

Review comments on Tsvlidou et al.: *Tropical tropospheric ozone and carbon monoxide distributions: characteristics, origins and control factors, as seen by IAGOS and IASI*

General comments

Tsvlidou et al. present a valuable study adding to the understanding of CO/O₃ distributions in the tropics. I very much appreciate the efforts the authors have gone through in their data analysis. I think the trajectory approach in the SOFT-IO model to disentangle the contribution of wildfire and anthropogenic emissions to the observed signals is sound and sufficiently well documented by Sauvage et al. (2017) to be used here.

However, I think that the manuscript is not yet suited for publication but needs to be significantly shortened and made more concise. A great part of the result section is dedicated to the lengthy and extremely detailed description of the observed and modelled profiles. Unfortunately the descriptions are difficult to follow, jumping back and forth between regions/locations, altitude regimes and the different figures, with the structure of the discussion often remaining unclear. Please comprise details and have a more structured discussion clearly presenting the similarities/differences between the regions and altitude regimes. The most relevant figures in the draft are Fig. 9 and Fig. 10. Given their importance and content they are not sufficiently discussed although they actually summarize the results of the preceding lengthy and detailed discussion.

Specific comments

- There are too many abbreviations used. I understand that this is an attempt to keep it short, but to a degree that the text gets close to unreadable. Please use full wording more often. In particular, the letter *T* is used in the abbreviations for *tropics* and *troposphere* which makes it even more difficult to keep things sorted. The way abbreviations are embedded into the text sometimes seems strange with

regard to grammar.

- I was confused by the term ‘observational site’ being used for a moving platform. ‘Locations’ would be more appropriate in my opinion. The term ‘site’ is usually used for a (temporarily) fixed installation of measurements equipment in one place.
- I trust the ACP editorial team will eventually take care of this but the usage of italics in subscripts and units is inconsistent and wrong in many instances.
- it is confusing to have both an appendix and a supplementary document
- Line 522: Table 2 referenced here is not part of the draft.

Abstract

In my opinion the abstract is too long and not well organized. The main findings are unclear. I suggest to remove some details and make the abstract more concise. It should become clear what the main conclusions of the analysis are and why these are relevant.

- L 11: ‘in above 6 km’ does not make any sense
- L 13: What do you mean by ‘The highest amount of transported CO’. Transport to Asia? Overall?

Introduction

- L 32: Why is stratospheric influence as the least important process mentioned first?
- L 37: I understand biomass burning throughout the manuscript refers to wildfires excluding usage of biogenic fuels which is attributed to anthropogenic emissions. This should be made clear here.

- L 59 constraint → constrain
- L 68: ‘offered’ – the choice of word reads strange here

Data and Methods

- Line 121f: what do you mean by ‘a distance criteria of 300-km’?
- Line 165: what do you mean by ‘with bias lower than 10-15 ppb’? Please specify what *bias* exactly refers to here.
- Line 173f: percentages are not absolute differences. The statement does not make any sense to me.
- Line 177: Why two? Which two backgrounds are referred here? The two mentioned pressure surfaces?

Results

All vertical profiles are discussed in terms of absolute altitude but in subsection 3.2. the different altitudes of the inbound/outbound airports are mentioned. In particular for the peaking altitudes presented in Fig. 8, I wonder what the results looked like if altitude differences relative to the local ground level were used.

- Line 234: my reading from the figure would be $62+6=68$
- Line 240 and 248: Throughout the manuscript mixing ratios are discussed, not concentrations. Similar on several instances in the following.
- Line 246: the ‘observed anomaly’ to me is not evident in the figure.
- Line 255f: no need to cite Adon et al. twice within two lines. Skip first one.
- Line 258: there is no obvious peak in the CO profile in the figures
- L265: space missing between brackets

- L271: Reference to Fig 2l does not make sense, Fig. 2l shows CO.
- L445: If the vertical layers are defined on pressure as the vertical coordinate then why are km shown in the figures?

Figures

Overall, there are too many figures with too many panels and too small fonts.

The presentation of observations on an absolute mixing ratio scale and the modelled contributions as ΔCO is difficult to compare. Why are the vertical profiles shown not background corrected?

- Fig. 2: I suggest to have the panel labels in some lighter colour to make them visible.
- Figures 3,5,6, 7 are poor resolution and cannot be zoomed which is essential given the small panels and fonts.
- Figure 8: I was wondering about the order in which locations are presented on the x-axis. It would be logical to have the locations by longitude which does not seem to be the case.

Summary

The Summary largely rephrases the detailed discussion from above. Conclusions are presented alongside but are not worked out well. Please be more precise and separate the shortened descriptive discussion of the observations from the conclusions drawn.

- Line 611 should be ‘NT’ only