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).