e2021JD034892. https://doi.org/10.1029/2021JD034892

- Figure 11: What is the advantage of showing monthly fire radiative power in a cumulative way?

We show the cumulative fire radiative power rather than daily maps as it provides a clearer indication of wildfire activity across the whole season which allows for better assessment of total impact and trend and is a tool to provide a distinction of the fire behaviour across the three years in the study.

- L422: Why is a cross-correlation analysis performed? How was that analysis performed? Why does this analysis help the reader to understand the temporal dynamics of plume transport better? The whole paragraph should be motivated and explained better. From the paragraph and Figure 17, I can hardly tell what "Lag" is shown here on the x-axis.

The cross-correlation analysis has been used to investigate the temporal relationship between burn depth and the gas-averaged total column, with the aim of estimating the timescale of plume transport from the surface to the mid- to upper troposphere.

The paragraph has been edited to better motivate and explain the cross-correlation analysis. A more detailed description of the method has been added, and the definition of "Lag" used on the x-axis of the figure has been clarified to help the reader understand the temporal dynamics of plume transport.

- L427: I think the peak is at -10 days instead of 10?

The paragraph has been edited for clarity to better describe the position of the peaks.

- L428: Earlier in the manuscript, it was stated that IASI has different maximum sensitivity altitudes for HCN and CO, here both species are mentioned together.

The sentence has been edited for clarity to accurately reflect the different maximum sensitivity altitudes of IASI for HCN and CO.

- L430: Again, it should be -10 instead of +10.

See previous comment.

- L452: The HCN EFs provided by this study heavily rely on previous work, which provided the CO EFs. Is there a recommended range for HCN EF? This recommendation would make the current manuscript very useful for readers looking for HCN EFs, instead of only providing the table with a set of different EFs.

The paper aims to demonstrate the range of HCN EFs achieved by the satellite data compared to lab-based estimates by burning peat collected from Indonesia. We wanted to show the range of EFs across the season which were observed in our IASI data, via combination with the correlation between the two species. As noted earlier for satellite studies it is generally not explored how the EF sensitivity changes depending on the relationship between the two target gases (here HCN:CO). In table 1 we present the range of values and discuss how these compare with lab-derived values of the HCN EF.

- L455: I would argue that this work only showed that GFED emissions are overestimating HCN emissions in September 2015. The rather general formulation in this paragraph is not supported by the findings of this manuscript.

Thank you for this comment. As discussed in Section 2, our results are consistent with the findings of Nechita-Banda et al. (2018), who demonstrated that GFED performs poorly in representing emissions from peat fires during the 2015 Indonesian wildfire season. In particular, their work highlights limitations arising from the reliance on burned area and fire radiative power (FRP), which are not well suited to capturing the prolonged subsurface smouldering characteristic of peat combustion. In combination with our analysis of HCN emissions, these insights further support our conclusion that GFED-based inventories misrepresent peat fire emissions and associated trace gas release. We have revised the manuscript text to improve clarity and to better reflect this connection.

Nechita-Banda, N., Krol, M., van der Werf, G. R., Kaiser, J. W., Pandey, S., Huijnen, V., Clerbaux, C., Coheur, P., Deeter, M. N., and Röckmann, T.: Monitoring emissions from the 2015 Indonesian fires using CO satellite data, Philosophical Transactions of the Royal Society B: Biological Sciences, 373, 20170 307, https://doi.org/10.1098/rstb.2017.0307, 2018.

- L460: I think I missed the part of the manuscript, where the FINNpeatSM emissions were used in TOMCAT to show that these match better the observations than the GFED emissions with scaling applied in September 2015?

We are sorry that this was not clear in the manuscript. We use GFEDv4s emissions in the TOMCAT model because FINNpeatSM does not include emissions for HCN. GFEDv4s uses a simple parameterisation to represent changes in burn depth between years. However, GFEDv4s does not provide any mechanism to investigate the influence of soil moisture and burn depth between different years, as we have done here using FINNpeatSM. Our analysis confirms that the new FINNpeatSM approach, which explicitly links burn depth to soil moisture, offers a more accurate description of the dynamics of peat fire emissions than GFEDv4s, as was also found in Kiely et al. (2019). To address this limitation in GFEDv4s, the new version of GFED (GFEDv5) now includes a more sophisticated representation of burn depth, which directly links satellite retrieved soil moisture to burn depth observations (van Wees, 2022).

van Wees D, van der Werf GR, Randerson JT, Rogers BM, Chen Y, Veraverbeke S, Giglio L, Morton DC. Global biomass burning fuel consumption and emissions at 500 m spatial resolution based on the Global Fire Emissions Database (GFED). Geoscientific Model Development. 2022 Nov 21;15(22):8411-37.

To address the reviewer comment, we have clarified this in the text:

Peatland fire emissions from GFEDv4s include a simple representation of burn depth based on soil moisture. However, it is not possible to use the dataset to explore soil moisture and burn depth interactions across different years. This limitation restricts our ability to investigate the drivers of differences in HCN retrievals between 2015, 2019, and 2023. To address this limitation in GFEDv4s, we examine fire detections (from VIIRS fire hotpots) and Soil Moisture Active Passive Data (SMAP, \citet{SMAP_2021}) over Indonesia peatlands. Using the FINNpeatSM method \citep{kiely_2019}, we can combine these datasets to estimate the burn depth of fires in 2015, 2019 and 2023.

- L480: I am missing here a platform for the VNP14IMG data (e.g. a website or if available a DOI). Further, are the IASI, IMERG, SMAP, SWI, GFED, FINNpeatSM data available? Please add these information.

Information has been added regarding the VNP14IMG platform, as well as the availability of IASI, IMERG, SWI, SMAP, and GFED4 data.

- L482: The TOMCAT data on Zenodo only contains HCN loss and gain values (HCN1-12), but (as long as I understand the file format) no HCN concentrations or columns, as they were used in the manuscript. Please also provide these data or improve the file format (e.g. using CF standards), so I can find these data entries in the files.

A new version of the TOMCAT data has been uploaded with clear naming, including HCN columns to improve usability.