Response to reviewer 1 comments

Bruno et al. analyse the conditions that drive Indonesian peat fire emissions. They identify the importance of hydrological conditions like soil water content and precipitation in addition to El Niño. While the results are interesting and advance our understanding of Indonesian peatland fires, the study misses key aspects required for publication in ACP. At its core, the study relies on newly derived CO and HCN retrievals based on IASI observations. The authors fail to provide a satisfactory explanation as to why alternative retrievals, in addition to the methodologies that are already in place, are required. No retrieval data are provided, the retrieval description is insufficient, and the retrievals have not been evaluated. The only reference provided is to another paper by the authors which is 'in preparation'. I recommend rejecting the manuscript for the time being, and reconsidering it once the new IASI retrievals for CO and HCN have been published and evaluated, and the retrieval method has undergone proper peer review. Before reconsidering the manuscript after this process has been finished, the authors need to address the following comments.

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 include a section describing the IASI retrieval fully, as well as a section validating the new HCN and updated CO products against NDACC measurements.

The ULIRS code dates back to 2011 and was originally focussed solely on CO (Illingworth et al., 2011). This algorithm was recently used to generate CO IASI data as part of the UK's Earth Observation Climate Information Service (EOCIS). These data can be accessed at https://dx.doi.org/10.5285/4b31d47716604b9f84714fab39ce973c. The ULIRS code has been peer-reviewed, and more recently we have begun investigating other trace gases such as carbonyl sulfide (Cartwright et al., 2025). This is the first time we have presented work on HCN; data will be provided on zenodo.

In terms of current IASI retrieval schemes, the most widespread is the ANNI (Artificial Neural Network for IASI) scheme. Our scheme provides an important point of difference because it utilises an optimal estimation methodology.

Illingworth, S. M., Remedios, J. J., Boesch, H., Moore, D. P., Sembhi, H., Dudhia, A., and Walker, J. C.: ULIRS, an optimal estimation retrieval scheme for carbon monoxide using IASI spectral radiances: sensitivity analysis, error budget and simulations, Atmos. Meas. Tech., 4, 269–288, https://doi.org/10.5194/amt-4-269-2011, 2011.

Cartwright, M. P., Harrison, J. J., Moore, D. P., Pope, R. J., Chipperfield, M. P., Wilson, C., and Feng, W.: Global optimal estimation retrievals of atmospheric carbonyl sulfide over water from IASI measurement spectra for 2018, Atmos. Chem. Phys., 25, 15913–15934, https://doi.org/10.5194/acp-25-15913-2025, 2025.

Further major comments:

I have major concerns about the TOMCAT model setup used, as it seems inadequate for the analysis performed:

(1) The authors themselves highlight the importance of circulation patterns and vertical transport (e.g. Figure 3), which are based on ERA5 data. However, their model setup relies on outdated ERA-Interim reanalysis data for meteorological forcing, which lacks improved vertical resolution (Hersbach et al., 2020). I strongly suggest switching to ERA5 reanalysis data as the meteorological driver. This would increase the temporal resolution of the meteorological input from 6 hours to hourly, greatly improving the representation of transport processes.
The reviewer should note that a paper on the modelling of HCN in the atmosphere using TOMCAT, forced by ERA-Interim, was published by (largely) the same group of authors several years ago. This publication revised the ocean and atmospheric sinks. This implementation of the model was also validated against NDACC and ACE-FTS data. As the present study can be considered a follow-up to this previous work, for consistency we kept the model setup the same.

To address the reviewer's concerns, we would like to point out that the resolution (vertical and horizontal) of TOMCAT is completely independent of the forcing analyses. The ECMWF fields are averaged onto the model grid. In the case of the vertical transport, the analysis ECMWF divergence fields are integrated over the model levels to derive the vertical velocities. This ensures that the model is consistent with the analysis. ERA5 does have a higher time resolution but global off-line CTMs do not necessarily use this full resolution. The meteorological database required would be very large. In the case of TOMCAT, even if using ERA5 we would need to take the analyses at a reduced time resolution.

Additionally, there has also been a previous study (Li et al, 2022) comparing the seasonal behaviour and long-term trends in total column ozone and mean age of air using data from TOMCAT, forced by both ERA-Interim and ERA5. Although for a different chemical species, the study indicates weaknesses in both reanalyses compared to observational data.

It is also interesting to note that ERA-Interim has been demonstrated to provide better age of air estimates (Vogel et al, 2024) than ERA5 for young air masses, such as found in the tropics.

