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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Global multi-year $O_3$-CO correlation patterns from models and TES satellite observations" *by* A. Voulgarakis et al.

**Anonymous Referee #1**

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Review of "Global multi–year O3–CO correlation patterns from models and TES satellite observations " by A. Voulgarakis et al.

In this paper, the authors report on O3-CO correlations derived from TES observations and two models in the middle and lower troposphere. Results are compared, their sensitivity to different processing steps and emission sources is investigated and the reasons for similarities and differences between model and measurements as well as between the two models is discussed in detail.

**General comments**

The paper is clearly organised, well written and focused. The analysis is thorough and well documented. The results presented are very interesting and fit into the scope of

ACP. I therefore recommend publication of this paper after minor revisions.

I do however have a general problem with the use of the O3-CO correlations which might be related to the fact that I'm not an expert in this field. To me, the usefulness of the method beyond adding just another test for CTMs is not really clear, in particular the use as a "benchmark in future studies".

- The authors state, that the ozone fields from the two models are quite similar (see Fig. 2) but the correlations between O3 and CO are not. So why do we learn more from evaluating the correlations than from comparing the CO fields of the two models and the observations?
- The authors propose to use the TES data set as a benchmark for future studies. However, as can be seen by comparing Fig. 3d and Fig. 6g, but also 6a and 6c, the correlation is strongly impacted by TES sampling and observation operator, in some regions even changing the sign of the correlation. In my opinion, this imposes a clear limitation on the usefulness of the data set.
- The sensitivity studies using perturbed emissions show that there is no clear link between emissions and O3-CO correlation in the model with the exception of biomass burning. Most of the regions with the highest correlation cannot be assigned to a specific emission, and even more confusing, correlations increase in some regions when removing emissions from vegetation. From this, I got the impression that the O3-CO correlation is not a particularly qualified quantity to identify the *origin* of problems in models. This suspicion is supported by the rather vague discussion of the reasons for the lower correlations observed in the UKCA model – the method is good in pointing out a problem, but less so in helping to identify the reason.

**Detailed comments**



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- In figures 6 and 7, it took me a while to realise that panel (g) is for another season. Please give this panel its own title (Dec+Jan) to avoid this confusion.
- In figure 8, it would be interesting to see a figure of the correlation where all the four emissions are switched off. I realise that this run is probably not available but I think we could learn a lot from this additional comparison.
- In the conclusions, the G-PUCCINI correlations are proposed as benchmark for future comparisons, probably with the argument they are validated by their better agreement with observed correlations. However, I think one should be careful here as a) the agreement with the TES data is not that good in winter and b) the additional information gained from using the raw model data is clearly not validated by the measurements.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 5079, 2011.