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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of biogenic hydrocarbons on tropospheric chemistry: results from a global chemistry-climate model" *by* G. A. Folberth et al.

G. A. Folberth et al.

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Specific Comments:

1) We have expanded section 3.2 of the original manuscript which now includes a brief discussion of prognostic methane concentrations as calculated by the model and compares them to observations. As argued in the manuscript, the methane soil sink appears to be non-negligible but small on the global scale. Furthermore, dry deposition at the surface in the model is calculated at each time step for all the species subject to this removal process. To our knowledge, there is no parameterization of the methane uptake by the soil currently in existence that can be used in global chemistry models.

In addition, we would argue that negligence of this sink will introduce some further uncertainty in our global methane budget in the model, but we would assume that the uncertainties already persisting around the numerous processes contributing to the methane budget, such as primary and secondary sources, parameterization of the photochemistry, etc., significantly exceeds the 5% mark.

2) The manuscript has been revised accordingly.

3) The refree's conclusion that the ozone upward flux has changed between the two model versions is in fact only part of the answer. As a consequence of the ongoing effort to keep up the model to the current knowledge and to improve existing parameterizations several portions of the model have evolved during the transition from LMDz-INCA-CH4 to LMDz-INCA-NMHC. These changes include a thorough revision of the dry depostion routines but also an evolution in the dynamical part (i.e., LMDz) of the CCM. Thus, the two model versions differ in more than just the chemical scheme which means that the estimates for the net stratospheric influx of ozone cannot directly be compared between the two model versions without taking these changes into account. One has to keep in mind that the net stratospheric ozone influx in LMDz-INCA is calculated as the residual of photochemical ozone production and loss as well as ozone dry peposition at the surface. Net O3 STE, therefore, is sensitive to changes not only in the dynamics of the model, i.e., the downward and upward fluxes of ozone themselves, but also to changes in physical and chemical processes. Preferably, net O3 STE should be determined directly from the difference between ozone downward and upward flux which would require thourough book-keeping of ozone transport not yet realized in LMDz-INCA. Hence, the ostensible difference in the ozone flux from the stratosphere is in fact a consequence of the sum of the afore mentioned changes in the model. The corresponding paragraph has been revised and this point has been made more transparent.

4) This is a very good point. There is much more to be learned from the the feedback between biogenic isoprene and anthropogenic NOx emissins. We have included a new

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Figure showing the isoprene and NOx surface emission rates for January and July. The discussion of the isoprene-NOx-separation and -coincidence is now discussed in more detail.

5) See response to specific comment 4)

6) We highly appreciate the comment and have revised the discussion of the isoprene experiment accordingly. Figure 16 in the original manuscript has been revised according to the referee's suggestion. A discussion of the impact of isoprene photooxidation on the tropospheric HOx budget has been included.

7) We agree with this comment. Eventually we have decided to omit this paragraph in view of the revisions that have been performed on the entire subsection.

8) The corresponding author would like to thank the referee for drawing the attention to this inconsistency. It did reveal a mistake in the calculation of those quantities which has been corrected in the revised version. The corresponding author would also like to apologize for this lack of diligence.

9) We would like to thank the referee for this very helpful comment which to our opinion helped to improve our analysis considerably. We have revised the entire subsection accordingly and also reworked Figure 18 in the original manuscript. As to the question whether averaging precedes calculation of the MVK/MACR ratio or not the answer is as follows: The model only stores diurnal mean concentrations of the chemical species. From these values monthly mean MVK and MACR concentrations have been prepared and then the MVK/MACR ratio has been calculated in the post-processing phase. It would seem, therefore, that the MVK/MACR ratios presented in the manuscript are actually weighted towards daytime photochemistry.

Technical Comments:

page 10521, line 3: Agreed, reference included

page 10525, line 19: Agreed

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page 10525, line 20: Agreed

page 10526, lines 4-9: The choice is made globally. In Table 1 species are marked "l" for "long-lived" and "s" for "short-lived", which was also intended as a label for the solver to be used with the individual species. This has been made more clear now in the text.

page 10532, line 13: Agreed

page 10533, line 21: Agreed, the text has been revised accordingly.

page 10534, line 23: Agreed

page 10535, line 25: Agreed

page 10536, line 3: Agreed

page 10536, line 25: Agreed

page 10544, lines 1-2: Agreed

page 10544, line 7: Agreed

page 10544, line 28: Agreed

page 10545, line 8: Agreed

page 10547, line 11: Agreed, text has been revised accordingly.

page 10547, line 15: Agreed

page 10551, lines 20-21: Agreed. To our knowledge there is no parameterization avilable at this stage which would allow to take in to account this sub-grid process in global chemistry-climate models. In view of this reason and the fact that CH4 dry deposition is significant but still small we have decided to neglect this process in the model at this stage. The text has been revised accordingly.

page 10554, line 8: Agreed

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page 10554, line 10: Agreed

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 10517, 2005.