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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.

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We wish to thank the 2nd referee for the positive and thoughtful comments. Below we describe the changes that we made, following this review:

1) Reference added.

2) Unfortunately we do not have a clear answer to this, based on the evidence that we have so far. Our results illustrate that the fact that a region can be characterized as "remote" (and thus with low ozone production efficiency) does not necessarily mean that O_3–CO correlations will be negative in this region. But what is driving the largely positive correlations in the Southern Ocean in the raw model output is not clear. We can say that in several of the sensitivity experiments, when removing certain emissions,

correlations get weaker in this region (see Fig. 8 & 9). But there is no specific emission sector that explains the correlation, though some are more drastic (e.g. lightning in the G–PUCCINI model).

We also need to note that when deseasonalizing the O3 and CO time-series, the correlations become weaker and, in some parts of the Southern Ocean, change sign. This indicates that the seasonal O3 and CO variations may be playing a somewhat larger role in driving the O_3–CO correlation patterns in this region. But still, this effect does not seem dominant.

We would prefer to avoid making any conclusive statements on this at this stage, since the evidence is limited.

3) The G–PUCCINI model has more ozone but also more CO in the upper troposphere than in the middle troposphere in some regions. This means that the vertical mixing from above could enhance the correlations. However, since this is quite speculative and we indeed think that it can be confusing, we include this in the parts of this discussion section that we remove from Section 5 in order to shorten it (see answer to comment 4).

4) Following the suggestion of the reviewer, the section has been shortened in various places. Since the discussion on possible transport influences is highly speculative at this stage, we decided that we will remove Figure 11 (this will also help making the section shorter), since it may be leading to conclusions about the influence of transport a bit too early. We agree that this kind of analysis should be done in a consistent way in a separate study (perhaps a follow–up) that could focus on explaining the main drivers of the O_3–CO correlation patterns, especially when it comes to dynamical influences.

On the other hand, note that we kept the discussion on the emissions and net chemical tendency (two first paragraphs of Section 5) almost as it was, since we think that this is an important aspect of this paper: we find that a positive O_3–CO correlation does not always indicate an ozone-producing region, as previous studies assumed, and

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generally should not be perceived as an indication of solely chemical processes.

5) We agree with the reviewer and remove this comment from the discussion.

6) No, we do mean “biogenic emissions”. Basically, the comment underlines that despite the fact that biogenic emissions can be large over Africa, they are not enough to sustain a positive O_3–CO correlation. We add “(which are very important in the tropics)” to make this statement clearer.

7) We agree that the way it was in the original manuscript, it did not allow for a direct comparison with the BASE run. To make the comparison even more direct, we do something different to what is suggested: we plot the absolute O_3–CO correlation for the sensitivity runs, just like for the BASE run. The text has also been altered to reflect this change.

8) This figure has been removed (see comment #4).

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