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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global modelling of secondary organic aerosol in the troposphere: A sensitivity analysis" *by* K.Tsigaridis and M. Kanakidou

Anonymous Referee #1

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General comments

The manuscript describes a global 3-D chemistry/transport model which is mainly focussed on organic aerosol components, especially secondary organic aerosol components. The goal of the paper is the identification of gaps in the understanding of the formation of organic aerosols. Several scenarios have been defined, in which physical and chemical parameters were varied, aiming on an estimation of the possible range of global SOA production and the investigation of uncertainties of the model. Certainly, the topic of the manuscript – a sensitivity analysis of global SOA modelling – is of interest for the readers of ACPD and ACP. The introduction appears to be complete and comprehensive. Earlier work is adequately recognised. The budget calculations – although connected with a tremendous uncertainty – are useful for a

larger number of scientists. Especially the incorporation of the height dependence of aerosol formation processes is interesting and – to my knowledge – a new aspect of current SOA modelling efforts. However, some critical remarks to improve the are given below.

Specific comments
Page 2883: The authors state that benzene is not able to form particles. This is not correct
(see e.g. http://diac.nerc.ac.uk/documents/WengenTalks/KlausWirtz.pdf).

Page 2897: The activity coefficient. I am not sure if I understand the authors correctly. Based on Table 2 and the scenarios S1.4 and S1.5, the authors claim that the results are not very sensitive to this parameter. However, how are the values in Table 2 justified (for example, what if values of interactions are 8x10^{-3} instead of 0.8) ? What exactly means *no activity coefficient* as well as *strong dependence*. The authors should elaborate this issue.

Page 2890: I completely understand that it is quite difficult to get information about ageing of primary hydrophobic aerosol. Nevertheless, applying an equation which is based on experiments with benzo(a)pyrene is – in my opinion – questionable. Chemically speaking, the PAHs are certainly a very special group of particle phase organics and therefore their use as surrogate for all kinds of primary carbonaceous aerosol components problematic. Perhaps it would be better not to quantify the insoluble-to-soluble decay of POA components than just correlate it with global humidity (which is of course a consequence of the model used).

Finally, I would recommend to structure the conclusions part a little bit better. Ideally,

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the reader should get a ranking list of the most important input parameters for SOA modelling, however, also a more detailed discussion of the uncertainty of the individual parameters should be presented.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 2879, 2003.