

Interactive comment on “

Ethane, ethyne and carbon monoxide concentrations in the upper troposphere and lower stratosphere from ACE and GEOS-Chem: a comparison study” by G. González Abad et al.

Anonymous Referee #2

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We found this to be an interesting paper and one which would prove a valuable contribution to the literature once several important issues relating to the validation and model comparisons have been addressed.

13107:26: The text states that “the comparison shows most of the profiles within one standard deviation” however this does not seem to be the case for the most relevant upper-tropospheric data where the ACE errors should be at their minimum. Comparing

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the ACE C₂H₂ to the 2003 MkIV data points (which are the only profiles presented for this altitude) the values seem very different, with ACE peaking at 0.070 ppb compared to just 0.045 ppb for the MkIV profiles, like-wise it appears that there are significant differences in the C₂H₆ (0.55 ppb ACE vs 0.35 ppb MkIV) and CO (110 ppb ACE vs 80 ppb MkIV). This is especially important as the comparisons later in the paper are performed with the 8.5 km data which we do not feel is shown to be in good agreement with the validation data provided.

We appreciate the difficulty with performing these comparisons and the need to average over large geographical areas to account for the sparse nature of the ACE measurements but feel that the author has not fully addressed the differences between the two datasets which are not in as close agreement as suggested by the text.

13109:3: The text states that the GEOS-Chem data has been interpolated to the satellite altitude grid. No mention has been made of the averaging kernels and whilst the satellite altitude grid is 1km, the width of the ACE averaging kernels is not as fine. We feel that for a correct comparison to the model data it is important that the averaging kernels are taken into account, especially for these hydrocarbons emitted by biomass burning which can have sharp vertical gradients.

13110:14: “These hotspots are due to. . .” We feel that it would be useful here to provide evidence for this statement, by either referring to the model emission inventories or to other data such as MODIS fire detections.

13110:22-25: These two sentences repeat information that has been provided earlier in the manuscript.

13112:27: “fairly well” – This is not very quantitative and should be revised.

Section 5 and Figures 12/13/14

Whilst potentially providing very interesting results we do not feel that the author has discussed the analysis at the end of Section 5 in enough detail. While there is some

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discussion on mean biases, there is none on the variability of the data.

For example, for all three species, the Eurasian data is much flatter for ACE than it is for GEOS-Chem. Is ACE failing to capture this variability due to the necessary spatial averaging or are the model emissions incorrect? For North America, again the ACE data doesn't seem to capture the same variability as the model data. This is especially true for C₂H₂ where the datasets are very different with huge biases in both winter periods and correlations of 0.02 for Summer/Autumn.

The statement that the mean biases are smaller than 40% for all molecules does not seem to be true based on the provided tables. We suggest that this statement is revised.

We also feel that we again need to emphasise our earlier point relating to averaging kernels. If these have not been correctly taken into account then the authors should at a minimum show what the effect of neglecting them is and the impact it has had upon these comparisons.

Technical/Typo/etc

13104: 14: non-linear

13108:23: Is "lumping" a technical term? If not we suggest changing it to "averaging" or something more appropriate.

13111:14: 17% lower "that" the satellite retrieved concentrations -> "than"

13111:20 by "a" 15

13114:1: add comma after "CO"

Figures: We also agree with Reviewer 1 regarding the faintness of some of the figures and are happy to see that this will be addressed.

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