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Atmospheric Chemistry and Physics Discussions

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 #1

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General comments:

The authors present a detailed analysis of the LMDz-INCA simulation of acetone, through comparisons to aircraft and (some selected) surface observations. It is obvious that a lot of effort has gone into this work. However, without any disrespect to the authors, I found it hard to keep interested in their results despite what is quite a decent model-observational comparison. I would therefore urge the authors to really make an effort to shorten the manuscript and make it much more 'punchier'. For example, during the comparisons there are lots of absolute quantities/values given in the text, when sometimes a mere percentage bias is more intuitive of the model performance.

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Similarly, it is hard to know what conclusions can be drawn when differences to the observations are found. In such instances (e.g., top left panel of Fig 2) are the model emissions, chemistry or transport incorrect? Therefore, after a good 3 hours of solid reading, I'm still left wondering what this analysis tells us about the acetone budget that we didn't already know from previous studies. That said, I still recommend publication in ACP, but only once the manuscript has been made more concise.

Specific comments:

General comment: Why are flight numbers inserted in the main text at certain points, when details of each flight are not given anywhere?

Page 9168, line 16: properly define OH and HO2.

Page 9168, line 17: Provide the reader with a suitable reference that explains tropospheric ozone production from CO and VOC oxidation.

Page 9168, line 23: The authors casually write '. . .considering 300 pptv acetone in a photochemical box model was sufficient to reach near agreement between measured and simulated OH concentration in the UT.' Without an in depth read of the cited Wennberg et al. (1998) paper, I could assume the difference between the measured and observed OH could be due to any number of issues; please explain this work in more detail.

Page 9171, line 18: What does '. . .)' imply?

Page 9171, line 19: Put the model resolution in degrees rather than the number of grid cells.

Page 9171, line 21: Does the chemical scheme include the latest findings from the Peeters et al (2010) work which allows extra HOx generation from isoprene oxidation?

Page 9172, line 8: What are 'primitive equations' – explain.

Page 9172, line 18-19: What were the sizes of these positive/negative biases from the

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Folberth et al. 2006 paper?

Page 9176, line 10: The vertical profile of the global budget also depends on transport, and from a modelling perspective our ability to correctly simulate boundary-layer mixing (to vent from the surface to higher altitudes) and full atmospheric mixing processes e.g., convection.

Comment on section 2.2.2: There are lots of numbers being thrown at the reader, which truthfully are not absorbed. Please consider isolating the truly important points/budget terms and make the section more concise.

Page 9177, line 13 to Page 9178, line 19: I appreciate the thoroughness of the authors, but I think this section of text could be made significantly shorter.

Page 9180, line 13: Shouldn't 'Fig. 5' actually be labelled as 'Fig 3' since this is the third figure referenced in the text.

Section 4.1 and Table 2: Maybe more useful metrics like the normalised mean bias and error would make it easier for the reader to diagnose the model performance.

Page 9184, line 28: Why wasn't the model compared with the MIPAS (or ACE) data? Surely the satellite observations would give a further opportunity to evaluate the model.

Caption to Table 2: Should 'bolt police' mean bold face.

Reference:

Peeters, J., and J.-F. Müller, HOx radical regeneration in isoprene oxidation via peroxy radical isomerisations. II: Experimental evidence and global impact, Phys. Chem. Chem. Phys., 12(42), 14227-14235, DOI:10.1039/C0CP00811G, 2010.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 9165, 2011.