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Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on “Simulating organic species with the global atmospheric chemistry general circulation model ECHAM5/MESSy1: a comparison of model results with observations” *by* A. Pozzer et al.

Anonymous Referee #1

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General comments

The paper describes the evaluation of organic species concentrations as simulated by an atmospheric chemistry general circulation model. Overall it is a nice paper that deserves publication in ACP although there is a need for more detailed discussions at some places as further discussed in the following. Also in some cases, the rationales for choosing (or omitting) some sensitivity simulations were not always entirely clear to me.

Specific comments

Section 2.1. *Model*. I think more details are needed in the model description and simulations setup. I understand that the model has been described in several other papers, but still information is missing on several points that are specifically important for the present paper. For example: what do the Authors mean by “weakly nudged”?; which chemical mechanism do they use (this is important for the representation of secondary oxygenated compounds, for example)? I also think a table that summarizes the global emissions (including the different source contributions) of the species examined in the manuscript is clearly missing in the present version of the paper.

Section 3.1. *Aircraft measurements*. What are the rationales for using Figure 2 rather than Figure 1? The Authors say very little about the differences between the two approaches but I think they should at least explain why (and in which way) they feel the original Taylor plots should be modified.

Section 5.1. *Alkanes*. I disagree with the statement that “a good agreement with aircraft observations from the TOPSE campaign is achieved”. The simulated C_2H_6 concentrations appear too low to me. Are there observed vertical profiles of C_2H_6 downwind of Asia? This would also provide indications on possible problems in the anthropogenic emissions, as suggested by the Authors. Also, why did they not conduct a sensitivity simulation with increased anthropogenic emissions of C_2H_6 if they feel that "The anthropogenic emissions in the model, in fact, are not sufficient to perfectly match the observed values", as stated in the manuscript?

Section 5.2. *Alkenes*. The Authors choose to reduce the oceanic emissions of ethene, but not that of propene, while a similar overestimate seems to be found over oceanic regions. Could they comment on that?

Section 6. *Isoprene*. What do the Authors mean by “Further analysis has revealed that simulation of too high surface temperature results etc.” Did they refer to the Bengtsson et al. 2004 paper? If yes, they should reference the paper at that point. But if they have conducted additional analysis/evaluation, they should provide more details. Also, the statement about isoprene emissions, i.e. “Consequently, a commonly applied approach in atmospheric chemistry studies, which do not focus on isoprene, is to use a 25 substantially smaller global flux, ranging from 220 to 350 TgC yr^{-1} (Brasseur et al., 1998; von Kuhlmann et al., 2004).” seems incorrect to me. At least two recently published studies, e.g. Horowitz et al. [2003] and Lathière et al. [2006], used substantially larger global emissions for isoprene, 410 and 460 TgC/yr for isoprene, respectively. Could the Authors comment on that and rephrase their discussion? Also it is unclear to me where are the isoprene measurements coming from (would it be possible to add error bars to the measurements for example?). Finally, I am not sure what I learn from Figure 18. I wonder whether the isoprene “problems” could be due to something else than emissions. For example, have the Authors looked to what extent their simulated isoprene concentrations change when they change the operator splitting in their model?

Section 7.5. *Oxygenated compounds, Acetone*. I would like to recommend to the Authors to include an additional sensitivity simulation with increased oceanic source for acetone (similarly to what they have done for the CH_3OH sources), in particular because a good representation of acetone has important implications for PAN, as stated in the manuscript. This would be interesting to conduct such a simulation with their model, as it includes the revised photolysis rates.

Section 8.1. *Sensitivity simulations and CO dry deposition*. I am a bit confused by the Authors’ discussion on the CO dry deposition. What are their conclusions in terms of the significance of this sink for the CO global budget? In their discussion, they mention the Horowitz et al. [2003]’s paper that reports similar results to what they find (i.e. CO dry deposition is likely to be insignificant) but they omit to mention other studies that find opposite results, for example Gergamaschi et al. [2000] and Folberth et al. [2006] that reported a small but non insignificant dry deposition of 301 and 135 Tg CO yr^{-1}, respectively. Could the Authors comment on that?

References

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