Li, Y., Dhomse, S. S., Chipperfield, M. P., Feng, W., Chrysanthou, A., Xia, Y., and Guo, D.: Effects of reanalysis forcing fields on ozone trends and age of air from a chemical transport model, Atmos. Chem. Phys., 22, 10635–10656, https://doi.org/10.5194/acp-22-10635-2022, 2022.

Vogel, B., Volk, C. M., Wintel, J., Lauther, V., Clemens, J., Grooß, J.-U., Günther, G., Hoffmann, L., Laube, J. C., Müller, R., Ploeger, F., and Stroh, F.: Evaluation of vertical transport in ERA5 and ERA-Interim reanalysis using high-altitude aircraft measurements in the Asian summer monsoon 2017, Atmos. Chem. Phys., 24, 317–343, https://doi.org/10.5194/acp-24-317-2024, 2024.

(2) During the 2015 peatland fires, the Asian monsoon anticyclone was ongoing. The model resolution used in this study (T42, 2.8° by 2.8°) is too coarse to adequately resolve these transport patterns. Please increase the model resolution as frequently employed in global model studies.

As noted above, the TOMCAT model resolution is independent of the meteorological analyses used to force the model. Moreover, in contrast to a dynamical model which needs sufficient resolution to generate circulation patterns, an off-line CTM can make full use of the information in the analyses, subject to the CTM resolution. The ECWMF reanalyses (including ERA-Interim) do capture the Asian monsoon anticyclone, so it is a question of the model resolution for the tracers. TOMCAT uses the Prather (1986) second-order moments advection scheme which stores subgridscale tracer information and performs well in generating and maintaining tracer gradients.

(3) The authors state that the TOMCAT model uses monthly averaged emissions, which are then resampled in time. This approach may be valid for emission sources that are not highly time-variable, but it is inappropriate for fire emissions due to their high time-variability. How is this interpolation performed for peatland emissions, and how representative is this approach? Why not rely on datasets that include daily wildfire emissions based on daily fire activity (e.g. GFEDv5; van der Werf et al., 2025)? What influence does your approach have on the overestimation of HCN emissions in September 2015?

At the time we undertook this study, there was no available database with daily HCN emissions, so we had no choice but to follow the approach of interpolating the monthly emissions linearly between the mid-points of consecutive months, the time at which the monthly values are assumed to apply. GFED5 was released in October 2025, after the present analysis was completed.

The outline and style of the current version need improvement. The order in which the figures are presented does not reflect their usage in the main body of the text. Section 2 provides a simple listing of all the datasets used in this study. The manuscript would benefit greatly if, at the beginning of Section 2, an overview was provided illustrating which datasets will be used and how they will be analysed. For example, the use of the precipitation and soil moisture data is unclear, as is its relevance to the study which only becomes evident later on.
We have revised the manuscript to improve the structure and clarity of Section 2. The order of the figures has been corrected so that it now follows the sequence in which they are discussed in the main text. In addition, we have added an introductory paragraph at the beginning of Section 2 providing an overview of the datasets used in this study and briefly explaining how each dataset is employed in the analysis.

Minor comments:

- Overall, not all abbreviations are properly introduced. In particular, the abstract introduces some abbreviations, while others are assumed to be familiar.
We have carefully revised the manuscript to ensure that all abbreviations are properly introduced and defined when they first occur.

- Lines 28–30: Please provide references for these statements.
A reference has now been added to support these statements. Specifically, we cite Watson et al. (2019).

Watson, J. G., Cao, J., Chen, L.-W. A., Wang, Q., Tian, J., Wang, X., Gronstal, S., Ho, S. S. H., Watts, A. C., and Chow, J. C.: Gaseous, PM2.5 mass, and speciated emission factors from laboratory chamber peat combustion, Atmospheric Chemistry and Physics, 19, 14 173–14 193, https://doi.org/10.5194/acp-19-14173-2019, 2019

- Line 51: HCN has already been introduced on line 29.
Thank you for pointing this out. The text has been edited to correct the repeated introduction of HCN.

- Lines 51–53: Provide reasons and references explaining why these are poorly suited.
The paragraph has been revised to clarify the reasons why burned-area estimates are poorly suited, and relevant references have been added. In particular, we now cite Nechita-Banda et al. (2018), Lohberger et al. (2018), and Hooijer and Vernimmen (2013).

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.

Lohberger, S., Stängel, M., Atwood, E. C., and Siegert, F.: Spatial evaluation of Indonesia's 2015 fire-affected area and estimated carbon emissions using Sentinel-1, Global Change Biology, 24, 644–654, https://doi.org/10.1111/gcb.13841, 2018.

