

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

**D. O’Donnell et al.**

declan.odonnell@env.ethz.ch

Received and published: 17 May 2011

We would like to thank the Reviewer for his/her helpful comments, which will contribute to an improved final paper. We reply to each of these comments below.

General Comments: "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."

Response: We agree that the novel elements are not as clear as they ought to be, and will improve this in the revised manuscript.

C3504

General Comments: 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).

Response: We find it difficult to see what the Reviewer has in mind here. §5.8 is a single, rather short paragraph, and, as the model has been evaluated against three measurement networks, we believe that a subchapter for each within §4.1 is appropriate.

General Comments: "Please, also consider the phrasing in different parts of the paper."

Response: We agree that the examples given by the Reviewer are not good, and we will revise the manuscript with this comment in mind. The specific examples given by the Reviewer will be rephrased, as will any similar such cases.

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".

Response: Comment accepted. The abstract will be substantially improved in the revised manuscript.

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.

Response: Comment accepted.

c) Page 2409, line18: "of the order of hundreds of Tg yr<sup>-1</sup>": Please quantify the estimates of global emissions of biogenic and of anthropogenic precursors in terms of TgC

C3505

yr-1.

Response: Comment accepted.

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

Response: References will be provided in the revised manuscript.

e) In section 2 (page 2412, line 28) the authors mention that "oxidation reactions with OH, O<sub>3</sub> and NO<sub>3</sub> are taken into account". As far as it concerns SOA production pathways, it is useful to mention that in case of aromatics both O<sub>3</sub> 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 NO<sub>3</sub> chemistry.

Response: We have made the simplifying assumption of one SOA production pathway per precursor, which is an acknowledged shortcoming of this model. We also point out that we have neglected the effect of NO<sub>2</sub> on SOA yields. These assumptions are made in order to keep the SOA chemistry scheme simple enough to solve analytically, so that the model does not require a numerical chemistry solver. This is done to keep the computational costs down.

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.

Response: Comment accepted.

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

Response: Comment accepted.

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.

C3506

Response: We find it difficult to see how this suggestion fits with the purpose of the paper. This does not mean it is an uninteresting idea, indeed a comparison of SOA model results using different emission inventories could be a useful exercise. However, comparison of emission inventories is not our purpose, and is not within the scope of this paper.

i) Model results and surface observations, Figs 13. and 15.: Provide statistics for the model evaluation.

Response: We have given mean and median values for the observations and for the relevant model runs in chapter 4.1. Their presentation will be improved in the final manuscript, so that the link to these figures will be clear.

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.

Response: Comment accepted.

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

Response: We will try to improve the figure in the final manuscript.

---

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

C3507