

Interactive
Comment

Interactive comment on “Estimating the influence of the secondary organic aerosols on present climate using ECHAM5-HAM” by D. O’Donnell et al.

Anonymous Referee #3

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General Comments:

This paper presents the coupling of a module suitable for the computation of the secondary organic aerosol (SOA) concentrations with the global aerosol-climate model ECHAM5-HAM. The model calculates SOA formation from the precursors but, for each precursor only the major SOA production pathways via gas phase reaction and subsequent partitioning of the semi-volatile products to the aerosol phase is considered. The innovation of this paper emerges from the consideration of size segregated gas-aerosol partitioning and the major outcome is an estimate of the direct and indirect effects of SOA. However, the authors have to push forward the new findings of this paper.

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Overall, I consider that the manuscript is suitable for publication in Atmospheric Chemistry and Physics after addressing the following points: There is a number of points in the manuscript that could be further improved in terms of reorganization (e.g §4.1, comparison with measurements and §5.8, model results and surface observations). Please, also, consider the phrasing in different parts of the paper. For instance, “we must consider what is meant by the term M0. . . (p.2414, l. 10)” or “So far, we have presented nothing new. . . (p2416, l. 13)” or even “We now come to the question of . . . (p.2416, l. 16)”.

Specific comments:

a) The abstract needs to be reorganized. The first part of the abstract (line 1-16) fits better in the introduction than in the abstract. The authors have to be more consistent - presenting the results and the conclusions of a work. It is also better to be as quantitative, as possible, than providing qualitative evaluations: “Longwave effects are small”.

b) In introduction (Line 12-19) the authors have to mention also recent advances in organic aerosol (OA) modeling that account for ‘traditional’ or not primary OA (POA), Secondary OA (SOA). For example, Robinson et al. (2007) and Pye and Seinfeld (2010) refer that a large fraction of “traditional” POA is also semi-volatile, being a mixture of compounds with vapor pressures that span several orders of magnitude.

c) Page 2409, line18: “of the order of hundreds of Tg yr⁻¹”: Please quantify the estimates of global emissions of biogenic and of anthropogenic precursors in terms of TgC yr⁻¹.

d) In section 2 (page 2412, line 20) “trimethylbenzene is a known SOA precursor”. Please provide reference.

e) In section 2 (page 2412, line 28) the authors mention that “oxidation reactions with OH, O₃ and NO₃ are taken into account”. As far as it concerns SOA production path-

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ways, it is useful to mention that in case of aromatics both O₃ and OH are taken into account in model calculations, if this is the case. The authors appear not to pay enough attention to the potential production SOA from NO₃ chemistry.

f) In paragraph 2.6 (page 2419, line 19-23), the appropriate references for wet and dry depositions schemes have to be provided together with a short outline.

g) Page 2420, line 8: Discuss more the importance of aqueous-phase chemistry in SOA production.

h) Page 2421, lines 3-7. Please provide the annual aromatic emissions from other databases (e.g. EGRAR v.2 or POET) and compare with the aromatic annual emissions of FT2000. i) Model results and surface observations, Figs 13. and 15.: Provide statistics for the model evaluation.

Minor comments:

a) p.2420, lines 16-17: it would be useful to provide abbreviations of the 3 simulations as in the capture of Fig. 10 (e.g. without SOA (no-SOA or 0-SOA), with SOA (SOA) and bSOA when only the biogenic SOA are taken into account.

b) p.2469, Fig. 12: The color scale of the figures requires modification to increase clarity.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 2407, 2011.

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