Hooijer, A. and Vernimmen, R.: Peatland maps for Indonesia. Including accuracy assessment and recommendations for improvement, elevation mapping and evaluation of future flood risk. Quick Assessment and Nationwide Screening (QANS) of Peat and Lowland Resources and Action Planning for the Implementation of a National Lowland Strategy - PVW3A10002., Agentschap NL 6201068 QANS Lowland Development, for Government of Indonesia and Partners for Water (Netherlands), 2013.

- Line 95: How does the TOMCAT model compare to other atmospheric chemistry models for the troposphere? What uncertainty in the retrieval would be introduced if another model would be used?

TOMCAT has participated in many model-model tropospheric chemistry transport comparisons and has performed well, such the following:

Ma, J., M. Remaud, P. Peylin, P. Patra, Y. Niwa, C. Rodenbeck, M. Cartwright, J.J. Harrison, M.P. Chipperfield, R.J. Pope, C. Wilson, S. Belviso, S.A. Montzka, I. Vimont, F. Moore, E.L. Atlas, E. Schwartz and M.C. Krol, Intercomparison of atmospheric carbonyl sulfide (TransCom-COS): 2. Evaluation of Optimized Fluxes Using Ground-Based and Aircraft Observations, J. Geophys. Res., 128, e2023JD039198, doi:10.1029/2023JD039198, 2023.

Remaud, M., J. Ma, M. Krol, C. Abadie, M.P. Cartwright, P. Patra, Y. Niwa, C. Rodenbeck, S. Belviso, L. Kooijmans, S. Lennartz, F. Maignan, F. Chevallier, M.P. Chipperfield, R.J. Pope, J.J. Harrison, I. Vimont, C. Wilson and P. Peylin, Intercomparison of atmospheric carbonyl sulfide (TransCom-COS; Part One): Evaluating the impact of transport and emissions on tropospheric variability, J. Geophys. Res., 128, e2022JD037817, doi:10.1029/2022JD037817, 2023.

Krol, M., M. de Bruine, L. Killaars, H. Ouwersloot, A. Pozzer, Y. Yin, F. Chevallier, P. Bousquet, P. Patra, D. Belikov, S. Maksyutov, S. Dhomse, W. Feng and M.P.

Chipperfield, Age of Air as a diagnostic for transport time-scales in global models Geosci. Model Dev., 11, 3109-3130, doi:10.5194/gmd-2017-262, 2018.

Thompson, R.L., P.K. Patra, K. Ishijima, E. Saikawa, M. Corazza, U. Karstens, C. Wilson, P. Bergamaschi, E. Dlugokencky, C. Sweeney, R.G. Prinn, R.F. Weiss, S. O'Doherty, P.J. Fraser, L.P. Steele, P.B. Krummel, M. Saunois, M. Chipperfield and P. Bousquet, TransCom N2O model inter-comparison Part 1: Assessing the influence of transport and surface fluxes on tropospheric N2O variability, Atmos. Chem. Phys., 14, 4349-4368, doi:10.5194/acp-14-4349-2014, 2014.

The CO a priori has an associated covariance to allow for variability in the retrieved profile. The use of TOMCAT for the lower portion of the CO a priori profile is not expected to contribute any more uncertainty than using a different model, although it's not quite clear what the reviewer really means by "uncertainty" here. In regions, e.g. near the surface, where the sensitivity is weakest, this part of the retrieved profile is going to be dominated by the a priori, however this is always taken into account when comparing with a model or NDACC data by smoothing the latter with the averaging kernels.

- Lines 93–97: How representative are the derived profiles?

The choice of a priori profiles for CO and HCN were carefully considered and addressed different considerations for either gas. For CO, the use of a single global a priori, with associated uncertainty, was used to exclude any bias in the resulting retrieval that could be attributed to changes in a priori as a function of latitude. For HCN, we have a sensitivity question to consider in the retrieval setup where HCN is only likely visible above the IASI instrument noise in scenarios where we observe an event such as a fire plume. Intex-B aircraft flights provide a number of vertically-resolved measurements of trace gas. We also apply a suitably large a priori covariance on HCN to weight the retrieval more to the IASI measurement and reduce a priori influence.

- Line 109: This is unclear. Does 'fixed' refer to a fixed yearly emission profile, or are the emissions averaged over a year and then fixed?

Here the word fixed means a single repeating year of the same emissions. Within a year the emissions vary in time and space.

- Lines 111–113: Provide details on the HCN chemistry used, as well as on other physical loss processes.

Additional information on the HCN chemistry and other physical loss processes has been included. To improve the structure of the manuscript, the relevant paragraph has been moved from Section 3.1.2 to Section 2.1.3.

