Review of Froidevaux et al. about ozone and carbon monoxide trends in the tropical upper troposphere

Froidevaux et al. presents ozone (O3) and carbon monoxide (CO) trends in the tropical upper troposphere observed by the Microwave Limb Sounder (MLS) satellite instruments and in comparison with simulations from different models. Although this study should have required an important effort from the authors, I found a number of limitations which would make the review process difficult to complete in a single revision of this manuscript.

Major Comments

I find this paper is very difficult to read and for different reasons. First, the paper has too many objectives. It discusses the trend of O3 and CO of MLS, but also from different models, which is already broad. Would that make sense to limit the paper to only one species, or only on the MLS trend without including comparison with models? Discussion of the trends are also difficult to read because the authors address many regions in comparison with previous study. I think the paper need to be restructured by having two distinct sections for O3 and CO (if not resubmitting two distinct papers), those being split in (1) presenting the MLS trends, (2) comparing the MLS trends with models, (3) discussing the results w.r.t. existing literature where a table or a figure compiling the different results might support the text.

During the first reading of the paper, it was not clear how many model simulations were used, two or three? One of the reasons is that two models are used, but one of them is used with two different configurations. It would be clearer to say that the paper is using three model simulations, to label these simulations clearly with label choice different from the name of the model, and to use these labels instead of the model name in the paper.

Throughout the reading of the paper, I did not find a clear motivation for this study. While the introduction discuss the processes affecting O3 and CO in the tropical upper troposphere, why O3 and CO trends matter in this region? Moreover, I do not see a clear take home message from the conclusions. At some point, splitting Sect. 4 in two would help in that sense. Also, while there is a discussion of the evaluation of MLS data, I did not find such a discussion about the models especially in the regions surrounding the tropical upper troposphere. In particular, how models represent surface observation of CO? This could help to interpret the difference between models and MLS CO in the tropical upper troposphere. In the case a model does not have a good representation of the regions surrounding the tropical upper troposphere, I would exclude it from this study in order to simplify your message.

When discussing differences between model and MLS and/or their trends, it is very important to make sure that differences and/or trends are significant. When citing trends from other papers, make sure they are significant at the 2-sigma level (and avoid citing no-significant trends). There are many discussions where this is not clear that it is the case (in particular around the hatched regions in Fig. 12 et 14). If these cases need to be discussed, then it should be justified, e.g. because other studies based on other datasets show differences and/or trends which are significant. Otherwise, I would not discuss these cases because they does not help to simplify the whole message of the paper.

Other General Comments

1. The multivariate linear regression method is quickly introduced in Sect. 2.3 where the reader is pointing to the Appendix 3 of Froidevaux et al. (2019) which is fine. However, the choice of the different proxies that are used in this study, and the way they are connected to the trend analysis carried out in the paper should be reminded to the reader (e.g. the connection between CO from biomass burning and ENSO).
2. MLS O3 profiles display vertical oscillations in the tropical UTLS as stated in the MLS Data Quality Document (Livesey et al., 2022). This should be mentioned in Sect. 2.1 where it should be justified that trend would not be affected by these oscillations. It is also necessary to give an estimation of the bias introduced by these oscillations that would be useful to remind when discussing the comparison between MLS and the model simulations. The justification of not using the averaging kernels should also be mentioned in that section.
3. I suggest discussing the time series plot (Fig. 7 and 8) before addressing the trend analyses. By using this order, the authors will visually introduce the evolution of O3 and CO in the tropical upper troposphere before addressing their trends.
4. There is a long discussion about the CO climatologies at 12°S and N (Fig. 9), mainly on the disagreement between MLS and models at 12°N. What is less discussed is that MLS CO at 12°N displays very different climatology than at 12°S, which is not the case for the models whose climatologies are rather similar at 12°S and N. What would be the reason of this difference between 12°S and 12°N in MLS CO?

Specific Comments

L13: Replace "…chemistry climate models. The models…" by "chemistry climate model simulations. The simulations are from…"

L26: "…CAM-chem and WACCM…". This is confusing, is there two or three model simulations?

L139-142: Again, it looks only two model simulations are used in the paper.

L57: Replace "carbon monoxide (CO),…" by "carbon monoxide – CO,…"

L68: CO produced by biomass burning are also from incomplete combustion so it is redundant with the first part of the sentence. Please, update.

L83: I am not sure to understand the word "priorihydrocarbons", could you define it?

L92: Is the trend from 2000-2010 significant?

L113: Same question here, is the trend significant?

L123-135: What is the point here?

L138-141: It is not clear how many model simulations are used in the paper? Two or three?

L186-192: Could you provide, roughly the precision of O3 and CO in % as well?

L212: "of70" => "of 70"

L305-320: The use of the averaging kernels and introducing the vertical oscillation in O3 profiles must be moved in Sect. 2.1.

L311: Could you mention the figure of Hubert et al. you are referring?

L325-326: replace “model/MLS deifferences” by “differences between models and MLS”, and later, “model/MLS bias” by “bias between models and MLS”.

L348: “Figure 3… and the two models…”. I see three model curves on the figure, not two. See also the Major Comments section above.

L351: “2-sigma level” of what?

L352-353: “The average…”. Is the average also for the three levels 147, 178 and 215? Please, clarify.

L353-355: I don’t understand the meaning of “(we have used the rms of these from the three pressure levels in Fig. 3)”?

L359-362: “If a larger…”. I don’t understand what you mean here, please, rephrase.

L366-380: Is the MLS trend significant or not? It seems not regarding the number given in L370. I must say that the Fig. 5 does not allow a proper reading of the error bars while, on the other hand, Fig. 6 do where it looks like the trend is significant. This must be clarified. If it turns out that the trend is not significant, it is difficult to credit sentence “However, there is not as negative a tendency in the latter two model UT CO trends as in the MLS CO trends…”

L384: Which CO time series? Those shown in Fig. 8?

L422-421: “The fits from the models to the CO behavior at 12°S are quite good.” Do you mean “The fits form the models to the MLS CO…”?

L422: “These curves…” Do you mean for MLS CO because I do not see double peaks in models at 12°N (Fig. 9a). Also, replace “peak” by “maxima” (and also later).

L485-486: “with an overall better/good agreement between the CAM and MLS mapped O3 trends.” I do not agree here, I don’t find that CAM agree better than WACCM with MLS at 215 hPa. Please, comment.

L501-506: I don’t see the point here, could you clarify?

L540: The R^2 figures at 215 hPa should be shown in the supplement.

L547: “over the South Atlantic region, which is likely linked to biomass burning periods in this region”. I guess there is no biomass burning above the Atlantic ocean, so I would revise this sentence.

L657-660: “The TCO…” I am completely lost with this sentence, please, clarify.

L695-695: “Therefore, …” I do not agree about the room for improvement for the models since the trends of MLS and models (CAM-chem-CEDS and WACCM) agree within their uncertainty.

L698-L699: “… clearly not matching the MLS derived negative… trends” As long as the MLS trend is not significant, I would not say that MLS has a negative trend.

L709-710: Is the negative trend between -0.5 and -1.5%/yr significant? If not, I would exclude this comment.

Table and Figures

Table 1: Add a new column for the Model Name and make sure the Model Designation is clearly different from the Model Name.

Fig. 3 and 5 to redo with better choice of colours and thicker error bars to improve readability.

Fig. 7 and 8: Around the linear trend lines, add the uncertainty of the trend in order to see if the trend is significant or not, and if the difference between model and MLS are also significant or not. Also, use different colours. Magenta is difficult to see, Orange and red are not easy to distinguish. And increase the thickness of the lines.