

Interactive comment on “Acetone variability in the upper troposphere: analysis of CARIBIC observations and LMDz-INCA chemistry-climate model simulations” by T. Elias et al.

Anonymous Referee #2

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This paper presents a detailed comparison of the LMDz-INCA CTM simulation of acetone with CARIBIC observations in the upper troposphere. The authors state that the goals of the paper are to describe the variability of acetone, define constraints to improve tropospheric modeling, and investigate how representative the dataset is. The authors do address the first and third goals, finding significant seasonal and geographical variability in the CARIBIC observations, which might be difficult to capture on a monthly basis. However, I find that the most interesting goal, related to actually improving understanding of the acetone budget and processes is not really addressed. In that I agree with the first referee that one is left a little bit unclear as to what was actually learned in terms of the budget of acetone and how could the model simulation

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be improved.

There are numerous language issues in the text, and I highly recommend that the authors have a native english speaker carefully read and edit their manuscript. This would greatly increase the readability of the results.

Given that most of the previous CTM evaluations relied on the dataset compiled by Emmons et al. (as noted by the authors), it would be useful to repeat the comparison with the LMDz-INCA model and see whether it presents an improved simulation relative to previous studies (including one with the same model). It would also be useful to address how information from this previous dataset (collected mostly over oceanic regions) and the CARIBIC dataset (with more info over continental regions) actually complement each other and are consistent with each other.

Minor comments

1) Could the authors elaborate on the actual differences between the Folberth study using the same model and their own? It seems from the description in the text (section 2.1) that the only changes relate to a slight increase in resolution and in the number of chemical species. It is unclear whether the acetone quantum yield was also updated in Folbert et al. or not. Table 1 shows that the emissions used in the present study and this previous study are actually different (biogenic + biomass burning emissions). A more in depth discussion of the differences would be useful. Why aren't the residence time and global atmospheric burden listed for the Folberth et al. study?

2) page 9179 line 25 (section 3.2). "the height above the tropopause defined by Spring and Zahn (2010)" a bit more detail on this would be useful. How are stratospheric measurements identified? Does the observed tropopause correspond well with the model tropopause on a flight by flight basis?

3) Figure 7. This figure shows only the model simualtion. It would be useful to show comparisons of observed vertical profiles.

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