- Lines 157–159: Please provide a source for this statement.

The text has been revised to include an appropriate reference supporting this statement. In particular, we have added a citation to Akagi et al. (2011).

Akagi, S. K., Yokelson, R. J., Wiedinmyer, C., Alvarado, M. J., Reid, J. S., Karl, T., Crounse, J. D., and Wennberg, P. O.: Emission factors for open and domestic biomass burning for use in atmospheric models, Atmospheric Chemistry and Physics, 11, 4039–4072, https://doi.org/10.5194/acp-11-4039-2011, 2011.

- Line 197: The plume of HCN is clearly visible in the 'left' panel, not the 'right' panel, of Figure 1.

The text has been edited to correct the reference to the appropriate panel in this figure.

- Lines 197–198: This statement reads more like a report than a scientific paper.

The text has been edited to improve the tone and style.

- Figure 1: How is the monthly average calculated? How is it ensured that the monthly average at a given location is not skewed towards a single observation, or a few observations, if all other observations within the month have been invalid due to for example cloud cover?

The analyzed box region is sufficiently large to encompass multiple swaths and, in cases of extensive smoke plumes or cloud cover, to include a substantial number of measurements. However, the reviewer needs to realise that the purpose of this plot is not quantitative. This is purely a qualitative plot aimed at showing the strong presence of HCN in the monthly average.

- Figure 2: Why are there two gaps between November and December? Was no output created for these periods by TOMCAT?

The two gaps correspond to periods with missing IASI measurements. These gaps are also present in the TOMCAT time series because the TOMCAT total column is calculated from model profiles smoothed using the IASI averaging kernels. When IASI averaging kernels are not available, the smoothing of the model outputs cannot be performed. To clarify this point, we have added the following sentence to the manuscript: "The IASI time series exhibits two gaps in November 2015 due to missing measurements. These same gaps also appear in the model time series, as the absence of IASI averaging kernels during this period prevents the smoothing of the model outputs."

- Line 212: Please introduce the ERA5 data in Section 2.

We have included a short paragraph introducing the ERA5 data used in our study.

- Lines 227–237: Should this not be included in Section 2.1.2?

The paragraphs have been moved to Section 2.1.3 to improve the organization and flow of the manuscript.

- Line 231: Is the daily sample from the model the same as the IASI overpass time? Does the model provide averaged or instantaneous data?

The model outputs are produced every 6 hours following the ERA-Interim temporal resolution and are provided in the UTC time system. Consequently, the simulations were performed at the standard model output times rather than at the exact local overpass time of the IASI instrument, while aiming to remain as close as possible to the two daily IASI observations.

However, HCN has a relatively long atmospheric lifetime; therefore, differences of a few hours between the model output times and the satellite observations are not expected to significantly affect the comparison.

- Line 241: Replace 'observed' with 'simulated'.

The term "observed" has been replaced with "simulated" as suggested.

- Line 253: This 75% reduction comes out of the blue. How was this number obtained? What could be the reason for such a significant overestimation of emissions?

A reference has been added to clarify the origin of the 75% reduction and the reasoning behind it. Specifically, we now cite Bruno (2024), which details the scaling applied to account for overestimation of emissions. As explained in the manuscript, the large overestimation is due to problems in determining emissions from large peat fires (Nechita-Banda et al., 2018).

Bruno, A. G.: Investigating The Trace Gas Emissions Of Biomass Burning In The Earth System, Thesis, University of Leicester, https://doi.org/10.25392/leicester.data.25680108.v1, 2024.

- Figure 3: Please ensure that the x-axis and y-axis labels are shared across all subplots. Remove the colour bars from (a), (c) and (e).

The plot has been remade with only a single colour bar at the bottom of the figure.

- Figure 3: Comment on how well ERA5 represents vertical velocities at the equator.

It is the question of scales which is relevant to this study. ERA5 is able to resolve mesoscale overturning (i.e. https://doi.org/10.1029/2019GL085333) which is the focus of the study, rather than microscale processing. It is known that ERA5 underestimates the magnitude of higher winds in the Inter-tropical convergence zone (ITCZ) in individual convective events, however we are aiming to capture the larger scale behaviour of the monthly vertical winds across the 3 years of our study.

- Figure 4: Add labels (a) and (b).

Labels (a) and (b) have been added to the figure as suggested.

- Figure 5: Correct the unit in the caption.

The unit in the caption has been corrected as suggested.

- Line 270: Please provide a reference for this statement.

A reference is already provided for this statement in the text, specifically Nechita-Banda et al. (2018).

<u>Response to reviewer 2 comments</u>

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.