**Atmospheric
Chemistry
and Physics
Discussions**

Interactive
Comment

*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.**

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We thank the anonymous referee for the constructive comments on this work.

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# Specific comments

## Section 2.1, Model

As replied to the referees #2 and #3 we agree to extend the model description section and to add more information. However, we do not intend to increase largely this section, being the necessary information already published in Jöckel et al. (2006), in the same special issue. For weakly nudging we intend "...the nudging is weak enough to not disturb the self-consistent model physics..." (Jöckel et al. 2006). The complete chemical mechanism used in the simulations has been presented in Jöckel et al. (2006) as electronic supplement. However, we will add these informations to the revised version of the manuscript. Table 5 in the manuscript summarizes the global emissions of the examined species. A more detailed description of the different source contributions is presented in Ganzeveld et al. (2006).

## Section 3.1

As replied to referee #3, we agree in expanding this topic and describe in more detail the differences between the two Taylor diagrams.

## Section 5.1

We completely agree with referee #1 that the simulated $C_2H_6$ mixing ratios are lower than the measured mixing ratios during the TOPSE campaign. It has been noticed that there is an underestimation of the anthropogenic emissions in North America. Nevertheless, the vertical profile shape is correctly reproduced and the simulated mixing ratios are within the measurement range. Compared with observations downwind of

Asia, the simulation does not show a clear overestimation of this trace gas. No sensitivity simulation has been conducted for this tracer, because of the difficulties to establish the correct increase needed to match the observations. Moreover, the total $C_2H_6$ emission used in this work is already one of the highest present in the literature (see Tab.5). The complete match between observed and simulated mixing rations needs therefore a more detailed analysis than "approximative" assumptions.

## Section 5.2

Both ethene and propene show the same behaviour over oceanic regions. However, in the case of propene it is clear that in the free troposphere this trace gases is drastically underestimated. We do not think it to be useful to "tune" the emissions as long as the chemistry has not yet been fully understood. In the case of propane, in fact, the incomplete understanding of its photochemistry would invalidate our attempt to constrain the emissions. Hence, as first attempt, we tried to improve the representation of the photochemistry. Because the results for propene have not significantly changed, we do not think that a simulation with a decreased oceanic emission could give useful results. Being ethene partially reproduced in the free troposphere (see fig.13 in the manuscript), a sensitivity simulation was performed, to analyse the effect of the reduction to the vertical profile of this trace gas.

## Section 6

As replied to referee #2 and #3, this section will be completely rewritten. We agree to rephrase the discussion, taking in account the studies of Horowitz et al. (2003) and Lathieré et al., (2006). The misleading Figs.17 and 18 will be removed, because they do not give any additional information. In the simulation analysed here, isoprene has

been emitted by on-line calculations. The measurements that have been compared with the simulation are from different sources and have different time coverage. As suggested by the anonymous referee, a brief analysis of the isoprene mixing ratios in the case of inverted emission/deposition processes (change in the operator splitting) do not show significant differences, compared to the original evaluation simulation. The changes in the mixing ratios in the emission regions are generally lower than 5%. This confirms that the on-line calculated emissions or non-understood chemistry are most probably the reason of the original isoprene overestimation. However, we agree with the referee that these informations cannot provide a clear picture of the problem.

## Section 7.5

We completely agree that acetone is extremely important as precursor of PAN. However we disagree to perform an additional sensitivity simulation with emissions of acetone from the oceans. The information available in literature (also mentioned in the manuscript) are not sufficient to constrain the oceanic emissions. We firmly believed that this topic needs to be addressed in a more specific and accurate way in future works.

## Section 8.1

In this work a dry deposition parametrisation for $CO$ has been tested; a fixed deposition velocity (0.04 $cm/s$) has been used, excluding desert and cold tundra (see chapter 8 in the manuscript). We have shown that with such a parametrization, based on the few measurements available, the dry deposition of $CO$ is negligible. We agree that the reference to Bergamaschi et al.(2000), which used a similar approach and obtained an opposite conclusion, must be included in the revised version and that the

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discussion must be extended. Nevertheless, the values obtained for the total amount of deposited $CO$ in this work (74 Tg/yr) and in Bergamaschi et al. (2000) ($\approx$ 300 Tg/yr) are discordant. However, fundamentally different approaches have been used in this work (forward calculation) and in in Bergamaschi et al. (2000) (posteriori estimates obtained with inverse modelling approach). We will expand the discussion of $CO$ dry deposition in the revised version of the manuscript, mentioning that large uncertainties still exist.

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